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Conditional Cash Transfers, Women and the Demand for Food

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Abstract

We examine the effect of large cash transfers on the consumption of food by poor households in rural Mexico. The transfers represent 20% of household income on average, and yet, the budget share of food is unchanged following receipt of this money. This is an important puzzle to solve, particularly so in the context of a social welfare programme designed in part to improve nutrition of individuals in the poorest households. We estimate an Engel curve for food. We rule out price increases, changes in the quality of food consumed and homotheticity of preferences as explanations for this puzzle. We also show that food is a necessity, with a strong negative effect of income on the food budget share. The decrease in food budget share caused by the large increase in income is cancelled by some other relevant aspect of the programme so that the net effect is nil. We argue that the program 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.

Keywords: demand, conditional cash transfer, Engel curves, income elasticities, QUAIDS, food, nutrition.

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1 Introduction

Conditional cash transfer programmes, such as Oportunidades in Mexico, have recently received considerable attention among policy makers and academics alike. Many of these programmes have brought to light an interesting fact: following the injection of relatively large amounts of cash in the budget of poor households, the share of the budget devoted to food is unchanged and, in some cases, even increased. This is in stark contradiction with what is universally observed regarding the relationship between household income and expenditure on food. There is a well known negative relationship, first documented by Engel in the 19th century, between total expenditure and the budget share of food, so that following the increase in household income induced by the program, food budget shares are expected to decrease. Yet, in Mexico as in a number of other countries, the opposite takes place when total expenditure is increased by a conditional cash transfer: sizeable cash transfers to women have been found to be associated with constant or higher shares of expenditure on food in Ecuador (Schady, Rosero 2008), Colombia (Attanasio, Battistin, Mesnard, 2008 (ABM08)), urban areas of Mexico (Angelucci, Attanasio, 2008) and other countries (WB report, 2009). The income effect appears to be exactly cancelled by some other relevant aspect of the programme, so that the net effect of CCT programmes on the share of food is nil or, in some cases, positive.

Conditional cash transfer programmes were put in place to improve investment in human capital in the dimension of education, health and nutrition. The large amounts of cash given to households are designed to enable households to send children to school where otherwise the children would not go to school, by providing a replacement income where the children used to work. They are also designed to enable better nutrition; both by relaxing the budget constraint, and by educating women as to the health benefits of a good nutrition. Most of them are explicitly targeted to women in that

the cash transfers is received by them and in that they have among their objectives the improvement of women conditions.

It is important to assess how the conditional cash transfer programmes influence food consumption; whether they meet the intended goal of improving nutrition; and to establish the channels through which they impact on behaviour. However, prior to the implementation of *Oportunidades*, little was known about the very poor and rural population of interest and how the programme would affect behaviour.

The goal of this work is twofold. The first goal is to characterise food demand for this population of poor households in rural Mexico. We accomplish this objective by estimating a demand system for the population in our sample. This exercise is of interest because little is known about food consumption of very poor households in general and in rural Mexico in particular. Estimation of demand systems on micro data is still rare and a number of methodological issues still require attention.

The second goal is to investigate the channels through which the transfer affects behaviour. Putting a structural interpretation on the observed phenomenon is what will enable to transport to other contexts and to improve on the design of welfare programmes.

Several mechanisms could have led to the observed changes (or otherwise) in the share of food. It could be that changes in the share of food are linked to changes in local prices. If this is the case, assessing the effect of the programme on welfare requires that careful attention is paid to modelling the activity and behaviour of households that are producer-consumers. For such households, the net effect of a price increase depends on the effect on income generated through sale of home produced goods, on the effect on expenditure on foods purchased on the market and on the opportunity cost of home produced goods. Here however, the population under investigation is so poor that few households are producer-consumers, and the value of production consumed is very small (cf Attanasio, Di Maro, Lechene, Phillips, 2009). There still remains the question of whether food prices increased concommitantly to the programme or because of the programme.

Alternatively, it could be that food is a luxury in the populations under investigation at very low levels of income. If that was the case, one would observe that the food share would increase for the poorest house-

holds and decrease for the least poor, so that in the aggregate one could observe either effect. It could also be that preferences of poor households in Latin America are homothetic. The universality of Engel's law, according to which food is a necessity and its relationship with income is negative has been empirically established using data from the North American continent and Europe, but few large data sets linking consumption and income for Latin America existed until the implementation of social welfare programmes such as *Oportunidades* in Mexico, and *Familias en Accion* in Colombia. Although it is unclear why households in Latin America would be different from households in Europe and North America, this cannot be ruled out on a priori grounds.

It could also be that the programme induces changes in preferences for quality of food, for instance because of the information about the importance of good nutrition for health. One important component of the programme is education on the benefits of healthy nutrition habits. Evaluating the impact of information on behaviour is important again for the design of cost effective welfare programmes. However, in this case, it is not clear what to expect on the budget share of food following the transfer. Improvements in nutrition might, but would not necessarily, lead to increased food budget shares, if the increase in quality and expenditure on food are proportionally greater than the increase in income. Without induced changes in preferences, the expected effect of monetary transfers to households is simple: if food is a necessity, the budget share of food should decrease as well as those of the necessities and inferior goods within the food basket, while the shares of luxuries should increase.

Another possibility that needs to be considered is that there is labeling of money, so that money received from a government agency is spent differently from money generated from labour supply choices. This effect has been also researched in the context of federal government transfers to local government. In this context, it is usually referred to as the fly paper effect. Regarding transfers to households, Kooreman (2000) investigates labeling of money.

Finally, it could be that the conditional cash transfer programme affects household decisions through yet another channel. Indeed, all conditional cash transfer programmes in which a constant or higher food budget share

has been reported following the injection of large amount of cash have the common feature that the transfer is paid to women. It could thus be that the increase in the share of resources held by women changes outcomes over and above the change induced by the transfer on the budget constraint. This could occur even if food is normal for both household members.

To summarise, a constant budget share of food could result from an increase in the price of food, from food being a luxury, from homothetic preferences, from modifications in preferences leading to the purchase of better quality food, from labeling of money or from the impact of the transfer on decision making.

