Micro-finance and Poverty:
Evidence Using Panel Data from Bangladesh

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Abstract

Micro-finance supports mainly informal activities that often have low market demand. It may be thus hypothesized that the *aggregate* poverty impact of micro-finance in an economy with low economic growth is modest or non-existent. The observed borrower-level poverty impact is then a result of income redistribution or short-run income generation. The paper addresses these questions using household level panel data from Bangladesh. The findings confirm that micro-finance benefits the poorest, and has sustained impact on poverty reduction among program participants. It has also positive spillover impact, reducing poverty at the village level. But the effect is more pronounced in reducing extreme than moderate poverty.

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I. Introduction

The objective of this paper is to estimate the long-run impacts of micro-finance on household consumption and poverty in Bangladesh, based on household survey data collected in 1991/92 and 1998/99. Since its advent in early 1980s micro-finance has been the focus of many development issues. Bangladesh is the pioneer of the micro-finance movement and the home of the largest micro-finance operation in the world.. Development practitioners have been keen to know the extent of poverty reduction possible with micro-finance operation that mostly supports the poor. Micro-finance means small-scale transactions of credit and savings. As such, it is largely meant to meet the needs of small- and medium-scale producers and businesses. The poor, especially women, are the target of micro-finance organizations in many countries, including Bangladesh. Besides financial services, micro-finance sometimes offers skill-based training to augment productivity or organizational support and consciousness-raising training to empower the poor.

How much poverty reduction is possible with micro-finance? Benefiting from a micro-finance program, unlike other transfer schemes, requires not only an individual's own entrepreneurship but also a favorable local market. Even if the marginal gains from micro-borrowing accrued to participants may be large, the accrued total benefits from micro-finance in reducing poverty are likely to be small, as micro-finance transactions are often too small in volume to have a sustained aggregate impact on poverty reduction. In an economy where there is not much growth, borrowing by the poor can improve income redistribution. Thus, it is of policy interest to know whether accrued benefits at the borrower level are due to sustained income impact or simple income redistribution. This involves an assessment of long-term poverty impacts of micro-finance.

This paper uses household panel data from Bangladesh to address three issues. First, it will determine whether the poor who lack both physical (such as land) and human capital (such as education) actually participate more in micro-finance programs. Second, the paper will assess the long-term impacts of micro-finance on poverty. Third, it will assess the aggregate impact of micro-finance to determine if the program is helping the poor beyond program participation.

The paper is organized as follows. Section two reviews current literature on the micro-finance programs in Bangladesh. Section three outlines the research framework, while section four discusses the data to be used in the impact analysis of micro-finance programs. Section five presents estimates of who participates in micro-finance programs in Bangladesh. Section six reports the impact estimates at the borrower level. Section seven discusses the spillover impacts of micro-finance. Finally, the results are summarized in the concluding section.

II. What we already know about micro-finance in Bangladesh

Unlike their formal counterpart, micro-finance organizations in Bangladesh have made their stride in delivering financial services (both savings and credit) to the poor, especially women, at a very low loan default cost. Strategies such as collateral-free group-based lending and mobilization of savings, even in small amounts, have helped them mitigate the problems of poor outreach and high loan default costs of their formal counterpart. However, they assume high transaction costs in order to keep credit discipline among borrowers through group pressure and monitoring of borrowers' behavior. The transaction cost is substantial and programs have been relying on donors for sustaining their operations (e.g., Khandker 1998; Khalily, Imam, and Khan, 2000; Morduch 1999; Yaron 1994). Nonetheless, the government and donors continue to support micro-finance programs in Bangladesh with the expectation that society benefits from such investment.

Micro-finance programs support the production and consumption of the poor. Loans in easy repayment terms help smooth consumption and create jobs for the unemployed. Resources from donors have alternative uses such as building community infrastructure, school or health facilities. Thus, there are many ways by which the poor can benefit from the resources used in micro-finance programs. For government and donors, the issue is clear: if it is evident that micro-finance programs do not benefit the poor in a sustainable way, then the resources channeled to micro-finance are misplaced.

Therefore, policymakers and program organizers are keen to learn the extent of socioeconomic impacts of micro-finance on borrowers and on society at large. At the household level, two types of impacts can be carried out—household and individual impacts of micro-finance. Household level impacts such as impacts on income, employment, and poverty are assessments without specifying the intra-household distribution of induced benefits of micro-finance. Intra-household impacts are examined to learn the distribution of benefits among different members of households, especially between men and women. Since women are disadvantaged in a society such as Bangladesh and constitute the overwhelming share of micro-finance membership, the policy question is: do women benefit from micro-finance and if so, how? At the societal level, the policy question is: do micro-finance programs benefit non-program participants or do they simply help redistribute income in a society?

While the financial review of micro-finance programs in Bangladesh is not as promising as one would expect, the literature on the impact assessment of socioeconomic benefits of micro-finance programs shows micro-finance as a promising instrument for poverty reduction. Findings, of course, differ from one study to another because of the differences in impact assessment methodologies used; however, these studies are in agreement that micro-finance programs help the poor, although all participants may not benefit equally. One of the early studies of Grameen Bank shows how Grameen Bank has been supporting the poor, especially women, in terms of employment, income generation and promotion of social indicators (Hossain 1988). Other BIDS (Bangladesh Institute of Development Studies) and non-BIDS studies also indicate the beneficial aspects of micro-finance operation in Bangladesh (e.g., Rahman 1996; Hashemi, Schuler, and Riley 1996; Schuler and Hashemi 1994). These studies show the positive correlation between micro-finance programs and their accrued benefits, but do not indicate the causality, meaning whether these programs actually matter in generating such benefits to the borrowers.

The most comprehensive and rigorous micro-finance impacts studies that have established causality were carried out in a joint research by BIDS and the World Bank (Khandker 1998; Pitt and Khandker 1998). This body of research provides a strong indication that the programs help the poor in consumption smoothing as well as in building assets. The findings also lend support to the claim that micro-finance programs promote investment in human capital (such as schooling) and contribute to increasing awareness to reproductive health (such as the use of contraceptives) among poor families. This major study also sheds lights on the role of gender-based targeting and its impact on household or individual welfare. Findings suggest that women do acquire assets of their own and exercise power in household decision-making.[1]

The positive impacts of micro-finance programs at the borrower level are thus tenable. Even then, the question arises: what are the long-run impacts of micro-finance? Are the program impacts found in 1991/92 sustainable over time? If poverty reduction is possible with micro-finance at the borrower level, what is the impact of micro-finance on aggregate poverty? Earlier estimates suggest that micro-finance can contribute to consumption at a rate of 18 percent in the case of female borrowing and at 11 percent in the case of male borrowing. Of course, this is a short-term impact and, hence, may be short-lived. It is possible that the proportion of program participants enjoying the benefits of micro-finance is very small and that the impacts of their accrued benefits on the overall economy are small as well and may not be sustainable over time.

