# The Impact of *Bolsa Família* on Child, Maternal, and Household Welfare

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## 1. Introduction

*Bolsa Família* provides financial assistance to approximately 12 million poor Brazilian families. It is a conditional cash transfer (CCT) program in which participants agree to a series of conditions regarding prenatal care, vaccinations, health checkups, school enrollment, and school attendance. In return, they receive a monthly payment per child attending school to a maximum of three children. Families with very low incomes also receive a Basic Payment that does not depend on household composition. Payments are made preferentially to the female head of household.

This report provides evidence on the impact of *Bolsa Família* on children, women, and households based on data collected in 2005 and 2009. Operational dimensions of the program have received extensive attention and a number of studies have documented how living standards of *Bolsa Família* beneficiaries have evolved over time. However, none of these previous studies demonstrate causality between these changes and participation in *Bolsa Família*. Here, we do so, answering the question, "Are *Bolsa Família* families better off in 2009 than they were in 2005 *because* of *Bolsa Família*?[1] We begin with a brief explanation of how we assess impact before turning to a summary of impacts on children, women, and households. The final section summarizes.

## 2. Understanding how to assess *Bolsa Família*'s impact

### (a) Principles

In this impact assessment, we use "double difference" and "single difference" methods. Both require data from households receiving *Bolsa Família* and those that do not ("with the program" / "without the program") and double difference methods require data on *Bolsa Família* beneficiaries and non-beneficiaries before *Bolsa Família* began and after its implementation ("before/after"). To see why these data are necessary, consider the following hypothetical situation. Suppose we only had data on *Bolsa Família* beneficiaries collected at two points in time: at baseline (before they started receiving benefits) and at sometime afterward (the "follow-up"). Suppose that in between the baseline survey and the follow-up, some adverse event occurred (such as a flood) that makes these households worse off. In such circumstances, it would appear that beneficiaries have been made worse off—because any benefits of *Bolsa Família* were more than offset by the damage inflicted by the flooding. More generally, restricting the evaluation to only "before/after" comparisons makes it impossible to separate program

[1] We draw attention to two related documents. The technical appendix to this report provides more detailed information on data and methods. de Brauw et al. (2010) provide an extensive review of changes in living standards using the data available to us.

impacts from the influence of other events that affect beneficiary households. To ensure that our evaluation is not adversely affected by such a possibility, it is necessary to know what these indicators would have looked like had the program not been implemented: we need a second dimension to our evaluation design that includes data on households "with" and "without" the program. The fundamental problem, of course, is that an individual, household, or geographic area cannot simultaneously undergo and not undergo an intervention. Therefore, as part of our evaluation, it is necessary to construct a counterfactual measure of what would have happened if the program had not been available, and this is why we also need the "with/without" comparison. We do so below.

Table 1 shows how the double difference method works. The columns distinguish between groups with and without the program. We denote groups receiving (with) the program Group *I* (*I* for intervention) and those not receiving (without) the program as Group *C* (*C* for control group). The rows distinguish between before and after the program (denoted by subscripts 0 and 1). Consider one outcome of interest—the measurement of school enrollment rates for children aged 7-15. Before the program, one would expect the average percentage enrolled to be similar for the two groups, so that the difference in enrollment rates ($I_0 - C_0$) would be close to zero. Once the program has been implemented, however, one would expect differences between the groups and so ($I_1 - C_1$) will not be zero. The double-difference estimate is obtained by subtracting the preexisting differences between the groups, ($I_0 - C_0$), from the difference after the program has been implemented, ($I_1 - C_1$). Under certain conditions (see below), this design will take into account preexisting observable or unobservable differences between the two assigned groups, thus giving average program effects.

**Table 1: Calculation of the double-difference estimate of average program effect**

| Survey round | Intervention group (Group *I*) | Control group (Group *C*) | Difference across groups |
|---|---|---|---|
| Follow-up | $I_1$ | $C_1$ | $I_1 - C_1$ |
| Baseline | $I_0$ | $C_0$ | $I_0 - C_0$ |
| Difference across time | $I_1 - I_0$ | $C_1 - C_0$ | Double-difference $(I_1 - C_1) - (I_0 - C_0)$ |

For certain outcomes, data constraints prevent us from using double difference methods either because information on the outcome was collected only in the follow-up survey or because information collected across time cannot be linked. In these cases, we construct a single difference estimate of impact based on the difference between $I_1$ and $C_1$. As described below, although we are unable to use baseline outcomes in these cases, the methods we use ensure that we have comparable baseline outcomes—so that $I_0 = C_0$—in which case double-differencing is equivalent to single-differencing.

**(b) Defining intervention and control households**

There are two challenges in applying differencing methods to Bolsa Família: (1) the fact that *Bolsa Família* built on prior programs effectively precludes the use of randomization as a means of identifying impact as has been done in other evaluations of CCTs in Latin America; and (2) when the baseline survey, called AIBF-1, was implemented in 2005, there were a significant number of households who had already started receiving *Bolsa Família* transfers, which makes the before/after comparison difficult. Given this, we do the following. AIBF-1 noted whether respondents were already receiving *Bolsa Família* payments and whether respondents had been registered in the *Cadastro Único para Programas Sociais (CadÚnico)*.[2] The follow-up survey, fielded in 2009 and called AIBF-2, reinterviewed the same households who had participated in AIBF-1 and collected detailed information on who was currently a *Bolsa Família* beneficiary. With this information, we can divide our sampled households into six groups.

**Table 2. Sampled households, by AIBF-1 and AIBF-2 groups**

| | AIBF-1 Group (2005) | | |
|---|---|---|---|
| | **Intervention Group** | **Control Group 1** | **Control Group 2** |
| | **BF Recipients** | **BF Non-recipients in *CadÚnico*** | **BF Non-recipients** |
| **AIBF-2 Group (2009)** | | | |
| BF Recipients | 1,844 | 1,121 | 1,707 |
| BF Non-recipients | 929 | 1,352 | 3,416 |

Notes: The 1,064 households that did not conform to these groups in AIBF-1 are omitted.

The Intervention Group households in AIBF-1 were already receiving transfers from *Bolsa Família* in 2005. Control Group 1 households were listed in the *Cadastro Único*, but were not yet receiving *Bolsa Família*. Control Group 2 includes all households not yet receiving transfers from *Bolsa Família*, regardless of whether they were listed in *Cadastro Único*; we might be concerned that this group contains households that are better-off than households that actually receive *Bolsa Família* payments. Each of these groups could be either a *Bolsa Família* recipient or a non-recipient in 2009. With this structure, we can consider three possible comparisons.

For Comparison 1, we note that two potentially useful groups of households to compare are those within Control Group 1. Just under half of those households began receiving *Bolsa Família* payments between AIBF-1 and AIBF-2, and these households are likely to have had broadly comparable income levels at baseline. However, it has two drawbacks. First, it omits the majority of data that are available for the evaluation. This point may be particularly important in cases when we use subsamples

[2] The *Cadastro Único* is the registry where the details on applicants to a number of Brazilian social programs, including *Bolsa Família* are recorded. It is used in the selection of beneficiaries.

of the data; sample sizes (particularly when we disaggregate by regions) may become too small in such cases to detect program impacts. Second, it ignores information about beneficiaries of the program, and if households included in Control Group 1 are systematically different than those in Control Group 2 who subsequently enter *Bolsa Família*, impact estimates may not reflect the true impacts of the program.

As a result, we consider a second comparison (Comparison 2), which combines new recipients in Control Groups 1 and 2 and compares them to the non-recipients in Control Groups 1 and 2. This strategy takes advantage of more of the sample, but it also potentially runs the risk of including a significant proportion of households within Control Group 2 that are not comparable with *Bolsa Família* recipients, as they are (and have always been) too wealthy to receive payments. As a result, we modify the groups above to remove all households in Control group 2 that both never received *Bolsa Família* payments and that do not appear in the *Cadastro Único*, and hence never even applied to receive *Bolsa Família* payments. This condition removes 2,114 households from the comparison, leaving 1,302 households in the Control Group 2 who are non-recipients.

Comparison 3 adds recipient households from the Treatment group in AIBF-1 to the treatment households, but does not change the Control group. The advantage of Comparison 3 is that it uses all of the available data on *Bolsa Família* recipients. However, there are two drawbacks. First, the Control Group in this comparison becomes small relative to the size of the Treatment group. We do not bring in non-recipients in the Intervention Group from AIBF-1 to increase the size of the Control group, as we know that they stopped receiving payments between the two surveys, and as a result they may systematically differ from recipients. Second, adding these households adds a group of households that have been receiving transfers for a long period of time. For both double-difference and single-difference estimates, the addition of these households may improve impact estimates either if new household members are affected or if impacts take some time to occur. However, for double-difference estimates, adding these households may actually detract from impact estimates if, for example, impacts of *Bolsa Família* are immediate; if so, then if we measure the change in outcomes among the Treatment group recipients, we should find no changes attributable to the transfers, since they were already receiving them in 2005.

Our strategy, then, is to estimate impacts of *Bolsa Família* using all three of the potential comparisons. Where we find statistically significant impacts, we then look for common results across the three comparisons, or at least consistent results.

**Table 3. Potential comparisons for impact evaluation**

| | Comparison definitions by number of households | | |
|---|---|---|---|
| **Group** | **Comparison 1** | **Comparison 2** | **Comparison 3** |
| Treatment | 1,121 | 2,828 | 4,523 |
| Control | 1,352 | 2,586 | 2,586 |

Notes: Households that did not conform to these comparison definitions are omitted.

### (c) Propensity score weighting[3]

A requirement of a robust impact evaluation study is that the intervention and control households must be as alike (or "as balanced") as possible at baseline. Properly implemented, randomization of households into intervention and control groups delivers this, and this is the reason why randomization is so often used in assessments of conditional cash transfer programs. In the case of *Bolsa Família*, while the manner in which the comparison groups are constructed helps meet this requirement, it does not guarantee it. For this reason, we need to apply a statistical method of estimating impact using a nonrandom methodology to generate what is called an unbiased estimate of the average treatment effect on the treated (ATT). In addition, given the sampling strategy that underpinned the collection of data in AIBF-1 and AIBF-2, we need a method that can account for two types of weighting. First, we want to use population weights that were constructed for AIBF-1 to account for the proportion of the population that each household in the dataset represents. Second, we want to be able to account for attrition between AIBF-1 and AIBF-2, which is described in detail in de Brauw et al. (2010).

An impact estimator that fulfills these requirements is propensity score weighting (Hirano, Imbens, and Ridder 2003). The basic intuition is as follows. We first estimate a "propensity score," the probability that any specific household is a *Bolsa Família* recipient. We then use the propensity scores to place weights on the control observations. The weights control for the fact that some households in the control group do not have high probability of being *Bolsa Família* recipients based on their observable characteristics; such households receive low weights in estimating the ATT. Other households in the Control group have observable characteristics such that they appear very likely to receive *Bolsa Família* payments, and these households are assigned higher weights. By placing higher weights on households that have characteristics more like recipients and less weight on households that

[3] Full details are found in the Technical Appendix to this report.

have characteristics like non-recipients, we balance observable characteristics between recipients and non-recipients.[4]

In brief, we first graph kernel densities to compare the distribution of propensity scores among recipients with the distribution of propensity scores among non-recipients; we do so for each of the three comparisons. We find that there is very good overlap between the treatment and control groups for each of the three comparisons. This gives us confidence that the estimated propensity scores will help correct imbalances between the treatment and control groups. Second, we test for differences in average characteristics between recipients and non-recipients. Before we use the propensity weights, we find statistically significant differences for many of the characteristics. After the propensity weights are applied, in all three comparisons the average differences are no longer statistically significant. Therefore we can comfortably state that the propensity scores appear to account for significant differences between the groups of recipients and non-recipients for all three comparisons.

## 3. Children's Welfare

We assess the impact of *Bolsa Família* on the following dimensions of child welfare: birthweight, anthropometry, vaccinations, education, and child labor, using Comparisons 2 and 3. Recall that in Comparison 2, intervention observations are households (or individuals in households) that were receiving *Bolsa Família* transfers in 2009 but were not receiving transfers in 2005. At that time, some of these households were registered in the *Cadastro Único*, while others were not yet registered. Control households were not receiving *Bolsa Família* transfers in 2005 or in 2009, although in 2005 some of them may have been registered in the *Cadastro Único*. In Comparison 3, we add to the intervention group households that were already *Bolsa Família* beneficiaries in 2005; the control group remains unchanged. Below we report results for the full sample.

### (a) Birthweight

Survey questions related to birthweight and infant health are available only in AIBF-2 and so we estimate impact using a single difference model. We focus on children aged 0-1 in the 2009 wave so as to make it more likely that the BF status categorizations in 2009 apply to the time frame relevant to our outcomes of interest. Results are reported in Table 4.

[4] The main drawback to the propensity score weighting method is that the variance associated with the estimator is high relative to other estimation strategies. As a result, we are at risk of making statistical Type II errors, which occur when the null hypothesis is accepted even though it is not true. This implies that we may miss significant impacts that *Bolsa Família* has on beneficiaries.

*Bolsa Família* does not have a statistically significant effect on birthweight. However, birthweights averaged 3.28kg for children whose mothers were *Bolsa Família* beneficiaries and 3.21kg for children whose mothers did not receive BF transfers. Only 8 percent of children born to BF mothers had low birthweights (i.e., birthweights below 2.5kg). Given these small differences in unconditional means and the relatively small sample sizes that we are working with, it is not surprising that we find no impact on mean birthweight. However, children whose mothers are *Bolsa Família* recipients in 2009 have a likelihood of being born full term that is 10.7 percentage points higher than children of non-*Bolsa* mothers. When we disaggregate by sex of child, we observe this impact for girls but not boys.

**Table 4: Single difference impact estimates on birthweight and on the proportion of children born full-term among children aged 0-1 in 2009**

| | Birthweight (kg) | | Proportion of children born full-term | |
|---|---|---|---|---|
| | **Comparison 2** | **Comparison 3** | **Comparison 2** | **Comparison 3** |
| | 0.022 | 0.026 | 0.107 | 0.079 |
| | (0.072) | (0.068) | (0.060) * | (0.053) |
| Number of observations | 361 | 561 | 411 | 629 |

Notes: Standard errors in parentheses. *, ** significant at the 10 percent and 5 percent level, respectively. Results are conditional on baseline covariates.

We explored the impact of *Bolsa Família* on breastfeeding. There is no impact on the likelihood of breastfeeding new-born children. This is not surprising, given that nearly all children are breastfed.

**(b) Anthropometry**

We assessed the impact of *Bolsa Família* on the anthropometry of children. We use current international standards (WHO 2006),[5] which express these measurements relative to well–nourished children of the same age and sex. We calculated height-for-age Z-scores (HAZ), weight-for-height Z-scores (WHZ), and Body Mass Index Z-scores (BMIZ) for children found AIBF-1 and in AIBF-2 and computed the prevalences of stunting, underweight, and wasting.

In general, non-recipients in our sample have higher Z-scores than *Bolsa* recipients, and improvements among *Bolsa* recipients are mirrored by improvements among non-recipients. For example, the average HAZ score improves from -0.57 among recipients to -0.34 between the baseline and the 2009 survey, and the stunting prevalence (not shown) improves from 13.5 percent to 9.4 percent. However, among non-recipients the average HAZ score improves even more, by 0.36 standard deviations, and the stunting prevalence also falls, from 11.2 percent to 5.0 percent.

[5] As is common practice, we drop from our analysis any Z-scores that are below -5 or above 5.

Table 5 shows the impact of *Bolsa Família* on three anthropometric outcomes all expressed as Z-scores: height-for-age, weight-for-height, and Body Mass Index. *Bolsa Família* has no impact on height-for-age, stunting, or wasting (not shown). It increases weight-for-height by 0.20 (Comparison 2) and 0.28 (Comparison 3) standard deviations, and body mass by 0.39 standard deviations. The anthropometry of children under the age of 5 in *Bolsa Família* households improved between 2005 and 2009. The change in weights can be attributed to participation in *Bolsa Família*; this is not the case for height.

**Table 5: Single difference impact estimates on HAZ, WHZ, and BMI for age Z-scores, under fives, 2009**

| | HAZ | | WHZ | | BMIZ | |
|---|---|---|---|---|---|---|
| | **Comparison 2** | **Comparison 3** | **Comparison 2** | **Comparison 3** | **Comparison 2** | **Comparison 3** |
| | -0.219 | -0.205 | 0.201 | 0.287* | 0.394** | 0.396** |
| | (0.150) | (0.136) | (0.211) | (0.161) | (0.187) | (0.161) |
| Observations | 1,012 | 1,453 | 974 | 1,403 | 993 | 1,425 |

Notes: Standard errors in parentheses. *, ** significant at the 10 percent and 5 percent level, respectively. Results are conditional on baseline covariates.

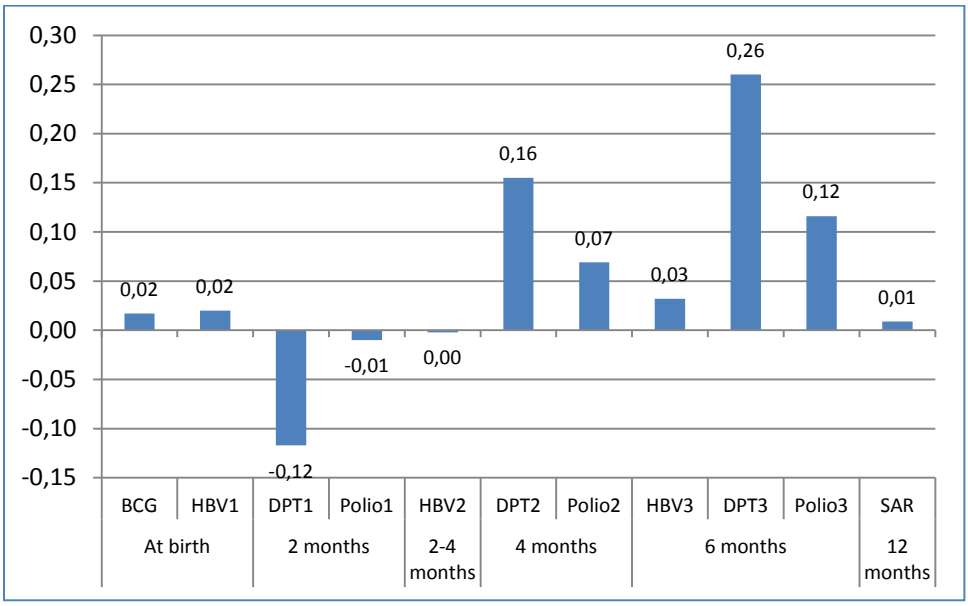
**(c) Vaccinations**

We assess the impact of *Bolsa Família* on the adherence to vaccination schedules in the first year of life because these play the greatest role in protecting the child against contracting communicable diseases. Tuberculosis (BCG) and Hepatitis B (HBV) vaccines should be given at birth. The HBV vaccine requires additional shots when the child is 2-4 months old and again at 6 months. The diphtheria-tetanus-pertussis (DTP) vaccine and polio vaccine (OPV) are both administered in 3 doses at ages 2, 4, and 6 months. The measles vaccine is given at 12 months. In 2005, 91 percent of children in *Bolsa Família* recipient households and 89 percent of children in *Bolsa Família* non-recipient households had vaccination cards. By 2009, this share had increased for both groups, to nearly 98 percent of children, indicating that nearly all children were receiving vaccinations. However, the proportions of children who receive timely vaccinations varies from 50 to 70 percent, depending on the vaccine (see de Brauw et al. 2010, Figure 6.3), suggesting that there is considerable scope for improvement.

Figure 1 shows the impact of *Bolsa Família* on the probability of receiving vaccinations on schedule in 2009 using Comparison 3.[6] Participation in *Bolsa Família* increases timely vaccinations and, generally, these effects are larger for vaccinations that occur later (4 and 6 months) then earlier. Even though we are dealing with relative small sample sizes, we find statistically significant effects, at the 5 percent level on the proportion of children receiving on-time vaccinations for the DTP2, DTP3, and polio3 vaccines.

[6] When we use Comparison 2, we obtain similar impact estimates, but with slightly higher standard errors.

**Figure 1: Single difference impact estimates of *Bolsa Família* on the probability of receiving vaccinations on schedule, 2009**




**(d) Education**

Increasing education attainments is a core objective of *Bolsa Família*. In assessing these, it is helpful to note patterns of enrollment in the AIBF-1 and AIBF-2 surveys. These are shown in Figures 2a and 2b. As shown here, enrollments among children between 6 and 15 are high. So, in addition to looking at all children, we pay particular attention to the impact on older children, those 16 and 17 who are at the highest risk of dropping out.

