Microfinance and Poverty: Evidence Using Panel Data from Bangladesh

Shahidur R. Khandker

Microfinance supports mainly informal activities that often have a low return and low market demand. It may therefore be hypothesized that the aggregate poverty impact of microfinance is modest or even nonexistent. If true, the poverty impact of microfinance observed at the participant level represents either income redistribution or short-run income generation from the microfinance intervention. This article examines the effects of microfinance on poverty reduction at both the participant and the aggregate levels using panel data from Bangladesh. The results suggest that access to microfinance contributes to poverty reduction, especially for female participants, and to overall poverty reduction at the village level. Microfinance thus helps not only poor participants but also the local economy.

Bangladesh has been a pioneer in the microfinance movement since its inception in the early 1980s and today is home to the most extensive microfinance operations in the world. In Bangladesh and elsewhere around the world, microfinance operations support mainly the poor and women engaged in informal activities. Microfinance involves small-scale transactions in credit and savings designed to meet the needs of small- and medium-scale producers and businesses. Microfinance programs also offer skill-based training to augment productivity and organizational support and consciousness-raising training to empower the poor. But even though microfinance has been the focus of development and poverty reduction activities for decades, development practitioners still know relatively little about the extent of poverty reduction possible through microfinance activities. This article seeks to shed some light on the question by

Shahidur R. Khandker is lead economist in the World Bank Institute's Poverty Reduction and Economic Management Division and the Development Research Group at the World Bank; his email address is skhandker@worldbank.org. This article is a revised version of a World Bank Discussion Paper of the same title (Khandker 2003). The article benefited from discussions with Gershon Feder, M. A. Latif, Mark Pitt, Martin Ravallion, and Binayak Sen and insightful comments from two anonymous referees and from Jaime de Melo and Alan Winters. The author is grateful to Hussain Samad for excellent research assistance and to the Bangladesh Institute of Development Studies research staff for help in collecting and processing data.

THE WORLD BANK ECONOMIC REVIEW, VOL. 19, NO. 2, pp. 263–286 doi:10.1093/wber/lhi008
Advance Access publication September 8, 2005

estimating the impact of microfinance on consumption poverty in Bangladesh using panel data from household surveys for 1991/92 and 1998/99.

Microfinance responds to the derived demand for borrowing to support self-employment and small business. Thus unlike other transfer schemes, it requires both entrepreneurial skill and a favorable local market. Without them, the returns to the investments financed by microfinance are likely to be small, and so, too, are any reductions in overall poverty. Even if the induced marginal gains from microborrowing are large for participants, the effect of the accrued total benefits on aggregate poverty are likely to be small, for several reasons. For one, microfinance transactions are too small to exert a large impact on aggregate poverty. For another, in an economy with low economic growth, borrowing may only redistribute income rather than boost growth.

Determining whether the benefits to program participants are sustainable and large enough to make a dent in the poverty of participants and society at large is important for guiding policy. Answering such questions requires an analysis of the dynamic consumption effects of microfinance interventions as well as village-level spillover effects on poverty reduction.

Khandker and Pitt (2003) examined the impacts of microfinance on a number of outcomes using panel household survey from Bangladesh. More specifically, they considered such issues as whether the effects of microfinance are saturated or crowded out over time, whether programs generate externalities, and whether the estimated impacts of microfinance found earlier with cross-section data analysis can be corroborated using an alternative method. They found a declining long-term effect of microfinance as well as the possibility of village saturation from microfinance loans.

This article uses the same household panel data to address related issues, focusing on the poverty reduction impact of microfinance. First, it examines whether household and individual factors such as land and education influence a household's demand for a loan in microfinance schemes where the decisions to borrow and how much to borrow are made by a group. Second, the article assesses whether microfinance reduces poverty and, if so, what the limits of poverty reduction through microfinance are. Third, it examines the spillover effects of microfinance, to determine whether the program benefits households beyond those that participate. Finally, it examines the aggregate poverty effect of microfinance to determine whether there is a society-level impact of microfinance on poverty reduction.

The article reviews the literature on microfinance and then discusses the econometric framework and the data. It estimates the demand for credit from a microfinance program based on both cross-sectional and panel data to determine whether individual and household factors matter in a household's demand for credit from a group-based microfinance program and reports the estimated effects of microfinance on the consumption of borrowers. It also discusses the spillover effects of microfinance programs and the effects on poverty reduction for both borrowers and society as a whole.

I. What We Already Know about Microfinance in Bangladesh

Microfinance organizations in Bangladesh, unlike their formal counterparts in the financial sector, have made great strides in delivering financial services (both savings and credit) to the poor, especially women, at very low loan default rates. Strategies such as collateral-free group-based lending and mobilization of savings, even in small amounts, have helped microfinance programs mitigate the problems that beset their formal counterparts, such as weak outreach and high loan default costs. However, the transaction costs are high for maintaining credit discipline among borrowers through group pressure and monitoring of borrowers' behavior, and programs have relied on donors to sustain their operations (Khandker 1998; Khalily and others 2000; Morduch 1999; Yaron 1994).

In Bangladesh such support is provided by the government and donors in the expectation that society benefits from such investments. In 1996 the World Bank provided a loan of $115 million to Palli Karma Shahayak Foundation (PKSF), an intermediary for wholesaling microfinance. The PKSF supports on-lending through small nongovernment organizations (NGOS) and a few large ones. This project was followed by a second project of $160 million in 2000. In 2000/2001 microfinance programs assisted some 10 million households (of a total of 30 million), with loans outstanding of about $1 billion. The organized NGO sector and Grameen Bank accounted for more than 86 percent of microfinance lending and commercial banks for just 14 percent.[1]

Because these government and donor resources have alternative uses from which the poor could also benefit, such as building community infrastructure, schools, and health facilities, it is important to know the extent of the socioeconomic impacts of microfinance. Are the impacts great enough to justify supporting microfinance over alternate uses? In particular, because women are disproportionately disadvantaged in countries such as Bangladesh and constitute the overwhelming majority of microfinance beneficiaries, an important policy question is whether women benefit from microfinance and, if so, how much? At the aggregate level the policy question is whether microfinance programs benefit nonparticipants or do they simply redistribute income in a society?

Despite differences in methodology, impact assessments show that microfinance in general helps the poor, although all participants may not benefit equally. An early study of Grameen Bank noted its support for the poor, especially women, through employment and income generation and improvements in social indicators (Hossain 1988). Some recent studies also find beneficial aspects of microfinance operations in Bangladesh (for example, Hashemi and others 1996).

1. There is a common misconception that Grameen Bank is an NGO. It is a specialized bank with its own charter approved by the government of Bangladesh.

The most comprehensive impact studies of microfinance, a joint research project of the Bangaldesh Institute of Development Studies (BIDS) and the World Bank, find strong evidence that the programs help the poor through consumption smoothing and asset building (Khandker 1998; Pitt and Khandker 1998). The findings support the claim that microfinance programs promote investment in human capital (such as schooling) and raise awareness of reproductive health issues (such as use of contraceptives) among poor families. The studies also shed light on the role of gender-based targeting and its impact on household and individual welfare, finding that microfinance helps women acquire assets of their own and exercise power in household decisionmaking.[2] The research project estimates that the marginal impact of microfinance on consumption was 18 percent for women and 11 percent for men (Pitt and Khandker 1998). The study finds that some 5 percent of borrowers may lift themselves out of poverty each year by borrowing from a microfinance program, if the estimated impacts on consumption continue over time (Khandker 1998). But even if this does happen, microfinance could lift less than 1 percent of the population out of poverty because it reaches only a quarter of the population.[3]

The robustness of these results still remains an issue, however, because impact studies are sensitive to the method applied. This article thus examines whether these findings can be substantiated by another method, such as panel data analysis. It looks at the long-run impacts of microfinance to see whether program impacts found in 1991/92 are sustainable over time. And if microfinance leads to poverty reduction at the borrower level, what is its impact on aggregate poverty?

