

Can Community-Based Microfinance Groups  
Match Savers with Borrowers?  
Evidence from Rural Malawi

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# Can community-based microfinance groups match savers with borrowers? Evidence from rural Malawi \*

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## **Abstract**

Village Savings and Loan Associations and Self-Help Groups have the features of both a borrowing and a savings technology. They may therefore bring together savers and borrowers, increasing financial intermediation in villages with low access to formal banking. On the other hand, such intermediation may be impeded by limited liability, or by broader social frictions. To test for evidence of intermediation, we analyse how 3,800 members sort across 150 groups in rural Malawi. The results show that there is positive assortative matching on occupation, implying unrealised intermediation possibilities between farming and non-agricultural households. In contrast, there is negative assortative matching on a measure of present-bias. Such sorting suggests that commitment savers may be gaining a commitment technology by lending to time-consistent borrowers. This may be welfare-enhancing for both present-biased individuals and credit-constrained individuals.

# 1 Introduction

Group-based informal financial institutions are common in developing countries, offering access to credit, insurance, and saving in contexts where formal institutions are lacking. Rotating savings and credit associations (ROSCAs) in particular are a widespread means of generating financial intermediation, creating both a source of credit (Besley et al., 1993) and a commitment savings technology (Anderson and Baland, 2002; Ambec and Treich, 2007; Basu, 2011). Joint-liability microfinance has also been used as a way to harness the group structure in order to expand formal lending on a large scale (Besley and Coate, 1995; Ghatak and Guinnane, 1999). Furthermore, some clients may also use microfinance loans as a commitment savings device (Bauer et al., 2012).

More recently, researchers and practitioners have focused considerable attention on community-based microfinance groups — often known as Village Savings and Loan Associations (VSLAs), or Self-Help Groups (SHGs).<sup>1</sup> Such groups encourage members to save and to distribute loans among their members out of the group’s accumulated savings. They therefore act as a source of microfinance, but do not rely on external lines of credit. These groups have been heavily promoted by NGOs, particularly in Sub-Saharan Africa and India, and have over 100 million members worldwide (Greaney et al., 2016).<sup>2</sup> Impact evaluations have shown positive effects of access to VSLAs on household food security (Ksoll et al., 2016; Beaman et al., 2014).<sup>3</sup> Researchers have also considered how the design of such groups might screen credit risk (Greaney et al., 2016), and how there may

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<sup>1</sup>Other names for such groups include VSLs, SBGs, SILCs, and SfCs depending on the NGO responsible.

<sup>2</sup>Such groups typically do not form without NGO involvement for several reasons. First, the NGO can help overcome any inherent coordination problems by bringing people together and highlighting the benefits of financial services and intermediation. Secondly, the system of governance and accounting used to protect members against theft, default or improper conduct by other members is quite sophisticated, and so NGO-provided training is key. Thirdly, the NGO typically provides access to resources such as account books and safe boxes, although the group is usually asked to purchase these out of its own funds. Section 2.1 describes the design of such groups in more detail.

<sup>3</sup>Ksoll et al. (2016) suggest that this may be linked to increased agricultural investments. Similar to evaluations of formal microfinance, neither of these studies finds significant effects of VSLAs on business profits, health, education or female empowerment. However, both sets of authors note that this may be an artefact of short evaluation time-frames, as most of those who joined Self-Help Groups had completed at most one savings cycle by the time of the endline surveys.

be a trade-off between encouraging poorer individuals to join and reducing groups' capacity to lend (Burlando et al., 2017). However, very little is known about how members actually sort across groups in equilibrium, and consequently the extent to which such groups may generate financial intermediation between savers and borrowers.

This paper therefore asks whether VSLAs bring together individuals who seek to save with individuals who may benefit from access to credit. In particular, we first examine whether those engaged in agriculture sort into groups with those engaged in non-farm activities, i.e. negative assortative matching on occupation. This could allow farmers to smooth income from the harvest over the year, whilst enabling those engaged in small business to take short-term loans for investment. Second, we also highlight how VSLAs can be seen as a multi-faceted commitment savings technology. In light of this, we ask whether present-biased individuals who have a demand for commitment sort into groups with time-consistent individuals, who may provide commitment in exchange for access to low-interest borrowing. This would again lead to negative assortative matching, this time on measures of present-bias. Both of these scenarios would imply that the groups help to reallocate capital away from those with a demand for saving and towards those with a demand for credit, enhancing efficiency. Moreover, in each case the welfare of both savers and borrowers can potentially be improved, if a fair interest rate on lending can be agreed upon.<sup>4</sup>

Yet despite the potential benefits, it may be that such sorting does not materialise. For example, there may be informational or enforcement frictions associated with interacting with members of another occupation. Alternatively, individuals' tastes or social norms may encourage grouping along the lines of characteristics like age and wealth rather than saving and borrowing needs. The extent to which sorting is actually achieved is therefore a key question in assessing the efficacy of such groups. It can also help inform the future design of such groups, for example whom NGOs should target when recruiting members, and whether and how they might chose to assign members across groups.

To test whether VSLAs appear to achieve financial intermediation, we provide an empir-

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<sup>4</sup>See section 7 for discussion of what might constitute a "fair" interest rate in this context.

ical analysis of sorting using data from 150 VSLAs in Malawi. In the Summer of 2013, we revisited all VSLA groups that were originally trained as part of an intervention in 2009-11.<sup>5</sup> We enumerated a census of all members, past and present, and elicited basic demographic and membership information for each member. Given that we collected this information two to four years after the groups were initially trained, our data is uniquely suited to studying the long-run equilibrium sorting of members across groups, and the long-term functioning of the groups more generally. In particular, individuals have had time to learn about the savings and borrowing technologies provided by VSLAs and the benefits of grouping with different members. Accordingly, they have had chance to join VSLA groups, switch across groups at the end of savings cycles, and indeed drop out of groups altogether.<sup>6</sup>

We employ a dyadic regression framework to test the predictions about sorting on occupation and present-bias. Specifically, we construct all pairs of members from the census. We then estimate multivariate logit equations to examine the determinants of any two members being members of the *same* group, given that by virtue of being in our dataset, both are both members of *some* group. If the fact that two members are different on a given characteristic is positively correlated with them being members of the same group, then this suggests that there is negative assortative matching on that characteristic, and vice versa for positive assortative matching. In light of the predictions, we test for evidence of negative assortative matching on occupation. We are also able to match a subset of members to a panel dataset containing measures of time preferences. For these individuals, we also test for evidence of negative assortative matching on present-bias.

Our results show a mixed picture in terms of how much financial intermediation is being realised through VSLAs. In stark contrast to the prediction of negative assortative matching on occupation, we in fact observe positive assortative matching. Within a village, if two members have the same primary occupation, this increases the probability that they are members of the same group by over half of the average probability of being in the same group. This lack of negative assorting on occupation suggests that there are

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<sup>5</sup>This is the intervention evaluated in [Ksoll et al. \(2016\)](#). See section 3.1 for details.

<sup>6</sup>We observe a significant number of late joiners, drop-outs and individuals who switch across groups or become members of more than one group; see section 3.

unrealised gains from financial intermediation between individuals from different occupations. This may be because individuals find that the informational and enforcement advantages of grouping with people from the same occupation outweigh the financial intermediation benefits of matching with individuals who have a demand for borrowing and saving at different times.

On the other hand, in line with the prediction for efficient matching on time preferences, there is strong evidence of negative assortative matching on present-bias. Again, the magnitude of this effect is large: if one individual is present-biased and the other is *not* present-biased, then this again increases the probability that they are members of the same group by over half of the average probability. This negative assortative matching suggests that the VSLA groups do at least create a degree of financial intermediation between commitment savers and borrowers. This may be welfare-enhancing — both for commitment savers, given the low access to commitment savings technologies in these communities, and for borrowers, given the low access to credit. Nevertheless, NGOs and policymakers might be concerned if present-biased individuals’ willingness to pay for commitment leads them to accept a very low interest rate when lending out their savings to borrowers. We offer evidence that this does not appear to be the case, as interest rates on lending are in line with the “fair” benchmark of members’ average long-run monthly discount rates.

## 1.1 Contribution to existing literature

The two papers most closely related to ours are [Greaney et al. \(2016\)](#) and [Burlando et al. \(2017\)](#), both of whom consider the design of saving and borrowing groups and the composition of their members. [Greaney et al. \(2016\)](#) run a field experiment in Kenya, Tanzania and Uganda to compare the performance of groups who pay for their own training to groups whose training is NGO-funded. They find that groups who pay for their own training do better across a number of group-level indicators, such as loan repayment rates and average member business profits. They model borrowing motives for joining Self-Help Groups, and argue that making groups pay for their own training

reduces adverse selection by making high-risk individuals select out of membership. On the other hand, this may also drive out the most needy.

Burlando et al. (2017) randomise the proportion of ultra-poor members assigned across groups in Uganda. They find that groups who are assigned a high proportion of ultra-poor members exhibit significantly lower total savings and borrowing midway through the cycle, although these differences disappear by the end of the cycle. This is suggestive of a trade-off between including ultra-poor members and increasing credit constraints at the group level, because ultra-poor members contribute fewer savings to the group’s fund. Moreover, the ultra-poor members are the ones who appear to suffer from the increase in credit constraints when a group is assigned a higher proportion of ultra-poor members.

Our analysis complements these papers in two ways. First, we consider the related but separate question of how, conditional on participating, members sort across groups in equilibrium. As outlined above, sorting may be crucial in determining how well these groups are able meet the financial needs of their members. Second, we also highlight the nature of these groups as offering a commitment savings technology. As we argue, the fact that many individuals likely join such groups out of a demand for commitment may have important implications in terms of interest rates, lending dynamics and ultimately welfare.

Our motivation is conceptually similar to a body of work which examines sorting in informal financial institutions more broadly: Ghatak (2000) and Ahlin (2016) develop theoretical models of how sorting might enable the efficient pricing of risk in the context of joint-liability microfinance; Banerjee et al. (1994) study a similar problem in the design of credit cooperatives; and Eeckhout and Munshi (2010) examine empirical patterns of sorting into chit funds in India.<sup>7</sup> Our contribution differs insofar as we examine sorting in groups when some members are primarily interested in saving, although others remain primarily interested in borrowing.<sup>8</sup>

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<sup>7</sup>“Chit fund” is the term used in India to denote a bidding ROSCA.

<sup>8</sup>As mentioned earlier, ROSCAs and chit funds also potentially offer a commitment savings technology. However, Eeckhout and Munshi (2010) model all members as joining with the sole motivation of getting an early turn in the rotation and thus obtaining a *de facto* loan.



Our empirical strategy builds on work by [Arcand and Fafchamps \(2012\)](#) who use a dyadic regression framework to study sorting and inclusiveness in community-based organisations. Other authors have used dyadic analysis to examine sorting on risk preferences for risk-sharing games ([Attanasio et al., 2012](#); [Barr et al., 2012](#)). Our approach adds to this literature in that we study sorting on time preferences. We also do so in the context of a fully-fledged programme setting rather than a framed field experiment.

The remainder of the paper is organised as follows. [Section 2](#) explains the design of VSLAs in more detail, and makes predictions about the sorting of members. [Section 3](#) describes the survey and key aspects of the data. [Section 4](#) outlines the empirical strategy to test the predictions, and to explore sorting in VSLAs more broadly. [Section 5](#) details the results and robustness checks, and discusses the potential welfare implications. [Section 7](#) concludes.

