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# Household Responses to Cash Transfers

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## Abstract

This paper exploits the experimental set-up of the cash transfer program PROGRESA in rural Mexico to estimate a collective model of the household in order to investigate how parents allocate household resources. We show that household decisions are compatible with the testable implications of the collective model, based on so-called distribution factors, at the beginning of the program, but reject them later on. We discuss a number of possible explanations for these findings and provide several arguments, consistent with our model, suggesting that this rejection may indicate that the treatment is not only empowering women, but possibly also changes the individual preferences.

**JEL Codes:** D13, I38, J12, J16, O15

**Keywords:** collective model, bargaining power, distribution factors, PROGRESA, conditional cash transfers.

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# 1 Introduction

Over the last decades, conditional cash transfer (CCT) programs have occupied a large percentage of governments' annual anti-poverty budgets (Fiszbein and Schady, 2009). PROGRESA, a CCT program implemented in rural Mexico in the late 1990s, is a prime example in which the cash transfers are exogenously targeted to mothers in order to give them a higher share of the household resources. It has been well documented that these large monetary incentives had a substantial effect on household behavior; see Hoddinott and Skoufias (2004), Bobonis (2009), Attanasio and Lechene (2014) and Angelucci and Garlick (2016) for some recent empirical results.

We exploit the experimental set-up of PROGRESA in order to structurally study how households respond to cash transfers in terms of the observed budget allocation of food. Focusing on the budget structure of food is a meaningful exercise as it accounts for around 80% of the expenditures of the targeted (poor) households in our sample. Moreover, Attanasio and Lechene (2014) convincingly show that the changes in the food decisions can not only be explained by the impact of the conditional cash transfer on household income, but are also due to changes in the intra-household decision process. In this paper we want to further investigate the latter, in particular to further explain the gradually impact documented in Hoddinott and Skoufias (2004).

The starting point of our analysis of the intra-household decision process is the collective model of the household, which was pioneered by Chiappori (1988, 1992) and Apps and Rees (1988) and further extended by Browning et al. (1994), Browning and Chiappori (1998), Blundell et al. (2005) and Chiappori and Ekeland (2006). In recent years this framework has become the main paradigm through which household allocation decisions are studied. There are two main reasons for this, which together make the framework suitable to study the distributional impact of public policies. First, the fundamentals of the model, namely individual preferences and the household decision process, can be identified under reasonable conditions (Chiappori and Ekeland, 2009). Second, the model is based on a small set of assumptions, mainly the (Pareto) efficiency of the household

In this paper, we estimate a theoretically consistent demand system on different subsamples and apply a test of the collective model developed by Bourguignon et al. (2009) (BBC hereafter).<sup>2</sup> This test is based on so called *distribution factors*, which impact the household decision process but do not change the preferences or the budget constraint. Data collected from randomized experiments are appealing to apply the BBC test, because these programs allow researchers to construct, in principle, exogenous distribution factors. In what follows we augment a structural QAIDS model a-la Banks et al. (1997) with two variables that the literature uses as credible distribution factors, and estimate it on household budget shares of food. The first distribution factor that we use is the treatment indicator. For the second distribution factor we follow Attanasio and Lechene (2014) by using data on the network of relatives present in the village. Subsequently, we run the BBC test by focusing on the most responsive demand equations with respect to the chosen distribution factors.

Our estimates show that households satisfy the testable implications of the collective model only in 1998, 6 months after the beginning of the program, but reject them if we use the data 12 months after the first cash transfer. This implies that our results are different from the existing evidence in favor of the collective model (see Bobonis (2009) and Attanasio and Lechene (2014)), but are in line with Hoddinott and Skoufias (2004). Our more precise conclusion with respect to the importance of heterogeneity across time (in terms of efficiency) are explained by (i) our focus on food and (ii) by the fact that our results are based on the inversion of the most responsive demand equations (see Section 5 for more details). The latter makes our statistical tests much more powerful.<sup>3</sup>

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<sup>1</sup>See Bourguignon et al. (1993); Browning et al. (1994); Browning and Chiappori (1998); Chiappori and Ekeland (2006) for testable implications in a parametric framework and Cherchye et al. (2007, 2009, 2011a) for a revealed preference approach.

<sup>2</sup>There is a long tradition on testing the Pareto efficiency hypothesis (i.e. the main hypothesis of the collective model) in a household context. Early papers find efficiency in commodity demand (Bourguignon et al. (1993), Browning et al. (1994), Browning and Chiappori (1998)), labor supply for childless couples (Chiappori et al. (2002), Vermeulen (2005)), children’s health (Thomas et al., 2002; Duflo, 2003) and female labor supply (Donni, 2007; Donni and Moreau, 2007). However, efficiency has been rejected in settings including household agricultural production (Udry, 1996), labor supply for couples with children (Fortin and Lacroix, 1997) and risk sharing activities (Dercon and Krishnan (2000), Robinson (2012)).

<sup>3</sup>In Appendix ?? we provide a comparison of the assumptions and statistical power across studies using PROGRESA data to test Pareto efficiency.

In principle, the rejection of the BBC test of the collective model in the second period leaves open a multitude of possible explanations, including the validity of auxiliary assumptions, strategic behavior or concerns about intertemporal commitment. However, as we discuss more in detail in Section 5.2, another possible explanation is that the PROGRESA program might not only impact the decision process (i.e. intra-household bargaining), but could potentially also affect the individual preferences over the period of observation. A change in preferences implies that the treatment variable can no longer be interpreted as a proper distribution factor, which only impacts the decision process. This demonstrates once more the difficulty of finding exogenous distribution factors. As a result the underlying assumptions of the BBC test are no longer valid, which may explain the rejection we find.

We like to highlight this alternative interpretation of changing preferences, given that it is in line with several pieces of existing evidence. First, and most importantly, there is ample empirical evidence of a change in preferences for different sorts of food items. Interestingly, as reported in Hoddinott and Skoufias (2004), the consumption of highly nutritious foods, such as fruits and vegetables, increased over and above the income effect, only 12 months after the start of the program. This is exactly consistent with the horizon over which we find evidence against the collective model. Second, and related to this, the cash transfers are provided to women in conjunction with an intensive training aimed at empowering them on several dimensions (e.g. on the importance of good quality food, but also on speaking up with respect to their rights, etc.). As it has been argued before, the impact of this training, which is a combination of more information and empowerment, only takes place gradually. Finally, survey questions in 1999 indicate that decision makers in households of the treatment group have statistically significant different aspirations and expectations than those of the control group. This indicates that cash transfers have second round effects that may be directly interpreted as changes in preferences, particularly if these aspirations and expectations are related to (food) decisions with respect to the children.

Although it is well documented that PROGRESA is likely to have an overall positive

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also provide further evidence to think more carefully, both empirically and methodologically, about second round effects of public interventions.<sup>4</sup> As such, our paper is also related to the treatment effect literature of CCT programs, which aims at identifying empirical facts on how to obtain the desired policy interventions. One of the focuses of this literature is to establish whether, why and to what extent targeting conditional cash transfers to women is effective (see Yoong et al. (2012) for a systematic review).

The rest of the paper is organized as follows. Section 2 describes the general theoretical framework, which motivates our empirical analysis. Section 3 discusses the data, the empirical strategy and the methodological issues related to the estimation of a demand system. Section 4 discusses our two potential distribution factors. Section 5 presents the results and motivates our interpretation in terms of changes in preferences. Section 6 concludes.

## 2 Theoretical framework

In this section we discuss the theoretical set-up of individuals’ interactions within the household and introduce the test of the collective model that we run in the empirical section. Consider a household comprising two decision makers  $i = m, f$  and any number of children, where  $m$  stands for mother and  $f$  for father. Children are not part of the decision making process and enter as a public good within the household. Household member  $i$  cares about her own private consumption  $\mathbf{c}^i$  and household public goods  $\mathbf{k}$ . Each member’s preferences are assumed to be representable by a continuously differentiable and strictly concave utility function  $U^i(\mathbf{c}^i, \mathbf{k}; \mathbf{d}, \epsilon)$ , where  $\mathbf{d}$  and  $\epsilon$  are a set of observable and unobservable characteristics that capture differences in preferences across individuals.

The resources of the family are derived from total household earnings  $x$ , potentially including an endowment entitled to member  $m$ . The budget constraint of the family can

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<sup>4</sup>Related to this, there is also empirical evidence that the program may have a negative impact by inducing a higher level of threats of violence and abuse of alcohol for some targeted households (see Angelucci (2008), Bobonis et al. (2013) and Bobonis et al. (2018)).

$$\mathbf{p}'\mathbf{c} + \mathbf{P}'\mathbf{k} = x, \quad (1)$$

where  $\mathbf{p}$  and  $\mathbf{P}$  are the price vectors of private and public goods respectively.

