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

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The objective of this paper is to estimate the impact of medium- and long-term participation in microcredit programs. It utilizes 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 difference-in-difference-in-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 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.

Key words: microcredit, Bangladesh, difference-in-difference-in-difference, propensity score matching, medium-term.

JEL codes: C21, G21, O12, Q00.

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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% 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 based mostly 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 used mainly for self-employment activities, are likely to vary with the length of participation. For example, Banerjee et al. (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 these authors, 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 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.

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

[2] The United Nations declared 2005 as the International Year of Microcredit. Prof. Yunus and the Grameen Bank won the Nobel Prize in 2006 for pioneering microcredit. World Bank (WB) Groups have a substantial investment in microfinance. According to the WB website it will raise investment in microfinance to $1.2 billion by 2010.

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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 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 years worth of monitoring and follow-up 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 ninety-one villages from 1997 to 2005. The availability of panel data enables us to address concerns regarding selection bias that are common in nonexperimental program evaluation.

The findings from the existing impact evaluation studies indicate that the effects of microcredit on income, consumption, and assets vary from place to place and depend on the particular settings and design of the program. Pitt and Khandker (1998), using an instrumental variable method, find that microcredit significantly increases consumption expenditure, reduces poverty, and increases nonland 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 outcome measures. Khandker (2005) finds more muted results than Pitt and Khandker (1998). Islam (2008) finds microcredit helps to increase consumption for only the relatively poor. In Thailand, Coleman (1999) finds that average program impact on assets, savings, and 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, and 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 six to twelve 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 groups of borrowers. Banerjee et al. (2009) report results of a randomized evaluation fifteen to eighteen months after the introduction of the program in the slums of Hyderabad in India. They find a significant positive impact on new business start-ups, profitability of existing businesses, 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).

Our data indicate that some microcredit members (treatment group) dropped out from the program, and some nontreated (control) households joined. 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 that continued their participation for at least eight years (*stayers)*. Because entry into the program and the timing of the participation 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 that departed the program. We track these drop-outs for up to eight years following their exit from the program. Using these households (*leavers*), we examine whether 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

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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 subsample of *leavers*.[3] We look at the impact on changes in self employment income, other income, food and nonfood 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 programs. Our approach to estimate the impacts of different lengths of program participation represents a significant contribution to the literature, as it rests on observation rather than extrapolation.

The Programs and the Data

The Context

Microcredit is small credit available to poor people who cannot enter the formal credit market. It requires no collateral and focuses on women. Loans are provided through informal groups mobilized as part of program strategies to reach the poor. The group-based credit program means that contracts effectively make borrowers cosigners to each other's loans, thus providing incentives for peer monitoring and mitigating problems created by informational asymmetries between lenders and borrowers. The program is thus based on a joint-liability, self-selective mechanism to generate group collateral and often offers targeted training and information sessions with the aim of best using loans.

Microcredit programs expanded rapidly in Bangladesh, generating a wave of enthusiasm in development circles. Bangladesh has one of the largest and oldest microfinance programs in the world.[4] Against the backdrop of a relatively undeveloped formal financial system, a large microfinance sector has developed in Bangladesh. The growth in the microcredit sector was phenomenal during the 1990s and is continuing. In 1990, the Government of Bangladesh established the Palli Karma-Sahayak Foundation (PKSF, Rural Employment Support Foundation) to mobilize funds from a wide variety of sources and provide these funds to its members for lending as microcredit. MCIs taking loans from PKSF, called partner organizations (POs), need to offer similar interest and terms as they operate under PKSF. Therefore, PKSF also works as a regulatory organization for its POs. In 2004 PKSF funds made up about 17% of the total microfinance industry in Bangladesh, which was 24% in 1998.

