CHAPTER 4

Dynamics of Microfinance Benefits

Introduction

As discussed in chapter 1, the most substantive evaluation of microcredit was carried out two decades ago by researchers at the World Bank. That evaluation found that microcredit helped promote household welfare, and that the impacts of credit were higher for women than for men (Khandker 1998; Pitt and Khandker 1996, 1998; Pitt and others 1999; Pitt, Khandker, and Cartwright 2006).[1] The evaluation's findings, resulting from the examination of three well-known credit programs in Bangladesh using the 1991/92 cross-sectional survey data, have since been debated because of restrictive statistical assumptions for model identification of program benefits (Morduch 1998; Roodman and Morduch 2009, 2014). As previously mentioned, the claims of statistical limitations were also invalidated (Pitt 1999, 2014; Pitt and Khandker 2012).

Various studies that have utilized less restrictive assumptions, using methods with randomized control trials (RCTs), have found limited or no benefits from microfinance.[2] But such studies have often been based on assessments of short-term program effects (e.g., RCT may be limited to 18 months of intervention). Since microfinance is not cash transfer, measuring its effects requires a certain minimum length of program membership. Furthermore, the benefits from self-employed activities also take time to be realized. Keeping these requirements in mind, the earlier study of Pitt and Khandker (1998) used a cross-sectional survey of households in villages where the microfinance program had been in place for at least three years prior to the survey. The follow-up study, as well as other panel data analysis from Bangladesh, confirms the earlier benefits of microfinance (e.g., Islam 2011; Khandker 2005) (annex 4A).

As RCT design was beyond the scope of the study carried out in 1991/92, the analysis of the cross-sectional survey and follow-up surveys can only be done within the framework of quasi-experimental design. Long panel household survey data provides a unique opportunity to utilize alternative quasi-experimental methods to estimate the benefits accrued to microfinance program participants. It also allows for examining certain dynamics of microfinance, such as its time-varying effects, market saturation owing to competition

among the microfinance institutions (MFIs), diseconomies of scale of borrowing, and the aggregate village-level effect of program participation.

This chapter uses data from the long panel survey spanning more than 20 years to compare alternative estimates of the socioeconomic benefits accrued by microfinance program participants. More specifically, it explores whether the benefits of program participation vary by estimation method, whether the benefit estimates vary by timing of program participation, whether the changing effects of market conditions contribute to market saturation and village-level diseconomies, and whether market saturation and multiple program membership are adversely affecting household welfare.

Dynamics of Microcredit Participation

The findings of the supply-side analysis presented in chapter 3 lend support to the common perception that the MFIs have peaked in terms of membership growth, loan disbursement, and savings mobilization and that government-led programs (e.g., Palli Karma-Sahayak Foundation [PKSF]) have played a key role in MFI growth and competition over the past two decades.[3] But program-level data only show aggregate MFI behavior and not whether or how much program participants have benefited from gaining access to various types of products offered by the MFIs. Studying the behavior of MFI program participants in terms of borrowing, savings, and loan repayment requires household survey data that cover a long time period. Fortunately for our study, the long panel household data span a period of 20 years (1991/92–2010/11), roughly corresponding to the three phases of MFI growth (box 3.1). Thus, the long panel data offer three historical snapshots of household behavior over this period (appendix A).

Descriptive Statistics of Behavioral Outcomes

The first round of the cross-sectional survey, conducted in 1991/92, was studied to determine the role of microfinance in the social and economic advancement of the poor. Carried out jointly by the World Bank and the Bangladesh Institute of Development Studies (BIDS), the survey covered 1,769 households randomly selected from 87 villages (72 program and 15 control villages) in 29 *upazilas* (rural subdistricts).[4] The second survey round, conducted in 1998/99 with the help of BIDS, could not retrace 131 of the original 1,769 households from the first round (1991/92); thus, the remaining 1,638 available households were surveyed, implying an attrition rate of 7.4 percent. The second round (1998/99) also included new households from the originally surveyed villages, along with newly selected ones. A total of 2,599 households were surveyed (2,226 from the originally surveyed villages and 373 from the new ones). Among the 2,226 households, 279 were newly sampled ones; the other 1,947 were from the 1,638 households sampled in the 1991/92 round, which had split off to form new households in the years between the two surveys.

A third survey round was conducted in 2010/11 with the help of the Institute of Microfinance (InM). This third round attempted to revisit all 2,599

Table 4.1 Rate of Household Participation in Microcredit Programs: 1991/92–2010/11

Survey year	*Grameen Bank*	*BRAC*	*BRDB*	*ASA*	*Other programs*	*Any program*	*Multiple membership*	*Non-participant*
1991/92	8.7	11.2	6.4	0	0	26.3	0	73.7
(*N = 1,509*)	(8.6)	(9.0)	(5.8)	(0)	(0)	(23.3)	(0)	
1998/98	15.1	16.2	8.3	4.1	14.9	48.6	8.9	51.4
(*N = 1,758*)	(13.6)	(10.1)	(4.4)	(3.6)	(11.4)	(38.0)	(4.5)	
2010/11	27.4	20.9	4.7	23.8	32.9	68.5	31.9	31.5
(*N = 2,322*)	(21.7)	(12.3)	(1.3)	(19.3)	(28.2)	(56.2)	(20.4)	

Sources: World Bank/BIDS survey 1991/92 and 1998/99; World Bank/InM survey 2010/11.
Note: Sample is restricted to 1,509 panel households from the 1991/92 survey common to all three surveys. Sample size is higher for 1998/99 and 2010/11 because of household split-offs. Figures in parentheses are percentages of borrowers. Sum of the figures across columns exceeds 100 percent for 1998/99 and 2010/11 because of household participation in multiple programs. BRAC = Bangladesh Rural Advancement Committee; BRDB = Bangladesh Rural Development Board; ASA = Association for Social Advancement.

households surveyed in the second round (1998/99). Of these, 2,342 households could be traced (with an attrition of about 10 percent). In all, 3,082 households were interviewed in 2010/11, with 740 households splitting off during this period to form new households. The survey began in March 2011 and was completed in September 2011.

This study's analysis is based on the 1,509 households from the first round (1991/92) that are common to all three surveys. Of course, because of household split-off, the number of households is higher for the second (1998/99) and third (2010/11) survey rounds, at 1,758 and 2,322 households, respectively.[5] Table 4.1 indicates that, over the 20-year period, household membership in microcredit programs grew steadily, from 26.3 percent in 1991/92 to 48.6 percent in 1998/99 and to 68.5 percent in 2010/11. The only exception was the Bangladesh Rural Development Board (BRDB) government program, which lost a good share of its members between the second and third survey rounds due to reorganization.[6] Grameen Bank, the largest of all the microcredit programs, increased its membership from 8.7 percent in 1991/92 to 15.1 percent in 1998/99 and to 27.4 percent in 2010/11. In addition to the four major programs—Grameen Bank, Bangladesh Rural Advancement Committee (BRAC), BRDB, and Association for Social Advancement (ASA)—many other programs developed over the past 20 years and are now serving rural communities in a large capacity. In 2010/11, coverage of these programs included nearly 33 percent of rural households, which was higher than that of Grameen Bank.

Recent Growth in Overlapping Membership across Multiple Programs

Today, overlapping membership in multiple programs is an important aspect of microcredit program participation in Bangladesh. This phenomenon hardly existed in the early 1990s. But the third survey round (2010/11) showed that nearly 61 percent of Grameen Bank members were also members of other programs (Khandker and Samad 2014). Overall, about 31.9 percent of rural households were members of multiple microcredit programs in 2010/11, compared to 8.9 percent in 1998/99 and zero in 1991/92 (table 4.1).

Participation in microcredit programs does not necessarily imply borrowing. In many programs, new members must wait for some time before they can borrow, and some programs feature a nonborrowing membership plan that allows individuals to save money with microcredit programs without having to borrow. That said, a great majority of microcredit members are borrowers. In 2010/11, about 69 percent of rural households were microcredit members; borrowers constituted about 56 percent, implying that 13 percent were nonborrowing members (table 4.1).

