Measuring the Impact of Microcredit Programs:
Evidence from Rural Households in Bangladesh

Abstract

This paper uses household-level panel data from Bangladesh and a fixed-effects approach to assess the impact of borrowing from microfinance institutions on household welfare measures such as household consumption, labor supply, and income. I provide insight into the mechanisms through which the effect of credit works on these welfare measures by isolating components of the whole and estimating the impact of credit on each of these. For example, I find that an increase in food consumption expenditure is driven by an increase in purchased food consumption as opposed to self-produced food consumption. Fixed-effects estimates are also compared to estimates that rely on a different, more easily implementable, technique that is commonly used in impact assessments. I find that the fixed-effects approach is necessary to accurately quantify the impact of program participation on most household welfare measures.

1. Introduction

Rarely has an idea for poverty reduction had as much potential or charisma as microfinance. In the 1970's when Muhammed Yunus began an experimental program to extend financial services to poor local villagers in Bangladesh, he was forced to risk his own savings to finance the project himself after banks, the government, and NGOs alike turned him down. With microfinance institutions (MFIs) now on five continents and serving over 67 million customers, many who had previously been too poor to receive financial services have been granted the opportunity to take out collateral-free loans.

The allure of microfinance comes from the dual possibility of being financially self-sustainable while maintaining a bottom line of helping the poor. However, many MFIs have not yet been able to achieve financial self-sustainability. In order to give momentum to a movement that has shown signs of such great success, billions in subsidies and donations have been flowing into the industry. The widely circulated success stories that first drove the movement were supported by the emergence of early 'impact assessments' that showed impressive effects of microfinance program participation on poverty measures. But as empirical testing moved from practitioners to academia, nearly all of these early studies were shown to overestimate the success of MFIs due to bias.

Impact assessments attempt to measure the causal impact of microcredit on household or individual welfare measures. They are necessary in order to better understand the success of MFIs and to justify the billions currently being allocated to keeping them afloat. Quantifying the impact of borrowing requires comparison of a 'treatment' group, consisting of program participants, to a 'control' group, of nonparticipants. However, these groups may be different in unobservable ways, such as is the case if program participants borrow because they have more ability or entrepreneurship than their nonparticipating counterparts. This problem of dissimilar

comparison groups is known as selection bias and will result in inaccurate estimates of the effect of program participation. Other sources of bias may also exist, such as nonrandom program placement or endogeneity arising from the decision to borrow being jointly determined with the outcome being measured, and are discussed in greater detail in section 2.

Impact assessments have been shown to be highly dependent on the methodology used to account for various sources of bias inherent in the data. As a result, there is still controversy over what studies accurately measure impact. In this paper, I attempt to give clarity to this issue by comparing a commonly used estimation technique, which controls for selection bias by comparing old or pre-existing program members to 'new' participants who have not had the chance to benefit from program participation, with a more reliable fixed-effects approach that uses panel data to more completely deal with bias. My paper also adds to the literature by more precisely controlling for substitutes to credit that may cause misestimates when left unaccounted for. Finally, this study is novel in that it provides insight into the mechanisms through which the effect of program participation works on measures of consumption, labor supply, and income. By breaking down these poverty indicators into the smaller portions that compose the total (i.e. consumption can be broken down into self-produced food consumption, purchased food consumption, and nonfood consumption), a better understanding can be had of where returns to credit are generated from and how they are used.

2. Literature Review

With the microfinance movement having only recently developed and literature on the subject lagging slightly behind the movement, the success of MFIs remains in question. Even so, a vast number of studies examining different areas of microfinance have emerged in the past decade. The most prevalent categories of research include analysis of the self-sufficiency and

sustainability of MFIs, products and services offered, best practices, client targeting, policy implications, and impact assessment. Each of these areas is important, and indeed most are highly interrelated. Many studies touch on multiple categories in order to get a better understanding of how well MFIs are working. For simplicity, this study focuses on answering the question of impact: what effects does microcredit have on households? Likewise, I will mainly discuss the literature that attempts to answer the same question. Nowhere do I mean to imply that finding an impact alone is enough to say that MFIs are successful. Nevertheless, finding and interpreting the impact of programs is necessary first step.

The first step in an impact assessment involves choosing what 'impacts' are to be measured. A major goal of MFIs is to reduce the poverty of their clients. Income, then, is a relevant –and commonly used-- measure. Hulme and Mosley (1996) is an early and widely cited study that focuses on changes in income for microfinance participants in Indonesia, India, Bangladesh, and Sri Lanka. The study concludes that income growth of microfinance borrowers consistently exceeds growth of the non-borrowers. Results showed gains as large as 30 percent in Bangladesh and India.[1] However, Wright (2000) notes that "it is important to recognize that there is a significant difference between 'increasing income' and 'reducing poverty'." The use to which income is put is necessary to identify to determine the net change in poverty.

The next step in an impact assessment is determining how to set up an equation that controls for all sources of bias. Bias may result from heterogeneity between control and treatment groups (oftentimes a result of self-selection bias). Bias may also result from endogeneity due to nonrandom program placement or correlation between unmeasured determinants of credit and the dependent (outcome) variable. In a study of 11 evaluations of the Grameen Bank by Chen (1992), none accounts for selection bias. Of a further review of 32

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[1] Hulme and Mosley (1996) Table 8.1

"research and evaluation" reports by Sebstad and Chen (1996), only Hulme and Mosley (1996) make a serious attempt at correcting for bias.

2.1 BIDS – World Bank Studies

In 1998, World Bank economist Shahidur Khandker wrote an influential book, *Fighting Poverty with Microcredit,* and a related paper, coauthored with Mark Pitt, "The Impact of Group-Based Credit Programs on Poor Households in Bangladesh: Does the Gender of Participants Matter?" (1998). Pitt and Khandker (1998) remains one of the few in-depth and statistically rigorous econometric studies investigating the effect of microfinance on a household's wellbeing, and is perhaps the most widely cited impact assessment of a microfinance program.

Pitt and Khandker (1998) used advanced econometric techniques to account for selection bias, nonrandom program placement, and endogeneity of credit with the measured outcomes (reverse causality). Their analysis was based on data from 1,798 rural Bangladeshi households, collected in 1991/92 by the Bangladesh Institute of Development Studies (BIDS) and the World Bank. These households consisted of MFI participants as well as nonparticipants; nonparticipants were comprised of both households with and households without access to programs. Nonparticipants with access and nonparticipants without access were further broken up into eligible or ineligible nonparticipants, based whether they owned one-half acre or less of tillable land. This land ownership criterion, defining "functionally landless" households, was shared by the three microfinance programs being studied: that credit is restricted to "functionally landless" households.

Pitt and Khandker (1998) estimated the conditional demands for various measures of household welfare, such as consumption, conditioned on the amount borrowed by a household. Consider equations for the demand for credit (C_{ij}) and some measure of household welfare

outcome (Y_{ij}):

$$C_{ij} = X_{ij}\beta_c + Z_{ij}\pi + \upsilon_j^c + \varepsilon_{ij}^c \quad (1)$$

$$Y_{ij} = D_{ij}\beta_y + C_{ij}\delta + \upsilon_j^y + \varepsilon_{ij}^y \quad (2)$$

The level of participation in a credit program, C_{ij}, is determined partially by a vector of household characteristics (X_{ij}) such as the age, education, gender, and marital status of the household head. These variables also determine outcome Y_{ij}, and thus the two equations are jointly determined, giving rise to the possibility of reverse causality. Additionally, unobserved differences between borrowers and nonborrowers, such as innate ability, may generate correlation between the error terms ε_{ij}^c and ε_{ij}^y, biasing results if left unaccounted for. The use of instrumental variables is typically used for both sources of bias. Z_{ij} is an instrument consisting of household or village characteristics that help determine C_{ij} but are independent of characteristics X_{ij} and household welfare measures Y_{ij}. Identifying variables Z_{ij}, however, are very difficult to find and justify. Instead, Pitt and Khandker exploited the landholding criterion, and used a weighted exogenous sampling maximum likelihood, limited information maximum likelihood (WESML-LIML) method.[2] Still, bias may remain due to possible correlation between the fixed village-level unobservables, υ_j^c and υ_j^y, as a result of nonrandom program placements. Village-level fixed-effects were thus employed.

A second-stage regression employed a difference-in-difference approach between eligible and ineligible households and program and non-program villages after first-stage Tobit estimations for credit were found from equation 1. Equation 2 is the regression-equivalent, where D is equal to 0 for eligible households and 1 for ineligible households, and *j* includes both

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[2] For more details on identification through a quasi-experimental survey design, see Pitt and Khandker (1998).

'treatment' and 'control' villages. Thus, the effect of credit, measured as δ, can be found.[3] The authors also tested for differences in returns to borrowing by men and women and found these differences to be significant, resulting in separate estimates of δ, for males and females.

The most influential and widely circulated finding in Pitt and Khandker (1998) is that "annual household consumption expenditure increases 18 taka for every 100 additional taka borrowed by women…, compared with 11 taka for men." The study draws a number of additional conclusions, however. Borrowing by women was found to have a significant effect on girls' and boys' schooling, women's and men's labor supply, and women's nonland assets. Significant results for borrowing by men, however, were only found on boys' schooling.

