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WOMEN'S EMPOWERMENT AND THE INTRINSIC DEMAND FOR AGENCY: EXPERIMENTAL EVIDENCE FROM NIGERIA

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Women's Empowerment and the Intrinsic Demand for Agency: Experimental Evidence from Nigeria

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ABSTRACT

Most studies of intrahousehold resource allocation examine outcomes and do not consider the decision-making process by which those outcomes are achieved. We conduct an original lab-in-the-field experiment on the decision-making process of married couples over the allocation of rival and non-rival household goods. The experiment measures individual preferences over allocations and traces the process of consultation, communication, deferral, and accommodation by which couples implement these preferences. We find few differences in individual preferences over allocations of goods. However, 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 men. Our study follows a randomized controlled trial that ended a year earlier and gave large cash transfers over fifteen months to half of the women in the study. We estimate the effect of treatment on the demand for agency among women and find that the receipt of cash transfers does not change women's bargaining process except in a secret condition when the decision to defer is shrouded from her husband: only in that case does the cash transfer increase women's expressed demand for agency.

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1 Introduction

Making progress on women's empowerment and 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 gender inequalities within the household. This literature has focused primarily on the *outcomes* experienced by women (see Duflo, 2012, for a review). For example, intrahousehold inequality in consumption and health outcomes between men and women has been documented worldwide (e.g., Altonji and Blank, 1999; Azmat et al., 2006; Beaman et al., 2017; Bertrand and Hallock, 2001). 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 such as cash transfers (e.g., Bandiera et al., 2017; Carneiro et al., 2021). Money given to women is more likely to be used for investments in education, children's nutrition, and housing than money in the hands of their husbands (e.g., 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 (e.g., de Aghion and Morduch, 2004) and increasing women's say in family finances has been shown to raise savings and investment (e.g., Armendáriz and Morduch, 2010; Ashraf, 2009; Duflo, 2012). At the same time, married women typically earn less than their husband, a pattern possibly explained by gender identity norms (e.g. Bertrand et al., 2015).

The dominant economic model of intrahousehold bargaining power assumes 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). When this assumption is combined with a symmetry assumption about the other-regarding preferences of the spouses,[1] it predicts that equality in bargaining power implies 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., Brown et al., 2021; Cherchye et al., 2017; Dunbar et al., 2013). Implicit in these models is the assumption that individuals do not care about the way these outcomes are achieved. Perhaps as a result, there is less evidence in economics on the *process* by which decisions are made within the household. Even when more attention is paid to the decision-making process, particularly whether women have executive agency and whether external interventions can improve female agency within the household (e.g., Dhar et al., 2022; Karimli et al., 2021; Pitt et al., 2006; Riley, 2022), the emphasis remains on the instrumental value of agency, that is, on its capacity to affect material outcomes, rather than agency's intrinsic value. With a few exceptions (e.g., Afzal

[1] E.g., spouses only derive utility from their own (rival) consumption; or they have symmetrical altruistic preferences with identical welfare weights.

et al., 2022; Fernandez et al., 2015)[2], 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 intrahousehold resource allocation.

This paper presents results from an original lab-in-the-field experiment conducted with married couples in Northern Nigeria, a lower middle-income country context 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 ultra-poor households received significant unconditional cash transfers for 15 months before the study took place) and unique lab–in–the–field elements designed by the team (three laboratory shopping stalls with separate categories of female, male and household items). Finally, the experiment includes a randomly assigned 'secret' treatment in which most of the decisions are shrouded so neither husband or wife can tell what their spouse chose or what processes they followed to reach that decision.

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 not), deferral, revision and accommodation of other spouse's preferences as well as a measure of the willingness to pay for agency over own consumption. From these measures we can examine a number of important features of the decision-making process. First, we test, in a controlled environment, whether the randomized cash transfer treatment increases the desire for private consumption and leads to higher material welfare. Second, we test whether treatment has an impact on willingness to exert agency or to relinquish control to a spouse, either secretly or openly. We do so in a way that enables us to 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.

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 cookies (following Afzal et al., 2022) 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,

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

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 his communicated preferences. 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: our subjects appear to know they have difficulty resisting implied demand and therefore prefer to avoid learning about it. We find weak evidence that women misrepresent their true preferences, but not by very much. Men, in contrast, report their true preferences to their wives.

The UCT intervention had a small effect on the social preferences of men and women; women allocate more goods or money to themselves if they received the UCT. Treated women are less likely to defer their choice to their husbands, but only when their deferral decision is shrouded and therefore kept secret from their husband. UCT treated women are also 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., Abbink et al., 2020; Afzal et al., 2022; 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 for themselves instead of their spouses. This, they argue, shows 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 could explain why CCT-receiving women exhibit a lower WTP to receive a cash windfall instead of their spouse.[3] Importantly, our secret condition treatment helps to unwrap some of the issues raised in that paper.

In contrast, the literature on Intimate Partner Violence (IPV) has examined the process by which decisions are made. But it generally assumes that violence, though inefficient, has no non-instrumental value and is simply a way of improving the material outcomes of the man (e.g., Erten and Keskin, 2021a,b; Heath et al., 2020; Jayachandran, 2015; Kotsadam and Villanger, 2020; Noble et al., 2020).[4] Evaluations of conditional cash transfer (CCT) programs have shown that transfers targeted to women can, in some cases, decrease the incidence of intimate partner violence (e.g., Angelucci (2008) and 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).[5] In our paper, random assignment to an unconditional cash transfer (UCT) to women increases their autonomy even though it does not have a strong affect on the preferences and outcomes of women within our laboratory setting. The IPV literature also notes that violence increases when women work outside the home and posits that men need to increase their threats when the wife's behavior is harder to observe. Echoing these findings, we find that random assignment to hidden or secret decision making affects demand for autonomy.

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

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[3]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.

[4]As an exception, Eswaran and Malhotra (2011) model IPV as a way of reducing women's autonomy in decision making. But since they assume selfish preferences, this autonomy is correlated with women's utility and therefore has the same affect as if it were about bargaining power. By instrumenting for autonomy, the authors can examine the causal impact of IPV on female autonomy and conclude that it reduces autonomy.

[5]In some cases transfers to women have had no effect or even a positive effect on IPV (see Buchmann, 2022, and the references cited therein).

2 Experimental design

We conducted a lab experiment with married couples in 27 villages from two rural regions in the state of Kebbi, in Northwest Nigeria. Households in the region are mostly Hausa ethnicity and Islamic faith, and are known for being patriarchal. The majority of households in this region live in extreme poverty. Restrictive social norms, low levels of education and low female labor force participation mean women in this setting experience very low levels of agency and some of the worst health outcomes in the world (Braimah, 2014; Ogu et al., 2016).

Unconditional cash transfers (UCT) were offered to ultra-poor women in this region to support livelihoods. A randomized controlled trial (RCT) impact evaluation of the UCT program that underlies the lab experiment described in this paper was designed by the World Bank's Africa Gender Innovation Lab (GIL).[6] The cash transfers were offered only to the most vulnerable households (those categorized as "extremely vulnerable" or "ultra-poor" by the program). In order to identify eligible households for the UCT program, a proxy means test was conducted using a modified Progress Out of Poverty Index (PPI) survey for all households across 120 villages selected for support in the region. Households identified as extremely vulnerable were then randomly assigned to either receive the cash transfer treatment or to serve as a control group.[7] The UCT paid a total of 75,000 Nigerian Naira (roughly USD 693 PPP) over a period of fifteen months ending in March 2017, in either monthly or quarterly disbursements, and was delivered to the primary female decision-maker in the household (see Bastian et al. (2017) for further details of the UCT program).

The cash transfers did not come with any explicit conditions of how the money should be spent or shared. However, during a sensitization campaign, households were told by traditional leaders in their communities that the money was for the female. In a forthcoming paper on the overall impacts of the UCT the authors show that female beneficiaries of the cash transfer increased investment in microenterprises even after the end of the UCT intervention (Papineni et al., 2022).[8] Consequently, UCT households enjoyed significantly higher

[6]The UCT program was one component of the development project: Feed the Future Nigeria Livelihoods Project (FtFNLP) that targeted to help 42,000 very poor households across rural communities in Northwest Nigeria between 2013-2018 and was implemented by Catholic Relief Services (CRS) Nigeria and funded by United States Agency for International Development (USAID).

[7]Note that before the household-level randomization of the cash transfer, villages were also 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 agricultural extension, 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 other programmatic component.

[8]Note Papineni et al. (2022) also find spillover effects of UCT, whereby some non-beneficiary women in

consumption and reduced food insecurity, and shifted away from husband-centered towards joint decision-making in the household. Importantly, Papineni et al. (2022) find that the UCT leads to an increase in a women's empowerment index as measured by a modified version of the Women's Empowerment in Agriculture Index for project use (pro-WEAI). We show, below, that the increase in women's empowerment from the UCT is robust among the sample included in the lab-in-the-field experiment that is the focus of this paper.

One year after the UCT intervention ended, respondents from 27 villages were invited as married couples to participate in lab experiments that took place between March and May 2018.[9] The one-year gap between the RCT and our experiment should diminish pure income effects and allow us to identify longer-term effects in decision–making within the household.

The protocol followed in the lab-in-the-field experiment is described further below. 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 C1). 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 Conceptual framework

As discussed in the introduction, in the standard model of intrahousehold consumption allocation, the decision that a couple makes is represented as the solution to a maximization problem:

$$Max_x \ \omega U_h(x_h, x_c) + (1-\omega)U_w(x_w, x_c) \qquad \text{subject to } p(x_h + x_w + x_c) = y$$

villages where cash transfers were paid also start microenterprises one year after the cessation of the transfers.

[9]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.

where p is a consumption price vector, y is the combined income of the couple, x_h and x_w are the rival consumption vectors of the husband and wife, respectively, and x_c is a vector of non-rival household consumption (e.g., Becker, 1964, 1981; Chiappori, 1988; McElroy and Horney, 1981). The relative welfare weights of the husband and wife ω and $1-\omega$ represent the bargaining power that individual spouses have on the final allocation of consumption: the larger ω, the more power the husband has, and the larger the budget share going to x_h relative to x_w. Hence, the relative bargaining power of the two spouses can be inferred from their consumption pattern (e.g. Cherchye et al., 2017): an empowered wife is one that consumes more rival goods (see, for example, recent papers on Engel curves within households by Bargain and Donni, 2012; Bargain et al., 2014; Brown et al., 2021; Calvi, 2020; Dunbar et al., 2013; Lechene et al., 2019; Sokullu and Valente, 2022; Tommasi, 2019).

