Control of Resources, Bargaining Power and the Demand of Food: Evidence from PROGRESA

Denni Tommasi
ECARES, Université libre de Bruxelles

August 2018

ECARES working paper 2018-22

ECARES
ULB - CP 114/04
50, F.D. Roosevelt Ave., B-1050 Brussels BELGIUM
www.ecares.org

Control of Resources, Bargaining Power and the Demand of Food: Evidence from PROGRESA

Denni Tommasi*

ECARES, Université libre de Bruxelles

August 22, 2018

Abstract

I use a structural model of households to recover how much resources each individual controls in the context of the Mexican PROGRESA program. I find that the eligibility to receive the cash transfers induces a redistribution of resources from the father to both the mother and children, although the mother is the one benefiting the most. With these information I compute individual poverty rates and quantify to what extent the program reduces within-household inequality. I also combine these measures to construct a proxy for women's bargaining power and, using causal identification techniques, I estimate its direct effects on household demand for food. Exploiting random assignment of the cash transfers as an instrumental variable for the treatment of interest, I show that mothers having majority control of household resources relative to fathers increase food consumption as a share of the household budget by 6.5-8.3 percent. I use these estimates to argue that, by knowing (i) The distribution of pre-program resources inside the household, and (ii) How much influence each decision maker can have on the desired policy outcome, a policymaker can improve the cost-effectiveness of a cash transfer program by targeting the cash to resource shares in addition to gender.

JEL Codes: D13, D11, D12, C31, I32.
Keywords: cash transfers, PROGRESA, structural model, collective model, resource shares, poverty, causality, LATE, engel curves, food.

*ECARES, Avenue F. D. Roosevelt 50, CP 114, B-1050 Brussels, Belgium (e-mail: dtommasi@ulb.ac.be).
This is a substantial revision of an earlier paper that circulated under the title "Control of Resources and Demand for Food". I am grateful to Bram De Rock and Arthur Lewbel for their invaluable guidance and support. I thank Øyvind Aas, Gani Aldashev, Laurent Bouton, Rossella Calvi, Laurens Cherchye, Pierre-André Chiappori, Paola Conconi, Thomas Demuynck, Antonio Estache, Victor Ginsburgh, Paula Gobbi, Glenn Magerman, Martina Menon, Mathieu Parenti, Federico Perali, Senay Sokullu, Benjamin Solow, Christine Valente, Frederic Vermeulen, and Alexander Wolf, and seminar participants at Boston College, Boston University, CERGE-EI (Prague), ECARES, Monash University, Munich LMU, Tilburg University, University of Surrey, University of Verona, the European Society for Population Economics (ESPE) in Antwerp, and the Society of Economics of the Household (SEHO) in Paris. I acknowledge financial support from the Fonds National de la Recherche Scientifique (FNRS). All errors are my own.

1 Introduction

Cash transfer programs are popular policy tools to fight poverty in developing countries (Fiszbein and Schady, 2009). The typical program provides cash to mothers conditional on taking certain actions, such as enrolling their children in school. This type of design is based on an influential literature documenting that resources accruing to mothers are more likely to be allocated to benefit household members, especially children, than those accruing to fathers (e.g. Haddad et al. (1997), Duflo (2003), Quisumbing and Maluccio (2003) and Smith (2003)).[1] The argument commonly used is that targeting the mothers induces a redistribution of resources that eventually affects household decisions. I argue that the current designs are based on the following implicit assumption which has not been verified in the data: Targeting increases maternal control of household resources.[2] Control of the household resources is unobserved in practice and hard to identify, but clearly it is the crucial determinant of the impact and effectiveness of these programs on desired outcomes.

The primary aim of the paper is to overcome this measurement issue and to structurally estimate individual control of resources inside the household and investigate its determinants. I focus on PROGRESA, which is a well-known conditional cash transfer (CCT) program that was implemented in rural Mexico in the late 1990s. The exogenous cash transfers are targeted only to mothers, which is aimed at modifying the amount of resources under their control. I recover the information on individual's control of resources from a structural model. I do so because consumption data are commonly collected at the household level and goods are partly shared, which makes it impossible to directly observe the information for each individual separately. Next, I build a proxy for women's bargaining power using my estimates of individual control of resources. I extend the analysis and estimate the direct effects of maternal control of resources on household demand for food. I focus on this relationship because food accounts for roughly 74% of the household budget and it is the primary determinant of welfare. This consists of estimating an Engel curve for food, which is a relationship between expenditure and household income, where the newly constructed measure of power is the main treatment of interest. Since it is endogenous with respect to unobservable factors that may influence the allocation of expenditure, I exploit the random assignment to receive the cash transfers as an ideal instrumental variable.

The analysis proceeds in three steps. First, using the available information in the dataset, I establish a causal effect of interest that will guide my structural analysis: The effect of targeting the cash transfers to mothers on self-reported indicators of controlling program resources. I find a positive Intention-To-Treat (ITT) effect of targeting on the probability that the mother controls the additional resources, versus a lower probability for the father.

[1] See Section 2 for an overview of this early literature which is linked to more recent designs where the father is also targeted by the cash transfers.

[2] Throughout the paper I define individual "control" as follows. Imagine that the intra-household allocation process is divided in two stages. In the first stage, decision makers pool their individual resources together and decide what share of this goes to each member. In the second stage, each member takes this share as their income and maximizes their utility function subjects to constraints. This share is the amount of resources that each individual controls and that can be used to purchase private goods and to contribute to the public good. More details will be given when I describe the theoretical framework.

Second, I use a collective model of the household (Chiappori, 1988, 1992) to structurally estimate mother's, father's, and children's *resource shares*, that is, the fraction of household resources allocated to each member, and investigate their determinants. In this framework, I recognize that households consist of individuals with their own rational preferences, and use the assumption that the intra-household decision process produces Pareto-efficient outcomes. The collective model has been used to show that the control over resources in the household determines its allocation. I adopt an attractive approach by Dunbar et al. (2013) to estimate resource shares through Engel curves of private assignable goods, that is, goods that are consumed exclusively by the mother, father, or children (e.g., clothing and footwear). I find evidence of a substantial increase in the mother's control of resources, relative to the father. The mean distribution shifts by 5%, which is consistent with the reduced-form regression analysis. A simple back of the envelope calculation demonstrates that, during the first year of the program, for every peso taken away from the father, 60-75 cents go to the pocket of the mother and the rest goes to the children. Moreover, given the information on the amount of resources controlled by each individual, I am also able to conduct a poverty analysis at the individual level. While the effects of PROGRESA on aggregate household poverty and inequality have been widely examined, I complement it by showing that, within the household, there is also a reduction in poverty rates for mothers and children, relative to the father, thereby reducing within-household inequality.

Third, I build a proxy for women's bargaining power to study its causal effects on household demand for food. In order to do so, I use the estimated resource shares to construct an indicator for whether the mother controls the majority of household resources. Due to estimation errors and possible model misspecification, my treatment variable is likely to be mismeasured for some households.[3] Since the treatment is binary, it means that some households who are in the true treatment group (the mother controls the *majority* of household resources) may be misclassified in the control group (the mother is observed to control the *minority* of household resources), and vice versa. To deal with misclassification errors of the binary treatment indicator, I use a recent estimation method introduced by Calvi, Lewbel, and Tommasi (2017) called MR-LATE (for Mismeasured Robust Local Average Treatment Effect), which can identify and consistently estimate LATE even when the endogenous binary treatment indicator contains measurement errors. Households where mothers control the majority of resources spend 6.5-8.3 percent more on food, which is roughly 2.5 times larger than ITT estimates obtained using eligibility as assignment to treatment. Moreover, accounting for specification, estimation, and measurement error, in the estimate of treatment is shown to be empirically important. Differences in results with respect to the standard 2SLS for LATE (Imbens and Angrist, 1994) are substantial, as the latter cannot account for these errors leading to an overestimate of the effects.

The contribution of the paper is twofold. First, by recovering maternal control of resources, I am able to provide a support for the numerous papers that have studied the effects of a CCT program like PROGRESA under the (implicit) assumption that, since the mother is targeted by the program,

[3]The same problem may arise even if the variable was observed and not estimated. This is the case, for instance, of standard reporting errors affecting the treatment indicator, or for individuals not taking the treatment that they are assigned to.

she is more likely to control the additional resources. The departure in my analysis is precisely in that I relax this assumption, as I can actually recover the total amount of resources controlled by the targeted individual and how this is affected by the policy. Moreover, I can use this new information to quantify the effects of the policy on within-household inequality and to construct a new proxy of bargaining power that has an immediate behavioral interpretation. Second, I establish a direct link between the eligibility to receive the conditional cash transfers, the actual control of resources by the targeted individual, and household demand for food. This is also a crucial departure from the literature on cash transfers. So far, the focus has been on estimating and comparing the effects of specific designs, using the eligibility to receive the cash as the treatment variable. Here, instead of asking what is the impact of a particular design of cash transfers program on demand, I ask a more general question: What is the impact of having a large control of resources on household demand for food?

In terms of policy implications, particularly with respect to the last exercise, I show that the current design of many CCT programs has a limitation which can be overcome to increase their cost-effectiveness. Indeed, concerning the latter, since maternal control of household resources is unobserved and must be estimated, as of now, the amount of cash assigned to mothers is independent of their pre-program bargaining position. This implies that, similar mothers, but with either large or little influence over the household budget, may receive the same amount of cash. Therefore, by inferring (i) How much pre-program resources mothers control, (ii) How the actual control of resources is influenced by the assignment, and (iii) How it impacts the desired outcome, one may refine the design such that less powerful mothers (to start with) become eligible to a differentiated assignment with respect to those who have already a large control over the household budget. In the case of PROGRESA, if we had known the pre-program distribution of household resources, we could have achieved the same increase in the aggregate consumption of food (for compliers) by reallocating the intensity of the assignment and by saving a (potentially) large amount of program resources.

The paper is organized as follows. Section 2 provides an overview of the related literature. Section 3 describes the experimental set-up of the PROGRESA program and presents the reduced-form results that establish a positive causal effects of targeting on control of additional resources by the mother and targeting on the demand for food. Section 4 presents the household model, the identification of resource shares and the structural estimation results. Section 5 outlines the policy insights that one can derive from the structural estimates of the model and in particular explores the links between the maternal control of resources and the demand for food. Section 6 concludes.

2 Related literature

This paper lies at the intersection of three strands of literature: (i) The literature investigating the importance of female versus male intra-household decision making power in developing countries, which is strongly linked to the literature on cash transfer programs; (ii) The literature on the non-unitary model of the household, specifically the one allowing the recovery of resource shares; and (iii) The literature accounting for mismeasured or misclassified treatment indicators.

The literature on female intra-household decision power has been strongly influenced by Thomas (1990, 1994, 1997) and Schultz (1990). These seminal works have contributed to shape the way that the first generation of cash transfer programs were implemented, (universally) giving mothers the eligibility to receive the cash transfers.[4] Along this line, and specifically related to my application on the demand for food, Schady and Rosero (2008), Attanasio and Lechene (2010), Attanasio et al. (2012), Angelucci and Attanasio (2013), show that, following the increase in the household income induced by a CCT program, sizeable cash transfers made to mothers are associated with constant or higher shares of expenditure on food. This empirical evidence, which is in contradiction with the established negative relationship between household income and expenditure on food (i.e. Engel's law), can be explained by the increase in the share of resources held by mothers, which may induce a change of allocation due to their different preferences.[5]

More recently, Benhassine et al. (2015), Akresh et al. (2016) and Haushofer and Shapiro (2016), randomize the gender of the recipient of the cash transfers and show that there is no significant difference in program effects on household consumption, production and investment decisions. Significant differences between male and female recipients, at least on food expenditure, are still found by Almas et al. (2015). These mixed results suggest that there still is a long way to go before completely understanding what mechanisms are at play and their magnitude on desired outcomes. Indeed, from a methodological point of view, all these studies share at least one common feature which might be limiting: They all look directly at the impact of the randomized treatment, which is not necessarily informative about the actual control of resources, or about how other changes in individuals' control over resources might impact the desired outcome. For instance, Akresh et al. (2016) randomize the gender of the recipient of the transfers in the context of Burkina Faso, and find no significant difference between the two arms of the experiment. However, given the strong cultural norm in West Africa prescribing that fathers are responsible for feeding their family, it might be that, regardless of who is the targeted individual, the new resources are going to be controlled by the father. In order to understand what is driving the empirical result, one should look at the actual redistribution and control of household resources. These are hard to observe directly, but, fortunately, we know a great deal about the economics of household consumption allocations, which can be used as a tool to shed new light on the working of these experiments. This is the approach that I follow in my paper.

This brings me to the second literature which I relate to. The collective model of the household was pioneered by Chiappori (1988, 1992) and Apps and Rees (1988), and subsequently elaborated by Browning et al. (1994), Browning and Chiappori (1998), Blundell et al. (2005) and Chiappori and Ekeland (2006). In recent years, this framework has become the main paradigm through which

[4] Yoong et al. (2012) review the results from several studies, on both conditional and unconditional cash transfer programs, and show that indeed transfers to mothers increase the overall welfare of eligible households, as long as the conditionality is attached.

[5] Attanasio and Lechene (2010) exclude other possible mechanisms that may contribute to explain this apparent violation of Engel's law, such as: changes in local prices, homothetic preferences, changes in preferences for quality of food, labeling of money.

household allocation decisions are now studied.[6] A handful of papers have developed techniques to recover the level of individuals' resource shares, which is the fraction of household resources devoted to each member. This is particularly appealing because they provide a measure of individuals' control of resources, which is what I need to overcome some of the limitations of the literature on cash transfers. Browning et al. (2013) (hereafter BCL) pioneered an approach that was then applied by Cherchye et al. (2012) and further developed by Lewbel and Pendakur (2008) and Bargain and Donni (2012).

Among the descendents of the BCL model, Dunbar et al. (2013) (DLP hereafter) provide one of the most prominent models in the literature. It differs from the BCL-type approach as they identify resource shares using Engel curves of private assignable goods and by imposing semiparametric restrictions on individual preferences. The DLP model is an attractive approach for practitioners because it combines a general theoretical structure with a lower data requirement and estimation complexity. Related to my context and data, Tommasi and Wolf (2016) study the identification strategy in the DLP model and use it to provide the first estimates of a cash transfer program (PROGRESA) on resource shares and poverty rates. They point out that a specific feature of the DLP model, given by the multiplication of resource shares with desired budget shares, is a potential source of imprecision and instability of the estimates. They propose a solution to stabilize the estimates that is embedded in the shrinkage estimation literature. In this paper, I minimize these issues of the DLP model in a different way, which makes the estimates robust to several sensitivity checks. This is explained at length in the robustnesses of Section 4.5.[7] The results that I obtain are in line with the recent contribution by Klein and Barham (2018), albeit they follow a completely different approach.

Finally, I also relate to the literature on measurement or misclassified errors in observed treatment, because one of the main policy insights that I derive is obtained by estimating a model where the treatment indicator comes from a structural model. Here I am interested in recovering the local average treatment effect (LATE) of Imbens and Angrist (1994), which is applicable when the true treatment is endogenous and heterogeneous, and an exogenous binary instrument is available. Identification of LATE with misclassified treatment has recently received some attention.[8] Calvi, Lewbel, and Tommasi (2017) propose a general (and trivial to implement) solution to recover LATE with binary mismeasured treatment indicator. They set-up an estimation problem that has

[6]Browning et al. (2014) provide a comprehensive review of the theoretical and empirical advances in this literature.

[7]Applications or variations of the DLP model include Calvi (2016), Calvi et al. (2017), Penglase (2017), Brown et al. (2018) and Bargain et al. (2018). I am also closely related to the recent contribution by Sokullu and Valente (2017), who recover resource shares by extending the DLP model to panel data and using PROGRESA to estimate resource shares and poverty rates of eligible households. Their crucial identifying assumption is that preferences are similar over time, rather than across people (or types). Although this is an interesting theoretical contribution, I wish to point out that the application on PROGRESA, using their specific identifying assumption, may not be the most suited. Indeed, a recent paper by De Rock et al. (2017) provides strong evidence of across-time heterogeneity in the efficiency of intra-household resource allocation. They rationalize this finding within a household model where decision makers may change their preferences over time as a result of a treatment that gives information about the importance of a public good. Notice that the possible misspecification of the (collective) model is taken into account by the (MR-LATE) estimation strategy adopted when I recover the effects of control on the demand for food.

[8]In the case of a binary misclassified treatment, Ura (2016) considers a general scenario and standard LATE instrument assumptions and obtains set identification bounds of the parameter of interest. In the case of a continuous misclassified treatment, Lewbel (1998), Song et al. (2015), Hu et al. (2015) and Song (2015), use instruments and further exclusion restrictions to obtain identification and estimation of average marginal effects with classical or nonclassical measurement error. In case of binary mismeasured treatment indicator, Battistin et al. (2014) use two measures of the misclassified treatment to obtain point-identification of LATE. DiTraglia and Garcia-Jimeno (2016) and Yanagi (2017) also obtain point-identification of LATE with mismeasured treatment, but their contribution is either less general or requires even more information.

the standard LATE structure, where a randomized instrument is correlated with treatment, and the true treatment affects an outcome. They then exploit two mismeasures of treatment to estimate the impact on the outcome of an underlying latent (true) treatment. This estimator allows for arbitrary correlation between the two mismeasured treatments and does not require homogeneity of treatment effects. Given these features, it is the most suited methodology to explore the links between the control of resources and the allocation of household budget in the context of the demand for food.