Despite using sophisticated econometric techniques to control for bias, the accuracy of the original results in Pitt and Khandker (1998) has been disputed. Morduch (1998) calls into question a number of assumptions made in the study. For instance, the basis of identification in Pitt and Khandker (1998) depends on a landholding requirement, which was not strictly enforced. Reexamination by Morduch (1998) showed that dependence on the unenforced requirement caused substantial bias. In a more simple difference-in-difference regression run by Morduch that accounted for this, no impact on consumption from program participation was found. Pitt and Khandker's (1998) use of a Tobit equation in the first-stage estimation of credit has also been criticized, as it assumes that microfinance impacts are identical across borrowers, and that marginal and average impacts of credit are equal. An unpublished paper by Pitt responds to Morduch (1998), addressing each of five major concerns he raised and upholding the earlier

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[3] As specified in Pitt and Khandker (1998): Consider a 'treatment' village 1 and a 'control' village 2, and an eligible household D = 0 and an ineligible household D = 1. Outcome Y for ineligible households would be measured as $\beta_y + \upsilon_1$, in treatment village 1 and as $\beta_y + \upsilon_2$ in control village 2. For eligible households, Y would be simply υ_2 for control village 2 and $\rho\delta + \upsilon_1$ for treatment village 1, where ρ is the proportion of eligible households that participated in a program.

results.[4]   Even so, Khandker (2005) acknowledges that, "the robustness of these results still remains an issue, however, because impact studies are sensitive to the method applied."

Khandker followed up the results of Pitt and Khandker (1998) in a number of studies using a panel dataset. The panel data are composed of two waves, the original 1991/92 base survey done on three MFIs in Bangladesh and used in Pitt and Khandker (1998), and a follow-up survey that revisited the same households in 1998/1999.   With the benefit of panel data, Khandker (2003) was able to use household-level fixed-effects to assess the impact of the microfinance program.

Khandker (2003) examines who demands microcredit, assesses the long-term reduction in poverty brought about by MFIs, and tests for village-level spillover effects of microcredit.   His estimation technique and results provide a starting point for my own work.

Using similar credit and outcome equations as the original Pitt and Khandker (1998) study, Khandker estimates outcomes Y_{ij}, conditioned on the demand for credit, C_{ijt}, by household i in village j during time period t.   The demand for credit (measured as cumulative credit) is also allowed to differ for males m and females f.

$$C_{ijmt} = X_{ijmt}\lambda + \eta^c_{ijm} + \upsilon^c_{jm} + \varepsilon^c_{ijmt} \qquad (1)$$

$$C_{ijft} = X_{ijft}\lambda + \eta^c_{ijf} + \upsilon^c_{jf} + \hat{\varepsilon}^c_{ijft} \qquad (2)$$

$$Y_{ijt} = X_{ijt}\alpha + C_{ijt}\delta_m + C_{ijt}\delta_f + \eta^y_{ij} + \upsilon^y_j + \hat{\varepsilon}^y_{ijt} \qquad (3)$$

where X_{ijt} is a vector of household, village, and group-level characteristics, η_{ij} is an unmeasured determinant of credit that is time-invariant and fixed within a household (such as ability, which was the source of correlation between the error terms in Pitt and Khandker (1998)), υ^c_j is an unmeasured determinant of credit that is time-invariant and fixed within a village.

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[4] Pitt's reply to Murdoch (1998) can be found online at *www.pstc.brown.edu/~mp/reply.pdf*

The impact of credit on outcome vector Y can be measured by estimating equation 3. Where an instrumental variable approach was used to correct bias from correlation between υ_j^c and υ_j^y or ε_{ij}^c and ε_{ij}^y in Pitt and Khandker (1998), a fixed-effects method can more accurately remove the sources of correlation[5], giving rise to an equation akin to:

$$\Delta Y_{ij} = \Delta X_{ij}\alpha + \Delta C_{ij}\delta_m + \Delta C_{ij}\delta_f + \Delta \varepsilon_{ij}^y \qquad (3')$$

Instead of comparing outcome levels for different households with various credit amounts, fixed-effects compare the conditional change in outcome levels within households. When this is computed for households with varying amounts of credit, the mean effect of credit can be estimated. Comparing within-household levels removes all time-invariant, or fixed, effects. Still, such an estimate will be biased if η_{ij} or υ_j^c vary over time. The true equation would be more properly estimated as:

$$\Delta Y_{ij} = \Delta X_{ij}\alpha + \Delta C_{ij}\delta_m + \Delta C_{ij}\delta_f + \Delta \eta_{ij}^y + \Delta \upsilon_j^y + \Delta \varepsilon_{ij}^y \qquad (4)$$

An instrumental-variable approach is reintroduced to remove such error. However, instrumental variables lead to larger standard errors and less efficient results and should not be used if unnecessary. Whereas an instrumental-variable approach is taken in Khandker (2003), a more recent study by Khandker (2005) using similar methodology[6] finds consistent results between fixed-effect and fixed-effect with instrumental variables estimates. The latter paper therefore concludes that instrumental variables are not needed.

Khandker (2003) tested six household welfare outcomes: per capita total expenditure, per capita food expenditure, per capita non-food expenditure, the incidence of moderate and extreme poverty, and household non-land assets. Extreme poverty was defined as holding no

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[5] Because fixed-effects are done at the household level, time-invariant village level effects consisting of υ_j^c are also removed.

[6] Khandker (2005) differs from Khandker (2003) only in that it measures the effect of credit as the sum of returns for past credit and current credit, allowing the effect of credit to differ for previous borrowing.

more than one-fifth of an acre (20 decimals) of land. Male borrowing was found to have much less of an impact than female borrowing. Overall, programs were found to raise per capita consumption, which occurred mainly through non-food expenditure, and household non-land assets.

After finding that microfinance increases consumption among borrowing households, Khandker tested for program spillover. Adding a proxy for 'village participation' in a program, measured as the average value of all microfinance borrowing in a village, Khandker (2003) finds significant positive results of spillover on household outcomes. Both participants and nonparticipants alike benefitted from increased average village-level borrowing. Such a finding is important because studies that do not account for spillover will underestimate the impact of borrowing on outcomes, assuming that spillover effects remain positive.

2.2. The Coleman Model and using new members as a control group

Studies using the BIDS- World Bank dataset take advantage of panel data and advanced econometric techniques to account for bias from heterogeneity, reverse causality, and non-random program placement. Like the BIDS- World Bank studies, Coleman (1999) took advantage of a quasi-experimental survey design. His approach to measuring impact, however, varied considerably from that of Pitt and Khandker (1998).

Coleman (1999) used household-level data from Northeast Thailand, collected four times during 1995/96, to measure the impact of microcredit. Fourteen villages were surveyed and information was collected from participating and nonparticipating households. At the first survey, seven villages had had a program for at least two years, and one had new access to a program. The remaining six 'control' villages had no programs as yet, but had been selected for program development. Further, households in control villages identified themselves as wanting

to participate in a program or not. By comparing 'old' borrowers who had already begun receiving loans to those who had self-selected into programs but had not yet borrowed, Coleman could account for selection bias. Nonmembers in both treatment and control villages were also included in regression analyses to allow for the use of village-level fixed-effects, which control for nonrandom program placement.

Coleman (1999) uses a relatively simple estimation technique to calculate the impact of program participation. He estimates outcome Y for household i in village j:

$$Y_{ij} = X_{ij}\alpha + V_j\beta + M_{ij}\gamma + M_{ij}*VBMOS_{ij}\delta + \varepsilon_{ij}$$

where X_{ij} is a vector of household characteristics, V_j is a vector of village-level characteristics, M_{ij} is a membership dummy equal to 1 for households that have self-selected into a program, *VBMOS* is the number of months during which a program has operated in a village, and ε_{ij} is an error term representing unmeasured village and household-level determinants of outcome Y. The parameter measuring program impact, δ, can be interpreted as the marginal effect of having access to credit for one more month. $M_{ij}*VBMOS_{ij}$ is only positive for members (M=1) and villages that currently have programs ($VBMOS > 0$).

The impact variable *VBMOS* is used, as opposed to the amount of credit borrowed, to eliminate endogeneity between the demand for credit and outcome Y. The amount of time a program is available is exogenous to households, so sources of correlation between unmeasured determinants of credit and program effects Y are removed. However, if the amount of time a program has been in a village is systematically determined, there may still be bias due to nonrandom program placement. Village-level fixed-effects are employed to eliminate such correlation.

Coleman (1999) argues that the membership dummy variable, M, is a proxy for

unmeasured determinants of credit, such as entrepreneurship or ability, that cause selection bias. A positive estimate of M, which Coleman generally finds, signifies that participants are initially better off than nonparticipants. Naïve estimates, which do not account for heterogeneity between members and nonmembers, thus overestimate impact.

Results from the study indicate that microfinance access has little or no positive impact on the households in the study. All in all, 72 outcome variables were tested relating to measures of physical assets; savings, debt and lending; production, sales, expenses, and labor; and healthcare and education. Coleman (1999) compares promising naïve estimates -which show significant, positive effects of microfinance- to fixed-effects estimates that also include the membership dummy, revealing the influence of bias in the naïve estimates. Even worse, when selection and program placement bias are accounted for, some negative impacts, such as women's high-interest debt, were positively and significantly correlated with borrowing, indicating that borrowers were using moneylenders or loansharks to avoid defaulting.

