Using Non-Experimental Methods To Evaluate The Impact of Microfinance: Evidence From Bangladesh

Asadul Islam[1]

June 2007

Abstract:

Bangladesh is well known for its pioneering provision of microfinance programs, principally in the form of tiny collateral-free loans to poor women. This paper evaluates the impact of microfinance on household consumption using a new, large and unique cross-section data set. It employs a number of non-experimental impact evaluation techniques, e.g., instrumental variables and propensity score matching. The findings 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 benefiting most from the scheme. The results hold across different specifications and methods, and when corrected for various sources of selection bias including possible spillover effects.

JEL Classification: C14, C31, H43, I30, L30, O12

Key Words: Microfinance, IV, LATE, TT, Matching, Consumption.

[1] Department of Economics, Monash University, Clayton VIC 3800, Australia, and Bangladesh Institute of Development Studies (BIDS), Email: asadul.islam@buseco.monash.edu.au, Phone: (613) 9905-2382. I thank BIDS for giving me the opportunity to do this research on leave from the institute. I thank Pushkar Maitra, Mark Harris, Dietrich Fausten, Stephen Miller, participants at the 3rd development workshop at Sydney University, and various seminar participants at Monash University and BIDS for very helpful comments and suggestions. The author bears sole responsibility for the contents of this paper.

Introduction

Interest in microfinance has been rekindled with the award of the 2006 Nobel Peace Prize to Professor Yunus and Grameen Bank (GB), the bank Yunus founded more than three decades ago. Microcredit[1] typically involves the provision of collateral-free small loans to jointly liable people for the purpose of self-employment and income generating activities. It focuses on women, and frequently it offers targeted training and information sessions for the best use of the loan. Microcredit programs expanded rapidly in Bangladesh, generating a wave of enthusiasm in development circles. The microfinance industry has been growing tremendously throughout the developing world. The number of people who received credit from the microfinance institutions (MFIs) rose from 13.5 million in 1997 to 113.3 million in 2005. During the same period the number of MFIs increased from 618 to 3133 (Daley-Harris 2006).

Microfinance has become a prominent element of development strategies. Most development practitioners, or policy makers, believe that microfinance can help to break out of poverty. The academic world has also shown increased interest in microfinance. A great deal has been written on microfinance theory (Banerjee, Besley and Guinnane 1994; Besley and Coate 1995; Ghatak and Guinnane 1999; Rai and Sjostrom 2004; Chowdhury 2005, Karlan 2007). Most theoretical literature focuses on joint liability group lending and its implications for reducing information asymmetries. That is, the emphasis is on "why" and "how" microfinance works (Herms and Lensink 2007) but not "whether" it actually works or "for whom" it works. In spite of the abundance of theoretical literature, empirical work on the impact of microfinance is relatively sparse compared to the worldwide scale of operation of this important program.

In the midst of popular demand for microcredit, policy makers and practitioners, mainly from developing countries, have renewed the pledge to expand such programs and to increase their outreach to a wider community. The United Nations (UN) declared 2005 as the International Year of Microcredit. The UN urged multilateral donor agencies and developed countries to support the microfinance movement to achieve its Millennium Development Goal of halving poverty by 2015. Therefore, it is important to know whether households actually benefited from microcredit loans, and if so, who the main beneficiaries of such small loans are. As Morduch (1998) points out "Tens of millions of dollars worth of subsidized resources support these programs, and the question now is whether these benefits are justified by their substantial costs".

The objective of this paper is to build a bridge between the theory and empirics of microfinance. We examine the impact of a large-scale microfinance program in Bangladesh. Bangladesh makes a good case study for evaluation of microfinance because microfinance institutions (MFIs) are very influential in its economy and rapidly expanding. In the macro context, in 2004 microcredit loans constituted around 5.3 percent of total private

[1] In this paper I use the term 'microcredit' and 'microfinance' interchangeably. Microfinance is something more than just the credit (microcredit plus).

sector credit in the economy (CDF 2005). We evaluate the impact of microfinance at the household level using a large, nationally representative and unique cross-section data set from Bangladesh. We use quasi-experimental survey design comparing outcomes of participants and non-participants. The data was collected by the Bangladesh Institute of Development Studies (BIDS) for the Palli Karma-Sahayak Foundation (PKSF) (Rural Employment Support Foundation) for purposes of monitoring and evaluating microfinance programs in Bangladesh's.[2] The survey encompasses a wide variety of information at the household, village and organization level. It covers 3026 households comprising participants in the program and control groups covering 91 villages spread over 23 thanas (sub-districts). The programs are the Association for Social Advancement (ASA), Proshikha and 11 other MFIs which are members of PKSF. As far as we know, ours is the largest dataset in the impact evaluation of microfinance program in the world.

One of the most visible recent changes in the lives of the rural women in Bangladesh is the significant increase in their access to credit (Mahmud 2003). This has been made possible by the emergence of many MFIs that have developed large-scale operations by offering a small number of highly standardised products. The GB is the flagship of the microfinance movement in the world, and it has been a source of ideas and model for the many institutions in the field of microcredit that have sprung up around the world (Nobel Committee 2006). In Bangladesh, the homeland of the GB, hundreds of microfinance organizations replicating the Grameen model have now emerged. The GB along with two other MFIs, the Bangladesh Rural Advancement Committee (BRAC) and the Bangladesh Rural Development Board (BRDB) is the focus of previous studies on microfinance (Pitt and Khandker 1998; Morduch 1999; Madajewicz 2003). However, expansion, competition and funding constraints have greatly changed the recent dynamics of microfinance in Bangladesh. For example, 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. They are more efficient in terms of credit delivery, services and access to poor borrowers (Zohir et al. 2001).

The existing evidence on the impact of the microcredit program in Bangladesh is not unambiguous. The best-known impact evaluation study of microfinance by Pitt and Khandker 1998, PK from here on, a joint research project of BIDS and the World Bank, suggests that microfinance significantly increases consumption and reduces poverty. PK, using data from GB, BRAC and the BRDB RD 12 program, also find the marginal impact of microfinance on consumption to be greater for women (18 percent) than for men (11 percent). They use an instrumental variable approach considering the choice based sampling, and employ the weighted exogenous sampling maximum likelihood (WESML) estimator. Morduch (1998) applies a difference-in-difference (DD)

[2] The data collection and preliminary analysis was 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 it authority over the MFIs. PKSF mobilizes funds from a wide variety of sources (such as World Bank, Government of Bangladesh, foreign governments, other international donors and lending agencies) and provides these funds to its members for lending as microcredit.

approach to the PK dataset and finds that microcredit has an insignificant, or even negative, effect on the welfare measures examined by PK. Madajewicz (2003) also uses instrumental variable method with PK data, and finds results similar to those of Morduch.

Using panel data Khandker (2005) finds a less strong effect of microfinance participation than he found in the earlier cross sectional studies. The results also cast doubt on the optimistic 5 percent drop in poverty by the PK study. The impact of microfinance in other countries is also not that promising (for example, Coleman 1999; Kaboski and Townsend 2005). Most of the other studies on impact evaluation of microfinance are descriptive and do not consider the selection bias problem. This bias, arising from the non-random program placement and self-selection into the program, may compromise the validity of the impact estimates. Armendáriz de Aghion and Morduch (2005) note that “there have been few serious impact evaluations of microfinance so far, though, so a collection of definitive results is still awaited” (p. 207).

This paper differs from previous empirical studies in the choice of household sample and microfinance institutions and also in terms of the techniques used to evaluate the impact of microfinance programs. The dataset we use is the largest and the most representative of the existing microfinance programs in Bangladesh (Mahmud 2003). Since programs are placed in certain villages and households self-select themselves into the program there are potential sources of bias in measuring program impact. Participants are likely to differ from non-participants in the distribution of observed characteristics, leading to a “selection-on-observables” bias (Heckman and Robb 1985). 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). With non-experimental data at hand, we cannot distinguish the bias generated by non-experimental estimator (Smith and Todd 2001). We therefore discuss a number of solutions to this selection bias problem, including parametric and semi-parametric strategies.

We first employ instrumental variable (IV) approach (based on selection-on-unobservables), where we use eligibility rule for receiving microfinance as an instrument for participation in microfinance. The IV estimation is the most common strategy for non-experimental impact evaluation using single cross-section data. However, less is well known about its interpretation. We interpret the IV estimator, following Imbens and Angrist (1994)(IA from here on) and Angrist, Imbens and Rubin (1996)(AIR from here on), as the local average treatment effect (LATE): the effect of treatment of those who are induced to participate only because of the instrument. In addition to IV methods, which only estimate the impact of a program for a subset of the participants, we use propensity score matching (PSM) of Resenbaum and Rubin (1983). Matching estimators, based on selection-on-observables, 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. It also enables us to examine easily the program effect on different groups according to eligibility, access, education and landownership and by the borrower's gender. It turns out that the IV estimation results are quite similar to the matching estimates. We also estimate the program impact using the difference-in-differences (DD) approach and get consistent results.

We estimate the impact of participation in microfinance on household food consumption. Improving economic well-being is the main objective of the microfinance program. Household food expenditure and the value of food consumption provide the most common measure of welfare. We use two measures of food consumption expenditures: total household food consumption and per-capita food consumption expenditures. Overall our findings are that the effects of micro loans on consumption are not robust across all groups of poor household borrowers. It appears that the poorest of the poor are among those benefiting most from participating in the scheme. The impact of microcredit loans on consumption is negative for relatively land-rich households. The effects of participation are, in general, stronger for male borrowers than for their female counterparts.

2. The Program and the Evaluation Methods

The Program and the Context

The microfinance sector of Bangladesh is one of the largest and oldest programs of the world.[3] Unlike other countries in South Asia, Bangladesh does not have a proper substructure of small banks operating at the local level. Against the backdrop of a relatively undeveloped formal financial system and an unregulated system of microfinance organization a large microfinance sector has developed in Bangladesh. The programs are implemented mainly by non-government organizations (NGOs). According to data gathered by the Microcredit Summit Campaign, by the end of 2003 Bangladeshi MFIs had 21.2 million active clients, including some 13.7 million poor women (Hulme and Moore 2006). 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. There are now more than 1500 MFI NGOs in the country of which approximately 200 have large microcredit programs (CDF 2005).

The PKSF is an apex organisation for microfinance that lends out donor and other funds to its partner organisations (POs) for microcredit. In 2004 PKSF funds made up about 17% of the total microfinance industry in Bangladesh, which was 24% in 1998(CDF 2005). We use data from thirteen POs (MFIs) of PKSF. The names and activities of these POs are given in Appendix Table A1. All of these POs follow the GB-style lending procedure and typically give access to microfinance to households having less than 50 decimal of land. Credit is given mainly to groups of people, and there is no collateral requirement. Loans are made for any profitable and socially acceptable income generating activity, such as: rural trading; rural transport; paddy husking; food processing, small shops; and restaurants. The amount of a loan usually lies within the range US$15 - $160. However, members may take larger loans after repaying their first loan.

---

[3] Around one quarter of the world's micro-credit customers are in Bangladesh with a further quarter in India. Sub-Saharan Africa and Latin America are poorly served and China still remains an untapped market (State of the microcredit summit campaign report, 2006).

The MFIs investigated here include organisations that are very large in terms of loan disbursements and area of coverage, most notable the ASA and Proshikha. ASA provides both credit and savings services on a remarkably large scale. It is considered the world's most efficient MFI. Proshika works with nearly 2.8 million members. It is the fourth largest microcredit program in Bangladesh. A particularly pertinent feature of Proshika's credit program is the relatively large portion of male group members. Unlike other MFIs (where groups are almost exclusively women), Proshika's male group members account for 45% of the total disbursement. Notable other MFIs which we study here include Society for Social services (SSS) and Thengamar Mohila Sabuj Sangha (TMSS). As of December 2004, SSS is the 10^{th} largest MFI in Bangladesh in terms of cumulative disbursements and outstanding borrowers. TMSS is one of top fifty MFI-NGOs in Bangladesh. The other MFIs are relatively small and have similar types of program activities (see Appendix Table A1)

Evaluation Problem and Potential Solutions

Major concerns in assessing the impact of microfinance are that programs are not placed at random and that participants self-select into the program. The program is targeted at particular villages and (poor) households. For example, the program village might be poorer than the control village. A prospective member decides that he/she wants to participate in the microfinance program. Also any household willing to participate must be accepted as a member by the other group members who have self-selected themselves into the program. The potential participant also has to be approved by officials of MFI. Thus there are likely to be observable and unobservable differences in characteristics between participants and non-participants. So, in order to evaluate the program properly we need to take into account potential selection bias that could arise from non-random placement of the credit program, and common village-specific, household-specific and individual-specific unobservable characteristics.

