The Economic Returns to Social Interaction: Experimental Evidence from Microfinance

Benjamin Feigenberg, Erica Field and Rohini Pande*

April 19, 2012

Abstract

Microfinance clients were randomly assigned to repayment groups that met either weekly or monthly during their first loan cycle, and then graduated to identical meeting frequency for their second loan. Long-run survey data and a follow-up public goods experiment reveal that clients initially assigned to weekly groups interact more often and exhibit a higher willingness to pool risk with group members from their first loan cycle nearly two years after the experiment. They were also three times less likely to default on their second loan. Evidence from an additional treatment arm show that, holding meeting frequency fixed, the pattern is insensitive to repayment frequency during the first loan cycle. Taken together, these findings constitute the first experimental evidence on the economic returns to social interaction, and provide an alternative explanation for the success of the group lending model in reducing default risk.

JEL: C81,C93, O12, O16

*The authors are from MIT (Feigenberg), Duke University (Field) and Harvard University (Pande). We thank Max Bode, 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 Financial 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 thank the editor, two anonymous referees and Attila Ambrus, Abhijit Banerjee, Tim Besley, Amitabh Chandra, Esther Duflo, Raquel Fernandez, Seema Jayachandran, Dean Karlan, Asim Khwaja, Dominic Leggett, Muriel Niederle, Aloysius Sioux, Jesse Shapiro and numerous seminar participants for helpful comments.

1 Introduction

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

Rigorous evidence on this question remains limited, largely due to the difficulty of accounting for endogenous social ties (Manski, 1993, 2000). For instance, if more trustworthy individuals or societies are characterized by denser social networks, we cannot assign a causal interpretation to the positive association between community-level social ties and public good provision. For similar reasons, it is also not possible to assign a causal interpretation to the higher levels of cooperation observed among friends relative to strangers in laboratory public goods games.[2] In short, without randomly varying social distance, it is difficult to validate the model of returns to repeat interaction and even harder to determine whether small changes in social contact can produce tangible economic returns.

The first contribution of this paper is to undertake exactly this exercise. By randomly varying how often individuals meet, we provide causal evidence on the returns to repeat social interaction. We do so in the context of a development program that emphasizes

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

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

group interaction: microfinance.[3] In the typical "Grameen Bank"-style microfinance program, clients meet weekly in groups to make loan payments. Our experiment varied social interaction by randomly assigning 100 first-time borrower groups of a typical microfinance institution (MFI) in India to either meet on a weekly or a monthly basis throughout their ten-month loan cycle. Using administrative and survey data we study the effect of short-run increases in group meeting frequency on long-run social contact and an important measure of economic vulnerability: default incidence in the subsequent loan cycle.

A second contribution of this paper is to identify a key mechanism through which group lending sustain high repayment rates: risk-pooling among clients. While the theoretical literature largely emphasizes the importance of joint-liability contracts for reducing default in microfinance, recent experimental evidence suggests that joint liability per se has little impact on default (Gine and Karlan, 2009), raising anew the question of how exactly group lending achieves risk reduction without collateral. Since our clients received individual-liability debt contracts, we can isolate how a less noted feature of the classic group lending contract – encouraging social interaction via group meetings – reduces default.[4] In other words, even absent the explicit incentives for monitoring and enforcement that joint liability provides, frequent group meetings can lower lending risk by increasing social interaction among group members and, as a consequence, strengthening risk-pooling arrangements within social networks.

Our evidence consists of several striking changes in client behavior associated with experimentally increasing the frequency of client contact. First, clients assigned to weekly

[3]Related work include Dal Bo (2005) who provide laboratory game evidence on returns to repeat *economic* interaction, where the likelihood of future rounds of exchange is randomly assigned and Humphreys et al. (2009) who use a field experiment to show that community development programs randomly assigned to villages encourage pro-social behavior (but cannot isolate the influence of social interaction from other program aspects).

[4]The remarkable success of microfinance in achieving very high repayment rates on collateral-free loans to poor individuals is widely recognized, evidenced by the awarding of the Nobel Peace Prize to the founder of Grameen Bank. Our findings complement both theoretical research that discusses the role of social collateral in microfinance and empirical work that identifies a significant correlation between social connections and default risk (Besley and Coate, 1995; Ghatak and Guinnane, 1999; Karlan, 2005). For instance, MFI clients in Peru who are more trustworthy in a trust game are less likely to default, and group-level default is lower in groups where clients have stronger social connections (Karlan, 2005, 2007). In Gine and Karlan (2009), the shift from joint to individual liability increased default among borrowers with ex-ante weak social ties.

groups during their first loan cycle increased social contact with group members outside of meetings, and sustained it in the long run. More than a year after the experiment ended, clients who had met on a weekly basis during their first loan saw each other 38% more often outside of group meetings.

Second, greater social interaction among clients on a weekly schedule was accompanied by increased willingness to pool risk relative to monthly clients. Here, our evidence comes from a field-based lottery game conducted roughly 16 months after the first loan cycle ended. The lottery operated much like a laboratory trust or solidarity game, but in a real-world setting. Each client was entered into a (separate) promotional lottery for the MFI's new retail store. The client started with a 1 in 11 chance of winning the lottery prize, a voucher redeemable at the MFI store. She was then offered the opportunity to give out additional lottery tickets to any number of members of her first loan group.

Since ticket-giving reduces a client's individual chances but increases the probability that someone from the group would win, it captures either her unconditional altruism towards or willingness to risk-share with members of her initial group. To distinguish insurance motivations from unconditional altruism, we randomized the divisibility of the lottery prize. Assuming the more easily divisible prize is perceived as more conducive to sharing, a client should give more tickets when the prize is divisible if she is motivated at least in part by risk-sharing considerations, but should not if her sole motivation is unconditional altruism.[5]

Relative to a monthly client, a client who had been assigned to a weekly group two years prior was 32% more likely to enter a group member into the lottery when the prize was divisible, but only 16% more likely when it was not. We also find strong order effects that indicate reciprocal risk-sharing motivations among weekly clients only.

Finally, we find evidence that clients on a weekly schedule were, in the long run, better able to endure financial shocks. Those who met weekly during their first loan cycle were

[5]Similar variations of dictator or trust games have been used to parse out motives for giving (Ligon and Schecter, 2008; Do et al., 2009; Carter and Castillo, 2004a). Most similar to us, Gneezy et al. (2000) 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.

three times (6.2 percentage points) less likely to default on their subsequent loan, despite the fact that all clients had reverted to the same repayment schedule. Importantly, the default rate difference is also evident when we compare monthly clients to clients randomly assigned to a second treatment arm where they met weekly but maintained a monthly repayment schedule. This implies that default risk falls on account of greater social interaction among weekly clients rather than differences in fiscal habits that could arise from requiring clients to initially *repay* at more frequent intervals.

Together, these patterns indicate that greater social interaction improved risk-sharing arrangements among clients and helped them insure against financial shocks. Our findings substantiate theoretical claims that repeat interaction can yield economic returns by facilitating informal economic exchange, and provide an alternative explanation for the success of the group lending model. More broadly, the findings demonstrate that tweaking the design of standard development programs to encourage social interaction can generate economically valuable social capital.

The paper is structured as follows: Section 2 describes the experimental design. Section 3 examines how randomized differences in meeting frequency, implemented only during the first loan cycle, influenced long-run social interaction and client willingness to share in the field-based lottery. Section 4 documents changes in long-run default rates and separates the role of meeting frequency from that of repayment frequency. Section 5 concludes.

2 Experimental Design

2.1 Setting

Our MFI partner, Village Financial Services (VFS), operates in the Indian state of West Bengal. In 2006 when we began our field experiment, it had $6.75 million in outstanding loans to over 56,000 female clients. VFS' gross loan portfolio to total asset ratio of 78% placed it slightly below the median Indian MFI (84%) while its portfolio at risk of 0.47%

(defined as payments outstanding in excess of 30 days) was identical to the median Indian MFI (MIX Market, 2012).

Our study population consisted of first-time VFS clients living in the peri-urban slums of the city of Kolkata. At the time of joining the MFI, over 70% of our client households owned a business and the median client's household income placed her just below the dollar-a-day poverty line. Demographics of our study population, such as income, home ownership, and home size, are largely comparable with similar MFIs operating in other Indian cities (Online Appendix Table 1). However, consistent with differences across Indian cities in the extent of MFI penetration, clients in our sample exhibit significantly lower rates of borrowing outside of the MFI.

2.2 Sample

Between April and September 2006 we recruited 100 first-time microfinance groups from neighborhoods in the catchment areas of three VFS branches. Following VFS protocol, the loan officer first surveyed the neighborhood and then conducted a meeting to inform female residents about the VFS loan product. Interested women were invited for a five-day training program, where clients met for an hour each day and learned about the benefits and responsibilities of the loan. At the end of the five days, the loan officer assigned women into groups of ten and identified a leader of each group.[6] Thus, clients in a single loan group lived in close proximity and were typically acquainted prior to joining. Although 63% of group members in our sample knew one another at group formation, most described their relationship with other group members as neighbors (48%) rather than friends (7%) or family (8%).

2.3 Experimental Design

Group Assignment At the end of the group formation process, each group member was offered an individual-liability loan of Rs. 4,000 (∼$100) and told that her repayment

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

schedule would be assigned at the time of loan disbursal. Prior to loan disbursal, groups were randomized into either weekly or monthly schedules. In total, 38 groups were assigned to the control arm in which group meetings were held on a monthly basis, and 30 groups were assigned to the treatment arm in which group meetings occurred weekly (Treatment 1). In addition, 32 groups were assigned to an alternative treatment in which they met weekly but repaid monthly (Treatment 2), an artificial contract design for the purpose of microfinance delivery, but one that allows us to disentangle the influence of meeting frequency from the influence of repayment frequency for scientific purposes.

At loan disbursal, Treatment 1 groups were informed that they were to repay their loans in 44 weekly installments of Rs. 100 (a reasonably small amount given average weekly household earnings of Rs. 1,267), while Control and Treatment 2 groups were told that they would repay in 11 monthly installments of Rs. 400. No client dropped out after her repayment schedule was announced.

Meeting Protocol Repayment in a group setting is an integral part of MFI lending practice, and VFS followed a relatively standard "Grameen Bank" group meeting model. Each group was assigned a loan officer who conducted the meeting in the group leader's house. The average meeting lasted 18 minutes, during which clients took an oath promising regular repayment and then the loan officer collected payment and marked her passbook.[7] Thus, a client's repayment behavior was observable to other group members, although in practice most clients socialized while awaiting their turn. Anecdotally, socializing happens en route to meetings, while waiting for the loan officer to arrive and begin meetings, and while waiting for one's turn to pay.[8]

Overall, Control and Treatment 1 groups closely followed the assigned meeting schedule: No Control group met less than five or more than eleven times and no Treatment 1 group met less than 23 or more than 44 times, which were the minimum and maximum meetings allowed by the respective contracts.[9] While in theory clients could skip meetings

[7]While the oath encourages group responsibility for loans, the loan contract is individual liability.

[8]Anthropologists have also documented that group lending increases women's opportunities for social interaction with members of their community (Larance, 2001).

[9]Variation in number of meetings *within* a repayment schedule reflects the fact that VFS allows a client to repay her outstanding balance in a single installment starting 23 weeks after loan disbursement.

and send their payment with another group member, it was rare for clients to do so, and average attendance at repayment meetings was 81%.

Treatment 2 groups did not strictly adhere to the experimental protocol: Only half of the groups met at least the minimum required number of times (23) and average attendance at meetings was only 56%. As this compliance issue necessitates a more complicated econometric strategy, we first present experimental estimates which compare Control and Treatment 1 groups only. Then, in order to identify the channels of influence, in Section 4.1.2 we reintroduce Treatment 2 and describe our econometric approach to isolate compliers in this arm.

