Abstract

India’s female labor force participation rate is among the lowest in the world. This paper shows that self-efficacy, a central concept in psychology that refers to beliefs in own ability to attain desired outcomes, can help explain this phenomenon. I outline a model in which women’s low self-efficacy constrains their employment and is self-reinforcing, and then test the model using a two-step experiment. In the model, low self-efficacy creates a vicious cycle: it keeps women from trying to work, which keeps them from learning whether they could. My experiment provides women an intervention in generalized self-efficacy (GSE). I cross-randomize an intervention to reduce family members’ opposition to women’s employment, a key external constraint to women’s work. I then randomize job offers amongst those who sign up for a local job. There are four main findings. First, I document gains in women’s GSE from the GSE intervention. Second, the GSE intervention increases women’s employment. Third, reducing external constraints raises employment, but there are no gains from combining the two interventions. Fourth, receiving a job offer raises GSE. Taken together, my results suggest that intervening in women’s GSE when employment opportunities are available can spark a virtuous cycle.
1 Introduction

Women’s employment in India is strikingly low. In 2018, India’s female labor force participation rate was 24%, a stark contrast both to a rate of 47% across low- and middle-income countries, and to a rate of 79% for men in India (World Bank, 2019a,b). The social acceptability of women’s employment is a widely discussed determinant of women’s work (Alesina, Giuliano and Nunn, 2013; Bertrand, Kamenica and Pan, 2015; Bursztyn, González and Yanagizawa-Drott, 2018; Fernández, 2013; Jayachandran, 2019) and likely constrains women’s employment in India, where traditional culture suggests women should tend to the home while men earn income. This paper considers the possibility that this norm suggests to women that they do not have the ability to work. Such beliefs could constrain employment even if women and their households are willing to break the norm.

Research in psychology has established the centrality of self-efficacy in motivating human behavior. In his textbook on self-efficacy, which summarizes much of this research, Bandura provides the following definition: “perceived self-efficacy refers to beliefs in one’s capabilities to organize and execute the courses of action required to produce given attainments” (Bandura, 1997, p.3). Despite its importance in the psychology literature, self-efficacy has received little attention in economics. This paper considers the role of self-efficacy in determining economic outcomes by studying how it influences women’s employment in India. Norms around employment may give women low self-efficacy in the employment domain. There may then exist a vicious cycle: low self-efficacy keeps women from attempting to work, which in turn prevents them from learning about their actual abilities, which keeps self-efficacy low.

I formalize this cycle with a two-period model. In period one, women choose whether or not to try to work. Whether they succeed should they try depends on their ability, and their self-efficacy is their expected value of their ability. In the second period, women who try either succeed in becoming employed or fail, then update their self-efficacy accordingly. Low self-efficacy could be self-reinforcing. It keeps women from trying, which means they do not learn whether they could succeed, and self-efficacy remains low. A vicious cycle could be broken with an intervention that increased women’s self-efficacy. An intervention that alleviated constraints to employment external to women’s psychology may not break a cycle, even if it increased employment; women who work because an external constraint is relaxed would not attribute this outcome to themselves and thus not update their self-efficacy, though employment may of course lead women to acquire other information on their capabilities.

I test the model using a randomized controlled trial (RCT) with 1,022 women in rural Uttar Pradesh, India. I partner with one of India’s largest carpet manufacturers. The firm offers paid training followed by employment in carpet weaving to women in this area.

I offered a random subset of the women a psychosocial intervention in generalized self-efficacy (GSE), i.e. self-efficacy across many domains in life. The intervention was designed with guidance from CorStone, an organization that provides evidence-based programs in psychology to similar populations. It was delivered via nine group meetings over four weeks. The curriculum asks women to recognize how their abilities could enable them to reach goals in their lives. The curriculum seeks
to raise self-efficacy in specific domains and considers a large number of domains. Importantly, the curriculum does not explicitly encourage working for income. This feature helps to isolate the effect of self-efficacy beliefs from changes in preferences for working. To control for effects of meeting attendance unrelated to GSE, I use an active control group; control-group women also attended meetings, but in these meetings, women completed group surveys about aspects of daily life in their villages.

A key constraint to women’s employment external to their psychology is the opposition they encounter from their family members. Evidence from my setting suggests husbands in India are less supportive of women working than are the women themselves (Bernhardt et al., 2018; Lowe and McKelway, 2019). Increasing GSE may give women the confidence to try to persuade their family members that they should work. An alternate approach is to target this external constraint directly. Following the GSE intervention, all women, regardless of treatment status, were informed about the job with the partner firm and given a promotion for it. I cross-randomized whether women’s family members were also given the promotion, an intervention that directly targets the external constraint. Women could have tried to convince their family members that they should take this job or another by conveying the arguments they heard in the promotion or private information on their ability to work. Whether offering both interventions produces greater employment than offering either one alone will depend on how much overlap there is in the information women and the promotion convey.

The GSE treatment produced substantial short-run increases in GSE, as measured by a standard GSE scale (Schwarzer and Jerusalem, 1995). In the weeks immediately following the intervention, GSE of women assigned GSE treatment was 0.2 standard deviations higher than that of women assigned neither treatment. The effect becomes smaller over time but does persist; one year after the intervention ended, GSE was 0.1 standard deviation higher amongst women assigned GSE treatment than women assigned neither treatment, though I am not powered to detect the effect statistically. The promotion treatment alone produced no effect on GSE in the short run, and a negative but non-significant effect at one year.

The GSE and promotion interventions meaningfully increased women’s employment. I measure employment using official records of sign-up for the firm’s program (sign-up occurred two weeks after the GSE intervention ended) as well as self-reports of work for income in any sector at multiple endlines. The GSE treatment alone had little effect on working for income in the weeks immediately following the intervention, but produced significant increases in take-up of the firm’s program and in working for income four months after the intervention. These effects are large in magnitude, at 29.9% (5.8 percentage points off a base of 19.4) and 37.5% (8.7 percentage points off a base of 23.2). This suggests that absent any intervention, there are women who could work but are constrained by low assessments of their own abilities. The promotion alone produces large increases of 51.4%, 56.7%, and 42.2% on these three outcomes, which suggests that family opposition may constrain women’s employment but can be overcome with a light-touch intervention. Combining the two interventions provided no further employment gains beyond offering either one alone.
There are no effects of either treatment on employment at one year. This is not because employment in the treated groups fell; in each of the three treated cells, at least as many women were working for income at the final endline as at the earlier ones. Instead, the control group caught up to the treatment groups. Spillovers are a likely explanation, given that treatment status varied within neighborhoods, but the project was not designed to test for them.

Survey evidence suggests the GSE treatment increased employment by giving women the confidence to persuade their family members that they should work. I test this channel using reports of household decision-making about sign-up for the firm’s program. GSE treatment increased family members’ interest in women signing up along with women’s predictions of this interest. Further, GSE treatment significantly reduced both woman and family member reports of the extent to which they disagreed about whether the woman should sign up. This suggests the intervention reduced the gap within the household in preferences for women’s work. I find some evidence that GSE treatment increased women’s interest in signing up and no evidence that it affected whether women made the sign-up decision.

One concern is that the GSE intervention could have led women to work who were not able to succeed in the workplace. Comparing women’s performance in the partner firm’s program across treatment groups, I find no evidence for this concern; if anything, women in one of the two GSE treatment cells performed better than women who were assigned neither treatment.

I test the model’s prediction that trying to work provides signals from which women form their GSE in a second experimental step. Exploiting oversubscription for the firm’s program, I randomly allocated 100 slots amongst a group of 256 women in my sample who signed up. All women who signed up can be seen as having tried to work and the job offer seen as the signal of whether they succeeded or failed. Women were told a lottery allocated slots, but subsequent qualitative work revealed it is not widely understood in this setting what a lottery is. Eleven months after job offers were given, just 17.2% of women believed offers had been allocated by chance or lottery. This suggests women may have drawn inferences about their capabilities from the job offer outcome.

Three months after offers were allocated, the GSE of women who were given an offer is a significant 0.2 standard deviations higher than that of women who signed up and were not given an offer. This is a large effect, about equal to the effect of GSE treatment on GSE in the weeks immediately following that intervention. This result suggests that women do indeed update beliefs of their capabilities based on the result of pursuing work. Additional analyses suggest this effect is driven by the positive signal the offer provided rather than the employment it allowed.

Taken together, these results suggest there may exist a vicious cycle in women’s psychology. Absent any intervention, some women who could work do not try to, and this means they do not learn whether they could. An intervention in women’s self-efficacy can spark a virtuous cycle when work opportunities are available to them.

My paper contributes to three bodies of literature. First is literature on the constraints to women’s employment in developing countries. There exists evidence on the employment effects of access to jobs suitable for women (Carranza, 2014; Heath and Mobarak, 2015; Jensen, 2012),
women’s education (Andrabi, Das and Khwaja, 2012; Erten and Keskin, 2018; Keats, 2018), trade (AlAzzawi, 2014; Gaddis and Pieters, 2017), norms (Alesina, Giuliano and Nunn, 2013; Bursztyn, González and Yanagizawa-Drott, 2018), and decision-making within the household (Dean and Jayachandran, 2019; Field et al., 2019; Heath and Tan, 2019). Notably, the existing literature focuses on external constraints; my results provide evidence of internal, psychological constraints. Closest to my findings, Baranov et al. (2018) find employment effects of cognitive behavioral therapy for depressed mothers.

Second, a nascent literature finds that psychological counseling can not only be implemented effectively and at low cost in poor settings, but also produces meaningful economic improvements (Baranov et al., 2018; Blattman, Jamison and Sheridan, 2017; Campos et al., 2017; Ghosal et al., 2019; Hanushek, John and Orkin, 2019; Heller et al., 2017; Rojas Valdes, Wydick and Lybbert, 2018). I find psychological counseling can cause meaningful economic impacts in a population that is more general than those in many existing studies. Moreover, though self-efficacy is a widely studied concept in psychology with important implications for poverty, my study is the first large-scale field experiment to investigate the economic effects of an intervention in GSE.

Finally, my results contribute to the empirical literature on poverty traps. Many have speculated that psychological poverty traps may exist (Haushofer and Fehr, 2014; Ridley et al., 2019). The existing evidence shows that improving mental wellbeing reduces poverty in some settings and that alleviating poverty improves mental wellbeing in others (Banerjee et al., 2015; Haushofer and Shapiro, 2016). My paper contributes to this literature by documenting both directions of causality in a single setting. Testing whether this vicious cycle indeed creates a trap is an important agenda for future research.

The remainder of the paper proceeds as follows. Section 2 provides relevant background information, discussing women’s employment in the setting, the concept of GSE, and the GSE intervention. Section 3 provides the theoretical framework. I detail the experimental design in section 4, and the data and empirical strategy in section 5. Effects of the GSE and promotion interventions on GSE are presented in section 6. Section 7 presents effects on women’s employment, documents channels behind these effects, and compares workplace performance across treatment groups. Effects of the job offer on GSE and tests of mechanisms driving the effects are presented in section 8. Section 9 concludes.

2 Background

2.1 Women’s Employment in Rural Uttar Pradesh, India

Uttar Pradesh is a state in the north of India. It is one of India’s poorest states, and rural areas are particularly impoverished. Levels of women’s employment in the setting are low. In my control group at baseline, 31.9% of women had done any work for income in the previous two weeks. The corresponding statistic for women’s husbands is 75.5%.

Women themselves report interest in working for income. On an endline survey, I asked women
how many days they would like to work for income in the coming month and how much income they
would like to earn in that period. 75.0% of women in the control group reported positive values for
either question. At that time, only 23.2% of these women were working for income. This mirrors
a national pattern; across India, over 30% of women who are occupied primarily with domestic
activities say they would accept work if it were available at their households (Fletcher, Pande and
Troyer Moore, 2017).

Two other features of women’s employment in the setting deserve note. First, women rarely
make decisions about their own employment. On an endline survey, just 29.9% of women in
the control group said they make, either alone or together with others, decisions about whether
they should work outside of the home. Second, women’s husbands report less support for women
working than women themselves do; Lowe and McKelway (2019) document this pattern in the
area of rural Uttar Pradesh where the present study was conducted, while Bernhardt et al. (2018)
provide evidence of it in a neighboring Indian state. Opposition from women’s family members to
women’s employment is a premise of the experiment of Dean and Jayachandran (2019).

2.2 Generalized Self-Efficacy (GSE)

I offer women in this setting an intervention in generalized self-efficacy (GSE). Understanding GSE
begins with understanding self-efficacy. Self-efficacy is a concept in psychology that was initially
proposed by Albert Bandura (1977) and became enormously influential. In his textbook on self-
efficacy, Bandura gives the following definition: “perceived self-efficacy refers to beliefs in one’s
capabilities to organize and execute the courses of action required to produce given attainments”
(Bandura, 1997, p.3). In other words, self-efficacy determines an individual’s beliefs about her
ability. Note that self-efficacy beliefs are domain specific in that they pertain to abilities to reach
particular outcomes.

Further understanding of self-efficacy can be gained by comparing it to related but distinct
concepts. Self-esteem is similar to self-efficacy in that it describes one’s thinking about oneself,
but self-esteem describes one’s overall evaluation of one’s own worth whereas self-efficacy describes
beliefs about one’s ability to attain given outcomes. Another related concept is locus of control.
One’s locus of control describes the extent to which one believes own ability and effort, as opposed
to external factors like luck or powerful other people, influence outcomes in life. Locus of control
describes the extent to which an individual believes that outcomes in her life respond to her ability,
whereas self-efficacy describes an individual’s belief that she possesses a particular ability. Self-
confidence is a term typically used outside of psychology and is often used synonymously with
self-efficacy but without the domain specificity.

A vast literature in psychology suggests self-efficacy is central in motivating human behavior.
Much of this literature is summarized in Bandura (1997). Much of the evidence comes from experi-
ments in labs, where self-efficacy can be manipulated in isolation of related aspects of psychology.
For example, Peake and Cervone (1989) ask participants to assess their own efficacy but experi-
mentally manipulated the assessments via anchoring. Those randomly induced to have higher
perceptions of their own efficacy persist longer in solving hard problems.

A challenge in designing my intervention was the choice of domain to intervene in. Employment would seem a natural domain, but in practice, a field intervention that sought to enhance women’s beliefs of their capacity to succeed in the workplace could have become a promotion of employment. Such an intervention could have shifted women’s preference for working instead of or in addition to their efficacy belief.

I address this concern by intervening in generalized self-efficacy (GSE). Psychologists discuss (Jerusalem and Schwarzer, 1992) the importance of a sense of self-efficacy across many domains of life, which is referred to as generalized self-efficacy. The curriculum of my intervention, detailed in section 4, asked women to recognize their capabilities in specific domains and considered a large number of domains. Recognition of capabilities in particular domains can be seen as increasing self-efficacy in those domains, and self-efficacy was then generalized by considering many domains. Note that to avoid the concern that the intervention might have promoted women’s employment, none of the domains included in the curriculum involved work for income.

The Schwarzer and Jerusalem (1995) scale is a standard scale from psychology that measures GSE. I use this scale with minor modifications to measure GSE in my context. The adapted version of the scale is provided in appendix table A.1.

3 Theoretical Framework

I outline a model in which women’s low GSE can constrain their employment and be self-reinforcing. I assess policies that might increase women’s GSE or employment in the context of the model. The model serves to guide interpretation of my empirical results.

I begin with the basic set-up of the model. I then embed the framework in a model of household decisions about women’s labor supply to derive predictions for women’s employment. Next, I discuss how pursuing work feeds back into GSE. The section ends with a discussion of the potential for low GSE to create a vicious cycle, and of when and which policies might spark a virtuous cycle.

3.1 Set-Up

The model has two periods. In period one, women must decide whether or not to try to reach a goal. For now this is a generic goal, but below it will be employment. Woman \( i \) reaches the goal if her output, denoted \( Y_i \), exceeds threshold \( Y \). Output of \( i \) is given by

\[
Y_i = [\theta_i + \varepsilon_i]1_i.
\]

\( 1_i \) is an indicator for \( i \) choosing to try to reach the goal. \( \theta_i > 0 \) represents \( i \)'s ability, and \( \varepsilon_i \) is a stochastic shock that is uniformly distributed between 0 and 1. I assume \( Y \in (0, 1) \) and \( \theta_i \in (0, Y) \), which together ensure that women cannot reach the goal if they do not try and that probabilities of succeeding and failing conditional on trying are between 0 and 1.
Importantly, women do not know their own ability ($\theta_i$). Women’s beliefs about the likelihood that $\theta_i$ takes a given value in its range is described by a prior distribution, $F(\theta_i)$. The distribution has mean $\hat{\theta}_i$ and variance $\sigma^2$. The shape of the distribution, $F(\cdot)$, and the variance, $\sigma^2$, are the same for all women, but the mean, $\hat{\theta}_i$, varies across women. The realization of $\varepsilon_i$ is unknown to women but the distribution from which it is drawn is known.

