The Effect of Gender-Targeted Transfers: Experimental Evidence From India

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DISCUSSION PAPER

Institutt for samfunnsøkonomi
Department of Economics

SAM DP 16/2020

ISSN: 0804-6824
September 2020

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Abstract

Women are the primary recipients of many welfare programs around the world. Despite frequent claims that targeting women induces beneficial consumption shifting and gender equality, the empirical evidence on the effect of targeting is relatively scarce. We report on a highly powered intervention that randomly allocates weekly transfers to a man or woman within the household. We use detailed financial diaries to look at the impact of the recipient's gender on expenditure, income, saving, nutrition and measures of decision-making. Our results show little evidence for consumption shifting at the household level but indicate that targeted transfers empower female recipients.

JEL Classification: D13, I14, O10

Keywords: Households, Consumption, Development, Gender Inequality

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Acknowledgements
We acknowledge the financial support of The Research Council of Norway (225871, 262675 and 250415). We are grateful to Orazio Attanasio, Jean-Marie Baland, Konrad Burchardi, Jonathan de Quidt, Johannes Haushofer, Stephan Klasen, Karl Ove Moene, Rohini Somanathan, Jakob Svensson and Bertil Tungodden for helpful discussions and input. We are also grateful to Hanfeng Chen, Rahul Mehrotra, Sanjay Prasad and Julia Seiermann for excellent research assistance and to Basix Sub-K's staff for their continued support and collaboration.

1. Introduction

Cash transfer schemes and welfare programs around the world to a large extent target women as recipients. Examples are many of the universal child support programs in high income countries (Bradshaw and Finch, 2002) and the conditional cash transfer programs in lower income countries (see e.g. Fiszbein et al., 2009). There are two main arguments for transferring money to women instead of men (see e.g. Banerjee and Duflo, 2019; Duflo, 2012; Jayachandran, 2015). First, the *child welfare* argument holds that giving to women induces consumption shifting that is beneficial for children. Second, the *empowerment* argument holds that such transfers contribute to gender equalization. However, the large amounts of money that target women based on these arguments stand in stark contrast to the relatively scarce empirical evidence on how targeted transfers affect allocation of resources and empowerment.

We present such evidence from a highly powered intervention in rural Chhattisgarh, a Central-Eastern State of India. We randomly allocate weekly unconditional transfers to a man or a woman within the household, and study the impact on household finances using detailed information from weekly financial diaries that cover a wide range of topics. To the best of our knowledge, ours is the first highly powered study of the effect of gender targeted unconditional transfers on the composition of expenditure, income, saving, nutrition and measures of decision-making within the household.

When analyzing decisions at the household level, we do not find evidence for consumption shifting: expenditure, saving, income and the nutritional content of food purchases are similar for households with a male and a female recipient. This holds for average effects; for the main composition of each of these categories, and along all the dimensions for which we test heterogeneous impacts, such as the expenditure level at baseline. We do, however, find evidence for gender equalization: female recipients have a larger say in a wide range of domains than the spouses of male recipients, and when we look at goods that we can identify as gender-specific within the main categories, we observe larger spending on female specific goods such as hair oil, lotion, perfumes and saris. As such, our results indicate that the most important argument for targeted transfers is female empowerment, rather than the potential effects on overall consumption and saving choices (see also Banerjee and Duflo, 2019).

Although the existing empirical evidence is relatively scarce, our paper certainly builds on important and influential studies. Correlational evidence describing how household consumption changes with the woman's share of income was presented early on by Thomas (1990, 1993) in

Brazil, Bourguignon et al. (1993) in France and Hoddinott and Haddad (1995) in Côte d'Ivoire. Later, the literature employed natural experiments in clever ways to look at gender-specific transfers and household allocations (Lundberg et al., 1997; Duflo, 2003; Duflo and Udry, 2004). These initial investigations showed that women spent money differently, i.e. reforms that gave women more control over resources were followed by consumption shifting.

A more recent contribution is the randomized control trial that Give Directly conducted in collaboration with researchers in Kenya (Haushofer and Shapiro, 2016). This study randomized whether a household receives a transfer, its timing and magnitude, and the gender of the recipient. It is ground breaking as it investigates the effect of large scale *unconditional* cash transfers, however, the experiment has relatively low statistical power on the last dimension (gender of recipient). The reported estimates are not significantly different from zero, but relatively large positive or negative effects cannot be ruled out.[1] de Mel et al. (2009) also provide unconditional cash grants to either men or women, but they focus on small enterprise owners and the impact of grants on business development (profits and capital stock) rather than household finances.

The final group of papers focuses on conditional cash transfer programs that target mothers or that randomize between male and female recipients. Many of the papers on programs that target mothers study PROGRESA, a program in Mexico that provides transfers to mothers conditional on the household undertaking a set of behaviors designed to improve health, education and nutrition.[2] The studies show a positive impact on reported female decision-making power and find evidence for consumption shifting. Indeed, the first evaluations show that the food share in expenditure decreased and the share of children's clothing increased (Attanasio et al., 2002). Later evaluations indicate an increase in the food share (Attanasio and Lechene, 2014; Angelucci and Attanasio, 2009, 2013). Similar effects have been reported in comparable programs in other countries (see Fiszbein et al., 2009). Among the studies on conditional cash transfer programs that randomize between mothers and fathers is a study in Morocco. Benhassine et al. (2015) analyze a large experiment designed to measure the effect on educational attainment of a cash transfer program that targeted the mother or the father as the recipient and that was either conditional on school attendance or just labeled as a transfer for educational support. The program led to a large increase in school attendance, but targeting mothers and fathers gave very similar results, as did labeling versus conditionality. In Macedonia, Armand et al. (2020) studied a program that randomized whether the cash transfer was given to the mother or the

[1] For example, Appendix A.1 reports a minimal detectable effect of 28.77 or 0.35 standard deviations for expenditure and of 29.71 or 0.33 standard deviations for revenues. The estimated effect on expenditure is equal to 2, with a standard error of 10.28 (table II), meaning that the authors cannot reject an effect equal to, for example, +15 or -15.

[2] The program was renamed Opportunidades in 2001.

father, conditional on the household having a child attending secondary school. When women were recipients, the authors find that food share is moderately larger overall and the diet is more nutritious at low expenditure levels.

The aforementioned studies of programs with conditionalities and labeling provide crucial inputs to the policy discussion on how to best improve the lives of the poor in the short and longer term. However, they face some challenges when studying consumption shifting more generally, as the conditionality and labeling in these programs may affect consumption allocations directly through nudging, preference shifts, and/or a will to comply with the program providers' intentions. The studies have strived to meet these challenges and account for such potential direct effects in various ways, among them by using models of household decision making in their (structural) estimations. Our first contribution is to provide evidence in a setting where there is no labeling or conditionality on spending and as such these challenges are mitigated directly through the design.

Our paper makes three additional contributions. First, to the best of our knowledge, our study is the first highly powered experiment that was designed to test the differential impact of providing unconditional transfers to women and men. It is highly powered in both a statistical sense (even small treatment effects can be detected) and in an economic sense (the transfers of ₹150 per week are substantial: this is about 50% of average food expenditure at baseline).

