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# Being Patient with Microfinance: The Impact of Training on Indi- an Self Help Groups

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## BEING PATIENT WITH MICROFINANCE: THE IMPACT OF TRAINING ON INDIAN SELF HELP GROUPS

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# Being Patient with Microfinance: The Impact of Training on Indian Self Help Groups

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## Abstract

We evaluate the impact of training provided by facilitators of Self Help Groups (SHGs). This evaluation provides one of the first studies of the impact of ‘microfinance plus,’ or the disbursement of services beyond credit. Indian SHGs are mainly NGO-formed microfinance groups but funded by commercial banks. We correct for membership selection bias with data on current as well as future SHG members. We then account for potential training endogeneity with propensity score matching. Regression and unadjusted matching results indicate that training does not aid in asset accumulation but can reverse the negative impact of credit on income. However, regression adjusted matching which controls for both participation and training selection bias reveals that training impacts assets but not income. These results are robust to sensitivity analyses performed on these estimates.

JEL Classification Numbers: G21, I32, O12.

Keywords: India, microfinance, training, impact studies, Self Help Groups.

## **I. Introduction**

Recently, in India, Self Help Groups (SHGs) have emerged as a serious alternative to private microfinance institutions (MFIs). Recent figures indicate that SHG members (47.1 million) comprise more than three times those of MFI members (14.1 million) (Srinivasan, 2009). In a recent impact and sustainability study of the SHG Bank Linkage Program, NCAER (2008) claims that SHGs have significantly improved the access to financial services of the rural poor. The NCAER study also finds that training improves members' skills such as communication, marketing, and human development. However, the study does not indicate whether the training translated into better outcomes.

This paper aims to explore whether training services have had their intended impact. It tests this objective using a unique data set from five Indian states with SHGs. The data were not only collected on current members and non-members but also on newly enlisted SHG members who have not yet received loans. We examine whether training affects outcomes over and above membership (which measures loan access). We employ two different outcome measures, a long term (assets) and a short term (income).

Why are we interested in the effects of training? As Karlan and Valdivia (2009) note, one would like to know if MFIs should teach skills. Some state that households already have the human capital and only need financial capital. Others claim that MFIs must also provide training, as households cannot effectively use the financial capital that they receive. Furthermore, since MFIs have already organized borrowers in order to obtain loans, the cost of providing additional services is small. A natural tension arises for MFIs on whether they should branch out to training or just lend.

Similarly, the impact of training on SHG members is also interesting in the light of the 'minimalist' and 'microfinance plus' debate. Believers of 'microfinance plus' combine the provision of credit with other important inputs like literacy training, farming inputs or business development services (Morduch, 2000). 'Minimalists' however argue that for MFIs to become sustainable and reach a viable scale they should only provide financial services. By investigating the impact of training on SHGs, we contribute to this ongoing discussion.

An argument for MFIs to focus on lending is that membership by itself 'trains' participants in a number of ways. First, by working, saving, and repaying, members adopt a disciplinary ethic. Second, by actually working on projects, members 'learn by doing' without any need of training. Third, regular meetings provide a setting for members to discuss and

learn from others about their work-related problems. Our data allows us to discern the effects of training from that of membership. We have data on new members (with no external credit) as well as mature members, thus controlling for member self-selection. We also have training data on the members, where not all mature and new members receive training.

The paper first examines the impact of training on assets and income. We have deliberately chosen short term and long term variables to measure outcome.<sup>1</sup> We first correct for participation bias with a pipeline method. We then use both matching and regression adjusted methods to adjust for both training and participation bias. Finally, we test the sensitivity of the results to unobservables.

The regression results (which correct for membership bias only) reveal that training has no effect on assets, but positively impacts income. We also find that the amount of training has no impact on these outcome measures. We observe a similar impact when we account for training endogeneity only using matching methods. However, when we correct for both membership selection bias and training bias, training positively impacts assets but has no impact on income. These results indicate that for SHGs, training may not translate into positive effects immediately but over time, they can help borrowers graduate from poverty.

This paper contributes to the literature of impact studies in microfinance in both the methods and the topic studied. In its methods, it corrects for two different types of selection bias by combining two nonexperimental methods: the pipeline and matching methods. Coleman (1999) proposed the pipeline approach, where he compares current members to future members who have not yet received loans. We adapt Coleman's approach to the SHG framework. While Coleman surveyed borrowers in both treatment and control villages, we observe new and mature groups in SHGs in different villages but in the same district.<sup>2</sup> We then use matching methods to control for training endogeneity. We propose the combination of these methods since for our data and setting, randomization is not feasible. In the absence of randomization, these methods provide an alternative for measuring impact.

Various authors have conducted a number of studies on the impact of SHGs with the NCAER (2008) report the most recent. Their report on 4,600 households from six states in India measures impact as the compound annual growth rate of the outcome measure from pre to post SHG participation. They find that income increases 6 %, assets increase 10 %, and

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<sup>1</sup> We will explore the impact on other outcome measures such as health and education in future work.

<sup>2</sup> See Karlan and Goldberg (2006) for a review of the major studies and their methodologies. Another influential paper on microfinance impact is Pitt and Khandker (1998) which relies on Grameen's eligibility rule (although see the recent rebuttal by Morduch and Roodman (2009)).

participants are more empowered.<sup>3</sup> EDA Rural Systems (2006) finds similar positive impacts. Throughout this paper, we will draw on these studies as they offer important insights into the functioning of SHGs and provides information on training and some other aspects of SHGs not covered in our data. Also, the NCAER study overlaps with our study in five of the six states (we do not have data on Assam).

Clearly, the NCAER analysis does not account for any changes in characteristics or broad economic changes through a control group. Furthermore, their study does not measure the effect of training per se except to note its inadequacy. Still, due to the scarcity of studies on SHGs, these types of studies have had much policy influence, widely quoted in a number of Reserve Bank of India and NABARD (India's agricultural development bank) documents. New insights in the methodology of impact studies can improve upon the previous studies of such an important credit institution.

Measuring the impact of training in general has spawned a large amount of literature, summarized in LaLonde (1995). Training has actually provided the subject matter for a healthy debate on the effectiveness of matching estimators (see for example Smith and Todd, 2005). The preponderance of the work has used data from developed countries and in particular, from one training program from the US. When accounting properly for selection bias, the studies find a positive impact of training. In contrast to this paper on SHGs, these studies do not need to account for participation choice in a particular group before training choice.

