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what is the impact of microfinance, and what does this imply for microfinance policy and for future impact studies?

James Copestake and Richard Williams, June 2011



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introduction

Popular expectations of microfinance (MF) are falling. Having been lauded as a way of lifting a generation out of poverty, as illustrated by numerous inspiring stories, it is facing increasing pressure to prove its worth. This report contributes to this debate by analysing published evidence of MF impact, and suggesting how it can best be augmented. It has been commissioned by MicroNed – a network of Dutch development finance organisations – for a conference in Utrecht in June, entitled “Taking stock of the evidence on impact, the way forward.” Its intended audience is donors and social investors engaged in supporting MF with socially responsible investments.

Increased pressure for MF to demonstrate its worth is a result of a number of factors, some specific to the MF sector and others reflecting wider currents in development thinking. First, the global expansion of a more commercially oriented MF (exemplified by the lucrative flotations of Compartamos in Mexico and SKS in India) has reinvigorated a longstanding debate over dynamic trade-offs between current impact, financial self-sustainability, long-term growth and hence future impact potential (Copestake, 2007). Second, growth bubbles and crashes, such as that centred on Andhra Pradesh, have illustrated the potential of MF to cause serious harm to those who become over-indebted. Third, a new wave of research based on randomised controlled trials (RCT) promises to offer more rigorous but less positive evidence of impact than earlier studies based on quasi-experimental methods.

Broader influences in development thinking include renewed clamours for evidence-based policy, with more reliable evaluation methodologies and systematic literature reviews. For example, in 2006, the Centre for Global Development issued the rallying call to “close the evaluation gap” through more rigorous impact assessments (Ramalingam 2011). These calls have been accentuated by the strain on budgets arising from the 2008-2009 financial crisis and subsequent global recession.

The rest of this paper comprises four sections. Section 1 reviews the nature and quality of impact studies and defines the scope of this report. Section 2 provides a summary of twelve selected studies. Section 3 augments these findings by summarising the conclusions of three recent reviews of impact evidence. Section 4 then offers a general assessment of the state of knowledge on MF impact, identifies knowledge gaps and considers options for further investment in the evidence base.

nature and scope of MF impact studies

1.1 Types of evidence of impact

When we talk about impact, we are concerned not only with what changes have occurred in selected indicators of welfare, but also with establishing to what extent such changes can be attributed to specific MF services or interventions. First, and most important, is evidence of positive and negative effects on *typical* clients, their immediate family and employees. These include impact on the following: income and asset holdings; economic resilience, including capacity to protect income and smooth consumption; learning, attitude change and empowerment; livelihood/employment creation and protection. Second, it is useful to understand the extent of *variation* in impact according to the nature of MF services provided, who uses them and in what context. Third, and given the possibilities that any one MF service may substitute or complement others, impact assessments ultimately need to be viewed in the context of local and national changes in all financial services and indeed the wider welfare systems of which they are a part (Copestake, 2010). Fourth, evidence is also needed on what forms of secondary support – technical as well as financial – most effectively promote more effective MF services and systems.

This paper is primarily focused on direct positive and negative effects of MF, particularly microcredit. This reflects the focus of current public interest and concern. More evidence is also available about the impact of microcredit than other MF services. However, we fully accept that this restricted focus is far from satisfactory from a policy perspective – as discussed in Section 4.2.

1.2 What constitutes ‘rigorous’ evidence?

The question what constitutes ‘rigorous’ evidence of impact is much debated. Scientific method aspires to a standard of rigour that is universal in the sense that it is reasonable for any sufficiently qualified reader of a study to accept the claims it makes to contribute to truth. This formally entails the reader being able to check that conclusions are logically derived from the data presented plus clearly stated assumptions. More realistically it entails the reader being satisfied that the data and logic underlying claimed evidence of impact is sufficiently open to scrutiny or peer review.

This definition rules out a great deal of evidence that people with direct personal experience of MF may nevertheless judge to be sufficiently reliable to be useful, on the basis of its congruence with other evidence available to them personally. For example, much data collected and used by organisations for internal social performance management falls into this category. In isolation its reliability is suspect, but cross-checked critically against other evidence (including direct observation and daily conversations with clients and operational staff) it may nevertheless contribute usefully to the reader's overall understanding of what does and does not work and why. While we have excluded such evidence from the scope of this report, we nevertheless recognise that such data may ultimately be more reliable, cost-effective and timely for those closer to the ground than impact assessment data that is more scientifically rigorous and intended primarily for readers lacking direct experience against which it can be evaluated. Box 1 below highlights the different types of approaches available to measuring MF impact.

Box 1: Approaches to measuring MF impact

- 1.** Independently conducted quantitative and qualitative impact assessments studies, usually commissioned by external agencies to inform public policy and strengthen accountability to providers of investors in microfinance institutions (MFIs) (see Sections 2 and 3).
- 2.** Focus groups, in-depth studies and satisfaction surveys, cross-checked for consistency with other evidence available, that report on respondents own attribution of impact; generally a component of social performance assessment, management and social auditing, and mostly intended to generate data for MFIs themselves (e.g. Copestake *et al.*, 2005a).
- 3.** Broader social science research into MF as one element of wider financial and welfare systems, at household, neighbourhood/village and financial sector level (e.g. Collins *et al.*, 2009; Fernando, 2006; Johnson, 2004a; 2004b).

We also recognise that evidence of impact based on scrupulous interpretation of diverse and highly contextual qualitative data (e.g. by trained anthropologists) can be at least as scientifically rigorous as findings based on statistical analysis of quantitative data (e.g. by trained economists). However, systematic reviews of the qualitative data are as yet not available and hence by default this review focuses mostly on quantitative studies.

1.3 Quantitative impact assessment

There are numerous issues and challenges in measuring the direct impact of MF which can significantly influence the size and direction of results. First, there is the nature of the underlying interview or encounter between client and researcher through which data is collected and codified, with possibilities of bias according to the incentives both face (since double blind trials are not possible), as well as due to problems of recall and recording accuracy. These can be controlled only to some extent; for example, there are good reasons to believe that such errors may systematically differ for interviews with a group of people taking loans when compared with an otherwise identical group of people who are not receiving loans.