The central problem concerning the selection bias lies in constructing appropriate counterfactuals. In our case, and in most of the evaluation literature, the main parameter of interest is the effect of "treatment on the treated" (TT). For simplicity, suppose that there are two states of the world: with or without treatment (specifically, participating in the microfinance program or not). Let $\Delta_\iota=1$ signify households who participate in microfinance and $\Delta_\iota=0$ households who do not participate. Denote the potential outcomes by $\Psi_{\iota 0}$ and $\Psi_{\iota 1}$ for states $\Delta_\iota=0$ and $\Delta_\iota=1$, respectively. Let Ξ_ι represent for each household ι a set of attributes (such as age or gender) that are unaffected by the treatment. For each household we observe only $\Psi_\iota= \Psi_{\iota 0}+(\Psi_{\iota 1}-Y_{\iota 0})\Delta_\iota$, so we observe $Y_\iota= \Psi_{\iota 0}$ only when $\Delta_\iota=0$ and $Y_\iota= \Psi_{\iota 1}$ only when $\Delta_\iota=1$. The evaluation problem arises from the fact that we cannot observe the outcome for any given observational unit in both the treated and non-treated states.

The parameter of interest is the average treatment on the treated (ATT) or TT, defined as:

$$TT=E[\Psi_{\iota 1}- \Psi_{\iota 0}|\Delta_\iota=1, \Xi_\iota]=E[\Psi_{\iota 1}|\Delta_\iota=1, \Xi_\iota]- E[\Psi_{\iota 0}|\Delta_\iota=1, \Xi_\iota]$$

The last term in the above expression is the counterfactual of interest, which is not observable in the data. What we observe is the average outcome in the untreated state $E[Y_{\iota 0}|\Delta_\iota=0| \Xi_\iota]$. In general we should expect that $E[\Psi_{\iota 0}|\Delta_\iota=1, \Xi_\iota] \neq E[\Psi_{\iota 0}|\Delta_\iota=0, \Xi_\iota]$ since participants and non-participants are selected groups that would experience different outcomes even in the absence of the program. Therefore, estimating TT by the difference, $E[\Psi_{\iota 1}|\Delta_\iota=1, \Xi_\iota]- E[\Psi_{\iota 0}|\Delta_\iota=0, \Xi_\iota]$ will lead to a ‘selection bias’ or ‘missing data’ problem.

In the presence of selection bias, there are different approaches for impact evaluation such as quasi-or natural experiments (Meyer 1995) depending on the data at hand. Some quasi-experimental or non-experimental studies take advantage of natural experiments. Apart from a truly randomised design program, all other methods require us to control for selection bias. In the absence of randomised trials or natural experiments we use quasi-experimental survey design to evaluate the program impact.

Quasi-experimental control groups may differ from program groups in many systematic ways other than the presence of the treatment. However, it does give us considerable control over selecting and scheduling measures over how non-random assignment is executed, over kinds of comparison groups with which the treatment group is compared, and over some aspects of how treatment is scheduled. Lalonde (1986) finds that a number of the standard non-experimental evaluation estimators generate estimates that do not provide alternatives to the experimental estimates. In the wake of Lalonde’s influential study new econometric techniques on impact evaluation have evolved. Lalonde’s study is also criticised on many grounds (Heckman, Ichimura and Todd (HIT) 1997, 1998; Heckman, Ichimura, Smith and Todd 1998) such as (i) inappropriate comparison groups (ii) different survey questionnaires (iii) poor quality of data in terms of distinguishing between individuals in the survey. Deheja and Wahaba (1999, 2002) study Lalonde’s (1986) data and conclude that matching approaches are generally more reliable than the non-experimental methods studied by Lalonde (1986). Subsequent work (e.g., Glazerman, Levy and Myers 2003; Smith and Todd 2005) suggests that matching may improve the results substantially even when only cross-section data is available. In the same vein, several authors (Heckman and Robb 1985; IA; AIR; Heckman 1997) have proposed IV methods to estimate the program effect when only a single cross section of data is available.

The common practice in non-experimental program evaluation is to employ an assumption about the determinants of participation in a program. For example, selection into a program may be based on variables that are either observable or unobservable. Using cross-section studies the latter can be dealt with by applying the IV method which allows us to consistently estimate the program effect free from bias caused by the missing variable problem without actually having data on the omitted variables, or even knowing what they are. Accordingly, we use interactions of land-based eligibility criteria and presence of microfinance as instrument(s) to estimate the program impact (PK; Madajewicz 2003; Kaboski and Townsend 2005). The IV approach measures the program impact of those participants who are induced to participate, and in the absence of instrument those households would not have been participated (IA; AIR). There are also some eligible households who did not participate while some ineligible households participated in the program. Therefore, IV cannot estimate the program impact of all participants.

Morduch (1999) points out that "PK have set out an important research agenda and have demonstrated the sensitivity of the results to methodological assumptions ..... the mixed results show that much more work is required to establish the case of strong microfinance benefits" (p.1606). Morduch's (1998) and PK's results highlight the sensitivity of the estimates with respect to the evaluation method. Moreover, given that we have non-experimental data, rather than rely on one non-experimental method with its strengths and limitations, and given the richness of our data[4], we also assume that selection bias can be eliminated by conditioning on observed covariates. We therefore turn to the PSM method - a technique gaining increasing attention as non-experimental impact evaluation. The PSM, based on selection-on-observables, is capable of estimating program impact of all participants when the appropriate comparison group is available. The matching estimator applies to all situations for treatment and control cases. The purpose of matching is to re-establish the conditions of an experiment when no randomised control group is available. The use of PSM for non-experimental program evaluation in developing countries is relatively few but growing (see, for example, Jalan and Ravallion 2003; Behrman, Cheng and Todd 2004).

3. The Data and Descriptive Statistics

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 study area includes 13 MFIs, each from a different district, 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 early1998. Besides collecting information at household level, separate modules were administered for village and institution level.

At first, 13 MFIs of various sizes were selected from a list of 138 MFIs funded by PKSF. The selection of MFIs was intended to capture the varieties of areas of operation and coverage. Since POs were not operating in all thana, selection of thana within the district of each MFI was made following village selection. The survey was conducted to obtain a nationally representative dataset for the evaluation of entire microfinance programs in Bangladesh. The geographic coverage of the survey was spread evenly over Bangladesh, and the thana level comparisons reveal that selected thanas were not different from the average (Mahmud 2003). Within a PO 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 PO-activities, number of PO groups, number of PO borrowers, infrastructure condition and existence of other NGO activities. Upon

[4] The work by Heckman, his co-authors and others (Deheja and Wahaba 1999, 2002; Michalopoulos, Bloom and Hill 2004; Diaz and Handa 2006) 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.

obtaining the information, a tentative sample of villages under each of the selected PO was drawn through stratified random sampling. The stratification was based on the presence or absence of NGO activity. The non-program villages were selected among neighbouring villages.

Of the 13 selected POs, 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 POs. Then, two control villages and six programme villages were chosen from each of the PO areas. However, since non-program villages could not be found under some of the POs, only 11 non-program villages could be included. So six to eight villages from each PO were selected depending on the availability of control villages and the area of operation by each PO. The village level information was gathered from local NGO offices, key informants in the village.

In selecting the survey households, the universe of households in program villages, drawn from the census, was grouped according to their eligibility status. The criterion for eligibility varies slightly across MFIs. However, the survey chose to define it by the more commonly applied criterion: those owning 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.[5]

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.[6] Our samples from control villages include those households whose heads expressed their willingness to participate (during census) in MFI programs, if available. Among the total surveyed households 2034 are eligible representing 67.2 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 while the rest are women.

Descriptive Statistics

We first check the observed differences in program and control villages that are potential determinants of program placement. Table 1 produces the descriptive statistics for different village level characteristics. 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 1 suggests that there are no systematic differences in terms of education and health characteristics. Among

[5] 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.

[6] 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.

transport and communication facilities, prominent differences include the presence of pucca (brick-built) roads in the village 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, post office and telephone office. However, there is a relatively higher presence of moneylenders in the program villages. In terms of irrigation facilities, no statistically significant differences are 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. The left-hand side presents the results for the full sample and the right-hand side shows the same results for eligible households. The K-Σ test column is based on the Kolmogorov-Smirnov test of equality of distribution. We see that the average landholding for the control group households is significantly higher than the program group households. When we look at the household size both K-Σ and τ-tests suggest it is different between treated and non-treated households. Average household size of participant household is 5.67 as opposed to 5.45 for the control household. There are significant differences between many household characteristics of participant and non-participant groups. Household level π-values and K-Σ tests reject the null hypothesis of equality of treatment and control groups for many variables. Thus there are some observed differences between program and control groups, 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 similar for samples of households with up to one acre of land (not shown here). Overall our findings are that the differences between treated and non-treated households are not completely systematic; however, the treated group has a higher average household size, more children and its members tend to be less educated.

We present summary statistics of consumption and credit variables in Table 3 (see also Appendix Table A2). It suggests 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 levels at both the household level and per-capita, and the differences between these consumption measures for female borrowers are statistically significant.[7] In terms of consumption of participants and non-participants, there are no statistically significant differences between treatment and control groups of households, though control groups have a little more household and per-capita consumption levels than the program group. Household level monthly consumption expenditures are not also different between program and control villages, however they are different in terms of per-capita monthly consumption expenditure, and households in control villages have higher per capita consumption expenditure than the program villages.

Tests at the village and household level indicate that the difference is more pronounced at the household level especially when we consider the full sample. The descriptive statistics largely indicate that the data is not observationally randomly allocated. Even when there is no difference in observed characteristics, we cannot be quite confident that there are no differences in unobservables. The evidence presented here in terms of different household and village level characteristics implies that we need to adopt various techniques (beyond the comparison of means) to control for confounding variables and selectivity bias. The results also suggest that we need to look at the effect of microfinance by land ownership of the household since both groups look observationally more homogeneous when sample households are restricted to lower amounts of land ownership.

4. Identification Using "Eligibility Criteria" as Instrument

A primary obstacle to identification is the non-random program placement. In particular, program placement is chosen by MFIs and they may choose a non-random sample of villages. 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. In practice, programs are typically designed by the head office, and usually managers and officials of MFIs working in local areas are not from the area where the program is located. This is also discouraged by the MFIs since it may induce loan selection to the employee's relatives or friends. There are also specific guidelines from the centre to select the program village. The MFI also does not set up its own branch in every village, and in most cases they coordinate their program from a remote area office set up usually at the Thana or union level. Most often it is the case that the local office of the MFI is located far from the village and the employees working for the MFI operate from the office to the village. Moreover in a country with 21 million microcredit borrowers from about 1500 MFIs, such information should be publicly known. 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".[8] We therefore address the village level selection using a wide range of village-level controls. We also use district level fixed effects to remove any unobserved heterogeneity across different geographic areas. These district level-fixed effects also partially control for unobserved factors across different villages.[9] Since we have 13 MFIs, each from a different district, this fixed effect also captures the differences across the MFIs.

However, identification also requires controlling the endogeneity that arises from household self-selection into the program. There are many eligible households in the program village who did not participate. On the other hand, some non-eligible households participated in the program. So, even conditional on a set of observed

[7] A participating household consists of either male or female member but not both in our sample.

[8] 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 suggest that NGO programs in a community in Bangladesh are not based on selection-on-unobservables.

[9] The program village otherwise presumably has the same unobservable characteristics as the control village. Moreover, a large number of non-participants are from the program villages, so the village level selection problem is not of much concern.

covariates, X's, 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 ι in village φ can be described as:

(1) $$\Psi_{\iota\varphi} = \pi_1\Delta_{\iota\varphi} + \text{э}_1\Xi_{1\iota} + \text{э}_2\Xi_{2\varphi} + \epsilon_{\iota\varphi}$$

where $X_{1\iota}$ is a vector of household specific variables, and $X_{2\varphi}$ is a vector of village-specific characteristics. $\Delta_{\iota\varphi}$=1 if household ι is a member of microfinance and $\Delta_{\iota\varphi}$=0 if ι is not a member. Alternatively, for identifying the effect of credit, $\Delta_{\iota\varphi}$ is the amount of microcredit borrowed by household ι in village φ. Selection into treatment on the basis of unobserved characteristics $\epsilon_{\iota\varphi}$ by households may generate a non-zero correlation between $\epsilon_{\iota\varphi}$ and $\Delta_{\iota\varphi}$. That is, $\Delta_{\iota\varphi}$ may be potentially endogenous. Therefore treatment effect estimated using OLS may not reflect the program's causal effect on household consumption.

