Does group inclusion hurt financial inclusion? Evidence from ultra-poor members of Ugandan savings groups[*]

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Abstract

Savings groups are bringing financial inclusion to millions of unbanked people in developing countries. However, their ability to meet the financial needs of the poorest members of local communities remains unclear. In this paper we show empirically that savings groups of different composition generate different levels of savings and borrowing for their poorest members. Our interpretation is that savings groups may not generate enough funds to meet the borrowing needs of their members, and that the severity of funds rationing is higher in groups that are, on average, poorer. In addition, within each group, the burden of scarcity falls disproportionately on ultra-poor members who are more likely to be prevented from borrowing compared to other members. Our data come from a RCT conducted in Uganda, in which we randomly assigned some ultra-poor, vulnerable households to groups with different proportions of other ultra-poor, vulnerable members.

JEL classification: O12, O16

Keywords: Savings groups, VSLA, Financial inclusion, Local finance, Microfinance, Self-help groups.

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1   Introduction

Savings groups are widely considered an innovative and effective instrument for bringing financial inclusion to poor, vulnerable households who are usually not reached by traditional banking or microfinance interventions. Savings groups are community-based financial institutions in which individual savings are accumulated on a weekly basis in a common pool stored in a safe box, and lent out to requesting members. At the end of the operating cycle (usually one year), all funds in the safe are shared among the group's members in proportion to the amount saved during the period of operation. Because they can be set up and maintained with minimal outside intervention, savings groups are spreading extremely fast in sub-Saharan Africa and elsewhere. In 2014, an estimated 10.5 millions households were members of savings groups, a tenfold increase relative to 2008.[1] In addition, savings groups are increasingly becoming an integral part of large-scale anti-poverty programs. These programs often create groups in which the majority of the members are ultra-poor.[2]

Despite the rise in popularity of savings groups, very little is know about what determines their ability to meet the financial needs of its members, especially of those who are ultra poor. Here we address this question by studying empirically how savings-groups composition affects the behavior and welfare of their ultra-poor members. Our data come from a RCT we conducted in collaboration with a large development program active in rural Uganda. The program targets households who are considered *vulnerable* because of their low socioeconomic status, and includes them into savings groups together with other members of the local community. In our experiment, we randomly assigned vulnerable households targeted by the program to savings groups of fixed size, but in which the proportion of other targeted households is either 1/4 or 1/2. In both group types, the remaining fraction of the group is composed by self-selected members of the community, who have, on average, better socioeconomic status compared to our target population.

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[1] In 2014, 1.2 million people belonged to savings groups in Uganda, where we conduct our intervention. These statistics are taken from the Savings-Led Working Group (SLWG) of SEEP (available at www.seepnetwork.org/filebin/docs/SG_Member_Numbers_Worldwide.pdf), and are likely to understate the true participation as they are constructed from data submitted by large NGOs, and therefore do not include groups trained by smaller organizations or independent agents. For example, Greaney, Kaboski, and Van Leemput (2013) estimate global participation to savings groups and self-help groups to 100 millions (see Section 2.1 for the difference between savings groups and self-help groups.)

[2] For instance, both the Colombian and Dominican Republic's conditional cash transfer programs (Red Unidos and PROSOLI respectively) have introduced savings groups in their development programs (Salas, 2014). Our study is also in partnership with a large antipoverty program called SCORE (see Section 2.3).

As a consequence, targeted households were randomly assigned to groups with higher or lower average socioeconomic status. We then carefully study the evolution over the operating cycle of individual savings and borrowing of all members of the savings groups created for the study.

We find that majority-vulnerable groups generate 21% fewer total savings and disburse 33% fewer cumulative loans during the central period of the cycle compared to minority-vulnerable groups. At the end of the cycle, performance metrics (cumulative savings, loans disbursed, return on savings, and default rates) are similar for both types of groups. We also find differences at the individual level: in the middle of the cycle, targeted vulnerable households save 23% less and borrow 48% less when randomly placed in more vulnerable groups, with this effect fading out toward the end of the cycle. Because our targeted participants were randomly assigned to groups, this difference in savings and borrowing can be attributed to the intervention. In contrast, savings and borrowing from self-selected participants does not vary significantly with the treatment. We also show that, within our targeted population, those with worse socioeconomic characteristics decrease their savings and borrowing by a larger amount when placed in a majority-vulnerable group.

The fact that individual savings and borrowing are lower in more vulnerable groups may be due to a number of reasons, which we discuss in detail in section 6. A particular mechanism that is both important and most plausible is the presence of rationing of funds. Because savings accumulate slowly over time, initially all borrowers are rationed out of funds. Groups composed of poorer members take longer to accumulate funds and meet the underlying demand for loans of its members. Therefore, in these groups rationing is both more severe and longer lasting than in minority-vulnerable groups. Also, according to the rules of savings groups, a member who wishes to borrow must first save. Hence, whenever members expect fewer loanable funds to be available, they may decrease the amount saved within the group, thus reinforcing fund scarcity. Eventually, both types of groups accumulate sufficient funds, and their differences fade out as the cycle ends. In practice, then, vulnerable households that are placed in majority-vulnerable groups satisfy their borrowing needs later than those placed in minority-vulnerable groups.

Lastly, we make use of responses from interviews carried out sixteen months after the groups are constituted to study the short-term effect of the intervention on households' welfare. We find little evidence that members of majority-vulnerable groups are significantly worse off in terms of total accumulated savings, asset ownership, household labor supply, and overall investments in productive activities. On the other hand, these

households report lower investments in housing and a higher probability of selling land compared to those in minority-vulnerable groups. Overall, the short-run effects of including a targeted vulnerable household in a majority-vulnerable group rather than a minority-vulnerable group are negative but small. However, it is important to note that the long-run impact of our intervention may be significantly different from its short-run impact. Firstly, because savings groups start each new cycle with zero funds, temporary limits on fund availability may occur cycle after cycle. Secondly, there is already evidence that participation in financial groups improves social ties within members of the group (Pande, Field, and Feigenberg, 2013). To the extent that less vulnerable groups offer "better" social ties, participation in such groups may provide additional future gains. Thus, the overall benefit of group participation in a group with more liquidity is likely to slowly accumulate over time.

These results are relevant for several reasons. First, savings groups that look very similar at the end of the cycle may operate very differently during the cycle. Hence, proper evaluation of savings group performance must take into account the dynamics of the group, and should not be limited to performance metrics obtained at the end of the cycle. Second, the presence of fund scarcity in savings groups may be an important determinant of group formation and group diffusion, and thus may hinder the ability of savings groups to bring financial inclusion to underprivileged populations. From a policy design perspective, the functioning of these groups can be improved by, for example, providing outside funds, encouraging early savings (that can be lent out multiple times and ease the rationing of funds), or allowing members to carry part of the savings accumulated with the group to a new saving cycle. Finally, our paper suggests that the benefits of savings group participation may be lower if all participants are ultra poor or vulnerable. This has implications for welfare programs that promote participation in savings groups. Poor members may be better served if the membership of their groups is opened up to a richer population.

Relation to the literature   The existing literature has found important benefits from the introduction of savings groups in rural, unbanked populations (for a complete list of these studies, see the review by Gash and Odell, 2013). For example, Beaman, Karlan, and Thuysbaert (2014) randomize at village level the creation of savings groups in Mali. They find that treated villages have higher savings (+30%), borrowing, consumption smoothing, food security, livestock holding compared to control villages. They also

report that the wealthiest member of each village tend to select into savings groups. Ksoll and Forskningsenhed (2013) find similar results employing a similar research design. Bundervoet (2012) finds large effects of savings groups participation on household welfare by randomizing the timing of the provision of the savings group training. Whereas these works establish that savings groups' participation is overall beneficial to participating households, our study is mainly concerned with how the composition of a savings group determines its overall effectiveness at providing savings and credit to its members, especially those who are ultrapoor.

With this respect, we are close to Greaney, Kaboski, and Van Leemput (2013) and Cassidy and Fafchamps (2015). Cassidy and Fafchamps (2015) find some evidence that the (fully endogenous) process of group formation is able to match people who are willing to save with people who desire to borrow when the propensity to save or borrow is measured in terms of present bias. However, they also find that people with similar professional background join the same groups. Greaney, Kaboski, and Van Leemput (2013) study group outcomes arising from alternative group formation processes. They show that groups performance improves when savings-groups training is paid by the group members rather than being provided for free, and use a standard credit market imperfection model to argue that high-risk, low-returns agents are driven out of groups with entry costs. In contrast, the evidence from savings groups in our study indicates that increasing the number of ultra-poor members of a group does not lead to worse repayments, but affects group outcomes through alternative channels such as rationing. A second difference is that, because of randomization into groups and because of our unique member-level data, we are able to show that participants' saving and borrowing behavior respond to their group composition.[3]

More broadly, our study is related to the literature on financial inclusion. Several existing papers demonstrate that microentrepreneurs benefit from access to finance (see, for example, Banerjee, Duflo, Glennerster, and Kinnan, 2015) and access to a safe way to store their money (see, for example, Dupas and Robinson, 2013). For other subpopulations, the evidence remains mixed, with some evidence of overindebtness from microfinance in Bangladesh (Karim, 2011) but not in Mexico (Angelucci, Karlan, and Zinman, 2015). In our experiment, groups with different composition are differentially able to satisfy the

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[3] To our knowledge, Salas (2014) is the only other paper that studies the internal borrowing and savings behavior of group members, and analyzes the dynamics of borrowing and savings during a single cycle. Salas focuses on the behavioral response to a mental accounting intervention where members choose savings goals.

demand for loans of its members, but are equally able to provide a safe way to store funds. Therefore, vulnerable households enrolled in groups with different vulnerability profiles are differentially able to access credit from the group during the first months of operation of the groups. By comparing the welfare of vulnerable households enrolled in different types of savings groups, we can measure the effect of extending credit to vulnerable households, which are usually not reached by microfinance interventions. Our data show that being temporarily prevented from borrowing has limited effect on the welfare of this population.

Finally, our intervention highlights the importance of the group in shaping the choices of the individual. Our results share some similarities with experimental studies of peer effects (see Sacerdote, 2014, for a recent review of the literature). However, our paper differs from that literature in several meaningful ways. First, peer-effect interventions usually fully randomize group assignment, and therefore eliminate any form of endogenous self-selection. This type of full randomization was not possible to implement in our context, where some group participants were endogenously selected. Second, most peer-effect experiments focus on schooling, and not on financial markets. In this regard, we are close to Pande, Field, and Feigenberg (2013), who study the impact of social interactions in microfinance groups in India. In their paper, they experimentally varied the frequency of interactions of microfinance groups, holding the composition of the group constant. In contrast, our intervention holds the frequency of group meetings constant, and experimentally vary the composition of the group.

The rest of the paper proceeds as follows. Section 2 provides some background information, including all details on the different types of savings groups and their rules of operation. Section 3 describes our intervention. Section 4 describes our empirical strategy. In Section 5 we present our empirical results relative to the functioning of different types of groups, and the behavior of preselected vulnerable members of different types of groups. We discuss possible mechanisms behind these results in Section 6. In Section 7 we discuss the effects of our intervention on household welfare. The last section concludes.

2 Background information

2.1 Savings groups

A savings group is community-based financial institutions that collects savings from its members into a common pool, lend those funds back to its members, and typically operates

over a limited period of time (the cycle). The most common type of savings group (and the one under analysis in our paper) is the Village Savings and Loans Association (VSLA), which was first introduced by CARE International in Niger in 1991. VSLAs operate in the following way. First, an association organizer recruits and trains potential members from the community (typically 20-30 people). Following the training period, the group agrees on the bylaws of the association, which include the length of the savings cycle, the interest rate charged on loans and the value of a *share*.

