# Evaluating the Impact of Training for a National Microfinance Program: The Case of Indian Self Help Groups

Ranjula BALI SWAIN* and Adel VARGHESE**

**Department of Economics, Uppsala University, Box 513, Uppsala, Sweden, 75120. Phone +46 18 471 1130. Fax +46 18 471 1478. (e-mail:Ranjula.Bali@nek.uu.se)*
*** Corresponding Author, Department of Economics, Texas A & M University, College Station, TX, USA, 77843.Phone 979.571.8760. Fax 979.847.8757.*
*(e-mail: avarghese@tamu.edu)*

**Abstract**

We evaluate the impact of training provided by facilitators of Self Help Groups (SHGs). Indian SHGs are mainly NGO-formed microfinance groups but funded by commercial banks. In our methods, we employ evaluation techniques applicable to current borrowers for a national program. We correct for membership selection bias with pipeline methods. We then account for training endogeneity with propensity score matching. Regression adjusted matching which controls for both participation and training selection bias reveals that training impacts assets but not income. Business training, in particular, has a greater impact than general training. We also confirm the robustness of these results through sensitivity analyses.

JEL Classification Numbers: G21, I32, O12.
Keywords: India, microfinance, training, impact studies, Self Help Groups.

# The Impact of Training by Indian Self Help Groups on Income and Assets


**Abstract**

We evaluate the impact of training provided by facilitators of Self Help Groups (SHGs). Indian SHGs are mainly NGO-formed microfinance groups but funded by commercial banks. In our methods, we employ evaluation techniques applicable to current borrowers for a national program. We correct for membership selection bias with pipeline methods. We then account for training endogeneity with propensity score matching. Regression adjusted matching which controls for both participation and training selection bias reveals that training impacts assets but not income. Business training, in particular, has a greater impact than general training. We also confirm the robustness of these results through sensitivity analyses.



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). Suppose policymakers seek to evaluate the impact of training for this SHG national microfinance program which initiated over ten years ago. So far, government sponsored studies have relied on pre-post evaluations, which do not pass minimum evaluation criteria (NCAER, 2008). Researchers face the dilemma of properly evaluating SHG programs. In this paper we will provide a method to 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. In complementary work (Author (2011)), we have focused on delivery mechanisms of training. In this study, our motive is simply explore whether training and which type has greater impact and to quantify its effects. The Indian government sponsors many training programs through its main agencies. 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 focus on two different outcome measures in this paper, assets and income.[2]

---

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

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

Other than the amout 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 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 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 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 one 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 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, we test the sensitivity of the results to unobservables.

The regression 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, similar to membership, training 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 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. We adapt Coleman's (1999) pipeline 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. We then use matching methods to control for training endogeneity. We propose the combination of these methods since for our data and setting, we argue for the limits of randomization. In the absence of randomization, these methods provide an alternative for measuring impact.[3]

Various authors have conducted a number of studies on the impact of SHGs with the NCAER (2008) report the most recent. Although a number of studies have taken none have systematically addressed selection issues place (apart from the Deininger and Liu study). In particular, in spite of the spate of training programs in India, none have measured the impact of training per se except to note its inadequacy. Still, due to the scarcity of studies on SHGs, many of the descriptive studies have had much policy influence, widely quoted in a number of

[3] 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.

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 beginning with Karlan and Valdivia (2009).[4] Using the popular randomization method with data from Peru, they find that business training improved business practices and revenues which in turn led to greater repayments and client retention. They do not separately measure the effects of membership, so 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.

## 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 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. 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, covers the beginning of this expansion strategy. 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

[4] For example, see Bruhn, Karlan, and Schoar (2010).

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).[5]

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.[6] General training to all SHG members covers group formation and an introduction to linkage methods.[7] 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

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

[6] Public information on the SHG training program is unavailable, 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 supplemented through NABARD circulars.

[7] 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.

‘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 (but the group they can check the log books to see whether people have shown up or not and acceptable excuses include emergencies such as illness, family funeral, etc.). Actually, many members other than those that initially request the training participate in the sessions. Training groups consist of thirty members but sometimes even eighty show up. Still, a minimum number of people must show or NABARD does not deem it cost efficient to hire a trainer for the specific skill. 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.[8] 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 (India’s agricultural bank) claims to have supported 8,356 REDP training programs with financial assistance of 400 million rupees covering 216,000 youth.[9] The training skills varied from soft skills such as spoken English, communication skills, computer awareness to vocational skills such as plumbing, marketing, and even call centers.