The main results of the paper are as follows. Conditional cash transfers have a double effect on choices. The first effect, induced by the increase in the total income of the household, is the expected negative effect on the budget share of food. Further to showing that food is a necessity and that preferences are not homothetic, we also show that prices of food have not increased. We argue that it is unlikely that the programme induces changes in preferences and we similarly find no grounds to the labeling of money hypothesis. The second effect of the programme is to raise the budget share of food by an amount matching the decrease due to the income effect. We argue that the only aspect of this, and other similar conditional cash programmes, which can explain the puzzle, is the fact that the transfer is put in the hands of women. Once we account for this shift in the Engel curve for food, the impact of the programme as evaluated by the structural estimates is in line with the impact observed on the raw data. In other words, there is an impact of the transfer on decision making, over and above its effect on the budget constraint.

We follow Attanasio, Battistin, Mesnard (2009, henceforth ABM09) who estimate an Engel curve for food using data from a conditional cash transfer similar to *Oportunidades* for poor households in rural Colombia. They establish the form of the Engel curve for food and document the effect of the programme. They find that instrumenting total expenditure is particularly crucial. Without instrumenting, food is a luxury at low levels of total expenditure and a necessity at higher levels. Instrumenting with an exogenous measure of expected income, food is found to be a necessity everywhere. ABM09 use comparison of structural and quasi-experimental estimates to

inform functional form choice. They find that the flexibility of a QUAIDS model, in which the coefficient on the square of the logarithm of total expenditure depends on prices, is essential in this context. Since they do not observe prices, they use village dummies to proxy for prices, in which case it is not possible to use village wages as an instrument for total expenditure. Given that we have partial information on prices, we take a similar route and control for spatial differences in relative prices with regional indicators.

We also follow Hoddinott and Skoufias (2004) who examine the effect of *Oportunidades* on food consumption. They pay particular attention to the survey design and practical aspects of the implementation of the programme. They find that there is an effect of the transfers on calorie intakes over and above the income effect of the transfer.

In section 2, we describe the Mexican context, the *Oportunidades* programme and the data collected for its evaluation. We present the sample used in the analysis and discuss the construction of prices. We document the fact that food budget shares are constant after households receive large cash transfers. In this section, we also show that the solution to the puzzle is not an increase in prices or a change in the quality of food consumed. In section 3, we revisit a number of methodological issues that arise when estimating Engel curves. Among these issues are the choice of functional form for the Engel curve and the instrumenting of total expenditure. We also detail two specific issues that arise in this context. The first one concerns the minimum school attendance requirement imposed by the conditionality of the programme, which might have bearing on food consumption. The second specific issue in this context is the fact that although we are interested in the estimation of an Engel curve, there exist spatial differences in relative prices which need to be accounted for. A further difficulty is that prices are only partially observed. Section 4 details the estimation results. We show that food is a necessity for poor households in rural Mexico. We show that a standard Engel curve fails to predict the effect of the programme. We further show how to modify the Engel curve to obtain predictions matching the observed effects. Section 5 summarises the results and discusses the implications of our preferred specification. Section 6 concludes.

2 Mexico, *Oportunidades*, the data and sample

Mexico After a major crisis in 1994/5, Mexico has enjoyed sustained economic growth until the global financial crisis of 2008. Although it is now a middle income country (ranked 12th out of 228 in terms of GDP and 83rd in terms of GDP per capita in 2009), growth has benefitted the population unequally, with southern states and rural areas in general remaining impoverished. Despite the rapid process of urbanisation, a large fraction of the population remains rural, poor, and vulnerable to shocks as reliant on food crops for subsistence and income generation.

Oportunidades A number of social welfare programmes have been in place in Mexico for several years, to try to alleviate poverty and improve nutrition and health. Most of these consisted in transfers in kind or food subsidies. The different interventions were consolidated into one large scale welfare program, *Progresa*, then renamed *Oportunidades*, introduced in 1997. *Oportunidades* is the first, most studied and best known Conditional Cash Transfer programme. It is also considered a success in many dimensions, and the gold standard of welfare programmes. The programme has had a positive impact in the short term on poverty. It also marks important changes in the design and delivery of interventions and welfare programmes. The price subsidies and transfers in kind of previous programmes 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 will come back to this aspect below.

At the start in 1997, 300,000 families were programme beneficiaries, for 5 million now, or 25 million individuals representing 25% of the population. *Oportunidades* has the largest budget of all human development programmes.

Hoddinott and Skoufias (2004), the World Bank CCT Policy Research Report (2009), and IFPRI reports (http://www.ifpri.org/) contain detailed descriptions and analysis of the effects of *Oportunidades*. The programme's

website contains up to date description of the programme and of its impacts: http://www.oportunidades.gob.mx/index.html.

The data The means of the evaluation of the effect of the programme was built in from the start, with randomisation of recipients and regular surveys of the populations treated and of control populations. In particular, 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. 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 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 are capped at 490 pesos per month and per household from January to June 1998 rising to 625 pesos from July to December 1999 (Hoddinott and Skoufias, 2004). The grants are paid to the households every two months.

Several practical aspects pertaining to the implementation of the programme are relevant for the analysis. *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 regards schooling. Receipt of the education grant is conditional on school attendance. In practice, nearly all children go to primary school, and the programme mainly targets secondary and "preparatoria" school age children. Other conditions are to do with health checks and attendance to meetings for mothers of young children. The grants are paid to the women, in person, by private bank employees, on the basis of fulfillment of the programme conditions during the preceeding period.

The sample Our interest is in the effect of the programme on outcomes for the poor, so that we select a sub sample of households considered as eligible for the programme in 1997, residing either in control or treatment villages. 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 14,397 households, of which 7,400 observed in October 1998 and 6,997 observed in May 1999. Of these, 62.47% (8994 households) are in treatment villages and 37.53% (5403 households) are in control villages.

In Table 1, we report some descriptive statistics from our 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).

The sample reflects the fact that we are dealing with a very poor population. The food budget share is on average 80% of total expenditure[1].

[1] We include a valuation of in-kind consumption.

Education of both head and spouse are low[2]. 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 7 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. We will control for this in the empirical analysis to correct for the direct effect on food expenditure of either.

Table 1: Descriptive statistics: Means

	Control	Treatment
Budget share of food	80.16	79.46
Education of head	2.19	2.22
Education of spouse	2.15	2.15
Head indigenous	0.38	0.38
Age of head	39.55	39.39
Head male	0.96	0.96
Townsize	405.67	390.35
Region Guerrero	0.07	0.10
Region Hildalgo	0.12	0.19
Region Michoacan	0.12	0.13
Region Puebla	0.15	0.16
Region Queretaro	0.05	0.04
Region San Luis Potosi	0.14	0.14
Region Veracruz	0.35	0.23
Household size	5.99	5.99
Nb young children	2.42	2.44
Nb old children	1.56	1.55
Children in primary school	1.49	1.54
Children in secondary school	0.30	0.35
Distance secondary school	2311	2162
Dummy secondary school	0.24	0.26
Distance primary school	0.61	0.23
Relatives eat in	0.07	0.08
Household members eat out	0.02	0.02
Nb obs.	5403	8994

[2]Education categories are: incomplete primary, primary, incomplete secondary, secondary and above.