While microcredit programs have offered various noncredit services in the past, they have become mostly credit-only institutions over time. Through access to credit, not just participation, households can reap the benefits.[7] As such, this chapter's analysis considers the cumulative amount of borrowing as the intervention variable. Cumulative borrowing from the two major microcredit programs, as well as from other microcredit sources, has increased by nearly 100 percent over time.[8] The total amount borrowed per household in 1991/92 was Tk. 9,252, compared to Tk. 17,006 in 2010/11, implying a simple growth rate of more than 4 percent a year over the 20-year period (table 4.2).[9]

The highest growth in borrowing occurred for the smaller programs (reported in the fourth column of table 4.2), which are relatively new compared to Grameen Bank and BRAC. The average borrowing for BRAC grew by 7.8 percent per year, compared to 11.0 percent a year for the smaller programs. More than two-thirds of the loan amounts were received by women, who are particularly targeted by the MFIs (table 4.2). In 2010/11, women's share of microcredit lending was the highest for Grameen Bank (89 percent) and the lowest for BRAC (38 percent). In earlier years, women's share of BRAC loans was much higher (e.g., 95 percent in 1998/99); but over time, many of the BRAC members sampled in our survey received loans for small and medium-size enterprises (SMEs), more of which are operated by men.[10]

Another feature of microfinance operations in Bangladesh is mandatory savings, mostly in the form of weekly savings and deposits, out of a certain

Table 4.2 Household Cumulative Loans and Savings from Microcredit Programs over Time: 1991/92–2010/11

Survey year	*Grameen Bank loans*	*BRAC loans*	*Other program loans*	*Aggregate loans from all programs*	*Aggregate savings for all programs*
1991/92	16,289.4	5,276.7	6,453.9	9,252.3	700.3
(*N = 769*)	(0.73)	(0.71)	(0.38)	(0.67)	(0.08)
1998/98	25,938.4	6,377.1	6,217.2	13,262.1	1,341.5
(*N = 1,099*)	(0.84)	(0.95)	(0.76)	(0.84)	(0.10)
2010/11	11,597.6	13,452.3	11,346.2	17,005.6	1,689.9
(*N = 1,770*)	(0.89)	(0.38)	(0.80)	(0.73)	(0.10)

Sources: World Bank/BIDS survey 1991/92 and 1998/99; World Bank/InM survey 2010/11.
Note: Findings are restricted to microcredit participants. Loans and program savings are consumer price index–adjusted (1991/92 = 100). Loans are cumulative for the five years preceding the surveys. Figures in parentheses represent sample size (column 1), share of female loans (columns 2–5), and share of program savings in cumulative loans (column 6). BRAC = Bangladesh Rural Advancement Committee.

percentage of the loans disbursed. In 1991/92, member savings represented about 8 percent of cumulative borrowing, increasing slightly to 10 percent in 1998/99 and 2010/11 (table 4.2), which nonetheless accounted for some 60 percent of MFI loans outstanding in Bangladesh (see chapter 3, figure 3.4).[11]

Before estimating the microcredit effects on the outcomes of particular interest, it is worthwhile to investigate how these outcomes vary by program participation status and across years. Table 4.3 shows that household income grew significantly over time for program participants and nonparticipants alike; in particular, household income more than doubled between 1998/99 and 2010/11.[12] The labor-supply pattern was inconsistent over time for program participants and actually experienced a drop for nonparticipants. The change in household nonland assets and net worth followed a pattern similar to that of income or expenditure, registering substantial monotonic growth.

Like the labor-supply and economic outcomes, the education outcomes also experienced consistent growth over time for both participants and nonparticipants. However, it is worth noting that school enrollment rates for girls have been consistently higher among program participants than nonparticipants.

Although the education outcomes improved over time, the differences between participants and nonparticipants showed no consistent pattern. While per capita household income (in real terms) was slightly higher for participants

Table 4.3 Household-Level Outcome Indicators by Microcredit Participation Status: 1991/92–2010/11

	1991/92		*1998/99*		*2010/11*	
Outcome	*Participants (N = 769, N_B = 816, N_G = 744)*	*Non-participants (N = 483, N_B = 425, N_G = 397)*	*Participants (N = 1,014, N_B = 883, N_G = 815)*	*Non-participants (N = 420, N_B = 305, N_G = 283)*	*Participants (N = 1,554, N_B = 180, N_G = 179)*	*Non-participants (N = 334, N_B = 1,105, N_G = 1,200)*
Per capita income (Tk./month)	521.8 (0.74)	495.6	502.7 (−0.86)	523.1	1,066.0 (−0.36)	1,114.3
Male labor supply (hours/month)	195.4 (1.10)	189.5	227.8 (2.31)	206.0	200.9 (8.32)	131.4
Female labor supply (hours/month)	38.8 (0.41)	37.2	30.1 (2.88)	20.1	56.2 (4.34)	39.6
Nonland asset value (Tk.)	18,273.0 (3.73)	12,830.7	20,089.2 (−2.46)	25,415.2	62,595.9 (−0.76)	68,294.3
Net worth (Tk.)	68,400.2 (6.15)	35,953.3	113,613.3 (−1.82)	144,981.7	287,625.0 (0.44)	269,349.1
Boys' school enrollment (ages 5–18)	0.549 (4.69)	0.417	0.562 (−1.75)	0.614	0.696 (−0.12)	0.701
Girls' school enrollment (ages 5–18)	0.510 (3.19)	0.415	0.655 (2.44)	0.581	0.712 (2.02)	0.643

Sources: World Bank/BIDS survey 1991/92 and 1998/99; World Bank/InM survey 2010/11.
Note: Monetary figures are consumer price index–adjusted (1991/92 = 100). The analysis is restricted to microcredit-eligible households of 1991/92 (those who participated and those who were eligible but did not participate in microcredit programs in 1991/92), which constituted 64, 62, and 61 percent of the surveyed households in 1991/92, 1998/99, and 2010/11, respectively. Figures in parentheses are t-statistics of the differences between participants and nonparticipants.

than nonparticipants in 1991/92 (with the difference being statistically nonsignificant), the opposite trend was observed for other years, showing slightly higher income for nonparticipants. A similar trend was observed for nonland assets and net worth, except that, in 2010/11, participants had a slightly higher net worth than nonparticipants.

While the descriptive statistics of the outcome variables can show a trend over time and by participation status, they cannot establish causality linking microfinance participation and changes in outcomes. Appendix B discusses alternative models to estimate program effects using the long panel household survey data. We apply these techniques to derive the benefits of microfinance programs accrued by rural households over time.

Pitfalls of Panel Data Analysis: Household Attrition and Split-Off

Panel data are useful for studying issues of dynamics and resolving the endogeneity of program participation and placement; however, they are not without limitations. Two major issues are sample attrition and split-off.[13] Thus, before applying the panel data to estimate the effects of microfinance, it is important to examine whether these two issues create a problem for long panel estimation.

Attrition is potentially damaging where it is nonrandom; that is, if attrition is selective, it is likely to bias estimates, and may well ruin the advantages that panel data analysis is supposed to have. Therefore, researchers try to minimize attrition and follow a rigorous procedure to locate previously surveyed households. As mentioned above, the survey data were subject to 7.4 percent attrition between the first and second survey rounds and 7.9 percent attrition between the second and third rounds, with an overall attrition rate of 14.7 percent (1991/92–2010/11), averaging less than 1 percent a year. However, the key factor is not the extent of attrition but whether it is nonrandom.

Testing for Nonrandom Attrition

To estimate the determinants of attrition, this study ran a probit regression, using the 1991/92 data with an attrition dummy (1 for households that were lost, 0 otherwise) as the dependent variable, and the outcomes (e.g., household income, expenditure, and school enrollment) and household- and village-level characteristics as explanatory variables.[14] Results of the regressions (not reported here) showed that attrition is positively correlated with households that feature less land or nonland assets and an absence of adult male or female members, as well as those located in villages with poor road conditions (a proxy for infrastructure); that is, attrition is more likely to occur among households with low socioeconomic status and in less developed villages.

These findings are consistent with previous studies on household attrition (Alderman and others 2000; Fitzgerald, Gottschalk, and Moffitt 1998; Thomas, Frankenberg, and Smith 1999; Ziliak and Kniesner 1998). For example, using the Michigan Panel Study of Income Dynamics (PSID), Fitzgerald, Gottschalk, and

Moffitt (1998) found that households with lower earnings, educational levels, and marriage propensities are more prone to attrition. But overall, these variables explain only 7–10 percent of the probability of household attrition, suggesting that up to 93 percent of the portion that cannot be explained by the explanatory variables may be random. We also ran the Wald joint significance test to determine whether the explanatory variables were jointly equal to zero (table 4B.1). The resulting Chi-squared statistics indicate that these variables jointly differ from zero at the highest level of significance (the *p*-value is 0.000), implying that these variables are significant predictors of attrition; that is, attrition may not be random.

We also performed the Becketti, Gould, Lillard, and Welch test to determine the randomness of attrition. This test involves regressing the outcome variable on household- and community-level exogenous variables, the attrition dummy, and interactions of the attrition dummy and the other explanatory variables (Becketti and others 1988). This is followed by performing a joint significance test of the attrition dummy and interaction variables to determine whether the coefficients of the explanatory variables vary significantly between the households that were lost and those that were resurveyed. If they do, then the null hypothesis that attrition is random can be rejected. The results showed that, at the 5 percent level, randomness of attrition is rejected for all outcomes (table 4B.2).

Correcting for Attrition Bias

If not corrected for, nonrandom attrition will introduce attrition bias in the estimated impacts. Attrition bias can be corrected for in various ways, such as estimating a selection model, which depends on finding suitable instruments (Heckman 1979); using inverse probability weights, which relies on auxiliary variables related to both attrition and the outcome variables (Fitzgerald, Gottschalk, and Moffitt 1998); and using nonparametric techniques (Das, Toepoel, and van Soest 2011). This analysis used the inverse probability weight since it is simple to implement and does not require strong conditions as required by the selection model. The rationale behind the calculation of inverse probability weights is that it gives more weight to the households subsequently lost than to those with similar initial characteristics who were more likely to remain in the panel.[15] We calculated such inverse probability weights for all outcomes and then used them in all of the estimations discussed from this point forward in the chapter.

