Evaluating the Impact of Training in a National Microfinance Program: Self Help Groups in India

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Abstract

This paper evaluates the impact of widespread training programs provided by the Self Help Group (SHG) program. Indian SHGs are mainly NGO-formed microfinance groups but funded by commercial banks. The paper employs evaluation techniques appropriate for current borrowers of a national program. Additionally, the paper addresses the double selection issue of membership and training. We correct for membership selection bias with a pipeline method. We then account for training endogeneity with propensity score matching. The results of regression adjusted matching (which controls for both participation and training selection bias) reveal that training aids in asset accumulation but not income generation. Specialized training such as business training has a greater impact on assets than general training. Sensitivity analyses also confirm the robustness of these results.

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I. Introduction

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). An integral component of SHG programs is the provision of widespread training programs. Policymakers would like to assess the impact of these popular training programs for this important national microfinance program which was initiated over ten years ago. Researchers face the difficulty of properly evaluating the SHG programs both due to their national scale and measuring impact on current borrowers. However, up to now impact studies of SHGs have limited themselves to pre-post evaluations, which do not pass minimum evaluation criteria (NCAER, 2008). In this paper we will provide a practical methodology which will measure the impact on current borrowers in a national program. [1]

In previous work (Author (2009)), we have explored the impact of SHG membership alone and find that participation helps assets but not income.[2] In this study, our motive is to explore the impact of training as well as specialized training. 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 focus on two different outcome measures in this paper, assets and income.[3]

Other than the amount of resources devoted to training, why are we interested in its effects? As Karlan and Valdivia (2009) note, one would like to know whether 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

[1] In private conversations with senior Indian policymakers, they have asked for methods of evaluation other than randomized control trials (RCTs).

[2] In complementary work (Author (2011)), we have focused on delivery mechanisms of training.

[3] In future work, we will explore the impact on other outcome measures such as health and education.

cannot effectively use the financial capital that they receive. Furthermore, since MFIs have already organized borrowers and have a mechanism to deliver loans, the cost of providing additional services is small. A natural tension arises for MFIs on whether they should also provide training or just limit themselves to financial services.

Similarly, the impact of training on SHG members can shed light on 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 sustainability and viability, MFIs should only provide financial services. As an argument for MFIs to focus on lending, membership by itself 'trains' participants in a number of ways. First, by saving, borrowing, working, meeting regularly 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 one to discern the effects of training from that of membership. We have data on new members (from new SHGs that have not been credit-linked) 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.

We first correct for participation bias with a pipeline method (i.e. some borrowers receive loans before others). We then use both matching and regression adjusted methods to adjust for both training and participation bias. Finally, sensitivity analysis tests the robustness of the results to unobservables. The pipeline-only results (which correct for membership bias only) reveal that membership positively affects assets, but negatively impacts income. When we correct for both membership and training endogeneity, training also positively impacts assets but has no impact on income. The type of training matters in that business training has greater impact especially when coupled with a specific linkage model of the SHG program. These results indicate that for SHGs, training may not immediately translate into positive effects but over time, the program can help borrowers graduate from poverty.

The paper contributes to the microfinance literature, both methodologically and by providing empirical evidence for resolving the 'Minimalist' or Microfinance 'Plus' approach. In its methods, it corrects for a double selection bias by combining two nonexperimental approaches: the pipeline method and propensity score matching. Adapting Coleman's (1999) pipeline approach to the SHG framework, the study observes new and mature groups in SHGs in different villages but within the same district. Matching methods control for training endogeneity. Combining these methods provides a method for properly measuring impact.[4]

Although various authors have conducted a number of studies on SHGs, none have systematically addressed selection issues (apart from Deininger and Liu, 2009). In particular, despite the spate of training programs in India, none have measured the impact of training in the literature except to note its inadequacy. Still, due to the scarcity of studies on SHGs, many of the descriptive studies have had much policy influence, and are widely quoted in a number of Reserve Bank of India and National Bank for Agriculture and Rural Development (NABARD, India's agricultural development bank) documents.