II. Econometric Issues in Using Panel Data to Assess the Poverty Effects of Microfinance

Income or consumption poverty can be reduced through interventions such as microfinance that help the poor become self-employed and generate income. But efforts to assess the impact of microfinance programs can be biased by nonrandom program placement and participation. Antipoverty programs such as the

2. Morduch (1998) found either small or nonexistent program effects using the same 1991/92 BIDS–World Bank survey data. However, this study applied the difference-in-difference technique, which is suitable only for a randomized experimental study, whereas the BIDS–World Bank survey is of the quasi-experimental type and hence endogeneity of program participation is a serious issue. Morduch also pointed out that because of mistargeting (about 25 percent), the impacts shown in Pitt and Khandker (1998) were upper bound. However, a reexamination by Pitt (1999) showed that mistargeting was a nonissue in the estimated impacts and reconfirmed the earlier findings in Pitt and Khandker (1998), even after relaxing the targeting criteria and excluding mistargeted households from analysis.

3. If borrowers make up less than 25 percent of rural households, and if 5 percent of borrowers can move out of poverty, that means that 1 percent of households moved out of poverty in rural areas in each year due to microfinance. This may be high, given the fact that aggregate national level poverty estimates over time show that Bangladesh has managed to reduce poverty by 1 percent every year over the last decade (World Bank 2003).

Grameen Bank are often placed in areas where the incidence of poverty is high. Thus simply comparing the incidence of poverty in program and nonprogram areas may lead to the mistaken conclusion that microfinance programs have increased poverty. Similarly, those who participate may self-select into a program based on unobserved traits such as entrepreneurial ability. In that case, simply comparing such outcomes as per capita consumption or the incidence of poverty between program participants and nonparticipants may lead to the mistaken conclusion that the programs have a high impact on poverty reduction, when the effects are due to the unobserved abilities of participants. Thus the estimated effects may be under- or overestimated depending on the type of analysis.

The BIDS–World Bank study shows that the endogeneity of microfinance program placement and participation must be taken into account in estimations (Pitt and Khandker 1996, 1998). The study also shows that because the impacts vary by gender, that too needs to be taken into account. Looking only at the impact of borrowing by households is thus misleading. The method used in the study by Pitt and Khandker (1998) was based on cross-section data, with a quasi-experimental survey design to resolve problems of endogeneity associated with nonrandom program placement and self-selected participation.

In the quasi-experimental survey design, households were sampled in villages with and without a program, both eligible and ineligible households were sampled in both types of villages, and both program participants and nonparticipants were sampled among the eligible households in villages with microfinance programs. The two central underlying conditions for identifying program impact were the program's eligibility restriction and its gender-based program design. Any household with a landholding of less than half of an acre is eligible to participate in all microfinance programs in Bangladesh. One can identify the program impact on participants by distinguishing who participates and who does not from among those who are eligible to participate in a microfinance program.[4] Because men can join only men-only groups and women can only join women-only groups, the gender-based restriction is easily enforceable and thus observable, whereas the land-based identification restriction, for various reasons, may not be. Thus, if the land-based restriction is not observable, using the gender-based program design to identify the program effect by gender of participation is far more efficient.

Program effects are conditioned by how certain villages are selected for a program by drawing randomly both program villages and nonprogram villages. The villages are further classified by women-only and men-only groups, which

4. The landholding requirement is not strictly enforced, and so nontarget households are included as program participants. In that case the identification restriction in cross-sectional analysis based on landholding may lose efficiency, although that hypothesis is subject to testing. However, identification restriction does not apply to fixed-effects analysis using panel data, because the household fixed-effect method resolves any time-invariant participation-related endogeneity.

in turn helps identify program impacts by gender. But the issue remains why certain villages have women-only groups and others have men-only groups. Although the village-level fixed-effect method used with data from both program and nonprogram villages and from both male-only and female-only groups can resolve the endogeneity of program placement, another exogenous eligibility requirement is needed at the household level to determine why certain households and not others participate. However, there were no conditions that met the requirements of household-level exogenous eligibility conditions. The Pitt and Khandker (1998) study uses instruments such as the interaction of the land-based eligibility rule with household and village-level characteristics to identify the program effect. However, if the land-based condition is not strictly enforced, the interaction variables based on changing land holding may not be as efficient instruments as they would be if the condition were strictly enforced.

Given the sensitivity to the instruments used, there are compelling reasons to use alternative methods to demonstrate whether microfinance matters. The quasi-experimental survey design used by Pitt and Khandker (1998) is one of many methods evaluators use to assess program effects (for a review, see Moffitt 1991 and Ravallion 2001). One alternative is the household-level fixed-effect method using panel data. There are strong reasons for using a panel survey over a cross-sectional survey in impact analysis. Cross-section results may not be robust, with some studies showing that measurement of program impacts depends significantly on the method used to treat program endogeneity (see, for example, Lalonde 1986). It is important to assess the robustness of the results using a method that, unlike the quasi-experimental method used in the Pitt and Khandker (1998) study, is less reliant on the landholding eligibility rule, which is not strictly enforceable but was nonetheless used in the cross-sectional analysis. Household-level panel data are thus used here to analyze the impact of borrowing on consumption and hence on poverty. To show how panel data can be used to estimate program effects, assume the following reduced-form borrowing (S_{ijt}) by the ith household living in the jth village in period t,[5]

$$S_{ijt} = X_{ijt}\lambda + \eta_{ij}{}^{s} + \mu_j^s + \epsilon_{ijt}{}^{s}, \tag{1}$$

where X is a vector of household-, village-, and group-level characteristics (such as age and education of household head), λ is a vector of unknown parameters

5. References to borrowing or credit and to the cumulative stock of borrowing since the start of program participation. Separate borrowing equations are not shown for men and women. This is subject to testing the restrictions on equality of parameters of the credit demand equations of men and women. A priori differential credit demand is expected for men and women. In a sex-segregated society, the credit constraints faced by women are expected to vary substantially from those faced by men. There is also a possibility that the demand for credit varies by source of finance such as Grameen Bank, BRAC, RD-12, or other NGOS. This is also tested.

to be estimated, η is an unmeasured determinant of credit demand that is time-invariant and fixed within a household (it also includes unobserved group characteristics), μ is an unmeasured determinant of credit demand that is time-invariant and fixed within a village, ε is nonsystematic error, and the superscript s refers to unobserved error terms specific to the credit-demand equation.[6]

Current outcome (say, consumption) is assumed to depend on both current and past characteristics, including borrowing. So the conditional demand for consumption (C_{ijt}) in period t is given as,[7]

$$C_{ijt} = X_{ijt}\alpha + X_{ij(t-1)}\beta + S_{ijt}\delta + S_{ij(t-1)}\gamma + \eta_{ij}{}^{c} + \mu_j^c + \epsilon_{ijt}{}^{c}, \tag{2}$$

where δ and γ measure the effects of current and past credit (stock), and superscript c refers to the error terms specific to the consumption equation. According to equation 2, the return to consumption in any given period (say, 1998/99) is the sum of returns from past credit (say, 1991/92) and current credit (1998/99). So this model assumes that even if current credit (S_{ijt}) is zero (that is, a household stopped borrowing after period 1), past credit ($S_{ij(t-1)}$) may continue to benefit the borrower ($\gamma > 0$). Therefore, allowance is made for differential impacts of borrowing over time.[8]

The impact of credit on household consumption can be measured by estimating equation 2. However, household demand for credit as given in equation 1 needs to be estimated jointly with equation 2. Using cross-section data ($t = 1$) raises the endogeneity of equation 2 with respect to equation 1 as a result of possible correlation of errors in the borrowing and consumption equations (Pitt and Khandker 1998). Although estimation of the credit impact on consumption is observable if consumption equation 2 includes variables that are not included in equation 1, or vice versa, this cannot happen under

6. Very few households change groups in the village over the period of borrowing, so that group effects are picked up by the unmeasured village-level effects.