2.4 Data

We tracked our experimental clients over two and a half loan cycles (on average 176 weeks). Figure 1 provides a detailed study timeline. Our analysis makes use of several data sources, which we describe in turn.

Baseline and Endline Data After group formation, we administered a baseline survey to 1016 out of 1028 clients. The short time period between group formation and loan disbursement led to a significant fraction of baseline surveys taking place after loan disbursement. We therefore exclude any potentially endogenous baseline variables from the analysis. Roughly 13 months after first loan disbursement, we conducted an endline survey with 961 clients that provides data on transfers and loan use. Attrition in both surveys was balanced across treatment and control clients.

Short-run Social Contact To gauge social interaction among group members, loan officers collected data at repayment meetings during the first loan cycle. The protocol was as follows: After marking passbooks, each client was pulled aside and asked broad questions about social ties with other group members, in order to provide multiple indicators of short-run contact. The first two of these indicators measure social interaction and are constructed as the maximum values of client responses to the two questions – "Have all of your group members visited your house?" and "Have you visited the houses of all group

Once a majority of group members have repaid, remaining clients repay at the VFS office.

members?" The next two indicators measure knowledge of group members: whether the client knew the names of her group members' immediate family and whether she knew if group members had relatives visit over the previous month.[10] Here, we report the average effect size across these measures, defined as the short-run social contact index.[11]

Long-run Social Contact and Lottery Data collection during group meetings allowed us to gather high frequency data in an economical way. However, collecting data in a group setting could create reporting bias that confounds experimental comparisons. For instance, when responses are potentially overheard, a client may be subject to conformity bias wherein she answers questions in a similar manner to others in the group, which could potentially bias experimental estimates. To gather more reliable data on interactions, roughly 16 months after the experimental loan cycle ended, we implemented a lottery game and survey with 866 clients in their homes.[12] Surveying occurred in two phases, and client assignment to phase was random. Section 3.2.1 describes the lottery protocol and data. After the lottery was conducted, the client was surveyed about her current contact with every member of her *first* loan cycle group. On average we have nine observations per client. In cases where both members of a pair were surveyed, we keep the maximum value (since social contact cannot vary, in the absence of measurement error, within a pair), giving 3,034 pairwise observations. The survey questions included: number of times over the last 30 days the client had visited or been visited by a group member (outside of repayment meetings), whether she talked to the group member about family, and whether they celebrated the Bengali festival (Durga Puja) together. We report all three outcomes and, for comparability with the short-run index, also report a long-run

[10] To preserve anonymity (given potential observability of responses by group members) we did not ask about interactions with specific group members. We consider the maximum value for all variables, except the relative visit for which we take the average (only the latter was reported for an explicit recall period). To account for the delay in starting the survey and the fact that groups could choose to repay early and stop meeting after week 23 of the loan cycle, we use data collected between month 3 and week 23 of the loan cycle.

[11] The index is the equally weighted average of its components' z-scores, where each measure is oriented so that more beneficial outcomes have higher scores. The z-scores are calculated by subtracting the Control group mean and dividing by the Control group standard deviation. By construction, the index has a mean of 0 for the Control group (for further details, see Kling et al., 2007).

[12] We excluded a randomly selected 130 clients with whom we piloted the lottery game and 32 clients could not be tracked.

social contact index defined at the pair level.

Default Data Our primary outcome of interest is default in the loan cycle *subsequent to* the experimental loan cycle (from now on, second loan cycle), during which all clients reverted to the same repayment and meeting frequency. Bank administrative records show that all clients (except one deceased) took out a loan within 176 weeks of their first loan due date. Appendix Table 1 shows that time between due date of first loan and disbursement of second loan does not differ by experimental arm, and we have confirmed that our default results are robust to controlling for this variable.

We define a client as having defaulted if she has not repaid her loan in full by 44 weeks after the official loan end date (i.e., one full loan cycle duration later).[13]

2.5 Randomization Balance Check

Panel A in Table 1 reports time-invariant characteristics from the baseline survey as a function of treatment assignment. Columns (1)-(3) report the randomization check for the full sample and columns (4)-(6) for clients in the lottery/long-run social interaction survey. On average, randomization created balance between treatment and control groups on observed characteristics. There is one statistically significant difference between Control and Treatment 1 clients: On average, Treatment 1 clients have lived in their neighborhood for 1.3 fewer years. With respect to the comparison between Control and Treatment 2, a higher fraction of Muslim clients fell into Treatment 2. This imbalance is related to residential segregation by religion, combined with a relatively small number of Muslim clients: 96% of our clients report living in religiously homogenous neighborhoods (89% Hindu; 7% Muslim). Our 55 Muslim clients are concentrated in eight groups, of which six were assigned to Treatment 2. Since Muslim clients tend to come from larger households, we observe a corresponding imbalance on household size. Since no variable is imbalanced in

[13] Although we cannot track all second loan clients for more than 44 weeks, we have verified that second loan default rates are relatively constant at the 64-week mark among those clients whom we can observe for this long. This, combined with the fact that the portfolio at risk statistic officially used for MFI credit rating is defined as the share of portfolio with loan payments outstanding 30 days after due date (CGAP, 2012) makes our default definition relevant.

both treatment arms, the robustness of our results to alternative treatment arms provides strong evidence that imbalances are not driving our results. Nonetheless, throughout this paper we report regressions with and without the controls listed in Panel A of Table 1. We have also verified that our main results are robust to excluding groups with Muslim clients.

Panel B reports an additional set of variables from the baseline survey that are potentially (though not likely, given the short amount of time between loan disbursement and data collection) influenced by loan receipt. We observe no systematic differences between control and treatment groups. Of the 20 comparisons, the only two (weakly) significant differences in means are that Treatment 1 clients were less likely to have a household member earning a fixed salary, and Treatment 2 clients were slightly less likely to report experiencing an illness during the last 12 months. Finally, comparing across columns we see similar patterns of mean differences in observables across the full sample and the client sample for the lottery/long-run survey.

3 Meeting Frequency and Client Relationships

In this section, we use data on social interactions to examine whether requiring first-time VFS clients to meet and repay weekly (Treatment 1) as opposed to monthly (Control) increased social interactions outside of group meetings, both during and beyond the experiment. To investigate whether clients also experienced long-run improvements in risk-sharing arrangements, we implemented a follow-up lottery game that measured willingness to pool risk. For ease of exposition, we restrict the sample to Control and Treatment 1 clients only, since compliance (in terms of meeting protocol) was perfect in these two arms.

In Section 4, we examine the economic impact of these changes by testing whether clients who met weekly in the first loan cycle exhibit lower default on their subsequent loan. Long-run financial behavior (and default) may be directly influenced by initial repayment frequency. We, therefore, complement our experimental analysis by an In-

strumental Variable (IV) analysis in which we compare default outcomes across clients who paid monthly in the first loan cycle but differ in whether they met on a weekly or monthly basis (that is we compare Treatment 2 to Control). The IV strategy is needed to address noncompliance in the Treatment 2 arm. Our IV estimates verify that differences in meeting frequency *not* repayment frequency underlie changes in default.

3.1 Impact on Social Interaction

Data obtained during repayment meetings provide a summary measure of a client's interaction with other group members during the experimental loan cycle.

For client i in group g with short-run contact index y_{gi} we estimate:

$$y_{gi} = \beta T_{1,g} + X_{gi}\gamma + \epsilon_{gi} \tag{1}$$

where $T_{1,g}$ is an indicator for assignment to the Weekly-Weekly treatment arm (Treatment 1) and X_{gi} represents individual covariates. β is interpretable as the effect of switching from a monthly to a weekly group lending model on a client's contact with group members outside of meetings. Standard errors are clustered by group.

As reported in Table 2, switching a client from monthly to weekly meetings increases her social contact with group members by over 2.6 standard deviations (column 1). We observe similar results with and without controls (throughout the paper, Panels A and B report estimates without and with controls, respectively).[14] This impact is large but plausible. Due to the aggregated nature of the question, the estimated treatment effect depends on the response to treatment of the weakest pair within a group. Since 76% of clients have at least one person in their group who is a stranger at baseline and 40% have at least one member who is a *distant* (geographically) stranger at baseline, the estimates are consistent with a scenario in which it takes 5-20 meetings for two strangers to become

[14]Component-wise regression results show large and significant effects of assignment to the Treatment 1 arm. For instance, while only 10% of Control clients report having met all group members outside of meetings, almost 100% of Treatment 1 members report having visited (or having been visited by) all other group members by the same point (results available from authors).

sufficiently connected to initiate social interaction (hence the index is low for Control groups after five months, but by week 23 virtually every pair of Treatment 1 clients has connected).

However, some caveats do apply. First, the presence of other clients during the survey raises the concern of aggregation and reporting biases in client responses. Second, the frequency of surveying may have influenced responses and generated artificial differences across treatment groups in reported interactions. A related concern is that surveying clients about social interactions may itself encourage friendship formation.

Two pieces of evidence suggest that survey frequency did not directly influence real or reported interactions. First, delays in fieldwork initiation meant that group meeting surveys were implemented more than five weeks after meetings began for 26 of the 68 groups. Data on social interactions from the first group meeting survey for these groups show significant differences across experimental arms in the reported level of interaction. Second, in a later intervention we randomized groups (typically on their third loan cycle) into Weekly-Weekly and Monthly-Monthly groups and loan officers surveyed them during meetings at the same frequency (monthly). We continue to see greater increases in social contact among groups that met weekly (both sets of results are available from authors).

That said, even in the absence of data quality concerns, our interest is in lasting, not transient, changes in social networks. Therefore, we turn to long-run measures of social interaction, collected 16 months after the experimental loan cycle ended. These data have the additional advantage of being collected through careful surveying, where each client was asked in the privacy of her home about her ongoing interactions with each member of her first loan group. As before, we compare clients assigned to the Weekly-Weekly (Treatment 1) schedule to those assigned to the Monthly-Monthly (Control) schedule. For member i matched with group member m in group g we estimate:

$$y_{gi}^{m} = \beta T_{1,g} + X_{gi}\gamma + s_{gi} + \epsilon_{gi}^{m} \tag{2}$$

s_{gi} is an indicator for whether individual i was surveyed in the first phase, with other

variables defined as in Equation (1) and standard errors clustered by group.[15]

Columns (2)-(5) of Table 2 reveal that clients engaged in a significant amount of social interaction with their first loan cycle group members at the time of the follow-up survey, and that this interaction was significantly higher among clients who met on a weekly basis during the first loan cycle. In column (2) we see that the average Control pair met 5.5 times over the last 30 days (outside of repayment meetings), and that the average Treatment 1 client pair met 38% more often than their Control counterpart. In total, 15% of Control client pairs versus 22% of Treatment 1 pairs celebrated the last Durga Puja festival together, and 23% of Control client pairs compared to 30% of Treatment 1 pairs report discussing family matters (column 4). Finally, for comparability with the short-run index we report the long-run social contact index, which aggregates outcome variables in columns (2)-(4), and see that Treatment 1 assignment increased long-run social contact by 0.18 standard deviations.

The persistence of differences in social interaction is particularly striking given that all clients took out at least one additional loan with VFS and roughly half report having a VFS loan outstanding at the time of the follow-up survey. Thus, we might expect social interaction rates to converge as monthly members slowly get to know one another over the long run. However, an important reason *not* to anticipate convergence is churning in group membership: Only 32% of client pairs were in the same group for their second loan, largely because a 2007 VFS policy change reduced the size of subsequent groups from ten to five members. Hence, many clients lost the opportunity to get to know one another at group meetings after the experimental loan cycle ended.

