

# Working Paper No. 23

## Has Microcredit Helped the Rural Poor of Bangladesh? An Analytical Review of the Evidence So Far

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## Abstract

This paper reviews the evidence that has accumulated so far on the impact of microcredit on poverty in rural Bangladesh. The early studies on the impact of microcredit almost invariably found that microcredit had made a positive contribution not only in reducing poverty but also in a host of other economic and social dimensions. These studies soon came to be questioned, however, on the grounds of econometric methodology. It was argued, in particular, that various kinds of 'selection bias' vitiate their findings and lend an 'upward bias' to their estimates of the impact of microcredit. The possibility of 'upward bias' was especially damaging since it meant that the claim of a positive contribution of microcredit could no longer be credibly made. Later studies used sophisticated techniques to get rid of this bias; but while the earliest of this second generation studies continued to find positive contribution of microcredit, others soon began to question their findings, resulting in a prolonged and sometimes obscure debate on econometric methodology. Some of the critics even claimed to find no evidence for the impact of microcredit at all, even by using the same datasets as used by those who had claimed to find a positive impact. The review presented in this paper comes to the conclusion that the original finding about the positive contribution of microcredit survives this debate even though there might be some doubt about the precise magnitude of the impact. More importantly, as most of these debates centred around studies based on cross-section data, they have become mainly irrelevant with the emergence of third generation studies based on panel and quasi-panel data which are able to deal with the problem of selection bias much more satisfactorily. These studies confirm that microcredit has indeed made a positive contribution towards reducing poverty in rural Bangladesh. According to a conservative estimate, microcredit has helped reduce overall rural poverty by about 5 per cent and extreme poverty by about 10 per cent. Considering the borrower households alone, microcredit has helped roughly 1 in 10 borrowers to come out of poverty and 1 in 5 borrowers to come out of extreme poverty. Another way of looking at these numbers is that with the help of microcredit roughly about 2 per cent of borrowers have been able to climb out of poverty every year on the average. If these figures look less than spectacular, there is no reason to expect otherwise because, firstly, microcredit is just one intervention among many that have a bearing on poverty and, secondly, considering that many of the borrowers were at the bottom of the rung to begin with and that loan amounts are but a very small fraction of even poor household's total income one could not possibly have expected any significantly larger number coming out of poverty. There are good reasons, however, why these numbers should not be belittled either. In the first place, there is hardly any other intervention that has been able to bring 1 out of 10 beneficiaries out of poverty anywhere in the world. Secondly, the benefit of microcredit goes well beyond the number of people it manages to pull above the poverty line. The discourse on microcredit should move on. Instead of taking rigid positions on the efficacy of microcredit in general, the protagonists should focus attention on the details of how microcredit can be made more useful for the poor.



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# Has Microcredit Helped the Rural Poor of Bangladesh? An Analytical Review of the Evidence So Far

S. R. Osmani\*

## 1. Introduction

In the early days of microcredit it was taken as axiomatic that access to credit will bring economic benefits for the poor. This belief was based on the recognition that credit plays an important economic role by acting as a bridge between the present and the future. Costs of production are incurred in the present but returns will accrue only in the future. If the producer doesn't have enough capital to cover the costs, access to credit becomes essential in order to ensure that profitable activities can in fact be undertaken. Similarly, access to credit enables a household to protect its current level of consumption in the face of negative shocks by borrowing against future income and thus helps avoid excessive hardship in adverse circumstances. Poorer households have traditionally been deprived of these economic benefits of credit because the formal banking system was not willing to lend to them while village moneylenders, who might be willing to lend, charged exorbitant rates of interest. Faced with the credit constraint, poor people were thus less able to undertake potentially profitable economic activities and to avoid excessive fluctuations in consumption. It was, therefore, reasonable to expect that once they gained access to credit, they should be able to improve their economic well-being by earning more from productive activities and by being better able to smooth consumption over time.

The early studies of the impact of microcredit in Bangladesh seemed to confirm these expectations. The first systematic attempts to measure the impact of microcredit on the economic well-being of borrowers were made by Mahabub Hossain in the 1980s. Using household surveys as well as official records of the Grameen Bank, by far the largest provider of microcredit at the time, he assessed the impact of credit on the Bank's borrowers in two separate studies (Hossain, 1984, 1988). The main findings of the follow up study, carried out in 1985 using a survey of 280 households in 7 villages, five of which were programme villages and two were control, are also presented in Hossain (2002). The economic well-being of the borrowers was compared with two control groups – eligible non-borrowers from programme villages and eligible households from non-programme villages. The borrowers were found to fare better than both type of control groups in terms of most of the economic indicators the study looked at – viz., household income, extent of poverty, level of employment, indebtedness to village moneylenders, and value of accumulated assets. The obvious implication was that by softening the credit constraint faced by the poor, Grameen Bank had enabled the borrowers to engage more fully and more gainfully in economic activities and thereby helped raise their living standard.

In yet another early study of the impact of microcredit, Rahman and Khandker (1994) evaluated the impact of three major microcredit programmes in Bangladesh on the employment and productivity of the rural poor. The study was based on a survey that subsequently became famous around the world as the basis of a large number of studies on the impact of microcredit, which, as we shall see, generated excitement and controversy in almost equal measure. The survey was carried out by the Bangladesh Institute of Development Studies (BIDS) with the help of World Bank in 1991/92 covering nearly 1800 rural households served by microfinance programmes of the Grameen Bank,

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the Bangladesh Rural Advancement Committee (BRAC), and the Bangladesh Rural Development Board (BRDB). The sample also includes a control group of households in areas not served by any microfinance programmes. Following a methodology similar to Hossain's, Rahman and Khandker compared participants in programme villages with both non-participants from the same villages and eligible households from non-programme villages. The findings clearly demonstrated the positive impact of microcredit. All three credit programmes were found successful in expanding the opportunities for productive self-employment. In particular, access to credit was found to enable the poor borrowers to switch from low-paid wage employment to more remunerative self-employment.

These early studies soon came to be questioned, however, on methodological grounds. Doubts were raised about whether the methodologies typically employed in these studies were able to correctly identify the causal effect of credit on economic outcomes – a problem known in the econometric literature as the 'identification problem'. The present paper tries to explicate the nature of this methodological problem, discusses how subsequent studies have tried to deal with it, how successful they were in doing so, and as a result of these investigations what the current state of knowledge is on the impact of microcredit in rural Bangladesh. Section 2 attempts to offer an intuitive explanation of what are rather complex methodological challenges. The next three sections review most of the important studies that have examined the impact of microcredit in Bangladesh, with the twin focus on what their salient findings are and how they dealt with the methodological issues discussed in section 2. The three review sections are divided according to the nature of data used by the studies – namely, cross-section data (section 3), panel data (section 4) and quasi-panel data (section 5). Finally, section 6 summarises the findings and offers some concluding observations.

## 2. Methodological Challenges

The primary methodological challenge in evaluating the impact of any intervention, including microcredit, is to identify the 'causal' effect of the intervention. In the simplest case, the causal effect can be easily 'identified' – i.e., the inference that an explanatory variable (in this case, credit) actually 'caused' the outcome (in this case, higher economic well-being of the borrowers) can be deemed to be valid – if the variable has a property called 'exogeneity'. This property is said to exist and the explanatory variable is defined as 'exogenous' when either (a) it has no correlation with any of the other possible explanatory variables that may also affect the relevant outcome, or, (b) if such correlated explanatory variables exist, they have all been included in the analysis so that their effects on the outcome can be separated out before estimating the impact of the variable in question. If these conditions are not met the explanatory variable in question is deemed to be 'endogenous' as opposed to 'exogenous'. Impact assessment of such endogenous explanatory variables is fraught with a serious problem – the so-called 'endogeneity' problem. The problem is that the measured impact of an endogenous variable will capture not only its own causal impact, if there is any, but also the impact of the excluded variables with which it is correlated. In that case, the true causal impact will not be 'identified' – the measured impact will be a 'biased' estimate of the true impact, unless measures are taken to eliminate the bias. Doubters of the methodologies used in the early studies of microcredit's impact argue that there are good reasons to suspect that the measured impacts of credit could suffer from such endogeneity bias. They further argue that there are reasons to believe that the bias is likely to be 'upward' i.e., the measured impact is likely to be greater than the true impact, if there is any, which means we might find a positive impact even when there is none. As a result, they conclude, the inference that microcredit has led to higher living standards of the borrowers cannot be trusted.