Treatment of Split-Off Households

Apart from attrition, households were subject to split-offs over time. In most such cases, household members grew up, married, and left their households after the initial survey to form new households. Thus, the households surveyed in the first round may have spawned one or more new households by the time the subsequent surveys were conducted. This analysis treats these households as separate units related by their same initial (first-round) characteristics.[16]

Estimates of Microfinance Impact Using Static Model

Following the general method outlined in appendix B, we consider the following outcome equation with time-varying heterogeneity:

$$Y_{it} = X_{it}\beta_c + C_{ift}\delta_f + C_{imt}\delta_m + \eta_{it} + \mu_i + \varepsilon_{it}, \tag{4.1}$$

where (Y_{it}) equals the outcome (e.g., income, labor supply, or nonland asset) of household i in survey year t, conditional on the level of credit demand by males (C_{imt}) and females (C_{ift}); X_{it} is a vector of characteristics at the household level (e.g., sex, age, and education of household head and landholding) and village level (e.g., extent of village electrification and irrigation, availability of infrastructure, and price of consumer goods); β_c is a vector of unknown parameters of X variables to be estimated; δ_m and δ_f measure the effects of borrowing; η_{it} is an unobserved household- or community-level determinant of the outcome that is time-varying; μ_i is an unobserved household- or community-level determinant of the outcome that is time-invariant; and ε_{it} is a nonsystematic error. The household fixed-effects (FE) estimation technique can eliminate the time-invariant parameter (μ_i) by transforming equation (4.1) as follows:

$$\begin{aligned} Y_{it} - \bar{Y}_i = (X_{it} - \bar{X}_i)\beta_c + (C_{ift} - \bar{C}_{if})\delta_f + (C_{imt} - \bar{C}_{im})\delta_m \\ + (\eta_{it} - \bar{\eta}_i) + (\mu_i - \bar{\mu}) + (\varepsilon_{it} - \bar{\varepsilon}_i), \end{aligned} \tag{4.2}$$

where the bar variables (e.g., $\bar{Y}_i$, $\bar{X}_i$, $\bar{C}_{if}$) are average values for each household. Since μ is constant, $\mu_i = \bar{\mu}$ and thus its effect is eliminated. However, since $\eta_{it} \neq \bar{\eta}_i$, the problem of unobserved effects cannot be disregarded completely, and thus the ordinary least squares (OLS) estimation of equation (4.2) will be biased.

The estimation strategy discussed in appendix B suggests that such bias can be eliminated by adopting one of two methods. One estimates FE with propensity-score weight (p-score weighted FE), and the other estimates FE with the instrumental variables (IV) method (FE-IV). The p-score weighted FE works on the assumption that a major source of time-varying heterogeneity is the difference in initial baseline characteristics between participants and nonparticipants, and implementing p-score weighted regression controls for such difference. It is implemented by creating a weight variable from the propensity score (i.e., the predicted probability of participating in microcredit programs, based on a participation equation in terms of X variables observed in the initial year of 1991/92), and using that weight in FE estimation on the panel data.[17] In the FE-IV method, some variables used as instruments enter into the participation equation only and do not directly appear in the outcome equation of (4.2). The instruments proposed are interactions of variables (e.g., whether the village has a microfinance program and whether the household is eligible to participate based on the eligibility condition imposed by the programs in the initial survey year of 1991/92).

Table 4.4 Summary Statistics of Microcredit Intervention Variables

Variable	*1991/92*	*1998/99*	*2010/11*
Cumulative household loans, male (Tk.)	813.8 (3,716.3)	1,833.9 (8,162.3)	5,223.8 (30,073.4)
Cumulative household loans, female (Tk.)	1,615.4 (4,947.2)	6,994.1 (15,901.6)	15,485.6 (27,015.9)
Village average of cumulative household loans, male (Tk.)	1,806.1 (2,767.5)	3,305.2 (5,678.6)	6,456.5 (8,336.0)
Village average of cumulative household loans, female (Tk.)	2,932.4 (3,607.1)	10,128.6 (11,098.4)	20,203.7 (17,383.0)
Household males that borrowed from multiple sources	0	0	0.010 (0.100)
Household females that borrowed from multiple sources	0	0.03 (17.4)	0.184 (0.388)
MFIs operating in village (no.)	0.9 (0.5)	3.9 (1.7)	7.3 (2.6)

Sources: World Bank/BIDS survey 1991/92 and 1998/99; World Bank/InM survey 2010/11.
Note: Monetary figures are consumer price index–adjusted (1991/92 = 100). Figures in parentheses are standard deviations. MFIs = microfinance institutions.

Since we are interested in the impacts of loans accumulated over 20 years (not the current loans reported for each survey year), the loan variable is the aggregate of all loans starting from 1991/92 up to that of the current year (table 4.4). Based on the FE-IV method, the findings show that borrowing by either men or women does not appear to increase per capita income. However, borrowing by women increases women's labor supply, nonland assets, household net worth, and girls' school enrollment. Men's loans lower household income and household nonland assets (table 4.5a).

Table 4.5a also reports statistics from various tests for the suitability of the IV model. While statistics from over-identification and under-identification tests have the correct sign for IV, the endogeneity test suggests that IV is not the appropriate model in most cases. Results using p-score weighted FE are considered appropriate estimates of microfinance borrowing (table 4.5b).

According to p-score weighted FE, men's and women's loans have no impact on household income. However, they both have significant positive impacts on the labor supply of men and women, household nonland assets, and net worth. A 10 percent increase in men's cumulative borrowing raises men's and women's labor supply by 0.5 percent and 0.4 percent, respectively. A similar increase in women's loans increases those outcomes by 0.4 percent and 0.6 percent, respectively. While loans for both genders raise household nonland assets by about the same percentage (0.4), men's loans have a comparatively higher impact on net worth. In terms of children's school enrollment, only women's loans matter. A 10 percent increase in women's loans increases enrollment by about 0.1 percentage point for both boys and girls. These findings confirm many of the earlier findings of the cross-sectional study (Pitt and Khandker 1998), which used only the 1991/92 data, as well as Khandker (2005), which used two-year panel data (1991/92 and 1998/99).

Table 4.5 Impacts of Microcredit Loans on Household and Individual Outcomes

a. Using 2SLS IV with household FE (N = 1,509)

Explanatory variable	*Log per capita total income (Tk./month)*	*Log male labor supply (hours/ month)*	*Log female labor supply (hours/ month)*	*Log household nonland assets (Tk.)*	*Log household net worth (Tk.)*	*Boys' school enrollment (ages 5–18)*	*Girls' school enrollment (ages 5–18)*
Log loans of household males (Tk.)	–0.156* (–1.96)	0.029 (0.23)	0.204 (1.40)	–0.201** (–2.07)	0.052 (0.70)	–0.017 (–0.36)	–0.028 (–0.50)
Log loans of household females (Tk.)	0.017 (1.25)	0.011 (0.40)	0.080** (2.92)	0.096** (5.16)	0.028* (1.64)	0.016 (1.47)	0.021** (2.45)
F-statistic for the model	$F(27, 85) = 15.24$, $p > F = 0.000$	$F(27, 85) = 54.72$, $p > F = 0.000$	$F(27, 85) = 26.45$, $p > F = 0.000$	$F(27, 85) = 85.50$, $p > F = 0.000$	$F(27, 85) = 166.65$, $p > F = 0.000$	$F(27, 85) = 11.11$, $p > F = 0.000$	$F(27, 85) = 7.48$, $p > F = 0.000$
Endogeneity test statistic	$\chi^2(2) = 3.87$, $p > \chi^2 = 0.144$	$\chi^2(2) = 1.81$, $p > \chi^2 = 0.405$	$\chi^2(2) = 9.05$, $p > \chi^2 = 0.011$	$\chi^2(2) = 15.72$, $p > \chi^2 = 0.000$	$\chi^2(2) = 6.74$, $p > \chi^2 = 0.034$	$\chi^2(2) = 0.86$, $p > \chi^2 = 0.649$	$\chi^2(2) = 2.09$, $p > \chi^2 = 0.352$
Over-identification test statistic	$\chi^2(3) = 3.53$, $p > \chi^2 = 0.318$	$\chi^2(3) = 4.87$, $p > \chi^2 = 0.181$	$\chi^2(3) = 0.02$, $p > \chi^2 = 0.999$	$\chi^2(3) = 3.24$, $p > \chi^2 = 0.357$	$\chi^2(3) = 0.51$, $p > \chi^2 = 0.916$	$\chi^2(3) = 7.25$, $p > \chi^2 = 0.064$	$\chi^2(3) = 0.15$, $p > \chi^2 = 0.988$
Under-identification test statistic	$\chi^2(4) = 19.05$, $p > \chi^2 = 0.001$	$\chi^2(4) = 18.88$, $p > \chi^2 = 0.001$	$\chi^2(4) = 19.22$, $p > \chi^2 = 0.001$	$\chi^2(4) = 19.07$, $p > \chi^2 = 0.001$	$\chi^2(4) = 18.21$, $p > \chi^2 = 0.001$	$\chi^2(4) = 12.26$, $p > \chi^2 = 0.016$	$\chi^2(4) = 12.97$, $p > \chi^2 = 0.011$

b. Using p-score weighted household FE model (N = 1,509)