7. Including borrowing in the dynamic consumption equation 2 can be justified by modifying the Ramsey consumption growth model and allowing the 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 is mentioned as a factor in consumption growth in China in Jalan and Ravallion 2002). Under the assumption that households are credit constrained, the marginal product of capital depends on borrowing, given by the rate of return on borrowing $r(B)$. An optimization of consumption over time subject to production constraints can lead to 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. The error terms are assumed to include the subjective rate of time preference and rate of depreciation, which may be household specific or area specific.

8. The credit demand function (equation 1) is a reduced-form equation. However, it could have been allowed to depend on past characteristics (lagged model) in addition to current characteristics. But that change in first-stage specification does not change the consistency of the second-stage consumption equation. Moreover, if the household fixed-effect method is used for the consumption equation in panel analysis, the first-stage credit equation becomes a nonissue.

normal circumstances. Thus estimation of equation 2 cannot be separated from that of equation 1.

Unlike the Pitt and Khandker (1998) study, which uses a village-level fixed-effect method with an instrumental variable to resolve program placement endogeneity and household-level endogeneity using the 1991/92 cross-sectional data, this study uses panel data for households with more than one observation ($t > 1$) to estimate program effects without using an instrumental variable method. This is done by estimating a household-level fixed-effects model, which resolves both household- and village-level endogeneity, based on the assumption that the error terms of the credit demand equation and consumption equation are uncorrelated, that is, $\mathrm{Corr}(\epsilon^{s}{}_{ii,}\epsilon^{c}{}_{ij}) = 0$.

A basic assumption of the household fixed-effect method is that the unobserved factors at the household and village level remain fixed over time. But these unobserved factors may change for various reasons. For example, unobserved household income, which may condition credit demand, may increase temporarily so that with a larger cushion against risk, households may be willing to assume more loans. Similarly, the unobserved local market conditions that influence a household's demand for credit may change over time, exerting a more favorable impact on credit demand. Equations 1 and 2 can be rewritten to incorporate variation in η and μ over time:

$$(1') \qquad S_{ijt} = X_{ijt}\lambda + \eta^{s}{}_{ijt} + \mu^{s}{}_{jt} + \epsilon^{s}{}_{ijt}$$

$$(2') \qquad C_{ijt} = X_{ijt}\alpha + X_{ij(t-1)}\beta + S_{ijt}\delta + S_{ij(t-1)}\gamma + \eta_{ijt}{}^{c} + \mu_{jt}{}^{c} + \epsilon_{ijt}{}^{c}.$$

Because the household-level fixed-effect method also resolves any village-level endogeneity, the credit and consumption equations can be simplified by omitting village-level unmeasured determinants (μ):

$$(3) \qquad S_{ijt} = X_{ijt}\lambda + \eta^{s}{}_{ijt} + \epsilon^{s}{}_{ijt}$$

$$(4) \qquad C_{ijt} = X_{ijt}\alpha + X_{ij(t-1)}\beta + S_{ijt}\delta + S_{ij(t-1)}\gamma + \eta_{ij}{}^{c} + \epsilon_{ijt}{}^{c}.$$

But the household fixed-effect method that controls for fixed unobserved attributes of households participating in microfinance programs may still not yield consistent estimates of the credit effects with panel data for two reasons. The unmeasured determinants of credit at both household and village levels may vary over time, and if credit is measured with errors (which is likely), the error gets amplified when differencing over time, especially with only two time periods. This measurement error will impart attenuation bias to the credit impact coefficients, biasing the impact estimates toward zero. A standard correction for both types of bias (one due to measurement error and one to time-varying heterogeneity in credit demand) is the reintroduction of instrumental variable estimation. The instruments can be similar to those applied in the cross-section analysis of Pitt and Khandker (1998).[9] This requires testing whether the household-level fixed-effect or the household-level fixed-effect instrumental variable method is appropriate in estimating household consumption behavior.

III. The Data and Their Characteristics

The BIDS–World Bank 1991/92 survey covered 1,798 households drawn from 87 villages in 29 thanas. Eight program thanas were drawn randomly from the project areas of brac, Grameen Bank, and the Bangladesh Rural Development Board's (BRDB) Rural Development 12 (RD-12) program; five nonprogram thanas were also drawn randomly. Three villages were drawn randomly from each thana in which the programs had been in operation for at least three years. The survey was conducted three times during 1991/92, during the three cropping seasons: round 1 during the Aman rice season (November–February), round 2 during the Boro rice season (March–June), and round 3 during the Aus rice season (July–October). However, because of attrition, only 1,769 households of the original 1,798 were available in the third round.

A follow-up survey conducted in 1998/99 included the same households but also added new households from the original villages, new villages in the original thanas, and three new thanas, raising the number of sample households to 2,599. Because this study relies on panel data to assess the impact of program participation, the study sample was restricted to the 1,638 households that were interviewed in both periods. Of the original group of 1,769 households,[10] 237 households had split into 546 households in 1998/99, resulting in 1,947 households. To maintain a one-to-one correspondence among matching households, the split households were treated as a single household in the resurvey data. Tests conducted to determine whether this merger was appropriate found no statistical difference in results between samples with merged and separated households.[11]

9. The purpose for using these instruments was different in the Pitt and Khandker (1998) study, which used it to correct for the endogeneity of program participation by a household member. In contrast, these instruments are used in the panel data analysis to correct for time-varying heterogeneity and measurement errors associated with credit variables.

10. The issue of sample household dropout (attrition) between survey periods is discussed in Khandker and Pitt (2003). The attrition rate was 7.4 percent (from 1,769 households to 1,638), which is quite low. Several studies (Alderman and others 2000; Fitzgerald and others 1998; Thomas and others 1999; Ziliak and Kniesner 1998) have shown that attrition bias is not a big issue as long as it is random. Khandker and Pitt (2003), however, formally tested for attrition bias and found that it can be ignored in a majority of outcomes.

11. See Khandker and Pitt (2003) for the details of the test.

Of the 1,638 panel households used in the analysis, 25.8 percent of those in the 1991/92 survey were program participants, 38.0 percent were eligible nonparticipants, and 36.2 percent were nontarget households.[12] In the 1998/99 resurvey 52.7 percent of households were program participants, 20.1 percent were eligible nonparticipants, and 27.3 percent were nontarget households.[13] More than 95 percent of the 1991/92 participants were still with the microfinance programs in 1998/99. A major shift occurred among the eligible nonparticipants, with 47 percent joining a microfinance program by 1998/99. Among the nontarget households observed in 1991/92, some 28 percent joined a microfinance program by 1998/99.