Second, our expenditure, income and saving data are of high quality relative to other studies, thanks to the financial diaries and weekly reporting over a period of 24 weeks. The recall period is just seven days and the repetition over half a year allows us to capture infrequent expenditure and income as well. By measuring the direct changes in expenditure, income and saving, we can document and study how transfers impact downstream outcomes.

Third, both the need for and the effects of, targeted transfers are likely to vary across countries, and we are the first to study the impact of gender targeted transfers in India. Despite India's high recent growth rates, around 20% of the population is classified as poor, both according to national and international poverty lines (World Bank, 2020). The prevalence of malnutrition is also severe. About 70% of children and 55% of women are anemic, about 50% of children have a Vitamin A deficiency, and about 45% of children and 35% of women are underweight (Gonmei and Toteja, 2018; International Institute for Population Sciences and ICF, 2017; Chaparro et al., 2014). This makes India a particularly interesting and important context for the study of the impact of gender targeting on consumption shifting. Chhattisgarh, which has a population of

around 25 million, is quite representative with a development level that is about the same as India as a whole. The Human Development Index equals 0.613 for Chhattisgarh and 0.647 for India overall (Smits and Permanyer, 2019).[3]

In the next section, we discuss the design, the data, and attrition. The results are in Section 3 and the conclusion is in Section 4. In the Appendix, we report the power calculations, describe the outcome variables at baseline, test for impact heterogeneity, and present robustness checks and bounded estimates.

2. Experimental Design and Data

In this section, we describe the experimental design of our study, introduce the data, provide baseline characteristics and discuss attrition.

2.1. Experimental Design

The data collection effort was part of a research project seeking to understand consumption and saving behavior among rural households. India is an ideal setting, as its financial landscape has changed markedly over the past decade. In 2006, the Reserve Bank introduced the Business Correspondents model, which led to a rapid increase in bank account penetration. The model allows banks to appoint local banking agents, who provide financial and banking services on their behalf (RBI, 2006). We collaborated with Basix Sub-K, which serves as a financial inclusion company for different formal banks, and selected eighteen villages in which they operate.[4] The villages are located in three different districts of Chhattisgarh.

In each village, we randomly sampled 32 villagers based on three eligibility criteria: (i) being the head of the household or the head's spouse, (ii) not having plans to migrate during the study, and (iii) belonging to a household in which nobody has a formal bank account with financial institutions other than the local banking agent.[5] To draw the sample, we allocated a

[3] The subnational HDI data are downloaded from GlobalDataLab, Subnational Human Development Index (4.0). *https://globaldatalab.org* date: June 29, 2020.

[4] Basix Sub-K's main responsibilities are selecting a villager to become the local banker, training the person, giving technical support and providing customer service to the clients.

[5] We allowed post office or other accounts that were opened to receive payments from welfare schemes. We also permitted cooperative bank accounts that were used to receive income from the sale of crops. Villagers cannot deposit into post office or cooperative bank accounts and rarely do so into the other accounts, either because there is no secret code or biometric authentication to protect savings, or because the bank is too far away.

random number to each villager on the voter list and on the local banker's customer list and we approached their households in ascending order. We only selected one person per household (the head or the head's spouse) and randomized whether we would select the man or the woman in that particular household. The person was retained for the study if he or she satisfied the eligibility criteria. Importantly, the sampled person could not be replaced by another household member. If the person did not satisfy the criteria, we moved on to the next household on the list. Based on this procedure, we sampled 14 villagers who had already opened an account from the agent's customer list, and 18 villagers without an account from the voter list. The latter received help to open an account (12 villagers) or not (6 villagers). To obtain a sample that is stratified on gender, we continued the process until we obtained an equal number of men and women in each village. As the allocation of households to a male or female respondent was random, we can use the full sample to examine the impact of the respondent's gender identity.[6]

After drawing the sample, we conducted a baseline survey, opened the accounts and started weekly interviews of up to 17 interviews over 24 weeks. The purpose of the interviews was to obtain detailed information on the economic lives of the respondents and their households. After each interview, the villagers received ₹150 ($\simeq$ \$2), which is close to the wage for a day of work under the Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGA). The villagers without an account were paid in cash, but those with an account were randomly allocated to being paid in cash or into their account.

2.2. Data

We use four sources of data. First, we conducted a baseline survey in the fall of 2013. The survey gathered information on the characteristics of the respondents and their household members, which allows us to test the sample's balance and the heterogeneity in the treatment's impacts. It also provided details on decision-making within the household. The decision-making elicitation was in line with the DHS survey questions often used to measure power in household decision-making (see e.g., Jayachandran (2015)).[7]

Villagers usually withdraw the money at once shortly after a payment is made.

[6] Somville and Vandewalle (2018) use the sample of 442 respondents with an account to document the impact of the payment method (in cash or into the account) on saving behavior. Somville and Vandewalle (2019) discuss how the randomized encouragement to open an account affects households' saving and consumption smoothing. To do so, they use the sample of 204 respondents who did not yet have an account at the onset of the study and who were paid in cash.

[7] We asked "Who makes most decisions about x?" where x was expenditure on "Food items", "Health", "Home repairs", "Marriages and festivals" and "Where to save", "Amount to save", "Education" and "Agricultural inputs". Our respondent's answer could include one or two of the household members. In the DHS surveys the respondent is asked "Who usually makes the decision on x, you, your (husband/partner), you and your

Second, Basix Sub-K gave access to all the transactions that were made during the survey period.

Third, inspired by Collins et al. (2009) and Dupas and Robinson (2013), we conducted up to 17 weekly interviews between February and May, and July and August 2014. These took place on the same day of the week at a centrally located room in the village. On average, the respondents needed about three hours to travel, wait their turn, be interviewed and return home. These weekly interviews gathered details on the financial lives of the households and have been used to construct most of our outcome variables. They are organized around three major categories: income, expenditure and saving. The income sections cover wage labor, self-employment, the sale of crops, goods, livestock, and forest products, renting out of assets and land, loans, and private and public transfers. For expenditure, we record the amounts purchased for a list of 195 consumption items, investments, education and health expenses. Saving includes money in the bank account, in cash, in assets, and money saved with informal groups and guarded by others. To collect the information, we introduced automatic consistency checks in the survey template that prompt the enumerators to double check values that seem implausible compared to previous records. We believe that this process — combined with the short recall period — greatly improved the quality of the data collected and minimized measurement errors. Note that the replication over almost six months allows us to capture infrequent economic activities as well. The first transfer was done *after* the first interview, and we use this first interview to obtain pre-treatment (baseline) values of expenditure, income, saving and nutrition.

Finally, we conducted an endline survey to update the baseline information. We use this information to estimate the impact on reported involvement in household decision-making.

2.3. Attrition

One agent decided to stop his banking activities shortly after we conducted the baseline survey. Because there is only one local banker per village, and because active banking activities are essential for our sampling frame and design, we had to exclude the village from our experiment. The analysis, therefore, is based on the remaining 17 villages. This should not impact our results, as the agent's decision was unrelated to our study (the income generated from other activities was higher than his profits from banking) and we randomized within villages.