In terms of measuring the training impact specifically on MFIs, Karlan and Valdivia (2009) provide the only rigorous study. Using the popular randomization method with data from Peru, they find that business training improved business practices and revenues. They also find that this increased knowledge led to greater repayments and client retention. They do not separately measure the effects of membership, so their results hold conditional on membership. Furthermore, due to their reliance on randomization the study is weak on external validity since it studies only one program in one specific place. In sum, for SHGs, microfinance and in general, for developing countries, not many studies have analyzed the contribution of training to outcome measures. Still, millions of rupees are spent on training,

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<sup>3</sup> Until the aforementioned NCAER study, impact studies on SHGs consisted mainly of the Puhazendhi and Badataya study (2002) commissioned by NABARD (India's rural development bank) with 115 members from three states. Their results find that SHG membership significantly increases the asset structure (30 %), savings, and annual net income.

where sixty percent of SHGs rely on outside support for training (NCAER, 2008). An analysis of the impact of training can reveal whether this expenditure is worthwhile.

For those unfamiliar with SHGs, in the next section, we outline the basic information, design, and training. Section three discusses the methodology and explains potential biases. In the fourth section, we describe our data set with the results presented in Section Five. The last section concludes and draws policy lessons.

## **II. Self Help Groups and Training**

Self Help Groups fall under the category of village banking which includes ten to twenty (primarily female) members. In the initial months the group members save and lend amongst themselves and thus build group discipline. Once the group demonstrates stability and financial discipline for six months, it receives loans of up to four times the amount it has saved.<sup>4</sup> The bank then disburses the loan and the group decides how to manage the loan. As savings increase through the group's life, the group accesses a greater amount of loans.

Initiated in 1992, the SHG program faced slow progress up to 2000. Our data, for members who joined from 2000, reflects the impact of this expansion strategy. Since then, the SHG program has mushroomed, growing to financing 687,000 SHGs in 2006-07 alone compared to 198,000 SHGs in 2001-02. The cumulative number of SHGs has grown to roughly three million by March 2007 reaching out to more than forty million families. According to NABARD (2006), 44,000 branches of 547 banks and 4,896 NGOs participate in the SHG bank linkage program. As with microfinance in India (or more generally with credit), the spread of SHGs has been concentrated in the Southern states.

As of March 2002, the cumulative number of linked SHGs in five states covered in this study follow a similar pattern. For these five states, their shares (in parentheses) of the cumulative SHG links are the following: Andhra Pradesh (48.5), Tamil Nadu (12.5), Uttar Pradesh (6.6), Orissa (4.1), and Maharashtra (3.9). Well aware of the concentrated spread of SHGs, NABARD recently has identified thirteen poorer states in which they would like to expand their program. As the program is predominantly rural, NABARD has recently identified urban areas for experimentation. Both of these initiatives demonstrate NABARD's confidence in the program and call for a careful examination of the program's impact.

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<sup>4</sup> More recently, NABARD has allowed the banks to lend five to six times the savings amount to reflect the growing requirement of SHG members.



The SHG program links with the poor through Self Help Group Promoting Institutions (SHPIs), which primarily includes NGOs, but also banks, and government officials. The agencies survey the village, provide the details of the program, enlist borrowers, and sometimes organize the training. Three types of linkages have emerged as the most common. In Linkage Model 1, banks both form and finance SHGs. According to NABARD (2006), roughly twenty percent of SHGs fall under this linkage model. In the most popular linkage model 2 (roughly three-fourths of all SHGs), NGOs and others form the groups but banks directly finance them. In the third linkage model banks finance the SHGs through NGOs (but only 5 % of linkages fall under this model).<sup>5</sup>

The program features of small loan size, frequent meetings, frequent repayment installments, and savings dissuade the non-poor from joining SHGs. Thus, SHGs do not use explicit eligibility criteria but rather rely on indirect methods for attracting the poor. In many SHGs, SHPIs provide training and outreach to members in fields such as primary healthcare, basic literacy, family planning, marketing, and occupational skills. The NCAER study finds that even after four years after formation, 61 % of SHGs are still dependent on these SHPIs.

Training and capacity formation can be broadly classified into two categories.<sup>6</sup> General training to all SHG members covers group formation and an introduction to linkage methods, which includes basic literacy, bookkeeping, group formation and dynamics.<sup>7</sup> The general training usually takes one day and each participant is awarded Rs. 250 (equivalent to a week of agricultural wages, a generous compensation). Since all participants receive this relatively homogeneous training, we do not include this aspect in our training measure.

The additional training module (the focus of our paper) relates to other types of training. These include skill formation training which aims at improving income-generating activities such as farming, craft or business. SHG members can demand the required skill training. However, their demand is not met in many cases because the viability of the training sessions require a critical number of potential trainees to make the ‘demanded’ training program cost effective. Moreover, SHPIs need to find local trainers for that specific skill.

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<sup>5</sup> In our data, 70 % of SHGs follow Model 2 while 12 % and 18 %, respectively, follow the first and third models.

<sup>6</sup> Public information on the SHG training program is unavailable, The discussion below is our attempt to distill the information in a concise manner drawing from different sources. Much of this information was provided through visits with NABARD’s regional office in Bhubaneswar, Orissa. Some of this information is also available through NABARD circulars.

<sup>7</sup> More specifically it includes training on group formation and functioning; functions and qualification of office bearers; rules and regulations; planning, management and monitoring; financial service provisions, conditions and procedures; training of group leaders; and training of book keepers.

Since some of the demand is internally driven, members participate out of interest and need. Actually, many members other than those that initially request the training participate in the sessions. Furthermore, NABARD's stipend provides an added incentive to participate.

Skill formation programs include the REDP (Rural Entrepreneurial Development Program), designed for unemployed but educated rural youth.<sup>8</sup> Other programs such as Reach provides tools with training to improve their business planning and take advantages of new opportunities. Other than the REDP, some SHPIs also provide additional education, health, and awareness creation training. In contrast to the base training, these additional modules of training are more haphazard and not homogenous or focused. Thus, the training covered in this paper is 'as delivered' and not optimal in any sense. This notion of training contrasts with the Karlan-Valdivia study where meetings started with training, and with penalties such as fees for tardiness and threat of expulsion.<sup>9</sup>

Different camps have touted the relative advantages of SHGs over private MFIs in training delivery. Defenders of NGO linked SHGs (linkage model 2) assert the following. First, NGOs perform effective development activities within their own district and so are the best equipped to provide training services. They do not need an extra incentive mechanism to monitor and train SHGs (as suggested by the detractors). If NGOs choose to deny training services to a particular group, then the NGOs have identified that group as low quality. At times, members initiate the training which differs from many standard Grameen style models. Furthermore, to a certain extent, the group self-governs as the elected office bearers (such as the president and treasurer) maintain the financial records. These officers are usually more educated and share the records with the group.

In contrast to SHGs, several MFIs are donor-driven and face pressure to obtain high repayment rates which may stifle their training efforts. In particular, training may have payoffs later but add to costs now and may damage current outcome measures. Since the government supports the SHG program with a development mission in mind, it may not face the same pressure. Overall, the SHG model reflects an institutional approach, while the private MFIs reflect a more market oriented outlook. We turn now to examine whether training impacts borrowers.

