Vicious and Virtuous Cycles: Self-Efficacy and Employment of Women in India *

Madeline McKelway †

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Abstract

Women's labor force participation is far below men's worldwide. I use a multi-stage field experiment in India to investigate whether low generalized self-efficacy (GSE) could constrain women's employment and be self-reinforcing. GSE is a key concept in psychology that refers to beliefs in own ability to attain desired outcomes. I outline a model in which low GSE can create a vicious cycle: it keeps women from trying to work and thus from learning whether they could. My experiment provides women a psychosocial intervention to raise GSE. I cross-randomize an intervention to reduce family members' opposition to women's employment, a key external constraint to women's work. I subsequently 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 increasing women's GSE when women have the ability to work can spark a virtuous cycle.

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†MIT Department of Economics, Email: mckelway@mit.edu

1 Introduction

Across the world, women's labor force participation (LFP) is far below men's. In 2019, the worldwide female LFP rate was 48%, much lower than the rate for men of 75% (World Bank, 2019*a*,*b*). Literature discusses a number of constraints to women's employment, such as low education of women, a lack of jobs suitable for women, discrimination, and responsibilities within the home (see, for example, the discussion and reviews in Altonji and Blank (1999); Goldin (1995, 2006); Heath and Jayachandran (2018)). Notably, the literature focuses on constraints that are external to individual women; there may also be internal, psychological constraints that keep women from working even when external constraints do not bind.

I investigate whether womens employment is constrained by low generalized self-efficacy. Self-efficacy refers to one's beliefs in one's own ability to succeed in a particular domain (Bandura, 1977), and generalized self-efficacy (GSE) describes one's sense of self-efficacy across domains in life (Eden, 1988; Scholz et al., 2002; Sherer et al., 1982). These concepts are central in the psychology literature but receive little attention in economics. GSE is seen as particularly important in forming beliefs about capabilities in new domains. It could be important in the context of female LFP as low LFP could imply many women lack experience in employment domains. However, women's GSE is lower than men's in many countries (Scholz et al., 2002). This parallels findings from economics, discussed in Bertrand (2011) and Niederle (2015), that women tend to be less confident than men. These beliefs may result in women having low assessments of their ability in employment-related domains, which could keep women from trying to overcome external barriers to their employment. Further, if women do not try, they cannot learn if they could succeed, which could contribute to the persistence of low GSE. This paper uses a multi-stage RCT in India to investigate (i) whether low GSE constrains women's employment, (ii) how GSE interacts with external constraints to employment, and (iii) whether not pursuing employment could contribute to low GSE.

I begin the paper with a two-period model that serves to formalize the relationships between GSE, external constraints, and employment, and to provide a framework for interpreting my experimental findings. In period one, women choose whether or not to try to overcome an external barrier to their employment. Whether women succeed in becoming employment should they try depends on their ability. Women do not know their ability, and their perception of it is determined by their GSE. In the second period, women who try either succeed in becoming employed or fail, then update their GSE accordingly. Low GSE can create a vicious cycle: it keeps women from trying to work, which means they do not learn whether they could, and their GSE remains low. A policy that increases women's GSE could lead women to try and spark a virtuous cycle for those who succeed. Whether policies that relax the external constraint directly spark a virtuous cycle depends on whether employment alone is sufficient to spark a cycle.

My experiment was conducted in a sample of 1,022 women in India. Women's employment in India is especially low. In 2019, India's female LFP rate was 23%, substantially lower than the rate across low- and middle-income countries of 47% and a stark contrast to a rate of 79% for men in India (World Bank, 2019*a*,*b*). I work in a rural area of the state of Uttar Pradesh, where 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 to increase GSE. 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 asked women to recognize how their abilities could enable them to reach goals in their lives. The curriculum sought to raise self-efficacy in specific domains and considered a large number of domains. Importantly, very few of the domains involved employment. This was done to minimize demand effects on the study's key outcome. 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. Following the GSE intervention, all women, regardless of treatment status, were informed about the job with the partner firm and shown a short video promoting it.

I cross-randomized whether women's family members were also given the video promotion. Men in India report less support for women working than their wives do (Bernhardt et al., 2018; Field et al., 2019; Lowe and McKelway, 2019), suggesting opposition from family members is an important external constraint to employment that women face. Increased GSE may give women the confidence to try to persuade their families that they should work, perhaps by conveying the arguments they heard in the promotion or private information on their ability to work. If women can succeed in doing so, the promotion intervention may be redundant to the GSE intervention as a means of raising employment; if not, there may be employment gains from combining the two.

The GSE treatment increased GSE. I measure GSE using a standard scale from psychology (Schwarzer and Jerusalem, 1995). GSE treatment women have significantly higher GSE than GSE controls in the weeks immediately following the intervention. Remarkably, an effect of similar magnitude is present one year later. Further, evidence from an incentivized task suggests the treatment also made women more likely to try to reach new attainments.

The GSE intervention 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 on its own significantly increased sign-up. The effect is a large 29.9% increase (5.8 percentage points off a base of 19.4). GSE treatment alone had little effect on working for income in the weeks immediately following the intervention, but produced a significant and large increase of 37.5% (8.7 percentage points off a base of 23.2) in working for income four months after the intervention. This suggests that absent intervention, there are women who could work but are constrained by low assessments of their own abilities. The promotion on its own produced enormous increases of 56.7%, 51.4%, 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.

Survey evidence suggests the GSE treatment increased employment by giving women the confi-

dence 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 in preferences for women's work within the household. 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.

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; norms around women's work may have been relaxed as households saw their female neighbors working. The project, however, was not designed to test for spillovers.

Results from a final experimental step suggest pursuing work provides information that influences GSE. 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 abilities from the job offer outcome.

Three months after offers were allocated, the GSE of women who were given an offer is significantly higher than that of women who signed up and were not given an offer. The effect is about equal to the effect of GSE treatment on GSE. This result suggests that women update GSE based on information they acquire in the pursuit of work.

Additional analyses suggest the job offer effect is driven by the positive signal the offer provided rather than the employment it allowed. Women who were not given an offer were placed on a randomly ordered waitlist and invited when those ahead of them dropped out. Those who were invited from the waitlist did not receive a positive signal from the offer but were given the opportunity to work. Comparing waitlisted women who were invited to those who were not thus isolates the effect of the job from the effect of the offer. I find no effect of invitation from the waitlist on GSE.

In sum, I find that an intervention in GSE leads women to try to work and that pursuing work provides information that informs GSE. Raising GSE could spark a virtuous cycle if women led to try have the ability to succeed. The persistence of the effect of the GSE intervention on GSE suggests a virtuous cycle may have been sparked for many women. In contrast, the promotion did not affect GSE. This, together with the waitlist result and the fact that there is no catch-up in control group GSE despite there being catch-up in employment, suggests employment is not

sufficient to spark a virtuous cycle. It could be that high GSE is required to exert the effort in the workplace that would provide positive signals, or that high GSE allows women to acquire positive signals in domains of life aside from employment. This implies policies that alleviate external constraints to employment may not break the vicious cycle in women's psychology. Taken together, my findings suggest psychosocial interventions should not be overlooked as a means of improving both the psychological and economic trajectories of women.

My paper contributes to three bodies of literature. First is literature on the causal determinants of women's employment in poor 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; Lowe and McKelway, 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 in Pakistan.

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; Haushofer, 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. I also contribute to this literature evidence on how psychology interacts with external constraints and on how psychological constraints might perpetuate themselves.

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 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. The theoretical framework is presented in section 3. I detail the experimental design in section 4, and the data and empirical strategy in section 5. Effects of the GSE intervention on GSE and effort exertion are presented in section 6. Section 7 explores effects on employment, while section 8 explores effects of the job offer. Sections 9 and 10 discuss and conclude.

2 Background

2.1 Setting

I study women's employment in India. Traditional Indian culture features strong gender norms. Patrilocality, concerns for women's "purity," and son preference are common across India, and Indian women report having little freedom of choice (Jayachandran, 2015). As discussed in section 1, levels of female LFP in India are particularly low; at just 23%, India's female LFP rate is much lower than the rate across low- and middle-income countries of 47% and the LFP rate for men in India of 79% (World Bank, 2019*a*,*b*).

My experiment was conducted in rural Uttar Pradesh. Uttar Pradesh is a state in the north of India. It is one of India's poorest states, and it adheres strongly to norms of traditional Indian culture. Levels of women's employment in this 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%. The baseline survey was conducted towards the end of a rice sowing season that employs female labor, meaning this level of women's employment is relatively high for the setting. On an endline survey conducted during an agricultural lean season, 23.2% of women and 77.8% of husbands were working for income.

Interestingly, women themselves report interest in employment. 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, just 23.2% of the control group was 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).

Women in this setting face a number of external constraints to their employment. 41.9% of women in the control group have no education,[1] suggesting a lack of skills could constrain women's work. The average woman in the control group has 2.7 children. Given the average woman is just 29.5 years old, these children are young, implying childcare demands may be another constraint.

An external constraint I investigate extensively is opposition to women's work from women's family members. Women in the setting rarely make decisions about their own employment. On an endline survey, just 30% of women in the control group said they make decisions, either alone or together with others, about whether they should work outside of the home. Further, 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) and Field et al. (2019) 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) conducted in southern India.

[1] This is a baseline variable that is imbalanced; the fraction of uneducated women is higher in the treated groups.

2.2 Generalized Self-Efficacy (GSE)

I investigate whether women in this setting face a particular internal constraint to their employment: low generalized self-efficacy (GSE). 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 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). In other words, self-efficacy determines an individual's beliefs about her ability to succeed in a particular domain. Generalized self-efficacy describes one's sense of self-efficacy across domains in life (Eden, 1988; Scholz et al., 2002; Sherer et al., 1982). I intervene in GSE rather than self-efficacy in the employment domain to avoid demand effects; intervening in GSE allowed me to enhance ability beliefs with very little mention of employment, which minimizes the concern that any effects on employment are driven by experimenter demand.

Psychologists have developed scales to measure GSE (Schwarzer and Jerusalem, 1995; Sherer et al., 1982). I use the Schwarzer and Jerusalem (1995) scale with minor modifications to measure GSE in my context. The adapted version of the scale is provided in appendix table A.1.

Further understanding of GSE can be gained by comparing it to related but distinct concepts. Self-esteem is similar to GSE in that it describes one's thinking about oneself, but self-esteem describes one's overall evaluation of one's own worth whereas GSE describes beliefs about one's abilities. 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. Whereas locus of control describes the extent to which an individual believes that outcomes in her life respond to her abilities, GSE describes an individual's belief that she possesses abilities. Self-confidence is a term typically used outside of psychology and is often used synonymously with GSE.

While GSE is distinct from other psychological constructs, it is closely related to them. Indeed, GSE scales are validated in part through their correlations with self-esteem, locus of control, and other constructs (Schwarzer and Jerusalem, 1995; Sherer et al., 1982). It would be difficult to design a field intervention that affected GSE but not the others, and I will not claim that my intervention affected GSE in isolation of related constructs.

A vast literature in psychology suggests self-efficacy in a particular domain is central in motivating behavior in that domain (see Bandura (1982, 1997) for summaries). Much of the evidence comes from experiments 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 experimentally manipulated the assessments via anchoring. Those randomly induced to have higher perceptions of their own efficacy persist longer in solving hard problems.

Own experience is the key source of self-efficacy; individuals develop beliefs about their abilities in particular domains through histories of success and failure in that domain (Bandura, 1977, 1982, 1997). Experiences in particular domains can also generalize to other similar domains (Bandura, 1977), suggesting own experience also shapes GSE. In settings that are new to individuals, GSE

is seen as important in forming self-efficacy beliefs (Eden, 1988; Scholz et al., 2002; Sherer et al., 1982).

There is reason to suspect low GSE may constrain women's employment in my setting. Low employment levels suggest women lack experience in employment-related domains. These need not only be domains in the workplace but could also include activities outside of the workplace required to work (e.g. managing household chores while working, persuading women's family that one should work, receiving a job offer, etc.). Women's lack of experience in these domains means their GSE could be important in forming their beliefs about their abilities in these domains. But in many countries worldwide, women's GSE is below men's (Scholz et al., 2002). Low GSE could thus give women low assessments of their ability in employment-related domains, and this could keep them from pursuing and gaining employment.[2]

Research suggests that ability beliefs do indeed affect employment and that they can be raised through short-term field interventions. Eden and Aviram (1993) offer 66 unemployed individuals in Israel a GSE intervention over several weeks. The intervention sought to enhance participants' beliefs of competence in a variety of job-search behaviors. This intervention increased job-search efforts and, for participants with low GSE, re-employment. This experiment was conducted in a very different setting than mine and in a small sample, but it does suggest a short-term field intervention to raise GSE could motivate women to take actions required to work.

3 Theoretical Framework

I outline a model that serves to formalize the relationships between GSE, external constraints, and employment, and to provide a framework for interpreting my experimental results. The key points of the model are summarized in Bandura's original paper on self-efficacy as follows:

> "Efficacy expectations determine how much effort people will expend and how long they will persist in the face of obstacles and aversive experiences. The stronger the perceived self-efficacy, the more active the efforts. Those who persist in subjectively threatening activities that are in fact relatively safe will gain corrective experiences that reinforce their sense of efficacy, thereby eventually eliminating their defensive behavior. Those who cease their coping efforts prematurely will retain their self-debilitating expectations and fears for a long time." (Bandura, 1977)

That is, self-efficacy motivates individuals to exert effort. Individuals who doubt their ability but can actually succeed gain information that raises their self-efficacy by exerting effort. Those who do not try do not learn, and they keep their pessimistic self-beliefs.

[2]Low GSE is by no means the only reason women may have low self-efficacy in domains related to employment. Hackett and Betz (1981) discuss a number of aspects of women's socialization that could constrain their career-related self-efficacy. In a separate paper, Betz and Hackett (1981) document that, relative to their male counterparts, female undergraduates have significantly lower self-efficacy in traditionally male occupations and significantly higher self-efficacy in traditionally female occupations.

3.1 A Model of GSE, External Constraints, and Employment

3.1.1 Set-Up

The model has two periods. In period one, women, indexed by i, decide whether or not to try to overcome a particular external barrier to their employment, j. Women have not tried to overcome this barrier previously; their ability to succeed is unknown to them, and their GSE determines their prior beliefs of it.

More specifically, woman i overcomes barrier j if her output, denoted $Y_{i,j}$, exceeds threshold $\underline{Y}_j$. Output is given by

$$Y_{i,j} = [\theta_{i,j} + \varepsilon_{i,j}]\mathbb{1}_{i,j}.$$

$\mathbb{1}_{i,j}$ is an indicator for woman i choosing to try to overcome barrier j. $\theta_{i,j} > 0$ represents i's ability, and $\varepsilon_{i,j}$ is a stochastic shock that is uniformly distributed between 0 and 1. I assume $\underline{Y}_j \in (0,1)$ and $\theta_{i,j} \in (0, \underline{Y}_j)$ which together ensure that women cannot overcome the barrier if they do not try and that probabilities of succeeding and failing conditional on trying are between 0 and 1.

Women do not know their own ability. i's beliefs about the likelihood that $\theta_{i,j}$ takes a given value in its range is described by the continuous density function $f_j(\theta_{i,j})$, i.e. the prior distribution. The distribution has mean $\bar{\theta}_{i,j}$ and variance σ_j^2. The shape of the distribution, $f_j(.)$, and the variance, σ_j^2, are the same for all women, but the mean, $\bar{\theta}_{i,j}$, varies across women. The realization of $\varepsilon_{i,j}$ is unknown to women but the distribution from which it is drawn is known.

