Evaluating Microfinance Program Innovation with Randomized Control Trials: An Example from Group versus Individual Lending

Xavier Giné, Tomoko Harigaya, Dean Karlan & Binh Nguyen

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FOREWORD

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CONTENTS

ABSTRACT

This paper presents an application of the randomized control trial methodology to evaluate modifications in the design of microcredit programs. As microfinance becomes an even more popular tool for fighting poverty, institutions innovate rapidly in their products and programs. Policymakers and practitioners should know the relative impact of different designs, both to the client (in terms of welfare) and to the institution (in terms of financial sustainability). We discuss the current approach to evaluating product or program changes, and the reasons why more rigorous evaluations are necessary. We then discuss why randomized control trials can prove useful to microfinance institutions in identifying effective program designs in different environments. In this paper, we focus on the choice of lending methodologies—group versus individual liability—to illustrate the benefits of randomized control trials as a business tool for measuring impact and learning how to improve sustainability and growth.

1. INTRODUCTION

In the last decade, microfinance institutions (MFIs) have experienced a boom in innovations of lending products, partly fueled by donors who see microfinance as the next promise to alleviate poverty. Examples of these new products are the combination of credit with health or life insurance, business and health education, savings products, and the adoption of (or conversion to) individual loan liability. The add-in features generally aim at reducing the vulnerability of clients while contributing to asset creation, hence improving their repayment rate and the sustainability of the service. The product innovations typically result from organizations striving to extend outreach, increase impact, and promote sustainability. As in other industries, MFIs typically decide whether to adopt new strategies based on other MFIs' success with the innovations. Many new micro-lending products and approaches continue to be developed. However, MFIs must generally rely on descriptive case studies and anecdotal evidence on the effectiveness of these innovations to decide whether to implement the new strategies. The usual case study approach does not provide tangible evidence that can enable other organizations to know what changes can be expected if they were to adopt similar product changes.

In this paper, we discuss how randomized control trials can help test the effectiveness of new lending products. Given the growing innovation of lending product designs among microfinance institutions, it is critical to establish a systematic and reliable evaluation method that measures the impact of specific characteristics of a lending product. Throughout the paper we present an ongoing randomized control evaluation of group-liability versus individual-liability loans in the Philippines as an example. Many of the issues discussed in this example, however, apply to evaluations of a wide variety of microlending product innovations. We discuss a few further examples at the end of the paper.

Many microfinance institutions test new product designs by allowing a few volunteer clients to use a new lending product, or by offering to a small group of chosen clients (often their best) a new product. Alternatively, a microfinance institution can implement a change throughout one branch (and for all clients in that branch). We argue that such approaches are risky for lenders, and inferences about the benefits of changes evaluated in such a manner can be misleading. As explained in Section II, one cannot conclude from such nonexperimental approaches that the innovation or change *causes* an improvement for the institution (or the client). Establishing this causal link should be important not only for the MFI implementing the change, but also for policymakers and other MFIs that want to know whether they should implement similar changes. This is a situation in which randomized control trials are a win-win proposition: less risky (and hence less costly in the long run) from a business and operations perspective, and optimal from a public goods perspective, in that through research the lessons learned can be disseminated to other MFIs.

The paper is written primarily for microfinance practitioners. The primary operational differences between experimental and typical nonexperimental evaluations are two-fold: First, experimental evaluations include random assignment (rather than self-selected or MFI-selected) of individual clients (or groups of clients) to different program designs or products. Second, experimental evaluations are *prospective* (i.e., both participants and the control group are randomly assigned at the outset of the study), whereas typical (but not all) nonexperimental evaluations are *retrospective* (i.e., a comparison group of nonparticipants that are considered similar to participants is chosen after the treatment). The prospective nature of randomized evaluations makes planning *before the innovation is launched* the most important stage of the evaluation. This paper hopes to generate insights into the motivations for and possibilities of using experimental evaluations to assess different microfinance product designs.

The rest of the paper is organized as follows. In Section II, we discuss the problems with nonexperimental approaches commonly used in microfinance to evaluate program innovation and the reasons why the random control trial methodology is preferable. We introduce an experimental pilot approach to product innovation and the steps to design a randomized control trial in Section III. In Section IV, we discuss some key issues needed to be considered when designing an experiment. Section V presents an example of a randomized control trial on group versus individual liability in the Philippines. Section VI provides further examples of different microfinance programs in which randomized control trials could be employed to evaluate the program impact. Finally, Section VII concludes.

II. WHY WE NEED A CONTROL GROUP AND HOW WE CAN GET IT

In evaluating a lending product innovation, we typically discuss an existing loan program in which some change is being made. Therefore an existing client base already exists. Program innovation evaluations seek to compare the actual outcomes of an innovation to a program with the outcomes that would have resulted in the absence of the innovation. Because a potential client can be either borrowing in the program with the new lending feature or not, and cannot do both, the outcomes in the absence of the new lending feature are unobservable for those who receive the new product. Any evaluation then amounts to establishing the counterfactual outcome: *What program client's outcomes would have occurred had the new lending feature not been introduced?*

A. Why We Need a Control Group

In the field of microfinance, practitioners frequently evaluate new lending products by using nonexperimental designs. Most often, they let a few volunteer clients use the new lending product under study, or offer it to a small group of select clients. Alternatively, they introduce a product change throughout an entire branch of a lending institution with all clients in that branch using it. The evaluator then attributes the observed change in the clients' outcome indicator to the product change introduced, without explicitly constructing the counterfactual outcome.