Second, there is the nature of the statistical sample, including its size, and variation in the characteristics of those who are offered and choose whether or not to take up MF services. More reliable studies not only include a comparison group, but ensure differences from those receiving MF services or 'treatments' are either minimised or can be quantified. This is essential to establishing a 'counterfactual' view of what would have happened to borrowers if they had not had access to a loan. When observable characteristics (e.g. age, education, social status) or unobservable characteristics (e.g. entrepreneurial spirit, informal business connections) of borrowers differ from non borrowers, selection biases arise that will lead to false attribution of impact unless somehow corrected. Other potential biases include placement bias arising from the decision of MFIs to operate in relatively rich (or poor) locations, and attrition bias arising from excluding those who drop-out or refuse to be interviewed.

The third issue affecting rigour is the extent to which data analysis includes methods that correct for such differences, or at least make the extent of such

possible biases transparent. Tedeschi (2008) is an example of a study that has attempted to demonstrate that failure of even relatively high quality studies to control fully for selection bias can lead to direct impact being seriously overestimated.

evidence from selected impact studies

Researchers distinguish between three approaches to MF impact assessment: experimental methods (including RCTs); quasi-experimental, and qualitative. Here we present twelve studies: three RCTs, seven quasi-experimental and two qualitative. These illustrate the differing methodological challenges and types of impact reported; but the twelve studies are neither representative of all studies, nor the product of a systematic selection process. The evidence they provide is supplemented in Section 3 by a summary of conclusions from three more comprehensive reviews.

2.1 RCT studies

Already established in fields such as health and agronomy, RCTs have gradually become more influential in other policy areas also.² Armendáriz and Morduch (2010:305) claim this approach has “been embraced as the gold standard for evaluations.” However, this viewpoint has been strongly contested, for reasons outlined in Section 4.3.³ RCTs work by randomly grouping households into treatment and control groups in advance of MF services being offered. The treatment group with access to MF is then compared with the control group, which does not have access. Proper randomisation ensures those individuals in treatment and control groups are equivalent in terms of observable and unobservable characteristics with the exception of the treatment status, assuming that no contamination effects exist (Blundell and Costa Dias, 2000, 2002 and 2008). Three RCTs that are cited widely are summarised below.

² Two books have recently been released by prominent ‘randomistas’: Banerjee and Duflo (2011), and Karlan and Appel (2011).

³ See for example: Heckman and Smith, 1995; Imbens, 2009; Pritchett, 2009; Deaton, 2010; Cartwright, 2011. For a non-technical discussion of both views in relation to MF see Karlan et al. 2009.

RCT of microcredit in Hyderabad (Banerjee *et al.*, 2009)

Summary 52 out of 104 neighborhoods in the city of Hyderabad were randomly selected for the opening of a branch of the MFI Spandana, with data collected 15-18 months afterwards. The study found increased borrowing, with 27% of households in Spandana-served areas taking microcredit compared to 19% in control areas. The authors evaluated the effect of this modest increase in borrowing on a range of measures, including consumption, business income, education, health and empowerment.

Findings The study found that business profits, inputs and revenue increased, although the results were not found to be statistically significant. Expenditure only gradually increased, but this was again statistically insignificant. However, there was a decrease in expenditure on temptation goods. Odell (2010) highlights the inter-temporal importance of these findings with a short-term increase in investment and expansion of business not leading to immediate wellbeing effects. A planned return to Hyderabad by the researchers should shed further light on the longer-term effects.

RCT of relaxing lending criteria in the Philippines (Karlan and Zinmen 2010)

Summary An RCT based on individual loans given by the First Macro Bank in the Philippines. The researchers focused on marginally creditworthy applicants who had not been chosen for a loan, then randomly selected a sub-sample of them to receive credit. Duvendack *et al.* (2011) criticised this methodology, highlighting the incentives for loan officers to select the relatively better-off rejected clients and to pay closer attention to them, thus reducing the validity of the findings.

Findings The study a statistically significant increase in business profits resulting from increased credit for male but not female borrowers. Profits were higher for households with higher income. Male borrowers were less likely to be employed outside of their own business, and employed fewer people. But their children were more likely to be sent to school. There was little effect on business investment, and no significant effects on poverty, income, food quality, visiting a doctor and subjective

measures of wellbeing, which may have actually declined (due to increased stress, for example). Odell (2010) states that these conclusions are confusing, with effects on male borrowers stronger yet little change on poverty and income. But lower income may in part be attributed to more children going to school.

RCT of relaxing savings constraints in Kenya (Dupas and Robinson 2009)

Summary The authors examined the effects of better access to micro-savings opportunities on business investment. A randomly selected sample of respondents were offered interest-free savings accounts in a village bank in Kenya. They were then asked to fill in daily log books to record their financial activity as were those randomly selected into the control group. One complication that arises in interpreting the findings is that access to a savings account may have impacted on respondents indirectly by influencing their response to the advice and support provided in filling out the log books.

Findings The study found that the usage of the accounts was highest amongst women and that business investment of women with savings accounts increased significantly by a minimum of 40%, with significant increases also in their personal expenditure, including on food. They further found that these savings accounts did not appear to crowd out use of other accounts.

2.2 Quasi-experimental studies

These studies attempt to control for observable and unobservable variables that differ between treatment and comparison groups using statistical techniques. Unlike RCTs, membership of these groups is not randomly assigned. **Pipeline studies** draw control groups from those that have self-selected and have been selected by peers or loan officers, but have yet to receive any MF services. **With and without** studies involve the comparison of treated groups with comparable untreated groups with potential bias mitigated by using a range of econometric techniques, for example **propensity score matching** that aims to strip out

observable differences between the two samples. Difference-in-difference methods do the same for changes in selected variables between two time periods. A third approach removes selection bias by identifying one or more **instrumental variables** that influence loan take up but not impact. All these approaches can be criticised for failing to fully mitigate bias (Duvendack *et al.*, 2011) and even the most complicated econometrics fail to make up for poor data quality and/or poor research design (Duvendack, 2010a).⁴ Examples of each type of quasi-experimental approach are presented below. The most famous and controversial example of this approach is Pitt and Khandker's study of group lending in Bangladesh. This provides a good example of the difficulties in measuring MF impact and is reviewed separately in Box 2.

