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Measurement of Welfare Increase in Microcredit Institutions: Program Evaluation Techniques

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Abstract

Existing microcredit literature lacks studies analyzing impact of microcredit programs on the economic condition of participants. Using conventional program evaluation techniques on the impact of microcredit programs, this study attempts to fill this void. Measures of program impact can be biased due to the presence of unobserved hetereogeneity in the sample. Using suitable instrument, we remedy this problem and find significant positive impact of microcredit programs on program participants.

1. Introduction

Microcredit programs are small-scale credit programs that provide production credit and other services to rural poor. Microcredit programs work exclusively with the poor. Although sequence of delivery and the provision of inputs vary a little from program to program, all programs essentially offer production credit to the landless rural poor (defined as those who own less than half an acre of land) formed into a group, using peer monitoring as a substitute for physical collateral. Loans are given to individual group members, but the whole group becomes ineligible for further loans if any member defaults. The groups meet weekly to make repayments on their loans as well as mandatory contributions to savings and insurance funds. These microcredit programs also provide noncredit services in areas such as consciousness-raising, training for skill development, literacy, bank rules, investment strategies, health, schooling, civil responsibilities, and alteration of the attitude of and toward women.[1]

Among the existing literature on evaluation of microcredit programs, we find several studies analyzing the impact of these programs on different aspects of women's welfare, e.g. reduction in fertility [Amin,

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[1] For more on micro credit programs in Bangladesh see Khandker (1998)

Hill, Li (1995)], increased empowerment of women in household [Hashemi, Schuler, Riley (1996)], having a greater impact on household welfare parameters [Pitt and Khandker (1998)], improving health status of children (Pitt et el., 2003) etc. But we did not come across any study which systematically analyzes the change in economic condition of program participants after participation in these programs. Using conventional program evaluation methods, we find significant improvement in the economic condition of microcredit participants.

This study also corrects for the self-selection bias that might arise due to the fact that program participants decide on whether to participate in these programs or not. Previous studies, that attempted to estimate program impact, simply compared outcomes between participating and nonparticipating households. For example, a widely cited study similar in scope to this (Bangladesh Institute of Development Studies 1990), carried out in the 1980s, did not address self-selection into the credit programs studies. To the extent that program participation is self-selective, it is not clear whether measured program effect reflects, in part, unobserved attributes of individuals (such as ability, health and preferences) that affect both the probability that they will participate in the programs (and the extent of the participation) or the impact on any welfare measure. We show that by using instrumental variable technique, this selection bias can be sufficiently removed.

The remainder of this paper is organized in the following manner. Section 2 discusses different suitable measures to evaluate women's performance in microcredit programs. Section 3 discusses available program evaluation techniques in the literature and section 4 provides a summary of the data. Section 5 reports results and section 6 draws conclusion.

2. Evaluating Performance In Microcredit Programs: Suitable Measures

The main objective of this study is to investigate whether participants in microcredit programs achieved success. Success in microcredit programs can be measured by improvement in economic condition or increase in welfare of the microcredit participants .

For measuring welfare, ideally we would like a survey-based measure that represents the individual welfare measures of economic theory. Particularly useful here is the concept of money metric utility where the indifference curves of individual preference orderings are labeled by the amount of money needed to reach them at some fixed set of prices. In

order to avoid the specification of a parametric utility function, money metric utility can be approximated by real income or real expenditure: the two leading candidates for practical welfare measures. However, there are other possibilities, indicators of nutritional status being perhaps the one of the most important among them, and even if we settle on income or expenditures, there are many other questions that have to be settled before going on to compute the measures.

Whether the welfare measure is income or consumption, it becomes difficult to measure it for a single member in a household. Typically, microcredit programs allow one individual from each household to join and in most cases it is the female members. It is difficult to find evidence of economic success of participants by looking at just household income or expenditure that involves other members. In our analysis, we choose the following three measures as possible indicators of women's economic welfare: non-agricultural assets, food expenditures and non-food expenditures.

Non-land assets measure the current value of non-land assets (equipment and goods) that the program participants were able to accumulate after joining microcredit programs. This measure is available for each adult member of a household. During the time of survey, each member is asked how much assets she had before joining these programs and how much assets she accumulated after joining. Following are the types of assets included in this category: gold or silver jewelry, household/kitchen utensils, furniture, processing equipment, tools like spade, hammer etc. *Food expenditures* measure the value of normal daily food consumption in a household in the last one year. *Total non-food expenditures* measure the value of expenditures incurred by households on non-food items in the last four months from the time of survey. In the data, expenditures on different categories are mentioned. These expenditures mainly include expenditures on childcare, home improvement, medicine, fuel, book/stationary.

