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YOU CAN PICK YOUR FRIENDS, BUT YOU NEED TO WATCH THEM: LOAN SCREENING AND ENFORCEMENT IN A REFERRALS FIELD EXPERIMENT

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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 examine a randomized trial that allows separate identification of peer screening and enforcement of credit contracts. A South African microlender offered half its clients a bonus for referring a friend who repaid a loan. For the remaining clients, the bonus was conditional on loan approval. After approval, the repayment incentive was removed from half the referrers in the first group and added for half those in the second. We find large enforcement effects, a $12 (100 Rand) incentive reduced default by 10 percentage points from a base of 20%. In contrast, we find no evidence of screening.

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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 can 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 estimate a large demand for additional credit – for consumers (Karlan and Zinman, 2010), microenterprises (Banerjee et al., 2009; Karlan and Zinman, 2011) and small and medium enterprises (Banerjee and Duflo, 2004). These studies, coupled with a literature showing high returns to capital (e.g., De Mel et al. 2008), suggest that there may be important returns to relaxing borrowing constraints.

So, the goal is clear, but how does one relax borrowing constraints? Information asymmetries, including ex-ante selection and ex-post incentive and enforcement problems, are often invoked as the root causes of borrowing constraints in theory (Stiglitz and Weiss, 1981) and practice (Armendáriz et al. 2010). If this is indeed the case, contracts that alleviate asymmetric information problems provide one route to greater credit market efficiency. A widespread approach in this vein is based on the presumption that a borrower's peers can counter information asymmetries by providing information or enforcement that is unavailable to (or more costly for) the lender. The peer-intermediation approach has been fleshed out over several hundred years of lending practice and can be seen in a range of guises including credit cooperatives, credit unions, rotating savings and credit associations, and microlenders such as the Grameen Bank. The peer approach has also been analyzed over several decades of theoretical work on optimal mechanism design in the face of different asymmetric information problems (e.g., Varian 1990, Stiglitz 1990, Besley et al. 1993, Banerjee et al. 1994, Besley and Coate 1995, Ghatak 1999, Ghatak and Guinnane 1999, Rai and Sjöström 2004, and Bond and Rai 2008).

Empirical work on peer contracting mechanisms has lagged behind theory and practice. Empirical work could play an important role by showing whether and how peer mechanisms actually alleviate asymmetric information problems. Such results would have implications for theory, by helping to identify which models are most descriptive, and hence most useful for policy analysis. Empirical results could also inform practice, as lending institutions are actively wrestling with the mechanism design question of how to implement peer mechanisms on a large scale (e.g. Giné and Karlan 2010). But empirically identifying the different channels through which peer contracting might work– e.g., disentangling ex-ante screening from ex-post monitoring, enforcement, incentives, or insurance – is difficult. The few existing studies taking this line of inquiry have focused on symmetric mechanism designs in which individuals are jointly liable for each other, and have found mixed results. (See, e.g., Ferrara 2003, Ahlin and Townsend 2007, Karlan 2007, Gine et al. 2010, Fischer 2010, Giné and Karlan 2010 and Attanasio et al. 2011).

We designed a field experiment to test whether peers improve screening and/or enforcement under an individual liability mechanism.[1] This focus allows us to address the basic questions of whether peers have information about their friends and whether they can help to enforce loan repayment, without needing to address the strategic interactions among multiple borrowers. Specifically, we worked with Opportunity Finance South Africa (a member of the Opportunity International microfinance network) to test its Refer-A-Friend program, which offered an existing client (the referrer) a 100 Rand ($12) bonus for referring a "friend" (the referred, who could also be a family member, associate, etc.) who met particular criteria.

Opportunity first randomly divided referrers into one of two ex-ante incentives: referrers in the *ex-ante approval incentive* group were told that they would receive the bonus if the referred was approved for a loan. Referrers in the *ex-ante repayment incentive* group were told that they would receive the bonus if the referred repaid a loan on time. The ex-ante repayment incentive referrers had both an ex-ante incentive to refer applicants of good credit quality (both observable and unobservable to Opportunity), and an ex-post incentive to encourage repayment. Referrers in the ex-ante approval incentive group had only the ex-ante incentive to refer applicants of good *observable* credit quality.

[1]See also Klonner and Rai (2010), which finds in a non-experimental setting that co-signers improve repayment performance in "organized" (intermediated) rotating savings and credit associations

Subsequently, Opportunity randomly surprised some referrers, whose referred applications had been approved, with an improvement to their bonus contract. [2] Half of the referrers with the ex-ante repayment incentive were given their bonuses as soon as the loan was approved, thus removing the enforcement incentive. Half of referrers given the ex-ante approval incentive were offered an additional bonus if the referred loan was repaid, thus creating an enforcement incentive. Thus, within each of the ex-ante groups half the referrers have an *ex-post repayment incentive* and half have an *ex-post approval incentive*.

The design thus produces four groups of referrers, each with a different combination of ex-ante and ex-post incentives (in the spirit of Karlan and Zinman 2009), that, under certain assumptions detailed below, enable us to identify whether:

1. Opportunity induced referrers to *screen* on information unobservable to (or unused by) Opportunity. We estimate this by comparing repayment rates across ex-ante incentives holding the ex-post incentive fixed. We find no evidence that peer screening improved repayment.

2. Opportunity induced referrers to help *enforce* loan contracts. We estimate this by comparing repayment rates across ex-post incentives, holding the ex-ante incentive constant. We find that enforcement incentives do signficantly increase repayment: the small bonus (100 Rand is equal to about 2% of the average referrers gross monthly income and 3% of the average loan size), decreased default from around 20% to 10% in most specifications. The magnitude of improvement in repayment performance is far above and beyond what referrers and borrowers could accomplish with side-contracting, and the improvement in collections (and savings in collection costs) far exceeded the lender's outlays for bonuses.

We discuss the conditions under which our screening treatment allows us to identify whether referrers have information that is unobservable and useful to the lender. We lay out a model which identifies the key assumptions necessary for this interpretation and show that our 2×2 design, which allows us to estimate selection and enforcement in two different ways, allows us to identify whether peers have information even in a setting where the unobserved components of

[2] Lenders frequently contact borrowers with promotions in this market and our cooperating lender continued with the program after the experiment. We, therefore, feel that the arrangement would have felt natural to the borrower.

creditworthiness and responsiveness to incentives are correlated. This identification strategy is a key contribution of the paper and generates a test of the identification assumptions in two-stage experiments that aim to isolate selection effects (e.g., Karlan and Zinman 2009, Cohen and Dupas 2010, Ashraf et al. 2010 and Beaman and Magruder 2009.)

Although our main focus is on testing whether peers have information and can enforce, our experiment also demonstrates the usefulness of a novel contract design. Referral bonuses proved profitable for this lender, and hence may be a useful complement to or substitute for other risk-sharing covenants like guarantors.[3]

The remainder of the paper is structured as follows. Section 2 introduces Opportunity and the South African microloan market. Section 3 provides details of the experiment. Section 4 outlines a simple model of the referrer's decision process, highlighting the conditions under which our experiment separately identifies enforcement and selection. Section 5 provides some summary statistics and discusses the integrity of the randomization. Section 6 provides our main results. Section 7 discusses a few alternative explanations of the data and section 8 concludes.

2 Market and Lender Overview

Our cooperating lender is a new entrant to the South African consumer microloan market. Opportunity Finance South Africa (Opportunity) is a for-profit, wholly-owned subsidiary of Opportunity International, which has 1.26 million microloan customers across 24 different countries. Opportunity operates in the state of Kwazulu Natal, South Africa, and expanded from one branch in Pietermaritzburg to 5 branches across the state during our study period (February 2008 through July 2009). Opportunity offers small, high-interest, uncollateralised debt with a fixed monthly repayment amount. Loans made during our study period

[3]Loans co-signed by third parties are common in many developed countries and help those new to the credit market to leverage the assets of their co-signers (often family members) in order to build credit. But in many developing country settings guarantees are less viable due to limited enforcement and/or limited wealth.

averaged around 3500R ($US400), with a modal (mean) duration of 9 (10) months, and a modal (mean) monthly percentage rate of 5% (4.1%).[4] There is a competitive market for these loans in Kwazulu Natal (see Karlan and Zinman 2010 for a description of a different lender in this market).

