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Dynamic Effects of Microcredit in Bangladesh

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Abstract

This paper uses long panel survey data spanning over 20 years to examine the dynamics of microcredit programs in Bangladesh. With the phenomenal growth of microfinance institutions representing 30 million members with over $2 billion of annual disbursement over the past two decades, it is important to understand the dynamics of microcredit expansion and its induced impact on household welfare. A dynamic panel model is used to address a number of issues, such as whether credit effects are declining over time, whether market saturation and village diseconomies are taking place, and whether multiple program membership, which is rising as a consequence of microcredit expansion, is harming or benefiting the borrowers. The paper's results confirm that microcredit programs have continued to benefit the poor by raising household welfare. The beneficial effects have also remained higher for female than male borrowers. There are diseconomies of scale caused by higher levels of village-level borrowing, especially for male members. Multiple program membership is also growing with competition from microfinance institutions, but this has rather helped raise assets and net worth more than it has contributed to indebtedness.

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Dynamic Effects of Microcredit in Bangladesh

1. Introduction

Microfinance has received donor endorsement as an anti-poverty program, because it targets and reaches the poor, especially women, as well as small producers and entrepreneurs who often have limited access to formal financial institutions. Despite its overwhelming success in reaching the poor, induced benefits of microfinance (measured in terms of income and consumption and other dimensions of household welfare) are debated. Some studies found substantial positive effects, while others found no or even negative effects. The most substantive evaluation of microcredit was carried out two decades ago by researchers at the World Bank, which examined three well-known credit programs in Bangladesh using the 1991/92 cross-sectional survey data. This evaluation found that microcredit helped promote household welfare, and the impacts of credit are higher for women than men (Khandker 1998; Pitt and Khandker 1996; 1998; Pitt et al. 1999; Pitt et al., 2006).[2] The findings have been debated because of restrictive statistical assumptions for model identification of program benefits (Morduch 1998; Roodman and Morduch 2009; 2014). However, the claims of statistical limitations were also invalidated (Pitt 1999, 2014; Pitt and Khandker, 2012).

A number of studies that employed less restrictive assumptions such as randomized control trial (RCT) methods find limited or no benefits of microfinance.[3] The RCT studies are often based on assessments of short-term program effects (such as 18 months of intervention). But a certain minimum length of program membership is necessary to measure the effects of microcredit since

[2]A study from Guatemala suggests that female 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 of borrowers in a given area, this leads to local economic growth in an economy (Kevane and Wydick 2001).

[3] Studies include the following, among others: Karlan and Zinman 2010; de Mel, McKenzie and Woodruff 2008; Augsburg et al., 2012; Attanasio et al., 2011; Banerjee et al., 2010; Crepon et al., 2011; Karlan and Zinman 2011).

microfinance is not cash transfer, and its benefits from self-employed activities take time to be realized. Keeping this in mind, the earlier study of Pitt and Khandker (1998) used a cross-sectional survey of households in villages where the program had been in place for at least 3 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).

Bangladesh's experience with microfinance has been phenomenal. In 2008, there were some 30 million members of MFIs who received an annual disbursement of US$1.8 billion with an outstanding balance of US$1.5 billion (Khandker et al. 2013). There were some 576 registered MFIs in June, 2011, compared with only a few in 1991/92. With rising competition among MFIs in a market with limited diversification of product design and marketing, continued borrowing by poor households may create conditions for market saturation, resulting in village diseconomies. Thus, microcredit growth may create micro-debt dependencies, where participants have the possibility of borrowing from multiple sources and are likely to be over-indebted or trapped in poverty.

It is alleged that too much credit or too many MFIs are neither good for borrowers nor good for the economy. A recent study based on a long panel survey of 1991/92-2010/11 shows that multiple program membership has increased manifold over the years. Nonexistent in 1991/92, it represented 8.9 percent of borrowers in 1998/99 and 31.9 percent in 2010/11 (Khandker, Faruqee, and Samad, 2013). But the growth of multiple program membership has not yet caused loan recoveries of MFIs to fall (Khandker, Koolwal and Badruddoza 2013), nor resulted in long-term borrowers necessarily being trapped in poverty or debt as many contended in recent years (Khandker and Samad 2013).

This paper, using data from the long panel survey spanning over 20 years, explores dynamic issues beyond what Pitt and Khandker (1998), Khandker (2005) and Khandker and Samad (2013) had explored. More specifically, this paper explores whether the effects of short-term loans vary from long-term loans,[4] if the changing effects of market conditions contribute to market saturation and village-level diseconomies, and whether market saturation and multiple program membership are causing adverse effects on household welfare.

The paper is organized as follows. Section 2 discusses the dynamics of microcredit using long panel data that were made available with a research grant from the World Bank, by re-surveying the households and communities in 1998/99 that were initially surveyed in Bangladesh during 1991/92 by the Bangladesh Institute of Development Studies. Section 3 discusses the pitfalls of panel data in terms of attrition and split-off. Section 4 presents the impact results using alternative household-level fixed-effects estimation methods and verifies whether estimation method matters. Section 5 addresses the hypothesis that the impact of credit varies over time, with the possibility that returns to microcredit programs decline over time. Section 6 addresses the possibility of market saturation in microcredit market in the sense that the returns to late entrants of microfinance borrowers are lower than those of their counterpart early entrants. Section 7 examines whether multiple program membership has any harmful effects on household welfare. Finally, the concluding section summarizes the results.

2. Dynamics of microcredit: A descriptive analysis

The data used in this study were derived from a long panel survey over 20 years. The first round cross-sectional survey, conducted in 1991/92, was studied to determine the role of microfinance in the economic and social 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 sub-

[4] This is different from the effects of continued versus irregular participation as addressed in Khandker and Samad (2014) which observed that the effects of continuous participation are higher than those of irregular participation. The current paper examines the impact of cumulative borrowing (in taka) over time on outcomes such as income, expenditure and net worth.

districts) of Bangladesh.[5] A second round, conducted in 1998/99 with the help of BIDS, could not retrace 131 of the 1,769 households from the 1991/92 survey, and so surveyed 1,638 available households, implying an attrition rate of 7.4 percent. The 1998/99 survey also included new households from old villages and newly selected villages; in total, 2,599 households were surveyed, of which 2,226 were from old villages and 373 were new ones. Among the 2,226 households in old villages, 279 were newly sampled ones and 1,947 were from the 1,638 households of 1991/92 which split to form new households in the years between the two surveys.

These households were surveyed a third time in 2010/11 with the help of the Institute of Microfinance (InM). The third survey round tried to revisit all of the households (2,599) surveyed in 1998/99. But 2,342 households were traced (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 2010/11 and was completed in September 2010/11.

The analysis of this study is based on 1,509 households from 1991/92 that are common in all three surveys. Of course, because of household split-off, we have higher number of households in 1998/99 (1,758) and 2010/11 (2,322) (Table 1). As Table 1 indicates, over the three survey years, 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 is the Bangladesh Rural Development Board (BRDB), a government program, which lost a good share of its members due to reorganization between the second and third survey rounds. Membership in Grameen Bank, the largest of all the programs, increased 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 (i.e., Grameen Bank, 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, the coverage of

[5] An *upazila* is an administrative unit smaller than a district, consisting of a number of villages.

these programs included nearly 33 percent of the rural households, which was higher than that of Grameen Bank.

An important aspect of microcredit participation membership today is the overlapping membership across multiple programs, a phenomenon that hardly existed in the early 1990s. Yet its substantial growth since then is evident in the third survey round (2010/11), which showed that nearly 61 percent of Grameen Bank members were also members of other programs (Khandker and Samad 2013). Overall, about 31.9 percent of households in rural Bangladesh were members of multiple microcredit programs in 2010/11, which increased from 8.9 percent in 1998/99 and zero in 1991/92.

Participation in microcredit programs does not necessarily imply borrowing. New members in many programs must wait for some time before they can borrow, and some programs feature a non-borrowing 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, while about 69 percent of rural households were microcredit members, borrowers constituted about 56 percent of the households, implying that 13 percent of rural households were non-borrowing members (shown in parentheses in Table 1).

While microcredit programs have offered various noncredit services in the past, they have become mostly credit-only institutions over time, and it is through access to credit, not just participation, that households can reap the benefits.[6] As such, this study 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 steadily by nearly 100

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[6] 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 from informal lenders who may charge interest rates as high as 240 percent per year (Faruqee and Khalily 2011).

percent over time.[7] 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 annually over the 20-year period (Table 2).[8]

The highest growth in borrowing occurred for the smaller programs (reported in the 4th column of Table 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 are received by women who are particularly targeted by the MFIs (Table 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 microloans was much higher (e.g., 95 percent in 1998/99); but over time most of the BRAC loans are extended to small- and medium-sized enterprises (SMEs) which are operated more by men than women.

