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Measuring the Effect of a Community-level Program on Women's Empowerment Outcomes

Evidence from India

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Abstract

This paper uses primary data from rural north India to show that participation in a community-level female empowerment program significantly increases access to employment, physical mobility, and political participation. The program provides support groups, literacy camps, adult education classes, and vocational training for rural women in several states of India; the data are from Uttarakhand. The paper uses instrumental variables and truncation-corrected matching on primary data to disentangle the program's mechanisms, separately considering its effect on women who work, and those who do not work but whose reservation wage is increased by participation. The analysis also finds significant spillover effects on non-participants relative to women in untreated districts. It finds consistent estimates for average treatment and intent to treat effects.

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1 Motivation

Empowerment allows individuals to reach their full potential, to improve their political and social participation, and to believe in their own capabilities. Gender empowerment also has important ramifications for the rest of the household; empowered women have fewer children and higher child survival rates (Rosenzweig and Schultz, 1982; Dyson and Moore, 1983), healthier and better-fed children (Lundberg, Pollak and Wales, 1997; Kanbur and Haddad, 1994), and a generally greater allocation of resources to children (Thomas, 1990; Handa, 1996). Development programs aim to empower women by increasing their control over contraceptive choices, by providing them access to credit, and through education.

Women's empowerment is particularly hard to achieve within a generation because it is driven not only by information about choices, but also by the acceptability of these choices, and what Kabeer (1994) calls the "power within", i.e. a woman's intrinsic belief in her ability to control resources and to make decisions. Communities are often governed by strict social norms, which can both be driven by and drive the choices traditionally made by women in the village. If the social stigma associated with working outside the home or using contraceptives is prohibitive, then mere access to education or birth control may not change empowerment outcomes. Instead, giving a woman access to others who have made different choices can expand her information set and demonstrate the outcomes associated with these choices. As an alternative to targeting individual women, empowerment for women may be affected by combining learning and influence through community action and peer networks.

In this paper, we use primary data from rural north India to examine the impact of a program called *Mahila Samakhya* on female empowerment outcomes. *Mahila Samakhya* aims to empower women by educating them. The program provides literacy camps, adult education classes, and vocational training. The program also creates support groups on issues of social importance, such as domestic violence and alcoholism. These support groups strengthen networks, which plays an important role in determining empowerment. We measure empowerment using (1) the ownership of identification cards for the national government's rural employment guarantee scheme, which proxies for access to outside employment, (2) the ability to leave the household without permission, which reflects physical mobility, and (3) participation in weekly village council meetings, which measures political participation. The literature identifies access to outside employment, physical mobility, and political participation as three important components of gender empowerment. These variables represent a wide variety of domains in which a program like *Mahila Samakhya* can empower women: economically, domestically, and socially.

Mahila Samakhya is an innovative approach to improving female empowerment. While a number of programs aim to improve female empowerment through education, *Mahila Samakhya* combines education with support groups, and has the explicit objective of increasing gender empowerment. We posit that this program affects female bargaining power in two ways. First, education provided by the program directly improves job prospects and increases the reservation wage. By increasing a woman's earning potential, the program helps improve her bargaining position, allowing her to control a greater share of the household's resources and to become a more active participant in her community. Further, the program may have an indirect effect through improved information flows that may change social norms. These social spillovers also empower participants who do not have access to outside employment and thus do not benefit from the direct employment aspect of *Mahila Samakhya* (Montgomery and Casterline, 1996). As a result, even unemployed participants and non-participants may be empowered by *Mahila Samakhya*.

In establishing whether *Mahila Samakhya* has a significant impact on female empowerment, we account for two potential sources of endogeneity: (1) the program's choice of communities in which to operate, and (2) the individual's choice of whether to participate. We conduct this analysis in five stages. First, we estimate an intent-to-treat (ITT) effect of the program by comparing the bargaining power outcomes of treated and untreated women. Second, we consider the potential of differences among women in communities that receive the program and those in communities without. We match non-participants in treated districts ("non-participants") and women in untreated districts ("the untreated"), to examine whether they are significantly different from each other. We also compare census data on indicators of female bargaining power in the blocks where the program was placed to the blocks where it was not placed from before the *Mahila Samakhya* intervention to find no significant differences between treated and untreated blocks. All of our results suggest that the program was not targeted in its placement.

Third, we test whether program participants are significantly more empowered than similar women from untreated districts to determine whether the program has a significant treatment effect. Using instrumental variables and matching, we compare pre-determined levels of empowerment and empowerment outcomes (similar to a differences-in-differences approach) to find that even after accounting for the pre-determined level of empowerment, the program significantly improves access to employment, physical mobility, and political participation. Our instrument relies on the roll-out of the program to control for selection in the participation decision. Using both approaches, we find a positive, significant treatment effect of the program on women's empowerment outcomes; participants are more likely to have access to outside employment, are able to leave the house without permission and are more likely to attend village council meetings, although this last effect is not significant in all specifications. The marginal effects from the IV approach emphasize the potential for large numbers of women to benefit from interventions like *Mahila Samakhya*. These results are qualitatively similar to the ITT effects estimated, and suggest that our instrument adequately controls for sample selection.

Fourth, to explore the program's mechanism, we focus on participants who do not work, comparing them to untreated women who also do not work. Using both instrumental variables and matching, we find that even participants who do not benefit from the enhanced employability from participation are significantly more likely to leave the house without permission. We also consider those women who participate in MS but do not have access to outside work. Results suggest that participants who do not have access to outside employment are more likely than non-participants without access to outside employment to leave the house without permission and to participate in the village council. Thus, whether through increasing her reservation wage or by changing social norms, MS appears to improve outcomes even for those women not able to benefit directly from the intended increase in own income.

Fifth, we we attempt to isolate the influence effect of MS by comparing non-participants to untreated women to test for the presence of spillover effects in treated areas. We are interested in knowing whether MS can affect non-participants in a village by changing social norms. We find that non-participants in treated villages have greater access to outside employment, greater physical mobility and higher attendance of village council meetings than untreated women, which point to the positive spillover effects of *Mahila Samakhya*.

Most studies of program impact analyze interventions targeted at the individual. Only a small number of papers examine community-level interventions because these programs often aim to change outcomes that are difficult to measure and use methods that combine direct individual intervention (education) with the process of the intervention (community meetings). Thus, evaluating

community-level programs often poses the dual difficulty of imprecisely-measured outcomes and a "treatment" that is hard to identify. In this paper, we use changes in outcomes and an instrument suggested by detailed field tests to disentangle the mechanisms of the program and identify the effect of the program on female empowerment.

Few other papers study *Mahila Samakhya*; one of these papers focuses on the program's effect on building village-level social capital and trust rather than studying its intended impact on female empowerment (Janssens, 2010). In other work, Kandpal and Baylis (2013) explore whether *Mahila Samakhya* affects the diversity of participants' peer group, while Kandpal and Baylis (2011) study whether peers' participation improves female bargaining power and child welfare outcomes; neither of these studies explicitly estimates treatment effects of the program's intended impact, which is to empower women through participation.

This paper contributes to the literature in several ways. It is the first to evaluate the impact of *Mahila Samakhya* on empowerment outcomes and provides robust estimates of the various effects of this program. We decompose the effect of community-level interventions like *Mahila Samakhya* into its three components: (1) a direct effect that works through employment opportunities outside the family farm, (2) a direct effect that works through higher reservation wages, and (3) an indirect effect that works through the channels of social influence and learning. As a result, this paper may provide valuable evidence on the effectiveness of community-level interventions in changing ingrained social outcomes like women's bargaining power. Our results establish that the *Mahila Samakhya* program directly and indirectly increases female empowerment.

2 Background on Uttarakhand and *Mahila Samakhya*

Uttarakhand is a a small rural state in the Indian Himalayas, comprising less than one percent of the Indian population. Only five cities in the state contain more than 100,000 people. On the surface, Uttarakhandi women may appear to be more empowered than the average. These women led the *Chipko* movement to prevent deforestation[1] as well as the demand for a separate state. However, while the state has a literacy rate of 72 percent, the Census reports that only 60 percent of all women are literate.[2] A more detailed measure of literacy from a nationally representative household survey finds 43 percent of Uttarakhandi women cannot read at all, while an additional 5 percent can only read parts of a sentence (International Institute for Population Studies and Macro International, 2007). Therefore, the effective literacy rate for females may be closer to 50 percent.

Although 43 percent of all Uttarakhandi women work, nearly two-thirds of these women (64 percent) are not paid for their work, and over 70 percent work in agriculture. These women are likely to work on their family's farmland, which does little to empower them. In addition, 23 percent of Uttarakhandi women have no say over household financial decisions, and almost 43 percent do not have the final say on their own healthcare. Over half (55 percent) did not have the final say on large purchases made by their household (International Institute for Population Studies and Macro International, 2007). Hence, Uttarakhandi women can have little say in the household or community.

In 1988, *Mahila Samakhya* was launched in three states of India to empower women through formal, informal, and vocational education. In theory, the community-level program was placed

[1]The Hindi word *Chipko* means "to stick". In the *Chipko* movement of the seventies, Uttarakhandi villagers, and women in particular, literally hugged trees to prevent deforestation.

[2]The national literacy rate is 65 percent, and female literacy of 54 percent. The most literate state is Kerala, with a 91 percent overall literacy rate and 88 percent female literacy rate.

in districts identified by (1) low rates of female education, (2) low school attendance by girls, (3) remoteness, and (4) lack of development and restricted access to infrastructure. In practice, as our results will highlight, the program does not appear to be targeted in any way, and the selection of districts into the program appears random. Participation in the program is voluntary, and no monetary incentives are offered.[3] The program entered Uttarakhand in 1995 and covers 2,416 villages in six of 13 Uttarakhandi districts. More than 42,000 women participate in this program, and over 2,500 girls have been educated in its centers.

The DFID-funded *Mahila Samakhya* conducts biweekly literacy camps and provides continuing education to women and girls. The camps and continuing education classes are provided to a cluster of three to five villages, depending on the size of the class and the proximity of the villages. The program also offers weekly vocational training to enable participants to earn an income. Such training is intended to improve the participant's employability, giving her access to job opportunities off the family farm, and hence improving her level of empowerment in the household and the community. Indeed, in pre-tests, we frequently observed that participants used this training to become midwives, herbal medicine manufacturers, bakers, grocers, candle makers, and tailors. In addition, the program provides special education on resolving domestic disputes and conflicts within the community. All classes are capped at 25 women, with an average size of 17 women per class or camp.

The program also encourages women to participate in village politics as a means of self-empowerment. In field tests, we observed participants hearing about the success women have had in the labor force and the important roles women can play in Indian society. They were also told about the benefits of having a daughter and of not discriminating against her. Groups of participants that meet on a weekly basis provide support on issues like domestic violence, alcoholism, dowry, and female infanticide. These groups vary in size from five to 15 women, and foster ties between participants. These interventions have the potential to generate significant spillover effects wherein even participants who cannot work outside the home or family farm, and therefore do not benefit directly from improved employability, can be empowered by their participation.

Mahila Samakhya enters a village through program workers called *sahayoginis*. The worker first conducts several rounds of talks with local women to determine what their needs are, and what they would like from the program. This process can take up to several weeks, but as a result, the program's activities are tailored to each village. The program often starts with literacy or education camps because these are the most frequently-voiced concerns. Initially, only a few women may participate, but as others see the benefits of participation, they muster up the courage to participate despite family opposition.

