

# Working Paper No. 36

## How Does Microcredit Work? Testing the Theories of Microcredit

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**May, 2015**



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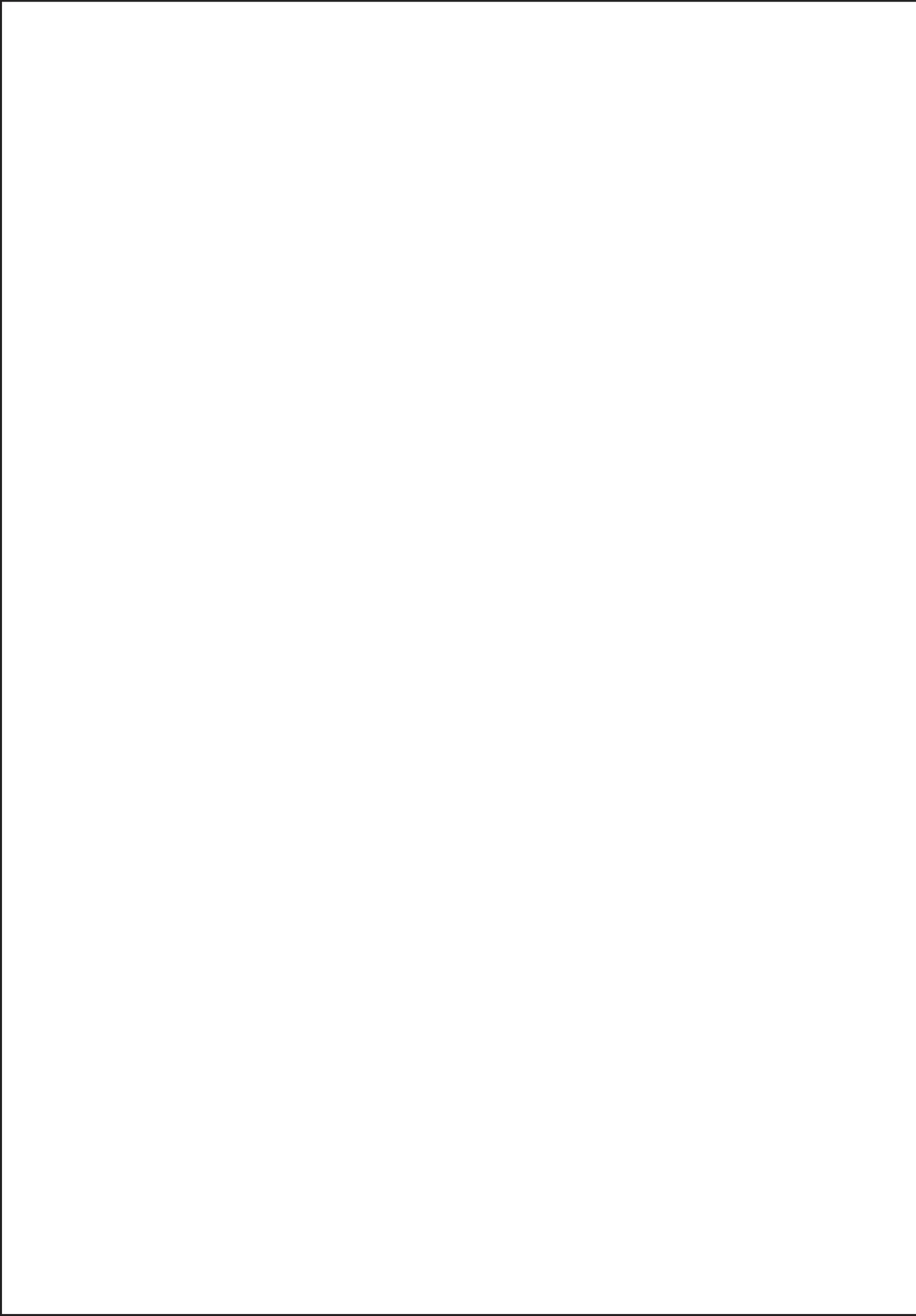
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## Abstract

Economists have used a great deal of ingenuity to delve underneath the practice of microcredit and understand the logic that underlies its operations. There is no single theory, however. What we have instead is a bewildering variety of theories; the only common thread binding most of them is the idea that the way microcredit is delivered in practice helps overcome certain market imperfections – in particular, imperfections in information and in the enforcement of contract. But they differ greatly in their understanding of exactly which imperfections are being addressed and precisely how they are being overcome. Most of these theories have some *a priori* plausibility. Therefore, the only way to discriminate among them is to check empirically which theories seem to fit the reality better than others. Accordingly, researchers have increasingly turned their attention towards testing microcredit theories against empirical data, often using highly sophisticated econometric techniques and increasingly drawing upon the tools of behavioral economics. This is currently a lively area of research and many valuable insights have already been gained. Although a lot more work still needs to be done, it is worthwhile to take stock of the current state of knowledge so that future research can build upon it in a more focussed manner. This is what the present paper purports to do. In particular, the paper examines the empirical evidence on four sets of issues: (a) does the rural credit market really suffer from the kinds of market failure that the theories of microcredit take as self-evidently true, (b) which of the pathways through which, according to theory, microcredit is supposed to overcome market failure are empirically relevant and which ones are matters of mere theoretical curiosity, (c) does the celebrated joint liability mechanism of microcredit models really outperform the traditional models of individual liability, and (d) what do data say about of the role of social capital in making microcredit operations effective?



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# How Does Microcredit Work?

## Testing the Theories of Microcredit

S. R. Osmani<sup>a</sup>  
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### 1. Introduction

Visionary practitioners have created the microcredit revolution and theorists have tried to explain exactly how they did it. But have the theorists correctly captured the essence of practitioners' innovations? This question can only be answered by confronting theory with reality. The answer is not of mere academic interest, though that is important too; it may have important practical implications as well. Since different theories emphasize different aspects of microcredit delivery and offer different hypotheses about how it works, an understanding of which theories are empirically 'relevant' and which are merely 'esoteric' may also help identify and strengthen the truly potent aspects of microcredit practice.

Economists have used a great deal of ingenuity to delve underneath the practice of microcredit and understand the logic that drives its success. There is no single theory, however; not even a unified theoretical framework with multiple extensions. What we find instead is a bewildering variety of theories; the only common thread binding most of them is the idea that the way microcredit is delivered in practice helps overcome certain market imperfections – in particular, imperfections in information and in the enforcement of contract.<sup>1</sup> But they differ greatly in their understanding of exactly which imperfections are being addressed and precisely how they are being overcome. Among the variety of imperfections, some pick out adverse selection for particular attention, some emphasize moral hazard, others focus on strategic default arising from imperfect enforcement, and yet others highlight the imperfections in informal insurance that lead to default even when a borrower is willing to repay. Among the mechanisms through which microcredit is presumed to overcome these problems, most theorists point to joint liability as the prime driver, others emphasize dynamic incentives, and yet others speak for some of the less discussed features such as frequent repayment, public meetings, and so on. Many of the theories emphasize the role of social capital in sustaining the mechanisms that make microcredit work, but they differ on the relative importance they attach to harnessing the existing social capital as opposed to creating new forms of social capital or strengthening the existing ones. They also differ in what they deem to be the most relevant aspect of social capital for the successful operation of microcredit – whether it is people's knowledge of each other, or their sympathy for each other, or their ability to impose social sanctions on the peers, and so on.

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<sup>1</sup> For an analytical review of the theories of microcredit, see Osmani and Mahmud (2015).

It is conceivable that there is some truth in all or most of the ideas that theorists have toyed with, but there still remains the question as to which features of microcredit are more important than others in practice. Moreover, are some features more universally potent than others, or is everything context-specific? Only a wide range of empirical studies, covering different times and places, can answer these questions satisfactorily. Such studies are already underway but there is still a long way to go. Our current state of knowledge has not reached a stage where we can offer definitive answers to most of the questions of interest. But a few tentative answers have begun to emerge to at least some of them.<sup>2</sup> In this paper, we shall present our understanding of the current state of knowledge with regard to the following three sets of issues:

- (a) Which aspect of market failure does the group lending mechanism help overcome most commonly and most successfully, if at all – is it adverse selection, or *ex ante* moral hazard, or strategic default, or inadequate informal insurance?
- (b) Is joint liability really the key to microcredit's success? Or, can individual liability do equally well, if not better, when supported by other features of microcredit delivery (for example, dynamic incentives, or frequent repayment, or public meetings, and so on)?
- (c) Does social capital really play a role in sustaining the microcredit delivery mechanisms that help overcome market failures? If so, which aspects of social capital are most relevant for this purpose?

Before discussing the answers that have emerged so far, we need to address a prior question, however. While the theories of microcredit differ greatly from each other, most of them share the premise that rural credit markets are characterized by pervasive asymmetries of information that lead to pathologies such as adverse selection and moral hazard, and also by the problem of inadequate enforcement. Together, these problems can be described under the common rubric of the 'agency problem'. Before examining whether and how microcredit helps redress the pathologies created by the agency problem, we need to ask: is the premise itself valid? Is there any empirical evidence for the existence of the agency problem or is it all a figment of the theorist's imagination? This question is addressed in section 2. Section 3 examines the evidence on the pathways through which, according to theory, microcredit is supposed to solve the agency problem. Empirical evidence on the putative efficacy of joint vis-à-vis individual liability is reviewed in section 4. Finally, section 5 examines the evidence on the role of social capital in ensuring the success of microcredit.

## 2. Is There Evidence for the Agency Problem in Rural Credit Market?

Economists have tried to look for the evidence but it has not proved an easy task. The very concept of asymmetric information implies that it would be difficult to observe, for if it were not so the asymmetry would not probably have existed in the first place. The challenge is one of 'observing unobservable' as the title of a paper in this field aptly describes (Karlan and Zinman 2009). The only feasible way of going about the task is to check if the observable predictions of

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<sup>2</sup> For previous surveys of the relevant literature, see especially Ghatak and Guinnane (1999), Guttman (2006), Hermes and Lensink (2007), Armendáriz and Morduch (2010), and Karlan and Morduch (2010).

a theory embodying the agency problem can be matched with evidence. For instance, the pioneering work of Stiglitz and Weiss (1981) on asymmetric information in credit market predicts that lenders will not raise the interest rate (or, alternatively, the loan size) beyond a certain level for fear of inducing either adverse selection or moral hazard to an extent that it begins to hurt their profits. Looking at a data set on a large number of microcredit lenders from around the world, Cull *et al.* (2007) have found evidence in support of this prediction.<sup>3</sup>

Three categories of lenders were covered in the data set – namely, those who lend mainly on individual liability basis, those who lend with group liability (the Grameen-type lenders), and those who lend on the basis of village-level group liability (the so-called village banks). For the subset of individual liability lenders the authors have found an inverted U-shaped relationship between interest rate and profitability – as interest rate rises, profitability first rises and then falls, just as the Stiglitz-Weiss theory predicts. The turning point comes at around 60 per cent interest rate, with very few lenders being observed charging above that rate, which is again consistent with theory.

There is, however, a problem with this evidence. In order to claim that we have found evidence for a theory, it is not enough that the predictions of the theory fit the facts. What we need is a 'discriminating test' i.e., a test that will assure us that the observed facts are consistent only with the theory in question and not with any alternative explanation. This is not the case here. As the authors acknowledge, the same inverted U-shaped relationship between interest rate and profitability is also consistent with a simple theory of downward-sloping demand curve for credit. At a high enough interest rate, the demand for credit may become so elastic that further increase in the rate will induce negative profit at the margin. In other words, the observed facts cannot discriminate between the competing hypotheses of agency problem and elastic demand.

Cull *et al.* (2007) then proceed to offer some additional evidence that does take them closer to a 'discriminating test'. They seek to establish a relationship between interest rate and the extent of delinquency as measured by 'loan portfolio at risk'. If the lenders are confronted with the agency problem, higher interest rate should lead to greater delinquency through either adverse selection or moral hazard, and this is also what is observed (up to a point, which is about 40 per cent interest rate). This is more of a discriminating test because neither the theory of demand nor any other known theory predicts a positive relationship between interest rate and delinquency.<sup>4</sup>

Once the existence of the agency problem is accepted, there arises the even trickier problem of trying to ascertain exactly what kind of agency problem exists – in particular, is it mainly a problem of adverse selection or of moral hazard or both, and if it is both which is more important? The kind of evidence Cull *et al.* presented on the positive relationship between

<sup>3</sup> The data set covers 124 microfinance institutions (MFIs) in 49 developing countries and was collected by the Microfinance Information Exchange (MIX), a not-for-profit private organization that aims to promote information exchange in the microfinance industry. These data, collected for publication in the MicroBanking Bulletin (MBB), were adjusted by the authors to help ensure comparability across institutions.

<sup>4</sup> The test is not fool-proof as the relationship between interest rate and delinquency turns negative beyond about 40 per cent interest rate, which is not consistent with agency theory.

interest rate and delinquency (or, alternatively between loan size and delinquency) is consistent with both an adverse selection explanation and a moral hazard explanation. The empirical task, therefore, becomes even more challenging if one tries to identify the precise nature of the agency problem, let alone ascertain the relative importance of different types of agency problem.<sup>5</sup>

In the jargon, this type of challenge is known as the 'identification problem' – a term we shall come across rather frequently throughout this paper. In essence, it refers to the problem of isolating the causal effect of one or more factors from that of others that might work in tandem to influence a particular phenomenon (in this case, the default rate). The problem becomes especially serious when some of the causal factors cannot be easily observed or measured, as is the case with adverse selection and moral hazard. Researchers try to devise an 'identification strategy' that will credibly isolate the effects of the causal factors of interest. Quite often, however, success in finding this strategy depends on the nature of data. Either there must be something special in the data that the researchers can 'exploit' for the purpose of identification or the right kind of data must be constructed with the help of 'experiments'. There are examples of both these approaches in the present case.<sup>6</sup>

An example of the former type is the study by Klonner and Rai (2007) who tried to ascertain the existence of adverse selection in the credit market by looking at the operations of a ROSCA (Rotating Savings and Credit Association) in India. There are various types of ROSCAs, and the particular one this study considered is known as a bidding ROSCA. Every month the members contribute to a pool of funds which goes to the highest bidder; the higher the bid the higher is the effective rate of interest the winner has to pay. Previous winners are barred from bidding in the subsequent auctions until everybody has had a chance to win the common pot, and then the process may start all over again. The way the system works, the early winners end up paying higher effective rates of interest compared to the later winners.<sup>7</sup> This means that in the ideal world the early winners would be those who have a higher willingness to pay for the privilege of using the common fund. However, in the real world the early winners may actually be the more 'risky types', who according to the theory of adverse selection, are more tempted by high interest rates compared to the 'safe types'. Thus if adverse selection exists one would expect to find a risk profile where early winners are riskier and the late winners are safer.