Another feature of microfinance operations in Bangladesh is the mandatory savings, mostly in the form of weekly savings and deposits out of a certain percentage of the loans disbursed. Member savings represented about 8 percent of cumulative borrowing in 1991/92, increasing slightly to 10 percent in 1998/99 and 2010/11 (Table 2), which nonetheless accounts for some 60 percent of MFI loans outstanding in Bangladesh.[9]

Before estimating the microcredit effects on the outcomes of particular interest, it is worthwhile to investigate how the outcomes vary by program participation status and across years. As Table 3 shows, household income and expenditures grew significantly over time for both the participants and non-participants; in particular, household income increased by more than 100%

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[7] For the purpose of reporting microcredit loan figures, we have distinguished Grameen Bank and BRAC from other programs because these two programs have been consistently dominant throughout last 20 years.
[8] These figures are CPI adjusted.
[9] Unlike other MFIs, Grameen Bank also mobilizes voluntary savings from its members and non-members; thus, Grameen's savings account for more than 80 percent of the loans outstanding in recent years.

from 1998/99 to 2010/11. Not surprisingly, poverty indices (moderate and extreme poverty) decreased during the last 20 years, but they decreased more for the participants (particularly, those in extreme poverty) than for the non-participants. While the labor supply for participants changed in an inconsistent way over time, it actually experienced a drop for the non-participants. The change in household non-land asset and net worth followed a pattern similar to that of income or expenditure by registering substantial monotonic growth. Table 4 reports the school enrollment status of boys and girls. Like the economic and labor supply outcomes, education outcomes also experienced a consistent growth over time for both the participants and non-participants.

As Tables 3 and 4 suggest, while the outcomes improved over time, the differences between the participants and non-participants do not show a consistent pattern. For example, while per capita expenditure of non-participants was higher than that of participants in 2010/11 (with the difference being statistically significant), the opposite trend is observed for extreme poverty – it is less for the participants than for the non-participants in 2010/11, and the difference is statistically significant. It is worth emphasizing here that while the descriptive statistics of the outcome variables can show a trend over time and by participation status, it cannot establish the causality linking microcredit participation and changes in outcomes. For that, we are going to resort to regression analysis later in this section.

3. Pitfalls of panel data analysis: Household attrition and split-off

Panel data are good for studying dynamic issues and resolving endogeneity of program participation and program placement, but they are not without any limitations. Two major issues with panel data are sample attrition and split-off.[10] 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

[10] The panel data are good for impact estimation but not immune to estimation bias. We discussed this later in the estimation section.

analysis is supposed to have. Researchers, therefore, make efforts to minimize attrition and follow a rigorous procedure to locate previously surveyed households. As mentioned before, our survey data were subject to attrition during the two follow-ups: 7.4 percent from 1991/92 to 1998/99 (131 households out of 1,769 from 1991/92 were not located in 1998/99) and 7.9 percent from 1998/99 to 2010/11 (129 households out of 1,638 from 1998/99 were not re-interviewed in 2010/11). Overall, the attrition rate is 14.7 percent from 1991/92 to 2010/11, which is less than one percent a year.

However, what is important is not the extent of attrition but whether attrition is non-random. To estimate the determinants of attrition, a probit regression is run using the 1991/92 data with attrition dummy (1 for households that were lost, 0 otherwise) as the dependent variable, and the outcome variable (income, expenditure, school enrollment, etc.), household- and village-level characteristics as explanatory variables. This is done separately for the 9 outcome variables. Results of the regressions (not reported) show that attrition is positively correlated with households with less land or non-land assets, absence of adult males or females, and those in villages with poor road conditions, a proxy for infrastructure. That is, attrition is more likely to occur among households with low socio-economic status and in less developed villages. These findings are consistent with other studies on household attrition (Alderman and others 2000; Fitzgerald, Gottschalk and Moffitt 1998; Thomas, Frankenberg and Smith 1999; Ziliak and Kniesner 1998). For example, Fitzgerald, Gottschalk and Moffitt (1998) found from the Michigan Panel Study of Income Dynamics (PSID) that households with lower earnings, lower educational levels and lower marriage propensities are more prone to attrition. Overall, these variables explain only 7 to 10 percent of the probability of household attrition, which implies that up to 93 percent of the attrition cannot be explained by the explanatory variables and may be random. We also perform the Wald joint significance test to see whether the explanatory variables are jointly equal to zero, and the results of the tests are reported in

Appendix Table A1. The resulting Chi-squared statistics indicate that these variables are jointly different from zero at the highest level of significance (the p-value is 0.000). This implies that these variables are significant predictors of attrition; that is, attrition may not be random.

We also perform the Becketti, Gould, Lillard and Welch test to determine whether attrition is random, which involves regressing the outcome variable on household and community level exogenous variables, the attrition dummy, and the interactions of the attrition dummy and the other explanatory variables.[11] Then a joint significance test of the attrition dummy and the interaction variables is performed 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 we can reject the null hypotheses that attrition is random. From the results (Appendix Table A2) we see that at the 5 percent level, the randomness of attrition is rejected for all outcomes.

This non-randomness of attrition will introduce attrition bias in the estimated impacts if not corrected for. There are a number of ways attrition bias can be addressed, for example, estimating a selection model, which depends on finding suitable instruments (Heckman 1979); using inverse probability weights, which relies on auxiliary variables are related to both attrition and the outcome variables (Fitzgerald, Gottschalk and Moffitt 1998), using non-parametric techniques (Das, Toepoel, and van Soest 2011), and so on. We 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 households who were subsequently lost than to those with similar initial characteristics but are more likely to remain in the panel. Details on the implementation of this procedure can be found in Baluch and

[11] See Becketti and others (1988).

Quisumbing (2011). We calculate such inverse probability weights for all outcomes and then use them in all estimations that are discussed from now on.

Apart from attrition, households are also subject to split-offs over time. In most of these cases, household members grow up, marry, and leave their households after the initial survey to form new households. Thus households surveyed in the first round may spawn one or more new households by subsequent surveys. In our analysis we have treated these households as separate units who are related by their same initial (first-round) characteristics.[12]

4. Validating the earlier estimates of microcredit effects using third round survey data

Earlier results of microcredit programs using the cross-sectional survey of 1991/92 in Pitt and Khandker (1998) and the panel survey of 1991/92-1998/99 in Khandker (2005) used the amount of cumulative borrowing over the past five years as a measure of intervening variable. Following Pitt and Khandker (1998), let us consider the following reduced-form demand equation for the amount of credit by women (C_{ijft}) and men (C_{ijmt}) of i-th household in j-th village in period t as[13]:

$$C_{ijft} = X_{ijt}\beta_{cf} + \eta^{c}_{ijf} + \mu^{c}_{jf} + \varepsilon^{c}_{ijft} \qquad (1)$$

$$C_{ijmt} = X_{ijt}\beta_{cm} + \eta^{c}_{ijm} + \mu^{c}_{jm} + \varepsilon^{c}_{ijmt} \qquad (2)$$

where X is a vector of household (e.g., sex, age and education of household head, landholding, and so on) and village (e.g., extent of village electrification and irrigation, availability of infrastructure, prices of consumer goods, etc.) characteristics, β is a vector of unknown parameters to be estimated, η is an unmeasured determinant of the credit demand that is time-invariant and fixed within a

[12] This is different from what was 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 make sure that such households can be aggregated without incurring bias in the estimation process. Since we have treated the spawned households as separate units we do not have to go through such tests.

[13] Unlike Pitt and Khandker (1998), we do not allow the credit effect to vary by program such as Grameen Bank, BRAC, and other MFIs on the assumption that programs do not have heterogeneous effects as the program design is very similar for all MFIs in Bangladesh.

household, μ is an unmeasured determinant of the credit demand that is time-invariant and fixed within a village, and ε is a non-systematic error.

