Efficient responses to targeted cash transfers

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Abstract

The unitary model has been rejected many times. In this paper, we start from one such rejection in the context of rural Mexico and propose a test of one of the main alternatives to the unitary model, the so called collective model, a model that assumes that intrahousehold allocations, however, determined, are efficient. The test we propose requires the consideration of a demand system with at least two commodities and at least two distribution factors, that is, variables that affect expenditure shares while not affecting preferences or budget constraints. Conditional cash transfer programmes, which have become extremely popular in developing countries and target poor households to receive large cash transfers, under certain conditions, offer a particularly attractive contexts because transfers are often targeted to women. Moreover, in Mexico, the CCT impacts were evaluated by randomly assigning 'treatment' and 'control' localities to an 'early' and 'late' start, therefore providing us with exogenous variation in a plausible distribution factor. The programme affects expenditure shares even after controlling for the additional resources given to the eligible households, therefore indicating a rejection of the unitary model of household behaviour. We also find that the relative strength of the family network of household members affects household choices. Having identified two 'distribution factors', we show that they do so in a way that is consistent with the collective model: they affect expenditure shares proportionally, indicating that they enter the demand system through a single factor. We test this hypothesis using z-conditional demands embedded within a Quadratic Almost Ideal Demand System for food components.

Keywords: Intrahousehold allocation, collective rationality, social experiment, conditional cash transfers, QUAIDS, food.

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Contents

1 Introduction

There is a growing consensus that households decisions are not accurately represented by the so-called unitary model, which assumes that the household acts as a single decision unit maximizing a common utility function. Many implications of the unitary model have been soundly rejected in empirical applications, although in many cases one could think of possible explanations of the empirical findings that would salvage the model. The main reason for this remaining ambiguity is that many tests focus on the role of what in the literature is referred to as distribution factors.[1] These are variables that do not affect preferences, prices or resources and, therefore, under the unitary model, should not affect the allocation of resources. The issue is then to identify variables whose variation is arguably not related to preferences and resources.

If one assumes that intrahousehold allocations are determined by the interaction of different agents with different objectives, then the issue is to characterize these allocations when one knows little of the bargaining processes that go on inside the household. An attractive approach is the so-called collective model proposed by Chiappori (1988), which does not take a stand on the specifics of intrahousehold decisions but only assumes that allocations are efficient. Among others, Browning and Chiappori (1998) and more recently Bourguignon, Browning and Chiappori (2009) have shown that this model does, in principle, impose strong restrictions on the data. Many of these restrictions, however, require the identification of multiple distribution factors, which can be difficult to observe in practice. In particular, it is difficult to find data containing information on variables that can be plausibly interpreted as distribution factors and whose variation is exogenous with respect to individual tastes.

The main innovation of this paper is to provide a test of the collective model in a context where we can identify two plausible distribution factors. Moreover, the variation of at least one of the factors we consider is, by construction, exogenous, as it is driven by the randomization implemented to evaluate a welfare programme. This context, therefore, constitutes a unique and novel opportunity

[1] There is a bit of a semantic issue here. In some papers, distribution factors are understood to be any factor that affects the intrahousehold allocation of resources. Here and throughout this paper, we mean by a 'distribution factor' a variable that affects the intrahousehold allocation of resources and does not affect either the budget constraint nor preferences. Under a unitary model, therefore, a distribution factor should not enter demand equations.

to provide a strong test of the collective model.

Since the implementation in 1998 of *PROGRESA* (subsequently renamed as as *Oportunidades*) in Mexico and (around the same time) of the predecessor of *Bolsa Familia* in Brazil, conditional cash transfer (CCT) programmes have been put in place in many developing countries and have become extremely popular. They have been shown to result in important reductions in poverty. Beneficiary households in many countries, ranging from Mexico to Brazil, to Colombia and others, have been shown to enjoy higher consumption, increased school enrolment and better children nutritional status (see Fiszbein and Schady, 2009).[2]

An important feature of most of the CCT programmes that have been implemented in many countries is that the transfers are targeted explicitly to women, often with the explicit objective to change the condition of women within the household. The mother of the children associated with the programme receives the cash transfers (and participates to the program's activities). The programme, therefore, explicitly and deliberately changes the control of resources within the households, increasing the share of total income controlled by women. Moreover, because of the programme, women are involved in new activities that imply that they go out more and have more frequent connections with other women in the locality. This structure makes it possible that the programme changes the balance of power within the household and, as a consequence, the allocation of resources. Implicit in this argument is, of course, that the allocation of resources within the households is a function of who controls them, a clear violation of the so-called unitary model.

The evaluation of many of CCT programmes has brought to light a remarkable fact: following the injection of cash in the budget of poor households induced by CCTs (in Mexico, about 20% of household income), as total expenditure and consumption increase as expected, the consumption of food increases, proportionally, at least as much, so that the share of food among beneficiaries either increases or stays constant. This contradicts the standard view that, as a necessity, food has an income elasticity less than unity so that when total

[2] In the case of PROGRESA/*Oportunidades,* there are many papers that have looked at the impacts of the programme on various outcomes. The initial impact evaluation was carried out by IFPRI and its results are summarized in Skoufias (2001). Other papers in this literature include Skoufias and Mcclafferty (2001) Skoufias and Parker (2001) and Schultz (2004).

consumption increases, the share of food should decrease. This fact has been documented in the context of the urban version of the Mexican programme by Angelucci and Attanasio (2009, 2012), in rural Mexico by Attanasio and Lechene (2010), in the context of a similar programme in Colombia by Attanasio, Battistin and Mesnard (2012), in the case of a cash transfer programme in Ecuador by Schady and Rosero (2008) and in Nicaragua. A recent World Bank Policy Research Report (see Fiszbein and Schady, 2009) documents the same phenomenon in other countries.

In Attanasio and Lechene (2010), we document the fact that the food budget share does not decrease in rural Mexico as total consumption increases as a consequence of the programme and rule out a number of reasons why this could be, such as price increases, changes in the quality of food consumed and homotheticity of preferences as explanations for this puzzle. By estimating a carefully specified Engel curve, we show that food is indeed a necessity, with a strong negative effect of income on the food budget share. In other words, higher levels of income or total expenditure are associated (in a cross section of observations not yet affected by a CCT) with lower levels of the food share.

In the case of PROGRESA/*Oportunidades*, therefore, as income and total consumption are increased substantially by the programme, the tendency of the food budget share to go down is counterbalanced by some other effect of the programme so that the net effect is nil. Whilst PROGRESA/*Oportunidades* is a complex intervention with many components, we argue that the programme has not changed preferences and that there is no labelling of money. We propose that the key to the puzzle resides in the fact that the transfer is put in the hands of women and that the change in control over household resources is what leads to the observed changes in behaviour. In this sense, the evidence points to a substantial and strong rejection of the unitary model, as we have argued in Attanasio and Lechene (2002).

In this paper, we take the rejection of the unitary model as given and use the same data to test the collective model. In particular, we ask if the effect that PROGRESA/*Oportunidades* and other distribution factors have on the demand of different commodities is consistent with the restrictions imposed by the collective model. The shift in the Engel curves induced by the programme is strong and well documented both in our case and in that of other CCTs. As we discuss below, one way to see our exercise is to ask whether the collective model can explain these shifts in the Engel curves. In this sense, our evidence constitutes a very strong test, both because some of the variation we use is truly random and because we burden the collective model with the task of explaining a strong shift in behaviour. A first important and original contribution of this paper, therefore, is to use the exogenous variation generated by the random assignment of a welfare programme to test the collective model.

We implement these ideas using the same data from the evaluation of PROGRESA we used in Attanasio and Lechene (2002) and in Attanasio and Lechene (2010). The execution of the exercise, however, is not trivial, as it has to solve a number of difficult issues. First, we need to identify a plausible distribution factor apart from the (exogenously determined) participation to the programme. One of the innovations of this paper is to use a variable that measures the relative bargaining strength of the husband and wife within the household by using data on the network of relatives present in the village and their wealth. Second, as in Attanasio and Lechene (2002), to guarantee that PROGRESA can be considered a genuine distribution factor, in addition to total expenditure we have to control for behaviour induced by the conditionalities of the programme and in particular school enrolment. The demand functions are all conditional on the school enrolment behaviour of children of various ages. We allow enrolment to be simultaneously (and endogenously) determined with the demand functions. Third, we need to deal with the issues that arise with the estimation of a demand system, and, in particular, with the endogeneity of total expenditure and, in the case of the conditional demand function, of the conditioning good. To control for the endogeneity of total expenditure, we use a control function approach, as discussed in Attanasio, Battistin and Mesnard (2012). To control for the endogeneity of the conditioning good, we use the excluded distribution factor.

Our main findings can be summarized as follows. Being in a village (randomly) targeted by PROGRESA turns out to have an important effect on the expenditure shares we model, over and above the effect of total consumption (which is also affected by the programme). Moreover, we find that our additional distribution factor (the relative size of husband and wife's networks) also enters significantly the demand system. These results can be interpreted as yet another rejection of the unitary model. However, we find that these two distribution factors enter in the five equation demand system in a proportional fashion, consistently with the predictions of the collective model. In particular, when we test the restriction that the PROGRESA program is not significant in what Bourguignon, Browning and Chiappori (2009) have defined as z-conditional demand, we cannot reject the null that the living in a PROGRESA village does not affect z-conditional demands. This is equivalent to testing a set of proportionality restrictions which are the necessary and sufficient conditions of the collective model. We conclude that the data are not inconsistent with the collective model. This finding is also confirmed by the fact that observed changes in consumption shares are not statistically different from the predictions using the program impacts on total consumption and the estimates of a demand system which allow the distribution factors to affect its intercepts.

The main contribution of our paper is not the rejection of the unitary model, which has been rejected many times already. The main point of the paper is that the collective model can explain a clean, specific and strong deviation from the unitary model. The rejection we consider is particularly salient because the variation in the control of resources is by construction exogenous.

The rest of the paper is organized as follows. In section 2, we present the framework and the theoretical results on which the empirical analysis is based. We show the form taken by the demand functions in the case of two distinct hypothesis on the intra-household negociation process: unitary rationality and collective rationality. We also present the tests of collective rationality based on *z conditional* demands. In section 3, we present the economic context and the data, a sample of poor households from the Mexican population randomly drawn to receive or not to receive large cash transfers. We then document the fact that motivates the analysis: the absence of effect of large cash transfers on the structure of the budget, in section 3.5. In section 4, we discuss our distribution factors. In section 5, we discuss the methodological issues pertinent to the estimation of a demand system in the context of a CCT programme. In section 6, we present the empirical results: we estimate a demand system to evaluate the impact of *Oportunidades* on food consumption, and we present tests of efficiency of decisions, using the conditional approach derived in Browning, Bourguignon and Chiappori (2009) within a modified Quaids. Section 7 concludes.

2 Theoretical framework

We consider households with 2 adult decision makers[3] A and B. There are n private consumption goods on which the household can spend, q_i^A and q_i^B, where q_i^j denotes the private consumption of good i by agent j and $i = 1, ..., n$, and Q denotes the m vector of household consumption of public goods. Household consumption of good i is $q_i = q_i^A + q_i^B$. Vector q^A is the vector of private good consumption of individual A and similarly for B. Household private consumption is $q = q^A + q^B$. Individual preferences are defined on the consumption of private goods and public goods, and they also depend on a set of demographic taste shifter d, called preference factors $v^A(q^A, q^B, Q; d)$ and $v^B(q^A, q^B, Q; d)$. Denoting exogenous total expenditure by x, the budget constraint is

$$p'(q^A + q^B) + P'Q = p'q + P'Q = x \tag{1}$$

where p and P are the price vectors of private and public goods respectively.

Individual preferences are in general not identical so that there must exist some mechanism by which households reach decisions. We consider two such mechanisms. One leads to a standard unitary model and the other to a general collective model. We show how the demand functions differ in these two cases. In what follows, we will denote ζ_i the demand function for good i, irrespective of whether it is a private or public good when we discuss properties which are shared by public and private goods. Browning, Chiappori, Lechene (2006) give a detailed discussion of the distinction between unitary and collective models when there are price variations.