This type of evaluation contains a strategic error. The problem is that in addition to the introduced product change there may be other factors that also contribute to the changes in clients' outcomes. These other factors may come from the environment in which the clients live, or may be peculiar to the clients. For example, suppose we are interested in the change in the clients' income. The observed increase in the clients' income may be due to several factors: (i) the product change introduced; (ii) general economic improvement in the region; (iii) new income-generating opportunities (e.g., a new factory in the region); (iv) the ability of the borrower to use the loan effectively; and so forth.

Consider a farming community that enjoyed unusually favorable weather conditions at the onset of the introduction of a new product. It is observed that clients' income rose during the study period. Given only this observation, an evaluator cannot be sure if the rise in income was completely attributable to the new product, or was mostly due to better harvest that results from good weather. The estimation error (also called bias) in this case is the growth in income due to better harvest brought about by favorable weather.

Accurate evaluations must control for these external intervening factors. If we could observe the same client at the same point in time both borrowing and not borrowing the new loan product, this would effectively account for any other observed and unobserved intervening factors. Since this is impossible, to isolate the effect of the product change from effects caused by other intervening factors, a control group is necessary. We need a comparator group of clients not availing the new lending product but having similar characteristics as those borrowing. Simply observing the change in clients' outcomes without a control group makes it impossible to assess the true, isolated impact of the product feature being evaluated.

Even evaluating the success of a product change based on the experience of one entire branch to which the innovation was introduced is erroneous. In such evaluations, the evaluator assumes that without the product change clients in the selected branch would have a similar experience as clients in other branches that did not receive the innovation. This approach is flawed because each branch is unique in its characteristics, with different geography, economic conditions, and human resources. As cited above, if the improvement in clients' incomes was caused by favorable weather, two branches with different characteristics can have quite divergent experiences. Or, if the branch with the product innovation being evaluated happened to have clients who are more entrepreneurial, comparing it with other branches could cause the MFI to falsely attribute the difference to the effect of the innovation.

An MFI with a sufficient number of branches could in fact compare *several* branches that receive the innovation to *several* branches that do not. This would require that branches be randomly assigned to treatment and control groups, as described below, and a sufficient number of branches be allowed for an adequate sample size.

B. Why the Control Group should be Randomly Chosen

The objective of product change evaluation is to establish a credible control group of individuals who are identical in every way to individuals in the treatment group, except that they have no access to the new product.

Establishing such a credible control group is a challenge in practice. The problem is that in reality borrowers and nonborrowers usually are different. Microfinance programs usually target certain groups of clients, such as women in poor neighborhoods. This endogenous program placement effectively makes borrowers and nonborrowers different in some set of characteristics (e.g., on average borrowers have a lower income than nonborrowers). When participation is voluntary, the fact that clients select themselves into the program indicates differences (observable or unobservable) between borrowers and nonborrowers. For instance, borrowers in microcredit programs designed to promote household businesses may be intrinsically more entrepreneurial than nonborrowers. Or in a program of credit with education designed to promote children's education, borrowers may choose to join the program because they value their children's education more than nonborrowers do.

Because of endogenous program placement and endogenous program participation, those who are not borrowing are often not a good comparison group for those borrowing. As a result, the observed difference in the outcomes can be attributed to both the program's impact and the pre-existing differences between the two groups. The comparison between the two groups will yield the accurate program impact only if the two groups have no pre-existing differences other than access to the product change being evaluated.

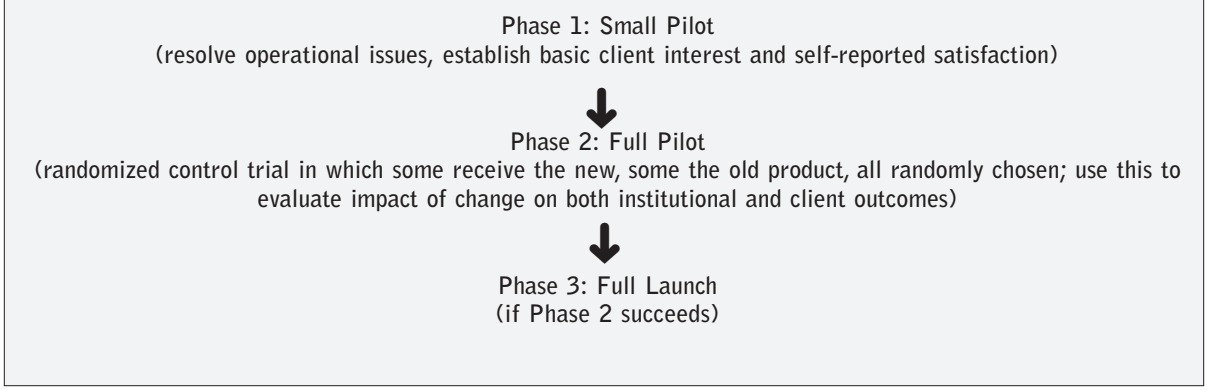
The key feature in experimental methods is random assignment. Random assignment removes any systematic correlation between treatment status and both observed and unobserved characteristics of clients. Clients (or groups of clients) are randomly assigned to a treatment group (who will borrow the new lending product under study) and a control group (who will not borrow). By construction, the randomization procedure ensures that the two groups are identical at the outset. Individuals in these groups live through the same external events throughout the same period of time, and thus encounter the same external intervening factors. The only thing different between the two groups is that those in the treatment have access to the new product and those in the control do not. Therefore, any difference in the outcomes between the two groups at the end of the study must be attributable to the product change. Random assignment assures the direction of causality: the product innovation or change *causes* an improvement for the client (or the institution).