The conditional demand for outcomes (Y_{ij}) (such as consumption, children's schooling or women's labor supply) conditional on the level of credit demand (C_{ij}) is

$$Y_{ijt} = X_{ijt}\beta_y + C_{ijft}\delta_f + C_{ijmt}\delta_m + \eta_{ij}^y + \mu_j^y + \varepsilon_{ijt}^y \qquad (3)$$

where δ_f and δ_m are the "common" effects for female and male credit, respectively.[14]

Our interest is to estimate the impact of male and female credit separately on outcomes of particular interest such as household per capita expenditure, non-land asset, boys' and girls' schooling, and so on. With cross-sectional data (where t=1), the endogeneity arises as a result of the possible correlation among μ_{jf}^c , μ_{jm}^c, and μ_j^y , and among ε_{ijm}^c , ε_{ijf}^c and ε_{ij}^y (Pitt and Khandker 1998). Pitt and Khandker (1998) used a village-level fixed-effect method to resolve program placement (or village-level) endogeneity of the 1991/92 data since a household-level fixed-effect method could not be applied with cross-sectional data. So Pitt and Khandker (1998) adopted a two-stage instrumental variable (IV) type method to resolve the endogeneity of a household's participation. In this IV framework, they used exogenous gender- and landholding-based exclusion restrictions to create a discontinuous household program choice variable. That variable was interacted with household's observable characteristics to create instruments.

With the availability of panel data (t>1), the household-level fixed-effects method can be used and two-stage identification restriction is not needed. This is simply done by differencing out the unobserved village and household attributes, which are the sources of correlation between the

[14] Here we do not differentiate the time-varying or dynamic effects of credit, e.g., effects of past and present credit. We'll relax this assumption later.

credit demand and household outcome equations. Differencing equation (3) at two points of time yields the following outcome equation:[15]

$$\Delta Y_{ijt} = \Delta X_{ijt}\beta_c + \Delta C_{ijft}\delta_f + \Delta C_{ijmt}\delta_m + \Delta\varepsilon^y_{ijt} \qquad (4)$$

Consistent estimates of the credit effect δ_f and δ_m can be obtained from equation (4) by using a household fixed-effects method without recourse to instrumental variable estimation, under the assumption that the error terms of the credit demand and outcome equations are uncorrelated. This is what was followed in Khandker (2005). However, error terms can still be related because the unobserved socio-economic factors, which are assumed to be fixed at the household level, may actually change over time. Under such circumstances, equations (1) and (2) can be rewritten after incorporating the variations in η and μ over time as,

$$C_{ijft} = X_{ijt}\beta_{sf} + \eta^c_{ijft} + \mu^c_{jft} + \varepsilon^c_{ijft} \qquad (5)$$

$$C_{ijmt} = X_{ijt}\beta_{sm} + \eta^c_{ijmt} + \mu^c_{jmt} + \varepsilon c^s_{ijmt} \qquad (6),$$

which in turn changes the outcome equation as follows:

$$Y_{ijt} = X_{ijt}\beta_c + C_{ijft}\delta_f + C_{ijmt}\delta_m + \eta^y_{ijt} + \mu^y_{jt} + \varepsilon^y_{ijt} \qquad (7)$$

Taking the difference over two periods (t=1 and 2), we get the following difference equation (4) as:

$$\Delta y_{ijt} = \Delta X_{ijt}\beta_y + \Delta C_{ijft}\delta_f + \Delta C_{ijmt}\delta_m + \Delta\eta^y_{ijt} + \Delta\mu^y_{jt} + \Delta\varepsilon^y_{ijt} \qquad (8)$$

Thus, we see that even household-level panel data may yield inconsistent estimates of program impacts, if the changes in outcome levels depend on changing unobserved household and village-level characteristics.

Another source of concern with panel data is the possibility of measurement errors. If credit is measured with error (which is likely), this error may get amplified when differencing is done over

[15] For panel data consisting more than two time points, this difference is taken between the value of a variable at each time point and the average value of the variable for all time points.

time, especially with only two time periods. This measurement will then impact "attenuation bias" of the credit coefficients, meaning that the impact estimates will be biased towards zero.

A standard correction for both types of problems (time-varying heterogeneity bias and measurement error bias) is the use of instrumental variables (Deaton 1997). Provided we have instruments, we could reintroduce the instrumental variable (IV) method to the FE method to estimate the credit effects by correcting such biases. The case for fixed-effects instrumental variables (FE-IV) method as opposed to fixed-effects (FE) method is tested by the Durban-Wu-Hausman test.

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

$$C = X_t\beta + Z_t\gamma + \varepsilon_t \qquad (9)$$

where Z is a set of household and village characteristics distinct from X's so that they affect C but not household outcomes dependent on C.

Selecting appropriate Z variables is critical. We created household-level male and female choice variables separately for 1991/92 and 1998/99 and used the interactions of the choice variables with all household characteristics and village-level fixed-effects as the instruments.[16] Although the same instruments were used in 1991/92 data analysis with cross-sectional data, the purpose then was to resolve the households' program placement endogeneity and measurement errors associated with credit variables, and this time it is to control for any measurement and time-varying errors correlated with credit variables. The identification of gender-specific choice variables is achieved by making use of the fact that all program groups are single sex and not all villages have both a male and a female group. The sample includes households from all types of villages. The

[16] Choice variables are created based on a household's land-based eligibility to join a program and a program's placement in a household's village.

necessary assumption is that the availability of a credit group by gender in a village is uncorrelated with the differenced household errors, ε, conditional on X.[17]

Three more variants of household level FE method may be utilized besides the simple FE-IV method. One variant includes initial conditions such as the household and community characteristics (X) in FE regression framework, assuming that these initial factors control for the time-varying heterogeneity that correlates the errors of the borrowing and outcome equations. More specifically, following Jalan and Ravallion (1998), we rewrite model (3) as follows:

$$\Delta Y_{ijt} = \alpha \Delta X_{ijt} + \rho \Delta C_{ijt} + \gamma X_{ij0} + \Delta \varepsilon_{ijt}^{y}, \qquad (10)$$

where X_{ij0} is a vector of household- and village-level characteristics for the initial survey year (i.e., 1991/92).

A second variant, following Heckman (1981), is the lagged dependent variable (LDV) method applied to the above equation (10). That is, we assume that current outcome depends on lagged (say one period lag) outcome besides the credit intervention variable and other time-varying exogenous characteristics plus the initial exogenous characteristics as given in equation (10). Thus, we have the following lagged model to estimate,

$$Y_{ijt} = \lambda Y_{ij(t-1)} + \alpha X_{ijt} + \rho C_{ijt} + \gamma X_{ij(t-1)} + \varepsilon_{ijt}^{y}, \qquad (11)$$

A third way to control for the bias due to time-varying heterogeneity is to apply a propensity score–weighted FE method. Following Hirano, Imbens and Ridder (2003), we first calculate the weight variable on the basis of the propensity score obtained from the participation equation, using the control variables obtained from the 1991/92 survey data. More specifically, the weight variable is given a value 1 for participants and $p/(1\text{-}p)$ for non-participants, where p is the propensity score (probability of receiving microcredit during any time). In the second stage, the impact of microcredit

[17] Note that the sample villages without any microcredit programs in the earlier rounds lost their control status because of proliferation of credit programs into them.

is estimated using household-level FE as before, but this time as a weighted regression incorporating the propensity score weight variable.[18]

Durban-Wu-Hausman test is carried out to determine whether FE, FE-IV, FE with initial conditions, and p-score weighted FE model is more appropriate for estimating program effects (not shown here). The model with FE weighted by p-score appears slightly more appropriate than others for most of the outcomes.[19] Yet the results are not much different among them. We present results of all four models for comparison. Table 5 presents the results of simple FE method and Table 6 presents those of FE with p-score weights, while Table A3 presents the results of FE-IV and Table A5 presents the FE with initial conditions.[20]

From the results of FE estimates, reported in Table 5, we find that borrowing by men increases per capita expenditure, men's labor supply, non-land assets, household net worth and girls' schooling enrollment rate. For example, a 10 percent increase in men's borrowing raises household per capita expenditure by 0.04 percent, male labor supply by 0.18 percent, non-land asset by 0.28 percent, and net worth by 0.2 percent. On the other hand, a 10 percent increase women's borrowing increases per capita income by 0.06 percent, male labor supply by 0.33 percent, female labor supply by 0.46 percent, household non-land asset by 0.25 percent, and boys' and girls' schooling enrollment rates by about 8 percentage points, while reducing extreme poverty by 5 percentage points.

The findings do not vary much from those obtained from p-weighted FE estimates (reported in Table 6), only difference being that the effect of female credit on household income is no longer significant, and male credit lowers extreme poverty. The findings reported in Tables 5 and

[18] Another estimation technique sometime used to control for time-variant heterogeneity is the Dynamic Panel Generalized Method of Moments (GMM) estimator. Unfortunately, with just 3 time periods in our data set, some necessary data transformations for GMM implementation cannot be achieved. For example, the endogenous program participation must be instrumented with its own second or deeper lagged variable because, unlike the first lagged variable, it is not correlated with the error term. With 3-time points (such as in our data), such second lagged variable is invalid for first two time points (that is, 1991/92 and 1998/99 in our data).

