Medium and Long-term Participation in Microfinance: An Evaluation Using a Large Panel Data Set from Bangladesh*

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Abstract

The objective of this paper is to estimate the impact of different lengths of participation in microfinance, and to distinguish the short-term participation effects from the medium- and long-term ones. It utilises a new, large and unique panel data set with detailed information on microfinance participation in Bangladesh. The three waves of data enable us to identify continuing participants, leavers from the program and new entrants. We estimate differing treatment effects based on a household's treatment status (as defined by their length of time in a particular program). Due to the long duration between the first and final (third) survey waves (8 years), and different categories of participants, we are able to estimate, for the first-time, the short-, medium- and long-run impacts of microfinance, and also the impacts of long-and medium-term duration in program participation. We employ different estimation strategies including "difference-in-difference-in-difference" and "propensity score" methods to control for selection bias. The overall results indicate that the regular participants gain in terms of increased consumption, self-employment income, other income and assets. The impact estimates indicate that larger benefits can accrue from long-term participation. The results also indicate that benefits may continue to exist even after program exit.

JEL Classification:

Keywords: Microfinance, difference-in-difference-in-difference, matching, short-term, long-term

*We wish to thank

1 Introduction:

Over the past two decades microfinance has become an important tool for poverty reduction. In Bangladesh in particular, it has gained tremendous international attention for its focus on women borrowers and providing collateral-free loans to them. A substantial proportion of the low-income families of the many developing countries are now served by the microfinance. The number of people who received credit from microfinance institutions (MFIs) reached 113.3 million in 2005. Coverage is particularly impressive in Bangladesh, where the modern microfinance system was born, and in 2005 microfinance reached more than 60 percent of the poor (World Bank 2007). Though there are a large number of microcredit schemes in operation around the word, there is currently no evidence about the medium- and long-term benefits of participating in a microfinance program. The impact evaluation of microfinance has thus far concentrated on measuring the short-term impacts which are mostly based on cross-section data (see, for example, Pitt and Khandker 1998; Coleman 1999; Kaboski and Townsend 2005; Islam 2007; Karlan and Zinman 2008).[1]

The objective of this paper is to measure the impact of different lengths of participation in microfinance, and to distinguish between the short-term participation effects from the medium- and long-run ones. We also estimate long-run program impacts using the follow-up survey of households who left the program but who were retained in the sample.[2] Extrapolating short-run estimates of social program impacts to the longer run can be misleading, especially with regard to microfinance programs. That is, the impact of microfinance is likely to vary with the length of program participation. It could be that the larger impact is realized in the short-run if there are increasing returns to capital in

[1]Pitt and Khandker (1998) find that microfinance significantly increases consumption expenditure, reduces poverty, and increases non-land asset. They use land-based eligibility criterion as the instument for program participation.The results, however, have raised methodological issue. For example, Morduch (1998), using the same dataset but different estimation strategy (difference-in-difference approach), finds that microcredit has insignificant or even negative effects on the same measures of outcomes as Pitt and Khandker examine. Khandker (2005), using a subsequent round data, finds results much more muted than the initial results based only on the cross-section data. McKernan (2002), using the same methodology as that of Pitt and Khandker, finds a large positive effects of participation on self-employment profits. Islam (2007) finds microfinance helps to increase consumption for those who are poorer, however, the effect is not that much signficant. In case of Thailand, Coleman (1999) finds that average program impact is insignificant on physical assets, savings, labor time, expenditure on education and health care. Kaboski and Townsend (2005) find only institutions with good policies can promote asset growth, consumption smoothing in Thailand but not the others. They find no measurable impacts of joint liability or repayment frequency. Karlan and Zinman (2008) examines the impact of expanding access to consumer credit using a data gathered from field experiment in South Africa. The results from household surveys following 6-12 months of the experiment indicate significant and positive effects on income, food consumption, and job retention.

[2]We abuse the the words "term" and "run" and differentiate between them. When we consider impact estimates of "leavers" from the program, the resulting estimates are called as medium or long-"run" depending on the length of post- participation, while the corresponding estimates for continuing participants are called "term".

household enterprises. On the other hand, many households may not obtain the potential return of their investment until they invest sufficient sums of money. Typically, it takes a member several years to obtain larger loan amounts from a MFI since bigger loans are usually only sanctioned once borrowers have established a trustworthy reputation with the MFI (by a favorable track record on previous loan servicing). Thus, differing investments will have differing time horizons in their returns' profile. So, results from solely short-term evaluations are likely to bias overall estimates of program participation. This suggests a need to evaluate program effects over a longer time horizon.

The recent availability of eight-year follow-up data offers an opportunity to explore important questions about microfinance program impacts. We use a completely new survey of three waves of a panel data set of treatment and control groups of microfinance households collected for a longer term periods, data that was not previously available. We track households over time in each village, including their transitions from participating (non-participating) to non-participating (participating) status. The survey encompasses about 3000 households from 91 villages and covers 13 different sizes of MFIs in Bangladesh. We exploit the data for the control groups to implement tests of some of the assumptions that justify the use of non-experimental methods. The survey encompasses household, village and organizational level data collected in 1997/1998, 1999/2000, and 2004/05. This is the largest comprehensive panel data set on microfinance households studied so far. We evaluate the impact on self-employment income, other income, food and non-food consumption expenditure and assets.

We consider a variety of approaches to estimate the program impact on participants. Estimates obtained from using different methods often yield different parameters of interests, and hence are interpreted accordingly. In the estimation strategy, we first employ different panel data models: fixed effects and random growth models. Then, we consider a simple difference-in-difference (DD) approach. However, the DD method cannot solve the problem when the subsequent outcome changes are a function of initial conditions that also influence participation. So, we combine the DD approach with the propensity score matching method of Rosenbaum and Rubin (1983) where both treatment and control groups are matched based on observable initial characteristics. So, our methods allow us to control for time-invariant observable and unobservable household and village characteristics, and minimize the differences in the distribution of participating households and their comparisons.

However, the above methods work well if we have a baseline information for both groups. In the absence of pre-program baseline data, estimating a "treatment on the treated" (TT) effect requires further identifying assumptions. Fortunately, with the availability of three waves of data, we can estimate the TT parameter under less restrictive identifying assumptions. In addition, the targeting of the program based on a land-based eligibility criterion does allow for refining the sample, and to use a different evaluation method than those used so far for microfinance program evaluation. It appears that the Difference-in-Difference-in-Difference (DDD) estimator, in our context, consistently identifies the mean gain in program participation for different time periods. The idea for using DDD to estimate program impacts on different groups of borrowers are as follows. The microfinance in Bangladesh is targeted at households who are eligible (although not strictly enforced). There are both eligible and non-eligible households in program and control villages. So, we can estimate the impact of microfinance on those targeted (eligible participant) by using DDD: difference-in-difference (DD) estimates for eligible minus DD estimates for ineligibles.

An ideal evaluation would be all participants observed in the first round would remain in the program throughout, and no control household received the treatment. However, some treated households dropped-out from, and some control household participated into, the program. So, if we treat initial participants and comparison groups as treated and non-treated, program-control differential would likely to be underestimated. Because of the presence of participation data in subsequent periods, we are able to examine whether controls actually participated in the program, or whether participants dropped-out from the program. We are also able to identify the change in participation status between two points of observation since we have year-to-year information on participation data. Many households who were members of microfinance in 1997/1998 left the program, while some non-participants in that period joined later. So, we are able to identify new participants, and permanent "leavers" from the microfinance program, and can estimate the impacts considering different lengths of membership in microfinance. The data allow us, using our DDD approach, to distinguish impacts of short-term and long-term program participation.

