The Power of Persuasion: Causal Effects of Household Communication on Women's Employment*

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Abstract

In many economic settings, agents lack decision rights but provide input. Household decision-making in conservative, developing settings can be seen through this lens, with husbands retaining decision rights and wives providing input. We study the role of wives' communication skills in one of the most consequential, policy-relevant decisions households make: female labor supply. We randomized whether women in India were given a training in assertive communication, techniques for expressing one's views clearly and respectfully. The treatment increased women's labor supply and earnings but, consistent with theory, only for women who were more interested than their husbands in the women working. These effects persist at least one year post-treatment and represent a 59% increase in earnings over this period.

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1 Introduction

In many economic settings, agents lack decision rights but provide input – these include most sender-receiver models, along with applied contexts such as bosses and subordinates, salespeople and buyers, or financial advisers and clients. Household decision-making in conservative, developing settings can also be seen through this lens. Patriarchal norms and gender inequities (Anderson, 2024; Jayachandran, 2015) may result in household dynamics wherein husbands take decisions and wives' influence is limited to providing input. Indeed, only half of women in our sample in rural India report being one of the household members with final say over various decisions, but virtually all of them (90%) report giving input.

In standard models of such settings, agents providing input communicate optimally; senders in the usual sender-receiver models who have credible signals that would tilt outcomes in their favor always send them, while benchmark models of the household leave out communication frictions entirely, assuming symmetric information. At the other end of the spectrum, popular culture is rife with tips for women on how to persuade their husbands,[1] and a vast literature across psychology, management, and experimental economics discusses the importance of effective communication (Antonakis et al., 2022; Coffman and Niehaus, 2020; Lazarus, 1973; Peneva and Mavrodiev, 2013; Tannen, 1995).

This paper studies the role of communication skills in decisions about female labor supply, one of the most consequential and policy-relevant decisions households make (Heath et al., 2024). There are large gender gaps in labor market outcomes globally, and India, where we work, has some of the widest gaps in the world (Agte et al., 2024). This is concerning in light of evidence that misallocation of female talent constrains economic growth (Ashraf et al., 2022; Chiplunkar and Goldberg, 2021; Chiplunkar and Kleineberg, 2024; Hsieh et al., 2019). Husband opposition is widely discussed as a constraint to female employment in India, as men tend to be less supportive of female employment than women, and husbands have a great deal of control over their wives' labor supply (Bernhardt et al., 2018; Bursztyn et al., 2024; Field et al., 2021; Heath and Tan, 2020; Lowe and McKelway, 2026; McKelway, 2025, 2026). Consistent with this, Fletcher et al. (2018) estimate that over 30% of women in India who are engaged primarily in domestic duties would like to work. Could communication skills allow women to overcome their husbands' opposition, increasing their labor supply?

We conducted a field experiment among women in India, randomizing whether they were given a communication training before a large expansion of new jobs for women. The jobs

[1] Figure A.1 provides just a few examples: an article from Good Housekeeping instructing women on how to turn a "no" from their husbands into a "yes", tips from Woman's Day on persuading others, and articles from the Ladies' Home Journal on the persuasive power of women's eye contact and on a language of intimate persuasion between spouses termed "husbandese."

are with India's largest carpet manufacturer, whom we partner with. They begin with four months of paid training in carpet weaving, followed by weaving employment for any women who complete training and wish to continue. The training pay is substantial, close to what women ultimately earn as weavers. Both training and employment are full-time and occur in all-female weaving centers located in participants' villages. We timed the study to coincide with these job openings, which provides two key benefits. First, it allows us to measure labor market outcomes in the firm's administrative data, both alleviating concerns about self-reports and capturing take-up of a real job for women. Second, this context relaxes the key labor demand-side constraint of job availability, and thus allows us to cleanly establish whether the supply-side, communication friction is binding.[2]

Our sample included 1,540 married women, aged 18-40, who would be eligible for the new jobs. Female labor supply is the topic couples in our sample most often hold different opinions about. This disagreement almost always takes the form of wives being more interested than their husbands in the wives working: 53% of couples in our sample have different levels of interest in the wives working, and in 81% of those couples, the wife is more interested.

The communication training was in *assertive communication*, which means expressing one's view clearly and respectfully. Note that this meaning of assertive, which is used in the communication literature and which we adopt throughout the paper, is different from how assertive is often used colloquially to refer to pushy or off-putting communication. The training was designed by WorldBeing, an organization that provides evidence-based psychosocial programs in developing countries. WorldBeing combined and adapted existing assertive communication techniques to develop a training relevant for women in our setting. The key technique taught was a *see-feel-want* statement (WorldBeing, 2022): the curriculum trained women when facing an interpersonal disagreement to describe how they *see* the situation objectively, the emotions it is making them *feel*, and what they *want* to happen. The training was delivered in six, one-hour sessions with groups of women over a month. Content was conveyed in a variety of formats, including instruction, storytelling, and group activities. Examples in the curriculum focused on communication between husbands and wives, but covered a range of different topics spouses might disagree about and was not focused on employment in particular. To control for the effects of attending sessions unrelated to communication, we use an active control group; the same sort of meetings were held with the control group, but in these meetings, women played games and took surveys.

The firm's program began three weeks after the intervention ended. We observe, in

[2]In particular, if we were not working in the context of job expansions and found a null effect on employment, we could not say whether that was because the communication constraint was not binding, or because jobs were not available for women.

administrative data, whether women applied for the program and their earnings in each of the first 10 months of the program. We also administered surveys with women at baseline, and at five weeks and six months post-treatment. Husbands were also surveyed at six months.

We estimate treatment effects overall and by a pre-specified dimension of heterogeneity: an indicator for women reporting at baseline that they were more interested than their husbands in the women working outside the home. All women may communicate more effectively with their husbands due to the treatment, but only those in the woman-more-interested subgroup would have an incentive to advocate for their labor supply.[3]

We find the overall treatment effect on application for the firm's program is positive but small and not statistically significant; this, however, masks heterogeneity by spousal disagreement. The treatment effect on women who were more interested than their husbands was significantly larger, at 6.0 percentage points (p.p.), a 35% increase beyond the application rate of control women in this subgroup. The effect in the rest of the sample was -2.5 p.p. and not statistically significant.

This translated into differences in actual program earnings, observed in the firm's administrative records. We code non-participants' earnings as zero, and note that participants' earnings are a function of their daily attendance and output on the job; thus, earnings are a comprehensive measure of participation at both the extensive and intensive margins. In the woman-more-interested subgroup, the treatment increased total earnings over the 10 months we observe them by 59%. Turning to earnings by month, we find positive effects in this subgroup in the program's first four months (when training occurred). These effects dissipated in months five through eight, which coincided with wedding and agricultural seasons that demand women's time, but re-emerged in months nine and ten. While the effects in rupee units are smaller in the long-run than the short-run, the overall level of earnings falls between the training and employment phases as some women choose to leave the program post-training, and hence the long-term effects are larger in percentage terms; the month-ten effect is a 124% increase.

At the six-month endline, the treatment increased the fraction of women in the woman-more-interested subgroup who reported *any* work for income in the previous three months. Thus, the treatment did not simply lead women to substitute their labor supply to the firm and away from another sector, but rather increased their overall employment rate. That said, the effect on overall employment was driven by a significant shift in work at the firm, with a positive but statistically insignificant effect on work elsewhere; this means we are

[3]Data from the second endline suggest most women – 71.1% – correctly predict whether they are more interested than their husbands, and those who are wrong are roughly evenly split between over- and under-estimating husband support. To the extent that perceived and actual disagreement differ, our heterogeneity is by the former, which is the more important of the two for women's decisions about how to communicate.

unlikely to be missing changes in earnings by focusing on earnings at the firm, and to the extent that we are, we are likely underestimating the earnings effects.

Is the heterogeneity driven by a correlate of spousal disagreement rather than disagreement itself? Several results suggest not. First, attendance at the intervention meetings does not differ by subgroup, nor does recollection of intervention content immediately post-treatment, at the first endline; this suggests women in the affected subgroup did not have greater exposure to the intervention or absorb the content better. Second, we proxy for unobserved characteristics of women who report facing opposition from their husbands by using reported opposition in other decisions. Reported opposition is correlated across decisions, but the pattern of heterogeneity in the labor supply effects remains when we control for an index of husband opposition in other decisions and its interaction with treatment. Third, the heterogeneity is robust to controlling in the same way for husbands' and wives' independent interest in women's employment. This suggests our heterogeneity is not just a result of women being interested or husbands disinterested, but rather is driven by disagreement in interest, precisely when communication skills should matter. Finally, we use Lasso to select baseline variables that predict being in the affected subgroup and find the heterogeneity is robust to controlling for these predictors.

We then explore mechanisms, starting with effects on women's communication styles. At six months, respondents were given vignettes in which a woman wanted to work but her husband did not want her to; women were asked what they would say to their husbands if they were in this situation, while husbands were asked what their wives would say to them. In the woman-more-interested subgroup, the treatment increased the amount of assertive responses women provided, with women being more likely to summarize the situation, express their emotions, and explain their rationales for wanting to work, but no effect on stating outright that they wanted to work. A data-driven classification of women's responses confirms the increase in these forms of assertiveness, while also highlighting a decline in passive communication. The treatment did not affect the level of assertiveness husbands predicted, but it did make husbands in our subgroup more likely to say their wives would ultimately change the husbands' minds in the vignette. This suggests the changes in communication were too subtle for husbands to identify precisely but nevertheless persuaded husbands that their wives should work.

These findings, plus additional suggestive evidence, indicate that women persuaded their husbands by providing narratives about their rationales for wanting to work, which in turn convinced husbands that female employment was in the husbands' interest. We also test, and do not find support for, alternative mechanisms including: spouses' knowledge of each others' preferences, husbands' altruism, husbands' information about female job opportuni-

ties, negotiation communication (whereby women propose compromises), bargaining power, women's self-efficacy, women's own preferences, beliefs about women's earnings potential, and gender attitudes. Finally, we provide a conceptual framework based on Schwartzstein and Sunderam (2021) and consistent with our results, which formalizes how communication training enables women to propose credible alternative narratives in a way that husbands find convincing. In this framework, husbands are fully rational, and persuasion is about re-framing existing information rather than providing new information.

In sum, an assertive communication training generated large, persistent effects in important household decisions about female labor supply. Contrary to standard models that assume optimal communication, our results imply women face frictions to communicating optimally in the status quo. In particular, women in the control group appear to overuse passive communication and not be assertive enough, perhaps because gender norms in the setting prevent them from developing assertive skills. Our findings suggest an approach policymakers could take to improve women's agency even when they lack decision rights; in settings with entrenched gender norms and inequalities, improving women's communication skills may be easier and more culturally acceptable than shifting power. For labor-market policy, our results imply that even when jobs are available for women, supply-side constraints within the household keep women from taking them up, but communication training can relax these constraints. We estimate communication training is a cost-effective way to raise female employment outcomes in South Asia among skills or empowerment trainings.

This paper contributes to three bodies of literature. The first is work on the economics of persuasion. There is a rich theory literature on persuasion, including on cheap talk models (Crawford and Sobel, 1982), Bayesian persuasion (Kamenica and Gentzkow, 2011) and information design more broadly (see Bergemann and Morris (2019); Farrell and Rabin (1996); Kamenica (2019); Sobel (2013) for excellent reviews of these literatures).[4] While our primary contribution is empirical, we develop a conceptual framework that is consistent with our results and builds on Schwartzstein and Sunderam (2021), who show how proposing narratives that map the likelihood of particular states to observed data can cause persuasion. We also provide descriptive evidence aligned with the framework suggesting persuasion fails when constructing credible narratives is likely to be challenging. DellaVigna and Gentzkow (2010) classify persuasion as theoretically occurring via beliefs or preferences of the receiver; our results are more consistent with the former mechanism. Coffman and Niehaus (2020) divide persuasion into strategies that appeal to the receiver's self-interest or engender the receiver's

[4]A related but distinct literature studies more behavioral models of persuasion, such as when receivers group situations into categories and apply the same model of inference to all situations within a category (Mullainathan et al., 2008).

other-regard. While our training does not exclusively map onto these two categories, our effects on how women report framing situations and their rationales, alongside findings that husbands do not become more altruistic, are more consistent with the former mechanism.

Empirical work on persuasion tends to be done in wealthy countries, in tightly-controlled lab environments, and/or between strangers (Antonakis et al., 2022; Coffman and Niehaus, 2020; DellaVigna and Gentzkow, 2010; Fréchette et al., 2022; Aristidou et al., 2019; Blume et al., 1998; Barron and Fries, 2024; Wang et al., 2010). There are many reasons why persuasion may manifest differently in real-world settings, such as higher stakes or pre-existing relationships. Likewise, differences in norms and education levels mean persuasion may have different effects in developing countries. Our results suggest that persuasion techniques can be trained through a short-term field intervention in a developing country, and that the returns to doing so can be large in a population of women who lack decision rights but can sway household outcomes through persuasion.

The second literature is the one on female labor supply (see Heath and Jayachandran (2018), Heath et al. (2024), Dahl and Løken (2024), and Olivetti et al. (2024) for summaries). Previous work has evaluated the impacts of soft skills trainings on female employment outcomes, with mixed results (Adhvaryu et al., 2023; Barrera-Osorio et al., 2023; Groh et al., 2016). We are the first to evaluate a communication training, which we show produces large, persistent effects on labor supply at relatively low cost. We also contribute to evidence of intra-household barriers to women's employment (Bursztyn et al., 2020; Field et al., 2021; Heath and Tan, 2020; Lowe and McKelway, 2026; McKelway, 2026; Subramanian, 2024), being the first to document that communication frictions within households constrain female labor supply.

Finally, the paper is related to the literature on intra-household decision-making. Benchmark models in this literature assume decisions are efficient but do not take a stance on the decision process, including the nature of spousal communication (Browning and Chiappori, 1998; Chiappori, 1988, 1992; Manser and Brown, 1980; McElroy and Horney, 1981). More recent work has documented the existence of communication frictions within the household (Ashraf et al., 2023; Conlon et al., 2021; Björkman Nyqvist and Jayachandran, 2017); we show that a short-term training can mitigate such frictions and have persistent impacts on women's labor supply. There are two papers that show related interventions produce important behavioral changes: Ashraf et al. (2020), which finds a negotiation training for adolescent girls in Zambia improved their educational outcomes, and Björkman Nyqvist et al. (2024), which finds a communication training for mothers in Uganda changed investments in child health and decision-making power. Our main contribution here is to document effects on female labor supply, an important economic outcome that is targeted

by numerous policies and has proven difficult to move. Likewise, our setting is distinct; we consider a different age group and household relationship from Ashraf et al. (2020), and rather than sub-Saharan Africa we work in India, where gender norms and inequities are especially pronounced (Anderson, 2024; Jayachandran, 2015) and are likely to influence the effectiveness of communication interventions for females. There are also differences between the interventions, with Ashraf et al. (2020) evaluating a negotiation versus assertive communication training, and Björkman Nyqvist et al. (2024)'s intervention being more participant-generated and less didactic than ours. Finally, our results are consistent with persuasion as opposed to negotiation, the mechanism in Ashraf et al. (2020), or bargaining power, at play in Björkman Nyqvist et al. (2024).

2 Setting

We work in rural Uttar Pradesh, India and begin by outlining six stylized facts about this setting which motivate our experiment. First, it is a setting with large gender inequalities in labor market outcomes. In our control group, 0.35 wives work for every husband who does, and the amount of money husbands expect they could earn in a month from working is 3.09 times what wives expect they could earn.

Second, gender inequities and norms result in women having limited decision rights within their households. Only around 50% of women in our sample report they are one of the household members with final say over various household decisions, versus about 80% for their husbands (Figure 1).

Third, despite having limited decision rights, women are active in the decision-making process. While only half have final say, around 90% report giving input (Figure 1).

Fourth, there is scope to raise women's assertiveness in communication. We measure communication styles at endline using vignettes, asking women what they would say to their husbands if they wanted to work but their husbands would not let them. Some assertive responses are provided often – describing the situation, saying what you want, and why you want it are each provided by 75% of the control group – but even these levels are far from 100%. At the same time, a passive response (saying you will do whatever he wants) and two similar in spirit to it (asking his permission and asking his opinion) are also common, provided by 25%, 52%, and 21% of the control group, respectively. Only 11% of the control group said at endline that it is alright for women to hold different opinions from their husbands, which could keep women from voicing their perspectives (key to assertiveness) and from developing effective ways to do so.

Fifth, women's labor supply is the most common topic of household disagreement. At

endline, surveyors asked women what decisions they and their husbands often have different opinions about, providing minimal examples and not reading options aloud.[5] The most common response in the control group, provided nearly 25% of the time, was whether the women could work outside the home (Figure 2). The second most common response, provided by nearly the same number of women, was that the couple did not have disagreements.

Finally, disagreement about wives' labor supply tends to take the form of wives being more interested than husbands. At endline, the average woman in the control group reported being somewhat interested (3 on a 1-4 scale) in working, while her husband reported being somewhat uninterested (2 on a 1-4 scale) in her working. This gap in interest is much larger than for other decisions spouses often disagree about: whether the wife can visit her natal village, go to the market, or get a new saree (Figure 3). The distributions in Figure 3 reveal that the interest gap for female labor supply is driven by behavior at the tails; husbands are much more likely than wives to be very uninterested (1 on a 1-4 scale), while wives are much more likely than husbands to be very interested (4 on a 1-4 scale).

3 Experimental Design

3.1 Partner Firm

We partnered with a firm that was introducing new jobs for women in our setting. The firm, Obeetee, is India's largest carpet manufacturer. As part of its Corporate Social Responsibility (CSR) initiatives and to alleviate shortages of male carpet weavers, the firm developed a program to train and employ women as weavers. The program begins with four months of paid training in weaving, followed by long-term weaving employment for any women who complete training and wish to continue. Training pay is substantial, close to what women ultimately earn as weavers. Program earnings are a function of women's daily attendance and their output on the job. Both training and employment are full-time and occur in all-female weaving centers located in participants' villages.

We partnered with Obeetee as it expanded this program in seven villages.[6] It constructed new female weaving centers in all villages, each with capacity for 20 weavers. These centers

[5]The question was *Husbands and wives often have different opinions about what choices their households should make ... For example, they might have different opinions on how money should be spent, on whether the wife can go out for various reasons, or on decisions related to their children. In what sorts of decisions do you and your husband often have different opinions?* Surveyors were instructed not to read answer options aloud.

[6]We have conducted prior studies with Obeetee on its female weaving program (Lowe and McKelway, 2026; McKelway, 2022, 2025, 2026), but the present experiment was conducted in a separate sample and separate villages from this prior work.

opened on December 1, 2022, starting with the four-month training phase, immediately followed by the long-term weaving employment phase (see Figure A.2 for a study timeline). The experiment was conducted among households living in the seven centers' catchment areas, i.e. village neighborhoods where the loom centers would have recruited women in the absence of our study. The areas typically included all neighborhoods in walking distance of the loom centers where scheduled castes or other backwards classes lived.[7]

Designing the study to coincide with the opening of these jobs offers two key benefits. First, it allows us to estimate effects on labor supply using the firm's administrative outcomes, both avoiding concerns with self-reports and capturing effects on take-up of a real job. Second, the introduction of these opportunities relaxed the key labor demand-side constraint of job availability for women, allowing us to more cleanly test whether the supply-side, communication constraint is binding net of the demand-side constraint. We ultimately see oversubscription for the jobs with the firm, meaning the demand-side constraint was not fully relaxed, but the problem would be more severe without this job expansion.

3.2 Sample Recruitment

In September 2022, surveyors went door-to-door in the seven catchment areas to recruit women for the study. Surveyors introduced themselves as part of a team from J-PAL collaborating with Obeetee's CSR team to understand women's daily lives and offer initiatives to promote their wellbeing. Surveyors explained that the J-PAL team would be surveying women and their families, and also hosting meetings with groups of women in which women would discuss aspects of village life or things to facilitate their household lives, in particular, how to best communicate with other household members. Surveyors also explained that the J-PAL team would be assessing women's interest in Obeetee's female weaving program but that the program was separate from the J-PAL team's activities, such that women could participate in the study but not in the female weaving program.

In the door-to-door visits, surveyors identified eligible women in each household and invited them to participate in the study. To be eligible, women needed to be married, aged 18-40, and not be the mother or mother-in-law of another eligible woman in their household.[8] We also required women be present when surveyors visited their homes to consent for the study, though surveyors re-visited homes of unavailable women who were otherwise eligible at a later time to seek consent. 98% of eligible women consented, giving a total sample

[7]Male weavers generally come from these castes.

[8]The 18-40 age range is the age range Obeetee targets for its program. We did not allow mothers/mothers-in-law of other eligible women to participate because their presence in the intervention meetings might have made their daughters/daughters-in-law reluctant to speak about household issues.

of 1,540 women from 1,416 households. A baseline survey was taken with all 1,540 women immediately after they consented.

3.3 Randomization

We randomized women to the communication training or an active control group in two steps. We first assigned women to meeting groups, the unit at which the treatment would be delivered. We formed 240 groups of around 6-7 women from the same neighborhood. Group assignment was at the household level. We used stratification within neighborhood to assign multi-women households to different groups and to generate age variation within the groups.[9] The second step of randomization assigned half of the meeting groups to treatment and half to control. We stratified this randomization by village and, within village, by neighborhood.[10]

3.4 Balance and Descriptive Characteristics

Table A.1 presents baseline characteristics in the control group and tests for balance by treatment. The randomization achieved balance, with a joint F-test $p = 0.730$. We also see balance within each of the two subgroups our main heterogeneity analyses consider: wives who were more interested than their husbands in the wives' employment at baseline, and those who were not (joint F-test $p = 0.833$ and 0.195, respectively).

In terms of baseline characteristics, the average respondent is 30 years old, and has three children. The modal woman (41% of the control group) has no education. 42% live in the same household as their mothers-in-law and 38% as their fathers-in-law. 33% had worked off their households' farms in the three months prior to baseline, with the most common form of employment (employing 19% of the control group) being agricultural work.

Table A.2 presents characteristics of husbands. Most of these variables come from endline surveys (husbands were not surveyed at baseline), so we only include control-group means. The means look very similar when we account for attrition using entropy balancing (Hainmueller, 2012) (columns (3)-(6)), so we discuss the unweighted means (columns (1)-(2)) here. Husbands tend to be older and more educated than their wives; the average husband is 34 years old and 75% of them completed between class 1 and 12. Nearly all of them (96%) are employed, with the most common forms of employment being casual non-farm work,

[9] Specifically, we stratified by a categorical variable that denoted whether a household had multiple women, and then among the single-woman households, whether the woman was above or below median age in her neighborhood. The purpose of the multi-woman household stratification was to keep group size consistent, while we stratified by age with the idea that younger women could benefit from the wisdom of older women.

[10] Any neighborhoods that had enough women to form only one meeting group were pooled with other small neighborhoods in their villages to form the neighborhood stratification variable.

employment with a firm, and work in the household's microenterprise. Husbands average three months a year of migrant labor.

Husbands tend to have conservative gender attitudes: just 65% say it is alright for women to work outside the home, only 8% say it is alright for women to have different opinions from their husbands, and nearly all (91%) say husbands should earn more than their wives. About half say husbands know better than wives about what is best for the family, which may seem egalitarian but is likely explained by the fact that women are typically responsible for managing the home. At the same endline, women in the control group expressed similarly conservative beliefs as their husbands on all dimensions except for the first, where 85% responded affirmatively.[11]

3.5 Intervention Delivery

The intervention was delivered over four weeks in October and November 2022 in a series of six, one-hour meetings with the assigned meeting groups. Meetings were held in private in various locations within participants' neighborhoods, such as homes or schools. Women were given small gifts for attending each meeting. In these meetings, the groups assigned treatment received the training in assertive communication, while the groups assigned control played games and took group surveys. All meetings were facilitated by 40 female members of our J-PAL field team. Facilitators were randomly assigned to lead treatment or control meetings, and randomly assigned particular meeting groups. An additional 40 members of the field team were assigned to support each facilitator with the logistics of hosting the group meetings, such as gathering the participants for the meeting, playing with their children during the meeting, or talking with passersby so they did not interrupt the meetings. We provide results on session attendance in Section 5.1 below.

3.5.1 Assertive Communication Curriculum

Communicating assertively means expressing your point of view clearly while still being respectful to others. The concept of assertive communication was initially proposed by the psychologist Arnold Lazarus (1973), and has been the focus of much research and practice in psychology since (Peneva and Mavrodiev, 2013). This work has developed specific strategies for communicating assertively, and documented the importance of assertive communication in helping individuals reach their objectives in joint decisions while maintaining good relationships with the people they communicate with. Note that the meaning of assertive in the

[11]The women's percentages for the second, third, and fourth statements were 11%, 93%, and 59%.

communication literature, which is the definition we adopt in this paper, differs from the common colloquial use of assertive to mean pushy or off-putting communication.

We use an assertive communication training designed by WorldBeing for our study (WorldBeing, 2022). WorldBeing is an organization that offers evidence-based, psychosocial programs in India and other developing countries. Randomized evaluations of their programs have found them to be effective in improving psychological, health, and economic outcomes (Kaur, 2024; Leventhal et al., 2015, 2016; McKelway, 2025).

WorldBeing combined and evolved existing assertive communication techniques to develop a training relevant for our sample. Drawing from the "I-message" technique (Gordon, 2008) and DESC model (King et al., 2008), WorldBeing developed the *see-feel-want* statement (WorldBeing, 2022). This is a technique for explaining your perspective to someone who may disagree with you that involves describing how you *see* the situation objectively, the emotions the situation is making you *feel*, and what you *want* to happen. Generally you also provide the rationale for your preferred outcome, either implicitly in the situation described in the see piece or as an elaboration of the want piece. A key element of all steps, and especially the feel step, is to use the pronoun "I" rather than "you" to explain your feelings without sounding accusatory, as in the I-message technique (Gordon, 2008). As we detail below, the intervention discussed communication in the context of many different household decisions, but an example see-feel-want statement from the curriculum about female labor supply was: *"The cost of our children's school expenses are rising. Because of this, I feel worried and anxious. You work hard... but it has become difficult these days to run a household with just one person's income. I think if I start working in NREGA, like Sunita, then we can increase our income and be able to spend more on our children's education too..."* In our population with low education, WorldBeing found the see-feel-want statement to be a useful and accessible heuristic which also lent itself to hand motions and visuals (Figure A.3). The see-feel-want statement consolidates a number of components of assertive communication that are consistent across models: the importance of clearly communicating your perspective (see), the importance of communicating your emotions (feel), and the centrality of communicating what you want and need (want).[12]

The curriculum was delivered over six, one-hour sessions with groups of women. The see-feel-want statement was taught in the final three sessions, while the first three built prerequisite skills to help women formulate see-feel-want statements and deliver them effectively. The first session provided an introduction to the curriculum and developed listening skills. The second developed women's abilities to recognize and manage emotions, giving them tools

[12]Assertive communication is conceptually distinct from negotiation (Ashraf et al., 2020), which involves proposing mutually beneficial compromises.

to express their feelings and to process their emotions before discussing difficult topics. The third session asked women to reflect on what they wanted in life so that they could clearly communicate their wants to others. The fourth session presented three communication styles – passive, aggressive, and assertive – and explained why assertive communication was the most effective of the three. It then introduced see-feel-want statements as a way to communicate assertively. Women practiced using see-feel-want statements in session five. The final session asked women to anticipate challenges they might encounter when communicating assertively and taught them a problem-solving strategy to overcome such obstacles.