2.1 Demand functions in the unitary model

One way to rationalise a unitary model based on individual preferences is to assume that households maximise a weighted sum of individual preferences where the weights are fixed.

$$Max_{q^A,q^B,Q} \mu v^A(q^A, q^B, Q; d) + (1 - \mu) v^B(q^A, q^B, Q; d) \tag{2}$$

[3] This assumption is not as restrictive as it may appear. First, a major part of the sample of poor households we consider are composed of a couple with any number of dependent relatives (children and others). Second, a number of the tests we describe can be extended to the case of households with any number of decision makers. For ease of exposition, we here limit the discussion to the case of nuclear households.

subject to the budget constraint (1). With *fixed* weights μ, this is equivalent to assuming the existence of a utility function $U(q^A, q^B, Q; d)$ which, maximised, gives rise to demand functions $\zeta_i(x, p, P, d)$ for $i = 1, ..., n$. [4]The quantity demanded for any good i depends on total expenditure x, prices p and P and taste shifters d. For well behaved individual utility functions, the demand functions must satisfy adding up, homogeneity, symmetry and the Slustky matrix of compensated price responses must be negative semi definite.

2.2 Demand functions in the collective model

A well known alternative to the unitary model is the so-called collective model (Chiappori 1988, 1992). Unlike in the unitary model, it is not assumed that the weights given to the utility of each individual in the household are fixed, but that they can vary with a variety of factors. The only restriction imposed on the negotiation mechanism in the collective model is that it yields efficient allocations of resources, that is that outcomes are Pareto efficient, given the preferences of the individuals in the household.

Within the collective model, the weights μ in equation (2) can depend on a variety of factors, including prices and factors that affect the budget constraint. We furthermore assume that there exist some observable factors z which play a role in the negotiation but do not affect either the budget constraint or individual preferences. Following the literature, these are called distribution factors. Notice that while variables that affect the weights but also enter the budget constraint or affect preferences (such as prices or total income) might be rationalized within the unitary model, distribution factors should not appear in the demand functions associated with such a model. Therefore, variables that can be plausibly be defined as distribution factors, are extremely useful to test the collective model as an alternative to the unitary model. As we argue below, if one can identify more than one distribution factor, one can construct powerful tests of the collective model as well, in that the model imposes strong restrictions on the way these factors enter the demand functions.

When there exist multiple distribution factors z, Pareto efficiency implies

[4] The representation of the unitary model in equation (2) is not the only possible and is somewhat restrictive. Most of the restrictions of the unitary model, such as income pooling, can be obtained from the maximization of a generic function $W(v^A, v^B)$. We use this representation to relate it to our formulation of the collective model, where μ depends on distribution factors.

restrictions on the manner in which they affect demand. These restrictions follow from the fact that distribution factors, as they do not affect preferences or budget constraints, enter only through the index that defines the relative weights of the two adults in the Pareto problem. Household decisions can be represented as resulting from the maximisation of a generalised household welfare function, subject to the household budget constraint (1):

$$Max_{q^A,q^B,Q}\mu(x,p,P,d,z)v^A(q^A,q^B,Q;d)+(1-\mu(x,p,P,d,z))v^B(q^A,q^B,Q;d) \tag{3}$$

For any good, private or public, the demand function for good i derived from the programme above is $\xi_i(x,p,P,d,z)$ which depends on total expenditure x, prices, p and P, preference factors d and distribution factors z. Demand functions in the collective model satisfy adding up and homogeneity. However, it is well known that they do not satisfy symmetry, but rather that the Pseudo Slustky matrix of compensated price responses is the sum of a symmetric matrix and a matrix of rank 1 (Browning, Chiappori, 1998).

When discussing tests of the collective model in the next section, we assume that it is possible to find a set of variables which are incontroversially distribution factors. Whether it is possible to find any such variables is of course an important question. In the absence of a theory of marriage and of the determination of power, the decision whether a given characteristic is treated as a distribution factor z or as a preference factor d is an (untestable) identifying assumption.

2.3 Tests of collective rationality

Tests of collective rationality differ depending upon whether the data contains price variation or not, and whether distribution factors are observed. We focus here on tests with distribution factors.

Browning, Bourguignon and Chiappori (2009) show that testing for collective rationality is equivalent to testing any of the following three conditions:

$$\xi_i(x,p,P,d,z)=\Xi_i(x,p,P,d,\mu(x,p,P,d,z)) \qquad \forall i=1,...,n \tag{4}$$

$$\frac{\partial\xi_i/\partial z_k}{\partial\xi_i/\partial z_l}=\frac{\partial\xi_j/\partial z_k}{\partial\xi_j/\partial z_l} \qquad \forall i,j,k,l \tag{5}$$

$$\frac{\partial\theta_i^j(x,p,P,d,z_{-1},C_j)}{\partial z_k}=0 \qquad \forall i\neq j, \quad \text{and } k=2,...,K \tag{6}$$

The first condition states that the functional form of the demand function is restricted so that the distribution factors only affect demands through an index. The second condition is a proportionality restriction which states that the ratio of partial derivatives of the quantities demanded with respect to the distribution factors have to be equal across goods. This restriction follows easily from the first and has been tested for instance in Bourguignon et al. (1993).

To derive the final condition, let us assume that there exists at least one good j and one observable distribution factor z_1 such that $\xi_j(x,p,P,d,z)$ is strictly monotonic in z_1. Then invert demand for j so that $z_1=\zeta(x,p,P,d,z_{-1},C_j)$. Replacing z_1 by this expression in the demand for any other good i, one obtains the $z-$ *conditional* demand for good i :

$$C_i=\xi_i(x,p,P,d,z_1,z_{-1})=\theta_i^j(x,p,P,d,z_{-1},C_j). \tag{7}$$

From this, the third condition is easily derived. It states that, conditional on C_j, the demand for any C_i should be independent of any z_k (other than z_1). Note that because the unobservables of the demand for C_j now appear in the demand for C_i, the former is endogenous in the demand for C_i. One obvious instrument for C_j is the omitted distribution factor z_1. Note also that all these tests require at least two distribution factors and at least two demand functions. It should also be stressed that one of the distribution factors has to be such that one can invert one of the demand functions: one therefore needs a continuous factor and that one demand function is monotonic with respect to that factor.

In this paper, we implement a test of collective rationality based on $z-$ *conditional* demands.The main difficulty in implementing such a test is the identification of two variables that can be plausibly labeled as distribution factors. One of the innovative features of this paper is the fact that we work with two such variables, which we discuss at length in Section 4. To our knowledge, there are no other such tests in the literature, apart from Bobonis (2009). However, whilst Bobonis developed some of the same ideas independently, his implementation is problematic. There are two main issues with his approach. Firstly, he uses rainfall as a distribution factor without justifying how rainfall could affect the intra-household allocation of resources in the Mexican context. Secondly, the distribution factor z_1 he uses to invert the demand function is an indicator variable indicating assignment to Progresa. A functional inversion requires a continuous distribution factor. We develop our criticisms in detail in Appendix A.

There are two parts to our approach to 'test' of the collective model. First, we show how that, unlike a unitary model, the collective model, once we consider explicitly the two distribution factors that we described (and, obviously, in particular the first one) can predict the impact of PROGRESA upon budget shares. Of course the impacts will be estimated with some error and one may argue that the failure to reject the collective model is a lack of power that comes both from the imprecision of the impact estimates and the imprecision with which we estimate the model's coefficients.

The second part of our approach takes a different tack and constitutes very powerful evidence in favour of the collective model. We start from a rejection: the fact that the coefficient on the PROGRESA dummy is strongly significant while (within the unitary model) it should not be. This effect is strong and it has been documented in many papers, both by us and others (see, for instance, Schady and Rosero (2008), and Angelucci and Attanasio (2012)). Conditional cash transfers targeted to women seem to shift Engel curves (rather than causing a movement in the demand of different commodities along an Engel curve). We show that within the framework of the $z - conditional$ demands that use a distribution factor completely unrelated to PROGRESA (the relative size of spouses networks), we can explain this shift. In other words, the BBC test which we implement and that uses the second distribution factor to construct the $z - conditional$ demands is able to account for the shift in Engel curves induced by the program. The coefficient on PROGRESA does not just become insignificant, but it goes down in size. That is, by considering the conditional demand system we are not just adding noise, we are actually explaining the shift in the Engel curves.

Notice that the consideration of two distribution factors is crucial here. If PROGRESA was the only distribution factor, we could not go further than the rejection of the unitary model and the collective model would saturate the data. Instead we are testing the hypothesis that under the collective model all distribution factors are channelled through a unique index (the Pareto weight or the sharing rule). This imposes a considerable amount of structure on the data and could in principle be rejected.

3 PROGRESA and its evaluation surveys

The data set we use is unique for a variety of reasons. First, it is a survey which has been collected to evaluate the impact of a welfare programme in part motivated by the desire to change the position of women within rural families in Mexico. Second, the evaluation design was based on a rigorous randomized design and involved the collection of a rich and high quality survey. Third, the nature of the data allows us to construct some credible distribution factors. In this section, we give some background information on the programme and the evaluation surveys and present some descriptive statistics.

3.1 PROGRESA.

After a major crisis in 1994/5, and partly in reaction to it, the Zedillo administration started an innovative programme, PROGRESA, one of the first of a new generation of 'conditional cash transfers' programmes that have since become extremely popular throughout Latin America and eslewhere. PROGRESA, which was later expanded to urban areas and changed its name into *Oportunidades,* was initially targeted to poor and marginalized rural areas and had, as its stated objectives, to introduce incentives to the accumulation of human capital while at the same time alleviating short run poverty by providing poor households with cash conditional on certain investments.

Several practical aspects pertaining to the implementation of the programme are relevant for our analysis. PROGRESA/ *Oportunidades* is a conditional cash transfer programme, in the sense that receipt of the grants is conditional on the fulfillment of criterions further to the fact of being identified as poor in the sense of the program. The first set of conditions is related to health seeking behaviour. Women have to take their young children to health centres and they have to attend a number of courses organized by the programme. The second set of conditions is pertinent only for the education component of the grant. Receipt of this component is conditional on school attendance. In practice, nearly all children go to primary school. However, as about 60% of children continue to secondary school, for households with children who have finished primary school, the conditions might be binding. Importantly, the grants are paid to the women, in person, on the basis of fulfillment of the programme conditions during the preceeding period.

PROGRESA is considered a success in many dimensions, and the gold standard of welfare programmes. Replicated in most of Central and South America, and even in poor areas of New York city, the programme has been found to lead to decreases in short term poverty, and to some improvements in health, educational attainment and investment in human capital.[5] It also marks important changes in the design and delivery of interventions and welfare programmes. Price subsidies and transfers in kind are replaced by monetary transfers; evaluation is conducted from the beginning of the programme; possibilities of appropriation of the programme money are removed by using private banks and other institutions to deliver the cash, and finally, the transfers are put in the hands of women. Women's role and involvement in the programme has been heralded as one of the keys of its success. We come back to this aspect below.

At the start in 1997, 300,000 families were PROGRESA beneficiaries. Now, *Oportunidades* covers 5 million households, or 25 million individuals representing 25% of the population. *Oportunidades* has the largest budget of all human development programmes in Mexico.

The aim of the programme is to increase human capital investment of the poorest households in rural Mexico, through investment in education, health and nutrition. The grants have three components, designed to address these three aims. The amount of the education grant varies with the gender and age of the child, from 65 pesos for a boy in third grade to 240 pesos for a girl in third grade in secondary school (Hoddinott and Skoufias, 2004). At the start of the school year, another component of the education grant is paid to beneficiary households, towards the cost of school supplies. The education grants, therefore, depend on the number, gender and school level of the children, but are capped at 490 pesos per month and per household from January to June 1998 rising

[5] Detailed information on PROGRESA/*Oportunidades* and its evaluation can be obtained from the *Oportunidades* website (http://www.oportunidades.gob.mx/EVALUACION/es/docs/docs2000.php), or Skoufias (2001) or in a recent World Bank Policy Research Report, (Fiszbein and Schady, 2009) Some evidence on the New York programme, which is relatively less well known, is in Riccio et al, (2010).

to 625 pesos from July to December 1999 (Hoddinott and Skoufias, 2004). The grants are paid to the households every two months. For rural households, the programme constitutes an important component of their income. For the average beneficiary, the PROGRESA grant constituted about 20% of household income.

3.2 The PROGRESA evaluation sample.

From its start, PROGRESA/*Oportunidades* was the subject of a rigorous impact evaluation. The evaluation exploited the fact that the expansion of the programme to the population targeted in the first phase would take about two years. The first phase of the programme was targeted to villages identified as poor, but in possession of a certain level of amenities in terms of school and health provision. Of the 10,000 localities included in the first expansion phase, 506 localities were included in the evaluation sample and 320 of them were randomly chosen to have an early start of the programme (in June 1998), while the remaining 186 were put 'at the end of the queue' and were excluded from the programme until the last months of 1999. In the 320 'treated' villages the households that in the initial (August 1997 and March 1998) surveys qualified as eligible, started receiving the cash transfers (subject to the appropriate conditionalities) in June 1998, while in the 186 'control' villages, although households were defined as eligible or non-eligible in the same fashion as in the treatment villages, no payment was made until November 1999.