[19] In particular, FE is more appropriate than FE-IV as shown in appendix table A4.

[20] Credit amount is in log form and except for poverty and school enrollment, all other outcomes are also expressed in log form. Using a log-log model reduces the effects of outliers and makes errors more homoskedastic.

6 also confirm many of the findings of the cross-sectional study of Pitt and Khandker (1998) that only used 1991/92 data. The results using the 3 rounds of data also corroborate the poverty reduction effects of microcredit as found in Khandker (2005), who used the data of first two rounds (1991/92 and 1998/99).

5. Estimating dynamic effects of microcredit

The estimates of credit effects might not be the same across years if timing of the borrowing matters; that is, if the credit received in the past affects behavior differently from the credit received recently. In other words, unlike the implied assumption in equation (3), parameters of the credit demand and other regressors may vary over time, allowing for differential credit impacts over time.[21]

Credit effects may vary by time for different 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, focus more on accumulating assets, and thus cement the new insurance network, a result of group participation. As time goes by, they may have a larger 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 non-credit effects of program participation are important, and if the changes in attitudes they engender are functions of time spent in the group, 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

[21] 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 participation from the effects of irregular or discontinuous program participation. Table A6 shows the test of whether male and female credit can be combined. The test shows that they cannot be combined for most of the outcomes considered here.

effects possibly because of the differences in group dynamics and the different type of self-employment activities they undertake over time.

Taking into account the separate effects of year-specific credit, we rewrite equation (3) as follows:

$$Y_{ijt} = \sum_{k=1}^{n} X_{ijkt}\beta_k + \sum_{k=1}^{n} C_{ijfkt}\delta_{fk} + \sum_{k=1}^{n} C_{ijmkt}\delta_{mk} + \eta_{ij}^{y} + \mu_{j}^{y} + \varepsilon_{ijt}^{y} \qquad (12)$$

Here, $k = 1, 2, \ldots, n$ refers to individual survey years and n=3 (that is, k=1 for 1991/92, 2 for 1998/99 and 3 for 2010/11). The FE estimates of this model (allowing the credit effects to vary over time) are shown in Table 7.[22]

Interestingly, the impacts of borrowing by either men or women on the outcomes are significantly different across survey years. For example, the response elasticity of household non-land asset with respect to female borrowing was 0.025 with 2010/11 credit, compared to 0.026 with 1991/92 credit and only 0.014 with credit obtained in 1998/99. On the other hand, the response elasticity of household non-land assets with respect to male borrowing was 0.043 with credit obtained in 2010/11, and 0.027 with credit obtained in 1991/92. That is, for both male and female credit the returns slightly declined over 1991/92-1998/99 but then increased between 1998/99-2010/11. The return declines for household net worth in case of both male and female borrowing.[23] These findings do suggest that the program effects change over time, and perhaps decline over time for some outcomes. In fact, the returns are either zero or negative for a few outcomes in the year of 1998/99.

Just as the credit effects vary by time, it is also possible that past credit has a lingering impact so that it affects not only the outcomes of the past but also those in the future. This question is

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[22] To verify whether credit effects vary by year, we performed a test of equality of the parameters of the regressors among three periods using the results reported in Table 7 and found that equality is rejected (see Appendix Table A7).

[23] In fact, the returns to borrowing are negative for borrowing obtained in 1998/99, which perhaps reflect the fact that the survey was done just after 3 months (more specifically, it started in February 1999) of the wide-scale flood of 1998 (during the month of October-November).

important in assessing the long-term impacts of microcredit as opposed to short-term impacts which are confined only to the period of borrowing. In order to assess the long-term impacts of microcredit we can further revise equation (12), allowing for the possibility that past credit might affect current outcomes

$$Y_{ijt} = X_{ijct}\beta_c + X_{ijpt}\beta_p + C_{ijfct}\delta_{fc} + C_{ijfpt}\delta_{fp} + C_{ijmct}\delta_{mc} + C_{ijmpt}\delta_{mp} + \eta_{ij}^y + \mu_j^y + \varepsilon_{ijt}^y \qquad (13)$$

Here, subscripts *c* and *p* refer respectively to current and past credits and also the set of current and past control variables (*X*). The current credit at a period *t* (where *t* is 1991/92, 1998/99 or 20101/11) is defined as the cumulative amount of borrowing by household males or females up to that period, and the past credit at period *t* is defined by the cumulative amount of borrowing by household males or females up to period (*t*-1).[24] So this model assumes that even if a household stopped borrowing after period 1 (that is, 1991/92), it can still benefit in period 2 (that is, 1998/99) as past credit may continue to benefit the borrower (provided δ_p>0). We allow, therefore, the impacts of borrowing to be long-term.[25] A test for equality of δ_p and δ_c shows (Appendix Table A8) that we can reject the equality hypothesis, implying the presence of distinct impacts of past and current borrowing on current outcomes.

The FE results of time-varying credit-effects model based on equation (13) are shown in Table 8.[26] The results clearly show that borrowing may affect not only the current outcomes, but also the future ones. For example, let us look at effects of male loans on household non-land assets and net worth. A 10 percent increase in the current male loans increases household non-land assets and net worth by 0.3 percent and 0.2 percent, respectively. But past male credit reduces those

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[24] 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.

[25]The credit demand function is a reduced form equation. However, we could also have allowed the credit demand (equation (1) or (2)) 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 use FE method (as opposed to FE-IV method) for the outcome equation in panel analysis, the first-stage credit equation becomes a non-issue.

[26] The FE results are shown. The other results with other methods are available upon request.

outcomes – a 10 percent increase in past male credit lowers household non-land asset and net worth by 0.25 percent and 0.30 percent, respectively. A similar pattern is observed for female credit in the case female labor supply. While a 10 percent increase in the amount of current female credit increases female labor supply by about 0.5 percent, the same increase in the past female credit lowers female labor supply by 0.23 percent. While these examples show that the past and current credit affect some outcomes in opposite ways, it is not the case for all outcomes. For example, both current and past female credit lowers extreme poverty. Also, for some outcomes it is only the current credit that matters. For example, male labor supply is affected only by the current credit of males and females, implying no lingering effects of past credit of either males or females. A 10 percent increase in male credit increases male labor supply by 0.44 percent, while the same increase in female credit increases it by 0.41 percent.

The fact that the response elasticity is negative for past borrowing and positive for current borrowing may be an indication of decreasing returns to borrowing for some outcomes. However, the returns are not necessarily decreasing for all outcomes. For example, in the case of per capita income and expenditure, although current female loans do not matter, past loans of females do in fact increase both. More specifically, a 10 percent increase in past female borrowing increases household income by 0.16 percent and expenditure by 0.04 percent. This shows increasing rather than decreasing returns to borrowing for females. The same sign of increasing rather than decreasing returns is also evident for male credit effect on per capita income. These examples clearly demonstrates that the timing of the borrowing matters to the credit effects on household outcomes, and past credit may have impacts independent of the current credit. And finally, the effect of past credit may be lingering, diminishing or nonexistent depending on the outcome.

6. Market saturation and village diseconomies of scale

Market saturation of one kind or another may result in decreasing returns to credit as participation rates increase in the village. There may be benefits of being first in the market, so that the later entrants compete away the returns of the earlier entrants. For instance, early entrants financed by group-based credit can choose from the most profitable self-employment activities. Later entrants will either compete away some of the benefits of being first to enter, or will enter into less rewarding activities. On the other hand, 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. For example, if an earlier entrant starts to produce a commodity which attracts investment from other borrowers in such a way that the village becomes the center of certain products or activities, then instead of returns being diminished, they may increase. We would then expect either positive or negative externality out of the market saturation.

