Gender Bias in Intrahousehold Allocation: Evidence from an Unintentional Experiment*

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Abstract

We use data from a Brazilian social program to investigate the existence of gender bias in intrahousehold allocation of resources. The program was designed to make cash transfers to mothers and pregnant women in the poorest households of Brazil. Bureaucratic mistakes—beyond the control of the applicants—have accidentally excluded many households who actually applied and were accepted to the program. These unintentional exclusions formed a control group in the molds of random experiments. This is used to identify the impact of an exogenous variation in female nonlabor income on household consumption. The results do not support the growing belief that empowered women divert household expenditures towards pro-child and pro-family goods.

Keywords: *Bolsa Alimentação*, Gender Bias, Collective Approach, Household Bargain, Unitary Model. JEL Classification: D12, J16, O54, H31.

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1 Introduction

Conditional Cash Transfer (CCT) programs have become pervasive in Latin America. The Brazilian CCT policy is the largest in the region and covers about one fifth of the country's population.[1] The general perception in the society is that it has strongly benefited the poor. A common feature of CCT programs is that the transfer is always made to a woman. This design hinges on the growing belief that women present expenditure patterns that are more pro-child and pro-family than men. Therefore, investigating the empirical validity of this belief is of great relevance for the design and implementation of social policies.

Studying gender bias in intrahousehold allocation of resources is also important from a theoretical perspective, as it allows us to confront alternative theories of household behavior. On the one hand, the unitary model of household assumes that preferences of different members can be represented by a single well-behaved utility function. In this case, an exogenous redistribution of nonlabor income across members should have no effect on household expenditures. On the other hand, different models suggest that an exogenous variation in the fraction of nonlabor income accruing to women should affect the bargaining power inside the household, changing household expenditures towards goods that are typically preferred by women. The empirical analysis carried in this paper confronts these alternative predictions for a sample of poor households in a developing social environment.

We use a special data set collected in 2002, in the Northeast of Brazil, with the goal of evaluating the impact of one of Brazil's first CCT programs, the *Bolsa Alimentação* (henceforth, BA). The BA program aimed at reducing infant mortality and nutritional deficiencies among children from very poor families. It started in 2001 and consisted of cash transfers to pregnant women and mothers of children younger than 7 years old. The program commanded compliance with vaccination schedules and regular attendance at prenatal care, child growth monitoring, and health education classes. The transfers were always made to the highest raking woman in the household, usually the mother of all children in the family. Payments were conducted by a large federal bank called *Caixa Econômica Federal* (henceforth, CEF). The CEF has issued a personalized electronic card (similar to an ATM card) to each beneficiary woman.

The amount transferred averaged approximately 8% of the household total ex-

[1] See Lindert, Skoufias, and Shapiro (2006).

penditure. Thus, participation in the program did represent a significant exogenous variation in female nonlabor income. This variation is likely to be perceived as long termed, since the beneficiary families knew that they were likely to continue to receive additional CCTs from the follow up program, the *Bolsa Escola* (BE), which was targeted to kids between 7 and 14 years old and was conditioned on school attendance.[2] Note however that even a transitory shock to female income could affect intrahousehold bargaining, since it is reasonable to suppose that poor Brazilian families are credit constrained (which typically results in anxious consumption behavior). Therefore, if preferences did present a gender-specific component and if intrahousehold decision-making power were affected by exogenous changes in the member's income share, then participation in the BA program should affect resource allocation in households with male and female members, net of pure income effects.

The uniqueness of the data lies in the fact that a group of eligible households—who actually applied and were accepted to the program—were randomly and unintentionally excluded from it. The exclusion of eligible households occurred due to three independent reasons, all of them beyond the control of the applicants and unrelated to household unobserved characteristics.

First, some files containing household-identifying information were lost during electronic transmission via the internet to the bank responsible for the payments (the CEF). This type of exclusion was due to local network problems and then, for each municipality, it is independent of household characteristics.

The second source of exclusion is somewhat exotic. While the software used by the municipal authorities in charge of program registration was adapted to the Portuguese language, the software used in the federal capital by the CEF bank for issuing the beneficiary identification number and processing payments was not. In fact, writing names in capital letters and without accent marks is a convention in the Brazilian banking system. Thus, because the CEF software was not able to read special characters (such as ç, ˜, ´, and ˆ), households in which at least one member had any special character in the name did not receive an identification number and were not included in the program roster for some time. These characters are commonly found in Brazilian names (e.g., João, José, Ângela, Andréa, Tânia, Mônica) and surnames (e.g., Aragão, Gonçalves, Magalhães, Mendonça, Simões). Furthermore, these names and surnames are homogeneously distributed across the population and are not linked to specific ethnic, gender, age, or income groups.

[2] These two programs, the BA and BE, were unified in 2003 and are now called *Bolsa Família*.

Hence, this second exclusion criterion is also exogenous, conditional on the number of members in the household.[3]

The third source of exclusion is related to misspelling problems. Many households that applied and were eligible for the BA program had children at schooling age and were enrolled in the *Bolsa Escola* (BE) program. The federal bank responsible for the transfers of both programs, the CEF, decided that it would issue only one identification number for each family, hoping that in the future this would become a single social security number to be used by all federal programs. Therefore, the CEF blocked the registration of households whose data arriving from the BA registration showed any inconsistency with the information recorded during the earlier BE registration. For instance, if during the BA registration a household member's name was spelled differently from what had been recorded in the BE registration, the entry in the BA program was frozen until this inconsistency was clarified. Since the electronic records in both programs were filled by staff members, this third type of exclusion is related to administrative problems beyond the control of the applicants. It is thus exogenous, conditional on the number of household members, the municipality where registration was conducted, and previous enrollment status in the BE program.

The analysis of the data is organized in the following manner. We start by reviewing the related literature in Section 2. We point out that our treatment and control households face the same relative prices, since they live in the same municipality and their choices are observed at the same period. This represents an important advantage over traditional randomizations—in which the program implementation is delayed in some randomly chosen locations—and over quasi-experimental before-after analyses.