In the evaluation sample, extensive surveys were administered roughly every six months from August 1997 to November 2000. In each of the selected villages, the survey is a census, which is crucial for the measurement of one of the variables we use. We use two survey waves, October 1998 and May 1999. In subsequent survey waves, starting from November 1999, poor households in control villages start being incorported in the programme and receive part or all of the transfer they are entitled to by the programme.

The evaluation sample contains 24077 households, of which 61.5% are couples with any number of children and no other individual living in the household, 6.5% are female headed households, with any number of children and no other individual living in the household, and 4% are male headed households with any number of children and no other individual living in the household. The remaining 28% of households are neither nuclear families nor single parent or single individual households; they contain members of extended families or non blood relatives.

One issue which is prevalent in some areas of Mexico but does not affect the rural evaluation sample of Oportunidades is that of households in which the husband works elsewhere and sends remittances. In the Oportunidades rural evaluation sample, of the 125 674 individuals, 97% live regularly in the house surveyed, and only 2% live regularly elsewhere, be it to study or work.

Skoufias (2001), Hoddinott and Skoufias (2004), the World Bank CCT Policy Research Report (2009), and IFPRI reports (see IFPRI,2006) contain detailed descriptions and analysis of the effects of PROGRESA/*Oportunidades*. The programme's website contains up to date description of the programme and of its impacts: http://www.oportunidades.gob.mx/index.html (see also the papers cited in footnote 1).

Our Sample. The evaluation sample, within each village, is a census that includes both beneficiaries and non-beneficiaries. As our interest is in using PROGRESA (to the extent that it was targeted to women) as a distribution factor, we select a sub sample of households considered as eligible for the programme in 1997, residing either in control or treatment villages.[6] In order to work with a homogenous sample in terms of number of decision makers, we also select households in which there are no more than two adults and any number of children. The sample contains 14,464 households, of which 7,522 observed in October 1998 and 6,942 observed in May 1999. Of these, 62.08% (8,979 households) are in treatment villages and 37.92% (5,485 households) are in control villages.

[6] In August 1997, on average, just about half the households in the targeted localities turned out to be eligible for PROGRESA. It was subsequently thought that the individual targeting had been too tight and, in March 1998, a new set of households was made eligible, so that, on average, about 78% of the households in the targeted localities turned out to be eligible. However, many of the new eligible households did not receive the transfer, for reasons that are not completely clear, for some time. To avoid dealing with these problems, in what follows we focused on the households that were originally defined as poor and that started receiving the program from its start. As the classification (and re-classification) was done both in 'treatment' and 'control' villages this does not constitute a problem.

3.3 Descriptive statistics

In Table 1, we report some descriptive statistics from the sample. In the first column, we report the average of each of the relevant variables in the control sample, while in the second, we report the same average in the treatment sample. A formal comparison of the two averages shows that the two samples are balanced, as reported in Behrman and Todd (1999).

Table 1
Descriptive statistics: Means

	C	T		C	T
Educ head	2.19	2.23	Household size	5.99	5.99
Educ spouse	2.15	2.16	Nb young children	2.42	2.44
Head indigenous	0.39	0.38	Nb old children	1.57	1.55
Age of head	39.52	39.36	Children in primary	1.25	1.54
Head male	0.96	0.95	Children in sec.pre.	0.30	0.35
Townsize	403.70	387.70	Distance sec. school	2347	2107
Guerrero	0.07	0.10	Dummy secondary school	0.24	0.26
Hildalgo	0.12	0.19	Distance primary school	0.61	0.23
Michoacan	0.13	0.13	Family network	0.42	0.42
Puebla	0.15	0.17	Relatives eat in	0.07	0.08
Queretaro	0.05	0.04	Household members eat out	0.02	0.02
San Luis Potosi	0.14	0.14			
Veracruz	0.35	0.23	Nb obs	5485	8979

The sample reflects the fact that we are dealing with a very poor population. Education of head and spouse, coded as 1 for incomplete primary, 2 for primary, 3 for incomplete secondary, and 4 for secondary and above, are low. About 60% of the sample has primary education only. The average family size is 6. Just under 40% of households are of indigenous origin. The sample is drawn from seven different states (Guerrero, Puebla, San Luis Potosi, Michoacan, Queretaro, Veracruz and Hidalgo). About a quarter of the localities have a secondary school in the village. Few households have relatives or other outsiders eating in the house, and similarly few household members declare eating outside the house[7]. We will control for this in the empirical

[7]In fact, the information on whether members of the household eat out is missing for 97% of households. Similarly, there are some missing values for other variables in the table (for less

analysis to correct for the direct effect on food expenditure of either. We will discuss the construction of the family network variable below, in section 4. For now, suffices to say that there does not appear to be a difference between the mean values of this variable in control and treatment villlages.

3.4 Definition of Commodities and Prices

In what follows, we implement a test of collective rationality on z-conditional demands. To do this, however, we have to consider at least two distribution factors (which we discuss below) and two commodities. We study the demand for the components of total food expenditure, which, in our sample, represents about 80% of non durable expenditure on average. The PROGRESA data contains very detailed information on food: the survey collects information on many narrowly defined commodities and includes information both on expenditure and consumption. In computing the shares of the different foods, we include a valuation of in kind consumption.

Obviously it would not be feasible to model the demand for several dozens food items: we therefore aggregate our data to create consumption and budget shares of 5 different commodities: (i) starches; (ii) pulses; (iii) fruit and vegetables; (iv) meat, fish and dairy; and (v) other foods. For each of the individual commodities that make our five commodities, we compute consumption so as to include both what has been bought and quantities obtained from own production, payments in kind and gifts. These quantities are valued in pesos using locality level price information derived from unit values. We take particular care to avoid duplication induced by household production.[8]

Unit values are very important for our analysis and are used for two purposes. First, as we mentioned above, we use them to evaluate consumption in kind. Second, we use them to compute price indexes for each of the composite commodities. Unit values can be computed for each household that purchases a given commodity, dividing the value of the purchase by the quantity, as they

than 1% of the sample, information is missing for the variables recordingthe age of the head of household, the size of the town, the number of children in school, and distance to school. For family network, there are as many as 15% of issing values, as we discuss below.

[8] If a household has consumed some tortilla that were produced in the house, we include the value of the tortillas (valued at average prices in the town) but do not include the value of the flour that was purchased to make the tortillas.

are both reported in the survey. 'Prices' for individual commodities at the locality level are obtained taking the median unit value of the households that purchased that product in a given locality. We use medians rather than means to avoid that our estimates of prices are dominated by a few outliers in the distribution of quantities.

Locality level prices for individual commodities are then used to compute price indexes for each of the composite commodities, averaging individual level prices and using as weights locality level budget shares in each of the individual commodities. Details on the computation of the unit values and their use to compute price indexes can be found in Attanasio et al. (2009).

Spatial and temporal differences in prices of foods mean it is important to condition demands on prices. It is worth noting that the prices of foods decreased considerably between October 1998 and May 1999. As mentioned above, prices do not seem to have moved differentially between treatment and control communities. Having said that, however, it is clear that the data present a considerable amount of price heterogeneity across communities. To estimate demand functions, therefore, it will be necessary to take into account price variability even if we were considering a single cross section. The necessity to take into account variation in prices is compounded by the fact that we use two separate waves of the survey, October 1998 and May 1999.

3.5 Effect of the *PROGRESA* transfers on budget structure

Given the availability of the experimental setup, we can estimate the impact of the programme on total expenditure, on the share of food and on the share of the five commodities in food in a very simple fashion and with a minimal set of assumptions. The strongest of these assumptions is probably that there is no effect (maybe through anticipation) on the control localities. [9]

As the programme was randomly allocated across localities and treatment and control samples have been proved to be well balanced in terms of baseline characteristics, the impact of the programme on any given variable can be simply obtained by comparing averages in treatment and control localities. In this

[9] Notice that this is different from the absence of spillover effects on individuals not receiving the transfer. As the program was randomized across communities, we can allow for spillover effects of the kind documented in these data by Angelucci and DiGiorgi (2009).

section, we document the effects of the programme on total consumption, the consumption of food and the share of food. We use some of these impacts as inputs in subsequent tests of the theoretical structure. Given a demand system in which, say, the demand for food depends on total consumption, one could take the impact of the programme on total consumption, feed it in an estimated relationship and test whether the model is able to predict the change in food consumption.

Table 2 shows averages for total non durable consumption, total food consumption and the budget share of food in treatment and control villages, in October 1998 and in May 1999. Not surprisingly, the consumption of non durable is considerably higher on average in treatment villages than in control villages. In May 1999, the average difference between non durable consumption in control and treatment villages is 16%, which, when converted in pesos, is still less than the amount of the grant, which accounted for about 20-25% of total consumption on average. This difference is estimated with considerable precision (the standard error is 0.03) and is therefore significantly different from zero. The increase in consumption in treatment villages in October 1998, when the programme had only just started, is considerably smaller, but still sizeable at 8% and statistically different from zero. Such a modest impact might be explained by the fact that the programme was not necessarily perceived as permanent at its inception and by administrative delays in the first few payments. The evidence on total consumption is consistent with what has been reported in the literature. The fact that the increase in total consumption is below the amount of the grant has been noted an interpreted by Gertler, Martinez and Rubio-Codina (2012), who present some interesting evidence that the part not consumed is saved and invested in productive assets (such as small animals) which allow a permanent increase in consumption in the long run.

The log of expenditure on food is 7% higher in treatment villages than in control villages in 1998. The difference between treatment and control villages increases to 16% in 1999. These average impacts of the programme, again strongly significant, are remarkably similar to the increases in total non-durable consumption, implying that the share of food does not change much. Indeed, we cannot reject the hypothesis that food shares are the same in treatment and control villages both in 1998 and 1999.

It is therefore the case that in Mexico, as in other countries where similar programmes have been operating, the share of food does not decrease after the transfer and after an increase in total consumption. This is a somewhat surprising result: if food is a necessity, one would expect its share to decrease with total expenditure.

Table 2

Comparison of total (log) consumption and the food share between control and treated villages in October 1998 and May 1999

	October 1998			May 1999		
	Cont.	Treat.	Diff.	Cont.	Treat.	Diff.
ln(cons. exp.)	6.71	6.80	0.08	6.69	6.85	0.16
	(0.47)	(0.46)	(0.03)	(0.48)	(0.49)	(0.03)
ln(food exp.)	6.52	6.59	0.07	6.45	6.61	0.16
	(0.46)	(0.46)	(0.02)	(0.47)	(0.48)	(0.02)
Share of Food	83.40	82.94	−0.45	80.04	79.48	−0.56
	(10.98)	(11.37)	(0.59)	(12.19)	(12.25)	(0.68)
Nb of obs	2874	4798		2611	4486	

Budget shares are multiplied by 100; Nb in parenthesis are standard errors for differences; standard deviations elsewhere.
Bootstrap clustered by village. 500 replications.

In Attanasio and Lechene (2010), we rule out a number of explanations for the lack of a significant decline in the share of food as total consumption increases, and argue that it might be explained by the fact that targeting the cash transfer to women might have changed the balance of power within the household. Here, we want to check whether the restrictions implied by a specific non-unitary model of intrahousehold resource allocation, the collective model, hold in the same data and can explain this evidence.

As discussed in Section 2, to perform this test, we need at least two distribution factors and at least two independent demand functions. The latter and adding up of expenditure shares imply considering three commodities. One possibility, therefore, would be to consider the demand for food and the demand for two other commodities. However, given that food accounts for such a large fraction of these families' budget and the fact that the quality of the information on non food items is not as high as that on food consumption, makes this strategy difficult to implement in practice. Therefore, in what follows we focus on the demand for food components. This choice is also motivated by the fact that the information we have on unit values seem to indicate a large level of heterogeneity in prices across villages. To test the predictions of the collective model on a demand system, it will therefore be important to control for prices and we do not have that information for non-food components of consumption. Finally, as we document below, even when food consumption increases, the programme seems to induce relatively small changes in the composition of food consumption. It is therefore particularly interesting to check whether the demand system we estimate is able to generate this type of patterns.

In table 3, we consider the effect of the programme on the composition of food consumption. We consider consumption of five food groups: starches, wheat and rice; pulses; fruit and vegetables; meat, fish and dairy products; and finally other foods. Our figures include a valuation of in-kind consumption.

