LSE Research Online

THE LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE

Gharad Bryan, Dean Karlan and Jonathan Zinman

Referrals: peer screening and enforcement in a consumer credit field experiment

Article (Accepted version) (Refereed)

Original citation:
Bryan, Gharad, Karlan, Dean and Zinman, Jonathan (2014) *Referrals: peer screening and enforcement in a consumer credit field experiment.* American Economic Journal: Microeconomics . ISSN 1945-7669 (In Press)

© 2014 American Economic Association

This version available at: http://eprints.lse.ac.uk/59009/
Available in LSE Research Online: August 2014

LSE has developed LSE Research Online so that users may access research output of the School. Copyright © and Moral Rights for the papers on this site are retained by the individual authors and/or other copyright owners. Users may download and/or print one copy of any article(s) in LSE Research Online to facilitate their private study or for non-commercial research. You may not engage in further distribution of the material or use it for any profit-making activities or any commercial gain. You may freely distribute the URL (http://eprints.lse.ac.uk) of the LSE Research Online website.

This document is the author's final accepted version of the journal article. There may be differences between this version and the published version. You are advised to consult the publisher's version if you wish to cite from it.

http://eprints.lse.ac.uk

Referrals: Peer Screening and Enforcement in a Consumer Credit Field Experiment

Gharad Bryan, Dean Karlan and Jonathan Zinman*

May 31, 2014

Abstract

Empirical evidence on peer intermediation lags behind many years of lending practice and a large body of theory in which lenders use peers to mitigate adverse selection and moral hazard. Using a simple referral incentive mechanism under individual liability, we develop and implement a two-stage field experiment that permits separate identification of peer screening and enforcement effects. We allow for borrower heterogeneity in both ex-ante repayment type and ex-post susceptibility to social pressure. Our key contribution is how we deal with the interaction between these two sources of asymmetric information. Our method allows us to identify selection on the likelihood of repayment, selection on the susceptibility to social pressure, and loan enforcement. We estimate peer effects on loan repayment in our setting, and find no evidence of screening (albeit with an imprecisely estimated zero) and large effects on enforcement. We then discuss the potential utility and portability of the methodological innovation, for both science and for practice.

JEL Codes: C93 D12 D14 D82 O12 O16

*Bryan: London School of Economics and Innovations for Poverty Action, g.t.bryan@lse.ac.uk. Karlan: Yale University, M.I.T. Jameel Poverty Action Lab, Innovations for Poverty Action and NBER, dean.karlan@yale.edu. Zinman: Dartmouth College, M.I.T. Jameel Poverty Action Lab, Innovations for Poverty Action and NBER, jzinman@dartmouth.edu. The authors would like to thank Manfred Kuhn and the employees of Opportunity Finance, Luke Crowley, Jon de Quidt and seminar participants at Yale, NEUDC, Warwick, Hebrew University, Stanford GSB, Wharton, The NBER summer institute and The Cambridge conference on consumer credit and bankruptcy. We would also like to thank The Bill and Melinda Gates Foundation for funding. This paper formed the third chapter of Gharad Bryan's dissertation – he would like to thank The Kauffman Foundation for financial support. All errors are, of course, our own.

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 unmet demand for additional credit from consumers, microenterprises and small and medium enterprises.[1] These studies, coupled with a literature that often finds high returns to capital (e.g., De Mel et al. 2008), lend credence to policy and programmatic efforts to relax borrowing constraints.

But how should one go about relaxing 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 a route to greater credit market efficiency.

One popular approach to tackling asymmetric information is based on the presumption that a borrower's peers can be harnessed to provide information/screening or enforcement that is unavailable to (or more costly for) the lender.[2] Peer-intermediation 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. Peer intermediation has also been analyzed in a large theoretical literature on optimal mechanism design in the face of different asymmetric information problems (e.g., Varian 1990, Stiglitz 1990, 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 has, however, lagged behind both theory and practice. Yet we need empirical tests of theories not just to inform and build on our theoretical models of credit markets, but for practical concerns to inform policy. Specifically, with more precise information on the absolute and relative importance and leverage of screening and enforcement, one can design better contracts that improve credit market efficiency. For example, if peers are able to provide high quality screening but weak enforcement, a mechanism that uses joint liability to select clients for a first loan but then moves to individual liability would harness much of the screening incentive – á la Ghatak (1999) – but minimise the possible negatives of joint liability lending such as tipping into strategic default (Besley and Coate 1995) and sub-optimal risk-taking (Fischer 2010 and Giné et al. 2011). But if enforcement is largely responsible for the success of peer schemes, then the appropriateness of peer mechanisms will decrease as ex-post enforcement is strengthened through debt collection, legal remedies or better verification of identities.

We formalize and implement a field experiment design that separately identifies peer screen-

[1] For consumers see e.g., Karlan and Zinman (2010) for microenterprises see e.g., Banerjee (2013) and the references therein, and for SMEs see e.g., Banerjee and Duflo (2004).

[2] Throughout the paper we use the term enforcement to refer to a set of actions that might include monitoring, peer pressure, mutual insurance or direct peer assistance.

ing and peer enforcement effects under weaker assumptions than previous work on asymmetric information. Our key innovations concern the interaction between hidden information and hidden action. Our framework allows borrowers to differ both with respect to their ability to repay (their repayment type) and their susceptibility to social pressure (their malleability type). Heterogeneity with respect to malleability raises the possibility of selection on malleability – that peers select based on their ability to enforce repayment – as well as the possibility that repayment type is correlated with malleability type. We show that these issues complicate the identification of selection on repayment type – the type of selection usually associated with peer mechanisms following Ghatak (1999) – but that our experimental design overcomes these difficulties. The possibility that estimates of the extent of hidden information (that can be remedied by peer screening) depend on the extent of hidden action effects (that can be remedied by enforcement) occurs in many contract design settings. Relative to other papers that have attempted to separate selection and enforcement in a variety of settings (e.g. Karlan and Zinman 2009 and Einav et al. 2013) our experimental design allows us to identify selection without having to assume a zero correlation between ex-ante type and ex-post responsiveness to incentives. Estimates of the extent of selection on malleability are also important for contract design. Positive selection on malleability implies that the success of peer enforcement will depend on the amount of selection induced by the contract. Further, under some reasonable assumptions, selection on repayment type and selection on malleability will be substitutes, opening up the possibility that the extent of selection on repayment type will be determined by the lender's choice of enforcement technology.