We also estimate if the benefits received by the participants last long after leaving the program using our household survey of program drop-outs. We track participants up to eight years following their end in participation in microfinance program. We have the baseline information for the households who joined the program after the first round of the survey, and can evaluate impacts for them using pre-program level information. We also know households who continue their participation for at least eight years. So, we obtain the income estimates of the long-term participation in programs based on eight years of continuing participation in program by treatment group and a corresponding eight years non-participation in program by control group. Similarly, long-run impacts are identified by comparing households who dropped-out after the first round of survey with the matched households who did not participate at all. Estimates obtained for other leavers and newcomers are also interpreted accordingly, depending on their length of exposure to the programs.

We argue that the concern regarding the timing of participation by households, and staying or leaving programs are not important with our panel dataset. In particular, we combine matching and DDD approach is to substantiate our claim. Such a long continuing and post-program outcome measures is unprecedented in the microfinance program evaluation, possibly unique to a program evaluation in a developing country. Our approach to estimate different term program impacts appears to be a significant contribution to the literature, as it rests on observations rather than extrapolation.

2 The Survey and the Data:

The paper uses three waves of household panel data of treated and control groups of microcredit households from Bangladesh, covering the period 1997/1998 to 2004/05.[3] Unlike the first two waves of the survey that were conducted in 1997/98 and 1999/2000, the third round was undertaken at a five year interval in 2004/05. All surveys took place during December-March period. The survey encompasses 91 villages in 23 sub-districts of 13 out of total 64 districts in Bangladesh. The first survey was administered after a census of all households in the 91 villages during October 1997. The survey was conducted so as to get a representative sample of microfinance households which could reflect the overall microcredit operations in Bangladesh. The participating households were drawn from 13 different sizes of MFIs (so as to be representative of MFIs in Bangladesh), each from separate districts. All these MFI are member of PKSF.[4] These MFIs have similar types of program activities and provide loans like Grameen. Most of the clients in our sample are women, and credit is not offered to a mix group of men and women together. Villages that have a women group do not have a men group. Of the 13 selected MFIs, two were deliberately chosen from the large category. The survey was designed initially to have two control villages and six programme villages from each of the areas where microfinance was operating. However, since enough control villages could not be found in all areas, only a total of eleven control villages could be included in the first round. Subsequent round of survey reveal that some of the control villages turned into program villages, and in the final round of survey there were only eight control villages.[5] Because of the absence of required number of control villages, nonparticipants from the program villages were also surveyed based on the observable characteristics including considering the eligibility criterion set by the MFI. The household data set is a stratified, clustered,

[3] The data was collected by BIDS for Bangladesh Rural Employment Support Foundation with the help of financial assistance from World Bank. The first author was involved in the fourth round of data collection, monitoring and report writing.

[4] PKSF stands for Palli Karma-Sahayak Foundation (PKSF), means Rural Employment Support Foundation. 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.

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

random sample (see Islam 2007 for more details).

The first and second waves consisted of 3026 and 2939 households, respectively; the final one 2729 households (from the same number of villages). So, the attrition rate is low: less than about 3.3 percent on average in each round of survey, and about 1.2 percent per year.[6]. We study a balanced panel of 2694 households as some of the outcome variables are missing for 35 households. The survey has different modules for household socio-economic condition, microfinance participation, village level information, MFI level information.

The data is comprehensive and covers information on all major socio-economic conditions of households. There is detailed household demographic information, including: 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 importantly, records data on loan use, purpose of getting loan, amount borrowed, duration of the membership, amount of loan to be repaid, defaulting of loan and if so for what reasons. The village level information is quite rich and it goes back to the pre-program level different characteristics at the village level. The descriptive statistics of key demogrpahic and loan variables are given in Appendix Table A1.

Observation units have not remained stable during the time period of the panel. Many of the participants dropped out of the program after one or more years, and some of the control households were found to be participants in the subsequent waves. However, dropouts and newcomers were also interviewed during each survey. Split-up of original units also took place due to demographic transition. We find 116 households split-up during the second round of survey, while there are 184 split-up households in the third round of survey. The split-up is not a major issue, as there are very few migration outside the own village or area. The survey followed the split-up households who were also re-interviewed, and we merged the split-up households to form a single household.

The sample households can be classified into three broad categories:

1. regular (continuing) participants – participants in any MFI program in all four [3?] waves;

2. non-participants – did not participate in any MFI program (control group); and

3. occasional participants– found to be participants one/two round of survey but not in all three waves.

We divide the occasional participants into the following categories:

[6]There was another round of survey in between the first two rounds. The outcome measures for this round is not comprehensive as is in other three rounds [MEANING?]. However, we do have information on participation status, and in defining the different status of participation in program, we also take into account household's participation status in that round. We have year-to-year information about household participation status for other years when there was no survey.

(i) Long-term drop-outs (leavers1)– participated in the first round, dropped out by the second round and did not participated since then in any other program;

(ii) Medium-term drop-outs (leavers2)–participated in the first two waves, but dropped out later and did not participate since then

(iii) New participants (newcomers1)– did not participate in the first round, but participated in the next two waves

(iv) Most recent participants (newcomer2)– non-participant in the first two waves and turned into a participant in the third round

(v) Other- the residual category of the occasional participants.

We do not consider the last category as there are very few observation in each of the subcategory of "other" group (also they are not interesting for our parameter estimation). Of the 1592 participants surveyed in our panel, 47.2 percent are the regular participants, 11.3 percent or 180 are long-term drop-outs, 352 dropped out after second round of survey. On the other hand, there are 144 households who were in control group but participated after the first round of survey while 76 participated after the second round of survey. There are 723 households who never participated in any round. For our analysis, we never consider a household, either eligible or not, as control if a it ever (during or before the study period) receives microcredit from any MFI. Total credit borrowed by different participating group differs, largely depending on their length of participation. The average microcredit borrowed by participants in each round are taka 7427, 10616, and 11682 for each three round, respectively.

2.1 Attrition

Even though the attrition rate from the survey is significantly low compared to many other panel data set from developing countries, we examine whether there is any attrition bias. 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 socio-economic variables reveal that the attritors are not different from those of stayers. There are 184 attritors from non-participant households and 147 from participating households in all three waves. So, the attrition rate is higher among the non-participants, as expected. However, a look at the comparison of means of attritors by their participation status in the first round reveal no statistically significant difference between participants and non-participants (see Table 9). In results not reported here we reject the hypothesis of the equality of the two distributions for any demographic variables using Kolmogorov-Smirnov test. In the spirit of Fitzgerald, Gottschalk and Moffit (1998) we began with an explanation of the correlates of attrition in our survey. We estimate a probit model of overall attrition, and attrition by participating status in the first round using lag demographic variable for current round's attrition. A statistical test is performed to test the equality of the 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 nonresponse. The full set of attrition results are available from the authors upon request.[7] The evidence is that the selection bias that is due to attrition is not a problem in our case. Moreover, we employ estimation strategy which can resolve many of the potential bias (including attrition bias) that is due to unobservable.[8]

2.2 Outcomes of Interest

The key outcome of interest are household income and consumption [REFS?]. The data reveal incomes from a wide range of sources. We are more interested in self-employment incomes since microcredit programs are more likely to increase the self-employment activities. For example, borrowers also use credit to purchase capital to enhance self-employment productivity. We define self-employment income as the sum of proceeds from all of the household's self-employment activities minus operating expense (excluding the value of household's own labor). However, not all borrowers invest their money in self-employment activities, and microcredit organizations do not strictly enforce the activities, and many organizations provide loans for both farming and off-farm activity. Therefore, we also estimate the treatment effect 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). Moreover, there is substitutability between capital – households borrowing from MFI can transfer their own assets and savings to different other activities, and hence pave the way to invest in a multiple and diversified projects.

Even though we have detail information about different sources of income in different time periods and income earned by different household members, income may still produce "noisy" data. This concern is more relevant in a developing country setting. Poor households in Bangladesh spend more than 70 percent of their income on food purposes. Most people who receive microcredit are poor, and we should expect the impact of microcredit should show up in additional purchases or food consumption. We have information about 200 commodities consumed by a household for a given period prior to each round of survey. For each food item, households were asked about the amounts they had consumed

[7] Studies that use longitudinal data from both developed (see Journal of Human Resources 1998 spring issue) and developing country (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 mechanism working for attrition, the effects of attrition on parameter estimates are mild or non-existent.

