Moral Hazard and Peer Monitoring in a Laboratory Microfinance Experiment*

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Abstract

Most problems with formal sector credit lending to the poor in developing countries can be attributed to the lack of information and inadequate collateral. One common feature of successful credit mechanisms is group-lending, where the loan is advanced to an individual if he/she is a part of a group and members of the borrowing group can monitor each other. Since group members have better information about each other compared to lenders, peer monitoring is often less expensive than lender monitoring. Theoretically this leads to greater monitoring and greater rates of loan repayments. This paper reports the results from a laboratory experiment of group lending in the presence of moral hazard and (costly) peer monitoring. We compare peer monitoring treatments when credit is provided to members of the group sequentially and simultaneously, and individual lending with lender monitoring. The results depend on the relative cost of monitoring by the peer vis-à-vis the lender. In the more typical case where the cost of peer monitoring is lower than the cost of lender monitoring, our results suggest that peer monitoring results in higher loan frequencies, higher monitoring and higher repayment rates compared to lender monitoring. In the absence of monitoring cost differences, performance is mostly similar across group and individual lending schemes, although loan frequencies and monitoring rates are sometimes modestly greater with group lending. Within group lending, although the dynamic incentives provided by sequential leading generate the greatest equilibrium surplus, simultaneous group leading provides equivalent empirical performance.

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1. Introduction

There now exists a significant body of research that examines the failure of formal sector credit lending programs aimed at the poor in developing countries. Evidence of this failure is shown in the inability to reach target groups and low overall repayment rates. This failure is attributed primarily to asymmetric information (both adverse selection and moral hazard) and inadequate enforcement.[1]

The last few decades have, however, witnessed the development of innovative and highly successful mechanisms for the provision of credit to the poor. The most common of these is group-lending. Rather than the bank (or the lender) making a loan to an individual who is solely responsible for its repayment, the bank makes a loan to an individual who is a member of a group and the group is jointly liable for each member's loans. In particular, if the group as a whole is unable to repay the loan because some members default on their repayment, all members of the group are ineligible for future credit. The Grameen Bank in Bangladesh is possibly the most well known of such group lending programs. The repayment rate in this lending scheme is around 92 percent, and less than 5 percent of loan recipients were outside the target group (Morduch, 1999). The success of the Grameen Bank has led policy makers and NGO's around the world to introduce similar schemes. Around 100 million people are estimated to have participated in some form of a microfinance project (see Gine, Jakiela, Karlan and Morduch, 2005). The 2006 Nobel Prize for Peace to microfinance pioneer Muhammed Yunus has also put the success of microfinance in the world spotlight. Micro-lending is increasingly moving from non-profit towards a profit-making enterprise, with big banks such as Citigroup now backing such loans (Bellman, 2006).[2]

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[1] For example, it has been argued that the percentage of ineligible beneficiaries in the Integrated Rural Development Program (IRDP) in India, one of the largest programs of provision of formal sector credit to the poor in the world, was between 15 and 26 percent, with the highest reported being 50 percent. The repayment rate for IRDP loans was only about 40 percent for the whole country (see Pulley, 1989).

[2] While microfinance programs are most widespread in less developed countries they are by no means confined to them. Microfinance programs have been introduced in transition economies like Bosnia and Russia and even

The success of these group lending programs arises, in part, because they are better able to address the enforcement and informational problems that generally plague formal sector credit in developing countries. Group lending programs typically help solve the enforcement problem through peer monitoring. Stiglitz (1990) and Varian (1990) argue that since group members are likely to have better information compared to an outsider (the bank), peer monitoring is relatively cheaper compared to bank monitoring, leading to greater monitoring and hence greater repayment. Banerjee, Besley and Guinnane (1994) argue that explanations based on peer monitoring do a better job of explaining the success of group lending programs than other explanations. Ghatak and Guinnane (1999) develop a model of moral hazard and monitoring and find that if the social sanctions are effective enough or if monitoring costs are low enough, the joint liability provided by group lending improves repayment rates. Chowdhury (2005), by contrast, is less optimistic. He finds that in the absence of sequential financing or lender monitoring, group lending programs will typically involve under-monitoring with the borrowers investing in undesirable projects.[3]

The empirical evidence on these issues, unfortunately, is rather limited. The theoretical propositions and results are often supported by anecdotal evidence but these results have not been established as empirical regularities. In recent years researchers have called for well designed economic experiments to help examine the roles of various mechanisms that drive performance in microfinance programs (Morduch, 1999, Armendariz de Aghion and Morduch, 2005).

The aim of this paper is to understand specific aspects of group lending schemes, using controlled experimental methods. We report the results from a laboratory experiment of

in developed countries like Australia, Canada and the US. See for example Conlin (1999), Armendariz de Aghion and Morduch (2000), Armendariz de Aghion and Morduch (2005) and Fry, Mihajilo, Russell and Brooks (2006).

[3] How group lending solves the problem of adverse selection is analysed by Ghatak (2000), Van Tassel (1999), Armendariz de Aghion and Gollier (2000). The argument is based on endogenous group formation (and positive assortative matching): group lending with joint liability will result in self selection with safe borrowers clubbing together and screening out risky borrowers.

group lending in the presence of moral hazard and (costly) peer monitoring.[4] We compare treatments when credit is provided to members of the group sequentially and simultaneously, as well as treatments when loans are given to individuals and monitored by lenders. We also examine the importance of monitoring costs on credit market performance.

Our work complements the growing body of research that can broadly be characterized as field experiments in microfinance (see for example Gine, Jakiela, Karlan and Morduch, 2005, Gine and Karlan, 2006, Kono, 2006, Cassar, Crowley and Wydick, 2007). The laboratory approach that we use in this paper can address issues in different ways compared to field experiments. It is difficult to vary specific properties of institutions in controlled experiments in the field due to problems of replicability, data accessibility and comparability (see for example Bolnik, 1988 and Hulme, 2000). Furthermore some relevant variables, such as actual monitoring costs, remain unobserved. The laboratory approach on the other hand can help us control for specific parameters and observe behavior in simulated microfinance institutions. In our case it can help in isolating and clarifying the impact of different design features on repayment rates and project choice, by implementing an environment that is carefully aligned with the theoretical models relating to moral hazard and peer monitoring in microfinance programs. Of course, the laboratory approach has some drawbacks. For example, while the laboratory experiment included human subject behavior, the subjects are university students making decisions for relatively low stakes.[5] In field experiments, by contrast, participants are often the actual borrowers who are involved in microfinance programs. This advantage of field experiments comes at the cost of some loss of experimental control, however. For example, spillover effects could exist from one village to another or from the treatment group to the control group, creating more noise in the data.

[4] In this paper, we focus on informational asymmetries due to moral hazard and not due to adverse selection. In particular we restrict ourselves to exogenously formed groups (with random re-matching) and leave the issue of endogenous group formation (positive assortative matching) for future research.

[5] We do, however, employ subjects both from a developed (Australia) and a developing (India) country to measure possible subject pool effects, and find virtually none.

Laboratory experiments that examine the impact of specific design features on performance of microfinance models are rare. Abbink, Irlenbusch and Renner (2006) and Seddiki and Ayedi (2005) both examine the role of group selection in the context of group lending. Both experiments are designed as investment games where each group member invests in an individual risky project whose outcome is known only to the individual, and both find that self-selected groups have a greater willingness to contribute. Neither of these papers analyze the role of peer monitoring.

Our experiment examines several aspects of group lending programs. The first is the argument that sequential lending is crucial to the success of group lending schemes. The Grameen Bank, for example, adopts this kind of a lending policy: groups have five members each and loans are initially given to two randomly chosen members, to be repaid in regular installments over a period of one year. If they pay their initial installments, then two more borrowers in the group receive the loan and so on. Ray (1998) argues that this kind of sequential lending minimizes the contagion effect associated with individual default. Sequential lending can also minimize the potential of coordination failure. Chowdhury (2005) and Aniket (2006) argue that in a simultaneous group lending scheme with joint liability and costly monitoring, peer monitoring by borrowers alone is insufficient and that sequential lending that incorporates dynamic incentives is required.[6] Our experiment examines the empirical validity of these predictions by comparing the performance of sequential lending and simultaneous lending in the presence of moral hazard and costly peer monitoring.

The second issue is whether peer monitoring indeed does better than active lender monitoring. The bank or the lender in general is an outsider who often has less information about the borrowers. Borrowers usually live near each other and are more likely to have closer social ties. Specifically, we study whether in the presence of moral hazard, group

[6] Dynamic incentives mean that banks make future loan accessibility contingent on full repayment of the current loan to prevent strategic default.

lending with peer monitoring does better than individual lending with bank monitoring.[7] In practice peer monitoring is usually less costly than direct lender monitoring; indeed, this cost advantage is regarded as one of the main benefits of peer monitoring. Hermes and Lensink (2007), for example, argue that the higher observed repayment rates in group lending with peer monitoring compared to individual lending with lender monitoring is driven by the greater effectiveness of screening, monitoring and enforcement within the group due to the closer geographical proximity and close social ties between the group members, which translate to lower monitoring costs in the case of group lending with peer monitoring compared to individual lending with lender monitoring. Nevertheless we also compare credit market performance when direct lender monitoring and peer monitoring involve the same monitoring cost. This allows us to examine the relative effectiveness of group lending with peer monitoring and individual lending with lender monitoring, holding monitoring costs constant.

The third issue is the relative benefits of individual and group lending. Over the years there has been a discernible shift from group lending to individual lending in microfinance programs, and there are a number of theoretical reasons that have been advanced to explain this shift.[8] First, clients often dislike tensions caused by group lending. Second, low quality clients can free-ride off high quality clients leading to an increase in default rates. Third, group lending can be more costly for the clients as they often end up repaying the loans of their peers. Theoretically the results are mixed.[9]

[7] Peer monitoring and peer enforcement have been observed to deter free riding in several experiments relating to other social dilemma situations, such as common pool resource environments and the voluntary provision of public goods. See Fehr and Gaechter (2000), Barr (2001), Masclet, Noussair, Tucker and Villeval (2003), Walker and Halloran (2004), and Carpenter, Bowles and Gintis (2006) for experimental evidence.

[8] The terms individual and group lending as defined in this paper essentially correspond to the terms individual and group (joint) liability. We use the term group lending to describe the situation where individuals are both borrowers and simultaneously guarantors of their partners' loans.

[9] Armendariz de Aghion and Morduch (2000) and Armendariz de Aghion and Morduch (2005) argue that group lending (joint liability) is just one element in successful microfinance schemes. Chowdhury (2005) argues that mere joint liability does not work and he emphasizes the role of dynamic incentives: in his model a combination of joint liability and dynamic incentives work best in terms of project choice and repayment. Che (2002) argues

Our laboratory experiment is able to address each of these issues. Our results show that when monitoring costs are lower for peer monitoring than lender monitoring, group lending (with peer monitoring) performs better compared to individual lending (with active lender monitoring), reflected in higher loan frequencies and repayment rates. This occurs even though repayment rates with individual lending considerably exceed the theoretical prediction, which may reflect social preferences such as reciprocity. However if we hold the cost of monitoring constant across the different monitoring regimes, then repayment rates are modestly higher under individual lending (with active lender monitoring), compared to group lending (with peer monitoring). Loan frequencies and monitoring intensity are modestly greater with group lending, however. Our results therefore partially corroborate those observed in the field by Gine and Karlan (2006) and Kono (2006), who find high performance in individual lending schemes. Their explanation is based on Greif (1994), who argues that a more individualistic society requires less information among players and is thus able to grow faster. However the relative effectiveness of peer versus active lender monitoring depends on the cost of monitoring. In the field experiment conducted by Gine and Karlan (2006) the existing field centers with group liability loans were converted to individual liability loans. Lenders therefore had prior information about the borrowers' characteristics from the group lending field sessions and this could be used in the individual lending sessions at no extra cost. As a result the monitoring costs did not necessarily change as they moved from group lending to individual lending. Furthermore, participants had some experience with group lending before branching off on their own in the individual lending schemes. What this suggests is that monitoring costs in that field experiment may have been no different under individual lending (with active lender monitoring), compared to group

that joint liability schemes create problems of free-riding and worsen repayment rates, but when projects are repeated multiple times, group lending dominates individual lending. Rai and Sjostrom (2004) emphasize the importance of cross-reporting in achieving efficiency in group lending.

lending (with peer monitoring). Our laboratory experiment results are consistent with that interpretation.

