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**WHO BENEFITS FROM MICROFINANCE? THE IMPACT EVALUATION OF
LARGE SCALE PROGRAMS IN BANGLADESH¹**

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ABSTRACT

This paper evaluates the impact of microfinance on household consumption using a new, large and unique cross-section data set from Bangladesh. The richness of the data and program eligibility criterion allow the use of a number of non-experimental impact evaluation techniques, in particular instrumental variable (IV) estimation and propensity score matching (PSM). Estimates from both IV and PSM strategies have been interpreted as average causal effects that are valid for various groups of participants in microfinance. The overall results indicate that the effects of micro loans are not robust across all groups of poor household borrowers. It appears that the poorest of the poor participants are among those who benefit most. The impact estimates are lower, or sometimes even negative, for those households marginal to the participation decision. The effects of participation are, in general, stronger for male borrowers. These results hold across different specifications and methods, including correction for various sources of selection bias (including possible spill-over effects).

JEL Classification: C31, H43, I30, L30, O12

Key Words: Microfinance, treatment effect, Matching, Consumption.

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1. Introduction

Microfinance has become a prominent element of development strategies. Over the last two decades microcredit¹ programs have expanded rapidly, firstly in Bangladesh and then around the developing world. Most development practitioners and policy makers believe that microfinance can help the poor to break out of poverty. The academic world has also shown increased interest in microfinance. A great deal has been written on microfinance theory (see, for example, Chowdhury 2005 and the reference therein).² Much of this literature has focused on joint liability group lending and its implications for reducing information asymmetries. In spite of the abundance of theoretical literature, empirical work on the impact of microfinance is relatively sparse compared to the worldwide scale of the operation of these programs (Armendáriz de Aghion and Morduch 2005, Herms and Lensink 2007).

Bangladesh's microfinance sector is remarkable for the speed with which it grew to its present size and prominence. This paper evaluates the impact of microcredit participation on household consumption using a large, nationally representative and unique cross-section data set from Bangladesh. The data comes from a survey conducted by the Bangladesh Institute of Development Studies (BIDS) for the Palli Karma-Sahayak Foundation (PKSF, Rural Employment Support Foundation) specifically for the purposes of evaluating microfinance programs in Bangladesh.³ The survey encompasses a wide variety of information at the household, village and organization level. It includes 3026 households comprising households in the program and control groups, covering 91 villages spread over 23 thanas (sub-districts). This is the largest survey of microfinance households ever conducted in Bangladesh, and possibly in the world.

The existing evidence on the impact of the microcredit program in Bangladesh is ambiguous. Different identification strategies have yielded different conclusions. The best-known impact evaluation study of microfinance by Pitt and Khandker (1998) (PK from here on) finds that access to microfinance significantly increases consumption and

¹ In this paper the terms 'microcredit' and 'microfinance' are used interchangeably.

² The reader can consult the "Group lending" special issues of the *Journal of Development Economics*, 1999, vol. 60(1), pp.1-269, and *Economic Journal*, 2007, vol. 117(517), pp. F1-F133.

³ The data collection and preliminary analysis were supported by the World Bank. PKSF, established in May 1990, works as an organization for MFIs. The micro-lending community regards it as a regulatory agency and it exercises its authority over the MFIs.

reduces poverty. However, Morduch (1998), using PK's dataset but different estimation methodology, finds that access to microfinance has an insignificant, or even negative, effect on the household welfare. More recently Madajewicz (2003), using the same dataset but again a different estimation methodology, finds results similar to those obtained by Morduch (1998).⁴

Using panel data from Bangladesh, Khandker (2005) finds a significantly weaker effect of microfinance participation than found in earlier cross-sectional studies (undertaken by the same author). The results also cast doubt on the optimistic 5 percent drop in poverty by the PK study. In the case of Thailand, Coleman (1999) finds that the average program impact is insignificant on physical assets, savings, and expenditure on education and health care. Kaboski and Townsend (2005) find institutions with good policies can promote asset growth, consumption smoothing and decrease the reliance on moneylenders in Thailand. However, they find no measurable impacts of the joint liability or repayment frequency. Karlan and Zinman (2008) examine the impact of expanding access to consumer credit using data gathered from a field experiment in South Africa. Their results indicate significant and positive effects on income, food consumption, and job retention.

We estimate both the effect of participating in microfinance programs (the treatment on the treated effect) and the effect of being offered the chance to participate in a microfinance program (the intention-to-treat effect). The intention-to-treat (ITT) effect suggests a smaller positive effect of assignment (eligibility). A good number of eligible households in the treatment village did not participate while some non-eligible (non-encouraged) households participated in the program. In our case, assignment (an eligibility criterion) is merely an encouragement to take treatment and there is non-compliance among those encouraged. So we use two different techniques to address the issue of selection bias, and to link ITT effects to treatment effects. We first use an IV

⁴ PK use an instrumental variable (IV) approach considering the choice based sampling, and employ the weighted exogenous sampling maximum likelihood (WESML) estimator of Manski and Lerman (1977). PK's IV approach is parallel to the use of limited information maximum likelihood (LIML) and village fixed effects (FE), and thus called their estimate LIML-WESML-FE. Morduch (1998), on the other hand, applies a simple difference-in-difference (DD) approach. Madajewicz (2003) uses an IV method which is very similar to that of PK's method, but she estimates the impact of lending programs on business profits of borrowers by poverty status. According to Greene (2000), "WESML and choice based sampling method are not the free lunch they may appear to be. In fact, what the biased sampling does, the weighting undoes", p.823. Armendáriz de Aghion and Morduch (2005) argues that despite PK use heavier statistical artillery than other microfinance studies it does not mean that they deliver results that are more reliable or rigorous than others.

approach, where the instruments are generated by village level program placement and by an eligibility rule for receiving microfinance. We also exploit the variation in the amount of credit borrowed across households of different villages - a feature that has not yet been utilized - based on the exposure to the program. The second approach uses the propensity score matching (PSM) method of Rosenbaum and Rubin (1983). Here treated are compared with matched untreated (based on propensity scores), while controlling for the characteristics used by the MFIs to select the households and other observable household characteristics that are potential determinants of participation in microfinance. We find a substantially lower effect on consumption applying matching methods than implied by the IV approach.

Improving economic well-being is the main objective of microfinance programs. Food consumption expenditure accounts for more than 70 percent of total household spending among the poor in the rural areas of Bangladesh.⁵ Overall, the results suggest that the effects of microfinance loans on food consumption expenditure are not robust across all groups of poor household borrowers. We find evidence against the “common effect” assumption using the analysis on subgroups. The results overwhelmingly support the fact that the poorest of the poor benefit most from participating in microfinance. The impacts are lower, or sometimes even negative, for those households marginal to the participation decision. The effects of participation are, in general, stronger for male borrowers. The empirical findings hold across different specifications and methods, and when corrected for various sources of selection bias including possible spillover effects.

2. The Program, the Data and the Descriptives

2.1 The Program and the Context

The microfinance sector of Bangladesh is one of the largest and oldest programs of the world.⁶ The growth in the MFI sector, in terms of the number of MFIs as well as total membership, was phenomenal during the 1990s and is continuing. The PKSF was established with a view to monitor the activities of these large numbers of MFI, and to lend out donor and other funds to its partner organisations (POs) for microcredit. In

⁵ Poor households’ savings in rural areas of Bangladesh is very negligible. Our hypothesis is that if household income increases significantly to affect their permanent income level, households’ consumption expenditure will increase. Moreover, difficulties with collecting income data are well-known, especially from the developing countries.

⁶ Around one quarter of the world’s micro-credit customers are in Bangladesh with a further quarter in India (State of the microcredit summit campaign report 2006).

2004 PKSF funds made up about 17% of the total microfinance industry in Bangladesh, which was 24% in 1998.⁷

Previous studies on microfinance in Bangladesh have primarily focused on GB (PK; Morduch 1998, 1999). However, expansion, competition and funding constraints have greatly changed the recent dynamics of microfinance in Bangladesh. For example, the Association for Social Advancement (ASA), which started its microfinance operations in 1991, has now become a dominant MFI in terms of number of beneficiaries and loan disbursement. Similarly, Proshikha has been able to increase its outreach remarkably during the 1990s reaching about 2.8 million borrowers by 2001. During that period the number of medium and small MFIs has grown from a very small base to more than a thousand institutions. In view of the growing importance of the non-GB MFIs in Bangladesh, in this study, we use a completely different set of data from thirteen MFIs of PKSF. The organizations investigated here are different from those studied previously, but include organisations that are very large in terms of loan disbursements and area of coverage, most notably the ASA and Proshikha. ASA provides both credit and savings services on a remarkably large scale. Proshika is the fourth largest microcredit program in Bangladesh. Notable other MFIs which we study here include the Society for Social sServices (SSS) and Thengamar Mohila Sabuj Sangha (TMSS). As of December 2004, SSS is the tenth largest MFI in Bangladesh in terms of cumulative disbursements and outstanding borrowers. TMSS is one of the top fifty MFIs in Bangladesh. The other MFIs are relatively small and have similar types of program activities. All of these MFIs follow the GB-style lending procedure and typically give access to microfinance to households having less than 50 decimals of land. Credit is given mainly to groups of people who are jointly liable for repayment of the loan, and there is no collateral requirement. Loans are primarily advanced for any profitable and socially acceptable income generating activity. The amount of a loan usually lies within the range US\$40 - \$160. However, members may take larger loans after repaying their first loan.

2.2 The Data and Survey Design

The data was collected initially to monitor and assess the impact of microfinance programs undertaken by MFIs of the PKSF. BIDS was responsible for the collection of data on behalf of PKSF. The survey includes 13 MFIs, each from a different district,

⁷ See <<http://www.bwtp.org>>.

covering 91 villages spread over 23 thanas. Following a census of all households in the 91 villages during October 1997, the survey was administered in early 1998. Besides collecting detailed information at the household level, separate modules were administered at the village and institution level.

The survey was conducted to obtain a nationally representative dataset for the evaluation of microfinance programs in Bangladesh. The selection of MFIs was intended to capture the various areas of operation and coverage. Since MFIs were not operating in all thana, selection of thana within the district of each MFI was made followed by village selection. The geographic coverage of the survey was spread evenly over Bangladesh, and the thana level comparisons revealed that selected thanas were not different from the average (Mahmud 2003). Within a MFI area, the selection of villages involved visiting the MFI local offices and interviewing some key informants to prepare a list of all villages in the area and compile village specific information regarding: type of MFI-activities; number of MFI groups; number of borrowers; infrastructure condition; and existence of other MFIs. Upon obtaining the information, a sample of villages under each of the selected MFI was drawn through stratified random sampling. The stratification was based on the presence or absence of microfinance activity. The non-program villages were selected among neighbouring villages.

Of the 13 selected MFIs, two were deliberately chosen from the large category (e.g., Proshika and ASA). Secondly, thanas were selected when more than one thana was covered by the MFIs. Then, two control villages and six programme villages were chosen from each of the MFI areas. However, since non-program villages could not be found under some of the MFIs, only 11 non-program villages could be included. So six to eight villages from each MFI were selected depending on the availability of control villages. In selecting the survey households, the universe of households in program villages, drawn from the census, was grouped according to their eligibility status. A household is said to be eligible if it owns 50 decimals (half-acre) or less of cultivable land. Participation was defined in terms of current membership as reported in the census in 1997. From the village census list, 34 households were drawn from each program and non-program village. Because the census found a good number of ineligible households in program villages the sample was drawn so as to maintain the proportion of eligible and ineligible households at about 12:5. The sample size within program and control villages was

determined accordingly.⁸ A total of 3026 households were drawn from program and control villages including 1740 participants. Of the 1286 non-participants, 277 were from control villages and 1009 were from program villages. Because of the absence of appropriate control villages, more non-participants were drawn from program villages.⁹ The samples from control villages (the control group) include those households whose heads expressed their willingness to participate (during census) in MFI programs, if available. Among the total surveyed households 2051 are eligible representing 67.7 percent of all households. The same proportion is also surveyed in the program village: 1835 are eligible out of 2735 households. Of the total number of 1740 borrowers 207 are men.