The World Bank study based on the 1991/92 household survey indicates that only about less than 5 percent of borrowers can lift themselves out of poverty each year by borrowing from a micro-finance program even if the estimated impacts on consumption are sustained over time (Khandker 1998). Such percentage represents only about 1 percent of the population; thus, the aggregate poverty impact of micro-finance program is quite negligible. Does it imply that micro-finance programs do not need to be supported?

[1] Morduch (1998), using the same BIDS-World Bank survey data but a different technique (difference-in-difference method), finds that program effects are either non-existent or very small. He argues that the Pitt and Khandker (1998) estimates of program impacts are over-estimated and, thus, the flagship programs such as the Grameen Bank do not really help the poor. Pitt (1999) re-examined Morduch's (1999) approach and concluded that Morduch applied a wrong method to the BIDS-World Bank data set, which in fact underestimated the program impacts. Pitt's re-estimation re-confirms that the impacts of Grameen Bank and other programs as shown in the Pitt and Khandker (1998) study are indeed well founded. For those who are interested in Pitt's analysis of Morduch's re-analysis of Pitt and Khandker (1998), go to http://pstc3.pstc.brown.edu/~mp/

Despite this miniscule aggregate impact, the micro-finance movement in Bangladesh has received continuous support from donors, and more recently from the World Bank. In 1996, the World Bank provided a loan of US$115 million to the country's autonomous body called *Palli Karma Shahayak Foundation* (PKSF). This body works as an intermediary for wholesaling micro-finance. It supports the on-lending of small non-government organizations (NGOs) and a few large NGOs such as the Bangladesh Rural Advancement Committee (BRAC). However, the Grameen Bank did not seek any loan or grant from this facility. The project ended in June 2000 and the World Bank, with the request from the government, supported a follow-up project of US$ 160 million. With the flow of funds from various micro-finance agencies to borrowers, about 8 million (out of a total of approximately 30 million) households received help from micro-finance programs in 1998/99.[2] Loan outstanding of micro-finance programs was about US$600 million in 1998/99. The organized NGO sector and the specialized Grameen Bank accounted for more than 86 percent of micro-finance lending, while only 14 percent came from the country's commercial banks.[3]

Despite the large inflow of micro-credit into the rural sector of Bangladesh, the incidence of rural poverty has been stubbornly high. Rural poverty was 54 percent in 1983/84 and has been above 50 percent over the last decade (Ravallion and Sen 1995). It has declined to about 45 percent in recent years; yet the incidence of poverty has remained high. Critics argue that this reflects the limitation of micro-finance programs as an instrument in arresting poverty in a country, which has the largest micro-finance operation in the world. Is the high incidence of poverty a result of the failure of micro-finance movement? Or is it an outcome of the persistently low economic growth rate (which has been only 4 percent over the last decade) in the country? If substantial poverty reduction largely depends on sustained high economic growth, what is the net overall contribution of the micro-finance movement in Bangladesh?

[2] This means about a quarter of rural households were direct beneficiaries of microfinance in Bangladesh in 1998/99.
[3] There is a common misconception among many people that Grameen Bank is an NGO. On the contrary, it is a specialized bank with its own charter approved by the government of Bangladesh.

III. Impact assessment using panel data

The sources of bias in any program impact assessment are the non-randomness of program placement and program participation. In many cases, antipoverty programs, such as the Grameen Bank, are placed in areas with high incidence of poverty. Thus, by comparing the incidence of poverty in program and non-program areas, researchers may mistakenly conclude that micro-credit programs have increased poverty. Similarly, those who participate actually may self-select into a program based on unobserved traits such as entrepreneurial ability. Thus, by comparing behavioral outcomes, such as per capita consumption of food and non-food, between participants and non-participants, evaluators may mistakenly conclude that the program has a high impact on poverty even if part of the effects is due to the unobserved ability of the participants and has nothing to do with the program. In other words, it is possible that the estimated program effects may well be under- or overestimated depending on the circumstances.

A careful study clearly shows that endogeneity of both micro-credit program placement and program participation is a serious issue and the findings could be misleading if the endogeneity is not taken into account in the estimation (Pitt and Khandker 1998). The method used by Pitt and Khandker (1998) is based on cross-section data but they employed a quasi-experimental survey design to resolve the problems of endogeneity associated with non-random program placement and self-selected program participation.

Three components of the quasi-experimental survey design are: (a) households are sampled in villages that are with and without programs; (b) both eligible and ineligible households are sampled in both types of villages; and (c) program participants and non-participants within eligible households are sampled. The two central underlying conditions for program impact identification are: (i) exogenous landholding and (ii) gender-based program design. Since only the households with landholding of less than half of an acre are eligible, this helps to identify the program impact on participants by distinguishing who participates and who does not even if both are eligible to join a micro-credit program. However, program effects are conditioned by why certain villages are treated with a program by drawing randomly both program villages and non-program villages. The villages are further identified by women-only and men-only groups, which in turn helped identify program impacts by gender of program participants.

The quasi-experimental survey design is one of many methods evaluators use to assess program effects. There are three compelling reasons for an impact analysis using a panel survey over a cross-sectional survey: (i) Cross-section results may not be robust as some studies show that measurement of program impacts depends importantly on the methods used to treat program endogeneity (e.g., Lalonde 1986). (ii) It is important to see the robustness of the results with a method other than the one used in Pitt and Khandker (1998). With panel data, the household-level fixed-effects method is less reliant on the exact application of the landholding rule by the micro-credit programs. (iii) Cross-section data provide short-term program effects; however, program takes a long time to influence outcomes such as assets or human capital investment in children. Panel data analysis will help measure the program effects on long-term household or individual welfare .

To show how the panel data can be used to estimate program effects, assume the following reduced-form borrowing by women (B_{ijf}) and men (B_{ijm}) of i-th households in j-th village in period t:

$$B_{ijft} = X_{ijt}\beta_{bf} + \eta^{b}_{ijf} + \mu^{b}_{jf} + \varepsilon^{b}_{ijft} \qquad (1)$$

$$B_{ijmt} = X_{ijt}\beta_{bm} + \eta^{b}_{ijm} + \mu^{b}_{jm} + \varepsilon^{b}_{ijmt} \qquad (2)$$

where X is a vector of household characteristics (e.g., age and education of household head), β is a vector of unknown parameters to be estimated, η is an unmeasured determinant of the credit demand that is time-invariant and fixed within a household, μ is an unmeasured determinant of the credit demand that is time-invariant and fixed within a village, and ε is a non-systematic error.

The conditional demand for consumption (C_{ijt}) in each period conditional on the level of borrowing by male (B_{ijmt}) and that by female (B_{ijft}) for each period, is given as[4]

[4] The justification for including borrowing into the dynamic consumption equation such as (3) can be done by modifying the Ramsey consumption growth model by allowing marginal product of capital to depend on the level of borrowing in the presence of constraints on capital mobility, making households credit constrained (a similar argument on geographical capital immobility has been mentioned as a factor in consumption growth in Jalan and Ravallion 2002). Assuming that households are credit constrained, the marginal product of capital depends on borrowing (B), given by r(B). An optimization of consumption over time subject to production constraints can lead to

$$C_{ijt} = X_{ijt}\beta_c + B_{ijft}\delta_f + B_{ijmt}\delta_m + \eta_{ij}^c + \mu_{ij}^c + \varepsilon_{ijt}^c \qquad (3)$$

where δ_f and δ_m are the effects of female and male credit, respectively.

