Female Empowerment and the Intrinsic Demand for Agency: Experimental Evidence from Nigeria*

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March 2022

Abstract

Most studies of intrahousehold bargaining examine outcomes. We conduct an original lab-in-the-field experiment on the decision-making process of couples over the allocation of household rival and non-rival goods. The experiment measures individual preferences over allocations and traces the process of consultation, communication, deferral, and accommodation by which spouses implement these preferences. We find few differences in the spouse's individual preferences over allocations. But wives and husbands have strong preferences over process: women prefer to defer budget allocation decisions to their husband even when deferral is costly and is not observed by the husband; the reverse is true for husbands. Since the study is paired with a randomized cash transfer, we estimate the effect of treatment on the demand for agency among women. We find that receiving cash transfers over several months does not change women's bargaining process, except when the decision to defer is hidden from the husband. In that case, having received the transfer in the past increases female demand for agency: women in the cash transfer treatment want to secretly make their own budget allocation decisions, even if it is the same as their husband's.

*This paper is an early draft and should not be cited without permission. We thank Pam Jakiela, Mushfiq Mobarak, Gautam Bastian, Eliana Carranza, Neslihan Uler, Joao Montalvao and Michael O'Sullivan for their helpful comments and guidance. We gratefully acknowledge Anjuman Ara Begum, Sk Md Bakhtiar Hossain, Tonima Tasnim Ananna, Tihitina Andarge, Julian Gomez, Marietou Sanogo, Haruna Sani and Garba Sallu for their support. Lawal Ishaq Simiyat, Oti Iheanyi, Ayuba Umar, Aaron David and Christy Shanding gave extraordinary assistance in carrying out the fieldwork. We are grateful to the Africa Gender Innovation Lab at the World Bank and the Bruce and Mary Ann Gardner Dissertation Enhancement Fellowship at the University of Maryland for funding the fieldwork of the lab-in-the-field experiments. The pre-analysis plan for this paper can be found at https://www.socialscienceregistry.org/trials/3360. The laboratory experiment was approved by the IRB at the University of Maryland: 1352047-1

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1 Introduction

Making progress on gender equality remains an important objective for policymakers. In addition to concerns about equal opportunity in the workforce and other public settings, research has examined on gender inequality within the household. Partly because it is easier to measure, this literature has focused primarily on the *outcomes* experienced by women (see Duflo, 2012, for a review). Intrahousehold inequality in consumption and health outcomes between men and women, for instance, has been documented worldwide (Altonji and Blank, 1999; Azmat et al., 2006; Bertrand and Hallock, 2001)(Beaman et al., 2017; Vasilaky and Leonard, 2018). In addition, large gains in measurable outcomes have been documented for women, children, and even the whole household, when women in poor households are provided access to additional resources, usually cash. Money given to women has been shown more likely to be used for investments in education, children's nutrition, and housing than money in the hands of their husbands (Adato et al., 2000; Duflo, 2003; Fiszbein et al., 2009; Hoddinott and Haddad, 1995; Thomas, 1994). Loans to women are more likely to be repaid (de Aghion and Morduch, 2004) and increasing women's say in family finances has been shown to raise savings and investment (Armendáriz and Morduch, 2010; Ashraf, 2009; Duflo, 2012). Evaluations of conditional cash transfer (CCT) programs have further shown that transfers targeted to women can, in some cases, decrease the incidence of intimate partner violence (see Angelucci (2008); Bobonis et al. (2013) in Mexico; Hidrobo et al. (2016) in Ecuador; Roy et al. (2018) in Bangladesh, Perova and Vakis (2013) in Peru; and Haushofer and Shapiro (2016) in Kenya).[1]

There is less work in economics on the *process* by which decisions are made within the household. The dominant economic models of intrahousehold decision-making assume that the utility function of individual members is solely defined over material outcomes (e.g., (Browning and Chiappori, 1998; Chiappori, 1988, 1992, 1997; Chiappori et al., 2002, 1998; McElroy, 1990; McElroy and Horney, 1981)). This assumption implies that individuals do not care about the way these outcomes are achieved. In contrast, the non-economic literature has paid more attention to the decision-making process, particularly whether women have executive agency and whether external interventions can improve female agency within the household. The emphasis nonetheless remains on the instrumental value of agency, that is, on its capacity to affect material outcomes. Agency per se is seen as having no intrinsic value. When this assumption is combined with a symmetry assumption about the other-regarding preferences of the spouses,[2] it predicts that equality in bargaining power implies

[1] In some cases transfers to women have had no effect or even a positive effect on IPV (references from Nina Buchmann's papers).

[2] E.g., spouses only derive utility from their own (rival) consumption; or they have symmetrical altrustic

equality in outcomes. It follows that the welfare weights or bargaining power of spouses can be summarized by a sharing rule and thus inferred from the intrahousehold division of rival consumption (e.g., Dunbar et al. 2013; Brown et al. 2020).

This approach denies any role for the process by which intrahousehold decisions are reached. It does not matter, for instance, that the items consumed by a married woman are all decided by a dictatorial but benevolent husband, as long as they correspond to the preferences of that woman. With a few exceptions (e.g., Fernandez et al. (2015)[3] and Afzal et al. 2021), the simple question of whether empowerment per se is valued by women (similar to the 'capabilities' view of Nussbaum, 2001; Sen, 1999) has been mostly ignored in the literature on households.

This paper presents results from an original lab-in-the-field experiment conducted with married couples in a rural and traditional developing country setting where women's agency and rights are severely constrained. We combine traditional elements of economic laboratory design (experimentally assigned controls and separation of individuals) with field elements (a random sample of wives in very poor households received significant cash transfers for the 18 months preceding the study) and unique lab–in–the–field elements designed by the team (three laboratory shops with separate categories of female, male and household items). Finally, the experiment includes a randomly assigned 'secret' treatment in which most the decisions of both husbands and wives are shrouded so neither can tell what their spouse chose or what processes they followed to reach that decision.

Using these elements, we can examine a number of important features of decision–making in this context. First, we test, in a controlled environment, whether the randomized treatment intended to empower women increases their desire for private consumption and leads to their higher material welfare. Secondly, we test whether this treatment affects subjects' willingness to exert agency or to relinquish control to their spouse, either secretly or openly. We do so in a way that enables us to indirectly test whether subjects' willingness to pay for agency is driven primarily by instrumental value, or whether they deviate from taking the action that would maximize their own material utility. Third, we test whether the experiment increased or decreased efficiency in decision making. Note that the experiment is designed to identify the effect of an unconditional cash transfer (UCT) program on both material outcomes and the process by which these outcomes are reached. We can observe key mechanics of the joint decision-making process, including consultation, communication (honest or dissimulated), deferral, revision and accommodation of other spouse's preferences as well as a measure of the willingness to pay for agency over own consumption.

preferences with identical welfare weights.

[3]Fernandez et al. (2015) examines the correlation between subjective well-being and the right to make particular decisions. Although provocative, the results are not identified.

We find that the average social preferences (over outcomes) are not very different between men and women; women care slightly more about goods they can buy in the experiment and men care slightly more about cash, but overall preferences are quite egalitarian and not different from standard dictator games results. However, individual social preferences differ significantly within households: couples do not agree before they are allowed to communicate. We included a game in which subjects can choose between two types of drink and two types of cookie (following Afzal et al. (2018)) and, as in that paper, we find subjects are no more likely than random to pick for their spouse what their spouse chose for themselves. Thus, households are not, in general, choosing selfish allocations, but nor are they acting in a way that suggests they know what their spouse would want to do. Despite what seems like a lack of overt disagreement, there are strong preferences over the way decisions are made. Women are far more likely than men to consult their husbands, defer their decision to their husbands and accommodate their husband's wishes. Men tend to make a decision and neither seek nor accommodate their wife's wishes. Furthermore, neither husbands or wives are choosing efficient outcomes; instead of allocating resources efficiently to produce the largest possible pie, they prefer to allocate resources directly—but not selfishly—ensuring a smaller pie.

The design of the experiment allows us to conclude that these preferences over process are not idle decisions. Women over-defer and men under-defer compared to optimal decisions. Interestingly we found that women who chose not to seek their husband's opinion on a choice nonetheless accommodate when they are presented with the information. This is reminiscent of DellaVigna et al. (2012) in which subjects were told that collectors of charitable donations would knock on their door the next day, reducing the likelihood that they would answer the door as well as total giving. We find weak evidence that women dissimulate their true preferences, but not by very much. Men report their true preferences to their wives.

The UCT element of the experiment had a small effect on the social preferences of men and women; women allocate more goods or money to themselves if treated by UCT and men in treated households allocate less. As a result, the gap between their social preferences increases. Treated women were less likely to defer their choice to their husbands, but only when their deferral decision was shrouded and therefore secret. UCT treated women were slightly less willing to consult. Taken together, these suggest a slight improvement in procedural empowerment, but not one that emboldens them to challenge the authority of their husband; the need for secrecy is reminiscent of Jakiela and Ozier (2016).

Several recent studies have proposed and tested alternative quantitative measures of women's empowerment within a household using experimental techniques. Lab-in-the-field experiments have been carried out in low-income settings to measure demand for agency and willingness to pay for agency in different types of joint decision-making within a household

(e.g., Afzal et al., 2018; Almås et al., 2018; Iversen et al., 2011; Jakiela and Ozier, 2016; Mani, 2011; Schaner, 2016, .) There are, however, only a few studies that look into the effect of a cash transfer on experimental measures of female empowerment. One such study was by Almås et al. (2018), who report results from a lab-in-the-field experiment in North Macedonia where a CCT (for children staying in school) was randomly allocated to either the head of household (usually a man) or a woman. They find that the female recipients of this long-term CCT — compared to those whose husbands were chosen as the recipients — exhibit a lower willingness to pay (WTP) to appropriate an additional windfall themselves instead of their spouses. This, they argue, is picking up an improved bargaining power of the CCT-receiving women: these women are willing to pay less than others because they have more say over the allocation of the windfall even if it is received by their husband. However, other channels in addition to an improved bargaining power could explain why CCT-receiving women exhibit a lower WTP to receive a cash windfall (instead of their spouse).[4]

In the following section, we outline the setting and details of our lab experiment, and in Section 3, we explain how we use the experimental design to develop a strategy for examining the behavior of households. Our empirical work is divided into two sections. First, in Section 4 we examine the behavior of the households in the control group, a baseline of behavior. Second, in Section 5 we examine how this behavior was changed by the UCT experiment. Finally, in Section 6, we conclude.

2 Experimental design

We conducted a lab experiment with married couples in two rural regions in the state of Kebbi, in north-western Nigeria. The region is known for being strongly patriarchal, and women in this setting experience very low levels of agency and some of the poorest health outcomes in the world (Braimah, 2014; Ogu et al., 2016).

To address these inequalities, the World Bank's Africa Gender Innovation Lab (GIL) designed an unconditional cash transfer (UCT) that was carried out as a randomized controlled trial by Catholic Relief Services (CRS) Nigeria. A baseline study was undertaken in 27 villages to identify extremely vulnerable households using the Progress Out of Poverty Index (PPI). Households identified as vulnerable were then randomly assigned to either receive the

[4]For example, fairness norms could explain why women who were beneficiaries of a CCT program are less likely to object to a new transfer to be given to their spouse instead of receiving it themselves again. Moreover, former recipients of a CCT might want to avoid 'taxation' from family and relatives (see Jakiela and Ozier, 2016) after experiencing it first-hand and may rather have their husbands deal with such attempts. This issue is salient in Almås et al. (2018) because the information on being a recipient of the CCT program was common knowledge as the program was implemented in a visible/public way.

UCT treatment or to serve as controls.[5] The UCT paid 75,000 Nigerian Naira (roughly USD 693 PPP) over a period of fifteen months ending in March 2017, delivered to the primary female decision-maker in the household.

One year after the UCT intervention ended, baseline respondents were invited as married couples to participate to lab experiments that took place between March and May of 2018.[6] By removing pure income effects from the UCT, the one-year gap between the RCT and our experiment serves to identify longer-term effects in decision–making within the household.

We now summarize the protocol followed in the lab experiment. Participants are first informed that, in addition to a show–up fee, one of all the decisions made by both husband and wife that day will be selected for implementation and pay-out. Husbands and wives are then sent to two separate gender-specific rooms where the bulk of the experiment takes place. The female room only includes female enumerators; the male room only has male enumerators.

As is common in settings where illiteracy is widespread, subjects are not asked to read instructions, write down their choices, or use any device such as a computer or tablet. Rather, all instructions are read to subjects by an enumerator who sits with each subject and asks them to make decisions by pointing to pictures that have been associated with choices (see Figure B2). This setup allows us to reach a wide range of subjects and avoids participants revealing their choices to others verbally. When allocating money to different decisions or budgets, participants use laminated photocopies of Naira denominations with which they are familiar.

2.1 Allocations and lab stalls

The experiment is designed to observe decision-making over the allocation of resources within couples. One difficulty is that any cash given to husband and wife during the experiment can be reallocated between them after the experiment in a way we do not observe, possibly undoing any allocation made in the lab. To avoid this, in most games subjects are presented with a small budget and asked how they wish to allocate it to pre-stipulated categories of

[5]Villages were randomly assigned to become either a "UCT-Only" village or a "Program+UCT" village. In both types of villages, households were randomized into receiving or not receiving the UCT. However, in "Program+UCT" villages, all eligible households had access to additional program components, which included nutrition messages, savings groups, farmer's groups, etc. This means that the control households in the "Program+UCT" villages were exposed to these programmatic components while the control households in the (pure) UCT villages were not exposed to either the UCT or any programmatic component.

[6]The UCT treatment was assigned to specific women and we invited those women and their husbands to the experiment. However, in a polygamous marriage setting, we cannot be certain that the husband attended with the wife who was assigned to the UCT treatment. All control households have no women in the UCT treatment, but it is possible that some of the women we assign to the UCT treatment, did not in fact receive a cash transfer.

goods they can only purchase from us at the end of the experiment. Once part of a budget has been assigned to a particular stall, that budget cannot be converted into cash or spent in another stall. To facilitate comparison with other experiments in which spouses are given cash, we also include a separate allocation decision in which subjects divide a cash amount between themselves and their spouse.

To implement this design, three market stalls are constructed in the lab, each of which contains only one category of items. These items were pre-tested to be recognizable and desirable, and they include goods that are easy to buy locally and items only available in town. All participants are shown pictures of the items available in each stall at the beginning of the experiment. One stall includes goods intended to appeal to women: jewelry, colorful fabric, and traditional dresses (see Figure B3). The second stall includes male-oriented goods such as hats, caps, belts, shoes, and fabric (see Figure B4). The third stall offers common household items such as cleaning supplies, mats, cups, plates, and mosquito coils (see Figure B5). At the end of the experiment, husband and wife are reunited, one allocation decision is randomly chosen to be implemented, and couples are given vouchers redeemable in specific stalls. This means that when a subject allocates funds to a stall expected to appeal to their spouse, the subject may intend to purchase something for their spouse or to let their spouse choose — in the context of this experiment, both choices are observationally equivalent. No attempt is made to record the specific items that subjects purchase with their vouchers in each stall.

By making two of the stalls gender-specific, the above design severly limits the scope for ex post reallocation of the objects between spouses. There still remains the possibility that subjects sell or give the objects to others after the experiment. To avoid this, we include a game in which subjects separately consume a food and drink of their choice directly in the lab, as in Afzal et al. 2021. In that case, reallocation across spouses is impossible.

Throughout the experiment, each spouse answers various allocation decisions regarding their preferred budgetary allocations over four possible choice pairs:

A female vs male items purchased in the lab stalls

B household items vs male items purchased in the lab stalls

C household items vs female items purchased in the lab stalls

D cash for wife vs cash for husband[7]

[7]In the experiment the decision is presented as money for self or money for spouse; throughout the analysis, however, we have adapted the responses to fit the other three domains with an unchanging order. Thus, women are shown as choosing how much to give to women (themselves) and men are also shown as choosing how much to give to women (their spouse).

In each case, the subject is given a budget to divided – in multiples of 100 Nairas – between each of the two options listed above. In the rest of the paper, we refer to each of these choice sets using the letters A to D above. The food-and-drink game is only played once with real stakes – i.e., actual food and drinks given to the subject at the end of the experiment. In what follows, we denote this game with the letter J. At the end of the experiment, subjects are also asked to choose between two input allocations that determine individual incomes – see below.

2.2 Decision blocks

The experiment is divided into a series of decisions grouped into 'blocks' that each subject makes silently and in isolation from their spouse – who is in another room. In terms of sequencing, Block 1 always comes first while Blocks 5 and 6 always come last. The order of Blocks 2, 3 and 4 is permuted at random across sessions.

At the end of the experiment, one decision from one of Block1 to 4 is selected at random for each couple to determine the cash and vouchers received by the couple. This means that each decision a subject makes is fully incentivized. The details of the public selection process are provided at then end of this Section. We describe below the choices in the secret treatment where choices are hidden or cloaked from the subject's spouse. The changes made in the no-secret treatment are detailed in the next sub-section.