Explanatory variable	*Log per capita total income (Tk./month)*	*Log male labor supply (hours/ month)*	*Log female labor supply (hours/ month)*	*Log household nonland assets (Tk.)*	*Log household net worth (Tk.)*	*Boys' school enrollment (ages 5–18)*	*Girls' school enrollment (ages 5–18)*
Log loans of household males (Tk.)	–0.002 (–0.11)	0.050** (3.56)	0.038* (1.95)	0.037** (3.69)	0.024** (2.97)	–0.003 (–0.50)	0.005 (0.54)
Log loans of household females (Tk.)	–0.008 (–0.16)	0.039** (4.91)	0.056** (5.54)	0.036** (5.70)	0.011** (2.06)	0.008** (2.35)	0.011** (2.57)
R^2	0.137	0.209	0.238	0.453	0.651	0.073	0.065
Test statistics for equality of male and female loans	$F(1, 86) = 4.61$, $p = 0.035$	$F(1, 86) = 3.89$, $p = 0.051$	$F(1, 86) = 7.18$, $p = 0.009$	$F(1, 86) = 4.79$, $p = 0.031$	$F(1, 86) = 5.76$, $p = 0.019$	$F(1, 86) = 2.91$, $p = 0.091$	$F(1, 86) = 5.31$, $p = 0.024$

Sources: World Bank/BIDS survey 1991/92 and 1998/99; World Bank/InM survey 2010/11.

Note: Figures in parentheses are t-statistics based on robust standard errors clustered at the village level. Loans refer to cumulative amount borrowed from all microcredit sources leading up to the survey year. Regressions include more control variables at the household level (e.g., age, sex, and education of household head and log of land assets) and village level (e.g., price of consumer goods; male and female wages; infrastructure, including schools and electricity availability; and proportion of irrigated land). IV = instrumental variables; FE = fixed effects.

Significance level: * = 10 percent, ** = 5 percent or less.

Estimates of Microfinance Impact Using Dynamic Model

The results presented thus far are based on a static model, which assumes past outcomes have no bearing on current outcomes. But in instances where it is believed that past outcomes affect future outcomes, it may be preferable to include a lagged dependent variable (LDV). Considering that this panel spans more than 20 years with three data points, we cannot rule out the possibility that past outcomes affect current ones (e.g., past nonland assets affect current asset holdings). Also, it is possible that participants whose incomes rose in earlier years invested wisely and in later years benefited even more from lucrative ventures. Even if we are not interested in the effects of the LDV on the outcomes, including it may be important for obtaining consistent estimates of other parameters (see "Dynamic Panel Models," appendix B).

This analysis estimated the dynamic panel model by instrumenting the LDV as described in appendix B. While the LDV captures the dynamics of the panel data and helps in measuring consistent estimates of other parameters, it cannot control for the time-varying heterogeneity (η_{it}), which can bias the estimates of credit effects. To control for such bias, we use p-score weight in the implementation of the dynamic model, just as we did in implementing the static panel model, and refer to this procedure as the p-score weighted LDV method.

Table 4.6 shows the findings based on the dynamic panel model.[18] As shown, female credit has positive impacts on household income, unlike the finding based on the static panel model reported in table 4.5. A 10 percent increase in women's loans increases household per capita income by about 0.2 percent. Like the findings of the static panel model, both men's and women's credit increase men's labor supply. Unlike the static panel findings, however, credit—either by men or women—does not affect women's labor supply. The credit impacts on household nonland assets and net worth are similar for both the static and dynamic panels,

Table 4.6 Impacts of Microcredit Loans on Household Outcomes Using the Dynamic Panel LDV Model
N = 1,509

Explanatory variable	*Log per capita total income (Tk./month)*	*Log male labor supply (hours/ month)*	*Log female labor supply (hours/ month)*	*Log household nonland assets (Tk.)*	*Log household net worth (Tk.)*
Log loans of household males (Tk.)	0.014 (1.62)	0.028** (3.09)	–0.024 (–1.35)	0.038** (3.65)	0.019** (2.82)
Log loans of household females (Tk.)	0.016** (2.76)	0.046** (4.70)	0.011 (0.65)	0.026** (3.32)	0.030** (4.50)
F statistics for the model	$F(26, 86) = 6.58$, $p > 0.000$	$F(26, 86) = 49.22$, $p > 0.000$	$F(26, 86) = 44.37$, $p > 0.000$	$F(26, 86) = 33.64$, $p > 0.000$	$F(26, 86) = 127.70$, $p > 0.000$

Sources: World Bank/BIDS survey 1991/92 and 1998/99; World Bank/InM survey 2010/11.
Note: Figures in parentheses are t-statistics based on robust standard errors clustered at the village level. Loans refer to cumulative amount borrowed from all microcredit sources leading up to the survey year. Regressions include more control variables at the household level (e.g., age, sex, and education of household head; log of land assets) and village level (e.g., price of consumer goods; male and female wages; infrastructure, including schools and electricity availability; and proportion of irrigated land). LDV = lagged dependent variable.
Significance level: * = 10 percent, ** = 5 percent or less.

while the effects vary in magnitude. A comparison of the model findings suggests that the estimates of credit effects are reasonably robust. For subsequent analysis, we report the findings for both types of estimation.

Short-Term versus Long-Term Impacts of Microfinance

The estimates of credit effects, as previously reported, might differ across years if timing of borrowing matters (i.e., if past credit received affects behavior differently than credit recently received). Unlike the implied assumption in equation (4.1), parameters of the credit demand and other regressors may vary over time, allowing for differential credit impacts over time.[19]

Credit effects may vary by time for a variety of reasons. For example, during the initial years of membership, participants may choose conservative projects (or be influenced to by other members of the group) until they have demonstrated an ability to repay. During this period, they may focus more on accumulating assets and thus cement the new insurance network, a result of group participation. With the passage of time, they may have a larger financial cushion, allowing for some risk-taking behavior with new loans. Second, the unobserved local market conditions that influence a household's demand for credit may change over time so as to exert a favorable impact on credit demand. Third, if the noncredit effects of program participation are important, and if changes in the attitudes they engender are functions of time spent in the group, then the total effect of participation may decline over time. On the other hand, if the knowledge gained through self-employment experience is an important component of the returns to self-employment, credit effects may rise over time. Finally, returns may fall as the rents that accrue to early participants get competed away. It is worth investigating if there are differences in these effects, possibly because of the differences in group dynamics and the types of self-employment activities they undertake over time.

One way to conceptualize the dynamics of credit effect is to assume that past credit has a lingering impact so that it affects both past and future outcomes. This issue is important in assessing the long-term impacts of microcredit, as opposed to its short-term impacts, which are confined to the period of borrowing. To assess its long-term impacts, we rewrite equation (4.1), allowing for the possibility that past credit might affect current outcomes, expressed as follows:

$$\begin{aligned} Y_{ijt} = {} & X_{ijct}\beta_c + X_{ijpt}\beta_p + \mathrm{C}_{ijfct}\delta_{fc} + \mathrm{C}_{ijfpt}\delta_{fp} + \mathrm{C}_{ijmct}\delta_{mc} \\ & + \mathrm{C}_{ijmpt}\delta_{mp} + \eta_{ij}^{y} + \mu_{j}^{y} + \varepsilon_{ijt}^{y}. \end{aligned} \tag{4.3}$$

In this equation, subscripts c and p, respectively, refer to current and past credit and current and past sets of control variables (X). The current credit at period t (where t equals 1991/92, 1998/99, or 2010/11) is defined as the cumulative amount of borrowing by household males or females up to that period, and past credit at period t is defined by the cumulative amount of borrowing by household males or females up to period $(t - 1)$.[20] Thus, this model assumes that, even if a

household stopped borrowing after period 1 (1991/92), it can still benefit in period 2 (1998/99) as past credit may continue to benefit the borrower (provided that $\delta_p > 0$). Thus, we allow the impacts of borrowing to be long term.[21]

Differencing out over time, we obtain the following revised static model:

$$\Delta Y_{ijt} = \Delta X_{ijct}\beta_c + \Delta X_{ijpt}\beta_p + \Delta C_{ijfct}\delta_{fc} + \Delta C_{ijfpt}\delta_{fp} + \Delta C_{ijmct}\delta_{mc} + \Delta C_{ijmpt}\delta_{mp} + \Delta\eta_{ij}^{y} + \Delta\mu_{j}^{y} + \Delta\varepsilon_{ijt}^{y}. \quad (4.4)$$

Introducing a dynamic LDV method on the above model, we obtain the first-differenced model, expressed as follows:

$$\Delta Y_{ijt} = \alpha\Delta Y_{(t-1)} + \Delta X_{ijct}\beta_c + \Delta X_{ijpt}\beta_p + \Delta C_{ijfct}\delta_{fc} + \Delta C_{ijfpt}\delta_{fp} + \Delta C_{ijmct}\delta_{mc} + \Delta C_{ijmpt}\delta_{mp} + \Delta\eta_{ij}^{y} + \Delta\mu_{j}^{y} + \Delta\varepsilon_{ijt}^{y}. \quad (4.5)$$

Like earlier model estimation, we have applied the p-score weighted FE static model to equation (4.4) and a p-score weighted LDV to estimate equation (4.5). A test for equality of δ_p and δ_c for both sets of models shows that the equality hypothesis cannot be rejected for three out of seven outcomes for the p-score FE model and four out of five outcomes for the LDV model (table 4.7a). This suggests the presence of distinct impacts of past borrowing on current outcomes for the majority of outcomes.[22]

However, the coefficients of the impact estimates for p-score weighted FE for the static model of equation (4.4) and the dynamic LDV model of equation (4.5) do not quite differ (table 4.7). In fact, out of 16 coefficients for 4 outcomes (ignoring boys' and girls' schooling, which are irrelevant for the LDV model), 9 coefficients are statistically significant. Thus, it makes no difference in terms of efficiency gains for the static or dynamic models.

