Medium and Long-Term Participation in Microcredit: An Evaluation Using a New Panel Dataset from Bangladesh

Islam, Asadul

29 April 2010

Medium and Long-Term Participation in Microcredit: An Evaluation Using a New Panel Dataset from Bangladesh*

Asadul Islam

Department of Economics, Monash University, Australia, and

Bangladesh Institute of Development Studies (BIDS)

Email: asadul.islam@buseco.monash.edu.au

Phone: +61 3 9903 2783, Fax: +61 3 9903 1128

April 2010

Abstract

The objective of this paper is to estimate the impacts of medium- and long-term participation in microcredit programs. It utilises a new and large panel dataset collected from treatment and control households from 1997 to 2005. The data enables us to identify continuing participants in the program as well as *newcomers* and *leavers*. We employ different estimation strategies including triple-difference and propensity score matching methods to control for selection bias. The impact estimates indicate that the benefits from microcredit vary more than proportionately with the duration of participation in a program. Larger benefits are realized from longer-term participation, and that the benefits continue to accrue beyond departure from the program. The findings indicate the need to observe longer periods of participation to provide a reliable basis for assessing the effectiveness of microcredit lending.

JEL Classification: C21, C33, H43, G21, I31, O12, O16

Keywords: Microcredit, Bangladesh, triple-difference, matching, medium-term, long-term.

*I wish to thank Mark Harris, Dietrich Fausten, Costas Meghir and John Gibson, and participants at the EEA-ESEM in Barcelona, the Far East and South Asian Meeting of the Econometric Society in Tokyo, the Australasian Meetings of the Econometric Society at the Australian National University and at Wellington, and seminar participants at the University of Sydney, Monash University, and University of Dhaka for their very useful comments. I also thank the Bangladesh Institute of Development Studies (BIDS) and the Palli Karma Sahayak Foundation (PKSF), and the survey team in collecting the data used in this paper. I am responsible for the contents of this paper.

1 Introduction

Over the past two decades microcredit has become an important tool for rural poverty reduction. A substantial proportion of low-income rural families from many developing countries are now served by microcredit institutions (MCIs). Coverage is particularly impressive in Bangladesh, where microcredit reached more than 60 percent of its poor by 2005 (World Bank 2007). At present, there are a large number of microcredit schemes in operation around the world, and each year international donors, lending agencies and national governments allocate tens of billions of dollars for microcredit programs. However, there is currently no evidence about the medium- and long-term benefits of participating in such programs, probably due to the difficulty in obtaining data. So far, the evaluation of microcredit has concentrated on short-term impacts, and these are mostly based on cross-sectional data. The worldwide scale of microfinance[1], and the importance it has been given by donor agencies and international organizations[2], indicates that evaluating impacts over a longer-term horizon can be very useful for program design, the targeting of aid, and poverty reduction.

The returns from microcredit, which is mainly used for self-employment activities, are likely to vary with the length of participation. For example, Banerjee, Duflo, Glennerster and Kinnan (2009) argue that impact estimates from short-term evaluation might be completely different from those of long-term participation. In the short term, according to them, it is possible that some households cut back on consumption to enable greater investment that might make them significantly richer and increase consumption in the long run. This paper reports, for the first time, the sensitivity of the impact of the microcredit program with respect to the length of participation in a program. The objective is to distinguish the short-term participation effects from the medium- and long-term participation effects. Many households may not obtain the potential return until they invest sufficient sums of money. Typically, it takes a member several years to establish a trustworthy reputation that is required to obtain larger loans. Different investments will

[1] Microfinance is wider in scope compared to microcredit, however, we use the two terms interchangeably in this paper.

[2] The United Nations declared 2005 as the International Year of Microcredit. The 2006 Nobel Peace Prize winner was Professor Yunus and the Grameen Bank. World Bank (WB) Groups have a substantial investment in microfinance, and according to their website it will raise investment in microfinance to $1.2 billion by 2010.

have different time horizons in their returns' profile. Therefore, findings of the short-term evaluations may not provide reliable assessment of the overall impact of the microcredit program.

Evaluating microcredit programs based on data over a long period of participation could improve our understanding of the contribution that microcredit programs may make to the development process. However, this requires researchers to observe treatment and control households over a significant period of time. Recent availability of eight-year monitoring and follow-up of microcredit program data offers an opportunity to examine important questions about longer-term participation. This paper uses four waves of a panel dataset of treatment and control groups of large-scale microcredit programs in Bangladesh. The survey encompasses about 3,000 households from 91 villages from 1997 to 2005.

Most development practitioners and policy makers believe that microcredit can help the poor to break out of poverty. However, we don't know whether microcredit actually reduces poverty. The findings from the existing impact evaluation studies indicate that the effects microcredit vary from place to place, and also depend on the particular settings and design of the program. Pitt and Khandker (1998), using instrumental variable method, find that microcredit significantly increases consumption expenditure, reduces poverty, and increases non-land assets. Morduch (1998), using the same dataset but applying a difference-in-difference approach, finds that microcredit has insignificant or even negative effects on the same measures of outcomes. Khandker (2005) finds more muted results than Pitt and Khandker (1998). Islam (2008) finds microcredit helps to increase consumption only for the relatively poor. In Thailand, Coleman (1999) finds that average program impact on assets, savings, expenditure on education and health care is insignificant. On the other hand, Kaboski and Townsend (2005) find that membership in certain types of institutions can have a positive impact on asset growth and consumption in Thailand. In their 2009 study they examine a village fund where the Thai government delivers a fixed amount of money to a village regardless of the number of individuals in the village. They report increased consumption, agricultural investment, income growth, but decreased overall asset growth.

Karlan and Zinman (2010) examine the impact of expanding access to consumer credit in South Africa. They use individual randomization of *marginal* clients and the results from surveys following 6-12 months of the experiment indicate significant and positive effects on income and food consumption. Using a similar experimental design, Karlan and Zinman (2009) find stronger treatment effects of credit borrowed by male and higher-income entrepreneurs. Their results also suggest some evidence of a decline in well-being for some group of borrowers. Banerjee et al. (2009) report results of a randomized evaluation 15 to 18 months after the introduction of the program in the slums of Hyderabad in India. They find significant positive impact on new business start-up, profitability of existing business and purchase of business durables, but find no effect on average consumption, health and education expenditure. The microcredit programs we study here are very much similar to those studied by Banerjee et. al. (2009).

Unlike Banerjee et. al. (2009) our data does not come from randomized experiment. However, the availability of panel data enables us to address the concern regarding the selection bias using less restricitive assumptions than that of non-experimental cross-sectional program evaluation. We consider a variety of approaches to identify the different treatment effects, and to check the robustness of the results. At first, we employ the household fixed effects (FE) model. We then combine the FE and the propensity score matching (PSM) method that account for time-invariant unobservables and minimize the differences in the distribution between treated and non-treated households. In order to allow for time-varying unobserved household-specific differences, we use the random growth model (see Heckman and Hotz 1989), and also combine this method with the PSM method. However, if there are shocks or changes specific to treatment villages that have nothing to do with the program then the FE-PSM estimate will still be biased. Therefore, we use triple-difference (DDD) matching and regression methods.[3]

Our data indicate that some microcredit member (treatment group) dropped out from the program, and some non-treated (control households) joined. Therefore, if we treat ini-

[3]Microcredit in Bangladesh is typically targeted at households who are eligible, although this is not strictly enforced. There are both eligible and non-eligible households in treatment and control villages. Therefore, we can estimate the impact of microfinance on those targeted (eligible households) by using triple-difference: the difference between double-difference estimates for eligible and ineligible households (with both these groups being matched based on their observed characteristics). The use of land-based eligibility criterion is subject to debate in the context of microcredit in Bangladesh, and we address this issue in Section 4. Unlike previous studies (e.g., Pitt and Khandker 1998) we use a different cut-off point for eligibilty based on household surveys.

tial participating and conparison groups as treated and non-treated, the treatment/control differential is likely to be underestimated. The dataset contains information regarding the participation status of households for each year during the survey period. Thus, we are able to identify different treatment groups such as latecomers (*newcomers)* and households who continued their participation for at least eight years (*stayers).* Because entry into the program and the timing of the particpation are not random, we compare the changes in outcome before and after (at least two years') participation in the program. We estimate the treatment effect depending on the length of exposure to the program. There are also households who departed from the program. We track these drop-outs for up to eight years following their exit from the program. Using these households ("leavers"), we examine if the benefits received by participants continue after leaving the program. We are thus able to estimate the lasting impact of participation in the program. This could help us to understand what might happen when a member leaves the program.