The survey design and some descriptive statistics are presented in Section 3. Next, in Section 4, we discuss the econometric conditions under which OLS regressions consistently identify the average treatment effect of the BA program. We also show that, when compared to nonbeneficiaries, BA participants expend proportionally less on utilities and more on vegetables and fruits. We find no statistically significant impacts of the program on the expenditure shares of dairy, family goods (such as health, schooling, clothes, personal hygiene, house-cleaning products, home

[3]It will be necessary to control for the household population in the econometric estimations to be carried out, because larger households are more likely to have at least one name with special characters, and household size might be correlated to the outcome variables of interest.

maintenance, furniture, and maids), and vices (alcohol, tobacco, and gambling). Therefore, if women indeed prefer more pro-child and pro-family goods, then our empirical evidence does not support intrahousehold bargaining models of resource allocation.

The relative increase in the consumption of vegetables and fruits could result from three different sources: (i) a nonlinear income effect resulted from the program transfer; (ii) a health-monitoring effect resulted from the fact that BA participants committed to regular attendance at public health centers; and (iii) a bargaining effect resulted from the fact that the program transfer was always made to a woman. In Section 5, we explore the family structure of our sampled households in order to disentangle bargaining effects from the other two possible effects of the program.

Among the 1,006 surveyed households, there are 77 families with no male adult. Most of these families are composed of single mothers living with their children. There are also 507 households in which the eligible woman has some extra source of personal income different from the BA transfer. Gender-specific bargaining effects must be absent within single-mother households. Moreover, it is likely that beneficiary women with some extra source of personal income have experienced a differentiated change in their ability to re-allocate expenditures. We then use a difference-in-difference model to measure and test the importance of intrahousehold bargaining.

This diff-in-diff strategy is valid under the following two identification assumptions: (a) the ability to re-allocate expenditures after receiving an exogenous cash transfer varies across households with more independent women and those in which the eligible woman had no other source of personal income; (b) participation into the BA program did not affect the fraction of single-mother households and the fraction of households whose women had an extra source of personal income. Naturally, the BA program could have affected the family structure through changes in divorces and female labor supply.[4] This could potentially challenge our second identification assumption. We show however that the conditional correlation between BA participation and the family structure is not statistically significant. Fortunately, it seems that those potential changes have not occurred during the first six month of the program (when our data were collected).

Our difff-in-diff results—presented in Section 5.2—do not support the thesis

[4]See Rangel (2006) for evidence about changes in female labor supply after a change in alimony rights and obligations to cohabiting couples in Brazil.

of gender-specific bargaining effects over consumption shares. BA participants (in the two household categories) have reduced the share expended with utilities, have increased their expenditure shares of vegetables and fruits, and have not changed their consumption shares of dairy, family goods, and vices. The BA program did impact household expenditures, but the average impact is identical across households in which the eligible woman had some or no extra source of personal earnings.

This same conclusion is obtained when: (i) we exclude households previously enrolled in the BE program from the sample; (ii) we exclude single-mother households from the sample; (iii) we define independent women as those earning the highest income in the household (as opposed to having some extra source of personal income). Additional robustness exercises are mentioned in Section 6, and a brief conclusion appears in Section 7.

2 Literature Review

We review some empirical tests for income pooling between husbands and wives.[5] We first present papers that use reduced-form econometric models and conventional expenditure data. Next, we discuss a work that derives and tests predictions from a structural model of household behavior. Finally, we comment on papers which, similarly to ours, rely on natural experiments and quasi-experiments.[6]

2.1 Reduced-Form Estimates

Most of the existing literature uses reduced-form models to estimate the effect of women leadership on household-choice variables. Handa (1996) presents evidence that female-headed households in Jamaica dedicate a greater budget share to child clothes, health, and food goods, while male-headed households spend more with alcohol and tobacco. Thomas (1990) uses a household survey from Brazil (*Estudo Nacional de Despesa Familiar*) to show that nonlabor income in the hands of women have a greater positive effect on children's anthropometrics than nonlabor income in the hands of men. Thomas (1994), using data from the United States, Brazil,

[5] For studies on households consisting of parents and their children or grandchildren, see Pezzin and Schone (1997), Costa (1997), Duflo (2000, 2003), and Edmonds, Mammen, and Miller (2005).

[6] We focus on papers analyzing policies that increased the amount of monetary income received by women. There is another important policy debate on whether social programs target to children should make monetary or in-kind transfers. Bingley and Walker (2009) use quasi-experimental evidence from the UK to argue that those two alternatives are equivalent.

and Ghana, shows that the mother's education level tends to have a stronger effect on the girl's height (relatively to boy's), while the father's education has a larger impact on the boy's height. Thomas, Contreras, and Frankenberg (2002) use data from the Indonesia Family Survey to study how child morbidities (such as diarrhea, coughs, and fever) are affected by the value of assets that wives and husbands bring to marriage (an indicator of economic independence). The results suggest that the unitary model performs well for most Indonesian regions, but not for Java and Sumatra.

In principle, none of the results fully above support the unitary model of household behavior. However, endogeneity of the leadership variables (e.g., headship, income share, education, and asset ownership) potentially bias the results. For instance, unobserved characteristics of the couple could be behind both leadership and the household wiliness to divert resources towards children-assignable goods.

2.2 Structural Estimates

Browning, Bourguignon, Chiappori, and Lechene (1994) model household decisions under the assumption that the interactions among household members with different preferences would lead to Pareto efficient outcomes. The parameters of the structural model are estimated using Canadian data on couples with no children. The main goal of the analysis is to investigate how final outcomes depend on the income each member brings into the household. They also compare expenditure behavior in single-person households with that of couples. They find that older and higher-income partners are able to divert a higher share of total household expenditure towards their own consumption. Besides, holding age differences and relative income shares constant, women are able to divert more income towards her own consumption in wealthier households.

