You Can Pick Your Friends, But You Need to Watch Them: Loan Screening and Enforcement in a Referrals Field Experiment

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Abstract

We implemented a natural field experiment that allows separate identification of peer screening and enforcement. Half the clients of a South African micro lender were asked to refer a friend and told they would receive a cash payment if the loan was *repaid*, the other half were offered the cash payment if the loan was *approved*. The first group has an incentive to choose wisely and enforce repayment, while the second group has neither incentive. A second randomization, after the loan was approved, removed the enforcement incentive from half of those in the first group and imposed it on half of those in the second group. We find that there is a large enforcement effect, with a small incentive of 100 rand reducing default from a base of 20% to 10%. In contrast we find no evidence that clients are better able to screen than the lender.

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1 Introduction

Economic theory assigns credit market failure a central role in explaining poverty and underdevelopment. Borrowing constraints reduce efficiency, increase inequality and, at worst, lead to poverty traps (Banerjee and Newman, 1993; Galor and Zeira, 1993). Credit rationing also appears to be empirically important. Making use of experimental or quasi-experimental supply shocks several recent papers show that there is a large demand for additional credit – for consumers (Karlan and Zinman, 2009a), microenterprises (Banerjee et al., 2009; Karlan and Zinman, 2009b) and small and medium enterprises (Banerjee and Duflo, 2004). These studies, coupled with the literature on the returns to capital (most notably De Mel et al. 2008) suggest that there may be large returns to relaxing borrowing constraints.

How then do we relax borrowing constraints? Information asymmetries provide the standard explanation for credit rationing (Stiglitz and Weiss, 1981) and mechanism design theory tells us that efficiency requires a principal to make use of all available information and enforcement mechanisms (carrots and sticks). A particularly influential school of thought suggests that a borrower's peers are a singularly useful source of both information and enforcement. For example Varian (1990) argued that peers could help a lender in variety of ways, monitoring each others actions to reduce moral hazard, providing mutual insurance, screening each other and providing assistance. He showed that a contract encouraging these actions could be efficient. This work was later extended by Stiglitz (1990), Besley and Coate (1995), Ghatak (1999) and others, particularly in the context of joint liability. The message of these papers is that if peers i) have better information than the lender regarding the repayment probability of their friends and ii) are able to use social pressure or mutual insurance to encourage loan repayment, then joint liability (and potentially other contractual forms) will be able to provide more efficient lending contracts, reducing the credit rationing problems highlighted by Stiglitz and Weiss (1981).

Contracts that make use of peer monitoring, therefore, seem to provide a natural mechanism through which to reduce credit rationing. However, despite this theoretical success, there is little empirical work testing the theory. Karlan (2007) provides evidence on peer monitoring from a group lender in Peru. The setting is such that group formation is plausibly random implying that there is no selection and Karlan can therefore identify combined monitoring and enforcement effects. He finds that groups with higher "social connection" (i.e. living closer together or having similar cultural background) have higher repayment rates, providing evidence that peers influence default. The findings, however, do not tell us whether high social connection leads to less default than individual liability – the findings are consistent with the idea that groups with low levels of social connection have repayment rates below individual liability.

In a second study, Giné and Karlan (2010) conducted a field experiment with a group lender in the Philippines. There are two parts to the experiment. First, a random selection of pre-existing groups were converted from joint liability to individual liability. Second, the lender expanded into new areas and randomly required either joint or individual liability. Analysis of repayments data shows that there is no difference in average repayment rates between group and individual liability in either part of the experiment. Again, however, this study does not address the question of whether peers are able to select or enforce. The results are consistent with two interpretations. First it may be the case that joint liability increases loan repayment for some groups but decreases it for others – implying at least some peer effect. Second, joint liability may just have no effect. The existing literature, therefore, does not provide evidence on whether peers can either monitor or select.

In this paper we aim to fill the gap in the empirical literature by providing direct evidence on whether peers i) are able to select high quality friends and ii) are able to enforce repayment. We conducted a field experiment with a South African lender who provides micro consumer loans. We worked with the lender to set up a Refer-A-Friend system which provided a bonus payment to existing clients who referred "good friends". The lender randomized the conditions on which this bonus would be received. The referrers were divided into two groups and offered different initial contracts. For the repayment group the bonus was was conditional on the friend repaying the loan and for the approval group the bonus was conditional on the friend being approved for a loan. Thus half the referrers had an ex-ante incentive to refer a high quality friend and an ex-post incentive to encourage repayment. The other half had no incentive beyond ensuring that the loan would be granted. After the referred loans had been approved we surprised the referrers, changing their incentives. Half of the repayment group were told that they would receive the bonus regardless of whether the loan was repaid, thus removing the enforcement incentive, and half of the approval group were told they could earn an additional bonus if the referred loan was repaid, thus creating an enforcement incentive. The structure of the experiment is similar to Karlan and Zinman (2007) and gives a two by two design with referrers divided into groups which received both the ex-ante and ex-post incentives, the ex-ante incentive only, the ex-post incentive only or neither incentive.

Our experimental design allows us to separately identify whether peers are able to screen their friends and whether they are able to enforce loan repayment. Identification is achieved because we are able to control for the ex-ante incentives when examining the effect of the ex-post incentive and to control for the ex-post incentive when considering the impact of the ex-ante incentive. Importantly our design implies that there are no insurance incentives and we therefore measure either social pressure, or peer monitoring.

Our results show that peers are able to encourage much higher repayment rates if incentivized by a small amount of money (100 Rands or $US 12), decreasing default from around 20% to 10% in most specifications. We however find no evidence that peers respond to the incentive to screen in a way that reveals more information than what is already held by the lender.

The remainder of the paper is structured as follows. Section 2 introduces the lender and market we study and section 3 provides details of the experiment. Section 4 outlines a formal model of the referrer's decision process highlighting the conditions under which our experiment separately identifies enforcement and selection. Section 5 provides our main results and we discuss our the interpretation of our results under different assumption in section 6. Finally, section 7 concludes.

2 Market and Lender Overview

Our cooperating lender is a new micro-lender operating in Kwazulu Natal, South Africa. During our study period (February 2008 and July 2009) the lender expanded from one branch in Pietermaritzburg to 5 branches across the state. The lender offers small, high interest, uncollateralised debt with a fixed monthly repayment rate. The market for these loans is highly competitive in Kwazulu Natal, although a recent alteration to the credit act has worked to reduce interest rates.