**Figure 2a: Proportion of children currently attending school in 2005, by age and sex**

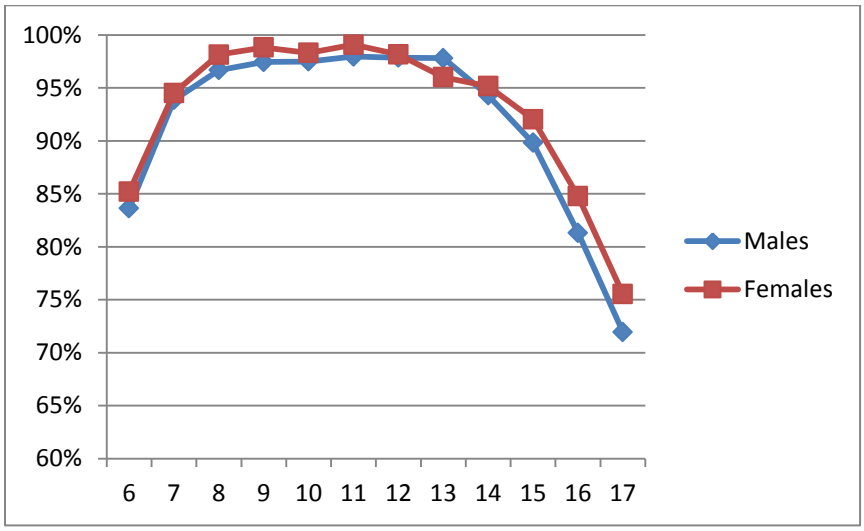


**Figure 2b: Proportion of children currently attending school in 2009, by age and sex**

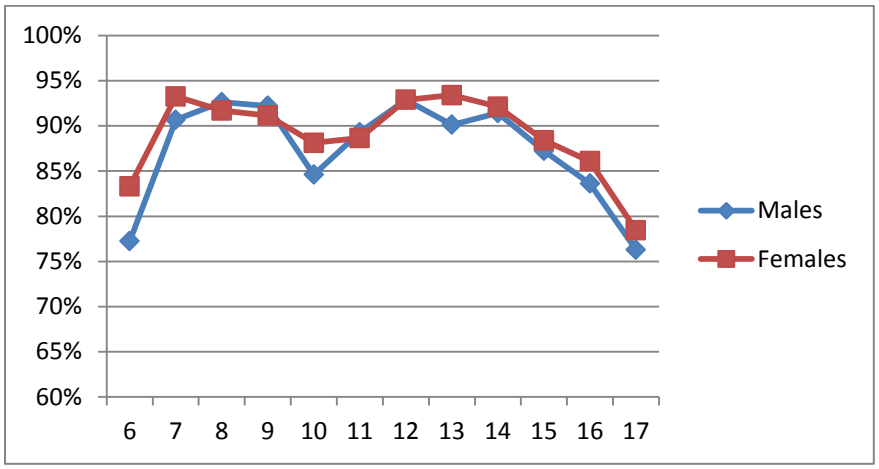



Table 6 shows that *Bolsa Família* increases school attendance by 4.5 (Comparison 2) and 4.1 (Comparison 3) percentage points. The impact is larger for females and somewhat more precisely measured. When we disaggregate by region, we see that these increases are concentrated in the North-East, where enrollments rise by 16.1 (Comparison 2) and 19.9 (Comparison 3) percentage points. As the North-East has historically lagged the rest of Brazil on many social indicators, this suggests that *Bolsa Família* is contributing to the regional reductions in disparities in school attendance.

**Table 6: Single difference impact estimates on the proportion of children aged 6-17 currently attending school, 2009**

| | All children | | Males | | Females | |
|---|---|---|---|---|---|---|
| | **Comparison 2** | **Comparison 3** | **Comparison 2** | **Comparison 3** | **Comparison 2** | **Comparison 3** |
| | 0.045 | 0.041 | 0.012 | 0.032 | 0.082 | 0.038 |
| | (0.026) * | (0.025) | (0.035) | (0.034) | (0.033) ** | (0.034) |
| Observations | 6514 | 10993 | 3374 | 5633 | 3133 | 5349 |

Notes: Standard errors in parentheses. *, ** significant at the 10 percent and 5 percent level, respectively. Results are conditional on baseline covariates.

Rising enrollments could occur because children in school are more likely to progress to the next grade or, rather than dropping out, children are more likely to repeat. Table 7 shows the impact of *Bolsa Família* on grade progression. It shows that children aged 6-17 who reside in households receiving *Bolsa Família* are more likely to progress from one grade to the next. In the Technical Appendix, we disaggregate this result by age and sex. This shows that the impact on grade progression is concentrated among girls aged 15 and 17 and that the effect size is large.

**Table 7: Single difference impact estimates on the proportion of children aged 6-17 in school last year that progressed to next grade level, 2009**

| | All children | | Males | | Females | |
|---|---|---|---|---|---|---|
| | **Comparison 2** | **Comparison 3** | **Comparison 2** | **Comparison 3** | **Comparison 2** | **Comparison 3** |
| | 0.037 | 0.069 | -0.033 | 0.006 | 0.099 | 0.099 |
| | (0.035) | (0.033) ** | (0.036) | (0.039) | (0.048) ** | (0.048) ** |
| Observations | 4539 | 7703 | 2312 | 3911 | 2222 | 3786 |

Notes: Standard errors in parentheses. *, ** significant at the 10 percent and 5 percent level, respectively. Results are conditional on baseline covariates.

Table 8 shows the impact on repetition. There is suggestive evidence (Comparison 3, significant at the 10 percent level) that children, particularly girls, are less likely to repeat a grade.

**Table 8: Single difference impact estimates on proportion of children aged 6-17 in school last year that are repeating grade level, 2009**

| | All children | | Males | | Females | |
|---|---|---|---|---|---|---|
| | **Comparison 2** | **Comparison 3** | **Comparison 2** | **Comparison 3** | **Comparison 2** | **Comparison 3** |
| | -0.008 | -0.050 | 0.057 | -0.009 | -0.057 | -0.084 |
| | (0.032) | (0.030) * | (0.033) * | (0.038) | (0.045) | (0.042) ** |
| Observations | 4539 | 7703 | 2312 | 3911 | 2222 | 3786 |

Notes: Standard errors in parentheses. *, ** significant at the 10 percent and 5 percent level, respectively. Results are conditional on baseline covariates.

### (e) Child labor

Several components of the *Bolsa Família* program may reduce the prevalence of child labor. The most direct effects are likely to come from the transfers that are conditioned on child schooling. In addition, the BVJ transfer to children age 16-17 could reduce the likelihood that children in this age group drop out of school for employment. These transfers may have significant impacts on child labor because this is an age when it is common for children to leave school in order to work.

Levels of child labor vary by age and sex. For children aged 5-10, there is virtually no participation in paid or unpaid work outside the home. Approximately 6 percent of children aged 11-15 work outside the home as do 16.2 and 29.3 percent of females and males aged 16 and 17, respectively. Given these relatively low levels of participation, it is not surprising that *Bolsa Família* has no statistically significant average impact on the proportion of children age 5-17 reporting doing any work in 2009. However, of equal interest is whether *Bolsa Família* affects the age of entry into the labor force for children aged 5-17. Table 9 shows that, on average, *Bolsa Família* delayed labor market entry by 0.8 (Comparison 2) years. The impact is larger for males than for females.

**Table 9: Single difference impact estimates of *Bolsa Família* on age of entry into the labor force by children 5-17, 2009**

| | All children | | Males | | Females | |
|---|---|---|---|---|---|---|
| | **Comparison 2** | **Comparison 3** | **Comparison 2** | **Comparison 3** | **Comparison 2** | **Comparison 3** |
| | 0.823 | 0.390 | 1.090 | 0.841 | 0.278 | -1.062 |
| | (0.454)* | (0.390) | (0.740) | (0.459)* | (0.501) | (0.614)* |
| Observations | 245 | 403 | 156 | 248 | 88 | 154 |

Notes: Standard errors in parentheses. *, ** significant at the 10 percent and 5 percent level, respectively. Results are conditional on baseline covariates.

AIBF-2 also captured information on participation in domestic work (e.g., washing clothes, cleaning, caring for children) and work hours in domestic activities. *Bolsa Família* had no impact on the proportion of children aged 5-17 participating in any domestic work, on average, in 2009. However, conditional on performing any domestic work, we find that *Bolsa Família* reduced the amount of time girls 5-17 spent undertaking domestic work by nearly three hours per week.

## 4. Women's Welfare

### (a) Impact on prenatal care

*Bolsa Família* provides cash transfers to pregnant women to support their health during the pregnancy, conditional on the requirement that they participate in prenatal care visits with a qualified health professional. Information on pregnancies and prenatal care was captured in both rounds of the AIBF survey. In the 2005 survey, the questionnaire captured whether any woman of child-bearing age in the household was pregnant, the month of the pregnancy, and the number of prenatal care visits received. The same information was captured in the 2009 survey.

de Brauw et al. (2010) report that in 2005, *Bolsa Família* recipients averaged 3.5 prenatal care visits; this increased to 4.4 prenatal care visits by 2009. Non-recipients had only 2.9 prenatal care visits, on average, in 2005, but had nearly caught up by 2009, with 4.3 prenatal care visits, on average. The trend of improving utilization of prenatal care services is also clearly demonstrated by the estimates of the proportion of pregnant women reporting receiving no prenatal care. In 2005, 20.9 percent of women had received no prenatal care. Among *Bolsa Família* recipients, this share was somewhat lower, at 17.7 percent, while 22.3 percent of pregnant women in non-recipient households had not received any prenatal care. However, by 2009, the share of women receiving no prenatal care fell sharply to 5.7 percent and was nearly the same for *Bolsa Família* recipients and non-recipients. While these descriptive trends are associations, not causal relations, they suggest that it may be difficult to find evidence of impact.

Table 10 shows that *Bolsa Família* increased use of prenatal care. *Bolsa* recipients who were pregnant at the time of the 2009 survey had 1.6 more prenatal care visits than pregnant women who were non-recipients. We caution, however, that this result is based on relatively small samples of women who were pregnant at the time of the interview in 2009.[7]

**Table 10: Single difference impact estimates of *Bolsa Família* on the number of prenatal care visits, for women pregnant during the AIBF-2 survey**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| | 1.701 | 1.602 |
| | (0.913)* | (0.800)** |
| Number of observations | 75 | 121 |

Notes: Standard errors in parentheses. *, ** significant at the 10 percent and 5 percent level, respectively. Results are conditional on baseline covariates.

In light of the descriptive statistics reported above, it is not surprising that we find no evidence that *Bolsa Família* reduced the proportion of pregnant women in the 2009 survey who had no prenatal care visits. Nor do we find evidence that participation in *Bolsa Família* decreased the probability that a woman's prenatal care visits were in consultation with a doctor, rather than a nurse or informal care provider.

**(b) Decisionmaking within the household**

Increasing women's decisionmaking power has both intrinsic and instrumental value: intrinsic in that greater equity in decisionmaking is desirable in its own right; instrumental in that increasing women's decision-making power is seen to be associated with a series of desirable outcomes, particularly as they relate to child welfare. Chapter 11 of de Brauw et al. (2010) describes how household decisionmaking has evolved over time in the AIBF surveys.

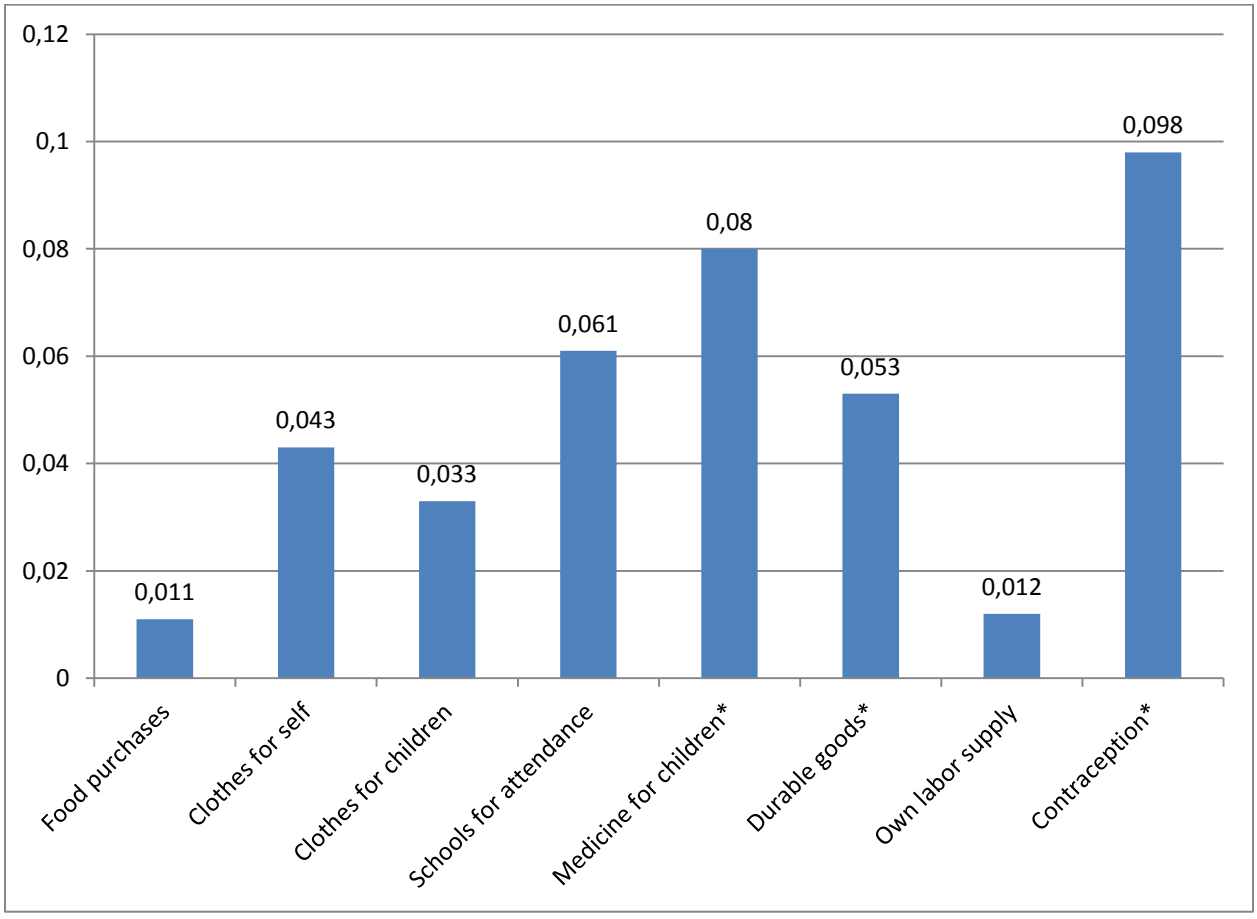
In AIBF-1 and AIBF-2, respondents were asked, "In your household, generally, who makes decisions about": purchases of food; clothing for yourself; clothing for your spouse or partner; clothing for children; when your child must stop attending school; health-related expenditures for children; the purchase of consumer durables for the home; if you work or not; if your spouse works; and your decision to use contraception. de Brauw at al. (2010) note that in most cases, the modal form of decisionmaking is joint; joint decisionmaking is reported in 40-65 percent of the domains described

[7] This small sample also means that estimates of impacts at the regional level are not very precise. However, the relative magnitude of the estimated regional effects is still informative. Results show that the impact of *Bolsa Família* on the number of prenatal care visits was largest in the North and was also quite large in the North-East and South-East.

here. Second, where changes had occurred, they had been in the direction of increased decisionmaking voice by women.

Figure 3 highlights key results based on the Comparison 3 sample. Where these effects are statistically significant at the 10 percent level (denoted by an asterisk), the percentage changes range from 5.3 percentage points (durable goods) to 9.8 percentage points (contraception). In all items where children benefit from expenditures, the provision of *Bolsa Família* increases the likelihood that woman can make decisions about these. The results regarding contraception are notable. It is sometimes claimed that families will have more children if they think they can obtain greater program benefits. The contraception results suggest a different dynamic; namely that the receipt of these transfers gives women more autonomy in decisionmaking regarding their own fertility. As such, they are not consistent with the claims that pregnancies are induced by poor to benefit from CCT programs.

**Figure 3: Single difference impact estimates of *Bolsa Família* on women's decisionmaking, 2009**



The magnitude of these changes is large given that there are instances where the husband was present when these answers were given. In the AIBF-1 survey, in most domains about one-sixth to one-third of women reported making these decisions. So as a percentage change, *Bolsa Família* raises women's decisionmaking power by 29.7 and 33 percent, depending on the outcome (see Table 11 below). Regional disaggregations (found in the Technical Appendix) show that these effects tend to be largest in the North-East and South regions.

**Table 11: Magnitudes of the impact of *Bolsa Família* on women's decisionmaking power**

| Domain | Impact in percentage points (from Comparison 2) | Percentage value at baseline | Percentage change relative to baseline |
|---|---|---|---|
| | | (percent) | (percent) |
| Medicine for children | 8.0 | 24 | 33.0 |
| Durable goods | 5.3 | 17 | 31.2 |
| Contraception | 9.8 | 33 | 29.7 |

## 5. Household Behavior and Welfare

### (a) Labor supply

A concern with any cash transfer program is that individuals in households that receive money will reduce the number of hours they work. AIBF-1 and AIBF-2 were specifically designed to ensure that this important issue could be addressed. Specifically, we recorded current labor force status (in the labor force or not; in the labor force, not working but searching for work; in the labor force and working) and the number of hours worked in a typical week for all adults aged 18-69. When we consider labor supply in terms of hours, we do so at the household level. Specifically, we sum this across all adults and divide by the number of individuals aged 18-69 to give a measure of household labor supply.

There is no impact of *Bolsa Família* on whether an individual aged 18-55 is in the labor force. This is the case when we estimate impacts on men and women separately and when we pool the sample. Conditional on being in the labor force, there is no statistically significant effect on the likelihood that men work or look for work. For women, conditional on being in the labor force, *Bolsa Família* weakly increases the proportion of females that have sought work among those not currently working, by about 0.05 to 0.07, which appears driven by the North-East, where the increase is significant and roughly 0.09 to 0.11 (depending on the use of Comparison 2 or 3). One interpretation is that receipt of *Bolsa Família* makes it possible for women to search for better jobs than would be the case if they did not receive these transfers.

Table 12 shows that there is a statistically insignificant impact on average total household weekly work hours among individuals aged 18-69 per individual aged 18-69. Disaggregated by region,

there is an insignificant impact in all regions.

Table 12: Impact on average household weekly work hours

| | Among all members aged 18-69, per member aged 18-69 | | Among males aged 18-69, per male aged 18-69 | | Among females aged 18-69, per female aged 18-69 | |
|---|---|---|---|---|---|---|
| | **Comparison 2** | **Comparison 3** | **Comparison 2** | **Comparison 3** | **Comparison 2** | **Comparison 3** |
| | 0.166 | -0.110 | -0.785 | -0.384 | 0.304 | 0.244 |
| | (1.146) | (0.949) | (1.599) | (1.336) | (1.495) | (1.344) |
| Observations | 3661 | 5391 | 3432 | 5078 | 3410 | 5066 |

Notes: Standard errors in parentheses. *, ** significant at the 10 percent and 5 percent level, respectively. Results are conditional on baseline covariates.

We can also see whether the type of work changed. AIBF-1 and AIBF-2 contain information that allow us to characterize each job worked by a household member as being in the formal sector or informal sector. We define a job as being in the formal sector if the household member either has a "card" for that job or contributes to social security through that job. [8] Overall, conditional on baseline covariates, there is a significant decrease in average formal household weekly work hours among individuals aged 18-69 per individual aged 18-69, by roughly eight hours. By contrast, conditional on baseline covariates, there is a significant increase in average informal household weekly work hours among individuals aged 18-69 per individual aged 18-69, by roughly eight hours, suggesting that there is indeed a shift across sectors.[9]

When we disaggregate by sex, we find a significant decrease in average formal household weekly work hours among males aged 18-69 per male aged 18-69, by roughly 4.6 hours (Comparison 2 and 3, statistically significant at the 5 percent level). This is driven by impacts observed in the North and North-East. By contrast, conditional on baseline covariates, there is a significant increase in average informal household weekly work hours among males aged 18-69 per male aged 18-69, by roughly 5.3 hours (Comparisons 2 and 3, statistically significant at the 5 percent level). Disaggregated by region, there is an increase in average informal household weekly work hours among males in the North-East by 3.4 – 4.2 hours (Comparisons 2 and 3 respectively, statistically significant at the 5 percent level). Among women, conditional on baseline covariates, there is a decrease in average formal household weekly work hours of 4.1 hours among females aged 18-69 per female aged 18-69 based on Comparison 2. This appears to be driven by an increase in informal sector work by women in the North-East.

