Institute for Financial Management and Research

Centre for Micro Finance

Working Paper

February 2011

The Economic Returns to Social Interaction

Experimental Evidence from Microfinance

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February 10, 2011

Abstract

We exploit random variation in the meeting frequency of microfinance groups during their first loan cycle to show that more frequent meeting is associated with long-run increases in social interaction and lower default. Relative to clients who met on a monthly basis during their first loan, those who met weekly are three and a half times less likely to default on their subsequent loan. Experimental and survey evidence suggests that the decline is driven by improvements in informal risk-sharing that result from more frequent social interaction outside of meetings. These findings constitute the first experimental evidence on the economic returns to social interaction, and provide evidence on an alternative theory for the success of the classic group lending model in reducing default risk.

JEL: C81,C93, O12, O16

*The authors are from MIT (Feigenberg) and Harvard University (Field and Pande). We thank Emmerich Davies, Sean Lewis Faupel, Sitaram Mukherjee and Anup Roy for superb field work and research assistance, Alexandra Cirone and Gabe Scheffler for editorial assistance and Village Welfare Society and Center for Micro-Finance for hosting this study, Theresa Chen, Annie Duflo, Nachiket Mor and Justin Oliver for enabling this work and ICICI, Exxon Mobil Educating Women and Girls Initiative (administered through WAPP/CID at Harvard) and the Dubai Initiative for financial support. We also thank Attila Ambrus, Abhijit Banerjee, Tim Besley, Amitabh Chandra, Esther Duflo, Raquel Fernandez, Seema Jayachandran, Dean Karlan, Dominic Leggett, Muriel Niederle, Aloysius Sioux, Jesse Shapiro and numerous seminar participants for helpful comments.

1 Introduction

Social capital, famously defined by Putnam (1993) as "features of social organization, such as trust, norms and networks, that can improve the efficiency of society by facilitating coordinated actions," is thought to be particularly valuable in low-income countries where formal insurance is largely unavailable and institutions for contract enforcement are weak.[1] Since economic theory suggests that repeat interaction among individuals can help build and maintain social capital, encouraging interaction may be an effective tool for development policy. Indeed, numerous development assistance programs emphasize social contact among community members under the assumption of significant economic returns to regular interaction. But can simply inducing individuals to interact more often actually facilitate economic cooperation?

Rigorous evidence on the economic returns to social interaction remains limited, largely due to the difficulty of accounting for endogenous social ties (Manski, 1993, 2000). For instance, if more trustworthy individuals or societies are characterized by denser social networks, we cannot assign a causal interpretation to the positive association between community-level social ties and public goods provision. Neither, in this case, can we assign a causal interpretation to the higher levels of cooperation observed among friends relative to strangers in laboratory public goods games.[2] In short, without randomly varying social distance, it is difficult to validate the basic model of returns to repeat interaction and even harder to determine whether small changes in social contact can produce tangible returns.

The first contribution of this paper is to undertake exactly this exercise. By randomly varying how often individuals meet, we are able to provide the first rigorous evidence

[1]Consistent with this idea, Guiso et al. (2004) demonstrate that residents in high social capital regions engage in more sophisticated financial transactions, and Knack and Keefer (1997) show that a country's level of trust correlates positively with its growth rate.

[2]The public good provision and community ties literature includes Costa and Kahn (2003); Alesina and La Ferrara (2002); DiPasquale and Glaeser (1999); Miguel et al. (2005); Olken (2009), while examples of laboratory games include Glaeser et al. (2000); Carter and Castillo (2004b); Do et al. (2009); Karlan (2005); Ligon and Schecter (2008)

of economic returns to repeat social interaction.[3] We do so in the context of a development program that emphasizes group interaction: microfinance.[4] In the typical "Grameen Bank"-style microfinance program, clients meet weekly in groups to make loan payments. Our experiment introduced exogenous variation in social interaction by randomly assigning one hundred first-time borrower groups of a typical microfinance institution (MFI) in India to meet either once per week (weekly groups) or once per month (monthly groups) throughout their ten-month loan cycle. Using administrative and survey data collected more than a year after the experiment ended, we then study the effect of short-run increases in mandatory group meetings on long-run social contact between individual group members and on subsequent financial transfers and rates of default on future loans.

By linking changes in interaction to changes in default, our second contribution is to shed new light on the mechanism through which the classic microfinance loan contract reduces credit risk. In particular, microfinance has had remarkable success achieving very high repayment rates on collateral-free loans to poor individuals, as recognized for instance by the awarding of the Nobel Peace Prize to the founder of the Grameen Bank. The key to mitigating default risk in microfinance is almost universally emphasized to be the use of joint-liability contracts. However, recent experimental evidence suggests that joint liability *per se* has little impact on default (Gine and Karlan, 2009), raising anew the question of how exactly group lending achieves risk reduction without collateral.

Since the clients in our experiment were on individual-liability debt contracts, our study provides direct evidence that a lesser noted feature of the classic group lending contract – encouraging social interaction – that has been ignored in theoretical models of group lending is actually responsible for reducing default.[5] In other words, our results

[3]Dal Bo (2005) provides rigorous experimental evidence of returns to repeat *economic* interaction, in which the likelihood of future rounds of exchange is randomly assigned, albeit in the context of a laboratory experiment.

[4]In a similar spirit, Humphreys et al. (2009) use village-level randomization to show that community development programs encourage pro-social behavior, but are unable to isolate the influence of social interaction.

[5]Our findings complement existing work on microfinance, which has identified a correlation between social connections and default risk (Besley and Coate, 1995; Ghatak and Guinnane, 1999; Karlan, 2005). For instance, MFI clients in Peru who are more trustworthy in a trust game are less likely to default, and group-level default is lower in groups where clients have stronger social connections (Karlan, 2005, 2007).

show that, even absent the explicit incentives for monitoring and enforcement that joint liability provides, frequent group meetings can lower lending risk by increasing social contact among group members and, as a consequence, the risk-sharing that occurs within social networks.

Our empirical evidence takes the form of two striking changes in client behavior that resulted from experimentally increasing the frequency of client contact. First, clients assigned to weekly groups during their first loan cycle increased social contact with other group members outside of meetings which they maintained for more than a year after the experiment ended. In the long run, clients who had met on a weekly basis saw each other 26% more often outside of group meetings, and the gains were concentrated among those who did not know each other well before joining the group but had the ability to sustain social contact via extended family networks or geographic proximity.[6]

Second, while clients in both groups were equally likely to continue borrowing, those who met weekly during their first loan cycle were 3.5 times (7.8 percentage points) less likely to default on their second loan, despite the fact that all clients had by that time reverted to the same repayment schedule. Furthermore, identical to the patterns of change in social contact, default reductions were concentrated among weekly clients who were grouped in their first loan cycle with individuals with whom they had the ability to sustain social interaction but only weak social ties before joining the MFI. Since this feature of group composition predicts differences in social contact but does not directly predict default, the observed pattern indicates that reductions in default risk are causally related to greater social contact, presumably through increases in clients' willingness to insure one another against income shocks.[7] Together, these patterns indicate that increases in

Moreover, the finding in Gine and Karlan (2009) that a shift from joint to individual liability did increase default among borrowers with ex-ante weak social ties highlights the importance of an intervention aimed at promoting social ties.

[6]Our findings on the relevance of geographic proximity and kinship ties are consistent with results reported in the risk-sharing network literature (Fafchamps and Gubert, 2007).

[7]If weekly meetings, instead, lowered default risk only through channels unrelated to social contact such as habit formation (e.g. helping a client develop fiscal discipline), then the effect of meeting frequency on default would be independent of group composition, or at least unlikely to depend on exactly the same features of group composition that affect changes in social contact.

social contact led to long-run improvements in risk-sharing arrangements.

The remainder of the paper presents direct evidence on improvements in risk-sharing to substantiate these claims. First, consistent with our interpretation, clients required to meet more frequently during their first loan were 29% more likely to report financial transfers to friends and relatives outside of their immediate family at the end of the first loan cycle.

Next, we studied clients' willingness to share risk with one another through a field-based lottery game conducted among a random sample of the original study clients roughly sixteen months after the first loan cycle ended. The lottery operated much like a laboratory trust game but in a less artificial setting so as to avoid triggering subjects' awareness that they were participating in an experiment. Each client was informed that she had been entered into a (separate) promotional lottery for the MFI's new retail store as a means of thanking her for participating in the first loan experiment. The client started with a 1 in 11 chance of winning the lottery prize, a voucher redeemable at the MFI store. She was then offered the opportunity to give out additional lottery tickets to any number of members of her first loan group.

Since giving out additional tickets would reduce her individual chances, but increase the probability that someone from the group would win, a client's willingness to give tickets captured either her unconditional altruism towards or willingness to risk-share with members of her initial MFI network. To distinguish insurance motivations from unconditional altruism, we randomized the lottery prize to take the form of either one Rs. 200 voucher or four Rs. 50 vouchers. Assuming the more easily divisible prize is perceived as more conducive to sharing, a client should give more tickets when the prize is divisible if she is motivated at least in part by risk-sharing considerations, but should not if her sole motivation is unconditional altruism.[8]

Relative to a monthly client, a client who had been assigned to a weekly group two

[8]Similar variations of dictator or trust games have been used to parse out motives for giving (Ligon and Schecter, 2008; Do et al., 2009; Carter and Castillo, 2004a). Closest to us is Gneezy et al. (2000), who use a sequence of trust games with varying constraints on the amount that can be returned to show that individuals contribute more when large repayments are feasible.

years prior was 48% more likely to enter a group member into the lottery when the prize was divisible, but no more likely when it was not. Furthermore, increased ticket-giving by weekly clients was driven by higher rates of giving to close neighbors and extended family, consistent with the observed patterns of change in social contact and reductions in default.

Finally, we examine whether the length of time for which social contact is increased matters. If social contact encouraged risk-sharing by improving the ability of clients to implement punishment-and-reward schemes that prevent opportunistic behavior, then risk-sharing should be higher among group members who had greater social contact both in the short *and* in the long run. However, if the primary channel of influence is learning about each other's type (e.g. trustworthiness), then short-run increases in social contact should suffice. To distinguish these two mechanisms, we again randomized meeting frequency for a subset of the clients in a later loan cycle. At this point, the group members were well acquainted after more than two years together, yet higher meeting frequency again led to greater reciprocity. This suggests that, in addition to any short-run learning effects, social interactions help sustain risk-sharing in the long run.

Taken together, our findings not only substantiate theoretical claims that repeat interaction has the potential to yield economic returns by facilitating informal economic exchange, but also provide an alternative explanation for the success of the classic group lending model. More broadly, they demonstrate that development programs can readily generate economically valuable social capital through simple changes in program design, at least in the context of a financial intervention such as microfinance.

The rest of this paper is structured as follows: Section 2 describes the meeting frequency experiment and Section 3 examines how randomized differences in meeting frequency implemented only during the first loan cycle influenced long-run social interaction and default behavior. In Section 4 we use data on transfers and client willingness to share with other group members in the field-based lottery to provide direct evidence of improved risk-sharing arrangements, and Section 5 concludes.

2 Setting and Experimental Design

Our partner MFI, the Village Welfare Society (VWS), started operations in the Indian state of West Bengal in 1982. At the start of our field experiments, it had 6.75 million dollars in outstanding loans to over 56,000 female clients in impoverished urban and peri-urban neighborhoods.