The policy change raises the possibility that treatment assignment influenced the likelihood that group members remain together in future loan cycles, which could be an

[15]Factors common across observations involving a single member imply observations in a pairwise (dyadic) regression are not independent (Fafchamps and Gubert, 2007). The error covariance matrix structure may also exhibit correlations varying in magnitude across group members. Group-level clustering of standard errors (which subsumes individual clustering) accounts for this potential pattern: 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).

independent channel through which average levels of social interaction between treatment groups diverge over time. We are able to track group membership of clients in 51 groups. For these clients, Appendix Table 1 shows no difference across experimental arms in the likelihood of being paired with first group members in the second loan cycle. Thus, our experimental differences in long-run contact are likely driven by the higher propensity of Treatment 1 (Weekly-Weekly) clients to stay in touch with members of their first group who did not remain with them for a subsequent loan.

3.2 Impact on Risk-sharing

The increases in social interaction documented in Table 2 are particularly meaningful if they were tangibly welfare-improving, for instance by enabling information spillovers or facilitating economic exchange.[16] For poor clients who face many shocks and rigid debt contracts, informal risk-sharing arrangements are likely to be particularly valuable. Hence, we directly examine whether increasing social interaction facilitated informal risk-sharing arrangements through a series of field-based lottery games. These lotteries, a variant of laboratory dictator and trust games (Forsythe et al., 1994; Berg et al., 1995), were designed to elicit client willingness to form risk-sharing arrangements.

Our methodology contributes to a growing experimental literature on risk-sharing, which finds that increased opportunity for commitment across individuals is associated with a higher willingness to undertake profitable but riskier investments, that close interpersonal relationships predict risk pooling, and that group lending improves implicit insurance against investment losses (Barr and Genicot, 2008; Attanasio et al., 2011; Gine et al., 2010). Experimental approaches to measuring risk-sharing, inside or outside of the laboratory, depart considerably from non-experimental empirical tests which most often examine differences in networks' ability to smooth consumption in response to shocks (e.g. Townsend, 1994; Mace, 1991). While the latter may provide a more direct test of standard hypotheses derived from models of risk-sharing, the experimental approach, in

[16] Indeed, in and of itself, being encouraged to spend time with strangers may be utility-decreasing if one does so out of convention or social pressure.

which outcomes are financially incentivized rather than merely reported, arguably enables a more reliable method of establishing risk-sharing between specific pairs of individuals.

That said, we complement our experimental measure of risk-sharing with survey data on financial transfers into and out of client households, and demonstrate similar patterns across the two types of data.[17]

Below, we describe the lottery protocol, and then key predictions of increased risk-sharing for client behavior in the lottery. Then we test these predictions using the lottery data and finally check for consistency of patterns in the financial transfers data.

3.2.1 Lottery Protocol and Data

Main Lottery Surveyors approached each client in her house and invited her to enter a promotional lottery for a new VFS retail store. The lottery prize consisted of gift vouchers worth Rs. 200 ($5) redeemable at the store (see Appendix for the surveyor script). The client was informed that, in addition to her, the lottery included 10 clients from different VFS branches, whom she was therefore unlikely to know. If she agreed to enter the draw (all agreed), she was given the opportunity to enter any number of members of her first VFS group into the same draw. Each chosen group member would receive a lottery ticket and be told whom it was from. To clarify how ticket-giving influenced her odds of winning, the client was shown detailed payoff matrices (Figure 2), and told that the other ten lottery participants could not add individuals to the lottery. Hence, she could potentially increase the number of lottery participants from 11 to as many as 20, 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%.

We randomized divisibility of the lottery prize at the client level.[18] For half of the sample, the prize was one Rs. 200 voucher, while for the other half it consisted of four

[17] We lack information on consumption and, therefore, cannot directly link potential improvements in risk-sharing with consumption smoothing (for related work which links risk-sharing and social networks, see Angelucci et al., 2011). Our findings on the comparability of survey and experimental estimates is consistent with Barr and Genicot (2008) and Ligon and Schecter (2012); both show that behavior of network members is correlated across laboratory and real-world settings.

[18] Randomization check, available from authors, shows balance on observables identical to Table 1.

Rs. 50 vouchers. Appendix Figure 1 provides pictures of these vouchers. A voucher could only be redeemed by one client and all vouchers expired within two weeks.

Supplementary Lottery Frequent interaction with group members could cause a client to either expand and strengthen her existing social network or to substitute microfinance group members for existing members of her network. To examine the nature of network change, we implemented a supplementary lottery. Our sample is drawn from five-member VFS groups formed between January and September 2008 (roughly a year and half after the experimental loan groups were formed). As before, groups were randomly assigned to either a weekly or a monthly schedule. For comparability with previous estimates, our lottery was restricted to new (first-time) borrowers, which encompasses 55 Control (Monthly-Monthly) and 51 Treatment 1 (Weekly-Weekly) clients (from 39 and 35 groups respectively). Clients were approached in the same manner as in the original lottery. The difference was that the new lottery first asked each client how many tickets she wanted to give to group members (up to four), and then how many tickets she wanted to give to individuals outside of the group (up to four). The voucher prize in this lottery was always divisible.

Lottery Data We use data on ticket-giving by a client. For each client in the main lottery, we have, on average, nine pairwise observations on whether she gave a ticket to each of her group members, and for each client in the supplemental lottery, we have eight pairwise observations.

How Artifactual Was the Lottery? Our lottery game shares many design features of the trust game. In using a lottery game in place of a trust game, our primary interest was to avoid triggering client awareness of being a participant in an experiment. Aside from banking, VFS undertakes many community interventions and conducts regular promotional activities in order to attract and retain clients. Thus, it is likely that clients perceived the invitation to participate in a VFS lottery as a natural VFS activity. The potential for the lottery to seem artifactual arises from the invitation to give tickets to other group members. However, the fact that client selection for the lottery was described as a reward for survey participation during her first loan cycle and the fact that the lottery

was linked to the VFS store made it more natural that clients were offered the chance to give tickets to their very first loan cycle group members.[19]

3.2.2 Testable Predictions

Since group members who receive a ticket from a client are not obligated to share their winnings (as in a trust game), no ticket-giving is a Nash outcome. Risk-pooling via ticket-giving increases a client's expected payoff only if she anticipates that informal enforcement mechanisms will ensure sharing of resources (such as lottery winnings).

To see this, suppose the client gives one group member a ticket. The pair's joint chances of winning the lottery rise from 9% to 17%. There are mutual gains from risk-pooling (e.g., if the pair equally shares winnings then giving a ticket increases a client's expected lottery winnings from Rs. 18 to 25 and the pair member's expected winnings rise from Rs. 0 to 8.3), but costs to the client if there is no sharing (since her individual probability of winning the lottery declines from 9% to 8% as the pool of lottery entrants rises to 12; see Appendix Figure 2 for a graphical illustration).[20]

We use the lottery game to test the hypothesis that higher frequency of interaction can improve a client's ability to enforce risk-pooling arrangement with group members (on this mechanism, also see Karlan et al., 2009; Besley and Coate, 1995; Ambrus et al., 2010). We have already shown that higher meeting frequency in the first loan cycle strengthened long-run social ties between group members. Hence,

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

However, a positive correlation between meeting frequency and ticket-giving is also consistent with a model where more frequent interactions simply increase a client's unconditional altruism towards group members or increases her desire to signal willingness to share.

[19]Furthermore, in the supplementary lottery, we expanded the set of people clients could give tickets to and, as described below, our findings are very similar across the two lotteries.

[20]The top and bottom lines show a client's expected payoff with full and no sharing, respectively. The idea that risk-sharing can increase potential winnings is shared by a trust game, though the increase occurs with certainty in the trust game but stochastically in the lottery game. In addition, unlike a trust game, pairwise returns in the lottery depend on total ticket-giving, generating more subtle predictions on ticket-giving as a function of group composition, which we do not exploit.

To isolate the importance of meeting frequency for risk-sharing arrangements we exploit random variation in the divisibility of the lottery prize. A more divisible lottery prize should induce greater ticket-giving if and only if the client cares about the ease of reciprocal transfers.[21] Hence,

Prediction 2 *If ticket-giving only reflects (unconditional) altruism or signaling, then the incidence of ticket-giving will be independent of the receiver's perceived ability to reciprocate.*

Moreover, if giving is motivated by altruism or signaling, then a client's ticket-giving behavior towards a member should be independent of the order in which they played the lottery game. Hence,

Prediction 3 *If ticket-giving only reflects (unconditional) altruism or signaling, then the incidence of ticket-giving will be independent of the order in which pair members play the game.*

Finally, to examine whether higher meeting frequency caused clients to substitute social ties with group members for ties with non-group members, we use the supplementary lottery in which a client could choose to give tickets to non-group-members. Hence,

Prediction 4 *If ticket-giving to group members is accompanied by substitution away from social ties with non-group members, then ticket-giving to non-group members will be lower for Treatment 1 (Weekly-Weekly) clients than for Control (Monthly-Monthly) clients.*

3.2.3 Results

Our outcome of interest is ticket-giving: 67.2% of main lottery participants gave at least one ticket. Figure 3 shows the ticket distribution across Control and Treatment 1 clients (in percentage terms to account for group size differences) for the main lottery. After zero tickets, the fraction of group members that received tickets declines gradually and levels off after 60%. Control clients are more likely to not give tickets and less likely to

[21] The behavioral response to the divisibility of the lottery prize could potentially reflect the fact that framing the prize as divisible, and therefore shareable, primes a participant to think in terms of reciprocal arrangements. However, this possibility leaves our prediction unchanged: Divisibility should not matter if motivations for giving are purely altruistic or driven by signaling.

give tickets to more than 60% of their group. Ticket-giving patterns in the supplementary lottery are qualitatively similar, with Control clients more likely to not give tickets and less likely to give multiple tickets.

In Table 3 we provide regression results from the specifications given by Equation (2). Looking across all clients, we see that Treatment 1 clients gave 23.8% more tickets than the Control group (column 1), consistent with stronger social ties among clients who meet weekly translating into higher willingness to risk-share in the lottery game.

Next, we evaluate the importance of risk-sharing relative to either unconditional altruism or a desire to signal reciprocity (independent of willingness to risk-share) in explaining the link between ticket-giving and meeting frequency. We provide two pieces of evidence to rule out the latter two mechanisms as the only explanations for ticket-giving.

First, in columns (2) and (3) we show results for clients who were randomized into either the indivisible or divisible prize lottery, respectively. Relative to the Control group, Treatment 1 clients were significantly more likely to give a ticket to a group member if and only if the lottery prize was divisible. Among clients offered the divisible voucher, Treatment 1 clients were 31.9% more likely to give tickets than Control clients (9.1 percentage points). We observe no significant difference between experimental arms when the prize was a single indivisible voucher. These patterns suggest that more frequent meetings increased ticket-giving by improving risk-sharing arrangements.

Furthermore, for clients in the Control group, ticket-giving behavior was similar across voucher categories. This either suggests a non-risk-sharing motivation for ticket-giving among Control clients or that only marginal risk-sharing arrangements were sensitive to small barriers to trust, such as prize divisibility. Consistent with the former explanation, 76% of ticket-giving in the Control group was to either individuals that clients had not seen in the last 30 days, individuals not identified as sources of help in the case of emergency, or immediate family members.