## 2 Conceptual framework

### 2.1 Design of VSLAs

The design of community-based microfinance groups and the procedures used to train members are highly similar across NGOs and countries. Our data come from VSLAs in Malawi.<sup>9</sup> In the programme we study, the NGO first holds an initial information meeting in each community targeted. After this, individuals are invited to self-select into groups of 15-25 members in order to participate. With the help of the NGO, each group then purchases a cash box with three separate locks and elects three different members to act as key-holders, thereby reducing the probability that any funds placed into the box will be subject to theft. The NGO or an NGO-trained field agent then assists the group in writing a constitution, and trains the group in financial literacy and account-keeping over a period of several weeks. The group then begins to hold weekly meetings, at which

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<sup>9</sup>VSLAs were the first version of “Self-Help Groups” to be introduced to Sub-Saharan Africa, by CARE International in Niger in 1991. See <http://www.vsla.net>.

each group member must make savings by purchasing between one and five “shares” of a fixed, small value. After a month, members can also start requesting to take loans, to be repaid after a month at a fixed monthly rate of interest. At the end of each cycle — usually a year — the group’s total remaining savings fund plus the successfully-recovered loans and the loan interest is “shared out” in proportion to individual members’ savings (hence the term “shares”).<sup>10</sup>

Comparing their structure to that of other savings and credit institutions, VSLAs lie somewhere between credit cooperatives and ROSCAs. VSLAs bear a strong resemblance to credit cooperatives and credit unions, but are generally much smaller and much less formalised — for example VSLAs typically have no legal status, unlike some of the larger rural credit unions. As a consequence, VSLAs are likely to be more reliant on interpersonal relationships for monitoring and enforcement of loan repayment (which may have both advantages and disadvantages). Insofar as VSLAs are informal savings and credit groups which rely on “social capital” to sustain them, they are close in spirit to ROSCAs. However, VSLAs are more sophisticated than ROSCAs in that they allow for a much greater degree of flexibility. On the savings side, each member can choose to buy between one and five shares each week, rather than committing to a fixed payment which is the same for all members and all weeks. On the lending side, members can demand the size and timing of the loans they receive, rather than having to wait for their turn in a rotation.<sup>11</sup> Meanwhile, compared to joint-liability microfinance, VSLAs are distinct insofar as they offer a pure savings technology as well as credit.<sup>12</sup>

VSLAs can be seen as a multi-faceted commitment savings technology. First, there is a deposit commitment, since all members are required to purchase at least one share per

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<sup>10</sup>In principle, each member is also required to make a small weekly contribution to the group’s insurance fund, to cover events such as illness or death of a group member’s relatives. However, in practice we found that most groups set the level of such contributions to be very small (around 20 Malawi Kwacha (MK), equivalent to \$0.06) and some groups had even dropped this component altogether, citing past disagreements about payouts.

<sup>11</sup>Bidding ROSCAs do allow members some choice over when they receive the pot; however, each member can still only receive the pot once and cannot choose the pot size.

<sup>12</sup>VSLA interventions are also typically targeted at communities which are under-served even by microfinance lenders.

week.<sup>13</sup> Second, VSLAs enforce a withdrawal commitment, since savings made into the box (i.e. all shares purchased) cannot be liquidated until the end of the savings cycle.<sup>14</sup> Third, the group meetings likely provide other “soft” deposit commitment technologies: active peer pressure to save; self-imposed pressure to save in order to appear reliable to one’s peers (Breza and Chandrasekhar, 2015); and reminders to save (Kast et al., 2012). Finally, any member requesting a loan must prove that it is for a good purpose, and the whole group must agree for the loan to be granted. Thus it is unlikely that members can undo the commitment savings features of the group by borrowing for consumption, except in the case of demonstrable emergencies. VSLAs may therefore offer an attractive package to individuals who have problems with self-control or with shielding resources from their spouse or relatives, and who are sophisticated enough to recognise the potential value of commitment.<sup>15</sup>

## 2.2 Sorting across VSLAs

### 2.2.1 Sorting on occupation

The most salient characteristic on which we would expect to see sorting for financial intermediation purposes is occupation. The population of the survey area is overwhelmingly engaged in one of two occupations: agriculture, and small business.<sup>16</sup> These occupations have very different cash-flow needs. Farming households have a large outflow of funds for investment during the planting season in January, and receive one major inflow of

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<sup>13</sup>In practice we see that some groups occasionally relax this requirement, allowing individuals to purchase zero shares in certain weeks. It is likely that the groups somehow strike a balance between commitment and liquidity to cover shocks, by relaxing the deposit commitment but keeping the withdrawal commitment strong.

<sup>14</sup>There is a provision that individuals can withdraw a small number of their own shares in the case of a medical emergency or similar. However, in practice we rarely see instances of this when examining the individual account books.

<sup>15</sup>Anecdotally, members do seem sophisticated in this way: when we asked about reasons for joining, many individuals told us that being in a VSLA “overcomes the temptation of spending savings kept at home”, or “addresses the problems encountered within the household”.

<sup>16</sup>Typical businesses in the area include selling vegetables and goods from nearby markets, bricklaying and carpentry, driving bicycle taxis, and sewing.

funds just after the maize and rice harvest in April.<sup>17</sup> Meanwhile households engaged in business and other non-farm activities typically have frequent inflows and outflows of income, and may require access to capital throughout the year. In terms of sorting, broadly three scenarios are therefore possible:

**1. Efficient sorting:** Given the cash-flow patterns described above, efficiency would imply *negative* assortative matching of VSLA members on occupation — i.e. those in agriculture sorting into groups with those engaged in small business. If this were to occur, the farmers would expect to act as savers for most of the cycle, except for occasional out-of-season agricultural investments and emergencies. Meanwhile those in non-farm activities would borrow out of the farmers' savings, thereby generating dividends for the farmers through loan interest repayments. Such sorting might be achieved through direct bargaining, or through the loan interest rate: competition for borrowing funds amongst non-farmers within a given VSLA would drive up the interest rate on lending, eventually pushing some non-farmers to move to other VSLA groups with relatively few non-farmers.

**2. Sorting with social constraints:** On the other hand, the fact that most VSLA lending is done on a limited-liability basis may prevent this efficient sorting from occurring.<sup>18</sup> If members' ability to monitor and punish delinquent borrowers is much stronger when the borrower has the same occupation, then this may overwhelm the benefits of negative assorting and instead lead to *positive* assortative matching on occupation. A similar argument would apply if transaction costs are much lower among those with the same occupation, or if individuals derive much greater social benefit from interacting in a VSLA with others in the same occupation.

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<sup>17</sup>There is also a second, smaller harvest for cassava which takes place in November. This is mainly used to supplement household consumption during the lean season of December-February.

<sup>18</sup>The member's own shares can be seized as collateral in the case of non-repayment, but these may not be enough to cover the value of the loan.

**3. Sorting with no credit:** Finally, it may be that VSLAs are unable to engage in providing much access to investment credit altogether. This may occur if even the most tightly-knit groups are unable to enforce high rates of loan repayment, or if savers are so risk-averse that they are unwilling to lend out their savings. In this case, members would join VSLAs almost exclusively in order to use them as a savings technology. We would therefore expect to see *zero* assortative matching on occupation, or *positive* assortative matching if there are social benefits as described above.

### 2.2.2 Sorting on time preferences

As noted above, VSLAs also offer a commitment savings technology, which may attract present-biased individuals even if they do not intend to borrow.<sup>19</sup> If so, again broadly three scenarios are possible.

**1. Efficient sorting:** The first scenario is if present-biased individuals who join as a way to gain a commitment savings technology sort into groups with borrowers. This would lead us to observe *negative* assortative matching on present-bias, since these borrowers will be mainly be time-consistent. The latter holds because the majority of members are time-consistent (see section 3). Moreover, time-consistent members are less likely to have a demand for commitment, and so are more likely to have joined for borrowing motives.<sup>20</sup> Further, even if some present-biased individuals also seek to borrow, groups may learn that these present-biased individuals are less reliable for repayment.

This scenario is efficient from a financial intermediation perspective, since it allows commitment savers' capital to act as a source of credit to borrowers. Again, the sorting may

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<sup>19</sup>We focus on self-control problems – i.e. present-bias – as a driver of demand for commitment savings. However, in our empirical analysis we also perform analogous tests of sorting along measures of “other-control” problems, i.e. household bargaining.

<sup>20</sup>Of course, time-consistent individuals may join for “other-control” motives, or indeed as a way to undertake precautionary savings. It is also theoretically possible that they might engage in speculative behaviour, if they seek to “pile in” towards the end of the savings cycle in an attempt to suck out any profits from lending up until that point. However, the scope for such behaviour is limited by the rule of a maximum share purchase of five shares per week.

occur directly, if members of a village have a good idea about one another’s preferences. Alternatively, it may occur via the interest rate: commitment savers by definition have a weakly positive willingness to pay for commitment, and thus are willing to accept a weakly lower interest rate (compared to regular savers or prospective borrowers) when lending out their savings deposits. Thus prospective borrowers who seek out the lowest loan interest rate should end up sorting into groups with commitment savers. Furthermore, there may be credit rationing even at the equilibrium loan interest rate (Burlando et al., 2017). Thus even at the equilibrium loan interest rate, competition for funds may further drive prospective borrowers into groups with commitment savers rather than borrowers.

**2. Sorting with social constraints:** On the other hand, it may be that social norms dictate sorting into groups on the basis of characteristics such as social status or family ties, and that these norms are so strong as to overwhelm any sorting along savings and borrowing characteristics. In this case we would observe *zero* assortative matching on present-bias, conditional on any characteristics determining sorting.

**3. Sorting with no borrowing:** A third possible scenario arises if the credit function of VSLAs becomes negligent, for example due to lack of repayment enforceability. If so, both present-biased and and time-consistent individuals may still join VSLAs, but those time-consistent individuals who join must also do so for savings motives.<sup>21</sup> In this scenario, time-consistent savers will plausibly be more desirable as fellow group members than present-biased savers: time-consistent savers are likely to save more reliably, and this may produce a positive peer effect on other fellow group members’ savings. This would imply a “vertical” preference for time-consistent partners — everybody would prefer time-consistent partners over present-biased partners. As a result we would expect to see *positive* assortative matching on present-bias: time-consistent savers would group

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<sup>21</sup>A time-consistent individual may still save in a VSLA at zero nominal interest if saving at home carries a negative interest rate, for example due to risk of theft. Alternatively, it may be that only commitment savers join, analogous to anti-addiction or weight-loss groups. We would therefore observe *zero* assortative matching on a binary indicator of individual members’ present-bias, because all members would be present-biased.

together and present-biased savers would group together.<sup>22</sup>

## 3 Survey and Data

### 3.1 Description of VSLA survey

In the Summer of 2013 we surveyed 150 VSLA groups in Karonga District, northern Malawi (see figure 1). These VSLAs were originally formed as part of a cluster-randomised controlled trial, which ran from 2009 to 2011. Forty-six villages were included in the initial study, half of which were invited to form groups and began to receive VSLA training in late 2009-early 2010 (treated villages), and half of which only received training in late 2011 (control villages). The intervention was implemented by the Rockwool Foundation and CCAP Synod of Livingstonia Development Department (SOLDEV), and the results of the impact evaluation are detailed in [Ksoll et al. \(2016\)](#).<sup>23</sup> Since we visited the area two years after the control villages were phased into treatment, our sample covers all VSLA groups that were eventually trained by SOLDEV in both treatment and control villages, and we do not exploit the initial randomisation.<sup>24</sup> Two very remote control villages dropped out of the programme in 2011 and never established any groups, hence our 2013 sample covers the remaining 44 villages.