The impact of credit on household outcomes such as consumption can be measured by estimating equation (3). However, the credit demand by either male or female as given in equations (1) and (2) needs to be estimated jointly with equation (3). Using cross-section data (t=1) raises endogeneity of equation (3) on equations (1) and (2) as a result of the possible correlation among errors between borrowing equations and errors among borrowing and consumption equations (Pitt and Khandker 1998). However, equation (3) includes no variables that are not included in equations (1) and (2) or vice versa. That means, the estimating equation (3) is not distinguishable from equations (1) and (2).

Pitt and Khandker (1998) used a village-level fixed-effect method to resolve program placement (or village-level) endogeneity of the 1991/92 data. But they could not use fixed-effect method at the household level to resolve endogeneity of household participation because of the non-availability of a household panel (t=1). So they adopted a two-stage instrumental variable (IV) method to resolve the endogeneity of a household's participation. In the IV method, they used exogenous gender- and landholding-based exclusion restrictions to create discontinuous household's program choice variable. That variable was interacted with household's observable characteristics to create instruments.

With panel data where households have more than one observation (t>1), such two-stage identification restrictions are not required. This is simply done by differencing out the unobserved village and household attributes, which are the sources of correlation between the credit demand and household outcome equations. Differencing equation (3) at two points of time yields the following outcome equation

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an optimal rate of consumption growth C(t) as a function of the rate of return to capital (which is constrained by borrowing), rate of depreciation, and subjective rate of time reference. Assume that the error terms include these subjective rate of time preference and rate of depreciation.

$$\Delta C_{ij} = \Delta X_{ij}\beta_c + \Delta B_{ijf}\delta_f + \Delta B_{ijm}\delta_m + \Delta\varepsilon^c_{ij} \qquad (4)$$

Consistent estimates of the credit effect δ_f and δ_m can be obtained from equation (4) using household fixed-effects method. This is based on the assumption that the error terms of the credit demand and outcome equations are uncorrelated, that is, Corr($\Delta\varepsilon^y_{ti}$, $\Delta\varepsilon^{ci}_{tj}$) = 0. However, the error terms may be correlated for reasons other than the endogeneity of program placement and participation. For example, unobserved socio-economic factors which are assumed fixed at the household level may change over time. Under such circumstances, equations (1) and (2) can be rewritten after incorporating the variation of η and μ over time as,

$$B_{ijft} = X_{ijt}\beta_{bf} + \eta^b_{ijft} + \mu^b_{jft} + \varepsilon^b_{ijft} \qquad (1')$$
$$B_{ijmt} = X_{ijt}\beta_{bm} + \eta^b_{ijmt} + \mu^b_{jmt} + \varepsilon^b_{ijmt} \qquad (2')$$

Since household-level fixed-effects method resolves any village-level endogeneity too, we can omit for simplicity village-level unmeasured determinants of credit (μ) from above two equations. Substituting these values of B_{ijft} and B_{ijmt} and excluding the μ terms in equation (3) and differencing it at two points, equation (4) can be rewritten as,

$$\Delta C_{ij} = \Delta X_{ij}\beta_c + \Delta B_{ijf}\delta_f + \Delta B_{ijm}\delta_m + \Delta\eta^c_{ij} + \Delta\varepsilon^c_{ij} \qquad (4')$$

Thus, we see that even household-level panel data may not yield unbiased estimates of program impacts. One possible solution to this problem is to introduce two-stage instrumental variable method along with the household-level fixed-effect method. This is adopted in this paper.

IV. The data and their characteristics

The BIDS and the World Bank together surveyed 1,769 households drawn from 87 villages in 29 thanas in 1991/92. Eight program thanas were drawn randomly from each of BRAC, Grameen Bank, and BRDB's RD-12 project areas; 5 non-program thanas were also drawn randomly. Three villages were drawn randomly from each thana, where the programs had been in operation for at least three years. The survey was conducted three times during 1991/92, based on the three cropping seasons: round 1 during *Aman* rice (November-February), round 2 during *Boro* rice

(March-June), and round 3 during *Aus* rice (July-October). However, because of attrition, only 1,769 households were available in the third round.

Out of 1,769 households surveyed in 1991/92 by program participation status, 8.5 percent were Grameen Bank members, 11.6 percent were BRAC members, 6.2 percent were RD-12 project members, 40.3 percent were eligible non-participants, and 33.1 percent were non-target households (Table 1). A follow-up survey of the same households was done in 1998/99. During the re-survey, new households from the old villages and new villages in old thanas, and from three new thanas were included, thereby augmenting the sample households to 2,599.[5] According to the re-survey, 14.3 percent households were Grameen Bank members; 9.3 percent BRAC members; 3.6 percent RD-12 project members; 11.1 percent other NGO members; 7.4 percent multiple program members; 25.6 percent eligible non-participants; and 28.8 percent non-target households. Comparing the two surveys, the extent of program participation among rural households has increased from 26.3 percent in 1991/92 to 45.6 percent in 1998/99.

If participation is restricted to eligible households, the extent of program participation increased to 64.2 percent in 1998/99 from 39.5 percent in 1991/92. The annual dropout rate, which was about 5.5 percent in 1991/92, increased to about 29.3 percent in 1998/99. Net program participation among the eligible households, after adjusting for the dropouts, was 37.1 percent in 1991/92 and 45.3 percent in 1998/99. Even after adjustments due to attrition, the program participation has increased over the years. This suggests that programs must have benefited participants; otherwise, the extent of program participation among the eligible households would not have increased.

Program participation among landless and land-poor households is higher than that among landed households. For example, the participation rate is 56 percent in 1991/92 and 59 percent in 1998/99 among the landless households (Table 2). Among those who hold land up to 50 decimals (first three groups in the table), the participation rate increased from 33.9 percent in 1991/92 to

[5] Among the 1,769 households surveyed in 1991/92 survey, 131 could not be re-traced in 1998/99 because of attrition, leaving 1,638 households available for the re-survey. However because of household split-offs, 237 original households split to form 546 households in 1998/99, resulting in 1,947 household counts from original survey. Added to them were 652 new households from 1998/99 survey, making total households to 2,599. Khandker and Pitt (2002), in a separate paper, addressed the issue of bias due to attrition and split-offs in the same survey.