Second, we exploit the fact that the order in which pair members i and j entered the lottery was random. We consider the sample of pairs in which (randomly) both members

were offered the divisible lottery prize, and estimate:

$$y_{gi}^{m} = \beta_1 y_{gm}^{i} + \beta_2 F_{gi}^{m} + \beta_3 y_{gm}^{i} \times F_{gi}^{m} + X_{gi}\gamma + \epsilon_{gi}^{m} \tag{3}$$

where y_{gi}^{m} and y_{gm}^{i} reflect client i and pair-member m's respective ticket-giving decisions. F_{gi}^{m} is an indicator variable for whether client i was surveyed after pair member m. A positive β_3 implies an order effect such that the pair member chosen to go second is more likely to give if her pair member had given her a ticket. This order effect indicates reciprocity-based motivations for giving, and hence should be absent if only unconditional altruism or signaling drives ticket-giving. Column (4) shows a positive and significant coefficient on the interaction term among the sub-sample of treatment clients, and in column (5) we see that this order effect is absent among the Control group. This provides additional evidence that reciprocity was not a primary motivation driving monthly clients' ticket-giving decisions, and is consistent with the absence of influence of lottery prize divisibility for the Control group.

In sum, the lottery results indicate that randomly induced social interaction increased willingness to pool risk in the lottery game. Finally, we use the supplementary lottery to test whether greater risk-pooling among group members was accompanied by substitution away from risk-pooling arrangements with non-group members. For each client we have eight observations, four pertaining to non-group members (we capped ticket-giving to non-group members at four tickets) and four pertaining to group members. We estimate:

$$y_{gi}^{m} = \beta_1 T_{1,g} + \beta_2 D_{gi}^{m} + \beta_3 T_{1,g} \times D_{gi}^{m} + X_{gi}\gamma + \epsilon_{gi}^{m} \tag{4}$$

where y_{gi}^{m} reflects client i's ticket-giving decision, and D_{gi}^{m} is an indicator variable for whether individual m is i's group member. We anticipate that β_3 is positive, i.e., ticket-giving is higher among group members of Treatment 1 clients. If there is substitution then β_1 (which captures ticket-giving to non-members) will be *negative*.

Column (6) shows that, consistent with the main lottery, treatment clients are signifi-

cantly more likely to give tickets to group members in the supplementary lottery ($\beta_3 > 0$). However, β_1 is close to zero and insignificant, suggesting no corresponding decline in the propensity to give tickets to non-group members. Hence, strengthening social ties among group members does not appear to cause clients to substitute away from risk-pooling arrangements with non-group members.

The lack of substitution is consistent with qualitative evidence, which suggests that isolation rather than time constrains friendship formation. In interviews, study clients stated that meetings provided them with a reason to leave their home and interact with others in the community. To measure this more systematically, in December 2011 we conducted a detailed time-use survey with 50 women (randomly selected from those who entered the supplementary lottery). The survey collected hourly data over the past 24 hours on what a respondent did and with whom they spent their time. On average, a woman spent 45 minutes per day watching television by herself, 45 minutes per day resting by herself, and 26 minutes engaging in other leisure time activities alone. At the end of the survey, each respondent was asked whether she would like to spend more time per week socializing with other women in her community and whether she had the spare time to do so. On average, 86% reported having time to speak with someone who wanted to talk with them, and 66% desired more friends with whom they could spend time.

Finally, we turn to financial transfers data from the endline survey conducted at the end of the first loan cycle. This both provides a consistency check on our risk-sharing interpretation of ticket-giving and tests whether behavior in the potentially artifactual field experiment correlates with behavior outside of the experiment. Since 43% of clients report no transfers, we focus on a binary outcome of whether the client reported transfers to or from individuals over the last year, grouped into three self-reported categories: (i) close family and friends, (ii) other relatives and neighbors and (iii) other non-relatives. Unfortunately, unlike in the lottery data, we cannot identify transfers to VFS members.

Columns (7) and (9) show that transfers with close family members or friends and "other non-relatives" are equally likely among Treatment 1 and Control clients. However, Treatment 1 clients are 39% more likely to report transfers to other relatives and neighbors

(column 8). Thus, consistent with the supplementary lottery ticket-giving results, we see increased risk-sharing and no displacement of risk-sharing arrangements within the immediate family or with other non-relatives.

4 Meeting Frequency and Loan Default

Mandating more frequent group meetings during the first loan cycle led to a persistent increase in social interactions and greater risk-pooling by group members. We now examine whether these impacts reduced household vulnerability to economic shocks.

In our setting, a carefully measured indicator of economic vulnerability that is observed for an extended period for all clients is loan default. While default reflects more than vulnerability to shocks, shocks are a strong predictor of default in our data and elsewhere, and informal insurance can be assumed to decrease the likelihood of individual default in the event of a shock (Besley and Coate, 1995; Wydick, 1999).[22]

We focus on default in the second loan cycle. All clients (except one who died) took out a second loan and were placed on an identical fortnightly (every two weeks) repayment schedule for the second loan cycle.[23] Appendix Table 1 Panel B reports summary statistics pertaining to clients' second loan cycle, and verifies that they do not vary systematically with treatment status in the first loan cycle. Clients took out a second loan roughly three months after the end of their first loan. The typical second loan was 85% larger than the first, reflecting VFS policy that has clients start well below credit demand and graduate slowly to larger loans. Loan size and timing of disbursement is uncorrelated with first loan repayment schedule. We also have second loan use data for a subset of clients, which reveals that most clients used the loan for business-related purposes. This also does not differ by treatment status during first loan cycle.

[22] In our data, we observe that illness episodes are strong predictors of default, and that transfers are associated with lower default risk. Also, personal savings reduce default risk and home ownership increases it, which likely reflects associated illiquidity. These results are available from the authors.

[23] A VFS policy change meant that clients in their second loan cycle were placed in five-member groups, and data analysis shows that clients who lived near each other tended to be in the same group.

4.1 Results

4.1.1 Experimental Estimates: Control versus Treatment 1

Table 4 presents regression estimates for default outcomes. Our regression specification parallels Equation (2), but the outcome of interest is now defined at the client level and is an indicator variable Y_{gi} which equals one if client i who belonged to group g in her first loan cycle defaulted on her second loan. We report both Probit and OLS specification.

As before, we first consider the sample of Control and Treatment 1 clients. In columns (1) and (2) we see that, despite the fact that all individuals faced the same loan terms for their second loan, a client who was previously assigned to a Treatment 1 schedule during her first loan cycle is more than three times (6.2%) less likely to default on her *second* loan relative to a Control client who was previously assigned to meet on a monthly basis. The difference is strongly significant with or without controls, and is virtually unchanged across Probit and OLS specifications.

4.1.2 IV Estimates: Meeting versus Repayment Frequency

In our comparisons thus far, clients who met more often during their first loan cycle also repaid at a higher frequency for the first loan cycle. By considering default in the subsequent loan cycle, we avoid the possibility that *contemporaneous* differences in repayment frequency influence default outcomes.[24] However, while initial differences in repayment frequency are unlikely to influence differences in social interactions per se, they may change long-run financial habits and, thereby, default.

To isolate the long-run influence of initial differences in meeting frequency from that of repayment frequency, we now examine whether the influence of higher meeting frequency remains when we compare second loan default outcomes across clients who all repaid on a monthly basis in their first loan cycle but differed in whether they met weekly or monthly.

As described in Section 2, for the purpose of disentangling these influences, our exper-

[24] Appendix Table 1 Panel A shows that frequent meetings did *not* influence default in the first loan cycle. An important caveat is the low overall default for first-time borrowers (0.5% among Control clients), which is unsurprising given low loan repayment burden.

imental design included a treatment arm in which clients were required to meet weekly but repay on a monthly basis (Treatment 2). To achieve this, we interspersed the standard monthly group repayment meetings with somewhat artificial weekly "non-repayment group meetings." During non-repayment meetings, loan officers recorded attendance and collected survey data from each individual. In addition, during the first eight meetings, loan officers led a brief (ten-minute) discussion on a topic of common interest, which varied from social concerns, like street safety, to social topics such as recipe exchange.[25]

Appendix Figure 3 documents the number of meetings held by repayment schedule. Roughly half of the Treatment 2 groups met less frequently than the minimum required by protocol, and thus can be considered non-compliers. According to interviews with loan officers (conducted after the experiment ended when noncompliance was detected), the fact that they did not need to collect and deliver money to VFS after a non-repayment meeting reduced their sense of accountability and made them more inclined to cancel non-repayment meetings (relative to repayment meetings) when inconvenient. Loan officers also acknowledged that meeting cancellations early in the loan cycle caused clients to view the institution of non-repayment meetings as dispensable, making it harder to sustain non-repayment meetings later in the loan cycle. An important reason for early cancellations was monsoon rains which caused waterlogging of neighborhoods and roads, increasing both loan officer and client commute time (60% of our loan groups were formed during monsoon months; on the impact of monsoon rains on daily life in Kolkata also see Beaman and Magruder, 2011).[26]

To address imperfect compliance in Treatment 2, we use an IV specification that makes

[25]For ethical reasons, we were requested to provide information useful to clients during non-repayment meetings to justify the cost they were being asked to incur by attending the meetings. We chose topics that we did not expect to directly influence business or social outcomes. Loan officers were provided scripts for each session and required only to read information from the script. Topics covered were: awareness about street safety; geographical knowledge about India; general knowledge about family ancestry; recipe exchange; questions on how they spend vacations or holidays; information on bus routes in their neighborhoods; basic physiology; basic information on state politics.

[26]A VFS loan officer's average work day lasts 12 hours, and consists of conducting group meetings in the morning and then returning to the branch office by early afternoon to deposit the repayments that had been collected and complete paperwork. On an average day, a loan officer would conduct five to six group meetings and cover a distance of 20 kms on bicycle.

use of this exogenous variation in monsoon rainfall shocks early in the loan cycle in order to predict Treatment 2 groups that met at least 23 times. This is the minimum number of times required by protocol, and also happens to be the median meeting rate for Treatment 2 groups. Our analysis sample for the IV estimates includes only Control and Treatment 2 clients, all of whom repaid monthly. If, among clients who repaid monthly, those who met weekly exhibit lower default incidence, then we will have identified an independent role for meeting frequency. The first stage of our IV regression is:

$$M_{gi}^{23+} = \beta_1 T_{2,g} + \beta_2 Heavy_g + \beta_3 T_{2,g} \times Heavy_g + X_{gi}\gamma + \epsilon_{gi} \tag{5}$$

M_{gi}^{23+}, now on *group met weekly*, is an indicator variable which equals one if individual i belonged to a group g which met at least 23 times during the loan cycle.[27] M_{gi}^{23+} equals 0 for all Control groups (since there was perfect compliance in this arm). $T_{2,g}$ is an indicator variable for assignment to Treatment 2 (Weekly-Monthly). $Heavy_g$ is the number of heavy rainfall days (defined as days with rainfall above the 90th percentile of rainfall distribution for the city) during the first month of meetings.[28] While it is possible that rainfall has a direct effect on social or economic outcomes, it is unlikely that rainfall shocks over such a short time period directly influence long-run social interactions and/or economic activity and, therefore, client ability to repay in the subsequent loan cycle. Hence, our exclusion restriction is likely to be satisfied. Furthermore, we have confirmed with baseline data that an additional day of heavy rain over the seven days before a client is surveyed does not influence a household's wage income or likelihood of employment. [29]

Column (3) of Table 4 reports this first stage regression. Treatment 2 clients at the mean value of days of heavy rain (5.7) were 75% less likely to meet the minimum required number of times than those who experienced zero days of heavy rain 31–60 days after group formation. Thus heavy rainfall very significantly influenced the sustainability of

[27] We define a meeting as having occurred if at least two group members attended.

[28] This corresponds to days 31–60 after group formation.

[29] Our results are also robust to extending the definition of Heavy Rain to include the first two months of the loan cycle, or to using the 80th or 85th percentile of the rainfall distribution as the cutoff (results available from authors).

non-repayment meetings over the loan cycle.