The endogeneity bias may take various forms. The most common, in the context of microcredit, is perceived to be the problem 'selection bias', which in turn may arise from different sources. For example, the MFIs may deliberately select the better off among the poor as their clients so



as to ensure a better chance of their loans being repaid. In that case, the evidence of higher income of the borrowers compared to the non-borrowers cannot be interpreted as evidence for the beneficial effect of credit because it may simply reflect MFIs' selection policy. Alternatively, individuals with better entrepreneurial ability may 'self-select' themselves into the MFI's programme while those with lesser ability self-select out of it.<sup>1</sup> Once again, higher income of borrowers cannot be interpreted as being caused by credit because the real cause may lie in unobserved differential abilities of borrowers and non-borrowers. In either case, the causal effect of credit is not 'identified'.

Yet another type of selection bias may arise because of the preferences of the MFIs regarding programme placement. They may deliberately select villages of certain types – for example, relatively better off villages – so that they can insure against possible loss by offering credit to 'safer' borrowers. If any such systematic difference between programme and control villages is not properly accounted for, then a straightforward attribution of the income gap between participants and non-participants to the effect of credit will be a mistake, because at least a part of the reason for the gap may lie in the type of villages in which the two groups of people happen to live. The estimate of the effect of credit will then be subject to what is known as the 'programme placement bias'. Once again, the causal effect of credit will not be 'identified'.

Moreover, in all these cases, if selection indeed occurs in the manner described, the resulting 'selection bias' will be 'upward' – i.e., the effect on income will be inflated as a result of MFIs selecting the better off clients or more prosperous villages, and more able clients self-selecting themselves into the programme. In consequence, the inference that access to credit has led to higher income will not be credible.<sup>2</sup>

The problem here is that the effect of credit is getting mixed up with the effect of the selection process. This means that the effect of credit could have been identified if somehow the selection process could be incorporated in the assessment exercise, in effect treating it as one of the explanatory variables, so that its effect could be separated out leaving only the pure effect of credit. But the problem is that it may not always be possible to include the selection process explicitly in the analysis – for example, when the clients self-select themselves on the basis of some unobservable characteristics (such as entrepreneurial ability), because by definition one cannot analyse what is unobservable. In that case, the credit variable will necessarily be endogenous owing to its correlation with the excluded variable, the 'selection process', which too has a bearing on the observed outcome. The identification problem emanates from the fact that at least part of the variation in the credit variable (for example, some individual gets credit and some doesn't) is correlated with the 'excluded' selection process. As a result of this correlation, when we find that variation in an outcome, such as income, is associated with variation in credit, we cannot be sure whether the observed variation in outcome is being caused by the observed variation in credit or by the correlated but hidden variation coming from the selection process.

This way of looking at the problem suggests a possible way out of it. The fact that the hidden selection process causes some variation in the credit variable is what makes this variable endogenous. But this does not mean that the entire variation in credit must be endogenous;

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<sup>1</sup> There is some evidence to suggest the possibility of such self-selection. For example, according to Hashemi (1997), nearly half of all non-participants in a region served by Grameen and BRAC indicated that they did not borrow for fear that they would not be able to generate high enough returns to be able to repay loans.

<sup>2</sup> The presumption of an 'upward' bias is a crucial element in this argument. For, even if the causal effect of credit could not be 'identified' but there were reasons to believe that any endogeneity bias can only be 'downward', the evidence of higher income of borrowers could still be credibly interpreted as a positive impact of credit because the true impact in this case is greater than the measured impact. The only problem would be that the exact magnitude of the positive impact could not be 'identified'.

there may be some part of the variation which is its 'own', so to speak, in the sense that it does not reflect variation caused by the hidden selection process or by any other excluded variable. This 'own' part of the variation in credit can be described as an 'exogenous' variation. If somehow the extent of this exogenous variation in credit could be identified, one could try to see how much variation in the outcome variable is associated with it; this would then enable us to measure the true impact of credit – i.e., the effect of credit would be identified despite its endogeneity. This is precisely what analysts try to do when they look for an 'identification strategy' that would permit drawing causal inference about an endogenous explanatory variable.

There are various ways of extracting this 'exogenous variation' in the endogenous variable. Correspondingly, there could be many different identification strategies, some being more effective than others depending on the nature of the data at hand.<sup>3</sup> As we noted, the early studies of the impact of microcredit did not in general address the issue of selection bias and as such did not concern themselves with identification strategies. But subsequent studies have been much more conscious of the problem and the search for an appropriate identification strategy has been at the heart of their enquiry. Indeed one sometimes gets the feeling that the methodological concern with identification has been all-consuming, often superseding the concern with substance. We can classify the subsequent studies into several groups depending on the nature of data they used, since as noted above the nature of identification strategy depends crucially on the nature of data. The first group is based on cross-section data in which a sample of households is surveyed at a point in time. The second group contains studies that use longitudinal or panel data i.e., repeated surveys of the same sample of households at more than one point in time. The third category of data may be described as quasi-panel in nature, which is essentially cross-section data but enriched by information about the past so that some sort of longitudinal analysis can be applied.<sup>4</sup>

### 3. Studies Based on Cross-Section Data

An early cross-sectional study that was alive to the methodological problem of causal identification is Zaman (1999). Using a dataset of 547 BRAC borrowers and 525 control households in ten villages in Matlab, the study tried to address the problem of self-selection by applying appropriate econometric technique and came up with two interesting findings. First, access to micro-credit does not reduce poverty for all borrowers – it does so only when cumulative credit taken over the years reaches a certain critical minimum threshold. What seems to matter here, however, is not so much the amount of credit but the length of experience of long-term borrowers. Accumulated experience seems to enable the borrowers to switch from traditional low-risk low-return on-farm

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<sup>3</sup> In some cases, the nature of data may not permit any identification strategy at all, either because there is not enough exogenous variation in the endogenous variable in question or there is not enough information in the data to extract that variation even if it exists. In other cases, the possibility of finding a suitable strategy is limited only by the imagination and econometric skills of the analyst.

<sup>4</sup> There is a possible fourth type of data, called experimental data, generated by randomized control trials (RCTs). Although this type of data is becoming increasingly popular around the world, for social impact assessment in general and assessment of the impact of microcredit in particular, they have yet to be used for assessing the impact of microcredit in Bangladesh. This omission is an important issue and we shall comment on it later.

activities to higher-risk higher-return off-farm activities over time and thereby to attain a higher income level.<sup>5</sup> Second, in addition to reducing poverty, access to credit also reduces the vulnerability to poverty, defined in the sense of the likelihood of remaining or becoming poor in the future. Zaman did not, however, measure vulnerability directly. Instead, he looked at a number of possible determinants of vulnerability – namely, (a) crisis-coping mechanisms such as additional loans, staggered repayment, etc. during crises, (b) asset building, and (c) women’s empowerment – and found that credit affected those determinants in ways that should reduce vulnerability.

Valuable as these findings are, their credibility remains in doubt despite the care the author took to deal with the identification problem. He used the standard instrumental variable approach for this purpose. For this method to work, however, one needs to find proper ‘instruments’ that can extract exogenous variation from within the endogenous variables. These instruments are variables that have a significant bearing on a household’s decision to participate in the credit programme but are not directly related to the outcome variable of interest (say, income or poverty). Such instruments are very difficult to find in practice because almost everything that affects the decision to take credit is also likely to affect outcome variables directly. By the author’s own admission, his instruments were not very ‘robust’, which means that doubts remain as to the extent to which any possible selection bias has actually been removed. This failure to find ‘robust’ instruments has been an Achilles’ heel of impact assessments of all kinds.

In a series of papers, Mark Pitt and Shahidur Khandker have tried to address this problem by adopting an innovative method to get rid of selection bias, using the same BIDS/World Bank data set that was used by Rahman and Khandker (1994) referred to earlier. In their most celebrated paper, Pitt and Khandker (1998), tried to assess the impact of microcredit on indicators of household well-being such as expenditure and assets, differentiated by gender. They found that loans taken by females had a strong positive effect on the well-being of the borrowing households but loans taken by males did not. Since women constituted the majority of borrowers, the overall effect was judged to be positive.<sup>6</sup>

The magnitude of the effects found by Pitt and Khandker was quite large. For example, total household expenditure was found to increase by 18 taka for every 100 additional taka borrowed by women, the corresponding figure for men being 11 taka for every 100 taka of additional loan. The facts that the measured impact was so large, that it was estimated by an apparently sophisticated method of dealing with the identification problem, and that the findings seemed to vindicate the MFIs’ decision to target credit mainly to women, all combined to elevate the paper

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<sup>5</sup> The evidence on delayed impact of credit is supported by Montgomery *et al.* (1996) who found sharp growth in productive assets for third time borrowers compared to first time borrowers. This finding is in contrast with that of Hossain (2002), however, who found that new borrowers were more likely to accumulate productive assets compared to the older ones. He speculated that in the initial years, when the level of capital is low and marginal productivity is high, it makes sense to accumulate in productive lines, while declining rate of return to increasing volume of capital induces older loanees to move towards alternative uses such as social investments (e.g., housing, education, sanitation) as well as conspicuous consumption. Two different processes thus seem to underpin these contrasting findings. For Zaman and Montgomery *et al.*, the driving force behind the delayed effect on productive asset accumulation is the experience and expertise that comes from learning-by-doing. In contrast, the underlying cause behind the fading effect found by Hossain is diminishing marginal productivity of capital. Both arguments seem plausible; the issue is, therefore, mainly empirical. Whether one observes delayed or fading effect in practice would depend on the relative strengths of learning-by-doing on the one hand and diminishing marginal productivity on the other.