III. EXPERIMENTAL PILOT APPROACH TO PRODUCT INNOVATION

In a randomized control trial, one program design is compared to another by randomly assigning clients (or potential clients) to either the treatment or the control group. If the program design is an "add-on" or conversion, the design is simple: The microfinance institution randomly chooses existing clients to be offered the new product. Then, one compares the outcomes of interest for those who are converted to those who remained with the original program. A similar approach is also possible with new clients, although it is slightly more difficult. In this section, we will discuss the logistics of how to change an existing product, where clients are already using some service in the program.

The flowchart in Figure 1 below presents the basic phases. Often, microfinance institutions innovate by doing a small pilot and the full launch (Phases 1 and 3), but not a full pilot (Phase 2). Hence, this paper focuses heavily on why this second step is important and outlines its basic steps.

FIGURE 1
TESTING A PRODUCT INNOVATION



A. Identify the Problem, Potential Solution, and Conduct a Small Pilot

Product innovation typically aims at solving a problem of the existing product or improving the impact and feasibility of the product. The first step is to identify the problem of the current product and potential solutions through a qualitative process. This should include examination of historical data, focus groups and brainstorming sessions with clients and staff, and ideally discussions with other microfinance institutions that have had similar problems. Once a potential solution is identified, the second step is to prepare an operating plan and small pilot.

An operating plan should include specifics on all necessary operational components to introduce the proposed change. This includes, for instance, development of training materials, training staff processes, changes to the internal accounting software, compensation systems, and marketing materials.

In order to resolve operational issues and depending on the complexity of the proposed change, a small pilot implementation should be done next. This can be done on a small scale, and is merely to test the operational success of the program design change. This initial pre-pilot does *not* answer the question of impact on the institution or the client. It instead intends to resolve operational issues so that the full pilot can reflect accurately the impact from a full launch.[1]

[1] This paper does not elaborate on this step, as much has been written on it already by organizations such as Micro-Save Africa. In this paper we put forth a process that begins where such organizations stop.

After the proposed solution has been identified and a small pilot is conducted, the "testing" is not over. It is important to know the impact of the product innovation on both the institution (repayment rates, client retention rates, operating costs, etc.) and the client (welfare, consumption, income, social capital, etc.). To measure such outcomes properly, one cannot merely follow the participants and report their changes. The flaws of this were discussed in the previous section. One needs a control group.

B. Identify Treatment Assignments

Often a proposed solution has one main change, but many minor issues that need to be decided. For instance, when testing Credit with Education in the FINCA program in Peru (Karlan and Valdivia 2005), the type of education modules to be offered was selected, and in testing individual liability, optimal loan size was determined. A careful experimental design can include tests of such subquestions. Specific examples will be provided below when we discuss testing group versus individual liability. These questions often arise naturally through the brainstorming questions. Any contentious decision is perfect for such analysis, since if it was contentious then the answer is not obvious!

C. Sample Frame and Sample Size

The sample frame is the pool of clients (or potential clients) who are included in the impact study. One will assign clients (or potential clients) randomly to "treatment" or "control" groups (that is, clients will be divided randomly into two groups. Members of one group will get the innovation and members of the other will not). Two types of sample frames should be considered: existing clients and new clients. When the innovation is a change to an existing product, an initial test can consist of existing clients. Defining a sample frame of potential clients can be more difficult. The following section shows how this is being done with the group versus individual liability evaluation in the Philippines.

Determining necessary sample size is also key to a successful evaluation. To calculate the necessary sample size, one needs to consider (i) what a "successful" outcome looks like (e.g., if repayment rates are 90%, would increasing them to 94% be considered satisfactory enough to then warrant a full conversion to a new product?); (ii) what the current level is for the outcome measure; and (iii) if the outcome measure is not a binary variable (e.g., being in default), then one needs to know the typical variation (i.e., the standard deviation) of the outcome of interest.[2]

[2] We recommend the free software *Optimal Design* for helping to determine sample sizes. It can be downloaded from http://sitemaker.umich.edu/group-based/optimal_design_software.

IV. ISSUES TO BE CONSIDERED WHEN DESIGNING AN EXPERIMENT

A. Spillovers

The internal validity of experimental designs rests on integrity of the data from treatment and control groups. The results are improved when treatment and control groups remain intact throughout the study.[3] However, in microfinance programs, this cannot always be guaranteed and spillovers may arise. With proper care and information about noncompliance, this can be dealt with in the analysis (although if the noncompliance is severe, this could irreparably damage the study).