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<sup>8</sup> The MEDP (Microenterprise Development Program) began only after 2006, which is after our data had been collected. However, it will be discussed more at length in the conclusion.

<sup>9</sup> Even with these conditions, Karlan and Valdivia (2009) found many detractors who chose not to attend the training sessions.

### III. Estimation Strategy

In assessing impact, researchers seek to disentangle the causal effect from the potential selection bias.<sup>10</sup> In particular, the decision to participate in SHGs and training depends on the same attributes that determine the impact variable (asset accumulation and income in this paper). Randomized studies have led the revolution in the new microeconometrics of development (Banerjee and Duflo, 2009). More recently, many researchers have questioned the validity of randomization as the gold standard in impact studies (summarized in Deaton, 2010). The data and nature of the SHG program preclude randomization as a viable option. In their overview on the benefits of randomization, Banerjee and Duflo (2009) carefully discuss when randomized experiments are appropriate. They argue that randomized experiments are particularly strong choices when implemented on a small scale with new interventions. Along with Heckman (1992), they note that the interpretation of randomized experiment becomes inappropriate if one is interested in evaluating the impact of the program over the population. As a wide scale national program, the SHG program falls into this category. One could envision a carefully constructed experiment in one village with one program but an immediate question would arise in its generalizability to other SHG programs.

Randomization also usually studies short term impact measures while here we are also interested in longer term impact. One cannot hold a control group without training (and/or credit) for long (as noted by Karlan and Goldberg (2006)). Thus, for the above reasons randomization was not adopted as the impact methodology. A second strategy (as adopted by Pitt and Khandker (1998)) exploits an exclusion rule on credit access to estimate unbiased impact. However, SHGs follow no such exogenous rule. Even if one were to find such an exclusion rule, many do not adhere to full compliance with results subject to controversy, as noted by Morduch and Roodman (2009). The inapplicability of randomization and exclusion rules for SHGs leads us to explore alternative avenues to evaluate the impact of training for SHG members.

In our framework we encounter a double selection problem: into participation and training. We will first establish the correction for selection into the program and later discuss the treatment of training endogeneity. We correct for the first selection bias using a pipeline

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<sup>10</sup> Selection bias in impact studies on microfinance has been discussed at length in Karlan and Goldberg (2006) and Coleman (1999). More generally, see the recent book by Angrist and Pischke (2009).

method and account for the second using matching methods. With the pipeline approach, at the data design stage Coleman (1999) interviewed both current and future members of a village bank. Since the credit decision is exogenous to the households, both types of members should have similar unobservables, thus correcting for member self selection.

Adapting this pipeline method to the SHG program requires some modifications. Our treatment and control villages reside in the same district. We have data from districts where some members are currently active members of SHGs for at least one year but in the same district (but different villages), members from newly formed SHGs have been selected but not yet received financial services from the bank.<sup>11</sup> NABARD's choice to expand the SHG program occurs at the district phase without any specific announced policy targeting certain villages over others. Thus, we choose to aggregate at the district level, the basic administrative unit within a state where credit decisions are made. Ideally, one would choose a control group from the same village (which would hold all external conditions constant) but then earlier signees of SHGs may have different reasons for joining than later signees.

Old SHGs are households who have been availing themselves of the program and they are recipients of the loans. New SHGs are new members who have passed the pre-selection test of being "SHG worthy." By design, SHG members have to wait to receive a loan from the bank (about six months) and we can exploit this design feature to identify the self-selected members who have not yet received a loan. Since these households have not yet received the benefits of the loans, they serve as a control group since they are pre-selected on attributes but have not received the benefits.<sup>12</sup> Our method differs from Coleman (1999) in that he intentionally interviewed people at the data design stage while we exploit the wait some households face.

The modified pipeline method that we adopt is in effect 'phased in randomization,' a randomization method suggested by Banerjee and Duflo (2009) with limited resources. In this method, experimenters delay the randomization over time. Similarly, here borrowers are

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<sup>11</sup> One caveat of this approach is that we need to assume the behavior of new SHG members has not changed while anticipating loans. In other words, while awaiting loans, SHG members do not begin asset accumulating knowing that they will receive SHG loans in the future. An advantage of SHGs is the following. Due to the slow incubation period of SHGs, members know for some time the nature of wait and will not change their behavior as radically as a one time boost in loans.

<sup>12</sup> To check for differences in the observable characteristics for old and new SHGs, we ran regressions of the following type:  $X_{ijs} = \alpha D_s + \beta M_{ijs} + \gamma T_{ijs}$  where  $X_{ijs}$  is the observable characteristic,  $D_s$  is a vector of district dummies,  $M_{ijs}$  is a member dummy which takes a value one for members and zero otherwise,  $T_{ijs}$  is a treatment variable which takes on the value one for old SHGs and zero for new SHGs. Thus, the significance of  $\alpha$  indicates any difference over and beyond district and self-selection differences. The results (available from the authors upon request) indicate that none of the variables were significant. The results from the observable characteristics also lend support to the idea that old and new SHGs are not very different.

pre-selected but denied access to loans for some time. By adopting the pipeline method, we adopt the core concept of a randomized experiment, while avoiding its limitations. Basically, we believe this avenue offers the best evaluation tool considering the nature of the SHG program.

<Insert Table 1 here>

For the village level, we first present evidence on the observables. Table 1, column (1) estimates a logit regression for old and new SHGs at the village level. Note that none of the village level variables are significant.<sup>13</sup> We have also confirmed these results with conversations with NABARD officials who assert, that conditional on district choice, they randomly choose the villages for old and new SHG placement.

What about NGOs, do they favor certain types of villages earlier than others for linkages? First, NGOs operate within villages without anticipation of a linkage, i.e. they move independently of the SHG linkage following their own development work. Second, by comparing linkage models (since some groups are bank formed and some are NGO formed), we do not find a discernible difference in linkage choice of villages with old and new SHG members.<sup>14</sup> Overall, we do not find any evidence of NGOs favoring certain villages first over others.

A remaining concern prevalent in impact studies is the issue of dropouts. We did not track data on dropouts but both the NCAER and EDA studies did. The dropout rate for SHGs is not severe in that the NCAER study estimated the dropout rate as 8.2 % (while the EDA study found the rate around 9.8 %), below the 20-30 % cited by Aghion and Morduch (2005) and Karlan (2001) as a severe problem. Additionally, this dropout rate was calculated for SHGs with an average age of 5.4 years, nearly double our average age, so we conservatively estimate that the dropout rate in our data as below 5 %. The NCAER study did find marginally a greater dropout rate for the poor. Thus, the mature SHGs potentially consists of a better off sample resulting in a slight over estimate of impact.

We still need to account for nonmembers from these districts who may be availing themselves of district specific policies, such as parallel government programs. We control for these differences with the use of district fixed effects. In that there may be district wide spillover effects from mature to new members and non-members, the estimates here would

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<sup>13</sup> We also ran this regression for the village level variables that we chose to use for our eventual impact regression and virtually find the same results.