2.3 Natural Experiments and Quasi-Experiments

We are aware of the following works which, similarly to ours, use natural experiments and quasi-experiments to test the incoming-pooling hypothesis. Attanasio and Lechene (2002) examine the effects of participation in a cash transfer program, the PROGRESSA, which started in 1998 in rural Mexico. The transfer is conditional on school enrollment and the mother is always the recipient. The program only started one and a half years later for a number of randomly selected villages that form the control group. The authors show that self-reported decision-making

power varies across recipients, non-recipients, and future recipients in the control villages. They also perform a more conventional test using data on expenditure shares. Eight types of non-durable expenditures are considered: food, alcohol and tobacco, transportation, services, and clothing for women, men, girls, and boys. The results indicate a positive impact of female income on girls' and boys' clothing and a negative impact on alcohol.

Lundberg, Pollak, and Wales (1997) use the 1979 reform in the UK family allowance policy. This reform replaced a tax allowance program by a nontaxable payment made directly to mothers. To the extent that tax allowances benefited mainly the fathers, this reform transferred a substantial amount of income to the hands of the mothers. The data include the average consumption of clothes for different family categories (defined according to income and number of children). They use the period 1973-1976 to represent the consumption regime before the policy change, and the period 1980-1990 to represent the regime after the reform. Comparing weighted averages, they find that the ratio of child/male and female/male clothing expenditures increased in the households benefited by the program. This analysis was limited to clothing expenditures and do not take into account eventual changes in relative prices (as well as other possible changes in the economic environment) over the years studied. In fact, by analyzing this tax reform, Hotchkiss (2005) found similar changes in expenditure patterns of families with no children and raised alternative explanations for the Lundberg, Pollak, and Wales' results.

Ward-Batts (2003) uses this same data source disaggregated to the household level and test the effect of the 1979 reform in the UK family allowance policy on household expenditure shares. The periods 1973-1976 and 1980-1983 represent the regimes before and after the reform. She uses data on households with: (i) one man; (ii) one woman aged less than 60 years and recorded as the wife of the household head; and (iii) one to three children (aged less than 18 years). She ends up with a sample consisting of 18,810 households over the 8 years studied in the paper. A demand system is estimated using data on consumption expenditures, a price index for each category of goods, and household characteristics. The demand curves of beneficiary households shifted up for children-assignable goods (such as clothes and toys) and shifted down for men's clothing and tobacco.

A different piece of evidence is presented by Bradbury (2004) who explores a similar policy change taken place in Australia. During the early 1990's, different reforms entitled the wives in married-couple households to be the primary receiver of

many social assistance benefits. (Before, Australian income support transfers were typically made to the husbands.) Three samples from the Australian Household Expenditure Survey were used. Those samples have been collected in 1988-89 (before any policy change), 1993-94 (when part of the reform had already taken place), and 1998-99 (after the last policy change). The results indicate that the within-household income redistribution caused a very small effect on expenditure patterns. The few significant estimates suggests an increase in the consumption of goods typically preferred by men (such as alcohol and tobacco) and a decrease in consumption of goods typically regarded as feminine (such as food at home).

Our work has one important advantage over the exercises in the experimental literature. When compared to Attanasio and Lechene (2002), our treated and non-treated households live in the same municipality. Moreover, different from the before-after exercises in Lundberg, Pollak, and Wales (1997), Ward-Batts (2003), and Bradbury (2004), we analyze treated and non-treated households during the same period. These two features allow us to avoid concerns about eventual changes in relative prices across municipalities or over time.

3 Data

We use data from a survey conducted by the International Food Policy Research Institute (IFPRI). This research was contracted by the Inter-American Development Bank (IDB) and the Brazilian Ministry of Health in order to evaluate the impact of the BA program on several nutritional and health outcomes.

The BA program consisted of cash transfers to low-income families with pregnant women or mothers of children younger than 7 years old. It started in 2001 and eligible households were those with estimated monthly per capita income below 90 BRL (equivalent to about 37.50 US dollars in 2001 values). The highest ranking woman in the household—typically the mother of all children—was the sole recipient of this cash transfer. The values transferred could be 15, 30, or 45 BRL per month (i.e., something between 6.25 and 18.75 US dollars). The exact value depended on the number of qualifying children.

The transfers were conditional on women committing to a 'Charter of Responsibilities' that commanded compliance with vaccination schedules and regular attendance at prenatal care, child growth monitoring, and health education classes. Eligibility for the BA expired when children completed 7 years of age. Poor families

with children between 7 to 14 years old would then become eligible for the *Bolsa Escola* (BE) program, which ensured continued cash transfer of the same amount as the BA program, but conditional on school attendance. As in the BA program, the transfer recipient was also the highest ranking woman in the household.

The special feature of the data is the existence of a control group formed by households who had actually applied for the program and were eligible to benefit from it, but were unintentionally excluded. Three types of accidental exclusion were detected: (i) some household files were lost during the electronic transmission to the CEF—the national bank responsible for issuing identification numbers and transferring the payments; (ii) the computer program used by the CEF was not adapted for the Portuguese language and then excluded households in which one or more members had special characters in their names (e.g., José, Ângela, and Assunção); and (iii) some households previously enrolled in the BE program were excluded from the BA program because the CEF blocked the registration of those whose data coming from the BA registration showed any inconsistency with the data previously recorded for the BE registration.

For each municipality, the first type of exclusion is completely exogenous. The second type of random exclusion, however, is only conditionally exogenous. That is, it is exogenous conditional on the number of household members, since larger households were more likely to have at least one name with a special character. Likewise, the third type of exclusion is exogenous conditional on the number of household members, the municipality where registration was conducted, and previous enrollment status in the BE program.

Accidentally excluded households were aware that registration did not guarantee participation, since cuts were expected to be made depending on the program budget allocated to each municipality. The municipality authorities were asked to register considerably more than the specified quotas because many of the registered households were likely to be found not eligible if their per capita incomes were estimated by the program administration to be above the cutoff level. In addition, households were not able to communicate directly with the CEF. Therefore, it is plausible that households typically interpreted these accidental exclusions as being caused by the lack of eligibility and did not act on it.