To be able to provide detailed feedback to the enumerators in the first weeks, we delayed the

(husband/partner) jointly, or someone else?" and the categories are decision-making on contraception use, visits to family or relatives, major household purchases, and the use of earnings or healthcare.

start of the weekly interviews in some villages. It also took us longer than expected to open the bank accounts in a number of villages. As a result, we conducted 17 interviews in 11 villages, 16 interviews in 2 villages, 13 interviews in 3 villages and 11 interviews in the last village. This gives a total of 8,608 interviews. The first interview with each respondent was conducted before the treatment took place and provides the baseline measure of the outcome variables (544 interviews). Nine planned sessions were cancelled due to festivals and bad weather conditions (288 interviews). The sample on which we conduct our analysis should thus contain 7,776 interviews. However, we face two forms of attrition. First, nine respondents did not attend any of the interviews and four respondents showed up for the first interview only. Without weekly interviews for these respondents, we had to exclude them from our sample (192 interviews). The remaining 531 respondents (97.6% of the sample), did on average not attend 13.6% of the weeks, which leaves us with 6,556 interviews altogether.

While the number of missing respondents is gender balanced (seven men and six women), the participation in the weekly interviews differs significantly between men and women. Indeed, attrition is 7.1% higher among men than women. To deal with this, we estimate bounds on the treatment effects using the approach of Kling and Liebman (2004). The results in Appendix C show that the bounded estimates are similar to those in the main tables. This implies that our results are robust to making unfavorable assumptions about the missing observations.

2.4. Baseline Characteristics and Balance Check

Table 1 presents baseline characteristics at the household level. We have information about 531 households from 266 male and 265 female respondents. The first column provides the means (and standard deviations) for the sample of male respondents and the second column for the sample of female respondents. The final column shows the coefficient estimates (and standard errors) of the difference between the baseline means. All the coefficient estimates are small and only two are significantly different from zero. The most remarkable difference is the proportion of married men and women, which reflects that - as in the population - the share of widows is higher than the share of widowers. Overall, these results suggest that the randomization was successful at making the treatment orthogonal to observed baseline characteristics.[8]

The population under study is relatively disadvantaged. The households on average own about

[8]The Tables A1 and A2 in Appendix A show that, apart from decision-making, the outcome variables are balanced at baseline as well. We control for the baseline value of the outcome variable in our main specification, but the results are robust to its exclusion.

Table 1: Summary Statistics and Balance Check of Baseline Characteristics

	Mean for men (Std. dev.)	Mean for women (Std. dev.)	Coefficient on *women* (Std. errors)
	(1)	(2)	(3)
New account (%)	.38	.38	.00
	(.49)	(.49)	(.04)
Had account (%)	.43	.43	.00
	(.50)	(.50)	(.04)
Paid into account (%)	.40	.40	.00
	(.49)	(.49)	(.04)
Caste category: ST (%)	.14	.13	.00
	(.34)	(.34)	(.03)
Caste category: SC (%)	.13	.12	-.01
	(.34)	(.33)	(.03)
Caste category: OBC (%)	.72	.74	.02
	(.45)	(.44)	(.04)
Caste category: FC (%)	.01	.00	-.01
	(.11)	(.06)	(.01)
Married (%)	.93	.82	-.11***
	(.25)	(.38)	(.03)
Land (acres)	1.31	.95	-.35**
	(1.79)	(1.55)	(.15)
Dwelling type: katcha (%)	.56	.51	-.05
	(.50)	(.50)	(.04)
Distance to the BCSA (km)	.28	.28	.00
	(.21)	(.20)	(.02)
Observations			531

The first and second column report means (and standard deviations) for male and female respondents, and the final column shows the coefficient estimates (and standard errors) of the difference between the means in both groups. $* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$.

1.1 acre of land and 54% live in a house that is made of mud (katcha). In terms of demographic characteristics, respondents are mainly Other Backward Classes.[9] The average distance from the respondent's house to the bank agent is about 279 meters as the crow flies. 43% of the respondents had already opened an account themselves and we provided help in doing so to another 38%. This implies that 19% of the respondents do not have a bank account (but can open one if they would like to do so). Finally, 40% received their transfers directly into their bank account, while the rest were paid in cash. As explained in Section 2.1, whether or not a respondent received help in opening an account and the method of payment were determined randomly.

[9]Castes are classified in the following categories: Scheduled Tribe (ST), Scheduled Caste (SC), Other Backward Classes (OBC), and Forward Castes (FC).

3. Results

We first discuss our empirical methodology. Then we compare the impact of transferring money to women or men on five main outcome variables: expenditure, saving, income, nutrition and decision-making. Finally, we investigate whether the main composition of each of those categories has changed. To do so, we divide the categories into components and estimate the treatment effect on each of those components.

3.1. Empirical Methodology

The project aims to evaluate whether transfers to women have a different impact on the financial lives of rural households than transfers to men. We combine the random allocation of households to a male or female respondent (see Section 2) with accurate information from the weekly interviews. Measuring financial details is a major challenge that has been discussed at length in the literature.[10] To measure this as accurately as possible, we use diaries that cover short recall periods for several months.[11] An important concern in our setting is that reported information may differ between men and women. The Tables A1 and A2 in Appendix A suggest this is not a major issue: apart from decision-making within the household, baseline values of the outcome variables do not differ significantly between male and female respondents. We use ANCOVA estimates that are more precise, but the results are robust to excluding the baseline value of the outcome variables.

Our main specification is a pooled panel model on a sample of 6,556 interviews taken from 531 respondents, with standard errors clustered at the household level.

$$Y_{ikt} = \alpha_0 + \alpha_1 F_i + \alpha_2 X_{ik} + \alpha_3 Y_{ik1} + V_k + W_t + \epsilon_{ikt} \tag{1}$$

Y_{ikt} is the outcome variable of interest for household i in village k measured in week t. F_i is a dummy variable equal to one if the recipient is a woman and zero if the respondent is a man. Further controls are the two strata variables X_{ik} indicating whether the respondent received a bank account and was paid into the account, the outcome variable at baseline Y_{ik1} (measured

[10] Angus Deaton addressed this issue in his Nobel lecture: "*Among the most difficult and pressing problems with household surveys is the quality of data; in some cases, the problems are severe enough to threaten even the most basic understanding of growth, poverty, and inequality.*" (Deaton, 2016, p. 1223).

[11] The recall period plays a central role in the discussion of measurement. We refer the reader to Deaton and Grosh (2000), Gibson (2006) and Beegle et al. (2012) for reviews. In cross-sectional surveys, longer recall periods are needed to capture infrequent expenditure, though this implies larger measurement errors. For the Indian context, the choice of the recall period has been discussed by Deaton and Kozel (2005).

during the first interview), and village V_k and time W_t fixed effects.[12] Finally, ϵ_{ikt} is the error term.