2.3 Descriptive Statistics

Table 1 contains the descriptive statistics for different village level characteristics. It shows that there are no systematic differences in terms of education and health characteristics. Among transport and communication facilities, there are differences in terms of the presence of *pucca* (brick-built) roads in the village (35 percent in program villages as opposed to 11 percent in the control villages) and the distance of the village from the nearest thana (the program villages are relatively closer to the thana). Also the program village has better electricity facilities than the control village. There are no statistically significant differences between program and control villages in terms of bazaar (market place for grocery), post office and telephone office. However, there is a relatively higher presence of money-lenders in the program villages. In terms of irrigation facilities, no statistically significant differences were found, though in all cases program villages have better facilities as indicated by the higher average number/proportion of facilities per village. Overall we see that program villages are more developed in terms of infrastructure and other related facilities.

Table 2 provides key descriptive statistics for the household level variables. It shows that the average landholding for the non-treated households is significantly higher than the treated households. For household size, both Kolmogorov-Smirnov (*K-S*) and *t*-tests suggest that it is different between treatment and comparison households. There are also some differences between many household characteristics of treatment and comparison

⁸ The sample size and its ratio between participating and non-participating households are different in a few villages because of the absence of the required number of appropriate households in each group.

⁹ Khandker (2005) also highlights the limitation of getting the control villages in Bangladesh. He finds that the villages that were controls in 1991-92 in his survey, all became program villages by 1997-98.

groups as indicated by p -values and $K-S$ tests, but these differences are minimal when we consider only the eligible group of households (households owning less than half acre of land). In fact, many of the characteristics are also very similar for samples of households with up to one acre of land (not shown here). Overall the findings here are that the differences between treatment and comparison households are not systematic; however, the treated group has a higher average household size, more children and its members tend to be less educated.

Table 3 presents summary statistics of food consumption and credit variables. Consumption expenditure data include expenditures of food consumed in the reference period. The information covers a wide range and types (e.g. food purchased, home produced) of food consumption, and is as good as the standard LSMS food consumption module. Table 3 suggests that there are no statistically significant differences between treated and non-treated groups of households in terms of food consumption, though non-treated groups have a little more household and per-capita consumption levels than the treated group. However, when we consider consumption expenditure by household landownership, total household monthly consumption expenditure is higher for treated households having two acres or less land (Figure 1a). Household consumption expenditures for both groups are a monotonically increasing function of household's land ownership. On a *per-capita* basis, non-treated households have higher consumption expenditure than the treated group (Figure 1b). Household level monthly food consumption expenditures between program and control villages show that they are not different (Table 3). However, when we consider *per-capita* monthly consumption expenditure, households in control villages have slightly higher consumption than those in program villages. Table 3 also shows that villages with male borrowers borrowed more than their female counterparts. Households with male participants also have, on average, a higher number of members in microfinance and have more exposure (length of membership in microfinance) to the program. They also have higher consumption at both the household and *per-capita* levels, and the differences between these consumption measures for female borrowers are statistically significant.¹⁰

¹⁰ A treated household consists of either male or female member but not both in our sample. Groups are never mixed by genders. A MFI selects the gender of the treatment group, and households do not have a choice of whether male or female will participate.

3. Empirical Strategy

There are a number of different potential sources of bias that need to be accounted for to examine the effect of participation in microfinance. First, participants are likely to differ from non-participants in the distribution of observed characteristics, leading to a “selection-on-observables” bias. There are also problems due to “selection-on-unobservables”—programs may be placed in a non-random sample of villages, and households may self-select into the program (and subsequently decide on how much to borrow). For example, the program village might be poorer than the control village. Microfinance programs are targeted to poor households. A prospective member decides that he/she wants to participate in the microfinance program. The potential participant also has to be approved by officials of MFI. Households are therefore self-selected into the program. Thus there are likely to be observable and unobservable differences in characteristics between participants and non-participants.

However, it is likely that the MFIs choose the program village based on some observable characteristics. There are many MFIs working in Bangladesh. If local officials of one MFI use some information then other MFIs would try to use the same and so it should be known to researchers interacting both with officials and borrowers. Discussions with program officials at the local-office levels indicate that programs are designed by the head office. It also appears that local branch managers and officials of MFIs are not from the same area where the program is located. This is also discouraged by PKSF, the supervising body of the MFIs we are studying, since it may induce loan selection to the employees’ relatives or acquaintances. There are also specific guidelines from the head-office to select the program villages. Given the size of the microfinance program and the number of MFIs working in Bangladesh, it is reasonable to assume that village level program placement is a problem of “selection-on-observables”.¹¹ We use a wide range of village-level controls to address the village level selection. We also use MFI level fixed effects to deal with the problem of unobserved heterogeneity across different MFIs. These MFI level-fixed effects also partially control for unobserved factors across different geographic areas. With controls for village and fixed effects, we assume that there are no contemporaneous village level unobservables that are correlated with microfinance program placement in a village and household’s consumption expenditure.

¹¹ Gauri and Fruttero (2003) find that NGO programs in Bangladesh are not targeted at the poor villages, and NGOs do not respond to local community needs. Their findings indicate that non-random selection of villages by NGOs (which mainly include MFIs) is not an important issue in Bangladesh.

However, identification also requires controlling for the endogeneity that arises from household self-selection into the program. So, even conditional on a set of observed covariates, X , there could be some unobservable factors that may determine a household's decision to join a microfinance program. This could be entrepreneurial ability, information advantage, attitudes, traditions, customs or family culture, etc. In order to understand the difficulties inherent in estimating the treatment effect of participation or credit, assume that the consumption of household i in village j can be described as:

$$(1) \quad Y_{ij} = \pi_1 D_{ij} + \varphi_1 X_{1i} + \varphi_2 X_{2j} + \epsilon_{ij}$$

where X_{1i} is a vector of household specific variables, and X_{2j} is a vector of village-specific characteristics. $D_{ij}=1$ if household i is a member of microfinance and $D_{ij}=0$ if i is not. (Alternatively, for identifying the effect of credit, D_{ij} is the amount of microcredit borrowed by household i in village j). Selection into microfinance programs on the basis of unobserved characteristics, ϵ_{ij} , by households may generate a non-zero correlation between ϵ_{ij} and D_{ij} . Therefore treatment effect estimated using OLS may not reflect the program's causal effect on household consumption.

To solve the problem of endogeneity we consider IV estimation techniques. We utilize the program eligibility criterion set by the MFIs, and use it as an instrument for participation. The eligibility rule is not completely followed so the treatment does not change from zero to one at the threshold of eligibility. If treatment was deterministic with respect to the eligibility rule, we could compare outcomes of households clustered just below the cut-off line to those just above, and apply the regression discontinuity (RD) design directly. Figure 2a shows that the participation rate falls sharply once households cross the threshold level of half-acre land, but it does not fall from one to zero. The eligibility rule could not be applied for many practical considerations, some of which are mentioned below. It therefore raises concern that there could be some variables observed by the loan officer but unobserved by the evaluator. So, we apply an approach which can be seen as an *indirect* application of (fuzzy) RD design (see Van der Klaauw 2002). Unlike sharp RD design, selection into microfinance program in fuzzy design is based on both observables and unobservables. We implement RD approach using IV approach such as those used by Angrist and Lavy (1999).

Figure 2a illustrates that eligible households residing in a program village have higher chances to join in a microcredit program (70 percent participants are eligible). It therefore seems reasonable to think of eligibility status in a program village as an instrument for program participation. Formally, define V_j as the presence of a program in a village j and E_i is a variable which takes the value of 1 if the household is eligible i.e. owns less than half acre of land, and zero otherwise. So our instrument is $Z_{ij}=V_j \times E_i$, where $Z_{ij} = 1$ if the household lives in the program village and is eligible. The eligibility criterion and program placement are exogenous to the household and hence our instrument is ‘as good as randomly assigned’.

Therefore our identifying assumption is that household ij 's participation, or the amount of credit borrowed, D_{ij} , in microfinance is governed by:

$$(2) \quad D_{ij} = \alpha_1 Z_{ij} + \varphi_3 X_{1i} + \varphi_4 X_{2j} + \omega_{ij}$$

where X_1 and X_2 are the same as in equation (1) and ω_{ij} is the household-specific error term embodying the unobserved influences on D_{ij} . We assume that Z and X are exogenous with respect to ϵ_{ij} and ω_{ij} . We also examine whether there is a differential effect of credit borrowed by male and female borrowers.

Are we using a valid Instrument?

Identification requires that land ownership is exogenous conditional on program participation. The exogeneity of land ownership is a plausible assumption. The validity of the land-based eligibility criterion as an instrument is also defended at length by PK, and Pitt (1999) in response to Morduch's (1998) critique. Morduch (1998) argues that PK data show a great deal of turnover in the land market. However, our data confirm very low turnover in the land market: only 12.8 percent of households purchased land and 9.5 percent of households sold land in the five year period prior to the survey. This turnover rate does not differ between program and control villages. So, the land market is not active in our survey area. We do not find evidence that households endogenously sort themselves out in response to the half-acre eligibility rule. Since credit is extended mainly for *non-agricultural purposes* (self-employment activities) households having more land are exogenously ruled out. However, there are some participating households that own more than half an acre of land. Those households are currently not actively engaged in agriculture or the land is not fertile for cultivation, or sometimes there is

mistargeting, as perfect monitoring is not possible. The eligibility rule is set to simply identify the poverty status of the household. Since land price and quality also vary between different regions, a household having more than half an acre of land is also considered to be poor in some regions. So, sometimes the loan officer or branch manager made their own judgement over the poverty status of the households upon their field visit. Note that, in general, richer households get credit at a softer term from formal markets, or through other means. Also there are social norms that bar them from becoming members of a microcredit organization. Rich people in rural areas still hesitate to become members of MFI, because they consider MFI as an organization for the poor. Thus the use of program eligibility criterion as an instrument for treatment in microfinance is well justified here. Moreover, in order to allow Y_{ij} to vary with the level of the landholding status, in our regression specification in equation (1), we also use the amount of land by household as an explanatory variable. So Z is likely to satisfy the exclusion restriction.¹² For Z to be a valid instrument the vector X_2 should include all the village level characteristics that the MFI may use to decide program placement. We do so by exploiting the rich information collected at the village level and so the vector X_2 includes variables such as education, health, electricity, irrigation, prices, labour market conditions and infrastructure in the village.¹³

We check whether the eligibility criterion does satisfy the properties of an instrument. First, we need a strong first-stage to ensure that we are not using a weak instrument. We estimate a probit model of participation in the first stage using equation (2). There is a strong first stage here though the relationship between participation and eligibility is not deterministic (see Figure 2b). The coefficient estimate is positive and also economically significant – implying that eligibility is significantly related to the participation. We then check whether eligibility affects consumption expenditure only

¹² The key identifying assumption that underlies estimation using Z as an instrument is that any effects of eligibility on consumption are adequately controlled by the household land ownership included in X_1 in equation (1) and partialled out of Z by the inclusion of land ownership in X_1 in equation (2).

