Coupling Labor Supply Decisions: An Experiment in India

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Abstract

We study household decision-making about female employment in India. We randomized which spouse was given a ticket enabling enrollment in a women's weaving job, and cross-randomized the other to receive no information about the ticket, information, or information and discussion with their spouse. Consistent with a bargaining model with frictions, most experts predict information and discussion should raise enrollment. Instead, information had no effect, and discussion reduced enrollment by 50%. Negative effects are largest among couples in which the non-ticketed spouse was less supportive of female weavers, consistent with a model in which involving a spouse strengthens their veto power. ***JEL codes:*** D13, O12, J22.

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1 Introduction

Many anti-poverty policies introduce new opportunities to households with the hope that households will take them up. Immunizations, agricultural technologies, and workfare programs are just a few examples. Policymakers must choose how to introduce such opportunities, and how they do so could affect take-up decisions. Household frictions are one reason to think take-up decisions may not be neutral to how opportunities are presented. Spouses may withhold information from each other (Ashraf 2009; Ashraf et al. 2014), fail to learn from each other (Nyqvist and Jayachandran 2017; Conlon et al. 2021; Ashraf et al. 2022), lack negotiation or communication skills (Ashraf et al. 2020b; Björkman-Nyqvist et al. 2023), or face bargaining costs (Coase 1960; Riedl 1995; Anderlini and Felli 2001; Coase 2005). If policymakers wish to maximize take-up of new opportunities, such frictions have implications for which spouse they should inform and whether they should involve the other spouse in the information campaign.

We study these issues in the context of women's employment opportunities in India. Female labor force participation in India is low, and raising it is a policy priority (Fletcher et al. 2017). Several aspects of female labor supply decisions mean household frictions are likely to influence take-up of women's work opportunities. First, women's employment is not concealable; couples have to come to some sort of joint agreement if the woman is to supply labor. Second, women in India voice more support for women's employment than men do (Bernhardt et al. 2018; Field et al. 2021; Bursztyn et al. 2023). Third, marriages in India are often arranged, which means spouses in young couples may not know each other well and may face especially high bargaining costs. These factors suggest that husbands may withhold information about female job opportunities to prevent their wives from working, and that even if information were symmetric, bargaining costs could prevent the communication that would be necessary for the wife to supply labor. Which spouse policymakers inform about female job opportunities and how they involve the other spouse could therefore matter for take-up decisions.

We partnered with a firm that offers a program to train and employ women as carpet weavers in rural Uttar Pradesh. The firm, Obeetee, is one of India's largest carpet manufacturers. The program entails four months of paid training in carpet weaving followed by employment. Both training and employment take place in all-female weaving centers located in the villages where participants live. Obeetee is interested in increasing take-up of this program to help meet shortages of male weavers. There is also implicit government support for the program – the payments made to women during training count towards Obeetee's government-required corporate social responsibility efforts.

We conducted an experiment with 495 married couples in which the wife was aged 18-30 and eligible for Obeetee's program. Just 13% of wives in our sample were employed at baseline, versus 82% of husbands. We observe substantial misalignment in preferences for women's employment within households

in our sample; at baseline, over 50% of couples disagreed with each other when answering a question on the perceived appropriateness of women working as weavers. Wives are significantly more supportive of women's work than husbands, though there are couples in our sample in which preference misalignment is in the opposite direction. Reported preferences of both husbands and wives are highly predictive of take-up of the firm's program, though preferences of husbands are significantly more predictive than those of wives, which is consistent with husbands enjoying a great deal of power in the household.

We randomized how the female employment opportunity was presented to couples. We first printed enrollment tickets for a randomly-chosen 90% of couples identified in a census. Women could not enroll in the program without their unique job ticket. We used a two-by-three design among the couples with tickets, cross-randomizing (i) which spouse was given the ticket and information about the program, and (ii) which of three information sets the non-ticketed spouse was assigned to. Non-ticketed spouses in the first group ($NoInfo$) received no information about the ticket or the program. The second group ($Info$) received information but received it separately from the ticketed spouse. The final group (*Discuss*) received information at the same time as the ticketed spouse, and the two were encouraged to discuss the opportunity for three minutes at the end of the meeting. In all cases, the ticketed spouse was told what information about the opportunity would be given to their spouse. Moreover, any spouse who was given information about the ticket was told that some couples (the 10%) would be surveyed but not receive tickets. As a result, ticketed spouses in $NoInfo$ had plausible deniability – they could plausibly deny the existence of the ticket to their spouse. All couples in the sample of 495 we analyze had tickets.

Women enrolled in the program by going to their local women's weaving center on enrollment day. They were required to present their enrollment ticket and to come with their husbands. The requirement that both spouses attend ensured that enrollment, like typical female labor supply decisions, was a joint household decision. Enrollment occurred five days after tickets were given on average. Pooling all treatment groups, 15% of ticketed wives enrolled – a non-trivial enrollment rate given that only 13% of wives were employed at baseline.

Our treatments were designed to overcome two frictions in household decision-making about enrollment: information withholding and bargaining costs. We had in mind a model in which the wife can only enroll if the household bargains and decides jointly for her to do so, but two frictions may prevent bargaining. The first is information withholding; if only one spouse learns about the opportunity, the informed spouse could prevent the wife from taking it by not telling the uninformed spouse about it. Withholding will happen when the informed spouse is personally opposed to the wife enrolling and when revealing the information would lead the household to decide to enroll. Supportive spouses will not withhold information. The second friction is a bargaining cost. We assume that if spouses disagree about whether the wife should enroll, one must pay a cost to start the bargaining process. The cost could

represent the disutility from striking up an uncomfortable conversation. This "bargaining with frictions" model has two key predictions. First, $Info$ should raise enrollment relative to $NoInfo$ because it makes information symmetric. The cross-randomization of which spouse was given the ticket allows us to test an additional prediction of information withholding: the effect of $Info$ should be larger when the ticket recipient is less supportive of women weaving than the non-ticketed spouse.[1] Second, *Discuss* should increase enrollment relative to $Info$. We view *Discuss* as eliminating the bargaining cost, by giving individuals an excuse to bring up the job, for example. The discussion effect should be larger when couples disagree – in the model, bargaining is costless when both spouses want the same thing.

We asked intra-household researchers for predictions, focusing largely on individuals who had done intra-household research in developing country settings. Almost all 70 experts we surveyed agree with the bargaining frictions model's two core predictions. On average, the experts predicted that enrollment would be 5.5 percentage points higher in $Info$ than in $NoInfo$. There was near-consensus: 90% of experts predicted a positive effect. Likewise, experts predicted that enrollment would be 6.1 percentage points higher in *Discuss* than in $Info$, with near-consensus again: 86% of experts predicted a positive effect.

We report two main findings, each contrary to our priors and to those of the experts. First, we find no evidence that information withholding reduces enrollment. The effect of $Info$ is negative four percentage points off a base of 18% in $NoInfo$ and is statistically insignificant. We reject the mean expert prediction of a 5.5 percentage point increase ($p = 0.01$). The effects of information are similar whether the husband or the wife received the ticket, suggesting that neither husbands nor wives withheld information to prevent enrollment. We also do not see positive effects when we focus on the subgroup of couples where information withholding would predict the largest positive effects – those couples in which the ticket recipient was less supportive of women weaving at baseline than the non-ticketed spouse.

Consistent with these results, we find direct evidence of information diffusion. In the $NoInfo$ condition, over 70% of non-ticketed spouses reported knowledge of the job ticket on an endline survey done in the week following enrollment, suggesting that over 70% of those who received tickets shared the information with their spouses. The $Info$ treatment did increase ticket awareness by around ten percentage points ($p = 0.06$), but given the null effects on enrollment, this increase may reflect a set of couples that would not enroll even if information were symmetric. Informed spouses in these couples in $NoInfo$ may

[1] This framework has an ambiguous prediction for whether enrollment should be higher when the husband or wife gets the ticket in $NoInfo$. Two forces push in opposite directions: the fact that husbands are less supportive of the job makes them more likely to withhold information, but the fact that husbands have more bargaining power over the ultimate decision (once information is revealed) makes them less likely to withhold information. Nevertheless, experts predict enrollment to be 3.2 percentage points higher when the wife receives the job ticket in $NoInfo$. We cannot reject this prediction at conventional levels, though we also cannot reject the null of no effect.

not have shared the information because they knew the household would not enroll even if they shared it.

Our second finding is that discussion lowered enrollment – by up to ten percentage points, or 56%, relative to $NoInfo$ ($p < 0.01$) and by up to six percentage points, or 40%, relative to $Info$ ($p < 0.1$). We reject the mean expert prediction of *Discuss* relative to $NoInfo$ ($p < 0.001$) and relative to $Info$ ($p < 0.001$). Focusing on the heterogeneity in expert opinion, we can reject 99% of experts' *Discuss* versus $NoInfo$ predictions at the 5% level, and 83% of the predictions relative to $Info$. Experts are not just miscalibrated on average – they are miscalibrated with near-unanimity. Effects are still negative when we focus on a subgroup for which our ex ante model predicts the effects should be the most positive – the couples that disagreed about the appropriateness of women weaving at baseline. Further, we find negative effects of discussion in all but one of the six villages where we ran the experiment sequentially. Though the within-village effects are generally not statistically significant given the small samples, this pattern suggests that the negative effect of discussion is not just a chance result and may replicate.

We then explore mechanisms that might explain our effects, though this section of our paper is more speculative. The null effect of $Info$ could be explained in our ex ante model by the bargaining power of husbands being high enough that they do not need to withhold information, and job preferences of wives being positive enough that they do not want to. It is also possible that spouses had internalized norms of honesty or did not believe they had plausible deniability. The negative effect of discussion is harder to understand. We do not see a way to reconcile it with our ex ante model. One explanation is that couples found our discussion treatment awkward, and subsequently avoided our research team and the job we suggested. However, the discussion treatment did not affect whether individuals took our endline survey, nor did it affect how desirable they perceived the job to be at endline. A second possibility is that the *Discuss* treatment reduced enrollment by causing arguments, but individuals in the *Discuss* treatment group did not report more disagreement with their spouse about the opportunity in the endline survey than those in $NoInfo$ or $Info$. A third possibility is that surveyors' presence during the three-minute discussions reminded couples of the norm that women not work outside of their homes. However, effects do not vary by how appropriate husbands or wives reported women working as weavers to be at baseline. Fourth, though *Discuss* couples talked about the job opportunity more often, it does not seem that deeper deliberation per se reduced enrollment. In particular, for couples not in *Discuss*, having more days between ticket receipt and enrollment – and thus having more time to make the enrollment decision – is not associated with lower enrollment. Section 6.4.2 discusses four additional explanations for which we do not find strong evidence.

We find suggestive evidence for an alternative explanation that rests on veto power. Suppose couples enroll if neither spouse vetoes enrollment, but spouses incur a private utility cost if they exercise a veto.

This cost captures internal or external pressure to be agreeable towards one's spouse,[2] and could make it preferable to accompany one's spouse to enroll, rather than to refuse, even if one is personally opposed to enrolling. Our treatments could have varied these costs by determining who felt they had a right to make the decision. Informing only one spouse, as in $NoInfo$, made that spouse feel entitled to make the decision. Interventions that nudge households towards joint decision-making, like *Discuss*, and to a lesser extent, $Info$, made a second spouse feel entitled to exercise a veto. This model can rationalize the lack of information withholding (the ticket-holder in *NoInfo* always feels comfortable vetoing so has no incentive to withhold) and the negative effect of discussion (enrollment is less likely when two people might veto rather than only one). Further, it implies a specific pattern of heterogeneity that is the near opposite of what our ex ante model predicts: interventions toward joint decision-making should only reduce enrollment among the couples in which the non-ticketed spouse is less supportive of women weaving than the ticketed one. The data confirm this prediction. Additionally, we find that among couples who disagreed in their support for women weaving, assigning the ticket to the more supportive spouse increased enrollment in $NoInfo$ but had no effect in $Info$ or *Discuss* – just what we would expect if veto power is unequal in $NoInfo$, but closer to equal in $Info$ and *Discuss*.

We note, however, that our evidence for the veto power model is only suggestive. Our experiment was not designed to test for veto decision-making directly, and the heterogeneity results provide only indirect evidence. We also note that while *Info* and *Discuss* made it more likely that individuals reported both spouses had equal influence over the enrollment decision, as the veto theory would predict, these effects are not statistically significant. Future research is needed to more carefully test whether couples make decisions through vetoes, along with possible alternative explanations.

Our results nevertheless have important implications for policy and research. The most immediate take-away is that nudging households towards joint decision-making through a treatment like *Discuss* might decrease the take-up of opportunities. This is an important result as nudging households to discuss an opportunity might seem a natural implication of bargaining costs (Coase 1960; Riedl 1995; Anderlini and Felli 2001; Coase 2005) and of non-experimental evidence that suggests spousal communication about family planning increases take-up (Bawah 2002; Shattuck et al. 2011; Hartmann et al. 2012). At a broader level, our results suggest that much remains to be learned about intervention into the household. Our findings are not what experts (or we) predicted. Further, experts were quite uncertain of how our results would play out, with the average expert predicting just 39% of their predicted enrollment rates would be within five percentage points of the truth, and even this low confidence proved to be overconfidence. A deeper understanding of household decision-making is important for policy. For instance, our ex ante bargaining with frictions model and the veto model we propose have entirely different im-

[2]This cost could vary by gender, and given gender norms, it would likely be higher for women than men.

plications for intervention. With bargaining frictions, intervention towards joint decision-making could promote decisions that move away from the status quo, in our case increasing female labor supply in India. If decision-making is instead determined by vetoes, intervention towards joint decision-making could have the opposite effect, giving both spouses veto power rather than just one, leading to status quo bias.

Our key contribution is to provide new evidence on the effects of intervention into household decision-making. While many papers document inefficient household decision-making in poor countries (Udry 1996; Duflo and Udry 2004; Ashraf 2009; De Mel et al. 2009; Robinson 2012; Ashraf et al. 2014; Schaner 2015; Hoff et al. 2017; Schaner 2017; Afzal et al. 2018; Ashraf et al. 2020a; Conlon et al. 2021; Buchmann et al. 2023), less is known about intervention into the household decision process. In a seminal lab-in-the-field experiment, Ashraf (2009) used treatments similar to ours to study the role of spousal observability and communication for saving and consumption decisions. In field experiments, Ashraf et al. (2014) study the effects of individual versus joint decision-making about contraceptive use, Ashraf et al. (2020a) and Nyqvist and Jayachandran (2017) compare the effects of giving husbands versus wives information about maternal and child health, and Dean and Jayachandran (2019) evaluate guided household conversations about female labor supply. Ashraf et al. (2020b) and Björkman-Nyqvist et al. (2023) evalute more intensive interventions that aimed to build girls' and wives' negotiation and communication skills. Our study is most similar to Ashraf et al. (2014), Ashraf et al. (2020a), Nyqvist and Jayachandran (2017), and Dean and Jayachandran (2019) in that we study light-touch interventions in the field. Our information treatment is similar to Ashraf et al. (2014) but implemented in the context of labor supply rather than contraception. These decisions are conceptually distinct; it is possible for women to conceal from their husbands whether or not they take up contraception, whereas the decision to work outside the home cannot be concealed from either spouse but one spouse could prevent the woman from working by not passing on information about female job opportunities. The conversations studied by Dean and Jayachandran (2019) are similar to our *Discuss* treatment, but a key distinction is that they study guided conversations whereas our *Discuss* treatment was more narrowly focused on overcoming bargaining costs by simply encouraging discussion.

Our paper also contributes to work on the determinants of female labor force participation in developing countries (see Heath and Jayachandran (2018) for a summary). We build especially on work that explores household constraints to women's labor supply (Bursztyn et al. 2020; Dean and Jayachandran 2019; Heath and Tan 2020; Field et al. 2021; McKelway 2023a,b), showing that efforts to encourage joint decision-making can lower labor supply. Finally, our work introduces expert prediction to work on household intervention. In doing so, we build on the growing effort to predict results in economics (DellaVigna and Pope 2018; DellaVigna et al. 2019).

2 Background on Female Labor Supply in Uttar Pradesh

2.1 Gender Norms in Uttar Pradesh

Our study takes place in rural villages in eastern Uttar Pradesh, India. Uttar Pradesh is one of India's poorest states. Reflecting this, the median husband in our experimental sample earns only INR 4,500 ($68) per month. Our setting also features strong adherence to traditional gender norms. Purdah is an important feature of local culture – women veil their faces and stay out of sight of men outside of the family. In a pilot survey of 50 women, 82% said that they practiced purdah, and 86% said that the practice is important. Arranged marriage and patrilocality are the norm: at the time of marriage, women generally leave their natal villages and become a part of a family they do not know well in a new village. Married women face constraints on their physical mobility and are responsible for many household chores, including child-rearing, cooking, tending to livestock, and household cultivation. Whereas 82% of husbands in our experimental sample had done activities to earn income in the previous three months, only 13% of their wives had done so.[3] This norm is strongly ingrained in cultural mindsets, with 87% of men and 84% of women believing that husbands should earn more income than their wives.

2.2 Partner Firm and Women's Job Opportunity

We partnered with Obeetee, one of India's largest carpet manufacturing and exporting firms. Carpet weaving has been an important industry in the region since the time of British rule, when the British set up the industry to take advantage of low labor costs. Weaving is generally considered a low-caste occupation and, as with most formal-sector employees in this setting, weavers are predominantly male.[4]

Obeetee imports wool, usually from Rajasthan, India, to its factory in eastern Uttar Pradesh, where it is converted to yarn. The yarn is then supplied to hundreds of loom owners located in villages in the region. These loom owners operate small loom centers in their villages and employ local men to weave carpets by hand. Obeetee purchases completed carpets from the loom owners, packages them, and ships them to buyers globally.

In recent years, Obeetee has begun a program to train and employ women as weavers. They had several motivations for doing so. A larger pool of labor allows Obeetee to more easily take advantage

[3] Low levels of women's employment are seen across India, not just in Uttar Pradesh. The country's female labor force participation rate is among the lowest in the world, and low even among countries with similar per capita income (21% among females aged 15+ in 2016, according to the World Bank's World Development Indicators).

[4] For example, 76% of Uttar Pradesh textile workers in the 2011 Indian Human Development Survey are male, and 92% are Other Backwards Castes (OBC) or Scheduled Castes (SC). Lower castes are over-represented given that only 72.4% of all Uttar Pradesh respondents are OBCs or SCs.

of periods of high export demand for carpets, while also helping to offset local labor shortages driven by increasing rural-urban migration of male weavers. Further, payments made to women during training count towards government-required corporate social responsibility.

Each center recruits women living in the center's village. Status quo recruitment tended to involve the firm advertising the job opportunity to existing male weavers, and asking them to spread the information to interested women in their households. In contrast, we experimented with alternative recruitment styles that ensure the diffusion of information to eligible women.

As women usually have no prior experience in weaving, the job begins with a four-month, well-paid training period.[5] By many objective measures, the job is desirable. It is near potential employees' homes, involves safe and comfortable work, requires reasonable hours, and demands no prior training. To respect gender norms, many steps are taken to ensure that women would interact only with other women while at work – only female weavers work in the women's centers and the centers are owned by females, though in practice the husband of the owner is involved with managing the center and the trainer is often male. Qualitative evidence suggests that participants also see the job as desirable: at baseline, 88% of our experimental sample say that workers in this job would be completely safe, and only 11% say that the job is low status.[6] Nevertheless, enrollment and retention rates are low. In our experiment, around 15% of women enrolled, and about half of those who enrolled left the program within the first month of training.

2.3 Preferences Regarding Women's Employment

We use data from our baseline survey ($N = 495$ couples) to describe systematic differences in preferences towards women's employment within the household. Before mentioning the actual weaving position, we asked both husbands and wives, separately, how appropriate they thought it would be for men or women in their household to hold a full-time job outside of the house in three different occupations: construction, weaving, and teaching. Construction and teaching were chosen to reflect the least and most "women-appropriate" occupations in this area,[7] while weaving was chosen to match the actual job opportunity we offer in the experiment. We record answers on a zero to two scale: inappropriate, somewhat appropriate, or completely appropriate. We use this data to establish three core facts that motivate our experiment.

First, wives are more supportive of women working as weavers than husbands. Specifically, while

[5] Even during the training period, the daily wage offered to women is only slightly below that which a skilled male weaver would earn. Initially, the women were to be paid monthly in cash. However, India's demonetization occurred in the middle of our study, resulting in a rushed transition toward paying directly into bank accounts.

[6] These questions were only asked to those in the treatment groups that were provided details about the job opportunity at baseline.

[7] Among Uttar Pradesh-based respondents to the 2011 Indian Human Development Survey, 91% of construction workers and 53% of teachers are male.

there are no gender differences in the perceived appropriateness of men working in each of the three occupations (columns 1 to 3, Table 1), there is a systematic divergence in the perceived appropriateness of women working (columns 4 to 6). Wives think it would be significantly more appropriate for women in their homes to work in all three jobs than their husbands do, with an effect of 38% of the husband mean for weaving (column 5).[8] Wives also show more interest in the specific job opportunity we advertised than their husbands (column 7). The level of interest is high, with 57% of women reporting being very interested and 20% being somewhat interested. Reflecting the gender gap in preferences, non-enrollment is often explained ex-post as due to opposition from husbands and their family members (Figure A1).[9] Our staff partners at Obeetee also frequently mentioned opposition from husbands as a key constraint to enrollment.

Second, couples often disagree with each other about whether women should weave. 58% of couples give different answers to each other when asked about the appropriateness of women weaving (panel (a), Figure 1). Consistent with the first result, this disagreement takes the form of women being more supportive most (67%) of the time, though this still leaves a sizeable percent of the disagreement (33%) taking the opposite form.