But suppose the effective interest rate is capped at some relatively low level, which can be done by putting a ceiling on how much one can bid (recall that the effective interest rate varies positively with the amount of the winning bid). Then almost everybody will want to bid up to the highest permissible level, and the allocation will have to be done by some other mechanism, say, a lottery. In that case, the risk profile will be random; the previous pattern of early winners being characterized by more risk will disappear. Thus a comparison of the risk profile before and after the imposition of the ceiling will give a clue to the existence of adverse selection. With

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<sup>5</sup> Ausubel (1999) offers an early discussion of this challenge in the context of the credit market. Chiappori and Salanie (2003) and Finkelstein and McGarry (2006) do so for the insurance market.

<sup>6</sup> For attempts to isolate the effects of adverse selection and moral hazard in other markets, see Chiappori and Salanie (2000) on the insurance market, Cardon and Hendel (2001) on health insurance, Edelberg (2003) on consumer loan market and Shearer (2004) on labour contracts.

<sup>7</sup> Klonner and Rai (2007) give a lucid explanation of why this is so, with the help of numerical examples.

adverse selection, the two risk profiles will differ systematically; without adverse selection there will be no systematic difference between the two. This is the 'identification strategy' adopted by Klonner and Rai. For this strategy to work of course a ceiling on the winning bid must be imposed by some exogenous source. In this case, the imposition came from an edict of the Indian Supreme Court, which restricted the winning bid up to a maximum of 30 per cent of the common pool in 1993 and then relaxed it in 2002. Both the imposition of the ceiling and its subsequent relaxation gave the authors an opportunity to apply their identification strategy (of comparing the risk profiles of early and later winners). They did find systematic differences in the risk profiles before and after these events, which attest to the existence of adverse selection.<sup>8</sup>

Another example of identifying the elements of asymmetric information by 'exploiting' a special feature of the data set is a recent study of payday loans in the USA by Dobbie and Skiba (2013).<sup>9</sup> The study 'exploits' the fact that the amounts of loan offered by the lenders are a discontinuous function of net pay. The sample firms offer loans in \$50 increments, up to but not exceeding half of an individual's net pay. This practice gives rise to several loan eligibility cut-offs around which very similar borrowers are offered different sized loans. The authors' identification strategy consists in comparing the average level of default for individuals earning just above and just below these cut-off points.<sup>10</sup>

Normally the agency problem will manifest itself in higher loan sizes being associated with higher default rates. This could happen through a combination of adverse selection and moral hazard, and without further information it will not be possible to separate the two. Fortunately, such separation becomes possible when there are discontinuities of the kind noted above. The discontinuities ensure that a borrower with income just above the cut-off point will get a much bigger loan compared to the borrower with income just below the same cut-off point. But this difference in loan size is not something that the borrowers voluntarily 'chose' to have; it was imposed on them by an exogenously imposed cut-off. As such, there is no reason to believe that a risky borrower chose a higher loan size and a safe borrower chose a smaller one, as would happen in the event of adverse selection. In other words, the possibility of adverse selection is ruled out if one compares the repayment performance of borrowers just below and just above the cut-off point. If one nonetheless finds that those who took out bigger loans have higher

<sup>8</sup> This is a drastically simplified account of what Klonner and Rai actually did to identify the existence of adverse selection, but it gives the essence of their strategy. The actual statistical work was made complicated by the fact that they had to empirically estimate the risk profiles of the winners (since 'riskiness' is not directly observable), and the process of estimation had to ensure that the 'observed' risk profiles truly reflected the innate riskiness of winners instead of being contaminated by other factors such as moral hazard. One implication of this procedure is that while the authors were able to identify the existence of adverse selection, after controlling for the possible effect of moral hazard, they were not able to infer anything about the existence of moral hazard itself.

<sup>9</sup> The system of payday loans (short-term unsecured loans to be paid back on the pay day) in the United States may seem far removed from the microcredit sector of the developing world, but there is actually one fundamental similarity between the two – in both cases borrowers have very little access to the formal banking sector due mainly to the high transaction costs of small loans and potential agency problems stemming from the absence of collateral.

<sup>10</sup> Technically, this strategy is called the 'regression discontinuity' approach to identification; we shall come across other examples of this approach later in the paper.

default rates (after controlling for observable personal characteristics), this must be due to moral hazard. This is how it becomes possible to separate out the effect of moral hazard on the default rate.

Furthermore, since the difference in the default rates of large and small loans in the overall sample (not just those around the cut-offs) reflects a possible combination of moral hazard and adverse selection, one can also obtain a separate estimate of adverse selection by subtracting the effect of moral hazard from the overall difference. By following this procedure, the authors estimate that borrowers who choose \$100 larger loans were 6.0 to 7.8 percentage points more likely to default – on account of adverse selection alone – than observationally equivalent borrowers who choose smaller loans.

The existence of adverse selection in the credit market is thus credibly established by Klonner and Rai (2007) for an Indian ROSCA and Dobbie and Skiba (2013) for the US payday loan market. But there are not enough studies of this kind to permit a broad generalization about the existence of adverse selection (or of moral hazard, for that matter) in the credit markets around the world where microcredit has spread in the recent years. The problem is mainly methodological. As we have seen, identification of adverse selection or moral hazard requires the existence of some special features of the data set – for example, an exogenously imposed cap on the winning bid as in the case of Indian ROSCA or exogenous eligibility cut-offs as in the US payday loan market. Most real world data on the credit market are not blessed with such special features which researchers can ‘exploit’ for the purpose of identification. As an alternative, some researchers have turned to generating the right kind of data with the help of experiments.

The use of experimental data has become increasingly common in the field of research on microcredit, as in economics generally. Using a terminology due to Harrison and List (2004), experiments in economics can be classified into three categories: laboratory experiments, framed field experiments, and field experiments. In laboratory experiments, non-standard subjects are put through some tasks to simulate people’s behaviour in real world situations; in field experiments, the subjects are actual agents whose behaviour is to be analysed but their choices and options are constrained in a manner designed by the researcher to allow credible inferences about causal connections; and framed field experiment is a kind of hybrid which uses non-standard subject pools but adds a “field context” familiar to the subjects to the task undertaken in the experiment. All three types of experiments are found in the research on microcredit and some of these will be discussed later in this paper. For the moment, we look at a study by Karlan and Zinman (2009), which used a novel field experiment in South Africa to isolate the effects of adverse selection from moral hazard.

The experiment was conducted among the clients of a South African finance company that typically lends at 200% per annum and involved two stages. In the first stage, offers of loans were sent out through mail-order to a large number of clients who were randomly assigned three alternative interest rates. In the second stage, those who accepted the initial mail-order offer were given a contract but at this stage an element of surprise was introduced by randomly offering to a subset of the clients a smaller interest rate ( $r^c$ ) than the original offer, while the remainder was offered the original rate ( $r^0$ ). Those who accepted the contract at this stage were

also offered two randomly assigned interest rates on future loans ( $r^f$ ) in order to test for the effect of dynamic incentive. The design thus produced a set of borrowers who selected in at identical rates but then faced different repayment incentives in the future, and another set of borrowers who selected in at different rates but then faced identical repayment incentives. It is this particular aspect of the experimental design that permits separation of the effects of adverse selection from that of moral hazard.

In the first stage, the individuals decided whether to take up the solicitation's offer rate ( $r^0$ ) which could be 'high' or 'low'. The subset of clients who took up the offer at the high  $r^0$  then splits up into groups: those who were randomly surprised with a new lower contract interest rate  $r^c < r^0$  (group A) and those who continued to receive the high rate  $r^c = r^0$  (group B). The members of groups A and B thus self-selected at the same rate of interest, but subsequently faced different repayment incentives. Self-selection at the same interest rate ensured that the two groups should not differ systematically in their average risk profile; therefore, any difference in their repayment performance (after controlling for the effects of observable personal characteristics) would be a consequence of moral hazard alone i.e., the result of differential incentives for repayment stemming from two different contract rates.<sup>11</sup>

Having thus identified moral hazard, the effect of adverse selection was then identified by noting that those who received the lower contract rate ( $r^c$ ) did not all receive the same initial mail offer ( $r^0$ ). Some of them were randomly offered a higher rate and some were offered a lower rate. Thus the defining characteristic of this sub-sample is that the clients self-selected at different offer rates but subsequently they all faced the same contract rate. Since the *ex post* repayment incentive was the same for all members of this sub-sample, any difference in their default rates (after controlling for the effects of observable personal characteristics) must be due to differences in their innate riskiness – this is the measure of the selection effect.<sup>12</sup>

The author's empirical results indicate weak evidence of selection effect and strong evidence of moral hazard. A rough estimate suggests that moral hazard explains perhaps 13 per cent to 21 per cent of default in their sample.

### 3. Identifying the Pathways of Joint Liability

Although the evidence is limited and the findings are somewhat mixed, the preceding discussion shows that the agency problem does exist in the credit market and that the problem manifests itself in the form of both adverse selection and moral hazard. Different forms of the agency problem may not be equally strong everywhere, but they do seem to exist. This establishes at least a *prima facie* case that the theories of microcredit, which purport to show

<sup>11</sup> Moral hazard was further identified by comparing the repayment behavior of borrowers who both selected in and contracted at identical rates, but face different dynamic repayment incentives from randomly assigned future interest rates if that are conditional on repayment of the initial loan.

<sup>12</sup> Karlan and Zinman note that this difference in default rates may reflect not just the differences in the hidden innate riskiness of the clients but also their hidden differential propensity to exert effort. Accordingly, they avoid the term 'adverse selection' in this context, which strictly speaking refers to differential riskiness, and instead uses the more general term 'hidden information', which is also commonly used in the literature on the economics of information. We use the term adverse selection loosely here to imply the effect of hidden information, and also use the term 'selection effect' synonymously.

how its various features, and especially joint liability, help overcome these problems has some broad relevance in reality. But this still leaves open the question: which kind of agency problem do microcredit practices in general, and joint liability in particular, are able to overcome in practice? Are they more successful in dealing with the problem of adverse selection, or with moral hazard, or with strategic default, or the imperfections in informal insurance? In other words, what exactly are the pathways through which joint liability works in practice, if at all?

One way of addressing this question is to set up a competition among alternative theories of microcredit embodying different pathways of joint liability and check whose predictions fit the data best. If, for example, one observes the data for some micro-lenders who practice joint liability and finds that the predictions of Ghatak's model of adverse selection fit the observed data better than, say, the Stiglitz-Weiss model of moral hazard, one could conclude that joint liability is better at removing adverse selection than moral hazard. In a classic paper, Ahlin and Townsend (2007) adopted precisely such an approach.

They empirically compared four canonical models of microcredit against data from rural Thailand. Of these four, two are moral hazard models - namely, those of Stiglitz (1990) and Banerjee *et al.* (1994); one is a limited enforcement (or strategic default) model, due to Besley and Coate (1995); and the final one is the adverse selection model of Ghatak (1999). The authors were able to compare these models empirically by exploiting the fact that under a common set of assumptions these models yield different predictions about the repayment rate. For this purpose, they analyzed the repayment data of a large sample of borrowers served by the Bank of Agriculture and Agricultural Cooperatives (BAAC), the largest microfinance institution in Thailand. But first, to ensure comparability, they had to extend and modify the standard models so as to bring them under a common set of assumptions. Once this was done, a number of differences emerged in their testable predictions.<sup>13</sup>

First, the models differ in the implications of raising the joint liability payment. Higher joint liability payment raises the gain from monitoring and hence raises repayment in the moral hazard model of Banerjee *et al.*, but reduces repayment in Ghatak's model of adverse selection by screening out more of the safer borrowers (if interest rate is kept fixed).

Second, the models also differ in the consequences of the presence or absence of co-operation among group members. In Stiglitz's model of moral hazard, the absence of co-operation leads to the adoption of more risky projects and thus reduces the repayment rate. By contrast, in the Besley-Coate model of strategic default, non-cooperation is consistent with carrying out the threat of penalties on a defaulting group borrower; so repayment rates are higher under non-cooperation.

Finally, the models differ in their implications of the assumption that the project returns of the borrowers are correlated with each other. Theory predicts that positive correlation in project returns can be a negative force for repayment in the Besley-Coate model, as it increases the possibility that project returns will be low at the same time, which might induce mass default. But positive correlation in returns is a positive force in Stiglitz and Ghatak.

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<sup>13</sup> For details of the model extensions, see Ahlin and Townsend (2002, 2007).

These differences in testable predictions about repayment rates allowed Ahlin and Townsend to test the relative validity of alternative models. Perhaps, not surprisingly, they found that no single model fits the data best. Instead, different models seemed to work better under different physical and socio-economic environments. Thus,

- The strategic default model of Besley and Coate fits best in the relatively poor and semi-arid Northeast.
- The Ghatak and Stiglitz models tend to fit best in the relatively well-developed central region, in predictions about screening and to a lesser extent about the covariability of returns.
- The monitoring prediction of Banarjee *et al.* fits well in the prosperous central region.
- Finally, the predictions of Banarjee *et al.* and Besley-Coate models that cooperation would lower repayment is evident in most cases, but the positive prediction for the Stiglitz model comes in quite strongly in the central region.