¹³ It may be argued that MFIs base their selections on the unobserved characteristics of the target population in each village, rather than on the entire population of the village. In that case, our estimations would be inconsistent. So, we also experimented with PK’s method of using separate fixed effects for target and non-target populations in each village (estimates involve more than 300 fixed effects). Our conclusions do not change with this specification (the results are available upon request). However, as we argued before, non-random selection of village is less important in our sample as most of the villages in the sub-districts we surveyed were under the microfinance program. Moreover, the largest sample of non-participants in our survey comes from program villages. So, we argue the concern regarding non-random program placement is not an important issue in our case once we control for village level observed covariates and fixed effects.

through the credit program participation. We estimate a semi-reduced form equation, in which participation is instrumented but eligibility enters the second stage regression directly (and naturally in the first stage regression). The results do not indicate any significant effect of eligibility in any of the specifications. We also estimate a reduced form regression regressing consumption expenditure on eligibility status, and we do not find any significant effect. Finally, we consider if there is a discontinuity in the conditional mean of consumption expenditure at the cut-off of eligibility. If we look at the Figure 1, we observe no discontinuity. We also check the possible discontinuity in outcomes in treatment villages but not in control villages and we do not find any such. This is expected since the relationship between land ownership and consumption expenditure is not obvious, and microcredit is provided to either landless households or households who are not much active in land cultivation.

4. Estimation Results

4.1 Differences-in-Differences Estimates

In the following, we evaluate the impact of microfinance on household total monthly food consumption expenditure and per-capita monthly food consumption expenditure. The dependent variable in the regression is the log of each expenditure measure. Based on household eligibility for the microfinance program, we first specify the following functional form:

$$(3) \quad Y_{ij} = \theta_0 + \delta_1 V_j + \delta_2 E_i + \delta_3 Z_{ij} + \varphi_7 X_{1i} + \varphi_8 X_{2j} + s_{ij}$$

where Y_{ij} is the log of consumption expenditure of household i in village j . With this specification, $(\delta_1 + \delta_3)$ measures the difference in the conditional expectation between eligible households in the program village and that of eligible households in the control village. Similarly, δ_3 is the difference-in-difference (DD) of mean log consumption expenditure. It captures the difference in conditional consumption expenditure between eligible and non-eligible in program villages that is over and above the difference in control villages.

Reduced form estimates of equation (3) using OLS are reported in Table 4. The covariates included in X_1 and X_2 are presented in the Appendix (see the list of variables). The top panel of Table 4 shows the coefficient estimates of the impact on the log of household total consumption expenditure by male and female households, and by

land ownership. The estimated coefficient δ_3 is always positive, indicating that the eligible households in the program village are better off due to the presence of the program. The results are similar for the coefficient estimates of the effect on per-capita consumption expenditures as shown in the bottom panel of Table 4. The coefficient δ_3 is also known as the ITT effect. The estimates in Table 4 indicate that the average ITT effect is approximately 4-8 percent. The results imply that eligible households in program villages are positively impacted by the presence of the program.¹⁴ It also shows that simple difference estimates with just the eligible in program and control villages would have understated the effect of eligibility by neglecting ineligible groups in both villages.

The advantage of using eligibility, rather than receiving the treatment, is that we have effectively eliminated the problem of non-compliance. There is no reason to believe that non-compliance would occur in the process of assigning households into the eligible group. The estimated impact on the corresponding participant is, however, likely to be biased downward since not all program eligibles in the treatment village received the treatment. Thus we cannot interpret the estimate as average effect per participant or TOT. Our DD estimates are thus diluted due to imperfect take-up rates. However, the estimation of the effect of eligibility is one of the most important parameters to estimate, and the estimation of ITT requires less restrictive assumptions than that of TOT. ITT thus likely provides a lower bound of the size of the TOT.

4.2 Instrumental Variable Estimates

We estimate the TOT effect using ITT as an instrument for treatment. Indeed, policy makers or practitioners are probably more interested in the TOT parameter. We consider two measures for D_i : (i) an indicator of whether the household is a current member of microfinance (binary treatment indicator); and (ii) the cumulative amount of credit borrowed (continuous treatment measure).

¹⁴ Most of the coefficients are statistically insignificant, but are sizeable in economic terms. This issue reappears throughout the study. We suspect this result is due to sampling error. However, this problem is common even with using U.S. CPS data. For example, Card (1992) encountered the same problem in his analysis of California's 1988 minimum-wage hike. See also Hamermesh and Trejo (2000) who also encountered similar problem to analyse the effect of overtime penalty on hours work. For more details on this issue, see McCloskey and Ziliak (1996) who suggest looking at economic significance of the results instead of its statistical significance. Note also that there need not be any relationship between weak reduced form and significance for IV estimates. So the statistical significance of the IV estimates of the effects of microfinance is independent of the reduced form estimates presented here.

We first consider a special case of an IV estimate —the Wald estimator, which is the ratio of the two ITTs: the effect of Z on Y divided by the effect of Z on D . Table 5 displays the results of the Wald estimates. The first panel reports the estimated treatment effect corresponding to the log of total consumption expenditure. In the first row we present estimates of the program impact using a binary treatment indicator. The coefficient estimates are negative and statistically significant for the whole sample and for the male and female samples individually. All the coefficient estimates are positive when we restrict each group to the eligible sample. The results are similar when we change the participation measure. Table 5 shows a statistically significant positive treatment effect for the eligible sub-sample of men and women groups when we consider per-capita consumption expenditure (second panel). The point estimate is stronger for eligible female borrowers compared to their male counterparts if we look at total consumption expenditure. However, the stronger positive effect is observed for the eligible male sub-sample when we consider the impact on per capita consumption.

The Wald estimator is based on the assumption that nothing other than the differences in the probability of participation is responsible for differences in consumption expenditure. A more efficient estimate would exploit all the available information that both accounts for the households' decision to participate in microfinance and for the outcomes of interest. Below we estimate treatment effects using equations (1) and (2) for various sub-samples of households based on their land ownership.

4.2.1 How Participation Impacts consumption

We present the estimated treatment effect using a binary treatment measure in the first row in each panel of Table 6-7. In the top panel of each Table we consider the samples of both men and women together. The middle panel reports results for female borrowers, and the bottom panel presents the same for the male group of borrowers. Consider panel 1 in Table 6 where we present IV estimates of program impact of participation of men and women on (the log of) total household monthly consumption expenditure.¹⁵ The estimated treatment effects are all positive when we limit our samples of households

¹⁵ The sample used here is choice-based: program participants were oversampled relative to the population. So we use weighted IV estimates (Hirano, Imbens and Rider 2003) where each program group member receives a weight of 1, and each comparison group member receives a weight of $p/(1-p)$, where p is the propensity score. The propensity score adjustment does not alter the qualitative conclusion, which holds whether we weight or not. So we report the unweighted results here (the weighted results are available on request).

with land ownership of less than or equal to 2 acres. The results show that participation in microfinance increases household consumption expenditure by about 5 percent for all households who own 2 acres or less land. For women, the treatment effects monotonically increase as the amount of land a household owns decreases. When we consider the full sample, the estimated impact on the log of total monthly consumption expenditure is negative. The corresponding estimates are positive, and are larger in the case of male group households for samples of 2 acres and 1 acre of land ownership, but then it gets weaker compared to the female group.

The mean impacts of participation on the log of monthly per-capita consumption expenditure are given in Table 7. The results are similar to the effects on total household consumption expenditure. For example, limiting the samples to households owning two acres or less of land, households' participation in microfinance increases the log of per-capita consumption expenditure by .037. The overall results indicate that treatment effects are positive when the samples are restricted to two acres of land. But for the male group, the positive impact is observed from 5 acres of land. Again we observe monotonically increasing effects of treatment for women borrowers as their land-holdings decrease. The treatment effects vary with land ownership and gender of participant, and they are typically higher for the male group. It should, however, be noted that male borrowers have higher averages of credit borrowed through microfinance. They also have more members, as participants in microfinance per household and the average length of participation in microfinance is also higher. The IV estimates suggest that effects of participation on eligible households are larger than the corresponding reduced-form estimates for all households having two acres or less land. The estimated coefficients are less precisely estimated as the sample size increasingly shrinks.¹⁶

4.2.2 How Credit Impacts Consumption

A weakness of the binary treatment approach above is that it classifies all treated beneficiaries in the same way, despite the fact that some households have received significantly larger amounts of credit than others. Since the extent of the treatment varies greatly among treated households, we report results using the amount of credit

¹⁶ Combining the regression by adding dummy variables for the sex of the borrowers, or by interacting dummies for different groups of land ownership with treatment status reduce the standard errors slightly, but not significantly. We prefer separate estimation for each group of land ownership and sex of the borrowers, as it allows us to compare IV estimates with those of PSM estimates (see next section).

borrowed as the treatment variable. The first stage involves estimating the credit demand equation using a Tobit model. The coefficient of the excluded instrument (eligibility) in the first stage is highly significant both statistically and economically. The second stage results, using the same specification as above, are reported in the second row of each panel of Table 6-7. The estimates are positive for samples of households having less than or equal to two acres of land, and for males it is positive from the 5 acres of land ownership. The average value of credit borrowed by the households of 2 acres or less of land is tk. 3849.5.¹⁷ So the estimate in row two in the top panel of Table 6 implies an increase in household total monthly consumption expenditure by about tk. 160, or 6.9 percentage points for both gender groups together. Similarly when the samples are restricted to only eligible group members, participating households enjoy an increase of about 13.3 percent of total consumption expenditures. The estimated effects are higher for male borrowers.

The effects of credit on household per-capita monthly food consumption are presented in row two of each panel of Table 7. The coefficients are positive from samples that include households of less than or equal to two acres of land. For male samples, the estimates are all positive except in column 1. In terms of magnitude, all eligible participants benefit from an increase in consumption expenditure of 13.6 percent. Using the binary treatment measure, we see that the estimated increase in consumption is 168 taka which corresponds to a percentage impact of 7.2. The corresponding increase in per-capita consumption is 8.2 percent when we consider all households of two acres or less land. We obtain different program effects when we consider men and women groups separately; we see the positive effects on men and women but the size of the effects differs widely between men and women borrowers. The effects of participation or credit are negative when we consider the entire sample of participants. In general, we find slightly larger coefficient estimates (especially for men) using continuous rather than the binary treatment measures.

4.2.3 Treatment Intensity as the Instrument

Households living in different villages borrowed varying amounts. It appears that there is wide variation in the amount of credit borrowed by participants across different villages (Figure 3). Thus, the IV method can be improved upon by recognizing that

¹⁷ In 1998, 35 taka =1US\$ (approx.)

some villages have participated in the programs longer than others.¹⁸ So we can exploit the across-village variation in the intensity of treatment to capture the variation in the amount of loan borrowed across households in different villages. Explicitly, the instrument is:

$$Z = V \times E \times \textit{treatment intensity}$$

where ‘treatment intensity’ is measured by the number of years of microfinance program placement in a particular village. We also use interactions with ‘year of program placement dummies’ as instruments. In particular, we use the following instrument:

$$Z = V \times E \times \sum_i \textit{Villyear}_i$$

where ‘Villyear’ is the year dummy variable for the introduction of program in different villages. We report results on the effect of the log of per-capita food consumption expenditure in Table 8. The first panel shows the coefficient estimates of the impact of microcredit using a single instrument - ‘years of program placement in a village’ - interacted with the indicator of eligibility status in a program village. We observe the positive program effect in all cases starting from the households owning two acres of land and less. The impacts typically vary between 8 and 14 percent depending on the gender of participant and samples of different land group. The effects are higher on the male borrower group than the female group. We present the corresponding 2SLS estimates using multiple instruments in the second panel of Table 8. The estimates constructed using larger instrument sets differ little from those using a single instrument (in the top panel). We find statistically significant positive effects of microfinance on all borrowers owning one acre or lower amounts of land. In the case of the landless, the coefficients are statistically significant for both groups, individually and jointly. All households, having one acre or less land, enjoy an increase in food consumption of 13 percent for participating in a microcredit program. If we consider just the landless households, they gain more (25 percent). So our results indicate that over-identified estimates computed using the multiple instrument set are more precisely estimated than the just-identified estimates. However, the resulting efficiency gains are not dramatic: the standard error of estimates falls slightly with similar coefficient estimates. The p-values of the F-statistics (for both men and women group samples) of the overidentifying restrictions test are shown in square brackets in the bottom panel of

¹⁸ 82% of the participants in our sample are members of a MFI for more than a year.