Microcredit programs in Bangladesh are implemented by nongovernmental organizations (NGOs); banks, such as the Grameen Bank (GB); other government or privately owned banks; and other special programs. While some programs are nationwide, others operate locally in different parts of the country. The GB is the leading MCI in Bangladesh. However, expansion, competition, and funding constraints have greatly changed the recent dynamics of microfinance in Bangladesh. For example, the Association for Social Advancement (ASA), which started its microfinance operations in 1991, has now become a dominant MCI in terms of number of beneficiaries and loan disbursement. Similarly, the Proshikha NGO has been able to increase its outreach remarkably during the 1990s, reaching about 2.8 million borrowers by 2001. During that period the number of medium and small MCIs has grown from a very small base to more than a thousand institutions (Zohir et al. 2001).

For the purpose of this study, we survey thirteen POs (MCIs) of PKSF. These MCIs offer mainly credit on similar terms, including interest on loans, because of the conditionalities imposed by PKSF. The MCIs include organizations that are very large in terms of loan disbursements and area of coverage, most notably the ASA and Proshikha. ASA provides both credit and savings services on a

[3] We use the words *short-run or long-run* for impact estimates of leavers, and *short-term* or *medium-term* for impact estimates of newcomers of microcredit.

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

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remarkably large scale. Proshikha is the fourth largest microcredit program in Bangladesh. Notable other MCIs studied here include the Society for Social Services (SSS) and the Thengamar Mohila Sabuj Sangha (TMSS). As of December 2004, SSS was the tenth largest MCI in Bangladesh in terms of cumulative disbursements and outstanding borrowers. TMSS is one of the top fifty MCI NGOs in Bangladesh. The other MCIs are relatively small and have similar types of program activities. They provide loans in a similar way to the GB. Credit is given mainly to groups of five people who are jointly liable for repayment of the loan, and there is no collateral requirement. The MCIs typically give access to microfinance to households having less than 50 decimals ($\frac{1}{2}$ acre) of land. Most of the clients of these MCIs are women, and credit is not offered to a mixed group of men and women together. Loans are advanced primarily for any profitable and socially acceptable income generating activity. The amount of a loan usually lies within the range of US$40–$160. However, members may take larger loans after repaying their first loan.

The Survey and the Data

This paper uses data from the surveys conducted by the Bangladesh Institute of Development Studies (BIDS) and the PKSF for the purposes of monitoring and evaluating microcredit programs in Bangladesh. The first survey was administered after a census of all households in the ninety-one villages during October 1997. The survey encompasses twenty-three subdistricts of thirteen of Bangladesh's sixty-four districts. The treated households were drawn from thirteen different-sized MCIs, each from a separate district. Of the thirteen 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 are 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 eleven 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 the survey there were eight control villages.[5] 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 is clustered at the village level.

While four rounds of the survey were conducted (in 1997–98, 1998–99, 1999–2000 and 2004–05), we use data from mainly the first, third, and fourth rounds because the second round did not collect comprehensive information on outcome variables such as consumption and income.[6] 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%: about 1.2% per calendar year. We study a balanced panel of 2,694 households to compare outcomes over time (we deleted 35 observations because of missing data on some key variables). The survey has different modules for household socioeconomic condition, microcredit participation, and village- and MCI-level information. The household dataset has several strengths. The data are comprehensive and cover 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 on 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 several 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

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

[6] One reason to have a follow-up survey in 2004–05 after a gap of about four 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.

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Table 1. Descriptive Statistics

	1997–1998			1999–2000			2004–2005		
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
No. 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**
No. of children	2.93	2.69	**0.24**	2.32	2.1	**0.22**	3	3.02	−0.02
No. of women	2.72	2.57	**0.15**	2.99	2.88	0.11	3.32	3.2	0.12
No. of old people	0.21	0.3	**−0.09**	0.35	0.44	**−0.09**	0.28	0.33	**−0.05**
No. 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
Nonfood exp (M) food	688.4	998.6	**−310.2**	507.5	674.2	**−166.7**	821.8	1114.1	**−292.3**
Nonland 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		
No. of clients	1592			1532			1280		
No. of obs.	2694			2694			2694		

Note: Differences that are statistically significant at 5 percent level are in bold. Exchange rate between taka and US$ in 1998 was 40/$.

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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 the survey, while 184 households had split up by the 2004–05 round. The survey followed most of the members of split-up households, who were re-interviewed. We merged the split-up households with the original intact households to form a single household. Hence, the fact that some households had split up is not a major issue in this study, as there is very little migration outside of the village.