Opportunity underwrites applications using a combination of internal and external credit scores (South Africa has well-functioning credit bureaus). A necessary condition for getting a loan is a documented, steady, salaried job. 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 for their children, attending/organizing a funeral, or purchasing a durable.

3 The Experiment

From February 2008 through July 2009, Opportunity offered each individual approved for a loan the opportunity to participate in its new "Refer-A-Friend"program. Individuals could participate in the program only once. Referrers received a referral card, which they could give to a friend (the referred). The referred earned R40 ($US5) if she brought in the card and was approved for a loan. The referrer could earn R100 ($US12)[5] for referring someone who was subsequently approved for and/or repaid a loan, depending on the referrer's incentive contract.

Opportunity first randomly assigned referrers to one of two ex-ante incentive contracts, corresponding to two different referral cards. Referrers given an ex-ante approval incentive would be paid only if the referred was approved for a loan. Referrers given the ex-ante repayment incentive would be paid only if the referred successfully repaid a loan.[6] Figure 1 shows examples of the referral cards, the top card was given to referrers in the ex-ante approval group and the bottom card to those in the ex-ante repayment group.

[4] The loans specify a fixed monthly repayment. The interest rate is calculated as that rate that would lead to the total cumulative repayment, calculated on the decreasing loan amount. So, for example, in a two period setting with loan amount L and payment x the interest rate R is the solution to $L(1+R)+(L-x)(1+R)=2x$.

[5] 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.

[6] Successful repayment was defined as having no money owing on the date of maturity of the

Figure 1: Referral Cards

Among the set of referrers whose referred friends were approved for a loan, Opportunity randomly selected half to be surprised with an ex-post incentive change. Among referrers who had been given the ex-ante approval incentive, half were assigned to receive an additional ex-post repayment incentive. Opportunity phoned referrers in this group and told them that, in addition to the R100 approval bonus, they would receive an additional R100 if the referred repaid the loan. The other half of referrers who had been given the approval incentive ex-ante were contacted by Opportunity and reminded to pick up their R100 bonus. (Opportunity did not provide any new information on the incentive contract to

loan, or successfully rolling over the loan.

Figure 2: 2 $\times$ 2 Experimental Design

		Ex-Ante Incentive	
		Approval	Repayment
Ex-Post Incentive	Approval	No Incentive	Screening Incentive
	Repayment	Enforcement Incentive	Screening and Enforcement Incentives

these referrers, but we wanted referrers in both ex-post arms to receive a phone call from Opportunity in case the personalized contact from the lender had some effect.) All phone calls were made on a weekly basis so that the phone call to the referrer occurred at most 5 working days after the loan was approved.

Among referrers who had been given the ex-ante repayment incentive, half of the referrers were assigned to have the ex-post repayment incentive removed. Opportunity phoned referrers in this group, told them that they would be paid R100 now, instead of conditional on loan repayment, and explained that this was the extent of the referrer's bonus eligibility (e.g., that the referrer would *not* receive an additional R100 if the loan was repaid). The other half of referrers who had been given the repayment incentive ex-ante were assigned to continue with an ex-post repayment incentive. Opportunity phoned these referrers with a reminder that they would receive a bonus if the loan was repaid.

Figure 2 summarizes the randomization and the incentives that the referrers face. Intuitively, any effect of peer screening can be identified by comparing the arms with and without an ex-ante repayment incentive, holding constant the ex-post incentive. Similarly, any effect of peer enforcement can be identified by comparing the arms with and without an ex-post repayment incentive, holding constant the ex-ante incentive.

4 Separate Identification of Selection and Enforcement

In this section we discuss identification. Identifying enforcement effects using the ex-post randomization is straightforward, so we focus on a more difficult problem - clarifying the conditions under which our experiment allows us to answer the question: "do peers have useful information about their friends that is not currently used by the lender in its screening process?" To facilitate the discussion we consider a stylized model of the referral and enforcement decision. Within the context of this model, we provide a definition of what it means for peers to have information that is not used by the lender and argue that, so long as referrers know how susceptible their friends are to social pressure and have more than one friend who would take out a loan, our experiment can tell us whether or not they have unobserved or unused information.

4.1 A Simple Model of The Referral Decision

We model a situation in which a referrer has N friends that could potentially be referred for a loan, and can encourage them to repay their loans by putting effort e into creating social pressure. Each potential referred is characterized by three parameters: a repayment type θ; a malleability type σ; and an approval type γ.[7] The repayment type and malleability type determine the probability that the referred will repay a loan according to the function

$$\pi(\theta, \sigma, e) = \min\{\theta + \sigma e, 1\},$$

where a high θ indicates creditworthiness. The approval type is simply the probability that the referred will be approved for a loan, which is determined by information observable to the lender.

We assume that the referrer has a subjective belief regarding his friend's type, which we denote $(\hat{\theta}, \hat{\sigma}, \hat{\gamma})$ and which may or may not be the same as the true type. In choosing whom to refer, referrers act on the basis of their subjective beliefs and assess (ex-ante – hence subscript a) utility from referring a friend of type $(\hat{\theta}, \hat{\gamma}, \hat{\sigma})$

[7] These parameters are assumed to be positive constants, suitably bounded when they represent probabilities.

given effort e to be

$$U_a(\hat{\theta}, \hat{\gamma}, \hat{\sigma}, e, A, R) = A\hat{\gamma} + R\hat{\gamma}\big(\pi(\hat{\theta}, \hat{\sigma}, e) - c(e)\big),$$

where c is a strictly increasing convex function measuring the cost of effort, A is an indicator variable taking on value 1 if the referrer is in the ex-ante approval treatment and R is a similar indicator for being in the ex-ante repayment group.[8] After approval we assume that referrers in the ex-post approval group choose $e = 0$ and referrers in the ex-post repayment group choose e to maximize ex-post utility

$$U_p(\hat{\theta}, \hat{\sigma}, e) = \pi(\hat{\theta}, \hat{\sigma}, e) - c(e).$$

We denote the maximizer (i.e., optimal enforcement effort) $e(\hat{\sigma})$.[9]

Our aim is to try to understand whether $\hat{\theta}$ contains information about true creditworthiness (θ) that is not already captured by the lender's approval process, γ. We cannot address this question with our experiment unless we make further assumptions. In particular, if perceived malleability ($\hat{\sigma}$) is completely erroneous and unrelated to σ (i.e. if referrers have entirely incorrect beliefs about malleability), then referral decisions can be based entirely on $\hat{\sigma}$ and, even though the referrer may know θ, our experiment will not be informative about the amount of information held by the referrer. We therefore assume:

Assumption 1 (Identification Assumptions)**.** *Let $\hat{N} \subseteq N$ be the set of friends that demand a loan from the lender. We assume*

1. *$\hat{\sigma} \propto \sigma$ – Referrers know how malleable their friends are; and*
2. *$\hat{N}$ has more than 1 element.*

Part 1 seems reasonable and we maintain it throughout although we are not able to test it. Part 2 is necessary because, given our setup, no information can be extracted if the referrer only has one friend that is interested in a loan. The importance of this assumption depends on why we wish to know if the referrer has

[8] We are implicitly normalizing the bonus payment to a value of 1, this is without loss.

[9] Note that, so long as there is an interior solution, the agent solves $\max_e[\theta + \sigma e - c(e)]$ and so maximiser does not depend on $\hat{\theta}$ except through any correlation between $\hat{\theta}$ and $\hat{\sigma}$.

information. Many potential contracts would use rankings, or choice between peers, as a means of extracting information. This is, for example, true of the mechanism discussed in Ghatak (1999). For mechanisms of this type it is irrelevant if the referrer has information about θ if $\hat{N} = 1$ and our experiment would test the relevant hypothesis that $\hat{N} > 1$ and the referrer has information about θ. One can, however, think of possible contracts for which this is not the case and our experimental design is less useful in those contexts.