During each weekly meeting, each member saves with the group by purchasing shares from the group. The maximum number of shares that each person can purchase in a given meeting is five, which imposes an upper bound to the amount that can be saved with the group during a single meeting. Hence, unlike a ROSCA where savings contributions are fixed, in a VSLA each member chooses each week how much to save. Savings deposits are recorded in a group ledgers and in an individual booklet. All cash deposits are pooled and kept in a metal safe box, which is opened only when the group is in session. Members are not allowed to withdraw their savings during the cycle.

Borrowing starts three months after the beginning of the cycle. Individual loans are extended to group members subject to three constraints: the group must agree on the stated purpose of the loan; loan sizes are restricted to three times the amount saved by the borrower until that point; and total loan disbursements should not exceed the amount available in the safe box. Loans must be repaid within three months, and the interest on the principal compounds monthly. Once the loan is paid back, the borrower is eligible to receive another loan.

Three months before the end of the cycle, loan disbursements end and outstanding loans are repaid. The last meeting is devoted to the *share out*: the content of the safe box is emptied and divided among the members of the group in a way that is proportional to the amount each person saved. A new cycle is eventually started. Between the end of the old cycle and the beginning of the new cycle the group composition may change and the rules governing the group may be modified.

Loans and share out provide participants with a sizable amounts of funds. In the groups we study, the single most common use (44% of loans and 39% of share out) is the payment of school fees. In addition, 35% of loans and 40% of share out amounts are used for some type of productive investment, including starting a new business, purchasing of farm inputs such as livestock and land, or other business investment. Loans are somewhat more likely than share out to be used for emergencies, such as a health incident

or unemployment (22% versus 16%.). Conversely, and quite predictably, households are almost twice as likely to consume their share out (29%) than their loans (16%).[4]

The VSLA model has been adopted and modified by other NGOs, generating substantial heterogeneity in the types of savings groups currently existing. Most of these variations maintain the basic rules described above but modify the way the group is trained or accounts are kept.[5] We also make a distinction between savings groups and *self-help groups.* Self-help groups developed in India simultaneously and independently to the VSLA model (and its variations). Typically, they do not return all funds to their members during share out, but instead distribute profits or dividends over time. For this reason, assuming a similar composition, savings groups and self-help groups are likely to generate very different levels of savings and loans.[6]

2.2   Conceptual framework

The primary role of a savings group is to channel funds from members who want to save to members who want to borrow. However, contrary to the way a frictionless credit market operates, the rules governing the functioning of a savings group do not guarantee that, in each period, the supply of funds will match the demand for funds. In this section, we discuss the causes of this potential mismatch of demand and supply of funds, and how group composition may alleviate it or worsen it.[7]

The main reason for the potential mismatch between demand and supply of funds within a savings group is that savings earn the same return independently on when they are contributed to the group, which creates the incentive to save as late as possible. This is inefficient because early savings are more beneficial to the group than late savings. Early savings can be lent our multiple times, and each time they are lent out they generate a return that can also be lent out. Note that the presence of an upper bound to the amount

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[4] Funds use is self reported by study participants. See appendix Table B1.

[5] See, for example, Savings and internal lending communities (SILC) promoted by Catholic Relief Services, and Oxfam's Saving for Change (SfC). However, some models also modify the way the share out is conducted (i.e., MUSO and Pact-WORTH model). These models are much less common than VSLA, SILC or SfC.

[6] For a more detailed discussion regarding the types of savings groups and the difference between savings groups and self help groups, see Allen and Panetta (2010), Ashe (2009), Vanmeenen (2010). Note that the distinction between self-help groups and savings groups described here is gaining popularity but is not universally adopted. For example, Greaney, Kaboski, and Van Leemput (2013) study SILCs (which, according to our classification are savings groups) but call these groups "self-help groups". Blattman, Green, Jamison, Lehmann, and Annan (2015) also follow the same terminology when referring to VSLAs.

[7] This framework is based on the theoretical model developed in Burlando and Canidio (2015).

that can be saved during each period mitigates this inefficiency, because it forces members who desire to save with the group more than this upper bound to spread their savings over multiple periods. Still, despite the presence of this constraint, within a savings group funds tend to be scarce when they are most valuable, that is at the beginning of the cycle. As a consequence, early on in the cycle a savings group may be unable to generate sufficient funds to meet the demand for loans, but rationing of funds should ease over time.

The second reason why supply and demand for funds may not match is that the interest rate on loans is chosen by the group at the beginning of the cycle. The process of choosing the interest rate is not part of the rules of functioning of a savings group, and different groups could potentially proceed in different ways. In any case, there is no presumption that the decision made by the group will lead to the efficient generation and allocation of funds. For example, any change in the preferences of one of the group members (for example, an increase in the desire to borrow) should have an effect on the market-clearing interest rate. However, if the group chooses its rules via voting, such change in preference may not affect the preferences of the "median" member and may not change the interest rate set by the group.

The possibility of a mismatch between demand and supply of funds implies that the benefit of group participation depends on the group composition, in ways that are potentially non-monotonic. Consider a group member who is a saver, and assume that this member is substituted by another person who is identical in all respects but has a higher propensity to save. As a result, the funds available to the group in each period increase exogenously. This shock to the group composition is beneficial to the other members of the group if funds are scarce, because these additional funds can be used to meet their demand for loans. However, this shock will be detrimental to the group whenever the demand for loans is already met, because it will reduce the return on savings of all other members of the group (including borrowers, who are required to save with the group). It follows that, for example, borrowers will want to be in the same group with people who will provide enough funds to meet their demand for loans, but not with people that will provide more than that amount.

Note also that, because a group member who wishes to borrow must first save, we should expect a positive correlation between level of borrowings and level of savings. That is, in groups in which more loanable funds are available, the possibility of receiving larger loans should increase the savings level of potential borrowers. This mechanism is

relevant because it tends to make scarcity of funds more severe. Also, in most groups, funds may be scarce during some periods (especially early on in the cycle) and abundant in others. In these cases, exogenously increasing the supply of savings will be beneficial to the group if scarcity is the most relevant case (and detrimental otherwise).

2.3   Project SCORE

Our research project is in partnership with project SCORE, a joint program of four NGOs based in Uganda (AVSI, CARE, TPO, FHI360) that provide services to 125,000 Vulnerable Children (VC) and their households in 35 districts across Uganda. Project SCORE (Sustainable COmprehensive REsponses for vulnerable children and their families) was launched in the fall of 2011 with a USD 9 million USAID grant. Project SCORE is a set of interventions implemented over a period of 5 years having the following goals: to identify vulnerable children in ultra-poor households across communities in Uganda; to improve their socio-economic status, food security, and nutrition status; and to increase the availability and access to protective, legal and other critical services. Targeted households receive a number of interventions, including classes on advanced farming techniques, cooking, nutrition, business training and business development. Contrary to other comprehensive antipoverty programs targeting ultra-poor households (such as the ones studied by Banerjee et al., 2015, and Blattman et al., 2015) SCORE offers no transfers to beneficiary households, neither in money nor in kind.

The most important intervention carried out under SCORE involves the enrollment of beneficiaries into SCORE-created savings groups, which follow CARE's VLSA model. Such groups are formed by first enrolling a core of SCORE recipients, and then enrolling other interested community residents. Crucially, SCORE requires that at least half the membership is composed of SCORE recipients. As a consequence, compared with VSLAs supported by other organizations, SCORE groups are generally more inclusive of vulnerable and marginal households.

Importantly, the four NGOs involved in program SCORE do not provide services directly. Rather, they outsource all interventions to a number of smaller community-based local organizations (which we refer to as Implementing Partners or IPs), which typically operate in small areas (few villages). 22 IPs participated in our research, giving rise to an interesting variation in the capabilities of the organizations carrying out the intervention.

3   The Intervention

Our experiment takes place in the context of an expansion of project SCORE to 90 mostly rural villages not previously served by the project. This expansion was widely geographically dispersed, involving 28 districts in the Western, Central, Eastern and Northern regions, starting in January 2013. In each study village, SCORE representatives worked with local organizations and government officials to identify ultra-poor households with children. Identified households were further screened through a questionnaire, aimed at assessing their level of vulnerability and their eligibility for inclusion into project SCORE (see Section 3.1 and Appendix A for more information on the vulnerability indicators employed). Once enrolled, social workers carried out additional interviews to determine whether the household was interested in joining a VSLA. The enrollment process stopped once SCORE identified 14 households per village who were willing to form a VSLA. We refer to these households as the *preselected* study participants. All preselected participants enrolled were given access to other standard SCORE services.

Using the information contained in the screening questionnaire, the research team randomly assigned 32 villages to a *sparse treatment*, and the remaining 58 villages to a *dense treatment*.[8] In dense treatment villages, one VSLA was formed, comprising of all 14 preselected participants. In sparse treatment villages, preselected households were evenly divided into two separate VSLAs. In both cases, the remaining members of each VSLA was made up by members of the local community, whom we refer to as *self-selected*. Since the size of the group was capped at 27, the experiment generated variation in the number of preselected ultra-poor households and community members allowed into the groups: Dense groups were composed of 51% preselected and 49% self-selected participants, whereas sparse groups were composed of 25% preselected and 75% self-selected participants. See Figure 1 for a simple schematic of the protocol. The expectation (which we verify in Section 5) was that dense groups would have, on average, a higher fraction of ultra-poor members than sparse groups.

Between April and July 2013, field officers established and trained the study VSLAs by following SCORE standard procedures. In particular, field officers sought to enroll self-selected participants through a process of engagement of the local community which included presentations in local markets, churches, and community-based organizations. After this process of group formation, field officers spent one month training the group

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[8] To avoid treatment spillovers, all study villages located in the same local administrative unit (the parish) were assigned to the same treatment.
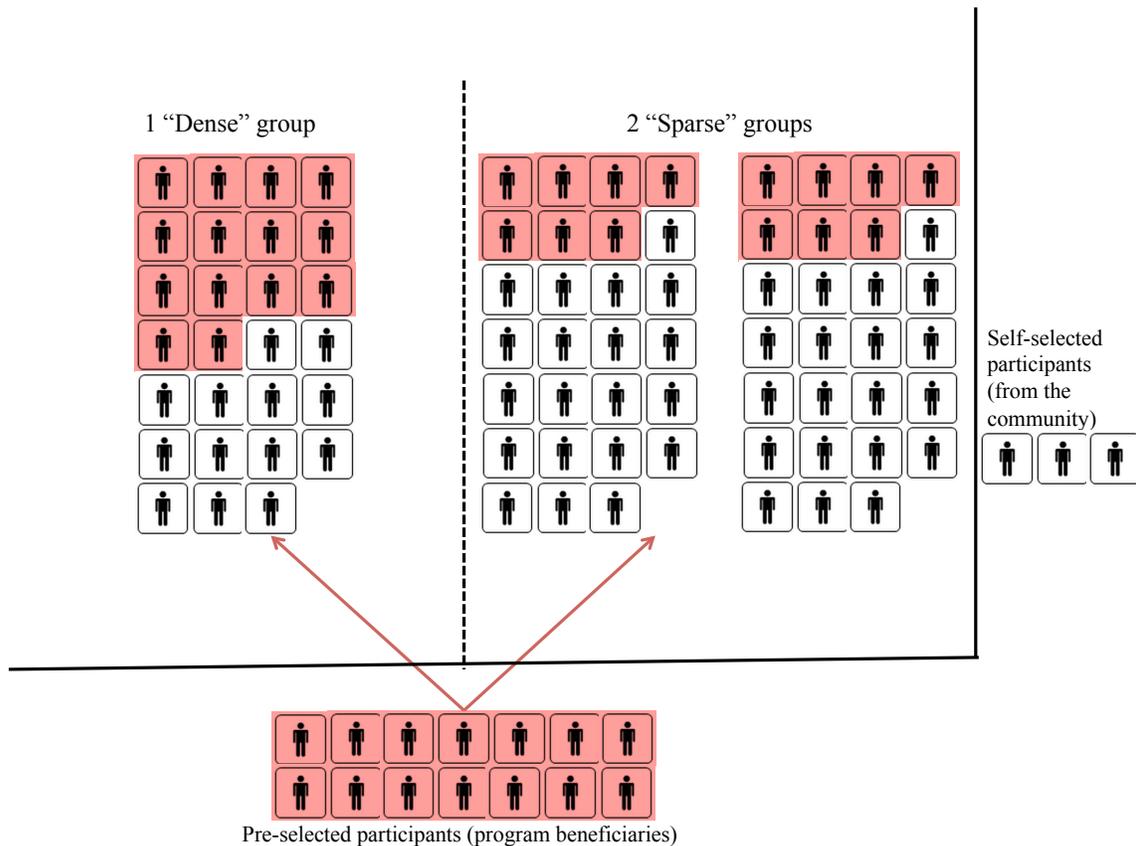
Fig. 1: Randomization strategy–Group formation stage

in the VSLA methodology. Qualitative assessments indicate that the training period was associated with some turnover in the membership: as participants learned about their group, some chose to leave and were replaced by others. Final membership was formalized during the first day of the cycle. Group finances were audited on a regular basis during the cycle. Our evaluation program ended at the end of the first cycle, which for most groups was approximately in August 2014.

