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 allocations of resources. The program makes cash transfers to mothers and pregnant women in poor households. Bureaucratic mistakes, beyond the control of the applicants, have inadvertently excluded many households that had applied and were accepted to the program. This unintentional natural experiment is used to identify the impact of an exogenous variation in female nonlabor income over household consumption. We find that program participation led to an increase in food expenditure, but this effect is not due to women being the benefit recipients.

I. Introduction

CONDITIONAL cash transfer (CCT) programs pervade virtually every country in Latin America, collectively benefiting more than 70 million people throughout the region. Following this lead, CCT programs have also been launched throughout the world, including Bangladesh, Cambodia, Indonesia, Malawi, Morocco, Pakistan, South Africa, and Turkey. More recently, Washington DC, and New York City have launched pilot programs that use CCTs to foster household investments in children's schooling. Overall, these programs are widely perceived as being effective in reducing poverty in both the short and long runs (see Lindert, Skoufias, & Shapiro, 2006; Fiszbein et al., 2009).

Almost all CCT programs share the common feature that the cash transfer is made to a woman. This policy design is motivated by a growing belief that women exhibit expenditure patterns that are more pro-child and pro-family than men do. This is also supported by a solid theoretical literature arguing that family expenditures may depend on factors such as the intrahousehold distribution of income.

This paper empirically assesses whether poor women empowered by CCTs do indeed divert household resources toward goods typically considered as pro-child and pro-family. In pursuing this goal, we use a special data set collected in 2002 to evaluate the impact of Bolsa Alimentação (BA), one of the first CCT programs implemented in Brazil. The empirical strategy relies on the fact that many eligible households that had applied and were accepted to the program were randomly and unintentionally excluded from it. The evaluation focuses on poor areas of the northeast region of the country.

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 under age 7. The program required compliance with vaccination schedules and regular visits to public centers for prenatal care, child-growth monitoring, and health education classes. The transfers were always made to the highest-ranking woman in the household, usually the mother of all the children in the family. Payments were conducted by a large federal bank, Caixa Econômica Federal (CEF), which issued a personalized ATM card to each beneficiary woman. These electronic cards could be used at CEF branches and network locations, as well as in authorized retail stores.

The amount transferred averaged approximately 8% of total household expenditure. Thus, participation in the program represented a significant exogenous variation in female nonlabor income. Beneficiary families likely believed the increase in income would last for a long period because as their children grew older, they would become eligible to the follow-up CCT program, the Bolsa Escola (BE). The BE program targeted families with children between ages 7 and 14 years of age and was conditional on school attendance.[1] Note, however, that even a short-term transitory shock on female income could affect intrahousehold consumption decisions, since it is reasonable to believe that poor families in Brazil are credit constrained.

A. Accidental Exclusions

The uniqueness of the data lies in the fact that a group of eligible households, which had 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 households' unobserved characteristics.

First, some files containing household-identifying information were lost during electronic transmission to CEF, the bank responsible for the payments. These exclusions were due to local network problems and staff mistakes. For each municipality, they are independent of household characteristics.

The second source of exclusion stems from the software used by municipal authorities in charge of program registration: although this software was adapted to the Portuguese language, the software used in the federal capital by CEF 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

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

not able to read special characters (such as ç, ã, é, and ô), 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 (such as João, José, Ângela, Andréa, Tânia, and Mônica) and surnames (such as Aragão, Gonçalves, Magalhães, Mendonça, and Simões). Furthermore, these names and surnames are homogeneously distributed across the population and are not linked to specific ethnic, gender, age, or income groups. Hence, this second exclusion criterion is also exogenous, conditional on the number of members in the household.[2]

The third source of exclusion is related to misspelling problems. Many households that applied and were eligible for the BA program had school-aged children and were also enrolled in the BE program. The federal bank responsible for the transfers of both programs, CEF, decided that it would issue a single 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, 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 out 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.

Households and local authorities were aware that registration did not guarantee participation. Eligibility needed to be confirmed by the program staff, who predicted per capita income for each family using national surveys and the household information collected during the registration process. Moreover, there was great uncertainty about the number of beneficiaries since the program budget allocated to each municipality was not announced in advance by the federal government. It is thus reasonable to assume that households interpreted these accidental exclusions as being caused by lack of eligibility and did not act on it. (The possibility of program reinclusions caused by households' actions is addressed in section IVB.)

B. The Analysis

We start our analysis by reviewing the related literature in section II. We point out that our accidentally excluded and matched beneficiary households face the same relative prices,

since they live in the same municipality and their choices are observed in the same period. This represents an important advantage over quasi-experimental before-and-after analyses, as well as over traditional randomizations in which the program implementation is delayed in some randomly chosen locations.

The survey design and some descriptive statistics are presented in section III. Next, in section IV, we discuss the econometric conditions under which OLS regressions consistently estimate the average treatment effect of the BA program on the subpopulation of accidentally excluded households in those areas. We also show that when compared to nonbeneficiaries, BA participants spend proportionally less on utilities and more on vegetables and fruits. We do not find statistically significant impacts of the program on expenditure shares of dairy, vices (alcohol, tobacco, and gambling), and family goods (such as health, schooling, clothes, personal hygiene, and house cleaning products).

The relative increase in the consumption share of vegetables and fruits could result from three sources: (a) a female empowerment effect due to the increase in the fraction of household income accruing to the beneficiary women, (b) an income effect resulting from the program transfer (and not fully captured by the budget share econometric representation), and (c) a health-monitoring effect resulting from the fact that BA participants committed to regular visits to public health centers.

In section V, we present a model of household behavior that illustrates how a change in income shares could affect household consumption choices. Next, in section VI, we explore the family structure of sampled households to disentangle potential female empowerment effects from the other two possible effects of the program. We measure the impact of BA participation on expenditure patterns in two groups: families with adults of different genders and families with no male adult living in the household, typically composed of single mothers and their children. We refer to these groups as mixed-gender and female households, respectively. Gender-specific empowerment effects should be present only in the first category of households.