Block 1 *Allocating a budget across different types of items or payments* [**split** and **resplit**]: Each participant is sequentially asked to split a budget of 2500 Naira in 50 Naira increments. This is done in each of the four domains: **split**A-D. Each subjects is then asked to split a budget of 2100 Naira[8] in domains A and D: **resplit**A and **resplit**D. The order of both sets of choices is the same for all subjects in a session. But it is randomized across sessions. These choices are never revealed to the spouse. The purpose of block is thus to elicit each subject's true preferences over all possible choice pairs.

Block 2 *Choosing whether to allow one's spouse to make the decision instead of making it on one's own* [**defer**]: Subjects are reminded of their initial allocation across **split**A-D and can choose to retain this allocation or to replace it with their spouse's allocation, which they have not seen. These decisions are labeled **defer**A-D. For example, if a woman chose to give 2000 Naira (about $23) to female items and 500 to male items in **split**A, she may in this block decide to implement the split chosen by her husband. This means that if her decision from block 1 is selected to be implemented, the couple will receive a 2000 Naira voucher to spend at the female stall and a 500 Naira voucher to spend at the male stall.

[8] 1800 for subjects in the last two sessions, 10% of subjects.

But if her decision from block 2 is implemented instead, the couple will receive the voucher allocation selected by her husband. More about this later. The object of this block is to elicit subjects' willingness to defer budget allocation to their spouse. This decision is not shown to the spouse.

Block 3 *Choosing whether to allow one's spouse to make the decision instead of making it on one's own with additional costs* [**defercost**, **deferbenefit**]: Having answered Blocks 1 and 2, subjects are then reminded of their initial allocation across **resplit**A and **resplit**D and they are given a choice between their decision over a budget of 2500 or their spouse's decision over a budget of 2100 (**defercostA** and **defercostD**). In addition, subjects are given a choice between their decision over a budget of 2100 Naira or their spouse's decision over 2500 Naira. This decision applies only to domain D, splitting money between husband and wife, (**deferbenefitD**). For **defercost** it is costly to defer the decision and for **deferbenefit** it is costly to retain the decision. The purpose of this block is the elicit subjects' willingness to pay to defer – or not defer – budget allocation to their spouse. These choices are not shown to the spouse.

Block 4 *Communicating preferences, consulting over preferences and revising decisions* [**communicate**B-D, **consult**B-D, **revise**B-D] Subjects are then asked to pick an allocation across domains B, C and D that will be communicated to their spouse **communicate**B-D). The purpose of this part of the experiment is to determine whether subjects choose to dissimulate their true choices from their spouse. These choices are communicated to the spouse.

After having done this, subjects are asked, for each of the three allocation domains, if they would like to see the communication of their spouse (**consult**B-D). The purpose of this question is to ascertain whether subjects are interested in learning about their spouse's preferences – something they would want to do in case they are willing to accomodate these preferences, in part or in full. It is not revealed to the spouse whether or not the subject chooses to see their spouse's communicated choice.

Finally, whether or not the subject chose to see their spouse's communication, the subject is shown the communication of their spouse and asked if they would like to revise their split decisions made in Block 1.[9] Variable **revise**B-D records the revised decision after receiving communication: if a subject did not want to change their split, **revise** is the same as the original **split**. The purpose of this part of the experiment is to determine the extent to which each subject accommodates the revealed preferences of their spouse. Whether or not the subject revises their original choice is not revealed to the spouse.

Block 5 *Food and drink choices* [**split**F,J, **spousesplitF,J**, **defer**F] In this Block, each

[9] All subjects retained the right not to look at the information they were given.

subject is asked to select one of two cookies (Food) and one of two drinks (Juice) for themselves (**split**F and **split**J) and for their spouse (**spousesplit**F and **spousesplit**J). There is ample supply of both types of drinks and cookies so that consumption is non-rival. The purpose of this design is to elicit each subject's true preference of cookie and drink, as in Afzal et al. (2021). This true preference is not revealed to the spouse.

Having completed the first part of this Block, each subject is given the option to defer their decision over both food and juice to their spouse. This decision is now shown to the spouse, and subjects do not know what their spouse has chosen for them before deciding whether to defer. The purpose of this question is to elicit the subject's willingness to defer their private consumption of food and drink to their spouse even when that own consumption has no effect on the spouse's own consumption and their consumption is not observed by the spouse. Subject who do not defer receive the food and drink of their choice; those who defer receives the food and drink chosen for them by their spouse.

In about a third of the sessions (13 sessions with 166 subjects), there is a penalty for deferral: the subject receives one cookie and a half glass of juice of their spouse's selection if they defer; but they receive two of their preferred cookies and a full glass if they do not defer. The purpose of this design is to elicit subjects' average willingness to pay to defer their own consumption choice to their spouse.

Block 6 *Allocating inputs across two production functions* [efficiency] In this final block, the focus of the choice is not consumption but production. Each subject is asked to pick one of four possible input allocations between self and their spouse. Each input allocation maps into a cash income for self and a cash income for the spouse. Subjects answered two versions of the game.[10] The four possible choices in both games are shown in the Table below and graphed in Figure 1. Each game has an efficient choice, that is, a choice that maximizes the total income of the household. In Game 1, the subject earns less than the spouse in the efficient choice; in Game 2 the opposite is true. As for other Blocks, each spouse picks a choice without their spouse being told.

[10]Except for a small number of sessions in which only one version was presented to subjects.

Choice	Own Input	Spouse input	Own Income	Spouse Income	Total Income
Game 1: efficient means less for self than spouse					
a	10	0	1500	600	2100
b (dominated)	7	3	1200	800	2000
c (efficient)	3	7	1000	1500	2500
d	0	10	400	1900	2300
Game 2: efficient mean more for self than spouse					
a	10	0	1900	400	2300
b (efficient)	7	3	1500	1000	2500
c (dominated)	3	7	800	1200	2000
d	0	10	600	1500	2100

This game design serves four objectives:

1. First, it shows whether subjects choose an efficient allocation, and whether they do so even when it is against their individual interest. This gives us a measure of spousal altruism.

2. Second, the fact that Game 1 is a mirror image of Game 2 allows us to test whether both spouses choose the same joint allocation. For instance, choosing a in Game 1 for self is equivalent to the spouse choosing d in Game 2. This tells us whether spouses share a focal division of inputs or income within their household.

3. Third, Games 1 and 2 are identical in the input domain. Hence if subjects base their selection purely on the input domain, they should pick the same option in both games. This tells us whether spouses put input fairness considerations above considerations of efficiency or intra-household distribution of income.

4. Fourth, each game has a choice that is dominated in the output domain, that is, a choice that violates any reasonable (altruistic or selfish) utility function over *income*. This choice, however, is not dominated if subjects evaluate choices exclusively in the input domain. This tells us whether subjects make production decisions based purely on input fairness considerations, irrespective of their income consequences.

2.3 Allocation selection and shrouding

At the end of the experiment, the enumerator determines the cash or voucher allocation that is given to each couple. This allocation is partly determined by the choices made by both spouses in Blocks 1 to 4 and Block 6, and partly random for shrouding purposes. The

enumerator starts by drawing a ball indicating whether it is the choices of the wife or husband that will be implemented. The enumerator then draws a ball from a bag that contains one ball for each of the relevant decisions made by subjects, namely: four **split** balls and two **resplit** balls from Block 1; four **defer** balls from Block 2; two balls for **defercost** and **deferbenefit** from Block 3; three **revise**B-D balls from Block 4; and two efficiency balls from Block 6 (one for each game) – in total, 18 balls. If a ball is drawn from Block1 or Block4, the allocation made by the subject is selected for implementation. When the subject chooses to defer in Block2 or Block3, the selected allocation is the corresponding choice made by the spouse in Block1.[11]

After an allocation has been selected for a couple, it is *shrouded* as follows. If the selected decision is **split**, the experimenter randomly draws a number x between 0 and 2500 in 50 Naira increments. This number determines the allocation going to the first option in **split** and 2500-x is allocated to the second option. If the decision is **resplit**, the process is the same but the randomly drawn number x ranges from 0 to 2100. The experimenter then puts two envelopes in a box and randomly draws one of them. One envelope contains the subjet's choice selected as indicated in the previous paragraph; the second envelope contains the random allocation implied by the randomly drawn number x. This implies that the spouse whose choices are not selected can not infer with certainty the choice made by his or her spouse. This ensures credible deniability for the selected subject, whose actual choices are thus shrouded from their spouse. This design is clearly explained and illustrated to subjects at the beginning of the experiment, and incentivizes preference revelation. In contrast, the **communicate**B-D allocation is *always* disclosed to the spouse.

In terms of payoff distribution, if the selected allocation is in the cash domain D (e.g., the husband receives y Naira and the wife 2500-y Naira), each spouse is given their assigned monetary amount separately, *but not privately*. This is explained to all subjects at the beginning of the experiment. For selected allocations in domains A, B, and C, couples are brought together and are given the corresponding stall-specific tokens for items they can purchase from the lab. As noted earlier, we make no attempt to influence couple's choices of items within each stall – i.e., a husband can impose his selection of female goods or let his wife choose. This approach was imposed by the cultural context, for fear of alienating subjects if we opted for secrecy.

For Block5, subject who do not defer receive the food and drink of their choice and those who defer receives the food and drink chosen for them by their spouse. If a subject chooses

[11]For instance, if the subject does not defer in Block2, the selected allocation is their own **split** choice from Block1; if the subject defers, the selected allocation is the **split** choice of their spouse from Block1. Similarly, if the subject does not defer in Block3, the selected allocation is their **resplit**A, **resplit**D, or **split**D choice from Block1; if the subject defers, it is their spouse's **split**A, **split**D, or **resplit**D choice from Block1.

to defer and there is a cost for deferral, this subject only receive one cookie and half a glass of the selection made for them by their spouse. The food and drink are consumed privately by each subject in their gender-specific room, and is therefore unseen by their spouse. This ensures that the spouse cannot determine what the subject selected for themselves.

For Block6, shrouding is achieved in a manner similar to as follows **split**. As for Blocks 1 to 4, the experimenter first draw a spouse at random and a game at random from the set $\{1, 2\}$. The experimenter then randomly draws one possible payoff allocation between spouses from the set $\{a, b, c, d\}$ for that game. The experimenter puts two envelopes in a box and randomly draws one of them. One envelope contains the allocation corresponding to the production choice of the selected spouse in the selected game; the second envelope contains the random allocation implied by the randomly drawn allocation in $\{a, b, c, d\}$ in that game. This design ensures credible deniability and is clearly explained to subjects at the beginning of the experiment.

2.4 No-secret treatment

We wish to ascertain the effect of spousal knowledge on the *deferral* and *accommodation* choices made by subjects in Blocks 2 to 4, while continuing to incentivize true preference revelation in Block1 – and similarly for Block5.

To this effect, we divide the sessions equally into a secret treatment, which is what we have described so far, and a no-secret treatment. Block1 is identical in both treatments, meaning that we continue to shroud the **split** and **resplit** decisions made in Block1 to ensure credible deniability. The same holds for Block 6, the production games. This means that Blocks 1 and 6 are identical under the secret and no-secret treatments.

In Blocks 2 and 3, subjects are told that their *deferral* decisions will be revealed to their spouse. If at the end of the experiment the experimenter draws a ball for Block 2 or 3 for subject i, the spouse of subject i is told *whether* i deferred the decision to the spouse in that game. The chosen allocation, however, remains shrouded as before, i.e., by using two envelopes as explained in the previous sub-section. The only revealed deferral decision is that for the chosen subject in the chosen domain and Block. This means that each deferral decision is fully incentivized in the sense that, in the no-secret treatment, it is disclosed to the spouse with strictly positive probability.

Block4 is where the difference between the secret and no-secret treatments is the largest. In the no-secret treatment, there is no random selection of two envelopes. If Block4 is selected for subject i at the end of the session, i's spouse is first told whether i chose to consult the spouse's communicated choice **communicate**B-D and is then told i's revised allocation **revise**. Given that the spouse knows his or her own communicated allocation

communicateB-D, this enables the spouse to observe how closely subject i accommodates these communicated preferences. The spouse, however, is *not* told whether subject i modified his or her allocation after seeing the spouse's communicated preferred allocation **communicate**B-D. Consequently, the spouse cannot determine whether i's **revise** choice is identical to – or different from – i's secret preferences **split** revealed in Block1. Hence the secrecy of i's **split** allocation to the spouse is maintained. All this is made clear to subjects, i.e., in the no-secret treatment each subject is told that the decisions **consult**B-D and **revise**B-D that they make for domain j in Block4 will be revealed to their spouse if the selected ball corresponds to domain j in Block4.

In Block5, the food and drink game, the decision to defer is always revealed to the spouse in the no-secret treatment, and never revealed in the secret treatment. In both treatments, subjects only learn what their spouse chose for them if they defer. But the selected food and drink are consumed in private and are thus not observed by the spouse.

3 Testing strategy

3.1 Conceptual framework

As mentioned in the introduction, the literature on the empowerment of women within the household has followed two main tracks. The first focuses on the relative material welfare of husband and wife (see, for example, recent papers on Engel curves within households by Bargain and Donni, 2012; Bargain et al., 2014; Brown et al., 2020; Calvi, 2020; Dunbar et al., 2013; Lechene et al., 2019; Sokullu and Valente, 2018; Tommasi, 2019). The second concentrates on the power structure within the couple. Starting with Becker (1964, 1981); McElroy and Horney (1981), and following with Chiappori (1988) and Lundberg and Pollak (1993), the economic literature has merged the two tracks into a standard model of intrahousehold consumption allocation in which the decision of the household can be represented as the solution to a maximization problem:

$$Max_x\ \omega U_h(x) + (1-\omega)U_w(x) \text{ subject to } px = y$$

where x is the combined consumption vector of the household, $U_h(x)$ is the utility that the husband derives from the household consuming x, $U_w(x)$ is the utility of the wife, p is a price vector, and y is household income. In many applications, $U_i(x)$ for $i = \{h, w\}$ is assumed to take the form $U_i(x_i, x_c)$ where x_i represents a vector of rival goods consumed by individual i (e.g., food, clothing, health care) and x_c is a vector of non-rival household public goods consumed by both spouses.

The relative welfare weights of the husband and wife, represented by parameter ω, are seen as summarizing the extent to which the bargaining power of the spouses affects whose interests are best served by how the household allocates consumption. These welfare weights are typically thought to be affected by factors influencing the exit option of both spouses, either upon dissolution of the household (e.g. McElroy and Horney, 1981) or during marriage when the spouses stop cooperating (Lundberg and Pollak, 1993). Empirical evidence exists to suggest that both types of factors affect the intrahousehold allocation of consumption (for example, Lundberg et al. (1997) on child welfare transfers and Browning et al. (1994) on differences in divorce law across time and US states). When the utility of each spouse takes the form $U_i(x_i, x_c)$, more bargaining power to individual i translates into more consumption of rival goods by that individual. Policy interventions inspired by this framework seek to manipulate intrahousehold bargaining power so as to re-balance household consumption in favor of the weaker member, typically the wife.

In this framework, having the power to influence intrahousehold decisions only matters because it has instrumental value: it increases the share of the household budget devoted to the material welfare of the person who exercises that power. Agency over one's own consumption has no *intrinsic* value. Furthermore, the framework assumes that, except for the instrumental benefits that agency, control, or evasion bring, one household member derives no satisfaction either from exerting control on another (For a deeper understanding of the difference between instrumental and intrinsic control, see the IPV literature Angelucci, 2008; Bobonis et al., 2013; Haushofer and Shapiro, 2016; Hidrobo et al., 2016; Perova and Vakis, 2013; Roy et al., 2018, e.g.), or from evading that control (Ashraf, 2009; Jakiela and Ozier, 2016, e.g.). In other words, the process by which a particular intrahousehold allocation is achieved does not matter for itself; only the end-result does.

In contrast, the non-economic literature on empowerment (Nussbaum, 2001; Sen, 1999) is more likely to recognize that people care about due process: they like to have a say in household affairs and national policy decisions and they value having control over their own destiny. In some cases, they even relish having control over the actions and welfare of others (e.g., spouse, children), even when these actions have no impact on their own material welfare. To achieve this, people often demonstrate a willingness to incur costs and to take risks that reduce their material welfare and, possibly, that of others – as in the case of IPV, for instance (e.g., Afzal et al., 2018).

3.2 Purpose of the experimental design

Our experiment is designed to provide relevant evidence on both tracks: does the Unconditional Cash Transfer (UCT) treatment improve the material welfare of wives; and does

the treatment improve their decision power? More importantly, we go beyond investigating these two issues in a reduced form, exploiting our experimental design to examine each of the steps in the couple's decision chain. We start by asking whether members of the household have different revealed preferences. This step is usually ignored in other studies: if spouses have relatively similar social preferences about household expenditures, an intervention that affects the power each has will not affect intrahousehold consumption allocation.

We then decompose the decision process of the couple into several steps, each of which is required for a change in social preferences to affect household consumption. This decomposition is intended to mimic, in a stylized but structured way, the essential constituent steps of intrahousehold bargaining. Note that none of these steps could be observed if we examined outcomes only.