The survey protocol was as follows. We contacted each group via the NGO, who invited

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<sup>22</sup>Side payments could theoretically complicate the sorting process, as commitment savers might be willing to make larger transfers to time-consistent savers in order to obtain their good influence as fellow group members. However, the possibility of sustaining multiple bilateral side payments amongst group members seems unlikely. More likely is that this willingness to pay for time-consistent partners would be reflected in the interest rate on lending, but this takes us back to the first scenario as outlined above.

<sup>23</sup>See <http://www.rockwoolfonden.dk> and <http://www.ccapsolinia.org/> for details. The training of these groups was funded by the Rockwool Foundation, rather than by members themselves. [Greaney et al. \(2016\)](#) show that whether NGOs or members pay for training affects who participates in VSLAs. Thus our empirical results on sorting may only be representative of groups in which NGOs pay for the training. However, this is still by far the most widely-used model for Self-Help Group interventions.

<sup>24</sup>Anecdotally, we learned that a number of “replication” groups did form without SOLDEV training, either autonomously or with the help of members of SOLDEV-trained groups who had been encouraged to teach others the VSLA methodology. Data on such informal groups was difficult to obtain. However, this is not a concern since our results are still valid for all members of official VSLA groups in the area.

Figure 1: Location of Karonga District (dark blue) within Malawi



all group members to a meeting at the group’s usual meeting place. We first explained the purpose of our survey, and obtained the consent of all group members to share their information. The data collection then proceeded in three steps. First, we used the set of individual account books to construct a roster of all group members, past and present. We then elicited basic demographic information for each member as well as their membership history, by reading out each member’s name to the group and then asking a series of questions about that individual. This constitutes the census data, which is the main focus of the analysis in this paper. Second, we also enumerated a short group survey covering the group’s history and practices, such as the typical use of savings and loan funds, the interest rate charged on loans, and the typical punishment for late loan repayments. Finally, we photographed each individual’s account book, which details their weekly savings decisions and their borrowing behaviour for the current cycle. In what follows we draw on the group survey and account books as supplementary data, for example when considering the use of funds and interest rates.



## 3.2 Matching to household survey data

To test whether members sort on present-bias, we require a measure of individuals’ time preferences.<sup>25</sup> Ideally this measure would have been elicited prior to members joining, to rule out reverse causation if being members of the same group leads individuals to have a greater similarity or differences in their choices over time. To obtain such a measure, we matched the members of our 2013 census by name back to the 2009-11 panel dataset which was collected for the initial impact evaluation.<sup>26</sup> The 2009-11 panel covers a stratified random sample of households from the treatment and control villages.<sup>27</sup> Since the 2009-11 panel contains only a random sub-sample of each village’s population — whereas our 2013 member census covers all members — many members in our member census were not interviewed as part of the 2009-11 panel. Overall we are able to match around a fifth of the members from our 2013 census (722 out of 3,801) to the 2009-11 panel. From now on we refer to this sub-sample as “matched individuals”.<sup>28</sup> These individuals are evenly spread across groups: we matched at least one member in 95.3% of groups, and on average we matched 4.7 members out of an average group size of 25.3 members. Of these, 352 randomly received the full time preference module and so can be used to test the predictions of sorting on present-bias.<sup>29</sup>

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<sup>25</sup>Given that we enumerated the 2013 census to the whole group simultaneously, we were restricted to asking about a reasonably small set of individual characteristics. We therefore did not attempt to measure preferences, and so cannot offer a test of the predictions on present-bias from section 2.2.2 on the whole census.

<sup>26</sup>We report time preference values from the 2010 wave, since the 2009 wave did not include the far frame for female respondents. However, only a very small number of the VSLA groups had begun to form by early 2010, and thus time preferences are still plausibly exogenous to the characteristics of other group members.

<sup>27</sup>By construction, the 2009-11 panel therefore includes some individuals who after baseline went to become members of VSLAs and whom we match to our 2013 member census, and others who did not and who therefore do not appear in our 2013 member census. The 2009 baseline was stratified insofar as households who declared an interest in joining VSLAs were over-sampled. For us this just increases the probability that we are able to match members of our 2013 census to the 2009-11 panel.

<sup>28</sup>The matched members should be more representative of the villages’ populations than the census of all members is. For example, whilst only 25% of members in our 2013 member census are male, since women disproportionately join VSLA groups, close to 50% of the respondents in the 2009-11 survey were male due to it being representative. Thus we are disproportionately likely to match a male 2013 member back to the 2009-11 survey compared to a female 2013 member. Section 6 describes how all of our results are robust to re-weighting to take account of this.

<sup>29</sup>For budgetary reasons, only a random subset of households received the full questionnaire including time and risk preference modules.

### 3.3 Group-level data on saving and borrowing

From the group survey, we see that the median share price in 2013 was 100 MK, equivalent to around \$0.30.<sup>30</sup> Members of the median group could therefore save between \$0.30 and \$1.50 per week, or \$16-\$80 per year.<sup>31</sup> Loan sizes vary greatly, but typical amounts for larger loans are 5,000 MK or 10,000 MK (\$15 or \$30).

The patterns of how savings and loan funds are used is quite distinct. The predominant use of savings is for agricultural inputs, with 58% of groups reporting that this is among the three largest uses by their members. Indeed, the groups all choose to share out in January which is during the planting season.<sup>32</sup> The other most prominent uses of savings are food — which makes sense since the January share-out also coincides with the lean season — and durable household items, such as kitchenware. Loans on the other hand are highly concentrated on trading and business purposes: 74% of groups say this is the most important use of loans, and altogether 95% say this is among the three most important uses of loan funds. The other most commonly-reported uses of loan funds are education, emergencies, and purchasing food. Thus overall there does seem to be some financial intermediation taking place within groups: some members save for the planting and lean season, whilst their savings are lent out to members who engage in trading/business activities, or who need to smooth consumption after shocks.

Turning to interest rates, in 2013 the average monthly loan interest rate set by groups was 17%. Whilst this may seem high, it is close to the most natural benchmark for a “fair” interest rate available in the data: namely, individuals’ average monthly discount rate in the long term. The latter is estimated to be 20% with linear utility, or lower if the curvature of the utility function is taken into account; see section 3.4.2 for details.<sup>33</sup>

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<sup>30</sup>The exchange rate at the start of the July 2013 survey was 330 MWK = 1 USD.

<sup>31</sup>For comparison, Malawi’s GNI per capita in 2013 was \$390 (<http://data.worldbank.org>); although the study area is a particularly poor and remote region.

<sup>32</sup>This choice of timing itself may reflect sophistication about the effect of having cash-in-hand just before input purchase is required (Duflo et al., 2011; Brune et al., 2011).

<sup>33</sup>Inflation in 2010 vs. 2013 should of course be taken into account in order to compare in real terms the interest rates in 2013 to the discount rates measured in 2010. Inflation in Malawi was fairly stable at around 8% y-o-y (corresponding 0.64% per month) from the beginning of 2009 until the beginning of 2012. After a devaluation of the Kwacha by 33% in May 2012, inflation spiked and ran at an average of

At the same time, this suggests that borrowers are not taking advantage of commitment savers' willingness to pay for commitment by gaining a very low interest rate. Indeed, savers appear to earn a healthy return on their shares: the median monthly interest on saving is approximately 3%, as calculated from each group's reported annual return per share at the end of the cycle.<sup>34</sup>

## 3.4 Individual member characteristics

### 3.4.1 Census of members

Table 1 describes some of the key demographic characteristics of the 3,801 individuals in our member census. 73% of members report farming as their primary economic activity, whilst 21% work in business (mainly family businesses). Although the NGO imposes no rules on the gender of participants, 75% of the members are female.<sup>35</sup> The data also suggest a large degree of churn individuals' membership, which again points to the scope for sorting across groups: almost a third of members (1,262 out of 3,801) join sometime after the first cycle, and 521 members have left the group at some point by 2013.

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28% in 2013 overall (corresponding to 2.1% per month). Groups do not appear to have taken this into account in the nominal loan interest rate set at the beginning of the 2013 cycle, which in most groups remained unchanged from previous cycles. However, even the high 2013 inflation rate is still negligible on a monthly basis compared to such a high monthly loan interest rate, and thus accounting for monthly inflation does not alter our conclusion that the interest rate on loans appears to be broadly in line with monthly discount rates.

<sup>34</sup>This is straightforward to reconcile with a monthly interest rate on borrowing of 17%, since only a fraction of the group's funds will be lent out at any given time. The median annual return per share is 45%, equating to 3% monthly interest compounded on shares purchased right at the beginning of the year. However, this is a conservative approximation insofar as the true effective monthly interest rate will be higher since individuals save throughout the year rather than just at the beginning.

<sup>35</sup>This could reflect a variety of factors: for example if women have lower transaction costs or higher social benefits of membership, lower access to other financial technologies, or indeed are viewed as more financially responsible.

Table 1: Individual member characteristics, 2013 member census

	<b>Full CSAE Sample</b>				
	<b>Mean</b>	<b>Std dev</b>	<b>Min</b>	<b>Max</b>	<b>N</b>
<b>Occupation</b>					
Farmer	0.73	(0.44)	0.0	1.0	3801
Businessperson	0.21	(0.41)	0.0	1.0	3801
Other	0.06	(0.23)	0.0	1.0	3801
<b>Demographic Variables</b>					
Male	0.25	(0.43)	0.0	1.0	3799
Female-headed household	0.21	(0.41)	0.0	1.0	3796
Age	36.15	(12.05)	12.0	83.0	3785
<b>Education</b>					
Some primary educ. (only)	0.76	(0.43)	0.0	1.0	3801
Some post-primary educ.	0.18	(0.39)	0.0	1.0	3801
Literate (read & understand newspaper)	0.83	(0.38)	0.0	1.0	3795
<b>Wealth</b>					
Father well-off in village (scale 1-5)	3.34	(1.31)	1.0	5.0	3573
Spouse's father well-off in village (scale 1-5)	3.42	(1.32)	1.0	5.0	3566
<b>Income Poverty Indicators</b>					
Household well-off in group (scale 1-9)	7.52	(1.28)	1.0	9.0	3770
HH owns a bicycle	0.51	(0.50)	0.0	1.0	3796
# Goats	1.29	(2.59)	0.0	40.0	3792

Notes: All variables presented were measured during the 2013 member census, N=3,801 members. 519 of these members were no longer active, but were included in the survey to avoid selection bias. Analysis is conducted with and without these individuals. Missing observations reflect answers of “do not know” or “not applicable”. Occupation denotes an individual’s primary economic activity, even if engaged in multiple activities.

Table 2: Individual economic activity and schooling, full 2013 member census

Category	N	% of re- sponses
<b>Occupation</b>	<b>3780</b>	<b>100%</b>
Farmer	2,786	73.34%
Business	793	20.87%
<i>Self-employed</i>	108	2.84%
<i>Family business worker</i>	685	18.03%
Fishing	61	1.61%
<i>Fishing, employed</i>	17	0.45%
<i>Fishing, self-employed</i>	44	1.16%
Employee	89	2.34%
Casual labour (ganyu)	24	0.63%
Student	4	0.11%
Unemployed, not seeking work	9	0.24%
Other	14	0.37%

Data from the VSLA member census conducted in July-August 2013. The total number of members is 3,801. The number of observations for each variable reflects missing values or non-applicability.

Notes: All variables presented were measured during the 2013 member census, N=3,801 members. 519 of these members were no longer active, but were included in the survey to avoid selection bias. Analysis is conducted with and without these individuals. Missing observations reflect answers of “do not know” or “not applicable”. Occupation denotes an individual’s primary economic activity, even if engaged in multiple activities.