This approach to empowerment, however, assumes that spouses do not have social preferences on each other's consumption. A more general treatment would allow the utility (W), in this case of the husband, to take the form:

$$W_h(x) = U_h(x_h, x_c) + \alpha_h U_w(x_w, x_c)$$

where α_h is the social preference parameter of the husband h towards his wife w. This parameter captures emotional empathy, principled altruism, social norms, or a sense of fairness.[10] A similar definition applies to the wife' $W_w(x)$. If spouses have social preferences that are not entirely symmetrical, as is likely, relative consumption is not informative about female empowerment.

The first objective of this paper is thus to test this assumption by granting, to each spouse separately and in secret, dictatorial power on how to allocate a budget y to either x_h or x_w. This is achieved by setting up stalls that carry gender-specific goods. If we find that both spouses share y relatively equally between them when they hold sole executive agency in secret, it means that $1-\omega$, the bargaining weight of married women, cannot be inferred solely from their consumption pattern, we also need to know the social preferences of each spouse. This also means that external interventions may affect consumption by changing social preferences themselves, rather than changing the balance of power ω. Our experiment is specifically designed to investigate this possibility. We use the same approach to examine social preferences over household goods x_c as well.

The literature on female empowerment has also paid attention to the process by which decisions are made in a couple (e.g., Nussbaum, 2001; Sen, 1999): who believes they have the right to decide or not decide how to allocate the household's consumption budget; who

[10] Paternalistic preferences are not discussed here as our experiment is specifically designed not to be affected by them.

consults and who is consulted on that decision; does the consulted spouse distorts his or her reported preferences; and are reported preferences taken into account by the other spouse. Afzal et al. (2022) offer a simple model of demand for agency and control. Our experiment is designed to capture each of these important dimensions of the decision making process within the couple, while controlling for the social preferences of each spouse. This enables us to test whether an external intervention – such as an unconditional cash transfer to married women – can modify the balance of power over the consumption allocation *process* within the couple, separately from its effect on the allocation itself.

We also investigate whether providing secrecy emboldens women to exert executive agency, e.g., by not deferring to their husband. This would indicate a 'pent-up' demand for agency that cannot express itself in public – e.g., because of social sanctions against gender norms or for fear of IPV. We test whether the UCT increases the expression of this pent-up demand by women in secret, or whether it instead encourages them to be more openly assertive.

Next, we examine whether spouses have preferences over the process by which individual consumption choices are made *within* a given budget or choice set. For instance, do husband insist on choosing what their wife eats or drinks, and do wives let husbands choose for them? In this context, the desire to overrule someone else's choice brings no instrumental benefit, since no transfer is allowed across spouses in this case. It must be driven purely by a pure demand for control (e.g. Afzal et al., 2022; Ashraf, 2009; Jakiela and Ozier, 2016).[11] Similarly, deferring this choice to someone else can only yield a less satisfying consumption bundle without increasing the other person's bundle. To this effect, we introduce a situation in which each spouse makes a choice over food and drink for themselves and for their spouse. Participants are then allowed to defer the choice of their food and drink consumption to their spouse. We test whether female recipients of the UCT intervention are less likely to defer.

The literature has shown that assigning (legal or customary) control over financial or productive assets within the couple can affect the allocation of consumption expenditures between spouses (e.g., Browning et al., 1994; Lundberg et al., 1997). In some cases, this can even lead to inefficient choices (e.g., Udry, 1996). The theoretical argument behind these findings is that non-cooperation within marriage is a threat point in the bargaining between spouses, and that asset endowments modify the resources that spouses can appropriate in case of non-cooperation, and thus their bargaining power (Lundberg and Pollak, 1993). A key assumption behind this reasoning is that, in case of non-cooperation, spouses would indeed assign productive resources to maximize their own income. For this to be true, it would have to be the case that spouses have no social preferences, either for efficiency or for a fair

[11] For a deeper understanding of the difference between instrumental and intrinsic control, see the literature on IPV (e.g., Angelucci, 2008; Bobonis et al., 2013; Haushofer and Shapiro, 2016; Hidrobo et al., 2016; Perova and Vakis, 2013; Roy et al., 2018).

distribution of income between spouses.[12]

This assumption is hard to test in practice because, in equilibrium, non-cooperation is not observed. To investigate this important – and untested – assumption, we make this counterfactual decision visible by asking participants to *individually* divide a productive input between two activities, one of which generates income for themselves and the other produces income for their spouse. These allocations generate different income profiles for the spouses, and some are inefficient. We examine whether participants prioretize efficiency over their own self-interest, which would indicate social preferences, or whether they divide the input to improve their material payoff at the expense of their spouse's. We also test whether the UCT intervention weakens or strengthens the efficient usage of the productive asset. A weakening of efficiency may arise if the intervention has created tension between spouses that results in a weakening of their social preferences.

2.2 Purpose of the experimental design

The full experiment in which our work is embedded was designed to provide relevant evidence on whether an Unconditional Cash Transfer (UCT) treatment improves the decision power and material welfare of wives. We go beyond a reduced-form investigation of these two issues by 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 allocations.

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 consumption 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

[12]In the context of our experiment, maximizing the sum of payoffs is equivalent to altruism with equal welfare weights, and is thus a social preference.

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 — they renounce having a direct effect on consumption decisions, which breaks the link between consumption and the UCT intervention, even if the UCT changes women's preferences.

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 an altruistic way by accommodating his wife's communicated preferences. We test this directly by comparing the revised consumption allocation choice that participants make after having been informed of 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.[13]

2.3 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 of 2500 Naira between two categories of goods. This choice by the subject is never revealed to their spouse but it has a positive 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 fixed budget. $y = 2500$.

For three of the four domains (A, B and C) subjects are presented with a small budget and asked how they wish to allocate it to pre-stipulated categories of goods they can only purchase from us at the end of the experiment, thereby reducing the chance of ex post transfers outside the experiment. Once that part of a budget has been assigned to a particular category, it cannot be converted into cash or spent in another category. To facilitate comparison with other experiments in which spouses are given cash, we also include a separate allocation decision (Domain D) 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

[13]This phenomenon, if observed, would be reminiscent of the findings of DellaVigna et al. (2012) whose subjects avoid being asked for something they have difficulty refusing when asked.

desirable, and they include some goods that are easy to buy locally and some goods that only available in town. All participants are shown pictures of the items available in each stall. One stall includes goods intended to appeal to women: jewelry, colorful fabric, and dresses (see Figure C2). The second stall includes male-oriented goods such as hats, caps, belts, shoes, and fabric (see Figure C3). The third stall offers common household items such as cleaning supplies, mats, cups, plates, and mosquito coils (see Figure C4). At the end of the experiment, husband and wife are reunited, one allocation decision is randomly chosen to be implemented, and if that decision is selected at the end of the experiment, 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 or to observe who makes the choices.

By making two of the stalls gender-specific, the above design severely 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 fifth domain in which subjects separately consume a food and drink of their choice directly in the lab, as in Afzal et al. (2022). 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[14]

J The choice between two kinds of cookies and two flavors of juice for both self and spouse.

Choice A 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 $i = \{w, h\}$.

[14]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 match domain D (money) 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).

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.

In each case A-D, the subject is given a budget to divide — in multiples of 100 Naira — 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.

2.4 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 4, 5 and 6 always come last. The order of Blocks 2 and 3 is permuted at random across sessions.

At the end of the experiment, one decision from one of Block 1 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. We describe first the choices in the secret treatment where choices are hidden or shrouded from the subject's spouse. The no-secret treatment is 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 100 Naira increments. This is done in each of the four domains: **split**A-D. Each subject is then asked to split a budget of 2100 Naira[15] 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 and randomized across sessions. These choices are never revealed to the spouse. The purpose of block is 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

[15] 1800 for subjects in the last two sessions, 10% of subjects.

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

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 made decisions in Blocks 1 and 2, subjects are first reminded of their initial allocation across **resplit**A and **resplit**D and then 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 in the secret treatment.

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 misrepresent their true choices from their spouse. These decisions are always visible to the spouse without shrouding.

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 accommodate these preferences, in part or in full. It is never 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[16] of their spouse and asked if they would like to revise their split decisions made in Block 1. 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

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

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 subject is asked to select one of two different cookies (Food) and one of two different 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. (2022). 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 (**defer**F). This decision is not 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. Subjects who do not defer receive the food and drink of their choice; those who defer receive 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 retaining the decision: the subject receives one cookie and a half glass of juice of their spouse's selection if they not defer; but they receive two of their preferred cookies and a full glass if they do defer. The purpose of this design is to elicit a bound on subjects' 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 themself and their spouse. Each input allocation maps into a cash income for themself and a cash income for the spouse. Subjects answered two versions of the game.[17] 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 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 with the other Blocks, in the secret treatment the choice of each spouse is shrouded.

[17]For a small number of sessions, 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: First, it shows whether subjects choose an efficient allocation, and whether they do so even when it is against their individual interest. 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. 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 intrahousehold distribution of income. 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.

2.5 Allocation selection and shrouding

At the end of the experiment, the enumerator determines the cash or voucher allocation that is given to each couple, partly determined by the choices made by both spouses in Blocks 1 to 4 and Block 6, but also shrouded for secrecy. In order that subjects understand how their decisions are implemented (and therefore their incentives) the following process was demonstrated to all subjects before any decisions were made.

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 Block 1 or Block 4, the allocation made by the subject is selected for implementation. When the subject chooses to defer in Block 2 or Block 3, the selected allocation is the corresponding choice made by the spouse in Block1.[18]

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 100 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 subject'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 cannot 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.

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.

For Block 5, subjects 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 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 Block 6, shrouding is achieved in a manner similar to as Blocks 1 to 4.

[18]For instance, if the subject does not defer in Block 2, the selected allocation is their own **split** choice from Block 1; if the subject defers, the selected allocation is the **split** choice of their spouse from Block 1.