Since theory, practice, and empirical work all suggest that information environments and the effectiveness of different remedies will vary across settings,[3] our experiment is designed to be portable: simple, and low-cost, to implement in different settings. The hope is that that this will allow the experiment to be used to tailor contract design to the specifics of particular markets. We demonstrate the experiment in one setting in conjunction with Opportunity Finance South Africa ("Opportunity"), a consumer lender located in Kwazulu Natal and a member of the Opportunity International microfinance network.

Opportunity offered existing clients a 100 Rand ($12) bonus for referring a new borrower who met particular criteria for Opportunity's individual liability loan. Opportunity first randomly divided referrers into one of two ex-ante (prior to referral being made) incentives: referrers in the ex-ante approval group were told that they would receive the bonus if the person they referred was approved for a loan. Referrers in the ex-ante repayment group were told that they would receive the bonus if the person they referred repaid a loan on time. Referrers in the ex-ante repayment treatment 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 group only had an incentive to refer someone they thought would be approved for a loan.

Ex-post (i.e. after the referral had been made), Opportunity randomly surprised some referrers

[3] E.g., Besley and Coate (1995) predicts that the success of peer lending will depend on the strength of "social collateral" in the community. For empirical evidence compare Giné and Karlan (2010) with Carpena et al. (2010).

whose referred applications had been approved with an improvement to their bonus contract.[4] 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.[5]

The design produces four groups of referrers, each with a different combination of ex-ante and ex-post incentives, as illustrated in Figure 1. Throughout we denote the ex-ante treatment group (i.e. what the referrer was promised prior to making the referral) with a lower case letter and the ex-post treatment group (i.e. what the referrer was given after making the referral) with a capital letter. So someone in group aR was promised an approval incentive, but also given a repayment bonus.

In Section 4 we provide a formal model of the referral decision and use it to show how the design can separate peer enforcement from selection on repayment type and selection on malleability. Intuitively, enforcement is measured by comparing repayment rates in different rows and selection by comparing repayment rates across columns. But selection on what? Two additional comparisons permit separate identification of selection on repayment type and selection on enforcement malleability. First, comparing repayment rates across columns only in the top row measures selection in the absence of an enforcement incentive. This comparison reveals the extent of selection on repayment type even if repayment type and malleability are correlated. Second, the design produces two estimates of the size of the enforcement effect, one with a selection incentive (comparing rows conditional on being in the right column) and one without (comparing rows conditional on being in the left column). Comparing these two estimates gives us a measure of selection on malleability.

We also show when our experiment can determine whether or not referrers have information about repayment type. This is a slightly more general question – rather than asking whether the experiment induced selection, we ask whether the group of referrers has information. (Referrers might have information but not reveal it if our mechanism was ineffective.) Guided by an extension to our model (Appendix B), we show in the empirical section that our repayment incentive induced referrers to substitute away from referring family toward referring friends. If this substitution increases the cost of making the referral, and selection on malleability is not too strong, then we can infer that referrers at least *attempted* to refer high repayment types. If this attempt was unsuccessful– if we find no evidence of *selection* on repayment type– we infer that referrers have a (noisy) belief that they have *information* about repayment type, but that in practice this information was already captured by the lender.

Results from our demonstration show strong enforcement effects. By comparing repayment rates across ex-post incentives holding the ex-ante incentive constant, we find that the small bonus

[4]Lenders frequently contact borrowers with promotions in this market.

[5]The lender also contacted those referrers for whom the contract was not changed and reminded them of the existence of the contract. This was to dampen any attention or signalling effects.

Figure 1: 2 × 2 Experimental Design – Repayment Incentives

		Ex-Ante Incentive: Bonus Promised	
		Approval	Repayment
Ex-Post Incentive: Bonus Given	Approval	No Enforcement or Screening Incentive (aA)	Screening Incentive (rA)
	Repayment	Enforcement Incentive (aR)	Screening and Enforcement Incentives (rR)

Bonus given can differ from bonus promised because, after offering the initial bonus ex-ante (prior to the referral), the lender later calls each referrer ex-post (after the referral is made) and randomly pleasantly surprises those in the aR and rA cells with a change to their incentives. Incentives refer to repayment incentives. All referrers have an incentive to refer a friend that will be approved for a loan.

(100 Rand is equal to about 2% of the average referrer's gross monthly income and 3% of the average loan size) decreased default from around 20% to 10% in most specifications.[6] The magnitude of improvement in repayment performance is far above and beyond what referrers and borrowers could accomplish with side-contracting, and the improvement (and savings in collection costs) far exceeded the lender's outlays for bonuses. Indeed, the lender continues to use the repayment incentive referral post-experiment.[7]

We do not find strong evidence of selection effects. Comparing repayment rates across ex-ante incentives, holding the ex-post incentive fixed, we find no evidence that peer selection on repayment type improved repayment, although this is an imprecise zero. Finally, we are unable to reject that there is no selection on malleability in our setting, although this is again an imprecisely estimated zero with confidence intervals that contain economically meaningful selection effects.

We make four main contributions relative to the existing literature. First, our experiment introduces peer influence in a simple individual liability setting where there is limited potential for complicating strategic interactions.[8] This makes identification possible under plausibly weaker assumptions than required in joint liability settings.[9] Second, we use a two-stage experiment to

[6]The emphasis of this paper is on designing contracts that encourage repayment so that the lender can relax borrowing constraints. From this perspective the lower default rate is positive. However, it is plausible that overall welfare is decreased if the social pressure is excessive. So we do not attach a welfare interpretation to any of our results.

[7]Besides the enforcement effect, another benefit for the lender is that referrers select on observables: referred borrowers are much more likely to be approved for a loan (55%) than non-referred borrowers (23%).

[8]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.

[9]For example, Ahlin and Townsend (2007) identifies selection effects only under the assumption that their model correctly captures the strategic situation.

identify selection and enforcement separately, in a peer referral environment.[10] The portability of the experimental design can help build evidence across settings and contexts. Theory predicts that peer intermediation will have effects that vary with, e.g., the lenders' ability to screen and enforce, the social environment and the heterogeneity of returns. Tests using this experimental design across such settings can help build a robust and empirically-validated theoretical framework. Third, as discussed above, we advance the study of selection on moral hazard by developing a treatment in which there is no enforcement incentive.[11] Fourth, we provide evidence on the extent of selection and enforcement effects in a particular setting, and highlight the potential for peer referral contracts to work in practice.