The borrower is required to have a job in order to qualify for a loan and must present a recent pay slip as well as the phone number of an employer. Each clients has an official ITC credit score (provided by TransUnion), and is given an internal application score. The lender determines whether a borrower qualifies for a loan, and if so what size of loan, by looking at a matrix of ITC score and application score. The loans are not tied to a specific purpose, but borrowers are asked the purpose of the loan and most report needing the money for paying school fees, attending/organizing a funeral or purchasing durables. The average loan size was around 3000R, with an average loan duration of 6 month.

3 The Experiment

The referrals program operated between February 2008 and July 2009. Each individual approved for a loan during the study period was given the chance to participate in the "Refer-A-Friend" program. Clients could participate in the program only once. Participants in the Refer-A-Friend program (referrers) were given a referral card which they could give to a friend (the referred) who needed a loan. The referee earned R40 ($US5) if she brought in the card and was approved for a loan. The referrer could earn R100 ($US13)[1] for referring the friend conditional on meeting the criteria applicable to their treatment group.

Referrers were randomly assigned to one of two treatment groups. In the *approval* group the referrer would be paid only if referee was approved for a loan. In the *repayment* group the referrer would be paid only if the referee successfully repaid the loan.[2] Figure 1 shows examples of the referral cards.

After referred clients were approved for a loan, we surprised referrers with two ex-post incentive treatments. Within the approval group, half of the referrers were assigned to an *approval + repayment* group. Referrers in this group were telephoned if their referred friend was approved for a loan and told that they would receive R100 because their referee had been approved, and that they would receive an additional R100 if the referred friend repaid the loan. The remainder of the approval group were telephoned and reminded to pick up their R100 bonus. Within the repayment group, half of the referrers were assigned to a *repayment to approval* group. Referrers in this group were telephoned if their referred friend was approved for a loan. They were told that their referred friend had been approved for a loan, and that they would be paid R100 now rather than when the loan was repaid. It was explained that they would not receive a further R100 when the loan was repaid. The remainder of the repayment group was telephoned, told that their referred friend had been approved for a loan and reminded that they would receive

[1]The bonus for the referrer was initially R60 but was changed to R100 in July 2008 at the request of the lender. The inclusion of this as a control makes no difference in any of our results.

[2]Successful repayment implied having no money owing on the date of maturity of the loan, or successfully rolling over the loan.

Figure 1: Referral Cards

a bonus if the loan was repaid.

To summarize, the randomization created four groups:

Approval: Referrers told ex-ante that they would receive a bonus if their referee was approved for a loan and no change was made ex-post. Ex-ante approval incentive, ex-post approval incentive.

Approval + Repayment: Referrers told ex-ante that they would receive a bonus if their referee was approved for a loan and ex-post were told they would receive an extra bonus if the loan was repaid. Ex-ante approval incentive, ex-post repayment incentive.

Repayment: Referrers told ex-ante that they would receive a bonus if their referee repaid a loan, and no change was made ex-post. Ex-ante repayment incentive, ex-post repayment incentive.

Repayment to Approval: Referrers told ex-ante that they would receive a bonus if their referee repaid a loan, and ex-post were told they would receive the bonus if the loan was approved. Ex-ante repayment incentive, ex-post approval incentive.

The randomization therefore creates a two by two experiment, which is shown graphically in figure 2. The intuition is that the effect of selection can be determined by comparing repayment rates across selection treatments holding constant the enforcement incentive, while the enforcement effect can be isolated by comparing repayment rates across repayment treatments holding constant the selection incentive.

Figure 2: 2×2 Experimental Design

	Selection Incentive	No Selection Incentive
Enforcement Incentive	Approval	Repayment to Approval
No Enforcement Incetive	Approval + Repayment	Repayment

4 A Model of The Referral Decision

In this section we provide a simple model of the referrer's selection and enforcement choices. Our aim is to provide a minimal set of assumptions necessary for our experiment to separate information and enforcement. We also formally state the two hypotheses we examine in the paper, and show that the model can be tested using our experimental data. We maintain the assumptions of this section throughout, but discuss the effect of relaxing the assumptions in section 6.

We model a situation in which a referrer j has N^j friends and can encourage them to repay their loans through social pressure, s. The referrer must choose one friend to referrer and an amount of effort, e, to put in to generating social pressure. We assume that N^j is a subset of $\mathbb{N}$, the set of all people who require a loan and that from the perspective of j, each friend i is described by 4 parameters: $(A_i, \theta_i, \lambda_i, \eta_i)$. These are j's *subjective* assessments of i's personal characteristics:

1. A_i is an indicator taking the value 1 if j believes that i will be approved for a loan. We denote the subset of N^j that would be approved for a loan N^j_A, with N^j_N denoting the set of friends who would not qualify for a loan.

2. θ_i is agent i's probability of repaying in the absence of social pressure ($s = 0$).

3. λ_i is the production function for social pressure. It maps from effort to social pressure, so that if j puts e units of effort into persuading i to repay a loan, then $s = \lambda_i(e)$.

4. $\eta_i \in [\underline{\eta}, \bar{\eta}]$ is a measure of the amount of altruism that j feels toward i.

We assume that the probability that agent i will repay a loan is $\theta_i + s$, that the set of friends is ranked such that $\theta_i > \theta_{i+1}$, that $\lambda_i(e)$ is strictly increasing and strictly concave in e, that $\lambda_i(0) = 0$ and that j's utility function is linear, taking the form

$$U^j = m_j + \sum_{i \in N^j} \eta_i m_i - e,$$

where m_l is the expected value of money held by agent l.

Given the linearity of effort in the utility function, social pressure could take two forms. First, it could be true social pressure, or second, e could simply be the act of passing cash to agent j conditional on repayment. Linearity then implies that agent j would be willing to pass up to the total referral bonus in order to ensure repayment. Our experiment cannot formally tell us whether the effects we see come from actual social pressure, or merely through side contracts and payments. Either result, however, implies that peer monitoring is possible and we provide some suggestive evidence in section 5 that the enforcement effect we do see is large in relation to the effect of a cash transfer directly to the borrower.