To solve the problem of endogeneity we first use IV estimation techniques. So we need to find a variable that is correlated with $\Delta_{\iota\varphi}$ and that should satisfy the exclusion restriction. It should also not be correlated with $\epsilon_{\iota\varphi}$ through unobservable characteristics, conditional on observable household and village level attributes. One potential candidate for an instrument is the presence of a program in a village (since we are controlling for village level characteristics) (Kaboski and Townsend 2005). However, a more suitable instrument would be the program eligibility criterion set by the microfinance institution, which is completely exogenous to the household. In our case though, the eligibility rule is not absolutely followed. However, eligible households residing in a program village have higher chances to join in a microcredit program (70 percent of participants in our sample are eligible).[10] It therefore seems reasonable to think of eligibility status in program village as an instrument for program participation. Our instrument therefore is the eligibility status of a household, interacted with the presence of the program in the village φ. Formally, define V_{φ} as the presence of a program in a village φ (a dummy) and E_{ι} is a dummy which takes the value of 1 if the household is eligible i.e., owns less than half acre of land. So our instrument is $Z_{\iota\varphi}=V_{\varphi}\times E_{\iota}$, where $Z_{\iota\varphi}$ = 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 (PK, Madajewicz 2003).

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

(2) $$\Delta_{\iota\varphi} = \alpha_1 Z_{\iota\varphi} + \text{э}_3\Xi_{1\iota} + \text{э}_4\Xi_{2\varphi} + \omega_{\iota\varphi}$$

[10] Our IV approach can be seen as an indirect application of (fuzzy) regression discontinuity designs (Angrist and Lavy 1999, Hahn, Todd and Van der Klaauw 2001).

where X_1 and X_2 's are the same as in equation (1) and $\omega_{\iota\varphi}$ is the household-specific error term embodying the unobserved influences on $\Delta_{\iota\varphi}$. Although participation depends on the presence of MFI we assume that the observable village characteristics X_2 (along with district fixed-effects) controls for this village selection bias. We also assume that Z and Ξ's are exogenous with respect to $\epsilon_{\iota\varphi}$ and $\omega_{\iota\varphi}$.

We also examine whether there is a differential effect of credit borrowed by male and female borrowers. We modify equation (1) and estimate the following equation:

$$\Psi_{\iota\varphi} = \pi_{11}\Delta_{\iota\varphi}\Sigma_{\iota\varphi} + \pi_{12}\,\Delta_{\iota\varphi}\,(1 - \Sigma_{\iota\varphi}) + \ni_5\Xi_{1\iota} + \ni_6\Xi_{2\varphi} + \xi_{\iota\varphi} \tag{3}$$

where we include interactions between indicators of participation in credit and gender, denoted by the indicator Σ which is one for female borrowers. In equation (3) the difference in average outcomes between female borrowers and control groups is represented by π_{11} and for males the difference is represented by π_{12}.

Concerns with Instrument

Identification requires that land ownership is exogenous conditional on program participation. The exogeneity of land ownership is a plausible assumption in the case of South Asia where land markets are not particularly active and households owning land consider it a source of wealth and social status (see Binswanger and Rosenzweig 1986 for discussion of exogeneity of land in context of South Asia). PK maintain this assumption but Armendáriz de Aghion and Morduch (2005) argue that the PK data show much turnover in the land market. Our data indicates that such a turnover rate is very low. Only 12.8 percent of households purchased land and 9.5 percent of households sold land in the five year period prior to survey. Also there is no evidence that households buy or sell land in order to get credit, making program eligibility endogenous. This is consistent with the fact that eligibility status is not strictly followed. Since credit is mainly extended for non-agricultural purposes 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. Some of the MFIs also provide loans for agricultural purposes and hence conditions for eligibility cannot be strictly enforced. The eligibility rule is set to simply identify the poverty status of the household. Since land price and quality also vary between different regions, households having more than half an acre of land are also considered to be poor in some regions. Richer households get credit 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 hesitate to become members of MFI, even in situations of credit need, because they consider MFI as organization for the poor. Thus the use of program eligibility criteria as instrument for participation in microfinance is well justified here.[11] Moreover, in order to allow $Y_{i\varphi}$ to vary with the level of the

[11] The validity of the eligibility criteria as an instrument is defended at length by PK and Pitt (1999).

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.

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. Our identification is thus a set of village and household level characteristics and district level dummies along with exogenously set rules of program eligibility that only affect the household's participation in the program village.

5. Estimation Results

5.1 Reduced Form Estimates

In the following, we measure program impact 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 microfinance program in treatment and control villages, we first specify the following functional form:

(4) $$\Psi_{\iota\varphi} = \propto_0 + \pm_1 \varsigma_\varphi + \pm_2 \mathrm{E}_\iota + \pm_3 Z_{\iota\varphi} + \ni_7 \Xi_{1\iota} + \ni_8 \Xi_{2\varphi} + \&_{\iota\varphi}$$

where $Y_{\iota\varphi}$ is the log of consumption expenditure of household ι in village φ. With this specification, $(\pm_1+\pm_3)$ 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 measures the DD estimate of the impact of the presence of the program over and above the village mean and being eligible.

Reduced form estimates based on equation (4) measure the impact of the presence of program on eligible households in the treatment village over different groups defined by land ownership. So we focus on estimating the impact of the program based on access rather than participation. We estimate equation (4) using OLS and present the results in Table 4. The covariates included in X_1 and X_2 are presented in appendix (see list of variables). The top panel of Table 4 shows the coefficient estimates of the effect on the log of household total consumption expenditure by male and female households, and by land ownership. Most of the coefficients are statistically insignificant. The estimated coefficient δ is always positive. This indicates 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 $(\pm_1+\pm_3)$ is known as the intention-to-treat (ITT) effect (see AIR). ITT is defined as the difference in mean outcomes between those who are assigned by the program (whether they take it up or not) and those not assigned. The coefficient estimates in Table 4 indicate that ITTs are positive. This means that eligible households in program villages are positively impacted by the presence of program.[12] The estimated impact on the corresponding participant is, however, likely to be downward biased since not all program eligibles in the treatment village received the treatment. Thus we cannot interpret the effect as average effect per participant or TT.

5.2 Instrumental Variable Estimates

Reduced-form estimates are diluted due to imperfect take-up rates and the availability of the alternative credit availability. This sub-section discusses IV estimates of the effects of participation using eligibility status as an IV. We use two participation measures for Δ_ι whether the household is a current member of microfinance (termed the 'binary' participation indicator) and the cumulative amount of credit borrowed (termed the 'credit' participation measure). We first consider a special case of IV estimate – the Wald estimator which can be described as follows. Write the observed outcome Ψ_ι in terms of the potential outcome as: $\Psi_\iota = \Delta_\iota \Psi_{\iota 1} + (1 - \Delta_\iota)\Psi_{\iota 0} = \Psi_{\iota 0} + (\Psi_{\iota 1} - \Psi_{\iota 0})\Delta_\iota = \beta_0 + \beta_\iota \Delta_\iota + e_\iota$, where β_ι is the heterogeneous causal effect, and $\beta_0 = E[\Psi_{\iota 0}]$. Similarly the first stage equation (equation (2), ignoring covariates) can be written as: $\Delta_\iota = Z_\iota \Delta_{\iota 1} + (1 - Z_\iota)\Delta_{\iota 0} = \Delta_{\iota 0} + (\Delta_{\iota 1} - \Delta_{\iota 0})Z_\iota = \rho_0 + \rho_\iota Z_\iota + v_\iota$. ρ_i is the causal effect of the instrument on Δ_ι. In our problem $\Delta_{\iota 0}$ tells whether household i would participate if not eligible and $\Delta_{\iota 1}$ tells us whether i would participate when eligible (Angrist and Krueger 1999). The Wald estimator is specified as:

$$\beta_w = \frac{E[Y_i \mid Z_i = 1] - E[Y_i \mid Z_i = 0]}{E[D_i \mid Z_i = 1] - E[D_i \mid Z_i = 0]} = E[Y_{i1} - Y_{i0} \mid D_{i1} > D_{i0}]$$

For the binary endogenous variable, the Wald estimator is the ratio of the two ITTs: the effect of Z on Y (differences in mean consumption expenditure between eligible households in the program village and the mean consumption expenditure of ineligible households) divided by the effect of Z on D (the differences in the probability of participation in microfinance between them). Intuitively, the Wald estimates adjust consumption expenditure differences by eligibility status in microfinance for the fact that not all eligible members joined in the microfinance program while some others who were not eligible participated in the program. When the amount of credit is used as an endogenous variable the Wald-IV estimate is the mean difference in the consumption expenditure of eligible households in program villages and all others divided by their mean difference in amount of credit borrowed.

Table 5 displays the results of the Wald estimates for the whole sample and the sub-samples of women and men. The effect on the household log of total consumption expenditure is reported in the first panel. In the first row we present estimates of the program impact using a binary participation variable. 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 do not

[12] However, none of the estimated coefficients are statistically significant and so we follow McCloskey and Ziliak (1996) who suggest

change by changing the participation measure. The effect on the log of per-capita consumption expenditure is shown in the second panel of Table 5 where we find a statistically significant positive impact of participation for the eligible sub-sample of men and women groups. The effect 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 effect on per capita consumption. These results imply that participation in microfinance benefits eligible households but not all households who participated.

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. However, as we have seen in the descriptive statistics presented in Table 2, there are differences between participants and non-participants in terms of observed covariates. A more efficient estimate would exploit all the available information that both accounts for the household's decision to participate in microfinance and for the outcomes of interest. Below we estimate IV regression models using equations (1)-(3) for various sub-samples of households based on their land ownership.

How Participation Impacts consumption

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 1). We do not report the first-stage results for brevity. The second stage estimates of program impact using a binary participation measure are shown 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 result for female borrowers, and the bottom panel presents the same for the male group of borrowers.[13] We estimate the treatment effects of microfinance based on samples of different groups according to land ownership and by gender. 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.[14] The estimated treatment effects are all positive when we limit our samples of households with land ownership of less than or equal to 2 acres. Since the outcome variable is in logarithms we multiply the coefficient by 100 to measure the percentage change in household consumption expenditure. Participation increases household consumption expenditure by about 5 percent for the sample of households who have less than or equal to 2 acres of land. For groups holding less land the impact is greater. However, for the whole sample the participation has a negative effect on household total consumption

looking at economic significance of the results instead of its statistical significance.

[13] The survey uses villages as clusters, so we also estimate regressions taking village level clustering into account. The statistical significance is not affected by village level clustering. So, we present the results ignoring the cluster design of the sample.

[14] 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 weight of $\pi/(1-\pi)$, where π is the estimated probability of participation. The results, not reported here, are qualitatively similar to that of unweighted IV estimates presented here (the results are available upon request).

expenditure. The estimated effects of participation are greater 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 household 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, the participation in microfinance increases household per-capita consumption expenditure by 3.7 percent. 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. The estimated 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 are more than 100 percent larger for eligible households than the corresponding reduced-form estimates.

How Credit Impacts Consumption

The first stage involves estimating the credit demand equation using a Tobit model. 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. 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 percentages of total consumption expenditures. The estimated effects are higher for male borrowers as compared to female 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 participation measure, we see that the increase is about 7.2 percentage points. 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 larger coefficient estimates (especially for men) using credit as the measure of participation then the binary participation measure.

Treatment Intensity as the Instrument

Households living in different villages have borrowed varying amount of money. It appears that there is wide variation in the amount of credit borrowed by participants across different villages. Thus, the IV method can be improved upon by recognizing that some villages have participated in the programs longer than others. So we can use the timing of the program placement in different villages as an instrument to capture the variation in the amount of loan borrowed across households in different villages. Explicitly, the instrument is the interaction between Z_{ij} and 'treatment intensity' as measured by the number of years of program placement of a particular village. We also use the interaction between Z_{ij} and 'year of program placement dummies' as instruments. Note that our participation variable is now the total amount of credit borrowed. 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 participation using a single instrument - 'years of program placement in a village' interacted with $Z_{\iota\varphi}$. 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 male borrower group than female. We present the corresponding 2SLS estimates[15] using products of $Z_{\iota\varphi}$ and dummies for the year of program placement at the village level as instruments in the second panel of Table 8. The results are similar to the single instrument cases (in the top panel). However, 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 microcredit program. If we consider just the landless households they gain more (25 percent). 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 Table 8. The p-values indicate that over-identifying restrictions cannot be rejected at any reasonable level for any sample of households.