These findings seem to suggest the general conclusion that joint liability may be better at dealing with strategic default in low-infrastructure areas, while it tackles information problems (in particular adverse selection) better in more developed areas. There exists, however, a subtle difficulty in drawing this conclusion too firmly. The empirical tests conducted by Ahlin and Townsend are in the first instance tests of the models, as modified by them. But they cannot necessarily be treated as tests of the pathways of joint liability as embodied in these models. The problem is that the models' predictions are conditioned as much by the workings of joint liability as by many other auxiliary assumptions that are also embedded in the models; and it is not easy to judge whether the differences in predictions are driven by joint liability as such or by its interactions with the auxiliary assumptions.<sup>14</sup>

The only way to get around this problem is to employ model-independent tests. In other words, one could directly try to find out whether the possible pathways of joint liability lead to better repayment performance. For example, instead of asking whether a joint liability model embodying peer monitoring yields predictions about repayment that fit data better than the predictions from alternative models, one could simply ask: is there any evidence that peer monitoring improves repayment performance among microcredit borrowers? Of course, this approach is not without its challenges either. In the first place, one will have to find credible measures of peer monitoring. Second, one will have to disentangle the effects of peer monitoring from those of other factors that may also have a bearing on repayment performance. Finally, further complications will arise if one is interested in learning not just whether peer monitoring works but also whether it works better than other pathways such as self-selection, peer support, etc.

Of the growing number of studies that have tried to identify the empirically relevant pathways of

<sup>14</sup> The authors themselves acknowledge as much: "The evidence is not direct evidence of a given impediment to trade. Rather, it is evidence about how well a model that features a given impediment to trade does in explaining repayment data. In this context, lack of evidence for a given model may be due to its featured impediment to trade being less important or to its auxiliary assumptions failing to hold." (Ahlin and Townsend 2007, footnote 3, p.F13.)

joint liability, some have met these challenges better than others. We shall first consider what these studies have to say about the empirical significance of self-selection in joint liability lending as emphasized in the models of Ghatak (1999, 2000) and others, and then examine what we have learnt so far about how successfully joint liability induces peer monitoring and peer pressure to avert *ex ante* moral hazard à la Stiglitz (1990) and *ex post* moral hazard à la Besley and Coate (1994).

### ***The Pathway of Peer Selection***

In one of the earliest studies to undertake a systematic empirical enquiry of the pathways of joint liability, Wenner (1995) found strong support for the role of self-selection. Using a survey among joint liability groups of a well-known microfinance institution called FINCA (Fundació Integral Campesina) in Peru, he carried out an econometric analysis of the determinants of repayment performance with a special focus on the role of selection. Two explanatory variables were used for this purpose: a variable that reflected whether the borrowers are screened on the basis of reputation at the stage of group formation, and another variable to capture whether the groups have a written code of conduct that members are expected to adhere to after the group is formed. Both variables are deemed to proxy the selection effect, the latter being taken as a stronger mechanism of self-selection than the former, and both variables were found to have a negative effect on loan default after controlling for other factors.

Wenner takes this as evidence in support of joint liability's success in inducing self-selection that weeds out risky borrowers and thus improves the repayment performance of the group. But the inference may not be that straightforward because the variable representing the presence of code of conduct can be given multiple interpretations. Wenner justifies its interpretation as a selection variable as follows: "While informal screening according to reputation may be seen as a porous device where in social customs, kinship, friendship ties may or may not result in group of truly creditworthy worthy individuals, the existence of a written code be seen as a formal device that sets a uniform minimum standard for membership selection. Thus, the written code can induce self-selection." (p.270) While the argument sounds plausible, it does not rule out the possibility that the code of conduct could also facilitate peer monitoring and peer pressure, leading especially to the kind of co-operative behavior envisioned in some of the moral hazard models.<sup>15</sup> In that sense, Wenner is not actually able to disentangle the different pathways through which joint liability is supposed to work.

In their study of group lending in rural Bangladesh, Sharma and Zeller (1997) also carried out an econometric analysis of the determinants of repayment performance, and included an explanatory variable to indicate whether the group self-selected themselves or were formed by the lenders. The groups that formed on their own were found to have a better repayment performance, after controlling for other factors. This result would seem to attest to the power of joint liability to mitigate adverse selection, when the borrowers are given an opportunity to

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<sup>15</sup> It is instructive to note that in his study of repayment performance in Malawi, Zeller (1998) also included a variable representing the presence of a code of conduct and found a positive impact on repayment as did Wenner, but he did not interpret it as a selection variable. Instead, he expected that "...such rules can increase transparency and therefore reduce intragroup frictions and costs of coordination..." (p.617), which is consistent with our alternative interpretation.

self-select, but the inference is not beyond doubt. What the results directly indicate is that self-selected groups perform better than lender-formed groups, but it does not say anything about why they do so. Avoidance of adverse selection, i.e., screening out of risky borrowers is indeed one possibility, but it's not the only one. It is also possible that self-selected groups have enough social capital among amongst themselves to be able to monitor each other better so as to avoid *ex ante* moral hazard, or to be able to exert peer pressure better so as to avoid *ex post* moral hazard (strategic default), or to be able to provide peer support better so as to avoid genuine default at times of distress. Thus, once again, the mere finding that self-selection has a positive influence on repayment does not enable us to identify the empirically relevant pathways of joint liability.

For such identification to be possible, a minimum requirement is the effects of alternative pathways should be accounted for in the analysis so that their independent effects can be separated out. In very different ways, this was done by Gomez and Santor (2003) and Simtowe and Zeller (2006).

Gomez and Santor (2003) studied a sample of clients of the microfinance institution called Calmeadow in Canada which offers both group and individual loans. The starting point of their analysis is a regression of default on a dummy variable representing whether a borrower belongs to a group or not and a long list of control variables. The coefficient of the dummy variable will reflect whether group lending yields superior repayment performance than individual lending. If it does, the coefficient will capture the overall effect of all the pathways through which group lending helps reduce default. In other words, the initial regression on its own cannot disentangle the pathways of joint liability - in particular, it cannot distinguish between the selection effect that mitigates adverse selection and the incentive effect that mitigates moral hazard. In the next step, however, the authors estimate another regression with the same explanatory variables by using a statistical technique called 'propensity score matching' (PSM) that under certain conditions removes the possible effect of self-selection.<sup>16</sup> If the two regressions yield identical estimates of the coefficient of group lending, one would conclude that the selection effect does not exist; otherwise, it does. The authors found that the coefficient is actually reduced by some 20 per cent in the second regression, which suggests that selection effect was indeed operating to help reduce default.

A rather different approach was adopted by Simtowe and Zeller (2006) in their study of group lending in Malawi. Unlike Gomez and Santor, they only considered clients belonging to groups and tried to explain differences in moral hazard found in the groups' behavior rather than differences in their repayment performance.<sup>17</sup> In the econometric analysis, the explanatory

<sup>16</sup> Essentially, this technique tries to create a counterfactual in which the two groups of borrowers are endowed with the same kind of innate attributes, including risk attributes. This ensures that if self-selected groups tend to be 'safer' than the average borrowers because of the screening out of risky borrowers, the counterfactual group of exogenously selected borrowers will also be equally 'safer'. Therefore, there should not any difference between the performances of the two groups on account of the selection effect. As a result, any difference found between the two groups' performance from PSM-based regressions can be attributed to factors other than self-selection.

<sup>17</sup> Although the authors motivated their paper by referring to the preponderance of *ex post* moral hazard (strategic default) in Malawi, their analysis defined moral hazard more broadly to include aspects of *ex ante* moral hazard (choice about the use of funds) as well: "The incidence of moral hazard in each credit group was captured by asking the chairperson of each group about whether some members had defaulted willfully, or whether they had misused loan funds that were meant for an investment." (p.15)

variables included separate variables for peer selection, peer monitoring and peer pressure, in addition to a number of control variables. They found that groups that were formed through peer selection suffered from less moral hazard compared to groups formed by outsiders. This result may seem similar to that found by Sharma and Zeller (1997) for Bangladesh, but there is a crucial difference. Since, the present study includes separate explanatory variables to capture peer monitoring and peer pressure, one can interpret the selection effect in this study as capturing mitigation of adverse selection with much greater confidence than in the case of Sharma-Zeller study.

None of these studies, however, directly tests for the mitigation of adverse selection – it is at best an inference drawn from the observed effect of self-selection on either repayment behavior or borrowers' choice of the use of funds. According to theory, adverse selection is avoided through assortative matching whereby borrowers with similar riskiness come together in a group, and in particular safe borrowers form groups with other safe borrowers. A direct test would therefore require learning something about the 'riskiness' of members of endogenously formed groups.

In their study of group lending in Georgia, Kritikos and Vigenina (2005) tried to extract this information by asking group members how he or she evaluated the riskiness of the business projects of his or her fellow group members, on a scale ranging from "all businesses were quite risky" to "all businesses were quite safe." This variable, which the authors denote as 'group quality', was found to be positively and significantly related to the respondent's own risk quality, suggesting the presence of assortative matching.

Giné *et al.* (2010) have tried to investigate the nature of risk matching in a 'framed field experiment' in urban Peru. Working with a group of subjects drawn from actual micro-entrepreneurs (potential microcredit borrowers), they carried out a number of laboratory experiments to ascertain the effects of various lending mechanisms.<sup>18</sup> In one segment of these experiments, some subjects were randomly given the opportunity to form groups endogenously while others were denied this opportunity. The purpose was to check whether the endogenously formed groups exhibit risk profiles consistent with the hypothesis of assortative matching. The assessment of risk profiles was made possible by the fact that background information about the subjects and a lottery game had already enabled the authors to grade the subjects on a scale of riskiness. When the subjects were given identical joint liability contracts, it turned out that the endogenously formed groups did exhibit assortative matching, i.e., safe subjects tended to associate with other safe subjects, thus validating theory.

Yet, the issue is not fully resolved. Apart from the legitimate question one may ask about whether laboratory environment alters people's behavior<sup>19</sup>, one must also take note of the fact that quite a few studies arrive at the opposite conclusion – namely, that self-selected groups often tend to be heterogeneous in risk attributes.

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<sup>18</sup> The experiments of Giné *et al.* had in fact a much larger scope and one of its main objectives was to judge the relative merits of joint and liability lending. We shall discuss the findings of these experiments in greater details below when we take up the evidence on this comparison.

<sup>19</sup> This is an important issue in experimental economics; see, for example, the insightful discussion in Levitt and List (2007).

One of the earliest empirical studies to find this apparent paradox is that of Zeller (1998). In an analysis of group lending in Madagascar, he came up with two rather startling conclusions. First, self-selection does not necessarily lead to homogeneous matching in terms of risk attributes, as measured by the riskiness of projects adopted.<sup>20</sup> Second, risk-heterogeneous groups are actually superior in terms of repayment performance. Although all groups were endogenously formed, both heterogeneous and homogenous groups were found in practice. Superior performance of heterogeneous groups was confirmed by econometric analysis of determinants of repayment performance, which showed that variability in the riskiness of assets held by group members was positively associated with repayment. Zeller explained the superior performance of heterogeneous groups in terms of the advantage of risk pooling among members with projects of different degrees of riskiness: "The results therefore indicate that heterogeneity of asset holdings among members, and related intra-group diversification of on- and off-farm enterprises, enables members to pool risks so as to better secure repayment of the loan." (p.618) The underlying hypothesis is that when some borrowers face shocks, variability in asset types within the group ensures that not everybody will be faced with the shock at the same time; so through a form of mutual insurance the 'lucky' members will be able to help out the 'unlucky' ones, thereby keeping up the repayment rate for the group as a whole.

It was precisely on this basis of this kind of argument involving risk-pooling and mutual insurance that Sadoulet (2000) built a theoretical model challenging the prediction of assortative matching à la Ghatak (1999, 2000) and suggesting instead that heterogeneous risk-matching will be the equilibrium outcome under plausible assumptions.<sup>21</sup> In a companion paper, Sadoulet and Carpenter (2001) went on to test the prediction of the theory by using data on group lending from Guatemala. Their findings were identical to those of Zeller, namely, that self-selection does not necessarily lead to homogeneous risk-matching and that joint liability induces mutual insurance within heterogeneous groups, but their empirical methodology was much more sophisticated. One difficulty with accepting the Zeller study at its face value is that the mere existence of heterogeneous groups does not invalidate the hypothesis of assortative matching. The reason is that even when assortative matching is the equilibrium outcome, heterogeneous groups may emerge due to various kinds of matching friction.<sup>22</sup>

The matching frictions theory states that homogeneous matching only holds in a frictionless world, and that all heterogeneity comes from matching frictions. Therefore, the hypothesis of assortative matching can be challenged only if the extent of heterogeneity found on the ground is deemed to be more than what can be expected on account of frictions alone. The methodological challenge is to figure out how to judge whether the observed heterogeneity is

<sup>20</sup> The projects undertaken were primarily agricultural. Those who had plots in the rain-fed upland, where returns were highly variable, depending on the vagaries of nature, were counted as having 'risky' projects, while those having plots in the irrigated low land were deemed to have 'safe' projects.

<sup>21</sup> See the discussion in Osmani and Mahmud (2015).

<sup>22</sup> "These frictions include limited partner availability, informational problems that limit borrowers' monitoring ability, social codes restricting enforcement sanctions and characteristics that impede borrowers' credibility in promising or requiring transfers." (Sadoulet and Carpenter 2001, p.3) In the general literature on equilibrium matching, the implications of market imperfections and other matching frictions have been examined, for example, by Kremer and Maskin (1996), Legros and Newman (2002) and Shimer and Smith (2000).

consistent with matching frictions or not. Sadoulet and Carpenter devise a novel methodology to meet this challenge. First they demonstrate analytically that if frictions were the only reason for heterogeneity, there would exist no systematic relationship between borrowers' first best choice of risks (i.e., the amount of risk that would be taken in the frictionless world) and the observed risk pattern - any deviation from the first best would be purely random. Therefore, if the deviation is found to be systematic, it would indicate the existence of heterogeneity over and above what can be explained by frictions alone. In other words, it would indicate that borrowers have actually chosen heterogeneity as an equilibrium outcome.

