

# Building Social Capital through Microfinance

Benjamin Feigenberg, Erica Field and Rohini Pande\*

## Abstract

Despite strong theoretical underpinnings, economic returns to social interaction have proven difficult to identify empirically. 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 contact and lower default. Relative to clients who met on a monthly basis during their first loan, those who met weekly were four times less likely to default on their subsequent loan. We provide experimental and survey evidence that the decline is driven by improvements in informal risk-sharing that result from greater social contact.

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, Dominic Leggett, Muriel Niederle, Aloysius Sioux, Jesse Shapiro, Anil Somani 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.<sup>1</sup> 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 people to interact more often actually facilitate economic cooperation?

Rigorous empirical 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 stronger or denser social networks, we cannot assign a causal interpretation either to the positive association between community-level social ties and public goods provision or to the higher levels of cooperation observed among friends relative to strangers in laboratory public goods games.<sup>2</sup> 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.

This paper undertakes this exercise in the context of a development program that emphasizes group interaction – microfinance. In the typical “Grameen Bank”-style microfinance program, clients meet weekly in groups to make loan payments. In addition

---

<sup>1</sup>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.

<sup>2</sup>The public good provision and community ties literature includes Costa and Kahn (2003); Alesina and 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). In an approach similar to ours, 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.

to facilitating debt collection by the loan officer, these meetings encourage regular interaction among members of highly localized communities. In order to isolate the role of social contact among microfinance clients, our experiment introduced exogenous variation in the level of client interaction by randomly assigning one hundred first-time borrower groups of a 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.<sup>3</sup>

Using administrative and survey data collected more than a year after the experimental loan cycle ended, we study long-run effects of initial increases in mandatory interactions on social contact between group members and rates of default on future loans. In the classic joint liability debt contract emphasized in the microfinance literature, group members have financial incentives to exploit social capital in order to monitor and enforce one another's loan investment and repayment (Besley and Coate, 1995). The clients in our experiment, however, were on individual liability debt contracts. We show that, even absent the explicit incentives for monitoring and enforcement that group liability provides, greater social contact among group members can improve financial outcomes by increasing the level of risk-sharing that occurs within social networks.

Our intervention led to two striking changes in client behavior. First, clients assigned to weekly groups during their first loan cycle maintained greater social contact with group members: over a year later, clients who had met on a weekly basis during the first loan cycle were 43% more likely to attend social events together and 33% more likely to discuss personal matters than those who had met on a monthly basis. The gains were concentrated among clients who did not know each other well before joining the MFI but had the potential to sustain social contact via extended family networks or geographic proximity.<sup>4</sup>

Second, while clients in both groups were equally likely to continue borrowing, weekly clients were significantly less likely to default on their second loan. Clients who met

---

<sup>3</sup>At enrollment borrowers were told that they would be assigned to weekly or monthly groups by lottery.

<sup>4</sup>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)).

weekly during their first loan cycle were *four* times (7.8 percentage points) less likely to default on their second loan, despite the fact that all clients 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 of results 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.<sup>5</sup> Together, these patterns suggest that increases in social contact led to long-run improvements in risk-sharing arrangements.

The remainder of the paper presents several pieces of evidence that the mechanism is indeed risk-sharing. First, consistent with our interpretation, clients required to meet more frequently in their first loan cycle were 23% more likely to report financial transfers to friends and relatives outside of their immediate family at the end of the first loan cycle. More than a year later, they remain 29% more likely to say that they would ask a former group member for help in the event of an emergency.

Next, we studied clients' willingness to share risk with one another through a field-based lottery game conducted among a random sample of study clients roughly sixteen months after the first loan cycle ended. Although the lottery operated much like a laboratory trust game, by implementing it in a natural setting we sought to avoid triggering clients' awareness of being participants in an experiment, which has been shown to influence subjects' choices (Levitt and List, 2009).

To achieve this, 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

---

<sup>5</sup>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 matter for changes in social contact.

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 doing so would reduce her individual probability of winning but increase the probability that someone from the group won the lottery, 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 for 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 not if her sole motivation is unconditional altruism.<sup>6</sup>

Relative to a monthly client, a client who had been assigned to a weekly group two years prior was 65% 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. Thus, the accumulated evidence suggests that building social capital among group members improved risk-sharing arrangements, which, in turn, reduced default risk.

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, for a subset of the clients, in a later loan cycle, we again randomized meeting frequency. At this point, the group members

---

<sup>6</sup>Similar variations of dictator or trust games have been used to parse out motives for giving (Ligon and Schechter, 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.

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.

Although joint liability is almost universally emphasized as the key to mitigating default risk in group lending, recent experimental evidence suggests that it has little impact on default (Gine and Karlan, 2009). We find informal risk-sharing arrangements that developed among clients in individual-liability lending groups significantly improved repayment rates, which provides an alternative explanation for the success of the classic group lending model. More broadly, our results 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 that were 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, 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. Like most MFIs, VWS loan groups typically consist of clients from a single neighborhood, which implies that members live in close proximity and are 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 (45%) or acquaintances (5%) rather than friends (5%) or family (8%).

A VWS loan group works as follows: After clients are screened and groups approved by

loan officers, members choose a group leader in whose home the loan officer will conduct weekly repayment meetings for the duration of the loan cycle. The first two meetings are for group nurturing and training. Loan repayment starts in the third week. During each meeting clients take an oath promising to make regular repayment, after which the loan officer collects payment from each member individually and marks passbooks.<sup>7</sup> A loan cycle lasts forty-four weeks and all clients must attend meetings for at least twenty weeks, after which point they may repay the remaining balance in a single installment.

For the experiment, between April and September 2006 we recruited one hundred ten-member groups of first-time clients from neighborhoods in the catchment areas of three VWS branches.<sup>8</sup> At the time of recruitment, clients were told that repayment schedules would be determined by lottery. Before loan disbursement, we randomly assigned thirty groups to the standard weekly repayment schedule and seventy groups to a monthly repayment schedule.<sup>9</sup> Each client received a Rs. 4000 ( \$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 of Rs. 100 starting two weeks after loan disbursement, and those assigned to the monthly schedule in eleven Rs. 400 installments starting one month after loan disbursement. No client dropped out after her repayment schedule was announced. Overall, the median weekly group met thirty seven times during a single loan cycle and the average meeting length was twenty-five minutes (excluding waiting time).

Between group formation and loan disbursement, we administered a baseline survey to 1016 of the 1026 clients. Table 1 and Appendix Table 1 provide a randomization check

---

<sup>7</sup>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 loan, remaining clients repay at the branch office (repayment in branch office is, otherwise, rare).

<sup>8</sup>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.

<sup>9</sup>We originally intended to have two monthly repayment treatment arms: One that met monthly and one that met weekly but only repaid monthly. However, loan officers almost immediately stopped holding weekly meetings among clients required to repay only monthly because they were unsure how to spend the time and because so few clients attended. Hence, we include both groups as monthly repayment.

based on these data. On average, monthly and weekly clients look similar at baseline across a wide range of observable characteristics. No baseline characteristics are significantly different at a 5% level, and only two differences are statistically significant at the 10% level: 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 is not associated with differences in degree of social ties. For instance, they were no more likely to claim they did not know another group member prior to joining VWS (Table 1, Panel B). We have verified that the results are robust to excluding groups with Muslim clients. However, because of these differences, throughout we report regressions with the controls listed in Table 1 and discuss any cases in which our results are sensitive to the inclusion of controls. In addition, Appendix Table 5 reports results for the main outcomes of interest from regressions with no controls.

### 3 Effect of Meeting Frequency on Client Behavior

To gauge the effect of meeting frequency on social capital formation, 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 Contact

We first study whether requiring clients to meet more often during their first loan cycle led them to develop closer friendships that outlasted the experiment. Inducing clients to socialize with one another before and after meetings and observe each other’s behavior with other clients and with the loan officer on a weekly basis may have encouraged friendship formation and thereby changed a client’s social network. Data collected by loan officers during the first loan cycle meetings demonstrated very significant increases in social contact during the loan cycle itself.<sup>10</sup>

---

<sup>10</sup>Results available from authors. To gauge social contact, after marking passbooks loan officers asked each client broad questions about their social ties with other group members (e.g. “How many group

To examine whether these changes persisted, we collected data on current contact with each member of their first loan cycle from a random sample of 432 clients roughly sixteen months after they had repaid their first loan. Our first contact measure is the number of times over the last thirty days the client had visited with a previous group member in either person’s home. The next two outcomes measure the strength of social contact – whether the client still talked to the group member about family and whether they celebrated the main Bengali festival (Durga Puja) together during the previous year. To avoid inferences based on selected outcomes, we compute the average effect size across these three outcomes within an index (on the construction and use of such an index, see Kling et al. (2007)), which we call the social contact index. We also show estimates based on individual components in Appendix Table 2.

Since we have roughly nine observations per client (all other group members), our analysis sample contains 4018 pairwise observations. To avoid double-counting, in cases in which we interviewed both members of a pair, we randomly drop one observation when the outcome is social contact (which cannot vary, in the absence of measurement error, within a pair) and keep all observations for outcomes that can differ within a pair.<sup>11</sup> For member  $i$  matched with group member  $m$  in group  $g$  we estimate

$$y_{gmi} = \beta_1 W_g + X_g \gamma_1 + X_i \gamma_2 + \delta_1 D_i + \delta_2 l_i + \alpha_g + \epsilon_{gmi} \quad (1)$$

where  $y_{gmi}$  is the social contact index,  $W_g$  is an indicator for weekly group,  $D_i$  controls for number of days between loan disbursement and survey,  $l_i$  controls for being the group leader, and  $\alpha_g$  is a loan officer fixed effect. Additional controls, listed in Table 1, are defined at group- and individual- level ( $X_g$  and  $X_i$  respectively).