We obtain the impact estimates of long-term participation based on at least eight years of continuing participation in a program. Estimates obtained for *newcomers* are interpreted as short-or medium-term impacts, depending on the length of their exposure to the programs. We also estimate *medium* or *long-run* impacts using a sub-sample of *leavers.*[4] We look at the impact on changes in self-employment income, other income, food and non-food expenditure and assets. The main finding of this paper is that the gains from microcredit programs vary with the duration of participation. The results show that the larger benefits accrue from longer-term participation. They also indicate that benefits may continue after the end of participation in a program but that such benefits are likely to be short-lived. The empirical results suggest that extrapolation using short-term participation data in the microcredit program may yield biased conclusions regarding the overall impact of the program. The findings from this study could provide a way to understand the impact of different lengths of participation in microcredit, and present a tentative time path of graduation from poverty. Our approach to estimate the impacts of different lengths of program participation appears to be a significant contribution to the literature, as it rests on observation rather than extrapolation.

[4] We use the words *run* and *term* to distinguish between impact estimates of leavers and existing clients of microcredit, respectively.

2 The Survey and the Data

This paper uses data from the surveys conducted by the Bangladesh Institute of Development Studies (BIDS) and the Palli Karma-Sahayak Foundation (PKSF, Rural Employment Support Foundation) for the purposes of monitoring and evaluating microcredit programs in Bangladesh. The first survey was administered after a census of all households in the 91 villages during October 1997. The survey encompasses 23 sub-districts of 13 of Bangladesh's 64 districts. One aim of the survey was to capture a representative sample of microcredit households that reflects the overall operation of microcredit in Bangladesh. The treated households were drawn from 13 different sizes of MCIs (so as to be representative of MCIs in Bangladesh), each from separate districts (out of 64 districts of Bangladesh). All these MCI are members of PKSF.[5] These MCIs have similar types of program activities and provide loans in a similar way to the Grameen Bank. Most of the clients in our sample are women, and credit is not offered to a mixed group of men and women together. Of the 13 selected MCIs, two were deliberately chosen from the four largest MCIs in Bangladesh. The survey was designed initially to have two control villages (these villages do not have any microcredit program but otherwise similar to the program villages in terms of geographical proximity and other village-level characteristics) and six treatment villages from each of the areas where microcredit was operating. However, since not enough control villages could be found in all areas, only 11 control villages were included in the first round. Subsequent rounds of the survey revealed that some of the control villages turned into program villages, and in the final round of survey there were 8 control villages.[6] Because of the absence of an adequate number of control villages, non-clients from the treatment villages who expressed their willingness to participate in the program were also surveyed. They were selected based on observable characteristics reported in the census. The household dataset is stratified, and clustered at the village level.

While four rounds of the survey were conducted (in 1997-98, 1998-99, 1999-2000 and 2004-05), we mainly use data from the first, third and fourth round because the second

[5]PKSF, established in 1990, works as a regulatory organization for the MCIs. It mobilizes funds from a wide variety of sources and provides these funds to its members for lending as microcredit.

[6]Khandker (2005) also highlights the difficulty of obtaining control villages.

round did not collect comprehensive information on outcome variables such as consumption and income.[7] All surveys took place during December to April. The first and third waves consisted of 3,026 and 2,939 households, respectively, and the final wave had 2,729 households from the same number of villages. The attrition rate over 1997-2005 was less than 10 percent: about 1.2 percent per calendar year. We study a balanced panel of 2,694 households to compare outcomes over time (we delete 35 observations because of missing data on some key variables). The survey has different modules for household socioeconomic condition, microcredit participation, village- and MCI-level information. The household dataset has several strengths. The data is comprehensive and covers information on all major socioeconomic conditions of households. There is detailed household information on income (from different sources and categories), possession, ownership, sales and purchases of all assets, expenditure on food and non-food items, and so on. It also records data on loan use, the amount borrowed, and the duration of the membership. The descriptive statistics of key demographic variables of treatment and control groups for different survey rounds are given in the top panel of Table 1.

Observation units have not remained stable. Many of the clients dropped out of the program after one or more years, and some of the control households became clients later. However, drop-outs from the program and newcomers into the program were also interviewed during each survey. Some splitting up of the original households also took place due to demographic transition. We found that 116 households had split up during the 1999-2000 round of survey, while 184 households had split up by the 2004/05 survey. The survey followed most of the members of split-up households who were also re-interviewed. We merged the split-up households with the original one to form a single household. The splitting up is not a major issue in this study, as there is very little migration outside of the village.

[7] One reason to have a follow-up survey in 2004-05 after a gap of about 4 years was to obtain impact estimates for those who dropped out and for those who participated for the first time. Therefore, an effort was made to obtain detail information on participation status during this interval. We have year-to-year information about household participation status for other years when there was no survey. The author was also personally involved in the last wave of data collection and administration.

2.1 Attrition

Here we examine whether there is any attrition bias even though the attrition rate from the survey is low compared to many other panel datasets from developing countries. Attrition bias arises if the variables that affect the probability of attrition have a non-zero correlation with the error term of an outcome equation with a sample that has been reduced by attrition. The sample comparison of means of demographic and other socioeconomic variables reveal that the attritors are not significantly different from the stayers. There are 147 attritors from treated households and 184 from control households in all three waves. Thus, the attrition rate is higher among the non-clients. However, a comparison of means of the attritors in terms of their demographic variables reveals no significant difference between clients and non-clients (see Table 8). In results not reported here we do not reject the hypothesis of the equality of the two distributions for any demographic variable using the Kolmogorov-Smirnov test. In the spirit of Fitzgerald, Gottschalk and Moffit (1998) we also began with an explanation of the correlates of attrition in our survey. We estimate a probit model of overall attrition, and attrition by participation status in the first round using a lagged demographic variable for the current round's attrition. We also test the equality of the probit regression coefficients for stayers and for attritors. We did not find any significant differences in the covariates that have a very strong correlation with future non-response. The full set of attrition results are available from the author upon request.[8] It is also possible that attrition is related shocks. That is some households might have experienced negative shocks, which led to business failure and exit from microcredit. Islam and Maitra (2009), using this dataset, find that attrition rate is not infleunced by household level health shcoks. Overall, the evidence is that any selection bias from attrition is not a problem in the present study. Moreover, we employ an estimation strategies which can resolve many potential biases (including attrition bias) that are due to unobservables.[9]

[8]Studies that use longitudinal data from both developed (see Journal of Human Resources 1998 Spring issue) and developing countries (Thomas, Frankenberg and Smith (2001) for IFLS data; Falaris (2003) for LSMS data from Peru, Cote d'Ivoire and Vietnam) find that even if demographic variables for attritors and stayers are different, and there are selective mechanisms working for attrition, the effects of attrition on parameter estimates are mild or non-existent.

[9]We also experiment with the most common approach of taking account of attrition bias in our regression estimation. We give weight to each observation by the inverse of the probability of staying in the sample, and carry out our estimation. The results are similar with or without weighting.

2.2 Outcomes of Interest and Descriptives

We are mainly interested in evaluating the impact on household income, consumption and assets. Self-employment income is of particular interest to us since microcredit programs are intended to enhance self-employment activities. Self-employment income is defined as the sum of the proceeds from all of the household's self-employment activities minus operating expenses (excluding the value of household's own labor). We also estimate the impact on "other income," much of which comes from some form of productive activity (households may buy a cow for agricultural activity or as an investment). Total income of a household would be equal to the "self-employment income" plus "other income". Moreover, money is fungible and there is substitutability between capital: households borrowing from MCI can transfer their own assets and savings to other activities, and hence pave the way to invest in multiple and diversified projects. As a result, we compute "total income" from a wide range of sources.