<sup>6</sup> In a set of related papers, the authors also explored the impact of microcredit on various other dimensions of household well-being and behaviour, using the same methodology and the same data set; see, for example, Khandker (1998) and Pitt *et al.* (1999, 2003, 2006).

to a cult status in the world community of practitioners and academics involved with microfinance.<sup>7</sup> It has remained the most frequently cited academic paper on microfinance to this day.

From the very beginning, however, there were also some rumblings of discontent within the academia, articulated most strongly in an unpublished note by Morduch (1998). He raised a number of concerns with the paper, the most important being the validity of the identification strategy employed. Pitt (1999) soon came out with a robust response, also in an unpublished note. And there the matter seemed to rest for nearly a decade, until the debate was resurrected in 2009 in a working paper which Jonathan Morduch co-authored with David Roodman (Roodman and Morduch 2009). This led to a series of responses and counter-responses and counter-counter-responses – principally between David Roodman and Mark Pitt, with their respective co-authors taking essentially a back seat. Just at the time this new round of debate was coming to a boil, a parallel debate cropped up between Mark Pitt on the one hand and Maren Duvendack and Richard Palmer-Jones on the other. A great deal of the contents of these two debates, especially the first, are too technical to go into here; also many of the claims and counter-claims have become redundant after being refuted during the course of the debate. The more important thing to assess is where the debate stands now, especially what remains of the original findings of Pitt and Khandker after all this scrutiny. For this, however, we need to go back to the origin of debate.

At the heart of the controversy lies the nature of the identification strategy employed by Pitt and Khandker. Although the matter is intensely technical, it is not a just a matter of technical detail, because as we explained earlier the very credibility of the substantive finding on the impact of credit depends crucially on the validity of the identification strategy used for dealing with the problem of selection bias. One of the reasons the controversy arose in the first place and persisted for so long is that Pitt and Khandker had to devise a very unusual econometric technique of estimation in view of some very unique features of the data at hand and the identification strategy implicit in this novel technique was not at all transparent. The matter was further complicated by two other factors. First, the nature of data precluded the possibility of using standard statistical procedures for testing the validity of instruments employed for the purpose of identification. As a result, the arguments both for and against the strategy had to rely on analogues and a priori reasoning, neither of which was conducive for resolving disagreements conclusively. Second, the particular analogue that Pitt and Khandker themselves used to illustrate the intuition behind their identification strategy, although seemed very illuminating at the beginning, turned out to be rather misleading in the end, driving their critics needlessly in wrong directions and fuelling futile controversies.

Pitt and Khandker's explanation of the intuition behind their identification strategy is closely related to an econometric technique known as the regression discontinuity method. To see the logic behind it, consider the fact that most MFIs employ some kind of eligibility rule to decide whom to offer credit. The most commonly applied rule in Bangladesh is that a household should own no more than half an acre of cultivable land. Starting from zero ownership, all those who have land up to the cut-off point are deemed eligible, but as soon as the cut-off point is crossed, the household becomes ineligible. That's a discontinuity, which may be unfortunate for a household marginally above the cut-off point who is eager to take loan, but it is a fortunate twist for an analyst looking for an identification strategy. If one compares the households just below the cut-off line (the eligible group) with those just above (the ineligible group), it is reasonable to argue that as a whole the two groups should not be fundamentally different from each other because all that separates them is a few decimals of land. In particular, there is no reason to suppose that the eligible group has superior unobservable characteristics such as entrepreneurial

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<sup>7</sup> Professor Yunus' oft-quoted remark that microcredit lifts 5 per cent of borrowers out of poverty every year in rural Bangladesh is widely believed to be based on these findings.

ability compared to the ineligible group because such differences have no bearing on who is eligible and who is not. Eligibility is determined by an external rule, not by the choice of the households. Who among the eligible group would eventually take the loan is of course a matter of choice and that choice may well be influenced by their unobserved characteristics, but eligibility is independent of choice. To put it differently, households may self-select themselves into the borrower category but they cannot self-select into the eligible category. Thus a comparison between eligible and non-eligible groups, as distinct from a comparison between borrower and non-borrower groups, should not be vitiated by any kind of selection bias. And since, as noted above, households just above and just below the cut-off point can be expected on the average to be very similar in all respects other than the fact that one group can take credit and the other cannot, any observed difference between them in the outcome variable can be correctly identified as the causal effect of credit.

In his initial criticism, Morduch (1998) accepted this logic of identification but argued that the logic does not apply to the Pitt-Khandker study as the eligibility rule was not strictly enforced by the MFIs – many households above the cut-off point were also offered loan. He then modified the methodology of estimation to account for this deviation from the official eligibility rule, and came out with the startling finding that the Pitt-Khandker results no longer hold – i.e., credit does not appear to have any significant impact on household expenditure. Pitt (1999), however, cast doubt on this counter-finding by pointing out various errors of factual and logical nature in Morduch's study.

In the absence of further response from Morduch, the Pitt-Khandker conclusion about the efficacy of microcredit continued to hold sway for nearly a decade until it was jolted again by Roodman and Morduch (2009). This time the line of attack was different. The authors claimed to show that even if one eschewed the modified methodology Morduch had applied in his original foray, and used instead the methodology applied by Pitt and Khandker themselves but applied it properly, the main Pitt-Khandker finding again disappeared – credit seemed to have no significant effect on household expenditure. This was a much stronger claim than the one made earlier in Morduch (1998) as it was purportedly based on the same methodology as the one used by Pitt and Khandker themselves. But once again, Pitt (2011a) came back strongly in self-defense pointing out a litany of errors in the Roodman-Morduch study and showing how the original findings still survived the new critique.

Then followed a series of exchanges of rather obscure nature, in the course of which Roodman repeatedly came up with new arguments and was repeatedly mowed down by Pitt, only to resurface with even newer arguments.<sup>8</sup> Eventually, Roodman and Morduch had to concede that the original Pitt-Khandker findings could not be overturned but continued to profess scepticism on the grounds that they were still not convinced about the validity of Pitt and Khandker's identification strategy (Roodman and Morduch, 2013).

Meanwhile, a new debate has emerged in which Davendack and Palmer-Jones have locked horns with Mark Pitt, although it started as a proxy war. The immediate target of attack by Davendack and Palmer-Jones (2012a) was not the Pitt-Khandker study but a study by Chemin (2008) who tried to replicate the findings of Pitt and Khandker (1998) by applying a new methodology to the same data. Chemin used the technique of Propensity Score Matching (PSM) to handle the problem of identification arising from selection bias. Since the essence of the selection problem is that households in the programme and control groups may be very different because of the selection process through which the programme group is formed, PSM seeks to remove the problem by creating a control group which is as similar to the programme group as possible.

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<sup>8</sup> For a blow by blow account of this prolonged debate, the interested reader may read the papers and blogs below in the order given: Pitt and Khandker (1998), Morduch (1998), Pitt (1999), Roodman and Morduch (2009), Pitt (2011a), Roodman (2011a), Pitt (2011b), Roodman (2011b), Roodman and Morduch (2011a), Roodman and Morduch (2011b), Roodman (2011c), Pitt and Khandker (2012), Roodman (2012) and Roodman and Morduch (2013).

The technique first tries to estimate how likely each member of the control group would have been to ask for credit if they had a choice to do so and assigns a 'propensity score' to them depending on the strength of this likelihood. Similar scores are also estimated for the members of the project group. The technique then matches members of the two groups who have similar scores; those with no matches are left out. Since the members of the two groups thus created have similar probability of asking for credit, any possible difference arising from the selection process is deemed to have been purged. The only remaining difference is that one group does participate in the credit programme and the other does not. As a result, the observed difference in the outcome variables of the two matched groups can be interpreted as the causal effect of credit, once other factors that may also affect the outcome variable have been controlled for.