---

members have you visited in their homes?”). Hence, one concern is that weekly meetings encouraged friendship formation simply because loan officers collected more frequent data on client interactions. While we cannot completely exclude this channel, we assume it is second order to direct interaction with other clients, 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.

<sup>11</sup>Our results are robust to averaging across observations within a pair.

Next, to examine how changes in social contact varied with baseline social distance between pair members, we define five social distance category: whether, prior to joining the MFI, the pair members were (i) immediate family and friends; (ii) relatives more than once removed (distant relatives); (iii) neighbors living within a block (close neighbors); (iv) neighbors living more than a block away (far neighbors); and (v) strangers.<sup>12</sup> We then estimate:

$$y_{gmi} = \beta_1 W_g + \psi \sum_{a=1,..4} W_g \times S_{agmi} + S_{agmi} \phi + X_g \gamma_1 + X_i \gamma_2 + \delta_1 D_i + \delta_2 l_i + \alpha_g + \epsilon_{gmi} \quad (2)$$

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  $X_i$  vector excludes the group composition covariates (i.e. fraction of group members in different social distance categories.)

In both of the above regressions, standard errors are clustered 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).<sup>13</sup>

Table 2 presents the results. Column (1) shows that more than a year after graduating

---

<sup>12</sup>Distances were measured using GPS coordinates collected at baseline. We define a city block as living within a 50-meter radius, which is half of the distance used to define city blocks in developed countries.

<sup>13</sup>We have conducted two other robustness checks. First, following Fafchamps and Gubert (2007) we have checked that our regressions are robust to allowing for spatial correlation of standard errors instead of group clustering. Second, we have checked that the main findings for regressions where we examine heterogeneity by initial social distance between a pair (equation 2) are robust to inclusion of an individual fixed effect.

from their first loan cycle, clients who met weekly were significantly more likely to interact than their monthly counterparts: Moving from a monthly to a weekly schedule led to a 0.15 standard deviation increase in long-run social contact between clients. This average effect is similar in magnitude but statistically insignificant without controls (Appendix Table 5). The individual index components indicate greater social contact among weekly clients (Appendix Table 2). In sum, higher levels of friendship among weekly clients persisted long after mandatory meetings ended.

Column (2) reveals that increases in long-run social contact that accrued to weekly groups were concentrated among client pairs who were distant relatives or close neighbors. Reassuringly, being required to see one another for half an hour once per week did not change social contact among immediate relatives 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 - those who were unknown prior to joining VWS and geographically distant neighbors. Given that clients live in fairly close geographic proximity, this pattern suggests that social capital can be strengthened by more frequent meetings, but cannot so easily be built from scratch. These results are robust to the exclusion of controls (Appendix Table 5).

Looking directly at the index component that measures repeat interaction – number of times the pair visited each other at home in the last thirty days – we observe that the average weekly client pair met 20% more often than their monthly counterpart (0.86 times more per 30 days), although the estimate is noisy (column 3). However, when we allow the effect to differ by initial social distance, we observe large and significant increases in the frequency of social interaction among distant relatives and close neighbors (column 4). Among weekly clients, close neighbors saw each other just as often as immediate family and friends while in monthly groups they saw each other 25% less often (3.7 times fewer per 30 days).

### 3.2 Returns to Social Contact: Impact on Loan Default

Requiring clients to meet more often for six to ten months led to persistent increases in social ties. But did these ties yield economic returns or simply change patterns of friendship? Here, we directly examine whether weekly meetings during a client’s first loan cycle are associated with reductions in one measure of economic vulnerability that is carefully measured and observed in our sample for an extended period: loan default.<sup>14</sup>

We use VWS transactions data to track client repayment behavior during both their first (experimental) and second loan cycles. At the end of our loan experiment (clients’ first loan cycle), 69% of clients took out a second loan with VWS. The retention rate did not differ across monthly and weekly clients (Table 3, column (2)) and a comparison of observable characteristics among clients who continued onto the next loan shows no differences in client selection across treatment arms (Appendix Table 3).<sup>15</sup> On average, the second loan was 35% larger than the first, and all clients repaid their second loan on a fortnightly schedule.<sup>16</sup>

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).<sup>17</sup> Across loan cycles, increases in loan size were accompanied by significant increases in default rates (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. Here, we investigate whether default rates varied with meeting frequency faced by a client in her *first* loan cycle. We estimate separate regressions for loan default in the first and second loan cycles. For client

---

<sup>14</sup>While default reflects more than just vulnerability to shocks, typically, 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 4).

<sup>15</sup>One characteristic out of 20 is significantly different between monthly and weekly who continue - whether someone in household earns fixed salary. However, there are no apparent differences in household income or other measures of socio-economic status, and fixed salary is not a significant determinant of default controlling for other household observables (Appendix Table 4).

<sup>16</sup>While there is some variation in loan amount in the second loan, we also find that second loan size is independent of first loan repayment schedule (see Table 3, column (3)).

<sup>17</sup>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.

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

$$y_{gil} = \beta_1 W_g + X_g \gamma_1 + X_i \gamma_2 + \delta_1 l_i + \alpha_l + \epsilon_{gli} \quad (3)$$

as before,  $W_g$  is an indicator variable for the client being on a weekly schedule in the first loan cycle,  $X_g$  and  $X_i$  are group and individual characteristics as measured during the first loan cycle.  $l_i$  controls for being the group leader in the first loan cycle, and  $\alpha_l$  is a loan officer fixed effect (for loan cycle being considered). Very low default rates imply we can estimate OLS but not Probit regressions for the first loan cycle. For the second loan cycle we report both Probit regressions (with no fixed effects) and OLS regressions (with controls which include loan size).

Column (1) of Table 3 shows that frequent meetings were *not* associated with lower default in the first loan cycle. An important caveat is that default was extremely low for first-time borrowers (1.8% among monthly clients), presumably due to the fact that, following standard MFI practice, first loan sizes were below client “carrying capacity.” However, once clients had graduated to larger loans, differences in default emerged despite the fact that all clients had by that point converged to the same meeting frequency (fortnightly). In column (4) we report Probit estimates showing that clients assigned to monthly meetings for their first loan were four times (7.8%) 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.

Next we examine whether default reductions were concentrated among weekly clients most likely to experience gains in social contact. The point estimates in columns (6) and (7) indicate that this is the case: There was a large and significant effect of weekly meeting on default only among clients with a sufficient number of close neighbors and/or distant relatives in their group. The point estimate on the interaction term is similar without controls, but loses significance (Appendix Table 5).

Arguably, meeting frequency during the first loan cycle may have directly influenced

default outcomes for the second loan. For instance, ability to repay in small installments could influence long-run income through investment choices or changes in long-run savings habits (Fischer and Ghatak, 2010). The systematic variation by group composition provides evidence that default patterns are not driven entirely by the direct effect of meeting frequency. That is, 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 that do not directly predict default. 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.

## 4 Evidence of Improvements in Risk-Sharing

An obvious channel through which increased social contact can reduce default is improvements in risk-sharing arrangements which help clients insure themselves against shocks to income or expenditure that might otherwise leave them unable to make required loan repayments. In the remainder of this paper we present direct evidence from survey and experimental data of improvements in informal insurance arrangements between clients which supports this interpretation.

### 4.1 Survey

We first explore differences in informal insurance across monthly and weekly groups by considering survey data collected at the end of the first loan cycle on the number and amount of transfers over the past year with individuals of different relationship categories. 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, (ii) other relatives, friends and neighbors and (iii) other non-relatives, a category that is unlikely to include group members.

Column (1) of Table 4 shows that weekly clients were no more likely than monthly clients to give transfers to close family members. On the other hand, they were 9.5

percentage points (23%) more likely to report transfers to friends and relatives outside the immediate family at the end of the experiment, and the difference is significant at the 6% level. Since this category includes non-group members as well as group members, the difference implies that improved risk-sharing among group members did not simply displace risk-sharing arrangements outside of the group. Nor do these transfers appear to displace transfers within the immediate family or to other non-relatives (columns 1 and 3), suggesting a net gain in informal insurance. Overall the transfers data demonstrate that increased meeting frequency expanded risk-sharing networks, as measurable returns to new relationships do not appear to be crowding out equally valuable relationships with people outside of the loan group.

A second set of survey data allows us to examine client perceptions of group members' trustworthiness and risk-sharing capacity. These data were collected at the same time as the social contact data (more than a year after the end of clients' first loan cycle). The outcome in column (4) is a client's perception of her average group member's trustworthiness.<sup>18</sup> More than a year after repaying the loan, those who were on a weekly schedule ranked average group member trustworthiness 0.25 points higher (on a 1-5 scale) and the difference is statistically significant with or without controls.<sup>19</sup> In column (5), we consider a client's response to a hypothetical question on whether she believed that a particular group member would help her in the event of illness. Weekly clients were 6.6 percentage points (29%) more likely to report that a group member would provide assistance in such an emergency, which is statistically significant with but not without controls. These results suggest that weekly clients are more trusting of group members, a prerequisite for increased willingness to provide informal insurance.

---

<sup>18</sup>Our regression specification is the individual level equivalent of equation (1).