3 Data, sample selection, and measures of control

The section is divided into two parts. First, I provide some background information on the PROGRESA program, details of my sample selection and some useful descriptive statistics, which will be useful throughout the paper. Second, I exploit the random assignment of the program to identify a causal effect of interest: The effects of targeting the transfers on (self-reported) indicators of controlling program resources. I present briefly the specifications of the empirical models and describe the estimation results of the effects of interest. Notice that, since in later sections I study the relationship between control of resources and demand for food, here it is interesting to also provide evidence of the effects of targeting on (self-reported) decisions on food expenditures.

3.1 Program design, sample selection and descriptive statistics

PROGRESA was the first conditional cash transfers (CCT) program of a new generation of welfare interventions, launched by the Mexican government in the late 1990s to help poor people in marginalized rural areas.[9] It was implemented based on a phase-in approach starting in 1997. Of 10,000 villages included in the first expansion phase, 506 villages were selected in the evaluation sample, 320 of them were randomly chosen to have an early start of the program, whereas the remaining 186 formed the control group. In practice, households in these latter villages were excluded from the program until late 1999 and became eligible for the grant only afterward. This means that households in treatment villages, who were qualified as "eligible", started receiving cash transfers subject to the appropriate conditionalities in April 1998, whereas "eligible" households in control villages received no payment until *after* November 1999.

The stated objective of the program was to introduce incentives to improve the accumulation of human capital of children and at the same time to alleviate short-term poverty. To achieve these objectives, the government provided poor households with cash transfers conditional on the fulfillment of certain behaviors. The first set of conditions were related to education. Eligible households could receive a (large) portion of the grant conditional on their child's school enrollment and attendance. Given that school attendance in primary school was nearly universal (whereas only about 60% of children continue to secondary education), the conditions were binding, in practice, only

[9] Hoddinott and Skoufias (2004), the World Bank CCT Policy Research Report (2009) and the IFPRI reports contain detailed descriptions and analysis of the effects of PROGRESA.

for households with older children. The second set of conditions were related to health seeking behavior. A further portion of the grant was conditional on women taking their young children to health centers and attending a number of courses organized by the program. Three aspects of the design are important. First, mothers were eligible to receive the cash which was given bi-monthly. Women's role and involvement in the program was decided under the assumption that this would allow them to gain bargaining power in the decision making process of the household. Second, price subsidies and transfers in kind were replaced by monetary transfers which directly affected total household expenditure. Third, the amount of transfers available for each family varied with the school-level, gender and age of the child, in order to match the different opportunity costs faced by the families.

Throughout the observational period, extensive surveys were administered roughly every six months from August 1997 to November 2000 and the surveys collected in each village were surveys of the population. The original evaluation sample contains 24,077 households, of which 61.5% are couples with any number of children and no other adult individual living in the household, 6.5% are female single-headed households with any number of children and 4% are male single-headed households with any number of children. The remaining 28% of households are extended families with more than two adult members. In the present paper, I use four waves from the beginning of the first trial: October 1998, May 1999, November 1999 and November 2000. I exclude households that were deemed non-poor (in the program sense) and therefore ineligible for the grant. My sample consists of nuclear (married) couples such that the only adults present are the mother and father who are the parents with one to three children, all under 12 years of age. I focus only on households *without* children eligible to attend (and hence receive the grant for) *secondary* school, because I want to have a sample where the binding constraints of the conditionality attached are limited as much as possible. More details for this choice will be given in Section 4.3 when I discuss the estimation strategy of the structural model. I also exclude households with no children and those with more than three to obtain a degree of homogeneity. The final sample is made of 9,017 observations.

Table 1 reports summary statistics for my sample of rural poor families. On average, the household head is 32 years old and the spouse is 28. They have a little more than 4 years of education and the vast majority of heads speaks Spanish (97%) and a large portion also speaks an indigenous language (38%). In order to maximize sample size, I pooled households observed in November 2000, which is when the Non-PROGRESA eligible group becomes eligible to receive the grant. This choice is discussed and motivated in Section 4.5 when I present the results and robustness checks of the structural estimates. This yields a sample where 6,470 households (72%) reside in PROGRESA villages, the rest belongs to the Non-PROGRESA group. Total expenditure is computed as the sum of all non durable expenditure including food. The average household's total non-durable expenditure is equal to 8,103 pesos (in 2010 prices), of which food makes up around 74% of the total. The average number of children in the household is 2.20, where 1.20 are below 6 years old and 0.99 between 6-12. All the children in primary school age are enrolled in school. The mean age of children is slightly above 5 years old, the mean minimum age is almost 4, and 48% of children are girls. Only 6% of households have at least 1 external member eating in their family, and 2% of households have an own family member eating somewhere else. The sample is balanced over the 4 waves. For village characteristics, I observe the mean number of inhabitants in the villages and the percentage of households living in each of the 7 states of the experimental sample. Finally, my assignable good expenditure is the sum of expenses for clothing and footwear. These are available separately for men, women and children. Assignable clothing, which will be useful for the structural estimations, makes up for a smaller portion of the total budget shares.[10]

3.2 Control of resources: Reduced-form results

The fact that mothers are targeted to receive the cash transfers does not imply that they will actually control these resources. In my sample, 65% of the respondents in the Non-PROGRESA recipient group report that both adults manage the extra resources received by the mother. This means that, for a large portion of the sample, I do not know who actually controls the resources when the cash transfers are distributed. There are good reasons to believe that, if the father is the decision maker commonly managing the finances, he may take possession of these transfers, no matter who is the targeted individual. Hence, as a starting point, the first challenge is to establish a causal relationship between targeting and actual control of resources using the information available in the PROGRESA dataset.

In the first set of regressions, I look at the answers to two specific questions: "Who decides how to use the extra income entitled to the mother?" and "Who decides the expenditures on food?". I look at the question of decisions on food because, as motivated in the introduction, much of the household budget is spent on food, and hence it is useful to compare the answers to this question with the information about who decides how to use the extra income entitled to the mother. I consider the following model:

$$y_i = \alpha + \delta \text{PROGRESA}_i + X_i^{'}\gamma + \epsilon_i \tag{1}$$

where y_i is measured for each household i and can take three possible values: "mother", "father", "both". PROGRESA_i is an indicator variable equal to one if the household i lives in a PROGRESA village and is entitled to receive the grant, whereas X_i is a vector of individual, household and village characteristics. This vector includes mother and father's education, a dummy for whether the head can speak an indigenous language, number of children in the household, number of children enrolled in primary school, number of people eating inside and outside the household, state dummies fixed effects, and a set of ten dummies accounting for the level of total household non-durable expenditure. The parameter of interest is δ, which captures the Intention-To-Treat (ITT) effect of being exposed to the exogenous cash transfers.

[10] These numbers are comparable in magnitude to those in Dunbar et al. (2013) and Penglase (2017) for Malawi and Calvi (2016) and Calvi et al. (2017) for India.

Table 1: PROGRESA data: Descriptive statistics of selected sample

	Mean	SD	Min	Max
Parents' characteristics				
Age of the head	31.77	8.23	18.00	65.00
Age of the spouse	28.04	7.34	17.00	64.00
Education of the head	4.15	2.56	0.00	18.00
Education of the spouse	4.12	2.51	0.00	18.00
Head can speak Spanish	0.97	0.17	0.00	1.00
Head can speak Indigenous language	0.38	0.48	0.00	1.00
Household characteristics				
PROGRESA	0.72	0.45	0.00	1.00
Log of total non-durable expenditure	9.00	0.45	7.61	10.18
Number of children	2.20	0.75	1.00	3.00
Number of children enrolled in school	1.03	0.90	0.00	4.00
Number of children aged below 6	1.20	0.81	0.00	3.00
Number of children aged 6-12	0.99	0.93	0.00	3.00
Mean age of children	5.34	2.96	0.00	16.00
Minimum age of children	3.66	2.88	0.00	16.00
Share of girls	0.48	0.37	0.00	1.00
Eat in	0.06	0.77	0.00	40.00
Eat out	0.02	0.14	0.00	4.00
1st wave	0.28	0.45	0.00	1.00
2nd wave	0.25	0.43	0.00	1.00
3rd wave	0.22	0.41	0.00	1.00
4th wave	0.25	0.44	0.00	1.00
Village characteristics				
Size of town in 1995	367.83	256.37	50.00	1534.00
Guerrero	0.08	0.28	0.00	1.00
Hidalgo	0.18	0.38	0.00	1.00
Michoacan	0.13	0.34	0.00	1.00
Puebla	0.16	0.36	0.00	1.00
Queretaro	0.04	0.19	0.00	1.00
San Luis Potosi	0.14	0.35	0.00	1.00
Veracruz	0.27	0.44	0.00	1.00
Output variables (%)				
Father, share of assignable goods	0.95	1.71	0.00	8.81
Mother, share of assignable goods	0.84	1.35	0.00	6.86
Children, share of assignable goods	3.72	3.82	0.00	18.49
Share of food	73.68	14.37	8.79	100.00

Notes: Descriptive statistics are for four waves from the beginning of the first trial: A wave of surveys from October 1998, May 1999, November 1999 and November 2000. Budget share on food includes 36 categories of items divided in 4 categories: 1) fruits and vegetables; 2) cereals and wheat; 3) food of animal origin; 4) other foods. Mother, Father and Children's assignable goods includes expenditure on individual clothes and footwear.

Table 2 reports the estimation results of these regressions using a Probit model and clustering the standard errors at the primary sampling unit (village) level.[11] As one can see on the left hand side, the PROGRESA indicator is associated with a larger probability that the mother will control the additional resources and a lower probability that it will be the father. This does not say that every additional peso entitled to the mother will go under her control, but on average it is more likely that she will be the one keeping these additional resources and not the father. These results are in accordance with Adato et al. (2000), who also find a positive and significant effect on mothers keeping their extra income.[12] These reduced-form results will help us guide the structural estimation of Section 4.4 in order to recover both the sign and magnitude of the effects of the grant on actual control of resources. On the right hand side, one can see the results of the effects of PROGRESA on whom make decisions about food expenditure. The estimates here are noisier and I am not able to pick up significant effects, but clearly these are not precisely estimated zeros. Still they show that, very likely, the indicator is associated with a larger probability that the mother will make decisions about food and a lower probability that it will be the father doing it.

Table 2: Effects of the exogenous cash transfers: Self-reported control

	Extra resources			Food expenditure		
	Mother	Father	Both	Mother	Father	Both
	(1)	(2)	(3)	(4)	(5)	(6)
PROGRESA	0.191***	-0.316***	-0.130*	0.112	-0.108	-0.014
	(0.071)	(0.132)	(0.070)	(0.073)	(0.080)	(0.064)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,226	2,226	2,226	2,226	2,226	2,226
Mean dep. var.	0.358	0.027	0.614	0.191	0.150	0.658

Notes: I look at two specific questions: "Who decides how to use the extra income entitled to the mother?" and "Who decides the expenditures on food?". The empirical model is estimated on data from May 1999, which is the survey wave where these self-reported information are available. These questions where asked one year after the introduction of the cash and are used to be representative of the program effects. The common controls in all specifications are: dummies for number of kids, dummies for number of kids enrolled in school, mean age of the kids, share of girls in the household, age and education of head and spouse, whether the head can speak indigenous language, number of people eating inside and outside the household, time and state dummies. I control for income level by using total expenditure deciles. Standard errors are bootstrapped 200 times and clustered at the primary sampling unit (village) level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

4 Intra-household allocation and control of resources

In this section I model the intra-household allocation process and quantify how much resources are controlled by each member. I model Mexican households using the collective model developed by Dunbar et al. (2013) (hereafter DLP). In the first two subsections I set up the optimization problem and briefly summarize the identification strategy. In the last two subsections I outline the estimation strategy and present the results of the structural estimates.

[11]Results are robust to a bivariate Probit regression using the two most interesting output variables: "mother" and "father".

[12]As for the rest of the responses to direct questions asking whether the mother, father, or both, are in charge of a number of household expenditures, they find that PROGRESA does not have a significant effect.

4.1 A collective model of Mexican households

Consider three types of individuals $t \in \{m, f, c\}$ in the household: these are the mother (m), the father (f), and the children (c). Households differ according to a set of observable characteristics, such as number of the children, age of the parents, location, and other socio-economic attributes. Members may have different preferences but must jointly decide on the purchase of K goods with prices $\mathbf{p} = (p^1, \ldots, p^K)$. $\mathbf{z} = (z^1, \ldots, z^K)$ is the vector of quantities purchased by the household, $\mathbf{x}_t = (x_t^1, \ldots, x_t^K)$ is the vector of quantities of private good equivalents consumed by member t of the household and y is the household's total expenditure. The DLP framework allows for economies of scale in consumption through a linear consumption technology, which takes the form of a $K \times K$ matrix A. This allows us converting the quantities $\mathbf{z}$ purchased by the household into private good equivalent quantities $\mathbf{x}_t$.[13] The private good equivalent quantity of a good may be up to three times as large as the purchased quantity, if the good is perfectly shared between members (perfectly public).

Let $U_t(\mathbf{x}_t)$ be the (monotonically increasing, twice continuously differentiable and strictly quasi-concave) utility function of member t over the bundle of K goods. In principle this may depend also on the utility of other household members, but for simplicity I assume that they are weakly separable over the sub-utility functions of goods. Also, the choice of restricting the utility functions among individuals of the same type is driven by the data. In order to limit this simplification, in estimation I allow the preference parameters and resource shares to vary with several characteristics. The key assumption in the literature of collective models is that, even if household members may have different preferences, they make consumption decisions efficiently, that is, their joint choices maximize the following (Bergson-Samuelson) social welfare function:

$$\tilde{U}(U_m, U_f, U_c, p/y) = \sum \mu_t(p/y)\tilde{U}_t \tag{2}$$

where the Pareto weights $\mu_t(p/y)$ depend on prices, individual characteristics and household expenditure. The form of the household's utility function (2) is in contrast to what is called the unitary model of the household, where choices are generated by maximizing a single well-behaved utility function. The household's program reads:

$$\begin{aligned} \max_{\mathbf{x}_m, \mathbf{x}_f, \mathbf{x}_c, \mathbf{z}} \; & \tilde{U}(U_m, U_f, U_c, p/y) \\ \text{s.t. } & \mathbf{z} = A(\mathbf{x}_m + \mathbf{x}_f + \mathbf{x}_c) \\ & y = z'\mathbf{p} \end{aligned} \tag{3}$$

The solution to program (3) yields the quantity of private good equivalents, $\mathbf{x}_t$, for each member t.

[13] Formally: $\mathbf{z} = A(\mathbf{x}_m + \mathbf{x}_f + \mathbf{x}_c)$. A practical example commonly reported in the literature is the following. Suppose a household is composed of 2 adults only. They ride their car together half of the time, in which case they share the cost of gasoline 50:50. When one of them rides alone, he or she pays alone. Then the consumption of gasoline, in private good equivalents, is 1.5 times larger than the purchased quantity of gasoline at the household level. If I assume that the consumption of gasoline does not depend on the consumption of other goods, then the k^{th} diagonal element of matrix A would read 2/3 such that: $z^K = 2/3 * (\mathbf{x}_m + \mathbf{x}_f)$. In this example, 2/3 represents the degree of publicness of good K within the household.

After pricing these at the shadow prices $A'\mathbf{p}$, I obtain the resource shares η_t, that is, the fraction of total household resources controlled by each individual t.

An implication of the efficiency assumption is that the collective allocation process (3) can be equivalently represented as a two-stage process (Chiappori, 1992). First members divide up non-labor income, then each makes choices according to individual preferences. Each member's optimization problem is to maximize her utility subject to a budget constraint characterized by a shadow price vector, which is the same for all household members, and a shadow budget, which is specific to that member. The difference between shadow and market prices reflects the scale economies in consumption from sharing. The optimal household's demand functions for each good k are given by:

$$z^k = A^k\big(h_m^k(A'\mathbf{p}, \eta_m y) + h_f^k(A'\mathbf{p}, \eta_f y) + h_c^k(A'\mathbf{p}, \eta_c y)\big) \tag{4}$$

where h_t^k are the individual demand functions, η_m, η_f and $\eta_c = 1-\eta_m-\eta_f$, are the resource shares attributed to each member t. This is the object of the next sub-section.