The estimates that Chemin obtained by using this technique confirmed the Pitt-Khandker finding that credit has a significantly positive effect on household expenditure, although the magnitude of the effect he found was somewhat smaller than what Pitt and Khandker had obtained. Chemin thus offered a vindication of Pitt and Khandker's central message regarding the effectiveness of microcredit, even if not of the magnitude of the effect, by applying a different statistical technique on the same dataset. Davendack and Palmer-Jones (2012) challenged this vindication, and by implication challenged the original finding of Pitt and Khandker as well. They claimed to show that if the PSM technique was properly applied to the same data, the positive effect of credit seemed no longer to exist – a very similar type of claim that Roodman and Morduch made following a very different route.

The specific criticism they made of Chemin's work was directed at a well-known limitation of the PSM technique, which according to them, Chemin did not pay adequate attention to. When PSM tries to create two groups that are similar in terms of the probability of participating in a credit programme, it estimates the probability on the basis of the observed characteristics of the sample households or individuals. But unobserved characteristics may also have a bearing on the probability of participation – for example, those with a more entrepreneurial bent of mind may want to participate more. One can, therefore, never be certain that PSM has obtained a genuine matching. Any such mismatch would mean that the selection bias may not have been removed. This is because the same unobserved characteristics that affect the probability of participation may also affect the outcome variable – for example, those with greater entrepreneurial spirit may want to participate more in the credit programme and may also earn higher income because of that spirit. In other words, mismatch in terms of propensity score may also entail mismatch in the ability to earn higher income, with or without credit. Comparison of the two mismatched groups will then confound the effect of credit with the effect of differential ability. As a result, PSM will fail to identify the true effect of credit.

This is an inherent limitation of PSM since unobservable characteristics cannot by definition be taken note of while estimating the propensity scores. Econometricians have, however, devised various kinds of sensitivity tests to see how badly the results are affected under alternative assumptions about the underlying distribution of unobserved characteristics. The confidence in the results will depend on how robust they are to these tests. This is precisely what Davendack and Palmer-Jones did, subjecting Chemin's PSM estimates to the sensitivity tests, and found that under most of the alternative assumptions the credit variable turned out to be statistically insignificant. The most reasonable inference, they concluded, was that credit had no significant effect on household expenditure, contrary to what Pitt and Khandker had found and Chemin had confirmed.

Both Pitt (2012) and Chemin (2012) then responded in self-defense, with quite devastating effect. They demonstrated that in carrying out their sensitivity analysis Davendack and Palmer-Jones had made a series of errors of both factual and statistical nature, and that the combined effect of these errors was serious enough to destroy the credibility of their analysis. In response, much like Roodman and Murdoch did, Davendack and Palmer-Jones (2012b,

2012c, 2012d) also conceded that they could no longer claim to prove that the Pitt-Khandker-Chemin finding on the efficacy of microcredit was wrong, but they continued to maintain their scepticism about its validity on the grounds that it was not subjected to adequate sensitivity analysis.

Whether Roodman-Morduch and Davendack-Palmer-Jones are justified in holding on to their skepticism is an issue that, in our view, cannot be resolved on a scientific basis, because the arguments at this stage seem to belong more to the realm of faith than of scientific discourse. It is reasonable to argue, however, that even from a purely objective point of view not all the qualms about the Pitt-Khandker results have been fully resolved.

The first qualm pertains, not surprisingly, to the identification strategy. When Pitt and Khandker (1998) first explained their strategy they pointed out two kinds of discontinuities which could potentially be exploited for the purpose of identification. The first kind was related to the 'half an acre' eligibility rule, and we have explained above how the discontinuity generated by this rule helps achieve identification. The second kind of discontinuity emerged from the facts that borrower groups were segregated by gender and that not all villages were offered groups of both types – some were offered to only male groups, some only to female groups and some to both. This meant that whether individuals could borrow or not did not depend initially on their choice – it depended on whether a particular gender group was allowed in a particular village or not. As in the case of 'half an acre' rule, eligibility was determined by an extraneous decision by the MFIs, in which household's choice did not play any role. This implies that if one compares, for example, two groups of females – namely, eligible females in a village where 'female' groups were allowed and females in another village where only male groups were allowed – one should not expect to find any systematic differences in their unobserved characteristics because these characteristics did not play any role in separating the two groups in the first place. Therefore, any observed difference between their outcome variables cannot be attributed even in part to differences in unobserved characteristics. This is just another way of saying that there is no scope of self-selection bias creeping in here. As a result, any difference in the outcome variables between these two groups will correctly identify the effect of credit, once the effects of any observed differences between them have been adequately controlled for.

Of the two eligibility rules – one related to the 'half an acre' cut-off point and the other to gender-specific programme placement – it is the former that Pitt and Khandker (1998) used to illustrate the intuition behind their identification strategy. In consequence, all the ensuing debate focused almost exclusively on it, and the other rule receded into the background. However, after all the criticisms and doubts that have been heaped by Roodman and Morduch on their identification strategy, Pitt and Khandker (2012) now maintain that these doubts are irrelevant anyway because they do not need the 'half an acre' rule to obtain identification – the other discontinuity, offered by gender-specific programme placement, is enough for this purpose.

This may sound like a plausible argument, except that Pitt and Khandker did not seem so confident about it the first time around. In their original paper, they first noted that even though programme placement was not influenced by households' choice, it could be influenced by the outcome variable of interest – for example, the MFIs may choose villages with higher average income. In that case, any estimation strategy that relies on comparison across villages could be biased, even after allowing for village-level fixed effects. They concluded that "...without further exogenous variation in program availability, the credit effect is not identifiable ..." and went on to suggest that this additional variation is provided by the exogenous rule that "...households owning more than half-acre of land are precluded from joining any of the three credit programs." (Pitt and Khandker, 1998: p.976) This line of argument is ostensibly at odds with the authors' current position. Since the identification strategy based on gender-specific programme placement must necessarily involve comparison across villages, and since according to the quote above such comparison cannot achieve identification without the additional exogenous variation afforded by the half-acre rule, it is not altogether clear exactly how the land-based rule can now be dispensed

with and identification be achieved solely from gender-specific placement as claimed by Pitt and Khandker (2012). Perhaps the two positions can be reconciled but we are not told how.

The second qualm emanates from a startling finding in Roodman and Morduch (2013) on the role of outliers. The authors demonstrate that the positive impact of credit on household expenditure that Pitt and Khandker find actually stems from the behaviour of a handful of borrowers who spend a disproportionately large amount of money. If only 14 of these big spenders are removed from the sample, the effect of credit disappears! Roodman and Morduch use this finding to support their scepticism about the robustness of Pitt and Khandker's identification strategy, but this line of attack seems specious. The problem of outlier is certainly a sign of fragility of the findings, but it may have nothing to do with identification.<sup>9</sup> The more genuine concern pertains to the fragility of the estimates itself – how much credence can one give to the estimates which cannot survive the withdrawal of a handful of outliers?<sup>10</sup>

The third qualm relates to an issue of specification that seems to have been overlooked in the literature so far.<sup>11</sup> The credit variable used in Pitt and Khandker (1998) is 'cumulative' loan, as distinct from current loan, whereas the outcome variable is 'current' household expenditure. This juxtaposition between cumulative and current values has some odd implications. Suppose person X has borrowed Tk. 1000 every year for 10 years so that her cumulative loan is Tk. 10,000, whereas person Y has borrowed Tk. 10,000 in the current year but nothing before so that for her cumulative loan is also Tk. 10,000. The specification implies that credit will have the same impact on both persons' household expenditure in the current year, even though the former's current loan is only one-tenth of the latter's! This is patently absurd. Consider, for example, the scenario where person X uses loan of Tk. 1000 every year as working capital, and earns an additional annual income of Taka T. If other things remain the same, she will keep on earning this extra income T every year. By the tenth year, her extra income, generated by credit, is, however, still T, not 10T because by definition income (which is a flow concept) does not accumulate. By contrast, person Y, who uses Tk. 10,000 for working capital in the current year, is likely to earn an extra income of 10T (or something close to it depending on scale economies). Clearly, the impact on current expenditure in the two cases cannot be even remotely similar. Of course, working capital is not the only possible pathway through which loan can affect household expenditure; but whatever pathway one considers, it is difficult to see how the two effects can be of similar order of magnitude.

While these criticisms do not necessarily nullify the central message of Pitt and Khandker that microcredit has a positive impact on the economic condition of the poor, they do call into question the accuracy of the magnitude of the effect they claimed to find. Chemin's study, which confirms Pitt and Khandker's central message but also at the same time points to a smaller size of the effect, serves to strengthen the concern with magnitudes. As a result, bold inferences such as the one that claims that 5 per cent of the borrowers climb out of poverty every year in rural Bangladesh ought to be eschewed.