As is standard in the literature on household consumption models (see e.g. Chiappori and Mazzocco (2017)), we assume that the household chooses a Pareto efficient allocation of resources. Furthermore, similar to Attanasio and Lechene (2014), we impose the widely used assumption of separability between food consumption and other (in particular public) expenditures.<sup>5</sup> This implies we assume that households solve the following (static) optimization problem:

$$\begin{aligned} \max_{\{\mathbf{c}^m, \mathbf{c}^f\}} \quad & U^f(\mathbf{c}^f, \bar{\mathbf{k}}; \mathbf{d}, \epsilon) + \mu(\mathbf{z})U^m(\mathbf{c}^m, \bar{\mathbf{k}}; \mathbf{d}, \epsilon) \\ \text{s. t.} \quad & \mathbf{c}^m + \mathbf{c}^f = \bar{\mathbf{c}}, \end{aligned} \quad (2)$$

where  $\mu(\mathbf{z})$  is the (relative) Pareto weight summarizing the (relative) individual decision power of the mother. This Pareto weight depends on so-called distribution factors  $\mathbf{z}$  that affect the household decision process, but do not directly impact the individual preferences or the budget constraint. The resulting demand equation for a generic (private) good  $j$  then takes the following form:

$$\theta_j(\bar{\mathbf{c}}, \mathbf{z}; \mathbf{d}, \epsilon) = \xi_j(\bar{\mathbf{c}}, \mu(\mathbf{z}); \mathbf{d}, \epsilon). \quad (3)$$

The important difference with standard demand analysis, is the presence of the Pareto weight function  $\mu^i(\mathbf{z})$  and its functional dependence on distribution factors  $\mathbf{z}$ . If one can find variables  $\mathbf{z}$  that only effect  $\mu^i$  and not preferences, in other words if the variables in  $\mathbf{z}$  are not part of  $\mathbf{d}$ , then these distribution factors can be used to test Pareto efficiency, the main underlying assumption of collective models.

BBC derive necessary and sufficient conditions for collective rationality that are valid for any type of good, either private or public. In order to understand the theoretical

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<sup>5</sup>We also refer to this paper for a further discussion of this assumption in the present context of targeted cash transfer programs.



*mand* functions. Consider the demand for good  $j$  resulting from program (2),  $\theta_j = \xi_j(\bar{\mathbf{c}}, \mu(\mathbf{z}); \mathbf{d}, \epsilon)$ , where some of the elements of  $\mathbf{z}$  may not be observed but at least one is. In particular, assume that there is at least one good  $j$  and one observable distribution factor  $z_1$  such that  $\theta_j(\bar{\mathbf{c}}, \mathbf{z}; \mathbf{d}, \epsilon)$  is strictly monotone in  $z_1$ . Given strict monotonicity, the demand function for good  $j$  can be inverted on this factor:  $z_1 = \zeta(\bar{\mathbf{c}}, \mathbf{z}_{-1}, \theta_j; \mathbf{d}, \epsilon)$ . We can now define the following:

**Definition 1.** *The demand function for any good  $l$  is a  $z$ -conditional demand if:*

$$\theta_l = \theta_l(\bar{\mathbf{c}}, \mathbf{z}_{-1}, z_1; \mathbf{d}, \epsilon) = \theta_l(\bar{\mathbf{c}}, \mathbf{z}_{-1}, \zeta(\bar{\mathbf{c}}, \mathbf{z}_{-1}, \theta_j; \mathbf{d}, \epsilon); \mathbf{d}, \epsilon) = \varphi_l(\bar{\mathbf{c}}, \mathbf{z}_{-1}, \theta_j; \mathbf{d}, \epsilon). \quad (4)$$

In other words, the demand for good  $l$  can be written as a function of expenditure  $\bar{\mathbf{c}}$ , all distribution factors but the first,  $\mathbf{z}_{-1}$ , and the quantity demanded for good  $j$ . Although conditional demands are often used in demand analysis, it is useful to refer to it as  $z$ -demands because it incorporates the idea that distribution factors play a central role in the intra-household allocation stage of collective models. Empirically, the restriction that involves the  $z$ -conditional demand says that if there exists a distribution factor such that:

$$\frac{\partial \theta_j}{\partial z_1} \neq 0 \quad , \forall j, \quad (5)$$

the demand for good  $l$  is compatible with collective rationality if and only if there exists at least one good  $j$  such that:

$$\frac{\partial \varphi_l(\bar{\mathbf{c}}, \mathbf{z}_{-1}, \theta_j; \mathbf{d}, \epsilon)}{\partial z_p} = 0 \quad \forall j \neq l \quad \text{and} \quad p = 2, \dots, s. \quad (6)$$

The meaning of this testable restriction is the following. If we invert the demand for good  $j$  on a distribution factor  $z_1$ , which is also significant for any other good  $l \neq j$ , and we replace this demand into the demand of any other good  $l \neq j$ , the effect of any second distribution factor  $z_p$  is going to be irrelevant. The intuition is that, by definition, distribution factors affect demand only through their effect upon the location of the final

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outcome on the Pareto frontier. That is, they do not shift the Pareto frontier, because

they do not impact the individual preferences nor the budget constraint. This implies that the effect of the bargaining weight is one-dimensional. Once the location on the Pareto set has been changed by the effect of the first distribution factor, the information brought by any other additional distribution factor is therefore uninformative.<sup>6</sup>

### 3 Empirical implementation

We investigate how households respond to monetary incentives using a sample drawn from the surveys collected to evaluate the impact of PROGRESA. This is a conditional cash transfer program implemented in rural Mexico in the late 1990s. The choice of this dataset is motivated by a variety of reasons. First, the monetary incentives were quite large and had a real effect on households by inducing them to change their consumption patterns. Second, the surveys are very detailed and of high quality allowing us to construct vectors of quantity and prices for various important commodities. Third, the available dataset contains two variables that has been shown in the literature to impact the decision process. We will use them to investigate if they can be considered as distribution factors by using them to test the main hypothesis outlined in the theory part.

The remainder of this section is divided in three sub-sections. First, we provide some background information on the program, we present the evaluation surveys, how prices and quantities are aggregated, and some descriptive statistics of the sample used in our empirical analysis. Second, we discuss the consumption behavior of our sample, that is, household preferences and the observed demand equations, and outline the z-conditional demand system that we are going to estimate. The final sub-section deals with the estimation strategy and the methodological issues that have been raised in the literature when one aims to identify the relationship of interest with data coming from a cash transfer programs such as PROGRESA (e.g. Attanasio and Lechene (2002,

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<sup>6</sup>Note that Proposition 2 of BBC provides three equivalent necessary and sufficient conditions for collective rationality. Empirically, the condition that we use, involving z-conditional demands, is the most powerful one, because single equation methods are more robust than tests of the equality of parameters across equations.

endogeneity of both total expenditure and the number of children enrolled in secondary school.

### **3.1 Program design, sample selection and descriptive statistics**

The original PROGRESA program was implemented between April 1998 and December 2000. Later it was extended to include new households both in rural and urban areas. From its start, PROGRESA was subject to rigorous evaluation based on random assignment. 10,000 villages were included in the first expansion phase, of which 506 were selected in the evaluation sample, 320 of them were randomly chosen to have an early start of the program, whereas the remaining 186 formed the control group. In practice, households in the these latter villages were not included in the program until late 1999, which means that they became eligible for the grant only after this date. “Eligible” households in treatment villages started receiving the cash transfers subject to the appropriate conditionality already in April 1998. Whereas “eligible” households in control villages were still observed during this time, but they started benefiting from the payment (in the same manner) only after November 1999.

The main objectives of the program were to introduce incentives to improve the accumulation of human capital of children and at the same time to alleviate short-term poverty. To be eligible, a household must be sufficiently poor (in the program sense). The transfers were paid to the mother every two months and were largely in the form of scholarships to four grades of primary school, except the first two and the initial three grades of secondary school. These transfers are conditional on certain behavior: first, children must attend at least 85% of classes; second, household members must undergo periodic health checks; third, the transfer recipients must attend nutrition and health classes. The strong involvement of the mother in the program was motivated by the assumption that they have stronger preferences for child well-being and are more responsible for managing household resources. Moreover, a change in relative income of spouses was motivated by the desire to change the position of women within rural families in

In the present paper we use two post intervention surveys, October 1998 and June 1999, which were collected 6 months and 12 months, respectively, after the households started receiving the cash transfers. The surveys include detailed information on expenditures at the household level and detailed information on members of the household. In order to have an homogeneous sample on which to test the hypothesis of interest, we use a sub-sample that satisfies the following restrictions. First, there are only households with both natural parents in our sample and between one and six children. This means that households with at least one other adult member are excluded and the mother is always the recipient of the cash transfers. Second, households with children aged 17 or above are also excluded from the sample, in order to exclude households with multiple decision makers besides the parents. The resulting sample consists of 5,125 households observed in 1998 and 4,932 households observed in 1999. In Tables ?? and ?? in Appendix ??, we present the means of various household-level characteristics for our households in treatment and control villages in both waves. As we can see from these tables, households are disadvantaged in a number of important ways. First, the education of the head and the spouse is quite low, as the average adult has only slightly more than a primary school diploma. Second, families are quite large as the average number of children is slightly below 4. Third, almost 40% of the households have an indigenous origin. Finally, for only a quarter of the villages there is a secondary school.

We are interested in studying the household responses to cash transfers in terms of demand for different types of food, which, in our sample, represents about 80% of non-durable expenditure.<sup>8</sup> The demand for it is modeled assuming separability of these goods

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<sup>7</sup>The program was so much a success that later it was expanded to other households in rural areas who were followed throughout the 2000s, as well as households in urban areas. Other countries also adopted this kind of cash transfers program, both in Latin America, Asia, Africa, and even some developed countries. PROGRESA has been found to increase education attainment (Schultz (2004), Attanasio et al. (2013)), to decrease short term poverty (Tommasi and Wolf, 2016; Tommasi, 2017), and to improve health (Gertler (2004), Behrman and Parker (2011)). Detailed information on the program and its evaluations can be found in Skoufias (2005) and Fiszbein et al. (2009).

<sup>8</sup>We focus on demand for food for a variety of reasons. First and foremost, food consumption is the most important commodity in the budget of the expenditure of the households in the sample. Second, prices for the non-food consumption are not observed and hence it is practically impossible to use these goods.