Third, the preferences of both husbands and wives are highly predictive of enrollment, but husbands' preferences are more predictive. Job enrollment is higher when husbands and wives deem women weaving to be appropriate (panel (b), Figure 1), with enrollment at 37% when both spouses consider women weaving to be "completely appropriate," and 1% when both answer "inappropriate." While the preferences of both spouses matter, those of the husband matter more. For example, enrollment is over twice as high when the husband answers "completely appropriate" and the wife answers "somewhat appropriate" than the opposite case. More generally, husbands' preferences are statistically significantly more predictive of enrollment than wives' preferences in specifications with and without controls (Table 2). These results are consistent with husbands having high bargaining power.

Summarizing, (i) wives are more supportive of women weaving than husbands, (ii) spouses often disagree with each other about the appropriateness of women weaving, and (iii) both husbands' and wives' preferences are predictive of enrollment but husbands' preferences are more predictive. These facts motivate our experiment design.

[8]The divergence is smaller for teaching, at 6% of the husband mean, consistent with it being a less male-dominated profession. The husband mean for each type of job held by a woman is notably lower than when men would hold the job, making the perceived appropriateness of women working on average lower than that for men.

[9]The most common reason provided for not enrolling is that there is no-one else to do household chores. It is likely that this reason also reflects the bargaining position of the husband and the husband's lack of support for the job. In particular, had the husband been willing to take on more household chores or ask his family to do so, the wife may have been able to work.

3 Experiment Design

3.1 Timeline

We conducted the experiment in conjunction with the opening of six new weaving centers, each of which had slots for 20 women weavers. The firm gave permission for us to run all recruitment activities for these centers. Recruitment and center openings occurred sequentially from September 2016 to January 2017 (see the timeline in Figure 2).

Census and Randomization. For each center, we first conducted a census of the catchment area. The catchment area was defined by the loom owner as the area from which the firm would have recruited women in the absence of the study. These areas typically consisted of the entire village surrounding the loom, but excluded high-caste hamlets.[10] Surveyors visited each home in the catchment area and surveyed the household head, asking him or her to list all adults in the household along with their gender, age, marital status, caste, and contact information. A catchment area's census typically took four to seven days. Using the census data, we identified all women in the firm's eligible age range (18 to 30) along with a "pair" for the woman. If the woman was married, the pair was her husband. If not, the pair was her household head. We dropped pairs whenever either the woman or her pair were not available for surveys in the next month. While we included eligible unmarried women in recruitment, our analysis focuses on decision-making in married couples and excludes unmarried pairs. We then randomly assigned treatment for the catchment area at the couple-level, stratifying by hamlet and an indicator for Other Backwards Castes (OBC).[11]

Baseline and Intervention. Following the randomization, we implemented a baseline survey containing our experimental intervention. This period lasted for six to eight days for each center and occurred one to six weeks after the census ended. The baseline involved individual surveys of all eligible women, and separate individual surveys of their pairs. These surveys began with a female surveyor meeting the participant at their home. Before beginning the survey, the surveyor and the participant moved to a private place where they could not be overheard. Each surveyor was randomly assigned a group of participants in a randomly ordered list, subject to the constraint that the two members of each pair were assigned to different surveyors. This constraint served to avoid the possibility of surveyors undoing "plausible

[10] High castes tend to not see weaving as a job appropriate for their class and are particularly opposed to women working outside of the home.

[11] The omitted category includes Scheduled Castes (SC), Scheduled Tribes (ST), and "Don't Know". In our experimental sample of 495 couples 54% are OBCs, 44% are SCs, 1% are STs, and 1% don't know.

deniability" by revealing that a spouse had received a job ticket. Both when setting appointments for the surveys and just before the surveys began, surveyors confirmed that both women and pairs were likely to be available to be surveyed, and only proceeded with the surveys if this was the case.

The baseline survey itself had two parts. The first part was a questionnaire that asked about demographics, employment, and attitudes towards women's employment, as already discussed in Section 2.3. The second part of the survey was our experimental intervention. The intervention varied according to whether, and if so how, the job opportunity was presented to the respondent. We describe each treatment in detail in Section 3.2 below.

Enrollment Day. On the day after the baseline survey ended, we hosted an enrollment day. Any woman wishing to enroll in the job was required to come with her pair to the weaving center between 7am and 7pm on that day. Those that enrolled were also required to present unique enrollment tickets given to one member of the pair during the baseline survey. The requirement for women to attend with their pairs is important, for both practical and conceptual reasons. Practically, it eliminated scenarios in which a woman would enroll without her husband's permission, thereby reducing dropout and subsequent intra-household discord. Conceptually, it means that we can interpret enrollment as a decision made jointly by the household.

Endline. Finally, we conducted an endline survey in the three to five days following enrollment. The purpose of this survey was to help us understand how enrollment decisions had been made. To this end, the survey included a quiz about job information to assess participants' knowledge of the job along with questions about the pair's decision-making process. 91% of respondents completed the survey by phone, while the remaining 9% completed the survey in person after we were unable to reach them by phone. For the final three centers we added several questions to the endline survey and amended our AEA pre-registration to reflect this change. As a result, we are missing data on the added questions for 45% of our endline respondents.

3.2 Treatments

Plausible Deniability. We began by printing enrollment tickets. Each ticket had the names of a particular pair written on it along with a unique identification number (Figure A2). No woman could enroll without her unique ticket. Crucially, we only printed tickets for a randomly chosen 90% of eligible women. The remaining 10% of pairs still completed baseline surveys, but following the survey, a randomly chosen member of the pair was told that the eligible woman in the pair had not received a ticket. Any participants

in the 90% assigned to receive information about the ticket (via randomizations detailed below) were also told that some participants would be surveyed and not receive tickets. It was therefore common knowledge that we had not printed tickets for all eligible women and that being surveyed did not signal that a ticket had been printed. This system ensured plausible deniability: whenever only one spouse was informed about and given the ticket, he or she could plausibly deny having received the ticket. Non-ticketed pairs exist only for this purpose and our analysis focuses on pairs that actually received tickets.

Assigning a Ticketed Spouse. The delivery of the ticket and job information to married couples varied according to two, cross-randomized treatments (Figure 3).[12] The first treatment determined which spouse would receive the ticket: 50% of the time the ticket was given to the wife, and 50% of the time to the husband. Anyone who received the ticket received full information about the job details, the enrollment process, and what their pair would be told by a surveyor.

Information Given to the Non-Ticketed Spouse. The second, cross-randomized treatment determined what information the non-ticketed spouse was told by a surveyor about the ticket and job opportunity.

No Information. In one third of couples, the non-ticketed spouse was told nothing about the job during the baseline survey. Importantly, ticketed spouses in $NoInfo$ could withhold information about job eligibility if they desired; a surveyor would never tell their non-ticketed spouses that a ticket had been given and the ticketed spouse could plausibly deny having received a ticket because 10% of women did not have tickets printed. Because tickets were required to enroll, such withholding was a means by which one spouse could prevent enrollment.

Information. In another third of couples, the non-ticketed spouse was told that their pair had or would receive an enrollment ticket along with details of the job and enrollment process. A priori, we expected any effect of this group would be driven by knowledge of the ticket's existence rather than knowledge of the job and enrollment details as ticket information is specific to an individual couple, the unit at which treatment was assigned, whereas job and enrollment details could spread across households in a village. However, we used an additional randomization to allow for the possibility that providing job and enrollment details would have a treatment effect beyond the effect of providing information about the ticket alone. In particular, we split the $Info$ treatment in two: in 50% of $Info$ couples (one-sixth of the

[12] We followed a simpler procedure for unmarried couples as they were to be excluded from the analysis. In a randomly chosen 50% of pairs, the eligible woman was notified of whether a ticket had been printed for her. If so, she also received the ticket and job details. In the other 50%, the household head received this information. The non-ticketed member of the pair received a baseline survey but no further information. This corresponds to the *NoInfo* row in Figure 3.

full sample) the non-ticketed spouse was told only that their spouse had or would receive an enrollment ticket ($TicketInfo$), while the rest of non-ticketed spouses in $Info$ couples received this information plus job and enrollment details ($FullInfo$). Non-ticketed spouses in $FullInfo$ have greater endline knowledge of job details than those in $TicketInfo$ and view the job as more desirable at endline, but the effect on enrollment is not statistically significant and points in the opposite direction as the effect on job desirability (Table A2).[13] Given this, to increase the power of our tests for information withholding, we combine these two subgroups into a single $Info$ group for the analysis.[14] In contrast to $NoInfo$ couples, both spouses in $Info$ couples knew that the wife could enroll in the job and this information could not be withheld. That said, spouses in $Info$ were told about the opportunity separately and the intervention did not directly affect how they interacted with one another.

Discussion. The final third of couples were assigned to the *Discuss* group. Here, the non-ticketed spouse was present while the job details were given. Surveyors then paused the survey for a full three minutes and encouraged the couple to discuss the opportunity together. Two surveyors were present during the discussion but remained silent and did not provide any additional information about the job. After the three minutes were up, the surveyor handed the enrollment ticket to the ticketed spouse. According to surveyor evaluations, 80% of *Discuss* couples discussed the opportunity during this time, with the other couples remaining silent or discussing something else. The goal of the discussion treatment was to alleviate possible costs to bargaining. While couples in both $Info$ and *Discuss* had full information about the opportunity, only the *Discuss* treatment directly affected how spouses interacted with one another about the opportunity.[15]

[13] With 84 couples in $TicketInfo$ and 87 couples in $FullInfo$, a limitation here is that we lack power to detect small effects on enrollment.

[14] For completeness, we also report the expert predictions and core enrollment results without any pooling in the Appendix.

[15] A key logistical difference between this treatment and the others is that husbands and wives had to be together when the job information was given, but separate when taking the baseline questionnaire. Initially, we scheduled husband and wife surveys simultaneously, separated the spouses to individually take the baseline questionnaire, and then brought them back together for the job information and discussion. However, this procedure could have introduced selective attrition as (a) the ease of participating differed across treatment groups, and (b) participants could have inferred their treatment status in advance based on how their appointments were set. To address this, we modified the procedure when we were roughly 25% of the way through the sample, at which point the experiment was ongoing in the third center's village. Under the new procedure, spouses were contacted individually to complete the baseline questionnaire in the same manner in which individuals in all other treatment groups were contacted. After the questionnaire was complete, the participant was told that surveyors had limited time to complete all surveys and therefore wanted to complete the second half of the survey with the participant's spouse present once the spouse had taken the individual questionnaire. All couples that completed individual baseline questionnaires were able and willing to schedule a second joint appointment. Encouragingly, we do not see any selective attrition (Section 5.2). One remaining concern is that the postponing of job information meant that, on average, *Discuss* couples received job information closer to the enrollment date. We show in Section 6.4.2 (and Table A12) that enrollment is not related to proximity to enrollment day.

3.3 Conceptual Framework

Our experiment was motivated by a simple model of household bargaining with frictions. We formalize the model in Appendix B and describe it in words here. In the model, a husband and wife decide whether the wife should take the job opportunity. She will only take the opportunity if the two bargain and decide jointly for her to do so. Both the husband and the wife have perfect knowledge about the net utility gain to each spouse from the wife enrolling. These utility gains can include utility from the extra income (which may or may not be shared), shifting bargaining power, stigma costs, and any disutility of the wife's effort.

We assume two possible frictions to efficient bargaining: incomplete information and bargaining costs. Incomplete information constitutes a friction whenever only one spouse is aware of the job opportunity. In this case, the knowledgeable spouse will withhold information when two conditions are met: (i) enrollment is costly to them, and (ii) enrollment would happen if the information was revealed. Given these conditions, the spouse that is more supportive of the job will never withhold information. Bargaining costs are at play even if knowledge is symmetric. Here we assume that there is a cost of bargaining whenever the two spouses disagree. If neither spouse pays the bargaining cost (which we can think of as starting an uncomfortable discussion about the job), we assume that the couple reverts to non-cooperative decision-making, preventing enrollment.

Our experimental treatments shut down each bargaining friction. The information treatment ensures that both spouses are informed about the job, shutting down the incomplete information friction. The discussion treatment encouraged spouses to enter into a discussion about the job, which we think of as reducing the bargaining cost, by providing an excuse for bringing up the topic, for instance. As a result, the model makes two unambiguous predictions: First, enforcing common knowledge in *Info* increases enrollment, by preventing the strategic withholding of information. This effect should be driven by couples in which the ticket recipient is less supportive of women weaving than the non-ticketed spouse. Second, encouraging bargaining in *Discuss* increases enrollment, by enabling joint decision-making. Importantly, in the model the positive effects of discussion are driven entirely by the couples that disagree about the job – for the couples that agree, there is no bargaining cost, and so no friction preventing bargaining. We test for this predicted heterogeneity by disagreement below.

Otherwise, the model is ambiguous on the sign of the interaction effect between $Info$ and the gender of the ticketed spouse. And while we have random variation in which spouse receives the ticket throughout, for the *Discuss* treatment we do not explicitly model a role for the ticket recipient. We nevertheless discuss the full set of treatment effects in Section 6 below, after first comparing the predictions of intra-household experts with those of the conceptual framework.

4 Expert Survey

Following DellaVigna et al. (2019), we elicited expert predictions to allow us to characterize how our results should update the beliefs of intra-household researchers. We drew up a list of academic experts that included (i) authors of papers on intra-household economics[16] published in the last 10 years in any of the top-5 economics journals, *American Economic Journal: Applied Economics,* or *Journal of Development Economics*; (ii) presenters of intra-household research at NEUDC in the last 10 years;[17] (iii) authors of intra-household papers we cited in our October 2019 draft; and (iv) authors of intra-household papers in the syllabi for the PhD development economics courses at MIT and Stanford. From this list, we dropped (i) inactive researchers, including the retired, emeritus faculty, and those who had shifted to industry;[18] (ii) those without publicly available email addresses; and (iii) two academics we were certain already knew the results. We emailed the expert survey to a final list of 361 researchers in May 2021.

Since the first draft of this paper circulated in May 2017, our expert survey screened out any researchers that had seen and could remember at least some of the results. In particular, we asked experts *"Have you seen or heard results from an experiment on household decision-making about female labor supply in India conducted by Matt Lowe (UBC) and Madeline McKelway (Stanford)?"* We asked this question twice – once before describing the context and details of the experiment, and once afterwards. We only asked experts for their predictions if they answered *"No"*, *"Don't Know"*, or *"Yes, but I do not remember any of the results at all"* each time.

90 experts (25% of those contacted) completed the survey, while 70 experts (19% of those contacted) got past the two screening questions and actually gave predictions. The 70 experts for which we have predictions skew somewhat more junior than the full sample of 361 experts – for example, while 34% of the experts that gave predictions are Assistant Professors, only 20% of the experts contacted are (Table A3). Reflecting this pattern, our predictors also have fewer Google Scholar citations. That said, our sample of predictors is not "inexperienced" overall – 23% are Full Professors, and they have 3,507 Google Scholar citations on average.

When eliciting predictions, we revealed the enrollment rate for one treatment cell, writing *"In the first treatment cell, i.e., when the husband got the ticket and the wife was given no information, 19% of women enrolled.* ***What percentage of women in the other seven treatment cells do you think enrolled?"*** The

[16] Papers with (a) "intra-household," "intrahousehold," "within the household," or something similar in the title or abstract, or (b) at least one "O" category JEL code and at least one of D13, J12, and J16.

[17] Some of the older NEUDC websites in the 10-year window no longer exist, so we did not include experts from those conferences.

[18] We kept researchers who had shifted to the private sector very recently.

experts made seven predictions, rather than five (recall Figure 2), since we elicited predictions separately for the two sub-treatments of the *Info* treatment group. Since we pool these two sub-treatments in the analysis, we also pool the expert predictions, by defining an expert's prediction for the pooled *Info* treatment group as the average of their predictions for the two sub-treatments.[19] We did not incentivize the predictions given that experts might have been able to find the results from a previously circulated draft.[20]

Experts agree with the two main predictions of the bargaining frictions model (Figure 4). They expect information withholding, predicting that enrollment will be 5.5 percentage points higher in *Info* than in *NoInfo*. These predictions reflect near-consensus: 90% of experts predict that *Info* will increase enrollment relative to *NoInfo*. Second, experts predict that *Discuss* will have 6.1 percentage points higher enrollment than *Info*. Experts are again nearly all in agreement: 86% expect *Discuss* to have higher enrollment than *Info*.

While the bargaining frictions model is ambiguous on whether husbands or wives should withhold information more often, experts expect husbands to be more secretive. In particular, while experts predict the *Info* treatment to increase enrollment regardless of which spouse received the ticket, they expect *Info* to increase enrollment by 2.5 percentage points more when the husband receives the job ticket. The patterns are similar if we include only full professors, or if we exclude the seven experts that had seen the paper but couldn't remember the results (Figure A4).

Following DellaVigna and Pope (2018), as a proxy for an expert's confidence we also asked: *"How many of your seven predictions do you expect are 5 percentage points or less from the actual enrollment rate in each treatment group?"* We discuss expert confidence in the conclusion.

5 Empirical Specification and Sample

5.1 Empirical Specification

We estimate variants of the following empirical specification:

$$y_i = \beta \mathbf{Treat}_i + \gamma \mathbf{X}_i + \alpha \mathbf{S}_i + \varepsilon_i$$

[19] Nevertheless, we report the unpooled expert predictions in Figure A3.

[20] Even without incentivizing, experts might try to find the results. To avoid this, we removed the draft of the paper from each of our websites while the survey was live. The inaccuracy of the expert predictions suggests those who made predictions were not aware of the results.

where i denotes a married couple, and $\mathbf{Treat}_i$ is a vector of indicators denoting treatment assignment that varies depending on the hypothesis being tested. The vector $\mathbf{X}_i$ includes the 12 baseline survey variables used below for balance checks.[21] We report core estimates with and without $\mathbf{X}_i$. $\mathbf{S}_i$ are controls for strata. The randomization was stratified by village, hamlet, and OBC status. However, due to attrition, some strata do not have all six treatment groups represented. Additionally, we investigate how effects differ across subgroups when exploring mechanisms, and some strata within the various subgroups do not have members from each treatment cell. To make use of all observations in identifying treatment effects, we do not control for strata fixed effects, but rather control for village fixed effects and for OBC status.[22] The probability of treatment assignment did not vary by strata, so exactly how we control for strata does not meaningfully affect our results. Our main outcome, y_i, is an indicator for the couple enrolling on enrollment day, though we consider additional outcomes for supplementary and mechanisms analyses, and describe these outcomes as we present effects on them. We estimate robust standard errors throughout.

5.2 Sample Size and Characteristics

We identified 817 married women that were eligible for the job from the census survey. We printed tickets for 732 (90%) of them. The 817 women and their husbands formed the sample of married couples to be approached for the baseline survey and intervention, but we focus now only on couples with tickets.

Attrition. We consider couples to have attrited from the study if either spouse could not take the baseline survey. The requirement that both spouses be surveyed raised attrition rates but was needed because our treatments would be difficult to interpret if only one spouse was approached in some couples. Overall, 32% of the 732 couples attrited from the study. While 32% may seem high, we estimate that, at most, around one third of couples in this 32% could have known their treatment status.[23] This implies that treatment status was necessarily orthogonal to attrition for the majority of the 32%, and that the rate of attrition amongst couples that could have known their treatment status was at most 14%. Consistent with this, attrition does not differ significantly by treatment group (column 1, Table A4), and as discussed below, we observe balance on baseline variables among the couples that did not attrit. The vast majority (75%) of attrition was due to one or both members of the couple being out of town, away all day for

[21] When controlling for baseline variables, we set any missing values in the variables to the variable average to avoid dropping observations from the regressions.

[22] For a similar reason, we do not include dummy variables for missingness of each baseline control variable.

[23] Couples in the 32% could have known their treatment status if (a) one spouse took the baseline survey but the other did not, or (b) the couple was assigned to the discussion treatment and could have been exposed to the initial procedure for delivering that treatment (see footnote 15).

work, or otherwise unavailable during the six- to eight-day baseline period (panel (a), Figure A5). The two other leading causes for attrition were surveyors running out of time to complete baseline surveys for one or both members of the couple (11%),[24] or one or both members of the couple not consenting to take the survey (9%). This leaves us with a sample of 495 married couples for analysis. Figure 3 provides the number of couples in the sample of 495 assigned to each treatment group.

Baseline Balance. Restricting to the analysis sample of 495 couples, the sample looks well-balanced on baseline variables. We regress each of 12 baseline variables on indicators for $Info$, *Discuss*, and Wife Gets Ticket, along with strata controls (columns 2 to 13, Table A4). Four of the 36 coefficients, and two of 12 tests of joint significance of the three treatment indicators, are statistically significant at the 10% level, which is close to the amount of imbalance we would expect from random chance alone. Nevertheless, some imbalances look potentially important. We see imbalances in whether women had worked for income in the last three months, but importantly, this variable does not predict our main outcome: enrollment (Table A5). While we see husbands in the *Discuss* treatment group are more supportive of female weavers, we note that any resulting bias would be towards a positive effect of *Discuss* on enrollment, while we find a negative effect. We anyway estimate the main effects with and without the set of 12 controls, and our core conclusions are robust to either approach.

