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Address:
The Rockwool Foundation Research Unit
Soelvgade 10, 2.tv.
DK-1307 Copenhagen K

Telephone	+45 33 34 48 00
Fax	+45 33 34 48 99
E-mail	forskningsenheden@rff.dk
Home page	www.rff.dk

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Impact of Village Savings and Loans Associations: Evidence from a Cluster Randomized Trial

CHRISTOPHER KSOLL, HELENE BIE LILLEØR, JONAS HELTH LØNBORG AND OLE DAHL RASMUSSEN

***Abstract**: Seventy percent of the world's poorest live in rural areas in developing countries with poor access to finance. Village Savings and Loan Associations (VSLAs) have become an increasingly widespread intervention aimed at improving local financial intermediation. Using a cluster randomized trial, we investigate the impact of VSLAs in forty-six villages in Malawi. We find positive and significant intent-to-treat effects on the number of meals consumed per day, total household consumption, and number of rooms in the dwelling over a two-year period. This effect is linked to an increase in savings and credit obtained through the VSLAs, which has increased agricultural investments.*

Christopher Ksoll, 120 University (8007), Ottawa, ON K1N6N5, Canada, cksoll@uottawa.ca; Helene Bie Lilleør, Rockwool Foundation Research Unit, hbl@rff.dk; Jonas Helth Lønborg, University of Southern Denmark; Ole Dahl Rasmussen, University of Southern Denmark and DanChurchAid, Campusvej 55, DK-5230 Odense M, Denmark, odr@sam.sdu.dk

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Introduction

The vast majority of the world's poor live in rural areas of developing countries with very limited infrastructure. Rural economies are characterized by long time spans between input and output of the agricultural production, uncertainty and weather dependency. This makes the ability to smooth consumption, to access credit and to employ risk coping strategies very important for household living under such circumstances (Conning and Udry 2007). Nevertheless, the history of rural financial intermediation is not encouraging and the recent explosive growth in microfinance globally has concentrated in urban and semi-urban areas (Demirgüç-Kunt and Klapper 2012, Allen and Panetta 2010, Daley-Harris 2009).

When formal financial institutions are not available, households use informal mechanisms instead. The widespread use of ROSCAs, ASCAs, susu-collectors, and similar informal financial networks is a testament to this (Rutherford 2001, Collins et al. 2009). One intervention which has gained increased popularity in rural Africa is the so-called savings group. Savings groups provide an alternative to existing informal networks and provide more flexibility, transparency, and security. One highly standardized type of savings groups, developed and promoted by CARE, are Village Savings and Loan Associations (VSLAs). Similar to other microfinance institutions, VSLAs combine multiple characteristics of formal financial markets: savings accounts, access to loans, and insurance. However, to provide credit and insurance services, each VSLA relies on its members having accumulated enough capital through savings, a minimum level of which is compulsory at the weekly meetings. For VSLAs to attract members, they therefore need to offer higher interest rates or more secure repayments than alternative forms of savings. This may be possible if groups are better at monitoring loans than individuals, if they have stronger enforcement

mechanisms, or simply because groups pool the risk on individual loans (Diamond, 1984).[1] In turn, the larger pool of savings coupled with better loan monitoring (and presumably lower interest rates on loans relative to money-lenders) expands access to credit within each VSLA.

According to data collected by VSL Associates, a consultancy, VSLAs have now been introduced in 61 countries and have reached six million active participants worldwide.[2] However, despite this prevalence and increasing popularity among donors as a means of improving the financial infrastructure in remote rural areas, very little is known about their impact on household welfare. Unlike other types of microfinance, where several randomized impact studies exists and show limited effects (see Stewart et al. (2010) and Copestake et al. (2011) for reviews of the existing evidence), no rigorous impact evaluation that addresses program placement and selection bias of VSLAs exists in the literature.[3]

In this paper, we provide one of the first rigorous impact assessments of VSLAs on three overall household outcomes. We do this through a cluster randomized control trials in forty-six villages in northern Malawi. Among these forty-six villages, twenty-three villages were randomly selected to participate in a VSLA project implemented by a local NGO, Soldev, from 2009 onwards. The remaining twenty-three villages were used as a control group, before the project was finally extended to these in 2011. We conducted household surveys of 1,775 households in 2009, just before the project was implemented in the treatment villages, and again in 2011, just before the intervention was introduced in the control villages.

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[1] Reducing the risk of the "low-risk" option can also enable households to adjust their portfolio of activities and undertake more risky investments elsewhere, for example in businesses or agriculture.

[2] Based on information on the web-site www.vsla.net, accessed on July 9th 2013.

[3] We know of a number of consultancy reports evaluating VSLA interventions. However, only two very recent evaluations use randomized control trials implemented over the same time period as ours; one from Burundi with roughly the same sample size as ours and a larger cross-country study covering Ghana, Malawi, Mali and Uganda. At the time of writing, none of these are yet available for citation.

We carefully tracked households that had moved in between the time of 2009 and 2011 surveys, resulting in a low attrition rate of only three percent.

We assess the impact of introducing VSLAs at village level on three groups of primary outcomes: food security, income-generating activities, and household income. These outcomes and a set of two to three indicators for each were defined by the implementing partner as part of the project design. Based on these pre-defined indicators, we have chosen a total of ten final outcome variables.

We find significant positive impacts on at least one final outcome variable for each of the three primary outcomes based on intention-to-treat estimates, while others are insignificant. Food security has significantly improved in that the number of meals per day has increased by 0.13. Income generating activities have improved in the sense that households hold significantly larger savings in VSLAs, although there are weak indications that the total number of income generating activities has decreased. Finally, there are indications of household income having gone up as the estimated total consumption using USAIDs Poverty Assessment Tool (PAT) methodology[4] increases by 4.8 percent, and the number of rooms in the dwelling increase by 0.16. Although only four out of ten indicators show significant results, we do believe this to be evidence of positive effects for two reasons. First, the estimated impacts have only had a two-year time horizon to materialize. Second, our impact estimates are given by the intention-to-treat estimator, i.e. they are average impacts across both participating and non-participating households at village level.

In the literature there are several suggestions of possible channels through which improved financial intermediation and active participation in savings groups can have a positive impact on the participating households and local

[4] PAT is an indicator-based estimate of household expenditure levels using twenty simple indicators, often indicating different types of asset ownership. See http://www.povertytools.org/ for further information about the Poverty Assessment Tools.

communities. Apart from the improved ability to smooth consumption over the agricultural cycle through savings or access to credit, the latter can also be used to facilitate investments. In addition, VSLAs offer a simple explicit insurance product. The groups themselves decide on the exact nature of the insurance product, but it almost always involves insurance against illness and death of household members. That is, the insurance is an additional risk coping device, which can encourage households to discard inefficient ex-ante coping strategies, such as low risk-low return activities. If households choose activities with higher risk and higher expected returns, this can lead to increased consumption in the long run even though the insurance itself does not pay out (Dercon, 1996; Carter and Zimmerman, 2003).

Furthermore, since VSLAs entail a minimum level of compulsory savings, participants with time-inconsistent preferences, who realize their self-control problems, may use VSLAs as a commitment device (O'Donoghue and Rabin, 1999; Ashraf, Karlan and Yin, 2006). Even if participants do not have time-inconsistent preferences, VSLAs might allow the participant to commit the rest of the household to his or her own preferences (Anderson and Baland, 2002).[5] VSLAs disburse savings once a year, and typically time these so-called share-outs to coincide with periods when most households need lump sums for particular investments, facilitating such commitment[6].

[5] Indeed, Jackson and Yariv (2012) show that, even when an *individual's* preferences are not time-inconsistent, all *households* will exhibit time-inconsistent preferences, *unless* discount rates are equal across household members or decisions are dictatorial (i.e. one household member's preferences decide). As cultural norms would suggest this would be the husband's preferences, which might be another reason for primarily women joining VSLAs.

[6] If for example share-out is at the beginning of the agricultural season, investment in agriculture might increase. Duflo et al. (2011) find that providing farmers with an opportunity to pay for fertilizer just after the harvest, where money is available, and having it delivered at the time of planting increases fertilizer use dramatically. In addition, the timing of share-out might serve as a so-called label, fixating the minds of the participants on using the money from the share-out on a particular asset – in this case fertilizer or other lump-sum investments in agriculture (Thaler,

Finally, the simple organization into groups may improve the level of social capital among group members by enhancing their mutual trust, information flows and joint decision making, all of which is thought to be conducive for economic activity (Coleman 1988, 1990).