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from the non-food consumption and labor supply. The PROGRESA data contains very

detailed information on both expenditure and consumption for many (narrowly defined) commodities. Following Attanasio and Lechene (2014), we use aggregated data to create budget shares of five different commodities: starches; pulses; fruit and vegetables; meat, fish and dairy; and other foods. As explained in detail by these authors, for each of the individual commodities that compose the five commodities that we use, consumption is computed as to include what has been bought as well as quantities obtained from own production, payments in kind and gifts.<sup>9</sup> The quantities are valued in pesos using village-level price information derived from unit values. Home produced consumption is also valued using local village-level unit values computed using information on purchases of the same commodities.<sup>10</sup>

We compute unit values of the five commodities which allow us to estimate the demand system. These are used to evaluate consumption in kind and to compute price indexes for each of the composite commodities. Unit values are computed for each household dividing the value of the purchase by its quantity. The value of the purchased commodity is computed by using village-level prices for individual commodities, where the village-level price is selected by looking at median unit value of the households that purchased that product in a given village. More details on the computation of these unit values and how price indexes are constructed can be found in Attanasio et al. (2013). This resulted in considerable variation in prices across villages and time in the data, which in turn allows us to get precise parameter estimates of the demand system.

## 3.2 Functional forms

In our empirical application we assume that households have preferences given by the integrable QAIDS demand system of Banks et al. (1997). QAIDS allows flexible prices responses and the quadratic income allows the Engel curves to display a great variety of

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<sup>9</sup>Notice that, although in principle it is important to control for consumption of home produced goods, only 6% of consumption is actually home produced.

<sup>10</sup>As for the issue of food consumption outside the home and food consumption inside the household by non-household members, since very few households in the sample have either of them, we control for this in the empirical analysis by correcting for their direct effect on the budget.

form:

$$V = \left\{ \left[ \frac{\ln x - \ln a(\mathbf{p})}{b(\mathbf{p})} \right]^{-1} + \lambda(\mathbf{p}) \right\}^{-1}, \quad (7)$$

where

$$\begin{aligned} \ln a(\mathbf{p}) &= \alpha_0 + \sum_{j=1}^n \alpha_j \ln p_j + \frac{1}{2} \sum_{j=1}^n \sum_{l=1}^n \gamma_{jl} \ln p_j \ln p_l, \\ b(\mathbf{p}) &= \prod_{j=1}^n p_j^{\beta_j}, \\ \lambda(\mathbf{p}) &= \sum_{j=1}^n \lambda_j \ln p_j. \end{aligned} \quad (8)$$

The parameters  $\alpha_j$ ,  $\beta_j$ ,  $\lambda_j$  and  $\gamma_{jl}$  ( $\forall j, l$ ) are to be estimated. Adding up requires that  $\sum_j \alpha_j = 1$ ,  $\sum_j \beta_j = 0$ ,  $\sum_j \lambda_j = 0$  and  $\sum_j \gamma_{jl} = 0$  (for all  $l$ ). Homogeneity is satisfied if  $\sum_l \gamma_{jl} = 0$  (for all  $j$ ).<sup>11</sup>

Applying Roy's identity to equation (7) we obtain the QAIDS budget share equations for each household and commodity  $j$

$$w_j = \frac{\theta_j}{x} = \alpha_0 + \phi' \mathbf{d} + \psi' \mathbf{z} + \sum_{l=1}^j \gamma_{il} \ln p_l + \beta_j \ln \left\{ \frac{x}{a(\mathbf{p})} \right\} + \frac{\lambda_j}{b(\mathbf{p})} \left[ \ln \left\{ \frac{x}{a(\mathbf{p})} \right\} \right]^2 + \epsilon_j, \quad (9)$$

where  $w_j$  indicates the  $j$ th budget share of a household facing a price vector  $\mathbf{p}$  and total expenditure level  $x$ , whereas  $\mathbf{d}$  and  $\mathbf{z}$  are vectors of, respectively, individual demographic characteristics and distribution factors. The impact of these variables runs through the coefficients  $\phi$  and  $\psi$ , whose estimates constitutes the main purpose of our empirical investigation. In principle both vectors  $\mathbf{d}$  and  $\mathbf{z}$  could of course affect the demand system in other ways, not necessarily through the intercept only. As a robustness check, we re-estimated the parameters of a general QAIDS model where demographic characteristics and distribution factors were allowed to change the curvature of the demand system in multiple ways. Almost all the additional parameters were not significant and did not impact the significance of the intercept, which indicates that it is not restrictive to focus only on changes in the intercept.

<sup>11</sup>As shown in Browning and Chiappori (1998), Slutsky symmetry no longer needs to hold, so we did not have to impose this. It would be satisfied if  $\gamma_{jl} = \gamma_{lj}$  ( $\forall j, l$ ).

In order to estimate the z-conditional demand for the budget share  $w_j$ , we have to allow

that the conditioning share  $w_l$  might be endogenous. This problem can be avoided because the excluded distribution factor on which the demand is inverted becomes a natural instrument for  $w_l$ . Let  $N$ , the relative family network, be the excluded distribution factor. The share for commodity  $l$  ( $l = 1, \dots, n$ ) can be inverted on this factor:

$$N = \frac{1}{\psi_N} w_l - \frac{\psi'}{\psi_N} \mathbf{z}_{-1} - \frac{1}{\psi_N} f_l(x, \mathbf{p}) - \frac{\phi'}{\psi_N} \mathbf{d} - \frac{1}{\psi_N} \epsilon_l,$$

where now  $\mathbf{z}_{-1}$  contains only the remaining distribution factor and, for notational simplicity,  $f_l(x, \mathbf{p}) = \sum_{j=1}^n \gamma_{lj} \ln p_j + \beta_l \ln \left\{ \frac{x}{a(\mathbf{p})} \right\} + \frac{\lambda_l}{b(\mathbf{p})} \left[ \ln \left\{ \frac{x}{a(\mathbf{p})} \right\} \right]^2$  for each good  $l$ . Substituting this equation for  $N$  in the share for all other goods results in the system of z-conditional demand functions:

$$w_j = \tilde{\alpha}' \mathbf{z}_{-1} + \tilde{\gamma} w_l + \tilde{\beta} f(x, \mathbf{p}) + \tilde{\phi}' \mathbf{d} + \tilde{u}_j \quad (10)$$

for all goods  $j \neq l$ . The test of collective rationality then boils down to a test of the significance of  $\tilde{\alpha}$ .

### 3.3 Endogeneity

Since our dataset comes from the evaluation of a cash transfer program, which has some important conditionality attached, the main methodological concern in estimating the demand system (9) is the endogeneity of total expenditure and child school enrollment. A further methodological concern is the non-linearity of the system, which makes the recovery of the parameter estimates more complicated. The latter issue is tackled by estimating the complete system with the iterated Feasible Generalized Non-Linear Least Squares (FGNLS) estimator. The former concern is tackled with a control function approach, as it is commonly applied in demand analysis (e.g. Blundell and Robin (1999)), where the residuals, estimated in the first stage, enter as a polynomial of second order. In the following paragraphs we explain the concern for each of the endogenous variables and how we deal with it.

For the endogeneity of total expenditure, notice that the implicit assumption behind

budgeting: first they decide how much to allocate to food and then how much to allocate to each of the five components of food. The residuals in (9) can be interpreted as the household's unobserved tastes that affect each budget share. There are two main arguments in the literature for why total expenditure  $x$  should be endogenous. One is that taste shocks which determine total expenditure  $x$  may be correlated with the unobserved shocks to a particular food component in the system. The other one is that measurement error in the budget shares may be correlated with measurement error in total expenditure. In the present paper we follow Attanasio and Lechene (2002, 2014) and instrument total expenditure  $x$  with the average agricultural wage in the village. This is a strong instrument and the implicit assumption in using it is that any measurement error in village-level income is not correlated with measurement error of household total expenditure, which is an assumption commonly used in the literature. As Attanasio and Lechene (2002, 2014) explain at length, this is a valid instrument if labor supply is separable from consumption. With respect to this, there is large evidence that PROGRESA did not affect adult labor supply and hence it is not a concern for us (e.g. Skoufias (2005)).

The second endogenous variable in system (9) is the number of children enrolled in school. As we explained before, eligible households receive a (large) portion of the grant if their children are enrolled and attend school. This conditionality requirement, which is controlled for in the demand equations, might affect consumption behavior if sending children to school imposes additional costs like books, uniforms, etc. Moreover, if children are fed in school, this would further impact the budget share of food. Enrollment in primary school is almost universal in rural Mexico and hence not affected by the grant. In order to allow for endogeneity of children in secondary school, we follow Attanasio and Lechene (2002, 2014) and instrument it with a dummy variable indicating the existence of a secondary school in the village and with the distance from the closest secondary school if no such school is present in the village. The implicit assumption made is that these two instrumental variables affect the schooling decisions of parents but not directly the structure of their expenditure on food.



Finally, before concluding this section, it is worth noticing that the QAIDS budget

share equations of the z-conditional demand depicted in equation (10) contains a third endogenous variable: the budget share of the conditioning good. As the conditioning good  $\theta_l$  is correlated with the unobserved taste shock of the demand for good  $\theta_k$ , this needs to be instrumented for. The natural instrument to use is already suggested by theory and by the z-conditional demand test that we run: the distribution factor used to invert the demand of the conditioning good satisfies the common requirements for valid instrumental variables. Hence, in estimating equation (10) we apply the same control function approach as before adding the residuals from the first stage of the conditioning good as well.