There are two types of spillovers to discuss. One merely affects the experimental design, and involves what to do when someone from the treatment (or control) group learns about the existence of the other group and asks why they are not receiving what the other person is receiving. We will call this the "experimental spillover." Experimental spillovers are often more of a concern in theory than in practice. However, this does not mean they should be ignored. They need to be minimized, and also should be carefully recorded because they may affect the results of the evaluation. For instance, in the group versus individual liability experiment in the Philippines (discussed in more detail in the next section), we identified "sibling" *barangays* (neighborhoods or villages) as those that are contiguous and for which there is much social interaction. We treated them as one *barangay* for the sake of randomization, thus ensuring that no "sibling" *barangays* were split and received different program designs.

Still, all experiments must be prepared for the groups to learn about each other. Staff must be trained in how to deal with these questions. We have found that candor in explaining the rationale, ie., that the MFI is considering making a change and is testing it out carefully on a subset of clients, works best when clients ask "why did I receive X when my cousin, who is also a client, is receiving Y?" Clients need to know that they had an equal and fair chance at being selected for the change, that it was not done preferentially, and that if the change works well, then it will be expanded fully. Ideally, the MFI can record information about all such inquiries, because learning about such interest (or disinterest) can help when evaluating the outcome and deciding whether to proceed with a full launch of the change.

The other type of spillover has to do with the indirect effects brought about by the program—not on clients but on others, including clients' families, neighbors, or community members. We will call this second type "impact spillovers." Impact spillovers can be both good and bad. A "good" spillover refers to the effect on other people of providing one person with a particular service or product. By only treating one person, often you treat many more.

[3] In some experimental designs, treatment is not mandatory for the treatment group, and/or control group is permitted to get treated. These are called *encouragement* designs, in which everyone receives the treatment but only the treatment group is given an encouragement to participate in the treatment (but is not required to participate). The control group is not given the encouragement (but is allowed to participate in the program if they choose so.) These work as long as the encouragement leads to a higher enough take-up rate in the treatment group than the control group. See Ashraf et al. (2006) for an example of such a design.

Deworming interventions are a perfect example of this. In a study in Kenya, researchers found that deworming school-aged children did *not* pass a cost–benefit analysis relative to other interventions when you only consider the *direct* effect. However, when you take into account the *indirect* effects as well (the "spillovers" in this situation take place because worms are passed from one child to another through the dirt in communal play areas), the intervention does indeed pass a cost–benefit analysis (Miguel and Kremer 2004). In microcredit, several examples exist for spillovers, both positive and negative. For credit with education programs, clients may share what they learn with others in their community or family. For credit itself, the increased business of one client may create employment in the community. For group lending, it may help build social capital among the group members, which may influence others to form similar bonds (due to observing the success of the group members). A bad spillover may come from competitive pressures: if the MFI funds an individual to start a particular type of business, this may adversely affect other similar businesses in the community (although it might increase aggregate welfare for the community by lowering prices or improving product quality for the consumers in the community).

B. Ethical Considerations

Stakeholders sometimes have ethical arguments about randomization, as some perceive them as arbitrary and "unethical", depriving the controls from positive benefits. This argument rests on two assumptions that typically are flawed.

First, this concern is based on the assumption that the program change is unequivocally good. If there is no doubt that the change should occur, that it not only will improve the situation but that it will do so more than any other change, then indeed testing the change would be a waste of resources. Such situations are rare, however. More often than not policy changes are debated and although strong hypotheses may exist, there is no adequate evidence to know unequivocally that the change will yield positive results for everyone. The MFI initiating a product change must decide the amount of resources it is willing to invest in testing the change based on how much uncertainty there is regarding the consequences of the change. If there is doubt about the efficacy of the change, then the experimental test may indeed be the most reasonable choice, so that the organization learns through the careful test not to implement the project further.

Second, this ethical criticism assumes unlimited resources for the change to reach everyone in the program. In many cases, this is not true for either budgetary or logistical reasons. For example, if the intervention is credit with education, the training of staff to provide the education modules is both costly and time consuming. Large organizations cannot do this all at once, but rather usually stagger the training of their employees on how to teach the material to the clients. In this way, a randomized rollout of the product can be offered to just as many clients as the organization has the capacity to reach, with or without the experiment.

C. Cost of Randomized Experiments

Experimental methodologies are often perceived as more costly than nonexperimental methodologies. Relative to no evaluation at all, certainly an experimental evaluation costs more in the short run. Yet an experimental evaluation may be *less* costly in the long run if the results from the evaluation help to guide the long-term decisions and planning for the institution. Take for example an MFI considering whether or not to increase the interest rates of its loans. An increase in interest rates may meet the MFI's financial targets in the short run. However, raising interest rates too high will drive away customers and reduce the loan repayment rates. This in the long run may erode the surpluses generated by the program's clients. It is therefore critical for the MFI to understand the net effect of an interest change to set the most desirable rate. Thus, spending some money now to have a credible assessment on the client's response to the proposed interest increase is by far less costly than saving the money and making a wrong decision.[4]

Relative to nonexperimental evaluations, experimental evaluations are often less costly in the short run and certainly even less costly in the long run, when the benefits of more accurate results are factored in. When household surveys are used they typically encompass the largest component of the budget. However, the cost of collecting data for a nonexperimental evaluation is often more expensive than for an experimental evaluation because nonexperimental evaluations usually require larger data sets to process complicated econometric models. The analysis for an experimental evaluation, if designed correctly, is quite simple: one can obtain the answer simply by comparing mean outcomes between treatment and control groups.