### 3.4.2 Time preference data

As described above, we matched 722 individuals by name to the baseline data of the 2009-11 impact evaluation. Matching to this additional dataset yields a much richer set of data on individual characteristics for the matched individuals, as summarized in table 3. Crucially, activities to measure time preferences were administered to both the head and the spouse in a random subset of the panel data households in 2010, and can be matched to 352 individuals. These activities took the form of multiple price lists. Participants were first asked whether they would prefer to receive 2000 Kwacha (approximately \$13) now or increasing amounts in one month.<sup>36</sup> This constitutes the near frame. Participants were then asked whether they would prefer to receive 2000 MK in one year or the same increasing amounts in one year and one month. This constitutes the far frame.<sup>37</sup> The average respondent prefers 2000 MK now to 2332 MK in one month, and 2000 MK in one year to 2402 MK in one year and one month. If participants answered such questions without considering their background consumption, and if utility was linear, this would imply an average near-frame monthly discount rate of 17% and an average far-frame monthly discount rate of 20%. However, taking into account any curvature of the utility function implies a lower discount rate (Andersen et al., 2008).

A more impatient choice in the near frame than in the far frame is taken to indicate “present-bias”. The choices of members classified as “present-biased” imply an average near-frame discount rate of 16% and an average far-frame monthly discount rate of close to zero, consistent with the idea of present-biased individuals exhibiting excessive short-run discounting but more modest long-run discounting.<sup>38</sup> Overall 11% of individuals are

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<sup>36</sup>The exchange rate at the start of 2010 was approximately 150 MWK = 1 USD. Due to practical constraints, responses were unincentivized. The limited evidence comparing incentivized and unincentivized responses to time preference questions suggests that lack of incentivization does not bias responses (although they may be more noisy), in contrast to the evidence on elicitation of risk preferences and beliefs (John, 2017).

<sup>37</sup>The frame order was not randomised, as this was not practical in a large paper-based household survey.

<sup>38</sup>Again, the estimate of 16% is an approximation if utility is linear, but is an upper bound if utility is concave. Those classified as “time-consistent” actually exhibit far-frame switch-points consistent with a higher far-frame discount rate than those classified as “present-biased”. However, these “time-consistent” individuals appear equally impatient in the near and the far frames.

classified as “present-biased”. This is very similar to the rate of 10% found by Brune et al. (2011) also in rural Malawi. Other estimates from developing countries find that a larger proportion of individuals appear to be “present-biased” (Ashraf et al., 2006; Giné et al., 2016; Janssens et al., 2017). However, as discussed in chapter one, estimates of “present-bias” over money may be exaggerated at times of tight liquidity constraints (Carvalho et al., 2016). Tight liquidity constraints are much less of a concern here, since the 2009-11 panel dataset was always enumerated shortly after the harvest. If anything, we may under-estimate the number of present-biased individuals, if some present-biased individuals have enough liquidity and are sophisticated enough to arbitrage experimental payments (Augenblick et al., 2015). This would reduce our chances of observing the any assortative matching, positive or negative, on this measure.

The fact that the far frame refers to one year after the near frame eliminates concerns that seasonality in consumption and liquidity constraints may act as a confound in the measure of “present-bias” (Epper, 2015). Nonetheless, individuals may still spuriously appear “present-biased” if they are expecting a decrease in the marginal rate of intertemporal substitution next year compared to this year, for example if this year’s harvest was particularly bad for their household. We therefore employ a number of tests to check whether measured “present-bias” appears to be capturing a decreasing marginal rate of intertemporal substitution, rather than truly present-biased preferences.

First, if individuals who appear “present-biased” are actually those facing a higher marginal rate of intertemporal substitution than they expect to face in one year’s time, we might expect the measure of “present-bias” to be correlated with measures of low recent consumption. Vice versa, we might expect individuals classified as “future-biased” (26% of the sample) to exhibit high recent consumption. In fact, the measures of “present-bias” and “future-bias” in early 2010 are completely uncorrelated with consumption from 2009. Moreover, whilst “future-bias” is correlated with better food security in 2009 (the household is 8.9 percentage points less likely to have had fewer than two meals per day on average in the last week, p-value 0.025), “present-bias” is also marginally correlated with better food security (14.0 percentage points, p-value 0.112). This suggests that individuals who appear “time-consistent”, not those who appear “present-biased”, are

those who have experienced recent hardship.

Data on saving and borrowing also points more strongly against the idea that individuals who appear “present-biased” are actually those who are expecting to be better off next year. If this was the case, we might expect these individuals to exhibit higher recent borrowing and lower saving. Instead, we observe a strong negative correlation between appearing “present-biased” in 2010 and data on borrowing from 2009 and 2010: in 2009, individuals categorised as “present-biased” are 13.7 percentage points less likely to have asked for a loan in the past year, are 7.3 percentage points less likely to have any current loans, and have 746 MK fewer in outstanding loans; whilst in 2010 they are 16.8 percentage points less likely to have asked for a loan in the past year, and have 4957 MK fewer in outstanding loans, although the latter is marginally insignificant (p-value 0.134). All of this is more consistent with the idea that the idea that present-biased individuals may not be deemed credit-worthy by others, or may avoid seeking credit if they are aware of their own tendency to over-consume. “Present-biased” individuals also have higher total savings from 2009 (4468 MK, p-value 0.075), which further goes against the idea that individuals appear “present-biased” because they are liquidity-constrained now but anticipate higher income in the future.<sup>39</sup> Finally, the measure of “future-bias” is uncorrelated with measures of saving and borrowing.

On the other hand, the measure of “present-bias” is marginally correlated with subjects’ subjective report that they have had a bad harvest in 2010 compared to the past decade (correlation of 0.292 on a 1-5 Likert scale, p-value 0.101). We thus cannot entirely rule out that the measure of “present-bias” may in part be capturing individuals’ expectations that they will be better off in a year’s time. Therefore in section 5 we run a further set of tests to check whether true present-bias or a decreasing marginal rate of intertemporal substitution appear to be driving our results on sorting. The 2009-11 panel dataset also provides measures of the matched individuals’ risk aversion — elicited using standard Binswanger lotteries — intra-household bargaining, and more detailed measures of con-

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<sup>39</sup>The fact that “present-biased” individuals may be able to save outside of VSLAs is still consistent with them having a strong demand for VSLAs as a commitment savings device, since VSLAs may offer a better return than other forms of saving such as cash-under-the-mattress. Moreover, exercising self-control by oneself may be costly (Gul and Pesendorfer, 2001; Toussaert, 2015).



sumption and food security. These are summarised in table 3 and are used as additional controls in robustness checks.

Table 3: Detailed individual member characteristics, matched sub-sample

	<b>Matched individuals</b>				
	Mean	Std dev	Min	Max	N
<b>Time Preferences</b>					
Present-biased	0.11	(0.31)	0.0	1.0	350
Future-biased	0.26	(0.44)	0.0	1.0	350
Minimum switch-point, near frame	2332.36	(369.62)	1900	2800	352
Minimum switch-point, far frame	2401.39	(378.38)	1900	2800	352
<b>Risk Preferences</b>					
Risk-neutral	0.11	(0.32)	0.0	1.0	330
<b>Intra-Household Bargaining</b>					
Ever hides money from spouse	0.44	(0.50)	0.0	1.0	307
Female HH decision-making power (index 0-8)	2.87	(1.81)	0.0	8.0	377
<b>Social Variables</b>					
HH important in village decisions (scale 1-6)	3.27	(1.14)	1.0	6.0	721
HH ever speaks at village meetings	0.57	(0.50)	0.0	1.0	718
<b>Income</b>					
Monthly consumption per capita, MK	2176.95	(973.09)	648.7	7811.1	721
Food security poor (dummy)	0.28	(0.45)	0.0	1.0	722
<b>Credit</b>					
HH asked for credit in last year	0.15	(0.36)	0.0	1.0	383
HH has any loans outstanding	0.09	(0.29)	0.0	1.0	383
Total value of loans outstanding, MK	779.90	(4238.64)	0.0	45000	383

Notes: N=722 individuals are matched from the 2013 census to the 2009-11 panel data. N=383 of these individuals are matched to the longer panel survey including preference modules. Missing values reflect “do not know”, “not applicable”, or inconsistent answers in the case of risk preferences. All variables presented here were measured in the 2009 wave of the panel survey, except time preferences, which are taken from the 2010 survey wave since the 2009 wave did not include the far frame for females. Present-biased (future-biased) is a dummy equal to one if the response to the near frame is more impatient (patient) to the response to the far frame. Minimum switch-point is the lower bound of the interval in which the respondent switched to preferring the one-month-later payment compared to a 2000 MK payment on the earlier date. 150 MK  $\approx$  1 USD at the time of the 2009 and 2010 surveys. Risk-neutral is a dummy equal to one if the respondent prefers a 50-50 lottery to its expected value for certain, and thus could indicate risk-neutral or risk-seeking behaviour. Female HH decision-making power is constructed from questions over four types of economic decisions, scoring one if the female has some say in the decision and two if she has complete control. To aid interpretation of the statistics on monthly consumption, Malawi’s GNI per capita in 2009 was \$26.6/month. However, these are particularly poor households in a very remote region. Food security poor is equal to one if the household reports consuming fewer than three meals yesterday.

### 3.5 Group composition in terms of member characteristics

Table 4 describes the distribution of groups across villages. Thirty-five villages have at least two groups, and some villages have up to fourteen groups.<sup>40</sup> The presence of more than one group per village is itself suggestive evidence of inefficiency: there is no secondary market for capital in these villages, and so VSLAs with excess capital cannot lend to other VSLAs; hence having one large VSLA per village would maximise the scope for lending out savings deposits, thereby alleviating credit constraints. However, transaction costs and ability to monitor and sanction borrowers likely become too large before a VSLA grows to the size of a whole-village institution.<sup>41</sup> This would explain why we observe multiple groups per village. Hence sorting across groups becomes a key determinant of efficiency in this “second-best” scenario.

Table 4: Number of VSLA groups per village

# groups in village	# villages	% of villages
1	9	20.5%
2	17	38.6%
3	4	9.1%
4	4	9.1%
5	3	6.8%
6	2	4.6%
7	2	4.6%
11	1	2.3%
13	1	2.3%
14	1	2.3%
<b>Total</b>	<b>44</b>	<b>100%</b>

Notes: From the 2013 member census and group survey, N=150 groups, N=44 villages. Eight of these groups were no longer active, but were included in the survey to avoid selection bias. Analysis is conducted with and without these groups.

Table 5 describes how groups are composed in terms of member characteristics. The

<sup>40</sup>Nine villages have just one group, and thus sorting is not identified in these villages.

<sup>41</sup>Indeed, anecdotally we were told that one VSLA of around 45 members had to split into two groups, because the members found it too difficult to reach consensus decisions in such a large group.

average group size is 25 members, although groups range in size from 10 to 45 members. Groups also range in gender composition from all-male to all-female, although most groups (i.e. groups within one standard deviation of the mean) are mixed but with a majority of female members. In some groups, as many as 62% of members come from female-headed households. There is also clear heterogeneity across groups in terms of occupational composition: some groups consist purely of farmers, whereas others contain almost no farmers. However, dyadic regression analysis is needed to determine whether such heterogeneity is evidence of individuals sorting across groups within villages, or whether it represents differences in population characteristics across villages.