This difference-in-differences (diff-in-diff) strategy is valid under the identifying assumption that the BA program did not affect the fraction of female households. Naturally the BA program could have affected the family structure through changes in divorce rates. 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 systematically occurred during the first six months of the program (when the data were recorded).

According to our results, the change in income shares of men and women did not affect household consumption patterns. The BA program did affect household expenditures on utilities, vegetables, and fruits, but the average impact is statistically equal across female and mixed-gender households. Similar results are obtained when we compare two groups

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

of mixed-gender households—one in which the beneficiary women have another source of personal income different from the BA transfer and another in which they do not. These results and additional robustness exercises appear in section VI. A brief conclusion is presented in section VII.

II.   Literature Review

A large literature is dedicated to the analysis of gender differences in household expenditure decisions. We are aware of the following works that, similar to ours, use natural experiments or quasi-experiments to study the effect of women's empowerment policies on household allocations.

Attanasio and Lechene (2002) examine the effects of participation in a cash transfer program, PROGRESA, which started in 1998 in rural Mexico. The transfer is conditional on school enrollment, and the mother is always the recipient. As part of the program design, a number of randomly selected villages did not start the program for a year and a half; these communities form the control group. The authors consider eight types of nondurable expenditures: food, alcohol and tobacco, transportation, services, and clothing for women, men, girls, and boys. The empirical results indicate a positive impact of female income on girls' and boys' clothing and a negative impact on alcohol. They also show that self-reported decision-making power varies across recipients, nonrecipients, and future recipients in the control villages.

In the quasi-experimental literature, Lundberg, Pollak, and Wales (1997) use the 1979 reform in the U.K. family allowance policy. This reform replaced a tax allowance program with 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 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 to 1976 to represent the consumption regime before the policy change and the period 1980 to 1990 to represent the regime after the reform. They find that in beneficiary households, expenditures increased for children's and women's clothing relative to men's. This analysis was limited to clothing expenditures and did 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 Lundberg et al.'s results.

Ward-Batts (2008) uses this same data source disaggregated to the household level to test the effect of the 1979 reform in the U.K. family allowance policy on household expenditure shares. The periods 1973 to 1976 and 1980 to 1983 represent the regimes before and after the reform. She uses data on households consisting of one man, his wife, and between one and three children under 18 years of age. 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 similar quasi-experiment took place in Australia during the 1990s and was analyzed by Bradbury (2004). During the decade, a series of public reforms resulted in significant changes in transfer payments to married-couple families. The previous system, in which payments were almost solely received by men, evolved to one in which more than half of the public transfers were directed to women. Bradbury estimates Engel curves for 23 different commodities in order to assess the effect of the changes on the intrahousehold distribution of income on expenditure patterns. The analysis focuses on the Australian Household Expenditure Survey, which is conducted every five years. Three surveys are used: 1988–1989 (before any policy change), 1993–1994 (when part of the reform had already taken place), and 1998–1999 (after the last policy change). Contradicting the previous literature, results indicate a very small effect of the income shift on household expenditure allocation. Furthermore, the few significant changes were frequently in the wrong direction—that is, increases in the expenditure share of goods such as tobacco and alcohol and decreases in the expenditure share of food consumed at home.

Our work has two important advantage over these exercises. Unlike those in Attanasio and Lechene (2002), our treated and nontreated households live in the same municipality. Moreover, different from the before-and-after exercises in Lundberg et al. (1997), Ward-Batts (2008), and Bradbury (2004), we analyze treated and nontreated households during the same period. These two features allow us to avoid concerns about eventual changes in relative prices across municipalities or over time. It is worth stressing that as in Attanasio and Lechene (2002) and unlike the other sources, our analysis refers to very poor families that spend most of their income on basic goods.

A.   Additional Related Works

In a related strand of the literature, Browning et al. (1994) explicitly model household decisions under the assumption that the interactions among household members with different preferences lead to Pareto-efficient outcomes. The structural parameters of the 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 toward their own consumption. Moreover, holding age differences and relative income shares constant, women are able to divert more income toward their own consumption in wealthier households.

There are also many relevant reduced-form studies that estimate the effect of women's leadership on household-choice variables. Handa (1996) presents evidence that female-headed households in Jamaica dedicate a greater budget share to children's clothes, health, and food goods, while male-headed households spend more on 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 has 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, and Ghana, shows that mothers' education level tends to have a stronger effect on girls' height (relative to boys), while fathers' education has a larger impact on boys' height. Thomas, Contreras, and Frankenberg (2002) use data from the Indonesia Family Survey to study how child morbidities (such as diarrhea, cough, and fever) are affected by the relative value of assets that wives and husbands bring to the marriage (an indicator of economic independence). The results suggest that gender matters in Java and Sumatra, although not in the other Indonesian regions considered.

Although important as an initial assessment of intrahousehold allocation decisions, this reduced-form evidence is potentially biased by the endogeneity of the leadership variables (for example, headship, income share, education, and asset ownership). Unobserved characteristics of the family could be correlated with both leadership and household members' willingness to divert resources toward children-assignable goods.[3]

III. Data

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

Initiated in 2001, the BA program consisted of cash transfers to low-income families with pregnant women or mothers of children under 7 years of age. Households were eligible if their estimated monthly per capita income was below 90 BRL (equivalent to about 37.50 U.S. dollars in 2001 values). The highest-ranking woman in the household, typically the mother of all the children, was the sole recipient of the cash transfer. The values transferred were 15, 30, or 45 BRL per month (something between 6.25 and 18.75 U.S. dollars). The exact value depended on the number of qualifying children in the household.