Table 8. The p-values indicate that over-identifying restrictions cannot be rejected at any reasonable level for any sample of households.

4.3 Interpreting the IV Estimates

As mentioned previously non-compliance exists – it is not the case that all eligible households in the treatment villages participate in the microfinance program. On the other hand, some ineligible (non-encouraged) households end up receiving the treatment. So we characterize the households affected by the IV approach. The relationship between microfinance program participation (D_i) and its effect on food consumption expenditure (Y_i) can be analysed only for the subpopulation that is affected by the instrument. Imbens and Angrist (1994), and Angrist, Imbens and Rubin (1996) (AIR from here on), identify this subgroup of units as ‘compliers’, and the resulting estimate is called local average treatment effect (LATE). In our case, when using the binary treatment indicator, LATE is the average program effect on food consumption expenditure for those households who choose to participate in microfinance only because they are eligible to borrow. Similarly, the IV estimator exploiting more than one instrument is the average of the various single instrument LATE estimators that we would obtain using each instrument separately. In this case the weights are proportional to the effect of each instrument on the treatment variable: the bigger the impact of the instrument on the regressor, the more weight it receives in the IV estimation (Angrist and Imbens 1995).¹⁹

The LATE-IV is based on the two assumptions: the conditional independence assumption (CIA) and the monotonicity assumption. The monotonicity assumption implies that anyone in the population who would take microcredit in the absence of eligibility would also take credit if they became eligible. The assumption requires that eligibility can make participation in microfinance more likely, not less, and that there is no one in the eligible households who actually was denied the credit (i.e., $D_i=1|Z_i=1 \geq D_i=1|Z_i=0$ or $D_i=0|Z_i=1 \leq D_i=0|Z_i=0$ for all i). The assumption assures that there are no *defiers* and that *compliers* exist. Since credit is offered for non-agricultural purposes, there is no reason to think that households choose not to participate in microcredit

¹⁹ The interpretation of LATE also applies in the case of non-binary IVs and non-binary endogenous regressors (see Angrist and Kruger 1999; Frolich 2007). In our case when D is the amount of credit borrowed, the compliance intensity can differ among units. Hence a change in Z induces a variety of different reactions in D , which cannot be disentangled. Only a weighted average of these effects can be identified. For more on this issue see Frolich (2007)

when they have little land while they would participate when they possess more land. The data also find little use of loan - only 18 percent of the respondents mentioned a fraction of the loan was used for land cultivation purpose. Moreover, we do not find a significant difference between eligible and ineligible groups among those who use the loan for main agricultural activities. The CIA is based on the two requirements: (i) comparison between outcomes for the households exposed to different values of Z identifies the causal impact of the instrument; and (ii) the instrument does not directly affect the outcomes. The first requirement is satisfied since eligibility status is assigned by the MFI and thus exogenous to the households. The second requirement is untestable but we have seen previously that our instrument plausibly satisfies the exclusion restriction.²⁰

The LATE has very appealing properties in terms of a policy perspective and it is a well-defined economic parameter. Although our estimates capture the treatment effect only for a particular subset of participants, this subset is of great interest from the program perspective. Most of the households in our samples, and the microfinance program in Bangladesh, in general, are compliers.²¹ Microfinance programs are generally designed for the poor landless (or marginal landholding) households to whom our estimates apply. The IV/2SLS allows causal effects among a very particular subset of household: those affected by eligibility criterion. Therefore, the results reported above need not generalize to the households of all participants. In a world of heterogeneous program impacts, LATE and TOT are likely to differ and the differences can be a matter for policy purposes.

5. Evaluation using an Alternative Approach

5.1 Propensity Score Matching (PSM) Method

Below we estimate the treatment effect using the PSM method of Rosenbaum and Rubin (1983). PSM estimators permit us to estimate the impact of a treatment on the treated, and to check the consistency of the results under different assumptions about specification and identification. The main purpose of using PSM is to examine whether

²⁰ AIR show that the stronger the first-stage, the less sensitive the IV estimand to the violations of the monotonicity and CIA assumptions. They also show that the smaller the proportions of defiers, the smaller the bias will be from violations of monotonicity assumption. Also if the causal effects of treatment on outcome are identical for defiers and compliers, violations of the monotonicity assumption generate no bias.

²¹ Following Angrist and Chen (2007) the proportion of households that are compliers is $P[D_{i1}-D_{i0}|D_i=1]=P[E=1]\{P[D_{i1}-D_{i0}=1/P[D_i=1]}\}$, where $E=1$ whether household is eligible, and P stands for probability. For the whole sample, this calculation is $.677*(.506/.575)=.596$.

our basic results hold across different evaluation methods. PSM also provides us a different parameter of interest (TOT) as opposed to IV estimates (LATE).²²

In order to identify the TOT parameter by the PSM technique, our identifying assumption is that outcomes in the untreated state are independent of D_i conditional on a set of observable village and household level characteristics. Rubin (1978) refers to the treatment status that is independent of potential outcomes as an *ignorable* treatment assignment. Although claims for ignorable are usually implausible in a non-experimental setting, it is more plausible in our context that microcredit program status among the program village is ignorable conditional on land holdings and a vector of other covariates. Households in program villages that have less land and non-land assets are likely to participate more. MFI selects households on the basis of eligibility and characteristics that can be observed by a loan officer and a branch manager. It is unlikely that a loan officer, who is unfamiliar to the villagers, could observe an applicant's entrepreneurial ability and drive. However, some treated households are not eligible and that all eligible households do not participate in the program introduces a potential selection bias. The sources of bias could be the differences in observable variables in terms of household size, sex ratio, schooling, age, family composition, and other household characteristics. The survey contains information on most of the characteristics (including the reasons for participation or not participation in the program) that are potential determinants of households' participation in microfinance. Given the richness of data available, we may be willing to assume conditional independence, i.e. selection bias can be eliminated using matching on the covariates.²³ In that case, identification is based on the claim that after conditioning on all observed characteristics that are known to affect participant status, participants and non-participants are comparable.

We move to a step further to alleviate concern regarding selection bias. In particular, we use the regression-adjusted matching estimator developed by Heckman, Ichimura, and Todd (1997). The research by Heckman and his co-authors, Hahn (1998) and many

²² If the selection on observable assumption does not hold the PSM estimates can be interpreted as ITT rather than TOT.

²³ The work by Heckman, his co-authors and others (Dehejia and Wahba 1999, 2002; Michalopoulos, Bloom and Hill 2004) points out that matching estimators perform well when (1) the same questionnaires are used for participants and non-participants; (2) participants and non-participants reside in the same geographic area; and (3) the data contain a rich set of variables relevant to modelling the program-participation decision. Our data meet all these criteria.

others suggest that regression adjustment improves the precision of matching estimate more than by conditioning on propensity score alone. In the regression-adjusted version the residual from the regression of Y_{0j} on a vector of exogenous covariates replaces Y_{0j} as the dependent variable in the matching. Formally, assume a conventional linear model for outcomes in the matched comparison group $Y_o = X\beta_o + U_o$ (the regression is only run on the matched comparison group so it is not contaminated by program participation). Using partial regression methods applied to the comparison group sample, estimate the components of $E[Y_o|X, D=0] = X\beta_o + E[U_o|X, D=0]$ imposing any desired exclusion restriction. We first use local linear regression (LLR) weight and use a bi-weight kernel in estimating local linear matching.²⁴ We then use Nearest Neighbour Matching (NNM). In our empirical work we use the five nearest neighbours. Each of these neighbours receives an equal weight in constructing the counterfactual means. We use matching with replacement where a given non-participant is allowed to match to more than one participant.

Our sample is choice-based, with program participants oversampled relative to their frequency in the population. Therefore, we use matching on the estimate of the odds ratio. We impose the common support restriction based on the method of trimming that was suggested by Heckman, Ichimura, and Todd (1997). In addition, we exclude the two percent of the remaining treatment observations that show the lowest odds-ratio of the non-treated observations. We follow Heckman, Ichimura, and Todd (1997), Behrman, Cheng, and Todd (2004), and Rubin and Thomas (2000) for variable selection to estimate the propensity score and regression adjustment. We include all the variables that may affect both program participation and outcomes. The estimates produced by matching can be quite sensitive to the choice of covariates used to construct propensity score (see Heckman, Ichimura, Smith and Todd 1998; Heckman and Navarro-Lozano 2004). So we use two different specifications to estimate the propensity score. First we use a covariate specification similar to the IV specification. Then we use a more generous specification that includes the detailed household demographic and socio-economic variable and village level characteristics. The list of the full controls is chosen from a set of larger controls, and we chose those that were most often significant in both outcome equation and estimation of propensity score. The final list of variables included

²⁴ The LLR is analogous to running a weighted regression for each program household on only a constant term using all the non-participant data. It is a nonparametric regression technique that improves upon kernel matching. It avoids the boundary points bias associated with kernel, and it can also adapt better to different data densities.

in the matching estimates is reported in the Appendix. Since the list of observed covariates is rich, and additionally we are using regression adjustment, we may claim that we are able to reach sufficiently plausible conclusion using a matching technique.

5.2 PSM Results

We estimate a standard logit model of participation to estimate propensity scores. The results, not reported for brevity, indicate that program participants are more likely to be eligible households. The empirical distribution of the estimated odds-ratio of participants and non-participants are shown in Figure 4 using the coarser set of covariates. It can be seen that there are very few regions of non-overlapping support. For our estimation we exclude non-participants in the non-overlap region, if there is any. Observations with very low or high logs of odds-ratio values are also eliminated as they may indicate a true value of zero or one. However, as is seen in Figure 4, we need to discard only very few observations of the treatment group.²⁵

In Table 9 we present estimates of the treatment parameters using two different matching estimators. The dependent variable is the level of consumption expenditure, as opposed to its logarithm, since many of the coefficient estimates are very small in percentage terms. The average difference in food consumption expenditure between treated households and their non-treated counterparts provides the basis for the estimation of the TOT parameter. The first panel of Table 9 shows the results of both male and female groups together using two covariate specifications of the propensity score. The left side of the Table reports the results based on a coarser set of covariates and the right side presents the results using the same covariate specification used in the IV estimation. The second and third panels represent the corresponding results for female and male groups of borrowers, respectively. All the results are based on the regression-adjusted covariates. Each column of Table 9 represents estimates based on the matched sample of households of different groups of land ownership.

The results for mean impact indicate that the effect of participation on total household consumption is negative for the whole sample. All the results point out that the treatment effect is positive for those households who own less land (\leq one acre of land). Both the LLR and NNM matching estimates give us similar results in both specifications

²⁵ It appears that the imposition of common support is not critical in our application using different sets of covariates for estimating propensity scores.

of the propensity score estimates. We observe similar results for the female group of borrowers. However, all TOT coefficients are positive for male groups. The results are also similar to those obtained from two different matching estimators. The impact estimate is higher for male than for female borrowers. Adding controls for estimating propensity score and regression adjustment does not affect the point estimates much.

Table 10 provides the coefficients of the estimated mean impact on monthly per-capita food consumption expenditure using the same matching estimators. Since the results are not affected by the choice of propensity score estimation we report results based on a broader set of specifications. The results are similar to that of impact on total monthly consumption. All the estimated coefficients are positive starting from the samples of households with one acre of land, and for male groups the treatment effects are always positive. The size of the estimated impact varies with respect to matching estimates and the different groups of land holding households. Overall we find here a stronger coefficient for men than women.