Results of the dynamic LDV estimations, presented in table 4.7b, show that current male borrowing affected current levels for three outcomes, but past borrowing affected none. Current female credit affected current levels for four outcomes, and past credit affected two outcomes. More specifically, current male borrowing affected current male labor supply, nonland assets, and household net worth, but past credit had no impact on current levels of these outcomes. However, both current and past female loans affected the current level of household outcomes. Although current female borrowing had no statistically significant effect on per capita income, past female borrowing had a positive effect on current per capita income. The findings show that a 10 percent increase in past female borrowing increased household per capita income by nearly 3 percent. Past female borrowing had a negative effect on the current period's female labor supply. Thus, for some outcomes, the past credit of female borrowers had lingering effects.

That the response elasticity is positive for female past borrowing on such outcomes as income may indicate increasing, rather than decreasing, returns to borrowing. On the other hand, female past borrowing has a negative effect on current labor supply, indicating decreasing returns to borrowing. These examples

Table 4.7 Impacts of Current and Past Microcredit Loans on Outcomes

a. Household and individual outcomes, using p-score weighted household FE (N = 1,509)

Explanatory variable	*Log per capita total income (Tk. month)*	*Log male labor supply (hours/ month)*	*Log female labor supply (hours/ month)*	*Log household nonland assets (Tk.)*	*Log household net worth (Tk.)*	*Boys' school enrollment (ages 5–18)*	*Girls' school enrollment (ages 5–18)*
Log loans of household males (Tk.)							
Current loans	–0.008	0.067**	0.043*	0.042**	0.021**	–0.013	–0.006
	(–0.47)	(3.25)	(1.78)	(3.89)	(2.34)	(–1.47)	(–0.71)
Past loans	–0.008	0.031	0.0003	–0.003	0.017	0.005	0.021*
	(–0.37)	(1.15)	(0.01)	(–0.21)	(1.30)	(0.31)	(1.71)
Log loans of household females (Tk.)							
Current loans	–0.001	0.046**	0.057**	0.020**	–0.003	–0.001	0.005
	(–0.09)	(3.44)	(4.22)	(2.76)	(–0.54)	(–0.09)	(1.05)
Past loans	0.010*	0.006	–0.009	–0.012	0.026**	–0.011	0.019**
	(1.83)	(0.43)	(–0.60)	(–1.10)	(3.07)	(–1.43)	(2.93)
R^2	0.138	0.234	0.387	0.491	0.681	0.099	0.103
Test statistics for equality of current and past loans	$F(2, 86) = 0.88$, $p > F = 0.417$	$F(2, 86) = 3.45$, $p > F = 0.036$	$F(2, 86) = 6.10$, $p > F = 0.003$	$F(2, 86) = 7.04$, $p > F = 0.002$	$F(2, 86) = 3.71$, $p > F = 0.028$	$F(2, 86) = 1.05$, $p > F = 0.355$	$F(2, 86) = 5.28$, $p > F = 0.007$

table continues next page

Table 4.7 Impacts of Current and Past Microcredit Loans on Outcomes *(continued)*

b. Household outcomes, using dynamic panel LDV model (N = 1,509)

Explanatory variable	*Log per capita total income (Tk. month)*	*Log male labor supply (hours/ month)*	*Log female labor supply (hours/ month)*	*Log household nonland assets (Tk.)*	*Log household net worth (Tk.)*
Log loans of household males (Tk.)					
Current loans	–0.007	0.031*	–0.002	0.055**	0.034**
	(–0.44)	(1.72)	(–0.08)	(4.98)	(3.24)
Past loans	0.008	–0.005	–0.035	–0.023	–0.019
	(1.01)	(–0.27)	(–0.98)	(–1.37)	(–1.26)
Log loans of household females (Tk.)					
Current loans	0.010	0.067**	0.061**	0.033**	0.034**
	(1.28)	(6.11)	(3.84)	(4.17)	(4.46)
Past loans	0.027*	–0.025	–0.064**	–0.009	–0.005
	(1.73)	(–1.59)	(–2.61)	(–0.99)	(–0.65)
F statistics of the model	$F(28, 86) = 7.00$, $p > F = 0.000$	$F(28, 86) = 45.25$, $p > F = 0.000$	$F(28, 86) = 38.80$, $p > F = 0.000$	$F(28, 86) = 33.16$, $p > F = 0.000$	$F(28, 86) = 132.20$, $p > F = 0.000$
Test statistics for equality of current and past loans	$\chi^2(2) = 1.30$, $p > \chi^2 = 0.523$	$\chi^2(2) = 26.82$, $p > \chi^2 = 0.000$	$\chi^2(2) = 12.12$, $p > \chi^2 = 0.000$	$\chi^2(2) = 14.20$, $p > \chi^2 = 0.001$	$\chi^2(2) = 11.24$, $p > \chi^2 = 0.004$

Sources: World Bank/BIDS survey 1991/92 and 1998/99; World Bank/InM survey 2010/11.

Note: Figures in parentheses are t-statistics based on robust standard errors clustered at the village level. Loans refer to cumulative amount borrowed from all microcredit sources leading up to the survey year. Regressions include more control variables at the household level (e.g., age, sex, and education of household head and log of land assets) and village level (e.g., price of consumer goods; male and female wages; infrastructure, including schools and electricity availability; and proportion of irrigated land). FE = fixed effects; LDV = lagged dependent variable.

Significance level: * = 10 percent, ** = 5 percent or less.

clearly demonstrate that the timing of borrowing matters to the credit effects, especially for female borrowers, for some household outcomes, and that past credit may have impacts independent of current credit. Finally, the effect of past credit may be lingering, increasing, decreasing, or nonexistent, depending on the outcome considered.

Market Saturation and Village Diseconomies of Scale

Market saturation of some type may result in decreasing returns to credit as village participation rates increase. That is, the benefits of being first in the market may compete away the returns of later entrants. For example, early entrants financed by group-based credit can choose from the most profitable self-employment activities, while later entrants will either compete away some of the benefits of being first to enter or will enter into less rewarding activities. Conversely, market saturation yields higher returns to individual borrowing, given that the village attracts investment from more borrowers to enhance village-level externality, producing specialization of certain activities. If, for example, an earlier entrant starts to produce a commodity that attracts investment from other borrowers in such a way that the village becomes the center of certain products or activities, returns may increase, in which case one would expect either positive or negative externality out of the market saturation.

This phenomenon can be captured by modeling the effect of credit that depends on some measure of the village rate of participation, after controlling for village-level FE. In its simplest form, we allow the credit effect (α) to vary by village, expressed as follows:

$$\alpha_j = \alpha_0 + \alpha_\Omega \Omega_j, \tag{4.6}$$

where α_j is the credit effect in village j, and Ω_j is some measure of village-level participation in microcredit programs, such as average number of participants in a village and the village average of household male or female credit participants. The parameter α_j is identified separately from the village FE only with panel data.

Panel data are thus needed to estimate any spillover effects. When there are spillover effects, unobserved village heterogeneity can be correlated with program placement, where the direction of causation is from program placement to unobserved village effects, not from village effects to program placement. This measurement problem implies that placement of a microfinance program may cause a village effect additional to any preexisting (time-invariant) village effects. Assuming that (a) a household's male or female program participation is measured by whether an individual participates in a program instead of the amount of borrowing and (b) the average village-level male or female participation rate is the village program participation intensity, we write the following regression equation:

$$Y_{ijt} = X_{ijt}\beta + C_{ijft}\delta_f + C_{ijmt}\delta_m + \Omega_{jft}\alpha_f + \Omega_{jmt}\alpha_m + \eta_{ijt} + \mu_{jt} + \varepsilon^c_{ijt}, \tag{4.7}$$

where Ω_js represent the external effects of a program in a village and have a value of zero if no program is located in the village. The coefficients δ_m and δ_f capture village-level program effects as well if Ω_j equals zero (i.e., none of the village-specific heterogeneity is caused by programs). If village externalities do exist (i.e., Ω_j does not equal zero), the spillover effects cannot be separately identified from the time-invariant village effects using cross-sectional survey data. With panel data, the extent of market saturation or village economies/diseconomies is captured by the village-level program participation rates. If the Ω_j terms are measured by the average village-level program participation rate, then the spillover effect is measured by the change in behavior of nonparticipants due to change in village-level program participation, captured by α.