4.2 Individual resource shares: Identification

The task of identifying the resource shares in DLP is accomplished by focusing on the consumption of *private assignable goods* for each household member. These are goods that do not have any economies of scale in consumption and thus are consumed exclusively by one member. The typical case is individual's clothing because it can be assumed that, e.g., women do not consume men's clothing and vice versa.[14] Furthermore, DLP make the restriction that η_t does not depend on household expenditure y (at low levels of y). The Engel curve setting does not generally allow for the testing of this assumption directly. However, in the literature there is some empirical evidence supporting the identification of resource shares based on this assumption (e.g. Menon et al. (2012)). Given this strategy, the household demand functions (4) simplify considerably, because the shadow price of a private assignable good is equal to its market price. By using a set of preference restrictions that are discussed below, DLP provide a model that identifies resource shares without needing for an identity restriction between preference of singles and married individuals.

In the case of a good that is private and assignable to member t, household demand (4) can be written simply as a product of η_t and an Engel curve in t's individual resources representing t's individual preferences. This is because, given that t's assignable good is not consumed by another household member, t^{-1}'s desired budget share for this good is zero. In my case, I observe an assignable good for all members and hence the household budget shares of each member t are given by:

$$W_t = \frac{z_t}{y} = \eta_t \cdot w_t(\eta_t y) \tag{5}$$

[14] Goods that are consumed by only one member are also sometimes called exclusive goods. The distinction lies in the availability of separate prices. Where the goods for men and women have the same price, I consider them the same good and call it assignable. The distinction is irrelevant here because price variation is not needed for identification purposes in this model.

where W_t is the share of total household expenditure spent on member t's private assignable good, η_t is the resource share attributed to that member and $w_t(\eta_t y)$ is the unobserved share of t's individual resources $\eta_t y$ that she would spend on her private good when maximizing her own utility function given the shadow price $A'\mathbf{p}$. The function $w_t(\eta_t y)$ can be thought of in terms of "desired budget share", which takes the shape of a (standard) Engel curve in t's resources.

In System (5), W_t and y are observable, and the goal is to identify the resource shares η_t. The challenge in identifying them is that for every observable W_t on the left hand side, there are two unknown functions on the right hand side: η_t and $w_t(\eta_t y)$. This is when the preference restrictions proposed by DLP become important. The authors impose that the functions $w_t(\eta_t y)$ have similar shapes, essentially fixed curvatures, either across household members or across household sizes (number of children). Under this structure, resource shares are identified without further restrictions on the shape of the preference function $w_t(\eta_t y)$.

Assume that each household member has PIGLOG utility function at all levels of expenditure (Muellbauer, 1976). Then, the Engel curve for the private assignable good of each household member (5) becomes linear in the logarithm of own expenditure and the system takes the following form:

$$\begin{aligned} W_m &= \alpha_m \eta_m + \beta_m \eta_m ln(\eta_m y) \\ W_f &= \alpha_f \eta_f + \beta_f \eta_f ln(\eta_f y) \\ W_f &= \alpha_c \eta_c + \beta_c \eta_c ln(\eta_c y) \end{aligned} \tag{6}$$

where α_t and β_t are linear indexes of underlying preference parameters, whereas η_t is the share of overall resources controlled by member t. Identification is achieved by imposing similarities of preferences, both across household members, which the authors call SAP ("Similar Across People"), and across households, called SAT ("Similar Across Types"). In particular, provided that $\beta_m = \beta_f = \beta_c = \beta$, DLP show that the system is identified.

Before concluding this section, two final remarks are in order. First, the Pareto weights μ_t in (2) are commonly referred to as measures of intra-household bargaining power: The larger they are, the more weight is attributed to preferences of individual t in the household. However, they are not invariant to arbitrary cardinalizations of the utility function. For this reason, resource shares are commonly preferred as summary of bargaining power inside the household, because they do not suffer from this drawback, and also because there exists a monotonic correspondence between Pareto weights and resource shares (see Proposition 2 of Browning et al. (2013)). Hence, in what follows, I interpret the resource shares used in my analysis both as measures of control of resources and of bargaining power, interchangeably. Second, it is important to point out that the budget shares on assignable clothing, W_t, and resource shares, η_t, are different objects. Importantly, the ratio of clothing of one member does not correspond to the ratio of resources controlled. In other words, the fact that in our data $W_c > W_f > W_m$, does not imply that $\eta_c > \eta_f > \eta_m$. In the Appendix A.1 I provide a simple example to show this intuition.

4.3 Estimation strategy

I estimate the system of equations (6) by appending an error term to each equation and by imposing the similarity of preferences assumption over private assignable goods: $\beta_m = \beta_f = \beta_c = \beta$. This is the system that I take to the data:

$$\begin{cases} W_m & = \alpha_m \eta_m + \beta \eta_m ln(\eta_m y) + \epsilon_m \\ W_f & = \alpha_f \eta_f + \beta \eta_f ln(\eta_f y) + \epsilon_f \\ W_c & = \alpha_c \eta_c + \beta \eta_c ln(\eta_c y) + \epsilon_c \end{cases} \tag{7}$$

where, like before, W_t are the budget shares spent on assignable clothing, y is the total expenditure (in pesos) reported for the month prior to the survey, α_t, β, η_t, are linear indexes of characteristics and $\eta_c = 1 - \eta_m - \eta_f$. I estimate the system using the Non-Linear Seemingly Unrelated Regression (NL-SUR) method because the error terms may be correlated across equations.[15] Standard errors are clustered at the village level.

The model is taken to the pooled sample of 4 waves, October 1998, May 1999, November 1999 and November 2000, as outlined in Section 3.1. The dataset is suitable to estimate system (7) for two main reasons. First, the consumption module includes, in the six-month recall period, household expenditures on clothing and shoes for the household head, spouse and children. This is the crucial information necessary to apply the DLP model. In my empirical implementation, I use a single private assignable good for each individual which is equal to the sum of clothing and footwear expenditures for that individual. Second, the dataset is very rich, and I can include several demographic variables, which may affect preferences and resource shares. I use a total of ten control variables plus time and state dummies in which the households were located. Four of these are characteristics of the parents: their education level as well as their age in years. Five relate to the children in the household: three dummies for the number of children present (except for the specification in which the number of children enters linearly), share of female children in the household, and mean age of the children. One variable of special interest is the PROGRESA indicator, which is a dummy indicating whether or not the household is eligible for the cash transfers. All demographic variables are allowed to affect both the allocation of resources across individuals (they enter the term η_t), and the preferences of all individuals in the household (the terms α_t and β).[16] This means that, in the preferred specification, I estimate a total of 194 parameters.

4.4 Results and robustness checks

Table 3 reports the estimated coefficients of the main covariates for the resource shares of mother (η_m) and father (η_f).[17] These are some of the possible determinants of the resource share allocation

[15] NLSUR is iterated until the estimated parameters and covariance matrix converge. Iterated SUR is equivalent to maximum likelihood with multivariate normal errors.

[16] Hence I control for the potential effects that PROGRESA may have on individual preferences (De Rock et al., 2017).

[17] The full set of estimated coefficients of the preferred specification (A) are reported in Section A.2 of the Appendix. Table A.1 reports the adults' resource shares and the slope coefficient β.

process and can be related to bargaining power, although one shall not consider these necessarily as causal parameters. Column (A) reports the estimation results of the preferred specification with dummies for each kid and with all households and waves included.

Results are threefold. First, the most interesting variable for my purposes is the PROGRESA dummy. In all specifications, the effect is positive for the mother and negative for the father, and always significant. Moreover, the effect on the father is larger in magnitude, which implies that resources are redistributed from the father to *both* the mother and children, although the mother is the one benefiting the most. Second, as the number of kids increases, both adults reduce their shares roughly by the same amount. For instance, by the third child the mother has reduced her shares by 5 percent on average, whereas the father by 3 percent. This simply shows us that household's composition matters, a result in line with the findings of DLP and references therein. Finally, the coefficients picked up by the 2nd and 3rd wave dummies are always negative for both adults, indicating that children gain resources over time into the program.

Table 3: Main parameters' estimates: Resource shares of mother (η_m) and father (η_f)

	(A) Preferred		(B) No last wave		(C) Linear in kids	
Main variables	Mother (1)	Father (2)	Mother (3)	Father (4)	Mother (5)	Father (6)
One kid	0.332***	0.363***	0.313***	0.355***		
	(0.041)	(0.043)	(0.042)	(0.047)		
Two kids	0.303***	0.331***	0.294***	0.299***		
	(0.039)	(0.043)	(0.042)	(0.047)		
Three kids	0.284***	0.330***	0.286***	0.284***		
	(0.039)	(0.044)	(0.043)	(0.048)		
Constant					0.280***	0.503***
					(0.041)	(0.048)
Number of kids					-0.037***	-0.057***
					(0.007)	(0.009)
Treatment	0.026**	-0.036***	0.026**	-0.036***	0.015*	-0.029**
	(0.011)	(0.014)	(0.010)	(0.013)	(0.012)	(0.015)
2nd wave	-0.017	-0.009	-0.010	-0.006	-0.001	-0.013
	(0.016)	(0.018)	(0.014)	(0.016)	(0.018)	(0.020)
3rd wave	-0.042***	-0.032**	-0.032***	-0.040	-0.020	-0.018
	(0.016)	(0.020)	(0.014)	(0.018)	(0.018)	(0.021)
4th wave	0.027*	0.005			0.028*	0.013
	(0.017)	(0.018)			(0.018)	(0.021)
Rest of the controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,017	9,017	6,720	6,720	9,017	9,017
No. Parameters	194	194	188	188	174	174

Notes: Main parameters' estimates of the resource shares for the mother and father. The rest of the controls include: children mean age, share of girls, age of mother and father, education of mother and father, 7 state dummies. Standard errors clustered at the primary sampling unit (village) level. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

My results are robust to specifications and sample restrictions. First, in my preferred specification, I use four waves of PROGRESA, including also November 2000, in order to have a large(r) sample size. This choice is motivated by the need to increase the stability and reliability of the estimates of the large and non-linear system of equations. Nevertheless, in my specific case, estimating the system on the first three waves only (which reduces the sample size by 1/4) does not change neither the qualitative nor the quantitative results. Estimates are reported in column (B). I still prefer the former sample selection for efficiency (and speed of convergence of the estimator). Second, the DLP system is a rather complex model to bring to the data (Tommasi and Wolf, 2018). Indeed, different specifications may lead to instability of the results. An example of common specification that may cause instability is the definition of the resource share index with the number of kids either entering as dummy variables or linearly.[18] In my specific case, this choice does not lead to any instability of the estimates. This is a powerful robustness check and the results are reported in column (C). Third, Calvi (2016) estimates the DLP model adding a 4th equation of food share expenditure to the system. Although it is not required for identification, the choice may improve efficiency, as the error terms are likely to be correlated across equations. In my case, results are virtually identical and are omitted.[19]

4.5 Redistribution of individual resources

Given the reliability and stability of the model estimates, I can compute the amount of resources distributed to each individual. In Figure 1, Panels (A)-(C), I plot the empirical distribution of resource shares $\hat{\eta}_m$, $\hat{\eta}_f$ and $\hat{\eta}_c$ for households in both PROGRESA and Non-PROGRESA eligible villages. The summary statistics are reported in Panel (A) of Table 4. A simple visual inspection allows one to see the redistribution of resources inside the household induced by the program. By doing a simple back of the envelope calculation, I estimate that for every peso taken from the father, 60-75 cents go under the mother's control, and the rest to the children.[20]

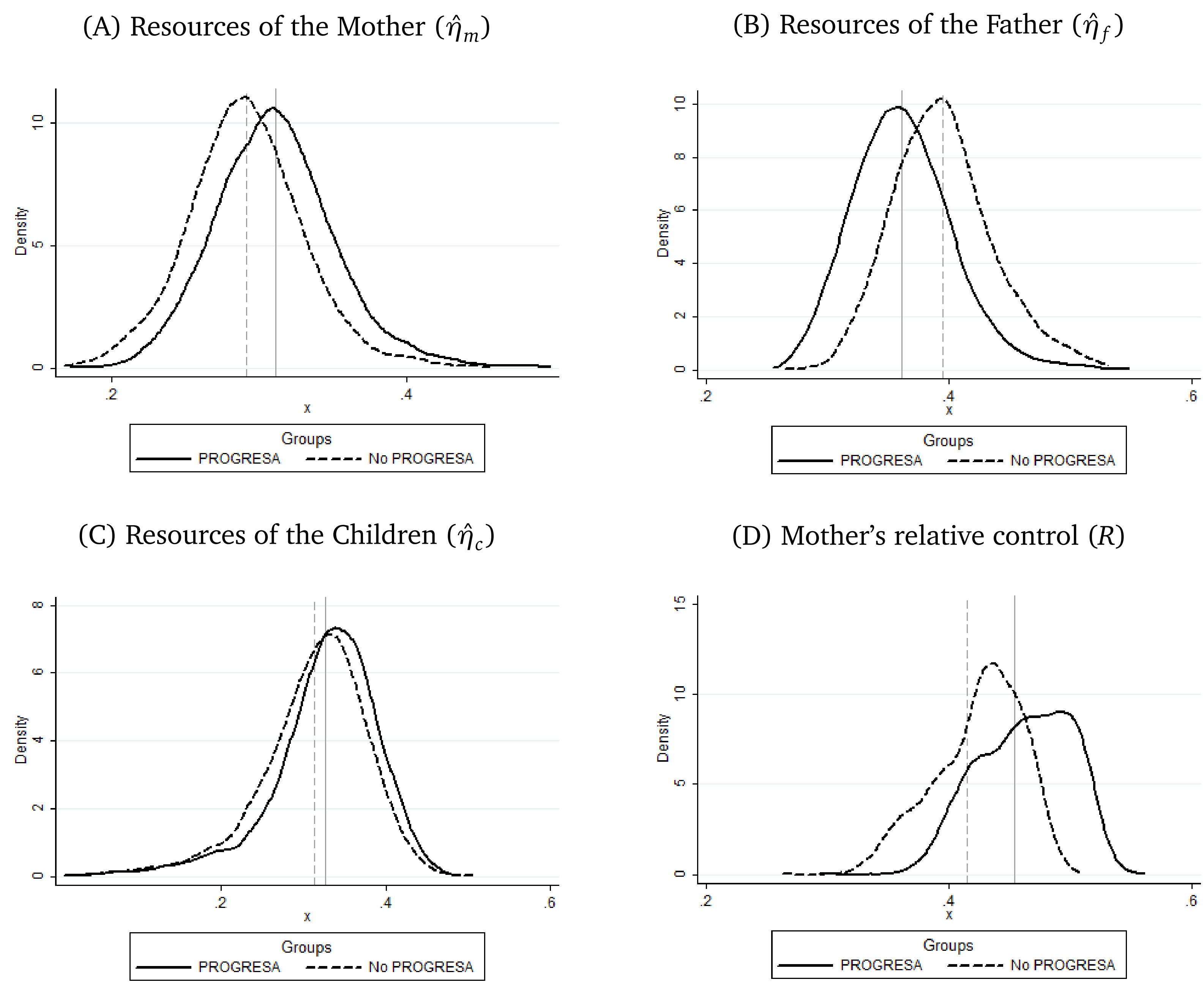
In Panel (D) of Figure 1, I plot the distribution of the resources controlled by the mother relative to the father, computed as $R_i = \frac{\hat{\eta}_{i,m}}{\hat{\eta}_{i,m}+\hat{\eta}_{i,f}}$. The descriptive statistics of Panel (D) are reported in Panel (B) of Table 4. The mother is estimated to control 47 and 42 percent of household resources in PROGRESA and Non-PROGRESA eligible villages, respectively, which is roughly an increase of 12% of her control relative to the father. These results are compatible with those of Klein and Barham (2018), for whom the mother largely increases her bargaining power relative to the father. Notice that one may argue that, at least in my context, a better measure of maternal control would be the sum of mother and child's resource shares, rather than mother's resources alone. This is because mothers are eligible to receive cash in part conditional on benefiting the children. While this may

[18] These authors show that the multiplicative feature of the model (between desired budget shares and resource shares) is a potential source of imprecision in the estimation of the empirical model. This is worsened, leading to weak identification, in case of relatively flatness of the Engel curves in the consumption of private assignable goods. Since the Engel curves of individual clothes and footware in PROGRESA are not that steep as one would wish for, in order to facilitate identification, I do two things: (i) I pool all the waves of PROGRESA and (ii) I set the starting values of the treatment effect parameter by using the sign (information) of the reduced-form estimates on control of resources, as reported in Section 3.2. This procedure limits the issue of searching for convergence in an undesirable region. This point could also contribute to explain the differences with Sokullu and Valente (2017).

[19] I also check for the internal validity of my estimates and the model assumptions. As as for the former, one important assumption in DLP is the independence of the resource shares with respect to total household expenditure. One can see this very clearly for my estimates in Figure A2 in the Appendix. As for the latter, I use data on singles to test for the validity of the Pareto efficiency assumption. Results are reported in Section A.1 of the Appendix.

[20] The fact that children benefit less than mothers, in relative terms with respect to the father, should not be a surprise. Recall that our sample is made of households with young children (up to 12 years old) where the school enrollment rate for those in the age rage 6-12 is almost universal. Hence, even in the absence of the cash transfers, the Non-PROGRESA group is already spending resources for child education, which is one of the main conditionality of the program.