2.1 Construction of prices for non durables

Under the assumption of two stage budgetting, food consumption is determined by reference to the price of food, the prices of other non durables, and total expenditure on non durables. We are interested in the relationship between total expenditure (on non durables) and the budget share of food, but given that the survey covers a large geographical area, it is conceivable that there are spatial differences in the relative price of food versus non food items of non durable consumption. To control for this, we would ideally control for the relative price of food in the food Engel curve. However, whilst it is possible to construct unit values for food stuff, this is not possible for other non durables. The survey records purchases of non durables over a period of one week for transport and tobacco, one month for personal hygiene, cleaning products, drugs, medical visits, school fees, and heating and lighting of the house and six months for utensils for the house, toys for children, clothes and shoes for all household members and ceremonies. The infrequency of purchases of these articles over the time periods stipulated is such that there are numerous households who report no purchases of these items over the period of observation. For twelve out of sixteen expenditure categories, the fraction of zeros is greater than 50%. Moreover, for most of these commodities, we only observe expenditure, unlike for food items where we also observe quantity. Therefore, it is not possible to construct unit values at the household level or even at the village level for the prices of these goods given the large proportion of zeros.

An alternative to the use of unit values is to use shop prices. It is the case that locality level information on prices has been collected in separate questionnaires from the household questionnaires, at the same survey dates as the household questionnaires. Unfortunately data on locality prices is available only for a very small number of localities.[3]

This discussion shows that it is not possible to construct credible variables of price information for non durables. This means that the question of how to control for spatial difference in relative prices must be dealt by proxying for relative price differences. One possibility is to use village indicators. However, in our context, where we have observations from 506

[3] Data on shop prices is available in further waves of the survey.

villages and two time periods, this is not an appealing route. Not only is it computationally cumbersome, but it also would preclude the use of village level variation in any other dimension. Moreover, it would imply assuming differences in relative prices across localities which are occasionally relatively close. Therefore we follow a pragmatic approach and allow for differences in relative prices in a given time periods across states, but assume that relative prices are constant within a state at a point in time. In the empirical analysis, therefore, we will be using state level dummy variables to control for spatial differences in relative prices.

2.2 Effect of the program on the budget share of food

Given the availability of the experimental setup, we can estimate the impact of the programme on total expenditure and on the share of food in a very simple way and with a minimal set of assumptions. These include the fact that there is no effect (maybe through anticipation) on the control localities. The impact can then be simply obtained comparing averages in treatment and control localities. In Table 2 we report the mean of the distribution of total non durable consumption and of the share of food in treatment and control villages both in October 1998 and in May 1999. The program, not surprisingly, increases the consumption of non durables considerably. In May 1999, the increase is 17%, 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. The increase in October 1998, when the programme had only just started, is considerably smaller, but still sizeable at 7%. Such a modest increase might be justified 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 that in the literature and the fact that the increase in total consumption is below the amount of the grant has been noted by Gertler, Martinez and Rubio-Codina (2009).

The standard errors of the differences in the total log consumption are respectively 0.011 and 0.012 for October 1998 and May 1999. Given the value of the corresponding differences (respectively 0.07 and 0.17), the differences in mean log consumption expenditure between control and treatment villages are significant. However, turning now to the differences between the food

budget shares in control and treatment villages, we find that we cannot reject that these are zero. The standard error of the difference is 0.03 in both periods, associated with differences of 0.48% and 0.70% respectively.

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			
	C	T	Diff	SE of diff	C	T	Diff	SE of diff
Total log consumption expenditure	6.72	6.79	0.07	0.011	6.68	6.85	0.17	0.012
Share of Food	83.37	82.89	−0.48	0.03	80.16	79.46	−0.70	0.03
Nb of obs	2844	4556			2559	4438		
Budget shares are multiplied by 100								

It is therefore the case that in Mexico, as in other countries where similar programmes have been operating, the share of food is constant after the transfer. A priori, the increase in income and total expenditure induced by the programme should lead to a decrease in the budget share of food, if food is a necessity. The simple comparison we present on the raw data shows that the share of food is constant after the introduction of the programme.

2.3 Prices and quality of food

Prices paid and the quality of food purchased can vary so as to generate constant or higher shares of food. This can happen in several manners. The amounts transferred by the programme are important and it could be that such large injection of cash has an effect on the local food markets. Increased demand for food stuff may lead to price increases, in which case the transfer does not necessarily reach its intended beneficiaries. Alternatively, it could be that to some extent, households substitute higher quality of foods following receipt of the transfer. This would be a desirable outcome of the programme.

The data should ideally enable to answer these questions directly by examining whether the prices paid for given quality have increased and whether the quality of the food purchased has changed. However, neither qualities nor prices for given qualities are observed. Instead, we observe expenditures and quantities of food items, from which we construct unit values. An increase in unit value can be due to an increase in price for constant quality or an incrase in quality of foods purchased. The unit values are constructed

using information on expenditure and quantity from purchase data from individual households, then aggreggated to the village level.