I define woman $i$’s GSE as her expected value of $\theta_i$, or $\text{GSE}_i = \hat{\theta}_i$. What makes $\hat{\theta}_i$ GSE rather than self-efficacy is that it describes beliefs in a generic domain.\footnote{The description of GSE in this framework has parallels in existing literature. Lybbert and Wydick (2018) model perceived self-efficacy as the returns to effort. Bénabou and Tirole (2002) and de Quindt and Haushofer (2016) model self-confidence and depression, respectively, as the returns to effort.}

Women will try to reach the goal if it maximizes their utility. I assume $i$ receives utility of $U_i > 0$ from reaching the goal and all women receive utility of 0 from not reaching the goal. Whether they succeed or fail, all women who try must pay utility cost $c \in (0, \max_i \{U_i\})$. All women are risk neutral.

In period two, any women who tried in period one learn whether or not they reach the goal. Those who succeed learn that $\theta_i + \varepsilon_i > Y$, and those who fail learn $\theta_i + \varepsilon_i < Y$. Women then revise their beliefs using Bayes’ rule. GSE at the end of period two is

$$\text{GSE}'_i = \begin{cases} 
E_i(\theta_i|\theta_i + \varepsilon_i > Y) & \text{if } i \text{ tries and succeeds} \\
E_i(\theta_i|\theta_i + \varepsilon_i < Y) & \text{if } i \text{ tries and fails} \\
\hat{\theta}_i & \text{if } i \text{ does not try}
\end{cases}$$

### 3.2 Period One: Effects on Employment

Given many women in my setting would like to work, I view employment as a goal of women. Trying to reach this goal may take a variety of forms. In this section, I consider a particular form of trying for which I observe empirical support: trying to persuade one’s husband that one should work.

#### 3.2.1 Household Decisions about Employment

I embed the first period of the model in a model of household decision-making about women’s labor supply. Following Chiappori (1992), I model the household as making decisions by maximizing a weighted sum of spouses’ utilities. Woman $i$ will work if and only if

$$\mu_i U_i + (1 - \mu_i) U_{h,i}((\theta_i + \varepsilon_i) I_i^*, \gamma_i) > 0.$$ 

The left-hand side of this equation is the net utility of $i$’s household from $i$ working. $i$ will work if and only if the net utility is positive. $\mu_i$ is $i$’s bargaining weight, $U_i > 0$ is $i$’s net utility from working, and $U_{h,i}(\cdot)$ is the net utility of $i$’s husband from $i$ working.

The first argument of $U_{h,i}(\cdot)$ represents persuasion from the wife. I view women as holding private information about the merits of their work, which could take the form of benefits of particular
job opportunities women learn about, women’s ability to succeed in the workplace, or women’s capacity to manage household chores while working. Women choose whether or not to try to persuade their husbands of these merits so as to sway the employment decision. There are parallels between this set-up and that of the Bayesian persuasion model of Kamenica and Gentzkow (2011).

I model women’s decisions to try to persuade using the framework from section 3.1. Woman $i$’s output is $[\theta_i + \varepsilon_i]1_i$, and $i$’s output enters the household decision as an input to her husband’s utility. In this context, $\theta_i$ describes $i$’s ability to persuade her husband of the merits of her employment. It might reflect her ability to identify the merits and convey them convincingly, or how receptive her husband is to such arguments from her. $\varepsilon_i$ represents random factors influencing how $i$’s husband responds to her persuasion. Trying to persuade is costly, and women are uncertain of how successful their efforts to persuade would be.

The second argument of $U_{h,i}(\cdot, \gamma_i)$, $\gamma_i$, reflects other influences on the husband’s utility aside from the wife. $U_{h,i}(\cdot)$ is strictly increasing in both arguments.

$i$’s decision to try to persuade her husband is given by

$$1^*_i = \begin{cases} 
1 & \text{if } \mu_i U_i + (1 - \mu_i) U_{h,i}(0, \gamma_i) < 0 \text{ and } U_i \times \hat{Pr}_i [\theta_i + \varepsilon_i > Y] - c > 0 \\
0 & \text{if not}
\end{cases}$$

where $Y = U_{h,i}^{-1} \left( \frac{\mu_i U_i}{1 - \mu_i}, \gamma_i \right)$. Women will try if and only if two conditions are met. Because trying is costly, the first condition is that women would not work if they did not try. The second condition is that the expected utility of trying exceeds that of not trying. $\hat{Pr}_i [\cdot]$ is $i$’s perceived probability that her trying will allow her to work and is determined by the prior distribution $F(\theta_i)$. This means $U_i \times \hat{Pr}_i [\cdot] - c$ is $i$’s expected utility of trying. $i$ tries if and only if this exceeds the utility from not trying, which is 0 given the first condition.

### 3.2.2 Effects of GSE on Employment

GSE enters the employment decision by influencing how likely women believe they are to successfully persuade their husbands should they try to. A policy that increased GSE would raise the perceived likelihood and make women more likely to try. This would increase women’s employment if and only if the women led to try can successfully convince their husbands.

Put differently, low self-efficacy constrains employment for any women who (i) will not work if they do not try (i.e. $\mu_i U_i + (1 - \mu_i) U_{h,i}(0, \gamma_i) < 0$), (ii) could succeed in convincing their husbands if they tried (i.e. $\theta_i + \varepsilon_i > Y$), and (iii) do not have the self-efficacy required to try (i.e. $U_i \times \hat{Pr}_i [\cdot] - c < 0$). A policy that raised self-efficacy and resulted in $\hat{Pr}_i [\cdot] > \frac{c}{U_i}$ for such $i$ could allow them to work.

This discussion highlights convincing husbands as the channel through which GSE may affect women’s employment. In principle, GSE could also affect employment through $\mu_i$ or $U_i$, but empirically, I find limited support for these alternate channels.
3.2.3 Effects of Targeting External Constraints to Employment

As discussed in section 1, existing research on constraints to women’s employment focuses on constraints external to women’s psychology. The external constraint women face in this model is opposition to their employment from their husbands. A policy that increased GSE may allow women to overcome this constraint themselves, but a more direct approach would be a policy that increased $\gamma_i$. This could be done, for example, through a campaign that promoted woman $i$’s employment to her husband.

Given $U_{h,i}(.)$ is increasing in $\gamma_i$, this policy could allow $i$ to work. How the increase in $U_{h,i}(.)$ under this policy would compare to the increase under a policy that increased GSE is not obvious. Increasing GSE may not lead all women to try, whereas an external campaign could ensure all husbands receive a message endorsing women’s employment. On the other hand, women may be able to convey information on their individual abilities to work that an external promotion could not. Even when women and promotional campaigns convey the same information, it is not clear which would be more persuasive. It is also not clear how offering both policies would compare to offering either alone; if the two gave husbands different information, the two together may result in higher $U_{h,i}(.)$ than either alone, but if they gave the same information, there would be no gains from combining the two.

3.3 Period Two: Effects on GSE

I now turn to period two of the model. In period one, women chose whether or not to pursue work, which may have entailed trying to persuade their husbands or some other form of effort with the goal of employment. I consider how successfully reaching the goal of employment affects GSE of those who tried.

In period two, women who tried and succeed have GSE of $E_i(\theta_i | \theta_i + \varepsilon_i > Y)$, while women who tried and fail have GSE of $E_i(\theta_i | \theta_i + \varepsilon_i < Y)$. The effect of successfully reaching the goal of employment on GSE for $i$ who tried is the difference between these two conditional expectations. Applying Bayes’ rule gives

$$E_i(\theta_i | \theta_i + \varepsilon_i > Y) - E_i(\theta_i | \theta_i + \varepsilon_i < Y) = \frac{\sigma^2}{(1 - Y + \theta_i)(Y - \theta_i)} > 0.$$

The result of trying thus affects GSE. GSE’$i$ will be higher if $i$ succeeds than if $i$ fails.

3.4 Vicious and Virtuous Cycles

3.4.1 A Vicious Cycle

In this model, low GSE can be self-reinforcing. If GSE is low, $i$ will not try. If $i$ does not try, she will not receive a signal of her true ability, and GSE will remain unchanged and at the low value.
3.4.2 Can Policy Spark a Virtuous Cycle?

An intervention that increases women’s GSE could spark a virtuous cycle for women whose low GSE constrains their employment. Increasing GSE could lead women to try to work. Those led to try would acquire a signal from which they would update their GSE. For women constrained by low GSE, signals would be positive, and initial increases in GSE could lead to subsequent increases.

An intervention that targets constraints to employment that are external to women’s psychology could increase women’s employment but may or may not spark a virtuous cycle. In this model, women update their GSE when the receive a signal that is informative of their capabilities. Alleviating an external constraint to employment, could allow women to work, but this work would not be a result of women’s capabilities and it would therefore not affect GSE. Of course working could provide women signals of their capabilities even if initially becoming employed did not. When these signals are positive, a virtuous cycle could be sparked.

Combining a policy that reduces external constraints with an intervention that raises GSE could crowd out some of the learning that a GSE intervention alone would provide. This is because trying is costly; if relaxation of an external barrier gives women the opportunity to work without trying, then even women with high GSE will not try. Not trying will keep them from receiving a signal of their abilities in period two. On the other hand, like women led to work through a policy reducing external constraints alone, these women could obtain signals of their capabilities from work itself.

4 Experimental Design

I test the predictions of this model using an experiment with 1,022 women in rural Uttar Pradesh. I offer randomly chosen women a psychosocial intervention to increase GSE. Following this intervention, all women, regardless of treatment status, were given details about a local job opening for women along with a promotion for it. Women could choose to pursue employment by trying to persuade their family members of the arguments made in the promotion or of privately known merits of their employment (e.g. their ability to succeed in the workplace or to manage household chores while working). I cross-randomize an intervention that directly targets the external constraint of opposition from family members; I cross-randomized whether women’s husbands, mothers-in-law, and fathers-in-law were also given the promotion. The model discusses women’s husbands rather than family members to follow the convention in the intra-household literature, but in practice, I involve parents-in-law in addition to husbands as parents-in-law are influential in household decisions in this setting.2 In a second step of the experiment, I randomize the outcome of trying to work. In particular, I randomly allocate job offers amongst a group of women who signed up for the job opening. The design and timeline of the interventions are visualized in figure 1. This section details the interventions and their implementation.

2Dean and Jayachandran (2019) also take the approach of involving both husbands and other influential family members.
4.1 Partner Firm

I partner with a large carpet manufacturer that offers an employment opportunity to women in rural Uttar Pradesh. Carpet weaving is a common occupation in the setting but typically employs men. The company recently began a program to train and employ women as weavers. The program occurs in newly constructed weaving centers, each of which employs 20 women from surrounding village neighborhoods; participating women would work in a new workspace, would live a relatively short walk from the center, and would only work with other women. The construction of these centers is undertaken through a partnership between the firm and a village loom owner. Women that enroll sign up for four months of training. Those who complete training may work as weavers in the center or elsewhere. The firm ensures women are paid a stable and respectable wage throughout the training period. After training, the firm orders carpets from the loom owner, who then distributes assignments and payments to the weavers. Post-training payments are determined by loom owners and are typically at least as high as training pay.

The experiment was conducted in the catchment areas for six new women’s weaving centers. Catchment areas were defined as a group of bastis (i.e. neighborhoods within villages that almost always consist of households from the same subcaste) from which the loom owner would recruit women for the program in absence of the study. Loom owners selected bastis in close proximity to the loom center and where weaving is a common occupation amongst men living in the basti. The latter condition meant bastis where the wealthiest (“general”) subcastes live were excluded.

4.2 Sample: Recruitment and Baseline Characteristics

Surveyors went door-to-door in the catchment areas to recruit women for the study. Based on questions asked of household heads, surveyors identified women who might be eligible to participate in the study. If they were part of the household and available to speak, the surveyor then asked such women’s husbands, mothers-in-law, and fathers-in-law for permission for the women to participate in the study. Loom owners selected bastis in close proximity to the loom center and where weaving is a common occupation amongst men living in the basti. The latter condition meant bastis where the wealthiest (“general”) subcastes live were excluded.

In total, women needed to meet seven criteria to be eligible to participate in the study: (i) were at least 18 years of age and no more than 40, (ii) were not disabled, (iii) were available to speak with the surveyor in person, (iv) had no plans to leave the village for an extended period anytime in the following six months, (v) were married, widowed, divorced, or separated, (vi) had not had permission to participate in the study denied by family members, and (vii) were not the mother or mother-in-law of another eligible woman in their household. (i) and (ii) are requirements from the partner firm for the women to be eligible to participate in their weaving program; (iii) eased logistics of inviting women to participate in the study and of completing baseline surveys; (iv) and (v) ensured the women were available for the study; (vi) and (vii) were to ensure the study was not over-enrolled.

The vast majority (99.2%) of women in the sample live in their in-laws’ villages. For those living in their own natal villages, parents replaced parents-in-law in study activities. Throughout the paper, I refer to parents-in-law rather than parents and parents-in-law for brevity.

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3The vast majority (99.2%) of women in the sample live in their in-laws’ villages. For those living in their own natal villages, parents replaced parents-in-law in study activities. Throughout the paper, I refer to parents-in-law rather than parents and parents-in-law for brevity.
(iv) and (v) were imposed to minimize attrition; (vi) helped to prevent future issues with women's households; and (vii) prevented individuals from participating in the study as both women and mothers-in-law. Across the 1,385 households surveyors spoke with, 1,039 women were deemed eligible. This represents 41.7% of all adult women on household rosters, and 62.8% of all women in the age range on household rosters.

In total, 1,022 (98.4% of the 1,039 eligible) women from 927 households enrolled in the study. Baseline characteristics of the women in the study assigned GSE and promotion control are in column (1) of table 1. The average age at baseline was 29.5. 98.4% of women were married and 99.2% lived in their in-laws' villages. Women had 2.7 children on average, and 7.8% of women were pregnant at baseline. 41.9% of women had no education (in contrast to 13.6% of husbands) and 31.9% were working for income (in contrast to 75.5% of husbands). The baseline survey coincided with the end of rice sowing season, an agricultural phase that involves much female labor, so this is a relatively high level of women's employment for the setting.

During this time, the research team introduced themselves as part of a J-PAL/IFMR team working to understand the daily lives of younger adult women from subcastes like theirs in rural India. Importantly, the team’s affiliation with the partner firm and the women’s weaving opportunity were not mentioned. This kept participants from selecting into (or out of) the sample based on their attitudes towards the opportunity or the firm, and from interpreting the GSE intervention as motivated by the partner firm.4

4.3 GSE and Promotion Treatment Assignment

Women who enrolled in the study were given GSE and promotion treatment assignments. The GSE intervention would be delivered in meetings with groups of women; the first step of randomization was assignment of women to meeting groups. Each group consisted of roughly six women from the same basti. I first assigned each household, possibly including multiple enrolled women, to a meeting group in its basti. I then randomly assigned each meeting group to GSE treatment or control, stratifying by geographic unit.5 Finally, whenever a meeting group contained multiple women from the same household and its basti had at least one other meeting group assigned the same treatment status, individual women from that household were reassigned to a different meeting group of the same status. The goal of reassignment was to allow women more freedom to discuss issues at home in a group meeting without another member of their household present. Reassigned women were chosen at random from all women in their households and assigned to the smallest other group in their basti of the same treatment.6 In total, 177 meeting groups were formed, each

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4 Regardless of their beliefs about the research team’s involvement in it, villagers were likely aware that the women’s weaving opportunity would soon become available given loom centers were under construction and loom owners were from local families.

5 The geographic units were roughly equal to bastis. Large bastis were divided into smaller geographic units. Small bastis that had only enough women for one meeting group were pooled with all other such bastis in their catchment areas to form a single geographic unit. If a catchment area contained only one basti with one meeting group, the pooled stratum included all bastis with one or two meeting groups in that catchment area.

6 If groups tied, the group was chosen at random from the tied groups.
including at least three and no more than eight women. I then assigned promotion treatment. Assignment was at the household level and randomization was stratified by GSE treatment and geographic unit.

Table 1 also tests for balance on baseline characteristics by GSE and promotion treatment assignment. Most tests suggest balance but, as one might expect with many characteristics and multiple comparisons for each, there are several imbalances. I use the post-double-selection LASSO method of Belloni, Chernozhukov and Hansen (2014) to select control variables when estimating GSE and promotion treatment effects, which should address imbalances.

4.4 GSE Intervention

The GSE intervention began one week after study enrollment ended and lasted for four weeks. During this period, women were invited to nine meetings with their assigned groups. The assignment of each group to GSE treatment or control determined meeting content; in these meetings, groups assigned GSE treatment received a psychosocial intervention to increase GSE, while those assigned GSE control took group surveys.

