Control of Resources and Demand for Food

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Abstract

I combine structural and reduced-form identification techniques to estimate the effects of controlling individual resources on household demand for food. Exploiting random assignment of the Mexican PROGRESA program as an instrument for maternal control of resources, I show that mothers having majority control of household resources increase the share of consumption on food by 6.5-8.3 percent. I use these estimates to argue that, by knowing the distribution of pre-program resources inside the household, and hence how much influence each decision maker can have on the final 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, causality, LATE, structural model, collective model, resource shares, engel curves, food.

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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 the 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] Targeting the mothers induces a redistribution of resources that eventually affects household decisions. I argue that the current design is based on an implicit assumption which has not been verified in the data: that the targeting increases the maternal control of household resources.[2] Controlling household resources is unobserved in practice and hard to identify, but clearly it is the crucial determinant for the impact and effectiveness of these programs on desired outcomes.

The aim of this paper is to overcome this measurement issue and to estimate the direct effect of maternal control of resources on household demand for food, in the context of PROGRESA. This is a well-known conditional cash transfer (CCT) program that was implemented in rural Mexico in the late 1990s. The exogenous cash transfers in this program 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, 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. I focus on household demand for food because it accounts for roughly 74% of household budget and it is the primal determinant of welfare and some of the desired outcomes, such as better health and nutrition. This consists of estimating an Engel curve for food, which is a relationship between expenditure and household income, where the control of resources is the treatment variable of interest. Since this 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 ideal instrumental variable.

The analysis proceeds in three steps. I start by establishing two causal effects of interest that will guide my structural analysis: (i) The effect of targeting the cash transfers to mothers on self-reported indicators of controlling program resources, and (ii) The effect of targeting on the observed demand for food. I find that targeting is associated with a larger probability that the mother controls the additional resources, and a lower probability that it is the father doing it. Moreover, by controlling for total income, including the transfers, I find a positive Intention-To-Treat (ITT) effect of targeting on the share of resources spent on food. I use these estimates as my benchmark for the main analysis that follows.

[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 "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 her income and maximizes her 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 estimate structurally 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 own rational preferences, and use the assumption that the intra-household decision process, whatever it may turn out to be, produces Pareto-efficient outcomes. The collective model has been used to show that the control over the 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 mother, father, or children (e.g. clothing and footwear). I find evidence of a substantial increase of mother's control of resources, relative to the father, due to the policy, with a shift of the mean distribution by 5%, which is consistent with the reduced-form regression analysis. A simple back of the envelope calculation tells me 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 this by showing that, within the household, there is also a reduction in poverty rates for the weaker individuals, mother and children, relative to the father, thereby reducing within household inequality.

Third, I use the estimated resource shares to construct an indicator for whether the mother controls a majority of household resources. I then estimate the causal effect of maternal control of resources on household demand for food. Notice that, 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 et al. (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. According to my preferred specification, 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, with substantial differences in results with respect to the standard 2SLS for LATE (Imbens and Angrist, 1994), which cannot account for these errors leading to an overestimate of the effects.

The most important contribution of my paper is that, with these latter estimates, I establish a direct link between the eligibility to receive the conditional cash transfers, the actual control of

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

resources by the targeted individual, and the household demand for food. This is a crucial departure from the literature on cash transfers because, 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 transfer program on demand, I ask a different (and more general) question: What is the impact of having a large control of resources on household demand for food? Numerous papers rationalize the observed results of a design under the (implicit) assumption that, whoever is targeted by the program, s(he) is more likely to control these resources, which may not necessarily imply an increase in actual control. 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 use it to provide an estimate of its magnitude on the demand for food.

In terms of policy implications, I argue that my results can be used to show that the current design has a limitation which can be overcome to increase its cost-effectiveness. Indeed, since the 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 how much pre-program resources mothers control, how the actual control of resources is influenced by the assignment, and 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 estimator that allows to study the causal links between the actual 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 estimating resource shares; and (iii) The literature dealing with 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), which were based on related issues raised by Becker (1965, 1974, 1981) and Sen (1983, 1988, 1989). These seminal works have contributed to shape the way the first generation of cash transfer programs were implemented, giving (universally) 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 also 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 anyway. In order to understand what is driving the empirical result, one should look at the actual redistribution and control of household resources. Unfortunately this is 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]Specific to our context an data, 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, labelling of money.

household allocation decisions are now studied.[6] Interesting for me, 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), Bargain and Donni (2012).[7]

Among the descendents of the BCL model, Dunbar et al. (2013) (DLP hereafter) provide one of the most prominent model in the literature and it differs from the BCL-type approach as they identify resource shares using Engel curves of private assignable goods and 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 issues in the DLP model and use it to provide the first estimates of a cash transfer program (PROGRESA) on resource shares and poverty rates. This paper points 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. I minimize these issues of the DLP model in a different way. First, I pool all the available data which increases the sample size by four. This allows me to increase the variation in expenditure on the private assignable goods (i.e. to have steeper Engel curves) and to reduce the problem of stability. Second, I drop households with children eligible to secondary school. This reduces the problem of endogeneity present in the system, thereby creating a more homogeneous sample of households that behaves in a more stable manner. Third, as for the estimation, I rely on external information (reduced-form evidence) to explore more reliable portions of the parameter space for some of the crucial parameters in the system. Most importantly, the effect of the treatment. The end result is that the structural estimates in the present paper become stable and robust to several sensitivity checks and are able to match several stylized facts.

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, which could explain the different results that I obtain. 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

[6] Donni and Chiappori (2011) and Browning et al. (2014) provide a comprehensive review of the theoretical and empirical advances in this literature.

[7] A completely different approach is taken by Cherchye et al. (2011) and Cherchye et al. (2015), who provide set identification of the resource shares on the basis of revealed preference theory. These papers are more general than BCL-type approach, but are not yet standard in the literature.

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.[8]

Finally, I also relate to the literature on measurement or misclassified errors in observed treatment, as the main results in the paper are obtained by estimating a model where the treatment indicator comes from a structural model. Errors of such kind were first documented by Bollinger (1996), Angrist and Krueger (1999), Kane et al. (1999), Card (2001), Black et al. (2003), and Hernandez et al. (2007). Numerous papers consider alternative techniques to deal with the problem. Aigner (1973), Kane et al. (1999), Black et al. (2000) and Frazis and Loewenstein (2003) use instrumental variables to estimate homogeneous (constant coefficient) treatment effects of mismeasured binary regressors. Mahajan (2006), Lewbel (2007) and Hu (2008) extend the approach by estimating mismeasured treatment effects without assuming homogeneity of the treatment. Klepper (1988), Manski (1990), Bollinger (1996), Kreider and Pepper (2007), Molinari (2008), Imai et al. (2010), and Kreider et al. (2012), obtain bounds on average treatment effects with misclassified treatment under more general conditions.

In the present paper 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. 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.

Calvi et al. (2017) propose the most general (and trivial to implement) solution available thus far in the literature to recover LATE with binary mismeasured treatment indicator. They set-up an estimation problem that has 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 its features, it is the most suited methodology for the purposes of my paper.

[8]Notice that the possible misspecification of the (collective) model is taken into account by the estimation strategy adopted when recovering the effects of control on the demand for food.

3 The effects of targeting resources on the demand for food

I begin the empirical inquiry by exploiting the random assignment nature of the program to identify two causal effects of interest: The effects of targeting PROGRESA on (self-reported) indicators of controlling program resources and decisions on food expenditures, and the effects of targeting on (observed) demand for food. For the latter, I draw heavily on the recent literature estimating Engel curves and hence leave some details to the Appendix. I focus on the main results for my sample which will constitute a benchmark for the analysis of Section 5. The rest of this section is divided into two parts. First, I provide some background information on the program, details of my sample selection and some useful descriptive statistics. Second, I present briefly the specifications of the empirical models and describe the estimation results of the two main effects of interest.

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 afterwards. 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 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 was providing poor households with cash transfers conditional on the fulfilment of certain behaviours. 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 enrolment 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 for households with older children. The second set of conditions were related to health seeking behaviour. A further portion of the grant was conditional on women taking their young children to health centres and attending a number of courses organized by the program. Three aspects of the design are important. First of all, 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

[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.

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 our sample of rural poor families. On average, the household head is 32 years old and the spouse is 28. They have a bit more than 4 years of education and the vast majority of heads can speak Spanish (97%) and a large portion also an indigenous language (38%). In order to maximize sample size, I pooled also households observed in November 2000, which is when the control group becomes eligible to receive the grant. This choice is discussed and motivated in Section 4.4 when I present the results and robustness checks of the structural estimates. This yields a sample where 6,470 households (72%) reside in treatment villages, the rest belongs to the control 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. Mean age of children is slighly above 5 years old, the mean minimum age is almost 4, whereas 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 between the 4 waves. Under the label village characteristics, we have 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 control 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 control 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 the first challenge is to establish a causal relationship between targeting and actual control of resources, using the information available in the dataset as a starting point.