Due to the different demands in training for the groups themselves, these additional modules of training will differ according to the group. This paper thus will measure whether specialized training has positive impacts and whether business training in particular has a positive impact. However due to the national extent of the program, the focus will not be on

---

[8] 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.
[9] Rupees 400 million is about 8 million US dollars at current exchange rates.

one particular program as in the randomized trials. 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 study where meetings began with training, and the MFI used "sticks" such as fees for tardiness and threat of expulsion.[10]

## III. Estimation Strategy

In assessing impact, researchers seek to disentangle the causal effect 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) have led the revolution in the new microeconometrics of development (Banerjee and Duflo, 2009). 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: one, the SHG program is wide scale and national, 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 in its generalizability to other SHG programs. Second, by 2007, the number of SHGs have already grown to three million reaching forty million families and thus policymakers anxiously await evaluating the impact on these *current* borrowers (Srinivasan, 2009). Our data which coincides with the beginning of SHG expansion will allow an evaluation on the existing families. RCTs would not allow a evaluation of a national program on current borrowers. Thus, other evaluation procedures have to be explored.[11]

---

[10] Even with these conditions, Karlan and Valdivia (2009) found many detractors who chose not to attend the training sessions.

[11] A related issue is that RCTs explore the impact on marginal new borrowers and withholding loans for new borrowers. For longer term measures such as assets this is problematic. Second, one has to hope that the marginal borrower is similar to previous borrowers (before loans). Add in political concerns, making the implementation very difficult.

In our framework we encounter a double selection problem: into program participation and training. We correct for the first selection bias using a pipeline method and account for the second using matching methods. We now discuss the pipeline approach. 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.[12] 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 but will not drop out as they are awaiting loans, they serve as a control group since they chose to join the program and were accepted on attributes but have not received the benefits.[13]

---

[12] 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.

[13] To check for differences in the observable characteristics for old and new SHGs, we ran regressions of the following type: $Xijs = \alpha Ds + \beta Mijs + \gamma Tijs$ 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

<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.[14] 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.[15]

We still need to account for nonmembers from these districts who may avail themselves of district specific policies, such as parallel government programs. We control for these differences with the use of district fixed effects. In that district wide effects may spillover from mature to new members and non-members, the estimates here would underestimate that impact. To account for the remaining village level variability, we employ village level characteristics.[16]

As mentioned in the earlier section, the SHPIs provide basic training to all SHGs. Then, the SHPIs organize additional training for some of the SHGs. The training variable

---

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.

[14] 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.

[15] 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.

[16] Dropouts remain a concern. We did not track data on dropouts but NCAER study estimated the dropout rate as 8.2 %, below the 20-30 % cited 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 our average age, so we conservatively estimate that the dropout rate in our data as below 5 %.

($T_{ijs}$) indicates whether either a new or mature 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:

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

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, exogenous to the households since chosen by the SHG program. The parameter of interest is $\varphi$ 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. 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.[17] 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

[17] 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.

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 previously, for training endogeneity we use propensity score matching and then test the sensitivity of our results to unobservables.[18] 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. 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, propensity score estimators match the households who received training to those who did not. Except for the treatment, 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. 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:

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

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

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

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.[19] The following 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.[20] 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. Equation (3) below provides the estimator:

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

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).

We now choose the matching algorithms. Since the probability of two households being matched exactly is close to zero, distance measures 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

---

[19] 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.

[20] 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.

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).

## IV. Data

One of the authors collected the data as part of a larger study that investigates the SHG-bank linkage program.[21] 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.[22] 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

---

[21] 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.

[22] 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).

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.[23] For the mature SHGs, 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. 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 such as whether the household received training in occupational skills or literacy for example. When comparing the means and variances of the training weeks for mature and new SHGs we find an expected but 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).[24] About half (48 per 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

[23] Only fourteen of these new respondents were members for more than six months, in which case SHGMON = date of formation - six months 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.
[24] A t-test with unequal variances revealed a t-ratio of 3.32 statistically significant at the 1 % level.
[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.

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 (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, 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.

<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. 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.

---

[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.

<Insert Table 3 here>

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. 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 present the results of membership impact (from previous work), we then present the results from training impact and then present the results of regression adjusted estimates which control for both membership and training. We then present results from business training and compare them to general training procedures. We close with results of sensitivity analyses.

We first examine the results of membership alone using the pipeline method. Drawn from previous work (Author (2009)), they serve as a benchmark before measuring the additional impact from training. As seen in Table 4, membership has a positive impact on training and surprisingly a negative impact on income. We will comment more on these results later but for now they indicate that asset accumulation begins immediately but translation into income takes time since members are moving towards other forms of income.

<Insert Table 4 here>

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.[27] 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.

< Insert Table 5 here >

We then matched the treated and comparison group based on the propensity score which controls only for training endogeneity. In Table 5, 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 with 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. 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

[27] 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.

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. Results from Table 4 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.