## 4 Potential distribution factors

In the present paper we want to investigate whether the eligibility to PROGRESA is only impacting the intra-household decision process or whether there is also evidence that it is impacting other channels. To perform this empirical exercise we need to find at least two variables that affect the allocation of resources but potentially not the preferences. These variables are called distribution factors and enter the Pareto weight function of the two agents within the household. Browning et al. (2014) report the most commonly used distribution factors in the literature. As these authors argue, it is a difficult exercise to find plausible distribution factors because theory does not give guidance as to what constitutes a distribution factor.

Our first, and most important, potential distribution factor is the eligibility to PROGRESA. This is a dummy variable taking value 1 if the household belongs to a treated village and 0 otherwise. Since the grant is targeted to the mother, receiving the transfers constitutes an exogenous increase in the share of the household income that she controls. This share of income is not an argument of preferences, and conditional on total resources available, it does not affect the budget constraint. Given the random assignment of the program, the treatment variable constitutes in principle an ideal distribution

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factor, which explains its popularity in the recent literature to test the collective model.

Note that the grant affects not only the distribution of resources within the household but also the total resources available. This implies that we need an appropriate specification of the demand system to control for total resources available after the treatment. Conditional on all the resources, including also those coming from the program, receiving the PROGRESA transfer should make no difference to the allocation of household resources among different commodities. If instead, after conditioning, the grant has a residual effect on allocation, it must be because it has shifted the demand as a consequence of a shift in the Pareto weights.

As second distribution factor we use the relative importance of the husband and wife's family network in the village. This information was collected by Angelucci et al. (2009) and used as a distribution factor to test the collective model by Attanasio and Lechene (2014). The main idea behind the use of the network information is the fact that a stronger presence of family members in the village affects the individual value of their outside option. Indeed, as these authors argue, it is possible that the relative weights of husband and wife in the allocation of resources depend, within the context of poor marginalized rural households, on the relative strength and influence of the two extended families in the village. The relative importance of the spouse's networks is constructed by Angelucci et al. (2009) as follows. The authors exploit the fact that the PROGRESA evaluation surveys are a census of each village and the convention of Spanish last names to map the network of relatives within each community. Indeed, in Spanish-speaking countries, individuals get two surnames, the first one from the father and the second one from the mother. Using the PROGRESA surveys it is possible to know the number of relatives, for each adult, that are present in the village. The relative importance of husband and wife's networks is then constructed in two ways: the size and wealth of the networks.<sup>12</sup>

At this point, one may be worried that, in the presence of altruism, if an adult member

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<sup>12</sup>More formally, for each individual  $i = m, f$ , they construct the relative size of the networks as the ratio of  $n_i/n_m + n_f$ , where  $n_i$  is either the number of relatives in the village or the value of their wealth for each individual  $i$ . Wealth is proxied by (food) consumption of the individual's relatives.

argue that if this adult has a relatively large family network, social norms may induce him or her to behave in a way that is closer to the preferences of the network. In other words, the number of siblings might affect preferences rather than bargaining. However, under the assumption that both adult members live under the same set of social norms, the construction of the distribution factor as a ratio of the two adults' network, would net away this concern. Next, concerning the effect on budget, the main reason why one could argue that the number of siblings in the village might have a direct effect on the demand for food is, if in rural Mexico it is common practice that siblings share meals. Although this fact would not invalidate that relative family network does not affect the budget, it does imply that if we do not account for the direct effect of the number of siblings on the demand for food, we might obtain biased estimates. Our empirical implementation avoids this potential bias because we indeed control for the number of relatives who share meals with the household as a determinant of expenditure shares.

Finally, we want to make two important remarks about our empirical implementation of the BBC test based on z-demands. First, the choice of the conditioning distribution factor and the conditioning good is crucial for the reliability of the empirical results. Theory indicates that the conditioning distribution factors must be statistically relevant and must affect the conditioning good monotonically.<sup>13</sup> In the empirical analysis we use the network variable as our preferred conditioning distribution factor, which satisfies all the requirements of the theory and is statistically significant in our own empirical exercise. Second, part of the discussion in the collective model literature is the nature and validity of the distribution factors used, whether discrete or continuous. We point out that, for the reliability of the results, it is important that the *second* distribution factor (the one on which the demand system is inverted on) is continuous. This is the case in our empirical

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<sup>13</sup>Historically, very few papers have rejected the collective model. This under-rejection of the efficiency hypothesis has been recently criticized by Dauphin et al. (2018). In relation to the z-conditional test, they argue that if we apply Bourguignon et al. (2009) strictly, the test requires that at least one distribution factor (locally) affects all demand equations. However, this assumption is hardly ever satisfied empirically, which means that test results are often based on the estimate of a parameter that is obtained by dividing two numbers that are very small. Therefore our test results are based on parameters attached to distribution factors that are both relevant for the household demand of both years.

## 5 Results and discussion

In this section, we first present the results of the BBC test of the collective model and show that, contrary to the existing literature, it is not rejected at the beginning of the program (first wave, 6 months after the start of the program), but it is rejected later in time (second wave, 12 months after the start of the program). Second, we discuss several possible explanations for these findings and provide suggestive evidence that one could interpret these results as an indication that the PROGRESA treatment may not only impacts the decision process, but also changes the individual preferences.

In all specifications we instrument total food expenditure with the village-level agricultural wage (and its square), and the number of children in secondary school with a dummy if there is a secondary school present in the village and the distance to the closest secondary school. We also control for a large set of pre-treatment village, household and individual characteristics. Village characteristics include the town size and prices. Household characteristics include the number of young children, the number of children enrolled in primary school, the number of children enrolled in secondary school, the number of relatives eating in the household and the number of household members eating outside the household. Individual characteristics include the level of education of both parents, the age of the household head and an indigenous head dummy. All the standard errors are clustered at the village level and bootstrapped 300 times.

### 5.1 Results of the BBC test

We first estimate the unconditional (QAIDS) demand system for various (sub)groups in our sample: respectively, the full sample, the subgroups defined by splitting the sample according to the two years in our dataset, and subgroups for each year based on different

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<sup>14</sup>Note that using the concept of “equivalent transfers” (i.e. transfers of non-labor income), Kapan (2009) shows that the identification results based on distribution factors are still valid if the distribution factor is discrete.

on the two cross-sections separately, because these are the only ones where there are at least two demand equations with two significant effects of the distribution factors. In all other subgroups that we have defined, the effects of the distribution factors are always too weak to provide reliable estimates of the z-conditional demand test, and hence no clear pattern was found.

The main parameters of interest are reported in Table 1. The estimated demand system is able to predict very well the observed budget allocation for both control and treatment groups in both periods, as reported in Table ?? of Appendix ?. Using these demand equations, we investigate whether the collective model is able to rationalize the observed budget allocation. In order to do so, we estimate z-conditional QAIDS demands by taking pairwise combinations of the demand equations that are responsive to the distribution factors. As we can see, these are *starches, fruits and vegetables* and *meat, fish and diaries* for the 1998 observation. Whereas for 1999 these are *starches, pulses, fruits and vegetables* and *other foods*. Hence, this means that in 1998 we first use *fruits and vegetables* as conditioning good, invert it on network and run the BBC test on the remaining goods where the treatment variable is significant.<sup>15</sup> Then use *meat, fish and diaries* to invert the system and test the model on the remaining goods. And so on for the remaining goods in 1998 and 1999. For completeness of the results, we report the estimates of all goods where at least one distribution factor is significant, but one should keep in mind that the most powerful results come from specifications where *both* demand equations are responsive to *both* distribution factors. Hence our preferred specifications and test results are the ones in columns (1) and (2) of Table 2. Note that these two goods represent 30% of the food budget in the first wave and 60% in the second wave.

[TABLE 1 ABOUT HERE.]

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<sup>15</sup>To verify if demand is monotone in family network, which we need to be able to invert, we added the squared value of this variable to our demand equations. These extra parameters turned out to be not statistically significant.

Table 2 shows that in 1998, 6 months after the first transfer, not only can we not reject the null hypothesis for all specifications, but also the magnitude of the coefficients always goes down, often close to zero, as theory predicts. In light of the model outlined before, this implies that we find convincing empirical evidence in favor of the collective model. A different story emerges, however, when we look at the 1999 data, 12 months after the households started receiving the cash transfers. In this case, the null hypothesis can be rejected in three out of six specifications and the magnitude of the z-conditional parameters never goes down to zero (as in the previous wave).<sup>16</sup>

As a robustness check, we attempted to estimate all the z-conditional QAIDS demands simultaneously and thereby performing a joint test of efficiency. However, due to the highly non-linear and collinear nature of the system, this implementation was not feasible. As an alternative, we estimate a linearized version of the z-demand system. This procedure, although imperfect, allows us to estimate the covariances of the parameters attached to the treatment indicator across the system. By doing so, we obtain that all these parameters are in magnitude smaller than  $10^{-4}$ . Interpreting this result as evidence in favor of the assumption that the treatment parameters might be independent *across* the system, we can construct a joint test statistic. This simply boils down to the sum of the squared t-statistics, which can be calculated using the information reported in Table 2. The resulting chi-square statistic for the two most responsive equations is equal to 4.54 in the 1998 wave and 15.97 in the 1999 wave. Therefore, on the basis of this joint test and the maintained assumption, we cannot reject the null hypothesis of efficiency in 1998, whereas we can reject it for 1999. We reach the same conclusions when we use all six equations. Although this is not the ideal joint test of the flexible QAIDS, it is reassuring that, under the maintained assumption of independence across equations, we can confirm the results for both 1998 and 1999.