Concepts were taught in a variety of formats, including instruction, visuals, group activities, discussion, and story-telling. Four stories were told across the six sessions, each about a husband and a wife having different opinions about a particular decision: the wife getting a new saree, where to send children to school, the wife working for NREGA, and the wife visiting the market. The curriculum therefore focused on communication between husbands and wives, but many different decisions households might communicate about were discussed; the curriculum would not have come across as promoting women's employment.

Facilitators were trained by WorldBeing to deliver the intervention. The training included both general facilitation skills and training on delivering this particular intervention.

3.5.2 Active Control Group Meetings

In the control group, meetings involved playing games and taking group surveys. The surveys covered a variety of topics related to day-to-day life in women's villages, such as entertainment, schooling, and NREGA.[13] The questions on the surveys were purely descriptive and avoided sensitive issues around gender, norms, and household decision-making. For instance, the questions about NREGA included how many women and men work for NREGA, what the NREGA work involves, and where it occurs. The games were simple and familiar games, but ones women enjoyed playing. The sixth session's game, for example, involved passing a parcel around the circle while a song played, with the woman holding the parcel when the song stopped having to sing a song for the group.

Control facilitators received training in general facilitation skills from WorldBeing, alongside treatment facilitators. The two groups of facilitators split up after this, with the control facilitators being trained on the control curriculum by our J-PAL team while the treatment facilitators were trained by WorldBeing on the communication curriculum.

The goal of using an active control group was to hold fixed effects of attending meetings unrelated to assertive communication, such as spending time outside of the home, meeting

[13]The full list of topics was: entertainment, health, sarees, schooling, voting, NREGA, sanitation, livestock care, local market, and cell phone ownership.

other women, or exposure to the research team.

3.6 Recruitment, Application, and Start of Firm's Program

One week after the intervention ended, surveyors visited women individually to deliver information about Obeetee's program. The process for delivering this information and the information itself was identical across the treatment and control groups. Surveyors were re-assigned villages following the intervention so that women were not informed about the program by their meeting group facilitators.

Surveyors explained that assessing interest in Obeetee's program was one component of the J-PAL team's project and that, with Obeetee's permission, the J-PAL team would administer application for the program to assess interest. Surveyors then provided information about the program, both reading from a script, and showing a video with shots of the female workplaces and discussion of the program from supervisors and participants. Finally, surveyors explained how women could apply for the program and gave women application tickets with their unique study IDs which they could present at the time of application to expedite the process. Any family members around when surveyors visited were free to hear the information as well. Section 5.1 discusses information delivery completion rates.

Women could apply for the program by going to their village's new female weaving center on one of two application days, held at the end of November 2022. Women were required to attend with their husband, parent-in-law, or household head to ensure they were applying with the support of their family. Once at the loom center, women and their family members completed a brief application process administered by a surveyor. If women had brought an application ticket, surveyors recorded the ID on the ticket and asked several questions to ensure it corresponded to the ID of the woman who had come to enroll; otherwise, surveyors asked more detailed identifying questions about the applicant. Women's ages were verified, either with identification cards presented during application or later with their village heads, and only those in Obeetee's target range (18-40) could apply.

There was oversubscription for all seven centers. We therefore held public lotteries at each center following the application days, where we determined which women could begin the program from its start along with a waitlist ordering for the rest. The research team delivered results of the lotteries to applicants in the final days of November 2022, and the program's training phase began December 1. The research team drew women from the waitlist if participants dropped out of the program in the initial weeks, before it was too late for a newcomer to catch up with training.

4 Data and Empirical Specifications

4.1 Outcomes Data

Our outcomes are from three data sources, visualized in our study timeline (Figure A.2). First, we observe whether each woman in our sample applied for Obeetee's program, using records we kept during the application process. We match applicants to women in our sample with IDs on application tickets and identifying information provided upon application.

Second, we digitized registers in each loom center for the first 10 months of the program. This includes the four training months (December 2022 – March 2023) and the first six employment months (April – September 2023). The registers are paper records each center maintains on women's daily attendance, daily productivity, and monthly earnings. We sent members of the research team to the loom centers regularly throughout the 10-month period to ensure the registers were being maintained and being maintained in a way that would allow us to identify women in our study in them. Members of our team then entered the information from each center's register in each month into a survey form. Each register's data was recorded separately by three members of our team to ensure accuracy. In practice, the loom centers' record keeping was imperfect and much of the daily data are missing. We therefore focus on women's monthly earnings (set to zero for non-participants), which are rarely missing. In particular, earnings are missing for 0.5% of the sample in December, 0% in January and February, 0.3% in March, 0.1% in April, 0.5% in May, 1.6% in June, 0% in July, 1.3% in August, and 0% in September; earnings are missing in at least one month 3.9% of the time. Rates of missingness do not differ by treatment in either of our main subgroups of interest (Table A.3). Importantly, earnings are only missing when women participated in the program (we know non-participants earned zero),[14] which facilitates imputation of missing earnings. We detail our preferred imputation strategy and show robustness to alternative approaches in Section 5.2.

Third, we conducted endline surveys, which provide key outcomes on women's general employment and communication styles. Women were surveyed five weeks (EL1) and six months (EL2) after the end of the treatment, and husbands were also surveyed (separately from wives) at EL2. Some additional outcomes are from lab-in-the-field games we invited couples to play, most of which were done at the end of December 2022, though we continued to conduct the games through March 2023 for couples who had not been available earlier. Surveyors were re-assigned villages following the intervention, meaning they were blind to treatment status. All surveys were completed in person, with the exception of some husband

[14]Monthly rates of missingness among participants are: 5.6% (Dec.), 0% (Jan.), 0% (Feb.), 4.2% (Mar.), 1.5% (Apr.), 9.7% (May), 32.0% (Jun.), 0% (Jul.), 34.5% (Aug.), and 0% (Sept.).

EL2 surveys being done over the phone. 91% of women were surveyed at EL1 and 88% at EL2.[15] Attrition rates were higher for husbands, who often work long hours or migrate for work; 75% of husbands were surveyed at EL2, 70% of couples were both surveyed at EL2, and 70% of couples completed the games. Attrition rates do not differ significantly by treatment, a result that holds in the full sample, in each of the two main subgroups of interest, and when we restrict to respondents randomly assigned the survey vignette about employment (Table A.4).[16] We also show that our main treatment effects on survey outcomes are similar in magnitude when we entropy weight (Hainmueller, 2012) the data to achieve exact balance between treatment and control on baseline characteristics among non-attritors, though one loses statistical significance as weighting decreases the effective sample size (Table A.5). We also confirm that attrition does not drive our results by producing a sample unrepresentative of the full sample: the main survey results are robust to entropy weighting non-attritors to resemble the full sample on baseline characteristics (Table A.5). Likewise, effects on the main survey outcomes reported by both husbands and wives are similar when we restrict to the subgroup of respondents where both spouses were surveyed (Table A.6).

We pre-registered outcomes and heterogeneity analyses, which we follow closely. The data appendix (Appendix B) provides additional information on the registration, including the minor deviations we make from the registration and their rationale.

4.2 Empirical Specifications

We estimate overall treatment effects with regressions of the form

$$Y_{i,m} = \beta T_m + \mu_s + \varepsilon_{i,m} \tag{1}$$

and estimate heterogeneous effects with

$$Y_{i,m} = \beta_1 T_m + \beta_2 W_{i,m} + \beta_3 T_m \times W_{i,m} + \mu_s + \varepsilon_{i,m} \tag{2}$$

where $Y_{i,m}$ is an outcome for woman i from meeting group m, and T_m is treatment assignment. We cluster standard errors by meeting group.

We expect communication training to increase our main outcome of female labor supply only among women who are more interested in working than their husbands are in them doing so; these are the women who would have an incentive to use their communication

[15] 83% of women were surveyed at both endlines.

[16] As detailed in Section 6.1, we use vignettes to measure communication. The vignette topic was randomized to be either women's employment or women visiting their natal village, but our vignette analyses focus on those given the employment topic as we use the data to understand effects on labor supply.

skills to advocate for their labor supply. Thus our key dimension of heterogeneity, $W_{i,m}$, is a function of women's responses to two baseline questions, asking how interested they were in working outside the home, and how interested they thought their husbands were in them doing so. Interest was recorded on a 1-4 scale.[17] 43% of women reported greater interest than their husbands, 47% reported the same level, and just 10% reported lower interest. $W_{i,m}$ is an indicator for being in the first category, and given this distribution, $W_{i,m}$ is equivalent to an indicator for being above median in wife-minus-husband interest. This dimension of heterogeneity, including the split at the median, was pre-specified.[18]

We rely on women's predictions of husbands' interest as husbands were not surveyed at baseline. How informative are women's predictions? We assess using control-group data from EL2, when both spouses were surveyed. Table A.7 shows that women's predictions of their husbands' interest are as predictive of our main labor supply outcomes as husbands' own reports (the two are not statistically distinguishable); if anything, point estimates for women's predictions are higher. We also use control-group data from EL2 to approximate error rates in $W_{i,m}$. We estimate 71% of women correctly predict whether or not they are more interested than their husbands. Incorrectness more often comes from women underestimating their husbands' opposition than the reverse, but the difference is relatively small in magnitude: 59% of women who were incorrect underestimated husband opposition versus 41% overestimating. We conclude that women's predictions are quite informative, but to the extent that perceived and actual disagreement differ, our heterogeneity is by the former not the latter; this is arguably the more important of the two for women's decisions about how to communicate with their husbands.

Finally, μ_s are stratification controls. Recall that assignment of meeting groups to treatment was stratified by village and neighborhood within village, while assignment of households to meeting groups within their neighborhood was stratified by age and multi-woman household. We have limited variation within each strata for heterogeneity analysis; 33% of our sample comes from a cell that does not have a woman from each of the four combinations of T_m and $W_{i,m}$. We therefore control for village fixed effects, our highest level of stratification, in place of full strata controls in all of our regressions. In principle, this should not affect our estimates as the probability of assignment to treatment was 50% for all women; in practice, it could matter as the fraction of treated women in a strata often deviates from 50%, driven by uneven numbers of meeting groups within neighborhoods. Given this, we also include fixed effects for bins of the fraction of participants treated within each

[17] Here, 1 meant very uninterested, 2 somewhat uninterested, 3 somewhat interested, and 4 very interested.

[18] Communication skills would, in theory, reduce labor supply when women are less interested than their husbands. However, just 10% of our sample meets this description so we pool them with equally interested couples. This pooling is implied by our pre-specified split at the median.

strata. Thus, throughout our regressions, μ_s denotes two sets of fixed effects: village and fraction-treated-within-strata fixed effects.[19]

5 Main Results

5.1 Compliance and Learning

Compliance at the intervention meetings was high and balanced by treatment. 90% of women attended at least one meeting and the average woman attended 4.6 meetings. Compliance does not differ by treatment in the full sample or in either of our two main subgroups of interest (Table A.9).

Next, we consider women's learning of the content taught in the communication training (Table A.9). The endline surveys asked women if they had heard of a see-feel-want statement and what the three parts of this strategy are. These questions were asked at the end of the surveys to avoid priming responses to other questions. At EL1, the treatment increased the percentage of the three parts women knew by around 30 p.p. ($p < 0.01$). The control group was largely unaware of this communication strategy, knowing only 3.6% of the components on average. We also consider effects on indicators for knowing each of the three parts, finding highly significant effects of around 30 p.p. on each. These effects are no different in the woman-more-interested subgroup, indicating women in both groups learned the intervention content equally well. We see erosion of knowledge between the two endlines, with the overall treatment effect declining by about two thirds over time. Interestingly, this decline was less pronounced in the woman-more-interested subgroup. Below, we will see effects on labor supply only for this group, and no effects on other topics couples disagree about; the heterogeneity in knowledge at EL2 is consistent with women retaining the content better when they use it in communication.

Since treatment status varied within neighborhood, control participants could have gained communication skills from interacting with the treatment group. While this would only lead us to underestimate effects, understanding whether spillovers occurred is useful for interpreting magnitudes of effects. To investigate, we exploit exogenous variation in the fraction of participants assigned treatment within each neighborhood, driven by odd numbers of meeting groups within neighborhoods. We estimate effects of the fraction of other participants in one's neighborhood who were treated on knowledge of the see-feel-want statement in the control group.[20] For reference, the fraction treated variable ranges from 0.27 to 0.78 in this

[19] Our main results look similar, though sometimes a bit noisier, if we exclude strata controls (Table A.8).

[20] We exclude women living in neighborhoods that were pooled with other neighborhoods to form the neighborhood stratification variable, as pooled neighborhoods are unlikely to form a network that would

sample, with 25th and 75th percentiles of 0.42 and 0.56. We present the same analyses for use of assertive communication, measured via the vignette outcomes detailed in Section 6.2 below. We find little evidence of spillovers; the coefficients are all statistically insignificant, not consistently positive or negative, and generally small in magnitude (especially given estimate correspond to moving from 0% to 100% of peers treated) (Table A.10). This could be because treated women did not communicate about the training with the control group, or because women found it difficult to teach the communication skills to other women (indeed, our facilitators were given five days of training before delivering the intervention).

Finally, we consider rates of information delivery about the firm's program. 90% of women were successfully informed, and the treatment increased this slightly, particularly for the woman-more-interested subgroup where it raised information delivery by 4.8 p.p. ($p = 0.029$, Table A.9). This is unlikely to be driven by women's availability to meet with a surveyor since we did not see significant effects on completion of EL1, done just a few weeks later (Table A.4). Instead, we note that successful information delivery can itself be seen as a revealed preference outcome related to female labor supply – it means women chose to take the time to hear about the program, a choice that would have been observable to their families – and thus we interpret the effect on it as an early indication of effects on labor supply. That said, a back-of-the-envelope calculation detailed in footnote 22 suggests the information differential is too small to explain our effects on take-up of the firm's program.

5.2 Effects on Labor Supply and Earnings

Next, we estimate impacts on women's labor supply and earnings, considering three main types of outcomes. The first is an indicator for applying for the firm's employment program.

Second, we consider earnings in the program, which we observe for each of the program's first 10 months. Participants' earnings are a function of their daily participation and their output (i.e. knots woven). Specifically, during the first four months (the training months), participants are paid a fixed amount per day they attend. The amount is reduced in months two through four if women do not reach knots targets. Following training, weavers are paid per knot they weave, adjusted for complexity; in practice, it can be hard to determine exactly how many knots individuals weave so pay is also driven by daily attendance. We set earnings to zero for non-participants. Thus program earnings are a comprehensive measure of participation at both the intensive and extensive margins. Women's intra-household communication could plausibly affect either margin, convincing families that women should join in the program or building support for women's regular attendance, and we decompose

facilitate spillovers.

effects across extensive and intensive margins below.

As discussed in Section 4.1, earnings are sometimes missing (for 0-1.6% of the sample in a given month and 3.9% of the sample in at least one month, with rates balanced by treatment). The fact that earnings are only missing for program participants enables us to impute missing earnings quite precisely. In particular, the vast majority (84.4%) of variation in observed monthly earnings is driven by participating versus not, giving us a relatively narrow range from which to impute earnings. Our preferred approach, which we use in the main-text tables, is to set missing monthly earnings to the average earnings among participants in the same month, center, treatment group, and subgroup; earnings vary substantially by month due to seasonality and to changes in attendance/compensation post-training, earnings differ across centers due to varying compensation practices and workplace cultures, and earnings may have been affected by treatment differentially by subgroup. Indeed, the month-center-treatment-subgroup combination explains 67.9% of the variation in observed monthly earnings.[21] We prefer imputing to dropping missing values because we wish to interpret earnings as a measure of participation and we would underestimate participation by dropping participants. We show robustness to alternative approaches to missing values later in this subsection.

Finally, we consider effects on an indicator for women reporting any work for income outside the home in the preceding three months. We focus on responses at EL2, as only one of the three months preceding EL1 was post-treatment.

We find no average effect on applying for the firm's program; however, this masks heterogeneity by intra-household disagreement (columns (1)-(2), Table 1). 19.9% of our sample applied for the program, and the treatment effect on this outcome is 8.5 p.p. ($p < 0.05$) larger for the woman-more-interested subgroup. Summing the coefficients on treatment and the interaction, we see that the treatment effect in this subgroup is 6.0 p.p. ($p = 0.095$). This represents an increase of 35% relative to the control mean in the subgroup.

This translated into changes in actual program earnings (column (3), Table 1). Across the 10 months for which we have data, the treatment raised earnings in the subgroup of interest by Rs.924 ($p = 0.071$), a 59% increase relative to this group's control mean.[22]

[21] Earnings are missing among all participants in a month-center-treatment-subgroup combination in a small number of cases (six women in May, 24 in June, and 20 in August); in these cases, we impute earnings as the average among all program participants in that month.

[22] As discussed in Section 5.1, treatment increased delivery of information about the firm's program by 4.8 p.p. in the woman-more-interested subgroup, which can be seen as an indication of effects on labor supply in this group. However, a back-of-the-envelope calculation suggests the information differential is too small to explain our effects on program take-up. We quantify how much of our effects could be explained with the information differential by combining the 4.8 p.p. effect with the relationship between information delivery and take-up in the control group. Women in our subgroup assigned control were 10.2 p.p. more likely to apply if they received information. Multiplying by 4.8 p.p. suggests the effect on information delivery can explain only 0.5 p.p. of the 6.0 p.p. effect on application ($p = 0.125$ for the test that the 6.0 p.p. effect equals

Turning to earnings by month in Table 2, we see positive treatment effects in the woman-more-interested subgroup for each of the first four training months (December - March). The effects dissipate in months five to eight (April - July), but re-emerge in months nine and 10 (August - September). While the effects in rupee units are smaller in the final two months than in the first four, the overall level of earnings falls between the training and employment phases as some women chose to leave the program after training, and hence the long-term effects are larger in percentage terms than the short-term effects. The month-10 effect, for instance, is Rs.86, a 124% increase relative to the control mean in this subgroup.

Why did the treatment effect disappear in the middle of this period? April is a time of low engagement in the firm's program due to an agricultural season, and out-of-work demands on women's time continue in May and June with additional agricultural work and a wedding season. We may see no treatment effects during these months because women's own interest in work was low; indeed, interest in employment declined significantly in both subgroups and both treatment arms between EL1 (done in December) and EL2 (done between March and May) among women surveyed at both endlines (Table A.19). The weaving centers understand external demands on weavers' time and permit them to return to work after long absences, allowing the treatment effect to re-emerge.

Next, we confirm our results are robust to how we handle missing earnings. Results are very similar if we impute missing values at the month-center-treatment participant average (rather than month-center-treatment-subgroup), or if we impute missing earnings for an individual in month x as the average of her earnings in months $x-1$ and $x+1$ (Table A.11). We also conduct a bounding exercise motivated by Lee (2009) where we drop missing earnings and further trim observations so that rates of attrition match in the treatment and control groups. We trim separately by subgroup as we are ultimately interested in the treatment effects for each group. Attrition in our case comes from participants of the firm's program, and thus we trim either the bottom or top of the participant distribution. We then estimate our usual heterogeneity specification among all participants whose earnings are not missing and who were not trimmed, first using the sample that generates lower bounds on the treatment effects and then upper bounds.[23] The effects in our subgroup of interest are robust to this bounding (Table A.12). The estimates bound the effect for total earnings between Rs.513 ($p = 0.129$) and Rs.897 ($p = 0.001$), 63% and 239% increases. The

0.5 p.p.). The same exercise suggests only Rs.38 of the Rs.924 effect on total earnings can be explained by information ($p = 0.084$ for the test that the effect equals Rs.38).

[23]Note that given how we do the trimming, the coefficient on treatment will be the lower (upper) bound outside of our subgroup of interest, and the sum of this coefficient and the coefficient on the interaction will be the lower (upper) bound in our subgroup. The interaction coefficient is thus the difference in the lower (upper) bound effects between the subgroups, not the lower (upper) bound of the difference.

bounds are tighter for the month-by-month effects (as there is less missingness in a particular month than in any month); for instance, the month-one effect is bounded between Rs.216 ($p = 0.037$, a 71% increase) and Rs.228 ($p = 0.028$, a 79% increase), while the month-nine effect is bounded between Rs.58 ($p = 0.085$, a 153% increase) and Rs.70 ($p = 0.008$, a 359% increase). Our longest-term (month-10) effects are identical across all methods as earnings were never missing in that month. Finally, our effects on total earnings remain when we total earnings across only the four months when earnings are never missing (Table A.3).

The impacts on earnings could be due to increases at the extensive or intensive margins. Table A.13 presents effects on indicators for positive earnings, along with differences in earnings among women who earned positive amounts. Combining these results and the decomposition of Attanasio et al. (2011), we estimate the intensive margin effect on total earnings is Rs.227 and the extensive margin effect Rs.718.[24] Thus most (76%) of the change in total earnings comes from a change in participation at the extensive margin.

These effects on women's labor supply, present nearly a year after the intervention, are persistent relative to related, one-time interventions in similar settings. In an experiment conducted in villages near those studied here, McKelway (2025) finds a self-efficacy intervention for women increased their employment several months later but effects had faded within a year. Also in nearby villages, McKelway (2026) finds that showing women's families a video promoting Obeetee's program increased women's employment and hours spent working in the short run, but only the effect on hours remained significant at one year. Likewise, Dean and Jayachandran (2019) find no employment effects at 13 months from a video intervention for female workers' families in India.[25] Many active labor market policies have been found to improve female employment outcomes several years later (Card et al., 2010, 2018), but these evaluations are largely done in developed settings where there are fewer constraints on female labor force participation.

Column (4) of Table 1 presents effects on the indicator for any employment in the last three months, reported at EL2. There is an effect in the woman-more-interested subgroup of 7.3 p.p. ($p = 0.099$), a 19% increase compared to control women in this group.[26] Thus, the

[24] Attanasio et al. (2011) decompose an effect on earnings as

$$\Delta = \left[E(Y \mid L{=}1, T{=}1) - E(Y \mid L{=}1, T{=}0) \right] P_0 + \left[P_1 - P_0 \right] E(Y \mid L{=}1, T{=}1)$$

where Y is earnings, L and T are indicators for employment and treatment, and P_1 and P_0 are employment rates in the treatment and control groups, respectively. The first term represents the intensive margin change and the second the extensive margin change. The sum of the intensive and extensive margin effects we estimate is close, but not exactly equal, to the overall earnings effect (Rs.924) due to the strata controls.

[25] Another related study is Bursztyn et al. (2020), which randomized whether husbands in Saudi Arabia were informed about other husbands' support for female employment, but the outcomes in this study were collected over a shorter time horizon (immediately post-treatment and four months later).

[26] Effects on the same outcome from EL1 are in Table A.16, but as mentioned earlier, we do not expect

treatment did not simply lead women to shift their labor supply away from other sectors and to the firm, but rather increased their overall employment rate. Table A.14 decomposes the EL2 employment effect into employment at the firm and employment elsewhere; our subgroup experienced a significant, positive effect on employment at the firm and an insignificant but directionally positive effect on work elsewhere. This suggests we are unlikely to be missing changes in earnings by focusing on earnings at the firm, and to the extent that we are, we are likely underestimating the effects on earnings.

Are the labor supply results influenced by spillovers, given the within-village randomization? Evidence from Section 5.1 suggests spillovers of communication skills to the control group, which could then have affected their labor supply, are unlikely. However, the oversubscription for the firm's program and subsequent random allocation of slots mechanically attenuates effects on program participation. Fortunately, the application outcome is not subject to this concern and the concern only applies to participation effects in levels, not percentages. Comparing the treatment effects on application and month-one participation in our subgroup (Tables 1 and A.13), we see the percentage-point effect on application is larger (6.0 vs. 4.1 p.p.) but the percentage effect is smaller (35% vs. 53%). This suggests that the participation effect comes from both an increased likelihood of applying and of taking up an available slot. We also note that women who applied but were not offered a slot could have sought work elsewhere, which provides an additional motivation for focusing on the any employment outcome and is consistent with the positive (though statistically insignificant) effect on work outside the firm (Table A.14).

Finally, Table A.15 presents effects by an additional pre-specified dimension of heterogeneity: women's age. The treatment effects are not significantly different for women above and below the median age in our sample for any of our main labor supply outcomes. Thus, the key dimension by which our effects vary is preference misalignment within the household.

5.3 What Drives the Heterogeneity?

While the subgroup patterns in labor supply effects could be caused by the intra-household disagreement (or women's perceptions of disagreement), spousal preferences are not randomly assigned. Could a correlate of (perceived) disagreement, not the disagreement itself, explain the heterogeneity?

We consider two classes of confounds. First is the possibility that women in the affected subgroup had more exposure to the intervention or absorbed the content better. Contrary to this, we saw no differences by subgroup in whether women attended any intervention

effects here as only one of the three months it covers was post-treatment. Indeed, we do not see effects on this outcome, overall or in the woman-more-interested subgroup.

session or in the number of sessions they attended (Table A.9). Likewise, we did not see heterogeneity in women's knowledge of the intervention content at EL1, the survey done soon after the intervention concluded (Table A.9).[27]

The second class of confounds is some other characteristic of this subgroup. To investigate this, we add potential confounds and their interactions with treatment to equation 2, and then ask whether we still see heterogeneity in labor supply effects by woman-more-interested. We consider several possible confounds. First, we proxy for unobserved characteristics of women who report facing opposition from their husbands by using reported opposition in other decisions. We form an index of indicators for women reporting greater interest than their husbands in the women visiting their natal village, visiting the market, and getting a new saree. This index is highly predictive of being in our subgroup of interest (column (1), Panel A, Table 3), but our main heterogeneity results are robust to controlling for this index and its interaction with treatment (columns (2)-(4)). The coefficients on this new interaction are generally small and, if anything, negative. This indicates that our results stem from disagreement about employment, not a confounding characteristic of women who report opposition from their husbands.

Another possibility is that the heterogeneity comes from individual preferences; larger effects in the woman-more-interested subgroup could simply reflect larger effects when women are more interested or husbands less interested. Panel B of Table 3 controls non-parametrically for individual interest, including indicators for each level of wife and husband interest, and their interactions with treatment. Our main heterogeneity results remain with these controls, with point estimates on our main interaction at least as large as in Table 1, though standard errors increase. Coefficients on the interactions between treatment and individual interest indicate effects actually tend to be larger when husbands are more interested. These results suggest our heterogeneity is driven specifically by preference disagreement, precisely when communication skills should matter.

Finally, we take an agnostic approach towards the source of confounding, using Lasso to select predictors of being in our subgroup of interest. We allow Lasso to select from the complete set of variables from our baseline survey, excluding the woman and husband interest in employment variables (Lasso selects these in lieu of other variables if they are included, and we confirmed above that our heterogeneity is robust to controlling for them non-parametrically). This means Lasso selects from 121 variables, including demographic characteristics, measures of household decision-making, and women's employment behavior. The selected variables include an indicator for being from one of the common subcastes in

[27]As discussed in Section 5.1, there is some heterogeneity at EL2 which could have reflected more use of the skills over time in the woman-more-interested subgroup.

the setting, along with variables capturing husbands' preferences in non-employment decisions and decision-making around female employment (column (1), Panel C, Table 3). Our heterogeneity results are robust to controlling for the Lasso-selected variables and their interactions with treatment (columns (2)-(4)). The added interactions themselves are generally not statistically significant.