Given this first stage, we turn to the IV estimate of the impact of increased meeting frequency, holding constant repayment frequency. Our structural equation of interest (i.e., second stage) is:

$$y_{gi} = \beta M_{gi}^{23+} + X_{gi}\gamma + \epsilon_{gi} \tag{6}$$

Column (4) reveals a negative and significant impact of higher meeting frequency in first loan cycle on default for the second loan.[30] The coefficient estimate is almost identical in magnitude (even slightly larger) and statistically indistinguishable from the experimental estimate in columns (1) and (2). Thus, we can rule out the possibility that lower long-run default rates among clients assigned to a weekly meeting schedule reflect improvements in their financial habits or business practices associated with having repaid their first loan on a weekly basis.

5 Conclusions

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

We provide experimental evidence that a development program that encourages repeat interactions can increase long-run social ties and enhance social capital among members of a highly localized community in a strikingly short amount of time. With only the outside stimulus of MFI meetings, close neighbors from similar socioeconomic backgrounds got to know each other well enough to cooperate in an economically meaningful way, which

[30] We employ a linear IV specification given the strong functional form assumptions associated with the biprobit model (Angrist and Pischke, 2009).

provided a buffer against economic shocks that lead to default. While many studies have suggested a link between social capital and MFI default rates, ours is the first to provide rigorous evidence on the role of microfinance in building social capital, and thereby broaden our understanding of the channels through which MFIs achieve low default rates without the use of physical collateral. Arguably, the improvements in risk-sharing we observe are even more striking because they were obtained in the absence of joint-liability contracts, and provide a rationale for the current trend among MFIs of maintaining repayment in group meetings despite the transition from joint- to individual-liability contracts (Gine and Karlan, 2009). While it is difficult to account for *all* of the increased transactions costs of weekly meetings with higher loan recovery rates alone, direct cost savings from lowering default go a long way towards explaining why weekly meetings persist as the standard MFI practice.[31] Furthermore, there are many reasons to believe that the typical MFI is sufficiently delinquency- and/or default-averse to make weekly meetings cost effective.[32]

Using meetings to improve risk-sharing in a setting characterized by weak formal institutions for contract enforcement is a potentially important source of welfare gains, at least for first-time clients. Although encouraging social interaction entails higher participation costs for clients, the benefits from social network expansion are likely to outweigh the cost. We estimate that weekly compared to monthly meetings entail approximately 15 additional hours of client time over the course of an average loan cycle.[33] The benefits are likely to include, in addition to lower default risk, utility gains from consumption smooth-

[31] We estimate an additional average cost per client of Rs. 85 for a weekly relative to a monthly meeting schedule. Loan officers spend an additional three hours per month per group, which amounts to 1.9% of their monthly wage for the average group (of ten clients), or Rs. 85. This includes an additional hour in meeting time and an additional two hours in commuting time. Average loan officers' commuting time from branch to group leaders' home is 20 minutes by bicycle. Meanwhile, our data indicate that the average client who met and repaid monthly during her initial loan cycle defaulted on only Rs. 30 more than one previously on a weekly repayment schedule.

[32] For instance, delinquency (even if it does not translate into default) reduces MFI liquidity and ability to expand lending, and MFI credit ratings are typically calculated based on the share of an MFI's portfolio in arrears.

[33] The estimate of two additional hours per month is based on an average meeting length of 20 minutes combined with an average commute distance of 500 meters, which corresponds to a commute time of ten minutes from client's home to group leader's home. The estimated 15 hours over the course of a loan cycle is based on the fact that a client repays her loan, on average, after 7.5 months.

ing in addition to other positive externalities from social interaction such as information sharing.[34]

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

References

Alesina, A. and E. La Ferrara (2002). Who Trusts Others? *Journal of Public Economics 85*(2), 207–234.

Ambrus, A., M. Möbius, and A. Szeidl (2010). Consumption Risk-sharing in Social Networks. mimeo.

Angelucci, M., G. de Giorgi, and I. Rasul (2011). Insurance and Investment within Family Networks. Working Paper.

Angrist, J. D. and J.-S. Pischke (2009). *Mostly Harmless Econometrics: an Empiricist's Companion*. Princeton, NJ: Princeton University.

Attanasio, O., A. Barr, J. C. Cardenas, G. Genicot, and C. Meghir (2011). Risk Pooling, Risk Preferences, and Social Networks. *American Economic Journal: Applied Economics,* forthcoming.

[34]While the cost to clients is likely to exceed the simple time cost of meeting attendance given the additional financial burden of making regular installments, this is presumably a less important cost for first time clients who are typically given very small loans. (Field et al., 2011).

Barr, A. and G. Genicot (2008). Risk Sharing, Commitment, and Information: An Experimental Analysis. *Journal of the European Economic Association 6*(6), 1151–1185.

Barrios, T., R. Diamond, G. Imbens, and M. Kolesar (2010). Clustering, Spatial Correlation and Randomization Inference. Harvard Economics Department Working Paper.

Beaman, L. and J. Magruder (2011). Who Gets the Job Referral? Evidence from a Social Networks Experiment. *American Economic Review,* forthcoming.

Berg, J., J. Dickhaut, and K. McCabe (1995). Trust, Reciprocity and Social History. *Games and Economic Behavior 10*(1), 122–142.

Besley, T. and S. Coate (1995). Group Lending, Repayment Incentives and Social Collateral. *Journal of Development Economics 46*(1), 1–18.

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.

Consultative Group to Assist the Poor (CGAP) (2012). http://www.cgap.org/p/site/c/.

Costa, D. and M. Kahn (2003). Understanding the Decline in American Social Capital, 1953-1998. *Kyklos 56*(1), 17–46.

Dal Bo, P. (2005). Cooperation Under the Shadow of the Future: Experimental Evidence from Infinitely Repeated Games. *American Economic Review 95*(5), 1591–1604.

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. Möbius, and T. Rosenblat (2009). Directed Altruism and Enforced Reciprocity in Social Networks. *Quarterly Journal of Economics,* forthcoming *124*(9).

Fafchamps, M. and F. Gubert (2007). The Formation of Risk-Sharing Networks. *Journal of Development Economics 83*(2), 326–50.

Field, E., R. Pande, J. Papp, and N. Rigol (2011). Debt Structure, Entrepreneurship, and Risk: Evidence from Microfinance. Working Paper.

Forsythe, R., J. Horowitz, N. Savin, and M. Sefton (1994). Fairness in Simple Bargaining Games. *Games and Economic Behavior 6*, 347–69.

Ghatak, M. and T. W. Guinnane (1999). The Economics of Lending with Joint Liability: Theory and Practice. *Journal of Development Economics 60*(1), 195–228.

Gine, X., P. Jakiela, D. Karlan, and J. Morduch (2010). Microfinance Games. *American Economic Journal: Applied Economics 2*(3), 60–95.

Gine, X. and D. Karlan (2009). Group versus Individual Liability: Long Term Evidence from Philippine Microcredit Lending Groups. Working Paper.

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. (2007). Social Connections and Group Banking. *Economic Journal 117*(517), F52–F84, 02.

Karlan, D., M. Möbius, T. Rosenblat, and A. Szeidl (2009). Trust and Social Collateral. *Quarterly Journal of Economics 124*(3), 1307–1361.

Kling, J., J. Liebman, and L. Katz (2007). Experimental Analysis of Neighborhood Effects. *Econometrica 75*(1), 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.

Larance, L. (2001). Fostering Social Capital Through NGO Design: Grameen Bank Membership in Bangladesh. *International Social Work 44*.

Ligon, E. and L. Schecter (2008). The Value of Social Networks in Rural Paraguay. University of Wisconsin Working Paper.

Ligon, E. and L. Schecter (2012). Motives for Sharing in Social Networks. *Journal of Development Economics,* forthcoming.

Mace, B. J. (1991). Full insurance in the presence of aggregate uncertainty. *Journal of Political Economy 99*(5), 928–956.

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 D. Levine (2005). Does Social Capital Promote Industrialization? Evidence from a Rapid Industrializer. *Review of Economics and Statistics 87*(4), 754–762.

MIX Market (2012). http://www.themix.org/.

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 1*(4), 1–33.

Putnam, R. (1993). *Making Democracy Work: Civic Traditions in Modern Italy*. Princeton, NJ: Princeton University Press.

Townsend, R. (1994). Risk and Insurance in Village India. *Econometrica 62*(3), 539–591.

Wydick, B. (1999). Can Social Cohesion be Harnessed to Repair Market Failures? Evidence from Group Lending in Guatemala. *Economic Journal 109*(July), 463–475.

Table 1. Randomization Check

	All Clients			Lottery/Long-Run Survey Clients		
	Control Mean (Monthly-Monthly)	Treatment 1 (Weekly-Weekly)	Treatment 2 (Weekly-Monthly)	Control Mean (Monthly-Monthly)	Treatment 1 (Weekly-Weekly)	Treatment 2 (Weekly-Monthly)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Age	33.969	-0.593	-1.110	33.832	-0.806	-0.920
	(8.553)	(0.813)	(0.724)	(8.418)	(0.810)	(0.764)
Literate	0.865	-0.012	-0.059	0.880	-0.012	-0.059
	(0.342)	(0.035)	(0.039)	(0.325)	(0.036)	(0.040)
Married	0.862	0.013	0.005	0.871	0.025	-0.009
	(0.345)	(0.031)	(0.030)	(0.336)	(0.030)	(0.029)
Household Size	3.821	0.153	0.207*	3.903	0.068	0.106
	(1.335)	(0.106)	(0.114)	(1.357)	(0.119)	(0.124)
Muslim	0.023	-0.023	0.118**	0.026	-0.026	0.122**
	(0.151)	(0.021)	(0.060)	(0.159)	(0.023)	(0.062)
Years Living in Neighborhood	14.218	-1.287**	-0.270	14.065	-1.310**	-0.010
	(6.578)	(0.582)	(0.567)	(6.615)	(0.614)	(0.604)
Number of Clients in Group	10.364	-0.086	-0.037	10.385	-0.073	-0.054
	(0.727)	(0.185)	(0.192)	(0.741)	(0.199)	(0.196)
Group Formed in Rainy Season	0.595	-0.147	-0.109	0.654	-0.154	-0.159
	(0.492)	(0.122)	(0.120)	(0.477)	(0.124)	(0.119)
Heavy Rain Days	5.951	-0.310	-0.486	6.191	-0.423	-0.685
	(2.249)	(0.619)	(0.519)	(2.230)	(0.648)	(0.533)
Panel B						
Client Worked for Pay in Last 7 Days	0.525	0.060	0.011	0.524	0.056	0.018
	(0.500)	(0.053)	(0.053)	(0.500)	(0.053)	(0.053)
Household Earns Fixed Salary	0.439	-0.076*	0.026	0.437	-0.065	0.048
	(0.497)	(0.044)	(0.049)	(0.497)	(0.046)	(0.050)
Household Owns Business	0.717	0.038	-0.080	0.718	0.034	-0.085
	(0.451)	(0.049)	(0.061)	(0.450)	(0.053)	(0.061)
Household Savings	1937.0	1679.3	1109.9	2267.0	1916.7	899.4
	(11005.6)	(1875.0)	(1189.2)	(12251.5)	(2298.8)	(1347.6)
Household Owns Home	0.808	-0.033	-0.035	0.828	-0.048	-0.047
	(0.395)	(0.044)	(0.047)	(0.378)	(0.046)	(0.048)
Education Expenditures	4639.8	371.1	-276.6	4987.9	-134.9	-600.2
	(5772.3)	(476.8)	(476.6)	(5892.2)	(546.0)	(535.4)
Health Expenditures	3311.4	-35.0	-399.4	3241.4	-87.7	-226.9
	(5262.1)	(522.2)	(432.4)	(5154.4)	(562.9)	(432.1)
Illness in Past 12 Months	0.314	0.029	-0.080*	0.307	0.016	-0.062
	(0.465)	(0.048)	(0.046)	(0.462)	(0.053)	(0.049)
Number of Transfers into Households	2.613	0.282	-0.253	2.563	0.311	-0.147
	(4.693)	(0.604)	(0.558)	(4.728)	(0.658)	(0.592)
Number of Transfers out of Households	1.374	0.182	-0.494	1.078	0.207	-0.185
	(6.762)	(0.539)	(0.445)	(4.645)	(0.360)	(0.333)
Days between Loan Disbursement and Lottery				788.597	-0.500	13.997
				(46.526)	(11.391)	(11.021)
N	385	306	325	309	250	297

Notes

1 Group Formed in Rainy Season is an indicator variable for whether the group was formed in June, July, August, or September. Heavy Rain Days is a count variable representing the number of days within 31-60 days after group formation in which rain was above the 90th decile for daily rainfall (14.3 mm). Illness in Past 12 Months is an indicator variable for whether any household member has been ill in past 12 months.