2.6 No-secret treatment

We divided the sessions equally (by random selection) into a secret treatment, which is what we have described so far, and a no-secret treatment. The no-secret treatment examines the impact of knowing that your spouse will know if you deferred or accommodated in Blocks 2 to 4, while continuing to incentivize true preference revelation in Block 1 – and similarly for Block 5. Thus, Block 1 is identical in both treatments, meaning that we continue to shroud the **split** and **resplit** decisions made in Block 1 to ensure credible deniability.

In Blocks 2 and 3, in the so secret condition 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.

Block 4 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 Block 4 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 (**communicate**B-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 Block 4 will be revealed to their spouse if the selected ball corresponds to domain j in Block 4.

In Block 5, the food and drink game, the decision to defer is 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 thus consumption (or the refusal to consume) not observed by the spouse.

In Block 6, the input allocation is revealed (if that block and spouse is chosen) in the no-secret treatment but not in the secret treatment.

3 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.

3.1 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.[19]

The first panel of Table 1 shows the four main split decisions with the full budget of 2500 Naira. 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. However, these averages hide a lot of variation across couples. Thirteen and 20 percent of women allocate less than 1000 to themselves in goods and money respectively and 30 and 22 percent of women allocate less than 1000 to their husband in goods and money respectively. For men, 22 percent and 11 percent allocate less than 1000 to themselves in goods and money and 20 and 36 percent allocate less than 1000 to their wife in good and money respectively. Note that men tend to allocate a larger budget share to female goods sold in the lab (1298>1250), but less to money for their wives (1062<1250); money that could spent outside the lab. Note that if we convert the amount a man gives to his wife in good or money into the amount he keeps for himself, women are more selfish in goods than men are (1410 > 1202, p-value<0.00) and women are less selfish in money than men are (1265 < 1438, p-value<0.00). This may reflect the fact that the items sold in the lab shop appeal more to women than men or that men had planned to buy female goods on their wife's behalf both in and outside of the lab.[20]

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 our sample population. From this evidence we conclude that spouses have social preferences, meaning that they incorporate the

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

[20]Recall that, although a couple must spend tokens in the stall for which they are designated, we have no way of forcing couples to buy things chosen by either the husband or wife.

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. In nearly 50% of the couples, the difference between the allocation chosen by the wife and that chosen by her husband is more than 400 Naira. Very few couples (around 10%) have an identical allocations. 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.

3.2 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. Our findings, summarized in Panel II of Table 1, offer arguably the most striking contrast between spouses in our study: 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.

The second 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 (**communicateB-D**)and the original split (**splitA-D**): 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.[21]

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 misrepresentation.

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

In Table 3 we examine three comparisons between the initial allocation (always shrouded), the communicated allocation (always visible) and the revised number (sometimes shrouded). The column labeled (s-c) compares the split to the communicated split. For both men and women, the changes are relatively small and the only difference large enough to be statistically significant at the 5% level for choice B for women the only decision in which there is no direct allocation for the women themselves.

The third 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.

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 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.

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 empowerment', meaning that a subject feels entitled to impose his or her consumption allocation preferences on their spouse. To capture this idea, we construct a categorical variable that compares the revised split (**revisedB-D**) to the original split (**splitA-D**). 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.

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 both women and men, however, these differences were already present in their original splitting decisions. The column labeled s-r in Table 3 compares the initial split to the revised split and shows the p-value of the difference: there

is no significant difference between the original and revised splits for women or men. The same pattern is apparent when we compare communicated and revised splits (column c-r). Note that the total changes across split, communication and revision are very small for men, but for women, there are large (though not statistically significant) changes in the amount of money they allocate to themselves, but most of that change is already present in their communication.

In Panel VI of Table 1 where we examine in more detail the extent to which husbands and wives accommodate the communicated split of their spouse when revising their original choices. Here, accommodation is measured compared to the communication received from the spouse: do subjects move towards their spouse's preferences? 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 B1, 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, vs 98 (91%) for domain D, husbands chose to not revise their decision even though they have been told their spouse prefers something else.

Another interesting pattern arises if we break down accommodation between those who consult their spouse or not; results are shown in the appendix as Table B2 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.[22]

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 B2 suggests that a large proportion of the women who do not ask to see their husbands' choices end up caving in

[22]In Table B4 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.

when shown these choices.

3.3 The 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 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 Block 5. 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, it can only satisfy j's desire for control. 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.

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. Do they believe their spouse knows what they want? Table B3 in the Appendix shows that partners do a bad job of picking for their spouse. 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.[23] This suggests that, on average, deferral has a material utility cost for subjects: they are less likely to consume the items they prefer.

In Table 4, we examine the rate at which subjects defer when they are faced with varying costs of deferral. Since the change in the size of the budget represents a cost of deferral, observed choices map out the demand for agency as a function of the cost.

In domain A (women's versus men's goods) subjects made three sets of deferral choices: 1) when the deferral used the spouse's decision over 2500 compared to the retention using the own decision over 2100; 2) when the budgets were both 2500 and 3) when the deferral used the spouse's decision over 2100 compared to the retention using the own decision over 2100.

In domain D (money for the woman versus money for the man) subjects made two sets of deferral choices: 1) when the deferral used the spouses decision over 2500 compared to the

[23] In a similar experiment, Afzal et al. (2022) show that subjects are no better informed about the preferences of their spouse than a stranger.

retention using the own decision over 2100; and 2) when the budgets were both 2500.

In domain J (non-rival choices over food and drink) subjects made one choice but with two randomly allocated deferral budgets: 1) deferral was for two cookies and a full glass of juice while retention was for one cookie and half a glass and 2) deferral and retention had the same budget. Note that, in domain J, no subject faced both choices: the differences are across, not within subjects.

In Table 4, we see, for women, a downward sloping demand for the choice to defer: the higher the price of deferring, the less likely women are to not defer their choice. 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 — a high and price-inelastic demand for agency.

It is possible that women (and men) defer because they believe their spouse will chose for them something that is more advantageous than what they would have chosen for themselves. If so, this should be particularly evident in the cases where we presented different budgets for the retain and defer the decision. The structure of our experiment is such that we observe both what someone chooses for themselves and what their spouse chooses over the same choice set and we can compare the individual payoffs for both choices to see if deferring is, indeed, sometimes optimal. 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 depends on the split decisions of their spouse. Using this procedure, we can calculate the proportion of cases in which it would be optimal for subject to defer. The results of this calculation are shown in Table 5. We see that, on average, women over-defer relative to what would be optimal, a difference that is always significant. In contrast, husbands massively under-defer: 20-21% compared with 60-64% of optimal cases, a difference that is always significant.

In the appendix, in Table B5, we examine whether the choice to defer is related to the potential gains from deferring, including gains to self and gains to one's spouse. We average 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. We also test whether subjects are deferring because they know it will benefit their spouse: if subjects had both perfect foresight and perfect information, and if subjects care about their partners allocation over their own, they would defer when their partner gains and not defer if their partner loses. We see no evidence of either pattern. Note that the information required to optimize these decisions could have been used to pick the preferred allocation to begin

with. In this setting, deferral is an inefficient way to improve your own payoff or to improve the payoff of your spouse. In the appendix, Table B6 demonstrates the same pattern for food and drink: subjects are no more likely to defer when their spouse correctly guesses their preference than when they do not.

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.[24] There is no evidence that deferral is a method for improving material outcomes, but, like a normal good, demand for deferral does respond to costs. 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.

3.4 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 Block 6, 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 intrahousehold allocation implicitly or explicitly assumes efficiency in production decisions. Yet we know that input endowment effects can impede production efficiency of households in Africa (see Fafchamps and Kebede, 2022; Udry, 1996).

The two production games played in Block 6 examine whether subjects make decisions that are efficient for the couple.[25] 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.

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

[24] In addition, the likelihood of deferral is unchanged when the decision to defer is not communicated to the spouse.

[25]

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 2. 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 Fafchamps and Kebede (e.g., 2022)? Since input allocations are the same in each game, if choices are made in the input domain, subjects should make the same choice in each game however, 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 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, 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 (2022) for teams: in both cases, these authors find evidence that entitlement effects impede 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 choose the "right" allocation rather than the "best" outcome and, ex post, reallocate. With this understanding in mind, we now examine whether offering an unconditional cash transfer to women modifies these preferences.

4 Effect of the UCT treatment and no-secret condition

Equipped 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 and the secret condition on the behavior of subjects in the lab.

As part of the UCT, women were asked a series of questions about decision making in their households before and after receiving the program (Papineni et al., 2022).[26] These questions were used to create a modified index of empowerment, the project Women's Empowerment in Agriculture Index (pro-WEAI). Table 6 shows that, in the baseline, the UCT and secret treatments were balanced but that, at endline, women who received the UCT report greater decision-making responsibilities (Papineni et al., 2022). In contrast, their reports are not affected by their assignment to the no-secret condition in the lab, which is as it should be .[27]

4.1 Testing strategy

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

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

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

w_i is any of the budget allocations or other decisions made by the subject, 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 $\beta_3 = \beta_1' + \beta_2' + \beta_3'$. 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 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

[26] Studies in contexts similar to ours have shown that men and women do not give the same answers when asked who makes decision in the household. But women's answers are correlated with actual power in the household, even when their husbands do not agree with their answers (Ambler et al., 2021).

[27] In both the baseline and follow-up empowerment measures, women in the secret and not secret treatments do not have different empowerment scores. Since the secret treatment only applies to the lab setting, the decision-making index for women should not differ by this assignment, which is what we find.

that their decisions to defer, consult, and accommodate all fall with treatment. The need to misrepresent 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.

4.2 Budget allocation decisions

We begin with the initial split decisions of husbands and wives. As explained in Section 3, initial split decisions (Block 1) 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. Since choices are shrouded, we expect no effect of the no-secret treatment.