A given village is either control or treatment. Prices are constructed using information on quantities purchased and expenditure, at the village level. Therefore, to see whether the price of food increases with the program, we compare prices of the five food groups which compose the food basket, in treatment and in control villages. Table 3 shows the mean price of foods in control and treatment villages in October 1998 (in pesos per kilogram) and the differences in mean prices between control and treatment villages. It is apparent in this table that households in treatment villages do not face systematically higher prices than households in control villages. On the contrary, prices are lower in treatment villages than in control villages for three of the five foods in October 1998, and of two foods in May 1999. For the other foods, prices are higher in treatment villages, but whilst the differences are significant, they are small, for all foods apart from corn. This pattern for prices in control and treatment villages has been further documented in Hoddinott, Skoufias and Washburn (2000) and Angelucci and DeGiorgi (2009). Over the period we study, food prices are relatively stable, whereas they increase considerably in the decade that follows, and particularly from 2005 to 2008.[4]

Table 3 Comparison of prices between control and treated villages in October 1998 and May 1999

	October 1998					May 1999				
	C	T	Diff	% diff	SE of diff	C	T	Diff	% diff	SE of diff
Corn	3.60	3.66	0.06	1.66	0.012	3.47	3.66	0.19	5.46	0.018
Pulses	11.36	11.30	-0.06	-0.53	0.021	10.12	10.18	0.06	0.59	0.009
Fruit and vegetables	6.89	6.82	-0.07	-1.02	0.019	5.27	5.19	-0.08	1.52	0.013
Meat, fish and dairy	14.83	14.97	0.17	1.15	0.054	15.45	15.89	0.44	2.85	0.053
Other foods	11.00	10.81	-0.19	-1.73	0.039	10.08	9.98	-0.1	0.99	0.038
Sample size	2844	4556				2559	4438			

[4] Around 2005, the price of tortilla, a staple food in Mexico, has increased by up to 50% in some areas, or more than 10 times the increase in the minimum wage over the same period. Attanasio, Di Maro, Lechene and Philipps (2009) study the effect of the enormous increase in prices on the welfare and food consumption of poor households in *Oportunidades*. The years we study are still in the period of declining food prices started more than 20 years ago, with price of rice, corn and wheat, which constitute the staple foods in Mexico as in many countries for poor households, not only low but stable (see, among others, Timmer, 2008).

The absence of difference between budget shares of households in control and treated villages is not due to increased prices, and since we are looking at unit values, we can also rule out increased quality of the foods purchased. The next step is to rule out that this is due to homothetic preferences or to food being a luxury. To do so, it is necessary to estimate an Engel curve for food. We now turn to a number of issues that need addressing in order to do so.

3 Engel curve for food: Methodological issues

We are interested in the understanding the effect of the CCT programme on the budget share of food. To this end, we propose to estimate an Engel curve for food, that is the relationship between income (or total expenditure) and the budget share of food, conditioning on relevant preference shifters and characteristics of the environment. Assuming that households maximise a utility function subject to a household budget constraint, the Engel curve is specified so as to be theory consistent, allowing to relate the results obtained here to results obtained when studying the demand system for foods in Mexico (for instance to those obtained in Attanasio, Di Maro, Lechene and Phillips, (2009)[5] and Attanasio and Lechene, (2010)). A number of issues concerning the functional form of the Engel curve and the measurement of the pertinent elements need to be addressed. The issues to consider are 1) whether the relationship between the food budget share and total expenditure is linear or quadratic, ie whether the Engel curve is derived from an Aids or a Quaids, 2) how to deal with the potential endogeneity of total expenditure, and 3) how to account for the fact that the transfers are conditional on school attendance, certainly inducing changes in spending patterns to cover some of the costs of attending school, 4) which other specific influences need to be accounted for in this context, for instance the support or the demands made on a household by their family network, 5) how to deal with the fact that some pertinent information, namely prices, is not observed for part of the system.

[5] The focus of Attanasio, Di Maro, Lechene and Phillips (2009) is to measure the impact of the price increases on the welfare of households in Mexico. They do not select on poor households as is done here, so that some of the results are not readily comparable.

3.1 Functional form: Aids or Quaids

In the context of the estimation of an Engel curve, the question of whether the demand for the good is derived from an Aids or a Quaids is the question of how income responses are allowed to vary with income.

The Almost Ideal Demand System (AIDS) introduced by Deaton and Muellbauer (1980) and the generalisation suggested by Banks, Blundell and Lewbell (1997) to the Quadratic Almost Ideal Demand System (QUAIDS) are both integrable. The latter allows for more flexible relationship between income and expenditure shares. In the case of interest here, it is important to allow for the possibility that a good might be a luxury at very low levels of income and a necessity afterwards, which is not possible with an AIDS. This would be consonant with the idea that for poor households at subsistence level, there might be some indivisibilities in the allocation of the budget so that as the budget constraint is relaxed, the budget share of food might initially increase, making food a luxury at very low levels of income[6]. In the event, we will reject this possibility, but it is worth noting that this can only be done in the framework of a quadratic Engel curve, ie corresponding to an underlying Quaids.

In a Quaids, the budget shares take the following form:

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

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

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

For this model to be consistent with utility maximisation, the following theoretical restrictions have to hold.

[6] This would be the case for instance if there was indivisibility in shelter, so that food would be a luxury at low income levels.

For adding-up:

$$\sum_{i=1}^{n} \alpha_i = 1; \qquad \sum_{i=1}^{n} \beta_i = 0; \qquad \sum_{i=1}^{n} \gamma_{ij} = 0 \qquad \forall j; \qquad \sum_{i=1}^{n} \lambda_i = 0$$

For homogeneity, the following additional restriction has to hold as well:

$$\sum_{j=1}^{n} \gamma_{ij} = 0 \qquad \forall i$$

And finally, for symmetry:

$$\gamma_{ij} = \gamma_{ji}$$

For homogeneity to hold, the price index $a(p)$ must be homogenous of degree 1 in prices and expenditure, and $b(p)$ homogenous of degree 0.

In this model, the income elasticity is

$$\eta_i = \frac{\mu_i}{w_i} + 1 \tag{2}$$

where

$$\mu_i = \frac{\partial w_i}{\partial x} = \beta_i + \frac{2\lambda_i}{b(p)} \left\{ \ln \left(\frac{x}{a(p)} \right) \right\}$$

In these equations, demographics are assumed to enter in the intercept term of the shares equations. In particular, for commodity i we assume that the parameter α_i is given by the following expression:

$$\alpha_i = \alpha_{0i} + \sum_{k=1}^{K} \alpha_{ki} z_k \tag{3}$$

where the z_k are the K demographic variables that enter the system. Homogeneity implies the additional restrictions:

$$\sum_{i=1}^{n} \alpha_{0i} = 1; \qquad \sum_{i=1}^{n} \alpha_{ki} = 0, \qquad \forall k;$$

As discussed in BBL, the demand system in equation (1) combines functional form flexibility to consistency with theory, in that it is integrable. The last term in equation (1) makes the demand system of rank 3, the highest admissible rank for a theory-consistent demand system that is exactly aggregable, in that it is linear in functions of total expenditure.

In the case of Aids, the budget shares take the form

$$w_i = \alpha_i + \sum_{j=1}^{n} \gamma_{ij} \ln(p_j) + \beta_i \ln\left(\frac{x}{a(p)}\right) \tag{4}$$

where $a(p)$ is the price index defined as before.