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 consider the following model:

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

where y_i is measured for each household i and can take three possible values: "mother", "father", "both". Treatment_i is an indicator variable equal to one if the household i lives in a treatment 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 β, which captures the treatment effect (ITT) of being exposed to the exogenous cash transfers.

Panel A of 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 treatment indicator is associated with a larger probability that the mother will control these additional resources and a lower probability that it will be the father doing it. 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 4.3 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

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

[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.

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				
Treatment	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.

of the effects of the treatment 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 tell me that, very likely, the treatment 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

Panel A: Self-reported control						
	Extra resources			Food expenditure		
	Mother	Father	Both	Mother	Father	Both
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	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
Panel B: Targeting and demand for food						
	Household income			Agricultural wage		
	2 waves	3 waves	4 waves	2 waves	3 waves	4 waves
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.020***	0.023***	0.023***	0.025***	0.035***	0.030***
	(0.006)	(0.005)	(0.006)	(0.008)	(0.007)	(0.008)
ln(x)	-0.113***	-0.090***	-0.080***	-0.226**	-0.253***	-0.183***
	(0.030)	(0.024)	(0.021)	(0.095)	(0.078)	(0.061)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,668	6,626	8,893	4,719	6,697	8,982
R-squared	0.201	0.199	0.187	0.205	0.202	0.189

Notes: The empirical model in Panel (A) is estimated on data from May 1999, whereas in Panel (B) we have data available for all four waves. In Panel (A) we 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 common controls in all specifications of Panel (A) and (B) 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. In Panel (A) we control for income level by using total expenditure deciles. In Panel (B), 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. In column (1)-(3) we use control function by instrumenting total expenditure with household income (and its square). In column (4)-(6) 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. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

3.3 Engel curves for food: Methodological issues and results

In this section I further explore decisions on the demand for food by looking at the value of purchased quantities. I am interested in modeling food share as a fraction of total non-durable expenditure. This task corresponds in practice to estimating an Engel curve, which is a relationship between total expenditure and the budget share, conditioning on preference shifters and relevant covariates. The Engel curve is derived under the assumption that households are maximizing a utility function subject to a budget constraint. These relationships have been estimated by Attanasio and Lechene (2010, 2014) and Attanasio et al. (2013) in the context of the Mexican PROGRESA. In this section, I review the main methodological issues that have been raised by these authors, which will be useful also for Section 5, where I estimate the effects of actual control (and not targeting) of resources on the demand for food. The main issues to consider are three: (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.

The PROGRESA data contain very detailed information on food. However, since it is not feasible to model the demand for several dozens of food items, I aggregate the data to construct the share of all food items over total non-durable expenditure. The main determinants are the price of food, the prices of other non-durables, and total expenditure on non-durables. 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, which is not possible in this dataset for other 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.

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. The AIDS was introduced by Deaton and Muellbauer (1980) and generalized to QAIDS by Banks et al. (1997). The latter allows for more flexible relationship between income and budget share. In a QAIDS, the budget shares take the following form:

$$w_i = \alpha_i + \beta \text{Treatment}_i + \sum_{l=1}^{j} \gamma_{il} \ln p_l + \eta_i \ln \left\{ \frac{x}{a(\mathbf{p})} \right\} + \frac{\lambda_i}{b(\mathbf{p})} \left[\ln \left\{ \frac{x}{a(\mathbf{p})} \right\} \right]^2 \quad (2)$$

where w_i is the share of commodity i on total expenditure, x is total expenditure on goods, $a(\mathbf{p})$ ad $b(\mathbf{p})$ are price indexes and α_i is a linear index of demographic variables. The system reduces to an AIDS when $\lambda_i = 0$. Since (2) is a fairly standard equation to estimate, I leave some details to the Online Appendix A.1. Two details are worth pointing out here. First, following Attanasio and Lechene (2010), the preferred specification for the Engel curve of food in this dataset is AIDS. Second, to control for price differences, I allow the intercept to shift by state, time, and their interaction. I follow both approaches here and in Section 5. Let r_i, $i = 1, \ldots, 28$, be the interaction between the 7 states and 4 time periods, the AIDS specification that I bring to the data is the following:

$$w_i = \alpha_i + \beta \text{Treatment}_i + \sum_{j=1}^{28} \gamma_j r_j + \eta \ln x + \epsilon_i \quad (3)$$

where α_i is a linear index including all the demographic variables as before and ϵ_i is the error. β and η are the coefficients of interest.

Total expenditure 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 issue and argue why household income and the average agricultural wage in a village are good instruments. 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. I report below the results I obtain with both set of instruments, separately.[13]

Panel B of Table 2 reports the results using a control function approach (Blundell and Robin, 1999).[14] Standard errors are bootstrapped 200 times and clustered at the village level. The effects on food share in Columns (1)-(3) 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 household income (and its square). Columns (4)-(6) are estimated in the same way using the average agricultural wage at village level (and its square) as instruments for total expenditure. By controlling for total expenditures, including the transfers, the parameter of interest β captures the treatment effect (ITT) of the exogenous change of favouring 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 of Columns (4)-(6) are (almost) numerically equivalent to these authors, both for the effect of treatment and for the slope of the demand curve. They will constitute my preferred specifications and benchmark result for Section 5.

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 Engel curve identification issue. In the last two subsections I outline the

[13]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.

[14]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.

estimation strategy and present the results of the structural estimates.

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 children (c). Households differ according to a set of observable characteristics, such as number of 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 $\mathbf{x}_t$.[15] 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:

$$\tilde{U}(U_m, U_f, U_c, p/y) = \sum \mu_t(p/y)\tilde{U}_t \tag{4}$$

where the Pareto weights $\mu_t(p/y)$ depend on prices, individual characteristics and household expenditure. The form of the household's utility function (4) 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 (Becker, 1991). The household's program reads:

$$\begin{aligned} \max_{\mathbf{x}_m, \mathbf{x}_f, \mathbf{x}_c, \mathbf{z}} \; & \tilde{U}(U_m, U_f, U_c, p/y) \\ \text{s.t. } \; & \mathbf{z} = A(\mathbf{x}_m + \mathbf{x}_f + \mathbf{x}_c) \\ & y = z'\mathbf{p} \end{aligned} \tag{5}$$

[15]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.

The solution to program (5) yields the quantity of private good equivalents, $\mathbf{x}_t$, for each member t. 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 (5) 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:

$$z^k = A^k\big(h_m^k(A'\mathbf{p}, \eta_m y) + h_f^k(A'\mathbf{p}, \eta_f y) + h_c^k(A'\mathbf{p}, \eta_c y)\big) \tag{6}$$

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 since they 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.[16] Furthermore, DLP make the restriction that η_t does not depend on household expenditure y (at low level 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)).[17] Given this strategy, the household demand functions (6) 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 (6) 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

[16]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.

[17]See the online Appendix of DLP for further discussion on this restriction.

given by:

$$W_t = \frac{z_t}{y} = \eta_t \cdot w_t(\eta_t y) \tag{7}$$

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 (7), 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 (7) becomes linear in the logarithm of own expenditure and the system takes the following form:

$$\begin{aligned} W_m &= \alpha_m \eta_m + \beta_m \eta_m ln(\eta_m y) \\ W_f &= \alpha_f \eta_f + \beta_f \eta_f ln(\eta_f y) \\ W_f &= \alpha_c \eta_c + \beta_c \eta_c ln(\eta_c y) \end{aligned} \tag{8}$$

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. In particular, provided that $\beta_m = \beta_f = \beta_c = \beta$, DLP show that the system is identified. This is the preference restriction that the authors call "Similar Across People" (SAP).[18]

Before concluding this section, two final remarks are in order. First, the Pareto weights μ_t in (4) 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, since I employ resource shares in my analysis, I may interpret them both as measures of control of resources and of bargaining powers, 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

[18]Notice that the authors also consider an alternative semi-parametric restriction called "Similar Across Types" (SAT), in which the slope parameter is unaffected by the number of children in the household. This is only a slight departure from the more standard idea of imposing equality across household's configurations and also turns out to have less bite than SAP as the authors themselves acknowledge. This is also the reason for which I put emphasis on SAP and conduct our empirical analysis using this restriction rather than the other one.

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 Online Appendix A.2 I provide a simple example to show this intuition.