The results from our experiment also show that within group lending, it matters little whether loans are made simultaneously or sequentially. Although the dynamic incentives provided by sequential lending can improve efficiency relative to simultaneous group lending, performance is equivalently high in the two group lending treatments because agents tend to play the efficient equilibrium in the simultaneous case.

While the primary aim of this paper is to analyze innovative lending schemes for developing countries, our work can be placed in a more general context. We seek to study different organizational forms for financing investments in the presence of borrower moral hazard and in situations where there are no assets to provide as collateral. Questions like this have been the focus of much of the large corporate finance literature (for example, Hart, 2001). Joint liability companies have been seen to facilitate lending as the lender could feel more protected both by the joint liability and by the incentives created for partners to monitor each others' earning potential. In some situations, however, this organizational structure might not be optimal as it increases ex-post hold up problems to the extent that either party can always threaten to veto any action and thus force the firm to a standstill. See Aghion and Bolton (1992) for a discussion of different kinds of financial contracting schemes.[10]

2. Theoretical Framework

Consider a scenario where two borrowers require one unit of capital (say $1) each for investing in a particular project. The bank, which provides this capital in the form of a loan, can either make the loan to an individual (individual lending) or it can loan to the borrowers as a group (group lending). Joint liability for the repayment of the loan exists in the case of

[10] There are alternative organizational forms like limited liability (see Basu, 1992) and more recently joint benefit schemes (see Bhattacharya, Banerjee and Mukherjee, 2008), however incorporating those issues are beyond the scope of this paper.

group lending. Borrowers can invest in two different types of projects: one project has a large verifiable income and no non-verifiable private benefit, while the other has a large non-verifiable private benefit and no verifiable income. The bank prefers the first project, where it can recoup its investment, but the borrowers prefer the second one. In the absence of monitoring, the borrowers will choose to invest in the second project and the bank, knowing this, will choose not to make the loan.

Let us briefly describe a theoretical framework, which closely follows Chowdhury (2005) and Ghatak and Guinnane (1999). Suppose that there are two borrowers: B_1 and B_2. Two projects are available to each borrower: project *S* (verifiable) and project *R* (non-verifiable). If Project *S* is chosen, the return is *H* (verifiable by monitoring) and if project *R* is chosen, then the return is *b* (not verifiable) with $b < H$. The 1 dollar cost of each project is financed by a loan from the bank (or a lender) since the borrowers do not have any funds of their own. When the two borrowers (B_1 and B_2) borrow together as a group, each borrower receives 1 dollar from the lender. The amount to be repaid is $r(>1)$ in the case of individual lending or $2r$ in the case of group lending. We assume that this r is fixed exogenously.[11]

In the case of the individual lending, if the borrower chooses project *S* the return to the bank is r; otherwise it is 0. The return to the borrower is $H - r$ if the borrower chooses project *S*, and is b if the borrower chooses project *R*. We assume that $H - r < b$ so that borrowers prefer project *R*. Banks on the other hand prefer project *S*. In the case of group lending, if both borrowers choose project *S*, the return to each borrower is $H - r$ and the return to the bank is $2r$. If both borrowers choose project *R*, the return to each borrower is b and the return to the bank is 0. Finally if one borrower chooses project *R* and the other chooses project *S*, then due to joint liability the return to the borrower choosing project *S* is 0

[11] While some authors (e.g. Ghatak, 2000) assume that the rate of interest is a choice variable for the bank, others (e.g., Besley and Coate, 1995) assume that it is exogenously determined. In this paper we follow Besley and Coate (1995) and assume that the rate of interest is exogenous. Essentially we assume that the government determines the interest rate on non-economic grounds.

while that of the borrower choosing project *R* is b and the return to the bank is H. We assume that $H \leq 2r$. In the case of group lending it is therefore in the interest of both the bank and the borrowers to ensure that the other member of the group chooses project *S*.

An informational asymmetry arises because each borrower knows the type of his own project, but the lender or the other borrower in the group (the partner) can find out the borrower's project choice only with costly monitoring. The monitoring process works as follows: Borrower i can, by spending an amount $c(m_i)$ in monitoring costs, obtain information about the project chosen by the other borrower in his group with probability $m_i \in [0,1]$. This information can be used by borrower i to ensure that the other borrower in the group chooses project *S*. The bank (lender) can also acquire this same information by spending an amount $\lambda c(m)$. We assume that $\lambda \geq 1$ in order to capture the notion that peer monitoring is less expensive than monitoring by the bank. We assume a quadratic monitoring cost function so that $c(m_i) = m_i^2/2$. Monitoring level m costs the bank $\lambda m^2/2$. If the borrower i chooses monitoring level m_i, then with probability m_i he can force the other borrower in the group to choose project *S*. [12]

Individual Lending

First consider individual lending (with bank monitoring). There are three stages to the game.

Stage 1: Bank chooses whether or not to lend $1 to the borrower.

Stage 2: Bank chooses the level of monitoring, conditional on deciding to lend.

Stage 3: Borrower chooses either project *R* or project *S*.

[12] We could think of different ways in which monitoring works in practice: information acquired by the borrowers about each other's project choice may be passed on to the lender who then uses this information to force the borrowers to choose project *S*. Alternatively, the borrowers can use some form of social sanctions or peer punishment to ensure that the other borrower chooses project *S*.

It is straightforward to solve for the sub game perfect Nash equilibrium of the game by backward induction. If the bank lends, it chooses *m* to maximize $mr - \frac{\lambda m^2}{2} - 1$, which gives $m^* = r/\lambda$. Therefore the expected return to the bank is $\frac{r^2}{2\lambda} - 1$, so the bank will provide the loan if and only if $\frac{r^2}{2\lambda} - 1 > 0$ i.e. if $r^2 > 2\lambda$. This gives rise to the first proposition:

Proposition 1: If the costs of monitoring relative to the return are sufficiently low $\left(\lambda < r^2/2\right)$, then individual lending is feasible, and the efficient (full monitoring/lending) equilibrium exists; otherwise, for monitoring costs above this threshold the unique equilibrium has no lending.

We will consider two specifications for our cost structure in the experiment. In the first we set $\lambda > r^2/2$, and call this the individual lending high cost treatment (Treatment 1). In the second we set $\lambda < r^2/2$, and call this the individual lending low cost treatment (Treatment 2).

Group Lending: Simultaneous

The sequence of events in group lending is as follows:

Stage 1: Bank chooses whether or not to lend $2 to the group. There is joint liability, so that if one borrower fails to meet his obligations, then if the other borrower has verifiable income he must pay back the bank for both borrowers.

Stage 2: The borrowers simultaneously choose the level of peer monitoring, m_i.

Stage 3: Both borrowers choose either project *R* or project *S*.

Note that here both monitoring and lending is simultaneous and we call this simultaneous group lending (Treatment 3). Again the sub game perfect Nash equilibrium is solved by backward induction. Borrower i will choose monitoring m_i to maximize

$$m_i\left[m_j\left(H-r\right)+\left(1-m_j\right)b\right]+\left(1-m_i\right)\left[m_j*0+\left(1-m_j\right)b\right]-\frac{m_i^2}{2}$$

The first order condition is: $m_j\left(H-r\right)-m_i=0$. Likewise the first order condition for borrower j is: $m_i\left(H-r\right)-m_j=0$. Clearly $m_i^*=m_j^*=0$ is a Nash equilibrium. We call this the *inefficient* (*zero-monitoring/zero-lending*) *equilibrium*. In this case there is a strategic complementarity between the monitoring levels of the two borrowers. A particular borrower knows that if the other borrower monitors and he does not, then he will end up with a payoff of 0. If however the other borrower does not monitor then he has no incentive to monitor as well. Mere joint liability and peer monitoring does not solve the moral hazard problem.

Remember however that $m\in\left[0,1\right]$. Now consider the reaction function $m_i=m_j\left(H-r\right)$ of borrower i with respect to that of borrower j. Since $H-r>1$ (the return on project *S* exceeds the amount that must be repaid), there exists a $m_j=\overline{m_j}<1$ (say) such that the best response is $m_i=1$ for $m_j\geq\overline{m_j}$. So the reaction function of borrower i with respect to that of borrower j can be written as:

$$m_i=\begin{cases}m_j\left(H-r\right)\text{ for }m_j\in\left[0,\overline{m_j}\right)\\1\text{ for }m_j\in\left(\overline{m_j},1\right]\end{cases}$$

In this case $m_i^{**}=m_j^{**}=1$ is also a Nash equilibrium. We can call this the *efficient* (*monitoring/lending*) *equilibrium*. Figure 1 presents the reaction functions for $H-r=1.75$. It is important to note that the reaction functions are upward sloping. We will return to this issue when we discuss the estimation results.

The bank's payoffs in these two monitoring game equilibria determine whether it will lend. For the inefficient $(0,0)$ case, the expected payoff to the bank is $-2<0$ and group lending is not feasible. The payoff to both borrowers in this case is 0. On the other hand, for the efficient $m_1^{**}=m_2^{**}=1$ case, the payoff to the bank is $2r>0$ and the payoff to both

borrowers is $H - r - \frac{1}{2}$. Clearly $m_1^{**} = m_2^{**} = 1$ is the payoff-dominant equilibrium. Although this also makes it a focal point equilibrium (Schelling, 1980, p. 291), previous experimental evidence indicates that this is not a sufficient condition for "behavioral" equilibrium selection (e.g., Van Huyck, Battalio and Beil, 1990).

Proposition 2: If $H - r \geq 1$ and agents coordinate on the payoff-dominant Nash equilibrium, then under a simultaneous group lending scheme lenders choose to make loans, borrowers choose a high level of monitoring and repayment rates are high leading to an efficient (monitoring/lending) equilibrium. However, an inefficient zero-monitoring equilibrium with no lending also exists.

Group Lending: Sequential

An alternative to simultaneous lending is to lend sequentially to group members with the order chosen randomly. Here initially only one (randomly chosen) member of the group receives a loan. Depending on whether this loan is repaid, the bank decides whether or not to lend to the other member of the group. This incorporates dynamic incentives, which have become increasingly popular among researchers and practitioners in microfinance. The sequence of events is as follows:

Stage 1: Bank chooses whether or not to lend \$1 to one of the members of the group. It puts the other dollar into alternative use, which yields 0.75.

Stage 2: The borrowers simultaneously choose their levels of monitoring m_i.

Stage 3: One of the borrowers is chosen at random (with probability 0.5) to receive the loan, if the bank lends. This borrower B_i decides whether to invest in *R* or *S*.

If B_i invests in project *R*, then he gets *b* and neither B_j nor the bank receives anything. The game stops here. If B_i invests in project *S* the game continues to round 2. In this case borrower B_i receives $H - r$ and the bank receives r. This amount $H - r$ is invested and gives $(H - r)\tilde{r}$. We explicitly assume that no side contracting is possible (we do not elaborate on how this kind of side-contracting is prevented) and therefore self-

financing among the borrowers is not possible. Of course if B_j is successful in her monitoring, then B_i has to invest in project *S*.