The transfers were conditional on women committing to a "charter of responsibilities" that required compliance with vaccination schedules and regular visits to health centers for prenatal care and child growth monitoring. Beneficiary women were also required to attend to health, nutrition, and child care classes. Eligibility for the BA expired when children turned 7 years old. Poor families with children between 7 and 14 years of age 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 that had applied for the program and were eligible to benefit from it but were unintentionally excluded. As explained before, three types of accidental exclusion were detected: (a) some household files were lost during the electronic transmission from the local registration offices to CEF, the national bank responsible for issuing identification numbers and transferring the payments; (b) the computer program used by CEF was not adapted for the Portuguese language and then excluded households in which one or more members had special characters in their names; and (c) some households previously enrolled in the BE program were excluded from the BA program because 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. Municipal authorities were asked to register considerably more than their specified quotas with the expectation that some registered households would later be found to be ineligible. The precise number of beneficiaries would also depend on the program budget allocated to each municipality. In addition, households were not able to communicate directly with CEF. Therefore, it is plausible that households typically did not react when excluded, believing their exclusion was due to lack of eligibility.

When the survey interviews were conducted, 19 (out of 282) households in the accidentally excluded group had been reincluded in the program and reported receiving the BA transfer. Similarly, 44 (out of 717) households in the matched beneficiary group reported not receiving transfers. These reinclusions and late exclusions were conducted by the program staff, who did not communicate directly with the households. They are likely independent of household

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[3] Two other classes of related works should be mentioned. A number of authors have studied intrahousehold allocation of resources in families consisting of parents and their children or grandchildren; see Costa (1997), Pezzin and Schone (1997), Duflo (2000, 2003), and Edmonds, Mammen, and Miller (2005). There is also an important policy debate on whether social programs targeted to children should make monetary or in-kind transfers. See Bingley and Walker (2009) for quasi-experimental evidence from the United Kingdom.

characteristics and are treated as such in most of the paper. We address the potential endogeneity problems associated with these late inclusions and exclusions in section IVB.

A.   Survey Design

Excluded households were found in 67 Brazilian municipalities. Two criteria guided the selection of the municipalities in the evaluation study. First, municipalities had to be located in the northeastern region of the country. About 60% of the beneficiaries resided in this poor region of Brazil. Second, for cost-saving reasons, only municipalities participating in the program for the previous six months and with at least forty excluded families were surveyed.

In April 2002, when the survey team went to the field, four municipalities fit these two criteria: Teotônio Villela, in the state of Alagoas; Mossoró, in the state of Rio Grande do Norte; and Itabuna and Teixeira de Freitas in the state of Bahia. All excluded households were surveyed, and a sample of matching beneficiaries was selected from the roster of receiving beneficiaries. The data collected during the registration process were used to match two beneficiary families to each excluded household. The matching criteria required that participants and nonparticipants exhibited residence in the same municipality, identical gender for each eligible child, and similar socioeconomic characteristics. After using the first two criteria to determine a preliminary pool of matched beneficiaries, the following variables were used to match in terms of preprogram socioeconomic characteristics: per capita self-reported monthly income, number of household members, and per capita monthly expenditures on rent, water, electricity, and gas. Using principal components analysis, these variables were reduced to a single factor. The matching algorithm consisted on finding, in a random order, optimal matches based on the weighted sum of the squared difference in the socioeconomic scores and the squared age difference of the eligible women. This technique is known as nearest-neighbor matching based on Euclidean distances (see Rosenbaum & Rubin, 1985). In a technical report, IFPRI (2003) describes the matching algorithm, questionnaire design, and instruction for interviewers, among other implementation details.

The use of household characteristics to construct a matched sample was first proposed by Donald Rubin in a series of studies summarized in Rubin (2006). This methodology consists of a systematic approach to sampling control and treatment groups in order to increase the overlap in the distribution of covariates of both groups. Regression analysis can be then conducted in matched samples. Rubin (1979) uses Monte Carlo simulation to show how regression adjustments coupled with a matched sampling procedure can improve estimation results in small samples.[4]

It is important to stress that we do not have access to the presurvey data collected during registration and used by IFPRI for matching beneficiaries to the accidentally excluded households. However, the survey data used in this paper contain more information about household characteristics than what was available for the sampling design. Using the survey data, we test whether the socioeconomic characteristics that are unlikely to have been affected by the program are in fact balanced across excluded and matched households. In table 1, we regress the BA participation dummy on different information about the education of the eligible woman, on a dummy variable describing whether the household is headed by a woman, and on the number of household members of different gender and age ranges. All regressions are conditional on the number of household members, the municipality where registration was conducted, and previous enrollment status in the BE program, since accidental exclusions are supposed to be random only after conditioning on these variables (see section IV for a formal argument on this linear conditioning approach).

The reported educational characteristics of the eligible woman, the fraction of female-headed families, and the household gender-age structure are conditionally uncorrelated to BA participation. Since these variables were not used in the matching algorithm, our findings strongly support the baseline assumption that exclusions were conditionally exogenous.[5]

B.   Summary Statistics

The database contains detailed information on household expenditures. The survey questionnaire uses modules 1 and 5 to collect information on nonfood items and module 6 for food items.[6] We aggregate these items in nine expenditure groups:

- General services: Water, telephone, gas, and electricity (module 1, all items), and gasoline, auto services, taxes, pensions, lawyers, home insurance, mobile phone, weddings, donations, and funerals (module 5, items gr1, gr5, gr9, gr11, gr13, gr15–gr18)
- Family expenses: Health, schooling, clothes, personal hygiene, house cleaning products, home maintenance, furniture, and maids (module 5, items gr2–gr4, gr6–gr7, gr12, gr14, gr19–gr21)
- Vices: Tobacco and gambling (module 5, items gr8 and gr10) and alcoholic beverages (module 6, questions 75 and 76)
- Grains: Module 6, questions 1–12
- Vegetables: Module 6, questions 13–27

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[4] See also Heckman, Ichimura, and Todd (1998), Abadie and Imbens (2006, 2008), and Imbens and Wooldridge (2009) for a discussion on the asymptotic properties of matching estimators.