4.3 Estimation strategy and poverty indexes

I estimate the system of equations (8) by appending an error term to each equation and by imposing the similarity of preferences (SAP) assumption over private assignable goods: $\beta_m = \beta_f = \beta_c = \beta$. This is the system that I take to the data:

$$\begin{cases} W_m & = \alpha_m \eta_m + \beta \eta_m ln(\eta_m y) + \epsilon_m \\ W_f & = \alpha_f \eta_f + \beta \eta_f ln(\eta_f y) + \epsilon_f \\ W_c & = \alpha_c \eta_c + \beta \eta_c ln(\eta_c y) + \epsilon_c \end{cases} \tag{9}$$

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.[19] Standard errors are clustered at the village level.

The model is taken to the same pooled data as in Section 3.1. The dataset is suitable to estimate system (9) 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 person. 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 dummy indicating the treatment status, meaning 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). This means that I estimate a total of 194 parameters in the preferred specification.

After estimating the DLP model, I use the parameters to construct poverty rates that take into account the inequality of resource allocation within the household. These are different from the standard poverty measures because the latter assume equal sharing of resources within the household. This is particularly interesting to estimate in this context, because the surveys have been

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

collected to evaluate the impact of a welfare program whose objective was, among other things, to fight poverty among marginalized households. Hence, in this way I am able to quantify 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.

One final detail regarding estimation is worth discussing. In principle I could use the average agricultural wage at village level to construct optimal instruments in a GMM framework to account for the endogeneity of total expenditure. However, this estimation procedure causes our system to be more unstable and estimates are less reliable. Hence, in the main results that I present here, I decided not to apply instrumental variables to address this potential source of endogeneity. Notice that this is not a severe concern for two reasons. First, by estimating the model on different subsets of the sample along the distribution of total expenditure, I provide some indirect evidence that the potential endogeneity problem does not change the main qualitative results of our application. More details are reported in the robustness checks of the next subsection. Second, the primary purpose of the exercise is to obtain a strong correlation and the potential misspecification of the structural model is anyhow taken care of by the estimator employed in Section 5.

4.4 Estimation results and welfare analysis of PROGRESA

Table 3 reports the estimated coefficients of the main covariates for the resource shares of mother (η_m) and father (η_f).[20] These are some of the possible determinants of the resource share allocation process and can be related to bargaining power, although one shall not consider these necessarily as causal parameters. The main qualitative results are robust to several specifications and sample restrictions which are discussed later. Column (A) reports the estimation results of the preferred specification with dummies for each kid and with all households and waves included. Column (B) reports the results of the same specification but leaving out the last wave (November 2000), when the control group also becomes eligible to receive cash transfers. Column (C) and (D) report the results considering either richer or poorer households for robustness checks, as will be explained later. Finally, column (E) considers the same sample as in specification (A) but here the number of kids enters linearly in all the indices.

Results are threefold. First, the most interesting variable for our purposes is the treatment dummy. In all specifications, the effect is positive for the mother and negative for the father, and

[20] The full set of estimated coefficients of the preferred specification (A) are reported in Section A.4 of the Online Appendix. Table A.2 there reports the adults' resource shares and the slope coefficient β. The rest of the coefficients, also for the other specifications (B)-(E), are available upon request. Parameters' estimates are the result of a grid search of starting values. As a rule of thumb, I set the starting values of the main coefficients for the dummies of the number of kids at conventional values, and for the treatment effect by using the sign of the reduced-form estimates on control of resources. I then select the specifications with the resulting highest log-likelihood.

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

	(A) Preferred		(B) No last wave		(C) 50% richer		(D) 50% poorer		(E) Linear in kids	
Main variables	Mother (1)	Father (2)	Mother (3)	Father (4)	Mother (5)	Father (6)	Mother (7)	Father (8)	Mother (9)	Father (10)
One kid	0.332***	0.363***	0.313***	0.355***	0.278***	0.313***	0.450***	0.359***		
	(0.041)	(0.043)	(0.042)	(0.047)	(0.071)	(0.083)	(0.077)	(0.074)		
Two kids	0.303***	0.331***	0.294***	0.299***	0.261***	0.306***	0.434***	0.292***		
	(0.039)	(0.043)	(0.042)	(0.047)	(0.071)	(0.084)	(0.077)	(0.072)		
Three kids	0.284***	0.330***	0.286***	0.284***	0.249***	0.301***	0.422***	0.299***		
	(0.039)	(0.044)	(0.043)	(0.048)	(0.071)	(0.084)	(0.077)	(0.073)		
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***	0.037**	-0.024	0.039**	-0.070***	0.015*	-0.029**
	(0.011)	(0.014)	(0.010)	(0.013)	(0.020)	(0.026)	(0.019)	(0.020)	0.012	0.015
2nd wave	-0.017	-0.009	-0.010	-0.006	-0.025	0.009	0.009	-0.019	-0.001	-0.013
	(0.016)	(0.018)	(0.014)	(0.016)	(0.026)	(0.032)	(0.023)	(0.024)	0.018	0.020
3rd wave	-0.042***	-0.032**	-0.032***	-0.040***	-0.026	-0.027	-0.043**	-0.034*	-0.020	-0.018
	(0.016)	(0.020)	(0.014)	(0.018)	(0.026)	(0.032)	(0.025)	(0.027)	0.018	0.021
4th wave	0.027*	0.005			0.053**	0.007	0.058**	-0.020	0.028*	0.013
	(0.017)	(0.018)			(0.026)	(0.031)	(0.027)	(0.029)	0.018	0.021
Rest of the controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,017	9,017	6,720	6,720	4,529	4,529	4,489	4,489	9,017	9,017
No. Parameters	194	194	188	188	194	194	194	194	174	174

Notes: Main parameters' estimates of the resource shares for mother and father. The rest of the controls include: kids' 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. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

always significant. Moreover, the effect on the father is (with the exception of the specification in column (C)) always larger in magnitude, which implies that resources are redistributed from the father to *both* mother and children, although the mother is the one benefiting the most. 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. In Panels (A)-(C) of Figure 1, I represent the empirical distribution of $\hat{\eta}_m$, $\hat{\eta}_f$ and $\hat{\eta}_c$ for households in both treatment and control villages. The descriptive statistics are reported in Panel (A) of Table A.1 in the Appendix. Here I can see also the change in poverty rates for all individuals, which are also affected by the redistribution of resources. Indeed, during the first year of cash transfers, the percentage of poor mothers goes down from 93.5 percent to 90.1 and of poor children from 93.4 to 90.7. 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's inequality. Second, as the number of kids increases, both adults reduce their shares roughly by the same amount, depending on the specification. For instance, looking at the preferred specification in column (A), by the third child the mother has reduced her shares by 5 percent on average, whereas the father by 3 percent. This simply tells 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 wave dummies are always negative for both adults, indicating that children gain resources over time into the program. Although most of these parameters are imprecisely estimated and sometimes very small, by the 3rd wave there is a sizeable increase of resources for children.[21]

Compliance with stylized facts. My results match a number of stylized facts present in the literature. First of all, I confirm some of the findings by Adato et al. (2000) and the reduced-form estimates in Panel A in Table 2: the program is positively associated with mothers controlling the extra resources, and negatively associated with fathers controlling them. The effect sizes are meaningful and sizeable. This is also in line with the intuition of Attanasio and Lechene (2002, 2010, 2014) and Rubalcava et al. (2009) according to whom, through PROGRESA, mothers increase the control of resources relative to fathers. Second, I also confirm findings by Skoufias et al. (2001) and Handa et al. (2001) in terms of reducing short term household poverty and inequality. I complement these results by showing that, within the household, there is a further reduction in inequality between richer (father) and poorer (mother and children) individuals.[22] Third, like in other developing countries, the father is the individual controlling the relative majority of resources in Mexico. I also find a similar declining pattern of mother's resources at older ages as documented specifically by Calvi (2016) for India. In Figure A1 in the Appendix I plot mothers' resources relative to fathers' on the y-axis, and mother's age on the x-axis, divided by treatment status. As one can see, a young mother living in a treatment village is controlling roughly 90% of her spouse's resources. As age increases, the resources are progressively reduced to 77%.