Table 1, panel A describes the group formation process. The randomization plan expected the formation of 58 dense VSLAs and 64 sparse VSLAs. Nonetheless, the process of creating groups was complicated and not always effective; for instance, only 116 (56 dense and 60 sparse) of the expected 122 VSLAs were formed (see Table 1). Setting up sparse groups took an average of 15 extra days (81 days on average from randomization

Tab. 1: Group formation statistics

	Total	Dense VSLAs	Sparse VSLAs
Panel A: Group Composition			
Number of VSLAs (planned)	122	58	64
Number of VSLAs formed (actual)	116	56	60
Fraction formed	95.1%	96.6%	93.8%
Time to form (days since randomization)	74.2	66.1	81.8
Number of SCORE beneficiaries enrolled	1,158	765	422
Number of non-beneficiaries enrolled	1,940	771	1,256
Fraction composed of SCORE beneficiaries	37.4%	49.8%	25.1%
Panel B: Dropouts and substitutes			
Pre-selected participants	1,234	798	436
Pre-selected participants not enrolled (dropped out)	264	164	100
Fraction of pre-selected who dropped out	21.4%	20.6%	22.9%
SCORE beneficiaries that were self-selected	204	131	86
Fraction of self-selected SCORE beneficiaries	16.5%	16.4%	19.7%
Panel C: Access to financial services in community			
Number of VSLAs in village	4.28	4.1	4.59
Number of formal Credit Union (SACCOs) in parish	0.34	0.37	0.26
Number of formal banks in parish	0.01	0.03	0
Number of MFIs	0	0	0

Notes: Drop out participants were assigned to a VSLA but never formally enrolled. This excludes enrolled participants who were assigned a savings booklet and a personal ID number but eventually dropped out. Panel C includes statistics from a community survey carried out one year after the start of the intervention. Statistics reported are average number of branches in 88 study villages (for VSLAs) excluding one outlier (with 100 VSLAs reported); and the average number of banks, SACCO and MFI branches in the 74 parishes. Recall the parish is a larger administrative unit than the village.

to first day of operations, relative to the 66 days needed in dense groups). Finally, the makeup of the groups follows the intended assignment (50% SCORE enrollees in dense and 25% in sparse groups).

Panel B describes movements in and out of the group by SCORE members. Only 79% of the preselected households that were assigned to a study VSLA were ultimately enrolled in one, suggesting significant turnover in the initial membership. Appendix Table B2 shows that the probability of enrolling in a VSLA is not correlated with the treatment assignment, but households who are food insecure, host orphan children, or have chronically ill members are more likely to remain enrolled. When a preselected study participant declined to enroll into her assigned VSLA, the field officers in charge of forming the groups typically replaced her with another SCORE beneficiary. Because the replacement beneficiaries are not part of the preselected group and joined after the randomization process was completed, we consider them as being self-selected in our

analysis.

Finally, panel C shows the availability of financial institutions (VSLAs, credit unions, banks) in dense and sparse villages. These statistics come from a census of financial institutions in study villages carried out approximately one year after the start of the intervention. Most villages have a number of savings groups operating, and the total number of VSLAs (including those created for the purpose of the study) is similar in the two types of villages. Regression analysis, reported in Appendix B Table B4, confirms that the difference in the number of groups between dense and sparse villages is not statistically significant. In addition, dense and sparse areas have the same population and access to financial services (i.e., no local access to MFI and other banks, and statistically insignificant differences in terms of local credit unions or SACCOs). In other words, our intervention did not change the availability of financial services in sparse villages relative to dense villages in any meaningful way.

3.1 Data

Our data consists of baseline and endline household surveys collected during one-on-one interviews of study participants, and financial records from three financial audits of group finances carried out during the first cycle of operation.

Baseline data Our baseline information includes the screening tool used to determine the vulnerability of prospective SCORE beneficiaries. The definition of vulnerability followed by project SCORE is multi-dimensional and not limited exclusively to economic vulnerability. Thus, the tool contains questions on the household's socioeconomic status (income per capita, food security, access to safe water and latrines) and the well-being of the children belonging to the household (including disability and HIV status, as well as history of physical or mental abuse). Appendix A has a full description of the variables and the indicators collected in the screening tool and used in this paper.

Because the baseline was collected as part of the SCORE enrollment process, these data are limited to those individuals enrolled in program SCORE. To obtain information on non-SCORE members of the study groups, a team of enumerators administered the same questionnaire to the self-selected participants of the study VSLAs shortly after these groups formed. By combining the data from the preselected participants with those collected from the self-selected participants, we are able to construct a profile of baseline characteristics for groups in the two treatment arms. This profile is subject to

two important caveats. First, the profile is constructed from pre-intervention data from preselected participants, and post-intervention data from self-selected members. While applying the "baseline" terminology may be a slight abuse of language in this context, it is unlikely that the treatment had an effect on vulnerability immediately after the groups were formed. Second, since SCORE's target population are households with vulnerable children, a significant portion of the screening questionnaire is devoted to understanding the well-being of children. For households without dependents, we thus collected only a subset of all vulnerability indicators present in the questionnaire.

Baseline characteristics of the 1,232 preselected households are reported in the first two columns of Table 2. These households face significant challenges in their daily life. For instance, one fifth has a child involved in child labor, 15% have a member with a chronic disease, 40% have a member with disabilities, and 50% of them contain an orphan child. In addition, households in the sample report very limited economic resources, with an average monthly income of 41,000 UGX (approximately US$15[9]). This is consistent with a very low reported consumption of little more than one meal per day. On the other hand, many (over 60%) of these households do have access to public infrastructure like latrines and well water. The remaining columns of Table 2 report that the vulnerability profile of participants in dense and sparse groups are similar, indicating that the treatment arms are balanced in the way it was intended.

VSLA administrative records   Information on savings and borrowing behavior of groups participants originates from administrative records collected by SCORE field officers during their regular financial audit of VSLAs. The assessment of study groups was carried out following standard procedures in SCORE: field officers reviewed transactions and record-keeping, reconciled discrepancies between cash ledgers and savings booklets, and finally reported audited figures in a standard audit form. The information collected included how much each group member saved, borrowed, and repaid up to the audit date; whether the borrower was in arrears; and whether the member dropped out of the group during the evaluation period. The visits were conducted approximately every four months, giving rise to three waves of data collection: after approximately 4 months of operation (*wave I*), after approximately 8 month of operation (*wave II*), and at the end of the operating cycle of the group (*share out*).[10] Auditors also reported the interest rate charged on loans

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[9] Exchange rate in January 2013 was 2,660 UGX per dollar.

[10] Seventeen groups chose an abbreviated cycle; for those groups, the last two audits are very similar or identical.

Tab. 2: Summary statistics for preselected SCORE beneficiaries

Summary Statistics and treatment balance

VARIABLES	All observations		Assigned to dense		Assigned to sparse		Difference	
	Mean	St.Dev.	Mean	St.Dev.	Mean	St.Dev.	Difference	T-Statistics
Child labor	0.226	0.419	0.247	0.432	0.189	0.392	0.058	0.594
Drug abuse at home	0.0885	0.284	0.105	0.307	0.0576	0.233	0.0474	1.199
Chronic disease	0.162	0.368	0.173	0.379	0.141	0.348	0.032	0.937
Disability in household	0.390	0.488	0.402	0.491	0.368	0.483	0.034	0.812
Food insecure	0.685	0.465	0.705	0.456	0.649	0.478	0.056	0.112
Quality diet	2.222	0.847	2.256	0.872	2.159	0.795	0.097	0.718
Number of daily meals	1.116	1.025	1.141	1.029	1.071	1.019	0.07	0.183
Informal employment	0.578	0.494	0.541	0.499	0.646	0.479	-0.105	-1.429
Household unemployed	0.155	0.362	0.158	0.365	0.149	0.357	0.009	0.928
Orphaned child in hhld	0.500	0.500	0.492	0.500	0.515	0.500	-0.023	-0.0173
Safe source of water	0.658	0.475	0.674	0.469	0.627	0.484	0.047	0.544
Access to latrines	0.768	0.422	0.769	0.422	0.766	0.424	0.003	-0.204
Income per capita	7,111	9,056	7,442	9,674	6,509	7,783	933	0.790
Assessor scale	1.927	0.584	1.906	0.612	1.966	0.527	-0.06	-0.992
Total vulnerability score	57.86	10.14	57.93	10.39	57.74	9.672	0.19	0.182
Household size	6.442	2.687	6.340	2.657	6.628	2.736	-0.288	-1.010
Vulnerability index	0.392	0.926	0.370	0.952	0.433	0.876	-0.063	-0.57
Dense VSLA	0.647	0.478	1	0	0	0		

Summary statistics for the sample of preselected only. See appendix A for a description of indicators.
T-statistics of difference between dense and sparse are clustered at the parish level.

and the value of the share.

Table 3 provides summary statistics of financial transactions from the financial audits. In our sample, groups applied interest rates on loans equal to 3%, 5%, 10% or 20% per month, with the average being 8.6% the mode being 10%. Share values average 888 UGX ($0.35), with most groups choosing either 500 UGX or 1,000 UGX, and five groups 2,000 UGX. By looking at share-out values, we find that, on average, groups members earned a return of 13% for every shilling saved with their respective VSLA.

The rest of the table describes the data collected during the three audit periods. Cumulative savings of all participants accumulate slowly, reaching 100,000 UGX (approximately $40) by the end of the cycle. Likewise, cumulative borrowed amounts increase from 41,000 UGX during the first audit wave to 166,000 UGX ($62) by the end of the cycle. The average member obtained 2.7 loans by the end of the cycle. By the time of the first wave of data collection, almost 70% of members had obtained at least one loan, and the proportion increases to almost 90% by the end of the cycle.