We can capture this phenomenon by modeling the effect of credit that depends on some measure of the village rate of participation, after controlling for village fixed effects. In simplest form, we allow the credit effect, α , to vary by village and take the following form:

$$\alpha_j = \alpha_0 + \alpha_\Omega \Omega_j \tag{14}$$

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 fixed effects 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 the placement of a microfinance program may cause a village effect additional to any preexisting (time-invariant) village effects. Assuming that 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 the average village-level male or female participation rate is the village program participation intensity, we write the following regression:

$$C_{ijt} = X_{ijt}\beta + S_{ijft}\delta_f + S_{ijmt}\delta_m + \Omega_{jft}\alpha_f + \Omega_{jmt}\alpha_m + +\eta_{ijt} + \mu_{jt} + \varepsilon^c_{ijt} \qquad (15)$$

where Ω_j's 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 $\Omega_j = 0$ (none of the village-specific heterogeneity is caused by programs). If village externalities do exist (i.e., $\Omega_j \neq 0$), the spillover effects cannot be not separately identified from the time-invariant village effects using a 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 terms Ω_j are measured by the average village-level program participation rate then the spillover effect is measured by the change in behavior of non-participants 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 re-write the outcome equation similar to (15) as:

$$Y_{ijt} = X_{ijt}\beta_y + C_{ijft}\delta_f + C_{ijmt}\delta_m + V_{ijft}\gamma_f + V_{ijmt}\gamma_m + \eta^y_{ij} + \mu^y_j + \varepsilon^y_{ijt} \qquad (16)$$

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

Table 9a presents the results of the estimates based on equation (16). Average village-level male borrowing does not seem to have an independent effect on household welfare above and beyond household-level male borrowing. This means that there is no externality or spillover effect due to male borrowing. However, this is not the case with female borrowing. We find that average village-level female borrowing increases household non-land assets and household net worth, but reduces girls' school enrollment. A 10 percent increase in the village average loans of females increases household non-land assets by 0.42 percent and household net worth by 0.47 percent. That is perhaps because both the female participation rate and their cumulative borrowing are much higher than the male participation rate and male borrowing, respectively, and hence, they exert an aggregate positive village-level effect as spillover on household welfare. Given that there are about 37 percent rural households with female borrowers, the spillover effects of female borrowing translate into a 0.38 percent increase in household non-land assets as a result of 10 percent increase in the female borrowing.[27]

The spillover effects estimated so far may not reflect the dynamics of credit impacts as current and past loans have differential effects on household outcomes. Therefore, the above model is re-estimated for past and current credit. The results are shown in Table 9b. Average village-level current borrowing of males reduces income and boys' schooling, while average village-level past borrowing of males increases female labor supply, but reduces non-land assets and household net worth. In contrast, the village-level average of current loans of females does not have any spillover effects on any outcome except for boys' schooling, while the average of past female loans increases per capita expenditure and boys' schooling but reduces household non-land assets.

The negative village-level borrowing effect implies negative externality or village diseconomies and the positive effect implies positive externality or spillover effect. The results therefore suggest that higher average village lending does not necessarily crowd out the benefits of

[27]Since 37 percent of households have female borrowers, a 10 percent increase in female borrowing implies a 3.7 percent increase in the village average of female borrowing, which in turn raises average non-land assets in the village by 0.16 percent (=0.042x3.7), which represents the spillover effects for a 10 percent increase in female credit. Since female credit has an independent impact of 0.22 percent in this model, the aggregate impact on non-land asset is 0.38 percent (=0.16+0.22) for a 10 percent increase in the female credit.

an individual's borrowing. The negative coefficients in the case of men's and women's past loans associated with household non-land assets are clear evidences of village diseconomies. While positive externalities can be seen for boys' schooling in case of female's past borrowing, negative externalities are evident for non-land asset in case of the past borrowing of both males and females.

7. Multiple program membership and MFI competition

As we have mentioned, multiple program membership (also known as overlapping) is a relatively new phenomenon. While there was no evidence of multiple program membership in the 1991/92 survey, some 9 percent of households were concurrent members of more than one program by 1998/99, and this figure more than doubled to 32 percent by 2010/11. What are the reasons behind such a growth in multiple memberships? Does multiple program membership reflect a higher demand for credit by borrowers that is not met by a single source, or is it for paying 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 of 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 and, consequently, lead to a higher incidence of multiple program membership if the extra credit demanded is not coming from a single source.[28] In this case, the village-level intensity of program participation would be seen 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 demand for credit is met by a single source of microcredit, we would observe a negative effect of the village-level borrowing.

[28] 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 is not met by a single source.

Why is there higher demand for credit, when village diseconomies lead to lower returns to borrowing? One possible reason for this 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 the household diversify its income earning activities. But, as village-level borrowing increases with higher level of individual level borrowing, the market becomes saturated, and the households are likely to specialize in case of positive externalities (village economies yield higher returns) but diversify in case of negative externality (village diseconomies yield lower returns). We have already seen that for some outcomes there are diminishing returns to individual-level borrowing. This means a higher demand for extra credit to support income diversification. If this is so, we will also observe a positive sign of the coefficient of the interaction terms of village- and individual-level participation in the demand for multiple income activities. In other worlds, the positive sign of the interactions between village-level and individual-level credit applies for both the 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 the incidence of multiple program membership provided that a single source does not satisfy the demand. A single source may not meet the demand for two reasons—either the lender has a ceiling per borrower or the lender's perceived credit risk is high.

Writing a revised version of equation (3) by introducing the multiple membership status (*M*) and the village intensity of MFIs (*V*), we get the following estimation equation:

$$Y_{ijt} = X_{ijt}\beta_y + C_{ijft}\delta_f + C_{ijmt}\delta_m + \kappa M_{ijt} + \theta V_{jt} + \eta_{ij}^y + \mu_j^y + \varepsilon_{ijt}^y \qquad (17)$$

Table 10A presents the household fixed-effects of the impact of multiple sources of borrowing with the village-level intensity of MFI operation as per the above equation (17). If multiple membership status does not matter, we get $\kappa = 0$. On the other hand, if the village density of MFIs does not have a separate and additional effect from what it induced through individual borrowing, we get an estimated coefficient of village density of MFIs that is insignificant, i.e., $\theta = 0$.

The results show that multiple sources of borrowing, independent of a household borrowing from any sources, does seem to have an independent negative effect on net worth and a positive effect on male labor supply. That is, if multiple sources of borrowing are increasing, they do pose a threat to individual- or household-level welfare. Yet we find that household-level borrowing continues to enhance welfare by reducing poverty and increasing non-land assets and net worth. This shows that 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 rather than a curse for microcredit borrowers. By providing additional funds the MFIs are likely to increase household net worth by raising labor supply for productive activities of both males and females family members. In other words, by supporting the income and productivity of the enterprises, a higher village density of MFIs increases household net worth rather than reduces it.

Extending the analysis to the dynamic context, where household welfare depends not only on present borrowing but also past borrowing as the past borrowing may have a lingering effect on welfare. That is, the above model (equation 17) can incorporate the past and current multiple borrowing as well as current and past level of village density of MFIs. Results are shown in Table 10b. Multiple sources of borrowing have no longer a negative effect on household net worth, which was not the case when we do not allow for the separate effects of borrowing from multiple sources by past and current borrowing. In fact, past level of multiple sources of credit has no distinct effect, implying there are no economies of scale accrued from multiple sources of borrowing. On the other hand, past incidence of MFI density seems to have a negative effect on welfare, implying diminishing economies of scale. Thus, while current density of MFIs has a positive effect on household net worth, there is a negative wealth effect from past density of MFIs, showing diminishing returns to MFI density. But household accrues benefits in terms of higher income from past incidence of higher village level MFI density.

8. Conclusions and policy implications

Household-level panel data enables the use of a household-level fixed-effects 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 offers a clear substantive advantage over cross-sectional data in estimating microcredit impacts: it helps analyze dynamic issues such as whether credit effects change over time (or decline for that matter), whether market saturation with village diseconomies is a possibility for microfinance expansion in a country such as Bangladesh which has a heavy microfinance program concentration, and whether the phenomenon of multiple program membership is a consequence of the credit expansion and an indication of income diversification due to village diseconomies. Panel data, however, is not a panacea in estimation as it has its own problems in estimation.

Using panel household survey data carried out at three points (1991/92, 1998/99 and 2010/11) over 20 years in 87 villages of rural Bangladesh, this paper investigates various dimensions of microcredit effects on a set of behaviors to validate earlier results obtained with cross-sectional data or short panel data. In particular, this paper provides separate estimates of female and male credit from all sources of microcredit on household per capita income, expenditures, poverty, non-land assets, household net worth, male and female labor supply, and schooling of children. It provides estimates of the time-varying effects of credit on the same set of behaviors, and the role of increasing level of market saturation and MFI competition on household welfare.

The results of the basic model unequivocally show that group-based credit programs have significant positive effects in raising household welfare including per capita consumption, household non-land assets and net worth. Microfinance increases income and expenditure, the labor supply of males and females, non-land asset and net worth as well as boys' and girls' schooling. Microfinance, especially female credit, also reduces poverty. 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 new borrowing are different from that of old borrowing. For example, past credit has a higher impact on income and expenditure than current credit. There is also a sign of village diseconomies due to market saturation with higher village-level participation in microfinance programs. For example, with higher village-level aggregate current male borrowing, the marginal effect of male borrowing on per capita income gets lower.