Examination of program participation rates by landholding shows participation to be higher among target households (with 50 decimals of land or less) than among nontarget households in both periods (table 1). But program participation among nontarget households indicates potential mistargeting. The extent of mistargeting has increased, rising from about 25 percent of program borrowers in 1991/92 to 31 percent in 1998/99.

Nonetheless, the extreme poor (households with 20 decimals of land or less) constituted a majority of participants (about 60 percent in 1991/92 and 54 percent in 1998/99).[14] Overall, the participation rate more than doubled between the surveys. The increase is significant even if program attrition is

TABLE 1. Program Participation Rate for Household Landholding Groups, 1991/92 and 1998/99 (%)

Landholding (decimals)	1991/92	1998/99
0	58.8 (8.4)	52.0 (5.3)
1–20	33.0 (51.6)	67.6 (49.1)
21–50	29.8 (15.3)	59.5 (14.7)
51–100	23.9 (9.7)	48.4 (10.9)
101–250	16.0 (11.4)	42.3 (13.8)
251+	6.8 (3.6)	22.4 (6.3)
All households	25.9 (100)	52.5 (100)
Number of observations	1,638 (824)	1,638 (1,104)

Note: Numbers in parentheses are the share of total program participants from each landholding group, except in the last row where they are the number of program participating households.

Source: Author's computations based on 1991/92 and 1998/99 household surveys in Bangladesh.

12. The sample households were drawn using a proportionate random distribution. Hence, the analysis uses weights both in the descriptive statistics and the econometric analysis.

13. Program participants in 1998/99 included members of such programs as Association for Social Advancement (ASA), PROSHIKA, Youth Development Program, Gano Shahajyo Sangstha (GSS), and some small local NGOS in addition to Grameen Bank, BRAC, and BRDB RD-12 members. They also included members of multiple programs.

14. Landholding is considered a proxy of household wealth and poverty in rural Bangladesh. Poverty can also be measured by the level of consumption.

considered (the annual dropout rate went from 5.5 percent in 1991/92 to 3.5 percent in 1998/99).[15]

Summary statistics for individual and household-level borrowing and consumption outcomes show that while average male borrowing for participating households declined from 3,472 taka (Tk) to Tk 2,483 in real terms, or by 28.5 percent over the seven-year period, average female borrowing for participating households increased by 94 percent in real terms (table 2). This suggests that microfinance programs provided loans mainly through female members of poor households, with female borrowing on average accounting for 82 percent of microfinance borrowing in 1998/99, up from 63 percent in 1991/92. The average loan size was higher in real terms for mistargeted borrowers (Tk 10,075) than for targeted borrowers (Tk 8,634) in 1991/92 (not shown in table 2), but slightly lower for mistargeted borrowers (Tk 12,606) than for targeted borrowers (Tk 12,728) in 1998/99. Mistargeted households constituted some 23 percent of borrowers and received some 25 percent of total credit supplied by microfinance programs.

Total household annual per capita expenditure grew by 30.5 percent over the seven-year period for all households compared with 34.6 percent for program participants. This is equivalent to a real increase of some 5 percent annually for program participants. How much of the change in consumption was due to borrowing from microfinance programs, and what were the impacts on poverty reduction? This is discussed in section VII.

IV. What Determines Demand for Loans from a Group-Based Microfinance Program?

The demand for credit is determined, as specified in equation 1, by a host of factors at the household, village, and group level, including physical endowments (such as land) and human capital (such as education), given the availability of the program in a village and the nature of group decisionmaking involved in individual lending. One testable hypothesis is whether the demand for microfinance differs by gender. Because credit markets are imperfect and labor markets are different for men and women, demand for credit is expected to differ by gender. An *F*-test for the equality of credit by gender rejects the hypothesis, so different demand equations are fitted for male and female borrowers.

A village-level fixed-effect method is applied to equation 1 based on the cross-section data for 1991/92 and 1998/99 and assuming no unmeasured household-level determinants of credit. If unobserved group characteristics are part of village-level unobserved factors, such a method would help eliminate the influence of the unmeasured village-level demand for credit by men and women

15. The dropout rate is defined as the proportion of past members that are no longer members of any microfinance program.

TABLE 2. Summary Statistics of Consumption and Credit Variables (1991/92 taka)

Variable	Participants	Target Nonparticipants	Nontarget Nonparticipants	All House holds
1991/92				
Household-level male borrowing	3,472.3	0	0	796.6
	(6,829.2)			(3,678.4)
Household-level female borrowing	5,852.9	0	0	1,582.8
	(8,037.9)			(4,974.4)
Village-level average male borrowing				1,746.7
				(2,774.1)
Village-level average female borrowing				2,944.3
				(3,612.8)
Household annual per capita total expenditure	3,910.3	3,790.9	5,635.2	4,452.4
	(1,585.6)	(1,677.8)	(3,665.5)	(2,554.7)
Household annual per capita food expenditure	3,051.1	2,965.5	3,704.7	3,237.3
	(794.7)	(878.6)	(1,123.1)	(987.4)
Household annual per capita nonfood expenditure	859.2	825.3	1,930.5	1,215.1
	(1,102.1)	(1,061.0)	(3,086.2)	(1,957.5)
Number of observations	824	535	279	1,638
1998/99				
Household-level male borrowing	2,483.1	0	0	1,088.0
	(9,013.3)			(5,790.7)
Household-level female borrowing	11,348.4	0	0	5,581.0
	(17,592.3)			(13,392.4)
Village-level average male borrowing				1,673.6
				(3,717.3)
Village-level average female borrowing				7,648.7
				(8,392.8)
Household per capita annual total expenditure	5,264.0	4,503.7	7,214.4	5,810.1
	(3,580.2)	(2,663.8)	(5,789.3)	(4,502.5)
Household annual per capita food expenditure	3,550.1	3,305.3	4,374.3	3,753.4
	(1,335.3)	(1,506.2)	(2,189.0)	(1,687.8)
Household annual per capita nonfood expenditure	1,713.9	1,198.4	2,840.1	2,056.7
	(2,848.2)	(1,579.2)	(4,570.6)	(3,575.1)
Number of observations	1,104	292	242	1,638

Note: Numbers in parentheses are standard deviations.
Source: Author's computations based on 1991/92 and 1998/99 household surveys in Bangladesh.

who form groups for the group-based microfinance programs. However, if unobserved household-level factors also contribute to group formation and group characteristics, then the village-level fixed-effect estimates of the credit demand function will not be unbiased. In that case, a household fixed-effect method using household-level panel data may be more efficient.

The estimated results of demand equations for men and women show that the errors in the credit demand equations for men and women are not correlated for cross-section data for either 1991/92 or 1998/99, whereas they are correlated for panel data (table 3). Therefore, the panel data analysis of the demand functions uses the household fixed-effect method with the correction for the nonzero covariance of the errors of men's and women's credit demand equations.