Consistency with theory is obtained if the same restrictions as those that need to be verified in the case of the Quaids are verified, except the restriction on the coefficient on the quadratic term in total expenditure.

In this model, the income elasticity has the same form as in the Quaids, again with the coefficient on the quadratic term in total expenditure set to zero, so that if $\beta_i > 0$, the good is a luxury at all levels of income.

3.2 Endogeneity of total expenditure on food

Under the assumption of two stage budgeting, households decide how much to allocate to each period and then how to allocate total expenditure in the period between the different goods. If unobserved taste shifters for goods are correlated with heterogeneity in time preference, for instance if impatient individuals are also more hedonistic, then total expenditure is endogenous. Measurement error in total expenditure is also a likely cause of endogeneity. One often used instrument is household income, if the measurement error in total expenditure does not affect 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. Even if these assumptions do not fail, so that income is uncorrelated with the errors in the demand equation, income can be a poor instrument in a context where large transitory shocks may weaken the relationship between income and total expenditure. An alternative is the wage, exogenous in demand if leisure is separable from commodities in the preferences. Another candidate is to use a locality level measure of the price of leisure. This has the double advantage to get around the problem of endogeneity due to non separability, as well as measurement error of the wage. However, this is mitigated by the fact that by using a locality level variable, we are losing the dimension of individual variability, which can be presumed quite important. Furthermore, this precludes the use of locality level variables to capture other relevant

aspects, in particular to proxy for unobserved prices, as we explain below. We report below the results we obtain with income and then with median locality level agricultural wage as instrument for total expenditure on food.

In the context of rural households, whose activity is in part or mainly agricultural, it is potentially important to control for consumption of home produced goods. The survey records the quantity consumed of 36 food items, and whether these are purchased or home produced. We value the home produced consumption using local village level unit values computed using information on purchases of the same foods. We find that home produced consumption represents less than 6% of consumption.

3.3 Schooling

Conditional cash transfer programmes impose minimum schooling requirements for children of the recipient households. The opportunity cost of schooling is one of the reasons put forward as a reason for the low school attendance of children in rural Mexico. 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. 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 distance from primary school, an indicator for the existence of a secondary school and distance from secondary school if it is not in the village.

3.4 Controlling for partially observed prices

When estimating the food Engel curve, under the maintained assumption of separability between durables and non durables in preferences, we do not need to control for prices. However, it is the case that relative prices of foods differ between villages, and furthermore the price of food relative to other

non durable might vary, which needs to be accounted for in the empirical application. As we mentioned above, it is not possible to construct prices for non durables using this data, because of the large proportion of zero in the purchase data.

There are three possibilities. The first possibility is that we could use village dummies for prices in the food Engel curve. In this case, we would not be able to use the village level agricultural wage as an instrument for total expenditure on non durables. We could in theory use household income as an alternative instrument. In some context, this might even be more appealing, as it varies between households. A second possibility is to impute wages using age, education, village dummies and the median village agricultural wage. The third possibility is to use some proxy for the variation in relative prices using some other price.

Here we chose to control for price differences by conditioning on indicators of region and time. This amounts to allowing for intercept shifts by region*time in an AIDS, and for intercept shifs as well as for different coefficients on log total expenditure and its square in a QUAIDS. Let r_i, $i = 1, ..., 14$ be the interactions between the 7 regions and the 2 time periods. In an AIDS, the budget share of food is written as:

$$w = \alpha + \sum_{i=1}^{14} \gamma_i r_i + \beta \ln(x) \tag{5}$$

In a QUAIDS:

$$w = \alpha + \sum_{i=1}^{14} \gamma_i r_i + \sum_{i=1}^{14} \beta_i r_i * \ln(x) + \sum_{i=1}^{14} \delta_i r_i \ln(x)^2 \tag{6}$$

4 Engel curve for food

In this section, we implement what we discuss above and estimate Engel curves for food. The randomised design of the programme opens an additional avenue of investigation. It gives the possiblity of establishing the properties of the Engel on the control localities, and to test whether the Engel curves are stable between control and treated localities. Our discussion is therefore organized as follows. We first address the set of methodological issues mentioned above, investigating the properties of the food Engel curve in

controls localities, that is in the absence of *PROGRESA/Oportunidades.* We investigage whether an AIDS or a QUAIDS representation is appropriate; whether to instrument total expenditure and if it is found to be necessary, which instrument to use; how to control for the conditional requirement that children attend school; whether to control for unobserved prices and whether to do so using village level indicators or region level indicators.

We then show that the Engel curve is 'different' in treatment localities. As the two sets of localities are statistically identical, as it has been showed repeatedly, we infer that the Engel curve is 'shifted' by the programme.

In the next step, we move on to show how to fit the data from treatment localities, that is how to modify the Engel curves to take into account the presence of the programme.

Finally, we speculate on how to interpret the shift in Engel curves we document.

4.1 Engel curve in control villages

In reporting the results we obtain investigating the food Engel curve, we focus on the shape of the relationship between the food share and the log of total expenditure. For this reason, we will report the coefficients on log total expenditure obtained under different approaches. The complete set of results, which include the coefficients on the control variables, is available upon request.

As discussed above, we deal with several issues: the shape of the Engel curves, the endogeneity of schooling and total expenditure and whether controlling for price variation across different regions makes a difference. Our approach is to present the results of different specifications and test the various alternatives. To deal with endogeneity, we will be using a control function approach which can be easily adapted to the linear and quadratic case. To implement the control function, as for Instrumental Variables, we need 'instruments'. We start by discussing the first stage regressions for schooling and total expenditure.

4.1.1 First stage regressions

The main purpose of this sub-section is to check whether the instruments we consider for schooling and total expenditure have additional explanatory power to explain the variability in the endogenous variable. We start with the two schooling variables: the number of children in primary school and the number of children in junior and senior high school. The results we summarize here are in Table A1 in the appendix. The instruments we use for these variables are an indicator for the distance to a primary school, an indicator of the presence of a secondary school and the distance to the secondary school. The instruments have the expected effects and are strongly significant. A greater distance to the closest primary school significantly reduces the number of children going to school. The indicator for the presence of a secondary and the distance to secondary school have no power in the equation of the number of children attending primary school. However, distance to secondary school influences the number of children attending secondary school in the expected direction, while the information regarding the primary school has no power in the equation for secondary school. Education of the mother plays a role in determining the number of children in secondary education but not in primary; the father's level of education is irrelevant.