<sup>19</sup>The client was described the following scenario: "Imagine a person walking down the street sees someone in front of him/her drop their wallet. Upon inspection, she finds that the wallet contains Rs 200 and the owner's name and phone number. The finder must decide whether to keep it or return it to its owner." She was asked to rank the likelihood that the finder would return the wallet if she was her average group member on a 1-5 scale described as follows: "1-Would not return the money. 2- Unless someone knows she has got the wallet, would not return it. 3-As likely to return as not. 4- Will return, but might take up to a week. 5- Will return immediately."

## 4.2 Lottery

The default and survey evidence supports the view that greater social contact expanded MFI clients' risk-sharing networks. However, a shortcoming of our survey data is that we do not directly observe instances of risk-sharing between group members, only aggregate outcomes such as total transfers. To provide direct evidence on the role of 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. This evidence is an important complement to our default results since the lottery was conducted with a random sample of *all* clients who entered the meeting frequency experiment as part of their first loan cycle (and is, therefore, free of any selection concerns related to client retention). The lottery occurred more than a year after clients completed their first loan cycle (average final repayment and survey dates were April 2007 and July 2008, respectively). 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. Table 1 provides a randomization check using group-level (Panel A) and client-level (Panel B) variables. Column (4) shows that the lottery sample is representative of the experimental population, and columns (5) and (6) examine the balance of voucher randomization (described below) on multiple characteristics. As before, the two characteristics which are unbalanced remain fraction Muslim and years in the neighborhood. We continue to report regressions with the full set of controls.

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.<sup>20</sup> The lottery prize

---

<sup>20</sup>Importantly, the lottery protocol was conducted before the Table 4 survey data were collected.

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 an effort to attract and retain clients. For this reason, our intervention was likely to seem natural in this setting. Furthermore, the lottery script was designed to convey the impression that participants had been selected in order to reward them for survey participation during the first loan cycle.<sup>21</sup>

The client was informed that, in addition to her, the lottery included ten clients from different VWS branches, whom she was therefore unlikely to know. If she agreed to enter the draw (all clients agreed), she was then given the opportunity to enter any number of other members from 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 be given 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 randomly varied the divisibility of the lottery prize offered to a participant. For half of the participants, the lottery 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 voucher divisibility randomization allows us to isolate cooperative from purely altruistic motives for ticket-giving. We discuss how the lottery timing allows us to examine the importance of moni-

---

<sup>21</sup>Hence, it should also seem natural that they were offered to give tickets to first loan cycle group members rather than third loan cycle group members.

toring versus just learning about other clients' type in driving ticket-giving behavior.

### 4.3 Predictions

A client belonging to a ten-member group made nine pair-wise choices over giving tickets 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 receiving group member 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 twelve). 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 clients awareness of being participants 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 a lottery the participant only shares an opportunity to win money (at some expense to herself). This suggests that the signalling 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 tickets given since total tickets determine the odds of any one person winning. If the sender trusts all other 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 further constrain ticket-giving to less trusted group members in order to maximize expected returns to the more trusted group members. Thus, relative to a trust game, group composition will play a more important role in a lottery game. In the current paper, however, we do not investigate further this group composition externality.

Next, we consider the implications of initial meeting frequency for ticket-giving behavior. In Section 3.1 we saw that higher meeting frequency in a client’s first loan cycle strengthened social ties. Stronger social ties 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 and make use of baseline heterogeneity across clients in self-reported financial autonomy to make transfers. Neither a more divisible lottery prize nor the receiver’s ability to make transfers should induce greater ticket-giving unless the sender cares about the ease of reciprocal transfers.<sup>22</sup> Hence,

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

---

<sup>22</sup>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.

Meeting more frequently during the first loan cycle can encourage reciprocal arrangements between client pairs in multiple 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 provide a public signal of a member’s action (Costa, 2007).<sup>23</sup> Hence, a higher frequency of meeting in the long run will continue to improve monitoring by improving the precision of the public signals.

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 the same 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 cycle and a high proportion of their current group members were the same as in their first loan cycle. These clients had interacted on a regular basis for close to two years. At the start of this third loan cycle clients had been re-randomized (at the group-level) into groups which 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 assigned to either a weekly or monthly group at the time of the lottery.

---

<sup>23</sup>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).

## 4.4 Results

Our primary 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, very similar to individuals’ propensity to give 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 the trust and dictator game literatures. Educated clients were more likely to give and receive tickets, while richer respondents, those who stated in baseline that they could make transfers outside of their household (financial control), and the group leader received more tickets. In contrast, respondents who participated in community and political events were more likely to give but not receive tickets.

### 4.4.1 Initial 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 form given by equation (1). 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 of columns (1) and (4) 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 65% more likely to give tickets than monthly clients (12.8 percentage points) whereas there was almost no difference between monthly and weekly clients when the prize was a single large voucher.<sup>24</sup>

---

<sup>24</sup>In the pooled sample, the estimate of the coefficient on the interaction between weekly and divisible prize is similar in magnitude but statistically insignificant, with a t-statistic of 1.54.

Significantly higher ticket-giving among weekly clients when the lottery prize was easily divisible suggests that more frequent meetings increased ticket-giving by increasing expectations of reciprocity.<sup>25</sup> If frequent meetings had only increased *unconditional* altruism, then ticket-giving would 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., square nodes) gift voucher randomization.

In columns (2) and (5), we examine whether ticket giving is sensitive to the level of financial control exercised by the receiver. In total, 89% of clients responded affirmatively to the baseline survey question, “If a close relative like your parent or sibling fell sick and needed money, would you be able to lend money to that relative, if you had the extra money?” If risk-sharing motivated the higher level of ticket-giving observed in the divisible voucher case, and assuming that this characteristic was to some extent observable to other members, we would expect a client to avoid giving tickets to those who were unable to reciprocate. In line with this, we see that marginal ticket-giving was concentrated among the set of potential receivers who reported that they had the financial independence to make transfers outside of the household.

Finally, 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 observed a significant effect on social contact (Table 2) supports our interpretation that higher social contact 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 (unreported)

---

<sup>25</sup>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.

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.

Monthly clients' ticket-giving behavior was similar across the two voucher categories and, in general, was independent of ability to reciprocate. This suggests that the primary motivation among monthly clients was not reciprocity and/or that 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 between baseline and follow-up surveys conducted when clients joined VWS and at the end of their first loan cycle respectively (unreported).

Six months after the lottery we surveyed 39 of the 47 clients who received a ticket from a group member and won that lottery.<sup>26</sup> In all cases winners redeemed their vouchers as opposed to giving them away. This suggests that the divisible voucher encouraged risk-sharing by priming participants to increase ticket-giving with group members with whom they had reciprocal relationships rather than motivating reciprocal behavior in lottery voucher redemption *per se*. Consistent with this, the same survey revealed that nearly all clients (85%) remembered who gave them their ticket, and a quarter reported greater willingness to share post-lottery. Strikingly, 7 of the 23 weekly clients (30.4%) but only 1 of the 16 monthly clients (6.3%) report such willingness.<sup>27</sup> This is consistent with higher

---

<sup>26</sup>Among this subset of “indirect” lottery winners, 25 were in weekly and 22 were in monthly groups.

<sup>27</sup>The specific question was, “If your friend asked to borrow money or goods from you in the future, do you think you are more, less, or equally likely to share with her than you were before the lottery?” In terms of actual sharing, the most commonly shared goods (post-lottery) were food and sarees. In two cases, winners reported lending money to the group member who had given them the ticket.

rates of risk-sharing relative to altruism among weekly clients.

#### 4.4.2 Hastening versus Sustaining Cooperation

As discussed in Section 4.3, 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. 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 there is 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 (unreported).

We consider the sub-sample of all 48 third loan cycle clients who had been on the weekly schedule in the first intervention. These clients were spread across fourteen weekly repayment and twelve 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 this subset of 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. Given the reduced sample size we only control for month of loan cycle formation

in the regressions (we observe similar but noisier estimates with controls) and 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”) saw one another significantly more often both at MFI meetings and outside of meetings than clients initially on the weekly schedule but later assigned to the monthly schedule (“weekly-monthly”).<sup>28</sup> Correspondingly, a weekly-weekly client was more than three times as likely (41.6 percentage points) to engage in pro-social behavior than a weekly-monthly client when the prize was easily divisible (column 3). As before, we find no evidence of increased giving for the indivisible voucher option (column 4). We interpret the difference for weekly-weekly clients in column (3) 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 can occur is uncertain, it is important to note that, by the time of the survey, clients in this subsample had been interacting regularly for 2.5 loan cycles during which time 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 the propensity to remember the names of their first loan group members at the time of survey (column 5).

## 4.5 The Cost of Building Social Capital

Evidence of 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 non-trivial transactions costs of meeting four times as often. For clients, we estimate that weekly meetings entail approximately two additional hours per month, or 15 hours over the course of an average loan cycle. Meanwhile, banks could cut

---

<sup>28</sup>In column (1), the dependent variable is the number of required MFI meetings across the first and third intervention (at the time of the survey). At the time of the lottery, a weekly-monthly client had met, on average, 41 times, while a weekly-weekly client had met roughly 30% more often.

transactions 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.<sup>29</sup> Hence, a conservative back-of-the-envelope calculation suggests that weekly meetings may be cost-effective for a MFI, which explains why MFIs persist with high frequency repayment schedules despite the higher transactions 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 total 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, and the total benefits of increases in social capital are likely to include, in addition to the reduction in default risk, additional positive externalities such as information transfers between clients.<sup>30</sup> That said, the direct importance of using meetings to obtain improvements in 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 obtained in the absence of group liability contracts, and provide a rationale for the current trend among MFIs of maintaining repayment in group meetings despite the transition from group to individual liability contracts (Gine and Karlan, 2009).