For the experiment, between April and September 2006 we recruited 100 ten-member groups of first-time clients from neighborhoods in the catchment areas of three VWS branches.[9] Following VWS protocol, the loan officer first surveyed the neighborhood and then conducted a meeting to inform potential clients about the VWS product. Interested women were invited for a five-day training program, where clients met for an hour each day and learned about the benefits and responsibilities of the loan. At the end of the five days, the loan officer assigned women into groups of ten, identified (with group members) a leader, and offered each group member an individual-liability loan. Thus, clients in a single loan group live in close proximity and are typically acquainted prior to joining. However, while 63% of group members in our sample knew one another at group formation, most described their relationship with other group members as neighbors (86%) rather than friends (5%) or family (8%).

At the time of group formation, clients who entered our experiment were told that they would meet and repay either weekly or monthly, and their meeting schedule would be determined by lottery prior to loan disbursal. Once groups were finalized, we randomly assigned 30 groups to the standard weekly repayment schedule and 70 groups to a monthly repayment schedule.[10] Each client received a Rs. 4,000 (~\$100) loan, a reasonably large amount given that the average client had assets worth \$250 at baseline. Clients assigned to the weekly schedule were required to repay their loans through 44 weekly installments

[9] Loan officers aimed to form ten-member groups. In practice, group size ranged between eight and thirteen members, with 77% of the groups consisting of ten members.

[10] We intended to have two monthly repayment treatment arms: one that met monthly and one that met weekly but only repaid monthly. However, very low attendance in the weekly meetings led us to stop the weekly meetings so that all monthly repayment groups met monthly for over 90% of their loan cycle. Hence, we include both groups as monthly repayment.

of Rs. 100 starting two weeks after loan disbursal, and those assigned to the monthly schedule in eleven Rs. 400 installments starting one month after loan disbursal. Clients could repay early only after 20 weeks and if they chose to do so they would have to repay the entire amount outstanding in one installment. No client dropped out after her repayment schedule was announced. Overall, the median weekly group met 37 times during a single loan cycle and the average meeting length was 25 minutes (excluding waiting time). During each meeting clients took an oath promising to make regular repayment, after which the loan officer collected payment from each member individually and marked passbooks.[11]

Between group formation and loan disbursement, we administered a baseline survey to 1016 of the 1026 clients. Table 1 provides a randomization check using client characteristics at baseline. Panel A presents variables which are included in regression specifications which include covariates and Panel B presents additional variables. On average, monthly and weekly clients look similar at baseline across a wide range of observable characteristics. Only two out of 24 differences are statistically significant: whether a client is Muslim and the number of years she has lived in her neighborhood. While monthly clients have been in their neighborhoods for slightly longer, the difference is relatively small and not associated with differences in degree of social ties between clients at baseline. For instance, monthly clients were no more likely to be in groups with extended family members or neighbors, nor to claim they did not know another group member prior to joining VWS (Table 1). However, as a further robustness check, throughout we report regressions with and without the controls listed in Table 1. We have also verified that the results are robust to excluding groups with Muslim clients (unreported).

[11]While the oath encourages group responsibility for loans, the loan contract is explicitly individual liability. During meetings, a client's repayment behavior is observable to other group members, although, in practice most clients socialize while awaiting their turn. Finally, once a majority of clients in a group have repaid their entire loan, remaining clients repay at the branch office (repayment in branch office is, otherwise, rare).

3 Effect of Meeting Frequency on Client Behavior

Our study tracks clients for two and a half loan cycles (roughly 100 weeks) beginning in April 2006. Figure 1 provides a detailed timeline.

3.1 Change in Social Interaction

We first study whether requiring clients to meet more often during their first loan cycle led them to develop friendships that outlasted the experiment and thereby changed a client's social network.

To gauge short-run changes in social interaction, loan officers collected data at each meeting during the first loan cycle. The data-collection protocol was as follows: after marking passbooks, loan officers pulled each client aside and asked her broad questions about her social ties with other group members, including whether she had ever interacted with each member of her group outside of meetings. Next, to examine whether these changes persisted beyond the experiment, we collected data on a client's current contact with each member of her first loan cycle. These data were collected for a random sample of 432 clients roughly 16 months after they had repaid their first loan. Our primary measure of long-run social interaction is the number of times over the last thirty days the client had visited a previous group member in either person's home (outside of loan repayment meetings).

To construct our short-run outcome variable y_{gi} for client i in group g, we take the maximum value of her response to the two survey questions – "Have all of your group members visited your house?" and "Have you visited the houses of all group members?".[12] We take the maximum over her responses in all meetings held during the first five months

[12]Since responses were potentially observable to others in the group, we sought to preserve anonymity by not asking clients to report on interactions with specific group members. One concern is that collecting data on social interactions directly encouraged friendship formation and the treatment/control difference is driven by more frequent surveying of clients in weekly groups. While we cannot completely exclude this channel, we assume it is second order, particularly given that questions on social interactions were framed in a neutral light and in both treatment arms the loan officer asked clients these questions at every meeting. Supportive evidence is provided by data from the third loan cycle (described later) where we asked survey questions at the same frequency (monthly) for weekly and monthly clients and continued to see greater increases in social contact among weekly groups.

of the loan cycle (as clients repaying loans before the due date begin to repay after five months). Therefore, we have one observation for every client and we estimate a regression of the form

$$y_{gi} = \beta W_g + X_{gi}\gamma + \epsilon_{gi} \tag{1}$$

where W_g is an indicator for weekly repayment schedule and X_{gi} represent individual covariates.

Our survey on long-run social interactions asked client i about her interaction with each of her first loan-cycle group members, m. On average we have nine observations per client giving us an analysis sample of 4,018 pairwise observations. To avoid double-counting, in cases in which we interviewed both members of a pair, we randomly drop one observation (since social contact cannot vary, in the absence of measurement error, within a pair), leaving 3,137 pairwise observations.[13] For member i matched with group member m in group g we estimate

$$y_{gmi} = \beta W_g + X_{gi}\gamma + \epsilon_{gmi} \tag{2}$$

where the outcomes are defined as in Equation (1).

Next, to examine how changes in social interaction varied with baseline social distance between pair members, we define four social distance categories for each pair, as measured at baseline: (i) immediate family and friends; (ii) relatives more than once removed (distant relatives); (iii) neighbors who are neither friends nor relatives and live within the first quintile of distance between clients' houses (close neighbors); or (iv) neighbors who are neither friends nor relatives and live outside of the first quintile of distance (distant neighbors).[14] We then estimate:

$$y_{gmi} = \beta W_g + \psi \sum_{a=1,..3} W_g \times S_{agmi} + S_{agmi}\phi + X_{gi}\gamma + \epsilon_{gmi} \tag{3}$$

[13]Our results are robust to averaging across observations within a pair.

[14]Distances were measured using GPS coordinates collected at baseline. The first quintile (within 60 meters) is roughly two-thirds of the distance used to define city blocks in developed countries (100 meters), so close neighbors can be thought of as those living more or less on the same block.

where S_{agmi} is an indicator variable for the (i, m) pair belonging to social distance category a (the omitted group is immediate family and friends). Here, the X_{gi} vector excludes the group composition covariates (i.e. number of group members belonging to different social distance categories).

We cluster standard errors by group. Individual-specific factors common to all observations involving a single member imply that observations in a pairwise (dyadic) regression are not independent (Fafchamps and Gubert, 2007). Furthermore, the structure of the error covariance matrix may exhibit correlations varying in magnitude across group members. Clustering standard errors at the group level (which subsumes individual-level clustering) accounts for this potential pattern. Specifically, with roughly equal sized clusters, if the covariate of interest is randomly assigned at the cluster level, then only accounting for non-zero covariances at the cluster level, and ignoring correlations between clusters, leads to valid standard errors and confidence intervals (Barrios et al., 2010).[15]

Table 2 presents the results. Columns (1) and (2) show very large differences in the degree of social contact between group members assigned to the monthly and weekly schedules during the experimental loan cycle itself. While only 10% of monthly clients have had social contact outside of group meetings with all other group members by the end of the loan cycle, almost 100% of weekly members report having met socially with all other group members by the same point. The result is highly significant and robust to the inclusion of demographic controls.

Columns (3) and (4) provide suggestive evidence that clients who met weekly in their first loan cycle were also significantly more likely to interact than their monthly counterparts more than a year after the experiment ended. While noisy, the point estimates in column (3) suggest that the average weekly client pair met 20% more often than their

[15]We conducted two other robustness checks. First, following Fafchamps and Gubert (2007) we checked that our regressions are robust to allowing for spatial correlation of standard errors instead of group clustering. Second, we checked that the main findings for regressions where we examine heterogeneity by initial social distance between a pair (Equation 3) are robust to inclusion of an individual fixed effect. For instance, in Table 2, the interaction between weekly meeting and distant relative retains significance at the 12% level, and the interaction between weekly meeting and close neighbor retains significance at the 11% level.

monthly counterpart (0.88 times more per 30 days). This effect is significant at the 10% level only after including covariates. However, when we allow these effects to differ by initial social distance (columns (5) and (6)), we find large and statistically significant increases in the frequency of social interaction among client pairs that were on the weekly schedule and shared either geographic or social proximity (i.e. were either close neighbors or distant relatives). Moving from monthly to weekly meeting closes the entire gap in level of interaction between distant relatives and close family and halves it in the case of close neighbors.

The data thus show heterogenous treatment effects across different client pairs. As we would expect, being required to see one another for half an hour once per week did not change social interaction among immediate family and those described as friends at baseline (the omitted group). We also observe no significant change among clients with few means of sustaining a social connection outside of group meetings - geographically distant neighbors. Given that clients live in fairly close geographic proximity, this pattern suggests that existing social capital can be strengthened by more frequent meetings, but cannot easily be built from scratch. Consistent with this, while the average effects (columns (3) and (4)) are not robust to the exclusion of controls, the more precise specification is statistically significant and almost identical with and without controls.

We focus on social interactions as our primary measure of social ties because it relates most directly to the economic theory literature. However, both our short- and long-run surveys collected data on a number of alternative measures of the strength of social ties between clients. In the short-run survey these measures included whether the client knew the names of her group members' immediate family and whether she knew if group members had relatives visit over the previous month. In the long-run survey they included whether she talked to a specific group member about family, whether she would ask this person for help in the event of a health emergency, and whether they celebrated the main Bengali festival (Durga Puja) together during the previous year. Hence, as a robustness check, we also compute the average effect size across all measures of social ties within an index, which we call the short-run and long-run social contact indices. This index is normalized to have a mean 0 for the control group.[16]

As shown in Appendix Table 2, moving from a monthly to a weekly schedule led to a 3.07 standard deviation increase in short-run social contact (columns (1) and (2)) and a 0.16 standard deviation increase in long-run social contact (columns (3) and (4)) between members of the same first loan group. The results are similar in magnitude with and without controls. The coefficient estimates on the interactions with social distance are qualitatively similar but noisy.