While the original Coleman setup exogenously denies credit to 'new' participants, others have replicated the method without the benefit of such a setup. Instead, they simply compare 'old' members to 'new' members. Karlan (2001) and others criticize such Coleman-type models. First, the composition of current or 'old' participants is not a true sample of the borrowing population. Dropouts are inherently undersampled, while those who continue to borrow compose the majority of 'old' clients. A study comparing these two groups would only produce unbiased estimates if dropouts and continuing clients had identical levels of the impact being studied, an improbable condition. Karlan (2001) also criticizes the implicit assumption that selection bias is static, pointing out that 'new' borrowers may not have borrowed in the past for some endogenous reason. Early participants may have borrowed at the first chance because

they knew that high returns to credit would be generated, whereas late participants were hesitant because of their lower ability. Ghatak (2000) and Hatch (1997) provide, respectively, theoretical and empirical evidence for this. The characteristics of old and incoming borrowers could also differ as a result of a change in a MFI's targeting of clients. Karlan (2001), however, only provides the theoretical argument and the accuracy of comparing old members to new members remains untested.

2.3. AIMS – USAID Studies

Despite the potential pitfalls of using new members as a control group, the benefits, including cheap and easily implemented impact assessments, has led to their use in many surveys. In 1995, the United States Agency for International Development (USAID) created the Assessing the Impacts of Microenterprise Services (AIMS) Project to defend the value of impact assessments. Together with the Small Enterprise Education and Promotion (SEEP) Network, the AIMS Project team developed a set of tools to guide cost-effective, useful, and credible impact assessments.[7] The SEEP/AIMS practitioner-oriented tools encouraged the use of a Coleman-type assessment, where current clients were compared to incoming clients. The first tests to use the tools resulted in the reports of Edgcomb and Garber (1998) and MkNelly and Lippold (1998). They found that clients had 75 percent higher profit levels, and 45 percent higher income levels, respectively.[8] However, these results are likely to suffer from bias, as discussed above.

In 2001 the publication of the exhaustive AIMS Core Impact Assessments greatly increased the depth of impact evaluations. The Core Impact Assessments took a different approach to these assessments, using longitudinal data collected in India, Zimbabwe, and Peru to estimate the impact of MFIs. All three tests were conducted at the individual, household, and

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[8] Table 2 from Edgcom and Garber (1998) and Table 3 from MkNelly and Lippold (1998).

microenterprise levels. Baseline data were collected in 1998, and the same households were revisited in 1999/2000. Additionally, common methodology was used and common hypotheses tested so that an overall comparison could be made. Chen and Snodgrass (2001) authored the assessment on the SEWA Bank in India. Barnes (2001) studied the Zambuko Trust in Zimbabwe, and Dunn and Arbuckle, Jr. (2001) examined Mibanco in Peru. Each of the three studies found different results, producing inconclusiveness about the impact of microfinance. Differences among the countries and programs seemed to have a great effect on impacts. Overall, enterprise-level findings were weaker than might be expected, possibly demonstrating the fungibility of loans, or use of loans for purposes other than those stated.   At the individual level, no impact was found uniformly across the three studies, but generally microfinance seems to have had a positive effect, with significant positive impacts being found across two countries for numerous social and financial outcomes. In addition to the effect of microcredit on household welfare, Chen and Snodgrass (2001) examined how microsavings affected households.

The results of the AIMS studies may not be accurate due to insufficient control for bias.[9] The researchers used gain score analysis and analysis of covariance (ANCOVA) to measure impact rather than a differencing technique, which would factor out any fixed-effects. The studies controlled for selection bias by including an extensive list of moderating variables such as individual and household demographics and a thorough comparison of non-client groups to try and quantify bias. The rationale of not using a differencing technique is that time-invariant factors such as gender or enterprise sector are eliminated from the estimated equation and their impact cannot be measured.

Alexander (2001) returned to the USAID Peru data to estimate the impact of credit after

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[9] For instance, see Alexander (2001) and Armendáriz de Aghion and Morduch (2005)

accounting for biases. Two models are employed to test for consistency: a model akin to Coleman (1999) that additionally considers the amount of loan borrowed, and a differencing technique similar to that of Khandker (2003). The researchers' findings of the effect of credit on microenterprise profits are significantly lower than those reported by Dunn and Arbuckle (2001) in the original impact assessment, suggesting that Dunn and Arbuckle (2001) may have overstated the impact estimates. However, Alexander's study was limited to microenterprise level outcomes and did not reanalyze the household or individual level impacts found in the original Core Impact Assessment.

In summary, the result of such methodologically different studies has been a smorgasbord of results; some positive, others negative, and many insignificant. Reanalysis of estimates using different approaches to account for the non-experimental nature of the data are often inconsistent. Consequently, the success of microfinance remains in question, and the validity of various techniques is unclear. Recent studies have made an attempt to summarize the most valid findings and present a better picture of 'what we know' about microfinance. Still, more empirical evidence is needed to determine what results of previous studies can be accepted. Additionally, the mechanisms through which microfinance works remain largely unexplored. Determining from where returns to microfinance are derived and to what use they are put is necessary to better understand the success, or failure, of MFIs.

3. Estimation Strategy / Model

In this paper I focus on measuring the impact of microcredit on poverty measures. Microcredit is not the only service offered by MFIs, however. Financial services such as savings and insurance are offered by many MFIs, while cultural and educational reforms are also often incorporated into program participation. Rather than quantifying the impact of MFIs on poverty

reduction, I only try to specify the impact of *credit* because it is easily measurable and is the most universal of services offered. I benefit from using an exhaustive panel survey carried out by the Bangladesh Institute of Development Studies (BIDS) and the World Bank. The data is discussed in detail in the proceeding section. I also have the benefit of starting with the modeling of Pitt and Khandker (1998) and Khandker (2003), who used the same dataset.[10]

The effect of microcredit on some outcome Y for household i in village j can be estimated as:

$$Y_{ijt} = X_{ijt}\beta + C_{ijt}\delta + R_{ijt}\gamma + \eta_{ij} + \mu_j + \varepsilon_{ijt} \qquad (1)$$

where X_{ijt} is a vector of household characteristics, C_{ijt} is the stock or cumulative amount of credit, R_{ijt} are time-varying substitutes for microcredit (e.g. loans from relatives), β, δ, and γ are the parameters to be estimated, η_{ij} are household-level unmeasured determinants of Y_{ijt}, μ_j are unmeasured determinants fixed at the village-level, and ε_{ijt} is a nonsystematic error term.

A simple estimation of equation 1 will result in biased estimates due to self-selection and nonrandom program placement, which would cause correlation between the unmeasured determinants of outcome Y_{ijt} with C_{ijt}, the stock of credit, as explained in section 2. In order to account for this, a household-level fixed-effects approach is employed, removing the sources of error, η_j and μ_j. A problem with using fixed-effects is also the reason it is so powerful- it removes all static characteristics from a model. Independent variables that are not time-variant, such as gender, drop out from the regression analysis when a fixed-effect method is used. Different effects to borrowing by males and females, however, have been well-documented by

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[10] At the time, the only data available to Pitt and Khandker (1998) was from the first round of the survey, in 1991/92. They were thus limited to a cross-section of the data.

MFIs and empirically shown by Pitt and Khandker (1998). In order to allow different effects for males and females, household credit (C_{ijt}) must be disaggregated by gender:

$$Y_{ijt} = X_{ijt}\beta + C_{ijmt}\delta_m + C_{ijft}\delta_f + R_{ijt}\gamma + \eta_{ij} + \mu_j + \varepsilon_{ijt} \qquad (2)$$

where m and f stand for male and female, and δ_m and δ_f measure the effects of male-borrowing and female-borrowing, respectively.

Still, household-level fixed-effects may not yield unbiased estimates. If unmeasured determinants of Y_{ijt} vary over time, equation 1 and 2 can be rewritten as:

$$Y_{ijt} = X_{ijt}\beta + C_{ijt}\delta + R_{ijt}\gamma + \eta_{ijt} + \mu_{jt} + \varepsilon_{ijt} \qquad (1')$$
$$Y_{ijt} = X_{ijt}\beta + C_{ijmt}\delta_m + C_{ijft}\delta_f + R_{ijt}\gamma + \eta_{ijt} + \mu_{jt} + \varepsilon_{ijt} \qquad (2')$$

Here, η_{ijt} and μ_{jt} do not drop out after applying a fixed-effects method, and if left unmeasured bias will result. An attempt was made to control for such time-variant factors, but dynamic unobservables, such as unmeasured income, may remain in the model.