To investigate this, we show in Table 7 the estimates obtained by regressing split decisions on a UCT treatment dummy.[28] We see that that women who experienced the UCT increased the allocation to themselves in goods and money by 67 Naira in domains A and D, for a total change of 134 Naira. Only the total allocation is significantly different between those who received the UCT and those who did not (the p-values for domain A and D are 0.103 and 0.107 respectively). The total change in A and D for men, by comparison is 12 Naira, an amount both statistically and economically non-significant. There is also suggestive evidence that women allocated more to the household versus their husband (domain B). The increase in household allocation is 51 Naira (p-value = 0.116)

To summarize, we see that the UCT treatment changed the amounts women are willing to allocate themselves (and their household compared to their husband). The changes are marginally significant, but economically small. The total change in the amount women allocate to themselves after an eighteen month cash transfer program is 134 Naira, 5% of the average allocation across domains A and D. Since men do not change their allocation, the treatment can be seen as widening (slightly) the divide between women's and men's preferences.

4.3 Decision Process

In Table 8 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 condition. The difference is large in magnitude – between 12 and

[28] We confirm in Table B7 that the secret/no-secret treatment has no separate significant effect on split decisions.

17 percentage points — and it is significant in all cases and in the aggregate (column A-D). No such changes are 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 B7, these results suggest that the UCT treatment has made women secretly want more consumption *and* agency. But it has not made them more vocal or openly 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.

Table 9 presents a similar analysis for the decisions to misrepresent, consult, and accommodate. Note that the initial allocation is always secret, the communicated allocation is never secret and the decision to consult is always secret. Therefore the only expected effect of the secret condition should be in accommodation, where, in the no-secret condition, the spouse could learn the final revised allocation.

In the decision to misrepresent, 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 misrepresent more in the secret condition; the effect is only significant at the 10% level in one regression. For husbands we observe more misprepresentation 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. There is no evidence for an effect of the UCT treatment alone.

In the second panel of Table 9 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 9 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. 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.

4.4 The demand for Agency

In the control group, we saw that there is a demand for agency: the willingness to defer is sensitive to the cost of deferral. In a parallel analysis, we show here that the demand curve is shifted by the treatment.

We examine the effect of the UCT and no-secret treatments in Table 12 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. 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.[29] 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.[30]

In the secret condition the deferral decision cannot be discovered by the spouse; otherwise, it can. 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 the UCT treatment but is afraid of retribution if it is revealed that she did not defer to her husband, she may refrain from deferring only in the secret condition. The impacts of the treatment 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.

4.5 Aggregate welfare and secrecy

Here, 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

[29] 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.

[30] This can be read as a reaction to experimental demands for deferral, but should not be interpreted as a reaction to female agency; men are pushing back on the experimenter, not necessarily their wives.

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.[31] These expected payoffs are what subjects can expect to receive purely for themselves at the end of the experiment, based on their own split and defer decisions and the decisions of their spouse.

Coefficient estimates are presented in Table 10. Although the point estimate for the UCT is positive it is not significant. The material payoff of women only increased in the UCT/secret treatment. The increase in this category is about 5 percent of the total possible payouts. 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.

4.6 Allocative Efficiency in Production

Finally, 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 that the UCT treatment should increase or decrease efficiency, since theory suggests efficiency is a dominant strategy in all households, irrespective of the decision-making

[31] The exact formulas used are:

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

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

process. Moreover, the game does not allow deferring production 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.

Table 11 summarizes 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 versions 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 in the no secret condition makes wives less 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. Recall that women cannot hide income from their husband in the secret condition, but they can hide whether they made the efficient choice. We did not see such an effect in the split decisions from Block 1, but recall that those allocations were always secret. The result we see here suggests that a lack of secrecy could have affected their choices.

Interestingly, the women who received the UCT in the secret condition are no different from the control women in the no-secret condition, but they are different from both the UCT and secret condition. This suggests that secrecy has a different effect for the control women than it does for the women in the UCT. For women who received the UCT, secrecy increases the probability that they will seek the efficient outcome whereas for women in the control group secrecy increases the chance that they will deviate from the efficient outcome.[32]

The strongest result in this table is that couples are jointly making 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.

5 Conclusion

In a setting in which women have very low bargaining power and agency within households, we use a lab-in-the-field experiment to study 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 year long unconditional cash transfer on the process by which

[32]In Appendix Table B8, we see that there are no significant effects on payments to wives or husbands for any of the treatment categories, either overall or on the margin.

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 we had expected in this setting. However, the *way* that allocations are chosen is highly biased. Women are more likely than men to consult their spouse when offered the option, to misrepresent the choices they communicate to their spouse (stating they want less than they originally chose), and to accommodate their spouse's choices after being informed of them. In addition to observing the negotiation process over outcomes, we allowed both men and women to skip this process by deferring their decision to their spouse. We find a marked contrast between men and women. Women defer 67% of their decisions to their spouse while men only defer between 20 and 25% of their decisions. Furthermore, men fail to defer even when the allocation is a choice between household items and female items, a choice over which they likely have little at stake in the context of our experiment.

The process by which decisions are made nonetheless leads to final allocations that are not, on average, different from the initial choices of either spouses. In fact, any examination of the initial and final choices of the couples would give the impression of significant female bargaining power. Our experiment however demonstrates that intrahousehold allocative fairness is achieved through 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: in the local context of our experiment, men 'need' to be decision makers and women 'need' to let their husband decide. Men in particular, show no sensitivity to the costs or benefits of deferral.

This leads to inefficiency. Although husbands on average make choices that are similar to the choices of their wives, women defer to them even when doing so reduces their joint surplus. Husbands are making similarly expensive decisions by failing to defer even when doing so reduces the couple's joint surplus, even though their wives would (on average) have made the same decisions that they make. The inefficiency is apparent in these costly decisions as well as in the two production games. 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 quite visible in the

production 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 this to happen – a result that is reminiscent of the findings of Bertrand et al. (2015) in a developed country context.

We do see some 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 as a result of wives receiving the UCT. 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. This effect is much more muted than the findings of Klein and Barham (2019) who estimate that the Progresa experiment in Mexico shifted women's bargaining power weights from 25% of their husband's weight before the cash transfer to 70% afterwards. It should be noted, however, that the baseline agency and bargaining power of women in that setting was much higher than in ours.

Our 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 the distribution of power within the household. In addition, our findings suggest that caution is warranted on the popular notion that giving cash to women is a sure way of changing relationships within the household. We find that 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 households decisions are shrouded and therefore effectively secret). But it is far from the assumption that a cash transfer will significantly improve agency in the household.

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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 cookie and juice	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: Differences between Husbands and Wives 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[1]	1176[1]	1.34	0.182	251
Avg. total alloc.						2249[2]	2208[2]	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) at p-value < 0.01
[2] significantly different from the most efficient outcome (2500) at p-value < 0.10

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	1328	1323	198	0.930	0.668	0.760
Husband	C	1222	1240	1230	223	0.182	0.606	0.556
Husband	D	1081	1076	1089	123	0.604	0.526	0.319

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 misrepresent. 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)

		Rate of deferral			
		The budget when deferring is			
	Choice:	larger	the same	smaller	N
Wife	A	74.1%	68.3%	53.2%	139
	D	73.2%	71.1%		97
	J	93.0%			86
	J		68.5%		165
Husband	A	20.1%	26.6%	22.3%	139
	D	19.6%	24.7%		97
	J	25.6%			86
	J		19.4%		165

Note: The dependent variable is the choice to defer the allocation to the spouse. The number of observations differs because some treatments only appear in certain sessions. Drink and Juice choices were only posed once for each participant. Some faced a larger budget if they deferred (two cookies and one full glass of juice) than if they did not (one cookie and a half glass) and some faced the same budget for either decision.

Table 5: Examining Deferral Efficiency

Comparing deferral propensity to optimal own deferral						
Domain: retain v. defer	decider	**Actual**	**Optimal**	t-stat	p-value	N
A 2500: 2500	Wife	67%	46%	4.85	0.000	223
	Husband	25%	46%	-5.08	0.000	223
D	Wife	67%	44%	5.46	0.000	251
	Husband	26%	44%	-4.65	0.000	251
A 2100: 2500	Wife	74%	61%	2.54	0.012	153
	Husband	21%	64%	-8.69	0.000	154
D	Wife	73%	49%	3.47	0.001	97
	Husband	20%	60%	-6.35	0.000	97
A 2500: 2100	Wife	53%	34%	3.27	0.001	154
	Husband	21%	36%	-2.97	0.003	154

Notes: All deferral choices refer to situation in which the subject must choose between their own split or the split choice made by their spouse. 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 (or the same) for self by deferring to spouse.

Table 6: Female empowerment in the experimental sample

	Treated and Untreated				
	Control	UCT treatment	t-stat	p-value	N
Baseline project-WEAI (6 questions)	1.94	2.09	-1.42	0.155	503
Final project-WEAI (12 questions)	4.42	5.02	-3.96	0.000	446
	Open and Secret Treatments				
	Not Secret	Secret	t-stat	p-value	N
Baseline project-WEAI (6 questions)	1.97	2.05	-0.78	0.433	503
Final Project-WEAI (12 questions)	4.60	4.83	-1.465	0.146	446

Women in treated and control households completed a survey on empowerment using a modified version of the Women's Empowerment in Agriculture Index, (project-WEAI) before the project began and after the project ended. They surveys conducted before and after the experiment were not the same so we do not present the double difference.