The paper also provides additional evidence on the presence of asymmetric information problems in developing-country credit markets. Our enforcement results imply the existence of moral hazard, similar to the results in Karlan and Zinman (2009), Giné et al. (2012), De Janvry et al. (2010) and Karlan et al. (2012). Our lack of a strong screening result -may- imply that there is little adverse selection in the market, again similar to findings in the existing literature. However, we emphasize that there are alternative interpretations for this null result. Our confidence intervals include economically meaningful selection effects. It could also be that there is in fact selection on hidden information, but that peers do not have any better access to this hidden information than the lender.

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 micro-loan 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 averaged around 3500R ($US400), with a modal (mean) duration of 9 (10) months, and a modal (mean) monthly percentage rate of 5% (4.1%). There is a competitive market for these loans in Kwazulu Natal (see Karlan and Zinman

[10] Beaman and Magruder (2012) conduct a peer referral experiment in the labor market and use it to show the presence of selection effects. They do not consider enforcement, nor the equivalent of selection on malleability.

[11] Recent work by Gunnsteinsson (2012) on crop insurance provides an example of how non-enforcement incentives can be applied in other product markets.

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).[12] The referred earned R40 ($US5) if she brought in the card and was approved for a loan. The referrer could earn R100 ($US12)[13] 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.[14] Referrers given the ex-ante repayment incentive would be paid only if the referred successfully repaid a loan.[15] Figure 2 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.

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.[16] Opportunity phoned referrers in the additional-incentive arm and told them that, in addition to the R100 approval bonus, they would receive an additional R100 if the referred repaid the loan. Opportunity also phoned the other half of referrers in the ex-ante approval incentive group– those who did not get the additional ex-post repayment incentive– with a remninder to pick up their R100 bonus. The phone call did not provide any new information on the incentive contract to this second arm, but we wanted every referrer to get a call from Opportunity in case the personalized contact from the lender had some

[12]Note that each referrer received only one card. Had we given each referrer many cards we could have increased power for the repayment experiment, but the lack of scarcity of cards to give out would have made it costless to make a referral and thus removed the possibility of studying selection.

[13]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.

[14]Because the bonus was conditional on approval there is the possibility that some referred clients were approved for a loan and then did not take out a loan. This does not occur in the data.

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

[16]This treatment design generates an additional difference between the treatment group and those receiving only the ex-ante incentive: the referrer receives two payments. Each payment is equivalent to a about a days wage. As long as this does not generate an income effect, it should not affect the interpretation of our results.

Figure 2: Referral Cards

effect. Importantly, while opportunity was aware of the assignment to treatment, no change was made to the intensity of enforcement in response to the incentives, and individual staff members responsible for enforcement action were not directly aware of the treatment assignment.

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 arm, 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 ex-ante repayment incentive 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 1 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. Two additional comparisons permit separate identification of selection on repayment type and selection on enforcement malleability. First, comparing repayment rates across columns only in the top row measures selection in the absence of an enforcement incentive. This comparison reveals the extent of selection on repayment type even if repayment type and malleability are correlated. Second, the design produces two estimates of the size of the enforcement effect, one with a selection incentive (comparing rows conditional on being in the right column) and one without (comparing rows conditional on being in the left column). Comparing these two estimates gives us a measure of selection on malleability.

4 Identification

In this section we provide a stylised model of the referral decision to help clarify the interpretation of our experiment. We provide the simplest model possible to discuss the interaction between selection on repayment type, selection on malleability, and enforcement. The purpose is not to provide new theory or derive new predictions, but rather to formalize what can be identified by the experimental treatments. In particular, we clarify the assumptions needed to interpret the experiment as separating out selection and enforcement effects. We outline the basic model in Section 4.1. Section 4.2 then relates the model to the data collected in the experiment. Section 4.3 interprets the different repayment rates induced by the experiment, giving formal definitions of selection on repayment type and selection on malleability and then discussing how these are identified by the experiment. Appendix B analyses the extent to which the experiment allows us to determine whether referrers have information about their peers' repayment types, even if the lender also has this information.

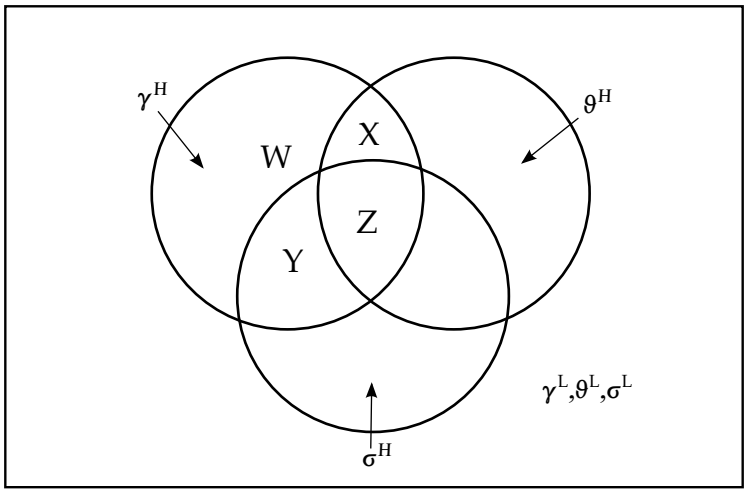
4.1 The Referral Decision

A market consists of N potential borrowers (referreds), each of whom is characterised by four parameters:

1. $\theta \in \{\theta^L, \theta^H\}$: a repayment type;
2. $\sigma \in \{\sigma^L, \sigma^H\}$: a malleability type;
3. $\gamma \in \{\gamma^L, \gamma^H\}$: the probability that the individual is approved for a loan; and
4. $\lambda \in (0, \bar{\lambda}]$: the net cost of recruiting the individual to the referral scheme, including all social and search costs and/or benefits, but excluding the referral bonus.

We assume that these characteristics are drawn from a population distribution F, and allow for characteristics to be correlated. In particular, it may be that repayment type is correlated with

Figure 3: Venn Diagram Showing the Distribution of Potential Referreds by Repayment Type (θ), Malleability Type (σ), and Approval Type (γ)



malleability; we discuss below how this would affect identification. We concentrate on the case with binary types for ease of exposition.[17] Figure 3 provides a simple diagram showing the potential distribution of repayment, malleability and approval types. It will be helpful in the discussion below.[18]

We assume that referrer j knows the cost of referral λ_{ij}[19] and holds beliefs $(\hat{\theta}_{ij}, \hat{\sigma}_{ij}, \hat{\gamma}_{ij})$ about each potential referred i. We further assume that each of these beliefs is binary. So, for example, $\hat{\theta} \in \{\hat{\theta}^L, \hat{\theta}^H\}$ with the interpretation that $\hat{\theta}_{ij} = \hat{\theta}^H$ implies that referrer j believes potential referred i to be a high repayment type.