Since income may produce "noisy" data, particularly in a developing country, we also consider alternative measures to evaluate the benefits from microcredit. Poor households in Bangladesh spend a significant part of their income on food. We have information about 200 commodities consumed for a given period prior to each round of survey. The information covers a wide range and types (e.g. food purchased, home produced) of food consumption, and is as good as the standard LSMS food consumption module. The non-food expenditure data includes items such as kerosene, batteries, soap, housing repairs, clothing, schooling and health expenditures, etc. The data for non-food consumption expenditure was collected for different recall periods, depending on how frequently the items concerned are typically purchased. We construct non-food expenditure to a uniform reference period of one year. Together with food expenditure, consumption expenditure provides an alternative measure of household welfare. Finally, many households can save in the form of durable and non-durable assets, and also many households buy assets (such as livestock) using credit. Therefore, we measure the impact on total non-land assets of households, also excluding the value of the house.

We deflate the outcome variables by the rural household agricultural index, which is set to 1997-98 = 100. To reduce the effect of a few outliers, we exclude those households reporting unreasonably high or low values of the outcome variables (although this did not significantly affect the results). The lower panel of Table 1 reports the results of the outcome variables by treatment status for different years. It shows that food consumption is not significantly different between treatment and control groups, the two groups have different outcome in other measures. The table shows that total microcredit borrowed by households are taka $7,427$, $10,616$, and $11,682$ for 1997-98, 1999-2000 and 2004-05, respectively.

3 The Baseline Empirical Strategy

There are two major concerns for evaluating the impacts of microcredit at the household level. First, MCIs choose to provide credit in particular villages. For example, it could be the case that poorer villages get priority for microcredit. Alternatively, the decision to provide services might come about because of strong demand from a local community. Second, households within the program villages self-select into the program. It is likely that the decision to participate in a program is driven by households' need for credit or the perceived benefit from such credit. As a result, participation into the microcredit program may not be orthogonal to unobservable factors that also affect the outcome of interest. Thus impact evaluation of microcredit programs requires controlling for selection bias. The availability of panel data allows us to address the selection bias, and to avoid restrictive assumptions that are common in a cross-sectional data. We also adopt estimation methodologies which further relax many of the identifying assumptions that are typical in panel data estimation.

At the outset, we consider the household fixed effects (FE) regression which ensures that any variable that remains constant over time (but are unobserved) and is correlated with the participation decision and the outcome variable will not bias the estimated effect. We include controls so that the differences in age, household size, etc. are controlled. We use the following FE linear regression model:

$$Y_{ijlt} = \alpha_i + \delta_t + \beta D_{it} + \phi \gamma_l + \theta X_{it} + G_j + \varepsilon_{ijlt} \tag{1}$$

where Y_{ijlt} is the outcome of interest, e.g., consumption expenditure or income, for household i living in village j in a microcredit area l at period t (expressed logarithmically except for the self-employment income). D_{it} is the treatment variable that takes on (i) the amount of credit borrowed from a MCI at period t ; and (ii) the value of 1 if a household receives treatment from a MCI in period t and 0 otherwise. X_{it} is a vector of household-specific control variables, G_j is village fixed effects which eliminates the problem if programs were placed non-randomly, α_i is fixed effects unique to household i, δ_t is a period effect common to all households in period t, γ_l is the MCI/district-level fixed effects. Household-level fixed effects method also resolve any village or any upper (e.g., district) level unobserved heterogeneity (hence γ_l and G_j are redundant in equation (1)) . The error term ε_{ijlt} is household's transitory shock that has mean zero for each period and is assumed to be distributed independently of the treatment status D_{it}.The errors might be correlated across time and space. We therefore compute standard errors clustered at the village-year level to allow for an arbitrary covariance structure within villages over time.

Equation (1) is somewhat restrictive because it assumes that the selection bias is due to (i) an unobserved household specific component (α_i) that is fixed over time, or (ii) observed differences between treatment and control groups that are due to X, and in the absence of program participation $\beta = 0$. However, differential unobserved trends of treatment and control groups is a matter of concern here. In the following specification, we assume that there is a household-specific fixed effect and a household-specific time trend. The model, known as a random growth model (see Ashenfelter and Card 1985, Heckman and Hotz 1989), takes the fixed effects model a step further by allowing unobserved household differences that change at a fixed rate over time. We specify this model as:

$$Y_{ijlt} = \alpha_i + \lambda_i t + \beta_1 CD_{it} + \theta_1 X_{itj} + \xi_{ijlt} \tag{2}$$

where Y_{ijlt} is the natural log of the outcome of interest, CD_{it} is the cumulative amount of credit borrowed up until period t. The later is introduced to allow past loans to have an effect on a household's current consumption, income and assets. Thus, λ_i can be interpreted as the average growth rate over a period (holding other covariates fixed). Adding $\lambda_i t$ to the set of covariates also accounts for differential unobserved trends between treatment and control groups.

We may eliminate household fixed effects by differencing the dependent variable. With a simple modification, we express the first-differenced model in the following form:

$$\Delta Y_{it} = \lambda_i + \pi D_{it} + \rho X_{it} + \eta_{it} \tag{3}$$

where D_{it} is the net amount of credit borrowed from MCI at period t. This model eliminates the selection bias that results from household-specific fixed effects and the household-specific time trend. Thus, first differencing the cumulative credit variable would yield the net amount of microcredit borrowed at period t. Since first differencing the righthand side variable will mean losing more variables (if we estimate fixed effects on differenced variables we eliminate many of our variables of interest (linear time trend variable)) that affect the growth in outcomes, we use the level of variables such as education, age, household size, etc. Equation (3) is then just the standard unobserved effects model. This means we can apply fixed effects methods to estimate the treatment effect. In the actual estimation strategy, we also incorporate village level time trend (3) to control for village-level shocks.[10]

3.1 Results: Fixed Effects and Random Growth Model

The fixed effect regression results using equation (1) are given in columns (1) and (2) of Table 2. Since treatment assignment is not randomized, we also report results based on the matching method. We match households from treated and non-treated groups based on their observed characteristics. The details of the matching procedure are given in the next section. The results indicate that microcredit can increase self-employment income, consumption, and assets of households. The largest impact observed is on self-employment income, and the lowest is on food consumption (when we consider matched sample). Table 2 shows that treated households are able to increase food consumption by 1.9 percent, non-food consumption by 3 percent, and assets by 4.85 percent. The point estimate for "other income" is negative and insignificant: only 0.77 percent. We use a random-effect Tobit model (since a fixed-effect Tobit model is biased and inconsistent) to estimate the treatment effect on self-employment income as there are zeros in many cases. The coefficient estimates indicate that household self-employment income increases by 14.7 taka by borrowing 100 taka from a MCI. The statistical significance of the results is

[10] An advantage of models such as equation (1-3) is that they are robust with choice-based sampling which characterizes most non-experimental datasets, such as the dataset used in this study.

not affected by the particular standard error (block bootstrapping or clustering by village-year level) we consider. Therefore, we report results using clustering at the village-year level.

Results for the random growth model (equation 3) are given in columns (3) and (4) of Table 2. We find results similar to that of fixed effects estimation except in the case of "other income." Column (4) indicates that non-food consumption has increased by 4.3 percent, and other income by 6.5 percent. The estimated increase in food consumption and assets are 0.47 percent and 4.9 percent, respectively, for households in the treatment group. The coefficient estimates from the random-effect Tobit model indicate that a 100 taka increase in borrowing from a MCI will add 31.7 taka to household income. The estimated coefficients from random growth model are larger than that of fixed effects model.

There are many clients of the microcredit we study here who dropped out later from the program. It also appears from the data that some control group members joined the program after the 1997-98 survey took place. As a result, some households received partial treatment in view of the entire survey period. In the presence of partial treatment, the estimator provides an estimate of assignment of the treatment rather than of the mean impact of the treatment itself (Heckman, Smith and Taber 1998). It is likely that our estimate is smaller than we would obtain in the absence of exit from the program or entry into the program. Below we consider an approach that can identify the impact of partial treatment separately from the impact of the full treatment on the fully treated.