---

<sup>29</sup>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.

<sup>30</sup>While there is one potentially important negative externality that we also do not observe – crowd out of other forms of social capital enjoyed by the client – as argued in Section 3.2, the reduction in client default arguably captures the net effect of meeting frequency on social capital inside and outside of loan groups.

## 5 Conclusions

A widely held belief across social scientists in many disciplines is that social interactions encourage norms of reciprocity and trust. In fact, participation in groups is often used to measure an individual's or community's degree of economic cooperation (see, for instance, Narayan and Pritchett, 1999; Alesina and Ferrara, 2002). 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. Using field experiments, we provide rigorous evidence that repeat interactions can in practice facilitate cooperative behavior 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 micro-finance 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.

Our findings support the idea that complementarities in social capital acquisition create the possibility of multiple equilibria (Glaeser et al., 2002). This, in turn, suggests potentially large gains from policies which facilitate interaction and help coordinate social capital investments, especially in low income countries where formal risk-sharing arrangements remain limited. 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 the most rapidly. Understanding how other development programs or public policies can be designed to enhance the social infrastructure of poor communities is a promising area of future research.

## References

- Abreu, D., P. Milgrom, and D. Pearce (1991). Information and Timing in Repeated Partnerships. *Econometrica* 59.
- Adams, W., L. Einav, and J. Levin (2009). Liquidity Constraints and Imperfect Information in Subprime Lending. *American Economic Review* 99(1), 49–84.
- Alesina, A. and E. L. Ferrara (2002). Who Trusts Others? *Journal of Public Economics* 85(2), 207–234.
- Ambrus, A., M. Mobius, and A. Szeidl (2010). Consumption Risk-sharing in Social Networks . mimeo.
- Barrios, T., R. Diamond, G. Imbens, and M. Kolesar (2010). Clustering, Spatial Correlation and Randomization Inference. Harvard Economics Department Working Paper.
- Berg, J., J. Dickhaut, and K. McCabe (1995). Trust, Reciprocity and Social History. *Games and Economic Behavior* 10, 122–142.
- Besley, T. and S. Coate (1995). Group Lending, Repayment Incentives and Social Collateral. *Journal of Development Economics* 46(1), 1–18.
- Cameron, C., J. Gelbach, and D. Miller (2008). Bootstrap-Based Improvements for Inference with Clustered Errors. *Review of Economics and Statistics* 90, 414–427.

- Carter, M. and M. Castillo (2004a). Does It Take More than Markets to Get Ahead? An Experimental Approach to Social Capital in South Africa. Agricultural and Applied Economics Staff Paper Series, Paper No. 448, University of Wisconsin.
- Carter, M. and M. Castillo (2004b). Morals, Markets and Mutual Insurance: Using Economic Experiments to Study Recovery from Hurricane Mitch. In C. Barrett (Ed.), *Exploring the Moral Dimensions of Economic Behavior*. Routledge.
- Costa, A. O. (2007). Frequent Monitoring in Repeated Games under Brownian Uncertainty. Mimeo, Universidad Carlos III de Madrid.
- Costa, D. and M. Kahn (2003). Understanding the Decline in American Social Capital, 1953-1998. *Kyklos* 56(1), 17–46.
- DiPasquale, D. and E. Glaeser (1999). Incentives and Social Capital: Are Homeowners Better Citizens? *Journal of Urban Economics* 45(2), 354–384.
- Do, Q.-A., S. Leider, M. Mobius, and T. Rosenblat (2009). Directed Altruism and Enforced Reciprocity in Social Networks. *Quarterly Journal of Economics*, forthcoming 124(4).
- Fafchamps, M. and F. Gubert (2007). The Formation of Risk-Sharing Networks. *Journal of Development Economics* 83(2), 326–50.
- Fischer, G. and M. Ghatak (2010). Repayment Frequency and Lending-Contracts with Present-Biased Borrowers. London School of Economics Working Paper.
- Forsythe, R., J. Horowitz, N. Savin, and M. Sefton (1994). Fairness in Simple Bargaining Games. *Games and Economic Behavior* 6, 347–69.
- Fudenberg, D. and D. Levine (2009). Repeated Games with Frequent Signals. *Quarterly Journal of Economics* 124, 233–265.
- Gine, X. and D. Karlan (2009). Group versus Individual Liability: Long Term Evidence from Philippine Microcredit Lending Groups. Working Paper.

- Glaeser, E., D. Laibson, and B. Sacerdote (2002). An Economic Approach to Social Capital. *Economic Journal* 112(483), 437–458.
- Glaeser, E., D. Laibson, J. Scheinkman, and C. Soutter (2000). Measuring Trust. *Quarterly Journal of Economics* 115(3), 811–846.
- Gneezy, U., W. Guth, and F. Verboven (2000). Presents or Investments? An Experimental Analysis. *Journal of Economic Psychology* 21(5), 481–493.
- Guiso, L., P. Sapienza, and L. Zingales (2004). The Role of Social Capital in Financial Development. *American Economic Review* 94(3), 526–556.
- Humphreys, N., J. Fearon, and J. Weinstein (2009). Can Development Aid Contribute to Social Cohesion After Civil War? *American Economic Review* 99(2), 287–291.
- Karlan, D. (2005). Using Experimental Economics to Measure Social Capital and Predict Real Financial Decisions. *American Economic Review* 95(5), 1688–1699.
- Karlan, D., M. Mobius, T. Rosenblat, and A. Szeidl (2009). Trust and Social Collateral. *Quarterly Journal of Economics* 124, 1307–1361.
- Kling, J., J. Liebman, and L. Katz (2007). Experimental Analysis of Neighborhood Effects. *Econometrica* 75, 83–119.
- Knack, S. and P. Keefer (1997). Does Social Capital Have an Economic Payoff? A Cross-Country Investigation. *Quarterly Journal of Economics* 112(4), 1251–1288.
- Levitt, S. D. and J. A. List (2009). What Do Laboratory Experiments Measuring Social Preferences Reveal About the Real World? *Journal of Economic Perspectives* 21(2), 153–174.
- Ligon, E. and L. Schecter (2008). The Value of Social Networks in Rural Paraguay. University of Wisconsin Working Paper.

- Manski, C. (1993). Identification of Endogenous Social Effects: The Reflection Problem. *Review of Economic Studies* 60(3), 531–542.
- Manski, C. (2000). Economic Analysis of Social Interactions. *Journal of Economic Perspectives* 14(3), 115–136.
- Miguel, E., P. Gertler, and David (2005). Does Social Capital Promote Industrialization? Evidence from a Rapid Industrializer. *Review of Economics and Statistics* 87(4), 754–762.
- Narayan, D. and L. Pritchett (1999). Cents and Sociability: Household Income and Social Capital in Rural Tanzania. *Economic Development and Cultural Change* 47(4), 871–97.
- Olken, B. (2009). Do Television and Radio Destroy Social Capital? Evidence from Indonesian Villages. *American Economic Journal: Applied Economics*, forthcoming.
- Putnam, R. (1993). *Making Democracy Work: Civic Traditions in Modern Italy*. Princeton, NJ: Princeton University Press.

Table 1. Group-level and Client-level Randomization Check

	Summary Statistics- All Clients		Weekly/Monthly Difference			
	Weekly	Monthly	All Clients	Lottery Clients	4-Rs. 50 Voucher Prize	1-Rs. 200 Voucher Prize
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A: Group-level</b>						
Number of Clients	10.233 [0.689]	10.300 [0.709]	-0.067 (0.153)	-0.063 (0.156)		
Month of Formation	5.667 [1.561]	5.657 [1.371]	0.010 (0.312)	0.043 (0.315)		
Fraction Muslim	0.000 [0.000]	0.077 [0.192]	-0.077 (0.045)	-0.079 (0.046)		
N	30	70				
<b>Panel B: Client-level</b>						
Age	33.376 [8.330]	33.461 [8.387]	-0.085 (0.683)	-1.056 (0.765)	-1.75 (1.086)	-0.328 (1.200)
Literate	0.853 [0.355]	0.838 [0.369]	0.015 (0.031)	0.021 (0.042)	0.030 (0.052)	0.011 (0.055)
Highest School Class Completed	6.556 [3.484]	6.670 [3.638]	-0.115 (0.367)	0.138 (0.491)	0.215 (0.639)	0.059 (0.640)
Married	0.876 [0.330]	0.865 [0.342]	0.011 (0.025)	-0.011 (0.035)	-0.018 (0.057)	-0.004 (0.043)
Household Size	3.974 [1.148]	3.915 [1.410]	0.058 (0.093)	0.059 (0.135)	0.233 (0.207)	-0.124 (0.174)
Worked for Pay in Last 7 Days	0.585 [0.494]	0.530 [0.499]	0.055 (0.046)	0.043 (0.061)	-0.004 (0.071)	0.092 (0.081)
Household Savings	3616 [31086]	2445 [12286]	1171 (1876)	-917 (811)	-1646 (1322)	-152 (894)
Value of Assets (Rs.)	10704 [27016]	9038 [21923]	1666 (1953)	547 (2118)	3042 (3849)	-2071 (1597)
Years Living in Neighborhood	15.327 [10.275]	16.997 [10.152]	-1.670 (0.739)	-2.635 (0.985)	-3.326 (1.320)	-1.910 (1.579)
Financial Control	0.905 [0.293]	0.868 [0.339]	0.038 (0.044)	0.024 (0.049)	0.034 (0.049)	0.014 (0.068)
Fraction Distant Relatives	0.067 [0.121]	0.052 [0.106]	0.015 (0.015)	0.005 (0.018)	0.019 (0.021)	-0.010 (0.019)
Fraction Close Neighbors	0.109 [0.200]	0.107 [0.167]	0.001 (0.033)	-0.001 (0.034)	0.055 (0.043)	-0.059 (0.029)
Fraction Distant Neighbors	0.385 [0.324]	0.418 [0.323]	-0.033 (0.045)	-0.029 (0.048)	-0.084 (0.053)	0.029 (0.062)
Fraction Didn't Know	0.334 [0.339]	0.326 [0.346]	0.008 (0.048)	0.007 (0.054)	-0.014 (0.059)	0.028 (0.069)
N	306	710	1016	428	219	209

## Notes

<sup>1</sup> Month of Formation refers to calendar month of group formation ("4" for groups formed in April, 2006, and so on). Close Neighbors are neighbors living within 50m. The omitted relationship type is Close Family/Friends, and all relationship types are defined at time of loan group formation. Financial Control is whether clients responds "Yes" to "If a close relative like your parents or siblings fell sick and needed money would you be able to lend money to that relative, if you had the extra money?"