Stage 4: The game moves to round 2 only if B_i (the randomly chosen first borrower) invests in project *S* in round 1. The bank lends \$1 to B_j who invests in either project *R* or project *S* (of course if B_i was successful in her monitoring, then B_j has to invest in project *S*).

If B_j invests in project *S*, then the bank gets *r*. Total surplus is $(H-r)(1+\tilde{r})$ and this is allocated among the two borrowers. B_j gets $\alpha(H-r)(1+\tilde{r})$ and B_i gets $(1-\alpha)(H-r)(1+\tilde{r})$. If however B_j invests in project *R*, then the bank gets $H\tilde{r}$, B_j gets *b* and B_i gets 0.

Let us assume that $\alpha(H-r)(1+\tilde{r}) \le b$ and $(1-\alpha)(H-r)(1+\tilde{r}) \le b$.[13] Both borrowers then still have an incentive to choose the non-verifiable project *R* but the sequential lending increases their incentive to monitor. In this case the reaction functions for the two borrowers are symmetric and are given by

$$m_k = \frac{1}{2\tilde{r}}\left[b + m_l\left\{\alpha(H-r)(1+\tilde{r}) - b\right\}\right]$$
$$m_l = \frac{1}{2\tilde{r}}\left[b + m_k\left\{\alpha(H-r)(1+\tilde{r}) - b\right\}\right]$$

Solving out we get

$$\overline{m}_k = \frac{b}{b + 2\tilde{r} - \alpha(H-r)(1+\tilde{r})} = \overline{m}_l = \overline{m}$$

The expected payoff to the bank is

$$\frac{m_k m_l r}{\tilde{r}} - \left(\frac{m_k + m_l}{2}\right)\left(r - \frac{1}{\tilde{r}}\right)$$

[13] The parameter values chosen (see Table 1) satisfy these restrictions.

Therefore a unique and positive level of monitoring exists, irrespective of the value of α. This positive level of monitoring occurs because even if borrower B_j does not monitor, B_i has an incentive to monitor. To see this, suppose that B_j receives the loan in round 1 (remember that the order of receiving the loan is determined randomly). If B_i does not monitor, B_j will invest in project *R* and then B_i will receive a payoff of 0. By choosing a positive level of monitoring, B_i can increase the probability that B_j invests in project *S* in which case the game continues onto the second round and B_i gets the loan. Moreover given that B_i is going to monitor, B_j has an even greater incentive to monitor due to the strategic complimentarity of monitoring. So the sequential nature of the lending scheme and the simultaneous choice of the level of monitoring (before a borrower knows whether he is the first or the second borrower) lead to an *efficient* (*monitoring/lending*) *equilibrium*, as long as the equilibrium monitoring levels are sufficient to provide positive returns to the lender. Knowing this the lender will choose to make the loan. This is sequential group lending (Treatment 4).

Proposition 3: If $\alpha\left(H-r\right)\left(1+\tilde{r}\right)\le b$ and $\left(1-\alpha\right)\left(H-r\right)\left(1+\tilde{r}\right)\le b$ then under sequential group lending, a unique Nash equilibrium exists in which lenders choose to make loans, borrowers choose a high level of monitoring and repayment rates are high leading to an efficient (monitoring/lending) equilibrium. The symmetric monitoring rates in this case are given by $\overline{m}_k=\dfrac{b}{b+2\tilde{r}-\alpha\left(H-r\right)\left(1+\tilde{r}\right)}=\overline{m}_l=\overline{m}$.

Notice that for the parameter values chosen for this experiment, ($H=4, b=2.5, r=2.25, \tilde{r}=0.5, \alpha=0.5$, see Table 1), $\overline{m}=1$. To be more specific, optimally each borrower would like to choose $\overline{m}>1$, but remember that monitoring is restricted in the interval $\left[0,1\right]$. In Figure 2 we present the best response of Borrower 1 to alternative monitoring rates chosen by Borrower 2. Panel A assumes that Borrower 1 is the first borrower; Panel B assumes that Borrower 1 is the second borrower and finally Panel C

assumes that Borrower 1 is the first borrower with a 50% probability These best responses correspond to the choice of monitoring rate that maximize his pay-offs. Since monitoring decisions are made *before* each borrower knows whether he is the first or the second borrower, the relevant figure is Panel C (and they know that they will be randomly chosen to be the first or the second borrower, with probability 0.5). Notice that irrespective of the level of monitoring chosen by Borrower 2, Borrower 1 should always choose the maximum level of monitoring. Since the borrowers are symmetric, Borrower 2 should also always choose the maximum level of monitoring. As a result we end up at the efficient (monitoring/lending equilibrium: the sequential nature of the lending scheme and the simultaneous choice of the level of monitoring (before a borrower knows whether he is the first or the second borrower) leads each borrower to choose the maximum level of monitoring and knowing this the lender will choose to make the loan.

3. Experimental Design

We designed four treatments to examine the equilibrium predictions described in Propositions 1 – 3, and conducted a total of 29 sessions in Australia and India across these treatments with 12 subjects in each session. Treatments 1 and 2 were individual lending treatments, with the 12 subjects randomly divided into groups of two with each group consisting of one borrower and one lender. Treatments 3 and 4 were group lending treatments, with the 12 subjects randomly divided into groups of three with each group consisting of two borrowers and one lender. The role of each subject (as a borrower or as a lender) was determined randomly and remained the same throughout each session, which ran for 40 periods. At the end of every period participants were randomly re-matched. After reading the instructions and before the actual session began, the participants answered a set of questions relating to the instructions and they were paid in cash (at the end of the experiment

in addition to their earnings from the actual experiment) A$0.50 or 5 Rupees for each correct answer. Subjects earned payments in experimental dollars, which were converted to local currency at a fixed and announced exchange rate.

The two projects available to borrowers, *S* and *R*, each cost $1, to be financed by a loan from the lender. In the individual lending treatments, the lender chose whether or not to invest $1 into this loan. In the group lending (simultaneous and sequential) treatments, the lender chose whether or not to invest $2 into the loan ($1 to each borrower). In this case the lender could choose to make the loan to both borrowers or to neither. She could not make a loan to only one borrower in the group. If the lender chose not to make the loan, she earned $1.50 (or $0.75 in the individual lending treatment) for the period. In the group lending treatments, if the borrower received the loan, he could monitor the project choice of the other borrower in the group by choosing to pay a monitoring cost (*C*). Both borrowers could monitor each other. If borrower *X* incurred a cost *C* on monitoring, there was a chance of *M* that the other borrower *Y* would automatically be required to choose project *S*. Otherwise the other borrower could choose either project *R* or project *S*. In the sequential lending treatment, the borrowers were randomly determined to be the first or the second borrower in the group to receive the loan. In this case if the first (randomly chosen) borrower's actual project choice was *R*, then the lender's second dollar was automatically allocated to her savings account where she earned $0.75 for this dollar. All monitoring decisions were made simultaneously. The theoretical predictions and the parameter values used are summarized in Table 1 (Panel A and Panel B respectively). These parameter values were chosen to satisfy the parameter restrictions in Propositions 1 – 3 and implement a test of the theoretical model, and were not calibrated to particular field conditions. These parameters imply specific earnings of the borrowers and the lender, shown in Table 2. It is worth noting that these numbers are gross returns. For example, in Table 2, Panel A if the actual project choice for both borrowers is S,

then the net earnings of the lender is $\frac{2r-2}{2} = \frac{2*(2.25)-2}{2} = 1.25$, i.e., the lender receives a net return of \$1.25 percent on each \$1.00 invested. the return to the borrower is \$1.75. Since in the experiment we do not include explicitly the borrower's returns from other opportunities not taken, the total return is, indeed, \$1.25 (to the lender) + \$1.75 (to the borrower) = \$3.00, or a 300% return on the \$1.00 initial investment. Some of this high social return arises from the fact that the borrower's opportunity cost is artificially set to zero in the experiment. In the field, borrowers who do not receive a loan will undertake some other economic activity that would generate a positive return, which would presumably be less than the potential return from the loan.[14]

We used the strategy method to elicit decisions from the borrowers.[15] The use of this method implies that the borrowers and lenders made decisions simultaneously and borrowers made their decision before they knew whether or not they had received the loan. In the case of sequential lending, the borrowers made monitoring decisions before they knew whether they were the first or the second borrower in their group to receive a loan. They did, however, know whether they were the first or the second borrower to receive the loan at the time of making their project choice.

[14] We did not include these complications in the experiment in part because we did not attempt to calibrate parameter values to particular field conditions. More broadly, however, we are following the traditions of experimental economics in providing strong relative incentives for choosing alternative actions so that subjects can learn and respond to the incentives in the relatively short time that they are in the laboratory. For example, our research is closely related to the seminal experimental papers by Fehr, Kirchsteiger and Riedl (1993) and Berg, Dickhaut and McCabe (1995), which also have experimental designs that study moral hazard and trust. The Fehr, Kirchsteiger and Riedl (1993) paper features employers who offer wages to workers, who then must return costly effort that benefits the employer. The relationship between efforts and total returns is nonlinear, so the social returns vary with the workers' choices. The observed returns in the experiment are quite high, with average social return to the observed levels of wages and effort typically ranging between 200% and 500%. For example, at the overall average wage and effort level observed in their experiment the total social return is 63.6, while the equilibrium with standard preferences generates a social return of 13.6. Thus, subjects obtain a 368% return from their investment in trust. In the original "trust game" paper by Berg, Dickhaut and McCabe (1995), any amount invested automatically is tripled by the experimenter, generating a fixed 200-percent return. Both of these papers have been followed by hundreds of published experimental papers that have used identical or similar parameters where social returns to trust are 200% or more, just as in our experiment.

[15] The strategy method simultaneously asks all players for strategies (decisions at every information set) rather than observing each player's choices only at those information sets that arise in the course of a play of a game. This allows us to observe subjects' entire strategies, rather than just the moves that occur in the game.

In the individual lending treatments, if the lender decided to invest $1 in a period (make the loan), she could monitor the project choice of the borrower by choosing to pay a monitoring cost (*C*). Treatments 1 and 2 differ in the lender's monitoring costs. In Treatment 1 the cost of monitoring is significantly higher for the lender, relative to the case of peer monitoring, consistent with the standard view that peers can observe each other's activities much more easily than can the lender (Hermes and Lensink, 2007). In Treatment 2 the lender faces the same monitoring cost as the peer. Although this is unlikely to be the case in practice, this intermediate treatment allows us to compare group and individual lending when holding the monitoring cost constant. Lenders paid their selected monitoring costs whenever they made the loan, regardless of whether or not the monitoring was successful. If unsuccessful, the borrower could choose either project *S* or project *R*. All decisions were revealed to all members of the two- or three- person group at the end of each period.

The 348 subjects who participated in the 29 sessions were graduate and undergraduate students at Monash University and University of Melbourne, Australia and Jadavpur University, Kolkata, India. We conducted sessions in two countries to examine whether subjects in India, who are perhaps more exposed to issues relating to microfinance and who share more cultural similarities to targeted borrowers, would exhibit behavioral differences from the subjects in Australia.[16] All subjects were inexperienced in that they had not participated in a similar experiment. Compared to the Australian sample, the Indian sample had a lower proportion of females, a greater proportion of Business/Economics/Commerce majors, and a higher proportion of subjects who lived in a major metropolis when they were aged 15. The z-tree software (Fischbacher, 2007) was used to conduct the experiment. Each session lasted approximately 2 hours, including instruction time. Subjects earned AUD 25 –

[16] Following Muhammad Yunus being awarded the Nobel Peace Prize in 2006, microfinance and Grameen Bank have received considerable media attention in India and in particular in Kolkata, which has cultural and linguistic similarities to Bangladesh.