3.2 Returns to Social Interaction: Impact on Loan Default

Our results indicate that requiring clients to meet more often for six to ten months led to a persistent increase in social contact. But did these interactions yield economic returns or simply change patterns of friendship? Here, we directly examine whether weekly meetings during a client's first loan cycle were associated with reductions in one indicator of economic vulnerability that is carefully measured and observed in our sample for an extended period: loan default.[17]

We use VWS transactions data to track repayment behavior during both the first (experimental) and second loan cycles. In both cases, we examine whether default rates varied with the meeting frequency a client was assigned in her *first* loan cycle. At the end of our loan experiment (clients' first loan cycle), 69% of clients took out a second loan with VWS. On average, the second loan was 35% larger than the first due to the fact that bank policy has clients start well below credit demand and graduate slowly to

[16] Aggregating the list of outcomes into such an index avoids inference based on selected outcomes. The summary index Y is defined to be the equally weighted average of z-scores of its components, with the sign of each measure oriented so that more beneficial outcomes have higher scores. The z-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation. For a discussion of the merits of such an index, see Kling et al. (2007). Each survey question that enters the index provides a measure of social contact, and is always reported as higher by weekly clients (Feigenberg et al., 2010). For instance, more than a year after our experiment ended, weekly clients were 29% more likely to say that they would ask a former group member for help in the event of an emergency, a result which is statistically significant at the 5% level.

[17] While default reflects more than just vulnerability to shocks, the less informal insurance an individual has access to, the more likely she is to default in the event of a shock (Besley and Coate, 1995). We find evidence of this in a basic regression analysis of determinants of default among our clients, in which transfers significantly reduces the likelihood of default (Appendix Table 1).

larger loans. In the second loan cycle, all clients repaid their second loan on a fortnightly schedule.[18]

We consider a client in default on either loan if she failed to repay in full 44 weeks after the loan was due (roughly the length of an additional loan cycle).[19] Across loan cycles, increases in loan size were accompanied by significant increases in default rates, presumably due to the fact that, as loan sizes increase, clients approach their debt-carrying capacity (on this correlation see, for instance, Adams et al. (2009)). Average default rates increased from 1.3% in the first loan cycle to 8.3% in the second loan cycle.

For client i who belonged to group g in her first loan cycle we estimate the following regression for loan cycle l:

$$y_{gil} = \beta W_g + X_{gi}\gamma + \alpha_{lg} + \epsilon_{gli} \tag{4}$$

As before, W_g is an indicator variable for the client being on a weekly schedule in a first loan cycle X_{gi} is a vector of individual characteristics reported in Panel A, Table 1. α_{lg} is a loan officer fixed effect (for loan cycle l). [20]

Our results on long-run social interaction suggest that default may also be sensitive to group composition. Specifically, features of group composition that are correlated with stronger social ties between a client and her group members may reduce the incidence of default. Motivated by the results in Table 2 we construct two measures of group composition: number of distant relatives or close neighbors in group and number of distant relatives in group (the omitted relationship type is close family and friends) and estimate:

$$y_{gil} = \beta W_g + \psi_1 W_g \times G_{df} + \psi_2 W_g \times G_{cn} + X_{gi}\gamma + \alpha_{lg} + \epsilon_{gli} \tag{5}$$

[18]While there is some variation in loan amount in the second loan, second loan size is uncorrelated with first loan repayment schedule; results available from authors.

[19]Results are very similar if we vary the time period over which default is defined. Note that we are not picking up variation in rates of delinquency within the loan cycle with this measure.

[20]Very low default rates imply we can estimate OLS but not Probit regressions for the first loan cycle. For the second loan cycle we have estimated Probit regressions (with no fixed effects) and find identical results, available from authors.

where G_{df} is number of distant family members in first loan group and G_{cn} is the number of close neighbors in first loan group. These group composition variables also enter the X_{gi} vector.[21]

Columns (1) to (3) of Table 3 show that frequent meetings did *not* significantly lower default in the first loan cycle. An important caveat is the low overall default for first-time borrowers (1.8% among monthly clients), which is unsurprising given small loan sizes. However, once clients had graduated to larger loans (columns (4)-(6)), differences in default emerged despite the fact that all clients had by that point converged to the same meeting frequency. Columns (4) and (5) show that clients assigned to monthly meetings for their first loan were 3.54 times (7.75%) more likely to default on their *second* loan relative to clients assigned to weekly meetings for their first loan, and the difference is statistically significant with or without controls. Furthermore, default reductions were concentrated among the weekly clients most likely to experience long-run gains in social interaction. In particular, there was a large and significant effect of weekly meeting on default only among clients with a sufficient number of close neighbors or distant relatives in their group (column (6)).

The systematic variation in default incidence among weekly clients by group composition is evidence that default patterns are not driven entirely by the direct effect of meeting frequency on client behavior. Potential direct effects include long-run income changes driven by investment choices that are influenced by meeting frequency or changes in long-run savings habits (Fischer and Ghatak, 2010). However, if the only channel of influence was the meeting schedule itself, we would not expect default rates to differ systematically by features of group composition, such as the number of group members who live in close proximity, that do not directly predict default.[22] Hence, we interpret the findings in Table 3 as *prima facie* evidence that meeting more frequently helped clients build stronger social ties and then leverage these social ties to maintain repayment.

[21] For expositional ease, Table 3 reports the regressions with controls. The results are almost identical in magnitude and significance without controls.

[22] Nor do we find any direct evidence of differences between treatment groups in savings or income at follow-up (unreported).

One complication in interpreting the default results is that, as one would expect, meeting frequency also appears to have influenced whether a client continued on to a second loan. Average continuation rates were not significantly different across treatment arms (columns (7)- (8)) and a comparison of observable characteristics among clients who continued on to the next loan reveals no differences in the nature of client selection (Table 1, column (4)). However, consistent with our default results, the likelihood of continuing on to the second loan cycle was significantly higher for those weekly clients that experienced a significant gain in social capital (column (9)).

While this is reassuring in that it provides further evidence that social capital accumulation influences repayment capacity, any form of differential selection according to treatment assignment also complicates interpretation of the default results in columns (4)-(6). However, it is important to note that, in this case the nature of selection effects biases *downward* rather than upward the estimated effect of first loan meeting frequency on second loan default. That is, since our experiment encouraged additional clients in the treatment group to take on debt when they otherwise would not have – hence, presumably those on the margin of defaulting in the absence of sufficient network support –, if anything our default results underestimate the true effect of social ties on default.

4 Evidence of Improvements in Risk-Sharing

Our previous results reveal that inducing social interaction among MFI clients has a causal effect on reducing long-run default risk. In an individual-liability setting such as our experiment, the most direct channel through which increased social interaction can improve financial outcomes such as default is improvements in risk-sharing arrangements. Such improvements can help clients insure themselves against shocks to their income or expenditure that might otherwise leave them unable to make loan payments. To substantiate this interpretation, here we present direct evidence from survey and experimental data of greater improvements in informal insurance arrangements between weekly relative to monthly clients.

4.1 Survey

We first explore differences in informal insurance across treatment groups by examining survey data on financial transfers over the past year to and from individuals in different relationship categories, collected at the end of the first loan cycle. As 43% of clients reported no transfers we focus on a binary outcome of whether the client reported any transfers to or from individuals grouped into three categories (as reported in the endline survey): (i) close family and friends, (ii) other relatives and neighbors, and (iii) other non-relatives. Unfortunately, our data does not let us directly identify transfers to VWS members.

Columns (1) and (2) of Table 4 show that weekly clients were no more likely than monthly clients to give transfers to close family members or friends. On the other hand, they were 9.8 percentage points (29%) more likely to report transfers to relatives outside the immediate family and neighbors (columns (3) and (4)), which is significant at the 10% level with or without controls. Since this category includes both group and non-group members, the difference implies any improvements in risk-sharing among group members likely did not displace risk-sharing arrangements with neighbors and distant relatives outside of the group. Nor do these transfers appear to displace transfers within the immediate family or to other non-relatives (columns (5) and (6)), suggesting a net gain in informal insurance. Overall these results suggest that increased meeting frequency expanded risk-sharing networks, as measurable returns to new relationships do not crowd out equally valuable relationships with individuals outside of the loan group.

4.2 Risk-sharing Experiment

The default and survey evidence indicate that greater social contact expanded MFI clients' risk-sharing networks. However, a shortcoming of our transfers data is that we do not directly observe instances of risk-sharing between group members. Hence, to provide direct evidence on improvements in risk-sharing arrangements, we turn to a field-based lottery game.

The lottery, a variant of laboratory dictator and trust games (Forsythe et al., 1994; Berg et al., 1995), was designed to elicit willingness to form risk-sharing arrangements in a field setting. We conducted this field experiment with a random subsample of *all* clients who entered the meeting frequency experiment during their first loan cycle. It is therefore free of any selection concerns related to client retention across loan cycles. The lottery occurred more than a year after clients completed their first loan cycle.[23] At this point, the typical client was on her third loan cycle unless she had stopped borrowing.

4.2.1 Design

We drew a random sample of 450 clients and successfully contacted 432 of them spread across 98 groups, yielding a final sample of 129 weekly and 321 monthly clients. Column (5) of Table 1 provides a randomization check for weekly and monthly clients entering the lottery sample, and columns (6) and (7) examine the balance of the voucher randomization (described below) sub-treatments. As before, the two characteristics that are unbalanced remain "fraction Muslim" and "years in the neighborhood", indicating that the lottery subsamples were both representative draws from the sample of study clients. In addition, weekly clients selected for the lottery are more likely to own a household business. While a test of joint significance still indicates that the samples are, overall, balanced on observables, because of these differences in mean characteristics across study groups, our preferred regression specification controls for basic demographic variables including those on which we observe an imbalance. Finally, since we also see that weekly clients who were selected for the 200 Rs. voucher randomization on average came from groups in which clients were geographically more dispersed (fewer close neighbors and more distant neighbors), empirical tests of differences across these sub-treatments are best suited for examining how treatment varies *within* a relationship category (i.e. heterogenous effects by group composition).

The protocol was as follows: Surveyors approached each selected client in her house and invited her to enter a promotional lottery for the new VWS retail store.The lottery prize

[23] Average final repayment and survey dates were April 2007 and July 2008, respectively.

consisted of gift vouchers worth Rs. 200 ($5) redeemable at the store (see Appendix for the surveyor script). Aside from banking, VWS undertakes many community interventions and conducts regular promotional activities in order to attract and retain clients. Hence, our intervention was likely to seem natural in this setting. Furthermore, the lottery script was designed to give the impression that participants had been selected in order to reward them for survey participation during the first loan cycle.[24]

The client was informed that, in addition to her, the lottery included 10 clients from different VWS branches, whom she was therefore unlikely to know. If she agreed to enter the draw (all agreed), she was then given the opportunity to enter any number of other members of her first VWS group into the same draw. Any group member she entered into the lottery would receive a lottery ticket delivered to her house and be told whom it was from. She was also told that the other ten participants would not have the opportunity to add individuals to the lottery.

To clarify how ticket-giving influenced her odds of winning, the client was shown detailed payoff matrices (Figure 2). Enumerators explained that she could potentially increase the number of lottery participants from eleven to as many as twenty, thereby increasing the fraction of group members in the draw from 9% to up to 50% while decreasing her individual probability of winning from 9% to as low as 5%.

Finally, we randomized the divisibility of the lottery prize offered. For half of the sample, the prize was one Rs. 200 voucher, while for the other half it consisted of four Rs. 50 vouchers. Appendix Figure 2 provides pictures of these vouchers. A voucher could only be redeemed by one client and all vouchers expired within two weeks.

Below we outline predictions for a client's ticket-giving behavior in relation to her expectations of cooperation by group members and describe how the divisible voucher randomization allows us to isolate cooperative from purely altruistic motives for ticket-giving. We also describe how the lottery timing allows us to examine the importance of monitoring versus just learning about other clients' type in driving ticket-giving behavior.

[24]Hence, it should also have seemed natural that they were offered to give tickets to first loan cycle group members rather than third loan cycle group members.