<sup>2</sup> Columns (3)-(6) report tests of differences of means (weekly minus monthly) for the subsamples. Standard errors are clustered by group.

Table 2. Meeting Frequency and Social Contact

	Social Contact Index		Total Times Met	
	(1)	(2)	(3)	(4)
Weekly	0.152 (0.058)	0.088 (0.177)	0.864 (0.597)	-0.961 (2.124)
Distant Relative		0.020 (0.168)		0.481 (1.885)
Close Neighbor (<50m)		-0.554 (0.137)		-3.669 (1.543)
Distant Neighbor(>50m)		-0.904 (0.099)		-8.839 (1.300)
Didn't Know		-1.208 (0.101)		-11.689 (1.316)
Weekly*Distant Relative		0.421 (0.231)		6.331 (3.443)
Weekly*Close Neighbor		0.320 (0.252)		4.890 (2.848)
Weekly*Distant Neighbor		0.031 (0.190)		1.890 (2.238)
Weekly*Didn't Know		-0.012 (0.174)		0.739 (2.174)
Mean of Monthly			4.384 [9.676]	
N	3137	3137	3137	3137

Notes:

- 1 Social Contact Index generates average effect size from three client-level variables: (1) Total Times Met (reference period past 30 days), (2) "Do you still talk to X about her family," and (3) "During the most recent Durga Puja, did you attend any part of the festival with X?"
- 2 Distant Relative, Close Neighbor, Distant Neighbor, and Didn't Know are responses to pairwise relationship type before joining VWS. 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 All regressions include one response per loan group pair. Regressions include group-level and client-level controls (Table 1, Panels A and B), days between loan disbursement and lottery survey, and whether member is group leader. Columns (2) and (4) exclude client-level controls for Fraction Distant Relatives, Fraction Close Neighbors, Fraction Distant Neighbors, and Fraction Didn't Know. Standard errors are clustered by group.

Table 3. Meeting Frequency and Loan History

	First Loan Default	Took Out Second Loan	Second Loan Size (Rs.)		Second Loan Default		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Weekly	-0.014 (0.014)	-0.015 (0.047)	-57.8 (124.2)	-0.078 (0.029)	-0.087 (0.039)	-0.046 (0.031)	-0.055 (0.040)
Fraction Distant Relatives or Close Neighbors in Group						0.096 (0.082)	0.147 (0.116)
Weekly*Fraction Distant Relatives or Close Neighbors in Group						-0.236 (0.123)	-0.205 (0.099)
Specification	OLS	OLS	OLS	Probit	OLS	Probit	OLS
Mean of monthly	0.018 [0.133]	0.667 [0.472]	7873.0 [1026.1]	0.108 [0.301]			
N	1026	1026	707	707	707	707	707

## 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.
- 2 Relationship type is defined before joining VWS.
- 3 OLS Regressions include loan officer fixed effects (for that cycle), controls for the variables in Table 1, and whether member is group leader. Regressions in Columns (5) and (7) also control for second loan size. Probit regressions report marginal effect coefficients and do not include controls or fixed effects. Standard errors are clustered at the group-level.

Table 4. Meeting Frequency and Transfers/Trust Measures

	Transfers			Trust Measures	
	Close Family	Neighbor/ Other Relative/ Friend	Other Non- Relative	Lottery Sample	
				Trust Group Members	Would Help if Sick
	(1)	(2)	(3)	(4)	(5)
Weekly	0.030	0.095	-0.016	0.246	0.066
	(0.052)	(0.049)	(0.021)	(0.116)	(0.028)
Specification	Client-level	Client-level	Client-level	Client-level	Pairwise
Mean of monthly	0.369	0.405	0.057	4.334	0.225
	[0.483]	[0.491]	[0.232]	[1.122]	[0.417]
N	961	961	961	432	4018

## Notes:

- 1 Transfers in Columns (1)-(3) refer to transfers given/received by clients' household in 12 months before first loan endline survey, and are indicator variables for whether clients' household gave/received any transfers to/from the relevant groups. Relationship type is defined at end of loan cycle for transfers. Trust Group Member is the client response on a 1 to 5 scale, increasing in the likelihood that the average group member would return a wallet with Rs. 200 that was found on the street. Would Help if Sick is the indicator variable "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?"
- 2 Regressions include loan officer fixed effects and controls for the variables in Table 1. Columns (4) and (5) also control for days between loan disbursement and lottery survey, and whether member is group leader. Standard errors are clustered at the group-level.

Table 5. Meeting Frequency and Ticket-Giving

	Gave Ticket					
	4-Rs. 50 Vouchers			1-Rs. 200 Voucher		
	(1)	(2)	(3)	(4)	(5)	(6)
Weekly	0.128 (0.048)	-0.011 (0.050)	0.093 (0.053)	0.003 (0.054)	-0.077 (0.065)	-0.020 (0.056)
Financial Control		0.051 (0.031)			0.017 (0.043)	
Weekly*Financial Control		0.145 (0.057)			0.088 (0.067)	
Distant Relative or Close Neighbor			-0.172 (0.169)			-0.199 (0.072)
Weekly*Distant Relative or Close Neighbor			0.263 (0.070)			0.123 (0.084)
Mean of monthly	0.196 [0.397]			0.241 [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. See notes to Table 1 for Financial Control and relationship type definitions.
- 2 Regressions include group-level and client-level controls (Table 1, Panels A and B), days between loan disbursement and lottery survey, and whether member is group leader. Standard errors are clustered at the group-level.

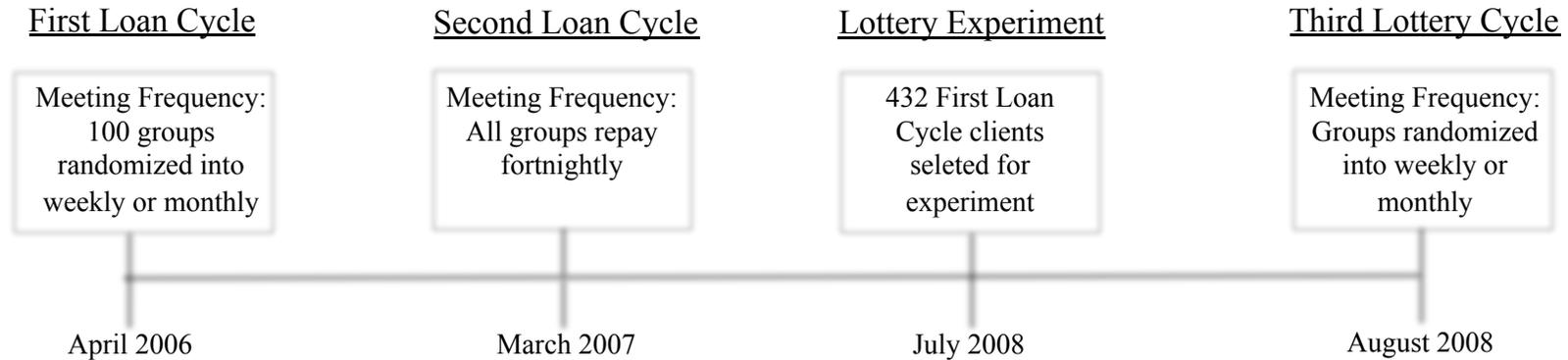
Table 6. Meeting Frequency across Loan Cycles and Pro-Social Behavior

	Number of Pre-Lottery MFI Meetings Attended	Third Loan Social Contact Index	Gave Ticket		Member Remember
			4-Rs. 50. Vouchers	1-Rs. 200 Voucher	
	(1)	(2)	(3)	(4)	(5)
Weekly in First Loan, Weekly in Third Loan	12.572 (1.837)	1.286 (0.430)	0.416 (0.070)	-0.150 (0.088)	0.011 (0.055)
P-value (Wild Bootstrap)	<i>0.020</i>	<i>0.152</i>	<i>0.094</i>	<i>0.554</i>	<i>0.952</i>
Mean of Weekly in First Loan, Monthly in Third Loan	41.388 [4.220]		0.126 [0.333]	0.281 [0.451]	0.669 [0.472]
Specification	Client-level	Client-level	Pairwise	Pairwise	Pairwise
N	48	47	251	204	455