For decision-making, we collected information at baseline and endline only. Therefore, the main specification is a cross-section of the 518 households that participated in the endline survey, with standard errors calculated using nonparametric bootstrapping.

To limit the influence of outliers, we transform Y_{ikt} and Y_{ik1} using the inverse hyperbolic sine transformation (Ravallion, 2017). We do so for all variables, apart from those measuring a share or being related to decision-making.[13] Bellemare and Wichman (2020) provide practical guidance on the interpretation of the results. If the transformed mean of the dependent variable is roughly greater than 3, the impact of a dummy variable can be interpreted as in a standard log transformation. This is the case for our main outcomes, as can be seen from the "mean control" row in Table 2.

Before we turn to the results, we want to emphasize that our study is highly powered. Indeed, in Appendix B, we show the power of our study is well above 80% for all the outcome variables at a 5% significance level.

3.2. The Main Outcome Categories

We first show the treatment effect on broad categories: (i) total expenditure, (ii) the share of food expenditure in total expenditure, (iii) total saving, (iv) total income, (v) an index of nutrition, and (vi) an index of decision-making within the household. Apart from decision-making, all the outcome variables are estimated over the past seven days during the weekly interviews.

The total values are calculated by summing up the detailed information we obtained on different types of expenditure, income and saving (see Section 2). The indices are calculated using a Principle Component Analysis (PCA). The index for nutrition is based on six aspects of the nutritional content of food expenditure: calories, proteins, iron, zinc, vitamin A and C. These nutrients are particularly important in India. Indeed, they reflect the main sources of deficiencies for children as well as for the general population (see e.g. Gonmei and Toteja, 2018; International Institute for Population Sciences and ICF, 2017; Chaparro et al., 2014). The only nutrient highlighted as important that we did not include is iodine, because we could not retrieve this

[12] As there is only one banker per village, the village fixed effects also absorb all banker fixed effects.
[13] We present the impact on the level in Appendix C.

Table 2: Impact on the Main Outcome Variables

	Total expenditure	Food share	Total saving	Total income	PCA of nutrition	PCA of decision-making
	(1)	(2)	(3)	(4)	(5)	(6)
Woman	-.031	.011	.058	.000	.001	.883***
	(.058)	(.011)	(.082)	(.067)	(.069)	(.117)
Mean Control	7.18	.47	8.09	6.40	.00	-.82
R^2	.200	.070	.840	.100	.130	.370
Obs.	6556	6556	6556	6556	6556	518

The columns (1) to (4) present the impact on the households' total expenditure, share of food in total expenditure, total saving and total income over the past seven days. Apart from food share, we transform these outcome variables by implementing an inverse hyperbolic sine transformation. The columns (5) and (6) show the impact on indices of nutrition and female decision-making within the household. The former is still measured over the past seven days, while the latter is measured at endline only. All columns include village fixed effects and control for the mode of payment, for bank account ownership and for the baseline value of the outcome variable. The columns (1) to (5) also include time fixed effects. Standard errors are clustered at the household level in the first five columns, and are bootstrapped in the final one. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

information from our dataset. The index of decision-making is based on decisions related to expenditure on food, health, home repair, or social activities, and on decisions related to the amount to save and where to save. It takes a higher value if the woman has more say.

The results in Table 2 show that there is no treatment effect on total expenditure, the share spent on food, total saving, total income or the nutritional value of purchased food. There is an important impact on decision-making though, suggesting that targeted women are more empowered within the household.

Table C1 in Appendix C shows the results are robust to alternative specifications: (i) measuring the outcome variables (and baseline values) in levels, (ii) omitting the baseline value of the dependent variable as a covariate, (iii) using one observation per household (by averaging the outcome variables over the survey weeks) and (iv) including the variables listed in Table 1 as additional covariates.

We now turn to investigating whether targeting women impacts the composition of these broad categories.

3.3. The Composition of the Main Outcome Categories

To investigate whether targeting women impacts the composition of our broad categories, we divide them into components and estimate the treatment effect on the sub-categories. We present

the results for each category in a separate table. As we have a large number of outcome variables, we indicate both the unadjusted p-values and the corresponding thresholds after correcting for multiple hypothesis testing using the method of Benjamini and Hochberg (1995). We adjust the p-values for the hypotheses in the set presented in each table.

The results are shown in Tables 3 to 7. For those categories that were not impacted on average —total expenditure, total saving, total income and the nutritional value of purchased food— the broad composition does not change either, in particular after correcting for multiple hypothesis testing. We have information on 195 consumption items, but only a small fraction of them can be classified as gender-specific. When we look at spending on these items, we observe that households with female recipients spend more on female specific goods. Finally, we find that the significant change in the PCA of decision-making within the household is driven by a change in each of its components. The remainder of this section discusses the tables in more detail.

Table 3 decomposes total expenditure into frequent and non-frequent expenditure, temptation goods, investment, and education and health expenditure.[14] We also estimate the impact on the total amount spent on food and split this further into items that have to be consumed within a short period of time (perishable) and items that can be stored (non-perishable).

We decompose saving into seven different saving vehicles: (1) the account with the local banker, (2) self-help groups (SHGs) and other informal neighborhood groups, (3) agricultural cooperatives, (4) the post office and other accounts, (5) cash at home, (6) money guarded by others and (7) the total stock of jewelry, grain and livestock. The impact on each of these tools is shown in Table 4.

Table 5 first presents the impact on the seven sources of income we observed in our sample, namely (1) wage labor, (2) self-employment, the sale of (3) crops, (4) livestock, (5) forest products and (6) goods, and (7) renting out of assets and land. Next, it shows the impact on the net inflow of transfers and loans. Column (8) presents the impact on transfers received from the government, column (9) on the net inflow of private transfers (the total amount received minus the total amount given) and column (10) on the net inflow of loans (the total amount borrowed minus the total amount lent, plus the net amount of reimbursements received).

[14] Frequent expenditure is the sum of expenses on goods that are bought frequently by the average household, i.e. at least once every three weeks. It includes spending on grains, cereals, pulses, lentils, milk products, edible oil, vegetables, fruit, sugar, salt, spices, fuels, soap and washing powder. Temptation goods are products that are not *survival necessities* (Banerjee and Mullainathan, 2010) and include spending on pan, alcohol, tobacco, drinks and snacks from the market, hair oil, lotion and perfumes.

Table 3: Treatment Effect on Expenditure Categories

	Frequent	Non-frequent	Temptation goods	Investments	Education	Health	All food	Perishable food	Non-perishable food
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Woman	.016	-.068	-.006	-.040	-.001	-.005	.021	.041	.104
	(.046)	(.108)	(.074)	(.133)	(.023)	(.098)	(.048)	(.050)	(.070)
Mean Control	6.19	5.09	4.32	1.69	.10	1.53	6.16	5.31	5.23
R^2	.155	.128	.265	.102	.028	.032	.161	.190	.087
Obs.	6556	6556	6556	6556	6556	6556	6556	6556	6556

The table presents the impact on different categories of household expenditure. The dependent variables are the hyperbolic sine transformations of weekly spending on (1) frequent goods, (2) non-frequent products, (3) temptation goods, (4) investments, (5) education, (6) health, (7) all food and (8) perishable and (9) non-perishable food. All columns include time and village fixed effects and control for the mode of payment, for bank account ownership and for the baseline value of the outcome variable. The standard errors, in parentheses, are clustered at the household level. Statistical significance is indicated by $* \ p < 0.1$, $** \ p < 0.05$, $* * * \ p < 0.01$ for unadjusted p-values and by $+ \ p < 0.1$, $++ \ p < 0.05$, $+ + + \ p < 0.01$ for p-values that are adjusted for multiple hypothesis testing.