35 or its purchasing power equivalent on average.[17] The instructions (included for the simultaneous lending treatment in the appendix) used the borrowing and lending terminology employed in this description.

4. Hypotheses to be Tested

The experiments were designed to test the following theoretical hypotheses, which follow from propositions 1 – 3:

Hypothesis 1: The lending rate, the average level of monitoring and the average repayment rate are all greater in both the group lending treatments with peer monitoring (Treatments 3 and 4) compared to individual lending with high cost lender monitoring (Treatment 1).

Hypothesis 2: The lending rate, the average level of monitoring and the average repayment rate in the simultaneous group lending treatment (Treatment 3) are less than or equal to the rates in the sequential group lending treatment (Treatment 4).

Note that the weak inequalities indicated in Hypothesis 2 follow from the theoretical predictions and parameter choices, which imply that the efficient (lend/monitor) equilibrium is unique in the sequential lending treatment, but both efficient and inefficient (no loan) equilibria exist in the simultaneous lending case.

Hypothesis 3: The lending rate, the average level of monitoring and the average repayment rate in the group lending treatments (Treatments 3 and 4) are less than or equal to the rates for individual lending with low cost lender monitoring (Treatment 2).

Hypothesis 4: The lending rate, the average level of monitoring and the average repayment rate are greater with low cost lender monitoring (Treatment 2) than high cost lender monitoring (Treatment 1).

Hypothesis 3 evaluates the impact of group lending compared to individual lending with lender monitoring, holding monitoring cost constant. Hypothesis 4 examines the change in monitoring cost, holding constant the aspect of individual lending with lender monitoring.

5. Results

[17] At the time of the experiment, 4 Australian dollars were worth about 3 U.S. dollars.

We present our results in the next three subsections, with each subsection addressing a specific aspect of the program performance: lending, monitoring, and repayment (and project choice). In each case we present conservative non-parametric tests for treatment differences which require minimal statistical assumptions and are based on only one independent summary statistic value per session. We also report estimates from multivariate parametric regression models which can isolate the contribution of different factors on lender and borrower behavior.

5.1: Lending

Figure 3 presents the average proportion of lenders making loans in the different periods, by treatment. Clearly the average proportion of lenders making loans is substantially lower at every period for treatment 1 (individual lending high cost) but there is very little difference in the early periods between treatments 2 (individual lending low cost), 3 and 4 (group lending). However the lending rate in the last 5 periods is significantly lower in treatment 2 compared to the group lending treatments. These results are supported by non-parametric Mann-Whitney rank sum tests with the session average as the unit of observation (Table 3, Panel A). These non-parametric tests suggest that over time lending rates are modestly lower in individual lending compared to group lending even holding monitoring costs constant. Differences in monitoring costs across the different monitoring regimes exacerbate the differences in lending rates between individual and group lending programs, as the individual lending high cost treatment has by far the lowest lending rate.[18]

Subjects participated in the experiment for 40 periods, allowing us to examine their behavior over time more systematically using panel regressions. Table 4 presents the random effect

[18] We also conducted a "direct" test of observed behavior against the theoretical predictions in Table 1. Using the Wilcoxon matched-pairs signed-ranks test we always reject the null hypothesis that on average subjects behave consistent with theory. This is not too surprising because the theoretical predictions have a boundary value (either 0 or 1), so deviations from the predictions can only go in one direction. Behavior, however, often moves towards the predictions in the later periods. For example, in average lending rates move towards 0 percent in the case of Treatment 1 and towards 100 percent for Treatments 3 and 4 .

probit estimation of the lender's loan decisions. These panel regressions incorporate a random effects error structure, with the subject (lender) representing the random effect. The dependent variable is 1 if the lender chooses to lend. We present the results from two different specifications. Specification 1 includes a dummy for group lending, and specification 2 replaces this with separate dummies for the two group lending treatments. Both specifications include a dummy for the individual lending with low cost treatment, and the reference category is always individual lending with high cost.

The estimates for 1/*t* and (1/*t*×*INDVLOWCOST*) indicate that lending decreased over time in the two individual lending treatments, but the (1/*t*×*GROUP*) estimates indicate increased lending over time in the two group lending treatments.[19] The null hypothesis that lending rates are not different between the group lending and individual lending with low cost treatment is rejected $\left(p-value=0.0001\right)$.[20] The probability of lending in period t is significantly lower if the lender received negative earnings in period $t-1$, which provides some simple evidence of a reinforcement-type learning. The Jadavpur University dummy is not statistically significant implying that that there is no difference in the probability of lending across the two locations. Most of the demographic control variables are not statistically significant (the exceptions being the age and the gender of the subject: lending

[19] Notice from Figure 3 that the time trend appears similar for the two group lending treatments but is very different for the two individual lending treatments. The non-interacted term (1/*t*) in this case captures the effect of time on the propensity of the lender to make a loan in the individual lending treatment with high cost; the interaction term $\left(1/t\right)\times\left(GROUP\right)$ captures the differential effect of time on the propensity of the lender to make a loan in the group lending treatment and the interaction term $\left(1/t\right)\times\left(INDVLOWCOST\right)$ captures the differential effect of time on the propensity of the lender to make a loan in the individual lending treatment with low cost. To obtain the total effect of time in the group lending treatments we need to add the coefficient estimates of (1/*t*) and $\left(\left(1/t\right)\times\left(GROUP\right)\right)$ and to obtain the total effect of time in the individual lending treatment with low cost we need to add the coefficient estimates of (1/*t*) and $\left(\left(1/t\right)\times\left(INDVLOWCOST\right)\right)$. The coefficient estimates of (1/*t*) and $\left(\left(1/t\right)\times\left(GROUP\right)\right)$ are jointly significant as are the coefficient estimates of (1/*t*) and $\left(\left(1/t\right)\times\left(INDVLOWCOST\right)\right)$.

[20]Note that the relevant test here is $\left(1/t\right)\times\left(GROUP\right)=\left(1/t\right)\times\left(INDVLOWCOST\right)$ *and* $GROUP = INDVLOWCOST$; , i.e., both the intercept and the slope are different. The test statistics (distributed as $\chi^2\left(2\right)$ under the null hypothesis) are shown in the lower section of the table.

rates are higher if the lenders are female and there is an inverted u-shaped relationship between the age of the participant and the propensity to make a loan).[21] The results from Specification 2 additionally show that there are only marginally statistically significant $(p-value=0.0931)$ treatment differences between the two group lending treatments.

In summary, we find support for hypotheses 1, 2 and 4, but not for hypothesis 3, for the loan frequency comparison. Compared to individual lending with high cost lender monitoring (Treatment 1), the lending rate is higher for both the group lending treatments with peer monitoring (Treatments 3 and 4) and for individual lending with low cost lender monitoring (Treatment 2). Compared to individual lending with low cost lender monitoring (Treatment 2), however, the lending rate is significantly higher for the group lending treatments (Treatments 3 and 4). This implies that loans are more frequent with group lending than with individual lending, even holding monitoring cost constant.

5.2: Monitoring

Figure 4 presents the average level of monitoring across periods. Monitoring rates are significantly lower in the high cost treatment (Treatment 1) compared to the low cost treatments (Treatments 2, 3 and 4). Controlling for monitoring costs however, there is little difference in monitoring rates between individual and group lending. Again using a rank sum test with the session average as the unit of observation, the difference in the monitoring rate between the group lending treatments and individual lending treatment with low cost and the difference in monitoring rates between the two group lending treatments are not statistically significant (Table 3, Panel B). Monitoring rates in Treatment 2 are significantly higher compared to those in treatments 3 and 4 in the first 5 periods, but this difference appears to be transitory and monitoring rates are actually lower in the later periods (though the late period

[21] We do not present the result for these demographic variables to save space. They are available on request.

difference is not statistically significant). The average monitoring rate is, however, always significantly lower for the higher monitoring cost of Treatment 1.[22]

The monitoring decision is made by the lender in the individual treatments and by a peer borrower in the group lending treatments. For the most part therefore we analyze the level of monitoring chosen in the individual and group lending treatments separately.[23] The level of monitoring chosen is restricted in the range [0,1] and is estimated using a tobit model.

Consider first the level of monitoring chosen (by the lender) in the individual lending treatments. Table 5, Panel A, presents the random effects tobit regression results and the Hausman-Taylor estimates for error component models.[24] The treatment dummy is positive and statistically significant, consistent with Hypothesis 4, indicating that monitoring rates are significantly higher in the low monitoring cost condition. Monitoring rates fall over time in both treatments and there are significant treatment differences (the coefficient estimates of $(1/t)\times INDVLOWCOST$ and the treatment dummy are jointly statistically significant $(p-value=0.0001)$). The level of monitoring in period $t-1$ has a positive and statistically

[22] Again using the Wilcoxon matched-pairs signed-ranks test we always reject the null hypothesis that on average subjects behave in accordance with the (boundary) point predictions of theory. However behavior seems to move towards the predictions in the later periods. For example, in Figure 4, average monitoring levels for treatment 1 are moving towards 0 and for the other three treatments towards 1.

[23] The propensity to make the loan is significantly lower in the individual lending treatments (particularly in the high cost treatment), implying that the data on the level of monitoring is often not observed in the case of treatment 1. The panel in this case is therefore unbalanced: the observed number of monitoring choices varies from 2 (i.e., in only 2 of the possible 40 cases, did the lender choose to make the loan) to 37.

[24] The tobit regression results presented in column (1) fail to account for the possibility that the lagged dependent variable (lagged level of monitoring) can be correlated with the time invariant component of the error term (the unobserved individual level random effect). Ignoring this could result in biased estimates. One way of obtaining unbiased estimates would be to use instrumental variables estimation (see Hausman and Taylor, 1981). It is assumed that none of the covariates are correlated with the idiosyncratic error term. The results for the Hausman-Taylor estimation for error component models are presented in Table 5, Panel A, column (2). Qualitatively the results are very similar to the tobit regression results presented in column (1): in particular, the greater the level of monitoring in period $t-1$, the greater the level of monitoring in period t and the level of monitoring falls over time. Following the suggestion of an anonymous referee we also estimated the monitoring regressions with the previous period project chosen by the borrower (other borrower in the group if group lending), rather than lagged monitoring. The results indicate that previous period non-verifiable project choices are associated with higher monitoring rates in the current period (though in the group lending treatment the effects are not statistically significant). We do not include these additional regressions in this version of the paper, because of the additional length they would add. They are however available on request.

significant impact on the level of monitoring in period t. The Jadavpur University dummy is however positive and statistically significant.[25]

As mentioned above in the case of group lending (with peer monitoring) the payoff for an individual borrower depends both on her level of monitoring and also on the level of monitoring of her partner. Subjects could construct expectations for the level of monitoring of the other member of the group in different ways. Here we consider the following two simple alternatives:

(1) *Cournot expectations*: each subject expects the monitoring level of the other member of the group to be the same as that in the previous period (Lagged Monitoring of the Other Borrower);

(2) *Fictitious play*: each subject expects the monitoring level of the other member of the group to be the average over all the previous periods (Average Lagged Monitoring of the Other Borrower). Hence each subject is assumed to have a long memory as opposed to the Cournot expectations case where each subject has a short memory.

Table 5, Panel B, presents the random effects tobit and the Hausman-Taylor estimation for error component models for both specifications of expectation formation in the group lending treatment. We find that monitoring increased over time and is modestly higher with sequential lending. This is consistent with Hypothesis 2. The positive and significant coefficient estimate of the other borrower's lagged monitoring level (in the Cournot expectations version) or its counterpart lagged average other borrower's monitoring (in the fictitious play version) is consistent with the upwardly-sloped reaction functions of the theoretical model. Note that the coefficient estimate on a borrower's own monitoring in the previous period is also positive, and is substantially larger than the reaction to the other borrower's monitoring level. The Jadavpur University dummy is always negative but is statistically significant only in one case (Hausman-Taylor specification with fictitious play).[26]

[25] It is interesting to note that if we restrict the sample to those born in South Asia (whether residing in Australia or India), the Jadavpur University dummy is no longer statistically significant. Twenty of the 240 subjects participating in the sessions conducted in Australia were born in South Asia.