[8] We also experiment with the most common approach of taking account of attrition bias into our regression estimation in the next section. 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.

out of purchases, out of own production and from other sources in the reference period. The reference period for the food item differ depending on the type of food consumed by rural households. Some foods (e.g., beef, chicken) are consumed occasionally, while others are more frequent (rice, lentil etc.). We aggregate all consumption and verify the value using the prices of the food item collected from local shopkeepers and groceries.

We also include non-food expenditure as an outcome of interest. Together with food expenditure, consumption expenditure provides an alternative, and possibly better, measure of household welfare considering the possibility of measurement error in income variable. The data for non-food consumption expenditure was also collected for different recall periods, for example, over the past one month, past three month, or past twelve months, depending on how frequently the items concerned are typically purchased. We use the price quoted by households since many items differ in terms of quality within a village (such as coarse rice, fine rice). The questionnaires include information on consumption of a wide range of non-food items. We construct non-food expenditure by converting all these reported amount to a uniform reference period of one year, and then aggregate across the various items. The non-food expenditure data includes items such as kerosene, batteries, soap, housing repairs, clothing, and excludes the item that are lumpy (e.g., dowry, wedding, costs of legal and court cases, etc.). We include both schooling and health expenditures. Finally, many households can save in the form of durable and nondurable assets, and also many households buy assets (such as livestock) using credit. So, we also measure the impact on household total non-land assets (also exclude the value of house). We deflate the outcome variables by rural household agricultural index which is set in 1997/1998=100. To exclude the effect of a few very large outliers, we exclude those households reporting unreasonably high or low value of the above outcome variables (although this did not significantly affect the results). Table A1 in Appendix reports the results of the outcome and loan variables for different years.

3 The Empirical Strategy

Evaluating the impact of microfinance requires comparing the outcomes when households participate in a microfinance to when they do not. The problem is that we do not observe a household in both states. Therefore, in principle, we would like to randomly assign the microfinance across households, and then compare the average outcome of the treatment and control groups. The major concerns for identification of impacts in our case, however, are that MFI choose to provide credit in particular villages, and households are self-selected into the program. Therefore, the decision to participtae in microfinance may not be orthogonal to unobservable factors that also affect the outcome of interest. It could be the case that poorer villages get priority for the microfinance operation. Or the decision to provide services came from strong demand from the local community people.

Similarly, since households choose to participate in microfinance, it is likely that the decision to participate in a program is driven by households' need for credit or perceived benefit from such credit. On the other hand, households willing to participate do not get credit automaticaly as they need to satisfy the eligibility criterion to get approval from local branch of a MFI. Though eligibility criteria are not strictly followed, it plays a central role in sanctioning loans by a branch manager of a MFI. Impact evaluation of microfinance therefore requires controls for selection bias. Fortunately, the availability of panel data allows us to address the selection issue [REFS?]. We adopt a variety of panel data approaches to estimate the treatment effect on the treated. So, identification of impact does not require restrictive assumptions that are required for the impact evaluation for a non-experimental cross-section sample data. Besides, we adopt an estimation methodology which further relaxes many of the identifying assumptions (e.g., unobservables are time invarying) that are typical in a panel data estimation.

First, we consider the difference-in-difference (DD) approach which ensures that any variable that remain constant over time (but are unobserved) that are correlated with the participation decision and the outcome variable will not bias the estimated effect. It can thus be interpreted as the causal effect of the program under the assumption that in the absence of program the growth in outcome would not have been systematically different between program and control groups. But this identification assumption should not be taken as granted. The pattern of increase in outcome could vary systematically across households and villages. The pre-treatment characteristics that are likely to be associated with the outcome variables over time may be unbalanced between program and control group (e.g., if a household joins in microfinance because of a transitory shocks, such as wedding of daughter, getting admission of school for son, etc.). The basic DD is most suitable when program participation is as good as random, conditional on time and group fixed effects. The restriction, implied by the model, may be too stringent if treated and control are unbalanced in covariates that are thought to be associated with the dynamics of the outcome variable. For example, in case of a job market training program, Ashenfelter (1978) finds that participants experience a decline in earnings prior to training (also known as "Ashenfelter dip"). This indicates that household decision to participate in microfinance may be affected by the pre-program characteristics. Ashenfelter and other studies suggest to use pre-program level income as the regressor, we include pre-program level household asset ownership to control for participation. We include regressors so that the differences in age, sex, household size, village attributes, etc. are controlled and the difference between treatment and control group over time reflect the true causal effect of program participation. The identifying assumption here is that conditional on household characteristics (X) and village fixed effects the biases are the same in different time periods so that differencing the differences between participants and non-participants eliminate the bias.

Formally (regression adjusted) the DD model can be specified as a two-way fixed effects 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 district l at period t (expressed in logarithm except the self-employment income). D_{it} is the participation variable that takes on (i) the amount of credit borrowed from the microfinance organization up until period t ; and (ii) the value of 1 if household is a participant in microfinance in period t and 0 if not participant. X_{it} is a vector of household-specific control variable, G_j is village fixed effects which eliminates the problem if programs were placed non-randomly, α_i is a fixed effects unique to household i and δ_t is a period effect common to all households in period t, γ_l is the microfinance institution-level fixed effects (this fixed effects eliminate unobserved charcteristics across different MFIs). Household-level fixed effects method also resolve any village or any upper (e.g., district) level endogeneity. The error term ε_{ijlt} is household's transitory shock that has mean zero for each period $t = 1, 2, 3$ and is assumed to be distributed independently of the treatment status D_{it}, but can be correlated with α_i. Including X help control for confounding trends and it also reduce variance of ε_{it}, which may reduce the standard error of the estimate β. While households in the treated group may still have lower or higher outcome measures than households in the untreated group, this sdifference is assumed to stem from differences in observed family background (X), or from unobserved family characteristics (α_i) that can be viewed as permanent and have time-invariant effects on outcome measures. The errors might be correlated across time and space. When treatment effects are constant within aggregate units, we must allow for errors to be correlated within the aggregate level. For example, recent analyses by Bertrand, Duflo and Mullainathan (2004) and Donald and Lang (2007) demonstrate that there can be pervasive serial correlation in household level difference models and thus may produce severely downward bias of estimates of standard error. We therefore need to adjust the standard errors for the correlated errors. So, we allow arbitrary covariance structure within village over time by computing our standard errors clustered at the village-year level. Donlad and Lang (2007) have pointed out that asymptotic justification of this estimator assumes a large number of aggregate units. Simulations in Bertrand et al (2004) show the cluster-correlated Huber-White estimator performs poorly when the number of cluster is small (<50), leading to over-rejection of null hypothesis of no effect. We have 91 cluster in our sample, and so we can potentially avoid the problem. In addition, we also report standard errors using block bootstrapping as also suggested by Bertrand et al (2004) when the number of groups is sufficiently large.[9]

[9] In block bootstrap sampling, any correlation across errors within the block will be kept intact, and

In the above model (1), with $T = 2$, β is the difference-in-difference estimate of the effect of the participation in microfinance on household's outcome. The model above assumes that the selection bias is due to (1) unobserved household specific component (α_i) that is fixed over time (2) observed differences between program and control group that are due to X, and in the absence of program participation $\beta = 0$. The identifying assumption in equation (1) is that the counterfactual outcomes in the absence of treatment are independent of treatment, conditional household effect α_i, and covariates X_{it}. Moreover, in order to strengthen our identification of treatment effect, we also report results based on the matched sample of treated and untreated groups. The details of the matching procedure are given in section ?