In Panel D of Figure 1 I plot the distribution of the resources controlled by the mother relative to the father, computed as $R_i = \frac{\hat{\eta}_{i,m}}{\hat{\eta}_{i,m}+\hat{\eta}_{i,f}}$.[23] The mother is estimated to control 47 and 42 percent of household resources in treatment and control villages, respectively. As will be explained in the next section, R is the measure that I use to build my estimated treatment indicator. Since this is an unconventional measure of control of resources (or women's empowerment), 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. Panel (A) of Figure 2 displays the results of a non-parametric regression of mothers' reported control of resources on our estimated resource share R. Whereas Panel (B) shows the non-parametric relationship between an index of mothers' participation in household decisions on our estimated resource share 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 where I estimate, based on expenditures, that they have substantial control over resources. Thus, these results corroborate

[21] This can be seen in Panel B of Table A.1 in the Appendix. Here I compute the resource shares and poverty rates for all household members, disaggregated by wave of observation. I do not look at the last period of observation, November 2000, because here also households in control villages become eligible and hence there is no control group to compare and it is also difficult to make a comparison with the trend of the previous year of observation. I can see that, as time goes by, there is a clear negative trend for child poverty rates in treatment villages compared to control ones, going from a difference of 1 percent in the first wave, to 6 percent by the third wave.

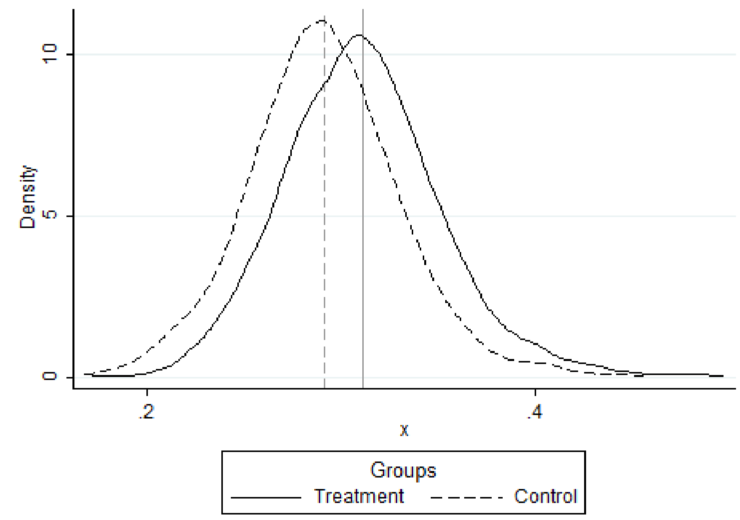
[22] First such estimates on individual poverty rates for PROGRESA are also present in an earlier version of Tommasi and Wolf (2016). Here I provide more stable results combining all the available (early) waves of the evaluation surveys.

[23] The descriptive statistics are in Table A.2 of the Appendix.

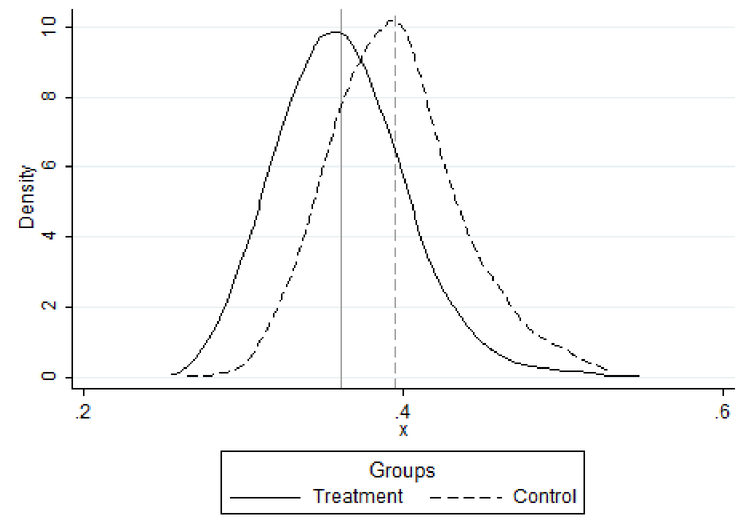
[24] The estimation results are reported in Panel A of Table A.3 of the Online Appendix.

Figure 1: Distribution of resource shares: treatment and control groups

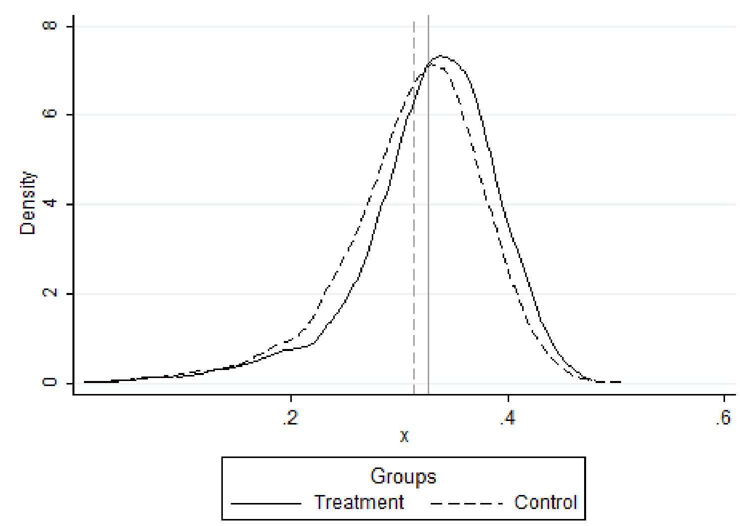
(A) Resources of the Mother



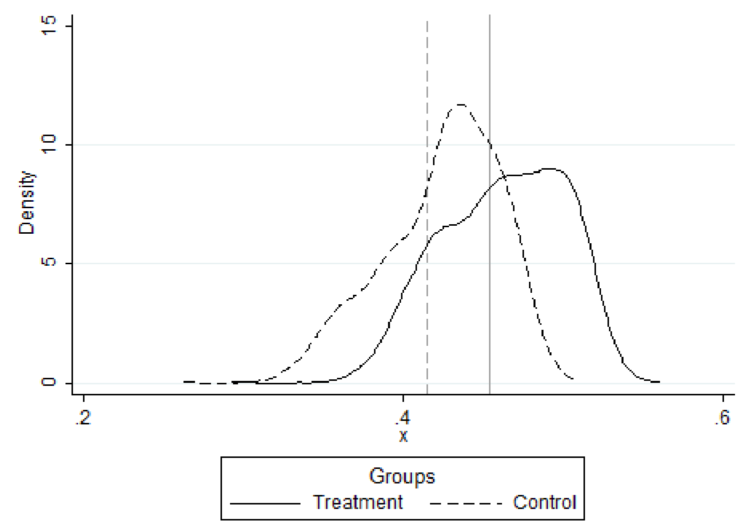
(B) Resources of the Father



(C) Resources of the Children



(D) Mother's relative control

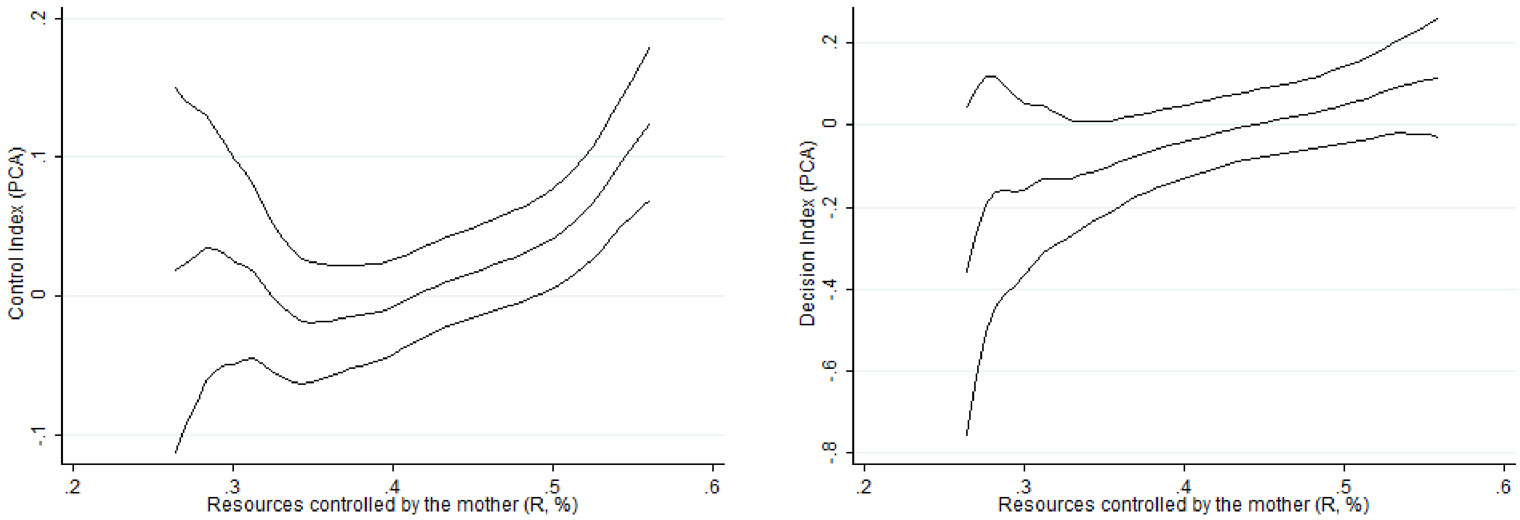


Notes: These figures provide information on the distribution of resources, between treatment and control households, for Mother ($\hat{\eta}_m$), Father ($\hat{\eta}_f$) and Children ($\hat{\eta}_c$). Moreover, subfigure (D) provides information on the distribution of Mother's relative control of resources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

the theory underlying our structural model: the larger is R, the higher are her self-reported decision making and bargaining powers within the household.