Pipeline study of microcredit in Northeast Thailand (Coleman, 1999; 2002)

Summary Pipeline studies are most associated with Brett Coleman, who developed this process in his early studies on Thailand. As Goldberg (2005) explains, Coleman established a control group by asking those interested but not yet receiving microcredit to sign up a year in advance; that way he could compare borrowers to people likely to have similar unobservable characteristics, including the same entrepreneurial spirit.

Findings Coleman reports positive MF impact based on 'naïve' estimates that did not control for selection bias, including a tendency for village bank members to be wealthier in the first place. The 'correct' estimates found no impact for the average recipient on asset accumulation, savings on school expenditures, and school expenditure. Overall, positive impact was identified only in the wealthier recipients in the form of increases in savings and boys' schooling. But he also concluded the Thailand results could be atypical, because loans were small relative to borrowers' wealth and their availability of credit from other sources (Goldberg, 2005; Odell, 2010).

⁴ See Leamer (1983) for a more critical take on econometrics more generally.

Pipeline study of microcredit in the Philippines (Kondo 2008)

Summary Kondo used a similar method to Coleman. Households had been organised in the expectation of an expansion of credit, but had not yet received it. The study then compared participating households and qualified households (i.e. eligible for credit but not yet receiving it).

Findings Poverty outreach of the programme was much lower than expected. But statistically significant increases were found on total income, total expenditure, food expenditure and savings. The study found no effect on wider wellbeing or education and health indicators. However, as Odell (2010:26) highlights in the review of the study, these results need a “major qualification” in when accounting for differential income according to borrowers’ income they become regressive. This is in line with Coleman’s finding that impact is most apparent among the better off.

Box 2: With and without studies of group-based microcredit in Bangladesh – three decades of debate⁵

The benefits of microfinance. A highly influential study by Pitt and Khandker in 1998, was entitled “The impact of group-based credit programs on poor households in Bangladesh: does the gender of participants matter?” Using complex econometric analysis they seemed to confirm MF enthusiasts’ claims that microcredit in Bangladesh was poverty reducing and particularly beneficial for women. The study was based on a World Bank survey conducted in 1991-1992 using a dataset including both MFI members and non-member comparison households. The study found an 18% return on microcredit for women compared to 11% for men, as well as increases in girls’ school enrolment. Khandker (2005), using an updated data

⁵ See Roodman’s blog at: http://blogs.cgdev.org/open_book/2011/03/response-to-pitts-response-to-roodman-and-morduchs-replication-of-etc.php

set, reexamined the study and found even more impressive results. Poverty, he claimed, declined in all villages with access to MF and expenditure increased, with extreme poverty declining more than moderate poverty, and MF accounting for 40% of the entire reduction of moderate poverty in rural Bangladesh (Goldberg 2005).

First doubts. Morduch refuted the claimed benefits of MF made in Pitt and Khandker's original study in his article "Does microfinance really help the poor? New evidence from flagship programs in Bangladesh". Criticisms included weak enforcement of the eligibility criteria within the MFIs 'treatment group' in contrast to more rigid application in the comparison group. Using the same data Morduch found no impact on household consumption although he did find evidence that MF led to consumption smoothing.⁶ A number of other studies have since attempted to replicate the original Pitt and Khandker analysis and apply alternative statistical methods (Chemin, 2008; Roodman and Morduch, 2009; Duvendack, 2010a; Duvendack and Palmer Jones 2011).⁷ These studies have shed further doubt on the beneficial claims made in the original findings with all studies concluding, to differing degrees, that the impact of microcredit was overstated due to methodological problems in the original study (Duvendack *et al.*, 2011). Pitt (2011a and 2011b) has responded by highlighting methodological problems with the Roodman and Morduch replication, particularly a missing variable. However, it seems the debate will continue, with Roodman provisionally sticking to his and Morduch's conclusions that, even if some correlations are found to be positive the direction of causation remains unproven.⁸

⁶ Pitt responded to Morduch's paper a year later, but neither paper was published in a peer-reviewed journal.

⁷ See Duvendack *et al.*, 2011:73-67 for a summary of various methods used and their limitations

⁸ Roodman, at http://blogs.cgdev.org/open_book/2011/03/response-to-pitts-response-to-roodman-and-morduchs-replication-of-etc.php

Lessons? This ongoing debate appears to undermine an important piece of empirical evidence that had a major influence on the expansion of microcredit. It also illustrates the methodological difficulties inherent in quasi-experimental approaches to measuring impact. It also highlights the importance of research transparency that permits replication of studies (Armendáriz and Morduch 2010). Given the cost and controversy arising from econometrics-intensive approaches it also highlights the need to explore alternative approaches.

Quasi-experimental study with propensity score matching in Ethiopia (Berhane 2009)

Summary This is a study of the Dedebit Credit and Savings Institution, which had 1.4 million borrowers in 2000. Analysis is based on a random sample of 211 borrower and 140 non-borrower households in 1997, all of whom were interviewed in 2000, 2003 and 2006. Only 40 didn't borrow at all, and only 79 borrowed in every period. The panel data allows for estimated impact of loans over time correcting for selection bias arising from both fixed and linear changes in unobservable characteristics of respondents. In a second piece of analysis propensity score matching with dynamic counterfactuals is used to investigate how impact depends upon the timing of becoming a borrower, controlling for differences in initial characteristics as well as attrition from the borrowing sample.

Findings Per capita consumption was found to rise as a result of taking even one loan, whereas improvements in housing arose only after multiple borrowing. Positive effects remained even for households that stopped borrowing. This study illustrates the importance of observing impact over time, with lags varying for different indicators and in some cases dependent upon sustained borrowing. The second analysis shows that early participants generally did better than late joiners, and their income also proved more resilient to a weather related shock in 2003. While selection bias cannot be fully removed, the study broke new

ground in reducing it by using more sophisticated econometric analysis made possible by the use of panel data.

