

## **Revisiting savings: a replication study of facilitating savings for agriculture**

By: Jesper Stage and Tharshini Thangavelu

### **Introduction**

The poor in developing countries have long, effectively, been shut off from financial services, making investment and capital formation problematic (Armendáriz and Morduch, 2005). There are a number of different reasons for this, but key reasons identified by economists for exclusion from credit access have included information problems, selection problems and other problems inherent in the credit allocation process (see e.g. Hoff and Stiglitz, 1993; Hermes and Lensink, 2007). Microfinance has seen massive interest from policymakers and donors in recent decades, due to the obvious potential for achieving poverty alleviation by making investment and capital formation accessible to the poor in developing countries. However, microcredit also faces key problems, for many of the same reasons that have previously kept financial services out of reach for many of the poor, and this is especially true for agriculture. Thus, for instance, Andersson et al. (2011), studying an aquaculture district in Bangladesh, found that formal microlenders continued to have substantially greater problems selecting those farmers who were good credit risks. In practice, what little credit has been made available to farmers has therefore tended to be linked to purchases of specific inputs, which have not necessarily been made available in the quantities that the farmers would have purchased if they had had better access to credit and greater leeway in allocating the credit themselves between different inputs.

In order to improve farmers' ability to build up capital in their farming activities through microfinance, many policymakers and donors have therefore favoured improving access to savings, rather than improved access to loans; the main reasons why farmers have had poor access to savings facilities in the past have been linked to e.g. the costs of maintaining bank branches or of managing large numbers of small deposits and withdrawals, problems which are potentially easier to address than the problems in credit markets. In theory, improved access to low frequency savings (e.g. consumption smoothing between years, and setting aside funds in order to finance large, but rare investments) and high frequency savings (e.g. consumption smoothing over a single year or shorter period, and setting aside funds in order to pay for inputs to be used in farming later in the same year) could both improve farmers' well-being and, in many cases, raise income and improve capital formation (Armendáriz and Morduch, 2005). The prevalence of informal savings mechanisms in many developing countries suggests that there is a real demand for such savings mechanisms (see e.g. Rutherford, 2000) and that improving access to formal savings, which would be less sensitive than informal savings to community-level income shocks (see e.g. Dercon, 2002), could improve well-being among the poor.

However, whether improved access to savings vehicles can also raise longer-term income and improve capital formation to the same extent as improved access to loans is uncertain; many of the targeted farmers have extremely low incomes, partly because they have such limited farming capital, and thus also low capacity to save out of their current income. Apart from poverty, there are numerous other issues that make it difficult for households to save money. Such issues include short-term pressures to spend money on relatives and other members of the household's social network (see e.g. Platteau, 2000, or Anderson and Baland, 2002); self-control problems linked to own impatience and/or hyperbolic behavior which lead to cash on hand being spent faster than initially planned (potentially providing a need for committed

savings mechanisms where deposits can only be accessed at pre-specified times, see e.g. Gugerty, 2007, or Ashraf et al., 2006); and thefts. Even with access to formal savings, many of these issues would remain (possibly in diminished form, but still) and it is thus uncertain how much households who are given access to new savings devices would actually save in practice and how much difference this would make for their farming. Given that in many developing countries improved access to savings has become the default option for improving farmers' access to financial services, it is surprising that so little research has been done on what impact this improved access actually has.

### **Brune et al. (2016)**

Brune et al. (2016), who study how improved access to savings and access to activities encouraging savings actually affect savings and subsequent farming patterns in Malawi, is thus an important contribution to the literature on microfinance. In their study, they collaborated with a local bank in Malawi in a field experiment where farmers (in both the treatment and the control groups) were given general encouragement to save income from their harvest in order to more easily pay for agricultural inputs at the beginning of the next planting season. Farmers in the treatment groups were also offered help with setting up personal bank accounts where their harvest income would be deposited, while farmers in the control group continued to be paid in cash. In one set of treatment groups, the bank accounts were ordinary bank accounts where deposits and withdrawals could be made at any time; in another set of treatment groups, apart from these ordinary bank accounts farmers were also offered committed bank accounts where some or all of their harvest income could be deposited and where withdrawals could only be made at a prespecified time. Finally, the bank account treatment was combined with a lottery treatment, which was also intended to spur savings; for both types of bank accounts, farmers were assigned into three groups, one "control" group with no lottery, one group where farmers could win money based on how much money they had on average in their bank accounts during selected qualifying periods, and one group where farmers could also win money in a similar lottery but where the money in the bank account was also publicly announced.

The researchers found that helping farmers set up accounts did indeed lead some of them to choose to have their harvest revenue deposited into the accounts after harvest, and this also led to more money being spent on inputs with resultant higher yields and higher income from the next harvest. It might thus seem that the intervention had exactly the intended effect. However, a surprising finding was that the extra money used to buy inputs at the beginning of the next planting season was, to a considerable extent, not bank deposits deposited subsequent to the harvest. Instead, farmers withdrew most of the money that they had deposited after the harvest well in advance of the next planting season, usually shortly after depositing it in the first place, but nonetheless spent more money on inputs than the control group at the beginning of the subsequent planting season. In a similar vein, the group with committed bank accounts spent more on inputs than either the control group or the group that was only offered help with setting up ordinary bank accounts, but the amounts deposited in the committed accounts were not great enough to account for the difference. It thus seems that although the intervention did indeed lead to more money being used for input purchases at the beginning of the next planting season, this effect did not come through the anticipated channel but, rather, through some other effect on farmers' behavior. The two different lottery treatments had no statistically discernible additional impact on people's behavior compared to the "control" groups who were given access to bank accounts but no lottery; these sets of groups were relatively small, so fairly large effects would have been needed to produce statistically

significant results, and as noted most farmers kept so little money in their accounts that the direct incentive effects of the lotteries must have been small.