Table 6 focuses on the nutritional content of food expenditure. The hypothesis we test is that even if the expenditure is equal in monetary value, men and women may buy food with different nutritional content. To do so, we estimate the treatment impacts on the total purchases of (1) calories, (2) proteins, (3) iron, (4) zinc, (5) Vitamin A and (6) vitamin C. To calculate the nutritional content, we use the reported quantity of each purchased food item and convert it using the *Indian Food Composition Table 2017* (IFCT), which is produced by the National Institute of Nutrition. For some goods, the IFCT is more detailed than our diaries. For example, while we recorded only one type of maize, the IFCT differentiates between three categories: A006 Maize dry; A007 Maize, tender, local; and A008 Maize, tender, sweet. In these cases, we measure the nutritional value by averaging the values of the corresponding IFCT categories. For the few food items that were recorded in the diaries but not in the IFCT, we use the *Food Composition Databases* published by the US Department of Agriculture.[15]

We cannot reject the hypothesis that there is no difference in the composition of expenditure, saving, income and nutrition for households with male and female recipients. The picture changes when we turn to decision-making. At both baseline and endline, we ask which household members take most decisions in eight different domains. The respondent could identify up to two different people. For each item, we construct a variable equal to one if the primary woman in the household (the head or the head's wife) is the only one to decide, zero if she is not involved in the decision and half if she is involved but not the only person making the decision.[16] The

[15]The vitamin A measure that we use was obtained by combining the retinol activity equivalents for preformed vitamin A and for beta-carotene as described by the National Institutes of Health (www.dsld.nlm.nih.gov/dsld/unitconversion.jsp).

[16]The results are the same if we use a categorical variable taking value one if the woman is involved in the decision.

Table 4: Treatment Effect on Saving Vehicles

	Bank account	SHGs	Cooperatives	Post office and other accounts	Cash at home	Money guarded	Jewelry, grain and livestock
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Woman	.262**	-.032	-.011	-.105	-.097	.008	.047
	(.120)	(.125)	(.047)	(.135)	(.081)	(.016)	(.055)
Mean Control	4.33	1.54	1.77	1.65	3.40	.02	4.62
R^2	.753	.790	.956	.589	.784	.027	.972
Obs.	6556	6556	6556	6556	6556	6556	6556

The table presents the impact on the different saving vehicles. For each asset, we present the impact on the hyperbolic sine transformation of the balance that was recorded during the weekly interviews. The different saving tools are: (1) bank account, (2) SHGs and other informal neighborhood groups, (3) agricultural cooperatives, (4) the post office and other accounts, (5) cash at home, (6) money guarded by others and (7) the total stock of jewelry, grain and livestock. See Table 3 for the list of control variables and for a description of the standard errors.

Table 5: Treatment Effect on Income Sources, Transfers and Loans

	Income sources									
	Wage empl	Self-empl	Agri-culture	Live-stock	Forestry	Sale of goods	Rental	Public transfers	Private transfers	Loans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Woman	.075	-.093	-.041	-.021	.011	-.009	-.028	-.001	-.034	-.186*
	(.061)	(.091)	(.071)	(.042)	(.010)	(.019)	(.020)	(.045)	(.072)	(.105)
Mean Control	6.01	.56	.52	.16	.01	.08	.06	.41	-.80	-.43
R^2	.108	.392	.041	.375	.013	.007	.144	.078	.099	.020
Obs.	6556	6556	6556	6556	6556	6556	6556	6556	6556	6556

The table presents the impact on the inverse hyperbolic sine transformation of the household's weekly sources of income. The different income sources are: (1) wage labor, (2) self-employment, the sale of (3) crops, (4) livestock, (5) forest products and (6) goods, (7) renting out of assets and land, (8) public transfers, (9) private transfers and remittances and (10) loans. See Table 3 for the list of control variables and for a description of the standard errors.

Table 6: Treatment Effect on Nutritional Values of Purchased Food

	Kj	Proteins	Iron	Zinc	Vitamin A	Vitamin C
	(1)	(2)	(3)	(4)	(5)	(6)
Woman	.031	.007	-.008	-.005	.040	.005
	(.079)	(.063)	(.008)	(.007)	(.086)	(.021)
Mean Control	11.75	6.69	.26	.20	10.64	.92
R^2	.080	.098	.102	.100	.146	.296
Obs.	6556	6556	6556	6556	6556	6556

The table presents the impact on the inverse hyperbolic sine transformation of the nutritional content of the household's food expenditure. We focus on six important components (1) Kj, (2) proteins, (3) iron, (4) zinc, (5) vitamin A and (6) vitamin C. See Table 3 for the list of control variables and for a description of the standard errors.

Table 7: Treatment Effect on Decision-Making

	Expenditure on						Expenditure on	
	Food items	Health	Home repair	Marriages, festivals	Where to save	Amount to save	Edu-cation	Agricultural inputs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Woman	.106$^{***}_{+++}$	.105$^{***}_{+++}$	.102$^{***}_{+++}$	.080$^{***}_{+++}$	.149$^{***}_{+++}$	.149$^{***}_{+++}$	.118$^{***}_{+++}$	.066$^{***}_{+++}$
	(.017)	(.014)	(.021)	(.017)	(.017)	(.018)	(.021)	(.025)
Mean Control	.40	.33	.34	.41	.35	.36	.29	.26
R^2	.255	.291	.206	.255	.246	.232	.202	.085
Obs.	518	518	518	518	518	518	346	375

The table presents the impact on the decision-making responsibilities of the spouse of the household head. We asked about the following subjects: (1) the food items to purchase, (2) health expenditure, (3) expenses for home purchases, improvement or repair, (4) expenditure on marriages, festivals and social functions, (5) where to save, (6) how much to save, (7) expenditure on education, and (8) the purchase of agricultural inputs. While the first six categories were answered by all the households, the latter two were conditional on having children and owning agricultural land. See Table 3 for a description of the control variables included and the standard errors.

eight different domains are as follows: decisions regarding (1) the food items to purchase, (2) health expenditure, (3) expenses for home purchases, improvement or repair, (4) expenditure on marriages, festivals and social functions, (5) where to save, (6) how much to save, (7) expenditure on education, and (8) the purchase of agricultural inputs. We only asked about the latter two subjects if there is a student in the household or if they own agricultural land. As the number of observations is smaller for these items, we did not include them in the calculation of the PCA index. The results are robust to including them, or using the average as an alternative index.