In terms of measuring the training impact specifically on MFIs, recently a number of studies have conducted randomized trials (Karlan and Valdivia, 2009).[5] They find that business training improved business practices and revenues and led to greater repayments and client retention. As the effects of membership are not measured separately, their results hold conditional on membership. 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.

[4] After completing our work, we have found that Deininger and Liu (2009) use similar methods for SHG groups though their study covers only one state, Andhra Pradesh. Since this state is the most microfinance thick state in India, their results are atypical (they find no impact on consumption and assets). Even though their study was a large scale World Bank funded study, they also find they cannot rely on randomization.

[5] For more recent work, see Bruhn, Karlan, and Schoar (2010) and others.

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 within the group and thus build group solidarity. Once the group demonstrates stability and financial discipline for six months, it receives loans of up to four times the amount it has saved. 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 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 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 per cent of linkages fall under this model).[6]

Training and capacity formation are broadly categorized into general and skill development training.[7] General training is imparted to all SHG members which educates and introduces them to group formation and linkage methods.[8] Since all participants receive this relatively homogeneous training, this aspect is not included in our training measure. The skill training module relates to other types of training and is the focus of our paper. This specialized training aims at improving income-generating activities such as farming, craft or

[6] In our data, 70 per cent of SHGs follow Model 2 while 12 per cent and 18 per cent, respectively, follow the first and third models.

[7] Public information and microfinance literature on the SHG training program is limited. The discussion below provides the first survey to our knowledge of the training programs offered to SHGs. Much of this information was provided through visits with NABARD's regional office in Bhubaneswar, Orissa, and supplemented by NABARD circulars.

[8] 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.

micro and small business. SHG members can demand the required skill training. However, their demand may not be met every time as 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.

Since 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. Training groups may consist of thirty but sometimes even eighty members. NABARD's stipend also provides an added incentive to participate. The group's log books reveal whether a member attended the training or not.

Skill formation programs also include government programs such as the Rural Entrepreneurial Development Program (REDP), designed for unemployed but educated rural youth.[9] The REDP has been in existence for over fourteen years. Training lasts for over two weeks, sometimes up to two months. As of March 2007, NABARD claims to have supported 8,356 REDP training programs with financial assistance of 400 million rupees covering 216,000 youth.[10] The training skills vary from soft skills such as spoken English, communication skills, computer awareness to vocational skills such as plumbing, marketing, and even staffing call centers .

Due to the different demands for training by the groups (members), the imparted modules of skill training will differ across groups. The paper thus measures whether overall skill development training has had a positive impact and whether business development training in particular leads to an increase in income and assets. In other words, the training covered in this paper is 'as delivered' and not optimal in any sense. This notion of training contrasts with the Karlan-Valdivia randomized control trial study where meetings began with training, and the MFI used deterrents such as fees for tardiness and threat of expulsion.[11]

[9] 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.

[10] Rupees 400 million is about 7.2 million US dollars at current exchange rates.

[11] 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, the causal effect needs to be separated from the potential selection bias. In particular, the decision to participate in SHGs and training depends on the same attributes that determine the outcome (asset accumulation and income in this paper). Randomized Control Trials (RCTs) are currently the most popular method for evaluation but lack external validity, critical for proper evaluation of the SHG program.[12] The data collected for the study and nature of the SHG program itself preclude randomization as a viable option.

The argument rests on two fundamental limitations of randomization: first, since the SHG program is a wide scale national program, randomization does not provide the proper interpretation of the impact over a population (Heckman, 1992). One could envision a carefully constructed experiment in one village with one program but an immediate question would arise about its generalizability to other SHG programs. Second, the number of SHGs by 2007 have already grown to three million reaching forty million families and thus policymakers anxiously await evaluating the impact on the existing borrowers (Srinivasan, 2009). RCTs limit the evaluation of a national program on existing borrowers, thus prompting the need to explore other options.[13]

In our framework we encounter a double selection problem: program participation and training. The pipeline method corrects for the first type of selection bias while matching methods accounted for the second bias. The pipeline approach as implemented by Coleman involves interviewing both current and future members of a village bank. Since the credit decision is exogenous to the household, both types of members should have similar

[12] In their overview on the benefits of randomization, Banerjee and Duflo (2009) carefully discuss where randomized experiments are appropriate. They argue that randomized experiments are particularly strong choices when implemented on a small scale with new interventions. Several researchers have questioned the validity of randomization. For a critical summary, refer to Deaton (2010).