[8] These results are based in Comparison 2. Results using Comparison 3 are very similar.

[9] A caveat to this finding is that some differences appear to exist in formal-sector work and informal-sector work between our Comparison 2 treatment and control groups even at baseline.

Given these results, we ask to what extent the proportion of work shifted between the formal and informal sector and to what extent has it shifted within the household. Conditional on baseline covariates, we find a statistically significant decrease in the proportion of total household weekly work hours devoted to the formal sector by roughly 20 percent when using either Comparison 2 or Comparison 3. This impact is statistically significant at the 5 percent level. This switch from the formal to informal sector is most marked in the North-East, where the proportion of total household weekly work hours devoted to the formal sector falls by roughly 22 percent based on Comparison 2. By contrast, there is an insignificant impact in the proportion of total household weekly work hours worked by females.

There are several possible explanations for the differences we observe in formal- and informal-sector work. *Bolsa Família* Program has adopted administrative procedures to cross-check households' self-reported incomes; however, these procedures are only possible when at least one member of the household is working in the formal sector. One explanation for our findings is that this procedure created an incentive to hide income through informal work, and some *Bolsa Família* beneficiaries were induced to switch from the formal sector to the informal sector. A second possibility is that, due to the administrative cross-checks, a disproportionate share of households already working in the formal sector were excluded from the program between our baseline and follow-up surveys, leading formal-sector workers to be underrepresented among beneficiaries at follow-up. A third explanation is that, among potential beneficiaries, workers with a more unstable trajectory in the labor market tended to prefer work in the informal sector with access to steady benefits, while workers with a more stable trajectory preferred to work in the typically-higher-paying formal sector even with the risk of losing the benefits. While our data do not allow us to readily distinguish between these explanations, all three may play some role.

**(b) Social capital**

In de Brauw et al. (2010), we noted that the level of participation in groups and networks was relatively low. Mindful of this, we estimated the effect of *Bolsa Família* on group participation. There is weak evidence that *Bolsa* increases group membership, but this is dependent on how membership is defined, the specific method of estimating impact used and the location of the recipient.

## 6. Summary

Using propensity score weighting, we have examined the impact of *Bolsa Família* on the welfare of children, mothers, and households. *Bolsa Família* improves welfare in the following ways:

- It ***increases*** the likelihood that children are born full-term, although this effect is imprecisely measured;
- It ***improves*** certain dimensions of children's anthropometry: their weight-for-height and body mass;
- There are statistically significant effects on the proportion of children receiving on-time DTP2, DTP3 and polio3 vaccines. These effects are large in magnitude;
- *Bolsa Família* ***increases*** school attendance by 4.5 (Comparison 2) and 4.1 (Comparison 3) percentage points. The impact is larger for females. These increases are concentrated in the North-East;
- Children in households receiving *Bolsa Família* are ***more*** likely to progress from one grade to the next. This impact is largest among girls aged 15 and 17 and that the effect size is large;
- There is some evidence that *Bolsa Família* children, particularly girls, are less likely to repeat a grade. However, these effects are imprecisely measured.
- Complementary to the schooling results, *Bolsa Família* ***delays*** children's labor market entry by about one year although this is imprecisely measured;
- *Bolsa Família* had no impact on the proportion of children aged 5-17 participating in any domestic work, on average, in 2009. However, conditional on performing any domestic work, we find that *Bolsa Família* ***reduced*** the amount of time girls 5-17 spent undertaking domestic work by nearly three hours per week;
- Pregnant women in households receiving *Bolsa Família* transfers receive, have 1.6 ***more*** prenatal visits with a health care professional; and
- In all items where children benefit from expenditures, the provision of *Bolsa Família* increases the likelihood that woman can make decisions about these with the largest impact found on contraceptive choice. It is sometimes claimed that families will have more children if they think they can obtain greater program benefits. The contraception results suggest a different dynamic; namely that the receipt of these transfers gives women more autonomy in decisionmaking regarding their own fertility. As such, they are not consistent with the claims that pregnancies are induced by poor to benefit from CCT programs.

- There is no meaningful evidence that *Bolsa Família* reduces labor supply. There is some evidence that in participant households, men have been working fewer hours per week in the formal sector and more hours in the informal sector.

## References

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# The Impact of *Bolsa Família* on Child, Maternal, and Household Welfare

## Technical Appendix

**Contents**

## Section 1. Estimation Methodology

### 1.1 Overview of propensity score weighting

In this appendix, we describe in more detail the methodology we use to estimate the impacts of *Bolsa Família*. We wish to estimate average treatment impacts on the treated (ATT): that is, the impact that *Bolsa Família* had on a range of outcomes for recipients, using non-recipients as a proxy for what their outcomes would have counterfactually been in the absence of *Bolsa Família*. The key challenge in evaluating these impacts, for a nonrandomly assigned program such as *Bolsa Família*, is accounting for characteristics that may be correlated both with receipt of the program and with outcomes of interest conditional on program receipt. If program recipients differ systematically from non-recipients, even preprogram, in ways that may also affect our outcomes of interest, we must take these differences into account in order to avoid biased impact estimates.

The *Bolsa Família* Program is targeted at poor households. Consequently, program recipients tend to look quite different from non-recipients, even preprogram. In evaluating *Bolsa Família*, we therefore turn to impact estimation methodologies designed for nonrandom program assignment. Our preferred methodology for this evaluation is propensity score weighting (Hirano, Imbens, and Ridder 2003), an approach that entails estimating and applying weights to statistically balance preprogram characteristics between *Bolsa Família* recipients and the specific selection of non-recipients we use for comparison.

As discussed in the main report, the basic intuition behind the propensity score weighting estimator is as follows. We first estimate a propensity score for each household, which indicates the predicted probability that the household is a *Bolsa Família* recipient rather than in a comparison group of non-recipients, based on a range of observable preprogram characteristics. We then use the propensity scores to place weights on the comparison observations. These weights adjust for the fact that some households in the comparison group do not have high predicted probability of being *Bolsa Família* recipients based on their observable characteristics; these households receive low weights in the estimation of ATT. Meanwhile, other households in the control group have observable characteristics very similar to households receiving *Bolsa Família* payments; these households are assigned higher weights. Intuitively, by placing higher weights on non-recipient households that have characteristics more like recipients and lower weights on non-recipient households that have characteristics less like recipients, we balance observable characteristics between recipients and non-recipients, even if they were unbalanced before weighting. Hirano, Imbens, and Ridder (2003) show that, under assumptions described below, applying the propensity score weights leads to unbiased impact estimates of ATT.

There are two key criteria that lead us to choose propensity score weighting as our preferred methodology for estimating the impacts of *Bolsa Família*. First, unlike other standard methodologies for impact estimation in nonrandomized settings, propensity score weighting allows us to take into account the sampling weights and attrition weights in our data. Incorporating these weights allows us to interpret our estimates of ATT as representative of the treated population, adjusting for oversampling of certain types of households in the baseline and selective attrition of certain types of households in the follow-up. Second, the methodology imposes a relatively smaller computational burden than alternative estimators for nonrandomized settings. For a dataset with such large sample size, use of more time-consuming procedures (such as covariate matching) would limit the feasibility of estimating impacts on a rich set of outcomes. The main disadvantage of using propensity score weighting as opposed to matching methods is the higher variance of the estimator (Freedman and Berk 2008). We describe below the measures we take to, first, reduce variance to the extent possible, and second, use alternative methods as robustness checks when impacts using propensity score weighting are borderline-significant.

### 1.2 Theoretical basis for propensity score weighting

We present here a brief overview of the theoretical basis for propensity score weighting, based on Hirano, Imbens, and Ridder (2003).

The aim of our evaluation is to construct, for a range of outcomes, an estimate of the average impact of *Bolsa Família* on those that receive it—referred to as the average impact of the treatment on the treated (*ATT*). The formalization of this concept is as follows.

Let $Y_t^1$ be a household's outcome in time period *t* if it is a recipient of *Bolsa Família,* let $Y_t^0$ be that household's outcome in time period *t* if it does not receive any program benefits, and let *D* be an indicator variable equal to 1 if the household receives program benefits and 0 if not (i.e., an indicator of "treatment"). The impact of the program is just the change in the outcome caused by receiving benefits: $\Delta = Y_t^1 - Y_t^0$. For each household, either only $Y_t^1$ or only $Y_t^0$ is observed in any period *t*.

We wish to estimate the difference between the outcome that treated households would realize if they receive the program and the outcome that treated households would realize if they do not receive the program in period *t*, given a vector *X* of observable characteristics of the households:

$$ATT = E(\Delta \mid X,D = 1) = E(Y_t^1 - Y_t^0 \mid X,D = 1) = E(Y_t^1 \mid X,D = 1) - E(Y_t^0 \mid X,D = 1).$$

However, only $Y_t^1$ and not $Y_t^0$ is observed for households treated in period *t*, i.e., those with $D = 1$. Because $E(Y_t^0 \mid X,D = 1)$ is not observed, we must construct a statistical comparison group for recipients out of our observations on non-recipients, i.e., households with $D = 0$. In particular, we must

construct a group of non-recipients and then adjust it in such a way that balances any observable characteristics *X* potentially correlated both with treatment status and the outcome conditional on treatment status.

One way of doing so involves estimating a "propensity score," $P(X) = \Pr(D = 1 \mid X)$. This propensity score is the predicted probability that any household is a program recipient based only on its observable characteristics *X*. The approach of propensity weighted regression entails the researcher selecting a set of non-recipients to use as a comparison group, then using estimated propensity scores for program receipt to more heavily weight the comparison observations with higher propensity scores.[10] The validity of this approach rests in part on two assumptions:

$$E(Y_t^0 \mid X, D = 1) = E(Y_t^0 \mid X, D = 0), \tag{A1}$$

and

$$0 < P(X) < 1. \tag{A2}$$

Expression (A1) assumes "conditional mean independence", i.e., that conditional on *X,* nonparticipants have the same mean outcomes as participants would have if they did not receive the program. Expression (A2) assumes that, based only on the set of observables *X,* all observations in the comparison group have positive predicted probability of being treated.

We first consider the case without sampling or attrition weights.

Under (A1), (A2), and several other technical assumptions, Hirano, Imbens, and Ridder show that we obtain an unbiased estimate of *ATT* through a weighted regression framework, if the ratio of assigned weights is $\frac{P(X)}{1-P(X)}:1$ for comparison : treatment observations .[11]

[10] We describe below in Section 1.3 how, in practice, we define possible comparison groups and how we estimate propensity scores.

[11] Note that this approach differs from matching methods, in that for matching, only certain observations out of the eligible comparison group are used—based on some metric of similarity to treated observations, depending on the particular method—but that typically each of those observations is then assigned a weight of 1. In propensity score weighting, all observations in the comparison group selected by the researcher are used, but each is assigned a weight based on its propensity score. (This approach is preferable for our context, since incorporating sample weights and attrition weights is then relatively straightforward.) In this respect, the researcher's selection of the comparison group is quite important for propensity score weighting, since all observations are used with nonzero weight. We discuss our selection of possible comparison groups for this evaluation in the main report and demonstrate their comparability in Section 1.3.3 of this appendix.

Hirano, Imbens, and Ridder also show that the observables *X* used to construct the propensity score can be directly included in this weighted regression to account for additional variation and thereby improve precision. [12]

It is straightforward to extend this methodology to the case where, as in this evaluation, there are also sampling weights and attrition weights. These weights can simply be multiplied to the propensity-score weights to derive an "effective weight" to be used in the weighted regression.

### 1.3 Implementation of propensity score weighting

As described above, there are two ways by which we adjust for differences in observable characteristics between the *Bolsa Família* recipients and non-recipients that we compare: (1) select a comparison group of non-recipients that, in the first place, is likely to be fairly similar to the treated group of recipients in terms of observable characteristics, and (2) use estimated propensity scores to weight each observation in the comparison group according to its similarity to treated observations. We assess the first by looking at overlap in estimated propensity scores between each treatment and comparison group. We assess the second by looking at the extent to which a set of observable characteristics is balanced between each treatment and comparison group once the propensity score weights are taken into account.

#### 1.3.1 Selection of potential comparison groups

The main report describes our logic in selecting three potential sets of treatment and comparison groups, defined as follows:

**Treatment status definition 1:**

- Treatment 1: Registered in the *Cadastro Único* and not receiving *Bolsa Família* in 2005; Receiving *Bolsa Família* in 2009.
- Comparison 1: Registered in the *Cadastro Único* and not receiving *Bolsa Família* in 2005; Not receiving *Bolsa Família* in 2009.

**Treatment status definition 2:**

- Treatment 2: Not receiving *Bolsa Família* in 2005; Receiving *Bolsa Família* in 2009.
- Comparison 2: Not receiving *Bolsa Família* in 2005; *Not receiving Bolsa Família* in 2009; Registered in *Cadastro Único* in either 2005 or 2009.

**Treatment status definition 3:**

- Treatment 3: *Receiving Bolsa Família* in 2009.
- Comparison 3: Not receiving *Bolsa Família* in 2009; Registered in *Cadastro Único* in either 2005 or 2009.

[12] We include these observables as covariates in all of our estimates.

Based on sample size considerations, particularly for disaggregations by age, sex, and region, we focus on presenting results only for Treatment status definition 2 (denoted as simply "Comparison 2") and Treatment status definition 3 (denoted as simply "Comparison 3").

### 1.3.2 Estimating propensity scores

Using propensity score weighting requires choosing a method for estimating propensity scores. Ideally, we wish to include all observable characteristics in the propensity score that are correlated both with the probability of receiving *Bolsa Família* and with outcomes related to *Bolsa Família* conditional on receipt status. We also would like to let the data tell us the relationship between the probability of treatment and these observable characteristics. In other words, we prefer to allow for as flexible a relationship as possible, rather than imposing a particular functional form. These considerations are taken into account in the approach described here.

We start by selecting a large set of observable preprogram characteristics that we perceive as having potential to be correlated with both program receipt and our outcomes of interest conditional on program receipt status.[13] This set of observables includes characteristics both at the household level and at the municipality level.

To then estimate propensity scores, we follow the following stepwise algorithm, which in essence follows Hirano, Imbens, and Ridder (2003). We start by estimating a logit model including only region dummy variables interacted with rural-urban dummies, to ensure that we account for broad differences in market conditions. We weight the regression by sampling weights from the AIBF-1 data multiplied by attrition weights. Next, we start to consider the set of *N* variables at the household and municipality level as possible covariates for inclusion in the logit model. We estimate *N* regressions, each sequentially and separately including one variable to the basic logit model. We keep the variable that reduces the log pseudo-likelihood the most. We then take the remaining list of *N*-1 variables, and sequentially add the remaining *N*-1 variables to the logit model, again keeping the one that maximizes the reduction of the log pseudo-likelihood. We follow this procedure until the reduction hits a threshold that roughly corresponds to adding a variable to the logit model that has a t-ratio of 1, indicating that the remaining variables in the list have little predictive power.[14]

[13] In the case of Comparison 3, since some in the treatment group are already receiving *Bolsa Familia* at baseline, we allow inclusion of only characteristics that are unlikely to be affected by already receiving treatment.

[14] We use the list of predictive explanatory variables for any regressions using covariate matching, when we run robustness checks. Since covariate matching does not parameterize the relationship between explanatory variables and treatment status, we do not need to include any second order terms.

We next take the *K* covariates that are chosen in the first step, and in the second step we square all of them and interact them with one another, creating an additional $K(K + 1)/2$ variables. We then add these second order terms to the model sequentially until no term exceeds a threshold that loosely indicates significant predictive power (e.g., a t-ratio that corresponds to a p-value of 0.1). We repeat this procedure for each of the three comparisons, therefore letting the data tell us the relationship between *Bolsa Família* eligibility and potential explanatory variables.

Table A1 shows the full set of covariates we allow to enter our constructed propensity scores. The table also indicates which covariates enter estimated propensity scores for each comparison group, following the algorithm described above.

**Table A1: Household- and municipality-level characteristics included as possible covariates for propensity score estimates**

| | | | Covariate in Comparison | | |
|---|---|---|---|---|---|
| **Level** | **Variable** | **Year** | **1[a]** | **2[a]** | **3[a]** |
| Household (from AIBF) | Number of children aged 0-15 at baseline | 2005 | X | X | X |
| Household (from AIBF) | Household size | 2005 | X | X | |
| Household (from AIBF) | Number of rooms in house (truncated at 10) | 2005 | | | X |
| Household (from AIBF) | Number of bedrooms in house | 2005 | | | |
| Household (from AIBF) | Number of bathrooms in house | 2005 | | | |
| Household (from AIBF) | Housing quality index, from 0-11 | 2005 | X | X | X |
| Household (from AIBF) | Whether household owns its house | 2005 | | | |
| Household (from AIBF) | Log of per-capita monthly expenditure (food + nonfood) | 2005 | X | X | [b] |
| Household (from AIBF) | Whether head is illiterate | 2005 | | X | |
| Household (from AIBF) | Head's years of education | 2005 | | | X |
| Household (from AIBF) | Head's sex | 2005 | | | |
| Municipality | Average family size | 2000 | X | X | |
| Municipality | Child dependency ratio | 2000 | X | | X |
| Municipality | Incidence of poverty | 2003 | | | X |
| Municipality | Incidence of extreme poverty | 2003 | X | | |
| Municipality | Percent of population working without card | 2000 | | X | X |
| Municipality | Percent of population working in agricultural sector | 2000 | | X | X |
| Municipality | Black population, as percentage of total population | 2000 | X | | |
| Municipality | "Pardo" population, as percentage of total population | 2000 | | | X |
| Municipality | Indigenous population, as percentage of total population | 2000 | | | |
| Municipality | Percent of households with "adequate housing" | 2000 | | | |
| Municipality | Percent of households with access to piped water | 2000 | | X | X |
| Municipality | Percent of households with access to solid waste collection | 2000 | | X | X |
| Municipality | Percent of households with access to general sewage network | 2000 | | | |
| Municipality | Percent of households with access to septic tanks | 2000 | X | | |
| Municipality | Percent of households with access to electricity | 2003 | | | |
| Municipality | Households with landline phones (per 1,000) | 2000 | | | |
| Municipality | Households with cell phones (per 1,000) | 2000 | | X | |
| Municipality | Average years of education | 2000 | | | |
| Municipality | School attendance rate: 7-14 y.o. | 2000 | X | X | X |
| Municipality | Illiteracy rate: 7 to 14 y.o. | 2003 | | | |
| Municipality | Number of public schools per capita | 2003 | | X | X |
| Municipality | Average number of students per class in elementary school | 2003 | X | | |
| Municipality | Number of clinics ("postos medicos") per thousand inhabitants | 2002 | x | | |

[a] All comparisons include as covariates the interactions of region and rural/urban dummies.
[b] Excluded from consideration.

### 1.3.3 Assessing similarity of each treatment and comparison group, per estimated propensity scores

Based on the algorithm above, we estimate propensity scores over the set of listed covariates for each of the three comparisons defined. Below we graph kernel densities to compare the distribution of estimated propensity scores among recipients vs. the distribution of estimated propensity scores among non-recipients, for each of the three comparison definitions (Figures A1-A3). If the two distributions did

not largely overlap one another, we would be concerned that the treatment and comparison groups we had defined were not comparable along observable characteristics. However, in each case we find very good overlap, suggesting that we can have confidence that the estimated propensity scores will help correct imbalances between the two groups.