[26] Again if we restrict the sample to those born in South Asia (whether residing in Australia or India), the Jadavpur University dummy is no longer statistically significant.

Estimates for the demographic controls (not shown) indicate that females choose a significantly lower level of monitoring, as do subjects with no previous participation experience.

Table 5, Panel C compares the level of monitoring chosen in the low cost treatments (Treatments 2, 3 and 4). We present the random effects tobit and the Hausman-Taylor estimation for error component model regression results for two different specifications: in specification 1 we include a group lending treatment dummy as defined above while in specification 2 we include separate dummies for the sequential and simultaneous lending treatments and the corresponding time interaction terms. The reference category in both cases is the individual lending low cost treatment. These estimations compare across lender and peer monitoring, holding the cost of monitoring constant. Note that specification 1 indicates a significantly different (upward) time trend for group lending, but the joint test $(1/t) \times INDVLOWCOST = 0$ and the treatment dummy = 0 cannot be rejected $\left(p - value = 0.1078 \right)$ for the tobit regression. Specification 1 does indicate differences in monitoring between individual and group lending for the Hausman-Taylor model, and specification 2 shows that this is due to the greater monitoring rate in sequential lending.

In summary, we find support for hypotheses 1, 2 and 4 but not for hypothesis 3 for the monitoring rates comparison. Compared to the individual lending with high cost lender monitoring (Treatment 1), the monitoring rate is significantly higher for both the group lending treatments with peer monitoring (Treatments 3 and 4) and for individual lending with low cost lender monitoring (Treatment 2). Compared to individual lending with low cost lender monitoring (Treatment 2), the monitoring rate is a bit higher for the sequential group lending (Treatment 4). This difference, which is contrary to Hypothesis 3, is only statistically significant in the panel regressions.

5.3: Repayment Rate

The repayment rate is not a choice variable but is the result of a combination of the *ex ante* project choice by the borrower, the level of monitoring chosen by the borrower, and the success of the monitoring process. Repayment occurs if the borrower chooses project *S* or if the borrower chooses project *R* and monitoring is successful. Panel C of Table 3 shows that repayment rates, like the other performance measures, are not significantly different across the two group lending treatments. Repayment rates are significantly lower in the individual lending high cost treatment compared to all three low monitoring cost treatments. The average proportion of subjects (*ex ante*) choosing project *R* is significantly lower, however, in both the individual lending treatments compared to the group lending treatments (Panel D of Table 3).[27]

Table 6 presents random effect probit regression results for repayment (columns 1 and 2) and ex ante choice of project *R* (columns 3 and 4). The explanatory variables are the same as in Table 5 and again as before we present the results from two alternative specifications. The repayment rates (Table 6, column 1) are not significantly different in the group lending treatments compared to the individual lending low cost treatment: the joint test of equality of the two treatment dummies and $(1/t) \times GROUP = (1/t) \times INDVLOWCOST$ cannot be rejected $(p - value = 0.1028)$ indicating that over all, group lending and individual lending with low cost treatments have similar effects on repayment. Column 2 indicates that repayment is lower for simultaneous group lending than for low cost individual lending.

Recall that the earnings of the borrower are greater if he chooses project *R*, but the earnings of the lender are lower if the borrowers choose project *R*. Columns 3 and 4 indicate that the borrowers are more likely to choose project *R* in the group lending Treatments 3 and 4 than in the individual lending Treatments 1 and 2. Table 5 earlier showed that borrowers in these group lending treatments are also more likely to choose a high level of monitoring to be

[27] Again using the Wilcoxon matched-pairs signed-ranks test we always reject the null hypothesis that on average subjects behave in accordance with the (boundary) point predictions of the theory.

able to switch the other borrower's project choice to *S*. In consequence the "actual project choices" are likely to be project *S* and the earnings of the lenders are positive and outcomes move toward an efficient (monitoring/lending) equilibrium. On the other hand in Treatment 1 monitoring rates are lower and even though borrowers are more likely to choose project *S*, lenders choose not to make the loan. Outcomes frequently correspond to the inefficient (low monitoring/no lending) equilibrium. Finally comparing Treatment 3 to Treatment 2, when holding monitoring cost constant the repayment rates are significantly higher in the individual lending treatment compared to the simultaneous group lending treatment. Since monitoring rates are not different across these treatments (Table 5C), the difference is driven by the fact that borrowers are significantly more likely to (*ex ante*) choose project *R* in this group lending treatment compared to the individual lending treatment.

In summary, we find support for hypotheses 2 through 4 but only partial support for Hypothesis 1 for repayment rates. Compared to individual lending with high cost lender monitoring (Treatment 1), the repayment is significantly higher for the sequential group lending treatment (Treatment 4) and for individual lending with low cost lender monitoring (Treatment 2), but is not significantly different for the simultaneous group lending treatment (Treatment 3). Compared to the sequential group lending treatment (Treatment 4), the repayment rate is significantly lower for the simultaneous group lending treatment (Treatment 3); and compared to individual lending with low cost lender monitoring (Treatment 2), the repayment rate is significantly lower for the simultaneous group lending treatment (Treatment 3).

One possible explanation for the lower rate that borrowers chose project *R* in the individual lending treatments could be that reciprocal motivations are triggered more in a two person game (Treatments 1 and 2) than a three person game (Treatments 3 and 4). Individual lending in the experiment shares some parallels with the trust game (e.g., McCabe, Rigdon

and Smith, 2003). When the lender trusts the borrower with the loan, the borrower is more likely to choose the verifiable project. Subjects appear to be less likely to exhibit reciprocal behavior when a fellow borrower is monitoring and can also compensate the lender for any bad outcomes. In other words, it is possible that the group lending environment reduced the borrower's feeling of responsibility to be reciprocal. [28]

6. Implication of our Results and some Concluding Comments

In this paper we use laboratory experiments to examine group lending in the presence of moral hazard and costly lender or peer monitoring. We compare treatments when credit is provided to members of the same group sequentially and simultaneously, and when loans are given to individuals and monitored by lenders. The results depend on the relative cost of monitoring by the peer vis-à-vis the outside lender. If (as is generally assumed), the cost of peer monitoring is lower than the cost of outside lender monitoring, then our results suggest that in the presence of moral hazard, peer monitoring results in higher loan frequencies, higher monitoring and higher repayment rates compared to bank monitoring. This occurs even though repayment rates with individual lending considerably exceed the theoretical prediction, which may reflect social preferences such as reciprocity. However in the absence of cost differences, individual lending with lender monitoring performs similar to group lending and monitoring. Loan frequencies and monitoring rates are sometimes modestly greater with group lending, however, even for equivalent monitoring costs.

Over the years there has been a discernible shift from group lending to individual lending in microfinance programs and there are a number of theoretical reasons that have been advanced to explain this shift. Theoretically the results are mixed. Our results suggest

[28] We examined the robustness of our results by including a dummy for whether or not the lender provided a loan in the previous period. When we included this variable in the regression (instead of the lagged project choice), we find that surprisingly the probability of choosing a non-verifiable project is significantly higher if the borrower received a loan in the previous period. The probability of repayment is, however, not affected by whether or not the borrower received a loan in the previous period. These results are available on request.

that monitoring costs could help in determining performance. In one of our treatments we assume similar costs for peer and lender monitoring. As discussed above and discussed in detail in Hermes and Lensink (2007), equivalent cost structures across the two monitoring regimes are unlikely in practice. Screening, monitoring and enforcement are more effective within the group due to the closer geographical proximity and close social ties between the group members, which translates into lower monitoring costs in the case of group lending with peer monitoring compared to individual lending with lender monitoring. If one could design contracts that reduced the cost of monitoring by the outside lender then there would be much less reason to choose group lending over individual lending.[29] In the absence of such a contract, monitoring costs are higher under individual lending with lender monitoring. If this is the case, our results support the conclusion that group lending (with peer monitoring) remains the preferred means of credit provision.

While our experiment is able to shed light on several important issues relating to the design of microcredit programs, but it is necessarily restrictive. For example, we do not consider the effect of group size. Group size may be of considerable importance in the simultaneous group lending model, where the inefficient (zero-monitoring/zero-lending) equilibrium can arise because of free riding on the part of the two borrowers in the group. We do not find evidence of free riding in the two person groups that we consider, as play converges toward the payoff dominant efficient (monitoring/lending) equilibrium. Borrowers might free ride if they are a part of a larger group, especially given that there is no explicit punishment. In this framework with mutual (peer) monitoring, however, as the size of the

[29] For example in the field experiment conducted by Gine and Karlan (2006) in the Philippines, the existing field centres with group liability loans were converted to individual liability loans; lenders therefore had prior information about the borrowers' characteristics from the group lending field sessions and this could be used in the individual lending sessions at no extra cost. As a result the monitoring costs did not necessarily change as they moved from group lending to individual lending. Alternatively, when a formal financial institution engages a member of the community to act as an intermediary/agent for screening and monitoring, since this local member has more information about the potential borrowers compared to the outside lender, the monitoring costs associated with this kind of an arrangement is no higher than that under peer monitoring (see for example Fuentes, 1996). Of course, in this case the gain from reduced monitoring cost has to be weighed against the cost of the payment made to this external agent.

group increases so do the number of people who monitor. If most people monitor, then the likelihood of being caught free riding is also higher and so is the opportunity to sanction. For example Carpenter (2007) finds that the extent of free riding with potential sanctions is no greater in larger groups, because the extent of monitoring and punishment is not negatively correlated with the size of the group. This explains the existence of large productive teams, particularly if there is transparency in the production process. Abbink, Irlenbusch and Renner (2006) found that the performance of experimental microcredit groups is robust to group size. While the larger groups do have a slightly higher tendency towards free riding, the superior dispersion of risk in the larger groups makes them perform as well as the smaller groups in that experiment. It is to be noted that these results are specific to the design and the chosen parameter constellation.

We have also not considered the possibility of group lending with active bank monitoring in this paper. As a purely theoretical construct, consider the following sequence of events (see Chowdhury, 2005). First the bank decides whether or not to lend \$2 to the group, which is divided equally among the two borrowers. Second, the bank decides on its level of monitoring. Let M_i denote the level at which the bank monitors the i^{th} borrower. Then with probability M_i the bank gets to know the identity of the i^{th} borrower's project and it passes on this information to both borrowers. Third, the borrowers simultaneously decide on their monitoring levels m_1 and m_2. Finally, both borrowers invest \$1 into one of the two projects. If there is no joint liability, borrowers have no incentive to invest in peer monitoring. However if there is joint liability, then the equilibrium involves a positive level of monitoring by the bank and this has a pump-priming effect: by undertaking relatively costly monitoring itself, the bank induces more efficient monitoring by the borrowers themselves. It is difficult however to justify this kind of a lending-monitoring scheme from the point of view of resource use. A key advantage of group lending with peer monitoring is

that banks are able to transfer the cost of monitoring to the group members who are likely to do it relatively cheaply (at least compared to the bank).