6. Interpreting the IV Estimates

We have seen that not all eligible households in program villages participate in the microfinance program. On the other hand, there are some ineligible households who do participate. It is likely to be the case that the eligibility status in microfinance induces some households in the program villages to participate in the program, but not all eligible participants in program villages participate because of the eligibility status. So we characterize the households affected by our IV. Consider, for example, the case where participation (Δ_ι) is binary. This means that all households do not switch to Δ_ι =1 from Δ_ι =0 or ϖιχε ϖερσα if Z_ι is changed from 0 to 1. Therefore the relationship between microfinance program participation (D_ι) and its effect on food consumption expenditure (Ψ_ι) can be analysed only for the subpopulation which is affected by the IV. AIR identify this

---

[15] We refer o multiple instrument estimates as 2SLS estimate to distinguish it from single IV estimate.

subgroup of units as 'compliers' – the group of people who are induced by the assignment of instrument, Z_ι. To describe it formally, recall that $\Delta_\iota = Z_\iota \Delta_{\iota 1} + (1 - Z_\iota)\Delta_{\iota 0}$, where variable Z_ι is 'assignment' i.e., the eligibility status in program village. Some program village households with land less than a half acre (Z_ι=1) did not participate in microfinance (Δ_ι=0), while some land-rich households (Z_ι=0) participated in microfinance (Δ_ι=1). Compliers are those households who have $\Delta_{\iota 1} > \Delta_{\iota 0}$ or equivalently $\Delta_{\iota 1} - \Delta_{\iota 0} = 1$($\Delta_{\iota 0}$=0 and $\Delta_{\iota 1}$=1). They are induced to take the treatment by assignment to the treatment and the causal effect of Z_ι on Y_ι is $\Psi_{\iota 1} - \Psi_{\iota 0}$ for households of this type. According to AIR 'never takers' are those with Δ_ι=0 for both values of Z_ι ($\Delta_{\iota 0} = \Delta_{\iota 1}$=0). Similarly 'always takers' are defined as households with Δ_ι=1 for both values of Z_ι($\Delta_{\iota 0} = \Delta_{\iota 1}$=1). Always-takers are always treated regardless of the value of Z to which they might be exposed. Finally households with Δ_ι=1 for Z_ι=0 (participate only when ineligible) and Δ_ι=0 for Z_ι =1 are termed 'defiers' ($\Delta_{\iota 0}$=1 and $\Delta_{\iota 1}$=0). The 'defiers' do the opposite of their assignment, and are induced to take the treatment by assignment to the control group.[16]

Since the 'always-takers' and 'never-takers' cannot be induced to change Δ_ι through a variation in the IV (exclusion restrictions ensure that causal effects are zero for these two groups), the impact of Δ_ι on Ψ_ι can at most be ascertained for the subpopulations of compliers and defiers. The monotonocity assumption assures that there are no δεφιερσ and that χομπλιερσ exist. Thus the causal effect of Δ_ι on Ψ_ι cannot be analysed for the full population, but only for the subpopulation of units that could be induced to change Δ_ι through a variation of IV. IA define this estimate as local average treatment effect (LATE) – mean effect on Ψ_ι of a change in Δ_ι for the subpopulation of compliers. Because treatment Δ_ι and instrument Z are not equal and Z_ι has been assigned to influence Δ_ι the difference $E[\Psi_\iota|Z_\iota=1] - E[\Psi_\iota|Z_\iota=0]$, which is equal to the causal effect of Z_ι on Ψ_ι, is called ITT effect. LATE, $E[\Psi_{\iota 1} - \Psi_{\iota 0}|\Delta_{\iota 1} > \Delta_{\iota 0}]$, is equal to ITT divided by differences in the probability of participation of treatment and control group under the assignment of instrument (i.e., $E[\Delta_\iota|Z_\iota=1] - E[\Delta_\iota|Z_\iota=0]$). Thus, in the absence of covariates, LATE is equal to the Wald estimator.

In our case (with a binary indicator of participation) 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. The IV estimator with covariates is a variance weighted average of the LATEs conditional on the covariates. 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). When 'amount of credit borrowed' is the participation measure LATE can be defined in a similar way: it is the weighted average marginal

[16] Following Katz, Kling and Liebman (2001), we can distinguish between treatment compliers and control compliers. The former are those with eligible household participants in program villages and the latter are those with eligible households in control villages who would have participated in the program if available. Similarly following them we can define 'treatment never takers' as those eligible non-participants in the program village, and non-eligible non-participants in program village as 'control never takers'.

benefit from an additional taka borrowed by the households for the subgroup affected by the eligibility-status instrument (Angrist and Krueger 1999).

The LATE-IV is based on the two assumptions: conditional independence assumption (CIA) and monotonocity assumption. The monotonocity assumption implies that anyone in the population who would take microcredit in the absence of eligibility would also take credit if they became eligible conditional on the program placement in the village. 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. The CIA of IV 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 outcomes are not directly affected by the instrument. The first requirement is satisfied since eligibility status is assigned by the MFI and thus exogenous to the households. The second requirement is un-testable and requires careful examination. In our case, the assumption is reasonable since we also use household land holding as covariate so that we allow consumption to vary with the landholdings. The un-testable case is —conditional on all observable covariates, eligibility status itself does not affect the consumption expenditure.

IA show that, under the independence and monotonocity assumption, LATE is ATET for those households who are induced by the instrument to take advantage of Z. ATET=LATE if no household in the comparison group is treated (if there are no always takers) or every household in the treatment group is treated (if all treated persons are compliers). In some cases LATE equals ATET but when households do not make decisions to react to the instrument based on factors that also determine treatment gains (Heckman 1997).

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. Microfinance programs are generally designed for the poor landless households to whom our estimates apply. The IV estimates represent households whose participation decision is affected by eligibility criteria. Because not every eligible household in the program village participated and a good number of ineligible households participated, we are also interested in estimating ATET. LATE estimates the TT parameter for those induced to participate because of eligibility and it excludes others. To the extent that the average program effect of these eligible groups is different from that of ineligible group and other eligible participants, LATE would be different than ATET. If no ineligible and non-program village households participate, and if every eligible household participates, then LATE=ATET. In a world of heterogeneous program impacts, when households have some information about the impacts, LATE and ATET will likely differ and the differences can be a matter for policy purposes. The LATE provides no information about the impact of treatment on the always takers, which could be large or small. Since our parameter of interest is the TT of all participants, using IV would imply a very important omission of our main parameter of interest.[17]

7. Evaluation using Propensity Score Matching

7.1 Identification Using Matching

The IV approach presupposes knowledge of the functional form of the relationship between outcome variable and the variable that determines the treatment. The estimates would be different depending on which instrument we use. Under a heterogeneous treatment world, one cannot use our estimates to say what would have occurred if all households had participated. We now examine heterogeneity of treatment effects across a variety of observable household characteristics such as education, gender, and land ownership using PSM of Rosenbaum and Rubin (1983). Matching allows estimation of mean impacts (treatment on treated) without any assumption about error distributions. It also facilities testing for the presence of interaction effects of land based eligibility and program area. We can even match on variables that are correlated with the error term in the outcome equation.[18] Matching is now a widely used method of evaluation that controls for potential bias due to non-random program placement and self-selection into the program. It can directly compare the outcomes for participant and non-participant households with the same (or similar) values of those variables thought to influence both participation in microfinance and outcomes in the absence of the program.[19] The matching method works by re-weighting the control-group sample so as to provide a valid estimate of the counterfactual of interest. After the re-weighting scheme, treatment and comparison units look the same in terms of observables. Under the matching assumption the only remaining difference between the two groups is program participation. So any difference in outcome between treatment and control groups could be attributed to the treatment effect provided we have clearly established that there is no further systematic difference between these two groups other than those observables.

In order to identify the TT parameter by the matching technique, our identifying assumption is that outcomes in the untreated state are independent of Δ_ι conditional on a set of observable village and household level characteristics. Rubin (1977) refers to the treatment status that is independent of potential outcomes as an ιγνοραβλε 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 who have less land and non-land assets are likely to participate more. MFI selects households on the basis of eligibility and

[17] Land-based eligibility criteria are not sufficient to determine the poverty status of a household. Many ineligible households are as poor as eligible households because of differences in land quality, its use and price of land by region. So even if our instrument identifies perfectly the impact of program participation on all eligible households, we still need to concern about other poor but ineligible households.

[18] Matching only requires that the mean of the error term be the same for participants and non-participants with given values of the conditioning variables, not that it be zero.

[19] According to HIT (1997), "simple balancing of the observables in the participant and comparison group sample goes a long way toward producing a more effective evaluation strategy" (p. 607).

characteristics that are observed. The fact that some participating households are not eligible and that all eligible households do not participate in the microfinance program introduces a potential selection bias. The sources of bias are the differences in observable variables in terms of household size, sex ratio, schooling, age, family composition, and other household characteristics. Identification in this case 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 in the sense that $E[Y_{\iota 0}|X_\iota, D_\iota=1]= E[Y_{\iota 0}|X_\iota, D_\iota=0]$.

The above identifying assumption that the program status can be ignorable conditional on the set of observables X_ι is called the ignorable treatment assignment of Rosenbaum and Rubin (1983) or CIA. It states that conditioning on X the non-treated outcomes is what the treatment outcomes would have been or, in particular, unobservables play no role in the treatment assignment. If CIA holds, the matching process is analogous to creating an experimental dataset in that, conditional on observed characteristics, the selection process is random.[20] Consequently, the distribution of the counterfactual outcome for the treated is the same as the observed outcomes for the non-treated.

It is sometimes the case empirically that for certain values of X or of $P(X)$ in the participant sample there will be no matching observation in the non-participant sample. So we need to make another assumption – the common support assumption ($\Pr(D_\iota=1|X_\iota)<1$) - in order to find comparable non-treated households for all treated observations. The matching on covariates removes the bias due to differences in observables between the treated group and the control group. However, when there are many covariates the matching procedure becomes difficult because of the curse of dimensionality. Rosenbaum and Rubin (1983) show that if outcome changes are independent of participation given X then they are also independent of participation given the propensity score. This means that we can match each treated unit to the control unit on the one-dimensional metric of the propensity score vector.

The above 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 (ι) the household's potential outcomes depend on its own participation only and not on the treatment status of other households; and (ιι) 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-effects and general equilibrium effects.[21]

7.2 Empirical Specifications of the PSM Methods

Our sample is choice-based, with program participants oversampled relative to their frequency in the population. Heckman and Todd (1995) show that matching methods can still be applied when weights are unknown because

---

[20] Blundell, Dearden and Sianesi (2005) argue that the plausibility of such an assumption should always be discussed on a case-by-case basis, thereby taking into account of the informational richness of the data.

[21] This is also one of the identifying assumptions of our IV estimate (see AIR). Without this assumption it is impossible to draw reliable inferences about the program effects on outcomes. We address the latter issue (ii) in section 7.4 and find no such spill-over effect.

the odds-ratio is a scalar multiple of the true odds ratio, which itself is a monotonic transformation of the propensity scores. Therefore, we use matching on the estimate of the odds ratio.[22]

A number of different matching methods exist in the literature. Each estimator involves the definition of a closeness criterion, a neighbour, and selection of an appropriate weight function to associate the set of non-treated observations to each participant. Smith and Todd (2005) find that there is no preferred matching algorithm. In cases where differences in covariates between matched pairs remain additional regression adjustments may be helpful to reduce such differences. Here we use the regression-adjusted matching estimator developed by HIT (1997, 998) to reduce the potential bias in the matching estimator due to differences in covariates.[23] We use the same set of covariate X used to estimate the propensity score. 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 $Y_0= X\beta_0+U_0$ (the regression is only run on the matched comparison group so it is not contaminated by program participation). 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 when using multiple nearest neighbours. We use matching with replacement where a given non-participant is allowed to match to more than one participant. The local linear matching estimates are replicated 100 times by bootstrapping. Abadie and Imbens (2006) show that bootstrapping standard errors are not valid for NNM due to a lack of smoothness. So we report Abadie and Imbens (2006) standard error for the nearest neighbour matching. We impose the common support restriction based on the method of trimming that was suggested by HIT (1997).[25] In addition, we exclude the two percent of the remaining treatment observations which show the lowest odds-ratio of the control observations.