I define woman i's period-one self-efficacy in domain j as $\bar{\theta}_{i,j}$. This description of self-efficacy has parallels in existing literature which also describes self-efficacy (Lybbert and Wydick, 2018), self-confidence (Bénabou and Tirole, 2002), and depression (de Quidt and Haushofer, 2016) as the perceived returns to effort.

$\bar{\theta}_{i,j}$ is determined by i's GSE. j is a task in which women have no experience so their beliefs about their ability to succeed in it will be formed by their generalized sense of self-efficacy. In particular, $\bar{\theta}_{i,j} = s_j(GSE_i)$, where GSE_i denotes i's period-one GSE and $s_j(.)$ maps GSE to self-efficacy in j. I assume that $s_j(.) \in (0, \underline{Y}_j)$ and that $s_j(.)$ is strictly increasing.

Women will try to overcome the barrier if doing so maximizes their utility. I assume i receives utility of $U_{i,j} > 0$ from overcoming barrier j, and for any woman and any barrier, the utility of not overcoming the barrier is 0. Whether they succeed or fail, all women who try must pay utility cost $c_j \in (0, max_i\{U_{i,j}\})$. All women are risk neutral.

In period two, any women who tried in period one learn whether or not they succeed, and update their beliefs accordingly. Those who succeed learn that $\theta_{i,j} + \varepsilon_{i,j} > \underline{Y}_j$ and those who fail learn that $\theta_{i,j} + \varepsilon_{i,j} < \underline{Y}_j$. Women who did not try retain their period-one beliefs. Thus self-efficacy in domain j at the end of period two is

$$\bar{\theta}'_{i,j} = \begin{cases} \mathbb{E}_{i,j}(\theta_{i,j} | \theta_{i,j} + \varepsilon_{i,j} > \underline{Y}_j) & \text{if } i \text{ tries and succeeds} \\ \mathbb{E}_{i,j}(\theta_{i,j} | \theta_{i,j} + \varepsilon_{i,j} < \underline{Y}_j) & \text{if } i \text{ tries and fails} \\ \bar{\theta}_{i,j} & \text{if } i \text{ does not try} \end{cases}$$

and the expectations are given by Bayes' rule.

Finally, revisions to beliefs of $\theta_{i,j}$ affect GSE. GSE at the end of period two is $GSE_i' = g_j(\bar{\theta}_{i,j}', GSE_i)$ for some function $g_j(.)$ that is strictly increasing in both arguments. I assume that $g_j(\bar{\theta}_{i,j}, GSE_i) = GSE_i$ so that GSE does not change if women do not try.

3.1.2 Period One: Decisions to Try

Women will try if and only if their utility from trying exceeds that of not trying. This means i's decision to try to overcome barrier j is given by

$$\mathbb{1}_{i,j}^* = \begin{cases} 1 & \text{if } \widehat{P}_{i,j}\left[\theta_{i,j} + \varepsilon_{i,j} > \underline{Y}_j\right] > \frac{c_j}{U_{i,j}} \\ 0 & \text{if not} \end{cases}$$

where $\widehat{P}_{i,j}[.]$ is i's perceived probability that she will succeed should she try.

GSE enters the decision via $\widehat{P}_{i,j}[.]$, shifting the mean of the random variable $\theta_{i,j} + \varepsilon_{i,j}$ but not changing its variance or the shape of its distribution. It follows that

$$\frac{\partial \widehat{P}_{i,j}\left[\theta_{i,j} + \varepsilon_{i,j} > \underline{Y}_j\right]}{\partial GSE_i} > 0.$$

A policy that increased women's GSE would make them more likely to try. If the women led to try have the ability to succeed, it would also increase employment.

Another approach to increasing employment would be to target the external constraint directly, i.e. decrease $\underline{Y}_j$. Because

$$\frac{\partial \widehat{P}_{i,j}\left[\theta_{i,j} + \varepsilon_{i,j} > \underline{Y}_j\right]}{\partial \underline{Y}_j} < 0,$$

this policy would also make women more likely to try to work. Further, reducing $\underline{Y}_j$ would also make it more likely that women succeed conditional on trying.

3.1.3 Period Two: Belief Updating

Pursuing employment provides information on $\theta_{i,j}$, which in turn influences GSE. Those who try and succeed observe that $\theta_{i,j} + \varepsilon_{i,j} > \underline{Y}_j$, and those who try and fail observe that $\theta_{i,j} + \varepsilon_{i,j} < \underline{Y}_j$. In the theoretical appendix, I show that

$$\mathbb{E}_{i,j}(\theta_{i,j} | \theta_{i,j} + \varepsilon_{i,j} > \underline{Y}_j) = \frac{\bar{\theta}_{i,j} - \underline{Y}_j \bar{\theta}_{i,j} + \sigma_j^2 + \bar{\theta}_{i,j}^2}{1 - \underline{Y}_j + \bar{\theta}_{i,j}} \tag{1}$$

and

$$\mathbb{E}_{i,j}(\theta_{i,j} | \theta_{i,j} + \varepsilon_{i,j} < \underline{Y}_j) = \frac{\underline{Y}_j \bar{\theta}_{i,j} - \sigma_j^2 - \bar{\theta}_{i,j}^2}{\underline{Y}_j - \bar{\theta}_{i,j}} \tag{2}$$

It follows from these expressions that $\mathbb{E}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j} > \underline{Y}_j) > \mathbb{E}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j} < \underline{Y}_j)$. Because $g_j(.)$ is strictly increasing,

$$g_j(\mathbb{E}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j} > \underline{Y}_j), GSE_i) - g_j(\mathbb{E}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j} < \underline{Y}_j, GSE_i) > 0.$$

Thus the outcome of pursuing work affects GSE; i's GSE at the end of period two will be higher if she tried and succeeds than if she tried and fails.

3.1.4 A Vicious Cycle

In this model, low GSE can create a vicious cycle. If GSE is low, women will not try to reach new attainments. If they do not try, women will not receive signals of their ability in the new domains, meaning GSE will remain unchanged and at the low value. This is analogous to the sort of poverty trap (de Quidt and Haushofer, 2016) suggest may result from depression.

3.1.5 Can Policy Spark a Virtuous Cycle?

An intervention that increased women's GSE could spark a virtuous cycle if (i) it led women to try to work, and (ii) those women had the ability to succeed. An initial increase in GSE would raise $\bar{\theta}_{i,j}$, which could lead women to try. If the women led to try succeed, their GSE would increase further because $\mathbb{E}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j} > \underline{Y}_j) > \bar{\theta}_{i,j}$. The initial increase in GSE would lead to a subsequent increase, meaning it would spark a virtuous cycle. Both conditions (i) and (ii) are important. If women do not try, they do not receive a signal, and GSE will not change. If women try but fail, the initial increase in GSE would lead to a subsequent decrease in GSE given $\mathbb{E}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j} < \underline{Y}_j) < \bar{\theta}_{i,j}$.

An intervention that reduced external constraints to employment would not have a direct effect on GSE but could lead to subsequent increases in GSE. If conditions (i) and (ii) are met, women could gain GSE from overcoming the barrier. However, this increases may be muted because

$$\frac{\partial[\mathbb{E}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j} > \underline{Y}_j) - \bar{\theta}_{i,j}]}{\partial \underline{Y}_j} > 0.$$

Reducing $\underline{Y}_j$ would thus reduce the increase in GSE that follows a successful attempt. Intuitively, if the threshold for success is lower, success provides a less favorable signal of ability.[3]

Thus either policy could lead women to overcome barriers to their employment and gain some amount of GSE in doing so. Subsequent changes in GSE for these women would depend on signals they receive in the workplace. If women receive positive signals from the workplace, there could be long-run effects on GSE under either policy. However, it is also possible that the sort of vicious cycle that keeps women from working absent intervention would also keep them from acquiring positive

[3]Bandura mentions this idea in his original paper on self-efficacy, saying: "Successes are more likely to enhance self-efficacy if performances are perceived as resulting from skill than from fortuitous or special external aids" (Bandura, 1977).

signals in the workplace. In this case, high GSE would be required to attain positive signals at work, meaning women led to work by a GSE intervention could attain such signals but women led to work through a policy that alleviated external constraints could not.

3.2 A Particular External Constraint: Family Opposition

The theoretical results discussed thus far could apply to any number of external constraints. This breadth is key in the GSE construct and also in interpreting my experimental findings. However, there is one key external barrier to employment considered in the experiment that requires additional theoretical structure to formalize: opposition to women's employment from women's family members. By family members, I mean women's husbands and parents-in-law, individuals aside from the woman who tend to be influential household decision-makers in this setting.

I model women's efforts to overcome this constraint as efforts to try to persuade their families that they should work; women's output in the generic model is an input to their family members' utility in this context. Women may hold private information about the merits of their work, which could take the form of benefits of particular job opportunities they learn about, their ability to succeed in the workplace, or their capacity to manage household chores while working. Women choose whether or not to try to persuade their families 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). In this context, $\theta_{i,j}$ describes i's ability to persuade her family of the merits of her employment; it could reflect her ability to identify the merits or convey them convincingly. Trying to persuade their families that they should work may be something women have not done before, meaning women have little information of $\theta_{i,j}$ and their GSE may determine their perceptions of it. In this setting, $\varepsilon_{i,j}$ represents random factors influencing how i's family responds to her efforts to persuade them.

After any persuasion, the household maximizes a weighted sum of utility of the woman and the utility of her family members.[4] For expositional simplicity, I assume that family opposition is the only external barrier women face so that they will work if they overcome it. Woman i will work if and only if

$$\mu_i U_{i,j} + (1-\mu_i) U_{f,j}([\theta_{i,j} + \varepsilon_{i,j}]\mathbb{1}^*_{i,j}, \gamma_{f,j}) > 0. \tag{3}$$

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. μ_i is i's bargaining weight, $U_{i,j} > 0$ is i's net utility from working, and $U_{f,j}(.)$ is the collective utility of i's family members from i working. The first argument of $U_{f,j}(.)$ is the output of the wife's persuasion and the second reflects other determinants of the family's utility. $U_{f,j}(.)$ is strictly increasing in both arguments.

I assume $\mu_i U_{i,j} + (1-\mu_i) U_{f,j}(0, \gamma_{f,j}) < 0$, meaning women cannot work unless they try to

[4]This is the model of household decision-making in Chiappori (1992) but the husband is replaced by a group of family members.

persuade. The threshold level of output required to succeed is

$$\underline{Y}_j = U_{f,j}^{-1}\left(\frac{-\mu_i U_{i,j}}{1-\mu_i}; \gamma_{f,j}\right).$$

Outcomes and decisions are as in the generic case but with this definition of $\underline{Y}_j$.[5]

A policy that increased women's GSE could lead them to try to persuade their families.[6] An alternative approach to raising employment would be to target the external constraint of family opposition directly by raising $\gamma_{f,j}$. This could be done, for example, through a campaign that promoted woman i's employment to her family. It is not obvious how offering both policies would compare to offering either alone. The comparison would depend on how far women are from the threshold absent any intervention, on whether the two interventions influence the same or different groups of people, on the form of $U_{f,j}$, and on how similar the information the promotion provides is to the information women provide.

3.3 Summary and Experiment

This model provides a framework for interpreting my experimental results. My experiment offers three randomized interventions (the experimental design is visualized in figure 1). The first intervention is a psychosocial treatment to increase women's GSE. The second, cross-randomized with the first, is an intervention that promotes women's employment to their family members. Finally, I randomly allocate job offers amongst a subsample of women who sign up for a local job. I view women who signed up for this job as having tried to work, and the job offer outcome as the signal of success or failure. The following predictions from this model map to my experimental results:

1. Effects on employment
    (a) The GSE intervention will increase employment if it leads more women to try to overcome external barriers and if those women succeed
    (b) The promotion intervention will increase employment by making it more likely that women successfully persuade their family members
2. Effects of employment: the job offer will increase GSE
3. Could policy spark a virtuous cycle?
    (a) The GSE intervention could spark a virtuous cycle if women led to try succeed in attaining work. Trying and succeeding in the workplace could provide subsequent gains in GSE.

[5] In this particular decision, $\underline{Y}_j$ varies across i. I nevertheless write $\underline{Y}_j$ rather than $\underline{Y}_{i,j}$ for consistency.
[6] In principle, GSE could also affect μ_i or $U_{i,j}$, but I find limited empirical support for these alternate channels.

(b) Sucessfully attaining work with the promotion may have little effect on GSE. Success in the workplace could provide subsequent gains in GSE, but women led to work by the promotion may not have the GSE required to receive those signals.

4 Experimental Design

I now detail the interventions and their implementation. The experimental design and timeline of the interventions are visualized in figure 1.

4.1 Partner Firm

I partner with one of India's largest carpet manufacturers. This firm 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.[7] Finally, surveyors asked to speak to any of the women whose family members had

[7]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.

not denied permission. Surveyors invited women to participate in the study and any who agreed were said to have enrolled in the study.

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) 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.[8]

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

[8]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.

control, stratifying by geographic unit.[9] 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.[10] In total, 177 meeting groups were formed, each 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.[11]

The curriculum was delivered over the nine meetings with groups of treated women. The curriculum asked women to recognize abilities they possess that would enable them to reach goals they have throughout their lives. This was done with a story and a discussion in the eighth meeting.

[9] 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.

[10] If groups tied, the group was chosen at random from the tied groups.

[11] Pilot participants are not included in the samples analyzed in this paper.

Meetings leading up to this one built knowledge of concepts required to understand the penultimate meeting. Early meetings developed an understanding of abilities and had women recognize their own. In particular, the second and third meetings taught women about talents and character strengths and asked women to recognize talents and strengths they possess. The fourth meeting asks women to recognize successes in their lives and identify the talents and strengths of theirs that contributed to those successes. Meeting five develops an understanding of what goals are and why they matter. The next meetings help women see paths they could follow to reach 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. Meeting eight ties these elements together, asking women to recognize talents and strengths they possess that would help them reach 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 activities. Concepts were illustrated with references to specific domains of life and participants considered many domains across the nine meetings. Very few of these domains involved employment. The only mention of employment in the curriculum was a story in session seven about women sowing rice. Any mention of employment aside from this would have been generated by participants in group discussion.

While the curriculum was designed to enhance GSE, it likely affected related aspects of psychology including locus of control, self-esteem, grit, or hope. As discussed in section 2.2, GSE is a distinct construct but intimately related to other psychological constructs. Indeed, GSE scales are validated in part through their correlations with measures of other constructs (Schwarzer and Jerusalem, 1995; Sherer et al., 1982). It would therefore have been difficult, and perhaps not desirable, to offer an intervention that affected GSE in isolation of these other aspects of psychology. The curriculum is also likely to have affected soft skills such as goal setting, planning, problem solving, or 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[12] 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

[12]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.

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.[13] 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.

Meetings with women were held for 84.1% of the control group. Family member meetings were held for 79.7% of control women who had eligible family members. Levels of woman and family member attendance were similar in the treated groups (columns (10) and (11) 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 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.[14] 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).[15] 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

[13] Women were not told whether their family members would be given the promotion.

[14] In a few cases, none of these family members could attend so surveyors accepted permission from one of them over the phone.