Figure 2: Alternative measures of bargaining power

(A) Mother's control, Index (PCA) (B) Mother's decision, Index (PCA)



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.

Robustness checks. My structural estimates are robust to several sensitivity checks which are reported in Table 3. First of all, 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. I still prefer the former sample selection for efficiency (and speed of convergence of the estimator). Estimates are reported in column (B) of Table 3. Second, since in the main results I do not instrument for total expenditure, I provide some evidence that under certain assumptions this may not be a concern for me. If I assume that measurement error is a severe problem in my dataset and it is varying with the level of expenditure, by estimating system (9) on different subsets of the sample based on this observable characteristic, I should be able to pick up a visible change in parameter estimates.[25] In column (C) and (D) I report the estimates for, respectively, the 50% richer and 50% poorer households only. As one can see, although there is some variation in magnitude (also due to the fact that the two samples are different), the sign of the main parameters do not change and hence the main qualitative story is robust. That is, the treatment effect is positive for the

[25]This simple check was motivated by a recent work of Gibson et al. (2015). By randomizing different consumption surveys, they show that errors in measured consumption are negatively correlated with true values ("mean-reverting measurement error"). These correlations are intuitive because survey reporting tasks are harder for richer households with more varied consumption, and imply that estimates for this subset of the sample are more likely to be biased.

mother and negative for the father. Note that these results are also robust to different sub-sampling still based on total expenditure, as long as we maintain a similar sample size. I still call for caution and wish to interpret these estimates as strong correlation, rather than causal effects. Third, the DLP system is a rather complex model to bring to the data. Indeed, different specifications may lead to instability of the results. An example of different specification that may cause instability is the specification of the resource share index with the number of kids either entering as dummy variables or linearly. In our specific case, this choice does not lead to any instability in the estimates. This is a powerful robustness check and the results are reported in column (E). Fourth, 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.

Finally, I also check for the internal validity of my estimates and the model assumptions. In the estimation of DLP, Tommasi and Wolf (2016) 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. However, by pooling all waves of PROGRESA, households in my dataset display enough variation in the consumption of individual clothes and footwear, which facilitates identification and lower the estimation and inferential issues raised in that paper (see Figure A3 in the Appendix). As for the internal validity of the estimates, 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. I also use data on singles to test for the validity of the Pareto efficiency assumption. Results are reported in Section A.2 of the Online Appendix.

5 The effects of controlling resources on the demand for food

The aim of this section is to estimate the effects of controlling resources in the household on the demand for food. The primary challenge associated with this task is to construct the treatment variable, since in practice it is unobserved. I accomplish this by combining the measures of resource shares estimated in the previous section. This is a new approach since most of the applications in the literature derive this variable from self-reported measures of control and decision power within the household (e.g. see Reggio (2011) for an application on the Mexican population). Although popular, these measures may be quite crude, imprecise, and often focused on very specific topics. The advantage of my approach is to construct a variable of control that is derived from a theoretical model and hence motivated by economic theory.

Using a treatment variable that is estimated, or derived, from a structural model poses a new challenge, in that I need 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 anyhow estimation and measurement errors. I approach this by using a new estimator recently introduced by Calvi et al. (2017) (Mismeasured

Robust LATE or MR-LATE), that allows to recover treatment effects when a (binary) treatment variable is misspecified, misclassified, or estimated with error. The section is divided in two parts. First, I briefly set up the identification problem of the MR-LATE estimator. I introduce some notation that complies with the MR-LATE framework and that defines the main elements for my empirical analysis. Second, I outline the estimation strategy and present the main results of the paper.

5.1 Mismeasured-Robust LATE: 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 assumption 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 to use R 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.[26]

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* $Z \perp (Y_1, Y_0, D_1, D_0)$.

[26]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.3 of the Online Appendix I provide a graphical illustration of this construction.

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

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

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

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

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 also 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),[27] 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:

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

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

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:

$$\text{MR-LATE} = \rho = \lambda^a - \lambda^b \qquad (10)$$

Calvi et al. (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 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: MR-LATE signs the true LATE when $0 < q^a - q^b$, and MR-LATE

[27] Note that this assumption, combined with unconfoundedness, this 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.

bounds the true LATE when $0 < q^a - q^b \leq 1$. Finally, if I do 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, MR-LATE can only do better than LATE in identifying the treatment effect.

5.2 Estimation strategy and results

I now fit my research question within the MR-LATE framework. The goal is to estimate how the demand for food changes if a household is exposed to a mother *controlling* a large fraction of household resources. I wish to estimate this treatment effect even though the true underlying value of the mother's share of resources controlled, R^*, is unobserved. 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. To perform this task, I employ the MR-LATE estimator using the same sample as in Section 3.3 in a standard Engel curve framework. In light of the identification result that I presented in the previous section, there are four issues that need to be discussed to understand the empirical results that follow.

First, I do not know what is the true relationship between the continuous variable R^* and Y. 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. I assume that the discrete value

$$D = \mathbb{I}(R^* \geq 0.50) \tag{11}$$

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. The implicit assumption is that there is a discontinuity in the decision making process when the mother controls more than 50 percent of household resources. 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)). Studying the effect of D on Y 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 counterfactual where she was given only a smaller fraction? Even though a discrete treatment may be criticizable, notice that there are at least two advantages in considering D. First, for this setting, I have an econometric tool that allows us 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 estimation bias caused by a misclassification of treated and control individuals is likely to be the most detrimental one.[28] Hence, by using MR-LATE on a binary (mismeasured) treatment indicator,

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

I am able to account for the largest source of bias in this setting.

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 4.4. For each household i:

$$R_i = \frac{\hat{\eta}_{i,m}}{\hat{\eta}_{i,m} + \hat{\eta}_{i,f}} \tag{12}$$

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 4 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 more resources than those in the control group. Importantly, my estimate R of the true R^* refers to resources controlled by the mothers, not necessarily those consumed by them. Indeed, recalling the results documented in Section 4.4, there is a clear positive relationship between our estimated R and some common self-reported measures of decision power and control of resources. Mothers are more likely to self-report having a control of resources in households that have $T = 1$ vs $T = 0$, even after conditioning on individual and household level controls. Results for the binary treatment are reported in Panel B of Table A.3 of the Online Appendix.

Table 4: 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 as fraction.

Third, my analysis 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, where I can use MR-LATE to set identify (sign) the LATE. 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). In the absence of an optimal strategy in the literature for the choice of these constants, I use the following algorithm. Let $\mathscr{K}$ be the percentage of individuals assumed to be misclassified in our sample, and let κ^a be the value such that $\mathscr{K}/2$ percentage of the sample has R in the interval $[50, \kappa^a]$ and κ^b be the value such that $\mathscr{K}/2$ percentage of the sample has R in the interval $[\kappa^b, 50]$.[29] I consider five percentages: $\mathscr{K} = \{0, 5, 10, 15, 20\}$. For each element of $\mathscr{K}$ 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 our dataset.

Finally, since both mismeasures of true treatment are endogenous, I use the targeting of PROGRESA as 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.

Main results Table 5 shows the results obtained with a linear specification in the log of total expenditure (AIDS) for three different sub-samples as before. Panel (A) reports the results of the preferred (ITT) specification of Section 3.3, which is our benchmark. The label CF refers to the fact that this was estimated using control function approach under the assumption of endogeneity of total expenditure. Panel (B) reports the new results of the effects of controlling resources on the demand for food under the assumption that $D = 1(R^* \geq 0.50)$ is the relevant treatment. In all specifications, I always control for the same set of covariates as I used for the standard Engel curve estimation of Section 3.3 (see notes below Table 2). I also instrument total expenditure in the same way using average agricultural wage at village level (and its square). Standard errors are always bootstrapped 200 times and clustered at village level. I provide the results for three set 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 require, 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 excluded instru-

[29] 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.

ment for my proxy of D, still under the assumption that there is no measurement error and hence no misclassification of the treatment indicator.[30] These are the standard 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). In the third line, I report the results of my preferred specification, which accounts also for misspecification, 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 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, I am able to account for 5 to 10 percent of possible misclassified individuals in our sample, as reported at the bottom of the Table. As one can see, the estimated parameters of the treatment indicator is 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.[31] Results say 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. The slope of the demand curve is still close in magnitude with respect to the specification that does not account for measurement error.