The bottom line is that the cost of random experiments must be judged within particular contexts. The literature on microfinance provides no specific information on the overall or unit costs of evaluations but a vast array from a few thousand to several million dollars, depending on the questions studied and the number of MFIs involved (Hulme 1997).[5] A simple experiment, such as an evaluation of a program innovation, may indeed not require surveys, but rather just the MFI administrative data (e.g., repayment rates), to measure the efficacy of the change. In this case, the data can be retrieved at no cost from the MFI's accounting software or management information system. The cost of the experiment is merely the management time required to design the experiment and train and motivate the staff in why the program innovation is being tested in this manner, as well as to analyze the data. If the organization is undergoing change to its products or processes, then enacting this change with an experiment rather than ad hoc may not even add any further costs.

[4] More generally, findings from good random experiments can help avoid costly mistakes. For example, Duflo and Hanna (2005) find that in an education program in India adding a second teacher to the classroom makes no improvement in students' test scores, influencing redirection of funds to other more effective initiatives. Such decisions can have vast financial implications for programs at a national level.

[5] One example of the cost of a comprehensive nonexperimental evaluation, reported by Montgomery et al. (1996), is the 1994 impact evaluation of BRAC's credit program that cost US$250,000.

V. EVALUATING GROUP-LIABILITY VERSUS INDIVIDUAL-LIABILITY LOANS: THE PHILIPPINE CASE

A randomized control experiment was designed in the Philippines to evaluate the impact of group-liability versus individual-liability lending programs. While group lending programs are still prominent in microfinance practice, a small but increasing number of microfinance institutions are expanding rapidly using individual lending. As these institutions explore the benefits of individual-liability loans for the poor, there is an opportunity to apply randomized control trials to rigorously evaluate the impact of the innovation compared to the group-liability program.

Indeed, given the popularity and apparent success of the two methodologies, as well as the lack of rigorous evaluations of both of them, it is difficult to know the real advantages and disadvantages of each—and therefore to formulate policies on this matter. An example like the one proposed in this paper can fill this void and provide useful guidance to the microfinance industry at large.

A. Motivation of the Study

Unlike individual liability, under which each borrower is only responsible for her own loan, group liability requires members of a defined group to help repay the debt of other members when they cannot repay. Unless the group as a whole repays every member's amount due, no member will be granted another loan. The Grameen Bank in Bangladesh developed a lending methodology based on group liability that is now employed by many nongovernment organizations and microfinance institutions around the world. The success and popularity of this approach can be linked to its numerous perceived advantages. (Some of the advantages, while associated with group liability, are not inherent to group lending alone, as will be shown below.) Such oft-cited advantages include:

(i) Clients face both peer and legal pressures to repay their loans.

(ii) Clients have incentives to screen other clients so that only trustworthy individuals are allowed into the program.

(iii) Transaction costs are low as clients meet and pay at the same time and location.

(iv) Training costs are cheaper as clients all gather periodically.

(v) Clients have incentives to market the program to their peers, thereby helping to bring in more clients.

(vi) Groups may help build social and business relationships.

As is the case with most methodologies, group liability is not without potential disadvantages. These include:

(i) Clients' dislike of the tension caused by peer pressure, which could lead to lower client satisfaction and hence higher dropout.

(ii) Older clients tend to borrow significantly more than newer clients, and this heterogeneity often causes tension within the group, because new clients do not want to be responsible for others' much larger loans.

(iii) Group lending could be more costly for clients since they are often required to repay the loans of their peers.

(iv) Clients dislike the longer meetings typically required for group lending.

(v) Default rates could be higher than if there were no group liability because bad borrowers can bring down good borrowers (i.e., once your peer has gone into default, you have less incentive to pay back the loan yourself).

(vi) Default rates could be higher than if there were no group liability because clients can "free ride" on good clients. In other words, a client does not repay the loan because the client knows that another client will pay it for them, and the bank will not care because they still will get their money back.

(vii) Villagers with fewer social connections might be hesitant (or even unwelcome) to join a borrower group.

Given the existence of these potential negative aspects and the fact that the last three advantages listed can be obtained without resorting to group liability,[6] there is a strong case to be made for an MFI to experiment with offering individual loans to their clients. This concern over the excessive tension generated among members by imposing group liability is precisely the main motivation for the shift from group-liability to individual-liability loans. Practitioners worry that the conflict among members could not only lead to high dropout rates and affect the sustainability of the program, but also potentially harm social capital so valuable to the poor who lack economic security.

Two features of this innovation make it a perfect case for a randomized control evaluation. First, there are conflicting arguments for and against individual liability loans, and the net impact of such programs compared to group-liability lending programs is not clear. Besides the obvious benefit of removing group liability for the clients (reducing pressure and tension among members), the individual liability loans may also benefit the lending institution by increasing the client retention rate (because clients prefer individual liability) and thereby the MFI's portfolio. However, the lender will lose a crucial enforcement mechanism when group liability is removed. It would negatively affect the repayment rate if none of the group members is willing to make a voluntary contribution to cover the repayment of defaulted members. Using a randomized control trial, the relative merits of group-liability versus individual-liability loans for both clients and institutions can be evaluated.