Table 5: Group composition in terms of member characteristics

<b>Variable</b>	<b>Average</b>	<b>Std Dev</b>	<b>Min</b>	<b>Max</b>
# members	25.34	5.69	10	45
% members farmers	74.2	26.3	3.3	100.0
% members businesspeople	20.2	22.2	0.0	93.3
% members fisherman/woman	1.6	5.5	0.0	30.4
% members female	74.9	19.0	0.0	100.0
% members female-headed HH's	20.9	13.0	0.0	61.9
Mean age of members	36	4.6	23	49
% members literate	82.5	12.2	40.9	100.0
% members some primary only	81.6	11.4	38.7	100.0
% members own bicycle	52.1	18.6	0.0	95.0
Mean # goats owned by members	1.32	0.98	0.04	7.48

Notes: From the 2013 member census and group survey, N=150 groups, N=44 villages. Eight of these groups were no longer active, but were included in the survey to avoid selection bias. Analysis is conducted with and without these groups. Similarly, 519 of these members were no longer active, but were included in the survey for completeness. Analysis is conducted with and without these members.

## 3.6 Dyad characteristics

For the dyadic regression analysis, we construct all possible pairs – dyads – of individuals in the same village from the 2013 member census.<sup>42</sup> Of these dyads, 17% comprise two individuals who are both members of the same group, whereas the other 83% comprise two individuals who are members of different groups in the same village. Table 6 describes the dyads in more detail.

When we restrict attention to the dyads in which both individuals are matched to the 2009-11 panel dataset, this gives us a sample size of 7,326 dyads. Of these, 1,641 are dyads in which both individuals are matched to the full version of the 2009-11 survey, which included the detailed preference modules.<sup>43</sup> Table 7 highlights the key additional data for the matched dyads, in terms of the absolute differences and the sums of  $i$  and  $j$ 's characteristics.

# 4 Empirical strategy

## 4.1 Dyadic regression framework

To test the predictions on sorting from sections 2.2.1 and 2.2.2, we employ a dyadic regression framework (Fafchamps and Gubert, 2007). The intuition behind this approach is as follows: if there are multiple groups in a village, and if there is *positive* sorting on a given characteristic, then in equilibrium two members who are less similar on that characteristic are, ceteris paribus *less* likely to be observed as members of the *same* group.

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<sup>42</sup>In practice it is extremely rare that an individual would join a group outside of his/her village of residence; thus *de facto* only the other members from an individual's village of residence are candidates to be members of the same group as that individual.

<sup>43</sup>In terms of basic characteristics, table 12 shows that the dyads which can be matched to the full version of the panel survey have small but significant differences from the whole universe of dyads from the 2013 member census. Therefore, we later re-run all of our time preference specifications weighting each dyad by the inverse probability of that dyad being matched to the full 2009-11 survey. This does not change our results, see section 6.

Table 6: Dyad characteristics, full 2013 member census

	<b>Full CSAE Sample</b>				
	<b>Mean</b>	<b>Std dev</b>	<b>Min</b>	<b>Max</b>	<b>N</b>
<b>Membership</b>					
Same VSLA group	0.17	(0.37)	0.0	1.0	289914
<b>Occupation</b>					
Same economic activity	0.56	(0.50)	0.0	1.0	289914
<b>Absolute differences - Demographic Variables</b>					
Male	0.38	(0.48)	0.0	1.0	289467
Female-headed household	0.32	(0.47)	0.0	1.0	288740
Age	12.64	(10.25)	0.0	65	286763
<b>Absolute differences - Education</b>					
Some post-primary educ.	0.33	(0.47)	0.0	1.0	289914
Literate (read & understand newspaper)	0.26	(0.44)	0.0	1.0	288342
<b>Absolute differences - Wealth</b>					
Father well-off in village (scale 1-5)	1.41	(1.10)	0.0	4.0	253041
Spouse's father well-off in village (scale 1-5)	1.43	(1.13)	0.0	4.0	252485
<b>Absolute differences - Income and Poverty</b>					
Household well-off in group (scale 1-9)	1.40	(1.15)	0.0	8.0	284485
HH owns a bicycle	0.46	(0.50)	0.0	1.0	288740
# Goats	1.84	(2.73)	0.0	40	288240
<b>Sum - Occupation</b>					
Farmer	1.37	(0.70)	0.0	2.0	289914
Businessperson	0.48	(0.63)	0.0	2.0	289914
<b>Sum - Demographic Variables</b>					
Male	0.53	(0.63)	0.0	2.0	289467
Female-headed household	0.40	(0.57)	0.0	2.0	288740
Age	72.12	(16.69)	24.0	163	286763
<b>Sum - Education</b>					
Some post-primary educ.	0.43	(0.58)	0.0	2.0	289914
Literate (read & understand newspaper)	1.68	(0.52)	0.0	2.0	288342
<b>Sum - Wealth</b>					
Father well-off in village (scale 1-5)	6.76	(1.85)	2.0	10.0	253041
Spouse's father well-off in village (scale 1-5)	6.85	(1.88)	2.0	10.0	252485
<b>Sum - Income and Poverty</b>					
Household well-off in group (scale 1-9)	15.04	(1.83)	3.0	18.0	284485
HH owns a bicycle	0.99	(0.73)	0.0	2.0	288740
# Goats	2.44	(3.38)	0.0	70	288240

Notes: All variables presented were measured during the 2013 member census, N=3,801 members. 519 of these members were no longer active, but were included in the survey to avoid selection bias. Analysis is conducted with and without these individuals. All possible dyads in which both individuals live in the same village are constructed, N=289,914. Missing observations reflect answers of “do not know” or “not applicable”. Occupation denotes an individual’s primary economic activity, even if engaged in multiple activities.

Table 7: Key dyadic regressors, matched sub-sample

	Matched dyads				
	Mean	Std dev	Min	Max	N
<b>Absolute differences</b>					
Present-biased	0.16	(0.37)	0.0	1.0	1641
Future-biased	0.37	(0.48)	0.0	1.0	1641
Minimum switch-point, near frame	366.05	(321.43)	0.0	900	1655
Minimum switch-point, far frame	351.92	(337.30)	0.0	900	1651
Risk-neutral	0.20	(0.40)	0.0	1.0	1513
Ever hides money from spouse	0.45	(0.50)	0.0	1.0	1269
Female HH decision-making power (index 0-8)	1.95	(1.57)	0.0	8.0	1914
HH important in village decisions (scale 1-6)	1.08	(1.11)	0.0	5.0	7314
HH speaks at village meetings	0.47	(0.50)	0.0	1.0	7266
<b>Sums</b>					
Present-biased	0.19	(0.42)	0.0	2.0	1641
Future-biased	0.53	(0.64)	0.0	2.0	1641
Minimum switch-point, near frame	4761.51	(558.00)	3800	5600	1655
Minimum switch-point, far frame	4908.17	(563.28)	3800	5600	1651
Risk-neutral	0.22	(0.44)	0.0	2.0	1513
Ever hides money from spouse	0.79	(0.71)	0.0	2.0	1269
Female HH decision-making power (index 0-8)	5.90	(2.71)	0.0	16.0	1914
HH important in village decisions (scale 1-6)	6.54	(1.69)	2.0	12.0	7314
HH ever speaks at village meetings	1.16	(0.71)	0.0	2.0	7266

Notes: N=722 individuals are matched from the 2013 census to the 2009-11 panel data. N=383 of these individuals are matched to the longer panel survey including preference modules. All possible dyads in which both individuals live in the same village are constructed, N=7,314 for the general survey and N=1,641 for the full survey including preference modules. Missing values reflect “do not know”, “not applicable”, or inconsistent answers in the case of risk preferences. All variables presented here were measured in the 2009 wave of the panel survey, except time preferences, which are taken from the 2010 survey wave since the 2009 wave did not include the far frame for females. Present-biased (future-biased) is a dummy equal to one if the response to the near frame is more impatient (patient) to the response to the far frame. Minimum switch-point is the lower bound of the interval in which the respondent switched to preferring the one-month-later payment compared to a 2000 MK payment on the earlier date. 150 MK  $\approx$  1 USD at the time of the 2009 and 2010 surveys. Risk-neutral is a dummy equal to one if the respondent prefers a 50-50 lottery to its expected value for certain, and thus could indicate risk-neutral or risk-seeking behaviour. Female HH decision-making power is constructed from questions over four types of economic decisions, scoring one if the female has some say in the decision and two if she has complete control. Food security poor is equal to one if the household reports consuming fewer than three meals yesterday.

Vice versa, if there is *negative* sorting on a given characteristic, then two members who are less similar on that characteristic are *more* likely to be members of the *same* group.

Our main estimating equations are therefore undirected dyadic logit models, with observations at the dyad level. These take the following form:

$$\begin{aligned} \Pr(D_{ij} = 1 | D_i = 1 \ \& \ D_j = 1; \mathbf{Z}_i, \mathbf{Z}_j, \mathbf{W}_{ij}, v) \\ = \Pr(\alpha + \beta|\mathbf{Z}_i - \mathbf{Z}_j| + \gamma(\mathbf{Z}_i + \mathbf{Z}_j) + \delta\mathbf{W}_{ij} + \mu_v + \varepsilon_{ij} > 0) \end{aligned} \quad (1)$$

where  $D_i$  and  $D_j$  denote dummies equal to one if  $i$  and  $j$  are members of some VSLA group,<sup>44</sup> and  $D_{ij}$  is a dummy equal to one if  $i$  and  $j$  are members of the *same* group.  $\mathbf{Z}_i$  and  $\mathbf{Z}_j$  are vectors of  $i$ 's and  $j$ 's individual characteristics, in which we include measures of present-bias. We also seek to minimize omitted variable bias by controlling for a rich set of characteristics which might also drive sorting and might be correlated with occupation and present-bias. Sections 5 and 6 detail the full set of controls used in the main specifications and robustness checks.  $\mathbf{W}_{ij}$  is a vector of characteristics of the dyad, such as whether  $i$  and  $j$  share the same category of occupation.  $\mu_v$  is a village fixed effect.<sup>45</sup> These control for the average probability of matching in the village, which depends on the number of groups and also on the relative size of each group. The inclusion of village fixed effects also absorbs a range of factors which might affect the probability of being in the same group but remain constant at the village level, for example whether the village is served by other NGO programmes.  $\varepsilon_{ij}$  is a dyad-specific error term, which we assume takes a logistic distribution. We cluster standard errors at the village level in all estimations.<sup>46</sup>

It follows from the logic outlined above that an estimate of  $\hat{\beta} < 0$  indicates *positive* assortative matching on the characteristic in question; an estimate of  $\hat{\beta} > 0$  indicates *negative* assortative matching on that characteristic; and an estimate of  $\hat{\beta} = 0$  indicates

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<sup>44</sup>This is for notational completeness: by construction both dummies will always be equal to one in our analysis, since our data contains only members.

<sup>45</sup>For notational convenience, we omit  $v$  subscripts from all regressors except the fixed effects.

<sup>46</sup>This is more conservative than the more common method of clustering by dyad (Fafchamps and Gubert, 2007).

that there is no evidence of sorting on that characteristic. Since we estimate equation 1 on a sample which only includes individuals who are members of *at least one* group, an estimate of  $\hat{\gamma} > 0$  ( $< 0$ ) indicates that conditional on being member of at least one group, individuals with a high value of that particular variable are more (less) likely to be members of *more than one* group.<sup>47</sup> This increases (decreases) the probability that such individuals are in the same group as a randomly-chosen other member, simply because such individuals are members of more (fewer) groups.