Much of the success of microcredit programs has been attributed to self-selected groups and social ties in rural communities. However successful application of these programs in other scenarios and economies requires more than strong social ties. In urban contexts of developing and transitional economies, for example, it might be more difficult to form self-selected borrowing groups compared to the more closely knit rural communities. For this reason several authors and policy makers suggest that optimal design of microcredit programs look beyond the issue of self-selection and even look beyond group lending. This experiment focuses on informational asymmetries due to moral hazard and restricts attention to exogenously formed groups. The results show that in the presence of moral hazard group lending performs better compared to individual lending, even with no self-selection in group formation. Introducing dynamic incentives (within group lending) helps, but only modestly and only for repayment rates. Overall, performance differences are minor between simultaneous and sequential lending. What is important is peer monitoring, which works much better than active lender monitoring.[30] Optimal design of microcredit programs needs to take advantage of the fact that it is less costly for group members to monitor each other, which can result in better project choices and higher repayment rates.

[30] It has been observed that in the absence of peer monitoring the success of such programs is quite limited. See Bhatt and Tang (2002) for evidence using data from microcredit programs in the US.

Table 1: Theoretical Predictions and Parameter Values in the Different Treatments

Panel A: Theoretical Predictions for Chosen Parameters

Criterion	**Treatment 1 (Individual Lending High Cost)**	**Treatment 2 (Individual Lending Low Cost)**	**Treatment 3 (Simultaneous Group Lending) inefficient equilibrium/efficient equilibrium**	**Treatment 4 (Sequential Group Lending)**
Make Loan	No	Yes	No/Yes	Yes
Monitoring Rate	0	1	0/1	1
(Exante) Project Choice	R	R	R/R	R

Panel B: Parameter Values

Parameter	**Treatment 1 (Individual Lending High Cost)**	**Treatment 2 (Individual Lending Low Cost)**	**Treatment 3 (Simultaneous Group Lending)**	**Treatment 4 (Sequential Group Lending)**
H	4	4	4	4
b	2.5	2.5	2.5	2.5
r	2.25	2.25	2.25	2.25
λ	4.5	1	1	1
$\tilde{r}$	-	-	-	0.5
α	-	-	-	0.5

Table 2: **Earnings of Borrowers and Lenders**

Panel A: Treatment 3 (Simultaneous Group Lending)

Actual project choice of borrower 1	Actual project choice of borrower 2	Earnings of borrower 1	Earnings of borrower 2	Earnings of lender
S	S	$\$1.75 - C_1$	$\$1.75 - C_2$	\$2.50
S	R	$\$0.00 - C_1$	$\$2.50 - C_2$	\$2.00
R	S	$\$2.50 - C_1$	$\$0.00 - C_2$	\$2.00
R	R	$\$2.50 - C_1$	$\$2.50 - C_2$	-\$2.00
No loan is provided		\$0.00	\$0.00	\$1.50

Panel B: Treatment 4 (Sequential Group Lending)

Actual project choice of the first borrower	Actual project choice of the second borrower	Earnings of first borrower	Earnings of second borrower	Earnings of lender
S	S	$\$1.75 - C_1$	$\$1.75 - C_2$	\$2.50
S	R	$\$0.00 - C_1$	$\$2.50 - C_2$	\$2.00
R	S	$\$2.50 - C_1$	$\$0.00 - C_2$	-\$0.25
R	R	$\$2.50 - C_1$	$\$0.00 - C_2$	-\$0.25
No loan	is provided	\$0.00	\$0.00	\$1.50

Note: C_1 and C_2 denote the monitoring costs incurred by borrower 1 and 2, and this cost depends on monitoring $m \in [0,1]$ and is given by $c(m) = \frac{m^2}{2}$.

Panel C: Treatments 1 and 2 (Individual Lending)

Actual project choice of borrower	Earnings of borrower	Earnings of lender
S	\$1.75	\$1.25 – C
R	\$2.50	–\$1.00 – C
No loan is provided	\$0.00	\$0.75

Note: C denotes the monitoring cost incurred by the lender, and this cost depends on monitoring $m \in [0,1]$ and is given by $c(m) = \frac{\lambda m^2}{2}$ .. ; $\lambda = 4.5$ in the high cost monitoring Treatment 1 and $\lambda = 1$ in the low cost monitoring Treatment 2

Table 3: Selected Descriptive Statistics
Panel A. Average Proportion Making Loans

	Full Sample	First 5 periods	Last 5 periods
Individual Lending High Cost Treatment (Treatment 1)	0.4738	0.5875	0.3026
Individual Lending Low Cost Treatment (Treatment 2)	0.6850	0.8000	0.6200
Simultaneous Lending Treatment (Treatment 3)	0.8115	0.7563	0.8013
Sequential Lending Treatment (Treatment 4)	0.7369	0.7000	0.7885
Group Lending Treatments (Treatments 3 and 4)	0.7747	0.7281	0.7949
Rank sum Test			
Individual Lending High Cost (T1) = Individual Lending Low Cost (T2)	-2.342**	-2.432**	-2.580***
Individual Lending Low Cost (T2) = Group Lending (T3 & T4)	-1.405	0.705	-1.910*
Individual Lending High Cost (T1) = Group Lending (T3& T4)	-3.124***	-1.965**	-3.381***
Simultaneous Lending (T3) = Sequential Lending (T4)	0.684	0.582	-0.318

Panel B. Average Level of Monitoring

	Full Sample	First 5 periods	Last 5 periods
Individual Lending High Cost Treatment (Treatment 1)	0.3425	0.4234	0.2681
Individual Lending Low Cost Treatment (Treatment 2)	0.5881	0.6292	0.6140
Simultaneous Lending Treatment (Treatment 3)	0.5750	0.5281	0.6628
Sequential Lending Treatment (Treatment 4)	0.6430	0.4996	0.7093
Group Lending Treatments (Treatments 3 and 4)	0.6069	0.5144	0.6859
Rank sum Test			
Individual Lending High Cost (T1) = Individual Lending Low Cost (T2)	-2.928***	-2.928***	-2.928***
Individual Lending Low Cost (T2) = Group Lending (T3 & T4)	-0.330	2.064**	-1.404
Individual Lending High Cost (T1) = Group Lending (T3& T4)	-3.613***	-1.408	-3.735***
Simultaneous Lending (T3) = Sequential Lending (T4)	-0.840	0.0000	-1.105

Panel C. Average Repayment Rates

	Full Sample	First 5 periods	Last 5 periods
Individual Lending High Cost Treatment (Treatment 1)	0.5828	0.6241	0.4203
Individual Lending Low Cost Treatment (Treatment 2)	0.7259	0.6917	0.7097
Simultaneous Lending Treatment (Treatment 3)	0.6535	0.6322	0.7200
Sequential Lending Treatment (Treatment 4)	0.6890	0.6429	0.7520
Group Lending Treatments (Treatments 3 and 4)	0.6712	0.6373	0.7359
Rank sum Test			
Individual Lending High Cost (T1) = Individual Lending Low Cost (T2)	-2.928***	-1.761*	-2.650***
Individual Lending Low Cost (T2) = Group Lending (T3 & T4)	1.156	1.404	-0.911
Individual Lending High Cost (T1) = Group Lending (T3& T4)	-2.481**	-0.092	-3.402***
Simultaneous Lending (T3) = Sequential Lending (T4)	-0.525	-0.420	0.211

Panel D. Average Proportion Choosing the Non-Verifiable Project R

	Full Sample	First 5 periods	Last 5 periods
Individual Lending High Cost Treatment (Treatment 1)	0.6289	0.6667	0.6623
Individual Lending Low Cost Treatment (Treatment 2)	0.7008	0.8067	0.6933
Simultaneous Lending Treatment (Treatment 3)	0.7979	0.7219	0.8718
Sequential Lending Treatment (Treatment 4)	0.7951	0.6906	0.8462
Group Lending Treatments (Treatments 3 and 4)	0.7965	0.7063	0.8590
Rank sum Test			
Individual Lending High Cost (T1) = Individual Lending Low Cost (T2)	-1.171	-1.848*	-0.220
Individual Lending Low Cost (T2) = Group Lending (T3 & T4)	-2.065**	1.865*	-1.987**
Individual Lending High Cost (T1) = Group Lending (T3& T4)	-3.185***	-0.675	-2.670***
Simultaneous Lending (T3) = Sequential Lending (T4)	0.053	0.053	0.792

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Random Effect Probit Regressions for Making Loans

	Specification 1	Specification 2
1/t	1.9116***	1.9102***
	(0.3472)	(0.3472)
1/t × GROUP	-2.4356***	
	(0.4914)	
1/t × INDVLOWCOST	0.2450	0.2445
	(0.6218)	(0.6216)
1/t × GROUP_SIMUL		-1.6209**
		(0.6809)
1/t × GROUP_SEQUEN		-2.9393***
		(0.5639)
Group Lending Treatment (Dummy)	1.1858***	
	(0.1829)	
Simultaneous Lending Treatment (Dummy)		1.2652***
		(0.2216)
Sequential Lending Treatment (Dummy)		1.0857***
		(0.2174)
Individual Lending Low Cost Treatment (Dummy)	0.8338***	0.8261***
	(0.2564)	(0.2554)
Negative Earnings in Previous Period (Dummy)	-0.3583***	-0.3551***
	(0.0525)	(0.0526)
Session at Jadavpur University (Dummy)	-0.2655	-0.2612
	(0.1835)	(0.1829)
Constant	5.3617**	5.3303**
	(2.3990)	(2.3886)
Observations	5282	5282
Number of groups (session subject)	138	138
σ_u	0.7928***	0.7889***
	(0.0585)	(0.0583)
ρ	0.3859***	0.3836***
	(0.0350)	(0.0349)
LR Test for $\rho = 0$	922.03***	908.99***
Treatment Effects (Joint Significance)		
Group Lending = Individual Lending Low Cost	18.78***	
Sequential Lending = Simultaneous Lending		4.79*
Sequential Lending = Individual Lending Low Cost		21.83***
Simultaneous Lending = Individual Lending Low Cost		6.65**

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

ρ denotes the fraction of total variance due to the time invariant component of the error term.

Regressions control for a set of demographic characteristics: proportion of correct answers in quiz, age and square of age of subject, gender of subject, whether subject is Business/Economics/Commerce major, location of residence when aged 15, year at university and previous experience in terms of participation in economic experiments.

Treatment effects (Test for joint significance of the dummy and the interaction term with (1/t)) is $\chi^2(2)$.

Table 5: Level of Monitoring Chosen.
Panel A: Individual Lending (Lender Monitoring)

	Random Effect Tobit Regression	**Hausman-Taylor Estimation for Error Component Models**
1/t	0.1871**	0.1709**
	(0.0830)	(0.0763)
1/t × INDVLOWCOST	-0.1318	-0.1277
	(0.1155)	(0.1056)
Individual Lending Low Cost Treatment (Dummy)	0.1659***	0.1278*
	(0.0385)	(0.0657)
Lagged Monitoring	0.4410***	0.3063***
	(0.0342)	(0.0290)
Session at Jadavpur University (Dummy)	0.0853**	0.1158**
	(0.0343)	(0.0524)
Constant	0.1497	4.2384
	(0.3511)	(3.2622)
Observations	1239	1239
Number of groups (session subject)	77	77
σ_u	0.0682***	0.1180
	(0.0109)	
σ_e	0.2102***	0.1874
	(0.0048)	
ρ	0.0952	0.2838
	(0.0283)	
LR Test for $\sigma_u = 0$	32.20***	
Left censored observations	123	
Uncensored observations	1084	
Right censored observations	32	
Treatment Effect (Joint Significance)	18.71***	4.56

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%;
ρ denotes the fraction of total variance due to the time invariant component of the error term.
Regressions control for a set of demographic characteristics: proportion of correct answers in quiz, age and square of age of subject, gender of subject, whether subject is Business/Economics/Commerce major, location of residence when aged 15, year at university and previous experience in terms of participation in economic experiments.
Treatment effects (Test for joint significance of the dummy and the interaction term with (1/t)) is $\chi^2(2)$.