Importantly, audit records include a measure of loan default (i.e., loans past their due

Tab. 3: Summary Statistics–Group characteristics and audit data

Summary Statistics--Group characteristics and audit data	(1)	(2)
	Mean	St. Dev.
Group Characteristics		
Pre-selected participants	0.302	0.459
SCORE beneficiaries	0.369	0.483
Interest rate	8.686	2.779
Share price	888.2	307.3
Audit Wave I		
Number of meetings (to date)	17.99	7.294
Savings per person (to date)-UGX	41,868	35,762
Average number of shares purchased per person per meeting	2.659	1.767
Member borrowed at least once	0.681	0.466
Number of loans per person	1.420	1.346
Average amount borrowed per person (to date)--UGX	68,338	116,943
Member has loans past due	1.503	2.859
	0.0159	0.125
Audit Wave II		
Number of meetings (to date)	34.57	13.32
Savings per person (to date)-UGX	86,226	60,835
Average number of shares purchased per person per meeting	2.578	2.277
Member borrowed at least once	0.806	0.396
Number of loans per person	2.357	1.637
Average amount borrowed per person (to date)--UGX	132,274	205,087
Member has loans past due	0.0826	0.275
Audit Wave III-End of cycle		
Number of meetings (to date)	46.67	9.072
Savings per person (to date)-UGX	98,790	64,926
Average number of shares purchased per person per meeting	2.500	1.514
Member borrowed at least once	0.893	0.309
Number of loans per person	2.675	1.900
Average amount borrowed per person (to date)--UGX	166,398	256,350
Member has loans past due	0.0309	0.173
Member enrolled in dense VSLA	0.477	0.500
Number of VSLAs formed	116	

Data from audits of savings groups. Each observation is a member of the group; includes both preselected who joined the group and self selected.

date). The amount of loans past due changes significantly over time. During the first audit wave, only 1.5% of loans were considered past due. That increased to 8.3% by the second audit wave, but dropped again to 3% at the end of the cycle. Note that having an outstanding loan at share out does not imply a default on a loan. The reason is that groups will seize the savings of borrowers with unpaid loans at share out. Hence, the actual defaults (always partial) are much fewer than 3%. Overall, the groups are effective at preventing defaults and late repayments.

Finally, the data suggest that, despite the significant number of loans given out during the cycle, funds are scarce within the group. This is most evident in Figure 2, which shows that the overall funds-utilization rate (total cumulative loans over total cumulative savings) increases over time, starting from 1.3 and reaching approximately 1.5. That is, loans disbursed in each period grow faster than savings contributed in each period. This is possible because as past loans are repaid to the group (with interest) they can be lent out again. This is suggestive of scarcity, because when extra funds are introduced into the group (in the form of loans repayment), they are quickly lent out again.
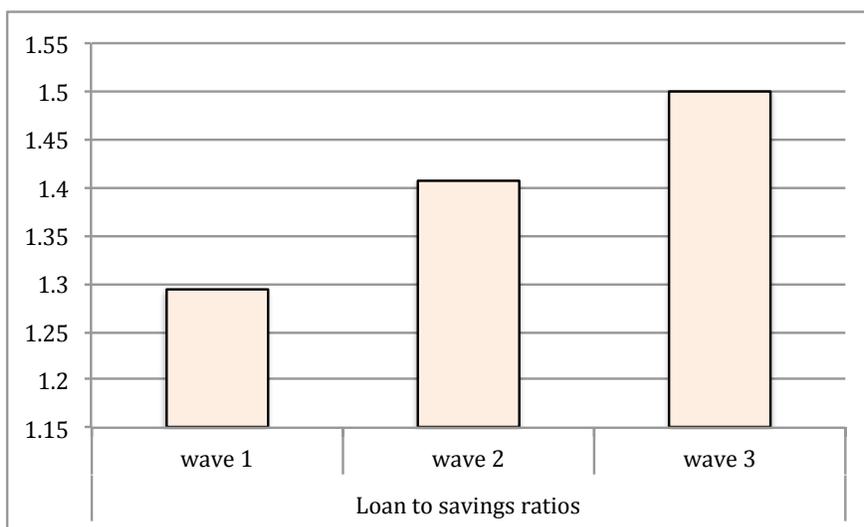
Fig. 2: Average savings-to-loan ratios

Endline data   Approximately one and a half years from the date of groups formation, we conducted an endline survey of all preselected households that were originally assigned to a savings group. The objective of the endline survey was to measure the effect of participation of preselected households into sparse and dense VSLAs on household welfare. The questionnaire covered asset ownership, food security, savings behavior, investment behav-

ior, and experience with the study savings group. The tracking team was able to find and survey 983 households, representing 77% of the original sample of study participants. Appendix Table B3 regresses the baseline vulnerability indicators on whether the household was tracked at endline. It finds that the attrition rate in dense and sparse villages is very similar, and that the probability of being found is higher for households having a member with a chronic disease and for households hosting orphaned children. Differential attrition is a concern: using the nonattrited sample, a regression of the treatment dummy on the set of baseline characteristics shows some imbalance for two vulnerability indicators (use of child labor and informal employment), even though the F-test test of the regression is statistically insignificant. Our empirical approach will specifically address the potential imbalance due to attrition.

4   Empirical strategy

Our empirical analysis has three main steps. We first establish whether the composition of dense and sparse groups differ. We then use data from financial audits of the groups to study the savings and borrowing behavior of the preselected members (i.e. the randomly assigned population) of these savings groups. Lastly, we use endline data to study the effect of the intervention on the welfare of participants.

Differences between dense and sparse groups   The first task is to compare the composition of dense and sparse groups. Because dense groups have a higher proportion of targeted program participants, our expectation is that members of dense groups are, on average, more vulnerable. Whether that is true in practice depends, crucially, on the characteristics of the self-selected population, and on whether these characteristics vary by the type of group. For instance, the two types of groups may be very similar (despite the randomization) if self-selected participants in dense groups are less vulnerable than those in sparse groups.

Using the vulnerability data we collected on both self-selected and preselected households, we thus regress vulnerability characteristic x for participant i in group g on her group assignment:

$$x_{ig} = \alpha_0 + \alpha_1 Dense_g + \alpha_2 Preselected_{ig} + \alpha_3 Dense_g \times Preselected_{ig} + \omega_{ig}, \quad (1)$$

where $Dense_g$ is an indicator variable that identifies the assignment to a dense group, and

$Preselected_{ig}$ is the indicator that identifies preselected from self-selected participants. The coefficient α_1 describes systematic differences between self-selected participants in the two types of groups;[11] α_2 describes differences between preselected and self-selected in sparse groups, and the coefficient α_3 is the difference-in-difference estimator. Due to randomization, we expect α_3 to be zero.

Ultimately, we expect the treatment to affect the performance of the group in some systematic way. To establish how group-level outcomes differ across treatment arms, we estimate

$$y_g = \alpha_g Dense_g + X_g \beta_1 + \epsilon_g, \tag{2}$$

where y_g is an outcome (aggregate cumulative savings, aggregate cumulative borrowing, return on savings, and default rates) measured during audit wave t; X_g is a group level control matrix which includes the number of completed VSLA meetings at audit date t and may include interest rate, share price, implementing partner fixed effects and a constant. $Dense_g$ identifies groups assigned to the dense treatment, and α_g is the coefficient that describes the difference in outcomes between dense groups relative to sparse groups.

Individual account behavior   Aggregate responses to the treatment captured by (2) can be thought of as the result of two effects: one arising from observed and unobserved compositional differences of the two groups, and one arising from participants behaving differently in dense groups and in sparse groups. While we cannot directly measure the former, due to random assignment we can measure the latter. Consider preselected person i in group g, and denote her individual-level outcome of interest at audit wave t (cumulative savings, borrowings, late repayment, and so on) by y_{igt}^{pre}. The empirical specification is

$$y_{ig}^{pre} = \alpha Dense_g + X_g \beta_1 + \epsilon_{ig}^{pre}, \tag{3}$$

where X_g are group-level controls from (2) and $Dense_g$ identifies groups assigned to the dense treatment. The key coefficient of interest is the intent to treat estimator, α. It describes the difference in saving and borrowing behavior of a person who is randomly assigned to a dense group as opposed to a sparse group. Because of random assignment, the preselected participants should not have observed or unobserved differences in their willingness to save or to borrow. Controlling for the interest rate and the share value,

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[11] It is important to emphasize that the results in equation (1) are only indicative of differential selection along observable criteria. Self selected participants in dense groups may differ along other unobservable characteristics, such as their demand for savings or credit.

then, any difference between dense and sparse (captured by the coefficient α) measures the individual members' causal response to their group assignment.

A few additional technical notes on the estimation strategy are required. First, due to noncompliance, we do not consider whether the final distribution in a VSLA was dense or sparse, but rather we use the randomization assignment. That is, we report intent to treat estimates. Second, it is likely that outcomes are autocorrelated within each village, and therefore errors are clustered at the village level. Finally, to deal with outliers and data entry mistakes, we trim the top 1% of individual savings, borrowings, and borrowing-to-savings ratio in each audit round dataset.[12]

Effects on welfare   We complete the paper by discussing the effect of our intervention on household welfare. Again, we focus our attention on those members who were randomly assigned to groups. For each outcome y_{ig}^{pre}, we estimate equation (3) on the full sample of preselected, including those who did not join their assigned VSLA. As before, the coefficient α is the intent to treat estimator. Because the sample includes preselected households who declined to join their group, our regression does not control for group characteristics. In addition, we also report the estimate of α obtained by limiting the sample to those preselected who joined their assigned VSLA. Significant effort was exerted in finding all preselected participants, both participants and group dropouts. This meant that interview teams revisited study areas multiple times in search of respondents, over a period of time that covered four months. A month-of-interview time-dummy is included to capture seasonal differences due to different interview periods. Despite our best efforts, as discussed earlier there is evidence of differential attrition between the two treatment arms. We use the approach in DiNardo et al. (1996) to "rebalance" the treatment arms. In practice, we use baseline characteristics to generate predicted probabilities of not being found in the endline, and use these as weights in a weighted least square (WLS) estimation model. This method is similar to inverse probability weights (see Imbens and Wooldridge, 2009) and is employed in other RCT studies to address attrition (i.e., Banerjee et al., 2015). Since the differential attrition problem is not severe, results from WLS are very similar to unweighted OLS.[13]

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[12] Estimates are larger in magnitude and more significant if trimming is not used; results available upon request.

[13] OLS results, as well as tests for common support of the predicted probabilities, are available from the authors upon request.

5   Results

5.1   Group composition

We first show the effect of the intervention on the vulnerability profile of each group. We estimate equation (1) on seven measures of vulnerability collected from all preselected and self-selected participant households, and report the results in Panel A of Table 4. Across almost all measures, the estimates on the *Preselected* indicator (i.e., coefficient α_1 in regression (1)) suggest that preselected households are more vulnerable than self-selected households. They have higher rates of physical or mental disability, are more likely to miss meals, have lower access to latrines, are more likely to be considered in need by the assessor who carried out the interview, and have 34% lower reported per capita income than the self-selected population. Taken together, preselected participants have a higher rank in the vulnerability index generated from these seven measures of poverty (last column of the table). This suggests that the screening process adopted by SCORE does capture some coarse dimensions of vulnerability. The other two coefficients in the regression (dense assignment and the interaction term) are small and statistically insignificant. This is indicative that the vulnerability profile of self-selected households and preselected households does not vary across the two types of groups. In other words, along observable characteristics there is no differential self selection into different types of groups.

Panel B pools preselected and self-selected participants, and compares the same seven vulnerability measures between dense and sparse groups. It shows that, on average, participants of dense groups are more likely to skip meals, have higher subjective measures of vulnerability as reported by assessors, but are more likely to have access to a safe source of water. In addition, the vulnerability index is significantly higher in dense groups. Overall and as expected, members of dense groups are, on average, poorer and more vulnerable compared to the members of sparse groups (with the exception of being more likely to have access to a safe source of water).