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<sup>9</sup> The switch from statistical significance in the full sample to insignificance in the sub-sample obtained on deletion of the offending outliers is not necessarily an indication of weak identification. It is possible that the significant estimates for the full sample and the insignificant estimates for the sub-sample are both robustly identified for their respective samples. There seems to be some confusion here between robustness of estimates and robustness of identification. Un-robust identification would certainly lead to un-robust estimates, but the converse implication is not necessarily true – un-robust estimates do not necessarily imply un-robust identification since they may stem from other sources (in this case, gross outliers).

<sup>10</sup> It may be worth noting that in their detailed critique of Roodman and Murdoch, Pitt and Khandker (2012) do not refer to this problem of outliers, even though they attempt rebuttals of almost every other point of criticism.

<sup>11</sup> I am grateful to Wahiduddin Mahmud for pointing out this issue to me.



At the same time, one has to recognize the brilliance of the Pitt-Khandker study in extending the frontier of research based on cross-section data. Their work shows how far one can possibly go towards extracting causal inferences from cross-section data by using imagination and sophistication in the use of econometric techniques. Any remaining limitation is possibly a reflection of the inherent limitations of cross-section data itself. In order to make firmer causal inferences, it is necessary to move on to different kinds of data.

#### 4. Studies Based on Panel Data

The use of panel data can offer a way of resolving the problem of identification in a more satisfactory manner than cross-section data ever can, even though it does not guarantee success in all circumstances. The fact that data are available for the same households (or individuals as the case may be) enables the analyst to compare the programme and control groups in terms of the difference in outcomes between two or more points in time – instead of comparing them in terms of the levels of outcome as with cross-section data. For instance, one would ask: by how much did the income of the two groups change between two points in time, instead of asking what were their income levels at a particular point in time? By looking at the change rather than the level of data, the analyst can get rid of the effects of unobserved characteristics, provided those characteristics can be assumed to remain constant over time, because these effects will then cancel out in the process of calculating change. This is known as the difference-in-difference (DD) method, or more generally the fixed effect (FE) method, of eliminating any selection bias that may arise from unobserved (but fixed) characteristics at the household or village level.

If there are reasons to believe that unobserved characteristics may change instead of remaining constant over time, the FE/DD method needs adjustment. There are several ways of doing so – for example, (a) while applying the FE method one can control for some initial observed characteristics that may have a bearing on how the relevant unobserved characteristics might change, (b) DD may be supplemented by the matching method described earlier (DD-PSM), or (c) one can go one step further by taking difference-in-difference-in-difference (DDD). Different methods are appropriate for different types of change in the unobserved characteristics. Since change can happen in a variety of ways, some of which might be unpredictable, one can never guarantee that the selection bias has been entirely eliminated by any of these methods. But by using alternative methods, and carrying out various sensitivity tests, it may be possible to get as close to the truth as non-experimental data will ever permit. As a result, when panel data analysis is carried out with due diligence, the estimates of programme effects obtained from them can be interpreted as a causal effect with a great deal more confidence than in the case of cross-section studies.

All the major panel studies that have been carried out to measure the impact of credit in Bangladesh have been reasonably conscious of the need for due diligence while applying panel data techniques. Accordingly, their findings can be accepted with a high degree of confidence. There are currently three main panel data sets available in Bangladesh that can be, and have been, used for the purpose of estimating the effect of microcredit. These are: (1) four rounds of surveys sponsored by PKSF covering the period from 1997-2005, and the studies based on it include Razzaque (2010), Islam (2011) and Imai and Azam (2012); (b) three rounds of surveys in 1991/92 to 1998/99 and 2011, the first two rounds of which were carried out jointly by BIDS and the World Bank, and the third was carried out jointly by the Institute of Microfinance and the World Bank; the studies include Khandker (2005), and Khandker and Samad (2012, 2013); and (c) several rounds of surveys carried out by BRAC to assess the impact of its ultra-poor programme; the most carefully conducted study based on this data set is Emran *et al.* (2009).

Using the PKSf panel data, Razzaque (2010) finds that the borrowing households had higher per capita income and lower probability of falling into poverty relative to comparable non-borrowers, after adjusting for unobserved heterogeneity and observable factors through appropriate panel data methods. Since the borrowing households included a substantial number of non-eligible i.e., relatively better off, households, the author wanted to test whether this result is driven mainly by the performance of these better-off households. For this purpose, he redid the estimation after dropping the non-eligible borrowers and the results remained essentially the same. This suggested that the benefits of credit were not cornered by the relatively better off borrowers; the positive impact accrued to the intended target group as well. Direct tests on the impact on the extreme poor confirmed that the benefit of credit reached also the poorest among the target group.

Several other results obtained by Razzaque are also worthy of note. First, access to credit benefits male and female borrowers differentially; for instance, longer participation in the credit programme confers additional benefit to females but not males. Second, cumulative household borrowing has a significantly positive effect on the accumulation of household assets, which indicates that the benefit of credit extends beyond the period of borrowing. Third, in contrast to the case of per capita income, the length of programme participation by both males and females positively and significantly influence the rate of asset accumulation. However, the effect of female participation on assets is more than double the comparable effect obtained for the entire sample, once again indicating that the effect of credit is differentiated by gender.

Islam (2011) used the PKSf data with the primary objective of distinguishing between the short-term and the long-term effects of credit. For this purpose, he compared the effects on borrowers who remained programme members continuously for the eight years covered in the study, on new borrowers who joined later and on the drop-outs. He used both the standard DD method and the more refined DDD method to allow for the possibility that programme and control villages may have been affected differentially by unobserved shocks. The magnitudes of the coefficients obtained under the two methods varied, often quite substantially, but the qualitative results i.e., the direction and the significance of the results remained the same.

The effect of credit was measured on three outcome variables – food consumption, self-employment income and assets. The main qualitative results may be summarised as follows. The major finding is that while the effect of credit on all three outcome variables is significantly positive, the strength of the effect is stronger in the longer run. This is evident firstly from the fact that the size of the coefficient of the credit variable is bigger for continuing borrowers compared to the newcomers, and secondly from the fact that even the dropouts seemed to be better off than the non-borrowers. The latter result indicates that the benefit of credit does not cease to exist when a borrower stops borrowing – the benefit continues to flow at least for a while.<sup>12</sup>

Islam draws two major conclusions from his findings – one methodological, one substantive. The methodological conclusion is that conventional programme evaluations that are based on the outcomes reported by continuing participants may underestimate the contribution of microcredit programme (because the continued effect on the drop-outs will be missed), and short-term treatment data in a microcredit programme may not provide a reliable estimate of the overall impact of the programme (because the extent of benefit tends to rise with the length of participation). The substantive conclusion is that graduation from poverty using microcredit requires longer-term participation, for it takes time for household entrepreneurs to achieve productive efficiency or to generate higher returns from self-employment.

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<sup>12</sup> The paper did not try to find out for how long the benefit continues to accrue once a person stops borrowing.

Imai and Azam (2012) also used the PKSf data to measure the impact of credit on two indicators of household well-being – namely, income and food consumption. In both cases, credit was found to have a significantly positive effect. However, the magnitude of the effect was found to be quite small. For example, doubling of loan raised per capita income by only about 0.5%, and raised per capita food consumption by 0.7% to 1%, depending on the estimation method used.<sup>13</sup>

A novel feature of this study is that it makes a distinction between productive and non-productive loans, and tries to assess whether the impact of credit varies by the type of loan. Not surprisingly, it finds that only productive loan is able to raise income. On the other hand, productive loan does not seem to have any impact on food consumption; only non-productive loan is able to raise it. The finding that productive loan raises income but not consumption may seem surprising at the first sight, but it is consistent with a great deal of anecdotal evidence which shows that borrowers often tighten their belt – sometimes consuming even less than before for the time being – in order to repay the loan out of additional income. In essence, what the productive borrowers seem to be doing here is an inter-temporal trade-off – consuming less or at least no more today in the hope of consuming more tomorrow out of the higher income generated with the help of credit. This is exactly opposite of the inter-temporal trade-off that non-productive borrowers engage in – consuming more today with the help of credit but reducing the level of consumption later. This latter type of behaviour – known as consumption smoothing – is entirely rational when households experience some urgent need to raise expenditure temporarily or are faced with a temporary shortfall of income.

The study by Emran *et al.* (2009) is part of BRAC's regular evaluation of its on-going Targeting the Ultra-Poor (TUP) programme, in which access to credit is combined with, and in some respects preceded by, other forms of support for the ultra-poor.<sup>14</sup> The panel consists of about 5000 households surveyed at two points in time – 2002 and 2005. The authors used both DD and DD-PSM techniques described above and carried out a number of sensitivity tests to the robustness of the estimates under alternative assumptions about time-variant unobservables. The evidence from the DD and DD-PSM approaches shows that there is significant positive effect of participation in the programme on net income, food security, and several indicators of assets such as quality of housing, household durables, and ownership of livestock. More refined analysis shows that programme benefits are not shared equally by all groups; in particular, the poorest 20% of the households benefit less in absolute terms compared to the top 20%.