Attrition

Here we examine whether there is any attrition bias even though the attrition rate from the survey is low compared with 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 reveals 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 nonclients. However, a comparison of means of the attritors in terms of their demographic variables reveals no significant difference between clients and nonclients (see appendix, table A1). 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 nonresponse. The full set of attrition results are available from the author upon request.[7] It is also possible that attrition is related to shocks. That is, some households might have experienced negative shocks, which led to business failure and exit from microcredit. Islam and Maitra (2008), using this dataset, find that the attrition rate is not influenced by household-level health shocks. Overall, the evidence is that any selection bias from attrition is not a problem in the present study. Moreover, we employ an estimation strategy that can resolve many potential biases (including attrition bias) that are due to unobservables.[8]

Outcomes of Interest and Descriptives

We are interested mainly in evaluating the impact of microcredit 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 the 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 on about 200 commodities consumed for a given period prior to each round of the survey. The information covers a wide range and different

[7] 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 Living Standards Measurement Study 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 nonexistent.

[8] 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.

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types (e.g., food purchased, home produced) of food consumption and is as good as the standard living standards measurement survey food consumption module conducted by the World Bank. The nonfood expenditure data include items such as kerosene, batteries, soap, housing repairs, clothing, schooling, and health expenditures, among others. The data for non-food consumption expenditure were 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 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 although food consumption is not significantly different between treatment and control groups, the two groups have different outcomes in other measures. The table shows that total microcredit borrowed by households, in taka, the Bangladeshi currency, are 7,427; 10,616; and 11,682 for 1997–98, 1999–2000, and 2004–05, respectively.

The Empirical Strategy

Random Growth Model

The major concerns in assessing the impact of microfinance are that programs are not placed at random and that participants self-select into the program. The availability of panel data allows us to address these selection bias problems, which are common in cross-sectional data. We also adopt estimation methodologies that relax many of the identifying assumptions that are typical in panel data estimation. At the outset, we consider the following random growth model (see Ashenfelter and Card 1985; Heckman and Hotz 1989):

$$(1) \qquad Y_{it} = \alpha_i + \lambda_i t + \gamma CD_{it} + \phi X_{it} + \varepsilon_{it}$$

where Y_{it} is the outcome of interest, e.g., consumption expenditure or income, for household i at period t. CD_{it} is the treatment variable, which is defined as the cumulative amount of credit borrowed up until period t. X_{it} is a vector of household-specific control variables. α_i is fixed effects unique to household i. $\lambda_i t$ is introduced to account for differential unobserved trends between treatment and control groups. λ_i can be interpreted as the average growth rate over a period (holding other covariates fixed). The error term ε_{it} is the household's transitory shock that has mean zero for each period and is assumed to be distributed independently of the treatment status CD_{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.

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:

$$(2) \qquad \Delta Y_{it} = \lambda_i + \pi D_{it} + \rho X_{it} + \eta_{it}$$

where D_{it} is the net amount of credit borrowed from an MCI at period t. This model eliminates the selection bias that results from household-specific fixed effects and the household-specific time trend. Since first differencing the right-hand-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 (2) is then just the standard unobserved effects model. This means that we can apply fixed-effects methods using equation (2) to estimate the treatment effect.

The above identification strategy takes the standard fixed-effects model a step further by allowing unobserved household differences that change at a fixed rate over time. However, like the fixed-effects model, it also assumes that there is no shock to the outcomes of the treatment and control groups contemporaneous to the program. The methodology in the next section relaxes this assumption and allows for different relative shocks affecting households in treatment and control villages. Finally, lack

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of pre-program (baseline) data makes it difficult to estimate reliably the cumulative effects of the microcredit program. However, we can estimate the marginal impact of short versus long duration of the program, which is the focus of this paper. Below we describe the methodology and identify the sub-sample to estimate the treatment effect considering the duration of participation in the program.