The results confirm that households that are resource poor, especially in land, demand more loans from microfinance programs than households that are resource rich. This means that landless households are likely to receive more loans from microfinance programs than landed households are. The results for

TABLE 3. Determinants of Microfinance Demand by Men and Women

	Village Fixed Effects				Household Fixed Effects	
	1991/92		1998/99		Panel Data	
Explanatory Variable	Log of Men's Loans	Log of Women's Loans	Log of Men's Loans	Log of Women's Loans	Log of Men's Loans	Log of Women's Loans
Maximum education of household male (years)	0.069	−0.046	0.021	0.020	0.016	−0.009
Maximum education of household female (years)	−0.056	−0.076	0.007	−0.108**	0.002	−0.049*
Log of household land (decimals)	−0.202**	−0.308**	−0.225**	−0.533**	−0.058	−0.338**
F-statistics	11.196	14.768	12.876	25.457	2.916	4.511
Number of observations	1,638	1,638	1,638	1,638	1,638	1,638
F-statistics (H_0: parameters of men's and women's borrowing equations are jointly equal to 0)	7.75		11.02		4.10	
Prob > *F*	0.0000		0.0000		0.0000	
Breusch-Pagan test of independence of male and female borrowing: χ^2 (1)	3.082		0.034		22.13	
Prob > χ^2	0.0792		0.853		0.0000	

**t*-statistic is significant at the 10 percent level or better.
***t*-statistic is significant at the 5 percent level or better.

Note: Regression also includes the following variables: sex; age and education of household head; whether parents, brothers, and sisters of household's head or head's spouse own land; year; and village level infrastructure and price variables to reflect the impact of time-varying changes in local economic conditions.

Source: Author's computations based on 1991/92 and 1998/99 household surveys in Bangladesh.

the panel data analysis show that a 10 percent increase in landholding from an average of 137 decimals of land reduces the total amount of borrowing by 3.4 percent for women's borrowing and has no effect on men's borrowing. Findings are similar with cross-sectional demand analysis. Moreover, even if landholding determines group formation and consequently an individual's demand for credit, the education of household members also affects demand for credit. More specifically, female education has a negative effect on the amount of borrowing from microfinance programs. One additional year of female education reduces the amount of female borrowing by less than 1 percent in the panel data analysis and by more than 1 percent in 1998/99 in the cross-sectional demand analysis. Although having the same sign, the coefficients are smaller in the panel demand analysis than in the cross-sectional analysis.

The results thus suggest that even in a group-based system in which demand for microfinance is largely derived from landholding eligibility conditions, human capital (education) matters in deciding how much a particular household borrows from a group-based microfinance program.

V. Consumption Effect of Microfinance

Poverty reduction is an overarching objective of targeted microfinance programs. Because poor people with little education and land are more likely to participate in microfinance programs than are other groups, impact assessments of microfinance and poverty reduction should assess whether program participation increases household consumption over its level before program participation. Because program participation varies by such observed and unobserved attributes as landholding and education, the level of consumption also varies by these same attributes, so that demand for credit and consumption are jointly determined.

The panel household survey data provide a way of controlling for the joint determination of consumption and credit demand and provide a framework for measuring the impact of credit on consumption using the household fixed-effect method. However, as mentioned, a test is needed to determine whether a simple fixed-effect method or a fixed-effect method with instrumental variables is more appropriate.

For the instrumental variable method within the household fixed-effect structure, the first-stage equation for the stock of credit (suppressing sub- scripts for male, female, household, and village) can, similar to equation 3, be written as:

$$S_t = X_t\lambda + Z_t\zeta + \epsilon^s{}_t, \tag{5}$$

where Z is a set of household and village characteristics distinct from household characteristics (X) so that they affect S but not household per capita consumption conditional on S.

Selecting appropriate Z variables is a crucial part of this exercise. A household-level choice variable is defined that determines whether a household has a choice of participating in a program. A household's choice depends on two factors: whether a microfinance program operates in the village where the household lives and whether the household qualifies to participate in the program based on the landholding criteria. The choice variable is considered for both 1991/92 and 1998/99 to take care of the differential impacts of the two periods and is then interacted with household-level exogenous variables and village fixed-effects to get the instruments.[16]

Before consumption equation 4 is estimated, tests are carried out on the equality of credit sources (the null hypothesis is that men's loans from different sources are equal and women's loans from different sources are equal) and on the equality of the gender of borrowers (the null hypothesis is that men's and women's loans are equal). The results (not shown here) indicate that at the 5 percent significance level, the hypotheses of the equality of sources cannot be rejected in five of six cases (it can be rejected only for per capita food expenditure in 1991/92, where $F(16, 1{,}572) = 2.44$, $p > F = 0.024$). Thus credit from all sources can be lumped together. The second set of tests indicates that the equality of the gender of borrowers can be rejected for per capita total expenditure [$F(2, 1{,}582) = 6.07$, $p > F = 0.002$] and food expenditures [$F(2, 1{,}582) = 9.85$, $p > F = 0.001$], but not for nonfood expenditures [$F(2, 1{,}582) = 2.17$, $p > F = 0.115$]. This suggests that borrowing by men and women cannot be combined. So the effects of credit pooled from various sources are estimated separately for men and women.

A specification test (Wu-Hausman test) is performed to determine which is more appropriate for estimating the consumption effects of borrowing from microfinance programs: the household-level fixed-effect method or the household-level fixed-effect with instrumental variable method, which depends on an alternative specification that suggests that the time-varying errors that affect credit demand have separate effects on that demand.[17] The test result (not shown here) for per capita consumption suggests that the credit volume as used in the fixed-effect method is not endogenously determined by factors such as

16. Unlike the case in 1991/92, there was no village without a program in 1998/99. Hence, there was no control village and no village-specific choice. Yet households have a choice based on the landholding eligibility condition.

17. Here the null hypothesis is that both fixed-effect and fixed-effect with instrumental variable estimates are consistent, and the alternate hypothesis is that only the fixed-effect with instrumental variable estimate is consistent. If the null hypothesis is true the fixed-effect model should be used because it is more efficient. Otherwise, the fixed-effect with instrumental variable model would be used. For the Wu-Hausman test credit variables are regressed on only instruments using the fixed-effect model, predicted credit variables are saved as fitted, and the second-stage equations are estimated using the fixed-effect model including both original and fitted credit variables as regressors. Then the test is run to determine if the coefficient on the fitted variables is zero (null hypothesis).

the time-varying heterogeneity or the measurement errors associated with credit variables.[18]

The household-level fixed-effect results suggest that male borrowing has no significant effect, while female borrowing has a significant positive effect on per capita consumption outcomes (table 4).[19] Based on the fixed-effect estimation, a 10 percent increase in the current stock of female borrowing increases household total expenditure by 0.09 percent and the same increase in the past stock of female borrowing increases per capita consumption by 0.10 percent.[20] Similar positive and significant impacts of stocks of female borrowing are also evident for household food and nonfood expenditure.

These fixed-effect estimates are used to calculate the marginal returns to borrowing for men and women (table 5). The marginal return estimates for women are used as the basis for calculating the impact of borrowing on poverty reduction presented later. At the mean an additional Tk 100 of cumulative borrowing by women during 1991/92 adds almost Tk 15 to total annual household expenditure—Tk 7 to food expenditure, and Tk 8 to nonfood expenditure.

TABLE 4. Household Fixed-Effects Estimates of the Impact of Microfinance Loan

Credit Variables	Log of Household per Capita Yearly Expenditure	Log of Household per Capita Yearly Food Expenditure	Log of Household per Capita Yearly Nonfood Expenditure
Log of men's current loans	−0.002	−0.005	0.008
Log of women's current loans	0.009**	0.006**	0.018**
Log of men's past loans	−0.004	−0.003	−0.005
Log of women's past loans	0.010**	0.008**	0.014**
Number of observations	1,638	1,638	1,638
F-statistics (56, 1582)	9.92	9.45	9.27

***t*-statistic is significant at the 5 percent level or better.

Note: Regression also includes the following variables: sex; age and education of household head; whether parents, brothers, and sisters of household's head or head's spouse own land; year; and village level infrastructure and price variables to reflect the impact of time-varying changes in local economic conditions.