As observed in previous studies, the measures are bunched at "both decide" (see e.g., Adato et al. (2000) and Almås et al. (2018) for a discussion). We also observe a possible reporting bias: female respondents are more likely to report the wife decides and male respondents that the husband decides (see the descriptive statistics in table A2). More importantly though, there is a substantial treatment effect: Table 7 shows that targeting transfers to women empowers them in all estimated domains.[17]

The suggested empowerment effect is further confirmed when we look at gender-specific items. We split temptation goods into products that are more likely to be used by women (hair oil, lotion and perfumes), by men (pan, alcohol and tobacco) or that are gender-neutral (snacks

[17]The descriptive statistics are particularly interesting. We pool the decisions across topics and households and compare the rate at which men and women report the husband or the wife takes the decision alone or they both decide together (we ignore decisions taken together with or by others, as those are only a small fraction). The share of decisions taken by women alone is stable: 1.8% at baseline and 2% at endline according to men; 18.4% at baseline and 16.2% at endline according to women. The results are driven by a shift from men deciding alone at baseline to joint decision-making at endline. While 28.4% of men report they take decisions alone at baseline, only 18.8% do so at endline. Taking decisions together increases from 55.8% to 64.7%. Women report a similar change. The share of men taking decisions alone decreases from 14.5% at baseline to 7.1% at endline, and the share of joint decisions increases from 53.4% at baseline to 64.2% at endline.

Table 8: Treatment Effect on Gender-Specific Goods

	Expenditure on					
	Female temptation goods (1)	Male temptation goods (2)	Neutral temptation goods (3)	Clothes for women (4)	Clothes for men (5)	Clothes for any gender (6)
Woman	$.098^{***}_{+++}$	-.021	$.041^{*}_{+}$	$.008^{*}$	-.003	$.023^{*}$
	(.021)	(.018)	(.022)	(.005)	(.003)	(.012)
Mean Control	.31	.86	.63	.03	.02	.19
R^2	.061	.397	.097	.010	.007	.026
Obs.	6556	6556	6556	6556	6556	6556

The table presents the impact on the probability the household bought a gender-specific product. We split temptation goods in products that are (1) more likely to be used by women, (2) more likely to be consumed by men (3) not gender-specific). We use a similar categorization for clothes. We differentiate between clothes (4) for women, (5) for man and (6) that cannot be classified. See Table 3 for a description of the control variables included and the standard errors. We adjusted the p-values for multiple hypothesis testing for temptation goods (column 1 to 3) and clothes (column 4 to 6) separately.

and drinks from the market). With respect to clothing, saris are worn by women, dhotis and lungis by men, but for the other categories in our survey – cloth, ready-made garments, hosiery articles, footwear and raincoat – the gender of the beneficiary is not clear. Table 8 shows a highly significant treatment effect on temptation goods that are more likely to be used by women and a weakly significant impact on spending on saris.

To sum up, our analysis suggests that there is no overall expenditure switching at the household level induced by targeting women rather than men with transfer schemes. However, such targeted transfers seem to have a strong positive effect on female empowerment and gender equalization. This empowerment effect has been put forward as a more important reason to target women than the potential impact on household consumption switching (Banerjee and Duflo, 2019).

4. Conclusions

We reported from a randomized control trial that offered unconditional cash transfers to either a man or a woman. We do not find a differential effect on household consumption and saving decisions, on income and on the nutritional content of food expenditure. However, our results suggest that targeted transfers empower the recipient and as such female targeting leads to gender equalization. We thereby found support for one of the two main arguments that have been raised in support of targeting women: the empowerment argument.

Throughout the paper we have been agnostic about the underlying model of household decision-making. Households in a unitary model are assumed to be a single unit, either because one person decides everything or because household members have fixed exogenous weights in decision-making (Becker, 1974). Since transfers affect the women's decision-making power, our results are not consistent with this model. Our main findings —changes in decision-making, but not in household consumption— are consistent with a collective model, in which spouses have the exact same trade-off between the different broad spending categories (Browning and Chiappori, 1998; Browning et al., 2014). Our results are also consistent with a non-cooperative model as long as there is a least some overlap in spending, i.e. as long as there exists at least one spending category that both spouses spend on (e.g., child education) (Blundell et al., 1994; Ulph, 2006).

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Appendix A Balance Check of Outcome Variables at Baseline

Tables A1 and A2 present the outcome variables at baseline, thus measured during the first weekly interview. They provide the means (and standard deviations) in levels and after implementing the inverse hyperbolic sine transformation (IHST). The final column shows the coefficient estimates (and standard errors) of the difference between the baseline means in the treatment and control groups after implementing IHST. Table A2 also reports the number of observations, as the sample is smaller for two variables. Indeed, we only asked about decision-making regarding educational expenditure if households have a student and regarding agriculture expenses if the family owns land.

Most of the coefficient estimates are small. The only exceptions are those related to decision-making, which is due to women being more likely to report that the wife decides and men being more likely to report that the husband decides. We take this into account by including the baseline value of the outcome variables in our main specification.

Table A1: Balance Check of Outcome Variables at Baseline: Expenditure and Income

	Level: Mean (Std. dev.) (1)	IHST: Mean (Std. dev.) (2)	Coefficient on *women* (Std. errors) (3)
Expenditure			
Total	1456	7.18	-.12
	(2794)	(1.26)	(.11)
Food share	46		.40
	(27)		(2.31)
Frequent	380	6.21	-.07
	(485)	(1.01)	(.09)
Non-frequent	409	4.91	.04
	(1399)	(2.34)	(.20)
Temptation goods	75	4.25	-.09
	(103)	(1.50)	(.13)
Investments	592	2.05	-.81***
	(1886)	(3.45)	(.30)
Education	9.45	.11	.08
	(101)	(.84)	(.07)
Health	181	1.95	.17
	(668)	(2.97)	(.26)
All food	367	6.14	-.13
	(492)	(1.05)	(.09)
Perishable food	144	5.28	-.18**
	(151)	(1.00)	(.09)
Non-perishable food	223	5.24	-.08
	(397)	(1.67)	(.14)
Income			
Total	668	3.37	-.22
	(1978)	(3.72)	(.32)
Wage employment	320	2.62	-.18
	(604)	(3.49)	(.30)
Self-employment	175	.47	-.11
	(1292)	(1.90)	(.16)
Agriculture	128	.42	.04
	(1087)	(1.73)	(.15)
Livestock	4.44	.15	.09
	(39)	(.89)	(.08)
Forestry	.00	.00	.00
	(.00)	(.00)	(.00)
Sale of goods	11	.02	-.04
	(260)	(.41)	(.04)
Rent	30	.04	.07
	(506)	(.59)	(.05)
Public transfers	10	.21	.20*
	(59)	(1.15)	(.10)
Private transfers	17	-.28	-.22
	(487)	(2.08)	(.18)
Loans	430	.08	-.27
	(4167)	(2.59)	(.22)
Observations			531