4 Other Evaluation Strategies

4.1 Triple-difference Matching Estimate

Our identification strategy in the last section also assumes that there is no shock to the outcomes of the treatment and control groups contemporaneously to the program. That may be a strong identification assumption, and any pre-program level difference between treatment and control groups may account for relative shifts in the outcome of interest. Thus, we control for different relative shocks affecting households in treatment and control villages by using a triple-difference strategy. microcredit in Bangladesh is typically offered to households who are eligible[11] in a program village[12]. Therefore, the potentially unaffected ineligible households from treatment and control villages can be used to difference away any relative trend in the treatment and control groups correlated with unobserved variables, but not due to participation in the program. For comparability, we include those ineligible households who are similar to that of eligible households in terms of observable characteristics. Thus, we can use a method that involves using a DD estimate for eligible and ineligible households.[13] In essence, this entails a triple-difference: a double-difference estimate for eligible households, minus a DD estimate for ineligible households. Ineligible households are not affected by the program, and the programs do not target them. In our sample about 17 percent of the participants are from the ineligible group. We exclude them from our estimation. This triple-difference estimator allows us to compare the effect of microcredit participation on eligible clients (in a treatment village) relative to eligible non-clients from a control village. It also provides a cleaner way to separate out some of the bias from the differential growth effects that may be caused by gaps in initial characteristics.[14]

To alleviate concern regarding comparability of the treatment and control groups,

[11]The MCIs set the official eligibility rule as households having less than 50 decimals (half an acre) of land in order to target the poorer households. By that criterion, a large number of ineligible households (30-40 percent, depending on the survey year) received the treatment. Discussions with local branch managers and field level officials of MCIs indicate that they treat households holding marginally more land with flexibility (on the grounds that land quality and price are not the same in every region, lack of perfect information about the borrowers' ownership of land, etc). The last survey asked households about the eligibility criterion, and many households reported that they are eligible if they hold less than one acre of land. Therefore, we adopt the eligibility criterion of households having less than one acre of land at the baseline (in 1997/98). According to this criterion, about 83 percent of the participants are eligible.

[12]Microcredit lenders do not lend outside the village in which they operate.

[13]The difference in changes in outcomes of *eligible* households across treatment and control villages is the DD estimate for eligible (treatment) group. Similarly, the difference in changes in outcomes of *ineligible* households across treatment and control villages is the DD estimate for the ineligible (unaffected) group.

[14]The identifying assumption here is that there are no household-level shocks driving participation in microfinance. Unfortuately, our data do not allow us to investigate if household takes loan to insure against a negative income shock. Islam and Maitra (2009), using this dataset, examine the shocks, in particular those related to health shocks, and do not find that shocks are systemtically different between treatment and control groups. In general, households cannot borrow from MCI against shocks. They have to borrow against business proposal/existing business. Moreover, a household experiencing shock cannot borrow unless she is already in a group or form a group. Thus shocks at the household level cannot directly be insured from MCI. The MCIs we study here do not provide any explicit insurance coverage for members to borrow against shocks. However, we do not completely rule out that possibility, and thus our estimates would subject o bias to the extent households form new groups with others to participate in microcredit when they are hit by shocks.

we use a non-parametric matching strategy. In particular, we use the propensity score matching (PSM) of Rosenbaum and Rubin (1983) to clean out observable heterogeneity prior to using the triple-difference estimator. Selection of variables for the propensity score is a crucial step in the estimation of treatment effects. In identifying the set of control variables to estimate propensity score we first consider the variables (e.g., household and village characteristics) that the MCIs use to select a household and that are likely to determine household demand for credit. We include all the variables that may affect both participation and potential outcomes (see the Appendix for variables used in estimating the propensity score). Using the first cross-section observation, we estimate a standard logit model where dependent variable is a dummy variable that takes a value of 1 if a household is a client of a MCI and 0 otherwise. The empirical distribution of the estimated odds-ratio of clients and non-clients shows that there are very few regions of non-overlapping support (see Figure 1).

We impose a tolerance level on the maximum propensity score distance to improve the quality of matching. We follow Dehejia and Wahba (1999, 2002) and apply a variant of caliper matching called *radius matching*. We use only as many comparison units as are available within the caliper. This matching estimator automatically imposes the common support condition and avoids the risk of bad matches. Therefore, households from the untreated group are chosen as matching partners for treated households that lie within the caliper. Observations closer to the treated group are given heavier weight. We use the biweight kernel and weights are given to each observation by the following kernel formula: $K = 15/16(1 - (d_i/b)^2)^2$, where d_i is the distance from the control observation to the treatment observation and b is the bandwidth (equal to 0.06). The weights are then normalized to sum to one for each observation. The normalized weights are used to create a comparison observation for each treatment observation.[15]

Thus, our strategy is to compare the observed outcome changes between eligible clients and eligible non-clients, with these two groups matched based on their odds-ratio of participating in a microcredit program. Since there may also be economy-wide changes that have nothing to do with the program and may have different implications for eligible

[15]We also experimented with an approach following Crump, Hotz, Imbens, and Mitnik (2009) to limit comparisons to a trimmed sub-sample for which there is sufficient overlap in the propensity scores. The results are very similiar, and are available from the author on request.

households in the absence of the program, we track outcome changes of ineligible non-clients between treatment and control villages. Our triple-difference matching estimate is given by DD1-DD2 where DD1=change in outcome of eligible clients in the treatment village minus change in outcome of eligible (non-clients) from the control village, and DD2=change in outcome of ineligible non-clients from the treatment village minus change in outcome of ineligible from the control village (all groups are matched).[16]

4.2 Impact on "newcomers" and "leavers"

In this section, we consider heterogeneity in the treatment effect considering households' duration of participation in the program. This consideration is important since a MCI may just attempt to enhance the short-term benefits of its borrowers, and not focus on long-term benefits, perhaps to gain popularity and to expand its program. Therefore, short-term program evaluation is likely to compromise the gains that accrue if the program continues to provide microcredit over a long period. Thus, we examine whether households who participate for longer periods benefit more compared with those participating for shorter periods.[17] The monitoring and follow-up of households over 8 years enables us to examine the impacts that are likely to vary with the duration of participation. We classify sample households into three broad categories:

1. *continuing participants* – clients of a MCI throughout 1997-2005;
2. *non-participants* – did not participate ever in any MCI program; and
3. *occasional participants* – clients in one or more years but not the entire period.

[16]The identification is also based on the SUTVA assumption. For our identification assumption to hold, we require eligible participants not to share their loan with eligible non-participants from the non-program villages. This is very unlikely given that there is now established literture which indicate that risk sharing in developing countries does not take place beyond the village level. So, such spillover is unlikely. Islam (2008), using the first cross-section data of this program, does not find any evidence in support of spillover effects. If the programs have any positive (negative) effect on ineligible households in later periods, then the impact estimates would be downward (upward) biased.

[17]When we observe small impacts in the first few years of follow-up and small impacts at the end, we can be reasonably certain that extending the program to the control group would have yielded small impacts. When we observe large impacts at the end of the eight-year follow-up, we can be fairly confident that extending the program to the control group would have yielded still larger impacts. In those cases where impacts were large at the beginning and smaller at the end we have reason to speculate whether an eight-year embargo would have increased treatment effects towards the end of the follow-up period.

We divide the occasional participants into the following categories:

(i) New participants (*newcomers1*) – households who joined the program after the 1999, and remained as clients up to 2004-05;

(ii) More recent participants (*newcomers2*) – households who joined in the program after 2001;

(iii) Long-term drop-outs (*leavers1*) – old clients who dropped out after 1998 and did not participate any any MCI;

(iv) Medium-term drop-outs (*leavers2*) – clients who participated until 2001 and then dropped out;

(v) Other – the residual category of the occasional clients.

We do not consider the last category as there are less than 50 observations in this group. Of the 1,592 clients surveyed in our panel, 47.2 percent are continuing clients, 11.3 percent are long-term drop-outs, and 11 percent are medium-term drop-outs. There are only 144 households who are *newcomers1*, and 76 who are *newcomers2*. 723 households never participated in any round. The comparison group in the sample are (matched) never participants who could potentially include (*i*) eligible households in control villages (so they do not have access to any program); (*ii*) ineligible to participate in a program; and (*iii*) eligible and staying in a program village but did not participate. The presence of the last group means that there is potential selection bias since they choose not to participate. We exclude them in our estimate. We also exclude ineligible clients.