**Figure A.1 Overlap in propensity scores for comparison between *Bolsa Família* recipients and non-recipients, using Comparison Definition 1**

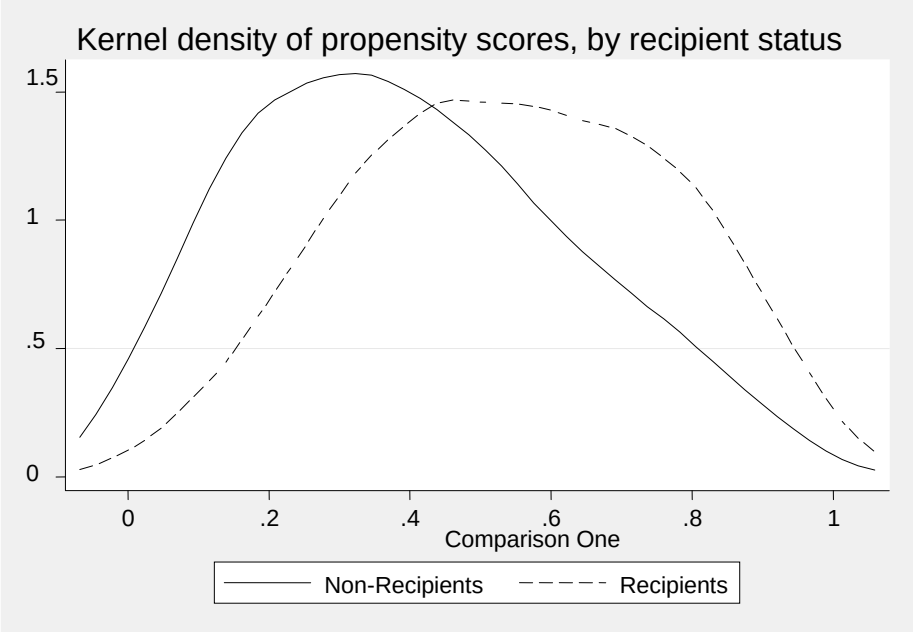



**Figure A2: Overlap in propensity scores for comparison between *Bolsa Família* recipients and non-recipients, using Comparison Definition 2**

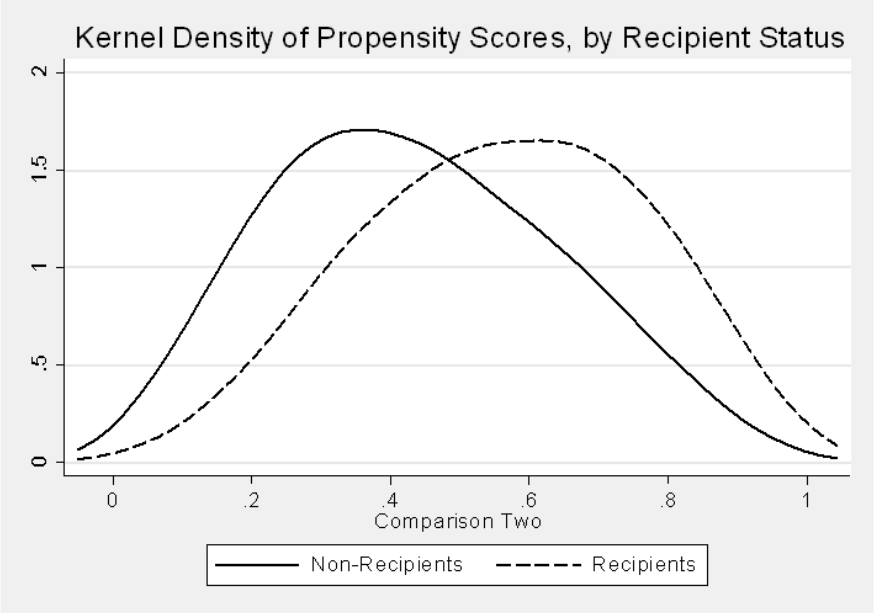
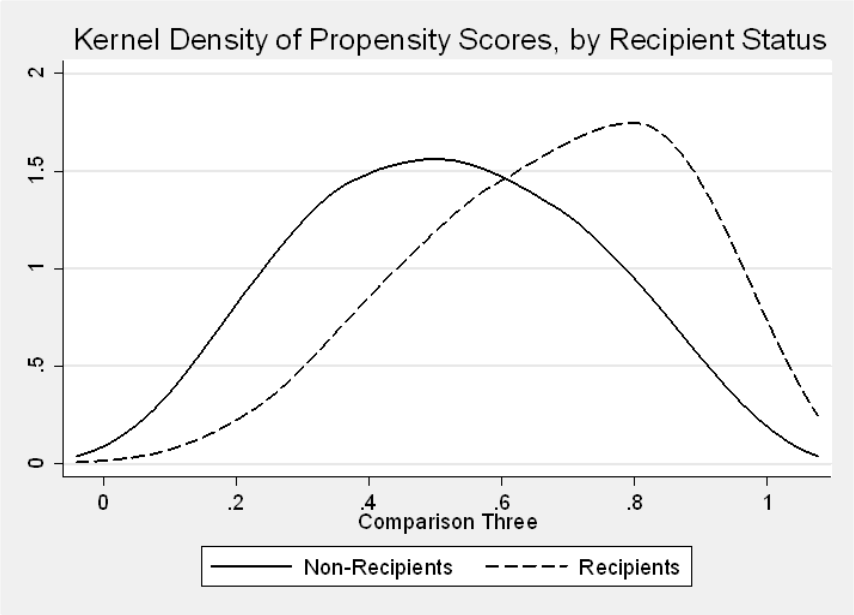


**Figure A3: Overlap in propensity scores for comparison between *Bolsa Família* recipients and non-recipients, using Comparison Definition 3**




### 1.3.4 Assessing balancing of observables using propensity score weights

To assess how well weights based on these estimated propensity scores balance observable characteristics between each treatment and comparison group, we test for average differences in those characteristics between the groups after applying the propensity score weights to the comparison group.

Tables A2 and A3 show the unweighted means and the weighted means for the treatment and comparison groups in Comparison 2 and Comparison 3 (as mentioned above, we do not present estimates from Comparison 1 due to small sample sizes). Before we use the propensity weights, we find statistically significant differences for many of the characteristics. After the propensity weights are applied, in both comparisons the average differences are no longer statistically significant. Therefore we can comfortably state that the propensity scores appear to account for significant differences between the groups of recipients and non-recipients for both comparisons.

**Table A2: Comparison 2—Balancing of covariates, unweighted and weighted**

| Variable | Included in construction of p-score | Unweighted means | | Weighted means | | p-value on difference in weighted means |
|---|---|---|---|---|---|---|
| | | Control | Treatment | Control | Treatment | |
| HH: Number of children age 0-15 at baseline | X | 1.42 | 2.10 | 1.72 | 1.68 | 0.71 |
| HH: Household size | X | 4.20 | 4.77 | 4.03 | 4.01 | 0.89 |
| HH: Number of rooms in house | | 5.18 | 4.88 | 5.06 | 5.07 | 0.96 |
| HH: Number of bedrooms in house | | 0.27 | 0.34 | 0.25 | 0.22 | 0.56 |
| HH: Number of bathrooms in house | | 0.89 | 0.77 | 0.83 | 0.86 | 0.54 |
| HH: Housing quality index, from 0-11 | X | 8.25 | 7.57 | 7.59 | 7.83 | 0.22 |
| HH: Whether household owns its house | | 0.67 | 0.67 | 0.68 | 0.63 | 0.26 |
| HH: Log of per-capita monthly expenditures | X | 5.20 | 4.96 | 5.01 | 5.10 | 0.18 |
| HH: Whether head is illiterate | X | 0.22 | 0.25 | 0.24 | 0.24 | 0.99 |
| HH: Head's years of education | | 3.49 | 3.52 | 3.51 | 3.57 | 0.82 |
| HH: Head's sex | | 0.36 | 0.36 | 0.37 | 0.38 | 0.79 |
| Muni: Average family size | X | 3.60 | 3.69 | 3.58 | 3.57 | 0.80 |
| Muni: Child Dependency Ratio | | 49.49 | 52.34 | 49.87 | 49.55 | 0.60 |
| Muni: Incidence of poverty | | 39.29 | 45.74 | 43.68 | 42.98 | 0.66 |
| Muni: Incidence of extreme poverty | | 0.13 | 0.16 | 0.14 | 0.14 | 0.51 |
| Muni: Percent without card | X | 39.64 | 43.54 | 42.99 | 43.43 | 0.78 |
| Muni: Percent in agricultural sector | X | 23.69 | 27.63 | 28.23 | 28.64 | 0.80 |
| Muni: Percent black | | 6.51 | 6.69 | 7.59 | 7.15 | 0.29 |
| Muni: Percent "pardo" | | 46.95 | 51.94 | 48.42 | 46.67 | 0.40 |
| Muni: Percent indigenous | | 0.38 | 0.42 | 0.43 | 0.49 | 0.26 |
| Muni: Percent with "adequate housing" | | 34.98 | 28.67 | 33.41 | 34.03 | 0.71 |
| Muni: Percent with piped water | X | 69.70 | 64.79 | 68.09 | 68.56 | 0.71 |
| Muni: Percent with solid waste collection | X | 71.24 | 64.64 | 67.17 | 67.31 | 0.94 |
| Muni: Percent with general sewage network | | 38.56 | 30.15 | 37.43 | 38.74 | 0.53 |
| Muni: Percent with septic tanks | | 14.28 | 14.71 | 12.79 | 12.09 | 0.56 |
| Muni: Percent with electricity | | 91.09 | 88.57 | 90.06 | 91.13 | 0.16 |
| Muni: Landline phones (per 1,000) | | 246.47 | 220.58 | 240.90 | 244.13 | 0.62 |
| Muni: Cell phones (per 1,000) | X | 146.18 | 131.30 | 140.03 | 143.73 | 0.52 |
| Muni: Average years of education | | 5.57 | 5.24 | 5.15 | 5.17 | 0.82 |
| Muni: School attendance rate: 7-14 y.o. | X | 94.51 | 93.77 | 93.40 | 93.57 | 0.70 |
| Muni: Illiteracy rate: 7 to 14 y.o. | | 12.77 | 15.64 | 14.22 | 13.55 | 0.38 |
| Muni: Number of public schools per capita | X | 0.001 | 0.001 | 0.001 | 0.001 | 0.94 |
| Muni: Number of students per elementary class | | 28.22 | 28.00 | 27.59 | 27.50 | 0.68 |
| Muni: Clinics (per 1,000) | | 2.04 | 1.80 | 1.66 | 1.63 | 0.81 |

**Table A3: Comparison 3—Balancing of covariates, unweighted and weighted**

| Variable | Included in construction of p-score | Unweighted means | | Weighted means | | p-value on difference in weighted means |
|---|---|---|---|---|---|---|
| | | Control | Treatment | Control | Treatment | |
| HH: Number of children age 0-15 at baseline | X | 1.42 | 2.23 | 2.00 | 1.87 | 0.20 |
| HH: Household size | | 4.20 | 4.84 | 4.38 | 4.22 | 0.23 |
| HH: Number of rooms in house | X | 5.18 | 4.84 | 4.86 | 5.02 | 0.24 |
| HH: Number of bedrooms in house | | 0.27 | 0.35 | 0.28 | 0.23 | 0.21 |
| HH: Number of bathrooms in house | | 0.89 | 0.75 | 0.76 | 0.81 | 0.28 |
| HH: Housing quality index, from 0-11 | X | 8.25 | 7.51 | 7.44 | 7.59 | 0.33 |
| HH: Whether household owns its house | | 0.67 | 0.66 | 0.66 | 0.64 | 0.57 |
| HH: Log of per-capita monthly expenditures | Excluded | 5.20 | 4.93 | 5.04 | 5.02 | 0.66 |
| HH: Whether head is illiterate | | 0.22 | 0.26 | 0.24 | 0.26 | 0.67 |
| HH: Head's years of education | X | 3.49 | 3.57 | 3.47 | 3.58 | 0.58 |
| HH: Head's sex | | 0.36 | 0.36 | 0.39 | 0.37 | 0.63 |
| Muni: Average family size | | 3.60 | 3.69 | 3.61 | 3.60 | 0.78 |
| Muni: Child Dependency Ratio | X | 49.49 | 52.46 | 50.54 | 50.45 | 0.88 |
| Muni: Incidence of poverty | X | 39.29 | 45.79 | 45.63 | 44.52 | 0.49 |
| Muni: Incidence of extreme poverty | | 0.13 | 0.16 | 0.14 | 0.14 | 0.81 |
| Muni: Percent without card | X | 39.64 | 43.84 | 45.80 | 44.75 | 0.51 |
| Muni: Percent in agricultural sector | X | 23.69 | 28.47 | 32.41 | 31.50 | 0.60 |
| Muni: Percent black | | 6.51 | 6.64 | 7.08 | 6.93 | 0.66 |
| Muni: Percent "pardo" | X | 46.95 | 51.53 | 46.44 | 46.46 | 0.99 |
| Muni: Percent indigenous | | 0.38 | 0.43 | 0.42 | 0.50 | 0.11 |
| Muni: Percent with "adequate housing" | | 34.98 | 28.53 | 30.97 | 32.36 | 0.41 |
| Muni: Percent with piped water | X | 69.70 | 64.45 | 65.66 | 66.97 | 0.38 |
| Muni: Percent with solid waste collection | X | 71.24 | 64.15 | 63.45 | 64.70 | 0.52 |
| Muni: Percent with general sewage network | | 38.56 | 30.62 | 34.17 | 36.79 | 0.17 |
| Muni: Percent with septic tanks | | 14.28 | 14.02 | 11.77 | 11.34 | 0.72 |
| Muni: Percent with electricity | | 91.09 | 88.38 | 87.83 | 89.87 | 0.07 |
| Muni: Landline phones (per 1,000) | | 246.47 | 225.19 | 237.68 | 241.78 | 0.44 |
| Muni: Cell phones (per 1,000) | | 146.18 | 135.04 | 139.46 | 144.01 | 0.37 |
| Muni: Average years of education | | 5.57 | 5.21 | 4.98 | 5.06 | 0.43 |
| Muni: School attendance rate: 7-14 y.o. | X | 94.51 | 93.73 | 93.44 | 93.65 | 0.56 |
| Muni: Illiteracy rate: 7 to 14 y.o. | | 12.77 | 15.70 | 14.85 | 14.21 | 0.41 |
| Muni: Number of public schools per capita | X | 0.001 | 0.001 | 0.001 | 0.001 | 0.30 |
| Muni: Number of students per elementary class | | 28.22 | 27.94 | 27.07 | 27.22 | 0.50 |
| Muni: Clinics (per 1,000) | | 2.04 | 1.76 | 1.47 | 1.51 | 0.61 |

### 1.3.5 Accounting for high variance

The main drawback to the propensity score weighting method is that the variance associated with the estimator is high relative to other estimation strategies (Freedman and Berk 2008). As a result, one consideration in using propensity score weighting is the potential to make statistical Type II errors—i.e., to accept the null hypothesis even though it is not true. In practical terms, Type II errors imply

potentially missing significant impacts of *Bolsa Família*, leading us to take two specific measures to deal with high variance.

First, to the extent possible, we lower the variance of the propensity score weighting estimator by including the covariates used to estimate the propensity score directly in all our weighted regressions estimating treatment effects. As mentioned above, these variables should no longer affect the point estimate of the treatment effect after propensity score weighting, but act only to improve its precision.

Second, when we find impact estimates that are borderline significant (i.e., statistically significant between the 5-10 percent level) even after adding covariates, we attempt to also estimate them via nearest neighbor matching, also known as "covariate matching" (Abadie and Imbens 2006) and confirm that results are significant. Covariate matching compares outcomes among treated observations with outcomes among selected observations in the comparison group for which a set of explanatory variables is closest to the treated observation, according to a distance measure. The estimated treatment effect is the average difference for the outcome between the treatment and "nearest neighbor" comparison observations. An advantage to using covariate matching is that it is a lower variance estimation strategy relative to propensity score weighting. Covariate matching is also entirely nonparametric and does not rely on the distributional assumptions that underlie the probit or logit model used to estimate propensity scores in propensity score weighting. However, covariate matching does not allow us to readily take into account sample weights and attrition weights, such that we would expect slight differences between the point estimates of impacts based on propensity score weighting vs. covariate matching. Moreover, covariate matching is very computationally intensive in large samples and therefore, for a dataset as large as our *Bolsa Família* study sample, is more suitable to use as a robustness check for borderline-significant results than as our primary estimation strategy.

## Section 2. Full Set of Impact Estimates

Note that, for all tables below, significance levels are denoted as follows:

** Significant at the 10 percent level, ** significant at the 5 percent level, *** significant at the 1 percent level.*

### 2.1 Children's welfare

#### 2.1.1 Birthweight

We consider the following outcomes related to infant health: mean birthweight in kilograms (Tables A4-A6), proportion of low-birthweight children (Tables A7-A9), proportion of children born full-term (Tables A10-A12), proportion of children ever breastfed (Tables A13-A15), and proportion of children breastfed exclusively for six or more months (Tables A16-A18).

In general, there are very few significant impacts, although sample sizes are too small to interpret the results conclusively as there being no impact. Among all the outcomes considered, impact coefficients on proportion of children born full-term are also mixed but tend to be either positive and significant or positive and insignificant. Among girls, both Comparison 2 and Comparison 3 suggest significant positive impacts.

**Table A4: Impact of *Bolsa Família* on mean birthweight (in kg) among children aged 0-1 in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.022 | 0.026 |
| | (0.072) | (0.068) |
| Observations | 361 | 561 |
| Regional mean impacts (unconditioned) | | |
| North | -0.483 | -0.379 |
| | (0.203) ** | (0.162) ** |
| North-East | -0.091 | -0.175 |
| | (0.134) | (0.145) |
| South-East | 0.128 | 0.447 |
| | (0.227) | (0.329) |
| South | -0.011 | -0.095 |
| | (0.060) | (0.097) |
| Centre-West | 0.494 | 0.439 |
| | (0.260) * | (0.248) * |
| Observations | 372 | 574 |

**Table A5: Impact of *Bolsa Família* on mean birthweight (in kg) among boys aged 0-1 in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.061 | 0.097 |
| | (0.075) | (0.092) |
| Observations | 184 | 273 |

**Table A6: Impact of *Bolsa Família* on mean birthweight (in kg) among girls aged 0-1 in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.084 | -0.070 |
| | (0.115) | (0.090) |
| Observations | 177 | 286 |

**Table A7: Impact of *Bolsa Família* on proportion of low-birthweight children among children aged 0-1 in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.031 | -0.054 |
| | (0.042) | (0.032) |
| Observations | 361 | 561 |
| Regional mean impacts (unconditioned) | | |
| North | 0.214 | 0.144 |
| | (0.164) | (0.116) |
| North-East | 0.030 | -0.016 |
| | (0.045) | (0.047) |
| South-East | -0.179 | -0.436 |
| | (0.120) | (0.238) * |
| South | 0.006 | 0.040 |
| | (0.008) | (0.037) |
| Centre-West | -0.185 | -0.189 |
| | (0.183) | (0.184) |
| Observations | 372 | 574 |

**Table A8: Impact of *Bolsa Família* on proportion of low-birthweight children among boys aged 0-1 in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.047 | -0.054 |
| | (0.041) | (0.033) |
| Observations | 184 | 273 |

**Table A9: Impact of *Bolsa Família* on proportion of low-birthweight children among girls aged 0-1 in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.067 | -0.014 |
| | (0.075) | (0.052) |
| Observations | 177 | 286 |

**Table A10: Impact of *Bolsa Família* on proportion of children born full-term among children aged 0-1 in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.107 | 0.079 |
| | (0.060) * | (0.053) |
| Observations | 411 | 629 |
| Regional mean impacts (unconditioned) | | |
| North | -0.001 | -0.001 |
| | (0.011) | (0.009) |
| North-East | 0.245 | 0.066 |
| | (0.142) * | (0.061) |
| South-East | 0.111 | 0.343 |
| | (0.126) | (0.251) |
| South | -0.021 | -0.112 |
| | (0.023) | (0.070) |
| Centre-West | 0.090 | 0.044 |
| | (0.141) | (0.193) |
| Observations | 422 | 642 |

**Table A11: Impact of *Bolsa Família* on proportion of children born full-term among boys aged 0-1 in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.068 | -0.025 |
| | (0.055) | (0.042) |
| Observations | 208 | 305 |

**Table A12: Impact of *Bolsa Família* on proportion of children born full-term among girls aged 0-1 in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.158 | 0.148 |
| | (0.087) * | (0.069) ** |
| Observations | 187 | 322 |

**Table A13: Impact of *Bolsa Família* on proportion of children ever breastfed among children aged 0-1 in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.012 | -0.019 |
| | (0.031) | (0.027) |
| Observations | 392 | 643 |
| Regional mean impacts (unconditioned) | | |
| North | 0.000 | -0.011 |
| | (0.000) | (0.011) |
| North-East | -0.024 | -0.021 |
| | (0.041) | (0.033) |
| South-East | -0.446 | -0.347 |
| | (0.256) * | (0.229) |
| South | -0.041 | -0.037 |
| | (0.049) | (0.035) |
| Centre-West | 0.254 | 0.097 |
| | (0.209) | (0.097) |
| Observations | 429 | 656 |

**Table A14: Impact of *Bolsa Família* on proportion of children ever breastfed among boys aged 0-1 in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.021 | -0.003 |
| | (0.052) | (0.040) |
| Observations | 217 | 316 |

**Table A15: Impact of *Bolsa Família* on proportion of children ever breastfed among girls aged 0-1 in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.002 | -0.037 |
| | (0.039) | (0.049) |
| Observations | 201 | 325 |

**Table A16: Impact of *Bolsa Família* on proportion of children ever breastfed exclusively for 6+ months among children aged 6-23 months in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.170 | -0.051 |
| | (0.102) * | (0.099) |
| Observations | 167 | 256 |
| Regional mean impacts (unconditioned) | | |
| North | -0.135 | -0.173 |
| | (0.099) | (0.100) * |
| North-East | -0.252 | 0.157 |
| | (0.169) | (0.242) |
| South-East | -0.384 | -0.570 |
| | (0.203) * | (0.210) *** |
| South | -0.407 | -0.322 |
| | (0.303) | (0.340) |
| Centre-West | 0.229 | 0.261 |
| | (0.325) | (0.306) |
| Observations | 173 | 264 |

**Table A17: Impact of *Bolsa Família* on proportion of children ever breastfed exclusively for 6+ months among boys aged 6-23 months in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.145 | -0.210 |
| | (0.140) | (0.158) |
| Observations | 75 | 113 |

**Table A18: Impact of *Bolsa Família* on proportion of children ever breastfed exclusively for 6+ months among girls aged 6-23 months in 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.135 | -0.049 |
| | (0.144) | (0.131) |
| Observations | 92 | 141 |

### 2.1.2 Anthropometry

We consider the following outcomes related to anthropometry for children under age 5: HAZ scores, WAZ scores, WHZ scores, BMI-for-age Z-scores, stunting prevalence, and wasting prevalence (Tables A19 through A24).