A second estimation technique is also employed in this study. A Coleman-type equation is set up for the 1991/92 cross-section of the data as:

$$Y_{ij} = X_{ij}\beta + C_{ij}\delta + M_{ij}\lambda + R_{ij}\gamma + \varepsilon_{ij} \qquad (3)$$
$$Y_{ij} = X_{ij}\beta + C_{ijm}\delta_m + C_{ijf}\delta_f + M_{ij}\lambda + R_{ij}\gamma + \varepsilon_{ij} \qquad (4)$$

where M_{ij} is a membership dummy equal to one for borrowers and 0 for nonborrowers. While only a cross-section is needed for such a regression, identification of future borrowers is found using the entire panel data.[11] Thus, M_{ij} equals one for both 'old' program participants who have borrowed by the first round of data in 1991/92 and 'new' program participants, who only started to receive credit after the time period of the cross-section. The membership dummy should

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[11] Panel data is not needed for identification. If there is a delay between the time participants receive loans and apply for loans, new members can be found with a cross-section. Due to limitations in my data and to maintain a constant sample size, I did –minimally- make use of both survey rounds of the data.

control for the unmeasured determinants of Y, represented as η_j and μ_j in equations 1 and 2. Estimation of equation 3 and 4 should then give an unbiased estimate, δ, on the credit variable(s).[12]

Finally, a simple "naïve" model using the 1991/92 cross-section regression that does not account for heterogeneity between program members and nonmembers is also estimated:

$$Y_{ij} = X_{ij}\beta + C_{ij}\delta + R_{ij}\gamma + \varepsilon_{ij} \tag{5}$$

This model assumes that the decision to borrow is exogenous to households. Including estimates for the naïve and Coleman-type equations help to show the importance of bias in impact assessments.

Various outcomes are tested using each model type: the naïve model, the Coleman-type model, and finally the more robust fixed-effects equations. Each equation that is estimated is reproduced below, renumbered to match the results in the following section.

(1) – Naïve: $Y_{ij} = X_{ij}\beta + C_{ij}\delta + R_{ij}\gamma + \varepsilon_{ij}$

(2) – Coleman: $Y_{ij} = X_{ij}\beta + C_{ij}\delta + M_{ij}\lambda + R_{ij}\gamma + \varepsilon_{ij}$

(3) – Coleman: $Y_{ij} = X_{ij}\beta + C_{ij}\delta + M_{ij}\lambda + R_{ij}\gamma + \varepsilon_{ij}$

(4) – Fixed-effects: $Y_{ijt} = X_{ijt}\beta + C_{ijt}\delta + R_{ijt}\gamma + \eta_{ij} + \mu_j + \varepsilon_{ijt}$

(5) – Fixed-effects: $Y_{ijt} = X_{ijt}\beta + C_{ijmt}\delta_m + C_{ijft}\delta_f + R_{ijt}\gamma + \eta_{ij} + \mu_j + \varepsilon_{ijt}$

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[12] In order to control for sample selection bias, resulting from the under-sampling of dropouts (see Karlan (2001)), credit was calculated as the cumulative amount borrowed. Doing so groups 'dropouts' with 'old' program participants, which provides a more accurate measure of the credit effect.

4. Data and Their Characteristics

This study is based on a quasi-experimental household survey conducted in 1991/92, and a follow-up survey of the same households in 1998/99. The Bangladesh Institute of Development Studies (BIDS) teamed up with the World Bank to create the dataset, which has been referred to as the "mother of all surveys" in microfinance studies. The survey consists of 29 randomly selected thanas (subdistricts) from 391 rural thanas, five of which had no access to microcredit programs in 1991/92. By the 1998/99 resurvey, all villages had at least one credit program. From the 24 thanas with programs in 1991/92, villages were identified which had a program in operation for at three years. Three villages were randomly selected from each of these thanas, and three villages were randomly selected for each of the remaining 5 thanas without initial access. A total of 87 villages were thus sampled. Each household in the 87 villages was identified as eligible or ineligible to join a program, and households were sampled using a stratified random sampling technique. Households eligible to participate in a program were required to have no more than ½ acre of cultivatable land. However, this criterion was not strictly enforced by programs. Interviews were conducted on site, and all household members over the age of 5 were encouraged to attend.

The 1991/92 survey was conducted in three rounds. Each round corresponded to one of the three major rice-based harvest seasons: *Aman* (November-February), *Boro* (March-June), and *Aus* (July-October). In all, 1,798 households were surveyed in the *Aman* season, but by the final round this number declined to 1,769 households because of attrition. The peak of production comes after the *Aman* harvest, which yields the largest crop in Bangladesh agriculture. This season exhibits the greatest impact on agricultural employment, income, and price levels. The same households were revisited just once during the 1998/99 survey. However, of the 1,769

households, only 1,638 households could be re-traced.[13] A high attrition rate would be of concern due to sample bias; fortunately, the attrition rate of the sample I study is low. Additionally, a comparison of the dropouts to the remaining sample does not reveal any worrisome characteristics. The 1998/99 survey also included new households from villages that had already been sampled as well as new villages, resulting in a total of 2,599 households.

This study limits the sample to panel data for which there are data from both the 1991/92 and 1998/99 survey. The panel data consist of 1,633 such households. Of the 1,633 households used in the analysis, 1,045 were borrowers and 588 never participated in a microcredit program. A household was identified as 'borrowing' if at least one member of the household received a loan from a program. Of the total 1,045 borrowers, 271 were 'new' borrowers, meaning they began borrowing after the 1991/92 survey. In 1991/92, all measures (at the mean) were higher for nonparticipants except for the amount spent on purchased food, female self-employment hours, and weekly nonfarm enterprise net revenue. By 1998/99, the majority of households participated in a microcredit program. Another major change that occurs in 1998/99 is that participating households increase their earnings and total self-employed labor hours over nonparticipants.

Table 1 provides means and standard deviations for consumption, income, and labor variables.   The data show that participants earn and spend more than nonparticipants. Participants spend an annual average of 20,342 taka[14] (Tk.) total on food and nonfood items, compared to Tk. 23,390 for nonparticipants.   Both groups live on under $2 a day, with nonparticipants spending an average 12.5 percent more per a day than participants. This trend of worse-off participants is to be expected, as participants are limited to those with less than one-

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[13] My number is actually 1633. I could not identify the missing 5 households, so they have been excluded.

[14] Taka (Tk.) is the Bangladeshi unit of currency. By the end of 1992, the exchange rate was Tk. 40 per 1 U.S. dollar.

half acre of tillable land. By 1998/99, borrowing by females increased by over 200 percent, compared to an increase of under 50 percent for borrowing by males. Participants continued to be less well-off in 1998/99, but showed more promise as wage income and self-employment hours increased over nonparticipants.

The main purpose of the BIDS-World Bank data collection was to analyze the impact of three major MFIs: Bangladesh Rural Advancement Committee (BRAC), Bangladesh Rural Development Board (BRDB), and the Grameen Bank. In 1991/92, these banks constituted nearly all of the programs in which households participated. Limiting the sample to the 1,633 households in both surveys, the 1991/92 data reveal that 13.04 percent of households belonged to BRAC, 15.25 percent to BRDB, 18.06 percent to the Grameen Bank, and 53 percent had never belonged to any MFI. By 1998/99, 19.41 percent of the 1,633 households had received at least one loan from BRAC, 17.45 percent from BRDB, 25.96 percent from the Grameen Bank, and 11.48 percent had received a loan from another MFI. Only 36 percent had never received a loan. Some households had received loans from multiple MFIs in the 1998/99 period.

Table 1. Summary Statistics of Consumption, Income, and Labor Variables

Variable	Participants	Nonparticipants	All Households
1991/92			
Total Household Borrowing[a]	9,066.53		4,297.30
	(7,974.83)		(7,115.52)
Borrowed By Household Females[a]	5,609.28		2,658.66
	(7,301.39)		(5,753.26)
Borrowed By Household Males[a]	3,457.25		1,638.65
	(6,448.34)		(4,762.02)
Annual Total Food Consumption	15,824.43	16,754.42	16,313.63
	(6,747.73)	(9,582.15)	(8,369.81)
Annual Purchased Food Consumption	11,636.81	11,461.92	11,544.81
	(5,669.4)	(6,935.33)	(6,365.45)
Annual Self-Produced Food Consumption	4,187.62	5,292.50	4,768.82
	(5,492.79)	(7,073.22)	(6,395.14)
Annual Nonfood Expenditure	4,518.45	6,635.33	5,631.98
	(5,526.2)	(12,989.88)	(10,212.43)
Weekly Nonfarm Enterprise Revenue	351.48	270.50	308.88
	(864.3)	(1,229.27)	(1,072.34)
Monthly Household Wage Income	614.38	763.14	692.63
	(930.99)	(1,235.84)	(1,104.09)
Monthly Female Wage Income	35.44	45.60	40.79
	(167.59)	(203.72)	(187.48)
Monthly Male Wage Income	578.93	717.54	651.84
	(921.9)	(1,203.)	(1,080.83)
Monthly Household Self-Employment Hours	84.92	87.14	86.09
	(100.91)	(119.15)	(110.85)
Monthly Female Self-Employment Hours	24.17	17.25	20.53
	(33.89)	(28.15)	(31.18)
Monthly Male Self-Employment Hours	60.75	69.89	65.56
	(94.05)	(114.06)	(105.12)
Number of Observations	774	859	1633
1998/99			
Total Household Borrowing[a]	25,819.93		16,522.86
	(28,457.41)		(25,918.29)
Borrowed By Household Females[a]	19,942.36		12,761.65
	(27,179.6)		(23,754.24)
Borrowed By Household Males[a]	5,877.57		3,761.21
	(15,859.97)		(12,995.21)
Annual Total Food Consumption	24,941.08	26,142.62	25,373.72
	(12,546.07)	(18,149.52)	(14,815.73)
Annual Purchased Food Consumption	19,239.76	18,987.11	19,148.79
	(11,979.66)	(14,842.8)	(13,079.04)
Annual Self-Produced Food Consumption	5,701.32	7,155.51	6,224.93
	(8,411.02)	(10,601.16)	(9,282.58)
Annual Nonfood Expenditure	12,707.74	16,370.98	14,026.78
	(22,978.64)	(31,088.34)	(26,239.22)
Weekly Nonfarm Enterprise Revenue	405.80	331.21	378.94
	(881.54)	(811.87)	(857.61)
Monthly Household Wage Income	761.17	641.19	717.97
	(1,089.79)	(1,050.61)	(1,077.06)
Monthly Female Wage Income	48.49	38.12	44.76
	(218.6)	(197.08)	(211.1)
Monthly Male Wage Income	712.68	603.07	673.21
	(1,062.22)	(1,015.54)	(1,046.66)
Monthly Household Self-Employment Hours	177.28	144.22	165.38
	(177.43)	(185.73)	(181.11)
Monthly Female Self-Employment Hours	20.80	11.39	17.42
	(49.75)	(40.36)	(46.8)
Monthly Male Self-Employment Hours	156.48	132.83	147.96
	(166.46)	(182.61)	(172.76)
Number of Observations	1045	588	1633