Although we do not aim to investigate all of these possible channels through which the observed impact may have come about, we do investigate intermediate outcomes to shed light on some potential mechanisms. Beyond the increase in savings in VSLA, we find significant increases in the use of credit, including credit used for investment purposes, according to participants' own statements. Likewise, respondents report that they use their savings for investments, primarily in agriculture. We follow this flow of money into agriculture and small scale businesses, finding a statistically significant increase in the use of fertilizer and irrigation, followed by an increase in the value of maize sold. Several other agricultural outcome variables showed no effect, and there was no significant increase the total income from enterprises, although there are weak indications of an increase in the number of businesses in treatment villages despite the number of income generating activities seem to decrease.

The remainder of the paper is structured as follows. In the following section, we describe the intervention and the research design. Section three explains the data collection and outlines the empirical strategies used to estimate the effects of the intervention. Section four presents the results on take-up and the three main predefined outcomes, including robustness tests. Section five sheds lights on some intermediate outcomes through which the intervention can have influenced the welfare of the households. Section six makes some back-of-the-envelope calculations of the cost-effectiveness of the intervention and concludes.

1990). These increased investments in for example agriculture can increase productivity and in turn lead to higher household income and increase in household welfare.

The Intervention and Research Design

VSLA Intervention

The purpose of the intervention is to encourage the formation of groups with fifteen to twenty-five members who are then trained to manage their own village savings and loan association. As no external capital is provided, the groups are essentially self-managed financial intermediaries.

Within the larger microfinance sector, community managed microfinance belongs to the category of member-based, community-managed, accumulating microfinance institutions (see table 1). Typically, microfinance impact evaluations have focused on the professionally managed microfinance institutions. Unlike these, the VSLAs do not rely on the injection of external funds, just as they do not rely on the sustainability of a professionally managed institution, but rather on the sustainability of the group formed within the local community.

The inspiration for VSLAs came from rotating savings and credit associations (so-called ROSCAs, see Bouman 1995), and was developed by CARE international and VSL Associates during the 1990s (Ashe 2002). The aim has been to improve on ROSCAs in two respects: To make the groups more sustainable and to make them more flexible. Increased sustainability comes from a series of accountability features that prevent theft of funds and elite capture. Flexibility is increased because members can at any time borrow the amount they want up to three times their own level of savings – provided that funds are available. Whereas ROSCAs multiply without external facilitation, VSLAs only do so to a small degree thus requiring the facilitation by, for instance, an NGO, perhaps due to reasonably complex accountability features. VSLAs are implemented in the following way (Allen and Staehle 2007): After conducting awareness meetings in every targeted village a local NGO facilitates the formation

and training of groups. Initially, groups are visited every week in the first three months to set up the procedures. Groups work as a member-owned financial intermediary with three products: Savings, credit and insurance. Savings are compulsory and are collected at the weekly meetings and conceptualized as buying shares. Every week, a member must buy at least one share and is permitted to buy up to five. The share value is set by the group and written in the group's constitution. It varies between 50 and 100 MK in our case.[7]

Loans are provided at every fourth meeting. If the funds requested by members exceed the amount of saved funds, the group decides who gets the loan by following a predetermined list of criteria written in the group's constitution. This typically assigns funds based on the stated use of the loan. The interest rate on loans is set by the group, and can thus be used to regulate excess supply or excess demand in the medium run, making VSLAs less flexible in that regard than bidding ROSCAS (Klonner 2008). Usually, the nominal interest rate on loans is set to between 5 and 20% per month, but extensions in repayment schedules and inflation make the real interest rate considerably lower (Rasmussen 2012). Loan contracts run for three months, with a grace period of one month. Rules for loan approval are set in the groups' constitution, but often focus on productive purposes.

The overall interest rate on savings is typically 4 to 5% per month, but materializes only after the end of a cycle, typically lasting around 12 months, when all savings and interest payments are divided by the number of shares and paid out – the so-called 'share-out' (Rasmussen 2012).[8] The actual date of the final share-out is set by the group and tends to be chosen according to when most households need cash, e.g. for purchasing seeds or fertilizer.

[7] Throughout the paper we report monetary values in Malawian Kwacha (MK). 1 USD corresponded to 91.91 MK using poverty-adjusted PPP exchange rates in 2009.

[8] It is lower than the interest on loans primarily due to the fact that not all the funds are lent out all the time and that savings accumulate over time.

Typically at the end of a cycle members decide whether to leave or remain in the VSLA group and whether the group should accept new members. Any impact found in our analysis below will thus be the impact of at most two full cycles of collecting savings, giving out loans and returning the savings with interest among the 100 groups established by mid-2011. Usually after one year of initial training and monitoring, groups "graduate, " which means they are no longer supervised by the NGO that helped set them up.

Apart from savings and loans, VSLAs also offer insurance that is financed by a small premium paid by each member each week, separately from the savings and credit activities. The insurance is paid out as a grant or an interest free loan when certain events occur that are outlined in the constitution, usually the death of family members, death of cattle, sudden illness or other emergencies.

A number of accountability features guard against theft and elite capture. With the help of the facilitator, each group develops a constitution that describes areas of potential conflict and their solution, for example lending rules, election procedures, exclusion of members as well as fines for delays and non-attendance. To achieve transparency in transactions, all transactions take place in the presence of all members at the weekly meetings and are counted independently and in public by two elected money counters. Between meetings, funds are stored in a cashbox, locked with three padlocks. Three different members hold a key to the box and the box is stored in the house of a fourth, so the ease of collusion is severely reduced.

Design of the Experiment

The crucial challenge for an impact evaluation is to construct a credible counterfactual that is not sensitive to selection bias typically due to non-random program placement and self-selection into program participation (Banerjee and Duflo 2009, Duflo, Glennerster, and Kremer 2007, Angrist and Pischke 2009).

We address the problem of non-random program placement by randomizing the roll-out of the VSLA intervention at the village level, i.e. a cluster randomized control trial. Out of 46 villages in the program area, we randomly chose 23 villages for implementation in the first year (the treatment villages) and 23 villages for implementation in the third year (control villages). As such, the study is a simple parallel trial, i.e. only one treatment group and one control group. To improve balance in observable and unobservable characteristics we grouped villages into strata based on some characteristics believed to be correlated with central outcomes of interest (Bruhn and McKenzie 2009).

The non-overlapping strata were defined as follows: large fishing villages, small fishing villages, particularly eager villages[9], large non-fishing villages, villages with a rice irrigation scheme, villages with another NGO-led intervention, and a final group of the remaining villages. The randomization was carried out under our supervision by field officers from the NGO drawing village names from seven hats each containing the villages of each strata. Figure 1 below shows the physical location of the village centers, with the shape of the dot indicating the block to which the village belongs, and the color surrounding each village centre indicating allocation to the treatment or control group.

As part of the VSLA intervention the awareness meetings were carried out by the implementing partner in all 46 villages prior to randomization. We used these awareness meetings to have field officers from the implementing partner collect lists with the names of the villagers who expressed an interest in joining the VSLA groups to allow for stratification on initial interest. Figure 2 provides an overview of the timing of the randomization, implementation and the data collection.

[9] These were identified by the field officers based on the reaction from villagers at the awareness meetings.

Choice of outcomes

Our choice of final outcome variables for the primary impact analysis was closely linked to the development outcomes originally stated by the implementing NGO in the project's logical framework matrix, in which the NGO describes the desired and anticipated outcomes and associated assumptions.[10] The matrix mentions indicators on which the intervention is expected to have an impact, but it does not specify exactly how each of these indicators is to be measured. We have sought to design final outcome variables, which come close to the original specification, see table 1b. Thus although the matrix has clearly directed our analysis in terms of choice of outcome variables, it does not serve the purpose of a pre-analysis plan used by for example Casey et al. (2011).

We also report extensively on non-predefined outcomes in order to investigate possible channels of the observed impact. By doing so, we explicitly distinguish between predefined, primary analysis and secondary analysis.