The program can meet with resistance from the men in the village, who may see the program as subversive and be unwilling to let their wives participate. In such cases, workers stress the educational rather than empowerment component of the the intervention. Once the men observe the benefits of participation, generally in the form of earnings, they reduce their opposition. Sometimes, as the women become more mobile, men might again oppose participation, but usually the women are sufficiently empowered at this point that the opposition no longer restricts their involvement.

[3]When participants travel to district-meetings, they are housed and fed at the program headquarters, and their travel expenses are reimbursed.

3 Literature Review

Kabeer (1999, p. 1) defines gender empowerment as "the process by which those who have been denied the ability to make strategic life choices acquire such an ability." She then decomposes empowerment into (1) resources, representing access, both present and future, to human and social resources; (2) agency, including decision-making and negotiating, where the latter can include deception or manipulation; and (3) achievements or outcomes. We posit that by providing women the access to outside employment, the *Mahila Samakhya* program improves the availability of human resources to women. In addition, by expanding and diversifying their social networks, as well as expanding their information sets, the program improves access to social resources. The program improves agency by informing women about the opportunities available to them and by teaching them how to negotiate, both inside and outside the household, and better equipping them to avail of the opportunities for outside employment made available to them by the program. Through this combined impact on resources and agency, the program improves women's achievements or outcomes.

A large body of literature considers the determinants of and proxies for gender empowerment. Gender empowerment is measured by a woman's ability to make household decisions, relative to her husband's ability to make household decisions. Since this ability cannot be explicitly measured, economists study whether variables such as education, contraceptive use, and asset-ownership are correlated with high female empowerment. These self-reported variables reflect the wide variety of choices and decisions at stake in the household bargain: employment, fertility, and resource allocation. Empowerment itself has been measured by a woman's relative physical mobility, economic security, decision-making ability, freedom from domestic violence, and political awareness and participation.

Another section of the literature finds that a woman's access to employment outside the house increases her household bargaining power (for a study in Bangladesh, see Anderson and Eswaran, 2009; for a study in India, see Rahman and Rao, 2004). The ownership of assets, in particular, is one important way through which access to employment helps empower women in developing countries (for example, see Agarwal, 2001, for evidence from India). In addition, several analyses have found that access to credit programs— micro-finance organizations or rotating savings and credit associations (ROSCA)— has a positive effect on female empowerment (for a study in Kenya, see Anderson and Baland, 2002; for a study in Bangladesh, see Hashemi, Schuler and Riley, 1996).

Past literature has highlighted the importance of the source of income in terms of its effect on female bargaining power. For instance, Lilja et al. (1996) distinguish between income from private plots versus communal household plots in terms of their effect on female intrahousehold bargaining power. The authors suggest that tradeoffs may exist between the amount earned from private plots and the amount of wages received from the communal plot. Further, Jones (1983) finds that if a woman is not paid or paid too little for her work, as is typical for work done on the household field, she may see no benefits to such work for the household bargain, and may even refuse to work on the communal field the following year. Hence, as Lilja et al. (1996) point out, if household members can influence their per unit labor payment on the communal field, then women's implicit wages would increase as their labor contribution rises.

Studies have also found a positive link between empowerment and contraceptive use (for a study in Bangladesh, see Schuler and Hashemi, 1994), as well as between the woman's influence on resource allocation and her family's social status (for a study in Bangladesh, see Quisumbing and de la Brière, 2000). In particular, the more educated she and her father are relative to her

husband, the more empowered she is. Hashemi, Schuler and Riley, 1996 find that physical mobility improves the degree of control over microcredit loans, since it reflects a woman's access to outside employment opportunities. A study of the determinants of female autonomy in India finds that a better-educated woman has greater bargaining power, as measured by physical mobility and say in household resource allocation, through the channel of increased information (Rahman and Rao, 2004). The same study also finds culture, as measured by state fixed-effects, to significantly increase bargaining power despite controlling for religion and caste. Further evidence from India shows strong positive correlations between female education as a proxy for bargaining power, and freedom of movement and better maternal health as bargaining outcomes (see Malhotra, Pande and Grown, 2003 for a review of this literature).

The literature further agrees that the clearing of marriage markets depends on the number of men and women in the market (for theoretical models, see Becker, 1973*a*,*b*; Neelakantan and Tertilt, 2008). As a result, the local sex ratio works through the spousal age ratio to influence marriage markets and therefore household bargaining power. Scholars have found that, particularly in the Indian context, women have less bargaining power if their husbands are significantly older (for evidence from India, see Caldwell, Reddy and Caldwell, 1983; Kantor, 2003).

Since empowerment is an unobservable latent variable, economists use its observable characteristics as proxies for empowerment. Women with a more equal spousal age ratio, greater access to outside employment or a high level of political participation are also likely to have greater bargaining power. Thus, the indicators of a high level of empowerment include (1) access to outside employment, (2) physical mobility, and (3) political participation (Anderson and Eswaran, 2009; Hashemi, Schuler and Riley, 1996; Rahman and Rao, 2004). The corresponding dependent variables we use to reflect high levels of female autonomy are (1) the ownership of identification cards for the national government's rural employment guarantee scheme, which proxies for access to outside employment, (2) the ability to leave the household without permission, which reflects physical mobility, and (3) participation in weekly village council meetings, which measures political participation. We choose these variables because they represent a diverse set of ways in which the *Mahila Samakhya* program can potentially empower women.

Employment and access to credit do not always improve empowerment. As Faulkner and Lawson (1991) demonstrate, if women's employment is concentrated in inferior positions, then access to employment alone may in fact make them worse off than before. However, as discussed above, the *Mahila Samakhya* not only provides access to employment, it also increases agency and access to social resources. The study by Rowlands (1997) of two similarly-intentioned Honduran development programs with very different implementation and outcomes provides some clues as to what program characteristics might lead to true empowerment. The first was a health promotion program, which trained local women in preventive health care and basic treatment, with the goal of having these women provide these services for payment in the future. This program had about a 50 percent dropout rate, and outcomes depended heavily on the individual health promoter. Only some of the women who successively completed the training program reported feeling individually empowered, although they all now had access to outside income. The second program grew out of the informal support group-like meetings of a group of local women, in which they discussed their experiences and problems. The expanded, formalized program worked in much the same way that *Mahila Samakhya* does, through an iterative, grassroots implementation process. Almost all participants reported increased levels of self-confidence and self-esteem as a result of participation, and Rowlands (1997) reports that these changes were very noticeable.

With the exception of analyses of credit extension mechanisms, the studies discussed above focus

on interventions targeted at the individual. Only a small number of papers look at community-level interventions. For instance, Imai and Eklund (2008) use survey data on a women's community-based organization in rural Papua New Guinea to assess the effectiveness of autonomous women's groups compared to those that receive external support. Their analysis— using a Heckman Selection Model as well as Propensity Score Matching— shows that the autonomous groups are more effective in improving child welfare. Thus, community-level interventions targeted at women can generate significant benefits to children's well-being.

One of the few analyses of *Mahila Samakhya* uses data from the state of Bihar to evaluate the program's effect on community-level trust and social capital (Janssens, 2010). The paper uses Propensity Score Matching to calculate Intent-to-Treat estimates of the program. Matching women from treated villages to those from untreated villages, results suggest that the program significantly increases trust and engenders social capital. Participants are more likely to contribute to local educational and infrastructural community projects. Significant spillovers also exist with non-participants; non-participant households in program villages exhibit higher levels of trust and are more likely to engage in community building activities than households in non-program villages.

Other work examines allied aspects of the program without explicitly evaluating it: Kandpal and Baylis (2013) ask whether participation affects the diversity of participants' peer group, while Kandpal and Baylis (2011) examine whether friends' participation improves female bargaining power and child welfare outcomes. None of these studies considers the causal model behind the mechanisms of community-level interventions. Next, we present a model that decomposes the effect of program participation in a community-level intervention.

4 Decomposing the Effect of Program Participation

Participation in *Mahila Samakhya* can have a direct effect by improving a woman's job prospects, and an indirect effect that changes perceived social norms through information spillovers. We attempt to decompose the effect of participation in *Mahila Samakhya* into these two effects. Participation in the program directly increases the woman's educational attainment, which in turn improves her job prospects (Phipps and Burton, 1998), potentially leading her to find employment outside her home. Further, even a participant who does not work may benefit directly from the education because when bargaining with her husband over household resources, knowing about better job opportunities and having more marketable skills increase her disagreement utility.[4] The direct effect of the program thus works through employability, skills, and reservation wages, which in turn affect bargaining power through outside options. The education gained through this program is therefore expected to raise bargaining power. By contrast, the indirect effect works by expanding the woman's information set about alternatives, and therefore affecting her perceived social norms.

Following Montgomery and Casterline (1996), we can think of a woman facing an optimization problem in which she chooses a course of action c in order to maximize her expected utility. Let the set $\{c_1, \ldots, c_N\}$ represent the universe of choices that a woman could potentially make, and let $\{c_1, \ldots, c_K\}$, where $K \leq N$, represent the subset of choices that the woman is aware of as being available to her. The choice that the woman makes as a result of her household bargain leads to one of several possible outcomes, indexed by the variable j, where each outcome is associated with a

[4]The disagreement utility is each spouse's intertemporal utility if they remained single or if they were non-cooperating in marriage, and depends on the spouse's own earning potential and the partner's earning potential as well as on the non-cooperative equilibrium outcome of investment in children.

vector of the "determinants of the woman's well-being", $\mathbf{Y_j}$ (Montgomery and Casterline, 1996, p. 157). Let $P(\mathbf{Y_j}|c, \mathbf{I})$ represent the probability of the woman experiencing the vector of determinants $\mathbf{Y_j}$, which is conditional on the choice made by her, c, and her information set, $\mathbf{I}$. Then, the vector of determinants $\mathbf{Y_1}$ occurs with probability $P(\mathbf{Y_1}|c, \mathbf{I})$, $\mathbf{Y_2}$ occurs with probability $P(\mathbf{Y_2}|c, \mathbf{I})$, and so on. The vector $\mathbf{Y_j}$ thus represents the outcome of the household bargain, given the components of the woman's information set. While we do not explicitly model the household bargain here, the formulation of the optimization problem presented below is fully compatible with a standard Nash bargaining framework.