The results also show certain patterns in participants' inclination to borrow from multiple sources and to diversify income. Membership in multiple programs has grown steadily from none to 33 percent in 2010/11. However, it does not have any effects on the household outcomes. However, microfinance competition (captured by the number of MFIs operating in a village) seems to have beneficial effects, especially on household non-land asset and net worth growth.

The 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. In fact, our data suggests that more than two-thirds of activities supported by microcredit programs in rural Bangladesh are in the trade sector, and this pattern of loan portfolio even increased over the study period of 1991/92-2010/11.

Trading is perhaps now saturated with microcredit loans and households have already started to experience diminishing returns. In such circumstances, households must be assisted through skill training and the development of improved marketing networks to expand activities in 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 to boost income and productivity, and, hence, sustained poverty reduction.

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Tables

Table 1. Microcredit program participation rate among households: 1991-2011

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

Note: Sample is restricted to 1,509 panel households from 1991/92 survey that are common to all three surveys. Sample size is higher in 1998/99 and 2011 because of household split-offs. Figures in parentheses are percentages of borrowers. Sum of the figures across columns for 1998/99 and 2010/11 exceeds 100% because of household participation in multiple programs.
Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/2011

Table 2. Household cumulative loans and savings from microcredit programs over time (Tk.): 1991-2011

Survey year	GB loans	BRAC loans	Loans from other programs	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)
1998/99	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)

Note: Findings are restricted to microcredit participants. Loans and program savings are CPI-adjusted Tk. with 1991/92=100. Loans are cumulative for 5 years preceding the surveys. Figures in parentheses are sample size in column 1, share of female loans in columns 2-7, and share of program savings in cumulative programs loan in column 8.
Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table 3. Household level outcome indicators by microcredit participation status: 1991-2011

Outcomes	**1991/92**		**1998/99**		**2011**	
	Participants (N=769)	**Non-participants (N=483)**	**Participants (N=1,014)**	**Non-participants (N=420)**	**Participants (N=1,554)**	**Non-participants (N=334)**
Per capita income (Tk./month)	521.8	495.6	502.7	523.1	1,066.0	1,114.3
	t=0.74		t=-0.86		t=-0.36	
Per capita expenditure	327.3	318.6	440.0	436.9	571.6	604.0
	t=1.04		t=0.17		t=-1.71	
Moderate poverty (%)	86.3	87.6	60.6	58.2	32.9	34.6
	t=-0.67		t=0.88		t=-0.62	
Extreme poverty (%)	75.1	78.5	43.6	46.5	16.2	23.1
	t=-1.38		t=-1.05		t=-3.19	
Male labor supply (hours/month)	195.4	189.5	227.8	206.0	200.9	131.4
	t=1.10		t=2.31		t=8.32	
Female labor supply (hours/month)	38.8	37.2	30.1	20.1	56.2	39.6
	t=0.41		t=2.88		t=4.34	
Non-land asset value (Tk.)	18,273.0	12,830.7	20,089.2	25,415.2	62,595.9	68,294.3
	t=3.73		t=-2.46		t=-0.76	
Net-worth (Tk.)	68,400.2	35,953.3	113,613.3	144,981.7	287,625.0	269,349.1
	t=6.15		t=-1.82		t=0.44	

Note: Monetary figures are CPI-adjusted Tk. with 1991/92=100. The analysis is restricted to 1991/92 microcredit eligible households only (those who participated and those who were eligible but did not participate in microcredit programs in 1991/92) which constitute 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 non-participants.
Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table 4. Educational outcomes by microcredit participation status (age 5-18): 1991-2011

Education outcomes	1991/92			1998/99			2011		
	Participants (N_B=816, N_G=744)	**Non-participants (N_B=425, N_G=397)**	**All (N_B=1,241, N_G=1,141)**	**Participants (N_B=883, N_G=815)**	**Non-participants (N_B=305, N_G=283)**	**All (N_B=1,188, N_G=1,098)**	**Participants (N_B=925, N_G=1,021)**	**Non-participants (N_B=180, N_G=179)**	**All (N_B=1,105, N_G=1,200)**
Boys'	0.549	0.417	0.477	0.562	0.614	0.580	0.696	0.701	0.697
enrollment	t=4.69			t=-1.75			t=-0.12		
Girls'	0.510	0.415	0.458	0.655	0.581	0.629	0.712	0.643	0.699
enrollment	t=3.19			t=2.44			t=2.02		

Note: N_B and N_G refer to observations for boys and girls, respectively. The analysis is restricted to 1991/92 microcredit eligible households only (those who participated and those who were eligible but did not participate in microcredit programs in 1991/92) who constitute 64, 62 and 61 percent households of the surveyed sample of 1991/92, 1998/99 and 2011 respectively.
Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table 5. Impacts of microcredit loans household outcomes: household-level fixed-effects estimates (N_{HH}=1,509)

Explanatory variables	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
Log loans of HH males (Tk.)	0.002	0.004*	-0.002	0.018*	0.005	0.028**	0.020**	-0.004	0.007
	(0.28)	(1.67)	(-0.82)	(1.96)	(0.39)	(5.19)	(3.27)	(-1.10)	(1.53)
Log loans of HH females (Tk.)	0.006*	0.0005	-0.005**	0.033**	0.046**	0.025**	0.004	0.007**	0.008**
	(1.63)	(0.27)	(-2.87)	(5.24)	(5.73)	(5.99)	(1.15)	(2.90)	(2.66)
R^2	0.109	0.373	0.329	0.264	0.207	0.456	0.658	0.126	0. 121

Note: Figures in parentheses are t-statistics based on robust standard errors clustered at village level. * and ** refer to statistical significance level of 10% and 5% (or less). Loans refer to amounts borrowed from all microcredit sources during last 5 years. Regressions include more control variables at household- (age, sex, education of head, log of land asset) and village- level (village price of consumer goods; male and female wage; infrastructure such as availability of electricity, and schools; and proportion of village land irrigated).
Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table 6. Impacts of microcredit loans: household-level fixed-effects estimates (propensity score-weighted regression) (N_{HH}=1,509)

Explanatory variables	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
Log loans of HH males (Tk.)	-0.007	0.005*	-0.005*	0.015	0.0002	0.030**	0.016**	-0.007	0.005
	(-0.92)	(1.95)	(-1.79)	(1.33)	(0.01)	(5.21)	(2.92)	(-1.50)	(1.08)
Log loans of HH females (Tk.)	0.005	-0.0002	-0.005**	0.031**	0.038**	0.021**	-0.001	0.006**	0.009**
	(1.26)	(-0.08)	(-2.52)	(4.64)	(4.64)	(4.37)	(-0.15)	(2.17)	(2.38)
R^2	0.121	0.376	0.334	0.209	0.239	0.457	0.651	0.075	0. 067

Note: Same as in Table 6.
Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table 7. Impacts of year-specific microcredit loans on outcomes: household-level fixed-effects estimates (N_{HH}=1,509)

Explanatory variables	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
Log 1991/92 loans of HH males (Tk.)	-0.017	0.002	0.001	-0.018	-0.016	0.027**	0.028**	-0.010	0.008
	(-2.08)	(0.67)	(0.15)	(-1.48)	(-0.77)	(3.63)	(3.10)	(-1.45)	(1.61)
Log 1998/99 loans of HH males (Tk.)	0.008	-0.002	-0.001	0.011	0.019	0.007	-0.017**	-0.011	0.015**
	(1.06)	(-0.40)	(-0.17)	(0.62)	(0.85)	(0.83)	(-1.75)	(-0.18)	(2.07)
Log 2010/11 loans of HH males (Tk.)	0.017	0.005	-0.003	0.047**	0.013	0.043**	0.020**	0.001	-0.001
	(1.52)	(1.40)	(-1.03)	(3.28)	(0.77)	(5.34)	(3.03)	(0.19)	(-0.19)
Log 1991/92 loans of HH females (Tk.)	-0.013*	-0.002	0.003	0.006	0.043**	0.026**	0.025**	0.010**	0.010**
	(-1.80)	(-0.55)	(0.81)	(0.49)	(3.01)	(3.85)	(3.32)	(2.05)	(2.05)
Log 1998/99 loans of HH females (Tk.)	0.003	0.004	-0.004	-0.002	0.041**	0.014**	-0.027**	0.001	0.013**
	(0.61)	(1.61)	(-1.53)	(-0.15)	(3.82)	(2.59)	(-4.10)	(0.27)	(3.11)
Log 2010/11 loans of HH females (Tk.)	0.016**	-0.001	-0.007	0.052**	0.044**	0.025**	0.015**	0.003	0.005
	(2.49)	(-0.42)	(-3.14)	(6.62)	(4.45)	(4.77)	(3.11)	(0.58)	(1.12)
R^2	0.138	0.400	0.361	0.264	0.278	0.482	0.681	0.086	0. 099