2 Columns (2)-(3) are the regression results of the characteristics in the title column on the two treatments for the full sample. The omitted group is clients in Control groups. In columns (5)-(6) we report the same coefficients for the sample that received the lottery. All lottery sample regressions control for survey phase. * , **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors are clustered by group.

Table 2. Meeting Frequency and Social Interactions in the Short Run and Long Run

	Short Run	Long Run			
	Social Contact Index	Total Times Met	Attend Durga Puja	Talk Family	Social Contact Index
	(1)	(2)	(3)	(4)	(5)
Panel A: No Controls					
Treatment 1	2.661***	2.085**	0.070*	0.071*	0.176**
(Weekly-Weekly)	(0.112)	(1.016)	(0.039)	(0.039)	(0.076)
Panel B: Controls Included					
Treatment 1	2.695***	2.078**	0.080**	0.069**	0.184***
(Weekly-Weekly)	(0.102)	(0.909)	(0.038)	(0.035)	(0.068)
Control Mean		5.459	0.152	0.229	
(Monthly-Monthly)		[10.375]	[0.359]	[0.420]	
Specification	OLS	OLS	Probit	Probit	OLS
N	683	3034	3034	3034	3034

Notes

1 Short-Run Social Contact Index generates average effect size from four client questions: (1) "Have you ever visited houses of all group members?" (2) "Have all of your group members visited your house?" (3) "Do you know the names of the family members of your group members?" and (4) "Do you know if any of your group members had relatives come over in the last 30 days?" The first three variables equal one if client responds yes at least once between month 3 and week 23 of her loan cycle, and the fourth is the mean value of client responses over this period. Long-Run Social Contact Index generates average effect size from three questions asked to each client during the lottery survey: (1) Total Times Met, (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 The sample is clients assigned to Treatment 1 (Weekly-Weekly) and Control (Monthly-Monthly) groups.

3 Regressions with controls include the variables in Table 1, Panel A. All long-run regressions also control for survey phase. * , **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors are clustered by group.

Table 3. Meeting Frequency and Risk-Sharing: Ticket-Giving and Transfers

	Main Lottery					Supplementary Lottery	Transfers		
	Gave Ticket								
	All	1-Rs. 200 Voucher	4-Rs. 50 Vouchers			All	Close Family/ Friend	Neighbor/ Other Relative	Other Non-Relative
			All	Weekly	Monthly				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: No Controls									
Treatment 1	0.067**	0.043	0.091*			-0.006	0.016	0.122**	-0.019
(Weekly-Weekly)	(0.034)	(0.041)	(0.048)			(0.071)	(0.065)	(0.061)	(0.028)
Surveyed Second				0.039	0.077				
				(0.073)	(0.061)				
Other Pair Member Gave				0.050	0.212***				
				(0.090)	(0.071)				
Surveyed Second*Other Pair Member Gave				0.158**	0.012				
				(0.067)	(0.060)				
Group Member						0.106***			
						(0.038)			
Treatment 1*Group Member						0.132*			
						(0.074)			
Panel B: Controls Included									
Treatment 1	0.069**	0.043	0.098**			-0.013	0.019	0.123**	-0.012
(Weekly-Weekly)	(0.032)	(0.039)	(0.047)			(0.072)	(0.066)	(0.059)	(0.024)
Surveyed Second				0.022	0.072				
				(0.072)	(0.060)				
Other Pair Member Gave				0.017	0.225***				
				(0.086)	(0.073)				
Surveyed Second*Other Pair Member Gave				0.189**	0.005				
				(0.074)	(0.065)				
Group Member						0.105***			
						(0.038)			
Treatment 1*Group Member						0.136*			
						(0.076)			
Control Mean	0.281	0.277	0.285	Weekly	Monthly	0.241	0.426	0.309	0.067
(Monthly-Monthly)	[0.450]	[0.448]	[0.452]	Clients	Clients	[0.428]	[0.495]	[0.463]	[0.250]
Specification	Probit	Probit	Probit	Probit	Probit	Probit	Probit	Probit	Probit
N	5282	2695	2587	526	572	848	651	651	651

Notes

1 For the lottery, the dependent variable equals one for a group member if the client gave her a ticket. For each client in the sample we have (on average) nine observations for columns (1)-(3). In column (4) we include only Treatment 1 (Weekly-Weekly) pairs in which both pair members were assigned four-Rs. 50 Vouchers, and in column (5) we include only Control (Monthly-Monthly) pairs in which both pair members were assigned four-Rs. 50 Vouchers. In column (6), we include only clients borrowing for the first time during the Third Loan Cycle (see Figure 1 for details). For this column, we have eight observations for each client (four for group member ticket-giving and four for non-group member ticket-giving). In columns (7)-(9), Transfers are indicator variables for whether client's household gave or received any transfers to or from the relevant groups in the 12 months before the first loan endline survey. We divide transfers into three categories based on client's stated relationship with transfer recipient/sender at time of survey. Close Family/Friend includes the following relationship types: sibling, parent, child, child-in-law, sibling-in-law, parent-in-law, uncle/aunt, cousin, grandchild, and friend. Neighbor/Other Relative includes all other relatives and unrelated neighbors. Other Non-Relative includes any other type of acquaintances.

2 The sample is clients assigned to Treatment 1 (Weekly-Weekly) and Control (Monthly-Monthly) groups.

3 Regressions with controls include the variables in Table 1, Panel A. All lottery regressions also control for survey phase. * , **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors are clustered by group.

Table 4. Meeting Frequency and Default: Evidence from the Second Loan Cycle

	Default		Group Met Weekly	Default
	(1)	(2)	(3)	(4)
Panel A: No Controls				
Treatment 1	-0.062***	-0.062**		
(Weekly-Weekly)	(0.024)	(0.024)		
Treatment 2 (Weekly-Monthly)*Heavy Rain Days			-0.139***	
			(0.018)	
Treatment 2			1.287***	
(Weekly-Monthly)			(0.145)	
Heavy Rain Days			0.007	
			(0.014)	
Group Met Weekly				-0.103**
				(0.047)
Panel B: Controls Included				
Treatment 1	-0.053**	-0.056**		
(Weekly-Weekly)	(0.021)	(0.023)		
Treatment 2 (Weekly-Monthly)*Heavy Rain Days			-0.147***	
			(0.021)	
Treatment 2			1.301***	
(Weekly-Monthly)			(0.153)	
Heavy Rain Days			0.009	
			(0.015)	
Group Met Weekly				-0.105**
				(0.049)
F Statistic			*27.21*	
p-value			*[0.000]*	
Control Mean (Monthly-Monthly)	0.084			
	[0.273]			
Specification	Probit	OLS	OLS	Linear IV
N	698	698	720	720

Notes

1 A client is defined as having defaulted if she has not repaid the total loan amount within 44 weeks after due date. Group Met Weekly is an indicator variable for whether a group met at least 23 times during First Loan Cycle. Heavy Rain Days is as defined in Table 1.

2 Column (3) provides the first stage regression for the IV regression in column (4).

3 Columns (1)-(2) include clients assigned to Treatment 1 (Weekly-Weekly) and Control (Monthly-Monthly) groups, and columns (3)-(4) include clients assigned to Treatment 2 (Weekly-Monthly) and Control (Monthly-Monthly) groups.

4 Panel A regressions in columns (3)-(4) include a control for Group Formed in Rainy Season, and regressions with controls (Panel B) include the variables in Table 1, Panel A. * , **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors are clustered by group.

Appendix Table 1. Robustness Checks: Impact of Meeting Frequency on Additional Outcomes

	Treatment 1 (Weekly-Weekly) **Explanatory Variable**				
	No Controls	Controls Included	Control Mean (Monthly-Monthly)	N	Data Source
Dependent Variable	(1)	(2)	(3)	(4)	(5)
Panel A: First Loan Cycle					
Default	-0.005	-0.006	0.005	699	Default Data
	(0.005)	(0.006)	[0.071]		
Late Repayment	-0.0004	-0.0006	0.0008	698	Group Meeting Survey
	(0.0006)	(0.0006)	[0.0092]		
Loan Officer Rank	-0.057	-0.050	2.674	675	Group Meeting Survey
	(0.054)	(0.050)	[0.310]		
Present	0.007	0.005	0.753	698	Group Meeting Survey
	(0.018)	(0.013)	[0.232]		
Late	0.230***	0.216***	0.177	696	Group Meeting Survey
	(0.062)	(0.055)	[0.258]		
Meeting Duration	-0.022	-0.022*	0.172	698	Group Meeting Survey
	(0.013)	(0.013)	[0.082]		
Household Member Attending School	0.002	-0.017	0.595	460	First Loan Cycle Endline Survey
	(0.046)	(0.048)	[0.492]		
Total Savings	-1005.8	-1295.4	1,381.7	448	First Loan Cycle Endline Survey
	(1305.1)	(1565.5)	[19755.0]		
Expanded Business in Past 30 Days	0.0008	-0.001	0.033	651	Endline Survey + Follow-up Survey
	(0.020)	(0.019)	[0.180]		
Panel B: Second Loan Cycle					
Days to Second Loan Takeup	-28.9	-30.3	116.3	698	Default Data
	(21.8)	(20.6)	[145.6]		
Second Loan Size	-14.1	8.2	7424.6	698	Default Data
	(143.6)	(134.0)	[913.6]		
Fraction Group Members in Second Loan Group	0.028	0.010	0.718	324	Administrative Data
	(0.064)	(0.060)	[0.319]		
Loan Used for Raw Materials	-0.033	-0.028	0.210	324	Second Loan Cycle Endline Survey
	(0.045)	(0.043)	[0.408]		
Loan Used for Business Equipment	-0.020	-0.018	0.270	324	Second Loan Cycle Endline Survey
	(0.062)	(0.058)	[0.445]		
Loan Used for Health Care Costs	0.056	0.068	0.065	324	Second Loan Cycle Endline Survey
	(0.054)	(0.047)	[0.247]		
Loan Used for Housing	0.052	0.057*	0.045	324	Second Loan Cycle Endline Survey
	(0.034)	(0.033)	[0.208]		

Notes

1 Late Repayment is the fraction of group meetings at which a client failed to make the scheduled repayment. Loan Officer Rank is measured on a four-point scale, with higher rankings reflecting a higher perceived ability to repay. Default is as defined in Table 4. Present and Late are averages taken for group meetings between month three and week 23 of the loan cycle. Meeting Duration is measured in hours and is averaged across all group meetings. Days to Second Loan Takeup is defined as the number of days between scheduled First Loan repayment and Second Loan takeup. Fraction Group Members in Second Loan Group is defined as the fraction of second loan group members also in first loan group. Loan Used for _____ are indicator variables and multiple loan uses may be listed for each loan in Second Loan Cycle.