Consider a woman who gets vocational training and then chooses to become a candle maker, c_j. Her decision to engage in this outside employment affects the amount of her household income she controls, $\mathbf{Y_j}$, which is an outcome of her household bargain. The effect of the bargaining outcome on her individual utility is denoted by $U(\mathbf{Y_j})$; thus, $\mathbf{Y_j}$ is the outcome of the household bargain, facilitated by choice c_j, and is a determinant of the woman's utility. Then, the woman's expected utility maximization problem is given by:

$$\max_{c \in \{c_1, \ldots, c_K\}} \quad \sum_j U(\mathbf{Y_j}) P(\mathbf{Y_j}|c, \mathbf{I}). \tag{1}$$

The woman's information set $\mathbf{I}$ consists of:

$$\mathbf{I} = (\mathbf{p_t}, \mathbf{q_t}, \mathbb{E}(\mathbf{p_{t+1}}), \mathbb{E}(\mathbf{q_{t+1}}), \mathbb{E}(\mathbf{Y_{t+1}}|c), \mathbf{\Sigma^2}, \mathbf{Z}), \tag{2}$$

where $\mathbf{p_t}$ and $\mathbf{q_t}$ are known current prices and quantities of the goods and services consumed by the woman. Expected future prices and quantities, $\mathbb{E}(\mathbf{p_{t+1}})$ and $\mathbb{E}(\mathbf{q_{t+1}})$, and the expected future vector of private determinants of well-being, $\mathbb{E}(\mathbf{Y_{t+1}}|c)$, have associated variances $\mathbf{\Sigma^2}$. Vector $\mathbf{Z}$ represents all remaining constraints and costs.

By educating a woman, *Mahila Samakhya* improves her household bargaining position and increases expected lifetime outcomes.[5] We can think of this education as adding a choice c_{K+1}, where $K + 1 \leq N$, to the woman's existing choice set. For example, choice c_{K+1} may be getting a job that would not have been feasible without the education. We assume that this choice can only be added by *Mahila Samakhya* because there is no other opportunity for formal or vocational education.

Some women may not select the newly available choice c_{K+1}. However, the education provided by *Mahila Samakhya* gives these women the potential to choose c_{K+1}, which increases their reservation wage and leads them to directly benefitting from the program. For example, the addition of c_{K+1} to the womans choice set can lead to an increase in $P(\mathbf{Y_n}|c_m, \mathbf{I})$, where c_m is a choice that was available before the educational component and $\mathbf{Y_n}$ is an improved outcome for the woman.

The program's indirect effect works through spillovers from social learning and social influence, as well as social norms. Participants learn about new opportunities and new behaviors from each other, which expands each woman's choice set and can improve her expected utility. So, while participation expands choice sets, it also changes expectations of future employment and empowerment outcomes, given the choices. Changing expectations to reduce the variance of outcomes associated with previously unknown choices can improve individuals' expected utility. Assuming that these women are risk-averse, even if certain choices were available to them, the unknown distribution of outcomes associated with these choices might discourage them from making these choices. For

[5]Even in cases where education is an irrelevant alternative, the woman is not worse off than before, hence participation leads to a weak improvement to her household bargaining position.

example, in our fieldwork, we encountered several women who lived in a village where all women only worked on the farm. As a result, for any individual woman considering working off the farm, the variance in possible outcomes associated with choosing to work off the family farm may be large. The earliest *Mahila Samakhya* participants from these villages told us that when they first considered off-farm employment, they were worried that their families would ostracize them or block access to their children. However, at *Mahila Samakhya* meetings they were introduced to women who do work outside the home, which gave them a more realistic picture of the outcomes from working outside the home. These women thus realized that the probability of some of the worst outcomes that they had feared was considerably lower than had previously thought. The fact that these women worked off the family farm was usually accepted by the families; none of them had been ostracized, nor had they lost access to their children.

By influencing the behavior of participants, the program also indirectly affects non-participant friends of participants. We can think of this indirect effect as working through the non-participant's information set. Exposure to participant friends may change the non-participant's expectations in two ways: (1) by expanding the set of choices known to her through her network, and (2) by changing her expectation of future distribution of outcomes, $\mathbb{E}(\mathbf{Y_{t+1}}|c)$ as well as the associated subjective variance, $\mathbf{\Sigma^2}$, by showing her what happens if she makes a certain choice. Over time, as more and more participants change their behavior (by becoming educated, getting a job, having greater bargaining power in the household), the social norm also changes to become less restrictive on all women, regardless of their participation status. As a result, the program's indirect effect may be substantial as the spillovers from the program increase over time.

Now consider the marginal effect of participation in *Mahila Samakhya*. If participation changes the utility-maximizing choice available to a woman from c_K to c_{K+1}, then the marginal effect of participation on her utility is as follows:

$$\Sigma_j U(\mathbf{Y_j}) P(\mathbf{Y_j}|c_{K+1}, \mathbf{I}) - \Sigma_j U(\mathbf{Y_j}) P(\mathbf{Y_j}|c_K, \mathbf{I}) \tag{3}$$

We assume that having more choices must make participants at least weakly better off, therefore implying that $P(\mathbf{Y_j}|c_{K+1}, \mathbf{I})$ first-order stochastically dominates $P(\mathbf{Y_j}|c_K, \mathbf{I})$. Corresponding to the marginal effect in equation 3, if spillovers change the information set from I to I', then the marginal effect of participation on utility is as follows:

$$\Sigma_j U(\mathbf{Y_j}) P(\mathbf{Y_j}|c, \mathbf{I'}) - \Sigma_j U(\mathbf{Y_j}) P(\mathbf{Y_j}|c, \mathbf{I}) \tag{4}$$

Since *Mahila Samakhya* may expose women to information that certain outcomes are in fact significantly worse than previously thought, first-order stochastic dominance of $P(\mathbf{Y_j}|c, \mathbf{I'})$ does not always follow. However, assuming that women are risk-averse, the variance-reducing effect of *Mahila Samakhya* on the information set, by itself, would increase participants' utility, even if having more choices might make participants worse off by making them aware of their subordinate status without giving them the ability to change it.

In summary, via the two direct and one indirect effects discussed above, *Mahila Samakhya* can expand the woman's choice set $\{c_1, ..., c_K\}$, and the newly available choice c_{K+1} increases the probability of the woman obtaining a more favorable value of Y_j (and decreases the probability of obtaining an unfavorable Y_j). Further, information can adjust expectations and perceived variance so as to also adjust the subjective probability distribution of outcomes.

5 Data

5.1 Survey Description

We use primary data on the participation in *Mahila Samakhya*, female empowerment, child welfare, and social networks of 487 Uttarakhandi women. The survey and sampling strategy, described in detail in Kandpal and Baylis (2011), covers 69 villages in six Uttarakhand districts, four with the program and two without. The villages in our sample were randomly chosen from the six districts. The sample size is 487 women. The survey was designed to trace self-reported networks, and hence was implemented using restricted snowball sampling. We first randomly sampled a woman in each village, asked her about five people she is in regular contact with, then conducted follow-up interviews with two randomly-chosen friends from these five. We repeated this process once, for each network. As a result, starting with one randomly chosen woman, we ended with networks of seven women. The top code of five for listing friends was introduced due to budgetary restrictions, but after considerable pre-testing. With and without a top code (as well as with larger top codes), we found that most women were not able to name more than five people outside the immediate family who they were in contact with on a regular basis.

The survey instrument includes the following key questions to help identify the effect of participation in the *Mahila Samakhya* intervention on an individual's level of empowerment. *Female Empowerment Dependent Variables:* (1) Access to Outside Employment: whether the respondent has an identification card for the National Rural Employment Guarantee Scheme (NREGS). (2) Physical Mobility: whether the woman can leave the house without permission.[6] (3) Political Participation: whether the woman participates in the local village council. (4) Proxy for Initial Bargaining Power: the woman's age relative to that of her husband.

Participation: (1) Whether the woman participates in the *Mahila Samakhya* intervention. (2) How long the program has been in her village. (3) Exposure to the program: the interaction of the number of years the program has been in her village with the woman's age minus sixteen.[7] *Other Socioeconomic Characteristics:* (1) Literacy and educational attainment.[8] (2) The number of male and female children born to the woman and their ages. (3) The woman's caste: since outcomes like physical mobility and political participation depend on social class, we find it important to control for the woman's caste, and wealth, as proxied by (4) the number of rooms in the house and the primary source of lighting. We also control for (5) the amount of time, in minutes, a woman spends collecting firewood each day, which reflects the amount of free time she has to participate in *Mahila Samakhya*.[9]

The National Rural Employment Guarantee Scheme, NREGS, guarantees at least a hundred

[6]Since this variable is difficult to verify, it might suffer from reporting bias: participants know the "correct answer" to this question is that they do not need permission to leave the house, and thus might be systematically more likely to overstate their physical mobility than non-participants. However, in field tests, we observed that participants were significantly more sensitive to their lack of household bargaining power and were likely to underreport the amount of say they had in the household because the program had made them aware of the entire feasible set of outcomes for women. Therefore, if we were to expect a sizable reporting bias by participants, it would be in the downward direction, i.e. participants would be likely to underreport their physical mobility.

[7]We subtract sixteen because women younger than sixteen cannot participate.

[8]We asked participants about literacy and educational attainment prior to participation. Because the program is not viewed as a school, many participants distinguished between their educational attainment and learning via *Mahila Samakhya* even without our asking.

[9]Note that Uttarkhand is a exogamous, patrilocal society, and women typically move to another village after marriage, and live in their in-laws' joint family home. Thus, they have little or no say in how far they need to travel to collect firewood.

days of paid work to the rural poor. Having an identification card (or having their name listed on the household card) gives the women access to outside employment. However, program supervisors sometimes deny women these cards or refuse to add their names to the existing household card because the work generated by NREGS is of a manual nature, and is thus considered "unsuitable" for a woman. *Mahila Samakhya* officers encourage participants to demand the cards, and where necessary, to report the supervisor to the local administrative officer. Kabeer (2000) raises several pertinent questions about the true meaning of choice in such a context, including does access alone represent choice? And, who or what influenced the decision to get access? Not all program participants choose to get this card, and indeed the process of getting one can be onerous on the woman, possibly involving several trips to the local and district NREGS offices, and making their case forcefully. Thus, owning an NREGS card represents a woman's choice to gain access to outside employment. While the decision to get a card is undoubtedly based on a confluence of economic needs and influence by program officers, women also learn about the feasibility and acceptability of women, in the form of other *Mahila Samakhya* participants owning these cards. The card not only represents access to employment, but also access to monetary income, the ability to work off the family farm, and increased physical mobility to women. The *Mahila Samakhya* program informs women of their right to claim these cards and advocates for them to get the cards, because NREGS officials typically tend to exclude women. However, the program does not guarantee access to the cards, nor does participation in *Mahila Samakhya* necessarily mean getting an NREGS cards. Hence, owning these cards represents access to the information that women are entitled to get NREGS cards, and the confidence to demand something not easily obtained, which also correlates with empowerment. Thus, participation in *Mahila Samakhya* can give women what Kabeer (2000) calls the "power to choose" to access outside employment.

We distinguish between pre-determined empowerment characteristics, like the spousal age ratio, and characteristics that might be affected by participation, such as owning an NREGS identification card or participation in village council meetings. Since *Mahila Samakhya* targets married women, and none of the women in the sample participated in the program before marriage, the spousal age gap is not likely to be affected by program participation. Program officials of the *Mahila Samakhya* intervention in Uttarakhand told us that women married to much older men have little say in the household, because often the age gap arises from a second marriage for the man, or some "undesirable" quality in the woman or her background. Hence, we treat a woman's spousal age gap as a proxy for her pre-participation level of empowerment. We cannot rule out the possibility that an older relative of the woman, say her mother, is a *Mahila Samakhya* participant and that therefore the respondent's age at marriage was not completely unaffected by participation. However, program participants tend to be young women, and the program only came into the region in 1995, so the influence of the participation of an older relative on a later participant's marriage decision is likely to be minimal.