5.2   Group-level performance

We now use the data from the three audit waves to study the effects of the intervention on the aggregate performance of the study groups. We look at the rules chosen by the group, aggregate savings and borrowing, return on savings, and default rates.

5 Results

Tab. 4: Vulnerability profile of savings groups

VARIABLES	Disability in household	Number of hhld members	Sometimes missing meals	Safe source of water	Access to latrines	Assessor scale of needs	Per capita income	Vulnerability index
Panel A: Differences across group type and selection								
Preselected beneficiary	0.097*	0.142	0.167**	0.018	-0.112***	0.480***	-3,041.632***	0.745***
	(0.050)	(0.429)	(0.080)	(0.077)	(0.034)	(0.079)	(1,122.107)	(0.142)
Dense VSLA	0.023	-0.239	0.045	0.103	-0.005	0.099	153.290	0.120
	(0.037)	(0.266)	(0.054)	(0.065)	(0.021)	(0.077)	(1,027.292)	(0.104)
Preselected X Dense	-0.033	-0.219	-0.010	0.070	-0.008	-0.069	379.677	-0.107
	(0.059)	(0.442)	(0.092)	(0.096)	(0.062)	(0.114)	(1,747.960)	(0.194)
Mean outcome var	0.34	6.46	0.58	0.64	0.85	1.66	8,904	0.03
Observations	2,744	2,758	2,695	2,393	2,770	2,484	2,727	2447
R-squared	0.138	0.038	0.192	0.358	0.120	0.160	0.131	0.222
Panel B: Difference by group type								
Dense VSLA	0.025	-0.314	0.072*	0.130**	-0.028	0.155***	-224.828	0.209***
	(0.032)	(0.227)	(0.042)	(0.063)	(0.026)	(0.051)	(768.320)	(0.054)
Observations	2,745	2,759	2,696	2,394	2,771	2,483	2,728	2,456
R-squared	0.132	0.037	0.169	0.355	0.098	0.086	0.121	0.158

Regressions on sample of VSLA participants only (preselected plus self-selected). Sample of preselected interviewed prior to group formation; sample of self-selected interviewed after group formation. See definitions in appendix A.

Tab. 5: Impact of selection on interest rate and share prices

VARIABLES	(1) Share Price (UGX)	(2)	(3) Share Price at least 1000 UGX	(4)	(5) Interest rate	(6)	(7) Interest rate at least 10%	(8)
Dense VSLA	110.0*	38.507	0.175**	0.142*	1.245**	0.281	0.141*	-0.068
	(56.942)	(70.712)	(0.080)	(0.083)	(0.534)	(0.570)	(0.082)	(0.057)
Constant	838.2***	961.493***	0.618***	0.858***	7.824***	9.719***	0.618***	1.068***
	(38.430)	(70.712)	(0.059)	(0.083)	(0.346)	(0.570)	(0.059)	(0.057)
Observations	126	116	126	116	126	116	126	116
R-squared	0.029	0.411	0.036	0.433	0.043	0.673	0.023	0.811
IP f.e.	NO	YES	NO	YES	NO	YES	NO	YES

Table reports coefficients on dense treatment from group level regressions. Dependent variable is column title.
No additional controls included. Heteroskedasticity-robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Share price and interest rates   At the group formation stage, members must decide on the interest rate and on the share price. Qualitative interviews suggest that groups make this decision with very limited information on what would be a reasonable rule, with many groups adjusting the rules in the following cycle.[14] In Table 5 we show that dense groups chose somewhat higher share prices and interest rates; however, differences in rules are quite small and become insignificant once the implementing partner fixed effects are included. A small difference remains in terms of the proportion of groups that chose a share price of 1,000 UGX or above. Note also that in Table 5, the R^2 of all regressions increase dramatically when they include IP fixed effect; which implies that a large fraction of the variation in interest rate and share price can be attributed to the organizations training the group. Our interpretation is that, for the most part, the groups maintained the default set of rules proposed by the IPs.

Aggregate savings and borrowings   Table 6 reports the estimation of coefficient α_g in equation (2), using data from wave I (panel A), wave II (panel B), and at share out (Panel C), where the outcomes of interest are aggregate cumulative savings and borrowings, rate of return on loans, and late repayment of loans. For each outcome variable, we report three separate specifications. Column 1 reports regressions that control for the number of meetings only; column 2 includes IP fixed effects; and column 3 adds rules fixed effects. As noted in section 4, the fact that the intervention was carried out by many different

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[14] 45% of groups increased their share price in the subsequent cycle, which suggests that the share price chosen in year one may have been suboptimal. On the other hand, only 7% of groups adjusted the interest rate.

organizations with different expertise injects significant variance in group performance; our preferred specifications thus controls for their effect. Whether one should include share price and interest rate dummies in the regression is, instead, debatable. On the one hand, the choice of rules may be considered as a channel through which the intervention affects group outcomes. In this case, it is best to exclude rule dummies and allow the dense variable to pick up the overall effect. On the other hand, including the dummies is necessary if one is interested in the effect of the intervention net of these rules. We report both sets of estimates, and note that, in general, estimates with rule dummies are more conservative.

The table shows that, shortly after groups are formed, there are no significant differences in cumulative savings or lending between the two treatment arms (panel A). This is not entirely surprising because the data were collected after, on average, four months of operation, and significant differences are not yet evident.

The situation is markedly different in the second wave of data collection (panel B). Looking at the last two columns of the table, our estimates indicate that dense groups accumulated over 400,000 UGX fewer savings, representing 21% of the 1.9 million UGX saved by the groups on average. They also dispersed over 900,000 UGX less relative to sparse groups. The implied magnitude of this difference is large, being 33% of the 2.7 million UGX average cumulative lending.

Differences in cumulative savings and loans disbursed narrow remarkably by share out (panel C). Point estimates from the last column indicate that dense groups accumulated 240,000 UGX fewer total savings (US$90, or 9% of the 2.6 million UGX average cumulative savings in the sample). They also disbursed approximately 400,000 fewer shillings in loans (US$150, or 8.8% of the 4.5 million cumulative loans). Neither result is statistically significant.

Return on savings and defaults The remaining of panel C looks at other end-of-cycle outcomes. The return on savings earned at share out is similar across the two types of groups. Defaults and late repayments are also statistically indistinguishable across types of groups.[15]

In summary, differences in the composition of sparse and dense groups translated into

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[15] Return on savings is measured as $r\frac{B}{S}$, where r is the interest rate on loans, B is aggregate end-of-cycle borrowing and S is aggregate end-of-cycle savings. Default is measured at the individual member level, as as an indicator for whether the person is late on payments or failed to repay completely by the end of the cycle.

Tab. 6: Impact of group composition on total
group savings and lending amounts

Dep var: row title	(1)	(2)	(3)
Coefficient on dense reported			
Panel A: Wave I			
Cumulative savings	99,485	62,036	73,789
	(111,786)	(108,804)	(116,224)
Cumulative loans	220,178	74,523	89,375
	(220,461)	(200,967)	(218,292)
Number of groups	115	115	115
Panel B: Wave II			
Cumulative savings	85,288	-405,597*	-390,147*
	(203,047)	(229,779)	(220,387)
Cumulative loans	-549,036	-959,215*	-917,091*
	(412,367)	(511,918)	(534,002)
Number of groups	103	103	103
Panel C: End of cycle			
Cumulative savings	30,213	-294,988	-238,482
	(192,548)	(224,198)	(219,116)
Cumulative loans	-313,338	-626,756	-404,539
	(420,064)	(504,731)	(498,552)
Return on savings	-0.466	-1.396	-0.938
	(1.174)	(1.124)	(1.087)
Defaults	0.002	0.007	0.006
	(0.006)	(0.010)	(0.012)
Number of groups	110	110	110
IP f.e.	NO	YES	YES
Rules f.e.	NO	NO	YES

Table reports coefficients on dense treatment from group level
regressions, as in equation (2). Each cell is a separate regression.
Cumulative savings and cumulative loans in UGX, aggregated
from individual savings and loans after trimming top 1% of savings,
loans and loan to savings ratios. Return on savings calculated at share
Default regressions (Panel C) run at the individual (i.e., member
of VSLA) level. Outcome variable is dummy for whether the particip:
failed to repay a loan in its entirety by shareout.
All regressions control for number of meetings at audit wave t.
End of cycle controls for the number of meetings in the cycle. Rules
fixed effects include dummies for the interest rate and the share price.
Heteroskedasticity robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

temporary differences in the ability to generate savings and provide loans to the groups members. In particular, groups whose members are, on average, in a worse socio-economic condition generated less savings and less borrowings for its members during an important part of the cycle. The incentive to save (given by the return on savings and the probability of default of other members) is the same between the two types of groups.

5.3   Individual savings and borrowing behavior

We next turn to the savings and borrowing decisions of study participants. Table 7 reports estimates of equation (3) on individual-level cumulative savings and cumulative loans of preselected participants. Because of random assignment, these estimates are the causal impact of assignment to a dense group on realized savings and borrowing. As in Table 5, we report three separate specifications: one with no controls other than the number of meetings, one including IP fixed effects, and the last one including IP fixed effects and rule dummies.

Panel A reports savings and borrowing as recorded during the first audit wave. Looking at the last two columns of the table, there is no evidence that assignment affects the savings decision of participants. However, there is some evidence (significant at the 10% level) that borrowers do not have as much access to capital in dense groups, because they borrow 14,000 fewer shillings ($5.25, or 23% of average borrowing). This is first evidence that these participants are effectively rationed. By the second audit visit, differences are more evident, both on the saving and borrowing side. Cumulative savings (resp. cumulative borrowing) is 18,000 to 21,000 UGX (resp. 55,000 to 64,000 UGX) lower among those who were assigned to dense groups. These are large differences in cumulative amounts: using the more conservative estimates from column 3, one can see that a reduction in accumulated savings of 18,000 UGX (US$ 6.75) is 22% of mean savings, and a reduction in accumulated loans of 55,000 UGX (US$20.70) is 48% of mean cumulative loans.

Similarly to the aggregate results, the gap in individual savings and borrowing levels narrows significantly by share out. At share out, controlling for the number of meetings and the IP effect (column 2), in dense groups cumulative savings are 15,400 UGX (US$5.80, or 16% of mean cumulative savings) lower and cumulative borrowing is 42,750 UGX (US$16, or 29% of mean borrowing) lower than in sparse groups. When rules are taken into account (column 3), estimates fall by half and become significantly noisier.