## Notes

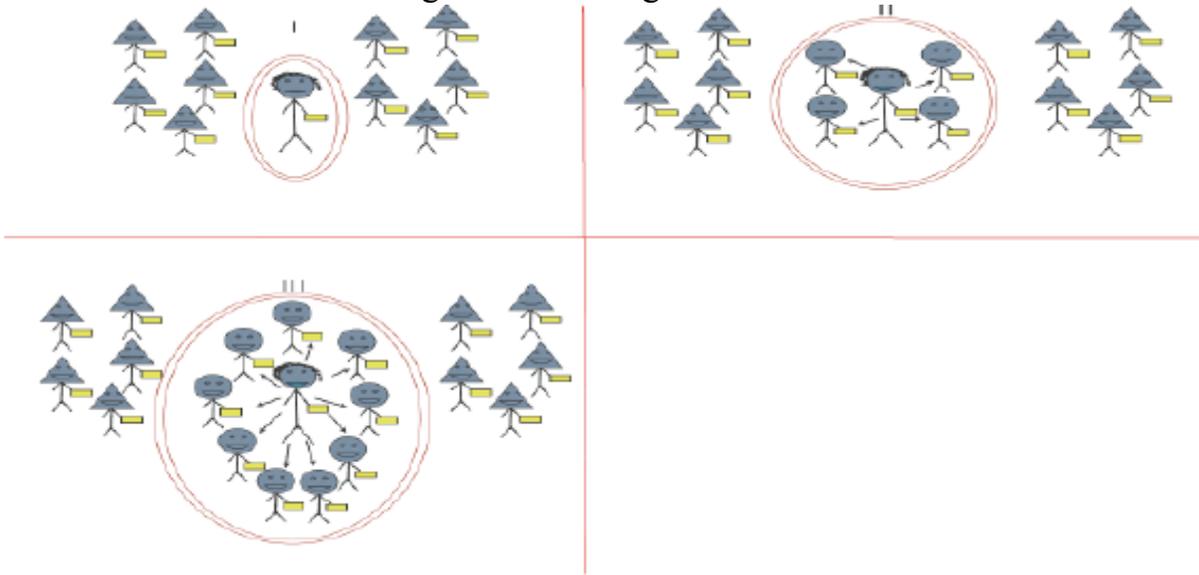
- 1 The sample is First Loan weekly clients who went on to take out a Third Loan with VWS. Number of Pre-Lottery MFI Meetings is the total number of times the client's First Loan and Third Loan group met to repay before the client was surveyed for lottery. Social Contact Index is the average of the normalized versions of five Third Loan group meeting variables: "For how many members of your group do you know whether there is a marriage ceremony in the family in the coming 30 days?", "How many group members have you visited in their houses in the last 2 weeks?", "How many group members have visited you in your house in the last 2 weeks?", "How many people in the group did you talk to about business matters in the last 2 weeks?", and "How many people in the group did you talk to about personal matters in the last 2 weeks?" For each variable, we use client-level average across all observations before month 4 of the Third Loan Cycle (when the average client was surveyed for the lottery). Gave Ticket is as defined in notes to Table 5. Member Remember is the indicator variable "Do you remember this group member?" Data in Columns (1)-(2), was collected over the course of Third Loan Cycle, while data in Columns (3)-(5) was collected at the time of lottery.
- 2 Regressions include month of First and Third Loan group formation fixed effects. Wild bootstrapping is used to correct standard errors.

Figure 1. Timeline



Notes: 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.

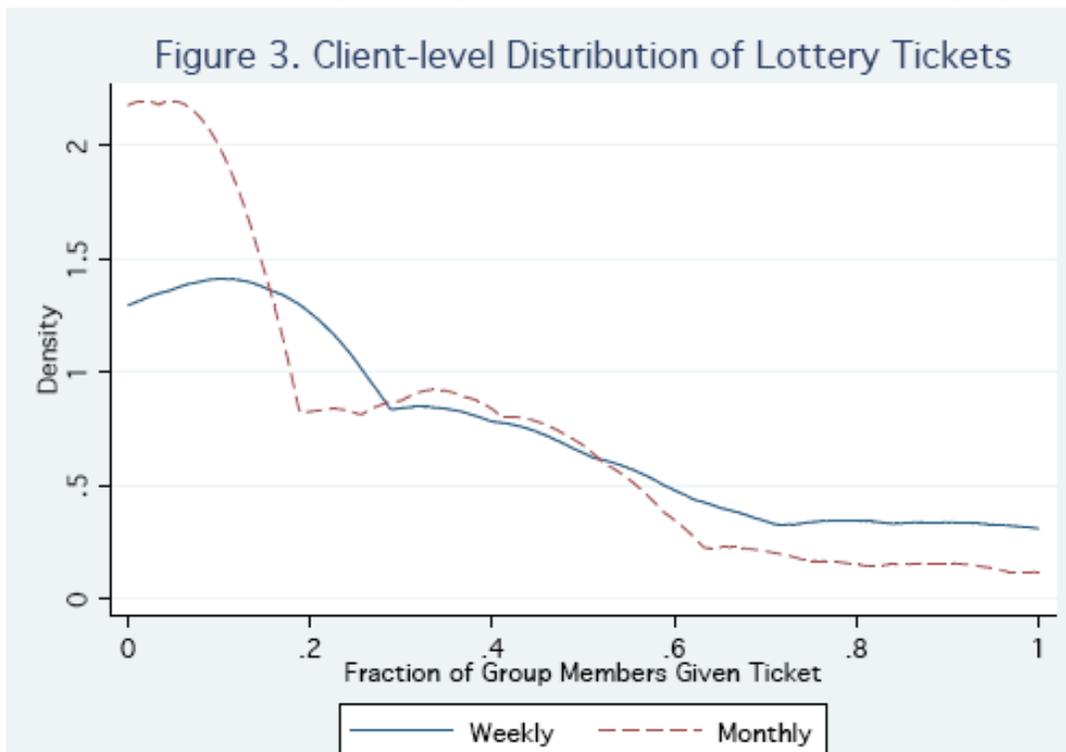
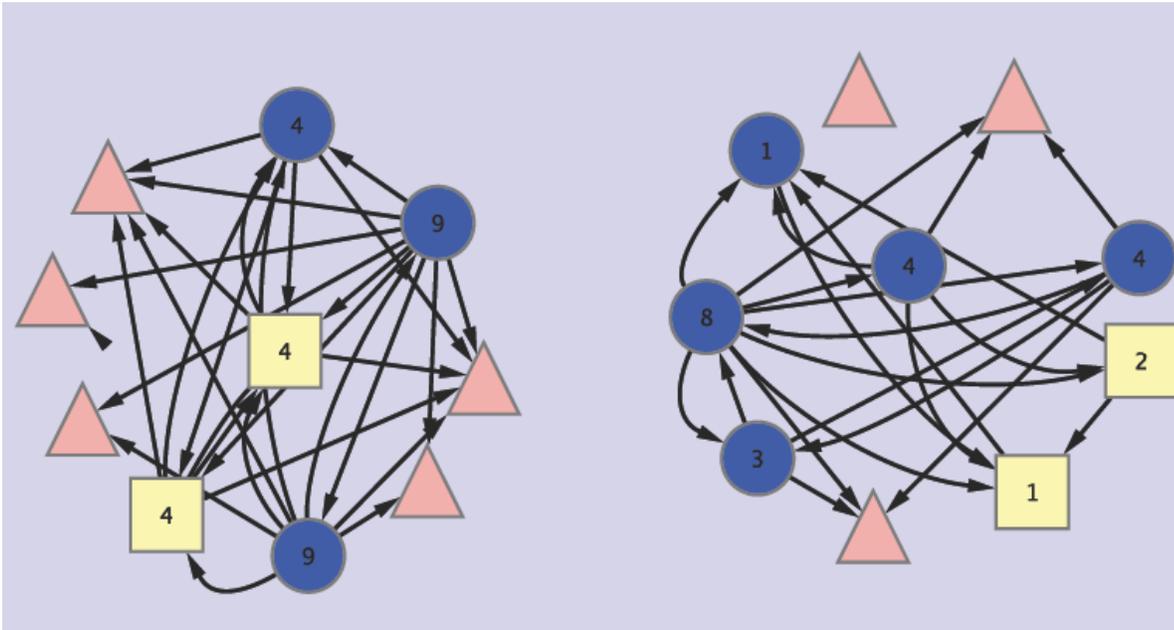
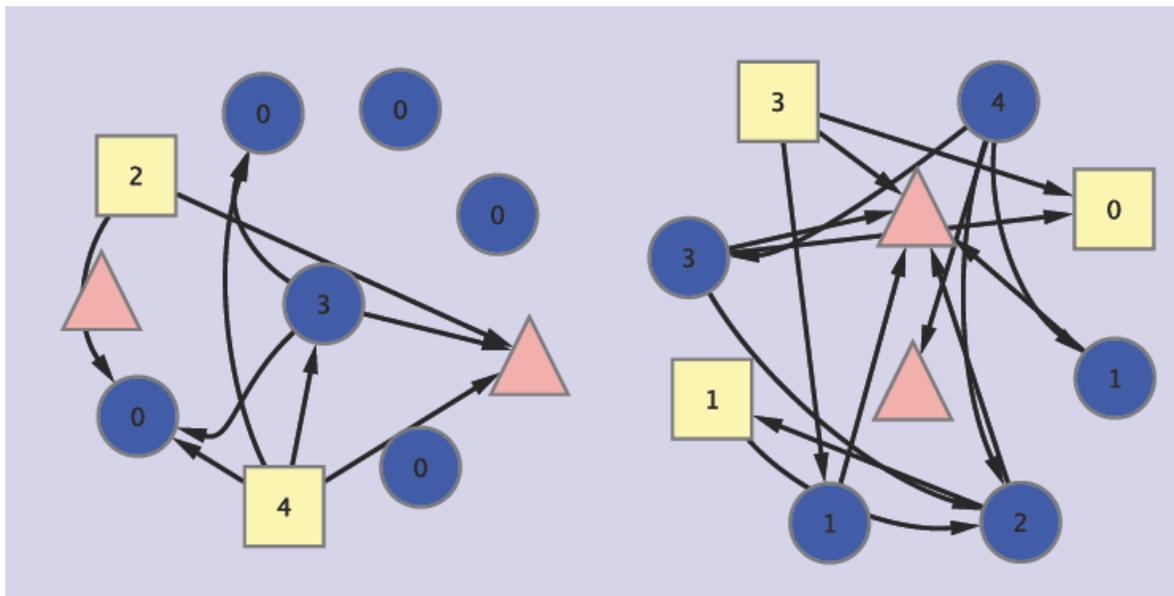


Figure 4. Network Structures

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.