The difference between matched pre-determined empowerment characteristics of participants and untreated women thus provides a baseline level of empowerment for participants. After establishing that matched participants and untreated women do not have significantly different pre-determined levels of empowerment, we use the spousal age ratio to control for differences in initial bargaining power when estimating the effect of participation on characteristics like owning an NREGS identification card.

5.2 Summary Statistics

Table 1 shows that the average woman in the sample is 32 years old, while her husband is 38 years old. She married at age 19 and has 9 years of education, one less than her husband. Her sons are, on average, eight years old, while her daughters are six. Only twenty women reported not having any children at all; the average number of children is 1.15, with an average age of 7.42. The average woman's house has three rooms and electricity. Sons and daughters have, on average, equal amounts of education; about three years.

Table 2 indicates that participants are significantly more empowered than non-participants. While on average, 61 percent of the women in the sample said they had NREGS cards, only 49 percent of non-participants did. In contrast, over 68 percent of participants had these cards. Similarly, while 71 percent of the sample said they did not need permission to leave the house, this was true for only 59 percent of non-participants while 78 percent of participants did not need permission. Finally, while only 14 percent of non-participants reported attending village council meetings, almost half of all participants did. In summary, whether in the form of access to employment, physical mobility, or political participation, women who participate in *Mahila Samakhya* have higher levels of empowerment. However, these statistics do not tell us whether more empowered women self-select into the program, or whether participation actually improves female autonomy.

Table 3 shows us balance across key characteristics of the four treated and two untreated districts in the sample. The only two significant differences are in the number of sons and the time spent on collecting firewood. On average, participants have 0.27 sons more than non-participants and spend significantly more time collecting firewood. The magnitude of the difference in the number sons suggests the economic impact, if any, is small. However, the difference in time to collect firewood is large, making it important to control for the differences in time constraints. We discuss this variable in further detail in the next section. Overall, table 3 suggests that the program does not appear to be targeted in placement because there are no other significant differences between women in treated and untreated districts. These data illustrate that the women we sample in treated and untreated districts are largely similar in covariates.

Another source of endogeneity could arise from the potential placement of the program in villages where women have a relatively high level of bargaining power and thus are more likely to respond favorably to the treatment. We use administrative block-level [10] data from the Indian censuses of 1991 and 2001 on village-level female bargaining power. Matching the year that *Mahila Samakhya* entered a block to the most recent census before that year, we compare the levels of bargaining power in blocks blocks which later received *Mahila Samakhya* with those that did not. Results for t-tests of equality, presented in table 4, show that there are no significant differences in any of the block-level measures of female bargaining power: sex ratio for the entire population and for children up to the age of six, the scheduled caste or scheduled tribe sex ratio, and the literacy ratio are all statistically similar across treatment assignment. Similarly, the sex ratio of workers and non-workers also does not vary between treated and untreated blocks. Thus, these t-tests also fail to yield evidence that would cause us to believe that the program was systematically targeted at villages where women were either less empowered or already more empowered, and hence more likely to respond to the treatment.

[10] Blocks are small groups of villages and are the administrative unit directly above the village council. Within each district, the placement decision of *Mahila Samakhya* is made by block, rather than by village, hence these blocks are the appropriate unit of analysis when examining whether the program is targeted in any way. For blocks without the program, we use data from the 2001 census.

It may still be that untreated districts in our sample are not representative of statewide trends and that women in these districts may be more or less empowered than average, implying that program placement may be targeted. However, the nationally-representative NFHS-3 (International Institute for Population Studies and Macro International, 2007) and DLHS-3 (Ministry of Health and Family Welfare and International Institute for Population Studies, 2010) show that the women in untreated districts in our sample do not differ significantly from the rest of the state. For instance, the average age at at marriage for Uttarkhandi women is 20.6, while in our untreated sample, it is 19.8; 43 percent of all Uttarkhandi women work while 45 percent of the untreated women in our sample do. The total fertility rate in the state is 2.6, which corresponds closely to the average family size of one boy and one girl in our untreated sample. Finally, while 84 percent of the state has access to electricity, 90 percent of our untreated sample does. This lack of significant differences suggests that the program is not targeted at districts by levels of female empowerment.

The next concern with identifying the effect of the program is self-selection. Table 5 indicates the presence of self-selection into *Mahila Samakhya*. The average participant is three percentage points closer in age to her husband than the average non-participant in treated districts, which suggests that women with greater initial bargaining power may self-select into the program. Further, participants tend to have older and more sons than non-participants, although the differences are not significantly different from zero. Participants are significantly more likely to be Brahmin than non-participants. Participants are less likely to live with their husbands; the difference of 19 percent is highly significant. However, in our pre-tests, we found that even women who do not live with their husbands live with other male relatives, including fathers, fathers- or brothers-in law, uncles, and sons. In all our fieldwork, we only encountered seventeen women who lived alone or without any older male relative. Of these seventeen women, all but six lived with their mothers- or sisters-in-law. The presence of these relatives in the house represents restrictions on the woman's empowerment, even if she lives without her husband.

Several other characteristics, such as the number and age of daughters, the spousal education ratio, and the woman's time to collect water, are not statistically different for participants and non-participants. Further, none of the wealth indicators, including number of rooms, electrification, improved toilet facilities, materials used in floor and wall construction, are different for these two groups, suggesting that poorer participants neither select into the program nor are they targeted based on indicators of wealth (number of rooms, electrification, access to improved toilet facilities, and nature of the construction materials used for the floor and walls of the house). Nonetheless, this table highlights the importance of controlling for selection in to the *Mahila Samakhya* program. In addition to the variables listed above in the Survey Description, we also control for the imbalanced characteristics discussed here.

6 Empirical Analysis

6.1 Methodology

We use several methods to identify the effect of the program in the presence of self-selection. We first match women by their pre-existing bargaining outcomes and other characteristics across treated and untreated districts. We then control for potential issues of truncation in the matching. Next, we use an instrumental variables approach, where our instrument is the interaction between the time the program has been in the village and the years over 16 of the woman to capture the exposure of the woman to the program. We separately control for the age of the woman in both first and

second stage regressions. Last, we estimate the intent-to-treat effect, where we match all women regardless of participation in treated districts to those in untreated districts to estimate the effect of the program.

We estimate three sets of treatment effects. The first simply estimates whether women living in treated districts, regardless of participation status, are more empowered than women living in untreated districts. The second treatment effect examines whether non-participants in treated and untreated districts are significantly different in terms of female empowerment outcomes. The third estimates the impact of the program on participants relative to untreated women with similar characteristics to account for any issues of self-selection. Although table 3 suggests the lack of any substantial differences between treated and untreated districts on observables, the first estimate more formally tests this assumption.

Intent-to-Treat: We estimate intent-to-treat effects where we treat all women living in villages with *Mahila Samakhya* — regardless of the woman's participation status– as covered by the program. If, even after ignoring the participation decision, we find that women living in treated villages (regardless of their participation status) are significantly more empowered than the untreated, we can conclude that the program is effective.

Instrumental Variables: Propensity Score Matching only accounts for selection or targeting on observables. While we have argued above that *Mahila Samakhya* may not be targeted in its placement, women still chose whether to participate, and PSM may not fully control for all the unobservable factors governing a woman's participation decision. To prevent contamination from unobservable characteristics influencing participation, we use two-stage least squares (2SLS) and instrument for participation using exposure to the program. Our instrument for participation in *Mahila Samakhya* is the number of years the program has been in a village interacted with the woman's age minus sixteen. The youngest participant we encountered in our field tests or data collection was sixteen; we subtract sixteen from the age of the woman to accurately reflect the number of years she could have participated in the program. The instrument tells us the years of exposure to the program, and any effect of this variable on female empowerment likely works through participation in the program, rather than directly. This variable is driven by the year the program started in the village as there is little migration among married women in the region. Since women often migrate at the time of marriage, and we do not know whether the woman's natal village had the program, migration at the time of marriage might lead to measurement error, which in turn would bias results downwards. However, unmarried women tend not to participate in the program, so exposure would have to be indirect, and thus the resultant bias would be small. We control separately for the woman's age so as to account for any trends in the dependent variable that vary by age.

A potential concern about our instrument may be that the instrument fails to account for village-specific trends in the dependent variable that vary in the woman's age and are correlated with the empowerment outcomes. If this concern were correct, then our instrument would not adequately control for the participation decision. Thus, by distinguishing between participants and non-participants in treated villages, we would be inflating our estimates. However, the ITT discussed above gets around this problem by treating participants and non-participants in treated districts alike and only distinguishing between treated and untreated districts. If the ITT is consistent with the results from the IV estimation, we will be able to conclude that the instrument adequately controls for such bias.

Propensity Score Matching: To account for potential targeted program placement or selection in participation, we use Propensity Score Matching (PSM) and instrumental variables to control for

self-selection. When treatment assignment or participation is not random but determined by observables, PSM allows us to compare treated individuals to untreated individuals (or non-participants to the untreated) using observables such as demographic and economic characteristics to construct the control group. Each individual in the dataset is assigned a propensity score that tells us the likelihood of an individual being treated. That propensity score is a conditional probability measure of treatment participation, given observable characteristics, $\mathbf{x}$, and is expressed as follows:

$$P_i(\mathbf{x}) = P[D_i = 1|\mathbf{X} = \mathbf{x}], \tag{5}$$

We conduct this analysis maintaining the unconfoundedness assumption (Imbens and Wooldridge, 2009):

$$D_i \perp (Y_i(1), Y_i(0))|P_i(\mathbf{x}) \tag{6}$$

where $\perp$ signifies independence, given that the balancing condition is satisfied (Cameron and Trivedi, 2005). The unconfoundedness assumption implies treatment assignment, D_i is independent of empowerment outcomes, Y_i (corresponding to the Y_i in section, after controlling for propensity scores. In other words, we assume there are no unobservables that affect empowerment outcomes and the probability of treatment.

Treated and untreated individuals are matched based on proximity of their propensity scores, $P_i()$, thus creating a control group. We then estimate treatment effects by comparing the outcome of interest for the treated and control groups. PSM eliminates selection bias if controlling for $\mathbf{x}$ eliminates selection bias from endogenous placement. Because treated and untreated districts do not differ significantly with respect to observable characteristics (table 3), it is a reasonable assumption that the distribution of individual unobservable characteristics is similar across treated and untreated districts. Because the program appears to have been distributed randomly across districts, and individual selection into the program does not differ by district, a PSM approach will give an unbiased measure of the program impact.

For the treatment effect comparing non-participants and the untreated, each non-participant is matched with replacement to an untreated woman based on the closeness of the propensity score. For the treatment effect comparing participants to untreated individuals, each participant is matched with replacement to an individual from an untreated district. We use kernel matching in which all treated observations are matched with a weighted average of the propensity score for all control observations. Weights are inversely proportional to the distance between the propensity scores of treated and control observations (Becker and Ichino, 2002).

Truncation may be a concern here because we are matching the full distribution of women in untreated districts to a subset in treated districts that has chosen to be treated. Untreated women represent the full distribution of outcomes, while participants represent a left-truncated sample of this full distribution. If uncorrected, this truncation could bias our treatment results upward. Hence, after matching the full sample of participants to untreated women, we truncate the sample of untreated to only include women whose propensity to participation is no lower than the lowest percentile of the participation propensity for participants. Similarly, we also re-estimate the match between non-participants and the untreated only including untreated women whose propensity score is no greater than the highest propensity score for non-participants. Note that all matches are conducted on the region of common support.