Table 8 reports coefficient estimates from regressions (3) with other sets of end-of-cycle outcomes. We report only the set of estimates that include IP fixed effects. Columns 1

Tab. 7: Impact of group composition on savings
and borrowing; Preselected only

Dep var: row title	(1)	(2)	(3)
Coeff on dense reported			
Panel A: Wave I			
Cumulative savings	3,509	-2,484	-2,404
	(5,197)	(4,131)	(4,084)
Observations	930	930	930
Mean outcome	40299	40299	40299
Cumulative loans	-1,913	-14,439*	-13,871*
	(9,606)	(7,496)	(7,255)
Observations	932	932	932
Mean outcome	59183	59183	59183
Panel B: Wave II			
Cumulative savings	6,262	-21,139***	-17,825**
	(8,217)	(7,244)	(8,004)
Observations	810	810	810
Mean outcome	82747	82747	82747
Cumulative loans	-34,508*	-63,799***	-54,708***
	(19,345)	(14,046)	(14,944)
Observations	810	810	810
Mean outcome	115484	115484	115484
Panel C: End of cycle			
Cumulative savings	-1,769	-15,390**	-9,545
	(10,260)	(7,216)	(8,269)
Observations	897	897	897
Mean outcome	96295	96295	96295
Cumulative loans	-20,556	-42,750***	-20,149
	(20,136)	(12,506)	(21,898)
Observations	897	897	897
Mean outcome	147802	147802	147802
IP f.e.	NO	YES	YES
Rules f.e.	NO	NO	YES

Regressions on the sample of preselected participants only. Each
cell reports the coefficient on the assignment to a dense group.
Dependent variables are in UGX. All regressions include number of
meetings as control. Rules fixed effects include interest rate and share
price dummies.
Standard errors clustered at the parish level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

and 2 report the loan-to-savings ratio; the two regressions are suggestive that borrowers in dense groups had more difficulty leveraging their savings. Columns 3 and 4 show that the intervention did not affect the decision to become a borrower, although it may have reduced the number of loans (columns 5 and 6) and not just the average size of those loans (columns 7 and 8). Columns 9 and 10 report rates of individual defaults. As in the aggregate, we find no difference across types of groups in this measure. Finally, the last two columns construct a measure of the money received at share out (i.e. the share-out value),[16] finding that the average share out in dense groups is 17,625 UGX (US\$6.25, or 16.2% of the average value) lower than in sparse groups. In general, we find that most results are sensitive to the inclusion of share price and interest rate, and one can conclude that performance at share out was no worse in dense groups.

Finally, in Table 9 we replicate Table 7 using the sample of self-selected participants. Across all specifications and reporting periods, we see that neither savings nor borrowing levels differ significantly between self-selected members of sparse and dense groups. This result cannot be interpreted causally, because self-selected participants are by definition not subject to random assignment and differential self-selection across unobservables may be present. It is, however, consistent with the idea that the effect of our intervention is stronger on the most vulnerable households, and not relevant for those who are not vulnerable.

Results on preselected are robust to a number of alternative specifications. Results do not change in magnitude or significance when the sample is limited to participants found in all three waves, when the sample includes trimmed data, when we control for baseline characteristics, or when using WLS to rebalance characteristics of preselected in dense and sparse, as discussed earlier. In addition, we explore heterogeneity of impacts at share out in Table 10, where we interact the dense variable with vulnerability measures at baseline. Looking at the interaction term, we see that those households who occasionally skip meals see a larger drop in borrowing if placed in a dense group relative to a sparse. Also, households with disabilities save less when assigned to a dense group. At least along some dimensions of vulnerability, the effect of the intervention was stronger on households who are more vulnerable. Finally, for certain categories of participants, the negative effect of inclusion in a dense rather than a sparse group on their borrowing levels is significant also at share out.

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[16] Our audit data do not contain direct measurement of share out. Our proxy for share out to individual i is constructed using the formula $(1 + r\frac{B}{S})s_i$, where s_i is total end-of-cycle savings for person i, B and S are aggregate borrowing and savings, and r is the interest on loans.

Tab. 8: Other individual outcomes within savings groups; Preselected only

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Loan to savings ratio		Has at least one loan		Number of loans		Average loan size		Has loans past due		Shareout value	
Dense VSLA	-0.215*	-0.132	-0.035	-0.021	-0.295*	-0.093	-15,398**	-8,412	-0.000	-0.000	-17,625**	-9,631
	(0.128)	(0.147)	(0.041)	(0.040)	(0.165)	(0.183)	(6,816)	(7,623)	(0.012)	(0.014)	(7,802)	(9,578)
Interest rate =5%		0.298		0.116***		0.979		-15,912**		0.037*		29,868
		(0.377)		(0.030)		(0.711)		(7,607)		(0.021)		(23,668)
Interest rate =10%		-0.038		0.112**		1.279*		-28,785***		0.014		49,629*
		(0.344)		(0.050)		(0.642)		(10,429)		(0.021)		(27,084)
Interest rate =20%		-0.691		0.068		-0.396		-110,925***		0.025		-41,678
		(0.431)		(0.067)		(1.023)		(17,506)		(0.049)		(29,922)
Share =1,000 UGX		-0.123		-0.002		0.148		5,635		-0.006		27,622***
		(0.146)		(0.042)		(0.546)		(9,072)		(0.006)		(8,312)
Share =2,000 UGX		-0.406		0.193***		0.515		32,090*		-0.021		84,984***
		(0.518)		(0.051)		(0.597)		(18,736)		(0.088)		(21,966)
Observations	896	896	896	896	844	844	754	754	917	917	897	897
R-squared	0.085	0.095	0.135	0.148	0.380	0.399	0.190	0.222	0.119	0.121	0.264	0.314
IP f.e.	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Mean outcome	1.600	1.600	0.890	0.890	2.742	2.742	60848	60848	0.0298	0.0298	108790	108790

Individual regressions on the sample of preselected participants only using data from the last wave of group audit data. Column title is the dependent variable. All regressions estimated using OLS. Loan to savings ratio is cumulative loans divided by cumulative savings for each individual. Has one loan is an indicator variable equal to 1 if a person obtained one loan or more, and zero if the person never borrowed. Number of loans is cumulative number of individual loans. Average loan size is the cumulative loan divided by the number of loans. Has loan past due is an indicator variable equal to one if a person failed to repay all loans in their entirety by shareout. Shareout value is the estimated shareout value--see main text for the equation. All regressions include number of meetings. Standard errors clustered at the parish level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Tab. 9: Impact of group composition on savings
and borrowing; Self-selected only

Dep var: row title Coeff on dense reported	(1)	(2)	(3)
Panel A: Wave I			
Cumulative savings	3,232	-3,221	-1,340
	(5,128)	(4,090)	(3,966)
Observations	2,106	2,106	2,106
Mean outcome	40299	40299	40299
Cumulative loans	10,198	-4,392	-3,373
	(9,817)	(6,951)	(7,410)
Observations	2,109	2,109	2,109
Mean outcome	59183	59183	59183
Panel B: Wave II			
Cumulative savings	7,296	-7,083	-1,957
	(11,002)	(10,228)	(10,076)
Observations	1,924	1,924	1,924
Mean outcome	82747	82747	82747
Cumulative loans	-4,705	-8,397	-3,271
	(22,630)	(24,569)	(26,120)
Observations	1,924	1,924	1,924
Mean outcome	115484	115484	115484
Panel C: End of cycle			
Cumulative savings	4,444	-1,532	1,768
	(10,609)	(8,494)	(8,402)
Observations	2,000	2,000	2,000
Mean outcome	96295	96295	96295
Cumulative loans	3,543	2,969	11,469
	(21,647)	(17,352)	(19,450)
Observations	2,002	2,002	2,002
Mean outcome	115484	115484	115484
IP f.e.	NO	YES	YES
Rules f.e.	NO	NO	YES

Regressions on the sample of self-selected participants only. Each
cell reports the coefficient on the assignment to a dense group.
Dependent variables are in UGX. All regressions include number of
meetings as control. Rules fixed effects include interest rate and share
price dummies.
Standard errors clustered at the parish level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Tab. 10: Heterogeneous treatment effects; Preselected only

Dep var:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Cumulative savings (at shareout)					Cumulative loans (at shareout)				
Interaction variables:	Low income	Bad situation	Disability	Food insecure	Highly vulnerable	Low income	Bad situation	Disability	Food insecure	Highly vulnerable
Dense	-13,652	-9,796	-2,420	-7,697	-11,591	-29,452	-18,706	-14,478	11,181	-15,656
	(8,923)	(8,654)	(8,650)	(10,472)	(8,178)	(26,186)	(23,965)	(25,308)	(22,679)	(23,567)
Low income	-17,946***					-30,805**				
	(5,084)					(14,617)				
Dense X low income	3,544					18,648				
	(7,606)					(21,595)				
Bad situation		4,109					3,255			
		(8,846)					(18,530)			
Dense X bad situation		3,086					-5,754			
		(11,722)					(29,400)			
Disability			12,456					11,785		
			(8,981)					(24,136)		
Dense X disability			-22,295**					-15,999		
			(9,947)					(24,420)		
Food insecure				-1,721					18,744	
				(9,107)					(19,005)	
Dense X food insecure				-2,133					-41,743*	
				(10,853)					(24,842)	
Highly vulnerable					-3,587					8,606
					(6,843)					(15,131)
Dense X highly vulnerable					4,366					-9,510
					(8,899)					(18,235)
Observations	897	895	895	893	897	897	895	895	893	897
R-squared	0.302	0.290	0.296	0.292	0.290	0.221	0.218	0.219	0.223	0.218
F test p-value	0.261	0.557	0.0127	0.294	0.496	0.579	0.319	0.184	0.240	0.306

Regressions on preselected participants only using end of cycle data. Controls include number of meetings in the cycle, IP fixed effec and rule dummies. Interaction variables are dummy variables identifying households with above median characteristic. Median taker from preselected sample only. Standard errors clustered at the parish level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

6   Mechanisms

Our empirical investigation can be summarized into three main points. First, our attempt to generate groups with different vulnerability profiles largely succeeded: members of dense groups are, on average, more vulnerable than members of sparse groups. Second, dense groups accumulated savings and disbursed loans at a slower pace than sparse groups during the initial part of the cycle, but "caught up" by the end of the cycle. Finally, being placed in a more vulnerable group caused randomly assigned vulnerable members to save and borrow less in the middle of the cycle. By some indications, they also partially "caught up" by the end of the cycle. In this section, we provide a discussion of the possible mechanisms at play.

Rationing   One explanation that is consistent with the results presented so far is the presence of differential rationing of funds between dense and sparse groups. More specifically, dense groups are less able than sparse groups to generate sufficient funds to meet the demand for loans of its members. As a consequence, during the central period of operation preselected members of dense groups are rationed out of funds and unable to meet their borrowing needs. Over time, however, both types of groups generate sufficient funds to meet the demand for loans of its members, both because of the addition of new savings and because of the payment of old loans. The difference between groups, as well as the difference in behavior between preselected in dense and sparse, thus fades over time. Note also that, to the extent that a member of a VSLA needs to save before she can borrow, the fact that preselected in dense also save less compared to preselected in sparse may be driven by the fact that they expect to borrow less.

Interestingly, our results suggest that the burden of rationing may be shared unevenly among the groups members. The fact that self-selected members do not seem to react to an increase in loanable funds available suggests that this groups may have been able to meet its demand for loans also within dense groups (in which resources are particularly scarce).[17] On the other hand, preselected members react dramatically to a change in group's composition, implying they are severely rationed out when placed in dense groups. Overall, it seems that, when funds are scarce, groups privilege less vulnerable households in their allocation of funds. As a consequence, rationing affects disproportionately vulnerable members of the VSLA.[18]

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[17] Again, a caveat applies because we do not control the process of self selection.

[18] This is consistent with our direct observation. Informal discussions with the members of our study

Other mechanisms   In addition to rationing, other mechanisms may be at play. For example, the presence of self-selected members may have affected the demand for loans of preselected through changes in the social network, aspirations or learning. Preselected in sparse groups may be more likely than preselected in dense groups to learn from a self-selected member how to run a business, and therefore are more likely to demand a loan to the group. Similarly, group participation may have an impact on the group members social network, and on the business opportunities available to them.

Note, however, that these mechanisms are hard to reconcile with the fact that the difference in borrowing levels between sparse and dense disappears over time. Mechanisms such as changes to the social network, changes in aspirations or learning should operate during the entire period of operation (or beyond, if the group continues to a second cycle) and, if anything, should become stronger the longer groups members interact with each other. Hence, while these mechanisms may be affecting the latent demand for loans, whether this demand can be satisfied by the group depends on the degree of funds scarcity.