While this argument suggests what would constitute a valid empirical test of heterogeneity, there remains the empirical challenge of finding credible measures of first best risks, which are by definition not observed. The authors devised a methodology for obtaining such measures from observed data and implemented it to data on group lending in Guatemala. They found evidence in support of their hypothesis that borrowers consciously self-select into heterogeneous groups as an optimum choice; it's not just a matter of frictions. Following exactly the same methodology, Lensink and Mehrteab (2007) arrive at the same conclusions for group lending in Eritrea.

The logic of heterogeneous risk matching is very different from that of assortative matching. Both are supposed to be induced by joint liability and both are expected to contribute to better repayment performance, but for very different reasons. Assortative matching is a mechanism for mitigating adverse selection and it improves repayment performance by screening out risky borrowers. By contrast, heterogeneous matching is a mechanism for risk-pooling and it improves repayment performance by inducing mutual insurance that prevents genuine default when borrowers face negative shocks. In other words, while both types of matching are induced by peer selection, the distinctive feature of heterogeneous matching is that peer selection is followed by peer support for mutual insurance. Therefore, one way of judging the prevalence of heterogeneous matching is to observe how widespread the phenomenon of peer support and mutual insurance is among self-selected groups.

There is indeed widespread evidence of the existence of peer support and informal insurance within self-selected groups. To cite a few<sup>23</sup>, Wenner (1995) found such evidence for Costa Rica. He noted that while 75 per cent of the groups faced repayment difficulties because of adversities faced by individual members, group delinquency (non-repayment or delayed repayment) was kept down to less than 50 per cent through peer support. In a study of women's groups in Burkina Faso, Paxton *et al.* (2000) observed that group was the main source of funds that the women relied on in the face of adversity. In bad times they rarely borrowed from their husbands, families, or other friends. Some indirect evidence also comes from Rai and Klonner's (2007) study of cosigned loans in India. Faced with the evidence that cosigning helps improve repayment, the authors tried to identify the pathways through which the improvement occurs. In particular, they tried to distinguish between the cosigners' role as a monitoring device and their role as an insurance device, and found evidence in support of the latter. Although this evidence relates to cosigning, which is a different mechanism from the ones employed by most group

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<sup>23</sup> More evidence will be cited later in connection with our discussion of the role of social capital in group lending.

lending schemes, the authors rightly note that “We believe our results on cosigners give some empirical support also to certain group lending theories, in particular the one of peer support, and help explain group lending’s remarkable popularity over the past 30 years.” (p.4)

In sum, the existing empirical studies provide support to the hypothesis that joint liability induces self-selection of a kind that improves repayment performance and thereby helps reduce the adverse effects of market imperfections. But some controversy remains as to precisely how selection effect operates. The standard theory suggests that the main effect of self-selection is to screen out risky borrowers through assortative matching. Although there is some evidence in support of this hypothesis, there is also a great deal of evidence in support of the alternative hypothesis that the main effect of self-selection is to create opportunities for peer support and mutual insurance, often through formation of groups that are heterogeneous in risk attributes, contrary to the predictions of standard theory.

### ***The Pathways of Peer Monitoring and Peer Pressure***

The first systematic study to empirically estimate the impacts of peer monitoring and peer pressure on group performance was undertaken by Wydick (1999) using data from Guatemala. Performance was judged by several criteria, including repayment and incidence of moral hazard (measured by the extent to which loan was misused i.e., fund was diverted from the purpose for which it was originally taken.)<sup>24</sup> Econometric analysis was used to identify the determinants of each of these indicators of performance, separately for rural and urban groups and also for the combined sample.

The explanatory measures included separate variables to measure peer monitoring and peer pressure, and these were supplemented by a host of control variables. Peer monitoring was measured by the following indicators: (a) average distance between the members’ businesses, (b) knowledge about other members’ weekly sales, (c) whether the members are engaged in the same line of business. The following indicators were used to measure peer pressure: (a) whether members are willing to apply pressure on others, (b) whether the member feel that applying pressure is difficult, (c) whether members state that they have a moral obligation to repay group loan, (d) whether the members say that they repay in order to stay in good terms with the group, and (e) group size.

The study found evidence for statistically significant, *albeit* moderate, effect of peer monitoring on the mitigation of moral hazard and a correspondingly positive effect on repayment performance. The effect of peer pressure was also observed but it was more limited, confined only to rural areas.

Following a methodology similar to Wydick’s, Hermes *et al.* (2005, 2006) also found in their study on Eritrea that peer monitoring helps reduce moral hazard but with the twist that only the monitoring by the group leader matters, while monitoring by other group members does not make much difference. The authors offer two possible explanations of this finding. First, other members do not try to monitor seriously because of the cost of monitoring; instead they try

<sup>24</sup> A third criterion was creation of mutual insurance; we shall discuss this aspect later in connection with the role of social capital.

free ride on the leader, who is the one that really has to answer to the MFI. Second, other members may try to monitor but their effort is not effective because those who are monitored do not take them seriously. Group members feel pressured to behave prudently only when the group leader monitors, perhaps because only the leader is believed to have the real power to sanction moral hazard behavior due to his/her status as the representative of the group to the MFI.

Although rightly hailed as a path-breaking attempt to empirically identify the pathways of joint liability lending, Wydick's methodology suffers from a number of shortcomings. First, it does use any variables that actually capture whether peer monitoring was applied or not. Three proxy variables are used, each of which only stands for the ease of peer monitoring. If monitoring did occur these variables would indicate where it is likely to be easier or more effective, but they do not themselves tell us anything about the extent to which monitoring actually occurred. Thus, the conclusion that peer monitoring is effective (albeit moderately) is mostly presumptive. Second, since the groups were self-selected, it is conceivable that the proxy variables used for peer monitoring and peer pressure also reflect to some extent the effect of peer selection. After all, if self-selection leads to formation of groups in which members know each other well, then an indicator such as knowledge about each other's weekly sales would capture the effect of self-selection as much as it would the effect of peer monitoring. This will create an upward bias in the estimated effect of peer monitoring.

Subsequent studies have tried to deal with these problems in different ways. Simtowe and Zeller (2006), in their study on Malawi, included a separate variable indicating whether the group was formed through screening and self-selection. The idea was that if the effect of self-selection is thus controlled for, the variable indicating peer monitoring would provide an unbiased estimate of monitoring itself. They did find that monitoring reduces moral hazard, defined broadly to include both misuse of funds and strategic default. While the methodology employed in this paper marks an improvement over Wydick's, there still remains the problem that the measures of peer monitoring do not reflect the extent to which monitoring actually occurred.<sup>25</sup>

Gomez and Santor (2003) followed an alternative technique in their study on Canada, which we have already discussed in the context of selection effect. While comparing the repayment performance of group and individual lending programs, they used the technique of 'propensity score matching' (PSM) to remove the selection effect that operates before the formation of group. Accordingly, any difference that was found in the repayment of group-based and individual borrowers could be attributed to influences such as peer monitoring and peer pressure that operate only after the group has been formed. The results show that these post-group-formation effects contribute much more towards improving repayment performance compared to the selection effect.

While Gomez and Santor used novel econometric technique to remove the selection effect from survey data, Karlan (2007) exploited special features of programme design for the same

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<sup>25</sup> The indicators were (a) the extent to which members have access to information about each other's business, (b) the extent to which group members are willing to engage in peer monitoring activities to enforce proper loan use and report misuse of loans, and (c) whether they had any rules that encouraged joint ownership of enterprises.

purpose and arrived at similar conclusions. His analysis is based on the clients of FINCA-Peru, whose process for assigning individuals to groups creates a natural experiment with quasi-random group formation. When lending groups are formed, the initial members do not select each other. Instead, when individuals seeking a loan come to FINCA, they are put on a list. Once this list contains thirty names, a group is formed. Since no self-selection is involved, any observed difference in the repayment performance of the groups (after controlling for the effects of personal characteristics) can be attributed to ex post factors such as peer monitoring and peer pressure. Going beyond this indirect attribution, Karlan also presents direct evidence of monitoring and enforcement, such as knowledge of each other's default status, as well as direct evidence of punishment, such as deterioration of relationships. To him, all this constitutes "solid evidence that peer monitoring and enforcement effectively reduce default rates" (p.F55).

Support was lent to this conclusion by an experiment, carried out in Paraguay by Carpenter and Williams (2010). These researchers combined elements of laboratory and field experiments. Their subjects were actual microcredit borrowers and their performance was observed in the field, not in the laboratory, but before they were given joint liability loans they were subjected to laboratory experiments to ascertain their 'propensity to monitor'. Borrowing from the behavioral literature, the researchers developed an experiment to measure individual propensities to monitor one's peers in a social dilemma game with incentives similar to group lending<sup>26</sup>, and then tested whether the monitoring propensities of women about to enter an actual group lending programme predicted loan performance six months later.

Their results showed a significant correlation between peer monitoring and group loan performance. Specifically, they found that individuals in groups populated by inherently "nosy" monitors were approximately 10 percent less likely to have problems repaying their loans. The estimates were robust to differences in the formulation of peer monitoring measure and the inclusion of a number of other important controls. In fact, when the controls were added, the point estimates increase substantially. It should be noted that the experiment measured only the propensity to monitor, not the actual extent of monitoring. Yet, the authors boldly conclude that "These results suggest that, regardless of whether or not group lending leads to measurable reductions in poverty, it is the case that the groups' moral hazard is attenuated by peer monitoring." (p.4)

A startlingly contradictory piece of evidence was found, however, by Giné and Karlan (2011). They too isolated the selection effect from the effects of peer monitoring and peer pressure, but they did so through field experiments carried out with the help of the Green Bank in the Philippines. In one of the experiments, they started out with a set of joint liability groups that were self-selected but at one point in time a randomly chosen subset of these groups were dismantled and their members were converted into individual liability borrowers, while the rest

<sup>26</sup>The 'dilemma game' was originally devised to measure propensities to cooperate (by making financial contribution) for a common cause and to punish free riders through social sanctions. In the present experiment, the game was slightly tweaked to permit measurement of the propensity to monitor. After the contribution stage, but before punishment was allowed, participants were asked if they wanted to have access to the contribution decisions of the other members of their experimental group. If the participant paid a small fee, she was shown, the contribution levels of all the participants. Only those who paid the monitoring fee were eligible to socially sanction the other participants. The willingness to pay the fee for the privilege of gaining knowledge about the peer's contributions was taken as the measure of the propensity to monitor.

of the groups continued in the joint liability mode. Since the whole set of borrowers were self-selected in the first place, the two subsets were expected to contain borrowers with similar risk characteristics. In other words, if joint liability did confer any benefit through the avoidance of adverse selection both subsets should have enjoyed those benefits equally. Therefore, if the performance of the two subsets were compared after the conversion occurred (after a reasonable lapse of time) and the joint liability group was found to perform better, this would constitute evidence in support of the positive effects of peer monitoring and peer pressure, uncontaminated by the effect of peer selection.

In the event, no difference was found in the repayment performance of the two subsets of borrowers, which led the authors to conclude that monitoring and enforcement are not the pathways through which joint liability helps, if at all. In fact, direct measures of monitoring showed that the extent monitoring went down after some of the joint liability groups were converted into individual liability. This suggests that joint liability does induce stronger monitoring, as theory predicts, but comparison of repayment performance demonstrated that whatever extra monitoring takes place it does not translate into any tangible benefit in terms of loan repayment.

To complicate matters further, yet another experiment, carried out by Cason et al (2012), seem to contradict the finding of Giné and Karlan and restore confidence in the peer monitoring pathway of joint liability. The experiment was carried out in laboratories in India and Australia, with an experimental design that ruled out the possibility of selection effect contaminating the incidence and impact of peer monitoring.

The experiment demonstrates that the effectiveness of peer monitoring depends critically on the cost of monitoring, something that Giné and Karlan did not consider. The researchers found that if the cost of peer monitoring is lower than the cost of lender monitoring, peer monitoring results in higher loan frequencies, higher monitoring and higher repayment rates compared to lender monitoring. However, in the absence of monitoring cost differences, lending, monitoring and repayment behavior is mostly similar across group-based and individual-based lending schemes.

On the whole, the weight of evidence seems to point to the effectiveness of the pathway of peer monitoring. All the studies that have examined the impact of peer monitoring, including that of Giné and Karlan (2011), agree that joint liability induces stronger peer monitoring. And all studies, with the exception of Giné and Karlan, also agree that through enhanced monitoring, perhaps combined with enhanced peer pressure, joint liability leads to reduced moral hazard and better repayment performance. The study by Giné and Karlan has the merit that it cleverly separates out the selection effect before pronouncing on the monitoring effect, but so do some of the other studies that have found a positive contribution of peer monitoring.

#### **4. Joint versus Individual Liability – Which Works Better?**

Of the various features of the practice of microcredit, one that has attracted most attention of academics is joint liability – the idea that if any member of a group fails to repay her loan the

lender will penalize the whole group, not just the member who defaults.<sup>27</sup> It is this mutual liability for each other that is supposed to render it feasible for the micro-lender to deliver small loans to poor people without any collateral. And it is this aspect of group lending that has been analyzed most extensively in the theories of microcredit, with theorists trying to explain precisely how it contributes to the success of microcredit and exactly which imperfections of the credit market it helps address and how.