The fixed effects model in equation (1) is somewhat restrictive is because it assumes that the differences between treatment and control groups are permanent. In the following specification we assume that there is household-specific fixed effect and household-specific time trend. The model, known as random growth model (See Heckman and Hotz 1989, Ashenfelter and Card 1985)[10] takes fixed effects model a step further by allowing unobserved household/individual differences that change at a fixed rate over time during the analysis of period. We specify the model as:

$$Y_{ijlt} = \alpha_i + \lambda_i t + \beta D_{it} + \phi\gamma_l + \theta X_{it} + \gamma G_j + \varepsilon_{ijlt} \tag{2}$$

Since Y_{ijlt} is the natural log of a variable, λ_i is the average growth rate over a period (holding other covariates fixed). [ARE WE INTERESTED IN THE FIXED EFFECTS AT LL? ALSO, THEY'RE INCONSISTENT IN N] The model allows for household-specific outcome growth that is the same for all periods. We may eliminate the household fixed effects by differencing the dependent variable. With a simple modification, the first-differenced model can be written as [CHECK SUBSCRIPTS BELOW]:

$$\Delta Y_{it} = \lambda_i + \beta D_{it} + \theta X_{it} + \eta_{it} \tag{3}$$

This model eliminates the selection bias that results from household-specific fixed effect and the household-specific time trend. This modification allows the past loan to have effect on household's current consumption, income and assets [HOW?]. Since first differencing the right hand sider variable will mean losing more variables (if we estimate fixed effects on differencd variables we eliminate many of our variables of interest (linear time trend variable) that affect the growth in outcomes, we use level of the variable such as education, sex, marital status of household head, etc. Equation (3) is just the standard unobserved effects model and so we can apply fixed effects or first-differencing methods in order to estimate the program impact. In the actual estimation strategy, we also incorporate year

should therefore be reflected in the standard error of estimates.

[10]Recent applications of such model include Papke (1994) ; Friedberg (1998); and Michalopoulos, Bloom, and Hill (2004).

fixed effects with equation (3) to allow for more flexibility that can account any further macroeconomic changes. An advantage with models presented above is that they are robust with choice based sampling which characterizes most non-experimental data set, the data set used in this study.

3.1 Results: Fixed Effects and Random Growth Model

The fixed effect results using equation (1) are presented in Table 1. We produce two sets of results, with and without time effects using a full set of controls, for all samples and only matched samples (matching is done using propensity score). We use the amount of credit borrowed as the measure of participation in a microfinance program. The results indicate that participation in microfinance program can increase households' all measures of welfare except "other income". The impact estimate is the highest in case of self-employment income, and the lowest in case of food consumption if we consider column (4) as our preferred specification (both in terms of sample and regression model). Table 1 shows that households can increase food consumption by 1.9 percent, non-food consumption by 3 percent and asset by 4.85 percent. The point estimate is negative and insignificant for "other income"– only 0.77 percentage points. As household reported self-employment income are zero in many cases, we estimate a random a random effects Tobit model (since fixed effect tobit is biased and inconsistent - CAN STILL DO, SEE BILL GREENE) to estimate the effect of credit on self-employment income. The coefficent estimates indicate that household self-employment income increase by 14.7 taka when if a borrower gets additional 100 taka as microcredit. The overall results indicate that the statistical significance of our results are not affected by which of the standard error (block bootstrapping or clustering by village-year level) we consider.

The results using random growth model of equation (3) are given in Table 2. We find similar results except in case of "other income" in which case the treatment effect is now postive. Again if we consider column (4) as our preferred specification, we see that the estimated treatment effects are higher in case of non-food consumption and both income measures, while the coefficients are lower for food consumption and the same in case of assets. Column (4) indicates that there is now an increase of 4.3 percent of non-food consumption, 6.5 percent of income. Since the dependent variable in case of self-employment income is in level form rather than in logarithm, the coefficient estimates indicate that a 100 taka increase in borrowing will add 15.6 taka more to household income. The estimated treatment effect on the treated for food consumption and assets are 0.47 percent and 4.9 percent, respectively. Table 2 shows that the inclusion of the time fixed effects reduces the value of the coefficient for all cases except asset.

The results above might not produce unbiased estimates of the mean impact of the treatment on the treated (TT), which is our main parameter of interest. This is because some participants drop-out later and some control group members participated later. In our estimation above, we include them in treatment group though they are not treated fully. In the presence of partial treatment by some households in the sample, the estimator provides us 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 less than that obtained (i.e.,TT parameter) if the control group had not participated and/or treatment group had not dropped-out. 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.[11]

4 Alternative Evaluation Strategies

4.1 DDD Matching Estimate:

The fixed effect estimates corrects the selection bias if we have a baseline data. In our case, there is an absence of baseline data and the first cross-section data was collected when the program was already in operation. The longitudinal observation, however, helps correct bias in a generalized DD framework. A general fixed effect/DD approach in equation (1) should isolate the impact of program. In identifying the causal effect using fixed effect, our assumption is that selection-bias into the program is additively separable (conditional on observed covariates) from outcomes and time invariant. In the standard DD setup, with two time periods, period 1 precedes the intervention and so DD gives the average impact of program participation. However, in our case program is in operation in period 1, ao, the results presented above may still be bias.

So, how should we estimate the parameter β in equation (1)? A nonparametric matching stratefy can be used to achieve even better equivalency of the treatment and control groups. So, we consider estimating treatment effects using a more flexible technique – the propensity score matching (PSM) estimator.[12] The conventional cross-sectional PSM

[11] Heckman, Smith and Taber (1998) consider evaluation of the program using instrumental variable approach, even though it is not sure how IV estimator can recover the TT paramter (see Imbens and Angrist 1994). In the presence of three rounds of panel dataset, we do not need to consider IV estimate even we have the availabilty of instrument using eligibility criterion (see Pitt and Khandker (1998) and Islam (2007) for IV estimates in the context of evaluation of microfinance program in Bangladesh, though there are also controveries of using such instrument (see Morduch (1998)). We do not go into the debate of using IV here. In particular, we use variants of matching and difference-in-difference approaches below. See also Heckman, Hohmann, Smith, and Khoo(2000) for different parameter of interest when the drop-outs leave the program for better alternative. In our case, drop-outs remain non-participants during the survey period, and they do not join in any alternative credit program.

[12] PSM is a standard technique in the evaluation of treatment for non-experimental impact studies. It compares outcomes of program participations with those of matched nonparticipants, where matches are chosen on the basis of similarity in observed covariates. Resenbaum and Rubin (1983) show that matching on covariates is equivalent to matching on propensity scores. PSM involves matching each treated unit to

estimate is based on the assumption that, conditional on set of observed characteristics, X, the counterfactual outcome distribution of program households is the same as that of control households. So, it assumes that there is no selection bias based on unobservables. The simple DD approach, on the other hand, assumes the same cross-section bias before and after the program participation so that the average change in outcome is presumed to be the same for both non-participants and participants if they had not participated. In other words, the participants and non-participants would do the same in terms of our outcome measures if participants were selected but did not participate in the program. But if the household's decision to participate in a program is affected by certain characteristics that also influence the outcome of interests, then the DD estimator is senstitive to functional form assumption (Ravallion 2006). To address this concern we combine DD with propensity score matching.[13] PSM is assumed to eliminate the bias in the post-program period (Heckman and Smith 2004) and it does not require the functional form assumptions implicit in a regression analysis (see Dehejia and Wahba (2002) for more details). Heckman, Ichimura, and Todd (1997) show that DD matching help control for heterogeneity in initial conditions and also allows for unobserved determinant of participation as long as it can be represented by separable household- and/or time specific components of the error term.

In order to improve the quality of matching, we impose a tolerance level on the maximum propensity score distance. In particular, we use a variant of caliper matching as suggested by Dehejia and Wahba (2002), called radius matching. Rather than using just the nearest neighbor within the caliper, we use only as many comparison units as are available within the caliper. This matching estimator shares the uses of the attractive feature of over-sampling, automatically imposes the common support condition, but avoids the risk of bad matches. We set the caliper equal to 0.0005 [MEANING, REFS?]. So, households from the untreated group are chosen as matching partners for treated household that lies within this range of the propensity score. A weight was placed on it so that observations closer to the treated group were given heavier weight. A number of weighting

the control unit on the one-dimensional metric of the propensity score vector. It works by re-weighting the control-group sample so as to provide a valid estimate of the counterfactual of interest. That is, it aims to construct the correct sample counterpart for missing information on the treated outcomes had they not received program benefits. PSM essentially assumes away the problem of endogenous placement, leaving only the need to balance the conditional probability i.e., the propensity scores.