We make a number of strong assumptions on the relationship between the parameters in order to derive our results and then consider the impact of relaxing them in section 6.

Assumptions:

1. *A_i is known with probability 1. That is, there is no uncertainty in j's mind as to whether or not i will get a loan;*

2. *θ_i is independent of η_i;*

3. *$\lambda_i = \lambda$ for all i;*

4. *$(\bar{\eta} - \underline{\eta})40 < (\theta_i - \theta_{i+1})100$, for all i;*

5. *N^j is a randomly chosen subset of $\mathbb{N}$; and*

6. *$N_A^j \geq 2$ for all j.*

The first assumption ensures that a referrer j with an ex-ante *approval* incentive does not refer a friend who has a high θ_j in order to increase the probability of approval. We could relax the assumption, instead assuming that A_i (a subjective value) is not correlated with the *true* probability that i will repay. The second assumption ensures that referrers with the ex-ante *approval* incentive refer a friend chosen randomly, reflecting altruism, but not repayment probability. The third assumption rules out the possibility that those with the ex-ante *repayment* incentive will refer friends who are "malleable" but have a low baseline probability of repaying. This assumption is key to our ability to separate selection and enforcement. Assumption 4 implies that choices of referrers with an ex-ante repayment incentive will be made on the basis of probability of repayment rather than altruism. Assumption 5 implies that individual referrers do not, on average, have friends who are "similar" with respect to repayment rates. Finally, assumption 6 is necessary if we are to have any chance of seeing selection.

4.1 Individual Solution

In this section we solve the model for a specific referrer j. In the model, j first decides which friend to refer and then decides how much effort to put into social pressure. We solve the model backward in two cases, first for an agent with the ex-ante approval incentive, and then for an agent with the ex-ante repayment incentive.

An agent j with ex-ante approval incentive, believes it will never be profitable to put any effort into creating social pressure to repay.[3] Therefore the probability of repayment for each friend $i \in N^j$ will be θ_i. As it does not matter whether the friend repays the loan or not, j will choose the friend i in the set N_A^j for whom $\eta_i > \eta_l$ for all $l \neq i$ – because the friend receives a R40 bonus for taking the loan. Assumption 1 ensures that the wish to choose a friend with $A_i = 1$ does not imply choosing a high θ_i. Assumption 2 then implies that the chosen agent, i, will have a θ_i which is the same as choosing randomly from the set N_A^j. We denote θ_A^j to be the subjective repayment type of the agent chosen in the approved group.

For an agent j in the repayment group, the decision is only a little more difficult. Having chosen agent i, j will choose effort to maximize $\theta_i + \lambda_i(e) - e$. The solution implies that effort e is given by the first order condition $\lambda_i'(e_i^*) = 1$, if the solution is interior. Assumption 3 then implies that effort is the same for all friends i. Notice that we may have corner solutions. If $\lambda'(0) < 1$ then no effort is put in, and if $\lambda'(b) > 1$ then $e^* = b$. What is crucial is that e^* doe snot differ across friends. Given e^*, j will choose from the set of friends in order to maximize utility. Assumption 4 implies that j will choose the agent $i \in N_A^j$ such that $\theta_i > \theta_l$ for all $l \neq i$. We denote θ_R^j to be the subjective repayment type of the agent chosen in the repayment group.

[3] For those in the approval + repayment group it will be optimal, but they do not know that they are in this group.

4.2 Aggregate Implications

In this section we derive aggregate implications of our model for repayment rates. We assume throughout the standard assumptions that randomization leads to groups that are homogenous with respect to unobservables (Randomization) and that the randomization does not impact those in different treatment groups (SUTVA).

Let $(\hat{A}_i, \hat{\theta}_i)$ be the *true* measures of (A_i, θ_i). That is, while j has a subjective view of the likelihood of approval and the likelihood of repayment, each agent i has an actual type which accurately measures approval and repayment. Further, denote AD_g the average default rate for borrowers referred by a friend in experimental group $g \in \{a, ar, r, ra\}$ where ar is the approval $\rightarrow$ repayment group and the remaining notation should be obvious, and let the set of referrers in this group be $\{1, \ldots, M_g\}$.

We have,

$$\begin{aligned}
AD_a &= 1 - \sum_{j=1}^{M_a} \frac{\hat{\theta}_A^j}{M_a}, && (1)\\
AD_{ar} &= 1 - \sum_{j=1}^{M_{ar}} \frac{\hat{\theta}_A^j + \lambda^j(e^*)}{M_{ar}}, && (2)\\
AD_r &= 1 - \sum_{j=1}^{M_r} \frac{\hat{\theta}_R^j + \lambda^j(e^*)}{M_r}, \text{and} && (3)\\
AD_{ra} &= 1 - \sum_{j=1}^{M_{ra}} \frac{\hat{\theta}_R^j}{M_{ra}}. && (4)
\end{aligned}$$

With these measures, we aim to test three hypotheses:

Hypothesis 1: *Social Pressure Improves Repayment*

We can reject this hypothesis if $\bar{\lambda}(e) = 0 \; \forall e$, where over bars will always denote population means. Our experiment will allow us to test this, within the range of efforts induced by our 100R referrals payment. Noting that as $M_g \rightarrow \infty$,

$$\begin{aligned}
(AD_a - AD_{ar}) &\rightarrow \bar{\lambda}(e^*), \text{and} && (5)\\
(AD_{ra} - AD_r) &\rightarrow \bar{\lambda}(e^*), && (6)
\end{aligned}$$

we can use the average default rates in our experiment to test hypothesis 1. Note that this is a valid test, even in the presence of heterogenous λ^j. Further, the model imposes the restriction that these two estimates are the same. If they are not it implies that there is a breach of one of the assumptions. We will test this implication below.