<sup>14</sup> For the (new) old SHGs the proportions were the following for the three linkages: Linkage 1 (13.6)11.2; Linkage 2 (71.7) 72.6; Linkage 3 (14.7)16.2. A two sample t-test of proportions confirmed no difference between the two.

underestimate that impact. To account for the remaining village level variability, we employ village level characteristics.

As mentioned in the earlier section, the SHPIs provide basic training to all SHGs. Then, the SHPIs organizes additional training for some of the SHGs. the training variable ( $T_{ijs}$ ) indicates whether the household received such training. Thus, this variable captures whether training has impact beyond membership duration and self selection of the members.

Keeping in mind the outlined procedure, we estimate the following regression:

$$I_{ijs} = a + \alpha X_{ijs} + \beta V_{js} + \lambda D_s + \gamma M_{ijs} + \delta SGHMON_{ijs} + \Pi T_{ijs} + \eta_{ijs} \quad (1)$$

Where  $I_{ijs}$  is the impact for household is measured in terms of asset accumulation or income generation, for household  $i$  in village  $j$  and district  $s$ ,  $X_{ijs}$  are the household characteristics;  $V_{js}$  is a vector of village-level characteristics, and  $D_s$  is a vector of district dummies that control for any district level difference. Here,  $M_{ijs}$  is the membership dummy variable, which controls for the selection bias. It takes the value one for both mature and new SHGs. It takes the value of zero for those villagers that have chosen not to access the program. Here,  $SGHMON_{ijs}$  is the number of months that SHG credit was available to mature members, which is exogenous to the households. The parameter of interest is  $\Pi$  which measures the impact of training. We now discuss how we address the selection bias of the trainees.

Training placement, as anticipated, is more complicated than actual program placement. One can conjecture that more remote villages are less likely to have training programs. In fact, we have examined through logit regressions, a check on the observables, and find in Table 1, column (2), that distance from paved roads affects training program location, as well as level of male wages. Somewhat surprisingly, the greater the distance from the bus stop, the more likely a training program, indicating that for villagers other means of transport are more important than buses.<sup>15</sup>

As described in the section on SHGs, actual training delivery must pass a three step process. Only in the first step, the household takes part by requesting training. In practice, some households who did not initially demand training, may take advantage of a training session in their village and attend. The other two steps of finding a trainer and hoping for a critical mass of trainees does not lie within the household's choice. The strength of

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<sup>15</sup> We also estimated separate logit regressions for new and old SHG villages. Results were for similar but for new SHGs, the presence of health clinic made training placement less likely.

endogeneity for training participation contrasts sharply with the vast literature on US public training programs where trainees play more of a role in their choice of participation.

As mentioned previously, for training endogeneity we use propensity score matching and then test the sensitivity of our results to unobservables.<sup>16</sup> In contrast to regression methods, propensity score does not depend on linearity and has a weaker assumption on the error term. We first examine the viability of using propensity score matching for this data set. Heckman *et al.* (1997) (hereafter HIT) have outlined three intuitive conditions for the least bias in this method. One, the survey questionnaire should be the same for participants and non-participants so that the outcome measures are measured the same for both. Two, both should come from the same local labor markets. Three, the availability of a rich set of observables for both outcome and participation variables. Our data set satisfies all three conditions, the first and third as described in the data section and for the second, both treatment and control households reside in the same districts.

For the households in this data set, the estimators match the households who received training to those who did not. Except for the treatment, the households are very similar. Thus, any differential can be attributed to the impact. Households with low or high probabilities cannot be matched and generally, these are dropped. In matching terminology, we keep the households on the common support. The probability ( $P(X)$ ) of being selected is first determined by a logit equation and then this probability (the propensity score) is used to match the households. Denote  $Y_1$  as the outcome variable of interest for those with training ( $T=1$ ), and denote  $Y_0$  as the outcome variable of interest for those without training ( $T=0$ ), then equation (2) denotes the mean impact of training:

$$\Delta = E[Y_1 | T = 1, P(X)] - E[Y_0 | T = 0, P(X)] \quad (2)$$

where the matched comparison group provides the data to calculate the second term, and the propensity score weights the whole expression for all households on common support.

For our purposes, we employ a version of matching which combines elements of regression. These regression adjusted matching estimators as in Barnow *et al.* (1980) allow for different covariates for the logit participation equation and the outcome equation. In our case these estimates are particularly important because of the need to account for the selection of participation into the program in which we use the pipeline method.<sup>17</sup> The following

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<sup>16</sup> See the excellent survey by Caliendo and Kopeinig (2008) on the main issues on propensity score matching.

<sup>17</sup> We are aware that this specific type of selection is actually a sequential or dynamic selection process. In other words, the subsequent choice of training depends upon the effect of participation on income or assets. But as

procedure explains the steps for regression adjusted matching estimators. First, run a regression for the outcome equation on the no training group  $Y_0 = x\beta + \varepsilon$ . We then calculate the fitted values.<sup>18</sup> Second, subtract these values from the outcome variables for both the no training and training group (since these fitted values are free of the effect of training). Third, match the new variables, outcome variables minus the fitted values. The estimator is given by equation (3):

$$\Delta RAM = \sum_{j=1}^T w_j (Y_{j1} - x_j \hat{\beta}_0) - \sum_{i=1}^C W_i (Y_{i0} - x_i \hat{\beta}_0) \quad (3)$$

where RAM refers to regression adjusted matching estimators, T (C) refers to the total number of treated (not treated), and  $w$  ( $W$ ) refers to the particular weight used in matching for the treatment (control).

We also have to choose which matching algorithms to use. Since the probability of two households being matched exactly is close to zero, distance measures are used to match households. Following Smith and Todd (2005), we first choose the neighbor to neighbor (NN) algorithm. This algorithm is the most straightforward and matches partners according to their propensity score. We employ both one and ten person matching, where the latter uses more information to match the partners. The NN algorithm is only used for simple (or unadjusted) matching since its performance is not well known in regression adjusted matching.

For regression adjusted matching, we turn to the local linear regression (LLR) method (for bandwidths 1 and 4). The theorems in HIT which justify regression adjusted matching are based on LLR, a generalized version of kernel matching which allows faster convergence at the boundary points. The LLR method uses the weighted average of nearly all individuals in the control group to construct the counterfactual outcome. For regression adjusted matching, the analytical standard errors are tedious to compute. We use bootstrapped standard errors for the LLR procedures since these are not subject to the general criticism of the use of bootstrap standard errors in matching models (see Abadie and Imbens, 2007 and HIT, 1997).

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Caliendo and Kopeinig (2008) state: ‘practical experiences with sequential matching estimators are rather limited’ we estimate the static framework with matching for the training selection problem.