We find no impact on HAZ scores or WAZ scores. Since HAZ scores and WAZ scores increased over time among both recipients and non-recipients, as shown in the descriptive report, this result is not surprising. There is a weakly significant positive impact on WHZ scores using Comparison 3 and strongly significant impacts on BMI-for-age using Comparison 2 or Comparison 3. There are no significant impacts on stunting prevalence. There are also no significant impacts on wasting prevalence, suggesting that the positive impacts on WHZ scores are not concentrated in the lower tail of the distribution.

**Table A19: Impact of *Bolsa Família* on HAZ scores, under 5 years old, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.219 | -0.205 |
| | (0.150) | (0.136) |
| Observations | 1012 | 1453 |
| Regional mean impacts (unconditioned) | | |
| North | -0.005 | -0.346 |
| | (0.408) | (0.355) |
| North-East | 0.347 | -0.429 |
| | (0.251) | (0.315) |
| South-East | -1.075** | -1.215 |
| | (0.435) | (0.820) |
| South | -0.501 | -0.413 |
| | (0.499) | (0.472) |
| Centre-West | 1.514** | 1.900** |
| | (0.751) | (0.790) |
| Observations | 1042 | 1485 |

**Table A20: Impact of *Bolsa Família* on WAZ scores, under 5 years old, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.037 | -0.080 |
| | (0.173) | (0.129) |
| Observations | 1048 | 1601 |
| Regional mean impacts (unconditioned) | | |
| North | 0.657 | 0.729 |
| | (0.437) | (0.395) |
| North-East | 0.583** | 0.066 |
| | (0.201) | (0.244) |
| South-East | -0.495** | -0.372 |
| | (0.233) | (0.213) |
| South | -0.698 | -0.505 |
| | (0.519) | (0.470) |
| Centre-West | -0.176 | 1.011 |
| | (0.725) | (0.682) |
| Observations | 1078 | 1635 |

**Table A21: Impact of *Bolsa Família* on WHZ scores, under 5 years old, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.132 | 0.287* |
| | (0.214) | (0.161) |
| Observations | 961 | 1403 |
| Regional mean impacts (unconditioned) | | |
| North | 0.743 | 0.713 |
| | (0.459) | (0.406) |
| North-East | 0.872** | 0.559* |
| | (0.232) | (0.293) |
| South-East | -0.053 | 0.102 |
| | (0.227) | (0.213) |
| South | -1.075 | -0.616 |
| | (0.638) | (0.543) |
| Centre-West | 0.862 | 1.101 |
| | (0.625) | (0.743) |
| Observations | 987 | 1431 |

**Table A22: Impact of *Bolsa Família* on BMI-for-Age Z-scores, under 5 years old, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.342 | 0.298 |
| | (0.197)* | (0.175)* |
| Observations | 974 | 1500 |
| Regional mean impacts (unconditioned) | | |
| North | 0.563 | 0.548 |
| | (0.431) | (0.388) |
| North-East | 0.755 | 0.568 |
| | (0.250) | (0.311) |
| South-East | 0.260 | 0.923 |
| | (0.334) | (0.692) |
| South | -0.048 | 0.316 |
| | (0.506) | (0.535) |
| Centre-West | 0.710 | 0.992 |
| | (0.617) | (0.717) |
| Observations | 1002 | 1533 |

**Table A23: Impact of *Bolsa Família* on stunting prevalence, under 5 years old, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.026 | 0.016 |
| | (0.036) | (0.025) |
| Observations | 1250 | 1906 |
| Regional mean impacts (unconditioned) | | |
| North | 0.033 | 0.054 |
| | (0.110) | (0.082) |
| North-East | -0.064 | 0.014 |
| | (0.071) | (0.035) |
| South-East | 0.050 | 0.056** |
| | (0.029) | (0.022) |
| South | 0.093 | 0.076 |
| | (0.145) | (0.126) |
| Centre-West | -0.407** | -0.461** |
| | (0.190) | (0.229) |
| Observations | 1290 | 1954 |

**Table A24: Impact of *Bolsa Família* on wasting prevalence, under 5 years old, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.025 | -0.013 |
| | (0.044) | (0.033) |
| Observations | 1177 | 1787 |
| Regional mean impacts (unconditioned) | | |
| North | -0.086 | -0.086 |
| | (0.107) | (0.088) |
| North-East | -0.076 | -0.125 |
| | (0.053) | (0.084) |
| South-East | -0.027 | -0.018 |
| | (0.037) | (0.033) |
| South | 0.372** | 0.282** |
| | (0.159) | (0.137) |
| Centre-West | -0.100 | -0.125 |
| | (0.100) | (0.141) |
| Observations | 1215 | 1833 |

### 2.1.3 Vaccinations

We consider the following outcomes related to vaccinations: the proportion of children with a vaccination card in 2009, the proportion of children receiving on-time vaccinations for specific vaccines, and the probability that a child receives all seven vaccines required by age 6 months on time.

There is no evidence of an impact of *Bolsa Família* on the proportion of children with a vaccination card in 2009 (Table A25). We note that there was little room for an impact on this outcome because the vast majority of children already had a vaccination card in 2009. We find evidence that *Bolsa Família* led to a significant increase in the proportion of children receiving on-time vaccinations for the DPT2, DPT3, and polio3 vaccines (Table A26) but a puzzling negative impact of BF on probability of receiving DPT1 vaccine on schedule for children in both age groups which in turn reduces the likelihood that children received all vaccines by age 6 months. The strong evidence of impact on the DPT3 vaccine adherence is important because this measure is often used as an indication of the effectiveness of a country's vaccination program.

**Table A25: Impact of *Bolsa Família* on proportion of children with a vaccination card, ages 6-23 months, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Mean proportion of children age 6-23 months with a vaccination card | 0.902 | 0.900 |
| Impact on *Bolsa Família* recipients | -0.165 | -0.030 |
| | (0.094)* | (0.058) |
| Observations | 338 | 512 |

**Table A26: Impact of *Bolsa Família* on probability of receiving vaccinations on schedule, 2009**

| | Age schedule for | | Age 6-23 months | | Age 6-35 months | |
|---|---|---|---|---|---|---|
| Vaccine | vaccination | Estimate | Comparison 2 | Comparison 3 | Comparison 2 | Comparison 3 |

| | | | | | | |
|---|---|---|---|---|---|---|
| BCG | At birth | ATT | 0.012 | 0.017 | 0.032 | 0.051 |
| | | Standard error | (0.050) | (0.035) | (0.047) | (0.035) |
| | | N | 267 | 403 | 477 | 734 |
| HBV1 | At birth | ATT | -0.029 | 0.020 | 0.004 | 0.015 |
| | | Standard error | (0.056) | (0.052) | (0.058) | (0.050) |
| | | N | 270 | 406 | 483 | 742 |
| HBV2 | 2-4 months | ATT | -0.010 | -0.002 | -0.020 | -0.030 |
| | | Standard error | (0.039) | (0.030) | (0.027) | (0.022) |
| | | N | 273 | 407 | 483 | 739 |
| DPT1 | 2 months | ATT | -0.148 | -0.117 | -0.107 | -0.116 |
| | | Standard error | (0.076)* | (0.061)* | (0.060)* | (0.045)** |
| | | N | 219 | 336 | 387 | 600 |
| Polio1 | 2 months | ATT | -0.089 | -0.010 | -0.051 | -0.052 |
| | | Standard error | (0.073) | (0.057) | (0.058) | (0.043) |
| | | N | 273 | 408 | 488 | 744 |
| DPT2 | 4 months | ATT | 0.258 | 0.155 | 0.298 | 0.239 |
| | | Standard error | (0.106)** | (0.074)** | (0.080)*** | (0.060)*** |
| | | N | 204 | 318 | 365 | 575 |
| Polio2 | 4 months | ATT | 0.116 | 0.069 | 0.141 | 0.132 |
| | | Standard error | (0.084) | (0.076) | (0.077)* | (0.066)** |
| | | N | 273 | 407 | 485 | 737 |
| HBV3 | 6 months | ATT | -0.011 | 0.032 | 0.020 | 0.057 |
| | | Standard error | (0.064) | (0.055) | (0.048) | (0.040) |
| | | N | 261 | 391 | 468 | 715 |
| DPT3 | 6 months | ATT | 0.348 | 0.260 | 0.325 | 0.253 |
| | | Standard error | (0.097)*** | (0.087)*** | (0.086)*** | (0.066)*** |
| | | N | 197 | 301 | 352 | 547 |
| Polio3 | 6 months | ATT | 0.148 | 0.116 | 0.170 | 0.128 |
| | | Standard error | (0.079)* | (0.070)* | (0.071)** | (0.058)** |
| | | N | 255 | 377 | 463 | 702 |
| SAR | 12 months | ATT | 0.121 | 0.009 | 0.220 | 0.023 |
| | | Standard error | (0.099) | (0.113) | (0.096)** | (0.074) |
| | | N | 99 | 143 | 189 | 287 |

**Table A27: Impact of *Bolsa Família* on aggregate measures of timely vaccinations, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Received the first seven vaccines by age 6 months | -0.208 | 0.009 |
| | (0.107)* | (0.071) |
| Observations | 338 | 512 |
| Number of vaccines received by age 6 months | -1.632 | -0.546 |
| | (0.616)*** | (0.392) |
| Observations | 338 | 512 |
| Received no vaccines by age 6 months | 0.188 | 0.080 |
| | (0.089)** | (0.059) |
| Observations | 323 | 489 |

Notes: The first seven recommended vaccines include BCG, HBV1, HBV2, DPT1, polio1, DPT2, and polio2.

### 2.1.4 Education

We consider the following outcomes related to education: the proportion of children currently attending school, the proportion of children in school last year that progressed to the next grade level, the proportion of children in school last year that are repeating the grade level, and the proportion of children in school last year that dropped out (Tables A28 through A51).

In general, we find the following: disaggregated by sex, schooling impacts tend to be concentrated among girls. Disaggregated by region, schooling impacts tend to be concentrated in the North-East. Disaggregated by age, schooling impacts tend to be concentrated among 15-year-olds and 17-year-olds.

Specifically, in the North-East, among children age 6-17, there are significant increases in the proportion currently attending, significant increases in the proportion progressing to the next grade level, insignificant changes in the proportion repeating the previous grade level, and significant decreases in the proportion dropping out.

There are significant increases in the proportion currently attending among 15-year-old girls and 17-year-old girls. There are highly significant decreases in the proportion dropping out among 17-year-old girls and weakly significant *increases* in the proportion dropping out among 16-year-old girls. There are (weakly) significant increases in the proportion progressing to the next grade level among 17-year-old girls and (weakly) significant decreases in the proportion repeating a grade level among 16-year-old girls. There appear to be no consistent impacts among males.

**Table A28: Impact of *Bolsa Família* on proportion of children "currently attending school", 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | | |
| Children age 6-17 | 0.045 | 0.041 |
| | (0.026) * | (0.025) |
| Observations | 6514 | 10993 |
| Regional mean impacts (unconditioned) | | |
| Children age 6-17 | | |
| North | -0.032 | -0.006 |
| | (0.051) | (0.042) |
| North-East | 0.161 | 0.199 |
| | (0.051) *** | (0.058) *** |
| South-East | -0.034 | -0.009 |
| | (0.063) | (0.052) |
| South | -0.156 | -0.111 |
| | (0.087) * | (0.073) |
| Centre-West | 0.040 | 0.020 |
| | (0.029) * | (0.033) |
| Observations | 6749 | 11361 |

**Table A29: Impact of *Bolsa Família* on proportion of children in school last year that progressed to next grade level, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | | |
| Children age 6-17 | 0.037 | 0.069 |
| | (0.035) | (0.033) ** |
| Observations | 4539 | 7703 |
| Regional mean impacts (unconditioned) | | |

| | | |
|---|---|---|
| Children age 6-17 | | |
| North | -0.058 | -0.051 |
| | (0.079) | (0.064) |
| North-East | 0.121 | 0.117 |
| | (0.070) * | (0.062) * |
| South-East | -0.012 | 0.087 |
| | (0.063) | (0.074) |
| South | 0.132 | 0.223 |
| | (0.183) | (0.153) |
| Centre-West | -0.082 | -0.074 |
| | (0.085) | (0.072) |
| Observations | 4704 | 7956 |

**Table A30: Impact of *Bolsa Família* on proportion of children in school last year that are repeating grade level, 2009**

| | **Comparison 2** | **Comparison 3** |
|---|---|---|
| Overall, conditional on baseline covariates | | |
| Children age 6-17 | -0.008 | -0.050 |
| | (0.032) | (0.030) * |
| Observations | 4539 | 7703 |
| Regional mean impacts (unconditioned) | | |
| Children age 6-17 | | |
| North | 0.059 | 0.060 |
| | (0.054) | (0.045) |
| North-East | 0.001 | 0.021 |
| | (0.052) | (0.040) |
| South-East | -0.017 | -0.128 |
| | (0.046) | (0.073) * |
| South | -0.250 | -0.283 |
| | (0.181) | (0.153) * |
| Centre-West | 0.076 | 0.060 |
| | (0.082) | (0.067) |
| Observations | 4704 | 7956 |

**Table A31: Impact of *Bolsa Família* on proportion of children in school last year that dropped out, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | | |
| Children age 6-17 | -0.029 | -0.019 |
| | (0.022) | (0.017) |
| Observations | 4539 | 7703 |
| Regional mean impacts (unconditioned) | | |
| Children age 6-17 | | |
| North | -0.000 | -0.009 |
| | (0.069) | (0.054) |
| North-East | -0.122 | -0.138 |
| | (0.048) ** | (0.058) ** |
| South-East | 0.029 | 0.041 |
| | (0.050) | (0.032) |
| South | 0.118 | 0.060 |
| | (0.045) *** | (0.022) *** |
| Centre-West | 0.007 | 0.015 |
| | (0.021) | (0.025) |
| Observations | 4704 | 7956 |

**Table A32: Impact of *Bolsa Família* on proportion of boys "currently attending school", 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | | |
| Males age 6-17 | 0.012 | 0.032 |
| | (0.035) | (0.034) |
| Observations | 3374 | 5633 |
| Regional mean impacts (unconditioned) | | |
| Males age 6-17 | | |
| North | -0.032 | -0.003 |
| | (0.063) | (0.056) |
| North-East | 0.114 | 0.229 |
| | (0.062) * | (0.090) ** |
| South-East | -0.068 | -0.011 |
| | (0.107) | (0.087) |
| South | -0.274 | -0.062 |
| | (0.135) ** | (0.111) |
| Centre-West | 0.089 | 0.060 |
| | (0.047) * | (0.054) |
| Observations | 3491 | 5817 |

**Table A33: Impact of *Bolsa Família* on proportion of girls "currently attending school", 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | | |
| Females age 6-17 | 0.082 | 0.038 |
| | (0.033) ** | (0.034) |
| Observations | 3133 | 5349 |
| Regional mean impacts (unconditioned) | | |
| Females age 6-17 | | |
| North | -0.030 | -0.008 |
| | (0.084) | (0.061) |
| North-East | 0.205 | 0.166 |
| | (0.074) *** | (0.066) ** |
| South-East | -0.004 | -0.013 |
| | (0.068) | (0.046) |
| South | -0.038 | -0.189 |
| | (0.041) | (0.086) ** |
| Centre-West | -0.004 | -0.022 |
| | (0.039) | (0.038) |
| Observations | 3250 | 5532 |

**Table A34: Impact of *Bolsa Família* on proportion of boys in school last year that progressed to next grade level, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | | |
| Males age 6-17 | -0.033 | 0.006 |
| | (0.036) | (0.039) |
| Observations | 2312 | 3911 |
| Regional mean impacts (unconditioned) | | |
| Males age 6-17 | | |
| North | -0.056 | -0.035 |
| | (0.110) | (0.096) |
| North-East | 0.082 | 0.146 |
| | (0.102) | (0.097) |
| South-East | 0.011 | 0.111 |
| | (0.060) | (0.098) |
| South | -0.075 | 0.130 |
| | (0.250) | (0.207) |
| Centre-West | -0.088 | -0.081 |
| | (0.136) | (0.122) |
| Observations | 2391 | 4030 |

**Table A35: Impact of *Bolsa Família* on proportion of girls in school last year that progressed to next grade level, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | | |
| Females age 6-17 | 0.099 | 0.099 |
| | (0.048) ** | (0.048) ** |
| Observations | 2222 | 3786 |
| Regional mean impacts (unconditioned) | | |
| Females age 6-17 | | |
| North | -0.059 | -0.068 |
| | (0.113) | (0.081) |
| North-East | 0.160 | 0.083 |
| | (0.086) * | (0.074) |
| South-East | -0.014 | 0.065 |
| | (0.105) | (0.108) |
| South | 0.338 | 0.348 |
| | (0.222) | (0.203) * |
| Centre-West | -0.030 | -0.047 |
| | (0.061) | (0.066) |
| Observations | 2307 | 3919 |

**Table A36: Impact of *Bolsa Família* on proportion of boys in school last year that are repeating grade level, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | | |
| Males age 6-17 | 0.057 | -0.009 |
| | (0.033) * | (0.038) |
| Observations | 2312 | 3911 |
| Regional mean impacts (unconditioned) | | |
| Males age 6-17 | | |
| North | 0.105 | 0.086 |
| | (0.093) | (0.079) |
| North-East | 0.033 | 0.057 |
| | (0.059) | (0.039) |
| South-East | -0.009 | -0.135 |
| | (0.057) | (0.099) |
| South | -0.126 | -0.201 |
| | (0.246) | (0.204) |
| Centre-West | 0.110 | 0.088 |
| | (0.134) | (0.117) |
| Observations | 2391 | 4030 |

**Table A37: Impact of *Bolsa Família* on proportion of girls in school last year that are repeating grade level, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | | |
| Females age 6-17 | -0.057 | -0.084 |
| | (0.045) | (0.042) ** |
| Observations | 2222 | 3786 |
| Regional mean impacts (unconditioned) | | |
| Females age 6-17 | | |
| North | 0.005 | 0.032 |
| | (0.030) | (0.031) |
| North-East | -0.031 | -0.019 |
| | (0.083) | (0.072) |
| South-East | -0.026 | -0.111 |
| | (0.071) | (0.103) |
| South | -0.399 | -0.400 |
| | (0.224) * | (0.204) * |
| Centre-West | -0.004 | 0.008 |
| | (0.042) | (0.046) |
| Observations | 2307 | 3919 |

**Table A38: Impact of *Bolsa Família* on proportion of boys in school last year that dropped out, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | | |
| Males age 6-17 | -0.024 | 0.003 |
| | (0.022) | (0.016) |
| Observations | 2312 | 3911 |
| Regional mean impacts (unconditioned) | | |
| Males age 6-17 | | |
| North | -0.049 | -0.051 |
| | (0.077) | (0.071) |
| North-East | -0.114 | -0.203 |
| | (0.071) | (0.099) ** |
| South-East | -0.002 | 0.024 |
| | (0.016) | (0.013) * |
| South | 0.202 | 0.071 |
| | (0.090) ** | (0.037) * |
| Centre-West | -0.022 | -0.006 |
| | (0.027) | (0.031) |
| Observations | 2391 | 4030 |