Data and Estimation Strategy

Data

. To enable oversampling of households who had expressed an interest in participating in the upcoming groups, we stratified the sample within each village by whether the household had declared an interest in participation, using the information gathered by the NGO during the awareness meetings. We sampled roughly 38 households in all villages. Due to differences in village size, this led to considerable variation in sampling probability between strata apart from the

[10] The logical framework matrix is a part of the logical framework analysis, by far the most common project management tool in international development (Dale 2003). The matrix is made by the implementing partner prior to initiating a project and specifies the expected impacts of the intervention. Since the matrix is made by practitioners rather than researchers it is not very detailed when it comes to measurement and analysis.

oversampling of interested households. Therefore, we use sampling weights in all the analysis shown below.[11]

Data was collected in 2009 and 2011 by IKI (the Invest in Knowledge Initiative) under the supervision of the authors. In 2009, data collection took place from July 26 to August 30. The endline data was collected between July 8 and August 14, 2011. In both the 2009 and 2011 data collection, 24 interviewers completed the interviews with approximately 1784 households. A sub-sample of 834 households were also interviewed with a more in-depth questionnaire. Figure 1 shows the geographical location of the surveyed households. As can be seen, the treatment villages (marked by green dots) and control villages (red dots) are contiguous, making it possible for some households from control villages to participate in VSLA groups in the treatment villages, which we address below.

To limit attrition, we implemented a tracking survey in 2011, leading to a total number of 1,775 households being surveyed in 2011. The resulting attrition between the 2009 and 2011 survey rounds was only 49 households or less than three percent of the initial sample, which compares favorably with other panel surveys (Glewwe and Jacoby 2000).

Since we also tracked households where one of the designated respondents had moved out due to e.g. divorce, we have a number of split households – i.e. where one household from the 2009 survey had become two households in 2011. In the

[11] Despite the agreement in the survey literature and part of the econometrics literature, the issue of weights warrants further discussions. Weights affect results greatly and the econometric literature outside survey research does not agree on the issue, at least at a first glance (Angrist and von Pischke, particularly pp. 91-94). If the population regression function is interpreted as causal in all its parameters, as would be the case in structural equation modelling, then weighting is in some cases not justified regardless of the underlying method of sampling (Cameron and Trivedi 2005). If, however, there is only one parameter of interest, for example because the causal interpretation stem from exogenous variation rather than from a correct structural specification, weights should be applied. This is the case within a counterfactual framework, or more specifically the Rubin causal model (Rubin, 1974, Wooldridge, 2001, Angrist and von Pischke, 2009). We show the unweighted regressions in the robustness section below.

estimations below we drop one of these new households at random to ensure a balanced panel. We assess the implications of this for the results in the robustness section by limiting the sample to intact households.

Baseline Balance

We test whether the randomization of villages into treatment and control groups did in fact lead to two groups with the same observable characteristics.

Table 2 below provides descriptive statistics for our final outcome variables and some characteristics of the households for the entire sample of households in 2009. 84% of households were headed by men. Household heads on average had just under seven years of education. Households had six household members, who at the time of the interview consumed approximately 1.17 2005 USD per person per day using the USAID PAT measurement, and had more than four months of the year where they consumed fewer than three meals a day.[12]

Columns four and five report the mean value of the variables for the households in the treatment villages and control villages respectively. Column six reports the t-values from a balance test of whether there are any significant differences between the two means done as an OLS regression, which allows us to take the blocks of the randomization into account while using sampling weights. On our predefined outcomes, only one variable differs between the treatment and control villages: land ownership.

While food consumption appears unbalanced, the log of food consumption is balanced, suggesting that the difference in means here is due to imbalance in outlier observations.[13] Overall, the observable household characteristics and, not

[12] Throughout the analysis, we used the 2005 poverty-adjusted purchase power parity exchange rates suggested by Deaton and Dupriez (2011).
[13] Consumption follows a log-normal rather than a normal distribution in our sample.

least, the predefined outcome variables appear balanced. However, below we also implement estimators that take account of any potential pre-program differences.

Empirical Strategy

The randomization allows for simple estimation strategies in order to estimate the treatment effects of introducing VSLAs to the villages. Several different estimators can be employed. We estimate the intention-to-treat effect using four different estimators as well as the local average treatment effect.

Following Duflo et al. (2007), let $Z = \{0,1\}$ indicate whether village j was assigned to treatment and let $Y_{ij}^{Z=z}$ be the outcome of household i in village j, given its treatment status z. The randomization ensures that the expected outcomes are the same in treatment villages and control villages in absence of treatment, $E\left[Y_{ij}^{Z=1}|Z=0\right] = E\left[Y_{ij}^{Z=0}|Z=0\right]$. Specifically, this ensures that there is no selection bias due to the placement of the program.

We estimate the intention-to-treat (ITT) effect i.e. the average effect of introducing the VSLA intervention on all households in the treatment villages compared to the control villages, irrespective of whether they actually participated.[14] We can estimate the intention-to-treat effect as $\delta = \hat{E}[Y_{ij}^{Z=1}] - \hat{E}[Y_{ij}^{Z=0}]$ where $\hat{E}[\cdot]$ is the sample analogue of $E[\cdot]$ by comparing the differences in post-treatment means. Specifically, we fit the following model:

$$y_{ij} = \alpha + \delta^{DM} Z_j + \theta Block_j + \varepsilon_{ij} \tag{1}$$

where y_{ij} is the outcome of household i in village j and Z_j is a dummy indicating whether village j was assigned to treatment and δ^{DM} is the difference-in-means ITT estimate.

[14] Note that because the compliance in control villages was imperfect, this will underestimate the impact of VSLAs.

We include the randomization strata, denoted blocks, in all our linear regressions as recommended by Duflo et al. (2007). We also weigh all regressions using the inverse sampling probability as the weight for each household and cluster standard errors at village level.

We extend the simple difference-in-means comparison to a comparison between the average change in the outcome variable in treatment and control villages over time using the pooled difference-in-differences estimator (Angrist and von Pischke, 2009):

$$y_{ijt} = \alpha + \beta Z_j + \gamma I_t^{2011} + \delta^{DiD}(Z \cdot I^{2011})_{jt} + \theta Block_j + \varepsilon_{ijt} \qquad (2)$$

where y_{ijt} is the outcome of household i in village j at time t, Z_j is a dummy indicating whether village j was assigned to treatment which is constant over time, I_t^{2011} indicate whether the observation is from the 2011 post-treatment survey and $(Z \cdot I^{2011})_{jt}$ is the interaction of the two — i.e. whether the observations is from a treatment village at the 2011 survey. δ^{DiD} is our parameter of interest — the ITT using pooled difference-in-differences. However, since we observe the same households both pre- and post-treatment, we can control for household level unobservables as opposed to only village level as in the pooled difference-in-difference. Hereby we can improve the efficiency of the estimator δ when using a first difference specification rather than the pooled difference-in-difference

$$\Delta y_{ij} = \alpha + \delta^{FD}\Delta(Z \cdot I^{2011})_j + \Delta\varepsilon_{ijt} \qquad (3)$$

Finally, even though the first-difference approach takes out any time-*in*variant household specific component it does not overcome two potential pitfalls of the difference-in-difference set-up; time-variant imbalances caused by trend differences in outcome variables between treatment and control households, or the possibility of heterogeneous effects across different types of households when the

change in the outcome variables is correlated with one or more covariates. Lin (forthcoming) shows that including baseline covariates interacted with the treatment dummy increases precision as long as the trial design is not very imbalanced.[15] As a robustness check, we therefore test the sensitivity of our findings by fitting difference-in-differences models where we include a full set of covariate-treatment interactions, demeaning the covariates with the full sample mean, $\bar{X}$. The intention-to-treat effect is denoted δ^{Adj} (Adj for adjusted regression in the trial literature) in the following regression model:

$$y_{ijt} = \alpha + \beta Z_j + \gamma I_t^{2011} + \delta^{Adj}(Z \times I^{2011})_{jt} + \pi X_{ij}$$
$$+\tau Z_j \times (X_{ij} - \bar{X}) + \theta Block_j + \varepsilon_{ijt}$$

In addition to the ITT estimates, we also estimate the average treatment effect for those households whose treatment status was affected by the randomization using the randomization as an instrument, the so-called local average treatment effect (LATE) (Angrist and Imbens 1996). We implement a two-stage least squares instrumental variables approach on first-differenced outcomes for estimating the LATE. Using the notation from above, and letting T_{ijt} indicate whether a household actually participated in a VSLA, we first estimate the probability of a household participating in a VSLA:

$$\Delta T_{ij} = \alpha + \beta \Delta(Z \cdot I^{2011})_j + \Delta e_{ij} \quad (5)$$

Here, the result of the randomization for village j, Z_j, should make it more likely that a household from a treatment village, $Z_j = 1$, participates in the VSLA.