## 5 Results

### 5.1 Sorting on occupation

We begin by estimating equation 1 on the full 2013 member census.<sup>48</sup> Table 8 describes the results. Most strikingly for the predictions in section 2.2.1, there is evidence of strong *positive* assortative matching on occupation: if two individuals share the same occupation then they are 8.6 percentage points more likely to be members of the same group (p-value  $< 0.01$ ). This is a large effect, equivalent to 53% of the baseline probability of being in the same group (16.1%). It therefore appears that the possibility of financial intermediation across farmers and non-farmers, via *negative* assortative matching, is not being realised. Instead, evidence of positive assorting suggests that informational or enforcement frictions are lower, or social benefits of participating in VSLAs are higher, within occupational groups than across them. This appears to dominate as a driver of sorting.

The other large effect in terms of size is that we observe positive assortative matching on gender: *ceteris paribus*, a male and a female are 5.3 percentage points less likely to be members of the same group than an all-male pair or an all-female pair are (p-value

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<sup>47</sup>We observe 146 individuals who are members of more than one group in the 2013 member census.

<sup>48</sup>In our main analyses as we include all individuals who have ever been a member of the VSLA groups surveyed in 2013. However, our results are all robust to including only those members who are still current members in 2013, see section 6.



Table 8: Dyadic regressions – full 2013 member census

	(1) <b>Full member census</b> Mfx / (s.e.)
<b>Occupation</b>	
Same economic activity	0.086*** (0.008)
<b>Absolute differences</b>	
Male	-0.053*** (0.015)
Female-headed household	-0.012*** (0.004)
Age	-0.001*** (0.000)
Some post-primary educ.	-0.008 (0.005)
Literate (read & understand newspaper)	-0.013* (0.007)
Father well-off in village (scale 1-5)	-0.013*** (0.002)
Spouse's father well-off in village (scale 1-5)	-0.013*** (0.003)
Household well-off in group (scale 1-9)	-0.010*** (0.002)
HH owns a bicycle	-0.003 (0.003)
# Goats	-0.006*** (0.001)
<b>Sums</b>	✓
<b>Village f.e.'s</b>	✓
Observations	219747
Pseudo $R^2$	0.129
Baseline predicted probability	0.161

Notes: \*, \*\* and \*\*\* represent  $p < 0.10$ ,  $p < 0.05$  and  $p < 0.01$  respectively. All variables presented were measured during the 2013 member census,  $N=3,801$  members. 519 of these members were no longer active, but are included here to avoid selection bias. Results are robust to excluding these individuals. All possible dyads in which both individuals live in the same village are constructed,  $N=289,914$ . Missing observations reflect answers of “do not know” or “not applicable”. Occupation denotes an individual's primary economic activity, even if engaged in multiple activities. Reported effects are marginal effects estimated at the mean.

$< 0.01$ ). Female-headed households are also more likely to group together, although the effect size is just 1.2 percentage points (p-value  $< 0.01$ ). A number of other characteristics are also highly significant, although the estimated marginal effects are small. Specifically, we observe positive assortative matching on: age; whether a member’s spouse’s father is relatively well off and whether a member’s own father is relatively well-off compared to the rest of the village (proxies of exogenous non-income wealth, or social class more generally); the number of goats and whether a household possesses a bicycle (standard poverty indicators for this region of Malawi); and whether a household is reported to be well-off at least compared to the rest of the group.<sup>49</sup> Such positive assorting may take place purely due to homophilous preferences, i.e. if members prefer to interact with other group members who are similar. Alternatively, it may be that members with similar social characteristics have similar financial needs, and so can more easily agree on the value of weekly shares, the size of the interest rate, and so on.

## 5.2 Sorting on time preferences

To test for evidence of sorting on present-bias, we re-estimate equation 1 for the subsample of matched individuals whose time preferences were measured in the 2009-11 panel. Table 9 shows the effect of adding the measure of “present-bias” for these individuals. The key result is that we see strong evidence of negative assortative matching on “present-bias”: the absolute difference between two members’ “present-bias” carries a large, positive coefficient of 16.6 percentage points (p-value 0.013). This is consistent with the idea outlined in section 2.2.2 that present-biased individuals who seek a commitment savings device sort into groups with time-consistent individuals who seek access to credit. The result is particularly striking given that we observe positive assortative matching on almost every other characteristic.

We still observe a large, positive effect of two individuals having the same primary occu-

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<sup>49</sup>Given that “household well-off in group” is a within-group ranking, we would expect its coefficient to be biased towards a positive value. The negative coefficient therefore suggests that individuals understood this question to be more about absolute consumption.

pation: 18.1 percentage points (p-value 0.019), which is again equivalent to over half the baseline probability of two members being in the same group in this sub-sample. The pattern of coefficients for the other controls is also very similar to that obtained in the full sample. Table 13 in appendix A.1 formally tests for equality of coefficients across the full sample and the matched sub-sample (excluding the measure of “present-bias”, since this is not available for the full sample) and finds few significant differences. Thus it does appear that the sorting observed in the matched sub-sample is broadly representative of the sorting observed in the full census. This suggests that the negative sorting on “present-bias” observed in the matched sub-sample would also hold in the full 2013 census if measures of “present-bias” were available for all individuals. The results are also robust to re-weighting the estimations in order to make the matched sub-sample exactly representative of the full sample, see section 6 for details.

Columns (1)-(5) of table 10 confirm that the negative assortative matching on “present-bias” is not driven by matching on short-run or long-discount rates (or marginal rates of inter-temporal substitution).<sup>50</sup> Column (1) repeats the preferred specification shown in table 9 for comparison. Columns (2) and (3) show that we observe no sorting on the respondent’s switch-point in the near or the far frame respectively. Similarly, column (4) shows that there is no evidence of sorting on whether the respondent is below or above the median patience in the near frame or the far frame. Thus sorting really does appear to be driven by the fact that “present-biased” individuals’ long-run choices are more patient than their short-run choices — whilst “time-consistent” individuals’ choices are equally impatient in the near and the far frame — as opposed to the fact that “present-biased” individuals have impatient short-run choices or patient long-run choices.

However, as discussed in section 3.4.2, it is still possible that our measure of “present-bias” is instead capturing individuals who were expecting to have a lower marginal rate of intertemporal substitution in the future. If so, such individuals should have had a demand for borrowing, at least when the VSLAs were first formed. Conversely, individuals classified as “future-biased” may in fact have been anticipating a higher marginal

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<sup>50</sup>Table 15 in appendix A.1 confirms that the results in table 10 also hold when the sub-sample is re-weighted.

Table 9: Dyadic regressions – matched sub-sample

	(1) Matched subsample Mfx / (s.e.)
<b>Occupation</b>	
Same economic activity	0.181** (0.077)
<b>Absolute differences</b>	
Present-biased	0.166** (0.067)
Male	-0.004 (0.035)
Female-headed household	-0.176** (0.070)
Age	-0.006** (0.003)
Some post-primary educ.	-0.038 (0.054)
Literate (read & understand newspaper)	-0.033 (0.042)
Father well-off in village (scale 1-5)	-0.020 (0.012)
Spouse's father well-off in village (scale 1-5)	-0.043*** (0.014)
Household well-off in group (scale 1-9)	-0.044** (0.019)
HH owns a bicycle	0.008 (0.030)
# Goats	-0.013* (0.007)
<b>Sums</b>	✓
<b>Village f.e.'s</b>	✓
Observations	1292
Pseudo $R^2$	0.222
Baseline predicted probability	0.296

Notes: \*, \*\* and \*\*\* represent  $p < 0.10$ ,  $p < 0.05$  and  $p < 0.01$  respectively.  $N = 722$  individuals are matched from the 2013 census to the 2009-11 panel data.  $N = 383$  of these individuals are matched to the longer panel survey including preference modules. All possible dyads in which both individuals live in the same village are constructed,  $N = 7,314$  for the general survey and  $N = 1,641$  for the full survey including preference modules. Missing values reflect “do not know”, “not applicable”, or inconsistent answers in the case of risk preferences. Time preferences are taken from the 2010 survey, wave since the 2009 wave did not include the far frame for females. Present-biased (future-biased) is a dummy equal to one if the response to the near frame is more impatient (patient) to the response to the far frame. Reported effects are marginal effects estimated at the mean.

rate of intertemporal substitution in the future, and thus may have had a demand for saving. If so, we would expect to see two things. First, in terms of who sorts into groups with “time-consistent” individuals, we would now expect those individuals spuriously classified as “future-biased” to do so. This is because individuals classified as “future-biased” would now be the ones providing savings, which “time-consistent” individuals could borrow if investment opportunities arose. We would therefore observe negative assortative matching on “future-bias”. We may still observe negative assortative matching on “present-bias” as well, if some “time-consistent” individuals are willing to lend to the individuals spuriously classified as “present-biased” if a sufficiently high interest rate can be agreed. Second, we would now expect to see the strongest matching between “present-biased” individuals, who in fact are individuals with a demand for credit, and “future-biased” individuals, who in fact are individuals with a demand for saving. That is, we would observe negative assortative matching on “present-bias” particularly when “time-consistent” individuals are dropped from the sample.

Columns (5)-(7) of table 10 show that neither of these predictions are supported by the data. Column (5) shows that, unlike “present-bias”, there is no observed sorting on “future-bias”. Column (6) reports the results when “time-consistent” individuals are dropped from the sample, and thus only “present-biased” and “future-biased individuals” remain. There is no significant evidence of negative assortative matching within this subsample, i.e. no evidence that “present-biased” individuals sort into groups with “future-biased” individuals.<sup>51</sup> In contrast, column (7) reports results when “future-biased” individuals are dropped, leaving a sample of only “present-biased” and “time-consistent” individuals. The estimated coefficient on “present-biased” is large and highly significant. Thus there is strong evidence that “present-biased” individuals are matching with “time-consistent” individuals, and that this is driving the matching observed in the full sample. This pattern of results is thus much more in line with the predictions in section 5.2 and thus the idea that our measure of “present-bias” is truly capturing present-biased individuals.

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<sup>51</sup>The sample size does drop considerably in column (6). However, the point estimate is much smaller, whilst standard errors do not increase substantially.

Table 10: Dyadic regressions – time preference measures, matched sub-sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)
<b>Occupation</b>							
Same economic activity	0.181** (0.077)	0.175** (0.081)	0.180** (0.079)	0.187** (0.079)	0.178** (0.077)	0.108 (0.073)	0.145 (0.106)
<b>Absolute differences</b>							
Present-biased	0.166** (0.067)		0.179** (0.073)	0.175** (0.072)	0.168** (0.067)	0.055 (0.060)	0.195** (0.083)
Future-biased					-0.017 (0.036)		
Minimum switch-point, near frame		0.000 (0.000)	0.000 (0.000)				
Minimum switch-point, far frame		-0.000 (0.000)	-0.000 (0.000)				
Patience above median, near frame				0.011 (0.036)			
Patience above median, far frame				-0.032 (0.031)			
Wealth & income controls (abs. diffs)	✓	✓	✓	✓	✓	✓	✓
Demographic controls (abs. diffs)	✓	✓	✓	✓	✓	✓	✓
<b>Sums</b>	✓	✓	✓	✓	✓	✓	✓
<b>Village f.e.'s</b>	✓	✓	✓	✓	✓	✓	✓
Observations	1292	1292	1292	1292	1292	184	665
Pseudo $R^2$	0.222	0.220	0.225	0.226	0.224	0.331	0.233
Baseline predicted probability	0.296	0.296	0.296	0.296	0.296	0.273	0.293

Notes: \*, \*\* and \*\*\* represent  $p < 0.10$ ,  $p < 0.05$  and  $p < 0.01$  respectively.  $N=722$  individuals are matched from the 2013 census to the 2009-11 panel data.  $N=383$  of these individuals are matched to the longer panel survey including preference modules. All possible dyads in which both individuals live in the same village are constructed,  $N=7,314$  for the general survey and  $N=1,641$  for the full survey including preference modules. Missing values reflect “do not know”, “not applicable”, or inconsistent answers in the case of risk preferences. All variables presented here were measured in the 2009 wave of the panel survey, except time preferences, which are taken from the 2010 survey wave since the 2009 wave did not include the far frame for females. Present-biased (future-biased) is a dummy equal to one if the response to the near frame is more impatient (patient) to the response to the far frame. Minimum switch-point is the lower bound of the interval in which the respondent switched to preferring the payment dated one month later compared to a 2000 MK payment on the earlier date. 150 MK  $\approx$  1 USD at the time of the 2009 and 2010 surveys. Patience above median is a dummy for having a switch-point below the median in that frame. Column (6) restricts the sample to “present-biased” and “future-biased” individuals, dropping “time-consistent” individuals. Column (7) restricts the sample to “present-biased” and “time-consistent” individuals, dropping “future-biased” individuals. Reported effects are marginal effects estimated at the mean.