Source: Author's computations based on 1991/92 and 1998/99 household surveys in Bangladesh.

18. The response elasticity is somewhat higher with the fixed-effect with instrumental variable model than with fixed-effect model estimates, although the significance levels are similar for both estimates, especially for female borrowing.

19. The estimated model is logarithmic function; hence, the coefficients of credit variables measure the response elasticity. Regressions are estimated using log-log models, due to possible highly skewed data and heteroskedasticity. Using a log-log model reduces the importance of high-value outliers and makes errors more homoskedastic.

20. During 1991/92 current borrowing is the stock at that time and past borrowing is zero (because 1991/92 is the first data point in the panel). During 1998/99 current borrowing is the stock in 1998/99 and past borrowing is the stock from 1991/92.

TABLE 5. Marginal Returns to Microfinance Loan Based on Household Fixed-Effects Estimates (taka per 100 taka in borrowing)

Gender and Period	Household Yearly Total Expenditure	Household Yearly Food Expenditure	Household Yearly Nonfood Expenditure
Women's borrowing			
Returns in 1991/92	14.7**	7.1**	8.0**
Returns in 1998/99	20.5**	11.3**	9.2**
Men's borrowing			
Returns in 1991/92	−6.5	−11.8	7.1
Returns in 1998/99	−16.6	−12.9	0.6

***t*-statistic is significant at the 5 percent level or better.

Note: Because the estimation equations are in log-log (elasticity) form, marginal returns are calculated using the formula, $dY/dX = \beta(\overline{Y}/\overline{X}$, where $\overline{Y}$ and $\overline{X}$ are sample means of Y (household expenditure) and X (women's credit, for example). Household expenditure figures are obtained by multiplying household per capita expenditure by household size (5.8 for the sample). The return in 1991/92 includes that from current credit only (because in 1991/92 past credit is zero), and the return in 1998/99 includes that from both current (4.2 percent in 1998/99) and past (16.3 percent in 1991/92) credit.

Source: Author's computations based on 1991/92 and 1998/99 household surveys in Bangladesh.

An additional Tk 100 in women's stock of credit during 1998/99 increases the household's total annual expenditure by almost Tk 21—food expenditure by Tk 11.3, and nonfood expenditure by Tk 9.2.

By assumption there was no borrowing before 1991/92 because the data start in 1991/92. For 1991/92, then, the contribution to marginal returns to current consumption comes only from current borrowing, which is 14.7 percent. For 1998/99, however, the contribution is the sum of impacts from both current borrowing (4.2 percent) and past borrowing (16.3 percent).[21] Thus the panel data analysis show a lower return for 1991/92 and a higher return for 1998/99 than the earlier Pitt and Khandker (1998) results for 1991/92 based on cross-sectional analysis, which show an 18 percent impact of women's borrowing on total consumption. The marginal returns to men's borrowing were also calculated. In most cases they are small and insignificant—effectively, the returns to male borrowing are zero. This may be because these programs have always emphasized women, and as a result men have lagged behind in both membership and loan volume, with the discrepancy increasing over time.

One concern is that program participants include noneligible households that own more than 50 decimals of land, which may bias the estimated coefficients of credit

21. This shows that return to cumulative borrowing in a particular year diminished over the years. For example, returns to current borrowing on current consumption dropped from 14.7 percent in 1991/92 to 4.2 percent in 1998/99. Diminishing returns are also obvious from the fact that in 1998/99 the return to current consumption is 16.3 percent from past borrowing (1991/92) and 4.2 percent from current borrowing (1998/99).

(Morduch 1998). To see how robust the estimated effects are, the impacts were reestimated by excluding mistargeted households (following Pitt 1999). The results (not shown here) suggest that the estimated coefficients are quite robust—they are not sensitive to whether mistargeted households are included as program participants.

VI. Spillover Effects of Microfinance

The results have shown that microfinance has a large impact on the welfare of borrowing households by raising consumption among program participants. Are there spillover effects that are felt beyond the program participants? Loans outstanding for microfinance organizations in Bangladesh totaled about \$600 million in 1998/99. This large inflow of microfinance to rural areas is expected to have an aggregate impact on the local economy.

Panel data are needed to estimate any spillover effects. When there are spillover effects, unobserved village heterogeneity can be correlated with program placement, with causation going from program placement to unobserved village effects, not from village effects to program placement. This measurement problem implies that the placement of a microfinance program may cause a village effect additional to any preexisting (time-invariant) village effects.

Omitting unmeasured village effects, as before, a consumption equation that captures village spillover can be written as:

$$(6) \qquad C_{ijt} = X_{ijt}\alpha + X_{ij(t-1)}\beta + S_{ijt}\delta + S_{ij(t-1)}\gamma + \Omega_{jt}\pi + \Omega_{j(t-1)}\rho + \eta_{ijt}{}^{c} + \epsilon_{ijt}{}^{c}$$

where the Ω_j terms represent the external village effects of a program (with a value of zero if no program is located in the village), and η_{ij} is the unobserved household-level fixed effect. The program-effect parameters, δ and γ, capture all program effects only if $\Omega_j = 0$ (none of the village-specific heterogeneity is caused by the program). If village externalities exist ($\Omega_j \neq 0$), the spillover effect is not separately identified from the time-invariant village effect. If the Ω_j terms are measured by the average value of all microfinance borrowing in a village, then the spillover effect is measured by the change in behavior of nonparticipants due to a change in village-level average microfinance borrowing, captured by π and ρ.

Equation 6 is estimated by the fixed-effect method that eliminates program placement bias. Both the consumption and the credit variables are in logarithmic form, and thus the credit coefficients measure response elasticities. The benefits for nonparticipants depend on the amount of credit obtained by all program borrowers living in a village, as measured by the average value of credit obtained by all households living in a village.

The village averages of women's current and past borrowing have significant positive impacts on per capita expenditure of an average household of a village. A 10 percent increase in the village average of women's current borrowing from microfinance programs increases household per capita total expenditure by 0.68 percent, food expenditure by 0.50 percent, and nonfood expenditure by 0.97

percent (table 6). Similarly a 10 percent increase in the village average of women's past borrowing from microfinance programs increases household per capita total expenditure by 0.69 percent, food expenditure by 0.45 percent, and nonfood expenditure by 1.19 percent.

A 10 percent increase in women's individual current borrowing increases borrowing household's per capita expenditure by 0.06 percent and the same increase in women's past borrowing increases per capita expenditure by 0.05 percent. Men's borrowing has no such effects.

The positive spillover effects suggest that microfinance programs have influenced the welfare not only of poor participants but also of nonparticipants. The total effect of a program is then a sum of the effects for participants and nonparticipants.[22]

TABLE 6. Household Fixed-Effects Estimates of Village Spillover Effects of Microfinance Loans

Credit Variables	Log of Household per Capita Yearly Expenditure	Log of Household per Capita Yearly Food Expenditure	Log of Household per Capita Yearly Nonfood Expenditure
Log of men's current loans	−0.001	−0.005	0.004
Log of women's current loans	0.006*	0.004*	0.014*
Log of men's past loans	−0.002	−0.002	−0.004
Log of women's past loans	0.005*	0.006**	0.004
Log of village average of men's current loans	0.0007	−0.004	0.024
Log of village average of women's current loans	0.068**	0.050**	0.097**
Log of village average of men's past loans	0.002	0.004	0.010
Log of village average of women's past loans	0.069**	0.045**	0.119**
Number of observations	1,638	1,638	1,638
F-statistics (60, 1578)	9.73	9.15	9.16

**t*-statistic is significant at the 10 percent level or better.

***t*-statistic is significant at the 5 percent level or better.

Note: Regression also includes the following variables: sex; age and education of household head; whether parents, brothers, and sisters of household's head or head's spouse own land; year; and village level infrastructure and price variables to reflect the impact of time-varying changes in local economic conditions.