Starches account for 40% of food consumption and, therefore, about 30% of total consumption. The size of this share is another reminder of the level of poverty of these households. By contrast, expenditure on meat, fish and dairy products, which are important sources of proteins, account for only 18% of total food, while fruit and vegetables, account for 12%. Notice that almost 8% of households report zero consumption of meat, fish and dairy products in the previous week.

The table also shows the impact of the program on the shares of the five food components we are considering. The structure of the budget is not very different between control and treatment villages both in October 1998 and in May 1999. In October 1998, the statistically significant differences are for pulses, whose share is 0.80 percentage point lower in treatment villages and for meat, fish and dairy, whose share is 1.16.percentage point higher. In May 1999, again, statistically significant differences are not large: the largest differences recorded are for starches and meat, fish and dairy, respectively -2.30 percentage points and 2.54 percentage points different between treatment and control villages. As we see in section 6, estimating the demand for food components on control villages identifies income elasticities much different from one for several commodities.

Starches, for instance, are identified as a necessity and meat a luxury. This implies that the size of the effects in Table 3 is surprisingly small.

Table 3
Composition of the food basket
in control and treated villages in October 1998 and May 1999

	October 1998			May 1999			Average %
	Cont.	Treat.	Diff.	Cont.	Treat.	Diff.	of zeros
Cash transfer	0	268		0	291		
Starches	40.26	40.04	-0.21	43.34	41.04	-2.30	0.14
	(14.72)	(13.73)	(0.72)	(15.37)	(14.79)	(0.81)	
Pulses	12.82	12.03	-0.80	11.42	10.63	-0.79	3.89
	(8.24)	(7.72)	(0.39)	(7.26)	(7.47)	(0.35)	
Fruit and vegetables	13.26	13.65	0.38	10.55	11.53	0.97	2.72
	(8.61)	(7.86)	(0.43)	(7.41)	(7.38)	(0.30)	
Meat, fish and dairy	16.03	17.20	1.16	15.99	18.53	2.54	8.23
	(12.47)	(12.36)	(0.64)	(12.75)	(12.78)	(0.65)	
Other	17.62	17.08	-0.53	18.69	18.27	-0.42	1.14
	(9.93)	(8.98)	(0.49)	(9.94)	(10.17)	(0.58)	
Nb of obs	2874	4798		2611	4486		

Budget shares are multiplied by 100; Nb in parenthesis are standard errors for differences; standard deviations elsewhere. Bootstrap clustered by village. 500 replications

The evidence we have shown in this section confirms that the share of food, suprisingly, has not declined in correspondence of the increased consumption, as one would expect if food is a necessity. Furthermore, the composition of the food basket changes very little even when total food (and total consumption) change substantially. This is the evidence that a structural demand system needs to match.

4 Distribution factors

Distribution factors are variables that affect the allocation of resources exclusively by changing the relative weights of the two agents within the households (μ and $(1-\mu)$ in equation (3) above). That is, these are variables that do not enter individual preferences or affect the amount of household resources and yet play a role in determining equilibrium outcomes. As such, they play a key role in testing the implications of the collective model. This is particularly so in the case of the Bourguignon, Browning and Chiappori approach we take, which requires the identification of at least two such factors.

Arguably, the identification of plausible distribution factors constitutes the main challenge of the exercise we propose and, more generally, it has always been a stumbling block in the development of the collective model. Theory gives no guidance as to what constitutes a distribution factor. For many variables that have been used in the literature, it is often possible to think of reasons why such a variable could affect preferences and/or budget constraints. One of the best examples, in the case of couples, is the share of income earned by the wife. While it is plausible, and documented (Bourguignon et al., 1993; Browning et al, 1994) that such a variable affects the distribution of resources within the family, if preferences are non separable between female leisure and consumption, one might find that the share of women's income, which is obviously related to female leisure, appears in the demand system even if the unitary model holds.

The context of *PROGRESA* and its evaluation data set is unique in several respects, which makes it possible to construct two convincing candidates for distribution factors. First, women are randomly selected to participate in the programme and to receive a cash transfer. For recipients, this leads to an exogenous increase in the share of the household income controlled by women. Second, the survey associated with the programme is a census of villages and it is possible to establish family ties of individuals in the villages. We use this information to construct a measure of family networks for both spouses, which we argue influence individual weights in the intra-household allocation of resources. The distribution factors we use in what follows are receipt of the *PROGRESA* transfer and the relative importance of the husband and wife's networks of relatives, in terms of size or of financial prowess.

In this section, we discuss these variables and the extent to which they can be considered plausible distribution factors. In sections 6.2.2, we document how the distribution factors affect demand patterns.

4.1 Receipt of *PROGRESA* transfer

Eligibility for *PROGRESA* within a village targeted by the programme was based on a multi-dimensional assessment of household's poverty. Women in eligible households were entitled to receive a cash transfer. However, within villages included in the evaluation survey, based on eligibility for the programme, effective receipt of the cash transfer was randomised across villages.

The motivation for targeting women as recipient of a transfer based on an assessment of the household's poverty, was an explicit attempt, on the part of the administration of the programme, to improve the condition of women within the household in rural Mexico. Therefore, unless a woman was controlling 100% of the household income independently from *PROGRESA,* receipt of the *PROGRESA* transfer corresponds to an increase in the share of household income she controls. Furthermore, because of the randomisation of the programme, *PROGRESA* generates an exogenous increase in the share of income controlled by the wife only for women in some of the surveyed villages.

The share of income controlled by the wife is not an argument of preferences, and conditional on total income, it does not affect the budget constraint. Thanks to the exogenous variation in this variable induced by the randomisation of the programme, *PROGRESA* assignment constitutes an ideal distribution factor.

Of course, the *PROGRESA* grant affects the total amount of resources that a household receives and, therefore, affects the budget constraint. However, if the demand system one uses is correctly specified, controlling for total expenditure should take care of this increase in resources. Conditional on total expenditure, whether a household receives or not *PROGRESA* grants should make no difference to the allocation of total expenditures among different commodities. In other words, if the standard model is correctly specified, one should be able to describe how shares change upon receiving *PROGRESA* grants by movements along the Engel curve and predict them conditioning on the effect that the program has on total expenditure.

If, instead, after conditioning on total expenditure (including that induced by the programme) in a flexible and yet theory consistent fashion, *PROGRESA* has an impact on commodity shares, it has to be because it shifts the Engel curves, possibly as a consequence of a shift of Pareto weigths within the households. Therefore, assignment to *PROGRESA* is a distribution factor as, within a unitary model, it should not affect share equations once the effect on total expenditure is taken into account.

There is an additional caveat that needs to be made to this argument. As discussed above, the *PROGRESA* grant is a *conditional* cash transfer, where some conditions, namely the enrollment in school of the household children, might be related to certain expenditures. However, the argument we have sketched above holds conditional on school enrolment behaviour. For this reason, in what follows, we estimate a conditional demand system where we consider expenditure shares *conditional* on schooling behaviour.

4.2 Relative importance of family networks.

The second distribution factor we consider is the relative importance of husband's and wife's networks. The main idea behind the use of the relative importance of the networks is the fact that the presence of such networks may impact, for a variety of possible reasons, on the balance of power within the household. It is plausible to assume that the position within the household and the relative weights of husband and wife in the allocation of resources depend, within the context of the rural villages we are studying, on the relative strength and influence of the two extended families in the village. A woman who can count on a network of siblings and relatives larger, wealthier and more resourceful than that of her husband is likely to be in a stronger position in the allocation of resources within the household. On the other hand, having a relatively larger and impoverished extended family network can arguably weaken one's position within the household.

Before justifying fully the use of such a variable as a distribution factor, we first describe how we construct it and present some descriptive statistics on it.

To construct the relative importance of the spouses's networks we use an idea developed in an innovative paper by Angelucci, De Giorgi, Rangel and Rasul (2009) (ADRR09, from now on). ADDR09 use the fact that the *PROGRESA* evaluation survey is a census within each locality and the convention of Spanish last names to map the network of siblings and cousins within each community. In Spanish speaking countries, individuals get two surnames. The first is the (first) surname of their father, while the second is the (first) surname of their mother. As in one of the waves of *PROGRESA*, both surnames of all individuals are available, one can identify the family network for a large fraction of the sample households. We construct an algorithm which is very similar to that used by ADDR08 and construct, for each individual in the evaluation sample, the number of siblings and cousins that are present in the same locality.

We use these data to construct our candidate distribution factor, the relative importance of husband and wife's networks, in two ways: the size and the wealth of the networks. The former is the relative number of siblings residing in the same village, $s_2/(s_1 + s_2)$, where s_i, $i = 1, 2$ is the number of siblings of the wife and the husband respectively. We can also take into account the relative economic resource of the siblings and not only their number. More specifically, we construct a second index as the ratio of the (food) consumption of the wife's siblings over the (food) consumption of all siblings (husband and wife), where consumption is proxying for wealth.

Out of 14,769 households, in 8,848 households either the wife or the husband or both have siblings present in the village. For these households, the family network variable is straightforward. For 3,513 households, neither wife nor husband have siblings living in the village. When both spouses have an identical positive number of siblings in the village, the relative strength of the family network F takes the value 1/2. We therefore code 1/2 for households with no siblings in the village, highlighting the fact that what matters is to have an equal number of siblings. Finally, for 2,408 households, it is not known whether husband or wife have siblings in the village.Missing information about the presence of siblings arises when there is ambiguity about last names.

In Table 4, we report some descriptive statistics for the two measures of relative family networks importance we have considered in the analysis. The first column contains information about the relative number of siblings, and the second column contains information about the relative wealth of the family network. This table shows that both variables exhibits a considerable amount of variation and, therefore, have the potential of capturing variation in the barganing strength of women in different households. The correlation coefficient between the two variables is very high, at 0.9906.

Table 4
Family networks

	Siblings	Wealth
Minimum	0	0
Maximum	1	1
25%	0	0
Median	0.5	0.5
75%	0.5	0.57
Mean	0.42	0.42
Std Dev.	0.35	0.35
Correlation	0.9907	
Nb missing	2,408	
Nb Obs	14,769	

Our first assumption, in using relative family networks as a distribution factor is that the extent of each spouse's network provides some support for that individual. In the psychological literature, there is some evidence about this. For instance, Procidano and Heller (1983) discuss three studies that measure perceived support from networks of family and friends. They report that 'symptoms of distress and psychopatology' were inversely related to both measures of network support but the relationship was particularly strong for family networks. We argue, therefore, that the relative size of the network will affect the relative position of the spouses in the household.[10]

As an additional check on whether the relative size of the spouses network affects the relative position of the spouses in the household, we use some information on decision making that is elicited in the evaluation survey. In particular, there are several questions about who makes decisions about certain issues (such as making major purchases, taking the children to the doctor, allocating additional resources), where the possible answers are 'the wife', the 'husband' or both. We construct an index of the bargaining power of the woman and regress it on the relative network size to find that the two variables seem to be associated. Incidentally, the same index is also affected by the assignment to PROGRESA. This evidence (available upon request) supports our choice of

[10]Several papers in the literature have looked at the effects that family networks have on various aspects of household behaviour, such as consumption and risk sharing (Altonji et al., 1992); inter-generational transfers (Altonji et al., 1997; Behrman and Rosenzweig 2006; Cox and Jakubson, 1995; La Ferrara, 2003); children's education choices (Loury, 2006); and non-resident parental investments into children (Weiss and Willis, 1985).

distribution factors.

For the relative importance of the family network to be a valid distribution factor, it has to be excluded from preferences and from the budget constraint, and yet influence choices. We document in section 6.2.2 the extent to which the relative importance of the family network influences choices. Here, we discuss why it can be excluded from both the budget constraint and preferences.

The number of siblings (of either or both spouses) might have a direct effect on the demand for food, if siblings share meals. Whilst this would not invalidate the relative size of the family network as a distribution factor, not accounting for the direct effect of the number of siblings on the demand for food might would bias the estimates.To avoid this potential bias and to account for the possible direct effects that siblings can have on consumption, we control for the number of relatives who share meals with the household as a determinant of expenditure shares in the demand system. The survey contains explicit information on this variable.