4.3 Predictions

A client belonging to a ten-member group made nine pairwise choices about ticket-giving to group members. Since members who received a ticket were not obligated to share their winnings (as in a trust game), the Nash outcome is to not give any tickets. Ticket-giving increases a client's expected payoff only if she trusts the recipient to reciprocate in some manner, e.g. share lottery earnings.

To understand the value of cooperation, consider the simple case of pairwise cooperation when the client gives a single group member a ticket. For this pair, expected joint earnings increase since their joint chances of winning the lottery rise from 9% to 17%. There are mutual gains from cooperation (e.g. if the receiver anticipates sharing half of her earnings, then entering a group member increases a client's expected lottery earnings from Rs.18 to 25 and the receiver's expected earnings rise from Rs.0 to 8.3), but costs to the client if there is no sharing (since her individual probability of winning the lottery declines from 9% to 8% as the pool of lottery entrants rises to 12). Appendix Figure 1 provides a graphical illustration. The top line shows a client's expected payoff when each group member who receives a ticket shares half her winnings with the client. The bottom line shows the reduction in her payoff if no receiver shares. If group members are perceived as likely to share, then the client always benefits from sharing.

Our lottery game shares many design features of the trust game. In using a lottery game, our primary interest was to avoid triggering client awareness of being a participant in an experiment. That said, there are some salient differences between the lottery and a classic trust game. In both cases sharing increases total potential earnings (with certainty in the trust game and stochastically in the lottery game). However, in a trust game the sender directly controls how much of her earnings she gives away whereas in our lottery the participant only shares an opportunity to win money (at some expense to herself). Hence, the signaling value of willingness to share may be greater in the lottery.

A second difference is that, unlike a trust game, pairwise returns in the lottery depend on total ticket-giving, generating a range of more subtle predictions on ticket-giving as a function of group composition, which we do not exploit in this paper. For instance, if the sender trusts all group members equally then she would give equally to all group members. Alternatively, if trust of group members varies, then recognition of this externality will constrain ticket-giving to less trusted group members.

In the present paper, we ignore these subtleties in order to test the more general predictions about ticket-giving behavior. In particular, in Section 3.1 we saw that higher meeting frequency in a client's first loan cycle strengthened social ties, which should positively impact pro-social behavior. Hence,

Prediction 1 *Higher meeting frequency in the first loan cycle will increase ticket-giving.*

Stronger social ties may increase ticket-giving for two broad reasons. First, in a setting in which clients lack access to explicit binding contracts, an increase in the frequency of interaction can improve clients' ability to sustain reciprocal economic arrangements, including informal insurance schemes (Karlan et al., 2009; Besley and Coate, 1995; Ambrus et al., 2010). Alternatively, more frequent meetings may increase a client's unconditional altruism towards group members.

To distinguish between these possibilities, we exploit random variation in the divisibility of the lottery prize. A more divisible lottery prize should only induce greater ticket-giving if the sender cares about the ease of reciprocal transfers.[25] Hence,

Prediction 2 *If the primary channel is (unconditional) altruism, then the incidence of ticket-giving will be independent of the perceived ability of the receiver to reciprocate.*

Meeting more frequently during the first loan cycle can encourage reciprocal arrangements between client pairs in several ways. First, under certain circumstances it may improve a client's ability to monitor group members. Consider the case in which members can influence their income through hidden actions. If different actions by members at the time of a meeting implies different initial conditions for the income generation process in the time period between two meetings, observing income (at meetings) will

[25]Part of the observed behavioral response to the divisibility of the lottery prize could stem from the fact that framing the prize as divisible and therefore shareable may simply prime a participant to think in terms of reciprocal arrangements. However, even if this is the only effect of varying the prize, our prediction is unchanged: divisibility should not matter if motivations for giving are purely altruistic.

provide a public signal of a member's action (Costa, 2007).[26] Hence, a higher frequency of meetings in the long-run will continue to improve monitoring by making public signals more precise.

An alternative channel through which repeat interaction at the start of a relationship can facilitate reciprocity is by hastening learning about other group members' ability and willingness to cooperate. Such a learning-based story by itself would imply that returns to mandating frequent interaction will diminish over time. Hence,

Prediction 3 *If the primary channel is learning, then requiring clients who met frequently in their first loan cycle to also do so in a subsequent loan cycle should not further increase ticket-giving.*

To examine this prediction, we exploit the fact that, at the time of the lottery, many clients were on their third VWS loan, and their current group contained a high proportion of members from their first loan group. These clients had interacted on a regular basis for close to two years. At the start of this third loan cycle, clients were re-randomized (at the group level) into groups that met either weekly or monthly. Thus, a subset of our lottery clients who had been on a weekly meeting schedule in their first loan cycle were randomly reassigned to either a weekly or monthly group at the time of the lottery.

4.4 Results

For all regressions, the outcome of interest is ticket-giving. For each member of a client's first loan group, we recorded whether the participant entered her into the lottery. In total, 57% of participants gave at least one ticket, which shows a very similar propensity to give as in dictator games (Levitt and List, 2009). Furthermore, in terms of individual characteristics that predict ticket giving and receiving, the patterns in the data are broadly consistent with findings from trust and dictator game literatures. Educated clients were more likely to give and receive tickets, while three types received more tickets: rich respondents, group leaders, and those who stated in baseline that they could make transfers

[26]If actions do not differentiate initial conditions, higher frequency signals may not increase the reliability of information extracted from public signals (Abreu et al., 1991; Fudenberg and Levine, 2009).

outside of their household. In contrast, respondents who participated in community and political events were more likely to give, but not receive, tickets.

4.4.1 First Loan Meeting Frequency and Ticket-Giving

Figure 3 shows the distribution of tickets for weekly and monthly clients (in percentage terms to account for group size differences). After zero tickets, the fraction of group members that received tickets declines gradually and then levels off after 60%. Weekly clients were substantially less likely to give no tickets and more likely to give tickets to more than half of their group.

In Table 5 we provide regression results of the forms given by equation (2) and (3). Columns (1) - (3) present results for clients offered the divisible lottery prize while columns (4) - (6) show results for clients randomized to the lottery with the less divisible prize. A comparison between columns (1)-(2) and columns (4)-(5) shows that, relative to her monthly counterpart, a client in a weekly group was significantly more likely to give a ticket to a group member if and only if the lottery prize was divisible. Weekly clients in the divisible randomization were 48% more likely to give tickets than monthly clients (9.4 percentage points) while there was almost no difference between monthly and weekly clients when the prize was a single large voucher, although it is worth noting that the indivisible voucher point estimates have large standard errors.

The fact that weekly clients showed a higher rate of ticket-giving when the lottery prize was easily divisible suggests that more frequent meetings increased ticket-giving by increasing expectations of reciprocity.[27] If frequent meetings only increased *unconditional* altruism, then ticket-giving should be independent of voucher divisibility. Figure 4 shows four loan group networks that highlight the empirical ticket-giving patterns found in the data. (The full set of ticket-giving networks are shown in Appendix Figure 3). Weekly clients' higher propensity to give tickets is reflected in the higher relative connectedness of the weekly networks in the divisible (i.e. circular nodes) but not the indivisible (i.e.

[27]Anecdotal evidence from conversations with clients also suggested that they believed multiple vouchers increased the likelihood that those they gave tickets to would share any future winnings.

square nodes) gift voucher randomization.

In columns (3) and (6), we examine whether the weekly effect differs by initial social distance. The coefficient estimates on the interaction terms indicate that increased ticket-giving by weekly clients was driven by increased giving to close neighbors and distant family. The fact that increased ticket-giving by weekly clients was concentrated among the categories of pairs in which we also observe a significant effect on social contact (Table 2) supports our interpretation that greater social interaction increased propensity to form risk-sharing arrangements.

The fact that moving from a monthly to a weekly repayment schedule did not influence ticket-giving to close family and friends provides an important placebo check: For immediate family or old friends, repayment schedules should not have influenced learning or monitoring since these pairs presumably knew each other well and saw each other often outside of meetings. Also consistent with this is the fact that ticket-giving was no higher among weekly, relative to monthly, clients who report that they never saw one another: Both sets of clients gave tickets to roughly 15% of group members whom they had not seen at all in the past 30 days (unreported).

Monthly clients' ticket-giving behavior was similar across the two voucher categories and, in general, was independent of ability to reciprocate. This suggests that either the primary motivation among monthly clients was not reciprocity or only marginal risk-sharing arrangements were sensitive to small barriers to trust such as prize divisibility. A few empirical patterns support this interpretation: First, the majority of tickets given by clients do not appear to be reciprocal arrangements. Specifically, 61% were given either to individuals they had not seen in the last 30 days, to individuals not identified as sources of help in the case of emergency, or to immediate family members. Second, monthly group members were no more likely to report giving or receiving transfers to individuals outside of the immediate family at the end of their first loan cycle relative to when they joined, suggesting that the bulk of risk-sharing arrangements among monthly group members predated our experiment and hence were likely to be stable to small variations in framing such as prize divisibility.

4.4.2 Hastening versus Sustaining Cooperation

More frequent interaction may help sustain cooperative arrangements indefinitely, or it may simply hasten the formation of cooperative arrangements through more rapid learning about other clients' types (see Section 4.3). One basic piece of evidence against the learning story is that, at the time of the lottery, the majority (69%) of clients had been in loan groups together for almost two years, by which point types should arguably have been revealed even among those who initially only met monthly.

For further evidence we exploit experimental variation in meeting frequency across multiple loan cycles. At the time of our lottery, roughly a third of the clients (137 out of 432) were on their third VWS loan cycle. At the start of the third loan cycle groups were re-randomized into weekly and monthly meetings (see Figure 1). VWS favors keeping client groups the same across cycles, but group members are replaced when they drop out. Sixty percent of the average lottery participant's third loan group members had also been members of her first group. Furthermore, the likelihood of a client having group members from the first loan in her third loan cycle group was independent of repayment frequency in the first loan cycle (unreported).[28]

We consider the sub-sample of 48 third loan cycle clients who had been on the weekly schedule in the first intervention. These clients were spread across 14 weekly repayment and 12 monthly repayment loan groups in the third cycle. We examine whether meeting frequency in the third cycle influenced levels of cooperation, in the form of lottery ticket-giving. This allows us to observe whether forcing clients who already know each other well to continue interacting regularly *further* increased cooperation. If so, then it is likely that, in addition to any short-run learning effects that hasten cooperation between members, meeting frequency also yields benefits via the monitoring channel.

For these clients, we examine whether being on a weekly meeting schedule in the third loan cycle influences ticket-giving by a client to her first loan cycle group members.

[28] Although this may seem surprising given the higher rates of default among monthly clients, it simply implies that those on the margin of influence for defaulting in the second loan cycle were not clients with a high propensity to continue onto the third loan, which makes sense given the small fraction of clients that graduate to third loans.

To account for reduced sample size we correct standard errors with wild bootstrapping (Cameron et al., 2008).

Columns (1) and (2) of Table 6 show that clients in loan groups that were randomly assigned to the weekly schedule in both the first and third loan cycle ("weekly-weekly") were approximately twice as likely (27 percentage points) to engage in pro-social behavior than a weekly-monthly client if and only if the prize was easily divisible. The results are robust to the inclusion of controls. As before, we find no evidence of increased giving for the indivisible voucher option (columns (3) and (4)). We interpret the difference for weekly-weekly clients as evidence that long-run contact among loan group members helps sustain long-run cooperative arrangements.