[15] If the woman did not present an identification card, her age was later verified with her village head (pradhan).

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.[16] 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.[17] 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 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.[18] The research team monitored attendance during the first three months of training and drew women from the waitlist when participants dropped out.

Though women were told the job offer outcome had been determined by a lottery, subsequent qualitative work and data from an endline survey suggest few women understood this. I return to this point and its implications in section 8.

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, baseline employment levels, and GSE of women in this subsample are similar to those in the full sample (statistics for the full sample are in table 1). Family support for women working as weavers is higher in the subsample than in the sample overall. 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.

[16]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.

[17]Note the fact that a separate list was created for each catchment area means the randomization was also stratified by catchment area.

[18]In one center, construction delays postponed the start of the program by two weeks.

Characteristics are balanced by job offer treatment.

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.

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.[19]

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

$$Y_{i,h,m} = \beta_1 T_{h,m_P}^{G\ Only} + \beta_2 T_{h,m_P}^{P\ Only} + \beta_3 T_{h,m_P}^{Both} + \mu_s + \rho X_{i,h,m} + \varepsilon_{i,h,m}. \quad (4)$$

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. $T_{h,m_P}^{G\ Only}$ is an indicator for assignment to GSE treatment and promotion control, $T_{h,m_P}^{P\ Only}$ an indicator for assignment to promotion treatment and GSE control, and T_{h,m_P}^{Both} 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

[19] 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.

both the household level and the meeting group × promotion treatment level. I allow for two-way clustering using the stata reghdfe package of Correia (2014).

μ_s denote strata (i.e. geographic unit) 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 several hundred 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 (4) 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

$$Y_{i,h} = \beta T_h^J + \mu_{s_J} + \rho X_{i,h} + \varepsilon_{i,h}. \tag{5}$$

T_h^J 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.

μ_{s_J} are fixed effects for strata used in the job offer and waitlist randomization (i.e. village × GSE treatment × promotion treatment). Whenever strata are "incomplete" in the sense that they do not have both job offer treatment and job offer control members, I pool all strata within a village. If the village is incomplete, I drop individuals from that village from the regression. I do not pool across villages because the probability of receiving a job offer differed across villages. $X_{i,h}$ are covariates selected using the post-double-selection LASSO method. For job offer regressions, I allow both baseline variables and measures of these variables at the two-week endline, the last survey done before job offers were given, to be selected.

6 Manipulation Check

I begin the empirical analysis by investigating the effects of the GSE intervention on GSE and exertion of effort. I estimate equation (4) but replace the three treatment indicators with an indicator for assignment to GSE treatment.

6.1 Effects of GSE Treatment on GSE

GSE is measured using the questionnaire in appendix table A.1. It is the Schwarzer and Jerusalem (1995) scale, a standard scale from psychology to measure GSE, with slight modifications to enhance comprehension of respondents in my setting. There were two key modifications: (i) all items were changed from statements that respondents assessed agreement with to questions, and (ii) for questions that tended to be difficult to understand, alternate phrasings and explanations were added and used when surveyors encountered issues with comprehension. Responses to each of the 10 items were recorded on a 1-4 scale (with higher values reflecting greater GSE), and a "don't know" option was also included. I measure GSE by averaging responses to questions that were answered and setting the average to missing if more than three responses are "don't know." This handling of missing responses is what is suggested by the authors of the original scale and was pre-specified, though I consider robustness to an alternate approach, also pre-specified, below.

I find positive and persistent effects of GSE treatment on GSE (panel A of table 3). In the surveys immediately following the final GSE treatment and control meetings, GSE was 0.178 points (on a 1-4 scale) higher amongst GSE treatment women than GSE controls, and the effect is highly significant (p-value = 0.002). As discussed in section 5.1.1, this result should be interpreted with caution given 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. The one-week and two-week endline effects are similar in magnitude to the week zero effect (0.173 and 0.154 points) and again highly significant (p-values = 0.001 and 0.001). The four-month effect is smaller (0.100 points) though still significant (p-value = 0.029). The effect is persistent; an effect of 0.135 points (p-value = 0.015) remains at one year.

The GSE 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 a second GSE measure that replaces "don't know" responses with the answer choice corresponding to the lowest level of GSE (i.e. 1) prior to averaging across the 10 items.

Effects of GSE treatment on this alternate measure of GSE are positive and significant at all five endlines (panel B of table 3). Effects are generally smaller in magnitude on this measure than on the first measure, suggesting treatment made women more likely to give "don't know" responses.

Both short-run (one week) and long-run (one year) effects appear to come from the middle of the baseline GSE distribution (figure 2).[20] There are a variety of possible explanations for this. It could be that women in the middle of the distribution are more uncertain about their abilities and thus more easily persuaded. It is also possible that women in the middle of the distribution are the ones who tend to be underconfident and that the treatment inspired introspection that allowed these women to recognize their underconfidence. Average responses are around 3 (on the 1-4 scale)

[20] The corresponding plots for the other three endlines are in figure A.2.

so there being no effect at the top of the distribution may also be due to ceiling effects.

A 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.6 uses data from the third week and compares GSE measures of women who were taking the GSE questionnaire for the first time to those of women who were taking the questionnaire for the third time. I find no effect of multiple assessment on either GSE measure. This suggests administering the questionnaire multiple times had no effect on the average response.

6.2 Effects of GSE Intervention on Exertion of Effort

In the model, GSE affects economic outcomes by leading women to try to reach new attainments. Evidence for this channel comes from an incentivized task. The task was done at both the four-month and one-year endlines. Women were asked to choose to either (i) receive a prize worth Rs.20, or (ii) attempt to complete a puzzle in two minutes, and win a prize worth more than Rs.20 if successful and worth Rs.10 if not. A randomization determined whether the prize for successfully completing the puzzle was worth Rs.30 or Rs.40. The puzzle at the four-month endline was a small jigsaw puzzle. On the one-year endline, the puzzle was assembly of small, plastic blocks to build a toy house. There are two key features of these puzzles: first, they are activities women in this setting have rarely done before, and second, success in them is largely a function of ability. The decision to attempt the puzzle can thus be seen as a decision to try to reach a new attainment that is driven by favorable evaluations of own ability.

GSE treatment increases the likelihood that women choose to attempt the puzzle (columns (1)-(4) of table 4). The effect at four-months is 5.6 percentage points, which is a relatively small effect off a base of 62% but nevertheless significant (p-value = 0.039). This is driven by women offered the high prize, for whom the effect is 7.0 percentage points (p-value = 0.110). The effect at one year is 4.4 percentage points though it is not significant (p-value = 0.172). At this later endline, the overall effect is driven by the group of women offered the low prize; the treatment effect in this group is 10.5 percentage points and significant (p-value = 0.022). These results suggest GSE treatment made women more likely to try to reach new attainments.

While these effects may be driven by increased assessments of own ability, they are also consistent with changes in risk taking. To separate these two explanations, I asked women to make another decision on the one-year endline. This decision was identical to the puzzle decision with one exception: instead of attempting a puzzle, participants could draw out a ball from a canvas bag without looking, winning if the ball was one of the two balls with happy faces and losing if the ball was the one with a sad face. This task is one in which success is purely a function of luck and thus isolates risk taking from ability beliefs.

I find no effect of GSE treatment on the choice to draw a ball (columns (5) and (6) of table 4). There is no effect overall or at either prize level. This suggests effects of puzzle choice, at least at one year, are driven by changes in ability beliefs rather than changes in risk taking.

7 Effects of GSE and Promotion Treatments on Employment

I now investigate the effects of the GSE and promotion interventions on employment as well as the channels underlying these effects.

7.1 Effects on Employment

7.1.1 Effects on Sign-Up

I first consider effects on decisions to sign-up for the firm's program.[21] 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.

The GSE treatment on its own significantly increased sign-up (column (1) of table 5). The effect is 5.8 percentage points (p-value = 0.087). Off a base of 19.4%, this represents a large 29.9% increase. This suggests that absent intervention, many women could sign up for work but are constrained by low beliefs of their abilities.

The promotion alone also increased sign-up. The effect is 11.0 percentage points, which represents an enormous 56.7% increase (p-value = 0.003). 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 gain beyond offering either alone and is worse for sign-up than the promotion only. The effects of offering both interventions relative to offering neither is positive (2.4 percentage points, or 12.4%) but not significant (p-value = 0.484). This effect is no larger than the effects of either intervention alone and significantly smaller than the effect of the promotion alone (the p-value for the test that the effect of offering both treatments equals the effect of offering the promotion alone is 0.025). The fact that employment is no higher when both interventions are offered than if either is offered alone is consistent with women and the promotion doing the same sort of persuasion of family members, perhaps because women conveyed messages they had heard in the promotion when attempting to persuade their family members. The finding that offering both interventions is worse for sign-up than offering the promotion alone is suggestive of direct crowd-out between the interventions, whereby the presence of one makes the

[21] I do not consider participation in the program as an outcome because oversubscription meant many women who signed up were not given a chance to participate.

other less effective. There may be family backlash to the promotion when women have been gaining GSE, or backlash from efficacious women who view the promotion as removing their autonomy.

Figure 3 visualizes where in the baseline GSE distribution these effects come from. The effect of GSE treatment alone on sign-up comes from a similar part of the distribution as the effect of GSE treatment on GSE (visualized in figure 2). This is reassuring for the notion that an increase in GSE produced the effect on sign-up. Interestingly, the effect of promotion treatment only on sign-up comes from a similar part of the distribution as the effect of GSE treatment only. This suggests the two interventions affect similar groups of women.

7.1.2 Effects on Any Employment

I also consider effects on women's reports of doing any work for income at the two-week, four-month, and one-year endline surveys.[22] On these surveys, I asked women whether they had worked for income in any of 10 common employment sectors[23] in the preceding two weeks; I consider a woman as working for income at a particular endline if she reported work in any sector.

GSE treatment alone did not affect employment in the two weeks immediately following the intervention but produced a large increase at four months (columns (2)-(3) of table 5). The effect on doing any work for income at the two-week endline is positive (3.0 percentage points, or 13.8%) but not significant (p-value = 0.448). 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). Thus the effect of GSE treatment only on employment took time to emerge but was substantial once it did. This suggests many women who do not work absent intervention have the ability to but are constrained by low ability beliefs.

The promotion alone has large effects on short- and medium-run employment. The effect on doing any work for income in the two weeks immediately following the GSE intervention is 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. An effect of the promotion alone is present at four months, when it increases any work for income by 9.8 percentage points, or 42.2% (p-value = 0.017). Like the effect on sign-up, these effects suggest that family member opposition is a key barrier to women's employment but that it can be alleviated through a light-touch intervention.

Offering both interventions is no better for short- and medium-run employment than offering

[22] I do not use data from the zero- or one-week endlines because neither asked about employment.

[23] 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.

either alone, and in the short-run, it is worse than offering the promotion alone. At two weeks, the employment effect of offering both interventions relative to offering neither is positive (3.7 percentage points, or 17.0%) but not significant (p-value = 0.298). This does not exceed the effect of either intervention alone on this outcome and is significantly smaller than the effect of the promotion alone (the p-value for the test that the effect of offering both treatments and the effect of offering the promotion alone are equal is 0.041). This parallels effects on sign-up and the same sources of backlash there could be at play here. At four months, however, offering both interventions rather than neither has a positive, large, and significant effect on employment (7.0 percentage points, or 30.2%, with p-value = 0.097). This effect does not differ from the effect of either intervention alone (p-values = 0.697 and 0.537). The two interventions appear redundant as a means of raising employment at four months, perhaps because they result in the same sort of persuasion of family members.

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 (column (4) of table 5). Interestingly, these null effects 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,[24] so I cannot say definitively that spillovers explain the null effects at one year.

Next, I consider additional measures of any employment and summary indices of all 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.[25] 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 indices of all measures of any employment from each endline. While I present effects on these alternate measures and on the summary indices, I note the measure of any employment considered thus far (i.e. having done any work for income in the preceding two weeks) is my preferred measure

[24] 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

[25] As much as possible, these surveys were scheduled so that the previous day would not have been a Sunday or a holiday.

of employment; the question 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.

Like effects on the preferred measure of any employment, effects on the summary indices suggest positive short- and/or medium-run effects of each intervention alone, no gains from combining the two interventions, and no long-run effects (table A.7).

7.1.3 Effects on Employment by Sector

Finally, I consider effects on employment by sector. Recall that the any work for income outcomes (in columns (2)-(4) of table 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 sector 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.

One-year endline surveys also asked women about work for income they did in a rice cultivation that occurred 10 months after the GSE intervention. Effects on this work are presented in appendix table A.11. Assignment to either of the two GSE treatment cells significantly increases work done in this cultivation on own household's land, but neither affects work off own household's land. These results potentially reflect demand effects given the story in the seventh session of the intervention was about women working in this cultivation. I find no effect of assignment to promotion only on work in this cultivation.

7.2 Channels: Effects on Household Decision-Making about Employment

I model GSE as raising women's employment by leading women to try to overcome external barriers to their work. A key external barrier women in this setting face is opposition to their employment from their family members. In section 3.2, I outline a framework in which women can overcome this constraint by trying to persuade their family members. The promotion targets this constraint directly, and a potential reason that combining the two interventions provided no employment gain beyond offering either alone is that the two led to the same sort of persuasion of family members.

This section presents evidence that supports these channels. I consider effects on variables that proxy for the three key determinants of the household's decision in equation (3): women's bargaining power (μ_i), women's utility from work ($U_{i,j}$), and family members' utility from women's work ($U_{f,j}$). 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 bargaining power, 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.[26] As proxies of women's utility, I use women's reported interest in signing up and family members' predictions of women's interest, and as proxies of family members' utility, 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 so as to avoid concerns of ex post rationalization of sign-up decisions. 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) and can be seen as proxies for the gap between women's and family members' utilities.

I find no evidence that either treatment affected women's influence over the sign-up decision and some evidence that GSE treatment increased women's interest in sign-up (columns (1)-(4) of table 6). There is no effect of assignment to any of the three treated cells on women's or family members' reports of women making sign-up decisions. This provides suggestive evidence that neither treatment influenced women's bargaining power in the employment decision. I find a positive effect of GSE treatment only on women's interest in signing up (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 utility but the effect was weak.[27] 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 persuade their family members.

Data suggest the GSE treatment gave women the confidence to try to persuade their family members and that many had the ability to change their family members' opinions (columns (5)-(8)). 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 women's and family members' utility from women's work. 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 inter-

[26] The indicators take the value of 1 when only the women or the women and others equally 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.

[27] An effect on women's interest does not mean GSE treatment explicitly encouraged employment; an effect could come from higher assessments of one's ability to succeed in the workplace, or from women being motivated to pursue goals and recognizing employment as a means of reaching goals.

vention did not teach negotiation skills, there are parallels between these results and the effects of interventions that do (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.

These results are also consistent with the proposed explanation for there being no employment gains from combining the interventions, i.e. that the two interventions produced the same sort of persuasion of family members. The promotion alone produced effects on disagreement that are similar to the effects of GSE treatment alone. There is surprisingly no effect of the promotion alone on family members' interest, though women in the promotion only treatment cell report significantly higher family member interest (p-value = 0.031). This effect on women's predictions is similar to the effect of GSE treatment alone on this variable. Moreover, combining the two interventions provides no further gains in family member interest beyond offering either alone, nor does it provide further reductions in disagreement. This also supports the notion that the two interventions were redundant in their persuasion of family members. Effects on women's reports of disagreement provide some evidence of backlash from combining the two interventions; the effect of both treatments on this outcome is about half the size of the effect of either treatment alone (the p-value for the equality of the both and GSE only treatment effects is 0.151 and for the both and promotion only effects is 0.092). Effects on family member disagreement are noiser so it is difficult to discern a similar pattern on that outcome.