We now turn to a definition of what it means for the referrer to have information not used by the lender:

Definition 1 (Referrer has additional useful information)**.** *We say that a referrer has additional useful information for the lender if:*

1. $\hat{\theta}_i > \hat{\theta}_j \Rightarrow \theta_i > \theta_j$ *for any* $i, j \in N$*; and*
2. $\hat{\theta}$ *is not perfectly correlated with* $\hat{\gamma}$*.*

The first part of the definition simply states that the referrer's subjective belief about her referred's repayment is correlated with reality. The second part states that the referrer's perceived probability of the referred's repayment is not perfectly correlated with the referrer's perceived probability of the referred getting approved – i.e., that the referrer believes she has useful information that is not captured by the lender's approval process. Such a belief is plausible in the empirical context here because referrers plausibly have good information about the approval process. Micro loans are common in the areas covered by our study, most referrers have received multiple loans in the recent past and/or are repaying a loan currently, and lenders do not differ greatly in their underwriting criteria.

We now argue that given Assumption 1, our experiment allows us to determine whether Definition 1 holds. To do this we first assume (in Subsection 4.2) that enforcement effort, e is independent of repayment type $\hat{\theta}$ (i.e. that σ is independent of $\hat{\theta}$ implying that $e(\sigma)$ does not depend on repayment type) and argue that if $\hat{N}$ has two or more elements and $\hat{\theta}$ is not perfectly correlated with $\hat{\gamma}$, then the $\hat{\theta}$ of those in the ex-ante repayment group will be higher than in the ex-ante approval group. Consequently testing whether the repayment rate in the ex-ante repayment group is higher than the ex-ante approval group (controlling for e) is sufficient to determine whether referrers have information that could be useful to the lender. We then argue (in Subsection **??**) that even if σ is correlated with $\hat{\theta}$, so that effort is not independent of θ, our 2 X 2 experimental design allows us to determine whether θ is higher in the ex-ante repayment group.

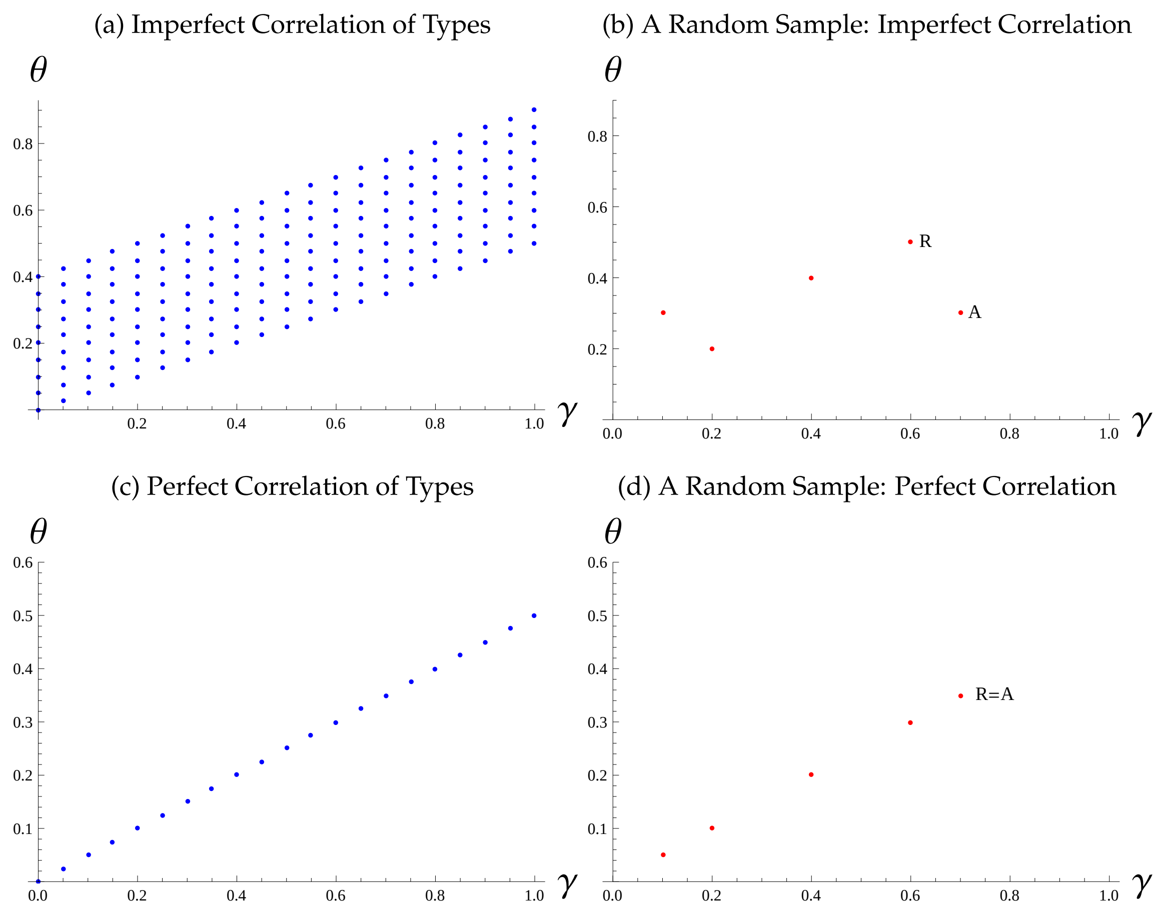
4.2 Extracting Information When Repayment is Correlated with Approval

In this subsection we illustrate that it is possible to extract information from the referred even when the probability of approval is correlated with the probability of repayment, as perceived by the referred. We begin by assuming that referrers in the ex-ante repayment group refer a friend in order to maximize

$$\hat{\gamma}(\pi(\hat{\theta}, \sigma, e^*) - c(e^*)),$$

where e^* is the optimal e (which we have assumed to be independent of σ). Referrers in the ex-ante approval group, however, simply choose the friend with the maximum $\hat{\gamma}$ because there is no return to exerting social pressure to repay. The result of these decisions are illustrated in Figure 3. Panel 3a shows the distribution of characteristics $\hat{\theta}$ and $\hat{\gamma}$ if they are imperfectly correlated. Panel 3b shows a possible random sample from this set: a set $\hat{N}$ of potential referreds. The point R shows the characteristics of the friend referred in the ex-ante repayment treatment, and A shows the characteristics of a friend referred in the ex-ante approval group. It should be clear that $\hat{\theta}_R \geq \hat{\theta}_A$ and that so long as $\hat{\theta}$ and $\hat{\gamma}$ are not perfectly correlated then this inequality will be strict for some referrers. Panels 3c and 3d show the case when $\hat{\theta}$ and $\hat{\gamma}$ are perfectly correlated. The characteristics of those in the approval and repayment groups will be the same. Thus if we determine that the repayment rate is not higher in the ex-ante repayment group, then it is either the case that either part 1 or part 2 of definition 1 does not hold, and we would conclude that the referrers have no more information than the lender.

Figure 3: Determining Whether the Referrer Has More Information Than the Lender



A denotes a type chosen in the ex-ante approval group and R a type chosen in the ex-ante repayment group. When there is perfect correlation between perceived repayment and approval types there is no variation in the perceived repayment type referred under the two treatments. However, if there is less than perfect correlation, it will always be the case that the individual referred in the ex-ante repayment treatment has a higher perceived repayment type.

4.3 Extracting Information When Malleability is Correlated with Repayment Type

In this section, we no longer assume that e is independent of repayment type ($\hat{\theta}$) by allowing malleability (σ) to be correlated with $\hat{\theta}$. *A priori*, it is not clear in which direction the correlation would go. One intuition suggests that there is much less scope for social pressure on those who are already diligently repaying - as a consequence we might suppose $\sigma(\theta_h) < \sigma(\theta_l)$ where $\theta_h > \theta_l$. Social pressure might even be counterproductive if high repayment types are intrinsically motivated and external pressure crowds out intrinsic motivation (e.g. Gneezy and Rustichini 2000, Benabou and Tirole 2003 & Besley and Ghatak 2005). A second intuition, however, suggests that high types will be those that are easiest to motivate; e.g., they already care the most about diligently repaying and hence will also care most about how they are viewed by their peers. We therefore might believe $\sigma(\theta_h) > \sigma(\theta_l)$.