[5] Morris et al. (2004) use a different set of questions from this same questionnaire and report that the originally excluded and the matched households are similar in terms of the type of floor material of their homes, access to water supply through a public network, and access to a telephone.

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

TABLE 1.—BA PARTICIPATION AND HOUSEHOLD CHARACTERISTICS

	BA Dummy					
	(1)	(2)	(3)	(4)	(5)	(6)
Beneficiary educational dummies						
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)		
Had been registered at school				0.002		
				(0.036)		
Female headship dummy					−0.009	
					(0.044)	
Household composition						
Number of males 0–6 years old						0.028
						(0.057)
Number of males 7–14 years old						−0.019
						(0.059)
Number of males 15–18 years old						−0.086
						(0.068)
Number of males 19–60 years old						−0.061
						(0.057)
Number of males older than 61 years						dropped
Number of females 0–6 years old						0.052
						(0.058)
Number of females 7–14 years old						−0.011
						(0.058)
Number of females 15–18 years old						0.016
						(0.071)
Number of females 19–60 years old						0.022
						(0.067)
Number of females older than 61 years						−0.022
						(0.093)
Number of household members	−0.003	−0.001	−0.002	−0.001	−0.001	0.007
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.056)
BE dummy	−0.230***	−0.229***	−0.229***	−0.229***	−0.228***	−0.209***
	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.037)
Municipality dummies	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,006	1,006	1,006	1,006	1,006	1,006

OLS regressions with constant; dependent variable in first row; controls in first column. Robust standard errors in parentheses. Significant at *10%, **5%, ***1%.

- Fruits: Module 6, questions 28–40
- Dairy: Module 6, questions 41–50
- Meat: Module 6, questions 51–63
- Oils, spices, and soft drinks: Module 6, questions 64–74 and 77–81

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 was already enrolled in 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 spent in each group ranges from 1.6% for vices to 20.1% for grains. (It is probably worth mentioning that grains such as 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 reasonably small for the remaining eight expenditure categories.

Table 3 shows the sample means of each variable for participant and excluded households. It is divided into two blocks according to BE enrollment status. Households enrolled in the BE program must have at least one school-aged child. On average, they spend more and have a slightly larger number of members.

IV. Average Treatment Effect

Consider the standard switching regression framework to model unobserved counterfactuals. Let Y_i be an arbitrary random outcome associated with household i. If household i is enrolled in the BA program, its outcome Y_i is assumed to be

$$Y_{1,i} = \mu_1 + \varepsilon_{1,i}. \tag{1}$$

TABLE 2.—SUMMARY STATISTICS

	Mean	S.D.	Minimum	Maximum	Frequency of Zeros
Expenditure shares					
General services (utilities, durables, 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, and 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, and 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	–

Number of observations: 1,006 households.

TABLE 3.—SUMMARY STATISTICS IN 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	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Expenditure shares								
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, and 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
Number of observations	509		149		188		160	

If the family is not enrolled in the BA program, this same outcome is assumed to be

$$Y_{0,i} = \mu_0 + \varepsilon_{0,i}. \tag{2}$$

The terms μ_1 and μ_0 are constant parameters, and $\varepsilon_{1,i}$ and $\varepsilon_{0,i}$ are random variables with 0 expected value.

In this setting, the average impact of the BA program on Y_i is

$$\alpha = E(Y_{1,i} - Y_{0,i}) = \mu_1 - \mu_0. \tag{3}$$

Let BA_i be a dummy variable that equals 1 for beneficiary households. Household i's outcome can be expressed as

$$Y_i = BA_i Y_{1,i} + (1 - BA_i) Y_{0,i}, \tag{4}$$

and, hence,

$$Y_i = \mu_0 + \alpha BA_i + u_i', \tag{5}$$

where $u_i' = \varepsilon_{0,i} + BA_i(\varepsilon_{1,i} - \varepsilon_{0,i})$.

If the program assignment were random, one would have $E(u_i' \mid BA_i) = 0$, and the OLS method would consistently estimate the parameters of equation (5). However, since BA participation was accidental, identification cannot be taken for granted.

There were three sources of accidental exclusions: data loss during electronic transmission, special characters in the name of some household members, and inconsistency with previously recorded data for households already registered in the BE program. The first type of exclusion is 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 some family member will have a name with some special character. Similarly, the third type of exclusion is exogenous only 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 X_i be a k-dimensional real-valued random vector containing different characteristics of household i, including (in particular) variables describing the number of household

TABLE 4.—BA EFFECT ON LOG EXPENDITURES

	Ln Total Expenditure	Ln Food Expenditure	Ln Nonfood Expenditure
BA dummy	0.053*	0.099***	0.006
	(0.031)	(0.033)	(0.049)
Number of household members, BE, and municipality dummies	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes
Number of observations	1,006	1,005	1,006

OLS regressions with constant; dependent variables in first row; controls in first column. Robust standard errors in parentheses. Significant at *1%, **5%, ***1%.

members, municipality, and participation in the BE program. According to the description of the accidental exclusions, the BA dummy is exogenous conditional on X_i. Therefore, one should have

$$E\big(u_i' \mid X_i, BA_i\big) = E\big(u_i' \mid X_i\big) = g(X_i). \tag{6}$$

Existence and almost everywhere uniqueness of the function g is well established in probability theory. We assume this function to be linear such that

$$g(X_i) = X_i \cdot \theta \tag{7}$$

for a given parameter vector $\theta \in \mathbb{R}^k$.