5.3 Spillover Effects

Our identification strategy is based on the implicit assumption that there is no spill-over effect. Formally we make the stable unit treatment value assumption (SUTVA) which assumes that (i) the household's potential outcomes depend on its own participation only and not on the treatment status of other households; and (ii) the microfinance program only affects the outcome of those who participate, and that there is no externality from participant to non-participant. Thus it rules out peer and general equilibrium effects. So, in order to interpret our estimates as TOT effect, the SUTVA must hold. We examine (ii) by estimating the spillover effects. Accordingly, we check whether program affects consumption of non-treated households who live in the treatment villages. The difference in the unconditional mean household monthly food consumption expenditure between non-treated in program and control villages is less than 1 percent of their household monthly consumption expenditure. The difference, though in favour of households in the program village, is not statistically significant. There is also no difference in unconditional food consumption expenditure between eligible non-participants in program and control villages. To increase the precision of estimates we add a set of conditioning variables and run OLS regressions for all non-treated households where the parameter of interest is an indicator variable of whether the household lives in a treatment village. The estimated coefficient is very small and

negative for the full sample of non-participants, while it is also very small but positive for the eligible sub-sample (results not reported here). We then use regression adjusted matching estimates to ensure that we are comparing similar non-participants, and find no support in favour of spillover. We also compare eligible non-treated households who have the same probability of participation, were the program available, and also find no indication of spillover effects. Thus there is no strong evidence in favour of a positive spillover effect.²⁶

6.1 IV versus Matching Estimates

We now compare the main results from IV and matching estimates. We see that the results are similar in terms of the sign of the coefficient estimate. Both estimation results suggest that the effects are positive for a subset of borrowers; those having less land. In terms of the magnitude of the coefficients, the matching estimates are substantially smaller than the IV estimates.²⁷ The estimated standard errors are larger in case of matching than the IV estimates. Therefore, unlike IV estimates that show modest positive effects on the consumption of eligible or other poorer participants, matching estimates leads to smaller positive and statistically insignificant effects on consumption. The lack of statistical significance in the coefficient estimates is partially the results of the smaller sample size (see also footnote 14). The divergence between IV and PSM estimates in terms of standard error is probably best explained by differences in the heterogeneity among households. The matching estimator combines propensity score weighting schemes to estimate the TOT. Households most likely to participate get the highest weights in matching estimates. On the other hand, IV produces covariate-specific variance weighted average effects. The two weighting schemes are likely to cause different estimates (Angrist and Krueger 1999).²⁸ Moreover, our matching estimates only consider extensive margin (where all treated households are classified in the same way) but not intensive margin (intensity of the treatment). Treating differently treated

²⁶ However, to the extent that the change in behaviour and therefore the resulting program impacts among the treated influence their peers within the group, we are not correct in claiming that there are no spillover effects. In the presence of violation of SUTVA, our estimates are the lower bound to the true effects.

²⁷ Since IV estimates are all in terms of logarithms we multiply IV estimates by household total consumption expenditures in order to compare the IV with the matching estimates.

²⁸ See Heckman and Navarro-Lozano (2004) for more details for comparisons between matching and IV based on treatment parameters.

households as the same, as a binary approach would do, thus seems likely to understate the potential effect of full treatment of microcredit.^{29,30}

The two estimates produce results for two different subgroups of borrowers. The IV estimate applies to a smaller treatment group than the matching estimates. The larger coefficient estimates by IV rather than matching implies that the impacts of microfinance for the ‘compliers’ are higher than ‘always-takers’. This result might be counterintuitive in the sense that the treatment effect for the marginal group (poorer households) should be smaller than the average treatment effect on the treated. However, this need not be the case here because IV estimates the impact of the program for those households that are more credit constrained and/or have greater immediate need to improve their consumption. They are also more likely to participate in a microcredit program. So, it is possible that gains from participation are higher for them.

While IV is a standard technique for non-experimental impact estimates, recent evidence in favour of matching is compelling. However, there is no guarantee that selection on observables will eliminate the total bias (unless they go in the same direction). So, matching estimates may still be biased if there are any latent factors correlated with participation decision and counterfactual outcomes.³¹ The IV method can overcome these problems: Under IV assumptions and the assumed functional form, IV estimation identifies the causal effect robustly to unobserved heterogeneity. However, the IV estimates are valid for the group of compliers and may not be informative for the other group (always-takers).³²

²⁹PSM techniques are generally confined to binary treatment scenarios. However, some possible extensions have been suggested. For example, Hirano and Imbens (2004) develop a generalized propensity score method when treatment is defined as a continuous variable.

³⁰Angrist (1998) finds larger standard error of estimates in covariate matching than the estimates obtained using IV. Zhao (2006) compares the performance of PSM and covariate matching estimators, and finds that PSM estimators have larger standard errors. So our results are consistent with both Angrist and Zhao.

³¹Blundell, Dearden and Sianesi (2005) argue that the plausibility of the selection on observable assumption of PSM method should always be discussed on a case-by-case basis. In our case, the assumption seems reasonable due to informational richness of the data, and the simple mechanism (land-based eligibility criterion) for participation into the program. However, there are some complexities as well since a number of households from the ineligible group received treatment and some eligibles did not take up the program. So, the divergence between IV and PSM could also be due to unobserved heterogeneity in the selection process. See Attanasio and Vera-Hernandez (2004) for a related issue in the context of evaluation of a large nutrition program in Columbia.

³²Our results indicate that the concern regarding selection bias in non-experimental data can be less problematic if the researcher establishes good interactions with borrowers and providers before evaluating the program. In particular, one needs to know how MFI selects villages and households, and why households enter programs. It is, however, to be noted that we do not rely on (regression-adjusted) matching results to conclude our findings since the possibility of selection based on observables could still

6.2 Summary and Conclusion

Using different non-experimental impact evaluation techniques we find similar results concerning the impact of microfinance. We use different instruments and our results are robust to the use of instruments. Overall, the results indicate that they do not entirely depend on different specification and identifying assumptions. We also estimate the heterogeneous treatment effect by estimating the sub-group specific mean treatment effect where the groups are categorized based on the targeting criterion. Our approach is novel in the sense that we do not estimate treatment effects based on an outcome variable, such as quantile treatment effects which suffers from the strong assumption of rank preservation (Bitler, Gelbach and Hoynes 2006). The results indicate that there is substantial heterogeneity in the causal impact of participation in microfinance.

The overall results suggest that the effect of microfinance on household consumption expenditure does not seem to be strong. It raises the doubt of whether microfinance can be a first-track poverty reduction program. The IV estimates indicate an increase of 6 to 14 percent in the consumption expenditure of the relatively poor participating households. Overall, results signify that, conditional on positive impacts, stronger coefficient estimates are observed for men participants. However, men participants borrow more, so larger treatment impact could be the results of increasing returns in household enterprises. The results for men are based on a very small sample size, and should be interpreted with caution. Note that these results are not directly comparable with PK's study since we are considering different set of MFIs, and there is no overlap in the households/MFIs evaluated by these two studies.

In general, we find an inverse relationship between household land ownership and the benefits from participation in microcredit program: the lower the amount of land a household has the stronger is the effect of participation in microfinance. The benefits are lower, or sometimes even negative, for those households marginal to the participation decision. The results indicate that the effects of microfinance loans are not strong across all groups of poor households. Rather, those among the poorest of poor participants are

be questionable (see, for example, Angelucci and Attanasio (2006) who argue against standard matching approach for non-random assignment of the program, and non-random participation into the program. They propose semi-parametric estimator by combining matching and IV approach). We do not use standard PSM estimate, and we think that the PSM results are at least indicative.

most likely to benefit from participating.³³ The results also imply that microcredit loans may not be effective for land-rich households. Moreover, they are not the focus of the microcredit loans as these groups are not officially eligible. They are also less likely to participate in a microfinance program. The findings indicate that the simple targeting mechanism of microfinance program in Bangladesh based on household land ownership is effective. Hence the efficiency of the microfinance program can be enhanced by allocating credit to those, namely the poorest marginal landholding households, for which the impact is the greatest.

³³ The results may have different interpretations. For example, credit may induce poorer households to increase their consumption while it may not have any sizeable effect on consumption of a relatively less poor because they might invest their money on a long-term project. However, MFI requires that loans to be repaid within a year with payment start about four weeks later upon receiving the loan. It may still be possible that the poorer households use their loan more to augment consumption. My field visits do not support the claim. Also the survey data regarding the use of loans indicate that more than 90 percent of all treated households use loan for productive investment and that there is no difference between poorer and less poor households in this respect. Moreover, MFIs monitor the use of loan, and because of the repayment concern (unlike cash transfer program like Progressa) almost in each week, households cannot sustain their higher level consumption without investing money.

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Table 1- Village Level Descriptive Statistics

Variable	Control village (I)	Program village (II)	Difference III=(II-I)	t-stat
Education Facilities:				
Primary school	90.91	86.25	-4.66	0.42
Secondary school	27.27	31.25	3.98	0.26
Maktab/ Madrasa (Religious School)	81.82	90.00	8.18	0.80
Health Facilities:				
Union health centre	10	17.5	7.5	-0.59
Allopathic doctor	50	42.5	-7.5	0.45
Homeopath doctor	40	38.75	-1.25	0.08
Allopathic medicine store	80	45	-35	2.12
Transport, Communication and Infrastructure:				
Electricity connection	17	26	9	3.2
Presence of pucca road	10.6	34.8	24.2	8.4
Distance to nearest Thana (in km)	11.91	7.14	-4.77	-2.07
Presence of grocery market	18.2	22.5	4.3	0.33
Presence or absence of frequent haat (big market)	27.3	32.5	5.2	0.35
Presence of bus stand	9.1	15	5.9	0.59
Presence of post office	18.2	20	1.8	0.14
Presence of telephone office	9.1	6.3	-2.8	-0.3
Presence of Union Parishad (local Government) office	18.2	13.8	-4.4	-0.35
Irrigation Equipment:				
Number of low lift pump	0.27	0.44	0.16	0.23
Number of shallow tube-well	11.82	12.13	0.31	0.05
Number of hand tube-well for drinking water	68	78.04	10.04	0.39
Credit related options:				
Percentage of crop received by land owner in sharecropping	49.55	47.53	-2.02	0.96
Number of money lenders from this village	3.73	7.91	4.19	2.4
Number of people who provides advances against crops	2.73	3.85	1.12	0.79
Number of small credit/savings groups	0.91	0.76	-0.15	-0.39

Notes: The first column of the Table presents the mean of each variable for the control villages, and the second presents the same for the treatment villages. The third column presents the difference between the two, and the fourth provides the t -statistics for the mean difference of participating and non-participating households.