Robustness checks I provide three robustness checks to support the results. First, since R is measured with 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.[32] 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 we have obtained, in Figure 3 I provide a graphical illustration showing that the results obtained are robust to different choices of cut-offs around my preferred value. 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

[30]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.

[31]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 there are more true treated individuals than true control individuals in the mismeasured treatment group, and there are more true control individuals in the mismeasured control group than true control individuals in the mismeasured treatment group.

[32]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.

Table 5: MR-LATE: Effects of the control of the resources

	Panel A: Targeting					
	2 waves		3 waves		4 waves	
	Treatment	ln(x)	Treatment	ln(x)	Treatment	ln(x)
	(1)	(2)	(3)	(4)	(5)	(6)
CF	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)
D endogenous	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 the preferred specification in Column (4)-(6) of Panel (B) in Table 2. 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 we 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. 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 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. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

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. 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. Second, the definition of R in (12) 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 qualitative similar to my preferred specification. Results are not reported but available upon request. Finally, as we can see in Table 5, the results are also consistent across samples of estimation.

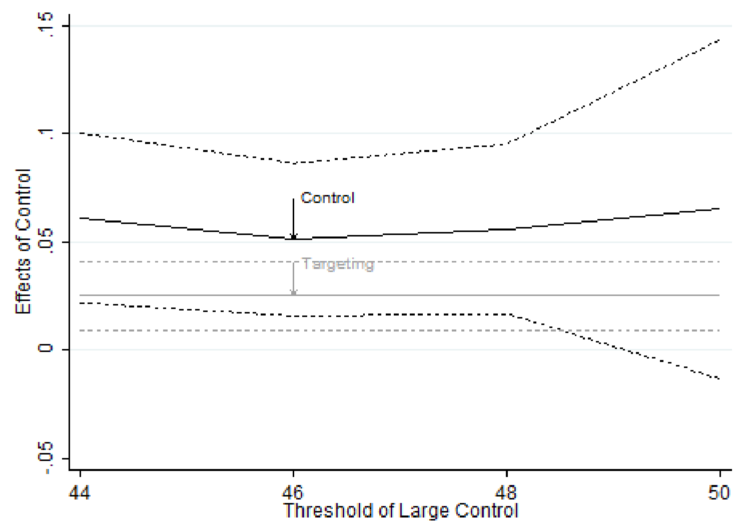
What do I learn? First of all, by taking a ratio of the ITT over MR-LATE estimates for our preferred specification in Table 5, I can obtain a rough estimate of how many compliers I have in my sample. Roughly 36 percent of observations are compliers, that is, households where the mother was controlling a minority of resources and were moved by the policy to control the majority of resources. This number is of course imprecise because MR-LATE is only an imperfect estimate of the LATE and because there is yet no theoretical result that relates 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. Moreover, if I take my estimates at face value, I can use the estimated Engel curves to predict the impact of mothers controlling household resources on the demand for food, at least for compliers. The formula that predicts the change on food share for compliers induced by the control of resources, under the assumption that there is no effect on prices, is:

$$w_{D=1} - w_{D=0} = 0.083 - 0.158 \times \left(\ln(\mathrm{x})_{D=1} - \ln(\mathrm{x})_{D=0}\right) \tag{13}$$

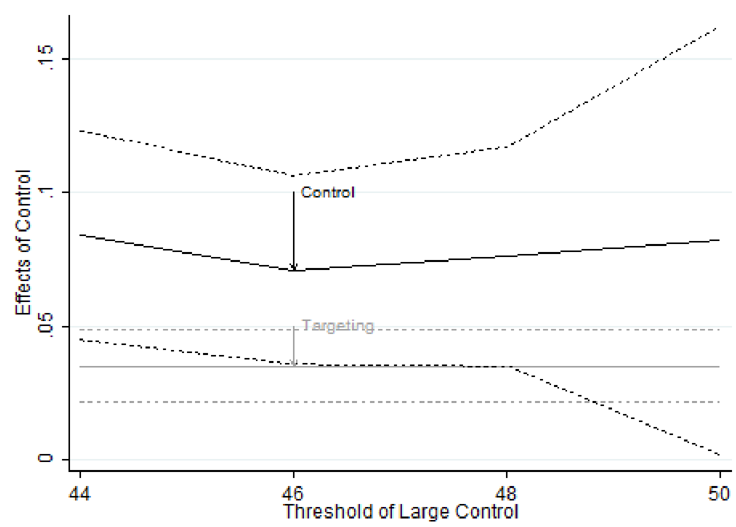
which can be used for policy exercises and simulations, using the newly defined treatment indicator, to predict the effect for compliers.

Figure 3: MR-LATE: Robustness

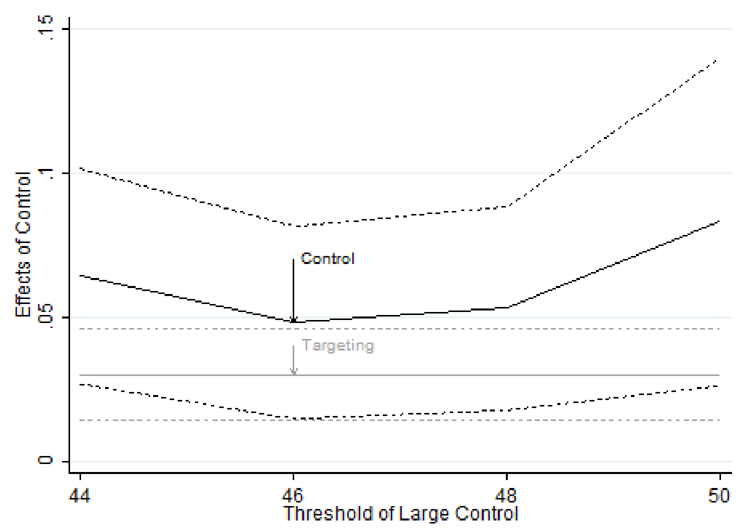
(A) 2 waves



(B) 3 waves



(C) 4 waves



6 Conclusion

In the present paper, I propose a novel approach to study the effect of mother's control of resources on household demand for food in Mexico, using the eligibility to PROGRESA as random assignment. My treatment indicator is binary and accounts for whether a mother controls the relative majority of household resources. 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.

My analysis is based on a two-step strategy. In the first step, I use a structural model to estimate the amount of resources controlled by each decision maker. The model predicts that mothers eligible to receive the grant increase their resources relative to the father, which is consistent with the reduced-form evidence. In the second step, 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 et al. (2017) (MR-LATE) to study the causal effect of interest.

Further research should focus on two extensions. First, 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 of interest Y, 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. Second, 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 unfold the reason why in certain contexts, like Burkina Faso or Morocco (see Akresh et al. (2016) and Benhassine et al. (2015), respectively), we obtain the same effects of the treatment by randomizing the gender of the recipient of cash transfers.

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Appendix

Additional tables

Table A.1: Predicted resource shares and poverty rates: Preferred specification

	Control					Treatment				
	Mean	**SD**	**Min**	**Max**	**Poor (%)**	**Mean**	**SD**	**Min**	**Max**	**Poor (%)**
	Panel A: Average over the entire period of observation									
Mother	0.291	0.039	0.169	0.457	0.935	0.311	0.041	0.172	0.498	0.901
Father	0.395	0.041	0.264	0.531	0.820	0.361	0.041	0.254	0.547	0.839
Children	0.314	0.064	0.032	0.506	0.934	0.328	0.065	0.011	0.479	0.907
	Panel B: Disaggregated by wave of observation									
October 1998										
Mother	0.306	0.038	0.205	0.457	0.919	0.326	0.039	0.230	0.498	0.899
Father	0.405	0.041	0.286	0.530	0.794	0.372	0.040	0.273	0.547	0.839
Children	0.290	0.062	0.032	0.463	0.947	0.302	0.062	0.011	0.429	0.938
May 1999										
Mother	0.292	0.039	0.169	0.450	0.935	0.312	0.039	0.172	0.479	0.902
Father	0.400	0.039	0.286	0.531	0.826	0.365	0.039	0.272	0.529	0.838
Children	0.308	0.060	0.060	0.459	0.937	0.323	0.059	0.064	0.447	0.917
November 1999										
Mother	0.273	0.035	0.171	0.396	0.955	0.292	0.037	0.195	0.463	0.902
Father	0.376	0.038	0.264	0.524	0.846	0.343	0.038	0.254	0.515	0.841
Children	0.351	0.055	0.080	0.506	0.913	0.365	0.057	0.099	0.479	0.857

Notes: Main parameters' estimates of the resource shares for mother and father. The rest of the controls include: kids' mean age, share of girls, age of mother and father, education of mother and father, 7 state dummies. Robust standard errors, clustered at the village level, in parentheses.