Endline Attrition and Balance. Of the 990 married adults in the sample, 830 (84%) completed the endline survey. 87% of endline attrition was due to the adult being unreachable over the phone or in person (panel (b), Figure A5). 10% of attrition was due to an adult not consenting to participate in the endline survey, and 4% was due to the participant not having a phone.[25] Attrition from baseline to endline is largely balanced across treatment groups (columns 1 and 2, Table A6), and is not selective: the sample that completed the endline look similarly balanced to the larger sample that completed the baseline, with the same handful of chance imbalances (columns 3 to 14, Table A6).

[24] Enrollment dates were set in advance and therefore imposed a hard deadline on when baseline surveys had to be completed. This meant that we could not guarantee canvassing of all those eligible within the time allotted for a center's recruitment activities. To help maximize the size of our experimental sample, we prioritized married couples in the random survey order so that if surveyors ran out of time it was unmarried participants that were excluded. 96% of couples excluded from the study because surveyors ran out of time came from the first center's village, before we had precise estimates of how long baseline surveys would require and before we had implemented a system of appointment setting.

[25] This was only a reason for attrition for the first two centers as starting from the third we conducted endline surveys in person for those that did not have phones.

6 Results

We find virtually the opposite of what the experts and the bargaining frictions theory predict. Figure 5 summarizes our results, visualizing enrollment rates for each of the six treatment cells, and also testing for differences relative to the status quo group (the $NoInfo$, Husband Gets Ticket group) and relative to the expert predictions (the same analyses for the full eight treatment cells are in Figure A6). We unpack the treatment effects in the subsections that follow, but two features of Figure 5 are worth highlighting now. First, our treatments did not raise enrollment, in fact, some lowered it. Second, we can reject four of five expert-predicted treatment effects at at least the 10% level.

6.1 Information Withholding

We first test whether spouses strategically withhold information to prevent enrollment. Including both couples where the wife was ticketed and those where the husband was, there is no evidence that providing information to the non-ticketed spouse increased enrollment (columns 1 and 2, Table 3). The point estimates suggest that the $Info$ treatment, if anything, reduced enrollment by three or four percentage points. We can reject the mean expert prediction of a 5.5 percentage point increase with 95% confidence, and we can reject a majority of the individual-level expert predictions at the 5% significance level. Contrary to the theory and the experts, there is no evidence that preventing information withholding increases enrollment, suggesting that there is no strategic withholding to begin with.

The lack of withholding might of course mask heterogeneity by gender – husbands may be more likely to withhold information than wives, as the experts predicted. However, the effects of $Info$ are similar whether we consider couples where the husband got the ticket or those where the wife did (columns 3 and 4, Table 3). In the case of ticketed husbands, informing the wife reduces enrollment insignificantly by four percentage points, and in the case of ticketed wives, informing the husband reduces enrollment insignificantly by three to four percentage points. We cannot reject that these two effects are equal ($p = 0.98$ with controls),[26] and each of these effects is again quite different to the mean expert prediction ($p = 0.05$ and $p = 0.12$ with controls). Neither wives nor husbands appear to be withholding information.

More surprising, there is no evidence for strategic withholding even among those the theory emphasizes. The point estimates continue to be negative when we restrict to the sample of couples where the ticketed spouse rates the appropriateness of women weavers more negatively than the non-ticketed spouse

[26] We also cannot reject the mean expert prediction that the effect of $Info$ will be 2.5 percentage points larger when the husband gets the ticket than when the wife does ($p = 0.76$ with controls). It follows that while the experts mispredict the level of the effects of $Info$ separately for the husband-ticket and wife-ticket cases, their prediction for the differential effect by gender is better calibrated. Even so, a predicted treatment effect of 2.5 percentage points is small enough that even if the true effect is zero, our study is not well-powered to reject it.

(columns 5 and 6, Table 3). While in this case we lack expert predictions for comparison, we can reject positive effects of more than 5.4 percentage points at the 95% level in the specification with controls.

To test directly for information withholding, we use an indicator for the non-ticketed spouse knowing at endline that a ticket had been given to their spouse. We asked this question to adults in couples that did and did not receive tickets, phrasing the question in a way that would not give away the correct answer. In particular, the surveyor first asked whether the participant themself was given a ticket when surveyed a few days prior, and then asked whether the participant's spouse had received a ticket when surveyed.

Consistent with $Info$ not increasing enrollment, information diffusion is high. In the $NoInfo$ treatment, 74% of wives are aware that their spouse received a job ticket when the ticket was given to the husband. Similarly, 71% of husbands are aware when the ticket was given to the wife. The $Info$ treatment increases this awareness by eight percentage points when the husband gets the ticket, and 11 to 13 percentage points (though not significantly different to the eight) when the wife gets the ticket (columns 7 and 8, Table 3, $p = 0.06$ for pooled $Info$ effect). These increases are small, and given the non-positive effects on enrollment, they suggest that information withholding only occurs when that information would not be decision-relevant – i.e. upon sharing, the wife would still not enroll. These effects on ticket knowledge show most directly that neither husbands nor wives are strategically withholding information. We discuss potential explanations for the lack of information withholding in Section 6.4 below.

6.2 The Effect of Discussion

The bargaining frictions theory predicts that discussion could kickstart bargaining, thereby increasing enrollment. Experts agree with the prediction, expecting enrollment to be 11.6 percentage points higher in *Discuss* than in $NoInfo$. In reality, discussion lowers enrollment by eight to ten percentage points relative to $NoInfo$ (columns 1 to 2, Table 4, $p < 0.01$ with controls), and lowers enrollment by five to six percentage points relative to $Info$ ($p = 0.06$ with controls). Given that $NoInfo$ enrollment is already low at 18%, discussion reduces enrollment by 44 to 56%.

Focusing on the specification with controls, we reject the mean expert prediction of *Discuss* relative to $NoInfo$ ($p < 0.001$) and of *Discuss* relative to $Info$ ($p < 0.001$). In addition, we can reject almost every single expert's *Discuss* versus $NoInfo$ prediction at the 5% level, and 86% of their predictions for *Discuss* relative to $Info$. It is not just that experts mispredict the effects of discussion on average; essentially no expert even comes close.[27]

[27] Two of 70 experts predicted a negative effect of *Discuss* relative to $NoInfo$, though using the specification of column 2 of Table 4 we still reject these predictions at the 10% level. We reject one for being too negative (-17 percentage points, $p = 0.06$) and the other for not being negative enough (-2 percentage points, $p = 0.02$).

Recall the theory predicts that positive effects of discussion should be driven by couples that disagree about whether the wife should enroll. Contrary to the theory, our discussion estimates remain negative when we focus on the set of spouses that disagree about the appropriateness of women weaving (columns 3 to 4, Table 4).

A potential concern with the surprising negative effect of discussion is that it may be a chance result that would not replicate. This is of course difficult to rule out with a single experiment, but we can speak to this concern by considering effects of discussion for each village separately. As discussed above, our sample comes from six villages and the experiment was run in each village sequentially. If the discussion effect is a fluke, we might expect a small number of villages to drive it. But the *Discuss* point estimate relative to $NoInfo$ is negative in five of the six villages (Table A7).

6.3 Did Effects on Enrollment Translate Into Effects on Participation?

Our main outcome is enrollment in the program, a revealed preference measure of women's labor supply. But enrolling in the program is of course not the same as actually participating. We collected data on whether women in our sample were in the program at the end of the first month of training.[28] Dropout rates are substantial, at around 50% in the first month. This is consistent with McKelway (2023a), which finds high dropout from this program in another recruitment cycle and provides evidence for a heavy burden of household chores as a reason women drop out of employment in this setting. Nevertheless, the treatment effects on participation look similar to those on enrollment (Table A1). The fraction of women who were in the program at the end of its first month did not differ between $NoInfo$ and $Info$, but *Discuss* reduced participation by around 50%. However, we have less power with the participation outcome than with the enrollment outcome given its lower mean.

6.4 Mechanisms

We find suggestive evidence for veto power decision-making as an explanation for our results. We discuss this story here, but first turn to other potential explanations for the observed effects of information and discussion, along with evidence for or against these other explanations. We note that our mechanisms analyses are speculative; future research should be done to more carefully test whether couples make decisions through vetoes, along with other potential explanations.

[28] We accounted for enrollment that happened after enrollment day as well as dropout. However, only 9 women in our sample enrolled after enrollment day, meaning the differences between our enrollment outcome and being in the program at one month primarily reflect dropout from the program. By dropping out, we mean never participating in the program, or participating and later leaving.

6.4.1 Potential Explanations for No Effects of Information

The null effects of $Info$ could be rationalized by our initial bargaining with frictions model. It could be that the bargaining power of husbands is high enough that they never need to withhold information, and job preferences of wives are positive enough that they never want to. We would then expect information withholding to be more common in situations where wives receive information about an opportunity they do not support.

There are also potential explanations outside of this model. Spouses may have internalized norms of honesty, and consequently feel compelled to share relevant information, even if doing so is personally costly. Honesty norms in marriage seem particularly plausible given that even in lab experiments people lie surprisingly little when they have economic motives to do so (Abeler et al. 2019). Supporting this idea, when asked *"Do you think that your spouse keeps secrets from you?"* at baseline, 84% of husbands and 69% of wives answer *"Never"*. Only 2% of husbands and 6% of wives answer *"Often"* or *"Always"*, with the remainder answering *"Sometimes"*.

Another possibility is that respondents did not understand or believe they had plausible deniability. We find a lack of understanding unlikely. Surveyors explained the nature of the plausible deniability at length, and there were comprehension checks throughout the script to ensure respondents understood the information being conveyed. For example, the surveyor would ask *"Will anyone else know we have given you a ticket?"* If the respondent answered incorrectly in the $NoInfo$ group, the surveyor would then explain *"We will not tell any others we have given you a ticket. Note that our team is surveying many men and women in this round of surveying. Often no ticket will be given at all in these surveys. So others may see us speaking now but will not know there is a printed ticket for you to enroll or that you have that ticket."* If the respondent answered correctly, the surveyor would say the answer was correct before continuing. While participants are likely to have understood what we told them, we cannot fully rule out that they did not believe it. Relatedly, 10% may have been too low for the fraction of couples without tickets – individuals may not have known enough couples without tickets for not having one to seem plausible.

6.4.2 Potential Explanations for Negative Effects of Discussion

The negative effect of discussion is harder to understand. Before presenting our proposed veto mechanism, we discuss several alternative explanations we considered but did not find strong evidence for. We note, however, that we cannot fully rule all of these stories out, and it is possible that some combination of these mechanisms and the veto mechanism were all at play.

Discussion was awkward. One possible explanation for the negative effect of discussion is that couples found our *Discuss* treatment awkward. They may have associated the job with the research team and decided not to enroll to avoid another uncomfortable experience. However, enrollment was not the only time individuals might have interacted with our team after the discussion – we also asked them to take an endline survey. If they were less likely to enroll because they were avoiding another awkward experience, it is plausible that they would be less likely to take our endline survey. While women in *Discuss* were significantly less likely to take the endline survey than women in $Info$, the corresponding difference for men points in the opposite direction and is not significant, and there are no differences in survey rates for either gender between *Discuss* and $NoInfo$ (columns 1 and 2, Table A6). In addition, we do not see negative effects of *Discuss* on perceptions of how desirable the job is, measured at endline, relative to $NoInfo$ or to $Info$ (Table A10). This suggests that the negative effects of *Discuss* are not due to negative effects on individual-level job preferences, but rather due to effects on how those preferences are aggregated into a joint decision, as in the veto power story we outline below.

Forcing discussion led to argument. The *Discuss* treatment encouraged couples to have a discussion about a high-stakes decision at a particular time and in the presence of two surveyors. Forcing a discussion in this way could have produced arguments, leading couples to default to the status quo of not enrolling. However, *Discuss* did not affect wives' or husbands' endline reports of spousal disagreement about the job opportunity (columns 1 and 2, Table A11). It also did not make individuals report their spouse was less considerate of their opinions about the job opportunity, and relative to $NoInfo$, wives and husbands actually report that their spouse was *more* considerate of their opinion (columns 3 and 4, Table A11).[29][30] We also note that surveyors recorded whether they perceived the three-minute discussions to be argumentative or not, and none of these discussions were classified as argumentative.

Forcing discussion prevented women from bringing up the topic at an optimal time. Another possibility is that women who have little say in their households choose when and how to have discussions with their husbands in a way that makes women most likely to get their way, for instance, bringing up a topic like their employment over dinner or when their husbands are in a good mood. The *Discuss* treatment could have reduced employment by preventing women from bringing up the topic at the optimal time. This story would predict *Discuss* should have a particularly negative effect when women get the

[29] The results discussed in this paragraph are not necessarily at odds with the veto power channel described below. Under that channel, *Discuss* and $Info$ change the "rules" of the household decision, shifting the costs of exercising a veto, but that need not produce argument.

[30] Adults outside of the *Discuss* treatment who said they had no discussions were not asked how considerate their spouse was. Non-ticketed adults in $NoInfo$ that did not believe they had received a ticket were not asked how much the couple disagreed about the enrollment decision.

ticket. However, effects of *Discuss* look similar when women and men get the ticket, and if anything, the effects are a bit more negative when men get the ticket (Figure 5). Of course not all women want to work, not all men are opposed to women's work, and men might use a similar strategy as women to get their way; a more general version of this story would predict effects of *Discuss* should be especially negative when the ticketed spouse is more supportive of women working as weavers, but *Discuss* should have positive effects when the ticketed spouse is less supportive. We explore this sort of heterogeneity below when testing the veto power explanation. Effects are indeed more negative when the ticketed spouse is more supportive, but *Discuss* does not have positive effects when the ticketed spouse is less supportive.

Surveyors' presence increased adherence to the norm that women not work. Another possible explanation is that surveyors being present while couples discussed the job increased the salience of the norm that women not work outside their homes, and this reduced enrollment. If this were true, discussion should have had a more negative effect where this norm is stronger. We cannot test this explanation carefully, given that we did not measure perceived social norms at baseline. If we take each respondent's stated appropriateness of women weavers as a proxy for the local social norm, we do not find that effects of discussion are significantly more negative when individuals say women weaving is less appropriate (Table A8). This is true when comparing *Discuss* to $NoInfo$ or to $Info$. This story may also predict negative effects of discussion on perceived job desirability at endline, but as mentioned above, we do not see such effects (Table A10). However, neither of these analyses is a perfect test of this story, so greater norm adherence as a result of discussion remains a possible channel for negative effects.

Surveyors' presence made couple conform to norm of wives being quiet and husbands domineering. To conform to gender norms, wives may be quieter when discussing the job opportunity in public than when discussing in private. The *Discuss* treatment could then have reduced enrollment by preventing the more supportive views of wives from being heard. Going against this, the effects of discussion were no more negative when wives were *more* supportive of weaving, relative to $NoInfo$ or to $Info$ (Table A8). Similarly, to conform to norms of male authority, husbands may be more firm in their beliefs when being watched. But the effects of discussion were no more negative when husbands were *less* supportive of weaving (Table A8).

Discussion led to deeper deliberation. The *Discuss* treatment may have prompted deeper deliberation – for one thing, *Discuss* couples discussed the job opportunity more often (columns 1 to 2, Table A12).[31] If

[31] We measure the number of discussions by asking for the number of times a participant discussed the job opportunity with their spouse. If the participant was in the *Discuss* group, we asked for the number of times the two discussed the opportunity excluding the discussion during the survey, and then added one to this number if the surveyor reported that the couple discussed the opportunity during their survey.

taking the job were actually the wrong decision for many couples, deeper deliberation could have reduced enrollment. Two facts speak against this deliberation channel. First, if the deliberation in the discussion treatment group leads to better informed decisions, we should expect that for those not in the *Discuss* group, those who had more days to decide should be less likely to enroll. In contrast, we if anything find the opposite (columns 3 to 4, Table A12). Second, as mentioned above, discussion did not affect perceived job desirability (Table A10).

Discussion treatment made misrepresenting information about the job harder. Another possibility is that informing couples together made it harder for the ticketed spouse to make the job seem better than it was to the non-ticketed spouse. This could explain the difference in enrollment between *Discuss* and $NoInfo$, but could only explain the effect of *Discuss* relative to $Info$ if this effect were driven by couples within $Info$ that were assigned $TicketInfo$ (the sub-treatment in which the non-ticketed spouse was informed about the ticket only) rather than $FullInfo$ (the sub-treatment in which the non-ticketed spouse was given information about the ticket, the job, and enrollment). The information sub-treatment provides evidence on the effects of receiving the "official" information about the job from the research team. We find the sub-treatment made non-ticketed spouses view the job as more desirable at endline, while this story would predict the opposite (column 5, Table A2). The sub-treatment did reduce enrollment (column 6, Table A2), particularly when women were ticketed (Figure A6), but neither the overall effect nor the effect when women were ticketed is statistically significant. In sum, we do not find strong evidence for this channel, though we lack power to completely rule it out.

Experimenter Demand. Experimenter demand effects (de Quidt et al. 2018) on enrollment may have been triggered by discussing the job in the presence of two surveyors. However, the most plausible demand story would predict *higher* effects on enrollment given the reasonable assumption that surveyors wanted women to enroll. Experimenter demand is therefore unlikely to explain why the *Discuss* treatment reduced enrollment.

6.4.3 Veto Power Mechanism: Discussion and Evidence

We find suggestive support for a model of veto decision-making as an explanation for our results.[32] In this world, couples enroll if neither spouse vetoes it. However, if they exercise a veto, spouses incur a private utility cost representing internal or external pressure to be agreeable towards one's spouse. Our treatments could have varied these costs by determining who felt they had a right to make the decision. In particular, informing only one spouse, as in $NoInfo$, could have made that spouse feel entitled to

[32] We describe the veto power theory more formally in Appendix 3.3.

make the decision. Interventions that nudge households toward joint decision-making, like *Discuss*, and to a lesser extent, $Info$, could make a second spouse feel entitled to exercise a veto. All spouses in our setting have *de jure* veto power since enrollment requires both spouses to attend enrollment day, but under this story, our treatments affect *de facto* veto power by varying how personally costly it is to veto what one's spouse wants – $Info$ reduces the cost of vetoing of the non-ticketed spouse relative to $NoInfo$, and *Discuss* reduces the cost still further relative to $Info$. The cost of vetoing need not be the same for both genders, and given gender norms in our setting, it is likely to be higher for women than men. This, coupled with the fact that wives are more supportive of women weaving than husbands, means the veto power model has no prediction for whether enrollment will be higher when the husband or the wife receives the ticket in $NoInfo$.

The veto power theory rationalizes the lack of information withholding and the negative effect of discussion. In this world, ticket holders always feel comfortable exercising a veto so they cannot lose from sharing information. This is the case even for supportive spouses, since sharing information is a pre-requisite to being able to enroll, given that both spouses must be present for enrollment. Likewise, both spouses in *Discuss* would feel comfortable vetoing, which would make enrollment less likely than if just one spouse felt comfortable exercising a veto.

The veto power story makes two additional predictions for which we find support. First, note that in the *NoInfo* treatment group, among disagreeing couples, it is random whether the ticket was assigned to the more supportive or to the less supportive spouse. According to the veto power story, assigning the ticket to the more supportive spouse should increase enrollment, even in the absence of information withholding – because ticketed spouses feel more entitled to veto enrollment than non-ticketed spouses. This is exactly what we find in Table 5, where we keep only disagreeing couples, and estimate the enrollment effects of the ticket being assigned to the more supportive spouse. In the *NoInfo* condition, assigning the ticket to the more supportive spouse increases enrollment by 12 percentage points (column 1). And consistent with the *Info* and *Discuss* treatments strengthening the veto power of the non-ticketed spouse, there is no effect of assigning the ticket to the more supportive spouse in the *Info* or *Discuss* arms (column 2).

The second, and related, prediction is that intervention toward joint decision-making should reduce enrollment only when the non-ticketed spouse is less supportive of enrollment than the ticketed one. While we hinted at the role of disagreement between spouses in Tables 3 and 4, we now estimate treatment effects separately for two subsamples: the couples in which the non-ticketed spouse rated women weaving as less appropriate at baseline than the ticketed spouse did, and the remaining couples (in which the non-ticketed spouse was equally or more supportive of female weavers). Our $Info$ and *Discuss* treatments both significantly reduce enrollment when the non-ticketed spouse is less supportive of female

weavers than the ticketed one (panel (b), Figure 6), and do not affect enrollment for the other couples (panel (a)). Table 6 refines this test by splitting the latter set of couples into two groups: those that agree at baseline, and those in which the non-ticketed spouse was more supportive of female weavers. Of the three subgroups, the effects of *Info* and *Discuss* are the most negative for the couples in which the non-ticketed spouse is less supportive. In our highest-power test in column 4, we see that being assigned to any joint decision-making intervention (*Info* or *Discuss*) has essentially null effects for the agreeing couples. Relative to effects among the agreeing couples, effects are slightly more negative when the non-ticketed spouse is more supportive but not significantly so, while effects are significantly more negative when the non-ticketed spouse is less supportive (with $p = 0.12$ for the test of equality of effects for the two types of disagreeing spouses).

One potential concern with the interpretation of these patterns of heterogeneity is that when one spouse is less supportive of female weavers than the other, it is more often husbands who are less supportive than wives (panel (a), Figure 1). Perhaps these patterns of heterogeneity by disagreement are not about disagreement per se, but rather are picking up on heterogeneity by wives' support or by husbands' opposition. But as mentioned above, we see no heterogeneity by wives' or husbands' individual reports on the appropriateness of female weavers (Table A8).