These findings are consistent with the notion that GSE leads women to work by giving them the confidence to try to overcome external barriers to their employment. My data allow me to document this channel for the particular barrier of family member opposition, but it is of course possible that GSE treatment produced efforts to overcome a number of other external barriers that I do not have data on.

8 Effects of the Job Offer on GSE

I now use the job offer experiment to investigate how pursuing work affects GSE.

8.1 Beliefs of How Job Offers Were Assigned

Though households were told slots in the program had been allocated by a lottery, it appears many did not understand this. Subsequent qualitative work suggested it is not widely understood in this setting what a lottery is. A question on the one-year endline survey, which occurred 11 months after job offer results were delivered, asked women how they thought job offers had been assigned. Appendix table A.4 tabulates the beliefs of women in the job offer experiment. Only 17.2% believed women who could begin the program immediately were chosen by chance or lottery. 15.7% believed women with the most skill or experience were chosen, and another 11.8% believed selection was based on which women and households were liked best. 20.1% provided another reason, and 35.3% said they did not know.

These responses are consistent with many women viewing the offer as a signal of their abilities. These abilities could encompass their potential to succeed in the job, how well liked they are by a potential employer, or other personal characteristics. This in turn implies women may have updated their GSE in response to the job offer.

8.2 Effects of the Job Offer on GSE

I use equation (5) to estimate effects of the job offer on GSE. I use the two GSE measures detailed in section 6.1. I consider GSE at the four-month endline, which occurred three months after job offers were given.

The job offer significantly increases GSE (columns (1) and (2) of table 7). Three months after job offers were given, women given an offer have GSE that is 0.166 points (on a 1-4 scale) above that of women who signed up for the job and were waitlisted (p-value = 0.013). The effect on the alternate measure is similar in magnitude (0.160 points) and also significant (p-value = 0.029). These effects are similar in magnitude to the effects of GSE treatment on GSE (table 3).

Though the job offer increases GSE, it does not affect exertion of effort in the incentivized task (columns (3) and (4) of table 7). I consider effects on women's decisions to choose to attempt the puzzle in the task at four months (see section 6.2 for details on this task). There is no effect of the job offer on this choice at either prize level or overall across the prize levels. This is somewhat surprising given that exertion of effort is seen as a key behavioral response of self-efficacy and that the GSE intervention increases effort in this task. One possible explanation is that women who signed up for the job had higher GSE than those who did not, and this means that the margin of women whose effort in this task can be changed by higher GSE choose to attempt the puzzle in both the job offer treatment and control groups. Indeed, GSE and puzzle sign-up rates in the job offer control group are higher than in the GSE treatment control group.

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

We have seen that the job offer increases 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. I distinguish between these two explanations by exploiting the random ordering of the waitlist. The waitlisted women who were invited to participate were not given the positive signal from the offer but were given the job. Because each village's waitlist was randomly ordered, invitation from the waitlist is randomly assigned. I investigate the effect of the job in isolation of the offer by comparing waitlisted women who were invited to participate in the first two months of the program to waitlisted women who were not.[28] These are the months that were completed before four-month endline surveys began.

I first show that invitation from the waitlist increased participation in the program (column (2) of table 8). Invitation raised the fraction of women who ever attended the program in its first two

[28] In particular, I restrict the sample to waitlisted women and estimate a regression that is analogous to equation (5) but replaces the job offer indicator with an indicator for being invited from the waitlist.

months by 51.2 percentage points (p-value = 0.000) off a base of 0.00%. The job offer also increased participation (column (1)), but the effect is much smaller, at 12.7 percentage points (p-value = 0.093) off a base of 30.8%.

Despite increasing program participation, invitation from the waitlist does not affect GSE (columns (3) and (4) of table 8). I do not find a significant effect on either measure of GSE. Effects are positive but they are smaller in magnitude than effects of the job offer, which is particularly remarkable given waitlist invitation produced a much larger increase in program participation. These waitlist analyses of course cannot capture effects of the job that were slow to evolve, but they do suggest the job had no short-run effects on GSE. I also find no effect of waitlist invitation on exertion of effort in the incentivized task (columns (5) and (6)).

These results suggest that it is the signal the offer provides and not the job that affects GSE. This is the story outlined in the second period of the model. Taken together, findings from the job offer experiment suggest that women acquire information in the pursuit of work from which they update beliefs of their abilities.

9 Discussion

9.1 Can Policy Spark a Virtuous Cycle?

I find that an intervention in women's GSE leads women to try to reach new attainments. It makes them more likely to try to complete a puzzle with the goal of winning a higher prize in an incentivized task. Data also suggest it motivates them to try to persuade their family members that they should work. In turn, results from the job offer experiment suggest that whether women succeed or fail in new endeavors affects GSE.

These results suggest that an intervention that raises women's GSE could spark a virtuous cycle if women led to try have the ability to succeed. Whether the GSE intervention did indeed spark a virtuous cycle depends on signals women received in their daily lives and across many domains. The persistence of the effect of the intervention on GSE suggests a virtuous cycle may have indeed been sparked for many women.

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.

In principle, a policy targeting external constraints to employment could also lead to a virtuous cycle. The model suggests increases in GSE individuals experience when they overcome a constraint may be muted when an external policy has alleviated that constraint. But if the intervention allows women to work, they could receive signals of their ability in the workplace.

Despite the large increase in employment from the promotion, I find no effect of the intervention on GSE (appendix table A.12). Assignment to promotion treatment only has no effect on either

GSE measure at any of the endlines. Further, the effects of assignment to GSE treatment only and to both treatments do not differ significantly from one another.

This not only suggests the promotion did not spark a virtuous cycle, but also that employment does not affect GSE. This is consistent with invitation from the waitlist not affecting GSE. It is also consistent with GSE in the control group not catching up to GSE in the treated group at one year despite employment doing so. It could be that women receive both positive and negative signals at work that cancel one another out, or that women succeed at work but this comes at the cost of success in other areas of life.

If employment itself does not increase GSE, how could the GSE intervention have sparked a virtuous cycle? It may be that receiving positive signals in the workplace requires exerting additional effort and sufficiently high GSE is required to exert that effort. Consistent with this explanation, I find that women in the promotion only treatment cell who signed up for the firm's program have significantly lower GSE than those in either GSE treatment cell who signed up (appendix table A.13). It is also possible that positive signals women in the GSE treatment group received came from domains outside of the workplace.

9.2 Welfare

The question of whether psychosocial interventions that increase self-confidence improve welfare is an important one, but one that is complicated and that I cannot fully answer. A key welfare concern is whether the intervention made women correct or overconfident. Effects of the GSE treatment on employment and household decision-making suggest that many women who do not try absent intervention could succeed if they did. These effects, however, do not rule out that some women who were led to try by the intervention failed.

Data on performance in the firm's program can speak to the concern about failure rates. 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. Data come from program participation records. 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 are imprecise given the small sample size, but if anything, women in one of the two GSE treatment cells perform better than women assigned neither treatment (appendix table A.14). 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, GSE treatment may have led women to try harder at work, and this could have improved their performance. It is also possible that women's efforts to convince their family members in the sign-up decision produced lasting support for participation, allowing women to miss less work and perform better as a result. This explanation is consistent with performance also being higher in the promotion only treatment cell.

These findings are reassuring, but many welfare-relevant questions remain. GSE treatment could have led women to try in many domains and performance in the firm's program says little

about their success in other domains. For instance, these results overlook women who tried and failed to sign up as well as women who signed up but were not invited to participate. Moreover, even for women who tried and succeeded in a particular domain, trying may not have been optimal in the sense that the true probability of success may not have exceeded the cost to benefit ratio, i.e. $\frac{c_j}{U_{i,j}}$. The welfare issue becomes more complicated when one considers the possibility that the GSE intervention affected other aspects of psychology or soft skills in addition to beliefs of ability. Further, it is not clear that employment is welfare-enhancing for the women who successfully attain it. On the one hand, women report a desire to work, and evidence from this setting in McKelway (2019) suggests working women can control how their earnings are spent. On the other hand, McKelway (2019) also finds that employment comes at the cost of women's leisure and reduces long-run happiness.

I conduct a final analysis that speaks to these broader welfare concerns; appendix table A.15 presents effects of the GSE and promotion interventions on self-reported happiness at each of the five endlines. There is some evidence that GSE treatment produced short-run increases in happiness, but there are no long-run effects. Promotion treatment only has no effect on happiness at the first four endlines, but causes a highly significant reduction in happiness at one year (this is the reduction in happiness discussed in McKelway (2019)). The results are consistent with the GSE intervention increasing happiness and many months of employment producing offsetting decreases. Under this interpretation, the welfare effects of the GSE intervention remain ambiguous. However, it is also possible that GSE treatment or employment alter the way women assess their happiness over time. Future research is needed to assess the welfare implications of psychological counseling interventions that are designed to increase self-confidence.

10 Conclusion

I find that a psychosocial intervention to increase GSE of women in India raises their employment. It appears to do so by giving women the confidence to overcome external constraint to their work. In turn, success in pursuing work increases GSE. Raising GSE could therefore spark a virtuous cycle when women have the ability to succeed, and the persistence of the effect of the psychosocial intervention on GSE suggests a virtuous cycle may have been sparked for many women. Promoting women's employment to women's family members, whose opposition to women's work represents a key external constraint, also increases employment but does not affect GSE. The promotion intervention was certainly a less expensive means of raising women's employment than the GSE intervention, but it does not appear to have broken a vicious cycle in women's psychology. Taken together, my findings suggest psychosocial interventions should not be overlooked as a means of improving both the psychological and economic trajectories of women.

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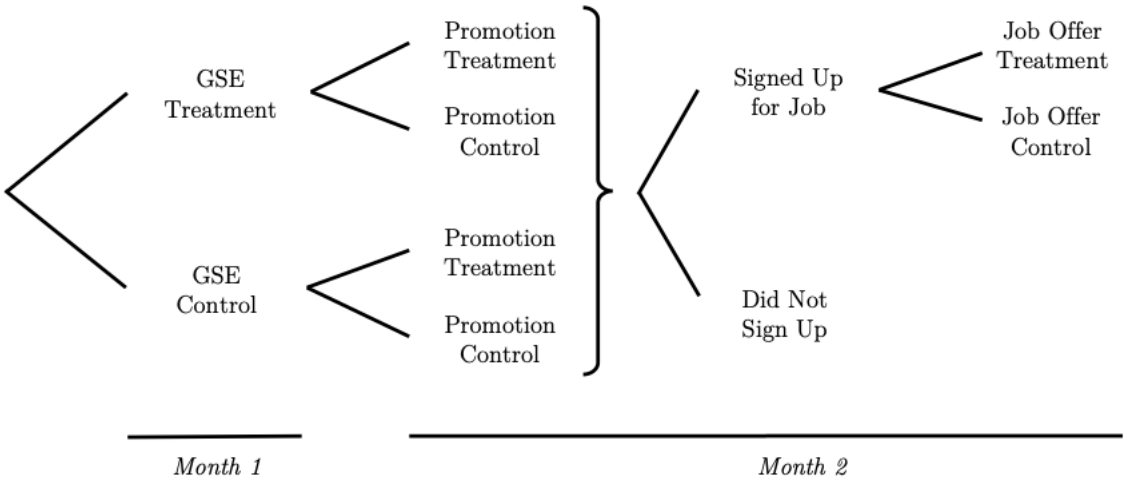
Figures and Tables

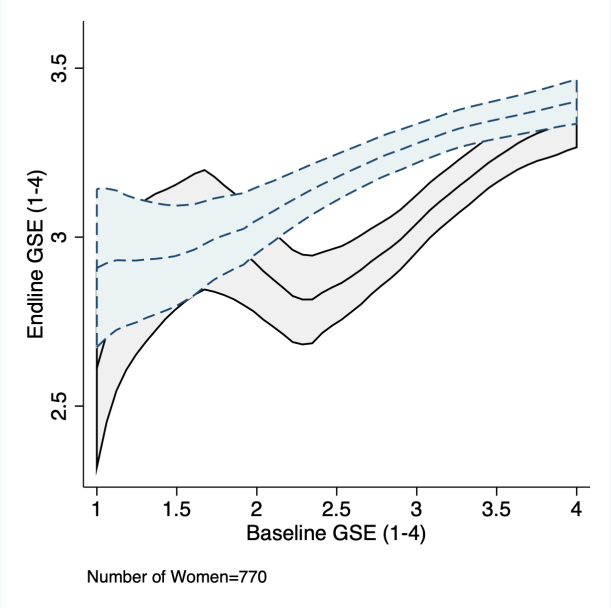
Figure 1: Experimental Design



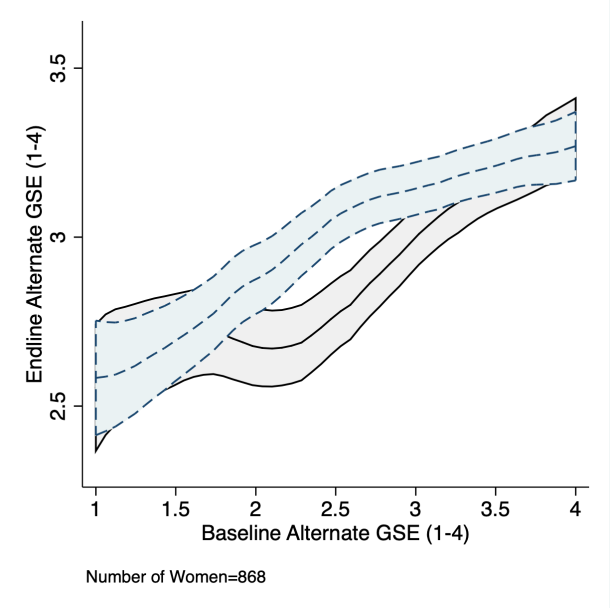
Notes: This figure visualizes the experimental design and timeline of the interventions. The 1,022 women who enrolled in the study were assigned GSE treatment or control. A cross-randomization determined promotion treatment status. Women were then invited to sign up for the partner firm's employment 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.