As indicated, market saturation or spillover may be good or bad, depending on whether the coefficients of these village-participation variables are positive or negative. To account for the spillover effects, we rewrite the outcome equation, similar to (4.1), as follows:

$$Y_{ijt} = X_{ijt}\beta_y + C_{ijft}\delta_f + C_{ijmt}\delta_m + V_{ijft}\gamma_f + V_{ijmt}\gamma_m + \eta_{ij}^y + \mu_j^y + \varepsilon_{ijt}^y, \quad (4.8)$$

where V equals the village average of male or female loans and a measure of spillover effects.

Table 4.8a presents the results of the static model estimates, while table 4.8b shows the results of the dynamic model estimations, based on equation (4.8). A comparison of the results shows slight qualitative differences. According to the dynamic model, average village-level male borrowing does not appear to have an independent effect on household welfare above and beyond household-level male borrowing. This means there is no externality or spillover effect due to male borrowing. However, this is not the case using the static model (table 4.8a). Like female borrowing, average village-level male borrowing has a significant effect for some outcomes. Average village-level female borrowing increases household net worth but reduces nonland assets and girls' school enrollment. A 10 percent rise in the average village loans of female borrowers increases household net worth by 0.55 percent but reduces household nonland assets by 0.43 percent. By contrast, average village-level male borrowing reduces income and boys' school enrollment.

The negative village-level borrowing effect implies negative externality or village diseconomies, while the positive effect implies positive externality or spillover effect. The results thus suggest that higher average village lending does not necessarily crowd out the benefits of an individual's borrowing. The negative coefficient in the case of women's loans associated with household nonland assets is clear evidence of village diseconomies. But there are positive externalities between household net worth and women's borrowing. Thus, even if there are negative externalities for women's borrowing on nonland asset holdings, women's borrowing appears to have a positive spillover effect on household net worth.

Table 4.8 Impacts and Village Intensity of Microcredit Loans

a. Using p-score weighted household FE, model 1 (N = 1,509)

Explanatory variable	*Log per capita total income (Tk./month)*	*Log male labor supply (hours/ month)*	*Log female labor supply (hours/ month)*	*Log household nonland assets (Tk.)*	*Log household net worth (Tk.)*	*Girls' school enrollment (ages 5–18)*	*Girls' school enrollment (ages 5–18)*
Log male loans (Tk.)	0.002	0.049**	0.039*	0.038**	0.025**	–0.001	0.005
	(0.12)	(3.44)	(1.97)	(3.81)	(3.13)	(–0.17)	(0.48)
Log female loans (Tk.)	–0.001	0.043**	0.053**	0.030**	0.004	0.007**	0.013**
	(–0.24)	(4.96)	(5.33)	(4.69)	(0.79)	(2.06)	(2.89)
Log village average, male loans (Tk.)	–0.026**	0.007	–0.009	–0.003	0.002	–0.014**	0.006
	(–2.47)	(0.36)	(–0.30)	(–0.21)	(0.13)	(–2.15)	(0.63)
Log village average, female loans (Tk.)	0.009	–0.027	0.013	–0.043**	0.055**	0.006	–0.013*
	(0.84)	(–1.24)	(0.46)	(2.59)	(3.64)	(1.03)	(–1.76)
R^2	0.140	0.209	0.238	0.458	0.654	0.075	0.066

b. Using dynamic panel LDV model (N = 1,509)

Explanatory variable	*Log per capita total income (Tk./month)*	*Log male labor supply (hours/month)*	*Log female labor supply (hours/month)*	*Log household nonland assets (Tk.)*	*Log household net worth (Tk.)*
Log male loans (Tk.)	0.016*	0.032**	–0.028	0.035**	0.020**
	(1.83)	(3.21)	(–1.48)	(3.81)	(3.51)
Log female loans (Tk.)	0.015**	0.044**	0.017	0.018**	0.013*
	(2.20)	(4.84)	(1.01)	(2.31)	(1.83)
Log village average, male loans (Tk.)	–0.008	–0.016	0.009	0.024	0.022
	(–0.64)	(–0.87)	(0.28)	(1.36)	(0.99)
Log village average, female loans (Tk.)	0.041	0.044	–0.168	0.009**	0.008**
	(0.55)	(0.57)	(–1.25)	(2.45)	(4.92)
F statistics for the model	$F(28, 87) = 6.98$, $p = 0.000$	$F(28, 86) = 46.30$, $p = 0.000$	$F(28, 87) = 33.08$, $p = 0.000$	$F(28, 87) = 33.50$, $p = 0.000$	$F(28, 87) = 131.96$, $p = 0.000$

Sources: World Bank/BIDS survey 1991/92 and 1998/99; World Bank/InM survey 2010/11.

Note: Figures in parentheses are t-statistics based on robust standard errors clustered at the village level. Loans refer to cumulative amount borrowed from all microcredit sources leading up to the survey year. Regressions include more control variables at the household level (e.g., age, sex, and education of household head and log of land assets) and village level (e.g., price of consumer goods; male and female wages; infrastructure, including schools and electricity availability; and proportion of irrigated land). FE = fixed effects; LDV = lagged dependent variable.

Significance level: * = 10 percent, ** = 5 percent or less.

Multiple Program Membership and MFI Competition

As previously mentioned, overlapping MFI membership is a relatively recent phenomenon. The 1991/92 survey showed no evidence of multiple program membership; but by 1998/99, some 9 percent of households had become concurrent members of more than one MFI, and this figure reached 32 percent by 2010/11. What accounts for such growth in multiple program membership? Does it reflect a higher demand for credit by borrowers not met by a single source? Is it paying back the loan from one source by borrowing from another? Or is it simply a matter of programs chasing the less credit-risky clients in an area?

The extent of multiple program membership could also be an outcome of market saturation, measured by the average village-level borrowing. Even if market saturation leads to diminishing returns to borrowing (i.e., village diseconomies), this can increase an individual's demand for credit, leading, in turn, to higher incidence of multiple program membership if the extra credit demanded comes from more than one source.[23] In this case, the village-level intensity of program participation would be viewed as a positive phenomenon; that is, the higher the amount of village-level borrowing, the higher the marginal effect of individual borrowing on the incidence of multiple program membership. Conversely, if the additional credit demand is being met by a single source, it would be viewed as having a negative effect on village-level borrowing.

Why is there higher demand for credit when village diseconomies lead to lower returns on borrowing? One possible explanation is that borrowers try to diversify income sources in order to cope with diminishing returns to borrowing and increasing income risks. Thus, higher access to credit helps households to diversify their income-earning activities. But, as village-level borrowing increases with growth in individual-level borrowing, the market becomes saturated; households are likely to specialize in case of positive externalities (village economies yield higher returns) and diversify in case of negative ones (village diseconomies yield lower returns). As already observed, individual-level borrowing has diminishing returns for some outcomes, suggesting a higher demand for extra credit to support income diversification. If this is so, the positive sign of the interactions between village- and individual-level credit must apply to both incidence of multiple income activities and multiple program membership. In either case of village economies and diseconomies, the additional demand for credit may lead to an increase in incidence of multiple program membership, provided that demand is satisfied by more than a single source.[24]

Writing a revised version of equation (4.1) by introducing multiple membership status (M) and village intensity of the MFIs (V) yields the following static model equation:

$$Y_{ijt} = X_{ijt}\beta_y + C_{ijft}\delta_f + C_{ijmt}\delta_m + M_{ijft}\kappa_f + M_{ijmt}\kappa_m + V_{jt}\gamma + \eta_{ij} + \mu_j + \varepsilon_{ijt}. \quad (4.9)$$

Similarly, we can have a dynamic LDV model corresponding to equation (4.9). Table 4.9a presents the p-score FE results according to the static

Table 4.9 Impacts of Borrowing from Multiple Sources and Microcredit Competition

a. Using p-score weighted household FE, model 1 (N = 1,509)

Explanatory variable	*Log per capita total income (Tk./month)*	*Log male labor supply (hours/month)*	*Log female labor supply (hours/month)*	*Log household nonland assets (Tk.)*	*Log household net worth (Tk.)*	*Boys' school enrollment (ages 5–18)*	*Girls' school enrollment (ages 5–18)*
Log loans (Tk.)							
Household males	−0.0002	0.050**	0.020	0.030**	0.021**	−0.006	0.005
	(−0.01)	(3.35)	(1.03)	(3.24)	(2.41)	(−1.12)	(0.54)
Household females	−0.002	0.034**	0.052**	0.034**	0.011**	0.006*	0.010**
	(−0.33)	(4.63)	(4.81)	(5.38)	(2.11)	(1.87)	(2.24)
Borrowing from multiple sources							
Household males	−0.220	−0.315	0.057**	0.138**	0.191	0.252	0.146
	(−0.79)	(−0.78)	(3.03)	(2.07)	(0.99)	(1.59)	(0.83)
Household females	0.011	0.131	0.051	0.015	−0.089**	0.076*	0.080**
	(0.23)	(1.28)	(0.40)	(0.31)	(−2.01)	(1.94)	(2.26)
MFI in village (no.)	0.032**	0.060**	0.055	0.012	0.025*	0.007	−0.007
	(2.20)	(2.98)	(1.51)	(0.82)	(1.78)	(0.85)	(−0.71)
R^2	0.141	0.212	0.242	0.457	0.652	0.078	0.067

table continues next page

Table 4.9 Impacts of Borrowing from Multiple Sources and Microcredit Competition *(continued)*

b. Using dynamic panel LDV model (N = 1,509)