While the period over which learning about other clients occurs is uncertain, it is important to note that, by the time of the survey, these clients had been interacting regularly for 2.5 loan cycles. During this time-period they saw each other weekly for at least six months (first loan cycle) and every other week for at least six months more (second loan cycle). Consistent with this, we see no difference across weekly-weekly and weekly-monthly clients in their propensity to remember the names of their first loan group members at the time of survey (columns (5) and (6)).

4.5 The Cost of Building Social Capital

Increased economic cooperation among clients meeting more frequently implies significant benefits to MFIs from building social capital. However, these benefits do not come free given the nontrivial transaction costs of meeting four times as often. We estimate that moving from monthly to weekly meetings entails approximately two additional hours of client time per month, or 15 hours over the course of an average loan cycle. Meanwhile, banks could cut transaction costs per client by nearly three-fourths - or reach nearly four times as many clients for the same cost - by moving from a weekly to a monthly schedule.

In terms of benefits, default data for the second loan cycle shows that the average client who repaid monthly during her initial loan cycle defaulted on Rs. 150 more than the average client previously on a weekly repayment schedule, which is almost the same as the bank's additional transaction cost per client of meeting weekly.[29] Hence, a conservative back-of-the-envelope calculation suggests that weekly meetings may be cost-effective for a MFI. This could explain why MFIs persist with high-frequency repayment schedules despite the higher transaction costs.

Evaluating the social planner's problem is less straightforward since the costs and benefits to *clients* of meeting weekly are difficult to calculate. The cost to clients of regular repayment is likely to exceed the simple time cost of meeting attendance given the additional financial burden of making regular installments. The total benefits of increases in social capital are likely to include, in addition to the reduction in default risk, positive externalities such as information transfers between clients.That said, the direct importance of using meetings to improve risk-sharing in a setting characterized by weak formal institutions for contract enforcement can be a very important source of welfare gains. These improvements in risk-sharing are made even more striking by the fact that they were obtained in the absence of joint-liability contracts, and provide a rationale for the current trend among MFIs of maintaining repayment in group meetings despite the transition from joint- to individual-liability contracts (Gine and Karlan, 2009).

5 Conclusions

A widely held belief across social scientists in many disciplines is that social interactions encourage norms of reciprocity and trust, which reap economic returns. In fact, participation in groups is often used to measure individuals' or communities' degree of economic cooperation (see, for instance, Narayan and Pritchett, 1999). However, while the notion is theoretically well-grounded, it is not clear from previous work whether the correlation between social distance and trust reflects the causal effect of interaction on economic cooperation. We provide rigorous experimental evidence that repeat interactions can, in

[29] We estimate that loan officers spent an additional 1.5 hours per month per group, which amounts to 3.75% of their monthly wage for every 10 customers, or Rs. 150. Given that a loan cycle is ten months and contains ten members, this implies an average cost per client of roughly Rs. 150.

practice, facilitate cooperation by enabling individuals to sustain reciprocal economic ties.

Furthermore, our results demonstrate that development programs can increase social ties and enhance social capital among members of a highly localized community in a strikingly short amount of time. In our study, close neighbors from similar socio-economic backgrounds got to know each other well enough to cooperate with only the outside stimulus of MFI meetings. An important caveat is that bringing people together to interact in a financial setting such as microfinance groups may have a particularly strong effect on economic cooperation relative to other forms of interaction. Finally, while many authors have suggested a link between social capital and MFI default rates, ours is the first study to provide rigorous evidence on the role of microfinance in building social capital, and thereby broaden our understanding of the various channels - other than joint liability - through which MFIs achieve low default rates without the use of collateral.

By broadening and strengthening social networks, the group-based lending model used by MFIs may provide an important impetus for the economic development of poor communities and the empowerment of women. While we cannot expect all communities to respond equally to such stimuli, our findings are likely to be most readily applicable to the fast-growing urban and peri-urban areas of cities in developing countries (such as Kolkata) where microfinance is spreading most rapidly. An important goal of future research would be to understand how other development programs and public policies can be designed to enhance the social infrastructure of poor communities.

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Table 1. Randomization Check

	Mean Values- All Clients		Weekly/Monthly Difference				
	Weekly	Monthly	All Clients	Second Loan Clients	Lottery Clients	4-Rs. 50 Voucher Prize	1-Rs. 200 Voucher Prize
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Main Controls							
Month of Loan Group Formation	5.697	5.688	0.010	0.116	0.200	0.184	0.216
	[0.088]	[0.051]	(0.326)	(0.349)	(0.360)	(0.398)	(0.388)
Muslim	0.000	0.076	-0.077***	-0.046**	-0.109**	-0.123**	-0.095**
	[0.000]	[0.010]	(0.029)	(0.021)	(0.044)	(0.052)	(0.038)
Age	33.376	33.461	-0.085	0.510	-1.056	-1.75	-0.328
	[8.330]	[8.387]	(0.683)	(0.742)	(0.765)	(1.086)	(1.200)
Literate	0.853	0.838	0.015	0.012	0.021	0.030	0.011
	[0.355]	[0.369]	(0.031)	(0.036)	(0.042)	(0.052)	(0.055)
Married	0.876	0.865	0.011	0.003	-0.011	-0.018	-0.004
	[0.330]	[0.342]	(0.025)	(0.027)	(0.035)	(0.057)	(0.043)
Household Size	3.974	3.915	0.058	0.137	0.059	0.233	-0.124
	[1.148]	[1.410]	(0.093)	(0.090)	(0.135)	(0.207)	(0.174)
Worked for Pay in Last 7 Days	0.585	0.530	0.055	0.022	0.043	-0.004	0.092
	[0.494]	[0.499]	(0.046)	(0.052)	(0.061)	(0.071)	(0.081)
Household Savings	3616	2445	1171	1043	-917	-1646	-152
	[31086]	[12286]	(1876)	(1161)	(811)	(1322)	(894)
Years Living in Neighborhood	15.327	16.997	-1.67**	-1.051	-2.635***	-3.326**	-1.910
	[10.275]	[10.152]	(0.739)	(0.896)	(0.985)	(1.320)	(1.579)
Number of Distant Relatives in Group	0.571	0.451	0.120	0.174	0.049	0.177	-0.085
	[1.062]	[0.944]	(0.119)	(0.131)	(0.164)	(0.192)	(0.173)
Number of Close Neighbors in Group	1.132	1.483	-0.351	-0.152	-0.422	0.170	-1.043***
	[1.804]	[1.959]	(0.313)	(0.374)	(0.352)	(0.439)	(0.332)
Number of Distant Neighbors in Group	6.061	6.109	-0.048	-0.281	0.107	-0.668	0.920**
	[2.606]	[2.581]	(0.434)	(0.486)	(0.476)	(0.582)	(0.455)
Panel B: Additional Variables							
Number of Clients in Group	10.277	10.348	-0.069	-0.059	-0.006	0.111	-0.130
	[0.040]	[0.028]	(0.166)	(0.172)	(0.179)	(0.212)	(0.165)
Household Owns Business	0.755	0.680	0.075	0.013	0.147**	0.152**	0.141*
	[0.431]	[0.467]	(0.047)	(0.052)	(0.061)	(0.077)	(0.081)
Owns Home	0.775	0.792	-0.017	-0.002	-0.003	-0.089	0.086
	[0.419]	[0.406]	(0.041)	(0.049)	(0.047)	(0.061)	(0.064)
Number of Group Members not Known	2.409	2.481	-0.073	-0.081	-0.048	-0.305	0.222
	[2.816]	[2.896]	(0.410)	(0.399)	(0.497)	(0.517)	(0.664)
Illness in Past 12 Months	0.343	0.277	0.066	0.047	0.050	0.022	0.080
	[0.476]	[0.448]	(0.044)	(0.047)	(0.066)	(0.075)	(0.080)
Number of Transfers into Households	2.895	2.497	0.398	0.251	0.177	-0.575	0.966
	[4.105]	[4.088]	(0.517)	(0.559)	(0.623)	(0.617)	(0.979)
Number of Transfers out of Households	1.556	1.148	0.408	-0.244	0.270	-0.293	0.860
	[7.128]	[5.423]	(0.436)	(0.394)	(0.510)	(0.568)	(0.886)
Health Expenditures	3514	4080	-566	-573	-673	-977	-354
	[5561]	[12428]	(613)	(706)	(961)	(1442)	(1061)
Education Expenditures	5011	4513	498	365	-221	-132	-314
	[5515]	[5693]	(403)	(490)	(654)	(853)	(870)
Fixed Salary Earned by Household	1460	1560	-100	-151	25	114	-68
	[2998]	[2602]	(251)	(278)	(331)	(385)	(471)
Fraction of Clients Surveyed					-0.009		
					(0.038)		
N	306	710	1016	704	428	219	209

Notes

1 Month of Formation refers to calendar month of group formation ("4" for groups formed in April, 2006, and so on). Close Neighbors are group members who live within the first quintile of distance (60m) and were not identified by the client as close family members, friends or distant relatives. Distant Neighbors are group members who live farther than 60m from one another, and were not identified by the client as close family members, friends or distant relatives. The omitted relationship type is Close Family/Friends, and all relationship types are defined at time of loan group formation. Number of Group Members not Known is also defined at time of loan group formation. Illness in Past 12 Months is an indicator variable for whether any household member has been ill in past 12 months. Number of Transfers into/out of Households is defined over past 12 months. Fraction of clients surveyed is the group-level fraction of clients receiving lottery survey.

2 Columns (3)-(7) report tests of differences of means (weekly minus monthly) for the subsamples. * , **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors are clustered by group.

Table 2. Meeting Frequency and Social Contact

	Short-Run		Long-Run			
	All Members Visited		Total Times Met			
	(1)	(2)	(3)	(4)	(5)	(6)
Weekly	0.904***	0.938***	0.884	1.124*	-1.319	-0.969
	(0.0292)	(0.0240)	(0.842)	(0.638)	(2.268)	(2.193)
Weekly*Distant Relative					6.679*	6.435*
					(3.529)	(3.546)
Weekly*Close Neighbor					5.507**	5.532**
					(2.528)	(2.431)
Weekly*Distant Neighbor					1.355	1.007
					(2.237)	(2.177)
Distant Relative					0.550	0.525
					(1.879)	(1.941)
Close Neighbor					-6.064***	-6.077***
					(1.431)	(1.438)
Distant Neighbor					-10.51***	-10.41***
					(1.301)	(1.296)
Controls	No	Yes	No	Yes	No	Yes
Mean of Monthly	0.096		4.384			
	[0.295]		[9.676]			
N	1027	1027	3137	3137	3137	3137

Notes:

1 All Members Visited is constructed from two questions asked to each client during the loan meeting: "Have all of your group members visited your house?" and "Have you ever visited houses of all group members?" The answers are coded as indicator variables which equal one if the client responds "yes." The variable All Members Visited equals one if the client's response to either question equals one over the first five months of her loan cycle. Total Times Met is the survey response to the question "On average how many times did you meet X (outside group meetings) in the last 30 days?" A client was asked to answer this question for each group member and the outcome variable is measured at the pair level.

2 Distant Relative, Close Neighbor, and Distant Neighbor are as defined in the notes to Table 1. The omitted group is Close Family/Friends.

3 Mean of monthly is average of the dependent variable for monthly clients, standard deviations in brackets.

4 Regressions in Columns (1)-(2) include one response per client, and regressions in Columns (3)-(6) include one response per loan group pair. Regressions with controls include the variables in Table 1, Panel A. Column (6) excludes client-level controls for Number of Distant Relatives in Group and Number of Close and Distant Neighbors in Group. * , **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors are clustered by group.