The theoretical models based on the concept of joint liability have generally viewed it as the single most important feature responsible for the widespread success of microcredit. But there has always been an undercurrent, even in mainstream theory, that there are some potential problems with joint liability – problems of such magnitude that could even call into question the presumed superiority of joint liability over individual liability. It has been long recognized, for example, that the demonstration in Stiglitz's (1990) classic paper that joint liability helps improve loan repayment by overcoming moral hazard is a 'local first-order result' in the sense that it applies only for a very small amount of joint liability penalty. Very high joint liability can be counter-productive, putting too much risk on risk-averse borrowing partners. The implication, as Townsend (2003, p.474) observes, is that "...if the interest rate, loan size and degree of joint liability are exogenous controls implemented by a formulaic lender, there would be no presumption that groups dominate individual loans, were borrowers allowed to choose."

Giné and Karlan (2011) provide a neat summary of some of the other concerns with joint liability. First, clients dislike the tension caused by the threat of punishment that group liability inevitably entails. Such tension among members could not only result in voluntary dropouts but could also harm social capital among members, with far-reaching consequences. Second, bad clients can free ride over good clients causing default rates to rise: some borrowers may not repay loans because she believes that her peers will pay it for her. As the model of strategic default developed by Besley and Coate (1995) shows, under certain conditions even the threat of social sanctions may not be able to prevent such behavior. If the peers indeed pay up for the defaulter, repayment will not suffer in the short term but repeated episodes of this kind will erode the whole foundation of group liability by creating mistrust and resentment. Third, group liability is inherently discriminatory against safer borrowers as they are required to repay the loans of their peers more often than the risky borrowers. This may eventually lead to disillusionment of safe borrowers and break-up of the group. Fourth, as a group matures tensions emerge over time due to divergence in the terms credit demanded by the members – some needing bigger loans than others, some preferring longer repayment period than others and so on. Such heterogeneity in credit demand may erode the basis of co-operative behavior; for example, clients with smaller loans may be reluctant to serve as a guarantor for those with larger loans.

The notion that joint liability may not be the 'miracle cure' it was hyped up to be has received a significant boost in recent years by an important shift that has been taking place in the practice of microcredit itself. More and more lenders are moving away from group lending towards

<sup>27</sup> The majority of theoretical models assume a particular form of group penalty, namely that if one member fails to repay other members of the group must pay up (either in part or in full) on her behalf. It is arguable that this concept of joint liability may not always conform to the way group penalty has actually been practiced on the ground. But we shall leave this issue aside for the moment, and treat the empirical debate about the relative merits of joint and liability as referring to the idea of joint liability as modeled in theories.

individual lending mode, the most dramatic example being the pioneer Grameen Bank itself, with its new style of microcredit delivery being dubbed as Grameen 2. In view of all this, it has become more important than ever, especially from a policy perspective, to ask which mode of liability works better in practice, or to be more nuanced, which mode works better in which conditions?

A growing body of empirical studies has begun to address these questions. We shall try to distil some lessons from these studies in this section. For this purpose, we shall classify the studies into two groups – those that purport to give a black and white answer to the question ‘which mode is better’, and the more nuanced ones that allow for the possibility that different modes may work better under different circumstances.

Among the former group of studies, we find all three possible answers – good, bad and indifferent. For instance, in his study of Malawi, Diagne (2000) gives his verdict against joint liability. He reports that groups that expected joint liability not to be fully enforced performed much better in terms of repayment than groups in which it was expected to be fully enforced. Furthermore, the majority of the partially paid delinquent loans consisted of good borrowers who defaulted because of the joint-liability nature of contracts. There are, however, reasons to take these findings with a grain of salt. Diagne tells us that for the microcredit groups he has studied the features of peer selection, peer monitoring, and peer pressure, etc. did not typically exist as the selection and monitoring activities were performed mainly by the lender. This suggests that joint liability didn’t work well in this case simply because the social capital that is necessary for joint liability to function well did not exist. It is pertinent to note here that the impact of joint liability on enforcement was found to worsen when some members had doubts about the repayment intentions of other members in their groups – a situation that occurred in 62 percent of credit groups. Obviously, trust, an essential foundation of group lending, was singularly lacking in this case.

A contrasting finding is presented by Gomez and Santor (2003) in their study on microcredit in Canada. The microfinance institution they studied (Calmeadow) offered both group and individual loans to clients in the same locality, with the borrowers themselves deciding which mode of lending to accept. The MFI maintained detailed records on various social, economic and demographic characteristics of the borrowers, which were supplemented by a survey carried out by the authors to elicit information on the borrowers’ attitudes and their social capital attributes. The data set thus provided a unique opportunity to compare the performance of the two mode of lending after controlling for other relevant factors. The authors found a lower propensity to default among group-based borrowers compared to the individual borrowers. Just as the absence of social capital was presumably the reason for group lending’s failure in Diagne’s sample in Malawi, so it is the presence of social capital that seems to make the difference here. This is evident from the observations that “Individuals who have known their fellow members before forming the peer group are less likely to default. Likewise, default is less likely if a great deal of trust exists in the group or if group members feel a moral obligation to their peers. Lastly, individuals who have “social capital” are less likely to default, since individuals who belong to an association, club, or sports team report higher repayment rates.” (Gomez and Santor 2003, pp. 7-8)

One potential ambiguity regarding the Gomez-Santor finding is that one cannot be sure whether the superior repayment performance of group borrowers is due to some unobserved differences between group and individual borrowers or due to the innate logic of joint liability itself. If the answer points to the former, it would not nullify the value of group lending as such; it would simply mean that joint liability may be the right mode of lending for some but not for others.<sup>28</sup> In any case, it would be useful to know which of the two interpretations is valid, and in order to find an answer Gomez and Santor employed the methodology of 'propensity score matching' (PSM) to create a counterfactual of individual borrowers who are 'like' the group borrowers in all relevant senses. If the counterfactual is accepted as reasonably valid, the observed superiority of group repayment can be interpreted as innate superiority of joint liability itself. Gomez and Santor recognize, however, that there are well-known limitations to the PSM methodology and that the best way to go about the task is to randomize i.e., to randomly assign prospective borrowers to group and individual lending modes so that the comparison of repayment performance can reveal the innate differences between the two modes of lending rather than the unobserved differences between the two groups of borrowers. The problem for them was that the MFI was not willing to go along with a randomized controlled trial (RCT).

Giné and Karlan (2011) were more fortunate; they were able to persuade the Green Bank in the Philippines to conduct a rich randomized experiment. We have already introduced this experiment earlier in the context of pathways of joint liability. As noted there, initially the MFI used to lend only to group borrowers. For the purposes of comparison, the researchers conducted two separate experiments to create random sets of individual borrowers. The first experiment was carried out in areas in which Green Bank had pre-existing group lending operations and the second experiment took place in new areas where the Bank had not operated before. In the pre-existing areas, the Bank randomly converted some centers from group lending mode into individual lending mode, but the borrowers were required to continue the practice of attending regular group meetings for repayment. Comparison of the performance of converted individual borrowers with that of continuing group borrowers provided one opportunity for judging the relative merits of the two modes of lending. Yet another opportunity came from the second experiment which was carried out in new areas. There the Bank randomly picked some centers for group lending, some for individual lending, and yet others for phased individual lending (group lending for the first loan cycle, converted into individual lending thereafter if the first loan was successfully repaid). Comparison between the new group-based borrowers and the new individual borrowers provided a second opportunity to judge the relative merits of group and individual lending. There was an essential difference between the two comparisons, however. In the pre-existing areas, both sets of borrowers were self-selected as group members before the experiment began. Therefore, one should not expect to find any difference between their performances on account of the selection effect of joint liability that operates before loan is taken (inclusion of only safe borrowers, for example). Any difference between them would reflect only the incentive effects (against moral hazard) that operate after loan is taken. By contrast, the second comparison is more comprehensive, encompassing both incentive and selection effects (since in this case only the group borrowers were self-selected).

<sup>28</sup> As we shall see presently, a good deal of evidence points towards this conclusion.

The conclusion from both experiments was that the two modes of lending do not differ significantly in terms of loan repayment performance, thus casting doubt on the presumed superiority of joint liability lending. This finding has caused a lot of stir in the literature on microfinance, and understandably so because of the careful manner in which the study was carried out with two different types of randomization providing highly credible identification strategies. One may still raise some queries about the interpretation, though.

Take first the result of the first comparison – the one between continuing group borrowers and converted individual borrowers in the pre-existing areas. The authors interpret this result as indicating that peer monitoring induced by joint liability is not really effective in solving moral hazard any more than individual liability. In order to explain this counter-intuitive result they speculate that peer monitoring may have been replaced by lender monitoring, but they admit to having no evidence to support it.<sup>29</sup> A number of alternative explanations were also suggested. First, if self-selected borrowers were inherently trustworthy, then the likelihood of which was demonstrated by another experiment by Karlan (2005), would repay whatever may be the lending mode. Second, if the selection effect allowed the inclusion of only borrowers with strong social networks, it is conceivable that they would continue to repay loan even without joint liability in order to protect their networks. Third, since the borrowers continued to repay as a group in public meetings, they had their reputation at stake if they were to default.

All these speculations are intended to support the conclusion that peers monitoring does not have any tangible effect. Yet, the result also admits of an alternative interpretation that is consistent with the effectiveness of peer monitoring; it is based on the idea of habit formation. If joint liability indeed induces the kind of behavior that prevents moral hazard (of both *ex ante* and *ex post* types) by encouraging peer monitoring, it is conceivable that once such behavior has become widespread and ingrained in the psyche of borrowers it would become a habit and its effect will persist even after joint liability has been dispensed with. It is instructive to note that the extent of peer monitoring was found to have declined after the conversion of groups into individual borrowers and yet repayment did not suffer. This is entirely consistent with the habit formation hypothesis.

The second comparison - the one between group borrowers and individual borrowers in the new areas - would seem to offer much stronger support to the conclusion of non-superiority of joint liability because the individual borrowers in this case were fresh clients, not having gone through the experience of working under joint liability. But even here the relevance of habit formation cannot be ruled out. Although the geographical areas in which the second experiment was conducted were new for the Green Bank, if they were previously served by other microcredit lenders a culture of regular repayment may have already been created in the society at large and that could have a spill-over effect on the Green bank borrowers as well. The possibility of such spill-over effect is actually evident from the first experiment in pre-existing areas. The experiment allowed new members to join the bank over time and they were found to maintain as good a repayment record as the converted individual borrowers and the continuing

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<sup>29</sup> There was no evidence, for example, that credit officers were working any harder after conversion into individual liability. The authors also note that it was not part of credit officers' training to engage in discussions with the clients about how they were investing their funds. (p.15)

group borrowers. This finding led the authors to comment: "This could be a result of group liability creating well-functioning groups, and even new members adhere to the practices and policies of the pre-existing members." (p.24), this is precisely what we mean by spill-over of the habit formation effect. In so far as this effect worked, even the second experiment cannot claim to have demonstrated the ineffectiveness of joint liability. The only problem is that we cannot be sure whether the spill-over actually worked because the authors do not provide any information about the prevalence of microcredit in the new areas before the entry of Green Bank, in contrast to the pre-existing areas where, we are told, the bank had competition in 72 per cent in the communities at the time the first experiment started.

There is another feature of the new areas that is worth mentioning. The authors found that in these areas "...credit officers were less likely to create groups under individual liability, and qualitatively this was reported to us as caused by unwillingness of the credit officer to extend credit without guarantors in particular barangays." (p.4). One must wonder why would credit officers be reluctant to offer individual liability contracts if, as the study claims, the mode of liability does not affect repayment? Do the credit officers know something that the researchers don't? It must be remembered that offering a particular form of contract was not a matter of choice for the bank officers; who will offer what was determined by a random assignment process. Thus, credit officers in charge of centers which were assigned the individual liability mode had to offer this contract no matter what they thought of it. The fact that they did not think of it very favorably probably implies the existence of some difficulty in implementing individual liability. One possibility is the increased cost of lender monitoring that individual liability could entail; the credit officers could be worried that if the cost of monitoring became too high they won't be able to maintain the repayment rate in the future.

Cost of monitoring is indeed one of the fundamental issues in any comparison of alternative modes of liability. Much of the theorizing on microcredit is in fact based on the presumption of lower cost of monitoring under joint liability. The importance of this presumption has recently been demonstrated empirically by Cason et al. (2012). They compare the relative efficacy of peer versus lender monitoring under laboratory conditions and finds that assumption about the cost of monitoring is crucial to the result. If the cost of monitoring under an individual liability programme is no different from that under a joint liability programme, then the two provide almost equivalent performance. If, however, the cost of peer monitoring is lower, compared to the cost of lender monitoring, joint liability dominates. The relative effectiveness of the two modes of liability is thus seen to be critically dependent on the relative costs of monitoring.

The study by Casanet al. belongs to an emerging group of empirical works that, instead of giving a black and white answer to the question of which mode of liability is better, take a more nuanced approach and try to establish the conditions under which one or the other mode can be expected to dominate. The other studies in this group can be classified into three categories: those that examine the possibility that different modes of liability may be appropriate for different types of clients, those that try to elucidate the trade-offs inherent in joint liability, and those that examine the viability of joint liability under 'crisis' situations.