Explanatory variable	*Log per capita total income (Tk./month)*	*Log male labor supply (hours/month)*	*Log female labor supply (hours/month)*	*Log household nonland assets (Tk.)*	*Log household net worth (Tk.)*
Log male loans (Tk.)	0.014*	0.029**	–0.031	0.033**	0.014**
	(1.66)	(2.98)	(–1.55)	(3.21)	(2.13)
Log female loans (Tk.)	0.014**	0.037**	0.005	0.021**	0.025**
	(2.12)	(3.71)	(0.32)	(2.40)	(3.85)
Household males borrowed from multiple sources (Tk.)	–0.016	–0.321	0.070**	0.304	0.071
	(–1.11)	(–0.65)	(3.51)	(1.57)	(1.62)
Household females borrowed from multiple sources (Tk.)	–0.001	0.091**	0.140	0.031	–0.110*
	(–0.01)	(2.21)	(1.09)	(0.42)	(–1.79)
MFIs in the village (no.)	0.025*	0.024	0.001	0.051**	0.110
	(1.79)	(1.38)	(0.01)	(3.06)	(5.13)
F statistics for the model	$F(29, 86) = 6.64$, $p = 0.000$	$F(29, 86) = 47.01$, $p = 0.000$	$F(29, 86) = 49.40$, $p = 0.000$	$F(29, 86) = 33.67$, $p = 0.000$	$F(29, 86) = 163.65$, $p = 0.000$

Sources: World Bank/BIDS survey 1991/92 and 1998/99; World Bank/InM survey 2010/11.

Note: Figures in parentheses are t-statistics based on robust standard errors clustered at the village level. Loans refer to cumulative amount borrowed from all microcredit sources leading up to the survey year. Regressions include more control variables at the household level (e.g., age, sex, and education of household head and log of land assets) and village level (e.g., price of consumer goods; male and female wages; infrastructure, including schools and electricity availability; and proportion of irrigated land). FE = fixed effect; MFI = microfinance institution; LDV = lagged dependent variable.

Significance level: * = 10 percent, ** = 5 percent or less.

model, while table 4.9b provides the results using the dynamic LDV model. As before, the results of the static and dynamic models do not differ qualitatively. Using the results of the dynamic LDV model (table 4.9b), κ equals zero if multiple membership status does not matter. However, if village density of MFIs does not have a separate and additional effect from what it induced through individual borrowing, the estimated coefficient of MFI village density is insignificant (i.e., γ equals zero).

Multiple sources of borrowing, independent of a household's borrowing from any source, appears to have an independent negative effect on net worth and a positive effect on male labor supply. That is, when higher amounts of individual borrowing are supported by multiple sources, the incidence of borrowing from multiple sources has an "above and beyond" negative effect on household net worth. This is a case of village diseconomies of higher level of borrowing from multiple sources. On the other hand, MFI competition has been a blessing for microcredit borrowers. By providing additional funds to support the income generation and productivity of microenterprises, a higher village density of MFIs is likely to increase (not decrease) household assets and net worth. This shows that village-level MFI density has positive but decreasing returns to household welfare.

Summary

Household-level panel data enable the use of a household-level FE method to treat program endogeneity without complicated estimation techniques and thus offers an alternative to investigate whether the results obtained from the analysis of cross-sectional data hold. Panel data, especially long panel data, also offer a clear substantive advantage over cross-sectional data in estimating microcredit impacts: They help to analyze dynamic issues, such as whether credit effects change (or decline) over time, whether market saturation with village diseconomies is a possibility for microfinance expansion in such countries as Bangladesh where MFIs are heavily concentrated, and whether the phenomenon of multiple program membership is a result of the credit expansion and an indication of income diversification due to village diseconomies. Panel data, however, are not a panacea as they have their own drawbacks in estimation.

Using long panel household survey data carried out at three points (1991/92, 1998/99, and 2010/11) over some 20 years in 87 villages of rural Bangladesh, this chapter investigated various dimensions of microcredit effects on a set of behaviors to validate earlier results obtained with cross-sectional or short panel data. In particular, it provided separate estimates of female and male credit from all sources of microcredit on household per capita income, nonland assets, household net worth, women's and men's labor supply, and schooling of children. It also provided estimates of the time-varying effects of credit on the same set of behaviors, as well as the role of an increasing level of market saturation and MFI competition on household welfare.

The estimates of microfinance effects on household income, assets, and other outcomes of interest thus far discussed indicate that male and female borrowing affect income, labor supply, household assets and net worth, and children's schooling; however, the effects are more pronounced for women's borrowing. The findings also show that past credit has a positive effect on some current outcomes, indicating increasing returns to borrowing—especially women's borrowing—for household outcomes. The static model results are either confirmed or strengthened by the dynamic model, which also controls for the effect of LDVs.

Results of the basic model unequivocally show that group-based credit programs have significant positive effects in raising household welfare, including per capita income, household nonland assets, and net worth. Microfinance increases income, the labor supply of men and women, household nonland assets and net worth, as well as boys' and girls' school enrollment. The results using long-panel data thus confirm most of the earlier findings—that microfinance matters a lot, and more for female than for male borrowers.

The results support the view that microcredit effects change over time, and show that the effects of current borrowing differ from those of past borrowing. For example, past credit has a higher impact on income than current credit. With higher village-level participation in microfinance programs, there is also a sign of village diseconomies due to market saturation. For example, with higher village-level aggregate, current male borrowing, the marginal effect of male borrowing on per capita income is lower.

The results also show certain patterns in participants' inclination to borrow from multiple sources and to diversify income. Multiple program membership, which has grown steadily over the past two decades, appears to have negative effects on some household outcomes. However, microfinance competition—captured by the number of MFIs operating in a village—appears to have beneficial effects, especially on growth in household nonland assets and net worth.

Results of the panel household analysis indicate the presence of market saturation, with possible village diseconomies and diminishing returns. This is due, in part, to credit expansion without much technological breakthrough in local economies. As discussed in chapter 2, our data suggest that more than two-thirds of the activities supported by microcredit programs in rural Bangladesh are in the trade and services sector, and this pattern of loan portfolio even increased over the study period (1991/92–2010/11). Trade is perhaps now saturated with microcredit loans, and households have already started to experience diminishing returns. In such circumstances, households must be assisted through skills training and development of improved marketing networks to expand activities into more rewarding sectors and beyond the local economy; otherwise, microfinance expansion cannot be sustained. In short, the current microfinance policy of credit expansion alone may not be enough for boosting income and productivity and thus sustained poverty reduction.

Annex 4A: Estimated Effects of Microfinance: Literature Review

There are two strands of studies on the beneficial role of microfinance. The first strand is based on nonexerimental research methods, while the second one is based on RCTs. Many of those in the first strand observed that microfinance promotes social, human, and economic development in numerous ways (e.g., Boonperm, Haughton, and Khandker 2009; Chemin 2008; Dunford 2006; Hossain 1988; Imai, Arun, and Annim 2010; Islam and Maitra 2012; Kaboski and Townsend 2005, 2012; Kevane and Wydick 2001; Khandker 1998, 2005; McIntosh 2008; Panjaitan-Drioadisuryo and Cloud 1999; Pitt and Khandker 1996, 1998; Pitt and others. 1999 Shaw 2004). For example, a recent study using panel data from 1997 to 2005 found that medium-term participants benefit more than short-term ones (Islam 2011). Another study using a long-panel survey (1991/92–2010/11) also found that households that remained with microfinance programs without breaks fared better than those who participated irregularly (Khandker and Samad 2014). A macro study using cross-country data also confirmed the positive impacts of microfinance (Imai and others 2012).

Findings from the RCT-based, second strand of studies were mixed. Notable among them are randomized evaluations of microfinance programs in six countries (India, Ethiopia, Mongolia, Morocco, Mexico, and Bosnia and Herzegovina), presented in a special issue of *American Economic Journal*, which we briefly describe here. In a study of 52 randomly selected neighborhoods in Hyderabad, India, Banerjee, Karlan, and Zinman (2015) found that, while expenditure on durable goods increased due to microcredit, overall consumption did not. The same study also found that small business investment and profits in preexisting businesses increased because of microcredit, but no changes were found in health, education, or women's empowerment. In rural areas of two provinces in Ethiopia, Tarozzi, Desai, and Johnson (2015) examined microcredit impacts on various outcomes (e.g., income, labor supply, education, and women's empowerment), using data from an RCT study conducted in 2003–06; they found no impacts for most of the outcomes of interest. A randomized study conducted in Mongolia found that group-based microfinance had positive impacts on female entrepreneurship and food consumption but none on employment hours or income (Attanasio and others 2015). In rural Morocco, a randomized evaluation of a microcredit intervention in 2006 found low uptake in the program areas (13 percent) but a significant increase in investment in self-employment activities among microcredit borrowers, as well as a gain in profit; however, this study also found a reduction in income from casual labor (Crépon and others 2015). Angelucci, Karlan, and Zinman (2015) conducted a randomized study of 16,000 households from the largest microlender in Mexico and Latin America, Compartamos Banco, attempting to assess community-level impacts on a wide range of outcomes (i.e., entrepreneurship, income, labor supply, expenditures, social status, and subjective well-being). The study also examined the distributional effects of microcredit benefits. However, it found no transformative impacts on the outcomes, and observed no heterogeneity in the benefits. Finally,

Augsburg and others (2015) conducted a randomized study of an MFI in Bosnia and Herzegovina, finding evidence of higher self-employment, business assets and profit for microfinance borrowers, with a reported drop in wage employment, consumption, and savings. Various other randomized studies found positive effects of microfinance (Coleman 1999, 2006; de Mel, McKenzie, and Woodruff 2008; Karlan and Zinman 2010; McKenzie and Woodruff 2008), while others did not (e.g., Karlan and Zinman 2011).