When the surveys were finally fielded, around 2% of the households in the sample that had been accidentally excluded had already been reincluded into the program and have reported receiving transfers. It is likely that these few reinclusions are

independent of household characteristics as they were conducted by the CEF staff who did not communicate directly with the households. This issue is analyzed in Section 4.1.

3.1 Survey Design

Excluded households were found in 67 Brazilian municipalities. The following criteria were established for the selection of the municipalities: only municipalities from the Northeastern region, participating in the program for at least six months, and with at least 40 excluded families were to be included in the evaluation study. These criteria were used for cost saving reasons. It is worth mentioning that 60% of the program beneficiaries resided in the Northeaster region of Brazil.

In April of 2002, when the survey team went to the field, there were four municipalities that fit those two criteria: *Teotônio Villela*, in the state of *Alagoas*; *Mossoró*, in the state of *Rio Grande do Norte*; *Itabuna* and *Teixeira de Freitas* in the state of *Bahia*. In terms of the number of randomly excluded households, *Teixeira de Freitas*, with 240 exclusions, was the municipality with the largest sample, followed by *Mossoró* with 116 excluded applicants, *Itabuna* with 87, and *Teotônio Vilela* with 63.

All excluded households were surveyed and a sample of matching households was selected from the roster of receiving beneficiaries. To increase the power of the statistical tests, two beneficiaries were matched to each excluded household. The matching criteria used were the following: (i) residence in the same municipality; (ii) same gender and age for the eligible woman and children; (iii) similar socioeconomic characteristics. After a pool of included and excluded households was matched by criteria (i) and (ii), the data collected during the registration process were used to further improving the matching in terms of socioeconomic characteristics (criterion iii). The variables used were: declared per-capita income, number of household members, rent paid, and the value of water, electricity, and gas expenses (all variables collected before the BA program had started). Morris et al. (2004) and IFPRI (2006) present further details on the matching mechanism employed.

Naturally, most socioeconomic characteristics must be balanced across BA participants and nonparticipants, since they have been used in the survey's matching criteria. We check the robustness of the survey design by regressing the BA participation dummy on different information about the education of the eligible woman. This exercise is interesting because these educational variables have not been em-

ployed in the matching mechanism.

Recall that the exclusion criteria are random conditional on the number of household members, the municipality where registration was conducted, and previous enrollment status in the BE program. Therefore, these variables are included in our regressions. (See Section 4 for a formal description of this procedure.) The results are presented in Table 1. Consistently with our randomization argument, the reported educational characteristics of the eligible woman are all uncorrelated to BA participation.

[Table 1]

3.2 Summary Statistics

The database contains detailed information on household consumption.[7] Non-food items are surveyed in Schedules 1 and 5, and food items are surveyed in Schedule 6. We aggregate these items in the following nine expenditure groups:

- General Services: water, telephone, gas, electricity, gasoline, auto services, taxes, pensions, lawyers, home insurance, mobile phone, weddings, donations, and funerals;
- Family Expenses: health, schooling, clothes, personal hygiene, house-cleaning products, home maintenance, furniture, and maids;
- Vices: alcoholic beverages, tobacco, and gambling;
- Grains: schedule 6, questions 1-12;
- Vegetables: schedule 6, questions 13-27;
- Fruits: schedule 6, questions 28-40;
- Dairy: schedule 6, questions 41-50;
- Meat: schedule 6, questions 51-63;
- Oils, Spices, and Soft Drinks: schedule 6, questions 64-74 and 77-81.[8]

[7] The data also display inputted values for rent. However, we fear this variable is poorly measured and decided not to use it.

[8] Questions 75 and 76 refer to beer and other alcoholic beverages that have been included in Vices.

The Household Monthly Expenditure, defined as the sum of all those expenditures, is used to construct the expenditure shares. Other variables such as the number of household members, a dummy variable describing whether the household were already enrolled on the BE program, and dummies for each municipality are also used in our analysis. Their summary statistics are presented in Table 2. The average fraction expended in each group ranges from 1.6% for vices to 20.1% for grains. (It is probably worth mentioning that rice and beans are very common in the Brazilian diet.) About 52% of households report no consumption of vices. The fractions of zero consumption are reasonable for the remaining eight expenditure categories.

[Table 2]

The next table presents the sample means of each variable for included and excluded households. It is divided in two blocks according to BE enrollment status. Households previously enrolled in the BE program must have at least one child at schooling age; they present higher total expenditure and are slightly more populated.

[Table 3]

Finally, it is important to mention that data display 77 (out of 1,006) households with no male older than 14 years old. These households are typically composed of single mothers living with their children. They deserve special attention since there is no bargaining effect on their choices. Unfortunately, the number of those households is very small. There are also 507 households in which the eligible woman has some extra source of personal income different from the BA transfer. The subsample of those households—single mothers and more independent women—contains 580 observations and will be used in Section 5 to disentangle bargaining from other effects of the BA program.

4 Identifying the Average Treatment Effect

Consider a scenario in which one is interested in identifying the average effect of the BA program over some arbitrary variable Y. If participation into the program were exogenously assigned, an OLS regression of Y against a constant and a dummy variable describing BA participation would consistently identify the average effect of

the program over Y. However, since BA participation was accidental, identification cannot be taken for granted.

Formally, consider a regression model:

$$Y = \alpha_0 + \alpha_1 BA + \varepsilon, \tag{1}$$

where α_0 and α_1 are parameters, BA is a dummy variable that equals 1 for beneficiary households, and ε is an arbitrary error term.

In this environment, one cannot guarantee that the error term ε is orthogonal to the BA dummy. There were three sources of accidental exclusions: (i) data loss during electronic transmission; (ii) special characters in the name of some household member; and (iii) inconsistency with previously recorded data, for households already registered in another federal program (the BE program). The first type of exclusion is related to network problems, and thus independent of household characteristics for each municipality. The second type of exclusion is affected by the number of members in the household—the larger the household, the more likely to have a name with some special character. Similarly, the third type of exclusion is only exogenous once we control for the number of household members, the municipality where registration was conducted, and the household enrollment status in the BE program.