First we examine whether spouses communicate their preferences truthfully to each other. This is the first logical step in allowing the spouses to converge to an agreed-upon higher consumption allocation for women. If wives are scared of telling their husbands they want more, it is unlikely they will get more.

Second, we examine whether husbands and wives wish to know what their spouse wants. If they do not want to know, they probably have no intention of accommodating their spouse's wishes. Hence if wives want more and are willing to tell their husband, but the husband does not want to hear it, this will break the causal chain between treatment and women's welfare.

Third, having communicated their desired consumption allocation to their husband, do women want to have power over the final decision or do they prefer to delegate this decision to their husband? If women prefer to delegate – and treatment has no effect on the social preferences of husbands – women renounce having a direct effect on consumption decisions, which too can break the causal chain.

Fourth, even if all decision power over household consumption allocation has been delegated to the husband, it remains possible for the husband to behave in a paternalistic way by accommodating his wife's communicated preferences. We test this directly by comparing the revised consumption allocation choice participants make after having been informed on their spouse's choice. It is indeed conceivable that a subject does not wish to be informed of his or her spouse's preference but, once informed, cannot resist the mental pressure of accommodating their wishes. This phenomenon, if observed, would be reminiscent of the findings of DellaVigna et al. (2012) whose subjects are less likely to answer the door for a team collecting philanthropic donations when they have been forewarned of the visit beforehand.

3.3 Budget allocation decisions

At the heart of our design is a set of budget allocation decisions that subjects are asked to make under several treatment conditions. In the first part of the experiment, each spouse independently and secretly chooses how to split a budget between two categories of goods. This choice by the subject is never revealed to their spouse but it has a non-zero probability of being implemented, making revealed preferences incentive compatible. Given this, choices made in this first step can be seen as defining the social utility of each spouse if he/she were given full control over household consumption – i.e., they measure $U_i(x)$ for a particular x vector and budget $y = 2500$. In choices A and D, the budget is split between the spouses.

Choice A is made over specific physical goods that can only be purchased within the confines of the lab, thereby reducing the chance of fungibility with consumption expenditures outside the experiment. It measures the social utility $U_i(c_w, c_h)$ of consumption bundles c_w and c_h for individuals $i = \{w, h\}$.

Choice D is over money and can thus potentially be undone outside the lab. But it offers the advantage that consumption is not restricted to the goods on sale in the lab. It measures the utility $U_i(x_w, x_h)$ of consumption budgets x_w and x_h for individuals $i = \{w, h\}$.

Choices B and C are between private goods and household goods, thereby revealing how husbands and wives differ in the extent to which they care for household public goods, including child consumption (x_c). They measure $U_i(x_w, x_c)$ and $U_i(x_h, x_c)$, respectively.

To investigate the effect of the UCT treatment and the secret condition in the lab on each of these social utilities, we estimate a model, in equation 1, that examines the interaction of secret and treatment as four independent categories and also, in equation 2, as the interaction of two treatments (S and T).

$$w_i = \beta_0 + \beta_1'[T_i^1 S_i^0] + \beta_2'[T_i^0 S_1^1] + \beta_3[T_i^1 S_i^1] + u_i \quad (1)$$

$$w_i = \beta_0' + \beta_1' T_i + \beta_2' S_i + \beta_3' T_i S_i + u_i \quad (2)$$

where w_i is the **split** budget allocation made by the subject in each of the four possible split decisions A, B, C and D; variables T_i and S_i indicate the UCT treatment and secret laboratory condition and u_i is an error term which we cluster at the level of the session to control for unobserved session effects that would lead to correlated effects. Equation 1 assumes four mutually exclusive treatment categories, with $T_i = 0$ and $S_i = 0$ as the omitted category. Equation 2 considers the treatments directly with $T_i S_i$ an interaction term. Note that $\beta_1 = \beta_1'$, $\beta_2 = \beta_2'$ and $\beta_1' + \beta_2' + \beta_3' = \beta_3$. Since the two regressions almost exactly the same, we only report the coefficients for equation 1 and the p-value of the coefficient β_3'.

Similar regressions are estimated for the two subsequent allocation decisions subjects are

asked to make. The first of these two is the consumption split that subjects provide when asked what allocation we should communicate to their spouse (**communicate**B-D). The second is the revised consumption split that subjects provide when have been told what split their spouse communicated to them (**revise**B-D). The purpose of this is to investigate whether any effect that the treatment may have on the desired allocation of each spouse is communicated to the spouse and also affects the allocation made by that spouse. As noted above, these steps are essential to see whether changes in social preferences translate into an actual reshuffling of intrahousehold consumption allocation. For memory, **split** is always secret, **communicate**B-D is never secret, and **revise**B-D is secret in the secret condition.

3.4 Decision process

Four variables are used to characterize the decision process of the couple. Two of these are measured directly; the other two are constructed from the sequence of decisions described above. The first of these measures is deferral (**defer**). We ask subjects whether they wish to *defer* the final allocation to their spouse. In the secret condition this deferral decision cannot be discovered by the spouse; otherwise it is. We expect the secret condition to potentially have an effect on deferral: if a wife is enticed to demand more agency as a result of treatment but is afraid of retribution if it is revealed that she did not defer full executive agency over consumption decisions to her husband, she may refrain from deferring only in the secret condition.

The second directly observed measure is the decision to *consult* (**consult**B-D): subjects are asked whether they wish to be told what allocation their spouse chose. If treatment makes wives more willing to exert executive agency, we should observe a negative effect on the willingness to consult. The act of consulting is always secret, but individuals may be planning to accommodate and therefore believe consulting is necessary.

The third measure in our experimental decision tree is the decision to *dissimulate* one's true preferences from the spouse. This is measured as the difference between the communicated split and the original split: if treatment makes women more willing to reveal their true preferences to husbands, we expect treated wives to report a higher consumption share for themselves for choices C and D.[12]

The fourth decision is whether to *accommodate* or dismiss the preferred split communicated by the spouse. Subjects may 'stick to their guns' and keep their original split choice even if it diverges from their spouse's; or they may opt to partially or fully accommodate their spouse's wishes. We regard this measure as the closest to the concept of 'procedural

[12]For choice B, the wife may prefer to allocate more to household than husband consumption.

empowerment', meaning that a subject feels entitled to impose his or her consumption allocation preferences on their spouse. This form of empowerment can be strongly dissociated from material welfare if spouses have similar social preferences, i.e., if they tend to divide consumption fairly equally irrespective of who makes the decision. To capture this idea, we construct a categorical variable that compares the revised split to the original split. If the subjects 'sticks to their guns', then their revised split is the same as their original split; if they fully accommodate their spouse's choice, their revised split is equal to the communicated split of their spouse. Partial accommodation is when the revised decision goes in the direction of the spouse's choice, but not completely. Over-accommodation is when the subject's revised allocation overshoots their spouse's. It is also conceivable that subjects are contrarian in the sense that they revise their allocation away from their spouse's preferred choice.

The three dichotomous measures of procedural empowerment – defer, consult, and accommodate – are regressed on treatment. If the UCT treatment empowers women in a procedural sense and makes them more openly assertive, we should observe that their decisions to defer, consult, and accommodate all fall with treatment. The need to dissimulate would also fall. Furthermore, if the treatment also reduces the power of husbands, or makes them take their wife's wishes into consideration, we should observe increases in the rate in which men defer, consult, and accommodate.

It is also conceivable that the UCT treatment emboldens women only when their decisions are unobservable to the husband – i.e., it empowers women to secretly exert more agency whenever the opportunity arises, as in Jakiela and Ozier (2012). Our experimental design allows us to investigate this possibility by comparing women's decision to defer depending on whether it is revealed to the husband or not. If women are only empowered in secret, then there will be no change treated women in the open (non-secret) treatment but we will observe changes in the treated women in the secret condition. This distinction does not apply to the decisions to consult since that is always kept private.[13] Hence if UCT-treated women are unwilling to openly exert agency, we should only observe a response to the UCT treatment in the secret condition.

3.5 Non-instrumental demand for agency

The discussion so far has focused on decisions where the interests of the two spouses are potentially divergent. In this context, agency has instrumental value because it allows each

[13]The constructed variable indicating dissimulation involves elements (the initial allocation, **split**) that are always secret and elements (**revise**) that are only secret in the secret condition. The decision to fully accommodate is potentially observed by the spouse in the no-secret condition because a spouse could observe that the final allocation is not equal to his or her communication.

spouse to allocate the household's consumption budget in a way more in line with their preferences.

To investigate the possibility of non-instrumental demand for agency, we examine the choices of food and drink that they make in Block5. The stated preferences **split**F and **split**J measure which of the four private consumption bundles c_i, has the highest private utility $U_i(c_i)$ for individual i. Since consumption is non-rival – what the husband consumes does not affect the wife's choice set, and vice versa – interfering with the consumption decision of a spouse has no instrumental value. Hence if i defers his/her consumption decision to spouse j, this action cannot increase that spouse's material utility $U_j(c_j)$. It can only satisfy j's desire for control (e.g., Afzal et al. 2021). This gives us a clean measure of deferral for non-instrumental reasons, i.e., as a way for i to increase j's non-material utility from the decision process itself.

The experimental design also allows us additional tests regarding instrumental reasons for agency. In the split decision, we ask subjects whether they wish to either split a given budget for themselves or defer the split to their spouse with a larger or a smaller budget. To the extent that subjects have an idea of how their spouse will split their budget, this gives us a way of bracketing the role of instrumental motivations in deferral decisions. We also introduce a cost-of-agency treatment in the drink-and-cookie game, whereby subjects can decide to either consume half a drink and one cookie that they choose for themselves, or to defer the choice to their spouse and receive a full drink and two cookies chosen by the spouse. Using these prices, we can estimate a demand function for agency separately for husbands and for wives.

Given this, the experimental design allows us to test whether demand for agency shifts outward in the UCT treatment or in the secret condition. This can be done using the same regression framework as shown in equations 1 and 2 above.

3.6 Allocative efficiency in production

So far we have focused on consumption decisions that are the object of Blocks 1 to 5. We now turn to Block6, which allows allocative inefficiency in production, either due to a desire to increase one's individual income, or driven by fairness considerations in input allocation. Most of the theoretical literature on intra-household allocation implicitly or explicitly assumes efficiency in production decisions. Yet we know (e.g., Udry (1996)) that input endowment effects can impede production efficiency of households in Africa (see also Fafchamps and Kebede, 2020).

The two production games played in Block6 examine whether subjects make decisions that are efficient for the couple. The games are a simplified version of the context of Udry

(1996) in which men and women farm separate plots and fertilizer should be allocated across the plots to achieve the maximum level of output. In one game, maximizing household production requires giving more input to the spouse and in the other, maximizing household production requires giving more input to oneself. Decisions made in Block 6 are always shrouded as explained in Section 2.

We examine the impact of the UCT treatment and other experimental conditions on the efficiency of couples production decisions. Based on the existing literature, we have no reason to believe the UCT treatment should increase or decrease efficiency, since theory suggests efficiency is a dominant strategy in all households, irrespective of the decision-making process. Moreover, the game does not allow deferring decisions to one's spouse, so there is no way to express or avoid agency – and thus no possible effect of the UCT through demand for agency.

4 Gender differences in the control group

To properly appreciate the impact of the UCT intervention, it is essential that we understand the behavioral patterns of experimental participants in the control group, that is, those who did not receive the UCT treatment. We examine the allocations themselves, the process of decision-making, and the efficiency of the decisions.

4.1 Budget allocation decisions

We present, in Table 1, summary statistics of all the key behavioral variables, broken down by gender. A t-statistic for the test that the means (pairwise within couples) are equal is also provided, together with the associated p-value.[14]

The first panel of Table 1 shows the four main split decisions with the full budget of 2500 Naira, as well as the two resplit decisions with a reduced budget. The number of observations varies somewhat across variables because some choices were not included in early sessions. We see that most split decisions are significantly different within couples, but the differences between men and women are not large in magnitude. On average, husbands and wives tend to divide budgets more or less equally for all four splitting decisions (1250 would be an equal split) – a finding reminiscent of equal sharing in dictator games. As illustrated in Figure 2, however, these averages hide a lot of variation across couples. The figures also show that while women tend to allocate a larger budget share to female goods sold in the lab (1298>1250), men allocate more money to themselves (1062<1250), to be spent outside the lab. This may reflect the fact that the items sold in the lab shop appeal more to women than men. Whatever

[14]Because each husband and wife pair is a unit, we can compare the allocations, not across the whole sample, but within couples.

the reason, the two panels of Figure 2 are more or less mirror images of each other, suggesting a relatively equal division of expenditures across spouses. This is further confirmed in Table 2, where we compare the share that each subject allocates to himself/herself compared to the share they allocate to their spouse. We see that while husbands appropriate less in the female vs male goods split, they appropriate more in the money split – and vice versa for women.

Given that these splitting decisions were always shrouded to ensure credible deniability, the findings violate the idea that, given the opportunity, spouses would like to appropriate a large share of the offered budget to themselves. This is true of women but also of men who, as we shall see shortly, wield most of the power in the our sample population. From this evidence we conclude that spouses have social preferences, meaning that they incorporate the expenditures of each other in their own utility function. We also note that women do not, contrary to common perception, wish to spend much more than men on household goods.

While it is true that husbands and wives have relatively similar divisions of expenditures *on average*, the same does not hold *within* individual households. We show, in Figure 3, the cumulative distribution of within-couple differences between split decisions. We see that for nearly 50% of the couples, the difference is more than 400 Naira. Very few couples (around 10%) have an identical split. This implies that, while spouses have social preferences, they need not agree on how to divide a budget between specific expenditure categories. Hence they may wish to influence household expenditure decisions in the direction of their own social preferences.

4.2 Decision process

Arguably the most striking contrast between spouses is seen in the decisions to defer, summarized in Panel II of Table 1. We see that, for all split choices, wives are much more likely to defer (67-68%) than husbands (20-26%). The difference is highly significant and there is no noticeable difference in women's proclivity to defer depending on the type of choice. This is consistent with the existence of strong social norms that men's control of household finances is expected to be acknowledged by their wife. Furthermore, women are not less likely to defer even when expenditures are fully rival – i.e., choices A and D – then when they are not – i.e., choices B and C. This suggests that deferral decisions by women are not affected by instrumental considerations, a point we revisit below.

Panel III of Table 1 presents the average splits that subjects choose to communicate to their spouse. Comparing the allocations reported to their husbands to those they chose when decisions were shrouded, wives allocate slightly more to their husbands in choices B and D and slightly less to themselves in choice C. The magnitude, however, of these changes is small,

suggesting minimal dissimulation. Note, that the difference is large enough to be statistically significant at the 5% level for choice B. This is shown in the first row of Panel A in Table 3.[15] A similar pattern can be seen for husbands, who report allocating less to themselves (more to their wife) in choice D, a difference that is significant at the 1% level.

Panel IV of Table 1 shows the subjects' willingness to consult their spouse's choice, which, in the context of the experiment, means asking to be shown the allocation that their spouse decided to communicate. We again see that wives are much more likely to consult than husbands, with few differences across goods. The only surprise is that wives' propensity to consult is less than their propensity to defer – perhaps because consultation is not required when the choice is deferred anyway.

Panel V of Table 1 shows the revised splitting decisions of husbands and wives after having been shown their spouse's communication. We see husbands allocating significantly less than wives to female goods in choice C while in choice D wives allocate more money to themselves than their husbands do. For women, these differences were already present in their original splitting decisions: as illustrated in Panel B of Table 3, there is no significant difference between the original and revised splits for women. Men, in contrast, significantly revise their splitting decision: on average they allocate more to their wife (less to household goods) in choice C and more money to their wife in choice D. The same pattern is apparent when we compare communicated and revised splits (Panel C of Table 3).

These findings could be interpreted as suggesting that husbands partially accommodate their wives' desires when revising their original choice. This, however, would be misleading – as illustrated in Panel VI of Table 1 where we compare the extent to which husbands and wives accommodate the communicated split of their spouse when revising their original choices. We see that mean accommodation by wives is much higher than that of husbands, and the difference is highly significant. This is presented in more detail in the appendix as Table A1, which we summarize here. We see that full accommodation is the modal behavior for women: 113 (57% of choices), 134 (60%) and 46 (38%) women chose full accommodation in choices B, C and D, respectively. In contrast, zero accommodation in spite of discordant choices is the overwhelming response of husbands: 150 (84%) for domain B, 168 (84%) for domain C, ns 98 (91%) for domain D, husbands chose to not revise their decision even though they have been told their spouse prefers something else. The average accommodation apparent in Panel B of Table 3 is driven by a very small number of husbands; most simply stick to their original choice, with no consideration for the expressed wishes of their wife.