[13] A related issue is that RCTs explore the impact on marginal new borrowers and withholding loans for new borrowers. For impact on a long term outcome such as assets this is problematic. Second, one has to hope that the marginal borrower is similar to the previous borrowers (before loans). Third, additional concern and political pressure may be generated by holding for long a control group without training (and/or credit) for long and itsimplementation very difficult.

unobservables, thus correcting for member self-selection. In adapting the pipeline method to the SHG program, we note that the treatment and control villages in our data reside within 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.[14] 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.

Mature SHG members have been linked to SHPIs for over a year and 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 this design feature is exploited to identify the self-selected members who have not yet received a loan. Since these households have not yet been credit linked and are awaiting loans, they serve as a control group. They choose to join the program and are accepted on attributes but have not received credit from the SHPIs.[15]

In order to check the observables for non-random program placement, we estimate a logit regression for SHGs and training placement. Table 1, column (1) estimates a logit regression for mature and new SHGs at the village level. Note that none of the village level

[14] 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.

[15] To check for differences in the observable characteristics for old and new SHGs, we ran regressions of the following type: Xijs = αDs + βMijs + γTijs where Xijs is the observable characteristic, Ds is a vector of district dummies, Mijs is a member dummy which takes a value one for members and zero otherwise, Tijs is a treatment variable which takes on the value one for old SHGs and zero for new SHGs. Thus, the significance of 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.

variables are significant.[16] We have also confirmed these results with conversations with NABARD officials who assert, that conditional on district choice, they randomly choose the villages for mature and new SHG placement. What about NGOs, do they favor certain types of villages earlier than others for to link with SHGs? First, NGOs operate within villages without anticipating a SHG 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), no discernible difference is found in linkage choice of villages between mature and new SHG members.[17]

<Insert Table 1 here>

One still needs to account for nonmembers from these districts who may avail themselves of district specific policies, such as parallel government programs. These differences are controlled with the use of district fixed effects. In that district wide effects may spillover from mature to new members and non-members across villages, this estimates underestimates this spillover impact. To account for the remaining village level variability, we employ village level characteristics.[18]

Training placement, as anticipated, is more complicated than actual program placement. Potentially, trainers are less likely to travel to more remote villages. 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 villagers employ other means of transport than buses. As

[16] We also ran this regression for the village level variables that we chose to use for our eventual impact regression (specify table numbers) and find the same results.

[17] For the (new) mature 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 confirms that there is no difference between the two.

[18] Dropouts remain a concern and were not tracked in our data. However, NCAER (2008) estimated the dropout rate as 8.2 per cent, below the 20-30 per centcited by Aghion and Morduch (2005) as a severe problem. Additionally, this dropout rate was calculated for SHGs with an average age of 5.4 years, nearly double the average age in our data, thus it is conservatively estimated that the dropout rate in our data is below 5 per cent.

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. As mentioned in the earlier section, the SHPIs provides general and skill development training to SHGs. The training variable ($Tijs$) indicates training by a new or mature member. Thus, this variable captures the training impact beyond the membership duration and self selection of the members.

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

$$I_{ijs} = a + \pi\alpha X_{ijs} + \beta V_{js} + \lambda D_s + \gamma M_{ijs} + \delta SGHMON_{ijs} + \phi T_{ijs} + \eta_{ijs} \tag{1}$$

Where $Iijs$ is the impact for household is measured in terms of asset accumulation or income generation, for household i in village j and district s, $Xijs$ are the household characteristics; Vjs is a vector of village-level characteristics, and Ds is a vector of district dummies that control for any district level difference. Here, $Mijs$ is the membership dummy variable, which controls for the selection bias. It takes the value of 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, $SHGMONijs$ is the number of months that SHG credit was available to mature members, exogenous to the households since chosen by the SHG program. The parameter of interest is ϕ which measures the impact of training. However, the selection bias of the trainees remains.