There might furthermore be reasons for the size of the spouses networks to affect the demand system outside of the collective framework. Three reasons come to mind: altruism, the reciprocity of caring and risk sharing possibilities. In the presence of altruism, one could argue that a relatively large number of relatives of, say, the wife, would give more weight to wife's preferences even if the right model is a unitary one. Moreover, if women care about their siblings, presumably their siblings care for them. Then, the number of siblings might affect preferences rather than bargaining, if women's preferences are different depending upon the number of siblings they have. Similarly, if there is insurance between households, the size of family networks might be related to the necessity of sharing risk.[11]

We ignore these worries for several reasons. First, these arguments refer to the *size* of the network rather than to the *relative size* of husband's and wife's networks. Second, if altruism effects are additive these considerations will not affect the demand system. Third, and especially relevant for risk sharing, we are considering the effect of distribution factors on expenditure shares, *conditional on the level of total expenditure.* The latter is more likely to be affected by the *size* of networks, maybe because of risk sharing considerations. For expendi-

[11] Rosenzweig and Stark (1989) presente evidence that, in South India, women might tend to marry to far away villages by the need to insure large idiosycnratic risks.

ture shares, however, standard two stage budgeting considerations make it less obvious that the relative size of networks would have a direct effect, once one conditions on total consumption.

5 The Demand System: Methodological issues

The estimation of a demand system on data such as those in the PROGRESA evaluation survey raises a number of methodological issues, most of which have been addressed in a number of previous papers, among which Attanasio and Lechene (2002 and 2010) and Attanasio, Di Maro, Lechene and Phillips (2009, 2013). Specific additional issues arise in the implementation of a test of collective rationality. We review these methodological issues here, starting with the functional form of the demand system, followed by the endogeneity of total expenditure and other conditioning variables, namely schooling and the conditioning good. We also discuss why conditioning on schooling enrollment is crucial in the context of PROGRESA. Finally, we discuss the assumption we make regarding separability of food from other goods and labour supply, and the question of endogeneity of prices.

5.1 Functional form of the demand system

In what follows, we model the demand for the components of food consumption ignoring non-food consumption. Specifically, we assume that the shares of the various components of food consumption are a function of total food consumption, demographics, relative prices and, possibly, distribution factors. This modeling assumption can be justified by two stage budgeting and separability of food and the rest of consumption. That is, we assume that households first decide how much to spend on food and then, conditional on that total, decide how much to allocate to each food component. Separability between food and other components implies that this subsystem does not depend on other commodities or on relative prices between food and other items. This is a strong assumption. To test it and possibly relax it, we could include in the demand system for food either the quantity of non food consumption or the relative price of non-food. However, we do not have information on prices of non-food items. Moreover, the expenditure on these items is collected in the survey at a different frequency and with much less detail than for food. As a consequence, the quality of these data (for which a large number of zeros is reported) is much lower than for food. We are therefore forced to assume separability and model only food, which, in any case, constitutes a very large fraction of total consumption for these households.

The first step in the analysis is the specification of a demand system. In the literature on demand, many researchers have estimated a version of the so called Quadratic Almost Ideal Demand System (QUAIDS) proposed by Banks, Blundell and Lewbell (1997) as an extension of Deaton and Muellbauer (1980)'s AIDS model. Banks, Blundell and Lewbell (1997) show how demand functions where expenditure shares are a function of relative prices, log total expenditure and its square can be consistent with utility maximization and how such a system is the highest rank theory consistent system in the class of perfectly aggregable systems. The extension proposed by Banks, Blundell and Lewbell (1997) to allow for quadratic effects is particularly important in our context where we want to predict changes in expenditure shares related to a relatively large change in total (food) consumption. In Attanasio, Di Maro, Lechene and Phillips (2009), we find that it is important to allow for income responses to vary with the level of income as permitted by a QUAIDS when estimating a demand system on the PROGRESA data.

The QUAIDS system can be derived from the maximization of a unitary utility function, in which case, the coefficients on the vector of prices have to satisfy a number of restrictions (so that, for instance, the resulting Slutsky matrix is symmetric and negative definite). In the context of a collective model with public goods within the family (that is goods that give utility to both maximizing agents) the shape of the demand functions that would arise is not obvious even when both agents have a utility function that would give rise to a QUAIDS system in the unitary case. Browning and Chiappori (1998) show that symmetry does not hold in the collective model, but that the Pseudo-Slutsky matrix of price responses is the sum of a symmetric matrix and a rank one matrix and, in their empirical application, have used the QUAIDS specification as a useful parametrization of the household demand function.

In our application, following Browning and Chiappori (1998) and Attanasio, Di Maro, Lechene and Phillips (2009), we specify a QUAIDS, in which expenditure shares are allowed to depend on log total (food) consumption and its square, on prices and on demographics, as they would in a standard QUAIDS. We do not impose symmetry of the Slutsky matrix, but only homogeneity and adding-up.[12] We also allow the effect of the two distribution factors we consider and assume that they enter the demand system additively.

As we saw in section 2, the assumption of efficiency of decisions imposes restrictions on household demands. For demand choices to be consistent with collective rationality, distribution factors must affect demands only through an index. This imposes restrictions which can be satisfied in a variety of ways. Suppose, for instance, that in an AIDS or QUAIDS setting, distribution factors enter the consumption shares equations additively, as we have assumed. The proportionality restriction is then a restriction on ratio of parameters on pairs of distribution factors across demands.

The assumption of an additive effect of distribution factors on a (Q)UAIDS is somewhat arbitrary. Nothing prevents the distribution factors to affect demands in more complicated manners. For instance, it could be that they enter demands multiplicatively on total expenditure. In that case the restrictions to be tested are much more complicated. However, when we tried to interact the distribution factors we have with expenditure, we did not find significant effects. That is, in our application, distribution factors seem to affect the intercept but not the slope of the Engel curves we have estimated. We did not investigate the possibility that distribution factors affect price elasticities. However, our exercise does not use the restrictions that the collective model imposes on the Slutsky matrix.

The specification of the QUAID system we use is:

$$w_i = \theta_i' z + \phi_i' d + \sum_{j=1}^{n} \gamma_{ij} \ln(p_j) + \beta_i \ln\left(\frac{x}{a(p)}\right) + \lambda_i \left(\ln\left(\frac{x}{a(p)}\right)\right)^2 + u_i \quad (8)$$

where w_i is the share of commodity i in total expenditure on goods, $i = 1, ...n$,

x is total expenditure on goods and the price index $a(p)$ is approximated by a Stone price index where expenditure shares are used as weights. z is a vector

[12] Browning and Chiappori (1998)'s restrictions on the Slutsky matrix from the demand system of a collective model can also be tested. We leave that exercise, however, for future work.

of demographic variables and d a vector of distribution factors. θ_i and ϕ_i are vectors of parameters.[13]

We have assumed that the 'intercept' of equation (8) is a function of various demographic variables that represent shocks to tastes and of the distribution factors. The former include the number of young children, controls for the education of the head of household and his spouse, for the age of the head of household, for whether the head of household is indigenous and the size of the town. We also control for household members eating out and for relatives eating in. The variable u_i represents unobserved taste heterogeneity.

Distribution factors also enter the 'intercept' term. What distinguishes distribution factors from demographics is the fact that there are additional restrictions in the manner in which they enter into the demand functions. These restrictions are equivalently the proportionality restrictions or the restrictions on conditional demands, as we saw in section 2.3.

Under the unitary model, the two distribution factors we consider should not enter the demand system, so that the evidence we present also constitutes a test of the unitary model. The collective model imposes cross-equation restrictions on ϕ_i that we will test. Before doing that, however, we need to tackle a number of econometric problems. In particular, we need to deal with the endogeneity of total (food) consumption and of school enrolment, which are important to take into account as the PROGRESA grant is conditional on schooling.

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[13] In a QUAIDS, the budget shares take the following form:

$$w_i = \alpha_i + \sum_{j=1}^{n} \gamma_{ij} \ln(p_j) + \beta_i \ln\left(\frac{x}{a(p)}\right) + \frac{\lambda_i}{b(p)} \left(\ln\left(\frac{x}{a(p)}\right)\right)^2 + u_i \tag{9}$$

where w_i is the share of commodity i in total expenditure on goods, $i = 1, ...n$, x is total expenditure on goods and $a(p), b(p)$ and are price indexes defined by the following equations:

$$\begin{aligned} \ln a(p) &= \alpha_o + \sum_k \alpha_k \ln(p_k) + \frac{1}{2} \sum_k \sum_l \gamma_{kl} \ln(p_k) \ln(p_l) \\ b(p) &= \prod_{i=1}^{n} p_i^{\beta_i} \end{aligned}$$

As the focus of the paper is not the estimate of price elasticities, we used the appoximated expressions in 8. The results are similar when we use the full specification.

5.2 Endogeneity of total expenditure and other conditioning variables

In what follows, we model the five components of food as a function of total food consumption, under the assumption of two stage budgeting. Households first decide how much to allocate to food and then, conditional on total food expenditure, how much to allocate to each food component. The residuals of our equations can be interpreted as unobservable components of tastes that affect budget shares. If taste shocks to the system that determines total food consumption are correlated to the unobserved shocks to food components, then total food will be endogenous in our system. Measurement error in total expenditure is also a likely cause of endogeneity.

An instrument for total expenditure often used in the literature is household income, which implicitely assumes that the measurement error in total expenditure is uncorrelated with measured income. Under the assumption that heterogeneity in tastes is the source of endogeneity of total expenditure, income is a valid instrument if labour supply is separable from consumption. It may be worthwhile to make explicit and formal these arguments within the framework of the unitary model. Suppose an individual household maximizes expected utility subject to an intertemporal budget constraint[14]:

$$MaxE_0 \sum_{t=1}^{T} \beta(z_t, v_t) U(\mathbf{q}_t, \mathbf{e}_t, l_t, u_t). \quad s.t.$$

$$\begin{aligned} \mathbf{p}_t \mathbf{q}_t &= x_t \\ x_t + S_t &= w_t(T - l_t) + y_t + (1+r)S_{t-1} \end{aligned}$$

where $\mathbf{q}$ and $\mathbf{p}$ are vectors of commodities and prices, x total expenditure, w wages, h hours worked, y non labour income, S savings, z some demographic variables, v is an intertemporal taste shock and e and u are taste shocks that affect the marginal utility of commodities and labour respectively. By two-stage budgeting, one could think that first the consumer chooses total expenditure and then how to allocate it among different commodities. If the function U can be written as:

[14] The argument can be developed identically in the case of the collective model. We present it here in the unitary case for ease of exposition.

$$U(\mathbf{q}_t, \mathbf{e}_t, h_t, u_t) = u(\mathbf{q}_t, \mathbf{e}_t) + V(h_t, u_t)$$

one can decouple the labour supply problem from the determination of total expenditure and the allocation of the latter across different commodities. Total expenditure would still be endogenous if the vector of unobservable taste shocks $\mathbf{e}$ is correlated in the cross section with the intertemporal taste shock v. In such a situation, total household income $w_t(T - l_t) + y_t$ can be used as an instrument, if one assumes that the taste shock u_t is uncorrelated with the vector $\mathbf{e}_t$.

If one thinks that such an assumption is too strong, a possible alternative is to use the component of income that the household takes as given (the wages) as an instrument for total expenditure. We considered the average agricultural wage in a village as an instrument. Such a variable would be a valid instrument (under separability) even when the three taste shocks we consider (u, e and v) are correlated.

There is an additional reason to consider aggregate wages (rather than individual income) as an instrument for total expenditure. Income can be a weak instrument in a context where large transitory shocks or measurement error may weaken the relationship between income and total expenditure. This argument is particularly relevant in the context of developing countries where, while consumption is relatively simple to measure, income (and its many components) might be difficult to capture. In Attanasio and Lechene (2002) we find that individual level expenditure is better explained by average wages than by individual income in the cross section.

Obviously, by using average wages, we loose the variability at the individual level since, in this case, we only exploit the variation across villages. However, given the high variance of measurement error in income, this is not necessarily a problem. We find that once we introduce distribution factors in the model, only results obtained with the village average agricultural wage are robust across different dimensions. We therefore present these results.

If consumption and leisure are not separable in the utility function, income or wages are not valid instruments for total expenditure. However, in that case, the entire demand system is mispecified as one should allow for the effect of hours of work on the marginal utility of consumption. Hours of work should enter in their own right as a determinant of the demand system. In the context we are studying we have decided to assume separability of food consumption from non food consumption and leisure for two reasons. First, our intuition is that labour supply behaviour is fairly inelastic in the present context for the poor households in our sample. Considering it as separable from consumption, therefore, might not be a bad approximation. Second, as we focus on the role of Oportunidades as a distribution factor in the demand system, we can appeal to the fact (reported in the literature, see Skoufias (2001) and Skoufias and DiMaro (2008)) that the programme has not affected adult labour supply.