[15] While Freedman showed that simple regression adjustment (including covariates in the regression) can actually hurt precision instead of improving it (Freedman 2008b, a), Lin finds that if the trial design is very imbalanced or there are heterogeneous treatment effects that are strongly correlated with covariates, then interacted adjustments (where there is a full set of treatment and covariate interactions) will tend to improve precision as long as the number of covariates is much smaller than the number of observations in the smallest trial group. Moreover, Lin shows that using the interacted adjustments cannot hurt precision.

Using the predicted value, $\hat{T}_{ijt}$, from this first-stage, we subsequently estimate the LATE, using first differenced outcomes as the dependent variable:

$$\Delta y_{ij} = \alpha + \delta^{IV} \widehat{\Delta T_{ij}} + \Delta \varepsilon_{ij} \tag{6}$$

Results on main outcomes

Take-up

Before assessing the impact of the intervention, it is important to know how successful it has been in terms of attracting participants to form VSLAs. Figure 4 gives a quick overview. Total membership increased during the 2009-2011 period and the levels were substantially higher at the 2011 survey in the treatment villages than in control villages.

Table 3 shows that, at the time of the baseline survey in 2009, 5-6% of the population in the control group as well as in the treatment group reported that they were members of a VSLA or a similar savings group. Two years later, the figure was 15% for the control group and 39% for the treatment group. This 23.3 percentage point difference between the take-up of the VSLA intervention between the treatment and control villages is significant at the one percent level, suggesting that the randomization was effective in inducing more treatment villagers to participate in the VSLAs. However, the compliance with treatment is not perfect and the randomization suffer from is two-sided non-compliance (Gerber and Green 2012). Not all households living in villages assigned to treatment participated, just as some households from the control villages found their way into a VSLA group. But as Figure 4 shows, the timing between treatment and control villages differs. Until mid-2009 there was a general, but very small, increase of membership in the area regardless of the random

assignment into treatment and control. In 2009, when the intervention commenced, membership takes off in treatment villages. In control villages, however, membership seems to follow the general pre-project trend until late 2010. This indicates that control group contamination happened relatively late and thus that the effect of the project on the control group is likely to be small, although it may still result in an underestimate of the true ITT effects estimated below.

Impact on Predefined Outcomes

The first three columns of table 4 below present the estimated intention-to-treat effects of the VSLA intervention using difference-in-means, pooled difference-in-differences, and first-differenced regressions methods on the predefined outcomes. The predefined outcomes are arranged in the three groups as suggested by the LFA: food security, income generating activities and household income.

Although a simple glance at the table reveals that there has not been a significant impact of introducing VSLAs in the treatment villages on all of our outcome variables over the two year intervention period, a consistent pattern do emerge once we use the panel structure of the data. Food security seems to have improved, particularly in terms of the number of meals consumed the day before the interview. Although the number of income generating activities may have decreased, the volume of savings has increased, particularly the VSLA savings which seems natural. Finally, there are also indications of improved household income in terms of total consumption estimated using PAT as well as the size of

the house, where the results regard the quality of the floor and land ownership are less consistent across specifications.[16]

Focusing on the difference-in-means estimates in column (1), we see that savings in the VSLA increases dramatically, some of which seems to spill over into higher total savings, although this result, albeit still positive, becomes insignificant once we move to the panel-based estimators. Similarly, the difference-in-means estimate of the impact on the number of income-generating activities is negative, suggesting that households in treatment villages have less of these. A priori it is unclear which impact to expect here. The implementing NGO had an aim of increasing the number of income generating activities of the involved households, but as mentioned in the introduction the fact that a household gains access to savings credit and insurance of larger unforeseen events may lead to more efficient economic choices, which could be to specialize in a few rather than to have a diverse set of income activities. A decrease in the number of income generating activities is consistent with such an explanation, although we cannot identify this to be the exact channel. The estimated ITT impact remains negative in the panel-based regressions, but turns insignificant.

In columns (2) and (3) we then control for baseline differences between treatment and control groups by estimating difference-in-difference regressions taking village level time invariant characteristics into account as well as household level time-invariant characteristics using first-differencing. The results across these two specifications are very stable: First of all, VSLA savings rise significantly. Moreover, the number of meals consumed yesterday has significantly increased: the households from the treatment villages consume on

[16] Note there are indications that the imbalance in land ownership in baseline may explain the difference between the difference-in-means estimate of the ITT effect and the other panel-based estimates, which take baseline imbalances into account.

average 0.147 more meals a day or one in seven households consumes an extra meal. Given that the baseline average is 2.65 meals per day, this is sizable.

There are also positive effects on consumption when measured by the USAID PAT as well as the number of rooms (both marginally significant at the 10% level in the difference in difference specification; and at the 5% level for first differences). The logged value of consumption increases by 0.041 – i.e. a 3% increase.[17] The number of rooms increases by 0.15. The latter might be surprising, but given the rural setting, where households usually live in huts built of mud on a wooden frame, it is quite common to build additional rooms onto the existing structure when money allows, or—if possible—to replace the mud hut with burnt or un-burnt brick structures. We find no significant effects, however, on the type of floor in the dwelling. We also find no significant effects on total food consumption, length of the hungry period, number of income-generating activities, and total savings.

Local Average Treatment Effects

The fourth column in table 4 displays the results of the two-stage least squares estimation of the LATE. Estimating the LATE has two hardly surprising consequences in general when compared to the ITT estimates: The estimated coefficient increases since LATE only averages the total impact found at village level over the households whose participation in VSLA was affected by the randomization, i.e. the 23.3 percentage point difference shown in table 3. The IV estimate is thus roughly 4 times larger than the intent-to-treat effect. At the same time the standard errors also increase, since we introduce more noise by using the estimated probability of being a VSLA participant rather than the actual

[17] Following Kennedy (1981), we interpret the implied effects of the coefficients using the formula $e^{\left(\hat{\beta} - \frac{var(\hat{\beta})}{2}\right)} - 1$.

assignment to treatment. The LATE estimates are significant for the same outcomes as the first-difference ITT and the estimated LATE effects are quite substantial: participants ate almost half a meal more than non-participants the day before the survey, and their consumption increased by thirteen percent using PAT. Furthermore, their VSLA-savings savings increased, and they expanded their dwelling with half a room more as a consequence of the intervention.

Robustness

We take a number of steps to assess the robustness of the results found above, focusing on the estimated intention-to-treat effects. First we use adjusted regressions. Second, we assess how sensitive the results are to the choice made regarding the use of weights and the split households.

Column five in table 4 reports the results from using the adjusted regressions. The results using adjusted regressions are very similar in size and significance to the results from our preferred specifications in column (2) and (3).

The un-weighted results are displayed in table 5. The sampling strategy of oversampling households that had expressed an interest in the VSLA intervention carries over to the weights. Although results are qualitatively similar, there are enough differences to say that our survey population differs from the entire population in the area as the estimated coefficients change somewhat and significance levels are affected.

During the 2011 survey, we took care to track any households that had moved or split up during the survey period. Thus, for a number of households, defined from the 2009 survey, we had two observations in the 2011 survey. In the predefined analysis above, we simply randomly drop one of each of these split households from the 2011 sample. In the third column of table 5 we estimate the ITT using first-differences estimator, while dropping all split households, i.e.

restricting the sample to intact households. The point estimates do not change much and the significance level of all estimates is identical to the preferred specifications where we include one randomly selected split household.

Results on Intermediate Outcomes

Having found positive and significant impacts on at least one indicator of each of the pre-defined outcomes, the next obvious step is to take a close look at some of the possible mechanisms at play.

We start out by investigating whether the intervention actually improved access to and take-up of the predefined financial services offered: savings and credit. We follow the self-reported use of money from share-outs and loans taken from the VSLAs. Households report using share-outs and loans for increased investments in agriculture and other household-owned small scale businesses and we therefore investigate the households' agricultural production as well as the evidence on other income-generating activities below.

Savings Volume and Share-Out

The core component of the VSLA intervention is savings, which are a prerequisite for subsequent credit opportunities and insurance grants. Above we showed the estimated ITT-effects on the total savings volume and savings in the VSLA intervention. In table 6, we extend this analysis by looking at the effects on other sources of highly-liquid savings. Apart from the results on savings in VSLAs, we do not observe any significant changes in the other savings options.