A more promising alternative to explain our results is that groups build trust and internal cohesion over time. For example, at the beginning the members of the group may not be sure whether the other members will repay their loans, or whether the people in charge of keeping the safe will not to steal the money. Over time, they learn that their fellow members are trustworthy and start both saving and borrowing more. This mechanism is consistent with our results as long as building trust and group cohesion takes longer in dense groups than sparse groups. However, one issue with this explanation is that all group members belong to the same village, and likely know each other before joining. Furthermore, defaults and late repayment are very rare occurrences in both types of groups, and therefore it is not clear why building trust in a dense group should take longer than in a sparse. Finally, and most importantly, it is not clear why the building of trust and social cohesion should be a determinant of the borrowing and savings behavior of vulnerable households but not of those who are less vulnerable. Again, without denying the importance of trust and social cohesion, we believe that this mechanism, if present, operated in conjunction with rationing of funds.

VSLA reveal that, in case of scarcity of funds, priority is given to individuals based on their cumulative savings with the group.

7 Effect of the intervention on the welfare of vulnerable households

The final task of the study is to explore whether ultra-poor households fare better when they join less vulnerable savings groups. Whether this is the case is likely to depend on the mechanisms discussed above. In a sparse group, vulnerable households are better able to access loans when encountering shocks or investment opportunities. In addition, sparse groups may provide access to better social networks, and may provide more opportunities to be inspired or learn from successful peers. For all these reasons, our preselected participants assigned to sparse groups may report different outcomes–rates of business and household investment, consumption, labor supply– than those assigned to dense groups. We check whether this is the case by estimating equation (3) on endline households' characteristics, and report the results in Appendix B Tables B5 to B9. In each table, panel A reports estimates from the full sample of preselected, while panel B limits the sample to VSLA members only. In general, estimates in panel B are larger and more likely to be significant than those from Table A, consistent with treatment effects operating through participation in VSLAs.

Table B6 studies the effect of the intervention on households' investments. VSLA members enrolled in a dense group invested a lower amount in housing improvements during the previous year (columns 1) compared to members enrolled in a sparse group. On the other hand, the assignment to a dense group has no predictive power over investment in productive assets or activities such as farming (column 2), new microenterprises creation (column 3), or land cultivation (columns 6-8). While productive activities seem unaffected by the intervention, a potential benefit of joining a sparse group over a dense group is protection against unlikely but expensive shocks. In favor of this view, we find that members of dense groups are significantly more likely to report having sold land in the previous year (column 5). Consistent with the view that the intervention did not spur increases in production, Table B7 reports regressions on weekly hours worked in the previous month and finds no differences in work hours or earnings between the two treatment arms. Table B5 studies the effect of the intervention on savings. While participants in vulnerable groups are less likely to save in a formal bank account (column 1), there is very little difference in overall level of monetary savings, animal savings, or durable good ownership across the two treatment arms.

One possible outcome of our intervention is that members of less vulnerable groups

are exposed to "better" social networks. While we do not have direct measures of changed is the social network, our survey included some useful proxies. In particular, we asked the respondents to explain whether members of their households participate in other social groups (Table B8). We find that group assignment did not have a differential effect on the probability of joining farm training programs (known locally as farmers field schools or FFS), women groups, financial groups such as ROSCAs, insurance groups, or other types of social groups. On the other hand, those assigned to dense groups are more likely to be members of other savings groups. This is consistent with the idea that preselected in dense groups are less able than preselected in sparse groups to meet their demand for loans, and respond by joining other VSLAs. Overall, social group participation outside of the study VSLA is not significantly more prevalent in either type of treatment group (column 7).

Finally, while our survey instrument lacked a consumption module, we had a food insecurity module (Table B9). Across the five measures of food insecurity, estimates are negative for participants of dense groups. However, all of these measures are insignificant except for one (having slept hungry in the previous month) being marginally significant.

Overall, the endline results paint a nuanced effect of the intervention. For the most part, being assigned to a dense rather than a sparse group has small but significant negative effects. Relative to preselected in dense groups, preselected in sparse groups were able to make additional investments in their housing structure, and perhaps avoided (relatively rare) dramatic events such as selling land. They are also less likely to participate in other VSLAs. However, there is also some weak indication that sparse participants have somewhat higher rates of food insecurity. We find no evidence that a "better" social group inspires ultra-poor participants to work more, to invest in productive activities, or to participate in other social groups (with the exception of other VSLAs).

It is, however, important to keep in mind two things. First, the endline evaluation was done relatively shortly after groups were formed. It may therefore miss the impact of the intervention if the benefit from being in a less vulnerable group (i.e., less constrained access to loans, better social networks) take some time to fully realize. Second, the estimates presented here are differences between the two treatment arms, and tell us nothing about the benefit of participation into a savings group.

8   Conclusion

Our research shows that the ability of savings groups to provide credit to its members – especially those who are vulnerable – depends on the composition of the group. In particular, during the initial period of operation of these groups, demand for loans is likely to be larger than the availability of funds. Groups accumulate funds over time and are eventually able to meet the demand for loans of its members. We interpret our empirical results as evidence that the speed of funds accumulation (and the duration of rationing) depends on the composition of savings groups. We argue that whenever the proportion of poor, vulnerable members of the group is larger, funds accumulation in the groups is slower, and the group is less able to meet the demand for loans of its members, especially during the central part of the cycle. Furthermore, despite the fact that the two types of groups look identical at the end of the cycle, participation in a dense rather than a sparse group generates small but significant negative effects on households' welfare.

To the extent that group composition affects behavior and welfare mostly though the rationing of funds, our results should be relevant for understanding the functioning of most savings groups, and not only VSLAs. As we discussed in Section 2.1, the most common savings groups operate under a set of rules that are very similar to those of a VSLA, but differ in the way the training is conducted and records are kept. For given group composition, all these savings groups may generate similar incentives to save and borrow, and therefore may be similarly affected by funds scarcity. However, our results may not extend to self-help groups because, as already discussed, these groups never share out.

Our results open several questions and potential concerns. First, our study may suggest that groups composed exclusively of self-selected members are better able to satisfy the demand for loans of their members. If this is the case, however, one concern is that fully self-selected groups may contain few ultra-poor vulnerable members, which would imply a trade off between financial inclusion of ultra-poor households and the well functioning of a savings group. On the brighter side, we also believe that, if such trade off exists, it can be made less stringent by improving the rules of functioning of a savings group. For example, savings groups could reward early savings, and/or auction off scarce funds (similarly to what is done in bidding ROSCAs). Addressing these questions and concerns is left for future work.

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Appendix A: Vulnerability Measures

- Indicators that are specific to households with children:

  child labor: whether the child has been involved in child labor.

  drug abuse: whether the child ever been involved in drug consumption (petroleum sniffing, glue sniffing, etc).

  quality diet: child usually (i.e. at least 3 times a week) eats three categories of food: "energy food" (rich in carbohidrates); "bodybuilding food" (rich in protein), and "protective foods" (fruits and vegetables). Indicator is 0-3 scale, with one point assigned to each category.

  number of daily meals: "How many times does the child have meals in a day?"

  orphaned child in household: whether there is an orphan (maternal, paternal or both) in the household.

- Indicators that are relevant to all households:

  chronic disease: whether the child, any of the parents/guardians has a chronic disease (HIV/AIDS, sickle cells, Epilepsy, etc).

  disability: whether the child, any of the parents/guardians is deaf, blind or has other physical or mental disabilities.

  food insecure: "Are there times when your household goes without meals due to failure to get food?" answer is "yes".

  informal employment: "What is your household's main source of income?" answer is "informal employment or casual labor"

  household unemployment: "What is your household's main source of income?" answer is "unemployed" or "remittances"

  safe source of water: "What is the main source of drinking water for members of your household?" answers are "Piped, bore-hole, harvesting" (coded as safe) and "surface" (coded as not safe).

  access to latrines: "Do you have Latrine facilities?" answers are "yes (private or shared)" and "no".

  income per capita: Self-reported household income divided by the number of household members.

  assessor scale: assessor's general impression of the household. From 0 ("can manage without support") to 10 ("critical situation").

  household size: "How many people live in your household?"

  vulnerability index: Principal component analyses of all variables that are relevant to all households.

  dense VSLA: Assigned to a dense VSLA.

Appendix B: Additional Tables

Tab. B1: Uses of VSLA loans and share out

Loan use	% who used it		Shareout use	% who used it
Panel A: Listed responses				
1 Pay school fees or other educational expenses	44.15	1	Pay school fees or other educational expenses	39.1
2 Investment in existing business	15.01	2	Consumption	17.51
3 Health problem	13.61	3	Buy livestock	16.19
4 Consumption	11.83	4	Investment in existing business	10.79
5 Other temporary difficulties	7.63	5	Health problem	9.69
6 Buy farm input (fertilizer, seeds, etc.)	7.51	6	Buy household durable	9.03
7 Start new business	6.87	7	Repay an old debt	7.38
8 Repay an old debt	4.83	8	Buy farm input (fertilizer, seeds, etc.)	6.39
9 Buy household durable	4.07	9	Other temporary difficulties	5.51
10 Buy livestock	3.31	10	Start new business	4.41
11 Buy stock for resale	2.29	11	Home improvement, repair or construction	4.07
12 Home improvement, repair or construction	2.16	12	Acquire other durables	2.64
13 Marriage, funeral, other ceremony	2.16	13	Buy stock for resale	1.54
14 Buy or rent land	0.76	14	Marriage, funeral, other ceremony	1.1
15 Acquire other durables	0.64	15	Buy or rent land	1.1
16 Unemployment	0.38	16	Gift and loans to family, friends	0.44
17 Gift and loans to family, friends	0.13	17	Unemployment	0.44
18 Other	0.25	18	Other	2.09
Panel B: Aggregated responses				
Any type of investment (excluding housing)	35.75		Any type of investment (excluding housing)	40.42
Any type of shock	21.62		Any type of shock	15.64
Consumption (durable + nondurable)	16.54		Consumption (durable + nondurable)	29.18
Risk sharing (gifts, cerimonies)	2.29		Risk sharing (gifts, cerimonies)	1.54

Listed responses to the endline question "what did you use the shareout/loan for?" Loan refers to the most important loan received in the previous cycle. Respondents could mention more than one use; percentages do not add up to 100%. Sample of preselected found in endline only.