# NOT FOR PUBLICATION

Appendix Table 1. Additional Client-level Control Variables

	Summary Statistics- All Clients		Weekly/Monthly Difference			
	Weekly	Monthly	All Clients	Lottery Clients	4-Rs. 50 Voucher Prize	1-Rs. 200 Voucher Prize
	(1)	(2)	(3)	(4)	(5)	(6)
Owens Enterprise	0.755	0.680	0.075	0.147	0.152	0.141
	[0.431]	[0.467]	(0.047)	(0.061)	(0.077)	(0.081)
Owens Home	0.775	0.792	-0.017	-0.003	-0.089	0.086
	[0.419]	[0.406]	(0.041)	(0.047)	(0.061)	(0.064)
Has Played Lottery in Past 12 Months	0.065	0.054	0.012	-0.018	-0.055	0.021
	[0.248]	[0.225]	(0.023)	(0.029)	(0.032)	(0.044)
Illness in Past 12 Months	0.343	0.277	0.066	0.050	0.022	0.080
	[0.476]	[0.448]	(0.044)	(0.066)	(0.075)	(0.080)
Total Transfers into Households	4.868	4.617	0.251	-0.253	-1.430	1.015
	[4.328]	[4.597]	(0.531)	(0.682)	(0.808)	(1.132)
Total Transfers out of Households	7.677	5.507	2.171	1.050	-1.575	3.604
	[14.363]	[10.848]	(2.273)	(2.329)	(2.543)	(3.709)
Health Expenditures	3514	4080	-566	-673	-977	-354
	[5561]	[12428]	(613)	(961)	(1442)	(1061)
Education Expenditures	5011	4513	498	-221	-132	-314
	[5515]	[5693]	(403)	(654)	(853)	(870)
Has Insurance	0.376	0.342	0.034	0.053	0.067	0.039
	[0.485]	[0.475]	(0.045)	(0.065)	(0.081)	(0.076)
Fixed Salary Earned by Household	1460	1560	-100	25	114	-68
	[2998]	[2602]	(251)	(331)	(385)	(471)
				-0.009		
Fraction of Clients Surveyed				(0.038)		
N	306	710				

**Notes**

- 1 Illness in Past 12 Months is an indicator variable for whether any household member has been ill in past 12 months. Total Transfers into/out of Household is defined for households which made/received at least one transfer over past 12 months. Fraction of clients surveyed is the group-level fraction of clients receiving lottery survey.
- 2 Columns (3)-(6) report tests of differences of means for the different subsamples. Standard errors are clustered by group.

Appendix Table 2. Meeting Frequency and Social Interaction

	Total Times Met	Talk Family	Attend Durga Puja Together
	(1)	(2)	(3)
Weekly	0.864 (0.597)	0.061 (0.025)	0.072 (0.038)
Mean of Monthly	4.463 [9.732]	0.183 [0.387]	0.166 [0.372]
N	3137	4018	4018

Notes:

- 1 Dependent variables in Columns (1)-(3) are as defined in Table 2.
- 2 Mean of monthly is average value of dependent variable for monthly clients with standard deviations in brackets.
- 3 Regressions include group-level and client-level controls (Table 1, Panels A and B), days between loan disbursement and lottery survey, and whether member is group leader. Standard errors are clustered at the group-level.

Appendix Table 3. Group-level and Client-level Randomization Check for Clients Continuing to Second Loan

	Weekly (1)	Monthly (2)	Weekly/Monthly Difference (3)
<b>Panel A: Group-level</b>			
Number of Clients	10.250 [0.701]	10.318 [0.727]	-0.068 (0.162)
Month of Formation	5.786 [1.548]	5.652 [1.364]	0.134 (0.320)
Fraction Muslim	0.000 [0.000]	0.062 [0.226]	-0.062 (0.043)
N	28	66	
<b>Panel B: Client-level</b>			
Age	33.650 [7.872]	33.250 [8.136]	0.400 (0.743)
Literate	0.858 [0.349]	0.842 [0.365]	0.016 (0.035)
Highest School Class Completed	6.487 [3.388]	6.834 [3.626]	-0.347 (0.399)
Married	0.885 [0.320]	0.882 [0.323]	0.003 (0.027)
Household Size	3.960 [1.109]	3.828 [1.303]	0.132 (0.091)
Worked for Pay in Last 7 Days	0.580 [0.495]	0.561 [0.497]	0.019 (0.052)
Household Savings	2516 [14468]	1464 [6192]	1051 (1165)
Value of Assets (Rs.)	10057 [21974]	9805 [24895]	252 (2064)
Years Living in Neighborhood	15.089 [9.492]	16.263 [9.897]	-1.174 (0.910)
Financial Control	0.889 [0.314]	0.861 [0.346]	0.028 (0.049)
Fraction Distant Relatives	0.073 [0.123]	0.051 [0.108]	0.022 (0.016)
Fraction Close Neighbors	0.129 [0.220]	0.104 [0.168]	0.025 (0.040)
Fraction Distant Neighbors	0.382 [0.325]	0.439 [0.329]	-0.057 (0.050)
Fraction Didn't Know	0.297 [0.324]	0.297 [0.341]	0.000 (0.050)
Fixed Salary	0.062 [0.242]	0.151 [0.359]	-0.089 (0.029)
Impatient	0.456 [0.499]	0.437 [0.497]	0.019 (0.066)
Risk Aversion	0.176 [2.445]	-0.018 [2.505]	0.194 (0.432)
N	226	476	

## Notes

1 Fixed Salary is an indicator variable for whether the client earns a fixed salary. Impatient is whether client prefers "Rs. 200 now" over "Rs. 250 in one month." Risk Index is an aggregate measure of risk aversion (higher risk index represents lower risk aversion) based on a series of questions asking clients whether they would accept higher/equal expected returns in exchange for higher dispersion of returns in a hypothetical coin flip scenario. Remaining variables are as defined in Table 1.

2 Columns (3) reports a test of differences of means (weekly minus monthly) for the pool of first time borrowers who went on to take out a second loan. Standard errors are clustered by first loan group.

Appendix Table 4. Determinants of Default

	Second Loan Default (1)
<b>Group-level Variables</b>	
Number of Clients	0.067 (0.047)
Fraction Muslim	0.056 (0.098)
<b>Client-level Variables</b>	
Age	-0.001 (0.002)
Highest School Class Completed	0.004 (0.005)
Household Size	0.005 (0.008)
Worked for Pay in Last 7 Days	0.006 (0.019)
Household Savings	-0.014 (0.005)
Value of Assets (Rs.)	0.003 (0.004)
Total Transfers into Households	-0.004 (0.002)
Years Living in Neighborhood	0.001 (0.001)
Financial Control	0.027 (0.039)
Fixed Salary	-0.006 (0.030)
Impatient	-0.036 (0.021)
Risk Aversion	-0.004 (0.007)
Health Spending (Rs.)	0.287 (0.113)
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 and Appendix Tables 1 and 3.