Figure 2: Endline GSE by Baseline GSE and GSE Treatment Status

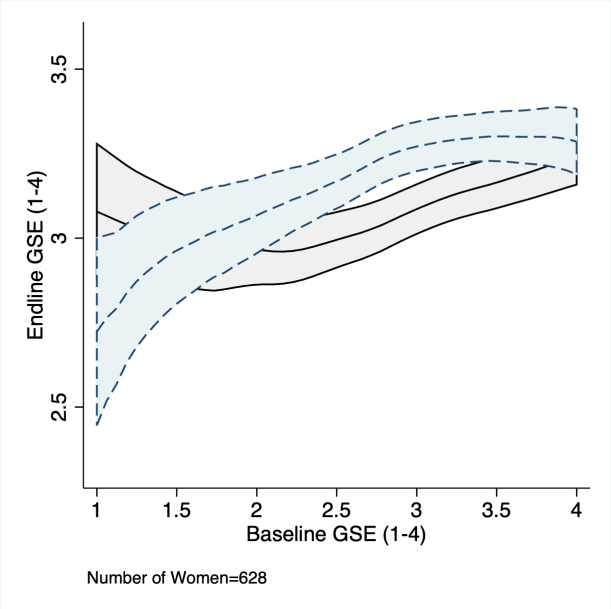
(a) One-Week Endline, GSE



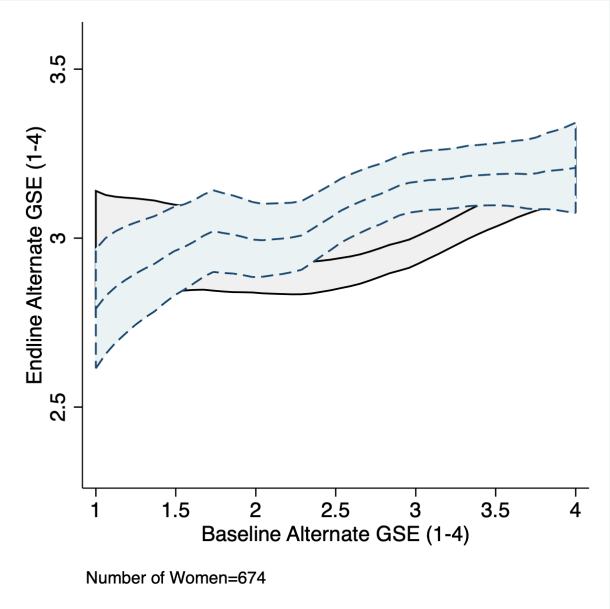
(b) One-Week Endline, Alternate GSE



(c) One-Year Endline, GSE



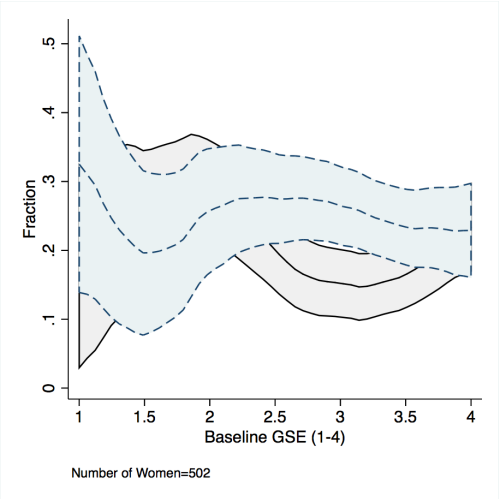
(d) One-Year Endline, Alternate GSE



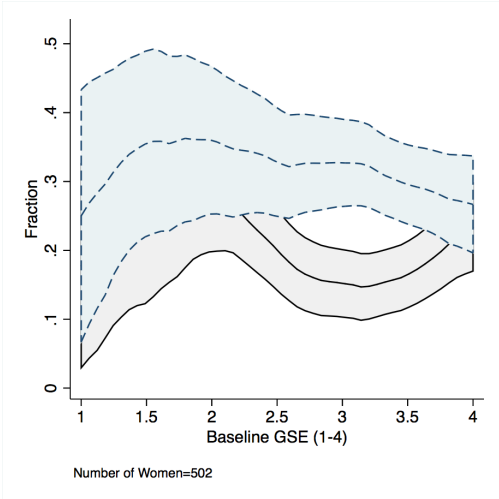
Notes: This figure plots endline GSE against baseline GSE, separately for GSE treatment and control women. Each panel plots smoothed values from kernel-weighted local polynomial regressions of endline GSE on baseline GSE in the GSE treatment group (blue, dotted line) and in the GSE control group (black, solid line). The lines are surrounded by 90% confidence intervals. Endline data in panels (a) and (b) come from the one-week endline survey, and endline data in panels (c) and (d) comes from the one-year endline survey. Panels (a) and (c) use the GSE measure from panel A of table 3 for endline GSE. I use a slightly different measure for baseline GSE in these panels; the measure is the average of responses to the 10-item GSE questionnaire, *not* set to missing if more than three are "don't know." I make this slight modification because there were many "don't know" responses at baseline. Panels (b) and (d) use the GSE measure from panel B of table 3 for baseline and endline GSE.

Figure 3: Sign-Up by Baseline GSE and Treatment Status

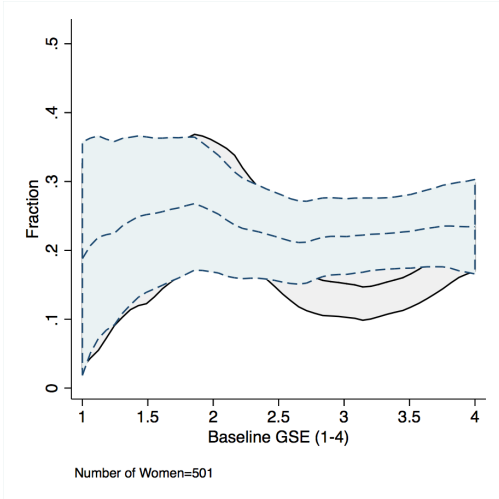
(a) GSE Treat Only, GSE



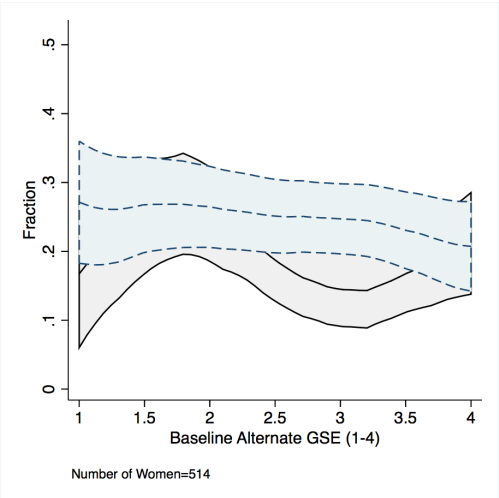
(b) Promo Treat Only, GSE



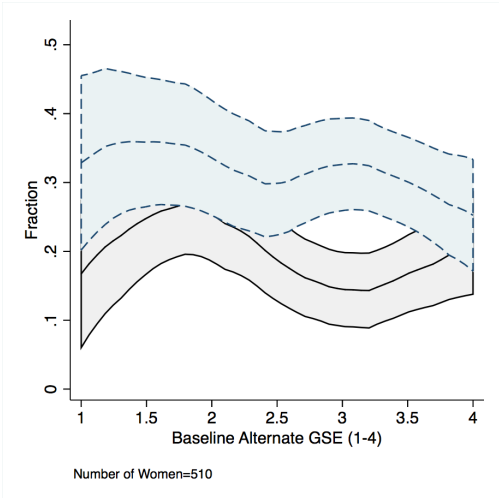
(c) Both Treat, GSE



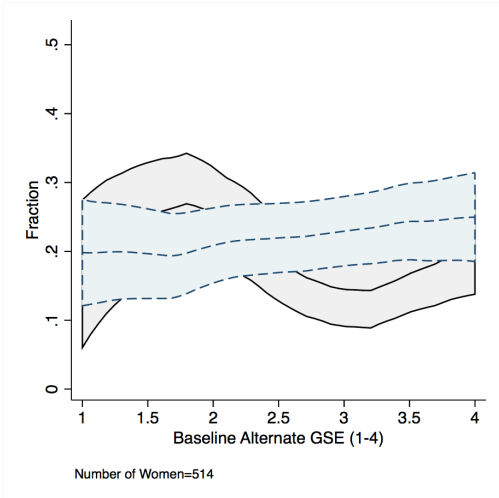
(d) GSE Treat Only, Alternate GSE



(e) Promo Treat Only, Alternate GSE



(f) Both Treat, Alternate GSE



Notes: This figure plots sign-up against baseline GSE, separately for treatment and control women. Each panel plots smoothed values from kernel-weighted local polynomial regressions of sign-up on baseline GSE in a treatment group (blue, dotted line) and in the control group (black, solid line). The control group is women assigned neither treatment; the treatment group is women assigned GSE treatment only in panels (a) and (d), women assigned promotion treatment only in panels (b) and (e), and women assigned both treatments in panels (c) and (f). The lines are surrounded by 90% confidence intervals. The sign-up variable is the outcome in column (1) of table 5. The GSE measure in panels (a)-(c) corresponds to the measure from panel A of table 3 but is slightly different; the measure in these panels is the average of responses to the 10-item GSE questionnaire, *not* set to missing if more than three are "don't know." I make this slight modification because there were many "don't know" responses at baseline. Panels (d)-(f) use the GSE measure from panel B of table 3.

Table 1: Baseline Characteristics and Balance in Main Experiment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					P-Value for Test that:					
	Control (C) Mean	G Only (G) − C (Std Error)	P Only (P) − C (Std Error)	Both (B) − C (Std Error)	G = P	G = B	P = B	G = 0 B = 0	P = 0 B = 0	N Women
Age	29.461	0.931*	-0.045	0.489	0.053*	0.423	0.304	0.209	0.551	1022
		(0.526)	(0.477)	(0.538)						
Married (=1)	0.984	-0.015	0.005	0.003	0.096*	0.115	0.830	0.277	0.895	1022
		(0.012)	(0.010)	(0.010)						
Husband's Age	33.545	0.096	-1.024*	-0.500	0.068*	0.355	0.406	0.618	0.241	924
		(0.638)	(0.607)	(0.658)						
Years Since Marriage	12.543	1.096*	0.147	0.470	0.133	0.316	0.590	0.237	0.732	979
		(0.646)	(0.598)	(0.609)						
Currently Pregnant (=1)	0.078	-0.000	0.004	0.042*	0.854	0.101	0.152	0.175	0.212	1022
		(0.023)	(0.023)	(0.025)						
Number of Children	2.674	0.164	-0.089	0.054	0.039**	0.402	0.262	0.441	0.515	1022
		(0.132)	(0.127)	(0.137)						
Lives in In-Laws' Village (=1)	0.992	-0.019*	-0.013	0.000	0.677	0.110	0.188	0.217	0.338	1022
		(0.011)	(0.010)	(0.009)						
Number of Adults in HH	4.097	-0.321	0.078	0.071	0.127	0.136	0.979	0.262	0.945	1022
		(0.253)	(0.253)	(0.272)						
Parent or Parent-in-Law in HH (=1)	0.543	-0.051	0.007	0.001	0.213	0.248	0.900	0.429	0.987	1022
		(0.046)	(0.045)	(0.045)						
Belongs to Scheduled Caste or Tribe (=1)	0.481	0.013	0.018	-0.027	0.890	0.203	0.133	0.424	0.322	1022
		(0.035)	(0.033)	(0.033)						
No Education (=1)	0.419	0.109***	0.079*	0.082*	0.453	0.511	0.951	0.031**	0.112	1018
		(0.042)	(0.043)	(0.045)						
Husband Has No Education (=1)	0.136	0.017	-0.011	0.038	0.465	0.644	0.255	0.674	0.510	634
		(0.040)	(0.038)	(0.043)						
GSE (1-4)	3.065	0.092	0.131*	0.056	0.572	0.596	0.279	0.408	0.181	735
		(0.069)	(0.071)	(0.070)						
Alternate GSE (1-4)	2.698	-0.010	0.079	-0.029	0.232	0.795	0.131	0.917	0.298	1022
		(0.075)	(0.073)	(0.071)						
Happiness (1-4)	3.164	-0.057	0.015	0.049	0.370	0.174	0.618	0.391	0.776	978
		(0.082)	(0.071)	(0.071)						
Working for Income (=1)	0.319	0.006	-0.053	0.011	0.138	0.916	0.112	0.968	0.220	927
		(0.042)	(0.040)	(0.042)						
Husband Working for Income (=1)	0.755	-0.047	-0.040	0.049	0.891	0.059*	0.076*	0.166	0.207	582
		(0.051)	(0.049)	(0.050)						
Appropriate for Women in HH to Work as Weaver (1-4), Family's Report	2.513	0.040	0.006	-0.071	0.846	0.528	0.666	0.813	0.890	594
		(0.179)	(0.176)	(0.175)						
HH Member Worked as Weaver in Last Year (=1)	0.351	0.009	0.004	0.004	0.924	0.937	0.993	0.986	0.997	649
		(0.053)	(0.052)	(0.058)						
HH Member Worked as Weaver at a Partner Firm Loom in Last Year (=1)	0.063	0.016	-0.048**	0.001	0.008***	0.610	0.067*	0.793	0.057*	649
		(0.025)	(0.023)	(0.028)						

Notes: This table presents average values of 20 baseline variables in the full sample and tests for balance by GSE and promotion 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 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 × 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

	(1) No Offer Mean	(2) Offer - No Offer (Std Error)	(3) N Women
Age	29.641	1.213	253
		(0.840)	
Married (=1)	0.974	0.015	253
		(0.018)	
Husband's Age	33.203	0.815	234
		(0.948)	
Years Since Marriage	13.554	0.176	243
		(0.890)	
Currently Pregnant (=1)	0.096	-0.043	253
		(0.039)	
Number of Children	2.833	-0.078	253
		(0.206)	
Lives in In-Laws' Village (=1)	0.994	-0.021	253
		(0.018)	
Number of Adults in HH	3.333	0.452	253
		(0.350)	
Parent or Parent-in-Law in HH (=1)	0.404	0.100	253
		(0.076)	
Belongs to Scheduled Caste or Tribe (=1)	0.558	-0.020	253
		(0.068)	
No Education (=1)	0.603	0.119*	253
		(0.068)	
Husband Has No Education (=1)	0.237	-0.055	152
		(0.068)	
GSE (1-4)	3.181	-0.174	175
		(0.122)	
Alternate GSE (1-4)	2.696	-0.016	253
		(0.129)	
Happiness (1-4)	2.980	-0.012	240
		(0.142)	
Working for Income (=1)	0.329	0.043	232
		(0.073)	
Husband Working for Income (=1)	0.744	0.021	134
		(0.086)	
Appropriate for Women in HH to Work as Weaver (1-4), Family's Report	3.033	0.208	145
		(0.265)	
HH Member Worked as Weaver in Last Year (=1)	0.453	0.038	156
		(0.084)	
HH Member Worked as Weaver at a Partner Firm Loom in Last Year (=1)	0.084	-0.025	156
		(0.046)	

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 a job offer treatment indicator. 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 of GSE Treatment on GSE

Panel A: Effects on GSE					
	(1)	(2)	(3)	(4)	(5)
			GSE (1-4):		
	0 Week EL	1 Week EL	2 Week EL	4 Month EL	1 Year EL
GSE Treat (=1)	0.178***	0.173***	0.154***	0.100**	0.135**
	(0.057)	(0.051)	(0.046)	(0.045)	(0.055)
Strata FE	Yes	Yes	Yes	Yes	Yes
PDSLASSO X	Yes	Yes	Yes	Yes	Yes
Control Mean	3.185	3.100	3.148	3.253	3.113
N Women	592	782	798	750	635
Panel B: Effects on Alternate GSE					
	(1)	(2)	(3)	(4)	(5)
			Alternate GSE (1-4):		
	0 Week EL	1 Week EL	2 Week EL	4 Month EL	1 Year EL
GSE Treat (=1)	0.281***	0.144***	0.106**	0.089*	0.096*
	(0.066)	(0.054)	(0.049)	(0.048)	(0.055)
Strata FE	Yes	Yes	Yes	Yes	Yes
PDSLASSO X	Yes	Yes	Yes	Yes	Yes
Control Mean	2.955	2.927	3.037	3.149	3.036
N Women	648	868	855	795	674

Notes: This table presents effects of GSE treatment on two measures of GSE. Measures come from the questionnaire in appendix table A.1, which was adapted from the scale of Schwarzer and Jerusalem (1995). Responses to each of the 10 items were recorded on a 1-4 scale (with higher values reflecting greater GSE), and a "don't know" option was also included. GSE in panel A is the average of responses to questions that were answered, and is set to missing if more than three responses are "don't know." For the alternate GSE measure in panel B, any "don't know" responses are assigned the value of 1 prior to averaging across the items. 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. * p<0.10, ** p<0.05, *** p<0.01.