These sorts of correlations make identification of the referrer's information difficult, because repayment rates will be determined by a combination of repayment type θ and the optimal social pressure $e(\sigma)$. If there is correlation between σ and θ, pressure will differ by type meaning that we are not making apples-to-apples comparisons. This issue arises also in other settings where two part experiments are used to separate selection. For example, in the moral hazard and adverse selection experiment of Karlan and Zinman (2009) typical formulations of adverse selection imply that high risk types put less effort into repayment, conditional on facing the same contract.[10] A direct comparison of repayment rates conditional on the same contract does not, therefore, identify what is usually thought of as an agent's "type" because agents of different types are also putting in different levels of effort.

Despite these challenges, our experimental design can identify whether referrers have information about θ regardless of the correlation between σ and θ. For the purposes of exposition we assume that there are only two types θ_l and θ_h where $\theta_l < \theta_h$ and that associated with each of these types is a (mean) malleability level σ_l and σ_h. We also assume that there is no correlation between referrers'

[10] This is somewhat akin to the argument in Einav et al. (2011).

subjective beliefs about approval types ($\hat{\gamma}$)and repayment types ($\hat{\theta}$). The analysis of Subsection 4.2 assures us that this assumption can be made without loss. Under these conditions we will be able to determine whether $\hat{\theta}$ is correlated with θ. Extending the discussion to more than two types is straightforward.

Figure 4: Identifying the Referrer Screening Effect

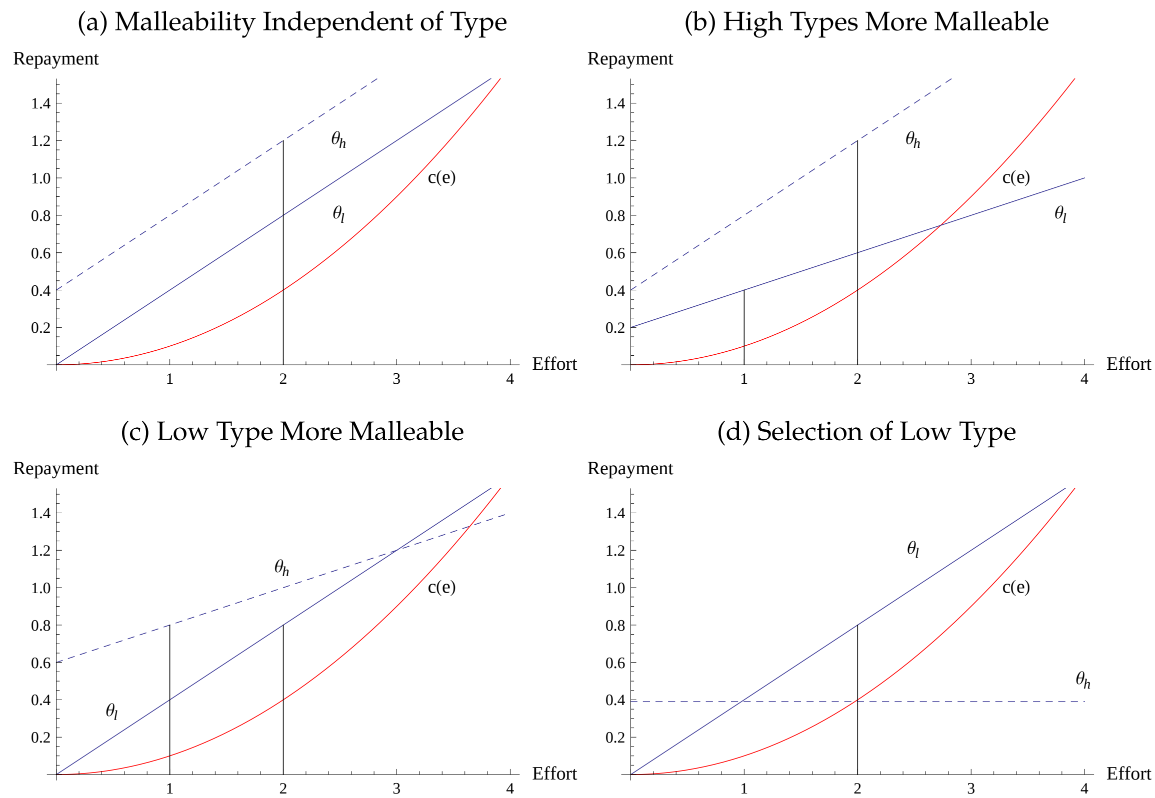


Figure 4 shows the four possible correlations between $\hat{\sigma}$ and θ in our field experiment. Denoting (θ_R, σ_R) and (θ_A, σ_A) as the types referred in the ex-ante repayment and approval groups respectively, our analysis will be based on two comparisons:

$$
\begin{aligned}
D(A) &= \pi(\theta_R, \sigma_R, 0) - \pi(\theta_A, \sigma_A, 0); \textit{and} \\
D(R) &= \pi(\theta_R, \sigma_R, e(\sigma_R)) - \pi(\theta_A, \sigma_A, e(\sigma_A)),
\end{aligned}
$$

the first of which is the difference in repayment rates across the ex-ante treatments conditional on being in the ex-post approval group and the second is conditional on being in the ex-post repayment group.

Figure 4a shows the most straightforward case: no correlation between θ and σ. Given our assumptions, referrers in the ex-ante repayment groups will refer the high type θ_h and $D(A) = D(R) = 0.5(\theta_h - \theta_l)$. Note that both estimates of the selection effect produce the same result; i.e., when unobserved credit quality and malleability to social pressure are uncorrelated, then loans referred by referrers with an ex-ante repayment incentive will perform the same, relative to those referred by referrers with an ex-ante approval incentive, regardless of the referrers' ex-post incentive. Conversely, if we find that $D(A) \neq D(R)$ empirically, then we learn that θ and σ are correlated.

Figure 4b illustrates the implications for identification when θ is positively correlated with σ – that is, when high types are more malleable. In this case, θ_h is chosen in the ex-ante repayment group but, as shown in the diagram, conditioning on the ex-post repayment incentive we make the comparison

$$D(R) = \pi(\theta_h, \sigma_h, 2) - 0.5(\pi(\theta_l, \sigma_l, 1) + \pi(\theta_h, \sigma_h, 2)) = 0.5\Big((\theta_h - \theta_l) + 2\sigma_h - \sigma_l\Big).$$

Without knowledge of σ_h and σ_l we are not able to identify the screening effect from this one comparison (i.e., in the absence of an additional empirical test, one cannot infer that $D(R) > 0$ implies a screening effect). Fortunately we do have an additional empirical test. Conditioning on the ex-post approval incentive gives

$$D(A) = \pi(\theta_h, \sigma_h, 0) - 0.5(\pi(\theta_l, \sigma_l, 0) + \pi(\theta_h, \sigma_h, 0)) = 0.5(\theta_h - \theta_l)$$

Putting the two results together, if high types are more malleable, then we have $D(R) > D(A)$, with $D(A)$ identifying the screening effect.

Figure 4c shows the case in which high types are less malleable, but it is still the case that the referrer refers the high type in the ex-ante repayment group (i.e., $\pi(\theta_h, \sigma_h, e(\sigma_h)) - c(e(\sigma_h)) > \pi(\theta_l, \sigma_l, e(\sigma_l)) - c(e(\sigma_l))$). Once again the comparison conditional on the ex-post repayment incentive is confounded by malleability:

$$D(R) = \pi(\theta_h, \sigma_h, 1) - 0.5(\pi(\theta_l, \sigma_l, 2) + \pi(\theta_h, \sigma_h, 1)) = 0.5\Big((\theta_h - \theta_l) + \sigma_h - 2\sigma_l\Big)$$

Indeed, the diagram suggests that one might mistakenly infer a negative screening effect from $D(R)$ even when the referrer actually does some valuable screening; i.e., when the referrer has some information regarding θ. Fortunately, as with case (b), conditioning on the ex-post approval incentive gives

$$D(A) = \pi(\theta_h, \sigma_h, 0) - 0.5(\pi(\theta_l, \sigma_l, 0) + \pi(\theta_h, \sigma_h, 0)) = 0.5(\theta_h - \theta_l)$$

and we can again identify the selection effect from $D(A)$. Putting the two results together for case (c), we have $D(A) > D(R)$, with $D(A)$ identifying the screening effect.