<sup>18</sup> HIT suggest a semi-parametric procedure which exploits a richer functional form. We attempted to fit this from our data with two candidates, age and SHGMON. We failed to reject the null hypothesis of linearity: P=0.664 and P=0.552 respectively for age and SHGMON.



## IV. Data

The data used for the empirical analysis in this paper were collected by one of the authors and forms part of a larger study that investigates the SHG-bank linkage program.<sup>19</sup> The household survey uses pre-coded questionnaire to collect cross-sectional data for two representative districts each, from five states in India, for the year 2003.<sup>20</sup> Furthermore, the years of SHG membership was restricted to three years or less. The reason was the following. First, the SHG movement as mentioned in the SHG description section only took off in 2000 and was not as well structured before 2000. Second, selecting SHGs of three years or less minimizes attrition and dropouts. Third, in order to measure the impact of a long term variable such as assets, one needs more than one or two years of participation. The sampling strategy randomly chose the respondents from the SHG members at the district level. The non-members were chosen to reflect a comparable socio-economic group as the SHG respondents.

For this particular study, the collected data was further refined. Of the total respondents, 114 were from villages with no SHGs. Since these households were not provided the opportunity to self-select, these were dropped. Sixty old and new SHG respondents were from the same village and this would contaminate the sample since the earlier signees may be of a different makeup than the later signees. Of the remaining sample, 593 respondents are from mature SHGs, 185 are from new SHGs, and 51 are non-members.

For SHGMON, or the number of months since a member has joined a SHG, we made the following adjustments. Since an SHG links with banks only six months after formation, we needed to account for those six months of no credit access. Almost all the new SHG respondents in our data had been members for less than six months and for these SHGMON=0. Only fourteen of these new respondents were members for more than six months, in which case SHGMON = date of formation - six months.<sup>21</sup> For the mature SHGs,

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<sup>19</sup> The process involved discussion with statisticians, economists and practitioners at the stage of sampling design, preparing pre-coded questionnaires, translation and pilot testing with at least 20 households in each of the 5 states (200 households in total). The questionnaires were then revised, reprinted and the data collected by local surveyors that were trained and supervised by the supervisors. The standard checks were applied both on the field and during the data punching process.

<sup>20</sup> These states (districts in parentheses) are Orissa (Koraput and Rayagada), Andhra Pradesh (Medak and Warangal), Tamil Nadu (Dharamapuri and Villupuram), Uttar Pradesh (Allahabad and Rae Bareli), and Maharashtra (Gadchiroli and Chandrapur).

<sup>21</sup> These respondents have been a SHG member for less than a year and have been identified as a new member in the SHG list available at the district level.

their SHGMON = date of formation - six months. Some mature SHG respondents (forty six) did not report the date of their SHG formation. For these households, we used the number of the months since they received the first SHG loan for SHGMON.

The data were not collected specifically for a training study. An advantage of this approach is that respondents do not overemphasize training and answer training questions subsumed in their other questions. A disadvantage is that some specific answers to training are missing. We primarily have information on the total training weeks that a household has received. We set the training variable to 1 for all households who reported positive weeks of training. Since both mature members and new members received training, we can differentiate the impact of training from that of loan access.

The survey yields other measures of training. When comparing the means and variances of the training weeks for mature and new SHGs we find a significant difference: the amount of training weeks (1.52 versus 1.15) and variability in training is larger for mature SHGs (2.42 versus 1.87).<sup>22</sup> About half (48 per cent) of the mature SHGs received training while 39 percent of the new SHGs reported the same.<sup>23</sup> These statistics are not surprising in that the longer length of membership of mature SHGs will provide them with more opportunities for training. Surprisingly, a sizable percentage of new SHGs receive training indicating a new commitment by policymakers.

Table 2 compares the characteristics of households who received training to those that did not. In general, those who received training were wealthier, had higher income, were older and lived in villages closer to paved roads and further from the market. These variables indicate that either more prosperous households receive training (who probably are not the target group of SHPIs) or training actually made households more prosperous. A t-test for the same variables before they received training and became members yield a similar difference: -4.34 for income and -2.26 for assets.<sup>24</sup> Still, we will need to condition on the full set of covariates and control for member self-selection in order to properly study the full impact of training.

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<sup>22</sup> A t-test with unequal variances revealed a t-ratio of 3.32 statistically significant at the 1 % level.

<sup>23</sup> NCAER(2008) also finds that nearly half of all the SHGs have had skill development training. About 35 per cent of the households received training only once in 2006 and another 15 per cent have received training multiple times.

<sup>24</sup> The data from the year 2000 is recall data and thus may have some measurement errors. Thus, we chose not to use that data for difference in difference estimators.

<Insert Table 2 here >

As suggested by Doss *et al.* (2007), we accumulate assets from six categories: land owned, livestock wealth, dwelling and ponds, productive assets, physical assets, and financial assets (includes savings and lending). Household income includes income from agriculture, poultry and livestock, wages, fisheries and forest resources, rent, remittances, and enterprise. Household characteristics include age, gender, education dummies and number of earning members in the family. We also include dependency ratios in that we expect households with larger dependency ratios to have greater (lesser) incentive for asset accumulation (income generation). In order to control for initial wealth, we employ land owned three years ago.<sup>25</sup> For village characteristics, in addition to male wage, we include the following distance variables: paved road, market, primary health care center, and bus-stop. Table 3 presents the non-training related descriptive statistics of the independent and dependent variables, respectively.

<Insert Table 3 here>

SHG members and non-members are about the same age, dependency ratio, similar level of education and a higher amount of land on average. In terms of village level variables, members are closer to most amenities but not surprisingly non-members are relatively closer to banks. On other variables, we further find that members on average have a relatively higher income, own more land and dwelling but have slightly lower amount of assets. Combining these statistics with those of Table 1 indicate that SHG members who had training have higher income and possibly higher amount of assets. This selection bias will affect the results.

## V. Results

This section presents and discusses the estimation results for the training impact of the SHG bank linkage program on asset accumulation and income. We first examine the results through regression methods since these serve as points of departure. Furthermore, these can be fully interpreted, along with the impacts of the covariates and interactions. We then compare these results to those obtained through matching methods. Table 4 provides the regression

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<sup>25</sup> Since land forms the bulk of assets and land turnover is infrequent in India (see Pitt-Khandker, 1998, for more discussion on this observation), this variable was the best choice for initial wealth.

results of Equation (1) for the impact of training on assets and income.<sup>26</sup> In Columns (1) and (2), we examine the impact of the binary training variable.