The GSE intervention was designed with guidance from CorStone. CorStone (corstone.org) is an organization that offers programs in resiliency, an aspect of psychology related to GSE, in disadvantaged communities worldwide. CorStone’s programs are guided by research from psychology and related fields, and are evaluated using rigorous research. The GSE curriculum was based on a resiliency program CorStone offers women in Bihar, India. With guidance from CorStone, I selected content from the resiliency program most relevant for GSE; identified conceptual gaps and created new content to fill them, modeling structure, language, and activities after the resiliency program; and adapted stories and examples to fit my setting. I piloted an initial version of the curriculum in my setting and finalized the version for the experiment based on feedback from the pilot.7

The curriculum was delivered over the nine meetings with groups of treated women. Early meetings had women recognize their own abilities. In particular, the second and third meetings ask women to think about and recognize their own talents and character strengths. The fourth meeting asks women to recognize successes in their lives and identify the talents and strengths of theirs that contributed to those successes. The next meetings help women see paths to reaching their goals. This is done by teaching women a strategy for planning to reach goals in meeting six, and by promoting problem-solving mindsets for facing obstacles that might arise in meeting seven. Women in this setting tend to be unfamiliar with the concept of goals so prior to meetings six and seven, meeting five develops an understanding of what goals are and why they matter. Meeting eight ties these elements together, asking women to recognize how they can employ their own talents and strengths in following paths to reach their goals. The first and ninth meetings introduce and conclude the curriculum. Appendix table A.2 provides additional details on the content of each meeting.

Content was delivered via instruction, story-telling, group discussion, personal reflection, and

7Pilot participants are not included in the samples analyzed in this paper.
activities. Concepts were illustrated with references to specific domains of life and participants considered many domains across the nine meetings. None of these domains involved work for income. However, much of the intervention involved group discussion, and it is of course possible that work for income was brought up organically in these discussions. While the curriculum was designed to enhance GSE, it likely affected related aspects of psychology (e.g. grit, self-esteem, hope) as well as soft-skills (e.g. goal setting, planning, problem solving, communication).

The curriculum was delivered by the research team. Each group’s meetings were facilitated by a single female surveyor assigned to the group. Two research team leaders received training in facilitation from CorStone’s India team, and trained the surveyors who were assigned to deliver the intervention. Surveyors who delivered the intervention were selected based on performance in an initial training for facilitating GSE intervention meetings.

In their meetings, women assigned GSE control took group surveys on aspects of daily life in their bastis and villages (e.g. men’s employment, health, agriculture). The survey topics for each meeting are provided in appendix table A.2. The questions were meant to be purely descriptive, and groups were encouraged to discuss answers for each question before surveyors recorded a single answer for the group. Like each of the treatment meeting groups, each of the control meeting groups was assigned a single female surveyor who facilitated meetings. The control group facilitators were a set of surveyors not selected to deliver the GSE intervention. Comparisons between GSE-treated women and women assigned to this active control group hold fixed effects of meeting attendance unrelated to GSE.

Treatment and control meeting groups met in private and in locations within their participants’ bastis. Surveyors were assigned to new catchment areas after the GSE intervention to minimize demand effects.

Columns (1)-(9) of appendix table A.3 present levels of meeting attendance and test for balance in meeting attendance by GSE treatment status. The attendance rate of GSE control women is roughly 65% at each of the nine meetings. Attendance does not differ by treatment status for any of the nine meetings, which suggests comparisons between GSE treated and control women do indeed hold fixed effects of meeting attendance unrelated to the content of the meetings. Appendix figure A.1 presents distributions of the number of meetings attended, separately for treated and control women. The two distributions look virtually identical. The distributions are bimodal; they imply many women never attended a meeting, but many who did attend attended most or all of the meetings.

4.5 Promotion Intervention

The promotion intervention began one week after the GSE intervention ended and lasted one week. During this time, surveyors held individual meetings with each woman and separate meetings with each woman’s family member(s). Family members eligible to participate were husbands of married women, and mothers-in-law and fathers-in-law who lived in women’s households. All but seven of the 1,022 women had at least one eligible family member.
During these meetings, surveyors delivered information about the partner firm’s program. The research team had not mentioned its affiliation with the partner firm up to this point in the study. In these meetings, surveyors explained the partnership prior to giving job information. Surveyors said the firm had asked the research team to provide information on the program, and the research team was interested in opinions on the program as part of its goal of understanding daily lives of women.

All women, regardless of treatment status, were given both job details and a promotion for the job. Job details were basic facts about the opportunity: it was a program arranged by the firm to train and employ women as weavers, the woman was eligible to participate, the location of the loom center, the compensation scheme, the hours, and information about how to sign up. The job promotion consisted of information on job perks\(^8\) and a six-minute, promotional video for the opportunity. The video features a message from a partner firm official and testimonials of loom owners, female participants, and the husband of a female participant from villages where the program was well established. Testimonials highlight merits of the program from the individuals’ perspectives, with an emphasis on points that would make women’s family members more supportive of women’s participation (e.g. testimonials stress that only women work in the loom center and that participants have time to do their usual household chores). Interspersed with shots of the speakers are shots of some of the finest carpets the firm produced, the women’s weaving centers, and women weaving. The video was filmed and edited by the research team in collaboration with the partner firm.

All women were thus given merits of their participation in the program that their family members may be particularly responsive to. Women could relay these merits to their family members if they chose to try to persuade them.

Promotion treatment assignment determined information given to family members; those in treated households received job details and the promotion, while those in control households received only job details.\(^9\) Delivering the promotion to family members represents a policy that directly targets external constraints to employment. If women convey merits highlighted in the promotion when trying to persuade their family members, there is a sense in which the GSE and promotion treatments could be redundant; if the two sources provide family members with different information, offering both treatments may produce higher employment that offering either alone. Family member meetings were held for 79.7% of control women who had eligible family members, and this level was similar in other treatment cells (column (10) of appendix table A.3).

4.6 Job Sign-Up

The research team organized a sign-up day at the six loom centers. The sign-up day was held two days after the promotion intervention ended. Women wishing to sign up came to their loom center

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\(^8\)In particular, information about the loom center and childcare policies was provided, and the fact that the only weavers in the center would be women from the area was highlighted.

\(^9\)Women were not told whether their family members would be given the promotion.
and completed a 10-minute sign-up process. I required women be accompanied by their husbands, mothers-in-law, fathers-in-law, or household heads so as to ensure they were signing up with the support of their households. Women were asked to present an identification card at sign-up to verify they met the partner firm’s age requirement for participation (participants need to be at least 18 and no more than 40). Being in this age range was an eligibility requirement for study participation, but the data used to determine whether women met this requirement was less official and women who were in this age range at the start of the study could have aged out of it by sign-up. Any women who did not meet the firm’s age requirement could not sign up.

Some women completed the sign-up process without attending the sign-up day. This occurred primarily through an alternate sign-up day. On an endline survey administered in the week after the sign-up day, women who had not signed up and family members of women who had not signed up were asked if they were interested in an alternate sign-up day. If both a woman and her family member(s) expressed interest (or if only one of the two was surveyed and expressed interest), the woman was invited to sign up at an alternate sign-up day, held at the loom centers two weeks after the first sign-up day. There were also a few cases of individuals completing the sign-up process informally after both sign-up days, having contacted the research team about their interest in the program.

4.7 Job Offer Experiment

There was oversubscription for the program at five of the six centers; across these five centers, there were 100 slots and 256 women who signed up at one of the two sign-up days. I randomly determined which 100 women could begin the program on its first day (“job offer treatment”) as well as a random waitlist ordering for the remaining 156 (“job offer control”), creating a second step, job offer experiment. I began by randomly ordering the households that had any women who had signed up (the 256 women came from 241 households). I created a separate list for each catchment area and stratified the order by GSE and promotion treatment. Any women from households in which multiple women had signed up were then randomly ordered within their households position to create a randomly ordered list of women. Women in the first 20 positions of their lists could begin the program at its start. If a participating woman dropped out, the next woman on her catchment area’s list who had not yet been asked was invited to participate.

Surveyors visited households of women who had signed up to tell them whether or not women could begin the program. Households were told that a lottery had been used to determine which women could begin the program immediately, that the others had been put on a waitlist, and that

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10 In a few cases, none of these family members could attend so surveyors accepted permission from one of them over the phone.
11 If the woman did not present an identification card, her age was later verified with her village head (pradhan).
12 The randomization was run before women who signed up after both sign-up days did so, so these women are not part of the job offer experiment. In the five centers with oversubscription, these women were placed at the end of their centers’ waitlists, and in the sixth, these women could begin the program at its first day.
13 Note the fact that a separate list was created for each catchment area means the randomization was also stratified by catchment area.
the order in which women on the waitlist would be invited was also determined by the lottery. Surveyors clarified that results of the lottery were up to chance, and that there was nothing the individuals or their households did or did not do to influence the outcome. Individuals were told whether women won the lottery and could begin the program on its first day, or had not won and had been placed on the waitlist. These visits occurred in the days following alternate sign-up, and the program began five days after alternate sign-up. The research team monitored attendance during the first three months of training and drew women from the waitlist when participants dropped out.

Baseline characteristics of women in the job offer experiment are presented in table 2. Note these characteristics come from the baseline survey done prior to the GSE intervention. Ages and baseline employment levels of women in this subsample are similar to those in the full sample (statistics for the full sample are in table 1). GSE is higher in the subsample than in the sample overall, as is family support for women working as weavers. Women in the subsample are more likely to come from households in which members work as weavers and are more likely to have no education. Characteristics are balanced by job offer treatment.

Households were told slots in the program had been allocated by a lottery. However, subsequent qualitative work suggested it is not widely understood in this setting what a lottery is. This suggests women may have updated GSE in response to the job offer. Appendix table A.4 tabulates beliefs of women in the job offer experiment about how job offers were assigned, recorded 11 months after job offer results were delivered. Only 17.2% believed women who could begin the program immediately were chosen by chance or lottery. This is consistent with many women viewing the offer as indicative of their abilities, meaning the offer could have affected their GSE.

5 Data and Empirical Strategy

5.1 Data

Data come from two sources: surveys and records of participation in the firm’s program.

5.1.1 Surveys

I conducted a baseline and five endline surveys with women. Women’s family members were also surveyed at baseline and at two of the five endlines. Baseline surveys were done two weeks before the GSE intervention began. At the end of the final GSE treatment and control meetings, women took a week zero endline survey. Surveyors met with women one week after the GSE intervention ended to deliver job details and promotion; immediately before this information was provided, women took a one-week endline survey. Additional endline surveys with women were administered two weeks, four months, and one year after the GSE intervention ended. Family members were also surveyed at the two-week and four-month endlines.

14 In one center, construction delays postponed the start of the program by two weeks.
The family members surveyed were husbands of married women as well as women’s mothers-in-law and/or fathers-in-law if they were part of women’s households. Whenever a woman had multiple family members who were eligible to be surveyed, the group was invited to complete the baseline and two-week endline surveys. If multiple family members agreed to be surveyed, each question was asked of one family member in particular. Each family member four-month endline survey was taken with just one family member. Surveyors prioritized having husbands complete these surveys, then mothers-in-law, and finally fathers-in-law.

Surveys were administered verbally. They were generally taken in person and in private, with either only a surveyor and a woman present, or only a surveyor and a family member (or set of family members) present; the only exception is that on the family four-month endline, surveyors asked to speak to husbands over the phone if they were not available in person. Surveys were of varying length; for example, the average length of women’s zero-week endline surveys was 15 minutes, while the average lengths for women’s and family members’ four-month endline surveys were 48 and 28 minutes.

Women’s baseline surveys were conducted immediately after women enrolled in the study and were conducted for all women in the sample. Women’s family members were asked to take their baseline survey immediately before women enrolled in the study; many were not available at this time and baseline surveys in such cases were not taken. The latter four of the women’s endlines and the two family endlines were scheduled at times convenient for participants within designated time frames. Women’s week zero endline surveys were taken immediately after the final GSE treatment or control meeting so there was less flexibility in scheduling and very few women who had not attended the meeting completed the survey.

Appendix Table A.5 presents levels of attrition on endline surveys and tests for balance in attrition across treatment groups. Around 80% of women in the control group were surveyed on the one-week, two-week, and four-month endlines. Levels are lower at the zero week and one-year endlines (at 64.5% and 67.4%) and higher in the job offer experiment sample (93.8% on the four-month endline and 79.0% at one year). Family members were surveyed on the two-week and four-month endlines for just under 75% of control women who had eligible family members. Reassuringly, attrition is largely balanced across treatment groups.

Surveyor assignments to endline participants sought to minimize experimenter demand. For the one-week and all subsequent endlines, surveyors were randomly assigned to catchment areas where they had not worked during the GSE intervention. For logistical reasons, women were very often interviewed at the week zero endline by the surveyors who had facilitated their group meetings; I therefore interpret data from week zero endline surveys with caution. For endlines that occurred after the promotion intervention, surveyors were almost always assigned catchment areas where they had not worked during the promotion.\textsuperscript{15}

\textsuperscript{15}The only exceptions occurred on the four-month endline survey, when, for logistical reasons, a few surveyors were reassigned to catchment areas where they had worked during the promotion.
5.1.2 Program Participation Records

I also use records of participation in the firm’s program. Because the research team orchestrated sign-up for the program, I observe official sign-up decisions for all women in the sample. Once training began, loom centers kept paper records of trainee attendance and performance. Surveyors digitized such data for any women in the study who had signed up for the program. Some of the women in this subsample were allowed to begin the program on its first day, some were invited late when others dropped out, and some were never invited. Data were digitized for the first three months of training in five of the centers. Training in the sixth center began two weeks after the others so this center’s data are complete for the first two months only.

5.2 Empirical Specifications

The specification for assessing the effects of the GSE and promotion treatments is

\[
Y_{i,h,m} = \beta_1 T^G_{h,m_p} + \beta_2 T^P_{h,m_p} + \beta_3 T^{Both}_{h,m_p} + \mu_s + \delta X_{i,h,m} + \varepsilon_{i,h,m}. \tag{1}
\]

Each observation is a woman \(i\) from household \(h\) and meeting group \(m\), and \(Y_{i,h,m}\) is the outcome of interest for that woman. \(T^G_{h,m_p}\) is an indicator for assignment to GSE treatment and promotion control, \(T^P_{h,m_p}\) an indicator for assignment to promotion treatment and GSE control, and \(T^{Both}_{h,m_p}\) an indicator for assignment to both treatments. The subscripts on the treatment indicators reflect the fact that each indicator takes the same value for all women in a household and for all women in a meeting group with the same promotion treatment assignment. I cluster standard errors at both the household level and the meeting group \(\times\) promotion treatment level. I allow for two-way clustering using the Stata reghdfe package of Correia (2014).

\(\mu_s\) denote strata fixed effects. For certain outcomes, endline survey attrition means some strata do not have at least one individual from each treatment cell. In regressions that consider effects on such outcomes, I pool these “incomplete” strata with the strata in their villages consisting of the group of small bastis that had been pooled for GSE treatment assignment. If this pooled strata is still incomplete, I pool it with the smallest (in terms of number of observations) complete strata in its village. \(X_{i,h,m}\) is a set of baseline covariates selected from 587 potential covariates using the post-double-selection LASSO method of Belloni, Chernozhukov and Hansen (2014). I implement this method in Stata using the pdslasso command written by Ahrens, Hansen and Schaffer (2018). This command does not allow two-way clustering; I therefore do post-double selection twice, clustering at each level once. Estimates of equation (1) use the union of selected covariates as \(X_{i,h,m}\) and cluster standard errors at both levels. Note that because patterns of attrition differ across outcomes and because different baseline variables predict different outcomes, the strata fixed effects and control variables will differ across regressions.

To estimate effects of the job offer, I restrict to women in the job offer experiment and estimate

\[
Y_{i,h} = \beta T^J_{h} + \mu_{s,J} + \varepsilon_{i,h}. \tag{2}
\]
$T^J_h$ is an indicator for assignment to job offer treatment. Its subscript denotes that it takes the same value for all women in a household. Estimates allow for clustering of standard errors at the household level.

$\mu_{s,j}$ are fixed effects for job offer experiment strata. While the job offer and waitlist randomization was stratified by village, GSE treatment, and promotion treatment, I only include fixed effects for villages. Conditioning on village is essential because the probability of receiving a job offer varied across villages, depending on the number of women in the villages who signed up for the job. I condition only on village because in one village, there is no variation by job offer treatment in a GSE treatment $\times$ promotion treatment cell. It is not essential to condition on GSE or promotion treatment because the probability of receiving a job offer did not depend on prior treatment assignment. I do not use post-double-selection LASSO for analyzing effects of the job offer because of the small sample size in the job offer experiment.