Another interesting pattern arises if we break down accommodation between those who

[15]The number of observations is slightly different across these two tables so the means are not the same, even for what appear to the be same decisions.

consult their spouse or not; results are shown in the appendix as Table A2 and summarized here. Presumably, wives who decline being told their husband's decisions do so because they have no intention of accommodating them. Yet when they are informed of their husbands' preferences anyway, many decide to accommodate them: among those wives who choose to consult, 65%, 69% and 40% of them fully accommodate their husband's stated preferences when they revise their own decisions in choices B, C and D, respectively. For women who did not wish to consult, the proportions fall to 48%, 49% and 35% – significantly lower, but still large. Of those who do not consult, less than 30% stick to their guns. In contrast, the minority of men who consult still stick to their original choices, albeit slightly less often.[16]

What these results indicate is a strong procedural inequality between husbands and wives in the study area – but much less inequality in the allocation of consumption expenditures. This suggests that intrahousehold allocative fairness is achieved through social preferences— which are largely (albeit not fully) shared between husbands and wives— not through procedural equality. Furthermore, the pattern documented in Table A2 suggests that a large proportion of the women who do not ask to see their husbands' choices end up caving in when shown these choices.

4.3 Non-instrumental demand for agency

Do these findings extend to non-rival consumption choices, such as those for the small amounts of food and drink offered to subjects to be consumed in the lab? Since consumption is non-rival and the food and drink on offer are completely innocuous, a desire to intrude in the choices of a spouse can only be driven by demand for control – unless subjects perfectly know the preferences of their spouse.[17]

We start by testing whether subjects know each other's preferences – see Table A3 in the Appendix. Since there are only two options, congruent choices should occur with a 50% probability if couples do not know each other's preferences. This is indeed what we find: the proportion of congruent choices is between 49% and 54%, with no noticeable difference between men and women. This suggests that, on average, deferral has a material utility cost for subjects: they are less likely to consume the items they prefer.

In spite of this, we find that significant numbers defer to their choice. In the last line of Panel II of Table 1 we see that 68% of wives and 19% of husbands delegate the selection of their food and drink to their spouse. This means that 68% of wives prefer to drink what their

[16] In Table A4 we show that the difference in accommodation behavior between those who consult and those who do not is statistically significant in about half of the domains.

[17] In a similar experiment, Afzal et al. (2021) show that subjects are no better informed about the preferences of their spouse than a stranger.

husband chose for them over what they chose for themselves even though there is no evidence that husbands know their wives' tastes. Since doing so reduces their chance of getting their preferred selection, deferral has a negative instrumental value – which implies that it must have a positive procedural value, especially for women. This leads to the conjecture that, in our study population, deferring to a husband (or a wife) is a behavior that is used because it has intrinsic value, not instrumental value. Put differently, most wives in our sample population have a non-instrumental demand for agency that is *negative*.

To better understand the decisions that subjects (particularly women) are making, we investigate the sensitivity of deferral to prices and the efficiency of deferral. Table 4 examines the rate at which subjects defer when they are faced with differing costs of deferral. For the choice between male and female items (domain A), subjects were offered two choices to defer: from Block2 when the budget for deferring and retaining the decision is identical (**defer**A); and from Block3 when the budget for deferring is 2500 and the budget for retaining is 2100 (**defercostA**). Similar information is available for domain D (**defer**D and **defercostD**), but we also observe deferral when the budget for retaining is 2500 and for deferring is 2100 (**deferbenefitD**).

Since the change in the size of the budget represents a cost of deferral, observed choices map out the demand for retaining the decision as a function of the cost of agency. For women, we see a downward sloping demand for the choice to retain the decision: the higher the price of not deferring (retaining), the less likely women are to not defer. In contrast, there is no change for men and most men chose to retain their choices, even when there is a cost for doing so – indicating a high but price-inelastic demand for agency.

To increase power, we combine in Table 10 all the deferral decisions subjects make allocations in domains A (female v. male goods) and D (cash for the wife v. the husband) and in the food and drink domain J. By combining decisions made in Blocks 2 and 3, we can estimate the response of the demand for agency (i.e., non-deferral) to its cost.[18] The regression includes an intercept and slope coefficient for each domain, with A serving as the excluded category. The results confirm that increasing the cost of agency reduces women's willingness to exert agency, i.e., it increases the likelihood of deferral. For men, the cost of agency has no significant effect on deferral, except for domain D (cash to the wife) where increasing the cost to deferral actually *increases* the propensity to defer.

To infer a demand for intrinsic agency, we need to be sure that deferring is not somehow instrumentally advantageous for women. Appendix A.1 examines these decisions in more detail, investigating whether subjects increase their payoffs by deferring the larger budget to

[18] Coefficient estimates for the cost variable in the J domain are across-subject comparisons since no subject made a decision over deferring a cookie and drink at two different costs.

their spouse. There we show that women, on average, over-defer relative to what would be optimal and that men massively under-defer relative to optimal. The Appendix also shows that women who defer gain more from deferral than women who do not defer – i.e., they behave as if know something about their husbands choices. Overall, however, women on average loose from deferral. Men, in contrast, do not show any indication that they know when to defer: many would benefit materially from deferring, or benefit very little from deferring when they do. In Appendix Table A5 we conduct a similar analysis for the deferral of food and drink choices and find similar results: women are more likely to defer when not deferring is costly, but men do not change their deferral decision. This confirms that instrumental motivations cannot explain men's reluctance to defer.

Taken together, these results indicate that deferral is driven primarily by non-instrumental considerations. This is particularly clear for men, who seldom defer and, when they do, show no responsiveness to instrumental concerns. Women, in contrast, tend to over-defer, and defer even when doing so would hurt the joint welfare of the spouses. But their deferral decisions do show some responsiveness to instrumental value. In addition, we find (see Appendix Table A6) that the likelihood of deferral is unchanged when the decision to defer is not communicated to the spouse. This serves as further confirmation that the deferral decision is mostly driven by intrinsic motivation, such as respect for the husband's authority or adherence to an internalized social norm.

4.4 Allocative efficiency in production

Turning to allocative efficiency in the production games, we see from Panel VII of Table 1 that, in three of the four cases, about half of the subjects choose the efficient outcome, that is, the outcome that maximizes their joint income. The one exception is that, unlike female subjects, male subjects are less likely to choose the efficient allocation when it yields less for them – a gender difference that is statistically significant. This is a priori surprising: since the subjects are paid in cash in front of each other for this Block, they could easily compensate each other after the experiment. For instance, if husbands have all the bargaining power in the household, they could appropriate any efficiency surplus from their spouse ex post. The fact that most of them do not opt for the efficient outcome when it benefits their wife suggests that they do not, in fact, expect to be able to appropriate the surplus ex post.

This finding is expanded upon in Table A7. There, we see that only 27% of women and 25% of men choose the efficient outcome in *both* games. This indicates that efficiency is not the dominant objective for subjects. Could this be because subjects focus instead on the input domain when choosing an allocation, e.g., because of mental accounting or an input entitlement effect (e.g., Fafchamps and Kebede 2020)? This is not what we find: only 33%

of women and 28% of men make the same choice in both games. Since input allocations are the same in games 1 and 2, this indicates that, for most subjects, choices are not made in the input domain. At the same time, some choices are difficult to explain based on income allocation alone: 17% of men and 19% of women choose the dominated allocation in Game 1, and 17% and 14% of them choose the dominated allocation in Game 2. These choices are very hard to justify on the basis of any reasonably convex indifference curve between own income and spouse income in the production game.

In Appendix A.2, we provide evidence suggesting that the decision model best able to account for the decisions made by our subjects is one that combines fairness considerations in both the output and input domains. The preferences implied by this model are costly for subjects in terms of material outcome. The data suggest that women are, on average, giving up about 150 Naira for themselves and 250 Naira for the couple, while men are giving up about 100 Naira for themselves and almost 300 Naira for the couple. These findings – and the loss of production income that they imply – echo those of Udry (1996) for couples and those of Fafchamps and Kebede (2020) for teams: in both cases, these authors find evidence that entitlement effects impede on the efficient allocation of inputs.

Combined with our earlier results showing that subjects often defer to their spouse even when doing so reduces the household surplus, these findings indicate the existence of preferences over the process by which a particular allocation is obtained: it is preferable to have the "right" allocation rather than the "best" outcome. This confirms earlier findings by Afzal et al. (2021) indicating that the preference for agency within the household goes beyond its instrumental value – the process matters too. With this understanding in mind, we now examine whether offering an unconditional cash transfer to women modifies these preferences.

5 Effect of the UCT treatment and no-secret condition

Equiped with a better understanding of the decisions made by couples who did not receive the unconditional cash transfer (UCT) intervention, we now add the treated subjects to the analysis and examine the effect of the UCT on the subsequent behavior of subjects under the secret and no-secret conditions in the lab.

5.1 Budget allocation decisions

We begin with the initial split decisions of husbands and wives. As explained in Section 3, initial split decisions (Block1) are always kept secret in the experiment. As a result, choices made by subjects can be interpreted as representing their social preferences. Hence any

treatment effect we find can be seen as a shift in social preferences induced by the UCT.

To investigate this, we show in Table 5 the estimates obtained using regression (1). Each regressor corresponds to a different treatment cell. The intercept gives the value of the dependent variable in the no-UCT/no-secret treatment cell. The first regressor is a dummy=1 if the couple was randomly selected to be enlisted in a one year conditional cash transfer program, and is assigned to the no-secret treatment. The second regressor is a dummy=1 if the deferral choice made by the subject is never revealed to their spouse and the subject did not receive the UCT treatment. The third regressor is a dummy=1 if the subject received the UCT and is assigned to the secret treatment. Its coefficient measures the difference between the control cell and the UCT + secret condition treatment cell. This is not the same as the *additional* (marginal) effect of being assigned to both treatments relative to being assigned to either the UCT or the secret condition. In column 1 of the top panel, for instance, this marginal effect is negative since $66.3 - 116.4 - 47.6 = -97.6$. For completion, the Table also reports the p-value of this marginal effect.

We see that women who received the UCT split budgets in a way that favors them more: point estimates show that in choices A and D women increase their share; in choice B they reduce their husbandâs share in favor of household goods; and in choice C they reduce household goods in favor of female goods. The magnitude of the effect is largest and statistically significant for A and D. For A and D we see qualitatively similar effects for husbands: they reduce the female share instead of increasing it. But point estimates are smaller in magnitude and not significant. Unsurprisingly since Block1 choices are always shrouded, we see no significant effect of the secret condition. Subjects assigned to the UCT x Secret treatment cells show changes splitting decisions similar in sign to those found in subjects assigned to the UCT x No-secret cell, but the difference with control subjects is never statistically significant. We cannot, however, reject the hypothesis that these coefficients are similar to those for the UCT alone: all the marginal effect p-values are non-significant. Somewhat similar findings are observed for husbands: the point estimates for the treatment effects of the UCT in the secret condition have the same sign as those in the no-secret condition, and the marginal effect of being in the combined treatment cell is never significant. To summarize, we see that the average social preferences of husband and wife in choices A and D (splitting cash) are much further apart as a result of the UCT treatment: from 57 to 214 Naira for A and from 177 to 318 Nairas for D in the no-secret condition; and slightly smaller in the secret condition. With social preferences less well aligned as a result of the UCT, we can expect the distribution of power in the household to have more of an effect on the distribution of material welfare. We return to this point below.

5.2 Decision process and secrecy

In Table 6 we estimate regression model (1) for deferral decisions. Recall that, defer decisions are only shrouded in the secret condition whereas all split decisions were shrouded. We find that, in all four rival domains, wives defer much less often when they receive the UCT treatment under the secret deferral condition. The difference is quite large in magnitude – between 12 and 17 percentage points – and it is significant in all cases and in the aggregate. No such change is observed for husbands who, as we noted earlier, are much less likely to defer on average. We see the results of the same magnitude for the non-rival food and juice (J) but the coefficients are not significant. Combined with the findings from Table 5, this result suggest that the UCT treatment has made women secretly want more consumption *and* agency. But it has not made them more vocal or assertive: they continue to defer to their husband if this decision is observed. This is quite a remarkable outcome, and not one that would be observable outside this experiment.

To confirm this analysis, we examine the effect of the UCT and no-secret treatments in Table 10 where we combined all the deferral decisions subjects make in domains A (female v. male goods) and D (cash for the wife v. the husband) and in the food and drink domain J. The results match those shown above: women (but not men) increase their demand for agency if they received the UCT treatment and are in the secret condition. This result is significant both in total – i.e., UCT recipients in the secret condition are different from non-recipients in the no-secret condition – as well as on the margin – i.e., the combined impact of the UCT and secret treatments is different from the sum of the UCT treatment and secret condition.

Table 7 presents a similar analysis for the decisions to dissimulate, consult, and accommodate. In the decision to dissimulate, presented in Panel A, the dependent variable is the difference between the communicated split and the initial (secret) split: a negative coefficient implies that the communicated split is smaller than the secret split. We find that women who received the UCT tend to dissimulate more in the secret condition, which is also when their secret split is the largest. The effect is only significant at the 10% level in one regression. For husbands we observe more dissimulation as a result of treatment, but only in choice D. The lack of systematic pattern across choices makes us suspect the result is not robust.

In the second panel of Table 7 the dependent variable equals 1 if the subject manifested a desire to see their spouse's communicated split. In five of the six regressions, we observe a large fall in the likelihood of consultation as a result of treatment, with all but one significant effect being concentrated in the secret condition. The magnitude of these effects is large, especially in the secret deferral case: wives reduce the likelihood of consulting by a combined 13 to 27 percentage points, while husbands reduce it by 16 to 23 percentage points (starting

from a much lower base.) The fact that these changes are significant primarily in the secret condition suggests the presence of hidden tension between treated spouses, tensions that they are trying not to learn about.

The third panel of Table 7 focuses on accommodation. Here we find little evidence of treatment effects: except for one significant coefficient at the 10% level, there is no dominant pattern across choices.[19] Even in secret, treated women are not less willing to accommodate their husbands' communicated allocation; the pressure is too direct.

These findings contribute to a coherent picture of the effect of treatment on procedural fairness in couples: treated women become secretly more demanding and less willing to defer and consult, as long as these decisions can be hidden; treated husbands also tend to shift their budget allocation towards a more selfish posture, although the effect is not statistically significant; and they consult less often, especially if this decision is less observable.

5.3 Aggregate welfare and secrecy

Before concluding we examine the effect of treatment on the expected payoff of experimental subjects. As explained in Section 2, one of a long list of possible choices made by subjects is drawn at random to determine final payoffs. Some of the choices in this list only appear in certain sessions, not in others. To keep things simple we focus on the most important choices made by both spouses, namely **split**A-D and **defer**A-D. Each of these choices has an equal chance of determining what couples take home. If the **split**A choice of the wife is drawn, for instance, the material payoffs of the wife and husband are **split**A_w and 2500-**split**A_w, respectively. The same holds if **split**D is drawn. If **split**B is drawn, the material payoff of the husband is 2500-**split**B and the material payoff of the wife for herself is 0. If **split**C is drawn, the material payoff of the wife is 2500-**split**C and the material payoff of the husband is 0. When one of the deferral decision for a particular choice, say A, is drawn to determine the final payoffs, the outcome vector is **split**B if the subject does not defer and the **split**B of the spouse if the subject defers. The rest is the same as above. Using these simple rules we construct a variable that measures the payoffs π_f and π_m that a male and female subject can expect to receive based on their own decisions.[20] These expected payoffs are what subjects can expect to receive purely for themselves at the end of the experiment, based on their own

[19] Appendix Table A9 examine this binary variable using the same regressors and finds no difference in whether women fully accommodate for any of the choices.

[20] The exact formulas used are:

$$\pi_w = \frac{1}{8}(\textbf{split}A_w(2-\textbf{defer}A_w) + (2500-\textbf{split}C_w)(2-\textbf{defer}C_w) + \textbf{split}D_w(2-\textbf{defer}D_w)$$

$$+\textbf{split}A_h\textbf{defer}A_w + (2500-\textbf{split}C_h)\textbf{defer}C_w + \textbf{split}D_h\textbf{defer}D_w)$$

split and defer decisions and the decisions of their spouse.

Coefficient estimates are presented in Table 8. We see that point estimates are positive and reasonably large for both receiving the treatment and for the interaction between the UCT treatment and the secret condition – but it is only significant for the interaction term. To the extent that the rule for selecting payoffs in the experiment mimics what happens at home, this suggests that the UCT treatment is most likely to raise the material welfare of women if it is accompanied by some form of secrecy. Without secrecy, women overwhelmingly delegate consumption decisions to their husband, whose social preferences are largely unaffected by treatment.

5.4 Allocative efficiency in production and secrecy

Table 9 examines the impact of the treatment and secret conditions on decision making in the production games. We examine two measures of efficiency: subjects who played both version of the game are said to be strongly efficient if they chose the efficient outcome in both games; subjects who played only one version, are said to be weakly efficient if they chose the efficient outcome in that game. We also show the results for each version of the game and look at households where both the husband and wife are strongly or weakly efficient.

