

Estimating the Long-Run Impact of Microcredit Programs on Household Income and Net Worth

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Abstract

This paper investigates whether the utilization of microcredit programs has a significant impact on the income and net worth of the participants. Several micro finance institutes are optimistic on the beneficial effects of microcredit programs. Others describe microcredit with interest rates in excess of 20 percent as a poverty trap. This paper uses

more than 20 years of panel data on households in Bangladesh to estimate bounds on the causal effects of microcredit programs. The analysis rejects the hypothesis that these microcredit programs are a poverty trap. Moreover, the paper finds moderately positive effects of such programs.

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Estimating the Long-Run Impact of Microcredit Programs on Household Income and Net Worth¹

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1. Introduction

Microcredit has improved credit access for the poor on a large scale. According to the Microcredit Summit, more than 200 million borrowers use microcredit.⁴ Several Micro Finance Institutes and their supporters are very optimistic on the beneficial effects of microcredit programs. See, for example, Yunus (1999 and 2007). Others describe microcredit with interest rates in excess of 20% as a “poverty trap”. Thus, it seems desirable to evaluate the record of microcredit without making controversial assumptions. The main controversy in the academic debate on microcredit is that participation in a microcredit program is assumed to be uncorrelated with unobserved characteristics of the potential participants.⁵ We allow for the unobserved characteristics to be correlated with the regressors and we are the first to estimate bounds on the causal effects of microcredit programs. We use more than 20 years of panel data on households in Bangladesh based on the surveys carried out by researchers in the World Bank’s Development Research Group, Bangladesh Institute of Development Studies (BIDS) and Institute of Microfinance (InM). We find moderately positive effects of such credit programs on household welfare.

Randomized controlled trials (RCT) are an alternative to bounds analysis. However, as experimental data about long-term impacts are not available at this time, it seems even more significant to consider an approach that allows for selection bias from

⁴ See <http://microcreditsummit.org>.

⁵ See the ongoing debate following the influential study of Pitt and Khandker (1998) who showed first the substantial positive effects of microcredit programs. The literature on the effects of microcredit is controversial: Several studies show the positive effects of microfinance (Coleman 2006; Karlan and Zinman 2010; McKenzie and Woodruff 2008; de Mel, McKenzie, and Woodruff 2008), while others find no evidence for income or consumption gains (Augsburg et al. 2011; Attanasio et al. 2012; Duflo et al. 2013; Karlan and Zinman 2011; Crépon et al. 2011). Other recent examples on this subject are Roodman and Murdoch (2014) and Pitt (2014).

program participation and the use of existing data sets from long panels. In particular, one would have to wait more than 10 years to estimate the long-term impacts using an RCT.⁶

This paper derives bounds on the causal effect of participating in a microfinance program on household income and household net worth. This is the first paper that uses bounds in this context. The previous papers assumed that an error term and the regressors were uncorrelated or independent. The fierceness of the debate in development literature shows that such an independence assumption is very controversial.

A related paper is Altonji, Elder, and Taber (2005). There, Altonji et al. propose using the degree of selection on observed variables as a guide to the degree of selection on the unobservables. In particular, Altonji et al. (2005) consider the bivariate probit model. Rather than estimating the correlation between the error terms in the selection and outcome equations, Altonji et al. (2005) regress the index of the selection equation on the index of the outcome equation. They then use this estimate as an upper bound for the correlation between the unobserved error terms.⁷ Our methodology differs from theirs since we do not use the correlation between indices of probit models. In particular, we weaken commonly used exclusion restrictions rather than improving the identification of the bivariate probit model.

⁶ Bounds may be also applied in the case of RCT design. Using bounds means that one does not have to impose the restriction that an error term is uncorrelated with an instrument or with a regressor. This makes sense with any behavioral data. If everything goes well with a RCT then one can just compare the mean outcomes (or distributions) and one would not need to use bounds. But if something goes wrong with a RCT (e.g. a fraction of the control groups manages to get treatment) then one could consider bounds as well. How to use the bounds in that context depends on the violations of the RCT design.

⁷Dujardin and Goffette-Nagot (2009) apply the methodology proposed by Altonji et al. (2005) and Falaschetti (2009) discusses the advantages and disadvantages of it.

This paper proceeds as follows. We describe the data in section 2. In section 3, we state the proposed estimation method and present our results. We conclude in section 4.