The underlying rationale for the PSM is to use the pre-intervention variables that are likely determinants of program participation. Since matching assumes that selection is based on observable characteristics we use as much information as possible so that there is no remaining potential bias.[26] We follow HIT (1997) and Behrman, Cheng, and Todd (2004) in selecting variables. We also follow Rubin and Thomas (2000) who advise against 'trimming' models in the name of parsimony. Recall that the validity of CIA means that all confounding variables

[22] The estimates will not differ in the case of nearest neighbourhood matching since the ranking does not alter whether we match based on the propensity score or odds-ratio. However, for other matching estimators, such as Kernel matching, the estimates will be different.
[23] Hahn (1998) investigates the role of the propensity score in the estimation of ATE and shows that when using non-parametric series regression, adjusting for covariates can achieve the efficiency bound and adjusting for propensity score alone cannot.
[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.
[25] Rubin and Thomas's (2000) simulation exercise shows that impact estimates based on full unmatched samples are generally biased and less robust to misspecification of the regression function than those based on the matched samples.
[26] For estimating the TT parameter, matching method allow selection into treatment to be based on possibly unobserved component of anticipated program impact but only insofar that the program participation decision are only based on the unobservable determinants of Y_1 and not those of Y_0.

are included in X_ι, and that any difference between the treatment and control groups are unsystematic or at least not related to potential outcomes. According to Rosenbaum and Rubin (1983) after conditioning on $\Pr(D_\iota=1|X_\iota)$ additional conditioning on X_ι should not provide new information about D_ι. If it does so then we need to provide more confounding variables in X_ι or include higher order and interaction terms in the model for a given X_ι. We include all the variables that affect both program participation and outcomes. Heckman, Ichimura, Smith and Todd (1998) show that estimates produced by matching can be quite sensitive to the choice of covariates used to construct propensity score. 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 general specification that includes a more detailed household demographic and socio-economic variable and village level characteristics. The list of variables is reported in the Appendix.

7.3 Matching Estimation Results

We estimate a standard logit model of participation to estimate propensity scores. The dependent variable is a dummy variable that takes a value of one if a household participates in the microcredit program and zero otherwise. The results, not reported for brevity, show 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 2 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. 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 2, we need to discard only very few observations of the treatment group.[27]

The estimated treatment impacts using two different matching estimators are presented in Table 9. The average difference in consumption expenditure between participants and their matches provides the basis for the estimation of the TT 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 women and men groups of borrowers, respectively. We report results of LLR matching in the first row of each panel. The results based on the nearest neighbour of five households followed the local linear estimates. 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. The coefficient, however, is not statistically significant. The results from two variations of

[27] It appears that the imposition of common support is not critical in our application using different sets of covariates for estimating propensity scores. We also experimented with using the 'min-max' definition of common support but found that this led to relatively more imbalance in covariates based on our chosen specification of the propensity score.

covariates for the male and female groups together point out that the program effect is positive for those household who own less land (≤ one acre of land). Both the local linear version and the nearest neighbour estimates give us similar results in both specifications of the propensity score estimates. The only exception is that in the nearest neighbour we see a positive coefficient for samples of households having 5 acres or less of land ownership while we find a negative coefficient for the same samples using local linear regression estimates.

We observe similar results for the female group of borrowers. However, for male groups all the coefficients are positive. Again, none of the coefficients presented in Table 9 are statistically significant. We see that the overall results are not affected by different specifications of covariates of the propensity score. The results are also similar to those obtained from two different matching estimators. The effect is higher for male groups than for female groups of borrowers.

Table 10 provides the coefficients of the estimated mean impact on household monthly per-capita 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 the impact on household monthly total consumption. All the estimated coefficients are positive starting from the samples of households with one acre of land, and for male groups individually the coefficients are all positive. The magnitude of the coefficients 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.[28]

Impact of Access to Credit

We now estimate the program impact based on household access to microfinance. We estimate the propensity score using the coarser set of covariates. Our treatment group is all (participant and non-participant) households of program villages and the comparison group of households in non-program villages. As shown in the top panel of Table 11 the estimates are all positive for different group of landholding households. Again we observe insignificant coefficients for all groups of households.

Within Village Matching

Now we use similar specifications but use only households in program villages and discard the control village observations. The control group represents the non-participant households in the program village and program group is all participants in the program village. The estimates, based on local linear matching, are presented in the second panel of Table 11. The coefficients are positive for households having two acres of land or less, but

[28]The matching results based on covariates used in IV are available upon request.

negative if we also include households with more land ownership as well. None of the coefficient estimates are statistically significant.

Across Village Matching

Here we estimate the program impact by using the participant households in the program village as the treatment group and non-participant households in the non-program village as the control group. We discard all the non-participants in the program village. The results are presented in the bottom panel of the Table 11. We find a positive but insignificant coefficient for all except in the case of the landless.

Impact Based on Education

In Table 12 we report the joint effects of land ownership and female education of either household head or spouse of the household head to test the hypothesis whether land ownership and female education interact jointly with the microfinance program in determining household monthly consumption expenditure. When we stratify by both land and education we find that the program effects are positive if the woman in the household has more than primary schooling. The effect of having less than half an acre of land is positive (but insignificant) in all cases except in the case of the nearest neighbour estimate of women in households with primary education. The results indicate that among participating households the education of women matters greatly to achieving the benefits from microfinance program participation.

7.4 Spillover Effects

In order to interpret our estimates as the effect of treatment on those treated the SUTVA must hold. If this condition is violated then the control group sample in fact contains both treated and untreated households. If we find that households which did not participate register positive effects then the estimates of program impact would be smaller than it would have been without the spill-over.

To examine the spillover effects we compare the outcomes for non-participants in program village and in the control village. The difference in the unconditional mean household monthly consumption expenditure between these groups 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 consumption expenditure of these two groups. In order to get the differences in conditional mean consumption expenditure we run OLS regressions with the same covariates used in the IV estimation using the sample of non-participants in both villages (except the participation variable). We use residence of the household in the program village as an indicator variable. The estimation coefficient of the presence of MFI is very small and negative for the full sample of non-participants, while it is positive for the eligible sample (results not reported here). Again none of the coefficients are statistically significant at the conventional levels. To ensure that we are comparing similar non-participants we use regression adjusted matching estimates and find no support in favour of spillover. Thus there is no strong evidence in favour of a positive spillover effect. This result contrasts with Khandker (2005) who finds positive spillover effects of microfinance on non-participants in program village households. Our results are quite consistent given the size of the impact of the programs on participants. 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.

8.1 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 and statistical significance. 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 of the IV and matching estimates they are somewhat different. Consider for example the first rows of Table 6 and Table 9 where we present the IV estimates of the effect of participation on household total consumption expenditure and corresponding matching estimates, respectively. 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. In all cases we observe larger coefficients of program impact for IV compared to matching. However, this is not surprising since the two estimates produce results for two different subgroups of borrowers. IV estimates of program impact are for that subgroup of participants who are induced to participate by changing *Z* (eligibility) from 0 to 1. That is, it measures the average effect of the program for compliers or LATE. In our context, LATE is the only treatment effect that can be estimated by IV. LATE excludes ineligible participant households and eligible participants who would have joined whether the eligibility rule was put in place or not. On the other hand, matching estimates the impact of those participants with common support of non-participants. Matching can include impacts of the program on both eligible and ineligible groups of participants. It excludes observations without common support even if we have an eligible participant who, in case of IV, has just participated because of the eligibility rule. However, we find good support conditions and therefore matching estimates the program effects for most of the participants. The IV estimate is for an unidentified group of participants and it is likely that in our context those groups are less in number than the 'common support' group in the matching estimate. LATE estimates from IV could also exceed matching estimates because households affected by the instrument are more credit constrained or have greater immediate need to improve their consumption.

In the case of PSM our main parameter of interests is TT. Unlike LATE, it does not depend on a particular instrument. LATE compliers are a subset of the treated. LATE depends on the particular value of *Z* used to evaluate the treatment and on the particular instrument chosen. The instrument has no effect on those participating households having access to loans from different similar sources. LATE can, however, be considered as the ratio of two matching estimators (for the effect of *Z* on *Y* and for the effect of *Z* on *D*). The matching estimator combines propensity score weighting schemes to estimate the TT. Households most likely to participate get the highest weights in estimates of TT. 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). Matching does not assume linear relations between participation, covariates, and outcomes as is the case with IV. Using both matching and regression Angrist (1998) also finds smaller matching estimates than regression estimates based on the same vector of controls.

While IV is a standard technique for non-experimental impact estimates, recent evidence in favour of matching is compelling (Deheja and Wahaba 1999, 2002; Smith and Todd 2005; Diaz and Handa 2006). However, there is no guarantee that selection on observables will eliminate the total bias (unless they go in the same direction). HIT (1997), however, show that the matching process can remove two of the three important sources of bias - the bias resulting from different ranges of X_{ι} for treated and control samples (comparing non-comparable households - failure of the common support condition) and the bias resulting from different distributions of X_{ι} across common support (by re-weighting observations). The remaining source of bias is econometric selection (resulting from selection on unobservables across groups), which is ruled out by matching assumptions. So, matching estimates may still be biased if selection into the microfinance program is not based on observable variables. 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 identifying assumption (exclusion restriction) upon which the IV estimand is based is questionable with only a single cross sectional data set.[29] The LATE–IV estimates are only valid for the group of compliers and may not be informative for the other group (always takers). The validity of causal inference, in the case of IV estimation, also depends on the αδ ηοχ functional form assumption. PSM, on the other hand, is not a panacea either. While matching removes the need to make decisions about functional form, it does not remove the problem of variable selection to be included in *X*. The PSM is reliable to the extent that the unobservable attributes of households that affect the likelihood of their participation in microfinance do not directly influence their behaviour. The bias will remain if there are any latent factors correlated with participation decision and counterfactual outcomes.

8.2 Conclusion

We find that two widely used non-experimental impact evaluation techniques, IV and PSM, generate similar results concerning the impact of participation in microfinance. We estimate the program impact by gender, ownership of land, education and access to program. We measure program effects using different instruments and find the results are consistent with the use of our instruments – demonstrating the robustness of our estimates. We also estimate the program impact using the DD approach. We therefore accommodate the approaches taken by both PK (use IV approach) and Morduch (1999) (use DD approach) and have largely been able to mitigate the controversy by estimating the impact of participation in microfinance programs using our large and unique dataset. The interpretation of IV estimates of program impact of microfinance participation, using land-based eligibility criteria as instrument, is new. We show that IV estimates capture the program effect of a subset of eligible participants. They exclude ineligible participants and others who are not induced due to the assignment of instrument.[30]

We also use the PSM methods to estimate the impact of participation in microfinance programs. The choice of the matching is also motivated by the richness and appropriateness of the data. We use more generous (regression-adjusted) two different matching estimators using two different covariate specifications for estimating the propensity score, and find similar results in all cases. Our findings of similar results using matching and IV methods illustrate that they can be used as viable impact estimators under certain specific conditions (see footnote (?)). They, along with DD estimate, also give support to the validity of our results. Given the paucity of the empirical investigations of the impact of microfinance programs, and given the significance of evaluating this gigantic program, we can claim that under right conditions the evaluation of such (non-experimental) program can produce credible estimates of program impact. Our analysis also implies that the selection problem in microfinance is not particularly problematic if the researcher makes good interactions with borrowers and providers before evaluating the program.

The use of PSM in the evaluation of microfinance programs is new. The matching estimator permits us to analyse the heterogeneous treatment effect directly simply by sub-grouping the analysis and rematching within the sub-group. We compare the matching estimates with the IV estimates. We also analyse the shortcomings of matching since matching only eliminates selection-on-observables and thus remains unsolved about the selection-on-unobservables. Although matching does not solve all (or even many) of the problems that prevent regression models from generating reliable estimates of causal effects, our use of the most generalized matching estimator, given suitable data, can succeed admirably in estimating the causal effects of participation in microfinance. Overall, our analysis shows that matching can provide reasonable estimates of non-experimental program impacts.

On the whole we find that the effect of microfinance on household consumption expenditure does not seem to be strong. We did not find statistically significant effects in most cases. We do emphasis economic significance of the results rather than statistical significance. We find that the IV estimates of program impact are larger - in general, they report an increase of 6 to 14 percent in the consumption expenditure of the relatively poor participating households. The impact varies when we consider different samples based on household land ownership. None of the estimated coefficient of PSM estimates is statistically significant, and they have lower magnitude than the corresponding IV estimates. The matching estimates are therefore less precisely estimated. Overall results indicate that the positive effects are found more for male as opposed to female borrowers.

---

[29] Exclusion restrictions are natural in the context of panel data models where the variables in the outcome equation are measured in the period after the decision to participate in the program is made.