Table 2- Selected Descriptive Statistics of Households

Variable	All Sample				Samples of Eligible Households					
	Non-participant (I)	Particip ant (II)	Difference III=(II-I)	p-value (IV)	K-S	Non-participant(I)	Particip ant(II)	Difference III=(II-I)	p-value (IV)	K-S
Age of household head	45.14	43.91	-1.224	0.013	0.000	41.18	41.66	0.481	0.393	0.001
Sex of household head	0.93	0.95	0.026	0.003	0.701	0.91	0.94	0.034	0.004	0.609
Marital status of household head	0.90	0.93	0.033	0.001	0.386	0.88	0.91	0.031	0.022	0.735
Whether household head is illiterate	0.35	0.31	-0.049	0.005	0.060	0.44	0.33	-0.103	0.000	0.000
Whether household head can sign only	0.22	0.34	0.115	0.000	0.000	0.23	0.37	0.134	0.000	0.000
Whether household head can read only	0.01	0.01	0.003	0.394	1.000	0.01	0.01	0.001	0.736	1.000
Whether household head can read and write	0.42	0.35	-0.069	0.000	0.002	0.33	0.29	-0.032	0.120	0.682
Highest education achieved by any member	5.72	5.26	-0.453	0.003	0.000	4.28	4.47	0.195	0.262	0.001
Highest education achieved by any male member	5.17	4.59	-0.583	0.000	0.000	3.72	3.78	0.065	0.719	0.052
Highest education achieved by any female member	3.52	3.19	-0.323	0.013	0.001	2.57	2.69	0.125	0.388	0.004
Total arable land owned by household	80.51	58.37	-22.1	0.000	0.000	7.12	7.46	0.340	0.583	1.000
Household size	5.45	5.67	0.220	0.009	0.000	5.03	5.40	0.364	0.000	0.000
Number of children age below 6 years	0.87	0.91	0.035	0.295	0.617	0.93	0.95	0.022	0.590	0.562
Number of children of age 6-15	1.29	1.53	0.237	0.000	0.000	1.21	1.45	0.240	0.000	0.000
Number of old people of age above 60 years	0.29	0.21	-0.077	0.000	0.004	0.22	0.17	-0.051	0.009	0.276
Number of 15-60 yrs old male member	1.59	1.59	-0.004	0.906	0.672	1.37	1.46	0.088	0.028	0.232
Number of 15-60 yrs old female member	1.41	1.43	0.029	0.288	0.794	1.30	1.36	0.064	0.031	0.120
Number of male member in the family	2.90	2.96	0.056	0.324	0.057	2.63	2.78	0.152	0.019	0.027
Number of female member in the family	2.55	2.72	0.164	0.001	0.005	2.41	2.62	0.212	0.000	0.004
Lives in a nuclear family (=1) or joint family (=0)	0.67	0.69	0.020	0.236	0.921	0.69	0.72	0.026	0.208	0.896
Whether household head is a farmer	0.35	0.32	-0.029	0.090	0.542	0.20	0.21	0.008	0.646	1.000
Whether head is a agricultural labour	0.18	0.16	-0.023	0.089	0.817	0.27	0.21	-0.063	0.001	0.043
Whether head is a non-agricultural labour	0.16	0.22	0.054	0.000	0.025	0.22	0.28	0.066	0.001	0.030
Whether head is self-employed or businessman	0.13	0.19	0.054	0.000	0.027	0.16	0.20	0.037	0.035	0.518
Whether head is salaried job holder	0.09	0.05	-0.040	0.000	0.178	0.08	0.04	-0.041	0.000	0.382
Whether head is doing any other job	0.08	0.06	-0.015	0.105	0.996	0.07	0.07	-0.007	0.541	1.000

Notes: The Table gives the summary statistics of the participant and non-participant households. The left-hand side presents the results for the full sample and the right-hand side shows the same results for eligible households. Reported p-values are the two-tailed tests of the null hypothesis that program and control group means are equal. The *K-S* test column is based on the Kolmogorov-Smirnov test of equality of distribution.

Table 3- Summary Statistics of Consumption and Credit Variables

Variable	Men (I)	Women (II)	Difference (I-II)	P- value	K-S
Total of Amount Credit (Taka)	4650.9 (3961.7)	3799.4 (2115.2)	851.6 (176.1)	0.000	0.015
Total Length of Membership (in years)	4.1 (3.4)	3.3 (2.7)	0.8 (0.2)	0.000	0.001
Number of Borrowers per Household	1.4 (0.6)	1.1 (0.3)	0.3 (0.0)	0.000	0
Household Total Monthly Consumption	2783.6 (2192.3)	2365.0 (1723.2)	418.5 (130.4)	0.001	0.003
Household Per-capita Consumption	497.6 (394.0)	436.5 (325.4)	61.1 (24.3)	0.013	0.002
Number of Observation	213	1565			
	Participant	Non- participant	Difference	P- value	K-S
Household monthly Food Consumption (tk.)	2415.1 (1801.1)	2456.6 (1890.9)	41.5 (67.7)	0.730	0
Household monthly Per-capita Food Consumption (tk)	443.3 (336.1)	467.7 (337.4)	24.4 (12.4)	0.049	0.022
Household monthly Food Consumption in program village (tk.)	2417.8 (1806.9)	2461.8 (1942.7)	44.1 (73.6)	0.550	0.112
Per-capita monthly Food Consumption in program Village (tk)	444.3 (337.0)	477.8 (358.3)	33.5 (13.7)	0.014	0.028

Notes: The top panel represents the descriptive statistics of the selected variables for men and women participants in microfinance. The bottom panel gives the descriptive statistics of the treatment and comparison households. Reported p-values are the two-tailed tests of the null hypothesis that column II and I are equal. (K-S) based on Kolmogorov-Smirnov test of equality of distribution, Standard errors are in parenthesis

Table 4- Difference-in-difference Estimates: The Impacts of Eligibility

Dependent Variable: Household Log of Total Monthly Food Consumption Expenditure						
Estimated Coefficient	Household Land Ownership					
	All sample	Land ≤ 500	Land ≤ 200	Land ≤ 100	Land ≤ 50	Landless
δ_1	-0.015 (0.046)	-0.021 (0.047)	-0.051 (0.054)	-0.027 (0.077)	0.038 (0.034)	0.051 (0.040)
δ_2	-0.104 (.0520)**	-0.0983 (.0549)+	-0.0788 (.0644)	-0.0361 (.0866)		
δ_3	0.0471 (.0523)	0.0532 (.0531)	0.0826 (.0588)	0.0628 (.0799)		
$\delta_1 + \delta_3$	0.0321	0.0322	0.0316	0.0358	0.038	0.051
Dependent Variable: Log of Per Capita Monthly Food Consumption Expenditure						
δ_1	-0.023 (0.046)	-0.026 (0.047)	-0.048 (0.054)	-0.025 (0.077)	0.039 (0.035)	0.048 (0.041)
δ_2	-0.104 (.0522)**	-0.092 (.0550)+	-0.068 (.0647)	-0.0337 (.0872)		
δ_3	0.0541 (.0525)	0.057 (.0532)	0.0794 (.0591)	0.0592 (.0804)		
$\delta_1 + \delta_3$	0.0311	0.031	0.0314	0.0342	0.039	0.048

Notes: Clustered standard errors are reported in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%. Coefficients are those from estimation of reduced form equation (4). Regressions also include household and village level characteristics and MFI fixed effects.

Table 5- Wald Estimates of Impacts of Microfinance

Dependent Variable: Household Log of Total Monthly Food Consumption Expenditure						
Treatment Variable	All	All eligible	Women	Eligible Women	Men	Eligible Men
Whether treated or not	-1.345 (0.146)*	0.249 (0.292)	-1.313 (0.148)*	0.219 (0.295)	-1.434 (0.146)*	0.102 (0.342)
Total amount of credit ¹	-0.8746 (0.0605)*	0.1616 (0.1183)	-0.8430 (0.0620)*	0.1381 (0.11589)	-0.8864 (0.0896)*	0.0631 (0.1385)
Dependent Variable: Log of Per Capita Monthly Food Consumption Expenditure						
Participation Variable	All	All eligible	Women	Eligible Women	Men	Eligible Men
Whether treated or not	-0.621 (0.121)*	0.312 (0.258)	-0.633 (0.122)*	0.273 (0.260)	-0.552 (0.190)*	0.386 (0.264)
Total amount of credit ¹	-0.4042 (0.0522)*	0.2024 (0.1020)**	-0.4080 (0.0535)*	0.1722 (0.0997)+	-0.3419 (0.0752)*	0.2392 (0.1173)**

Notes: Each cell in the Table corresponds to a separate regression. The first row in each panel represents regression of log of consumption expenditure on a dummy for treatment status using eligibility status as instrument for treatment. The second row of each panel reports the corresponding estimated coefficients using continuous treatment measure (the amount of credit borrowed). Regressions do not include any other covariate. Clustered standard errors are reported in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%. ¹ Coefficient estimates and the corresponding standard errors are multiplied by the average amount of credit borrowed by households of the respective group of land ownership (assuming constant marginal benefit from the credit).

Table 6- IV Estimates of Impact of Microfinance on Household Consumption

(Dependent variable: Household Log of Total Monthly Food Consumption Expenditure)

Both Men and Women Treatment Variable	Household Land Ownership (in decimal)						Adjusted R ²
	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless	
Whether treated or not	-0.216 (0.102)**	-0.131 (0.113)	0.049 (0.129)	0.128 (0.140)	0.126 (0.164)	0.156 (0.193)	(0.46-0.49)
Amount of credit ¹	-0.1282 (0.0660)+	-0.0660 (0.0738)	0.0692 (0.0885)	0.1173 (0.0984)	0.1335 (0.1186)	0.1745 (0.1374)	(0.46-0.49)
Mean Consumption (Tk.)	2432.7	2384.3	2319.1	2228.9	2126.3	2082.8	
Observations	3026	2960	2780	2462	2034	1471	
Women							
Whether treated or not	-0.21 (0.107)+	-0.157 (0.116)	0.027 (0.132)	0.109 (0.144)	0.149 (0.167)	0.218 (0.197)	(0.46-0.49)
Amount of credit ¹	-0.1308 (0.0688)+	-0.0978 (0.0754)	0.0357 (0.0880)	0.0830 (0.0976)	0.1160 (0.1191)	0.1886 (0.1387)	(0.46-0.49)
Mean Consumption (Tk.)	2406.1	2359.5	2302.8	2216.5	2108.0	2067.3	
Observations	2813	2755	2591	2299	1904	1377	
Men							
Whether treated or not	-0.151 (0.154)	-0.013 (0.168)	0.146 (0.190)	0.21 (0.210)	0.051 (0.248)	0.124 (0.299)	(0.52-0.55)
Amount of credit ¹	-0.0360 (0.122)	0.0849 (0.1340)	0.2602 (0.1500)+	0.2898 (0.1548)+	0.2299 (0.1753)	0.2520 (0.2159)	(0.52-0.55)
Mean Consumption (Tk.)	2505.2	2436.8	2350.0	2215.4	2120.8	2085.2	
Observations	1461	1420	1305	1127	922	673	

Notes: Each cell in the Table corresponds to a separate regression. The first row in each panel represents a separate regression of log of total household monthly consumption expenditure on a dummy for treatment status, controlling for household and village level characteristics and MFI fixed effects, and using eligibility status of household as instrument for treatment indicator. The second row of each panel reports the corresponding estimated coefficients using continuous treatment measure (the amount of credit borrowed). Clustered standard errors are reported in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%. ¹ Coefficient estimates and the corresponding standard errors are multiplied by the average amount of credit borrowed by households of the respective group of land ownership (assuming constant marginal benefit from the credit).