Main Estimation Methodology

Difference-in-Difference-in-Difference Matching Estimate

Microcredit in Bangladesh is typically offered to households that are eligible[9] in a program village. Therefore, the potentially unaffected ineligible households in 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. Thus, we can use a method that involves using a difference-in-difference (DD) estimate for eligible and ineligible households. The difference in changes in outcomes of *eligible* households across treatment and control villages is the DD estimate for the 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. In essence, we consider a difference-in-difference-in-difference (DDD) approach: a DD 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. This DDD estimator allows us to compare the effect of microcredit participation on eligible clients (in a treatment village) relative to eligible nonclients 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.[10]

To alleviate concern regarding comparability of the treatment and control groups, we use propensity score matching (PSM) of Rosenbaum and Rubin (1983) prior to using the DD/DDD estimator. In cross-sectional matching, the identifying assumption is that outcomes in the untreated state are independent of the treatment conditional on a set of observable characteristics. Rubin (1978) refers to the treatment status that is independent of potential outcomes as an ignorable treatment assignment. Although claims for ignorable are harder to justify in a quasi-experimental setting, it is justifiable in our context (though we do not rely on this assumption) that microcredit program status among the program villages is ignorable conditional on land holdings and a vector of other covariates. Households in program villages that have less land and fewer nonland assets are more likely to participate in the program. MCI selects households on the basis of eligibility and characteristics that can be observed by a loan officer and a branch manager. However, some treated households are not eligible, and the fact that all eligible households do not participate in the program introduces a potential selection bias. The sources of bias could be the differences in observable variables in terms of household size, sex ratio, schooling, age, family composition, and other household characteristics.

The selection biases that are due to unobservables are taken into account by combining PSM and DD methodologies. The conventional cross-sectional PSM estimate is based on the assumption that, conditional on the 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

[9] The MCIs set the official eligibility rule as households having less than 50 decimals (1/2 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% of the participants are eligible. We exclude all ineligible participants from the estimation below.

[10] 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 whether a household takes a 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 a shock cannot borrow unless it is already in a group or forms 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 be subject to bias to the extent households form new groups with others to participate in microcredit when they are hit by shocks.

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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. But if the household's decision to participate in a program is affected by certain characteristics that also influence the outcome of interest, then the DD estimator is sensitive to the functional form assumption (Ravallion 2007). Heckman, Ichimura, and Todd (1997) show that DD matching helps control for heterogeneity in initial conditions and also allows for unobserved determinants of participation. In this paper, we use generalized DD matching proposed by Heckman et al. (1997, 1998) that allows for temporally invariant differences in outcomes between participants and nonparticipants. It does not require assumptions about the process governing selection into the program (Behrman, Cheng, and Todd 2004). It also allows selection into the program to be based on anticipated gains from the program.

The first step in implementing a matching estimator is the estimation of the propensity score. We estimate the propensity score using a standard logit model where the dependent variable takes a value of 1 if a household is a client of an MCI and 0 otherwise. 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). The empirical distribution of the estimated odds ratio of clients and nonclients shows that there are very few regions of non-overlapping support (see figure 1).

Next we choose a matching algorithm. We follow Dehejia and Wahba (1999, 2002) and apply a variant of caliper matching called *radius matching*. 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. 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 1 for each observation. The normalized weights are used to create a comparison observation for each treatment observation. DD matching estimation is then carried out by matching differences in outcome over time for the program and control group using the same weight as mentioned above. The differences are matched on the probability of treatment exposure conditional on the propensity score.

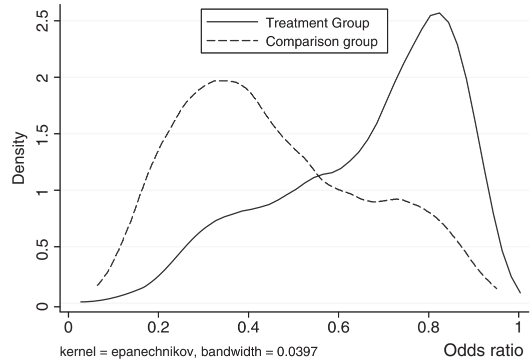

Figure 1. Estimated odds ratio for treatment and comparison groups

Thus, our strategy is to compare the observed outcome changes between eligible clients and eligible nonclients, 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 households in the absence of the program, we track outcome changes of ineligible nonclients between treatment and control villages. Our DDD 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 nonclients from the control village, and $DD2 =$ change in outcome of ineligible nonclients from the treatment village minus change in outcome of ineligible clients from the control village (all groups are matched).[11]