Hypothesis 2: *Referrers Have More Information Than the Lender About Creditworthiness*

There are many ways in which this hypothesis could be tested, our experiment allows a particularly simple test. If hypothesis 2 is false, conditional on A_i, the referrer has no information about the repayment probability of their friends – i.e. θ is independent of $\hat{\theta}$ – then our model implies that $\bar{\bar{\theta}}^j_A = \bar{\bar{\theta}}^j_R$. That is, the friend considered to be the most likely to repay according to j is in fact, not more likely to repay than a randomly chosen friend. Once again we test this by noticing that as our sample size goes to infinity

$$(AD_{ra} - AD_a) \rightarrow \bar{\bar{\theta}}^j_A - \bar{\bar{\theta}}^j_R, \text{and} \tag{7}$$

$$(AD_r - AD_{ar}) \rightarrow \bar{\bar{\theta}}^j_A - \bar{\bar{\theta}}^j_R. \tag{8}$$

This is a test of knowledge because under the twin assumptions that θ is heterogenous in the population $\mathbb{N}$ and that N^j is chosen randomly from this population, $\bar{\theta}^j_A > \bar{\theta}^j_R$, where $\bar{\theta}$ is the mean of the subjective probability. Again, our model also implies that the estimates from these two tests should be the same, and allows for a test of the model which we discuss below.

It is important to note that because *all* referrers have the incentive to refer a friend who will be approved for a loan ($A_i = 1$), our test only tells us whether the referrer has more information than the lender. If, as we assume, the referrer knows a lot about creditworthiness (A_i), but the correlation between θ and $\hat{\theta}$ is zero, then the referrer will have no opportunity to show his knowledge of creditworthiness. We will provide some suggestive evidence below that this is in fact the case.

5 Results

5.1 Integrity of the Randomization

4453 referrals cards were handed out to clients during the study period. Table 1 shows a comparison of means of baseline characteristics of clients to whom referrals cards were given. This is the set of individuals who were randomized into the study, and if the randomization was successful, we expect there to be no difference in the characteristics across treatment groups. In general, we pass the orthogonality test. There are two instances in which treatment groups have different baseline characteristics. First, those in the approval group have slightly less education than those in the repayment group and this difference is significant at the 5% level. Second, those in the approval + repayment group are slightly more likely to receive their pay monthly than those in the repay to approval group. This difference is signicant at the 10% level. The two differences are in line with the number we would expect to find when making the 54 comparisons in table 1. Importantly, we test whether the baseline characteristics are jointly significant in predicting assignment to all four groups and find that this is never the case. These results, therefore, suggest that the randomization was successful in assigning similar referrers to each group.

Table 1: Verification of Orthogonality for Referrals Experiment
Comparison of Means of Referrers Baseline Characteristics

	Approval	Approval + Repay	Repayment	Repay to Approval	p-value for test of equality across cells
Female	0.414	0.432	0.421	0.419	0.858
	(0.015)	(0.015)	(0.015)	(0.015)	-
Age	38.044	37.394	37.715	38.002	0.468
	(0.328)	(0.331)	(0.328)	(0.320)	-
Education	0.618[1]	0.627	0.658	0.644	0.190
	(0.024)	(0.023)	(0.024)	(0.025)	-
Income	1717.726	1712.329	1744.288	1809.880	0.518
	(47.236)	(52.726)	(42.871)	(61.197)	-
Application Score	198.669	194.913	195.170	200.877	0.846
	(5.581)	(5.529)	(5.337)	(5.673)	-
Itc Score	703.911	703.730	703.314	701.367	0.683
	(1.673)	(1.668)	(1.606)	(1.662)	-
Requested Loan	5094.737	5167.098	5019.454	4816.004	0.629
	(192.290)	(222.236)	(194.603)	(184.349)	-
Requested Term	10.910	10.638	10.710	10.910	0.750
	(0.200)	(0.185)	(0.182)	(0.185)	-
Salary Monthly	0.680	0.703[2]	0.678	0.667	0.325
	(0.014)	(0.014)	(0.014)	(0.014)	-
p-value for test of joint signficance	0.7615	0.1328	0.9194	0.2125	-
N	1083	1082	1136	1106	-

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.1$. $^{1} \Rightarrow$ different from column 3 at 5%. $^{2} \Rightarrow$ different from column 4 at 10%. p-values are for F-test of whether characteristics are jointly significant in predicting assignment to treatment. Application score is an internal credit score. ITC score is official credit score. Salary monthly is a dummy variable taking value 1 if the client receives his or her salary monthly. Approval: Pre-selection approval incentive. Approval + Repayment: Pre-selection approval incentive, and post selection repayment incentive. Repayment: Pre-selection repayment incentive. Repayment to Approval: Pre-selection repayment incentive, reduced ex post to approval incentive.

The randomization into subgroups – (i.e. splitting the approval group into approval and approval + repayment, and splitting the repayment group into repayment and repayment to approval) – was hidden to the referrers and also to all staff members at the lender. Randomization therefore implies that baseline characteristics of the *referred* clients should not differ across subgroups.[4] Table 2 shows means of baseline characteristics of referred clients. The subgroups are different from each other only in relation to applications scores with the score being higher in the repayment to approval group than in the repayment group. We have reason to believe that this reflects a change in the lenders policy. The score is calculated internally by the lender and is subject to frequent change. In particular, a change to the system late in the study meant that scores could now be in the 600 to 900 range, while they had previously been below 200. There are comparatively few clients in the repayment group who came in after this change, which likely accounts for the discrepancy. If we redo the orthogonality test separately for scores above and below 200 we no longer reject equality of means in either or the two subsamples and if we do not consider the application score, then the other characteristics do not predict assignment within the repayment groups. Further, in testing hypothesis 1 if anything, the difference in applications scores suggest that those in the repayment to approval group were subject to more strenuous approval requirements which will bias our finding against hypothesis 1 - that social pressure is effective.

5.2 Summary Statistics

5.2.1 Default

To test our hypoetheses we make use of three different metrics for repayment. First we use an indicator which takes on value 1 if the client has had any days in which a payment was overdue within the first 100 days of the loan term. Second, we use a measure of how many days in the first 100 the loan was in default. We use the 100 days times frame because some loans have not yet reached maturity. Third, we use an indicator which takes on value 1 if the client fully repaid the loan, or had their loan rolled over, before the maturity date.[5] We have a smaller sample when using this metric. The first three rows of Table 3 shows summary statistics of the outcomes across treatment groups. The tables shows our key findings. Comparing column 1 to 2 and 3 to 4 shows that there is a substantially higher default rate among those who were not given the repayment incentive ex-post. Conditional on having the approval incentive ex-ante columns 1 and 2 show that those with the ex-post repayment incentive are roughly 10% less likely to be in default according to both binary measures and were in default for 6 fewer days. Similarly, conditional on the repayment incentive ex-ante columns 3 and 4 show that those with the repayment incentive ex-post had a lower probability of being in default under both binary measures - around 8.5% when considering whether the loan was repaid on time and a larger 20% when considering incidence of default in the first 100 days. Further, the

[4]Comparison across the main groups, however, are endogenous. That is, we cannot compare characteristics of those in the approved groups to those in the repayment groups as part of the experiment aims to generate difference in these characteristics.