5.4 Other Outcomes

We next consider effects on other decisions couples often have diverging preferences over, using two sources of data. First, the endline surveys asked women if they had visited their natal village, gone to the market, or gotten a new saree in the preceding three months. We consider effects on indicators for each, along with an index of these three indicators and the analogous indicator for employment. Like we did for employment, we estimate effects by an indicator for women reporting greater interest than their husbands in the women doing the activity at baseline. For the index, heterogeneity is by an index of the four woman-more-interested indicators. The second source of data is the lab-in-the-field games. Couples played two games: a dictator game (in which husbands were endowed with 10 tokens and chose how many to give to their wives) and a trust game (in which any of 10 tokens husbands sent were doubled, and wives could send back tokens to their husbands). Couples were given a chance to communicate in private before the husband's choice in one of the games, and we randomized which one. Individuals in randomly selected couples could redeem the tokens for prizes, selected to be either women's or men's goods.[28] The outcome here is the number of tokens women ended each game with.

We find few effects on these other outcomes. Starting with the survey outcomes, we see negative effects on the index and saree outcome among equally or less interested women, potentially due to uninterested women advocating against the activities (Table A.16). However, we see no effects on the index or on any of the components aside from employment at EL2.[29] Likewise, we see no treatment effects on women's tokens in any of the four versions of the games (trust and dictator, crossed with pre-play communication) (Table A.16).

We can only speculate as to why our treatment led women to successfully advocate for their employment and not for these other outcomes. One possibility is that these other decisions are not as big a priority to women, so not worth the time and effort of persuading

[28]There were 10 optional prizes, each of which cost between one and five tokens: cologne, lipstick, men's sunglasses, earrings, a male watch, an anklet, a male necklace chain, a jewelry set, cloth for men's shirts, and a saree. We randomized which couples got prizes due to budget constraints, but the randomization was done after the games were played so did not affect incentives in the games.

[29]We also see no significant difference in effects on this index by age, the other pre-specified dimension of heterogeneity (Table A.15).

one's husband. While women's average stated interest in these other activities is at least as high as for employment (Figure 3), the interest questions may be best suited for identifying immediately enjoyable activities versus long-term priorities. Another explanation is that intra-household disagreement is weaker for these other decisions. The heterogeneity analyses should help with this, but perhaps our measures of disagreement do not fully capture the intensity of spousal misalignment about employment versus other domains. Another possibility is that there is more scope for persuasion when husbands are more uncertain about costs and benefits of the activity in question. There was likely to be much more uncertainty in the labor supply decision than in the others given female employment is low and the job with the firm was new. A final explanation is that the communication skills work better in decisions like employment where there could be joint gains; the potential for women's income to benefit other members of the household could have given women persuasive arguments for their employment, whereas natal village visits, new sarees, and, to some extent, trips to the market are more zero-sum.[30] We note that this final story cannot explain the null effects in the trust game, though one of the first three stories could.

6 Mechanisms: Changes in Women's Communication and Household Decision Processes

We next examine the mechanisms behind the effects on labor supply in the woman-more-interested subgroup. Our results indicate that the training shifted women's communication styles from passive to assertive, and this was persuasive in that husbands became more likely to report their wives would ultimately change their minds about employment. We provide suggestive evidence that this persuasion worked by helping husbands understand their wives' rationale for wanting to work, increasing husbands' confidence that female employment would benefit the household versus threatening husbands' control, identity, or social image. We provide evidence against alternative explanations before formalizing our proposed mechanism in a conceptual framework in Section 7.

6.1 Vignettes to Measure Communication Style

To assess women's communication styles, we developed vignettes that posed a hypothetical situation where a wife wanted to do something that her husband opposed her doing,

[30] In this setting, husbands are generally responsible for acquiring groceries and household necessities at the market, whereas women's interest in visiting the market is often more leisure-motivated, though it is of course possible that women could acquire household necessities when they go.

randomized to be either working outside the home or visiting her natal village. At EL2, spouses were assigned the same vignette topic as each other. We asked women what they would do in this situation, and asked husbands what they thought their wives would do. Specifically, we focus on responses to two questions. The first asked what women would say to their husbands, assuming the women initiated a discussion about the topic.[31] Surveyors matched responses to one or more of several possible answer options, but did not read the options aloud. Informed by feedback from our partner who designed the treatment, the options included: four assertive responses the treatment would have taught (summarize the situation, describe the emotions she is feeling, tell him what she wants, and tell him why she wants it); options corresponding to other communication styles, namely passive (tell him she will do whatever he wants), aggressive (tell him he's wrong, unreasonable, stupid, etc.), and negotiation (offer a compromise[32]); and responses that would likely come up but cannot be classified neatly into the above communication styles (ask his permission, ask his opinion, describe the emotions he is making her feel,[33] plead/beg, and an "other" category). Second, we consider responses to a question asking whether respondents thought the women would ultimately change their husbands' minds. Appendix B provides the script and questions from the women's EL2 employment vignette.

We used vignettes rather than asking respondents about their own household decisions for two reasons. First, piloting revealed households were uncomfortable with surveyors asking detailed questions about their own decision-making processes and disagreements. Second, vignettes fixed the topic and direction of spousal disagreement, avoiding confounds that could arise if treatment affected which disagreements respondents reported on.[34]

As our goal is to understand the labor supply effects, we restrict the sample to those given the employment vignette and estimate the heterogeneity regression (equation 2). We focus on women's EL2 (versus EL1) responses since we also have husbands' reports at EL2.

[31] We also consider effects on whether the women would initiate, but the main question on what women would say was asked whether or not respondents said they would initiate.

[32] This could be something in between what the two want in this decision, or going with what he wants in another decision in exchange for what she wants in this decision.

[33] Surveyors were provided examples to help them distinguish between this option and the assertive one. The assertive one would match something like "I feel disappointed," whereas this one would match a statement like "you make me feel angry."

[34] Likewise, we did not attempt to record spousal conversations as prior work in this setting found couples to be highly averse to conversations being recorded. Conversations are unlikely to have been naturalistic even for those who agreed.

6.2 Effects on Women's Communication

We begin by considering effects on women's use of assertive communication. At EL2, the treatment increased the percent of the four assertive responses that women in our subgroup provided in the context of an employment decision by 8.4 p.p. ($p = 0.004$, Table 4, column (1)). This is 14%, or 0.322 standard deviations, relative to the control mean and standard deviation in this subgroup. We also see a significant effect in the subgroup at EL1 (Table A.17) but it is a bit smaller than at EL2. At both endlines, the effect is larger in the woman-more-interested subgroup, though not significantly so. These latter results are consistent with the idea that women retain communication skills better when they use them.

Columns (2)-(5) in Table 4 present effects on indicators for providing each of the four assertive responses in the employment vignette at EL2. We see significant increases in our subgroup in women: summarizing the situation (increase of 9.9 p.p. or 14% relative to the control mean, $p = 0.037$), describing their emotions (increase of 13.0 p.p. or 57% relative to the control mean, $p = 0.011$), and explaining why they want their preferred course of action (increase of 9.5 p.p. or 12% relative to the control mean, $p = 0.039$). In contrast, there is no change in our subgroup in women communicating what they wanted. The pattern of results looks similar at EL1, though at that endline there was no increase in describing emotions (Table A.17). Thus, these results indicate that the treatment did not lead women to say outright that they wanted to work, but led them to communicate information on how they saw the situation, how they were feeling about it, and why they wanted to work.

Why did women want to work? At EL1, we asked women what reasons they saw for applying to the firm's program (this was asked whether or not they had applied). The most common reasons were financially-motivated: having more money in general (provided by 62.8% of the sample), learning a valuable skill (47.2%), having money in women's own control (47.0%), and having more money for a specific purpose (35.9%). We do not see treatment effects on the reasons women in our subgroup provided (Table A.18). Of course, the reasons women gave surveyors may have differed from the reasons they gave their husbands. Nevertheless, these results indicate that the treatment did not change why women wanted to work but rather whether they conveyed rationales to their husbands.

We discuss two concerns about reporting in the vignette. First, treated women may have said they would use assertive communication simply to please surveyors. However, the effects above on labor supply from administrative data, and the effects below on husbands' beliefs that their wives would convince them, suggest a real change in communication, not just reporting bias. Additionally, demand effects are not fully aligned with the pattern of effects on assertive components: we see effects on women in our subgroup explaining the situation, their emotions, and their rationale but not on stating what they want. Yet the treatment

encouraged all of these and there were highly significant effects on knowledge of each part of the see-feel-want statement at EL2 (Table A.9). The second concern is that the control group used assertive communication as much as the treated group, but treated women were better at articulating these strategies, making their responses easier for surveyors to match to the assertive answer options. Contrary to this, uncategorizable, "other" responses were not significantly more likely in the control group (Table A.17), and better articulation would itself reflect more assertive communication.

The data considered thus far come from a question asking women what they would say to their husbands if they initiated a discussion with the husbands, but we also asked whether women would initiate discussion. At both endlines, the treatment increased the likelihood that women in our subgroup say they would initiate conversation about employment (Table A.17). This is consistent with greater assertiveness to the extent that speaking up at all is an assertive behavior. That said, the effect on initiating is small in magnitude, especially relative to the control group where virtually all women said they would initiate; the impact at EL2, for instance, is 5.5 p.p., a 6% increase compared to the control.

Finally, we consider effects on women's confidence in their ability to ultimately change their husbands' minds in the employment vignette. At EL2, the treatment increased the fraction of women in our subgroup responding affirmatively to this question by 8.0 p.p. ($p = 0.105$), an 11% increase from the control mean. We also see a positive effect on a similar outcome from EL1 (Table A.17). These results suggest the treatment trained women to use assertive communication and women saw the techniques as effective.

6.2.1 Data-Driven Classification of Women's Communication Strategies: LCA

In a complementary analysis that reduces dimensionality, we use a data-driven method to classify types of communication and test which types are affected by treatment. Specifically, we use Latent Class Analysis (LCA) to partition women's responses to the vignette question on what they would say to their husbands. LCA is a statistical technique used to classify binary data comprising an unknown number of latent classes (communication types in our case) to probabilistic assignment into specific classes, and has been widely used for classification across several disciplines, including sociology (Goodman and Clogg, 1992), psychiatry (Ulbricht et al., 2018), and political science (Alvarez et al., 2017). We provide an overview of LCA in Appendix C.[35] We use eleven indicators for providing each of the answer options to the vignette question, excluding "other." As before, we use responses from EL2 and only include those given the employment vignette. The classification is conducted over the whole sample, pooling both treatment arms and subgroups.

[35] See Hagenaars and McCutcheon (2002) for further detail.

The LCA with the lowest value of the Bayesian Information Criterion (BIC, best model fit with penalties for greater number of classes) is with three classes.[36] Columns (7)-(9) of Table 4 present treatment effects on the probability assigned by LCA of women being in each class. In our subgroup, treatment reduces the likelihood of being in Class 1 by 10.3 p.p ($p = 0.006$), has no effect on the likelihood of being in Class 2, and increases the likelihood of being in Class 3 by 7.8 p.p. (p =0.065). Thus, the LCA classification shows that treatment clearly separates communication strategies about employment in our subgroup of interest.

What communication characterizes Classes 1 and 3? Using the LCA's binary assignment of women into the class with the greatest likelihood, we illustrate the fraction of women in Classes 1 and 3 who provided each answer option, alongside the fractions across women of any class (Figure 4). The largest difference is for expressing why the respondent wanted to work; 97% of women in Class 3 report doing this, compared to less than 9% of those in Class 1, a difference of 88.5 p.p. The overall sample mean for this behavior is 88.4%, which translates into a difference of 100% relative to the mean. The second largest increase comes from summarizing the situation, provided by 20% of Class 1 respondents and 87% of those in Class 3, a difference of 67 p.p. There are increases, but much smaller ones, in Class 3 expressing the emotions they are feeling (24 p.p.) and saying what they want (13 p.p.). On the other hand, three responses are more likely in Class 1 as opposed to Class 3: the passive option (tell him she will do whatever he wants), along with asking his permission and asking his opinion, two options that are similar in spirit to the passive one. Overall, this clustering approach shows that treatment makes it more likely that women engage in a class of assertive responses, with summarizing the situation and explaining why women wanted to work being especially defining characteristics of this class; at the same time, the treatment reduces the likelihood of women providing more passive responses.

We show robustness to using the model that maximizes entropy (model separation) as opposed to minimizing BIC. The LCA that maximizes entropy is that with two classes.[37] Once again, we see a clear separation, with the treatment increasing the likelihood of women in our subgroup being in Class 2 by 8.8 p.p. (p =0.013, Table A.17). Figure A.4 shows the fraction of women in the overall sample providing each response, alongside the fraction in each of the two classes. The results are quite similar to those in Figure 4, with summarizing the situation and explaining why being the largest differentiators of Class 2 as opposed to Class 1. Describing one's emotions also differentiates the two but less so, and there is no difference by class in women saying what they want. Also similar to Figure 4, the class

[36] We conduced the LCA for two through ten classes, and the one with the lowest BIC is three classes.

[37] The entropy value of the LCA with two classes is 0.85, greater than the 0.80 cutoff usually used to denote well-separated groups (Weller et al., 2020).

which the treatment moves women away from is characterized by the passive response and two responses similar in spirit to it (asking his permission and asking his opinion).

6.3 Husbands' Reports of Wives' Communication

Do husbands detect changes in their wives' communication? We find no effects on husbands' beliefs about their wives' assertiveness; in our subgroup, the effect on the percentage of the four assertive responses husbands thought their wives would use in an employment decision is small and statistically insignificant (-0.9 p.p., $p = 0.783$, column (1), Table 5). Likewise, we see no effects on indicators for husbands providing each of the four responses (Table A.17).[38] However, husbands were more likely to say their wives would ultimately change the husbands' minds. In the employment vignette and in our subgroup, the treatment increased the fraction of husbands responding affirmatively to this question by 9.5 p.p., or 14% relative to the control mean ($p = 0.097$, column (2), Table 5). The differing pattern of effects on husbands' and wives' reports cannot be explained by husbands and wives from different couples being surveyed; effects look similar when we restrict to couples where both were surveyed (Table A.6) and when we entropy weight (Hainmueller, 2012) non-attritors to resemble the full sample on baseline characteristics (Table A.5). The results thus suggest that husbands were unable to pinpoint exactly how their wives' communication had changed – perhaps because the change was subtle or inoffensive – but they did recognize their wives' ability to ultimately persuade them about female labor supply.

We also consider effects on husbands' interest in women's employment, based on women's predictions of their husbands' interest at EL1 and EL2, and husbands' stated interest at EL2 (Table A.19). In the affected subgroup at EL1, the treatment increased women's reports of husbands' interest (by 0.175 on a 1-4 scale, $p = 0.133$) and decreased the fraction of women who said they were more interested than their husbands (by 8.4 p.p., $p = 0.045$). However, there were no effects on these outcomes at EL2, based on both women's predictions and husbands' own reports. We see two possible explanations for this. First, EL2 was done during agricultural and wedding seasons, when women's interest in work, and thus their incentive to persuade their husbands, was low; indeed, among those surveyed at both endlines, women's own interest and predictions of their husbands' interest declined between EL1 and EL2 in both treatment arms and both subgroups (Table A.19). This is not inconsistent with effects on convincing husbands in the vignette at EL2, as the vignette asked about a hypothetical scenario where the wife wanted to work and husband opposed it, nor is it inconsistent with the effect on employment at EL2, as the survey question asked about work in the preceding

[38] There is also no impact on whether husbands predicted their wives would initiate conversation (Table A.17).

three months. The second explanation is that women changed their husbands' minds in the sense that the husbands would permit the wives to work, but did not change husbands' deep-seated preferences about female employment, generating the null effects on husband interest. In the classification of DellaVigna and Gentzkow (2010), persuasion may have been belief- rather than preference-based, with women shifting husbands' beliefs about the benefits of female employment.

6.4 What Persuaded Husbands?

We found in the vignette that the treatment enabled women in our subgroup to change their husbands' minds about employment, but what did women say that persuaded them? We conjecture that it was narratives about women's rationale for their employment. We did not see impacts on women stating outright that they wanted to work, but they were much more likely to explain why they wanted to work. We also saw impacts on women summarizing the situation, which can implicitly convey a rationale by highlighting a problem employment would solve. Finally, we saw impacts on women describing emotions; this could have brought sincerity and credibility to the rationales women provided. The sample see-feel-want statement quoted in Section 3.5.1 and taken from the curriculum illustrates: the woman conveys that she wants to work to fund the children's education both in the "see" portion and in the elaboration of "want" describing why she wants to work, while also expressing she feels worried and anxious about the rising school expenses.

Such narratives may be especially important for convincing husbands in decisions about female employment. Husbands could gain from women's employment if the earnings are spent for the household's benefit, but lose if women's work threatened men's bargaining power, identity, or social image. Women's ability to credibly explain their rationale for working could have convinced husbands that the former was more likely. The narratives could have provided husbands with new information, but need not have done so; they could have given husbands a new way to interpret information they already had (as in Schwartzstein and Sunderam (2021)), and we outline a model of this in Section 7.

To investigate, we identify couples where wives' narratives are less likely to have convinced husbands that they would benefit from women's employment. In the first version of this analysis, "unconvincing" wives are those whose husbands believed the wives' potential earnings were high relative to their own; conceptually, these are couples where the husband perceived a threat to their bargaining power from the wife working, making it more difficult for women to change the husbands' minds. We use husbands' EL2 predictions of how much they and their wives could earn if each worked for a month (asked regardless of either person's

employment status), and define an indicator for husband minus wife potential earnings being below median. This indicator was not significantly affected by treatment (Table A.22). The second version identifies "unconvincing" wives using the reasons for applying to the firm's program they provided at EL1. There were two options for this question that, in theory, should be unambiguously bad from husbands' perspectives: women wanting to have money in their control and wanting to spend time outside the house (in violation of gender norms in this setting). We hypothesize that women who held these reasons would have been less effective at persuading their husbands, not necessarily because they stated these reasons to their husbands but because previous interactions would have made the husbands skeptical of alternative rationales. We thus compare women who provided either rationale to those who provided neither. This "unconvincing" indicator was also not affected by treatment (Table A.18).

The results from both versions of the analysis are consistent with our story (columns (3)-(4), Table 5). For women in our subgroup who were convincing according to the first, earnings threat version, the treatment increased husbands' reports of being convinced in the employment vignette by 18.8 p.p. ($p = 0.030$); for women in our subgroup who were unconvincing according to this proxy, the treatment effect was just 2.7 p.p. ($p = 0.722$). Likewise, the treatment effect for women in our subgroup who were convincing according to the second, reasons-for-applying definition was 17.6 p.p. ($p = 0.052$), versus 3.4 p.p. ($p = 0.702$) for those who were unconvincing. We view these analyses as only suggestive – they are indirect tests of our theory, involve conditioning on endline variables (though ones that did not change with treatment), and statistical power is low – but they are consistent with our proposed explanation.

6.5 Alternative Mechanisms

Before formalizing our proposed story in a theoretical framework, we present evidence that tests for, and rules out, several alternative mechanisms.

Husbands' Knowledge of Women's Preferences. The see-feel-want statement could have made husbands aware that their wives wanted to work. However, we saw above that the treatment made women in our subgroup more likely to use all of the assertive responses except for stating outright what they wanted. Table A.20 presents effects on how accurately husbands' predicted their wives' interest in employment at EL2; we see no significant treatment effects in our subgroup across seven different measures of accuracy,[39] nor are the point

[39]The seven measures are indicators for husbands being exactly correct about their wives' interest on the 1-4 scale, correct that the wife was overall interested versus uninterested, correct about the wife being more

estimates in this group consistently indicative of greater accuracy. Separate from accuracy, the treatment did not affect how interested husbands in our subgroup predicted their wives were at EL2 (Table A.19).

Husbands' Altruism. Even if the change in communication did not affect husbands' beliefs about their wives' preferences, it could have changed how much husbands cared about the wives' preferences. However, we see no effects on husbands' giving in the basic dictator game (with no communication) in our subgroup of interest (Table A.22) or in the full sample (Table A.16).

Husbands' Information about Female Job Opportunities. Another possibility is that women persuaded husbands by providing information about job opportunities (e.g. pay, hours, etc.). However, it seems unlikely that women would be better informed on these dimensions than men, and such information would likely spread quickly across villages. It is true that husbands may have been out of the home when our surveyors delivered information about the firm's job to their wives. While we did not record whether husbands were present during information delivery, we use a proxy for husbands' presence: whether husbands had migrated for work in the previous year as reported by wives at EL2, a variable that was not significantly affected by treatment (Table A.22). Consistent with cross-village information spread, our effects on application are no larger when husbands were less likely to be present for information delivery (Table A.22). In the same table, we also see no effects on husbands' beliefs at EL2 of how much their wives could earn in a month of work, ruling out changes in husbands' beliefs about women's earnings potential.[40]

Negotiation. Another story is that women learned negotiation skills in addition to assertive communication in the treatment, and it was negotiation rather than assertive communication that allowed women in our subgroup to start work. However, we do not find significant effects on women providing negotiation-style communication in the vignette; this is true if we look at women's EL1 or EL2 reports, or husbands' EL2 reports (Table A.21). This is also consistent with the LCA results, which show negotiation-style communication is not a differentiator of the communication classes the treatment affected (Figure 4). We also do not see evidence that women in our subgroup compensated for their employment in the months preceding EL2 by engaging less in activities they often face opposition from their husbands in (visiting their natal villages, visiting the market, or getting a new saree; Table A.21).

interested than the husband versus not, maximally overestimating the wife's interest, maximally underestimating the wife's interest, as well as the absolute error on the 1-4 scale and the amount of underestimation on the 1-4 scale.

[40] This is not inconsistent with effects on women being trained as carpet weavers, as daily wages for female carpet weavers are similar to those from other occupations women might participate in.

Bargaining Power. An increase in women's bargaining power could potentially explain our results; communication skills could be a "distribution factor" (Browning and Chiappori, 1998) that raises the weight households give to women's preferences, in turn raising women's labor supply when women are more interested in it than their husbands. Two results suggest bargaining power is not the mechanism. First, we do not find evidence that the treatment increased women's final say over various household decisions, based on their own reports at EL1 and EL2, or their husbands' reports at EL2 (there is actually some evidence that final say went down outside of our subgroup, Table A.21). We note that while reports of final say are commonly used in the literature, their limitations are also widely-acknowledged (Donald et al., 2017). Thus, we also point to results that do not rely on direct measures of bargaining power: we saw no effects on other activities couples often disagree about, even among couples where the wife was more interested at baseline (Section 5.4). We also saw no effects on these activities in the woman-more-interested-in-employment subgroup (Table A.21). A shift in bargaining power would predict increases in other activities that women are interested in doing, not just labor supply.

Women's Knowledge of Husbands' Preferences. Our subgroup is formed based on women's predictions of their husbands' interest; another possibility is that the treatment motivated women in this group to discuss employment with their husbands, the discussions corrected pessimistic beliefs about husbands' support for female employment, and this drove the effects on labor supply. Table A.20 presents effects on the same seven measures of women's accuracy about their husbands' preferences as we considered above for husbands' accuracy about their wives' preferences. The treatment produced a reduction in wives maximally underestimating their husbands' interest (i.e. predicting their husbands were very uninterested when the husbands were very interested) in our subgroup that is significant at the 10% level. However, the effect on the amount of underestimation is not significant ($p = 0.362$), nor are the effects on any of the five other outcomes. Maximally underestimating is also a rare outcome, occurring in just 7.3% of control couples in the subgroup. Thus there is not strong evidence that the mechanism is women becoming more accurate about their husbands' preferences.

General Self-Efficacy. Another explanation is that the treatment not only taught women assertive communication but also raised their general sense of self-confidence, and the latter empowered women in our subgroup to initiate, persist, and ultimately succeed in conversations with their husbands. However, we do not see significant impacts of the treatment on women's general self-efficacy in either subgroup at either endline (Table A.22).

Women's Interest in Employment. Another possibility is that the treatment raised

employment by making women themselves more interested in employment, e.g. by making work seem more appealing or by influencing what they believed they could feasibly aspire to. It is not obvious why this effect would be more pronounced in our subgroup, but perhaps the impact would be larger for women who already had some interest in work at baseline or women for whom husband opposition to employment was salient. However, we see no evidence that the treatment raised women's own interest in work, in (or out of) our subgroup; this is true if we look at women's own reports from either endline or husbands' predictions of women's interest at EL2 (Table A.19). Likewise, we saw above that the treatment did not affect the reasons women in our subgroup saw for applying to the firm's program (Table A.18).

Beliefs about Women's Earnings Potential. Better communication in the workplace, or expectations thereof, could increase female employment and earnings. We believe this does not drive the treatment effects, for two reasons. First, it is unlikely that only those in the woman-more-interested subgroup would have experienced improved workplace communication. Second, we examine treatment effects on perceptions of women's earnings potential. We do not find effects in our subgroup on women's or husbands' EL2 perceptions of how much the women could earn if they worked for a month (Table A.22).[41]

Gender Attitudes. A final possibility is that the treatment shifted attitudes about the acceptability of female employment. This could have occurred directly through the messages in the curriculum, or indirectly if the group meetings changed the peer reference group and early adopters of employment in the treatment group then influenced labor supply of other women in their groups. This seems unlikely to explain our results, for two reasons. First, meeting group only explains around 20% of the variation in women's and husbands' EL2 reports that it is alright for women to work outside the home. Second, it is not clear why any effects on gender attitudes would be stronger for our subgroup of interest. Indeed, we find no treatment effects on women's or husbands' reports on the acceptability of women's employment in (or outside of) our subgroup (Table A.22).

7 Conceptual Framework

We adapt the framework of Schwartzstein and Sunderam (2021) to provide a micro-founded formalization of our proposed mechanism, outlined in Section 6.4. In the model, a

[41] As mentioned in footnote 40, this is not inconsistent with treatment leading women in this subgroup to be trained as carpet weavers, as daily wages in the firm's job are similar to those in other jobs women might take.

husband has decision rights and forms beliefs about the likelihood he would benefit from his wife's employment using his prior beliefs, observed history of behavior in the household, and a likelihood mapping between the history and underlying states. The training teaches women to propose credible alternative narratives or models of the world (i.e. likelihood mappings), which alter husbands' beliefs about the benefits of their wives' labor supply. In this way, persuasion is not about providing new information per se, but about re-framing existing information. These narratives are constrained by the husband's priors and the observed history (thus, husbands are fully rational). We end the section with a discussion of when persuasion can fail.[42]

7.1 Model Setup

The husband holds decision rights in the household, with women providing input (consistent with Figure 1).

States. There are three possible states of the world:

- State N: the household does not need additional resources;
- State $R_\emptyset$: the household does need additional resources, but the woman working only increases resources in her control, not joint household resources;
- State R_S: the household does need additional resources, and the woman working increases joint household resources.