[11] The above identification strategy is based on the implicit assumption that there is no spill-over effect. Formally we make the stable unit treatment value assumption (SUTVA), which assumes that (*i*) the household's potential outcomes depend on its own participation only and not on the treatment status of other households; and (*ii*) the microfinance program affects the outcome of only those who participate, and that there is no externality from participant to non-participant. Thus it rules out peer and general equilibrium effects. For example, it could be the case that the education or occupation level of partner households would impact the outcomes of other participating households. To the extent that the peer effects

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DDD in Regression Framework

In order to increase the precision of DDD 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 income, consumption, and assets over the study period. We run the following reduced-form regression:

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

where Y_{it} is the logarithm of outcome variables (except self-employment income); 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; and D_i is the treatment variable. Here, we first consider the continuing participants as the treatment group and exclude occasional members of the microcredit program. In equation (3), 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, and 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 nontreated 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.

Impact on newcomers and leavers There were many clients of the microcredit program who dropped out later. It also appears 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. Entry and exit from the program allows us to consider heterogeneity in the treatment effect, considering households' duration of participation in the program.[12] Thus, we examine whether households that participate for longer periods benefit more compared with those participating for shorter periods.[13] The monitoring and follow-up of households over eight years enables us to examine the impacts that are likely to vary with the duration of participation. We classify treated households into two broad categories:

(i) *Continuing participants*—clients of an MCI throughout 1997–2005
(ii) *Occasional participants*—clients in the program for one or more years but not the entire period.

We divide the occasional participants into the following categories:

(i) New participants (*newcomers1*)—households that joined the program after 1999 and remained as clients up to 2004–05
(ii) More recent participants (*newcomers2*)—households that joined in the program after 2001
(iii) Long-term dropouts (*leavers1*)—old clients who dropped out after 1998 and did not participate in any MCI
(iv) Medium-term dropouts (*leavers2*)—clients who participated until 2001 and then dropped out
(v) Other (drifters)—the residual category of the occasional clients.

We do not consider the last category, considering their instability in participation in microcredit programs. Of the 1,592 clients surveyed in our panel, 47.2% were continuing clients, 11.3% were long-term dropouts, and 11% were medium-term dropouts. There were only about 9% of households that were *newcomers1*, and

are important, our results would be overestimated. Islam (2008), using the first cross-section data of this program, finds no evidence in support of enternalities from participants in treatment village to non-participants from the control villages. This result is also consistent with the risk-sharing literature which suggest that risk sharing, if any, in developing countries is limited within the villages.

[12] This consideration is also 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.

[13] 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.

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about 5% that were *newcomers2*. The rest were drifters. The comparison group in the sample were (matched) never participants that could potentially include (*i*) eligible households in control villages (so they do not have access to any program); (*ii*) those ineligible to participate in a program; and (*iii*) those eligible who were from a program village but did not participate. The presence of the last group means that there is potential selection bias, since they chose not to participate. We exclude them in our estimate. We also exclude ineligible clients.

We estimate the *long-term* treatment effect by comparing households that were continuing clients (for at least eight years) with those who could never participate in the program. The entry into the program by some households at a later period represents a challenge to evaluating the program because of concerns regarding the timing of the participation and the consequent selection bias. Discussion with household members indicates that many eligible households applied for the program later because they were initially unaware of the availability of the microcredit program. There was also uncertainty 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 were 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 "short-term effects," while the corresponding estimates for *newcomers1* are termed "medium-term effects," considering their length of participation in microcredit.

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. We track dropouts for up to eight years post-program and compare leavers with those nonclients 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 (see Alexander-Tedeschi and Karlan 2009). Results using a sample of *leavers1* are referred to as the *long-run effect* considering their length of nonprogram 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.