Note: Same as in Table 6.
Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table 8. Impacts of current and past microcredit loans on outcomes: household-level fixed-effects estimates (N_{HH}=1,509)

Explanatory variables	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
Log current loans of HH males (Tk.)	0.007	0.008	-0.002	0.044**	0.032*	0.032**	0.023**	-0.003	0.004
	(0.73)	(1.51)	(-0.53)	(3.55)	(1.77)	(3.47)	(2.87)	(-0.66)	(0.50)
Log past loans of HH males (Tk.)	0.015*	-0.003	0.001	0.015	0.011	-0.025**	-0.030**	0.001	-0.001
	(1.92)	(-0.88)	(0.22)	(1.51)	(0.66)	(-3.71)	(-3.66)	(0.29)	(-0.22)
Log current loans of HH females (Tk.)	0.004	0.002	-0.007**	0.041**	0.049**	0.031**	0.010**	0.005	0.008**
	(0.83)	(0.77)	(-2.62)	(5.41)	(4.95)	(5.93)	(2.18)	(1.50)	(2.08)
Log past loans of HH females (Tk.)	0.016**	0.004*	-0.005**	0.0001	-0.023*	-0.012**	-0.008	-0.005	-0.004
	(3.18)	(1.89)	(-2.22)	(0.01)	(-1.94)	(-2.28)	(-1.35)	(-1.54)	(-1.02)
R^2	0.122	0.387	0.342	0.219	0.276	0.465	0.667	0.082	0. 086

Note: Current loans refer to the amounts borrowed from all microcredit sources during last 5 years as collected in the current survey, and past loans are constructed by adding current loans from current and earlier surveys. The rest is same as in Table 6.
Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table 9a. Impacts of microcredit borrowing and village intensity of microcredit participation (N_{HH}=1,509)

Explanatory variables	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
Log loans of HH males (Tk.)	0.004 (0.65)	0.004 (1.47)	-0.002 (-0.81)	0.015 (1.48)	0.007 (0.58)	0.029** (5.37)	0.019** (3.33)	-0.004 (-0.97)	0.005 (1.13)
Log loans of HH females (Tk.)	0.005 (1.41)	0.001 (0.36)	-0.005** (-2.74)	0.035** (5.45)	0.044** (5.63)	0.022** (5.20)	-0.0005 (-0.13)	0.005* (1.94)	0.011** (3.42)
Log village average of HH male loans (Tk.)	-0.011 (-1.36)	0.002 (0.51)	0.0002 (0.07)	0.014 (1.07)	-0.009 (-0.49)	-0.004 (-0.63)	0.0005 (0.06)	-0.002 (-0.44)	0.005 (1.09)
Log village average of HH female loans (Tk.)	0.011 (1.02)	-0.002 (-0.34)	0.003 (-0.50)	-0.026 (-1.39)	0.019 (0.81)	0.042** (3.23)	0.047** (3.64)	0.006 (1.02)	-0.015** (-2.53)
R^2	0.107	0.373	0.323	0.202	0.249	0.459	0.661	0.073	0. 070

Note: Same as in Table 6

Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table 9b. Impacts of current and past microcredit loans and village intensity of microcredit participation (N_{HH}=1,509)

Explanatory variables	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/ month)	Log female labor supply (hours/ month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
Log current loans of HH males (Tk.)	0.012	0.008	-0.002	0.042**	0.032*	0.036**	0.026**	-0.001	0.005
	(1.22)	(1.47)	(-0.69)	(3.31)	(1.74)	(4.03)	(3.52)	(-0.29)	(0.66)
Log past loans of HH males (Tk.)	0.018*	-0.002	0.0001	0.023*	-0.003	-0.018**	-0.021**	0.001	0.001
	(1.93)	(-0.83)	(0.08)	(1.95)	(-0.20)	(-2.65)	(-2.29)	(0.25)	(0.21)
Log current loans of HH females (Tk.)	0.002	0.001	-0.006**	0.041**	0.050**	0.028**	0.007	0.004	0.009**
	(0.37)	(0.55)	(-2.33)	(5.41)	(4.81)	(5.25)	(1.49)	(1.16)	(2.26)
Log past loans of HH females (Tk.)	0.018**	0.003	-0.004*	-0.003	-0.023**	-0.004	-0.002	-0.006*	-0.007*
	(3.49)	(1.12)	(-1.95)	(-0.30)	(-2.05)	(-0.92)	(-0.43)	(-1.65)	(-1.75)
Log village average of current loans of HH males (Tk.)	-0.033**	0.003	0.002	0.019	0.002	-0.019	-0.014	-0.011*	-0.005
	(-2.76)	(0.41)	(0.39)	(0.92)	(0.08)	(-1.43)	(-1.00)	(-1.83)	(-0.58)
Log village average of past loans of HH males (Tk.)	-0.017	0.002	0.001	-0.020	0.054*	-0.032*	-0.035**	-0.0001	-0.009
	(-1.41)	(0.38)	(0.12)	(-0.92)	(1.94)	(-2.50)	(-2.23)	(-0.03)	(-1.25)
Log village average of current loans of HH females (Tk.)	0.025	0.009	-0.009	-0.012	0.014	0.017	0.023	0.015**	-0.012
	(1.50)	(1.13)	(-1.20)	(-0.49)	(0.47)	(1.03)	(1.21)	(2.02)	(-1.10)
Log village average of past loans of HH females (Tk.)	0.004	0.018**	-0.010	0.015	0.010	-0.042**	-0.025	0.016*	0.012
	(0.32)	(2.51)	(-1.40)	(0.57)	(0.26)	(-2.69)	(-1.34)	(1.76)	(1.27)
R^2	0.126	0.389	0.343	0.220	0.277	0.472	0.672	0.084	0. 089

Note: Current loans refer to the amounts borrowed from all microcredit sources during last 5 years as collected in the current survey, and past loans are constructed by adding current loans from current and earlier surveys. The rest is same as in Table 6

Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table 10a. Impacts of borrowing from multiple sources and microcredit competition: household-level fixed-effects estimates (N_{HH}=1,509)

Explanatory variables	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
Log loans of HH males (Tk.)	0.001	0.004*	-0.002	0.015*	0.004	0.028**	0.021**	-0.004	0.006
	(0.13)	(1.71)	(-0.69)	(1.69)	(0.29)	(5.10)	(3.43)	(-1.22)	(1.21)
Log loans of HH females (Tk.)	0.004	0.001	-0.004**	0.028**	0.043**	0.025**	0.006	0.004	0.008**
	(1.17)	(0.38)	(-2.63)	(4.47)	(4.99)	(5.54)	(1.51)	(1.53)	(2.65)
HH borrows from multiple sources	0.056	-0.015	-0.022	0.138*	0.067	0.015	-0.093**	0.063	0.038
	(1.23)	(-0.72)	(-0.97)	(1.78)	(0.70)	(0.36)	(-2.31)	(1.48)	(1.07)
Number of MFIs operating in the village	0.017	0.006	0.001	0.053**	0.052*	0.014	0.026*	0.010	-0.003
	(1.17)	(0.98)	(0.11)	(2.70)	(1.69)	(1.02)	(1.87)	(1.38)	(-0.28)
R^2	0.107	0.374	0.329	0.204	0.251	0.456	0.659	0.074	0. 068

Note: Same as in Table 6.
Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table 10b. Impacts of current and past borrowing from multiple sources and microcredit competition: household-level fixed-effects estimates (N_{HH}=1,509)