The timing of the referral decision is as follows:

1. Referrer j chooses a referred i and pays a cost λ_{ij} (or chooses not to make a referral);
2. The referred is approved for a loan with probability γ_i;
3. The referrer applies social pressure e to encourage repayment and incurs cost $c(e)$, where c is a strictly convex cost function;
4. The loan is repaid with probability $\theta_i + \sigma_{ij}e$.[20]

[17] Allowing for more types does not alter the thrust of the arguments. However, we comment in the text where allowing for a richer type space would lead to a slightly different interpretation.

[18] We leave out correlation with λ as this is assumed to be continuous and it leads to a very complicated diagram.

[19] This last assumption seems reasonable given that we assume the cost is paid prior to the referral. Beliefs could be derived from a more primitive model in which the referrer has a prior belief about types, receives a signal and uses Bayes' rule to form the beliefs $(\hat{\theta}_{ij}, \hat{\sigma}_{ij}, \hat{\gamma}_{ij})$.

[20] σ is potentially specific to the referrer-referred match, and we allow for that in our notation.

We assume that $\{\theta^H, \theta^L\}$ and $\{\sigma^H, \sigma^L\}$ are such that the probability of repayment always lies between zero and one given an optimal effort choice. We could allow for a more complicated repayment function without altering our discussion. An advantage of the linear form is that σ encodes all the information needed to determine the impact of enforcement effort.

In making the referral decision, a referrer takes into account the possibility that she will exert pressure on the referred to repay the loan. We therefore consider the decision backward, first analysing the effort decision and then returning to the referral decision.

Consider first the effort decision. If the referrer is in the ex-ante approval treatment she knows she will exert no effort. If the referrer is in the ex-ante repayment treatment she foresees choosing her level of effort to solve

$$\max_e \left\{ B_R(\hat{\theta} + \hat{\sigma} e) - c(e) \right\}$$

where B_R is the bonus payment in the repayment treatment. We denote the unique maximiser $e^*(\hat{\sigma})$ which depends on $\hat{\theta}$ only through a correlation between $\hat{\sigma}$ and $\hat{\theta}$. For a borrower of type (θ, σ) perceived to be of type $(\hat{\theta}, \hat{\sigma})$ the repayment probability is $\theta + \sigma e^*(\hat{\sigma})$.

Now consider the referral decision. Given the effort decision discussed above, referrer j in the ex-ante approval group chooses who to refer by solving

$$U_j(A) \equiv \max_{i \in N_j} \left\{ \hat{\gamma}_i B_A - \lambda_{ij} \right\} \tag{1}$$

where B_A is the bonus paid in approval treatment. Because the bonus in the ex-ante approval treatment is expected to arrive earlier in time (immediately after approval) $B_A \geq B_R$. In the ex-ante repayment group referrer j solves

$$U_j(R) \equiv \max_{i \in N_j} \left\{ \hat{\gamma}_i \Big(B_R(\hat{\theta} + \hat{\sigma} e^*(\hat{\sigma})) - c(e^*(\hat{\sigma})) \Big) - \lambda_{ij} \right\}. \tag{2}$$

Equation (2) makes clear that the selection decision depends on both the belief about repayment type *and* the belief about malleability. Finally, the model is completed by noting that referrer j makes a referral in the ex-ante repayment treatment if $U_j(R) > 0$ and in the approval treatment if $U_j(A) > 0$.

4.2 Map from Model to Data

Our analysis focuses on the repayment behaviour of individuals referred under different referral incentives. We denote the ex-ante (prior to referral being made) treatment assignment with a lower case letter and the ex-post (after the referral is made) assignment with an upper case letter. So, for example, $E(\lambda|aR)$ is the expectation of λ conditional on the referrer being in the ex-ante approval group and ex-post repayment group, while $E(\lambda|a)$ is the expectation of λ conditional on being in the ex-ante approval group, unconditional on ex-post treatment assignment.

We have two potential measures of selection. Define $S(A)$ as the difference in repayment

probabilities across ex-ante treatments conditional on being in the ex-post approval treatment (where there is no enforcement incentive). $S(A)$ is given by the expression

$$S(A) = E(\theta|rA \ \& \ app) - E(\theta|aA \ \& \ app),$$

where app implies that the observation is conditional on the referred individual being approved for a loan, and expectations are taken over the population of referrers. We can also define $S(R)$ as the same comparison conditional on being in the ex-post repayment treatment:

$$S(R) = E(\theta + \sigma e^*(\hat{\sigma})|rR \ \& \ app) - E(\theta + \sigma e^*(\hat{\sigma})|aR \ \& \ app).$$

We also have two potential measures of enforcement. We define

$$N(r) = E(\theta + \sigma e^*(\hat{\sigma})|rR \ \& \ app) - E(\theta|rA \ \& \ app),$$

as the different in repayment rates across enforcement treatments conditional on the ex-ante repayment incentive. We can also define the equivalent measure conditional on being in the ex-ante approval treatment

$$N(a) = E(\theta + \sigma e^*(\hat{\sigma})|aR \ \& \ app) - E(\theta|aA \ \& \ app).$$

4.3 Identifying Selection and Enforcement

The identification of enforcement effects is straightforward if the ex-post treatment does not affect the referral or approval decision. This is a reasonable assumption, and a testable one (Section 5.2), given that the referral and approval decisions occur without the referrer or lender staff knowing the ex-post treatment assignment. Under this assumption, repayment type cancels out of the expressions for both $N(a)$ and $N(r)$ so that $N(a) = E(\sigma e^*(\hat{\sigma})|aR \ \& \ app)$ and $N(r) = E(\sigma e^*(\hat{\sigma})|rR \ \& \ app)$. Therefore each of the expressions provides an estimate of the extent to which social pressure can encourage loan repayment: our experiment generates two estimates of the peer enforcement effect. These estimates may differ if malleability differs across the ex-ante treatments.

We now turn to identification of selection, focusing first on repayment type and then on malleability.

Definition 1. *[Selection on Repayment Type] We say that our repayment incentive induced selection on repayment type if*

$$E(\theta|r \ \& \ app) > E(\theta|a \ \& \ app).$$

That is, on average, the set of clients referred under the ex-ante repayment incentive, and subsequently approved for a loan, are more likely to repay than those referred under the ex-ante approval incentive.