As mentioned previously, to correct for training endogeneity we employ propensity score matching and the test sensitivity of the results to unobservables.[19] We first examine the viability of using propensity score matching for this data set. Heckman et al. (1997) have outlined three intuitive conditions. One, the survey questionnaire should be the same for

[19] See the excellent survey by Caliendo and Kopeinig (2008) for the main issues on propensity score matching.

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.[20]

Propensity score estimators match the respondents who received training to those who did not. Except for the treatment (after the matching), the matched households are very similar in the observables. Thus, with propensity score matching any differential can be attributed to the impact. A logit equation determines the probability $P(X)$ of selection and then this probability (the propensity score) matches the households. Then, Y_1 is the outcome variable of interest for those with training ($T = 1$), and Y_0 is the outcome variable of interest for those without training ($T = 0$). Equation (2), thus denotes the mean impact of training:

$$\Delta = E\left[Y_1 \mid T = 1, P(X)\right] - E\left[Y_0 \mid T = 0, P(X)\right] \tag{2}$$

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

To account for both program participation and training, propensity score matching combines with 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. The following procedure explains the steps for regression adjusted matching estimators. First, run a regression for the outcome equation on the no training group and retain the fitted values.[21] 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

[20] The first and third as described in the data section and for the second, both treatment and control members reside in the same districts.

[21] 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 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.

training). Third, match the corrected outcome , (outcome variables minus the fitted values). Equation (3) provides the estimator:

$$\Delta RAM = \sum_{j=1}^{T} w_j \left[\left(Y_{j1} - x_j \overset{\wedge}{\beta}_0 \right) - \sum_{i=1}^{C} W_{ij} \left(Y_{ij0} - x_i \overset{\wedge}{\beta}_0 \right) \right] \tag{3}$$

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).

Following Smith and Todd (2005), the neighbor to neighbor (NN) matching algorithm is chosen. This algorithm is the most straightforward and matches partners according to their propensity score. Both the one and ten person matching is employed, 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) is used. The theorems in Heckman et al. (1997) 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 Heckman et al., 1997).

IV. Data

One of the authors collected the data as part of a larger study that investigates the SHG-bank linkage program.[22] 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.[23] 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 respondents were not provided the opportunity to self-select, they 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.

The data were not collected specifically for a training study. It primarily has information on the total training weeks that a household has received. The training variable is set to 1 for all respondents that reported positive weeks of training. Since both mature members and new members received training, the impact of training can be differentiated from that of loan access. The survey yields other measures of training such as whether the household received training in occupational skills or literacy for instance. When comparing the means and variances of the training weeks for mature and new SHGs an expected but significant difference is found. The amount of training weeks (1.52 versus 1.15) and the variability in training (2.42 versus 1.87) is larger for the mature SHGs.[24] About half (48 per

[22] 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.

[23] 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).

[24] A t-test with unequal variances revealed a t-ratio of 3.32 statistically significant at the 1 % level.

cent) of the mature SHGs received training while 39 percent of the new SHGs reported the same.[25] These statistics are not surprising in that the longer length of membership of mature SHGs will provide them with greater opportunities to undertake training. Surprisingly, a sizable percentage of new SHGs receive training, indicating a new commitment by SHPIs.

Table 2 compares the characteristics of households who received training to those that did not (regardless whether they were new or old members). In general, those who received training were wealthier, older, and had higher income, 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.[26] Still, these need to be conditioned on the full set of covariates and control for member self-selection in order to properly study the full impact of training.

<Insert Table 2 here >

As suggested by Doss et al. (2007), gross assets are calculated 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. Dependency ratios are also included as members with larger dependency ratios are expected to have greater (lesser) incentive for asset accumulation (income generation). In order to control for initial wealth, land owned three years ago is employed.

[25] 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.

[26] 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 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.