**Table A39: Impact of *Bolsa Família* on proportion of girls in school last year that dropped out, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | | |
| Females age 6-17 | -0.042 | -0.015 |
| | (0.032) | (0.024) |
| Observations | 2222 | 3786 |
| Regional mean impacts (unconditioned) | | |
| Females age 6-17 | | |
| North | 0.054 | 0.036 |
| | (0.116) | (0.082) |
| North-East | -0.129 | -0.064 |
| | (0.064) ** | (0.040) |
| South-East | 0.040 | 0.046 |
| | (0.095) | (0.063) |
| South | 0.061 | 0.052 |
| | (0.034) * | (0.023) ** |
| Centre-West | 0.034 | 0.039 |
| | (0.035) | (0.041) |
| Observations | 2307 | 3919 |

**Table A40: Impact of *Bolsa Família* on proportion of children "currently attending school", by age, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | | |
| Children age 6 | 0.027 | 0.147 |
| | (0.101) | (0.079) * |
| *Obs* | *404* | *677* |
| Children age 7 | 0.010 | -0.015 |
| | (0.025) | (0.036) |
| *Obs* | *420* | *745* |
| Children age 13 | 0.073 | 0.081 |
| | (0.039) * | (0.045) * |
| *Obs* | *595* | *1021* |
| Children age 14 | -0.081 | -0.076 |
| | (0.035) ** | (0.035) ** |
| *Obs* | *617* | *1020* |
| Children age 15 | 0.187 | 0.158 |
| | (0.048) *** | (0.057) *** |
| *Obs* | *651* | *1055* |
| Children age 16 | -0.049 | -0.006 |
| | (0.054) | (0.049) |
| *Obs* | *688* | *1068* |
| Children age 17 | 0.070 | 0.004 |
| | (0.071) | (0.069) |
| *Obs* | *601* | *945* |

**Table A41: Impact of *Bolsa Família* on proportion of children in school last year that progressed to next grade level, by age, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | | |
| Children age 6 | - | - |
| | - | - |
| Children age 7 | -0.223 | -0.159 |
| | (0.084) *** | (0.063) ** |
| *Obs* | *223* | *379* |
| Children age 13 | -0.015 | 0.085 |
| | (0.085) | (0.072) |
| *Obs* | *472* | *816* |
| Children age 14 | -0.092 | -0.104 |
| | (0.046) ** | (0.042) ** |
| *Obs* | *469* | *777* |
| Children age 15 | 0.182 | 0.172 |
| | (0.058) *** | (0.057) *** |
| *Obs* | *441* | *726* |
| Children age 16 | 0.072 | 0.029 |
| | (0.066) | (0.079) |
| *Obs* | *474* | *729* |
| Children age 17 | 0.037 | 0.047 |
| | (0.054) | (0.054) |
| *Obs* | *392* | *608* |

**Table A42: Impact of *Bolsa Família* on proportion of children in school last year that are repeating grade level, by age, 2009**

| | | Comparison 2 | Comparison 3 |
|---|---|---|---|
| Overall, conditional on baseline covariates | | | |
| Children age 6 | | - | - |
| | | - | - |
| Children age 7 | | 0.216 | 0.090 |
| | | (0.080) *** | (0.055) |
| | *Obs* | *223* | *379* |
| Children age 13 | | 0.042 | -0.064 |
| | | (0.088) | (0.069) |
| | *Obs* | *472* | *816* |
| Children age 14 | | 0.004 | 0.031 |
| | | (0.022) | (0.025) |
| | *Obs* | *469* | *777* |
| Children age 15 | | -0.058 | -0.052 |
| | | (0.055) | (0.049) |
| | *Obs* | *441* | *726* |
| Children age 16 | | -0.119 | -0.093 |
| | | (0.059) ** | (0.062) |
| | *Obs* | *474* | *729* |
| Children age 17 | | 0.064 | 0.060 |
| | | (0.047) | (0.042) |
| | *Obs* | *392* | *608* |

**Table A43: Impact of *Bolsa Família* on proportion of children in school last year that dropped out, by age, 2009**

| | | Comparison 2 | Comparison 3 |
|---|---|---|---|
| Overall, conditional on baseline covariates | | | |
| Children age 6 | | - | - |
| | | - | - |
| Children age 7 | | 0.008 | 0.068 |
| | | (0.025) | (0.029) ** |
| | *Obs* | *223* | *379* |
| Children age 13 | | -0.027 | -0.020 |
| | | (0.023) | (0.024) |
| | *Obs* | *472* | *816* |
| Children age 14 | | 0.088 | 0.073 |
| | | (0.039) ** | (0.035) ** |
| | *Obs* | *469* | *777* |
| Children age 15 | | -0.124 | -0.120 |
| | | (0.040) *** | (0.042) *** |
| | *Obs* | *441* | *726* |
| Children age 16 | | 0.048 | 0.064 |
| | | (0.056) | (0.048) |
| | *Obs* | *474* | *729* |
| Children age 17 | | -0.102 | -0.108 |
| | | (0.038) *** | (0.047) ** |
| | *Obs* | *392* | *608* |

**Table A44: Impact of *Bolsa Família* on proportion of boys "currently attending school", by age, 2009**

| | | Comparison 2 | Comparison 3 |
|---|---|---|---|
| Overall, conditional on baseline covariates | | | |
| Males age 6 | | 0.134 | 0.148 |
| | | (0.061) ** | (0.066) ** |
| | *Obs* | *217* | *341* |
| Males age 7 | | 0.002 | 0.034 |
| | | (0.041) | (0.041) |
| | *Obs* | *220* | *378* |
| Males age 13 | | 0.055 | 0.091 |
| | | (0.056) | (0.061) |
| | *Obs* | *317* | *546* |
| Males age 14 | | -0.042 | -0.035 |
| | | (0.037) | (0.025) |
| | *Obs* | *311* | *531* |
| Males age 15 | | 0.222 | 0.244 |
| | | (0.070) *** | (0.074) *** |
| | *Obs* | *323* | *527* |
| Males age 16 | | 0.039 | 0.086 |
| | | (0.056) | (0.049) * |
| | *Obs* | *363* | *551* |
| Males age 17 | | -0.012 | -0.033 |
| | | (0.078) | (0.093) |
| | *Obs* | *305* | *492* |

**Table A45: Impact of *Bolsa Família* on proportion of girls "currently attending school", by age, 2009**

| | | Comparison 2 | Comparison 3 |
|---|---|---|---|
| Overall, conditional on baseline covariates | | | |
| Females age 6 | | 0.244 | 0.254 |
| | | (0.095) ** | (0.076) *** |
| | *Obs* | *186* | *335* |
| Females age 7 | | -0.001 | -0.031 |
| | | (0.028) | (0.028) |
| | *Obs* | *199* | *366* |
| Females age 13 | | -0.011 | -0.038 |
| | | (0.021) | (0.027) |
| | *Obs* | *277* | *473* |
| Females age 14 | | -0.076 | -0.084 |
| | | (0.047) | (0.046) * |
| | *Obs* | *306* | *488* |
| Females age 15 | | 0.178 | 0.143 |
| | | (0.053) *** | (0.064) ** |
| | *Obs* | *328* | *528* |
| Females age 16 | | -0.083 | -0.078 |
| | | (0.081) | (0.067) |
| | *Obs* | *325* | *517* |
| Females age 17 | | 0.200 | 0.189 |
| | | (0.073) *** | (0.079) ** |
| | *Obs* | *295* | *452* |

**Table A46: Impact of *Bolsa Família* on proportion of boys in school last year that progressed to next grade level, by age, 2009**

| | | Comparison 2 | Comparison 3 |
|---|---|---|---|
| Overall, conditional on baseline covariates | | | |
| Males age 6 | | - | - |
| | | - | - |
| Males age 7 | | -0.179 | -0.114 |
| | | (0.133) | (0.093) |
| | *Obs* | *112* | *185* |
| Males age 13 | | -0.152 | -0.110 |
| | | (0.078) * | (0.054) ** |
| | *Obs* | *249* | *435* |
| Males age 14 | | -0.059 | -0.082 |
| | | (0.048) | (0.044) * |
| | *Obs* | *233* | *403* |
| Males age 15 | | 0.125 | 0.237 |
| | | (0.082) | (0.091) *** |
| | *Obs* | *219* | *358* |
| Males age 16 | | -0.009 | 0.074 |
| | | (0.072) | (0.069) |
| | *Obs* | *246* | *373* |
| Males age 17 | | -0.013 | -0.111 |
| | | (0.083) | (0.079) |
| | *Obs* | *197* | *310* |

**Table A47: Impact of *Bolsa Família* on proportion of girls in school last year that progressed to next grade level, by age, 2009**

| | | Comparison 2 | Comparison 3 |
|---|---|---|---|
| Overall, conditional on baseline covariates | | | |
| Females age 6 | | - | - |
| | | - | - |
| Females age 7 | | -0.186 | -0.118 |
| | | (0.132) | (0.065) * |
| | *Obs* | *111* | *194* |
| Females age 13 | | 0.076 | 0.098 |
| | | (0.113) | (0.069) |
| | *Obs* | *222* | *380* |
| Females age 14 | | -0.065 | -0.101 |
| | | (0.060) | (0.055) * |
| | *Obs* | *236* | *374* |
| Females age 15 | | 0.209 | 0.173 |
| | | (0.069) *** | (0.059) *** |
| | *Obs* | *222* | *368* |
| Females age 16 | | 0.106 | 0.070 |
| | | (0.085) | (0.103) |
| | *Obs* | *228* | *356* |
| Females age 17 | | -0.032 | 0.073 |
| | | (0.076) | (0.070) |
| | *Obs* | *194* | *297* |

**Table A48: Impact of *Bolsa Família* on proportion of boys in school last year that are repeating grade level, by age, 2009**

| | | Comparison 2 | Comparison 3 |
|---|---|---|---|
| Overall, conditional on baseline covariates | | | |
| Males age 6 | | - | - |
| | | - | - |
| Males age 7 | | 0.206 | 0.112 |
| | | (0.106) * | (0.088) |
| | *Obs* | *112* | *185* |
| Males age 13 | | 0.167 | 0.104 |
| | | (0.076) ** | (0.047) ** |
| | *Obs* | *249* | *435* |
| Males age 14 | | 0.011 | 0.031 |
| | | (0.032) | (0.036) |
| | *Obs* | *233* | *403* |
| Males age 15 | | -0.027 | -0.117 |
| | | (0.087) | (0.089) |
| | *Obs* | *219* | *358* |
| Males age 16 | | 0.029 | -0.098 |
| | | (0.059) | (0.062) |
| | *Obs* | *246* | *373* |
| Males age 17 | | 0.065 | 0.130 |
| | | (0.068) | (0.056) ** |
| | *Obs* | *197* | *310* |

**Table A49: Impact of *Bolsa Família* on proportion of girls in school last year that are repeating grade level, by age, 2009**

| | | Comparison 2 | Comparison 3 |
|---|---|---|---|
| Overall, conditional on baseline covariates | | | |
| Females age 6 | | - | - |
| | | - | - |
| Females age 7 | | 0.175 | 0.084 |
| | | (0.126) | (0.057) |
| | *Obs* | *111* | *194* |
| Females age 13 | | -0.071 | -0.144 |
| | | (0.113) | (0.069) ** |
| | *Obs* | *222* | *380* |
| Females age 14 | | -0.018 | 0.019 |
| | | (0.034) | (0.034) |
| | *Obs* | *236* | *374* |
| Females age 15 | | -0.096 | -0.054 |
| | | (0.059) | (0.048) |
| | *Obs* | *222* | *368* |
| Females age 16 | | -0.120 | -0.104 |
| | | (0.070) * | (0.085) |
| | *Obs* | *228* | *356* |
| Females age 17 | | 0.110 | 0.034 |
| | | (0.077) | (0.073) |
| | *Obs* | *194* | *297* |

**Table A50: Impact of *Bolsa Família* on proportion of boys in school last year that dropped out, by age, 2009**

| | | Comparison 2 | Comparison 3 |
|---|---|---|---|
| Overall, conditional on baseline covariates | | | |
| Males age 6 | | - | |
| | | - | |
| Males age 7 | | -0.027 | 0.002 |
| | | (0.064) | (0.035) |
| | *Obs* | *112* | *185* |
| Males age 13 | | -0.015 | 0.006 |
| | | (0.038) | (0.037) |
| | *Obs* | *249* | *435* |
| Males age 14 | | 0.048 | 0.050 |
| | | (0.038) | (0.024) ** |
| | *Obs* | *233* | *403* |
| Males age 15 | | -0.098 | -0.120 |
| | | (0.047) ** | (0.050) ** |
| | *Obs* | *219* | *358* |
| Males age 16 | | -0.021 | 0.023 |
| | | (0.045) | (0.043) |
| | *Obs* | *246* | *373* |
| Males age 17 | | -0.052 | -0.019 |
| | | (0.053) | (0.062) |
| | *Obs* | *197* | *310* |

**Table A51: Impact of *Bolsa Família* on proportion of girls in school last year that dropped out, by age, 2009**

| | | Comparison 2 | Comparison 3 |
|---|---|---|---|
| Overall, conditional on baseline covariates | | | |
| Females age 6 | | - | - |
| | | - | - |
| Females age 7 | | 0.011 | 0.034 |
| | | (0.019) | (0.024) |
| | *Obs* | *111* | *194* |
| Females age 13 | | -0.004 | 0.046 |
| | | (0.020) | (0.031) |
| | *Obs* | *222* | *380* |
| Females age 14 | | 0.083 | 0.082 |
| | | (0.050) * | (0.046) * |
| | *Obs* | *236* | *374* |
| Females age 15 | | -0.113 | -0.119 |
| | | (0.054) ** | (0.050) ** |
| | *Obs* | *222* | *368* |
| Females age 16 | | 0.014 | 0.033 |
| | | (0.082) | (0.058) |
| | *Obs* | *228* | *356* |
| Females age 17 | | -0.077 | -0.107 |
| | | (0.063) | (0.075) |
| | *Obs* | *194* | *297* |

### 2.1.5 Child labor

We consider the following outcomes related to child labor: the proportion of children aged 5-17 doing any work, age of entry into the labor force, the proportion of children doing domestic work, and the typical number of hours per week spent on domestic work (both conditional on performing some domestic work and unconditional on performing any domestic work).

We find that *Bolsa Família* led to a small reduction in the proportion of children age 5-17 reporting doing any paid work. This small effect is not surprising, as the proportion of children participating in child labor in the baseline is relatively low, at 22-24 percent, and it fell to 13-21 percent by 2009.

There is weak evidence that *Bolsa Família* delayed age at entry into the labor force for children age 5-17 in 2009 (Table A53). The results show that this effect derives from the program's impact on males. Males in this age group had a 0.8-1.0 year increase in age at which they entered the labor force. We also examine how impacts might differ for an older age cohort, those aged 11-20 (Table A54). For that age cohort, the impact of *Bolsa Família* on age at entry to the labor force was insignificant overall, but there are pronounced regional differences. *Bolsa Família* induced a delay in the start of work for children age 11-20 years in the North.

There is little or no impact on the proportion of children age 5-17 participating in domestic work, on average, in 2009 (Table A55), but there are important changes in how much work is being done and by whom. The number of hours spent on domestic work falls sharply by 4 hours per week when using Comparison 3 for individuals who report doing any work. This effect is significant for both males and females in our sample. We also estimate the "unconditional" impact of *Bolsa Família* on hours of domestic work over all *Bolsa Família* beneficiaries, not only those doing some positive amount of domestic work, and find that the patterns are similar to the conditional estimates (Table A57) but are much less precisely measured.

**Table A52: Impact of *Bolsa Família* on proportion of children doing any work, 2009**

| | Children age 5-17 | | Males age 5-17 | | Females age 5-17 | |
|---|---|---|---|---|---|---|
| | Comparison 2 | Comparison 3 | Comparison 2 | Comparison 3 | Comparison 2 | Comparison 3 |
| Overall, conditional on baseline covariates | -0.013 | -0.019 | -0.016 | -0.033 | -0.014 | -0.007 |
| | (0.006)** | (0.008)** | (0.009)* | (0.013)** | (0.008)* | (0.008) |
| Number of observations | 6320 | 10630 | 3276 | 5424 | 3038 | 5196 |
| Regional mean impacts (unconditioned) | | | | | | |
| North | -0.022 | 0.001 | -0.030 | 0.003 | -0.012 | -0.001 |
| | (0.017) | (0.003) | (0.030) | (0.006) | (0.012) | (0.003) |
| North-East | -0.004 | -0.012 | 0.005 | -0.025 | -0.018 | 0.004 |
| | (0.014) | (0.022) | (0.022) | (0.039) | (0.019) | (0.018) |
| South-East | -0.010 | -0.007 | -0.004 | 0.005 | -0.015 | -0.018 |
| | (0.008) | (0.008) | (0.013) | (0.012) | (0.010) | (0.011) |
| South | -0.002 | -0.025 | -0.011 | -0.044 | 0.013 | 0.006 |
| | (0.019) | (0.026) | (0.030) | (0.043) | (0.019) | (0.009) |
| Centre-West | -0.029 | -0.005 | -0.063 | -0.019 | -0.002 | 0.006 |
| | (0.014)** | (0.008) | (0.031)** | (0.016) | (0.005) | (0.005) |
| Number of observations | 6500 | 10913 | 3364 | 5563 | 3129 | 5339 |

**Table A53: Impact of *Bolsa Família* on age of entry into the labor force, age 5-17, 2009**

| | Children age 5-17 | | Males age 5-17 | | Females age 5-17 | |
|---|---|---|---|---|---|---|
| | Comparison 2 | Comparison 3 | Comparison 2 | Comparison 3 | Comparison 2 | Comparison 3 |
| Overall, conditional on baseline covariates | 0.823 | 0.390 | 1.090 | 0.841 | 0.278 | -1.062 |
| | (0.454)* | (0.390) | (0.740) | (0.459)* | (0.501) | (0.614)* |
| Number of observations | 245 | 403 | 156 | 248 | 88 | 154 |
| Regional mean impacts (unconditioned) | | | | | | |
| North | -0.192 | -0.325 | -0.251 | 0.290 | | -0.964 |
| | (1.362) | (0.642) | (1.431) | (1.105) | | (0.877) |
| North-East | 0.029 | -1.276 | 1.766 | -0.566 | -1.616 | -2.458 |
| | (0.932) | (0.956) | (0.810)** | (1.507) | (1.265) | (1.075)** |
| South-East | 0.108 | 0.667 | 1.023 | 0.693 | -0.880 | 0.608 |
| | (0.880) | (0.787) | (1.128) | (1.061) | (1.188) | (1.195) |

| | | | | | | |
|---|---|---|---|---|---|---|
| South | 3.342 | 2.948 | 4.432 | 3.258 | -1.650 | -1.326 |
| | (1.721)* | (1.182)** | (1.792)** | (1.433)** | (0.637)** | (0.690)* |
| Centre-West | 2.005 | 1.253 | 2.188 | 1.882 | 1.196 | -0.471 |
| | (0.820)** | (0.681)* | (0.922)** | (0.763)** | (1.751) | (1.472) |
| Number of observations | 253 | 421 | 161 | 258 | 91 | 162 |

**Table A54: Impact of *Bolsa Família* on age of entry into the labor force, age 11-20, 2009**

| | Individuals age 11-20 | | Males age 11-20 | | Females age 11-20 | |
|---|---|---|---|---|---|---|
| | Comparison 2 | Comparison 3 | Comparison 2 | Comparison 3 | Comparison 2 | Comparison 3 |
| Overall, conditional on baseline covariates | 0.250 | 0.524 | 0.366 | 0.815 | -0.531 | -0.344 |
| | (0.298) | (0.331) | (0.374) | (0.375)** | (0.395) | (0.377) |
| Number of observations | 787 | 1213 | 503 | 776 | 284 | 437 |
| Regional mean impacts (unconditioned) | | | | | | |
| North | 1.256 | 0.973 | 1.316 | 0.505 | 1.634 | 1.457 |
| | (0.680)* | (0.473)** | (0.647)** | (0.529) | (1.004) | (0.621)** |
| North-East | 1.413 | 0.174 | 1.814 | 0.971 | -0.201 | -1.282 |
| | (1.076) | (0.811) | (1.030)* | (1.009) | (1.399) | (0.788) |
| South-East | -0.037 | 0.526 | 0.558 | 0.948 | -1.114 | -0.304 |
| | (0.612) | (0.623) | (0.751) | (0.762) | (0.791) | (0.988) |
| South | 1.166 | 1.694 | 2.600 | 3.573 | -0.578 | -1.502 |
| | (1.454) | (1.604) | (1.640) | (1.452)** | (1.729) | (1.648) |
| Centre-West | 1.685 | 0.743 | 0.969 | 0.287 | 2.102 | 1.064 |
| | (0.737)** | (0.608) | (0.885) | (0.714) | (1.021)** | (0.861) |
| Number of observations | 813 | 1254 | 518 | 799 | 295 | 455 |