Equations (5) to (7) lead us to the following regression model:

$$Y_i = \mu_0 + \alpha BA_i + X_i \cdot \theta + u_i, \tag{8}$$

where $u_i = u_i' - g(X_i)$.

The error term u_i is orthogonal to (X_i, BA_i). This follows from the fact that BA participation is conditionally exogenous, after one controls for the number of household members, the municipality of the household, and the previous status in the BE program. These variables must always be included in X_i in order to guarantee consistency of the OLS method. However, following Rubin (1979), we also include in X_i all household characteristics available in the database (see the list of these characteristics in the first column of table 1). Although the random nature of the program exclusions would balance covariates across large treated and control groups, the small sample results can be improved by surveying a matched sample and implementing regression adjustments. In Rubin (1979), the regression adjustments account for the fact that matched households are similar but not identical since the matching index algorithm reduces, but does not eliminate, differences across multiple characteristics. In our case, the regression adjustments also control for household characteristics that became available only after the data were collected.

A. OLS Results

We first analyze the average impact of the BA program over three alternative dependent variables: log total expenditure, log food expenditure, and log nonfood expenditure. These regressions appear in table 4. The estimated coefficients for

the BA dummy are 5.3% (significant at the 10% level) for the log total expenditure regression, 9.9% (significant at the 1% level) for the log food expenditure regression, and 0.6% (not statistically significant at the 10% level) for the log nonfood expenditure regression. The average BA transfer amounted to about 8% of the household average expenditure. Thus, the increase of 5.3 log points in total expenditure is meaningful. The higher total consumption in beneficiary households is almost entirely due to higher food consumption.

We then perform disaggregated regressions in which the dependent variable is the household expenditure in each good category. These results appear in table 5. The estimated coefficients associated with the BA dummy are positive for all good categories except general services, family goods, and vices. They are statistically different from zero for four good categories: grains, vegetables, fruits, and meats. This means that in absolute values, BA participants consume more of these three types of goods than the families that were accidentally excluded from the program.

Next, the regressions in table 6 use the share of total expenditure diverted to each good category as the dependent variable. On average, BA participants spent proportionally more on vegetables and fruits and less on general services (such as utilities) when compared to nonparticipants.

Later, in section VI, we focus on understanding which part of these changes in expenditure shares is due to a female empowerment effect as opposed to income and health-monitoring effects. (Recall that BA beneficiaries experienced an increase in their income and were required to attend health education classes, where they probably learned about the importance of a healthy diet.)

B. Late Inclusions and Exclusions

There is an important issue to address before proceeding. The accidental exclusions described earlier were not immediately detected by program managers. However, once an error was detected and fixed by CEF bank, the household was reincluded in the program. When the survey team went to the field to conduct the interviews, they found 19 (of 282) households in the accidentally excluded group that had been reincluded in the program and reported receiving the BA transfer. They also found 44 (of 717) households in the original matched beneficiary group that reported not receiving the transfers.

Since these late inclusions and exclusions were conducted by CEF staff without any influence from the municipality authorities and households themselves, they are likely to be conditionally independent of household observed and unobserved characteristics. Nevertheless, to address this issue, we estimate equation (8) using instrumental variables.[7]

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[7] IV estimation is consistent for the average treatment effect (ATE) under the assumption of homogeneous impacts across all households in our population of interest. Under weaker assumptions, IV methods are consistent for the local average treatment effect (LATE), which is the ATE for the subpopulation of compliers, that is, those who comply with the accidental experiment (see Angrist, Imbens, & Rubin, 1996).

TABLE 5.—BA EFFECT ON EXPENDITURE LEVELS

	General Services	Family	Vices	Grains	Vegetables	Fruits	Dairy	Meats	Oils, Spices, and Soft Drinks
BA dummy	−3.920	−2.480	−1.541	4.305*	3.286***	2.765**	1.284	6.302**	0.201
	(3.048)	(4.646)	(1.458)	(2.348)	(1.030)	(1.273)	(1.514)	(3.137)	(1.201)
Number of household BE, and municipality dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,006	1,006	1,006	1,006	1,006	1,006	1,006	1,006	1,006

OLS regressions with constant; dependent variables in first row; controls in first column. Robust standard errors in parentheses. Significant at *10%, **5%, ***1%.

TABLE 6.—BA EFFECT ON EXPENDITURE SHARES

	General Services	Family	Vices	Grains	Vegetables	Fruits	Dairy	Meats	Oils, Spices, and Soft Drinks
BA dummy	−0.012*	−0.007	−0.003	0.003	0.008***	0.008***	0.001	0.007	−0.004
	(0.006)	(0.007)	(0.002)	(0.006)	(0.003)	(0.003)	(0.004)	(0.006)	(0.003)
Number of household members, BE, and municipality dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,006	1,006	1,006	1,006	1,006	1,006	1,006	1,006	1,006

OLS regressions with constant; dependent variables in first row; controls in first column. Robust standard errors in parentheses. Significant at *10%, **5%, ***1%.

TABLE 7.—BA EFFECT ON LOG EXPENDITURES: IV METHOD

	Ln Total Expenditure	Ln Food Expenditure	Ln Nonfood Expenditure
BA dummy	0.072*	0.108***	0.031
	(0.038)	(0.039)	(0.058)
Number of household members, BE, and municipality dummies	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes
Number of observations	998	997	998

IV regressions with constant; dependent variables in first row; controls in first column. Instrumented variable: BA participation dummy; instrumental variable: BA initial status Dummy. Robust standard errors in parentheses. Significant at *10%, **5%, ***1%.