Table 5 (continued): Level of Monitoring Chosen.
Panel B: Group Lending (Peer Monitoring)

	Cournot Play		Fictitious Play	
	Random Effects Tobit Regression	**Hausman-Taylor Estimation for Error Component Models**	**Random Effects Tobit Regression**	**Hausman-Taylor Estimation for Error Component Models**
1/t	0.0061	-0.0097	0.0052	-0.0074
	(0.0809)	(0.0617)	(0.0814)	(0.0622)
1/t ×GROUP_SEQUEN	-0.2913***	-0.3041***	-0.2780**	-0.2871***
	(0.1104)	(0.0847)	(0.1113)	(0.0856)
Sequential Lending Treatment (Dummy)	0.0543*	0.0579**	0.0533*	0.0597**
	(0.0292)	(0.0238)	(0.0297)	(0.0241)
Lagged Own Monitoring	0.5035***	0.3493***	0.4955***	0.3395***
	(0.0216)	(0.0158)	(0.0218)	(0.0160)
Lagged Monitoring of the Other Borrower	0.1313***	0.1038***		
	(0.0184)	(0.0140)		
Average Lagged Monitoring of the Other Borrower			0.2679***	0.2440***
			(0.0557)	(0.0433)
Session at Jadavpur University (Dummy)	-0.0494	-0.0454	-0.0512	-0.0570**
	(0.0323)	(0.0283)	(0.0329)	(0.0286)
Constant	-0.2964	-0.5616	-0.3915	-3.0257
	(0.9435)	(2.2314)	(0.9601)	(2.3108)
Observations	3530	3530	3530	3530
Number of groups (session subject)	120	120	120	120
σ_u	0.1225***	0.0998	0.1249***	0.1008
	(0.0107)		(0.0107)	
σ_e	0.3000***	0.2368	0.3011***	0.2376
	(0.0045)		(0.0045)	
ρ	0.1429***	0.1509	0.1469***	0.1525
	(0.0216)		(0.0218)	
LR Test for $\sigma_u = 0$	190.97***		202.05***	
Left censored observations	337		337	
Uncensored observations	2634		2634	
Right censored observations	559		559	
Treatment Effect (Joint Significance)	8.07**	14.82***	7.30**	13.53***

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
ρ denotes the fraction of total variance due to the time invariant component of the error term.
Regressions control for a set of demographic characteristics: proportion of correct answers in quiz, age and square of age of subject, gender of subject, whether subject is Business/Economics/Commerce major, location of residence when aged 15, year at university and previous experience in terms of participation in economic experiments.
Treatment effects (Test for joint significance of the dummy and the interaction term with (1/t)) is $\chi^2(2)$.

Table 5 (continued): Level of Monitoring Chosen.
Panel C: Comparing Peer Monitoring and Lender Monitoring with Low Cost

	Random Effects Tobit Regression		Hausman-Taylor Estimation for Error Component Models	
	Specification 1	**Specification 2**	**Specification 1**	**Specification 2**
1/t	0.0679	0.0682	0.0164	0.0247
	(0.1112)	(0.1110)	(0.0930)	(0.0904)
1/t × GROUP	-0.2596**		-0.2206**	
	(0.1233)		(0.1028)	
Group Lending Treatment (Dummy)	0.0311		0.1053*	
	(0.0423)		(0.0587)	
1/t × GROUP_SIMUL		-0.1032		-0.0710
		(0.1326)		(0.1072)
1/t × GROUP_SEQUEN		-0.4348***		-0.4102***
		(0.1351)		(0.1097)
Simultaneous Lending Treatment		0.0005		0.0464
(Dummy)		(0.0462)		(0.0604)
Sequential Lending Treatment		0.0541		0.0974*
(Dummy)		(0.0433)		(0.0552)
Lagged Own Monitoring	0.5148***	0.5098***	0.3564***	0.3533***
	(0.0197)	(0.0197)	(0.0153)	(0.0149)
Session at Jadavpur University	-0.0467	-0.0501*	-0.0089	-0.0211
(Dummy)	(0.0290)	(0.0293)	(0.0319)	(0.0314)
Constant	0.4833	0.4730	13.1844**	8.6271
	(0.6896)	(0.6894)	(5.8950)	(5.4271)
Observations	4191	4191	4191	4191
Number of groups (session subject)	150	150	150	150
σ_u	0.1212***	0.1212***	0.1195	0.1189
	(0.0094)	(0.0094)		
σ_e	0.2876***	0.2870***	0.2316	0.2311
	(0.0039)	(0.0039)		
ρ	0.1509***	0.1514***	0.2102	0.2092
	(0.0202)	(0.0204)		
LR Test for $\rho = 0$	241.25***	242.96***		
Left censored observations	392	392		
Uncensored observations	3215	3215		
Right censored observations	584	584		
Treatment Effects (Joint Significance):				
Group Lending	4.45		7.13**	
Simultaneous Group Lending		0.65		0.93
Sequential Group Lending		10.44***		15.67***
Sequential Lending = Simultaneous Lending		10.93***		17.14***

Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.
ρ denotes the fraction of total variance due to the time invariant component of the error term.
Regressions control for a set of demographic characteristics: proportion of correct answers in quiz, age and square of age of subject, gender of subject, whether subject is Business/Economics/Commerce major, location of residence when aged 15, year at university and previous experience in terms of participation in economic experiments.

Treatment effects (Test for joint significance of the dummy and the interaction term with (1/t)) is $\chi^2(2)$.

Table 6: Random Effect Probit Regressions for Repayment and Choice of Non-Verifiable Project (R)

	Repayment	Repayment	Non-Verifiable Project Choice	Non-Verifiable Project Choice
1/t	-0.0560	-0.0566	0.3716**	0.3715**
	(0.2267)	(0.2265)	(0.1885)	(0.1885)
1/t × GROUP	-0.1127		-1.3089***	
	(0.2650)		(0.2247)	
1/t × INDVLOWCOST	-0.2510	-0.2505	0.3466	0.3471
	(0.3345)	(0.3343)	(0.3303)	(0.3303)
1/t × GROUP_SIMUL		0.0202		-1.3996***
		(0.3001)		(0.2612)
1/t × GROUP_SEQUEN		-0.2422		-1.2332***
		(0.2965)		(0.2512)
Group Lending Treatment (Dummy)	0.2140**		0.8127***	
	(0.0839)		(0.1451)	
Simultaneous Lending Treatment		0.1177		0.9013***
(Dummy)		(0.0931)		(0.1689)
Sequential Lending Treatment		0.3062***		0.7363***
(Dummy)		(0.0921)		(0.1629)
Individual Lending Low Cost	0.4912***	0.5105***	0.4562*	0.4365*
Treatment (Dummy)	(0.1398)	(0.1382)	(0.2595)	(0.2600)
Session at Jadavpur University	-0.0823	-0.0989	-0.2573*	-0.2390
	(0.0785)	(0.0778)	(0.1470)	(0.1481)
Constant	-0.2677	-0.4074	1.0843	1.1770
	(1.5764)	(1.5533)	(3.0383)	(3.0354)
Observations	5330	5330	7732	7732
Number of groups (session subject)	198	198	198	198
σ_u	0.3353***	0.3269***	0.7741***	0.7727***
	(0.0290)	(0.0290)	(0.0502)	(0.0501)
ρ	0.1011	0.0966***	0.3747***	0.3738***
	(0.0157)	(0.0155)	(0.0304)	(0.0303)
LR Test for $\rho = 0$	122.86***	111.63***	1108.10***	1105.52***
Treatment Effects (Joint Significance):				
Group Lending = Individual Lending Low Cost	4.55		31.51***	
Sequential Lending = Simultaneous Lending		5.23*		1.27
Sequential Lending = Individual Lending Low Cost		2.58		24.96***
Simultaneous Lending = Individual Lending Low Cost		8.10***		29.73***

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

ρ denotes the fraction of total variance due to the time invariant component of the error term.

Regressions control for a set of demographic characteristics: proportion of correct answers in quiz, age and square of age of subject, gender of subject, whether subject is Business/Economics/Commerce major, location of residence when aged 15, year at university and previous experience in terms of participation in economic experiments.

Treatment effects (Test for joint significance of the dummy and the interaction term with (1/t)) is $\chi^2(2)$.

Figure 1: Reaction Functions in Simultaneous lending. Note that reaction functions intersect in two places (at (0, 0) and at (1, 1)), which leads to multiple equilibria.