Figure 1: Distribution of resource shares: PROGRESA and Non-PROGRESA recipients



Notes: These figures provide information on the distribution of resources, between PROGRESA and Non-PROGRESA households, for Mother ($\hat{\eta}_m$), Father ($\hat{\eta}_f$) and Children ($\hat{\eta}_c$). Moreover, subfigure (D) provides information on the distribution of Mother's relative control of resources (or empowerment), which is computed as: $R = \hat{\eta}_m/(\hat{\eta}_m + \hat{\eta}_f)$.

be true in principle, I prefer to define maternal control using only her shares because this is a conservative measure of her influence.[21]

The results of the structural model match a number of stylized facts documented in the literature. First of all, I confirm some of the findings by Adato et al. (2000) and the reduced-form estimates of Table 2: The program is positively associated with mothers controlling the extra resources, and negatively associated with fathers controlling them. The size of the effect is meaningful. This is also in line with the intuition of Attanasio and Lechene (2002, 2010, 2014) and Rubalcava et al. (2009), who find that, through PROGRESA, mothers increase the control of resources relative to fathers. Second, like in other developing countries, the father is the individual controlling the

[21]Moreover, by defining R_i in this alternative way I would not change the main qualitative results of the paper (but rather I would strengthen it), although it may reconcile the results with Sokullu and Valente (2017).

Table 4: Distribution of resource shares: Summary statistics

	No PROGRESA				PROGRESA			
	Mean	**SD**	**Min**	**Max**	**Mean**	**SD**	**Min**	**Max**
	Panel A: Resource shares over the entire period							
Mother ($\hat{\eta}_m$)	0.29	0.04	0.17	0.46	0.31	0.04	0.17	0.50
Father ($\hat{\eta}_f$)	0.40	0.04	0.26	0.53	0.36	0.04	0.25	0.55
Children ($\hat{\eta}_c$)	0.31	0.06	0.03	0.51	0.33	0.07	0.01	0.48
	Panel B: Mother's relative control							
$R = \frac{\hat{\eta}_m}{\hat{\eta}_m+\hat{\eta}_f}$	0.42	0.04	0.26	0.51	0.47	0.04	0.29	0.57

Notes: I report the descriptive statistics of the resource shares for mother, father, and children, depicted in Figure 1.

relative majority of resources in Mexico. Finally, I also find a similar declining pattern of mother's resources at older ages as documented specifically by Calvi (2016) for India. In Figure A3 in the Appendix, I plot mothers' resources relative to fathers' on the y-axis, and mother's age on the x-axis, divided by eligibility status. As one can see, a young mother living in a PROGRESA village is controlling roughly 90% of her spouse's resources. As age increases, the resources are progressively reduced to 77%.

5 Insights for policy

The structural estimates obtained thus far can be used in several ways to provide new insights for policy. The following section is divided in three main parts, one for each of the insight that I wish to highlight. First, I use the estimates of the DLP model to construct poverty rates that take into account the inequality of resources within the household, and not only across households, as traditionally done. Second, I use resource shares to construct a new proxy for women bargaining power that may be more informative to policy makers because it is inferred from actual individual behavior inside the household. Finally, I use this proxy of power to explore the link between the control of resources and the allocation of household budget in the context of the demand for food. My aim is to provide estimates of the direct effects of maternal control of household resources on the desired outcome. This corresponds to estimating an Engel curve, which is a relationship between total expenditure and budget shares, using a structurally-motivated measure of bargaining power as the main treatment of interest.

5.1 Intra-household poverty analysis

Standard poverty measures computed by, e.g., the World Bank and other Institutions commonly assume equal sharing of resources within the household. This is a strong limitation that can be overcome by taking into account the inequality of resource allocation among individuals. This is particularly interesting to estimate in my context, because the surveys have been collected to evaluate the impact of a welfare program whose objective was, among other things, to fight poverty among marginalized households. Hence, by using the information on resource sharing that I estimated, I am able to quantify in a more appropriate way the welfare effects of PROGRESA both in terms of change in individual consumption and poverty of each household's member. I compute individual-level resources as the product of total household non-durable resources and the estimated individual resource shares. I construct poverty head count ratios by comparing these individual's level expenditures to poverty lines. As a reference, I use the thresholds set by the World Bank for extreme poverty (1.90 US/day). As in Dunbar et al. (2013), I set the poverty lines for adults to be the same, and for children to be 60% of that of adults, in order to account for the fact that they may have different needs.

The results are reported in Table 5. Panel (A) contains the resource shares and poverty rates over the entire period of observation. As one can see, during the first year of cash transfers, the percentage of poor mothers goes down from 94 percent to 87 and of poor children from 93 to 92. Notice that, in levels, both mothers and children are always poorer than fathers, hence one further positive aspect of the policy is an improvement of within household inequality. Panel (B) reports the change of resource shares and poverty rates disaggregated by the number of children. As one can see, PROGRESA is beneficial to mothers regardless of the number of children, whereas children in smaller households are better off both in levels and, differentially, by PROGRESA. Panel (C) reports the change of resource shares and poverty rates over time. Here I compute the resource shares and poverty rates for all household members, disaggregated by wave of observation. One can see that, as time goes by, there is a clear negative trend for child poverty rates in PROGRESA villages compared to the Non-PROGRESA ones, going from a difference of 1 percent in the first wave, to 6 percent by the third wave.[22]

Overall, these results confirm findings by Skoufias et al. (2001) and Handa et al. (2001) in terms of reducing short term household poverty and inequality. I complement them by showing that, within the household, there is a further reduction in inequality between richer (father) and poorer (mother and children) individuals.[23]

5.2 A structurally-motivated measure of bargaining power

In order to proxy bargaining power, researchers often use ad-hoc measures of power based on, e.g., self-reported indicators of control and decision power within the household (e.g. see Reggio (2011) for an application on the Mexican population). Other proxies commonly found in the literature are: unearned income (e.g. Schultz (1990) and Thomas (1990)), shares of income earned by woman (e.g. Hoddinott and Haddad (1995)), assets at marriage (e.g. Quisumbing (1994) and Thomas et al. (2002)), and education difference (e.g. Quisumbing and Maluccio (2003), Gitter (2008), Schady and Rosero (2008)). Although popular, these type of measures are quite crude, imprecise,

[22] I do not look at the last period of observation, November 2000, because here also households in Non-PROGRESA villages become eligible and hence there is no comparison group and it is also difficult to make a comparison with the trend of the previous year of observation.

[23] First such estimates on individual poverty rates for PROGRESA are also present in an earlier version of Tommasi and Wolf (2016). Here I provide more stable results combining all the available (early) waves of the evaluation surveys.

Table 5: Predicted resource shares and poverty rates

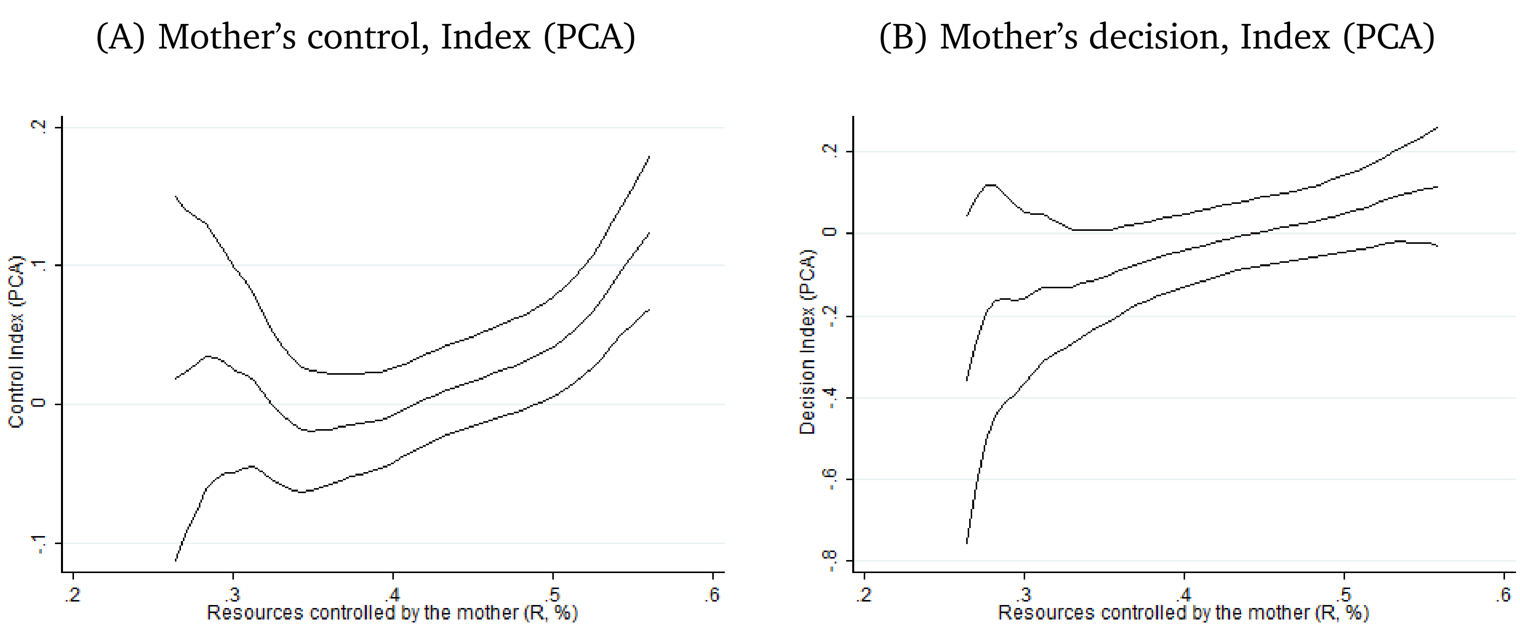
	No PROGRESA					PROGRESA				
	Mean	**SD**	**Min**	**Max**	**Poor (%)**	**Mean**	**SD**	**Min**	**Max**	**Poor (%)**
	Panel A: Average over the entire period of observation									
Mother	0.29	0.04	0.17	0.46	0.94	0.31	0.04	0.17	0.50	0.87
Father	0.40	0.04	0.26	0.53	0.82	0.36	0.04	0.25	0.55	0.82
Children	0.31	0.06	0.03	0.51	0.93	0.33	0.07	0.01	0.48	0.92
	Panel B: Disaggregated by the number of children									
1 child										
Mother	0.31	0.04	0.20	0.45	0.95	0.34	0.05	0.21	0.52	0.89
Father	0.41	0.04	0.32	0.53	0.87	0.38	0.05	0.28	0.55	0.85
Children	0.28	0.07	0.04	0.41	0.79	0.27	0.08	0.00	0.48	0.71
2 children										
Mother	0.29	0.04	0.18	0.43	0.94	0.32	0.04	0.21	0.48	0.87
Father	0.39	0.04	0.27	0.52	0.83	0.35	0.04	0.25	0.50	0.83
Children	0.33	0.06	0.11	0.51	0.94	0.32	0.06	0.08	0.48	0.94
3 children										
Mother	0.28	0.04	0.17	0.38	0.93	0.31	0.04	0.18	0.47	0.86
Father	0.40	0.04	0.31	0.53	0.79	0.36	0.04	0.26	0.49	0.79
Children	0.33	0.06	0.14	0.47	1.00	0.32	0.06	0.10	0.47	1.00
	Panel C: Disaggregated by wave of observation									
October 1998										
Mother	0.31	0.04	0.21	0.46	0.92	0.33	0.04	0.23	0.50	0.90
Father	0.41	0.04	0.29	0.53	0.79	0.37	0.04	0.27	0.55	0.84
Children	0.29	0.06	0.03	0.46	0.95	0.30	0.06	0.01	0.43	0.94
May 1999										
Mother	0.29	0.04	0.17	0.45	0.94	0.31	0.04	0.17	0.48	0.90
Father	0.40	0.04	0.29	0.53	0.83	0.37	0.04	0.27	0.53	0.84
Children	0.31	0.06	0.06	0.46	0.94	0.32	0.06	0.06	0.45	0.92
November 1999										
Mother	0.27	0.04	0.17	0.40	0.96	0.29	0.04	0.20	0.46	0.90
Father	0.38	0.04	0.26	0.52	0.85	0.34	0.04	0.25	0.52	0.84
Children	0.35	0.06	0.08	0.51	0.91	0.37	0.06	0.10	0.48	0.86

Notes: Panel (A) reports the descriptive statistics of the resource shares for mother, father, and children, depicted in Figure 1. Specifically I report the mean, standard deviation and minimum and maximum values. I also report the percentage of individuals labelled "poor". Panel (B) reports the same information disaggregated by the number of children, whereas Panel (C) disaggregated by wave of observation.

and often focused on very specific topics. This implies that they may be problematic if used to motivate policy interventions.

Hence, differently from these approaches, I argue that one can use resource shares to construct a variable of power that is closely related to observed individual behavior. This is a more useful measure for policy analysis because resource shares have an immediate behavioral interpretation. However, since using resource shares is an unconventional approach to measure empowerment or control of resources, I compare my structural estimates with conventional measures of control or decision power. A similar exercise was conducted by Calvi et al. (2017). I construct two indices of mother's control and mother's decision by combining information on a set of self-reported indicators using principal component analysis (PCA). Panel (A) of Figure 2 displays the results of a non-parametric regression of mothers' reported control of resources on my estimated maternal control R. Whereas Panel (B) shows the non-parametric relationship between an index of mothers' participation in household decisions and R. In both cases, the presence of a positive relationship emerges clearly. These correlations remain significant even if I regress each of the two indexes on R, controlling for individual and household's characteristics.[24] Overall I am able to match also the stylized fact that mothers are significantly more likely to report participating in decisions in households when they have substantial control over resources, estimated using expenditures.

Figure 2: Alternative measures of bargaining power



Notes: Mother's control and Mother's decision are two indices constructed by combining information on a set of self-reported indicators using principal component analysis. The former is constructed using two answers: whether the mother controls the household budget and whether the mother makes important expenditure decisions. The latter is constructed using nine answers about different smaller expenditure decisions, on schooling of the children and other measures of independence.

5.3 The effects of controlling resources on the demand of food

The effectiveness of cash transfer programs depends crucially on their ability to shift the control of household resources towards the targeted individuals. In case of PROGRESA, this would mean that mothers can allocate the available budget such that it is closer to their preferences. In this section I explore the link between the maternal control of resources and the allocation of household budget in the context of the demand of food. I focus on food because this commodity accounts for roughly 74% of the household budget and it is the primary determinant of welfare for households in my dataset.

[24] The estimation results are reported in Panel (A) of Table A.2 of the Appendix.

There are two main challenges associated with the task at hand. First, to construct an appropriate Engel curve system for the demand of food. In order to do so, I draw heavily on the recent literature estimating Engel curves and I briefly discuss the appropriate empirical specification for my context in Section 5.3.1. Second, to construct the appropriate treatment variable for maternal control of resources, since in practice it is unobserved. Using a treatment variable that is estimated, or derived, from a structural model requires to take into account the fact that the model may be misspecified and, even if it is not, the fact that the treatment variable contains estimation and measurement errors. I outline the estimation strategy that accounts for this issue in Section 5.3.2.

The remaining two sub-sections are dedicated to present the main results of the analysis and the policy implications that follow.

5.3.1 Engel curves for food: Specification

In the context of the Mexican PROGRESA, Engel curves of food have been estimated by Attanasio and Lechene (2010, 2014) and Attanasio et al. (2013). Although the dataset contains very detailed information on food, it is not feasible to model the demand for several dozens of items. Hence I aggregate the data to construct the share of all food items over total non-durable expenditure, where the main determinants are the price of food, the prices of other non-durables, and total expenditure on non-durables.

In order to estimate Engel curves of food, I have to consider the following methodological issues: (i) Whether the relationship between budget and total expenditure is linear or quadratic; (ii) How to control for price variation; and (iii) How to control for endogeneity of total expenditure. Following Attanasio and Lechene (2010), the preferred specification for food in this dataset is AIDS. Moreover, to control for price differences, I allow the intercept to shift by state, time, and their interaction. Whereas, average agricultural wage (and its square) in a village is used to account for endogeneity of total expenditure. For several other details about the specification, estimation strategy, and benchmark results with respect to the literature, see Appendix A.3.

Let r_j, $j = 1, \ldots, 28$, be the interaction between the 7 states and 4 time periods, the ("simplified") AIDS specification that I consider is the following:

$$w_i = \alpha_i + \sum_{j=1}^{28} \gamma_j r_j + \beta \ln x_i + \epsilon_i \tag{8}$$

where w_i is the budget share of food spent by household i, x_i is total expenditure on goods, α_i is a linear index including the demographic variables, ϵ_i is the error term.