We use the BA status when the program started (before reinclusions) as the instrumental variable for BA participation when the data were recorded (six months after the program had started). The unconditional correlation between instrumental and instrumented variables is 84.99%. The IV results are reported in tables 7 to 9. The coefficients are very similar to the OLS estimates presented before in tables 4 to 6.

We claim that the original exclusions are exogenous, so the IV estimator is consistent. We implement Hausman tests under this identification assumption. We do not reject the null hypothesis that the OLS estimator is consistent and efficient (in the class of linear estimators) for all of these regression models. This suggests that BA participation was conditionally exogenous. Therefore, for the remainder of this paper, we focus our analysis strictly on the OLS method.

V.   Theory Background

We now present a theoretical background for the prediction that an exogenous increase in nonlabor income accruing to a particular household member could affect the final allocation of resources. There is a vast literature modeling household expenditure decisions as resulting from different bargaining mechanisms (see Manser & Brown, 1980; McElroy & Horney, 1981; Lundberg & Pollak, 1993; and Chen & Woolley, 2001). Given the illustrative purpose of this section, we focus our discussion on the collective approach, which assumes that household choices are Pareto efficient regardless of the specific details about the underlying bargaining process (see Chiappori, 1988; Browning, 1992; Browning, Chiappori, & Lechene, 2006).

Consider a household composed of one female and one male, respectively indexed by f and m, who consume $q_f \in \mathbb{R}_+^L$ and $q_m \in \mathbb{R}_+^L$ units of $L > 1$ different commodities. Define the household utility function as:

$$V(q,p,w,d) = \max_{(q_f,\,q_m)\in\mathbb{R}_+^{2L}} \lambda(p,w,d)u_f(q_f) + u_m(q_m),$$
$$\text{s.t. } q_f + q_m \leq q. \qquad (9)$$

The functions $u_f(q_f)$ and $u_m(q_m)$ represent the individual preferences of the female and male members, respectively. The Pareto weight $\lambda(p,w,d) > 0$ measures the female influence on the household consumption decisions. It is specified as a function of prices $p \in \mathbb{R}_{++}^L$, the household total income $w > 0$, and a vector of exogenous variables d that affect intrahousehold decision power but not individual preferences over consumption goods. The variables in d are sometimes referred to as distribution factors (DFs). Examples of DFs that have been used in the empirical literature are intrahousehold income distribution and the wealth contributed by each member at marriage.

Notice that the intrahousehold allocation derived from problem (9) is Pareto optimal. Now assume for simplicity that u_f and u_m are continuous, locally nonsatiated, and strictly quasi-concave. The household demand function is then given by

$$q^*(p,w,d) = \arg\max_{q\in\mathbb{R}_+^L} V(q,p,w,d) \text{ s.t. } p\cdot q \leq w. \qquad (10)$$

TABLE 8.—BA EFFECT ON EXPENDITURE LEVELS: IV METHOD

	General Services	Family	Vices	Grains	Vegetables	Fruits	Dairy	Meats	Oils, Spices, and Soft Drinks
BA dummy	−3.251	0.725	−1.157	5.552*	3.388***	2.806*	1.334	5.959	0.808
	(3.581)	(5.579)	(1.701)	(2.848)	(1.251)	(1.581)	(1.835)	(3.738)	(1.443)
Number of household members, BE, and municipality dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	998	998	998	998	998	998	998	998	998

IV regressions with constant; dependent variables in first row; controls in first column. Instrumented variable: BA participation dummy; instrumental variable: BA initial status dummy. Robust standard errors in parentheses. Significant at *10%, **5%, ***1%.

TABLE 9.—BA EFFECT ON EXPENDITURE SHARES: IV METHOD

	General Services	Family	Vices	Grains	Vegetables	Fruits	Dairy	Meats	Oils, Spices, and Soft Drinks
BA dummy	−0.012	−0.002	−0.003	0.004	0.007**	0.008**	−0.001	0.002	−0.004
	(0.008)	(0.009)	(0.003)	(0.007)	(0.003)	(0.004)	(0.005)	(0.007)	(0.004)
Number of household members, BE, and municipality dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	998	998	998	998	998	998	998	998	998

IV regressions with constant; dependent variables in first row; controls in first column. Instrumented variable: BA participation dummy; instrumental variable: BA initial status dummy. Robust standard errors in parentheses. Significant at *10%, **5%, ***1%.

An increase in female income affects the household demand through an increase in total income (income effect). But it could also influence household decisions through an empowerment effect due to the increase in the female income share, typically viewed as a distribution factor with important impact on the Pareto weight. When the Pareto weight does not depend on relative prices and DFs, the collective model reduces to the classical unitary model in which household preferences are represented by a single utility function defined on $\mathbb{R}^L_+$.

For our purpose, nonunitary models can be classified into two groups: one in which the Pareto weight depends on only prices and total income, hence demand functions are independent of DFs, satisfy income pooling, but may violate the Slutsky symmetry condition; and another group in which the Pareto weight, hence the demand functions, also depends on DFs such as the fraction of nonlabor income in the hands of the female member. In the latter case, the female empowerment resulting from participation in the BA program should affect household consumption decisions net of pure income effects.

VI. Identifying Female Empowerment 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. In principle, BA participation can affect household consumption patterns through two different economic channels: income effects and a potential increase of the influence of the beneficiary woman over household purchase decisions. In addition to these two economic effects, BA beneficiaries were also required to make regular visits to public health centers, which might have affected their consumption choices in some arbitrary way.

In order to disentangle potential empowerment effects from income and health-monitoring effects, we perform diff-in-diff analysis across groups of households for which empowerment effects have different intensity. Different subsamples are used for that purpose in order to check the robustness of the results.

A. Female Households

We use 77 households with no male adult as the comparison group in a diff-in-diff model. These households are typically composed of single mothers living with their children. No gender-specific empowerment effect should appear in their consumption decisions, since females already control all spending decisions.