One of the earliest studies in the first category is by Madajewicz (2003) on Bangladesh. In a companion theoretical paper, she had established that among the credit-constrained borrowers,

joint liability may be preferable for the poorer borrowers, but individual liability may be preferable for the less poor (Madajewicz 2004, 2011). The reasons for the negative effect of joint liability on the wealthier borrowers are two-fold. First, they receive a smaller loan under joint liability compared to individual lending. Second, they choose less productive (also less risky) projects under joint liability than under individual lending. These findings were based on the assumption that larger investments allow adoption of more profitable but also more risky projects. Since joint liability discourages risk-taking (via elimination of moral hazard), wealthier borrowers who can otherwise afford to take the risk in search of profitability are prevented from undertaking large investments. For them, individual liability is the better option. Evidence from Bangladesh confirmed this prediction: beyond a point, the wealthier among the credit-constrained borrowers were found to earn lower profits from joint liability lending compared to individual lending. While interesting, this finding is compromised by an identification problem. As the author herself acknowledges, lower profits at higher wealth levels under joint liability could be due to diminishing returns rather than to the negative effect of joint liability as such.<sup>30</sup>

Vigenina and Kritikos (2005) also find that the relative merit of the two modes of liability depends on the nature of clients, but the relevant characteristic here is not just the level of wealth but also the nature of the enterprise. They start from the premise that anyone who has sizeable collateral to offer will normally opt for individual lending because of the costs involved in joint liability lending. On the other hand, only those with no or little collateral to offer will opt for joint liability. But studying the clients of two micro-lenders in Georgia, one of whom offers individual loans and the other joint liability loans, they noticed that joint liability clients included many borrowers who did have sizeable assets to offer as collateral. This suggests that there are other determinants of the choice between the two modes of borrowing besides collateralizable wealth.

The authors postulate that an important consideration is the nature of the enterprise. If the enterprise holds promise of expansion, the borrower would like to have increasing loan size over time, but this is only available from individual lending because under joint liability loan size will be restricted in order to avoid moral hazard. By contrast, if the enterprise is essentially static in nature, so that repetition of small loans is all that is needed, joint liability may be chosen. In short, the hypothesis is that: "If an individual and a group lending organization operate in the same market niche, there will be a self-selection process not only with respect to the wealth status but also with respect to the financial needs which are determined by the expected dynamics of the borrower's business." (p.14). Empirical tests supported this hypothesis: borrowers with less dynamic business were shown to have a higher probability of choosing the joint liability mode after controlling for other possible determinants of choice such as interest rate and the level of education.<sup>31</sup>

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<sup>30</sup> There is also the problem that while her theory requires that both wealthier and poorer borrowers are credit-constrained, the author has not established that the wealthier borrowers in her sample are really credit-constrained. She simply assumes that they are because they too are poor, whereas actually they own land between 1.5-2 acres, which is beyond the reach of most people commonly regarded as poor in rural Bangladesh.

<sup>31</sup> The methodology may be questioned, however, on the ground of endogeneity. The authors use information on whether business expanded since the first loan to measure the dynamism of the enterprise, but the problem is that expansion is likely to be endogenous to the availability of credit and the size of the loan offered by the MFI.

In the second category, there are several studies that suggest that there are inherent trade-offs in joint liability and whether it will outperform individual liability or not depends on where the balance of trade-off lies in particular circumstances. Giné et al. (2010) and Fischer (2013) use experimental methods to examine the trade-off between risk-taking and risk-sharing induced by joint liability. Giné et al. focus on *ex ante* moral hazard in project choice (i.e., excessive risk-taking) and abstract from *ex post* moral hazard and strategic default considerations. The question they ask is: which mode of liability is better able to extract the benefits of dynamic incentives (i.e., the practice of giving future loans with favorable terms contingent on timely repayment of current loan) by preventing *ex ante* moral hazard. It is acknowledged that adding dynamic incentive to a loan contract would reduce moral hazard and improve repayment performance regardless of the liability mode used. The issue is, under which mode the benefit will be larger.

For this purpose, they first modify the existing moral hazard models of microcredit by changing a crucial assumption. Following the footsteps of Stiglitz (1990), most of the models assume that safer projects have either the same or a higher expected return than riskier project. Giné et al. (as well as Fischer) replace it with the more plausible assumption that riskier projects have higher expected return (since investors must be compensated for higher risks.). Armed with this assumption, they are able to show theoretically that joint liability under dynamic incentives induces two opposite forces. On the one hand, it encourages more risk-taking than under individual liability. The reason is that risk-averse borrowers, who would normally choose low-risk, low-return investments under individual liability, will switch to the risky investment under joint liability whenever they are matched with a less risk-averse joint liability partner.<sup>32</sup> On the other hand, under dynamic incentive joint liability induces more risk-sharing and mutual insurance compared to individual liability. The first effect works towards worsening the relative repayment performance of joint liability, while the second effect tends to improve it. The net effect will depend on the balance of the two effects.

The experimental results of Giné et al. demonstrate that joint liability increases risk-taking under dynamic incentives, as expected, but it also simultaneously improves the repayment rate. Repayments rise due to the insurance effect: joint liability forces the borrowers to insure each other more – passing the cost of limited liability back to the clients. Econometric estimate suggests that including a joint-liability clause increases loan repayment by about 20 per cent via the insurance effect.

Fischer (2013) conducts a similar exercise but allows for more variations in the type of contracts offered to the experimental subjects. Five contractual terms were allowed: autarky, individual lending (with possibility of voluntary transfer), joint liability, joint liability with explicit peer approval of project, and an equity-like contract. Two levels of monitoring were allowed – perfect and imperfect. As in the case of Giné et al., the difference in repayment performance across the contract types arises from the opposite effects of risk-taking and risk-sharing. According to the experimental results, the net effect is that adding informal transfers (moving from autarky to the individual liability treatment) reduces default rates by two percentage points from 4.83% to

<sup>32</sup> This result is diametrically opposite to the predictions of most models of microcredit and is explained solely by the difference in assumption about the distribution of project return.

2.80%. Moving from individual to joint liability further reduces default rates to 1.35%, or 1.51% when approval rights are explicit. Finally, equity generated no defaults as increased risk was almost always hedged across borrowers, with the worst possible joint outcome still sufficient for loan repayment. Each of the differences in default rates is significant at the 5% level. Thus his results lend support to the finding of Giné *et al.* that repayment does improve with joint liability and the reason lies in higher levels of mutual insurance.<sup>33</sup>

A slightly different kind of trade-off was examined by Abbink *et al.* (2006) and Kono (2013). Instead of *ex ante* moral hazard, they looked at *ex post* moral hazard (strategic default) and explored the trade-off between free riding and risk-sharing. The first study was conducted as a laboratory experiment while second was carried out as a framed field experiment in Vietnam. Joint liability encourages free riding, as some group members feel tempted to default strategically in the hope that her peers will pay up so as not incur joint liability fine.<sup>34</sup> At the same time, joint liability also encourages risk-sharing and mutual insurance, especially with dynamic incentive.<sup>35</sup> In order to assess the net effect of these two opposite forces, Abbink *et al.* first established a best case scenario of repayment under individual lending (though there was no individual lending in the experiment) and compared it with repayment under group lending. The game was set up in such a way that free riding would lead to no repayment in a single-round game. Although there is dynamic incentive in the experiment, the game was to end after a finite number of rounds, and the terminal point was known to all in advance. In this setting, the logic of backward induction suggests that there should be no repayment even in a repeated game. However, the experiment showed that group members actually repaid well and with a repeated game they repaid even more frequently than in the best case individual lending scenario. This was made possible by the fact that dynamic incentive under joint liability created a high level of risk-sharing and mutual insurance, which was large enough to outweigh the free riding effect.<sup>36</sup>

The same trade-off – between free riding and risk-sharing – was also investigated by Kono (2013), but this time through a framed field experiment in Vietnam, rather than a laboratory experiment, and the results turned out to be different. Joint liability was again found to instigate free riding and promote risk-sharing at the same time, but on this occasion the balance of forces was in the wrong direction. The negative effect of free riding overwhelmed the positive effect of

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<sup>33</sup> Fischer himself emphasizes the finding that equity-like contracts outperform all other types. He actually argues against joint liability on the ground that it reduces efficient risk-taking. But this is slightly misleading. His results show that when joint liability is not linked with explicit peer approval of projects, risk-taking under joint liability is statistically no different from risk-taking under individual liability. It is only with explicit project approval that risk-taking under joint liability falls below that of individual liability. But then explicit project approval by peers is hardly a common practice in the world of microcredit; it is more of an artificial construct created for the purpose of Fischer's experiments. One must acknowledge, of course, that equity-like contract proved to be the best for both risk-taking and repayment, but one must also ask whether this type of contract is feasible in reality, especially since it involves third-party enforcement of equal income distribution in Fischer's formulation.

<sup>34</sup> See the discussion on the Besley-Coate model in Osmani and Mahmud (2015) for elaboration of the free riding effect.

<sup>35</sup> The tension between free riding and risk-sharing is theoretically investigated by Impavido (1998) and Arellano-dáriz (1999).

<sup>36</sup> Of course, this requires deviation from individual rationality assumed in game theory, but such deviation has been found to occur in several 'public goods' experiments, where free riding has been curtailed for the sake of gaining 'social approval'. See, for example, Gächter and Fehr (1999).

risk-sharing, leading to lower rate of repayment under joint liability. Even after introducing a cross-reporting system or punitive measures among borrowers, joint liability could not outperform individual lending.

These studies on trade-offs bring an important lesson to the fore. The choice between joint and individual liability cannot be posed as an absolute. Their relative merit will depend critically on joint liability's power to prevent moral hazard and promote risk-sharing in any given context. This power in turn will depend greatly on the social capital that either pre-exists among group borrowers or develops over time as they interact with each other repeatedly. If social capital is of the kind and magnitude that permits on the one hand adequate peer monitoring and peer pressure to eliminate moral hazard and creates on the other enough peer support to promote a high degree of risk-sharing and mutual insurance, the balance of trade-off will lie in favour of joint liability; otherwise individual liability will prevail. Much will also depend on what other measures, apart from joint liability, the microfinance institutions add to their practice of group lending for the prevention of moral hazard and promotion of mutual insurance.

Let us turn finally to the category of studies that examine the viability of joint liability lending at times of crisis. In what is perhaps the first published paper to provide empirical evidence on the relative merits of joint and individual liability, Bratton (1986) compared the performances of three types of agricultural lending in Zimbabwe. Two of them were offered by the Agricultural Finance Corporation (AFC); they gave both individual loan and mandatory group loan (the term mandatory implies that the farmers were required to sell their crops collectively to the official marketing board). In contrast, an NGO named Silveira House (SH) offered voluntary group loans based on joint liability typical of most microfinance institutions. Bratton compared the repayment performance, along with several other performance indicators, of the three modes of lending in three consecutive years, two of which were normal weather-wise but the third was afflicted by a severe drought. His results showed that groups performed better in normal years but did very poorly, compared to individual borrowers, in the drought year. He explains the findings thus: "Group lending...appears viable under 'normal' conditions, but counterproductive when farmers are exposed to extreme environmental stress. The logic of collective action in different organizational settings supports this view. Individuals will struggle to repay even when they are stringently deprived in order to maintain eligibility for credit. By contrast, farmers with joint liability loans have little incentive to pay their share unless they expect other group members to do the same." (p.126)

The final sentence in this quotation holds the key. Joint liability induces an inter-dependence among group members. Under normal circumstances, such inter-dependence may stand them in good stead by raising a protective shield against moral hazard and boosting the practice of mutual insurance, but at times of crisis the same inter-dependence may cause the whole system to unravel through a 'domino effect' – as one person defaults under genuine pressure it encourages others to do so, even if they could have repaid, simply because they want to avoid the burden of joint liability.

A couple of recent episodes of this kind of unravelling of joint liability under crisis situations have been analysed insightfully by Giné *et al.* (2011) and Breza (2012). The crisis in both cases was

caused by human action rather than natural causes. Giné *et al* examined a case in the south Indian state of Karnataka where a Muslim religious body called Anjuman Committee of the town of Kolar issued a *fatwa* (religious edict) in January 2009 banning all Muslims from repaying their microcredit loans claiming that charging interest was *haram* (forbidden). As expected, the *fatwa* engendered a serious crisis for the microcredit institutions, but in so doing it also provided a unique opportunity to researchers to study precisely how the domino effect operates in a crisis. In particular, it provided a natural experiment in which the domino effect on loan repayment could be isolated from other possible causes.

Giné *et al* set out the nature of inter-dependence implicit in the idea of domino effect in the form of the following hypothesis: members of a joint liability group are more likely to default on their loans when the proportion of defaulting members in a group increases. The problem in testing this hypothesis is that repayment rates are also the result of selection, incentive effects and correlated observed and unobserved shocks. An identification strategy is, therefore, needed to separate out the effects of these other factors. The authors found such a strategy by exploiting two facts of the data. First, the *fatwa* directly affected the repayment rates of the Muslim borrowers, not of the Hindus. As a result, Muslim-dominated groups faced a greater repayment crisis compared to Hindu-dominated groups. Second, many borrowers had loans from several groups, which differed in the density of Muslims. The existence of borrowers with multiple loans provided an opportunity to control for the possibility that borrowers from Muslim-dominated groups may be inherently different from those in Hindu-dominated groups. The identification strategy in this case consisted of observing the variation in the behaviour of the same individual across multiple groups of differing density of Muslims. The idea is that since the initial default will be higher in the Muslim-dominated groups, the domino effect, if it exists, will be stronger in these groups compared to the Hindu-dominated ones. This can be tested by comparing the propensity to default of the borrowers who are members of both types of groups. The central finding of the study is that the same borrowers indeed had higher default rates for the loans they had taken as members of Muslim-dominated groups compared to the loans they had taken as members of Hindu-dominated groups. The implication is that once the Muslims started to default following the *fatwa*, the domino effect took hold affecting the larger body of the clientele.

The existence of the domino effect clearly demonstrates the vulnerability of joint liability at times of crisis. Any comprehensive assessment of the relative merits of joint and individual liability lending must, therefore, weigh up any putative benefits of joint liability in good times against its vulnerability in bad times. It should be noted, however, that even at times of crises, it may not be all bad news for joint liability. While joint liability may face bigger repayment problems than individual liability during crisis, one must also ask how the two systems would fare in the immediate post-crisis period of recovery. And if one looks at the bigger picture embracing both crisis and recovery, which system comes out better? A recent study by Breza (2012) sheds some light on this question, albeit indirectly.