6 Effects of GSE and Promotion Interventions on GSE

I begin the empirical analysis by investigating the effects of the GSE and promotion interventions on GSE. GSE is measured using the Schwarzer and Jerusalem (1995) scale. The scale asks respondents to assess, from 1-4, the extent to which 10 statements reflecting high GSE describe their lives. Through a phase of piloting, I developed a slightly modified version of the scale in the local language that could be understood by participants but was as close to the original scale as possible. The modified version of the scale is presented in appendix table A.1. I measure GSE by aggregating across the items. I form the aggregate using the following procedure: I average the 10 responses, set the average to missing if more than three of the responses are “don’t know,” and standardize against the distribution of this variable in the control group at baseline. This handling of missing responses is what is suggested by the authors of the original scale, though I consider robustness to an alternate approach below.

I find large effects of GSE treatment on GSE (table 3). In the surveys immediately following the final GSE treatment and control meetings, GSE was 0.232 standard deviations higher amongst GSE-treated women than GSE controls (p-value = 0.005). As discussed in section 5.1.1, this result should be interpreted with caution given that participants in this survey were very often interviewed by the surveyor who facilitated their GSE intervention meetings. Reassuringly, the effect is present on subsequent endlines. At one week, GSE was 0.194 standard deviations higher amongst women assigned GSE treatment and promotion control than amongst women assigned neither treatment (p-value = 0.056), and 0.222 standard deviations higher amongst women assigned both treatments than amongst women assigned neither (p-value = 0.020). Effects are similar at the two-week endline. At the four month endline, there is a significant effect of assignment to GSE treatment only (0.196 standard deviations, p-value = 0.039), but not of assignment to both treatments (0.062 standard deviations).

16 Key modifications included rephrasing items to be questions rather than statements, standardizing surveyor explanations for words and phrases that often generated questions from participants, and requiring participants to answer all questions while also adding a “don’t know” option to the 1-4 scale.
deviations, p-value = 0.528). Effects remain at one year, though they are smaller in magnitude (0.111 and 0.133 standard deviations) and I am not powered to detect them (p-values = 0.265 and 0.219). Effects on GSE immediately following the intervention reflect the direct effects of the intervention, while those measured later would reflect this direct effect as well as the learning that occurred in response to the trying the direct effect engendered. The short-run effects suggest the intervention produced a direct increase in GSE. The tapering of the effect over time is consistent with some of the women who were led to try receiving negative signals, while the persistence is consistent with many such women receiving positive signals, possibly from the labor market or from other domains.

The promotion on its own does not increase GSE at any of the endlines. Effects of promotion treatment only are small and non-significant at the one-week, two-week, and four-month endlines. At one year, there is a negative effect (-0.111 standard deviations), though it is not significant (p-value = 0.332). However, as we will see below, the promotion produces large increases in women’s employment. These results thus imply employment itself is not sufficient to raise GSE in my setting. The model suggests that women who reach the goal of employment because an external constraint is relaxed will not update their GSE based on this attainment because the attainment is not due to their capabilities. However, in the process of working, women may acquire signals of their capabilities. That the promotion has no effect on GSE could be because work provides both positive and negative signals which cancel one another out.

I now turn to effects on an alternate GSE index that uses a different handling of “don’t know” responses. The index considered thus far is defined only if seven or more questions are answered, which parallels the scoring procedure suggested by the authors of the GSE scale. However, it is possible that the treatment affects the number of “don’t know” responses (e.g. by changing how familiar women are with words in the questions or how women think about the topics the questions address), which would produce selective attrition. I therefore pre-specified two versions of a GSE index. The second version replaces “don’t know” responses with the answer choice corresponding to the lowest level of GSE (i.e. 1). The index is then formed by averaging the 10 responses after replacement of “don’t know”’s and then standardizing the average using the distribution of this average in the control group at baseline. Effects on the alternate GSE index are presented in appendix table A.6. Effects of GSE treatment are generally smaller in magnitude, suggesting treatment makes women more likely to give “don’t know” responses, but are qualitatively similar.

Another potential concern is the fact that I administer a single questionnaire many times. This may affect how comparable indices from different endlines are to one another. To investigate this concern, I surveyed a group of women living in a village outside of the six catchment areas who were demographically similar to women in the main sample. Separate sample women were surveyed once a week for three weeks, and a randomization determined whether the GSE questionnaire was on their survey each week or only in the final week. Appendix table A.7 uses data from the third week and compares GSE indices of “control” women who were taking the GSE questionnaire for the first time to those of “treated” women who were taking the questionnaire for the third time. I
find no effect of multiple measurement on either index.

7 Effects on Employment

I begin this section by presenting effects of the GSE and promotion interventions on employment. I then use reports of household decision-making about sign-up for the firm’s program to examine the underlying channels. I end by investigating the concern that GSE treatment leads women to work who cannot succeed in the workplace.

7.1 Effects on Employment

I measure employment using decisions to sign up for the firm’s program; reports of any work for income from women’s two-week, four-month, and one-year endline surveys; and reports of work for income in a rice cultivation that occurred 10 months after the GSE intervention. The sign-up outcome is an indicator for attending the official sign-up day and being in the correct age range to participate in the program. The official sign-up day occurred two weeks after the GSE intervention ended. On the two-week, four-month, and one-year endlines, I asked women whether they had worked for income in any of 10 common employment sectors\textsuperscript{17} in the preceding two weeks; I consider a woman as working for income at a particular endline if she reported work in any sector. Finally, I consider effects on an indicator for working for income in the rice sowing season that occurred 10 months after the GSE intervention. This outcome comes from women’s reports of their participation in this rice cultivation on the one-year endline survey. I do not consider participation in the firm’s program as an outcome because oversubscription meant many women who signed up were not given a chance to participate, and I do not use data from the zero- or one-week endlines because neither asked about employment.

GSE treatment alone has meaningful effects on employment in the short- and medium-term (columns (1)-(3) of table 4). The effect on doing any work for income in the two weeks following the GSE intervention is positive (3.0 percentage points, or 13.8%) but not significant (p-value = 0.448). GSE treatment alone has a large effect on signing up for the partner firm’s program, increasing sign-up by 5.8 percentage points (p-value = 0.087), which represents a 29.9% effect off a base of 19.4. The effect on working for income at the four-month endline is substantial: GSE treatment alone increases work by 8.7 percentage points, or 37.5% (p-value = 0.026). These are meaningful results. An intervention that increases women’s beliefs about their abilities throughout life, and not specifically in the employment domain, raises their employment. This suggests that absent intervention, many women who do not work could work but are constrained by low beliefs of their abilities.

\textsuperscript{17}The 10 sectors were agriculture on own household’s land, agriculture off own household’s land, husbandry of animals owned by own household, husbandry of animals owned by others outside of own household, own household’s microenterprise, casual non-farm labor, employed by a firm, anganwadi work, teaching, and NREGA. Participants were able to report work that did not fit into one of these 10 as work in some other sector. On the four-month endline, after the firm’s program had begun, the program was added as a sector.
The promotion alone has large effects on short- and medium-run employment. It increases any work for income in the two weeks following the GSE intervention by 11.2 percentage points, a massive 51.4% increase (p-value = 0.003). This is particularly remarkable given the promotion intervention was delivered over a week within this two-week window. Further, the partner firm’s program began a month after the GSE intervention ended so this work cannot reflect participation in the program; though the promotion advertised this program in particular, it appears to have increased family members’ support for women’s work in general. The effect on sign-up is also enormous: 11.0 percentage points, or 56.7% (p-value = 0.003). The effect of the promotion alone persists to the four-month endline, when it increases any work for income by 9.8 percentage points, or 42.2% (p-value = 0.017). Thus an intervention targeting intra-household opposition to employment, a key barrier to employment external to women’s psychology, produces large increases in women’s employment. That a light-touch intervention targeting the preferences of women’s family members for women’s employment has meaningful effects on employment decisions parallels results of Bursztyn, González and Yanagizawa-Drott (2018). My results contrast Dean and Jayachandran (2019), who find no effects of a video intervention designed to make family members more supportive of women’s employment. This may be due to a difference in the populations under study; Dean and Jayachandran study a sample of women who were employed at baseline, whereas my sample was largely unemployed at baseline.

Combining the two interventions provides no employment gain beyond offering either alone and, on some outcomes, is worse for employment than the promotion only. For any work at two weeks and for sign-up, the effects of offering both interventions relative to offering neither are positive (3.7 and 2.4 percentage points, or 17.0% and 12.4%) but not significant (p-values = 0.298 and 0.484). These effects are no larger than the effects of either intervention alone on these outcomes and much smaller than the effects of the promotion alone (the p-values for the tests that the effects of both treatments equal the effects of the promotion alone are 0.041 and 0.025). Offering both interventions rather than neither has a positive, large, and significant effect on employment at four months (7.0 percentage points or 30.2%, p-value = 0.097), but this effect does not differ from the effect of either intervention alone (p-values = 0.697 and 0.537).

Recall that in the model, the GSE intervention affects employment by leading women to try to persuade their family members that they should work, while the promotion persuades family members directly. The fact that employment is no higher when both interventions are offered than if either is offered alone is consistent with messages from women and from the promotion containing the same content, perhaps because women conveyed what they had heard in the promotion when attempting to persuade their family members. The finding that offering both interventions is worse for some employment outcomes than offering the promotion alone is suggestive of direct crowd-out between the interventions, whereby the presence of one makes the other less effective. There may be family backlash to the promotion when women have been gaining GSE, or it could be that efficacious women respond negatively to their family members insisting they work.

I find little or no effects on employment in the long-run (columns (4) and (5)). There is a
positive effect of assignment to GSE treatment only on work in the rice cultivation (the effect is
8.1 percentage points, or 16.8%, with a p-value of 0.088), but no effect of assignment to promotion
only or to both treatments. I find no effect of assignment to any of the three treatment cells on
doing any work for income one year after the GSE intervention ended.

The null effects on any work at one year do not appear to be due to employment falling in the
treated groups but rather to employment rising in the control group. At the two-week and four-
month endlines, 21.8% and 23.2% of women assigned neither treatment were working, in contrast to
37.2% at the one-year endline. That is, work in this group increased by 70.6% in the year between
the two-week and one-year endlines. In each of the three treated cells, at least as many women
worked for income at one year as at two weeks or four months. Spillovers seem a likely explanation
for the control group catching up given treatment status varied within bastis. The short- and
medium-term treatment effects on employment could have shifted norms around employment in
these neighborhoods and produced the long-term rise in work amongst women assigned neither
treatment, as in the model of Fernández (2013). However, the project was not designed to test for
spillovers,\textsuperscript{18} so I cannot say definitively that spillovers explain the null effects at one year.

Next, I consider effects on work by sector. Recall that the any work for income outcomes (in
columns (1), (3), and (5)) were measured by asking women whether they had worked in any of
several common employment sectors. I group these into seven categories and consider effects on
doing work for income in each of the seven. Results at the two-week, four-month, and one-year
endlines are presented in appendix tables A.8, A.9, and A.10. The effect of the promotion alone on
any work for income at two weeks appears to be driven by work for income in farming of household
property, household microenterprises, and the other category. At the four-month endline, the effect
of the promotion alone appears driven by employment with a firm (a category that includes the
partner firm’s program), while the effects of assignment to one of the two GSE treatment cells
appear driven by work in several of the categories.

Finally, I consider robustness of effects on any employment to alternate measures. A question on
the two-week, four-month, and one-year endlines asked whether, on a normal day in the preceding
two weeks, women had mainly spent their time: (a) working or being engaged in economic activity,
(b) not working but seeking or available for work, or (c) not working and also not seeking or
available for work. The first alternate measure of employment is an indicator for selecting (a). The
four-month and one-year endlines included a time use module, which asked women how they had
spent each hour of the previous day.\textsuperscript{19} The second alternate measure of employment is an indicator
for doing any work for income or in-kind payment the previous day. Lastly, I consider effects on
an index of all measures of any employment from each endline. While I present effects on these
alternate measures, I note the measure of any employment considered thus far (having done any
work for income in the preceding two weeks) is my preferred measure of employment; the question

\textsuperscript{18}I did not collect data on social networks, and the stratification of GSE and promotion treatment assignment
means there is very little geographic variation in treatment intensity

\textsuperscript{19}As much as possible, these surveys were scheduled so that the previous day would not have been a Sunday or a
holiday.
that generates it is more concrete than the question about how women mainly spent their time, and considering work over two weeks is more powered than work over one day.

Effects on the summary indices (table A.11) are qualitatively similar to effects on any employment discussed thus far. The exception is the effect of assignment to both treatments at the four-month endline. The difference between assignment to both treatments and assignment to neither is positive but not significant (p-value = 0.411). It is less than half the size of the effect of GSE or promotion treatment alone, though in neither case can I reject that the effect of both is equal to the effect of one alone (p-values = 0.280 and 0.206).

7.2 Effects on Household Decision-Making about Women’s Employment

Recall that in the model, woman \(i\) will work if and only if

\[
\mu_i U_i + (1 - \mu_i)U_{h,i}([\theta_i + \varepsilon_i]) > 0,
\]

where \(\mu_i\) is \(i\)'s bargaining weight, \(U_i\) is her net utility from working, and \(U_{h,i}(\cdot)\) her husband’s net utility from her working. In practice, \(U_{h,i}(\cdot)\) can be seen as the aggregate of utilities of household decision-makers aside from women, including husbands and parents-in-law. The GSE intervention could affect the first argument of \(U_{h,i}\) by making women more likely to try to persuade their family members, while the promotion targets the second argument. In this section, I test for these channels and also consider whether treatments affected the two other parameters determining the household’s decision: \(\mu_i\) and \(U_i\).

I use reports of household decision-making about sign-up for the firm’s program. Note that sign-up decisions were made before there is an effect of GSE treatment on employment, which means any effects of GSE treatment on these outcomes are not effects of employment. To proxy for \(\mu_i\), I use indicators for women making the final sign-up decision, as reported by women and family members on two-week endline surveys, which were done in the week following the official sign-up day.\(^{20}\) To measure \(U_i\), I use women’s reported interest in signing up and family members’ predictions of women’s interest, and to measure \(U_{h,i}\), I use family members’ reported interest in women signing up and women’s predictions of family members’ interest. Interest and predictions of interest come from surveys done immediately after job details and any promotion were given. Finally, I consider reports from women and family members on the extent to which women and family members disagreed about whether the women should sign up. These outcomes comes from the surveys done in the week following the official sign-up day (the two-week endline surveys). These reports reflect the gap between \(U_i\) and \(U_{h,i}\).

There is no effect of assignment to GSE treatment only, promotion treatment only, or both on women’s or family members’ reports of women making sign-up decisions (columns (1) and (2) of table 5). This is suggestive evidence that neither treatment influenced \(\mu_i\).

\(^{20}\)The indicators take the value of 1 when only the women or the women and others were said to make the decisions, and the value of 0 when others and not the women were said to make the decisions.
There is some evidence that GSE treatment could have increased employment by increasing $U_i$ (columns (3) and (4)). I find a positive effect of GSE treatment only on women’s interest (p-value = 0.064), but no effect of assignment to both treatments. The effects of assignment to one of the two GSE treatment cells are not jointly significant (p-value = 0.177). This suggests GSE treatment may have increased women’s interest in working but the effect was not particularly strong. An effect on women’s interest does not contradict the claim that the GSE treatment did not explicitly promote employment; an increase in $U_i$ could come from higher assessments of one’s ability to succeed in the workplace, or from the treatment motivating women to pursue goals throughout life and women recognizing employment as a means of reaching those goals. Family members of women in both GSE treatment cells predict greater interest of women (p-values = 0.044 and 0.123); this could reflect accurate assessments of higher interest or could be evidence of women trying to convince their family members. The promotion alone has no effect on either measure of $U_i$.

Several pieces of evidence support the idea that GSE treatment increased employment by giving women the confidence to persuade their family members (columns (5)-(8)). Data suggest GSE treatment increased $U_{h,i}(.)$; family members of women assigned GSE treatment only or both treatments report greater interest in sign-up (p-values = 0.071 and 0.116). Likewise, assignment to either of the GSE treatment cells produces highly significant increases in women’s predictions of family member interest (p-values = 0.002 and 0.006), which could reflect accurate assessments of greater interest or greater confidence in ability to persuade. Effects on disagreement suggest GSE treatment reduced the gap between $U_i$ and $U_{h,i}$. Women in the GSE treatment cells report they and their family members disagreed significantly less about whether they should sign up (p-values = 0.001 and 0.099). GSE treatment alone also reduces family member assessments of disagreement (p-value = 0.076), though both treatments do not. Interestingly, while the intervention did not teach negotiation skills, there are parallels between these results and effects of interventions that do teach such skills (Ashraf et al., 2018). It may be that persuading family members is a key route through which women can reach outcomes they desire and giving them confidence leads them to pursue this route.