2 For First Loan Cycle questions on savings and school attendance, the sample excludes First Loan Cycle clients who received the follow-up survey (which did not ask about these topics and was administered to clients who repaid their initial loans faster than anticipated). For Second Loan Cycle loan use questions, the sample includes only First Loan Cycle clients who remained research clients during the Second Loan Cycle (and so continued to be surveyed regarding loan use).

3 The sample is clients assigned to Treatment 1 (Weekly-Weekly) and Control (Monthly-Monthly) groups.

4 Regressions with controls include the variables in Table 1, Panel A (except for Days to Second Loan Takeup specification which includes all controls except for Group Formed in Rainy Season). * , **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors are clustered by group. Note that First Loan Default regressions employ linear specifications given lack of default among Treatment 1 (Weekly-Weekly) clients in the First Loan Cycle.

Online Appendix Table 1: Representativeness of VFS Borrowers

	VFS	SEWA	Spandana
	(1)	(2)	(3)
Income in Last Month	5069.3	6614.2	3672.2
	(3492.5)	(5171.3)	(5134.1)
Household Owns Business	0.703	0.368	0.492
	(0.457)	(0.483)	(0.500)
Number of Paid Employees	0.232	0.697	0.262
	(1.206)	(3.110)	(1.049)
Profit Last Month (Rs.)	3092.0	2861.4	-
	(3133.8)	(2872.5)	-
Number of Loans in Past Year	1.030	1.611	4.412
	(0.175)	(0.956)	(2.703)
Largest Loan (Rs.)	5936.5	27416.6	40265.7
	(13132.0)	(52423.9)	(86775.8)
Fraction Households with Savings	0.263	-	0.719
	(0.440)	-	(0.450)
Household Owns Home	0.786	0.757	0.787
	(0.410)	(0.429)	(0.409)
Number of Rooms in Home	1.757	1.822	2.221
	(1.087)	(0.918)	(1.143)
Household Owns TV	0.783	0.882	0.607
	(0.412)	(0.323)	(0.489)
Household Owns Two-wheeler	0.046	0.293	0.275
	(0.210)	(0.456)	(0.447)
Number of Household Members	3.933	5.703	5.902
	(1.336)	(2.265)	(2.248)
Has Insurance	0.352	-	0.495
	(0.478)	-	(0.500)
N	1016	853	1599

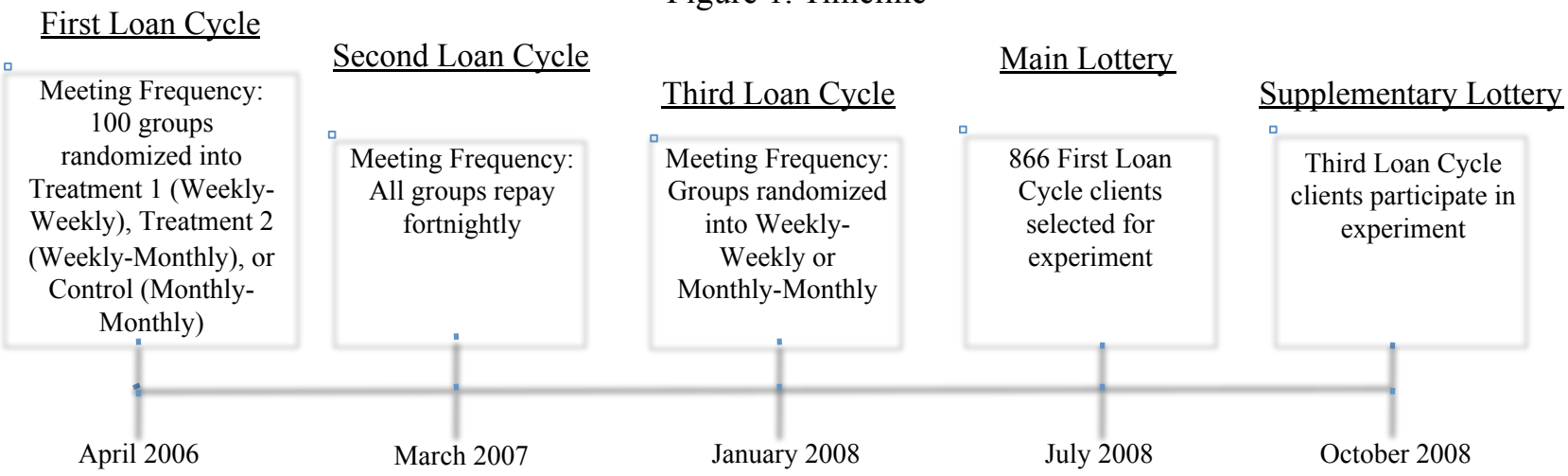
Notes

1 Number of paid employees is defined only for business owners. Profit last month is defined as the average of minimum and maximum monthly profits for VFS borrowers.

2 Each column presents the mean and standard deviation for the relevant sample and the given outcome variable.

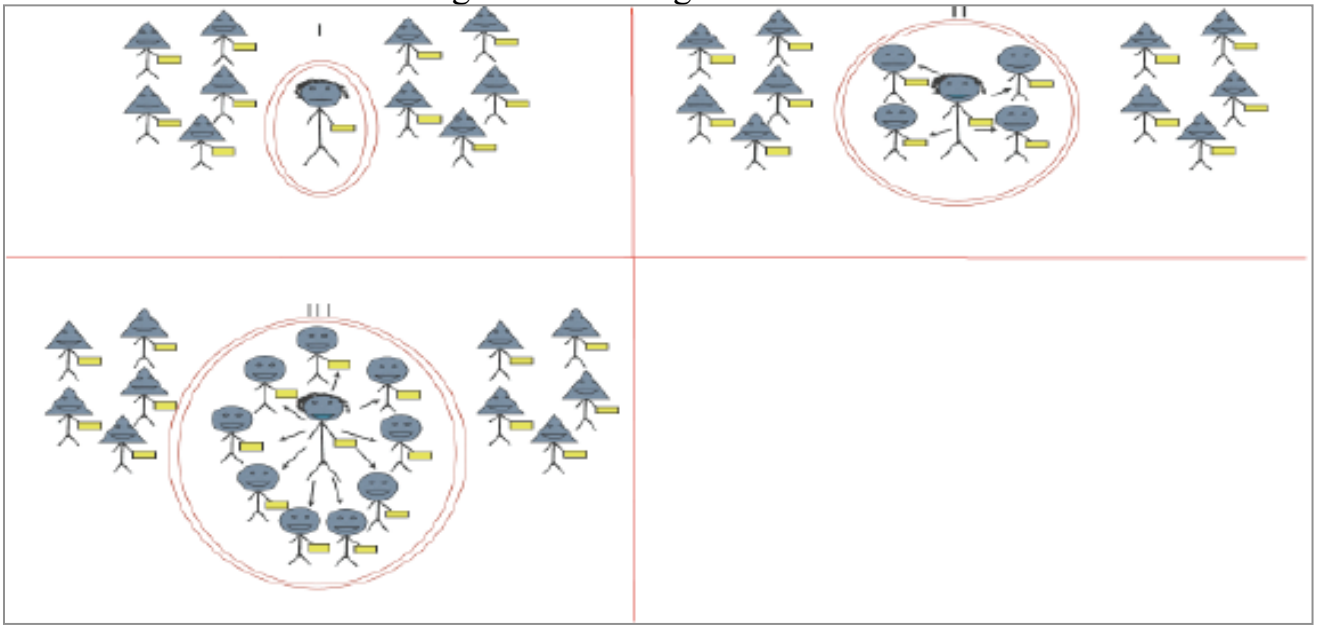
3 VFS data comes from the 2006 First Loan Cycle baseline survey. SEWA data comes from a 2009-2010 survey of SEWA clients conducted (and made available) by Field and Pande. Spandana data comes from a 2007-2008 endline survey conducted (and made available) by Banerjee and co-authors, and is restricted to respondents who have an outstanding MFI loan.

Figure 1. Timeline



Notes: Dates reflect the start of each loan cycle and of lottery surveying. Our sample population consisted of 1028 clients who joined VFS in 2006. For their first loan cycle 392 of these clients were randomly assigned to monthly meeting and monthly repayment (38 Control groups), 307 were assigned to weekly meeting and weekly repayment (30 Treatment 1 groups), and 329 were assigned to weekly meeting and monthly repayment (32 Treatment 2 groups). All but one client 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 in the third loan cycle were randomized into Weekly-Weekly or Monthly-Monthly groups. To examine the effects of meeting frequency on giving to non-group members, we restrict our sample to clients who were borrowing for the first time in the third loan cycle and who were in groups with at least one returning borrower. There are 106 such clients.

Figure 2. Winning Probabilities



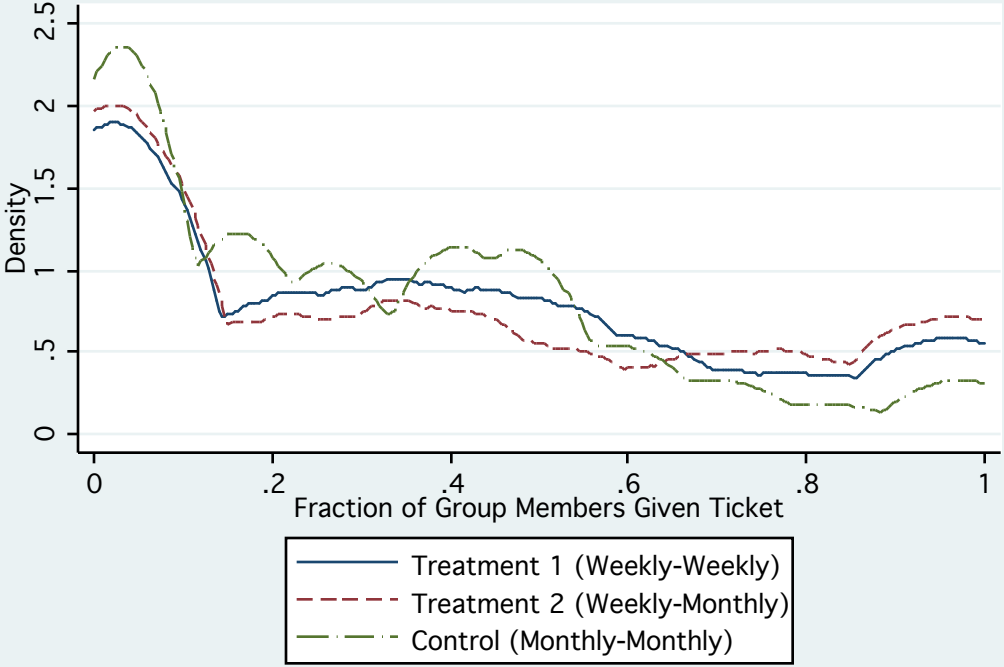
Notes:

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 VFS group, you have a 1 in 11 chance of winning.