Table 7: Effect of UCT on split decisions

Dependent Variable is Split Decision by the Wife for choice:	A	B	C	D	A + D
UCT	66.83	51.40	-16.81	66.81	133.6**
	(1.674)	(1.610)	(-0.535)	(1.650)	(2.164)
Constant	1,410***	1,396***	1,168***	1,265***	2,674***
	(61.07)	(49.22)	(40.89)	(41.66)	(74.57)
Observations	503	503	502	503	503
R-squared	0.008	0.004	0.000	0.007	0.014
Dependent Variable is Split Decision by the Husband for choice:	**A**	**B**	**C**	**D**	**A + D**
UCT	-8.921	5.295	-11.96	-3.422	-12.34
	(-0.287)	(0.131)	(-0.327)	(-0.104)	(-0.248)
Constant	1,298***	1,316***	1,214***	1,062***	2,360***
	(50.96)	(46.06)	(37.08)	(40.28)	(64.73)
Observations	503	503	502	503	503
R-squared	0.000	0.000	0.000	0.000	0.000

Notes: The dependent variable is the initial split decision (between 0 and 2500) made by each subject in each of the four domains: A: female v. male goods; B: household v. female goods; C: household v. male goods; D: female v. male money. UCT refers to the subjects who received the UCT treatment.
Robust t-statistics in parentheses, clustered by session. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Treatment effects on deferral decisions

Dependent Variable is Deferral Decision by the Wife for choice:	A	B	C	D	J	A-D
UCT/no-Secret	-0.050	0.011	-0.064	-0.013	-0.089	-0.141
	(-0.871)	(0.149)	(-1.112)	(-0.182)	(-1.167)	(-0.619)
no UCT/Secret	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/Secret	-0.143**	-0.123*	-0.124*	-0.168**	-0.133	-0.591**
	(-2.148)	(-1.861)	(-1.784)	(-2.509)	(-1.416)	(-2.395)
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
p-value of marginal effect‡	[0.280]	[0.075]	[0.729]	[0.098]	[0.952]	[0.126]
Dependent Variable is Deferral Decision by the Husband for choice:	A	B	C	D	J	A-D
UCT/no-Secret	-0.052	-0.030	0.008	-0.092	-0.049	-0.196
	(-1.229)	(-0.656)	(0.173)	(-1.578)	(-0.851)	(-1.450)
no UCT/Secret	-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/Secret	-0.002	-0.022	0.061	-0.068	-0.009	-0.043
	(-0.036)	(-0.484)	(1.040)	(-1.331)	(-0.124)	(-0.235)
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
p-value of marginal effect‡	[0.434]	[0.468]	[0.287]	[0.106]	[0.026]	[0.119]

Notes: The dependent variable is the deferral decision made by each subject in each of the four deferA-D decisions and in the deferJ decision from Block 5. A: female v. male goods; B: household v. female goods; C: household v. male goods; D: female v. male money, J: food and drink. The dependent variable 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.: Each regressor corresponds to a different treatment category with the intercept corresponding to the no-UCT/no-secret category.
‡ is the p-value of the F-test that UCT / Secret = UCT + Secret and tests the marginal contribution of the combined treatments compared to the sum of the contributions.
Robust t-statistics in parentheses, clustered by session. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Treatment effects on decisions to misrepresent, consult, and accommodate

decision by spouse:	Wife			Husband		
for choice:	B	C	D	B	C	D
Panel A: Dependent variable is Misrepresent						
UCT treatment	-0.992	-25.84	-28.91	-15.52	18.24	-19.85
	(-0.0308)	(-1.234)	(-0.718)	(-1.081)	(1.447)	(-1.557)
Secret treatment	-19.54	-24.89	-16.56	17.47	-17.95	-6.405
	(-0.655)	(-1.266)	(-0.650)	(1.196)	(-1.526)	(-0.632)
Intercept	-24.16	22.48	-8.997	-7.197	-9.038	8.002
	(-1.179)	(1.215)	(-0.320)	(-0.665)	(-0.720)	(0.951)
Observations	394	450	245	394	450	245
R-squared	0.001	0.008	0.005	0.007	0.007	0.013
Panel B: Dependent variable is Consult						
UCT treatment	-0.0310	-0.0801	-0.0582	-0.0483	-0.0381	-0.0822
	(-0.607)	(-1.597)	(-1.032)	(-1.394)	(-1.150)	(-1.732)
Secret treatment	-0.0594	-0.0706	-0.113*	-0.0768	-0.0761*	0.0190
	(-1.051)	(-1.282)	(-1.969)	(-1.463)	(-1.903)	(0.332)
Intercept	0.578***	0.598***	0.649***	0.263***	0.233***	0.186***
	(13.39)	(14.18)	(20.77)	(6.962)	(6.558)	(3.799)
Observations	394	450	245	394	450	245
R-squared	0.005	0.012	0.018	0.014	0.013	0.013
Panel C: Dependent variable is Accommodate						
UCT treatment	0.0157	-0.0286	0.00611	0.00382	-0.0522	-0.0165
	(0.381)	(-0.694)	(0.111)	(0.0944)	(-1.342)	(-0.503)
Secret treatment	-0.0564	-0.0381	-0.0159	-0.0247	0.00496	-0.0735*
	(-1.027)	(-0.709)	(-0.291)	(-0.554)	(0.117)	(-1.801)
Intercept	0.661***	0.691***	0.462***	0.214***	0.190***	0.174***
	(16.56)	(17.40)	(10.67)	(5.016)	(5.139)	(5.331)
Observations	392	449	243	394	450	245
R-squared	0.004	0.003	0.000	0.001	0.005	0.013

Notes: Regression results are shown for three types of decisions relating to choices B-C-D. In Panel A, the dependent variable Misrepresent 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 Misrepresent 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. Each regressor corresponds to a different treatment category with the intercept corresponding to the no-UCT/no-secret category.

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

Table 10: Treatment effects on expected material payoff for self

Regressors:	Wife	Husband
UCT/no-Secret	49.70	11.30
	(1.639)	(0.382)
no UCT/Secret	4.964	23.36
	(0.169)	(0.618)
UCT/Secret	59.84*	0.907
	(2.004)	(0.025)
Intercept	1,295***	1,256***
	(79.89)	(54.13)
Observations	449	449
R-squared	0.015	0.001
p-value of marginal effect‡	0.906	0.410

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. Each regressor corresponds to a different treatment category with the intercept corresponding to the no-UCT/no-secret category.
‡ is the p-value of the F-test that UCT / Secret = UCT + Secret and tests the marginal contribution of the combined treatments compared to the sum of the contributions.
Robust t-statistics in parentheses, clustered by session. *** p<0.01, ** p<0.05, * p<0.1

Table 11: Treatment effects in the Production Game: Efficiency

efficiency type	strong	weak	game 1	game 2
Dependent variable is the decision by the wife				
UCT/no-Secret	-0.194***	-0.173***	-0.142*	-0.089
	(-2.858)	(-2.883)	(-1.807)	(-1.557)
no UCT/Secret	-0.140*	-0.121*	-0.051	-0.094
	(-1.779)	(-1.787)	(-0.658)	(-1.297)
UCT/Secret	-0.063	-0.034	-0.032	-0.041
	(-0.849)	(-0.516)	(-0.543)	(-0.474)
Constant	0.396***	0.412***	0.536***	0.598***
	(7.127)	(8.439)	(11.130)	(10.949)
Observations	392	503	434	461
p-value of marginal effect‡	[0.011]	[0.004]	[0.131]	[0.153]
Dependent variable is the decision by the husband				
UCT/no-Secret	-0.036	-0.019	-0.067	0.015
	(-0.545)	(-0.327)	(-0.872)	(0.214)
no UCT/Secret	0.085	0.074	0.071	0.031
	(1.560)	(1.265)	(1.245)	(0.514)
UCT/Secret	0.086	0.088	-0.013	0.131**
	(1.513)	(1.512)	(-0.223)	(2.151)
Constant	0.238***	0.267***	0.500***	0.369***
	(5.251)	(5.605)	(10.683)	(8.017)
Observations	392	503	434	461
p-value of marginal effect‡	[0.648]	[0.650]	[0.847]	[0.346]
Dependent variable is the decision by the couple				
UCT/no-Secret	-0.019	0.011		
	(-0.719)	(0.353)		
no UCT/Secret	-0.001	0.008		
	(-0.040)	(0.249)		
UCT/Secret	0.078**	0.101***		
	(2.332)	(2.983)		
Constant	0.079***	0.092***		
	(3.390)	(4.234)		
Observations	392	503		
p-value of marginal effect‡	[0.041]	[0.081]		

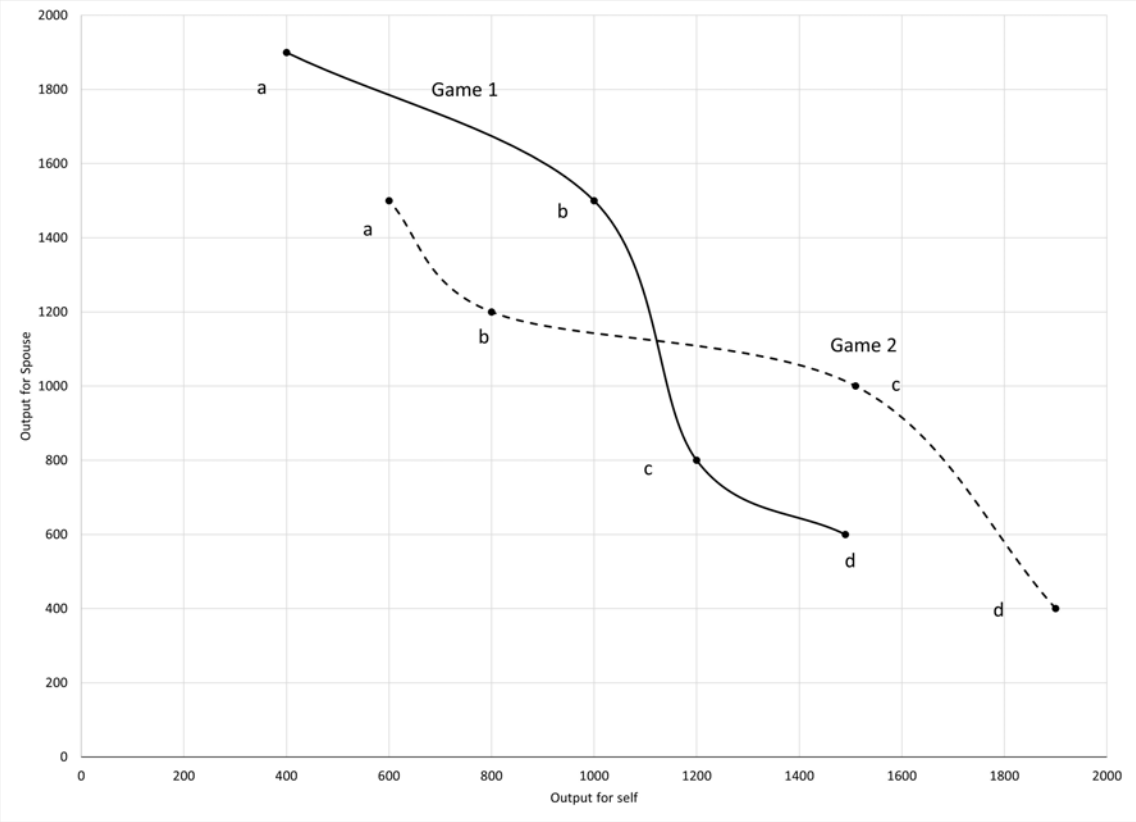
Notes: The dependent variable is a different dummy variable in each column. In the strong efficiency column, the dependent variable is 1 if the subject chooses the efficient outcome in both games. In the weak efficiency column, it is 1 if the subject chooses the efficient outcome in one game and was 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. Each regressor corresponds to a different treatment category with the intercept corresponding to the no-UCT/no-secret category.
‡ is the p-value of the F-test that UCT / Secret = UCT + Secret and tests the marginal contribution of the combined treatments compared to the sum of the contributions.
Robust t-statistics in parentheses, clustered by session. *** p<0.01, ** p<0.05, * p<0.1