Source: Author's computations based on 1991/92 and 1998/99 household surveys in Bangladesh.

22. This is, however, not a simple algebraic aggregation. According to this estimate, participants benefit from both own effects and spillover, whereas nonparticipants benefit only from spillover. A 10 percent increase in women's current loans increases participants' per capita expenditure directly by 0.06 percent and through spillover by 0.034 percent (coefficient of the village average of women's borrowing is 0.068 which, for a 20-household village, translates into a 0.034 percent increase at the household level). Nonparticipants' per capita expenditure increases by 0.034 percent because of the spillover. As nonparticipants constitute roughly about 48 percent of the village population during the 1998/99 survey period, a 10 percent increase in women's current borrowing increases per capita expenditure of an average household by 0.065 percent ($=0.52*0.094+0.48*0.034$).

VII. Poverty Effects of Microfinance

Data on consumption and the consumption poverty line show that moderate poverty in the sample villages declined overall by 17 percentage points between 1991/92 and 1998/99 and extreme poverty by 13 percentage points (table 7). The follow-up survey provides a means of gauging the extent of mistargeting of microfinance programs based on consumption poverty, when mistargeting is defined as program participation by households that are not poor based on their consumption. For households in nonprogram areas in 1991/92 that joined microfinance programs after the 1991/92 survey, the average incidence of poverty was 90.8 percent before they joined the

Table 7. Poverty Status (Headcount) by Program Participation Status and Survey Period

	Moderate Poverty		Extreme Poverty	
Program Participation Status[a]	1991/92	1998/99	1991/92	1998/99
Program villages				
Program participants (targeted)	93.0**	75.5**	57.3**	36.8**
Program participants (mistargeted)	82.3**	57.2**	37.8**	22.9**
All program participants	90.3**	70.1**	52.5**	32.7**
Target nonparticipants	91.1**	72.0**	58.9**	44.0**
Nontarget, nonparticipants	69.8**	50.8**	23.6	19.3
Total	83.7**	65.5**	45.0**	31.4**
Nonprogram villages				
Program participants (targeted)[b]	(89.3)*	79.0*	(60.2)	51.6
Program participants (mistargeted)[b]	(93.0)**	61.2**	(51.4)	32.8
All program participants[b]	(90.8)**	71.6**	(56.6)*	43.8*
Target nonparticipants	87.4	82.9	57.0	51.2
Nontarget, nonparticipants	72.7	53.2	35.5	26.0
Total	80.3**	67.7**	46.6*	38.3*
All villages				
Program participants (targeted)	93.0**	75.8**	57.3**	38.5**
Program participants (mistargeted)	82.3**	57.9**	37.8**	24.7**
All program participants	90.3**	70.3**	52.5**	34.2**
Target nonparticipants	90.2**	75.1**	58.4**	45.5**
Nontarget, nonparticipants	70.5**	50.5**	26.3	20.0
Total	83.1**	65.8**	45.3**	32.6**

*Change in poverty from 1991/92 to 1998/99 is significant at the 10 percent level or better.

**Change in poverty from 1991/92 to 1998/99 is significant at the 5 percent level or better.

[a]Program participation status is based on program placement in 1991/92. By 1998/99 all sample villages that had not had programs in 1991/92 had programs.

[b]There were no program participants in nonprogram villages in 1991/92. Figures in parentheses show headcount in 1991/92 for households in those villages that became program participants in 1998/99 (whose headcount in 1998/99 is shown in next right cell).

Source: Author's computations based on 1991/92 and 1998/99 household surveys in Bangladesh.

microfinance programs. Thus, only 9 percent of households were mistargeted based on consumption poverty.[23]

Poverty reduction is substantial among both program participants and non-participants and in both program and nonprogram areas. The net reduction in moderate poverty is about 18 percentage points in program areas, 13 percentage points in nonprogram areas, and 17 percentage points overall between 1991/92 and 1998/99. Did microfinance play a role?

The substantial reduction in poverty (19 percentage points) after 1991/92 among participants in previously nonprogram areas suggests that microfinance programs were successful in reducing village-level poverty, even with the possibility of spillover effects. Extreme poverty also dropped over this period, but the reduction in nonprogram areas is not statistically significant in most cases.

Poverty rates declined by more than 20 percentage points over the seven years among the households that were program participants in 1991/92—about 3 percentage points a year. How much of this reduction is due to microfinance? To quantify that contribution, consumption estimates for program participants and the village as a whole are used. The marginal returns to household consumption on women's loans (see table 5) are used to calculate the change in per capita consumption due to microfinance borrowing.[24] This change in per capita consumption is subtracted from the current consumption of participants to get a preborrowing level of consumption,[25] which is then used to derive a preborrowing level of poverty. Poverty reduction for nonparticipants can be calculated similarly from table 6, first by calculating village-level marginal impact and then by dividing that by the number of households in the village to get household-level marginal impacts. Applying marginal impacts to the per capita expenditure of nonparticipants yields their poverty rate before microfinance intervention. Together, these two estimates give the aggregate poverty estimates at the village level (table 8).

The reported poverty reduction for participants amounts to a 1.6 percentage point annual reduction in moderate poverty and a 2.2 percentage point annual reduction in extreme poverty (using average program duration for female participants of 5.2 years).[26] Thus more than half of the 3 percentage point annual decline in moderate poverty among program participants (see table 7) can be attributed to microfinance programs alone. Microfinance reduces both moderate

23. Contrast this with the land-based mistargeting of 31 percent in 1998/99 (table 1).

24. Ideally, average return (not marginal return) should be used to calculate the consumption change. Since the marginal return is usually smaller than the average return, the return to consumption based on marginal impacts underestimates the impact of microfinance on poverty.

25. The change in per capita consumption among the participants is about 14 percent of their preborrowing (simulated) per capita expenditure, which, after dividing by the program duration of 5.2 years, amounts to 2.7 percent per year or 54 percent of the actual change observed in the descriptive statistics (table 2).

26. Although membership duration is as high as 15 years for some women, there are significant numbers of new entrants to the programs with short duration, brining the average program duration to 5.2 years.

TABLE 8. Predicted Impacts on Poverty of Women's Microfinance Loans

	Poverty	
Participation Status and Poverty Level	Before Borrowing	After Borrowing
Participants		
Moderate poverty headcount	0.793**	0.711**
Extreme poverty headcount	0.467**	0.352**
Nonparticipants		
Moderate poverty headcount	0.641**	0.625**
Extreme poverty headcount	0.334**	0.309**
All households		
Moderate poverty headcount	0.700**	0.658**
Extreme poverty headcount	0.386**	0.326**

**Significant at the 5 percent level or better.

Source: Author's computations based on 1991/92 and 1998/99 household surveys in Bangladesh.

and extreme poverty among nonparticipants as well (see table 8). At an aggregate level microfinance reduces moderate poverty by about 1.0 percentage point and extreme poverty by 1.3 percentage points a year.[27]

Thus microfinance can account for some 40 percent of the overall reductions in moderate poverty in rural Bangladesh (1 percentage point out of the 2.5 percentage point reduction each year).[28] The impact of microfinance is slightly higher for extreme poverty than for moderate poverty, at both the individual and the village level. The microfinance impacts are much stronger for female borrowing than for male borrowing.