Dependent Variables: The dependent variables measuring empowerment are of two kinds: (1) pre-determined characteristics (proxied for by spousal age ratio) which cannot be affected by *Mahila Samakhya*, and (2) characteristics that can be affected by participation, like having an NREGS identification card, leaving the house without permission, and attending village council meetings. The independent variables on which we conduct the match and the 2SLS regression include observed factors that likely affect both program participation and female empowerment: (1) spousal age ratio, defined as the respondent's age over her husband's age, (2) the number and age of her children,[11] (3) her years of education, whether she is literate (in the case of participants, whether she was literate prior to participation), and whether she has less than four years of education (and is thus likely to need the education provided by the program). Time constraints may play an important factor in determining participation and bargaining outcomes, so we also include (4) the time spent each day by the respondent on collecting firewood (reflecting time constraints),[12] and (5) whether she lives with in-laws and the number of sisters-in-law living with her (reflecting whether she can leave her children in someone's care while participating in the program), (6) whether she is a Brahmin, (7) the number of rooms in her house, and (8) whether her house has electricity. Village fixed effects are also included.

7 Results

Table 6 summarizes the estimated effects of the impact of *Mahila Samakhya* on proxies of female empowerment, using an intent-to-treat estimate, 2SLS, PSM, and truncation-corrected PSM. Comparing estimates across methods highlights the robustness of our findings: all four methods suggest that participation in *Mahila Samakhya* increases ownership of NREGS cards as well as a woman's ability to go out without permission. However, while the PSM results do not find a significant effect on participation in village council meetings, 2SLS estimates suggest otherwise. Overall, these results indicate that participation in the program empowers women by giving them greater access to employment and by increasing their physical mobility. In the rest of this section, we discuss these results in detail.

7.1 Intent-to-treat

Although we instrument for selection into the program using years of exposure to *Mahila Samakhya*, our instrument may not account for trends in the dependent variable that both vary in the woman's age and that are specific to treated villages. Thus, first we estimate a simple intent-to-treat effect where we treat all women living in villages with *Mahila Samakhya* — regardless of the woman's participation status– as covered by the program. These intent-to-treat estimates, presented in

[11] To address concerns over whether the spousal age ratio and the age and number of children are truly exogenous, we re-estimated all the results presented below without these three variables. The corresponding results are stronger in significance than the results including these variables, but the signs and magnitudes are similar. If the spousal age ratio and age and number of children are not influenced by participation, but indicate pre-existing levels of empowerment, and if we have self-selection into *Mahila Samakhya*, our estimates of the effect of participation would be biased upward if we excluded these variables.

[12] The variable time spent collecting firewood reflects an exogenous time constraint on the woman because Uttarakhand is exogamous and patrilocal. Sons tend to live with their parents, and the location of a married woman's house is not chosen by her and is therefore exogenous to the amount of time she spends collecting firewood. The more time a woman spends each day on firewood collection the less time she has to participate in the program. However, it is also possible that participants who spend a large amount of time collecting firewood may feel isolated and may thus be more interested in the social capital building activities of the program.

table 6 show a significant and positive impact of participation on owning an NREGS card and on being able to leave the house without permission. The results do not, however, show a significant impact on political participation. Nonetheless, we find evidence that the *Mahila Samakhya* program was successful in empowering women in two of the three ways considered.

7.2 Instrumental Variables Results

The robust first stage results presented in table 7 show that the program exposure instrument is highly significant and positively correlated with participation in the program, validating the use of this instrument to predict participation. The first-stage results also tell us that Brahmin women are significantly more likely to participate, as are women with greater firewood collection times (suggesting that the increased interest in community building activities dominates the time constraint), while women who live with their parents-in-law are less likely to participate. The F-statistic for this first stage is 12.38 for the physical mobility regression, 11.90 for political participation, and 13.55 for the access to outside employment regression. All three values are greater than the rule-of-thumb cutoff of 10 proposed by Staiger and Stock (1997). To test the validity of the instrument in the exactly-identified regression, we used the procedure outlined in Nichols (2007), and added a non-linear transformation of the instrument (non-logged exposure to the program), which allowed us to use Sargan's test for misspecification. Sargan's test results suggest that the instruments are valid.

Robust second-stage results (table 8) show that *Mahila Samakhya* participants are significantly more empowered than non-participants in all three ways: participants are more likely to leave the house without permission, to participate in village council meetings, and have NREGS cards. The increase in significance of the physical mobility and political participation dependent variables highlights the importance of correcting for sample selection by instrumenting for participation. Other than program participation, the empowerment outcomes seem to be the result of different data generating processes, with little overlap in significance of explanatory variables across the three regressions. Results suggest that older women are more likely to participate in village council meetings, but that the number and age of children are important determinants of whether the woman has an NREGS card— women with more young children are less likely to have NREGS cards, perhaps because the time constraints imposed by raising children do not permit them to work outside the house. Women who live with their sisters-in-law are more likely to leave without permission and to participate in the village council, which may be because having the additional help around the house enables women to leave more easily. Brahmins are less likely to have NREGS cards, while women whose houses have electricity are more likely to go out without permission. [13]

The predicted outcomes based on the highly significant 2SLS estimates are presented in figures 2 and 3. These outcomes tell us that while 67.0 percent of non-participants can go out without permission, only 49.4 percent of the untreated can do so. Similarly, 71.3 percent of non-participants

[13]To address concerns over the robustness of our standard errors, we also clustered by village and network— the significance of results does not change in either case. We also used randomization inference (RI) (Rosenbaum, 2002) to estimate the z-scores for the three empowerment outcomes. Rather than drawing repeated samples of observations from the known full population, RI assumes that the population is restricted to the observed sample. The treatment assignment is assumed to be the only random variable. All observed outcomes and covariates are assumed fixed. Using predicted participation from the first-stage of the 2SLS regression as the continuous treatment for RI, we find participants to be more empowered than non-participants for all three outcomes. The z-scores are 6.53 for owning an NREGS card, 1.56 for going out without permission, and 4.73 for participating in the village council. These results suggest that, with the exception of physical mobility, participants are significantly more empowered than non-participants.

have NREGS cards, only 26.6 percent of the untreated do. Predicted political participation is low for both groups; six percent of non-participants attend village council meetings, while 3.9 percent of the untreated do.[14] The fact that all predicted outcomes are higher for non-participants than for the untreated suggests that *Mahila Samakhya* generates sizable spillover effects for non-participants living in treated districts. Figure 3 tells us that 78.2 percent of participants can leave the house without permission, 53.7 percent participate in the village council, and 79.7 percent have NREGS cards. Compared to the predicted values for the untreated, these outcomes represent significantly higher levels of empowerment for participants. Participants are 28.8 percent more likely to go out with permission, 49.8 percent more likely to participate in the village council, and 53.1 percent more likely to have access to NREGS cards. The 2SLS estimates are similar in magnitude and significance to the ITTs and suggest that any omitted age-variable trends do not substantially change our findings.

Since our sampling strategy relied on networks, we do not have a randomly-selected sample. Statewide data[15] on NREGS show that in 2010, women in untreated districts used an average of 35 percent of the person-days of work generated by NREGS, while in treated districts, they used an average of 41 percent, which is consistent with *Mahila Samakhya* having a positive effect on access to outside employment and suggests that our estimates reflect statewide trends.

7.3 PSM Results

Table 11 presents the two sets of treatment effects discussed above estimated using PSM: the first comparing non-participants to the untreated, and the other comparing participants to the untreated. The upper panel of the table shows the results comparing non-participants to the untreated. These results tell us that a non-participant is not significantly more empowered by simply living in a treated district. Without matching, only the NREGS cards variable is significantly different, with non-participants being significantly more likely to own NREGS cards. The decrease in significance in NREGS card ownership after matching highlights the importance of controlling for selection in to the program. Indeed, given that treated and untreated districts are very similar, these estimates tell us that *Mahila Samakhya* does not target districts with particularly low (or high) levels of empowerment.

The lower panel of table 11 presents treatment effects of the program on participants. These results show that participants and untreated women have statistically equal spousal age ratios, suggesting that individuals do not choose to participate based on initial bargaining power. Hence, any differences in the other measures of empowerment likely stem from the effect of the program. Evidence suggests that the program significantly increases access to outside employment, as 80.9 percent of participants own NREGS identification cards, compared to only 14.4 percent of untreated, which translates to a difference of 66.5 percentage points. Participants are also significantly more likely to leave the house without permission. However, according to the matched results, participants are not significantly more likely to participate in village council meetings. A woman's ability to participate in village-level politics may depend on a high-stakes bargain with her husband and in-laws, while the decision to get an NREGS card may be the result of a lower-stakes bargain because outside employment will earn the household extra income. Political participation may depend

[14] Thirty-six percent of all women in our sample attend village council meetings, but only 8 percent of untreated women do. Fourteen percent of non-participants attend village council meetings, so the predicted outcome of 8 percent is significantly lower. However, this discrepancy may simply highlight the importance of spillover effects generated by *Mahila Samakhya*.

[15] District-level data on women's access to NREGS identification cards are not available for the entire state.

not only on program participation but also on the behavior of peers and support from them; this link is studied in greater detail in Kandpal and Baylis (2011).

A potential concern related to the use of PSM in comparing participants to women in untreated districts is that our treated sample is truncated by only including those women who participate. The true propensity of having access to *Mahila Samakhya* for women living in the treated districts is one, so the best control-group matches would be women with high propensity scores in the untreated districts. Therefore, by definition, women in villages without the program represent the full distribution of outcomes, while treated women represent a left-truncated sample of this full distribution. However, in the above matching process, we are comparing the full distribution of women in untreated districts to a subset in treated districts that has chosen to be treated, and therefore likely has higher propensity scores. If uncorrected, this truncation could bias our treatment results upward.

Thus, we may have a right-truncated distribution of non-participants and a left-truncated distribution of participants in treated districts. The distributions of propensity scores for program participation of treated and untreated women highlight the problem of truncation (figure 1). The distribution of the untreated is bimodal, so we should not compare individuals in or around the lower mode of the untreated distribution to participants. Similarly, comparing non-participants to individuals around the higher mode of the propensity scores for the untreated would also be misleading. We correct for truncation by re-estimating the treatment effects comparing non-participants to the untreated (presented in the upper panel table 11) with the sample of the untreated limited to those whose propensity scores are below the lowest percentile of propensity scores for participants. For the comparison between participants and the untreated, we limit the sample of untreated to women with propensity scores for program participation greater than the lowest percentile of propensity scores for participants. Results presented in table 14 indicate that truncation does not influence the outcomes presented in table 11: the sign, size, and significance of the estimates do not change for either comparison set (non-participants versus untreated, and participants versus untreated).

7.4 Comparing PSM and 2SLS Estimates

The marginal effect estimates obtained from 2SLS cannot be interpreted as average treatment effects, and thus cannot be compared directly to the estimates obtained from PSM. Multiplying 2SLS marginal effect estimates with individual propensity scores for participation gives us the distribution of treatment effects. The average of this interaction is, then, the average treatment effect. In table 10, we present the 2SLS estimates for average treatment effects of participation for the entire sample, participants, non-participants, and the untreated.