We estimate the *long-term* treatment effect by comparing households who are continuing clients (for at least 8 years) to those who could never participate in the program. The entry into the program by some households at a later period possess a challenge to evaluate the program because of concerns regarding the timing of the participation and the consequent selection bias. Discussion with household members indicate that many eligible households applied for the program later because they were initially unaware of the availability of the microcredit program. There was also uncertainity over their eligibility status, and the waiting period to obtain a microcredit loan. Also, the program was not available in all villages at the same time. We estimate the treatment effects for the new participants under the identifying assumption that those who joined later in the program are systematically no different, conditional on observables and time-invariant characteristics, from those who joined earlier. We can further relax this assumption using the baseline information collected in 1997-98 for this group. For newcomers, the estimated impacts are based on the changes in outcome before and after participation in the program. The estimates obtained using *newcomers2* are termed as "short-term effects," while the corresponding estimates for *newcomers1* are termed "medium-term effects," considering their length of participation in microcredit.[18]

We consider leavers from the program separately to examine whether the impacts of the program last beyond the period when the households left the program. It may be argued that those who benefit most stay in the program while those who fail to gain immediate benefits drop out, or vice versa. Alexander-Tedeschi and Karlan (2009) argue that the cross-sectional impact estimates will be biased if we exclude drop-outs from the treatment group. Fortunately, with the availability of the panel dataset such concern is not important in our case. We track drop-outs for up to 8 years post-program, and compare leavers with those non-clients who would have dropped-out had they participated in microcredit. *Leavers1* left the program immediately after 1998 and did not participate in any other program. We estimate the changes in outcome before and after the departure from the program. Thus our estimates are not biased as they would have been under a cross-sectional impact assessment. Results using a sample of *leavers1* are referred to as the *long-run effect* considering their length of the non-program status. Similarly, estimates obtained using *leavers2* are referred to as the *medium-run effect*. Using PSM helps us to isolate the control households who would themselves drop out if they had been allowed to participate. We argue that impacts occurring in subsequent years should add to the accumulated impact amounts (impact estimates for continuing clients) to measure the overall impact of participation in the program. Insofar as leavers from the program do reap benefits from their short-lived participation, these benefits ought to be included in the assessment of the value of any microcredit program. Therefore, the treatment effect of microcredit is underestimated if we exclude the leavers since the total impact of a program is equal to benefits to continuing participants plus leavers.[19]

[18] The results for these two groups are based on their baseline characteristics as we could observe these households before their participation in the program.

[19] The identifying assumption is that there is no dynamic sorting of households with high or low potential outcomes participating early or late in the program. If there is a dynamic sorting, then the duration of the participation in microfinance or decisions of when to leave or to participate in the program is

4.3 Results: Double- and Triple-Difference Estimates

Table 3 presents the results for continuing participants using the double and the triple-difference matching approach.[20] The lefthand side compares the changes in outcomes for eligible clients in the program village (column 1) to the change for eligible households (non-clients) in villages that do not have any program (column 2). Each cell in the first two columns contains the mean average outcome variables for these two groups. The third column represents the difference between two groups at a given point in time, and its standard error is reported in the fourth column. Panel B shows the difference in outcome variables over time (8 years) for each group. Total food consumption increased by taka 736 in the client group, and by taka 601 in the control group. Thus there was a relative increase of taka 134 in food consumption of eligible treated households in the program village; this is the double-difference estimate of the impact of continuous participation over 8 years in microcredit. This figure represents a relative increase in food consumption of 6.6 percent by continuing clients. Similar calculations show that non-food consumption expenditure of clients increased by about 12.4 percent. The double-difference estimates of self-employment income, and assets for the continuing participants are even higher, representing increases of 28.1 and 14.8 percent, respectively. The results show that there is very little or no impact on "other income."

However, if there was a distinct shock to the treatment villages over this period or if a MCI selects a village observing certain shocks in that village then double-difference estimate does not correctly identify the impact of the treatment. We examine this in the righthand side of Table 3, where we perform the same exercise for ineligible groups who did not receive any treatment (Columns 5 and 6, respectively). These households are unaffected by the microcredit program, and we use them to difference away any relative

likely to depend on the unobserved potential outcomes perceived by households but not by researchers. In cross-sectional data, the difference between groups is important and may invalidate the results. Our results do not suffer from such problems since we are differencing out any differences that could exist in terms of outcome variables. We also find, using the first round of this dataset, no significant differences between eligible households in treatment and control groups in terms of either outcome or observable characteristics (results not reported but are available upon request).

[20] The reported results are based on the first and last rounds of the survey. We use these two rounds because there was a flood at the end of 1998 in Bangladesh, and many of the outcome variables could be disproportionately affected by post-flood rehabilitation programs, and damage from floods. Although the 1999-2000 survey took place more than one year after the flood, a shock of that magnitude is likely to have impact in the following year or so on household behaviour and outcomes.

trend in the treated and control groups correlated with unobservables, but not due to the program. We find a slight decrease in food consumption and large reduction in assets, little increase in non-food consumption, but a significant increase in income and self-employment income among ineligible households in the treatment village compared to the control village.

Taking the difference between the two sides of Table 3 (Panel C, columns 1 and 5), there is a 7 percent and 11.7 percent gain in food and non-food consumption, respectively, for continuing participants. The gain in self-employment income is 15.1 percent, and while "other income" is increased by 6.5 percent. The gains in terms of accumulating assets is 14.8 percent (DD estimate) and 41.3 percent (DDD estimate). Thus, if the DDD strategy is taken to be more suitable than the DD strategy in separating out the treatment effect of microcredit, impact estimates are overstated for long-term clients in the case of self-employment income and non-food consumption, but understated in the case of income, food consumption and assets using the double-difference method.[21]

The medium and short-term treatment effects are reported in Table 4. They are represented by the triple-difference estimates for *newcomers1* and *newcomers2*. The estimates for the newcomers are obtained using the baseline and post-program outcome in 2004-05. The results show that *newcomers2* enjoy a large increase in food consumption, while *newcomers1* experience a moderate fall in food consumption. Combining food and non-food expenditure we observe an increase of expenditure for *newcomers2* while a decline in expenditure for *newcomers1*. The estimated impact for *newcomers1* indicates a smaller positive effect on self-employment income and other income, and a large increase of non-land assets. *newcomers2* also gain more self-employment income and other income. The exception is assets, where we find a large decline. While it is not obvious why consumption of *newcomers1* are declining while their income is increasing, the results from Kaboski and Townsend (2005) and Banerjee et al. (2009) suggest that such an outcome is not unusual. The differential results between medium-term and long-term impacts may be

[21] The reported standard errors are larger and this is partly due to non-parametric matching estimates, and partly due to the smallness of the sample in each category. See Angrist (1998) and Zhao (2006) who compare the performance of matching and regression methods and find the former estimators have larger standard errors. In fact, we will see in the next section that a regression method of our approach that includes controls gives a tighter confidence interval, and so the coefficents become statistically more significant.

attributable to the additional gains resulting from longer participation or larger amounts of credit being borrowed from the MCI. The overall results of newcomers indicate that the gains are lower than the continuing clients. In fact, we obtain some results which show a very insignificant (or sometimes negative) impact for newcomers. But, these results are not statistically significant at the conventional level. The results do, however, indicate that if one examines microcredit households using short-term program participation data, the impact estimates could be biased.[22]

Table 5 gives the triple-difference estimates for leavers. The lefthand side shows the results of *leavers1* while the righthand side reports the corresponding results for *leavers2*. The results indicate that the impact estimates are positive for all outcome measures except for food consumption. The results indirectly indicate that drop-outs leave the program not because they are unsuccessful. Only about 5 percent reported that they dropped out because of difficulty in repaying their loan. About 60 percent of households mentioned the frequency of loan repayment and the necessity to attend a weekly meeting as the major reason of dropping out. The descriptive statistics (unreported here) show no significant difference between leavers and continuing clients in terms of other observable (demographic) variables. It is likely that there are some unobserved factors that might predict why these households dropped out. They could include, for example, pressure from husband/family not to attend the meeting, impatience with following the procedure of getting and paying the loan, busy with other activities such as taking care of children or parents, and so on. Causal observations and discussion with borrowers indicate these factors influence their decision to leave the program. However, the results indicate that households can achieve a substantial gain if they are patient and can wait a few years to obtain a larger loan. The potential for gains are larger than simply leaving the program as can be seen when comparing the results of stayers and leavers. In table 5, when we compare two groups of leavers the resulting impacts are very similar for these two groups except for assets. *Leavers2* (more recent drop-outs) still have a sizeable increase in assets than their older counterpart, *leavers1*. Comparing *leavers1* with *leavers2* we find that the increase in the size of treated-untreated differentials are decaying. This also implies that the size of the effects, beyond the years during which households were participants,

[22] These results are based on a smaller sample size.

is diminishing.