When we turn to the first stage regressions for total log expenditure, table A2 in the appendix. The first column gives the results obtained when using the median village agricultural wage as instrument for total expenditure and the second using income. We find that village median agricultural wage has power in explaining total expenditure for control households. In particular, the F-statistic for the hypothesis that the coefficients on the median agricultural wage and its square are zero in the first stage regression for total expenditure is above 30.

The alternative instrument, household income, also has power as instrument for total expenditure: the F-statistic for the hypothesis that the coefficients on household income and its square is zero is 60. As with the first instrument, we strongly reject the hypothesis of exogeneity of total expenditure.

4.1.2 Second stage regressions : Linear specifications (AIDS)

Table 4 shows the results obtained with a specification which is linear in the log of total expenditure. In column 1, total expenditure and schooling are exogenous; in column 2, we allow for endogeneity of schooling; in column 3, we also instrument total expenditure with the median village level agricultural wage, and in column 4 we instrument total expenditure with household income. The results in columns 2 to 4 are obtained using a control function approach. Specifically, having estimated a first stage regression where the endogenous variables are regressed on all exogenous variables in the regression and the instrument, we add the residuals of this equation and their powers in the second stage regression. Obviously this approach is identical to Instrumental Variables in the linear case, while it will differ from IV in the non-linear case. Note that for specifications (1), (2), (4), (5), (6) and (8), the sample is trimmed for the bottom and top percentiles of the sample in income, whilst for (3) and (7), it is trimmed for the corresponding values of wage. We have checked that the trimming is without qualitative effect on the results, it has very marginal quantitative effect.

In columns 1 to 4, we do not account for the relative differences in prices. In columns 5 to 8, we repeat the order of columns 1 to 4, but allowing for price differences by adding regional dummies. Notice that theory implies that regional differences in prices are reflected in intercept shifts if the Engel curve is linear. In Table 4, we report the coefficient on total expenditure and the corresponding t-value for the hypothesis that the coefficient is zero. The controls that are included in all specifications are: number of children in primary school, number of children in secondary and preparatoria school, the number of young children, household size, the square of household size, townsize, education of head of household and education of spouse, an indicator of whether the head of household is indigenous, the age of the head of household. We also control for the fact that either relatives eating with the household, or members of the household eating out, can influence the budget share of food. An instance of the latter would be if children of the household eat in school.

Several things are apparent from these results. Whether one controls for prices or not, within a linear framework, does not appear to matter greatly.

Instrumenting schooling and especially total expenditure, however, is crucial: the coefficient on total log consumption moves from -0.05 in column (1) to -0.2 in column (3) and (4). Which of the two instruments considered for total expenditure we use does not matter for the point estimates of the coefficient on total consumption. This coefficient, however, is estimated more precisely when total household income is used, especially when we control for regional effects.

The control function approach we use yields as a by-product a test of endogeneity of the variables that are considered as such: in particular, the null hypothesis that the powers of the residuals of a first stage regression are jointly zero corresponds to the hypothesis that that variable is exogenous. Primary school appears to be exogenous in the Engel curve for food, while we reject the assumption of exogeneity for secondary school attendance. Our preferred specification will be to treat both schooling levels as endogenous in the food Engel curve. The residuals of the first order regression for total expenditure are strongly significant in the food budget share equation, therefore rejecting the hypothesis of exogeneity of total expenditure.

Table 4: Food Engel Curve

Coefficients on $ln(x)$

	No price heterogeneity				Regional price differences			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Endog. Instr.		Educ	Educ & ln(x) Wage	Educ. & ln(x) Income		Educ	Educ & ln(x) Wage	Educ. & ln(x) Income
$ln(x)$	-0.048	-0.074	-0.205	-0.196	-0.042	-0.067	-0.160	-0.181
	(-7.92)	(-8.01)	(-4.53)	(-8.05)	(-7.09)	(-4.02)	(-2.83)	(-5.48)

T values in parentheses, standard errors clustered at the village level

These results show that, conditional on linearity, food is a necessity. Preferences are thus not homothetic. If we take the results in the last column as our favourite specification, an increase in total expenditure by 10 percent would decrease the budget share of food by 2 percentage points.

While we do not report the full set of coefficients, the effect of other controls can be summarized as follows. Although members of the household regularly eating out significantly decreases the budget share of food (close to 4 percentage points per member of the household eating out), less than 5%

of the households report one or more household members eating out (this is for the whole sample, not only the controls). Regularly hosting outsiders for meals has no significant effect on the budget share of food. Here again, it is the case that very few households are concerned by this, with less than 5% of households reporting the regular presence of outsiders at their table. The effect of other socio-demographic variables are as follows. Children, whether in primary or secondary and preparatoria, decrease the budget share of food, for a given household size. Given that we select households with two adults and any number of children, the positive and significant effect of household size on the budget share of food captures the effect on food of children not in school (either because they are older or because they are not attending school even if of school age). The level of education of the spouse influences the budget share of food but not that of the head of household. Finally, the budget share of food is on average lower at the second date of observation, in May 1999, than in October 1998.

When we control for differences in regional prices, these results, by and large, hold (cf columns 5 to 8 in table 4). The only noticeable difference is the result we obtain when we control for prices and use average wages in the locality as instrument. In that case, the coefficient on total log expenditure is slightly smaller in absolute value (-0.16). At the same time, however, the estimate is much less precise, so that our estimate is not significantly different from -0.2, which is what we obtain when ignoring prices or when using individual income as an instrument. The fact that average locality wages only vary across localities explains the fall in precision when we introduce regional dummies in the second stage regression.

4.1.3 Second stage regressions: quadratic specification (QUAIDS)

We now allow for the possibility of non linearity in total expenditure in the Engel curve. The first four columns of table 5 give the results obtained without controlling for prices, while the last four report the results obtained from the same specifications, but controlling for price differences with regional dummies. In column 1, total expenditure and schooling are exogenous; in column 2, schooling is instrumented, in column 3 schooling is instrumented and total expenditure is instrumented with village median agricultural wage and finally in column 4, total expenditure is instrumented

with income. In the QUAIDS model, if prices are not constant, they interact with both expenditure terms, so that if we control for regional differences, we have to add not only intercept shifts but also 'slope' shifts (cf section 3.4). In the table below, we report the average coefficients. The trimming is done similarly as in the exercise above.