Conditional on positive impacts, stronger coefficient estimates are also observed for men participants than women. This is in contrast to PK's study, which finds stronger positive effects for women than men borrowers. These results are not affected by possible spillover effects. Though we find little support in terms of the significance of the coefficients of the participation variable, the results are consistent across different specifications and estimation methods. The similarity of the results helps strengthen the robustness of the estimated program effects.

Consumption expenditure accounts for more than 80 percent of total household spending among the poor in the rural areas of Bangladesh. The results support the fact that the effects of micro loans are not strong across all groups of poor households. Rather those among the poorest of poor participants are most likely to benefit from participating. In fact, we find some negative participation effects on comparatively land-rich household. For land-rich households such small loans may not be that important because they require larger amounts of money for their activities. Moreover, they are not the focus of the microcredit loans. These groups are not officially eligible. The result does not imply that some relatively land-rich borrowers take loans which reduce their consumption. Rather, the effect of microfinance represents a comparison of the effect of microfinance loans and the effect of loans available to those who do not obtain a microfinance loan.

In general, we find an inverse relationship between household land ownership and the coefficient of program impact: the lower the amount of land a household has the stronger is the effect of participation in microfinance. Thus, a broader perspective is required to screen out households from high loan demand groups (typically households with more land) to that of low loan demand groups who can use the loan effectively. So if the existing MFIs continue to provide loans to participants in the same size and scale, it should provide more loans to the poorer clients and enhance the outreach since a large number of very poor households are still outside the net of microfinance. The challenge is therefore to induce them to take loans and/or to make more loans available for them (possibly at a lower interest rate) and provide required training to run the self-employment schemes.

[30] So in interpreting the program impact of the PK study we also need to recognize the limitations of their estimates.

References

Abadie, A., and G. Imbens. 2006. "Large Sample Properties of Matching Estimators for Average Treatment Effects." Εχονομετριχα, 74: 1, pp.235-267.

Armendáriz de Aghion, B., and J. Morduch. 2005. Τηε Εχονομιχσ οφ Μιχροφινανχε: The MIT Press.

Angrist, J. 1998. "Estimating the Labor Market Impact of Voluntary Military Service Using Social Security Data on Military Applicants." Εχονομετριχα 66:2, pp. 249-288.

Angrist, J., and G. Imbens. 1995. "Two-Stage Least Squares Estimation of Average Causal Effects in Models with Variable Treatment Intensity." ϑουρναλ οφ τηε Αμεριχαν Στατιστιχαλ Ασσοχιατιον, 90:430, pp. 431-42.

Angrist J., G. Imbens, and D. Rubin 1996."Identification of Causal Effects Using Instrumental Variables." ϑουρναλ οφ τηε Αμεριχαν Στατιστιχαλ Ασσοχιατιον, 91:434, pp. 444-55.

Angrist, J., and A. Krueger. 1999."Empirical Strategies in Labor Economics". in Ηανδβοοκ οφ Λαβορ Εχονομιχσ, edited by C. Ashenfelter. and. D. Card: Elsevier.

Angrist, J., and V. Lavy. 1999. "Using Maimonides' Rule to Estimate the Effect of Class Size on Scholastic Achievement." Θυαρτερλψ ϑουρναλ οφ Εχονομιχσ, 114:2, pp. 533-75.

Banerjee A., T. Besley, and T. Guinnane. 1994. "Thy Neighbors Keeper - The Design of a Credit Cooperative with Theory and a Test." Θυαρτερλψ ϑουρναλ οφ Εχονομιχσ, 109:2, pp. 491-515.

Behrman, J., Y. Cheng, and P.Todd 2004. "Evaluating Preschool Programs When Length of Exposure to the Program Varies: A Nonparametric Approach." Ρεϖιεω οφ Εχονομιχσ & Στατιστιχσ, 86:1, pp.108-132.

Besley, T. and S. Coate. 1995 "Group Lending, Repayment Incentives and Social Collateral." ϑουρναλ οφ Δεϖελοπμεντ Εχονομιχσ, *46*, pp. 1-18.

Binswanger, H. and M. Rosenzweig 1986. "Behavioral and Material Determinants of Production Relations in Agriculture." ϑουρναλ οφ Δεϖελοπμεντ Στυδιεσ*,* 32, pp. 503-39.

Blundell, R., L. Dearden and B. Sianesi, 2005 "Evaluating the Effect of Education on Earnings: Models, Methods and Results from the National Child Development Survey." ϑουρναλ οφ τηε Ροψαλ Στατιστιχαλ Σοχιετψ: Σεριεσ Α, 168, pp. 473-512.

CDF (Credit and Development Forum). 2005. "Microfinance Statistics." 17. Credit and Development Forum: Dhaka.

Chowdhury, P. 2005. "Group-lending: Sequential financing, lender monitoring and joint liability." ϑουρναλ οφ Δεϖελοπμεντ Εχονομιχσ, 77:2, pp. 415-39.

Coleman, B. 1999. "The impact of group lending in Northeast Thailand." Jουρναλ οφ Δεϖελοπμεντ Εχονομιχσ, 60:1, pp. 105-41.

Daley-Harris, S. 2006. "State of the Microcredit Summit Campaign Report 2006." Washington, DC: Microcredit Summit Campaign

Dehejia, R. and S. Wahba. 1999. "Causal Effects in Non-experimental Studies: Reevaluating the Evaluation of Training Programs." ϑουρναλ οφ τηε Αμεριχαν Στατιστιχαλ Ασσοχιατιον, 94:448, pp. 1053-62.

Dehejia, R. and S. Wahba. 2002. "Propensity Score Matching Methods for Nonexperimental Causal Studies." Ρεϖιεω οφ Εχονομιχσ ανδ Στατιστιχσ, Vol. 84: 151-61.

Diaz, J., and S. Handa. 2006. "An Assessment of Propensity Score Matching as a Nonexperimental Impact Estimator." ϑουρναλ οφ Ηυμαν Ρεσουρχεσ, 41:2, pp. 319.

Gauri, V., and A. Fruttero. 2003. "Location Decision and Nongovernmental Organization Motivation: Evidence from Rural Bangladesh." World Bank Policy Research Working Paper, 3176.

Ghatak, M., and T. Guinnane, " Economics of Lending with Joint Liability: Theory and Practice."ϑουρναλ οφ Δεϖελοπμεντ Εχονομιχσ, 2003, 70, pp. 195-228.

Glazerman, S., D. Levy, and D. Myers. 2003. "Nonexperimental Versus Experimental Estimates of Earnings Impacts." Τηε ΑΝΝΑΛΣ οφ τηε Αμεριχαν Αχαδεμψ οφ Πολιτιχαλ ανδ Σοχιαλ Σχιενχε, 589:1, pp. 63-93.
Hahn, J. 1998. "On the Role of the Propensity Score in Efficient Semiparametric Estimation of Average Treatment Effects." Εχονομετριχα, 66:2, pp. 315-31.
Hahn, J., P. Todd, and W. der Klaauw.2001. "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." Εχονομετριχα, 69:1, pp. 201-09.
Heckman, J. 1997. "Instrumental variables - A study of Implicit Behavioral Assumptions used in Making Program Evaluations." ϑουρναλ οφ Ηυμαν Ρεσουρχεσ, 32:3, pp. 441-62.
Heckman, J., H. Ichimura, J. Smith, and P. Todd. 1998. "Characterizing Selection Bias Using Experimental Data." Εχονομετριχα, 66, pp. 1017-98.
Heckman J., H. Ichimura, P. Todd. 1997. "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme." Ρεϖιεω οφ Εχονομιχ Στυδιεσ, 64:4, pp. 605-54.
Heckman, J., H. Ichimura, and P. Todd. 1998. "Matching As an Econometric Evaluation Estimator." Ρεϖιεω οφ Εχονομιχ Στυδιεσ, 65, pp.261-94.
Heckman, J., and R. Robb.1985. "Alternative Methods for Evaluating the Impact of Interventions; An Overview." ϑουρναλ οφ Εχονομετριχσ, 30:1-2, pp.239-267.
Heckman, J., and P. Todd.1995."Adapting Propensity Score Matching and Selection Models to Choice-based Samples." Manuscript, University of Chicago.
Hermes, N., and L. Robert. 2007."The Empirics of Microfinance: What Do We Know?" Εχονομιχ ϑουρναλ, 117:517, pp. F1-F10.
Hirano, K., G. Imbens, and G. Ridder. 2003. "Efficient Estimation of Average Treatment Effects Using the Estimated Propensity Score." Εχονομετριχα, 71:4, pp. 1161-89.
Hulme, D., and K. Moore. 2006. "Why Has Microfinance Been a Policy Success? Bangladesh and Beyond" Working Paper, Institute for Development Policy and Management, University of Manchester
Imbens, G., and J. Angrist. 1994. "Identification and Estimation of Local Average Treatment Effects." Εχονομετριχα, 62:2, pp. 467-75.
Jalan, J., and M. Ravallion 2003. "Estimating the Benefit Incidence of an Antipoverty Program by Propensity Score Matching." ϑουρναλ οφ Βυσινεσσ ανδ Εχονομιχ Στατιστιχσ, 21:1pp.19-30.
Kaboski, J., and R. Townsend. 2005."Policies and impact: An analysis of Village Level Microfinance Institutions." ϑουρναλ οφ τηε Ευροπεαν Εχονομιχ Ασσοχιατιον, 3:1, pp. 1-50.
Karlan, D. 2007. "Social Connections and Group Banking." Εχονομιχ ϑουρναλ, 117:517, pp. F52-F84.
Katz, L., J. Kling, J. Liebman. 2001. "Moving to opportunity in Boston: Early results of a randomized mobility experiment." Θυαρτερλψ ϑουρναλ οφ Εχονομιχσ, 116:2, pp. 607-54.
Khandker, S. 2005. "Microfinance and Poverty: Evidence Using Panel Data from Bangladesh." Ωορλδ Βανκ Εχονομιχ Ρεϖιεω, 19:2, pp. 263-86.
LaLonde, R. 1986. "Evaluating the Econometric Evaluations of Training Programs With Experimental Data." Αμεριχαν Εχονομιχ Ρεϖιεω, 76, pp. 604-20.
Madajewicz, M. 2003. "Does the Credit Contract Matter? The Impact of Lending Programs on Poverty in Bangladesh." Ωορκινγ Παπερ, Columbia University.
Mahmud, S. 2003. "Actually How Empowering is Microcredit?" Δεϖελοπμεντ ανδ Χηανγε, 34:4, pp. 577-605.
McCloskey, D. and S. Ziliak. 1996. "The Standard Error of Regressions." ϑουρναλ οφ Εχονομιχ Λιτερατυρε, 34:1, pp. 97-114.
Meyer, B. 1995. "Natural and Quasi-Experiments in Economics." Jουρναλ οφ Βυσινεσσ ανδ Εχονομιχ Στατιστιχσ, 13:2, pp. 151-61.
Michalopoulos, C., H. Bloom, and C. Hill. 2004. "Can Propensity Score Methods Match the Findings from a Random Assignment Evaluation of Mandatory Welfare-to-Work Programs?" Ρεϖιεω οφ Εχονομιχσ ανδ Στατιστιχσ, 86, pp. 156-79.

Morduch, J. 1998. "Does Microfinance Really Help the Poor? New Evidence from Flagship Programs in Bangladesh." New York University: New York.

Morduch, J. 1999. "The Microfinance Promise." ϑουρναλ οφ Εχονομιχ Λιτερατυρε, 37:4, pp. 1569-614.

Pitt, M., and S. Khandker. 1998. "The Impact of Group-Based Credit Programs on Poor Households in Bangladesh: Does the Gender of Participation Matter?" Jουρναλ οφ Πολιτιχαλ Εχονομψ, 106:5, pp. 958.

Pitt, M. 1999. "Reply to Jonathon Morduch's: Does Microfinance Really Help the Poor? New Evidence from Flagship programs from Bangladesh." Manuscript, Department of Economics, Brown University

Rai, A., and T. Sjostrom. 2004. "Is Grameen Lending Efficient? Repayment Incentives and Insurance in Village Economies." Ρεϖιεω οφ Εχονομιχ Στυδιεσ, 71:1, pp. 217-34.

Rosenbaum P., D. Rubin 1983. "The Central Role of the Propensity Score in Observational Studies for Causal Effects." Βιομετρικα, 70:1, pp. 41-55.

Rubin, D. 1977. "Assignment to Treatment Group on the Basis of a Covariate." ϑουρναλ οφ Εδυχατιοναλ Στατιστιχσ, 2:1, pp. 1-26.