Among the above three possible indicators of economic condition of program participants we find that only non-land assets are individual specific whereas food and non-food expenditures are household specific. As we have discussed above, it is difficult for household specific measures to identify impact on individual members. Therefore, we choose the only individual specific measure that we have which is non-land assets accumulated by the program participants.

3. Research Methodology

The most famous model for evaluating program effect[2] is the difference in difference (DID) estimator. Basic idea behind DID estimator is very simple (for details see Annex). To get an unbiased estimate of program effect, we have to get rid of time effect and group effect. Time effect is the impact on time variables that might naturally occur over time. Group effect is the effect that occurs due to different characteristics of program participants and non-participants. [3] If we compare the mean value of outcome variable for program participants in two time periods (before and after taking participation in the program) then the difference will reflect the sum of two effects: program effect and time effect. Group effect will be differenced out because the it is the same group. On the other hand, if we compare the mean value of outcome variable for non-participants then the difference will only reflect time effect. Now if we take the difference between two differences, we will get the program effect while the time effect will be differenced away. Hence the name difference in difference to calculate program effect.

To check whether the program impact is statistically significant, we have to measure the program impact in a regression formulation. Without using the regression formulation we cannot conveniently say whether the program impact is significant or not. We should essentially find that the value of program effect coefficient is exactly same as we measured by the DID estimate Moreover, it is possible that the program impact that we have measured so far is biased as we do not control for different characteristics of the individuals and also those of the households or villages they live in. Incorporating these characteristics provides a simple way to adjust for observable differences between the observations in different groups. [4]

The research design is the essence of two often cited studies in the program evaluation literature. Card and Krueger (1994) examined the effects of an increase in the New Jersey state minimum wage on employment. Their sample consists of fast-food restaurant from four chains in New Jersey before (t = 0) and after (t = 1) the increase in the

[2] For more on program evaluation, see Moffitt (1991).

[3] An example of time effect might be, as years go by, asset accumulation might increase, with or without participating in a program. For group effect, participant group might be more hardworking, skillful than non-participants and they participated because they would be successful using those characteristics.

[4] For details on the econometric formulation of measuring program impact, please see Annex.

minimum wage. In addition, they examined employment at a sample of similar restaurants in eastern Pennsylvania over the same time period. This sample from Pennsylvania provides a group (j = 0) that is plausibly subject to the same changes over time as the group in New Jersey, except that Pennsylvania did not change the minimum wage. In another study, Meyer (1994) examined the effects of two large workers' compensation benefit increases on the length of claims. They also relied on a untreated comparison group, as well as before and after groups. The untreated comparison group is those individuals within a state who were not subject to the increases in workers' compensation benefits because they had average or low earnings. These comparisons workers were likely to be subject of any other changes in program administration or insurers' claim-monitoring procedures.

In our study, the program impact (β) can be estimated directly by applying ordinary least squares to the regression formulation as discussed above. In essence, the dependent variable will be the outcome variable and among the independent variables there would be a dummy variable capturing the group effect (participants or non-participants), another dummy variable would capture the time effect (before or after participation) and remaining variable would capture individual, household or village characteristics. Incorporating these characteristics provides a simple way to adjust for observable differences between the observations in different groups.

4. Data

The dataset comes from a multi-purpose household survey, which was conducted in 87 villages of 29 thanas (sub-districts) in rural Bangladesh, during the year of 1991-92. The sample consists of 29 thanas randomly drawn from 391 thanas in Bangladesh, of which 24 had one (or more) of the three credit programs understudy in operation, while 5 thanas had none of them. See Table 1 for the distribution of the three microcredit programs among 87 villages.

Three villages in each program thana were randomly selected from a list of villages, supplied by the program's local office, in which the program had been in operation for at least three years. A household census was conducted in each village to classify households as target (i.e., those who qualify to join a program) or non-target households, as well as to identify program participating and non-participating households among the target households. A stratified random sampling

technique was used to over-sample households participating in one of the credit programs and target nonparticipating households. Of the 1798 households sampled, 1,538 were target households and 260 non-target households. Among the target households, 905 households (59 percent) were credit program participants. This survey was conducted three times over the crop cycle year 1991-92 to match the three crop seasons, and information on village-level prices and wages was collected in the same manner.