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<sup>16</sup>Note that there is potentially a concern with weak instruments. Even though instrument strength is not that different from similar papers in the literature (i.e our chi-square statistics range from 13 to 28), it is also true that it is in most specifications borderline. See Table ?? in Appendix ?? for more details.

These results are somewhat different from those of the recent literature (in particular Bobonis (2009), Attanasio and Lechene (2014) and Angelucci and Garlick (2016)). This can be explained by several reasons. First, our sample selection strategy and variables choice is slightly different. Our main, and most informative, results focus on the two waves separately, while all the other papers pool the waves. As our empirical results demonstrate, they fail as such to fully capture the heterogeneity over time. Next, similarly to Attanasio and Lechene (2014), but differently from Bobonis (2009) and Angelucci and Garlick (2016), we use only two waves of data after PROGRESA began to distribute cash transfers and we focus on food consumption. The other authors use three waves and also model non-food consumption. The problem with this implementation is that the surveys do not contain information on prices for non-food commodities and hence it is not possible to implement the QAIDS model that we specified above. Finally, again similarly to Attanasio and Lechene (2014), but differently from Bobonis (2009) and Angelucci and Garlick (2016), we use treatment and relative size of the wives’ family network as distribution factors.

Besides these differences in the sample selection strategy and the variables choice, a second main difference is our implementation of the test of Pareto efficiency. As explained above, to implement the BBC test, one has to invert the demand equations. To obtain statistically reliable results, it is therefore crucial to have unbiased estimates and to focus on the most responsive demand equations. Therefore, our test is based on the parameters estimated from a fully fledged QAIDS model, whereas the other papers are based on a linear version of it, called  $\ell$ -QAIDS. Although the BBC test does not, in principle, require neither price variation, nor the estimate of the parameters attached to prices, bypassing a proper estimation of the demand system may lead to biases in the parameter estimates.<sup>17</sup> Next, with respect to inverting the demand functions, some of our sample

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<sup>17</sup>See, for instance, Pashardes (1993), Buse (1994), Moschini (1995), Buse (1998) and Matsuda (2006) for more discussion on how biased estimates of a demand system may or may not influence the empirical conclusions. In Appendix ?? we argue more in detail why the BBC test is an example where these biases

a consequence this makes the BBC test very unreliable, since (after the inversion) it is based on the ratio of two small numbers. This explains why we do not obtain similar conclusions in term of cross-sectional heterogeneity as in Angelucci and Garlick (2016).

As a final remark, one may be concerned that household composition (and hence sample composition) is changing through time in ways that might contribute to the change in efficiency through time. In particular: (i) some households may age out of the sample over time, (ii) transfer eligibility may induce some households to migrate (Stecklov et al., 2005; Angelucci, 2015), or (iii) transfer eligibility may change separation and cohabitation behavior (Bobonis, 2011).<sup>18</sup> In Appendix ?? we provide several arguments in support of the claim that none of these issues is impacting our results.

## 5.2 Interpreting these results in light of the collective model

In principle, the rejection of the BBC test, and thus the collective model, in the second period leaves open a multitude of possible explanations. Part of these are related to the decision process of the household and the corresponding underlying assumptions (see Baland and Ziparo (2017) for some recent empirical discussion in the context of developing countries). First, it could be interpreted as an indication of noncooperative (or strategic) behavior, which in turn leads to suboptimal decisions.<sup>19</sup> However, Chiappori and Naidoo (2017) show that distribution factors in a noncooperative model should satisfy the same testable implications as the ones we tested (in addition to some extra partial differential equations), which excludes this explanation and in our opinion the same conclusion extends to the so-called semi-cooperative models introduced in d’Aspremont and Dos Santos Ferreira (2014) and Cherchye et al. (Forthcoming).<sup>20</sup>

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may be influential.

<sup>18</sup>We thank an anonymous reviewer for pointing out these potential issues.

<sup>19</sup>In this context, noncooperative behavior stands for household decisions that are a Nash equilibrium in a public good game with voluntary contributions of the household members. Lechene and Preston (2011), Cherchye et al. (2011b) and d’Aspremont and Dos Santos Ferreira (2014) present testable implications of this model on the basis of price-income variation and Chiappori and Naidoo (2017) on the basis of distribution factors.

<sup>20</sup>Recently Lewbel and Pendakur (2019) introduces the notion of conditional efficiency to indicate that noncooperative behavior could be linked to changes in the household technology driven by choices of one of the partners. These changes alter the utility possibility set, which in turn interferes with the demand



Second, given that we study the heterogeneity of household behavior (in terms of compliance with the assumption of Pareto efficiency) over time, there is a possible need to extend our static framework to include intertemporal effects, while maintaining the assumption of exogenous bargaining weights. This would allow us, for instance, to focus on commitment in household decisions, which could in turn lead to an ex-post inefficient decision. As shown in Mazzocco (2007), the significance of our distribution factors indicate that there is only limited commitment. That is, due to changes in the distribution factors, participation constraints to stay inside the marriage become binding and trigger revisions in intra-household bargaining power. The rejection of the collective model could therefore be interpreted as an indication that households could not reach a new Pareto optimal outcome. Implying that the spouses should divorce. In Appendix ?? we discuss in more detail that there is however too little divorce for our data at hand to explain our results.

Third, one may be concerned that our results depend on our specific structural model, which excludes, for instance, household production, endogenous bargaining weights and imperfect information (see Basu (2006), Baland and Ziparo (2017) and Walther (2018)). This is of course a possible explanation and, relaxing some of the assumptions of the model, would in principle allow us to better grasp the mechanisms underlying the (potential) sources of (in)efficiency. Also, and in a similar vein, one may be concerned with our empirical strategy, which is based on the effects of two very specific distribution factors in a QAIDS model a-la Banks et al. (1997) and a limited dataset. Having access to better data, including alternative distribution factors or measures of preference shocks, would in principle allow for better empirically investigating the heterogeneity in (in)efficiency across households. For instance, we could not explicitly investigate preference changes due to lack of data or pursue further analysis at different subgroup levels (see e.g. Angelucci and Garlick (2016)) since the effects of our distribution factors were always too weak to provide statistically reliable test results.

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for food. This alternative interpretation can therefore also explain our rejections of Pareto efficiency. To properly test for this, one needs to extend our basic model to allow for a household technology (see Browning et al. (2013)) and to observe covariates related to endogenous changes in the household technology (e.g. increase in violence or alcohol consumption).

This being said, we can provide three related pieces of evidence, consistent with our collective model, which suggest that this rejection may indicate that the treatment is not only empowering women, but is also changing the individual preferences. This new interpretation implies that the treatment variable can no longer be interpreted as a proper distribution factor (that only impacts the decision process), which in turn implies that the BBC test is no longer valid. In terms of our structural model presented in Section 2, this suggests that the treatment dummy is not yet a preference shifter in the first period (i.e. it is not part of the  $\mathbf{d}$  variables), whereas it should be interpreted as a preference shifter in the second period.

First, and most importantly, as cited above there is ample empirical evidence showing that the treatment has changed the composition of food expenditures significantly over time. Interestingly, Hoddinott and Skoufias (2004) find that approximately one and a half years after the installment of PROGRESA there is a significant increase in caloric intake, whereas there seems to have been no significant effect in the first periods of treatment by PROGRESA. Importantly, they also show that the higher impact was found on the consumption of highly nutritious foods, such as fruits and vegetables, after controlling for income effects. The authors point to the training courses (*platicas*) of the PROGRESA program on health and nutrition issues as the main driving force for this dynamic effect.<sup>21</sup> This is exactly in line with the results we present in Tables 1 and 2.

Second, and related to this substantial educational component, mothers receive intensive educational and programmatic meetings, with the aim to empower them on several dimensions (e.g. on the importance of good quality food, but also on speaking up with respect to their rights vis a vis health care providers, etc). It could be that the impact of this training, which is a combination of more information and empowerment, may only take place gradually. In terms of our structural model, this means that mothers have a higher preference for expenditures on the public good, e.g. children, (see Barber and Gertler (2010)), which explains the drastic change in food expenditures over time. Although this is only circumstantial evidence it is in line with our results and those of

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<sup>21</sup>These kind of welfare programs have been coined "incentive-based welfare" (Gertler and Boyce, 2001).

Finally, the June 1999 survey round of the PROGRESA data, the one that gives us the rejection of the collective model, contains a series of questions regarding aspirations and expectations of decision-makers within the household. Attanasio and Lechene (2002) show that, after the implementation of the program, the answers to the decision making questions are substantially different between the treatment and control group. Most of these results are explained by the husbands in treatment villages making less decisions on their own. The magnitude of these differences are not very large, but they are statistically significant. Unfortunately these questions pertaining to decision making within the household were only asked in May 1999, so it is impossible to use them to test for dynamic effects in the change of intra-household decision making. Notwithstanding this limitation, it does suggest at least that the PROGRESA program may have significantly affected the households' way of thinking about joint decisions and public goods.<sup>22</sup>

Summarizing through the lens of our model, although PROGRESA is of course likely to have an overall positive effect on the welfare of children and women in rural Mexico, empowering women might have changed the preferences of the spouses in different ways. In this respect it also relevant to refer to the empirical findings on the incidence of violence and alcohol abuse among spouses in the targeted households (e.g. Angelucci (2008), Bobonis et al. (2013) and Bobonis et al. (2018)).<sup>23</sup> Not only does this formally exclude the underlying assumptions of the BBC test to verify the validity of the collective model, it is also intuitive that this may create frictions within the household that could lead to some second order negative effects in terms of suboptimal household decisions. This implies that the observed budget allocation of food can not be solely explained by an induced shift of bargaining power towards the mother, but should be accompanied by (empirical) models that allow for changing preferences over time. Although our interpretations are

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<sup>22</sup>Unfortunately our data, with the unique family network variable, does not contain the household IDs, which implies that we could not directly integrate this extra information in our empirical analysis. The same applies to linking explicitly households in both waves.