Table 4: Effects of GSE Treatment on Exertion of Effort

	(1)	(2)	(3)	(4)	(5)	(6)
	Chose Puzzle (=1):					
	4 Month EL		1 Year EL		Chose Drawing (=1)	
GSE Treat (=1)	0.056**	0.037	0.044	0.105**	0.003	0.023
	(0.027)	(0.045)	(0.032)	(0.045)	(0.029)	(0.042)
Prize High (=1)		0.038		0.126**		0.067*
		(0.050)		(0.050)		(0.039)
GSE Treat X Prize High (=1)		0.033		-0.115		-0.040
		(0.069)		(0.074)		(0.060)
P-Value: $\beta_1 + \beta_3 = 0$		0.110		0.853		0.693
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
PDSLASSO X	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.620	0.606	0.626	0.566	0.810	0.777
N Women	791	791	672	672	674	674

Notes: This table presents effects of GSE treatment on effort exertion. Effort exertion is measured using an incentivized task. Women chose between receiving a prize worth Rs.20 or attempting a timed puzzle, winning a prize worth Rs.30 or Rs.40 if successful (a randomization determined which was offered) and a prize worth Rs.10 if not. The task was done at both the four-month and one-year endlines. The puzzle on the four-month endline was a jigsaw puzzle, and the puzzle at one year was assembly of small blocks to build a toy house. The outcome in columns (1) and (2) is an indicator for attempting the puzzle at four months, and the outcome in columns (3) and (4) is an indicator for attempting the puzzle at one year. To investigate whether effects on puzzle choice are driven by changes in risk taking, the one-year endline asked women to make another decision. Options were identical to those in the puzzle decision except that instead of attempting a puzzle, women could try to draw a winning ball from a canvas bag. The outcome in columns (5) and (6) is an indicator for choosing to draw out a ball. The "prize high" covariate is an indicator for being offered Rs.40 (rather than Rs.30) if successful. 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. * p<0.10, ** p<0.05, *** p<0.01.

Table 5: Effects of GSE and Promotion Treatments on Employment

	(1)	(2)	(3)	(4)
		Working for Income (=1):		
	Signed Up (=1)	2 Week EL	4 Month EL	1 Year EL
GSE Treat & Promo Control (=1)	0.058*	0.030	0.087**	-0.017
	(0.034)	(0.039)	(0.039)	(0.054)
GSE Control & Promo Treat (=1)	0.110***	0.112***	0.098**	-0.012
	(0.037)	(0.037)	(0.041)	(0.052)
GSE Treat & Promo Treat (=1)	0.024	0.037	0.070*	-0.052
	(0.034)	(0.035)	(0.042)	(0.052)
P-Value: $\beta_1 = \beta_2$	0.185	0.043**	0.788	0.906
P-Value: $\beta_1 = \beta_3$	0.327	0.862	0.697	0.499
P-Value: $\beta_2 = \beta_3$	0.025**	0.041**	0.537	0.422
P-Value: $\beta_1 = 0$ & $\beta_3 = 0$	0.230	0.553	0.065*	0.594
P-Value: $\beta_2 = 0$ & $\beta_3 = 0$	0.010**	0.009***	0.045**	0.573
Strata FE	Yes	Yes	Yes	Yes
PDSLASSO X	Yes	Yes	Yes	Yes
Control Mean	0.194	0.218	0.232	0.372
N Women	1022	813	793	657

Notes: This table presents effects of the GSE and promotion treatments on women's employment. The outcome in column (1) is an indicator for attending the official sign-up day for the firm's program, held two weeks after the end of the GSE intervention. It comes from official sign-up records. The outcomes in columns (2)-(4) 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. 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 GSE and Promotion Treatments on Household Decision-Making about Sign-Up

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Woman Made Sign-Up Decision (=1):		Woman's Interest in Sign-Up (1-4):		Family's Interest in Sign-Up (1-4):		Extent Disagreement (1-4):	
	Woman's Report	Family's Report	Woman's Report	Family's Prediction	Family's Report	Woman's Prediction	Woman's Report	Family's Report
GSE Treat & Promo Control (=1)	0.002	-0.022	0.208*	0.284**	0.239*	0.454***	-0.393***	-0.254*
	(0.036)	(0.038)	(0.112)	(0.141)	(0.132)	(0.147)	(0.122)	(0.143)
GSE Control & Promo Treat (=1)	-0.010	-0.052	0.027	0.160	0.091	0.352**	-0.412***	-0.175
	(0.038)	(0.039)	(0.112)	(0.133)	(0.122)	(0.162)	(0.114)	(0.138)
GSE Treat & Promo Treat (=1)	-0.007	0.001	0.089	0.206	0.198	0.454***	-0.201*	-0.141
	(0.037)	(0.038)	(0.117)	(0.133)	(0.126)	(0.165)	(0.122)	(0.142)
P-Value: $\beta_1 = \beta_2$	0.749	0.480	0.107	0.406	0.282	0.506	0.877	0.563
P-Value: $\beta_1 = \beta_3$	0.808	0.560	0.302	0.599	0.771	1.000	0.151	0.410
P-Value: $\beta_2 = \beta_3$	0.936	0.212	0.592	0.746	0.417	0.554	0.092*	0.800
P-Value: $\beta_1 = 0$ & $\beta_3 = 0$	0.969	0.799	0.177	0.098*	0.128	0.003***	0.006***	0.207
P-Value: $\beta_2 = 0$ & $\beta_3 = 0$	0.967	0.354	0.738	0.253	0.290	0.014**	0.002***	0.418
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PDSLASSO X	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.834	0.837	2.825	2.457	2.580	2.363	2.713	2.609
N Women	789	687	846	623	762	600	672	550

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 equally 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 7: Effects of Job Offer on GSE and Effort Exertion

	(1)	(2)	(3)	(4)
		Alternate		
	GSE (1-4)	GSE (1-4)	Chose Puzzle (=1)	
Job Offer Treat (=1)	0.166**	0.160**	-0.054	-0.008
	(0.066)	(0.073)	(0.066)	(0.085)
Prize High (=1)				0.030
				(0.075)
Job Offer Treat X Prize High (=1)				-0.034
				(0.120)
P-Value: $\beta_1 + \beta_3 = 0$				0.663
Strata FE	Yes	Yes	Yes	Yes
PDSLASSO X	Yes	Yes	Yes	Yes
Control Mean	3.341	3.228	0.718	0.692
N Women	223	237	236	236

Notes: This table presents effects of the job offer on GSE and effort exertion. The outcome in column (1) is the GSE measure from panel A of table 3, and the outcome in column (2) is the GSE measure from panel B of table 3. The outcome in columns (3) and (4) is the measure of effort exertion from table 4. The outcomes in this table come from the four-month endline survey, which occurred three months after job offers were given. Strata are the strata used in the job offer and waitlist randomization. PDSLASSO X denote pre-job offer covariates (i.e. covariates from the baseline survey and from the last endline survey done before job offers were given) selected using the post-double-selection lasso method of Belloni, Chernozhukov and Hansen (2014). The sample is limited to women in the job offer experiment. Standard errors are clustered at the household level. * p<0.10, ** p<0.05, *** p<0.01.

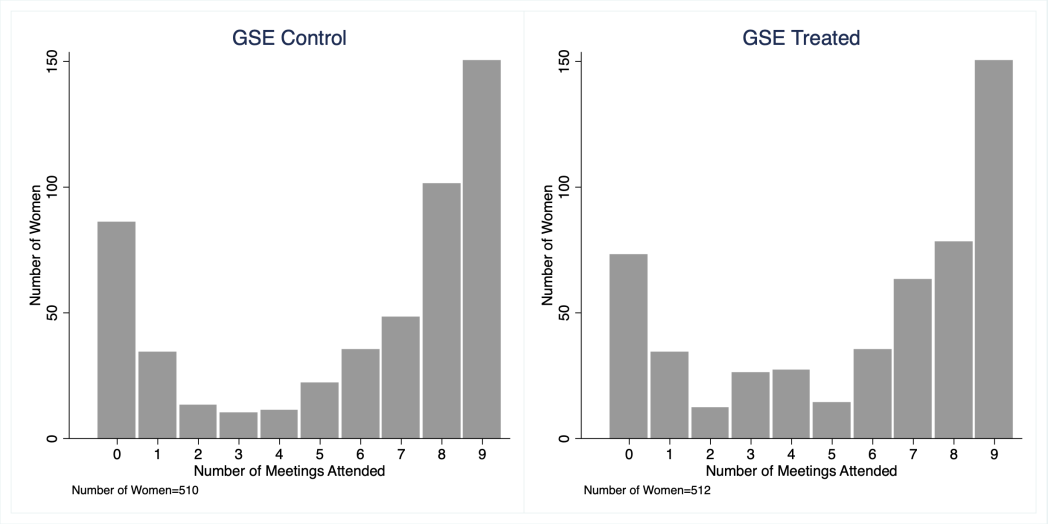
Table 8: Are Job Offer Effects due to the Job or to the Offer?

	(1)	(2)	(3)	(4)	(5)	(6)
	Ever Attended in First 2 Months (=1)		GSE (1-4)	Alternate GSE (1-4)	Chose Puzzle (=1)	
Job Offer Treat (=1)	0.127*					
	(0.075)					
Invited from Waitlist in First 2 Months (=1)		0.512***	0.105	0.077	0.045	0.044
		(0.080)	(0.079)	(0.091)	(0.091)	(0.138)
Prize High (=1)						0.014
						(0.122)
Invited from Waitlist in First 2 Months X Prize High (=1)						0.025
						(0.161)
P-Value: $\beta_1 + \beta_3 = 0$						0.566
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
PDSLASSO X	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.308	0.000	3.312	3.204	0.765	0.680
N Women	256	126	108	114	113	113

Notes: This table exploits random ordering of the waitlist to investigate whether effects of the job offer are due to the signal conveyed in the offer or to the employment the offer enabled. The outcome in columns (1) and (2) is an indicator for ever attending the partner firm's program in the first two months of training, which are the months of training that were completed 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 outcome in column (3) is the GSE measure from panel A of table 3, and the outcome in column (4) is the GSE measure from panel B of table 3. The outcome in columns (5) and (6) is the measure of effort exertion from table 4. The outcomes in columns (3)-(6) come from the four-month endline survey, which occurred three months after job offers were given. The "invited from waitlist" regressor is an indicator for waitlisted women being invited to participate in the program in the first two months of training (or first six weeks of training for the center that opened late). Strata are the strata used in the job offer and waitlist randomization. PDSLASSO X denote pre-job offer covariates (i.e. covariates from the baseline survey and from the last endline survey done before job offers were given) selected using the post-double-selection lasso method of Belloni, Chernozhukov and Hansen (2014). The sample is limited to women in the job offer experiment in column (1). In columns (2)-(6), the sample is limited 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

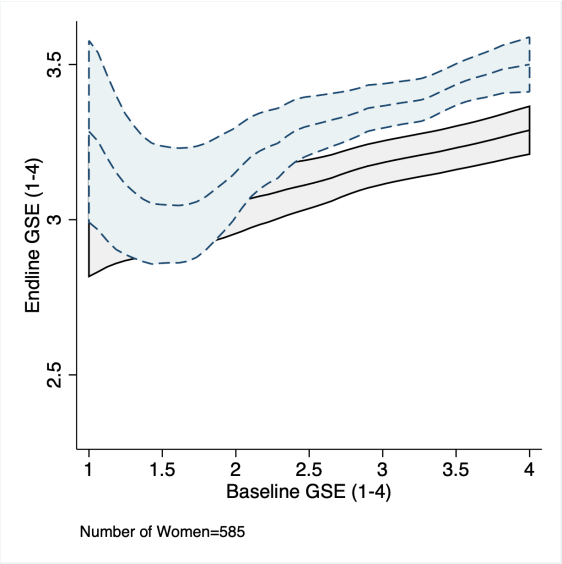
Figure A.1: Distribution of Meeting Attendance by GSE Treatment Status



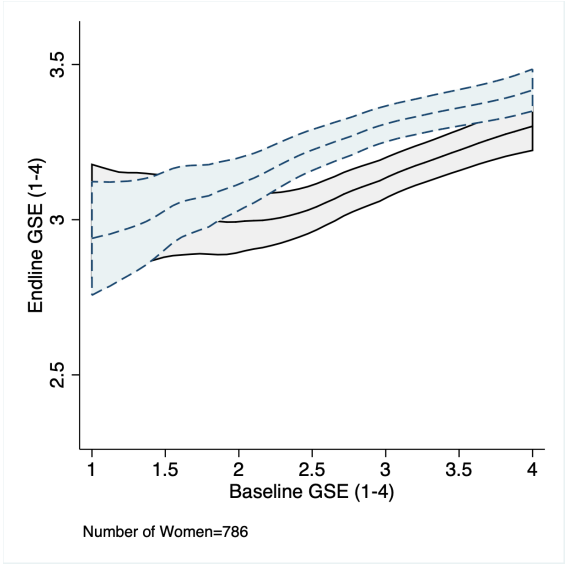
Notes: This figure presents the distribution of the number of meetings attended by GSE treatment status. Women could attend up to nine group meetings during the GSE intervention period. For GSE treatment women, each meeting was part of the GSE intervention, and for GSE control women, each meeting involved a group survey. Each bar in the figure represents the number of women of a given GSE treatment status who attended a given number of meetings.