[6] For instance, under the methodology employed by the MFI Association for Social Advancement, clients still meet together but are individually liable for their loans.

Secondly, in recent years individual-liability loans in the microfinance community have gained popularity around the world. Although replication of the study is necessary to generalize the results of this particular evaluation, it will help identify the effective environment and design of the program, benefiting not only the lending institution and its clients, but also the entire microfinance community. As such, it can play an important role in both policy making and product design.

B. Objective of the Study and Hypotheses

We collaborate with Green Bank, a commercial bank based in Mindanao, as it expands its microfinance operation in the Visayas islands, to conduct a pilot-testing experiment to evaluate individual-liability loans (see Box 1). In this experiment, we seek to evaluate the following impacts:

(i) relative impact of group versus individual liability on clients and their communities,

(ii) relative cost and benefit of group-liability versus individual-liability loans for Green Bank, and

(iii) impact of credit on individuals and their communities.

Specifically, we pose the following questions:

(i) How does group relative to individual liability affect institutional outcomes such as repayment, client retention, loan size, and operating (labor) costs?

(ii) Does group liability motivate peers to monitor and/or enforce repayment of loans?

(iii) Does group liability motivate peers to select less risky clients for a bank?

(iv) How does selection on other dimensions (e.g., poverty, social connectedness) differ under group versus individual liability? Are those less-connected (hence perhaps less likely to have good informal social safety nets) less likely to participate in group lending than individual lending programs?

(v) What is the impact on the household, enterprise and community from a microfinance institution offering credit in their community? How does this impact differ for group versus individual liability loans?

(vi) What are the impacts, positive and negative, on social networks from group versus individual liability loans?

C. Experimental Design

The experimental design employs one strategy for "existing areas" and one for "new areas." The "existing areas" strategy involves converting existing centers to individual-liability loans. The advantage of this approach is that one can attribute the differences between group

and individual liability to differences in the loan liability, and not to differences in the individual *characteristics* of the clients per se. This is true because all existing clients joined the program under a group liability scheme. Thus there is no selection bias as would be inherent in comparing the outcomes of clients who have chosen group liability to the outcomes of clients who have chosen individual liability. The disadvantage is that there may be differences between clients who have enrolled in a group-liability program and the borrowers that would enroll in an individual-liability program. Therefore while the results from the "existing areas" strategy will be accurate for those who are willing to sign up for group liability, we cannot determine from this strategy alone how the product will work among clients who know from the outset that they are joining an individual-liability program.

It is then important to understand these potential differences among borrowers, especially when generalizing the results of the cost and benefits of group liability. For this reason, the study includes a "new areas" strategy by working with Green Bank as it expands to new areas in the eastern coast of Leyte (Tacloban) and the neighboring islands of Cebu and Bohol.

Box 1
Group vs. Individual Loan Programs in Green Bank

Green Bank is a commercial bank established in 1975 and currently operates in northern Mindanao and the Visayas islands. Its microfinance department was formed in the late 1990s, and the group-liability lending program BULAK was launched in 2000.

BULAK follows a modified version of the Grameen approach. In BULAK there are four different units: individuals, groups, centers, and branches. Up to five low-income women come together to form a group. The group is formed by them and not by the bank. Then, three to six groups come together to form a center. The center is where all of the groups hold their weekly meetings and collect payments. Typically a *barangay* (submunicipality) will have one center. In total, Green Bank has over 15,000 clients.

All loans given under the BULAK program are to be used for expanding the client's microenterprise. The initial loan is between 1,000-5,000 pesos (roughly $18–90) and increases by P5,000 after every cycle, such that the maximum loan size in the 5th cycle is P25,000. However, the loan size is a function of the repayment on the last loan, attendance at meetings, business growth, and contribution to their personal savings. Loans are charged an interest rate of 2% per month over the original balance of the loan. The client has between 8-25 weeks to repay the loan, but payments must be made on a weekly basis.

As part of the BULAK program, clients are also required to make mandatory savings deposits at each meeting. Each member has 100 pesos ($1.80) deducted from every loan release. In addition, 10% of their weekly due amount (principal plus interest) is deposited in their individual savings account. Member savings may be used to repay debts and may also be used as collateral, although in this last case there are no fixed rules. Finally, P10 ($.36) per meeting are required for the group and center savings. These center savings cover mostly the construction of the center meeting place, and are only used as a last resort to repay member loans.

The individual-liability program (BULAK II) being pilot tested in the experiment has all the features of BULAK program, including weekly repayment meetings and consolidation of repayment by center groups, except the following two features: first, no client is liable for her group members' loans, and second, there are no mandatory center and group savings. All Center activities will now be paid individually on a per-activity basis.

This expansion also provides a unique opportunity to test the impact of the credit itself. A randomized program placement strategy is employed to assign *barangays* to either individual or group liability, and also to a control group. This allows us to test the impact on household, enterprise, and community outcomes from receiving either group or individual liability loans.