Numbers in parentheses are standard deviations
a. Borrowing is measured as cumulative borrowing

5. Results
5.1 Comparing Models

In this section I present and interpret the results from estimating equations 4 and 5 from section 3. In addition to the fixed-effects estimates, which use panel data to accurately account for program endogeneity and selection bias as discussed above, I report alternative OLS estimates from a cross-section of the data using equations 1 - 3. These estimates are widely inconsistent with those of the fixed-effects equation, revealing the importance of controlling for bias. A direct comparison between OLS and fixed-effect estimates cannot be made, however. OLS regressions estimate the impact of microcredit on various poverty indicators as of 1991, whereas the fixed-effects regressions estimate the impact between 1991 and 1998. Nevertheless, if microcredit does have an impact, the direction and (loosely) the magnitude of the alternative estimates should parallel those of the fixed-effects estimates.[15]

The first OLS estimate is a "naïve" model that ignores any heterogeneity bias. It assumes that the choice to participate in credit programs is exogenous to the household. If credit programs target the poorest, the impact of borrowing will generally be an underestimate. However, if program participants endogenously choose to borrow because of some factor such as innate ability or entrepreneurship, the coefficient measuring the impact of credit will be biased upwards. The net effect of these two sources of bias depends on the magnitude of each bias.

The other two OLS estimates build upon the "naïve" model by controlling for such heterogeneity between participants and nonparticipants with a technique similar to that developed by Coleman (1991). Such estimates have received criticism mainly due to potential

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[15] An argument can be made here using Khandker (2005), who finds decreasing returns to microcredit. Given my model, this should cause my estimates to be biased downwards, resolving issues with OLS magnitude being naturally smaller. Results were also compared to Pitt and Khandker (1998), who used an IV approach for the 1991 data.

differences between new and old members.[16] However, the technique remains used in a number

of studies and has not previously been empirically tested. Consistent findings between the

Coleman-type regressions and the more robust fixed-effects estimates would have important

implications. First, it would refute the argument against the validity of using new members as a

control group. Secondly, consistent findings would allow MFIs interested in conducting accurate

impact assessments to do so without the high financial costs and time delay associated with

collecting panel data. However, my results generally refute the validity of a Coleman-type

estimate, showing (limited) evidence for differences between old and new households as well as

differences between households with male borrowers and households with female borrowers.

5.2 Consumption and Expenditure Effects
    5.2.1 Total Food Consumption
       Table 2 reports estimates of the impact of credit on yearly food consumption. Fixed-

effects estimates of equation 5 show that participation in microcredit programs has a significant

and positive impact at the household level. Household yearly food consumption increases by Tk

6.7 for every additional Tk 100 borrowed, indicating a 6.7 percent return to credit. Table 1

shows that the average participating household had accumulated Tk 25,819 in stock of credit by

1998/99, and spent an average of Tk 24,941 a year on food consumption, indicating a 7 percent

increase in food consumption due to borrowing for program participants.

When returns are observed to differ by gender, the effect of female borrowing is found to

increase total food consumption by one and one half times the effect of borrowing by males.

Additionally, only borrowing by females remains highly significant at the 5 percent level. Such

findings are consistent with the majority of the previous studies, which found higher returns to

female program participants. This can be explained easily if females face greater constraints to

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[16] For instance, see Karlan (2001).

financial services. This is probable, especially in Bangladesh, where gender inequality is rampant. Table 2 shows that for every Tk 100 borrowed by a female, annual total food consumption increases by Tk 7.1, whereas annual total food consumption only increases by Tk 4.8 taka for every Tk 100 borrowed by males. This estimate is slightly smaller than that of Khandker (2005), who finds that "an additional Tk 100 in women's stock of credit during 1998/99 increases the household's… food expenditure by Tk 11.3."

Comparing fixed-effect results to alternative OLS estimates reveal unanimously biased results. Simple OLS and improved Coleman-type equations do not accurately measure the impact of credit on food consumption. Regression 1, the naïve OLS model, dramatically underestimates the impact of credit on household food consumption. This is partially explained by programs targeting functionally landless households; because poorer households are targeted, program participants can be expected to have lower consumption measures. The Coleman-type regressions of equations 2 and 3 improve upon the naïve model by accounting for this heterogeneity between participants and nonparticipants with the addition of the 'program member' dummy variable. As expected, the coefficient estimate on the *member* variable is negative. However, membership status does not appear to fully control for bias. Estimates in regressions 2 and 3 continue to underestimate impact when compared to the fixed-effects estimates. While insignificant and small, female borrowing remains greater in magnitude than male borrowing. However, the coefficient on amount borrowed by males in regression 3 is curiously negative. Given a positive coefficient for the fixed-effect estimate, this could indicate that households with male borrowers are even worse off than the average participating household

TABLE 2. Estimates of the Impact of Microcredit on Food Consumption

Explanatory Variables	Annual Total Food Consumption					Annual Purchased Food Consumption					Annual Own Food Consumption				
	(1) Naïve	(2) Coleman	(3) Coleman	(4) Fixed-Effects	(5) Fixed-Effects	(1) Naïve	(2) Coleman	(3) Coleman	(4) Fixed-Effects	(5) Fixed-Effects	(1) Naïve	(2) Coleman	(3) Coleman	(4) Fixed-Effects	(5) Fixed-Effects
Program member		-971.7***	-986.1***				1121***	1080***				-2092***	-2066***		
		(272)	(272)				(296)	(294)				(313)	(313)		
Amount borrowed by household	-0.0250	0.00659		0.0674***		-0.00615	-0.0426**		0.0483***		-0.0188	0.0491**		0.0191**	
	(0.017)	(0.019)		(0.013)		(0.018)	(0.020)		(0.013)		(0.019)	(0.022)		(0.0091)	
Amount borrowed by household males			-0.0232		0.0481*			-0.125***		0.0252			0.102***		0.0229
			(0.026)		(0.029)			(0.028)		(0.028)			(0.029)		(0.020)
Amount borrowed by household females			0.0289		0.0711***			0.0194		0.0527***			0.00947		0.0184*
			(0.023)		(0.014)			(0.025)		(0.013)			(0.026)		(0.0097)
Highest education of household male	441.1***	439.4***	443.2***	457.3***	455.0***	32.43	34.33	44.71	310.7***	308.0***	408.6***	405.1***	398.5***	146.6**	147.1**
	(70.4)	(70.1)	(70.1)	(87.7)	(87.8)	(76.5)	(76.1)	(75.8)	(84.2)	(84.3)	(81.7)	(80.6)	(80.5)	(60.7)	(60.8)
Highest education of household female	334.2***	322.7***	321.1***	412.5***	411.4***	282.9***	296.1***	291.6***	267.0***	265.7***	51.25	26.61	29.52	145.6**	145.8**
	(52.2)	(52.1)	(52.1)	(89.7)	(89.7)	(56.7)	(56.6)	(56.3)	(86.1)	(86.2)	(60.6)	(59.9)	(59.8)	(62.1)	(62.1)
Amount received from relative loans	0.0113	0.0103	0.0124	0.0337	0.0335	-0.0389	-0.0378	-0.0319	0.0738***	0.0735***	0.0502	0.0481	0.0443	-0.0401**	-0.0400**
	(0.058)	(0.057)	(0.057)	(0.027)	(0.027)	(0.063)	(0.062)	(0.062)	(0.026)	(0.026)	(0.067)	(0.066)	(0.066)	(0.019)	(0.019)
Observations	1633	1633	1633	3266	3266	1633	1633	1633	3266	3266	1633	1633	1633	3266	3266

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: All regressions also include village controls, number of household members, and the gender, marital status, age, and education of household head.

5.2.2 Own Food and Purchased Food Consumption

Considering the relatively long time span of the survey, the fixed-effects results are promising. For instance, the results show that the effect of microcredit on food consumption continues over a 7 year period. Given that a household's food consumption increases with microcredit, an interesting question to analyze is the mechanism through which this effect works. Are households able to spend more money on purchased food, or are they better able to grow their own crops?