Rubin, D., and N. Thomas, 2000."Combining propensity score matching with additional adjustments for prognostic covariates." ϑουρναλ οφ τηε Αμεριχαν Στατιστιχαλ Ασσοχιατιον, 95:450, pp. 573-85.

Smith, J. and P. Todd. 2001. "Reconciling Conflicting Evidence on the Performance of Propensity Score Matching Methods." Αμεριχαν Εχονομιχ Ρεϖιεω, 91:2, pp. 112-18.

Smith, J. and P. Todd. 2005a. "Does Matching Overcome LaLonde's Critique of Nonexperimental Methods?" ϑουρναλ οφ Εχονομετριχσ, 125, pp. 305-53.

Zohir,S., S. Mahmud, B. Sen. and M. Asaduzzaman. 2001. "Monitoring and Evaluation of Microfinance Institutions." Bangladesh Institute of Development Studies, August, http://www.pksf-bd.org/bids_report.html.

Table 1: V1illage 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: coeducation	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
Whether regular bazaar (market for grocery) is present	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 LLP	0.27	0.44	0.16	0.23
Number of STW	11.82	12.13	0.31	0.05
Number of HTW 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: LLP=Low Lift Pump, STW= Shallow Tube-well, HTW=Hand Tube-well

Table 2: Selected Descriptive Statistics of Households

Variable	All Sample: Control (I)	All Sample: Program (II)	All Sample: Difference III=(II-I)	All Sample: p-value (III)	All Sample: K-S	Samples of Eligible Households: Control (I)	Samples of Eligible Households: Program (II)	Samples of Eligible Households: Difference III=(II-I)	Samples of Eligible Households: p-value (III)	Samples of Eligible Households: 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 in the family	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 in the family	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
Whether 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 household 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 household 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 household 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 household 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 household 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: Reported p-values are the two-tailed tests of the null hypothesis that program and control group means are equal. (K-S) based on 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 amount borrowed	4650.9	3799.4	851.6	0.000	0.015
	(3961.7)	(2115.2)	(176.1)		
Total Length of membership	4.1	3.3	0.8	0.000	0.001
	(3.4)	(2.7)	(0.2)		
Number of Borrowers per household	1.4	1.1	0.3	0.000	0
	(0.6)	(0.3)	(0.0)		
Household Consumption	2783.6	2365.0	418.5	0.001	0.003
	(2192.3)	(1723.2)	(130.4)		
Household Per-capita consumption	497.6	436.5	61.1	0.013	0.002
	(394.0)	(325.4)	(24.3)		
Number of Observation	213	1565			
	Participant	Non-participant	Difference	P-value	K-S
Household monthly Food Consumption (tk.)	2415.1	2456.6	41.5	0.730	0
	(1801.1)	(1890.9)	(67.7)		
Household monthly Per-capita Food consumption (tk)	443.3	467.7	24.4	0.049	0.022
	(336.1)	(337.4)	(12.4)		
Household monthly Food consumption in program village (tk.)	2417.8	2461.8	44.1	0.550	0.112
	(1806.9)	(1942.7)	(73.6)		
Per-capita monthly Food consumption in program Village (tk)	444.3	477.8	33.5	0.014	0.028
	(337.0)	(358.3)	(13.7)		

Notes: Reported p-values are the two-tailed tests of the null hypothesis that column I and Column II are equal. (K-S) based on Kolmogorov-Smirnov test of equality of distribution, Standard errors are in parenthesis

Table 4: Reduced form Estimates of the Impacts of Microfinance

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	-0.021	-0.051	-0.027	0.038	0.051
	(0.046)	(0.047)	(0.054)	(0.077)	(0.034)	(0.040)
δ_2	-0.104	-0.0983	-0.0788	-0.0361		
	(.0520)**	(.0549)+	(.0644)	(.0866)		
δ_3	0.0471	0.0532	0.0826	0.0628		
	(.0523)	(.0531)	(.0588)	(.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	-0.026	-0.048	-0.025	0.039	0.048
	(0.046)	(0.047)	(0.054)	(0.077)	(0.035)	(0.041)
δ_2	-0.104	-0.092	-0.068	-0.0337		
	(.0522)**	(.0550)+	(.0647)	(.0872)		
δ_3	0.0541	0.057	0.0794	0.0592		
	(.0525)	(.0532)	(.0591)	(.0804)		
$\delta_1+\delta_3$	0.0311	0.031	0.0314	0.0342	0.039	0.048

Notes: Standard errors 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 district dummies.

Table 5: Wald Estimates of Effects of Microfinance on Consumption

Dependent Variable: Household Log of Total Monthly Food Consumption Expenditure						
Participation Variable	All	All eligible	Women	Eligible Women	Men	Eligible Men
Whether participate or not	-1.345	0.249	-1.313	0.219	-1.434	0.102
	(0.093)*	(0.182)	(0.097)*	(0.183)	(0.145)*	(0.224)
Total amount of credit[1]	-0.8746	0.1616	-0.8430	0.1381	-0.8864	0.0631
	(0.0605)*	(0.1183)	(0.0620)*	(0.11589)	(0.0896)*	(0.1385)
Dependent Variable: Log of Per Capita Monthly Food Consumption Expenditure						
Participation Variable	All	All eligible	Women	Eligible Women	Men	Eligible Men
Whether Participate or not	-0.621	0.312	-0.633	0.273	-0.552	0.386
	(0.081)*	(0.157)**	(0.083)*	(0.158)+	(0.122)*	(0.189)**
Total amount of credit[1]	-0.4042	0.2024	-0.4080	0.1722	-0.3419	0.2392
	(0.0522)*	(0.1020)**	(0.0535)*	(0.0997)+	(0.0752)*	(0.1173)**

Notes: Standard errors in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%. Coefficients are those from estimation of equations (1) and (3) using the estimated value of Δ obtained from the first stage estimation of equation (2) (ignoring household and village level characteristics). The estimates of the first-stage equation (2), not reported here, are obtained using the household eligibility status in the treatment village as instrument [regression do not include any other covariate].

[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 Participation in Microfinance

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

Both Men and Women Participation Variable	Household Land Ownership						
	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless	R^2
Whether participate or not	-0.216	-0.131	0.049	0.128	0.126	0.156	(0.46-0.49)
	(0.102)**	(0.113)	(0.129)	(0.140)	(0.164)	(0.193)	
Amount of credit[1]	-0.1282	-0.0660	0.0692	0.1173	0.1335	0.1745	(0.46-0.49)
	(0.0660)+	(0.0738)	(0.0885)	(0.0984)	(0.1186)	(0.1374)	
Observations	3026	2960	2780	2462	2034	1471	
Women							
Whether participate or not	-0.21	-0.157	0.027	0.109	0.149	0.218	(0.46-0.49)
	(0.107)+	(0.116)	(0.132)	(0.144)	(0.167)	(0.197)	
Amount of credit[1]	-0.1308	-0.0978	0.0357	0.0830	0.1160	0.1886	(0.46-0.49)
	(0.0688)+	(0.0754)	(0.0880)	(0.0976)	(0.1191)	(0.1387)	
Observations	2813	2755	2591	2299	1904	1377	
Men							
Whether participate or not	-0.151	-0.013	0.146	0.21	0.051	0.124	(0.52-0.55)
	(0.154)	(0.168)	(0.190)	(0.210)	(0.248)	(0.299)	
Amount of credit[1]	-0.0360	0.0849	0.2602	0.2898	0.2299	0.2520	(0.52-0.55)
	(0.122)	(0.1340)	(0.1500)+	(0.1548)+	(0.1753)	(0.2159)	
Observations	1461	1420	1305	1127	922	673	

Notes: Standard errors in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%. Coefficients are those from estimation of equations (1) and (3) using the estimated value of Δ obtained from the first stage estimation of equation (2). The estimates of the first-stage equation (2), not reported here, are obtained using the household eligibility status in the treatment village as instrument. Regressions also include household and village level characteristics and district dummies.

[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 Program Impact of Participation in Microfinance

(Dependent Variable: Log of Per-capita Monthly Food Consumption Expenditure)

Both Men and Women	Household Land Ownership						
Participation Variable	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless	R^2
Whether participate or not	-0.203	-0.119	0.037	0.080	0.072	0.086	(0.26-0.28)
	(0.103)**	(0.113)	(0.129)	(0.141)	(0.165)	(0.197)	
Amount of credit[1]	-0.1071	-0.0437	0.0826	0.1109	0.1361	0.1670	(0.26-0.28)
	(0.0677)	(0.0756)	(0.0885)	(0.0977)	(0.1196)	(0.1415)	
Observations	3026	2960	2780	2462	2034	1471	
Women							
Whether participate or not	-0.177	-0.14	0.019	0.066	0.104	0.151	(0.25-0.28)
	(0.108)	(0.116)	(0.133)	(0.145)	(0.168)	(0.201)	
Amount of credit[1]	-0.098)	-0.0734	0.0494	0.0784	0.1234	0.1809	(0.25-0.28)
	(0.0690)	(0.0756)	(0.0884)	(0.0983)	(0.1199)	(0.1412)	
Observations	2813	2755	2591	2299	1904	1377	
Men							
Whether participate or not	-0.143	0.014	0.171	0.188	0.038	0.099	(0.30-0.33)
	(0.156)	(0.170)	(0.192)	(0.213)	(0.252)	(0.309)	
Amount of credit[1]	-0.0142	0.1256	0.3015	0.2992	0.2443	0.2470	(0.30-0.33)
	(0.1230)	(0.1352)	(0.1515)**	(0.1571)+	(0.1782)	(0.2229)	
Observations	1461	1420	1305	1127	922	673	

Notes: Standard errors in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%. Coefficients are those from estimation of equations (1) and (3) using the estimated value of Δ obtained from the first stage estimation of equation (2). The estimates of the first-stage equation (2), not reported here, are obtained using the household eligibility status in the treatment village as instrument. Regressions also include household and village level characteristics and district dummies.

[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 Program Impact using Years of program Placement as Instrument

(Dependent Variable: Household Log of Total monthly Food Consumption Expenditure, Participation Variable: Amount of loan borrowed)

Instrument: Z interacted with the number of years in microfinance in program village

	Household Land Ownership						
	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless	R^2
All	-0.0645	-0.0080	0.0812	0.1183	0.0579	0.1360	(0.26-0.28)
	(0.0671)	(0.07160	(0.0785)	(0.08380	(0.0956)	(0.1118)	
Women	-0.0491	-0.0186	0.0674	0.1039	0.0583	0.1440	(0.26-0.28)
	(0.0702)	(0.0741)	(0.0813)	(0.0866)	(0.0988)	(0.1156)	
Men	-0.0481	0.0684	0.2044	0.1883	0.1357	0.3045	(0.31-0.33)
	(0.1234)	(0.1319)	(0.1431)	(0.1447)	(0.1572)	(0.1959)	

Instrument: Z interacted with the number of dummies of years in microfinance in program village

	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless	R^2
All	-0.0323	0.0187	0.1040	0.1304	0.1127	0.2467	(0.26-0.28)
	-.0581	-.0625	.0689	(.07.33)+	-.0813	(.0952)*	
	[p= 0.008]	[p= 0.004]	[p= 0.000]	[p= 0.000]	[p= 0.000]	[p= 0.000]	
Women	-0.0241	.0001	0.0813	0.1094	0.0969	0.2446	(0.26-0.28)
	(0.05887)	(0.06246)	(0.06904)	(0.0740)	(0.0823)**	(0.0962)**	
Men	-0.0252	0.07257	0.1834	0.1677	0.1428	0.3014	(0.31-0.33)
	(0.1053)	(0.1129)	(0.1222)	(0.1221)	(0.1279)	(0.158012)+	

Notes: Standard errors in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%. Coefficients are those from estimation of equations (1) and (3) using the estimated value of Δ obtained from the first stage estimation of equation (2). Regressions also include household and village level characteristics and district dummies. 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).