We find that the UCT treatment makes wives less allocatively efficient in production. This is consistent with a model in which demand for agency is counterproductive to efficiency. We also see that women who did not receive the UCT treatment are somewhat less efficient in the secret condition: women cannot hide income from their husband in the secret condition, but they can hide whether they made the efficient choice or not. However, when we look the decisions of women who received the UCT, we see that they made the same decision in the secret condition and non-recipients in the no-secret condition. Put differently, secrecy has the opposite effect among UCT recipients and non-recipients.[21]

The strongest result in this table is that couples are jointly making much better decisions in the UCT treatment under the secret condition. This result is significantly different from the control (in the non-secret condition) as well as from the combined effects of treatment and secrecy.

$$\pi_h = \frac{1}{8}((2500 - \mathbf{split}A_h)(2 - \mathbf{defer}A_h) + (2500 - \mathbf{split}B_h)(2 - \mathbf{defer}B_h) + (2500 - \mathbf{split}D_h)(2 - \mathbf{defer}D_h)$$
$$+(2500 - \mathbf{split}A_w)\mathbf{defer}A_h + (2500 - \mathbf{split}B_w)\mathbf{defer}B_h + (2500 - \mathbf{split}D_w)\mathbf{defer}D_h)$$

[21] In Appendix Table A10, we see that there are no significant effects on payments to wives or husbands.

6 Conclusion

In a setting in which women likely have very low bargaining power and agency within households, we use a lab-in-the-field experiment to observe the *process* by which households make decisions over the allocation of rival and non-rival goods. Using this lens into decision-making, we examine the impact of a long term unconditional cash transfer on the way that households make decisions. Our use of the experimental laboratory setting allowed us to break decisions into constituent parts and test how decisions would be made both in a secret and non-secret condition.

By examining the behavior of women in the control group — what is most likely to have been a baseline condition — we reveal some important and interesting conditions. Both men and women have social preferences over each others' consumption, even with fully rival goods and we do not see the gender imbalance in outcomes that most assume is present in this setting. However, the *way* that allocations are chosen is highly biased. Women are more likely than men to consult their spouse, to dissimulate their communication to their spouse (stating they want less than they originally chose), and to accommodate their spouses wishes. In addition to observing the process of negotiating over outcomes, we allowed both men and women to skip this process by deferring their decision to their spouse. This decision shows a marked contrast between men and women. Women defer 67% of their decisions to their spouse and men only defer between 20 and 25% of their decisions. Men refuse to defer even when the allocation is a choice between household items and female items, a choice over which they should have much less at stake.

Note that this process of communicating leads to in final allocations that are not, on average, different from the initial choices; any examination of initial and final choices would give the impression of significant female bargaining power. However, intrahousehold allocative fairness is achieved through a social preferences, not through procedural fairness. In addition, by varying the costs of decisions and examining their efficiency, both at the individual level and household level, we see strong willingness to incur costs to follow the process outlined above. Women over- and men-under defer relative to optimal. This strongly suggests an intrinsic value to the process: men 'need' to be decision makers and women 'need' to avoid decisions in this context. Men in particular, show no sensitivity to the costs or benefits of deferral.

Women are making choices that are highly inefficient. Although husbands on average make choices that are similar to the choices of their wives, women defer to them even when it is immediately costly. Husbands, on the other hand are making similarly expensive decisions by refusing to defer even when it is costly to retain and their wives would have made the

same decisions (on average) that they make. The inefficiency is apparent in these costly decisions as well as in the two games that test efficiency directly. Women are trying to balance inputs and outputs across the couple instead of just picking the clearly largest pie and then reallocating after the fact. Men are also making inefficient decisions and their poor choices are very visible in the game where efficiency requires them to let their wives earn more than they do; these men could easily capture the gain in income, but they are unwilling to allow the inequality within the process.

We do see changes in these patterns in households that received the UCT. There are small changes in the initial choices of allocations for both men and women, suggesting some impact on the weights each member places on their own consumption due to the experiment. However, the changes in processes are small. By introducing a treatment in which all decisions are shrouded (for both UCT and non-UCT households) we show that women are less likely to defer their decisions but only when doing so is not observable to their husband.

These results call into question the ability to infer bargaining power by focusing on outcomes; in settings with strong social preferences, outcomes may bear little resemblance to power within the household. In addition, these findings suggest caution is warranted on the popular notion that giving cash to women changes relationships within the household. The UCT benefited women, children and the household, but it had a small impact on the way decisions are made in the household; women demand more agency only in secret. This small change is not unimportant (many decisions are shrouded and therefore 'secret') but it is far from the assumption that a cash transfer will significantly improve agency in the household.

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7 Table and Figures

Table 1: T-tests of difference between wife and husband means in control sample

	Sample mean				
	Wife	Husband	t-stat	p-value	N
Panel I: Split the budget secretly					
splitA– female vs male goods	1410	1298	3.22	0.001	251
splitB– household vs male goods	1396	1316	2.36	0.019	251
splitC– household vs female goods	1168	1214	-1.37	0.171	250
splitD– money for wife vs husband	1265	1062	5.67	0.000	251
Panel II: Decision to delegate splitting decision to spouse:					
deferA– female vs male goods	67%	25%	10.19	0.000	223
deferB– household vs male goods	68%	22%	11.22	0.000	251
deferC– household vs female goods	67%	20%	12.38	0.000	251
deferD– money for wife vs husband	67%	26%	9.77	0.000	251
deferFJ–choice of juice and cookie	68%	19%	10.32	0.000	165
Panel III: Split shown to spouse					
communicateB – household vs male goods	1316	1328	-0.30	0.761	198
communicateC – household vs female goods	1178	1240	-1.75	0.082	223
communicateD – money for wife vs husband	1226	1076	2.90	0.004	123
Panel IV: Whether decides to see the split shown by spouse					
consultB – household vs male goods	55%	23%	6.83	0.000	198
consultC – household vs female goods	57%	20%	8.63	0.000	223
consultD – money for wife vs husband	59%	20%	7.24	0.000	123
Panel V: Revised split after having seen the spouse's split					
reviseB – household vs male goods	1329	1323	0.20	0.840	198
reviseC – household vs female goods	1161	1230	-2.70	0.007	223
reviseD – money for wife vs husband	1172	1089	1.72	0.088	123
Panel VI: Extent of accommodation of spouse's communicated split*					
accommodateB – household vs male goods	1.94	0.47	11.70	0.000	197
accommodateC – household vs female goods	1.91	0.41	12.69	0.000	223
accommodateD – money for wife vs husband	1.27	0.22	6.00	0.000	121
Panel VII: Whether chooses efficient allocation in production					
efficientG1 – efficient is less for self	56%	38%	3.74	0.000	227
efficientG2 – efficient is more for self	51%	53%	-0.48	0.633	215

Notes: Each row reports the results for a t-test between sample means, only using observations on control households. The number of observations varies because some decisions were only introduced in later experimental sessions. Split choices A to D = split 2500 between two goods. The amount selected for the first good is shown in the graph. The order is the same for both spouses. A = between female and male goods; B = between household and male goods; C = between household and female goods; D = money for wife or husband. All goods are purchased in a shop set up in the lab. Resplit decisions are made on a smaller amount (either 2100 or 1800). The choices for the communicate and revise decisions are identical to those in the original split decisions.
(*) Categorical variable taking the following values: -1 (move away from spouse's choice); 0 (keep same choice, different from spouse's choice); 1 (keep same choice=communicated choice of spouse); 2 (partially accommodate the spouse's choice); 3 (fully accommodate spouse's choice); 4 (over-accommodate spouse's choice).

Table 2: Comparing self-appropriation across spouses

Type of split:	Wife choice for self	Husband choice for self	t-stat	p-value	N
Female vs male good	1410	1202	5.87	0.000	251
Money to wife or husband	1265	1438	-4.64	0.000	251

Notes: Each row reports the results for a t-test between sample means, only using observations on control households.

Table 3: Within-subject comparison of splitting decisions

		Sample mean of:			N	p-value		
Spouse:	Choice:	split	communicate	revise		S-C	S-R	C-R
Wife	B	1349	1316	1329	198	0.044	0.554	0.696
Wife	C	1167	1178	1161	223	0.474	0.847	0.562
Wife	D	1243	1226	1172	123	0.508	0.130	0.190
Husband	B	1329	1316	1329	198	0.741	0.987	0.959
Husband	C	1222	1178	1161	223	0.205	0.011	0.001
Husband	D	1081	1226	1172	123	0.006	0.068	0.052

Notes: Each row reports the results for a t-test between sample means, only using observations on control households. Three t-tests are reported for three pairs of comparisons. The number of observations varies because some decisions were only introduced in later experimental sessions.
Split choices B to D = split 2500 between two goods. The amount selected for the first good is shown in the column. The order is the same for both spouses. B = between household and male goods; C = between household and female goods; D = money for wife or husband. All goods are purchased in a shop set up in the lab.
The comparison Split to Communicate (S-C) is the extent to which subjects dissimulate. The comparison split to revise (S-R) is the degree to which subjects revise. The comparison communicate to revise (C-R) is the degree to which subjects exhibit visible revision.

Table 4: The Demand for agency (choosing not to defer)

Sensitivity of deferral to cost of deferral					
	Choice:	Efficient to defer	Indifferent to defer	Costly to defer	N
Wife	A	74.1%	68.3%	53.2%	139
	D	73.2%	71.1%		97
Husband	A	20.1%	26.6%	22.3%	139
	D	19.6%	24.7%		97

Note: Indifferent to defer means that the allocated budget value is the same (₦2500). Efficient to defer means the allocated budget when not deferring is smaller (e.g., 2100 or 1800) than when deferring (2500). Costly to defer means the allocated budget when not deferring is larger (2500) than when deferring (2100). The number of observations differs because some treatments only appear in certain sessions. The only pairwise difference that is statistically significant at the 5% level is the difference between indifferent and costly deferral for the wife. The difference between efficient and indifferent deferral for choice A is significant at the 10% level for husbands – but has a sign opposite to that predicted by theory. Similar results are obtained if we combine treated and control couples.

Table 5: Treatment effects on split decisions

Dependent Variable is Split Decision by the Wife for choice:	A	B	C	D
UCT treatment	116.4**	69.27	-11.80	87.31*
	(2.067)	(1.386)	(-0.228)	(1.710)
Secret condition	47.56	51.16	4.099	-16.69
	(1.039)	(0.907)	(0.0704)	(-0.275)
UCT X Secret (total)	66.31	81.58	-17.50	34.15
	(1.221)	(1.562)	(-0.382)	(0.606)
marginal effect (p-value)	[0.222]	[0.550]	[0.882]	[0.653]
Constant	1,387***	1,372***	1,166***	1,273***
	(45.08)	(34.72)	(36.15)	(30.00)
Observations	503	503	502	503
R-squared	0.012	0.006	0.000	0.010
Dependent Variable is Split Decision by the Husband				
for choice:	A	B	C	D
UCT treatment	-40.84	41.84	-29.66	-53.45
	(-0.914)	(0.848)	(-0.564)	(-1.288)
Secret condition	-67.24	55.65	-87.81	-70.77
	(-1.376)	(0.970)	(-1.388)	(-1.349)
UCT X Secret (total)	-41.26	23.29	-74.98	-23.22
	(-0.763)	(0.370)	(-1.340)	(-0.430)
marginal effect (p-value)	[0.273]	[0.361]	[0.557]	[0.117]
Constant	1,330***	1,289***	1,256***	1,096***
	(33.07)	(39.47)	(28.17)	(42.14)
Observations	503	503	503	503
R-squared	0.004	0.002	0.007	0.005

Notes: The dependent variable is the initial split decision made by each subject in each of the four splitA-D decisions. It is a number between 0 and 2500 in increments of 50. Each regressor corresponds to a different treatment cell. The intercept gives the value of the dependent variable in the no-UCT/no-secret treatment cell. The first regressor is a dummy=1 if the couple was randomly selected to be enlisted in a one year conditional cash transfer program, and is assigned to the no-secret treatment. The second regressor is a dummy=1 if the deferral choice made by the subject is never revealed to their spouse and the subject did not receive the UCT treatment. The third regressor is a dummy=1 if the subject received the UCT and is assigned to the secret treatment. Its coefficient measures the difference between the control cell and the UCT + secret condition treatment cell. For completion, the Table also reports the p-value of the the marginal effect of being assigned to both the UCT and secret condition, relative to being assigned to either the UCT or the secret condition. Robust t-statistics in parentheses, clustered by session. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Treatment effects on deferral decisions

Dependent Variable is Deferral Decision by the Wife for choice:	A	B	C	D	J	A-D
UCT treatment	-0.050	0.011	-0.064	-0.013	-0.089	-0.141
	(-0.871)	(0.149)	(-1.112)	(-0.182)	(-1.167)	(-0.619)
Secret condition	0.000	0.036	-0.029	-0.013	-0.051	0.013
	(0.006)	(0.486)	(-0.361)	(-0.190)	(-0.578)	(0.046)
UCT X Secret (total)	-0.143**	-0.123*	-0.124*	-0.168**	-0.133	-0.591**
	(-2.148)	(-1.861)	(-1.784)	(-2.509)	(-1.416)	(-2.395)
marginal effect (p-value)	[0.280]	[0.075]	[0.729]	[0.098]	[0.952]	[0.126]
Constant	0.672***	0.664***	0.679***	0.679***	0.709***	2.707***
	(14.561)	(13.710)	(13.096)	(14.424)	(10.563)	(14.571)
Observations	450	503	503	503	337	450
R-squared	0.015	0.017	0.010	0.022	0.011	0.026

Dependent Variable is Deferral Decision by the Husband for choice:	A	B	C	D	J	A-D
UCT treatment	-0.052	-0.030	0.008	-0.092	-0.049	-0.196
	(-1.229)	(-0.656)	(0.173)	(-1.578)	(-0.851)	(-1.450)
Secret condition	-0.007	-0.037	-0.023	-0.097*	-0.129	-0.203
	(-0.104)	(-0.873)	(-0.412)	(-1.858)	(-1.703)	(-1.073)
UCT X Secret (total)	-0.002	-0.022	0.061	-0.068	-0.009	-0.043
	(-0.036)	(-0.484)	(1.040)	(-1.331)	(-0.124)	(-0.235)
marginal effect (p-value)	[0.434]	[0.468]	[0.287]	[0.106]	[0.026]	[0.119]
Constant	0.250***	0.237***	0.206***	0.305***	0.256***	1.034***
	(6.783)	(7.842)	(5.181)	(8.142)	(4.426)	(8.495)
Observations	450	502	503	503	337	449
R-squared	0.002	0.001	0.006	0.008	0.015	0.004

Notes: The dependent variable is the deferral decision made by each subject in each of the four deferA-D decisions and in the deferF decision from Block 5. It is a dummy equal to 1 if the subject chooses to defer, 0 otherwise. The dependent variable in the last column is the sum of the dependent variables in columns A to D; it takes values from 0 to 4. The deferral question for choice A was not asked in four sessions, which explains the smaller number of observations. For Block5 (column J), we only use observations for which the subject receives a full share when not deferring. This explains the smaller number of observations. Each regressor corresponds to a different treatment cell. The intercept gives the value of the dependent variable in the no-UCT/no-secret treatment cell. The first regressor is a dummy=1 if the couple was randomly selected to be enlisted in a one year conditional cash transfer program, and is assigned to the no-secret treatment. The second regressor is a dummy=1 if the deferral choice made by the subject is never revealed to their spouse and the subject did not receive the UCT treatment. The third regressor is a dummy=1 if the subject received the UCT and is assigned to the secret treatment. Its coefficient measures the difference between the control cell and the UCT + secret condition treatment cell. For completion, the Table also reports the p-value of the the marginal effect of being assigned to both the UCT and secret condition, relative to being assigned to either the UCT or the secret condition. Robust t-statistics in parentheses, clustered by session. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Treatment effects on decisions to dissimulate, consult, and accommodate

decision by spouse:	Wife			Husband		
for choice:	B	C	D	B	C	D
Panel A: Dependent variable is Dissimulate						
UCT treatment	-24.44	-8.692	7.750	8.095	2.647	-23.25*
	(-0.712)	(-0.339)	(0.163)	(0.666)	(0.170)	(-1.814)
Secret condition	-42.58	-7.740	17.06	40.68	-33.54	-9.524
	(-1.309)	(-0.247)	(0.293)	(1.692)	(-1.460)	(-0.561)
UCT X Secret (total)	-20.63	-50.47*	-43.62	2.058	0.0556	-26.43**
	(-0.444)	(-1.826)	(-1.078)	(0.118)	(0.00356)	(-2.192)
marginal effect (p-value)	[0.473]	[0.418]	[0.389]	[0.097]	[0.217]	[0.800]
Constant	-13.33	14.41	-25.40	-18.10	-1.695	9.524
	(-0.738)	(0.697)	(-0.748)	(-1.624)	(-0.113)	(1.261)
Observations	394	450	245	394	450	245
Panel B: Dependent variable is Consult						
UCT treatment	-0.0365	-0.0514	-0.0626	-0.0556	-0.0468	-0.163**
	(-0.472)	(-0.663)	(-0.729)	(-1.483)	(-1.035)	(-2.272)
Secret condition	-0.0648	-0.0419	-0.117*	-0.0839	-0.0849	-0.0556
	(-0.848)	(-0.575)	(-1.776)	(-1.267)	(-1.265)	(-0.688)
UCT X Secret (total)	-0.0904	-0.150**	-0.172***	-0.125**	-0.114**	-0.0673
	(-1.412)	(-2.150)	(-3.204)	(-2.132)	(-2.461)	(-0.838)
marginal effect (p-value)	[0.915]	[0.573]	[0.944]	[0.838]	[0.794	[0.105]
Intercept	0.581***	0.585***	0.651***	0.267***	0.237***	0.222***
	(12.10)	(11.87)	(27.14)	(6.924)	(5.597)	(3.619)
Observations	394	450	245	394	450	245
Panel C: Dependent variable is Accommodate						
UCT treatment	0.0315	0.0162	-0.0252	-0.00794	-0.0616	0.0177
	(0.482)	(0.355)	(-0.271)	(-0.123)	(-1.059)	(0.285)
Secret condition	-0.0409	0.00670	-0.0452	-0.0363	-0.00444	-0.0421
	(-0.587)	(0.0921)	(-0.631)	(-0.615)	(-0.0749)	(-0.823)
UCT X Secret (total)	-0.0406	-0.0662	-0.0114	-0.0209	-0.0474	-0.0883*
	(-0.551)	(-1.171)	(-0.139)	(-0.341)	(-0.784)	(-1.781)
marginal effect (p-value)	[0.708]	[0.280]	[0.605]	[0.776]	[0.813	[0.356]
Intercept	1.942***	1.873***	1.333***	0.505***	0.424***	0.270***
	(15.47)	(14.09)	(8.236)	(3.277)	(4.160)	(3.702)
Observations	392	449	243	394	450	245