Finally, I investigate whether the promotion alone increased $U_{h,i}(.)$. Surprisingly, there is no effect of the promotion alone on family members’ interest; it may be that participants needed time to process the promotion before their overall interest moved. Consistent with this, women in the promotion only treatment cell report significantly higher family member interest and significantly less disagreement in their households about whether they should sign up. Receiving the promotion only also reduces family member assessments of disagreement slightly, though the effect is not significant (p-value = 0.206).

### 7.3 Differences in Job Performance

One concern about the GSE intervention is that it made women overconfident in their ability to work, leading many women to work who could not succeed in the workplace. I investigate this issue using data on performance in the partner firm’s program from program participation records. I
measure performance using attendance, achievement of knots targets, and pay (which is a function of attendance and meeting knots targets) in the first three months of training. I then compare performance across treatment groups in the subsample of women who signed up and were allowed to begin at the start of the program.

Differences across treatment groups are imprecise given the small sample size, but if anything, women in the three treated cells perform better (appendix table A.12). Though imprecise, many of these differences are quite large in magnitude. It could be that any negative selection effects of GSE treatment were overcome by treatment effects. In particular, women’s efforts to convince their family members in the sign-up decision could have produced lasting support for participation, allowing women to miss less work and perform better as a result; this is consistent with higher performance in the promotion only treatment cell. It is also possible that GSE treatment led women to try harder at work, which improved their performance. Regardless of the reason, the fact that differences are positive suggests that, at least in the partner firm’s program, GSE treatment did not lead many women to work who could not succeed.

8 Effects of the Job Offer

I use the job offer to test the period-two prediction of the model: that the result of trying to work affects GSE. All women who signed up for the job tried to work, and the offer represents the outcome of this effort. Given few women believed offers were allocated randomly (appendix table A.4), they may have seen the offer as indicative of their abilities and updated their GSE in response to it. I begin this section by presenting the effect of the job offer on GSE. In the model, it is the signal the offer contained and not the job that affects GSE, but empirically, an effect of the offer could reflect an effect of the job. In the second part of this section, I exploit the random ordering of the waitlist to test whether the job affected GSE independently of the offer.

8.1 Effects of the Job Offer on GSE

I use equation (2) to estimate effects of the job offer on GSE. I measure GSE using the GSE index and also consider effects on the alternate version of this index (both indices are detailed in section 6). I consider GSE at the four-month endline, which occurred three months after job offers were given.

The job offer significantly increases GSE (column (1) of table 6). Three months after job offers were given, women given an offer have GSE that is 0.230 standard deviations above women who signed up for the job and were waitlisted (p-value = 0.051). This is a large effect and occurs in a sample of women who had high GSE at baseline (table 2). The effect is similar in magnitude to the effects of the GSE intervention on GSE in the weeks immediately following the intervention (table 3).

There is a positive effect on the alternate GSE index though it is not statistically significant (column (1) of appendix table A.13). The effect on the alternate index is sizeable at 0.157 standard
deviations. This is comparable to the magnitudes of the effects of the GSE intervention on the alternate index in the weeks immediately following the intervention (appendix table A.6). However, the 0.157 effect is not statistically significant in the relatively small job offer experiment sample (p-value = 0.187).

In sum, the job offer raises GSE. This could be due to the positive signal the offer provided, which would be consistent with the story outlined in the model. However, it is also consistent with employment increasing GSE. Job offers do significantly increase the likelihood that women participated in the firm’s program in its first two months, or in the months before the GSE measurements affected by the job offer were taken (column (2) of table 6). It is therefore plausible that effects of the job offer on GSE reflect effects of employment rather than, or in addition to, effects of the signal. I now turn to analyses that isolate effects of employment from effects of the signal.

8.2 Is the Effect Due to the Job or to the Offer?

I exploit the random ordering of the waitlist to isolate effects of the job on GSE from effects of the signal contained in the offer itself. Women high on their villages’ waitlists were not given the positive signal but were given the job. Indeed, 30.8% of waitlisted women participated in the program during its first two months and the randomly assigned waitlist position has a highly significant effect on participation (column (3) of table 6). I therefore compare waitlisted women who were invited to participate in the first two months of the program to waitlisted women who were not, conditional on strata (village) fixed effects. I restrict to the two villages with the longest waitlists, where some waitlisted women had not yet been invited two months into the program. Because each village’s waitlist was randomly ordered, invitation from the waitlist is randomly assigned conditional on village.

I find no effect of being invited to participate on GSE (column (4)). There is also no effect on GSE measured using the alternate index (column (2) of appendix table A.13). One concern with this analysis is that effects of employment on GSE may be slow to evolve such that estimating effects of invitation in the first two months on GSE measured a few weeks later misses an underlying effect. I conduct an analogous exercise that compares waitlisted women who were invited in the first two weeks of the program to waitlisted women who were not. I followed a policy of not drawing from the waitlist for the first week of the program so the first two weeks captures the first week in which waitlisted women could have been invited. There is again no effect on the GSE index (column (5) of table 6) or on the alternate index (column (3) of table A.13).

These results suggest that it is the signal the offer provides and not the job that affects GSE. More generally, these results suggest employment itself does not affect GSE. They are consistent with the finding that the promotion does not increase GSE despite increasing employment. It could be that employment gives both positive and negative signals that cancel one another out.

Results of the job offer experiment are consistent with the mechanism outlined in the second period of the model. Women who try to work obtain a signal of their ability from the knowledge of whether they succeed or fail, and then update their GSE in response to this signal. There is
thus scope for self-reinforcing cycles. Without high GSE, women may not try, and without trying, women may not revise low GSE. Increasing GSE can lead women to try to work, and when women are able to succeed, GSE could increase further.

9 Conclusion

This paper suggests that low GSE could help explain low levels of women’s employment in India. I begin by outlining a model in which women’s low GSE can constrain their employment and be self-reinforcing. In the model, low GSE keeps women from trying to work. This can constrain the employment of women who would be able to work if they tried. Women who do not try do not learn whether they could succeed and thus cannot update their beliefs.

I test this model in a two-step experiment. I first provide randomly chosen women an intervention to increase GSE. I find this intervention motivates women to try to work, as evidenced in them trying to persuade their family members that they should work. Further, the intervention produces large increases in their employment. This suggests that absent intervention, many women could work but are constrained by low beliefs of their abilities. Results of a second-step experiment suggest not pursuing work could keep such beliefs low. I randomly assign job offers amongst a group of women in the sample who sign up for a local job. The job offer significantly raises GSE, confirming that the result of pursuing employment influences GSE. Taken together, my findings suggest that intervening to increase women’s GSE when employment opportunities are available to them can spark a virtuous cycle.

An open question in the literature is whether a psychological poverty trap exists. A contribution of this paper is to provide evidence on both directions of causality from a single setting; women’s GSE affects their work, and their work affects their GSE. This is a necessary condition for the existence of a trap but is not sufficient. Investigating whether such a cycle does indeed produce a trap is an important direction for future research.
References


Correia, Sergio. 2014. “reghdfe: Stata module to perform linear or instrumental-variable regression absorbing any number of high-dimensional fixed effects.” Statistical Software Components, Boston College Department of Economics.


Notes: This figure visualizes the experimental design and timeline of the interventions. The 1,022 women who enrolled in the study were assigned GSE treatment or control. A cross-randomization determined promotion treatment status. Women were then invited to sign up for the partner firm’s employment opportunity. There was oversubscription for the program. A randomization allocated 100 job offers amongst a group of 256 women in the sample who signed up. The GSE intervention was delivered over one month, and the promotion intervention, sign-up, and delivery of job offers occurred in the following month.
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<td>0.049</td>
<td></td>
<td>0.891</td>
<td>0.059*</td>
<td>0.076*</td>
<td>0.166</td>
<td>0.207</td>
<td>582</td>
</tr>
<tr>
<td>Appropriate for Women in HH to Work as Weaver (1-4), Family’s Report (0.051)</td>
<td>2.513</td>
<td>0.006</td>
<td>-0.071</td>
<td></td>
<td>0.846</td>
<td>0.528</td>
<td>0.666</td>
<td>0.813</td>
<td>0.890</td>
<td>594</td>
</tr>
<tr>
<td>HH Member Worked as Weaver in Last Year (=1)</td>
<td>0.351</td>
<td>0.004</td>
<td>0.004</td>
<td></td>
<td>0.924</td>
<td>0.937</td>
<td>0.993</td>
<td>0.986</td>
<td>0.997</td>
<td>649</td>
</tr>
<tr>
<td>HH Member Worked as Weaver at a Partner Firm Loom in Last Year (=1)</td>
<td>0.063</td>
<td>-0.048**</td>
<td>0.001</td>
<td></td>
<td>0.008***</td>
<td>0.610</td>
<td>0.067*</td>
<td>0.793</td>
<td>0.057*</td>
<td>649</td>
</tr>
</tbody>
</table>

**Notes:** This table presents average values of 20 baseline variables in the full sample and tests for balance across treatment groups in the main experiment. The unit of observation is a woman. Variables come from baseline surveys with women and other adults in their households. Column (1) presents means of the variables in the group assigned neither treatment. Columns (2)-(4) present coefficients and standard errors from regressions of the baseline variables on indicators for assignment to GSE treatment and promotion control, to GSE control and promotion treatment, and to GSE treatment and promotion treatment. The regressions include strata fixed effects. Standard errors are clustered at both the household and the meeting group x promotion treatment levels. Columns (5)-(7) present p-values from tests that each pair of coefficients from columns (2)-(4) are equal. Column (8) presents p-values from the tests that the coefficients in columns (2) and (4) are jointly equal to zero, and column (9) presents p-values from the tests that the coefficients in columns (3) and (4) are jointly equal to zero. The number of observations is provided in column (10) and is less than 1,022 (the number of women in the study) when the baseline variable is missing for some women. * p<0.10, ** p<0.05, *** p<0.01.
Table 2: Baseline Characteristics and Balance in Job Offer Experiment

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2) No Offer - No Offer</th>
<th>(3) N Women</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Age</strong></td>
<td>29.641</td>
<td>1.213</td>
<td>256</td>
</tr>
<tr>
<td></td>
<td>(0.854)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Married (=1)</strong></td>
<td>0.974</td>
<td>0.017</td>
<td>256</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Husband’s Age</strong></td>
<td>33.203</td>
<td>0.746</td>
<td>237</td>
</tr>
<tr>
<td></td>
<td>(0.950)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Years Since Marriage</strong></td>
<td>13.554</td>
<td>0.157</td>
<td>246</td>
</tr>
<tr>
<td></td>
<td>(0.881)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Currently Pregnant (=1)</strong></td>
<td>0.096</td>
<td>-0.045</td>
<td>256</td>
</tr>
<tr>
<td></td>
<td>(0.038)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>N Children</strong></td>
<td>2.833</td>
<td>-0.064</td>
<td>256</td>
</tr>
<tr>
<td></td>
<td>(0.207)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Lives in In-Laws’ Village (=1)</strong></td>
<td>0.994</td>
<td>-0.022</td>
<td>256</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>N Adults in HH</strong></td>
<td>3.333</td>
<td>0.451</td>
<td>256</td>
</tr>
<tr>
<td></td>
<td>(0.357)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Parent or Parent-in-Law in HH (=1)</strong></td>
<td>0.404</td>
<td>0.106</td>
<td>256</td>
</tr>
<tr>
<td></td>
<td>(0.077)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Belongs to Scheduled Caste or Tribe (=1)</strong></td>
<td>0.558</td>
<td>-0.027</td>
<td>256</td>
</tr>
<tr>
<td></td>
<td>(0.070)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>No Education (=1)</strong></td>
<td>0.603</td>
<td>0.111</td>
<td>256</td>
</tr>
<tr>
<td></td>
<td>(0.071)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Husband Has No Education (=1)</strong></td>
<td>0.237</td>
<td>-0.062</td>
<td>159</td>
</tr>
<tr>
<td></td>
<td>(0.068)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>GSE Index</strong></td>
<td>0.157</td>
<td>-0.210</td>
<td>179</td>
</tr>
<tr>
<td></td>
<td>(0.168)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Alternate GSE Index</strong></td>
<td>-0.003</td>
<td>-0.019</td>
<td>256</td>
</tr>
<tr>
<td></td>
<td>(0.148)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Happiness (1-4): Baseline</strong></td>
<td>2.980</td>
<td>-0.038</td>
<td>242</td>
</tr>
<tr>
<td></td>
<td>(0.141)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Working for Income (=1)</strong></td>
<td>0.329</td>
<td>0.043</td>
<td>234</td>
</tr>
<tr>
<td></td>
<td>(0.072)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Husband Working for Income (=1)</strong></td>
<td>0.744</td>
<td>0.008</td>
<td>147</td>
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<tr>
<td></td>
<td>(0.084)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Appropriate for Women in HH to Work as Weaver (1-4), Family’s Report</strong></td>
<td>3.033</td>
<td>0.196</td>
<td>152</td>
</tr>
<tr>
<td></td>
<td>(0.247)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>HH Member Worked as Weaver in Last Year (=1)</strong></td>
<td>0.453</td>
<td>0.033</td>
<td>164</td>
</tr>
<tr>
<td></td>
<td>(0.091)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>HH Member Worked as Weaver at a Partner Firm Loom in Last Year (=1)</strong></td>
<td>0.084</td>
<td>-0.020</td>
<td>164</td>
</tr>
<tr>
<td></td>
<td>(0.043)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: This table presents average values of 20 baseline variables in the job offer experiment sample and tests for balance by job offer treatment. The unit of observation is a woman. Variables come from baseline surveys with women and other adults in their households. Column (1) presents means of the variables in the group assigned no offer. Column (2) presents coefficients and standard errors from regressions of the baseline variables on indicators for being assigned a job offer. The regressions include fixed effects for strata used in the job offer and waitlist randomization. Standard errors are clustered at the household level. The number of observations is provided in column (3) and is less than 256 (the number of women in the job offer experiment) when the baseline variable is missing for some women. * p<0.10, ** p<0.05, *** p<0.01.
Table 3: Effects on GSE

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>GSE Index:</td>
<td>0 Week EL</td>
<td>1 Week EL</td>
<td>2 Week EL</td>
<td>4 Month EL</td>
<td>1 Year EL</td>
</tr>
<tr>
<td>GSE Treat (=1)</td>
<td>0.232***</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.082)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>GSE Treat &amp; Promo Control (=1)</td>
<td>0.194*</td>
<td>0.262***</td>
<td>0.196**</td>
<td>0.111</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.101)</td>
<td>(0.096)</td>
<td>(0.094)</td>
<td>(0.100)</td>
<td></td>
</tr>
<tr>
<td>GSE Control &amp; Promo Treat (=1)</td>
<td>-0.051</td>
<td>0.067</td>
<td>-0.019</td>
<td>-0.111</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.101)</td>
<td>(0.090)</td>
<td>(0.094)</td>
<td>(0.114)</td>
<td></td>
</tr>
<tr>
<td>GSE Treat &amp; Promo Treat (=1)</td>
<td>0.222**</td>
<td>0.194**</td>
<td>0.062</td>
<td>0.133</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.095)</td>
<td>(0.091)</td>
<td>(0.098)</td>
<td>(0.108)</td>
<td></td>
</tr>
<tr>
<td>P-Value: $\beta_1 = \beta_2$</td>
<td></td>
<td>0.015**</td>
<td>0.025**</td>
<td>0.012**</td>
<td>0.036**</td>
</tr>
<tr>
<td>P-Value: $\beta_1 = \beta_3$</td>
<td></td>
<td>0.766</td>
<td>0.425</td>
<td>0.123</td>
<td>0.826</td>
</tr>
<tr>
<td>P-Value: $\beta_2 = \beta_3$</td>
<td></td>
<td>0.003***</td>
<td>0.126</td>
<td>0.362</td>
<td>0.030**</td>
</tr>
<tr>
<td>P-Value: $\beta_1 = 0 &amp; \beta_3 = 0$</td>
<td></td>
<td>0.049**</td>
<td>0.020**</td>
<td>0.088*</td>
<td>0.409</td>
</tr>
<tr>
<td>P-Value: $\beta_2 = 0 &amp; \beta_3 = 0$</td>
<td></td>
<td>0.007***</td>
<td>0.089*</td>
<td>0.647</td>
<td>0.093*</td>
</tr>
<tr>
<td>Strata FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>PDSLASSO X</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Control Mean</td>
<td>0.075</td>
<td>0.067</td>
<td>0.097</td>
<td>0.253</td>
<td>0.114</td>
</tr>
<tr>
<td>N Women</td>
<td>592</td>
<td>782</td>
<td>798</td>
<td>750</td>
<td>635</td>
</tr>
</tbody>
</table>