Results

Random Growth Model

Results for the random growth model using equation (2) are given in table 2. All the outcome variables are expressed logarithmically except for self-employment income, as there are many households who do not have any self-employment income. To interpret the regression coefficient for self-employment income as percentage change, we divide the corresponding estimated coefficent by the mean value of the self-employment income. The top panel reports results using household-level outcome variables, while the bottom panel reports the corresponding results using per-capita measures. The coefficient estimates in the top panel of table 2 indicate that food and nonfood consumption have increased by 3% and 13.5%, respectively. Income excluding self-employment income increased by 5.5%. The estimated increase in assets are about 2.5%. The estimated treatment coefficient in equation (2) has been converted using the average of self-employment income to interpret it in terms of percentage change. The results, as reported in column (4), indicate that treated

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Table 2. Impact Estimates Using Random Growth Models

	(1) Food Expenditure	(2) Nonfood Expenditure	(3) Other Income	(4)[1] Self-emp. Income	(5) Nonland Asset
Household level					
Treatment effect	0.0314 (0.0153)	0.1356 (0.0127)	0.0545 (0.0142)	0.1769 (0.0624)	0.0247 (0.0090)
R-squared	0.017	0.155	0.030	0.047	0.304
Per-capita level					
Treatment effect	0.0381 (0.0189)	0.1866 (0.0161)	0.0640 (0.0230)	0.2206 (0.0572)	0.0251 (0.0173)
R-squared	0.011	0.094	0.020	0.091	0.242
Number of hhs	2694	2694	2694	2694	2694

Note: [1]The resulting coefficient estimates were divided by the average self-employment income to obtain the percentage change. Standard errors presented in parentheses are corrected for clustering at the village and year level level.

Table 3. Impact of (informal) Nonmicrocredit Loan

	(1) Food Expenditure	(2) Nonfood Expenditure	(3) Other Income	(4) Self-emp. Income	(5) Nonland Asset
Other loan	−0.0071 (0.0028)	0.0018 (0.0034)	0.0075 (0.0028)	.0618 (.0302)	−0.0056 (0.0012)
R-squared	0.015	0.135	0.026	.0183	0.304
Number of hhs	2694	2694	2694	2694	2694

Note: [1]The resulting coefficient estimates were divided by the average self-employment income to obtain the percentage change. Standard errors presented in parentheses are corrected for clustering at the village and year level.

households are able to increase their self-employment income by about 17.7%, using the log of self-employment income directly in the regression results in a smaller, but positive, coefficient estimate. However, the latter considers less than half of the full sample. We also used a random-effect Tobit model (since a fixed-effect Tobit model is biased and inconsistent) for self-employment income, as there are zeros in many cases. The resulting estimates are similar to the OLS fixed-effects results reported here. The results using per-capita measures, as reported in the bottom panel of table 2, indicate very similar coefficient estimates. Using a per-capita measure, we find that the estimated coefficients are higher for nonfood expenditure and self-employment income but very much the same for other outcome measures.

We also report results using equation (2) where the treatment variable is nonmicrocredit loans taken in the last year. The results are mixed; in all but one case, the coefficients are not economically significant. The estimated coefficients, reported in table 3, show that the impact is less than 1%, except for self-employment income. The estimates are negative but insignificant for food expenditure and nonland assets, while they are positive and insignificant for nonfood expenditure and other income. The results from self-employment income indicate that microcredit clients likely use other loans for their ongoing project purpose and that both microcredit loan and nonmicrocredit loans can be complementary.

In the absence of baseline data, the impact estimates reported in table 2 do not identify total or cumulative impact of the program. It can be regarded as the incremental average effects of treatment. Below we report our main results, which are based on different durations of participation in the program.

Main Results

Long-Term Impact Table 4 presents the results for continuing participants using the DD and the DDD matching approach.[14] The

[14] 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 had an impact in the following year or so on household behavior and outcomes.