56.1 percent in 1998/99. Not all households among program participants, however, strictly meet the land-based eligibility criteria of micro-finance programs. The extent of potential mistargeting has not changed much since 1991/92. About 23 percent of program borrowers came from non-target households (those having 50+ decimals of land) in 1991/92 compared to 25 percent in 1998/99 (Table 2). Among the participants, the ultra poor (households owning less than or equal to 20 decimals of land) constitute about 33 percent in the 1991/92 survey and 58 percent in the 1998/99 survey.[6]

The extent of multiple membership is a new phenomenon that surfaced in the re-survey. Cases of households being members of more than one program at the same time have been reported in the 1998/99 survey but not in the 1991/92 survey. As shown in Table 2, the extent of multiple program membership was 16 percent. Multiple membership is higher among landed households than among land-poor households. It was 13 percent among landless households in contrast to 19 percent among households who hold land up to more than 100 decimals.

The paper's assessment of the impact of program participation relies on panel data so the sample is restricted to households who form the panel, i.e., those who were interviewed in both periods. That leaves us with 1,638 households from the 1991/92 survey. But as mentioned earlier, 237 original households split to form 546 households in 1998/99, resulting in 1,947 households. In order to have a one-to-one correspondence among matching households, we logically combined these split households (including the original household) and treated them as a single household in the re-survey data. Having done this logical integration, we arrived at 2,290 households in the 1998/99 survey, of which 1,638 are panel households. Expectedly, we conducted statistical tests to identify if this merger was appropriate. The tests suggest that the merging does not produce any statistically different results than in the case of keeping them separate.[7]

A detail summary statistics of all remaining explanatory variables is given in the Appendix Table A. And Table 3 shows only the descriptive statistics of household- and individual-level outcomes that are of particular interest and the levels of male and female borrowing. They are

[6] Landholding is considered a proxy of household wealth and poverty in rural Bangladesh.
[7] See Khandker and Pitt (2002) for the details of the test.

contrasted among participants and non-participants (both target and non-target households) and between 1991/92 and 1998/99. The monetary values of outcomes such as borrowing and consumption are adjusted by the consumer price index with 1991/92 as the baseline. While average male borrowing of participant households declined from Tk. 2,730 to Tk. 2,198 in real terms, or by 24 percent over the seven-year period, average female borrowing of participants increased by 126 percent in real terms. This also suggests that micro-finance programs provided loans mainly through female borrowers, showing that female credit on the average accounted for 85 percent of micro-borrowing in 1998/99 compared to 66 percent in 1991/92.

Based on the consumption data and the poverty line consumption, we found that aggregate moderate poverty has declined from 83 percent in 1991/92 to 66 percent in 1998/99 (17 percent points overall reduction over seven years).[8] The reduction in the incidence of moderate poverty was 20 percent points among program participants compared to 15 percent points among target non-participants. The aggregate level of extreme poverty was 45 percent in 1991/92 compared to 33 percent in 1998/99 (overall reduction of 12 percent points). At the same time, extreme poverty decreased by 19 percent points among program participants, 13 percent points among target non-participants and 5 percent points among the non-target group. The levels of consumption (food, non-food, and overall) had also increased for program participants over this period, as well as the non-land asset. The question is: how much changes in consumption and poverty were due to borrowing from micro-finance programs?

V. Do the poor participate in micro-finance programs?

Before addressing the above question, we would like to address who are the participants of micro-finance anyway. There is an issue in the micro-finance literature that the very poor do not participate in these programs. Indeed, program participation is determined by a host of factors (both household and village level) including physical endowments (such as land) and human capital (such as education), given the availability of the program in a village. To determine the relative roles of physical and human capital endowments in micro-finance program participation, we estimated borrowing equations given in (1) and (2) using cross-section and panel data.

[8] Moderate poverty is based on an expenditure that is required to meet the FAO (Food and Agriculture Organization) guidelines of 2,112 calories of daily dietary requirement of food items and a non-food expenditure that is 30 percent

Program participation is represented by the amount of cumulative amount borrowed from micro-finance programs. This takes the value of zero for non-borrowers.

A village-level fixed-effects method was applied to equations (1) and (2) based on the cross-section data of 1991/92 and 1998/99, assuming there is no unmeasured household-level determinants of credit. Such method would help eliminate the influence of the unmeasured village-level demand for credit by men and women. The results are shown in Table 4. Econometric results with cross-section data confirm that households that are resource-poor in either landholding or education participate more in micro-finance programs. A 10-percent increase in landholding from an average of 137 decimals during 1991/92 reduces total amount of borrowing by 1.3 percent for men and 2.8 percent for women. This trend continues in 1998/99. Similarly, a one-year increase in female education reduces micro-finance borrowing by 6 to 7 percent for women in both periods. In contrast, an increase in male education increases micro-finance borrowing of men, particularly during 1998/99. However, since female borrowers are much more dominant in terms of the number and amount of loan than their male counterpart, the impact of female borrowing will be much more pronounced at the aggregate level.

As previously noted, those who are poor in landholding and formal education seem to participate more in micro-finance programs. An overwhelming percentage (about 60 percent) of micro-finance program participants in Bangladesh actually belong to households holding less than 20 decimals of land. In addition, more than 60 percent of the micro-finance borrowing households are headed by individuals with no formal education at all. Given this, the obvious question is: do the extreme poor who lack both land and human capital assets really benefit from micro-finance programs, where borrowing requires entrepreneurship and some kind of skills?

If unmeasured household demand for credit is indeed an important factor, the village-level fixed-effects method would yield biased estimates of the impact of observed variables such as education and landholding. In this case, a household fixed-effects method is appropriate. Table 4 also presents the household-level fixed-effects estimates of the same explanatory variables using panel data. Although cross-section demand equations show the importance of either physical or

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of food expenditure. Extreme poverty uses a lower calorie requirement of 1,739 for food items and is usually 80 human resources, this is not the case with the household-level fixed effects method. This means that unobserved ability, such as the entrepreneurship of a household, matters most in influencing the demand for micro-credit over time, although landholding and education, perhaps proxy for such unobserved household attributes, may indicate the participation in a program. In other words, landholding and education are poor indicators of who participate and how much they borrow from a micro-finance program among males and females from eligible households. Yet in other words, even if micro-finance programs have been able to draw participants from the poor with lower landholding and education, these factors do not matter over time when unobserved ability such as entrepreneurial ability or other household attributes perhaps matter most in program participation.

VI. Estimates of poverty impacts on participants

Poverty reduction is an overarching objective of a targeted program such as targeted micro-finance programs. As the poor with unobserved skills or attributes are likely to participate more in micro-finance programs, the impact assessments of micro-finance on poverty reduction must take care of these unobserved factors associated with participation.

The household fixed-effects method, which controls for fixed unobserved attributes of households participating in such programs, may not yet yield consistent estimates of credit impacts with panel data when unmeasured determinants (at both household and village level) of credit vary over time. Also there is another problem to worry about: If credit is measured with errors (which is likely), this error gets amplified when differencing over time, especially with only two time periods. This measurement error will impart "attenuation bias" on the credit impact coefficients, meaning that the impact estimates are biased toward zero. A standard correction for such bias is the use of instrumental variable (IV) estimation. The IV method is also relevant to taking care of the problem that arises when the unobserved characteristics are not fixed but time-varying.