Table 7- IV Estimates of Impact of Microfinance on Per-capita Consumption
(Dependent Variable: Log of Per-capita Monthly Food Consumption Expenditure)

Both Men and Women Treatment Variable	Household Land Ownership(in decimal)						Adjusted R ²
	All sample	Land ≤500	Land ≤200	Land≤100	Land ≤50	Landless	
Whether treated or not	-0.203 (0.103)**	-0.119 (0.113)	0.037 (0.129)	0.080 (0.141)	0.072 (0.165)	0.086 (0.197)	(0.26-0.28)
Amount of credit ¹	-0.1071 (0.0677)	-0.0437 (0.0756)	0.0826 (0.0885)	0.1109 (0.0977)	0.1361 (0.1196)	0.1670 (0.1415)	(0.26-0.28)
Per-capita Consumption (Tk.)	453.7	449.9	444.4	435.2	423.6	415.1	
Observations	3026	2960	2780	2462	2034	1471	
Women							
Whether treated or not	-0.177 (0.108)	-0.14 (0.116)	0.019 (0.133)	0.066 (0.145)	0.104 (0.168)	0.151 (0.201)	(0.25-0.28)
Amount of credit ¹	-0.098 (0.0690)	-0.0734 (0.0756)	0.0494 (0.0884)	0.0784 (0.0983)	0.1234 (0.1199)	0.1809 (0.1412)	(0.25-0.28)
Per-capita Consumption (Tk.)	450.5	446.8	442.4	433.2	420.1	413.1	
Observations	2813	2755	2591	2299	1904	1377	
Men							
Whether treated or not	-0.143 (0.156)	0.014 (0.170)	0.171 (0.192)	0.188 (0.213)	0.038 (0.252)	0.099 (0.309)	(0.30-0.33)
Amount of credit ¹	-0.0142 (0.1230)	0.1256 (0.1352)	0.3015 (0.1515)**	0.2992 (0.1571)+	0.2443 (0.1782)	0.2470 (0.2229)	(0.30-0.33)
Per-capita Consumption (Tk.)	472.8	466.1	459.2	444.7	437.5	427.2	
Observations	1461	1420	1305	1127	922	673	

Notes: Each cell in the Table corresponds to a separate regression. The first row in each panel represents a separate regression of log of per-capita monthly consumption expenditure on a dummy for treatment status, controlling for household and village level characteristics and MFI fixed effects, and using eligibility status as instrument for treatment. The second row of each panel reports the corresponding estimated coefficients using continuous treatment measure (the amount of credit borrowed). Clustered standard errors are reported in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%. ¹ Coefficient estimates and the corresponding standard errors are multiplied by the average amount of credit borrowed by households of the respective group of land ownership (assuming constant marginal benefit from the credit).

Table 8- 2SLS Estimates of Impact of Participation in Microfinance

(Dependent Variable: Household Log of Per-Capita monthly Food Consumption Expenditure)

Instrument: $V \times E \times$ the number of years in microfinance in program village							
Household Land Ownership(in decimal)							
	All sample	Land ≤ 500	Land ≤ 200	Land ≤ 100	Land ≤ 50	Landless	Adjusted R ²
All	-0.0645 (0.0671)	-0.0080 (0.07160)	0.0812 (0.0785)	0.1183 (0.0838)	0.0579 (0.0956)	0.1360 (0.1118)	(0.26-0.28)
Women	-0.0491 (0.0702)	-0.0186 (0.0741)	0.0674 (0.0813)	0.1039 (0.0866)	0.0583 (0.0988)	0.1440 (0.1156)	(0.26-0.28)
Men	-0.0481 (0.1234)	0.0684 (0.1319)	0.2044 (0.1431)	0.1883 (0.1447)	0.1357 (0.1572)	0.3045 (0.1959)	(0.31-0.33)
Instrument: $V \times E \times$ separate dummies for each year in microfinance in program village							
	All sample	Land ≤ 500	Land ≤ 200	Land ≤ 100	Land ≤ 50	Landless	Adjusted R ²
All	-0.0323 -.0581	0.0187 -.0625	0.1040 .0689	0.1304 (.07.33)+	0.1127 -.0813	0.2467 (.0952)*	(0.26-0.28)
F-test	[p= 0.008]	[p= 0.004]	[p= 0.000]	[p= 0.000]	[p= 0.000]	[p= 0.000]	
Women	-0.0241 (0.0588)	.0001 (0.0624)	0.0813 (0.0690)	0.1094 (0.0740)	0.0969 (0.0823)**	0.2446 (0.0962)**	(0.26-0.28)
Men	-0.0252 (0.1053)	0.07257 (0.1129)	0.1834 (0.1222)	0.1677 (0.1221)	0.1428 (0.1279)	0.3014 (0.158012)+	(0.31-0.33)

Notes: Each cell in the Table corresponds to a separate regression of log of per-capita monthly consumption expenditure on amount of credit borrowed as treatment variable, controlling for household and village level characteristics and MFI fixed effects. Clustered standard errors are reported in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%. All the Coefficient estimates and the corresponding standard errors are multiplied by the average amount of credit borrowed by households of the respective group of land ownership (assuming constant marginal benefit from the credit). The F-test is for whether the coefficients on the excluded instruments are jointly equal to zero, conditional on all other controls.

Table 9- Matching Estimates of Impact of Participation in Microfinance
 (Dependent Variable: Household Total Monthly Food Consumption Expenditure (in Taka))

Regression Adjusted Estimates	(Estimation based on full set of covariates)					(Estimation based on IV set of covariates)					
	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	
Both Women and Men											
Local Linear	-17.01 (68.57)	-4.47 (69.97)	-16.88 (70.61)	19.47 (62.09)	37.65 (65.17)	50.01 (72.04)	-20.66 (77.34)	8.20 (91.36)	40.39 (70.04)	30.50 (67.51)	38.47 (88.65)
Nearest 5-neighbour	-31.93 (75.24)	32.29 (73.67)	-8.75 (74.03)	37.85 (76.54)	53.33 (73.47)	49.14 (85.16)	-14.97 (74.37)	-5.66 (73.20)	34.92 (72.45)	23.23 (76.41)	45.41 (80.60)
Women											
Local Linear	-42.28 (93.45)	-28.20 (83.39)	-7.19 (95.16)	28.51 (64.28)	28.24 (65.23)	32.11 (77.64)	-44.30 (73.11)	-0.29 (82.97)	32.17 (70.57)	17.31 (73.18)	4.24 (81.80)
Nearest 5-neighbour	-33.84 (80.53)	-29.18 (74.89)	74.89 (76.64)	-0.39 (75.90)	18.05 (71.48)	19.72 (84.20)	-30.70 (76.84)	22.22 (76.57)	24.34 (77.56)	67.60 (72.03)	20.37 (77.24)
Men											
Local Linear	90.00 (175.26)	85.98 (187.78)	137.47 (191.44)	156.04 (161.95)	322.13 (207.65)	302.08 (219.44)	162.55 (188.98)	128.54 (196.29)	160.94 (173.84)	249.43 (168.08)	249.22 (229.54)
Nearest 5-neighbour	89.40 (161.10)	69.98 (165.51)	83.81 (169.77)	51.61 (175.04)	197.60 (211.81)	301.09 (269.22)	189.42 (138.03)	208.08 (146.19)	181.48 (157.92)	208.84 (209.66)	274.78 (221.80)

Notes: Bootstrapped standard errors are shown (in parentheses) for local linear estimator. They are based on 100 replications with 100% sampling. Standard errors for the nearest neighbour estimator are based on Abadie and Imbens (2006). In the estimation of LLR matching the densities were estimated using a bi-weight kernel and a fixed bandwidth of 0.06. IV set of covariates include those variables included in X in the estimation of equation (1). The full set of covariates includes a coarser set of specifications listed in the Appendix. All the coefficients estimation is carried out using regression-adjusted version of the corresponding matching estimator.

Table 10- Matching Estimates of the Impact of Participation in Microfinance
 (Dependent Variable: Household Monthly Per-capita Food Consumption Expenditure (in Taka))

Regression Adjusted Estimates of	(Estimation based on full set of covariates)					
	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless
	Both Women and Men					
Local Linear	-1.26 (14.67)	0.70 (14.52)	-1.34 (15.08)	7.10 (15.86)	8.08 (16.20)	5.79 (19.12)
Nearest 5-neighbour	-10.53 (15.40)	7.29 (15.31)	0.77 (16.04)	7.23 (16.68)	10.66 (16.73)	7.36 (20.21)
	Women					
Local Linear	-4.56 (15.23)	-1.49 (15.03)	1.38 (15.72)	10.44 (16.54)	8.15 (16.65)	9.39 (20.35)
Nearest 5-neighbour	-2.66 (16.40)	-1.20 (15.96)	1.76 (16.38)	13.28 (16.72)	6.67 (16.27)	6.52 (19.79)
	Men					
Local Linear	10.11 33.43	14.35 32.83	12.43 32.98	27.22 42.93	59.91 63.56	26.58 43.14
Nearest 5-neighbour	15.81 (31.60)	14.60 (33.60)	5.23 (33.90)	7.68 (34.06)	27.75 (41.14)	18.20 (49.05)

Notes: Bootstrapped standard errors are shown (in parentheses) for local linear estimator. They are based on 100 replications with 100% sampling. Standard errors for the nearest neighbour estimator are based on Abadie and Imbens (2006). In the estimation of LLR matching the densities were estimated using a bi-weight kernel and a fixed bandwidth of 0.06. The full set of covariates includes a coarser set of specifications listed in the Appendix. All the coefficients estimation is carried out using regression-adjusted version of the corresponding matching estimator.

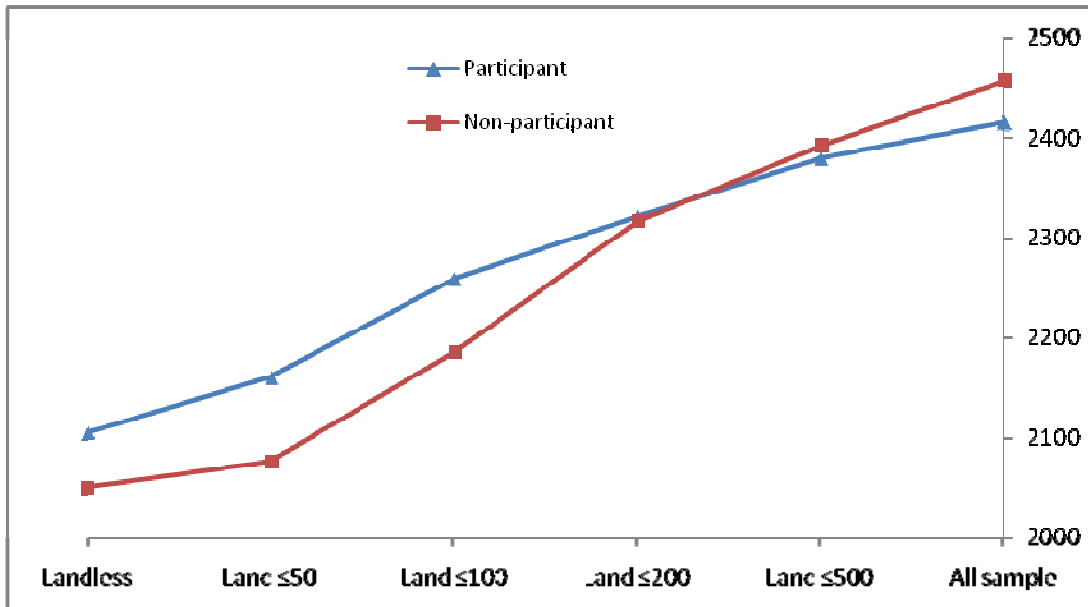


Figure 1a: Household consumption expenditure by land ownership

Notes: Figure shows regression un-adjusted household monthly consumption expenditure (in taka) by land ownership. Land ≤ 50 implies household who own less than or equal to 50 decimal (half-acre) land, and so on.