Table 1. Distribution of villages by credit program and group type

	BRAC	BRDB	GB	None	Total
Female only	7	3	12	0	22
Male only	0	9	1	0	10
Female and male	17	12	11	0	40
No program	0	0	0	15	15
Total	24	24	24	15	87

Note: Sample size is 87 villages, 1775 households, and 9215 individuals

5. Econometric Analysis and Results

5.1 Description of the variables

Table 2 briefly describes the variables in our analysis which includes total non-land assets (*TNLND)* along with observed characteristics of these women and also the characteristics of the household or village they live in.

On the average, these non-land assets amount to 6854.34 Taka. To put these values into perspective, per capita GDP of Bangladesh was $1225[5] during the same time period and by converting it to prevailing exchange rate, it amounts to approximately Taka 4000 per month.

The sample of individuals (participating in microcredit programs) aged between 15 and 64 years is middle aged: mean age (*AGE*) is around 35 years. The educational level (*SCHOOL*) of program participants is very low, averaging merely two years. The average of the highest education attained by the household head (*EDUHEAD*) is also around 2 years of schooling. Around 11 percent of household does not have any spouse present (*NOSPOU*) in the household. Distance of the current residence of program participants to the nearest microcredit program office (*DISTPRG*) is, on the average, around 10 kilometers. The explanatory variables also include availability of rural health center (*RURHLTH*) (6 percent), family planning center (*FAMPLN*) (12 percent).

[5] World Development Indicators, World Bank, 2005.

Average hourly male wage (*AMWG*), which might be an indicator of household income, is around TK 36. The distance of a finished road (*DISTRD*) from the village where program participants live is on the average 2 kilometer. Around half of the villages have access to electricity (*ELEC*).

Table 2. Description of the variables

Variable Name	Description	Mean	Standard Deviation
*TNLND**	Non-agricultural Assets (Taka**)	6854.34	17831.68
AGE	Age of program participants (Years)	35.55	9.381
SCHOOL	Schooling (Number of years in school)	1.83	2.465
PARENT	Whether parents live in the same residence	0.472	0.499
DISTPRG	Distance of current residence from lending microcredit program (Kilometers)	10.705	30.361
NOSPOU	Spouse not present in household?	0.014	0.119
EDUHEAD	Highest grade completed by household head (years)	2.028	3.090
PFEM	Percent of female members in household	0.496	0.106
FAMPLN	Whether the village has any family planning center	0.122	0.328
RURHLTH	Whether the village has any rural health center	0.069	0.254
DISTRD	Distance of the nearest paved road from the village (Kilometers)	2.228	3.079
AMWG	Average hourly male wage in the village	36.635	8.292
ELEC	Whether electricity is available or not in the village	.5223	.50

** Dependent variable*

***Taka is the official currency of Bangladesh. Between December 31, 1990 and January 1, 1992 (the same duration of time for the survey), the exchange rate (Taka/$1 dollar) ranged from 35.79 Taka to 39.90 Taka (IFS by IMF, 1990-1992).*

5.2 Regression results where participation is exogenous

Table 10 presents the regression results for the regression equation (3) formulated in section 3 . As explained in the previous section, the main focus of our study is in the value of the coefficient. β since it measures the actual program impact. Other explanatory variables are also important as they explain how the participation decision is affected by different characteristics of participants but that is not the main area of interest for this study. We are mainly interested in the effect of participation on

microcredit borrowers compared to non-participants. Nevertheless, we will discuss some of the important characteristics that might have significant impact on participation decision.

Table3. Effect of participation on non-land Assets of Microcredit borrowers

Explanatory Variables	***Estimates (participation exogenous)***
Time Effect (α_1)	11428.72 (10.42)
Participation Effect (α^1)	3928.598 (4.54)
Program effect (β)	-7357.131 (-6.2)
AGE	-1117.03 (-3.74)
WSCHOOL	210.33 (6.37)
NOSPOU	-1151.96 (-1.06)
EDUHEAD	-213.896 (-1.71)
GENHEAD	-1747.81 (-1.2)
AGEHEAD	-200.7519 (-6.43)
PFEM	-4203.368 (-2.43)
PARENT	1073.904 (2.16)
PRSCHL	165.4897 (0.33)
RURHLTH	-4644.355 (-5.29)
FAMPLN	1123.829 (1.45)
DISTRD	-117.3392 (-1.54)
AMWG	141.455 (6.67)
ELEC	1276.208 (2.78)

Note.- Figure in parentheses indicates t-ratio.

We find that in the above table 3 the value of coefficient β is -7357.13.