We set up these states to match the sample see-feel-want statement provided in Section 3.5.1, but the states need not match these exactly; we just need the states to have implications for the husband's payoffs from the wife's employment, and to be dimensions husbands could learn about from the forms of assertive statements the treatment increased (in Table 4).

Priors. Husbands hold prior beliefs over the probabilities of the three states, given by

$$\mu_0(N) = a, \qquad \mu_0(R_\emptyset) = b, \qquad \mu_0(R_S) = c,$$

with $a, b, c > 0$ and $a + b + c = 1$.

History: There is a two-dimensional history of observed signals about the state: $h = (h_1, h_2)$:

[42] We also provide a numerical example in Appendix D to fix intuition.

- $h_1 \in \{r, n\}$: a signal about whether the household needs additional resources (e.g., an increase in school fees). r indicates that there is a need for resources, while n indicates there is not.

- $h_2 \in \{s, ns\}$: past behavior of the wife that is informative about whether she would share her earnings if she worked. A realized signal of s indicates that the wife would share, while ns indicates she would not.

Payoffs. The husband prefers the wife work under state R_S only and will agree to her working if he believes the likelihood of this state is sufficiently high (formalized below). Specifically, the husband's payoff from the wife working is 1 in R_S, but is $-k$ in $R_\emptyset$ or N, with $k > 0$.[43] The wife not working in any state gives the husband a payoff of 0. We assume the wife always prefers to work, which only implies the wife wants to persuade the husband about employment and sets up the intra-household disagreement characterizing our subgroup of interest.

Default Narrative. The husband has a default narrative, i.e. his model of the world, which is a likelihood mapping from candidate states to the probability of the observed history. Under the default narrative d, the two signals are conditionally independent, each is informative about its own dimension,[44] and they are given by:

$$\pi_d(r \mid R_\emptyset) = \pi_d(r \mid R_S) = p, \qquad \pi_d(r \mid N) = 1 - p, \qquad p \in (\tfrac{1}{2}, 1),$$

$$\pi_d(s \mid R_S) = q, \qquad \pi_d(s \mid R_\emptyset) = 1 - q, \qquad q \in (\tfrac{1}{2}, 1), \qquad \pi_d(s \mid N) = \rho \in (0, 1)$$

where p indexes the informativeness of the need signal, q that of the sharing signal, and ρ is the probability of observing a sharing signal in a household with no need. As we detail below, this default narrative constrains the set of alternative narratives the wife is able to propose, in that the husband rejects any narrative that fits the data worse than the default.

Realized History. For what follows, we assume that the realized history is $h = (r, s)$. The joint likelihoods under the default narrative are therefore:

$$L_N \equiv \pi_d(h \mid N) = (1 - p)\rho, \qquad L_{R_\emptyset} \equiv \pi_d(h \mid R_\emptyset) = p(1 - q), \qquad L_{R_S} \equiv \pi_d(h \mid R_S) = pq.$$

[43] We could generalize these payoffs such that they are different for states $R_\emptyset$ and N, which would not change the main results relative to this simpler exposition.

[44] So a realized signal of r is equally informative of need regardless of whether the woman would share.

7.2 Belief Updating and Decision-Making Under Default Model: Pre-Training Status Quo

Likelihood of Observed History. The husband computes the likelihood of the observed history given the priors and default narrative:

$$F_d \;\equiv\; \Pr(h \mid d, \mu_0) \;=\; aL_N + bL_{R_\emptyset} + cL_{R_S}. \tag{3}$$

F_d reflects how well the default narrative fits the data. As we will discuss below, husbands are open to alternative narratives if they fit the data at least as well as the default one, given the husband's priors.

Posterior Beliefs. Using Bayes' rule, the husband's posterior belief about the state being R_S under the default narrative is given by:

$$\mu_p(R_S \mid h, \mu_0(R_S), d) = \frac{c\,L_{R_S}}{F_d} = \frac{c\,pq}{a(1-p)\rho + b\,p(1-q) + c\,pq}. \tag{4}$$

Decision-Making. The husband chooses for his wife to work if his expected payoff from her working is weakly greater than his expected payoff from her not working:

$$\mu_p(R_S) - k\,\mu_p(R_\emptyset) - k\,\mu_p(N) \;\geq\; 0 \tag{5}$$

where his posterior beliefs determine the likelihood of each state. This reduces to the following threshold rule:

$$\mu_p(R_S) \geq \tau \equiv k/(1+k) \tag{6}$$

Combining with equation (4), the husband favors his wife working iff

$$cL_{R_S} \geq \tau F_d$$

i.e.

$$c\,pq\,(1-\tau) \;\geq\; \tau\big[a(1-p)\rho + b\,p(1-q)\big]. \tag{7}$$

Model Implication 1. In the pre-training equilibrium, in households where the husband's belief about the true state being R_S are sufficiently optimistic (the inequality in equation (7) is satisfied), husbands do not oppose their wife working and persuasion is not required.

7.3 Impact of Communication Training

We model the training as teaching women to propose credible alternative narratives. The treatment did not provide new information about the costs and benefits of female employment, but it led women to communicate interpretations of how they saw the situation (e.g. by explaining their household's need for resources), how this made them feel (e.g. worry about the household's ability to meet expenses), and their rationale for wanting to work (e.g. to contribute to their household's financial needs). The first and third map directly to the possible states in the model, while the second could lend sincerity to the wife's statements. As mentioned above, the particular states we outline are simply illustrative; any states that vary in the husband's returns to the wife's employment and are dimensions husbands could learn about from the observed changes in wives' communication would work.

This change in communication can be interpreted as re-framing information husbands already have, proposing an alternative narrative. An alternative narrative assigns likelihoods $(\pi^a_N, \pi^a_{R_\emptyset}, \pi^a_{R_S}) \in [0,1]^3$ to the realized history, with fit $\Phi = a\,\pi^a_N + b\,\pi^a_{R_\emptyset} + c\,\pi^a_{R_S}$. The wife cannot simply make any claim; her husband adopts the narrative only if it fits the observed history given his priors at least as well as the default narrative, i.e. $\Phi \geq F_d$. If so, he updates to form a posterior belief

$$\mu_p(R_S) \;=\; \frac{c\,\pi^a_{R_S}}{\Phi}.$$

This illustrates the key tradeoff in proposing a narrative: a narrative with better fit (higher Φ) is more likely to out-perform the default model and be adopted, but Φ is in the denominator of the posterior belief expression so, all else equal, a higher Φ is less likely to convince the husband that the state is R_S.

Proposition 1. *The highest posterior belief in R_S that any adoptable narrative can induce is*

$$\overline{\mu} \;=\; \min\left\{1,\; \frac{c}{F_d}\right\}, \tag{8}$$

Furthermore, this is attained under the above assumptions.

Proof. See Appendix D. □

Corollary 1. *Persuasion is successful in securing husbands' support for employment if*

$$k(F_d - c) \;\leq\; c. \tag{9}$$

Proof. See Appendix D. □

Model Implication 2. The training allows the wife to propose a credible model that can induce the husband to believe R_S is sufficiently likely to allow her to work. This is consistent with the result that the treatment made husbands in our subgroup more likely to say their wives would change the husbands' minds in an employment decision, and ultimately raised female labor supply in this group.

Model Implication 3. Persuasion fails if:

1. The husband's prior about R_S, c, is pessimistic enough. The suggestive results presented in Section 6.4 are consistent with this; husbands in our subgroup were not persuaded by the treatment when the wives wanted to work to gain control over money or for a reason that violates norms. We can view such husbands in the model as having pessimistic priors about the wife's work benefiting the household overall, making persuasion less effective.[45]

2. The cost to the husband from the wife working under the other two states, k, is large. An empirical proxy we use for this is the husband's beliefs that his wife's earnings potential is high relative to his own. Again, descriptive results in Section 6.4 are consistent with persuasion failing in this case.

Model Implication 4. The alternative model proposed by the wife is over the two-dimensional history relevant to the household decision (need for resources and her willingness to contribute to joint household resources), implying communication provides relevant information about both dimensions. This is supported by the data in that the treatment motivated women to summarize the situation and provide their reasons for wanting to work. As mentioned above, the states and women's communication need not have been precisely about a need for resources and women's willingness to share earnings; the point is that two-dimensional learning of this general form would predict the particular changes in communication we observe.[46]

8 Conclusion

This paper finds that intra-household communication skills can have large, persistent effects on labor supply when women face opposition from their husbands. Decisions about

[45] One could alternatively view such husbands in the model as having a history that signals N or $R_\emptyset$.

[46] We also see changes in women expressing their emotions. As discussed above, this could be interpreted as lending sincerity to their statements on how they saw the situation and/or their rationales.

women's labor supply are among the most consequential households make, and despite numerous policies aimed at increasing female labor supply, sustained increases have proven difficult to achieve (Heath et al., 2024).

Our findings suggest women face frictions to communicating optimally in the status quo. This is in contrast to standard sender-receiver or household models, which feature communication that is friction-less or at least optimal from the perspective of the agent. In particular, our LCA results suggest women in the control group over-rely on passive communication and are not assertive enough. This friction could result from gender norms in our setting: just 11% of women and 8% of husbands in our control group say it is ok for women to have different opinions from their husbands. Such attitudes could make women reluctant to express their perspectives, a core part of communicating assertively, and instead use more passive approaches. This reluctance would keep women from experimenting and gaining assertive communication skills over time. Our treatment taught women to convey their perspectives to their husbands while still being respectful, and we find these skills were persuasive.

These results are consistent with others in the literature that suggest crafting persuasive narratives is genuinely hard. This includes the expanse of academic work developing such skills (Peneva and Mavrodiev, 2013) as well as research studying what types of arguments can be persuasive in lab (Coffman and Niehaus, 2020; Brandts and Cooper, 2007) and field settings (DellaVigna and Gentzkow, 2010). This suggests that a lack of assertive communication skills is unlikely to be limited to our setting.

Was the increased female employment Pareto-enhancing for the household? It seems reasonable to conclude that women were better off,[47] but it is less clear whether husbands were. Our mechanisms analyses suggest husbands maintained decision rights, but the treatment led women to convey information that changed their husbands' minds about female employment. This should have made husbands better off to the extent that they were better informed. Of course it is possible that women provided biased information, but the persistence of the labor supply effects suggests many husbands did not reverse their decisions after experiencing their wives working.

In Appendix E, we explore whether our treatment was a cost-effective way to raise female employment outcomes. We estimate the per-person marginal cost of our treatment was $17. It only increased labor supply in the woman-more-interested subgroup, but this group could be identified and targeted via baseline screening; adding the cost of our baseline

[47] We note that we do not find effects on women's overall happiness in life in the subgroup that experienced employment increases (Table A.22). It is possible that the framing of this question encouraged respondents to think about their lives in too broad or too historical terms for us to detect changes in happiness resulting from greater labor supply.

survey, we get a per-person cost of $22. We assess cost-effectiveness in two ways. First, we compare the cost to the effect on total earnings in our subgroup (Table 1), finding the average woman recouped half the $22 cost in 10 months of work with the firm. This is a clean measure of cost-effectiveness, but it does not necessarily capture a number of benefits of women's employment that interest policymakers, e.g. women's empowerment, children's wellbeing, and economic growth (Heath et al., 2024). Thus Appendix E also assesses cost-effectiveness in a second way: estimating the cost of raising an employment outcome by one unit with communication training, and comparing this to the same statistic from other interventions previously evaluated in South Asia (Dean and Jayachandran, 2019; de Mel et al., 2014; Garlick et al., 2026; Maitra and Mani, 2017; McKelway, 2025, 2026). We find that communication training is cost-effective among skills or empowerment trainings, though this class of intervention is unlikely to be the most cost-effective available.[48]

Our results have two key policy implications. First, they suggest an approach for improving women's agency and outcomes even when husbands have decision rights. In settings with entrenched gender norms and inequities, improving women's communication skills may be easier and more culturally acceptable than shifting the underlying power dynamics. Second, our findings imply that even when jobs are available for women – the outcome of the many demand-side interventions to boost female employment (Heath et al., 2024) – supply-side constraints within the household may keep women from taking them up. However, a relatively low-cost communication training can overcome such constraints.

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[48]We also note that our intervention was about half the length of the closely-related ones studied in Ashraf et al. (2020) and Björkman Nyqvist et al. (2024) (both discussed in Section 1), speaking to the importance of the assertive communication skills our curriculum taught.

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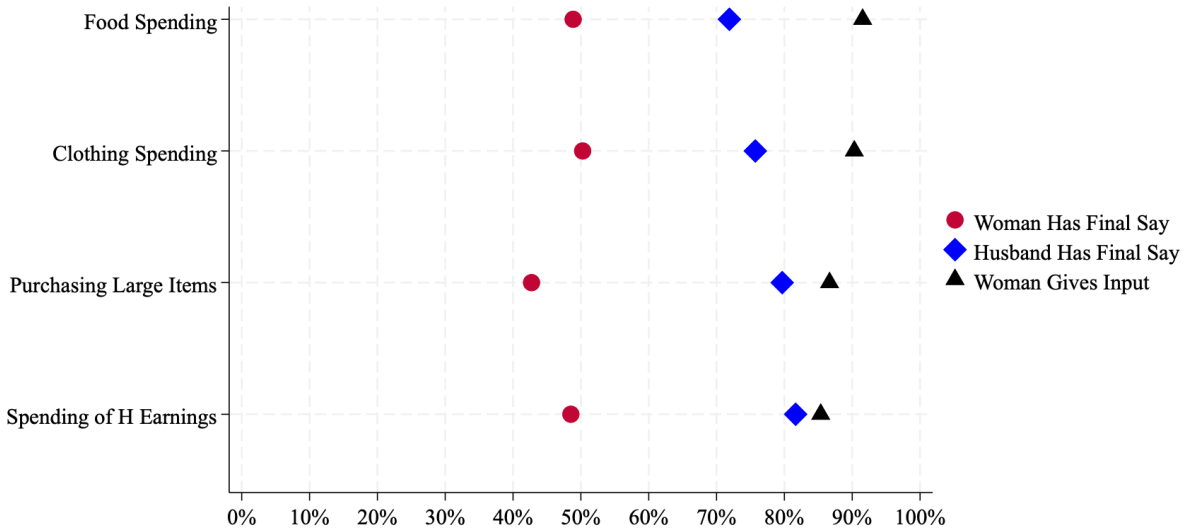
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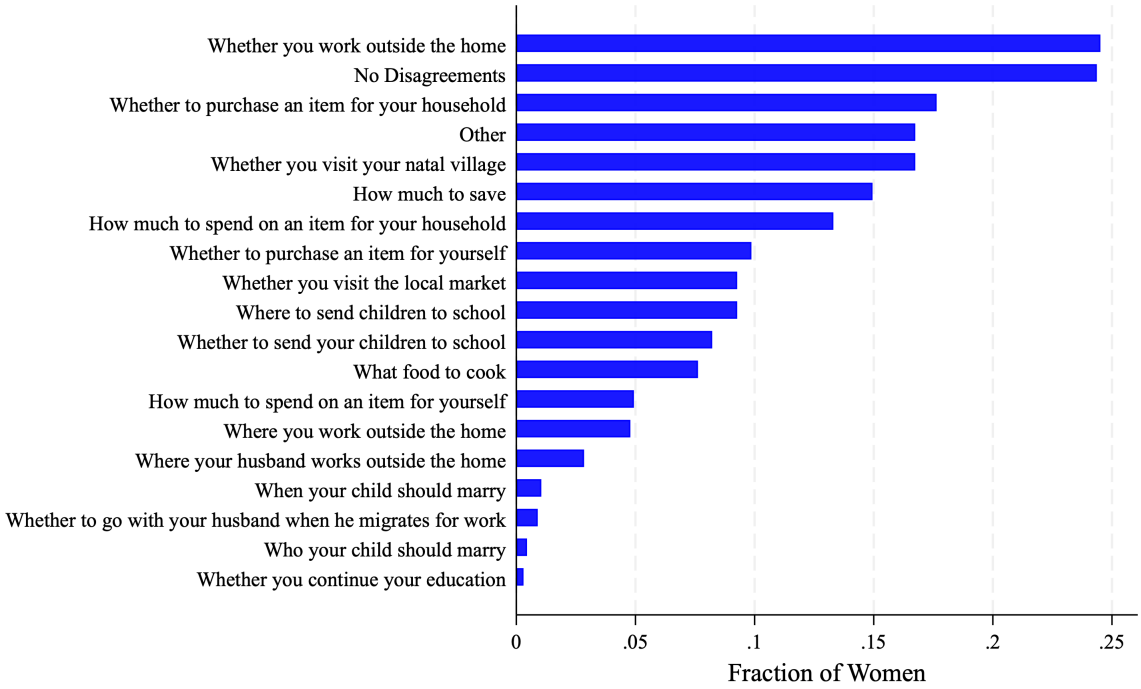
Figures and Tables

Figure 1: Women's Final Say Versus Input



Notes: This figure visualizes women's baseline responses to questions asking who in the household has final say over various decisions and whether the women give input. "Woman (Husband) Has Final Say" means women reported they themselves (their husbands) are at least one of the people with final say. The sample sizes range from 1,526–1,538, based on the number of refusal and not-applicable responses for each question.

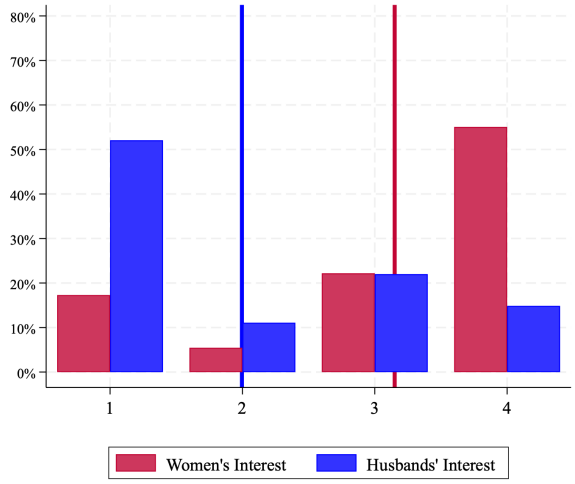
Figure 2: Decisions in Which Couples Hold Different Opinions



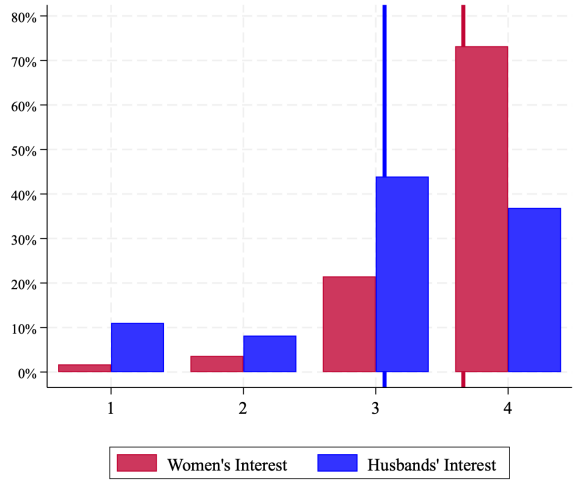
Notes: This figure visualizes the distribution of women's responses to the question asking what decisions they and their husbands often have different opinions about. Responses are from the control group at EL2, and respondents could select multiple responses. The sample size is 669.

Figure 3: Interest in Women Doing Various Activities

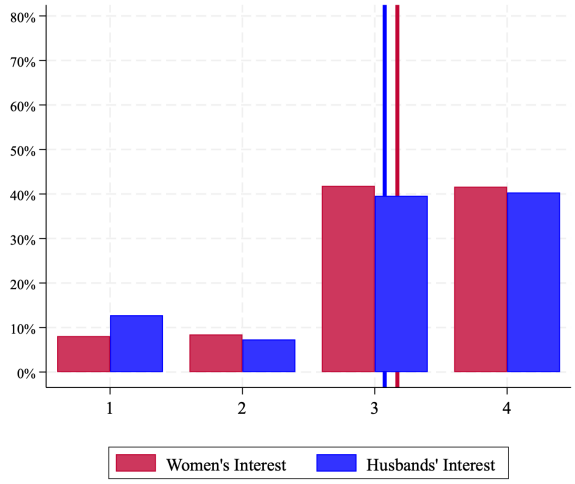
(a) Employment



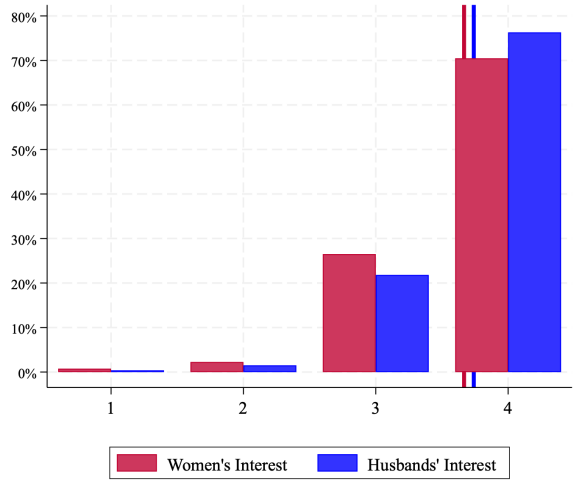
(b) Visit Natal Village



(c) Visit Market

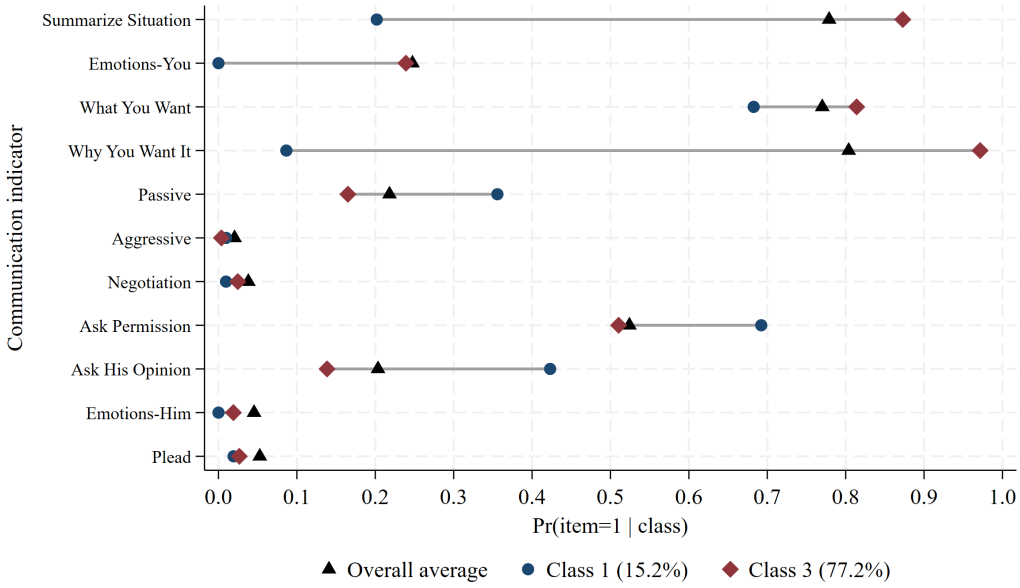


(d) Get New Saree



Notes: This figure visualizes the distributions of women's and husbands' interest in the women working outside the home, visiting their natal village, going to the local market, and getting a new saree. Interest was recorded as: 1 = very uninterested, 2 = somewhat uninterested, 3 = somewhat interested, or 4 = very interested. The bars show the percentages of women and husbands providing a given level of interest, while the solid vertical lines denote average woman and husband interest levels. Data come from EL2 surveys with women and husbands. The sample for each activity is restricted to control-group couples where both spouses provided their interest in the activity, giving samples sizes of 526–533 couples across the activities.

Figure 4: Differences in Communication across LCA Classes: Classes 1 versus 3



Notes: All outcomes are from the vignettes in women's EL2 surveys, and the sample is restricted to those given the employment vignette. See Section 6.1 for details on the vignettes. The variables on the y-axis are binary variables for providing each of the answer options (excluding "other") to the question on what women would say to their husbands. "Summarize Situation" means the woman would summarize the situation. "Emotions-You" means she would describe the emotions she is feeling. "What You Want" and "Why You Want It" mean the woman would tell him what she wants and why she wants it, respectively. "Passive" means telling him she will do whatever he wants. "Aggressive" means telling him he's wrong, unreasonable, stupid, etc. "Negotiation" means offering a compromise. "Ask Permission" and "Ask His Opinion" mean she would ask his permission and ask his opinion, respectively. "Emotions-Him" means she would describe the emotions he is making her feel. "Plead" means she would plead or beg. The black triangles denote the fraction of women in the full sample who provided each communication response, the blue dots the fractions for the sample classified as Class 1, and the red diamonds the fractions for the sample classified as Class 3. Class 2 is not impacted by treatment and is omitted from this figure.