In the three cases so far $D(A)$ identifies the screening effect. Figure 4d helps illustrate that, in the fourth case, $D(R)$ helps identify the screening effect. Suppose that high types are less malleable and, in contrast to Case (c), $\pi(\theta_h, e(\theta_h)) - c(e(\theta_h)) < \pi(\theta_l, e(\theta_l)) - c(e(\theta_l))$; i.e., that here, the difference in malleability leads referrers to choose the low type in the ex-ante repayment group. As discussed above, this could happen if extrinsic motivation (social pressure) crowds-out internal motivation (which may comprise some or all of θ). Regardless of the underlying mechanism(s), the ex-ante repayment group in Case (d) consists entirely of θ_l, while the ex-ante approval group is a combination of low and high types. In this case, estimating the screening effect conditional on *either* ex-post incentive will give the incorrect result:

$$D(R) = \pi(\theta_l, \sigma_l, 2) - 0.5(\pi(\theta_l, \sigma_l, 2) + \pi(\theta_h, \sigma_h, 0)) = 0.5\Big((\theta_h - \theta_l) + 2\sigma_l\Big),$$

and

$$D(A) = \pi(\theta_l, \sigma_l, 0) - 0.5(\pi(\theta_l, \sigma_l, 0) + \pi(\theta_h, \sigma_h, 0)) = 0.5(\theta_l - \theta_h).$$

So $D(R)$ is once again confounded by malleability, and $D(A)$ recovers exactly the negative of the true screening effect, *if there is one*. This possible outcome of the model is indicated by a negative screening effect as measured by $D(A)$ and a larger and positive screening effect as indicated by $D(R)$. A negative $D(A)$ is, therefore, consistent with the model presented and suggests that the referrers have information regarding σ and (at least indirectly) information about θ. Only in the case that $D(A)$ and $D(R)$ are both negative would we conclude that there is adverse screening from the lender's perspective.

Summarizing all four cases, we can accurately identify $\theta_h - \theta_l$ given our 2 X 2 design:

a. If $D(A) = D(R) = x$ then $2x = \theta_h - \theta_l$.

b. If $D(A) < D(R)$ and $D(A) \geq 0$ then $2D(A) = \theta_h - \theta_l$.

c. If $D(A) > D(R)$ then $2D(A) = \theta_h - \theta_l$.

d. If $D(A) < D(R)$, $D(A) < 0$ and $D(R) > 0$ then $-2D(A) = \theta_h - \theta_l$.[11]

Finally, if $D(A) < 0$ *and* $D(R) < 0$ (a situation that is not possible in our model) we must infer that $\hat{\theta}$ is either uncorrelated with θ or negatively correlated with θ. Thus, under the assumptions that $\hat{\sigma} = \sigma$ and that $\hat{\gamma}$ is not perfectly correlated with $\hat{\theta}$, we can identify the selection effect regardless of the correlation between σ and $\hat{\theta}$.

Combining the two arguments of this section we conclude that if Assumption 1 holds then our experiment allows us to determine whether or not the referrers have additional useful information for the lender (according to Definition 1).

5 Data

5.1 Summary Statistics

Table 1 provides a summary of the characteristics of Opportunity borrowers over the period in which the experiment was run.

[11] In this case it is not clear that the referrer actually knows θ. As argued above we believe in this case that the referred does know θ, but the knowledge is indirect.

Table 1: Demographic Variables of all Borrowers During Experiment

	Mean	Median	Std Dev
Female	0.418	-	0.493
Age	37.789	36.000	10.785
High School Education	0.637	-	0.481
Disposable Income	1753	1265	1703
Requested Amount	5049	3000	6615
Requested Term (Months)	10.743	9	6.265
N		4383	

Disposable income is income remaining after rent, debt repayments and recurring obligations. An individual has a high school education if they have matriculated or gone on to tertiary education.

5.2 Integrity of the Randomization

Opportunity handed out 4408 referral cards to borrowers approved for new loans during the study period. Table 2 presents regressions of treatment assignment on a range of background characteristics of the potential referrers. If the randomization is valid, we would expect baseline characteristics to be uncorrelated with treatment. In all cases an F-test of the restriction that the coefficients are jointly zero fails to reject at the usual significance levels. Further, most individual coefficients are not statistically different from zero and the total number of significant coefficients is in line with what we would expect to see by chance. Below we also show that our results are robust to including controls for referrer baseline characteristics.

Of the 4408 cards that were handed out, 430 were returned and 245 of these referred clients were approved for a loan. The surprise nature of the second randomization (i.e. the change in ex-post incentives) provides another opportunity to check the integrity of the experimental implementation. Because the second-stage assignments were not known to potential referrers ex-ante (nor to Opportunity staff members delivering referral cards), baseline characteristics of those

Table 2: Testing The Balance of Referrer Characteristics Across Treatments: OLS

Ex-Ante Incentive	Approval		Repayment	
Ex-Post Incentive	Approval	Repayment	Approval	Repayment
Female	-0.006	0.008	0.005	-0.008
	(0.014)	(0.014)	(0.014)	(0.014)
Age	0.000	-0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
High School Education	-0.027	0.027	-0.007	0.008
	(0.023)	(0.023)	(0.023)	(0.023)
Salary Earner	-0.004	0.022	-0.016	-0.003
	(0.016)	(0.016)	(0.016)	(0.016)
Disposable Income	-0.003	-0.006	0.010*	-0.001
(Thousands of Rand)	(0.004)	(0.004)	(0.004)	(0.004)
Application Score	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
ITC Score	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
ITC Score Missing	0.072	0.039	-0.058	-0.053
	(0.109)	(0.109)	(0.109)	(0.110)
Requested Amount	-0.001	0.004*	-0.004*	0.000
(Thousands of Rand)	(0.002)	(0.002)	(0.002)	(0.002)
Requested Term	0.002	-0.004*	0.002	0.000
(Months)	(0.002)	(0.002)	(0.002)	(0.002)
Government Worker	0.005	-0.002	0.022	-0.025
	(0.031)	(0.031)	(0.032)	(0.032)
Cleaner/Builder/Miner	0.010	-0.006	0.007	-0.011
	(0.031)	(0.031)	(0.031)	(0.031)
Security/Mining/Transport	0.021	-0.004	0.020	-0.037
	(0.033)	(0.033)	(0.033)	(0.033)
Retail Worker	-0.002	0.008	0.003	-0.009
	(0.031)	(0.031)	(0.032)	(0.032)
IT/Financial Woker	0.010	-0.025	0.014	0.001
	(0.035)	(0.035)	(0.035)	(0.035)
Agriculture/Manufacturing	0.008	-0.005	0.027	-0.030
	-0.029	-0.029	-0.029	-0.029
Constant	0.189	0.224	0.273**	0.315*
	(0.118)	(0.118)	(0.119)	(0.120)
F-test of joint significance	0.560	0.930	0.810	0.440
p-value of F-test	0.916	0.533	0.679	0.971
N	4408	4408	4408	4408

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.1$. Each column represents a separate OLS regression where the LHS variable is assignment to the particular treatment. Education is a dummy variable taking on value 1 if the referrer has matriculated. Application score is an internal credit score. ITC score is external credit score. Salary monthly is a dummy variable taking value 1 if the client receives his or her salary monthly.

referred and approved for a loan should not differ within the ex-ante treatment groups.[12] To test for balance we run regressions similar to those presented in Table 2 where the outcome variable is being assigned to the ex-post repayment incentive. The first two columns of Table 3 shows the results of the regressions. Within the group given the ex-ante approval incentive, the F-test shows that the baseline coefficients do not significantly predict assignment to treatment in the joint test. Among the individual tests, only one of the sixteen variables is significant, which is about what one would expect to happen by chance.