If we do not control for the endogeneity of total expenditure, the quadratic terms in total log consumption seems to be significant, especially when we control for price differences (see columns 2, 5 and 6). However, whenever we allow for the possibility that total expenditure is endogenous, the significance of the quadratic terms disappears. Total expenditure, appears to be endogenous, in that the terms in the residuals of the first stage regression are significant. Our preferred specification, therefore, are those where we instrument both schooling decisions and total expenditure.

The evidence in Table 5, therefore, indicates that there are no significant quadratic effects. Not only are the coefficients on the quadratic terms insignificantly different from zero, the fitted Engel curves are not too dissimilar from the linear ones. From this evidence we conclude that the best fit in the control sample for the food Engel curve, is a linear one where both total expenditure and schooling decisions are treated as endogenous. This specification rejects homotheticity and implies that food is a necessity.

Table 5: Food Engel Curve

Coefficient on $ln(x)$ and $ln(x)^2$

	No price heterogeneity				Regional price differences			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Endog. Instr.		Educ	Educ & ln(x) Wage	Educ & ln(x) Income		Educ	Educ & ln(x) Wage	Educ & ln(x) Income
$ln(x)$	-0.194	-0.309	-0.09	-0.072	-0.320	-0.533	-0.36	-0.310
	(-1.98)	(-3.04)	(-0.63)	(-0.50)	(-2.95)	(-3.14)	(-2.08)	(-1.69)
$ln(x)^2$	0.011	0.017	-0.01	-0.009	0.020	0.034	0.02	0.012
	(1.47)	(2.32)	(-0.79)	(-0.82)	(2.42)	(2.84)	(1.34)	(0.89)

T-values in parentheses, standard errors clustered at the village level

4.2 Predicting the impact of the programme with the estimated Engel curves.

We have established that the preferred specification for a food Engel curve in the population under study is linear in (log) total expenditure, controlling for relative prices differences using region indicators interacted with a time indicator. We also established that schooling and especially total expenditure should be endogenous. To estimate the coefficients we could use either of the instruments considered, but we obtained more precise estimates with total household income. If we take our estimates at face value we can use the estimated Engel curve and the experimental estimates of the impact of the program on total log expenditure to predict the impact of the program on the food share.

The difference in log total expenditure between treatment and control sample over the two waves of the survey is 6.82-6.71=0.11. If the slope of the Engel curve is -0.2, the model would predict that the programme generates a decline of 2.2 percentage points in the food budget share. If we consider the May 1999 data, the effect of the programme on total consumption is an increase of 17%, which, with the same coefficient of -0.2, would imply a decline in the food share of 3.4 percentage points.

These predicted declines can be compared with estimates derived from the comparison between treatment and control localities. Pooling the two sample dates, the average budget share of food is 81.74% for the control households, and 81.52% for the treated households. The difference is thus 0.22 percentage points, to compare with the predicted impacts obtained using the estimation results from above. If we consider the May 1999, the estimate is -0.21 percentage points to be compared with the -3.4 percentage points predicted by the Engel curve estimates.

Table 6 below computes, for some of the specifications we have considered, the estimated impact, for the total sample and for the two waves separately. Interestingly, the only specification that predicts something similar to the observed impact is the QUAIDS model estimated by OLS, that is ignoring the endogeneity of schooling and, crucially, of total expenditure. However, the evidence we presented above rejects strongly the exogeneity of total expenditure. We are therefore led to conclude the best specification of

the Engel curve estimated on pre-programme data is not able to predict the impact of the programme.

Table 6 Predicting food share
with different specifications

	Total sample	October 1998	May 1999
Experimental impact	-0.22	-0.23	-0.21
QUAIDS, Exog. exp., no price control	-0.84	-0.68	-1.09
QUAIDS, Endog exp. no price control	-2.18	-1.34	-3.18
QUAIDS, Exog. Exp., price controls	-0.004	-0.002	-0.006
QUAIDS, Endog. Exp., price controls	-0.011	-0.005	-0.017
AIDS, Endog. Exp., no price controls	-2.22	-1.44	-3.18
AIDS, Endog. Exp., price controls	-2.06	-1.33	-2.95

Note: In the specifications where total expenditure is considered endogenous total household income is used as an instrument. Schooling is also instrumented.

4.3 Engel curve on treatment and controls: accounting for the programme

We have thus established that the Engel curve estimated in the absence of the programme cannot predict its impact. From this starting point, we now turn to the issue of how to modify our specification in a parsimonious way so as to fit the impact of the programme. As we are looking at the relationship between the share of food and total log consumption (which is obviously affected by the programme) we can potentially fit a shift in this relationship induced by the programme as an intercept shift or as a shift in the slope of the relationship. We then turn to the data to discriminate between these two possibilities.

Table 7 shows that the effect of the programme is best captured by an intercept shift. Starting with our favourite specification (an AIDS model, where price differences are captured by region dummies and where total con-

sumption is instrumented by total household income), we let the presence of the programme (in the treatment communities) shift both the intercept and the slope of the relationship between food share and total consumption. Specifically, in column (1) we reproduce the benchmark Engel curve, estimated on controls localities data only. In columns (2) and (3) we report the results obtained in the pooled sample. In particular, we estimate the parameters of the following equation:

$$w = \alpha + \delta P + \sum_{i=1}^{14} \gamma_i r_i + (\beta + \phi P) \ln(x) + \theta' Z + u \tag{7}$$

where we omit an individual index for simplicity. The variable P takes the value 1 for observations living in treatment villages. The regional dummies r_i, as before, capture differences in local relative prices. The vector Z includes a number of controls, such as family composition variables. As in our favourite specification, we consider total expenditure (as well as schooling) as endogenous and estimate the coefficients in equation (7) by a control function approach.

We report our estimates for β, δ, and ϕ in Table 7. A complete set of estimates is available upon request. As expected, the effect of the programme is strongly significant, when entered as an intercept shift. However, when we let the programme shift both the intercept and the slope, only the former is affected significantly.

Table 7 Food Engel Curve
with programme specific intercepts and slopes

	(1)	(2)	(3)
β	-0.181	-0.220	-0.194
	(-5.48)	(-8.23)	(-7.76)
δ		0.020	0.357
		(3.68)	(1.78)
ϕ			-0.050
			(-1.67)
# of observations		10615	

Note: Total expenditure instrumented with income. Schooling instrumented.