Notes: Regression results are shown for three types of decisions relating to choices B-C-D. In Panel A, the dependent variable Dissimulate is constructed as communicateB-D (the split communicated to the spouse in Block4) minus splitB-D (the initial split decision from Block1). In choice D a negative value of Dissimulate indicates, for a wife, how much self-appropriation in splitD she is hiding from her spouse; the sign is reversed for husbands. In Panel B, the dependent variable Consult is decision consultB-D from Block4 (one if subject asked to see their spouse's communicateB-D split). In Panel C, the dependent variable Accommodate is constructed as in Tables 1 and A1; it takes values from -1 (Contrarian) to 4 (Over-accommodation). The intercept gives the value of the dependent variable in the no-UCT/no-secret treatment cell. The first regressor is a dummy=1 if the couple was randomly selected to be enlisted in a one year conditional cash transfer program, and is assigned to the no-secret treatment. The second regressor is a dummy=1 if the deferral choice made by the subject is never revealed to their spouse and the subject did not receive the UCT treatment. The third regressor is a dummy=1 if the subject received the UCT and is assigned to the secret treatment. Its coefficient measures the difference between the control cell and the UCT + secret condition treatment cell. For completion, the Table also reports the p-value of the the marginal effect of being assigned to both the UCT and secret condition, relative to being assigned to either the UCT or the secret condition. Robust t-statistics in parentheses, clustered by session. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Treatment effects on expected material payoff for self

Regressors:	Wife	Husband
UCT Treatment	49.70	11.30
	(1.639)	(0.382)
Secret Treatment	4.964	23.36
	(0.169)	(0.618)
UCT & Secret (total)	59.84*	0.907
	(2.004)	(0.0246)
marginal eff. (p-value)	0.906	0.410
Intercept	1,295***	1,256***
	(79.89)	(54.13)
Observations	449	449
R-squared	0.015	0.001

Notes: The dependent variable is a constructed variable combining the 8 most common payoff vectors at the end of the experiment, namely, **split**A-D and deferA-D. Each of these is randomly drawn with equal probability at the end of the experiment. If **split**A or **split**D is drawn, the material payoffs of the wife and husband are **split** and 2500-**split**, respectively. If **split**B is drawn, the material payoff of the husband is 2500-split; the material payoff of the wife is 0. If **split**C is drawn, the material payoff of the wife is 2500-**split**, and the material payoff of the husband is 0. When one of the DeferA-D decisions is drawn, the outcome vector is **split**A-D if the subject does not defer and **split**A-D of the spouse if the subject defers. The dependent variable is the sum of these 8 equal probability outcomes, divided by 6. Each regressor corresponds to a different treatment cell. The intercept gives the value of the dependent variable in the no-UCT/no-secret treatment cell. The first regressor is a dummy=1 if the couple was randomly selected to be enlisted in a one year conditional cash transfer program, and is assigned to the no-secret treatment. The second regressor is a dummy=1 if the deferral choice made by the subject is never revealed to their spouse and the subject did not receive the UCT treatment. The third regressor is a dummy=1 if the subject received the UCT and is assigned to the secret treatment. Its coefficient measures the difference between the control cell and the UCT + secret condition treatment cell. For completion, the Table also reports the p-value of the the marginal effect of being assigned to both the UCT and secret condition, relative to being assigned to either the UCT or the secret condition. Robust t-statistics in parentheses, clustered by session.*** p<0.01, ** p<0.05, * p<0.1

Table 9: Treatment effects in the Production Game: Efficiency

efficiency type	strong	weak	game 1	game 2
Dependent variable is the decision by the wife				
UCT treatment	-0.194***	-0.173***	-0.142*	-0.089
	(-2.858)	(-2.883)	(-1.807)	(-1.557)
Secret condition	-0.140*	-0.121*	-0.051	-0.094
	(-1.779)	(-1.787)	(-0.658)	(-1.297)
UCT X Secret (total)	-0.063	-0.034	-0.032	-0.041
	(-0.849)	(-0.516)	(-0.543)	(-0.474)
marginal effect (p-value)	[0.011]	[0.004]	[0.131]	[0.153]
Constant	0.396***	0.412***	0.536***	0.598***
	(7.127)	(8.439)	(11.130)	(10.949)
Observations	392	503	434	461
Dependent variable is the decision by the husband				
UCT treatment	-0.036	-0.019	-0.067	0.015
	(-0.545)	(-0.327)	(-0.872)	(0.214)
Secret condition	0.085	0.074	0.071	0.031
	(1.560)	(1.265)	(1.245)	(0.514)
UCT X Secret (total)	0.086	0.088	-0.013	0.131**
	(1.513)	(1.512)	(-0.223)	(2.151)
marginal effect (p-value)	[0.648]	[0.650]	[0.847]	[0.346]
Constant	0.238***	0.267***	0.500***	0.369***
	(5.251)	(5.605)	(10.683)	(8.017)
Observations	392	503	434	461
Dependent variable is the decision by the couple				
UCT treatment	-0.019	0.011		
	(-0.719)	(0.353)		
Secret condition	-0.001	0.008		
	(-0.040)	(0.249)		
UCT X Secret (total)	0.078**	0.101***		
	(2.332)	(2.983)		
marginal effect (p-value)	[0.041]	[0.081]		
Constant	0.079***	0.092***		
	(3.390)	(4.234)		
Observations	392	503		

Notes: The dependent variable is a different dummy variable in each column. In the strong efficiency column, the dependent variable is 1 if the subject chooses the efficient outcome in both games. In the weak efficiency column, it is 1 if the subject chooses the efficient outcome in one game and was not presented with a second game. In the Game1 column, the dependent variable is 1 if the subject chooses the efficient decision which, in this game, means a lower payoff for self. In the Game 2 column, the dependent variable is 1 if the subject chooses the efficient decision which, in this game, means a higher payoff for self. Each regressor corresponds to a different treatment cell. The intercept gives the value of the dependent variable in the no-UCT/no-secret treatment cell. The first regressor is a dummy=1 if the couple was randomly selected to be enlisted in a one year conditional cash transfer program, and is assigned to the no-secret treatment. The second regressor is a dummy=1 if the deferral choice made by the subject is never revealed to their spouse and the subject did not receive the UCT treatment. The third regressor is a dummy=1 if the subject received the UCT and is assigned to the secret treatment. Its coefficient measures the difference between the control cell and the UCT + secret condition treatment cell. For completion, the Table also reports the p-value of the the marginal effect of being assigned to both the UCT and secret condition, relative to being assigned to either the UCT or the secret condition. Robust t-statistics in parentheses, clustered by session. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Treatment effects in the Demand for Agency

Dependent variable is deferral by	wife	husband
A (female v. male goods)	omitted	
D (cash for wife v. husband)	0.0161	0.0172
	(0.776)	(0.874)
J (drink and cookie)	0.0273	-0.0140
	(0.951)	(-0.553)
A X cost	-0.0220***	-0.00160
	(-6.009)	(-0.491)
D X cost	-0.0150*	0.0129*
	(-1.773)	(1.813)
J X cost	-0.0623***	-0.00106
	(-6.989)	(-0.109)
UCT treatment	-0.0182	-0.0417
	(-0.426)	(-1.079)
Secret condition	0.0106	-0.0385
	(0.266)	(-1.003)
UCT X Secret (total)	-0.128***	-0.0168
	(-3.047)	(-0.438)
marginal effect (p-value)	[0.046]	[0.230]
Constant	0.648***	0.249***
	(21.54)	(8.211)
Observations	2,284	2,284
R-squared	0.045	0.003

Notes: This Table combines observations on all the deferral decisions taken by the wife (column 1) and the husband (column 2) in decision domains A, D, and J. The dependent variable equal 1 if the subject defers, and 0 otherwise. As in Tables 5 to 9, regressors UCT treatment, Secret condition, and UCT x Secret, each corresponds to a different treatment cell. The other regressors are added on top of that. Deferral choices made in domain A are the omitted category. Dummies for domains D and J (Block5) are included. We also include dummies for deferral decisions made for domains A and D in Block2, when deferral either decreases or increases the allocatable budget; the dummy is 1 if deferral is costly and -1 if non-deferral is costly. We also include a dummy equal to -1 if non-deferral is costly in the fooddrink game (Block5). The intercept gives the value of the dependent variable in the no-UCT/no-secret treatment cell, domain A, and no cost condition. The first regressor is a dummy=1 if the couple was randomly selected to be enlisted in a one year conditional cash transfer program, and is assigned to the no-secret treatment. The second regressor is a dummy=1 if the deferral choice made by the subject is never revealed to their spouse and the subject did not receive the UCT treatment. The third regressor is a dummy=1 if the subject received the UCT and is assigned to the secret treatment. Its coefficient measures the difference between the control cell and the UCT + secret condition treatment cell. For completion, the Table also reports the p-value of the the marginal effect of being assigned to both the UCT and secret condition, relative to being assigned to either the UCT or the secret condition. The coefficients of the D and J domain dummies and the A x cost and D x cost variables are identified within subjects, since all/most subjects play Blocks2 and 3. The J x cost coefficient is only identified across subjects. Robust t-statistics in parentheses, clustered by session. *** p<0.01, ** p<0.05, * p<0.1

Figure 1: The production games in output space

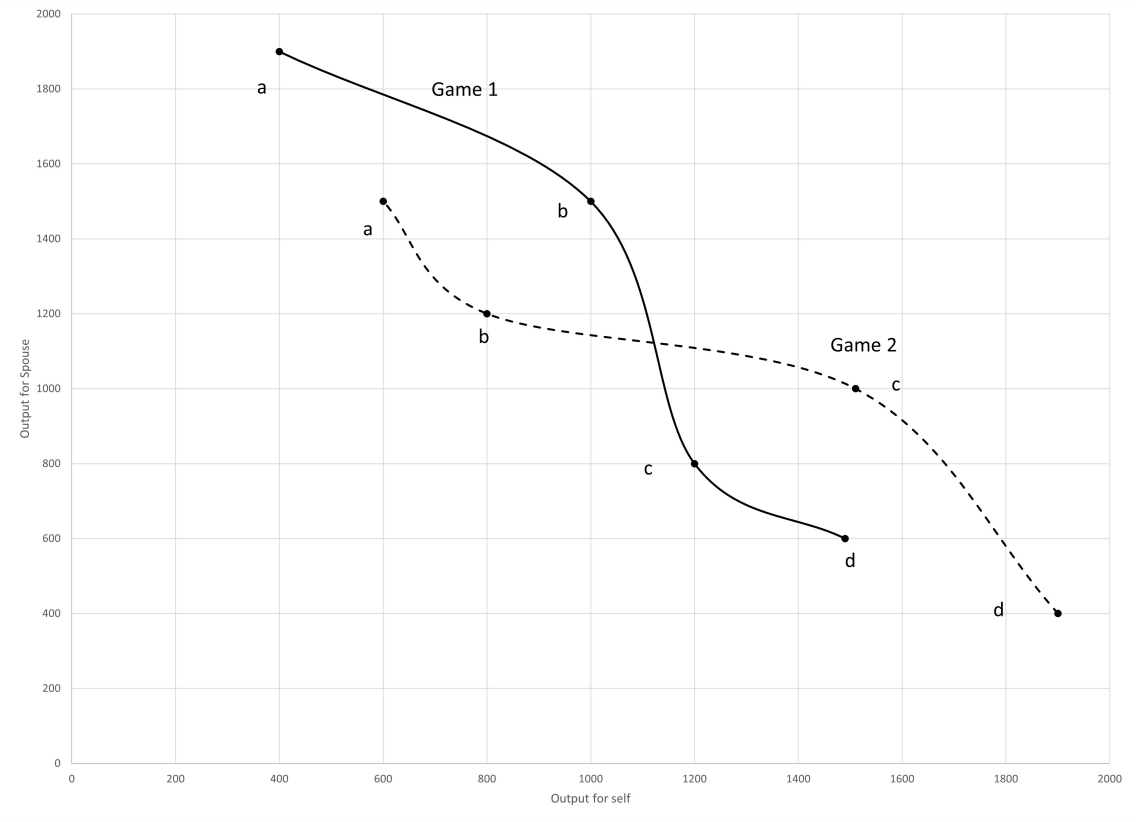


Figure 2: Difference across husband and wife of the cumulative share allocated to self

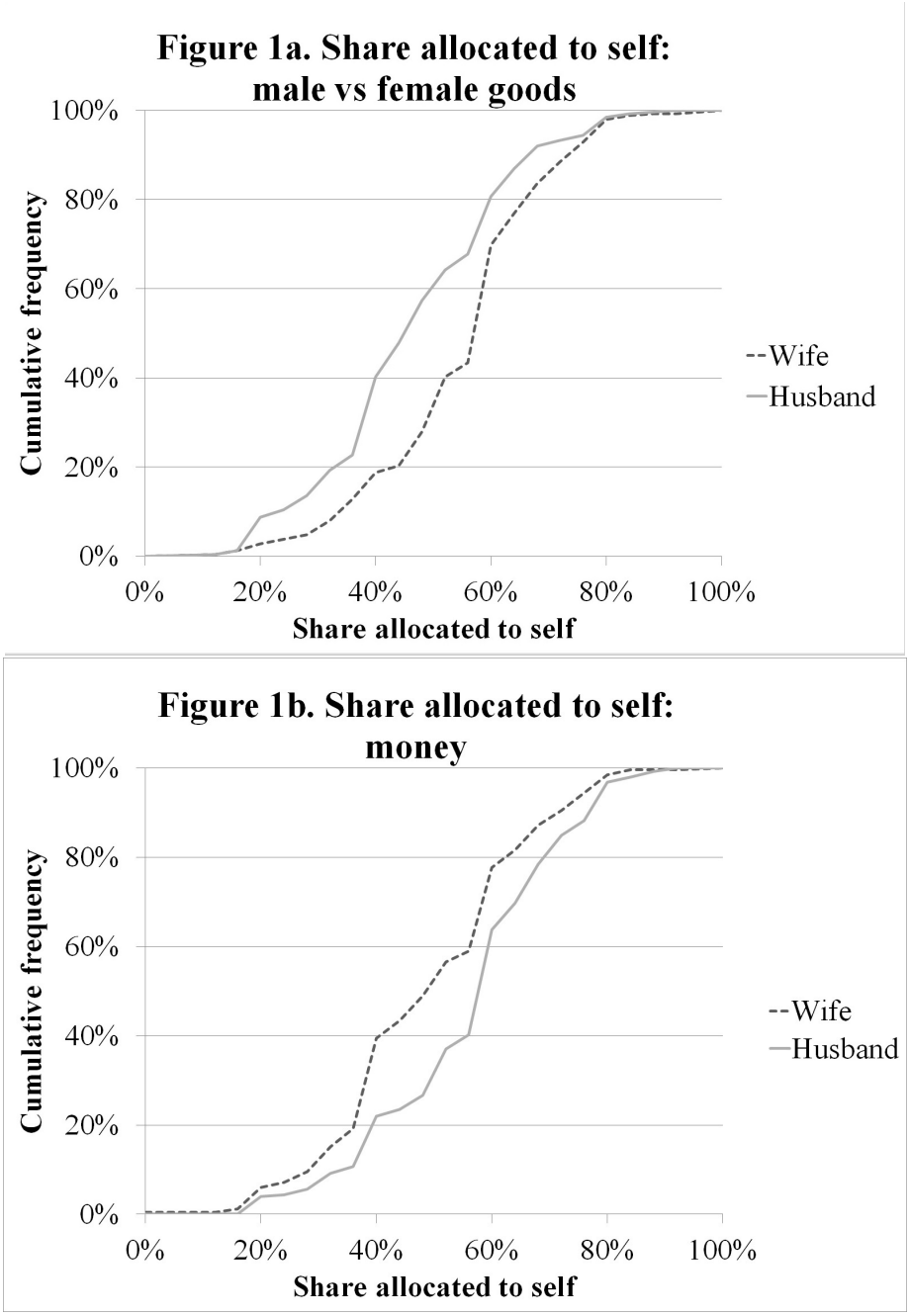
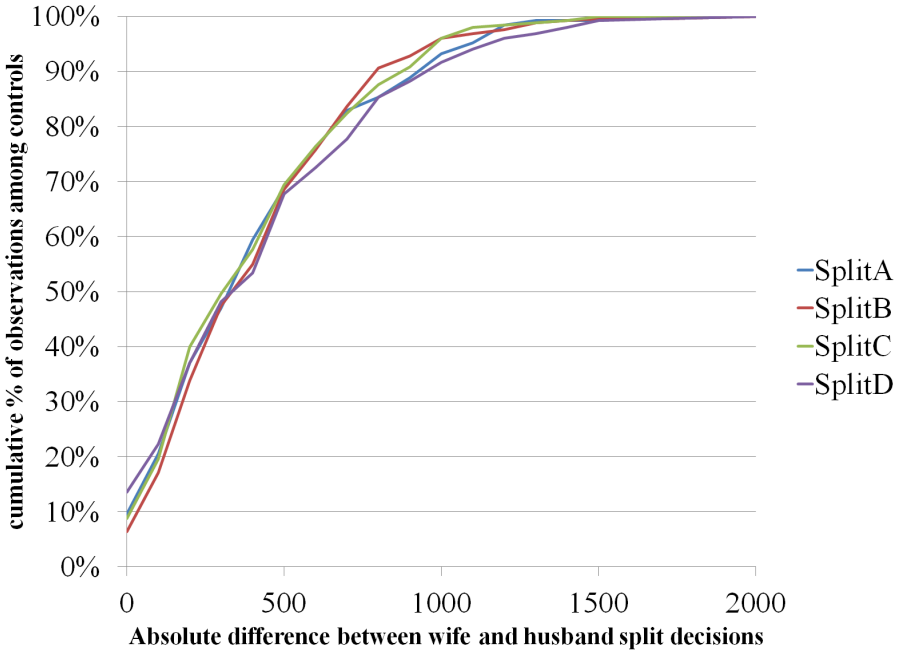