**Table A55: Impact of *Bolsa Família* on proportion of children doing domestic work, 2009**

| | Children age 5-17 | | Males age 5-17 | | Females age 5-17 | |
|---|---|---|---|---|---|---|
| | Comparison 2 | Comparison 3 | Comparison 2 | Comparison 3 | Comparison 2 | Comparison 3 |
| Overall, conditional on baseline covariates | -0.035 | -0.004 | -0.034 | -0.038 | -0.043 | 0.016 |
| | (0.036) | (0.030) | (0.045) | (0.038) | (0.046) | (0.040) |
| Number of observations | 6341 | 10693 | 3285 | 5459 | 3050 | 5224 |
| Regional mean impacts (unconditioned) | | | | | | |
| North | 0.029 | 0.096 | 0.111 | 0.168 | -0.075 | 0.013 |
| | (0.057) | (0.047)** | (0.073) | (0.057)*** | (0.086) | (0.072) |
| North-East | -0.043 | 0.008 | -0.131 | -0.029 | 0.049 | 0.026 |
| | (0.073) | (0.053) | (0.132) | (0.076) | (0.078) | (0.072) |
| South-East | -0.039 | -0.028 | -0.021 | -0.080 | -0.116 | -0.027 |
| | (0.051) | (0.056) | (0.053) | (0.076) | (0.082) | (0.080) |
| South | -0.103 | -0.066 | -0.014 | -0.059 | -0.234 | -0.126 |
| | (0.133) | (0.109) | (0.200) | (0.170) | (0.154) | (0.121) |
| Centre-West | 0.063 | 0.056 | 0.003 | -0.020 | 0.117 | 0.129 |
| | (0.116) | (0.111) | (0.155) | (0.153) | (0.169) | (0.161) |
| Number of observations | 6522 | 10976 | 3373 | 5596 | 3142 | 5369 |

**Table A56: Impact of *Bolsa Família* on typical number of hours per week spent on domestic work, conditional on performing some domestic work, 2009**

| | Children age 5-17 | | Males age 5-17 | | Females age 5-17 | |
|---|---|---|---|---|---|---|
| | Comparison 2 | Comparison 3 | Comparison 2 | Comparison 3 | Comparison 2 | Comparison 3 |
| Overall, conditional on baseline covariates | -1.688 | -4.486 | -0.293 | -5.163 | -1.803 | -2.955 |
| | (1.313) | (1.329)*** | (2.099) | (1.776)*** | (1.609) | (1.712)* |
| Number of observations | 2469 | 4151 | 911 | 1490 | 1555 | 2657 |
| Regional mean impacts (unconditioned) | | | | | | |
| North | -0.365 | -3.173 | -3.446 | -9.420 | 1.961 | 0.626 |
| | (2.060) | (2.946) | (3.188) | (5.627)* | (2.707) | (3.310) |
| North-East | 3.031 | -0.208 | 4.462 | 1.514 | 0.379 | -1.900 |
| | (3.475) | (2.277) | (2.423)* | (2.486) | (3.010) | (2.273) |
| South-East | -5.053 | -12.020 | -11.736 | -25.445 | -0.935 | -0.220 |
| | (3.325) | (6.787)* | (6.849)* | (10.337)** | (2.434) | (2.337) |
| South | -8.514 | -6.993 | -6.195 | -7.415 | -11.845 | -6.020 |
| | (3.551)** | (3.761)* | (5.347) | (4.889) | (4.432)*** | (5.481) |
| Centre-West | -3.385 | -2.300 | -3.994 | -4.231 | -2.795 | -0.632 |
| | (1.736)* | (1.659) | (2.984) | (2.803) | (1.767) | (1.782) |
| Number of observations | 2530 | 4248 | 928 | 1517 | 1599 | 2727 |

**Table A57: Impact of *Bolsa Família* on typical number of hours per week spent on domestic work, unconditional on performing some domestic work, 2009**

| | Children age 5-17 | | Males age 5-17 | | Females age 5-17 | |
|---|---|---|---|---|---|---|
| | Comparison 2 | Comparison 3 | Comparison 2 | Comparison 3 | Comparison 2 | Comparison 3 |
| Overall, conditional on baseline covariates | -0.461 | -0.673 | -0.461 | -1.187 | -0.362 | 0.173 |
| | (0.706) | (0.772) | (0.814) | (0.856) | (1.102) | (1.095) |
| Number of observations | 7021 | 11711 | 3624 | 5976 | 3391 | 5725 |
| Regional mean impacts (unconditioned) | | | | | | |
| North | 0.158 | 0.484 | 0.872 | 0.498 | -0.746 | 0.368 |
| | (0.984) | (0.988) | (1.355) | (1.353) | (1.447) | (1.451) |
| North-East | 1.640 | 0.950 | 0.739 | 0.454 | 2.870 | 1.097 |
| | (0.889)* | (0.793) | (0.420)* | (0.479) | (1.640)* | (1.505) |
| South-East | -1.122 | -3.558 | -1.719 | -6.828 | -1.009 | 0.105 |
| | (1.128) | (2.775) | (1.611) | (4.463) | (1.277) | (1.312) |
| South | -6.853 | -3.923 | -4.124 | -4.032 | -10.094 | -4.073 |
| | (4.334) | (3.709) | (6.128) | (5.112) | (5.756)* | (5.247) |
| Centre-West | -0.017 | 1.471 | -0.889 | 0.545 | 0.791 | 2.386 |
| | (0.943) | (0.757)* | (1.334) | (1.119) | (1.299) | (1.018)** |
| Number of observations | 7228 | 12026 | 3722 | 6128 | 3494 | 5882 |

### 2.2 Women's welfare

#### 2.2.1 Impact on prenatal care

We consider the following outcomes related to prenatal care, both among a sample of women pregnant during the 2009 AIBF survey and among a sample of women pregnant between the 2005 and 2009 survey: the number of prenatal care visits, the proportion of women having no prenatal care visits, the proportion of women receiving adequate prenatal care, and the proportion of women receiving prenatal care from a doctor rather than a nurse or informal care provider.

We find qualified evidence that BF increased use of prenatal care. BF recipients who were pregnant at the time of the 2009 survey had 1.7 more prenatal care visits than pregnant women who were non-recipients (Table A58). We find this result across models using both comparison groups; however, the result is qualified because of the relatively small samples of women pregnant at the time of the 2009 interview. With such a small sample of women who were pregnant at the time of the interview, estimates of impacts at the regional level are not very precise, but the relative magnitudes of the estimated regional effects is still informative. Results show that the impact of *Bolsa Família* on the number of prenatal care visits was largest in the North and was also quite large in the North-East and South-East.

**Table A58: Impact of *Bolsa Família* on the number of prenatal care visits, for women pregnant during the 2009 AIBF survey**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 1.701 | 1.602 |
| | (0.913)* | (0.800)** |
| Observations | 75 | 121 |
| Regional mean impacts (unconditioned) | | |
| North | 5.042 | 4.864 |
| | (1.261)*** | (1.142)*** |
| North-East | 6.155 | 5.842 |
| | (1.146)*** | (0.860)*** |
| South-East | 3.071 | 2.648 |
| | (1.235)** | (1.420)* |
| South | -2.500 | -1.149 |
| | (0.755)*** | (0.981) |
| Centre-West | -1.834 | -2.146 |
| | (0.929)* | (0.575)*** |
| Observations | 78 | 125 |

There is no evidence that Bolsa Família reduced the proportion of pregnant women in the 2009 survey who had no prenatal care visits across the full sample (Table A59). At the regional level, *Bolsa Família* appears to have reduced the proportion of women having no prenatal care visits, but the size of this effect cannot be precisely estimated. We also cannot conclude that *Bolsa Família* had an impact on the proportion of women who obtained timely and adequate prenatal care for their stage of pregnancy among women who were pregnant in the 2009 survey (Table A60). Estimated regional effects show positive impacts of *Bolsa Família* on the proportion of women who obtained adequate prenatal care in the North-East and negative impacts in the North, but samples are too small for these estimates to be considered reliable. There is also no evidence that participation in *Bolsa Família* decreased the probability that a woman's prenatal care visits were in consultation with a doctor, rather than a nurse or informal care provider (Table A61).

For pregnancies reported between the 2005 and 2009 survey rounds, there is no evidence for the full sample that *Bolsa Família* reduced the proportion of women who had no prenatal care visits (Table A62), increased the proportion of women who had at least six prenatal care visits (Table A63), or increased the probability that a woman's prenatal care visits were attended by a doctor (Table A64), if anything there is an imprecisely measured negative effect.

**Table A59: Impact of *Bolsa Família* on the proportion of women having no prenatal care visits, for women pregnant during the 2009 AIBF survey**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.029 | -0.033 |
| | (0.080) | (0.106) |
| Observations | 75 | 121 |
| Regional mean impacts (unconditioned) | | |
| North | -0.820 | -0.770 |
| | (0.210)*** | (0.228)*** |
| North-East | -0.862 | -0.892 |
| | (0.134)*** | (0.108)*** |
| South-East | 0.119 | 0.117 |
| | (0.125) | (0.092) |
| South | 0.000 | 0.000 |
| | (0.000) | (0.000) |
| Centre-West | 0.000 | 0.108 |
| | (0.000) | (0.113) |
| Observations | 78 | 125 |

**Table A60: Impact of *Bolsa Família* on the proportion of women receiving adequate prenatal care, for women pregnant during the 2009 AIBF survey**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.010 | 0.152 |
| | (0.176) | (0.125) |
| Observations | 119 | 207 |
| Overall, unconditional | 0.522 | 0.308 |
| | (0.136)*** | (0.153)** |
| Observations | 124 | 213 |
| Regional mean impacts (unconditioned) | | |
| North | -0.174 | -0.232 |
| | (0.261) | (0.278) |
| North-East | 0.727 | 0.479 |
| | (0.109)*** | (0.116)*** |
| South-East | 0.009 | 0.194 |
| | (0.364) | (0.257) |
| South | n/a | n/a |
| Centre-West | -0.524 | -0.350 |
| | (0.246)** | (0.182)* |
| Observations | 124 | 213 |

Note: Results for the South region are not available because of very small samples.

**Table A61: Impact of *Bolsa Família* on the proportion of women receiving prenatal care from a doctor rather than a nurse or informal care provider, for women pregnant during the 2009 AIBF survey**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.008 | 0.079 |
| | (0.137) | (0.122) |
| Observations | 119 | 207 |
| Regional mean impacts (unconditioned) | | |
| North | 0.550 | 0.393 |
| | (0.238)** | (0.218)* |
| North-East | 0.502 | 0.324 |
| | (0.173)*** | (0.120)*** |
| South-East | -0.051 | 0.115 |
| | (0.360) | (0.260) |
| South | -0.391 | -0.500 |
| | (0.310) | (0.298)* |
| Centre-West | -0.259 | -0.553 |
| | (0.333) | (0.227)** |
| Observations | 124 | 213 |

**Table A62: Impact of *Bolsa Família* on the proportion of women having no prenatal care visits, for women pregnant between the 2005 and 2009 AIBF surveys**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.001 | 0.019 |
| | (0.020) | (0.016) |
| Observations | 675 | 1015 |
| Regional mean impacts (unconditioned) | | |
| North | -0.047 | -0.031 |
| | (0.051) | (0.053) |
| North-East | 0.024 | 0.035 |
| | (0.025) | (0.021)* |
| South-East | -0.005 | 0.076 |
| | (0.016) | (0.058) |
| South | -0.003 | 0.004 |
| | (0.005) | (0.018) |
| Centre-West | 0.069 | 0.115 |
| | (0.079) | (0.075) |
| Observations | 695 | 1038 |

**Table A63: Impact of *Bolsa Família* on the proportion of women receiving adequate prenatal care, for women pregnant between the 2005 and 2009 AIBF surveys**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.064 | -0.026 |
| | (0.054) | (0.049) |
| Observations | 675 | 1015 |
| Regional mean impacts (unconditioned) | | |
| North | -0.069 | -0.168 |
| | (0.155) | (0.122) |
| North-East | 0.069 | -0.027 |
| | (0.118) | (0.092) |
| South-East | 0.170 | 0.227 |
| | (0.091)* | (0.165) |
| South | -0.090 | -0.148 |
| | (0.088) | (0.087)* |
| Centre-West | -0.177 | -0.179 |
| | (0.142) | (0.102)* |
| Observations | 695 | 1038 |

**Table A64: Impact of *Bolsa Família* on the proportion of women receiving prenatal care from a doctor rather than a nurse or informal care provider, for women pregnant between the 2005 and 2009 AIBF surveys**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.106 | -0.122 |
| | (0.063)* | (0.052)** |
| Observations | 789 | 1189 |
| Regional mean impacts (unconditioned) | | |
| North | 0.244 | 0.171 |
| | (0.149) | (0.144) |
| North-East | -0.210 | -0.214 |
| | (0.114)* | (0.115)* |
| South-East | 0.027 | -0.080 |
| | (0.063) | (0.070) |
| South | 0.043 | 0.094 |
| | (0.160) | (0.221) |
| Centre-West | -0.406 | -0.455 |
| | (0.191)** | (0.139)*** |
| Observations | 812 | 1216 |

### 2.2.2 Decisionmaking within the household

We consider the following outcomes related to women's decisionmaking power: the proportion of women who report being the sole decisionmaker on food purchases, the proportion of women who report being the sole decisionmaker on clothes for herself, the proportion of women who report being the sole decisionmaker on clothes for her children, the proportion of women who report being the sole decisionmaker on school expenditures, the proportion of women who report being the sole decisionmaker on medicine for children, the proportion of women who report being the sole decisionmaker on durable goods, the proportion of women who report being the sole decisionmaker on

labor, and the proportion of women who report being the sole decisionmaker on contraception (Tables A65 through A72).

We find strong positive impacts in the full sample on women's decisionmaking power related to medicine for children, durable goods, and contraception. Although several of these results are only weakly significant using propensity score weighting, we confirm that when we re-estimate impacts for these outcomes using the more computationally-intensive but lower-variance method of covariate matching, the impacts are significant at the 5 percent level.

**Table A65: Impact of *Bolsa Família* on women's decisionmaking power, food purchases, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.002 | 0.011 |
| | (0.046) | (0.038) |
| Observations | 2440 | 3663 |
| Regional mean impacts (unconditioned) | | |
| North | 0.139 | 0.067 |
| | (0.088) | (0.074) |
| North-East | -0.103 | -0.117 |
| | (0.085) | (0.074) |
| South-East | 0.105 | 0.101 |
| | (0.112) | (0.088) |
| South | 0.257* | 0.395** |
| | (0.164) | (0.141) |
| Centre-West | -0.094 | -0.205 |
| | (0.152) | (0.151) |
| Observations | 2497 | 3754 |

**Table A66: Impact of *Bolsa Família* on women's decisionmaking power, clothes for self, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.031 | 0.043 |
| | 0.046 | (0.039) |
| Observations | 2424 | 3636 |
| Regional mean impacts (unconditioned) | | |
| North | 0.176* | 0.132 |
| | (0.104) | (0.085) |
| North-East | -0.109 | -0.116* |
| | 0.077 | (0.060) |
| South-East | 0.112 | 0.071 |
| | (0.100) | (0.091) |
| South | 0.239 | 0.431** |
| | (0.163) | (0.131) |
| Centre-West | 0.101 | 0.100 |
| | (0.165) | (0.149) |
| Observations | 2480 | 3725 |

**Table A67: Impact of *Bolsa Família* on women's decisionmaking power, clothes for children, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|

| | | |
|---|---|---|
| Overall, conditional on baseline covariates | 0.044 | 0.033 |
| | (0.048) | (0.040) |
| Observations | 2035 | 3135 |
| Regional mean impacts (unconditioned) | | |
| North | 0.084 | 0.082 |
| | (0.096) | (0.080) |
| North-East | -0.010 | -0.055 |
| | (0.092) | (0.075) |
| South-East | 0.052 | -0.030 |
| | (0.108) | (0.094) |
| South | 0.317* | 0.525** |
| | (0.173) | (0.131) |
| Centre-West | 0.095 | -0.055 |
| | (0.191) | (0.177) |
| Observations | 2088 | 3217 |

**Table A68: Impact of *Bolsa Família* on women's decisionmaking power, school, 2009**

| | **Comparison 2** | **Comparison 3** |
|---|---|---|
| Overall, conditional on baseline covariates | 0.072 | 0.061 |
| | (0.045) | (0.038) |
| Observations | 1918 | 2937 |
| Regional mean impacts (unconditioned) | | |
| North | 0.100 | 0.024 |
| | (0.092) | (0.078) |
| North-East | 0.079 | 0.063 |
| | (0.104) | (0.089) |
| South-East | 0.114 | 0.071 |
| | (0.124) | (0.106) |
| South | 0.282 | 0.126 |
| | (0.180) | (0.121) |
| Centre-West | -0.019 | 0.005 |
| | (0.109) | (0.107) |
| Observations | 1968 | 3012 |

**Table A69: Impact of *Bolsa Família* on women's decisionmaking power, medicine for children, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.063 | 0.080** |
| | (0.046) | (0.038) |
| Observations | 2151 | 3292 |
| Regional mean impacts (unconditioned) | | |
| North | 0.142 | 0.082 |
| | (0.087) | (0.078) |
| North-East | 0.014 | -0.012 |
| | (0.090) | (0.074) |
| South-East | 0.101 | 0.114 |
| | (0.117) | (0.099) |
| South | 0.392** | 0.515** |
| | (0.163) | (0.128) |
| Centre-West | -0.168* | -0.129 |
| | (0.094) | (0.105) |
| Observations | 2208 | 3380 |

**Table A70: Impact of *Bolsa Família* on women's decisionmaking power, durable goods, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.079* | 0.053* |
| | (0.042) | (0.031) |
| Observations | 2409 | 3617 |
| Regional mean impacts (unconditioned) | | |
| North | 0.038 | -0.019 |
| | (0.085) | (0.071) |
| North-East | 0.050 | 0.028 |
| | (0.082) | (0.068) |
| South-East | 0.222** | 0.141* |
| | (0.108) | (0.083) |
| South | 0.071 | 0.078 |
| | (0.088) | (0.061) |
| Centre-West | -0.106 | -0.101 |
| | (0.073) | (0.082) |
| Observations | 2466 | 3709 |

**Table A71: Impact of *Bolsa Família* on women's decisionmaking power, labor, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.035 | 0.012 |
| | (0.045) | (0.038) |
| Observations | 2444 | 3650 |
| Regional mean impacts (unconditioned) | | |
| North | 0.037 | 0.076 |
| | (0.096) | (0.076) |
| North-East | 0.038 | -0.002 |
| | (0.081) | (0.071) |
| South-East | 0.009 | -0.012 |
| | (0.104) | (0.092) |
| South | 0.331** | 0.204* |
| | (0.151) | (0.107) |
| Centre-West | -0.233 | -0.291** |
| | (0.167) | (0.128) |
| Observations | 2501 | 3741 |

**Table A72: Impact of *Bolsa Família* on women's decisionmaking power, contraception, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.010** | 0.098** |
| | (0.045) | (0.036) |
| Observations | 2334 | 3509 |
| Regional mean impacts (unconditioned) | | |
| North | 0.109 | 0.085 |
| | (0.083) | (0.066) |
| North-East | 0.097 | 0.065 |
| | (0.086) | (0.074) |
| South-East | 0.134 | 0.071 |
| | (0.111) | (0.010) |
| South | 0.271* | 0.265** |
| | (0.162) | (0.123) |
| Centre-West | 0.102 | 0.091 |
| | (0.148) | (0.118) |
| Observations | 2391 | 3596 |

### 2.3 Household behavior and welfare

#### 2.3.1 Labor supply

We consider the following outcomes related to labor supply decisions: proportion of all individuals aged 18-55 that are currently working, proportion of individuals aged 18-55 in the labor force that are currently working, and proportion of all individuals aged 18-55 not working that have sought work in the past 30 days (Tables A73 through A75). Here, we define the "labor force" as all individuals aged 18-55 who either are currently working or are currently not working but have sought work in the past 30 days.

In general, there are very few significant impacts on these outcomes, either in the aggregate or by sex (Tables A76 through A81). Aggregating over males and females, there appear to be no significant

impacts on the proportion of all individuals aged 18-55 that is currently working, on the proportion of individuals aged 18-55 in the labor force that is currently working, and the proportion of individuals aged 18-55 not currently working that is seeking work. This tends to be the case disaggregated by region, as well. Only in the North, there appears to be a significant or weakly significant decrease in the proportion of individuals currently not working that have sought work in the past 30 days, by about -0.08 to-0.11 (depending on the use of Comparison 2 or 3).