Since it is the accumulated credit or stock of credit that influences consumption, we can rewrite the outcome equation (suppressing subscripts for male and female) as,

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percent below the moderate poverty line.

$$C_{ijt} = X_{ijt}\beta_c + S_{ijt}\delta_b + \eta^c{}_{ijt} + \varepsilon^c{}_{ijt} \qquad (5)$$

where we replaced the credit variable *B* with stock of credit *S*. Now for two periods we write the above consumption equation as,

$$C_{ij1} = X_{ij1}\beta_c + S_{ij1}\delta_b + \eta^c{}_{ij1} + \varepsilon^c{}_{ij1}$$
$$C_{ij2} = X_{ij2}\beta_c + S_{ij2}\delta_b + \eta^c{}_{ij2} + \varepsilon^c{}_{ij2}$$

Taking the difference we obtain,

$$\Delta C_{ij} = \Delta X_{ij}\beta_c + \Delta S_{ij}\delta_b + \Delta\eta^c{}_{ij} + \Delta\varepsilon^c{}_{ij}$$

Since the difference between the stocks of credit in two periods is in fact the credit reported during second period, that is B_2, we can write the above equation as,

$$\Delta C_{ij} = \Delta X_{ij}\beta_c + B_{ij2}\delta_b + \Delta\eta^c{}_{ij} + \Delta\varepsilon^c{}_{ij} \qquad (6)$$

Now for the implementation of IV method, let us write the first-stage equation for the stock of credit (suppressing subscripts for male, female, household and village) as,

$$S_t = X_t\beta_b + Z_t\gamma_t + \varepsilon^b{}_t$$

where *Z* is a set of household and village characteristics distinct from *X*'s so that they affect *S* but not other household outcomes dependent on *S*. The impact of *Z* variables on *S* is allowed to vary with time because of the possibility of differential effects of instruments over time on credit demand. For two periods we can write the above equation as,

$$S_1 = X_1\beta_b + Z_1\gamma_1 + \varepsilon^b{}_1$$
$$S_2 = X_2\beta_b + Z_2\gamma_2 + \varepsilon^b{}_2$$

Taking the difference we obtain,

$$\Delta S_2 = \Delta X\beta_b + Z_2\gamma_2 - Z_1\gamma_1 + \Delta\varepsilon^b$$

which becomes,

$$B_2 = \Delta X\beta_b + Z_2\gamma_2 - Z_1\gamma_1 + \Delta\varepsilon^b \qquad (7)$$

Selecting appropriate Z variables is a crucial part of this exercise. In order to do that we define a household-level choice variable which determines whether or not a household has a choice to participate in a program. A household's choice in program participation depends on two factors: whether a credit program operates in the village where the household lives in and whether the household itself qualifies to participate in the program, based on the landholding criteria (that is if the household's landholding is 0.5 acre or less). A male's or female's choice to participate in a program depends not only on whether the village has a program and whether the household satisfies the land eligibility condition but also whether the village has a male or female group, as participation is gender-based. The choice variable so defined is considered for both 1991/92 and 1998/99 to take care of the differential impacts of two periods and is then interacted with household-level exogenous variables and village fixed-effects to get the instruments.

Table 5 shows a distribution of survey villages by male and female choice group. A common trend is obvious from this table that groups with both males and females have increased in the program areas over the years. Another interesting phenomenon is expansion of program outreach in areas that were previously non-program. Although number of villages have increased from 1991/92 to 1998/99, number of non-program villages with no program has decreased from 15 to 5.

Table 6 presents household fixed-effects IV estimates of program impacts on six outcomes: per capita total expenditure, per capita food expenditure, per capita non-food expenditure, the incidence of moderate and extreme poverty, and household non-land asset. The results are quite interesting. Male borrowing has no impact on household per capita total expenditure and moderate poverty, but some impact on per capita non-food expenditure. For the expenditure and non-land asset equations, we used logarithmic equation, in which case the coefficients of credit measure the response elasticity. In contrast, the poverty incidence equations are log-linear equation, in which case the coefficient on credit measures the probabilistic change in outcomes with respect to percentage changes in borrowing. Thus, a 10-percent increase in male borrowing increases household non-food expenditure by 0.2 percent.

In contrast, a 10-percent increase in female borrowing increases household per capita total expenditure by 0.2 percent, food expenditure by 0.1 percent, non-food expenditure by 0.5 percent and household non-land asset by 0.2 percent.

Unlike consumption and asset variables which are continuous, the poverty incidence variables are binomial (meaning 1 if the household is moderately or extremely poor and zero otherwise). Household fixed-effects method is non-applicable with such discontinuous variables. Two-stage method is even harder. Thus, we used linear probability functional form to estimate the impact of borrowing in a two-stage fashion just to indicate the direction of change in these outcomes due to borrowing. The results support the view that micro-finance borrowing reduces poverty, especially extreme poverty.

In order to quantify the contribution of micro-finance to poverty reduction, we can alternatively use the consumption estimates.[9] The results of this exercise are shown in the first two columns of Table 9. The findings indicate that participants' moderate poverty dropped 8.5 percentage points over the period of seven years, while extreme poverty dropped about 18.2 points over the same period. Interestingly enough, the returns to borrowing have declined over time. Based on the panel data estimates, the returns to female borrowing are 10.5 percent, which was 18 percent according to cross-sectional estimates of 1991/92 data. Overall, the results suggest the following: (1) micro-finance impacts are much stronger for female borrowers than for male borrowers and that returns to borrowing have declined over time; (2) the impacts on expenditure are more pronounced for non-food expenditure than for food expenditure; and (3) the poverty

[9]Since a household's per capita consumption is directly linked with its poverty status, the impact of micro-credit borrowing on per capita consumption can also be used to determine the total change in household's poverty status due to micro-credit borrowing. A simulation exercise is done where marginal impact of micro-credit borrowing (in this case only female borrowing as male borrowing has no significant impact) on household per capita expenditure is calculated from the regression output shown in table 6. That marginal impact is used to calculate the increase in participant household's total consumption due to micro-credit borrowing. This amount when subtracted from the current consumption of participants, gives a pre-borrowing level of consumption and this consumption can be compared with the poverty line to get a pre-borrowing poverty status of the participants.

impacts of female borrowing are much stronger in reducing extreme poverty than it is in reducing moderate poverty at the participant level.

VII. Estimates of spillover and aggregate impacts

The total loan outstanding of micro-finance organizations in Bangladesh was about US$600 million in 1998/99. This indicates a large inflow of micro-funds in the rural areas, which is expected to make an aggregate impact on the local economy. We have just seen that micro-finance has sizeable effects on the welfare of borrowing households in terms of raising consumption and non-land asset as well as in reducing moderate and extreme poverty. But are these effects felt beyond the program participants? How do we account for estimating micro-finance effects on non-participants?