[5]The terms of the referrals bonus required that the loan be repaid before maturity or that it be rolled over.

Table 2: Verification of Orthogonality for Referrals Experiment
Comparison of Means of Referred Client Characteristics Across Surprise Treatments

	Approval	Approval + Repayment	p-value for test of equality	Repayment	Repayment to Approval	p-value for test of equality
Female	0.443	0.450	0.922	0.530	0.464	0.337
	(0.051)	(0.046)	-	(0.047)	(0.051)	-
Age	35.597	35.183	0.748	37.183	35.474	0.228
	(0.948)	(0.870)	-	(0.957)	1.029	-
Education	0.588	0.558	0.666	0.560	0.619	0.393
	(0.050)	(0.046)	-	(0.046)	(0.050)	-
Income	1149.776	1076.789	0.739	1078.125	1092.634	0.936
	(147.203)	(157.231)	-	(134.377)	(123.618)	-
Application Score	177.618	196.617	0.432	147.704	209.361	0.003***
	(15.936)	(17.474)	-	(9.669)	(20.688)	-
Itc Score (conditional on having one)	679.756	683.010	0.734	685.000	686.319	0.885
	(6.773)	(6.612)	-	(6.454)	(6.254)	-
Client Has No ITC	0.196	0.200	0.940	0.258	0.258	0.988
	(0.041)	(0.037)	-	(0.041)	(0.045)	
Requested Loan	3968.041	4812.500	0.2315	3746.087	4149.485	0.460
	422.612	532.548	-	319.251	457.855	-
Requested Term	10.113	9.808	0.535	9.609	9.722	0.834
	(0.389)	(0.309)	-	(0.375)	(0.383)	-
Salary Monthly	0.680	0.583	0.143	0.690	0.608	0.216
	(0.048)	(0.045)	-	(0.043)	(0.050)	-
N (& p-value for test of joint signficance)	97	120	0.446	116	97	0.116
Referrals Cards Returned	0.089	0.110	0.099*	0.100	0.087	0.285
	(0.009)	(0.009)	-	(0.009)	(0.008)	-
N	1091	1090	1153	1153	1119	-
Loan Approved	0.598	0.517	0.233	0.569	0.557	0.858
	(0.050)	(0.046)	-	(0.046)	(0.051)	-
Application Score (Conditional on < 200)	133.787	135.374	0.506	137.393	133.893	0.157
	(1.996)	(1.404)		(1.499)	(2.022)	
Application Score (Conditional on > 200)	665.250	700.692	0.720	532.667	697.000	0.192
	(68.201)	(63.734)		(130.948)	(40.818)	

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.1$. p-values are for F-test of whether characteristics are jointly significant in predicting assignment to treatment. Education is a dummy taking value 1 if the client has matriculated from high school. Application score is an internal credit score. ITC score is official credit score. Salary monthly is a dummy variable taking value 1 if the client receives his or her salary monthly and 0 for other pay frequencies. Approval: Pre-selection approval incentive. Approval + Repayment: Pre-selection approval incentive, and post selection repayment incentive. Repayment: Pre-selection repayment incentive. Repayment to Approval: Pre-selection repayment incentive, reduced ex post to approval incentive. Other customers are all walk in customers who requested a loan at any branch during the study period. Sample size is smaller than in other tables as baseline characteristics are missing for a small subset of clients.

clients with the ex-post repayment incentive had approximately 4 fewer days in default. These summary statistics clearly suggest that $\bar{\lambda}(e^*) > 0$ and that we will be able to reject hypothesis 1 when we control for other characteristics of the borrower.

Table 3: Summary Statistics for Key Outcome Variables Across Referrals Treatments

	1	2	3	4	5	6
	Approval	Approval + Repay	Repay	Repay to Approval	All Referrals	Other Customers
Any Default in First 100 Days	0.220 (0.054)	0.125 (0.042)	0.167 (0.046)	0.370 (0.066)	0.214 (0.026)	0.253 (0.008)
Days in Default in First 100	10.203 (2.975)	4.172 (1.894)	7.358 (2.590)	11.185 (2.673)	8.058 (1.276)	10.277 (0.398)
N	59	64	66	54	243	2973
Loan Not Repaid On Time	0.189 (0.054)	0.096 (0.041)	0.141 (0.044)	0.245 (0.062)	0.165 (0.027)	0.224 (0.008)
N	53	52	64	49	218	2555
Loan Approved	0.598 (0.050)	0.517 (0.046)	0.569 (0.046)	0.557 (0.051)	0.559 (0.024)	0.239 (0.003)
N	97	120	116	97	430	28129

Approval: Pre-selection approval incentive. Approval + Repayment: Pre-selection approval incentive, and post selection repayment incentive. Repayment: Pre-selection repayment incentive. Repayment to Approval: Pre-selection repayment incentive, reduced ex post to approval incentive. Other customers are all walk in customers who requested a loan at any branch during the study period.

Second, a comparison of columns 2 to 3 shows that conditional on having the repayment incentive ex-post those who had the repayment incentive ex-ante have a *higher* incidence of default. Similarly, a comparison of columns 1 and 4 shows that conditional on having the approval incentive ex-post, those who had the repayment incentive ex-ante again had a higher incidence of default. This suggests that we will not be able to reject hypothesis 2, and indeed it seems that the ex-ante incentive may have reduced the likelihood of repayment. We will discuss some reasons why this may be the case in section 6.