This basically means that compared to (targeted) non-participants, participants in microcredit programs accumulated 7357.13 Taka worth less non-land assets. It implies that participants were worse off after joining microcredit programs. The time effect (α_1), irrespective of joining or not joining microcredit programs, the value of non-land assets has gone up by Tk 11428.72. Again, the time effect measures the accumulation of non-land assets which grows naturally over the time for various reasons, increase in family size might be an example. The participation effect (α^1) measures the time invariant component of the program effect. From our estimates in the above table, at any given point of time, the program participants accumulated Tk 3928.598 more than non-participants. The reason, overall program effect came out negative is because the time effect outweighs the participation effect by a fair margin.

Before we draw any conclusion from the above results, we need to have a closer look at the econometric formulation of the model that we are working with. One possible explanation for this seemingly unrealistic[6] result might be the participation decision itself is endogenous which might make β and ε (in equation 3) correlated. This problem is intrinsically related to the self-selection bias mentioned earlier. This problem arises because the same unobserved characteristics (captured in ε) that affect asset accumulation might also affect the participation decision. For example, one of the unobserved characteristics might be the ability to work hard. It might affect how much asset the person accumulates. It should also affect whether the person would join a program where hard work is needed. This problem is intrinsically related to the issue of self-selection of these members. The members decide whether they would join the program or not and that makes the participation decision endogenous.

5.3 Finding instrument for endogenous participation decision

The usual way to tackle the endogeneity of explanatory variable is to find an instrument for it. The essence of instrumental variable technique is to find a variable which is correlated with the endogenous variable (for which we are looking for an instrument) and likely to be uncorrelated with the error term. A suitable candidate for an instrument for participation decision might be a measure of distance of program participant's residence from the program office of microcredit disbursement centers. In table 2, *DISTPRG* measures the distance (in Kilometer) to the nearest program office from where the participating microcredit households get loans. It might affect the participation

[6] Unrealistic in the sense that whereas there are numerous indirect evidence (Khandker 1998) that microcredit programs are an important tool to fight poverty, our direct measures here are contradicting this widely accepted phenomenon.

decision as the transportation in the village is primitive and most of the people walk. These microcredit programs require weekly group meetings in their program branch offices to assess performance and to provide training. Therefore if the program office is situated a little far, program participants might be discouraged to get more involved in the credit programs. But there is little doubt that this measure of distance would be uncorrelated with the unobserved characteristics of program participants.

This instrumental variable technique is inherently related to the natural experiment process. Natural experiment tries to provide exogenous variation in an endogenous explanatory variable. By applying above instrumental variable method, we are exactly following the process of natural experiment. We are trying to relate endogenous participation decision to a variable which is exogenous to the model. Participants are being offered the program membership. All of them are receiving treatment or controlled status in a random manner based on their distance to the nearest program office.

There is a probability that the characteristics of villages, where program offices are actually situated, might influence the performance of women - if those villages are different from the villages where program participants live. But our data shows that there is no village which has a program office and does not have any program participants residing in. Therefore, those characteristics of villages where program office are situated are already controlled for in our study.

5.4 Regression Results where participation is endogenous

Table 4 shows how the use of this instrumental variable technique alters the results provided by original regression. We find that the measured program effect stands at Tk 73.48 and the effect is now significant at least at 10 percent level of significance. While without using any instrument we observed negative effect of program participation, by using instrument for endogenous program participation we found, albeit much smaller, positive impact of program participation.

Among other important explanatory variables, education level (*SCHOOL*) has significant impact on the non-land assets accumulated by the program participants. One year increase in schooling increases the non-land assets by Tk 858. Among household characteristics, absence of spouse (*NOSPOU*) does not seem to have any significant impact on participants' performance in microcredit programs. We can hypothesize that absence of spouse might have a negative impact on the performance of microcredit programs in the sense that participant alone has to support the family expenditures and it would negatively affect his/her performance in microcredit programs. On the other hand, we can also argue that absence of spouse might allow the participant to work independently and that might positively affect performance. From our results in table 4, we get the evidence for the latter hypothesis but the effect is not statistically significant.

Among the village characteristics, the presence of a family planning center (FAMPLN) actually increases participant's non-land assets by Tk 916.73. This might be because family planning center helps to keep the family size small on the average and as a result program participants can spend more on the accumulation of non-land assets. Among other village characteristics, availability of electricity (ELEC) has a strong impact on the accumulation of non-land expenditures. Program participants who have access to electricity accumulated Tk 1154 more non-land assets compared to program participants who do not have any access to electricity. Electricity might make the program participants more productive in their microcredit activities and that might have been reflected in increased accumulation of non-land assets.