Table 3. Meeting Frequency and Loan History

	First Loan Default			Second Loan Default			Took Out Second Loan		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Weekly	-0.0181	-0.0160	-0.0263	-0.0775**	-0.0831**	-0.0389	0.0752	-0.00915	-0.0258
	(0.0121)	(0.0131)	(0.0295)	(0.0295)	(0.0382)	(0.0960)	(0.0548)	(0.0483)	(0.0899)
Weekly*Number of Distant Relatives or Close Neighbors in Group			0.00498			-0.0388**			0.0347**
			(0.00469)			(0.0154)			(0.0172)
Weekly*Number of Distant Neighbors in Group			0.000351			0.00341			-0.00682
			(0.00241)			(0.0122)			(0.0121)
Number of Distant Relatives or Close Neighbors in Group			-0.00193			0.0258			-0.0194
			(0.00222)			(0.0166)			(0.0146)
Number of Distant Neighbors in Group			0.00213			-0.00628			-0.0271***
			(0.00185)			(0.0125)			(0.00748)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Mean of monthly	0.018			0.108			0.667		
	[0.133]			[0.301]			[0.472]		
N	1026	1026	1026	707	707	707	1026	1026	1026

Notes:

1 For each loan, a client is defined as defaulted if she has not repaid the total loan amount within forty-four weeks after due date. Data on loan repayment was still collected weekly after the end of the loan cycle. Took Out Second Loan is an indicator variable for whether client took out a second loan with VWS within 104 weeks of first loan repayment. Relationship type is defined before joining VWS.

2 All regressions are Ordinary Least Squares (OLS) specifications. Regressions with controls include loan officer fixed effects (for the corresponding loan cycle), and controls for the variables in Table 1, Panel A. Regressions in Columns (5) and (6) also control for second loan size. * , **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors are clustered by first loan group.

Table 4. Meeting Frequency and Transfers

	Transfers					
	Close Family/Friend		Neighbor/ Other Relative		Other Non-Relative	
	(1)	(2)	(3)	(4)	(5)	(6)
Weekly	0.0427	0.0583	0.0982*	0.103*	-0.0133	-0.0125
	(0.0569)	(0.0517)	(0.0548)	(0.0531)	(0.0201)	(0.0178)
Controls	No	Yes	No	Yes	No	Yes
Mean of monthly	0.399		0.333		0.057	
	[0.490]		[0.472]		[0.232]	
N	961	961	961	961	961	961

Notes:

1 Transfers refer to transfers given/received by client's household in 12 months before first loan endline survey, and are indicator variables for whether client's household gave/received any transfers to/from the relevant groups. Relationship type is defined at end of loan cycle for transfers. Other Non-Relative includes, for example, acquaintances.

2 Regressions with controls include the variables in Table 1, Panel A. * , **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors are clustered by group.

Table 5. Meeting Frequency and Ticket-Giving

	Gave Ticket					
	4-Rs. 50 Vouchers			1-Rs. 200 Voucher		
	(1)	(2)	(3)	(4)	(5)	(6)
Weekly	0.0941*	0.108**	-0.0129	0.0179	-0.00746	-0.0992
	(0.0529)	(0.0488)	(0.121)	(0.0564)	(0.0540)	(0.106)
Weekly*Distant Relative or Close Neighbor			0.314**			0.188
			(0.120)			(0.121)
Weekly*Distant Neighbor			0.0743			0.0855
			(0.112)			(0.109)
Distant Relative or Close Neighbor			-0.255***			-0.298***
			(0.0711)			(0.0809)
Distant Neighbor			-0.333***			-0.432***
			(0.0586)			(0.0752)
Controls	No	Yes	Yes	No	Yes	Yes
Mean of monthly	0.196			0.241		
	[0.397]			[0.428]		
N	2027	2027	2027	1991	1991	1991

Notes

1 For each client in the sample we have (on average) nine observations. The dependent variable equals one for a group member if the client gave her a ticket. Relationship type is defined before joining VWS.

2 Regressions with controls include the variables in Table 1, Panel A. * , **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors are clustered by group.

Table 6. Meeting Frequency across Loan Cycles and Pro-Social Behavior

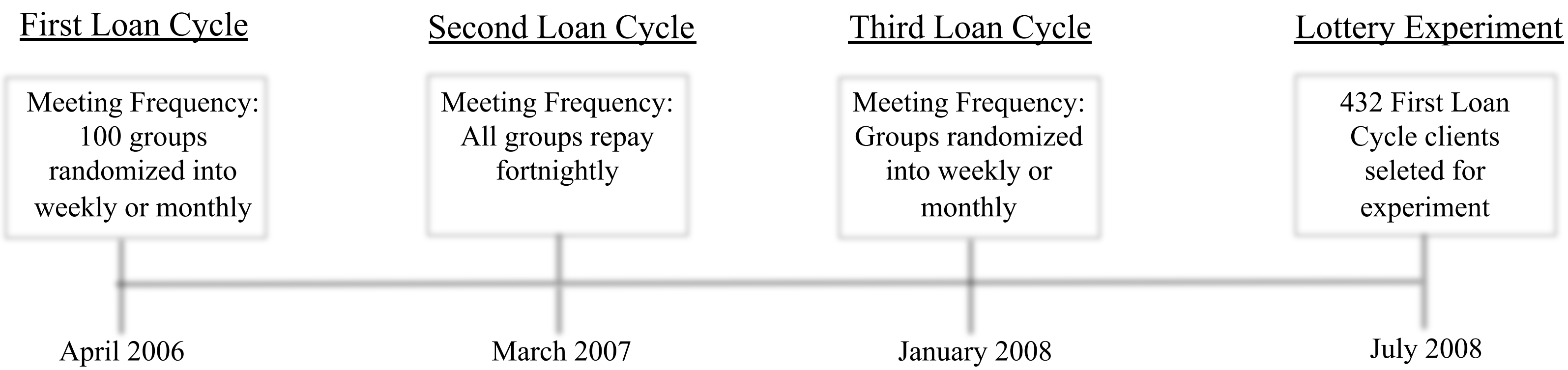
	Gave Ticket					
	4-Rs. 50. Vouchers		1-Rs. 200 Voucher		Member Remember	
	(1)	(2)	(3)	(4)	(5)	(6)
Weekly in First Loan, Weekly in Third Loan	0.267*	0.416*	0.0202	-0.150	-0.0222	0.0115
	(0.113)	(0.0987)	(0.122)	(0.175)	(0.0676)	(0.0954)
P-value (Wild Bootstrap)	*0.054*	*0.094*	*0.878*	*0.554*	*0.766*	*0.952*
Controls	No	Yes	No	Yes	No	Yes
Mean of Weekly in First Loan, Monthly in Third Loan	0.126		0.281		0.669	
	[0.333]		[0.451]		[0.472]	
N	251	251	204	204	455	455

Notes

1 The sample is First Loan weekly clients who went on to take out a Third Loan with VWS. Gave Ticket is as defined in notes to Table 5. Member Remember is the indicator variable "Do you remember this group member?" Data was collected at the time of lottery.

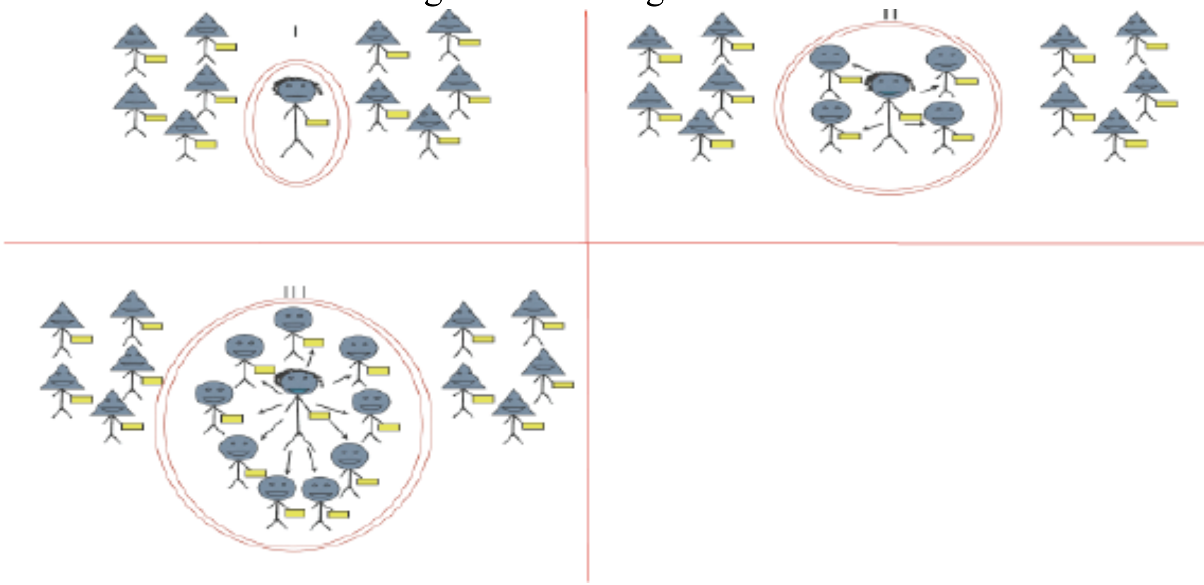
2 Even-numbered regressions include month of First and Third Loan group formation fixed effects. * , **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Wild bootstrapping is used to correct standard errors and to determine significance levels.

Figure 1. Timeline



Notes: Dates reflect the start of each loan cycle and of lottery surveying. Our sample population consisted of 1028 clients who joined VWS in 2006. For their first loan cycle 721 of these clients were randomly assigned to monthly meeting groups (there were 70 monthly groups) and 307 were assigned to weekly meeting groups (there were 30 weekly groups). Of these, 707 continued to a second loan cycle during which all clients met for repayment on a fortnightly basis. We use this sample to evaluate second loan cycle default outcomes. Finally, clients who took out a third loan were re-randomized into weekly or monthly groups. To examine the effects of long run variation in meeting frequency we restrict our sample to clients who took out a third loan and were on a weekly meeting schedule in their first loan cycle. There are 48 such clients.

Figure 2. Winning Probabilities



Notes:

This picture was used to explain how ticket-giving affected lottery probabilities. The explanation provided was "In Picture 1 in which you don't give out any tickets to members of your VWS group, you have a 1 in 11 chance of winning.

In Picture 2, you choose to have us give a ticket to four other members of your VWS group and there are 15 tickets total. In that case, you would have a 1 in 15 chance of winning and each of the members of your VWS group you gave a ticket to would have a 1 in 15 chance of winning.

In Picture 3, you choose to have us give a ticket to nine other members of your VWS group and there are 20 tickets total. In that case, you would have a 1 in 20 chance of winning and each of the members of your VWS group you gave a ticket to would have a 1 in 20 chance of winning." In each picture, those outside of the red circle are non-group members.