Let Z be a vector containing variables describing the number of household members, municipality, and participation in the BE program. According to the description of the accidental exclusions, the BA dummy is exogenous conditional on Z. Then, define F as the distribution function describing the exclusion process, namely:

$$\Pr\left(BA = 1 \mid Z\right) = F\left(Z \cdot \theta\right); \tag{2}$$

and assume for parsimony that F is represented by the linear probability model:

$$BA = Z \cdot \theta + u. \tag{3}$$

Given the features of the exclusion process, the term u is fully exogenous, and then orthogonal to Z and all other household characteristics embedded in ε.

Now, consider the linear projection of ε on Z:

$$L\left(\varepsilon \mid Z\right) = Z \cdot \theta, \tag{4}$$

and rewrite equation (1) as:

$$Y = \alpha_0 + \alpha_1 BA + Z \cdot \theta + v, \tag{5}$$

where $v = \varepsilon - L(\varepsilon \mid Z)$.

By definition of linear projection, v is orthogonal to Z. Since u is exogenous, we have that BA is also orthogonal to v. Hence, the OLS estimator consistently identifies the parameters of equation (5).

Table 4 presents the regressions when the dependent variable is the household expenditure in each good category. Notice that, in this case, the municipality dummies also account for different relative prices faced by the families living in different cities. On average, BA participants expend proportionally more on vegetables and fruits, and less on general services (such as utilities), as compared to nonparticipants. These results are not in line with some findings in the literature—see Section 2—in which the empowered women reduce the consumption of vices and increase the consumption of child clothes and other family goods.

[Table 4]

4.1 Reincluded Households

As previously mentioned in Section3, the accidental exclusions were not immediately detected by program managers. However, once an error was detected and fixed by the CEF bank, the household were reincluded into the program. When the survey team went to the field to conduct the interviews, less than 2% of the originally excluded households had been reincluded into the BA program. Since the reinclusions were conducted by the CEF staff without any influence from the municipality authorities and households themselves, they are also likely to be conditionally independent of household observed and unobserved characteristics. Nevertheless, potential endogeneity associated with these reincluded households are considered here.

Table 5 reproduces the regressions in Table 4 by using the Instrumental Variable Method, where the BA status when the program started (i.e., before reinclusions) is the instrumental variable for BA participation when the data were collected (i.e., six month after the program had started). The correlation between instrumental and instrumented variables is 84.99%.[9] The results are qualitatively identical to those from OLS regressions.

[9] Besides the 19 re-included households, there is also other 44 households who were scheduled to receive the BA transfer when the program started but were not receiving it when the data were collected.

[Table 5]

5 Underlying Theory and Empirical Strategy

The theory of household expenditure behavior can be divided in two broad groups: one predicting that households behave as if they maximized a well defined objective function that only depends on the amount of goods consumed (the unitary approach); and another predicting that the distribution of nonlabor earnings across household members would affect the final allocation (the collective approach). We briefly describe them here.

The Unitary Approach

Consider a household composed of a male and a female member earning (respectively) $y_m \geq 0$ and $y_f \geq 0$, and consuming L different goods represented by $x_m \in \mathbb{R}^L_+$ and $x_f \in \mathbb{R}^L_+$. Consumption goods are traded at prices $p \in \mathbb{R}^L_{++}$ and household preferences are represented by a function $V(x_m, x_f)$. Hence, the optimal consumption bundle must solve:

$$\begin{aligned} d_u\left(y_m, y_f\right) = \quad & \arg\max \; V(x_m, x_f) \\ & s.t. \; (x_m, x_f) \in \mathbb{R}^{2L}_+ : p \cdot (x_m + x_f) \leq y_m + y_f. \end{aligned} \tag{6}$$

In this model, the demand for goods depend only on prices and household total income $(y_m + y_f)$. If preferences were homothetic, changes in $(y_m + y_f)$ would not affect the share of income expended on each good consumed by this household. If V were not homothetic, then the income effects associated to changes in $(y_m + y_f)$ would affect the household expenditure shares. In any case, the impact does not depend on whether the change in $(y_m + y_f)$ resulted from a change in y_m or in y_f. Formally, we have: $\frac{\Delta d_u(\cdot)}{\Delta y_m} = \frac{\Delta d_u(\cdot)}{\Delta y_f} = \frac{\Delta d_u(\cdot)}{\Delta (y_m + y_f)}$.

The Collective Approach

Suppose now that household members have distinct preferences and allocate the household income through a Pareto efficient bargaining process, where each member bargaining weight depends on the household income composition (y_m, y_f). In this case, the household choices should solve:

$$\begin{aligned} d_c\left(y_m, y_f\right) = \quad & \arg\max \; u_m(x_m) + \lambda\left(y_m, y_f\right) u_f\left(x_f\right) \\ & s.t. \; (x_m, x_f) \in \mathbb{R}^{2L}_+ : p \cdot (x_m + x_f) \leq y_m + y_f, \end{aligned} \tag{7}$$

where the female bargaining power $\lambda(y_m, y_f)$ is increasing in the female income participation, y_f/y_m.

The demands will now depend on the income composition, since (y_m, y_f) affect the objective function. Changes in (y_m, y_f) would affect the share of income expended in each good due to income effects and bargaining effects resulted from the fact that members weights depend on the income vector. In other words, $\frac{\Delta d_c(\cdot)}{\Delta y_m} \neq \frac{\Delta d_c(\cdot)}{\Delta y_f}$.

5.1 Identifying Bargaining Effects

Ideally, we would like to have an experiment where the cash benefits were randomly assigned to males and females in different households. Unfortunately, such an experiment does not exist. Our data present some households with a female beneficiary and others with no beneficiary. When studying families with male and female adults, BA participation can impact household consumption patterns through two different channels: income effects and a potential increase of female bargaining power. In addition to these two economic effects, BA beneficiaries were also exposed to regular attendance at public health centers, which might have affected their consumption choices in some arbitrary way. Therefore, although our data allows for a fairly clean identification of the average effect of the program (see Section 4), additional assumptions are needed to disentangle potential bargaining effects from income and health-monitoring effects.