[13]Ravillion and Chen (2005) use first PSM to clean out the initial heterogeneity between targeted village and comparison villages, before applying DD using longitudinal observations for both sets of villages. Because of the inappropriateness of the baseline data, they also use post-program initial cross-section data to estimate the propensity score. In order to avoid the potential contamination of the observed covariates due to program participation we emphasise on the following variables: (i) variables observed prior to the participation decision (under the assumption that the density of these variables does not change due to anticipation of program) (ii) variables that we expect to be stable over the time period of observation (such as household head education, spouse education, household characteristics, family structure) and (iii) variables that are deterministic with respect to time (such as age).

methods have appeared in the literature. The literature suggests that weighting functions should be chosen to ensure consistency in estimation and identifies a number of appropriate weighting functions. The estimator chosen here is 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.

We include all the variables that may affect both program participation and outcomes (see appendix for variables used in estimating propensity score) and estimate a standard logit model of participation for estimation of propensity score using the first cross-section observation in 1997/98. We match households in the subsequent round based on the same propensity score.[14]

The DD matching or fixed effect estimates will still be biased if some of the differences between treatment and control groups are temporary, owing to transitory shocks.[15] So, we estimate the treatment effect using difference-in-difference-in-difference (DDD) strategy.[16] The microfinance in Bangladesh is typically offered to households who are eligible [17] in program village[18]. Because we have non-participants from both program and control villages, we can use a method that involves using DD estimate for eligible and DD estimate for ineligible households. In essence, this entails DDD: DD estimates for eligible minus DD estimate for ineligible. Such DDD estimator would allow us to compare effect of microfinance participation on eligible households (in program village) relative to eligible households from non-program village but also provides a cleaner way to "separating out"

[14]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 empirical distribution of the estimated odds-ratio of participants and non-participants show that there are very few regions of non-overlapping support. We restirct our analysis to the samples for which we find common support.

[15]The estimates will, however, be unbiased if the potential outcomes without the treatment is independent of the treatment, and hence can be interpreted as the average treatment effect on the treated.

[16]See Gruber (1994), Hamermesh and Trejo (2000) and Ravallion et al. (2005) for application of DDD estimators.

[17]Most of the microfinance institutions set the official eligibility rule as households having less than 50 decimal of land in order to target the poorer households. However, a large number of non-eligible households also participated in the program. In practice, MFIs are flexible for those holding a marginally more land (on grounds such as land quality and price are not the same in every region, lack of perfect information about the borrowers' ownership of land, etc.). Also over time, many MFIs relax the rule to expand their covergae in a growing competitive environment of microlending organization. Also during our survey we asked households about eligibility criterion, and many households reported that they are eligible if hold less than one-acre of land. However, when we cross-checked with their lending MFI, we find, in many cases, that the offical eligibility criterion is set as households owning less than half-acre of land. So, we set the eligibility criterion as households having less than 100 decimal (one acre) land. According to our criterion, about 83 percent of the participants in 1997/98 are eligible.

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

some of the bias from the differential growth effects that arise due to gaps in initial characteristics (if there are other program in different villages with similar objectives triple differencing strategy would take that into account). So, our approach is to compare the observed outcome changes between those who are eligible participant, those non-participant, with these two groups matched by propensity scores. Since there may also be economy wide changes that have nothing to do with program and may have different implications for eligibles in the absence of program, we also track outcome changes of ineligible non-participants between program and control villages. So, our DDD estimate is given by: (1) DD1=outcome of eligible participant in program village - outcome of eligible (non-participant) from the control village; (2) DD2=outcome of ineligible non-participant from the program village - outcome of ineligible (non-participant) from the control village; (3) take the difference: (1)-(2) which is DDD estimate of the effect of microfinance.

4.1.1 Long–and Short- Run Impact:

There are some important considerations in the evaluation of a microfinance program, such as, whether or not changes generated by program on its participants persist over time, or last beyond the period when the participants drop-out. We investigate the last question by comparing the effect of being participant in the first round but dropped-out later versus those who did not participate at all (with both groups matched by the propensity score).[19] Since, we are able to track leavers for about 8 years post-program, we match them with never participants. The objective here is to estimate the impact that might have happened after the end in participation in a microfinance program. This should help us project treatment effect into future. Impacts occurring in subsequent years will add to the accumulated eight-year impact amounts (impact estimates for regular participants). Predicting these future effects can help us complete our assessment of overall program impact. We call the resulting estimates as long-run effect.[20] Similarly, we observe households who joined the program in 1997/98 and left the program in 1999/00 and did not joined since then. Impact estimates obtained for this two groups are termed

[19] It could be the case that those who stay in the program benefit most while those who are worse-off dropped-out. On the other hand, it is also possible that most successful entrepreneurs graduate from the program. So, the estimated impact could either be overestimated or underestimated if we exclude leavers from our analysis. Alexander and Karlan (2006) also raised this issue, and argue that the cross-sectional impact estimates will be biased if we exclude drop-outs from the treatment group. Similar question can be raised for newcomers regarding their timing of participation. We track both drop-outs and newcomers in our survey. We consider here leavers as a separate treated group as is newcomers. The treatment effect in each group is based on a comparison of means with never participants who could be matched based on the propensity score. We apply DDD methodology here as well to mitigate concern based on selection on unobservable.

[20] For a discussion on long-term and short-term effects in the context of labour market program evaluation see Friedlander and Burtless (1995) and Hotz, Imbens, and Klerman (2006).

as "medium-run participation effect" since they turned into non-participants for about 5 years.

Because of the heterogeneity in the participation status, we estimate different paramter of interest. For example, in estimating program impact, it is helpful to distinguish between households for whom the participation in microfinance or the credit receipt are of short or intermediate duration, and a large number of households who continue to benefit from microfinance rolls for several years into the future. The distinction between short/intermediate-term and long-term microfinance recipients allows us to distinguish the impacts due to long-term participation and other terms (short-and medium- termed). A program may just attempt to enhance the short-term benefits of its borrowers, and not focus on long-term benefits, to gain the popularity and coverage of the program elsewhere very quickly. Therefore, short-term program evaluation is likely to undermine the gains that accrue if program continues to provide credit over a long-term. So, another most important consideration is whether households who participate for longer period continue to gain or benefit more compared with those participanting for shorter periods.[21] We estimate the long-term participation impacts by comparing households who continuing participants for at least eight years to those who continue to become non-participants during the same period.

Many households who were not members of microfinance in 1997/1998 joined in the program later. So, we can estimate the effects of program for new participants under the identifying assumption that those who joined later are systematically no different, conditional on observables and time invariant characteristics, than who joined earlier. Moreover, we have the baseline information (period 1997/98) for this group (call newcomers1) since they stayed as non-participant in at least one previous round of survey. We can also identify the impacts for another group: households who joined a year or two for the first time before the last round of survey and continued their participation till the survey. We call them newcomers2, and estimate the treatment effect for this group as newcomers1. In order to strengthen our identifying assumption, we use matching estimator. We follow the standard matching protocol outlined above. The control group in each case is never participants who were either denied the program because they are not eligible or who do not have access to any program or who did not participate despite being eligible. The first two groups do not produce any contamination bias in our estimates since they cannot get the treatment. However, the presennce of last group, eligible non-participants in program village, means that there is potential selection bias

[21] 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 control group would have yielded still a larger impacts, too. When we observe large impacts at the end of eight year follow-up, we can be fairly confident that extending the program to 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 eight-year embargo would have increased treatment effects towards the end of follow-up period.

since they were eligible to participate and also had access to program, but choose not to participate. Because of such concern wealso exclude them them in our estimate below. The DDD estimates obtained using households of different lengths of membership in microfinance are termed as short-term participation effect or long-term participation effects. Impact estimates obtained using 1-2 years (newcomers2) participation are termed as "short-term participation effects", while those participating 5 years (newcomers1) are termed as "medium-term participation effects".