Summing up, we see two pieces of evidence in line with the idea that joint decision-making interventions strengthen the veto power of the non-ticketed spouse: ticket assignment matters for enrollment in the *NoInfo* treatment, and joint decision-making interventions reduce enrollment especially among couples where the non-ticketed spouse is less supportive of women weaving. That said, other evidence is mixed. First, while $Info$ and *Discuss* make it more likely that ticket-holders report at endline that both spouses had equal influence over the enrollment decision, as the veto power story would predict, these effects are not statistically significant (Table A9). Second, while tests of the veto power theory can rationalize the negative effect of *Discuss* relative to $NoInfo$, they do little to explain the effect of *Discuss* relative to $Info$. In particular, if *Discuss* reduced enrollment relative to $Info$ through the same channel, we would expect the interaction effect between *Discuss* and the indicator for the non-ticketed spouse being less supportive to be more negative than the same interaction effect for $Info$. In practice, the two interactions are similar in magnitude, and we cannot reject that they are equal (Table 6).

In sum, the bargaining frictions model predicts positive effects of information and discussion. The lack of information withholding and negative effect of discussion that we observe are instead consistent with a model with more substantial deviations from the efficient model. In particular, these results are consistent with households making decisions about enrollment through vetoes. In this world, interventions that treat spouses symmetrically give each veto power, while informing only one spouse weakens the other's veto power. While we only find suggestive support for this model, we find even less support for

alternative explanations. Given this, we consider the veto power story the most plausible rationalization of our results, and an important candidate model for future empirical tests.

7 Conclusion

Our paper has three key take-aways. First, we find that how opportunities are presented to households matters for eventual take-up decisions. Specifically, we learn that nudging households towards joint decision-making through an intervention like our discussion treatment may decrease take-up. This is an important result since many anti-poverty policies introduce new opportunities to households – immunizations, agricultural technologies, and workfare programs, to name a few – with the hope that households will take them up. Nudging households to discuss an opportunity might seem a natural implication of bargaining costs (Coase 1960; Riedl 1995; Anderlini and Felli 2001; Coase 2005) and of non-experimental evidence that suggests spousal communication about family planning increases take-up (Bawah 2002; Shattuck et al. 2011; Hartmann et al. 2012).

Second, our results suggest that much remains to be learned about intervention into the household. This is illustrated not only by the fact that our findings are not what experts (or we) predicted on average, but also by data we collected on experts' confidence in their predictions (Figure A7). Experts were quite uncertain of how our results would play out, with the average expert predicting just 2.7 of their 7 predictions would be within 5 percentage points of the truth. But even this low confidence was overconfidence; only 1.1 predictions were in the 5 percentage point margin of the truth on average. Furthermore, confidence is uncorrelated with accuracy, as with the academic experts in DellaVigna and Pope (2018). The more experienced experts in our sample are not more confident, but they tend to give *less* accurate predictions (Table A13). In particular, Full Professors give significantly less accurate predictions than Assistant Professors (column 4), while those with more Google Scholar citations are less accurate than those with fewer (columns 5 and 6). Confidence is still uncorrelated with accuracy after controlling for citations, as is a proxy for effort: whether the expert spent above-median time on the survey (column 6).

An important question remains: are experts miscalibrated about intervening in the household in general, or were our study results somehow unusually difficult to predict? While we cannot answer this question well without a meta-analysis of intra-household expert surveys,[33] we note the results of a study with important parallels to ours. Dean and Jayachandran (2019) ran a field experiment, concurrent with ours, on the retention of female teachers in South India. Their *Conversation* treatment has parallels with our *Discuss* treatment: a surveyor guided a conversation between the female teacher and her family

[33] Such a meta-analysis may eventually be possible given the increasing popularity of the Social Science Prediction Platform (socialscienceprediction.org, DellaVigna et al. 2019).

members about the pros and cons of her working. While Dean and Jayachandran (2019) cannot reject the null hypothesis of no effect (given a smaller sample of $N = 171$ teachers), the estimated effect of *Conversation* on retention is negative, and similar to our own estimated effects of *Discuss* at –6 percentage points. Furthermore, if experts predicted the same treatment effect for *Conversation* as they did for *Discuss* (relative to $NoInfo$, i.e. an 11.6 percentage point effect), this prediction would be rejected by Dean and Jayachandran (2019) at the 5% level. While only suggestive, these results are consistent with experts more generally mispredicting the effects of interventions into household decision-making in India.

Finally, our results point to the importance of research on household decision-making processes. Our ex ante bargaining with frictions model and the alternative veto model have entirely different implications for policy intervention. With bargaining frictions, intervention towards joint decision-making could promote decisions that move away from the status quo, in our case increasing female labor supply in India. Instead, with decision-making driven by vetoes, intervention towards joint decision-making could have the opposite effect – such interventions could give both spouses veto power rather than just one, leading to status quo bias. Future research should test more directly for this veto decision-making, and explore household decision processes more broadly. The success of intervention into the household hinges on having accurate mental models of how households make decisions.

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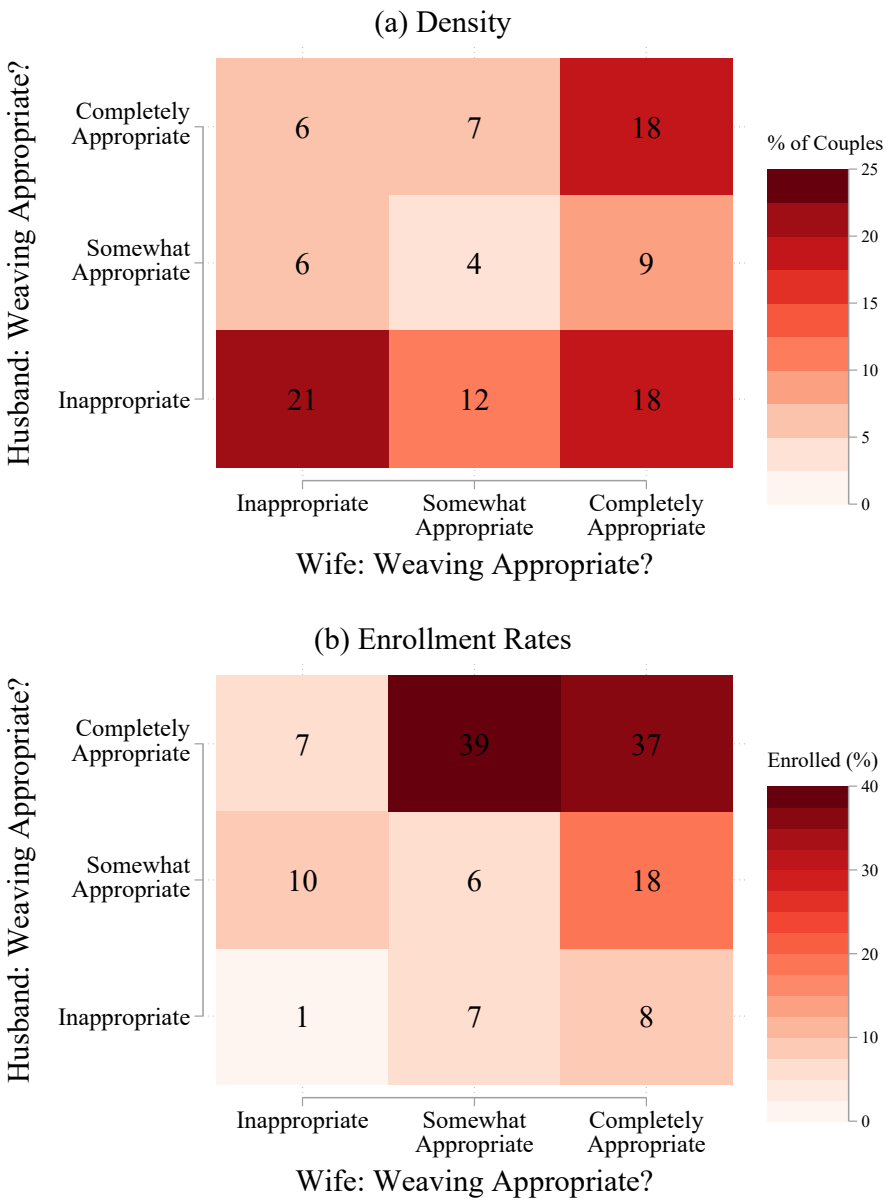
Figures

Figure 1: Misaligned Preferences and Enrollment



Notes: Panel (a) shows a heatmap of the percentage of couples with each possible combination of answer given to the baseline question: *"How appropriate would it be for women in your household to hold a full-time job outside the home as a weaver?"* Panel (b) shows job enrollment rates in the experiment, pooling across all treatments, separately by each baseline preference combination.

Figure 2: Experiment Timeline

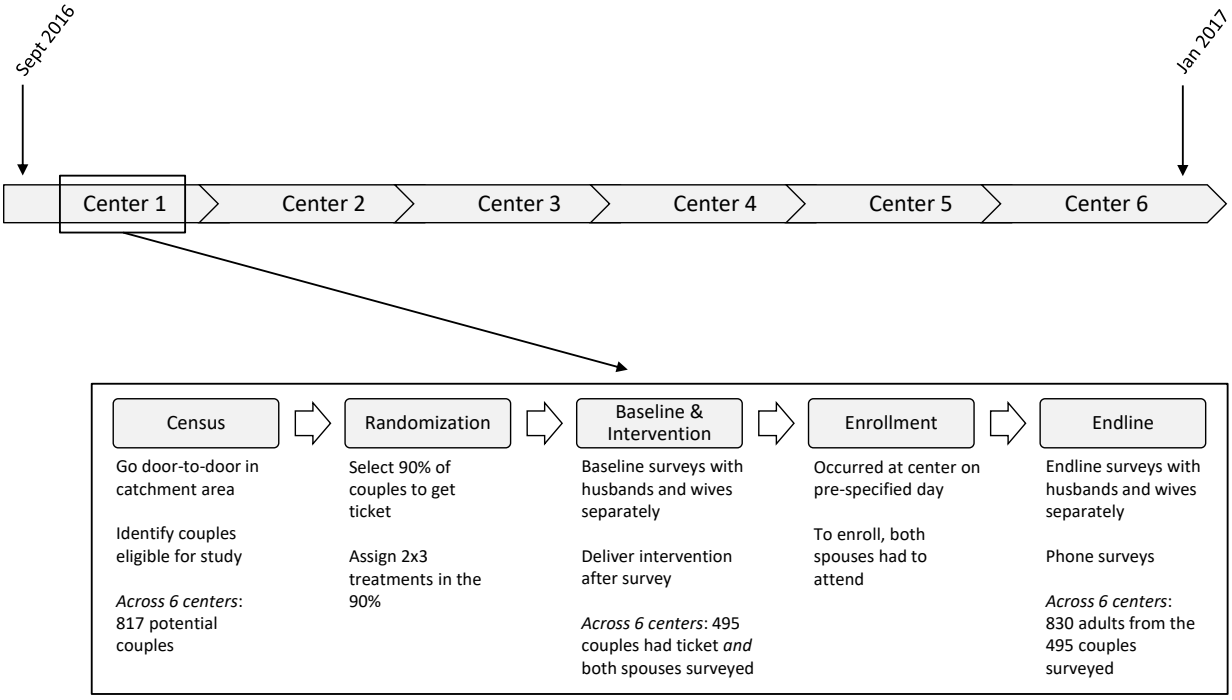
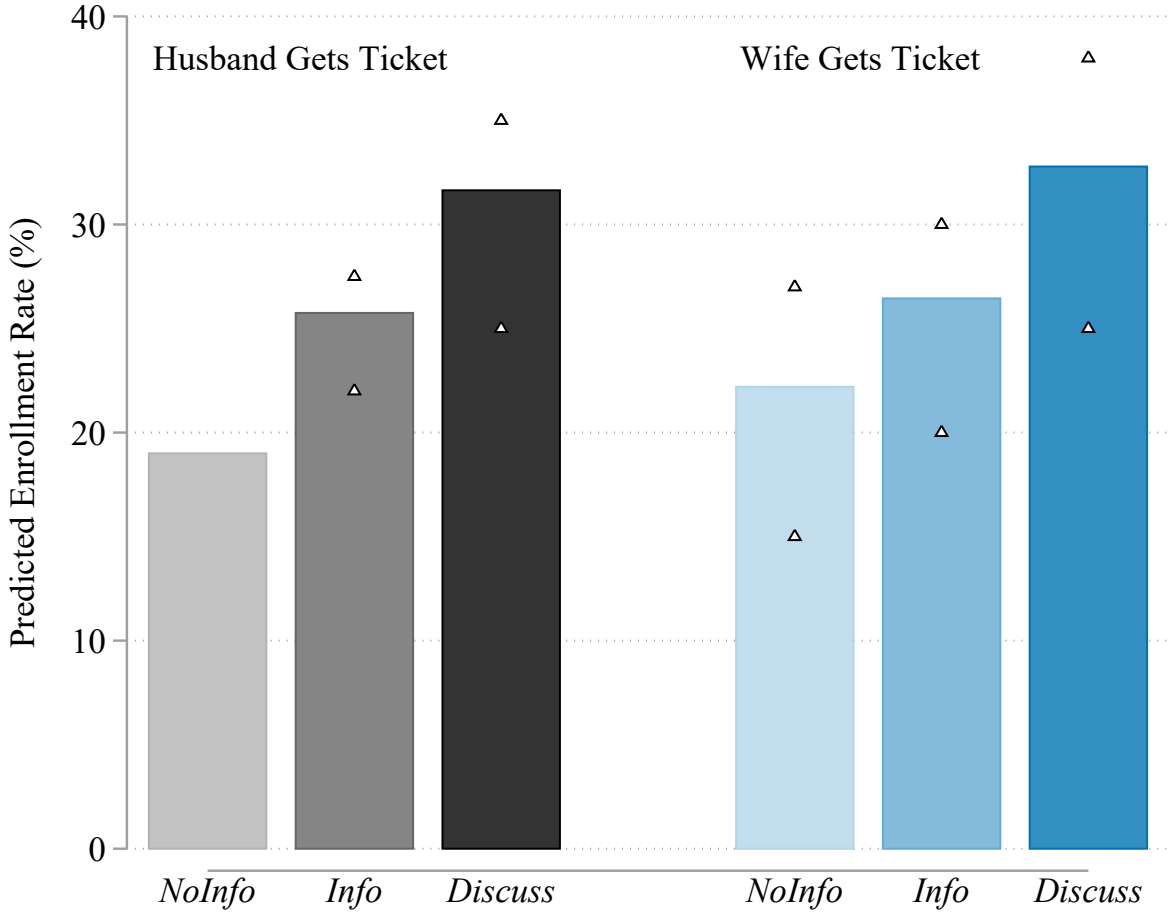


Figure 3: Experiment Design

Non-Ticketed:	*Ticketed Spouse:* **Husband**	**Wife**	
NoInfo	*77 couples*	*82 couples*	159 couples
Info	*83 couples*	*88 couples*	171 couples
Discuss	*88 couples*	*77 couples*	165 couples
	248 couples	247 couples	

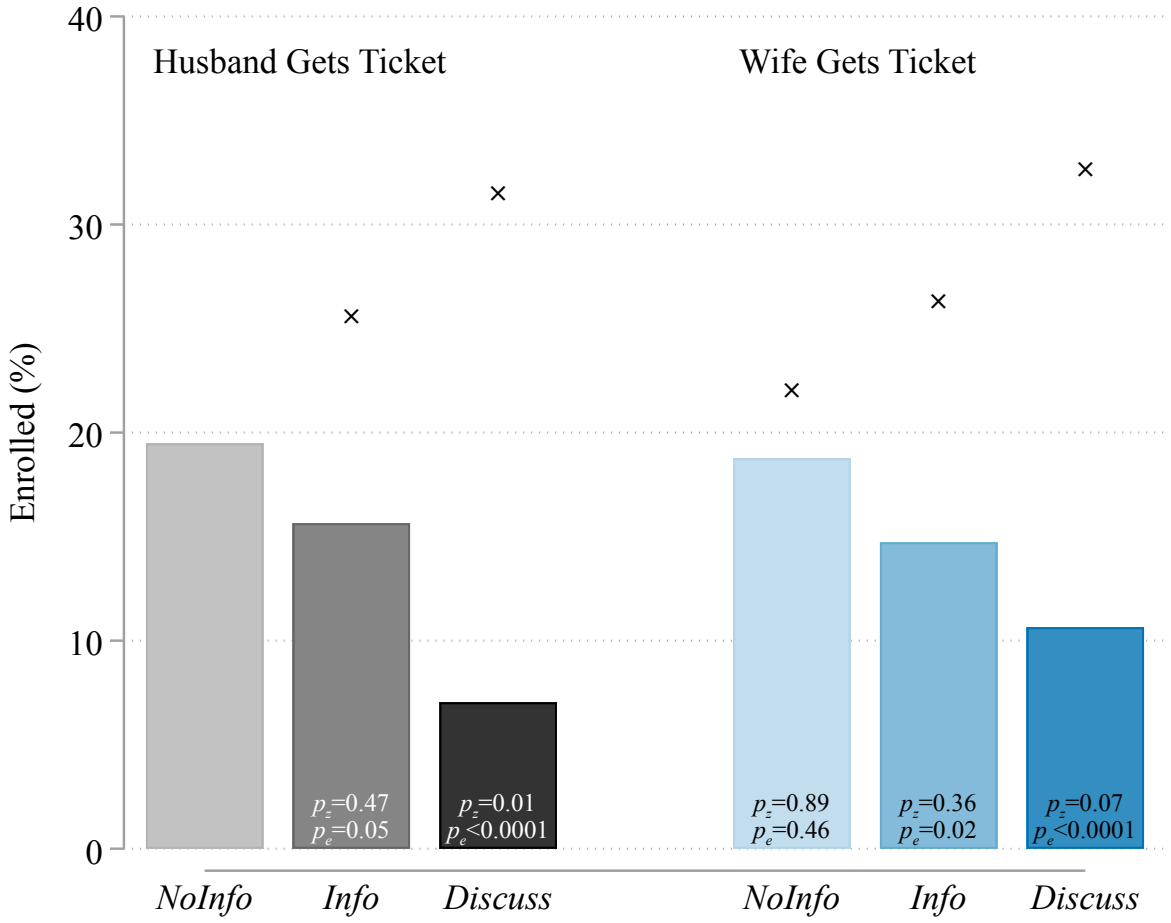
Notes: The figure visualizes our 3-by-2 experimental design, with randomization at the couple-level. The columns indicate which spouse was given the ticket required to enroll in the job. The rows indicate the information treatment for the non-ticketed spouse. In *NoInfo* the non-ticketed spouse is not aware that their spouse has a job ticket, and is aware that not all couples get job tickets, giving their spouse plausible deniability. In *Info* the non-ticketed spouse knows of the existence of the job ticket. In *Discuss* the non-ticketed spouse is present when the spouse receives the job ticket, and the couple are encouraged to discuss the opportunity together for three minutes. The numbers indicate the number of couples in our analysis sample of 495 couples who were assigned a given treatment.

Figure 4: Expert Predictions Line Up With Predictions of Bargaining Frictions Model



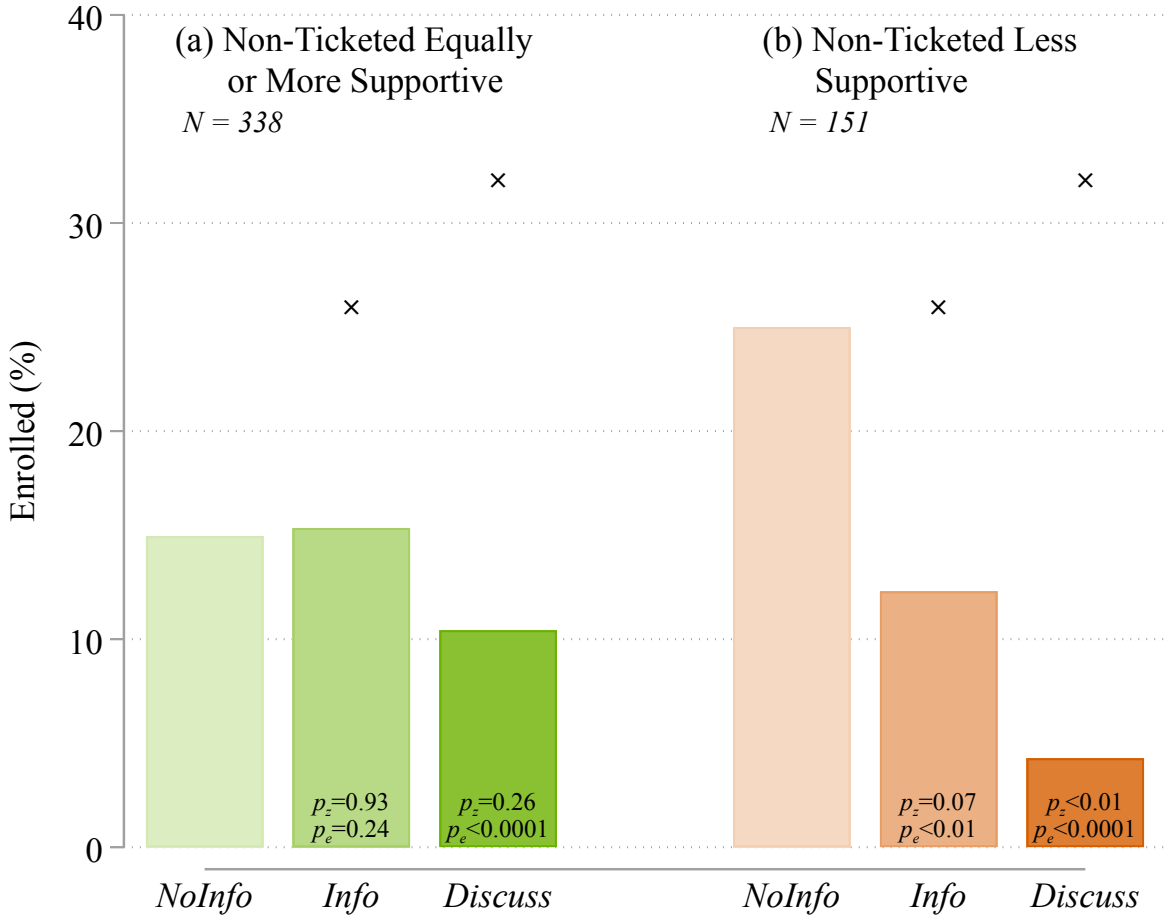
Notes: The figure shows the mean predicted enrollment rate for each treatment according to our full sample of 70 experts on intra-household economics. Each expert was told the true enrollment rate (19%) of the treatment in which the husband gets the job ticket and the wife doesn't know, while the other five bars reflect predictions. Triangles denote the 25th and 75th percentile prediction for each treatment.