5.2.2 Approval

The final row of table 3 considers the approval rates across the groups. We see that while there are no large differences in approval across groups within the experiment, clients who presented a referrals card were much more likely to be approved for a loan. This comparison is not based on any experimental variation and therefore it is hard to say with precision what drives the results. One reasonable interpretation would be that referrers have good information about their friends credit worthiness, but this information is reflected in the lenders approval decision rather than in the repayment decision. If this interpretation is correct, it suggests that in markets in which lenders have less information, in particular where there is not a well established credit scoring system, peers may well be a valuable source of information for the lender. There are, however, two other possible explanations which limit the extent to which we can maintain this argument. First, all of the referrers have approved loans and if there is correlation in the credit worthiness of peers then we would assume that referred clients would be more likely to be approved. Second, it may be the case that the lender felt that referred clients were more likely to be credit worthy and applied their selection criteria more leniently. This seems to be unlikely as the lender uses a strict statistical approach to loan approval, but we cannot rule it out completely.

5.3 Regressions

We now turn to confirming our main results in a regression framework, this allows us to add in additional controls in order to increase power and ensure that our results are robust. We run two types of regressions. First we run the regressions

$$y_i = \alpha_i + \beta T_i + \gamma X_i + \epsilon_i, \tag{9}$$

where y_i is a measure of default, T_i is a dummy variable which takes on value 1 if i is "treated" and X_i is a set of controls. We run four regressions of this kind. First, to test hypothesis 1 we estimate (9) restricted to the sample of referred clients whose referrers had the ex-ante approval incentive where T_i indicates that the agent was in the approved + repayment group. Second, we estimate (9) restricted to the sample of referred clients whose referrers had the ex-ante repayment incentive and T_i indicates that the agent was in the repayment group.

The results of these first two regressions, controlling for a branch fixed effect are shown in panel A of table 4. The results confirms our finding from the summary statistics - we see that the ex-ante repayment incentive significantly reduced the amount of default. The impact is economically large and statistically significant. Conditional on the ex-ante approval incentive the ex-post repayment incentive reduced both default in the first 100 days and failure to pay the loan on time by 8% from a base of about 20%. The incentive also resulted in 6 fewer days in default from a base of around 10 days. Conditional on the ex-ante repayment incentive the ex-post repayment incentive reduced default in the first 100 days by 20% and failure to pay the loan by 10% from bases of 37% and 24% respectively. This effect translated into 4 fewer days in default from a base of 11%. These results are economically significant when we consider that the incentive is only 100R at the most, while an average loan is in the order of 3000R. Appendix table **??** shows that these results are robust to including controls. In panel A of that table we control for demographic characteristics of the referrer. The results remains qualitatively the same, in particular, the results are robust to including application score as a controls, which was shown to be correlated with treatment in table 2. We conclude that we are able to reject hypothesis 1, and that referrers are able to use social pressure to encourage loan repayment.

Second, to test hypothesis 2 we estimate (9) first on the sample of those who received the ex-post approval incentive and second on the sample who received the ex-post repayment incentive. In each case T_i is an indicator taking value 1 when the referrer had the ex-ante repayment incentive. The results in panel B of table 4 show that, if anything, those with the ex-ante repayment incentive are *more* likely to see default. In one specifications, the ex-post approval group (the odd columns), the ex-ante repayment incentive leads to a large increase in default which is statistically significant when we use the default in first 100 days metric. We discuss possible reasons for this finding in section **??**. Again these results are robust to including controls. In panel B of Appendix table **??** we control for baseline characteristics of the referrer. We are not able to control for the characteristics of the referred client, as this is endogenous given the change in the selection incentive. The results remain qualitatively the same when controls are included. We conclude that we are not able to reject hypothesis 2 - the data is consistent with the hypothesis that referrers have no information about their friends.

Finally, we also run a combined test of hypotheses 1 and 2. We estimate

$$y_i = \alpha_i + \beta_1 S_i + \beta_2 E_i + \epsilon_i \tag{10}$$

where y_i is a measure of default, S_i an indicator taking on value 1 if the referrer had an ex-ante repayment incentive - the selection incentive - and E_i is an indicator taking on value 1 if the referred had an ex-post repayment incentive - the enforcement incentive. The results are shown in panel C of table 4 and confirm our earlier findings. There is a large and significant reduction in default associated with the ex-post repayment - enforcement - incentive and a 0 or positive impact of the ex-ante - selection - incentive on defaults. Once again, these results are replicated using a probit specification and with controls in the appendix.

5.4 Size of the Enforcement Effect

The enforcement effect we see above is very strong, reducing default by between 8 and 20%. It is interesting to ask how the size of the effect compares to the impact of an incentive given directly to the borrower – rather than to a peer. We have two sources of evidence on this question. First, we attempted to conduct a dynamic incentives experiment at the same time as the referrals experiment. We provide a subset of the lenders clients with a randomly assigned bonus of either 50, 100 or 150 R if they repaid their loans on time. We were not happy that the randomization of this experiment was effective and,

Table 4: Tests of Hypotheses 1 and 2
OLS Regressions With Branch Fixed Effects

Panel A: Hypothesis 1: Social Pressure Improves Repayment

Dep Var	Any Default in First 100 Days		Days In Default First 100		Loan Not Repaid On Time	
Sample	App & App + Repay	Repay & Repay to App	App & App + Repay	Repay & Repay to App	App & App + Repay	Repay & Repay to App
Approval	Omitted	-	Omitted	-	Omitted	-
App + Repay	-0.080*	-	-5.800***	-	-0.082**	-
	(0.040)	-	(0.130)	-	(0.026)	-
Repay	-	-0.219**	-	-4.541**	-	-0.103
	-	(0.053)	-	(1.435)	-	(0.491)
Repay to App	-	Omitted	-	Omitted	-	Omitted
N	123	120	123	120	96	113
R^2	0.016	0.0536	0.0243	0.009	105	0.0176
Mean Dep Var in Omitted	0.220	0.370	10.203	11.185	0.189	0.245

Panel B: Hypothesis 2: Referrers Have Information About Referees

Dep Var	Any Default in First 100 Days		Days in Default in First 100		Loan Not Repaid On Time	
Sample	App & Repay to App	App + Repay & Repay	App & Repay to App	App + Repay & Repay	App & Repay to App	App + Repay & Repay
Approval	Omitted	-	Omitted	-	Omitted	-
App + Repay	-	Omitted	-	Omitted	-	Omitted
Repay	-	0.012	-	2.583	-	0.036
	-	(0.044)	-	(2.078)	-	(0.035)
Repay to App	0.158*	-	1.549	-	0.060	-
	(0.063)	-	(2.218)	-	(0.075)	-
N	113	130	113	130	102	116
R^2	0.027	0.004	0.001	0.0075	0.005	0.0046
Mean Dep Var in Omitted	0.220	0.125	10.203	4.172	0.189	0.096