An additional endogeneity issue arises when we condition the demand system on certain forms of behaviour (such as school enrolment) or when we estimate the z-conditional demand system in equation 7. In both cases, we use a control function approach analogous to that we use for taking into account the endogeneity of total expenditure. That is, we estimate a reduced form for the endogenous variables which includes variables that are assumed to affect those choices but not to enter directly the demand system. In the case of school enrolment, we use variables that represent the cost of schooling (such as the distance from the nearest school). In the case of the z-conditional demands, the instrument that identifies the model is suggested directly by the theory and by the test we propose: if the collective model is valid, the distribution factor used for inverting the demand of one of the commodities is a valid instrument as, conditional on the demand for commodity j in equation 7, no distribution factor affects the demand for any commodity $i \neq j$. This is the sense of the test in equation 6. The distribution factor used to invert the demand for commodity j is therefore a valid instrument for C_j in equation 7. We can use the significance of the second distribution factor (in our case the PROGRESA/Oportunidades dummy) as a test of the validity of the model.

5.3 Schooling

Conditional cash transfer programmes impose minimum schooling requirements for children of the recipient households to receive the largest component of the grant. The grant amounts are devised with the aim to cover the opportunity cost of schooling for the household, which is why they vary with the age and gender of the child. The conditionality might affect consumption behaviour, if sending children to school imposes related costs, such as for uniforms, shoes or books. Conversely, children might be fed in school, which would also have an impact on the budget share of food and its components. It is thus necessary to control for schooling of children, over and above controlling for household composition. However, it could be that unobserved taste for school is correlated with unobserved taste for certain foods, so that schooling could be endogenous in the demand system. To allow for this possibility, we instrument schooling with an indicator for the existence of a secondary school and distance from secondary school if it is not in the village (and zero if it is in the village). The average distance to a secondary school is 2.2 kilometers, with a maximum distance of 14 kilometers. In only about 25% of villages is there a secondary school.

The assumption that we are making is that the distance to a secondary school affects schooling decisions but does not affect the structure of expenditures between foods, conditional on the size of the village and the other controls in the demand system. As is always the case, it is not possible to test the identification restriction that we are using. One possible worry, for instance, is that the presence of a school in a nearby village proxies for other variables (such as the presence of a market) that might affect demand. However, we feel that conditional on village size and the other variables (including prices) we are considering, the information on the distance to a secondary school can be excluded from the demand system.

Primary school is not considered as endogenous in the demand system. Primary school attendance is high (over 90%) and not affected by the programme. However, we follow previous literature on PROGRESA in that we condition on the number of children attending primary school in the demand system, for the reasons mentioned above.

5.4 Additional issues

There are two additional methodological issues worth mentioning briefly. The first one has to do with separability and the collective model. When we specify a demand system for foods arising from a unitary model, we call upon an assumption of separability between food and non food to justify conditioning quantities of food components on total food consumption and prices of these food components, but not, say, on prices of other non durables. When considering the collective model, however, assuming that individual preferences are separable between food and the rest is not sufficient to get a household demand system for food components that does not depend on non-food relative prices. To obtain such a specification we need to make an additional assumption, namely that the sharing rule or Pareto weights of the individuals in the household welfare function does not depend on the prices of non-food items. This is true if the household decides on the allocation of the total between food and non food, and then on the allocation between foods. Following this argument, the Pareto weight, or the sharing rule for this problem will depend on the prices of the foods, and the total expenditure on the same, and the distribution factors if any.

Analogous considerations apply to the issue of how to treat labour supply. In our application we have assumed that leisure and consumption enter the utility function in a separable fashion. This allows to ignore labour supply choices and consider the income process that would get from solving the labour supply problem as a given exogenous process. Implicit in this choice is the assumption that wages do not enter the individual weights. The main motivation for assuming separability of consumption and leisure was the desire to keep the discussion simple, the focus of the paper on the effect of distribution factors on expenditure shares and the fact that, as documented, for instance, in Skoufias (2001) and Skoufias and DiMaro (2008), Oportunidades had no impact on adult labour supply.

The second methodological issue is the possible endogeneity of prices. Suppose that tastes shocks are correlated within villages. In this case, it could be that in a village where people like meat a lot, the price of meat will be high and yet, there will be a high demand for meat. One way around this would be to instrument prices with supply conditions (number of shops, distance from big markets, etc...). We do not have this information, and we therefore make the assumption that this effect is absent. We should also note that our main interest is in the effect that the introduction of PROGRESA has on the demand system and that studies that have looked at the effect of PROGRESA on prices (such as Angelucci and DeGiorgi, 2009) have not found any effects.

6 Empirical results

We are now ready to present our empirical results. We divide this section in three parts. First we present results for the first stage regressions we estimate to deal with the endogeneity of total food consumption and school enrolment. We then discuss our estimates of the demand systems and informally compare the predictions implied by the model for the impact of the grant on consumption shares. Finally, we present the formal tests of the restrictions of the collective model.

6.1 First stage regressions

As we discussed in the previous section, there are two potentially endogenous variables in the demand system: total expenditure on food and number of children in secondary and "preparatoria" school. Table 5 shows the first stage regressions for the log of total expenditure and the number of children in secondary school and "preparatoria" school. These regressions include both the instruments and the other conditioning variables that enter the second stage. In the first two columns of the table, the instrument for total expenditure is the village average agricultural wage, whilst in the last two columns is household income. The other instruments we use (for secondary school enrolment) are an indicator of the presence of a secondary school and the distance to the secondary school.

The results reported in Table 5 refer to the entire sample, which includes both treatment and control localities. Results on the control sample, for which we report estimates of the demand system in the next section, are substantially similar and are available upon request.

The instruments have the expected effects in the first stage regressions. In the equation for log total expenditure, we find that both village median agricultural wage and income have power in explaining total food expenditure and that both the linear and the quadratic terms are important. As for the equation for secondary school enrolment, we find that distance to secondary school influences the number of children attending secondary school in the expected direction.

The first stage results are also conditional on the distribution factors we consider: namely an indicator for the receipt of PROGRESA and the relative family network size ratio. The first stage results obtained with the alternative measure of relative family network, based on consumption, as well as those obtained with the perceived power index, are not different from those presented in Table 5.

Table 5

First stage regression for total expenditure and schooling

Variable	Tot.Exp.	Ch.High Sch.	Tot.Exp.	Ch.High Sch.
Instrument	Village wage		Income	
ln(instr)	-0.37	0.462	-0.15	0.12
	(0.09)	(0.12)	(0.07)	(0.011)
ln(instr)^2	0.18	-0.17	0.0016	-0.005
	(0.03)	(0.05)	(0.005)	(0.008)
Receipt of PROGRESA	0.10	0.055	0.10	-0.011
	(0.008)	(0.012)	(0.009)	(0.018)
Family network size	-0.024	-0.003	-0.026	-0.011
	(0.011)	(0.016)	(0.013)	(0.018)
Distance high school.	-0.00	-0.02	0.001	-0.02
	(0.002)	(0.003)	(0.003)	(0.004)
Indicator high school.	-0.000	0.057	0.00	0.045
	(0.01)	(0.017)	(0.014)	(0.02)
Children in primary school	0.08	0.078	0.07	0.067
	(0.004)	(0.005)	(0.004)	(0.006)
Townsize	-0.001	-0.001	-0.001	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Nb. young. children.	0.02	-0.035	0.023	-0.037
	(0.003)	(0.04)	(0.003)	(0.005)
Educ. spouse	-0.018	0.004	-0.018	0.006
	(0.005)	(0.008)	(0.006)	(0.009)
Head indigenous	-0.079	-0.004	-0.063	-0.008
	(0.01)	(0.015)	(0.011)	(0.016)
Head age	0.001	0.009	0.002	0.012
	(0.000)	(0.001)	(0.001)	(0.001)
Educ. head	-0.000	0.014	-0.007	0.015
	(0.005)	(0.008)	(0.006)	(0.009)
Relatives eat in	0.007	-0.01	0.003	-0.01
	(0.005)	(0.007)	(0.006)	(0.008)
Hhld member eat out	0.012	0.049	-0.012	0.059
	(0.02)	(0.03)	(0.023)	(0.033)
R2	0.14	0.07	0.13	0.08
N	12155		9364	
	F(2,12134)		F(2,9343)	
Test of instrument for total expenditure	24.90	7.53	22.26	6.91
$p-value$	0.00	0.00	0.00	0.00
Test of instrument for schooling	0.02	43.68	0.07	28.17
$p-value$	0.98	0.00	0.93	0.00

Prices of foods and a constant are also included. Standard errors in parenthesis.

6.2 Demand System

As mentioned above, the demand system is specified as a QUAIDS, which allows both linear and quadratic terms in the log of total consumption of food. This variable and schooling are treated as endogenous and we use a control function approach to deal with this issue. In particular, we add to the equations we estimate a second degree polynomial in the residuals of the first stage regressions reported in Table 5. The significance of first stage regression residuals in the demand system indicates a strong rejection of exogeneity of both total expenditure on food and secondary school enrolment in the structure of the food budget. In what follows, we present the results obtained with the agricultural wage as an instrument for total consumption of food. The results obtained with income as instrument are qualitatively similar to those obtained with the wage, but they are less precise.

6.2.1 Demand system without distribution factors

We first estimate the demand system without considering any distribution factors. This system, which corresponds to equation (8) above, includes demographic variables (household head age, the number of young chidlren, an indicator for indigenous head, education of the head of household and the spouse, townsize, and information about household members eating out and relatives eating with the household considered). It also controls for the number of children in secondary and primary school (with the former being considered as endogenous), and prices. In Table 6, however, we report only the coefficients on the linear and quadratic log total expenditure terms; the coefficients on the other variables are available upon request.

The first three columns of Table 6 are obtained using information from households in the control villages only. The first two columns contain the estimates of the coefficients (and their standard errors) on total consumption of food and its square for the five food components we consider in the demand system. In the third column, we give the value of the χ^2 test of joint significance of the two income terms in the equation of each good (and its p-value).

From the results in the first three columns of table 6, we see that the income effects estimated in control villages differ significantly from zero for starches, fruit, meat and other foods. The estimated coefficients indicate that starch is a necessity over most of the range of total food expenditure, while fruit and meat are luxuries over most of the range. The category "other foods" appears to be a necessity at low levels of total expenditure and a luxury at high levels of total food consumption.The relationship between pulses and total food consumption is not precisely estimated and we cannot reject the hypothesis that the coefficients on log consumption and its square are jointly zero, indicating that the share of expenditure on pulses does not change with total food consumption.

The next exercise consists in re-estimating the same demand system, pooling control and treatment households. The results of coefficients on log total food consumption (and its square) are shown in columns 4 and 5 of table 6, while we report the χ^2 test of the joint significance of these coefficients in Column 6.

The treatment consists in the injection of (relatively) large amounts of cash in the budget of treated households. As the treatment was allocated randomly, if the structural model we estimated was well specified, including the treated households in the estimation sample should not make much difference to the point estimates of the income effects, but should increase their precision. What we find, however, is entirely different. Rather than increasing precision, increasing the sample size by incorporating poor households from the treated villages leads to changes in the values of the coefficients. The estimated coefficients change substantially and in the case of starch, fruit, meat and other foods, the curvature of the relationship between total expenditure and the budget share changes. If we believed these estimates, we might be led to conclude that starch is a necessity at low level of expenditure, then a luxury and that fruit and meat become necessities at high level of total expenditure. Since we have no reason to presume that the nature of goods should be different in the preferences of households in treated villages, we interpret this lack of stability as indicating that the model is not able to capture the relationship between total expenditure and the structure of the budget following the cash transfers.

Table 6
Income effects, demand system with no distribution factors

	(1) Control villages			(2) Control and treatment villages		
	Tot. Food	Tot.Food^2	$\chi^2(2)$ $(P > \chi^2)$	Tot. Food	Tot.Food^2	$\chi^2(2)$ $(P > \chi^2)$
Starch	5.08	-0.56	12.8	-9.02	0.89	18.00
	(2.30)	(0.24)	(0.00)	(2.36)	(0.24)	(0.00)
Pulses	-0.35	0.04	0.39	-0.95	0.09	3.59
	(1.13)	(0.12)	(0.67)	(1.09)	(0.11)	(0.03)
Fruit	-1.66	0.19	10.66	5.35	-0.53	28.55
	(1.07)	(0.11)	(0.00)	(1.05)	(0.11)	(0.00)
Meat	-0.96	0.12	7.77	3.26	-0.31	13.23
	(1.60)	(0.17)	(0.00)	(1.75)	(0.18)	(0.00)
Other foods	-2.11	0.21	2.73	1.37	-0.14	0.43
	(1.32)	(0.14)	(0.07)	(1.56)	(0.16)	(0.65)
Nb obs		5485			14769	

The demand system is as in equation 8 and controls are included for children in primary school, children in secondary school, nb of young children, town size, education of head and spouse, age of head, indigenous head dummy, relatives eating in, and household, members eating out, as well as homogenous prices. Average agricultural wage as instrument for total expenditure; indicator for the presence of a school and distance to school as instrument for children in secondary school.
Bootstrap clustered by village, 500 replications.