Within the group given the ex-ante repayment incentive, there appears to be more cause for concern (Column 2). A higher application score (i.e., internal credit score) significantly predicts assignment to the ex-post approval group. Given that application score is a key measure of the observed credit quality of the applicant, this is troubling. It turns out that Opportunity changed its application score in May 2009. Before this time, scores are out of 200, while after they are out of 800. Only 12 referred clients from the ex-ante repayment group were approved for loans after this point and 9 were from the ex-post approval group. This is not out of line with what we would expect from random arrival times, but does create a problem in testing orthogonality. Columns 3 and 4 of Table 3 take two approaches. First, in Column 3 we leave out the application score. With application score not included, the p-value for the F-test of joint significance rises to 0.326 from 0.077 and we are more confident that the allocation is random. Second, in Column 4 we restrict the sample to prior to May 2009. This restriction also implies that the baseline characteristics are no longer significantly predictive of assignment. We gain further confidence by considering the impact of ITC score on assignment. The ITC score is an externally provided credit score and is likely to be another good predictor of credit worthiness and it is never predictive of treatment status. Overall it seems that the randomization was succesful. Regardless, we show below that our results are not sensitive to including these baseline

[12]Comparison across the ex-ante incentive groups are, however, endogenous. That is, we cannot compare characteristics of those in the ex-ante approval groups to those in the ex-ante repayment groups as part of the experiment aims to generate difference in these characteristics. We can run similar regressions on those who were referred not conditioning on being approved. The results are similar.

characteristics as controls.[13]

6 Results

We identify screening and enforcement rates by comparing the repayment performance of loans referred by referrers facing different incentives. We have four different and complementary measures of repayment performance. Each proxies for the costs a lender bears when borrowers don't repay (on time), without needing to impose additional assumptions on what the lender's cost structure actually is (since in our experience many lenders lack precise data on marginal costs of collections). First, we have an indicator variable, for all 245 referred clients, of whether or not the borrower was charged penalty interest for paying late at any time during the course of the loan. Second, we measure whether the loan was fully repaid on the date of maturity for the 240 loans that have reached maturity.[14] Third, for those 240 loans we also calculate the proportion of principal still owed at maturity date (this value is zero for loans repaid on time, and positive for loans in arrears). Fourth, Opportunity charges off loans deemed unrecoverable and has made a chargeoff decision (yes or no) on all but one of the 240 loans that have reached maturity as of this writing.[15]

Each panel in Table 4 shows the mean of these four loan performance measures, organized by treatment groups. It also shows the difference in means holding either the ex-ante or ex-post repayment fixed. These differences are our key results.

The "Difference" row in the first two columns of each panel in Table 4 shows an estimate of the enforcement effect that is created by a difference in the ex-post incentives. So altogether the table provides eight estimates of the enforcement effect (two for each measure of default). The point estimate for each of the eight differences is negative, suggesting that adding the ex-post repayment incentive decreases the incidence of default. In each case the implied magnitude of the

[13] We can only control for these differences when studying the enforcement question, when we consider selection, referred characteristics are endogenous.

[14] A loan that was rolled over was considered to be repaid.

[15] The results do not change qualitatively if we arbitrarily assign this loan as being charged off or not.

Table 3: Testing The Balance of Referred Characteristics Across Ex-Post Treatments. Dependent Variables is Assignment to Ex-Post Repayment Incentive: OLS

	Whole Sample		App. Score Exlcuded	Before May 2009
Ex-Ante Incentive	Approval	Repayment	Repayment	Repayment
Female	0.041	0.098	0.088	0.147
	(0.113)	(0.104)	(0.107)	(0.113)
Age	0.001	0.003	0.004	0.002
	(0.005)	(0.005)	(0.005)	(0.006)
High School Education	0.131	0.022	-0.005	-0.008
	(0.148)	(0.164)	(0.169)	(0.173)
Salary Earner	0.029	-0.117	-0.086	-0.106
	(0.110)	(0.113)	(0.116)	(0.133)
Disposable Income (Thousands of Rand)	0.034	0.028	0.037	0.023
	(0.057)	(0.059)	(0.061)	(0.069)
Application Score	0.000	-0.001***	-	0.002
	(0.000)	(0.000)	-	(0.004)
ITC Score	0.002	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
ITC Score Missing	1.197	-0.116	-0.276	-0.077
	(0.893)	(0.895)	(0.922)	(0.935)
Requested Amount (Thousands of Rand)	0.010	-0.027*	-0.026	-0.020
	(0.011)	(0.016)	(0.016)	(0.021)
Requested Term (Months)	-0.013	0.014	0.010	0.012
	(0.015)	(0.017)	(0.018)	(0.019)
Government Worker	-0.389	0.103	0.023	0.115
	(0.266)	(0.257)	(0.264)	(0.273)
Cleaner/Builder/Miner	-0.094	0.098	0.028	0.027
	(0.207)	(0.211)	(0.217)	(0.225)
Security/Mining/Transport	-0.330	-0.321	-0.450*	-0.355
	(0.226)	(0.251)	(0.255)	(0.275)
Retail Worker	-0.212	-0.105	-0.166	-0.129
	(0.203)	(0.220)	(0.226)	(0.231)
IT/Financial Woker	-0.570**	0.495	0.445	0.427
	(0.279)	(0.533)	(0.550)	(0.562)
Agriculture/Manufacturing	-0.222	-0.168	-0.214	-0.199
	-0.187	-0.222	-0.229	-0.234
Constant	-0.547	0.642	0.692	0.348
	(0.923)	(0.932)	(0.962)	(1.059)
F-test of joint significance	0.810	1.640	1.150	0.990
p-value of F-test	0.669	0.077*	0.326	0.478
N	123	120	120	108

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.1$. Each column represents a separate OLS regression where the LHS variable is assignment to the particular treatment. Education is a dummy variable taking on value 1 if the referrer has matriculated. Application score is an internal credit score. ITC score is external credit score. Salary monthly is a dummy variable taking value 1 if the client receives his or her salary monthly.

Table 4: Key Outcome Variables: Mean Differences Across Treatment Groups

(a) Penalty Interest Charged by Lender (N=245)

Ex-Post Incentive	Ex-Ante Incentive: Approval	Ex-Ante Incentive: Repayment	Diff
Approval	0.389 (0.064)	0.518 (0.069)	0.129 (0.093)
Repayment	0.258 (0.054)	0.272 (0.055)	0.015 (0.077)
Difference	-0.132 (0.083)	-0.246*** (0.087)	0.114 (0.122)

(b) Positive Balance Owing at Maturity (N=240)

Ex-Post Incentive	Ex-Ante Incentive: Approval	Ex-Ante Incentive: Repayment	Diff
Approval	0.206 (0.054)	0.226 (0.058)	0.019 (0.079)
Repayment	0.095 (0.037)	0.152 (0.044)	0.056 (0.058)
Difference	-0.111* (0.064)	-0.075 (0.072)	0.036 (0.098)

(c) Portion of Loan Value Owing at Maturity (N=240)

Ex-Post Incentive	Ex-Ante Incentive: Approval	Ex-Ante Incentive: Repayment	Diff
Approval	0.187 (0.054)	0.257 (0.076)	0.070 (0.091)
Repayment	0.076 (0.039)	0.109 (0.039)	0.033 (0.055)
Difference	-0.110* (0.066)	-0.147* (0.081)	0.037 (0.108)

(d) Loan Charged off By Lender (N=239)

Ex-Post Incentive	Ex-Ante Incentive: Approval	Ex-Ante Incentive: Repayment	Diff
Approval	0.155 (0.048)	0.188 (0.054)	0.034 (0.072)
Repayment	0.047 (0.027)	0.092 (0.036)	0.045 (0.045)
Difference	-0.108** (0.054)	-0.096 (0.063)	0.011 (0.085)

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.1$. Penalty interest is charged by the lender if a borrower is late in making an expected payment. A loan is charged off if the lender deems that there is no probability that it will be repaid. Standard errors in parentheses and p-values in square brackets. p-values are for a χ^2-test of the hypothesis that the difference in differences is equal to zero. Ex-Ante incentive is the incentive that the referrer faced when choosing a friend to refer. Ex-Post incentive is the incentive that the referrer faced *after* the loan had been approved. Approval implies the loan had to be approved in order to earn the bonus and repayment implies the loan had to be repaid in order to earn the bonus.

enforcement effect is large; e.g., an 11 percentage point reduction in chargeoff likelihood, on a base of 16%. Five of the eight estimates are statistically significant from zero, despite our small sample. In all, the results suggest that small referral incentives create social pressure that lead to large reductions in default.