Notes: This table presents effects on GSE. GSE is measured using the scale in appendix table A.1, which was adapted from the scale of Schwarzer and Jerusalem (1995). The index is computed by averaging responses to the 10 items on the scale, setting the average to missing if more than three of the responses are “don’t know,” and standardizing against the distribution of this variable in the omitted group at baseline. This handling of missing responses is suggested by the scale’s authors; appendix table A.6 presents effects on a version of the index that uses an alternate handling of missing responses. Each column presents effects at one of the five endlines. PDSLASSO X denote baseline covariates selected using the post-double-selection lasso method of Belloni, Chernozhukov and Hansen (2014). Standard errors are clustered at both the household and the meeting group levels in column (1), and at both the household and the meeting group $\times$ promotion treatment levels in columns (2)-(4). * p<0.10, ** p<0.05, *** p<0.01.
Table 4: Effects on Employment

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>GSE Treat &amp; Promo Control (=1)</td>
<td>0.030 (0.039)</td>
<td>0.058* (0.034)</td>
<td>0.087** (0.039)</td>
<td>0.081* (0.047)</td>
<td>-0.017 (0.054)</td>
</tr>
<tr>
<td>GSE Control &amp; Promo Treat (=1)</td>
<td>0.112*** (0.037)</td>
<td>0.110*** (0.037)</td>
<td>0.098** (0.041)</td>
<td>-0.007 (0.048)</td>
<td>-0.012 (0.052)</td>
</tr>
<tr>
<td>GSE Treat &amp; Promo Treat (=1)</td>
<td>0.037 (0.035)</td>
<td>0.024 (0.034)</td>
<td>0.070* (0.042)</td>
<td>0.039 (0.051)</td>
<td>-0.052 (0.052)</td>
</tr>
<tr>
<td>P-Value: ( \beta_1 = \beta_2 )</td>
<td>0.043**</td>
<td>0.185</td>
<td>0.788</td>
<td>0.057*</td>
<td>0.906</td>
</tr>
<tr>
<td>P-Value: ( \beta_1 = \beta_3 )</td>
<td>0.862</td>
<td>0.327</td>
<td>0.697</td>
<td>0.391</td>
<td>0.499</td>
</tr>
<tr>
<td>P-Value: ( \beta_2 = \beta_3 )</td>
<td>0.041**</td>
<td>0.025**</td>
<td>0.537</td>
<td>0.353</td>
<td>0.422</td>
</tr>
<tr>
<td>P-Value: ( \beta_1 = 0 ) &amp; ( \beta_3 = 0 )</td>
<td>0.553</td>
<td>0.230</td>
<td>0.065*</td>
<td>0.232</td>
<td>0.594</td>
</tr>
<tr>
<td>P-Value: ( \beta_2 = 0 ) &amp; ( \beta_3 = 0 )</td>
<td>0.009***</td>
<td>0.010**</td>
<td>0.045**</td>
<td>0.620</td>
<td>0.573</td>
</tr>
<tr>
<td>Strata FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>PDSLASSO X</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Control Mean</td>
<td>0.218</td>
<td>0.194</td>
<td>0.232</td>
<td>0.483</td>
<td>0.372</td>
</tr>
<tr>
<td>N Women</td>
<td>813</td>
<td>1022</td>
<td>793</td>
<td>674</td>
<td>657</td>
</tr>
</tbody>
</table>

Notes: This table presents effects on women’s employment. Outcomes are ordered chronologically. The outcomes in columns (1), (3), and (5) are indicators for having done any work for income in the preceding two weeks. They come from women’s two-week, four-month, and one-year endline surveys. The outcome in column (2) is an indicator for attending the official sign-up day for the firm’s program, held about two weeks after the end of the GSE intervention. It comes from official sign-up records. The outcome in column (4) is an indicator for working for income in the rice sowing season that occurred 10 months after the GSE intervention. It comes from women’s one-year endline surveys (given two months after the rice sowing). PDSLASSO X denote baseline covariates selected using the post-double-selection lasso method of Belloni, Chernozhukov and Hansen (2014). Standard errors are clustered at both the household and the meeting group \( \times \) promotion treatment levels. * \( p<0.10 \), ** \( p<0.05 \), *** \( p<0.01 \).
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>GSE Treat &amp; Promo Control (=1)</td>
<td>0.002 (-0.036)</td>
<td>0.208* (0.112)</td>
<td>0.239* (0.132)</td>
<td>-0.393***</td>
<td>-0.254***</td>
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</tr>
<tr>
<td>GSE Control &amp; Promo Treat (=1)</td>
<td>-0.010 (-0.038)</td>
<td>0.027 (0.112)</td>
<td>0.091 (0.147)</td>
<td>-0.412***</td>
<td>-0.175***</td>
<td></td>
<td></td>
</tr>
<tr>
<td>GSE Treat &amp; Promo Treat (=1)</td>
<td>-0.007 (-0.038)</td>
<td>0.089 (0.117)</td>
<td>0.198 (0.126)</td>
<td>-0.201*</td>
<td>-0.141</td>
<td></td>
<td></td>
</tr>
<tr>
<td>P-Value: $\beta_1 = \beta_2$</td>
<td>0.749 (0.037)</td>
<td>0.107 (0.117)</td>
<td>0.282 (0.126)</td>
<td>0.877 (0.122)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>P-Value: $\beta_1 = \beta_3$</td>
<td>0.808 (0.037)</td>
<td>0.302 (0.117)</td>
<td>0.771 (0.122)</td>
<td>0.151 (0.122)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>P-Value: $\beta_2 = \beta_3$</td>
<td>0.936 (0.037)</td>
<td>0.592 (0.117)</td>
<td>0.417 (0.122)</td>
<td>0.092* (0.114)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>P-Value: $\beta_1 = 0$ &amp; $\beta_3 = 0$</td>
<td>0.969 (0.037)</td>
<td>0.177 (0.117)</td>
<td>0.128 (0.122)</td>
<td>0.006*** (0.122)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>P-Value: $\beta_2 = 0$ &amp; $\beta_3 = 0$</td>
<td>0.967 (0.037)</td>
<td>0.738 (0.117)</td>
<td>0.290 (0.122)</td>
<td>0.002*** (0.122)</td>
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<tr>
<td>Strata FE</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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</tr>
<tr>
<td>PDSLASSO X</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Control Mean</td>
<td>0.834 (837)</td>
<td>2.825 (846)</td>
<td>2.580 (762)</td>
<td>2.713 (672)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N Women</td>
<td>789</td>
<td>687</td>
<td>846</td>
<td>672</td>
<td>550</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: This table investigates household decision-making channels underlying effects on employment. Outcomes in columns (1) and (2) are indicators for women making the final sign-up decision, as reported by women and family members. The indicators take the value of 1 when only the women or the women and others were said to have made the decision, and the value of 0 when others and not the women were said to have made the decision. These outcomes come from surveys done in the week after sign-up with women and, separately, with women’s family members. The outcome in column (3) is women’s interest in signing up, and the outcome in column (4) is family members’ prediction of women’s interest. The outcome in column (5) is interest of women’s family members in women signing up, and the outcome in column (6) is women’s prediction of family members’ interest. Interest was recorded on a 1-4 scale (with higher values reflecting greater interest). Reports and predictions of interest come from surveys done in the week before sign-up with women and, separately, with women’s family members. The outcomes in columns (7) and (8) are reports from women and family members on the extent to which women and family members disagreed about whether the women should sign up. Extent of disagreement is recorded on a 1-4 scale (with higher values reflecting greater disagreement). Disagreement outcomes are not observed when respondents reported there were no discussions between women and family members about the opportunity. These outcomes come from surveys done in the week after sign-up with women and, separately, with women’s family members. PDSLASSO X denote baseline covariates selected using the post-double-selection lasso method of Belloni, Chernozhukov and Hansen (2014). Standard errors are clustered at both the household and the meeting group × promotion treatment levels. * p<0.10, ** p<0.05, *** p<0.01.
Table 6: Effects of Job Offer on GSE

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>GSE Index</td>
<td>Ever Attended in First 2 Months (=1)</td>
<td>GSE Index</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Job Offer Treat (=1)</td>
<td>0.230*</td>
<td>0.131*</td>
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</tr>
<tr>
<td></td>
<td>(0.117)</td>
<td>(0.075)</td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Waitlist Position</td>
<td></td>
<td></td>
<td>-0.007***</td>
<td></td>
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<tr>
<td></td>
<td></td>
<td></td>
<td>(0.002)</td>
<td></td>
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<tr>
<td>Invited from Waitlist in First 2 Months (=1)</td>
<td></td>
<td></td>
<td>0.018</td>
<td></td>
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<tr>
<td></td>
<td></td>
<td></td>
<td>(0.172)</td>
<td></td>
<td></td>
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<tr>
<td>Invited from Waitlist in First 2 Weeks (=1)</td>
<td></td>
<td></td>
<td></td>
<td>-0.050</td>
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</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.161)</td>
<td></td>
</tr>
<tr>
<td>Strata FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Control Mean</td>
<td>0.375</td>
<td>0.308</td>
<td>0.313</td>
<td>0.303</td>
<td></td>
</tr>
<tr>
<td>Mean</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.308</td>
</tr>
<tr>
<td>N Women</td>
<td>223</td>
<td>256</td>
<td>156</td>
<td>108</td>
<td>108</td>
</tr>
</tbody>
</table>

Notes: This table investigates effects of the job offer on GSE. Column (1) presents the effect of the job offer on GSE, while columns (2)-(5) investigate whether the effect is driven by the signal conveyed in the offer or by the employment the offer enabled. The outcome in columns (1), (4), and (5) is the GSE index from the four-month endline (see notes for table 3 for details on the GSE index). The four-month endline survey occurred three months after job offers were given. The outcome in columns (2) and (3) is an indicator for ever attending the partner firm’s program during the first two months of training, which are the months of training that had elapsed before four-month endline surveys began. For the loom center that opened two weeks after the others, this is an indicator for attending the program during the first six weeks of training. The “invited from waitlist” regressors are indicators for waitlisted women being invited to participate in the program. The first is an indicator for being invited in the first two months (or first six weeks for the center that opened late). The second is an indicator for being invited in the first two weeks. I followed a policy of not inviting waitlisted women until one week into the program so this is an indicator for being invited in the first week that waitlisted women could be invited. Because villages’ waitlists were randomly ordered, these two variables are randomly assigned conditional on village (i.e. strata). The sample is limited to women in the job offer experiment in columns (1) and (2), and to waitlisted women in column (3). Columns (4) and (5) restrict to waitlisted women in the two villages with the longest waitlists, where some waitlisted women had not yet been invited two months into the program. Standard errors are clustered at the household level. * p<0.10, ** p<0.05, *** p<0.01.
Appendix Figures and Tables

Figure A.1: Distribution of Meeting Attendance by GSE Treatment Status

Notes: This figure presents the distribution of the number of meetings attended by GSE treatment status. Women could attend up to 9 group meetings during the GSE intervention period. For GSE-treated women, each meeting was part of a psychosocial intervention in GSE, and for GSE control women, each meeting involved taking a group survey on aspects of daily life. Each bar in the figure represents the number of women of a given GSE treatment status who attended a given number of meetings.
Table A.1: Adapted Schwarzer and Jerusalem (1995) Generalized Self-Efficacy Scale

<p>| | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>1.</td>
<td>Can you always manage to solve difficult problems if you try hard enough?</td>
</tr>
<tr>
<td>2.</td>
<td>If someone opposes you, can you find some way to get what you want?</td>
</tr>
<tr>
<td>3.</td>
<td>Is it easy for you to stick to and accomplish your goals?</td>
</tr>
<tr>
<td>4.</td>
<td>Are you confident that you could deal efficiently with unexpected events?</td>
</tr>
<tr>
<td>5.</td>
<td>Do you know how to handle unforeseen situations by using your resourcefulness?</td>
</tr>
<tr>
<td>6.</td>
<td>Can you solve most problems if you invest the necessary effort?</td>
</tr>
<tr>
<td>7.</td>
<td>Can you remain calm when facing difficulties by relying on your coping abilities?</td>
</tr>
<tr>
<td>8.</td>
<td>When you are confronted with a problem, can you usually find several solutions?</td>
</tr>
<tr>
<td>9.</td>
<td>If you are in trouble, can you usually think of a solution?</td>
</tr>
<tr>
<td>10.</td>
<td>Can you usually handle whatever comes your way?</td>
</tr>
</tbody>
</table>

*Notes:* This table presents the questionnaire used to measure GSE. It is the Schwarzer and Jerusalem (1995) scale with a few modifications for my context. The key modification was to rephrase items so that respondents were asked questions rather than asked to state their agreement with statements. All questions were answered on a 1-4 scale and a “don’t know” option was also included. For questions that tended to be difficult for participants to understand, I developed alternate phrasings and explanations for surveyors to use if they encountered issues with comprehension.
### Table A.2: GSE Treatment and Control Meeting Content

<table>
<thead>
<tr>
<th>Meeting #</th>
<th>GSE Treatment</th>
<th>GSE Control</th>
</tr>
</thead>
<tbody>
<tr>
<td>Meeting #1</td>
<td>Introduction</td>
<td>Group Survey Topics:</td>
</tr>
<tr>
<td></td>
<td>• Story: woman’s GSE beliefs helped in building home</td>
<td>• Entertainment</td>
</tr>
<tr>
<td></td>
<td>• Discussion: introduction to GSE</td>
<td></td>
</tr>
<tr>
<td>Meeting #2</td>
<td>Talents</td>
<td>Group Survey Topics:</td>
</tr>
<tr>
<td></td>
<td>• Discussion: talents</td>
<td>• Identification cards</td>
</tr>
<tr>
<td></td>
<td>• Activity: identify our own talents</td>
<td>• Access to</td>
</tr>
<tr>
<td></td>
<td>• Activity: we can do things we think we cannot (tablet sketchpad task)</td>
<td>• Use of</td>
</tr>
<tr>
<td>Meeting #3</td>
<td>Character Strengths</td>
<td>Group Survey Topics:</td>
</tr>
<tr>
<td></td>
<td>• Discussion: character strengths</td>
<td>• Cell phone use</td>
</tr>
<tr>
<td></td>
<td>• Activity: identify people in our lives with strengths</td>
<td>• Social interactions</td>
</tr>
<tr>
<td></td>
<td>• Activity: identify our own strengths</td>
<td></td>
</tr>
<tr>
<td>Meeting #4</td>
<td>Talents, Strengths, and Success</td>
<td>Group Survey Topics:</td>
</tr>
<tr>
<td></td>
<td>• Activity: identify times we felt proud or successful</td>
<td>• Daily schedule</td>
</tr>
<tr>
<td></td>
<td>• Activity: identify talents and strengths that led to our successes</td>
<td>• Sanitation</td>
</tr>
<tr>
<td>Meeting #5</td>
<td>Goals</td>
<td>Group Survey Topics:</td>
</tr>
<tr>
<td></td>
<td>• Activity: guided reflection</td>
<td>• Men’s employment</td>
</tr>
<tr>
<td></td>
<td>• Story: woman pursued goal of learning to sew</td>
<td>• Daily work</td>
</tr>
<tr>
<td></td>
<td>• Discussion: understanding goals</td>
<td>• Migratory work</td>
</tr>
<tr>
<td></td>
<td></td>
<td>• Government schemes</td>
</tr>
<tr>
<td></td>
<td></td>
<td>• Transportation facilities</td>
</tr>
<tr>
<td>Meeting #6</td>
<td>Goal Planning</td>
<td>Group Survey Topics:</td>
</tr>
<tr>
<td></td>
<td>• Activity: strategy for goal planning</td>
<td>• Availability of health services</td>
</tr>
<tr>
<td></td>
<td>• Overview of three-step strategy</td>
<td>• Utilization of health services</td>
</tr>
<tr>
<td></td>
<td>• Visualize three-steps in last story</td>
<td>• Childbearing and fertility</td>
</tr>
<tr>
<td></td>
<td>• Apply strategy to our goals, using worksheet</td>
<td>• Illness</td>
</tr>
<tr>
<td></td>
<td></td>
<td>• Veterinary care</td>
</tr>
<tr>
<td>Meeting #7</td>
<td>Problem Solving</td>
<td>Group Survey Topics:</td>
</tr>
<tr>
<td></td>
<td>• Story: woman overcame obstacles in agriculture</td>
<td>• Livestock</td>
</tr>
<tr>
<td></td>
<td>• Discussion: problem-solving mindset</td>
<td>• Land</td>
</tr>
<tr>
<td></td>
<td>• Activity: anticipate obstacles to our goals and brainstorm solutions</td>
<td>• Loans</td>
</tr>
<tr>
<td>Meeting #8</td>
<td>Putting It All Together</td>
<td>Group Survey Topics:</td>
</tr>
<tr>
<td></td>
<td>• Story: girl set exam goal, made plan, and identified own abilities to use</td>
<td>• Schools</td>
</tr>
<tr>
<td></td>
<td>• Discussion: importance of recognizing abilities in pursuing goals</td>
<td>• Voting</td>
</tr>
<tr>
<td></td>
<td>• Activity: identify abilities to use to reach our goals</td>
<td></td>
</tr>
<tr>
<td>Meeting #9</td>
<td>Conclusion</td>
<td>Group Survey Topics:</td>
</tr>
<tr>
<td></td>
<td>• Discussion: summarize curriculum</td>
<td>• Drawn from multiple topics above</td>
</tr>
</tbody>
</table>