D. BULAK-2: Pilot Phase

Since the change to individual liability is significant, careful testing is required before it can be fully implemented. A small pilot test was conducted in Leyte, which also served as the location of the full study. Green Bank has 169 lending centers in Leyte, with an average membership of 25 individuals (or 5 groups) per center. For the pilot phase, one center from each credit officer's portfolio was randomly chosen, 11 centers in all, to convert to the new individual-liability methodology. This random selection of centers is critical. If, for instance, one were to pick only the best centers, then one would not know whether the results were generalizable to the inferior centers. One might falsely conclude that individual liability is better, when in fact perhaps it is only good for the best groups.

This first pilot phase began in August 2004. In early November 2004, 24 more centers were randomly converted. The full pilot phase as of May 2005 includes 85 converted centers and 84 original (group liability) centers.

E. New Area Plan

Evaluating the relative impact of group-liability versus individual-liability loans poses a challenge in the conventional nonexperimental evaluation method because the two programs attract different types of clients—unobservable heterogeneity between the two groups of clients may confound the results. In a randomized control trial, random selection of the sample allows comparison between the two groups. The procedure to start operations in new areas is novel and another contribution of the study. It consists of two parts, the identification of eligible *barangays* and of potential clients through a marketing meeting.

(i) ***Identification of the Barangays:*** The first step is to gather basic information about the barangays from the municipality office. This information is mainly used to exclude barangays with low population density as it is deemed too costly to start operations in these areas. The credit officer visits the selected barangays and conducts a survey to verify the following criteria: (a) number of microentreprises, (b) residents' main sources of income, (c) *barangays'* accessibility and security, and (d) perceived demand by the residents for microcredit services. The survey is administered to the secretary of the *barangay*, typically the person with the most information about the administrative aspects of the *barangay*.

(ii) ***Census of Microentrepreneurs:*** The purpose of the census is to construct the sample framework to assess which businesses are interested in credit and could eventually be clients of Green Bank. The census records basic information regarding the size

of their business and their credit history. While it is being conducted, they are told about the marketing meeting.

The sample villages identified are randomly assigned to the following four groups.

(i) *BULAK*: Green Bank will offer group-liability loan program

(ii) *BULAK to BULAK II*: Green Bank will offer group-liability loans and remove group liability after the first loan cycle

(iii) *BULAK II*: Green Bank will offer individual-liability loan program

(iv) *NO CREDIT*: Green Bank does not offer their services (control group)

It is important to note that our sample in Groups 1, 2, and 3 is NOT the actual borrowers, but rather the "potential clients." This is because if we were to compare those who choose to participate in the program in the areas in which the program is offered to those in the control group, our estimate of impact will suffer from self-selection bias. We would capture, in addition to the true effect of the program, the extra motivation of the clients who decide to enroll. However, instead of watering down our estimate of average impact (calculating the average outcomes among those who do participate, as well as all those who do not) we can improve our estimate—and keep it unbiased—by employing a technique called propensity score matching and weighting the impact estimate by the likelihood that each individuals becomes a client. The key in this sample formation is to identify those who "would" receive a loan from Green Bank if Green Bank were to operate in the village. Propensity score matching uses the baseline characteristics of the potential clients to statistically identify those most likely to participate in the program. We measure the impact on each client by comparing their outcomes to the outcomes of those in the control group with a similar propensity to participate.

Because the sample selection in the four groups is consistent, sample bias in subsets from these groups is consistent, and we can compare the impact between any of the four groups. This experimental design provides a unique opportunity to measure the clean impact of credit by comparing Groups 1, 2, and 3 with Group 4.

F. Measuring Impact

We measure the impact by comparing different outcome measures between the treatment groups and control groups. The impact of the program can be measured at three different levels: individual client, community, and institutional. By looking at the impact not only at the client and institutional levels but also at the community level, we can evaluate the broader implication of the program and how it could affect the local economic status. In order to make necessary comparisons, the data are collected in three different methods:

(i) *Baseline Survey*: the information on sample villages and clients is collected *before* the experiment takes place. This information is used in validating the randomization as well as in analyzing the *post*-experiment impact. In the Green Bank study, we collected information on loan history, business status, household well-being

(economic and psychological), social networks, and risk preferences of the sample individuals. By definition, randomization will create comparable treatment and control groups; however, it is always a good idea to validate the random assignment by checking some key variables from the baseline survey before the launch of the experiment (comparing the means of the variables for treatment and control groups and ensuring they do not differ significantly).

(ii) *Follow-up Survey on Clients*: The survey conducted after the study period will be used to evaluate the program impact. The information collected will include clients' performance in the Green Bank program, clients' business performance, as well as their household welfare.

(iii) *Activity-based Cost Exercise*: This exercise records all activities of development (loan) officers. By comparing the total time spent on BULAK II versus BULAK centers, we will be able to calculate the cost for the institution of the individual-liability program relative to the group-liability program.

G. Replication of the Study

Given the decision by several MFIs to employ individual-liability loans, it is not only in Green Bank's interest, but also in the interest of the microfinance community as a whole to learn the impact of group-liability versus individual-liability programs. However, we cannot draw a general conclusion from the result of this specific program evaluation in the Philippines. Only after replications of the evaluation, with different MFIs in different places and with different clients, can we make more general statements about the impact of group-liability versus individual-liability loans.