Table 1: Effects on Women's Labor Supply

	Applied (=1) (1)	Applied (=1) (2)	Total Earnings from Firm (Rs.) (3)	Any Emp. (=1) (4)
Treat	0.012	-0.025	-331.592	-0.055
	(0.027)	(0.033)	(423.845)	(0.040)
W More Interested		-0.034	-210.622	-0.039
		(0.028)	(454.519)	(0.046)
Treat x W More Interested		0.085**	1255.665**	0.128**
		(0.041)	(596.417)	(0.059)
P-Val: Treat + Treat x W More = 0		0.095*	0.071*	0.099*
Strata Controls	Yes	Yes	Yes	Yes
Omitted Group Mean	0.192	0.205	1786.210	0.430
N	1540	1523	1523	1344

Notes: The outcome in columns (1) and (2) is an indicator for applying for the partner firm's program. The outcome in column (3) is the total earnings from the firm's program, summed over the 10 months for which we have earnings data. Earnings are in rupees and are set to 0 for non-participants. Earnings that are missing in a given month are imputed using the month-center-treatment-subgroup participant mean. See Sections 4.1 and 5.2 for details on missing earnings and robustness to alternative approaches for handling missing earnings. The outcome in column (4) is from women's EL2 surveys. Women were asked if they had worked in any of a comprehensive list of sectors (including the partner firm's program) to earn income in the preceding three months. Column (4)'s outcome is an indicator for women reporting any work for income off their own household's farm. "W More Interested" is an indicator for women reporting greater interest in their employment than their husbands at baseline. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table 2: Effects on Firm Earnings by Month

	Earnings from Firm (Rs.)									
	Dec.	Jan.	Feb.	Mar.	Apr.	May	Jun.	Jul.	Aug.	Sep.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat	-43.865	-61.523	-62.820	-51.238	-7.283	-45.684	-27.181	17.104	-21.926	-27.176
	(94.465)	(69.832)	(66.330)	(66.083)	(20.338)	(37.576)	(34.470)	(26.996)	(32.440)	(35.443)
W More Interested	-126.780	-62.187	-56.505	-47.495	33.912	5.835	50.375	40.777	-13.075	-35.478
	(94.614)	(71.120)	(66.138)	(65.725)	(25.727)	(43.526)	(50.271)	(36.129)	(37.800)	(34.372)
Treat x W More Interested	266.186**	233.074**	199.455**	184.703**	15.507	80.808	49.313	29.851	83.912*	112.857**
	(123.653)	(97.516)	(88.644)	(87.759)	(36.207)	(55.599)	(59.112)	(44.089)	(45.576)	(49.199)
P-Val: Treat + Treat x W More = 0	0.031**	0.042**	0.060*	0.069*	0.787	0.423	0.667	0.238	0.097*	0.033**
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	428.616	304.424	279.595	269.371	46.443	109.070	95.859	52.599	95.619	104.614
N	1523	1523	1523	1523	1523	1523	1523	1523	1523	1523

Notes: The outcomes are earnings from the firm's program in each of the 10 months for which we have earnings data. Earnings are in rupees and are set to 0 for non-participants. Earnings that are missing in a given month are imputed using the month-center-treatment-subgroup participant mean. See Sections 4.1 and 5.2 for details on missing earnings and robustness to alternative approaches for handling missing earnings. "W More Interested" is an indicator for women reporting greater interest in their employment than their husbands at baseline. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table 3: Robustness of Heterogeneity to Controlling for Confounds

	W More Interested (=1) (1)	Applied (=1) (2)	Total Earnings (Rs.) (3)	Any Emp. (=1) (4)
Panel A: Husband Opposition in Other Decisions				
Treat x W More Interested		0.073*	1389.895**	0.170***
		(0.042)	(643.441)	(0.062)
(Treat x) W More Interested in Other Activities Index	0.139***	0.020	-250.052	-0.068**
	(0.011)	(0.020)	(343.211)	(0.028)
Panel B: Individual Interest in W Employment				
Treat x W More Interested		0.199**	2440.994**	0.137
		(0.078)	(1120.502)	(0.113)
(Treat x) W Interest is 2 (=1)	0.574***	-0.135	-1402.804	0.079
	(0.052)	(0.125)	(1446.440)	(0.137)
(Treat x) W Interest is 3 (=1)	0.579***	-0.088	-817.052	0.063
	(0.029)	(0.079)	(1149.688)	(0.111)
(Treat x) W Interest is 4 (=1)	0.900***	-0.143*	-1890.526	0.036
	(0.027)	(0.084)	(1255.398)	(0.120)
(Treat x) H Interest is 2 (=1)	-0.049*	0.072	992.014	0.171*
	(0.025)	(0.072)	(1059.485)	(0.101)
(Treat x) H Interest is 3 (=1)	-0.311***	0.114*	1891.063**	0.110
	(0.019)	(0.061)	(936.981)	(0.076)
(Treat x) H Interest is 4 (=1)	-0.963***	0.180**	1811.301	0.109
	(0.018)	(0.089)	(1291.845)	(0.119)
Panel C: Lasso-Selected Variables				
Treat x W More Interested		0.087**	1470.953**	0.159***
		(0.041)	(640.988)	(0.059)
(Treat x) W Gives Input on Spending W Earnings (=1)	-0.057	0.080	594.305	0.020
	(0.043)	(0.076)	(1148.618)	(0.093)
(Treat x) H Interest (1-4) in W Visiting Natal Village	-0.091***	-0.015	239.722	0.057*
	(0.014)	(0.020)	(266.733)	(0.030)
(Treat x) H Interest (1-4) in W Visiting Market	-0.041***	0.028	351.008	0.013
	(0.014)	(0.023)	(366.842)	(0.032)
(Treat x) H Interest (1-4) in W Getting New Saree	-0.037**	-0.046	-172.705	-0.000
	(0.018)	(0.030)	(510.331)	(0.038)
(Treat x) HH Makes Decisions on Spending W Earnings (=1)	0.075*	0.022	1131.495	0.054
	(0.046)	(0.079)	(1188.690)	(0.091)
(Treat x) From Yadav Subcaste (=1)	-0.227***	0.027	483.425	0.092
	(0.046)	(0.038)	(471.873)	(0.101)
Treat	No	Yes	Yes	Yes
W More Interested	No	Yes	Yes	Yes
Strata Controls	No	Yes	Yes	Yes
Predictors of W More Interested	No	Yes	Yes	Yes
N	1523	1523	1523	1344

Notes: This table explores the robustness of our heterogeneity results to controlling for predictors of "W More Interested," the variable defining our main subgroup of interest. The predictor in Panel A is an index of reported husband opposition in other activities. Specifically, it is an index of three indicators for women reporting greater interest than their husbands at baseline in the women: visiting their natal village, visiting the market, and getting a new saree. The predictors in Panel B are indicators for women's own level of interest in women's employment at baseline, and for their predictions of their husbands' interest in women's employment at baseline. Interest was reported on a 1-4 scale and indicators for each level but 1 (the omitted group) are included. The predictors in Panel C are selected by Lasso from our baseline data. Column (1) presents regressions of "W More Interested" on each set of predictors. Columns (2)-(4) present the interaction effects from Table 1 controlling for each set of predictors and their interactions with treatment. These columns show the coefficients on our main interaction effect, along with the coefficients on the treatment-predictor interactions; Treat, W More Interested, strata controls, and the un-interacted predictors are included in the regressions but their coefficients not shown. Standard errors are robust in column (1) and clustered by meeting group in the rest of the columns. * p <0.10, ** p < 0.05, *** p < 0.01.

Table 4: Effects on Women's Communication in Employment Vignette

	% Assertive Used	Use Assertive Response (=1):				Change His Mind (=1)	Probability of LCA Class:		
		Summarize Situation	Describe Emotions	Tell Him What	Tell Him Why		1	2	3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treat	5.027**	0.034	-0.015	0.086**	0.095**	0.088**	-0.042	-0.011	0.053
	(2.473)	(0.044)	(0.046)	(0.042)	(0.040)	(0.043)	(0.034)	(0.024)	(0.035)
W More Interested	-0.204	-0.027	-0.014	0.010	0.023	-0.022	0.026	0.009	-0.036
	(3.030)	(0.047)	(0.048)	(0.051)	(0.051)	(0.053)	(0.041)	(0.026)	(0.041)
Treat x W More Interested	3.355	0.065	0.145**	-0.077	0.000	-0.008	-0.061	0.035	0.025
	(3.713)	(0.062)	(0.066)	(0.064)	(0.060)	(0.066)	(0.049)	(0.037)	(0.053)
P-Val: Treat + Treat x W More = 0	0.004***	0.037**	0.011**	0.843	0.039**	0.105	0.006***	0.393	0.065*
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	61.938	0.758	0.242	0.736	0.742	0.725	0.192	0.103	0.705
N	678	678	678	678	678	678	678	678	678

Notes: All outcomes are from the vignettes in women's EL2 surveys, and the sample is restricted to those given the employment vignette. See Section 6.1 for details on the vignettes. The outcomes in columns (1)-(5) are from the question asking women what they would say to their husbands. Assertive communication is represented by women: summarizing the situation, describing the emotions they are feeling, telling their husbands what they want, and telling their husbands why they want it. The outcome in column (1) is the percentage of these four responses women provided, while the outcomes in columns (2)-(5) are indicators for providing each of the four. The outcome in column (6) is an indicator for women saying they thought they would ultimately change their husbands' minds. The outcomes in columns (7)-(9) are from the three-class LCA, selected by BIC. The three outcomes are the probabilities that the respondent is in class 1, 2, and 3, respectively. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table 5: Husbands' Reports of Wives' Communication in Employment Vignette

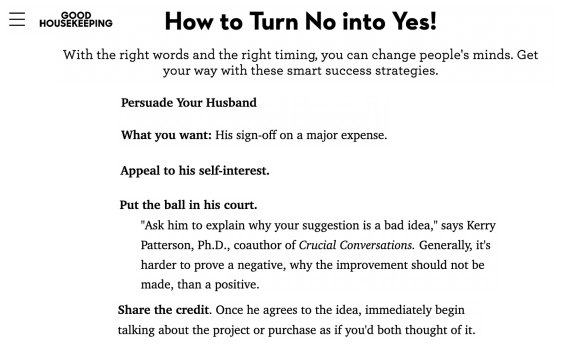
	% Assertive Used	Change His Mind (=1)		
			Unconvincing = Earnings Threat	*Unconvincing = W's Reasons*
	(1)	(2)	(3)	(4)
Treat	3.427	0.052	0.033	0.053
	(2.895)	(0.058)	(0.078)	(0.086)
W More Interested	-1.238	0.022	-0.068	-0.040
	(3.001)	(0.059)	(0.080)	(0.096)
Treat x W More Interested	-4.335	0.043	0.155	0.123
	(4.281)	(0.075)	(0.102)	(0.118)
Unconvincing			-0.045	0.027
			(0.082)	(0.081)
Treat x Unconvincing			0.111	0.018
			(0.114)	(0.111)
W More x Unconvincing			0.192	0.138
			(0.119)	(0.125)
Treat x W More x Unconvincing			-0.272*	-0.160
			(0.156)	(0.164)
P-Val: Treat + Treat x W More = 0	0.783	0.097*	0.030**	0.052*
P-Val: Treat + Treat x W More + Treat x Unconvincing + Treat x W More x Unconvincing = 0			0.722	0.702
Strata Controls	Yes	Yes	Yes	Yes
Omitted Group Mean	58.854	0.667	0.667	0.656
N	542	545	505	491

Notes: The outcomes are from the vignettes in husbands' EL2 surveys, and the sample is restricted to those given the employment vignette. See Section 6.1 for details on the vignettes. The outcome in column (1) is the percentage of the four assertive responses husbands thought their wives would provide; it is the same outcome as in column (1) of Table 4, except it is based on husbands' rather than wives' reports. The outcome in columns (2)-(4) is an indicator for husbands saying they thought their wives would ultimately change the husbands' minds. "Unconvincing" is a proxy for women's narratives being unconvincing to husbands, as discussed in Section 6.4. In column (3), "Unconvincing" is based on husbands' beliefs at EL2 of how much they and their wives could make if they worked for a month; it is an indicator for the husband's minus wife's earnings potential being below median. In column (4), "Unconvincing" is an indicator for the wife saying at EL1 that she wanted to work to have money in her control or to spend time out of the house. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Online Appendix A: Additional Tables and Figures

Figure A.1: Communication Strategies Recommended to Wives in Popular Culture

(a) Example 1, from Good Housekeeping


GOOD HOUSEKEEPING

How to Turn No into Yes!

With the right words and the right timing, you can change people's minds. Get your way with these smart success strategies.

Persuade Your Husband

What you want: His sign-off on a major expense.

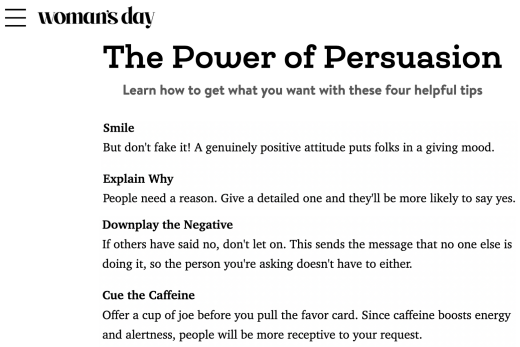
Appeal to his self-interest.

Put the ball in his court.

"Ask him to explain why your suggestion is a bad idea," says Kerry Patterson, Ph.D., coauthor of *Crucial Conversations.* Generally, it's harder to prove a negative, why the improvement should not be made, than a positive.

Share the credit. Once he agrees to the idea, immediately begin talking about the project or purchase as if you'd both thought of it.

(b) Example 2, from Woman's Day


woman's day

The Power of Persuasion

Learn how to get what you want with these four helpful tips

Smile

But don't fake it! A genuinely positive attitude puts folks in a giving mood.

Explain Why

People need a reason. Give a detailed one and they'll be more likely to say yes.

Downplay the Negative

If others have said no, don't let on. This sends the message that no one else is doing it, so the person you're asking doesn't have to either.

Cue the Caffeine

Offer a cup of joe before you pull the favor card. Since caffeine boosts energy and alertness, people will be more receptive to your request.

(c) Example 3, from Ladies' Home Journal


LADIES' HOME JOURNAL | JULY 2005

THE POWER OF EYE CONTACT

Getting people to like you is as easy as looking into their eyes. Women who fix their gaze on the people they talk to are perceived as more likable than those who don't, found a new study from Dartmouth College, in Hanover, New Hampshire. "It tells you that you're more interesting than anything else to that person," explains Malia Mason, Ph.D. candidate and lead author of the study.

That's just the start: Making eye contact also boosts your powers of persuasion. If other people find you likable, says Mason, they're more likely to pay attention and be convinced by what you say. So if you're trying to get your husband to commit to helping with your garage sale, for example, it may be efficient to phone him at work, but Mason says he'll be more likely to lend a hand if you wait till you can talk to him face-to-face. *—Meredith Bodgas*

(d) Example 4, from Ladies' Home Journal


Ladies' Home Journal □ *February, 1968*

"HUSBANDESE"—THE NEW LANGUAGE OF INTIMATE PERSUASION BETWEEN HUSBAND AND WIFE □ BY DR. HAIM GINOTT

Figure A.2: Study Timeline

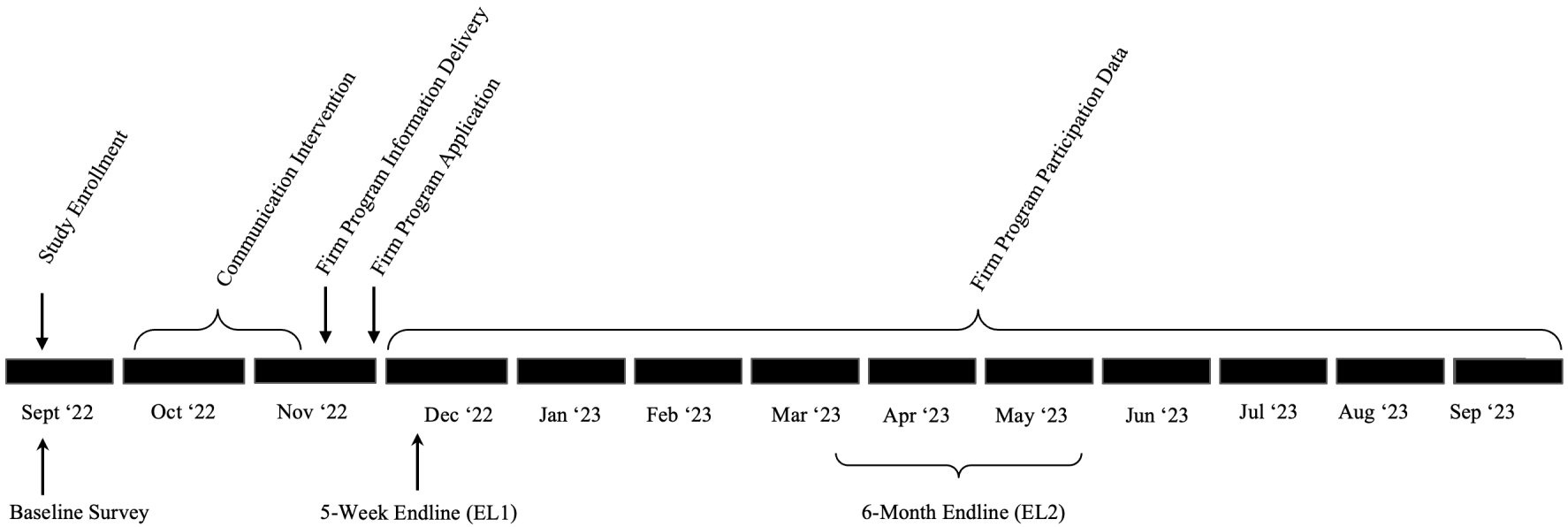


Figure A.3: See-Feel-Want Visual from Communication Curriculum

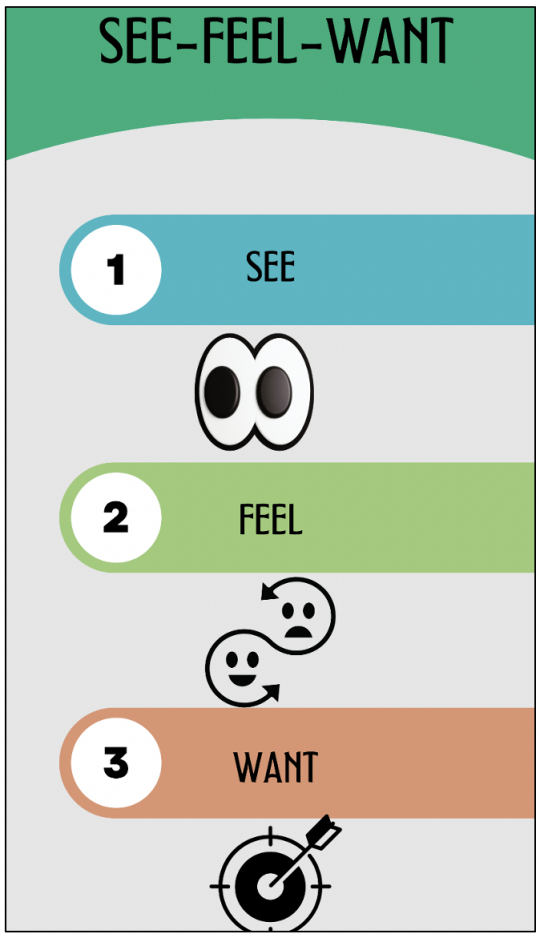
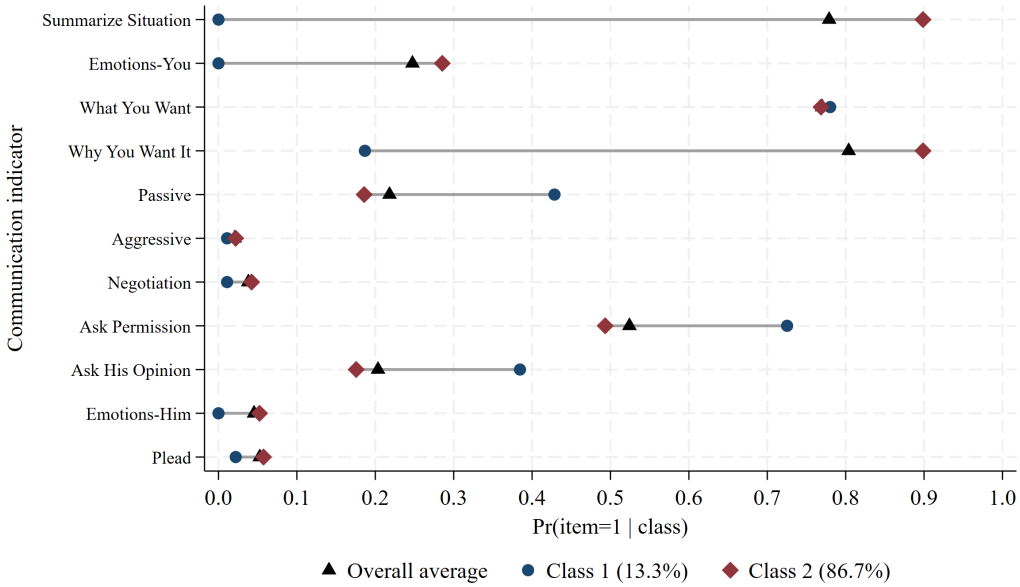


Figure A.4: Differences in Communication across LCA Classes, Two-Class Version



Notes: All outcomes are from the vignettes in women's EL2 surveys, and the sample is restricted to those given the employment vignette. See Section 6.1 for details on the vignettes. This figure presents the same analysis as 4, except it is based on the two-class LCA preferred by entropy rather than the three-class LCA preferred by BIC. The black triangles denote the fraction of women in the full sample who provided each communication response, the blue dots the fractions for the sample classified as Class 1, and the red diamonds the fractions for the sample classified as Class 2.

Table A.1: Baseline Characteristics and Balance

	Full Sample			*W Not More Interested*			*W More Interested*		
	C Mean (SD) (1)	T – C (SE) (2)	N (3)	C Mean (SD) (4)	T – C (SE) (5)	N (6)	C Mean (SD) (7)	T – C (SE) (8)	N (9)
Age	29.717	0.285	1540	29.961	0.056	866	29.364	0.426	657
	(5.834)	(0.295)		(6.007)	(0.370)		(5.531)	(0.473)	
No Education (=1)	0.407	-0.030	1539	0.428	-0.056	865	0.373	-0.002	657
	(0.492)	(0.027)		(0.495)	(0.036)		(0.484)	(0.040)	
Class 1-8 Education (=1)	0.291	0.024	1539	0.261	0.057	865	0.332	-0.023	657
	(0.454)	(0.025)		(0.440)	(0.035)		(0.472)	(0.038)	
Class 9-12 Education (=1)	0.213	0.011	1539	0.211	0.019	865	0.218	0.009	657
	(0.410)	(0.020)		(0.408)	(0.027)		(0.414)	(0.031)	
Beyond Class 12 Education (=1)	0.089	-0.005	1539	0.101	-0.021	865	0.076	0.017	657
	(0.286)	(0.017)		(0.301)	(0.022)		(0.265)	(0.021)	
From Scheduled Caste or Tribe (=1)	0.368	-0.026	1540	0.304	0.002	866	0.453	-0.077*	657
	(0.483)	(0.038)		(0.460)	(0.041)		(0.499)	(0.046)	
Mother(-in-Law) in HH (=1)	0.420	0.016	1540	0.422	0.042	866	0.418	-0.013	657
	(0.494)	(0.029)		(0.495)	(0.038)		(0.494)	(0.040)	
Father(-in-Law) in HH (=1)	0.376	0.019	1540	0.372	0.059	866	0.380	-0.034	657
	(0.485)	(0.028)		(0.484)	(0.037)		(0.486)	(0.039)	
Number of Adults in HH	4.030	0.064	1540	4.169	0.067	866	3.839	0.057	657
	(2.500)	(0.158)		(2.598)	(0.201)		(2.367)	(0.175)	
Number of Children	2.826	-0.001	1526	2.778	0.003	860	2.892	-0.021	650
	(1.444)	(0.077)		(1.459)	(0.101)		(1.426)	(0.115)	
Pregnant (=1)	0.071	0.000	1540	0.064	0.021	866	0.079	-0.024	657
	(0.257)	(0.014)		(0.245)	(0.019)		(0.270)	(0.019)	
Percent Assertive Responses in BL Vignette	52.030	-0.407	1494	49.645	2.166	836	55.323	-3.053	643
	(35.631)	(2.027)		(36.182)	(2.730)		(34.777)	(2.824)	
W's Interest (1-4) in Employment	3.282	-0.087	1535	2.993	-0.161	866	3.690	-0.015	657
	(1.050)	(0.064)		(1.213)	(0.099)		(0.557)	(0.049)	
H's Interest (1-4) in W Employment (W Predicted)	2.656	-0.106	1526	3.281	-0.164*	866	1.791	0.034	657
	(1.259)	(0.072)		(1.106)	(0.091)		(0.895)	(0.070)	
W More Interested in Employment (=1)	0.419	0.016	1523	0.000	0.000	866	1.000	0.000	657
	(0.494)	(0.027)		(0.000)	(0.000)		(0.000)	(0.000)	
Any Employment (=1)	0.334	-0.018	1527	0.345	-0.004	857	0.325	-0.042	654
	(0.472)	(0.026)		(0.476)	(0.039)		(0.469)	(0.039)	
Work in Agriculture, Off Own HH Land (=1)	0.193	-0.035	1527	0.208	-0.044	857	0.175	-0.024	654
	(0.395)	(0.022)		(0.407)	(0.031)		(0.381)	(0.034)	
Women's Activities Index	-0.000	0.002	1538	-0.025	0.031	865	0.029	-0.015	657
	(1.000)	(0.054)		(1.040)	(0.074)		(0.946)	(0.076)	
F-Test P-Val		0.730			0.195			0.833	

Notes: Data in this table come from the baseline survey. Column (1) presents the means and standard deviations of given baseline variables in the control group. Column (2) presents the coefficients and standard errors from regressions of each baseline variable on a treatment indicator. The regressions include strata controls and cluster standard errors by meeting group. The sample sizes for each regression are in column (3). The final row presents the p-value for joint significance of all baseline variables, from a regression of treatment on the baseline variables and strata controls, with standard errors clustered by meeting group. The same analyses in the subgroup of women who were less or equally interested in their employment than their husbands are in columns (4)-(6), while the corresponding analyses for the woman-more-interested subgroup are in columns (7)-(9). * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.2: Descriptive Statistics on Husbands, Control Group Only

	Unweighted		*Weighted, v1*		*Weighted, v2*	
	Mean (SD)	N	Mean	N	Mean	N
	(1)	(2)	(3)	(4)	(5)	(6)
Age	33.787 (6.742)	751	33.708	751	33.877	663
No Education (=1)	0.097 (0.296)	567	0.095	567	0.098	533
Class 1-8 Education (=1)	0.390 (0.488)	567	0.388	567	0.391	533
Class 9-12 Education (=1)	0.363 (0.481)	567	0.364	567	0.367	533
Beyond Class 12 Education (=1)	0.150 (0.357)	567	0.154	567	0.144	533
Any Employment (=1)	0.959 (0.197)	567	0.960	567	0.966	533
Casual Non-Farm Work (=1)	0.508 (0.500)	567	0.500	567	0.507	533
Employed by a Firm (=1)	0.339 (0.474)	567	0.348	567	0.347	533
Work in Own HH Microenterprise (=1)	0.194 (0.396)	567	0.194	567	0.195	533
Potential Monthly Income (Rs)	15744.071 (10878.704)	565	15989.930	565	15806.814	531
Migrated for Work in Last Year (=1)	0.471 (0.500)	671	0.474	671		
N Months of Migrant Work in Last Year	3.355 (4.270)	670	3.376	670		
OK for Women to Work (=1)	0.654 (0.476)	567	0.655	567	0.658	533
A Husband Should Earn More than Wife (=1)	0.912 (0.284)	566	0.909	566	0.910	532
Husbands Know Better than Wives What's Best for Family (=1)	0.481 (0.500)	567	0.482	567	0.475	533
OK if Women Have Diff Opinions than Husb (=1)	0.079 (0.271)	567	0.079	567	0.082	533

Notes: Column (1) presents means and standard deviations for variables on husbands. The sample is limited to the control group, with sample sizes in column (2). Husbands' age comes from the baseline survey, the two variables on husbands' migration come from women's EL2 surveys, and the rest come from husbands' EL2 surveys. Columns (3)-(6) present the descriptive statistics with entropy balancing (Hainmueller, 2012) to adjust for attrition. In columns (3)-(4), weights are generated such that weighted averages of the baseline characteristics in Table A.1 among each estimation sample here exactly match the unweighted averages for the full control group of N = 761. For this exercise, the small number of missing values in the baseline variables from Table A.1 are set to the median. In columns (5)-(6), weights are instead generated to match the averages of the two migration variables in column (1). These columns omit observations with missing values for either migration variable.