Difference-in-difference studies in India, Zimbabwe and Peru (AIMS, 2002)

Summary In the 1990s USAID sponsored major studies of microcredit in Peru, India and Zimbabwe as part of the AIMS programme (Assessing the Impact of Microenterprise Services). All three studies were based on a repeat survey of clients of a selected MFI, comparing them over a two year period with a sample of non-clients in the same vicinity who were pre-screened for eligibility (Goldberg 2005; Odell 2010). This does not in itself fully remove selection bias, because the issue remains why some clients in an area borrow first (Karlan, 2001). Attrition bias caused by difficulties in re-interviewing clients who exit was also a problem.

Findings The India study, of the Self Employed Women's Association (SEWA), found borrower incomes to be 25% higher than savers, with saving only households income still 24% greater than that of non-participants and Goldberg (2005:7) concludes from this that MF can be "quite effective." However, a reinvestigation of the study by Duvendack (2010a and 2010b) found that these findings only hold if differences in unobservable characteristics are absent and, reflect weaknesses in the methodology for constructing comparison groups. The Zimbabwe study found that while MFI clients' initial incomes were higher, after two years the difference was no longer statistically significant. Stewart *et al.* (2010) highlight that in a context of wider economic deterioration more continuing clients than non-clients fell into poverty. Regression analysis of the Peru study, controlling for observables, suggested a \$372 increase in net annual enterprise revenue, and a \$377 rise in household income, per year of credit receipt. Tedeschi (2008) re-examined the data and concluded that these findings did not control for selection bias to the extent that the panel data permitted. Doing so suggested lower but still positive impact on microenterprise profits (Tedeschi 2008:515).

2.3 Qualitative studies

For the reasons highlighted in Section 1.2 this paper largely takes stock of the rigorous quantitative studies available. However, as we will argue later, qualitative studies can also make an important contribution to the evidence base, being particularly useful for understanding causal pathways, and variation (heterogeneity) in impact on different kinds of client. Studies of this nature may therefore be more useful to policy practitioners in designing MF programmes or products, despite the suspicions of those who hold to a narrower and more positivist methodological position. The most influential of those studies in “Portfolios of the Poor” (Collins *et al.*, 2009) is presented below along with a study in Mozambique that made systematic use of clients’ self-attribution of impact.

Financial diary based studies in South Africa, India and Bangladesh (Collins *et al.*, 2009)

Summary To shed light on how poor families used financial services, skilled interviewers met up with 250 individuals in India, Bangladesh and South Africa on a two week cycle over a whole year, to help them keep a diary of their detailed financial transactions. The data was used to create household balance sheets and cash flow statements.

Findings The major finding was that the diaries documented the diverse and flexible way respondents handled their money, as highlighted by previous work of Rutherford (2001) in particular. This provided the evidence base for a call to reappraise at least four aspects of MF. First, they revealed the importance to poor people of savings as well as borrowing mechanisms. Second, they highlighted the importance of MF not solely as a function of demand for financing microenterprises, but also for smoothing consumption, coping with emergencies, acquiring asset and paying for ‘big-ticket’ expenditure items. Third, the data revealed the importance of access to a portfolio of regulated and unregulated financial services. Fourth, the diaries reinforced the point that poverty persists not just because of lack of income but because of risk and variability of income. This is summed up by the simple but important statement that “one of the least remarked on problems of living on two dollars a day is that you don’t literally get that amount a

day” (Collins *et al* 2009:2). A limitation of the study is that it is unclear how representative the sample is, and of what, particularly given its own emphasis on the heterogeneity of poor people’s lives and livelihoods (Micro Save 2010).

Self-reported impact assessment in Mozambique (Athmer *et al.*, 2006)

Summary This study was commissioned by the Netherlands Platform for Microfinance and linked poverty outreach and impact assessment of three MFIs in Maputo (NovoBanco, SOCREMO and Tchuma). The research used a mix of research methodologies, including a sample survey to assess poverty outreach (relative to a national household survey) and to measure changes in poverty indicators over two year period. Impact assessment was based on in-depth interviews with 90 clients and relied upon self-reported attribution, following the QUIP methodology described in Copestake *et al.*, 2005b. The survey from which these clients were drawn randomly covered 1,287 clients still borrowing after two years and 78 ex-clients who had left in the previous two years.

Findings Results from the sample survey revealed significant growth in loan sizes for those that remained in the programme but also high drop-out rates. There were increases in business sales, particularly for women clients. Despite this there was little evidence of MF leading to job creation, with loans being used for working capital and house improvements more than capital investment. Of the clients interviewed in depth 77 out of 90 self-reported positive impact of credit on household welfare over the two year period. However, the study acknowledges an upward bias in these results arising from low coverage of clients who dropped out within the two year reference period.

section 3

summary of conclusions from secondary reviews of microfinance

This section summarises findings from three recent reviews of microfinance impact (Odell, 2010; Stewart *et al.* 2010; Duvendack *et al.*, 2011).⁹ These mostly cover quantitative studies on the impact of group lending programmes, although some studies of savings promotion are also included. Studies of other MF services are fewer but growing: see for example Dercon and Kirchberger (2008) on micro-insurance, and Leatherman *et al.* (2011) for a review of evidence on linking MF and health programmes.

- Odell's (2010) review is a non-systematic update of Goldberg's (2005) review, incorporating more recent studies: six RCTs, nine quasi-experimental and two qualitative.
- Stewart *et al.*'s (2010) review was commissioned by DFID. It covers fifteen studies: eleven deemed to be of medium quality and four of high quality. Eleven of these were of micro-credit, two combined credit and savings and two were of savings schemes alone.
- Duvendack *et al.* (2011) is also a systematic review commissioned by DFID. After an exhaustive literature search it used a systematic screening and selection process to select 58 papers for in-depth review. This included two RCTs, nine pipeline studies and 47 difference-in-difference or with-and-without studies. No fewer than 31 of these studies were of MF in Bangladesh, with 21 papers based on the Pitt and Khandker (1998) data set.

The conclusions from these reviews are addressed separately below, as they each provide a slightly different view on overlapping but different sets of literature. Appendix 2 also provides a tabular summary of the three studies.