Many factors may make the results of the evaluation unique to Green Bank and its context. The following are some of such factors:

(i) *Initial Social Network*: The importance of social networks among program members depends on many exogenous factors: culture, village size, village economic activities. The more economically vulnerable clients are, the more they rely on their social networks for support. If this is the case, removing group liability among uncollateralized clients may result in better repayment performance among lower-income groups than among those with more stable income flows.

(ii) *Type of Clients*: For example, Green Bank targets small female entrepreneurs in rural areas. There is a large volume of literature that concludes that female borrowers repay better than male borrowers. The impact of group-liability versus individual-liability loans could well be different between the gender groups.

(iii) *Type of Institution*: Green Bank is a commercial bank, thus the financial sustainability of its microfinance programs is a critical part of its operational goal. The implications of cost–benefit analysis would be different for Green Bank than for subsidized institutions.

(iv) *Context*: In most areas where Green Bank operates, it competes for clients with other lenders. For the most part, other lenders tend to offer group-lending loans, so the impact of introducing group liability will be affected by the presence of other lenders and their specific products.

VI. PILOT EXPERIMENTAL APPROACH FOR OTHER LENDING PRODUCT INNOVATIONS

In the previous section we used an example from a randomized control trial designed to evaluate the impact of group-liability versus individual-liability programs with Green Bank in the Philippines. This experimental pilot approach is applicable to many other innovations whose net impact on clients and benefit for the institution is not known. Below are some examples of such cases.

A. Credit with Education

Credit with Education is one of the most popular add-in features of lending programs; yet, the impact of education programs is not well known. Educational programs such as business training may help clients on financial management and lead to more efficient allocation of credit, a service clients may value. However, if these educational programs are made mandatory, clients might find it time-consuming and leave the program all together. See McKernan (2002).

B. Mandatory/Voluntary Savings Rules for Lending Programs

Savings schemes in lending programs aim to reduce clients' vulnerability to unexpected negative economic shocks, as well as to improve clients' financial management skills by encouraging them to make small regular savings. However, if clients lack the discipline to save, they might view mandatory savings merely as an additional burden, reducing the number of borrowers.

C. Savings Products with Commitment Features

Due to self-control or household (e.g., spousal) control issues, some people prefer to have commitment savings products in which deposits are withheld from their access until a specific savings goal is reached. Such products take on many forms, but little empirical evidence of their effectiveness currently exists (Ashraf et al. 2003, Ashraf et al. 2006).

D. Frequency of Payments

Frequency of payment varies from program to program. MFIs generally demand relatively frequent repayment schedules (often weekly) while clients often prefer less frequent payment. Particularly for those who have inconsistent income flows, frequent repayment schedule could increase the default rate.

E. Health/Life/Disability Insurance

Insurance offered with credit aims at reducing vulnerability of clients. Clients as well as microfinance institutions may benefit from the insurance services as they are insured for certain types of economic shocks. However, insurance services may cause *adverse selection* by attracting riskier clients to the program, which could lead to higher default rates. Or insurance could cause *advantageous selection* by attracting risk-averse clients, which could lead to lower default rates.

F Local Public Goods (Community "Empowerment" Training)

The mission of some microfinance institutions is not merely to increase credit access for the poor, but also to empower the economically/socially marginalized sector of the population. Empowerment training may increase impact on clients by improving women's mobility and ability to make economic decisions; or it could increase client exit if the clients do not have an interest in the training.

G. Human Resource Policies

Providing credit officer incentives could improve repayment rates if credit officers use enforcement power appropriately. However, the incentive schemes could cause conflicts between the officers and clients because the officers now have a personal stake in better repayment rates. Such friction between the credit officers and clients may affect the retention rate.

H. Interest Rate Policies

Little is known empirically about the elasticity of demand with respect to interest rates (the extent to which clients are willing to accept higher interest rates, and the extent to which demand for loans increases at lower interest rates). Furthermore, much economic theory has been written about how higher interest rates might drive down repayment rates through information asymmetries such as adverse selection and moral hazard. Some authors try to examine these issues using survey data; see for example Dehejia et al. (2005) and Gross and

Souleles (2002). Experimental studies can be done to study the relationship between interest rates, demand for credit, and repayment rates. See Karlan and Zinman (2005) and Ashraf, Karlan et al. (2006) for an example of such a study.

VII. CONCLUSION

In this paper we have examined the flaws in methods commonly used to assess the impact of microfinance programs and showed that modifications to the design of microfinance programs may be best evaluated through randomized control trials. Randomized evaluations can be performed ethically and cost-effectively, and the accuracy of their results makes them valuable both to the institution implementing the evaluation and to the microfinance community at large. Through the example of individual-liability loans in the Philippines we showed the steps involved in performing an experimental evaluation. Many questions remain, however, and until an evaluation has been replicated in a variety of settings, it remains unknown whether a particular innovation is likely to work for other programs. This is the nature of *all* evaluative work, regardless of the methodology employed. To stimulate the experimental evaluations of more program innovations we have provided a list of several modifications that could be tested using a similar methodology.

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