Figure 5: Impacts on Enrollment Are Far Less Positive Than Experts Predict



Notes: The figure visualizes enrollment rates across the six treatments, derived from a regression ($N = 495$) of enrollment on treatment indicators, village fixed effects, a dummy variable for OBC caste status, and the following controls: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, (11) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (12) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each of these variables set to the mean when missing. Crosses denote mean predictions from the full sample of 70 experts. p_z is the p-value from a test of whether the treatment effect relative to the control group category (the husband gets the ticket, the wife doesn't know) is equal to zero. p_e is the p-value from a test of whether the treatment effect is equal to the mean expert prediction.

Figure 6: Joint Decision-Making Interventions Reduce Enrollment Only When Non-Ticketed is Less Supportive



Notes: The figure visualizes enrollment rates across the three information treatments, separately for two subsamples: couples in which the non-ticketed spouse rated women weaving as less appropriate at baseline than the ticketed spouse did (panel (b)); and the rest of the couples, in which the non-ticketed spouse rated women weaving as equally or more appropriate (panel (a)). The enrollment rates are then derived from two regressions, each regressing enrollment on indicator variables for $Info$ and *Discuss*, village fixed effects, a dummy variable for OBC caste status, and the following controls: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, (11) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (12) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each of these variables set to the mean when missing. Crosses denote mean predictions from the full sample of 70 experts. These predictions do not differ by subsample as experts were only asked to predict for the entire sample. p_z is the p-value from a test of whether the treatment effect relative to $NoInfo$ is equal to zero. p_e is the p-value from a test of whether the treatment effect is equal to the mean expert prediction.

Tables

Table 1: Wives Are More Supportive of Women Working Outside the Home Than Husbands

	Appropriate for Man?			Appropriate for Woman?			
	Construction	Weaver	Teacher	Construction	Weaver	Teacher	Job Interest
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Wife	-0.04	0.05	-0.02	0.29***	0.31***	0.11***	0.14**
	(0.05)	(0.05)	(0.02)	(0.05)	(0.05)	(0.04)	(0.06)
Observations	976	974	975	973	973	971	500
Husband Mean	1.4	1.4	1.9	.57	.81	1.7	1.2

Notes: Robust standard errors in parentheses. The outcomes for the first three columns are the perceived appropriateness for men in the household to work full-time outside the house in construction, teaching, and weaving. The outcomes for the second three columns are the same three perceived appropriateness measures, but for women. These appropriateness outcomes take values: 0 = Inappropriate, 1 = Somewhat Appropriate, 2 = Completely Appropriate. During the baseline survey, all wives informed about the job opportunity were asked *How interested are you in this training opportunity?*, while all informed husbands were asked *How interested are you in this training opportunity for [wife's name]?* Job Interest in column (7) is the answer to this question, taking values: 0 = Not At All/Not Very Interested, 1 = Somewhat Interested, 2 = Very Interested. The final column restricts to only those couples who were both asked about their interest during baseline. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Husbands' Preferences Are More Predictive of Enrollment Than Wives'

	Enrolled	
	(1)	(2)
Husband: Weaving Appropriate for Women in HH?	0.12***	0.11***
	(0.02)	(0.02)
Wife: Weaving Appropriate for Women in HH?	0.06***	0.05***
	(0.02)	(0.02)
Observations	489	489
p(Husband = Wife)	0.03	0.03
Outcome Mean	0.15	0.15
Controls	No	Yes

Notes: Robust standard errors in parentheses. Enrolled is an indicator for enrolling on enrollment day. Appropriateness outcomes take values: 0 = Inappropriate, 1 = Somewhat Appropriate, 2 = Completely Appropriate. Controls are: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, with each of these variables set to the mean when missing. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Neither Husbands nor Wives Withhold Job Information

	Enrolled						Non-Ticketed Knows	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Info	-0.03	-0.04			-0.05	-0.09		
	(0.04)	(0.04)			(0.07)	(0.07)		
Info × Husband Gets Ticket			-0.04	-0.04			0.08	0.08
			(0.06)	(0.05)			(0.07)	(0.07)
Info × Wife Gets Ticket			-0.03	-0.04			0.13*	0.11
			(0.05)	(0.05)			(0.07)	(0.07)
Observations	495	495	495	495	133	133	414	414
Omitted Group Mean	0.18	0.18	0.19	0.19	0.11	0.11	0.74	0.74
p(*Info* = Expert Mean)	0.03	0.01						
% Experts Rejected	50	60						
p(*Info* × Husb. = *Info* × Wife)			0.90	0.98			0.63	0.79
p(*Info* × Husb. = Expert Mean)			0.06	0.05				
p(*Info* × Wife = Expert Mean)			0.20	0.12				
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Discuss	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Discuss × Husband Gets Ticket	No	No	Yes	Yes	No	No	Yes	Yes
Husband Gets Ticket	No	No	Yes	Yes	No	No	Yes	Yes

Notes: Robust standard errors in parentheses. Enrolled is an indicator for enrolling on enrollment day. Non-Ticketed Knows is an indicator for the non-ticketed spouse knowing their spouse received an enrollment ticket. Columns (5) and (6) include only the sample of couples where the ticketed spouse rates the appropriateness of women weavers more negatively than the non-ticketed spouse. % Experts Rejected is the percentage of 70 expert predictions of the effect of *Info* relative to *NoInfo* that can be rejected at a 5% significance level. Strata controls are village fixed effects and an indicator for OBC status. Controls are: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, (11) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (12) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each set to the mean when missing. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Discussion Reduces Enrollment

	Enrolled			
	(1)	(2)	(3)	(4)
Info	-0.03	-0.04	-0.08	-0.08
	(0.04)	(0.04)	(0.05)	(0.05)
Discuss	-0.08**	-0.10***	-0.12**	-0.16***
	(0.04)	(0.03)	(0.05)	(0.04)
Observations	495	495	284	284
Omitted Group Mean	0.18	0.18	0.19	0.19
p(*Discuss* = *Info*)	0.22	0.06	0.34	0.07
p(*Discuss*-*Info* = Expert Mean)	0.003	<0.001		
% Experts Rejected (*Discuss* vs. *Info*)	64	86		
p(*Discuss* = Expert Mean)	<0.001	<0.001		
% Experts Rejected (*Discuss* vs. *NoInfo*)	99	99		
Strata	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Notes: Robust standard errors in parentheses. Enrolled is an indicator for enrolling on enrollment day. Columns (3) and (4) include only the sample of disagreeing couples: where each spouse rates the appropriateness of women weavers differently. % Experts Rejected is the percentage of 70 expert predictions that can be rejected at a 5% significance level, first considering the effect of *Discuss* relative to *Info*, then the effect of *Discuss* relative to *NoInfo*. Strata controls include village fixed effects and an indicator for OBC status. Controls are: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, (11) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (12) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each set to the mean when missing. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Ticket Assignment Affects Enrollment in *NoInfo* Condition But Not Others

	Enrolled	
	(1)	(2)
Ticketed More Supportive	0.12*	0.00
	(0.06)	(0.05)
Observations	96	188
Outcome Mean	0.19	0.11
Sample	*NoInfo*	*Info/Discuss*
Strata	Yes	Yes
Controls	Yes	Yes

Notes: Robust standard errors in parentheses. Sample includes only couples that disagree about the appropriateness of women weavers at baseline. Ticketed More Supportive is an indicator variable for the ticket being assigned to the spouse that is more supportive of women weavers. Strata controls are village fixed effects and an indicator for OBC status. Controls are: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, (11) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (12) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each set to the mean when missing. *** p<0.01, ** p<0.05, * p<0.1.

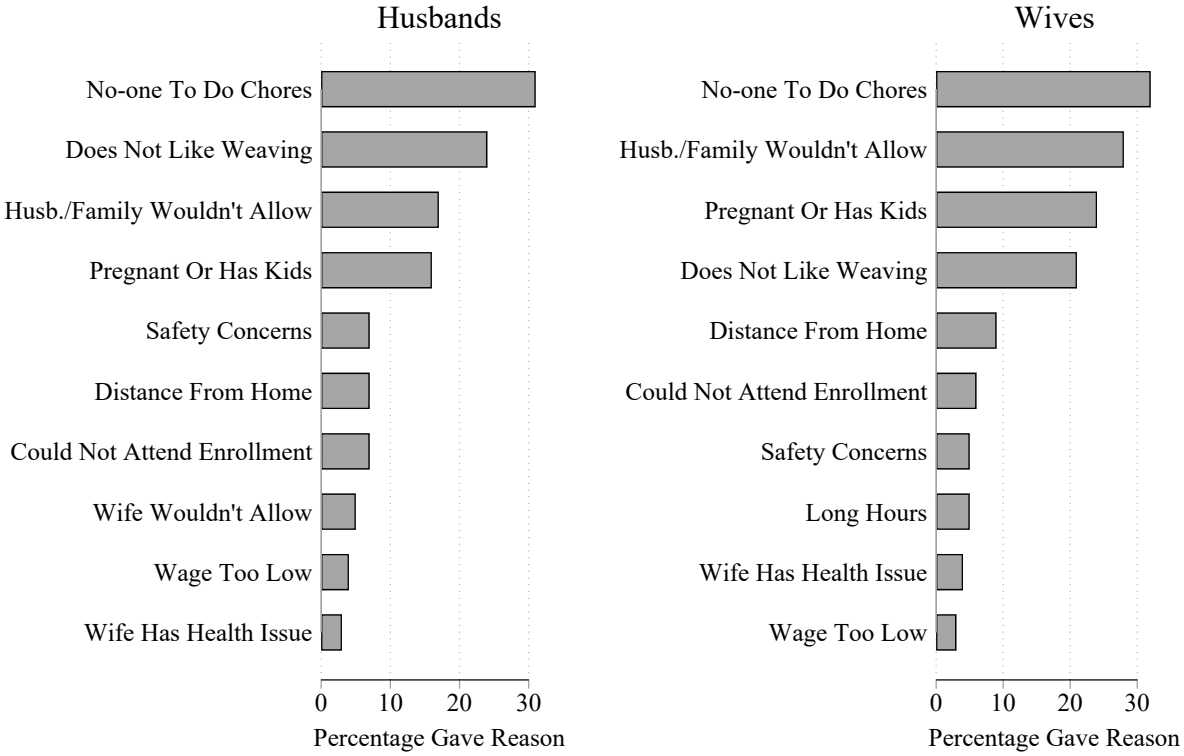
Table 6: Effects Are More Negative When Non-Ticketed is Less Supportive

	Enrolled			
	(1)	(2)	(3)	(4)
Info	0.04	0.03		
	(0.06)	(0.06)		
Discuss	-0.03	-0.03		
	(0.06)	(0.05)		
Non-Ticketed More Supportive	-0.01	-0.01	-0.01	-0.01
	(0.06)	(0.06)	(0.06)	(0.06)
Non-Ticketed Less Supportive	0.07	0.11	0.07	0.11
	(0.07)	(0.07)	(0.07)	(0.07)
Info × Non-Ticketed More Supportive	-0.07	-0.06		
	(0.09)	(0.09)		
Discuss × Non-Ticketed More Supportive	-0.04	-0.05		
	(0.08)	(0.08)		
Info × Non-Ticketed Less Supportive	-0.16*	-0.17*		
	(0.10)	(0.09)		
Discuss × Non-Ticketed Less Supportive	-0.14	-0.20**		
	(0.09)	(0.09)		
Info or *Discuss*			0.01	0.00
			(0.05)	(0.05)
(*Info* or *Discuss*) × Non-Ticketed More Supportive (i)			-0.06	-0.05
			(0.08)	(0.07)
(*Info* or *Discuss*) × Non-Ticketed Less Supportive (ii)			-0.15*	-0.18**
			(0.08)	(0.08)
Observations	489	489	489	489
p(Less Supportive = More Supportive)	.27	.074	.27	.076
p((i) = (ii))			.3	.12
Strata	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Notes: Robust standard errors in parentheses. Enrolled is an indicator for enrolling on enrollment day. Non-Ticketed Less (More) Supportive is an indicator for the non-ticketed spouse rating women weaving as less (more) appropriate at baseline than the ticketed spouse did. Strata controls include village fixed effects and an indicator for OBC status. Controls are: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, (11) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (12) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each control variable set to the mean when missing. *** p<0.01, ** p<0.05, * p<0.1.

A Appendix [For Online Publication]

Figure A1: The Most Common Reasons for Non-Enrollment

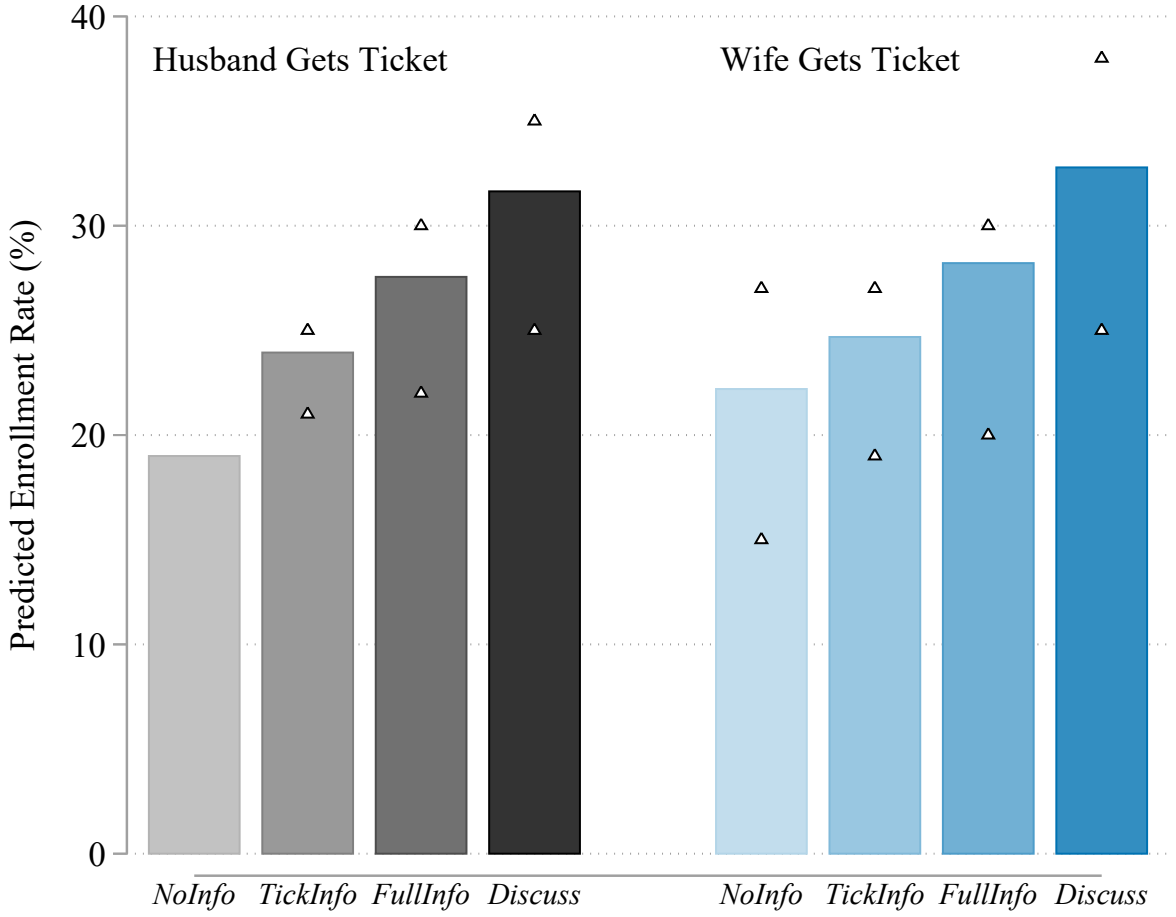


Notes: The figure shows the ten most common reasons for non-enrollment given by 334 husbands and 339 wives during the endline survey. Specifically, during the endline survey we asked wives and husbands of non-enrolling households: *What are the main reasons your household decided not to enroll in this opportunity?* Respondents could select multiple answers from among the following options: (1) fear security/safety issue, (2) no-one to take over housework, childcare, and agricultural work at home, (3) wife is pregnant or has young child, (4) hours too long, (5) wife already has employment, (6) worried what others will think, (7) husband/in-laws/family wouldn't allow, (8) wife wouldn't allow, (9) don't need the money, (10) wage too low, (11) don't like this particular job (weaving), (12) worry that a job after training wouldn't be guaranteed, (13) center too far from home, (14) woman has health issue, (15) one or both could not attend, or forgot, enrollment, (16) lost or did not have ticket, and (17) don't know. Among non-enrolling households, we did not ask this question to non-ticketed spouses in the *NoInfo* treatment group when these spouses were not aware of any job ticket, since these spouses did not have any say in the decision not to enroll.

Figure A2: An Enrollment Ticket

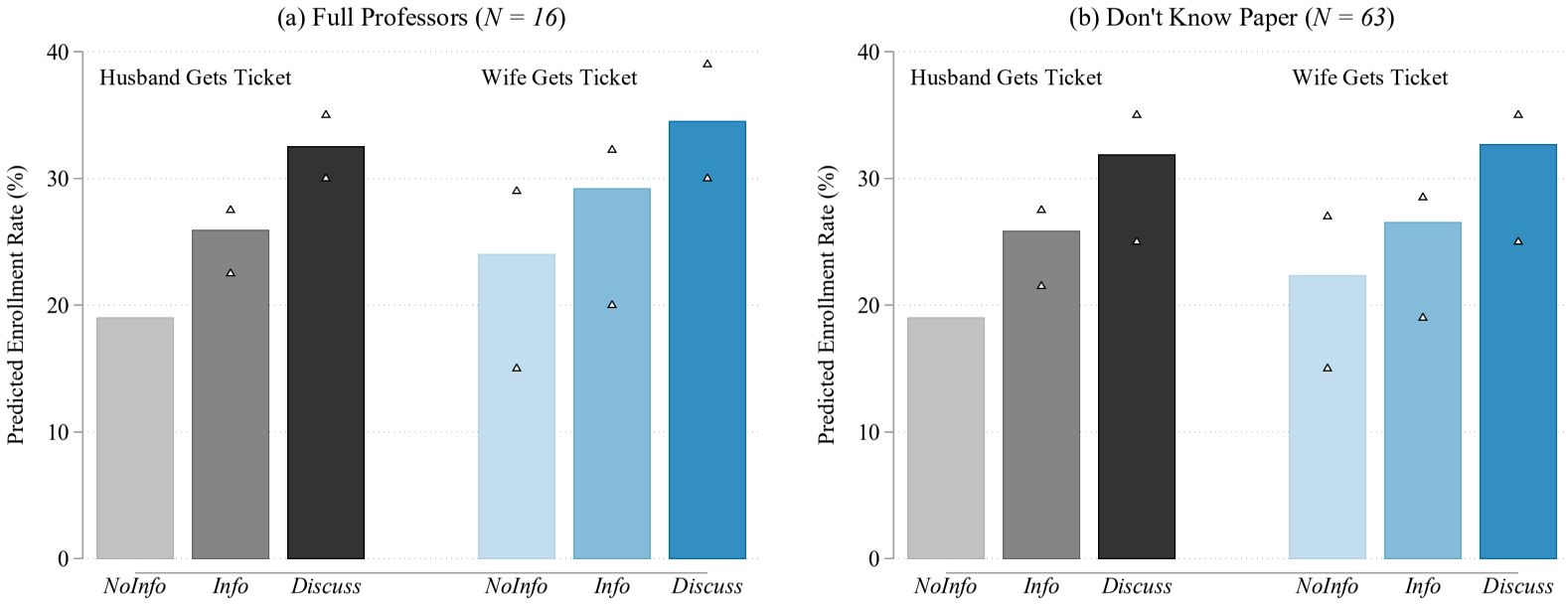
Notes: This is a photo of an enrollment ticket. The first line says "enrollment ticket." The second provides the names of the husband and wife to which this ticket corresponds. The names are blurred for confidentiality. The third line has the unique identification number we assigned to this couple. The fourth and fifth lines provide the date and time of enrollment. The last line has the location of enrollment, which was the female weaving center in the couple's village that was owned by a woman in the village and adjacent to her house. This last line has the names of the loom owner and her husband, and is blurred for confidentiality.