Table A.2: Estimated resource shares and Mother's control: treatment and control villages

	Obs.	Mean	SD	Min	Max
Full sample:					
Mother's Resource Share (*R*)	9,010	0.46	0.04	0.26	0.57
Household eligible to PROGRESA:					
Mother's Resource Share (*R*)	2,546	0.42	0.04	0.26	0.51
Household non-eligible to PROGRESA:					
Mother's Resource Share (*R*)	6,464	0.47	0.04	0.29	0.57

Notes: Household level data for all waves combined. *R* as fraction.

Additional figures

Figure A1: Resource shares and age profile in rural Mexico

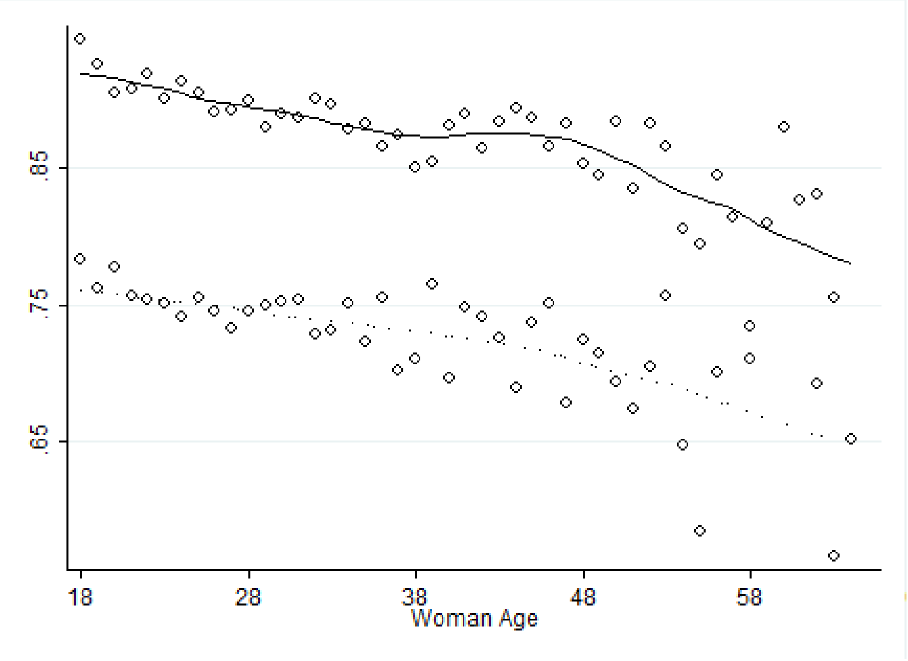


Figure A2: Independence of resource shares and total expenditure: Mother, Father, Each kid

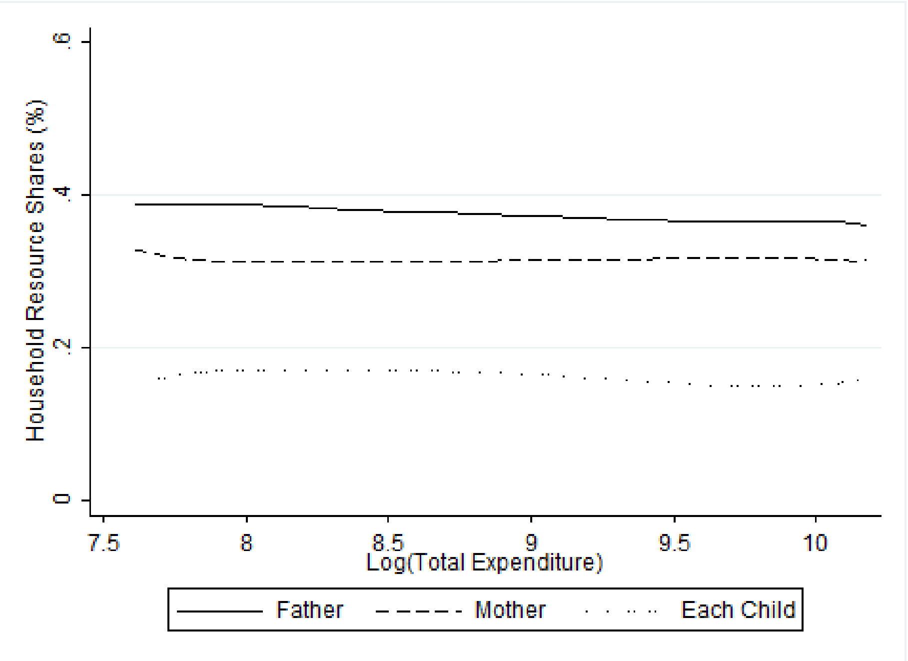
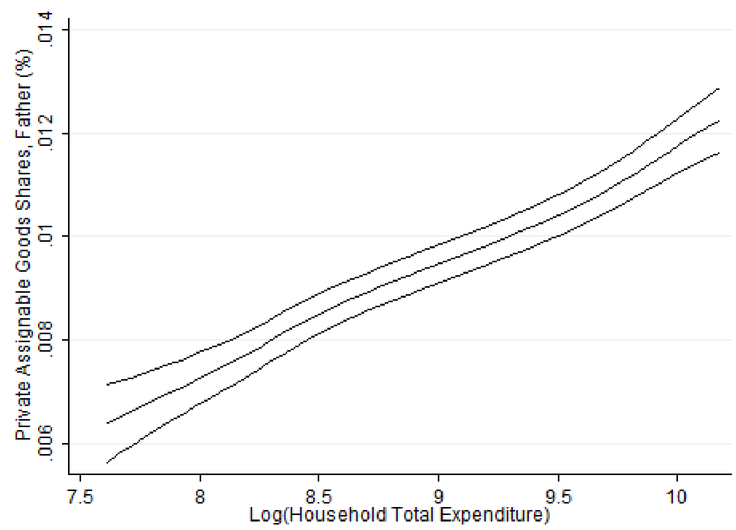
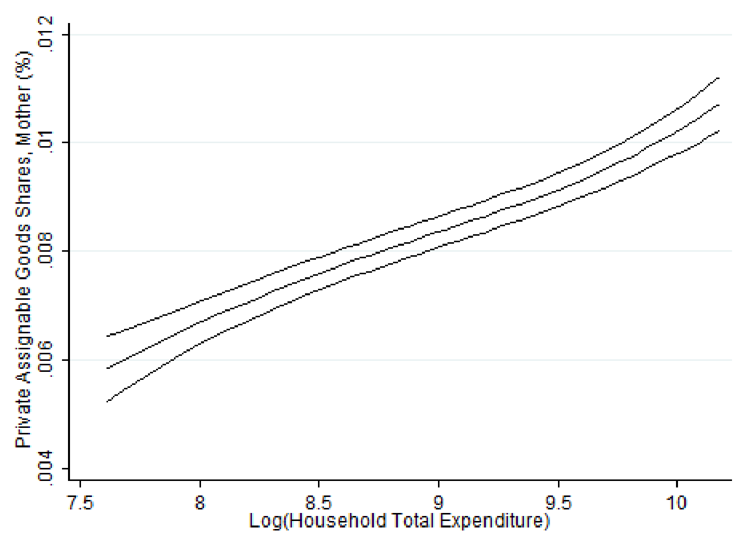


Figure A3: Slope of the Engel Curves: Man, Woman, Children

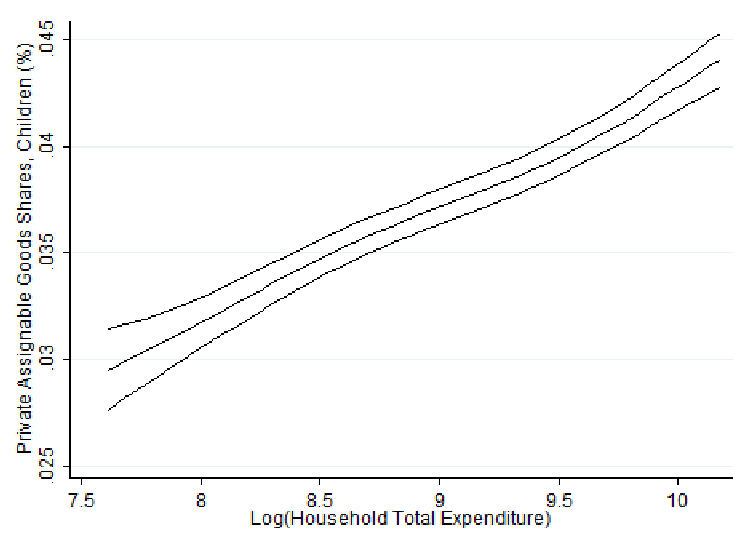
(A) Men Assignable's Clothing



(B) Women Assignable's Clothing



(C) Children Assignable's Clothing



Online Appendix of:
“Control of Resources and Demand for Food"

This Online Appendix contains four 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 presents a short discussion of QAIDS and AIDS demand system with some further estimation results obtained using our sample. Appendix A.2 uses additional data on singles to empirically test the Pareto efficiency assumption of the collective model framework. Appendix A.3 provides a graphical illustration of the MR-LATE estimator. Appendix A.4 contains further tables of results.