We also estimate the treatment effects using a triple-difference approach proposed by Ravallion, Galasso, Lazo and Philipp (2005). According to this approach, we need to compare changes in outcomes of continuing participants and matched leavers, after netting out the outcome changes for a matched comparison group that never participated. The estimation method requires the following steps: (1) calculate the (propensity score weighted) outcome difference between continuing participants and matched non-participants; (2) calculate the outcome difference between leavers from the program and matched non-participants; (3) take the difference of each of (1) and (2) at two points of observation; and (4) take the double differences calculated in (3). In essence, this requires the subtraction of the double-difference estimates of continuing clients (Table 3) from the double-difference estimate of *leavers1* (Table 5). The resulting impact estimates are positive in all cases as shown in Table 6. When compared to estimates obtained for continuing participants in Table 3, we find impacts that are larger in the case of food consumption and slightly smaller for all other outcome variables (see last column, Table 6). Since drop-outs from microcredit are expected to receive partial treatment (e.g., due to the continuing return from an old investment project, or training received from an MCI) which could increase their income/assets, these triple-difference estimates are likely to be understated. However, it also gives estimates of what drop-outs could have gained had they not left the program.

4.4 Triple-Difference in Regression Framework

In order to increase the precision of triple-difference estimates we use a regression framework. By adding controls, we hope to net out the influence of factors such as household age, gender, education and family composition, etc., that may have influenced the income, consumption and assets over the study period. We run the following reduced-form regression:

$$\begin{aligned} Y_{it} &= \alpha_i + \theta X_{it} + \beta_1 \delta_t + \beta_2 villeli + \beta_3 (villeli \times \delta_t) \times + \beta_4 D_i + \beta_5 (D_i \times villeli) \\ &\quad + \beta_6 (D_i \times \delta_t) + \beta_7 (D_i \times villeli \times \delta_t) + \varepsilon_{it} \qquad (4) \end{aligned}$$

where Y_{it} is the logarithm of outcome variables (except the self-employment income), and X is a vector of control variables. *villeli* is a dummy variable (=1 if eligible and staying in program village, 0 otherwise), δ_t is the fixed year effect which controls for macroeconomic changes, D_i is the treatment variable. Here, first we consider the continuing participants as the treatment group, and exclude occasional members of microcredit. In equation (4), β_3 controls for changes that happened for eligible households in the treatment village over time versus ineligible households, β_5 captures the secular differences between eligible and ineligible households in the treated group, β_6 captures changes over time of the treatment group. The third level of interaction coefficient β_7 captures all variations in outcomes specific to the treatment group (relative to the non-treated group) in the program village (relative to the control village) in 2004-05 (relative to 1997-98). This is the DDD estimate of the impact of microcredit program on (continuing) participants.

We present results for both the matched sample and the full sample in Table 7 using equation (4). The estimated treatment effects for continuing participants are shown in columns (1) and (2). The results indicate that households can increase food consumption, income, self-employment income, and assets. However, the resulting estimate is negative for non-food consumption. The results are similar using both the matched sample and the full sample of continuing clients. In columns (3) and (4), we report results of the treatment effects using all participants, including occasional participants. It was computationally cumbersome to estimate separate regression for each type of participant (leavers, newcomers). Thus, we include all categories of clients. The results in Table 7 show that households' participation in a microcredit program can increase self-employment income by 60-70 percent, and assets by 40-50 percent. Regression adjustment increases the magnitude of the estimated coefficients, and reduces the standard errors of coefficient estimates. This suggests that there is a negative correlation between household socio-economic status and the participation/microcredit demand. These results are still lower than McKernan (2002) who finds that households more than double their self-employment earnings (126 percent increase in self-employment profits) by participating with the Grameen Bank.[23] de Mel, McKenzie and Woodruff (2008, 2009) find similar results in terms of the returns

[23] Our results are not directly comparable with Pitt and Khandker (1998), McKernan (2002), Khandker (2005) since we are using a different dataset in terms of the MCIs and households, as well as different time periods.

from microenterprise owned by men in Sri Lanka. The results on self-employment income are qualitatively similar to our earlier estimates for continuing clients. The estimated treatment effects are larger using continuing clients, indicating that the treatment effect of microcredit is higher for continuing participation than occasional participants.

5 Conclusion

This research utilizes a new and significantly extended database for Bangladesh to examine the impact of microcredit programs. An important contribution of this paper is the investigation of the sensitivity of the impact estimates with respect to the length of participation in microcredit programs, and the quantification of any ongoing effects subsequent to the departure from the program. To this end, a clear distinction is drawn between short-, medium- and long-term effects to reflect the length of participation in the program and to capture the post-program consequences of microcredit lending.

The findings of the study enable us to draw several conclusions about medium-term and long-term impacts of microcredit lending schemes in Bangladesh. The results show that continuing participants gain in all outcome measures, and the treated-untreated differentials are larger for these households. This signifies that long-term participation in microcredit can help households proportionately more than short-term participation. There is also sufficient evidence that the gains accrue beyond the participation period. The estimated treatment effects are lower when we include drop-outs in the treatment groups. Although we are uncertain about the precise magnitude of impacts over the long run, the results indicate that the benefits may not accrue indefinitely following withdrawal from the program.

The main conclusion of this study is that the graduation from poverty using microcredit in Bangladesh requires longer-term participation. It takes time for household entrepreneurs to achieve productive efficiency or to generate higher returns from self-employment activities. Since existing members of microcredit obtain larger loans by participating in a program over a longer-term, our results indirectly point out that MCIs may provide larger loans sooner rather than later. This is an argument also put forward by Ahlin and Jiang (2008). Our results suggest that conventional program evaluations that are based on the outcomes reported by continuing participants may underestimate the contribution of microcredit programs. The results also imply that using short-term treatment data in a microcredit program may not provide a reliable estimate of the overall impact of the program.

The results for leavers and newcomers are, however, subject to the small sample problem. The results are therefore only indicative, and more research will be needed to draw more definite conclusions. In this study we aim to tackle an important and largely ignored question in development economics, which is the impact of medium and long-term participation in a microcredit program. The results in this paper are based on large, representative, mainstream MCIs from Bangladesh. It would also be important to examine whether these findings hold true in other countries with similar programs. Hopefully, this paper will generate more debate on this issue and will encourage further research on the impact of different lengths of participation in microcredit.

References

Ahlin, C. and N. Jiang. 2008. "Can micro-credit bring development?" *Journal of Development Economics*, 86(1): 1-21.

Alexander-Tedeschi, A. and D. Karlan. 2009 "Cross-sectional Impact Analysis: Bias from drop-outs." *Perspectives on Global Development and Technology, microcredit special issue*, forthcoming.

Angrist, J. 1998. "Estimating the labor market impact of voluntary military service using social security data on military applicants." *Econometrica* , 6(2): 249-288.

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

Bertrand, M., E. Duflo, and S. Mullainathan. 2004. "How much should we trust differences-in-differences estimates?" *Quarterly Journal of Economics*, 119(1): 249-75.

Coleman, B. 1999. "The impact of group lending in Northeast Thailand." *Journal of Development Economics*, 60(1):105-41.

Crump, R., J. Hotz, G. Imbens, and O. Mitnik. 2009. "Dealing with Limited Overlap in Estimation of Average Treatment Effects," *Biometrika*, 96(1):187-199

Dehejia, R. and S. Wahba. 1999. "Causal effects in nonexperimental studies: reevaluating the evaluation of training programs." *Journal of the American Statistical Association*, 94(448): 1053-62.

Dehejia, R. and S. Wahba. 2002. "Propensity score matching methods for noexperimental causal studies." *Review of Economics and Statistics*, 84:151-61.

de Mel, S., D. McKenzie, and C. Woodruff. 2008. "Returns to Capital: Results from a Randomized Experiment." *Quarterly Journal of Economics*, 123(4): 1329-72.

de Mel, S., D. McKenzie, and C. Woodruff. 2009. "Are women more credit constrained? Experimental evidence on gender and microenterprise returns." *American Economic Journal: Applied Economics*, 1(3): 1–32.