The cross-section data estimates of Pitt and Khandker (1998) show the effects of participation above and beyond the non-participation in a program. A program may affect the non-participants and, thus spillover effects are non-zero. Only longitudinal data allow us to estimate the spillover effects. We therefore need panel data to do so. When there are spillover effects, unobserved village heterogeneity would be correlated with program placement, but the causation would go from program placement to village unobserved effects, not from village effects to program placement. This measurement problem implies that the placement of a credit program may cause a village effect in addition to a pre-existing (time-invariant) village effect.

$$C_{ij} = \beta X_{ij} + \delta S_{ij} + \eta_{ij} + \mu_j + \Omega_j + \varepsilon_{ij} \qquad (8)$$

where Ω_j represents the external effects of a program in a village and has the value of zero if no program is located in the village, μ_j is unobserved village-level fixed-effect, and η_{ij} is unobserved household-level fixed-effect. The program effect parameter, δ, estimated with cross-section data captures all program effects only if $\Omega_j = 0$. (None of the village-specific heterogeneity is caused by programs). If village externalities exist ($\Omega_j \neq 0$), then the spillover effect is not separately identified from the time-invariant village effect (μ_j). With panel data, it is possible to estimate the spillover effect with the following equation:

$$C_{ijt} = \beta X_{ijt} + + \delta S_{ij} + \eta_{ij} + \Omega_{jt} + \varepsilon_{ijt} \qquad (9)$$

We excluded the μ-term, since with panel data, household fixed-effects sweep away the village effects and make it possible to estimate the impact of Ω_{jt}. Suppose Ω_{jt} is measured by the aggregate credit obtained in a village, then the spillover effect is measured by the change in behavior of non-participants due to change in aggregate micro-credit obtained in the village. That is, for non-participants,

$$C_{kjt} = \beta X_{kjt} + \delta \sum_{i=1}^{n(j)} S_{ijt} + \eta_{kj} + \varepsilon_{kjt}, \quad t = 1,2, \ \& \ S_{ijt} = 0 \qquad (10)$$

where k refers to the non-participants of micro-finance programs living in j-th village.

Since measurement errors are diminished with aggregation, there is no need to use a two-stage method for taking care of attenuation bias with credit variables. The above equation is estimated by the household fixed-effects method that eliminates also program placement bias. The estimated effects of credit on non-participants are presented in Table 7.

The benefits of non-participants depend on the amount of credit obtained by all program borrowers living in a village. We find an evidence of externality of micro-credit programs. Male borrowing from micro-finance programs affects the food, non-food, and total per capita expenditure of non-participants but has no effect on the non-land asset. In contrast, although female borrowing has no significant effect on consumption of non-participants, it has a substantial effect on their non-land asset. For instance, a 10-percent increase in aggregate village micro-credit borrowing by female members increases household non-land asset of non-participants by as much as 1.1 percent. Micro-finance also affects the poverty incidence among non-participants. The negative spillover effect on poverty is, however, more pronounced for extreme poverty than for moderate poverty. The two-stage logit estimates indicate that the probability of reducing extreme poverty can be as much as 0.09 percentage points. Based on alternative consumption estimates, the net contribution of female borrowing on poverty is calculated and presented in table 9. Accordingly, moderate poverty for non-participants declined by 1.1 percentage points and extreme poverty by 4.8 points over the study period because of female borrowing in the village from micro-finance programs.

We may also assess the aggregate effect of micro-finance by examining the program effects for an average household by estimating similar equation. For all households we can write:

$$C_{ljt} = \beta X_{ljt} + \delta \sum_{i=1}^{n(j)} S_{ijt} + \eta_{lj} + \varepsilon_{ljt}, \quad t = 1,2, \ \& \ S_{ijt} = 0 \qquad (11)$$

where $l = i+k$. Unlike the earlier equation, every household in the village in this equation is affected by the same male and female credit irrespective of participation status in a micro-finance program. A simple household-level fixed-effects method is used to estimate the micro-finance aggregate effects on welfare of all households living in a village.

Table 8 presents the aggregate micro-finance impact for male and female borrowings. The results strongly support the view that micro-credit not only affects the welfare of participants and non-participants but also the aggregate welfare at village level. Male borrowing increases average household welfare by increasing household total consumption, as well as food and nonfood consumption, and by reducing extreme poverty, with only a minimal effect on moderate poverty. Female credit, on the other hand, reduces extreme poverty and increases household non-land asset for an average household in a village.

Micro-finance operation in a village, therefore, reduces the incidence of extreme poverty for an average household living in a village but it does not affect the incidence of moderate poverty. The probability of reducing aggregate extreme poverty is approximately .03 percentage points for female borrowing and .08 percentage points for male borrowing. However, the net contribution of female borrowing over the study period (based on alternative consumption estimates and presented in table 9) is 1.7 percentage points for moderate poverty and 5.5 percentage points. Non-borrowers seem to benefit from micro-finance partly due to the externality of borrowing by program participants and partly because of externality due to program placement. Therefore, micro-finance contributes to the overall welfare of the society.

VIII. Summary and conclusions

Program evaluation compares outcomes of treatment groups with those of control groups. Finding control group in a non-experimental setting is very difficult. Traditionally, resorting to instruments for identifying program effects is done with cross-section data. However, finding good instruments is equally difficult. Pitt and Khandker (1998) used quasi-experimental method relying on exogenous eligibility conditions as a way of identifying program effects. Some of the conditions are restrictive and might not be reliable, for example, the non-enforceability of landholding criterion for program participation. Results may be sensitive to methods used in impact assessment. An impact assessment was carried out using a follow-up survey to see the sensitivity of the findings (see Khandker and Pitt (2002) for details).

This paper carried out a similar exercise by estimating the effects of micro-finance on consumption, poverty and non-land assets for participants, non-participants, and an average villager, assuming that micro-finance programs have spillover (externality) effects. The results are resounding: micro-finance matters a lot for the very poor borrowers and also for the local economy. In particular, micro-finance programs matter a lot to the poor in raising per capita consumption, mainly on non-food, as well as household non-land asset. This increases the probability that the program participants may be able to lift themselves out of poverty. The welfare impact of micro-finance is also positive for all households, including non-participants, indicating that micro-finance programs are helping the poor beyond income redistribution with contribution to local income growth. Programs have spillover effects in local economies, thereby increasing local village welfare. In particular, we find that micro-finance helps reduce extreme poverty more than moderate poverty at the village level. Yet the aggregate poverty reduction effects are not quite substantial to have a large dent on national level aggregate poverty. This concern brings to the fore the effectiveness of micro-finance as an instrument to solve the problem of poverty in Bangladesh.

To exhibit a stronger impact on poverty reduction, micro-finance should perhaps go beyond the provision of financial services. It should find ways to improve the skills of its poor borrowers to improve their productivity and income. It should also assist its borrowers in marketing and improving the quality of their products. Micro-finance is, however, only one of the many instruments of poverty reduction. Growth matters too—even more significantly than other instruments. Investment in human capital and other means to empower the poor also matter. To achieve substantial poverty reduction, the other avenues must be explored as well.