3.1 “Measuring the impact of microfinance: taking another look” (Odell, 2010)

The review stresses the difficulty of making any generalised conclusions given the heterogeneity of MF interventions, contexts and impact assessment approaches. Despite this caveat, Odell concludes that overall the studies show positive impacts on micro-businesses for both savings and credit MF clients, with the impact on income, poverty, education, health and empowerment being “less clear”.

⁹ Another by 3IE (International Initiative for Impact Evaluation) is due out this year.

3.2 “What is the impact of microfinance on poor people: a systematic review of evidence from sub-Saharan Africa” (Stewart *et al.*, 2010)

This review finds little positive impact of MF on income, with microcredit in some cases having a negative impact. In contrast, it suggests that both microcredit and microsavings services have a positive impact on savings rates, accumulation of assets, expenditure, health and food security. They report no evidence of increased job creation. Evidence of impact on nutrition, empowerment and education was more mixed, but they highlight negative impact on education arising from parents not being able to afford school fees. Their overall conclusion is that as microcredit has the potential for harm, promoting microsavings is a less risky strategy for reaching the poorest.

3.3 “What is the evidence of the impact of microfinance on the well-being of poor people: a DFID systematic review” (Duvendack *et al.*, 2011)

This review takes a tougher line on methodology, casting doubt on the reliability of nearly all impact assessments completed to date. It finds no rigorous evidence for MF impact on increased incomes or empowerment. Where there is impact, both positive and negative, these occur earlier in the causal nexus (see Appendix 1): i.e. affecting micro-business activities more than indicators of wellbeing. Given the focus of most studies on group lending, and given the methodological problems identified, the review avoids making a general conclusion about the impact of the MF sector as a whole. In contrast to Odell (2010), the authors are not willing to give MF the benefit of the doubt, concluding in an unequivocal fashion that *“not only does the evidence presented by the various MF [impact evaluations] not provide robust support for the idea that MF is highly beneficial to the poor, rather than perhaps benefitting a slightly better off group, or being no better than alternative, less hyped, sources of credit, they leave open the question of whether MF is of any real benefit at all”* (Duvendack *et al.*, 2010:95).

what can we conclude and how best to find out more?

4.1 A summary of available evidence

Generalising about the impact of MF is fraught with danger given large variation in types of service, socio-economic characteristics of users, impact indicators, context and the level of methodological rigor demanded by different audiences. We concur with other reviewers in concluding (from theory as well as evidence) that the impact of even the same service will vary widely in different contexts. It should be no surprise, for example, that restoring access to credit for experienced entrepreneurs early in the recovery phase of countries that have experienced severe financial repression and economic stagnation can have dramatic positive effects on their business activity and income. Likewise it should come as no surprise to anyone that microcredit can result in severe over-indebtedness, especially if fuelled by speculative bubbles about the extent of unmet demand, as recently experienced in both the US sub-prime crisis and the crisis of non-banking financial institutions in Southern India. Hence we concur with Odell (2010:12) that “each impact study must be interpreted as a small piece of a growing body of knowledge about how MF works, in all its forms and functions in the world.”

Despite this caveat, we accept that generalisation is unavoidable, if only as a point of departure for more focused investigation, and to this end suggest the following four broad propositions.

- 1. Microcredit on its own cannot be relied upon to deliver sustained income growth and falling poverty rates.** This position is supported by the evidence that methodologically the most rigorous studies have not yielded clear evidence of positive average impact, while studies that have done so are mostly open to criticism on methodological grounds. There also appears to be limited rigorous evidence of positive impact on direct wellbeing measures such as health expenditure and nutrition.
- 2. Evidence of impact on intermediate indicators including business activity, business profitability and asset ownership is generally more positive.** However this in turn has not been shown to increase income or reduce poverty, at least in the short term (with studies over the longer term not yet available).
- 3. Microcredit can be harmful to a significant minority of recipients.** Incentive structures for MF agencies have often encouraged debt capacity (including entrepreneurial flair) of poor people to be systematically over-estimated. Poor people are no different from other people in having very unequally distributed

entrepreneurial flair. Some will prosper but many will have neither the ability nor the inclination to generate sufficient surpluses to pay back commercial loans. Like many richer people, poor people cannot be relied upon to avoid taking on more debt than they can manage, especially those with limited prior experience of financial services.

- 4. There is a scattering of evidence that positive impact on a range of other indicators may be important.** This includes consumption stability over time, intra-household relations, aspirations, and financial capability. Rosenberg (2010a:2) emphasises this by asking “are we looking for impact in the right place?” He goes on to suggest that the most important role of microfinance may ultimately be to enable many poor households to smooth rather than augment consumption, as highlighted particularly by Rutherford (2001) and Collins *et al.* (2009).¹⁰

4.2 Implications for policy and the case for more impact assessment.

In the absence of stronger and more consistent evidence on the impact of MF, donors and social investors can adopt one or a combination of three strategies. The first and most radical would be to cut support until such evidence is available, redirecting it towards activities that can demonstrate greater impact effectiveness. Duvendack *et al.* (2011) come close to taking this position by observing that MF has already had nearly two decades to demonstrate benefits convincingly to uninterested parties, but largely failed to do so. A second option, given continued doubts about the timeliness, reliability and generalisability of so-called rigorous impact assessment, is to give more credence to evidence that people with direct personal experience of MF judge to be sufficiently reliable, on the basis of its congruence with other evidence available to them (as mentioned in section 1.2). On that basis, they could continue with promoting carefully selected and designed

¹⁰ An important supplementary point here is that alternative mechanisms for consumption smoothing such as informal borrowing, forced migration and distress sales of assets can be very expensive. Hence cost-effective consumption smoothing over time should also raise net incomes.