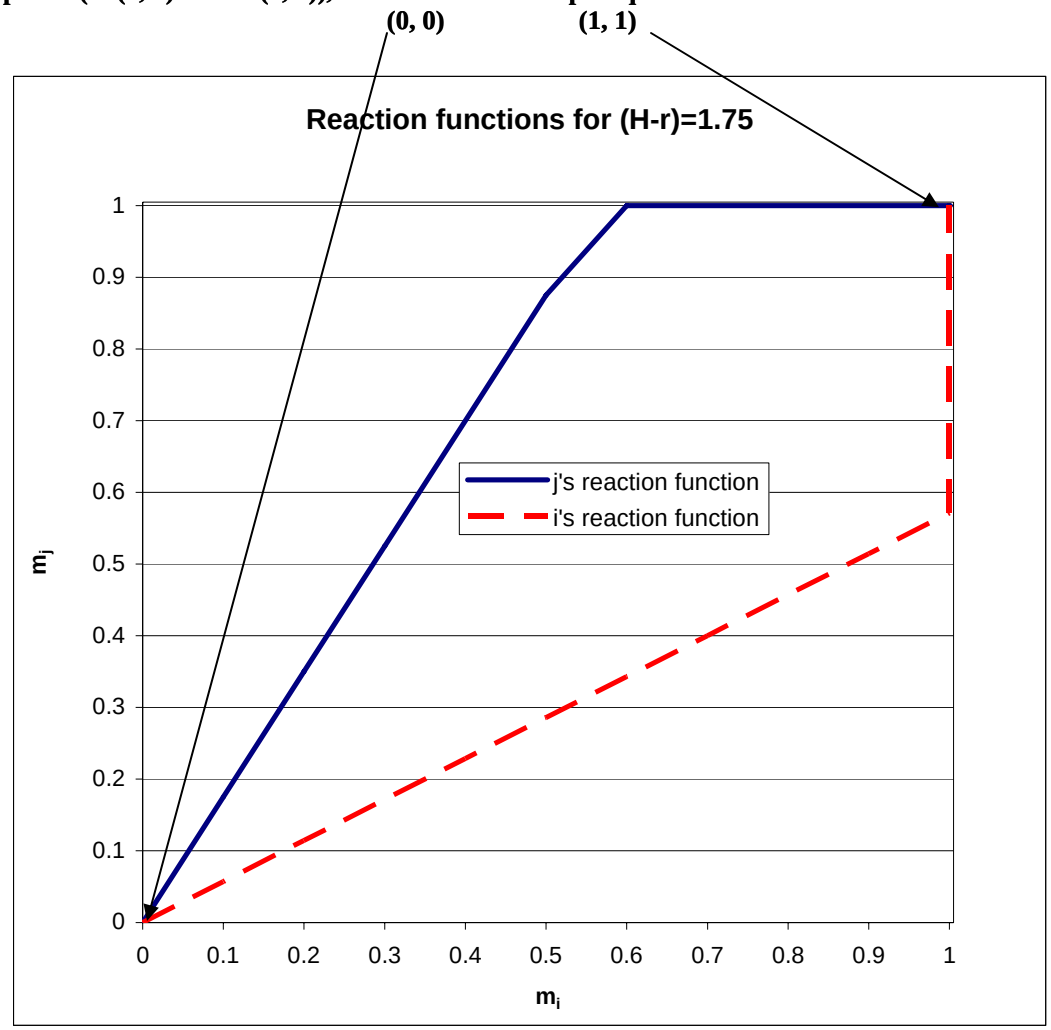


Figure 2: Best Response of Borrower 1. Sequential Lending Treatment

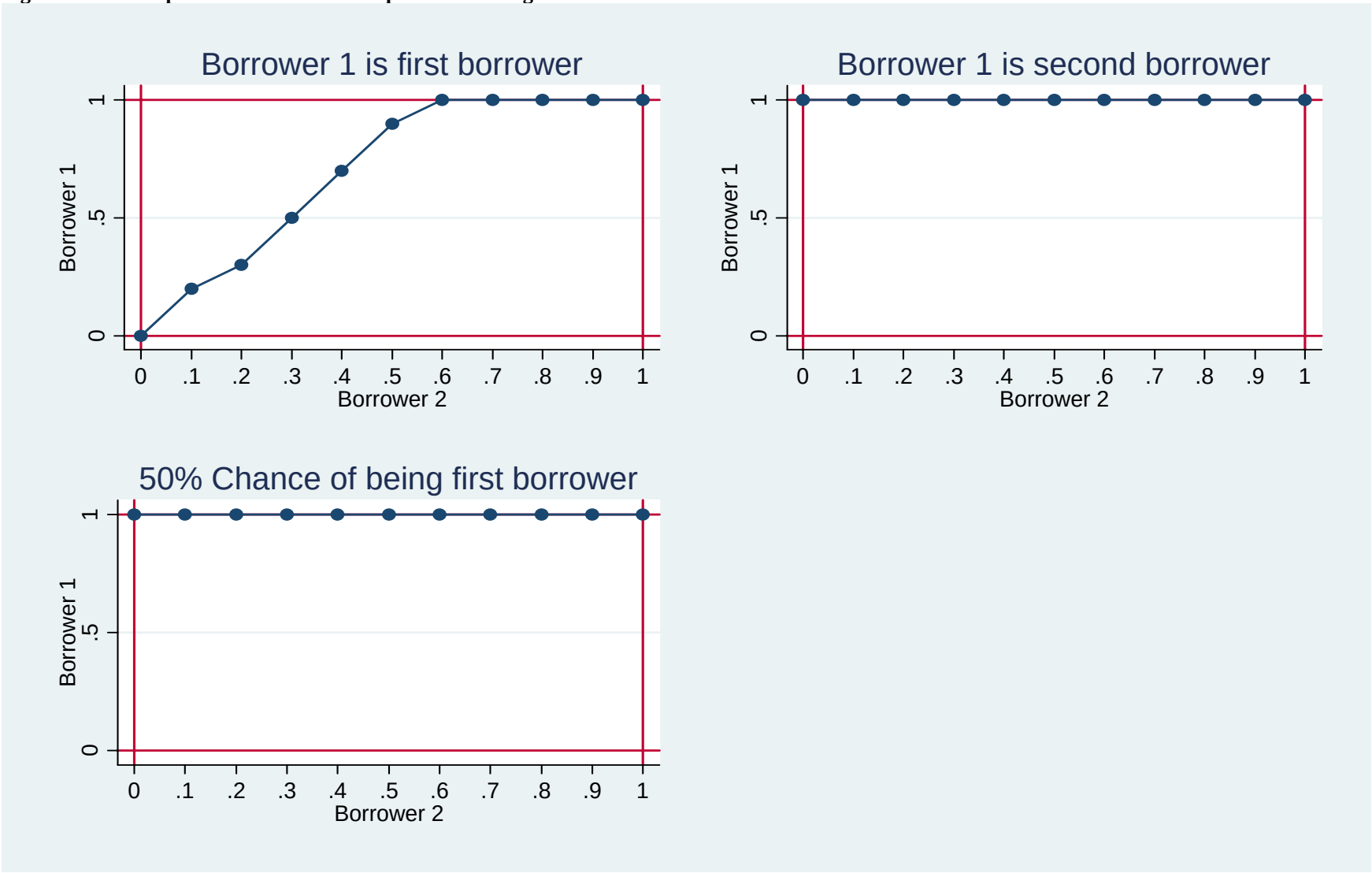


Figure 3: Average Proportion Making Loan, by treatment

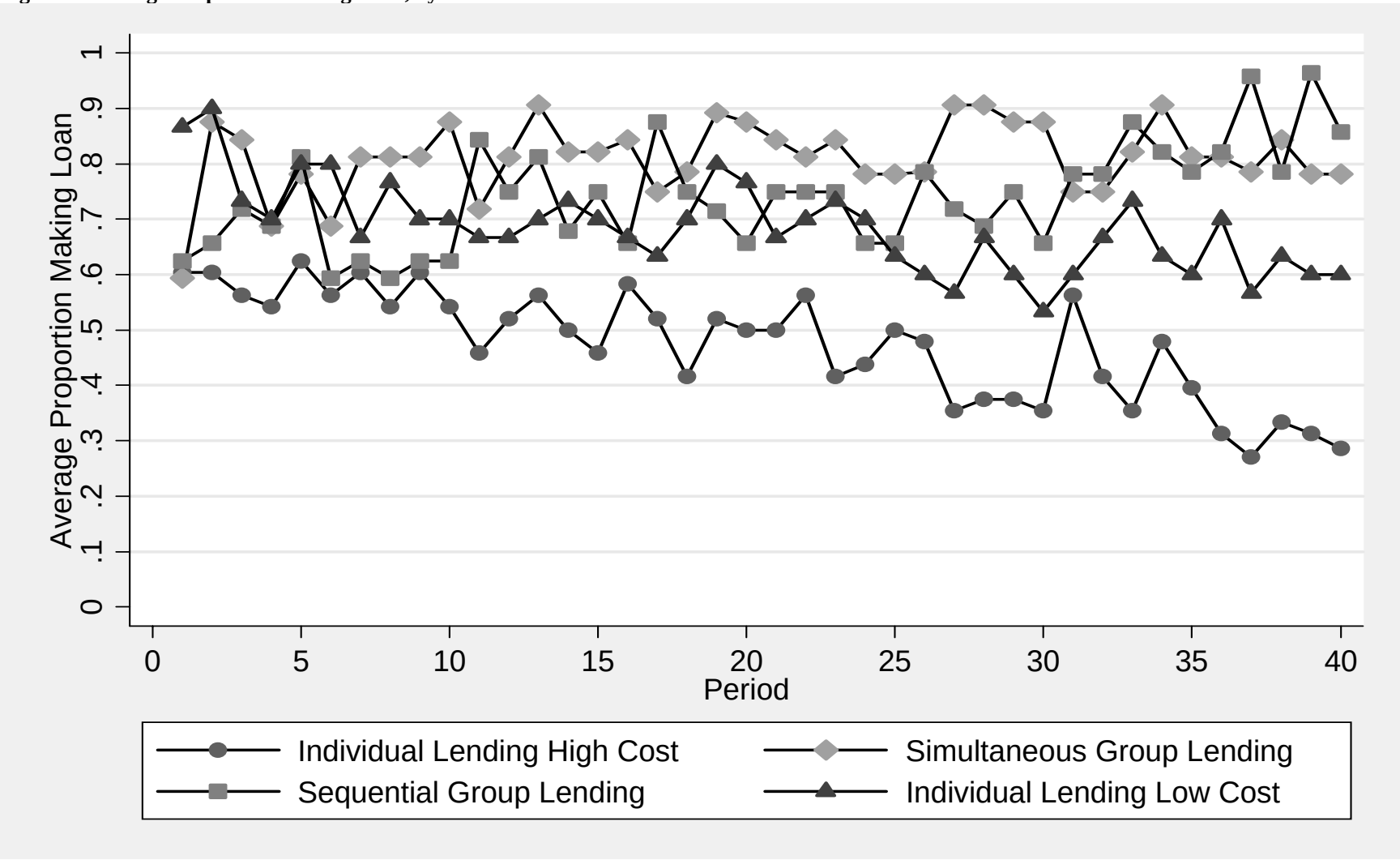
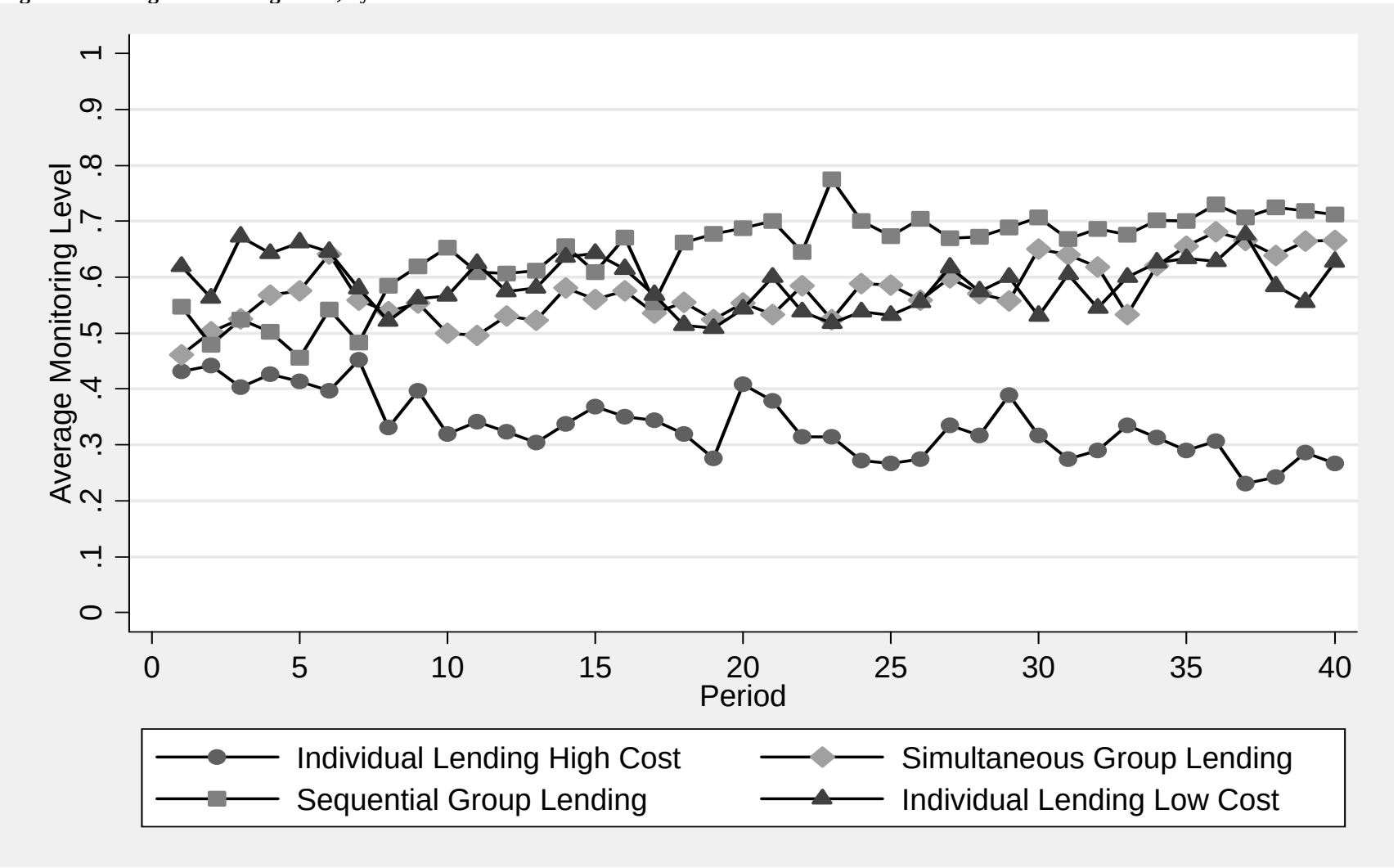


Figure 4: Average Monitoring Level, by Treatment



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