**Notes:** This table outlines the contents of the group meetings held during the GSE intervention period. Meeting groups assigned to GSE treatment were given a psychosocial intervention in GSE, while those assigned control took group surveys on aspects of daily life. There were 9 meetings for both treated and control groups.
<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
<th>(9)</th>
<th>(10)</th>
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<td>(0.036)</td>
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</tr>
<tr>
<td>P-Value: ( \beta_1 = \beta_2 )</td>
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<tr>
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<td>0.680</td>
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<tr>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Control Mean</td>
<td>0.661</td>
<td>0.641</td>
<td>0.641</td>
<td>0.625</td>
<td>0.639</td>
<td>0.627</td>
<td>0.653</td>
<td>0.647</td>
<td>0.645</td>
<td>0.797</td>
</tr>
<tr>
<td>N Women</td>
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<td>1022</td>
<td>1022</td>
<td>1022</td>
<td>1022</td>
<td>1022</td>
<td>1022</td>
<td>1022</td>
<td>1022</td>
<td>1015</td>
</tr>
</tbody>
</table>

**Notes:** This table presents levels of compliance for the GSE and promotion interventions, and tests for balance in compliance across treatment groups. The outcomes in columns (1)-(9) are indicators for women attending each of the 9 group meetings during the GSE intervention period. For GSE-treated women, each meeting was part of a psychosocial intervention in GSE, and for GSE control women, each meeting involved taking a group survey on aspects of daily life. The meetings occurred before the promotion intervention so regressions in these columns do not include promotion treatment variables. The outcome in column (10) is an indicator for the women’s family attending the meeting in which job information was given. For those assigned promotion treatment, job information included job details and job promotion, and for those assigned promotion control, job information included job details only. Column (10) includes only women with family members eligible to participate in study activities for family members. Standard errors are clustered at both the household and the meeting group levels in columns (1)-(9), and at both the household and the meeting group × promotion treatment levels in column (10). * p<0.10, ** p<0.05, *** p<0.01.
Table A.4: Beliefs about How Job Offers Were Assigned

<table>
<thead>
<tr>
<th>How do you think the women who could begin the program immediately were chosen?</th>
<th>Percent</th>
</tr>
</thead>
<tbody>
<tr>
<td>By chance/lottery</td>
<td>17.2%</td>
</tr>
<tr>
<td>Research team and/or firm chose women/households they liked best</td>
<td>11.8%</td>
</tr>
<tr>
<td>Research team and/or firm chose women with the most skill or experience</td>
<td>15.7%</td>
</tr>
<tr>
<td>Other</td>
<td>20.1%</td>
</tr>
<tr>
<td>Don’t know</td>
<td>35.3%</td>
</tr>
</tbody>
</table>

N Women 204

Notes: This table presents the distribution of beliefs about how job offers were assigned. Women who received a job offer could begin the partner firm’s program immediately, while those who did not were put on a waitlist. Job offers were assigned randomly. Households were told a lottery allocated slots in the program but may not have understood what this meant. The sample is restricted to women in the job offer experiment. Beliefs were assessed on the one-year endline survey, which occurred eleven months after job offers were assigned.
### Table A.5: Endline Survey Attrition

<table>
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<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
<th>(9)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Woman Surveyed at:</strong></td>
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<td></td>
<td></td>
<td></td>
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<td>0 Week EL (=1)</td>
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<tr>
<td>1 Week EL (=1)</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>2 Week EL (=1)</td>
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</tr>
<tr>
<td>4 Month EL (=1)</td>
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<td>1 Year EL (=1)</td>
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<td>2 Week EL (=1)</td>
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</tr>
<tr>
<td>4 Month EL (=1)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td><strong>Strata FE</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td><strong>Control Mean</strong></td>
<td>0.645</td>
<td>0.845</td>
<td>0.829</td>
<td>0.767</td>
<td>0.917</td>
<td>0.674</td>
<td>0.769</td>
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<tr>
<td><strong>N Women</strong></td>
<td>1022</td>
<td>1022</td>
<td>1022</td>
<td>1022</td>
<td>256</td>
<td>1022</td>
<td>256</td>
<td>1015</td>
<td>1015</td>
</tr>
</tbody>
</table>

**Notes:** This table presents levels of endline survey attrition, and tests for balance in attrition across treatment groups. The outcomes in columns (1)-(7) are indicators for women being surveyed at one of the five endlines, which occurred 0 weeks, 1 week, 2 weeks, 4 months, and 1 year after the GSE intervention ended. The 0 week endline occurred before the promotion intervention so the regression in column (1) does not include promotion treatment variables. For the two endlines that occurred after job offers were given (the 4 month and 1 year endlines), I consider attrition both in the main experiment and in the job offer experiment. The outcomes in columns (8) and (9) are indicators for the women’s family being surveyed at the two family member endlines, which occurred 2 weeks and 4 months after the GSE intervention ended. Columns (5) and (7) include only women in the job offer experiment. Columns (8) and (9) include only women with family members eligible to participate in study activities for family members. Strata in columns (5) and (7) are the strata used in the job offer and waitlist randomization. Standard errors are clustered at both the household and the meeting group levels in column (1); at both the household and the meeting group × promotion treatment levels in columns (2)-(4), (6), and (8)-(9); and at the household level in columns (5) and (7). * p<0.10, ** p<0.05, *** p<0.01.
<table>
<thead>
<tr>
<th>Alternate GSE Index:</th>
<th>0 Week EL</th>
<th>1 Week EL</th>
<th>2 Week EL</th>
<th>4 Month EL</th>
<th>1 Year EL</th>
</tr>
</thead>
<tbody>
<tr>
<td>GSE Treat (=1)</td>
<td>0.304***</td>
<td>(0.075)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>GSE Treat &amp; Promo Control (=1)</td>
<td>0.120**</td>
<td>0.192**</td>
<td>0.141*</td>
<td>0.112</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.092)</td>
<td>(0.089)</td>
<td>(0.082)</td>
<td>(0.089)</td>
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</tr>
<tr>
<td>GSE Control &amp; Promo Treat (=1)</td>
<td>-0.056</td>
<td>0.075</td>
<td>0.019</td>
<td>-0.028</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.089)</td>
<td>(0.081)</td>
<td>(0.083)</td>
<td>(0.094)</td>
<td></td>
</tr>
<tr>
<td>GSE Treat &amp; Promo Treat (=1)</td>
<td>0.198**</td>
<td>0.122</td>
<td>0.093</td>
<td>0.102</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.088)</td>
<td>(0.081)</td>
<td>(0.086)</td>
<td>(0.091)</td>
<td></td>
</tr>
<tr>
<td>P-Value: $\beta_1 = \beta_2$</td>
<td>0.051*</td>
<td>0.156</td>
<td>0.125</td>
<td>0.146</td>
<td></td>
</tr>
<tr>
<td>P-Value: $\beta_1 = \beta_3$</td>
<td>0.373</td>
<td>0.397</td>
<td>0.557</td>
<td>0.904</td>
<td></td>
</tr>
<tr>
<td>P-Value: $\beta_2 = \beta_3$</td>
<td>0.003***</td>
<td>0.528</td>
<td>0.364</td>
<td>0.177</td>
<td></td>
</tr>
<tr>
<td>P-Value: $\beta_1 = 0 &amp; \beta_3 = 0$</td>
<td>0.079*</td>
<td>0.091*</td>
<td>0.226</td>
<td>0.382</td>
<td></td>
</tr>
<tr>
<td>P-Value: $\beta_2 = 0 &amp; \beta_3 = 0$</td>
<td>0.008***</td>
<td>0.323</td>
<td>0.510</td>
<td>0.352</td>
<td></td>
</tr>
<tr>
<td>Strata FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>PDSLASSO X</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Control Mean</td>
<td>0.253</td>
<td>0.293</td>
<td>0.372</td>
<td>0.511</td>
<td>0.407</td>
</tr>
<tr>
<td>N Women</td>
<td>648</td>
<td>868</td>
<td>855</td>
<td>795</td>
<td>674</td>
</tr>
</tbody>
</table>

* Notes: This table presents effects on alternate indices of women’s GSE. Alternate GSE indices differ from the GSE indices in table 3 in their handling of missing responses. To compute the alternate GSE indices, any “don’t know” responses are assigned the response value corresponding to the lowest level of GSE prior to averaging across the 10 items. The two indices are otherwise identical, and both were pre-specified. Each column presents effects at one of the five endlines. PDSLASSO X denote baseline covariates selected using the post-double-selection lasso method of Belloni, Chernozhukov and Hansen (2014). Standard errors are clustered at both the household and the meeting group levels in column (1), and at both the household and the meeting group × promotion treatment levels in columns (2)-(4). * p<0.10, ** p<0.05, *** p<0.01.
Table A.7: Robustness of GSE Measures to Multiple Measurement

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>GSE Index</td>
<td>Alternate GSE Index</td>
</tr>
<tr>
<td>Multiple Measurement Treat (=1)</td>
<td>-0.087 (0.174)</td>
<td>0.061 (0.160)</td>
</tr>
<tr>
<td>Strata FE</td>
<td>Yes (M)</td>
<td>Yes (M)</td>
</tr>
<tr>
<td>Control Mean</td>
<td>-0.000</td>
<td>-0.000</td>
</tr>
<tr>
<td>N Women</td>
<td>155</td>
<td>168</td>
</tr>
</tbody>
</table>

Notes: This table investigates the robustness of the GSE measures to being measured multiple times. Data come from surveys with women in the separate sample. “Treated” women in this sample were given the GSE questionnaire once a week for three weeks, while “control” women were given the questionnaire only in the third week (and asked other survey questions in the first and second weeks). This table presents effects of this multiple measurement treatment on measures of GSE in the third week. Column (1) presents effects on the GSE index, while column (2) presents effects on the alternate GSE index. See notes for Table 3 and Appendix Table A.6 for details on these indices. There was no baseline survey in this sample so the indices are standardized against the distribution of the control group in the third week. “M” strata denote strata used in the multiple measurement randomization. Standard errors are clustered by household. * p<0.10, ** p<0.05, *** p<0.01.
Table A.8: Effects on Employment by Sector, 2 Week Endline

<table>
<thead>
<tr>
<th>Working for Income:</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Farming, HH Property (=1)</td>
<td>-0.013</td>
<td>0.018</td>
<td>0.006</td>
<td>0.032**</td>
<td>-0.014</td>
<td>-0.013</td>
<td>0.018</td>
</tr>
<tr>
<td>Farming, Non-HH Property (=1)</td>
<td>(0.029)</td>
<td>(0.020)</td>
<td>(0.020)</td>
<td>(0.016)</td>
<td>(0.013)</td>
<td>(0.010)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>HH’s Micro-Enterprise (=1)</td>
<td>0.036</td>
<td>0.016</td>
<td>0.031*</td>
<td>0.006</td>
<td>-0.016</td>
<td>-0.010</td>
<td>0.028*</td>
</tr>
<tr>
<td>Casual Labor (=1)</td>
<td>(0.035)</td>
<td>(0.019)</td>
<td>(0.018)</td>
<td>(0.014)</td>
<td>(0.012)</td>
<td>(0.010)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>Firm Employment (=1)</td>
<td>0.044</td>
<td>-0.000</td>
<td>0.027</td>
<td>0.005</td>
<td>-0.010</td>
<td>-0.018*</td>
<td>0.010</td>
</tr>
<tr>
<td>Anganwadi Work or Teaching (=1)</td>
<td>(0.032)</td>
<td>(0.020)</td>
<td>(0.018)</td>
<td>(0.013)</td>
<td>(0.014)</td>
<td>(0.009)</td>
<td>(0.013)</td>
</tr>
</tbody>
</table>

P-Value: $\beta_1 = \beta_2$

P-Value: $\beta_1 = \beta_3$

P-Value: $\beta_2 = \beta_3$

P-Value: $\beta_1 = 0$ & $\beta_3 = 0$

P-Value: $\beta_2 = 0$ & $\beta_3 = 0$

Strata FE Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes

PDSLASSO X Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes

Control Mean 0.126 | 0.044 | 0.024 | 0.010 | 0.019 | 0.019 | 0.024

N Women 813 | 813 | 813 | 813 | 813 | 813 | 813

Notes: This table presents effects on women’s employment by sector. The outcomes are indicators for having done work for income in a particular sector in the preceding two weeks. The outcomes comes from endline surveys with women done two weeks after the GSE intervention ended. The seven sectors considered in the seven columns are: farming of household property, farming of non-household property, work in household’s micro-enterprise, casual non-farm labor, firm employment, anganwadi work or teaching, and other. Farming of household property includes both agriculture work on own household’s land and husbandry of animals owned by own household. Farming of non-household property includes both agriculture work off own household’s land and husbandry of animals not owned by own household. Firm employment does not include work in the partner firm’s program as the program had not begun at the two-week endline. The outcome in column (1) of table 77 equals 1 if any of the outcomes in this table equal 1. PDSLASSO X denote baseline covariates selected using the post-double-selection lasso method of Belloni, Chernozhukov and Hansen (2014). Standard errors are clustered at both the household and the meeting group $\times$ promotion treatment levels. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. 
<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Working for Income:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Farming, HH Property (=1)</td>
<td>-0.003</td>
<td>0.027**</td>
<td>0.004</td>
<td>0.012</td>
<td>0.033</td>
<td>0.006</td>
<td>0.025**</td>
</tr>
<tr>
<td>Farming, Non-HH Property</td>
<td>(0.024)</td>
<td>(0.013)</td>
<td>(0.019)</td>
<td>(0.010)</td>
<td>(0.030)</td>
<td>(0.007)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>Farming, HH’s Micro-</td>
<td>-0.006</td>
<td>-0.005</td>
<td>-0.004</td>
<td>0.059*</td>
<td>0.000</td>
<td>0.008</td>
<td></td>
</tr>
<tr>
<td>Enterprise (=1)</td>
<td>(0.027)</td>
<td>(0.013)</td>
<td>(0.017)</td>
<td>(0.006)</td>
<td>(0.031)</td>
<td>(0.007)</td>
<td>(0.013)</td>
</tr>
<tr>
<td>Casual Non-Farm Labor (=1)</td>
<td>0.031</td>
<td>0.023</td>
<td>0.022</td>
<td>0.007</td>
<td>0.006</td>
<td>0.001</td>
<td>-0.002</td>
</tr>
<tr>
<td>Employed by a Firm (=1)</td>
<td>(0.028)</td>
<td>(0.017)</td>
<td>(0.019)</td>
<td>(0.008)</td>
<td>(0.032)</td>
<td>(0.004)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>Anganwadi Work or Teaching</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(=1)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Other (=1)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>P-Value: $\beta_1 = \beta_2$</td>
<td>0.809</td>
<td>0.251</td>
<td>0.591</td>
<td>0.008*</td>
<td>0.419</td>
<td>0.515</td>
<td>0.298</td>
</tr>
<tr>
<td>P-Value: $\beta_1 = \beta_3$</td>
<td>0.218</td>
<td>0.356</td>
<td>0.344</td>
<td>0.558</td>
<td>0.420</td>
<td>0.447</td>
<td>0.053*</td>
</tr>
<tr>
<td>P-Value: $\beta_2 = \beta_3$</td>
<td>0.365</td>
<td>0.416</td>
<td>0.118</td>
<td>0.174</td>
<td>0.117</td>
<td>0.882</td>
<td>0.464</td>
</tr>
<tr>
<td>P-Value: $\beta_1 = 0$ &amp; $\beta_3 = 0$</td>
<td>0.434</td>
<td>0.073*</td>
<td>0.479</td>
<td>0.447</td>
<td>0.529</td>
<td>0.673</td>
<td>0.105</td>
</tr>
<tr>
<td>P-Value: $\beta_2 = 0$ &amp; $\beta_3 = 0$</td>
<td>0.525</td>
<td>0.352</td>
<td>0.281</td>
<td>0.374</td>
<td>0.126</td>
<td>0.948</td>
<td>0.752</td>
</tr>
</tbody>
</table>

| Strata FE                  | Yes                     | Yes                      | Yes                      | Yes                      | Yes                      | Yes                      | Yes                      |
| PDSLASSO X                 | Yes                     | Yes                      | Yes                      | Yes                      | Yes                      | Yes                      | Yes                      |
| Control Mean               | 0.076                   | 0.020                    | 0.025                    | 0.005                    | 0.106                    | 0.015                    | 0.015                    |
| N Women                    | 793                     | 793                      | 793                      | 793                      | 793                      | 793                      | 793                      |