The context is a large scale default episode that took place in the Krishna District of Andhra Pradesh, India in March 2006. Prior to this incident, the microcredit movement launched by the NGOs was coming under intense criticism amid fears of over-indebtedness of poor borrowers

and allegations of usurious interest rates and abusive collections practices, which according to the detractors even led to a spate of borrower suicides. In this backdrop, the District Collector (the government bureaucrat in charge of district administration) announced that his constituents should stop repaying their microloans and launched his own alternative programme of financial inclusion. Within days of the announcement, all borrowers ceased repaying loans, causing a serious crisis in the microcredit sector. A retraction was made in mid-2006 and the worst of the crisis was finally over in early 2007.<sup>37</sup> Soon after the defaults, the local MFIs, including Spandana, one of the biggest in India, began to re-establish collections in the affected villages. They also suspended the joint liability feature of the loans and offered new loans for those who fully repaid their outstanding debt. Gradually, some borrowers began to repay the loans they had earlier defaulted on and by November 2009, some 40 to 50 per cent of individuals had fully repaid their liabilities. The objective of Breza's study is to investigate whether repayment was helped or hindered by 'peer effects' i.e., the effect that the peers' repayment behavior has on one's own incentive to repay.

She first established that peer effect exists i.e., if the peers start to repay a borrower will also feel more inclined to repay.<sup>38</sup> She also quantified the effect: she estimated, for example, that if a borrower's peers shift from full default to full repayment, she is 10-15 percentage points more likely to repay. The peer effect of course cuts both ways, for if the peers start to default a borrower might be inclined to default too. In order to capture the net effect, Breza simulated a model of borrower's behavior capturing both the negative effect when the crisis unfolds and the positive effect when the process of recovery starts. She finds that the peer effect is asymmetric in the sense that the positive effect during recovery is stronger than the initial negative effect. Thus, she concluded that peer effects actually improved repayment rates relative to a counterfactual without peer effects.

It should be noted that Breza's focus was not on the mode of liability as such but on 'peer effects' on repayment. Since peer effects can operate even without joint liability – for example, if borrowers are individually liable but are required to repay together in group meetings – the study does not directly address the debate on joint versus individual liability. However, since joint liability is the prime vehicle through which inter-dependence among peers has been established by microfinance institutions, it has lessons for the present debate as well. The main lesson is that the fact that joint liability is vulnerable to the domino effect during crises does not necessarily constitute a case against it; how it operates during the recovery phase should also

<sup>37</sup> However, subsequently the Krishna default crisis was repeated several times, and on a much larger scale. The biggest crisis came in October 2010, when the government of the state of Andhra Pradesh issued an emergency ordinance severely restricting the operations of all MFIs in the state.

<sup>38</sup> The technique was to test whether a borrower's own probability to repay was correlated with the peer's probability to repay. This is a tricky exercise, however, because the two probabilities may be correlated due to many common characteristics that the borrower might share with her peers and may not therefore reflect the 'pure' inter-dependence that peer effect stands for. The author used a regression discontinuity approach to solve this problem of identification.

be taken into account. The net effect will almost certainly be context-specific; positive in some cases, negative in others.<sup>39</sup>

## 5. Does Social Capital Play a Role in the Success of Joint Liability?

Almost all theories of microcredit invoke the help of social capital in sustaining the pathways through which group lending is supposed to work. A rare exception is Armendáriz and Gollier (2000), who built a model to show that even if complete strangers were randomly put together in a group, the very logic of joint liability will ensure that their repayment performance will be superior to that of individual borrowers in the face of asymmetric information.<sup>40</sup> All other theories accord a central role to social capital in some form or the other. One may broadly distinguish two kinds of social capital – namely, informational social capital and relational social capital. Both of them are evident in the theories of microcredit. Informational social capital plays a role in either avoiding adverse selection through peer selection, which requires intimate knowledge of each other's risk characteristics; or in mitigating moral hazard through peer monitoring, which requires knowledge of each other's projects or effort or use of funds. Relational social capital plays a role either in preventing strategic default through peer pressure, which requires a kind of social relationship that permits some members to impose sanctions on others; or in avoiding genuine default through peer support, which requires a degree of social cohesion. In other words, social capital is ubiquitous in the theories of microcredit, and its role is almost invariably assumed to be supportive of group lending.

Yet, surprisingly, most of the early attempts to empirically assess the role of social capital in group lending found its effect to be either insignificant, or even more curiously, negative (after controlling for other factors)! For example, Sharma and Zeller (1997) found that closer 'social ties' among group members actually accentuated moral hazard and worsened repayment performance in Bangladesh. In Wydick's (1999, 2001) study of Guatemala, closer 'social ties' seemed to have had no effect on mitigating moral hazard and actually had a negative effect on the provision of mutual insurance. In Burkina Faso, Paxton *et al.* (2000) found that 'social homogeneity' added to the group's repayment problem. In another study on Bangladesh, Godquin (2004) observed that 'group homogeneity' had no effect on repayment performance while 'social ties' had a negative effect. Simtowe and Zeller's (2006) study of Malawi used as many as six different indicators of 'social ties' and found a significant effect for only one of them (distance among the villages from which the group members came). In their study of rural Thailand, Ahlin and Townsend (2007) also observed a negative relationship between 'social ties' and repayment performance. The only study that came up with an unambiguously positive result is an earlier one on Malawi by Zeller (1998), who found that measures of 'social cohesion' were positively correlated with good repayment performance.

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<sup>39</sup> Breza's own judgement (not an empirically validated statement, however) is that abandonment of joint liability in the immediate aftermath of the Krishna crisis was probably a good idea because its presence might have hindered the emergence of early repairs whose action would trigger more widespread repayment through the peer effect. Even if she is right in this judgement, it does not necessarily detract from the possible value of joint liability in instilling a culture of peer effect in the first place.

<sup>40</sup> As pointed out in Osmani and Mahmud (2015), however, their conclusion is critically dependent on particular assumptions about the distribution of returns to the borrowers. Under alternative, but equally plausible assumptions, the conclusion no longer holds.

The overwhelmingly negative nature of the findings has obviously demanded some serious explanations. A couple of them have been offered. The most common explanation is that closeness of social ties actually inhibits group members from imposing social sanctions on each other. The reason is that peer pressure and imposition of social sanctions inflicts a cost on the imposers themselves, which has implications for peer behavior. As Sadoulet and Carpenter (2001) explain: "If enforcement through social collateral is more expensive than direct enforcement mechanism..., groups having to resort to such methods will have to be faced with higher default rates"(p.7). The second explanation has to do with the failure of social ties to provide adequate mutual insurance when fellow members are faced with genuine difficulties. Wydick (1999) explains this phenomenon in the following terms: "The best explanation for this appeared to be that if insurance between members is manifest as shock-contingent credit, groups of friends (with a high rate of time preference) seemed to prefer that the lending institution suffer the consequences of the shock instead of an unfortunate friend in the group"(p.479, fn. 5).

While these explanations of the negative finding sound plausible enough, there are reasons to question the validity of the finding itself. Most of the studies discussed above suffer from one or more of the following four problems. First, the proxies used to capture the relevant variables, especially the extent of social capital, leave a lot to be desired. Second, the econometric techniques used by most of them were not refined enough to capture all the pathways through which social capital might have influenced repayment behavior. Third, identification of the effect of social capital remains problematic because of insufficient attention paid to an endogeneity problem. Finally, most of these studies ignore the fact that there are different types of social capital, and the analyst must be careful about which particular type to focus on, bearing in mind the purpose for which it was being used and the context in which it was being used. Subsequent studies have been much more alive to these problems and their findings are much more supportive of the positive role of social capital.

An example of dubious proxies is Sharma and Zeller's use of the number of relatives in a group as a measure of social ties. To confine the existence of social ties only to relatives is clearly unduly restrictive and may well explain the negative result; for after all, it may be far more difficult to impose sanctions on one's own relatives than on non-relative peers. Godquin's measure of 'social ties' – the number of years for which a group has been in existence – is equally problematic. Based on this measure, the author is able to explain the negative effect of social ties on the ground that as borrowers become more familiar with each other over time it becomes more difficult to impose sanctions on each other. But there are other interpretations of the negative effect that render age of the group a dubious proxy for social ties. For instance, Matin (1997) and Paxton *et al.* (2000) have noted that older groups may face greater difficulties in keeping up loan repayment because of a 'matching problem'. <sup>41</sup>To take another example of

<sup>41</sup> "Members of a group may be more likely to repay the loan in their first credit cycle rather than in subsequent loan cycles, since in the first time period each member has explicitly sought a loan and agreed to the terms and conditions that are usually fairly similar if not identical across members. However, as loan cycles pass, some individuals may continue with the group and accept the subsequent terms and conditions even if they do not match their individual preferences and changing economic situation." (Paxton *et al* 2000, p.641). This may create tension within the group, resulting in greater likelihood of default on the one hand and less likelihood of peer support to prevent default on the other. von Pischke *et al.* (1998) offer a number of other reasons why repayment may decline over time.

dubious proxies, Godquin measures social homogeneity by similarity in age and education, without offering any convincing explanation as to why homogeneity along these particular dimensions should be relevant for influencing the repayment behavior of a group.

In some cases, measurement of the dependent variable is also problematic. Many of the studies use 'the extent of misuse of funds' as the measure of moral hazard – misuse being defined as diversion of funds from the original purpose (e.g., Wydick, 1999; Hermes et al 2005). But fund diversion is not the only conceivable type of moral hazard; in fact, the kinds of moral hazard that the theories are mostly concerned with are choice of risky projects and shirking of effort. Indeed, it is conceivable that peers may not even regard fund diversion as a moral hazard if they are aware that this was being done to meet some urgent family needs, something they themselves would have done under similar circumstances. In that case, it is entirely plausible that closeness of social ties may not result in peer pressure for preventing diversion of funds, even though the same peers may well have tried to prevent shirking or adoption of unduly risky projects. Therefore, the observed inability of social capital variables to influence moral hazard, as measured, does not necessarily imply that social capital is impotent in this regard.

Simtowe and Zeller (2006) use an additional indicator to capture moral hazard in the form of strategic default – by asking the chairperson of the group whether some members willfully defaulted. But this is not a very convincing measure because the real test of the effectiveness of social capital lies in the extent to which it was able to prevent willful default – i.e., the number of times willful default could have happened but did not because of peer pressure, rather than the number of times it did happen. The problem that may arise from using 'observed' default as the dependent variable is evident from the following comments of Paxton et al. (2000, p.651): "...availability of information within the relatively small groups facilitated monitoring and enforcement. As a result, most of the reasons for default could be classified as "uncontrollable," and thus strict social sanctions were not imposed. Instead of pressure, the groups felt sympathy for the member with arrears and offered assistance." This statement implies that peer monitoring has already taken care of moral hazard and peer pressure has already taken care of strategic default. Two sources of default have thus already been eliminated. Consequently, the effect of social ties on reducing default through peer monitoring and peer pressure is not captured simply because the propensity of such default is not observed.

The second problem arises from the fact that the econometric procedures used do not always allow for a comprehensive assessment of the role of social capital. For example, Wydick (1999) explicitly defines peer monitoring, peer pressure and social ties as three dimensions of a broad conception of social cohesion, but uses a methodology that fails to capture all the dimensions. He actually uses all three dimensions as explanatory variables but the way he uses them creates the problem: all three are used at the same time in the same regression on moral hazard. When the coefficient on 'social ties' turns out to be insignificant, Wydick is led to conclude that social ties have no effect on moral hazard. But this is misleading because the other two variables had positive effects, and social ties may well have operated through them, especially in view of Wydick's recognition that peer monitoring and peer pressure are also dimensions of social cohesion, of which social ties are a component.

The point is that it may be more appropriate to treat peer monitoring and peer pressure as functions of social ties and other aspects of social cohesion. In that case, the appropriate methodology would consist of a two-stage procedure: the first step would assess the effect of peer monitoring and peer pressure on moral hazard, and the second step would assess the effect of social ties on monitoring and pressure. The danger of putting them all together in a single one-stage regression is that if social ties work through strengthening peer monitoring and peer pressure, an insignificant coefficient of social ties may simply mean that once its effects on monitoring and pressure are controlled for there remains no further effect of social ties. But that would not mean the impotence of social ties. The same problem exists in the work of Simtowe and Zeller (2006) on Malawi. They recognize that social ties are a potential factor in determining the quality of peer monitoring and peer selection. And yet, they use measures of social ties in the same regression on moral hazard that also includes measures of monitoring and selection.

The third problem – that of endogeneity – is neatly explained by Karlan (2007): “Typically, showing that higher social connections cause higher loan repayments is a difficult task due to selection and group formation issues. Using observational data, since most group lending programs rely on peers to screen each other and form groups, fundamental endogeneity problems exist when analyzing the impact of social connections on lending outcomes. For instance, if groups are formed within neighborhoods, and neighborhoods with stronger social network also have more economic opportunities, then empirically one should observe a correlation between the social connections of a group and its likelihood to repay.” (p.F53) But such correlation could in reality be the consequence of economic opportunities rather than of social connections. Karlan further argues that if social capital is measured by activities or involvement with others in the community (as is common), then an omitted variable problem may arise because those with stronger entrepreneurial spirits may also have stronger social connections. In that case, it would be difficult to disentangle the effect of social connections from that of entrepreneurial spirits.<sup>42</sup>

Researchers have resorted to a number of devices to get around this problem of identification. Karlan (2007) himself makes use of the special programme design of FINCA-Peru that (as discussed earlier) eliminates the selection effect by instituting a quasi-random process of group formation. In eliminating the selection effect, the process also eliminates the possible effect of unobserved dimensions such as economic opportunities that could be correlated with social connections through ‘neighborhood effects’. As some of the groups will be randomly endowed with more social connections than others, the outcome is a natural experiment in which the ‘pure’ effects of social connections can be identified. Karlan finds that strong social connections of the group are indeed correlated with better loan repayment and higher savings.