<sup>23</sup>Angelucci (2008) notes that the likelihood to receive more violent threats is related to the size of the cash transfers received by the household. Similar arguments, albeit in different contexts from PROGRESA, have been put forward by Hidrobo and Fernald (2013) and Ramos (2017). Moreover, experimental evidence suggesting behavioral changes arising as a result of cash transfers have been found also in a recent paper by Almas et al. (2018).

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rather suggestive, at the same time they indicate multiple interesting avenues for further research.

## 6 Conclusion

We structurally analyzed whether the collective model can rationalize the demand equations of food for a sample of households affected by the PROGRESA conditional cash transfer program. This CCT program was implemented in rural Mexico in the late 1990s and targeted poor families. The large monetary incentives had a substantial effect on households' behavior inducing them to change their food consumption patterns. As shown by Attanasio and Lechene (2014) this change can only be explained by the impact of the conditional cash transfer on the intra-household decision process.

In this paper we further investigated this impact. Based on the test introduced in Bourguignon et al. (2009) we show that households are consistent with the collective model only in 1998, 6 months after the beginning of the program, but reject the test 12 months after the first cash transfer. We discuss several potential explanations for this rejection and provide suggestive arguments that our findings may indicate that the PROGRESA program is not only impacting the decision process, but may also be changing individual preferences over time. This in turn is an indication of the invalidity of using the treatment variable as a proper distribution factor. The differences in our results with those of the existing literature demonstrate the need for using a fully flexible demand system in order to capture the impact of price variation. Moreover, our paper also shows that in order to obtain a powerful and reliable application of the BBC test of the collective model, it is crucial that both demand equations are responsive to both distribution factors.

Furthermore, our results are suggestive for the need of new structural models, including intertemporal and/or noncooperative features, to capture the impact of so-called distribution factors, and corresponding empirical evidence to analyze second round effects of CCT programs such as PROGRESA. Alternatively, one could also fully integrate

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the marriage market in a structural model in order to circumvent the use of distribution

factors (e.g. Cherchye et al. (2017)). Related to our empirical findings, future policy intervention, such as CCT programs, could be complemented with a measurement of the preferences for public goods (e.g. children) of the parents both at the beginning of the intervention and after some time. This would allow to estimate explicitly their impact on the demand for private and public goods and to disentangle changes in preferences from changes in the household decision process. Subsequently, this could then be used to (structurally) investigate the (un)observed heterogeneity of the impact of the policy intervention on the individual well-being of the recipients.

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Table 1: Unconditional (QAIDS) demand system

<b>Budget shares</b>	starches	pulses	fr. & veg.	m., f. & d.	other foods
Distribution factors: <span style="float: right;">October 1998, 6 months after the 1st transfer</span>					
Treatment	0.020** (0.009)	0.004 (0.006)	-0.012** (0.005)	-0.016** (0.008)	0.004 (0.003)
Network	-0.013* (0.007)	-0.005 (0.004)	0.011*** (0.004)	0.013** (0.005)	-0.006 (0.004)
Joint test of:					
Treatment	10.24 (p-value = 0.04)				
Network	18.54 (p-value = 0.00)				
Distribution factors: <span style="float: right;">June 1999, 12 months after the 1st transfer</span>					
Treatment	-0.049*** (0.007)	-0.021** (0.010)	0.021*** (0.005)	0.007 (0.006)	0.041*** (0.003)
Network	0.013** (0.007)	0.003 (0.003)	-0.000 (0.003)	-0.002 (0.005)	-0.013*** (0.004)
Joint test of:					
Treatment	88.55 (p-value = 0.00)				
Network	13.16 (p-value = 0.01)				

Notes: We report only the parameter estimates (and standard deviation) of the main distribution factors. Network refers to the relative family network of the wife. The sample size in the two waves is 5,125 and 4,932 observations, respectively. In all specifications we instrument total food expenditure with the village-level agricultural wage (and its square), and the number of children in secondary school with a dummy if there is a secondary school present in the village and the distance to the closest secondary school. We control for a large set of pre-treatment village, household and individual characteristics. Village characteristics include the town size and prices. Household characteristics include the number of young children, the number of children enrolled in primary school, the number of children enrolled in secondary school, the number of relatives eating in the household and the number of household members eating outside the household. Individual characteristics include the education of both parents, the age of the household head and an indigenous head dummy. All the standard errors are clustered at the village level and bootstrapped 300 times. Under joint tests of each distribution factor we report the chi-square statistic and p-values for the tests. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table 2: z-conditional demand (BBC) test

	October 1998, 6 months after the 1st transfer											
	(1) m., f., & d. QAIDS z-cond.		(2) fr. & veg. QAIDS z-cond.		(3) starches QAIDS z-cond.		(4) starches QAIDS z-cond.		(5) m., f., & d. QAIDS z-cond.		(6) fr. & veg. QAIDS z-cond.	
Treatment	-0.016** (0.008)	0.007 (0.010)	-0.012** (0.005)	-0.002 (0.006)	0.020** (0.009)	0.011 (0.013)	0.020** (0.009)	0.006 (0.010)	-0.016** (0.008)	-0.003 (0.010)	-0.012** (0.005)	0.010 (0.006)
Conditioning good	Both d.f. significant at $p \leq 0.10$				At least one d.f. significant at $p \leq 0.10$							
p-value	fr. & veg. 0.46				fr. & veg. 0.41				m., f. & d. 0.51			
Treatment											starches 0.11	
Joint test:												
All equations	$\chi^2(6) = 4.54$ [p-value = 0.60]											
Most responsive eq.	$\chi^2(2) = 0.60$ [p-value = 0.74]											
	June 1999, 12 months after the 1st transfer											
	(1) starches QAIDS z-cond.		(2) other foods QAIDS z-cond.		(3) starches QAIDS z-cond.		(4) fr. & veg. QAIDS z-cond.		(5) starches QAIDS z-cond.		(6) other foods QAIDS z-cond.	
Treatment	-0.049*** (0.005)	-0.042*** (0.011)	0.041*** (0.003)	0.026 (0.022)	-0.049*** (0.005)	-0.034 (0.051)	0.021*** (0.003)	0.041* (0.022)	-0.049*** (0.005)	-0.034*** (0.010)	0.041*** (0.003)	0.106 (0.149)
Conditioning good	Both d.f. significant at $p \leq 0.10$				At least one d.f. significant at $p \leq 0.10$							
p-value	other foods 0.00				fr. & veg. 0.51				starches 0.06			
Treatment											pulses 0.00	
Joint test:												
All equations	$\chi^2(6) = 32.16$ [p-value = 0.00]											
Most responsive eq.	$\chi^2(2) = 15.97$ [p-value = 0.00]											

Notes: We use network as conditioning distribution factor. We report only the parameter estimates (and standard deviation) of the treatment indicator. The sample size in the two waves is 5,125 and 4,932 observations, respectively. All regressions contain the same set of regressors as outlined in Table 1. All the standard errors are clustered at the village level and bootstrapped 300 times. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

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## Appendix

In this Appendix we report additional information and discussion of the main results of the paper. We preferred to leave this further analysis here to minimize the length of the manuscript. The information in this Appendix is organized as follows. Section A.1 reports some useful information related to the recent literature testing Pareto efficiency using PROGRESA data. Section A.2 reports the summary statistics of our sample of households (i) in the treatment and control villages and (ii) in wave 1 and 2. Section A.3 discusses why estimating QAIDS (in contrast to  $\ell$ -QAIDS) may be important to test for Pareto efficiency. Section A.4 reports the table of results of the first stage regressions of the control function approach. Section A.5 shows that our model fits the data very well. Finally, Section A.6 reports additional robustness checks related to the sample composition over time.

### A.1 BBC tests of the collective model using PROGRESA

Table A.1: Review of the literature

Paper	Waves	Sample	Budget shares	Demand specification	D.F.	Significance of D.F.: Chi-square (p-value)	
B2009	Oct-98, Jun-99 and Nov-1999	Pooled	Food and non-food	l-QAIDS	PROGRESA Rainfall shocks	-	-
AL2014	Oct-98, Jun-99	Pooled	Food	l-QAIDS	Network PROGRESA	91.53 (p<0.00)	48.22 (p<0.00)
AG2016	Oct-98, Jun-99 and Nov-1999	Split by age	Food and non-food	l-QAIDS	Sex ratio PROGRESA	-	-
DPT2018	Oct-98, Jun-99	Split by time	Food	QAIDS	Network PROGRESA	Oct-98 16.15 (p<0.00)	Jun-99 11.88 (p<0.02) 70.00 (p<0.00)

Notes: This table compares the main papers testing collective rationality using PROGRESA data, in terms of their main features and statistical power. B2009 refers to Bobonis (2009), AL2014 refers to Attanasio and Lechene (2014), AG2016 refers to Angelucci and Garlik (2016) and DPT2018 refers to our paper. “D.F.” refers to distribution factors. Each paper uses two distribution factors, the first in line is the distribution factor used to invert the demand system and the second in line is the distribution factor used to test collective rationality. The “Significance of D.F.” is computed by looking at each D.F. separately across all the demand equations in the system. p<0.05 for the first D.F. means that the test statistic satisfies the requirement of BBC. We report the symbol “-” when the test statistic is either not provided by the authors or not possible for us to replicate.