For village characteristics, in addition to male wage, the following distance variables: paved road, market, primary health care center, and bus-stop are included. Table 3 presents the non-training related descriptive statistics of the independent and dependent variables, respectively. SHG members and non-members are about the same age, share similar 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. Members on average have a relatively higher income, own more land and dwelling but have slightly lower amount of assets. Combining these statistics with Table 1 indicates that SHG members who had training have higher income and possibly higher amount of assets.

<Insert Table 3 here>

V. Results

This section discusses the estimation results for the training impact of the SHG bank linkage program on asset accumulation and income. We present the revised results of membership impact, followed by the results from training impact.[27] Subsequently, we discuss the results of regression adjusted estimates which control for both membership and training. The evidence on business training is further analyzed and compared to general training delivery. Sensitivity analyses checks the robustness of the results.

Drawn from previous work (Author (2009)), the membership impact results serve as a benchmark for measuring the additional impact from training. As seen in Table 4, membership has a positive impact on training and a negative impact on income. This will be discussed further but for now it indicates that asset accumulation begins immediately but translation into income may take time as members move towards other forms of income.

<Insert Table 4 here>

[27] Updated results of Author (2009).

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 in training.[28] 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 are chosen through a statistical significance and 'hit or miss' method while 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.

< Insert Table 5 here >

Matching the treated and comparison group based on the propensity score which controls only for training endogeneity, in Table 5 column (1), we find no impact of training on assets. But there is an impact on income using both the local linear regression and neighbor to neighbor techniques (table 5 column (2)). These estimates, however, do not take into account member selection bias and only account for training endogeneity. The regression adjusted matching results in columns (3) and (4) take both training endogeneity and member selection bias into account. The impact effects reverse with the training impact on assets significant with the impact on income as not significant.

< Insert Table 6 here >

In Table 6, the different estimates are compared. In column (1), the unadjusted t-stat difference suggests that training impacts both assets and income strongly. 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

[28]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.

on assets (on the order of 16 per cent) 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.

Comparing the impact of membership to that of training, the results from Table 4 indicate that membership (evaluated at SHGMON means for mature members) provides a return of 15 per cent on assets. From the regression adjusted estimates in Table 6, 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.

Investigating further on specific type of training, members were asked about the type of training and services that they participated in. If they reported that they received marketing or skill training advice, the business training variable was set to 1. Business training has a stronger significant impact on assets but again not on income (as seen in Table 7). We also investigate the breakdown by linkage model.[29]

<Insert Table 7 here>

Table 8 shows the regression adjusted matching estimates of training impact on asset and income by the type of linkage used. The results confirm Authors (2011) finding that only when NGOs specialize in training and banks in lending (the more popular Linkage model 2), impact of skill development and marketing training has a strong positively significant impact on assets. To gauge the impact a crude measure of returns on assets we compute by examining the point estimates. The return of 16 per cent of basic training can be increased to 23 per cent with more specialized training such as business training. Finally, with business training and linkage model 2 these returns increased to 34 per cent. Thus, the combination

[29] We did not find much difference for general training by linkage model with only linkage model 1 resulting in less impact for income.

of business training and model 2 yields the largest returns.

<Insert Table 8 here>

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 for productive purposes and hence 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." Second, Author (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. The specialized type of training matters where business training has the greatest impact especially when these training programs are delivered by NGOs who focus only on the groups themselves.

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 translate into 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

This section explores the robustness of the regression adjusted matching results through sensitivity analyses. The sensitivity of the results to the inclusion of unobservables is tested in terms of the pscore specification and the matching algorithms.[30] In terms of the pscore specification, in general, when the logit equation is even more parsimonious than the one specified that excludes 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 adding a large number of variables to the logit equation, balancing problems are encountered. Thus, our chosen equation balances two variables but is robust to the addition and subtraction of a few variables. The kernel algorithms for the matching and regression adjusted matching is also used with different bandwidths yielding similar results. Finally, since the bootstrapped standard errors are not analytical, the matching results are run several times for a check of the robustness of the bootstrapped standard errors.