The first column reports means (and standard deviations) in levels and the second column after implementing the inverse hyperbolic sine transformation (IHST). The third column shows the coefficient estimates (and standard errors) of the difference between the means in the treatment and control groups after implementing the IHST. $* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table A2: Balance Check of Outcome Variables at Baseline: Saving, Nutrition and Decision-Making

	Number of observations	Level: Mean (Std. dev.)	IHST: Mean (Std. dev.)	Coefficient on *women* (Std. errors)
	(1)	(2)	(3)	(4)
Saving				
Total	531	12647	7.48	-.05
		(25031)	(3.23)	(.28)
BCSA account	531	240	3.39	.01
		(1039)	(2.96)	(.26)
SHGs	531	411	1.30	.12
		(1596)	(2.94)	(.26)
Cooperatives	531	665	1.60	-.37
		(4613)	(3.12)	(.27)
Post Office and other accounts	531	224	1.29	-.09
		(1633)	(2.70)	(.23)
Cash at home	531	243	2.81	-.19
		(667)	(3.17)	(.28)
Money guarded	531	.00	.00	.00
		(.00)	(.00)	(.00)
Jewelry, grain and livestock	531	10865	4.76	.29
		(22647)	(5.06)	(.44)
Nutrition				
PCA of nutrition	531	.04	.02	-.17
		(2.03)	(2.00)	(.17)
Kj	531	239665	12	-.20
		(866783)	(2.06)	(.18)
Proteins	531	1191	6.53	-.10
		(2254)	(1.75)	(.15)
Iron	531	.25	.22	-.02
		(.45)	(.27)	(.02)
Zinc	531	.18	.16	-.02
		(.34)	(.27)	(.02)
Vitamine A	531	47763	11	.05
		(47520)	(1.99)	(.17)
Vitamine C	531	1.56	1.10	-.08*
		(1.38)	(.53)	(.05)
Decision making				
PCA of decision-making	531	.00		1.59***
		(2.16)		(.17)
Food items to purchase	531	.44		.19***
		(.29)		(.02)
Health expenditure	531	.38		.22***
		(.30)		(.02)
Expenses for home purchases, improvement or repair	531	.39		.21***
		(.31)		(.03)
Expenditure on marriages, festivals and social functions	531	.43		.18***
		(.27)		(.02)
Where to save	531	.40		.20***
		(.33)		(.03)
Amount to save	531	.41		.19***
		(.33)		(.03)
Education expenditure	383	.36		.14***
		(.30)		(.03)
Purchase of agricultural inputs	406	.30		.16***
		(.29)		(.03)

The first column provides the number of observations. The second column reports means (and standard deviations) in levels and the third column after implementing the inverse hyperbolic sine transformation (IHST). The fourth column shows the coefficient estimates (and standard errors) of the difference between the means in the treatment and control groups after implementing the IHST. $* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$.

Appendix B Power

Calculating the power of our study is not trivial. We not only have repeated observations and thus potential auto-correlation, but also control for the randomization blocks, the baseline value of the outcome variable, and time and village fixed effects. Furthermore, we have to take into account that our standard errors are clustered at the household level. Burlig et al. (2019) discuss these issues in detail and suggest calculating the power using simulations based on real data. We implement their simulation algorithm using data from the control group.

The simulations are based on our main specification, assuming we have one pre-treatment and sixteen post-treatment periods, seventeen villages and 32 observations per village (which are equally divided into two arms). We focus on three main outcome variables: total expenditure, saving and income. For each of these variables we calculate the power to obtain a minimal detectable effect of 0.1, 0.15 and 0.2 standard deviations. We choose a level of significance of 0.05 and run 1000 simulations per outcome variable and per hypothesized minimal detectable effect.

Table B1 reports the results. The simulations suggest we have high power to detect a minimum effect of 0.10 standard deviations for saving and income and of 0.15 standard deviations for expenditure. Given that most studies rely on a power of 80% and a minimum detectable effect of 0.2 standard deviations at a significance level of 0.05, we consider that our study is well powered.[18]

Table B1: Power Calculations

	Power for		
Minimum detectable effect (s.d)	Total expenditure (1)	Total saving (2)	Total income (3)
.10	.57	.94	.94
.15	.89	1.00	1.00
.20	.98	1.00	1.00

[18] The power calculations assume a balanced panel since we have some attrition, the realized power is moderately lower than reported.

Appendix C Robustness and Attrition

In this section, we check the robustness of our main results to alternative specifications and to the calculation of bounds on the treatment effects using unfavorable assumptions about missing observations.

C.1 Robustness of the main tables

Our main specification (i) transforms the outcome variables using an inverse hyperbolic sine transformation, (ii) includes the baseline value of the dependent variable, (iii) is based on one observation per household per week and (iv) controls for strata dummies (the mode of payment and bank account ownership), but not for additional covariates. We now show that our main results - which were presented in Table 2 - are robust to relaxing any of these constraints. Table C1 presents the results.

The specification in Panel A measures the dependent variable (and its baseline value) in levels. As the share of food and the PCA of decision-making were estimated in levels already, the columns (2) and (6) remain the same. In Panel B we omit the baseline value of the dependent variable as a covariate, and in Panel C we include only one observation per household by averaging the outcome variable over the survey weeks. Standard errors are now calculated using nonparametric bootstrapping. Finally, in Panel D we add the following covariates: the household's caste category, the amount of land owned (in acres), the type of dwelling, the distance to the local banker and whether or not the respondent is married.

C.2 Attrition

As explained in Section 2.3, we face two forms of attrition. First, we do not have post-treatment interviews for 13 sampled respondents (192 interviews). Second, the remaining 531 respondents did not attend 13.6% of the interviews. While the former is gender balanced (seven men and six women), the latter is not: attrition is 7.1% higher among men than women.

To deal with the attrition, we follow the approach of Kling and Liebman (2004) and estimate bounds on the treatment effects under different assumptions about the missing observations. We assume that - if the missing values were observed - they would have been 0.05 (or 0.1) standard

Table C1: Robustness checks

	Total expenditure	Food share	Total saving	Total income	PCA of nutrition	PCA of decision-making
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dependent variables measured in levels						
Woman	27.5	.000	-53.0	-44.5	-.100	.883***
	(129.5)	(.000)	(445.1)	(98.8)	(.000)	(.117)
Mean Control	1339.3	.47	14394.2	1120.9	.03	-.82
Obs.	6556	6556	6556	6556	6556	518
Panel B: Not controlling for the baseline value of the outcome variables						
Woman	-.078	.012	-.023	-.013	-.029	1.65***
	(.069)	(.012)	(.159)	(.070)	(.076)	(.205)
Mean Control	7.18	.47	8.09	6.40	.00	-.82
Obs.	6556	6556	6556	6556	6556	518
Panel C: One observation per household (average over the weeks)						
Woman	-.052	.011	.043	-.030	-.036	.883***
	(.068)	(.012)	(.074)	(.075)	(.028)	(.117)
Mean Control	7.56	.45	8.19	7.40	-.02	-.82
Obs.	531	531	531	531	531	518
Panel D: Including additional control variables						
Woman	.017	.000	.042	.026	.058	.874***
	(.060)	(.011)	(.083)	(.069)	(.073)	(.116)
Mean Control	7.18	.47	8.09	6.40	.00	-.82
Obs.	6556	6556	6556	6556	6556	518

Each panel presents a different robustness check of the main results that were presented in Table 2. The specification in Panel A measures the dependent variable (and its baseline value) in levels, in Panel B it omits the baseline value of the dependent variable as a covariate; in Panel C it includes only one observation per household, and in Panel D it includes additional covariates.

deviations away from the weekly observed group means. More precisely, we calculate the upper bounds by replacing the missing values in the treatment group by the mean among the treated in the same village and week plus 0.05 (or 0.1) standard deviations. For the control group, we do a similar exercise, but we replace the missing values by the mean among the control in the same village and week minus 0.05 (or 0.1) standard deviations. To calculate lower bounds, we follow the opposite procedure: for the treatment group, we take the mean minus 0.05 (or 0.1) standard deviations and for the control we take the mean plus 0.05 (or 0.1) standard deviations.