Explanatory variables	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
Log current loans of HH males (Tk.)	0.005	0.009*	-0.002	0.038**	0.027	0.030**	0.022**	-0.005	0.003
	(0.49)	(1.63)	(-0.53)	(2.91)	(1.48)	(3.28)	(2.71)	(-1.16)	(0.38)
Log past loans of HH males (Tk.)	0.013	-0.003	0.001	0.016	0.012	-0.025**	0.029**	-0.0001	-0.002
	(1.58)	(-0.97)	(0.32)	(1.58)	(0.68)	(-3.73)	(-3.49)	(-0.03)	(-0.41)
Log current loans of HH females (Tk.)	0.001	0.003	-0.006**	0.034**	0.045**	0.029**	0.010**	0.003	0.007*
	(0.29)	(1.06)	(-2.51)	(4.58)	(4.23)	(5.57)	(2.12)	(0.80)	(1.84)
Log past loans of HH females (Tk.)	0.013**	0.004*	-0.004*	-0.001	-0.025*	-0.013**	-0.008	-0.007**	-0.005
	(2.62)	(1.87)	(-1.92)	(-0.15)	(-1.92)	(-2.28)	(-1.26)	(-2.10)	(-1.31)
HH currently borrows from multiple sources	0.052	-0.027	-0.016	0.141*	0.131	0.039	-0.058	0.074*	0.044
	(1.14)	(-1.24)	(-0.68)	(1.93)	(1.49)	(0.96)	(-1.46)	(1.79)	(1.39)
HH borrowed from multiple sources in	0.020	-0.029	-0.015	0.051	0.189	0.049	0.111	0.104	-0.023
	(0.21)	(-0.55)	(-0.34)	(0.25)	(0.79)	(0.55)	(1.15)	(1.05)	(-0.23)

the past									
Current number of MFIs operating in the village	0.026*	0.0004	0.007	0.063**	0.036	0.017	0.038**	0.006	-0.006
	(1.89)	(0.06)	(1.00)	(3.13)	(1.53)	(1.00)	(2.27)	(0.76)	(-0.58)
Past number of MFIs operating in the village	0.047*	0.016	-0.010	-0.052	-0.066	-0.012	-0.053**	0.003	0.019
	(1.92)	(1.38)	(-0.73)	(-1.27)	(-1.17)	(-0.50)	(-2.22)	(0.18)	(0.98)
R^2	0.125	0.388	0.343	0.222	0.277	0.466	0.670	0.084	0. 087

Note: Same as in Table 6.

Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Appendix

Table A1. Joint significance test for the explanatory variables in the attrition equation (N_{HH}=1,769)

Test statistics	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
$\chi^2(24)$	108.5	109.3	88.30	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	0.000	0.000

Note: For each outcome (income, expenditure, etc.), 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.
Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table A2. Joint significance test for attrition and attrition-interacted explanatory variables in outcome equations (N_{HH}=1,769)

Test statistics	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
$F(24, 86)$	3.17	4.74	3.30	4.10	4.84	2.43	4.43	4.34	4.94
p-value	0.000	0.000	0.000	0.000	0.000	0.001	0.000	0.000	0.000

Note: Outcome variable is regressed on attrition dummy, all exogenous variables, and interactions of attrition and the exogenous variables in the 1991/92 sample. Then a joint significance test is run for attrition variable and the interaction of attrition and the exogenous variables.
Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table A3. Impacts of microcredit loans: household-level fixed-effects with 2-stage instrumental variables estimates (N_{HH}=1,509)

Log microcredit loans in last 5 years	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
Loans of HH males	-0.022	0.005	0.019	0.141*	-0.131	0.090	0.168**	0.008	0.006
	(-0.42)	(0.27)	(0.79)	(1.74)	(-1.10)	(1.40)	(2.16)	(0.26)	(0.33)
Loans of HH females	-0.012	-0.023	0.013	0.007	0.151**	0.166**	0.174**	0.011	-0.009
	(-0.37)	(-1.30)	(0.67)	(0.10)	(2.05)	(3.01)	(2.73)	(0.71)	(-0.56)
R^2	0.095	0.335	0.298	0.163	0.173	0.244	0.373	0.122	0. 108

Note: Figures in parentheses are t-statistics based on robust standard errors clustered at village level. * and ** refer to statistical significance level of 10% and 5% (or less). Regressions include more control variables at household- (age, sex, education of head, log of land asset) and village- level (village price of consumer goods; male and female wages; infrastructure such as availability of electricity, and schools; and proportion of village land irrigated).
Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table A4. Endogeneity test for loan variables from the 2-stage IV estimates as reported in Table A3

Test statistics	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
$\chi^2(2)$	0.363	3.828	0.097	3.235	1.695	8.474	17.566	0.774	1.338
p-value	0.834	0.148	0.953	0.198	0.429	0.015	0.0002	0.679	0.512

Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table A5. Impacts of microcredit loans: household-level fixed-effects estimates after controlling for initial conditions (N_{HH}=1,509)

Log microcredit loans in last 5 years	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
Loans of HH males	0.002	0.003	-0.001	0.017*	0.003	0.029**	0.015**	-0.004	0.007*
	(0.29)	(1.17)	(-0.44)	(1.92)	(0.25)	(5.40)	(2.63)	(-1.02)	(1.72)
Loans of HH females	0.005	0.001	-0.005**	0.031**	0.044**	0.024**	0.004	0.007**	0.007**
	(1.48)	(0.56)	(-3.03)	(5.06)	(5.56)	(6.04)	(1.28)	(2.80)	(2.61)
R^2	0.121	0.386	0.344	0.214	0.261	0.467	0.667	0.139	0. 138

Note: Figures in parentheses are t-statistics based on robust standard errors clustered at village level. * and ** refer to statistical significance level of 10% and 5% (or less). Regressions include more control variables at household- (age, sex, education of head, log of land asset) and village- level (village price of consumer goods; male and female wages; infrastructure such as availability of electricity, and schools; and proportion of village land irrigated).

Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table A6. F test for equality of male and female loans in the impact regressions of Table 5

Statistics	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
F(1,86)	0.34	1.46	1.08	2.07	7.12	0.15	6.30	8.31	0.04
p>F	0.563	0.230	0.301	0.153	0.009	0.704	0.014	0.005	0.846

Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table A7. F test for equality of male and female loans across years in the impact regressions of Table 7

t-statistics	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
Equality among male loans across years	F(2, 86)=4.78, p>F=0.011	F(2, 86)=0.75, p>F=0.477	F(2, 86)=0.27, p>F=0.762	F(2, 86)=6.49, p>F=0.002	F(2, 86)=0.82, p>F=0.445	F(2, 86)=4.95, p>F=0.009	F(2, 86)=7.32, p>F=0.001	F(2, 86)=0.85, p>F=0.433	F(2, 86)=1.56, p>F=0.216
Equality among female loans across year	F(2, 86)=4.17, p>F=0.019	F(2, 86)=1.61, p>F=0.206	F(2, 86)=3.03, p>F=0.053	F(2, 86)=9.55, p>F=0.0002	F(2, 86)=0.04, p>F=0.965	F(2, 86)=1.38, p>F=0.257	F(2, 86)=16.25, p>F=0.000	F(2, 86)=1.19, p>F=0.310	F(2, 86)=1.27, p>F=0.287
Equality among male and female loans	F(2, 86)=0.17, p>F=0.845	F(2, 86)=1.68, p>F=0.192	F(2, 86)=1.11, p>F=0.334	F(2, 86)=0.25, p>F=0.778	F(2, 86)=1.57, p>F=0.213	F(2, 86)=1.88, p>F=0.158	F(2, 86)=0.54, p>F=0.584	F(2, 86)=0.07, p>F=0.936	F(2, 86)=0.33, p>F=0.718

Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11

Table A8. F test for equality of current and past loans in the impact regressions of Table 8

t-statistics	Log per capita total income (Tk./month)	Log per capita total expenditure (Tk./month)	Extreme poverty	Log male labor supply (hours/month)	Log female labor supply (hours/month)	Log HH non-land asset (Tk.)	Log HH net-worth (Tk.)	Boys' school enrollment (age 5-18)	Girls' school enrollment (age 5-18)
Current and past loans of males	F(1, 86)=0.58, p>F=0.449	F(1, 86)=3.74, p>F=0.057	F(1, 86)=0.22, p>F=0.637	F(1, 86)=3.10, p>F=0.082	F(1, 86)=0.60, p>F=0.439	F(1, 86)=24.42, p>F=0.000	F(1, 86)=17.50, p>F=0.0001	F(1, 86)=-0.46, p>F=0.498	F(1, 86)=0.28, p>F=0.601
Current and past loans of females	F(1, 86)=2.87, p>F=0.094	F(1, 86)=0.52, p>F=0.472	F(1, 86)=0.36, p>F=0.549	F(1, 86)=13.77, p>F=0.0004	F(1, 86)=23.13, p>F=0.000	F(1, 86)=35.35, p>F=0.000	F(1, 86)=6.13, p>F=0.015	F(1, 86)=5.33, p>F=0.023	F(1, 86)=5.39, p>F=0.023

Source: WB-BIDS surveys 1991/92 and 1998/99, and WB-InM survey 2010/11