Table 12: 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/no-Secret	-0.0182	-0.0417
	(-0.426)	(-1.079)
no UCT/Secret	0.0106	-0.0385
	(0.266)	(-1.003)
UCT/Secret	-0.128***	-0.0168
	(-3.047)	(-0.438)
Constant	0.648***	0.249***
	(21.54)	(8.211)
Observations	2,284	2,284
R-squared	0.045	0.003
p-value of marginal effect‡	[0.046]	[0.230]

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 food and drink 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. Each regressor corresponds to a different treatment category with the intercept corresponding to the no-UCT/no-secret category.
‡ is the p-value of the F-test that UCT / Secret = UCT + Secret and tests the marginal contribution of the combined treatments compared to the sum of the contributions.
Robust t-statistics in parentheses, clustered by household. *** p<0.01, ** p<0.05, * p<0.1

Figure 1: The production games in output space



Online Appendix

A 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. 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.

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 solution will follow the same principle..

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' Fafchamps and Kebede (e.g., 2022).

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 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 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 $j \neq i$.[33] 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 –[34] and we allow function $U(.)$ to be concave to capture satiation/risk aversion, 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.[35]

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.

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

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

[35] 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.

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).

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.

B Additional Tables and Figures

Table B1: 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 B2: 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 B3: 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 B4: 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 B5: Examining Deferral Benefits in the Control Group

Domain	retain vs. defer	decider: beneficiary	When does **not defer**	When does **defer**	t-stat	p-value	N_1	N_2
A	2500: 2500	Wife for Self	-132	-99	-0.40	0.686	73	150
		Husband for Self	-149	10	-1.83	0.069	168	55
D		Wife for Self	-259	-175	-1.10	0.272	82	169
		Husband for Self	-235	-108	-1.57	0.117	186	65
A	2100: 2500	Wife for Self	23	145	-1.32	0.190	40	113
		Husband for Self	129	206	-0.82	0.411	121	33
D		Wife for Self	-123	10	-1.02	0.312	26	71
		Husband for Self	90	16	0.50	0.615	78	19
A	2500: 2100	Wife for Self	-222	-284	0.81	0.421	73	81
		Husband for Self	-309	-203	-1.05	0.295	121	32

The gain to self is defined as the subject's own gain when comparing own allocation choice to the allocation they would receive if they deferred to their spouse. Each row compares the gain when the subject defers to when they do not. The number of observations in each cell N_1 and N_2 depends on the subject's own deferral decision.

Domain	retain vs. defer	decision: beneficiary	When does **not defer**	When does **defer**	t-stat	p-value	N_1	N_2
A	2500: 2500	Husband for Wife	-149	10	-1.83	0.069	168	55
		Wife for Husband	132	99	0.40	0.686	73	150
D		Husband for Wife	-235	-108	-1.57	0.117	186	65
		Wife for Husband	259	175	1.10	0.272	82	169
A	2100: 2500	Husband for Wife	130	206	-0.81	0.419	120	33
		Wife for Husband	378	255	1.32	0.190	40	113
D		Husband for Wife	90	16	0.50	0.615	78	19
		Wife for Husband	523	390	1.02	0.312	26	71
A	2500: 2100	Husband for Wife	-140	-172	0.34	0.735	121	32
		Wife for Husband	-308	-268	-0.49	0.621	73	80

The gain to self is defined as the subject's own gain when comparing what they would receive from their spouses choice to what they would receive if their spouse decided to defer the choice (in which case the self allocation is the decision made by the subject). Each row compares the gain when the subject's spouse defers to when they do not. The number of observations in each cell N_1 and N_2 depends on the subject's spouse's deferral decision.

Table B6: 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 choice for subject	-0.0537	-0.0113	0.0215	0.0473
	(-1.135)	(-0.259)	(0.870)	(0.701)
Intercept	0.743***	0.206***	0.909***	0.210**
	(11.08)	(3.463)	(21.29)	(2.424)
Number of observations	165	165	86	86
R-squared	0.006	0.000	0.003	0.004

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 or the choice of cookie is the same, and 2 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.Robust t-statistics in parentheses, clustered by experimental session. *** p<0.01, ** p<0.05, * p<0.1

Table B7: Treatment effects on split decisions

Dependent Variable is Split Decision by the Wife for choice:	A	B	C	D
UCT/no-Secret	116.4**	69.27	-11.80	87.31*
	(2.067)	(1.386)	(-0.228)	(1.710)
no-UCT/Secret	47.56	51.16	4.099	-16.69
	(1.039)	(0.907)	(0.070)	(-0.275)
UCT/Secret	66.31	81.58	-17.50	34.15
	(1.221)	(1.562)	(-0.382)	(0.606)
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
p-value of marginal effect‡	[0.222]	[0.550]	[0.882]	[0.653]
Dependent Variable is Split Decision by the Husband				
for choice:	A	B	C	D
UCT/no-Secret	-40.84	41.84	-29.66	-53.45
	(-0.914)	(0.848)	(-0.564)	(-1.288)
no-UCT/Secret	-67.24	55.65	-87.81	-70.77
	(-1.376)	(0.970)	(-1.388)	(-1.349)
UCT/Secret	-41.26	23.29	-74.98	-23.22
	(-0.763)	(0.370)	(-1.340)	(-0.430)
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
p-value of marginal effect‡	[0.273]	[0.361]	[0.557]	[0.117]

Notes: The dependent variable is the initial split decision (between 0 and 2500) made by each subject in each of the four domains: A: female v. male goods; B: household v. female goods; C: household v. male goods; D: female v. male money. Each regressor corresponds to a different treatment category with the intercept corresponding to the no-UCT/no-secret category.
‡ is the p-value of the F-test that UCT / Secret = UCT + Secret and tests the marginal contribution of the combined treatments compared to the sum of the contributions.
Robust t-statistics in parentheses, clustered by session. *** p<0.01, ** p<0.05, * p<0.1

Table B8: Treatment effects in the Production Game: Outcomes

output decision by		wife			husband	
recipient	wife	husband	total	wife	husband	total
UCT/no-Secret	-45.538	22.293	-12.080	-27.023	56.978	-23.667
	(-1.011)	(0.487)	(-0.537)	(-0.452)	(1.193)	(-0.972)
no UCT/Secret	-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/Secret	-16.325	14.747	1.346	22.977	-1.448	19.064
	(-0.445)	(0.381)	(0.058)	(0.440)	(-0.031)	(0.827)
	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
p-value of marginal effect‡	[0.102]	[0.602]	[0.693]	[0.385]	[0.256]	[0.400]

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. Each regressor corresponds to a different treatment category with the intercept corresponding to the no-UCT/no-secret category.
‡ is the p-value of the F-test that UCT / Secret = UCT + Secret and tests the marginal contribution of the combined treatments compared to the sum of the contributions. Robust t-statistics in parentheses, clustered by session. *** p<0.01, ** p<0.05, * p<0.1

C Experimental Instructions and Materials

C.1 Introduction

Upon arrival, subject pairs (i.e., married couples) were assigned to their respective gender-specific room. This means that all the husbands were put in one room, while all the wives were put be in a separate room. Thus, husbands and wives could not directly interact with each other during the sessions.

Our plan was to conduct two experimental sessions for each experimental site on the same day. However, this depended on the size of the impact evaluation sample in the nearby villages. If we had enough participants to have two sessions, we conducted both sessions consecutively, allowing no possibility for interaction between experimental subjects in the different sessions. We ensured this by having the subjects from the first round leave the premises of the experiment immediately after their session ended, without providing any opportunity to interact with subjects from the following round, who would wait in a separate room. Research Assistants (RAs) at the site implemented this "no interaction" protocol between subjects in consecutive sessions. At the end of the first round, one RA was asked to stand outside the waiting room for the subjects of the second round, while another RA guided subjects from the first round out of the experimental site. Both RAs were instructed to ensure that experimental subjects from the two sessions did not interact with each other.

Experimental subjects were seated in compartments separated by curtains made of opaque clothing material. Thus, subjects were not able to see each other or observe each others' experimental decisions. This ensured the privacy of decisions during the experiment. Any contact between subjects during the session was strongly discouraged. Moreover, subjects were not allowed to verbally communicate any of the experimental decisions to their assigned enumerators. Rather, they communicated all their decisions using experimental prompts, e.g, envelopes and pictures that denoted binary choices and (laminated) cash prompts to allocate between binary choices. This further protected privacy and subdued any mimicking effort.

Subjects were thanked for coming to the session and were handed the participation fee: "Thanks for coming today. Before we start, we would like to give you 250 NGN as a compensation for your time. This amount is not part of today's activities and is yours to keep. You and your spouse, who is in a different room now, will make a number of decisions today, through which your household can earn additional money. At the end of this session, only one of the decisions that you and your spouse make will be chosen via a lottery, which is likely to be implemented as final pay-off for your household. Since any of your decisions can be chosen, you should take each of your decisions seriously and carefully. In addition to the participation fee, the pay-off for your household can be between 1,800 NGN and 2,700 NGN, in cash or in kind, depending on which round is chosen from the lottery for pay-off. Again, only one decision of all the decisions that both you and your spouse make today will be chosen through a lottery as final pay-off for your household."