A natural question is where the funds saved in VSLAs come from, since the other savings options do not decline. Another interesting issue is why total savings do not increase. Several explanations are possible. The non-VSLA savings actually do decline, although they are not statistically significant, while

total savings increase a little. So VSLA savings might come in part from the other savings options, in part from extra income, or reduced consumption, although the latter is unlikely given the results above. The only certain conclusion seems to be that households now use the VSLA option.

When further exploring the use of VSLA, it is important to note that saving in the VSLA groups is not identical to saving in a regular liquid savings account: a core feature of the intervention is the annual share-out of all savings along with any interests earned from loans made during the cycle, i.e. savings in VSLAs are tied to the VSLA cash box until the date of the share-out and therefore also become a commitment device. In order to understand the mechanisms behind the observed impacts of the intervention, it is thus also important to understand how the share-out savings are spent. Table 7 below shows the self-reported primary use of money from share-outs. The first column shows the estimated number of households reporting different uses, and the second column the percentage of households that have shared out. While we estimate that thirty-two percent of households in the treatment villages have joined VSLAs (table 3), only sixteen percent of households have actually shared-out at the time of the survey in 2011 (table 7).[18] Among households that have received share-outs, forty-four percent report using the money primarily for agricultural inputs or investments. This corresponds well to the timing of the share-out observed, which typically has been during the planting time and thus when there is a need for seeds and fertilizer, see figure 5.

[18] From the information recorded by the implementing NGO, only 3 of the 102 groups that had been initiated by September 2011 had shared-out twice, while another 40 groups had shared-out once.

Credit Volume

The other key component of the intervention is the use of pooled savings as credit for the VSLA members. Table 8 below shows the estimated ITT-effects on a range of credit-related outcomes using our pooled difference-in-differences as well as the first-difference strategy. Living in a treatment village increased the number as well as the value of loans active within the past twelve months (total loan amount). The intervention also increased the take-up of loans for investment purposes: The probability of having taken a loan for investment purposes and the number of loans taken for investment purposes increase, just as the value of loans taken for investment in agriculture increases significantly. The increases are sizeable and generally significant. Total loan amounts increase by 216%, and the amount borrowed for agriculture increase by a 48%, both using the first-differencing, whereas there is no significant increase in the amount borrowed for business purposes.[19]

The background for these large changes is the very modest starting point, however. Loan values were almost zero at the time of baseline and so was the share of households with loans. If we count all loans, the share of households with loans tripled from 7% to 21%. It is therefore fair to say that the intervention successfully increased access to credit.

Although the total loan value for agricultural investments has increased considerably more than that for business purposes, we see a more balanced pattern when, in Table 9, turning to the self-reported use of any loans obtained from the VSLA in the 12 months preceding the survey in 2011. Here, 24 percent report agricultural input or investments as the primary use of the loan, whereas 41 percent report trading and business as the primary loan use. We therefore

[19] As above, we use Kennedy (1981) to get from the log point estimates to percentage change: $e^{\left(\hat{\beta} - \frac{var(\hat{\beta})}{2}\right)} - 1$.

investigate these two potential channels, i.e. whether VSLAs seem to have influenced agricultural inputs or outputs and whether VSLAs have influenced other types of income generating activities.

Agriculture

Table 10 and table 11 below show the ITT effects on a range of agricultural inputs and outputs, respectively. In each table, the first two columns show the weighted baseline means and the relevant sample size[20], whereas columns 3, 4 and 5 provide results for the difference in means, the pooled difference-in-difference and the first differences regressions, respectively.

On the input side, we find evidence that households from treatment villages are more likely to use fertilizer when growing maize and the first-difference estimator show a positive result with regard to irrigation, something particularly necessary together with fertilizer.

On the output side, we find evidence that there is a positive effect on the likelihood that households in treatment villages sell part of their maize production and that the total value of maize sales has increased.[21]

This evidence, although suggestive, points to a change in agricultural practices and possibly an increased specialization. Treatment households seem to invest more in growing their primary crop, maize, and by doing this they also get a higher output.

[20] Some questions were only asked in the in-depth questionnaires to a sub-set of households.
[21] We have also looked at the productivity, defined as output (kg) per acre, and found similar although insignificant results.

Income-Generating Activities

Table 12 reports the estimated ITT effects on a number of outcome measures related to income-generating activities. As in the results tables above, the first two columns show estimated baseline means and the sample size for the pooled difference-in-differences regression, respectively.

Households from the treatment villages increase the number of businesses by the time of the endline survey compared to the households from the control villages (though they did not increase the number of different income-generating activities). We are, however, unable to determine whether this is due to more businesses starting up, or whether the existing businesses are more likely to survive in the treatment villages. While we do see indications that the number of businesses increased in the treatment area, the total income from businesses did not increase.

In conclusion, this suggests that whereas the program seems to have led to significant increases in investments in agriculture, the VSLAs were less successful in increasing the income from other income generating activities and businesses, although this was stated as part of the original project objectives.

Cost Effectiveness

In order to assess whether the VSLAs seem worthwhile, this section provides some simple back-of-the-envelope cost effectiveness calculations. The intervention runs for three years and by that time should reach all villages in the survey area, i.e., both the treatment villages and control villages. We estimate the ITT and LATE effects of the intervention on households from the treatment villages after two years. We estimate a cost per year of implementation as total costs (USD $201.000) divided by three. In this setting it is worth noting that the specific intervention investigated is quite costly compared to the average VSLA

intervention due to the relatively small scale of the intervention and the fact that VSLAs often use agents to a greater extent than the randomization allowed in the current setup.[22]

The implementing partner reported having a total number of 1,783 VSLA members by September 2011 out of the 3,800 households in the treatment villages. The cost per member was thus USD $75,[23] while the cost per household from a treatment village was USD $35.

As for the benefits, we face the problem of not knowing whether the estimated effects are permanent or whether they are only temporary. Based on the estimated effect on total household consumption using USAIDs PAT methodology, we found an increase of 3.3% for all households in the treatment villages as reported in table 4 corresponding to an increase in the average household income of USD $0.24 per household per day. If this effect is only for one day, the increase in household income comes at quite a high cost—the USD $35 estimated above. The project breaks even after 146 days of such a sustained impact on the PAT consumption measure only. Should these average effects last for a year, the estimated benefits amount to USD $88.99 per household in the treatment villages, well above the estimated implementation costs of USD $35. Of course, as we have seen, the introduction of VSLAs has affected other measures above and beyond their impact on household consumption as measured by PAT.

[22] Normally, VSLAs are introduced to a single village, where some VSLA members become village agents, who subsequently start VSLAs in the neighboring villages. While this limits the costs of the intervention, we do not know what the effects of relying on village agents rather than trained field officers would be.

[23] This is quite high compared to the typical cost of implementing VSLAs as reported by Allen and Panetta (2010), which was USD $18 to $48.

Conclusion

This paper provides evidence that local financial markets and financial intermediation can have a significant impact on household welfare and economic activities in remote rural areas of a developing country. We analyze an intervention that creates a financial institution, Village Savings and Loan Associations, in the hope of remedying what has been termed the "last mile problem of microfinance": that is providing good access to finance in rural areas where it is not profitable to operate a micro-finance institution and much less a bank. VSLAs on the other hand do not rely on any outside injection of funds, but only harness savings in the local community, which can be re-invested in the local community. This paper investigated the effects of introducing VSLAs in rural Malawi on household welfare and finds that this introduction raises welfare along a number of dimensions. As such it provides credible evidence that households in the poorest, most rural areas are constrained by lack of access to financial markets which provide both savings and credit opportunities. Through the introduction of VSLAs, we see both improved food security and strengthened household income indicators. Having analyzed the impact only two years after the onset of the project, we find these indications of positive effects of simply organizing savings groups which also create credit opportunities for their members rather encouraging.

Following how money from share-outs and credit are used, we see that participants report spending their savings on agriculture and their loans on agriculture and small-scale businesses. After investigating overall impacts, we therefore look closer these potential mechanisms. We find indications that both agricultural investments and output increase, specifically we find an increase in the use of fertilizer and irrigation and in the total value of maize sales. Although

one of our specifications showed that households in treatment villages started slightly more businesses than households in control villages, we find no effect on total income from these businesses. As such, agricultural investment carries more weight in explaining the results.