A.1 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:

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

where

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

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.1) where $\lambda_i = 0$ for all goods. Applying Roy's identity to equation (A.1) we obtain the QAIDS budget share equations for each household and commodity i $(i = 1, \ldots, n)$:

$$w_i = \alpha_i + \sum_{l=1}^{i} \gamma_{il} \ln p_l + \beta_i \ln \left\{ \frac{x}{a(\mathbf{p})} \right\} + \frac{\lambda_i}{b(\mathbf{p})} \left[\ln \left\{ \frac{x}{a(\mathbf{p})} \right\} \right]^2 \tag{A.3}$$

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.

For my specific task and dataset, Attanasio and Lechene (2010) show that AIDS is the appropriate specification to estimate the demand for food, which amounts to assuming $\lambda_i = 0$ for all goods, and appending an error term to equation (A.3). The demand system is estimated with control function approach instrumenting total expenditure x with the average agricultural wage at village level (and its square). I estimate this equation with a crucial simplification. As explained in the main text, I do not have information on prices for other non-durable items different from food, because the quality of these data is not as high as that for food. Hence, in my context, I can only estimate an approximation of equation (A.3). 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.

Table A.1: 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. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

In order to show that this simplification, and overall my estimate of the demand system, yields an appropriate representation of the observed budget shares, I present the following additional set of results. In Table A.1 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.

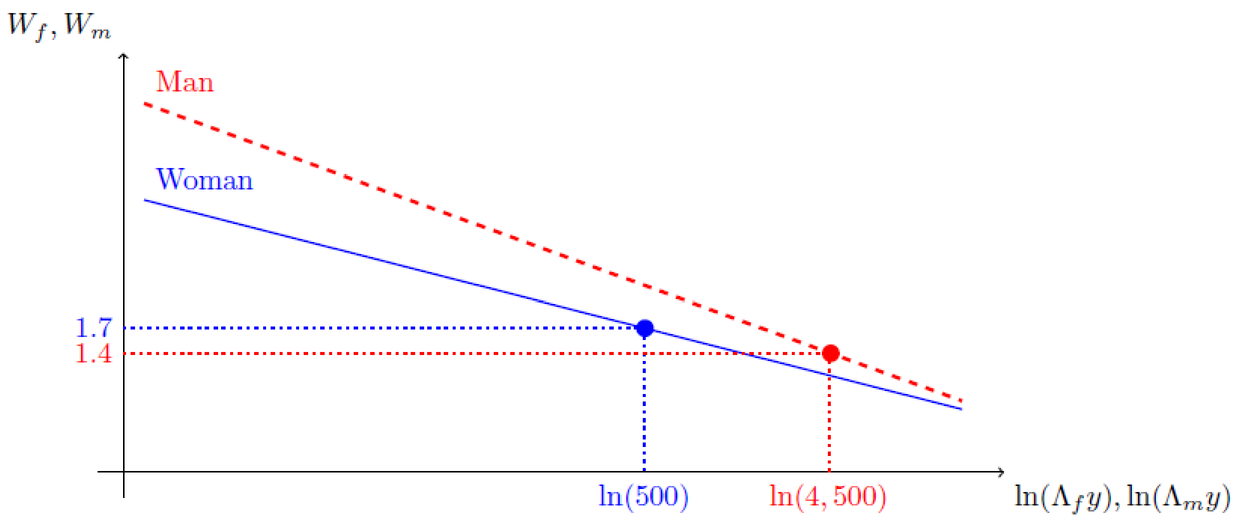
A.2 DLP: Identification and model assumption

In this paper we 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 A4. 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).

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

Figure A4: Engel curves for assignable clothing: an illustrative example



$W_f > W_m$, $\Lambda_m > \Lambda_f$

to provide validation of the model assumption.

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

$$\begin{aligned} W_t^{couple} &= \eta_t(\alpha_t + \beta_t ln\eta_t) + \eta_t\beta_t lny \\ W_t^{single} &= \alpha_t + b_t lny \end{aligned} \tag{A.4}$$

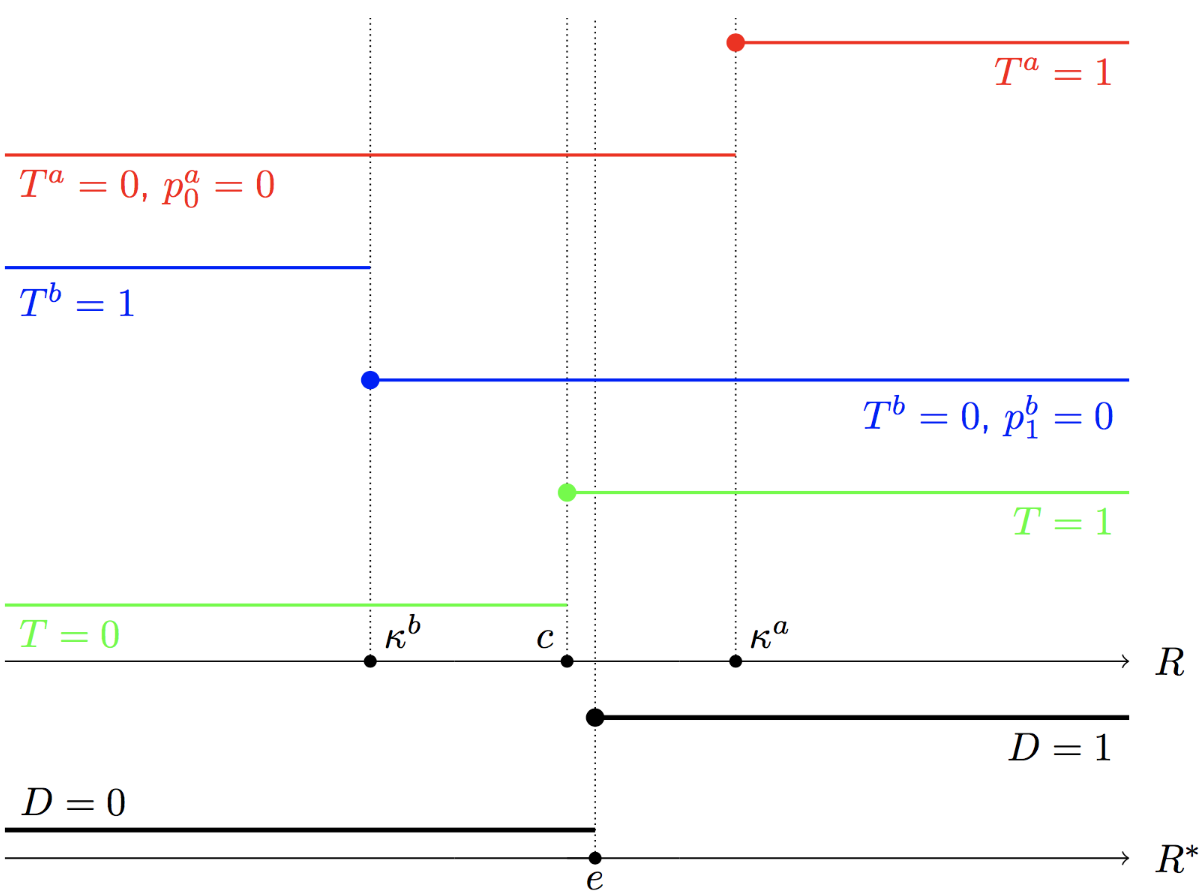
for $t = f, m$. The restrictions imposing similarity of Engel curves required for identification constrain are: $b_m = b_f = \beta_m = \beta_f$. These restrictions give rise to two testable implications. First, since η_t cannot be negative, the slopes of men's and women's private assignable have the 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.3 MR-LATE: Illustrative example

I draw on Calvi et al. (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 $\lambda^a - \lambda^b = E[Y_1 - Y_0 \mid C]$. 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



A.4 Additional tables

Table A.2: 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. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A.3: 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. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.