<Insert Table 4 here>

Column (1) indicates that membership duration matters for asset accumulation and training does not provide any additional benefit.<sup>27</sup> Presumably, the in-built savings mechanism within the SHG framework provides sufficient incentives for members to build assets with training providing no additional advantage.<sup>28</sup> The results in column (2) indicate that surprisingly membership duration negatively affects income creation (though the point estimates are small). Thus, a trade-off emerges for SHG members to choose between short term income creation and long term asset creation. Moreover, training can reverse the negative impact by positively increasing the income generation of the household. Roughly, up to three years of membership can be reversed with training (assuming constant returns to membership).<sup>29</sup>

Of the household characteristics, we find that the dependency ratio positively impacts asset accumulation but negatively affects income generation. Households with a greater number of dependents have greater interest in creating assets than income. Education carries the expected signs in that households with greater education are more adept at asset creation (since no education is the dropped dummy) and less interested in current income generation. Initial wealth (as in the amount of land holdings) also clearly influences the current asset and income position of a household. Of the village characteristics, distance from paved road (as expected negatively) and distance from the bus stop are significant (though somewhat surprisingly positively) for assets but no influence on income.

We now test the robustness of our results to the endogeneity of the training variable. As discussed in the methodology section, matching methods take into account the selection bias from training. A parsimonious logit equation determines the probability of participating

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<sup>26</sup> The censoring of the income variable was roughly 7 %. We estimated Tobit regressions both with and without robust standard errors which yielded similar results.

<sup>27</sup> In related work, we examine the impact of SHG participation. We find that not including the membership dummy would actually erroneously indicate no impact of length of SHG membership leading one to conclude that length of participation has no impact on assets.

<sup>28</sup> We have also estimated regressions with alternative asset specifications: gross assets minus SHG savings and also with net assets (gross assets – other borrowings). The results were substantially the same.

<sup>29</sup> We also estimated an alternative specification testing whether training had greater impact on older SHG members. We failed to find any significance.

in training.<sup>30</sup> Covariate candidates are variables that influence both the participation and the outcome variable and ones that are not affected by participation and its anticipation. The variables were chosen through a statistical significance and ‘hit or miss’ method and at the same time keeping the balancing in mind (see Caliendo and Kopeinig, 2008)). We chose to include: age, age squared, gender, education dummies, shock in 2000, distance from bank, health care center, marketplace, and paved road, linkage model 2 and interaction of age and model 2. We will discuss the sensitivity of the results to the variables chosen later.

< Insert Table 5 here >

We then matched the treated and comparison group based on the propensity score which controls for training endogeneity only. The results in Table 5 confirm the regression results in terms of significance. We find no impact of training on assets but we find impact on income using both the local linear regression and neighbor to neighbor techniques. These estimates, however, do not take into account member selection bias and only take into account training endogeneity. The regression adjusted matching results in columns (3) and (4) take into account member selection bias. The impact effects reverse with the training impact on assets significant while the impact on income as not significant.

< Insert Table 6 here >

In Table 6, we compare the different estimates. In column (1), the unadjusted t-stat difference suggests that training impacts both assets and income strongly. With regression covariates, the impact of training on income falls by one half and the significance on assets disappears. The matching estimates suggest a greater impact of training but does not correct for participation bias. Finally, the regression adjusted estimates indicate a greater and significant impact of training on assets (on the order of 16 %) while the impact on income disappears. These results indicate that households that received training had already higher income but that training did aid in their asset accumulation.

We can now compare the impact of membership to that of training. We have separately run a large number of regression estimates (with the pipeline method) in measuring the impact of membership on assets and consistently find point estimates similar to those

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<sup>30</sup> The issue of a simple versus a quasi-saturated logit model is a contentious one. As noted by many, though, the purpose of the logit equation here is not only to predict training participation (as in selection models) but also for covariate balancing.

found in the regression estimates of Table 4. These results indicate that membership (evaluated at SHGMON means for mature members) provide a return of 15 % on assets. From the regression adjusted estimates above, training can double these returns. These estimates provide a partial resolution at least in this context to the question posed in the introduction of whether MFIs should only focus on lending. They should not. The regression estimates on income here and elsewhere suggest that membership has a zero or negative impact on income. The regression adjusted estimates of income also indicate that training has no impact either.

The lack of impact on income generation contrasts sharply with the impact on asset accumulation. Within the SHG program, the loans are not necessarily bound to be used for the productive purposes and hence there may not provide a positive impact on income in the short run. As NABARD (1992) states, "... the purposes for which the group lends to the members will be left to the group." Secondly, Bali Swain and Varghese (2009) shows that SHG participation leads to a movement away from agriculture to livestock raising, thus indicating a transitional loss in current agricultural income but a gain in assets.

Furthermore, the in-built savings requirement of the program and training will help asset accumulation immediately but may not translate into an immediate impact on income. The results suggest patience in training's impact. Movement away from agriculture and developing alternate sources of income might take time but training helps provide discipline in asset accumulation that could transmit in results later. The estimates here echo a recent large scale randomized study from Indian slums where microfinance participation has had no impact on current variables such as consumption but borrowers have moved towards consuming more durable goods and starting new businesses (Banerjee, *et al.*, 2009). Actually generating income from new businesses might take an extra time due to the new skills, uncertainty in business, and reliance on external markets. These reasons are still not fully understood in microfinance research.

## **VI. Sensitivity Analyses**

In this section, we perform sensitivity analysis of various types to explore the robustness of the regression adjusted matching results. We predominantly examine sensitivity to the inclusion of unobservables. We first explore the sensitivity of the results in terms of the

pscore specification and the matching algorithms.<sup>31</sup> In terms of the pscore specification, in general, when the logit equation is even more parsimonious than the one specified even excluding village level characteristics, the impact disappears. This result arises from the simplicity of the propensity score which does not correctly provide a proper match.

When we add a large number of variables to the logit equation, we encounter balancing problems. Thus, our chosen equation balances the two but is robust to the addition and subtraction of a few variables. We have also used kernel algorithms for the matching and regression adjusted matching with different bandwidths yielding similar results. Finally, since the bootstrapped standard errors are not analytical, we ran the matching results a number of times for a check of the robustness of the bootstrapped standard errors.

Since for the conditional independence assumption, selection relies on observables, we tested the sensitivity of our results to the inclusion of unobservables (Ichino *et al.*, 2007). We have already discussed how our data meets the three conditions where the Conditional Independence Assumption appears plausible. However, propensity score matching hinges on the conditional independence or unconfoundedness assumption and unobserved variables that affect the participation and the outcome variable simultaneously, may lead to a hidden bias due to which the matching estimators may not be robust. The data cannot directly reject the unconfoundedness assumption but Heckman and Hotz (1989) and Rosenbaum (1987) have developed indirect ways of assessing this assumption. These methods rely on estimating a causal effect that is known to be equal to zero. If the test suggests that this causal effect differs from zero, the unconfoundedness assumption is considered less plausible.

Building on Rosenbaum (1987) and others, Ichino, Mealli and Nannicini (2007) propose a sensitivity analysis. They suggest that if the CIA is not satisfied given observables but is satisfied if one could observe an additional binary variable (confounder), then this potential confounder could be simulated in the data and used as an additional covariate in combination with the preferred matching estimator. The comparison of the estimates obtained with and without matching on the simulated confounder shows to what extent the baseline results are robust to specific sources of failure of the CIA, since the distribution of the simulated variable can be constructed to capture different hypotheses on the nature of potential confounding factors.