## 6 Robustness

**Additional controls:** The coefficients on economic activity and present-bias are robust to the inclusion of a host of additional controls from the 2009-11 panel survey, as shown in table 11. Column (1) repeats the preferred specification shown in table 9 for reference. Column (2) also shows that the result of negative assorting on present-bias holds unconditional on occupation. Columns (3) and (4) show there is no evidence of sorting on being risk-neutral as compared to risk-averse, or indeed on the degree of risk-aversion. Columns (5) and (6) show that there is no evidence of sorting on measures of intra-household bargaining, either unconditionally or conditional on the measure of present-bias. This suggests that participants do not sort on “other-control” motives, in contrast to the strong sorting on “self-control” motives as proxied by present-bias.<sup>52</sup> Finally, column (7) shows that there is significant evidence of negative assorting on whether the individual comes from a household that speaks at village meetings — a proxy of how active or powerful the household is in local civil society. This is intuitive, as it is likely that certain “leaders” encourage other “followers” to join within each group. This does not however change the estimated effect of present-bias, suggesting the two effects are uncorrelated.

**Weighting:** As explained in section 3.2, the 2009-11 panel survey was enumerated on a random sample of the population in each of the villages in the survey area. However, this does not mean that the sub-set of 2013 members that can be matched back to the 2009-11 panel are a random sub-set of all 2013 members. Table 12 in appendix A.1 shows that the matched dyads are indeed slightly unrepresentative of the full population of 2013 dyads. However, table 13 shows that only the estimated coefficients on gender and female-headed household are significantly different across the matched sub-sample

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<sup>52</sup>The coefficients on the dyadic sums of “ever hides money from spouse” and “female HH decision-making power” are also insignificant (not shown). Thus there is not evidence that women with lower or higher household bargaining power join more groups conditional on being a member of at least one group — our best proxy of the strength of demand for VSLA participation. Other literature has examined effects of the square of household decision-making (Anderson and Baland, 2002). However, this does not make sense in a dyadic setting, as the sum or difference of two individuals’ squared decision-making has no natural interpretation.

Table 11: Dyadic regressions – effects of controls, matched sub-sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)
<b>Occupation</b>							
Same economic activity	0.181** (0.077)		0.204** (0.097)	0.207** (0.096)	0.150** (0.075)	0.154** (0.072)	0.181** (0.075)
<b>Absolute differences</b>							
Present-biased	0.166** (0.067)	0.164*** (0.063)	0.107* (0.055)	0.115** (0.050)		0.188** (0.077)	0.167*** (0.064)
Risk-neutral			0.016 (0.058)				
Risk aversion above median				-0.019 (0.039)			
Ever hides money from spouse					-0.006 (0.029)	-0.017 (0.027)	
Female HH decision-making power (index 0-8)					0.006 (0.011)	0.006 (0.012)	
HH important in village decisions (scale 1-6)							-0.038** (0.019)
HH speaks at village meetings							0.009 (0.031)
Wealth & income controls (abs. diffs)	✓	✓	✓	✓	✓	✓	✓
Demographic controls (abs. diffs)	✓	✓	✓	✓	✓	✓	✓
<b>Sums</b>	✓	✓	✓	✓	✓	✓	✓
<b>Village f.e.'s</b>	✓	✓	✓	✓	✓	✓	✓
Observations	1292	1292	1031	1031	980	958	1277
Pseudo $R^2$	0.222	0.212	0.214	0.216	0.194	0.206	0.228
Baseline predicted probability	0.296	0.296	0.285	0.285	0.294	0.295	0.292

Notes: \*, \*\* and \*\*\* represent  $p < 0.10$ ,  $p < 0.05$  and  $p < 0.01$  respectively. N=722 individuals are matched from the 2013 census to the 2009-11 panel data. N=383 of these individuals are matched to the longer panel survey including preference modules. All possible dyads in which both individuals live in the same village are constructed, N=7,314 for the general survey and N=1,641 for the full survey including preference modules. Missing values reflect “do not know”, “not applicable”, or inconsistent answers in the case of risk preferences. All variables presented here were measured in the 2009 wave of the panel survey, except time preferences, which are taken from the 2010 survey wave since the 2009 wave did not include the far frame for females. Present-biased (future-biased) is a dummy equal to one if the response to the near frame is more impatient (patient) to the response to the far frame. Risk-neutral is a dummy equal to one if the respondent prefers a 50-50 lottery to its expected value for certain, and thus could indicate risk-neutral or risk-seeking behaviour. Female HH decision-making power is constructed from questions over four types of economic decisions, scoring one if the female has some say in the decision and two if she has complete control. Reported effects are marginal effects estimated at the mean.



and the full sample (although they maintain the same sign and significance across both). Nevertheless, as a robustness check, we re-weight the estimations in order to estimate the effect sizes we hypothetically would have obtained if the matched sub-sample were a fully random sub-sample of all 2013 members. To construct the weights, we first estimate a probit equation for the probability of each dyad in the 2013 data also appearing in 2009-11 data, as function of the full set of dyad characteristics listed in table 6 and village fixed effects (results available on request).<sup>53</sup> We then use the estimated coefficients to generate the predicted probability that each 2013 dyad is matched to the 2009-11 data, and take the inverse of this predicted probability as the dyad-specific weight. We then re-run the various estimations shown in table 11 re-weighted by these weights.<sup>54</sup> Tables 14, 15 and 16 in appendix A.1 show that re-weighting slightly reduces the size of the estimated coefficients on occupation and present-bias, but never significantly so and they always maintain the same level of significance.

**Other robustness checks:** We run a set of further robustness checks on both the full sample and the matched sub-sample, results of which are available on request. Results do not significantly change if we restrict the samples to current members only (i.e. dropping any individuals who are listed in the 2013 member census as having been members but having left) and if we drop the eight groups which had disbanded in 2013.<sup>55</sup> Nor do the results significantly change if we restrict attention to dyads in which both members are female; thus the pattern of sorting holds irrespective of gender. If we split “same occupation” and separately estimate the effect of both being a farmer, or of both being engaged in business or trade, both coefficients are still highly positive and significant, and not significantly different across the two occupations. Thus the effect of “same occupation” is not being driven by farmers or non-farmers in particular.

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<sup>53</sup>This is not equivalent to the the product of the separate probabilities that individual  $i$  and individual  $j$  are matched to the 2009-11 data, because of differences in the number of groups per village and members per group, and hence in the number of dyads across villages and groups.

<sup>54</sup>To our knowledge this is the first use of inverse probability weighting in a dyadic specification. Such a strategy is valid since inverse probability weighting is valid for all M-estimators, of which logit models are a type.

<sup>55</sup>We still obtained the membership data for these disbanded groups, as we wanted to avoid a selected sample of groups when examining sorting.

For the sub-sample of matched individuals, we run additional estimations including further controls for income and food security from the 2009-11 panel (as described in table 3). We also re-estimate the main specifications using measures of individuals' occupations as recorded in the 2009 baseline rather than in the member census in 2013. The coefficients on same occupation and absolute difference in present-bias remain positive and highly significant. Finally, we re-run the estimations dropping anyone who never switched to the later amount in one or both time frames in the time preference elicitation activity — a potential sign of misunderstanding or lack of trust. The coefficient on difference in present-bias remains positive and highly significant.

## 7 Conclusion

This paper has highlighted the potential role of VSLAs and similar groups for financial intermediation. In particular, we have emphasised that VSLAs offer not only a source of credit but also a commitment savings technology. In light of this, we have asked whether individuals who have a demand for saving, and in particular commitment saving, successfully sort into groups with individuals who have a demand for borrowing. This issue has been overlooked in the literature on VSLA-type groups, but is likely to be crucial in determining the efficiency and equity of the financial outcomes that they generate for members.

The fact that we find positive, not negative assortative matching on occupation suggests that members may have high costs of screening, monitoring or enforcing loan repayment when other group members are from a different occupation, thus limiting the scope for VSLAs to provide intermediation across occupational groups. Furthermore, positive assortative matching on occupation implies that VSLA groups may be heavily exposed to common shocks. This could increase competition for emergency consumption loans after negative covariate shocks, and more generally could increase the chance that several borrowers will default at once. A possible solution may be to integrate VSLAs and similar groups into larger credit unions, or indeed to channel government funding or additional

funding from MFIs through such groups. The latter has been already achieved with Self-Help Groups in India, and more research is needed to understand whether and how this has been successful. Moreover, it is important to ask whether and how it has changed the nature of who participates, and how members sort across groups.

On the other hand, the fact that we observe negative assortative matching on a measure of “present-bias” suggests that VSLAs are already bringing together present-biased commitment savers with time-consistent individuals including prospective borrowers. This may enhance efficiency by reallocating capital from savers to borrowers. Nevertheless, there may be a concern about equity if present-biased individuals’ willingness to pay for commitment leads them to accept very low interest rates when lending out their savings to borrowers. There is nothing exploitative *per se* in allowing sophisticated present-biased individuals to pay for commitment, in this case by accepting a lower interest rate.<sup>56</sup> However, if savers were receiving a return of close to zero or even negative values, or were experiencing high rates of default from borrowers without apparently being compensated for that risk through a high loan interest rate, then intuitively this could be grounds for concern.<sup>57</sup>

Reassuringly, we see that monthly interest rates on lending are broadly in line with discount rates, and that savers earn substantial positive nominal interest on their savings. Thus it does not appear that commitment savers are paying a high price for commitment. However, the risk of loan defaults should also be accounted for in any welfare calculations. Moreover, a fuller assessment of the welfare implications of VSLA-type institutions — and indeed their cost-benefit implications as an intervention — would need to take into account the returns on the projects that these groups fund. These remain interesting topics for future research.

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<sup>56</sup>Heidhues and Koszegi (2010) show that lenders can exploit partial naifs. However, in the context of VSLAs such exploitation of partial naiveté is less of a concern, since there are no penalties for failing to meet the minimum share-purchase requirement each week.

<sup>57</sup>On the other hand, it is not clear what commitment savers’ outside option would be in the absence of VSLAs, and thus what the correct counterfactual is in assessing whether commitment savers are receiving “too low” an interest rate.