MF programmes where there are good theoretical grounds for believing that past success can be replicated.¹¹

A third option, not incompatible with the second, is to diversify investment into a wider range of MF and related services in a way that generates additional and more reliable evidence from which to proceed. Diversification is already being taking place on a large scale. It includes funding of savings-led and user-controlled financial services, micro-insurance and new mechanisms for facilitating cash payments. In reaction to the backlash against MF the CGAP CEO, Tilman Erhbeck, has reiterated the argument that “donor money remains important in market development, to go into promising yet unknown territory where private money will not yet go. Donor money is able to capitalise the innovations and research and development that microfinance needs.”¹²

That said, it is clearer now than ever before that developing new financial products and services to unmet demands, and with potential to ‘go to scale’ commercially is more complex, costly and risky than many MF enthusiasts have suggested. For every success there have been numerous failures, and continue to be. Greater realism reinforces the importance of more systematic learning, partly through improved evaluation and impact assessment. This is particularly important in current growth areas such as micro-savings and financial literacy in relation to claims that they can reduce vulnerability and counter over-indebtedness. However, as Box 3 highlights, there are powerful incentives why past investment in impact assessment has been inadequate in both quality and quantity. Investment in impact assessment needs to grow with the scale, diversity and importance of the sector.

¹¹ Cartwright (2011) observes that this is most scientific endeavour: theory is “vouched for” by the accumulation of evidence rather than “clinched” by one or two landmark studies. A key concept issue here is what she calls the “capacity” of causal claims to apply in multiple contexts, hence to sustain the conclusion that “it will work for us” rather than just that “it works somewhere”.

¹² <http://www.microfinancegateway.org/p/site/m/template.rc/1.26.15517/>

Box 3: Why investment in impact assessment has been inadequate

1. Incentives for those operating at different levels in the sector have been misaligned towards growth in access to services rather than quality or impact of services.
2. Too many people believed in “the market test” - that use of services itself was sufficient evidence of positive impact – a myth that the 2008-9 financial crisis has done more to explode than any number of studies could have done.
3. Impact assessment is a public good, and investment in MF is increasingly coming from the private and for-profit sector who have less interest in sharing what they learn.
4. There has been a tendency to underestimate the complexity and diversity of MF impact pathways in different contexts and to different people, and hence to over-generalise about expected impacts from standard products (see Appendix 1). The result has been excessive optimism about likely impact and an emphasis on overly standard and simplistic models that are easily scaled up through replication.
5. Divergent incentives and mental models of MF practitioners and specialist researchers have limited the scale and quality of collaboration, when inputs from both are needed.

4.3 What sort of impact assessment to invest in?

This raises a key set of questions about both what form future impact assessment should take, and who should be responsible for it. One strong and influential position here is that only RCT based studies can be trusted. Roodman (2011:42), for example, states that “unless or until randomized microfinance trials show strong average benefits, the most convincing case that can be made for charitably supporting microfinance must rest on grounds less compelling than hard evidence of impact—but also more honest.” However, debate over this is far from settled,

with suggestions that investment in RCT research itself may already have become a somewhat unhealthy speculative bubble.¹³

While RCTs offer a simple way around the selection bias problem that has proved problematic for many quasi-experimental studies they fail to resolve other challenges and also throw up new problems. These have now been quite widely reviewed and debated (e.g. Deaton, 2010; Pritchett, 2009; Imbens, 2009; Heckman and Smith, 1995; Karlan *et al.*, 2009; Armendáriz and Morduch, 2010; Copestake, 2011; Duvendack *et al.*, 2011:63-66). Like all survey-based statistical approaches they generate estimates of average impact across a population and are a blunt instrument for revealing heterogeneous impact. As Armendáriz and Morduch (2010:293) put it “zero may be a clean estimate of the average impact...but it hides the action.” There is also a limited amount of time during which to measure impact before treatment and control groups get contaminated or weakened by sample attrition. Furthermore, unless ‘blinded’ the response of treatment and control respondents differs systematically because of their contrasting status as objects of research. Such differences also raise ethical questions about arbitrarily treating people in different ways, while restricting RCTs only to contexts where such concerns can be addressed causes placement bias. As with other approaches findings are specific to precise treatments and contexts, whose wider relevance is always a matter of judgement. As Cartwright (2011:1400) states, they are ideal for supporting “it works somewhere” claims, but of limited use in supporting “it will work for us” claims.

Ultimately, RCTs offer just one approach to building up a stronger body of empirically informed theory about the relative importance of the diverse causal pathways that link financial services to well-being outcomes in different contexts and for different sorts of people; indeed, even the most ardent advocates of RCTs do not deny the case for other approaches also. This in turn suggests a strong case for more qualitative work also. However, standards for conducting such research in ways that are subject to rigorous review, and can support systematic theorization are even less advanced than is the case for quantitative impact assessments.

¹³ Odell (2010) reports that RCTs are underway in Mexico, Mongolia, Bosnia, Mali and Philippines.

Box 4 sets out a few criteria of what a more systematic approach to qualitative research entails.

Box 4: Characteristics of high quality qualitative research.¹⁴

- Systematic and transparent sample or case-study selection – even more important for ‘small-n’ than ‘large-n’ studies.
- Using qualitative methods to complement systematic monitoring of client welfare indicators, so that findings can be related to wider trends.
- Proper and fully documented pre-testing of research instruments and stronger standards for training of interviewers.
- Systematic and documented cognitive debriefing of interviewers.
- Systematic qualitative data cleaning and analysis, including use of qualitative data analysis software such as NVivo.
- Exposing such work to external validation, audit and review.

Smaller and more flexible studies based on careful interpretation of systematically collected self-attributed impact data can provide faster and more context-specific feedback, and hence do more to strengthen learning, experimentation and improved practice in complex and fast changing environments than a smaller number of larger and lengthier studies.¹⁵ Such an approach can be incorporated into routine

¹⁴ Drawn from discussion of the “qualitative in-depth interview protocol” (QUIP) in Copestake *et al.* 2005a and 2005b.

¹⁵ Herein lies the danger of irrational exuberance over the potential of RCTs: huge resources invested in demonstrating causal links of limited general validity. An interesting case in point is the recent study reported by Desai and Tarrozi (2011). How, we may wonder could funds used to survey 6,400 respondents twice have been used to facilitate more flexible institutional learning from clients?

social performance management of MFIs along with activities designed to ensure compliance with monitoring of client characteristics, customer protection, human resource management, product transparency and so on. But there is also a case for more investment in independent qualitative studies, building on the kind of insights generated by the “Portfolios of the Poor” study. In addition there is a case for more investment in monitoring not only of financial inclusion using national household surveys, but also using them to test hypotheses about causal impact chains, building on some of the ideas set out in Appendix 1.