Khandker and Samad (2012, 2013) have made extensive use of the three-period panel data (1991/92, 1998/99 and 2011) generated by the World Bank first in collaboration with BIDS and then with the Institute of Microfinance.<sup>15</sup> Based as they are on a source of data that covers the longest period (two decades) among all the panel data available in Bangladesh, these studies are particularly valuable in shedding light on the long term impact of microcredit on the living conditions of the poor. Like the preceding studies, they too employ different variants of panel data techniques and use sensitivity analysis to check for robustness of the estimates.

The starting point of the analysis in Khandker and Samad (2012) is the hypothesis that a critical factor for an assessment of a programme such as microcredit is the duration of programme intervention. They argue that unlike programmes such as conditional cash transfers (CCTs) which benefit the participants within a short period of time, microcredit takes time to have

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<sup>13</sup> The authors report that some of the significant results obtained through FE and refined FE methods seem to disappear when the DD-PSM method is used, but they attribute it to the fact that the matching process under PSM reduces the sample size too drastically to leave enough variation in the data to derive significant results.

<sup>14</sup> The evaluation is, therefore, strictly speaking not an assessment of the impact of credit alone but of a credit-plus intervention.

<sup>15</sup> The first round of this panel generated the data set that Pitt and Khandker (1998) used in their pioneering study.

an impact that cannot be measured appropriately within a year or two after treatment. The long-period panel data they have at their disposal is especially suited to test this hypothesis. Like Islam (2011), they test the hypothesis by making a separate assessment for those participants who continued to borrow for the entire period and comparing the effects on these borrowers with the overall effect. They find that overall participation has no significant effect on moderate poverty, but continuous participation does – it brings poverty down by 5.5 percentage points. The effect of microcredit is even more pronounced on extreme poverty. While participation in general does not seem to have any significant impact on moderate poverty, it does so for extreme poverty, which is reduced by 3.5 percentage points. The effect of continuous participation is even stronger – it brings extreme poverty down by 7.1 percentage points.

Using the same dataset, Khandker and Samad (2013) show that the effect of microcredit is not only differentiated by the duration of participation but also by gender, which is congruent with the earlier finding of Razzaque (2010) based on PKSF data. They find that male participation has hardly any impact on either moderate or extreme poverty, but female participation reduces extreme poverty by about 4 percentage points, although it too has no significant effect on moderate poverty (Table 1).

Khandker and Samad (2013) explore a number of other issues that are highly relevant in the context of assessing the impact of microcredit. There has been much discussion of late on whether rapid expansion of microcredit is pushing many borrowers into a debt trap by inflicting an unsustainable debt burden on them. The argument is that while borrowing might provide a temporary boost to the living standard of the poor, it is possible that their long-term economic viability is being undermined in the process. The evidence we have cited above indicating positive impact of credit on asset accumulation – as found by a number of panel studies using different datasets – already suggests that this fear is unlikely to be valid in general. However, the issue is certainly important enough to deserve a thorough investigation, which is exactly what the authors provide by examining data on both debt accumulation and asset accumulation, and the consequent evolution of net assets. Their evidence shows that even if participants borrowed and afterwards accumulated debt, they accumulated more assets than debt over time, so that net worth has increased and debt-asset ratio has declined as a result of programme participation (Table 2). They conclude, “Thus, in contrast to the common perception about poverty and indebtedness, we find that microcredit participants are not necessarily trapped either in poverty or debt.” (p.24)

**Table 1**  
**Impact of Credit on Poverty by Gender**

Microcredit borrowing variable	Moderate poverty	Extreme poverty
Household males	-0.015 (-0.62)	-0.019 (-0.87)
Household females	-0.003 (-0.15)	-0.037 (-2.37)
R <sup>2</sup>	0.299	0.327

**Source:** Khandker and Samad (2013), Table 7, p.37.

**Note:** The figures within parentheses are t-statistics.

**Table 2**  
**Debts and Assets of Microcredit Borrowers**

Microcredit borrowing variable	Log household non-land assets (Tk)	Log household net worth (Tk)	Log household debt-asset ratio
Household males	0.264 (4.84)	0.188 (3.16)	-0.270 (-4.23)
Household females	0.236 (5.51)	0.053 (1.64)	-0.475 (-10.00)
R <sup>2</sup>	0.454	0.655	0.187

**Source:** Khandker and Samad (2013), Table 7, p.37.

**Note:** The figures within parentheses are t-statistics.

Table 2 brings into focus an interesting contrast between male and female borrowers that deserves comment in the light of our earlier observation about the gender-differentiated effect of microcredit. We noted earlier that male participation in credit programmes does not seem to have any positive impact on poverty while female participation appears to have a strong poverty reducing effect, especially when it comes to extreme poverty. Table 2 shows, however, that male borrowing has a much stronger effect on asset accumulation compared to female borrowing; indeed the coefficient of female borrowing is not even statistically significant. These contrasting gender-mediated effects of credit on poverty and asset accumulation lead to the following inference. Female borrowing imparts a short term impact on the household's living standards by boosting household consumption – either directly by spending the loan for consumption purposes or indirectly by using the income of loan-financed investment for this purpose – instead of saving the money with a view to augmenting household assets. By contrast, any benefit from male borrowing is directed more towards asset accumulation than towards short term consumption. If this interpretation is valid, the earlier observation on the gender-differentiated effect of poverty needs to be seen in a different light. The real difference in female and male borrowing is not that the former is effective in reducing poverty while the latter is not but that female borrowing helps raise the living standard of the household more in the shorter run while male borrowing secures more in terms of the longer run viability of the household.

### 5. A Study Based on Quasi-Panel Data

In 2010, the Institute of Microfinance conducted a large scale rural survey covering 6300 households from 180 villages spread over all the districts of Bangladesh (except Rangamati). The sample was drawn through a stratified random sampling procedure very similar to the one used by the Bangladesh Bureau of Statistics for its Household Income and Expenditure Surveys and the sample size was very similar too. This was meant to be a benchmark survey for a longitudinal study on the dynamics of poverty in rural Bangladesh, and for this purpose a panel data set was to be created by re-surveying the same households every three years or so. The intended panel data does not yet exist as the first re-survey is being carried out only now (2013). However, the benchmark survey of 2010 was designed in such a way that the spirit of a longitudinal study could be captured to some extent. For this purpose, information was collected, on a recall basis, about the land and non-land physical assets owned by each household at the time it was formed – through inheritance or otherwise. Comparison of those initial assets with the current ones could give some clue as to the life trajectories of the households, revealing the manner in which different households have moved up or down the asset ladder. It is this longitudinal information on asset transition that gives this survey a quasi-panel character.

The information on asset transition was exploited by Osmani (2012) to throw light on the impact of microcredit on poverty over a long time horizon, which extends back to the times when the sample households first took part in a microcredit programme.

One of the salient findings of this study was that the microcredit borrowers started their journey line life (as independent households) with a serious disadvantage in terms of initial assets relative to the non-borrowers. This is of course to be expected for the overall sample since borrowers are likely to come from the pool of relatively worse off households anyway. But the interesting feature is that even if one considers only the poorer segment of the population, borrowers are found to have started with a relative disadvantage compared to non-borrowers, as shown by Table 3. In respect of land ownership, non-land physical assets and even human capital (as measured by years of schooling of the household head), the initial endowments of borrowers were less than those of non-borrowers.<sup>16</sup> What is also remarkable, however, is that over the years the borrowers have been able to reduce the degree of disadvantage to some extent, as revealed by the narrowing of gap in the ownership of non-land physical assets.<sup>17</sup> The value of such assets owned by the borrowers at the time the households were formed was 78 per cent of that of non-borrowers; by the year 2010 this ratio had gone up to 88 per cent. This suggests that access to credit enabled the borrowers to accumulate non-land physical assets faster than their non-borrower peers in the poorer segment of the population.<sup>18</sup>

**Table 3**  
**Initial and Current Assets of Microcredit Borrowers and Non-Borrowers**

Category of Households	Initial land assets (decimal)	Initial non-land physical assets ('000 Tk)	Years of schooling of household head	Current land assets (decimal)	Current non-land physical assets ('000 Tk)
Borrowers (B)	25	28.5	0.74	25	52.1
Non-borrowers (N)	37	36.5	0.88	39	59.0
Ratio (B/N)	0.68	0.78	0.84	0.64	0.88

**Notes:** (1) The sample is restricted to the poor and marginally non-poor households, who accounted for 42% of all rural households in 2010.

(2) 'Initial' refers to the time when the respective households were formed. 'Current' refers to the year 2010.