The "Diff" column in the first two rows of each panel in Table 4 shows an estimate of the screening effect that is created by moving from the ex-ante approval incentive to the ex-ante repayment incentive. The point estimate for each of the eight differences is statistically insignificant and *positive*. So there is no evidence that small referral incentives induce screening that reduces default.

The bottom-right cell in each panel of Table 4 estimates whether malleability is correlated with repayment type, by taking the difference-in-differences (DD) across the two different estimates of the referral incentive effects on default rates. Recall from Section 4 that, under Assumption 1, a zero estimate of the DD indicates that ex-post malleability is uncorrelated with ex-ante repayment type. And indeed none of the four estimates is significantly different than zero. It bears emphasizing, however, that these are very imprecisely estimated zeros: each of the four confidence intervals includes economically large correlations between malleability and type.

Under the assumption that malleability is uncorrelated with repayment type, we can estimate the enforcement and selection effects with greater precision with regressions that pool across all four treatment arms:

$$y_i = \alpha + \beta^1 enforce_i + \beta^2 select_i + \epsilon_i$$

where y_i is one of the four measures of default, $enforce_i$ is an indicator taking on value 1 if client i was referred by someone with the ex-post repayment incentive, and $select_i$ is an indicator taking on value 1 if the client was referred by a referrer with the ex-ante repayment incentive. Results from this regression (without controls) are presented in Table 5. For each of the four outcome measures we see a large and statistically significant reduction in default associated with the enforcement incentive, and a smaller and statistically insignificant increase in default coming from the selection incentive. These results sharpen the key inferences from the means comparisons in Table 4: there is a large enforcement effect, and no (or a perverse, as discussed below) selection effect.

Table 5: Pooled Impact of Selection and Enforcement Treatments on Key Outcome Variables: OLS Without Controls

Outcome Measure	Penalty Interest	Not Paid on Time	Portion Owing	Loan Charged Off
Enforcement	-0.188***	-0.094*	-0.129**	-0.100**
	(0.061)	(0.049)	(0.054)	(0.042)
Selection	0.067	0.039	0.050	0.040
	(0.060)	(0.047)	(0.052)	(0.041)
Constant	0.419***	0.197***	0.196***	0.149***
	(0.054)	(0.045)	(0.046)	(0.039)
N	245	240	240	239

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.1$. Penalty interest is charged by the lender if a borrower is late in making an expected payment. A loan is charged off if the lender deems that there is no probability that it will be repaid. Standard errors in parentheses. Ex-Ante incentive is the incentive that the referrer faced when choosing a friend to refer. Ex-Post incentive is the incentive that the referrer faced *after* the loan had been approved. Approval implies the loan had to be approved in order to earn the bonus and repayment implies the loan had to be repaid in order to earn the bonus.

Appendix A shows that these results are robust to various specifications that control for the baseline characteristics of borrowers or referrers.

6.1 Size of the Enforcement Effect

The enforcement effects we see above are very strong, reducing default by between 9 and 19 percentage points in Table 5. 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 one bit of evidence from a similar context. Karlan and Zinman (2009) conducted a dynamic incentive experiment with a similar, although much larger, South African lender in 2004. That 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. On average, the dynamic incentive reduced the interest rate on a future loan by 3.85% and led to a roughly 2.5% point increase in likelihood that the current loan was paid on time. This result 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). This again suggets 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 the borrower.

7 Alternative Explanations

In this section we discuss alternative interpretations of the results.

7.1 Income Effects

In theory, the enforcement effect could be driven by side-payments from the referrer to referred that produce an income effect on loan repayment. In practice this channel seems implausible, for several reasons. First, the bonus was not paid out until after the loan was repaid, and the borrowers in our sample are liquidity constrained (as evidence by the fact that they are borrowing at high rates). Second, even our smaller point estimates imply default reductions that seem too large (about R500 on the average loan) to be explained by a small increase in income (maximum R100). Third, as discussed above, the enforcement effects here are large in comparison to similar estimates when bonuses were paid directly to the borrower.

7.2 Signaling

The repayment rates in Table 4 consistently show that the highest default rates occur for those clients that were in the ex-ante repayment group and were moved to the ex-post approval group. In this treatment group, Opportunity phoned the referrer and told her that the bonus would no longer be paid upon repayment. It is possible that this signaled that the lender was not really interested in repayment. If this explanation is correct, then our estimate of selection conditional on being in the ex-post approval group would be biased in favor of showing no screening, while our estimate of enforcement conditional on the ex-ante repayment incentive would be biased in favor of finding an enforcement effect. There are three reasons why this should not be a concern. First, even if we ignore these two means of estimating the effects, the other comparisons support the conclusions of the paper. Second, as discussed above, it is never the case that the difference-in-differences is statistically different from zero, implying that these potentially biased estimates of selection and enforcement are not statistically different from the unbiased ones. Third, and most importantly, if the signaling story were correct we would anticipate that repayment rates for the referrer would also be affected. The default rate of the "signaled", minus the default rate of the "un-signaled" are -0.060 ($p = 0.414$), -0.030 ($p = 0.473$), -0.019 ($p = 0.664$) and -0.006 ($p = 0.895$) for the four default measures (interest, balance owing, portion owing and charged-off respectively) indicating that the data does not support the signaling story. If anything the point estimates suggest that the "signaled" were better repayers.

7.3 Impatience

Referrers that were assigned to the ex-ante repayment incentive were promised a bonus that would not be paid until the referrer repaid their loans. One might therefore expect fewer referrers to make a referral in this treatment group, and/or that those making referrals would be more patient (and hence be more willing to and effective at enforcing loans). Either difference could, in principle, create issues for the identification of screening effects. In practice, such issues do not loom large. First, the number of referred clients does not differ across the ex-ante treatment groups (99 in the ex-ante approval group v. 94 in the ex-ante repayment group $p = 0.516$). Second, if those referring clients in the ex-ante repayment group were more patient and this impacted on how much social pressure they placed on their referreds then we would expect to see evidence for this in the size of the enforcement effect. As discussed above, there is no evidence for this.

7.4 Interpretation of the Selection Effect

The interpretation of the screening finding is open to several caveats. First, South Africa has a well established credit scoring system, and our lender has extensive experience with its internal scoring model as well. The extent to which our results would generalize to markets where lenders rely more heavily on ″soft″ information is uncertain. 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 creditworthy, 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 likely to be approved than an average client. These two possibilities make it hard to give a causal interpretation to the correlation. 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 peer information is difficult to extract.

8 Conclusions

We used a novel field experiment to separately assess whether peers have information about the creditworthiness 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 a referral scheme may be a cost-effective complement or substitutes for mechanisms – like group lending – that are designed to mitigate moral hazard/limited enforcement problems. The results also suggest that mechanisms that rely on selection effects are unlikely to be effective in the study location.

Our analysis was based on a novel "two-stage" randomization that follows the basic methodology of Karlan and Zinman (2009). Unlike that experiment and others like it, our experiment allows for two different estimates of the selection effect. We show that in our setting this feature of the experiment allows us to cleanly identify selection effects even when enforcement efforts are correlated with the "type" that is selected. We hope that this analysis of identification will be useful for the growing literature that uses multi-stage experiments (e.g. Cohen and Dupas 2010, Ashraf et al. 2010, Beaman and Magruder 2009 and Chassang et al. 2010) .

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A Robustness to Controls

We now check whether the results are robust to adding controls. We start by estimating the enforcement or screening effect separately using equations of the form:

$$y_i = \alpha_i + \beta T_i + \gamma X_i + \epsilon_i, \tag{1}$$

where y_i is again 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 for either referrer or borrower baseline characteristics (these sets of characteristics are highly collinear). When estimating the enforcement effect here, $T_i = 1$ if the referrer was given the ex-post repayment incentive. We condition on the ex-ante incentive by running regressions separately for the samples that received the ex-ante approval incentive (Tables A.1 and A.2, Panel (a)) or the ex-ante repayment incentive (Panel (b)). When controlling for the referred's application score we include a dummy variable for whether the client came in after the change in application score procedure and also interact that term with the application score. Tables A.1 and A.2 show that adding controls does not alter the coefficients appreciably.