Panel C: Combined Tests of Hypotheses 1 and 2

Dep Var	Any Default in First 100 Days	Days in Default in First 100	Loan Not Repaid On Time
Sample	All	All	All
Approval	Omitted	Omitted	Omitted
Selection (1=Repay & Repay to App)	0.0869***	2.129*	0.048
	(0.017)	(0.898)	(0.041)
Enforcement (1=Repay & App+Repay)	-0.148***	-4.979**	-0.102***
	(0.027)	(1.215)	(0.018)
N	243	243	218
R^2	0.044	0.018	0.029
Mean Dep Var in Omitted	0.220	10.203	0.189

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.1$. Robust standard errors in perentheses. Fixed effect for referred branch panel A and for referrer branch in panels B and C. Approval + Repayment: Pre-selection approval incentive, and post selection repayment incentive. Repayment: Pre-selection repayment incentive. Repayment to Approval: Pre-selection repayment incentive, reduced ex post to approval incentive.

therefore, we treat the evidence as suggestive. Nevertheless, our best estimate is that a 100 R incentive had a zero impact on the probability that a loan was in default. Second, Karlan and Zinman (2007) conducted a dynamic incentive experiment with a similar, although much larger, South African lender in 2004. Their intervention is somewhat different in that the dynamic incentive did not come in the form of a cash bonus, but rather in the form of a reduced rate on a future loan. Nevertheless it is interesting to compare the size of our effect to the impact seen in Karlan and Zinman (2007). On average, the dynamic incentive in that study reduced the interest rate on a future loan by 3.85% and led to a roughly 2.5% point increase in likelihood that the account be paid on time. This suggests that to have a similar impact as our study, a direct incentive would need to be very large - in the order of a 12% reduction in the interest rate (effectively making the interest rate on the next loan zero). These comparisons provide limited evidence that at least part of the enforcement effect in our experiment reflects social pressure, rather than simply the transfer of cash from the referrer to referred friend, as discussed in section 4.

6 Robustness to Relaxing Assumptions

The test of hypothesis 1 presented above does not depend to a large degree on whether or not the assumptions of our model are correct – the interpretation is relatively straightforward. The assumptions of our model are, however, essential to interpreting the tests of hypothesis 2, and the test of hypothesis 2 can be thought of as joint tests of the assumptions of the model and the null hypothesis. In this section we test the model, as well as considering the impact of relaxing the assumptions of our model.

6.1 Tests of the Model

The two key implications of the model are that as the sample size increases 1) $AD_a - AD_{ar} = AD_{ra} - AD_r$ and 2) $AD_{ra} - AD_a = AD_r - AD_{ar}$. We can test these restrictions using the results in table 4. There are a total of six tests, one for each equality for each dependent variable, and in none of these cases can we reject the model. It should be kept in mind, however, that these are not particularly strong tests of the model. Perhaps better evidence against the model comes from the negative impact of the ex-ante incentive which our model rules out. We, therefore, turn to discussing the impact of relaxing the assumptions of the model.

6.2 Knowledge of C_i

Our model assumes that C_i is known with certainty to the referrer. We make this assumption because we are concerned that referrers with an ex-ante approval incentive will refer a friend with a high θ because this increases the probability of loan approval. As noted above, we can relax the assumption somewhat. Let $C_i' \in [0,1]$ be the subjective probability that agent i will be approved for a loan. If C_i' is not correlated with $\hat{\theta}_i$ - that is the referrers perception of the likelihood of the friend being approved is not correlated with actual quality of the friend - then all our results above hold. If, however, this assumption does not hold, then our test of hypothesis 3 may lose much of its power as referrers in the approval group will tend to refer high $\hat{\theta}$ friends.

There are two ways to defend the assumption. First, micro loans are very common in the communities served by the lender we work with. Most loan recipients have received multiple loans in the past and have current outstanding loans or hire purchase agreements. Further, lenders do not differ greatly in their lending policy. It, therefore, seems that referrers should have a pretty good idea of who would be able to get a loan, simply from their previous ability to get loans. Second, if C_i is correlated with $\hat{\theta}_i$, then we should see that friends referred by agents in the approval group have *higher* repayment rates than loan takers off the street. We find no evidence that this is the case. Specifically, we estimate the impact of being in the approval group on repayment rates on a sample of all borrowers during the loan period. The coefficient on being in the approval group is $-0.025(0.056)$ when any default in the first 100 days is used, $.206(3.132)$ when days in default in first 100 is used and $-0.034(0.058)$ when on time repayment is used. Neither is significant at conventional levels. The results show that those in the approval group (group 1) are not more likely to repay a loan than an average person off the street. This is suggestive that referrers did not refer higher quality friends in order to ensure approval, although the interpretation is subject to two caveats. First, the coefficient on the approval group, although insignificant, is economically large, and in the direction we would expect if correlation between C_i and $\hat{\theta}_i$ were a problem. Second, there are substantial differences between the sample of people who walk in off the street and those who are referred. We have no way of knowing which direction this would bias the test, therefore the evidence is at best suggestive.

Related to this assumption, it might be argued that referrers do have information about credit worthiness, but that this information is essentially "absorbed" by the approval system used by the lender. This would imply that a referrals system, or peer information system would be worthwhile in a context in which there was no credit scoring system, but that it is not useful here. We have discussed this point above.

6.3 Independence of Altruism and Repayment

Assumption 2 is used to argue that referrers in the approval group will choose randomly from within their set of friends when referring. If this assumption does not hold and altruism is correlated with $\hat{\theta}$, then again our test of hypothesis 2 is a weak one. This does not seem to be directly testable, but there are two reasons to think that it does not matter. First, the test above which shows that the approval group does not have a lower default rate than individual off the street suggests that there is no systematic selection of better repayers in the approval group. Second, there does not seem to be any reason to believe that altruism would be correlated with ability of the friend to repay a loan.