Fortunately, a special schedule of the data (Schedule 9) contains information on whether the beneficiary women has some extra source of earning different from the BA transfer. Those women were likely to be more independent. Thus, if bargaining effects are important, they should probably differ across these two types of families.

Define S as the share of the total expenditures that is allocated by each household to some general expenditure group; and let I and BA be dummy variables such that $I = 1$ indicates that the eligible woman either lives with no male adult or has some extra source of income, and $BA = 1$ denotes participation into the BA program. Consider then the following version of the diff-in-diff model:

$$S = \beta_0 + \beta_1 (I * BA) + \beta_2 BA + \beta_3 I + \varepsilon, \tag{8}$$

where $(\beta_0, \beta_1, \beta_2, \beta_3)$ is a parameter vector and $*$ indicates the iteration of the two dummy variables.

Just like in Section 4, one needs to control for the vector Z—which contains variables describing the number of household members, municipality, and participation in the BE program—in order to consistently identify the parameters of equation (8) through an OLS regression. The model becomes then:

$$S = \beta_0 + \beta_1 (I * BA) + \beta_2 BA + \beta_3 I + Z \cdot \theta + v, \tag{9}$$

where $v = \varepsilon - L(\varepsilon \mid Z)$.

The parameter β_1 captures the differentiated impact of that BA transfer on households with more independent women. To see this, notice that the average effect of the BA transfer on households in which the eligible woman had some extra source of earning is given by:

$$ATE_1 = E(S \mid I = 1, BA = 1) - E(S \mid I = 1, BA = 0) = \beta_1 + \beta_2. \tag{10}$$

Similarly, the average effect of the BA transfer on households in which the eligible woman did not have some extra source of earning is given by:

$$ATE_0 = E(S \mid I = 0, BA = 1) - E(S \mid I = 0, BA = 0) = \beta_2. \tag{11}$$

Thus, the difference-in-difference effect is then given by:

$$ATE_1 - ATE_0 = \beta_1. \tag{12}$$

Notice also that the average effect of the BA program over the expenditure shares—which was denoted by α_1 in Section 4—is given by

$$\alpha_1 = pATE_1 + (1 - p) ATE_0 = p\beta_1 + \beta_2, \tag{13}$$

where $p = \Pr(I = 1)$.

5.2 Empirical Results

We estimate equation (9) for the same expenditure categories used before. Under the assumption that the change in bargaining power of beneficiary women with an extra source of income is different from those without it, the coefficient associated with the dummy $BA * I$ should be statistically different from zero. This is not in line with our OLS estimates presented in Tables 6.

[Tables 6]

It is important to recall that many households that applied and were eligible for the BA program had children at schooling age and were also enrolled in the BE program. Like in the BA program, the BE transfers are always made to the highest ranking woman in the household. Thus, BE participants are always classified in the group of single mothers and more independent women. However, it is possible that BE participants differ in important unobserved characteristics as they must have children at schooling age. Therefore, we re-estimated the previous regressions excluding BE participants from the sample. The results are qualitatively identical to the previous ones, as is shown in Tables 7. In particular, the coefficients associated with the $BA*I$ dummies are not statistically significant in all regressions. Moreover, the estimated magnitude for the average impact of the program over expenditure shares is similar to those presented in Tables 4 and 5. In other words, although the BA program has increased the consumption shares of vegetables and fruits (and reduced the expenditure share of general services such as utilities), this effect is not related to our measure of female independence.

[Table 7]

5.3 Accessing the Identification Assumptions

There are two key identification assumptions behind the results in this section. First, our identification strategy relies on the assumption that the beneficiary women with no previous personal income have experienced a differentiated change in their ability to expend the BA transfer. The nature of this assumption is very subjective and, in the absence of bargaining effects, no household choice should reflect this assumed differentiated ability to re-allocate household expenditures.

A second and equally important identification assumption requires that—at least for the first six month of the program (when the data were gathered)—participation into the BA program did not affect the fraction of single-mother households and (especially) the fraction of households whose women had an extra source of personal income. Naturally, the BA program could have affected the family structure through changes in divorces and female labor supply. We use regression analysis to show in Table 8 that the conditional correlation between BA participation and the family structure is not statistically significant for the two subsamples used in Tables 6 and 7. Potential changes in family structure that compromise our identification assumption have not been statistically important during the first six month of the program.

[Table 8]

6 Robustness Exercises

In a first attempt to check the robustness of our results, we re-define our comparison category to include only households with single mothers and eligible women whose personal earning is the largest in the household. This is a different measure of female independence. Under this concept, there is 180 (out of 1,006) households with independent women, where 103 of them are not single mothers.

Tables 9 presents the identification tests (analogous to Table 8), while Tables 10 and 11 present the results for the full sample and for the subsample of BE nonbeneficiaries (analogous to Tables 6 and 7). Like before, the results do not support the existence of bargaining effects on those households' consumption choices.

[Tables 9-11]

In addition to that, we have excluded single-mother households from the sample and re-estimated all different models presented before. Moreover, for sake of statistical efficiency, we pursue two alternative estimation techniques for all previous models. First, we have included household characteristics—such as the education characteristics of the eligible woman (see Table 1) and the gender and age composition of each family—into the OLS regression. Second, we have used the Zellner's seemingly unrelated regression model for the system of expenditure shares. The qualitative and quantitative results of all these different exercises are identical to those presented in Tables 4, 6, and 7. They are omitted here and are available upon request.

7 Conclusion

We study gender bias in the intrahousehold allocation of resources using data from a recent social program called *Bolsa Alimentação* (BA). The BA program was designed to reduce nutritional deficiencies and infant mortality among the poorest households in Brazil. It relies on demand-side incentives by means of money transfers to pregnant women and mothers of young children in very low-income families. Due to bureaucratic mistakes, many eligible applicants did not receive the cash

benefit. These unintentional exclusions formed a control group in the molds of random experiments.