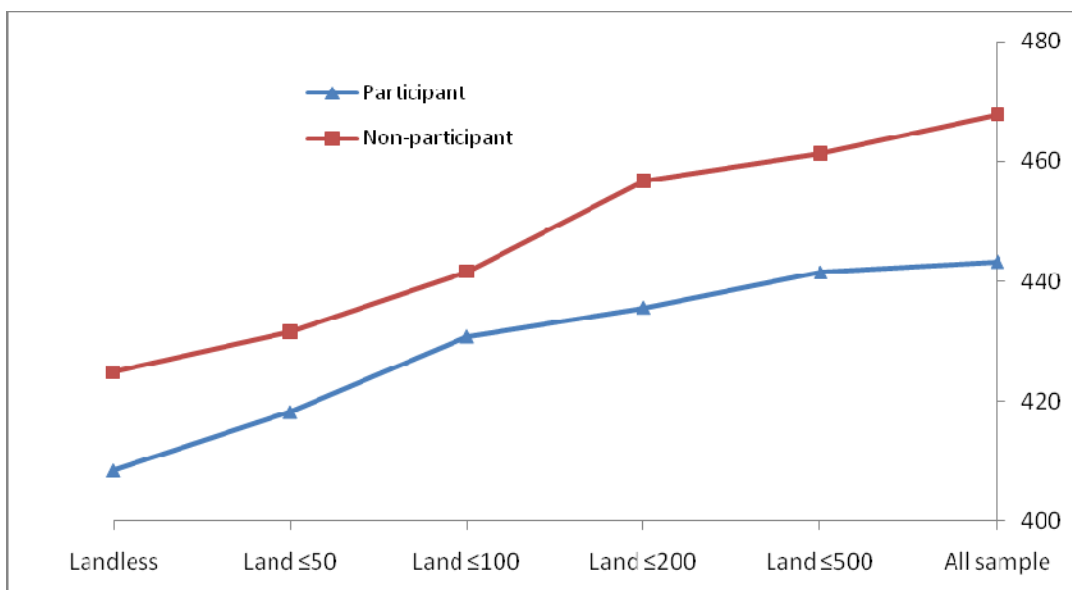


Figure 1b: Per-capita consumption expenditure by land ownership

Notes: Figure shows regression un-adjusted per-capita consumption expenditure (in taka) by land ownership. Land ≤ 50 implies household who own less than or equal to 50 decimal (half-acre) land, and so on.

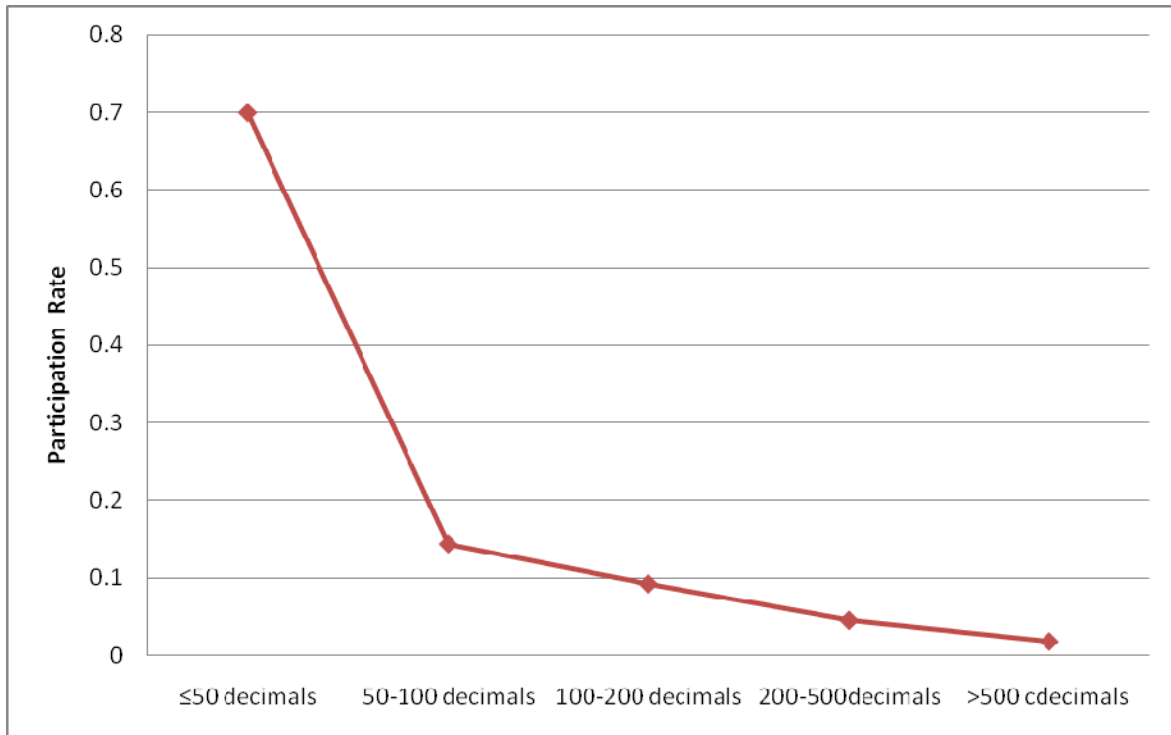


Figure 2a: Participant Rate by Household Land Holding

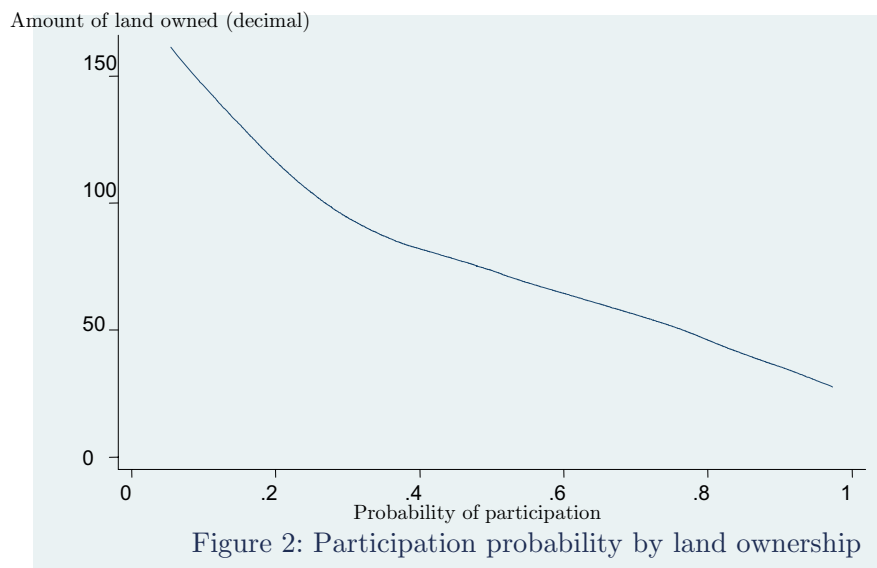
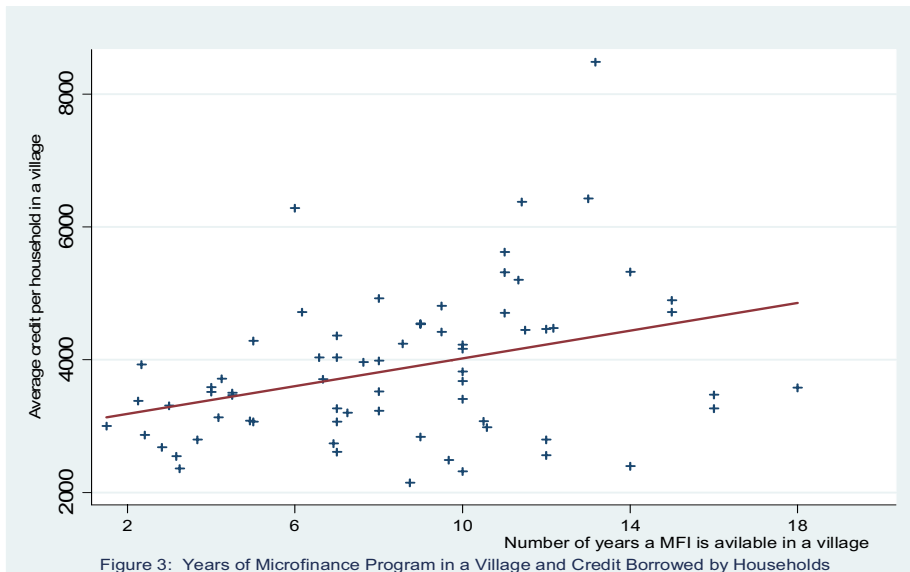


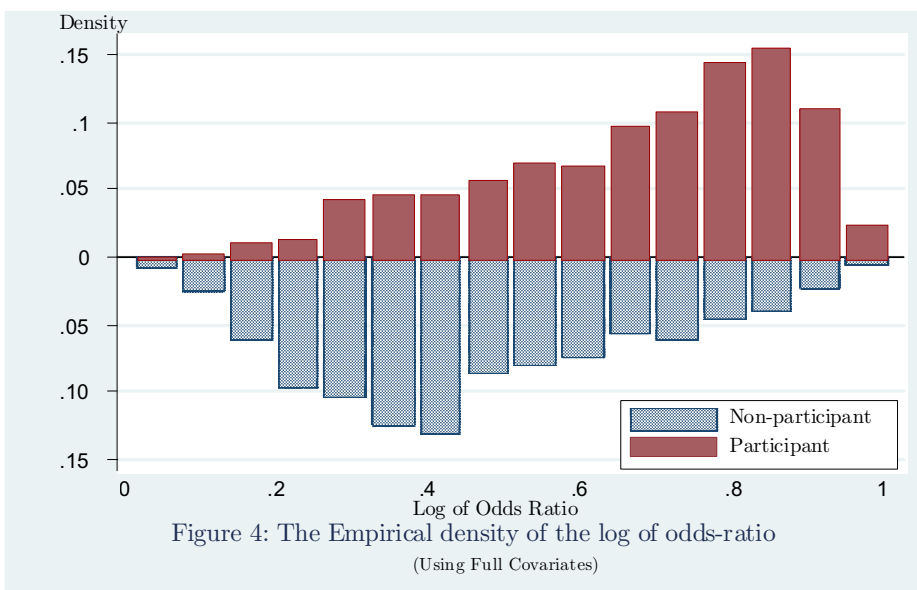
Figure 2: Participation probability by land ownership

Figure 2b: The Distribution of Per-capita Food Consumption Expenditure

Notes: Figure shows lowest locally weighted regression of the amount of land owned by households on their probability of participation in microfinance (using quartic kernel with bandwidth of 0.8). Regression-adjusted probability of participation is obtained by conditioning on household and village level characteristics, MFI fixed effects, and instrument (eligibility status of household in program village)



Notes: Average credit per household in a village is the amount of credit borrowed (in taka) by all households divided by the number of participating households in a program village. Number of years a MFI is available in a village is the period from which microfinance is first available in a program village.



Appendix

List of Variables:

Variables used in IV estimation and Program Participation Model

Household Level variables:

Age of household head, Square of the age of household head, Sex of household head, Marital status of household head, Education level of household head and spouse (illiterate, can sign only, can read only, can read and write), Whether household head has spouse, Highest grade achieved by a member in the household, total arable land owned by household, Number of children age below 6 years, age 6-15, Dependency ratio, Number of 15-60 years old male and female member, Type of family (joint family or semi-nuclear, nuclear), Dummies for occupation of the household head (farmer, agricultural labour, non-agricultural labour, self-employed or businessman, professional or salaried job holder, any other job), Electricity connection, Number of living room (beside bathroom/kitchen), If cement or brick used in any of the living room, Whether condition of house is good, liveable, or dirty, Whether household has separate kitchen, toilet facility.

Village level Variable:

Presence or absence of primary school, secondary school or college, health facility, Adult male wage in the village, presence of brick-built road, regular market, post office, local government office, youth organization, Distance to nearest thana, Number of money lenders, large farmers/traders who provides advances against crops in the village, Number of small credit/savings groups in the village, Price of Rice, wheat, oil, potato.

Additional Variables used in estimating the Propensity Score:

Additional covariates used in the PSM estimator are household demographic and socio-economic variables decomposed into various categories (e.g., age is divided into different groups), additional household level variable (e.g., number of daughter, son) additional village level characteristics (e.g., average male, female daily wage). This is a larger set of variables and interactions that are selected to maximize the percentage of observation classified under the model.