Table C2 presents the results. In panel A, we follow this procedure for the sample of 531 households for which we have at least one post-treatment weekly interview. This is the sample that was used throughout the paper and allows the implementation of our main specification. In panel B, we include all 544 sampled households. As we do not have values for the outcome

variables at baseline for some households, we can no longer run ANCOVA regressions.[19] The results confirm our main conclusions: there is an important treatment effect on decision-making within the household only.

[19]These results should be compared to panel B in Table C1 (which, in fact, do not differ from those in Table 2).

Table C2: Main Results - Bounded Estimates

	Total expenditure	Food share	Total saving	Total income	PCA of nutrition	PCA of decision-making
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Balanced panel of 531 households						
Mean +/- 0.05 std. dev.						
Lower	-.044	.006	-.061	-.022	-.002	.364***
	(.052)	(.010)	(.079)	(.057)	(.027)	(.054)
Upper	-.015	.013	.016	.028	.038	.366***
	(.051)	(.010)	(.079)	(.057)	(.027)	(.054)
Mean +/- 0.1 std. dev.						
Lower	-.059	.003	-.100	-.046	-.022	.363***
	(.052)	(.010)	(.079)	(.057)	(.027)	(.054)
Upper	-.001	.016*	.054	.052	.057**	.367***
	(.051)	(.010)	(.079)	(.057)	(.027)	(.054)
Mean Control	7.16	.47	8.05	6.35	.54	.31
Obs.	7584	7584	7584	7584	7584	531
Panel B: Balanced panel of 544 households						
Mean +/- 0.05 std. dev.						
Lower	-.087	.007	-.128	-.039	-.013	.814***
	(.058)	(.010)	(.138)	(.058)	(.029)	(.134)
Upper	-.053	.015	-.038	.017	.033	.821***
	(.058)	(.010)	(.138)	(.058)	(.029)	(.134)
Mean +/- 0.1 std. dev.						
Lower	-.103*	.003	-.172	-.067	-.036	.811***
	(.058)	(.010)	(.138)	(.059)	(.029)	(.134)
Upper	-.036	.019*	.007	.045	.056*	.824***
	(.058)	(.010)	(.138)	(.058)	(.029)	(.135)
Mean Control	7.16	.47	8.05	6.35	.53	.30
Obs.	7776	7776	7776	7776	7776	544

We estimate bounds on the treatment effects under different assumptions about the missing observations. Panel A presents bounded estimates for the sample of households employed in the paper, based on our main specification. Panel B shows bounded estimates for the full sample of 544 households, based on a specification that does not include the baseline values of the outcome variables.

Appendix D Heterogeneous effects

To test for heterogeneity in the treatment effects, we adjust equation (1) by including an interaction term between the treatment variable F_i and the baseline characteristic B_i:

$$Y_{ikt} = \beta_0 + \beta_1 F_i + \beta_2 B_i + \beta_3 F_i * B_i + \beta_4 X_{ik} + \beta_5 Y_{ik1} + V_k + W_t + \epsilon_{ikt} \quad (2)$$

We test for heterogeneity in the treatment effects along four dimensions: wealth, poverty, literacy and the presence of the woman's mother-in-law in the household. We proxy wealth by an indicator taking value one if the house is a robust construction (not made of mud, or katcha). A household is considered relatively rich if its baseline expenditure are above median. A literate respondent is able to read and write. Finally, "mother-in-law" is an indicator variable equal to one if the woman's mother-in-law is a household member.

Table D1 shows that most coefficients are not statistically significant. The impacts on saving and on decision-making within the household, though, are stronger in "low expenditure" households than in "high expenditure" households.

Table D1: Impact on Main Outcome Variables - Heterogeneous Effects

	Total expenditure	Food share	Total saving	Total income	PCA of nutrition	PCA of decision-making
	(1)	(2)	(3)	(4)	(5)	(6)
Woman	-.07	.01	-.06	.03	-.04	1.07***
	(.08)	(.01)	(.12)	(.09)	(.10)	(.21)
Dwelling type: good quality	.19**	-.04**	.11	.13	.19*	.28
	(.08)	(.02)	(.12)	(.11)	(.10)	(.25)
Women ×	.06	.01	.22	-.07	.07	-.43
Dwelling type: good quality	(.12)	(.02)	(.16)	(.14)	(.14)	(.40)
Woman	-.07	.02	.25**	-.03	.03	1.30***
	(.09)	(.02)	(.13)	(.08)	(.11)	(.24)
Expenditure: above median	-.03	-.02	.18	.20**	.38***	.37
	(.11)	(.02)	(.12)	(.10)	(.10)	(.27)
Women ×	.08	-.02	-.38**	.07	-.06	-.79**
Expenditure: above median	(.13)	(.02)	(.17)	(.13)	(.15)	(.34)
Woman	.00	.01	.08	.04	.01	.90***
	(.08)	(.01)	(.13)	(.09)	(.09)	(.17)
Literate	.04	-.03*	-.03	.17*	-.03	-.22
	(.08)	(.02)	(.13)	(.10)	(.10)	(.22)
Women ×	-.05	-.02	-.11	.08	-.08	-.27
Literate	(.14)	(.02)	(.19)	(.14)	(.17)	(.28)
Woman	-.02	.01	.04	.04	.04	.91***
	(.06)	(.01)	(.09)	(.07)	(.08)	(.16)
Mother-in-law	.28**	-.06***	-.10	.44***	.38***	-.24
	(.11)	(.02)	(.13)	(.14)	(.11)	(.44)
Women ×	-.04	.01	.16	-.26	-.21	-.24
Mother-in-law	(.16)	(.03)	(.18)	(.18)	(.19)	(.57)
Mean Control	7.18	.47	8.09	6.40	0.00	-.82
Obs.	6556	6556	6556	6556	6556	518

See Table 2 for a description of the outcome variables, control variables and calculation of the standard errors. Each block presents the heterogeneous effect for a different baseline characteristic: (i) the household lives in a house with a robust construction and (ii) the household has above median baseline expenditure, (iii) the respondent is literate and (iv) the mother-in-law is a household member. $* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$.

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