2. Data on Microcredit Programs

The survey data that we use in our study are derived from a long panel survey of over 20 years. The first survey round, conducted in 1991/92, studied the role of microfinance in the economic development of the poor (e.g. Pitt and Khandker 1998; Khandker 1998). This survey was carried out by the researchers in the World Bank Development Research Group and the Bangladesh Institute of Development Studies (BIDS) and consists of 1,769 households that were randomly selected from 87 villages in 29 rural subdistricts (so called *upazilas*) in Bangladesh. A second survey was conducted in 1998/99. This survey retraced 1,638 of the earlier households so that the attrition rate is 7.4 percent. The 1998/99 survey also included new villages and new households from old villages. In total, 2,599 households were surveyed (Khandker 2005). A third survey was carried out in 2010/11 by researchers in the World Bank Development Research Group and the Institute of Microfinance (InM). This third survey round tried to revisit the 2,342 households of the 1998/99 survey plus 740 households that split off to form new households. In all, 3,082 households were interviewed in 2010/11 and the attrition rate was about 10 percent (using the 1991/92 survey as a base). A total of 1,509 households were common to all three survey rounds. We use the 1991/92 survey (no attrition) when we derive the bounds for the first round in order to make our results directly comparable to Pitt and Khandker (1998). The results did not change when we used the data on

households that were common in all three survey rounds. Khandker and Samad (2013) use the same data but did not use bounds. They describe the data in more detail.

3. Estimating the Effect of Microcredit Loans

In this section, we state our model for dealing with the link between unobservable determinants of both microcredit loans and household net worth. Let the variable X_i be one if a household member of household i is enrolled in a microcredit program during the first (1991/92) survey and let X_i be zero otherwise where $i=1, \dots, N$ and N is the sample size. Let the variable Y_{it} be the net worth (or the income) of household i in period t where $t=1, 2, 3$ and $i=1, \dots, N$.

Hypothesis 1: Preying on the disadvantaged or “sucking blood from the poor”.

Preying on the disadvantaged or vulnerable implies negative selection into participating in a microcredit program. In particular, the more able individuals are presumably less likely to participate in a blood-sucking program. Therefore, consider the following selection equation.

$$(1) X_i = \gamma V_i + \delta + u_i,$$

where V_i is a measure of vulnerability of a household and u_i is uncorrelated with V_i by construction. Now consider the outcome equation.

$$(2) Y_{it} = \beta X_{it} + \alpha_{\text{village } it} + \varepsilon_{it},$$

where ε_{it} is an unobserved error term and $\alpha_{\text{village } it}$ is a village fixed effect that depends on t (i.e. it is allowed to change if a household moves to another village in the data set although that basically never happened). The parameter β denotes the causal effect of

participating in the microcredit program on household net worth (or income). In the empirical analysis, we also include other regressors in some specifications but we omit them here so that we can focus on the main ideas.⁸ If one believes that microcredit programs ‘prey’ on disadvantaged individuals, then $\gamma > 0$. Thus, the correlation between V_i and the unobserved error term in equation (2) causes the least squares fixed effect estimator of β in equation (2) to be biased downwards. That is, the least squares fixed effect estimator yields a lower bound (see appendix for further discussion). The p-value of the hypothesis that the causal effect on net worth *and* income is negative is always low. In particular, it is below 3.1% for all specifications and for all periods.⁹

We now derive a lower bound on the causal effects using another assumption. This assumption does not contradict hypothesis 1 and should be interpreted as a complement or as an alternative to hypothesis 1.

Hypothesis 2: Fewer opportunities in the poorer villages

The first survey round, conducted in 1991/92, studied the role of microfinance in the economic development of the poor. The World Bank and the Bangladesh Institute of Development Studies selected villages that were relatively poor compared to other villages. See, for example, Pitt and Khandker (1998) or Khandker (1998). One could initially only get microcredit in the selected villages. This suggests using ‘living in a selected village’ as an instrument but the resulting instrumental variable estimator is

⁸ The regressors that we use are age of the household head, the maximum education of the male and the maximum education of the female all at the time of wave 1.

⁹ The p-values for testing that the causal effect on net worth is negative is reported in table 2, column 1; similarly, the p-values for testing that the causal effect on income is negative is reported in table 2, column 2. The p-value of the joint hypothesis is equal or smaller than the minimum of these two values.

downward biased since the poorer villages were selected. The following selection equation makes this explicit. Let the participation depend on whether one lived in a selected village, denoted by the binary variable Z_i , and an error term that is uncorrelated by construction,

$$(3) X_i = \lambda Z_i + \kappa + u_i.$$

Thus, the instrument that we use is the dummy variable that indicates whether the village was selected for treatment in the first round. Given this instrument, one does not want to use a village fixed effect in the outcome equation since the instrument would have no predictive power in that case. Consider, therefore, the following outcome equation.

$$(4) Y_{it} = \beta X_{it} + \alpha + \eta_{it},$$

where η_{it} is an unobserved error term. As before, the parameter β denotes the causal effect of participating in the microcredit program on household net worth (or income). In the empirical analysis we also include the same regressors as before in some specifications. Selecting poorer villages yields an instrumental variable estimator that is downward biased since the instrument, Z_i , is negatively correlated with the error term η_{it} . That is, the instrumental variable estimator yields a lower bound (see appendix for further discussion). The p-value of the hypothesis that the causal effect on net worth *and* income is negative is low in wave 1 and 2. In particular, it is below 2.2% for all specifications in these two waves. In the third wave the lower bounds are positive but less precisely estimated so that the p-values are larger for that wave.