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Table 4. DDD Estimates of the Impact of Microfinance on Continuing Participants

	Eligible Group (N = 574)				Ineligible Group (n = 149)			
	Treated	Control			NT	NT		
	Program Village	Control Village	Location Difference	S.E of Location Difference	Program Village	Control Village	Location Difference	S.E of Location Difference
1997/98 ***(Panel 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
Nonfood 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
Nonfood 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 (Panel B)		S.E				S.E		
Food consumption	736.0	601.7			703.2	712.0		
Nonfood 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		
DD (Panel C)		S.E	% Gain			S.E		
Food consumption	134.3	167.1	6.6		−8.8	347.4		
Nonfood 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		
DDD (Panel D)		S.E	% Gain					
Food consumption	143.2	385.5	7.0					
Nonfood 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					

Note: NT = Non-treated ineligible households. DD = difference-in-difference, DDD = difference-in-difference-in-difference. S.E = Standard error. The results are obtained using matching without replacement, and a caliper <.0005 (.005 for ineligible group). 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.

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left-hand side compares the changes in outcomes for eligible clients in the program village (column (1)) with the change for eligible households (nonclients) 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 (eight 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 an increase of taka 134 in food consumption of eligible treated households in the program village; this is the DD estimate of the impact of continuous participation over eight years in microcredit. This figure represents an increase in food consumption of 6.6% by continuing clients. Similar calculations show that nonfood consumption expenditure of clients increased by about 12.4%. The DD estimates of self-employment income and assets for the same group of participants are even higher, representing increases of 28.1% and 14.8%, 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 an MCI selects a village observing certain shocks in that village, then a DD estimate does not correctly identify the impact of the treatment. We examine this in the right-hand side of table 4, where we perform the same exercise for ineligible groups that 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 trend in the treated and control groups correlated with unobservables but not due to the program. We find an insignificant increase in nonfood consumption, and a slight decrease in food consumption and other income among ineligible households in the treatment village compared with their counterparts in the control village. We also find a significant reduction in assets, but a small increase in self-employment income for the corresponding ineligible households (Panel C, column (5)).

Taking the difference between the two sides of table 4 (using columns (1) and (5) of Panel C), there is a 7% and 11.7% gain in food and nonfood consumption, respectively, for continuing participants (Panel D). The gain in self-employment income is 15.1%, while "other income" is increased by 6.5%. The gains in terms of accumulating assets are 14.8% (DD estimate) and 41.3% (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 nonfood consumption, but understated in the case of income, food consumption, and assets using the DD method.[15]

Short- and Medium-Term Impact The short- and medium-term treatment effects are reported in table 5. They are represented by the DDD 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* and 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 nonland 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* is 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 attributable to the additional gains resulting from longer participation or larger amounts of credit being borrowed from the MCI. The overall results for newcomers indicate that the gains are lower than that for the continuing clients. In fact, we obtain some results that show a very insignificant (or sometimes negative) impact for newcomers. But these results

[15] The reported standard errors are larger, and most of the coefficients are statistically insignificant. But they are sizeable in economic terms. We suspect these results are due to (1) sampling error; (2) non-parametric matching estimates; and (3) 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 most of the coefficents become statistically significant.

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Table 5. 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)		DD	DDD	Single Difference (N = 110)		DD	DDD
	1997/98	2004/05	(Treatment Group)		1997/98	2004/05	(Treatment Group)	
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)
Nonfood 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)
Nonland assets	−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)

Note: [1]Households that joined the program after the 1999, and remained as clients up to 2004–05. [2]Households who joined in the program after 2001. DD (treatment group) is obtained by subtracting column (2) from column (1). DDD 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 matches available in each case.

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.[16]

Medium- and Long-Run Impact Table 6 gives the DDD estimates for leavers. The left-hand side shows the results of *leavers1*, while the right-hand 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% reported that they dropped out because of difficulty in repaying their loan. About 60% of households mentioned the frequency of loan repayment and the necessity to attend a weekly meeting as the major reason for 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, being busy with other activities, such as taking care of children or parents, and so on. Casual observation and discussion with borrowers indicate that these factors influence their decision to leave the program. However, the results indicate that households can achieve a substantial gain if they 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 6, when we compare two groups of leavers, the resulting impacts are very similar for these two groups, except for assets. *Leavers2* (more recent dropouts) still have a more sizable 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, is diminishing.