Figure 3. Client-level Distribution of Lottery Tickets

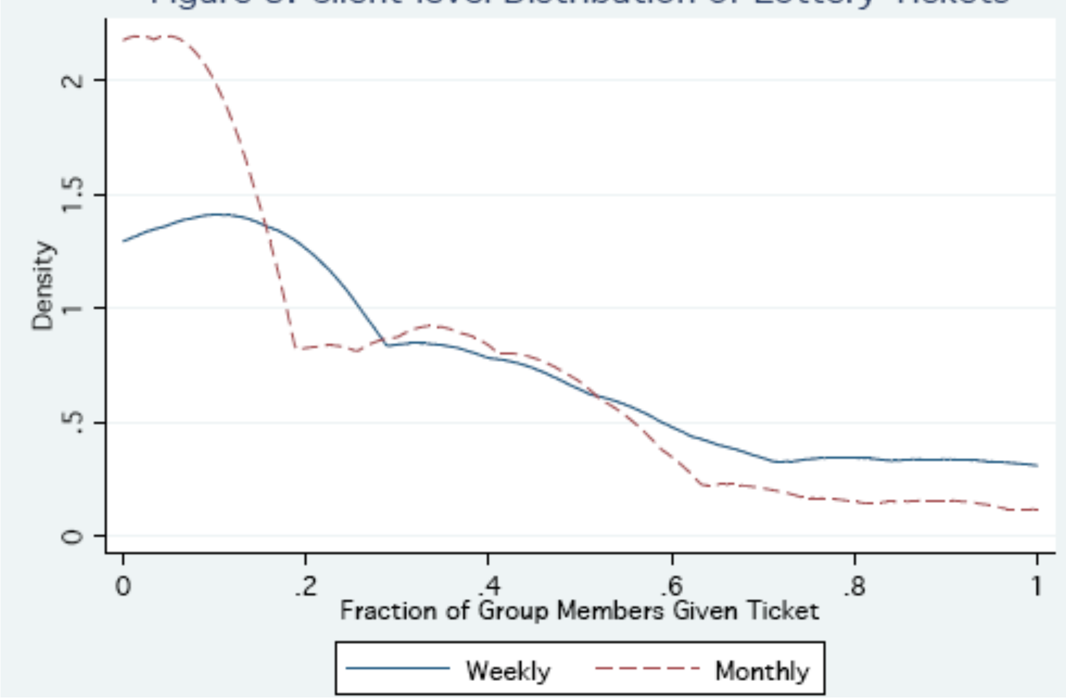
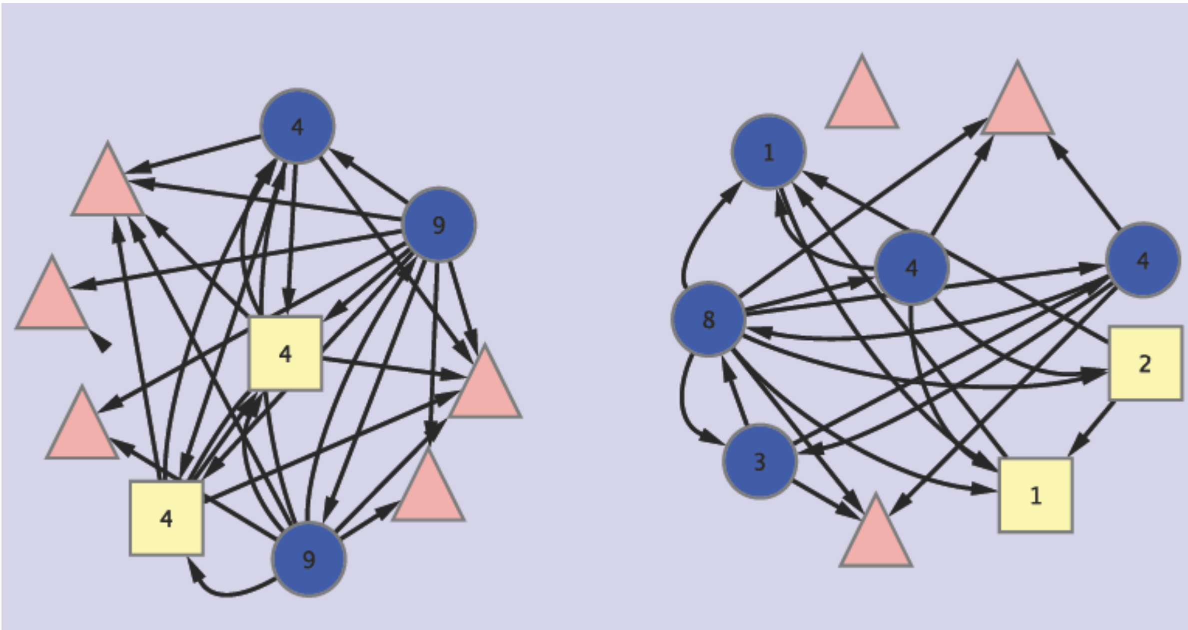
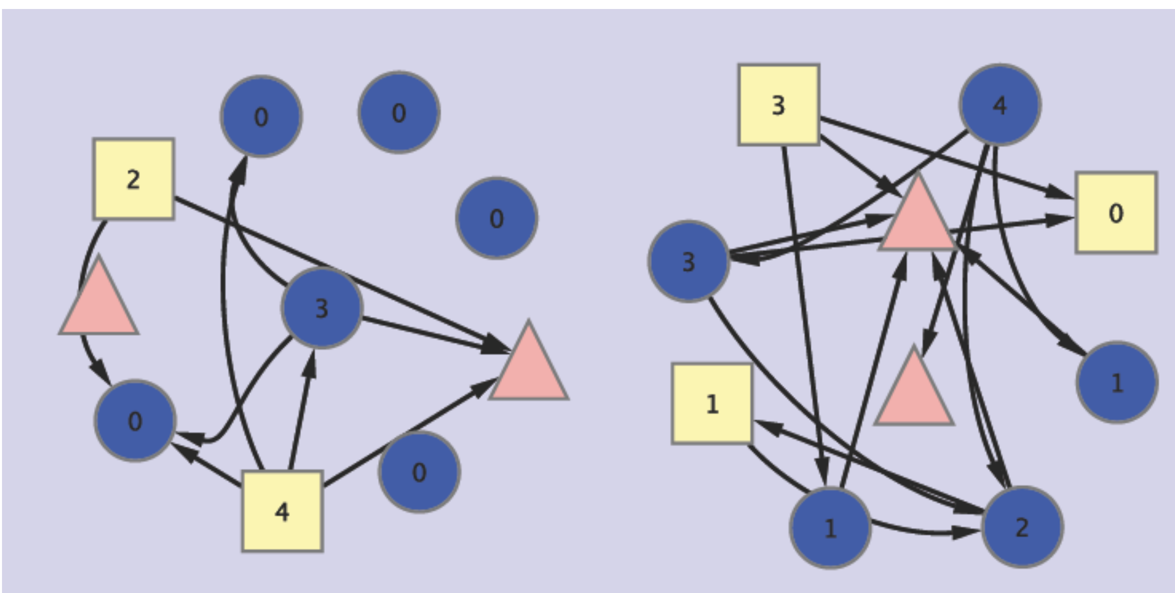


Figure 4. Network Structures

Panel A: Weekly Clients



Panel B: Monthly Clients



Circular nodes are clients from the 4-Rs. 50 gift voucher randomization, square nodes are clients from the 1-Rs. 200 gift voucher randomization, and triangular nodes are clients who were not surveyed for the lottery. Nodes are labelled by the number of tickets given out by client, and edges depict direction of ticket-giving.

Appendix Table 1. Determinants of Default

	Second Loan Default
	(1)
Muslim	0.032
	(0.109)
Age	-0.001
	(0.002)
Literate	0.018
	(0.027)
Household Size	0.000
	(0.007)
Worked for Pay in Last 7 Days	0.009
	(0.019)
Household Savings	-0.014***
	(0.004)
Years Living in Neighborhood	0.001
	(0.002)
Number of Clients in Group	0.077*
	(0.043)
Household Owns Business	0.022
	(0.017)
Owns Home	0.050**
	(0.024)
Illness in Past 12 Months	0.028
	(0.023)
Number of Transfers into Households	-0.003
	(0.002)
Health Spending (Rs.)	0.262**
	(0.109)
N	707

Notes

1 Second Loan Default is as defined in Table 3. Health Spending is defined as household spending on most recent illness within past 30 days. Remaining variables are as defined in Table 1.

2 * , **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors are clustered by first loan group.

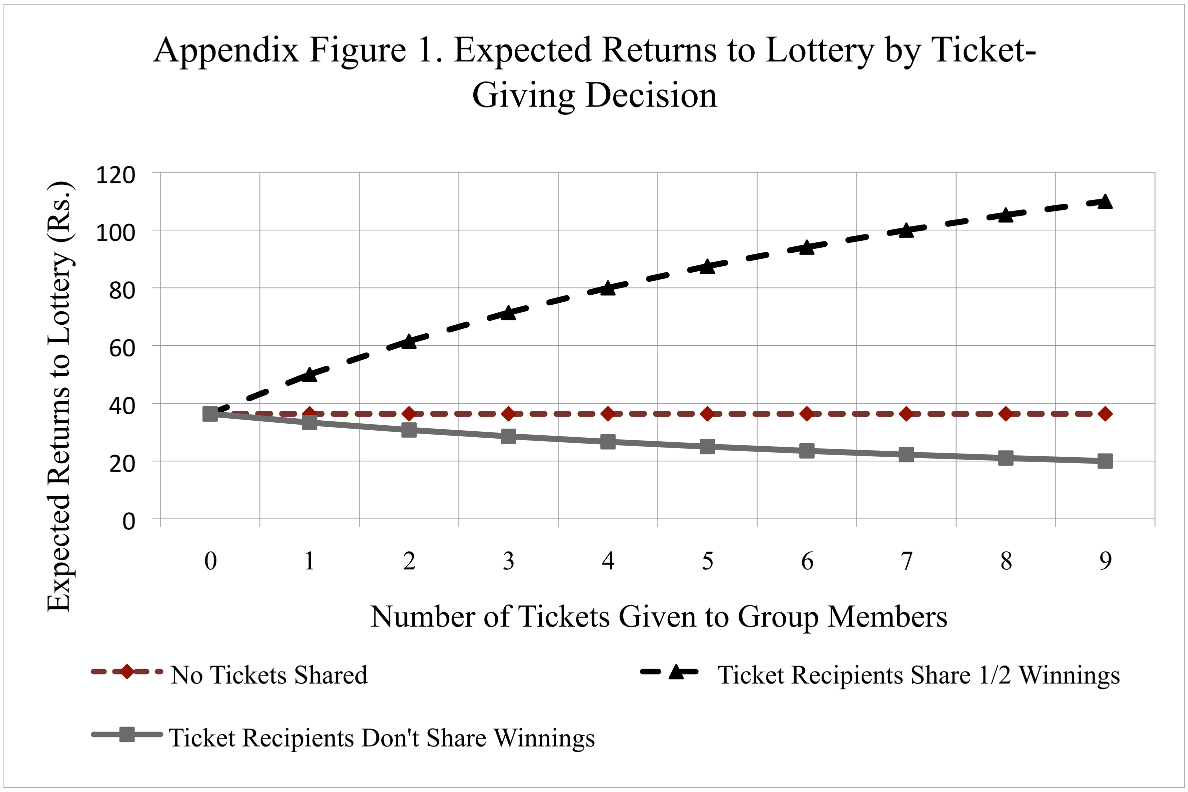
Appendix Table 2. Meeting Frequency and Social Contact

	Short-Run		Long-Run			
	Social Contact Index		Social Contact Index			
	(1)	(2)	(3)	(4)	(5)	(6)
Weekly	2.944***	3.071***	0.104	0.161**	0.0426	0.111
	(0.156)	(0.160)	(0.0768)	(0.0668)	(0.163)	(0.177)
Weekly*Distant Relative					0.400*	0.403*
					(0.203)	(0.217)
Weekly*Close Neighbor					0.254	0.259
					(0.213)	(0.213)
Weekly*Distant Neighbor					-0.0103	-0.0445
					(0.170)	(0.178)
Distant Relative					0.109	0.0739
					(0.155)	(0.157)
Close Neighbor					-0.767***	-0.764***
					(0.120)	(0.114)
Distant Neighbor					-1.110***	-1.095***
					(0.0981)	(0.0970)
Controls	No	Yes	No	Yes	No	Yes
N	1027	1027	3137	3137	3137	3137

Notes:

1 Short-Run Social Contact Index generates average effect size from four questions asked to each client during the loan meeting: (1) "Have all of your group members visited your house?", (2) "Have you ever visited houses of all group members?”, (3) "Do you know the names of the family members of your group members?", and (4) "Do you know if any of your group members had relatives come over in last 30 days?" The answers are coded as indicator variables which equal one if the client responds "yes." The first three variables equal one if the client's response to the question equals one over the first five months of her loan cycle, and the fourth is the mean value of client responses over this period. Long-Run Social Contact Index generates average effect size from four questions asked to each client during the lottery survey: (1) Total Times Met (defined in Table 2), (2) "Do you still talk to X about her family," (3) "If you had a sick family member and had to leave your house for a few hours for an emergency, would you ask X to come to your home and look after him/her?", and (4) "During the most recent Durga Puja, did you attend any part of the festival with X?" For all but the first of these questions, the answers are coded as indicator variables which equal one if the client responds "yes." Relationship type is defined before joining VWS.