Conclusion

This paper has outlined the major issues arising from impact assessment of MF at the same time as taking stock of the available evidence. It has largely focused on quantitative assessments and the impact of microcredit, given that this is the focus of most of the available stock of published studies. We have highlighted the main challenges regarding such evidence, which together with the heterogeneity of MF makes evidence-based policy making difficult. We tentatively concluded that microcredit cannot, on its own, be relied upon to deliver sustained income growth and falling poverty rates, and that it can indeed be harmful to a significant minority of recipients. Evidence of impact on intermediate indicators including business activity, business profitability and asset ownership is generally more positive, but this in turn has not been shown to increase income or reduce poverty, not least because of the opportunity cost of time taken up with such activities. However, there is a scattering of evidence of positive impact on a range of other broader indicators of wellbeing, including reduced vulnerability through ability to smooth consumption over time. This suggests a need to broaden the criteria on the basis of which the impact of MF is assessed.

Accepting that the evidence is limited - in scope, quality and generalisability – we have also argued for further investment in impact assessment. More specifically we argued for use of a range of methods, warned against over concentration of research using RCTs in favour of more and better qualitative studies to help fill gaps in our understanding of different causal pathways from MF to wellbeing in diverse contexts.

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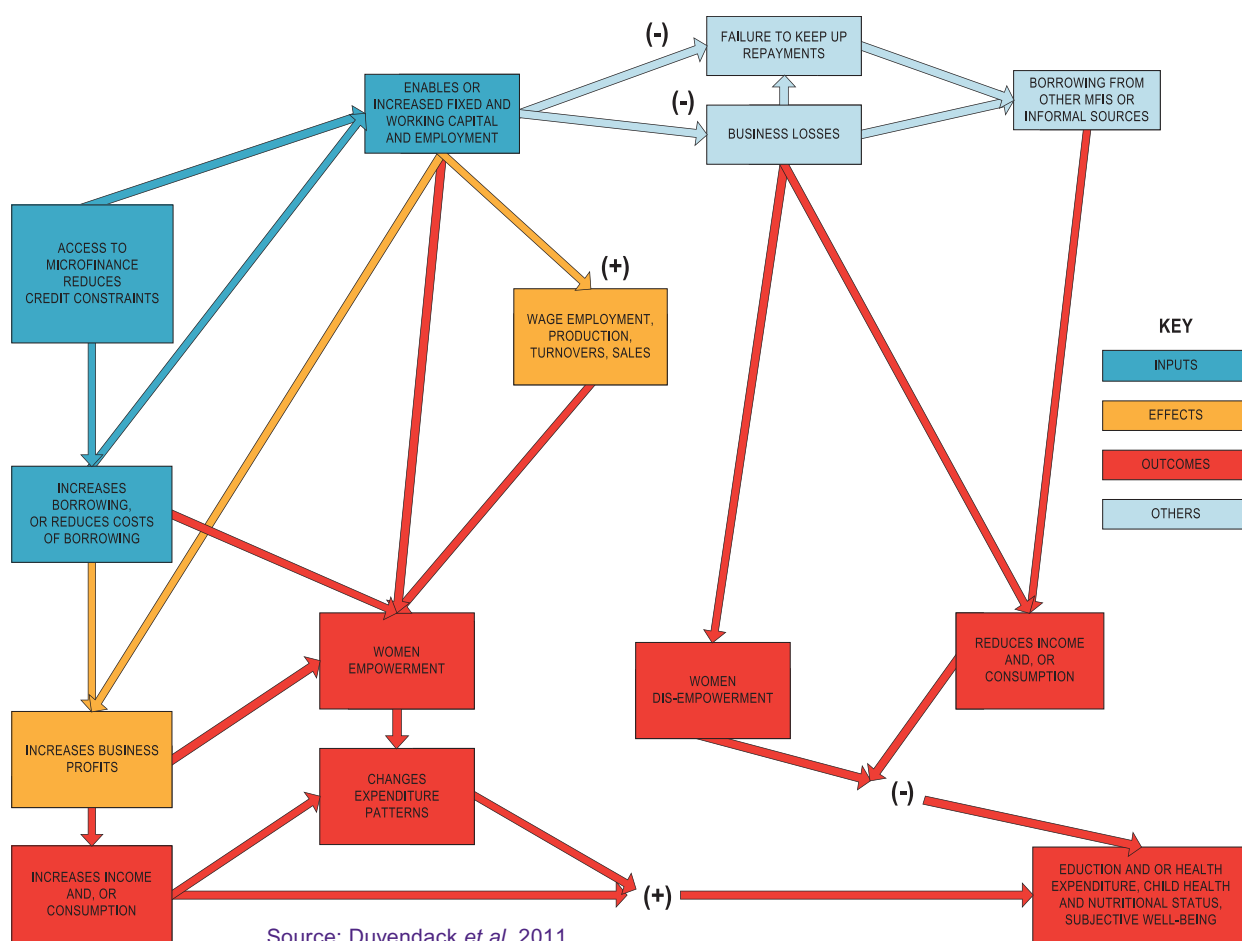
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Appendix 1: Causal Pathways and Wellbeing Indicators

There are many pathways through which effects of MF can impact positively and negatively on indicators of well-being. The diagram below follows a simple explanation of credit (or accumulation of savings) easing capital constraints for a sole borrower allowing productive investment to generate income, profits or outputs. Indeed most research into the impact of credit on income and poverty is framed in this way, with relatively simplistic models that link credit to an exogenous 'treatment' that lead to outcomes through effects on livelihoods and inter personal relations. Figure 1 follows Duvendack *et al.* (2011) in separating MF inputs (blue) which lead effects (orange) which in turn will lead to positive/negative wellbeing outcomes (red).

The simplest theories of microcredit impact assume the borrower is the sole operator of a single income generating activity, the output of which is constrained either by lack of capital or by the high marginal cost of credit relative to its marginal returns. Easing the capital constraint permits the operator to increase output, net income and hence their welfare (de Mel *et al.*, 2008). Their ability to borrow, or debt capacity, depends on the capacity of actual or potential income from the business to meet borrowing costs.