6.4 No Heterogeneity in the Impact of Social Pressure

Assumption 3 implies that there is no heterogeneity with respect to social pressure. We make this assumption because, in its absence, referrers in the repayment group might refer friends who are "malleable" in the sense that $\lambda_i(e_i^*)$ is large. This will then imply that $\bar{\hat{\theta}}_R - \bar{\hat{\theta}}_A$ is an *under* estimate of the extent to which referrers have information about the credit worthiness of their friends. Indeed, if there is a large amount of heterogeneity in $\lambda(e^*)$, and and if this is negatively correlated with θ, then it is possible we will see $\bar{\hat{\theta}}_R - \bar{\hat{\theta}}_A < 0$ - which is indeed what we see in at least one specification in table 4.

To formulate a test for assumption 3, suppose that it is the case that referrers in the repayment group choose more malleable friends. Then we would expect that $\lambda(e^*)$ would be larger for those agents referred from the repayment group than the approval group.[6] This then implies that $AD_{ra} - AD_r > AD_a - AD_{ar}$. The test we presented above, showing that we cannot reject that $AD_a - AD_{ar} = AD_{ra} - AD_r$, bears directly on this assumption and shows that we cannot reject that there is no heterogeneity in λ. Of course, this test is open to the criticism that it does not have much power.

6.5 Altruism is Relatively Small

Assumption 4 essentially requires that referrers consider first the likelihood of repayment and second the value of the card to a friend. We cannot test this, but it does seem reasonable.

6.6 N_j Randomly Chosen

Given the observation that $\hat{\theta}$ is likely to be heterogeneous in the population, assumption 5 has two implications. First, it implies that there will, on average, be variation in the likelihood of repayment within a set of friends. Second, it implies that $\bar{\hat{\theta}}_R - \bar{\hat{\theta}}_A$ could be compared to the population in order to determine what portion of the heterogeneity in payment is known by referrers. The second use of the assumption is obviously irrelevant given that we cannot reject the hypothesis that referrers have no information. We could, therefore, relax assumption 5 and assume directly that there is heterogeneity within the friendship group with respect to $\hat{\theta}$. If this is satisfied, then our test still has some power. However, if this assumption is not met, then it is possible that referrer's friends are homogenous with respect to $\hat{\theta}$, in which case our test will say they have no information

[6] Note that this also requires that $\lambda(e^*)$ is not correlated with C_i, along a similar align to the discussion of correlation between θ_i and C_i.

regarding their friends. Our test is, therefore, a joint test of the hypotheses that individuals have information about their friends *and* that there is heterogeneity with respect to repayment rates within friendship groups. The rejection of this hypothesis is useful regardless and, in fact, information about friends would not be useful in a contract design situation unless there is heterogeneity among friends. Therefore, we are equally happy with the second interpretation of the test.

6.7 Referrers Have More than One Credit Worthy Friend

We assume that $C^j(1) \geq 2$ for all referrers, implying that they have at least two friends to choose from in making a referral. Thus, our test of hypothesis 3 is a joint test of the fact that people know things about their friends, and also that they have friends who could actually take out loans. We have no way to test whether this assumption holds. However, if the aim of contract design is to use information that is available in the community, it does not seem that it is possible to take advantage of that information if people do not know enough people. We, therefore, consider the result to indicate that information based schemes are unlikely to be effective in this environment, irrespective of the exact interpretation, much as in the section above. Indeed from a contract design perspective we see little difference between the finding that a) the referrers have no information, b) the referrers have homogenous friends and c) the referrers have no friends.

6.8 Other Behavioral Interpretations

The discussion so far suggests that our findings are relatively robust to alterations in the structure of our model. It is, however, possible that our results are driven by things which are not captured in our model. In particular, it could be argued that the increase in default due to the ex-ante repayment incentive seen in one specification in table 4 is the result of some sort of signaling. Specifically, by phoning up the referrer and telling him or her that the bonus no longer depends on repayment, the lender may be signaling that it does not care about repayment. Under this interpretation the repayment difference between approval and repayment to approval may reflect the signal, rather than information held by the referrer. We did take considerable effort to ensure that all phone calls were scripted and had a similar "tone", but the signaling interpretation is still possible. If these suggestions are correct, it implies that our test for hypothesis 3 is not a strong one. We can offer some suggestive evidence on this front. Table 3 shows that those who walk in off the street have a lower default rate than those in the repay to approval group, and that this difference is statistically significant. This suggests that the increase in default rates seen when comparing across groups with the ex-post approval incentive may be due to this kind of signaling effect. In fact this may be the best interpretation of the results that the ex-ante repayment incentive led to high default rates, especially as the tests presented above tend to cast doubt on the fact that heterogeneity in λ is the driving force.

7 Conclusions

We used a novel field experiment to separately assess whether peers have information about the credit worthiness of their friends and/or can use social pressure to enforce loan repayment. The results show that peers are extremely effective in enforcing repayment, but have no more information than the lender. Our findings have implications for the design of micro-credit contracts, suggesting that peer monitoring schemes – for example group lending – may be effective in deterring moral hazard but less effective in controlling adverse selection.

The interpretation of the screening finding is open to several caveats. First, South Africa has a well established credit scoring system and the lender there has quite good information on credit worthiness. Our results may therefore have limited applicability to a market with less well informed lenders, although in such markets lenders potentially work harder to gain information on borrowers through other, more informal, means. Some evidence exists to suggest that credit scoring in the South African market is far from perfect (Karlan and Zinman, 2009a) and one interpretation of our results is that peer monitoring is an ineffective means to improve the scoring system. Second, we do find some evidence consistent with peers having information about credit worthiness. The lender's approval rate for clients off the street is around 23%, but for clients referred through the Refer-A-Friend program the approval rate is around 55%. This observation is consistent with two interpretations: i) peers know which of their friends are credit worthy, but this information duplicates information already held by the lender; and ii) peers have correlated credit scores and, because the referrers were all approved borrowers, their peers are more liked to be approved than an average client. Third, peers can only be useful in screening borrowers if they have multiple friends who need a loan, if this is not the case then our results do not imply that peers have no information, but rather suggest that this is a market in which this information is difficult to extract.

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