For the conditional independence assumption, selection relies on observables, thus we tested the sensitivity of our results to the inclusion of unobservables. We have already discussed how our data meets the three conditions where the conditional independence assumption (CIA) appears plausible. However, propensity score matching hinges on the unconfoundedness assumption in which unobserved variables affect the participation and the outcome variable simultaneously. Ichino, Mealli and Nannicini (2007) suggest that if the CIA is not satisfied given observables but satisfied if one could observe an additional binary variable (confounder), then one could simulate this potential confounder in the data and use an additional covariate in combination with the preferred matching estimator. The comparison of the estimates obtained with and without matching on the simulated confounder indicate the robustness of the baseline results The distribution of the simulated variable captures different hypotheses on the nature of potential confounding factors.

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

[30] All of these results are available from the authors.

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 9 here>

Since our outcome variables are continuous, the confounder is simulated on the binary transformation of the outcome median. Table 9 presents the results of these two covariates to simulate confounders for both assets and income. Note that for both variables, the selection effect is not significant. The results indicate that the regression adjusted results are robust with respect to the confounder.

VII. 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 (though we do find a greater impact of business training). 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 payoffs in the future. We do not foresee much work in RCTs with SHGs in the future for reasons mentioned in the text and for limits due to political concerns. We will have to rely on the methods similar to the ones outlined here in order to make a statement on the current impact of SHGs.

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.

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TABLE 1

Logit estimates of Placement of SHG programs and Training Programs (x10^{-2})

Village Level Variables(in kms. unless noted)	*(1) SHG placement*	*(2) Training*
Distance from Block	-2.19 (0.98)	-2.77 (1.25)
Distance from haat (local market)	-0.45 (0.05)	4.02 (0.62)
Distance from Paved Road	-11.55 (0.98)	-36.3 (2.58)***
Distance from Bank	1.19 (0.37)	4.521 (1.19)
Distance from Market	7.02 (0. 78)	-3.92 (0.56)
Distance from HealthCare	-0.156 (0.02)	-4.56 (0.65)
Distance from 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

Variable Name	*No Training (T=0)* *Mean (S.D.)*	*Training (T=1)* *Mean (S.D.)*	*T-test for equality of means*
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 Edu.	0.19 (0.39)	0.17 (0.37)	0.68
Secondary Edu.	0.20 (0.40)	0.14 (0.35)	2.30**
College Edu.	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

Variable Name	*Mature SHGs* *Mean (S.D)*	*New SHGs* *Mean (S.D.)*	*Non-Members* *Mean (S.D.)*
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 Edu.	0.20 (0.40)	0.12 (0.33)	0.24 (0.43)
Secondary Edu.	0.17 (0.38)	0.18 (0.39)	0.12 (0.33)
College Edu.	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 membership on asset creation and income (pipeline) ($x10^3$)

	(1) Total Assets	(2) Income
Member	-42.5 (2.11)**	3.59 (1.40)
SHGMON	0.65 (1.99)**	-0.07 (1.75)*
Age (yrs.)	0.09 (0.16)	0.13 (1.82)*
Gender (Female=1)	7.91 (0.60)	-0.33 (0.13)
Dep. Ratio	41.43 (2.23)**	-10.65 (4.01)***
Primary Ed.	22.64 (1.89)*	-1.85 (1.17)
Secondary Ed.	31.84 (2.65)***	-3.38 (1.99)*
College Ed.	47.70 (1.85)*	-6.04 (1.80)*
Land 3 years ago (acres)	44.20 (8.41)***	1.73 (4.31)***
Average Shock	-0.12 (0.01)	2.00 (1.50)
Distance Paved Rd. (kms.)	-7.30 (2.35)**	-0.22 (0.56)
Distance Bank (kms.)	0.84 (0.76)	-0.13 (1.01)
Distance Market (kms.)	-1.66 (1.58)	-0.05 (0.31)
Distance Healthcare (kms.)	2.39 (1.00)	-0.06 (0.25)
Distance Bus Stop (kms.)	4.16 (1.32)	0.02 (0.06)
Male Wage (Rs.)	-0.47 (1.01)	0.001 (0.02)

Notes: *** Significant at the 1 % level. ** Significant at the 5 % level. * Significant at the 10 % level. All regressions include district dummies. Analysis based on 840 observations. Absolute t-ratios in parentheses computed with White heteroskedasticity-consistent standard errors clustered by village. Income is a tobit regression with non-White standard errors. See text for definitions of variables.