Figure A.2: Endline GSE by Baseline GSE and GSE Treatment Status, using Additional Endlines

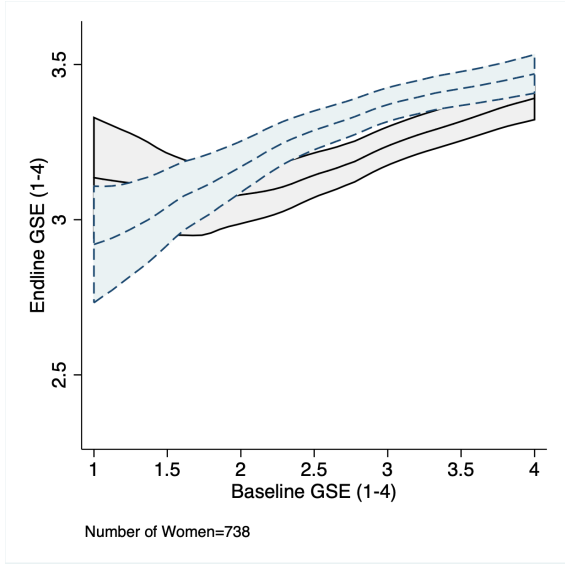
(a) Zero-Week Endline, GSE



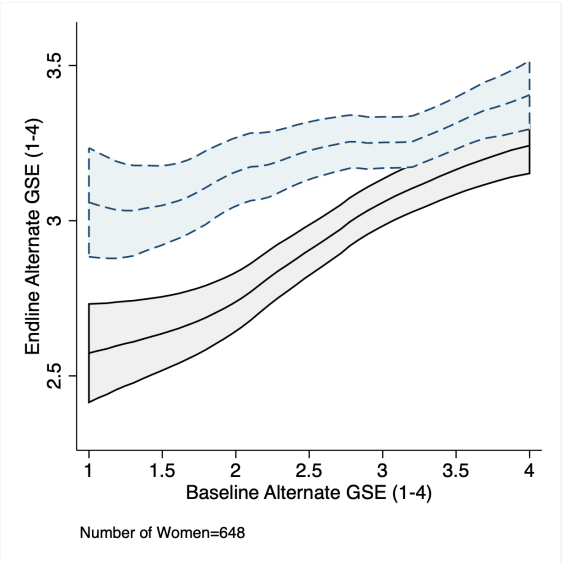
(b) Two-Week Endline, GSE



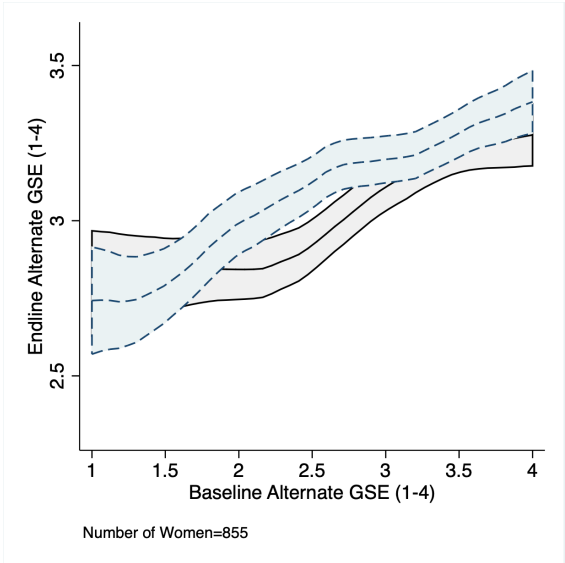
(c) Four-Month Endline, GSE



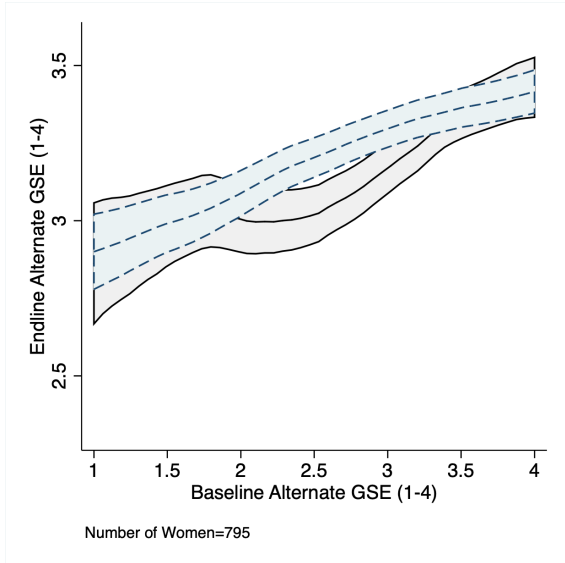
(d) Zero-Week Endline, Alternate GSE



(e) Two-Week Endline, Alternate GSE



(f) Four-Month Endline, Alternate GSE



Notes: Analyses in figure 2 use data from the one-week and one-year endlines; this figure presents the same analyses but uses data from the other three endlines. Zero-week endline data is in panels (a) and (d), two-week endline data in panels (b) and (e), and four-month endline data in panels (c) and (f). The baseline and endline GSE measures in panels (a), (b), and (c) of this figure correspond to those in panels (a) and (c) of figure 2, and the baseline and endline GSE measures in panels (d), (e), and (f) of this figure correspond to those in panels (b) and (d) of figure 2.

Table A.1: Adapted Schwarzer and Jerusalem (1995) Generalized Self-Efficacy Scale

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

Notes: This table presents the questionnaire used to measure GSE. It is the Schwarzer and Jerusalem (1995) scale with slight modifications to enhance comprehension of respondents in my setting. There were two key modifications: (i) all items were changed from statements that respondents assessed agreement with to questions, and (ii) for questions that tended to be difficult to understand, alternate phrasings and explanations were added and used when surveyors encountered issues with comprehension. Responses to each of the 10 items were recorded on a 1-4 scale (with higher values reflecting greater GSE), and a "don't know" option was also included.

Table A.2: GSE Treatment and Control Meeting Content

Meeting	GSE Treatment	GSE Control
#1	Introduction • Story: woman's GSE beliefs helped in building home • Discussion: introduction to GSE	Survey Topic: • Entertainment

#2	Talents
	• Discussion: talents
	• Activity: identify our own talents
	• Activity: we can do things we think we cannot (tablet sketchpad task)
		Survey Topic:
		• Identification cards
		  • Access to
		  • Use of

#3	Character Strengths • Discussion: character strengths • Activity: identify people in our lives with strengths • Activity: identify our own strengths	Survey Topics: • Cell phone use • Social interactions
#4	Talents, Strengths, and Success • Activity: identify times we felt proud or successful • Activity: identify talents and strengths that led to our successes	Survey Topics: • Daily schedule • Sanitation

#5	Goals
	• Activity: guided reflection
	• Story: woman pursued goal of learning to sew
	• Discussion: understanding goals
		Survey Topics:
		• Men's employment
		  • Daily work
		  • Migratory work
		  • Government schemes
		• Transportation facilities

#6	Goal Planning
	• Activity: strategy for goal planning
	  • Overview of three-step strategy
	  • Visualize three-steps in last story
	  • Apply strategy to our goals, using worksheet
		Survey Topics:
		• Availability of health services
		• Utilization of health services
		• Childbearing and fertility
		• Illness
		• Veterinary care

#7	Problem Solving • Story: woman overcame obstacles in agricultural work • Discussion: problem-solving mindset • Activity: anticipate obstacles to our goals and brainstorm solutions	Survey Topics: • Livestock • Land • Loans
#8	Putting It All Together • Story: girl set exam goal, made plan, and identified own abilities to use • Discussion: importance of recognizing abilities in pursuing goals • Activity: identify abilities to use to reach our goals	Survey Topics: • Schools • Voting
#9	Conclusion • Discussion: summarize curriculum	Survey Topics: • Drawn from multiple topics above

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 to increase GSE, while those assigned control took group surveys on aspects of daily life. There were nine meetings for both treatment and control meeting groups. Each group included three to eight participants, and meetings were led by surveyors assigned to the groups.

Table A.3: Attendance at GSE Intervention and Job Information Meetings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Woman Attended GSE Treatment or Control Meeting (=1):									Attended Job Info Meeting (=1):	
	# 1	# 2	# 3	# 4	# 5	# 6	# 7	# 8	# 9	Woman	Family
GSE Treat (=1)	0.035	0.040	0.008	-0.002	-0.026	-0.010	-0.029	-0.037	-0.017		
	(0.025)	(0.028)	(0.026)	(0.028)	(0.026)	(0.029)	(0.027)	(0.029)	(0.029)		
GSE Treat & Promo Control (=1)										0.040	0.010
										(0.029)	(0.037)
GSE Control & Promo Treat (=1)										-0.009	-0.027
										(0.033)	(0.036)
GSE Treat & Promo Treat (=1)										0.013	-0.029
										(0.029)	(0.036)
P-Value: $\beta_1 = \beta_2$										0.105	0.288
P-Value: $\beta_1 = \beta_3$										0.307	0.263
P-Value: $\beta_2 = \beta_3$										0.478	0.961
P-Value: $\beta_1 = 0$ & $\beta_3 = 0$										0.343	0.510
P-Value: $\beta_2 = 0$ & $\beta_3 = 0$										0.767	0.680
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.661	0.641	0.641	0.625	0.639	0.627	0.653	0.647	0.645	0.841	0.797
N Women	1022	1022	1022	1022	1022	1022	1022	1022	1022	1022	1015

Notes: This table presents levels of attendance at GSE intervention meetings and job information meetings, and tests for balance in attendance across treatment groups. The outcomes in columns (1)-(9) are indicators for women attending each of the nine group meetings during the GSE intervention period. For GSE treatment women, each meeting was part of the GSE intervention, and for GSE control women, each meeting involved a group survey. The meetings occurred before the promotion intervention so regressions in these columns do not include promotion treatment variables. The outcomes in columns (10) and (11) are indicators for women and women's family members attending meetings in which job information was given. All women who attended their job information meetings were given details about and a promotion for the partner firm's program. Family members of treated women who attended their meetings were given both details and promotion, while those of control women were given details only. Column (11) 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 columns (10) and (11). * p<0.10, ** p<0.05, *** p<0.01.

Table A.4: Beliefs about How Job Offers Were Assigned

How do you think the women who could begin the program immediately were chosen?	Percent
By chance/lottery	17.2%
Research team and/or firm chose women/households they liked best	11.8%
Research team and/or firm chose women with the most skill or experience	15.7%
Other	20.1%
Don't know	35.3%
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. Households were told a lottery allocated slots in the program but may not have understood what this meant. I assessed beliefs about how job offers were assigned on the one-year endline survey, which occurred eleven months after job offer results were delivered. The sample is restricted to women in the job offer experiment.

Table A.5: Endline Survey Attrition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Woman Surveyed at:							Family Surveyed at:	
	0 Week EL (=1)	1 Week EL (=1)	2 Week EL (=1)	4 Month EL (=1)		1 Year EL (=1)		2 Week EL (=1)	4 Month EL (=1)
GSE Treat (=1)	-0.019								
	(0.028)								
GSE Treat & Promo Control (=1)		0.036	0.051*	0.048		-0.002		0.030	0.031
		(0.029)	(0.030)	(0.035)		(0.038)		(0.039)	(0.036)
GSE Control & Promo Treat (=1)		-0.012	-0.016	0.010		-0.032		0.024	0.014
		(0.033)	(0.035)	(0.037)		(0.036)		(0.039)	(0.037)
GSE Treat & Promo Treat (=1)		0.009	0.020	0.008		-0.035		0.040	0.046
		(0.030)	(0.033)	(0.038)		(0.037)		(0.038)	(0.036)
Job Offer Treat (=1)					-0.004		0.104*		
					(0.034)		(0.059)		
P-Value: $\beta_1 = \beta_2$		0.112	0.052*	0.245		0.434		0.871	0.651
P-Value: $\beta_1 = \beta_3$		0.314	0.338	0.239		0.399		0.776	0.658
P-Value: $\beta_2 = \beta_3$		0.496	0.319	0.952		0.932		0.660	0.389
P-Value: $\beta_1 = 0$ & $\beta_3 = 0$		0.413	0.239	0.319		0.584		0.564	0.424
P-Value: $\beta_2 = 0$ & $\beta_3 = 0$		0.793	0.603	0.964		0.555		0.575	0.418
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.645	0.845	0.829	0.767	0.917	0.674	0.769	0.734	0.742
N Women	1022	1022	1022	1022	253	1022	253	1015	1015

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 zero weeks, one week, two weeks, four months, and one year after the GSE intervention ended. The zero 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, 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 members being surveyed at the two family member endlines, which occurred two weeks and four 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 $\times$ 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 A.6: Robustness of GSE Measures to Multiple Assessments

	(1) GSE (1-4)	(2) Alternate GSE (1-4)
Multiple Assessment Treat (=1)	-0.057	0.049
	(0.113)	(0.128)
Strata FE	Yes	Yes
Control Mean	3.260	3.022
N Women	155	168

Notes: This table investigates the robustness of the GSE measures to being assessed multiple times. Data come from surveys with women in a separate sample. Treated women in this sample were given the GSE questionnaire (in appendix table A.1) 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 assessment treatment on measures of GSE in the third week. Column (1) presents effects on the GSE measure used in panel A of table 3, and column (2) presents effects on the measure used in panel B of table 3. Strata are the strata used in the multiple assessment randomization. Standard errors are clustered by household. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.7: Effects of GSE and Promotion Treatments on Additional Measures of Any Employment and on Indices of All Measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2 Week EL		*4 Month EL*			*1 Year EL*		
	Work/Econ Main Use of Time (=1)	Employment Index	Work/Econ Main Use of Time (=1)	Did Paid Work Yesterday (=1)	Employment Index	Work/Econ Main Use of Time (=1)	Did Paid Work Yesterday (=1)	Employment Index
GSE Treat & Promo Control (=1)	0.084	0.139	0.051	0.040	0.174*	-0.093*	0.039	-0.044
	(0.053)	(0.103)	(0.045)	(0.034)	(0.090)	(0.055)	(0.037)	(0.115)
GSE Control & Promo Treat (=1)	0.033	0.169*	0.016	0.075**	0.197**	-0.049	0.033	0.005
	(0.044)	(0.086)	(0.044)	(0.035)	(0.092)	(0.053)	(0.032)	(0.111)
GSE Treat & Promo Treat (=1)	0.052	0.106	-0.005	0.009	0.071	-0.044	0.028	-0.045
	(0.049)	(0.095)	(0.044)	(0.034)	(0.086)	(0.051)	(0.037)	(0.110)
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: This table presents effects of the GSE and promotion treatments on additional measures of any employment. The preferred measure of any employment is the working for income variable used in columns (2)-(4) of table 5. Here I use additional measures of any employment as outcomes. 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 indictors 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. The two-week endline survey did not include the time-use module. 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 (2) of table 5 and the outcome in column (1) of this table. The four-month endline index includes the outcome in column (3) of table 5 as well as the outcomes in columns (3) and (4) of this table. The one-year endline index includes the outcome in column (4) of table 5 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.8: Effects of GSE and Promotion Treatments on Employment by Sector, 2 Week Endline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Working for Income:						
	Farming, HH Property (=1)	Farming, Non-HH Property (=1)	HH Micro-Enterprise (=1)	Casual Non-Farm Labor (=1)	Employed by a Firm (=1)	Anganwadi Work or Teaching (=1)	Other (=1)
GSE Treat & Promo Control (=1)	-0.013	0.018	0.006	0.031**	-0.014	-0.013	0.018
	(0.029)	(0.020)	(0.020)	(0.016)	(0.013)	(0.010)	(0.015)
GSE Control & Promo Treat (=1)	0.036	0.016	0.031*	0.006	-0.016	-0.010	0.028*
	(0.035)	(0.019)	(0.018)	(0.013)	(0.012)	(0.010)	(0.017)
GSE Treat & Promo Treat (=1)	0.044	-0.000	0.027	0.005	-0.010	-0.018*	0.010
	(0.032)	(0.020)	(0.018)	(0.012)	(0.014)	(0.009)	(0.013)
P-Value: $\beta_1 = \beta_2$	0.138	0.896	0.238	0.138	0.835	0.683	0.565
P-Value: $\beta_1 = \beta_3$	0.058*	0.408	0.340	0.107	0.723	0.446	0.573
P-Value: $\beta_2 = \beta_3$	0.822	0.458	0.804	0.946	0.588	0.205	0.258
P-Value: $\beta_1 = 0$ & $\beta_3 = 0$	0.153	0.610	0.341	0.134	0.554	0.144	0.458
P-Value: $\beta_2 = 0$ & $\beta_3 = 0$	0.358	0.663	0.153	0.894	0.394	0.088*	0.228
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PDSLASSO X	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 of the GSE and promotion treatments 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 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 micro-enterprise, casual non-farm labor, firm employment, anganwadi work or teaching, and other sectors. 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 (2) of table 5 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.9: Effects of GSE and Promotion Treatments on Employment by Sector, 4 Month Endline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Working for Income:						
	Farming, HH Property (=1)	Farming, Non-HH Property (=1)	HH Micro-Enterprise (=1)	Casual Non-Farm Labor (=1)	Employed by a Firm (=1)	Anganwadi Work or Teaching (=1)	Other (=1)
GSE Treat & Promo Control (=1)	-0.003	0.027**	0.004	0.012	0.033	0.006	0.025**
	(0.024)	(0.013)	(0.019)	(0.010)	(0.030)	(0.007)	(0.012)
GSE Control & Promo Treat (=1)	0.003	0.008	-0.005	-0.004	0.059*	0.000	0.008
	(0.027)	(0.013)	(0.017)	(0.006)	(0.031)	(0.007)	(0.013)
GSE Treat & Promo Treat (=1)	0.031	0.023	0.022	0.006	0.006	0.001	-0.002
	(0.028)	(0.017)	(0.019)	(0.008)	(0.032)	(0.004)	(0.010)
P-Value: $\beta_1 = \beta_2$	0.809	0.251	0.591	0.098*	0.419	0.515	0.298
P-Value: $\beta_1 = \beta_3$	0.218	0.856	0.344	0.552	0.420	0.447	0.053*
P-Value: $\beta_2 = \beta_3$	0.365	0.416	0.118	0.173	0.117	0.882	0.464
P-Value: $\beta_1 = 0$ & $\beta_3 = 0$	0.434	0.073*	0.479	0.441	0.529	0.673	0.105
P-Value: $\beta_2 = 0$ & $\beta_3 = 0$	0.525	0.352	0.281	0.372	0.126	0.948	0.752
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 of the GSE and promotion treatments 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 micro-enterprise, casual non-farm labor, firm employment, anganwadi work or teaching, and other sectors. 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 5 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.10: Effects of GSE and Promotion Treatments on Employment by Sector, 1 Year Endline