Table 1: Distribution of households by program participation

Program participation status	1991/92 survey	1998/99 survey
Grameen Bank members	8.5	14.3
BRAC members	11.6	9.3
BRDB RD-12 members	6.2	3.6
Other NGO members	0	11.1
Multiple program members	0	7.4
Target non-participants	40.3	25.6
Non-target households	33.4	28.8
No. of observations	1,769	2,599

Note: Other NGO households include members of ASA, PROSHIKA, GSS, Youth Development and other small NGOs.

Table 2: Household participation in micro-credit programs

Landholding (decimal)	1991/92 survey		1998/99 survey		
	Participation rate in each landholding group	Distribution of participants by landholding group	Participation rate in each landholding group	Distribution of participants by landholding group	Participation rate in multiple programs in each landholding group
0	56.4	8.3	58.8	10.9	12.5
1-20	33.1	53.8	58.0	49.8	15.8
21-50	29.5	15.3	48.3	14.5	13.3
51-100	24.3	9.4	43.7	11.3	20.3
101-250	16.0	10.3	35.0	10.6	18.1
251+	7.1	2.9	12.0	2.9	21.8
All households	26.0	100.0	45.6	100.0	16.1
Observations	1,769	894	2,599	1,630	1,630

Table 3: Summary statistics of outcome and credit variables

Variables	1991/92				1998/99			
	Program participants	Target non-participants	Non-target group	All households	Program participants	Target non-participants	Non-target group	All households
Male borrowing (taka)	2,730.0 (6,341.1)	0	0	705.7 (3,437.5)	2,198.3 (8,112.7)	0	0	1,173.3 (6,007.9)
Female borrowing (taka)	5,311.6 (7,573.4)	0	0	1,373.0 (4,497.5)	12,008.7 (18,371.9)	0	0	6,367.4 (14,658.1)
Household per capita yearly total expenditure (taka)	3,923.4 (1,566.6)	3,838.0 (1,795.5)	5,586.0 (3,442.8)	4,462.7 (2,578.7)	5,276.9 (3,490.4)	4,782.0 (2,977.9)	7,587.6 (6,317.4)	5,810.1 (4,502.5)
Household per capita yearly food (taka)	3,057.4 (786.3)	3,018.7 (948.7)	3,629.2 (1,050.6)	3,239.2 (987.9)	3,526.9 (1,304.2)	3,466.0 (1,634.9)	4,400.8 (2,156.3)	3,753.4 (1,687.8)
Household per capita yearly nonfood (taka)	866.0 (1,098.8)	819.3 (1,118.6)	1,956.8 (2,875.2)	1,223.5 (1,982.8)	1,750.0 (2,770.7)	1,316.0 (1,753.9)	3,186.8 (5,287.0)	2,056.7 (3,575.1)
Head count ratio for moderate poverty	0.903 (0.296)	0.896 (0.306)	0.702 (0.458)	0.831 (0.375)	0.705 (0.456)	0.747 (0.435)	0.503 (0.501)	0.658 (0.474)
Head count ratio for extreme poverty	0.526 (0.500)	0.581 (0.494)	0.245 (0.431)	0.451 (0.498)	0.343 (0.475)	0.454 (0.499)	0.200 (0.401)	0.326 (0.469)
Household non-land asset (taka)	17,891.9 (25,606.1)	14,251.3 (27,122.4)	53,914.7 (85,190.3)	28,867.4 (57,331.1)	31,941.4 (100,778.0)	29,044.5 (69,022.2)	72,199.2 (109,925.1)	42,358.1 (99,700.5)
Observations	824	567	247	1,638	1,122	279	237	1,638

Note: Figures in parentheses are standard deviations.

Table 4: Fixed-effects Tobit estimates of micro-credit borrowing

Explanatory variables	Village-level fixed effects				Household-level fixed effects	
	1991/92 data		1998/99 data		Panel data	
	Log of male borrowing	Log of female borrowing	Log of male borrowing	Log of female borrowing	Log of male borrowing	Log of female borrowing
Maximum education of household male (years)	0.03 (0.70)	-0.003 (-0.05)	0.03 (1.72)	0.003 (0.08)	0.05 (1.51)	-0.04 (-1.01)
Maximum education of household female (years)	-0.04 (-1.10)	-0.07 (-1.63)	-0.02 (-0.43)	-0.06 (-1.74)	-0.01 (-0.43)	0.01 (0.36)
Log of household land assets (decimal)	-0.13 (-2.55)	-0.28 (-4.56)	-0.09 (-2.21)	-0.55 (-8.38)	-0.01 (-0.13)	-0.15 (-1.22)
F-statistics	3.14	7.05	3.42	13.40	5.51	11.98
Number of observations	1,769	1,769	2,290	2,290	3,276	3,276

Notes: 1. Figures in parentheses are t-statistics.
2. Complete regressions include, in addition to variables given, sex and age of household head, if parents; brothers and sisters of household head; household head's spouse own land.

Table 5: Distribution of villages by credit program and group type

Group type	Village type									
	1991/92					1998/99				
	BRAC	BRDB	GB	None	Total	BRAC	BRDB	GB	None	Total
Female only	7	3	12	0	22	10	1	12	14	37
Male Only	0	9	1	0	10	0	0	1	0	1
Female and male	17	12	11	0	40	17	23	14	7	61
No program	0	0	0	15	15	0	0	0	5	5
Total	24	24	24	15	15	27	24	27	26	104

Note: In 1991/92 non-program villages were those villages which did not have any credit programs. But in 1998/99 none-program villages include old non-program villages and newly included villages that are outside old BRAC, BRDB and GB thana. Many of these villages now have programs

Table 6: Two-stage fixed-effects linear estimates of micro-finance on household welfare of participants

Explanatory variables	Log of household per capita yearly total expenditure (taka)	Log of household per capita yearly food expenditure (taka)	Log of household per capita yearly non-food expenditure (taka)	If household is below moderate poverty line	If household is below extreme poverty line	Log of household non-land asset (taka)
Log of male borrowing	0.01 (1.08)	-0.001 (-0.15)	0.02 (1.68)	-0.01 (-1.04)	-0.003 (-0.38)	-0.021 (-1.00)
Log of female borrowing	0.02 (3.60)	0.01 (2.14)	0.05 (4.95)	-0.01 (-2.68)	-0.02 (-3.78)	0.02 (2.36)
Adjusted R squared	0.07	0.08	0.05	0.07	0.04	0.11
Observations	1,638	1,638	1,638	1,638	1,638	1,638

Notes: 1. Figures in parentheses are t-statistics.
2. Complete regressions include, in addition to above variables, household level variables mentioned in Table 2 plus village-level price and infrastructure variables.
3. All variables reported here are in logarithmic form except for poverty variables, which are dummy variables defining whether a household is below poverty line.