Table *2* breaks down total food consumption, presenting separate results for own food consumption and purchased food consumption. Own food consumption is defined as the marketplace value of food that has been *produced* and consumed by the household. Yearly estimates are extrapolated from weekly averages. Annual purchased food consumption is the amount a household has *spent* on food at the marketplace and is similarly based on weekly averages. Fixed-effects results reveal a positive and significant correlation between total household credit and both dependent variables. The impact of credit on purchased food consumption is more than twice the size of that on own food consumption, implying that the majority of the increase in food consumption can be explained by an increase in purchased food. Borrowing an extra Tk 100 increases the value of yearly own food consumption by Tk 1.9 and increases yearly purchased food consumption by Tk 4.8.

While male and female borrowing has a significant and positive impact on total food consumption, this significance drops completely from male borrowers when own food and purchased food are examined independently. Still, the coefficients on the amount borrowed by household males for both own and purchased food are of a similar magnitude, showing that males may use credit similarly for both purposes. The effect of credit to females, however, is much higher and more significant for purchased food consumption than it is for own food

consumption. An additional Tk 100 in the stock of female credit increases purchased food consumption by Tk 5.3, compared to an increase of only Tk 1.8 for own food consumption. Females who receive credit are thus more likely to be spending their loans or returns from loans to buy food, rather than to produce their own.

Loans from relatives may be a substitute for microcredit. Previous studies that have neglected to account for such substitutes may produce biased estimates in measuring the effect of program participation. My results show that borrowing from relatives increases the value of purchased food, but has a negative effect on the value of own food consumption. Leaving loans from relatives unaccounted for would thus result in overestimates of the credit coefficients for purchased food consumption and underestimates for own food consumption. The positive sign of the relative loans coefficient for purchased food consumption may indicate that such loans are used for immediate or emergency consumption, while the negative sign for own food consumption may signify that households who have decreased their own food consumption (e.g. due to a poor harvest) seek out loans from relatives. As relative loans are generally free of interest, it is unsurprising that their marginal impact on purchased food consumption is greater than the impact of microcredit programs.

Table 2 also shows that alternative OLS estimates continue to produce very different results when compared to fixed-effects estimates. The naïve regression continues to understate the impact of credit for both own and purchased food, as it did for total food consumption. Coleman-type regressions produce estimates of the impact of household credit that are no better than naïve estimates; the impact of credit on purchased food is actually even more understated, while the estimate on own food consumption is grossly overestimated. A possible explanation for this is revealed when the impact of credit is examined by gender. The magnitudes of

Coleman-type estimates on the effect of female borrowing are similar to those of fixed-effect estimates, while estimates of the impact of male borrowing are quite conflicting. If borrowing by household males occurred for a purpose, such as to switch from buying food to producing it, such a trend may be observed.

5.2.3 Nonfood Expenditure

Table 3 presents estimates for annual nonfood expenditure. Marriage-, birth-, and death-related costs, land and housing purchases, and development/extension of homes are typically the greatest expenses. Fixed-effects results show that participation in microcredit programs by a household increases nonfood expenditure by Tk 11 for every additional Tk 100 in the 1998/99 stock of household credit. This increase is entirely due to borrowing by females, as the coefficient on male borrowing is insignificant and very near zero. An additional Tk 100 borrowed by household females' increases annual nonfood expenditure by Tk 13, an estimate slightly higher than what other studies have found. While the trend of a higher impact found on borrowing by females is consistent with other results, the magnitude of the difference between men and women is much larger than in the other consumption measures. Loans from relatives also appear to be a substitute for microcredit borrowing, as its impact is also positive and significant.

Alternative OLS estimates are again inconsistent with those of the fixed-effects approach. Coleman-type regression estimates of the impact of borrowing are insignificant and understate program impact for household and female borrowing. Additionally, the direction of impact is reversed for both males and females when compared to fixed-effect estimates. This is further indication that males and females may borrow for different reasons and that households with female borrowers may be systematically different than households with male borrowers. This

theoretical source of bias is potentially very prevalent since the vast majority of households in the 1991/92 household survey, upon which the naïve model and Coleman regressions rely, only had either male or female borrowers.

TABLE 3. Estimates of the Impact of Microcredit on Nonfood Expenditure

Explanatory Variables	Annual Nonfood Expenditure				
	(1)	(2)	(3)	(4)	(5)
	Naïve	Coleman	Coleman	Fixed-Effects	Fixed-Effects
Program member		-1785***	-1752***		
		(541)	(541)		
Amount borrowed by Household	-0.0664**	-0.00842		0.110***	
	(0.033)	(0.037)		(0.029)	
Amount borrowed by household male			0.0602		-0.00512
			(0.051)		(0.063)
Amount borrowed by household female			-0.0598		0.132***
			(0.046)		(0.031)
Highest education of household male	413.4***	410.4***	401.8***	806.5***	793.1***
	(140)	(139)	(139)	(191)	(191)
Highest education of household female	794.0***	773.0***	776.7***	971.5***	965.0***
	(104)	(104)	(103)	(195)	(195)
Amount received from relative loans	0.0286	0.0268	0.0219	0.441***	0.440***
	(0.11)	(0.11)	(0.11)	(0.059)	(0.059)
Observations	1633	1633	1633	3266	3266

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: All regressions also include village controls, number of household members, and the gender, marital status, age, and education of household head.

5.3. Labor and Income Effects
   5.3.1 Labor Supply
      Positive effects of microcredit on consumption measures are promising, but not sufficient

to indicate a movement out of poverty for program participants. Because other factors are

important in determining the extent of poverty, it is necessary to consider additional welfare

measures. Estimating the effect of participation on labor supply and income measures are

important to better understand both the impact of microcredit programs as well as the avenues

through which participants make use of their loans. Table 4 presents results for total monthly

hours of household self-employment work.

   Fixed-effects estimates show small, positive effects of borrowing on self-employment hours.

The impact of program participation by males and females is nearly identical. An extra Tk 1000

of cumulative credit increases the amount of monthly household hours worked by 1.7. While

these results are not very meaningful in terms of an increase in labor supply, they do show that

program participants do not cut back on work in response to borrowing.

      Table 4 also presents results of monthly self-employment hours for men and women,

showing that the effect of credit on male self-employment hours is greater than the effect on

females by a factor of ten. Work hours are separated because if men and women use credit for

different purposes, the effect on labor supply may be lost when estimating the effect at the total

household level. Women's labor supply also serves as a measure for female empowerment.

Gender inequality is a problem that many MFIs attempt to tackle. Extending credit opportunities

to women is hypothesized to increase female empowerment in itself, as women are forced to

leave their homes and be part of a social group.[17] Another way that females may be empowered

is through increased bargaining power and work-hours.

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[17] The Grameen Bank, BRAC, and BRDB are the primary MFIs in this study. Also three banks engage in group-type loaning. Program participants are placed in groups before receiving loans. Individuals rather than the group are

My results, however, show that women increase their self-employment hours only minimally. Male monthly hours increase by 1.5 for every Tk 1000 borrowed, while an equal amount of credit increases female self-employment by only 0.13 hours. Whereas the coefficients on both male and female borrowing are significant and positive for male hours, only female borrowing is significant for female hours. The implication here is that borrowing by females may increase female empowerment, is not limited to female use.

Alternative OLS estimates are much better predictors of total, male, and female self-employment hours at the household level than they are for consumption measures. Still, they appear to be less efficient, as high standard deviations cause most results to be insignificant. A likely explanation for the increased accuracy is that choice-based factors causing bias in OLS estimates are smallest for labor supply measurements. For instance, unmeasured determinants, such as ability, may only minimally factor into the decision to do self-employed work.

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given credit, but the continuation of credit is dependent on group repayment. Thus, the group lowers transaction costs by enforcing repayment. For more information on the economics of group loans, see Armendariz de Aghion and Morduch (2005), chapter 4.

TABLE 4. Estimates of the Impact of Microcredit on Labor Supply

Explanatory Variables	Monthly Household Self-Employment Hours					Monthly Male Self-Employment Hours					Monthly Female Self-Employment Hours				
	(1) Naïve	(2) Coleman	(3) Coleman	(4) Fixed-Effects	(5) Fixed-Effects	(1) Naïve	(2) Coleman	(3) Coleman	(4) Fixed-Effects	(5) Fixed-Effects	(1) Naïve	(2) Coleman	(3) Coleman	(4) Fixed-Effects	(5) Fixed-Effects
Program member		-13.13**	-12.77**				-16.57***	-16.10***				3.439*	3.333*		
		(6.020)	(6.020)				(5.670)	(5.660)				(1.790)	(1.790)		
Amount borrowed by total household	-0.0001	0.0003		0.00166***		-0.0004	0.0002		0.00153***		0.00024**	0.0001		0.00013**	
	(0.0004)	(0.0004)		(0.00023)		(0.0004)	(0.0004)		(0.0002)		(0.00011)	(0.00012)		(0.0001)	
Amount borrowed by household males			0.00104*		0.00161***			0.00112**		0.00158***			-0.00009		0.00003
			(0.001)		(0.001)			(0.001)		(0.000)			-0.00017		-0.00015
Amount borrowed by household females			-0.00026		0.00167***			-0.000559		0.00153***			0.000296**		0.000148**
			(0.0005)		(0.0003)			(0.0005)		(0.0002)			(0.0002)		(0.0001)
Highest education of household male	7.641***	7.619***	7.525***	3.731**	3.725**	8.115***	8.087***	7.966***	4.130***	4.135***	-0.474	-0.468	-0.441	-0.399	-0.41
	(1.55)	(1.55)	(1.55)	(1.56)	(1.56)	(1.46)	(1.46)	(1.46)	(1.46)	(1.46)	(0.46)	(0.46)	(0.46)	(0.44)	(0.44)
Highest education of household female	-0.136	-0.29	-0.249	1.518	1.515	0.0356	-0.16	-0.107	2.27	2.272	-0.171	-0.131	-0.143	-0.752*	-0.758*
	(1.15)	(1.15)	(1.15)	(1.59)	(1.59)	(1.09)	(1.09)	(1.08)	(1.50)	(1.50)	(0.34)	(0.34)	(0.34)	(0.45)	(0.45)
Amount received from relatives	-0.00047	-0.0005	-0.00053	0.0001**	0.0001**	0.00005	0.00003	-0.00004	0.0009**	0.0009**	-0.00052	-0.00051	-0.00050	0.00010	0.00010
	(0.0013)	(0.0013)	(0.0013)	(0.0005)	(0.0005)	(0.0012)	(0.0012)	(0.0012)	(0.0005)	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0001)	(0.0001)
Observations	1633	1633	1633	3266	3266	1633	1633	1633	3266	3266	1633	1633	1633	3266	3266

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Note: All regressions also include village controls, number of household members, and the gender, marital status, age, and education of household head.