2 Standard errors are clustered by first loan group.

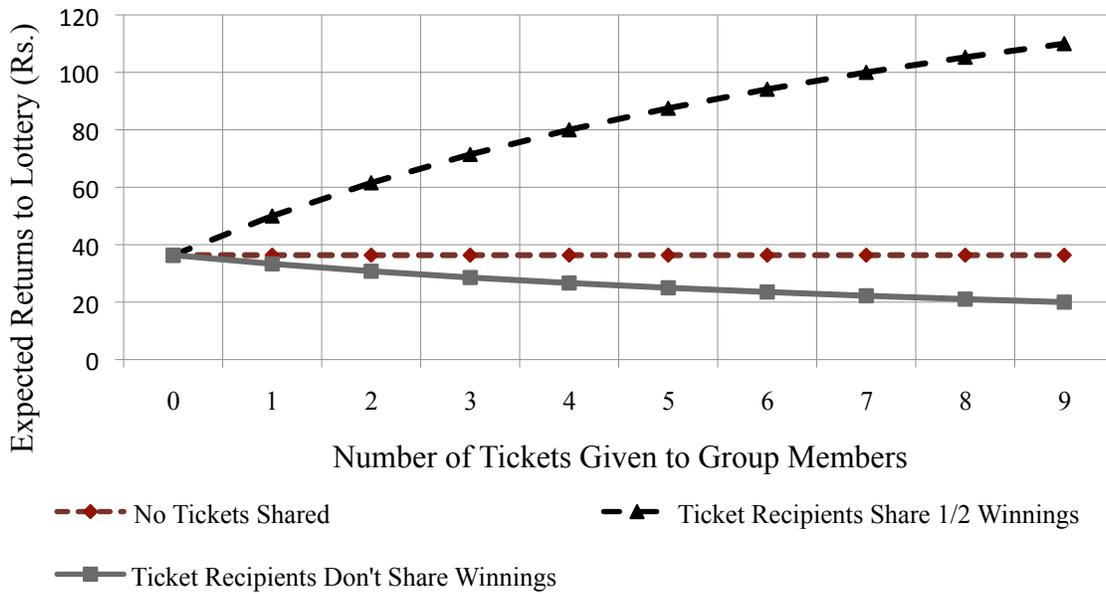
Appendix Table 5. Robustness Check: Principal Regressions without Controls

	Social Contact Index		Gave Ticket		Second Loan Default	
	(1)	(2)	4- Rs. 50	1- Rs. 200	(5)	(6)
			Vouchers	Voucher		
Weekly	0.112 (0.078)	0.001 (0.171)	0.094 (0.053)	0.018 (0.056)	-0.078 (0.033)	-0.047 (0.031)
Distant Relative		0.034 (0.172)				
Close Neighbor (<50m)		-0.571 (0.149)				
Distant Neighbor(>50m)		-0.909 (0.105)				
Didn't Know		-1.242 (0.108)				
Weekly*Distant Relative		0.428 (0.224)				
Weekly*Close Neighbor		0.325 (0.259)				
Weekly*Distant Neighbor		0.064 (0.189)				
Weekly*Didn't Know		0.041 (0.173)				
Fraction Distant Relatives or Close Neighbors in Group						0.142 (0.134)
Weekly*Fraction Distant Relatives or Close Neighbors in Group						-0.190 (0.137)
Mean of Monthly			0.196 [0.397]	0.241 [0.428]	0.108 [0.301]	
N	3137	3137	2027	1991	707	707

Notes:

- 1 Social Contact Index is as defined in Table 2. Gave Ticket is as defined in Table 5. Second Loan Default is as defined in Table 3.
- 2 Distant Relative, Close Neighbor, Distant Neighbor, and Didn't Know are responses to pairwise relationship type before joining VWS. 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 In all regressions, standard errors are clustered at the group-level.

Appendix Figure 1. Expected Returns to Lottery by Ticket-Giving Decision



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



Whoever claims 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: \_\_\_\_\_



Whoever claims 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.

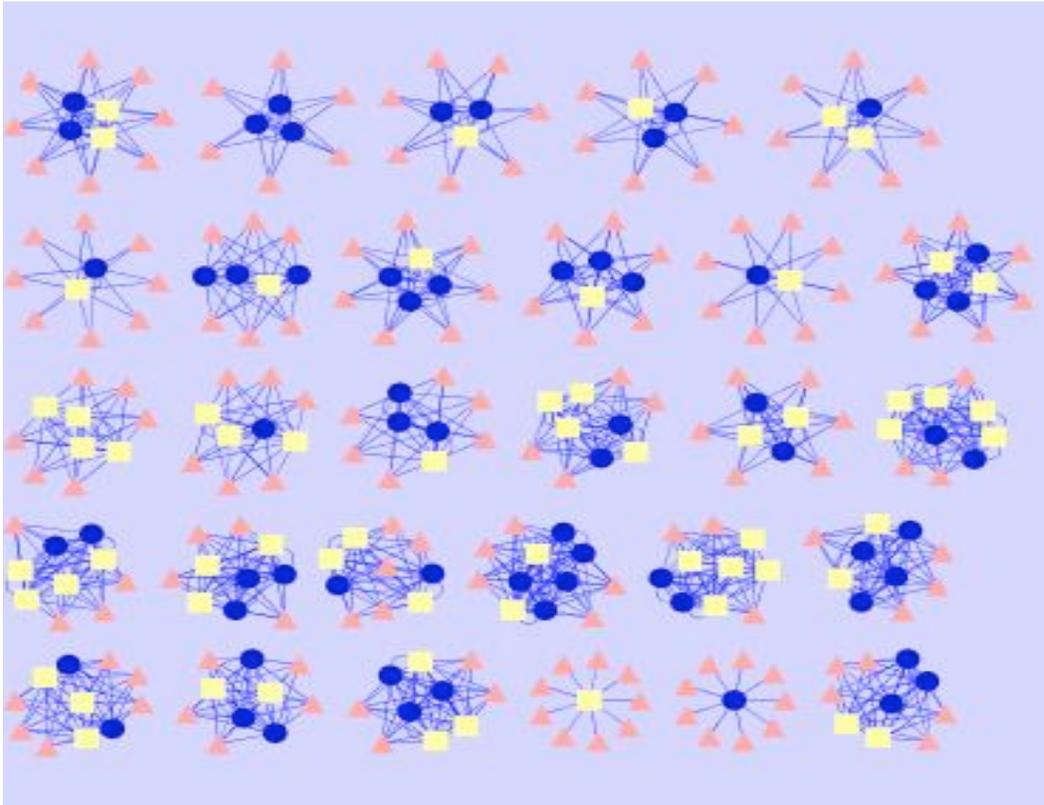
Date of Lottery: \_\_\_\_\_ Deadline to Claim: \_\_\_\_\_  
Group Name: \_\_\_\_\_ Name of Claimant: \_\_\_\_\_  
Name of Winner: \_\_\_\_\_ Signature of Claimant: \_\_\_\_\_  
Signature of Winner: \_\_\_\_\_

### Note:

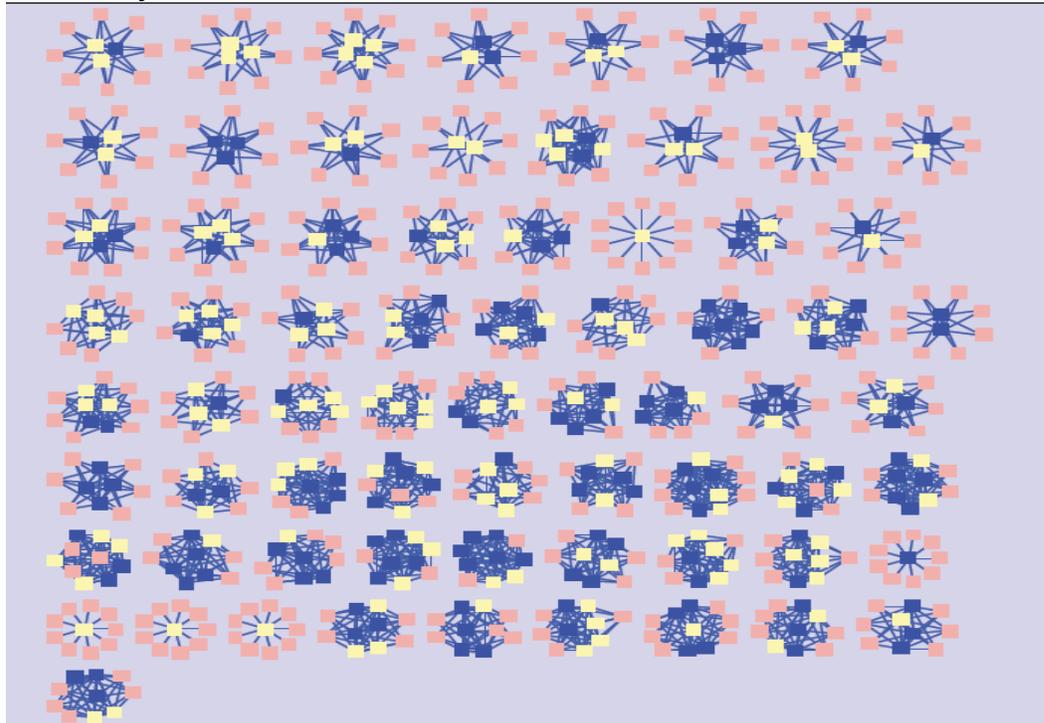
Clients were randomly offered the choice of joining the 1-Rs. 200 Voucher or the 4-Rs. 50 Voucher lottery. This figure shows the final vouchers which were given to the winner of the two lotteries.

### Appendix Figure 3. All Network Structures

Panel A: Weekly Clients



Panel B: Monthly Clients



# APPENDIX: NOT FOR PUBLICATION

## VWS Protocol for Group Formation

The loan officer surveys the demographic make-up of a potential neighborhood. If appropriate, then s/he conducts a meeting to inform potential clients about the VWS loan product and invites them to a five-day Continuous Group Training (CGT) program. The program runs an hour each day, and introduces clients to the benefits and responsibilities associated with the loan product. Each potential loan group is assigned a separate CGT program. At the end of the CGT, the loan officer forms women who were considered sufficiently informed and interested into a group, identifies (with group members) a group leader and offers each member of the group a loan.<sup>1</sup>

## 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 you would like us to give tickets to.

We will review the effect giving out tickets has on chances of winning. In picture 1 in which you donot give out any tickets to members of your VWS group, you would 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

---

<sup>1</sup>Group leader selection criteria include: (i) communicates well with group members and VWS staff; (ii) is responsible and well accepted by group members; (iii) has a house or place to organize group meeting.

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 value of the voucher must be used when the voucher is 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.