Falaris, E. 2003. "The effect of survey attrition in longitudinal surveys: evidence from Peru, Cote d'Ivoire, and Vietnam." *Journal of Development Economics*, 70:133-157.

Fitzgerald, J., G. Peter, and R. Moffit. 1998. "An analysis of sample attrition in panel data: The Michigan panel study of income dynamics." *Journal of Human Resources,* 33(2):251-299.

Heckman, J. and V. Hotz. 1989. "Choosing among alternative nonexperimental methods for estimating the impact of social programs: the case of manpower training." *Journal of the American Statistical Association*, 84(408): 862-74.

Heckman, J., J. Smith, and C. Taber. 1998. "Accounting for dropouts in evaluations of social programs." *Review of Economics and Statistics*, 80(1): 1-14.

Islam, A. 2008. "Who Benefits from microcredit? An impact evaluation of large scale programs in Bangladesh." Working Paper, Department of Economics, Monash University

Islam, A. and P. Maitra. 2009. "Health shocks and consumption smoothing in rural households: Does microcredit have a role to play? Working Paper, Department of Economics, Monash University

Kaboski J.and R.Townsend. 2009. "The Impact of credit on village economies." Working Paper, Department of Ecnomics, MIT.

Kaboski J.and R.Townsend. 2005. "Policies and impact: An analysis of village-level microcredit institutions." *Journal of the European Economic Association*, 3(1): 1-50.

Karlan, D. and J. Zinman. 2010. "Expanding credit access: Using randomized supply decisions to estimate the impacts." *Review of Financial Studies*, 23(1):433-464.

Karlan, D. and J. Zinman. 2009."Expanding Microenterprise Credit Access: Using randomized supply decisions to estimate the impacts in Manila." Working Papers, Yale University

Khandker, S. 2005. "microcredit and poverty: Evidence using panel data from Bangladesh." *World Bank Economic Review*, 19(2): 263-86.

McKernan, S. 2002."The impact of microcredit programs on self-employment profits: Do noncredit program aspects matter?," *Review of Economics and Statistics*, 84(1):93-115,

Morduch, J. 1998. "Does microcredit really help the poor? New evidence from flagship programs in Bangladesh." Working Paper, New York University.

Pitt, M. and S. Khandker. 1998. "The Impact of group-based credit programs on poor households in Bangladesh: Does the gender of participation matter?" *Journal of Political Economy*, 106(5): 958-96.

Ravallion, M., E. Galasso, T. Lazo, and E. Philipp. 2005. "What can ex-participants reveal about a program's impact?" *Journal of Human Resources*, 40(1): 208-30.

Rosenbaum P. and D. Rubin 1983. "The central role of the propensity score in observational studies for causal effects." *Biometrika*, 70(1): 41-55.

Thomas, D., E. Frankenberg, and J. Smith. 2001. "Lost but not forgotten: Attrition and follow-up in the Indonesia family life survey." *Journal of Human Resources,* 36(3):556-592.

World Bank. 2007. "microcredit in South Asia: Toward financial inclusion for the poor." Washington D.C., December.

Zhao, Z. 2006. "Matching estimators and the data from the national supported work demonstration again," IZA Discussion Paper.

Appendix:

Variables used in the estimation of propensity score:

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

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

Table 1: Descriptive statistics

	1997-1998			1999-2000			2004-05		
Demographic Variables	Treat	Control	Diff	Treat	Control	Diff	Treat	Control	Diff
Age of the Head	44.01	45.24	**-1.23**	46.26	47.52	**-1.26**	47.62	47.87	-0.25
# of working people	2.8	2.81	-0.01	2.98	3.06	-0.08	3.63	3.56	0.07
Household size	5.71	5.5	**0.21**	6.09	6.03	0.06	7.23	7.23	0
Max education by any member	5.29	5.75	-0.46	5.92	6.64	**-0.72**	7.08	7.43	-0.35
Area of arable land	57.72	84.01	**-26.29**	64.92	101.72	**-36.8**	54.55	91	**-36.45**
# of children	2.93	2.69	**0.24**	2.32	2.1	**0.22**	3	3.02	-0.02
# of women	2.72	2.57	**0.15**	2.99	2.88	0.11	3.32	3.2	0.12
# of old people	0.21	0.3	**-0.09**	0.35	0.44	**-0.09**	0.28	0.33	**-0.05**
# of married people	2.4	2.37	0.03	2.72	2.68	0.04	3.2	3.11	0.09
If women is head	0.05	0.07	**-0.02**	0.05	0.06	-0.01	0.09	0.13	**-0.04**
Outcome Variable (in taka)									
Food Cons. (M)	2419.2	2452.4	-33.2	2833.7	3101.9	-268.2	3232.6	3197.8	34.8
Non-Food exp (M)	688.4	998.6	**-310.2**	507.5	674.2	**-166.7**	821.8	1114.1	**-292.3**
Non-land total Asset	17787.7	23153.7	**-5366**	21575.1	23866.4	**-2291**	17906.4	25605.8	**-7699**
Other Income (M)	34263.1	31114.4	**3148.7**	31708.9	41044.4	**-9336**	38457.9	51403.2	**-12945**
Self-emp. income (M)	6703.7	5007.3	1696.4	8312.6	1507.6	**6805**	11659	2378.8	**9280.2**
Amount of credit	7427.3			10616.8			11682.5		
# of clients	1592			1532			1280		
# of obs.	2694			2694			2694		

Note: Difference that are statistically significant at 5 percent level are in bold.

Exchange rate between taka and US$ in 1998 was 40/$

Table 2: Impact Estimates using Fixed Effects and Random Growth Models

	Fixed Effects Model		Random Growth Model	
Outcome of Interest	All Sample (N=2694)	Matched Sample (N=1874)	All Sample (N=2691)	Matched Sample (N=1872)
Food cons. exp	0.0168	0.0184	0.0129	0.0047
	(0.0069)	(0.0094)	(0.0202)	(0.0263)
Non-food cons. exp	0.1172	0.0304	0.0458	0.0434
	(0.0121)	(0.0082)	(0.0177)	(0.0191)
Other income	-0.0094	-0.0077	0.0504	0.065
	(0.0064)	(0.0087)	(0.0194)	(0.0234)
Self- emp. Income[1]	0.0977	0.147	0.1284	0.3166
	(0.0239)	(0.0291)	(0.0658)	(0.0777)
(Non-Land) Asset	0.0462	0.0485	0.03884	0.0486
	(0.0084)	(0.0109)	(0.01421)	(0.0159)

Notes:
The regressions include household demographic and socio-economic variables as controls. Standard errors presented in parenthesis are corrected for clustering at the village and year level. The matched sample is based on the propensity score estimated using first cross-section data using a wider set of household and village level variables.
[1] Estimated coefficient is based on random effect tobit model since fixed effects tobit are biased and inconsistent

Table 3: DDD Estimates of the impact of Microfinance on Continuing Participants

	Eligible Group (N=574)				Ineligible Group (n=149)			
	treated	non-treated			non-treated	non-treated		
	Program Village	Control Village	Location Difference	S.E of Location Difference	Program Village	Control Village	Location Difference	S.E of Location Difference
1997/98 **(A)**	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Food Consumption	2047.6	1982.0	65.6	97.0	2632.8	2578.6	54.3	200.9
Non-food Consumption	478.3	610.7	-132.5	69.2	1248.1	927.4	320.7	241.5
Other Income	2731.8	3122.7	-390.9	201.6	3709.2	2812.5	896.7	403.5
self-employment income	912.4	856.7	55.7	144.0	171.2	469.6	-298.4	195.9
Asset	14442.7	18146.9	-3704.2	2231.2	38396.2	26016.5	12379.7	5083.1
2004/05								
Food Consumption	2783.6	2583.7	199.9	136.1	3336.0	3290.6	45.4	283.5
Non-food Consumption	638.2	711.2	-73.1	76.8	1454.1	1129.9	324.3	273.8
Income	3286.7	3674.8	932.5	419.7	7095.0	6374.2	720.8	814.0
self-employment income	963.1	651.1	312.0	164.2	135.2	315.1	-179.9	253.6
Asset	14645.5	16209.9	-1564.4	2113.3	42049.8	33494.6	8555.3	6732.0
Time Difference **(B)**								
Food Consumption	736.0	601.7			703.2	712.0		
Non-food Consumption	159.9	100.5			206.0	202.5		
Other Income	554.9	552.1			3385.8	3561.7		
self-employment income	50.7	-205.6			-36.0	-154.5		
Asset	202.8	-1937.0			3653.7	7478.1		
Double Difference **(C)**		s.e	%Gain			s.e		
Food Consumption	134.3	167.1	6.6		-8.8	347.4		
Non-food Consumption	59.4	103.4	12.4		3.5	365.2		
Other Income	2.8	465.6	0.1		-175.9	908.5		
self-employment income	256.3	218.4	28.1		118.5	320.4		
Asset	2139.9	3073.1	14.8		-3824.4	8435.5		
Triple Difference **(D)**		s.e	%Gain					
Food Consumption	143.2	385.5	7.0					
Non-food Consumption	55.9	379.5	11.7					
Other Income	178.7	1020.9	6.5					
self-employment income	137.8	387.8	15.1					
Asset	5964.3	8977.9	41.3					

Notes: Matching without replacement, caliper <.0005 (.005 for ineligible group). Observations with too high or too low values are omitted in the final estimation. The sample size change slightly depending on the number of match available in each case.