Define F_i to be a dummy variable such that $F_i = 1$ indicates a household with no male adult (female households). The model in section IV is modified to be such that household i's outcome depends on F_i. When the household is enrolled in the program,

$$Y_{1,i}^{F_i} = \mu_1^{F_i} + \varepsilon_{1,i}^{F_i}, \tag{11}$$

and otherwise,

$$Y_{0,i}^{F_i} = \mu_0^{F_i} + \varepsilon_{0,i}^{F_i}. \tag{12}$$

We will use expenditure shares as the outcome variable Y_i in this section. For female households, the average impact of the BA program on Y_i is given by

$$\alpha^1 = \mu_1^1 - \mu_0^1. \tag{13}$$

TABLE 10.—DECOMPOSING THE BA EFFECTS ON EXPENDITURE SHARES

	General Services	Family	Vices	Grains	Vegetables	Fruits	Dairy	Meats	Oils, Spices, and Soft Drinks
BA × Single-Female Dummy	0.010	0.056*	−0.003	−0.029	−0.008	0.009	−0.009	−0.021	−0.004
	(0.024)	(0.030)	(0.006)	(0.025)	(0.015)	(0.010)	(0.014)	(0.026)	(0.012)
BA dummy	−0.012*	−0.011	−0.003	0.005	0.008***	0.007**	0.002	0.008	−0.004
	(0.007)	(0.008)	(0.003)	(0.006)	(0.003)	(0.003)	(0.004)	(0.006)	(0.003)
Single-female dummy	−0.031	−0.040	−0.020**	0.043*	0.016	0.002	0.008	0.016	0.006
	(0.025)	(0.027)	(0.008)	(0.025)	(0.016)	(0.008)	(0.014)	(0.026)	(0.012)
Number of household members, BE, and municipality dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,006	1,006	1,006	1,006	1,006	1,006	1,006	1,006	1,006

OLS regressions with constant; dependent variables in first row; controls in first column. Robust standard errors in parentheses. Significant at *10%, **5%, ***1%.

Similarly, for mixed-gender households, this average impact is given by

$$\alpha^0 = \mu_1^0 - \mu_0^0. \tag{14}$$

The observed outcome Y_i can be expressed as

$$Y_i = BA_i\big[F_i Y_{1,i}^1 + (1 - F_i)Y_{1,i}^0\big] \\ + (1 - BA_i)\big[F_i Y_{0,i}^1 + (1 - F_i)Y_{0,i}^0\big]. \tag{15}$$

This representation leads us to

$$Y_i = \beta_0 + \beta_1 BA_i F_i + \beta_2 BA_i + \beta_3 F_i + \upsilon_i', \tag{16}$$

where $\beta_0 = \mu_0^0$, $\beta_1 = (\alpha^1 - \alpha^0)$, $\beta_2 = (\mu_1^0 - \mu_0^0)$, $\beta_3 = (\mu_0^1 - \mu_0^0)$. The new error term is given by

$$\upsilon_i' = \varepsilon_{0,i}^0 + BA_i F_i\big[(\varepsilon_{1,i}^1 - \varepsilon_{0,i}^1) - (\varepsilon_{1,i}^0 - \varepsilon_{0,i}^0)\big] \\ + BA_i(\varepsilon_{1,i}^0 - \varepsilon_{0,i}^0) + F_i(\varepsilon_{0,i}^1 - \varepsilon_{0,i}^0). \tag{17}$$

In equation (16), the parameter β_1 captures the differentiated impact of the BA transfer on female households, in which the gender empowerment effect is necessarily absent. Consider, for instance, the case in which income and health-monitoring effects are homogeneous across female and mixed-gender households. Then, if female empowerment effects were in place, the β_1 parameter should be negative for goods typically preferred by women. That is, the relative effect of the BA transfer over expenditure shares of female-specific goods should be higher in mixed-gender households in which the empowered women would have increased influence over household decisions.

As in section IV, we control for the vector X_i, 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 (16) through an OLS regression. By assuming $E(\upsilon_i' \mid X_i) = X_i \cdot \theta$, the model becomes

$$Y_i = \beta_0 + \beta_1 F_i BA_i + \beta_2 BA_i + \beta_3 F_i + X_i \cdot \theta + \upsilon_i, \tag{18}$$

where $\upsilon_i = \upsilon_i' - E(\upsilon_i' \mid X_i)$.

The program assignment is conditionally random. Therefore, under the assumption that BA participation did not alter the fraction of female households, the error term υ_i is independent of the covariates in equation (18).

The OLS estimates appear in table 10. Notice that the estimates for β_1 are statistically nonsignificant for all categories except family expenses. However, for this category, the sign of the coefficient is positive, while it should be negative under the female empowerment argument.

B. Subsample of BE Nonparticipants

Recall that many households that applied and were eligible for the BA program had school-aged children and were also enrolled in the BE program. As in the BA program, the BE transfers are always made to the highest-ranking woman in the household. It is then possible that BE participants differed in important unobserved characteristics as they had children at schooling age and the women in such households would likely have already experienced some empowerment effect from the BE program. For this reason, we reestimated the previous regressions excluding BE participants from the sample.

The results are similar to the previous ones, as shown in table 11. In particular, the coefficients associated with the $BA_i F_i$ dummies are statistically insignificant in all regressions. Moreover, the estimated magnitude for the average impact of the program over expenditure shares is similar to those previously presented.

C. Subsample of Mixed-Gender Households

In many families, the eligible woman had some extra source of income apart from the BA transfer. The empowerment effect of the BA program could potentially be different for such households compared to those with women without an additional source of income. We exclude female households from the sample and divide the families with male and female adults into two groups: one in which the eligible woman had some extra source of income different from the BA transfer and another in which the BA transfer was the only resource of the eligible woman.