Figure A3: Expert Predictions Including *Info* Sub-Treatments



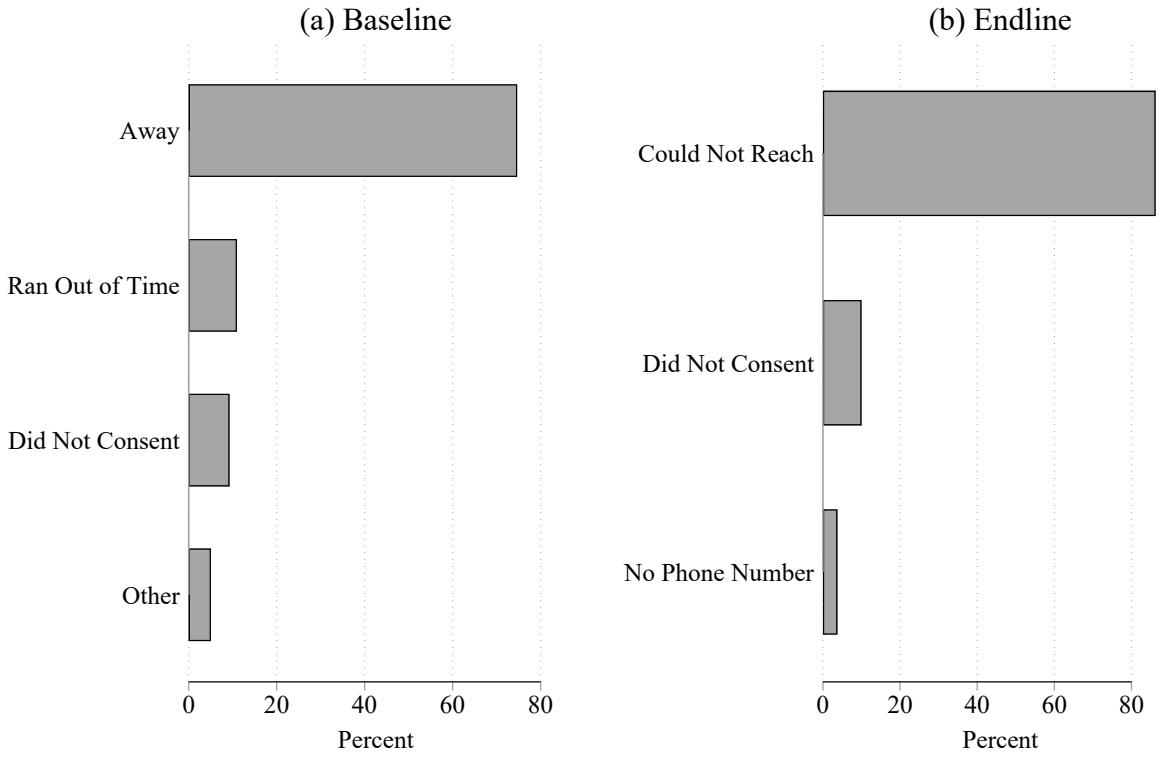
Notes: The figure shows the mean predicted enrollment rate for the full eight treatment cells according to our full sample of 70 experts on intra-household economics. Each expert was told the true enrollment rate (19%) of the treatment in which the husband gets the job ticket and the wife doesn't know, while the other seven bars reflect predictions. Triangles denote the 25th and 75th percentile prediction for each treatment. The *TickInfo* and *FullInfo* treatment cells each contain half of the sample size of the other cells, and we pool them into a combined *Info* treatment in our analysis.

Figure A4: Robustness of Expert Predictions



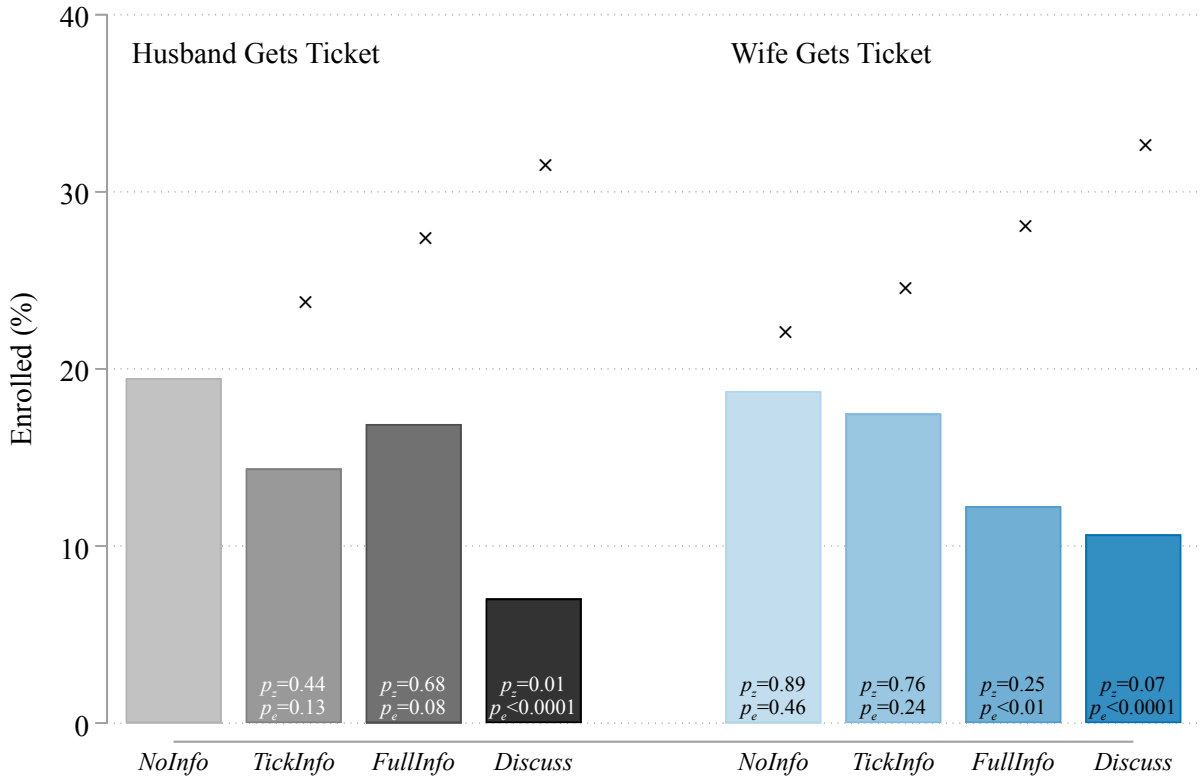
Notes: The figure shows the mean predicted enrollment rate for each treatment cell according to experts on intra-household economics. Panel (a) shows the predictions of the 16 experts who are Full Professors. Panel (b) shows the predictions of the 63 experts that answered *No* to the question *Have you seen or heard results from an experiment on household decision-making about female labor supply in India conducted by Matt Lowe (UBC) and Madeline McKelway?* after reading the details of the experiment. This sample excludes the seven experts that had seen the results of the experiment but couldn't remember them. Each expert was told the true enrollment rate (19%) of the Husband Gets Ticket-NoInfo treatment cell, while the other five bars reflect unincentivized predictions. Triangles denote the 25th and 75th percentile prediction for each treatment.

Figure A5: Reasons for Attrition



Notes: Panel (a) shows the reasons for non-completion of the Baseline survey ($N = 237$ couples). *Away* means that the husband, wife, or both, were out of town, away all day, or otherwise not available for the Baseline survey. *Ran Out of Time* means that the canvassing period prior to enrollment day ended before the couple could be surveyed. Panel (b) shows the reasons for non-completion of the Endline survey ($N = 160$ individuals, from the $N = 990$ that completed the Baseline survey).

Figure A6: Results Summary Including *Info* Sub-Treatments



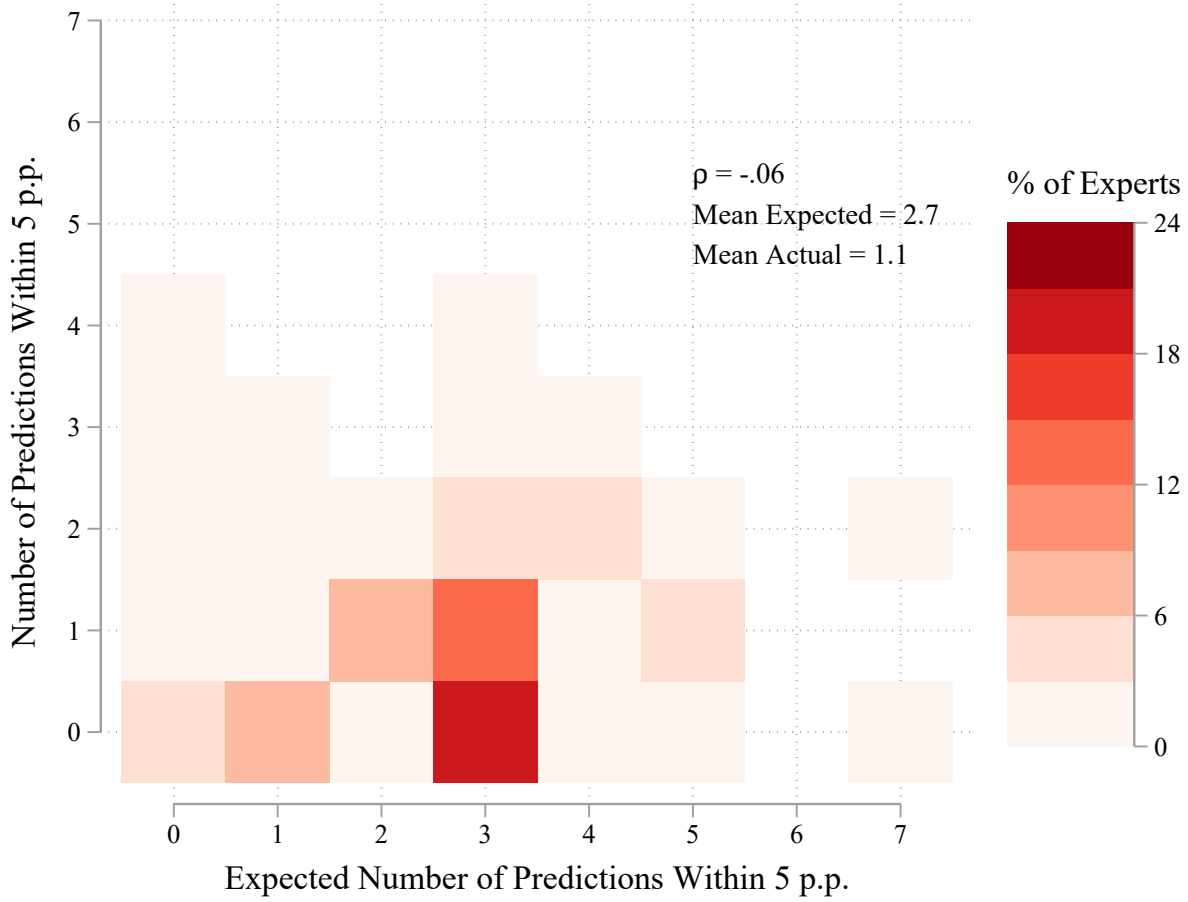
Notes: The figure visualizes enrollment rates across the eight treatments, including the two sub-treatments (with half the sample size of the other cells) of the *Info* treatment. The figure is derived from a regression ($N = 495$) of enrollment on treatment indicators, village fixed effects, an indicator for OBC status, and the following controls: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, (11) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (12) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each set to the mean when missing. Crosses denote mean predictions from the full sample of 70 experts. p_z is the p-value from a test of whether the treatment effect relative to the control group category (the husband gets the ticket, the wife doesn't know) is equal to zero. p_e is the p-value from a test of whether the treatment effect is equal to the mean expert prediction.

Table A1: Treatment Effects on Participation in the First Month

	In Program at End of First Month	
	(1)	(2)
Info	0.01	0.01
	(0.03)	(0.03)
Discuss	-0.04	-0.05*
	(0.03)	(0.03)
Observations	495	495
Omitted Group Mean	.094	.094
p(*Discuss* = *Info*)	.048	.019
Strata	Yes	Yes
Controls	No	Yes

Notes: Robust standard errors in parentheses. The outcome is an indicator for the wife having enrolled in the weaving program within the first month, and not having dropped out in that month. Strata controls include village fixed effects and an indicator for OBC status. Controls are: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, (11) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (12) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each set to the mean when missing. *** p<0.01, ** p<0.05, * p<0.1.

Figure A7: Experts Are Overconfident



Notes: The figure shows a heatmap of the percentage of experts with different levels of confidence in their predictions and different levels of ex-post accuracy. Confidence is measured from zero to seven on the x-axis as the answer to the question: *How many of your seven predictions do you expect are 5 percentage points or less from the actual enrollment rate in each treatment group?* Accuracy is measured from zero to seven on the y-axis as the actual number of predictions within 5 percentage points of the actual enrollment rate. Actual enrollment rates come from a regression of the enrollment indicator on seven treatment dummy variables (the omitted category being husband gets ticket, *NoInfo*), strata fixed effects, and controls. Suppose an expert predicts that treatment x will have an enrollment rate that is Y percentage points higher than the reference category (husband gets ticket, *NoInfo*). This prediction counts as one within 5 percentage points of the true enrollment rate if abs $\left(Y - \hat{\beta}_x\right) \leq 5$, where $\hat{\beta}_x$ is the estimated effect of x relative to the omitted category. ρ denotes Pearson's correlation coefficient between the two variables.

Table A2: No Detectable Enrollment Effect of *Full Info* Versus *Ticket Info*

	Correct Answer About...					
	Pay	Pay Cut	Start Time	End Time	Job Desirability	Enrolled
	(1)	(2)	(3)	(4)	(5)	(6)
Full Info	0.32***	0.45***	0.29***	0.26***	0.38*	-0.02
	(0.08)	(0.08)	(0.08)	(0.07)	(0.21)	(0.06)
Observations	143	143	128	128	78	171
Ticket Info Mean	0.51	0.36	0.52	0.61	0.85	0.18
p(*Full Info* = *Ticket Info*)	<0.01	<0.01	<0.01	<0.01	0.08	0.68
Strata	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. All columns include only those assigned to the *Info* treatment. Columns (1) to (5) include only the non-ticketed spouses, i.e. those affected by the two sub-treatments within *Info*. The unit of observation in these columns is the individual. For column (6), the unit of observation is the couple. The outcomes in columns (1) to (5) come from the endline survey. Columns (1) to (4) are dummy variables equal to one if the respondent knew the initial pay for the women's weaving program, the lower pay level from the second month if targets were not met, the start time of the working day, and the end time. Job Desirability equals zero if the respondent considers the job to be completely undesirable, one for somewhat desirable or somewhat undesirable, and two for completely desirable. Enrolled is an indicator for the wife of a couple enrolling on enrollment day. The start and end time knowledge questions were not included for the first center's respondents, while the Job Desirability question was asked only for the final three centers. Strata controls include village fixed effects and an indicator for OBC status. Couple-level controls included throughout are: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, (11) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (12) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each set to the mean when missing. *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Representativeness of Experts Surveyed

	All Experts Contacted	Experts That Predicted
PhD Student	0.03	0.06
Assistant Professor	0.20	0.34
Associate Professor	0.18	0.17
Full Professor	0.32	0.23
Other Position	0.26	0.20
Years Since PhD	14.92	12.09
Has Google Scholar Profile	0.84	0.87
Google Scholar Citations	6104.09	3506.54
Number of Experts	361	70

Notes: The first column shows the mean characteristics for the full set of experts we invited to complete the expert survey. The second column shows the mean characteristics for only those experts that completed the survey and gave predictions. Years Since PhD is calculated only for those with known PhD completion years and for PhD candidates, for which we set Years Since PhD to zero. Years Since PhD is then non-missing for 350 of 361 experts in the first column, and for 69 of 70 experts in the second column. Google Scholar Citations is the total citations as of September 2021, only for those with a Google Scholar profile.

Table A4: Baseline Characteristics and Balance

	Attrit:	Age of		No Education			Years Married			Employed		Women Weaver Preference	
	Baseline	Wife	Husb.	Wife	Husb.	Fertility	10+	5-10	0-5	Wife	Husb.	Wife	Husb.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Info	-0.06	0.52	0.12	0.07	0.07*	0.10	0.03	-0.01	-0.02	0.01	0.02	-0.12	-0.00
	(0.04)	(0.57)	(0.60)	(0.05)	(0.04)	(0.18)	(0.05)	(0.05)	(0.05)	(0.03)	(0.04)	(0.09)	(0.09)
Discuss	-0.03	0.83	0.46	0.03	0.03	0.19	0.00	0.04	-0.04	0.09**	0.03	-0.12	0.24**
	(0.04)	(0.58)	(0.62)	(0.05)	(0.04)	(0.17)	(0.06)	(0.05)	(0.05)	(0.04)	(0.04)	(0.09)	(0.10)
Wife Ticket	0.02	0.28	0.53	0.03	-0.04	-0.07	-0.04	-0.01	0.04	0.05*	0.01	0.07	-0.04
	(0.03)	(0.48)	(0.51)	(0.04)	(0.03)	(0.14)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.08)	(0.08)
Observations	732	490	493	490	493	490	485	485	485	490	493	490	493
Joint p-value	0.47	0.53	0.72	0.54	0.17	0.71	0.77	0.85	0.60	0.05	0.86	0.40	0.04
Outcome Mean	.32	26	29	.39	.13	2.3	.4	.32	.28	.13	.82	1.1	.81
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. Strata controls include village fixed effects and an indicator for OBC status. Outcome variables are: (1) indicator for attrited (at least one spouse did not complete the Baseline), (2) age of wife, (3) age of husband, (4) indicator for wife has no education, (5) indicator for husband has no education, (6) number of children, (7) indicator for married ten or more years, (8) indicator for married five to ten years, (9) indicator for married zero to five years, (10) indicator for wife has worked for income in the last three months, (11) indicator for husband has worked for income in the last three months, (12) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (13) husband's answer to how appropriate for woman in household to work as weaver (zero to two). *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Wife's Past Employment Does Not Predict Enrollment

	Enrolled			
	(1)	(2)	(3)	(4)
Wife Employed	-0.005	0.012	-0.009	0.004
	(0.047)	(0.047)	(0.044)	(0.045)
Observations	495	495	495	495
Outcome Mean	0.15	0.15	0.15	0.15
Strata	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Treatments	No	No	Yes	Yes

Notes: Robust standard errors in parentheses. Enrolled is an indicator for enrolling on enrollment day. Wife Employed is an indicator for whether the wife has worked for income in the last three months. All columns include an indicator for Wife Employed being missing (five observations). Strata controls include village fixed effects and an indicator for OBC status. Controls are: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for husband has worked for income in the last three months, (10) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (11) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each set to the mean when missing. Treatments are indicators for *Info*, *Discuss*, and Wife Gets Ticket. *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Baseline Characteristics and Balance in Endline Sample

	Attrit:		Age of		No Education			Years Married			Employed		Women Weaver Preference	
	Endl., Wife	Endl., Husb.	Wife	Husb.	Wife	Husb.	Fertility	10+	5-10	0-5	Wife	Husb.	Wife	Husb.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Info	-0.05	0.06	0.30	0.02	0.03	0.07*	0.07	0.01	-0.02	0.00	0.02	0.01	-0.14	-0.04
	(0.04)	(0.04)	(0.59)	(0.63)	(0.06)	(0.04)	(0.19)	(0.06)	(0.06)	(0.05)	(0.04)	(0.05)	(0.10)	(0.10)
Discuss	0.05	0.02	0.96	0.39	-0.02	0.03	0.18	-0.02	0.06	-0.03	0.10**	0.06	-0.06	0.23**
	(0.04)	(0.04)	(0.64)	(0.68)	(0.06)	(0.04)	(0.19)	(0.06)	(0.06)	(0.06)	(0.04)	(0.05)	(0.10)	(0.11)
Wife Ticket	0.01	0.01	0.17	0.24	0.04	-0.05	-0.07	-0.03	-0.01	0.05	0.07**	-0.00	0.10	-0.05
	(0.03)	(0.03)	(0.51)	(0.55)	(0.05)	(0.03)	(0.16)	(0.05)	(0.05)	(0.05)	(0.03)	(0.04)	(0.08)	(0.08)
Observations	495	495	411	411	411	411	411	409	409	409	411	411	411	411
Joint p-value	0.14	0.42	0.52	0.93	0.74	0.17	0.79	0.86	0.59	0.62	0.05	0.59	0.35	0.05
Outcome Mean	.17	.16	26	29	.4	.13	2.3	.4	.32	.28	.12	.82	1.2	.83
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. Outcome variables are: (1) indicator for wife attrited (completed Baseline but not Endline), (2) indicator for husband attrited (completed Baseline but not Endline), (3) age of wife, (4) age of husband, (5) indicator for wife has no education, (6) indicator for husband has no education, (7) number of children, (8) indicator for married ten or more years, (9) indicator for married five to ten years, (10) indicator for married zero to five years, (11) indicator for wife has worked for income in the last three months, (12) indicator for husband has worked for income in the last three months, (13) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (14) husband's answer to how appropriate for woman in household to work as weaver (zero to two). *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Discussion Effect by Village

	Enrolled					
	(1)	(2)	(3)	(4)	(5)	(6)
Discuss	-0.19	-0.15	0.01	-0.06	-0.12**	-0.13
	(0.15)	(0.19)	(0.04)	(0.09)	(0.06)	(0.09)
Observations	64	52	90	88	110	91
Omitted Group Mean	0.48	0.33	0.00	0.11	0.14	0.21
Strata	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Info	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. Enrolled is an indicator for enrolling on enrollment day. Each column includes only couples from one of the six villages. Strata controls include village fixed effects and an indicator for OBC status. Controls are: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, (11) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (12) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each set to the mean when missing. *** p<0.01, ** p<0.05, * p<0.1.