Notes: This table presents effects on women’s employment by sector. The outcomes are indicators for having done work for income in a particular sector in the preceding two weeks. The outcomes come from endline surveys with women done four months after the GSE intervention ended. The seven sectors considered in the seven columns are: farming of household property, farming of non-household property, work in household’s micro-enterprise, casual non-farm labor, firm employment, anganwadi work or teaching, and other. Farming of household property includes both agriculture work on own household’s land and husbandry of animals owned by own household. Farming of non-household property includes both agriculture work off own household’s land and husbandry of animals not owned by own household. Firm employment includes work in the partner firm’s program. The outcome in column (3) of table 77 equals 1 if any of the outcomes in this table equal 1. PDSLASSO X denote baseline covariates selected using the post-double-selection lasso method of Belloni, Chernozhukov and Hansen (2014). Standard errors are clustered at both the household and the meeting group $\times$ promotion treatment levels. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. 
Table A.10: Effects on Employment by Sector, 1 Year Endline

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Working for Income:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Farming, HH Property (=1)</td>
<td>Farming, Non-HH Property (=1)</td>
<td>HH's Micro-Enterprise (=1)</td>
<td>Casual Non-Farm Labor (=1)</td>
<td>Employed by a Firm (=1)</td>
<td>Anganwadi Work or Teaching (=1)</td>
<td>Other (=1)</td>
</tr>
<tr>
<td>GSE Treat &amp; Promo Control (=1)</td>
<td>-0.020</td>
<td>0.016</td>
<td>-0.038**</td>
<td>0.003</td>
<td>-0.000</td>
<td>0.004</td>
<td>0.007</td>
</tr>
<tr>
<td></td>
<td>(0.038)</td>
<td>(0.025)</td>
<td>(0.019)</td>
<td>(0.008)</td>
<td>(0.021)</td>
<td>(0.013)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>GSE Control &amp; Promo Treat (=1)</td>
<td>-0.037</td>
<td>-0.021</td>
<td>-0.021</td>
<td>0.022</td>
<td>0.021</td>
<td>-0.003</td>
<td>-0.009</td>
</tr>
<tr>
<td></td>
<td>(0.038)</td>
<td>(0.024)</td>
<td>(0.021)</td>
<td>(0.013)</td>
<td>(0.022)</td>
<td>(0.013)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>GSE Treat &amp; Promo Treat (=1)</td>
<td>-0.053</td>
<td>-0.014</td>
<td>0.004</td>
<td>-0.006</td>
<td>0.018</td>
<td>-0.016</td>
<td>0.007</td>
</tr>
<tr>
<td></td>
<td>(0.037)</td>
<td>(0.022)</td>
<td>(0.024)</td>
<td>(0.006)</td>
<td>(0.021)</td>
<td>(0.012)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>P-Value: $\beta_1 = \beta_2$</td>
<td>0.648</td>
<td>0.136</td>
<td>0.365</td>
<td>0.176</td>
<td>0.387</td>
<td>0.509</td>
<td>0.192</td>
</tr>
<tr>
<td>P-Value: $\beta_1 = \beta_3$</td>
<td>0.374</td>
<td>0.165</td>
<td>0.050**</td>
<td>0.231</td>
<td>0.427</td>
<td>0.020**</td>
<td>0.987</td>
</tr>
<tr>
<td>P-Value: $\beta_2 = \beta_3$</td>
<td>0.676</td>
<td>0.762</td>
<td>0.294</td>
<td>0.035**</td>
<td>0.916</td>
<td>0.128</td>
<td>0.206</td>
</tr>
<tr>
<td>P-Value: $\beta_1 = 0 &amp; \beta_3 = 0$</td>
<td>0.347</td>
<td>0.378</td>
<td>0.052*</td>
<td>0.381</td>
<td>0.646</td>
<td>0.041**</td>
<td>0.897</td>
</tr>
<tr>
<td>P-Value: $\beta_2 = 0 &amp; \beta_3 = 0$</td>
<td>0.342</td>
<td>0.672</td>
<td>0.482</td>
<td>0.092*</td>
<td>0.540</td>
<td>0.165</td>
<td>0.374</td>
</tr>
<tr>
<td>Strata FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>PDSLASSO X</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Control Mean</td>
<td>0.198</td>
<td>0.052</td>
<td>0.058</td>
<td>0.006</td>
<td>0.035</td>
<td>0.023</td>
<td>0.023</td>
</tr>
<tr>
<td>N Women</td>
<td>657</td>
<td>657</td>
<td>657</td>
<td>657</td>
<td>657</td>
<td>657</td>
<td>657</td>
</tr>
</tbody>
</table>

Notes: This table presents effects on women’s employment by sector. The outcomes are indicators for having done work for income in a particular sector in the preceding two weeks. The outcomes comes from endline surveys with women done one year after the GSE intervention ended. The seven sectors considered in the seven columns are: farming of household property, farming of non-household property, work in household’s micro-enterprise, casual non-farm labor, firm employment, anganwadi work or teaching, and other. Farming of household property includes both agriculture work on own household’s land and husbandry of animals owned by own household. Farming of non-household property includes both agriculture work off own household’s land and husbandry of animals not owned by own household. Firm employment includes work in the partner firm’s program. The outcome in column (3) of table ?? equals 1 if any of the outcomes in this table equal 1. PDSLASSO X denote baseline covariates selected using the post-double-selection lasso method of Belloni, Chernozhukov and Hansen (2014). Standard errors are clustered at both the household and the meeting group × promotion treatment levels. * p<0.10, ** p<0.05, *** p<0.01.
Table A.11: Effects on Additional Measures of Any Employment

<table>
<thead>
<tr>
<th></th>
<th>2 Week EL</th>
<th></th>
<th>4 Month EL</th>
<th></th>
<th>1 Year EL</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Work/Econ</td>
<td>Main Use of Time</td>
<td>Did Paid Work</td>
<td>Employment Index</td>
<td>Work/Econ</td>
<td>Did Paid Work</td>
</tr>
<tr>
<td>GSE Treat &amp; Promo Control (=1)</td>
<td>0.084</td>
<td>0.139</td>
<td>0.051</td>
<td>0.040</td>
<td>0.174*</td>
<td>-0.093*</td>
</tr>
<tr>
<td></td>
<td>(0.053)</td>
<td>(0.103)</td>
<td>(0.045)</td>
<td>(0.034)</td>
<td>(0.090)</td>
<td>(0.053)</td>
</tr>
<tr>
<td>GSE Control &amp; Promo Treat (=1)</td>
<td>0.033</td>
<td>0.169*</td>
<td>0.016</td>
<td>0.075**</td>
<td>0.197**</td>
<td>-0.049</td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td>(0.086)</td>
<td>(0.044)</td>
<td>(0.035)</td>
<td>(0.092)</td>
<td>(0.053)</td>
</tr>
<tr>
<td>GSE Treat &amp; Promo Treat (=1)</td>
<td>0.052</td>
<td>0.106</td>
<td>-0.005</td>
<td>0.009</td>
<td>0.071</td>
<td>-0.044</td>
</tr>
<tr>
<td></td>
<td>(0.049)</td>
<td>(0.095)</td>
<td>(0.044)</td>
<td>(0.034)</td>
<td>(0.086)</td>
<td>(0.051)</td>
</tr>
</tbody>
</table>

P-Value: $\beta_1 = \beta_2$

| 0.319 | 0.769 | 0.459 | 0.326 | 0.819 | 0.392 | 0.885 | 0.650 |

P-Value: $\beta_1 = \beta_3$

| 0.553 | 0.752 | 0.230 | 0.369 | 0.280 | 0.326 | 0.795 | 0.994 |

P-Value: $\beta_2 = \beta_3$

| 0.694 | 0.497 | 0.644 | 0.069* | 0.206 | 0.927 | 0.893 | 0.638 |

P-Value: $\beta_1 = 0 \& \beta_3 = 0$

| 0.256 | 0.333 | 0.412 | 0.469 | 0.158 | 0.236 | 0.543 | 0.905 |

P-Value: $\beta_2 = 0 \& \beta_3 = 0$

| 0.552 | 0.146 | 0.887 | 0.071* | 0.102 | 0.603 | 0.547 | 0.875 |

| Strata FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| PDSLASSO X | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Control Mean | 0.430 | 0.000 | 0.590 | 0.168 | 0.000 | 0.651 | 0.109 | 0.000 |
| N Women | 825 | 849 | 788 | 793 | 794 | 653 | 674 | 674 |

Notes: The preferred measure of any employment is the working for income variable (effects on this variable are presented in columns (1), (3), and (5) of table 4); this table presents effects on additional measures of any employment. One additional measure is based on women’s reports of their main use of time. Women were asked whether on a usual day in the preceding two weeks, they mainly spent their time: (a) working or being engaged in economic activity, (b) not working but seeking or available for work, or (c) not working and also not seeking or available for work. The outcomes in columns (1), (3), and (6) are indicators for selecting option (a) on the two-week, four-month, and one-year endlines. The next measure comes from a time use module that asked women what they did each hour of the previous day. The outcomes in columns (4) and (7) are indicators for reporting any work for pay (including both income and in kind payment) the previous day at the four-month and one-year endlines. This outcome is not observed at two weeks because the two-week endline survey did not include the time use module. Finally, the outcomes in columns (2), (5), and (8) are indices of all measures of any employment at the two-week, four-month, and one-year endlines. The two-week endline index includes the outcome in column (1) of table 4 and the outcome in column (1) of this table. The four-month endline index includes the outcome in column (3) of table 4 as well as the outcomes in columns (3) and (4) of this table. The one-year endline index includes the outcome in column (5) of table 4 as well as the outcomes in columns (6) and (7) of this table. PDSLASSO X denote baseline covariates selected using the post-double-selection lasso method of Belloni, Chernozhukov and Hansen (2014). Standard errors are clustered at both the household and the meeting group × promotion treatment levels. * p<0.10, ** p<0.05, *** p<0.01.
### Table A.12: Performance in Weaving Training Program by Treatment Group

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>N Days Attended:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Month 1</td>
<td>3.316</td>
<td>1.777</td>
<td>0.167</td>
<td>0.077</td>
<td>0.075</td>
<td>0.309</td>
<td>0.173</td>
<td>0.043</td>
</tr>
<tr>
<td>Month 2</td>
<td>(3.172)</td>
<td>(3.455)</td>
<td>(3.795)</td>
<td>(0.078)</td>
<td>(0.089)</td>
<td>(0.296)</td>
<td>(0.289)</td>
<td>(0.326)</td>
</tr>
<tr>
<td>Month 3</td>
<td>3.122</td>
<td>1.379</td>
<td>2.314</td>
<td>0.102</td>
<td>0.137</td>
<td>0.291</td>
<td>0.147</td>
<td>0.237</td>
</tr>
<tr>
<td>Month 2</td>
<td>(3.033)</td>
<td>(3.312)</td>
<td>(3.524)</td>
<td>(0.070)</td>
<td>(0.085)</td>
<td>(0.283)</td>
<td>(0.274)</td>
<td>(0.301)</td>
</tr>
<tr>
<td>Month 3</td>
<td>3.613</td>
<td>3.090</td>
<td>-2.067</td>
<td>0.053</td>
<td>0.032</td>
<td>0.337</td>
<td>0.266</td>
<td>-0.159</td>
</tr>
<tr>
<td>Month 3</td>
<td>(3.243)</td>
<td>(3.866)</td>
<td>(3.703)</td>
<td>(0.072)</td>
<td>(0.089)</td>
<td>(0.302)</td>
<td>(0.323)</td>
<td>(0.323)</td>
</tr>
<tr>
<td><strong>P-Value:</strong> $\beta_1 = \beta_2$</td>
<td>0.951</td>
<td>0.895</td>
<td>0.503</td>
<td>0.732</td>
<td>0.452</td>
<td>0.951</td>
<td>0.918</td>
<td>0.475</td>
</tr>
<tr>
<td><strong>P-Value:</strong> $\beta_1 = \beta_3$</td>
<td>0.929</td>
<td>0.717</td>
<td>0.512</td>
<td>0.734</td>
<td>0.622</td>
<td>0.929</td>
<td>0.759</td>
<td>0.494</td>
</tr>
<tr>
<td><strong>P-Value:</strong> $\beta_2 = \beta_3$</td>
<td>0.880</td>
<td>0.623</td>
<td>0.159</td>
<td>0.458</td>
<td>0.213</td>
<td>0.880</td>
<td>0.680</td>
<td>0.143</td>
</tr>
<tr>
<td><strong>P-Value:</strong> $\beta_1 = 0$ &amp; $\beta_3 = 0$</td>
<td>0.452</td>
<td>0.722</td>
<td>0.769</td>
<td>0.598</td>
<td>0.700</td>
<td>0.452</td>
<td>0.699</td>
<td>0.774</td>
</tr>
<tr>
<td><strong>P-Value:</strong> $\beta_2 = 0$ &amp; $\beta_3 = 0$</td>
<td>0.459</td>
<td>0.727</td>
<td>0.365</td>
<td>0.347</td>
<td>0.233</td>
<td>0.459</td>
<td>0.709</td>
<td>0.327</td>
</tr>
<tr>
<td><strong>Village FE</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Control Mean</strong></td>
<td>7.783</td>
<td>9.652</td>
<td>9.400</td>
<td>0.043</td>
<td>0.050</td>
<td>-0.000</td>
<td>0.000</td>
<td>-0.000</td>
</tr>
<tr>
<td><strong>N Women</strong></td>
<td>113</td>
<td>113</td>
<td>93</td>
<td>113</td>
<td>93</td>
<td>113</td>
<td>113</td>
<td>93</td>
</tr>
</tbody>
</table>

**Notes:** This table compares performance in the first three months of the partner firm’s weaving training program across treatment groups. Outcomes in columns (1), (2), and (3) are the number of days women attended the program in months 1, 2, and 3. In months 2 and 3 of the program, women’s daily pay is reduced if they cannot weave a targeted number of knots per day by the end of the month; outcomes in columns (4) and (5) are indicators for meeting the targets for months 2 and 3. Outcomes in columns (6), (7), and (8) are women’s pay in months 1, 2, and 3. Women are paid a fixed amount for each day they attend in a month. The daily amount is reduced in months 2 and 3 if women do not meet knots targets for those months. Pay is reported in units of standard deviations from the control group mean. The sample is limited to women who signed up for the program and were allowed to begin on the first day (in the five catchment areas with oversubscription, these are women in the job offer experiment who were assigned job offers, and in the catchment area without oversubscription, these are all women who signed up). The program began two weeks later in one catchment area than in the others so month 3 outcomes are not observed for that catchment area. Standard errors are clustered at both the household and the meeting group × promotion treatment levels. * p<0.10, ** p<0.05, *** p<0.01.
Table A.13: Job Offer Effects using Alternate GSE Index

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Alternate GSE Index</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Job Offer Treat (=1)</td>
<td>0.157</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Invited Off Waitlist in First 2 Months (=1)</td>
<td>-0.003</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Invited Off Waitlist in First 2 Weeks (=1)</td>
<td>-0.089</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Strata FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Control Mean</td>
<td>0.616</td>
<td>0.562</td>
<td>0.553</td>
</tr>
<tr>
<td>N Women</td>
<td>237</td>
<td>114</td>
<td>114</td>
</tr>
</tbody>
</table>

Notes: This table reproduces all job offer analyses of GSE using the alternate GSE index (see notes for table A.6 for details on the alternate GSE index). Column (1) reproduces the analysis in column (1) of table 6, and columns (2) and (3) reproduce the analyses in columns (4) and (5) of table 6. The outcome in all three columns is the alternate GSE index from the four-month endline, which occurred three months after job offers were given. The “invited from waitlist” regressors are indicators for waitlisted women being invited to participate in the program. The first is an indicator for being invited in the first two months, which are the months of training that had elapsed before four-month endline surveys began. For the loom center that opened two weeks after the others, this is an indicator for being invited in the first six weeks. The second is an indicator for being invited in the first two weeks. I followed a policy of not inviting waitlisted women until one week into the program so this is an indicator for being invited in the first week that waitlisted women could be invited. Because villages’ waitlists were randomly ordered, these two variables are randomly assigned conditional on village (i.e. strata). The sample is limited to women in the job offer experiment in column (1). Columns (2) and (3) restrict to waitlisted women in the two villages with the longest waitlists, where some waitlisted women had not yet been invited two months into the program. Standard errors are clustered at the household level. * p<0.10, ** p<0.05, *** p<0.01.