The above conclusions have limitations, however, which should be kept in mind. Out of the 80,000 VSLAs across the world, we have only studied a few. The results are specific to the cultural and economic context in which they appear, and a similar intervention in a change in location, another implementing organization, or just a different time period might give different results. Future assessments from other places, including the ones we know are under way, will shed light on the extent to which these findings can be generalized.

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Tables and Figures

Table 1a. Microfinance overview

Ownership	Management	Fund Accumulation	Examples
Member-based institutions	Professionally managed microfinance	Large accumulating savings and credit associations (ASCAs)	FECECAM, Benin Savings and Credit Cooperatives (SACCOS)
	Community-managed microfinance	Rotating savings and credit associations (ROSCAs)	Tontines, susu, upatu, merry-go-rounds, chit, pasanakus
		Small accumulating savings and credit associations (ASCAs)	Village savings and loan associations, Savings for Change, the WORTH model, savings and internal lending communities[24]
For-profit institutions	Professionally managed		FINADEV, Benin Equity Bank, Keyna SKS, India
Non-profit institutions	Professionally managed		Grameen Bank, Bangladesh BRAC, Bangladesh Opportunity International

[24] Some also include self-help groups, primarily found in India, as savings and loan associations, but they have very different procedures than the ones listed here.

Figure 1: Villages and Randomization Blocks

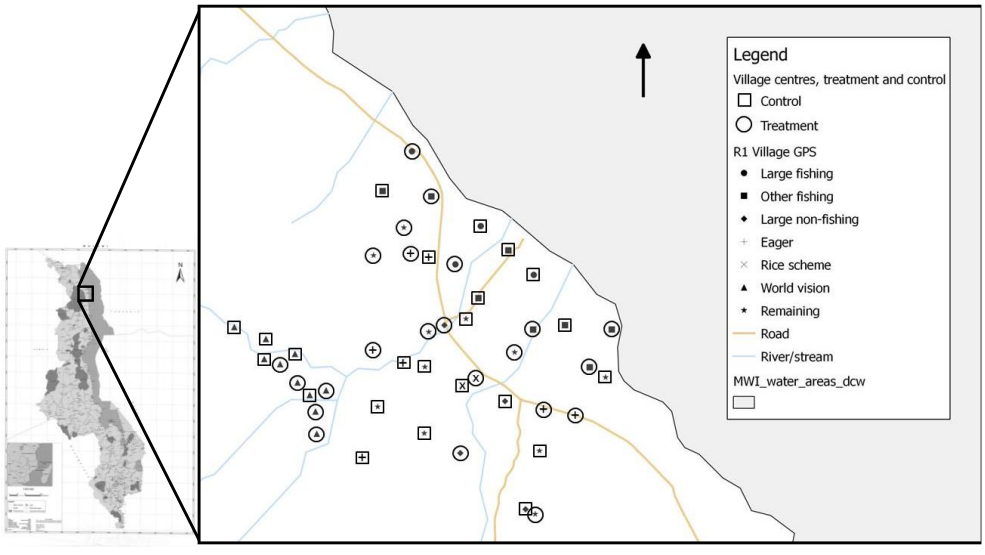

Figure 2. Timeline

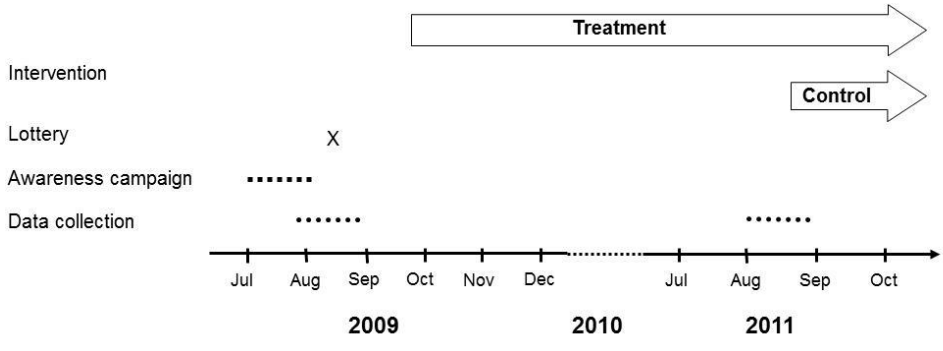

Table 1b: Predefined Outcome Measures

Logical framework development outcome (Soldev's choice)	**Logical framework indicator (Soldev's choice)**	**Corresponding final outcome variable (Authors' choice)**
Increased food security	Hunger period is reduced	1) Number of months in past year with less than three meals per day.
	Increase in the consumption of food	2) Number of meals yesterday. 3) Food consumption per week per adult equivalent (log)
Increase in income-generating activities	The average number of IGAs carried out by the VSLA participants has increased	4) Number of income generating activities in each household.
	Increase in the volume of savings by the VSLA groups from project related activities by 2012	5) Total savings (log) 6) Savings in VSLAs (log)
Improved household income	The share of the targeted population living below 1.25 USD/day has decreased as measured using USAID's PAT	7) USAID PAT's prediction of per capita consumption (log)
	HHs have improved their housing standards	8) Number of rooms in dwelling 9) House has cement floor
	Increase in household asset ownership	10) Land ownership (acres)

Figure 3. Location of Surveyed Households

Table 2. Baseline Characteristics and Balance

Variable	N	Mean	SD	Treatment Average	Control Average	Difference (t-value)
Project Outcomes						
Number of months with fewer than three meals a day	1732	4.10	4.03	4.26	3.92	1.20
Number of meals yesterday	1732	2.65	0.56	2.61	2.70	-1.49
Total food consumption per week per adult equivalent no infl (MK, log)	1732	6.10	0.58	6.06	6.14	-1.56
Number of income-generating activities (including agriculture and livestock)	1732	1.99	1.10	1.94	2.04	-1.66
Total savings (log)	825	5.70	3.80	5.98	5.40	0.73
VSLA savings (log)	825	0.17	1.16	0.21	0.13	1.13
USAID PAT per capita consumption (log)	1732	0.16	0.42	0.15	0.16	-0.40
Size of house (number of rooms)	1732	2.75	1.25	2.73	2.77	-0.34
House has cement floor	1732	0.10	0.30	0.11	0.09	1.48
Land ownership (acres)	1732	2.70	2.35	2.49	2.92	-3.01***
Other Household Characteristics						
Food consumption per adult equivalent per day (MK)	1732	75.64	48.62	71.54	80.06	-2.24**
Age of household head	1721	38.98	15.33	39.06	38.89	0.24
Number of household members at time of interview	1722	5.77	2.46	5.72	5.83	-0.61
Household is female-headed	1732	0.16	0.36	0.17	0.14	0.89
Years of education of household head	1729	6.86	3.25	7.06	6.65	1.56
Household owns land	1732	0.96	0.19	0.95	0.97	-1.48
Household is member of VSLA	1729	0.06	0.23	0.06	0.05	0.58

Note: Standard errors are clustered at the village level and based on weighted regressions. Displayed results are on the sample of 1,732 households for which we have information for all prespecified outcomes; i.e. the sample used for the pooled difference-in-differences estimations below, unless values are missing on the specific variable. * p<0.10, ** p<0.05, *** p<0.01.

Table 3: VSLA membership

	Control Villages	Treatment Villages	Differences
Baseline (2009)	5.5%	6.2%	0.7%
Endline (2011)	15.1%	39.1%	24%***
Differences	9.6%***	32.9%***	23.3%***

Note: Standard errors used in calculating significance are clustered at the village level. *** indicates significance at the 1% level.