Figure 3: Cumulative distribution of within-couple differences in splitting decisions



A Appendix

A.1 The efficiency of deferring

Here we ask whether subjects increase their extrinsic welfare by choosing to defer to their spouse – is deferral an optimal decision? To investigate this idea further, we revisit the deferral decisions made for splits. When the budget for wife and husband is the same, there is, in general, no instrumental reason for deferral – hence we should not observe such deferral if the only motivation for deferral is instrumental. The fact that we observe a lot of deferral by women is an indication that it has an intrinsic value. The experiment, however, also creates situations in which the potential for an instrumental value of deferral can arise. Respondents were asked to either split a budget smaller than 2500 Naira between themselves and their spouse, or to defer to their spouse with a 2500 Naira budget. We know the subjects' private material payoff if they do not defer: it is the budget share they allocated to themselves in the first, hidden split decision with a reduced budget (**resplit**A and **resplit**D in Panel A of Table A8). This information is also known to subjects, by construction. Since subjects were not asked what they expect their spouse to do in case of deferral, we do not know what each of them expects to gain or lose from deferral. But if we are willing to assume that subjects form rational expectations on splits, on average their guesses should be correct.

With these assumptions, we can compare subjects' private material payoff without deferral to the budget share they would receive if they deferred — which is given by the revised split decision of their spouse. Using this procedure, we can calculate the proportion of cases in which it would be optimal for subject with perfect foresight to defer. The results of this calculation are shown in Panel A of Table A11. We see that, on average, women over-defer relative to what would be optimal, a difference that is statistically significant. In contrast, husbands massively under-defer: 20-21% compared with 60-64% of optimal cases. Furthermore, deferral propensities in a situation where deferring is jointly efficient for the couple is only slightly higher for women (73-74% compared to 67% in Table 1)) and even lower for men (20-21% vs 25-26% in Table 1) for choices A and D).

In Panel B of Table A11 we push these calculations one step further by averaging the gain from deferring separately for those who defer and those who do not: if subjects had perfect foresight, they would defer when they gain from it and not defer when they lose. This indeed what we observe, on average, for women: those who did not defer would, on average, have lost from deferring while those who did defer on average benefited from it. The same finding does not hold for men, many of whom would benefit materially from deferring but do not, or benefit very little from deferring when they do. This suggests that instrumental motivations are unlikely to explain men's deferral decisions.

As a final test, in Panel C of Table A11, we compare the deferral frequencies depending on whether deferral is jointly efficient, indifferent, or jointly inefficient for the couple. To test the latter, some subjects were asked to either split a 2500 budget between themselves and their spouse, or defer to the choices made by their spouse with a smaller budget. In this case, it is jointly inefficient to defer. Results show that the deferral decisions of women do respond to joint efficiency: more deferral when it is efficient (73-74%), less when it is not (53%). But we still find that more than half of female subjects defer when spouses jointly lose from doing so. In contrast, men show no relationship between the probability of deferral

and its efficiency cost or benefit.

In Table A5, we perform a similar thought experiment for deferral of the food and drink choice. We create a variable that takes value 0 if the spouse chooses the wrong drink and cookie; 1 if the spouse chooses the correct drink; 1 if the spouse selects the correct cookie; and 2 if both selections are correct. We then regress deferral on this variable under two different experimental treatments: when the cost of not deferring is null, and when subjects lose half of the food and drink if they do not defer. As predicted by instrumental considerations, we do indeed observe more deferral among women (91% vs 68%, significant at the 1% level) when not deferring is costly. But again we find no difference in husbands' deferral decisions and, as shown in Table A5, whether or not the spouse selected the right thing has no predictive effect on deferral.

A.2 A model of decision making in the production game

As outlined below there are four ways to make choices in the production game.

First, households may decide to choose the most efficient allocation in each game. We can analyze whether the probability changes with treatment categories (cash and secret), both in men and women and for pairs. Efficiency makes the most sense in households no matter what sharing rule is used. Even if unearned income is allocated more to the person who receives it (the philosophy of the cash transfer program), the individual should choose the efficient payoff and then insist on allocation after the fact. This suggests that either communication or secrecy could affect the degree to which people choose efficiency. For example, a woman could believe that their husband would let her keep money she had directly received, but would not agree to give her money she had chosen to be paid to her husband. Thus, increases in communication would increase the degree to which someone chooses the efficient outcome. Since communication is easier to explain when the choice is observed than when the choice is obscured, we might see more efficient outcomes when choices are not secret.

Second, a household might choose the allocation that best matches their preferences, ignoring efficiency. This suggests that redistribution after the fact is difficult, so it is better to pick a distribution that most neatly matches the desired final allocation. This might coincide with the efficient allocation, but not necessarily. This should be similar to the allocation of cash observed earlier. Even though the game is different, the rules are the same.

Third, the household might choose to make decisions based on inputs. Since payoffs are determined solely by outputs, subjects who are consequentialists should ignore input values. It is nonetheless conceivable that subjects made choices partially or wholly based on the input domain. Since the framing of the experiment encourages subjects to think of inputs as being shared and outputs being produced by inputs, subjects who follow an 'equality of opportunity' reasoning may allocate inputs according to a particular welfare function, and consider the fact that inputs produced different outputs as irrelevant for making a choice. This is equivalent to viewing the mapping between inputs and outputs (i.e., the 'production function') as an entitlement that the subject is justified to benefit from since 'it is not their choice' (e.g., Fafchamps and Kebede 2020).

Fourth, a household might blend the output and input model. Note that this fourth type is not efficient, but could end up choosing an allocation which is efficient.

There is a straightforward test for each of the three models: An efficient household should

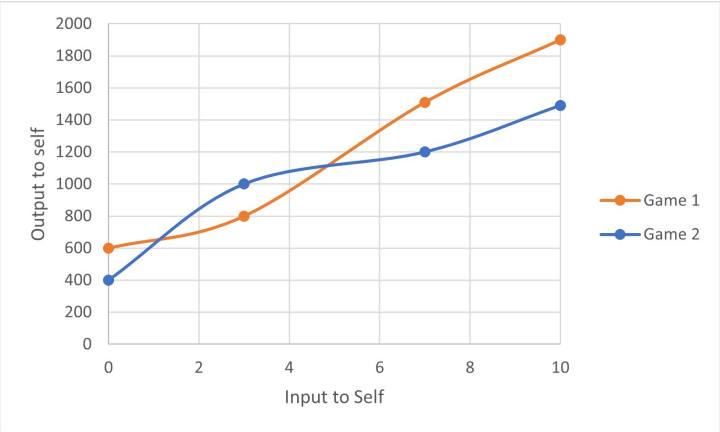


Figure A1: Production functions for Game 1 and 2

be efficient in both versions of the game. An output based decision maker will never choose the 'interior' allocation present in both games. An input based household will make the same decision in both games. Our tests show that, the average husband or wife (in either treatment or control or in secret or observable decisions making domains) does not adhere to any of these three models alone, suggesting a model in which people balance both inputs and outputs.

We now examine the behavioral predictions made by either of these choice domains, before aggregating them into a unified model.

Efficiency There is only one choice in each game associated with efficient outcomes at the household level. Thus, any efficient individual should choose these options in both games and any efficient household should have both partners choosing these outcomes in both games.

The data show that for households in the control treatment 30% of men and 35% of women always make the efficient choice, which is higher than random, but not very high. Only 10% of husband/wife pairs make the efficient choice, a similarly low number.

Output Domain To better understand the output domain, we show the production functions for each game in Figure A1.

Figure 2 shows the games in output space, assuming that utility functions have some weight on the output of both the husband and the wife.

Let us assume that each subject has other-regarding preference of the standard altruistic type:

$$W_{im} = \omega_{im} U_i(x_i) + (1 - \omega_{im}) U_j(x_j)$$

where $i \in \{\text{husband, wife}\}$, m denotes a treatment or treatment combination, ω_i is a welfare weight specific to i, and $j \neq i$.[22] We further assume that $U_i(x) = U_j(x)$ for all x – which is equivalent to saying that subjects believe their spouse to enjoy the financial payoff as much as they do –[23] and we allow function $U(.)$ to be concave to capture satiation/risk aversion,

[22]There is no point assuming a more complex other-regarding welfare function since we cannot identify it from our experimental data.

[23]Again, this is a simplification but we cannot falsify this assumption with our data.

e.g., $U(x) = x^\beta$. As we will show below, the value of β does not, in fact, matter for our main test of interest.

When choosing between different $\{x_i, x_j\}$ pairs, each subject picks the one that gives the highest welfare value:

$$argmax_{\{x_i,x_j\}} \left(\omega_{im} x_i^\beta + (1-\omega_{im}) x_j^\beta\right)$$

For each value of β, the above function defines intervals of values of ω_{im} for which each of the four possible choices available in the two games would be optimal. For instance, for $\beta = 1$ (utility linear in payoffs), the intervals are as shown below:

Altruism bounds in the output domain

$\beta = 1$	Game 1		Game 2	
Choice	lower bound	upper bound	lower bound	upper bound
A	0.6	1.0	0.643	1
B	0.357	0.6	n.a.	n.a.
C	n.a.	n.a.	0.4	0.643
D	0	0.357	0	0.4

Similar tables can be produced for other values of β. They produce different interval bounds, but choice C is never optimal in Game 1 and choice B never optimal in Game 2. This is illustrated on Figure 1 where we plot the payoff for self on the x axis and the payoff for the spouse on the y axis. Indifference curves for linear or concave preferences would themselves be linear or concave. Since choice 3 in Game 1 is below the line joining the payoffs from choices 2 and 4, it can only be optimal for a limited set of unlikely indifference curves which can be ruled out by checking choices in the other game.[24]

Eighteen percent of women and 16 percent of men in the control treatment chose one of the dominated points in these games, which suggests a statistically significant number of people are not playing only in the output domain.

Input domain We now write a similar welfare function for the different input choices, assuming that subjects judge the equity of their choice by only considering the input domain. We have:

$$argmax_{\{z_i,z_j\}} \left(\theta_{im} z_i^\alpha + (1-\theta_{im}) z_j^\alpha\right)$$

where z_i is the input share allocated to self, z_j is the input share allocated to the spouse, θ_{im} is the welfare weight parameter for individual type i in treatment m, and α is a curvature parameter.

As earlier, we can identify the interval of θ_{im} values for which each of the four possible choices is optimal. We start by noting that these intervals are identical for Games 1 and 2 since the input value pairs to choose from are the same. Next, we observe that for linear utility ($\alpha = 1$), choices 2 and 3 are not optimal – except in the knife-edge case where $\theta_{im} = 0.5$, in which case all four choices are equivalent).

[24]For example, a Leontief utility function could lead the subject to choose B in Game 1, but then they would always chose B in Game 2, which we can easily reject.

Things are different when utility is concave: in this case, there exist values of θ_{im} at which choices 2 and 3 are optimal. One such case is illustrated in the table below, which shows that, in the input domain, choice 3 in Game 1 and choice 2 in Game 2 are not always dominated: they can be optimal.

Altruism bounds in the Input game

$\beta = 0.9$	Both games	
Choice:	lower bound	upper bound
A	0.551	1
B	0.5	0.551
C	0.449	0.5
D	0	0.449

This implies that subjects who pick choice 3 in Game 1 or choice 2 in Game 2 *must* be playing (at least partly) in the input domain. We also note that if subjects make a choice purely based on the input domain, these choices should be identical for Games 1 and 2 – something we can test: if subjects pick different choices in Games 1 and 2, they *must* be playing (at least partly) in the output domain. The data clearly reject the hypothesis that subjects make the same input choices in each game because over 50% of women chose the efficient outcome in each game even though it requires different input choices.

Under the maintained assumption that subjects play in the input domain, we can use the above approach to test whether the altruism of husbands and wives varies by treatment and treatment combination. This is done by estimating, for different values of α, an interval regression in which the bounds of the intervals are those given in a Table like Table 2 and the regressors are treatment dummies. Observations from the two games are pooled and the model is estimated separately for husbands and wives.

Recall that, the input game is the same in both versions, so if someone was playing only in that domain they would make the same choice in both versions. Twenty-seven percent of women and 25% of men in the control treatment make the same input choice in both versions of the game, suggesting that most people are not playing solely in the input domain.

Combining both domains We now consider the situation in which we can reject that subjects play exclusively in the output domain, and also reject that they play exclusively in the input domain. Our approach is to start from the output domain, since it determines payoffs directly, but adjust it for the distance from equal sharing of inputs. The reasoning is that subjects may wish to deviate from a selfish division of payoffs is that implies a very unequal division of inputs. In that case, they may be willing to sacrifice some of their own payoff to achieve a more equitable division of inputs.

We first examine whether it is possible to obtain all four choices if we set $\omega_i = \theta_i$ and $\alpha_i = \beta_i$, i.e., utility functions in the two domains are identical. The issue then is whether there is a blending parameter value that can account for all choices. The objective function now is:

$$W_{im} = b\left(\omega_{im}x_i^{\beta} + (1-\omega_{im})x_j^{\beta}\right) + (1-b)\left(\omega_{im}z_i^{\beta} + (1-\omega_{im})z_j^{\beta}\right)$$

where b is a blend parameter capturing the weight subjects put on the two objective function,

and where the input variables z_i and z_j have been multiplied by the mean of x_i over both games. The purpose of this transformation is to make the two utility functions have the same approximate weight in forming welfare function W_{im}.

By choosing values for β and b and then testing whether the model can explain the behavior of individuals in the control group, we find (through grid search) optimal values of approximately 0.63 and 0.3 respectively. This suggests that the average person is indeed blending an output and input model with a heavier weight on the output domain, but a significant though smaller weight on the input domain.