Marginal effect estimates point to the significant benefits from participation. In the untreated districts in our sample, 57.5 percent of all women (69 of 120 women) can go out without permission. If these districts were to be covered by *Mahila Samakhya*, our results suggest that 75 percent (89 of 120 women) would be able to do so. Similarly, only 19 percent of all untreated women (25 of 131 women) participate in village council meetings, whereas if the program covered these districts, 41 percent of these women (55 of 131 women) would participate in village council meetings. Finally, only 19 percent of the women had NREGS cards (27 of 140 women), but if they were to receive the program, 88 percent (122 of 140 women) would have access to outside employment.

7.5 Disentangling Direct and Indirect Effects

Social learning, strategic interactions, and modeling information flows can tell us a lot about the mechanisms underlying observed behavior (Maertens and Barrett, 2012). Therefore, in this section, we attempt to disentangle the direct effect of participation in the program from its indirect effect that may work through learning from peers, new information, and social interactions. Table 11 also shows that program participation increases the likelihood of a woman working, compared to untreated women; the associated t-statistic is 1.52, making this difference short of statistically significant at the ninety percent level. In addition to an effect of the program on empowerment through increased employability, there may also be a sizable effect even on participants who do not work. Since participation in *Mahila Samakhya* does not affect the woman's employment, it must instead work either by increasing the woman's disagreement utility and therefore affecting the household bargain, or through the peer network effects of social learning and social influence. Table 12 presents treatment effects of the program on these women by matching them to the untreated. Participants who do not work are still more likely to have an NREGS card and to participate in village council meetings. They are not, however, more likely to leave the house without permission, perhaps because without working and earning an income, they do not have adequate intrahousehold bargaining power. The fact that women who don't work still own NREGS cards maybe because NREGS only generates a hundred days of employment; hence participants may not have been working at the time of the interview, but still had access to the NREGS program.

However, simply observing improved outcomes for women who do not work does not let us disentangle the effect of any changes in opportunity cost from an indirect, information-related effect. To do so, we would need to isolate the effect of the program on women whose opportunity cost does not change due to the program. Women who do not own NREGS cards are least likely to have had their opportunity cost changed by the program: they do not work, and also do not have access to outside employment, as represented by NREGS cards. Table 13 presents these treatment effects, and shows that women who do not have NREGS cards are still more likely to participate in village council meetings and to leave the house without permission. Thus, even women who do not face increased opportunity costs from participation are more empowered than non-participants.[16] We tried various metrics for the matching process; results are robust.

The only exception is that non-participants are significantly more likely to own an NREGS identification card than the untreated, after controlling for truncation. In our field tests, respondents reported not having known that working outside the home was a possibility for *them* simply because they had never seen anyone in the village do so. As a result of having participant friends, these women may realize that working outside the home is in fact part of their choice set. The increase in significance in the truncation-corrected estimation thus suggests that having participant friends may increase a woman's choice set by showing her that working on NREGS-generated projects is a possibility for her. The access to employment off the family farm and not involving household chores likely increases the woman's intrahousehold bargaining power.

Table 9 presents robust second-stage results for treatment effects on women who do not have NREGS cards or those do not work using 2SLS. These estimates are similar to those obtained from matching: for women who do not work, we find that participants are significantly more likely to have NREGS cards than non-participants, although 2SLS does not yield a significant effect of

[16]Further restricting the sample to women who do not work and do not have NREGS cards shrinks the sample to 60 observations for the physical mobility estimation; the resultant treatment effect of 0.409 has an associated standard error of 0.311 (t-statistic of 1.31). The political participation estimation, with 67 observations, yields a treatment effect of 0.391, and a standard error of 0.235 (t-statistic of 1.66).

participation on attending village council meetings. For women without NREGS cards, we find that participation in *Mahila Samakhya* significantly increases the ability to go out without permission. The positive effect on attending village council meetings is also significant at the 90 percent level. We thus find that *Mahila Samakhya* improves the bargaining power of not only women who work, but also of those who do not have access to outside employment. This effect may come from a combination of increasing the woman's reservation wage and increasing her information set about alternative choices. However, all but one of the first-stage F-statistics for these regressions are lower than 10, ranging from 7.27 to 8.54. The only exception is the physical mobility regression for women without NREGS cards, where the F-statistic is 11.75. While we might be concerned about weak instruments in this case, the Sargan test indicate that the instruments are exogenous.

Restricting the sample further to those women who do not work *and* do not have NREGS cards, we have 73 observations in the physical mobility regression and 78 in the political participation regression. The first-stage F-statistic for the physical mobility regression is 8.23, but is only 6.33 for the political participation regression; as a result, we do not report the results from this regression. We find that program participation increases a woman's ability to leave the house without permission; the associated t-statistic is 1.83, meaning that the estimate is significant at the 90 percent level. Although the sample is small and weak instruments are a concern, these results suggest that the indirect effect of *Mahila Samakhya*, working through increased reservation wages and expanded information sets, can improve women's physical mobility, and thereby empower them.

In summary, using PSM, 2SLS, and ITTs, we find that the *Mahila Samakhya* program empowers women in a variety of ways, via both direct and indirect routes. In addition to directly increasing access to outside employment, political participation, and physical mobility, our results suggest the program generates significant spillovers that allow non-participants to benefit via greater access to outside employment. The program also empowers participants who do not work (and therefore only benefit from an increased reservation wage) to participate in village council meetings. Finally, we find that participants who do not have access to off-farm work still have greater physical mobility and political participation than untreated women.

8 Conclusion

This paper uses primary data from the north Indian state of Uttarakhand to study the impact of a community-level rural women's empowerment program called *Mahila Samakhya*. *Mahila Samakhya* aims to empower women through education and information, taking a grassroots approach to its implementation. We conceptually disentangle the effect of the program into a direct component that works through access to outside employment and an increased reservation wage, as well as an indirect component that works through information spillovers. This distinction between direct and indirect effects extends beyond the *Mahila Samakhya* program to other programs operating at the community-level that may have spillover effects. We find evience that Mahila Samakhya both directly and indirectly succeeds in helping empower rural women.

Our empirical approach addresses two sources of potential bias. First, the program may be placed in villages and districts that are inherently different from those without the program, which might bias our estimates. Second, the characteristics of women who participate in the program may be different from those who choose not to participate, again, biasing our results. We first consider program placement, and find that the characteristics of the women are similar across treated and untreated districts. We further formally estimate bargaining power outcomes for non-participants in treated districts and their matched counterparts in untreated villages and find no significant

differences. To address the concern of self-selection into the program, we use PSM to compare the outcomes of participants to those of untreated women. We also use 2SLS to instrument for the decision to participate using the roll-out of the program as well as estimating the intent to treat, and thus avoiding the problem of self-selection. By comparing participants to untreated women and using 2SLS to control for self-selection in the participation decision, we provide accurate estimates of the impact of the *Mahila Samakhya* program on women's economic, social, and intrahousehold empowerment. We test the validity of our instrument by estimating an intent to treat effect and find similar results. Had we directly compared participants to non-participants within the same district or failed to control for selection, we would have misestimated the true effect of participation.

We find that the program has resulted in significant increases in women's access to outside employment, ability to go out without permission, and political participation, all of which are associated with higher levels of empowerment. We also find that these benefits were not restricted to those women who found outside work after the vocational training. Our results suggest that participants who do not work are still more able to leave the house without permission and have more access to outside employment. After correcting for truncation in the distribution of participants, we find that non-participants are significantly more likely to own NREGS cards than untreated women. This result may be evidence of positive spillover effects of the program on non-participating neighbors.

The marginal effects of the impact of *Mahila Samakhya* on empowerment translate to significant increases in the number of women with higher empowerment outcomes. If the untreated districts in our sample were to be covered by *Mahila Samakhya*, 17 percent more women (an increase of 20 out of 120 women) would be able to go out without permission, 33 percent more (an increase of 30 out of 131 women) would participate in village council meetings, and 58 percent more (an increase of 95 out of 140 women) would have NREGS cards. These numbers highlight the potential of *Mahila Samakhya* in effecting significant social change.

Results also show that even participants without access to outside employment are more empowered than untreated women. One may criticize the program for spending scarce resources on individuals who do not then use their new-found skills to find employment, but we show that the increased reservation wage is beneficial in and of itself. Further, we find that *Mahila Samakhya* had an indirect effect on the empowerment of non-participants relative to the untreated, indicating that the program has a spillover effect. This indirect effect likely works through either increasing access to information such as with a demonstration effect, or by changing social norms within the village. Understanding these indirect effects calls for further research.

The *Mahila Samakhya* intervention adopts a slow and careful grassroots approach to rolling out its activities. Thus, our results cannot be generalized to programs following a faster, more individual-focused, or a top-down approach. Further, these results should be interpreted with some caution if selection on unobservables is a serious concern. That said, any bias from unaccounted-for program placement would likely be in the downward direction because the program would target women with low levels of empowerment. One might worry that areas with relatively empowered women are more effective in lobbying for the program to be implemented in their district, however we show that the characteristics of empowerment that are not likely to be affected by participation are not significantly different across treated and untreated districts and that other empowerment outcomes do not significantly differ over treated and untreated blocks before the introduction of the program, suggesting that empowered women are not lobbying for the program.

The *Mahila Samakhya* program is unique, but it may be fruitfully replicated elsewhere in the developing world because it attempts to harness local peer networks to empower women. Similar

programs, including one in Honduras (Rowlands, 1997), indicate the potential of replicating a program like the *Mahila Samakhya* elsewhere. The success of this program has encouraging implications not just for female empowerment goals, but also for the other factors affected by empowerment, such as child welfare. By empowering women to have greater say in their households and communities and to engage in income-generating activities, the program may generate significant benefits to the rest of the participant's household.