Figure 4: Cumulative VSLA-membership by village type

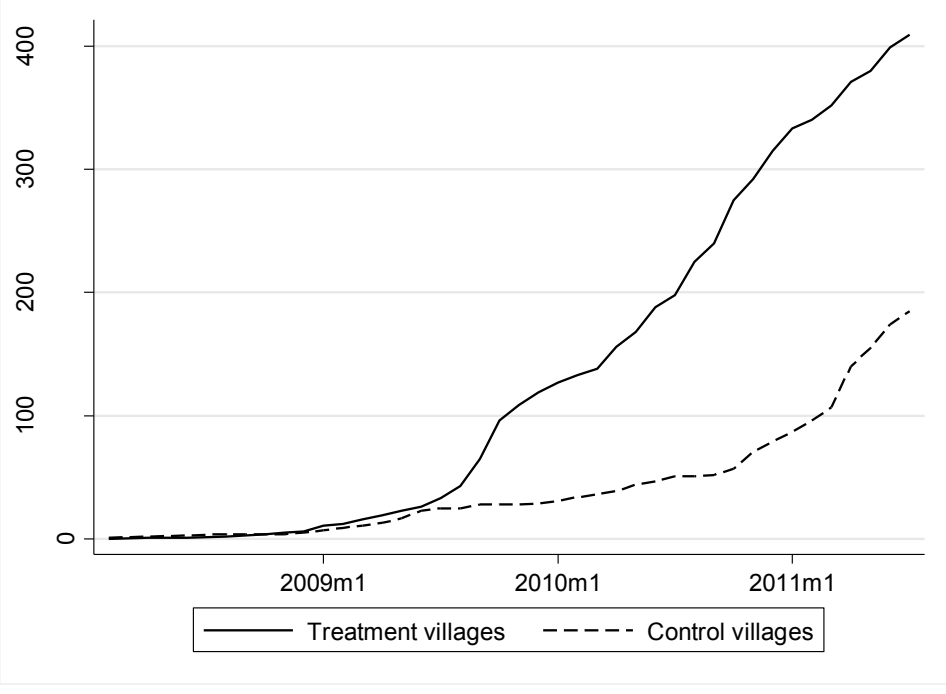

Table 4: Effect on Predefined Outcomes

Outcome	N	(1) Difference in Means	(2) Difference-in-Difference	(3) First-Difference	(4) IV	(5) Adjusted Regressions
Increased Food Security						
Number of months with fewer than three meals a day	3422	-0.205	-0.501	-0.571	-2.168	-0.374
		(0.38)	(0.45)	(0.38)	(1.40)	(0.31)
Number of meals yesterday	3422	0.055	0.147**	0.128**	0.487**	0.071**
		(0.04)	(0.07)	(0.05)	(0.20)	(0.03)
Total food consumption per week per adult equivalent (MK, log)	3422	0.023	0.079	0.093	0.351	0.101*
		(0.05)	(0.07)	(0.06)	(0.23)	(0.06)
Increase in Income Generating Activities						
Number of income-generating activities (including agriculture and livestock)	3422	-0.175***	-0.105	-0.056	-0.213	-0.053
		(0.06)	(0.11)	(0.07)	(0.26)	(0.07)
Total savings (log)	1625	1.384***	0.969	0.948	2.974	0.577
		(0.51)	(0.90)	(0.79)	(2.20)	(0.48)
VSLA savings (log)	1625	2.492***	2.413***	2.393***	7.505***	1.926***
		(0.58)	(0.66)	(0.57)	(0.63)	(0.36)
Improved Household Income						
USAID PAT per capita consumption (log)	3422	0.03	0.041*	0.042**	0.159**	0.029*
		(0.03)	(0.02)	(0.02)	(0.08)	(0.02)
Size of house (number of rooms)	3422	0.104	0.133*	0.154**	0.584**	0.134**
		(0.10)	(0.07)	(0.06)	(0.26)	(0.06)
House has cement floor	3422	0.019	0.001	-0.011	-0.041	-0.009
		(0.02)	(0.02)	(0.02)	(0.07)	(0.02)
Land ownership (acres)	3422	-0.272*	0.11	0.179	0.681	0.08
		(0.14)	(0.16)	(0.14)	(0.52)	(0.13)

The table shows effects on food security and household income. Column (1) ignores baseline information and therefore only uses half the sample, whereas column (2) controls for the baseline value of the outcome. Number of observations, N, refer to sample sizes in column (2)-(5). Column (3) improves precision by using a first-differencing transformation of data. All specifications include dummies for stratification blocks (not reported). Regressions are run on a sample where none of the outcome variable have missing values and number of observations are for the difference-in-differences estimation. Standard errors in parentheses, clustered at the village level. * p<0.10, ** p<0.05, *** p<0.01.

Table 5. Importance of split households and interviewers

Outcome	N	First-Difference	First-Differences Unweigthed	First-Differences Without Split Households
Increased Food Security				
Number of months with fewer than three meals a day	3422	-0.571	-0.288	-0.547
		(0.38)	(0.34)	(0.38)
Number of meals yesterday	3422	0.128**	0.036	0.131**
		(0.05)	(0.04)	(0.05)
Total food consumption per week per adult equivalent (MK, log)	3422	0.093	0.078	0.092
		(0.06)	(0.06)	(0.06)
Increase in Income Generating Activities				
Number of income-generating activities (including agriculture and livestock)	3422	-0.056	-0.059	-0.069
		(0.07)	(0.07)	(0.07)
Total savings (log)	1625	0.948	0.713*	1.01
		(0.79)	(0.39)	(0.80)
VSLA savings (log)	1625	2.393***	2.275***	2.431***
		(0.57)	(0.36)	(0.57)
Improved Household Income				
USAID PAT per capita consumption (log)	3422	0.042**	0.027	0.040*
		(0.02)	(0.02)	(0.02)
Size of house (number of rooms)	3422	0.154**	0.125*	0.159**
		(0.06)	(0.07)	(0.06)
House has cement floor	3422	-0.011	-0.008	-0.01
		(0.02)	(0.02)	(0.02)
Land ownership (acres)	3422	0.179	0.018	0.195
		(0.14)	(0.15)	(0.15)

Table 6. ITT-effects on savings outcomes

Outcome	Baseline Mean	N	Difference-in-Difference	First-Difference
Total savings (log)	5.662	1637	0.677	0.712
	[3.811]		(0.91)	(0.77)
VSLA savings (log)	0.17	1625	2.413***	2.393***
	[1.163]		(0.66)	(0.57)
Non-VSLA savings (log)	5.583	1645	-0.536	-0.367
	[3.832]		(1.00)	(0.70)
Savings with friend/relative (log)	0.524	1637	-0.037	-0.086
	[2.104]		(0.43)	(0.39)
Savings at home (log)	4.826	1637	-0.792	-0.677
	[3.816]		(0.89)	(0.62)
Savings with bank (log)	0.767	1636	0.361	0.544
	[2.642]		(0.53)	(0.45)

Note: The table shows that the intervention increased savings in VSLAs, but not enough to cause an increase in the total level of savings. Standard errors in parentheses, clustered at the village level. Standard deviations are in square brackets. * p<0.10, ** p<0.05, *** p<0.01. Dummies for stratification blocks included in the regression, but not reported.

Table 7. Self-reported primary use of money from share-out

Use	Number	Percentage
Agricultural inputs	186	30%
Agricultural investments	85	14%
Buy livestock	15	2%
Trading	89	15%
Education	15	3%
Health	14	2%
Ceremonies	0	0%
Food consumption	61	10%
Emergency	5	1%
Items for the household	79	13%
Other	63	10%
Total share-outs	613	100%
Percentage who have shared-out	16%	

Note: The table shows that the primary use of share-out was agricultural inputs and investments. Totals and percentages are estimated using sampling weights.

Figure 5. Timing of Share-Outs

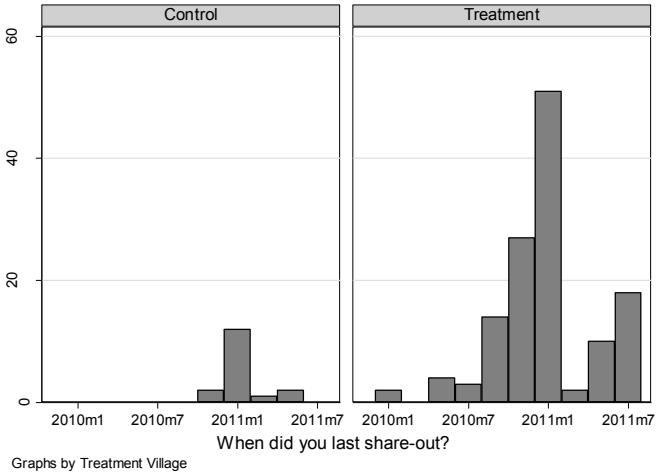

Table 8. ITT-effects on credit outcomes

Outcome	Baseline Mean	N	Difference-in-Difference	First-Difference
Household had any loan in past 12 months	0.07	832	0.134**	0.142***
	[0.26]		(0.06)	(0.05)
Household took out loan for investment purposes	0.044	832	0.075*	0.079*
in past 12 months	[0.21]		(0.04)	(0.04)
Number of loans active within past 12 months	0.073	832	0.148**	0.157***
	[0.28]		(0.06)	(0.05)
Total loan amount (log)	0.606	832	1.267**	1.338***
	[2.24]		(0.51)	(0.43)
Number of investment loans	0.046	832	0.084*	0.088**
	[0.22]		(0.04)	(0.04)
Total amount borrowed for agricultural	0.25	832	0.549*	0.546**
investments (log)	[1.52]		(0.29)	(0.22)
Total amount borrowed for business purposes	0.148	832	0.153	0.183
(log)	[1.12]		(0.26)	(0.24)

Note: The table shows that the intervention caused an increase in household borrowing across various types of loans and different specifications. Standard errors in parentheses, clustered at the village level. Standard deviation in square brackets. * p<0.10, ** p<0.05, *** p<0.01. Dummies for stratification blocks included in the regression, but not reported.