We further substantiate our interpretation of these results, by using the estimates from Table 6 and the estimated experimental impact on total food consumption (and schooling) to predict the impact of the PROGRESA grant on the shares of the five commodities that we are considering. We do this in Tables 7 and 8, respectively for October 1998 and May 1999.

In the first column of the tables, we report the average impact of the program as estimated comparing treatment and control communities. We will be referring to this impact as the 'actual' impact, as it is based on experimental evidence. As discussed in section (3.5), what is most notable about the actual impacts of the cash transfers on the structure of the budget is how small they are. The budget share of starch is 2.30 percentage point lower in treated households than it is in control households in May 1999. This is consistent with the fact that starch has been found to be a necessity. The share of pulse also decreases significantly at both dates, albeit not dramatically. The shares of fruits and especially of meat increase.

In the second column of Tables 7 and 8, we use the estimates of the demand system estimated using only data from control villages (and reported in the first two columns of Table 6) to predict the impacts of the program on expenditure shares. In the third column of Tables 7 and 8, we use the estimates of the demand system obtained from both treatment and control villages. In terms of point estimates, the predictions are of poor quality for all the goods at both dates. However, note that since the actual impacts are mostly zero, we are trying to predict zero, which can arise also through lack of precision.

In the last two columns of Tables 7 and 8, we test the hypothesis that the 'actual' impacts of the programme on expenditure shares, as reported in the first column of the two tables, are statistically different from those predicted in columns 2 and 3, respectively. The standard errors of these differences are obtained bootstrapping both the estimates of the actual impacts, those of the demand system and of the predictions. The standard errors are clustered at the village level.

We find that in many cases, we reject the null that the actual and predicted impacts are the same. On the other hand, in a number of instances, we cannot reject the hypothesis that these predictions are the same as the actual impacts. This result, however, is mainly due to the low precision of our estimates based on the demand system, whose variability reflects both the variability of the estimates of the impact on total consumption and that of the coefficients of the demand system.

Table 7
Actual and predicted effects of the program on the budget structure
No Distribution factors - October 1998

	Actual impact	Predicted impacts on controls	Predicted impacts pooling treated and controls	Diff. btw actual. impact and on controls	Diff. btw actual. impact and pooling treated and controls
Starch	-0.21	-1.97	-3.80	-1.75	-3.59
	(0.72)	(1.15)	(1.84)	(1.26)	(1.80)
Pulses	-0.80	0.20	-0.92	1.00	-0.12
	(0.39)	(0.36)	(0.46)	(0.57)	(0.61)
Fruit	0.38	0.99	2.17	0.61	1.78
	(0.43)	(0.51)	(1.00)	(0.62)	(0.96)
Meat	1.16	1.51	2.43	0.35	1.27
	(0.64)	(0.73)	(1.01)	(0.78)	(0.97)
Other foods	-0.53	-0.73	0.12	-0.20	0.66
	(0.49)	(0.52)	(0.46)	(0.68)	(0.68)

Predictions are obtained from 1998 data. Bootstrap clustered by villages; 500 replications. Bootstrap standard errors in parentheses.

Table 8
Actual and predicted effects of the program on the budget structure
No Distribution factors - May 1999

	Actual impact	Predicted impacts on controls	Predicted impacts pooling treated and controls	Diff. btw actual. impact on controls	Diff. btw actual. impact pooling treated and controls
Starch	-2.30	-6.36	-5.14	-4.06	-2.83
	(0.81)	(1.80)	(2.25)	(1.73)	(2.45)
Pulses	-0.79	0.59	-1.71	1.37	-0.92
	(0.35)	(0.75)	(0.74)	(0.80)	(0.83)
Fruit	0.97	2.85	2.84	1.88	1.87
	(0.30)	(0.74)	(1.07)	(0.73)	(1.03)
Meat	2.54	3.76	4.24	1.23	1.71
	(0.65)	(1.18)	(1.35)	(1.07)	(1.30)
Other foods	-0.42	-0.85	-0.24	-0.43	0.18
	(0.59)	(0.91)	(0.78)	(0.98)	(1.00)

Predictions are obtained from 1999 data. Bootstrap clustered by villages; 500 replications. Bootstrap standard errors in parentheses.

These estimates are obtained taking the estimated demand system (without distribution factors) as a structural relationship and inputing into it the impact that the programme has on total food consumption and schooling. However, it might be the case that the demand system is *not* a structural relationship, if other features of the programme might be relevant, such as the fact that the transfer is put in the hands of women. We will now investigate whether this is the case by introducing distribution factors in the demand system.

6.2.2 Demand system with distribution factors

We now re-estimate the demand system allowing the expenditure shares to be affected by two distribution factors: the receipt of the PROGRESA transfer and the variable measuring the relative size of wife and husband family networks that we discussed above. The consumption shares we estimate correspond to equation (8) and include the same demographic variables used when estimating the demand system reported in Table 6. We enter the first distribution factor as an indicator which equals to one if the household lives in a village targeted by PROGRESA (and therefore receives the programme).[15]

The second distribution factor is crucial for the test of the collective model that we report below. We therefore investigate the possibility that it enters non-linearly the consumption shares equations. Allowing for the presence of quadratic (or higher order) terms is important for two reasons. First, the theory is silent about the specific form in which this (or any other) distribution factors enters the share equations. Second, the test of the collective model we propose requires that there is at least one commodity for which one of the distribution factors enters monotonically, so that the relationship can be inverted.

In Table 9, we report the estimates of the coefficients on the PROGRESA dummy and on the family network size and its square that we obtain on the whole sample using the agricultural average wage as an instrument for total food consumption. This set of results is representative and robust across different specifications. The main finding is that, while the coefficient on the PROGRESA dummy is significantly different from zero in four out of five share equations, the coefficient on the quadratic terms of the family size network is never significantly different from zero[16]. The results were virtually identical when we used the index based on the relative wealth of husband and wife's networks. We also tried higher polynomial for the relative network variable and we could not identify any significant higher order terms. We also notice that, in the case of Meat, not

[15] Unlike in urban areas, take up of the programme among eligible is virtually universal.

[16] When we estimate the demand system on single mothers, we find that the treatment indicator is not significant in any of the foods, thus confirming that it is a valid distribution factor, since it plays a role when there are two decision makers, but not when there is only one. These results, that are available on request, confirm the finding of Shady and Rosero (2008).

only the coefficient on the quadratic term is not significantly different from zero, but the point estimates of the two coefficients imply an increasing relationship between the share and the relative network size variable.

Table 9

Effect of distribution factors on the consumption shares

	Starch	Pulses	Fruit	Meat	Other foods
Treatment	0.035	0.0022	-0.017	-0.019	-0.0016
	(0.010)	(0.0062)	(0.004)	(0.008)	(0.006)
Family network size	-0.015	0.0045	0.014	0.007	-0.010
	(0.012)	(0.0063)	(0.006)	(0.010)	(0.008)
Family network size^2	0.00027	-0.0043	-0.004	0.003	0.006
	(0.012)	(0.0064)	(0.006)	(0.01)	(0.007)
Nb obs			12361		

Instrument for total food is village average wage. Bootstrap clustered by villages, 500 replications. Standard errors in parentheses.

Given these findings, we decided to use a specification of equation (8) that is linear in relative network sizes. In Table 10, we report the estimates we obtain for the coefficients on the two distribution factors and for the linear and quadratic log total food consumption. Several comments are in order. First, both distribution factors are strongly significant. The PROGRESA dummy is significantly different from zero at standard levels in four of the five share equations. The relative size of family networks is also significantly different from zero in three of the five shares equations. Notice that under the unitary model, neither of these variables should enter the demand system. The linearity of the relationship between shares and the relative size of the family networks implies that we can use any of these relationships to perform the inversion described in Section 2.3 and construct the z-conditional demands.

Table 10					
Effect of distribution factors on the budget, inst:wage					
	Starch	Pulses	Fruit	Meat	Other foods
Treatment	0.035	0.0022	-0.017	-0.019	-0.0015
	(0.01)	(0.006)	(0.004)	(0.008)	(0.006)
Family network size	-0.015	0.00	0.01	0.009	-0.004
	(0.005)	(0.002)	(0.0025)	(0.0037)	(0.003)
ln Tot. Exp.Food	-8.96	0.037	4.26	3.39	1.27
	(2.52)	(1.09)	(1.06)	(1.89)	(1.65)
ln Tot.Exp.Food^2	0.88	-0.01	-0.42	-0.32	-0.14
	(0.26)	(0.001)	(0.11)	(0.20)	(0.17)
Nb obs			12361		
Estimates of some of the coefficients of Equation (8).					
Bootstrap clustered by village					

As for the coefficients on total log food consumption, the quadratic effects are strongly significant in three of the five foods. Starches, as before, are a necessity over most of the range of food consumption, as are pulses and other foods, while meat and fruit are luxuries.

In the next sub-section, we present the formal test of the restrictions implied by the collective model. One more informal but informative way to check whether our specification fits the data generated by the PROGRESA experiment, which explicitly changed the control of resources within the family in a controlled way, is to check whether the specification of the demand system in equation (8) is able to predict the changes in consumption shares reported in Table 4. We, therefore, re-do the exercise reported in Tables 7 and 8, but using the coefficients of the demand system that includes the distribution factors we have considered. The results of this exercise are reported in Table 11 and 12. In column (1) of these tables we report again the 'experimental' average impact. In column (2) the prediction of the demand system that incorporates the distribution factors and, in column (3) the difference between the two. As in Tables 7 and 8, the standard errors of all these estimates are computed by bootstrapping all the relevant components, with clustering at the village level.

The results we obtain now are much different from those in Tables 7 and 8. All predicted impacts are of the same sign as the observed changes in consumption shares. Most importantly, they are considerably closer to the actual experimental impacts and the difference is never statistically different from zero.

Table 11:
Actual and predicted impacts of the program on
the commodity shares with distribution factors October1998

	Impacts		Difference
	Actual	Predicted	
Starch	-0.21	-0.33	-0.11
	(0.72)	(1.75)	(1.59)
Pulses	-0.80	-0.39	0.41
	(0.39)	(0.47)	(0.36)
Fruit	0.38	0.32	-0.06
	(0.43)	(0.86)	(0.74)
Meat	1.16	0.49	-0.67
	(0.64)	(1.13)	(0.89)
Other foods	-0.53	-0.10	0.43
	(0.49)	(0.51)	(0.50)

Predicted impacts computed using the model in Table 10.
Bootstrap clustered by villages, 500 replications.

Table 12:
Actual and predicted impacts of the program on
the commodity shares with distribution factors May 1999

	Impact		Difference
	Actual	Predicted	
Starch	-2.30	-1.88	0.42
	(0.81)	(1.83)	(1.98)
Pulses	-0.79	-0.91	-0.13
	(0.35)	(0.45)	(0.38)
Fruit	0.97	1.00	0.03
	(0.30)	(0.84)	(0.80)
Meat	2.54	2.45	-0.09
	(0.65)	(1.12)	(0.88)
Other foods	-0.42	-0.66	-0.24
	(0.59)	(0.57)	(0.66)

Predicted impacts computed using the model in Table 10.
Bootstrap clustered by villages, 500 replications.

We interpret this evidence as indicating that there is scope, in the context of the PROGRESA programme, for investigating the role played by features of the programme which cannot be rationalised within the standard framework of unitary household choices, but need to be accounted for. In the next subsection, we turn to the formal test of the collective model. We will not comment here on other aspects of the estimation of the demand system. Interested readers are referred to Attanasio, Di Maro, Lechene and Phillips (2009 and 2013) for in depth analysis of income and price responses and welfare analysis in this context.

6.3 Test of Efficiency

The test for collective rationality we discussed above requires that we can observe at least two goods and two distribution factors. One aspect which is crucial in the analysis is that the distribution factor and the good which is used for the test have to have a statistically significant link, otherwise the test has no power. This is not a problem here, since the relative size of husband and wife's networks is significant in the demands for three goods out of five. Similar considerations apply to the results that we obtain with the reported power and the alternative measure of relative networks.