In terms of Figure 3 the probability of repayment conditional on approval is proportional to

the number of referreds in areas X and Z (i.e. those that will be approved and have a high repayment type) relative to area W and Y (i.e. those that will be approved, but have a low repayment type). We denote this ratio XZ/WY. There is selection on repayment type if XZ/WY is larger with the ex-ante repayment incentive than with the ex-ante approval incentive. We use average repayment rates because they are relevant to the firms profitability and, therefore, its contract design choice.[21]

We use $S(A)$ to identify selection on repayment type, keeping in mind two issues. One is whether $S(A)$ identifies repayment type per se (Definition 1), given that referrers in the rA group have incentives to refer borrowers with relatively high malleability and referral cost. Figure 3 illustrates the issue with respect to malleability: the ex-ante repayment incentive may not only change the ratio XZ/WY, but also the ratios X/Z and W/Y. The key assumption under which $S(A)$ identifies selection on repayment type is that referrers in treatment group rA do not exert enforcement effort: given this assumption, the repayment probability is given by θ^H for referrers in areas X and Z, and θ^L for those in areas W and Z, and the difference XZ/WZ captures differences in repayment type.[22] Note that $S(A)$ itself does not reveal anything about other characteristics of referreds besides repayment type. Below we discuss how we can use $S(A)$ in conjunction with $S(R)$ to identify malleability. The second issue concerns the referrers intent. Suppose that referrers make referrals solely on the basis of malleability type, but malleability type happens to be positively correlated with repayment type. Definition 1 includes this kind of incidental selection; even though it is unintentional on the referrers part, it is relevant for the lender and the lender may intentionally design a contract to induce this kind of selection. The question of referrer intent does highlight the distinction between identifying selection and identifying whether referrers have information per se about repayment type. We pursue this distinction in Appendix B.

Turning to $S(R)$, we now clarify that this comparison only identifies selection on repayment type under stronger assumptions. Figure 3 illustrates that WX/YZ may be smaller in the ex-ante repayment group indicating that this group has more high malleability types. Although this would not affect $S(A)$ because $e = 0$, it would affect $S(R)$ where $e \neq 0$, both in theory and as shown empirically below. If the effectiveness or extent of social pressure depends on the repayment type (in our model this would occur through a correlation between repayment type and malleability type) then $S(R)$ could over- or under-estimate the extent of selection on repayment type. Moreover, theory does not pin down the sign of the correlation between repayment type and malleability. One intuition suggests that there is less scope for social pressure on those who are predisposed to repay: $corr(\hat{\sigma}, \hat{\theta}) < 0$ and $corr(\sigma, \hat{\theta}) < 0$. So in this case high types would be less malleable. Social pressure might even be counterproductive if high repayment types are intrinsically motivated and external pressure crowds out intrinsic motivation (e.g. Gneezy and

[21] We condition on approval because the approval decision is necessary in credit (and insurance, and labor) markets. But we need not condition on approval: the extent of selection on repayment type under our definition will be proportional to the extent of selection on repayment type under the unconditional definition, so long as we are willing to assume that the probability of approval is strictly increasing in repayment type. This seems like a reasonable assumption in most settings.

[22] Repayment probability is defined as $\theta + \sigma e$as discussed above. As a consequence when $e = 0$ the repayment probability is θ.

Rustichini 2000, Benabou and Tirole 2003 & Besley and Ghatak 2005). A second intuition, however, suggests that high types will be more malleable; e.g., they already care the most about repaying and hence will also care most about how they are viewed by their peers. If this is the case we would expect that $corr(\hat{\sigma}, \hat{\theta}) > 0$ and $corr(\sigma, \hat{\theta}) > 0$.[23]

We refer to this issue as the comparability problem because $S(R)$ does not provide an apples-to-apples comparison: the two groups potentially differ in the extent of social pressure and malleability. The comparability problem occurs in settings where there is both hidden information, and hidden action that could depend on the extent of hidden information.

We now consider selection on malleability.

Definition 2. *[Selection on Malleability] We say that our incentive induced positive (negative) selection on malleability if*

$$E(\sigma e^*(\hat{\sigma})|rR \,\&\, app) > (<) E(\sigma e^*(\hat{\sigma})|aR \,\&\, app).$$

That is, on average the set of clients referred under the ex-ante repayment incentive, and subsequently approved for a loan, are more (less) susceptible to the chosen social pressure of their referrer than those referred under the ex-ante approval incentive.

In terms of Figure 3 we say that there is selection on malleability if YZ/WX depends on the treatment assignment.[24] We condition on the optimal choice of social pressure in the ex-post repayment treatment. An alternative would be to define positive (negative) selection on malleability to be

$$E(\sigma|r \,\&\, app) > (<) E(\sigma|a \,\&\, app).$$

This may seem a more natural definition, but a more malleable set of referrers is not helpful to the lender if the referrers do not in fact put the appropriate pressure on these referrers.

Again assuming that the ex-post treatment does not affect the referral or approval decision we have

$$S(R) - S(A) = E(\sigma e^*(\hat{\sigma})|rR \,\&\, app) - E(\sigma e^*(\hat{\sigma})|aR \,\&\, app).$$

Therefore, $S(R) - S(A) > (<) 0$ implies positive (negative) selection on malleability.[25]

In Appendix B we use the model to show that if $E(\lambda|r) > E(\lambda|a)$, so that the cost of referrals in the ex-ante repayment treatment is higher than in the ex-ante approval treatment, and $S(A) = 0$

[23] In the more familiar setting of moral hazard and adverse selection in the credit market, Karlan and Zinman (2009) provide a simple model that implies high types put more effort into ensuring project success. If that model is true, correlation between hidden information and the extent of hidden action would tend to lead to an underestimate of the extent of selection on risk type. Even in that case, however, it is possible to tweak the simple model and reverse the comparative static. The extent and direction of these interaction effects is, therefore, an empirical matter.

[24] As above, we condition on approval and look at average repayment rates as we believe that this is the most appropriate measure when considering how to design contracts.

[25] There is a meaningful distinction between intentional and incidental selection on malleability if referrers are not forward-looking or do not actually have a good signal of malleability type. Although these contingencies seems unlikely to arise, we note two points. First, it is the possiblity of incidental selection on malleability– in particular of high repayment types being less malleable– that leads to the possibility of negative selection on malleability. Second, observing $S(A) < 0$ and $S(R) > 0$ is only possible if there is positive intentional selection on malleability, and if selection on repayment type and selection on malleability are substitutes.

then we can infer that referrers do not have information about repayment type, conditional on the information used by the lender for approval. The intuition is simple: if the referral is more costly in the ex-ante repayment group, then we can infer that referrers tried to chose someone who is likely to repay. But if $S(A) = 0$, there is no effect of the referrers' selection, and we can infer that referrers failed in their attempt to screen on repayment type. Appendix B clarifies some additional assumptions required to complete the argument.