Table 9. Self-reported use of VSLA credit

	Treatment 2011		Control 2011	
	Number	**%**	**Number**	**%**
Agricultural inputs	76	10%	1	0%
Agricultural investment	104	14%	4	0%
Buy livestock	4	1%	0	0%
Trading/business	311	41%	118	13%
Education	15	2%	0	0%
Health	22	3%	5	1%
Ceremonies	0	0%	16	2%
Food consumption	50	7%	4	0%
Emergency	21	3%	5	1%
Household items	149	20%	3	0%
Other	9	1%	0	0%
No loan	0	0%	0	0%
Total	**761**	**100%**	**156**	**100%**
Observations used in estimating total number of loans	113		22	

The table shows estimated total number of loans using sampling weights

Table 10. ITT-Effects on Agricultural Input

Outcome	Baseline Mean	N	Difference in Means	Difference-in-Difference	First-Difference
Household uses any fertilizer	0.689	1616	0.052	0.068	0.066
	[0.463]		(0.05)	(0.08)	(0.07)
Household uses any fertilizer on maize	0.449	3429	0.132**	0.107*	0.093*
	[0.498]		(0.05)	(0.06)	(0.05)
Household cultivates vegetable plot	0.248	3446	-0.046	0.036	0.034
	[0.432]		(0.03)	(0.04)	(0.03)
Household has any irrigated plots	0.25	1637	-0.004	0.078	0.106**
	[0.434]		(0.05)	(0.06)	(0.05)
Household used any purchased seeds	0.775	1616	-0.006	0.015	0.025
	[0.418]		(0.05)	(0.05)	(0.06)
Total area cultivated (acres)	2.865	1616	-0.262	0.012	0.016
	[1.728]		(0.19)	(0.22)	(0.21)
Area with maize (acres)	1.369	3429	-0.310*	-0.109	-0.149
	[0.9]		(0.17)	(0.14)	(0.16)
Area with local maize (acres)	0.527	3429	-0.306**	-0.174	-0.219
	[0.783]		(0.15)	(0.12)	(0.13)
Area with composite maize (acres)	0.079	3429	-0.009	-0.02	-0.019
	[0.359]		(0.01)	(0.03)	(0.03)
Area with hybrid maize (acres)	0.762	3429	0.005	0.086	0.09
	[0.858]		(0.08)	(0.08)	(0.07)
Area with tobacco (acres)	0.183	1616	0.021	0.064	0.067
	[0.438]		(0.06)	(0.06)	(0.06)
Area with cotton (acres)	0.315	1616	-0.025	0	-0.02
	[0.642]		(0.06)	(0.07)	(0.07)
Area with rice (acres)	0.313	1616	-0.181	-0.035	-0.022
	[0.591]		(0.13)	(0.11)	(0.11)

Note: The table shows signs of a change in agricultural practices. In particular, households use more fertilizer. N is the number of observations for the regression on the pooled data. Standard errors in parentheses, clustered at the village level. Standard deviations are in square brackets. * p<0.10, ** p<0.05, *** p<0.01. Dummies for stratification blocks included in the regression, but not reported.

Table 11. ITT-Effects on Agricultural Outputs

Outcome	Baseline Mean	N	Difference in Means	Difference-in-Difference	First-Difference
Quantity of maize harvested (kg, log)	5.471	3429	-0.12	0.115	0.061
	[1.545]		(0.15)	(0.15)	(0.14)
Local maize harvested (kg, log)	2.328	3429	-0.517	-0.187	-0.28
	[2.741]		(0.31)	(0.23)	(0.20)
Composite maize harvested (kg, log)	0.339	3429	0.001	-0.031	-0.04
	[1.395]		(0.07)	(0.14)	(0.12)
Hybrid maize harvested (kg, log)	3.533	3429	0.275	0.284	0.321
	[2.849]		(0.24)	(0.29)	(0.23)
Quantity of maize harvested per acre (kg, log)	5.245	3429	-0.005	0.251	0.174
	[1.686]		(0.11)	(0.20)	(0.18)
Local maize harvested per acre (kg, log)	2.212	3429	-0.44	-0.122	-0.215
	[2.686]		(0.29)	(0.22)	(0.20)
Composite maize harvested per acre (kg, log)	0.323	3429	0.013	-0.014	-0.025
	[1.339]		(0.07)	(0.14)	(0.12)
Hybrid maize harvested per acre (kg, log)	3.381	3429	0.298	0.329	0.362
	[2.84]		(0.22)	(0.29)	(0.25)
Household sold any crops	0.588	1616	-0.047	-0.012	0.015
	[0.493]		(0.06)	(0.08)	(0.08)
Household sold any maize	0.201	1616	0.094*	0.142**	0.197***
	[0.401]		(0.05)	(0.07)	(0.06)
Value of agricultural sale (MK, log)	5.355	1616	-0.684	-0.569	-0.237
	[4.788]		(0.67)	(0.86)	(0.77)
Value of maize sale (MK, log)	1.505	1616	0.725*	1.001*	1.427***
	[3.141]		(0.41)	(0.57)	(0.48)

Note: The table shows signs of a change in agricultural practices. In particular, the total value of maize went up. N is the number of observations for the regression on the pooled data. Standard errors in parentheses, clustered at the village level. Standard deviations are in square brackets. * p<0.10, ** p<0.05, *** p<0.01. Dummies for stratification blocks included in the regression, but not reported.

Table 12: ITT-effects on income generating activities

Outcome	Baseline Mean	N	Difference in means	Difference-in-Difference	First-Difference
Number of income-generating activities (including agriculture and livestock)	1.98	3454	-0.181***	-0.104	-0.06
	[1.106]		(0.06)	(0.11)	(0.07)
Number of businesses (excluding agriculture and livestock)	0.92	1645	0.176	0.258	0.291*
	[0.779]		(0.13)	(0.19)	(0.15)
No of businesses started with VSLA	0.00	1546	0.038	0.032	0.036
	[0.033]		(0.02)	(0.02)	(0.02)
Total income from all businesses	15289.58	1263	8846.643	-136.579	-443.021
	[59466.8]		(10318.70)	(11064.17)	(12291.89)
Total income from all businesses (log)	8.03	1201	0.486	0.383	0.509
	[1.823]		(0.39)	(0.50)	(0.54)
Total income from all businesses (no businesses = zero income)	10429.14	1645	6432.549	448.083	-765.38
	[49612.5]		(8797.31)	(7801.45)	(8119.99)
Total income from all businesses (no businesses = zero income) (log)	5.37	1583	0.343	0.597	0.446
	[4.066]		(0.54)	(0.70)	(0.61)
Total income from all businesses - given business in 2009	15271.69	1273	7791.096	-1006.884	-956.436
	[59434.1]		(9956.58)	(10633.41)	(11815.35)
Total income from all businesses - given business in 2009 (log)	8.02	1211	0.538	0.37	0.585
	[1.844]		(0.47)	(0.56)	(0.59)
Total stock of petty trade business (log)	5.32	629	1.145	-1.207	1.074
	[3.98]		(0.71)	(0.95)	(1.51)
Number of non-household members employed in enterprises	0.51	1645	0.017	-0.042	-0.033
	[1.541]		(0.16)	(0.17)	(0.16)
Any non-household members employed in enterprises	0.16	1645	0.026	-0.021	-0.009
	[0.369]		(0.02)	(0.04)	(0.04)

Note: The table shows that there is no effect on income generating activities. Standard errors in parentheses, clustered at the village level. Standard deviations are in square brackets. N is the number of observations for the pooled regression. * p<0.10, ** p<0.05, *** p<0.01. Dummies for stratification blocks included in the regression, but not reported.