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## A.2 Summary statistics

Table A.2: Summary statistics: Treatment vs Control-1998 wave

Variables	Observations	Control	Observations	Treatment	Difference
Town size	1,949	42.20	3,176	39.70	2.49***
N children in primary school	1949	1.39	3,176	1.42	-0.04
Household size	1,949	5.64	3,176	5.68	-0.04
N of children	1,949	3.64	3,176	3.68	-0.04
N of young children	1,949	2.29	3,176	2.33	-0.04
N of older children	1,949	1.35	3,176	1.35	0.00
Education of the spouse	1,949	2.17	3,176	2.18	-0.01
Education of the head	1,949	2.25	3,176	2.26	-0.02
Head is indigenous	1,949	0.39	3,176	0.39	0.00
Age of head	1,949	37.23	3,176	37.03	0.21
ln(price of starches)	1,949	1.27	3,176	1.29	-0.02***
ln(price of pulses)	1,949	2.43	3,176	2.42	0.01**
ln(price of fruit and vegetables)	1,949	1.93	3,176	1.91	0.01***
ln(price of meat, fish and diary)	1,949	2.68	3,176	2.69	-0.01***
ln(price of other foods)	1,949	2.39	3,176	2.37	0.02***
Secondary school	1,949	0.26	3,176	0.26	0.00
Network	1,949	0.42	3,176	0.41	0.01
Education	1,949	1.01	3,176	1.01	0.01
Expenditure on food	1,949	739.79	3,176	803.75	-63.96***

Notes: Mean values and differences between eligible households in control and treatment villages. The data refer to the 1998 wave. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table A.3: Summary statistics: Treatment vs Control-1999 wave

Variables	Observations	Control	Observations	Treatment	Difference
Town size	1858	40.67	3074	38.85	1.82**
N children in primary school	1858	1.44	3074	1.51	-0.06*
Household size	1858	5.64	3074	5.68	-0.04
N of children	1858	3.64	3074	3.68	-0.04
N of young children	1858	2.27	3074	2.31	-0.04
N of older children	1858	1.36	3074	1.37	0.00
Education of the spouse	1858	2.19	3074	2.21	-0.02
Education of the head	1858	2.27	3074	2.3	-0.03
Head is indigenous	1858	0.4	3074	0.38	0.01
Age of head	1858	37.68	3074	37.54	0.14
ln(price of starches)	1858	1.23	3074	1.28	-0.05***
ln(price of pulses)	1858	2.31	3074	2.32	-0.01***
ln(price of fruit and vegetables)	1858	1.66	3074	1.64	0.02***
ln(price of meat, fish and diary)	1858	2.73	3074	2.75	-0.02***
ln(price of other foods)	1858	2.3	3074	2.29	0.01**
Secondary school	1858	0.26	3074	0.26	0.00
Network	1858	0.42	3074	0.42	0.01
Education	1858	1.01	3074	1.01	0.01
Expenditure on food	1858	687.73	3074	815.96	-128.23***

Notes: Mean values and differences between eligible households in control and treatment villages. The data refer to the 1999 wave. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.



Table A.4: Summary statistics: 1st vs 2nd wave

Variables	October 1998	June 1999	Difference
Age, Head	37.104	37.591	-0.487**
Education, Head	2.257	2.291	-0.034*
Education, Spouse	2.172	2.202	-0.029
Indigenous, Head	0.388	0.390	-0.001
Household size	5.664	5.664	0.000
# young children	2.315	2.299	0.016
# old children	1.350	1.365	-0.016
Relative eating in	0.066	0.105	-0.039**
Member eating out	0.014	0.036	-0.022***
# children in primary	1.410	1.482	-0.073***
# children in secondary	0.332	0.293	0.039***
Family network	0.417	0.418	-0.001
Treatment	0.620	0.623	-0.004
Town size	40.651	39.534	1.117**
Dummy secondary school	0.258	0.258	0.000
Guerrero	0.082	0.080	0.002
Hidalgo	0.158	0.163	-0.005
Michoacan	0.129	0.124	0.004
Puebla	0.156	0.166	-0.010
Queretaro	0.041	0.038	0.003
San Luis Potosi	0.151	0.143	0.008
Veracruz	0.284	0.286	-0.003
Observations	5,125	4,932	

Notes: Mean values and differences between eligible households observed in October 1998 and June 1999. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

### A.3 Do we need to estimate QAIDS to implement the BBC test?

To explain the point that using  $\ell$ -QAIDS may not be appropriate to implement the BBC test of the collective model, assume that we have a system of two demand equations, derived from a QAIDS model, and two distribution factors  $T$  and  $N$ . As in the main text, assume that the distribution factors enter only as an intercept in the demand equations. The two structural demand equations are:

$$w_1 = \alpha_1 + \psi_1 T + \delta_1 N + f_1(x, \mathbf{p}) + \epsilon_1,$$

$$w_2 = \alpha_2 + \psi_2 T + \delta_2 N + f_2(x, \mathbf{p}) + \epsilon_2,$$

where  $f_i(x, \mathbf{p}) = \sum_{j=1}^2 \gamma_{ij} \ln p_j + \beta_i \ln \left\{ \frac{x}{a(\mathbf{p})} \right\} + \frac{\lambda_i}{b(\mathbf{p})} \left[ \ln \left\{ \frac{x}{a(\mathbf{p})} \right\} \right]^2$ , for  $i = 1, 2$ . In order to construct the z-conditional demand system, we invert the demand  $w_2$  on the second distribution factor,  $N$ , substitute this equation in the demand for good 1, and simplify the expression as follows<sup>24</sup>:

$$w_1 = \tilde{\alpha}_1 + \tilde{\psi}_1 T + \tilde{f}(x, \mathbf{p}, w_2) + \tilde{\epsilon}_1, \quad (\text{A.1})$$

where the parameter of interest is:

$$\tilde{\psi}_1 = \left( \psi_1 - \frac{\delta_1 \psi_2}{\delta_2} \right). \quad (\text{A.2})$$

Suppose now that the data are generated by a QAIDS model, but we estimate the following  $\ell$ -QAIDS of 2 equations:

$$w_1 = \hat{\alpha}_1 + \hat{\psi}_1 T + \hat{\delta}_1 N + \hat{f}_1(\hat{x}) + \hat{\epsilon}_1,$$

$$w_2 = \hat{\alpha}_2 + \hat{\psi}_2 T + \hat{\delta}_2 N + \hat{f}_2(\hat{x}) + \hat{\epsilon}_2,$$

where, slightly abusing the notation from before, we have that:

- $\hat{f}_i(\hat{x}) = \sum_{j=1}^2 \gamma_{ij} \ln p_j + \beta_i \ln \hat{x} + \lambda_i \ln \hat{x}^2$
- $\hat{x} = \frac{x}{P^*}$

<sup>24</sup>Which is a simplification of the following fully specified equation:

$$\begin{aligned} w_1 &= \alpha_1 + \psi_1 T + \frac{\delta_1}{\delta_2} w_2 - \frac{\delta_1 \alpha_2}{\delta_2} - \frac{\delta_1 \psi_2}{\delta_2} T - \frac{\delta_1}{\delta_2} f_2(x, \mathbf{p}) - \frac{\delta_1}{\delta_2} \epsilon_2 + \delta_1 N + f_1(x, \mathbf{p}) + \epsilon_1 \\ &= \left( \alpha_1 - \frac{\delta_1 \alpha_2}{\delta_2} \right) + \left( \psi_1 - \frac{\delta_1 \psi_2}{\delta_2} \right) T + \frac{\delta_1}{\delta_2} w_2 + \left( f_1(x, \mathbf{p}) - \frac{\delta_1}{\delta_2} f_2(x, \mathbf{p}) \right) + \left( \epsilon_1 - \frac{\delta_1}{\delta_2} \epsilon_2 \right). \end{aligned}$$

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- $\hat{\alpha}_i = \alpha_i - \beta_i \xi_0 - \frac{\lambda_i \xi_0}{P^* + \xi_0}$

- $\hat{\epsilon}_i = \epsilon_i - \beta_i [\xi_i - \xi_0] - \frac{\lambda_i}{P^* + \xi_0} [\xi_i^2 - \xi_0^2]$

for  $i = 1, 2$ . Like before, we now invert the demand  $w_2$  on the second distribution factor, and substituting this equation for  $N$  in the demand for good 1 results in the  $\ell$ -QAIDS equivalent of the z-conditional demand function:

$$w_1 = \tilde{\alpha}_1 + \tilde{\psi}_1 T + \tilde{f}(\hat{x}, w_2) + \tilde{\epsilon}_1, \quad (\text{A.3})$$

where the parameter of interest is:

$$\tilde{\psi}_1 \neq \tilde{\psi}_1. \quad (\text{A.4})$$

Equation (A.4) tells us precisely why our empirical approach is important. If we believe that the indirect utility functions of our households should be more flexible with respect to prices, then the true parameter we are after is  $\tilde{\psi}_1$ . However  $\ell$ -QAIDS recovers  $\tilde{\psi}_1$ . These two parameters differ and it is not intuitive in which direction the bias of  $\tilde{\psi}_1$  is going.