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

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	Landless	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless
Both Women and Men												
Local Linear	-17.01	-4.47	-16.88	19.47	37.65	50.01	-20.66	-4.85	8.20	40.39	30.50	38.47
	(91.20)	(73.97)	(80.96)	(65.61)	(67.83)	(79.50)	(77.34)	(70.81)	(91.36)	(70.04)	(67.51)	(88.65)
Nearest 5-neighbour	-31.93	32.29	-8.75	37.85	53.33	49.14	-14.97	51.84	-5.66	34.92	23.23	45.41
	(75.24)	(73.67)	(74.03)	(76.54)	(73.47)	(85.16)	(74.37)	(71.56)	(73.20)	(72.45)	(76.41)	(80.60)
Women Only												
Local Linear	-42.28	-28.20	-7.19	28.51	28.24	32.11	-44.30	-32.45	-0.29	32.17	17.31	4.24
	(93.45)	(83.39)	(95.16)	(79.78)	(66.06)	(77.64)	(73.11)	(72.80)	(82.97)	(70.57)	(73.18)	(81.80)
Nearest 5-neighbour	-33.84	-29.18	74.89	-0.39	18.05	19.72	-30.70	-29.20	22.22	24.34	67.60	20.37
	(80.53)	(74.89)	(76.64)	(75.90)	(71.48)	(84.20)	(76.84)	(74.16)	(76.57)	(77.56)	(72.03)	(77.24)
Men Only												
Local Linear	90.00	85.98	137.47	156.04	322.13	302.08	162.55	220.00	128.54	160.94	249.43	249.22
	(175.26)	(187.78)	(191.44)	(161.95)	(207.65)	(219.44)	(188.98)	(182.37)	(196.29)	(173.84)	(168.08)	(229.54)
Nearest 5-neighbour	89.40	69.98	83.81	51.61	197.60	301.09	189.42	193.38	208.08	181.48	208.84	274.78
	(161.10)	(165.51)	(169.77)	(175.04)	(211.81)	(269.22)	(138.03)	(155.51)	(146.19)	(157.92)	(209.66)	(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 biweight kernel and a fixed bandwidth of 0.06.
IV set of covariates include those variables included in Ξ in the estimation of equation (1). The full set of covariates includes a coarser set of specifications including those included in IV set (see appendix). + significant at 10%; ** significant at 5%; * significant at 1%. All the coefficients estimation is carried out using regression-adjusted version of the corresponding matching estimator. The propensity score is estimated using logit model where the covariates are household, village level characteristics and district dummies. The regression-adjustment is made on the same set of covariates used to estimate propensity score.

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

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	0.70	-1.34	7.10	8.08	5.79
	(14.67)	(14.52)	(15.08)	(15.86)	(16.20)	(19.12)
Nearest five neighbour	-10.53	7.29	0.77	7.23	10.66	7.36
	(15.40)	(15.31)	(16.04)	(16.68)	(16.73)	(20.21)
	Women Only					
Local Linear	-4.56	-1.49	1.38	10.44	8.15	9.39
	(15.23)	(15.03)	(15.72)	(16.54)	(16.65)	(20.35)
Nearest five neighbour	-2.66	-1.20	1.76	13.28	6.67	6.52
	(16.40)	(15.96)	(16.38)	(16.72)	(16.27)	(19.79)
	Men Only					
Local Linear	10.11	14.35	12.43	27.22	59.91	26.58
	33.43	32.83	32.98	42.93	63.56	43.14
Nearest five neighbour	15.81	14.60	5.23	7.68	27.75	18.20
	(31.60)	(33.60)	(33.90)	(34.06)	(41.14)	(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 biweight kernel and a fixed bandwidth of 0.06.
The full set of covariates includes a coarser set of specifications including those included in IV set (see appendix). + significant at 10%; ** significant at 5%; * significant at 1%. All the coefficients estimation is carried out using regression-adjusted version of the corresponding matching estimator. The propensity score is estimated using logit model where the covariates are household, village level characteristics and district dummies. The regression-adjustment is made on the same set of covariates used to estimate propensity score.

Table 11: Impact Estimates based on Access and Program Village

(Dependent Variable: Household Total Monthly Consumption Expenditure)

Impact based on access to Microfinance[1]					
All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless
70.41	76.22	78.21	149.78	146.22	306.56
(214.02)	(213.05)	(209.86)	(236.33)	(214.34)	(323.00)
Program impact over and above non-participant in program village[2]					
All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless
-18.42	-3.67	1.24	41.95	36.41	34.40
(79.83)	(75.18)	(76.47)	(76.67)	(76.33)	(83.01)
Participant in program village and non-participant in control village[3]					
All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless
415.62	223.25	185.68	197.43	730.57	-[a]
(297.85)	(307.61)	(298.72)	(385.57)	(714.04)	-

Notes: Bootstrapped standard errors are shown in parentheses. They are based on 100 replications with 100% sampling. The densities were estimated using a biweight kernel and a fixed bandwidth of 0.06. All the coefficients estimation is carried out using regression-adjusted version of the corresponding matching estimator. + significant at 10%; ** significant at 5%; * significant at 1%. The propensity score is estimated using logit model where the covariates are household, village level characteristics and district dummies. The regression-adjustment is made on the same set of covariates used to estimate propensity score.

[1] Based on access to microfinance rather than actual participation. Impact is estimated as the weighted average of the difference in mean between households of program village and that of control village.

[2] Based on just program village households. Impact is estimated by using participant in program village and non-participant in program village.

[3] Non-participant in program village is excluded. Impact is estimated based on participant in program village and non-participant in control village.

[a] The sample size for the comparison group is too small.

Table 12: Estimates of Program Impact by Education and Land Ownership
(Dependent variable: Household Total Monthly Food Consumption Expenditure)

	Illiterate		At most primary		Secondary or higher secondary	
	Local Linear Matching	Nearest Neighbour Matching	Local Linear Matching	Nearest Neighbour Matching	Local Linear Matching	Nearest Neighbour Matching
All sample	-37.11	-8.09	-878.94	-178.21	108.80	160.03
	(76.98)	(79.94)	(518.99)	(287.04)	(375.97)	(504.12)
Land ≤500	-21.00	-11.47	-81.04	-198.08	275.20	228.87
	(75.94)	(77.91)	(597.17)	(254.13)	(449.05)	(355.71)
Land ≤100	43.00	3.55	88.25	-127.37	Small Sample	Small Sample
	(80.56)	(82.13)	(653.26)	(350.65)	No treated unit	No treated unit
Land ≤50	13.65	14.41	74.92	-13.66	Small Sample	Small Sample
	(18.44)	(86.64)	(191.78)	(332.49)	No treated unit	No treated unit
Landless	4.72	23.08	Small Sample	Small Sample	Small Sample	Small Sample
	(22.30)	(91.15)	No treated unit	No treated unit	No treated unit	No treated unit

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 biweight kernel and a fixed bandwidth of 0.06.
All the coefficients estimation is carried out using regression-adjusted version of the corresponding matching estimator using the full set of covariates. The propensity score is estimated using logit model where the covariates are household, village level characteristics and district dummies. The regression-adjustment is made on the same set of covariates used to estimate propensity score.

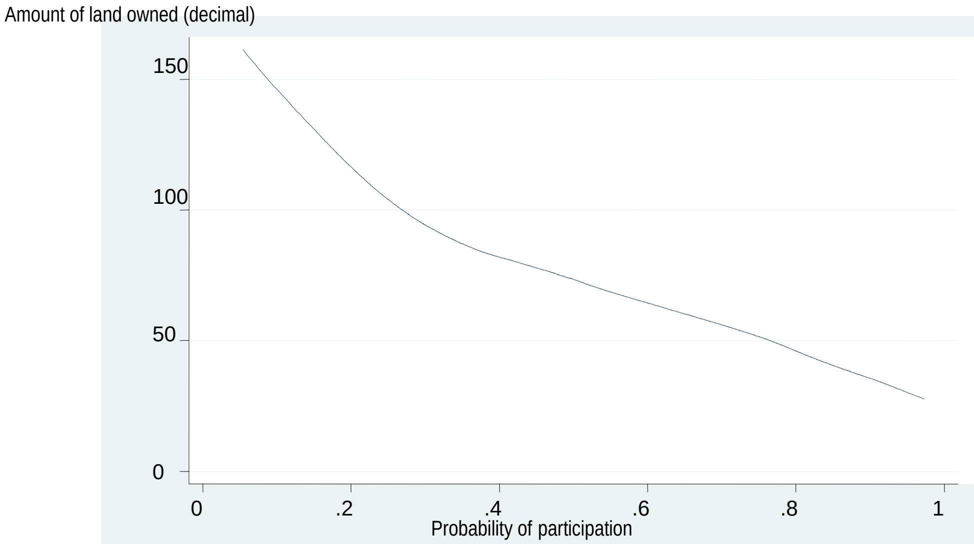

Figure-1: Participation probability by land ownership

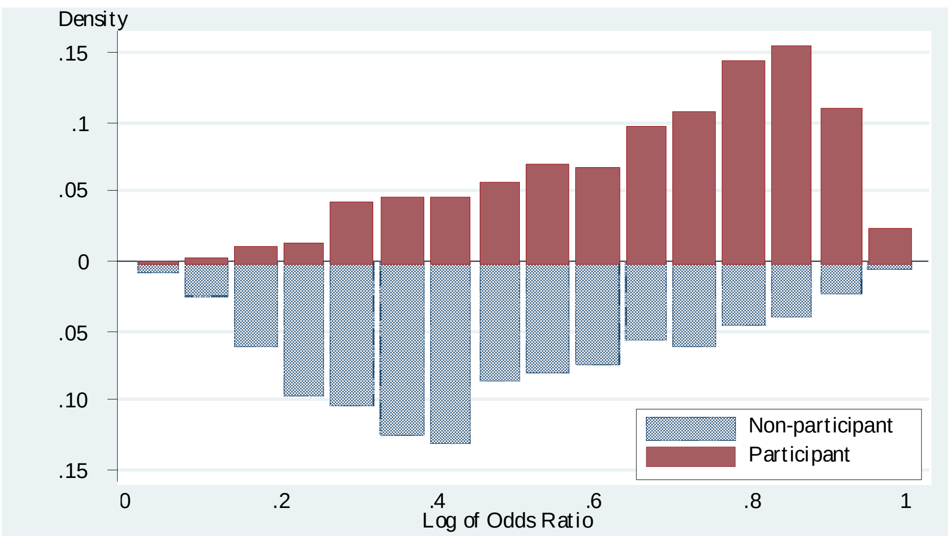

Figure-2: The Empirical density of the log of odds-ratio
(Using Full Covariates)

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 yrs 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, s 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 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 which are selected to maximize the percentage of observation classified under the model.

Table A1: List of MFIs by Activities in Program District

Name of POs	District	Activities
Association for Social Advancement (ASA)	Feni	Credit
Proshika	Barisal	Credit, Forestry/ Nursery, Education, Fisheries.
Thengamar Mohila Sabuj Sangha (TMSS)	Bogra	Credit, sanitation, Crop Diversification, Forestry, Training, Education, Fisheries, Handicrafts, Family Planning, Poultry and Diary, Community Health.
Society for Social services (SSS)	Tangail	Credit, Sanitation, Education, Insurance, Forestry/Nursery, Training, Crops Diversification.
Anyvab	Panchagarh	Credit, Forestry, Nursery, Informal Education.
Solidarity	Kurigram	Credit, Forestry, Education, Sanitation, Family Planning, Training, Legal Aid.
Program for People's Development (PPD)	Sirajganj	Credit, Education, Training, Poultry.
Draidra Bimochan Sangsta (PRP)	Meherpur	Credit
Gano Unnoyan Prochesta (GUP)	Madaripur	Credit, Education, Legal Assistance, Training, health Care, Fisheries & Livestock.
Sabalamby Unnoyan Samity	Netrokona	Credit, Forestry/Nursery, Education, Family Planning, Poultry, Training, Education, Sanitation, Health Care, Income Generating.
Nobaeki ganomukti Somabay Samity Ltd.	Satkhira	Credit, Sanitation, Preliminary Education on Health, Family Planning
OSDER	Munshiganj	Credit, education, Training, Health, Handicrafts, Marketing Support.
Prottayasi	Chittagong	Credit, Mother and Child Health, Forestry, Poultry

Source: Author's compilation based on the annual report of the above MFIs

Table A2: Mean of Consumption and Credit variables by Household Land ownership Status

All	All sample	Land $\leq$500	Land $\leq$200	Land $\leq$100	Land $\leq$50	Landless

Consumption	2432.7	2384.3	2319.1	2228.9	2126.3	2082.8
Per-capita consumption	453.7	449.9	444.4	435.2	423.6	415.1
Credit	3887.4	3886.3	3849.5	3786.0	3709.1	3714.5
Women						
Consumption	2406.1	2359.5	2302.8	2216.5	2108.0	2067.3
Per-capita consumption	450.5	446.8	442.4	433.2	420.1	413.1
Credit	3781.1	3776.5	3748.0	3724.1	3672.1	3656.9
Men						
Consumption	2505.2	2436.8	2350.0	2215.4	2120.8	2085.2
Per-capita consumption	472.8	466.1	459.2	444.7	437.5	427.2
Credit	4674.4	4718.7	4646.9	4299.8	4027.5	4200.2