A couple of caveats to this finding should be noted, however. First, while the process of quasi-random group formation successfully solves the identification problem arising from neighborhood effects, it is not clear that the process also solves the identification problem arising from possible correlation between social connections and innate entrepreneurial spirits. If the two are indeed correlated, as Karlan argues, then a group endowed with more social

<sup>42</sup> For a fuller discussion of the problems of identification involved in estimating the effects of social capital, see, in particular, Manski (1993, 2000).

connections will also be endowed with more entrepreneurial spirits even if the members of the group came together through a random process. Second, by eliminating the selection effect, the process of group formation also eliminates one route through which social connections may help improve repayment performance – namely, avoidance of adverse selection. As a result, Karlan's estimates can at best be regarded as a lower bound of the positive effect of social connections.

Abbink *et al.* (2006) adopted the technique of laboratory experiment in which students in the social sciences at the University of Erfurt participated in a microfinance game. Student subjects were formed into 31 borrowing groups of varying sizes. The game involved a stochastic element: each student-borrower faced a 1/6 probability of a negative shock, forcing her to depend on fellow members to repay the amount due on the group loan. To isolate the effect of social ties, two separate recruitment techniques were used. Some groups were formed of students registering individually for the experiment, which minimized the degree of social ties between members. Other participants registered together in groups; in these groups social ties were found to be stronger. Since self-selected groups were expected to select on prior social ties, comparison of the two group types (holding other factors constant) would indicate the impact of social ties on repayment. The experiment found that social ties had a positive effect on loan repayment, but only at the beginning. The effect faded away in the subsequent rounds.

The fading away of the effect of social ties seems puzzling. One possible clue lies in a loophole which the authors themselves recognize. The experiment allowed both types of groups the same degree of interaction after the group was formed. In the real world setting, however, the pre-acquainted group is more likely to interact *ex post* and that may make a difference to their repayment performance over time. Perhaps, a more important reason, as observed by Armendáriz and Murdoch (2010), is that the participants were required to play the game for a known fixed number of times. It is well-known from the theory of repeated games that it is difficult to sustain co-operation in a finitely repeated game. The intuition is easy to grasp through a process of backward induction. If co-operation has to be sustained either through the lure of rewards or through the fear of punishment in the subsequent rounds, there can be no incentive for co-operation in the final round. But if it is understood by all concerned that there is not going to be any co-operation (no reward and no punishment) in the final round, there will be no co-operation in the penultimate round either. Thus, through a process of backward induction, it can be established that non-cooperation will be the equilibrium outcome in every round.

Clearly, the feature of finite games must be dispensed with if the experimental method is to offer any meaningful insight into the effect of social capital on group lending. This was precisely what was done by Cassar *et al.* (2007), following a methodology commonly adopted in the literature on experimental public good games. Since an infinitely repeated game cannot possibly be played in a laboratory, the trick is to simulate it by introducing an element of uncertainty about when a particular group was going to be dissolved. After thus taking care of the problem of finite interactions, and also after eliminating the endogeneity problem of self-selection by using only exogenously formed groups, Cassaret al.(2007) found that relational social capital had a sustained positive effect on the group's repayment performance.

Feigenberg *et al.* (2013) adopted a clever experimental approach to isolate the effect of social

interactions. Their study is based on a combination of field and experimental data from a West Bengal MFI (VFS), which gives loans to individuals but requires regular group meetings. The experiment consisted of varying the frequency of meeting from weekly to monthly and observing the impact on social interactions. Microfinance clients were randomly assigned to repayment groups that met either weekly or monthly during their first loan cycle, and then graduated to identical meeting frequency for their second loan. By randomizing the extent of social interactions, the paper is able to establish its causal role.

Long-run survey data and a follow-up public goods experiment reveal that clients initially assigned to weekly groups interacted more often and exhibited a higher willingness to pool risk with group members from their first loan cycle nearly two years after the experiment. They were also three times less likely to default on their second loan. It should be noted that study has nothing directly to say on group lending with joint liability since the clients were all individual borrowers, who were required to attend group meetings for repayment.<sup>43</sup> However, since the joint liability mode of lending typically requires regular meetings of group members, its findings are relevant to the question of whether and how the social interactions required by group lending can promote repayment performance.

The reader would have noticed that we have been using many different terms, such as 'social ties', 'social cohesion', 'social homogeneity', 'social connections', etc. to denote social capital. This is because different studies have deployed different terms, and that is indeed one source of confusion in drawing a clear picture about the role of social capital in group lending. This is not simply a matter of terminological profligacy, for they are not different names for the same idea. Rather they stand for different aspects of social capital, which is an inherently multi-faceted concept.<sup>44</sup> This raises the question: are all aspects of social capital relevant for good performance of group lending, or only some of them are, and if so which ones? Also, one might ask: is it possible that different aspects of social capital contribute to the success of group lending in different ways and that their relative importance may vary depending on the socio-economic environment in which group lending operates?

A number of recent studies make it clear that the forms of social capital matter, and that the context matters too. When Ahlin and Townsend (2007) found in rural Thailand that social ties had a negative effect on group performance, they were quick to add that "This idea must be qualified. Social structures that enable penalties can be helpful for repayment, while those which discourage them can lower repayment." (p. F43) The reason for this qualification was their finding that, contrary to the generally negative effect of social ties, social sanctions proved to be an effective tool for reducing default in the poor northeast region of Thailand. That's why they were keen to differentiate between different aspects of social capital, arguing that the aspects of social capital that facilitate social penalties for non-repayment of group loans can be helpful to group lending, while the aspects that inhibit social penalties can be harmful. Furthermore, the context was also important because whatever aspects of social capital permitted the imposition of sanctions in the poorer northeast did not either exist or were not

<sup>43</sup> The study thus supports Fischer's (2013) finding that social interactions promote risk-pooling regardless of the liability mode.

<sup>44</sup> For a very helpful analytical survey of the literature, see Sobel (2002).

effective in the richer central region, where sanctions did not seem to have any significant impact on group behavior.

In their framed field experiments in South Africa and Armenia, Cassar *et al.* (2007) were able to identify more clearly which aspects of social capital worked and which didn't to support group lending. They found that relational social capital in the form of personal trust between individuals and social homogeneity within groups had a positive effect on group performance. In contrast, informational social capital, in the form of simple acquaintanceship with other individuals, or an individual's general trust in society had no impact.

Finally, adopting a modern sociological approach to the measurement of social capital, Dufhues *et al.* (2011) confirmed the idea that different aspects of social capital were effective in different contexts. The starting point of their analysis is observation that the literature on loan performance within credit groups concentrates on intra-group ties to the exclusion ties to persons outside the group. An important aspect of social capital is thus left out. Furthermore, measurement of social ties has usually been rather crude, focusing for instance on role relationships like friends, relatives, or neighbors. The authors' approach to measuring social ties is more elaborate. They use a survey tool from the field of sociology that involves the use of instruments referred to as 'name generator' and 'position generator' to measure the 'personal network' respondent's personal network. These network data are then used to create measures of the 'individual social capital' of borrowers.<sup>45</sup> Using these measures and building on the well-known distinction between 'bonding' and 'bridging' social capital, the authors create four categories of social capital: bonding, bridging, bonding-link and bridging-link.<sup>46</sup> Econometric analysis relating loan repayment of groups to these measures of social capital along with a host of controls found that the effects of social capital varied according to socio-cultural context. For instance, bonding social capital was found to be effective in Thailand while bridging-link social capital was effective in Vietnam.

One final point needs to be stressed in the context of identifying the aspects of social capital that are most relevant for group lending. The aspect that has been emphasized most repeatedly in both theories of microcredit and in empirical tests of theories is the ability of group members to impose social sanctions on each other, to penalize the delinquent member who either can't or won't pay up to protect her fellow members from the obligations of joint liability. This emphasis on penalties and sanctions as the basis of co-operative behavior accords well with Coleman's (1993) observation that social capital functions as a source of social control.

But it is necessary to make a distinction between 'control and sanctions' on the one hand and 'trust and reciprocity' on the other as the basis of social co-operation. It is not clear from the accumulated evidence that punishment is the principal mechanism through which social capital

<sup>45</sup> See Glaeser *et al.* (2002) for elaboration of the concept and measurement of 'individual social capital', as distinct from 'community-level social capital' as defined in the classic works of Coleman (1988) and Putnam (1993). In the context of the credit market, and the economic arena generally, a helpful definition is provided by Karlan (2005, p.1689): "Individual-level social capital can be defined as the social skills and networks that enable an individual to overcome imperfect information problems and form contracts with others."

<sup>46</sup> Roughly speaking, bonding refers to the strength of intra-group ties and bridging refers to the strength of external ties. Bonding-link and bridging-link refer to social distance in, as distinct from the strength of, the two types of ties.

works. To cite just a couple of examples, Paxton *et al.* (2000) observe in their study on Burkina Faso: "It is interesting that very little peer pressure was measured in the face of default (one on a scale to four). Most respondents valued village harmony over the continued access to these loans." (p.645) And again, "In rural areas of Burkina Faso where most village members are related and very much interconnected in their daily lives, village social harmony is important. In addition, the existence of a hierarchical social system giving certain caste members and elders a privileged social position makes peer pressure difficult in some situations. In fact, some elderly women who had defaulted were never pressured even though the other women privately felt resentment." (p.650) Similarly, in a framed field experiment in Vietnam, Kono (2013) report that "We introduced penalties in order to capture the effect of social sanctions on the repayment decision, however, in our experiment penalties were no longer exacted once the group ended in default."

By contrast, a growing of body of evidence is beginning to emerge, which, although still quite small, highlights the importance of trust in the context of group lending. A pioneering work in this field is by Karlan (2005). He set out to investigate whether trust played any role in influencing the repayment behavior of microcredit borrowers. For this purpose, he selected a sample of the clients of FINCA-Peru and carried out a laboratory experiment to obtain measures of trust among them.<sup>47</sup> He then observed their repayment performance and tried to ascertain whether any link existed between trust, as measured in the laboratory, and group repayment, as observed in the field.

He found that to understand the role of trust, it is necessary to make a distinction between 'trustworthy' and 'trusting'. His results show that persons identified as more 'trustworthy' were more likely to repay their loans one year later, but persons identified as more 'trusting' saved less and had more repayment problems. 'Trustworthiness' is thus seen to be the key to good group performance, as opposed to the social connections etc. that purport to stand for the group member's ability to impose social sanctions. Thus, in answering the question as to why some groups perform badly, Karlan proposes the simple answer: some individuals are inherently untrustworthy.

Karlan also tried to make a finer distinction between 'innate' trustworthiness and what might be called 'contingent' (not his term) trustworthiness driven by a fear of reprisal. Although he finds evidence for both types in his experiment, the overwhelming evidence lent support to the view that innate trustworthiness was the main driver of good loan repayment.

The importance of trust is also evident from the study of Gomez and Santor (2003) on microcredit borrowers in Canada. They too measured the level of trust among potential borrowers and found that the propensity for moral hazard was reduced when 'low trust' groups are removed from the sample, leaving groups within which there existed a higher degree of trust before applying for the loan.

Finally, we can recall the findings of Cassar *et al.* (2007) that relational social capital in the form of 'personal trust' made a positive contribution to group repayment while informational social

<sup>47</sup> The type of experiment he carried out is known as a 'trust game' in the literature on experimental economics. Examples of how this experiment works can be found in Berg *et al.* (1995), Glaeser *et al.* (2000) and Barr (2003).

capital in the form of mere acquaintances did not make any difference. Their interpretation of the latter finding is especially instructive: "Since social sanctions are generally ineffectual without at least weak social ties between individuals, our study suggests that potential social sanctions may not be the most important component of relational social capital to influence group loan repayment; interpersonal trust appears to be more important." (p.F91).

The authors also note how the effectiveness of dynamic incentives also depends critically on the level of trust among group members: "Group lending is heavily dependent on dynamic incentives. Individuals have an incentive to repay group loans if they believe a critical mass of other members will do the same in order to receive future group loans. The belief that other members will contribute in the current round is partially a function of the social capital that exists within the borrowing group." (p. F90) The belief that the authors are referring to here is nothing other than the trust the group members have of each other.

It would appear that the academic literature on microcredit, at least the empirical part of it, is finally coming round to a view that the pioneers of microcredit has all along believed to be true. In a recent attempt to clarify what his original conception of microcredit really stood for, Yunus (2011) has stated that "Most distinctive feature of Grameencredit is that it is not based on any collateral, or legally enforceable contracts. It is based on "trust", not on legal procedures and system." Although he did not state it explicitly, the exclusive reference to trust implies that his conception of microcredit does not rely on informal social systems of penalties and social sanctions either. Under group lending, members are expected to repay loans not out of fear of punishment by the peers but out of trust that if they pay up either for themselves or on behalf of their peers in distress others will not try to free ride on them.<sup>48</sup> The practice of microcredit would thus seem to be based on a much more elevated conception of human nature than the self-centered *homo economicus* that the theories of microcredit typically assume. Recent empirical evidence seems to suggest that the practitioners were more right than the theorists.

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<sup>48</sup> The trust-based experiment carried out by Cassaret al (2007) indeed found evidence of reciprocity rather than free riding: "when a member experiencing a negative shock is helped by others to repay the group loan, the benefiting member is more likely to contribute in the subsequent round." (p. F91)

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The Institute of Microfinance (InM) is an independent non-profit organisation established primarily to meet the research and training needs of national as well as of global microcredit programmes. Initiated and promoted by Palli Karma-Sahayak Foundation (PKSF) on 1 November 2006, the Institute is principally funded by UKaid, Department for International Development (DFID) through its Promoting Financial Services for Poverty Reduction (PROSPER) Programme. InM has an excellent team of professionals in research, training and knowledge management. InM draws research scholars from reputed universities here and abroad. The major services that InM provides are research on poverty, microfinance, enterprise development, impact assessment and evaluation of microfinance programmes. Beside research, InM provides microfinance related training, capacity building support and knowledge management services to microfinance institutions and other development organisations.

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