(3) All values are in constant prices of 2010.

**Source:** InM Benchmark Survey 2010, *Dynamics of Poverty in Rural Bangladesh*.

Further analysis shows that the effect of credit on assets accumulation also translated into higher living standards of the borrowers as measured by poverty, income and consumption.

<sup>16</sup> All differences are statistically significant.

<sup>17</sup> Although we may speculate that the same thing happened to financial assets, we cannot demonstrate this as it was not possible to collect information on initial financial assets, which we thought could not be reliably collected on recall basis.

<sup>18</sup> A more rigorous demonstration of this proposition is offered in Osmani (2012), where the effect of credit on asset transition is estimated after controlling for the effects of other factors. This finding corroborates Khandker and Samad (2013) and several other panel data studies discussed earlier, who also demonstrated the positive effect of microcredit on asset augmentation.

The main findings in this regard may be summarised as follows.<sup>19</sup> First, access to credit helped reduce poverty, and it reduced extreme poverty even more strongly than overall poverty. Second, even those borrowers who failed to cross the poverty line benefited from having access to credit. The precise nature of the benefit, however, varied to some extent depending on the use of credit. Those who used credit mainly for productive purposes enjoyed both higher income and higher consumption; those who used it mainly for non-productive purposes enjoyed higher consumption but not higher income compared to the non-borrowers. Finally, all borrowers – regardless of how they used it – benefitted by being able to cope with crises better. In particular, they didn't have to take recourse to 'erosive' coping strategy – i.e., the strategy of selling off assets – as frequently as non-borrowers had to. In other words, access to credit enabled them to preserve their assets better in the face of crises, thereby improving their resilience and non-term economic viability.

Osmani (2012) also made an estimate of the contribution of microcredit towards overall reduction of poverty in rural Bangladesh. The question was posed as follows: what would have been the extent of poverty in rural Bangladesh if microcredit had not existed? The difference between this counterfactual poverty rate and the actual poverty rate gives a measure of microcredit's contribution. A partial measure of the counterfactual poverty rate was estimated by using Amartya Sen's concept of entitlement mapping.<sup>20</sup> In the present context, a household's entitlement mapping shows how its initial endowments (i.e., the physical and human capital it had at the time the household was formed) translate into current standard of living. The entitlement mapping was estimated separately for borrower and non-borrower households. It was then asked: if the borrowers had the entitlement mapping of the non-borrowers how much poverty would they have had at present given their initial endowments? The answer provided the counterfactual poverty rate among borrowers. Combining it with the actual poverty rate among non-borrowers yielded the counterfactual poverty rate for rural Bangladesh as a whole. The results are reported in Table 4.

**Table 4**  
**Contribution of Microcredit towards Poverty Reduction in Rural Bangladesh**

	Actual Poverty rate	Counterfactual poverty rate	Percentage points diff.	Percentage difference
<i>Borrower households</i>				
Overall poverty	37.4	40.6	3.2	8.5
Extreme poverty	22.0	26.0	4.0	18.3
<i>All households</i>				
Overall poverty	33.1	34.7	1.6	4.7
Extreme poverty	19.9	21.9	2.0	9.9

**Note:** Poverty rate is measured as percentage of persons below the relevant poverty line. For detailed discussion of the poverty lines and data used for the purpose of poverty measurement, see Osmani and Latif (2013) and Osmani *et al.* (2013).

**Source:** Osmani (2012), Table 32, p.33 (after correcting a typographical error in the bottom row under the column 'Counterfactual poverty rate').

<sup>19</sup> For details, see Osmani (2012), Osmani and Ahmed (2013) and Osmani *et al.* (2013).

<sup>20</sup> The measure was only partial in the sense that the methodology used was not capable of capturing the 'spill-over' or the general equilibrium effects of microcredit.

The estimates for 'all households' show that in rural Bangladesh as a whole overall poverty would have been close to 5 per cent higher and extreme poverty would have been nearly 10 per cent higher. To put it differently, the direct contribution of microcredit towards poverty reduction in rural Bangladesh has been about 5 per cent for overall poverty and 10 per cent for extreme poverty.

The estimates for 'borrower households' show that in the absence of microcredit overall poverty would have been almost 9 per cent higher among the borrowing households and extreme poverty would have been 18 per cent higher among them. One might thus conclude that roughly speaking about 1 in 10 households has been pulled out of poverty while about 1 in 5 households has been pulled out of extreme poverty by microcredit over the years. The average duration of participation in credit programme in the sample (which is a representative sample for rural Bangladesh as whole) is about 5 years; 70 per cent of borrowers have duration of 5 years or less and 90 per cent have duration of 10 years or less. Taking the average duration of 5 years, the figure of 1 in 10 coming out of poverty during the overall period can be roughly translated as 2 per cent of the borrowers coming out of poverty per year on the average.

A number of caveats should, however, be borne in mind while interpreting these results. First, the estimates do not take into account the spill-over effects or the general equilibrium effects of microcredit, concentrating only on the direct benefits accruing to the borrowers. Second, even for the direct benefit, they measure the contribution only partially by defining benefit as the number of borrowers who crossed the poverty line with the help of microcredit, ignoring the benefit to those who remained below the poverty line and yet enjoyed higher income and consumption because of microcredit. Third, they do not take into account the benefit that accrues in the long term by enabling the borrowing households to preserve their assets better at times of crises. For all these reasons, our estimates should be regarded as underestimate of the true contribution of microcredit towards poverty reduction in rural Bangladesh.

## 6. Concluding Observations

The review of the evidence presented in this paper points to an overwhelming consensus that microcredit has made a positive contribution towards improving the living condition of the rural poor in Bangladesh. A number of interesting aspects of this contribution that have emerged in the course of the review are worth recalling: (a) while the benefits of microcredit accrue to the borrowers generally, the extreme poor among them gain the most; (b) female borrowing has a stronger short run impact on the economic well-being of the household compared to male borrowing, but male borrowing appears to have a stronger impact in the long run through accumulation of assets; (c) the longer the duration of participation the stronger is the positive impact of credit; (d) the extent and nature of benefit vary depending on the use of credit, but there is no basis for the popular perception that sustainable benefits accrue only when credit is used for productive purposes; (e) microcredit has enabled the vast majority of borrowers to strengthen the long-term economic viability of their households by expanding their asset base and by helping them to preserve assets in the face of periodic crises; and (f) while in many cases debts have also increased along with assets, this has not on the whole led to an unsustainable debt burden as assets growth has outstripped the growth of debt, with the result that on the average the net worth of the borrowers has improved relative to non-borrowers and the debt-asset ratio has declined.

That microcredit should benefit the rural poor should not come as a surprise. Indeed, it's a surprise that anyone should have thought otherwise, especially when one recalls the old gory tales of how 'unscrupulous' moneylenders used to suck the blood out of their hapless victims, the sad tales of how banks closed their door to the 'uncultured' village folk whose need for loans was too small to be worth bothering about, and the sordid tales of how subsidised government



credit was cornered by the rural elite leaving the poor permanently ensconced in the stranglehold of moneylenders. Microcredit changed all that. For the first time, the rural poor had genuine access to credit and at rates of interest far below what they had traditionally been charged by the moneylenders. If the pre-existing scenario of non-availability of credit at affordable rates of interest was deleterious to the interest of the poor, as it was almost universally believed to be the case at the time, simple logic suggests that the new scenario opened up by the advent of microcredit should, by implication, also be believed equally universally to be advantageous for them. But that is far from the being case. On the contrary, a climate of opinion seems to have emerged in recent times, which is at best agnostic and at worst hostile to the idea that microcredit helps the poor. It seems to me that at least two strands of thought have contributed to the emergence of this counter-intuitive attitude towards micro-credit. The source of one of these strands lies in the academia and the other in the perception of a section of social activists.

The academic strand has its origin in recent debates among social scientists on the relative merits of non-experimental and experimental data as the basis for evaluating the impact of policy interventions. Until very recently, non-experimental data has been the primary basis for impact evaluation in social science, including economics. In the specific context of microcredit, the study by Pitt and Khandker (1998) used to be widely regarded as embodying the pinnacle of sophistication in extracting causal relationship from non-experimental data. As we have seen, however, its reputation was almost fatally threatened by persistent attacks from various corners. The central message of the study that microcredit helps the poor began to seem vacuous, which led to the nihilist view that if as sophisticated a study as this cannot demonstrate the effectiveness of microcredit why should anyone give credence to the claim that microcredit works. At about the same time, a small number of experimental studies began to emerge, using randomized controlled trials (RCT), that claimed not to find any appreciable impact of microcredit on the income and expenditure of the poor (e.g. Banarjee *et al.*, 2010; Karlan and Zinman, 2010). The great merit of RCT is that it neatly solves the identification problem by the very simple device of randomly assigning the intervention to one sample and denying it to another so that no selection problems can arise to complicate the assessment of causality. Since this apparently sure-shot solution to the identification problem could not find any causal impact of microcredit, it served to tilt the balance of opinion decisively in favour of the nihilist view about the efficacy of microcredit.