Given these results, we can check whether our new specification fits the impact of the programme. For such a purpose, we re-do the exercise in Table 6, but instead of using the specification estimated on pre-programme data, we use the specification in Column (2) in Table 7. Such a specification predicts that the change on food shares induced by the programme, under the assumption that it has no effect on prices, is given by:

$$\Delta w = \delta + \beta \Delta \ln(x) \tag{8}$$

where $\Delta \ln(x)$ is the impact of the programme on total expenditure. If we take $\Delta \ln(x) = 0.12$, the average measured impact in 1998 and 1999, equation (8) implies an effect of 0.02-0.22*0.12=-0.0004, which is effectively zero. If we consider the impact in 1999, even equation (8) overpredicts the small declined in the food share given by the experimental results (0.02-0.22*0.17=0.0174, to be compared with 0.007). These differences, however, are well within the estimation error margin and are not statistically significant. [7]

5 Interpretation

The fact that the modified Engel curves are able to predict the impact of the programme is not surprising as they are fitted on programme data. A test of our parsimonious representation of the impact of the programme could only be performed with additional data, so as to perform an out-of-sample forecast. It is however interesting to speculate about why the programme would induce a shift in the Engel curve that would ultimately explain the surprising impact of the cash transfers on food shares.

We have already ruled out possible simple explanations of the impact that do not involve changes in the shape of the Engel curve such as an effect on relative prices or non-linearities in the Engel curve. It might therefore be interesting to consider factors, within the programme, that change the nature of the Engel curve.

PROGRESA/Oportunidades is a complex programme involving a number of different components. In addition to the cash transfers, two components strike as being particularly important in the present context. Bene-

[7] In theory it would be possible to modify the Engel curve in equation

ficiaries of PROGRESA had to attend a number of courses, some of which provided information on nutritional practices. It is therefore possible that such courses changed the taste for food and result in a shift in the intercept of the Engel curve. However, it is worth noting that similar effects on the food budget share are present even in the case of unconditional cash transfer programs, such as in Ecuador (Schady and Rosero, 2008).

The second feature of PROGRESA that might be important is the fact that the cash transfer is targeted to women. If demand functions are not derived from the maximization of a single utility function but reflect the relative weight of preferences of husband and wives, it is possible that a shift in power towards women might generate the results we have observed.

The evidence we have presented is suggestive in this sense, even because similar impacts are observed in other programmes targeted to women. To test explicitly this theory further work is necessary, however. Two possible approaches come to mind. First, it might be informative to look at similar programmes where the transfer is not targeted to women. At the moment, we are aware of at least two programmes (in Morocco and Macedonia) where the evaluation of a new conditional cash transfer involves the random allocation of different versions of the programme, some of which are targeted to women and some of which are not. Hoddinott and Skoufias, 2004 suggest that changes in information are potentially important.

Second, one can take a more structural approach and model explicitly the effect of different distribution factors (the targeted cash transfer being one of them) to test the hypothesis that the observed intercept shift can be accounted for within such a framework. Attanasio and Lechene (in progress) using a recent approach suggested by Bourguignon, Browning and Chiappori (2010), use Progresa and an additional distribution factor to pursue such a strategy.

6 Conclusion

In this paper, we have estimated Engel curves on data from poor Mexican rural families, collected to evaluate a conditional cash transfer programme. By considering carefully a number of methodological issues including the endogeneity of total expenditure and the possibility of differences in local

relative prices, we came to the conclusion that, the best specification for the share of food is linear in log total consumption, which has to be considered as endogenous. The estimation of such a relationship yields results that imply that food is a necessity, so that the share of food should decline with the increase in total consumption induced by the programme.

This result contrasts sharply with the evidence on the impact of the programme, which indicates that the share of food is effectively unchanged. Similar results have been observed for other similar programmes. We argue that the programme, in addition to increasing total consumption, shifts the Engel curve. Our data indicate that such a shift is well fitted by an intercept shift. We speculate that one of the possible reasons for such a shift could be a shift of power towards women, induced by the fact that the cash transfers are targeted to women.

Evaluating the impact of information on behaviour is important again for the design of cost effective welfare programmes.

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Table A1: First stage regression School

	Children in Primary School	Children in Secondary-Preparatoria
Log Total Expenditure	0.071	0.11
	(2.00)	(4.74)
Distance to secondary school	0.014	-0.027
	(1.16)	(-3.01)
Dummy for secondary school	0.027	0.05
	(0.41)	(0.77)
Distance to primary school	-0.023	0.005
	(-2.37)	(0.68)
Household size	0.379	0.069
	(37.05)	(8.54)
Townsize	0.0001	-0.0001
	(2.02)	(-0.99)
Education of head	0.039	0.015
	(1.61)	(2.24)
Education of spouse	-0.012	0.037
	(-0.66)	(0.84)
Head Indigenous	0.052	0.069
	(1.34)	(1.93)
Age of head	0.002	0.009
	(1.21)	(9.36)
Relatives eat in	0.017	-0.004
	(0.68)	(-0.34)
Household members eat out	-0.031	0.032
	(-0.50)	(0.80)
Time 4	0.069	-0.04
	(3.86)	(-3.34)
constant	-1.552	-1.27
	(-6.13)	(-6.97)
R^2	45.69	11.40
Nb of obs.	4073	4073

t values in parenthesis

Table A2: First stage: Total expenditure		
	Wage instrument	Income instrument
Instrument	0.44	-0.768
	(0.77)	(-1.27)
Square of instrument	-0.04	0.074
	(-0.18)	(1.68)
distance secondary school	0.008	0.008
	(0.89)	(0.99)
dummy for secondary school	0.05	0.044
	(1.38)	(1.32)
distance primary school	0.00	-0.003
	(0.02)	(-0.46)
household size	0.06	0.0603
	(18.37)	(16.60)
townsize	-0.00	-0.0001
	(-2.21)	(-2.49)
education of head	0.012	0.0114
	(1.32)	(1.23)
education of spouse	0.00	-0.0022
	(0.00)	(-0.22)
head indigenous	-0.08	-0.0999
	(-2.86)	(-3.77)
age of head	-0.00	0.0016
	(-0.09)	(2.34)
relatives eat in	0.014	0.0056
	(1.37)	(0.56)
household members eat out	0.013	0.0168
	(0.41)	(0.52)
cons	5.78	8.026
	(15.06)	(3.82)
F(26,175)	31.01	60.61
P>F	0.00	0.00
R^2	19.00	21.60

t values in parenthesis
Regressions control for time and region, not reported here