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# A Appendices

## A.1 Representativeness of matched sub-sample

Table 12: Test of representativeness – matched dyads vs. all 2013 dyads

	Mean All Dyads	Mean Matched Dyads	Diff.	Std. Error	T stat	Full N	Matched N
<b>Group membership</b>							
Same VSLA group	0.17	0.30	-0.13***	(0.01)	-24.27	289914	7309
<b>Occupation</b>							
Same economic activity	0.56	0.71	-0.15***	(0.01)	-27.11	289914	7309
<b>Absolute differences</b>							
Male	0.38	0.39	-0.02**	(0.01)	-2.89	289467	7309
Female-headed household	0.32	0.34	-0.03***	(0.01)	-4.64	288740	7309
Age	12.64	12.17	0.46***	(0.12)	3.94	286763	7309
Some post-primary educ.	0.33	0.26	0.07***	(0.01)	13.29	289914	7309
Literate (read & understand newspaper)	0.26	0.29	-0.03***	(0.01)	-5.12	288342	7309
Father well-off in village (scale 1-5)	1.41	1.42	-0.00	(0.01)	-0.12	253041	6415
Spouse's father well-off in village (scale 1-5)	1.43	1.40	0.03*	(0.01)	2.44	252485	6954
Household well-off in group (scale 1-9)	1.40	1.29	0.11***	(0.01)	8.31	284485	7309
HH owns a bicycle	0.46	0.46	0.01	(0.01)	1.21	288740	7309
# Goats	1.84	2.30	-0.45***	(0.04)	-11.14	288240	7309
<b>Sums</b>							
Farmer	1.37	1.61	-0.24***	(0.01)	-33.75	289914	7309
Businessperson	0.48	0.32	0.16***	(0.01)	24.67	289914	7309
Male	0.53	0.55	-0.02**	(0.01)	-2.68	289467	7309
Female-headed household	0.40	0.45	-0.05***	(0.01)	-6.69	288740	7309
Age	72.12	77.04	-4.92***	(0.19)	-25.52	286763	7309
Some post-primary educ.	0.43	0.31	0.11***	(0.01)	18.76	289914	7309
Literate (read & understand newspaper)	1.68	1.63	0.04***	(0.01)	6.87	288342	7309
Father well-off in village (scale 1-5)	6.76	6.64	0.12***	(0.02)	5.11	253041	6415
Spouse's father well-off in village (scale 1-5)	6.85	6.87	-0.02	(0.02)	-0.84	252485	6954
Household well-off in group (scale 1-9)	15.04	15.41	-0.37***	(0.02)	-18.06	284485	7309
HH owns a bicycle	0.99	1.09	-0.10***	(0.01)	-12.09	288740	7309
# Goats	2.44	3.32	-0.89***	(0.05)	-16.90	288240	7309

Notes: All variables presented were measured during the 2013 member census, N=3,801 members. N=722 of these members could be matched by name to the 2009-11 panel dataset. All possible dyads in which both individuals live in the same village are constructed for the full sample (N=289,914) and for the matched subsample (N=7,301). Missing observations reflect answers of “do not know” or “not applicable”. Occupation denotes an individual's primary economic activity, even if engaged in multiple activities.



Table 13: Dyadic regressions – full 2013 member census – full sample and matched subsample

	(1) Full CSAE sample Mfx / (s.e.)	(2) Matched subsample Mfx / (s.e.)	P-value $\beta_1 = \beta_2$
<b>Occupation</b>			
Same economic activity	0.086*** (0.008)	0.206*** (0.045)	0.132
<b>Absolute differences</b>			
Male	-0.053*** (0.015)	-0.034 (0.028)	0.054*
Female-headed household	-0.012*** (0.004)	-0.065*** (0.023)	0.034**
Age	-0.001*** (0.000)	-0.001 (0.001)	0.629
Some post-primary educ.	-0.008 (0.005)	-0.005 (0.021)	0.724
Literate (read & understand newspaper)	-0.013* (0.0067)	-0.009 (0.018)	0.417
Father well-off in village (scale 1-5)	-0.013*** (0.002)	-0.025*** (0.010)	0.577
Spouse's father well-off in village (scale 1-5)	-0.013*** (0.003)	-0.030** (0.012)	0.379
Household well-off in group (scale 1-9)	-0.010*** (0.003)	-0.020** (0.010)	0.659
HH owns a bicycle	-0.003 (0.003)	0.009 (0.009)	0.159
# Goats	-0.006*** (0.002)	-0.012*** (0.004)	0.407
<b>Sums</b>	✓	✓	
<b>Village f.e.'s</b>	✓	✓	
Observations	219747	5878	
Pseudo $R^2$	0.129	0.142	
Baseline predicted probability	0.161	0.168	

Notes: \*, \*\* and \*\*\* represent  $p < 0.10$ ,  $p < 0.05$  and  $p < 0.01$  respectively. All variables presented were measured during the 2013 member census,  $N=3,801$  members.  $N=722$  of these members could be matched by name to the 2009-11 panel dataset. All possible dyads in which both individuals live in the same village are constructed for the full sample ( $N=289,914$ ) and for the matched subsample ( $N=7,301$ ). Missing observations reflect answers of “do not know” or “not applicable”. Occupation denotes an individual's primary economic activity, even if engaged in multiple activities. Reported effects are marginal effects estimated at the mean.

Table 14: Dyadic regressions – full 2013 member census

	(1)
	<b>Matched subsample</b>
	Mfx / (s.e.)
<b>Occupation</b>	
Same economic activity	0.098*** (0.035)
<b>Absolute differences</b>	
Present-biased	0.096*** (0.032)
Male	0.000 (0.014)
Female-headed household	-0.094*** (0.032)
Age	-0.004*** (0.001)
Some post-primary educ.	-0.013 (0.025)
Literate (read & understand newspaper)	-0.035 (0.028)
Father well-off in village (scale 1-5)	-0.009 (0.006)
Spouse's father well-off in village (scale 1-5)	-0.016** (0.007)
Household well-off in group (scale 1-9)	-0.027*** (0.008)
HH owns a bicycle	0.008 (0.016)
# Goats	-0.005 (0.004)
<b>Sums</b>	✓
<b>Village f.e.'s</b>	✓
Observations	1280
Pseudo $R^2$	0.280
Baseline predicted probability	0.197

Notes: \*, \*\* and \*\*\* represent  $p < 0.10$ ,  $p < 0.05$  and  $p < 0.01$  respectively.  $N = 722$  individuals are matched from the 2013 census to the 2009-11 panel data.  $N = 383$  of these individuals are matched to the longer panel survey including preference modules. All possible dyads in which both individuals live in the same village are constructed,  $N = 7,314$  for the general survey and  $N = 1,641$  for the full survey including preference modules. Missing values reflect “do not know”, “not applicable”, or inconsistent answers in the case of risk preferences. Time preferences are taken from the 2010 survey, wave since the 2009 wave did not include the far frame for females. Present-biased (future-biased) is a dummy equal to one if the response to the near frame is more impatient (patient) to the response to the far frame. Reported effects are marginal effects estimated at the mean.

Table 15: Dyadic regressions – time preference measures, matched sub-sample, weighted

	(1)	(2)	(3)	(4)	(5)
	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)
<b>Occupation</b>					
Same economic activity	0.098*** (0.035)	0.093** (0.038)	0.098*** (0.034)	0.102*** (0.034)	0.097*** (0.035)
<b>Absolute differences</b>					
Present-biased	0.096*** (0.032)		0.099*** (0.031)	0.097*** (0.030)	0.096*** (0.032)
Future-biased					0.002 (0.015)
Minimum switch-point, near frame		0.000 (0.000)	0.000 (0.000)		
Minimum switch-point, far frame		0.000 (0.000)	-0.000 (0.000)		
Patience above median, near frame				0.015 (0.017)	
Patience above median, far frame				-0.022 (0.016)	
Wealth & income controls (abs. diffs)	✓	✓	✓	✓	✓
Demographic controls (abs. diffs)	✓	✓	✓	✓	✓
<b>Sums</b>	✓	✓	✓	✓	✓
<b>Village f.e.'s</b>	✓	✓	✓	✓	✓
Observations	1280	1280	1280	1280	1280
Pseudo $R^2$	0.280	0.272	0.282	0.286	0.281
Baseline predicted probability	0.197	0.196	0.197	0.197	0.197

Notes: \*, \*\* and \*\*\* represent  $p < 0.10$ ,  $p < 0.05$  and  $p < 0.01$  respectively.  $N=722$  individuals are matched from the 2013 census to the 2009-11 panel data.  $N=383$  of these individuals are matched to the longer panel survey including preference modules. All possible dyads in which both individuals live in the same village are constructed,  $N=7,314$  for the general survey and  $N=1,641$  for the full survey including preference modules. Missing values reflect “do not know”, “not applicable”, or inconsistent answers in the case of risk preferences. All variables presented here were measured in the 2009 wave of the panel survey, except time preferences, which are taken from the 2010 survey wave since the 2009 wave did not include the far frame for females. Present-biased (future-biased) is a dummy equal to one if the response to the near frame is more impatient (patient) to the response to the far frame. Minimum switch-point is the lower bound of the interval in which the respondent switched to preferring the payment dated one month later compared to a 2000 MK payment on the earlier date.  $150 \text{ MK} \approx 1 \text{ USD}$  at the time of the 2009 and 2010 surveys. Patience above median is a dummy for having a switch-point below the median in that frame. Reported effects are marginal effects estimated at the mean.

Table 16: Dyadic regressions – effects of controls, matched sub-sample, weighted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)	Mfx / (s.e.)
<b>Occupation</b>							
Same economic activity	0.098*** (0.035)		0.117** (0.049)	0.119** (0.050)	0.088** (0.038)	0.094*** (0.035)	0.098*** (0.035)
<b>Absolute differences</b>							
Present-biased	0.096*** (0.032)	0.093*** (0.028)	0.052** (0.022)	0.057** (0.023)		0.117*** (0.035)	0.096*** (0.031)
Risk-neutral			0.026 (0.036)				
Risk aversion above median				-0.020 (0.015)			
Ever hides money from spouse					-0.002 (0.019)	-0.005 (0.015)	
Female HH decision-making power (index 0-8)					0.005 (0.007)	0.003 (0.007)	
HH important in village decisions (scale 1-6)							-0.013 (0.009)
HH speaks at village meetings							-0.001 (0.012)
Wealth & income controls (abs. diffs)	✓	✓	✓	✓	✓	✓	✓
Demographic controls (abs. diffs)	✓	✓	✓	✓	✓	✓	✓
<b>Sums</b>	✓	✓	✓	✓	✓	✓	✓
<b>Village f.e.'s</b>	✓	✓	✓	✓	✓	✓	✓
Observations	1280	1280	1021	1021	969	947	1265
Pseudo $R^2$	0.280	0.264	0.270	0.271	0.242	0.268	0.285
Baseline predicted probability	0.197	0.191	0.189	0.189	0.193	0.197	0.194

Notes: \*, \*\* and \*\*\* represent  $p < 0.10$ ,  $p < 0.05$  and  $p < 0.01$  respectively.  $N = 722$  individuals are matched from the 2013 census to the 2009-11 panel data.  $N = 383$  of these individuals are matched to the longer panel survey including preference modules. All possible dyads in which both individuals live in the same village are constructed,  $N = 7,314$  for the general survey and  $N = 1,641$  for the full survey including preference modules. Missing values reflect “do not know”, “not applicable”, or inconsistent answers in the case of risk preferences. All variables presented here were measured in the 2009 wave of the panel survey, except time preferences, which are taken from the 2010 survey wave since the 2009 wave did not include the far frame for females. Present-biased (future-biased) is a dummy equal to one if the response to the near frame is more impatient (patient) to the response to the far frame. Risk-neutral is a dummy equal to one if the respondent prefers a 50-50 lottery to its expected value for certain, and thus could indicate risk-neutral or risk-seeking behaviour. Female HH decision-making power is constructed from questions over four types of economic decisions, scoring one if the female has some say in the decision and two if she has complete control. Reported effects are marginal effects estimated at the mean.