But as Mark Twain would have said – if he were wearing the shoes of Muhammad Yunus – the reports of the death of microcredit were grossly exaggerated! Neither the attacks on Pitt and Khandker nor the evidence adduced by RCTs justify the nihilist position. As our brief technical review of the Pitt-Khandker controversy has sought to demonstrate, their central message still survives although the magnitude of the impact they claimed to find seem less plausible today. As for RCT, its main limitation is that by the very nature of the enterprise it can only investigate relatively short run effects of an intervention, mainly because continuing to deny the intervention to the control group for a long time raises a whole host of ethical and practical issues. And yet, as we have seen, a number of panel data studies have demonstrated that the benefit of microfinance rises with the length of participation; so it is entirely plausible that RCTs will fail to detect the effect even if it exists.<sup>21</sup> On this ground alone, the evidence produced by the panel studies in support of the efficacy of microcredit is much more credible, in my view, despite the technical superiority of RCTs in resolving the identification problem.

The strand of scepticism that emanates from the camp of social activists has mainly to do with the interest rates charged by the MFIs. Although these rates are well below the ones traditionally charged by moneylenders, there is a widespread perception that the rates are not low enough

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<sup>21</sup> Some of the best exponents of RCTs admit as much. See, for example, Banarjee and Duflo (2011).

to serve the best interest of poor borrowers. It is somewhat ironic that the allegations of excessively high interest rates should be heard in Bangladesh, where the interest rates charged by the MFIs happen to be among the lowest in the world. The biggest irony of all is the allegation of 'blood-sucking' being levelled against the Grameen Bank (and its founder), which actually charges the lowest interest rate among all the large-scale MFIs in the world. Evidently, there is more politics than economics in the more outlandish allegations, but there are some genuine economic issues as well.

The issues of genuine concern are best distinguished by the nature of the use of credit. For the credit that is used mainly for productive purposes, the major concern is: do the borrowers generally earn a high enough rate of return to be able to pay an interest that is well above those charged by commercial banks to their clients in the formal sector of the economy? And for credit that is used mainly for non-productive purposes, the main question is: since the borrowers are not earning any extra income by investing the loan, how can they repay it at such a high interest without getting into a cycle of debt? These questions are perfectly legitimate and more research needs to be done to answer them fully satisfactorily. But some tentative answers can already be given from the existing stock of knowledge.

Evidence has begun to emerge that for the majority of productive users the rates of return are indeed high enough to justify the existing rates of interest. Some indirect evidence was found in the course of the *InM Benchmark Survey of 2010* referred to earlier. The productive users were asked: what was the breakeven rate of interest above which, in their own judgment, their enterprise would no longer be economically unviable. In the vast majority of cases, the self-reported breakeven rates are above the interest rates they were actually paying, indicating that their rates of return were high enough to cover the interest costs (Osmani *et al.* 2013). Khandker *et al.* (2013) provide more direct evidence by calculating the rates of return in various lines of activities in which borrowers engage themselves; once again these were found to be no lower than the reported interest rates.

As for non-productive uses, the first point that needs to be clarified is that such uses should not be scoffed at as either wasteful or necessarily harmful for the borrowers. Not everybody needs credit for productive purposes. Credit is essentially a means of correcting mismatch between the flow of needs and the flow of cash (or, liquidity). This mismatch can happen in the spheres of both production and consumption. For some people, meeting the mismatch in the sphere of production is the priority; for others it is consumption. Neither can claim any moral or economic superiority over the other. Millions of people around the world routinely take loan from the formal banking system to correct for the mismatch in the sphere of consumption. There is no justification for denying this privilege to the rural poor.

The only relevant question is whether on the whole they can manage the cash flow well enough so that they can repay the loan without falling into a debt trap. Anyone who doubts that they can should read the book entitled *Portfolios of the Poor* (Collins *et al.*, 2009) which documents in great detail, from carefully collected data from many different parts of the world, how intelligently most poor people manage their finances, even though their cash flow patterns are often highly complicated because of multiple occupations pursued by the household members. At the same time, it is the overlapping cash flows emanating from multiple occupations that allow the poor people to repay their loans in regular instalments even though no income may be earned from the loan taken. No doubt some households occasionally fail to manage loans properly and get into trouble as a result, but the same thing happens to households in richer part of the world as well. From Khandker and Samad's (2013) finding that microcredit borrowers have lower debt-asset ratios compared to non-borrowers, we can infer that on the whole poor borrowers succeed more than they fail in managing their portfolios in rural Bangladesh. Thus, by and large, there is no general basis for the presumption that interest rates are too high to be viable for

either productive or non-productive users, but this is surely an area where further research would be useful.

We conclude that neither academic scrutiny nor the negative perception about interest rate that prevails in some quarters provides strong enough ground to negate the cumulative evidence we have produced in this paper which shows that microcredit has helped the rural poor of Bangladesh in a significant way. At the same time, we should caution that there is no justification for making sky-high claims about the contribution of microcredit. The best estimate we can make at the current state of our knowledge is that access to microcredit has helped reduce overall rural poverty by about 5 per cent and extreme poverty by 10 per cent. Considering the borrowing households alone, we find that roughly speaking microcredit has helped to bring 1 in 10 borrowers out of poverty and 1 in 5 borrowers out of extreme poverty over the years. Given that the average duration of participation has been about 5 years in our sample, we may conclude that with the help of microcredit about 2 per cent of the borrowers have come out of poverty per year on the average.

While these figures may not seem spectacular, there is no reason to expect otherwise. Considering that many of the borrowers were at the bottom of the rung to begin with and that loan amounts are a very small fraction of even poor household's total income, one could not possibly have expected any significantly larger number coming out of poverty. There are a number of reasons, however, why these numbers should not be belittled either.

In the first place, it must be considered a great achievement for any single intervention targeted to the poor to be able to bring 1 in 10 beneficiaries out of poverty. I am not aware of any other intervention that can boast a similar level of success anywhere in the world.

Second, just because only 1 in 10 borrowers has been pulled out of poverty, it should not be thought that microcredit has failed with respect to the remaining 9. For, the vast majority of these remaining borrowers have been able to improve their living standards relative to the comparable non-borrowers even though they have still remained below the poverty line. In fact, relatively speaking, those who have remained below the poverty line have improved their living standards even faster than those who have crossed the poverty line, as shown by the fact that 1 in 5 borrowers has been pulled out of 'extreme poverty' compared to 1 in 10 being pulled out of poverty in general (Osmani and Latif 2013).

Third, microcredit has generally been able to improve the living standards of borrowers in a sustainable manner as Khandker and Samad's (2013) findings on debt-asset ratios clearly demonstrate.

Fourth, even among those poor borrowers who have not enjoyed any significant improvement in living standards there are many who were at least able to protect themselves from falling deeper into poverty as microcredit helped them to cope with serious crises without having to sell off whatever meagre assets they had (Osmani and Ahmed 2013).

Fifth, while we would all like to see improvement in the living standards of the poor, it should not be forgotten that apart from a developmental role microcredit also has a purely banking service role which the poor were traditionally deprived of. With the help of this service, the poor are now better able to match the flow of liquidity with the flow of their needs, which enables them to enjoy benefits such as consumption smoothing, whose significance should not be underestimated.

Finally, the figures cited exclude the spill-over effects, which could be considerable for both non-borrowers and borrowers.

Ignoring the spill-over effects, in particular, implies that our estimates of the impact of microcredit on poverty are likely to be underestimates. We suspect, however, that even after correcting

for possible underestimation, the contribution of microcredit will still remain a relatively small part of overall poverty reduction in Bangladesh. One should not expect it to be otherwise, for after all microcredit is just one of many interventions that have a bearing on the lives and livelihoods of the rural poor. To keep the matter in perspective, we may note that by following the same methodology that was used to estimate the contribution of microcredit, the contribution of foreign remittances, the other great driver of change in the rural economy, was found to be only 6.7 per cent for overall poverty and 6.5 per cent for extreme poverty (Osmani, 2012).

The discourse on microcredit should move on. Instead of taking rigid positions on the efficacy of microcredit in general, the protagonists should focus attention on the details of how microcredit can be made more useful for the poor – by altering the terms and conditions of the loan, by improving the efficiency of MFIs, by exploring the means and implications of complementing credit with other microfinance services such as savings and insurance, and so on. The dividends from such a shift in discourse should be highly rewarding.

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