5 Data

5.1 Summary Statistics

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

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		4408	

Disposable income is monthly income remaining after rent, debt repayments and recurring obligations. It is measured in Rand. 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 potential referrers– borrowers approved for new loans– during the study period.[26] Table 2 presents regressions of treatment assignment on referrer characteristics at baseline. If the randomization is valid, we would expect these characteristics to be uncorrelated with treatment. In all cases an F-test of the restriction that the coefficients are

[26] Referred clients were not eligible for a card. This restriction was aimed to reduce learning about the treatments among potential referreds.

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 treatment effect estimates are robust to including controls for referrer baseline characteristics. Finally, our main significant result is a large enforcement effect: this is unlikely to be driven by the imbalance as a lower application score for those in the repayment treatment would suggest a lower ability to repay.

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 referred and approved for a loan[27] should not differ within the ex-post treatment groups.[28] Table 3 presents tests this hypothesis from regressions where the outcome variable is being assigned to the ex-post repayment incentive.

Within the group given the ex-ante approval incentive (Table 3 Column 1), 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 (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 the change, with 9 coming from the ex-post approval group. This is not out of line with what we would expect from random arrival times, but it does create a problem in testing orthogonality. Columns 3 and 4 of Table 3 take two approaches. Column 3 omits the application score, and the p-value for the F-test of joint significance rises to 0.326 (from 0.077 in Column 2). Column 4 drops the 12 post-change observations (so the sample size falls from 120 to 108), and the p-value is 0.478. We also note that the ITC (external) credit score– which is provided by a credit bureau and is another key predictor of credit worthiness– is never predictive of treatment status.

Overall it seems that the randomization was successful. We also show below that our results are not sensitive to including these baseline characteristics as controls.[29]

[27] Results are similar if we do not condition on approval, and instead consider the full sample of 430 referreds.

[28] 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 because the experiment aims to generate difference in these characteristics.

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

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.

Table 3: Testing The Balance of Referred Characteristics Across Ex-post Treatments. Dependent Variable 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	0.034	0.028	0.037	0.023
(Thousands of Rand)	(0.057)	(0.059)	(0.061)	(0.069)
Application Score/100	0.016	-0.071***	-	0.020
	(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	0.010	-0.027*	-0.026	-0.020
(Thousands of Rand)	(0.011)	(0.016)	(0.016)	(0.021)
Requested Term	-0.013	0.014	0.010	0.012
(Months)	(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.

6 Results

6.1 Who Refers and Who Gets Referred?

430 of 4408, or 10%, of eligible clients made a referral. Making a referral is not correlated with the ex-ante incentive: a regression of the referral rate on a dummy for ex-ante treatment gives a coefficient of -0.002 with a standard error of 0.040. This suggests that the higher net present value of the bonus in the ex-ante approval group was not an important consideration in the referrers' calculations and did not induce greater search. And it bodes well for using the assumption that $E(\lambda|a) < E(\lambda|r)$ to help identify whether referrers have information (see Appendix B).

We also collected data on the relationship between referrers and referreds. Specifically, the lender asked the referred "How do you know the person that gave you the voucher?" 426 of our referreds answered this question, and most stated that the referrer was a relative or work colleague. A small number answered church. Table 4 shows the results from a multinomial logit of the impact of being in the ex-ante repayment group on the likelihood of referring a co-worker or someone from the same church, relative to referring a family member. The table shows that the selection incentive leads referrers to substitute away from relatives toward those they know through church and their work colleagues.

We argue that this finding implies that $E(\lambda|r) > E(\lambda|a)$. First, we believe that it is intuitive that colleagues and co-religionists are more costly to refer than family members. This is both because colleagues may be harder to approach and because there may be a positive obligation to refer family members given the R40 bonus given to the referred. Second, Beaman and Magruder (2012) find similar substitution toward referring colleagues in a labor market experiment in India. Their experiment also documents that referred colleagues are more productive workers than referred family members, which would only occur if colleagues are indeed more costly to refer. Consequently, following our analysis in Appendix B, if we can now show that $S(R) = S(A)$ and $S(A) = 0$ we will infer that referrers have no information.

We also find evidence that referred clients are more likely to be approved for a loan. 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%. We will argue below that referrers were not successful in selecting on repayment type. The observation that referred clients are more likely to be approved is, therefore, 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.[30] These two possibilities make it hard to give a causal interpretation to the correlation.

[30] It is also possible that the lender took into account whether the borrower was referred in making its approval decision. We find this unlikely for two reasons. First, the approval process is quite mechanical. Second, the lender was genuinely agnostic as to whether the referral system would work. This was one of the main reasons for implementing the experiment.

Table 4: The Impact of Ex-ante Treatment Group on Referred Characteristics: Multinomial Logit, Compared to Referring a Relative

	Same Church	Co-Worker
Ex-Ante Repayment Incentive	1.023**	0.434*
	(0.434)	(0.224)
Constant	-1.932***	0.686***
	(0.338)	(0.148)
N	426	

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.1$. Results from a multinomial logit. Ommitted category is referral of a relative. Standard errors in parentheses.

6.2 Measuring Repayment Performance

We identify screening and enforcement effects 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. 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.[31]

Each panel in Table 5 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 incentive fixed. These differences are our key results. Relating these back to Section 4.2 the right most cell in the top row of each table gives $S(A)$, the right most cell in the second row gives $S(R)$ the bottom cell in the first column gives $N(a)$ and the second column gives $N(r)$. Finally, the bottom right cell in each table gives $|S(R) - S(A)|$.

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

Table 5: 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.

6.3 Enforcement Effects

Table 5 provides eight estimates of the peer enforcement effect, two for each measure of default. The bottom cell in the first column of each sub-table shows an estimate of $N(a)$ and the bottom cell in the second column shows an estimate of $N(r)$. The point estimate for each of the eight differences is negative, suggesting that adding the ex-post repayment incentive decreases the incidence of default. Five of the eight estimates are statistically significant from zero, despite our small sample.

In each case the implied magnitude of the enforcement effect is large; e.g., an 11 percentage point reduction in chargeoff likelihood, on a base of 16%. The size of the impacts is also large given the size of the bonus (R100) and the average loan size. It seems unlikely that these results could be driven by referrers transferring R100 to the referred to encourage repayment. In all, the results suggest that the small referral incentive created social pressure that led to large reductions in default.

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. While we do not have the ideal data, we do have one bit of evidence from a similar context with which to do a back of the envelope calculation. 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 percentage point reduction in the interest rate (effectively making the interest rate on the next loan zero). This again suggests 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.