Tab. B2: Determinants of VSLA membership

Dep var: Enrolled in a study VSLA	(1)	(2)	(3)
Dense VSLA	0.0238		0.0285
	(0.0247)		(0.0248)
Child labor		-0.0497	-0.0520
		(0.0322)	(0.0322)
Drug abuse at home		0.0585	0.0547
		(0.0414)	(0.0415)
Chronic disease		0.0891**	0.0877**
		(0.0364)	(0.0363)
Disability in household		0.0220	0.0200
		(0.0338)	(0.0339)
Food insecure		0.0842***	0.0825***
		(0.0317)	(0.0315)
Quality diet		-0.0155	-0.0161
		(0.0160)	(0.0160)
Number of daily meals		-0.0106	-0.0111
		(0.0116)	(0.0116)
Informal employment		0.0612**	0.0650**
		(0.0288)	(0.0289)
Household unemployed		-0.000665	0.000510
		(0.0396)	(0.0394)
Orphaned child in hhld		0.0679**	0.0682**
		(0.0268)	(0.0268)
Access to latrines		-0.0521	-0.0535
		(0.0386)	(0.0385)
Income per capita		-2.86e-07	-1.68e-07
		(3.60e-06)	(3.61e-06)
Assessor scale		-0.0164	-0.0172
		(0.0303)	(0.0303)
Total vulnerability score		0.00103	0.00110
		(0.00201)	(0.00201)
HHMembers		0.0135	0.0133
		(0.00941)	(0.00940)
Vulnerability index		-0.0140	-0.0117
		(0.0427)	(0.0428)
Constant	0.771***	0.614***	0.597***
	(0.0202)	(0.167)	(0.167)
Observations	1,234	1,223	1,223
R-squared	0.001	0.044	0.045

Notes: Regression is linear probability model. Sample of preselected only. Heteroskedasticity-robust standard errors in parenthesis.
Results are robust to probit specification or inclusion of IP fixed effects.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Tab. B3: Endline attrition; Preselected only

Dep var: Household completed endline questionnaire	(1)	(2)	(3)
Assigned to Dense VSLA	-0.0196		-0.0318
	(0.0461)		(0.0437)
Child labor		-0.00960	-0.00703
		(0.0357)	(0.0363)
Drug abuse at home		0.0296	0.0342
		(0.0732)	(0.0721)
Chronic disease		0.0950**	0.0969**
		(0.0415)	(0.0413)
Child with disability		0.0240	0.0253
		(0.0347)	(0.0346)
Food insecure		0.0713	0.0731
		(0.0460)	(0.0456)
Quality diet		0.0101	0.0107
		(0.0238)	(0.0238)
Number of daily meals		0.0229	0.0236
		(0.0228)	(0.0226)
Informal employment		-0.00564	-0.0102
		(0.0361)	(0.0368)
Household unemployed		0.0482	0.0469
		(0.0568)	(0.0570)
Orphaned child in hhld		0.106***	0.106***
		(0.0291)	(0.0289)
Disabled child guardian		0.0381	0.0396
		(0.0361)	(0.0360)
Access to latrines		0.116**	0.116**
		(0.0482)	(0.0478)
Enumerator assessment: good/fair situation		0.000905	0.00268
		(0.0375)	(0.0383)
Total vulnerability score		-0.00428	-0.00437
		(0.00290)	(0.00289)
Monthly income		-4.85e-07	-4.79e-07
		(3.53e-07)	(3.48e-07)
Constant	0.783***	0.767***	0.789***
	(0.0346)	(0.192)	(0.193)
Observations	1,277	1,268	1,268
R-squared	0.000	0.044	0.045

Table reports outcomes from a linear probability model. Data on preselected only. Errors clustered at the parish level in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1

Tab. B4: Financial access in study villages after one year

VARIABLES	(1) Parish Population	(2) Number of SACCOS in parish	(3) SACCOs per 1000 people in parish	(4) Number of VSLAs in village	(5) Number of Non-SCORE VSLAs in village	(6) Number of SCORE VSLAs in village
Dense village	-991.8	0.0658	0.0168	1.196	1.594	-0.398
	(664.1)	(0.114)	(0.0434)	(1.781)	(1.785)	(0.249)
Constant	4,681***	0.250***	0.0750**	4.594***	2.406***	2.188***
	(525.9)	(0.0774)	(0.0302)	(0.419)	(0.399)	(0.187)
Observations	89	89	89	89	89	89
R-squared	0.025	0.003	0.001	0.003	0.005	0.026

Unit of observation in all regressions is the village. Parishes are the smallest administrative unit and are
composed of multiple villages. Savings groups are counted within the village while other
financial institutions like SACCOs are counted within the parish. SACCOs are local credit unions.
Linerar regression results shown; results are robust to Poisson estimation.
Heteroskedasticity-robust standard errors in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1

Tab. B5: Household savings at endline; Preselected only

VARIABLES	(1) Bank account	(2) Mobile money account	(3) Other SBG	(4) Other type of group	(5) At home	(6) Value of total savings (UGX)	(7) Livestock index	(8) Asset index
Panel A: All preselected								
dense	-0.0574*	-0.0134	0.0350	0.0271	-0.0357	-5,633	0.186	0.270
	(0.0317)	(0.0423)	(0.0258)	(0.0427)	(0.0469)	(10,606)	(0.124)	(0.222)
Observations	962	958	948	948	904	836	976	976
R-squared	0.182	0.130	0.221	0.276	0.264	0.075	0.183	0.184
Mean of outcome in sparse	0.138	0.181	0.108	0.208	0.319	27935	-0.0813	-0.0893
Panel B: Preselected in savings groups only								
dense	-0.0717*	-0.0208	0.0106	0.0498	-0.0231	-10,661	0.237	0.202
	(0.0377)	(0.0438)	(0.0218)	(0.0537)	(0.0566)	(12,759)	(0.144)	(0.253)
Observations	774	773	763	763	722	684	788	788
R-squared	0.191	0.154	0.228	0.325	0.234	0.086	0.198	0.214
Individual controls	YES	YES	YES	YES	YES	YES	YES	YES
IP fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Mean of outcome in sparse	0.142	0.169	0.116	0.181	0.289	28403	-0.0708	-0.103

Weighted least squares regression with attrition weights on preselected only. Each regression includes month of interview dummy.
Household controls are vulnerability controls. Columns 1-5 are indicator variables for whether the household made a savings deposit in the
account in the previous year. Savings amounts (column 6) computed from reported savings in bank accounts, mobile money
accounts, savings group accounts, savings at home, and other sources. Livestock index is the first factor of a principal component
analysis of 9 livestock variables. Asset index is the first factor of a principal component analysis of 24 household items owned by
the household. All regressions with IP fixed effects. Errors are clustered at the parish level.
*** p<0.01, ** p<0.05, * p<0.1

Tab. B6: Household investments at endline; Preselected only

VARIABLES	(1) investments in housing (UGX)	(2) investments in farming (UGX)	(3) Started new enterprise	(4) Purchased land	(5) Sold land	(6) Land cultivation last season	(7) Land cultivation this season	(8) Land cultivation change
Panel A: All preselected								
dense	-10,252	3,173	0.00279	-0.00486	0.0340**	0.0264	-0.00745	-0.0203
	(8,688)	(1,982)	(0.0274)	(0.0165)	(0.0142)	(0.131)	(0.152)	(0.0918)
Observations	965	976	810	973	973	971	973	969
R-squared	0.058	0.090	0.158	0.061	0.082	0.259	0.303	0.131
Mean of outcome in sparse	19585	1600	0.131	0.0346	0.0131	1.189	1.205	0.0156
Panel B: Preselected in savings groups only								
dense	-27,539**	3,307	0.00141	-0.00640	0.0437**	0.0322	-0.0991	-0.118
	(11,038)	(2,259)	(0.0312)	(0.0156)	(0.0184)	(0.157)	(0.145)	(0.0835)
Observations	780	788	640	786	787	785	786	783
R-squared	0.073	0.106	0.159	0.072	0.112	0.283	0.325	0.145
Individual controls	YES	YES	YES	YES	YES	YES	YES	YES
IP fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Mean of outcome in sparse	24567	1582	0.131	0.0243	0.0127	1.223	1.250	0.0275

Weighted least squares regression with attrition weights on preselected only. Each regression includes month of interview dummy. Household controls are vulnerability controls. Investment in housing is the total amount invested in housing materials over the value of 25,000 UGX in the previous 12 months. Farming investments include any expenditure on chemical fertilizer, seeds, pesticides, or herbicides in the previous six months. Purchased and sold land are indicators for whether the household purchased or sold land in the previous year. Started new enterprise is an indicator for whether the household started a new business in the previous year. Land cultivation is measured in acres. All regressions with IP fixed effects. Errors clustered at the parish level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Tab. B7: Household labor supply and income at endline; Preselected only

VARIABLES	(1) Indicator: Household works	(2) Log hours (Respondent)	(3) Log hours (Spouse)	(4) Log hours (Household)	(5) Per capita income
Panel A: All preselected					
dense	-0.00942	0.00554	-0.123	-0.00268	-1,279
	(0.0356)	(0.0768)	(0.149)	(0.0919)	(2,452)
Observations	851	625	394	673	639
R-squared	0.235	0.242	0.350	0.259	0.178
Mean of outcome in sparse	0.859	33.87	39.10	57.84	15032
Panel B: Preselected in savings groups only					
dense	0.0102	-0.0235	-0.0428	-0.0732	1,288
	(0.0441)	(0.0925)	(0.179)	(0.109)	(3,087)
Observations	665	473	309	512	484
R-squared	0.257	0.271	0.383	0.280	0.184
Individual controls	YES	YES	YES	YES	YES
IP fixed effects	YES	YES	YES	YES	YES
Mean of outcome in sparse	0.848	34.34	38.80	59.22	12913

Weighted least squares regression with attrition weights on preselected only. Each regression includes
month of interview dummy. Household controls are vulnerability controls, plus age age squared and
education level of respondent. Log hours and household income are as reported by main respondent.
All regressions with IP fixed effects. Errors clusteres at the parish level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Tab. B8: Household participation in external social groups

VARIABLES	(1) Score FFS	(2) non-Score FFS	(3) Women group	(4) Financial group	(5) VSLA nonscore	(6) Other group	(7) Any group
Panel A: All preselected							
dense	0.00816	-0.00140	0.000900	0.0492	0.0988***	0.0247	0.0999*
	(0.0339)	(0.0459)	(0.0349)	(0.0367)	(0.0343)	(0.0192)	(0.0574)
Observations	976	976	976	976	976	976	976
R-squared	0.127	0.114	0.164	0.346	0.199	0.125	0.209
Mean of outcome in sparse	0.0887	0.107	0.0907	0.188	0.100	0.0293	0.530
Panel B: Preselected in savings groups only							
dense	-0.0100	0.0143	-0.0505	0.0424	0.0990***	0.0204	0.0490
	(0.0406)	(0.0486)	(0.0441)	(0.0413)	(0.0285)	(0.0237)	(0.0631)
Observations	788	788	788	788	788	788	788
R-squared	0.137	0.129	0.205	0.395	0.227	0.127	0.221
Individual controls	YES	YES	YES	YES	YES	YES	YES
IP fixed effects	YES	YES	YES	YES	YES	YES	YES
Mean of outcome in sparse	0.0922	0.106	0.104	0.194	0.0922	0.0348	0.549

Weighted least squares regression with attrition weights on preselected only. Each regression includes month of interview dummy. Household controls are vulnerability variables. Dependent variables are indicators for whether a member of the household participantes in a social group. All regressions with IP fixed effects. Errors clustered at the parish level in parenthesis.

Tab. B9: Household experience with food insecurity

VARIABLES	(1) Worried enough food	(2) Consumed limited variety	(3) Skipped meals	(4) Slept hungry	(5) Didn't eat for whole day
Panel A: All preselected					
dense	-0.0820	-0.0686	-0.0178	-0.0700	-0.00464
	(0.0558)	(0.0501)	(0.0470)	(0.0571)	(0.0508)
Observations	969	961	960	960	954
R-squared	0.197	0.191	0.164	0.187	0.166
Mean of outcome in sparse	0.704	0.700	0.622	0.316	0.186
Panel B: Preselected in savings groups only					
dense	-0.101	-0.0891	-0.0300	-0.110*	-0.0137
	(0.0609)	(0.0563)	(0.0558)	(0.0643)	(0.0550)
Observations	782	777	773	772	766
R-squared	0.215	0.215	0.179	0.205	0.181
Individual controls	YES	YES	YES	YES	YES
IP fixed effects	YES	YES	YES	YES	YES
Mean of outcome in sparse	0.738	0.734	0.627	0.359	0.207

Weighted least squares regression with attrition weights on preselected only. Each regression includes month of interview dummy. Household controls are vulnerability variables. Dependent variables are indicators for whether members of the household experienced a food insecurity problem the preceding month. All regressions with IP fixed effects. Errors clustered at the parish level in parenthesis.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$