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9 Tables and Figures

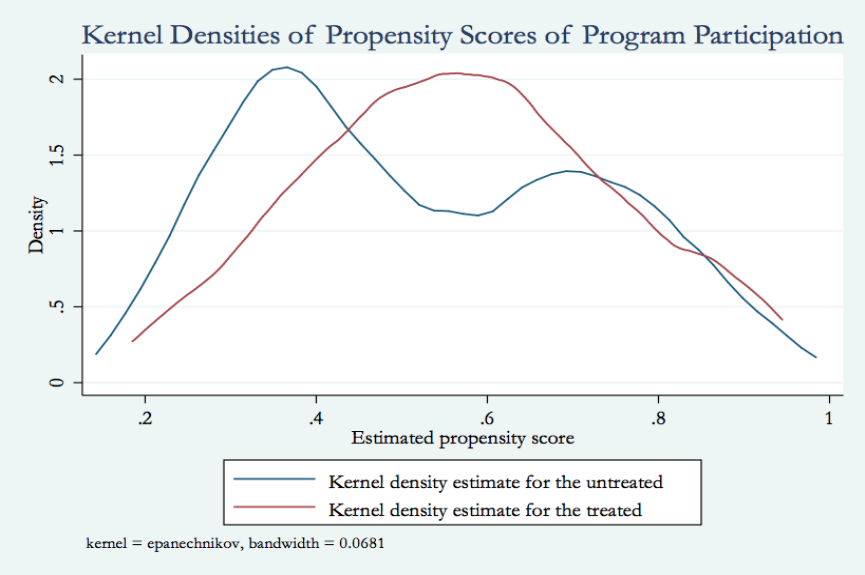


Figure 1: Propensity Scores for Program Participation

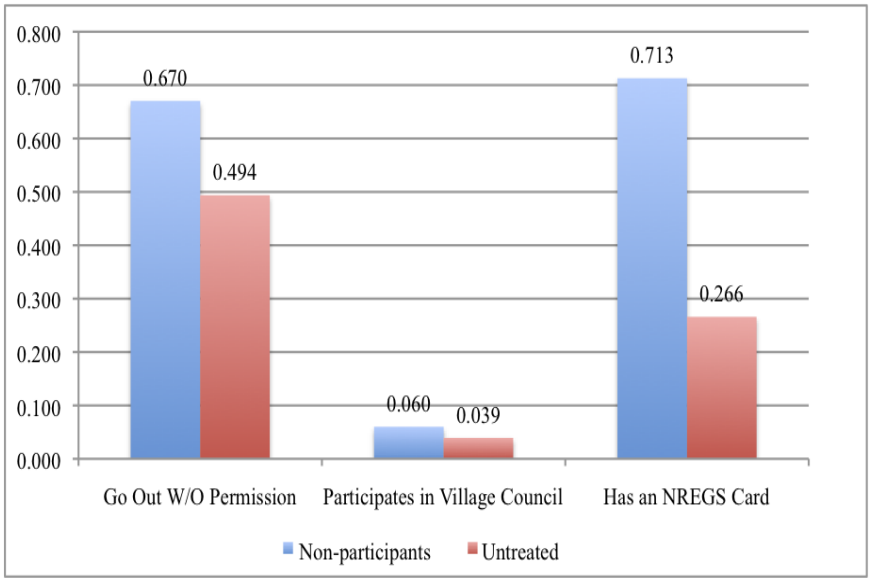


Figure 2: Predicted Outcomes from 2SLS: Non-participants versus Untreated

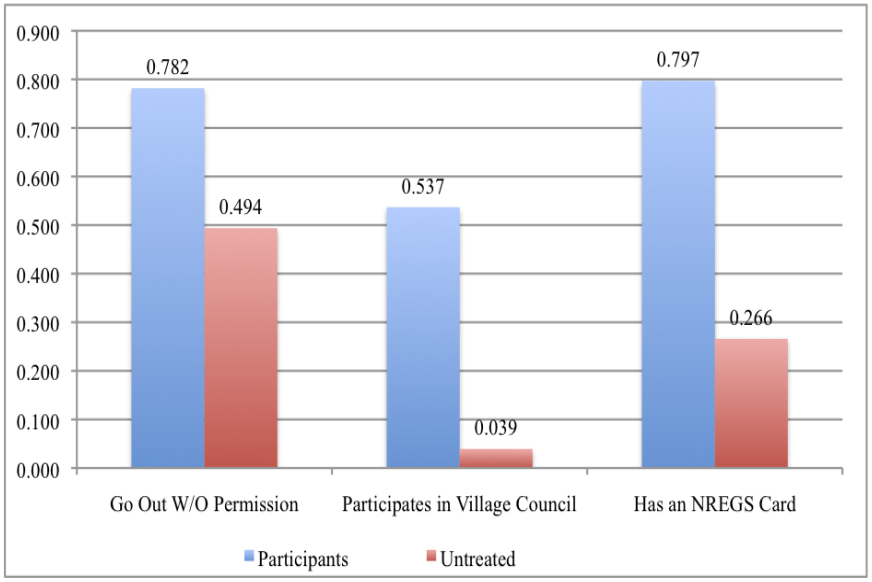


Figure 3: Predicted Outcomes from 2SLS: Participants versus Untreated

Table 1: Descriptive Statistics

Variables	Observations	Mean	Std. Dev	Min	Max
Respondent's Age	472	32.18	8.11	20	65
Husband's Age	437	37.89	9.25	23	80
Respondent's Age at Marriage	463	19.25	3.34	1	30
Average age of sons	487	8.09	7.79	0	36
Average age of daughters	487	6.20	6.70	0	30
Respondent's Years of Education	483	7.19	5.02	0	17
Husband's Years of Education	415	10.11	3.71	0	17
Sons' Years of Education	487	3.766	4.45	0	17
Daughters' Years of Education	487	3.11	4.29	0	17
Number of Rooms	487	3.33	2.12	0	19
Electrification	487	0.89	0.31	0	1

Table 2: Dependent Variables

Dependent Variables	Percent Yes	Observations
Has NREGS ID Card		
All	60.62	485
Non-participants	48.94	188
Participants	68.02	297
Can Leave House Without Permission		
All	70.89	454
Non-participants	58.82	170
Participants	78.17	284
Participates in Village Council Meetings		
All	36.36	473
Non-participants	14.20	176
Participants	49.49	297

Table 3: Treated and Untreated Districts

Variables	Untreated	Treated	Difference	t-test	Observations
Demographics					
Spousal Age Ratio	0.85	0.85	-0.03	-0.17	487
	(0.01)	(0.01)	(0.02)		
Age at Marriage	19.76	18.69	1.08	1.33	487
	(0.05)	(0.54)	(0.81)		
Age of Sons	6.96	9.03	-2.07	-1.66	487
	(0.84)	(0.76)	(1.25)		
Age of Daughters	5.45	6.98	-1.52	-1.78	487
	(0.46)	(0.84)	(1.29)		
Number of Sons	1.09	1.38	-0.29	-2.27*	487
	(0.04)	(0.08)	(0.13)		
Number of Daughters	0.99	1.13	-0.14	-1.29	487
	(0.05)	(0.07)	(0.11)		
Own-to-husband's education	0.65	0.61	0.03	0.35	487
	(0.12)	(0.04)	(0.09)		
Lives with Husband‡	0.83	0.76	0.07	0.42	449
	(0.09)	(0.09)	(0.04)		
Lives with In-laws‡	0.56	0.45	0.11	1.18	487
	(0.11)	(0.04)	(0.09)		
Works‡	0.45	0.65	-0.08	-1.11	454
	(0.07)	(0.12)	(0.18)		
Brahmin‡	0.21	0.14	0.07	0.45	487
	(0.21)	(0.06)	(0.16)		
LN(Firewood Collection Time)	3.65	4.66	-1.02	-5.3***	487
	(3.43)	(3.18)	(5.63)		
Wealth Indicators					
Number of Rooms	3.58	3.07	0.51	0.96	487
	(0.49)	(0.29)	(0.53)		
House has Electricity‡	0.90	0.88	0.02	0.21	487
	(0.004)	(0.05)	(0.08)		
Improved Toilet‡	0.18	0.21	0.04	-0.30	487
	(0.07)	(0.07)	(0.05)		
Floor†	1.41	1.55	-0.14	-0.43	487
	(0.41)	(0.14)	(0.33)		
Walls†	1.39	1.76	-0.37	-1.85	487
	(0.18)	(0.11)	(0.19)		

‡ No=0; Yes=1. † Impermeable=1; semi-permeable=2; permeable=3

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Block-level Data from Indian Censuses of 1991 and 2001 on Female Bargaining Power

Variables	Untreated	Treated	Difference	t-test	Observations
Sex Ratio (M/F)	1.02	0.99	-0.02	-0.52	47
	(0.03)	(0.02)	(0.04)		
Sex Ratio 0-6 (M/F)	1.07	1.05	0.02	1.76	47
	(0.01)	(0.01)	(0.01)		
Ratio of Scheduled Caste Pop (M/F)	1.06	1.04	0.02	0.61	47
	(0.03)	(0.01)	(0.03)		
Ratio of Scheduled Tribe Pop (M/F)†	0.34	0.32	0.01	0.08	47
	(0.14)	(0.09)	(0.17)		
Literacy Ratio (M/F)†	1.05	1.08	-0.03	-1.86	47
	(0.01)	(0.01)	(0.02)		
Ratio of Total Workers (M/F)†	1.05	1.01	0.04	1.75	47
	(0.02)	(0.01)	(0.03)		
Ratio of Main Workers (M/F)†	1.09	1.04	0.05	1.73	47
	(0.03)	(0.01)	(0.03)		
Ratio of Non-workers (M/F)	0.87	0.95	-0.09	-1.57	47
	(0.05)	(0.02)	(0.05)		

†The distributions of the underlying variables for this ratio were significantly different from normal; they were thus logged and the ratio of the resultant variables was used here.

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Participants and Non-participants in Districts with *Mahila Samakhya*

Variables	Non-part.	Part.	Difference	t-test	Observations
Demographics					
Spousal Age Ratio	0.84	0.86	-0.03	-2.53**	303
	(0.01)	(0.01)	(0.01)		
Age at Marriage	18.48	19.17	-0.69	1.63	327
	(0.38)	(0.21)	(0.42)		
Age of Sons	7.26	8.97	-1.71	-1.81	345
	(0.77)	(0.50)	(0.95)		
Age of Daughters	6.33	6.54	-0.21	-0.25	345
	(0.73)	(0.44)	(0.84)		
Number of Sons	1.16	1.37	-0.21	-1.87	345
	(0.09)	(0.06)	(0.11)		
Number of Daughters	0.98	1.14	-0.16	-1.31	345
	(0.08)	(0.07)	(0.12)		
Own-to-husband's education	0.66	0.58	0.07	1.38	345
	(0.05)	(0.03)	(0.05)		
Low Education‡	0.29	0.31	-0.14	-0.26	345
	(0.05)	(0.03)	(0.06)		
Lives with Husband‡	0.85	0.67	0.19	3.10**	312
	(0.04)	(0.03)	(0.06)		
Lives with In-laws‡	0.55	0.44	0.12	1.90	345
	(0.05)	(0.03)	(0.06)		
Works‡	0.52	0.59	-0.06	-1.02	336
	(0.05)	(0.03)	(0.06)		
Brahmin‡	0.05	0.21	-0.16	-3.51***	347
	(0.02)	(0.03)	(0.04)		
LN(Firewood Collection Time)	3.91	4.91	-0.99	-4.39***	347
	(0.22)	(0.11)	(0.23)		
Wealth Indicators					
Number of Rooms	3.09	3.30	-0.21	-0.81	345
	(0.21)	(0.13)	(0.26)		
House Has Electricity	‡0.89	0.89	0.00	0.01	345
	(0.03)	(0.02)	(0.04)		
Improved Toilet‡	0.28	0.26	0.02	0.35	345
	(0.03)	(0.05)	(0.06)		
Floor†	1.63	1.86	-0.23	-1.71	345
	(0.08)	(0.11)	(0.14)		
Walls†	1.77	1.81	-0.03	-0.25	345
	(0.08)	(0.10)	(0.13)		

‡No=0; Yes=1. †Impermeable=1; semi-permeable=2; permeable=3

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Does Participation in *Mahila Samakhya* Empower Women?