Besides the disparity in findings on the impacts of microfinance, there is an ongoing debate on the findings and methodology of Pitt and Khandker (1998), perhaps the most referenced study on microfinance impacts. The study has been questioned on various grounds (e.g., replicability, methodology, and findings) in a sequence of papers and postings by David Roodman (Roodman 2011, 2012; Roodman and Morduch 2009, 2011). Subsequently, Pitt and Khandker (2012) showed that Roodman's claim of nonreplicability of the findings of Pitt and Khandker (1998) was based on flawed techniques, which, upon correction, replicates those findings quite well. Pitt and Khandker (2012) also addressed a question raised by Roodman on the validity of the exclusion restrictions of the instruments used in Pitt and Khandker (1998), showing that the results reported hold up extremely well in the new analysis (Pitt and Khandker 2012). More recently, Pitt (2014) addressed Roodman and Morduch (2014), which sought to refute the findings of Pitt and Khandker (1998), claiming that those findings were not robust to deviations from normality of the second-stage errors, and that this nonnormality is an important source of bias. In response, Pitt (2014) showed that the claim of Roodman and Morduch (2014) is based on a "flawed econometric understanding and a lack of due diligence in formulating and interpreting statistical models." While such debates will continue and perhaps remain inconclusive, they may offer readers a deeper insight into the techniques and methodologies—particularly randomized versus nonrandomized impact methods—adopted in estimating microfinance impacts, or estimating any program effect.

Annex 4B: Joint Significance Test Results

Table 4B.1 Results for Explanatory Variables in the Attrition Equation
N = 1,769

Test statistic	*Log per capita total income (Tk. month)*	*Log male labor supply (hours/ month)*	*Log female labor supply (hours/ month)*	*Log household nonland assets (Tk.)*	*Log household networth (Tk.)*	*Boys' school enrollment (ages 5–18)*	*Girls' school enrollment (ages 5–18)*
$\chi^2(24)$	108.5	97.95	85.39	88.86	98.92	75.95	101.60
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Sources: World Bank/BIDS survey 1991/92 and 1998/99; World Bank/InM survey 2010/11.
Note: For each outcome, attrition is estimated with a probit regression using 1991/92 sample where explanatory variables include all household- and community-level exogenous variables and the outcome variable itself. Then a joint significance test is run for all explanatory variables. The null hypothesis is that explanatory variables do not matter to attrition.

Table 4B.2 Results for Attrition and Attrition-Interacted Explanatory Variables in Outcome Equations
N = 1,769

Test statistic	*Log per capita total income (Tk. month)*	*Log male labor supply (hours/ month)*	*Log female labor supply (hours/ month)*	*Log household nonland assets (Tk.)*	*Log household net worth (Tk.)*	*Boys' school enrollment (ages 5–18)*	*Girls' school enrollment (ages 5–18)*
F(24, 86)	3.17	4.10	4.84	2.43	4.43	4.34	4.94
p-value	0.000	0.000	0.000	0.001	0.000	0.000	0.000

Sources: World Bank/BIDS survey 1991/92 and 1998/99; World Bank/InM survey 2010/11.
Note: The outcome variable is regressed on the attrition dummy, all exogenous variables, and the interactions of attrition and exogenous variables in the 1991/92 sample. Then a joint significance test is run for the attrition variable and the interaction of attrition and exogenous variables.

Notes

1. For example, a study conducted in Guatemala suggests that women entrepreneurs benefit from microcredit programs as they move from self-employment or perhaps from a single hired laborer to two or more hired laborers. As this performance is replicated across a large number of borrowers in a given area, it leads to local economic growth in an economy (Kevane and Wydick 2001).
2. Such studies include the following, among others: Attanasio and others 2011; Augsburg and others 2012; Banerjee and others 2010; Crépon and others 2011; de Mel, McKenzie, and Woodruff 2008; Karlan and Zinman 2010, 2011).
3. The supply-side analysis presented in chapter 3 examines the growth trend in MFI performance indicators (e.g., outreach, savings mobilized, loans outstanding, profitability, loan recovery, and portfolio diversification), using both aggregate-industry and program-level data over the past two decades.
4. The term *upazila* refers to the lowest formal administrative unit in Bangladesh (under division and district units, comparable to a rural subdistrict); in the geographical hierarchy, an upazila is above the village level and consists of about 10–50 villages.
5. Unlike Khandker (2005), the spilt-off households in this study's data analysis are not merged with parent households but are shown having the same parent identification code.
6. After reorganization, BRDB-12 was renamed *Palli Daridra Bimochon Foundation* (PDBF) (Rural Poverty Reduction Foundation). Though this organization is no longer part of BRDB, it is still within the same ministry of local government and rural development. To avoid confusion, the name BRDB is used throughout this book.
7. It should be noted that MFIs in Bangladesh charge interest rates as high as 35 percent, compared to about 13 percent charged by commercial banks; however, commercial banks do not lend to the poor, whose only option is to borrow from the MFIs or, alternatively, from informal lenders, who may charge interest rates as high as 240 percent per year (Faruqee and Khalily 2011).
8. For the purpose of reporting microcredit loan figures, this analysis distinguishes Grameen Bank and BRAC from other microcredit programs because these two programs have been consistently dominant throughout the last 20-year period.
9. These figures are consumer price index—adjusted.
10. It should be noted that women's share of loans (e.g., 38 percent in 2010/11) does not represent the overall distribution of BRAC's membership by gender. BRAC's

microloans, known as "dabi," are available only to women, while its SME loans, called "progoti," are available to both men and women. While it is true that more men than women take out SME loans, the vast majority of BRAC's clients are women. For example, in 2013, BRAC had 4 million microloan clients and 300,000 SME clients; disbursements under the microloans (dabi) totaled US$810 million, compared to US$668 million under the SME loans (progoti). Thus, women's share of BRAC loans was not more than 55 percent if all progoti loans were received by men and was slightly higher if a certain percentage of women received progoti loans.

11. Unlike other MFIs, Grameen Bank also mobilizes voluntary savings from both members and nonmembers; in recent years, its savings accounted for more than 80 percent of loans outstanding.
12. Because of microfinance's critical role in alleviating poverty in rural Bangladesh, we devote a separate chapter to examining the expenditure and poverty aspects of household welfare (chapter 6). We also consider various categories of income (e.g., farm and nonfarm) to determine whether microfinance helps the agriculture sector (chapter 7).
13. The panel data are useful for impact estimation but are not immune to estimation bias, as discussed later in this chapter.
14. This was done separately for the nine outcome variables.
15. Details on implementing this procedure can be found in Baluch and Quisumbing (2011).
16. This procedure differs from that done in Khandker (2005), which analyzed the two-point panel data (consisting of 1991/92 and 1998/99 surveys) by aggregating the split-off households at the second round after testing to ensure such households could be aggregated without incurring bias in the estimation process. Since the spawned households in the current analysis are treated as separate units, it is not necessary to perform such tests.
17. Propensity score weight is defined as follows: $pw = 1/p$ for participants, and $pw = 1/(1 - p)$ for nonparticipants, where p is the propensity score and pw is the propensity score weight. In this way, it is possible to obtain efficient estimates of average treatment effect using weight created from propensity score (Hirano, Imbens, and Ridder 2003).
18. Constructing second LDVs of boys' and girls' school enrollment is not possible since these variables are at the individual level; thus there are no regression results for school enrollment outcomes.
19. Similar time-varying effects of program participation (measured by a dummy variable) can be estimated. Khandker and Samad (2014) differentiated the effects of regular or continuous program participation from the effects of irregular or discontinuous participation.
20. That is, the past credit of a household in 1998/99 is equal to its current credit in 1991/92, and its past credit in 2010/11 is equal to its current credit in 1998/99. The past credit in 1991/92 is defined as zero for all households.
21. The credit demand function is a reduced form equation. However, we could also have allowed the credit demand to depend on past characteristics (lagged model), in addition to current characteristics. But that change in first-stage specification would not have changed the consistency of the second-stage outcome equation. Moreover, if we used the FE method instead of the FE-IV method for the outcome equation in the panel analysis, the first-stage credit equation would become a nonissue.

22. However, for such outcomes as consumption and poverty, the opposite is the case (see chapter 6).
23. In the case of village economies (market saturation with increasing returns), the demand for credit would induce multiple membership if the additional demand for credit were not met by a single source.
24. A single source may not be used to meet demand because (a) the lender has a ceiling per borrower or (b) the lender's perceived credit risk is high.

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