## A.4 First stage

Table A.5: First stage results

	(1)	(2)	(3)	(4)
	Log of household expenditure		# children in secondary school	
	1998	1999	1998	1999
Log(village wage)	-0.267** (0.111)	-0.853*** (0.171)	0.436*** (0.154)	0.500** (0.217)
Log(village wage) <sup>2</sup>	0.183*** (0.044)	0.327*** (0.065)	-0.156** (0.061)	-0.184** (0.083)
Distance sec. school	0.010*** (0.004)	-0.003 (0.004)	-0.018*** (0.005)	-0.016*** (0.005)
Dummy sec. school	0.012 (0.018)	-0.010 (0.019)	0.053** (0.025)	0.045* (0.024)
Town size	-0.001*** (0.000)	-0.001*** (0.000)	-0.000 (0.000)	-0.000 (0.000)
# children in primary	0.062*** (0.006)	0.076*** (0.006)	0.049*** (0.008)	0.048*** (0.007)
# young children	0.012** (0.005)	0.012** (0.005)	-0.091*** (0.007)	-0.088*** (0.007)
Education, Spouse	-0.026 (0.024)	0.039 (0.024)	-0.028 (0.033)	-0.024 (0.031)
Education, Head	-0.008 (0.024)	-0.032 (0.024)	0.050 (0.033)	0.043 (0.031)
Indigenous, Head	-0.091*** (0.015)	-0.060*** (0.016)	-0.014 (0.021)	-0.015 (0.020)
Age, Head	0.003*** (0.001)	0.004*** (0.001)	0.012*** (0.001)	0.011*** (0.001)
Relative eating in	0.022* (0.011)	0.008 (0.006)	-0.006 (0.016)	-0.009 (0.007)
Member eating out	0.074* (0.044)	-0.012 (0.026)	0.109* (0.061)	0.020 (0.034)
Log(price of starches)	0.485*** (0.053)	0.102** (0.045)	-0.039 (0.073)	-0.009 (0.057)
Log(price of pulses)	0.703*** (0.099)	1.470*** (0.203)	0.238* (0.136)	0.858*** (0.257)
Log(price of fr. & veg.)	-0.257*** (0.054)	-0.541*** (0.066)	0.012 (0.075)	-0.128 (0.083)
Log(price of m., f., & d.)	-0.044 (0.042)	0.081* (0.044)	0.132** (0.058)	0.027 (0.056)
Log(price of other foods)	0.281*** (0.045)	-0.383*** (0.046)	-0.129** (0.063)	-0.034 (0.059)
Treatment	0.083*** (0.012)	0.130*** (0.013)	0.061*** (0.017)	0.038** (0.016)
Family network	-0.045*** (0.017)	-0.012 (0.017)	-0.027 (0.023)	-0.026 (0.022)
Constant	2.238*** (0.345)	3.241*** (0.470)	-1.009** (0.476)	-2.166*** (0.596)
	Joint test results:		Joint test results:	
Relevant instruments	Village wage		School proximity	
Chi-square	28.76	12.93	16.36	14.16
p-value	0.00	0.00	0.00	0.00
Observations	5,125	4,932	5,125	4,932
R-squared	0.116	0.160	0.129	0.124
Controls	Yes	Yes	Yes	Yes

Notes: \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

## A.5 Fit of the data

Table A.6: Actual and Predicated effect of PROGRESA, full sample

		1998						
		Actual			Predicted			Predicted - Actual
		C	T	D*100	C	T	D*100	D*100
starches	<i>mean</i>	0.40	0.40	-0.20	0.41	0.40	-0.65	-0.45
	<i>sd</i>	0.15	0.14	0.20	0.03	0.03	0.05	0.20
pulses	<i>mean</i>	0.13	0.12	-0.91	0.13	0.12	-0.72	0.20
	<i>sd</i>	0.08	0.08	0.11	0.02	0.03	0.04	0.12
fr. & veg.	<i>mean</i>	0.13	0.14	0.48	0.12	0.13	0.69	0.20
	<i>sd</i>	0.09	0.08	0.12	0.02	0.02	0.03	0.12
m., f., & d.	<i>mean</i>	0.16	0.17	1.15	0.16	0.17	1.17	0.02
	<i>sd</i>	0.13	0.12	0.18	0.05	0.05	0.06	0.19
other foods	<i>mean</i>	0.18	0.17	-0.52	0.18	0.18	-0.49	0.02
	<i>sd</i>	0.10	0.09	0.13	0.04	0.04	0.05	0.14
		1999						
		Actual			Predicted			Predicted - Actual
		C	T	D*100	C	T	D*100	D*100
starches	<i>mean</i>	0.43	0.41	-1.91	0.42	0.41	-1.26	0.65
	<i>sd</i>	0.15	0.15	0.21	0.04	0.04	0.05	0.22
pulses	<i>mean</i>	0.11	0.10	-0.92	0.11	0.10	-1.14	-0.22
	<i>sd</i>	0.07	0.07	0.10	0.02	0.02	0.03	0.11
fr. & veg.	<i>mean</i>	0.10	0.11	1.07	0.11	0.12	0.88	-0.19
	<i>sd</i>	0.07	0.07	0.10	0.02	0.02	0.02	0.10
m., f., & d.	<i>mean</i>	0.16	0.19	2.32	0.17	0.19	2.11	-0.21
	<i>sd</i>	0.13	0.13	0.18	0.05	0.05	0.07	0.19
other foods	<i>mean</i>	0.19	0.18	-0.56	0.18	0.18	-0.60	-0.04
	<i>sd</i>	0.10	0.10	0.14	0.04	0.04	0.06	0.15

Notes: Predicted impacts computed using the QAIDS model. C, T and D stand for Control and Treatment groups and Difference between the two.

## A.6 Sample composition through time

One may be concerned that household composition (and hence sample composition) is changing through time in ways that might contribute to the change in efficiency through time. In what follows we provide several arguments in support of the claim that this issue is not a concern for us.

First, it is useful to point out the main PROGRESA rules to receive the more conspicuous part of the grant: the one related to education. In short, a household is eligible to receive the grant depending on four main conditions: 1) age range of the children (prior to enrollment to the new school year); 2) number of completed years of education (prior to enrollment to the new school year); 3) enrollment to the new school year; 4) attending at least 85% of the classes. The schooling decision of children is taken during the summer, and if she complies with the rules, the mother will receive the entitled money every two months. Since we are using two waves within the same school year, if a child complies with 1)-4), the mother will be eligible throughout the period of observation. If either condition 3) or 4) are not met during the school year in progress, at some point the mother will stop receiving the money. If this is the case, then we are able to control for it with the variables “# children in primary” and “# children in secondary”. It is not clear what would happen if 1) is not met anymore during the school year in progress. However we can check for the number of these marginal students as we explain next.

Second, let us consider Table A.4 of summary statistics in the Appendix A.2. Here we compare the mean in October 1998 and June 1999 for the most important covariates in our dataset. As one can see, for most of the characteristics, the differences over time are either non statistically significant or very small. In particular, the household size, the number of young children (0-5 years old) and the number of older children (6 years or more), are practically the same in the two waves. This tells us that the proportion of households eligible to different subsets of the PROGRESA grant, depending on the age range of their children, remains fixed over time. Hence there seems to be no evidence in our sample that a significant portion of children of eligible households “ages out” from PROGRESA, or changes eligibility status.

Third, one other concern is related to migration. Stecklov et al. (2005) show that PROGRESA reduces U.S. migration but not domestic migration. Whereas Angelucci (2015) finds that PROGRESA increases Mexican migration to the U.S. In either case, if there is migration

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 and this happens to a different extent among different type of households, residing in different

states, we may be able to provide evidence for it in a simple way. That is, by looking at the fraction of households living in different states, a change in the number of relatives eating in the household, or the number of members eating out of the household. As for the former, there is virtually no change in the proportions between 1998 and 1999. As for the latter, the number of households with at least one relative eating in the household goes from 146 in 1998 to 131 in 1999. Whereas the number of households with at least one member eating out the household goes from 131 to 144 during the same time. In total, only 4% of households in 1998 has at least one in or out, and only 5.6% in 1999 has at least one in or out. To be sure that these possible outliers, which might be correlated with the problem of migration, are not driving the results, we re-estimate the demand systems for both 1998 and 1999 without these households. As Table A.7 below shows, point estimates for the large majority of the parameters are virtually unchanged, and in particular they do not change the effects of the distribution factors on the budget shares. This reassures us that this concern does not affect the sample composition in a significant manner.

Table A.7: QAIDS without households with members eating in or out

	starches	pulses	fr. & veg.	m., f., & d.	other foods
<i>October 1998</i>					
Treatment	0.019*** (0.005)	0.004 (0.003)	-0.010*** (0.003)	-0.017*** (0.004)	0.004 (0.003)
Network	-0.011* (0.006)	-0.005* (0.003)	0.010*** (0.004)	0.013*** (0.005)	-0.007* (0.004)
Observations	4,921	4,921	4,921	4,921	4,921
<i>June 1999</i>					
Treatment	-0.048*** (0.005)	-0.018*** (0.005)	0.021*** (0.003)	0.004 (0.004)	0.041*** (0.003)
Network	0.013** (0.006)	0.003 (0.003)	-0.001 (0.003)	-0.003 (0.005)	-0.012** (0.004)
Observations	4,665	4,665	4,665	4,665	4,665

Notes: We report only the parameter estimates (and standard deviation) of the distribution factors. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Finally, Bobonis (2011) documents that the overall share of women in union does not change as a result of the program, whereas marital turnover increases. However, what he finds is that intact families eligible for the transfers experience a very modest increase in separation rates, from 0.47% (23) households, to 0.80% (63) households in the treatment group. Although we do

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not have information on the marital status of our couples, we believe that these numbers are

very small to be able to argue that there is going to be a substantial change in household (and hence sample) composition over time.