Our results do not support the idea that household consumption decisions are affected by exogenous changes in the income share of male and female members. It is important to stress that the households in our sample are very poor and expend most of their income on basic goods. Therefore, our results are not representative of how middle-class households react to exogenous changes in female nonlabor income. Instead, they contribute to the literature on development economics by questioning the growing belief that CCT programs affect intrahousehold bargaining.

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Table 1. Randomization Test

	BA Dummy	BA Dummy	BA Dummy	BA Dummy
The Elegible Woman				
reads well	-0.030			
	(0.035)			
reads poorly	-0.037			
	(0.036)			
writes well		-0.012		
		(0.034)		
writes poorly		0.003		
		(0.036)		
understands basic math			-0.017	
			(0.034)	
poorly understands basic math			-0.024	
			(0.036)	
is currently registered at school				0.034
				(0.050)
has been registered at school				0.002
				(0.036)
Numb. Household Members	-0.003	-0.001	-0.002	-0.001
	(0.008)	(0.008)	(0.008)	(0.008)
BE Dummy	-0.230***	-0.229***	-0.229***	-0.229***
	(0.034)	(0.034)	(0.034)	(0.034)
Municipality Dummies:				
Itabuna	0.754***	0.729***	0.740***	0.714***
	(0.063)	(0.063)	(0.061)	(0.067)
Mossoró	0.834***	0.814***	0.825***	0.802***
	(0.051)	(0.050)	(0.051)	(0.058)
Teixeira de Freitas	0.800***	0.777***	0.788***	0.764***
	(0.051)	(0.051)	(0.049)	(0.055)
Teotônio Villela	0.837***	0.817***	0.828***	0.803***
	(0.066)	(0.065)	(0.066)	(0.072)
Observations	1,006	1,006	1,006	1,006

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 2. Summary Statistics

	Mean	St. Dev.	Min.	Max.	Freq. of Zeros
Expenditure Shares (BRL)					
General Services (Utilitiies, etc.)	16.0%	8.9%	0.0%	54.0%	0.7%
Family Expenses (Clothes, Education, Health, etc.)	17.0%	10.7%	0.0%	89.7%	0.3%
Vices (Alcohol, Tobacco, & Gambling)	1.6%	3.2%	0.0%	32.8%	52.2%
Grains	20.1%	8.4%	0.0%	66.4%	0.1%
Vegetables	5.2%	3.9%	0.0%	28.9%	4.6%
Fruits	4.7%	4.1%	0.0%	33.7%	12.4%
Dairy	7.4%	5.6%	0.0%	50.6%	5.3%
Meat	18.6%	8.6%	0.0%	47.7%	1.0%
Oil, Spices, & Soft Drinks	9.4%	4.8%	0.0%	63.9%	0.4%
Total Monthly Expenditure (BRL)	373.29	166.61	29.00	1,116.72	0.0%
Number of Household Members	5.5	2.2	2	16	0.0%
Participation in the BE Program	34.6%	47.6%	0	1	-
Municipality Dummies:					
Itabuna	17.1%	37.7%	0	1	-
Mossoró	26.7%	44.3%	0	1	-
Teixeira de Freitas	42.8%	49.5%	0	1	-
Teotônio Villela	13.3%	34.0%	0	1	-

Observations: 1,006 households

Table 3. Summary Statistics - Subsamples

	Households Not Enrolled in the BE Program				Households Previously Enrolled in the BE Program			
	Treated Group (BA=1)		Control Group (BA=0)		Treated Group (BA=1)		Control Group (BA=0)	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Expenditure Shares (BRL)								
General Services	15.7%	8.3%	17.0%	9.3%	15.0%	8.8%	17.0%	10.3%
Family Expenses	17.3%	11.0%	17.9%	11.3%	15.7%	9.5%	16.8%	10.3%
Vices	1.5%	3.3%	2.1%	3.9%	1.5%	2.9%	1.5%	2.8%
Grains	19.1%	8.0%	18.6%	8.7%	22.3%	7.6%	22.1%	9.1%
Vegetables	5.6%	4.2%	4.8%	3.6%	5.1%	3.2%	4.5%	3.7%
Fruits	5.1%	4.1%	4.2%	3.9%	4.9%	4.9%	4.0%	3.3%
Dairy	7.7%	5.6%	8.0%	6.7%	7.2%	4.9%	5.9%	5.0%
Meat	18.9%	8.6%	17.9%	8.7%	18.6%	8.1%	18.0%	8.9%
Oil, Spices, & Soft Drinks	9.1%	4.8%	9.4%	4.6%	9.6%	4.4%	10.1%	5.1%
Total Monthly Expenditure (BRL)	371.72	162.70	363.38	169.60	384.21	149.08	374.66	194.24
Number of Household Members	4.9	1.9	5.0	2.2	6.6	2.2	6.5	2.1
Observations	509		149		188		160	

Table 4. ATE on Expediture Shares: OLS Regressions

	General	Family	Vices	Grains	Vegetables	Fruits	Dairy	Meats	Oils, Spices, & Soft Drinks
BA Dummy	-0.014**	-0.007	-0.003	0.002	0.008***	0.008***	0.002	0.008	-0.004
	(0.006)	(0.007)	(0.002)	(0.006)	(0.003)	(0.003)	(0.004)	(0.006)	(0.003)
Numb. Household Members	-0.003***	-0.004**	0.001	0.009***	-0.001	-0.002***	-0.001*	-0.001	0.002***
	(0.001)	(0.002)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant, BE, and Municipality Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,006	1,006	1,006	1,006	1,006	1,006	1,006	1,006	1,006

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5. ATE on Expediture Shares: IV Regressions

	General	Family	Vices	Grains	Vegetables	Fruits	Dairy	Meats	Oils, Spices, & Soft Drinks
BA Dummy	-0.013*	-0.002	-0.003	0.003	0.008***	0.008**	0.000	0.004	-0.004
	(0.008)	(0.009)	(0.003)	(0.007)	(0.003)	(0.003)	(0.005)	(0.007)	(0.004)
Numb. Household Members	-0.003***	-0.004**	0.000	0.009***	-0.001	-0.002***	-0.001*	0.000	0.002***
	(0.001)	(0.002)	0.000	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant, BE, and Municipality Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	998	998	998	998	998	998	998	998	998