To test for selection effects we repeat the above exercise with T_i being an indicator for whether the referrer was given an ex-ante repayment incentive. The results are reported in Table A.3. In Panel (a) we restrict the sample to those given the ex-post approval incentive and in Panel (b) we restrict the sample to those given the ex-post repayment incentive. For these regressions we control for referrer characteristics as the referred characteristics are endogenous. Again, the results are robust to including controls.

Finally, we again pool the data and assume that the enforcement and selection effects are independent of each other. That is we run the regression

$$y_i = \alpha + \beta^1 enforce_i + \beta^2 select_i + \beta^3 X_i + \epsilon_i$$

where X_i is a set of controls. In this case we can only control for referrer characteristics as once again the referred characteristics are endogenous. Table A.4 contains the results, which do not differ significantly from those reported in Table 5 without controls.

Table A.1: Enforcement Effects. The Impact of Ex-Post Repayment Incentive Within Ex-Ante Treatment Group: OLS with Controls for Referrer Characteristics

(a) Ex-Ante Approval Incentive

	Penalty Interest	Not Paid on Time	Portion Owing	Charged Off
Ex-Post Approval	Left Out	Left Out	Left Out	Left Out
Ex-Post Repayment	-0.144 (0.097)	-0.188** (0.084)	-0.184** (0.084)	-0.166** (0.068)
Mean in Ex-post approval	0.389 (0.064)	0.206 (0.054)	0.186 (0.054)	0.155 (0.047)
Controls	All	All	All	All
N	125	121	121	121

(b) Ex-Ante Repayment Incentive

	Penalty Interest	Not Paid on Time	Portion Owing	Charged Off
Ex-Post Approval	Left Out	Left Out	Left Out	Left Out
Ex-Post Repayment	-0.208* (0.107)	-0.115 (0.084)	-0.157* (0.091)	-0.128* (0.076)
Mean in Ex-post approval	0.519 (0.069)	0.226 (0.058)	0.256 (0.076)	0.189 (0.054)
Controls	All	All	All	All
N	120	117	117	117

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.1$. Penalty interest is charged by the lender if a borrower is late in making an expected payment. A loan is charged off if the lender deems that there is no probability that it will be repaid. Standard errors in parentheses. Ex-Ante incentive is the incentive that the referrer faced when choosing a friend to refer. Ex-Post incentive is the incentive that the referrer faced *after* the loan had been approved. Approval implies the loan had to be approved in order to earn the bonus and repayment implies the loan had to be repaid in order to earn the bonus. Controls: Female, Age, Disposable Income, Salary Occurrence, Education, Application Score, ITC Score, Job Type, Requested Loan Amount, Requested Term, Branch, Application Month, Application year. All controls are for referrer characteristics. Categorical variables are entered as fixed effects.

Table A.2: Enforcement Effects. The Impact of Ex-Post Repayment Incentive Within Ex-Ante Treatment Group: OLS with Controls for Referred Characteristics

(a) Ex-Ante Approval Incentive

	Penalty Interest	Not Paid on Time	Portion Owing	Charged Off
Ex-Post Approval	Left Out	Left Out	Left Out	Left Out
Ex-Post Repayment	-0.100** (0.034)	-0.127** (0.045)	-0.120** (0.039)	-0.115** (0.046)
Mean in Ex-post approval	0.389 (0.064)	0.206 (0.054)	0.186 (0.054)	0.155 (0.047)
Controls	All	All	All	All
N	125	121	121	121

(b) Ex-Ante Repayment Incentive

	Penalty Interest	Not Paid on Time	Portion Owing	Charged Off
Ex-Post Approval	Left Out	Left Out	Left Out	Left Out
Ex-Post Repayment	-0.312** (0.095)	-0.072* (0.033)	-0.066 (0.040)	-0.098* (0.046)
Mean in Ex-post approval	0.519 (0.069)	0.226 (0.058)	0.256 (0.076)	0.189 (0.054)
Controls	All	All	All	All
N	120	119	119	118

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.1$. Penalty interest is charged by the lender if a borrower is late in making an expected payment. A loan is charged off if the lender deems that there is no probability that it will be repaid. Standard errors in parentheses. Ex-Ante incentive is the incentive that the referrer faced when choosing a friend to refer. Ex-Post incentive is the incentive that the referrer faced *after* the loan had been approved. Approval implies the loan had to be approved in order to earn the bonus and repayment implies the loan had to be repaid in order to earn the bonus. Controls: Female, Age, Disposable Income, Salary Occurrence, Education, Application Score, Application Score Post May 2009, ITC Score, Job Type, Requested Loan Amount, Requested Term, Branch, Application Month, Application year. All controls are for referrer characteristics. Categorical variables are entered as fixed effects.

Table A.3: Selection Effects. The Impact of Ex-Ante Repayment Incentive Within Ex-Post Treatment Group: OLS with Controls

(a) Ex-Post Approval Incentive

	Penalty Interest	Not Paid on Time	Portion Owing	Charged Off
Ex-Ante Approval	Left Out	Left Out	Left Out	Left Out
Ex-Ante Repayment	0.046 (0.041)	0.046 (0.035)	0.035 (0.041)	0.027 (0.031)
Mean in Ex-Ante Approval	0.389 (0.064)	0.206 (0.054)	0.186 (0.054)	0.155 (0.047)
Controls	All	All	All	All
N	113	111	111	111

(b) Ex-Post Repayment Incentive

	Penalty Interest	Not Paid on Time	Portion Owing	Charged Off
Ex-Ante Approval	Left Out	Left Out	Left Out	Left Out
Ex-Ante Repayment	0.007 (0.100)	0.009 (0.080)	0.018 (0.075)	0.028 (0.063)
Mean in Ex-Ante Approval	0.258 (0.054)	0.095 (0.037)	0.076 (0.039)	0.047 (0.027)
Controls	All	All	All	All
N	132	129	129	128

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.1$. Penalty interest is charged by the lender if a borrower is late in making an expected payment. A loan is charged off if the lender deems that there is no probability that it will be repaid. Standard errors in parentheses. Ex-Ante incentive is the incentive that the referrer faced when choosing a friend to refer. Ex-Post incentive is the incentive that the referrer faced *after* the loan had been approved. Approval implies the loan had to be approved in order to earn the bonus and repayment implies the loan had to be repaid in order to earn the bonus. Controls: Female, Age, Disposable Income, Salary Occurrence, Education, Application Score, Job Type, Requested Loan Amount, Requested Term, Branch, Application Month, Application year. All controls are for referrer characteristics. Categorical variables are entered as fixed effects.

Table A.4: Pooled Impact of Selection and Enforcement Treatments on Key Outcome Variables: OLS With Controls (same as Table 5 but with controls)

Outcome Measure	Penalty Interest	Not Paid on Time	Portion Owing	Loan Charged Off
Enforcement	-0.168***	-0.117**	-0.130**	-0.109**
	(0.065)	(0.055)	(0.054)	(0.047)
Selection	-0.009	0.021	0.018	0.032
	(0.074)	(0.061)	(0.062)	(0.053)
Mean in Left Out	0.389	0.206	0.186	0.155
	(0.064)	(0.054)	(0.054)	(0.047)
Controls	All	All	All	All
N	245	240	240	239

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.1$. Penalty interest is charged by the lender if a borrower is late in making an expected payment. A loan is charged off if the lender deems that there is no probability that it will be repaid. Standard errors in parentheses. Ex-Ante incentive is the incentive that the referrer faced when choosing a friend to refer. Ex-Post incentive is the incentive that the referrer faced *after* the loan had been approved. Approval implies the loan had to be approved in order to earn the bonus and repayment implies the loan had to be repaid in order to earn the bonus. Controls: Female, Age, Disposable Income, Salary Occurrence, Education, Application Score, ITC score, Job Type, Requested Loan Amount, Requested Term, Branch, Application Month, Application year. All controls are for referrer characteristics. Categorical variables are entered as fixed effects.