There are two important assumptions we made above. First, it is assumed that there are no households who change participation status twice between any two periods. Secondly, there is no dynamic sorting of households with high (low) potential outcomes participate early in the program. If there is a dynamic sorting, then the duration of the participation in microfinance or decisions of when to participate microfinance program is likely to depend on the unobserved potential outcomes perceived by households and not by us (researchers). Given that we have year-to-year information for households participation status, the first assumption can reliably be checked. The number of households who change participation status twice is very low, and we do not consider them in our estimation in this section. We argue that any reamining potential bias is adequtely controlled by using DDD matching estimator.

4.1.2 Results: DD and DDD Estimates

Table 3 illustrates DDD estimation of the effect of microfinance participation on consumption, income and asset for regular participants using data from first and last round of survey.[22] The left side compares the changes in outcomes for eligible participating households in the program village to the change for eligible households in villages that do not have any program. Each cell in the first two columns contains mean average outcome variables for the group labeled in the respective column. The third column represents the difference between two groups at a point in time, and standard error of the difference is represented in the fourth column. In the middle, panel B shows the time difference in outcome variables for each eligible group. It indicates that there was an increase in food consumption of taka 736 in program group, compared to a taka 601 increase in control group. Thus there was a taka 134 relative increase in food consumption of eligible participating households in program village; this is the DD estimate of the impact of continuing participation in microfinance. This figure represents a relative increase in consumption of 6.6 percent by the long-term participants. The similar calculations for non-food consumption expenditure indicates an increase in 12.4 percent for participating households compared to control households. There is almost no increase in other income.

[22] In the following we focus on these two rounds, as there was a flood at the end of 1998 in Bangladesh, and many of the outcome variables are likely to be affected due to post-flood rehabilitation program, and damage due to floods.

The DD estimates of self-employment income, and asset for the regular participants are even higher, representing 28.1, and 14.8 percentage points increase, respectively.

However, if there was a distinct shock to the program villages over this period, this estimate does not identify the impact of microfinance. We examine this in the right hand side of the Table 3, where we perform the same exercise for the ineligible group in both program and control villages. We find a slight fall in food consumption and large fall in assets, little increase in non-food consumption, but a significant increase in income and self-employment income among ineligible households in program village compared to control village.

Taking the difference between the two sides of Table 3, there is a 7 percent and 11.7 percent gain in food and non-food consumption by regular participants. The increase in self-employment income reduces to 15.1 percent, but other income increases to 6.5 percentage in DDD estimates. There is a sharp increase in assets in DDD estimate, from 14.8 percent in DD estimate to 41.3 percent in DDD estimate, because of sharp fall in asset holding in comparison group of ineligible households. So, if DDD strategy is taken to be more suitable than DD strategy in separating out the impact of the participation in microfinance program (as we argued in our methodology), program impacts are overstated for long-term participants in case of self-employment income and non-food consumption but understated in case of income, food consumption and assets using DD method.

Table 4 gives the DDD estimates for leavers– the left side shows the results of long-term leavers (leavers1) while the right side reports the results for medium-term leavers (leavers2). Here the leavers are compared with never participants. The implicit assumption here is that the treatment group drop-outs have the same mean outcome as their counterparts in the control group who would have been drop-outs if they had been in the treatment group, and that any potential differences between them would have been controlled by our DDD estimate. The results indicate that DDD estimates for food consumption is negative for both types of leavers. The impact estimates are positive for all other outcome measures. However, when we compare between these two groups, we observe the resulting positive effects are lower for long-term drop-out (except non-food consumption) implying that the size of the effects, beyond the years during which households were participants, is falling.

The DDD impact estimates for households who participated after the first round of survey (newcomers1) and second round of survey (newcomers2) are shown in left and right side, respectively, of the Table 5. The results indicate that recent newcomers enjoy a substantial increase in food consumption, while their older counterpart experience a fall in food consumption. However, recent participants (newcomers2) experience more decline in non-food consumption. Combining food and non-food expenditure, most recent participants are able to enhance consumption expenditure more than their older counterpart. They also gain more self-employment income and other income. It is not clear why we obtain such results. But, it could be the case that recent participants are able to smooth consumption consumption using credit through smoothing income. Or, it could due to be smaller sample size of the recent participants. The DDD estimates for self-employment income is larger for recent participants, but they observe a large decline in assets.

We also estimate treatment effects using an analogue of our DDD approach proposed by Ravallion et al. (2005) . In that case, we compare changes in outcomes of regular participants and matched "leavers", after netting out the outcome changes for a matched comparison group who never participated. The estimation method requires the following step: (1) calculate the first difference between regular participant and matched non-participant; (2) calculate the first difference between "leavers" from the program and matched non-participant ; (3) difference of each of (1) and (2) at two points of observation; and (4) difference of the differences in (3). So, to obtain this DDD results, we need to subtract DD estimates of regular participants group in Table 3 from DD estimate of leavers1 from Table 4. The resulting estimates are positive in all cases, and are larger in case of food consumption and smaller for all other outcome variables when compared to DDD estimates obtained for regular participants in Table 3 which is reproduced in the last column of Table 6. Since drop-outs from microfinance are expected to receive partial treatment (e.g., continuing return from old investment project, training received from MFI) which increases their consumption, income, and other income, this DDD estimates are likely to be understated. However, it also gives estimates of what drop-outs could have gained had they not left the program. [RESULTS NEED TO BE ELABORATED]

4.1.3 DDD in Regression Framework:

Consider, first, a DD estimate of being assigned to microfinance program, known as the intention-to-treat (ITT) effect. 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. We estimate the following equation:

$$Y_{it} = \theta_{0i} + \theta_1 X_{it} + \theta_2 \delta_t + \theta_3 villeli + \theta_4 (villeli \times \delta_t) + \varepsilon_{it} \quad (4)$$

where Y_{it} is the logarithm of outcome variables (except the self-emplpyment income), $villeli$ is a dummy variable (1 if eligible and staying in program village, 0 otherwise), δ_t is fixed year effect which controls for macroeconomic changes. θ_4 represents the DD estimate of impact of program of being eligible. The last column of Table 7 reports the ITT effects using data from first and third round of survey. The ITT results show that household food consumption and non-food consumption are increased by 15 and 6 percentages, respectively. It also shows that self-employment income and asset increases but other income decreases. This estimator, however, estimate the effects of eligibility rather than participation. So, we focus on eligible participants below.

We run the following regression:

$$Y_{it} = \alpha_i + \theta X_{it} + \beta_1 \delta_t + \beta_2 villeli + \beta_3 (villeli \times \delta_t) \times + \beta_4 D_i + \beta_5 (D_i \times villeli) + \beta_6 (D_i \times \delta_t) + \beta_7 (D_i \times villeli \times \tag{5}$$

where D_i is the treatment dummy variable, δ_t is fixed year effect and controls for the macroeconomic changes in outcome. Because treatment and control groups differ in terms of the demographic and socio-economic characteristics, the observed differences in participation outcomes may reflect underlying differences between the treatment and control groups rather than a treatment effect. So we also use control variables to account for underlying differences between the two groups. In the equation (5), β_3 controls for changes that happened for eligible households over time in the program village versus non-eligible households, β_5 captures the differences between eligible participant relative to others, β_6 captures the changes over time of the treatment group. The third level of interaction coefficient β_7 captures all variation in outcomes specific to the treatment group (relative to controls) in the program village (relative to control village) in year 2004/05 (relative to year 1997/98). This is the DDD estimate of the effect of microfinance program on participants in the program village.