A visual demonstration was then shown which demonstrated how one decision - from all the decisions made by the husband and the wife in the experimental session - was going to be chosen by a lottery as final pay-off for the household. The decision rounds for female experimental subjects were represented with white numbered balls while the decision rounds

for male subjects were represented by orange numbered balls. The numbers on the balls denoted different rounds. In the demonstration, subjects were told that at the end of all the decisions by the husband and the wife, all the white and orange numbered balls would be put in a basket. One ball would be taken out and the corresponding decision was likely to be implemented as the final pay-off for the household.

C.1.1 Plausible deniability ("secret-keeping" round):

The pay-off process, at the end of the experimental session was designed to ensure that the individual, private choices made during the experiment were not revealed to the subject's spouse or anyone else (unless warranted by the experimental treatment as explained in the following discussion point). Recall that at the end of a session, based on which numbered ball was randomly drawn out of the basket, a particular decision round would be chosen as pay-off for each household. This decision round could have been played by either the husband or the wife. The session supervisor would then find out the recorded decisions and write the response on a piece of paper and insert it in an envelope. The envelope would then be put in a tin box.

One envelope randomly chosen from a bag that contained all the possible allocations written inside different envelopes, would also be put in the tin box. The tin box would then be shaken several times and one of the envelopes would be taken out, based on which the household would receive a pay-off.

For example, at the end of all the decisions, if a white ball with number "1" is picked from the lottery basket, then the female participant's decision to allocate 2,500 NGN between men's and women's items (the specific rounds will be discussed in Sub-section 11.4.1) would be recorded on a paper which would then be put inside an envelope. The envelope would later be placed inside a tin box. Another envelope would be drawn from a bag of envelopes which contained all the possible allocations for 2,500 NGN (e.g., 0 NGN for men's items and 2,500 NGN for women's items, 50 NGN for men's items and 2,450 NGN for women's items, ..., 2,450 NGN for men's items and 50 NGN for women's items, 2,500 NGN for men's items and 0 NGN for women's items). Thus, either one of the decisions made by a participant or her spouse or a randomly chosen allocation (from all possible allocations) could be chosen as final pay-off for a household. This process offered plausible deniability to a subject for any choice she makes within the experimental session. The possibility of a random allocation to be selected as the final pay-off was explained in the beginning of the session as the "secret-keeping" choice, which, unless otherwise explicitly stated, would keep the subjects' experimental decisions secret.

C.1.2 Experimental Treatments:

Recall that conditional on the availability of subjects from the UCT impact evaluations sample, each experimental site was targeted to have two consecutive experimental sessions. One session would be randomly selected to the "Secret" condition while the other to the "No Secret" condition. A married couple would always be in the same "Secret"/"No-Secret" condition. In the "Secret" condition, none of the decisions made by a subject could be observed by his/her spouse. However, in the "No Secret" condition, any decision made after

the first round (i.e., 'round: allocation') could potentially be observed by the participant's spouse, if that round was selected for pay-off in the lottery. Thus, the actual allocation decisions could never be observed by one's spouse (or anyone else). This is due to the possibility of the "secret-keeping" choice to be chosen as the pay-off, i.e., a random choice of allocation from all possible choices to be drawn as the final pay-off (explained above under "Plausible deniability") instead of one's own decision.

For example, suppose in the "No Secret" treatment a white colored ball was chosen that represented a decision from 'round: defer' of the female participant, and suppose, the female participant had not deferred her decision. Then the couple while receiving the pay-off would be informed that she had not deferred the final decision-making (for the chosen decision to her husband. For pay-off, either the female subject's own decision or the "secret-keeping" choice will be selected as pay-off for the household, allowing for plausible deniability for the actual allocation decision made by the wife.

Experimental subjects, in all the decision rounds will be requested not to communicate their experimental decisions verbally. Rather, every decision will be made using cash prompts, envelopes and other experimental prompts.

C.2 Description of the Different Experimental Rounds

C.2.1 Individual Preference Elicitation (round 1: allocation)

Individual preference elicitation is carried out on experimental subjects in different decision spheres in the very beginning, i.e., in round 1. This means that this round, irrespective of the randomly chosen order of rounds to be carried out on any given experimental session, will not be part of the randomization of the order. The decisions in this round will involve dividing an experimental endowment between two options. For each of the following decisions, experimental subjects will use plastic laminated Colored photocopies of four 500 NGN bills, four 100 NGN bills and two 50 NGN bills (adding up to 2,500 NGN) as cash prompts. Subjects will divide the cash prompts between two envelopes, one representing allocation for the husband and the other for the wife. The different decisions are denoted by numbers 1 through 4. The order of these decisions will be randomized across sessions. The four different (preference elicitation) decisions are:

1. Men's vs. Women's items
2. Men's vs. Everyone's items
3. Women's vs. Everyone's items
4. Money Allocation - husband vs. wife

In preparatory fieldwork before carrying out the experiments, we have carried out market and consumer surveys to make sure that the "men's items" in the experiment are indeed identified as distinctly male items in the study area, while "women's items" are considered to be distinctly female items. Similarly, "everyone's items" will consist of materials that are not typically assigned to any gender and are usually used by everyone in a household.

In the first three numbered decisions above, subjects will be asked to divide an experimental endowment between binary choices of commonly used household items (e.g., between men's items vs. women's items). A 'lab shop' will be set up for the experiment and if any of the decisions numbered 1, 2 or 3 is chosen through the end-of-the-session lottery as the final pay-off for the household, participants can choose items from the 'lab shop' according to either their endowment choices they would have made earlier or a randomly chosen allocation from all possible allocations (which gives participants plausible deniability). Either way, depending on the round chosen for pay-off, participants will be given tokens with a type of good (e.g., women's items) and a Nigerian Naira amount written on it which they can show at the lab shop. Suppose, a female participant chooses to allocate 2,100 NGN for women's items and 1,000 NGN for men's items using a 2,500 NGN endowment (in Decision #1) and this decision is chosen for final pay-off through the lottery at the end of the session (i.e., a white ball numbered '1' is randomly drawn in the lottery). Assume, further, that her own decision is chosen instead of a random allocation.15 Then, the female participant will be given a token for "Men's items" with 1,000 NGN written on it and another token for "Women's items" with 2,100 NGN written on it. Since, the female round is chosen, the wife is given the tokens. The subject can herself visit the 'lab shop' with the tokens, or she can give them to her husband or they can both go together. An example script for this round is:

- "For this decision round, you are given 2,500 NGN [Enumerator: Please, give participant the cash prompts]. You will need to decide how much you want to spend for items in Picture A vs. items in Picture B. The items in Picture A and Picture B are ... [Enumerator: Please, describe the items]. As you can see, these items are also displayed in the middle of the room.

- These are just sample items. Varieties/options in fashion and color are available in the shop we have set up outside.

- You will need to make a decision NOW on how much to spend. You can decide LATER which items you want to pick.

- [Enumerators: Please use two envelopes: one envelope for Items in A which you put near picture A and the other envelope for Items in B which you put near picture B]

- Remember that this decision is private. No one else, including your spouse, will know what your exact decision is. This is because of the "secret-keeping" choice explained earlier.

- How much of this 2,500 NGN will you spend for items in A vs. items in B?"

In the last decision of this round (Decision # 4), participants will allocate 2,500 NGN between himself/herself and his/her spouse.

- – "For this decision round, you are given 2,500 NGN. [Enumerator: Please, give participant the cash prompts]. You will need to decide how much you want to keep for yourself and how much you would like to give to your spouse?

- [Enumerators: Please put two envelopes in front of the participant] Please use these two envelopes. One envelope, the one near you, is for Yourself while the other envelope is for Your Spouse.

- Remember that this decision is private. No one else, including your spouse, will know what your exact decision is. This is because of the "secret-keeping" choice explained earlier."

For the purposes of playing "efficiency games" (in a different round), we will also have the subjects replay one of the decisions from Decision # {1, 2, 3} as well as Decision # 4 (money allocation between self vs. spouse), but both with a smaller endowment of 2,100 NGN. This is explained in detail in Sub-section 11.4.6.

C.2.2 Deferral Decision (round: defer)

For each of the different decisions from the allocation round, participants will be reminded of their previous choice and that their spouse has made the same decision in another room. Participants will then be told that they can choose whether they would like to use their previous choice, or change their choice to their husband's choice. The deferral decision for the different spheres will be elicited in the same order as the original preference decisions. The script for the "Secret" Treatment will be:

- "Recall the decision that you made earlier on _____. Your spouse has also been asked to make the same decision in the other room. You can choose to either: (a) use the choice you made earlier or (b) change your choice to your spouse's choice Put the matchbox on the envelop near you to suggest that you will use the choice you made earlier. Put the matchbox on the envelop away from you to suggest that you will change your decision to your spouse's choice.

- [Enumerator, please show the experimental subject the two envelopes representing each of the two choices. Placing the matchbox on one of the envelopes, represents making a particular choice. At no point should the subject relay his/her response verbally]

- This round is represented by this white ball with a number [Enumerator, please mention number] on it that I am putting in this basket. At the end of the session today, if this ball is picked from the bag, then your decision now may determine the final pay-off. For example, if you decide to use your spouse's choice then your spouse's choice will be chosen as your household's final pay-off. Otherwise, your previous choice will be chosen as your household's final pay-off.

- However, you can also get the "Secret-keeping" choice. Thus, from this decision, your spouse will not know whether you used your own choice or his choice."

The script for the "No-Secret" Treatment will have the following line, instead of the last line in the "Secret" Treatment script:

- "If this round is selected, your spouse will be told whether you decided to use your earlier choice or your spouse's choice. However, your earlier allocation choice on how to divide the money will still remain private/secret, due to "secret-keeping" option."

C.2.3 Communication Decision (round: communication):

For each of the different decisions from the allocation round, participants will be asked to communicate their preferences to their spouse for the different allocation decisions. The script for this round will be:

- "Recall the decision that you made earlier on _____. Your spouse is making a similar decision in another room. We are going to pass on information about what you choose to your spouse before he or she makes a decision for a second time. What would you like this to be? You made the following allocation earlier, which you can pass on to your spouse: _____. Or, you can decide to send him/her a different choice. _____"

- [Enumerator, please use cash prompts and envelopes to collect information on their communication for their spouse. No verbal communication, please.]