6.4 Selection Effects

Table 5 also provides eight estimates of the peer selection effect, two for each measure of default. Relating these back to Section 4.2, the rightmost cell in the top row of each table gives $S(A)$, and the rightmost cell in the second row gives $S(R)$. Each of the eight point estimates is statistically insignificant and *positive*, so there is no evidence that a small referral incentive induces screening on repayment type that reduces default. In fact, if we take the point estimates seriously they imply *negative* selection on repayment type. This outcome is consistent with our model if there is strong intentional positive selection on malleability. As discussed in Section 4.3, this could lead to the crowding out of the incentive to select on repayment type.

The bottom-right cell in each panel of Table 5 estimates the difference-in-differences (DD) across the two different estimates of the referral incentive effects on default rates. Recall from

Section 4 that if $S(A) = S(R)$ we can rule out selection on malleability. A zero estimate of the DD indicates that malleability is uncorrelated with ex-ante repayment type. And indeed none of the four estimates is significantly different than zero. This suggests that, in our setting, selection on malleability and the comparability problem are not that important. It bears emphasizing, however, that these are very imprecisely estimated zeros: each of the four confidence intervals includes economically large selection on malleability.

The DD estimate and the estimates of the selection effect are also relevant to determining whether referrers have information. In Section (6.1) we argued that $E(\lambda|r) > E(\lambda|a)$. This assumption, following the analysis of Appendix B, in combination with the findings above that $S(A) = 0$ and $S(R) - S(A) = 0$, suggest that referrers have no useful information for the lender (again subject to the above mentioned caveats on the distribution of repayment types and our wide confidence intervals). While this result should not be over interpreted (it requires quite strong assumptions) it is worth noting that the alternative approach to measuring whether people have information - getting people to bet on the loan performance of their friends - will not allow for the removal of enforcement effects.

6.5 Improving Precision and Checking Robustness

Under the assumption that malleability is uncorrelated with repayment type, which is born out by the above DD estimates, our model implies that we can estimate the extent of peer enforcement and selection with greater precision, using 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 with value 1 if client i was referred by someone with the ex-post repayment incentive, and $select_i$ is an indicator with value 1 if the client was referred by someone with the ex-ante repayment incentive. Results from this regression (without controls) are presented in Table 6. 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 5: there is a large enforcement effect, and no (or a perverse) selection effect.

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

7 Alternative Explanations and Other Considerations

In this section we discuss alternative interpretations of the results.

Table 6: 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.

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 evidenced 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 (Section 6.3), the enforcement effects here are large in comparison to effects from bonuses paid directly to the borrower.

7.2 Signaling

The repayment rates in Table 5 consistently show that the highest default rates occur for those clients who were in the ex-ante repayment group and 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.

Although we cannot completely rule out the lender perversely signaled referrers in the rA arm, three factors suggest that it is not a first-order concern. First, recall that we can estimate the enforcement effect and selection effect without relying on the rA arm. Second, recall that our estimates of selection and enforcement effects do not actually differ whether we use the rA arm or not: we do not find any evidence, statistically speaking, that estimates using the rA arm are any different, as they would need to be to be biased. (This inference is of course subject to the caveat that our confidence intervals are wide and hence any null result in this paper does not rule out large differences). Third, if the signaling story were correct the repayment rates for the referrer would also be affected, since the referred would only receive the perverse signal through the referrer (the lender does not directly provide the borrower with any information about referral incentives). But the difference between the default rates of the "signaled" and the "un-signaled" is not significantly different from zero for any of the four default measures (-0.060 ($p = 0.414$), -0.030 ($p = 0.473$), -0.019 ($p = 0.664$) and -0.006 ($p = 0.895$)): the data does not support the signaling story. If anything the point estimates suggest that the "signaled" were better repayers.

7.3 Impatience

Referrers who 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 matter much. 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 referrers 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; i.e., estimates using the ex-ante repayment group should show larger effects As discussed above, there is no evidence for this, albeit with large confidence intervals.

7.4 Reputation and Other Considerations

Most borrowers probably see themselves in an ongoing relationship with the lender. They may, therefore, value their reputation with the lender and this may give an incentive to enforce the loan even if they did not receive an ex-post repayment incentive. This could have two effects. First, it may weaken our ability to identify enforcement effects if the proportional effect of reputation on enforcement is larger in the ex-post approval than than the ex-post repayment arms. This is not a major concern in the current implementation, given that we find large enforcement effects in spite of any downward bias. Second, it could reduce our ability to identify selection effects if the effect

of reputation on screening is proportionally larger in the ex-ante approval than the ex-ante repayment arms. This would occur, for example, if reputation effects imply that all referrers engage in the maximal amount of selection. But, the fact that we see referrers move toward referring work mates in response to the selection incentive suggests that our incentive was effective in inducing an attempt at screening so we believe that the reputation effect cannot have been too strong.

Another issue arises because of the asymmetry of the incentives when comparing across ex-ante incentive holding fixed the ex-post repayment incentive - i.e. comparison $S(R)$. In this case, those in the aR group have already received a payment from the lender when they consider enforcement, while those that are in the rR group have not. This may mean that those in the aR group believe more strongly in the lender's willingness to pay, and may have a larger incentive to enforce. This effect will tend to lead us to underestimate any selection effect when using the comparison $S(R)$. This logic will not, however, affect the validity of the $S(A)$ comparison as a measure of selection on repayment type, but will weaken our ability to use the difference $S(R) - S(A)$ as a measure of selection on malleability.

A related issue arises from the timing difference in the offer of ex-ante and ex-post repayment incentives. Referrers in the rR group are notified of their incentive earlier than those in the aR group and may thus engage in different kinds of enforcement (e.g., influencing their referred's project choice). If this makes enforcement more effective in rR than aR, then $S(R)$ we will overestimate selection effects. But the impact of this potential bias is mitigated by the fact that we can and do use $S(A)$ instead of $S(R)$ to identify selection on repayment type. Note also that in practice we do not find statistically significant selection effects, meaning than any overestimation using $S(R)$ is not producing a false positive, at least in our setting.

8 Conclusions

We formalize and implement a field experiment design and show it is possible to identify peer selection and peer enforcement effects separately, even when peers select on malleablity to enforcement pressure. Novel features of the design include experimental arms without enforcement incentives (see also Gunnsteinsson (2012)), combinations of experimental arms that produce different estimates of selection and enforcement effects, and a combination that estimates the extent of selection on hidden action (malleability). We hope that our analysis of identification will pave the way for further progress in identifying interactions between hidden information and hidden action, using multi-stage experiments as well as other methods.