TABLE 5

Matching and regression adjusted matching estimates of training impact on assets and income ($x10^{-2}$)

Matching Algorithm	*(1) Gross Assets*	*(2) Income*	*(3) Gross Assets (Regression Adjusted)[a]*	*(4) Income (Regression Adjusted)*
1 NN (S.E.)	176.50	42.34**	------------	-------------
	(1.23)	(2.59)		
10 NN (S.E.)	212.76*	47.18**	-------------	--------------
	(1.92)	(3.75)		
LLR (bw 1) (S.E.)	165.61	49.72**	201.24**	8.15
	(1.49)	(3.54)	(1.99)	(0.60)
LLR (bw 4) (S.E.)	165.61	49.72**	201.24**	8.15
	(1.52)	(3.83)	(2.12)	(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. [a]Covariates of regression same at Table 4, (1) and (2). See text for definitions of variables.

TABLE 6

Comparison of estimates of training impact on assets and income($x10^{-2}$)

Variable	*(1) Unadjusted (T-test)*	*(2) Matching (LLR, bw 1)*	*(3) Regression Adjusted Matching (LLR, bw 1)*
Assets	321.75**(3.11)	165.61(1.49)	201.24** (1.99)
Income	58.51**(4.99)	49.72** (3.54)	8.15 (0.64)

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

TABLE 7

Regression adjusted matching estimates of business training impact on assets and income ($x10^{-2}$)

Matching Algorithm	*(1)*	*(2)*
	Gross Assets	*Income*
LLR (bw 1) (S.E.)	258.0**	-10.5
	(106.6)	(14.7)
LLR (bw 4) (S.E.)	258.0**	-10.5
	(111.6)	(13.6)

Notes: ** Significant at the 5 % level. * Significant at the 10 % level. NN = neighbor to neighbor, bootstrap standard errors in parentheses. LLR= local linear regression, p-values in parentheses, standard errors created by 200 bootstrap replications.

TABLE 8

Regression adjusted matching estimates of business training impact on assets and income by Linkage Model ($x10^{-2}$)

Matching Algorithm	*Model 1*		*Model 2*		*Model 3*	
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
	Gross Assets	*Income*	*Gross Assets*	*Income*	*Gross Assets*	*Income*
LLR (bw 1)	-650.6	-21.1	371.8***	-19.2	-215.0	37.9
	(458.9)	(53.1)	(134.8)	(17.1)	(227.0)	(41.2)
LLR (bw 4)	-650.6	-21.1	371.8***	-19.2	-215.0	37.9
	(458.5)	(52.5)	(132.9)	(17.5)	(215.6)	(38.1)

Notes: ** Significant at the 5 % level. * Significant at the 10 % level. NN = neighbor to neighbor, bootstrap standard errors in parentheses. LLR= local linear regression, p-values in parentheses standard errors created by 200 bootstrap replications.

TABLE 9

Simulation-Based Sensitivity Analysis for Matching Estimators†

Average treatment on treated effect (ATT) estimation on regression adjusted assets and income with simulated confounder General multiple-imputation standard errors ($x10^{-2}$)††

***Variable**/Covariate for simulated confounder*	*(1)* *ATT*	*(2)* *Standard Error*	*(3)* *Outcome effect*	*(4)* *Selection effect*
Training				
Assets				
Age	144.8	6.6	1.2	0.8
Education	140.6	7.2	1.3	1.3
Income				
Age	4.5	0.9	1.0	0.8
Education	4.3	0.8	1.1	1.3
Business Training				
Assets				
Age	240.0	4.3	1.1	0.93
Education	241.3	7.8	1.0	1.32
Income				
Age	-14.8	0.7	0.9	0.9
Education	-14.6	1.1	1.1	1.3

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).