Table A.3: Attrition in Partner Firm Earnings

	Earnings from Firm Missing (=1)							Total Earnings (Rs.)
	Ever (1)	Dec. (2)	Mar. (3)	Apr. (4)	May (5)	Jun. (6)	Aug. (7)	No-Missing Mon. Only (8)
Treat	-0.009	0.001	-0.003	0.000	-0.006	-0.006	-0.000	-134.415
	(0.015)	(0.006)	(0.004)	(0.000)	(0.005)	(0.008)	(0.010)	(179.119)
W More Interested	-0.016	-0.008**	-0.007*	0.000	0.001	0.003	-0.005	-113.393
	(0.014)	(0.004)	(0.004)	(0.000)	(0.008)	(0.009)	(0.007)	(184.095)
Treat x W More Interested	0.028	0.001	0.008	0.003	0.002	0.011	0.012	575.237**
	(0.018)	(0.007)	(0.005)	(0.003)	(0.008)	(0.013)	(0.009)	(250.326)
P-Val: Treat + Treat x W More = 0	0.201	0.453	0.174	0.314	0.558	0.677	0.140	0.041**
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	0.046	0.007	0.007	0.000	0.007	0.014	0.016	741.232
N	1523	1523	1523	1523	1523	1523	1523	1523

Notes: The outcome in column (1) is an indicator for partner firm earnings ever being missing across the 10 months for which we have data. The outcomes in columns (2)-(7) are indicators for earnings being missing in a given month. January, February, July, and September are not included because earnings are never missing in those months. Column (8) presents effects on total earnings when we add up earnings only in these four months with no missingness. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.4: Endline Survey Attrition

	Full Sample					Employment Vignette			
	W EL1 Surveyed (=1)	W EL2 Surveyed (=1)	H EL2 Surveyed (=1)	W & H EL2 Surveyed (=1)	Played Games (=1)	W EL1 Surveyed (=1)	W EL2 Surveyed (=1)	H EL2 Surveyed (=1)	W & H EL2 Surveyed (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Overall Effects									
Treat	0.007	-0.014	-0.019	-0.028	-0.042	-0.001	0.006	0.035	0.035
	(0.016)	(0.019)	(0.026)	(0.027)	(0.026)	(0.023)	(0.024)	(0.036)	(0.036)
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	0.909	0.882	0.745	0.700	0.723	0.903	0.896	0.719	0.676
N	1540	1540	1540	1540	1540	770	760	760	760
Panel B: Heterogeneous Effects									
Treat	0.003	0.009	-0.013	-0.013	-0.030	-0.021	0.032	0.054	0.072
	(0.021)	(0.024)	(0.030)	(0.032)	(0.033)	(0.030)	(0.031)	(0.046)	(0.049)
W More Interested	0.003	0.023	-0.038	-0.029	-0.003	-0.018	0.023	0.005	0.027
	(0.023)	(0.021)	(0.032)	(0.033)	(0.035)	(0.031)	(0.034)	(0.048)	(0.052)
Treat x W More Interested	0.012	-0.052*	-0.004	-0.026	-0.024	0.049	-0.052	-0.037	-0.077
	(0.029)	(0.031)	(0.045)	(0.047)	(0.049)	(0.042)	(0.042)	(0.063)	(0.067)
P-Val: Treat + Treat x W More = 0	0.489	0.100	0.662	0.332	0.170	0.390	0.530	0.727	0.925
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	0.909	0.870	0.753	0.703	0.728	0.910	0.883	0.712	0.659
N	1523	1523	1523	1523	1523	764	754	754	754

Notes: The outcomes in columns (1)-(4) are indicators for women being surveyed at EL1, women being surveyed at EL2, husbands being surveyed at EL2, and both spouses being surveyed at EL2. Column (5)'s outcome is an indicator for the couple completing the lab-in-the-field games. The outcomes in columns (6)-(9) are the same as in columns (1)-(4), but the samples in columns (6)-(9) are restricted to individuals assigned the employment vignette at a given endline. See Section 6.1 for details on the vignettes. Columns (7)-(9) additionally exclude 21 women who requested to leave the study before EL2 and were not assigned a vignette topic. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.5: Main Results from Endline Surveys with Entropy Balancing for Attrition

	Labor Supply	Women's Reports						Husbands' Reports	
	Any Emp. (=1) (1)	% Assertive Used (2)	Summarize Situation (=1) (3)	Describe Emotions (=1) (4)	Tell Him What (=1) (5)	Tell Him Why (=1) (6)	Change His Mind (=1) (7)	% Assertive Used (8)	Change His Mind (=1) (9)
Panel A: Treatment-Control Balance									
Treat	-0.042	4.792*	0.033	-0.021	0.082*	0.097**	0.110**	3.325	0.097
	(0.041)	(2.751)	(0.047)	(0.049)	(0.045)	(0.044)	(0.047)	(2.967)	(0.063)
W More Interested	-0.026	-1.507	-0.043	-0.041	0.015	0.009	-0.045	-1.343	0.095
	(0.047)	(3.412)	(0.056)	(0.050)	(0.053)	(0.059)	(0.064)	(3.374)	(0.068)
Treat x W More Interested	0.117*	4.637	0.082	0.172**	-0.082	0.013	0.013	-4.205	-0.027
	(0.060)	(4.071)	(0.069)	(0.067)	(0.067)	(0.067)	(0.075)	(4.484)	(0.082)
P-Val: Treat + Treat x W More = 0	0.097*	0.002***	0.028**	0.002***	0.988	0.030**	0.027**	0.799	0.261
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean, Weighted	0.419	62.189	0.757	0.257	0.736	0.737	0.702	58.956	0.607
N	1344	678	678	678	678	678	678	542	545
Panel B: Full Sample Balance									
Treat	-0.050	5.457**	0.044	-0.013	0.088**	0.099**	0.095**	3.411	0.050
	(0.040)	(2.473)	(0.043)	(0.046)	(0.042)	(0.040)	(0.043)	(2.912)	(0.059)
W More Interested	-0.038	-0.208	-0.025	-0.016	0.008	0.024	-0.015	-1.407	0.025
	(0.045)	(3.061)	(0.048)	(0.048)	(0.050)	(0.051)	(0.054)	(3.034)	(0.060)
Treat x W More Interested	0.122**	3.266	0.058	0.146**	-0.071	-0.003	-0.015	-4.551	0.052
	(0.059)	(3.747)	(0.062)	(0.066)	(0.064)	(0.061)	(0.067)	(4.320)	(0.076)
P-Val: Treat + Treat x W More = 0	0.104	0.003***	0.036**	0.010***	0.709	0.038**	0.104	0.732	0.081*
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean, Weighted	0.423	61.780	0.755	0.242	0.733	0.741	0.717	59.075	0.665
N	1344	678	678	678	678	678	678	542	545

Notes: This table replicates the main results from the endline surveys using entropy weighting (Hainmueller, 2012) to adjust for attrition. Column (1) replicates analyses from Table 1, columns (2)-(7) replicate analyses from Table 4, and columns (8)-(9) replicate analyses from Table 5. Two different versions of entropy balancing are implemented in the two panels. In Panel A, entropy weights are generated to produce exact balance between treatment and control non-attritors on the baseline characteristics in Table A.1, with missing values in the baseline variables set to the median. Weights are generated separately by subgroup to produce exact balance within each group. In Panel B, weights are generated to produce exact balance on the baseline characteristics between the sample in a given regression and the full sample of 1,540. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.6: Main Communication Analyses among Couples with Both Husband and Wife Surveyed at EL2

	Women's Reports						Husbands' Reports	
	% Assertive Used (1)	Summarize Situation (=1) (2)	Describe Emotions (=1) (3)	Tell Him What (=1) (4)	Tell Him Why (=1) (5)	Change His Mind (=1) (6)	% Assertive Used (7)	Change His Mind (=1) (8)
Treat	4.320	0.010	-0.027	0.095**	0.094**	0.096**	3.131	0.051
	(2.769)	(0.047)	(0.055)	(0.046)	(0.046)	(0.048)	(2.969)	(0.061)
W More Interested	0.835	-0.020	0.005	0.031	0.018	-0.040	-1.385	0.024
	(3.405)	(0.053)	(0.059)	(0.057)	(0.058)	(0.060)	(3.008)	(0.061)
Treat x W More Interested	3.840	0.074	0.145*	-0.070	0.005	0.054	-3.771	0.054
	(4.200)	(0.070)	(0.077)	(0.072)	(0.068)	(0.074)	(4.354)	(0.078)
P-Val: Treat + Treat x W More = 0	0.013**	0.120	0.050**	0.646	0.059*	0.008***	0.848	0.080*
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	62.689	0.773	0.265	0.727	0.742	0.720	59.280	0.667
N	522	522	522	522	522	522	515	517

Notes: This table replicates the main analyses of women's communication, but restricts to couples in which both the husband and wife were surveyed at EL2. Columns (1)-(6) replicate analyses from Table 4, while columns (7)-(8) replicate analyses from Table 5. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.7: Actual Versus Predicted Husband Interest

	Applied (=1) (1)	Total Earnings from Firm (Rs.) (2)	Any Emp. (=1) (3)
H Interest (1-4), EL2 H Report	0.043***	515.219**	0.044**
	(0.016)	(236.952)	(0.019)
H Interest (1-4), EL2 W Report	0.063***	758.681***	0.062***
	(0.016)	(232.235)	(0.018)
P-Val: H Report = W Report	0.422	0.519	0.525
Outcome Mean	0.214	1797.534	0.415
N	533	526	533

Notes: The sample is restricted to the control group. The outcomes are our main women's labor supply outcomes from Table 1: an indicator for applying to the firm's program, total amount earned in the program over the 10 months for which we have data (in rupees, set to 0 for non-participants), and an indicator for employment in the last three months at EL2. The regressors are husbands' interest in their wives working outside the home at EL2. The first regressor is husbands' own reports of interest, while the second is women's reports of their husbands' interest. Interest was assessed on a 1-4 scale (1 = very uninterested, 2 = somewhat uninterested, 3 = somewhat interested, and 4 = very interested). Standard errors are robust. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.8: Main Results Without Strata Controls

	Labor Supply			Women's Reports						Husbands' Reports	
	Applied (=1) (1)	Firm Earnings (Rs.) (2)	Any Emp. (=1) (3)	% Assertive Used (4)	Summarize Situation (=1) (5)	Describe Emotions (=1) (6)	Tell Him What (=1) (7)	Tell Him Why (=1) (8)	Change His Mind (=1) (9)	% Assertive Used (10)	Change His Mind (=1) (11)
Treat	-0.023	-480.810	-0.065	4.519*	0.020	-0.031	0.088*	0.103**	0.099**	2.637	0.037
	(0.033)	(396.297)	(0.041)	(2.539)	(0.042)	(0.045)	(0.045)	(0.042)	(0.044)	(2.947)	(0.056)
W More Interested	-0.035	-328.056	-0.045	0.395	0.002	-0.035	0.017	0.032	-0.011	-0.665	0.017
	(0.028)	(452.968)	(0.046)	(3.007)	(0.047)	(0.047)	(0.051)	(0.052)	(0.054)	(3.021)	(0.059)
Treat x W More Interested	0.084*	1251.870**	0.141**	2.684	0.054	0.162**	-0.093	-0.015	-0.016	-4.628	0.058
	(0.043)	(610.617)	(0.060)	(3.828)	(0.064)	(0.066)	(0.066)	(0.063)	(0.067)	(4.274)	(0.075)
P-Val: Treat + Treat x W More = 0	0.124	0.134	0.084*	0.016**	0.132	0.009***	0.918	0.067*	0.090*	0.551	0.077*
Strata Controls	No	No	No	No	No	No	No	No	No	No	No
Omitted Group Mean	0.205	1786.210	0.430	61.938	0.758	0.242	0.736	0.742	0.725	58.854	0.667
N	1523	1523	1344	678	678	678	678	678	678	542	545

Notes: This table presents our main results without any strata controls. Columns (1)-(3) replicate analyses from Table 1, columns (4)-(9) replicate analyses from Table 4, and columns (10)-(11) replicate analyses from Table 5. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.9: Compliance and Learning

	Compliance			See-Feel-Want Knowledge, EL1				See-Feel-Want Knowledge, EL2			
	Attended Any Meet. (=1)	N Meet. Attended (0-6)	Info. Delivered (=1)	% Parts Known	Knows See (=1)	Knows Feel (=1)	Knows Want (=1)	% Parts Known	Knows See (=1)	Knows Feel (=1)	Knows Want (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Overall Effects											
Treat	-0.001	0.019	0.035**	30.120***	0.301***	0.303***	0.299***	12.566***	0.090***	0.157***	0.129***
	(0.017)	(0.108)	(0.015)	(1.844)	(0.021)	(0.022)	(0.022)	(1.769)	(0.017)	(0.022)	(0.023)
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	0.901	4.607	0.882	3.613	0.025	0.025	0.059	5.856	0.035	0.039	0.102
N	1540	1540	1540	1406	1406	1406	1406	1352	1352	1352	1352
Panel B: Heterogeneous Effects											
Treat	-0.004	0.015	0.029	30.155***	0.307***	0.304***	0.294***	10.011***	0.071***	0.128***	0.102***
	(0.022)	(0.142)	(0.020)	(2.439)	(0.027)	(0.027)	(0.029)	(2.084)	(0.021)	(0.026)	(0.027)
W More Interested	0.005	0.054	0.008	0.824	0.001	0.003	0.021	0.324	-0.007	-0.007	0.024
	(0.021)	(0.141)	(0.020)	(1.198)	(0.013)	(0.013)	(0.018)	(1.382)	(0.015)	(0.015)	(0.023)
Treat x W More Interested	0.003	0.015	0.019	-0.549	-0.018	-0.009	0.010	5.407*	0.041	0.064*	0.058
	(0.031)	(0.206)	(0.029)	(3.373)	(0.038)	(0.038)	(0.041)	(2.852)	(0.032)	(0.035)	(0.039)
P-Val: Treat + Treat x W More = 0	0.981	0.851	0.029**	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	0.902	4.596	0.879	3.434	0.025	0.025	0.053	5.981	0.040	0.045	0.095
N	1523	1523	1523	1392	1392	1392	1392	1336	1336	1336	1336

Notes: The outcome in column (1) is an indicator for women attending at least one of their six assigned treatment/control sessions, while the outcome in column (2) is the number of assigned sessions attended out of a total of six sessions. The outcome in column (3) is an indicator for the information about the partner firm's program being successfully delivered to women. The outcomes in columns (4)-(7) are from women's EL1 surveys, while those in columns (8)-(11) are from women's EL2 surveys. At both endlines, women were asked if they had heard of a see-feel-want statement, which they were told was a topic discussed in some of the group meetings held by the research team. If they said yes, they were then asked what the three parts of a see-feel-want statement are. The outcomes in columns (5)-(7) and (9)-(11) take the value of 1 if women provided a given part at a given endline; they take the value of 0 if women did not provide the part or replied "no" to the first question asking if they had heard of a see-feel-want statement. The outcomes in columns (4) and (8) are the percentages of the three parts women knew at a given endline. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.10: Within-Neighborhood Spillovers to Control Group

	See-Feel-Want Knowledge				Women's Communication in Employment Vignette				
	% Parts Known	Knows See (=1)	Knows Feel (=1)	Knows Want (=1)	% Comp. Used	Summarize Situation (=1)	Describe Emotions (=1)	Tell Him What (=1)	Tell Him Why (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: EL1									
Frac Treat	0.921	0.010	0.008	0.010	-6.701	-0.084	0.171	-0.299	-0.056
	(4.569)	(0.039)	(0.053)	(0.061)	(15.218)	(0.252)	(0.238)	(0.219)	(0.279)
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean	3.389	0.025	0.025	0.052	53.190	0.638	0.259	0.648	0.583
N	600	600	600	600	290	290	290	290	290
Panel B: EL2									
Frac Treat	-3.844	0.039	-0.037	-0.117	5.892	0.137	-0.080	-0.055	0.234
	(4.975)	(0.056)	(0.055)	(0.084)	(10.919)	(0.178)	(0.206)	(0.211)	(0.209)
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean	5.956	0.033	0.041	0.105	61.198	0.736	0.222	0.743	0.747
N	582	582	582	582	288	288	288	288	288

Notes: The outcomes in columns (1)-(4) are the same as those in Table A.9, while the outcomes in columns (5)-(9) are the same as those in Tables 4 and A.17. Panel A presents outcomes from EL1, and Panel B presents outcomes from EL2. "Frac Treat" is the fraction of other women in one's neighborhood who were treated. Strata controls here are village fixed effects. The sample is restricted to individuals in the control group whose neighborhoods were not pooled with others to form the neighborhood stratification variable. Columns (5)-(9) additionally restrict to women given the employment vignette at a given endline. See Section 6.1 for details on the vignettes. Standard errors are clustered by neighborhood. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.11: Effects on Earnings from Partner Firm with Alternative Imputations

	Earnings from Firm (Rs.)										
	Total	Dec.	Jan.	Feb.	Mar.	Apr.	May	Jun.	Jul.	Aug.	Sep.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Month-Center-Treatment Mean											
Treat	-330.027	-41.251	-61.523	-62.820	-52.558	-7.246	-45.449	-27.181	17.104	-21.926	-27.176
	(423.538)	(94.435)	(69.832)	(66.330)	(65.824)	(20.331)	(37.546)	(34.470)	(26.996)	(32.440)	(35.443)
W More Interested	-209.225	-125.824	-62.187	-56.505	-47.312	33.962	6.042	50.375	40.777	-13.075	-35.478
	(454.568)	(94.389)	(71.120)	(66.138)	(65.710)	(25.729)	(43.526)	(50.271)	(36.129)	(37.800)	(34.372)
Treat x W More Interested	1254.212**	264.339**	233.074**	199.455**	184.801**	15.980	80.630	49.313	29.851	83.912*	112.857**
	(596.812)	(123.722)	(97.516)	(88.644)	(87.779)	(36.229)	(55.601)	(59.112)	(44.089)	(45.576)	(49.199)
P-Val: Treat + Treat x W More = 0	0.071*	0.031**	0.042**	0.060*	0.071*	0.774	0.422	0.667	0.238	0.097*	0.033**
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	1785.049	427.716	304.424	279.595	269.303	46.443	108.878	95.859	52.599	95.619	104.614
N	1523	1523	1523	1523	1523	1523	1523	1523	1523	1523	1523
Panel B: Average of Preceding and Following Month											
Treat	-292.892	-45.184	-61.523	-62.820	-46.463	-6.975	-37.739	-3.457	17.104	-18.660	-27.176
	(414.605)	(90.798)	(69.832)	(66.330)	(64.039)	(20.279)	(36.124)	(31.398)	(26.996)	(29.700)	(35.443)
W More Interested	-138.113	-93.628	-62.187	-56.505	-32.243	34.331	7.163	67.398	40.777	-7.741	-35.478
	(448.098)	(91.661)	(71.120)	(66.138)	(64.267)	(25.741)	(41.159)	(49.528)	(36.129)	(36.380)	(34.372)
Treat x W More Interested	1219.254**	260.495**	233.074**	199.455**	162.696*	19.472	85.192	39.952	29.851	76.210*	112.857**
	(595.399)	(121.966)	(97.516)	(88.644)	(87.397)	(36.585)	(54.283)	(58.877)	(44.089)	(43.739)	(49.199)
P-Val: Treat + Treat x W More = 0	0.070*	0.037**	0.042**	0.060*	0.106	0.686	0.265	0.481	0.238	0.114	0.033**
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	1702.774	399.929	304.424	279.595	254.766	46.443	99.556	72.467	52.599	88.381	104.614
N	1523	1523	1523	1523	1523	1523	1523	1523	1523	1523	1523

Notes: This table replicates the analyses of earnings from the partner firm in Tables 1 and 2, but uses alternative imputations for missing earnings. In Panel A, the earnings of weavers with missing earnings are imputed at the month-center-treatment participant mean. In Panel B, missing earnings for a woman in a given month x are set to the average of her earnings in months $x-1$ and $x+1$. See Sections 4.1 and 5.2 for details on missing earnings. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.12: Bounds for Earnings Effects

	Earnings from Firm (Rs.)										
	Total	Dec.	Jan.	Feb.	Mar.	Apr.	May	Jun.	Jul.	Aug.	Sep.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Lower Bounds											
Treat	-440.116*	-45.975	-61.523	-62.820	-64.340	-7.411	-53.850	-22.944	17.104	-31.659	-27.176
	(243.586)	(91.925)	(69.832)	(66.330)	(63.537)	(20.365)	(33.131)	(28.383)	(26.996)	(21.400)	(35.443)
W More Interested	-203.423	-97.135	-62.187	-56.505	-32.764	33.782	4.369	41.820	40.777	-21.277	-35.478
	(341.556)	(92.207)	(71.120)	(66.138)	(64.802)	(25.817)	(40.360)	(46.110)	(36.129)	(33.644)	(34.372)
Treat x W More Interested	953.341**	262.102**	233.074**	199.455**	184.359**	13.849	79.166	35.933	29.851	89.521**	112.857**
	(417.058)	(122.657)	(97.516)	(88.644)	(87.588)	(36.277)	(49.374)	(52.121)	(44.089)	(40.263)	(49.199)
P-Val: Treat + Treat x W More = 0	0.129	0.037**	0.042**	0.060*	0.096*	0.833	0.508	0.775	0.238	0.085*	0.033**
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	1015.736	402.607	304.424	279.595	248.764	46.443	94.932	62.678	52.599	59.190	104.614
N	1452	1513	1523	1523	1515	1521	1513	1496	1523	1497	1523
Panel B: Upper Bounds											
Treat	-250.949	-39.437	-61.523	-62.820	-50.507	-7.359	-33.336	-19.475	17.104	-28.281	-27.176
	(261.752)	(91.624)	(69.832)	(66.330)	(64.264)	(20.326)	(36.220)	(26.921)	(26.996)	(20.967)	(35.443)
W More Interested	-641.261**	-103.016	-62.187	-56.505	-40.692	24.709	1.831	24.140	40.777	-39.582*	-35.478
	(254.816)	(92.739)	(71.120)	(66.138)	(64.458)	(24.305)	(40.635)	(38.755)	(36.129)	(23.326)	(34.372)
Treat x W More Interested	1147.503***	267.664**	233.074**	199.455**	178.316**	23.075	74.782	46.460	29.851	98.632***	112.857**
	(351.861)	(122.894)	(97.516)	(88.644)	(87.876)	(35.204)	(53.171)	(45.319)	(44.089)	(33.590)	(49.199)
P-Val: Treat + Treat x W More = 0	0.001***	0.028**	0.042**	0.060*	0.072*	0.594	0.310	0.495	0.238	0.008***	0.033**
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	1015.736	392.728	304.424	279.595	248.764	46.443	94.932	62.678	52.599	59.190	104.614
N	1452	1513	1523	1523	1515	1521	1513	1496	1523	1497	1523

Notes: This table presents results from a bounding exercise inspired by Lee (2009) for the effects on partner firm earnings. For this exercise, we drop observations with missing earnings and further trim observations so that rates of attrition match in the treatment and control groups. We trim separately by subgroup. Attrition in our case comes from participants of the firm's program, and thus we trim either the bottom or top of the participant distribution to generate lower and upper bounds. We then estimate the heterogeneity specification (equation (2)) among all participants whose earnings are not missing and who were not trimmed. Panel A uses the sample that generates lower bounds on the treatment effects, while Panel B uses the sample that generates upper bounds. We trim without regards to strata but include strata in the regressions here for power and consistency with other tables. Thus the coefficients on "Treat" reflect lower (upper) bounds for the treatment effect outside of our subgroup of interest, while the sum of coefficients on "Treat" and "Treat x W More Interested" reflect lower (upper) bounds in our subgroup; the coefficients on "Treat x W More Interested" reflect the differences in the lower (upper) bounds between the subgroups, not the lower (upper) bounds of the differences in effects. See Sections 4.1 and 5.2 for details on missing earnings. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.13: Extensive Versus Intensive Margin Effects on Firm Earnings

	Positive Firm Earnings (=1) / Firm Earnings (Rs.):										
	Total	Dec.	Jan.	Feb.	Mar.	Apr.	May	Jun.	Jul.	Aug.	Sep.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Extensive Margin											
Treat	-0.009	-0.006	-0.016	-0.021	-0.018	-0.013	-0.018	-0.011	0.002	-0.008	-0.013
	(0.024)	(0.022)	(0.020)	(0.020)	(0.019)	(0.015)	(0.016)	(0.015)	(0.013)	(0.014)	(0.015)
W More Interested	-0.018	-0.020	-0.018	-0.018	-0.017	0.021	0.011	0.010	0.007	-0.006	-0.013
	(0.024)	(0.023)	(0.021)	(0.021)	(0.020)	(0.018)	(0.018)	(0.017)	(0.014)	(0.015)	(0.015)
Treat x W More Interested	0.045	0.047	0.062**	0.060**	0.058**	0.007	0.015	0.022	0.023	0.031*	0.040**
	(0.031)	(0.030)	(0.028)	(0.028)	(0.026)	(0.021)	(0.021)	(0.021)	(0.018)	(0.019)	(0.018)
P-Val: Treat + Treat x W More = 0	0.164	0.102	0.061*	0.102	0.066*	0.756	0.887	0.499	0.097*	0.118	0.070*
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	0.107	0.098	0.089	0.089	0.084	0.043	0.048	0.043	0.030	0.041	0.046
N	1523	1523	1523	1523	1523	1523	1523	1523	1523	1523	1523
Panel B: Intensive Margin											
Treat	-1817.363	-227.649	-116.610	76.818	188.082	-297.004**	-411.211	-232.368	-6.793	186.956	271.272
	(1743.283)	(203.796)	(199.062)	(246.027)	(205.608)	(138.542)	(264.211)	(261.208)	(293.611)	(201.159)	(280.392)
W More Interested	816.010	-480.578	-1.002	114.425	298.192	-16.339	-240.443	-28.106	-28.671	313.569	273.051
	(2121.984)	(293.744)	(144.945)	(252.884)	(208.120)	(150.261)	(174.937)	(285.451)	(353.192)	(238.282)	(336.599)
Treat x W More Interested	4359.016	721.371**	247.170	-43.148	-344.392	219.837	1020.967**	600.622	78.734	-14.669	185.585
	(2920.135)	(348.694)	(280.570)	(394.831)	(328.451)	(305.042)	(387.990)	(380.898)	(363.731)	(226.033)	(374.662)
P-Val: Treat + Treat x W More = 0	0.294	0.081*	0.548	0.903	0.545	0.792	0.062*	0.318	0.821	0.378	0.117
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	16645.954	4365.907	3418.910	3140.064	3188.767	1070.632	2274.890	2209.799	1772.192	2326.727	2291.050
N	153	143	125	121	116	66	67	65	51	55	57

Notes: This table decomposes the effects on earnings in Tables 1 and 2 into extensive and intensive margin effects. In Panel A, the outcomes are indicators for total earnings being positive (column (1)) and earnings in a given month being positive (columns (2)-(11)). The outcomes in Panel B are total and month-by-month earnings, but the samples are restricted to women for whom each outcome is positive. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.14: Effects on Employment Outside Firm

	Any Emp. (=1) (1)	Emp. at Firm (=1) (2)	Emp. Not at Firm (=1) (3)	Any Emp. (=1) (4)
Treat	-0.055	-0.012	-0.042	-0.047
	(0.040)	(0.022)	(0.039)	(0.038)
W More Interested	-0.039	-0.010	-0.029	-0.033
	(0.046)	(0.022)	(0.042)	(0.042)
Treat x W More Interested	0.128**	0.058*	0.070	0.092*
	(0.059)	(0.030)	(0.055)	(0.055)
Emp. at Firm				0.633***
				(0.019)
P-Val: Treat + Treat x W More = 0	0.099*	0.076*	0.501	0.276
Strata Controls	Yes	Yes	Yes	Yes
Omitted Group Mean	0.430	0.087	0.344	0.430
N	1344	1344	1344	1344