Table A8: No Heterogeneity by Individual Preferences

	Enrolled	
	(1)	(2)
Info	-0.06	-0.06
	(0.05)	(0.05)
Discuss	-0.11***	-0.13***
	(0.04)	(0.04)
Info × Wife's Weaving Preference (0 to 2)	0.05	0.06
	(0.04)	(0.04)
Discuss × Wife's Weaving Preference (0 to 2)	0.03	0.05
	(0.04)	(0.04)
Info × Husband's Weaving Preference (0 to 2)	-0.03	-0.05
	(0.05)	(0.05)
Discuss × Husband's Weaving Preference (0 to 2)	-0.03	-0.04
	(0.05)	(0.05)
Observations	489	489
p(*Info* × Wife's Weaving Pref. = *Discuss* × Wife's Weaving Pref.)	.66	.77
p(*Info* × Husband's Weaving Pref. = *Discuss* × Husband's Weaving Pref.)	.87	.7
Strata	Yes	Yes
Wife's and Husband's Weaving Preference Level Variables	Yes	Yes
Controls	No	Yes

Notes: Robust standard errors in parentheses. Enrolled is an indicator for enrolling on enrollment day. Wife's (Husband's) Weaving Preference is the wife's (husband's) answer to how appropriate it is for a woman in her household to work as a weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate). Strata controls include village fixed effects and an indicator for OBC status. Controls are: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, with each set to the mean when missing. *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Who Had Most Influence over the Enrollment Decision?

	Me	Equal Influence	My Spouse
	(1)	(2)	(3)
Info	-0.02	0.04	-0.02
	(0.06)	(0.05)	(0.06)
Discuss	-0.05	0.05	0.00
	(0.06)	(0.06)	(0.06)
Observations	407	407	407
Omitted Group Mean	.33	.31	.36
Strata	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. The three outcomes come from the endline question: *Out of you and your spouse, who would you say had the most influence over the decision of whether to enroll or not?* Me is an indicator for whether the respondent said *Me*, Equal Influence is an indicator for whether the respondent said *Me and my spouse had same influence*, and My Spouse is an indicator for whether the respondent said *My spouse*. The sample includes only ticketed spouses, as non-ticketed spouses in *NoInfo* that did not know about the ticket (a selected sample) were not asked the question. Strata controls include village fixed effects and an indicator for OBC status. Controls are: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, (11) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (12) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each set to the mean when missing. *** p<0.01, ** p<0.05, * p<0.1.

Table A10: Effects on Perceived Job Desirability

	Job Desirability	
	(1)	(2)
Info	0.19**	0.20**
	(0.10)	(0.09)
Discuss	0.12	0.09
	(0.10)	(0.09)
Observation Unit	Individual	Individual
Observations	478	478
Outcome Mean	1.09	1.09
Strata	Yes	Yes
Controls	No	Yes

Notes: Couple-clustered standard errors in parentheses. Job Desirability equals zero if the respondent considers the job to be completely undesirable, one for somewhat desirable or somewhat undesirable, and two for completely desirable. This question was asked at endline, but only for the final three centers. Strata controls are village fixed effects and an indicator for OBC status. Controls include a dummy for female, and the following couple-level controls: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, (11) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (12) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each set to the mean when missing. *** p<0.01, ** p<0.05, * p<0.1.

Table A11: Effects on Spousal Disagreement and Inconsiderateness

	Spousal Disagreement		Spousal Inconsiderateness	
	(1)	(2)	(3)	(4)
			(a) Wives	
Info	-0.06	-0.08	-0.29**	-0.31**
	(0.12)	(0.11)	(0.13)	(0.13)
Discuss	-0.06	-0.05	-0.28**	-0.28**
	(0.12)	(0.11)	(0.13)	(0.13)
Observations	240	240	217	217
Omitted Group Mean	1.3	1.3	1	1
p(*Discuss* = *Info*)	.95	.8	.92	.82
			(b) Husbands	
Info	0.01	0.01	-0.02	-0.06
	(0.13)	(0.13)	(0.10)	(0.10)
Discuss	0.05	0.15	-0.16*	-0.12
	(0.13)	(0.13)	(0.09)	(0.09)
Observations	239	239	223	223
Omitted Group Mean	1.1	1.1	.39	.39
p(*Discuss* = *Info*)	.75	.25	.14	.47
Strata	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Obs. Unit	Individual	Individual	Individual	Individual

Notes: Robust standard errors in parentheses. Spousal Disagreement is the level of reported disagreement between spouses about the job opportunity, measured on a 0-2 scale. Spousal Inconsiderateness is the extent to which one's spouse was inconsiderate of one's own opinion about the job opportunity, measured on a 0-2 scale. The two questions were asked only for the final three centers. Panel (a) uses only the reports of wives, and Panel (b) uses only the reports of husbands. Strata controls include village fixed effects and an indicator for OBC status. Controls are: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, (11) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (12) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each set to the mean when missing. *** p<0.01, ** p<0.05, * p<0.1.

Table A12: Exploring Deeper Deliberation Explanation for Discussion Effect

	Number Of Discussions		Enrolled	
	(1)	(2)	(3)	(4)
Info	0.02	0.09		
	(0.35)	(0.35)		
Discuss	1.00**	0.99**		
	(0.41)	(0.43)		
Days Before Enrollment			0.02*	0.02**
			(0.01)	(0.01)
Observations	829	829	330	330
Omitted Group Mean	3	3		
Outcome Mean			.17	.17
p(*Discuss* = *Info*)	.005	.015		
Strata	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Wife Indicator	Yes	Yes	No	No
Sample	All	All	Exclude *Discuss*	

Notes: Standard errors in parentheses clustered at couple-level for columns (1) and (2), otherwise robust standard errors. Regression is at the individual-level for columns (1) and (2), otherwise couple-level. Number Of Discussions is the endline-reported number of discussions spouses had about the job opportunity. Days Before Enrollment is the number of days in between ticket receipt and the enrollment day. Enrolled is an indicator for enrolling on enrollment day. Strata controls include village fixed effects and an indicator for OBC status. Controls are: (1) age of wife, (2) age of husband, (3) indicator for wife has no education, (4) indicator for husband has no education, (5) number of children, (6) indicator for married ten or more years, (7) indicator for married five to ten years, (8) indicator for married zero to five years, (9) indicator for wife has worked for income in the last three months, (10) indicator for husband has worked for income in the last three months, (11) wife's answer to how appropriate for woman in household to work as weaver (0 = inappropriate, 1 = somewhat appropriate, 2 = completely appropriate), (12) husband's answer to how appropriate for woman in household to work as weaver (zero to two), with each set to the mean when missing. *** p<0.01, ** p<0.05, * p<0.1.

Table A13: The Predictions of Experienced Researchers Are Less Accurate

	Confidence (0 to 7)			Mean Prediction Accuracy		
	(1)	(2)	(3)	(4)	(5)	(6)
Associate Professor	.625			-.84		
	(.636)			(1.85)		
Full Professor	-.458			-3.94**		
	(.445)			(1.77)		
Other Position	.0694			-6.44*		
	(.542)			(3.29)		
Has Google Scholar Profile		-.712	-.606		-8.97	-11.1
		(1.55)	(1.61)		(8.46)	(8.15)
Google Scholar Citation Ranking (1 to 61)		.00014	-.00103		.0962*	.124**
		(.0111)	(.0117)		(.0527)	(.0612)
Above-Median Time on Survey			.143			-3.47
			(.414)			(2.63)
Confidence						.651
						(.514)
Observations			70			
Outcome Mean		2.73			-14.62	

Notes: Robust standard errors in parentheses. Confidence is the number of predictions (out of 7) that the expert expected to be within 5 percentage points of the actual enrollment rate. Mean Prediction Accuracy is the negative of the mean absolute forecast error across the 7 predictions. The omitted academic rank category is Assistant Professor, while Other Position includes, among others, PhD students and postdoctoral fellows. Has Google Scholar Profile is a dummy variable equal to one if the expert has a Google Scholar profile. Google Scholar Citation Ranking is the rank among the 61 experts with a Scholar profile of total Google Scholar citations as of September 2021 (lower means more citations). Above-Median Time on Survey is a dummy variable equal to one if the expert spent above-median time completing the survey (a proxy for effort). *** p<0.01, ** p<0.05, * p<0.1.

B Theory

B.1 Bargaining Frictions Model

We first consider a simple model of household decision-making with bargaining frictions which maps to our experimental treatments. The model yields two intuitive predictions: enforcing common knowledge and enforcing bargaining both increase enrollment. We outline the alternative model with veto rights in Section 6.2.

Agents and Utility. A household consists of a husband, h, and a wife, w. If the wife does not take the job opportunity with Obeetee, the husband and wife receive utility U_h and U_w. If instead she does take it, the husband and wife receive utilities $U_h' = U_h + v_h$ and $U_w' = U_w + v_w$. The net utility gains to the husband and wife from enrolling, v_h and v_w, can be considered reduced form outcomes from a more complete model with any combination of the following features associated with the wife working:[34] (i) the wife earns extra income and this income is shared as per a consumption sharing rule, (ii) the wife has some disutility of effort, (iii) the husband and wife face a stigma cost from breaking social norms, (iv) bargaining power shifts in the wife's favor, increasing her consumption share.[35] While the features that underlie the net utility gains are not important for our purposes, what is important is that v_h and v_w are known with certainty by both the husband and the wife.

We assume a simple form of heterogeneity for the net utility parameters. They are independently and uniformly distributed: $v_h \sim U\left[\mu_h - \frac{\varphi}{2}, \mu_h + \frac{\varphi}{2}\right], v_w \sim U\left[\mu_w - \frac{\varphi}{2}, \mu_w + \frac{\varphi}{2}\right]$, with the same variance, but different means, such that $\mu_w > \mu_h$.[36] These assumptions match key features of our data on preferences: wives are on average more positive about women weaving than husbands, but there exist couples in which the husband is more positive than the wife. Building on this, we also assume that pro-job and anti-job types exist among both wives and husbands, i.e. $\mu_w - \frac{\varphi}{2} < 0 < \mu_h + \frac{\varphi}{2}$.

Household Decision-Making. We now consider three types of decision-making, from the most to the least efficient, and interpret our experimental treatments as shifting households between these different types.

[34] We detail a collective model with these features in Appendix B.3, showing how the net utility parameters can be micro-founded using standard assumptions.

[35] For example, Atkin (2009) finds that Mexican women induced to work in manufacturing jobs have taller children and report stronger bargaining power, while Jensen (2012) and Heath and Mobarak (2015) find that labor market opportunities for women lead to delayed marriage and childbirth in Indian and Bangladesh respectively. However, the model we describe in Appendix B.3 includes features (i) to (iii), but does not include feature (iv).

[36] As we show in Appendix B.3, the assumption of uniformly distributed net utility parameters is an implication of a collective model with uniformly distributed, additive, costs to each spouse of the wife working. These costs can be understood as stigma costs as in Field et al. (2021), but provided that they are additive, the particular interpretation is not important.

Collective Model. Households may decide as in the collective model (Chiappori 1992; Browning and Chiappori 1998) – they bargain efficiently, and the wife enrolls whenever $\beta U_h' + (1-\beta) U_w' > \beta U_h + (1-\beta) U_w$, which is whenever

$$\beta v_h + (1-\beta) v_w > 0 \tag{1}$$

where β is the bargaining weight of the husband. We assume that $\beta > \frac{1}{2}$ given our specific evidence on the predictiveness of husbands' preferences of enrollment, as well as wide-ranging evidence in general for the high bargaining power of men in India. To simplify subsequent derivations, we additionally assume that:

$$\begin{aligned} \beta \left(\mu_h - \frac{\varphi}{2}\right) + (1-\beta) \left(\mu_w + \frac{\varphi}{2}\right) < 0 \\ \beta \left(\mu_h + \frac{\varphi}{2}\right) + (1-\beta) \left(\mu_w - \frac{\varphi}{2}\right) > 0 \end{aligned} \tag{2}$$

which is to say that the most anti-job husband would never reach a collective decision to enroll, while wives of the most pro-job husband would always enroll. This is a natural assumption, given that $\beta > \frac{1}{2}$, but not a necessary one for the two empirical predictions we will emphasize.[37]

Bargaining Costs. Alternatively, households may fail to bargain efficiently if there is some cost to starting the negotiation process. Here we assume that there is a cost $c > 0$ of bargaining efficiently whenever there is disagreement, with $v_h v_w < 0$. Intuitively, this cost reflects the difficulty in striking up an uncomfortable discussion. After either spouse pays this cost, a collective decision, as above, will follow. Instead, when $v_h v_w \geq 0$,[38] the cost of bargaining is zero. In this case, the husband and wife want the same thing, making a conversation about the job a simple one.

If neither spouse pays the cost, we assume that the couple reverts to non-cooperative decision-making. In our case, spouses could not unilaterally enroll – both had to go to the weaving center on enrollment day if the wife was to enroll. Instead, with non-cooperative decision-making, either spouse can veto enrollment – by refusing to attend on enrollment day, for example. It follows that whenever either of v_h or v_w is negative, and neither spouse pays the bargaining cost, the woman will not enroll.

Summarizing, with bargaining costs, couples will enroll without needing to pay the cost whenever both spouses have positive net utility gains from enrollment. Couples will enroll after paying the cost whenever $v_w > c$ or $v_h > c$, and equation 1 is satisfied. Finally, we make the assumption that $-\frac{1-\beta}{\beta}\left(\mu_w - \frac{\varphi}{2}\right) \leq c < \mu_h + \frac{\varphi}{2}$. This ensures that the cost is low enough that at least some husbands

[37] These assumptions restrict the number of cases (i.e. parameter regions) we have to solve for, such that the expressions for enrollment probabilities do not depend on parameter regions.

[38] Recall the assumption that each spouse knows the net utility gain of the other with certainty.

and some wives pay it, and high enough to simplify subsequent derivations, without affecting the key empirical predictions.

Incomplete Information. If only one spouse is aware of the job opportunity, the household decision operates in two stages. In the first stage, the knowledgeable spouse decides whether to pass on the information to the other spouse or to withhold it. We assume that there is no direct cost of passing on the information. If the information is not passed on, there is no enrollment, because in our setting enrollment requires both spouses to go to the weaving center on enrollment day. If the information is passed on, we reach the second stage, in which spouses decide whether to pay the bargaining cost as above. If one spouse pays the cost, the couple bargain efficiently, as in the first decision-making type. If neither pay the cost, the couple bargains non-cooperatively.

Spouse i will withhold the information whenever (i) enrollment is net costly to them ($v_i < 0$), and (ii) enrollment would happen if information were revealed ($v_h, v_w > 0$, or $\beta v_h + (1-\beta) v_w > 0$ and $\max\{v_h, v_w\} > c$). Combining these conditions, husbands and wives respectively withhold job information when

$$\begin{aligned} -\frac{1-\beta}{\beta} v_w < v_h < 0, v_w > c \\ -\frac{\beta}{1-\beta} v_h < v_w < 0, v_h > c \end{aligned} \tag{3}$$

It follows that, ceteris paribus, a spouse is more likely to strategically withhold information when they have low bargaining power – this is precisely when the second stage decision is more likely to go against their interests.

Intuitively, this stylized model aims to capture the idea of an opportunity arising for one spouse that is considered in the household's best interest but requires the other spouse to make a sacrifice. In our context, the opportunity is a job for the wife, and a possible sacrifice would be the cost for the husband from breaking a local social norm. More familiar to academics, one might instead imagine a promotion for one spouse that would require relocating to a new city. The other spouse may prefer to stay, but the household nevertheless reaches a joint decision to move. In each of these cases, a spouse has an incentive to withhold information.

Enrollment Rates. We map our main treatments to the decision-making types above, and derive enrollment rates implied by the model.[39] First we consider the condition closest to the status quo: the husband is approached and given the job information, and can withhold that information from his wife

[39] Proofs in Appendix B.4.

with plausible deniability ($Husband - NoInfo$ in Figure 3). In this case, the enrollment rate is

$$E_h^{\text{NoInfo}} = \frac{\varphi\left(\mu_h + \frac{\varphi}{2}\right) + c\left(\mu_w - \frac{\varphi}{2}\right)}{\varphi^2}$$

The enrollment region is shown graphically as the shaded area in panel (a) of Figure A8. Intuitively, wives only enroll when the husband benefits on net from enrollment, and when the husband is willing to pay the bargaining cost for the cases where the wife would lose out from enrolling. Enrollment is increasing locally in both the average net utility gain for wives and husbands, and falling in the bargaining cost.

Next consider the parallel case. The wife is approached and given the job information, and can withhold the information ($Wife - NoInfo$ in Figure 3):

$$E_w^{\text{NoInfo}} = \frac{\left(\mu_w + \frac{\varphi}{2}\right)\left(2\beta\left(\mu_h + \frac{\varphi}{2}\right) + (1-\beta)\left(\mu_w + \frac{\varphi}{2}\right)\right) - (1-\beta)c^2}{2\beta\varphi^2}$$

The enrollment region is the shaded area in panel (b) of Figure A8. Now enrollment only occurs when the wife benefits on net, and is willing to pay the bargaining cost for the cases where the husband would lose out.

As we discuss below, our experts predicted that enrollment would be 3.2 percentage points higher when the wife receives the job information, with plausible deniability, than in the status quo. While this seems intuitive, the sign of $E_w^{\text{NoInfo}} - E_h^{\text{NoInfo}}$ is ambiguous in the model, due to two offsetting forces. The more anti-job preferences of husbands push husbands toward withholding information more often than wives. In contrast, their high bargaining power ($\beta > \frac{1}{2}$) pushes them to share information more often, since their preferences matter more for any subsequent collective decision.[40]

Enforcing Common Knowledge. In the experiment, we shut down the possibility of strategic withholding by removing the plausible deniability of ticket-receiving spouses. Through the lens of the model, this increases enrollment, which is now reflected by the shaded area in panel (c) of Figure A8 ($Info$ treatment in Figure 3, with no modelling here of the gender of ticketed spouse treatment). Now couples have full information, and enrollment occurs whenever both spouses benefit ($v_h, v_w > 0$) or the couple collectively benefits ($\beta v_h + (1-\beta) v_w > 0$) and one spouse is willing to pay the bargaining cost. The impact on

[40] A third unmodelled force would also push in the same direction: if the bargaining cost c is lower for husbands than wives they would have an additional incentive for sharing information. Given the imbalance in bargaining power, this extra feature is perhaps plausible – it may be less costly for husbands to start a conversation about their spouse making a sacrifice for the household than vice versa.

enrollment relative to when the wife gets the ticket is

$$\theta_w = \frac{\left(\mu_h + \frac{\varphi}{2} - c\right)\left(\frac{\varphi}{2} - \mu_w\right)}{\varphi^2} > 0$$

which is increasing in μ_h and decreasing in μ_w. In this sense, enforcing common knowledge after the wife gets the job ticket will not matter that much if women tend to be very pro-job and men tend to be very anti-job. This is because women rarely have the incentive to strategically withhold information. The impact on enrollment relative to when the husband gets the ticket is

$$\theta_h = \frac{1-\beta}{\beta}\frac{\left(\mu_w + \frac{\varphi}{2} - c\right)\left(c + \frac{1}{2}\left(\mu_w + \frac{\varphi}{2} - c\right)\right)}{\varphi^2} > 0$$

which is increasing in μ_w and falling in β. The more bargaining power husbands have, the less they need to strategically withhold information to prevent household decision-making going against their interests. As explained above, the sign of $\theta_w - \theta_h$ is ambiguous given the two offsetting forces of job preferences and bargaining power. As a result, the model predicts that enforcing common knowledge will increase enrollment, but not necessarily that this effect will be larger when forcing husbands to share information as opposed to wives.

Enforcing Bargaining. In the experiment, we try to kickstart bargaining by having couples receive the information together and giving them several minutes to discuss it (*Discuss* treatment in Figure 3). Through the lens of the model, we consider this treatment as one that eliminates the bargaining cost, by giving individuals an excuse to bring up the job, for example. This would shift couples to making decisions as in the collective model (moving from panel (c) to panel (d) of Figure A8) and again increase enrollment, with couples now enrolling whenever $\beta v_h + (1-\beta) v_w > 0$. The increase in enrollment is

$$\theta^{\text{Discuss}} = \frac{\frac{1-\beta}{2\beta}\left(c^2 - \left(\frac{\varphi}{2} - \mu_w\right)^2\right) + c\left(\frac{\varphi}{2} - \mu_w\right)}{\varphi^2} > 0$$

The discussion effect is driven entirely by the couples that disagree – i.e. those with $v_h v_w < 0$. We test for heterogeneity by agreement below.

B.2 Veto Power Model

In the veto power model, a couple enrolls only if neither spouse vetoes it, but spouses incur a private utility cost from exercising a veto. Our treatments shift the costs by changing who feels entitled to

veto; ticketed spouses always feel entitled to veto, while *Info* and *Discuss* make non-ticketed spouses also feel comfortable exercising a veto. Suppose for simplicity that being assigned the ticket or being in *Info* or *Discuss* eliminates the cost entirely. Then in the *Info* and *Discuss* groups, enrollment only occurs when *both* spouses prefer enrollment over non-enrollment ($v_h, v_w > 0$, panel (c), Figure A9). In *NoInfo* on the other hand, the couple enrolls whenever the ticketed spouse supports enrollment ($v_i > 0$, for $i = h$ or w) and the non-ticketed spouse is not too opposed to it ($v_j > -c_j$, where $c_j > 0$ is the cost of vetoing and $j = w$ or h) (panels (a) and (b), Figure A9). c_h and c_w need not be equal, and gender norms in our setting might suggest that $c_w > c_h$ (i.e. women incur a greater private utility cost from vetoing their husbands' preferred outcome than husbands do from vetoing their wives' preferred outcome). We draw the enrollment regions in Figure A9 with $c_w > c_h$. This, coupled with the fact that wives are more supportive of women weaving than husbands, means the veto power model has no prediction for whether enrollment will be higher when the husband or the wife receives the ticket in $NoInfo$.