4. Conclusion

This paper investigates whether the utilization of a microcredit program has a significant impact on the household income and the household net worth of the program participants.

We estimate bounds and reject the hypothesis that microfinance does not help the poor.

We do not assume that unobserved characteristics of the families and participating in a microcredit program are independently distributed, making our estimates more credible.

As experimental data about long-term impacts are not available at this time, it seems even more significant to consider an approach that allows for selection and the use of existing data sets. We are confident that our approach gives a credible answer to the question of whether microcredit causes a poverty trap and we reject that hypothesis.

Appendix:

1. The bound implied by the least squares with village fixed effects.

Consider equation (2),

$$Y_{it} = \beta X_{it} + \alpha_{\text{village } it} + \varepsilon_{it}.$$

Let the error term ε_{it} be uncorrelated across villages so that the least squares estimator with village fixed effects for β converges in probability¹⁰ to $\beta + \text{Cov}(X_{it}, \varepsilon_{it}) / \text{Var}(X_{it})$. Hypothesis 1 states that Micro Finance Institutes prey on the vulnerable. Given the village fixed effect (and the regressors in some specifications) this implies that the error term ε_{it} is negatively correlated with participation in microcredit X_{it} . This implies that $\text{Cov}(X_{it}, \varepsilon_{it}) / \text{Var}(X_{it})$ is negative so that the least squares estimator with village fixed effect is a lower bound for β . Testing the hypothesis that microcredit is a poverty trap is equivalent to testing the one-sided hypothesis that $\beta < 0$. We reject the poverty trap hypothesis in every specification.

2. The bound implied by the instrumental variable estimator.

Consider equation (4),

$$(4) Y_{it} = \beta X_{it} + \alpha + \eta_{it},$$

Let the error term η_{it} be uncorrelated across villages so that instrumental variable estimator for β converges in probability to $\beta + \text{Cov}(Z_i, \eta_{it}) / \text{Cov}(Z_i, X_{it})$. Hypothesis 2 states that there are fewer opportunities in poorer villages. This implies that the error term η_{it} is negatively correlated with living in a poor village, Z_i , so that $\text{Cov}(Z_i, \eta_{it}) / \text{Cov}(Z_i,$

¹⁰ Note that the regressor is bounded and varies across individuals; therefore the law of large numbers applies.

X_{it}) is negative. This yields that the instrumental variable estimator for β gives a lower bound. This lower bound also shows the poverty trap hypothesis is rejected by the data.

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Tables

Table 1: Village fixed effects

	Net Worth (taka)	Income (taka)
Wave 1	20885 (8101)	4338 (3085)
Wave 1 (regressors)	17399 (7251)	3034 (3299)
Wave 2	17294 (12681)	5597 (1889)
Wave 2 (regressors)	8650 (12091)	4886 (1884)
Wave 3	39231 (62814)	13571 (7159)
Wave 3 (regressors)	21192 (57117)	13297 (7120)

Table 2: Village fixed effects: p-values for testing $\beta \leq 0$

	Net Worth (taka)	Income (taka)
Wave 1	0.0049675960	0.079838483
Wave 1 (regressors)	0.0082080426	0.17887193
Wave 2	0.086319577	0.0015235632
Wave 2 (regressors)	0.23717844	0.0047513563
Wave 3	0.26613052	0.029002786
Wave 3 (regressors)	0.35530838	0.030911980

Table 3: Instruments

	Net Worth (taka)	Income (taka)
Wave 1	40482 (11078)	9335 (6682)
Wave 1 (regressors)	35118 (8073)	7270 (5260)
Wave 2	-18965 (66177)	6497 (3208)
Wave 2 (regressors)	-33739 (60329)	5438 (2573)
Wave 3	97862 (104540)	9332 (19122)
Wave 3 (regressors)	65480 (89195)	8532 (19314)

Table 4: Instruments: p-values for testing $\beta \leq 0$

	Net Worth (taka)	Income (taka)
Wave 1	0.00012895751	0.081201250
Wave 1 (regressors)	6.8051465e-006	0.083466005
Wave 2	0.61278301	0.021420882
Wave 2 (regressors)	0.71200448	0.017279587
Wave 3	0.17460567	0.31276633
Wave 3 (regressors)	0.23143723	0.32933430