Notes: Column (1) reproduces the analysis in column (4) of Table 1, presenting effects on the indicator for women reporting any work for income off their household's farm in the three preceding months at EL2. The outcome in column (2) is an indicator for women working in the partner firm's program in the three months preceding EL2, constructed based on the date of their EL2 survey and information from the registers on whether women attended the program at least once in a given month. This outcome is only defined for women who took the EL2 survey. The outcome in column (3) is an indicator for employment outside of the partner firm; specifically, it is an indicator for column (1)'s outcome equaling 1 *and* column (2)'s outcome equaling 0. A limitation with this outcome is that it would treat women who worked both in the firm and somewhere else as not having worked somewhere else. Hence we take an alternative approach to estimating effects on employment outside the firm in column (4), where we estimate effects on any employment but control for employment at the firm (i.e. estimate effects on the outcome from column (1), controlling for the outcome from column (2)). Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.15: Heterogeneity by Age

	Women's Labor Supply			Women's Activities Index	
	Applied (=1)	Total Earnings (Rs.)	Any Emp. (=1)	EL1	EL2
	(1)	(2)	(3)	(4)	(5)
Treat	0.031	236.375	-0.018	-0.145*	-0.057
	(0.032)	(385.105)	(0.038)	(0.078)	(0.076)
Above Median Age	0.017	722.730	0.074*	0.072	-0.019
	(0.027)	(454.196)	(0.043)	(0.085)	(0.078)
Treat x Above Median Age	-0.046	-128.642	0.038	0.076	0.053
	(0.042)	(658.299)	(0.056)	(0.109)	(0.110)
P-Val: Treat + Treat x Above = 0	0.700	0.855	0.646	0.410	0.962
Strata Controls	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	0.185	1324.389	0.381	-0.028	0.014
N	1540	1540	1360	1407	1360

Notes: "Above Median Age" is an indicator for women being above the median age in our sample (i.e. above 30 years old). The outcomes in columns (1)-(3) are the same as in Table 1. Note that as in that table, missing earnings are imputed with month-center-treatment-subgroup participant means, but here subgroup is defined by "Above Median Age" rather than "W More Interested." See Sections 4.1 and 5.2 for details on missing earnings. The outcomes in columns (4) and (5) are indices of indicators for women reporting at EL1 and EL2, respectively, that in the previous three months they had: worked outside the home, visited their natal village, visited the market, and gotten a new saree. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.16: Effects on Other Outcomes

	Women's Activities Index		Any Employment (=1)		Visited Natal Village (=1)		Visited Market (=1)		Got New Saree (=1)		N Tokens
	EL1	EL2	EL1	EL2	EL1	EL2	EL1	EL2	EL1	EL2	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Treat	-0.112*	-0.032	-0.005	-0.055	-0.063	-0.026	-0.034	0.009	-0.070**	-0.023	-0.023
	(0.060)	(0.058)	(0.042)	(0.040)	(0.046)	(0.038)	(0.028)	(0.020)	(0.031)	(0.034)	(0.187)
W More Interested in Activities Index	-0.008	-0.055									
	(0.036)	(0.039)									
Treat x W More Interested in Activities Index	0.009	0.004									
	(0.051)	(0.053)									
W More Interested in Activity			-0.009	-0.039	0.043	-0.013	-0.025	-0.048	-0.077*	-0.027	
			(0.042)	(0.046)	(0.046)	(0.039)	(0.039)	(0.030)	(0.045)	(0.044)	
Treat x W More Interested in Activity			0.016	0.128**	0.002	0.020	0.012	0.008	0.076	-0.038	
			(0.055)	(0.059)	(0.065)	(0.050)	(0.052)	(0.040)	(0.060)	(0.060)	
Pre-Play Communication											0.280*
											(0.160)
Trust											1.526***
											(0.239)
Treat x Pre-Play Communication											0.010
											(0.243)
Treat x Trust											0.406
											(0.355)
Pre-Play Communication x Trust											0.112
											(0.346)
Treat x Pre-Play Communication x Trust											-0.385
											(0.506)
P-Val: Treat + Treat x W More = 0			0.787	0.099*	0.229	0.872	0.625	0.651	0.902	0.255	
P-Val: Treat + Treat x Communication = 0											0.942
P-Val: Treat + Treat x Trust = 0											0.200
P-Val: Treat + Treat x Communication + Treat x Trust + Treat x Comm. x Trust											0.978
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	-0.000	-0.000	0.379	0.430	0.562	0.638	0.825	0.893	0.438	0.586	5.170
N	1403	1356	1393	1344	874	1331	1391	1345	1394	1349	2164

Notes: The outcomes in columns (1) and (2) are indices of indicators for women reporting at EL1 and EL2, respectively, that in the previous three months they had: worked outside the home, visited their natal village, visited the market, and gotten a new saree. The outcomes in columns (3)-(10) are the indicators that make up these indices. "W More Interested in Activities Index" is an index of indicators for women reporting at baseline they were more interested than their husbands in the women doing each of the four activities. "W More Interested in Activity" is an indicator for women reporting at baseline they were more interested in the given outcome activity; that is, "W More Interested in Activity" varies by column to match the outcome, and the four "W More Interested in Activity" variables are the components of the "W More Interested in Activities Index." A programming error in the EL1 survey resulted in the natal village question not being asked on initially-fielded versions of EL1, hence we only observe that outcome at EL1 for 57% of the sample. The effects on the EL1 index of activities look similar if we exclude the natal village outcome from the index. The outcome in column (11) is from the lab-in-the-field games (see Section 5.4 for details on the games). The data in this column are at the game × couple level. The outcome is the number of tokens the woman ended the game with. "Pre-play communication" is an indicator for the couple being able to communicate before the husband made his decision in the game. "Trust" is an indicator for the game being the trust game rather than the dictator game. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.17: Additional Analyses of Women's Communication in Employment Vignette

	% Assertive Used (1)	Summarize Situation (=1) (2)	Describe Emotions (=1) (3)	Tell Him What (=1) (4)	Tell Him Why (=1) (5)	Other (=1) (6)	Initiate (=1) (7)	Get What I Want (=1) (8)
Panel A: EL1								
Treat	2.430	0.027	0.017	0.028	0.026	-0.039	0.056	-0.103**
	(2.775)	(0.050)	(0.044)	(0.052)	(0.050)	(0.028)	(0.036)	(0.049)
W More Interested	1.699	-0.033	0.050	0.066	-0.016	-0.057**	-0.006	-0.166***
	(3.261)	(0.054)	(0.044)	(0.060)	(0.057)	(0.027)	(0.038)	(0.053)
Treat x W More Interested	3.041	0.100	-0.069	-0.062	0.153**	0.109***	0.010	0.288***
	(4.133)	(0.072)	(0.064)	(0.077)	(0.074)	(0.041)	(0.048)	(0.072)
P-Val: Treat + Treat x W = 0	0.085*	0.021**	0.274	0.547	0.002***	0.032**	0.064*	0.002***
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	52.368	0.653	0.226	0.616	0.600	0.116	0.859	0.691
N	687	687	687	687	687	687	688	686

	Women's Reports			Husbands' Reports				
	Other (=1) (1)	Initiate (=1) (2)	Pr LCA Class 2 (3)	Summarize Situation (=1) (4)	Describe Emotions (=1) (5)	Tell Him What (=1) (6)	Tell Him Why (=1) (7)	Initiate (=1) (8)
Panel B: EL2								
Treat	-0.010	-0.001	0.035	0.082	0.066	-0.046	0.023	0.016
	(0.023)	(0.028)	(0.034)	(0.058)	(0.046)	(0.048)	(0.052)	(0.035)
W More Interested	-0.038*	-0.018	-0.022	0.034	-0.010	-0.053	-0.026	0.013
	(0.020)	(0.033)	(0.039)	(0.065)	(0.044)	(0.047)	(0.057)	(0.040)
Treat x W More Interested	0.044	0.056	0.053	-0.071	-0.062	0.037	-0.054	-0.068
	(0.032)	(0.040)	(0.048)	(0.086)	(0.064)	(0.068)	(0.079)	(0.055)
P-Val: Treat + Treat x W = 0	0.145	0.052*	0.013**	0.874	0.932	0.866	0.613	0.215
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	0.056	0.921	0.826	0.621	0.174	0.826	0.750	0.895
N	678	679	678	515	515	515	515	518

Notes: All outcomes are from the vignettes at EL1 (Panel A) and EL2 (Panel B). The samples are restricted to those given the employment vignette at a given endline. See Section 6.1 for details on the vignettes. The outcomes in columns (1)-(5) of Panel A are the same as in Table 4, just measured at EL1 rather than EL2. The outcomes in column (6) of Panel A and column (1) of Panel B are from the vignette question asking women what they would say to their husbands; they are indicators for providing a response which was recorded in the "other" category at EL1 and EL2. The outcomes in column (7) of Panel A and columns (2) and (8) of Panel B are indicators for women at EL1, women at EL2, and husbands at EL2 saying the women would initiate a discussion with their husbands. The outcome in column (8) of Panel A is from a question in the EL1 vignette asking women if they thought they would ultimately get what they wanted; it is an indicator for women saying they would get exactly what they wanted, as opposed to saying no or saying they would get something like they wanted but not exactly. The outcome in column (3) of Panel B is from the two-class LCA, preferred by entropy. It is the probability of being Class 2 as opposed to Class 1. The outcomes in columns (4)-(7) of Panel B are the same as in Table 4, except they are husbands' predictions of their wives providing a given assertive response rather than women's own reports. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.18: Reasons for Applying to Firm's Program

	A Reason to Apply Was (=1):										
	Money, Specific (1)	Money, General (2)	Money, Control (3)	Valuable Skill (4)	Time w/ Other W (5)	Time Outside (6)	Knew Applicants (7)	H/Family Wanted (8)	Other (9)	No Reason (10)	Control or Time Out. (11)
Treat	0.043	0.006	0.046	-0.018	-0.041	-0.035*	0.018	-0.027	0.065**	-0.033	0.016
	(0.037)	(0.038)	(0.034)	(0.039)	(0.030)	(0.021)	(0.016)	(0.018)	(0.026)	(0.022)	(0.035)
W More Interested	0.042	-0.021	0.059	-0.023	-0.029	0.036	0.011	-0.000	0.005	-0.037*	0.045
	(0.035)	(0.042)	(0.037)	(0.041)	(0.030)	(0.030)	(0.017)	(0.018)	(0.026)	(0.020)	(0.036)
Treat x W More Interested	-0.014	0.030	-0.036	-0.002	0.074*	-0.013	-0.014	0.030	-0.042	0.030	-0.015
	(0.053)	(0.054)	(0.054)	(0.056)	(0.041)	(0.037)	(0.026)	(0.026)	(0.038)	(0.030)	(0.053)
P-Val: Treat + Treat x W More = 0	0.483	0.375	0.816	0.637	0.320	0.114	0.841	0.875	0.456	0.880	0.976
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	0.328	0.633	0.436	0.488	0.181	0.126	0.042	0.068	0.123	0.118	0.507
N	1347	1347	1347	1347	1347	1347	1347	1347	1347	1347	1347

Notes: The outcomes are from a question at EL1 that asked women what reasons they saw for applying to the firm's program. The question was asked whether or not women had applied. The outcomes in columns (1)-(10) are indicators for providing each of the optional responses: to have more money for some specific purpose, to have more money in general, to have money in my control, to learn a valuable skill, to spend time with other women, to spend time out of the house, I knew other people who were applying/working there, my husband and/or other family members wanted me to apply, other, and I didn't see any reasons to apply. Multiple options could be selected (unless the final option was chosen, in which case the survey only allowed that response). The outcome in column (11) is an indicator for selecting the third or sixth option; this outcome is the "Unconvincing" variable from column (4) of Table 5. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.19: Effects on Women's and Husbands' Interest in Women's Employment

	W Interest (1-4)			H Interest (1-4)			W More Interested (=1)			EL2–EL1 Change	
	EL1 W Report (1)	EL2 W Report (2)	EL2 H Report (3)	EL1 W Report (4)	EL2 W Report (5)	EL2 H Report (6)	EL1 W Report (7)	EL2 W Report (8)	EL2 W&H Report (9)	W Interest, W Report (10)	H Interest, W Report (11)
Treat	0.037	-0.001	-0.120	-0.100	-0.088	0.037	0.032	0.043	-0.018		
	(0.083)	(0.089)	(0.092)	(0.102)	(0.089)	(0.092)	(0.038)	(0.035)	(0.040)		
W More Interested	0.155*	0.018	-0.072	-0.352***	-0.240***	-0.141	0.152***	0.081**	0.017		
	(0.080)	(0.093)	(0.098)	(0.106)	(0.092)	(0.098)	(0.038)	(0.035)	(0.046)		
Treat x W More Interested	-0.030	0.063	0.107	0.275*	0.204	0.021	-0.116**	-0.059	0.058		
	(0.113)	(0.130)	(0.142)	(0.153)	(0.127)	(0.137)	(0.054)	(0.048)	(0.060)		
Control, W Less or Equally Interested										-0.138**	-0.347***
										(0.069)	(0.072)
Control, W More Interested										-0.201***	-0.183**
										(0.069)	(0.081)
Treat, W Less or Equally Interested										-0.109*	-0.336***
										(0.062)	(0.068)
Treat, W More Interested										-0.132*	-0.276***
										(0.072)	(0.072)
P-Val: Treat + Treat x W More = 0	0.937	0.537	0.910	0.133	0.278	0.605	0.045**	0.684	0.376		
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Omitted Group Mean	3.216	3.116	2.718	2.480	2.165	2.045	0.470	0.516	0.612		
N	1393	1342	1124	1386	1339	1126	1386	1338	1055	1277	1269

Notes: The outcomes in columns (1)-(3) are women's interest in working outside the home, measured on a scale of 1 to 4 (1 = very uninterested, 2 = somewhat uninterested, 3 = somewhat interested, and 4 = very interested). Women's interest is reported by women at EL1 in column (1), women at EL2 in column (2), and husbands at EL2 in column (3). The outcomes in columns (4)-(6) are husbands' interest in their wives working outside the home, as reported by women at EL1, women at EL2, and husbands at EL2. The outcomes in columns (7)-(9) are indicators for women's interest being higher than their husbands', i.e. our main heterogeneity variable, just measured at endline rather than baseline. The indicators in columns (7) and (8) are based on women's reports of their own and their husbands' interest at EL1 and EL2, respectively. Column (9)'s indicator instead uses women's and husbands' reports of their own interest at EL2. The outcomes in columns (10)-(11) are changes in interest on a 1-4 scale between the two endlines, i.e. an individual's interest at EL2 minus their interest at EL1. Column (10) shows the change in women's own reported interest, while column (11) shows the change in women's reports of their husbands' interest. The regressors in columns (10)-(11) are indicators for each of the four possible combinations of Treat and W More Interested, and these regressions do not include intercepts. Standard errors are clustered by meeting group in columns (1)-(9) and robust in columns (10)-(11). * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.20: Effects on (In)Correctness about Spouse's Interest in Women's Employment

	H (In)Correctness about W Interest							W (In)Correctness about H Interest						
	Correct, Exactly (=1)	Correct, Overall (=1)	Correct, W More (=1)	Absolute Error (1-4)	Amount Under (1-4)	Max Over (=1)	Max Under (=1)	Correct, Exactly (=1)	Correct, Overall (=1)	Correct, W More (=1)	Absolute Error (1-4)	Amount Under (1-4)	Max Over (=1)	Max Under (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Treat	-0.114***	-0.028	-0.049	0.168**	0.054	0.005	0.028	-0.001	0.014	0.009	0.048	0.105	0.011	0.031
	(0.038)	(0.038)	(0.037)	(0.084)	(0.119)	(0.014)	(0.027)	(0.042)	(0.043)	(0.039)	(0.095)	(0.122)	(0.022)	(0.019)
W More Interested	-0.099**	-0.026	-0.079*	0.154	0.000	0.020	0.011	-0.003	0.052	-0.004	0.011	0.123	-0.002	0.037*
	(0.041)	(0.044)	(0.046)	(0.094)	(0.125)	(0.017)	(0.029)	(0.046)	(0.045)	(0.040)	(0.105)	(0.116)	(0.023)	(0.020)
Treat x W More Interested	0.166***	0.012	0.037	-0.184	0.131	-0.015	-0.018	0.034	0.019	0.043	-0.183	-0.235	-0.015	-0.072**
	(0.058)	(0.060)	(0.060)	(0.130)	(0.182)	(0.023)	(0.040)	(0.062)	(0.063)	(0.056)	(0.143)	(0.177)	(0.032)	(0.029)
P-Val: Treat + Treat x W More = 0	0.252	0.734	0.798	0.874	0.189	0.583	0.741	0.509	0.508	0.288	0.245	0.362	0.852	0.091*
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	0.420	0.658	0.684	1.020	0.446	0.029	0.104	0.442	0.588	0.710	1.013	-0.117	0.065	0.036
N	1053	1053	1053	1053	1053	1053	1053	1056	1056	1055	1056	1056	1056	1056

Notes: The outcomes are from the EL2 surveys with women and husbands. The outcomes in columns (1)-(7) reflect husbands' (in)correctness about their wives' interest in the wives working outside the home. Interest was measured on a 1-4 scale (1 = very uninterested, 2 = somewhat uninterested, 3 = somewhat interested, and 4 = very interested). The outcome in column (1) is an indicator for husbands predicting their wives' interest correctly on the 1-4 scale, while the outcome in column (2) is an indicator for husbands correctly predicting their wives were overall interested (responses of 3 or 4) or overall uninterested (responses of 1 or 2). Column (3)'s outcome is an indicator for husbands correctly predicting their wives were either more or equally/less interested than the husbands. Column (4)'s outcome is the absolute value of the deviation between husbands' predictions and wives' actual interest on the 1-4 scale, while column (5)'s outcome is the amount husbands underestimated their wives' interest on the 1-4 scale. The outcome in column (6) is an indicator for husbands maximally overestimating their wives' interest (i.e. predicting their wives' interest was a 4 and their wives' actual interest being a 1), while the outcome in column (7) is an indicator for husbands maximally underestimating their wives' interest (i.e. predicting their wives' interest was a 1 and their wives' actual interest being a 4). The outcomes in columns (8)-(14) are the same as in (1)-(7), except they reflect women's (in)correctness about their husbands' interest, rather than the reverse. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.21: Effects on Negotiation and Final Say

	Negotiation (=1) in Employment Vignette			Did Activity in Last 3 Months (=1), EL2			Aggregate of Activities		W Final Say Index		
	EL1 W	EL2 W	EL2 H	Visited Natal Village	Visited Market	Got New Saree	Activity Index	Did Any Activity (=1)	EL1 W	EL2 W	EL2 H
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Treat	0.033	0.005	-0.039	-0.016	0.010	-0.005	-0.007	0.009	-0.156**	-0.100	0.006
	(0.032)	(0.022)	(0.027)	(0.037)	(0.024)	(0.038)	(0.075)	(0.013)	(0.067)	(0.078)	(0.079)
W More Interested (in Work)	-0.027	-0.002	-0.016	-0.029	-0.014	-0.064	-0.114	-0.008	-0.201***	-0.094	-0.081
	(0.028)	(0.021)	(0.029)	(0.037)	(0.027)	(0.043)	(0.082)	(0.017)	(0.073)	(0.079)	(0.085)
Treat x W More Interested (in Work)	-0.002	0.002	0.013	-0.000	-0.019	-0.055	-0.088	-0.018	0.108	0.140	0.048
	(0.039)	(0.030)	(0.036)	(0.052)	(0.038)	(0.059)	(0.116)	(0.024)	(0.097)	(0.103)	(0.115)
P-Val: Treat + Treat x W More = 0	0.272	0.740	0.336	0.690	0.754	0.187	0.293	0.658	0.511	0.606	0.524
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	0.089	0.039	0.062	0.647	0.887	0.598	0.041	0.958	0.072	0.022	0.006
N	687	678	542	1325	1343	1343	1344	1344	1393	1343	1128

Notes: The outcomes in columns (1)-(3) are from the vignettes in women's EL1 surveys, women's EL2 surveys, and husbands' EL2 surveys, respectively. The samples are restricted to respondents given the employment vignette. See Section 6.1 for details on the vignettes. The outcomes are from the question asking what women would say to their husbands, and are indicators for the respondent saying she would offer a compromise. Columns (4)-(8) investigate whether women gave up other activities to compensate for employment. These outcomes are from women's EL2 surveys, the endline when we see significant effects on employment. The outcomes in columns (4)-(6) are indicators for women reporting they had performed the following activities in the last three months: visited their natal village, visited the local market, and got a new saree. These activities were selected as activities that wives are likely to be more interested in doing than their husbands are in them doing. The outcome in column (7) is an index of the indicators for women doing the three activities, while the outcome in column (8) is an indicator for women doing any of the three. The outcomes in columns (9)-(11) come from women's EL1 surveys, women's EL2 surveys, and husbands' EL2 surveys, respectively. They are indices of indicators for respondents saying women have final say over the following household decisions: spending on food, spending on clothing, whether to purchase a large household item, and spending of the husband's earnings. We consider a woman as having final say if respondents said she was at least one of the people with final say. "W More Interested (in Work)" is the usual heterogeneity variable, i.e. the indicator for women reporting they were more interested in their employment than their husbands at baseline. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Table A.22: Additional Mechanisms Analyses and Effects on Women's Happiness

	H Migrant (=1)	Applied (=1)	W Potential Earnings (Rs.)		Threat (=1)	N Tokens	GSE Index		OK for Women to Work (=1)			Happiness (1-4)	
	EL2 W		EL2 H	EL2 W	EL2 H		EL1 W	EL2 W	EL1 W	EL2 W	EL2 H	EL1 W	EL2 W
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Treat	0.005	-0.061	107.884	-908.643***	0.053	-0.057	0.029	-0.079	0.003	-0.031	-0.001	-0.062	-0.127**
	(0.040)	(0.043)	(366.032)	(288.011)	(0.044)	(0.248)	(0.083)	(0.076)	(0.021)	(0.027)	(0.037)	(0.049)	(0.056)
W More Interested	0.043	-0.052	-246.914	-553.766	0.026	0.029	0.033	-0.120	-0.002	-0.027	-0.000	-0.060	-0.103*
	(0.040)	(0.041)	(331.901)	(343.449)	(0.042)	(0.221)	(0.083)	(0.076)	(0.023)	(0.026)	(0.045)	(0.057)	(0.057)
Treat x W More Interested	0.049	0.114*	-57.486	1083.987**	0.002	-0.051	-0.041	0.183*	-0.003	0.056	-0.049	0.071	0.133*
	(0.054)	(0.062)	(499.498)	(447.236)	(0.060)	(0.346)	(0.115)	(0.109)	(0.032)	(0.037)	(0.062)	(0.080)	(0.080)
H Migrant		-0.082**											
		(0.040)											
Treat x H Migrant		0.081											
		(0.056)											
W More x H Migrant		0.040											
		(0.063)											
Treat x W More x H Migrant		-0.037											
		(0.092)											
P-Val: Treat + Treat x W = 0	0.143		0.887	0.606	0.269	0.680	0.881	0.218	0.995	0.404	0.295	0.883	0.913
Strata Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Omitted Group Mean	0.446	0.246	6320.952	5415.984	0.435	5.143	-0.017	0.051	0.917	0.863	0.658	3.472	3.543
N	1341	1341	1059	1300	1059	538	1392	1342	1393	1338	1126	1391	1342

Notes: The outcome in column (1) is an indicator for husbands having migrated for work in the last year, as reported by women at EL2. This variable is also the "H Migrant" regressor in column (2). Column (2)'s outcome is the application outcome from Table 1. The outcomes in columns (3) and (4) are husbands' and wives' predictions of how much the wives could earn if they were to work for income for one month. They come from husbands' and wives' EL2 surveys and are in rupees. The outcome in column (5) is an indicator for earnings threat; it is the "Unconvincing" variable from column (3) of Table 5. Specifically, it is an indicator for husbands' prediction of their own potential earnings minus their prediction of their wives' potential earnings being below median at EL2. Column (6)'s outcome is the number of tokens husbands sent their wives in the dictator game. The sample in this column is restricted to couples given the basic dictator game (with no communication). See Section 5.4 for details on the lab-in-the-field games. The outcomes in columns (7) and (8) are indices of women's responses to items from the Generalized Self-Efficacy Scale (Schwarzer and Jerusalem, 1995) at EL1 and EL2. This scale includes 10 items, with responses on a 1-4 scale where higher values indicate greater self-efficacy. To reduce the survey duration, women were randomly assigned to a set of five of the 10 items (either items 1, 2, 4, 7, and 8, or items 3, 5, 6, 9, and 10). The indices are formed over the items the women responded to. The outcomes in columns (9)-(11) are indicators for women at EL1, women at EL2, and husbands at EL2 saying they thought it is alright if women go out for work to earn money. This was an attitudinal question asked about women's work in general, not work of the wife in particular. The outcomes in columns (12) and (13) are women's overall happiness in life, on a 1-4 scale, at EL1 and EL2. The happiness question is taken from the World Values Survey. Standard errors are clustered by meeting group. * p <0.10, ** p < 0.05, *** p < 0.01.

Online Appendix B: Data Appendix

B.1 Pre-Registration

We registered our experiment on the AEA RCT Registry (AEARCTR-0010192) on October 17, 2022, shortly after the intervention had begun but before any outcomes data had been collected. We revised the registry to flesh out the secondary outcomes on November 30, 2022, which was before any secondary outcomes, in either version of the registry, had been collected. We follow closely the outcomes and heterogeneity analyses specified in the registration, making just the following minor deviations:

1. We specified presenting effects on any wages women earned (not just in the firm's program). We asked about income at EL1 but removed the question from EL2 following reports from surveyors of women responding imprecisely and feeling uncomfortable providing their incomes. We therefore do not present effects on this outcome.

2. We specified measuring interest in employment using indices of interest in employment overall and in the firm's program, but only use the former for two reasons. First, our questions about interest in the firm's program asked women to recall their and their husbands' *initial* interest in the program, when they first heard about it; effects on these outcomes would in theory miss any effects of persuasion. Second, the questions about interest in the firm's program were not symmetric for women and men, asking how interested women were and whether men were more or less interested than women.

3. We specified considering effects on an index of women's interest in employment, visiting their natal village, visiting the market, and getting a new saree to understand mechanisms driving effects on the index of these outcomes. Since we do not ultimately find many effects on the index of these outcomes (Table A.16), we do not present effects on this index of interest.

4. We specified presenting effects on an index of indicators for women giving input over various household decisions. We omit this outcome as it is not helpful for ruling in or out mechanisms, especially relative to our vignette data; in particular, it does not tell us what women say when they give input.

5. We present several analyses which were not pre-specified but are useful for understanding mechanisms. In particular, we add effects on: reasons for applying to the firm's program, women's potential earnings, and knowledge of spouse's preferences around

the wife's employment. We also add heterogeneity by husband's migration and by proxies for women's narratives being unconvincing.