## **Replication strategy**

### **- Push-button replication**

We will begin with a push-button replication, rerunning the authors' code on the dataset compiled by the authors and confirming that we get the same results as the original authors. The results from this first step will be clearly demarcated from the results from other parts of the research.

### **- Pure replication**

The next step will be to write our own code and run this on the same data. We do not know at this stage what statistics package(s) the authors used for the original study, but will ensure that we use a different package for this step in the replication in order to confirm that the results are not sensitive to idiosyncracies in individual software packages. As the regressions that the authors ran on their dataset were relatively simple, we do not anticipate major issues here, other than possibly issues related to reconstructing the variables used. The results from this second step will be clearly demarcated from the results from other parts of the research.

### **- Robustness checks**

The key challenges will appear when we carry out sensitivity analyses in order to examine how robust the results are to minor variations in the regressions and in the data selection strategy. We would like to note a few issues that we think deserve further study.

It is clear from the results that the farmers with access to committed savings vehicles tended to maintain higher savings balances and, in particular, spend more on inputs than the control group. However, it appears from the results that most of the statistical significance in the pooled group is also being driven by the committed savings group for both these outcome variables; the results for the farmers who only had access to savings vehicles where they could withdraw the entire balance at will are not statistically significant for either of the variables. We would therefore like to examine how sensitive the results for the committed savings group are to the removal of potential outliers. Two approaches frequently used in the literature to reduce the importance of outliers are trimming and winsorizing (Dixon, 1960), and we propose to try both for the input expenditure variable. For preset percentages, this entails identifying values for the input expenditure variable that are in the lowest or highest percentile in question of the variable, and then either dropping those observations altogether (trimming) or replacing the input expenditure variable with the variable's value at the edge of that percentile (winsorizing). It is clear from the paper that some outlier management has already been carried out in the reported results, but we propose to do this in a systematic fashion and (a) trim and (b) winsorize the 1 and 5% of respondents with the highest input expenditure from each of the groups, in order to examine how much impact this has on the results. Some reduction in statistical significance is of course only to be expected from this procedure, but if removing a very small group of extreme outliers affects the results dramatically, that would in itself be an important finding. From a policy perspective, outcomes for a few outliers is usually not going to be of interest, so if those outliers account

for most of the estimated impact of the intervention, then that would affect how generalizable the results are likely to be.

Linked to this, the “ordinary savings vehicles” groups reported spending significantly more on inputs, on average, in the baseline survey carried out at the beginning of the experiment. We will therefore remove outliers in initial input spending as well, again trimming and winsorizing the 1 and 5% of respondents with the highest values from all groups, in order to examine how much impact this initial discrepancy has on the results.

More generally, while it is reported in the paper that baseline variables from the first round of the survey were included in the regressions, the estimated coefficients for these variables are not reported in the paper or in the online appendices. This makes it difficult to judge how important these variables actually are for the outcomes. Given our current understanding of the paper, we intend to examine variable exclusion in two separate approaches. The first of these approaches will be to drop variables that are not statistically significant in any regression and see whether this affects the results for the variables that remain. The second approach will be to drop all covariates except the stratification variables.

#### - **“Theory of change” analysis**

Here as well, we would like to note one point which we think deserves further attention and which we think could help clarify what the results imply for our understanding of farmers’ behavior and for future policy interventions.

Not all farmers who were offered assistance in opening an account actually chose to open an account; only about one in five did so. The authors focus on intent-to-treat estimates and include farmers who declined assistance in their respective treatment groups. While we agree that this is the main set of estimates to focus on, it would presumably also be of interest to policymakers to know what effect a policy has on those who actually choose to use it, not merely what average effect a policy has on those who are eligible for it, whether they make use of it or not. We would note, for instance, that a common approach in other agricultural extension activities is to ensure takeup by some farmers, in the hope that the success of these early adopters will gradually lead to takeup by other farmers as well. Something similar could potentially happen here; even if only relatively few farmers adopt the new savings vehicles at an early stage, if they have noticeable success with this then that should encourage adoption by other farmers as well. At the same time, it appears likely that the farmers who did adopt the new savings vehicle may have had different characteristics from those who did not, such that merely comparing actual outcomes risks over- (or under-)estimating the impact of adopting the vehicle. We therefore propose comparing outcomes for the actually treated farmers to outcomes for the control group, in order to provide an indication to policymakers of how big an example the success of the “early adopters” of savings vehicles is likely to set for other farmers. The authors estimated two “likelihood of take-up” probits as functions of baseline characteristics (one for opening accounts and one for opening and using it), reported in Table C7 in their online appendix C, and we propose reestimating these probits and using them for a counterfactual analysis; in the second estimation stage we will estimate effects for adopters and non-adopters of the savings vehicles separately and then use the results from this stage to estimate counterfactual scenarios where the adopters did not adopt the savings vehicle and where the non-adopters did.

#### **Concluding remarks**

The research reported on in Brune et al. (2016) is an important contribution to the academic literature on the impact of microfinance. We would like to stress that although we hope that our proposed replication and our proposed extensions can also contribute to the literature, that we wish to carry out this replication and extension work is not due to any misgivings on our side about the research already presented in the original paper. It is, rather, due to the fact that we think the research is interesting and hope to contribute to making it even more relevant for policy use and for future research. We would also like to stress that at this point we have read the paper and the online appendices but have not yet examined the dataset, the code, or the original project plan that led to the field experiment that the original paper was based on. While we will of course look at all of these in detail before beginning our study, our proposal is thus not influenced by any prior knowledge (other than that provided by the published work) of what the data and code look like.

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