The good we use for the test of conditional rationality is animal protein, or meat, fish and dairy. Both distribution factors significantly influence the demand for animal protein, and the relationship between demand and the family network is monotonic.

Table 13 gives the results of test of collective rationality using z-conditional demand with animal proteins as the conditioning good and relative family network size as the distribution factor used to invert the demand for meat. To deal with the endogeneity of the conditioning good, we use, once again, a control function approach, where the identifying instrument, consistently with the collective model, is the distribution factor that is used to invert the demand for the conditioning good. In the Table, we only report the coefficient on the PROGRESA dummy and, in the case of the conditional demands, the coefficient on meat and the coefficient on the residual for the first stage regression for meat, denoted with u_{meat}. For each good in the table, we also report the results for the unconditional estimation.

The results are striking: in the unconditional demand system, the treatment dummy is significant for three goods. In the z conditional demand system it is nowhere significant. Moreover, it is not because of an increase in the standard errors that the estimates of the treatment dummy become statistically insignificant: rather the point estimates drop, for three of the four commodities, dramatically. There results imply that we cannot reject the collective model. We should note that the test of the collective model based on $z - conditional$ demands is equivalent to a test of the proportionality restrictions.

Table 13								
Collective rationality test with family network distribution factor,								
	Starches		Pulses		Fruit		Other foods	
	Uncond.	z cond.	Uncond.	z cond.	Uncond.	z cond.	Uncond.	z cond.
Treat	0.035	0.004	0.0022	0.0023	-0.017	0.004	-0.0015	-0.010
	(0.01)	(0.027)	(0.006)	(0.038)	(0.004)	(0.06)	(0.006)	(0.013)
Meat		-1.61		0.007		1.07		-0.47
		(1.57)		(2.43)		(3.76)		(0.70)
u_{meat}		0.92		-0.13		-1.11		0.32
		(1.57)		(2.43)		(3.76)		(0.69)
u^2_{meat}		0.15		0.00		-0.05		-0.10
		(0.06)		(0.027)		(0.035)		(0.04)
Nb obs				12361				

Instrument for total food consumption: average agricultural wage.
Bootstrap clustered by villages, 500 replications.

7 Conclusions

The unitary model has been rejected a number of times. In this paper, we go beyond that result and test some of the implications of one of the main alternatives to the unitary model, the so-called collective model that postulates that, however intrahousehold allocations are achieved, they are such that there is no waste of resources and are therefore efficient.

We implement a test of the collective model that has been recently proposed by Bourguignon et al (2009) which requires the analysis of the demand for at least two commodities and at least two distribution factors. The idea is relatively simple: an important implication of the collective model and efficiency is that distribution factors only affect demand through the Pareto weights that defines the efficient allocation. If this is the case, then two or more distribution factors have to enter proportionally or, equivalently, if the relationship between the demand for one good and a distribution factor is monotonic, one can condition on that commodity and 'explain away' all other distribution factors.

We apply this test to the context of rural Mexico and on the data set collected to evaluate the conditional cash transfer program PROGRESA. This data set is ideal for a variety of reasons. First, the programme is targeted to women with the explicit purpose of changing the balance of power within the households that receive it. The programme itself, whose allocation is randomized across localities within the evaluation sample, is an ideal distribution factor. Second, the fact that the evaluation factor is a census within each village gives researchers the opportunity to map out the family network and allows us to construct an additional distribution factor, the relative size of husband and wife family networks. This measure is continuous and turns out to be an important determinant of the demand for food.

We use the PROGRESA data to estimate a state of the art demand system both with and without the distribution factors we considered. We first confirm that the demand system we estimate without distribution factors is not stable and is unable to predict the impact of the programme on consumption shares. The distribution factors we consider are not only significant, but seem to enter in a fashion which is not inconsistent with the implications of the collective model. Moreover, we are able, with these distribution factors, to predict the impacts of the programme much better than the standard unitary model.

In the process of testing the collective model, our results also offer an explanation for a phenomenon that has been observed in the context of a number of conditional cash transfer programs, namely the fact that in the face of the large change in total consumption that follows the injection of cash implied by these programs, the structure of consumption, that is how total consumption is allocated to different commodities changes in ways that are hard to reconcile with perceived wisdom or even with the estimates of state of the art demand systems. We suggest that this might be due to a violation of the unitary model and to the fact that the cash transfers delivered by these programmes are targeted to women. Furthermore, we show that the deviations from the standard model are not inconsistent with the collective model.

Our results are important because they constitute the first test of this nature of the collective model in a context where the intrahouseholds allocation of resources is certainly salient, as witnessed by the attempt on the part of the government, to change it. Moreover, the fact that we do not reject the implications of the collective model is important because it points to a specific model of intrahousehold resources that can be used to study the household behaviour and establish the consequences of different policies. In future work, we plan to test additional restrictions of the collective model, and in particular those on the demand price elasticities.

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9 Appendix A. A discussion of Bobonis (2009)

Bobonis (2009) developed, independently from us, some of the same ideas we use in our paper. In particular, Bobonis wants to implement the test developed by Bourguignon, Browning and Chiappori (2009, henceforth BBC09) using the same data we use, the evaluation data set for the PROGRESA program. Unfortunately, Bobonis's implementation of the test is severely flawed in many dimensions, so that his results are difficult if not impossible to interpret. In this Appendix, we explain our criticisms of Bobonis' approach.

The main issues we have with Bobonis' paper are:

1. The use of rainfall as a distribution factor.
2. The use of PROGRESA as the distribution factor to invert the conditional demands.
3. A variety of technical problems with the estimation of the demand system.

We discuss these issues in turn.

1. *Rainfall as a distribution factor.* Bobonis uses village level rainfall (or rather an indicator dummy that takes a value of 1 if rainfall exceeds a certain level) as a distribution factor, which we find most puzzling. Bobonis provides little conceptual justification for the use of rainfall as a distribution factor. He variously states that rainfall is a valid distribution factor because it is a proxy for "joint household income" or for [shocks to] "household income that are not exclusive of female partners". As we mention in section 4, a distribution factor is a variable which does not affect preferences and budget constraints and yet affects choices, through an effect on the decision process. There are thus two separate questions, one conceptual and the other empirical. First, under what assumptions can partner specific and jointly held incomes constitute valid distribution factors? Second, how does rainfall affect incomes in the context considered?

It is generally accepted that partner specific incomes, or rather the share of household income controlled by each partner in the total of household income is a valid distribution factor, if preferences are separable between consumption and the activities that generate those income. Under such an assumption income shares do not affect the marginal utility of consumption and, for a given level of household resources, partner specific incomes do not enter the budget constraint. Furthermore, whether income shares influence the allocation of resources by the household can be empirically tested, so as to establish that partner specific incomes do indeed affect choices, for a given level of household income.

Bobonis makes implicitly the assumption of separability (which we will not take issue with) and assumes that total household resources, y, can be decomposed as:

$$y = y^A + y^B + y^0$$

where y^A is income controlled by the wife, y^B income controlled by the husband and y^0 is what he calls 'jointly held income'. Under the unitary model (and income pooling) only the total should matter. Notice that in order to have two distribution factors out of this decomposition, it is crucial to have three components of income. If there were only two (for instance if y^0 was uniformly zero), one could only obtain one distribution factor: the share of women's income in the total.

The argument for PROGRESA assingment to be a valid distribution factor is that for a constant y, assignement to PROGRESA changes y^A/y. The argument for rainfall to be a valid distribution factor would have to be that, for a constant y, rainfall changes y_B/y (or, identically, $(y_0/y) = (y - y_A - y_B)/y)$. It seems Bobonis assumes that rainfall affects only one of the three components of income, namely either y^B or y^0.In other words, the aim is to capture exogenous movements in y^A by an indicator of whether the household is a PROGRESA beneficiary and movements in y^0 by rainfall shocks.

While the former strategy is acceptable, there is no reason to believe that rainfall affects only y^0 and not both y^0 and y^B or even y^A, y^B and y_0. In other words, one can get independent variation in y_B/y if one can find some source of independent variation in y_0. In Bobonis's sample, unearned income is zero for 65% of the sample, social insurance for 87%, profits are zero for 95% of the sample and agricultural profits are missing for 96% of the sample. There is no second distribution factor.

Let us examine nonetheless the second question we need to consider: how does rainfall affects incomes in the context of rural Mexico. As we have shown in the discussion above, for rainfall to be a distribution factor, it needs to have differential effects on different sources of income, as well as influencing choices, independently from its effect on total expenditure or household income. In the paper, there is no evidence about the differential effects of rainfall on different components of income or indeed of independent variation of y^B and y^0 across households or villages.

Empirical justification for the use of rainfall and Progresa assignment as distribution factors is provided in section IV-C, page 468, of the paper by showing that these two variables predict total expenditure. But this argument is completely inconsequential. These variables might be affecting consumption because of their effect on total resources. The point is to establish whether they move different components of income, controlled by the two spouses, differently and independently. In the case of PROGRESA this is obvious because the cheques are given to the mothers. In the case of rainfall there is no such evidence. On page 474, paragraph 1, Bobonis explains that rainfall affects total household income through effects on farm profits and on labor earnings. However, it is not clear that many households in the survey have farm profits since most don't own farms and as we have mentioned above, agricultural profits are missing for 96% of households in the sample. The discussion does not address the point of how rainfall would affect men and women's labour incomes differently. Yet,differential effect of rainfall on different sources of income is essential for rainfall to be a valid distribution factor.

The use of rainfall as a distribution factor has appeared in the literature following Duflo and Udry (2004) (see also Goldstein and Udry, 2008). Duflo and Udry, however, study a specific context in the Ivory Coast, where men and women control different plots and engage in the production of different goods that require different amounts of rainfall. Therefore, in that context, a rainfall shock that is good for the husband, can be bad (or less good) for the wife as it affects differently the production of the gender specific commodities. It will therefore change the relative bargaining power within a couple. This is not the case in Mexico, where the majority of our households are wage labourers. We are not aware of any evidence that, for rural Mexico, points to situations similar to that studied by Duflo and Udry (2004) where women and men control the production of different agricultural products. Paradoxically, in the paper, Bobonis provides some evidence that demonstrates that in the Mexican context the use of rainfall as a distribution factor is not warranted. On page 459, he discusses the *ejido* system in Mexico where most women control no land and stresses how the Mexican context is very different from what is prevalent in the West African economies studied by Goldstein and Udry (2008) where women, instead, control and put to productive use several plots of land by engaging in the cultivation of produce which is different from that typically raised by men.

2. *The use of PROGRESA to invert one of the z-demands.* As we have discussed in the text, the BBC09 approach requires at least two distribution factors and the existence of a monotonic relationship between one of the distribution factors used in the analysis and at least one of the commodities in the demand system. If such a relationship exists, one can invert it and use that commodity to summarize the effects of all distribution factors in the rest of the demand system. Monotonicity is required to guarantee the invertibility of the function, that is to be able to express the distribution factor as a function of the commodity. The variable used by Bobonis to invert a commodity demand is the indicator variable for the assignement to PROGRESA. There is no sense in which a monotonic relationship can be defined on a binary variable and, therefore, the relationship between any of the commodities considered and the distribution factor used by Bobonis is not invertible.

Even if one were able to avoid the technical issue (for instance by using the potential Progresa grant as the distribution factor), there is an important argument why one would want to use Progresa as the distribution factor to be 'explained away' rather than as the distribution factor used to construct the z-demand functions. Indeed, it has been widely documented (in our papers and in others we cite) that PROGRESA and similar conditional cash transfers induce a shift in the demand system that is inconsistent with the unitary model. This is powerful evidence because it uses variation in the allocation of resources that is exogenous by construction. Then the interesting question is: can one use a completely unrelated distribution factor together with the structure of the collective model to explain such a shift? This is what we do in our paper and in doing so we set a fairly high hurdle for the collective model.

3 *The demand system estimation.* The demand system Bobonis estimates is specified in terms of log shares, as linear functions of distribution factors and log total expenditure. In the estimation results, all commodities have the same number of observations, 11,733 (contradicting section IV-B, where it is stated that the sample used for the analysis is 3,900 households). In the data, however, expenditures on commodities are zero for many household and it would be impossible to take logs of shares in such instances. In the sample used by Bobonis, and for the goods he analyses, the proportions of zeros are as follows: female clothing, 54%; male clothing, 62%; children's clothing, 30%; education, 56%; transport, 67%; alcohol and tobacco, 95%; medicine, 85%; other household goods, 21%. It appears, from the programs Bobonis posted on line, that he adds a small number to the zeros to be able to take logs. Such a practice is completely arbitrary and should not be used.

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