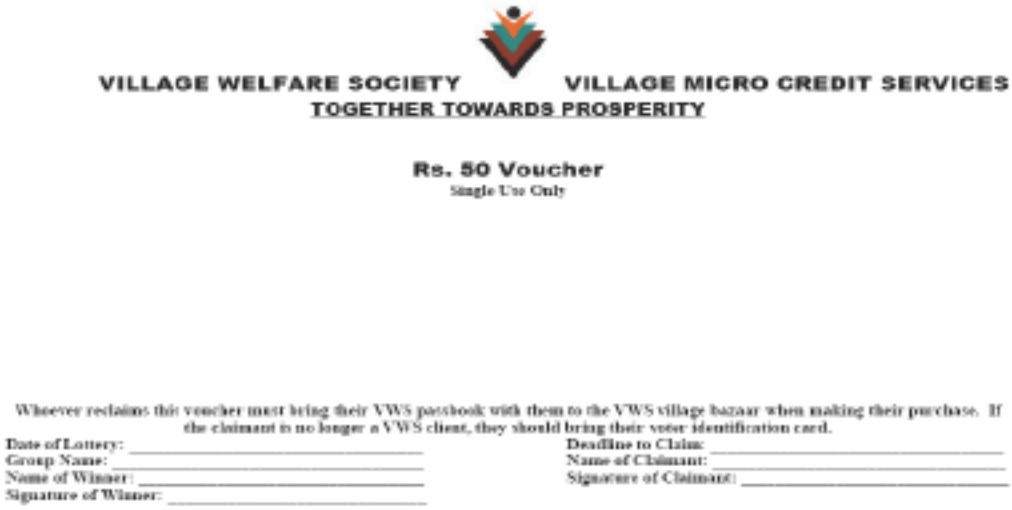
In Picture 2, you choose to have us give a ticket to four other members of your VFS group and there are 15 tickets total. In that case, you would have a 1 in 15 chance of winning and each of the members of your VFS group you gave a ticket to would have a 1 in 15 chance of winning.

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

Figure 3: Client-Level Distribution of Ticket-Giving



Appendix Figure 1. Lottery Vouchers



VILLAGE WELFARE SOCIETY VILLAGE MICRO CREDIT SERVICES

TOGETHER TOWARDS PROSPERITY

Rs. 50 Voucher

Single Use Only

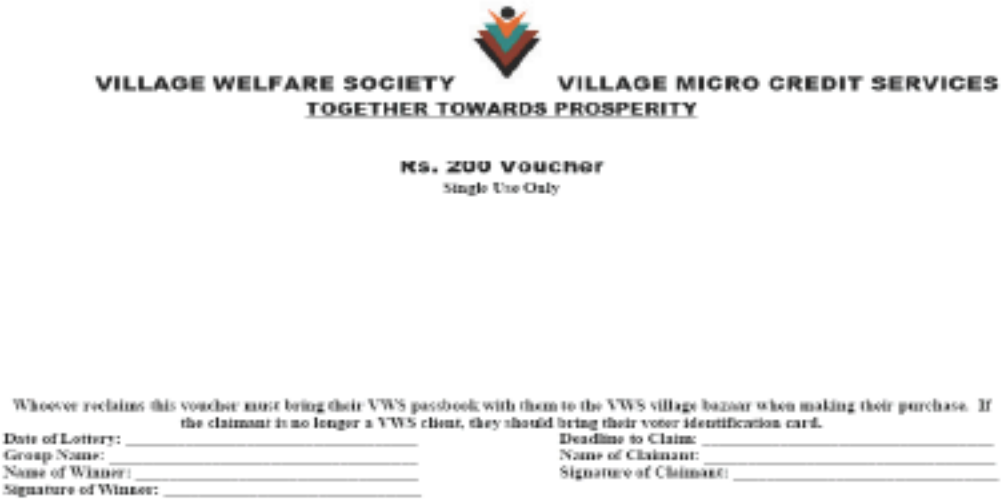
Whoever reclaims this voucher must bring their VWS passbook with them to the VWS village bazaar when making their purchase. If the claimant is no longer a VWS client, they should bring their voter identification card.

Date of Lottery: _____ Deadline to Claim: _____

Group Name: _____ Name of Claimant: _____

Name of Winner: _____ Signature of Claimant: _____

Signature of Winner: _____



VILLAGE WELFARE SOCIETY VILLAGE MICRO CREDIT SERVICES

TOGETHER TOWARDS PROSPERITY

Rs. 200 Voucher

Single Use Only

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

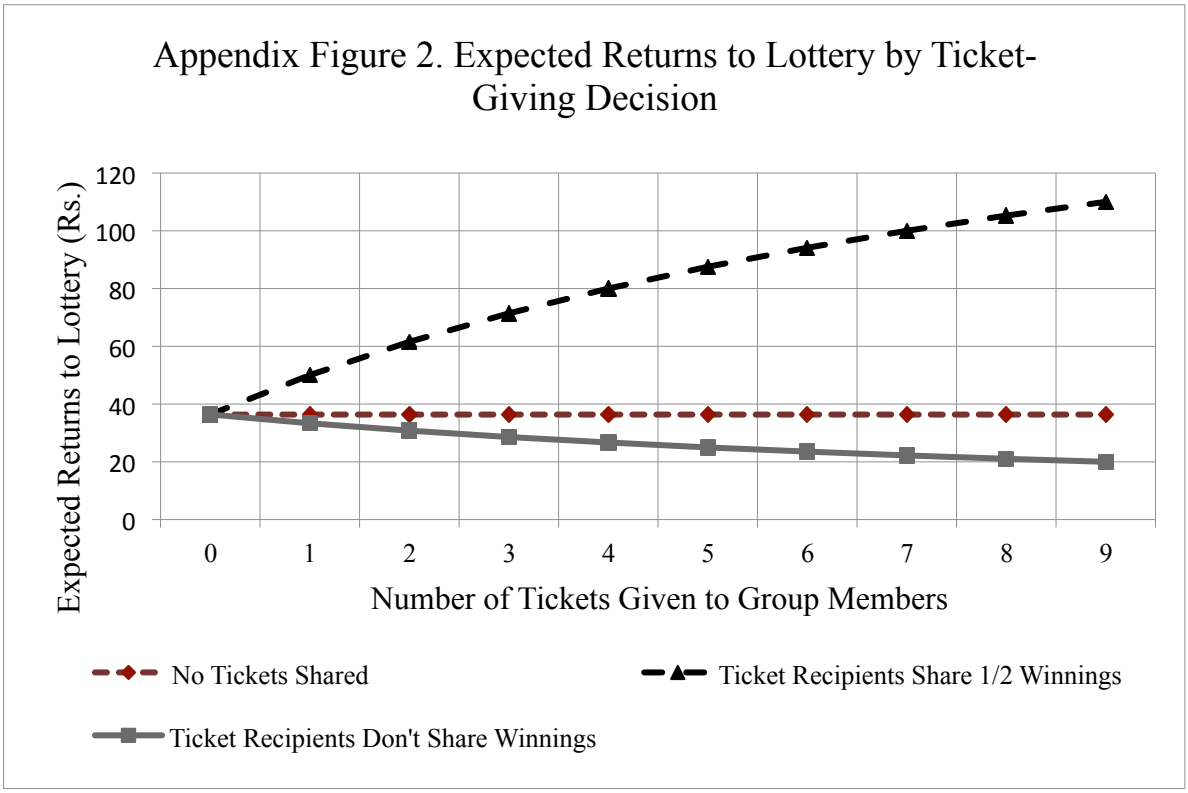
Date of Lottery: _____ Deadline to Claim: _____

Group Name: _____ Name of Claimant: _____

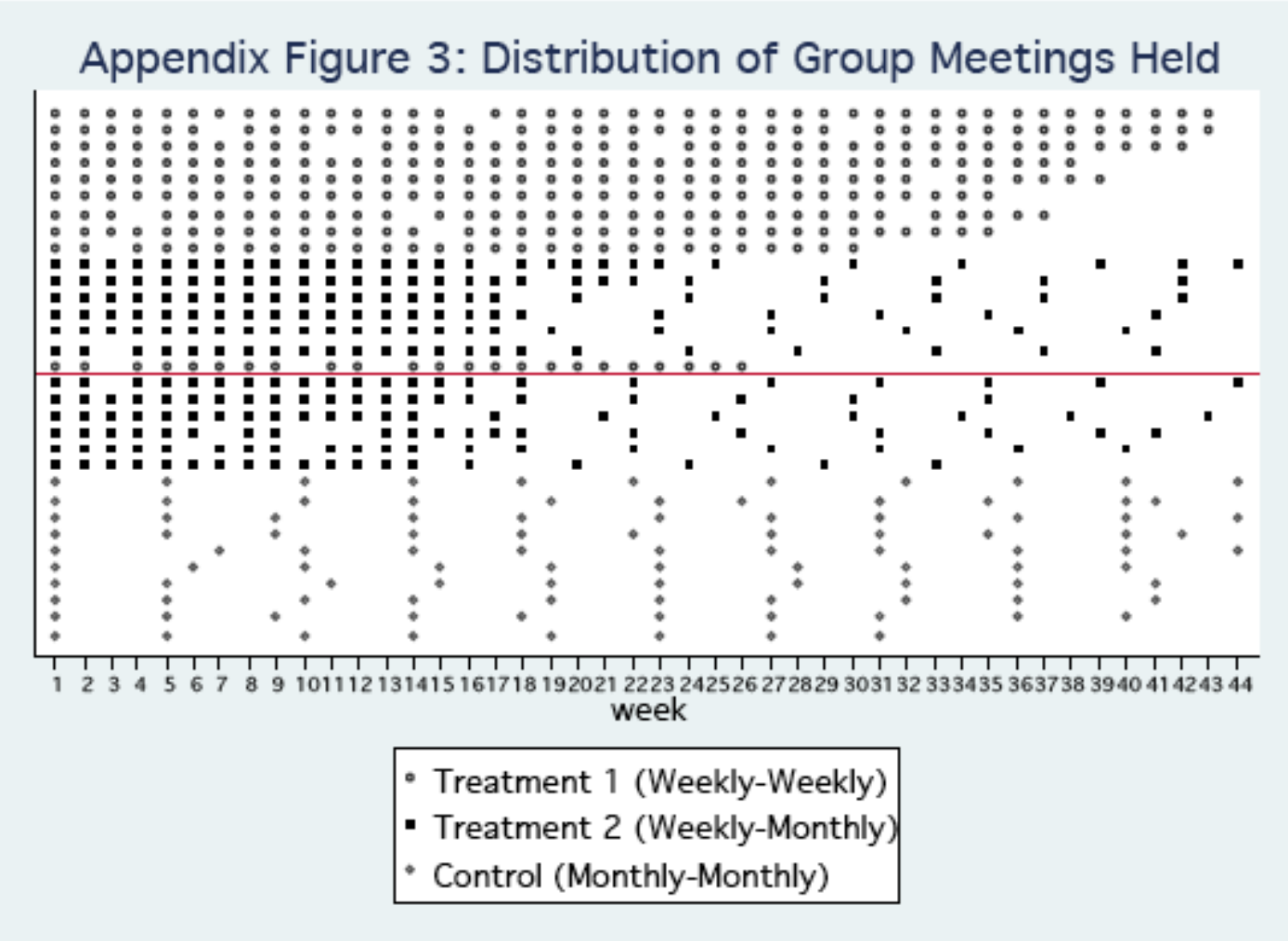
Name of Winner: _____ Signature of Claimant: _____

Signature of Winner: _____

Note:
Clients were randomly offered entry into the lottery for a Rs. 200 Voucher or four Rs. 50 Vouchers. This figure shows the final vouchers which were given to the winner of the two lotteries.



Notes:
Appendix Figure 2 shows the expected returns to the lottery based on ticket-giving decision, and extent of reciprocal behavior by ticket recipient.



Notes:

We sample one-third of groups from each experimental branch, and stratify by quartile of number of group meetings held within each branch to ensure representativeness. Groups above the horizontal line met 23 or more times over First Loan Cycle.

APPENDIX: Lottery Script

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

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

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

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

win the lottery, you will be asked to sign the 200 Rs. voucher when you receive it. However, you are still free to decide whether to keep or give away the voucher that you receive.

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