VIII. CONCLUSIONS

Program impact evaluation compares outcomes for treatment groups with those for control groups. However, finding control groups in a nonexperimental setting is difficult. Alternatively, program effects can be identified by resorting to instruments with the availability of cross-section data. However, finding good instruments is also difficult. Pitt and Khandker (1998) used a quasi-experimental method relying on exogenous eligibility conditions as a way of identifying program effects. When conditions are not adequately restrictive they may not be reliable, as with the weak enforcement of the landholding criterion

27. Annual poverty reduction of nonparticipants is obtained by dividing total poverty reduction (1.6 percent from table 8) by average village level program duration (9.3 years). Aggregate poverty reduction per year is the average of the per year reductions of participants and nonparticipants (weighted for the distribution of program participants).

28. If the spillover estimate from table 7 is used to simulate poverty reduction among participants and nonparticipants, a 30 percent overall poverty reduction at the village level can be attributed to microfinance.

for program participation. Results may also be sensitive to the methods used in impact assessment.

This study carried out an impact assessment using the 1998/99 follow-up survey to the 1991/92 survey to assess the sensitivity of the earlier findings on the poverty effects of microfinance in rural Bangladesh. The panel data analysis helps estimate the effects on poverty using an alternative estimation technique and also helps estimate the impacts of past and current borrowing, assuming that gains from borrowing, such as consumption gains, vary over time.

An earlier study using 1991/92 cross-section data found returns of about 18 percent to women's borrowing (Pitt and Khandker 1998). If this rate were sustained given the level of poverty among program participants in 1991/92, this could have led to an estimated 5 percentage point reduction in poverty for participants and a 1 percentage point reduction for the village as a whole over the program period.

Are these projected gains robust and sustainable over time? This article sought to answer these questions using panel data analysis and a dynamic model to estimate the time-varying borrowing effects on consumption for participants and nonparticipants as well as for average villagers, through spillover effects.

The results are resounding. Microfinance continues to reduce poverty among poor borrowers and within the local economy, albeit at a lower rate. It raises per capita household consumption for both participants and nonparticipants. The average returns to cumulative borrowing for female members of microfinance programs are as much as 21 percent in 1998/99, up from 18 percent in 1991/92. Despite higher returns to cumulative borrowing, the impact on poverty reduction among program participants was lower in 1998/99 (2 percentage points) than in 1991/92 (5 percentage points). This is due to diminishing returns to additional borrowing, so that despite the increase in the stock of borrowing by female members, the resulting increases in consumption were not large enough to reduce poverty as expected. Moreover, because of better economic conditions, the gains in real consumption were much higher for both program participants and nonparticipants over the study period. The consumption level of borrowers, for example, was only 8 percent below the poverty line in 1998/99 compared with 31 percent in 1991/92. And despite the diminishing returns to microfinance and the overall better economic conditions, the results indicate that microfinance accounts for more than half of the 3 percentage points observed annual reduction in poverty among program participants.

The panel data analysis also estimated the aggregate impacts of microfinance on consumption and poverty. Not only does the increase in consumption resulting from borrowing raise the probability that program participants will escape poverty but the microfinance intervention also benefits nonparticipants through growth in local income. In particular, microfinance reduces the average village poverty level by 1 percentage point each year in program areas, some 40 percent of the observed village-level poverty reduction. Microfinance has a slightly higher impact on extreme poverty than on moderate poverty for everybody.

References

Alderman, Harold, Jere R. Behrman, Hans-Peter Kohler, John A. Maluccio, and Susan C. Watkins. 2000. "Attrition in Longitudinal Household Survey Data: Some Tests for Three Developing-Country Samples." Policy Research Working Paper 2447. World Bank, Washington, D.C.

Fitzgerald, John, Peter Gottschalk, and Robert Moffitt. 1998. "An Analysis of Sample Attrition in Panel Data." *Journal of Human Resources* 33(2):251–99.

Hashemi, Syed M., Sidney R. Schuler, and Ann P. Riley. 1996. "Rural Credit Programs and Women's Empowerment in Bangladesh." *World Development* 24(4):635–53.

Hossain, Mahabub. 1988. "Credit for Alleviation of Rural Poverty: The Grameen Bank in Bangladesh." IFPRI Research Report 65. International Food Policy Research Institute, Washington, D.C.

Jalan, Jyotsna, and Martin Ravallion. 2002. "Geographic Poverty Traps? A Micro Model of Consumption Growth in Rural China." World Bank, Washington, D.C.

Khalily, M. B., M. O. Imam, and S. A. Khan. 2000. "Efficiency and Sustainability of Formal and Quasiformal Microfinance Programs—An Analysis of Grameen Bank and ASA." In Rushidan I. Rahman and Shahidur R. Khandker, eds., *The Bangladesh Development Studies, A Special Issue on Microfinance and Development: Emerging Issues* 26(June/September):103–46.

Khandker, Shahidur R. 1998. *Fighting Poverty with Microcredit: Experience in Bangladesh*. New York: Oxford University Press.

———. 2003. "Micro-Finance and Poverty: Evidence Using Panel Data from Bangladesh." Policy Research Working Paper 2945. World Bank, Washington, D.C.

Khandker, Shahidur R., and Mark M. Pitt. 2003. "The Impact of Group-Based Credit on Poor Households: An Analysis of Panel Data from Bangladesh." World Bank, Washington, D.C.

Lalonde, R. 1986. "Evaluating the Econometric Evaluations of Training Programs with Experimental Data." *American Economic Review* 76(4):604–20.

Moffitt, Robert. 1991. "Program Evaluation with Nonexperimental Data." *Evaluation Rev*iew 15(June): 291–314.

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

———. 1999. "The Role of Subsidies in Microfinance: Evidence from the Grameen Bank." *Journal of Development Economics* 60(October):229–48.

Pitt, Mark M. 1999. "Reply to Jonathan Morduch's 'Does Microfinance Really Help the Poor? New Evidence from Flagship Programs in Bangladesh.'" Brown University, Department of Economics. Available online at www.pstc.brown.edu/∼mp.

Pitt, Mark M., and Shahidur R. Khandker. 1996. "Household and Intrahousehold Impact of the Grameen Bank and Similar Targeted Credit Programs in Bangladesh." Discussion Paper 320. World Bank. Washington, D.C.

———. 1998. "The Impact of Group-Based Credit Programs on Poor Households in Bangladesh: Does the Gender of Participants Matter?" *Journal of Political Economy* 106(5):958–96.

Ravallion, Martin. 2001. "The Mystery of Vanishing Benefits: An Introduction to Impact Evaluation." *World Bank Economic Review* 15(1):115–40.

Thomas, Duncan, Elizabeth Frankenberg, and James P. Smith. 1999. "Lost but Not Forgotten: Attrition and Follow-up in the Indonesian Family Life Survey." rand Labor and Population Program Working Paper 99–01. Santa Monica, Calif., RAND.

Yaron, Jacob. 1994. "What Makes Rural Finance Institutions Successful?" *World Bank Research Observer* 9(1):49–70.

World Bank. 2003. *Poverty in Bangladesh: Building on Progress*. Washington, D.C.

Ziliak, James P., and Thomas J. Kniesner. 1998. "The Importance of Sample Attrition in Life Cycle labor Supply Estimation." *Journal of Human Resources* 33(2):507–30.