5.3.2 Estimation strategy

My goal is to estimate the effects of maternal control of resources on the demand of food. The mechanism that I have in mind is intuitive: the larger a mother's control over resources, the closer to her preferences is the observed household behavior. I wish to estimate this treatment effect even though the true underlying value of the mother's share of resources controlled, R^*, is unobserved. In order to get around this identification problem, I use a new estimator recently introduced by Calvi, Lewbel, and Tommasi (2017) (Mismeasured Robust LATE or MR-LATE). MR-LATE allows to recover treatment effects when a (binary) treatment variable is misspecified, misclassified, or estimated with error. I employ this strategy using the pooled sample of Section 3.1 in a standard Engel curve framework augmenting equation (8). Details of the estimator are presented in Appendix A.4. Here I wish to outline the four main issues that need to be discussed in order to understand the empirical results that follow.

First, I do not know what is the true relationship between the continuous variable R^* and w. This means that, in my setting, I do not know what is the appropriate specification that relates the control of resources to the demand for food. Following recent insights by Bertrand et al. (2015), I assume that the discrete value

$$D = \mathbb{I}(R^* \geq 0.50) \tag{9}$$

is a relevant treatment for the demand for food. This is a reasonable assumption to make as long as controlling the majority of resources gives the right to determine most of the expenditure decisions.[25] Studying the effect of D on w corresponds to the following thought experiment: What would be the change in the demand for food of a household if in one counterfactual the mother was given the control of the majority of resources, versus a counter factual where she was given only a smaller fraction? The model I bring to the data is then:

$$w_i = \alpha_i + \delta D_i + \sum_{j=1}^{28} \gamma_j r_j + \beta \ln x_i + \epsilon_i \tag{10}$$

where δ is the main parameter of interest. Even though a discrete treatment may be criticizable, notice that there are at least two advantages in considering D. First, the estimation bias caused by a misclassification of treated (large control) and control (small control) individuals is likely to be the most detrimental one.[26] Hence, by accounting for the largest potential source of bias, I can obtain more credible estimates. Second, for this setting, I have an econometric tool that allows me to recover, under certain conditions, the parameter of interest. Although further research is needed to develop econometric tools to deal with continuous mismeasured treatment variables, I believe that my choice and estimates may constitute a valid starting point to study the effects of controlling resources.

Second, the variable accounting for the share of resources controlled by the mother is constructed using the estimates $\hat{\eta}_m$ and $\hat{\eta}_f$ obtained in Section 4.5. For each household i:

$$R_i = \frac{\hat{\eta}_{i,m}}{\hat{\eta}_{i,m} + \hat{\eta}_{i,f}} \tag{11}$$

[25] This is analogous to voter models, where the policy outcome is primarily determined by the candidate obtaining the largest number of votes. A similar argument was used in the empirical application of Calvi et al. (2017).

[26] In other words, if I imagine classifying the sample in different smaller groups based on resources controlled, mistaking an individual for another group nearby, is less problematic (yields less bias) than mistaking a treated (large control) individual for a control (small control) one, or vice versa.

My preferred specification and best estimate of D is $T = \mathbb{I}(R \geq 0.50)$. In the robustness checks I study the sensitivity of the results to different thresholds of control around this value. Table 6 reports the main statistics for my estimated T. In the sample, mothers who have $T = 1$ (18% of the sample) have on average $R = 0.52$, while those having $T = 0$ (82% of the sample) have on average $R = 0.44$. Hence, while I cannot know the actual fraction of resources controlled by the true treated and untreated households, i.e., $E(R^*|D)$, my rough estimate of $E(R|T)$ indicates that mothers in the treated group control 8 percent points more resources than those in the control group. This means that I consider the effects of a large change in control by 18 percent with respect to the control group.

Table 6: Estimated resource shares and Mother's control

	Obs.	Mean	SD	Min	Max
Full sample:					
Mother's Resource Share (R)	9,010	0.46	0.04	0.26	0.57
$T = \mathbb{I}(R \geq 0.50)$:					
Mother's Resource Share (R)	1,629	0.52	0.01	0.50	0.57
$T = \mathbb{I}(R < 0.50)$:					
Mother's Resource Share (R)	7,381	0.44	0.04	0.26	0.50

Notes: Household level data for all waves combined. R is computed as outlined in Equation (11).

Third, my analysis using MR-LATE is based on two proxies of the true treatment, i.e., $T_i^a = \mathbb{I}(R_i \geq 0.50 + \kappa^a)$ and $T_i^b = \mathbb{I}\left(R_i < 0.50 - \kappa^b\right)$, which are defined on the basis of the chosen constants κ^a and κ^b. $T^a = 1$ if a mother controls the *majority* of household resources, 0 otherwise, and $T^b = 1$ if a mother controls the *minority* of household resources, 0 otherwise. Since the measurement error that relates R^* and R, $R^* = R + \epsilon$, is unknown and unbounded, then also the optimal constants are unknown. This places me in the second result of Theorem 1 of Appendix A.4, where I can use MR-LATE to set identify (bound) the LATE. In the absence of an optimal strategy in the literature for the choice of these constants, I use the following algorithm. Let $\mathcal{K}$ be the percentage of individuals assumed to be misclassified in our sample, and let κ^a be the value such that $\mathcal{K}/2$ percentage of the sample has R in the interval $[50, \kappa^a]$ and κ^b be the value such that $\mathcal{K}/2$ percentage of the sample has R in the interval $[\kappa^b, 50]$.[27] I consider five percentages: $\mathcal{K} = \{0, 5, 10, 15, 20\}$. For each element of $\mathcal{K}$ I choose the corresponding κ^a and κ^b and estimate MR-LATE. The preferred specification is the one where the assumed percentage of misclassified individuals yields two mismeasured indicators T^a and T^b whose F-test, with respect to the excluded instrument Z, in the first stage is above the threshold 20. That is, by doing this, I am defining two treatment indicators which are still informative and, at the same time, I am taking care of as much misclassification as possible given my dataset.

Finally, since the mismeasured treatment is endogenous, I use the targeting of PROGRESA as

[27] Notice that this is consistent (but does not require) having e being centered around 50 percent, implying that households with $D = 1$ are the ones in which the mother has control over the majority of household resources.

an ideal (randomized) instrumental variable, where $Z = 1$ if a household is eligible to receive the grant, 0 otherwise. Recall that total expenditure is also endogenous in my system, and I use the average agricultural wage at village level (and its square) to instrument for it. Hence, in practice, I am instrumenting two endogenous variables with three external instruments.

5.3.3 Results and robustness checks

Table 7 reports the results obtained from estimating equation (10). The effects on food share in Columns (1)-(6), are estimated on, respectively, the first 2 waves (October 1998 and May 1999), 3 waves (adding November 1999), and the full sample (adding November 2000). Panel (A) reports the benchmark (ITT or Attanasio and Lechene (2010)) results of Section A.3.1. Panel (B) reports the new results of the effects of maternal control over resources on the demand of food under the assumption that $D = 1(R^* \geq 0.50)$ is the relevant treatment. In all specifications, I control for the same set of covariates as I used for the estimation of Section 4.3. I also instrument total expenditure by using the average agricultural wage at the village level (and its square). Standard errors are bootstrapped 200 times and clustered at village level. I provide the results for three sets of estimation techniques.

In the first line, I report the results of the model under the assumption that the new treatment variable is exogenous and measured without error. In this case the effect of the treatment is positive, smaller than the ITT estimates, and never significant. Whereas the slope of the demand curve is negative, as theory would explain, but much smaller than the ITT estimates. I conclude that, without taking into account the endogeneity and measurement error of the new treatment indicator, the estimates become quite distorted. In the second line, I relax the assumption that D is exogenous. This amounts to estimate the model with a control function approach, using Z as an excluded instrument for my proxy of D, still under the assumption that there is no measurement error and hence no misclassification of the treatment indicator.[28] These are the standard 2SLS-LATE estimates, which are positive, significant and quite large, which is consistent with the idea that LATE is larger than ITT. Notice that the slope of the demand curve goes back to the numerical values of the ITT estimates of Panel (A). Two points are worth highlighting. First, these are the estimates that a practitioner would obtain in the absence of the MR-LATE estimator. Second, these estimates are numerically equivalent to applying the MR-LATE estimator under the assumption that there is no measurement error ($\kappa^a = \kappa^b = 0$, see result 3 of Theorem 1 of Appendix A.4).

In the third line, I report the results of my preferred specification, which accounts also for mis-specification, misclassification, and estimation errors of the binary treatment indicator. For $j = a, b$, the estimation procedure consists of regressing $Y_i T_i^j$ on a constant, T_i^j, and X_i using the control function approach (with Z_i being the excluded instrument for T_i^j). The MR-LATE parameter is then obtained as the difference between the estimated coefficients of treatment in these two 2SLS regressions, that is: $\widehat{\rho} = \widehat{\lambda}^a - \widehat{\lambda}^b$. Using the procedure described before for the choice of κ^a and κ^b,

[28] As before, I generate third degree polynomial of the residuals from the first stage and add them to the main structural equation. As expected, the residuals from the first stage are significant in the demand equation, which indicates a strong rejection of exogeneity of the new treatment indicator.

Table 7: MR-LATE: Effects of the control of the resources

	Panel A: Targeting					
	2 waves		3 waves		4 waves	
	PROGRESA	ln(x)	PROGRESA	ln(x)	PROGRESA	ln(x)
	(1)	(2)	(3)	(4)	(5)	(6)
Benchmark	0.025***	-0.226**	0.035***	-0.253***	0.030***	-0.183***
	(0.008)	(0.095)	(0.007)	(0.078)	(0.008)	(0.061)
	Panel B: Control $D = 1(R^* \geq 0.50)$					
	2 waves		3 waves		4 waves	
	Treatment	ln(x)	Treatment	ln(x)	Treatment	ln(x)
	(1)	(2)	(3)	(4)	(5)	(6)
D exogenous	0.007	-0.152***	0.011	-0.144***	0.010	-0.134***
	(0.010)	(0.012)	(0.008)	(0.012)	(0.007)	(0.011)
LATE	0.111***	-0.226**	0.146***	-0.252***	0.139***	-0.180***
	(0.039)	(0.094)	(0.041)	(0.074)	(0.034)	(0.052)
MR-LATE	0.065*	-0.224**	0.082**	-0.232***	0.083***	-0.158***
	(0.035)	(0.092)	(0.041)	(0.071)	(0.029)	(0.044)
Controls	Yes		Yes		Yes	
Observations	4,719		6,697		8,982	
Misclassified (%)	5		5		10	

Notes: The results in Panel (A) correspond to the results of reported in Section A.3.1. The results in Panel (B) use the new treatment indicator of mothers controlling resources under the assumption that the relevant treatment is $D = 1(R^* \geq 0.50)$. In all specifications I control for: dummies for number of kids, dummies for number of kids enrolled in school, mean age of the kids, share of girls in the household, age and education of head and spouse, whether the head can speak indigenous language, number of individuals eating in the household and outside the household, time and state dummies. I control for price variation by interacting time and state dummies. As for total expenditure, I follow the standard (AIDS) approach in Engel curve estimation by instrumenting with average agricultural wage in the village (and its square). Standard errors are bootstrapped 200 times and clustered at the primary sampling unit (village) level. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

I am able to account for up to 5 to 10 percent of possible misclassified individuals in my sample, as reported at the bottom of the Table. As one can see, the estimated parameters of the treatment indicator are substantially lower with respect to the results of the second line. This means that misclassification is a relevant problem in my sample and I am able, at least partially, to account for it.[29] Results establish that households whose mother goes from controlling the minority to the majority of resources increase the demand for food by 6.5-8.3 percent, depending on the specification. Also these magnitures are in line with the recent contribution by Klein and Barham (2018), albeit they follow a completely different approach. The slope of the demand curve is still close in magnitude with respect to the specification that does not account for measurement error.

I provide three main robustness checks to support the results. First, since R is measured with

[29]Recall that MR-LATE $= (q^a - q^b)$ LATE. The fact that the point estimates of the parameter ρ goes down (closer to ITT) as κ^a, κ^b increase, it tells us something about the unknown objects q^a and q^b and about the composition of the misclassified individuals in our exercise. Indeed, in the ideal scenario of no misclassification, $q^a = 1$ and $q^b = 0$. With misclassification, $q^a > 1$ and $q^b < 0$, which makes MR-LATE estimates larger than LATE. By increasing κ^a, κ^b, we get $q^a \to 1$ from the right, and $q^b \to 0$ from the left. Moreover, having MR-LATE > LATE and decreasing as $\kappa^a, \kappa^b \neq 0$ means that, for compliers, the share of actual treated in T^a is larger than the share of misclassified actual untreated, and analogously, the share of actual untreated in T^b is larger than the share of misclassified actual treated.

error, the values of κ^a and κ^b around the 50 percent cut-off may not be large enough to contain the true threshold. This is equivalent to having 50 percent as an inappropriate cut-off for a large part of the sample.[30] For instance, if it is enough for a large part of the mothers in the sample to control 46 percent (the average of the distribution) of household resources to become sufficiently influential on the choices of food budget, then I would also fail to capture the relevant threshold. In order to study how sensitive this choice is with respect to the estimates that I have obtained, in Figure A4 of the Appendix I provide a graphical illustration showing that the results obtained are robust to different choices of cut-offs around my preferred value (as long as the cut-offs are not too far from the 50% threshold). On the x-axis I have four different choices of cut-off: 44, 46, 48, and 50 percent of household resources controlled by the mother. I study the sensitivity of the threshold on the left side of 50 both because the largest density of observations is around 46 and also because, for choices above 51, the standard errors of the estimated parameters become very large and unreliable. On the y-axis I report the estimated effect of the treatment, $\hat{\rho}$, for each model and accounting for 5 to 10 percent of misclassified households like in the main results. As one can see, the choice of the cut-off does not lead to substantially different results.[31] Second, the definition of R in (11) may also be arbitrary. Instead of the ratio of mother's resources over adults' resources, one may consider the ratio of mother to father's resources, or the resources directly controlled by the mother, $\hat{\eta}_m$. By re-running the regressions using these two newly defined variable R, I obtain estimates that are qualitatively similar to my preferred specification. Results are not reported but available upon request. Finally, as we can see in Table 7, the results are also consistent across samples of estimation.

5.3.4 Implications

The first implication of my analysis is that, by taking a ratio of the ITT over MR-LATE estimates for our preferred specification in Table 7, I can obtain a rough estimate of how many compliers I have in my sample. Roughly 36 percent of observations are compliers, that is, households where the mother was controlling a minority of resources and was moved to control the majority of resources. This number is of course imprecise because MR-LATE is only an imperfect estimate of LATE and there is yet no theoretical result relating MR-LATE to the standard ITT estimator to be able to calculate the exact number. But this is already an indication that a substantial portion of the households may have actually be moved by the policy. The second implication of my results is that, within the ITT estimates of the effects of targeting on food, there is substantial heterogeneity. That is, mothers with little control (to start with) of resources are "more reactive" to cash transfers. I argue that this heterogeneity is a valuable information and it can be used to think differently about the current design of cash transfer programs.

[30] Recall that, for each observation i, the true treatment is $D = \mathbb{I}(R^* \geq e_i)$, where e_i may vary across observations. That is, different observations may have a different cut-off beyond which the household decision making process changes. 50 percent constitutes a guessed mid-point for many of these observations.

[31] One additional point is important highlighting. By changing the cut-off, I also change the definition of our treatment indicator. Hence the new estimates may refer to different compliers and would be difficult to compare to my preferred specification. However, if by changing the cut-off, the share of compliers remain substantially the same, then also the estimates are comparable and robustness is valid.

Table 8: Policy implications: Simple numerical example

	Current design		
	Family 1	Family 2	Family 3
Pesos eligible	800	800	800
% ot total income	10	10	10
Treatment group	$1 \rightarrow 1$	$1 \rightarrow 1$	$0 \rightarrow 0$
	Alternative design		
	Family 1	Family 2	Family 3
Pesos eligible	550	550	1300
% ot total income	20	5	5
Treatment group	$1 \rightarrow 1$	$1 \rightarrow 1$	$0 \rightarrow 1$

Although it must be taken with caution, the numerical example outlined in Table 8 is useful to understand my point. Suppose in the sample of eligible households there are three types of families, 2 of which have powerful mothers controlling, for example, 51% of household resources, whereas 1 family has a weaker mother controlling only 43% of resources. On average, each eligible household is entitled to receive a constant 800 pesos in the form of cash to the mother, which is roughly 10% of household income in the sample. My argument is that this sum of money is given irrespective of the pre-program bargaining position or distribution of resources inside the household. Following the implementation of the program, and according to my estimates, the mothers in the first type of family would improve further their position, but they would not influence the aggregate demand for food. Whereas the mother in the second type of family would not improve enough her position to be able to influence the household budget and hence the aggregate demand for food. This implies that if we had allocated roughly 550 pesos to the first 2 families, instead of 800, and 1300 pesos to the second type of family, we could have increased the control of the latter mother beyond the threshold to make her a powerful decision maker of the household budget. That is, with the same program resources, but with a more articulated design of the transfers accounting also for the pre-program distribution of resources, we could have increased further the aggregate demand of food (for compliers).