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<sup>31</sup> All of these results are available from the authors.

To check the robustness of our ATT estimates we use two covariates to simulate the confounder: young (respondents under the age of 26 years) and education (with no education). These covariates are chosen with the intention to capture the effect of unobservables like ability, entrepreneurial skills, and risk aversion which have an impact on both participation in the training program and assets and income of the household. If the estimates change dramatically with respect to the confounders, then it would imply that our results are not robust.

<Insert Table 7 here>

Since our outcome variables are continuous, the confounder is simulated on the binary transformation of the outcome median. The results of these two covariates to simulate confounders are presented in Table 7 for both assets and income. Note that for both variables, the selection effect is not significant. The results indicate robustness of the regression adjusted results with respect to the confounder.

## **VI. Conclusion**

In this paper, we evaluated the impact of training in Self Help Groups on two outcome measures, income and assets. Using regression adjusted matching methods, we find that training impacts assets and not income. These results are consonant with parallel work where we find that membership positively impacts asset creation and not income. The impact of training on assets reveal that training strengthens members' skills in savings and asset accumulation. The lack of impact on income indicates that much more needs to be established for income generation. For example, marketable goods, infrastructure, and other factors play a part and that paradoxically, the effects on income generation may take more time than asset accumulation.

We now comment on future directions, both in terms of research and policy. In terms of the survey, even though the data provides the best to date on training for SHGs, more work needs to be done for data collection. One, our measure of quantity of training is provided in weeks, if one were to obtain a finer measure such as hours that may provide different results. Two, a better distinction of the types of training programs would help differentiate the ones that had most impact. Three, in future work we will examine the relationship between softer skills of training such as education and its impact on other outcome measures such as schooling. Though this type of training may incur costs now, it has future payoffs.



In terms of implementation, according to NCAER (2008), more than eighty per cent of the SHGs face problems in developing the skills of their members. Major reasons cited are: lack of time, lack of interest, inadequate literacy among members and insufficient training facilities. The SHGs in all the states suggested that the SHPIs allow more time in training and group discussions. They further require support from financial institutions in training on book keeping, reviewing and advice on SHG financial activities and health. Furthermore, the training program is not homogenized and varies by NGOs so it is difficult to grade the quality of the training program.

Two recent implementations offer future improvements for training programs. In a recent microenterprise study (Nussbaum *et al.* , 2005)), trainers employed by SHGs were asked for feedback on how the training program could be improved. These same trainers were then asked to conduct training programs based on their insights. Recipients perceived these programs as much higher quality. Another program initiated by the SHG program itself is Microenterprise Development Program (MEDP) which began in 2006 and thus does not impact this data set. This training program targets skill development for mature SHGs. Here, the initial demand for skill training comes from the SHGs and the SHPIAs apply for grants to impart the relevant skill training. Another appealing aspect of this program is that the length of the training is limited to two weeks and can also be a minimum of three days. Future data collection on this program can evaluate its impact.

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TABLE 1

*Logit estimates of Placement of SHG programs and Training Programs ( $\times 10^{-2}$ )*

<i>Village Level Variables(in kms. unless noted)</i>	<i>(1) SHG placement</i>	<i>(2) Training</i>
Distance Block	-2.19 (0.98)	-2.77 (1.25)
Distance haat	-0.45 (0.05)	4.02 (0.62)
Distance Paved Rd	-11.55 (0.98)	-36.3 (2.58)***
Distance Bank	1.19 (0.37)	4.521 (1.19)
Distance Market	7.02 (0.78)	-3.92 (0.56)
Distance HealthCare	-0.156 (0.02)	-4.56 (0.65)
Distance Bus Stop	11.30 (1.08)	38.69 (2.93)***
Male Wage (Rupees)	1.78 (1.30)	-2.47 (1.73)*
Female Wage (Rupees)	-2.96 (1.44)	3.43 (1.20)

*Notes:* \*\*\* Significant at the 1 % level. \*\* Significant at the 5 % level. \* Significant at the 10 % level. All regressions include district fixed effects. Analysis based on 220 observations. Absolute t-ratios in parentheses computed with White heteroskedasticity-consistent standard errors.

TABLE 2

*Training t-tests*

<i>Variable Name</i>	<i>No Training (T=0)</i>	<i>Training (T=1)</i>	<i>T-test for equality of means</i>
	<i>Mean (S.D.)</i>	<i>Mean (S.D.)</i>	
N	367	474	---
Gross Assets (Rs.)	94535 (127418)	126710 (163750)	-3.11***
Income (Rs.)	13805 (14394)	19656 (18549)	-4.99***
Months in SHG	17 (16.05)	21.20 (15.33)	-3.95***
Age (yrs.)	33.76 (7.76)	35.87 (9.0)	-3.58***
Gender (Female=1)	0.95 (0.22)	0.95 (0.22)	0.21
Dep. Ratio	0.66 (0.21)	0.66 (0.22)	0.12
No Education	0.51 (0.50)	0.57 (0.50)	-1.75*
Primary Ed.	0.19 (0.39)	0.17 (0.37)	0.68
Secondary Ed.	0.20 (0.40)	0.14 (0.35)	2.30**
College Ed.	0.03 (0.16)	0.04 (0.19)	-0.86
Owned Land in 2000 (acres)	0.66 (1.24)	1.10 (1.61)	-4.32***
Distance from Paved Road (kms.)	3.22 (3.80)	2.85 (2.55)	1.67**
Distance from Bank (kms.)	7.26 (7.73)	7.45 (5.60)	-0.42
Distance from Market (kms.)	5.13 (4.16)	5.72 (3.81)	-2.14**
Distance from Healthcare (kms.)	3.49 (2.99)	3.62 (2.63)	-0.68
Distance from Bus Stop (kms.)	3.61 (3.72)	3.92 (3.31)	-1.26
Male Wage (Rs.)	46.32 (16.04)	46.25 (13.09)	0.07

*Notes:*\*\*\* Significant at 1 % level. \*\* Significant at 5 % level. \* Significant at 10 % level.