Method	Has NREGS Card	Can Go Out W/o Permission	Council Meetings
Intent-to-Treat	0.313	0.488	-0.07
	$(0.13)^{**}$	$(0.15)^{*}$	(-0.049)
2SLS Estimate	1.475	0.367	0.487
	$(0.26)^{***}$	$(0.15)^{*}$	$(0.487)^{***}$
PSM	0.665	0.433	0.088
	$(0.14)^{***}$	$(0.16)^{**}$	(0.14)
Truncation-corrected PSM	0.678	0.429	0.098
(women who do not work)	$(0.13)^{***}$	$(0.16)^{**}$	(0.14)

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Robust First Stage Estimates

	(1) Program Participation
Participation Instrument	0.033***
	(0.00)
Own Age	0.001
	(0.00)
Spousal Age Ratio	0.171
	(0.27)
Literate	0.054
	(0.07)
Less Than Four Years of Education	0.010
	(0.08)
Own Years of Education	−0.003
	(−0.01)
Number of Children	0.066
	(0.04)
Age of Children	−0.006
	(0.01)
Brahmin	0.309***
	(0.05)
Lives with In-laws	−0.103*
	(0.05)
Lives with Sister-in-law	−0.524
	(0.42)
LN(Firewood Collection Time)	0.040***
	(0.01)
Number of Rooms	0.004
	(0.01)
House Has Electricity	−0.076
	(0.06)
Constant	0.022
	(0.26)

Standard errors statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Does Participation in *Mahila Samakhya* Empower Women? (Robust 2SLS Estimates)

	(1) Has NREGS Card	(2) Can Go Out W/o Permission	(3) Village Council Participation
Participation	1.475***	0.367*	0.487***
	(0.26)	(0.15)	(0.14)
Own Age	−0.005	−0.002	0.006*
	(0.01)	(0.00)	(0.00)
Spousal Age Ratio	−0.026	−0.499	−0.224
	(0.41)	(0.28)	(0.26)
Less than Four Years of Education	0.002	−0.008	−0.002
	(0.01)	(0.01)	(0.01)
Literate	−0.116	−0.092	0.005
	(0.12)	(0.06)	(0.07)
Own Years of Education	0.001	0.007	−0.002
	(0.02)	(0.01)	(0.01)
Number of Children	−0.122	0.008	−0.028
	(0.07)	(0.04)	(0.04)
Age of Children	0.016	0.004	0.003
	(0.01)	(0.00)	(0.00)
Brahmin	−0.588***	−0.032	−0.065
	(0.12)	(0.07)	(0.07)
Lives with In-laws	0.172	−0.044	0.015
	(0.09)	(0.06)	(0.06)
Lives with Sister-in-law	0.902	1.012*	0.905
	(0.76)	(0.44)	(0.47)
LN(Firewood Collection Time)	−0.034	−0.015	0.008
	(0.02)	(0.01)	(0.01)
Number of Rooms	0.007	−0.018	0.001
	(0.02)	(0.01)	(0.01)
House Has Electricity	0.147	0.239***	0.077
	(0.11)	(0.07)	(0.07)
Constant	−0.099	1.068***	−0.082
	(0.42)	(0.27)	(0.25)
Observations	421	391	411
First-stage F-stat	13.55	12.38	11.90
p-value for Sargan's test	0.579	0.242	0.803

Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: Does Participation in *Mahila Samakhya* Increase Reservation Wages? (Robust 2SLS Estimates)

	Women Who Do Not Work			*Women Without NREGS Cards*	
	(1)	(2)	(3)	(4)	(5)
	Can Go Out W/o Perm. Has NREGS Card	Village Council Part. Can Go Out W/o Perm.	Has NREGS Card Village Council Part.	Can Go Out W/o Perm. Can Go Out W/o Perm.	Village Council Part. Village Council Part.
Participation	2.220**	0.034	−0.082	1.134**	0.638*
	(0.87)	(0.40)	(0.35)	(0.47)	(0.38)
Own age	−0.012	−0.000	0.018***	−0.008	0.002
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Spousal Age Ratio	−0.430	−0.642	−0.731*	−1.011	−0.231
	(0.92)	(0.48)	(0.44)	(0.65)	(0.43)
<4 Years of Ed.	0.397	0.059	0.011	−0.150	0.087
	(0.41)	(0.17)	(0.17)	(0.22)	(0.18)
Literate	−0.210	−0.098	0.070	0.169	−0.147
	(0.35)	(0.11)	(0.14)	(0.20)	(0.15)
Own Years of Ed.	0.040	0.000	−0.006	−0.015	0.004
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)
Number of Children	−0.175	0.046	−0.016	−0.034	−0.073
	(0.15)	(0.08)	(0.07)	(0.06)	(0.06)
Age of Children	0.006	0.003	0.019**	0.009	0.014*
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Brahmin	−1.215***	−0.032	0.196	−0.371*	−0.227
	(0.42)	(0.21)	(0.18)	(0.22)	(0.15)
Lives with In-laws	0.393**	−0.057	0.081	0.195	0.136
	(0.20)	(0.09)	(0.09)	(0.15)	(0.11)
Lives with Sis.-in-law	2.166	1.504**	0.442	0.925	0.407
	(1.78)	(0.75)	(0.86)	(0.85)	(0.64)
LN(Firewood Time)	−0.064	−0.008	0.020	−0.069**	0.002
	(0.06)	(0.03)	(0.02)	(0.03)	(0.02)
Number of Rooms	0.062*	−0.012	−0.016	−0.044	−0.028
	(0.04)	(0.02)	(0.02)	(0.04)	(0.02)
House Has Electricity	0.119	0.290**	0.138	0.480***	0.087
	(0.22)	(0.12)	(0.10)	(0.18)	(0.13)
Constant	−0.473	1.107**	0.072	1.376*	0.029
	(0.98)	(0.44)	(0.41)	(0.72)	(0.39)
Observations	178	163	175	144	157
First-stage F-stat	8.54	8.03	8.54	11.75	7.27
p-value for Sargan's test	0.391	0.818	0.908	0.511	0.569

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10: Average Treatment Effects from 2SLS Estimates

Average Treatment Effect	
NREGS	0.818
Can Go Out W/O Permission	0.204
Village Council	0.269
Average Treatment Effect on Participants	
NREGS	0.893
Can Go Out W/O Permission	0.222
Village Council	0.295
Average Treatment Effect on Non-participants	
NREGS	0.643
Can Go Out W/O Permission	0.159
Village Council	0.212
Average Treatment Effect on Untreated	
NREGS	0.621
Can Go Out W/O Permission	0.155
Village Council	0.205

Table 11: Does Participation in *Mahila Samakhya* Empower Women? (PSM Results)

	Non-Participants vs. the Untreated				
Unmatched	Spousal Age Ratio	Has NREGS Card	Can Go Out W/o Permission	Council Meetings	Respondent Works
Non-participants	0.835	0.719	0.671	0.152	0.641
Untreated	0.849	0.256	0.578	0.0986	0.523
Difference	-0.015	0.463	0.093	0.053	0.118
	(0.014)	(0.071)***	(0.081)	(0.055)	(0.097)
Matched	Spousal Age Ratio	Has NREGS Card	Can Go Out W/o Permission	Council Meetings	Respondent Works
Non-participants	0.835	0.719	0.671	0.152	0.641
Untreated	0.839	0.634	0.747	0.089	0.297
Difference	-0.004	0.085	-0.076	0.063	0.344
	(0.033)	(0.213)	(0.221)	(0.148)	(0.031)
Observations	160	160	143	150	108
	Participants vs. the Untreated				
Unmatched	Spousal Age Ratio	Has NREGS Card	Can Go Out W/o Permission	Council Meetings	Respondent Works
Participants	0.862	0.809	0.793	0.502	0.581
Untreated	0.850	0.177	0.654	0.205	0.440
Difference	0.0119	0.632	0.139	0.297	0.141
	(0.009)	(0.044)***	(0.052)**	(0.06)***	(0.06)**
Matched	Spousal Age Ratio	Has NREGS Card	Can Go Out W/o Permission	Council Meetings	Respondent Works
Participants	0.862	0.809	0.793	0.502	0.581
Untreated	0.848	0.144	0.361	0.414	0.321
Difference	0.014	0.665	0.433	0.088	0.261
	(0.026)	(0.142)***	(0.158)**	(0.142)	(0.172)
Observations	341	339	312	332	315

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 12: Does Participation in *Mahila Samakhya* Empower Women Who Do Not Work? (PSM Results)

	Participants vs. the Untreated		
Unmatched	Has NREGS Card	Can Go Out W/o Permission	Council Meetings
Participants	0.730	0.667	0.416
Untreated	0.179	0.708	0.315
Difference	0.552	-0.042	0.101
	$(0.072)^{***}$	(0.085)	(0.084)
Matched	Has NREGS Card	Can Go Out W/o Permission	Council Meetings
Participants	0.730	0.667	0.416
Untreated	0.101	0.500	0.146
Difference	0.629	0.167	0.269
	$(0.118)^{***}$	(0.184)	$(0.132)^{*}$
Observations	145	132	143

Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 13: Does Participation in *Mahila Samakhya* Empower Women Who Do Not Have NREGS Cards? (PSM Results)

	Participants vs. the Untreated	
Unmatched	Can Go Out W/o Permission	Council Meetings
Participants	0.821	0.525
Untreated	0.675	0.189
Difference	0.146	0.336
	(0.087)	$(0.081)^{***}$
Matched	Can Go Out W/o Permission	Council Meetings
Participants	0.821	0.525
Untreated	0.308	0.025
Difference	0.513	0.500
	$(0.263)^{*}$	$(0.144)^{***}$
Observations	122	135

Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 14: Does Participation in *Mahila Samakhya* Empower Women Who Do Not Work? (Truncation-corrected PSM Results)

	Non-Participants vs. the Untreated				
Unmatched	Spousal Age Ratio	Has NREGS Card	Can Go Out W/o Permission	Council Meetings	Respondent Works
Non-participants	0.822	0.667	0.722	0.130	0.579
Untreated	0.824	0.147	0.629	0.100	0.440
Difference	-0.003	0.519	0.093	0.030	0.139
	(0.029)	(0.110)***	(0.146)	(0.089)	(0.060)***
Matched	Spousal Age Ratio	Has NREGS Card	Can Go Out W/o Permission	Council Meetings	Respondent Works
Non-participants	0.822	0.667	0.722	0.130	0.579
Untreated	0.729	0.125	0.611	0.000	0.341
Difference	0.093	0.54	0.111	0.130	0.238
	(0.075)	(0.254)**	(0.213)	(0.072)	(0.168)
Observations	58	58	45	53	44
	Participants vs. the Untreated				
Unmatched	Spousal Age Ratio	Has NREGS Card	Can Go Out W/o Permission	Council Meetings	Respondent Works
Participants	0.861	0.808	0.797	0.505	0.582
Untreated	0.851	0.177	0.654	0.205	0.427
Difference	0.01	0.631	0.143	0.299	0.155
	(0.009)	(0.044)***	(0.052)**	(0.06)***	(0.061)**
Matched	Spousal Age Ratio	Has NREGS Card	Can Go Out W/o Permission	Council Meetings	Respondent Works
Participants	0.861	0.808	0.797	0.505	0.582
Untreated	0.846	0.131	0.367	0.407	0.647
Difference	0.015	0.678	0.429	0.098	-0.066
	(0.025)	(0.132)***	(0.156)**	(0.142)	(0.192)
Observations	340	338	311	331	314

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$