There are two main concerns associated with the feasibility of this policy implication: (i) What should (in practice) a policy maker do to implement such a design? (ii) Would this be feasible to do? As for the first concern, I argue that policy makers should not do any different from what they already do now to implement such an alternative CCT program. This design would still require a data collection pre-intervention to determine who is eligible to receive the cash (and how much) and who is not. The only difference is that, in my case, it would require an additional question in the survey about the distribution of pre-program resources (e.g. expenditure on assignable goods). As for the second concern, one may argue that such an alternative design is not feasible because it would determine some discrimination across households in terms of amount of cash they are eligible and they may be contrary to it. I argue that common designs already embed some discrimination because the eligibility to receive the cash is still determined by some threshold which forces a fraction of households not to be eligible. Yet, this does not seem to be a concern in practice.

6 Conclusion

In the present paper, I study how resources are distributed to each individual inside the household and investigate their determinants. I focus on the context of PROGRESA, which is a well known conditional cash transfers program that was implemented in rural Mexico at the end of the 1990s. In order to recover the amount of resources controlled by each decision maker, I use a structural model of the household. My estimates show that mothers eligible to receive the grant increase their resources relative to the father, which is consistent with the reduced-form evidence.

I use these estimates to provide three main policy insights. First, I conduct a poverty analysis at the individual level and show that, within the household, PROGRESA reduces poverty rates for mothers and children, relative to the fathers. Second, I build a new proxy for women bargaining power and argue that this is a more useful measure for policy analysis because resource shares have an immediate behavioral interpretation. Third, I construct a mismeasured binary treatment indicator of mothers' control, under the identifying assumption that mothers controlling the majority of household resources have the right to determine most of the expenditure decisions. I then use a novel estimation strategy introduced by Calvi, Lewbel, and Tommasi (2017) (MR-LATE) to investigate its causal effect on the demand for food in a standard Engel curve context. I show that if one is able to move resources away from the father to the mother so that the mother becomes the primary holder of resources inside the household, there is a large positive effect on household demand for food. Importantly, this can be achieved in different ways, regardless of the experimental design, as long as the mother controls the majority of resources.

Further research should focus on two extensions. First, a similar study should be conducted in experimental settings where both mothers and fathers were assigned to receive the cash transfers. This would allow to compare the magnitude of the two mechanisms at play: Redistribution favoring mothers versus favoring fathers. It would determine the reason why in certain contexts, like Burkina Faso or Morocco (Akresh et al., 2016; Benhassine et al., 2015), and not in others (Almas et al., 2015), we obtain the same effects of the treatment by randomizing the gender of the recipient of cash transfers. Second, in terms of the main policy insight, one should generalize the thought experiment of giving mothers either a large control (majority) of household resources or a small control (minority). In other words, one should better understand the relationship between the unobserved continuous variable R^*, which is the actual relative amount of resources controlled by the mother, and the observed output, in order to provide a more general estimate of the relationship of interest. This, however, requires also to generalize the MR-LATE framework that allows to study the effects of mismeasured treatment variables.

References

ADATO, M., B. D. LA BRIERE, D. MINDEK, AND A. QUISUMBING (2000): "The impact of PROGRESA on womens status and intrahousehold relations," Final report, international food policy research institute, washington dc. [11], [18]

AKRESH, R., DE WALQUE, D., AND H. KAZIANGA (2016): "Evidence from a Randomized Evaluation of the Household Welfare Impacts of Conditional and Unconditional Cash Transfers Given to Mothers or Fathers," World Bank Policy Research Working Paper 7730. [5], [30]

ALMAS, I., A. ARMAND, O. ATTANASIO, AND P. CARNEIRO (2015): "Measuring and Changing Control: Women's Empowerment and Targeted Transfers," NBER Working Paper 21717. [5], [30]

ANGELUCCI, M. AND O. ATTANASIO (2013): "The Demand for Food of Poor Urban Mexican Households: Understanding Policy Impacts Using Structural Models," *American Economic Journal: Economic Policy*, 5, 146–78. [5]

APPS, P. F. AND R. REES (1988): "Taxation and the Household," *Journal of Public Economics*, 35, 355 – 369. [5]

ATTANASIO, O., E. BATTISTIN, AND A. MESNARD (2012): "Food and Cash Transfers: Evidence from Colombia*," *The Economic Journal*, 122, 92–124. [5], []

ATTANASIO, O. AND V. LECHENE (2002): "Tests of Income Pooling in Household Decisions," *Review of Economic Dynamics*, 5, 720–748. [18]

——— (2010): "Conditional cash transfers, women and the demand for food," . [5], [18], [23], [26], []

ATTANASIO, O., V. D. MARO, V. LECHENE, AND D. PHILLIPS (2013): "Welfare consequences of food prices increases: Evidence from rural Mexico," *Journal of Development Economics*, 104, 136 – 151. [23]

ATTANASIO, O. P. AND V. LECHENE (2014): "Efficient responses to targeted cash transfers," *Journal of Political Economy*, 122, 178–222. [18], [23], []

BANKS, J., R. BLUNDELL, AND A. LEWBEL (1997): "Quadratic Engel Curves And Consumer Demand," *The Review of Economics and Statistics*, 79, 527–539. []

BARGAIN, O. AND O. DONNI (2012): "Expenditure on children: A Rothbarth-type method consistent with scale economies and parents' bargaining," *European Economic Review*, 56, 792–813. [6]

BARGAIN, O., G. LACROIX, AND L. TIBERTI (2018): "Validation of Collective Models for Individual Welfare Analysis using Direct Evidence on Sharing," mimeo. [6]

BATTISTIN, E., M. D. NADAI, AND B. SIANESI (2014): "Misreported schooling, multiple measures and returns to educational qualifications," *Journal of Econometrics*, 181, 136 – 150. [6]

BENHASSINE, N., F. DEVOTO, E. DUFLO, P. DUPAS, AND V. POULIQUEN (2015): "Turning a Shove into a Nudge? A "Labeled Cash Transfer" for Education," *American Economic Journal: Economic Policy*, 7, 86–125. [5], [30]

BERTRAND, M., E. KAMENICA, AND J. PAN (2015): "Gender Identity and Relative Income Within Households," *The Quarterly Journal of Economics*, 130, 571–614. [24]

BLUNDELL, R., P.-A. CHIAPPORI, AND C. MEGHIR (2005): "Collective Labor Supply with Children," *Journal of Political Economy*, 113, 1277–1306. [5]

BLUNDELL, R. AND J. ROBIN (1999): "Estimation in Large and Disaggregated Demand Systems: An Estimator for Conditionally Linear Systems," *Journal of Applied Econometrics*, 14, 209–232. []

BROWN, C., J. PENGLASE, AND R. CALVI (2018): "Sharing the Pie: Undernutrition, Intra-household Allocation, and Poverty," Boston college. [6]

BROWNING, M., F. BOURGUIGNON, P.-A. CHIAPPORI, AND V. LECHENE (1994): "Income and outcomes: A structural model of intrahousehold allocation," *Journal of Political Economy*, 102, 1067–1096. [5]

BROWNING, M. AND P. A. CHIAPPORI (1998): "Efficient Intra-Household Allocations: A General Characterization and Empirical Tests," *Econometrica*, 66, pp. 1241–1278. [5]

BROWNING, M., P.-A. CHIAPPORI, AND A. LEWBEL (2013): "Estimating Consumption Economies of Scale, Adult Equivalence Scales, and Household Bargaining Power," *Review of Economic Studies*, 80, 1267–1303. [6], [14], []

BROWNING, M., P.-A. CHIAPPORI, AND Y. WEISS (2014): *Economics of the Family*, Cambridge University Press. [6]

CALVI, R. (2016): "Why Are Older Women Missing in India? The Age Profile of Bargaining Power and Poverty," *Mimeo*. [6], [9], [17], [19], []

CALVI, R., A. LEWBEL, AND D. TOMMASI (2017): "LATE with Mismeasured or Misspecified Treatment: An Application to Women's Empowerment in India," ECARES working paper 2017-27. [3], [6], [9], [22], [24], [30], []

CHERCHYE, L., B. DE ROCK, AND F. VERMEULEN (2012): "Economic well-being and poverty among the elderly: An analysis based on a collective consumption model," *European Economic Review*, 56, 985–1000. [6]

CHIAPPORI, P.-A. (1988): "Rational household labor supply," *Econometrica: Journal of the Econometric Society*, 63–90. [3], [5]

——— (1992): "Collective Labor Supply and Welfare," *Journal of Political Economy*, 100, pp. 437–467. [3], [5], [13]

CHIAPPORI, P. A. AND I. EKELAND (2006): "The micro economics of group behavior: General characterization," *Journal of Economic Theory*, 130, 1–26. [5]

DE ROCK, B., T. POTOMS, AND D. TOMMASI (2017): "Household Responses to Cash Transfers," mimeo. [6], [15]

DEATON, A. AND J. MUELLBAUER (1980): "An Almost Ideal Demand System," *The American Economic Review*, 70, 312–326. []

DITRAGLIA, F. AND C. GARCIA-JIMENO (2016): "On mis-measured binary regressors: new results and some comments on the literature," mimeo. [6]

DUFLO, E. (2003): "Grandmothers and Granddaughters: Old-age Pensions and Intrahousehold Allocation in South Africa," *The World Bank Economic Review*, 17, 1–25. [2]

DUNBAR, G. R., A. LEWBEL, AND K. PENDAKUR (2013): "Children's Resources in Collective Households: Identification, Estimation, and an Application to Child Poverty in Malawi," *American Economic Review*, 103, 438–71. [3], [6], [9], [11], [20], []

FISZBEIN, A. AND N. R. SCHADY (2009): *Conditional cash transfers: reducing present and future poverty*, World Bank Publications. [2]

GITTER, S. R. (2008): "Women's Power, Conditional Cash Transfers, and Schooling in Nicaragua," *World Bank Economic Review*, 22, 271–290. [20]

HADDAD, L. J., J. HODDINOTT, H. ALDERMAN, ET AL. (1997): *Intrahousehold Resource Allocation in Developing countries*, Johns Hopkins University Press. [2]

HANDA, S., R. PEREZ, AND B. STRAFFON (2001): "Poverty, inequality, and spillover in Mexico's education, health, and nutrition program," Final report, international food policy research institute, washington dc. [20]

HAUSHOFER, J. AND J. SHAPIRO (2016): "The Short-term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya*," *The Quarterly Journal of Economics*, 131, 1973–2042. [5]

HODDINOTT, J. AND L. HADDAD (1995): "Does Female Income Share Influence Household Expenditures? Evidence from Cote D'Ivoire," *Oxford Bulletin of Economics and Statistics*, 57, 77–96. [20]

HODDINOTT, J. AND E. SKOUFIAS (2004): "The Impact of PROGRESA on Food Consumption," *Economic Development and Cultural Change*, 53, 37 – 61. [7]

HU, Y., J.-L. SHIU, AND T. WOUTERSEN (2015): "Identification and estimation of single-index models with measurement error and endogeneity," *The Econometrics Journal*, 18, 347–362. [6]

IMBENS, G. W. AND J. D. ANGRIST (1994): "Identification and Estimation of Local Average Treatment Effects," *Econometrica*, 62, 467–475. [3], [6], []

KLEIN, M. AND B. BARHAM (2018): "Point Estimates of Household Bargaining Power Using Outside Options," University of wisconsin-madison, staff paper no. 590 march 2018. [6], [17], [27]

LEWBEL, A. (1998): "Semiparametric Latent Variable Model Estimation with Endogenous or Mismeasured Regressors," *Econometrica*, 66, 105–121. [6]

LEWBEL, A. AND K. PENDAKUR (2008): "Estimation of Collective Household Models with Engel Curves," *Journal of Econometrics*, 147, 350–358. [6]

MENON, M., K. PENDAKUR, AND F. PERALI (2012): "On the Expenditure-dependence of Children's Resource Shares," *Economics Letters*, 117, 739–742. [13]

MUELLBAUER, J. (1976): "Community preferences and the representative consumer," *Econometrica: Journal of the Econometric Society*, 979–999. [14]

PENGLASE, J. (2017): "Consumption Inequality among Children: Evidence from Child Fostering in Malawi," Boston college. [6], [9]

QUISUMBING, A. R. (1994): "Intergenerational transfers in Philippine rice villages," *Journal of Development Economics*, 43, 167 – 195. [20]

QUISUMBING, A. R. AND J. A. MALUCCIO (2003): "Resources at Marriage and Intrahousehold Allocation: Evidence from Bangladesh, Ethiopia, Indonesia, and South Africa*," *Oxford Bulletin of Economics and Statistics*, 65, 283–327. [2], [20]

REGGIO, I. (2011): "The influence of the mother's power on her child's labor in Mexico," *Journal of Development Economics*, 96, 95 – 105. [20]

RUBALCAVA, L., G. TERUEL, AND D. THOMAS (2009): "Investments, time preferences and public transfers paid to women," *Economic Development and cultural change*, 57, 507. [18]

SCHADY, N. AND J. ROSERO (2008): "Are cash transfers made to women spent like other sources of income?" *Economics Letters*, 101, 246 – 248. [5], [20]

SCHULTZ, T. P. (1990): "Testing the Neoclassical Model of Family Labor Supply and Fertility," *Journal of Human resources*, 599–634. [5], [20]

SKOUFIAS, E., B. DAVIS, AND S. DE LA VEGA (2001): "Targeting the Poor in Mexico: An Evaluation of the Selection of Households for PROGRESA," Final report, international food policy research institute, washington dc. [20]

SMITH, L. C. (2003): *The Importance of Women's Status for Child Nutrition in Developing Countries*, vol. 131, Intl Food Policy Res Inst. [2]

SOKULLU, S. AND C. VALENTE (2017): "Individual Consumption in Collective Households: Identification Using Panel Data with an Application to PROGRESA," mimeo. [6], [17], [18]

SONG, S. (2015): "Semiparametric estimation of models with conditional moment restrictions in the presence of nonclassical measurement errors," *Journal of Econometrics*, 185, 95 – 109. [6]

SONG, S., S. M. SCHENNACH, AND H. WHITE (2015): "Estimating nonseparable models with mismeasured endogenous variables," *Quantitative Economics*, 6, 749–794. [6]

THOMAS, D. (1990): "Intra-household Resource Allocation: An Inferential Approach," *Journal of human resources*, 635–664. [5], [20]

——— (1994): "Like Father, like Son; Like Mother, like Daughter: Parental Resources and Child Height," *The Journal of Human Resources*, 29, 950–988. [5]

——— (1997): "Incomes, Expenditures, and Health outcomes: Evidence on Intrahousehold Resource Allocation," *Intrahousehold Resource Allocation in Developing Countries*, 142–64. [5]

THOMAS, D., D. CONTRERAS, AND E. FRANKENBERG (2002): " Distribution of power within the household and child health," Rand, santa monica, california. [20]

TOMMASI, D. AND A. WOLF (2016): "Overcoming Weak Identification in the Estimation of Household Resource Shares," ECARES working paper 2016-12. [6], [20], []

——— (2018): "Estimating household resource shares: A shrinkage approach," *Economics Letters*, 163, 75 – 78. [17]

URA, T. (2016): "Heterogeneous Treatment Effects with Mismeasured Endogenous Treatment," Mimeo, Duke University. [6]

YANAGI, T. (2017): "Inference on Local Average Treatment Effects for Misclassified Treatment," mimeo. [6]

YOONG, J., L. RABINOVICH, AND S. DIEPEVEEN (2012): "The impact of economic resource transfers to women versus men," . [5]

Appendix of:
"Control of Resources, Bargaining Power and the Demand of Food: Evidence from PROGRESA"

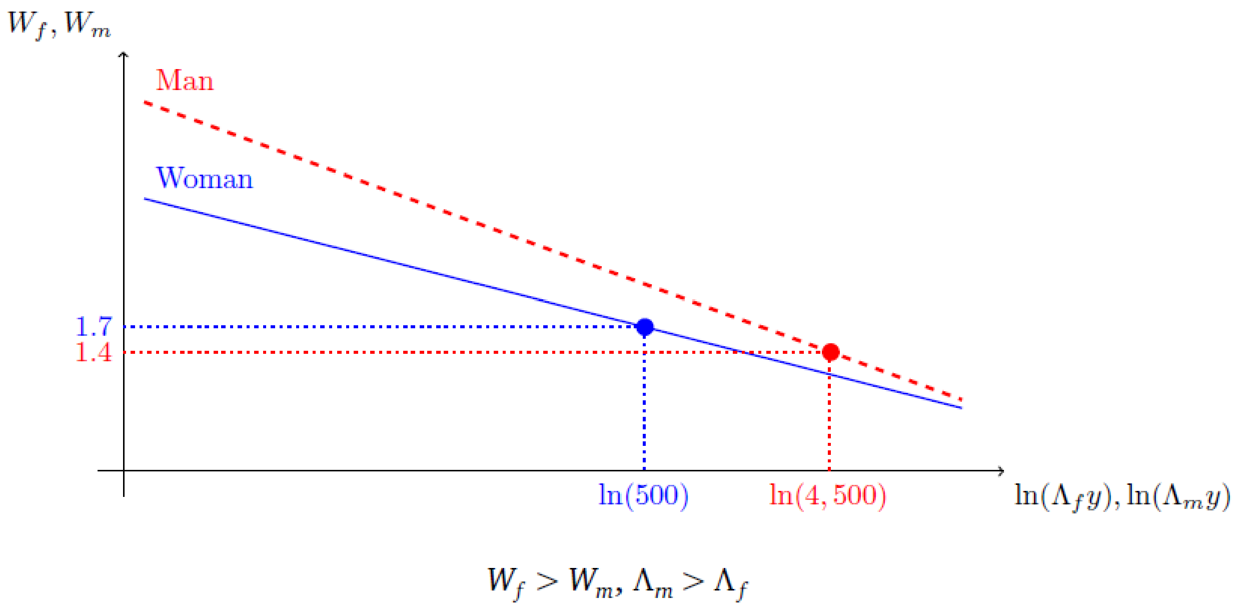
This Online Appendix contains five sections with further details and analysis. I preferred to leave this here to minimize the length of the manuscript. The information in this Appendix are organized as follows. Appendix A.1 uses additional data on singles to empirically test the Pareto efficiency assumption of the collective model framework. Appendix A.2 contains further tables and figures of results. Appendix A.3 presents a short discussion of QAIDS and AIDS demand system with some further estimation results obtained using our sample. Appendix A.4 provides the identification result(s) of MR-LATE. Appendix A.5 provides a graphical illustration of the MR-LATE estimator.