Table 4: DDD Matching Estimates of the impact of Microfinance on "Newcomers"

	Medium-term participation Impact (Newcomers1)[1]				**Short-term participation effects (newcomers2)[2]**			
	Single difference (N=228))		Double difference (Treatment group)	Triple difference	Single difference (N=110)		Double difference (Treatment group)	Triple difference
	1997/98	2004/05			1997/98	2004/05		
Food Consumption	-162.38	-239.99	-77.61	-68.77	-332.14	-13.51	318.63	327.47
	(125.36)	(120.99)	(174.22)	(388.64)	(179.95)	(240.47)	(300.34)	(459.23)
Non-food consumption	-100.02	-101.73	-1.71	-5.24	-90.86	-164.49	-73.63	-77.15
	(101.39)	(81.40)	(130.02)	(387.61)	(89.58)	(100.72)	(134.79)	(389.24)
Other Income	-656.92	-730.75	-73.83	102.1	-156.84	-120.64	36.19	212.13
	(270.56)	(424.90)	(503.73)	(1038.82)	(326.45)	(391.95)	(510.09)	(1041.92)
Self- Empl. Income	-3.91	127.82	131.73	13.25	427.84	647.71	219.88	101.4
	(163.41)	(145.56)	(218.84)	(388.04)	(240.51)	(390.42)	(458.56)	(559.43)
Asset	-6244.59	-5962.75	281.84	4106.28	1576.83	-4048.3	-5625.12	-1800.68
	(3129.44)	(2682.04)	(4121.50)	(9388.52)	(3476.51)	(2572.95)	(4325.07)	(9479.66)

Notes:

[1] households who joined the program after the 1999, and remained as clients up to 2004-05. [2] households who joined in the program after 2001.

Double difference (treatment group) is obtained by subtracting column (2) from column (1). Triple difference is obtained using ineligible households in program and control villages. Matching is done without replacement using caliper <.0005 (.005 for ineligible group because of smaller sample size). Observations with too high or too low values are omitted in the final estimation. The sample size changes slightly depending on the number of match available in each case.

Table 5: DDD Matching Estimates of the impact of Microfinance on "Leavers"

	Long-run effects of participation (leavers1)[1]				Medium-run effects of participation (leavers2)[2]			
	Single difference (N=160)		Double difference (Treatment group)	Triple difference	Single difference (N=365)		Double difference (Treatment group)	Triple difference
	1997/98	2004/05			1997/98	2004/05		
Food Consumption	124.09	-90.11	-214.21	-205.3	101.65	-71.97	-173.62	-164.78
	(135.80)	(197.06)	(239.32)	(421.8)	(99.87)	(139.25)	(171.36)	(387.36)
Non-food consumption	10.53	62.93	52.4	48.88	-40.37	11.02	51.39	47.87
	(121.72)	(144.68)	(189.07)	(411.2)	(77.08)	(92.57)	(120.46)	(384.51)
Other Income	232.07	161.7121	-70.36	105.57	-25.34	237.1092	262.45	157.27
	(321.10)	(506.46)	(599.67)	(1088.5)	(217.11)	(341.49)	(404.67)	(994.57)
Self- Empl. Income	-158.23	38.94	197.18	78.7	-23.61	115.96	139.57	21.08
	(152.77)	(275.79)	(315.27)	(449.5)	(156.18)	(188.67)	(244.92)	(403.33)
Asset	-442.08	-661.73	-219.64	3604.8	-3866.56	-663.87	3202.69	7027.13
	(3373.77)	(3014.01)	(4524.00)	(9572.0)	(2664.35)	(2371.47)	(3566.88)	(9158.62)

Notes:
[1] Old clients who dropped out after 1998 and did not participate any any MFI;
[2] Clients who participated until 2001 and then dropped out.
The sample size changes slightly depending on the number of match available in each case. Triple difference is obtained using ineligible households in program and control villages. Matching is done without replacement using caliper <.0005 (.005 for ineligible group because of smaller sample size). Observations with too high or too low values are omitted in the final estimation. The sample size changes slightly depending on the number of match available in each case.

Table 6: DDD Matching Impact estimates: Stayers versus leavers

Table 6: DDD estimates of program participation: Stayers versus leavers

	DD regular	DD leaver	DDD (Ravallion)	DDD
Food Consumption	134.32	-214.21	348.52	143.16
	(167.09)	(239.32)	(386.51)	(385.49)
Non-food consumption	59.4	52.4	7.0	55.88
	(103.38)	(189.07)	(103.62)	(379.50)
Other Income	1323.33	-70.36	1393.69	1499.26
	(465.62)	(599.67)	(1469.41)	(1020.89)
Self- Employment Income	256.27	197.18	59.09	137.79
	(218.42)	(315.27)	(226.27)	(387.81)
Asset	2139.85	-219.64	2359.5	5964.29
	(3073.13)	(4524.00)	(3874.45)	(8977.85)

Notes:
DDD (Ravallion) are estimated following Ravallion et al. (2005) and is derived by subtracting column (1) from column (2). The last column labelled as DDD is taken from previous estimates to compare results with column (3)

Table 7: Regression Adjusted DDD Impact estimates

Outcome Variable	Continuing participants Unmatched Sample	Continuing participants Matched Sample	All Participants Unmatched Sample	All Participants Matched Sample
	(1)	(2)	(3)	(4)
Food Consumption	0.0888	0.0666	0.0604	0.0688
	(0.0636)	(0.0752)	(0.0554)	(0.0666)
Non-food Consumption	-0.1513	-0.0228	-0.2514	-0.1266
	(0.0929)	(0.1038)	(0.0940)	(0.0903)
Other Income	0.0306	0.0316	0.057	0.0313
	(0.0961)	(0.1043)	(0.0831)	(0.0888)
Self-employment income	0.8257	0.7081	0.6960	0.6070
	(0.2622)	(0.2651)	(0.2420)	(0.2490)
Asset	0.5341	0.4852	0.4029	0.3846
	(0.2221)	(0.2201)	(0.1775)	(0.1765)
	N=1470	N=1397	N=2694	N=1874

Notes: Standard errors presented in parenthesis are corrected for clustering at the village and year level. The matched sample is based on the propensity score estimated from first cross-section data. A household is chosen in the matched sample if its propensity score lies within the probability distance of 0.0005

Table 8: Descriptive statistics by Participation status of Attritors (1997/98)

Variables	Treatment	Control	Difference	p-value
Age of household head	43.02	44.59	-1.57	0.3
Number of adult working people in the household	2.52	2.54	-0.02	0.9
Household size	5.28	5.15	0.13	0.62
Highest Grade/class passed by any family member	5.05	5.55	-0.5	0.29
Total arable land owned by household	65.79	60.02	5.78	0.78
Number of children aged 0-15 in the household	2.67	2.59	0.08	0.66
Number of female member in the household	2.64	2.48	0.17	0.28
Number of old people of age above 60 yrs	0.25	0.25	-0.01	0.91
Whether women is the head of the household	0.06	0.11	-0.05	0.12
Number of married people in the household	2.24	2.33	-0.09	0.32
Average age of all member in the household	24.43	25.33	-0.9	0.41
Sample Size	184	147		

Figure-1: Estimated Odds Ratio for Treatment and Comparison Groups