Among males only, there tend to be no significant impacts on these outcomes. Only according to Comparison 2, there is a significant increase in the proportion of males currently working by about 0.06, as well as a significant increase in the proportion of the labor force currently working by about 0.06. In Comparison 3, these impacts are not significant. Among females only, there tend to be no significant impacts on proportion currently working or proportion of the labor force currently working. Only in the North-East, there appears to be a significant decrease in the proportion of females in the labor force currently working by about -0.09 to -0.13 (depending on the use of Comparison 2 or 3). There appears to be a weakly significant increase in the proportion of females that have sought work among those not currently working, by about 0.05 to 0.07, which appears driven by the North-East, where the increase is significant and roughly 0.09 to 0.11 (depending on the use of Comparison 2 or 3).

We also consider outcomes related to the contribution to household labor supply from different household members and the allocation of household labor supply to different sectors of the labor market: average total household weekly work hours among individuals aged 18-69, average formal household weekly work hours among individuals aged 18-69, average informal household weekly work hours among individuals aged 18-69, average total household weekly work hours among males aged 18-69, average total household weekly work hours among females aged 18-69, average formal household weekly work hours among males aged 18-69, average informal household weekly work hours among males aged 18-69, average formal household weekly work hours among females aged 18-69, average informal household weekly work hours among females aged 18-69, the proportion of total household weekly work hours among individuals aged 18-69 devoted to formal sector, and the proportion of total household weekly work hours among individuals aged 18-69 worked by females (Tables A82 through A91). Here, we define a job as being in the formal sector if the household member either has a “card” for that job or contributes to social security through that job.

Overall, there appears to be an insignificant impact on average total household weekly work hours among individuals aged 18-69 per individual aged 18-69. This is the case for both males and females. This is also the case when disaggregated by region, overall or by sex. However, there is a highly

significant decrease in average formal household weekly work hours among individuals aged 18-69 per individual aged 18-69, by roughly 8 hours. Results suggest this can be broken down into a significant decrease among males by roughly 4 hours, as well as a significant decrease among females by roughly 4 hours. Disaggregated by region, these results are significant in the North-East consistently across both Comparison 2 and Comparison 3, and also tend to be significant in the North, South, and South-East according to Comparison 2 and/or Comparison 3. In turn, there is a highly significant increase in average informal household weekly work hours among individuals aged 18-69 per individual aged 18-69, by roughly 8 hours. Results suggest this can be broken down into a significant increase among males by roughly 4 hours, as well as a significant increase among females by roughly 4 hours. Again, disaggregated by region, these results are significant in the North-East consistently across both Comparison 2 and Comparison 3, and also tend to be significant in the North, South, and South-East according to Comparison 2 and/or Comparison 3. Overall, there appears to be a highly significant decrease in the proportion of total household weekly work hours devoted to the formal sector, by roughly 0.2. Disaggregated by region, these results are significant in North, North-East, South-East, and South. Conditional on baseline covariates, there is an insignificant impact in the proportion of total household weekly work hours worked by females. This is also the case when disaggregated by region.

**Table A73: Impact of *Bolsa Família* on proportion of all individuals ages 18-55 that are currently working, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.009 | 0.001 |
| | (0.027) | (0.023) |
| Observations | 10426 | 15518 |
| Regional mean impacts (unconditioned) | | |
| North | -0.072 | -0.034 |
| | (0.049) | (0.042) |
| North-East | 0.020 | 0.014 |
| | (0.057) | (0.042) |
| South-East | 0.004 | 0.015 |
| | (0.055) | (0.045) |
| South | 0.039 | -0.016 |
| | (0.118) | (0.093) |
| Centre-West | 0.099 | 0.051 |
| | (0.089) | (0.083) |
| Observations | 10802 | 16045 |

**Table A74: Impact of *Bolsa Família* on proportion of individuals aged 18-55 in the labor force that are currently working, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.022 | 0.009 |
| | (0.031) | (0.021) |
| Observations | 6452 | 9505 |
| Regional mean impacts (unconditioned) | | |
| North | 0.054 | 0.054 |
| | (0.047) | (0.045) |
| North-East | 0.047 | 0.008 |
| | (0.089) | (0.050) |
| South-East | -0.001 | 0.013 |
| | (0.047) | (0.035) |
| South | 0.001 | -0.012 |
| | (0.115) | (0.071) |
| Centre-West | 0.043 | -0.017 |
| | (0.060) | (0.043) |
| Observations | 6672 | 9819 |

**Table A75: Impact of *Bolsa Família* on proportion of all individuals aged 18-55 not working that have sought work in the past 30 days, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.006 | 0.007 |
| | (0.043) | (0.032) |
| Observations | 4643 | 7022 |
| Regional mean impacts (unconditioned) | | |
| North | -0.111 | -0.084 |
| | (0.056) ** | (0.050) * |
| North-East | -0.044 | 0.007 |
| | (0.111) | (0.069) |
| South-East | 0.011 | -0.006 |
| | (0.070) | (0.051) |
| South | -0.033 | -0.019 |
| | (0.136) | (0.109) |
| Centre-West | -0.006 | 0.065 |
| | (0.096) | (0.067) |
| Observations | 4831 | 7271 |

**Table A76: Impact of *Bolsa Família* on proportion of all males aged 18-55 that are currently working, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.064 | 0.026 |
| | (0.034) * | (0.029) |
| Observations | 4781 | 7033 |
| Regional mean impacts (unconditioned) | | |
| North | -0.077 | -0.042 |
| | (0.072) | (0.060) |
| North-East | 0.125 | 0.073 |
| | (0.094) | (0.061) |
| South-East | 0.038 | 0.022 |
| | (0.061) | (0.050) |
| South | 0.107 | -0.041 |
| | (0.128) | (0.119) |
| Centre-West | 0.176 | 0.112 |
| | (0.072) ** | (0.091) |
| Observations | 4966 | 7274 |

**Table A77: Impact of *Bolsa Família* on proportion of males aged 18-55 in the labor force that are currently working, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.061 | 0.031 |
| | (0.031) ** | (0.024) |
| Observations | 3637 | 5333 |
| Regional mean impacts (unconditioned) | | |
| North | 0.038 | 0.017 |
| | (0.052) | (0.047) |
| North-East | 0.160 | 0.079 |
| | (0.125) | (0.078) |
| South-East | 0.034 | 0.015 |
| | (0.027) | (0.026) |
| South | 0.050 | -0.025 |
| | (0.105) | (0.062) |
| Centre-West | 0.101 | 0.022 |
| | (0.075) | (0.044) |
| Observations | 3757 | 5500 |

**Table A78: Impact of *Bolsa Família* on proportion of all males aged 18-55 not working that have sought work in the past 30 days, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.036 | -0.021 |
| | (0.054) | (0.042) |
| Observations | 1489 | 2205 |
| Regional mean impacts (unconditioned) | | |
| North | -0.119 | -0.053 |
| | (0.104) | (0.085) |
| North-East | -0.200 | -0.108 |
| | (0.161) | (0.136) |
| South-East | -0.073 | -0.028 |
| | (0.081) | (0.073) |
| South | -0.300 | -0.254 |
| | (0.194) | (0.174) |
| Centre-West | -0.142 | 0.067 |
| | (0.193) | (0.138) |
| Observations | 1557 | 2298 |

**Table A79: Impact of *Bolsa Família* on proportion of all females aged 18-55 that are currently working, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.011 | -0.006 |
| | (0.037) | (0.032) |
| Observations | 5628 | 8474 |
| Regional mean impacts (unconditioned) | | |
| North | -0.059 | -0.019 |
| | (0.058) | (0.050) |
| North-East | -0.038 | -0.023 |
| | (0.066) | (0.055) |
| South-East | -0.003 | 0.030 |
| | (0.078) | (0.060) |
| South | -0.010 | 0.002 |
| | (0.179) | (0.142) |
| Centre-West | 0.048 | -0.009 |
| | (0.116) | (0.127) |
| Observations | 5832 | 8760 |

**Table A80: Impact of *Bolsa Família* on proportion of females aged 18-55 in the labor force that are currently working, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.048 | -0.015 |
| | (0.044) | (0.030) |
| Observations | 2813 | 4165 |
| Regional mean impacts (unconditioned) | | |
| North | 0.072 | 0.104 |
| | (0.083) | (0.080) |
| North-East | -0.128 | -0.086 |
| | (0.050) ** | (0.039) ** |
| South-East | -0.033 | 0.022 |
| | (0.089) | (0.070) |
| South | -0.047 | 0.002 |
| | (0.192) | (0.120) |
| Centre-West | -0.039 | -0.071 |
| | (0.091) | (0.081) |
| Observations | 2913 | 4312 |

**Table A81: Impact of *Bolsa Família* on proportion of all females aged 18-55 not working that have sought work in the past 30 days, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.069 | 0.047 |
| | (0.039) * | (0.028) * |
| Observations | 3152 | 4810 |
| Regional mean impacts (unconditioned) | | |
| North | -0.105 | -0.099 |
| | (0.066) | (0.061) |
| North-East | 0.110 | 0.086 |
| | (0.045) ** | (0.037) ** |
| South-East | 0.048 | 0.005 |
| | (0.090) | (0.064) |
| South | 0.029 | 0.018 |
| | (0.158) | (0.127) |
| Centre-West | 0.063 | 0.074 |
| | (0.083) | (0.074) |
| Observations | 3272 | 4966 |

**Table A82: Impact of *Bolsa Família* on average total household weekly work hours among individuals aged 18-69, per individual aged 18-69, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.166 | -0.110 |
| | (1.146) | (0.949) |
| Observations | 3661 | 5391 |
| Regional mean impacts (unconditioned) | | |
| North | -2.402 | -3.383 |
| | (2.938) | (2.446) |
| North-East | 0.997 | 1.054 |
| | (2.070) | (1.625) |
| South-East | -0.739 | -1.513 |
| | (2.258) | (1.610) |
| South | -2.432 | -2.389 |
| | (2.575) | (2.427) |
| Centre-West | 1.468 | 0.909 |
| | (4.690) | (4.773) |
| Observations | 3782 | 5562 |

**Table A83: Impact of *Bolsa Família* on average formal household weekly work hours among individuals aged 18-69, per individual aged 18-69, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -7.980 | -8.869 |
| | (1.512) *** | (1.318) *** |
| Observations | 3661 | 5391 |
| Regional mean impacts (unconditioned) | | |
| North | -13.546 | -10.757 |
| | (3.828) *** | (3.901) *** |
| North-East | -8.169 | -8.037 |
| | (2.407) *** | (2.140) *** |
| South-East | -5.951 | -6.318 |
| | (3.190) * | (2.928) ** |
| South | -12.561 | -15.709 |
| | (6.543) * | (5.603) *** |
| Centre-West | -4.291 | -3.291 |
| | (5.833) | (6.084) |
| Observations | 3782 | 5562 |

**Table A84: Impact of *Bolsa Família* on average informal household weekly work hours among individuals aged 18-69, per individual aged 18-69, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 7.801 | 8.748 |
| | (1.710) *** | (1.429) *** |
| Observations | 3661 | 5391 |
| Regional mean impacts (unconditioned) | | |
| North | 11.826 | 7.350 |
| | (3.990) *** | (3.588) ** |
| North-East | 8.952 | 9.288 |
| | (2.976) *** | (2.519) *** |
| South-East | 5.064 | 4.775 |
| | (3.652) | (3.155) |
| South | 7.464 | 12.189 |
| | (6.130) | (5.289) ** |
| Centre-West | 6.664 | 4.464 |
| | (7.966) | (7.879) |
| Observations | 3782 | 5562 |

**Table A85: Impact of *Bolsa Família* on average total household weekly work hours among males aged 18-69, per male aged 18-69, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.785 | -0.384 |
| | (1.599) | (1.336) |
| Observations | 3432 | 5078 |
| Regional mean impacts (unconditioned) | | |
| North | -3.580 | -3.895 |
| | (4.403) | (3.776) |
| North-East | -1.133 | 0.667 |
| | (3.169) | (2.299) |
| South-East | -3.039 | -4.388 |
| | (2.907) | (2.380) * |
| South | 2.632 | 0.797 |
| | (7.555) | (6.095) |
| Centre-West | -3.077 | 1.425 |
| | (6.867) | (7.355) |
| Observations | 3542 | 5237 |

**Table A86: Impact of *Bolsa Família* on average total household weekly work hours among females aged 18-69, per female aged 18-69, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.304 | 0.244 |
| | (1.495) | (1.344) |
| Observations | 3410 | 5066 |
| Regional mean impacts (unconditioned) | | |
| North | -0.761 | -1.050 |
| | (4.242) | (3.865) |
| North-East | 1.239 | 0.044 |
| | (2.742) | (2.190) |
| South-East | 3.047 | 3.791 |
| | (3.765) | (2.812) |
| South | -4.415 | -2.891 |
| | (7.817) | (6.373) |
| Centre-West | 2.467 | -1.464 |
| | (6.094) | (7.427) |
| Observations | 3527 | 5232 |

**Table A87: Impact of *Bolsa Família* on average formal household weekly work hours among males aged 18-69, per male aged 18-69, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -4.605 | -4.697 |
| | (1.485) *** | (1.136) *** |
| Observations | 3432 | 5078 |
| Regional mean impacts (unconditioned) | | |
| North | -9.547 | -8.045 |
| | (3.717) ** | (3.738) * |
| North-East | -4.703 | -4.321 |
| | (2.630) * | (2.179) ** |
| South-East | -3.622 | -4.688 |
| | (3.248) | (2.841) * |
| South | 4.144 | 1.920 |
| | (4.582) | (3.158) |
| Centre-West | -7.170 | -6.456 |
| | (4.821) | (5.698) |
| Observations | 3542 | 5237 |

**Table A88: Impact of *Bolsa Família* on average informal household weekly work hours among males aged 18-69, per male aged 18-69, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 3.460 | 4.228 |
| | (1.483) ** | (1.163) *** |
| Observations | 3432 | 5078 |
| Regional mean impacts (unconditioned) | | |
| North | 6.754 | 4.186 |
| | (4.191) | (3.433) |
| North-East | 3.273 | 4.989 |
| | (2.578) | (1.982) ** |
| South-East | 0.547 | 0.355 |
| | (3.424) | (3.320) |
| South | -4.454 | -2.432 |
| | (4.711) | (4.344) |
| Centre-West | 5.090 | 8.508 |
| | (8.143) | (5.579) |
| Observations | 3542 | 5237 |

**Table A89: Impact of *Bolsa Família* on average formal household weekly work hours among females aged 18-69, per female aged 18-69, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -4.142 | -4.627 |
| | (1.209) | (1.093) *** |
| Observations | 3410 | 5066 |
| Regional mean impacts (unconditioned) | | |
| North | -5.496 | -3.889 |
| | (2.774) ** | (2.942) |
| North-East | -4.187 | -4.208 |
| | (1.673) ** | (1.570) *** |
| South-East | -2.240 | -1.526 |
| | (1.797) | (1.381) |
| South | -17.524 | -18.307 |
| | (7.220) ** | (5.958) *** |
| Centre-West | 1.830 | 2.760 |
| | (5.133) | (3.897) |
| Observations | 3527 | 5232 |

**Table A90: Impact of *Bolsa Família* on average informal household weekly work hours among females aged 18-69, per female aged 18-69, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 4.459 | 4.937 |
| | (1.296) *** | (1.238) *** |
| Observations | 3410 | 5066 |
| Regional mean impacts (unconditioned) | | |
| North | 4.674 | 2.783 |
| | (3.670) | (3.282) |
| North-East | 5.511 | 4.463 |
| | (2.091) *** | (1.775) ** |
| South-East | 5.167 | 5.234 |
| | (4.086) | (2.831) * |
| South | 13.311 | 15.557 |
| | (5.149) *** | (4.416) *** |
| Centre-West | 0.602 | -4.574 |
| | (4.396) | (7.487) |
| Observations | 3527 | 5232 |

**Table A91: Impact of *Bolsa Família* on proportion of total household weekly work hours among individuals aged 18-69 devoted to formal sector, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | -0.198 | -0.219 |
| | (0.036) *** | (0.031) *** |
| Observations | 3616 | 5324 |
| Regional mean impacts (unconditioned) | | |
| North | -0.217 | -0.144 |
| | (0.076) *** | (0.071) ** |
| North-East | -0.220 | -0.218 |
| | (0.057) *** | (0.053) *** |
| South-East | -0.146 | -0.140 |
| | (0.069) ** | (0.063) ** |
| South | -0.278 | -0.358 |
| | (0.168) * | (0.143) ** |
| Centre-West | -0.105 | -0.103 |
| | (0.148) | (0.152) |
| Observations | 3734 | 5492 |

**Table A92: Impact of *Bolsa Família* on proportion of total household weekly work hours among individuals aged 18-69 worked by females, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.021 | 0.025 |
| | (0.034) | (0.029) |
| Observations | 5129 | 7563 |
| Regional mean impacts (unconditioned) | | |
| North | -0.023 | -0.006 |
| | (0.056) | (0.050) |
| North-East | 0.055 | 0.033 |
| | (0.050) | (0.040) |
| South-East | 0.044 | 0.075 |
| | (0.062) | (0.046) |
| South | -0.133 | -0.060 |
| | (0.174) | (0.153) |
| Centre-West | 0.120 | 0.004 |
| | (0.097) | (0.119) |
| Observations | 5288 | 7802 |

### 2.3.2 Social capital

We consider the following outcomes related to social capital: whether the household has membership in any group and whether the household has membership in any group excluding church (Tables A93 through A96). Single-difference estimates using only 2009 data show no strongly significant impacts on these social capital outcomes. Double-difference estimates for all groups using 2005 and 2009 data show a small positive impact but this does not persist when church-based groups are excluded.

**Table A93: Impact of *Bolsa Família* on membership in any group, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.041 | 0.031 |
| | (0.033) | (0.025) |
| Observations | 5112 | 7541 |
| Regional mean impacts (unconditioned) | | |
| North | 0.014 | -0.004 |
| | (0.061) | (0.051) |
| North-East | 0.035 | 0.031 |
| | (0.060) | (0.046) |
| South-East | -0.034 | 0.006 |
| | (0.064) | (0.050) |
| South | 0.317** | 0.203** |
| | (0.113) | (0.083) |
| Centre-West | -0.052 | 0.003 |
| | (0.049) | (0.043) |
| Observations | 5238 | 7724 |

**Table A94: Impact of *Bolsa Família* on change in membership in any group (Difference-in-Difference)**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.083** | 0.050* |
| | (0.040) | (0.033) |
| Observations | 5238 | 7541 |
| Regional mean impacts (unconditioned) | | |
| North | 0.017 | -0.036 |
| | (0.083) | (0.069) |
| North-East | 0.086 | 0.036 |
| | (0.069) | (0.051) |
| South-East | 0.004 | 0.020 |
| | (0.082) | (0.073) |
| South | 0.264** | 0.185* |
| | (0.102) | (0.097) |
| Centre-West | 0.089 | 0.096 |
| | (0.115) | (0.115) |
| Observations | 5112 | 7724 |

**Table A95: Impact of *Bolsa Família* on membership in any group excluding church, 2009**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.020 | 0.019 |
| | (0.025) | (0.022) |
| Observations | 5112 | 7541 |
| Regional mean impacts (unconditioned) | | |
| North | -0.046 | -0.027 |
| | (0.047) | (0.039) |
| North-East | 0.029 | 0.022 |
| | (0.040) | (0.038) |
| South-East | 0.003 | 0.003 |
| | (0.065) | (0.047) |
| South | 0.166* | 0.130** |
| | (0.084) | (0.056) |
| Centre-West | -0.067 | -0.027 |
| | (0.035) | (0.025) |
| Observations | 5238 | 7724 |

**Table A96: Impact of *Bolsa Família* on change in membership in any group excluding church (Difference-in-Difference)**

| | Comparison 2 | Comparison 3 |
|---|---|---|
| Overall, conditional on baseline covariates | 0.026 | 0.024 |
| | (0.030) | (0.026) |
| Observations | 5112 | 7541 |
| Regional mean impacts (unconditioned) | | |
| North | -0.040 | -0.041 |
| | (0.062) | (0.052) |
| North-East | 0.075 | 0.038 |
| | (0.046) | (0.041) |
| South-East | -0.015 | -0.014 |
| | (0.073) | (0.053) |
| South | 0.048 | 0.062 |
| | (0.077) | (0.067) |
| Centre-West | -0.026 | 0.066 |
| | (0.077) | (0.112) |
| Observations | 5238 | 7724 |

## References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