A.1 DLP: Identification and model assumptions

In this paper I estimate a collective model of the household to recover resource shares, under the assumption that the Engel curves for the private assignable goods are linear in lny and that resource shares are independent of y. However, these assumptions do not invoke restrictions on other goods' demand function. For the description of a fully specified collective household model that delivers linearity of Engel curves and resource shares that are independent of y, see the Online Appendix of the original Dunbar et al. (2013) paper. Alternatively, see also the Online Appendix of Calvi (2016), Calvi et al. (2017), or the description in the main text of Tommasi and Wolf (2016).

Identification: illustrative example For a graphical intuition of how resource shares are identified in DLP, consider the simple case of a household with no children, a total household expenditure equal to 5,000 Pesos and observable budget shares for female and male clothing equal to 1.7 and 1.4, respectively. This example is taken from Calvi (2016) as it is useful also for my exercise. Let the Engel curves for assignable clothing be as in Figure A1. The relationship between assignable clothing budget shares (W_f and W_m, on the vertical axis) and the logarithm of the total expenditure devoted to each type t household member ($\eta_t y$, on the horizontal axis) is linear under the functional form assumptions discussed above. The Engel curve displayed here are depicted for illustrative purpose only. By inverting these Engel curves, I can identify two points on the horizontal axis, equal to $\ln(500)$ (≈ 6.21) and $\ln(4,500)$ (≈ 8.41). These, together with the constraint that the resource shares must sum to one, make it possible to compute individuals' resources shares at any level of y. At a total household expenditure of 5,000 Pesos, $\eta_f = 0.1$ and $\eta_m = 0.9$. The graph depicts a situation where $W_m < W_f$ and $\eta_f < \eta_m$. In this specific numerical example, resources are split extremely unequally between the two household members, with the woman getting only 10 percent of the total household expenditure, whereas the budget share spent on female assignable clothing (W_f) is about 20 percent larger than the share spent on male clothing (W_m).

Figure A1: Engel curves for assignable clothing: an illustrative example



Test of model assumption The collective model of the household that I presented in Section 4, and that allows me to recover the unobserved treatment indicator used in Section 5, relies on the assumption that households Pareto efficient decisions regarding the consumption of goods. The test of this assumption in this context is equivalent to checking the validity of the Browning et al. (2013) (hereafter, BCL) structure of the household demand functions. BCL is a model of household demands, which are connected via the structural model to singles' demands. In my empirical application I do not need to impose the BCL assumptions regarding comparability of preferences between singles and couples. However, in order to test for Pareto efficiency here I need to make this assumption. Following Dunbar et al. (2013) and Calvi (2016), I use additional data on singles to provide validation of the model assumption.

The BCL framework can be summarized by the following system of demand equations:

$$\begin{aligned} W_t^{couple} &= \eta_t(\alpha_t + \beta_t ln\eta_t) + \eta_t\beta_t lny \\ W_t^{single} &= \alpha_t + b_t lny \end{aligned} \tag{A.1}$$

for $t = f, m$. The restrictions imposing similarity of Engel curves required for identification constrain are: $b_m = b_f = \beta_m = \beta_f$. These restrictions give rise to two testable implications. First, since η_t cannot be negative, the slopes of men's and women's private assignable have the same sign. Second, the slopes of household demands must be proportional to those of singles' demands, with factors of proportionality that sums to 1.

In order to test for the first implication, I compare the predicted slopes with respect to log-expenditure for men's and women's clothing obtained by estimating the model using 2,757 observations for nuclear households without children only. All of the predicted slopes in my sample are positive, both for women and for men. Moreover, the restriction that the slopes of men's and women's private assignable have the same sign is satisfied all the time. In order to test for the second implication, I combine the previous sample of nuclear households without children with a sample of 2,731 singles without children and estimate a linear regressions of the men's and women's clothing budget share on the log of total expenditure and all demographic variables (except those relating to other household members). I interact all regressors with a dummy for couples, so that all coefficients can differ between couples and single households and then combine the estimation results into one parameter vector. The ratios of slopes in couples versus single households are 0.7 for women and 0.5 for men, but their sum is not statistically different from 1 at the 10 percent level of significance.

A.2 Additional tables and figures

Table A.1: Preferred specification: full set of parameters

	η_m		η_f		β	
Variable	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
One kid	0.332***	0.040	0.363***	0.043	0.018	0.005
Two kids	0.303***	0.039	0.331***	0.043	0.003	0.002
Three kids	0.284***	0.039	0.330***	0.044	0.001	0.002
Treatment	0.026**	0.011	-0.036**	0.014	-0.004*	0.002
2nd wave	-0.017	0.016	-0.009	0.018	0.005**	0.002
3rd wave	-0.042**	0.016	-0.032	0.020	0.001	0.002
4th wave	0.027	0.017	0.005	0.018	0.004*	0.002
Kids' mean age	0.011*	0.002	0.005*	0.002	0.000	0.000
No. Of girls	0.009	0.013	0.023	0.015	-0.004	0.002
Age man	0.000	0.000	0.001	0.001	0.000	0.000
Education man	0.002	0.002	-0.003	0.002	0.000	0.000
Age woman	-0.001	0.001	0.001	0.001	0.000	0.000
Education woman	-0.004	0.002	-0.002	0.003	0.000	0.000
Hidalgo	-0.064***	0.023	0.039	0.025	-0.004	0.003
Michacan	-0.004	0.025	-0.022	0.027	-0.003	0.003
Puebla	-0.039*	0.022	0.025	0.024	0.006*	0.003
Queretaro	-0.008	0.038	-0.034	0.042	-0.005	0.005
San Luis Potosi	0.003	0.023	-0.010	0.025	0.005	0.003
Veracruz	-0.023	0.020	0.008	0.022	0.002	0.003

Notes: Robust standard errors, clustered at the village level, in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Figure A2: Independence of resource shares and total expenditure: Mother, Father, Each kid

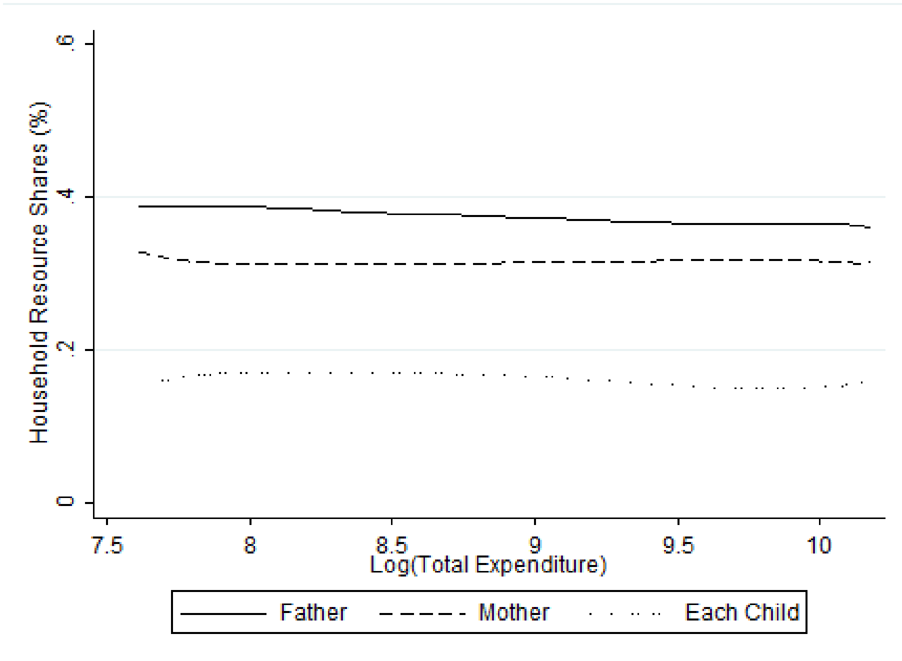


Table A.2: Self-reported measures of control and mother's control of resources

Control index (PCA)	Decision index (PCA)	Control index (PCA)	Decision index (PCA)
Panel A Mother's Resource share (R)		Panel B $T = I(R \geq 0.50)$	
1.261***	3.069***	0.228***	0.167
(0.341)	(1.097)	(0.075)	(0.150)

Notes: Mother's control and Mother's decision are two indices constructed by combining information on a set of self-reported indicators using principal component analysis. The former is constructed using two answers: whether the mother controls the household budget and whether the mother makes important expenditure decisions. The latter is constructed using nine answers about different smaller expenditure decisions, on schooling of the children and other measures of independence. All specifications include individuals and household controls. Bootstrap standard errors in parenthesis are clustered at the village level. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Figure A3: Resource shares and age profile in rural Mexico

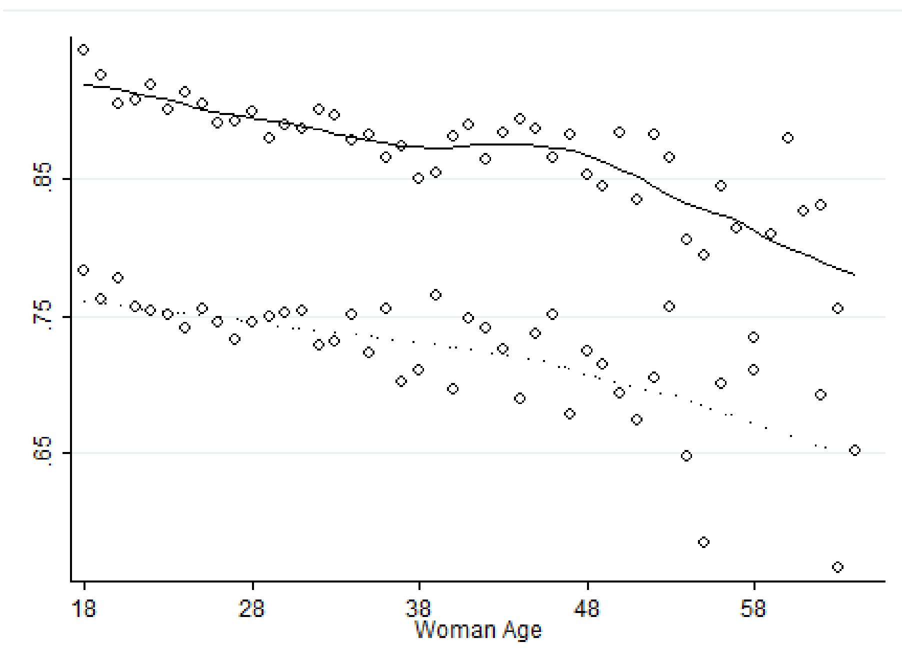
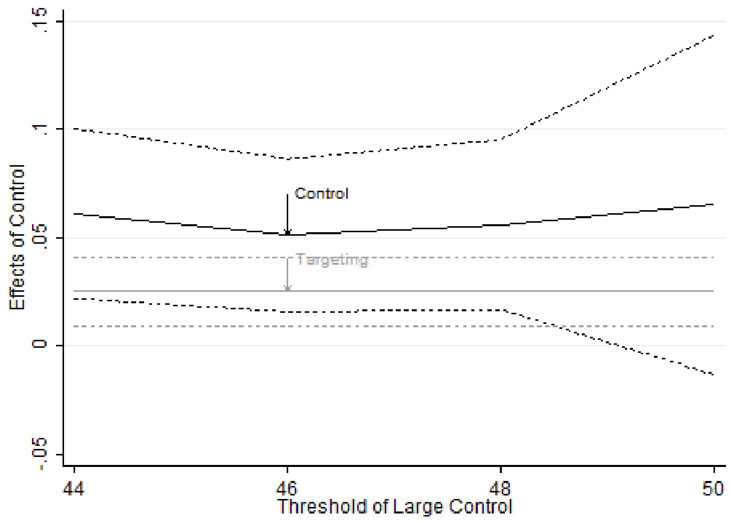
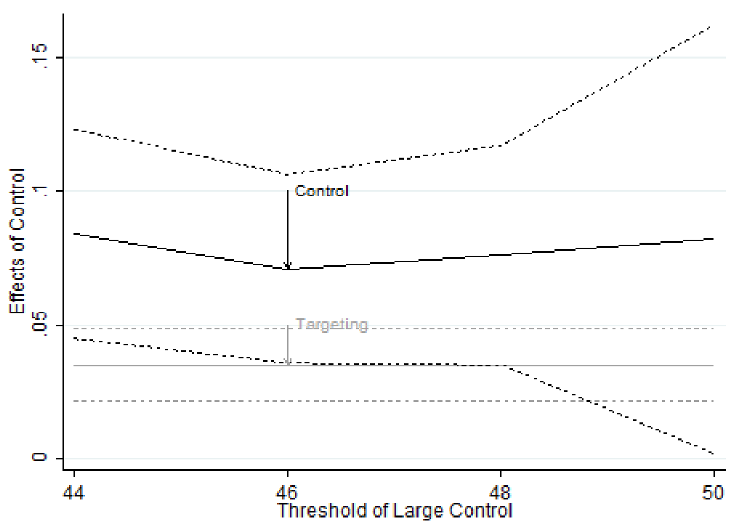


Figure A4: MR-LATE: Robustness

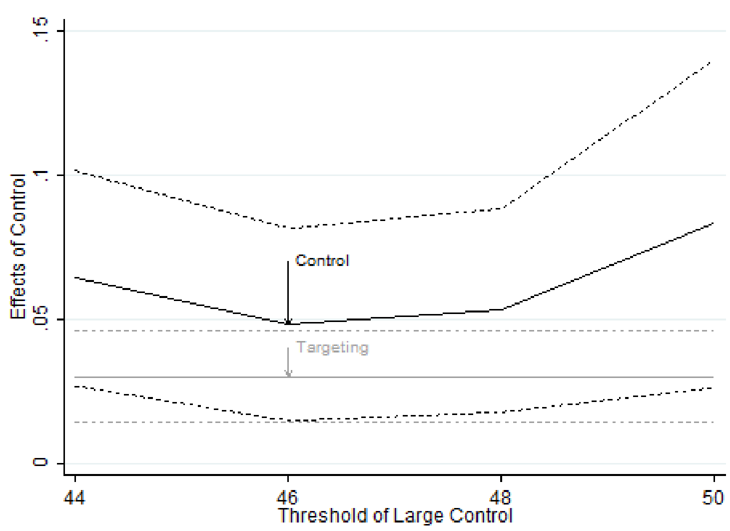
(A) 2 waves



(B) 3 waves



(C) 4 waves



A.3 Engel curves

Assume that households have preferences given by the integrable QAIDS demand system of Banks et al. (1997). QAIDS is quite popular in demand analysis because it allows flexible prices responses, the quadratic income allows the Engel curves to display a great variety of shapes, and at the same time the system of demand equations derived preserves theoretical consistency.