Source: BIDS-World Bank household surveys, 1991/92 and 1998/99

Table 7: Fixed-effects estimates of aggregate village credit on household welfare of nonparticipant households

Explanatory variables	Log of household per capita yearly total expenditure (taka)	Log of household per capita yearly food expenditure (taka)	Log of household per capita yearly non-food expenditure (taka)	If household is below moderate poverty line (conditional logit)	If household is below extreme poverty line (conditional logit)	Log of household non-land asset (taka)
Log of male borrowing	0.048 (2.142)	0.032 (1.772)	0.093 (2.152)	-0.419 (-1.248)	-0.484 (-2.033)	0.022 (0.401)
Log of female borrowing	-0.006 (-0.391)	0.001 (0.081)	-0.031 (-1.041)	0.045 (0.253)	-0.247 (-1.918)	0.105 (2.797)
F/Chi^2 statistics	F(18,479) = 5.61	F(18,479) = 2.87	F(18,479) = 5.70	Chi^2 (18) = 71.71	Chi^2 (18) = 36.09	F(18,479)=11.73
Observations	994	994	994	994	994	994

Notes: 1. Figures in parentheses are t-statistics.
2. Complete regressions include, in addition to above variables, household-level variables mentioned in Table 2.
3. All variables reported here are in logarithmic form except for poverty variables, which are dummy variables defining whether a household is below poverty line.
Source: BIDS-World Bank household surveys, 1991/92 and 1998/99

Table 8: Fixed-effects estimates of aggregate village credit on household welfare of all households

Explanatory variables	Log of household per capita yearly total expenditure (taka)	Log of household per capita yearly food expenditure (taka)	Log of household per capita yearly non-food expenditure (taka)	If household is below moderate poverty line (conditional logit)	If household is below extreme poverty line (conditional logit)	Log of household non-land asset (taka)
Log of male borrowing	0.045 (3.688)	0.029 (3.058)	0.110 (4.448)	-0.245 (-1.485)	-0.412 (-3.488)	0.019 (0.662)
Log of female borrowing	-0.003 (-0.301)	-0.002 (-0.310)	0.0003 (0.021)	-0.094 (-0.871)	-0.144 (-2.026)	0.118 (5.672)
F/Chi^2 statistics	F(18,1620) = 19.27	F(18,1620) = 11.63	F(18,1620) = 22.70	Chi^2 (18) = 245.50	Chi^2 (18) = 169.80	F(18,1620)=32.66
Observations	3,276	3,276	3,276	3,276	3,276	3,276

Notes: 1. Figures in parentheses are t-statistics.
2. Complete regressions include, in addition to above variables, household-level variables mentioned in Table 2.
3. All variables reported here are in logarithmic form except for poverty variables, which are dummy variables defining whether a household is below poverty line.
Source: BIDS-World Bank household surveys, 1991/92 and 1998/99

Table 9: Simulated impact of micro-credit borrowing on poverty

Poverty indicators	Participants		Non-participants		All households	
	Before borrowing	After borrowing	Before village-level aggregate borrowing	After village-level aggregate borrowing	Before village-level aggregate borrowing	After village-level aggregate borrowing
Head count ratio for moderate poverty	85.5	77.0	73.9	72.8	76.2	74.5
Head count ratio for extreme poverty	58.5	40.3	42.7	37.9	44.3	38..8
Observations	1,946		1,330		3,276	

Source: BIDS-World Bank household surveys, 1991/92 and 1998/99

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Appendix
Table A: Summary statistics of explanatory variables

Variables	1991/92	1998/99	Whole sample
Cumulative male borrowing (taka)	705.68 (3,437.55)	1,173.27 (6,007.89)	939.48 (4,899.30)
Cumulative female borrowing (taka)	1,372.98 (4,497.54)	6,367.38 (14,658.06)	3,870.18 (11,124.09)
Program duration of males (years)	0.39 (1.38)	0.80 (2.51)	0.59 (2.04)
Program duration of females (years)	0.67 (1.61)	2.22 (3.51)	1.45 (2.84)
Village aggregate male borrowing (taka)	30,678 (48,701)	49,051 (111,060)	39,628 (86,470)
Village aggregate female borrowing	47,983 (62,978)	222,971 (316,508)	136,584 (244,141)
Highest grade completed by household head (years)	2.77 (3.68)	2.49 (3.64)	2.63 (3.66)
Sex of household head (male=1, female=0)	0.95 (0.21)	0.87 (0.34)	0.91 (0.28)
Age of household head (years)	41.29 (12.94)	48.50 (13.18)	44.89 (13.55)
Maximum education by household male (years)	3.44 (3.93)	4.57 (4.53)	4.00 (4.28)
Maximum education by household male (years)	1.79 (3.04)	3.02 (3.83)	2.40 (3.51)
No adult male lives in household	0.03 (0.17)	0.03 (0.16)	0.03 (0.17)
No adult male lives in household	0.01 (0.11)	0.01 (0.08)	0.01 (0.10)
Household land (decimals)	130.03 (316.56)	133.05 (325.85)	131.54 (321.19)
No spouse lives in household	0.11 (0.31)	0.17 (0.37)	0.14 (0.34)
Parents of household head own land	0.21 (0.41)	0.17 (0.37)	0.19 (0.39)
Brothers of household head own land	0.39 (0.49)	0.37 (0.48)	0.38 (0.49)
Sisters of household head own land	0.40 (0.49)	0.31 (0.46)	0.35 (0.48)
Parents of household head's spouse own land	0.35 (0.48)	0.32 (0.47)	0.34 (0.47)
Brothers of household head's spouse own land	0.39 (0.49)	0.38 (0.49)	0.39 (0.49)
Sisters of household head's spouse own land	0.40 (0.49)	0.31 (0.46)	0.35 (0.48)
If village is accessed by road all year	0.94 (0.23)	0.85 (0.36)	0.90 (0.31)
Proportion of village land irrigated	0.44 (0.31)	0.57 (0.32)	0.51 (0.32)
If village has electricity	0.50 (0.50)	0.60 (0.49)	0.55 (0.50)
Village price of rice	9.71 (0.97)	10.38 (1.68)	10.04 (1.41)

Table A: Summary statistics of explanatory variables (continued)

Variables	1991/92	1998/99	Whole sample
Village price of wheat flour	8.64 (1.37)	7.42 (0.85)	8.03 (1.29)
Village price of mustard oil	54.22 (4.45)	39.45 (4.04)	46.83 (8.52)
Village price of egg	2.34 (0.35)	1.89 (0.33)	2.11 (0.41)
Village price of milk	12.15 (3.20)	10.61 (3.10)	11.38 (3.24)
Village price of potato	8.63 (0.93)	7.03 (1.14)	7.83 (1.31)
Village male daily wage	35.35 (8.15)	44.56 (11.43)	39.95 (10.94)
Village female daily wage	19.59 (9.09)	25.71 (8.81)	22.65 (9.46)
No female wage in village	0.10 (0.30)	0 (0)	0.05 (0.22)

Note: Figures in parentheses are standard deviations.