2 Regressions in Columns (1)-(2) include one response per client, and regressions in Columns (3)-(6) include one response per loan group pair. Regressions with controls include the variables in Table 1, Panel A. * , **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors are clustered by group.



Notes:
Appendix Figure 1 shows the expected returns to the lottery based on ticket-giving decision, and extent of reciprocal behavior by ticket recipient.

Appendix Figure 2. Lottery Vouchers

VILLAGE WELFARE SOCIETY **VILLAGE MICRO CREDIT SERVICES**

TOGETHER TOWARDS PROSPERITY

Rs. 50 Voucher

Single Use Only

Whoever reclaims this voucher must bring their VWS passbook with them to the VWS village bazaar when making their purchase. If the claimant is no longer a VWS client, they should bring their voter identification card.

Date of Lottery: ______________________ **Deadline to Claim:** ______________________

Group Name: ______________________ **Name of Claimant:** ______________________

Name of Winner: ______________________ **Signature of Claimant:** ______________________

Signature of Winner: ______________________

VILLAGE WELFARE SOCIETY **VILLAGE MICRO CREDIT SERVICES**

TOGETHER TOWARDS PROSPERITY

Rs. 200 Voucher

Single Use Only

Whoever reclaims this voucher must bring their VWS passbook with them to the VWS village bazaar when making their purchase. If the claimant is no longer a VWS client, they should bring their voter identification card.

Date of Lottery: ______________________ **Deadline to Claim:** ______________________

Group Name: ______________________ **Name of Claimant:** ______________________

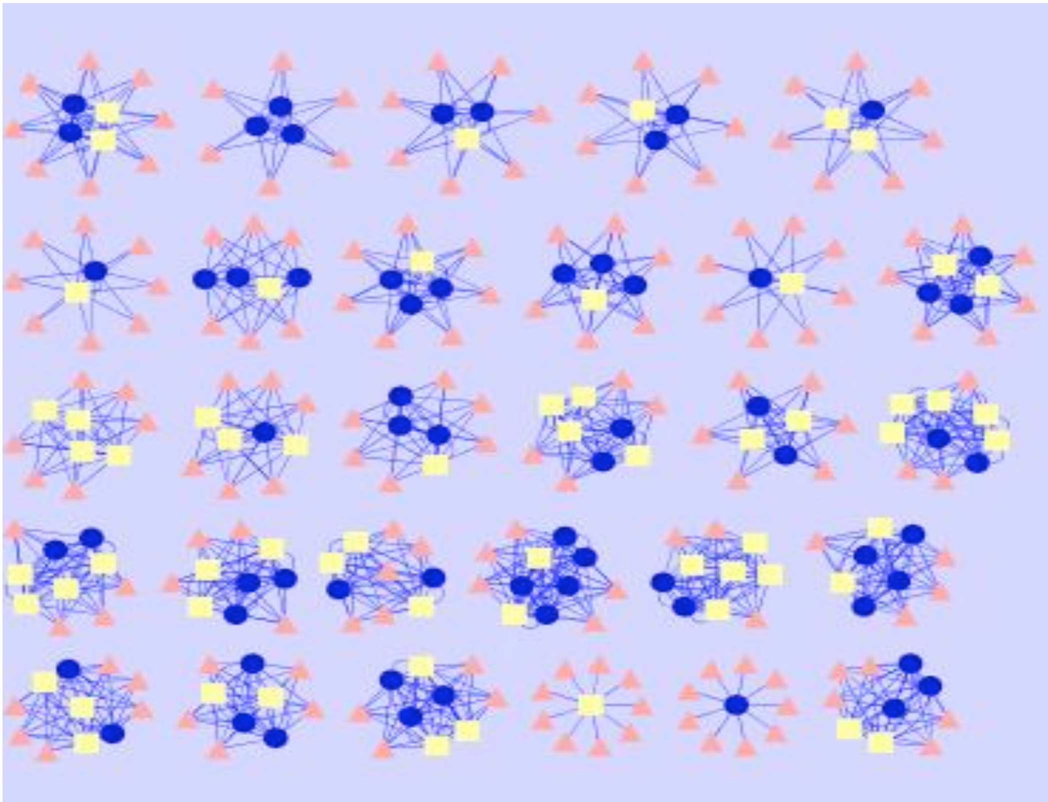
Name of Winner: ______________________ **Signature of Claimant:** ______________________

Signature of Winner: ______________________

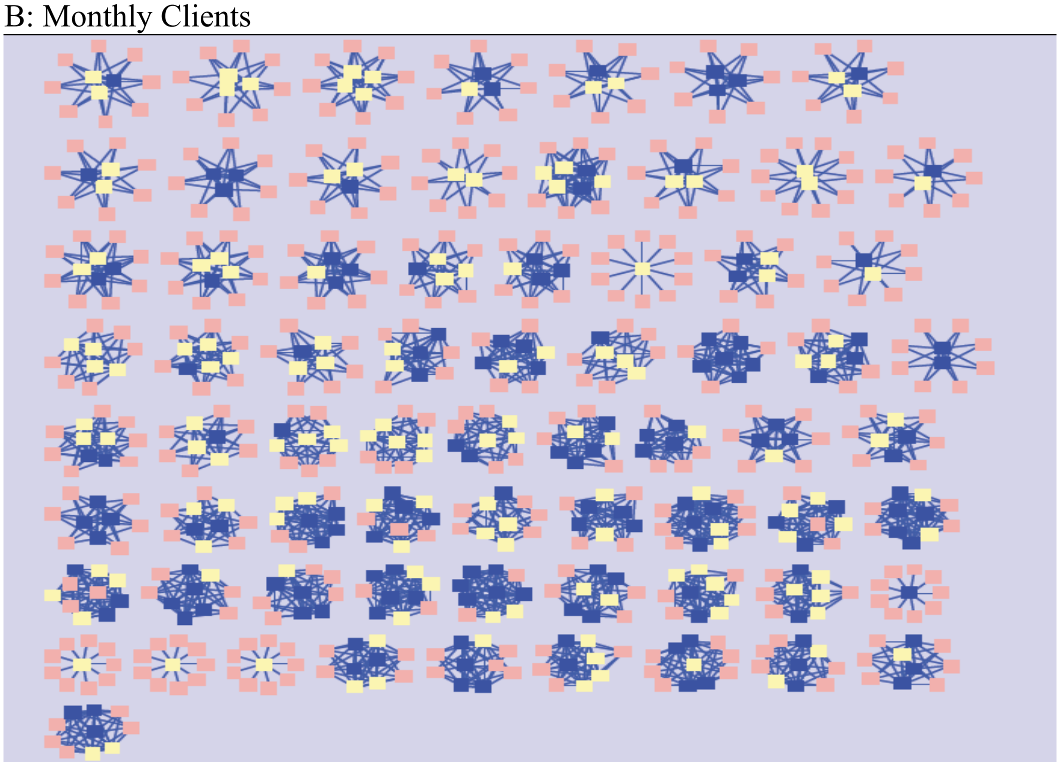
Note:
Clients were randomly offered the choice of joining the 1-Rs. 200 Voucher or the 4-Rs. 50 Voucher lottery. This figure shows the final vouchers which were given to the winner of the two lotteries.

Appendix Figure 3. All Network Structures

Panel A: Weekly Clients



Panel B: Monthly Clients



APPENDIX: Lottery Script

Probability Script for Main Lottery: In the lottery, you and ten other VWS clients will receive a ticket. Additionally, you have the option of selecting additional members of your VWS loan group that you would like us to give tickets to. You can tell us not to give anybody else in your VWS loan group a ticket, you can tell us to give each person in your group a ticket, or you can tell us which specific members to give tickets to.

Before that, let us review the effect giving out tickets has on chances of winning. In picture 1 in which you do not give out any tickets to members of your VWS group, you have a 1 in 11 chance of winning. In picture 2, you choose to give a ticket to four other members of your VWS group and there are 15 tickets total. In that case, you would have a 1 in 15 chance of winning and each of the members of your VWS group you gave a ticket to would have a 1 in 15 chance of winning. In picture 3, you give a ticket to nine other members of your VWS group and there are 20 tickets total. In that case, you would have a 1 in 20 chance of winning and each of the members of your VWS group you gave a ticket to would have a 1 in 20 chance of winning.

These are only a few examples of what odds of winning you may have after you decide how many tickets to give out. Remember that whether or not you give out tickets to other members of your first VWS loan group, you keep the lottery ticket we have given you. Now, before we continue, do you have any questions about how the lottery will work?

Additional Script for one 200 Rs. voucher: If you win the lottery, you will receive a single 200 Rs. voucher redeemable at the VWS village bazaar. You can use the voucher yourself or give it to someone in your first VWS group. Either way, the voucher must be used within two weeks. Additionally, only one person can redeem the voucher at the VWS store and the entire voucher value must be redeemed (so, for example, you cannot use 100 Rs. one day and save 100 Rs. for another day). To summarize, if you win the lottery, you will be asked to sign the 200 Rs. voucher when you receive it. However, you are still free to decide whether to keep or give away the voucher that you receive.

Additional Script for four 50 Rs. vouchers: If you win the lottery, you will receive four 50 Rs. vouchers redeemable at the VWS village bazaar. You may choose to use all four vouchers yourself, to give away 1-3 of the vouchers to members of your first VWS group and keep the rest for yourself, or to give away all of the vouchers to members of your first VWS group. In any case, the vouchers must be used within two weeks. Additionally, the entire value of each of the vouchers must be used when the voucher is redeemed (so, for example, you cannot use 25 Rs. of a 50 Rs. voucher one day and save 25 Rs. for another day). To summarize, if you win the lottery, you will be asked to sign each of the 50 Rs. vouchers when you receive them. However, you are still free to decide whether to give away or keep each of the four vouchers that you receive.