We now investigate whether a more specific type of training, termed business training would have greater impact. Households were asked about their type of training and services, if they reported that they received marketing or skill training advice, we set the business training variable 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.[28]

Table 8 shows the regression adjusted matching estimates of training impact on asset and income by the type of linkage used. Our results show 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. We can compute a crude measure of returns on assets by examining the point estimates. We find a return of 16 % of basic training. These returns can increase to 23 % with more specialized training such as business training. Finally, with business training and linkage model 2 these

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

returns increase to 34 %. Thus, the combination of business training and model 2 yields the largest returns.

<Insert Table 7 here>

<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." Secondly, 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

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.[29] 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.

For the conditional independence assumption, selection relies on observables, thus 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 (CIA) appears plausible. However, propensity score matching hinges on the unconfoundedness assumption in which unobserved variables affect the participation and the outcome variable simultaneously. 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

---

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

causal effect that is known to equal zero. If the test suggests that the 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 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 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.

## 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 (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.

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 SHPAs 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.

## References


Abadie, A. and Imbens, G. 2007. Bias Corrected Matching Estimators for Average Treatment Effects. Working Paper, Harvard University.

Aghion, B. and Morduch, J. 2005. The Economics of Microfinance. MIT Press, Cambridge, Mass.

Author. 2009.

Author. 2011

Banerjee, A., Duflo, E., Glennerster, R., and C. Kinnan. 2009. The Miracle of Microfinance? Evidence from a Randomized Evaluation. Working Paper, MIT Department of Economics.

Banerjee, A. and Duflo, E. 2009. The Experimental Approach to Development Economics. Annual Review of Economics 1, 151-178.

Barnow, B., Cain, G. and A. Goldberger 1980. Issues in the Analysis of Selectivity Bias. In: E. Stromsdorfer and G. Farkas (Eds), Evaluation Studies Review Annual, Vol.5. Sage, San Francisco.

Bruhn, M., Karlan, D. and A. Schoar. 2010. The Impact of Offering Consulting Services to Small and Medium Enterprises: Evidence from a Randomized Trial in Mexico. Working Paper, MIT, Department of Economics.

Caliendo, M. and Kopeinig, S. 2008. Some Practical Guidance for the Implementation of Propensity Score Matching. Journal of Economic Surveys 22, 31-72.

Coleman, B. 1999. The Impact of Lending in Northeastern Thailand. Journal of Development Economics 60, 105-141.

Deininger, K. and Liu, Y. 2009. Economic and Social Impacts of Self-Help Groups in India. Working Paper, The World Bank.

Doss, C., Grown, C., and C.D. Greene 2007. Gender and Asset Ownership. Working Paper, The World Bank.

Heckman, J. 1992. Randomization and social policy evaluation. In: Manski, C, Garfinkel, I. (Eds.), Evaluating Welfare and Training Programs. Harvard University Press, Cambridge, MA.

Heckman, J., Ichimura, H., and Todd, P. 1997. Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme. Review of Economic Studies 64, 605-654.

Ichino, A., and Becker, S. 2002. Estimation of Average Treatment Effects Based on Propensity Score. The Stata Journal 2, 358-377.

Ichino A., Mealli F. and Nannicini T. 2007. From Temporary Help Jobs to Permanent Employment: What Can We Learn from Matching Estimators and their Sensitivity? Journal of Applied Econometrics 23, 305-327.

Karlan, D. And Valdivia, M. 2009. Teaching Entrepreneurship: Impact of Business Training on Microfinance Clients and Institutions. Working Paper, Yale University.

Leuven, E. and Sianesi, B 2009. PSMATCH2: Stata module to perform full Mahalanobis and propensity score matching, common support graphing, and covariate imbalance testing. Statistical Software Components S432001, Boston College Department of Economics.

Morduch, J. 2000. The Microfinance Schism. World Development 28, 617-629.

NABARD. 2006. Progress of SHG-Bank Linkage in India: 2005-06. Working Paper, NABARD.

NCAER. 2008. Impact and Sustainability of SHG Bank Linkage Programme. Working Paper, NCAER.

Nussbaum, M., Kumar, A., and A. Miehlbradt. 2005. Integrating Microenterprise into Markets: The Case of EDA's Leather Subsector Project in India. The Seep Network Case Study #2, Seep Network.

Rosenbaum, P. 1987. Sensitivity Analysis to Certain Permutation Inferences in Matched Observational Studies. Biometrika 74, 1.

Smith, J. and Todd, P. 2005. 'Does Matching Address Lalonde's Critique of Nonexperimental Estimators?' Journal of Econometrics 125, 305-353.

Srinivasan, N. 2009. Microfinance India: State of Sector Report. Sage Publications, New Delhi

TABLE 1

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

| *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)* <br> *Mean (S.D.)* | *Training (T=1)* <br> *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*<br>*Mean (S.D)* | *New SHGs*<br>*Mean (S.D.)* | *Non-Members*<br>*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) (x$10^3$)*

| | (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 (x$10^{-2}$)††*

| ***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).