4.1.4 Regression-adjusted Results

We provide results for both matched sample (columns 3 & 4) and full sample (columns 1 & 2) using equation (5) in Table 7. The estimated treatment effects for regular or continuing participants are shown in the top panel. The DDD results indicate that households can increase food consumption, income, self-employment income and assets, but not non-food consumption. The results hold whether we consider only matched sample or full sample of regular participants and non-participants. In the panel B, we report results based on all samples. It was computationally cumbersome to estimate separate regression for each type of participants (leavers, newcomers) in this regression framework, so we include all categories of participants and compare the results with regular participants. The results overall support the same conclusion as that of regular participants except in case of assets using only matched sample. The results, however, indicate that the coefficient estimates are larger when we consider only the regular participants. This means that the impact estimates are higher for regular participants than other participants, broadly supporting our results in section ? that the estimated effect of microfinance participation is higher for continuing participation.

Our results are also robust if there is any selection bias in terms of composition of leavers, newcomers and regular participants since we are differencing out any differences that could exist in terms of outcome variables. In a cross-section data, the difference among groups are important and may invalidate the results. Moreover, using our first round of survey data, we find that there are no significant differences among these groups in terms of both outcome variables and demographic and other socio-economic variables. We do not provide results here since the selection bias in terms of timing of participation or leaving the program are not main concern as we are using panel data and are able to track all these households. The detail results on checking this type of selection bias are available upon request from the authors.

5 Conclusion

This research utilizes new data to track the effect of different term participation in microfinance, and the lasting impacts of program participation for those who dropped-out from the program. Using the longest and largest ever panel data of microfinance households, we have been able to investigate the longer, as well as shorter, run impacts of microfinance programs.

Based on our analysis of the evaluation from the full set of data using the regression based fixed effects and random growth model estimation, we find that impact estimates are positive for different outcome measures (except income in some cases). Our use of regression-adjustment DDD methods also broadly confirms the findings based on matching approach. However, matching approach allows us to estimate the heterogenous treatment effects for different participation depending on their length of participation in microfinance. It has the advantages that it does not rely on functional form assumption that regression-based approach requires. However, matching produces a larger standard error than the regression estimates, hence matching estimates are less efficient. We focus on magnitude of the coefficient rather than its statistical significance (see McCloskey and Ziliak (1996) and Hamermesh and Trejo (2000) for the rationales of looking at economic significance instead of its statistical significance).

Some conclusions about medium term and long-term impacts are possible from our findings. Although we are uncertain about the precise magnitude of the ultimate long-term impacts, the gains beyond the participation period lasted in all cases except in case of consumption expenditure. The regular participants gain in all outcome measures, and the treated-untreated differentials are larger imply that long-term participation in microfinance can in fact help households more than not participating at all. The impact estimates are larger than those who participated newly–implying that larger benefits can accrue in the long-term participation. The increase in size of treated-untreated differentials are decaying if we compare long-term drop-outs and recent drop-outs. Since we do not have year-to-year information for outcome variable, we do not know exactly when the benefits of the program for the leaver started to decline or halted. Despite the findings that the impact estimates are lower including drop-outs in the treatment groups, we cannot tell precisely that drop-outs leave the program because they are unsuccessful (as they no longer borrow from a MFI). Rather the differential results could be attributed to the additional gains due to longer time participation or more credit borrowed from MFI. Our results, however, indicate that benefits may not last indefinitely following withdrawals from the program. The overall impact estimates are positive for leavers, so in calculating the total impact of the participation, we should also add the impact of leavers with the impact estimates of regular participants. The estimated impact is underestimated if we exclude the leavers since total impact of the program is equal to benefits to continuing participants plus drop-outs.

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Appendix:

Variables used in the estimation of propensity score:

Household Level variables:

Age of household head (age is divided into different groups), 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, number of daughter, son, 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: Impact Estimates using Fixed Effects

	All Sample (N=2694)		Matched Sample (N=1874)	
Outcome of Interest	‘(1)	‘(2)	‘(3)	‘(4)
Food Consumption Expenditure	0.0349	0.0168	0.0379	0.0184
	‘(0.0089)*	‘(0.0069)**	‘(0.0109)*	‘(0.0094)+
	‘[0.0104]*	‘[0.0076]**	‘[0.0127]*	‘[0.0100]+
Non-food consumption expenditure	0.1172	0.1172	0.1184	0.0304
	‘(0.0121)*	‘(0.0121)*	‘(0.0129)*	‘(0.0082)*
	‘[0.0131]*	‘[0.0092]*	‘[0.0121]*	‘[0.0111]*
Income ‘(excluding self-employment income)	0.0059	-0.0094	0.0082	-0.0077
	‘(0.0068)	‘(0.0064)	‘(0.0092)	‘(0.0087)
	‘[0.0084]	‘[0.0085]	‘[0.0106]	‘[0.0106]
Self- Employment Income1	0.0983	0.0977	0.1389	0.147
	‘(0.0232)*	‘(0.0239)*	‘(0.0299)*	‘(0.0291)*
Asset	-0.0169	0.0462	-0.0171	0.0485
	‘(0.0134)	‘(0.0084)*	‘(0.0137)	‘(0.0109)*
	[0.0167]	‘[0.0132]*	‘[0.0169]	‘[0.0151]*
Household Fixed effects	Yes	yes	Yes	yes
Time Effects	No	yes	No	yes

Notes:

Standard errors in parenthesis. + significant at 10%; ** significant at 5%; * significant at 1%.

The regressions include household demogrpahic and socio-economic variables as controls. Standard errors presented in parenthesis are corrected for clustering at the village and year level, while those in brackets are corrected using block bootstrapping

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. The matching protocol is given in section ?

1 Estimated coefficent is based on random effect tobit model since fixed effects tobit are biased and inconsistent

Table 2: Impact Estimates using Random Growth Models

	All Sample (N=2691)		Matched Sample (N=1872)	
Outcome of Interest	‘(1)	‘(2)	‘(3)	‘(4)
Changes in				
Food Consumption Expenditure	0.0494	0.0129	0.0422	0.0047
	‘(0.0241)**	‘(0.0202)	‘(0.0289)	‘(0.0263)
Non-food consumption expenditure	0.2132	0.0458	0.2152	0.0434
	‘(0.0199)*	‘(0.0177)**	‘(0.0197)*	‘(0.0191)**
Income ‘(excluding self-employment income)	0.0857	0.0504	0.1035	0.065
	‘(0.0223)*	‘(0.0194)*	‘(0.0251)*	‘(0.0234)*
Self- Employment Income	0.0709	0.0324	0.1917	0.1561
	‘(0.1090)	‘(0.1150)	‘(0.1588)	‘(0.1718)
Asset	-0.09762	0.03884	-0.08662	0.0486
	‘(0.02632)*	‘(0.01421)*	‘(0.02189)*	‘(0.0159)*
Household Fixed effects	Yes	yes	Yes	yes
Time Effects	No	yes	No	yes

Notes:

Standard errors in parenthesis. + significant at 10%; ** significant at 5%; * significant at 1%.

The regressions include household demogrpahic and socio-economic variables as controls. Standard errors presented in parenthesis are corrected for clustering at the village and year level, while those in brackets are corrected using block bootstrapping

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. The matching protocol is given in section ?

Notes:

1participants in 1st round but not in 2nd or 3rd round,

2participants in 2nd and 3rd round but not in 1st round)

The sample size change slightly depending on the number of match available in each case. It indicates the number of matched sample only.

Double difference (treatment group) is obtained by subtracting column (2) from column (1). Double difference (treatment group) is similarly obtained using ineligible households in program and control villages.

Matching without replacement, caliper <.0005 (.005 for ineligible group). Observations with too high or too low values are omitted in the final estimation.