TABLE 11.—DECOMPOSING THE BA EFFECTS ON EXPENDITURE SHARES: SUBSAMPLE OF BE NONBENEFICIARIES

	General Services	Family	Vices	Grains	Vegetables	Fruits	Dairy	Meats	Oils, Spices, and Soft Drinks
BA × Single-Female Dummy	0.031	0.028	−0.004	−0.018	0.012	0.005	0.003	−0.057	−0.001
	(0.028)	(0.043)	(0.010)	(0.033)	(0.009)	(0.014)	(0.018)	(0.036)	(0.012)
BA dummy	−0.013	−0.005	−0.005	0.007	0.006*	0.007*	−0.004	0.012	−0.005
	(0.009)	(0.011)	(0.004)	(0.007)	(0.004)	(0.004)	(0.006)	(0.009)	(0.004)
Single-female dummy	−0.075**	−0.001	−0.025*	0.057*	−0.003	0.006	0.000	0.053	−0.012
	(0.030)	(0.040)	(0.014)	(0.033)	(0.011)	(0.012)	(0.018)	(0.037)	(0.011)
Number of household members, BE, and municipality dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	658	658	658	658	658	658	658	658	658

OLS regressions with constant; dependent variables in first row; controls in first column. Robust standard errors in parentheses. Significant at *10%, **5%, ***1%.

TABLE 12.—FEMALES WITH POSITIVE EXTRA-PROGRAM INCOME

	General Services	Family	Vices	Grains	Vegetables	Fruits	Dairy	Meats	Oils, Spices, and Soft Drinks
BA × Female-Income Dummy	0.007	−0.018	0.004	−0.009	−0.002	−0.004	0.014	0.008	0.000
	(0.014)	(0.016)	(0.006)	(0.012)	(0.005)	(0.006)	(0.010)	(0.014)	(0.007)
BA dummy	−0.018*	−0.006	−0.006	0.014	0.010**	0.010**	−0.004	0.004	−0.004
	(0.011)	(0.013)	(0.005)	(0.009)	(0.004)	(0.005)	(0.008)	(0.011)	(0.006)
Female-income dummy	−0.013	0.035**	−0.002	−0.006	0.000	0.007	−0.012	0.001	−0.01
	(0.014)	(0.016)	(0.006)	(0.012)	(0.005)	(0.006)	(0.010)	(0.013)	(0.007)
Number of household members, BE, and municipality dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	891	891	891	891	891	891	891	891	891

OLS regressions with constant; dependent variables in first row; controls in first column. Robust standard errors in parentheses. Significant at *10%, **5%, ***1%.

TABLE 13.—IDENTIFICATION TEST

	Single-Female Dummy (Full Sample)	Single-Female Dummy (BE Nonbeneficiaries)	Female-Income Dummy (Mixed-Gender Households)
BA dummy	0.006	0.000	0.038
	(0.014)	(0.017)	(0.026)
Number of household members, BE, and municipality dummies	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes
Number of observations	1,006	658	891

OLS regressions with constant; dependent variables in first row; controls in first column. Robust standard errors in parentheses. Significant at *10%, **5%, ***1%.

This leads us to a sample with 891 households (after excluding the 77 families with no male adult). Among them, 697 families receive the BA transfer. In 507 families, the eligible woman had an extra source of income (321 of whom were BA beneficiaries).

We test whether empowerment effects differ across the two groups of households. (We do not have a theoretical prediction for sign of this effect, since that would depend on the sensitivity of the Pareto weight for different levels of female income.) The estimates appear in table 12, and they bring no evidence of a differentiated empowerment effect across the two groups studied. In other words, although the BA program increased the consumption shares of vegetables and fruits (and reduced the expenditure share of general services such as utilities), this effect is not gender related. Under our maintained assumptions, this must be due to income effects (not fully captured by the budget-share econometric representation) or to the impact of the mandatory health monitoring activities.

D. Estimation Issues

Assessing the identification assumptions. The key identification assumption behind our empirical strategy in this section is that participation in the BA program did not affect the fraction of female households or the fraction of households whose women had an extra source of income. Naturally the BA program could have affected the family structure through changes in divorces and female labor supply.[8] We show, however, in table 13 that the conditional correlation between BA participation and the family structure is not statistically significant. This supports the identification assumption that potential changes in family structure that could compromise our empirical strategy have not been statistically important during the first six months of the program (when the data were gathered).

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

Additional robustness exercises. It is important to report two other classes of robustness exercises that we have performed. First, in order to account for potential correlation on unobservables of different equations, we have estimated the entire system of budget share equations by using Zellner's SURE method. The estimated coefficients are identical by construction, and the confidence intervals do not change considerably. The qualitative results are identical to those presented before.

Moreover, we have also included the household total expenditure as a control variable in OLS regressions equivalent to those presented in tables 5 to 6 and 8 to 12. Under the strong assumption that total expenditure is independent of an observable characteristics of the household, this exercise would disentangle potential income effects (captured by the total expenditure variable) from health-monitoring effects (which would remain captured by the BA dummy). The qualitative results are identical—that is, the main impact of the BA program (conditional on total expenditure) was an increase in the budget share allocated to vegetables and fruits and a decrease in the share allocated to general services (such as utilities). This suggests that compliance with vaccination schedules and regular visits to public centers for prenatal care, child-growth monitoring, and health and nutrition classes may be behind the observed increase in the consumption of healthy foods, such as vegetables and fruits.

VII. Conclusion

We study gender bias in the intrahousehold allocation of resources using data from a Brazilian social program, 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 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 mold of a random experiment.

We do not find evidence that household consumption decisions were affected by the fact that the program transfer was directed to a woman instead of a man. It is important to stress that the households in our sample are very poor and spend 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 gender-oriented transfers affect intrahousehold expenditure decisions.

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