A.3 Additional Tables and Figures

Table A1: Extent to which subjects accommodate their spouse's choice when revising after being informed of their choice

Panel I: Choice B	**Husband's behavior:**						
Wife's behavior:	Contrarian	No revision different choice	No revision same choice	Partial accomm.	Full accomm.	Over-accomm.	Total
Contrarian	0	11	1	0	0	0	12
No revision, different choice	1	37	0	0	5	0	43
No revision, same choice	1	1	9	0	1	0	12
Partial accomm.	0	9	0	0	3	0	12
Full accomm.	2	89	5	0	15	2	113
Over-accomm.	1	3	0	0	1	0	5
Total	5	150	15	0	25	2	197
Panel II: Choice C	**Husband's behavior:**						
Wife's behavior:	Contrarian	No revision different choice	No revision same choice	Partial accomm.	Full accomm.	Over-accomm.	Total
Contrarian	1	13	0	0	1	0	15
No revision, different choice	4	35	1	2	6	0	48
No revision, same choice	0	2	14	0	0	0	16
Partial accomm.	1	8	0	0	0	0	9
Full accomm.	2	109	5	1	16	1	134
Over-accomm.	0	1	0	0	0	0	1
Total	8	168	20	3	23	1	223
Panel III: Choice D	**Husband's behavior:**						
Wife's behavior:	Contrarian	No revision different choice	No revision same choice	Partial accomm.	Full accomm.	Over-accomm.	Total
Contrarian	0	19	3	1	2	0	25
No revision, different choice	1	25	0	0	1	0	27
No revision, same choice	0	0	8	0	1	0	9
Partial accomm.	2	9	0	1	0	0	12
Full accomm.	1	44	0	0	1	0	46
Over-accomm.	0	1	1	0	0	0	2
Total	4	98	12	2	5	0	121

Notes: The table shows the number couples in different cells based on their behavior. Only observations from control subjects are used to construct this Table. The number of observations varies because some decisions were only introduced in later experimental sessions. Behavior is defined as follows. Each subject first make a split choice on B, C, and D. Then the subject is told the choice of their spouse, as reported by that spouse (i.e., the communicate decision). They are then allowed to revise their original split. Full accommodation means replacing one's original split by the split communicated by their spouse. Partial accommodation means revising one's original split in the direction of the spouse's choice, but only partly. Over-accommodation means revising one's original split by overshooting the spouse's choice. No revision, different choice means that the subject does not revise their original split even though they have been informed their spouse chose a different split. No revision, same choice means the subject does not revise their original split knowing that it is identical to the split communicated by their spouse. Contrarian means that the subjects revises their original split to be further away from the spouse's communicated split than it was originally.

Table A2: Accommodation and consultation

Panel I. Wife	Not consult	consult	Not consult	consult	Not consult	consult
Choice:	B		C		D	
Contrarian	11.4%	1.8%	7.2%	6.3%	20.8%	20.5%
No revision, diff. choice	29.5%	15.6%	29.9%	15.1%	27.1%	19.2%
No revision, same choice	5.7%	6.4%	7.2%	7.1%	4.2%	9.6%
Partial accommodation	5.7%	6.4%	7.2%	1.6%	12.5%	8.2%
Full accommodation	47.7%	65.1%	48.5%	69.0%	35.4%	39.7%
Over-accommodation	0.0%	4.6%	0.0%	0.8%	0.0%	2.7%
Number of observations	88	109	97	126	48	73

Panel II. Husband	Not consult	consult	Not consult	consult	Not consult	consult
Choice:	B		C		D	
Contrarian	2.6%	2.2%	2.8%	6.8%	4.0%	0.0%
No revision, diff. choice	80.4%	62.2%	77.7%	65.9%	81.8%	79.2%
No revision, same choice	7.8%	6.7%	8.9%	9.1%	11.1%	4.2%
Partial accommodation	0.0%	0.0%	1.1%	2.3%	1.0%	4.2%
Full accommodation	8.5%	26.7%	8.9%	15.9%	2.0%	12.5%
Over-accommodation	0.7%	2.2%	0.6%	0.0%	0.0%	0.0%
Number of observations	153	45	179	44	99	24

Table A3: Choices of drink and cookie

Choice for spouse made by:		
Choice made by:	**Husband**	
Wife	Drink 1	Drink 2
Drink 1	18%	24%
Drink 2	22%	35%
Proportion of congruent choices:		54%
Choice for spouse made by:		
Choice made by:	**Wife**	
Husband	Drink 1	Drink 2
Drink 1	39%	36%
Drink 2	10%	15%
Proportion of congruent choices:		54%
Choice for spouse made by:		
Choice made by:	**Husband**	
Wife	Round cookie	Square cookie
Round cookie	28%	23%
Square cookie	24%	24%
Proportion of congruent choices:		52%
Choice for spouse made by:		
Choice made by:	**Husband**	
Wife	Round cookie	Square cookie
Round cookie	24%	25%
Square cookie	26%	25%
Proportion of congruent choices:		49%

Notes: Each panel reports the proportion of couples falling in each of the four possible choice categories. Only control households are used for constructing this Table. The number of observations varies across panels between 249 and 251 due to a small number of subjects who refuse the two choices that are offered.

Table A4: Is accommodation more likely if the subject previously chooses to see their spouse's choice?

				Asks to see spouse's choice					
Dependent variable	Choice		Spouse	Coef	t-stat	Intercept	t-stat	N	R-squared
Accomm. index for choice	B	by	Wife	0.823***	(3.441)	1.489***	(8.571)	197	0.078
Accomm. index for choice	B	by	Husband	0.600***	(2.919)	0.333***	(3.772)	198	0.054
Accomm. index for choice	C	by	Wife	0.545***	(2.754)	1.598***	(11.04)	223	0.034
Accomm. index for choice	C	by	Husband	0.171	(1.020)	0.374***	(4.800)	223	0.005
Accomm. index for choice	D	by	Wife	0.210	(0.799)	1.146***	(6.305)	121	0.004
Accomm. index for choice	D	by	Husband	0.348	(1.730)	0.152**	(2.858)	123	0.037

Notes: Each row is a different regression of the dependent variable described on the left on the secret condition dummy. Only control observations are used. Robust t-statistics in parentheses, clustered by experimental session. *** p<0.01, ** p<0.05, * p<0.1

Accommodation behavior is defined as follows. Each subject first make a split choice on B, C, and D. Then the subject is asked whether they want to be told they split choice of their spouse. Irrespective of what they answer, the subject is told the choice of their spouse, as reported by that spouse (i.e., the communicate decision). They are then allowed to revise their original split. Full accommodation means replacing one's original split by the split communicated by their spouse. Partial accommodation means revising one's original split in the direction of the spouse's choice, but only partly. Over-accommodation means revising one's original split by overshooting the spouse's choice. No revision, different choice means that the subject does not revise their original split even though they have been informed their spouse chose a different split. No revision, same choice means the subject does not revise their original split knowing that it is identical to the split communicated by their spouse. Contrarian means that the subjects revises their original split to be further away from the spouse's communicated split than it was originally. The regressor is a dummy equal to 1 if the subject asked to be told the split choice of their spouse.

Table A5: Is deferral in food and drink game predicted by correct choice of spouse?

		Dependent variable is deferral to spouse in the drink and cookie game			
		Wife	Husband	Wife	Husband
		Share received if no deferral:			
		Full	Full	Half	Half
Match quality of spouse's	Coef.	-0.0188	0.000	0.0105	0.0540
choice for subject	t-stat	(-0.577)	(-0.0009)	(0.552)	(1.130)
	Intercept	0.715***	0.194***	0.914***	0.181**
		(10.58)	(3.169)	(22.86)	(2.602)
Number of observations		165	165	86	86
R-squared		0.002	0.000	0.002	0.015

Notes: The dependent variable is a dummy=1 if the subject's chooses to defer the choice of cookie and drink to what their spouse has selected for them. The regressor is an index of match quality between the subject's own choice and the choice of cookie and drink that their spouse has made for them. The index equals 0 if there is no match; 1 if the choice of drink is the same; 2 if the choice of cookie is the same, and 4 if both match. Only observations on control households are used in the estimation. The number of observations vary across regression because some sessions received the full share treatment while others received the half share treatment.

Table A6: Effect of the secret condition on deferral decisions

			Secret condition dummy					
Dependent variable:	Choice:	Spouse	Coef	t-stat	Intercept	t-stat	N	R-squared
Own split with 2500 or defer to spouse split with 2500	A	Wife	0.0005	(0.006)	0.672***	(14.58)	223	0.000
Own split with 2500 or defer to spouse split with 2500	A	Husband	-0.007	(-0.104)	0.250***	(6.790)	223	0.000
Own split with 2500 or defer to spouse split with 2500	D	Wife	-0.013	(-0.190)	0.679***	(14.44)	251	0.000
Own split with 2500 or defer to spouse split with 2500	D	Husband	-0.097*	(-1.860)	0.305***	(8.151)	251	0.012
Own split with 2500 or defer to spouse split with 2500	J	Wife	-0.051	(-0.578)	0.709***	(10.58)	165	0.003
Own split with 2500 or defer to spouse split with 2500	J	Husband	-0.129	(-1.705)	0.256***	(4.432)	165	0.027
Own split with 2100 or defer to spouse split with 2500	A	Wife	-0.001	(-0.014)	0.741***	(13.29)	154	0.000
Own split with 2100 or defer to spouse split with 2500	A	Husband	0.009	(0.136)	0.210***	(3.940)	154	0.000
Own split with 2100 or defer to spouse split with 2500	D	Wife	0.058	(0.905)	0.702***	(15.85)	97	0.004
Own split with 2100 or defer to spouse split with 2500	D	Husband	-0.074	(-1.133)	0.234***	(6.088)	97	0.009
Half of own food and drink choice or full choice of spouse for me	J	Wife	0.040	(0.730)	0.911***	(20.02)	86	0.006
Half of own food and drink choice or full choice of spouse for me	J	Husband	0.117	(1.011)	0.200**	(2.774)	86	0.018
Own split with 2500 or defer to spouse split with 2100	A	Wife	0.094	(1.128)	0.481***	(7.430)	154	0.001
Own split with 2500 or defer to spouse split with 2100	A	Husband	-0.004	(-0.066)	0.210***	(4.604)	154	0.002

Notes: Each row is a different regression of the dependent variable described on the left on the secret condition dummy. Only control observations are used.

Robust t-statistics in parentheses, clustered by experimental session. *** p<0.01, ** p<0.05, * p<0.1

Table A7: T-test of difference between wife and husband in production game choices

	input		output		total	sample mean				
	own	spouse	own	spouse		wife	husband	t-stat	p-value	N
Game 1										
a	10	0	1500	600	2100	10%	24%	-4.29	0.000	227
b †	7	3	1200	800	2000	19%	17%	0.61	0.542	227
c ⋆	3	7	1000	1500	2500	56%	38%	3.74	0.000	227
d	0	10	400	1900	2300	15%	20%	-1.48	0.140	227
Game 2										
a	10	0	1900	400	2300	11%	16%	-1.45	0.149	215
b ⋆	7	3	1500	1000	2500	51%	53%	-0.48	0.633	215
c †	3	7	800	1200	2000	17%	14%	0.93	0.355	215
d	0	10	600	1500	2100	20%	17%	0.93	0.354	215
Same input choice in both games						33%	28%	1.08	0.282	191
Efficient outcome in both games						27%	25%	0.47	0.639	191
Avg. alloc. to wife			1104[1]	1124[1]				-0.7	0.483	251
Avg. alloc. to husband			1213[2]	1176[1]				1.34	0.182	251
Avg. total alloc.			2249[2]	2208[1]				2.99	0.003	251

⋆ The efficient outcome : † The outcome dominated by any reasonable utility function over outcomes.
[1] significantly different from the equal distribution of efficient outcome (1250) or most efficient outcome (2500) at p-value < 0.01
[2] significantly different from the equal distribution of efficient outcome (1250) or most efficient outcome (2500) at p-value < 0.10

Table A8: T-tests of difference between wife and husband means in control sample

		Sample mean				
		Wife	Husband	t-stat	p-value	N
Panel I: Split the budget secretly						
resplitA	– female vs male goods	1190	1136	1.54	0.124	184
	– with reduced budget					
resplitD	– with reduced budget	1077	904	4.05	0.000	128
	– with reduced budget					

Notes: Each row reports the results for a t-test between sample means, only using observations on control households. The number of observations varies because some decisions were only introduced in later experimental sessions.

Table A9: Treatment effects on decisions to fully accommodate

for choice:	B	C	D	B	C	D
made by spouse:	Wife	Wife	Wife	Husband	Husband	Husband
Regressors:						
Received UCT treatment	0.0315	0.0162	-0.0252	-0.00794	-0.0616	0.0177
	-0.482	-0.355	(-0.271)	(-0.123)	(-1.059)	-0.285
Secret condition	-0.0409	0.0067	-0.0452	-0.0363	-0.00444	-0.0421
	(-0.587)	-0.0921	(-0.631)	(-0.615)	(-0.0749)	(-0.823)
Received UCT x	-0.0406	-0.0662	-0.0114	-0.0209	-0.0474	-0.0883*
secret condition	(-0.551)	(-1.171)	(-0.139)	(-0.341)	(-0.784)	(-1.781)
Intercept	0.654***	0.669***	0.476***	0.219***	0.195***	0.159***
	-13.25	-17.01	-8.594	-4.456	-4.397	-3.985
Observations	392	449	243	394	450	245
R-squared	0.004	0.005	0.001	0.001	0.005	0.016

Notes: Regression results are shown for three types of decisions relating to three choices (B-C-D). Full accommodation is defined as either no revision, same choice or accommodate. The first regressor is a dummy=1 if the couple was randomly selected to be enlisted in a one year conditional cash transfer program. Randomization is done within village. The second regressor is a dummy=1 if the deferral choice made by the subject is never revealed to their spouse; this treatment is randomized across villages. The intercept gives the value of the dependent variable in the no-UCT/no-secret condition cell. Robust t-statistics in parentheses, clustered by session. *** p<0.01, ** p<0.05, * p<0.1.

Table A10: Treatment effects in the Production Game: Outcomes

output decision by		wife			husband	
recipient	wife	husband	total	wife	husband	total
UCT treatment	-45.538	22.293	-12.080	-27.023	56.978	-23.667
	(-1.011)	(0.487)	(-0.537)	(-0.452)	(1.193)	(-0.972)
Secret condition	-48.176*	13.949	24.480	-11.809	12.683	17.241
	(-1.897)	(0.582)	(1.351)	(-0.216)	(0.255)	(0.690)
UCT X Secret (total)	-16.325	14.747	1.346	22.977	-1.448	19.064
	(-0.445)	(0.381)	(0.058)	(0.440)	(-0.031)	(0.827)
marginal effect (p-value)	[0.102]	[0.602]	[0.693]	[0.385]	[0.256]	[0.400]
	1,125***	1,202***	2,254***	1,137***	1,148***	2,219***
Constant	(45.487)	(44.896)	(146.766)	(28.144)	(37.021)	(124.863)
Observations	503	503	503	503	503	503
R-squared	0.004	0.001	0.006	0.003	0.005	0.013

Notes: The dependent variable is the portion of the outcome paid to the wife or husband (or the total) based on choice a, b, c or d in both versions of the game. Included in brackets is the p-value for the test that the UCT treatment in secret is different from the omitted category as well as both the UCT and secret treatments independently. The intercept gives the value of the dependent variable in the no-UCT/no-secret treatment cell. Robust t-statistics in parentheses, clustered by session. *** p<0.01, ** p<0.05, * p<0.1

Table A11: Examining Deferral Efficiency

Panel A: Comparing deferral propensity to optimal deferral with full information

Deferral in choice		Spouse	**Actual**	**Optimal**	t-stat	p-value	N
A (2100 vs 2500)	by	Wife	74%	61%	2.54	0.012	153
A (2100 vs 2500)	by	Husband	21%	64%	-8.69	0.000	154
D (2100 vs 2500)	by	Wife	73%	49%	3.47	0.001	97
D (2100 vs 2500)	by	Husband	20%	60%	-6.35	0.000	97

Notes: All deferral choices refer to situation in which the subject must choose between their own split with a budget of 2100 or the split choice made by their spouse split with a budget of 2500. Each row reports the results for a t-test between sample means, only using observations on control households. Deferral is defined to be optimal if the subject would receive more for self by deferring to spouse. This assumes that the subject knows the split that the spouse has selected. It serves as a benchmark to gauge whether a subject would benefit from deferring more often. The number of observations varies because some decisions were only introduced in later experimental sessions.

Panel B: Comparing the potential gain from deferring when the subjects defers or not

			When does					
Pot. gain from deferral in choice			**not defer**	**defer**	t-stat	p-value	N_1	N_2
A (2100 vs 2500)	by	Wife	-23	145	-1.80	0.074	40	113
A (2100 vs 2500)	by	Husband	-129	206	-3.58	0.000	121	33
D (2100 vs 2500)	by	Wife	123	10	0.87	0.389	26	71
D (2100 vs 2500)	by	Husband	-90	16	-0.72	0.473	78	19

Notes: The gain to self from deferring to the spouse is defined as the subjects own gain when the spouse splits a 2500 budget, minus the subjects own gain when he/she splits a 2100 budget. This assumes that the subject knows the split that the spouse has selected. It serves as a benchmark to gauge whether the subject benefits from deferring or not deferring. The total number of observations varies because some decisions were only introduced in later experimental sessions. The number of observations in each cell N_1 and N_2 depends on the subject's deferral decision.

B Experimental Instructions and Materials

Figure B2: The typical set up of the laboratory experiment

Note that one enumerator (back to camera) is working with two women who can both see the enumerator but not observe each other's choices due to the cloth separator.

Figure B3: Women's Items Prompt: example of typical women's items available in the shop

Figure B4: Men's Items Prompt: example of typical men's items available in the shop

Figure B5: Household Items Prompt: example of typical household items available in the shop