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

	Eligible Group (N=574)				Ineligible Group (n=149)			
	Treated in Program Village	Untreated in Control Village	Location Difference	S.E of Location Difference	Control Program Village	Untreated in Control Village	Location Difference	S.E of Location Difference
1997/98								
Food Consumption	2047.6	1982.0	65.6	97.0	2632.8	2578.6	54.3	200.9
Non-food Cons.	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-empl. 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 Cons.	638.2	711.2	-73.1	76.8	1454.1	1129.9	324.3	273.8
Income	3286.6979	3674.759	932.5	419.7	7095.0	6374.2	720.8	814.0
self-empl. 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								
Food Consumption	736.0	601.7			703.2	712.0		
Non-food Cons.	159.9	100.5			206.0	202.5		
Other Income	554.9	552.1			3385.8	3561.7		
self-empl. income	50.7	-205.6			-36.0	-154.5		
Asset	202.8	-1937.0			3653.7	7478.1		
Double Difference		s.e	%Gain			s.e		
Food Consumption	134.3	167.1	6.6		-8.8	347.4		
Non-food Cons.	59.4	103.4	12.4		3.5	365.2		
Other Income	2.8	465.6	0.1		-175.9	908.5		
self-empl. income	256.3	218.4	28.1		118.5	320.4		
Asset	2139.9	3073.1	14.8		-3824.4	8435.5		
Triple Difference		s.e	%Gain					
Food Consumption	143.2	385.5	7.0					
Non-food Cons.	55.9	379.5	11.7					
other Income	178.7	1020.9	6.5					
self-empl. income	137.8	387.8	15.1					
Asset	5964.3	8977.9	41.3					

Table 4: DDD Estimates of the impact of Microfinance on "Leavers"

	Long-run effects of participation (leavers1)(N=160)1				Medium-run effects of participation (leavers2)(N=365)2			
	Single difference		DD	DDD	Single difference		DD	DDD
	1997/98	2004/05	Treated		1997/98	2004/05	Treated	
Food Cons.	124.09	-90.11	-214.21	-205.37	101.65	-71.97	-173.62	-164.78
	'(135.80)	'(197.06)	'(239.32)	'(421.85)	'(99.87)	'(139.25)	'(171.36)	'(387.36)
Non-food Cons	10.53	62.93	52.4	48.88	-40.37	11.02	51.39	47.87
	'(121.72)	'(144.68)	'(189.07)	'(411.20)	'(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)	'(1088.58)	'(217.11)	'(341.49)	'(404.67)	'(994.57)
self-empl. inc.	-158.23	38.94	197.18	78.7	-23.61	115.96	139.57	21.08
	'(152.77)	'(275.79)	'(315.27)	'(449.54)	'(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)	'(9572.05)	'(2664.35)	'(2371.47)	'(3566.88)	'(9158.62)

Table 5: DDD Estimates of the impact of Microfinance on "Newcomers"

	Medium-term participation Impact (Newcomers1)1				Short-term participation efffects (newcomers2)2			
	Single difference (N=228))		DD	DDD	Single difference (N=110)		DD	DDD
	1997/98	2004/05	Treated		1997/98	2004/05	Treated	
Food Cons.	-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 Cons.	-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. inc.	-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:

1participants in 2nd and 3rd and but not in 1st

2participants in only 3rd and but not in 1st or 2nd round

Double difference (treatment group) is obtained by subtracting column (2) from column (1). Double difference (treatment group) is similarly obtained using ineligible households in program and control villages.

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. It indicates the number of matched sample only.

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

	DD regular	DD leaver	DDD (Ravallion)	DDD
Food Cons.	134.32	-214.21	348.52	143.16
	‘(167.09)	‘(239.32)	‘(386.51)	‘(385.49)
Non-food Cons.	59.4	52.4	7	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-empl. 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 levelled as DDD is taken from previous estimates to compare results with column (3)

Table 7: Regression adjusted DDD estimates of the Impact of Participation in Microfinance

Continuing Participants	Unmatched Sample		Matched Sample (Caliper<.0005)		ITT
and never participants	Without Control	Including Control	Without Control	Including Control	
	N=1430		N=1005		
Food Cons	0.0985	0.0888	0.0805	0.0666	
	‘(0.0648)	‘(0.0636)	‘(0.0768)	‘(0.0752)	
Non-food Cons	-0.1546	-0.1513	-0.0205	-0.0228	
	‘(0.0935)	‘(0.0929)	‘(0.1064)	‘(0.1038)	
Other Income	-0.0031	0.0306	-0.0091	0.0316	
	‘(0.0948)	‘(0.0961)	‘(0.1138)	‘(0.1043)	
self-empl income/1000 taka	7.08	7.50	6.44	6.89	
	‘(2.439)*	‘(2.462)*	‘(2.792)**	‘(2.738)**	
Asset	0.1878	0.2538	0.119	0.1401	
	‘(0.2395)	‘(0.2316)	‘(0.2407)	‘(0.2308)	
All Samples	N=2622		N=1827		N=2670
Food Consumption	0.043	0.0604	0.066	0.0688	0.1502
	‘(0.0559)	‘(0.0554)	‘(0.0671)	‘(0.0666)	‘(0.0265)*
Non-food Consumption	-0.2539	-0.2514	-0.1221	-0.1266	0.0619
	‘(0.0933)*	‘(0.0940)*	‘(0.0894)	‘(0.0903)	‘(0.0410)
Other Income	0.0662	0.057	0.0486	0.0313	-0.0979
	‘(0.0836)	‘(0.0831)	‘(0.0945)	‘(0.0888)	‘(0.0336)*
self-empl income/1000 taka	5.65	6.46	4.84	5.65	3.53
	‘(2.222)**	‘(2.250)*	‘(2.603)+	‘(2.594)**	‘(0.892)*
Asset	0.0729	0.1302	0.0106	0.0527	0.1044
	‘(0.1974)	‘(0.1905)	‘(0.1982)	‘(0.1880)	‘(0.0662)

Notes:

Regression only includes observations using1997/98 and 2004/05 data, while taking into account of participation status in the survey round 1999/00

The regressions include household demogrpahic 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. A household is chosen in the matched sample if its propensity score lies within the probability distance of 0.0005

Table 8: Descriptive statistics

	1997-1998		1999-2000		2004-05	
Demographic Variables	Mean	Std. Deviation	Mean	Std. Deviation	Mean	Std. Deviation
Age of the House Head	44.52	13.36	46.81	13.34	47.75	12.20
Number of working people	2.81	1.38	3.02	1.53	3.59	2.12
Household size	5.63	2.29	6.06	2.48	7.23	3.85
Max edu by any member	5.48	4.13	6.23	4.07	7.27	6.53
Area of arable land	68.47	146.66	80.79	159.03	73.68	225.92
Number of children	2.83	1.66	2.22	1.46	3.01	2.39
Number of women	2.66	1.40	2.94	1.52	3.26	2.00
Number of old people	0.25	0.49	0.39	0.60	0.31	0.54
Number of married people	2.38	1.10	2.70	1.37	3.16	1.98
If women is the head	0.05	0.23	0.05	0.23	0.11	0.31
Outcome Variable (in taka)						
Food Cons. (monthly)	2432.8	1832.2	2949.5	2721.1	3214.4	3296.1
Non-Food cons (Monthly)	762.1	2466.8	525.9	1767.1	1333.2	3136.6
Non-land total Asset	19084.3	34992.2	22557.2	20796.8	21957.9	51826.9
Income (Monthly)	2747.9	2797.7	2977.8	4233.7	3771.0	4209.6
Self-empl inc.(Monthly)	500.8	8671.6	448.1	2403.5	565.7	5332.3
Number of observations	2694		2694		2694	
Amount of loan from MFI	7427.3	7165.0	10616.8	11332.4	11682.5	17378.7
Number of borrowers	1592		1532		1280	

Table 9: 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	0.30
Number of working people in the household age 18-60	2.52	2.54	-0.02	0.90
Household size	5.28	5.15	0.13	0.62
Highest Grade/class passed by any family member	5.05	5.55	-0.50	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	-0.90	0.41
Sample Size	184	147		