Instrumented Variable: BA participation when the data were collected (with reincluded households)

Instrumental Variable: BA status when the program started (before reinclusions)

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6. Intrahousehold Bargaining: Decomposing the ATE on Expenditure Shares

	General	Family	Vices	Grains	Vegetables	Fruits	Dairy	Meats	Oils, Spices, & Soft Drinks
BA * Female Income	0.008	-0.011	0.002	-0.013	-0.001	-0.002	0.011	0.006	0.001
	(0.014)	(0.016)	(0.005)	(0.012)	(0.005)	(0.006)	(0.010)	(0.013)	(0.007)
BA Dummy	-0.020*	-0.006	-0.005	0.012	0.010**	0.010**	-0.003	0.005	-0.005
	(0.011)	(0.013)	(0.005)	(0.009)	(0.004)	(0.005)	(0.008)	(0.011)	(0.006)
Female Income Dummy	-0.014	0.032**	-0.002	0.001	0.000	0.007	-0.010	-0.003	-0.010
	(0.013)	(0.016)	(0.005)	(0.011)	(0.005)	(0.006)	(0.009)	(0.013)	(0.007)
Numb. Household Members	-0.003***	-0.004**	0.001	0.009***	-0.001	-0.001**	-0.001*	0.000	0.002***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant, BE, and Municipality Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	968	968	968	968	968	968	968	968	968

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 7. Intrahousehold Bargaining: Excluding BE Participants

	General	Family	Vices	Grains	Vegetables	Fruits	Dairy	Meats	Oils, Spices, & Soft Drinks
BA * Female Income	0.014	-0.029	-0.001	-0.010	0.000	-0.001	0.009	0.017	0.003
	(0.017)	(0.023)	(0.007)	(0.017)	(0.007)	(0.008)	(0.012)	(0.018)	(0.008)
BA Dummy	-0.020*	-0.006	-0.005	0.013	0.010**	0.010**	-0.003	0.005	-0.005
	(0.011)	(0.013)	(0.005)	(0.009)	(0.004)	(0.005)	(0.008)	(0.011)	(0.006)
Female Income Dummy	-0.017	0.047**	0.001	0.000	-0.001	0.006	-0.009	-0.015	-0.011
	(0.016)	(0.021)	(0.007)	(0.015)	(0.006)	(0.007)	(0.010)	(0.016)	(0.007)
Numb. Household Members	-0.002	-0.005**	0.001	0.010***	-0.001	-0.002***	-0.003***	-0.002	0.003***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Constant and Municipality Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	620	620	620	620	620	620	620	620	620

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 8. Identification Test: The BA Impact on Other Female Earnings

	Full Sample	*BE Nonbeneficiaries*
	Female Income Dummy	**Female Income Dummy**
BA Dummy	0.026	0.044
	(0.026)	(0.046)
Numb. Household Members	-0.014**	-0.023**
	(0.006)	(0.010)
BE Dummy	0.650***	-
	(0.022)	
Constant and Municipality Dummies	Yes	Yes
Observations	968	620

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9. Robustness: The BA Impact on Other Female Earnings

	Full Sample	*BE Nonbeneficiaries*
	Female Income Dummy	**Female Income Dummy**
BA Dummy	0.029	-0.017
	(0.029)	(0.036)
Numb. Household Members	-0.026***	-0.029***
	(0.007)	(0.008)
BE Dummy	0.112***	
	(0.030)	
Constant and Municipality Dummies	Yes	Yes
Observations	968	620

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 10. Robustness: Decomposing the ATE on Expenditure Shares

	General	Family	Vices	Grains	Vegetables	Fruits	Dairy	Meats	Oils, Spices, & Soft Drinks
BA * Female Largest Income	0.024	0.019	0.000	-0.023	-0.007	-0.011	-0.013	0.008	0.001
	(0.017)	(0.020)	(0.006)	(0.016)	(0.008)	(0.007)	(0.010)	(0.017)	(0.008)
BA Dummy	-0.021***	-0.015*	-0.004	0.009	0.011***	0.011***	0.007	0.007	-0.004
	(0.007)	(0.009)	(0.003)	(0.006)	(0.003)	(0.003)	(0.005)	(0.007)	(0.004)
Female Largest Income Dummy	-0.018	-0.015	0.002	0.019	0.008	0.009*	0.006	-0.010	-0.001
	(0.015)	(0.016)	(0.006)	(0.014)	(0.007)	(0.005)	(0.009)	(0.015)	(0.007)
Numb. Household Members	-0.003***	-0.004**	0.001	0.009***	-0.001	-0.002**	-0.001*	-0.001	0.002***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant, BE, and Municipality Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	954	954	954	954	954	954	954	954	954

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 11. Robustness: Excluding BE Participants

	General	Family	Vices	Grains	Vegetables	Fruits	Dairy	Meats	Oils, Spices, & Soft Drinks
BA * Female Largest Income	0.045**	0.001	0.002	-0.033	0.005	-0.010	-0.005	-0.007	0.002
	(0.019)	(0.031)	(0.008)	(0.024)	(0.008)	(0.010)	(0.014)	(0.026)	(0.009)
BA Dummy	-0.023**	-0.015	-0.006	0.014*	0.009**	0.011***	0.001	0.012	-0.004
	(0.010)	(0.012)	(0.004)	(0.008)	(0.004)	(0.004)	(0.007)	(0.009)	(0.005)
Female Largest Income Dummy	-0.048***	0.011	-0.001	0.030	-0.002	0.010	0.002	0.006	-0.008
	(0.016)	(0.027)	(0.008)	(0.022)	(0.006)	(0.008)	(0.013)	(0.023)	(0.007)
Numb. Household Members	-0.002	-0.005**	0.001	0.010***	-0.001	-0.002***	-0.003***	-0.001	0.003***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Constant and Municipality Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	620	620	620	620	620	620	620	620	620

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%