TABLE 5. Estimates of the Impact of Microcredit on Employment Income

Explanatory Variables	Monthly Household Wage Income					Monthly Male Wage Income					Monthly Female Wage Income				
	(1) Naïve	(2) Coleman	(3) Coleman	(4) Fixed-Effects	(5) Fixed-Effects	(1) Naïve	(2) Coleman	(3) Coleman	(4) Fixed-Effects	(5) Fixed-Effects	(1) Naïve	(2) Coleman	(3) Coleman	(4) Fixed-Effects	(5) Fixed-Effects
Program member		60.47	56.86				61.45	58.26				-0.983	-1.403		
		(60.0)	(60.0)				(58.9)	(58.8)				(10.6)	(10.6)		
Amount borrowed by total household	-0.00995***	-0.0119***		0.000131		-0.00971***	-0.0117***		0.000374		-0.0002	-0.0002		-0.0002	
	(0.0037)	(0.0041)		(0.0018)		(0.0036)	(0.0041)		(0.0017)		(0.0007)	(0.0007)		(0.0003)	
Amount borrowed by household males			-0.0194***		0.00188			-0.0183***		0.00145			-0.0011		0.0004
			(0.0057)		(0.0039)			(0.0055)		(0.0038)			(0.0010)		(0.0007)
Amount borrowed by household females			-0.00635		-0.000202			-0.00678		0.000170			0.0004		-0.0004
			(0.0051)		(0.0019)			(0.0050)		(0.0018)			(0.0009)		(0.0003)
Highest education of household male	50.64***	50.74***	51.67***	16.66	16.87	59.31***	59.41***	60.23***	18.93	19.05*	-8.666***	-8.667***	-8.559***	-2.266	-2.187
	(15.5)	(15.5)	(15.5)	(11.9)	(11.9)	(15.2)	(15.2)	(15.2)	(11.5)	(11.5)	(2.73)	(2.73)	(2.73)	(2.11)	(2.11)
Highest education of household female	67.91***	68.62***	68.22***	-16.70	-16.60	58.88***	59.61***	59.25***	-18.12	-18.05	9.029***	9.018***	8.970***	1.414	1.453
	(11.5)	(11.5)	(11.5)	(12.2)	(12.2)	(11.2)	(11.3)	(11.3)	(11.8)	(11.8)	(2.03)	(2.03)	(2.03)	(2.16)	(2.16)
Amount received from relatives	0.0349***	0.0349***	0.0354***	-0.0149***	-0.0149***	0.0376***	0.0376***	0.0381***	-0.0142***	-0.0142***	-0.0027	-0.0027	-0.00268	-0.0007	-0.0007
	(0.013)	(0.013)	(0.013)	(0.0037)	(0.0037)	(0.012)	(0.012)	(0.012)	(0.0036)	(0.0036)	(0.0022)	(0.0022)	(0.0022)	(0.0007)	(0.0007)
Observations	1633	1633	1633	3266	3266	1633	1633	1633	3266	3266	1633	1633	1633	3266	3266

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Note: All regressions also include village controls, number of household members, and the gender, marital status, age, and education of household head.

5.3.2 Income Effects

Given increased consumption measures and the limited amount of increased self-employment hours, households may either be using credit to improve the efficiency and value of their self-employment work, or they may be using credit to increase their wage income. Improved human capital or physical assets, such as equipment, could result in increased wage income. Development economics asserts that the extreme poor can become trapped in a situation where low income restricts their work capacity (due to a lack of nutrition and thus physical energy), and low work capacity further constrains their ability to generate income. As a result, they cannot maintain wage income.

Table 5 shows disappointing results for monthly wage income. Fixed-effects estimates of the impact of credit on total household wage income, male wage income, and female wage income show no significant results. Cross-sectional OLS estimates are generally negative and inconsistent with fixed-effects estimates, with the difference between Coleman-type estimates and fixed-effects estimates being greater than with the naïve estimates. Overall, borrowing appears to have no effect on wage income, while loans from relatives have a negative effect on wage income. The negative sign on the coefficient of loans from relatives, however, could also indicate that households receive aid from relatives when their income has experienced a negative shock. Another interpretation of the insignificant coefficients on the credit variables is that households do not borrow to invest in human capital or physical assets that increase their likelihood of improvement. Instead, households may borrow to invest more directly in their self-employment activities.

5.3.3 Microenterprise Revenues

If program participants are investing in own employment activities, their revenue from such activities should increase. Table *6* presents results for weekly net revenue of nonfarm enterprises (NFEs). Revenues from agricultural sales are troublesome to compute and vary considerably with the different harvest seasons. Revenues from NFEs activities, however, are much easier to measure. NFEs are also a good indicator to measure because many MFIs focus on microenterprise investment, with some MFIs *requiring* loans to be used for microenterprises.

TABLE 6. Estimates of the Impact of Microfinance on Nonfarm Enterprises

Explanatory Variables	Weekly Nonfarm Enterprise Net Revenue				
	(1) Naïve	(2) Coleman	(3) Coleman	(4) Fixed- Effects	(5) Fixed- Effects
Program member		131.0**	130.6**		
		(59.3)	(59.3)		
Amount borrowed by Household	0.00730**	0.00304		0.00258*	
	(0.0036)	(0.0041)		(0.0015)	
Amount borrowed by household male			0.00219		0.00499
			(0.0056)		(0.0032)
Amount borrowed by household female			0.00368		0.00212
			(0.0050)		(0.0016)
Highest education of household male	-0.0208	0.201	0.308	11.86	12.14
	(15.3)	(15.3)	(15.3)	(9.72)	(9.72)
Highest education of household female	6.871	8.414	8.367	8.422	8.560
	(11.3)	(11.3)	(11.4)	(9.94)	(9.94)
Amount received from relative loans	0.157***	0.158***	0.158***	-0.0187***	-0.0186***
	(0.013)	(0.013)	(0.013)	(0.0030)	(0.0030)
Observations	1633	1633	1633	3266	3266

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: All regressions include village controls, number of household members, gender, marital status, age, and education of household head.

Fixed-effects estimates demonstrate that household credit has a significant and positive impact on NFE net revenue. An extra Tk 100 of credit increases weekly NFE revenue by .26 taka, signifying an annual return of Tk 13.5. Table 1 shows that in 1998/99, program participants earned an average of Tk 405.80 in weekly NFE revenue, 22.5 percent more than nonparticipants. Of this increase, nearly 90 percent can be explained by borrowing.

Alternative estimates are remarkably similar to fixed-effects estimates. Naïve estimates at first overstate impact, due to higher initial NFE revenue for participants. After controlling for this difference, bias appears to be removed. While a high standard deviation eliminates the significance of the credit measures, the measure of total household program impact is nearly identical to that of fixed-effect estimates. The disparity in the standard deviations, however, may indicate that some of the unobserved characteristics removed from the fixed-effects technique remains in the OLS approach. The similarity, on the other hand, between the impact coefficients of the Coleman-type equations and the fixed-effects equations indicates that the membership control is successfully able to capture heterogeneity between program and nonprogram members. This would occur when there are no systematic differences, in terms of determinants of NFE revenue, between future borrowers and current borrowers, as well as between households with male borrowers and households with female borrowers. If heterogeneity existed between either of the two sets of groups, Coleman-type estimates would be biased.