Table 4. Effect of participation on non-land Assets of microcredit borrowers

(*DISTPRG* is used as instrument for participation decision)

Explanatory variables	**Estimates**
Time Effect (α_1)	4956.121 (10.39)
Participation Effect (α^1)	-35.61634 (-1.24)
Program effect (β)	73.48225 (1.67)
AGE	202.626 (6.28)
SCHOOL	858.3506 (6.51)
NOSPOU	908.1369 (1.18)
EDUHEAD	-315.308 (-2.53)
GENHEAD	6.519 (0.16)
AGEHEAD	-190.3304 (-6.18)
PFEM	-374.087 (-0.23)
PARENT	958.8827 (1.94)
PRSCHL	-163.0713 (-0.33)
RURHLTH	-4226.724 (-4.83)
FAMPLN	916.7345 (1.18)
DISTRD	-97.22221 (-1.27)
AMWG	130.8099 (6.21)
ELEC	1154.003 (2.51)

Note.- Figure in parentheses indicates t-ratio.

6. Summary and Conclusion

In this study we have used standard program evaluation technique applied to measure the program impact of microcredit programs. Among the usual program evaluation techniques, for example, the across time or group comparison does not include any controls for different types of characteristics. As a result, there is a good possibility that these results are positively biased.

Another danger lies in the endogeneity of participation decision. The fact that members decides to join the microcredit programs makes the participation decision endogenous. We use instrumental variable technique to get around this problem. We find that the variable measuring distance of participant's residence to the nearest program office qualifies as a good instrument for participation decision. Using this instrument, we find significant program impact for program participants even after controlling for individual, household or village characteristics.

One problem that lies with this study is that time frame for program participants is not defined very homogeneously. Program participants are asked about their non-land asset accumulation before participating and after participating in microcredit programs. But all the program participants did not join the microcredit programs at the same time period. Therefore, it is likely that there might considerable heterogeneity in program effect which is not captured in this cross-sectional model. Future research which involves time series data might be free from these biases and provide more accurate measure of program impact for participants in microcredit programs.

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APPENDIX 1

Econometric formulation of program impact

When one has a comparison group over the same time period as the before and after groups, often the underlying model of the outcome variable is of the form:

$$y_{it}^{j} = \alpha + \alpha_1 d_t + \alpha^1 d^j + \beta d_t^j + \varepsilon_{it}^{j}, \qquad (1)$$

where the outcome *y* is now also indexed by *j* for the group, *j*=0,1(where 0 and 1 represents control and treatment group respectively), and d_t = 1 if *t* = 1 (before participation) and 0 (after participation) otherwise, d^j=1 if *j* =1 and 0 otherwise, and d_t^j=1 if *t*=1 and *j*=1 and 0 otherwise. The key idea behind this approach is that α_1 summarizes the way that both group *j* = 0 and group *j* = 1 are influenced by time. There may be a time-invariant differences in overall means between the groups *j* = 0 and group *j* =1, but this aspect is captured by α^1.

Finally, d_t^j is a dummy variable for being in the treatment group after it receives the treatment, and β is the true causal effect of the treatment on the outcome for this group. Again the key identifying assumption is that β would be 0 in absence of the treatment, of $E(\varepsilon_{it}^{j} \mid d_t^j) = 0$. In this case, an unbiased estimate of β can be obtained by DID as

$$\begin{aligned} \hat{\beta}_{dd} &= \Delta\bar{y}_0^1 - \Delta\bar{y}_0^0 \\ &= (\bar{y}_1^1 - \bar{y}_0^1) - (\bar{y}_1^0 - \bar{y}_0^0) \qquad (2) \end{aligned}$$

where again a bar indicates an average over *i*, the subscript denotes the time period, and the superscript denotes the group.

In our study, the program impact (β) can be estimated directly by applying ordinary least squares to equation (1). This method would reproduce $\hat{\beta}_{dd}$ the same way it did for the one group design. Without using the regression formulation we cannot conveniently say whether the program impact is significant or not. Moreover, it is possible that the program impact that we have measured so far is biased as we do not control for different characteristics of the individuals and also those of the households or villages they live in. Incorporating these characteristics provides a simple way to adjust for observable differences between the observations in different groups. Using this formulation may also improve the efficiency of the estimate of program impact by reducing the residual variance. By adding these additional variables, the program impact equation takes the following form:

$$y_{it}^{j} = \alpha + \alpha_1 d_t + \alpha^1 d^j + \beta d_t^j + \beta_1 I + \beta_2 X + \beta_3 Z + \varepsilon \qquad (3)$$

I = individual characteristics (age, education etc.)

X=household characteristics (education of household head, female members etc.)

Z = village characteristics (availability of rural health center, electricity etc.)