The indirect utility function of each household is assumed to be of the following form:

$$V = \left\{ \left[\frac{\ln x - \ln a(\mathbf{p})}{b(\mathbf{p})} \right]^{-1} + \lambda(\mathbf{p}) \right\}^{-1} \tag{A.2}$$

where

$$\begin{aligned} \ln a'(\mathbf{p}) &= \alpha_0 + \sum_{i=1}^{n} \alpha_i \ln p_i + \frac{1}{2} \sum_{i=1}^{n} \sum_{l=1}^{n} \gamma_{il} \ln p_i \ln p_l \\ b(\mathbf{p}) &= \prod_{i=1}^{n} p_i^{\beta_i} \\ \lambda(\mathbf{p}) &= \sum_{i=1}^{n} \lambda_i \ln p_i \end{aligned} \tag{A.3}$$

The parameters α_i, β_i, λ_i and γ_{il} $(\forall i, l)$ are to be estimated. Adding up requires that $\sum_i \alpha_i = 1$, $\sum_i \beta_i = 0$, $\sum_i \lambda_i = 0$ and $\sum_i \gamma_{il} = 0$ $(\forall l)$. Homogeneity is satisfied if $\sum_l \gamma_{il} = 0$ $(\forall i)$. Slutsky symmetry is satisfied if $\gamma_{il} = \gamma_{li}$ $(\forall i, l)$. Notice that the indirect utility function underlying Deaton and Muellbauer (1980) Almost Ideal Demand System corresponds to equation (A.2) where $\lambda_i = 0$ for all goods. Applying Roy's identity to equation (A.2) we obtain the QAIDS budget share equations for each household and commodity i $(i = 1, \ldots, n)$:

$$w_i = \alpha_i + \sum_{l=1}^{i} \gamma_{il} \ln p_l + \beta_i \ln \left\{ \frac{x}{a(\mathbf{p})} \right\} + \frac{\lambda_i}{b(\mathbf{p})} \left[\ln \left\{ \frac{x}{a(\mathbf{p})} \right\} \right]^2 \tag{A.4}$$

where w_i indicates the ith budget share of a household facing a price vector $\mathbf{p}$ and total expenditure level x, whereas α_i is a linear index containing a vector of demographic characteristics. Notice that in principle the vector of demographic variables could affect the demand system in other ways, not necessarily through the intercept only.

A.3.1 Engel curves of food: Benchmark results

In order to estimate Engel curves of food, one has to consider the following methodological issues: (i) How to control for price variation; (ii) Whether the relationship between budget and total expenditure is linear or quadratic; and (iii) How to control for endogeneity of total expenditure. We discuss them in turn.

Concerning how to control for relative price variation, since the program covers a large geographical area, it is possible that there are spatial and temporal differences in the relative price of food versus non-food items. The ideal scenario to control for this difference would be to have prices for both types of items. However this is not possible in my dataset for non-durable items different from food, because the quality of these data is not as good as that for food. Attanasio and Lechene (2010) and Attanasio et al. (2012) suggest to follow a pragmatic approach and to control for relative prices by using state-level and time dummies, and their interaction, under the assumption that relative prices are constant within a state at a point in time. This is what I do here as well.

The second issue is about the specification of the demand system. In the estimation of an Engel curve, the choice of whether the demand for a good is derived from an AIDS (Almost Ideal Demand System) or QAIDS (Quadratic-AIDS) is driven by how income responses vary with the level of income. Following Attanasio and Lechene (2010), the preferred specification for the Engel curve of food in this dataset is AIDS. Let r_j, $j = 1, \ldots, 28$, be the interaction between the 7 states and 4 time periods, the ("simplified") AIDS specification that I bring to the data is the following:

$$w_i = \alpha_i + \delta \text{PROGRESA}_i + \sum_{j=1}^{28} \gamma_j r_j + \beta \ln x + \epsilon_i \tag{A.5}$$

where w_i is the share of commodity i on total expenditure, x is total expenditure on goods, α_i is a linear index including all the demographic variables and ϵ_i is the error. δ and β are the coefficients of interest.

The third issue is how to account for the endogeneity of total expenditure. This is endogenous either because taste shocks that determine total expenditure may be correlated to the unobserved taste shifts for goods in the system, or because measurement errors in the budget shares may be correlated with measurement error on total expenditure. Attanasio and Lechene (2010, 2014) discuss a set of instruments to deal with this problem and argue why the average agricultural wage in a village is a good candidate to solve it. The implicit assumption is that any measurement error in household or village-level income will not be correlated with measurement error of household total expenditure. Notice that, since the dataset used for estimation comes from the evaluation of a cash transfer program, which has some important conditionality attached, a second source of endogeneity commonly considered is the number of children enrolled in school. Indeed, recall that in my sample eligible households receive a (large) portion of the grant only if their children are enrolled and attend school. This conditionality requirement, which is controlled in the demand equations, might affect consumption behavior if sending children to school imposes additional costs. Endogeneity arises from the fact that the unobserved taste for school may be correlated with unobserved taste for foods. Attanasio and Lechene (2010, 2014) point out that the concern is only for the number of children eligible to *secondary* school, as the enrolment in primary school is almost universal in Mexico (it is for our selected sample) and hence not affected by the grant. I deal with this second source of endogeneity by simply selecting a sample of households whose oldest children are not in secondary-school age (12 years and younger at baseline) and hence ineligible for this part of the grant. Hence school enrolment is not a concern for me.

Table A.3 reports the results using a control function approach (Blundell and Robin, 1999).[32] Standard errors are bootstrapped 200 times and clustered at the village level. The effects on food share in Columns (1)-(6), are estimated on, respectively, the first 2 waves (October 1998 and May 1999), 3 waves (adding November 1999), and the full sample (adding November 2000), instrumenting total expenditure with the average agricultural wage at village level (and its square). Columns (1), (3), (5), report the effects of PROGRESA for each subsample, whereas Columns (2), (4), (6), report the slopes of the Engel curves. By controlling for total expenditures, including the transfers, the parameter of interest δ captures the treatment effect (ITT) of the exogenous change of favoring mothers. Results are consistent with the estimates of Attanasio and Lechene (2010): the effect of targeting the cash to mothers is positive and significant, that is, households in treatment villages spend more money on food items, and the slope of the Engel curve is negative and significant as in standard demand analysis. Particularly the point estimates are numerically equivalent to these authors, both for the effect of treatment and for the slope of the demand curve. They constitute my benchmark results for Section 5.3.

Table A.3: Effects of targeting on demand for food

	2 waves		3 waves		4 waves	
	PROGRESA	ln(x)	PROGRESA	ln(x)	PROGRESA	ln(x)
	(1)	(2)	(3)	(4)	(5)	(6)
Benchmark	0.025***	-0.226**	0.035***	-0.253***	0.030***	-0.183***
	(0.008)	(0.095)	(0.007)	(0.078)	(0.008)	(0.061)
Controls	Yes		Yes		Yes	
Observations	4,719		6,697		8,982	

Notes: In all specifications I control for: dummies for number of kids, dummies for number of kids enrolled in school, mean age of the kids, share of girls in the household, age and education of head and spouse, whether the head can speak indigenous language, number of individuals eating in the household and outside the household, time and state dummies. I control for price variation by interacting time and state dummies. As for total expenditure, I follow the standard (AIDS) approach in Engel curve estimation by instrumenting with average agricultural wage in the village (and its square). Standard errors are bootstrapped 200 times and clustered at the primary sampling unit (village) level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

In order to further support the idea that the above simplification yields an appropriate representation of the observed budget shares, I present the following additional set of results. In Table A.4 I report the predicted budget shares for different sub-samples and compare it with the actual difference between treatment and control groups. As one can see, the estimation of the demand system fits very well the observed data.

[32] This is a convenient approach because it allows us to account for the non-linearity of the variable instrumented. Specifically, I generate third degree polynomial of the residuals from the first stage and add them to the main structural equation. The significance of first stage regression residuals in the demand equation indicates a strong rejection of exogeneity of total expenditure.

Table A.4: Actual and predicted impact of the program on budget structure

2 waves					
	Actual			Predicted	
Control	Treatment	Difference	Control	Treatment	Difference
0.748	0.755	0.007	0.748	0.756	0.008***
(0.004)	(0.003)	(0.005)	(0.002)	(0.001)	(0.002)
3 waves					
	Actual			Predicted	
Control	Treatment	Difference	Control	Treatment	Difference
0.736	0.747	0.011***	0.736	0.747	0.012***
(0.003)	(0.002)	(0.004)	(0.001)	(0.001)	(0.002)
4 waves					
	Actual			Predicted	
Control	Treatment	Difference	Control	Treatment	Difference
0.736	0.737	0.001	0.736	0.738	0.002
(0.003)	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)

Notes: The empirical model is estimated with AIDS for different sub-samples. The common controls in all specifications are: dummies for number of kids, dummies for number of kids enrolled in school, mean age of the kids, share of girls in the household, age and education of head and spouse, whether the head can speak indigenous language, number of people eating inside and outside the household, time and state dummies. We control for price variation by interacting time and state dummies. As for total expenditure, we follow the standard (AIDS) approach in Engel curve estimation: we use average agricultural wage in the village (and its square). Standard errors are bootstrapped 200 times and clustered at the primary sampling unit (village) level. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A.4 The effects of an unobserved treatment variable: Identification

Define the indicator function $\mathbb{I}(\cdot)$ to equal one if its argument is true, and zero otherwise. Suppose that the true treatment D is determined by a typical threshold crossing model:

$$D = \mathbb{I}(R^* \geq e)$$

for some threshold e, which may vary across observations for unobserved reasons. The crucial feature is that R^* (and hence D) is unobserved. Instead, I estimate the variable R, which is related to the true R^* as follows:

$$R = R^* + \varepsilon$$

where ε is an unknown disturbance due to specification, estimation or measurement error. Furthermore, let Z be a randomized binary instrument that is correlated with D. The random variables D_0 and D_1 denote the potential treatments $D_z = D(z)$ for possible realizations z of Z. Finally, let Y be the outcome variable and let random variables Y_0 and Y_1 be the potential outcomes $Y_d = Y(d)$ for possible realizations d of D such that: $Y = (1-D)Y_0 + DY_1$.

In the absence of D, the MR-LATE framework requires that R be used to construct *two* different proxies (or mismeasures) of D, which are called T^a and T^b. To do so, let κ^a and κ^b be two constants chosen by the researcher such that:

$$T^a = \mathbb{I}(R \geq e + \kappa^a) \quad \text{and} \quad T^b = \mathbb{I}\left(R < e - \kappa^b\right)$$

These two indicators of treatment status tell me that, if κ^a is large enough, then an individual with $T^a = 1$ is likely to be in the *true treatment* group ($D = 1$), and if κ^b is large enough, then an individual with $T^b = 1$ is likely to be in the *true control* group ($D = 0$). These likelihoods are defined (for compliers) by $p_d^a = E\left(T_d^a \mid C\right)$ and $p_d^b = E\left(T_d^b \mid C\right)$, where T_d^a and T_d^b are the potential mismeasured treatments associated with T^a and T^b.[33]

Let us summarize the identifying assumptions:

Assumption 1. *(Calvi-Lewbel-Tommasi, 2017) Y, D and Z, T^j, $j = \{a, b\}$, satisfy the following set of assumptions:*

i. $0 < E(D) < 1$, $0 < E(Z) < 1$ *and* $Z \perp (Y_1, Y_0, D_1, D_0)$.

ii. (Y_1, Y_0, D_1, D_0, Z) *are independent across individuals and have finite means.*

iii. There are no defiers, so $\Pr(D_0 = 1 \text{ and } D_1 = 0) = 0$.

iv. $Z \perp \left(T_1^j, T_0^j\right)$

v. $\left(T_1^j, T_0^j\right) \perp (Y_1, Y_0) \mid C$

[33] In other words, p_1^a is the probability that a complier would have their treatment correctly observed if they were assigned to the true treatment $D = 1$ and p_0^a is the probability that a complier is wrongly assigned to the treatment group. Whereas p_0^b is the probability that a complier would have their treatment correctly observed if they were assigned to the true treatment $D = 0$ and p_1^b is the probability that a complier is wrongly assigned to the control group. In Section A.5 of the Appendix I provide a graphical illustration of this construction.

vi. $E\left(T_1^j - T_0^j \mid C\right) \neq 0$

Assumptions (i)-(iii) are standard in the LATE (Imbens and Angrist, 1994) framework and say that if D was observed, then I could implement the standard LATE estimator. This is not feasible in my case. The remaining assumptions say that (iv) Z is as good as randomly assigned with respect to the potential proxies of treatment T_d^a and T_d^b, (v) the potential proxies of treatment are (for compliers) independent of the potential outcomes (Y_1, Y_0),[34] and (vi) T^a and T^b provide information regarding D (i.e. the correlation between D and T^j is nonzero).

Finally, define further the following objects of interest:

$$q^a = \frac{p_1^a}{p_1^a - p_0^a}, \quad q^b = \frac{p_1^b}{p_1^b - p_0^b},$$

$$\lambda^a = \frac{cov\left(T^a Y, Z\right)}{cov\left(T^a, Z\right)}, \; \lambda^b = \frac{cov\left(T^b Y, Z\right)}{cov\left(T^b, Z\right)}.$$

where q^a and q^b are given by the ratios of the (unobserved) probabilities defined earlier, whereas λ^a and λ^b are the two new mismeasured-robust potential outcomes for treatment and control groups, respectively. The MR-LATE estimator is defined by:

$$\text{MR-LATE} = \rho = \lambda^a - \lambda^b \tag{A.6}$$

Calvi, Lewbel, and Tommasi (2017) prove the following:

Theorem 1. *(Calvi-Lewbel-Tommasi, 2017) Let Assumption 1 hold with T^a and T^b. Then:*

1. *If ϵ is bounded, with known bounds, I can set κ^a and κ^b such that MR-LATE $=$ LATE.*
2. *If ϵ is unbounded: MR-LATE $= (q^a - q^b)$ LATE.*
3. *If κ^a and κ^b are set to zero: MR-LATE is numerically equivalent to LATE.*

I can summarize the theoretical results as follows. First, MR-LATE equals the true LATE if, among compliers, when $D = 0$ then $T^a = 0$, and when $D = 1$ then $T^b = 0$. A sufficient condition for MR-LATE to equal LATE is that ϵ is bounded and κ^a and κ^b are set to make $p_0^a = p_1^b = 0$. Second, if the error is unbounded, there is no optimal κ^a and κ^b and hence T^a and T^b. In this case, p_0^a and p_1^b will be close to zero, making MR-LATE close to (not equal to) the true LATE if T^a is rarely one when $D = 0$, and if T^b is rarely one when $D = 1$. In this scenario, the result in Theorem 1 can be used for set identification: If $q^a - q^b > 0$, MR-LATE signs LATE. If $q^a - q^b \geq 1$, LATE lies between 0 and MR-LATE. A sufficient condition for set identification is $p_1^a > p_0^a$ and $p_0^b > p_1^b$.[35] Finally, if I do

[34] Note that this assumption, combined with unconfoundedness, corresponds to the standard assumption that measurement errors are unrelated to outcomes. To put it in another way, it amounts to assume that specification, estimation and measurement errors are not endogenous in our regression model.

[35] In this scenario, I could guarantee that p_0^a and p_1^b are zero by taking κ^a and κ^b to be as large as possible. However, in this case, T_i^a and T_i^b would equal zero for almost every observation, and hence both p_0^a and p_1^a, and p_1^b and p_0^b, would be close to zero, which would bring to a violation of assumption (vi). This means that, in practice, I have a trade-off in the selection of κ^a and κ^b. The larger these are, the lower is the bias caused by misclassification, but also the less informative T^a and T^b become as indicators of treatment and control status (i.e. the lower is the correlation between T^a and D, and T^b and D).

not account for measurement error, then MR-LATE boils down to the standard LATE, regardless of whether LATE is biased or not. This means that, in a setting where the true treatment is unobserved, Calvi, Lewbel, and Tommasi (2017) show that MR-LATE can only do better than LATE in identifying the treatment effect.

A.5 MR-LATE: Illustrative example

I draw on Calvi, Lewbel, and Tommasi (2017) and show how point identification is achieved within the MR-LATE framework. Assume that $supp\,(\varepsilon) \subset \left[\kappa^b - e, \kappa^a - e\right)$. Then it follows that for $T = T^a$ we have $p_1^a = 1$ with $p_0^a = 0$, and for $T = T^b$ we have $p_1^b = 0$ and $p_0^b = 1$, and so $\lambda^a - \lambda^b = E\left[Y_1 - Y_0 \mid C\right]$. Given Theorem 1, LATE can be point identified. Figure A5 provides a graphical representation of this. If there was no measurement error, the true treatment and control groups would coincide with the respective observed groups. All individuals on the black line on the right hand side of e, would have a R^* larger than the threshold value; otherwise, they would be on the black line on the left hand side of e. One could construct a treatment proxy $T = \mathbb{I}(R \geq c)$, where R is an estimate of R^* and c is one's best guess of the midpoint between $\varepsilon + e$. This approach, however, will not identify the treatment effect of interest. To achieve point identification of LATE in presence of measurement error or misclassification error, I need to have two treatment indicators, T^a and T^b, such that $q^a = p_1^a / \left(p_1^a - p_0^a\right) = 1$ and $q^b = p_1^b / \left(p_1^b - p_0^b\right) = 0$. By knowing the bounds κ^a and κ^b, I am able to define a T^a such that for all individuals on the red line on the left hand side of κ^a, $p_0^a = 0$. That is, with probability 0, these individuals, who are observed in the control group, belong to the true treatment group. Analogously, I am able to define also a T^b such that for all individuals on the blue line on the right hand side of κ^b, $p_1^b = 0$. That is, with probability 0, these individuals, who are observed in the treatment group, belong to the true control group.

Figure A5: Illustrative Example