Figure 1: Causal Pathways



A more comprehensive causal framework takes into account that debt capacity is also bound up with business vulnerability, risk and uncertainty. In the absence of insurance services credit not only eases the capital constraints but can serve as a mechanism for spreading risks. For example, access to credit (even if not actually taken up) can raise income by reducing the management of risk through livelihood diversification (Diagne, 2001). Borrowers' imperfect knowledge and limited computational capacity means that new forms of credit may have an important impact on the mental models that guide their business decisions (Nino-Zarazua and Copestake, 2008). More generally, research into the psychology of borrowing

among poor people is fundamentally challenging the cosy neo-liberal assumption that credit is unlikely to do harm on borrowers because if it did then they wouldn't have borrowed in the first place or come back for more loans (Rosenberg, 2010b).

A further complication arises because poor people's management of livelihood related resource allocation, risk and uncertainty cannot be separated from decisions about household reproduction (e.g. Gertler *et al.*, 2009). As a factor in the management of diversified and seasonally volatile "household economic portfolios" (Sebstad *et al.*, 1995) the impact of credit on the cost of consumption smoothing may be as important as its impact on enterprise promotion (Morduch, 1995; Rutherford, 2001; Collins *et al.*, 2009). Because portfolios are co-produced by household members both credit transactions costs and the potential benefits of credit can also profoundly affect intra-household relationships, including the gender division of labour, income and power. Induced changes in social relations inside and beyond the household are also associated with important changes in individuals' aspirations and understanding (e.g. Mayoux, 2001; Johnson, 2005; Hoelvet, 2005).

Since changes in credit relations have direct effects on all aspects of poor people's households (and indeed wider kinship and neighbourhood networks) theoretical pathways can readily be traced, at least in theory, from credit to almost any indicator of individual socio-economic status or human well-being (e.g. Kabeer, 2005). For example, improved access to credit for cash crop production controlled by men may result in reallocation of resources away from food crop production controlled by women with adverse effects on their children's nutrition. Likewise, improved access to credit for women's trading activities raise the opportunity cost of their time with possible adverse impact on child care. Empirical testing of multiple pathways (e.g. using structural equation modelling) is relatively rare, perhaps because the lines of causation are so complex, with many relevant variables having both intrinsic and instrumental value (Sen, 1999). It cannot be assumed, for example, that credit impact is only mediated via its effect on business income: direct relational, attitudinal and cognitive effects on individuals can be equally profound (Chen and Mahmud, 1995). One potential response to this suggested by Scheffer (2009) is to regard the household economy as a complex dynamic system and credit as a variable capable of triggering critical system transitions.

Despite these complications, most research into the impact of credit on poverty continues to be framed by relatively simplistic causal models that link credit as an exogenous 'treatment' on individual borrowers to one or more indicators of well-being mediated via induced effects on household livelihoods and inter-personal relations. An alternative approach (not covered by this paper) is to explore the effect of aggregate changes in financial systems on higher units of social organisation, from villages to national states. For example, credit supply may be treated as a resource constraint on a multi-sector input-output model, with distributional effects on poor people identified through use of a social accounting matrix (e.g. Subramanian and Sadoulet, 1990). Alternatively, simulation models or cross-country multiple regression analysis can be used to explore the link between credit and indicators of national performance such as GDP, which in turn have testable relationships with poverty (e.g. Honohan, 2004). An important example of this approach established positive links between rural credit expansion in India, district level growth performance and associated changes in poverty incidence (Binswanger and Khandker, 1995; Burgess and Pande, 2005).

In summary, the theoretical case for microcredit rests on the potential for joint liability and other innovations by MFIs including individual liability with joint monitoring, to resolve issues such as adverse selection and moral hazard and reduce MFI transaction costs. Mitigating financial intermediation constraints could lead to expansion of economic activities, higher net returns to household assets, and higher income. Furthermore, subsequent theory expanded on positive and negative potential relational, cognitive and attitudinal impact of access to credit.

Higher net returns to household assets may, of course, be goods in themselves, and may also lead to human developments which are income elastic. In so far that credit is successfully targeted on women, it may benefit them specifically, and by enhancing their status and empowering them, and may beneficially affect the pattern of household resource allocation, particularly benefitting children, especially females, at least in some patriarchal societies (Hashemi et al, 1996). These assumptions can be contested on the grounds that improved returns to assets, especially labour power and entrepreneurship, are neither necessary nor sufficient grounds for improvements in health and education developments, may not exist, or may anyway be captured by males (Goetz and Sen Gupta, 1996; Kabeer, 2001, 2005b).

Appendix 2: Summaries of Summaries

Review	No. of Studies reviewed	General conclusion on wellbeing	Future Agenda
		Effects	Outcomes
Odell (2010)	17 Microcredit and savings	(+) microbusiness • business ownership • investment • profits	<p>More understanding needed of;</p> <ul style="list-style-type: none"> • Causal pathways • Links between better business investments and outcomes. • On impact on microsavings • Macroeconomic effects of MF
Stewart (2010)	15 Microcredit and savings	Mixed impact on business wealth. • (-) Microcredit on incomes • (+) Microcredit on incomes • (+) Household accumulation of assets and expenditure. • (+) Vulnerability • (+) Health expenditure • (+) Food security • Mixed impact on education • Mixed impact on empowerment • No evidence of increased job creation.	<ul style="list-style-type: none"> • More caution needed given potential to do harm. • Rhetoric around MF is problematic (too much optimism and a lack of engagement with the evidence). • Engagement with the causal pathways needed for better designs of interventions. • Reflecting microcredit potential harmful outcomes more focus should be given to microsavings and less to trying to reach the poorest groups.
Duwendack et al., (2011)	58 Microcredit (and microcredit + and ++)	Most tests gave some positive effects e.g. some evidence on increased business activities (RCT, Pipeline studies)	<ul style="list-style-type: none"> • Preferences conclusions that there is very little evidence <i>for</i> impact. • Suggests that it may be time to seek alternatives to microcredit.



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