C.2.4 Consultation and Revision (round: consult-and-revision)

Consult: For each of the different decisions from allocation round, male participants will be asked whether they would like to use their previous decision as the final decision, or or they would like to see their spouse's choice before making the final decision. The script for this round will be:

- "Recall the decision that you made about Decision # {1, 2, 3, 4}. You made the following allocation: _____. Your spouse has also been asked to make the same decision in the other room. You can choose to either: (a) still use the choice you made earlier or (b) see your spouse's choice before making the choice again."

For "Secret" Treatment, the following line is added:

- "No one will know exactly what your decision is going to be. This means that your spouse will not know if you decide to see your spouse's choice before making the choice again."

The "No-Secret" Treatment will have the following line added:

- "If this round is selected, your spouse will be told whether you decided to see her choice or not before making the final decision. However, your earlier allocation choice still remains private and secret. "

Revision: For each of the different decisions allocation round, subjects will be told their spouse's choices (irrespective of whether they wanted to see their wives' decisions or not) and will be given the option to redo each of the decisions in {2, 3, 4} or keep their original decision. Participants will also be reminded what their original decision was. The script for this round will be:

- "Recall the decision that you made about Decision # {2, 3, 4}. You will now be shown the decision you made earlier and a decision that your spouse has made. You can choose your earlier decision, your spouse's decision or something else. Please make the decision again _____."

For "Secret" Treatment, the following line is added:

- "This round is represented by this white ball with a number [Enumerator, please mention number] on it that I am putting in this basket. At the end of the session today, if this ball is picked from the bag, then your decision now may determine the final pay-off. For example, if you decide to use your spouse's choice then your spouse's choice will be chosen as your household's final pay-off. Otherwise, your previous or a new choice will be chosen as your household's final pay-off. However, you can also get the "Secret-keeping" choice. Thus, from this decision, your spouse will not know whether you used your own choice or his choice."

The "No-Secret" Treatment will have the following line added:

- "If this round is selected, your spouse will be told what your choice was. However, your earlier allocation choice still remains private and secret. "

Round: consult-and-revision is a joint round and will be jointly implemented if chosen in the final lottery.

C.2.5 Decision on Immediate Consumption in the Lab (round: lab-consumption)

During the piloting of the experiments, we were able to identify popular food and drink items in the study area which have similar market prices. For drinks, a plastic cup of Coke or Fanta, valued locally at around 150 NGN, will be made available for subjects. For food items, one of two different types of cookies will also be offered, which are of similar market prices. Two cookie pieces of the type chosen by the participant will be offered.

Subjects will be asked to choose which food and drink pair he/she wants to consume in the experimental session. It will be explained that he/she can only have it before the session ends. Moreover, it will be explained that participants will not be allowed to take the food and drink outside (this is because we want to observe choices when consumption is completely private from spouse).

Each subject is asked to guess the choice of their spouse.

Each participant selects which food and drink pair they want their spouse to consume.

Enumerators from both rooms exchange the above information, i.e., which food and drink pair participants want their spouses to consume.

Each subject could now choose to either: (a) use the choice she had made earlier or (b) change her choice to her spouse's choice. For some sessions, we lowered the cost of deferring this decision-making to spouse by offering half the amount for choice (a)

Half the participants (in the "Secret" Treatment) will be told that their decision to consume their own choice or their spouse's choice of food and drink will remain completely private from their spouse or anyone else.

The other half of the participants (in the "No-Secret" Treatment) will be told that their decision to consume their own choice or their spouse's choice of food and drink maybe disclosed to their spouse at the end of all the decisions.

Based on their decisions, subjects will be offered a food and drink pair.

If anyone refuses to drink (either their own choice or their spouse's choice) we will still record that data.

Each subject could now choose to either: (a) use the choice she had made earlier or (b) change her choice to her spouse's choice. For some sessions, we lowered the cost of deferring this decision-making to spouse by offering half the amount for choice (a).

C.2.6 Efficiency Rounds

• *Allocative Efficiency* (round: allocative-eff): We plan to measure inefficiency in the decision to divide money between household members (i.e., between the husband and the wife). To do this we will, at first, play an additional decision game in the individual preference elicitation round (i.e., allocation rounds). This decision is similar to Decision # 4 (i.e., "How would you like to divide some money between yourself and your spouse?") but instead of a (larger) 2,500 NGN endowment, this will involve a smaller endowment of 2,100 NGN. Secondly, to be able to measure allocative efficiency in this later round (round: allocative-eff), we will remind the subject of these two decisions that they made earlier, one with a smaller endowment and the other with a larger endowment and that their spouse had done the same. We will now ask subjects whether they would like to choose the allocation decision they made earlier with an endowment of 2,100 NGN or the decision their spouses made but with an endowment of 2,500 NGN. Conversely, we will ask subjects whether they would like to choose the decision they made earlier with an endowment of 2,500 NGN or the decision their spouses made but with with an endowment of 2,100 NGN. The following script will be used:

- "Remember the previous decision in which you were given 2,500 NGN [just show cash prompts; no need to give]. You decided how much to keep for yourself and your spouse. You also made a similar decision, in which you were given 2,100 NGN [just show cash prompts; no need to give]. You decided how much to keep for yourself and your spouse.
- Your spouse has also made the same decisions in the other room. You can choose to either:
  - (a) use the choice you made earlier you made with 2,500 NGN
  - OR
  - (b) change your choice to your spouse's choice which was made with 2,100 NGN
- Put the matchbox on the envelop near you to suggest that you will use the choice you made earlier with 2,500 NGN. Put the matchbox on the envelop away from you to suggest that you will change your decision to your spouse's choice made with 2,100 NGN.
- What do you choose?
  - (a) use the choice you made earlier you made with 2,500 NGN
  - OR
  - (b) change your choice to your spouse's choice which was made with 2,100 NGN"

Consumption Efficiency (round: consumption-eff): We plan to measure inefficiency in household consumption decisions. For this, we will, at first, play an additional money allocation

game for consumption items in the individual preference round (i.e., allocation rounds). This decision will be similar to Decision # 1, 2 or 3 (for example, men's vs. women's items) but with a smaller endowment, e.g., with 2,100 NGN instead of 2,500 NGN. Secondly, to be able to measure consumption efficiency in this later round (round: consumption-eff), we will ask participants whether they would like to choose the decision they made earlier with an endowment of 2,100 NGN or the decision that their spouses have made but with an endowment of 2,500 NGN. Conversely, we will ask subjects whether they would like to choose the decision they made earlier with an endowment of 2,500 NGN or the decision their spouses have made but with an endowment of 2,100 NGN. The following script will be used:

- "Remember the previous decision in which you were given 2,500 NGN [just show cash prompts; no need to give]. You decided how much you want to spend for items in Picture A vs. items in Picture B. [Show the pictures and describe some of the items to remind her]. You were also given 2,100 NGN to make the same choice, i.e., how much to spend for items in Picture A vs. items in Picture B.
- Your spouse has also made the same decisions in the other room. You can choose to either:
  - (a) use the choice you made earlier you made with 2,500 NGN OR
  - (b) change your choice to your spouse's choice which was made with 2,100 NGN
- Put the matchbox on the envelop near you to suggest that you will use the choice you made earlier with 2,500 NGN. Put the matchbox on the envelop away from you to suggest that you will change your decision to your spouse's choice made with 2,100 NGN.
- What do you choose?
  - (a) use the choice you made earlier you made with 2,500 NGN
  - OR
  - (b) change your choice to your spouse's choice which was made with 2,100 NGN"

"Secret" and "No-Secret" Treatment will be implemented as in earlier rounds.

C.2.7 Incentivized Norm elicitation (round: incentivized-guessing):

We will elicit social norms on the lab-based measures of deferral and consultation decisions - at the session level. Moreover, we will elicit social norms on gender-specific agency regarding trivial and important household decisions, using Likert scale based statements on attitude towards issues regarding female mobility and agency.

Participants will play an incentivized norm elicitation and 'guessing' round, in which they would get an additional pay-off (200 Nigerian Naira) for correctly guessing the number of times a randomly chosen participant from the same session and room had deferred or consulted with his/her spouse; as well as guessing the response to different questions on 'socially appropriate' behavior of men and women of a randomly matched person from the same room. One of all the 'guesses' from this round would be randomly chosen for pay-off for the subject.

C.3 Experimental Protocol: Pay-off to Household

At the end of all the rounds, for each household, all the numbered white balls, representing the wife's decisions and all the numbered orange balls, representing the husband's decisions will be put in one bag. One ball will be taken out to determine the pay-off for the household.

The enumerator will then find out the recorded decision (that corresponds to the selected numbered ball) and write the response on a piece of paper and insert it in an envelope. The envelope will then be put in a tin box.

There will be a bag which will contain all the possible allocations written inside different envelopes, for the chosen decision round. One of the envelopes from this bag will also be put in the tin box.

The tin box will be shaken several times and one of the envelopes will be taken out, based on which the household will receive the pay-off.

A 'lab shop' will be set up for the experiment and if any of the decisions numbered 1, 2 or 3 is chosen through the end-of-the-session lottery as the final pay-off for the household, participants can choose items from the 'lab shop'. Depending on the round chosen for pay-off, participants will be given tokens with a type of good (e.g., women's items) and a Nigerian Naira amount written on it which they can show at the lab shop.

If Decision # 4, or any decision that involves cash as pay-off, is chosen, the husband and the wife will be given the cash separately.Suppose, a female participant chooses to allocate 1,500 NGN for women's items and 1,000 NGN for men's items using a 2,500 NGN endowment (in Decision # 1) and this decision is chosen for final pay-off through the lottery at the end of the session (i.e., a white ball numbered '1' is randomly drawn in the lottery). Assume, further, that her own decision is chosen instead of a random allocation through the "secret choice". Then, the married couple will be given a token for "Men's items" with 1,000 NGN written on it and another token for "Women's items" with 2,100 NGN written on it. The couple can visit the 'lab shop' with the tokens, or either the husband or the wife can go.

An enumerator will be at the 'lab shop' and will hand out the items.

Figure C1: 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 C2: Women's Items Prompt: example of typical women's items available in the shop

Figure C3: Men's Items Prompt: example of typical men's items available in the shop

Figure C4: Household Items Prompt: example of typical household items available in the shop