We also estimate the treatment effects using a DDD approach proposed by Ravallion et al. (2005). According to this approach, we need

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

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Table 6. DDD Matching Estimates of the Impact of Microfinance on "Leavers"

	Long-run Effects of Participation Run (Leavers1)[1]				Medium-run Effects of Participation (Leavers2)[2]			
	Single Difference (N = 160)		DD		Single Difference (N = 365)		DD	
	1997/98	2004/05	(Treatment Group)	DDD	1997/98	2004/05	(Treatment Group)	DDD
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)
Nonfood 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	161.71	−70.36	105.57	−25.34	237.11	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)	(4524.0)	(9572.0)	(2664.35)	(2371.47)	(3566.88)	(9158.62)

Note: [1]Old client who dropped out after 1998 and did not participate any MFI 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. DD (treatment group) is obtained by subtracting column (2) from column (1). DDD 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.

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 nonparticipants; (2) calculate the outcome difference between leavers from the program and matched nonparticipants; (3) take the difference of each of (1) and (2) at two points of observation; and (4) take the DD calculated in (3). In essence, this requires the subtraction of the DD estimates of continuing clients (table 4) from the DD estimate of *leavers1* (table 6). The resulting impact estimates are positive in all cases, as shown in table 7. When compared with estimates obtained for continuing participants in table 4, we find impacts that are larger in the case of food consumption and slightly smaller for all other outcome variables (see last column, table 7). Since dropouts 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 DDD estimates are likely to be understated. However, it also gives estimates of what dropouts could have gained had they not left the program.

DDD Regression Results We present results for both the matched sample and the full sample in table 8 using equation (3). 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. The results in table 8 show that households' participation in a microcredit program can increase self-employment income by 60–70%, and assets by 40–50%. 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 socioeconomic status and the participation/microcredit demand. These results are still lower than those of McKernan (2002), who finds that households

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Table 7. DDD Matching Impact Estimates: Stayers versus Leavers

	DD Regular	DD Leaver	DDD (Ravallion)	DDD
Food consumption	134.32 (167.09)	−214.21 (239.32)	348.52 (386.51)	143.16 (385.49)
Nonfood consumption	59.4 (103.38)	52.4 (189.07)	7.0 (103.62)	55.88 (379.50)
Other income	1323.33 (465.62)	−70.36 (599.67)	1393.69 (1469.41)	1499.26 (1020.89)
Self-employment income	256.27 (218.42)	197.18 (315.27)	59.09 (226.27)	137.79 (387.81)
Asset	2139.85 (3073.13)	−219.64 (4524.0)	2359.5 (3874.45)	5964.29 (8977.85)

Note: DDD (Ravallion) is estimated following Ravallion et al. (2005) and 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 8. Regression Adjusted DDD Impact Estimates

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

Note: Standard errors presented in parentheses 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.

more than double their self-employment earnings (126% increase in self-employment profits) by participating with the Grameen Bank.[17] Similar results are found by de Mel, McKenzie and Woodruff (2008, 2009) in terms of the returns 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 than for occasional participants.

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

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

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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. The results in this paper are based on 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.

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Appendix

Variables Used in the Estimation of Propensity Score:

Household Level Variables. 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), Presence of head's spouse, Highest education achieved by a member, Total arable land, 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/semi-nuclear, nuclear), Dummies for occupation of the head (farmer, agricultural labour, non-agricultural labour, self-employed or businessman, professional or salaried job holder, any other job), A dummy for electricity connection, Number of living room (beside bathroom/kitchen), If cement or brick used in any of the living room, Condition of house (good, liveable, or dirty), Whether household has separate kitchen, good toilet facility.

Village Level Variable. Presence of primary school, secondary school or college, health facility, Adult male wage, 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, Number of small credit/savings groups in the village, Price of rice, wheat, oil, and potato.

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Table A1. Descriptive Statistics by Participation Status of Attriters, 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		

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