6. Critique of Methodology

In this paper, I rely on a fixed-effects approach in estimating the impact of borrowing on household consumption, labor supply, and income measures. Fixed-effects allows for time-invariant explanatory variables to drop out of the equation, thereby eliminating bias from such unmeasured determinants. To the extent that unmeasured determinants are time-variant, bias will return. A solution to this is to instrument credit. I did not choose to do this for three reasons. First, using the same data and similar methodology, Khandker (2005) conducted a specification test and concluded that a household fixed-effects technique was more appropriate than a household fixed-effects with instrumental variables approach. Second, I was unable to do

so due to time constraints and econometric difficulty. Finally, an instrumental variables approach would have relied on heavily on the quasi-experimental nature of the survey, which has been shown to be unreliable.[18]

Downward bias may also exist in my estimates of credit variable coefficients for two reasons. First, there may be village-level spillover effects, evidence for which is presented in Khandker (2005). Spillover exists if both participants and nonparticipants benefit from microcredit programs. Since my estimation model only tests the benefit of program participants *over* nonparticipants, microcredit will have an unaccounted for, and presumably positive, impact. Downward bias may also exist because I only test for the effect of cumulative, or stock of, credit (as of 1998/99) on household welfare measures. This means that a person who increases their stock of credit by Tk 1000 in 1994 is treated the same as a person who increases their stock of credit by Tk 1000 in 1998. As my results estimate the impact of credit on, for example, annual consumption over a seven-year period, households that increase their stock of credit in later years will put downward pressure on my estimate.

7. Summary
Microfinance has come under increasing attention as the industry continues to expand. However, there still remains much uncertainty as to the validity of how impact assessments have addressed issues of endogeneity and self-selection bias. Estimates have been shown to be conditional on the technique used to approach this problem. The most commonly used method, which compares new program participants who have not yet started to receive loans -and thus have not yet benefited- to old program participants who have been using microcredit for some

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[18] Instrumenting credit in impact assessments is notoriously difficult. The quasi-experimental survey design of the survey used refers to the "functionally landless" landholding requirement of program participants. Murdoch (1999), however, showed that enforcement was of this criterion was extremely lax, and therefore may bias results.

time, has been criticized by some, but not empirically tested. In addition, there has been only little evidence on the mechanisms through which the impact of microcredit works.

Using perhaps the most extensive panel data ever collected on the issue, this paper examines the avenues through which participation in microfinance programs affects a household's consumption, labor, and income by estimating the impact of borrowing on these measures. Alternative estimates using a simpler econometric approach are compared to my fixed-effects estimates, demonstrating that cheaper, more easily implemented studies generally give cheaper, less efficient estimates. The accuracy of alternative methods depends on the extent of similarity between households with male borrowers versus those with female borrowers as well as similarity between current program participants and future participants. Only for limited household wellbeing measures, such as nonfarm enterprises, do these groups appear similar.

The influence of borrowing on household welfare measures is estimated using a fixed-effects approach, and separate estimates are allowed for men and women. Consistent with the findings in other studies, annual food consumption is shown to increase 6.7 taka for every 100 additional taka borrowed by a household, with the majority of the effect coming from borrowing by women. The increase in food consumption is driven by an increase in the consumption of purchased food, as opposed to self-produced food, indicating that credit may not be used extensively for investment into farms. An additional 100 taka borrowed by a household increases the value of annual purchased food by 4.8 taka, as compared to an increase of only 1.9 taka for self-produced food. The impact of borrowing on nonfood consumption is also positive, with an additional 100 taka in credit estimated to increase annual nonfood consumption expenditure by 13 taka. The observed impact on consumption can be partially explained by

increased nonfarm enterprise revenue among participants, an estimated Tk 13.5 increase for an additionally 100 taka of credit.

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My analysis relies on household-level data across two time periods, 1991/92 and 1998/99. Independent variables used in regressions are presented in Table A-1.

Table A-1		
Explanatory Variables	**Definition**	**Unit**
headage_	Age of household head	Years
headedu_	Education of household head	Years
headmar_	Marital Status of household head	
headsex_	Gender of household head	
hhfemaleformcred_	Cumulative amount borrowed by household females	Taka
hhformalcredit	Total cumulative credit for household	Taka
hhmaleformcred_	Cumulative amount borrowed by household males	Taka
hhrelativeloans	Amount received from relatives	Taka
maxfemaleedu_	Education level of highest education household female	Years
maxmaleedu_	Education level of highest education household male	Years
member	Membership status. Member = 1 if household ever borrows, 0 otherwise	
people_	Number of people in a household	
villegg_	Village-level price of hen egg	4- count
villmilk_	Village-level price of milk	Kg
villmoil_	Village-level price of mustard oil	Kg
villpotato_	Village-level price of potatos	Kg
villrice_	Village-level price of coarse-type rice	Kg
villwheat_	Village-level price of wheat flour	Kg

To construct variables of interest, I aggregated the data by household, where relevant. Households were matched across the two time periods through a unique identification code constructed from a one or two-digit thana ID, a one-digit village ID, a two-digit household identifier used within villages, and a one-digit 1991/92 program participation status ID. Individuals within a household could not be matched across the two time periods.

Table 1 is reprinted below and contains the means and standard deviations of the credit variables, as well as dependent variables. Data was collected at the individual level, but analyzed at the household level. Thus, individual amounts were aggregated into household totals. Where gender-specifications are made, the data had to be merged first with individual information and then aggregated at the household level.

Households were only surveyed once in 1998/99, while they were surveyed during each of the three major rice seasons in 1991/92. Thus, I had to consider seasonality, which might affect my results when matching data across the two time periods. Nonfood expenditure, for instance, was collected as a yearly average in 1998/99, but as a monthly average in 1991/92. In this case, all three rounds of data were used in the 1991/92 survey to generate the best yearly estimate, which was based on a monthly average. Similar averages had to be constructed using the three rounds for labor supply and wage-employment variables.

Other dependent variables, however, were only matched from a single round of the 1991/92 survey. Food consumption and NFE revenues were collected as a weekly average, based on the past month, or past four months. Interview dates from the later survey were considered and data from the corresponding season in the first survey were used. The great majority of such interviews in 1998/99 took place between July and October of 1999. Thus, data from the 1998/99 survey were compared with 1991/92 data from the *Aus* season, which corresponds to the same months.

Table 1. Summary Statistics of Consumption, Income, and Labor Variables

Variable	Participants	Nonparticipants	All Households
1991/92			
Total Household Borrowing[a]	9,066.53		4,297.30
	(7,974.83)		(7,115.52)
Borrowed By Household Females[a]	5,609.28		2,658.66
	(7,301.39)		(5,753.26)
Borrowed By Household Males[a]	3,457.25		1,638.65
	(6,448.34)		(4,762.02)
Annual Total Food Consumption	15,824.43	16,754.42	16,313.63
	(6,747.73)	(9,582.15)	(8,369.81)
Annual Purchased Food Consumption	11,636.81	11,461.92	11,544.81
	(5,669.4)	(6,935.33)	(6,365.45)
Annual Self-Produced Food Consumption	4,187.62	5,292.50	4,768.82
	(5,492.79)	(7,073.22)	(6,395.14)
Annual Nonfood Expenditure	4,518.45	6,635.33	5,631.98
	(5,526.2)	(12,989.88)	(10,212.43)
Weekly Nonfarm Enterprise Revenue	351.48	270.50	308.88
	(864.3)	(1,229.27)	(1,072.34)
Monthly Household Wage Income	614.38	763.14	692.63
	(930.99)	(1,235.84)	(1,104.09)
Monthly Female Wage Income	35.44	45.60	40.79
	(167.59)	(203.72)	(187.48)
Monthly Male Wage Income	578.93	717.54	651.84
	(921.9)	(1,203.)	(1,080.83)
Monthly Household Self-Employment Hours	84.92	87.14	86.09
	(100.91)	(119.15)	(110.85)
Monthly Female Self-Employment Hours	24.17	17.25	20.53
	(33.89)	(28.15)	(31.18)
Monthly Male Self-Employment Hours	60.75	69.89	65.56
	(94.05)	(114.06)	(105.12)
Number of Observations	774	859	1633
1998/99			
Total Household Borrowing[a]	25,819.93		16,522.86
	(28,457.41)		(25,918.29)
Borrowed By Household Females[a]	19,942.36		12,761.65
	(27,179.6)		(23,754.24)
Borrowed By Household Males[a]	5,877.57		3,761.21
	(15,859.97)		(12,995.21)
Annual Total Food Consumption	24,941.08	26,142.62	25,373.72
	(12,546.07)	(18,149.52)	(14,815.73)
Annual Purchased Food Consumption	19,239.76	18,987.11	19,148.79
	(11,979.66)	(14,842.8)	(13,079.04)
Annual Self-Produced Food Consumption	5,701.32	7,155.51	6,224.93
	(8,411.02)	(10,601.16)	(9,282.58)
Annual Nonfood Expenditure	12,707.74	16,370.98	14,026.78
	(22,978.64)	(31,088.34)	(26,239.22)
Weekly Nonfarm Enterprise Revenue	405.80	331.21	378.94
	(881.54)	(811.87)	(857.61)
Monthly Household Wage Income	761.17	641.19	717.97
	(1,089.79)	(1,050.61)	(1,077.06)
Monthly Female Wage Income	48.49	38.12	44.76
	(218.6)	(197.08)	(211.1)
Monthly Male Wage Income	712.68	603.07	673.21
	(1,062.22)	(1,015.54)	(1,046.66)
Monthly Household Self-Employment Hours	177.28	144.22	165.38
	(177.43)	(185.73)	(181.11)
Monthly Female Self-Employment Hours	20.80	11.39	17.42
	(49.75)	(40.36)	(46.8)
Monthly Male Self-Employment Hours	156.48	132.83	147.96
	(166.46)	(182.61)	(172.76)
Number of Observations	1045	588	1633

Numbers in parentheses are standard deviations
a. Borrowing is measured as cumulative borrowing