B.3 Micro-founding the net utility parameters

Adapting Field et al. (2021), we can write the maximization problem of a collective household as:

$$\begin{aligned}\max_{(h_i,c_i)_{i\in\{h,w\}}} \quad & \beta\left[u_h\left(1-h_h,c_h\right)-\gamma_h 1\left(h_h>0\right)\right]+\\ & (1-\beta)\left[u_w\left(1-h_w,c_w\right)-\gamma_w 1\left(h_w>0\right)\right]\\ & \text{subject to } c_w+c_h\leq \Sigma_{i=w,h}\left[y_i+w_i h_i\right]\\ & 0\leq h_i\leq 1 \text{ for } i\in\{h,w\}\end{aligned}$$

To solve for the net utility parameters v_h and v_w, we solve this maximization problem twice: once for the case in which the wife doesn't work ($h_w = 0$) and again for the case in which she does ($h_w > 0$). The optimum in the former case yields utility U_h for the husband and U_w for the wife, while in the latter case the resulting utility is U_h' and U_w'. The difference in utility from having the spouse work is then $U_h' - U_h = v_h$ for the husband and $U_w' - U_w = v_w$ for the wife. To make this concrete, we now solve for U_i and U_i' for the case where $u_i\left(1-h_i,c_i\right) = \ln\left(c_i\right) + \phi\ln\left(1-h_i\right)$.

Case I: Wife Does Not Work. Here, we take the wife's labor supply to be constrained at $h_w = 0$. For

simplicity, call combined unearned income $y = y_h + y_w$. The household's problem now collapses to:

$$\max_{h_h, c_h, c_w} \beta\left(\ln c_h + \phi \ln\left(1 - h_h\right)\right) + \left(1 - \beta\right)\left(\ln c_w\right)$$
$$\text{subject to } c_w + c_h \leq y + w_h h_h$$
$$0 \leq h_h \leq 1$$

The Lagrangian is:

$$\mathscr{L} = \beta \ln\left(c_h\right) + \beta\phi \ln\left(1 - h_h\right) + \left(1 - \beta\right)\ln\left(c_w\right) + \lambda\left(w_h h_h + y - c_h - c_w\right)$$

The first order conditions are:

$$\frac{\beta}{\lambda} = c_h,\ \frac{1-\beta}{\lambda} = c_w,\ \frac{\beta\phi}{\lambda w_h} = 1 - h_h$$

Using the budget constraint to solve for optimal consumption and the husband's labor supply, we get:

$$c_w^* = \frac{1-\beta}{1+\beta\phi}\left(w_h + y\right),\ c_h^* = \frac{\beta}{1+\beta\phi}\left(w_h + y\right),\ h_h^* = \frac{w_h - y\beta\phi}{w_h\left(1+\beta\phi\right)}$$

Given that $w_h, y, \beta, \phi > 0$, labor supply is at an interior solution provided that unearned income is sufficiently small relative to wages ($y\beta\phi \leq w_h$). This is clearly the relevant parameter region to consider given that 82% of husbands in our experimental sample had worked for income in the three months prior to the baseline survey. Equilibrium utils are then:

$$U_h = \left(1+\phi\right)\ln\left(\frac{\beta}{1+\beta\phi}\left(w^h + y\right)\right) + \phi\ln\left(\frac{\phi}{w^h}\right),\ U_w = \ln\left(\frac{1-\beta}{1+\beta\phi}\left(w^h + y\right)\right)$$

Case II: Wife Works. Assuming that the wife works, the household's problem becomes:

$$\max_{(h_i, c_i)_{i\in\{h,w\}}} \beta\left[\ln c_h + \phi\ln\left(1 - h_h\right) - \gamma_h\right] +$$
$$\left(1-\beta\right)\left[\ln c_w + \phi\ln\left(1 - h_w\right) - \gamma_w\right]$$
$$\text{subject to } c_w + c_h \leq y + \Sigma_{i=w,h} w_i h_i$$
$$0 \leq h_i \leq 1 \text{ for } i \in \{h, w\}$$

Since the γs do not affect the optimization problem, the Lagrangian becomes:

$$\mathscr{L} = \beta\ln\left(c_h\right) + \beta\phi\ln\left(1 - h_h\right) + \left(1-\beta\right)\ln\left(c_w\right) + \left(1-\beta\right)\phi\ln\left(1 - h_w\right) + \lambda\left(w_h h_h + w_w h_w + y - c_h - c_w\right)$$

The first order conditions are now:

$$\frac{\beta}{\lambda} = c_h, \ \frac{1-\beta}{\lambda} = c_w$$

$$\frac{\beta\phi}{\lambda w_h} = 1 - h_h, \ \frac{(1-\beta)\phi}{\lambda w_w} = 1 - h_w$$

Using the budget constraint, and with several steps of rearranging, we can solve for optimal labor supply as:

$$h'_h = \frac{1+\phi(1-\beta)}{1+\phi} - \frac{\phi\beta}{1+\phi}\frac{w_w+y}{w_h}$$

$$h'_w = \frac{1+\phi+\beta(1-\beta)\phi^2}{(1+\phi)(1+\phi(1-\beta))} - \frac{\phi(1-\beta)}{1+\phi}\frac{w_h+y}{w_w}$$

We again assume that parameters are such that these expressions satisfy the labor supply constraints that $0 \leq h_i \leq 1$.[41] Substituting in to the budget constraint and simplifying, we can solve for total earned income as:

$$y'_e = w_h h'_h + w_w h'_w = \frac{w_h + w_w - \phi y}{1+\phi}$$

It follows that $c'_h = \beta y'_e$ and $c'_w = (1-\beta) y'_e$. With optimal consumption and labor supply in hand, we solve for equilibrium utility:

$$U'_h = (1+\phi)\ln\left(\frac{\beta}{1+\phi}(w_h + w_w + y)\right) + \phi\ln\left(\frac{\phi}{w_h}\right) - \gamma_h$$

$$U'_w = (1+\phi)\ln\left(\left(\frac{1-\beta}{1+\phi}\right)(w_h + w_w + y)\right) + \phi\ln\left(\frac{\phi}{w_w}\right) - \gamma_w$$

The Collective Decision Of Whether To Work. The household collectively decides that the wife should work whenever:

$$\beta U'_h + (1-\beta) U'_w > \beta U_h + (1-\beta) U_w$$

$$\Rightarrow \beta\left(U'_h - U_h\right) + (1-\beta)\left(U'_w - U_w\right) > 0$$

$$\Rightarrow \beta v_h + (1-\beta) v_w > 0$$

[41] Specifically, $w_h, y, \beta, \phi > 0$ and $\beta < 1$ ensure that $h'_h, h'_w < 1$, while for $h'_h > 0$ we require that $\frac{w_w+y}{w_h} < \frac{1+\phi(1-\beta)}{\phi\beta}$, and for $h'_w > 0$ we require that $\frac{w_h+y}{w_w} < \frac{1+\phi+\beta(1-\beta)\phi^2}{\phi(1-\beta)(1+\phi(1-\beta))}$. These last two conditions are more likely to hold when unearned income is small and gender-specific wages not too different.

Our net utility parameters are:

$$v_h = U_h' - U_h = (1+\phi)\ln\left(\frac{1+\beta\phi}{1+\phi}\frac{(w_h+w_w+y)}{(w_h+y)}\right) - \gamma_h$$

$$v_w = U_w' - U_w = (1+\phi)\ln\left(\left(\frac{1-\beta}{1+\phi}\right)(w_h+w_w+y)\right) - \ln\left(\frac{1-\beta}{1+\beta\phi}(w_h+y)\right) + \phi\ln\left(\frac{\phi}{w_w}\right) - \gamma_w$$

In our model in Section 3.3 we assume that the net utility parameters are independently and uniformly distributed. Given the expressions above, this assumption can be microfounded by assuming (i) no couple-level heterogeneity in gender-specific wages (w_i), bargaining power (β), labor-leisure preferences (ϕ), and unearned income (y), and (ii) independently and uniformly distributed norm costs (γ_i).

B.4 Solving for enrollment rates

The enrollment rate when husbands receive the ticket and can withhold information is the fraction of couples with $v_h, v_w > 0$ plus the fraction of couples with $v_h > c, v_w < 0$:

$$\begin{aligned} P(\text{enroll} \mid \text{husband-ticket,bargaining-cost}) &= \frac{\left(\mu_w + \frac{\varphi}{2}\right)\left(\mu_h + \frac{\varphi}{2}\right)}{\varphi^2} + \frac{\left(\frac{\varphi}{2} - \mu_w\right)\left(\mu_h + \frac{\varphi}{2} - c\right)}{\varphi^2} \\ &= \frac{\varphi\left(\mu_h + \frac{\varphi}{2}\right) + c\left(\mu_w - \frac{\varphi}{2}\right)}{\varphi^2} \end{aligned}$$

The equivalent rate for when wives receive the ticket is the fraction of couples with $v_h, v_w > 0$ plus the fraction of couples with $v_w > c, \beta v_h + (1-\beta) v_w > 0$. Equivalently, the enrollment rate is the fraction of couples with $v_w > 0, \beta v_h + (1-\beta) v_w > 0$ less the fraction of couples with $v_w < c, \beta v_h + (1-\beta) v_w > 0$:

$$\begin{aligned} P(\text{enroll} \mid \text{wife-ticket,bargaining-cost}) &= \frac{\left(\mu_w + \frac{\varphi}{2}\right)\left(\mu_h + \frac{\varphi}{2}\right)}{\varphi^2} + \frac{\frac{1-\beta}{2\beta}\left(\mu_w + \frac{\varphi}{2}\right)^2}{\varphi^2} - \frac{\frac{1-\beta}{2\beta}c^2}{\varphi^2} \\ &= \frac{\left(\mu_w + \frac{\varphi}{2}\right)\left(2\beta\left(\mu_h + \frac{\varphi}{2}\right) + (1-\beta)\left(\mu_w + \frac{\varphi}{2}\right)\right) - (1-\beta)c^2}{2\beta\varphi^2} \end{aligned}$$

The effect of enforcing common knowledge on enrollment when the wife receives the ticket is equal to the fraction of couples with $v_h > c, v_w < 0$. This is clearly $\theta_w = \frac{\left(\mu_h + \frac{\varphi}{2} - c\right)\left(\frac{\varphi}{2} - \mu_w\right)}{\varphi^2}$,and is positive given the assumptions that $\mu_w - \frac{\varphi}{2} < 0$ and $c < \mu_h + \frac{\varphi}{2}$.

The effect of enforcing common knowledge on enrollment when the husband receives the ticket is equal to the fraction of couples with $v_w > c, \beta v_h + (1-\beta) v_w > 0$. This is the sum of the area of a

rectangle and a triangle (see panel (c), Figure A8) divided by φ^2:

$$\begin{aligned}\theta_h &= \frac{\frac{1-\beta}{\beta} c \left(\mu_w + \frac{\varphi}{2} - c\right)}{\varphi^2} + \frac{\frac{1-\beta}{2\beta} \left(\mu_w + \frac{\varphi}{2} - c\right)^2}{\varphi^2} \\ &= \frac{1-\beta}{\beta} \frac{\left(\mu_w + \frac{\varphi}{2} - c\right) \left(c + \frac{1}{2}\left(\mu_w + \frac{\varphi}{2} - c\right)\right)}{\varphi^2}\end{aligned}$$

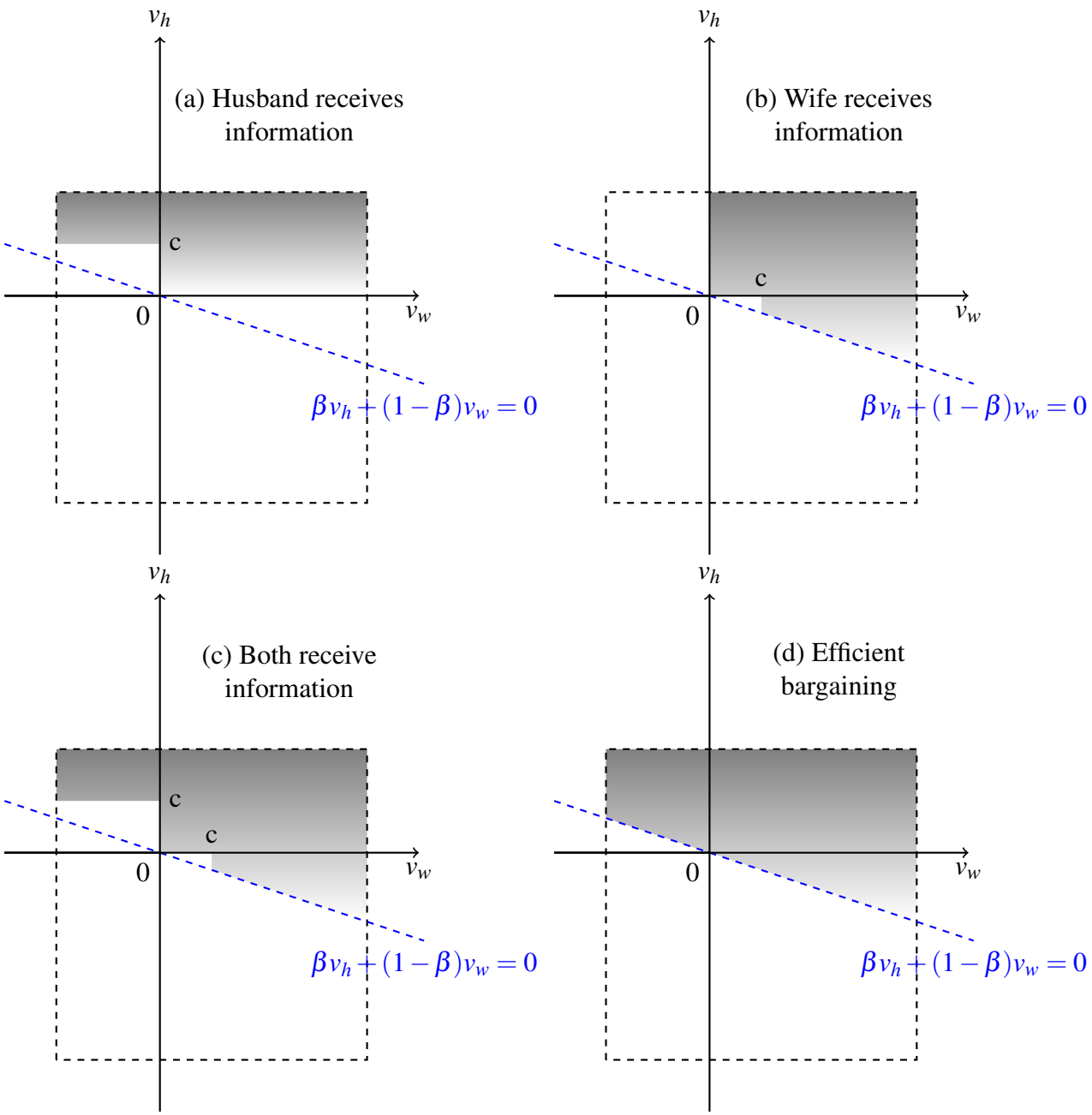
which is positive given that $c < \mu_h + \frac{\varphi}{2} \Rightarrow c < \mu_w + \frac{\varphi}{2}$, since $\mu_w > \mu_h$.

The effect of enforcing bargaining on enrollment is equal to the fraction of couples with $v_w < c, v_h < 0, \beta v_h + (1-\beta) v_w > 0$ plus the fraction of couples with $v_w < 0, v_h < c, \beta v_h + (1-\beta) v_w > 0$:

$$\begin{aligned}\theta^{\text{Discuss}} &= \frac{\frac{1-\beta}{2\beta} c^2 + \frac{1-\beta}{2\beta} \left(\frac{\varphi}{2} - \mu_w\right)^2 + \left(c - \frac{1-\beta}{\beta} \left(\frac{\varphi}{2} - \mu_w\right)\right) \left(\frac{\varphi}{2} - \mu_w\right)}{\varphi^2} \\ &= \frac{\frac{1-\beta}{2\beta} \left(c^2 - \left(\frac{\varphi}{2} - \mu_w\right)^2\right) + c \left(\frac{\varphi}{2} - \mu_w\right)}{\varphi^2}\end{aligned}$$

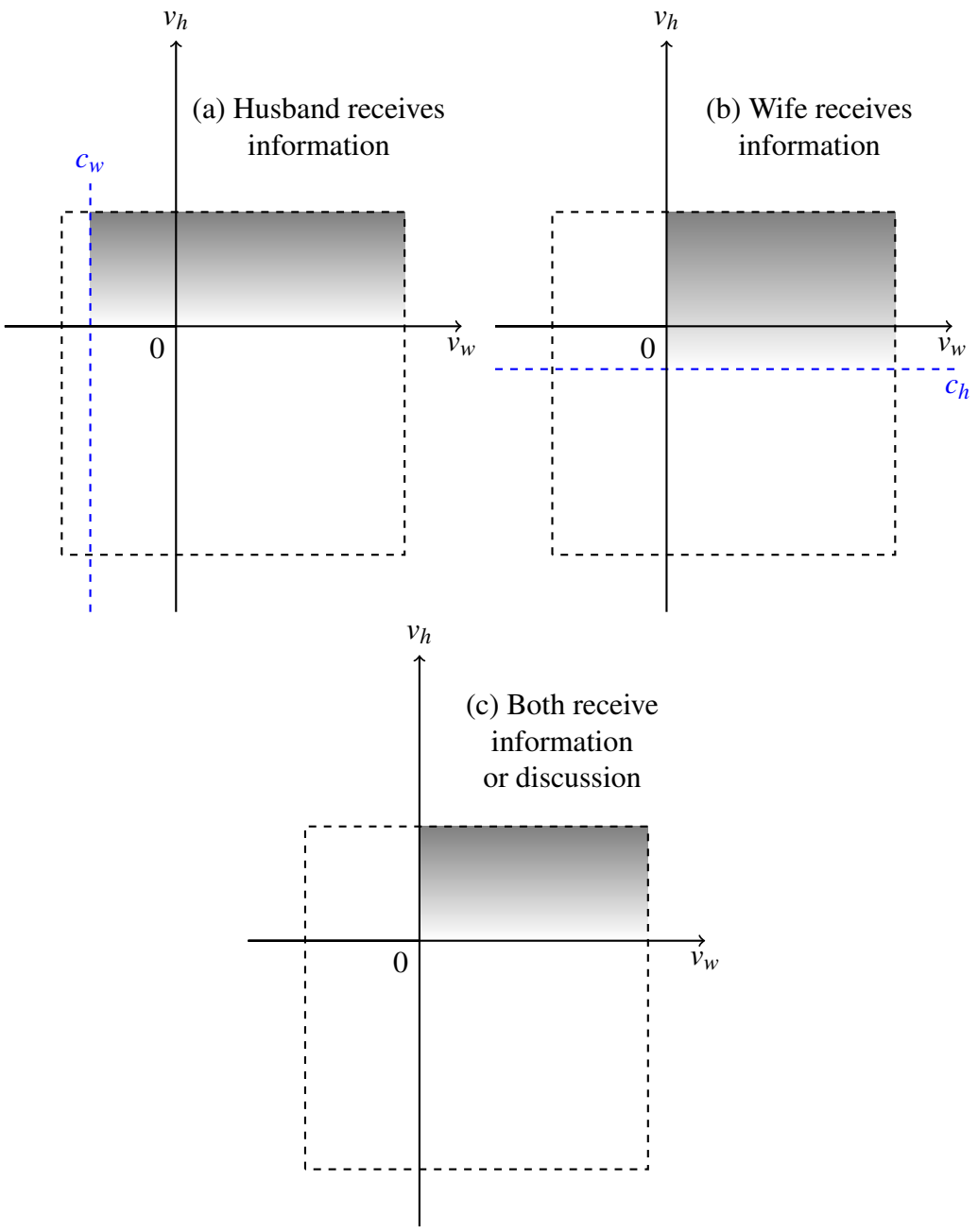
which is positive since $c \geq -\frac{1-\beta}{\beta} \left(\mu_w - \frac{\varphi}{2}\right)$.

Figure A8: Enrollment Regions Under Bargaining Frictions Model



Notes: The figure is drawn for $v_h \sim U[-4,2], v_w \sim U[-2,4], \beta = \frac{3}{4}, c = 1$. The black dashed line box shows the region in which the preferences of husbands and wives are independently and uniformly distributed. The blue dashed line is the line of indifference for collective households. The shaded regions indicate the preference regions in which wives enroll in the job under different assumptions about household decision-making: (a) husband receives information and can withhold, (b) wife receives information and can withhold, (c) both receive information but there remains a cost of bargaining, (d) both receive information and efficient bargaining.

Figure A9: Enrollment Regions Under Veto Power Model



Notes: The figure is drawn for $v_h \sim U[-4,2], v_w \sim U[-2,4], c_h = -0.75, c_w = -1.5$. The black dashed line box shows the region in which the preferences of husbands and wives are independently and uniformly distributed. The blue dashed lines represent the level of utility below which the uninformed spouse will veto enrollment. The shaded regions indicate the preference regions in which wives enroll in the job under different assumptions about household decision-making: (a) husband receives information, (b) wife receives information, and (c) both receive information with or without discussion. Unlike Figure A8, we now assume couples make decisions through vetoes, there is no cost to either spouse from vetoing in (c), there is a cost from vetoing for the uninformed spouse in (a) and (b), and there is full information diffusion.