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

Notes: This table presents effects of the GSE and promotion treatments 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 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 micro-enterprise, casual non-farm labor, firm employment, anganwadi work or teaching, and other sectors. 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 (4) of table 5 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 of GSE and Promotion Treatments on Work in Rice Cultivation

	(1)	(2)	(3)
	Worked in Rice Cultivation (=1):		
	Any Work	On HH Land	Off HH Land
GSE Treat & Promo Control (=1)	0.081*	0.101**	0.023
	(0.047)	(0.047)	(0.041)
GSE Control & Promo Treat (=1)	-0.007	-0.005	0.013
	(0.048)	(0.040)	(0.041)
GSE Treat & Promo Treat (=1)	0.039	0.096**	-0.036
	(0.051)	(0.046)	(0.042)
P-Value: $\beta_1 = \beta_2$	0.057*	0.025**	0.816
P-Value: $\beta_1 = \beta_3$	0.391	0.934	0.155
P-Value: $\beta_2 = \beta_3$	0.353	0.026**	0.246
P-Value: $\beta_1 = 0$ & $\beta_3 = 0$	0.232	0.045**	0.360
P-Value: $\beta_2 = 0$ & $\beta_3 = 0$	0.620	0.054*	0.489
Strata FE	Yes	Yes	Yes
PDSLASSO X	Yes	Yes	Yes
Control Mean	0.483	0.247	0.236
N Women	674	674	674

Notes: This table presents effects of the GSE and promotion treatments on women's work in a rice sowing season that occurred 10 months after the GSE intervention. The outcome in column (1) is an indicator for doing any work for income in this cultivation, while outcomes in columns (2) and (3) are indicators for working in this cultivation on and off own household's land. Outcomes come 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 A.12: Effects of GSE and Promotion Treatments on GSE

Panel A: Effects on GSE	(1)	(2)	(3)	(4)	(5)
			GSE (1-4):		
	0 Week EL	1 Week EL	2 Week EL	4 Month EL	1 Year EL
GSE Treat (=1)	0.178***				
	(0.057)				
GSE Treat & Promo Control (=1)		0.139*	0.192***	0.148**	0.082
		(0.074)	(0.071)	(0.068)	(0.074)
GSE Control & Promo Treat (=1)		-0.041	0.047	-0.007	-0.082
		(0.074)	(0.067)	(0.068)	(0.084)
GSE Treat & Promo Treat (=1)		0.166**	0.143**	0.063	0.098
		(0.070)	(0.068)	(0.072)	(0.080)
P-Value: $\beta_1 = \beta_2$		0.016**	0.025**	0.014**	0.036**
P-Value: $\beta_1 = \beta_3$		0.692	0.443	0.180	0.826
P-Value: $\beta_2 = \beta_3$		0.002***	0.115	0.292	0.030**
P-Value: $\beta_1 = 0$ & $\beta_3 = 0$		0.047**	0.022**	0.085*	0.409
P-Value: $\beta_2 = 0$ & $\beta_3 = 0$		0.005***	0.086*	0.530	0.093*
Strata FE	Yes	Yes	Yes	Yes	Yes
PDSLASSO X	Yes	Yes	Yes	Yes	Yes
Control Mean	3.185	3.114	3.137	3.252	3.149
N Women	592	782	798	750	635

Panel B: Effects on Alternate GSE	(1)	(2)	(3)	(4)	(5)
			Alternate GSE (1-4):		
	0 Week EL	1 Week EL	2 Week EL	4 Month EL	1 Year EL
GSE Treat (=1)	0.281***				
	(0.066)				
GSE Treat & Promo Control (=1)		0.103	0.159**	0.109	0.090
		(0.080)	(0.076)	(0.071)	(0.077)
GSE Control & Promo Treat (=1)		-0.050	0.059	0.008	-0.030
		(0.077)	(0.071)	(0.071)	(0.081)
GSE Treat & Promo Treat (=1)		0.169**	0.101	0.076	0.083
		(0.076)	(0.071)	(0.073)	(0.077)
P-Value: $\beta_1 = \beta_2$		0.050*	0.153	0.136	0.148
P-Value: $\beta_1 = \beta_3$		0.385	0.407	0.626	0.934
P-Value: $\beta_2 = \beta_3$		0.003***	0.513	0.330	0.161
P-Value: $\beta_1 = 0$ & $\beta_3 = 0$		0.085*	0.107	0.307	0.430
P-Value: $\beta_2 = 0$ & $\beta_3 = 0$		0.008***	0.359	0.512	0.336
Strata FE	Yes	Yes	Yes	Yes	Yes
PDSLASSO X	Yes	Yes	Yes	Yes	Yes
Control Mean	2.955	2.950	3.018	3.138	3.048
N Women	648	868	855	795	674

Notes: This table presents effects of the GSE and promotion treatments on the two measures of GSE. The outcomes in panel A are the outcomes in panel A of table 3, and the outcomes in panel B are the outcomes in panel B of table 3. The zero-week endline occurred before the promotion intervention so the regression in column (1) does not include promotion treatment variables. 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)-(5). * p<0.10, ** p<0.05, *** p<0.01.

Table A.13: GSE of Women who Signed Up by Treatment Group

	(1) GSE (1-4)	(2) Alternate GSE (1-4)
GSE Treat & Promo Control (=1)	0.208	0.128
	(0.166)	(0.173)
GSE Control & Promo Treat (=1)	-0.166	-0.223
	(0.178)	(0.189)
GSE Treat & Promo Treat (=1)	0.140	0.141
	(0.166)	(0.174)
P-Value: $\beta\_1 = \beta\_2$	0.009***	0.025**
P-Value: $\beta\_1 = \beta\_3$	0.595	0.923
P-Value: $\beta\_2 = \beta\_3$	0.033**	0.022**
P-Value: $\beta\_1 = 0$ & $\beta\_3 = 0$	0.457	0.699
P-Value: $\beta\_2 = 0$ & $\beta\_3 = 0$	0.101	0.071*
Control Mean	3.163	3.064
N Women	226	243

Notes: This table compares the GSE of women who signed up for the firm's program across treatment groups. The outcome in column (1) is the GSE measure from panel A of table 3, and the outcome in column (2) is the GSE measure from panel B of table 3. The outcomes in this table come from the one-week endline survey, which occurred in the week before the official sign-up day. The sample is limited to women who attended the official sign-up day. 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.14: Performance in Partner Firm Program by Treatment Group

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

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.15: Effects of GSE and Promotion Treatments on Happiness

	(1)	(2)	(3)	(4)	(5)
	Happiness (1-4):				
	0 Week EL	1 Week EL	2 Week EL	4 Month EL	1 Year EL
GSE Treat (=1)	0.273***				
	(0.058)				
GSE Treat & Promo Control (=1)		0.129	0.043	-0.006	-0.097
		(0.079)	(0.061)	(0.068)	(0.071)
GSE Control & Promo Treat (=1)		0.080	0.048	0.008	-0.192***
		(0.073)	(0.065)	(0.070)	(0.070)
GSE Treat & Promo Treat (=1)		0.134*	0.190***	0.102	-0.062
		(0.072)	(0.058)	(0.070)	(0.072)
P-Value: $\beta_1 = \beta_2$		0.525	0.945	0.858	0.249
P-Value: $\beta_1 = \beta_3$		0.943	0.021**	0.128	0.670
P-Value: $\beta_2 = \beta_3$		0.448	0.034**	0.215	0.114
P-Value: $\beta_1 = 0$ & $\beta_3 = 0$		0.135	0.004***	0.235	0.356
P-Value: $\beta_2 = 0$ & $\beta_3 = 0$		0.178	0.004***	0.301	0.025**
Strata FE	Yes	Yes	Yes	Yes	Yes
PDSLASSO X	Yes	Yes	Yes	Yes	Yes
Control Mean	3.142	3.175	3.319	3.368	3.485
N Women	636	849	849	788	661

Notes: This table presents effects of the GSE and promotion treatments on women's overall happiness in life. Happiness is self-reported and recorded on a 1-4 scale (with higher values reflecting greater happiness). Each column presents effects at one of the five endlines. The zero-week endline occurred before the promotion intervention so the regression in column (1) does not include promotion treatment variables. 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)-(5). * p<0.10, ** p<0.05, *** p<0.01.

Theoretical Appendix

Derivation of equation (1).

By definition,

$$\mathbb{E}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j}>\underline{Y}_j) = \int_0^{\underline{Y}_j}\theta_{i,j}\hat{P}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j}>\underline{Y}_j)d\theta_{i,j}$$

$\hat{P}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j}>\underline{Y}_j)$ is given by Bayes' rule. Note that because $\varepsilon_{i,j}\sim U[0,1]$, the probability that $\theta_{i,j}+\varepsilon_{i,j}>\underline{Y}_j$ for a particular $\theta_{i,j}$ value is $1-\underline{Y}_j+\theta_{i,j}$. Applying Bayes' rule,

$$\begin{aligned}\hat{P}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j}>\underline{Y}_j) &= \frac{f_j(\theta_{i,j})[1-\underline{Y}_j+\theta_{i,j}]}{\int_0^{\underline{Y}_j}f_j(\tilde{\theta}_{i,j})[1-\underline{Y}_j+\tilde{\theta}_{i,j}]d\tilde{\theta}_{i,j}}\\ &= \frac{f_j(\theta_{i,j})[1-\underline{Y}_j+\theta_{i,j}]}{1-\underline{Y}_j+\bar{\theta}_{i,j}}\end{aligned}$$

Combining this with the definition of $\mathbb{E}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j}>\underline{Y}_j)$ gives

$$\begin{aligned}\mathbb{E}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j}>\underline{Y}_j) &= \int_0^{\underline{Y}_j}\theta_{i,j}\frac{f_j(\theta_{i,j})[1-\underline{Y}_j+\theta_{i,j}]}{1-\underline{Y}_j+\bar{\theta}_{i,j}}d\theta_{i,j}\\ &= \frac{\int_0^{\underline{Y}_j}\theta_{i,j}f_j(\theta_{i,j})d\theta_{i,j}-\underline{Y}_j\int_0^{\underline{Y}_j}\theta_{i,j}f_j(\theta_{i,j})d\theta_{i,j}+\int_0^{\underline{Y}_j}\theta_{i,j}^2f_j(\theta_{i,j})d\theta_{i,j}}{1-\underline{Y}_j+\bar{\theta}_{i,j}}\\ &= \frac{\bar{\theta}_{i,j}-\underline{Y}_j\bar{\theta}_{i,j}+\sigma_j^2+\bar{\theta}_{i,j}^2}{1-\underline{Y}_j+\bar{\theta}_{i,j}}\end{aligned}$$

Derivation of equation (2).

Similar logic gives $\mathbb{E}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j}<\underline{Y}_j)$. By definition,

$$\mathbb{E}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j}<\underline{Y}_j) = \int_0^{\underline{Y}_j}\theta_{i,j}\hat{P}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j}<\underline{Y}_j)d\theta_{i,j}$$

$\hat{P}_{i,j}(\theta_{i,j}|\theta_{i,j}+\varepsilon_{i,j}<\underline{Y}_j)$ is again given by Bayes' rule. The assumption that $\varepsilon_{i,j}\sim U[0,1]$ means

the probability that $\theta_{i,j} + \varepsilon_{i,j} < \underline{Y}_j$ for a particular $\theta_{i,j}$ value is $\underline{Y}_j - \theta_{i,j}$. Bayes' rule then gives,

$$\begin{aligned}
\hat{P}_{i,j}(\theta_{i,j}|\theta_{i,j} + \varepsilon_{i,j} < \underline{Y}_j) &= \frac{f_j(\theta_{i,j})[\underline{Y}_j - \theta_{i,j}]}{\int_0^{\underline{Y}_j} f_j(\tilde{\theta}_{i,j})[\underline{Y}_j - \tilde{\theta}_{i,j}]d\tilde{\theta}_{i,j}} \\
&= \frac{f_j(\theta_{i,j})[\underline{Y}_j - \theta_{i,j}]}{\underline{Y}_j - \bar{\theta}_{i,j}}
\end{aligned}$$

Combined with the definition of $\mathbb{E}_{i,j}(\theta_{i,j}|\theta_{i,j} + \varepsilon_{i,j} < \underline{Y}_j)$, this gives

$$\begin{aligned}
\mathbb{E}_{i,j}(\theta_{i,j}|\theta_{i,j} + \varepsilon_{i,j} < \underline{Y}_j) &= \int_0^{\underline{Y}_j} \theta_{i,j} \frac{f_j(\theta_{i,j})[\underline{Y}_j - \theta_{i,j}]}{\underline{Y}_j - \bar{\theta}_{i,j}} d\theta_{i,j} \\
&= \frac{\underline{Y}_j \int_0^{\underline{Y}_j} \theta_{i,j} f_j(\theta_{i,j}) d\theta_{i,j} - \int_0^{\underline{Y}_j} \theta_{i,j}^2 f_j(\theta_{i,j}) d\theta_{i,j}}{\underline{Y}_j - \bar{\theta}_{i,j}} \\
&= \frac{\underline{Y}_j \bar{\theta}_{i,j} - \sigma_j^2 - \bar{\theta}_{i,j}^2}{\underline{Y}_j - \bar{\theta}_{i,j}}
\end{aligned}$$