TABLE 3

*Non- training related descriptive statistics*

<i>Variable Name</i>	<i>Mature SHGs</i>	<i>New SHGs</i>	<i>Non-Members</i>
	<i>Mean (S.D)</i>	<i>Mean (S.D.)</i>	<i>Mean (S.D.)</i>
N	604	186	51
Gross Assets (Rs.)	109423 (145763)	104933 (136447)	111818 (170171)
Income (Rs.)	16841 (16458)	15460 (17942)	13905 (12269)
Months in SHG	26 (13)	0.31 (1.34)	0
Age (yrs.)	35.2 (8.70)	32.6 5(7.30)	35.60 (8.08)
Gender (Female=1)	0.96 (0.20)	0.92 (0.27)	0.96 (0.20)
Dep. Ratio	0.66 (0.22)	0.69 (0.19)	0.62 (0.23)
No Education	0.51 (0.50)	0.60 (0.50)	0.51 (0.50)
Primary Ed.	0.20 (0.40)	0.12 (0.33)	0.24 (0.43)
Secondary Ed.	0.17 (0.38)	0.18 (0.39)	0.12 (0.33)
College Ed.	0.03 (0.17)	0.04 (0.19)	0.02 (0.14)
Owned Land in 2000 (acres)	0.86 (1.43)	0.89 (1.50)	0.48 (1.12)
Distance Paved Road (kms.)	3.04 (3.43)	2.95 (2.99)	3.60 (3.04)
Distance from Bank (kms.)	7.90 (7.40)	6.30 (5.70)	4.96 (3.20)
Distance from Market (kms.)	5.70 (4.20)	4.34 (3.50)	5.50 (3.20)
Distance from Healthcare (kms.)	3.40 (2.64)	3.61 (3.21)	5.00 (3.30)
Distance from Bus Stop (kms.)	3.80 (3.70)	3.36 (3.15)	4.71 (2.80)
Male Wage (Rs.)	46.00 (12.41)	45.00 (20.00)	54.71 (16.40)



TABLE 4

*Regression estimates of impact of training on asset creation and income ( $\times 10^{-2}$ )*

	(1) <i>Gross Assets</i>	(2) <i>Income</i>	(3) <i>Gross Assets</i>	(4) <i>Income</i>
Member	-459.02 (2.32)**	19.38 (0.92)	-437.71 (2.28)**	25.48 (1.24)
SHGMON	6.34 (1.93)*	-0.74(1.68)*	6.37 (1.92)*	-0.72 (1.66)*
Training (Yes=1)	108.99 (1.18)	27.13 (1.83)*	----	----
Weeks of Training	----	----	14.87 (0.76)	3.01 (1.03)
Age	1.17 (0.20)	1.28 (2.08)**	1.25 (0.21)	1.31 (2.13)**
Gender (Female=1)	101.17 (0.76)	-0.53 (0.02)	100.84 (0.76)	-0.88(0.03)
Dep. Ratio	402.15 (2.13)**	-109.8 (3.32)***	403.56 (2.15)**	-109.7(3.32)***
Primary Ed.	234.22 (1.92)*	-19.28 (1.10)	233.06 (1.90)*	-19.58 (1.11)
Secondary Ed.	292.87 (2.43)**	-33.15 (2.22)**	287.54 (2.36)***	-34.32(2.31)**
College Ed.	566.93 (2.09)**	-55.65 (1.70)*	567.37 (2.09)***	-55.50 (1.69)*
Land 3 years ago	423.55 (7.89)***	16.04 (2.74)***	426.00 (7.99)***	16.73(2.86)***
Distance Paved Rd.	-74.96 (2.43)***	-0.27(0.08)	-77.53 (2.53)**	-1.04(0.30)
Distance Bank (kms.)	8.33 (0.72)	-0.92 (0.72)	7.93 (0.69)	-1.06(0.81)
Distance Market	-17.59 (1.57)	-0.002(0.00)	-18.22 (1.62)	-0.14(0.06)
Distance HealthCare	16.65 (0.68)	-1.83(0.66)	17.86 (0.72)	-1.54(0.55)
Distance Bus Stop	46.92 (1.53)	-1.03 (0.32)	47.91 (1.58)	-0.63(0.19)
Male Wage	-4.93 (1.07)	-0.02(0.03)	-4.85 (1.05)	-0.003(0.01)

*Notes:* \*\*\* Significant at the 1 % level. \*\* Significant at the 5 % level. \* Significant at the 10 % level. All regressions include district fixed effects. Analysis based on 841 observations. Absolute t-ratios in parentheses computed with White heteroskedasticity-consistent standard errors clustered by village. See text for definitions of variables.

TABLE 5

*Matching and regression adjusted matching estimates of training impact on assets and income ( $\times 10^{-2}$ )*

<i>Matching Algorithm</i>	<i>(1) Gross Assets</i>	<i>(2) Income</i>	<i>(3) Gross Assets (Regression Adjusted)<sup>a</sup></i>	<i>(4) Income (Regression Adjusted)</i>
1 NN (S.E.)	176.50 (1.23)	42.34** (2.59)	-----	-----
10 NN (S.E.)	212.76* (1.92)	47.18** (3.75)	-----	-----
LLR (bw 1) (S.E.)	165.61 (1.49)	49.72** (3.54)	201.24** (1.99)	8.15 (0.60)
LLR (bw 4) (S.E.)	165.61 (1.52)	49.72** (3.83)	201.24** (2.12)	8.15 (0.64)

*Notes:* \*\* Significant at the 5 % level. \* Significant at the 10 % level. NN = neighbor to neighbor, t-stats in parentheses. LLR= local linear regression, p-values in parentheses standard errors created by bootstrap replications of 200 replications. <sup>a</sup>Covariates of regression same at Table 3, (1) and (2), omitting the training variable. See text for definitions of variables.

TABLE 6

*Comparison of estimates of training impact on assets and income( $\times 10^{-2}$ )*

<i>Variable</i>	<i>(1) Unadjusted (T-test)</i>	<i>(2) Regression</i>	<i>(3) Matching (LLR, bw 1)</i>	<i>(4) Regression Adjusted Matching (LLR, bw 1)</i>
Assets	321.75**(3.11)	108.99 (1.18)	165.61(1.49)	201.24** (1.99)
Income	58.51**(4.99)	27.13** (2.11)	49.72** (3.54)	8.15 (0.64)

*Notes:* \*\* Significant at the 5 % level. \* Significant at the 10 % level. For (1) and (2), t-stats in parentheses. For columns (3) and (4), p-values with bootstrap standard errors of 200 replications. (1) is the simple t-test comparison. Column (2) is from Table 3, columns (1) and (2). (3) and (4) are from Table 5.

TABLE 7

*Simulation-Based Sensitivity Analysis for Matching Estimators†*  
*General multiple-imputation standard errors ( $\times 10^{-2}$ )††*

<i>Variable/Covariate simulated confounder</i>	<i>for</i>	<i>(1) ATT</i>	<i>(2) Standard Error</i>	<i>(3) Outcome effect</i>	<i>(4) Selection effect</i>
<b>Assets</b>					
Age		144.78	6.64	1.24	0.78
Education		140.55	7.23	1.27	1.31
<b>Income</b>					
Age		4.51	0.89	1.03	0.77
Education		4.28	0.76	1.07	1.27

*Notes:* † Based on the sensitivity analysis with kernel matching algorithm with between-imputation standard error. The binary transformation of the outcome is along the median. †† Age variable (=1 if age is less than 26 years; and = 0 otherwise) and education (=1 if no education; and zero otherwise).

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