B.2 Vignettes to Measure Communication Style

The script for the employment vignette in women's EL2 surveys read as follows:

> *Now I will tell you a story about a household. This is an imaginary household but I would like you to pretend it actually exists.*
>
> *The household lives in another village nearby. The household consists of a man named Rajkumar, his wife Anita, and their children. Rajkumar is 32 years old and Anita is 30. They have three children.*
>
> *They are making a decision about whether Anita should work outside the home. Anita wants to work outside but Rajkumar doesn't think she should.*
>
> *Imagine you were Anita in this situation. I know you may not want the same thing as Anita does in this situation, but I want you to imagine you did. Likewise, your husband may not want the same thing as Rajkumar in this situation, but I want you to imagine he did.*
>
> *I'm going to ask you some questions about what you would do if you were in Anita's situation. When you answer these questions, please tell me what you think you would actually do, not what you think you should do. I know you might not be sure what you would do in Anita's situation. If youre unsure, just tell me what you think youd be most likely to do.*

We consider responses to the following questions:

- *Would you bring up the topic with your husband or would you wait for him to bring it up?*
    1. *I would bring up the topic with my husband*
    2. *I would wait for my husband to bring it up*
- *Suppose you brought up the topic with your husband. What would you say to him? (Surveyor: do not read options aloud)*
    1. *Summarize the situation / decision*

2. *Describe the emotions you are feeling about the situation (for example, "I feel disappointed / worried" or "I feel excited / happy")*
3. *Tell him what you want to do*
4. *Tell him why you want to do it*
5. *Ask him for permission to do what you want*
6. *Ask him what he thinks should be done*
7. *Tell him you will do whatever he wants*
8. *Tell him he's wrong / unreasonable / stupid / never lets you get what you want*
9. *Describe the emotions he is making you feel (for example, "you make me feel angry / sad / unhappy")*
10. *Offer a compromise (for example, offer to go with an option in between what you two want in this decision, or offer to go with what he wants in another decision in exchange for going with what you want in this decision)*
11. *Plead / beg*
12. *Other*

- *If you were in this situation, do you think you would ultimately change your husband's mind?*
    1. *Yes*
    2. *No*

Multiple responses were allowed for the second question, while the first and third only accepted a single response.

Online Appendix C: Overview of LCA

LCA Overview. Let $i = 1, \ldots, N$ index respondents, each of which answered J binary items. Let $y_{ij} \in \{0, 1\}$ denote respondent i's response to item j, and write $\mathbf{y}_i = (y_{i1}, \ldots, y_{iJ})$. We posit a discrete latent class variable $C_i \in \{1, \ldots, K\}$ assigning each respondent to one of K unobserved classes. We use two methods to select the number of classes, which we define below.

The marginal probability of a response pattern is a finite mixture over the K classes,

$$\Pr(\mathbf{y}_i) = \sum_{k=1}^{K} \pi_k \Pr(\mathbf{y}_i \mid C_i = k), \qquad \pi_k \geq 0, \quad \sum_{k=1}^{K} \pi_k = 1, \tag{10}$$

where $\pi_k = \Pr(C_i = k)$ is the (unconditional) class share. Assuming items are conditionally independent within a class (local independence, which is a working assumption for LCA), the class-conditional likelihood becomes:

$$\Pr(\mathbf{y}_i \mid C_i = k) = \prod_{j=1}^{J} p_{kj}^{y_{ij}} \left(1 - p_{kj}\right)^{1 - y_{ij}}, \qquad p_{kj} \equiv \Pr(y_{ij} = 1 \mid C_i = k). \tag{11}$$

Each item-by-class success probability p_{kj} is parameterized through a logit link function, and estimated using maximum likelihood, maximizing the observed-data log-likelihood

$$\ell(\boldsymbol{\theta}) = \sum_{i=1}^{N} \ln \left[\sum_{k=1}^{K} \pi_k \prod_{j=1}^{J} p_{kj}^{y_{ij}} \left(1 - p_{kj}\right)^{1 - y_{ij}} \right]. \tag{12}$$

After estimation, each respondent's posterior class-membership probability follows from Bayes' rule:

$$\tau_{ik} \equiv \Pr(C_i = k \mid \mathbf{y}_i) = \frac{\pi_k \prod_{j=1}^{J} p_{kj}^{y_{ij}} (1 - p_{kj})^{1 - y_{ij}}}{\sum_{\ell=1}^{K} \pi_\ell \prod_{j=1}^{J} p_{\ell j}^{y_{ij}} (1 - p_{\ell j})^{1 - y_{ij}}}. \tag{13}$$

Respondent i is also given a binary classification, assigning them to the class with the largest posterior likelihood:

$$\hat{C}_i = \underset{k \in \{1, \ldots, K\}}{\arg\max} \ \tau_{ik}. \tag{14}$$

Picking the Number of Classes. We estimate models allowing from two up to ten different classes. We use the Bayesian Information Criterion (BIC, which balances model fit with a penalty for additional parameters) to select the number of classes, picking the number of classes with the lowest BIC. In practice, because the BIC picks the model with three classes (and the alternative measure we discuss next picks two classes), estimating models

with a maximum of ten classes is not a constraint in model selection. We additionally show robustness to the model with maximum normalized entropy, $E_K \in [0, 1]$, which is a measure of clear separation of classes:

$$E_K = 1 + \frac{\sum_{i=1}^{N} \sum_{k=1}^{K} \tau_{ik} \ln \tau_{ik}}{N \ln K}, \qquad \tau_{ik} \ln \tau_{ik} \equiv 0 \text{ when } \tau_{ik} = 0. \tag{15}$$

The summation is (minus) the total Shannon entropy of the posterior memberships; dividing by $N \ln K$ rescales against maximum uncertainty (uniform $\tau_{ik} = 1/K$) and the construction orients the index so that larger values (closer to 1) indicate greater separation. We use this measure as a secondary measure since this is a more descriptive measure of model separation, not a goodness of fit measure.

Online Appendix D: Theoretical Appendix

D.1 Numerical Example

In this section, we provide a specific numerical example of the more general model in Section 7 to fix intuition.

D.1.1 Setup

Payoffs and Decision Rule. The husband's payoff from his wife working is $+1$ in state R_S and -1 in states $R_\emptyset$ and N. Not working yields 0. He is supportive of her labor force participation if

$$\mu_p(R_S) \;\geq\; \tfrac{1}{2}. \tag{16}$$

Prior. The husband's prior beliefs over the different states are

$$\mu_0(N) = 0.3, \qquad \mu_0(R_\emptyset) = 0.5, \qquad \mu_0(R_S) = 0.2,$$

Note that in this example he is pessimistic that earnings would be shared.

History. As in Section 7, we fix the realized history as $h = (r, s)$, meaning the history indicates the household needs resources and the wife would share.

Default Narrative. The husband's default model is as follows:

	$\pi_d(h_1 = r \mid \omega)$	$\pi_d(h_2 = s \mid \omega)$	$\pi_d(h \mid \omega)$
$\omega = N$	0.25	0.50	0.125
$\omega = R_\emptyset$	0.75	0.25	0.1875
$\omega = R_S$	0.75	0.75	0.5625

The sharing signal is uninformative under N (i.e. $\rho = 0.5$).

D.1.2 Default Model Fit and Belief Updating

The fit of the default model is

$$\Pr(h \mid d, \mu_0) = 0.3(0.125) + 0.5(0.1875) + 0.2(0.5625) = 0.0375 + 0.09375 + 0.1125 = 0.244,$$

and the husband's posterior on the favorable state is

$$\mu_p(R_S) = \frac{0.2 \times 0.5625}{0.244} = 0.461 \ < \ \tfrac{1}{2}.$$

This does not meet the 0.5 likelihood threshold for state R_S, so in the absence of any persuasion, the husband is not supportive of his wife working despite the positive observed signals.

D.1.3 Deciding Whether to Adopt Very Optimistic Alternative Model Proposed by Wife

Suppose the wife proposes an alternative narrative that the history perfectly reveals R_S:

$$\pi_a(h \mid R_S) = 1, \qquad \pi_a(h \mid N) = \pi_a(h \mid R_\emptyset) = 0.$$

Then $\Pr(h \mid a, \mu_0) = 0.2 \times 1 = 0.2 < 0.244$, so the narrative is rejected.

D.1.4 Alternate Model that Is Adopted

Now suppose the wife proposes a narrative that has $\pi_a(h \mid R_S) = 1$ but concedes the history could occasionally arise in an alternative state, placing the residual likelihood on N:

$$\pi_a(h \mid R_S) = 1, \qquad \pi_a(h \mid N) = 0.146, \qquad \pi_a(h \mid R_\emptyset) = 0.$$

Its fit is $0.3(0.146) + 0.2(1) = 0.04375 + 0.2 = 0.244$, tying the default, so it is adopted.

The induced posterior is

$$\mu_p(R_S) = \frac{0.2}{0.24375} = 0.82, \qquad \mu_p(N) = 0.18, \qquad \mu_p(R_\emptyset) = 0,$$

This crosses the 0.5 likelihood threshold for state R_S, so persuasion succeeds and the wife works.

D.2 Proof of Proposition (1) and Corollary (1)

D.2.1 Proof of Proposition (1)

Proof. The wife wants to maximize the husband's posterior likelihood of the state being R_S, which is given by:

$$\mu_p(R_S) \;=\; \frac{c\,\pi^a_{R_S}}{\Phi}.$$

- The largest possible value the numerator can be is c, because $\pi^a_{R_S} \leq 1$. That is, the largest claim about $\pi^a_{R_S}$ the wife can make is that it equals 1, capping the numerator at c, the prior.
- Since the alternative model fit must be weakly better than the default model, $\Phi \geq F_d$, the smallest possible value of the denominator is F_d.

Combining the best case for each,

$$\mu_p(R_S) \;\leq\; \frac{c}{F_d}.$$

Since $\mu_p(R_S) \leq 1$, we obtain

$$\mu_p(R_S) \;\leq\; \min\left\{1,\; \frac{c}{F_d}\right\} \;\equiv\; \overline{\mu}.$$

Next, we show that this is attained.

Case 1: $F_d > c$**.** Here, c/F_d is the binding ceiling (since in this case, $\min\left\{1,\; \frac{c}{F_d}\right\} = \frac{c}{F_d}$). The wife sets $\pi^a_{R_S} = 1$, allowing the numerator to attain its maximum, c. She then needs the denominator to equal exactly F_d. State R_S already contributes $c \cdot 1 = c$ to Φ, so the other two states must supply the remaining $F_d - c$ likelihood:

$$a\,\pi^a_N + b\,\pi^a_{R_\emptyset} = F_d - c, \qquad \pi^a_N, \pi^a_{R_\emptyset} \in [0,1].$$

Next, we show that this is feasibly achieved. The most the other two states can supply to the model fit is $a \cdot 1 + b \cdot 1 = a + b$, the least is $a \cdot 0 + b \cdot 0 = 0$, and we need them to supply $F_d - c$. We are in the case where $F_d > c$, so $F_d - c > 0$. Likewise, $F_d - c \leq a + b$ holds, because F_d is a convex combination of likelihoods that are each at most 1, we have $F_d \leq 1$, hence

$$F_d - c \;\leq\; 1 - c \;=\; a + b.$$

The induced posterior is then

$$\mu_p(R_S) = \frac{c \cdot 1}{F_d} = \frac{c}{F_d},$$

attaining the ceiling.[49]

Case 2: $F_d \leq c$**.** Here, the binding ceiling is 1. The wife sets

$$\pi^a_{R_S} = \frac{F_d}{c} \ (\leq 1), \qquad \pi^a_N = \pi^a_{R_\emptyset} = 0.$$

Then the numerator is $c \cdot \frac{F_d}{c} = F_d$ and the denominator is $\Phi = c \cdot \frac{F_d}{c} = F_d$, so $\mu_p(R_S) = 1$.

In both cases, $\Phi = F_d$, meaning the proposed narrative ties the default narrative for fit and is adopted.

□

D.2.2 Proof of Corollary (1)

Proof. Under the threshold rule, the husband permits work iff $\mu_p(R_S) \geq \tau$. Since the wife can induce a posterior belief of at most $\bar{\mu}$, persuasion is successful iff

$$\bar{\mu} = \min\left\{1, \frac{c}{F_d}\right\} \ \geq \ \tau.$$

First suppose we are in the second piece of the min, meaning we are in the case with $c < F_d$. In this piece, the condition is met if and only if $c/F_d \geq \tau$. Re-arranging and substituting for $\tau = k/(1+k)$ we have

$$c \ \geq \ \frac{k}{1+k} F_d \quad \Longleftrightarrow \quad c\,(1+k) \ \geq \ k\,F_d \quad \Longleftrightarrow \quad c \ \geq \ k\,(F_d - c),$$

which is Equation (9).

Now suppose we are in the first piece of the min, meaning we are in the case with $F_d \leq c$. Here, the condition for persuasion being successful is always met since $\tau = k/(1+k) < 1$. Furthermore, Equation (9) will always hold since $c > 0$, $k > 0$, and $F_d - c < 0$.

□

[49]Note that any split that splits the likelihood $F_d - c$ across the unfavorable states ($R_\varnothing$ and N) would work. One example would be $\pi^a_{R_\emptyset} = \pi^a_N = \frac{F_d - c}{a+b}$, while setting $\pi^a_{R_S} = 1$.

Online Appendix E: Cost Effectiveness

Here, we explore the cost-effectiveness of our intervention. We first estimate the cost-effectiveness of our intervention at raising female employment outcomes, and then compare this to other interventions in the literature.

E.1 Cost-Effectiveness of Communication Training

E.1.1 Cost Estimates

We begin by estimating the per-person cost of our intervention. The goal is to estimate the marginal cost of delivering the intervention to one woman, excluding fixed costs such as curriculum development and facilitator training. The fixed costs are incurred once and, at scale, would be spread across a much larger number of participants, driving the fixed cost per participant toward zero. Marginal cost therefore provides the more policy-relevant metric for assessing cost at scale. Importantly, our intervention was only effective in the woman-more-interested subgroup; an initial screening survey would be needed to identify this group. Thus, we also include the per-person marginal cost of the baseline survey.

Table E.1 summarizes the cost of delivering the communication training. We hired four classes of field personnel through JPAL to deliver the intervention: (1) surveyors, to either facilitate the meeting groups or support with logistics of group meetings, (2) supervisors, for quality monitoring, (3) monitors, for quality monitoring and administrative support, and (4) a project associate, for hiring and training, and overall field supervision and management. We hired 80 surveyors in total during our intervention phase, but half were involved in delivery of the control rather than treatment meetings so we include 40 in this cost exercise. Accordingly, the numbers of other personnel types hired in Table E.1 are half of the total number we hired. The unit daily costs reflect what we paid when hiring through JPAL at the time of the intervention.

We estimate paying each person for 22 days, based on the following assumptions. Each facilitator was assigned six meeting groups and met with two per day, meaning sessions were completed in 18 work days. Two meetings per day was a conservative assignment, which gave time for re-training on some days or holding a third meeting if one from another day had to be re-scheduled. JPAL field staff work six days a week, and have paid offs on Sundays and 12 JPAL-wide holidays per year; thus there will be four paid offs for the average stretch of 18 work days, bringing us to 22 days of pay.

We include two additional costs: transportation and office expenses. We paid for several cars and drivers to transport members of our field staff around the field. Using expense

Table E.1: Communication Training Costs

	N Units (1)	N Days (2)	Unit Daily Cost (Rs.) (3)	Total Cost (4)
Panel A: Itemized Costs				
Surveyors	40	22	804	707520
Supervisors	5	22	966	106260
Monitors	2	22	1174	51656
Project Associates	1	22	1475	32450
Transportation				195639
Office Expenses				14782
Panel B: Totals				
Total Cost (Rs.)				1108307
Cost per Participant (Rs.)				1423
Cost per Participant (USD)				17

records from around the time of the intervention, we estimate the total transportation costs for intervention delivery and divide by two since our cost estimate is for half the field team. The office expenses include rental for the field office (where the team would convene when not in the field) and printing expenses (for the facilitator manual and visuals used in the communication curriculum). For the former, we take the monthly rate we paid during the study and scale it to 22 days for a field team of half the size. We estimate the latter using printing expenses incurred around the time of the intervention and dividing by two (since the cost estimate should exclude costs of printing materials for the control meetings).

Panel B of Table E.1 totals the above costs in rupees, divides by the number of treated participants (779), and divides by the dollar-rupee exchange rate from November 1, 2022 (82.68). This gives a per-person cost of the communication intervention of $17.

We follow a similar approach to construct the cost estimates for baseline, summarized in Table E.2. We assume the same field team structure and unit daily costs as for the intervention. To obtain the number of days of field team pay, we compute the actual number of surveyor $\times$ days used to conduct the baseline survey, divide by two since we are only getting an estimate for the treatment group, divide by the number of surveyors we would hire for this cost exercise (40), and finally inflate for the paid offs as above. This gives six paid days, one of which is a paid off.

We estimate the transportation costs by assuming the same daily rate as during the intervention. For baseline office expenses, we include field office rental, tablet rentals, and a

Table E.2: Baseline Survey Costs

	N Units (1)	N Days (2)	Unit Daily Cost (Rs.) (3)	Total Cost (4)
Panel A: Itemized Costs				
Surveyors	40	6	804	192960
Supervisors	5	6	966	28980
Monitors	2	6	1174	14088
Project Associates	1	6	1475	8850
Transportation				54344
Office Expenses				20670
Panel B: Totals				
Total Cost (Rs.)				319892
Cost per Participant (Rs.)				411
Cost per Participant (USD)				5

SurveyCTO subscription (all surveys were completed on tablets through SurveyCTO). We obtain the field office rental rate by assuming the same daily rate as during the intervention. We assume a tablet would be rented for each of the 48 members of the field team and multiply by the daily per-tablet rental price implied by JPAL's weekly tablet rental rate. Finally, for SurveyCTO, we take the monthly rate we paid for our study's subscription around the time of the intervention and scale for the number of days of baseline.

As before, we total the costs, divide by the number of treated participants, and convert to dollars using the exchange rate above, giving a per-person baseline survey cost of $5.

This gives $22 for the total marginal cost of the intervention plus screening survey.

E.1.2 Cost-Effectiveness

We estimate cost-effectiveness in two ways. First, we compare the cost to the effect on total program earnings in our subgroup. Identifying and training the subgroup in communication skills cost $22 per person, and this increased average program earnings in this group by $11 (Rs.924 at the 82.68 exchange rate above, Table 1). In other words, the average woman recouped half of the cost in 10 months of work through the firm.

While this is a clean measure of cost-effectiveness, much of the policy interest in female employment comes from its benefits for women's empowerment, children's wellbeing, and economic growth (Heath et al., 2024), which this first measure does not necessarily capture. Thus, we also assess cost effectiveness in a second way: we compute the cost of raising a

female employment outcome by one unit with communication training, and compare this to the same statistic from other studies below. As before, we focus on the effects in the woman-more-interested subgroup as the $22 includes the cost of screening for this group. The treatment increased employment at EL2 in this subgroup by 7.3 p.p. (Table 1). The effect on our longest-run measure of employment – participation in month 10 of the firm's program – is 2.7 p.p. (Table A.13). Thus, the cost of getting one woman employed at EL2 is $301, and the cost of getting one woman to participate in the firm's program at 10 months is $815.

E.1.3 Discussion

Before comparing our intervention's cost-effectiveness to that of others, we discuss factors that may lead our numbers to over- or under-state the cost-effectiveness. There are two main reasons these numbers may under-state cost-effectiveness. First, we used an active control group. While we cannot estimate it directly, we suspect the active control group boosted employment relative to no intervention, e.g. by making women and their families more comfortable with the women leaving home, improving women's social networks, or empowering women more generally. Thus the treatment effects are likely to underestimate the effect of communication training relative to status quo, the comparison relevant for the policy decision around scale-up. Second, we include the baseline survey in the cost simply to identify the woman-more-interested subgroup, but the baseline costs reflect the full duration of our baseline survey, not just the questions needed to identify the subgroup.

On the other hand, there are a couple reasons why our estimates may over-state cost-effectiveness. First, our study was conducted in the context of an expansion of jobs for women. We of course cannot know what our effects would be in a different context, but we note that many of the interventions we compare ours to below were also done in this sort of context.[50] Second, we do not include participant compensation in the $22 estimate, even though women were given small gifts for completing the baseline survey and each communication training session. Our assumption is that if the government were to roll out this intervention at scale, it would not compensate participants for attendance. We also suspect it would be easier for the government to generate high attendance without gifts than it was for our team, as JPAL is not an organization participants are familiar with. That said, one could round our $22 cost to $25, which would make our measures of cost-effectiveness:

[50]McKelway (2025) and McKelway (2026) were also conducted in the context of Obeetee job expansions (in different villages from those studied in the present paper), Dean and Jayachandran (2019) was conducted among teachers who already had jobs, Garlick et al. (2026) was conducted on a job matching platform, where application was measured conditional on an offer, and the ITI vocational training includes placement cells to help graduates find jobs.

women recouped 44% of the cost in 10 months, while it cost $342 to get one woman employed at EL2 and $926 to get one woman to participate in the firm's program in month 10.

E.2 Cost-Effectiveness of Other Interventions

We compare the cost-effectiveness of our intervention to that of others evaluated in the literature. We focus on interventions meeting three criteria: (1) their causal impacts on female employment outcomes have been evaluated, (2) cost estimates are available, and (3) they were implemented in South Asia. We impose the final criterion because constraints to women's employment vary by region (Field et al., 2026; Heath et al., 2024) and influence an intervention's effectiveness. We have identified five such interventions and compute cost-effectiveness measures comparable to those computed above for our study. We detail our findings in Table E.3 and the bullets below, before drawing conclusions on cost-effectiveness at the end of this section.

Table E.3: Cost-Effectiveness of Other Interventions

Intervention Number	Intervention	Citation(s)	Cost-Effectiveness	Comparable Communication Statistic
(1)	(2)	(3)	(4)	(5)
1	Vocational Training	ASER Centre (2022), Maitra and Mani (2017), NITI Aayog (2023)	$6536 per employed woman	$301 per employed woman
2	Self-Efficacy Training	McKelway (2025)	$481 per employed woman	$301 per employed woman
3	Business Training	de Mel et al. (2014)	$12-88 per monthly $ earned	$20 per monthly $ earned
4	Female-Only Job Transport	Garlick et al. (2026)	$390 per employed woman	$301 per employed woman
5	Family Video Messaging	Dean and Jayachandran (2019), McKelway (2026)	$39 per employed woman (minimum)	$301 per employed woman

1. *Vocational Training.* Vocational training is a standard active labor market policy that has been a significant policy focus for governments in developing countries. India has a large network of Industrial Training Institutes (ITIs) that provide vocational training

as well as placement cells to help graduates find jobs. In 2022, India's ITI network had an annual training capacity of about 3 million people (ASER Centre, 2022) and an annual expenditure of $1.176 billion (NITI Aayog, 2023). Prior work has estimated that vocational training for women in India increased their employment six months later by 6 p.p. (Maitra and Mani, 2017).[51] Using a conservative assumption that ITIs are 100% full each year, this implies a cost of getting one woman into a job through an ITI of roughly $6536. The comparable number from our study – the $301 to get one woman employed at EL2 – is much lower.

2. *Self-Efficacy Training.* McKelway (2025) evaluates a psychosocial intervention to raise women's generalized self-efficacy in villages very near those studied in the present paper. While the paper does not compute cost, the treatment was delivered in a very similar format to our communication training, except that it was nine rather than six sessions. Thus we inflate our communication training cost by 50%, estimating the self-efficacy training cost $26 per person. Pooling data from three endline surveys done over the following year and pooling across a cross-randomized intervention, the self-efficacy treatment overall had a positive but statistically insignificant effect on women's employment of 1 p.p. If one instead compares the effect of the self-efficacy treatment alone relative to neither treatment, the effect is 5.4 p.p. and significant, though even this effect is concentrated in the shorter-run surveys. Even if we take the 5.4 p.p. effect, the cost per woman employed is higher than with communication training: $481 versus $301.

3. *Business Training.* de Mel et al. (2014) evaluate business training for women in Sri Lanka, randomizing access to the most commonly-used business training course in developing countries. They consider impacts separately for current and potential business owners; we focus on the latter group, which is more comparable to our sample and experiences stronger effects from the training. In this group, the treatment increased total monthly work income by 212 and 1494 Sri Lankan Rupees 3-8 months and 15-25 months post-training, respectively, though only the latter is statistically significant. The training cost 18,600 Sri Lankan Rupees per person. This means the intervention cost per monthly participant dollar earned is $88 at the first time horizon and $12 at the second. The comparable number from our intervention, taking the costs above and converting the total earnings effect in our subgroup (Table 1) into a monthly effect, is $20, on par with the cost-effectiveness of business training.

[51] This is consistent with estimates from work in other developing countries (Field et al., 2019), including a meta-analysis (Stöterau et al., 2022).

4. *Female-Only Job Transport.* Garlick et al. (2026) study the effects of female-only transport to work on women's job application rates in Pakistan, where concerns about women's safety and propriety constrain female labor supply. Their experiment varies whether applicants are offered no commute service, a female-only commute service, or a male-only commute service, and further randomizes the subsidy level among those offered commute. We focus on the female-only commute option at the highest (80%) discount, the pairwise comparison that produces a significant impact on application. We assume that this impact on application translated into an effect on employment, and note that the commute offer only goes into effect if women got and took a job. The discount was off an average monthly base price of 5,669 Pakistani Rupees and the commuting service was guaranteed for at least a year; assuming, conservatively, that the service was offered for only a year, the 80% discount was thus worth 54,422 Pakistani Rupees. Thus the cost per woman employed was $390,[52] which is most comparable to, and slightly higher than, our cost of $301 per woman employed at EL2.

5. *Family Video Messaging.* McKelway (2026) and Dean and Jayachandran (2019) both evaluate video interventions that sought to raise support for women's employment from women's family members; the video in McKelway (2026) specifically promoted the Obeetee job studied in the present paper, while Dean and Jayachandran (2019) was done in the context of female teaching jobs in southern India. The video in McKelway (2026) increased overall employment and employment at a firm by 10.3 and 5.9 p.p. four months later, but at one year the corresponding impacts were -3.4 and 2.8 p.p., neither of which was statistically significant. Likewise, Dean and Jayachandran (2019) evaluate impacts at 13 months finding effects on overall employment and employment in the teaching jobs of 0.7 and -4.9 p.p., both statistically insignificant. Neither paper reports costs, but we can closely approximate the cost for McKelway (2026) as it was done in the same setting as the present study and the intervention was very similar to our information delivery activity. We estimate the cost of intervention delivery using the same approach used above to estimate the cost of the baseline survey, which gives a cost of $4 per person. The cost-effectiveness number most favorable for video interventions would be the cost of getting one woman employed at the four-month endline in McKelway (2026) of $39. This is much lower than the most comparable number from our study ($301 at EL2), but comes with the important caveat that the videos did not produce persistent effects.

[52] We use the exchange rate on January 1, 2019, as their experiment ran from September 2018 through October 2019.

We conclude that our communication training is cost-effective among skills or empowerment trainings in South Asia; our intervention is more cost-effective than the vocational and self-efficacy trainings, and on par with the business training. There is more heterogeneity outside this class of intervention. We are cost-effective relative to female-only job transport. Family video messaging is much more cost-effective than communication training in the short-run, but does not produce persistent effects. Finally, we note that cost is not available for many interventions that boost female employment in South Asia; the female bank account intervention studied in Field et al. (2021) is one such example, and this intervention is likely much cheaper than our communication training.