Our empirical results imply large peer enforcement effects. They also imply peer selection that is only partly effective: peers refer applicants who are likely to be approved, but do not end up producing any ex-ante information on repayment type that is useful to the lender on the margin (despite evidence suggesting that they exert at least some effort to select better repayment types).

Both theory and practice suggest that results may be different elsewhere – e.g., in settings that lack a well-functioning credit bureau, or with contracts that use larger referral bonuses – and

hence our experiment is designed to be portable. Our experiment can be layered on top of any individual liability lending operation, and could be modified to work with joint liability lending. Given the implications for increased profits for the lender, the results from our demonstration could help convince lenders (and firms in other product markets) that it is worth experimenting with referral mechanisms.

References

Ahlin, C. and R.M. Townsend, "Using repayment data to test across models of joint liability lending," *Economic Journal*, 2007, *117* (517), 1253–67.

Armendáriz, B., J. Morduch, and Inc ebrary, *The economics of microfinance*, MIT press, 2010.

Banerjee, A. and E. Duflo, "Do Firms Want to Borrow More? Testing Credit Constraints Using a Directed Lending Program," *CEPR Discussion Papers*, 2004.

Banerjee, Abhijit Vinayak, "Microcredit Under the Microscope: What Have We Learned in the Past Two Decades, and What Do We Need to Know?," *Annu. Rev. Econ.*, 2013, *5* (1), 487–519.

Banerjee, A.V. and A.F. Newman, "Occupational choice and the process of development," *Journal of Political Economy*, 1993, *101* (2), 274–298.

_ , T. Besley, and T.W. Guinnane, "Thy neighbor's keeper: The design of a credit cooperative with theory and a test," *The Quarterly Journal of Economics*, 1994, *109* (2), 491.

Beaman, Lori and Jeremy Magruder, "Who gets the job referral? Evidence from a social networks experiment," *The American Economic Review*, 2012, *102* (7), 3574–3593.

Benabou, R. and J. Tirole, "Intrinsic and extrinsic motivation," *Review of Economic Studies*, 2003, *70* (3), 489–520.

Besley, T. and M. Ghatak, "Competition and incentives with motivated agents," *The American economic review*, 2005, *95* (3), 616–636.

_ and S. Coate, "Group lending, repayment incentives and social collateral," *Journal of Development Economics*, 1995, *46* (1), 1–18.

Bond, P. and A.S. Rai, "Cosigned vs. group loans," *Journal of Development Economics*, 2008, *85* (1-2), 58–80.

Carpena, F., S. Cole, J. Shapiro, and B. Zia, "Liability Structure in Small-Scale Finance: Evidence from a Natural Experiment," *World Bank Policy Research Working Paper Series, Vol*, 2010.

Einav, Liran, Amy Finkelstein, Stephen P Ryan, Paul Schrimpf, and Mark R Cullen, "Selection on Moral Hazard in Health Insurance," *The American Economic Review*, 2013, *103* (1), 178–219.

Fischer, G., "Contract structure, risk sharing, and investment choice," *London School of Economics working paper*, 2010.

Galor, O. and J. Zeira, "Income distribution and macroeconomics," *The Review of Economic Studies*, 1993, *60* (1), 35–52.

Ghatak, M., "Group lending, local information and peer selection," *Journal of Development Economics*, 1999, *60* (1), 27–50.

_ **and T.W. Guinnane**, "The economics of lending with joint liability: theory and practice1," *Journal of development economics*, 1999, *60* (1), 195–228.

Giné, X. and D.S. Karlan, "Group versus Individual Liability: A Field Experiment in the Philippines," 2010. Working Paper.

Giné, Xavier, Jessica Goldberg, and Dean Yang, "Credit Market Consequences of Improved Personal Identification: Field Experimental Evidence from Malawi," Technical Report, National Bureau of Economic Research 2011.

_ **,** _ **, and** _ , "Credit market consequences of improved personal identification: Field experimental evidence from malawi," *The American Economic Review*, 2012, *102* (6), 2923–2954.

Gneezy, U. and A. Rustichini, "A Fine is a Price," *The Journal of Legal Studies*, 2000, *29* (1), 1–17.

Gunnsteinsson, Snaebjorn, "Information Assymmetries in Crop Insurance," 2012. mimeo.

Janvry, Alain De, Craig McIntosh, and Elisabeth Sadoulet, "The supply-and demand-side impacts of credit market information," *Journal of Development Economics*, 2010, *93* (2), 173–188.

Karlan, D. and J. Zinman, "Observing Unobservables: Identifying Information Asymmetries With a Consumer Credit Field Experiment," *Econometrica*, 2009, *77* (6), 1993–2008.

_ **and** _ , "Expanding credit access: Using randomized supply decisions to estimate the impacts," *Review of Financial Studies*, 2010, *23* (1), 433.

Karlan, Dean, Melanie Morten, and Jonathan Zinman, "A personal touch: Text messaging for loan repayment," Technical Report, National Bureau of Economic Research 2012.

Klonner, S. and A.S. Rai, "Cosigners as Collateral," *Journal of Development Economics*, 2010.

Mel, S. De, D. McKenzie, and C. Woodruff, "Returns to Capital in Microenterprises: Evidence from a Field Experiment*," *Quarterly Journal of Economics*, 2008, *123* (4), 1329–1372.

Rai, A.S. and T. Sjöström, "Is Grameen lending efficient? Repayment incentives and insurance in village economies," *The Review of Economic Studies*, 2004, *71* (1), 217.

Stiglitz, J.E., "Peer monitoring and credit markets," *The World Bank Economic Review*, 1990, *4* (3), 351.

_ **and A. Weiss**, "Credit rationing in markets with imperfect information," *The American economic review*, 1981, *71* (3), 393–410.

Varian, Hal, "Monitoring Agents With Other Agents," *Journal of Institutional and Theoretical Economics: JITE*, 1990, *146*, 153–174.

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:

$$y_i = \alpha_i + \beta T_i + \gamma X_i + \epsilon_i, \tag{3}$$

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 6 without controls.

B Do Referrers Have Information?

The discussion in the text shows what is identified by the the experiment. In this section we argue that we can use the experiment to understand whether referrers have information regarding repayment types. This is a more general question than asking whether there is selection on repayment type. For example, if we see no selection on repayment type it may be that referrers have information, but the experiment did not lead them to reveal that information. We will argue in this section and the empirical work that follows that the experiment did lead referrers to attempt to select high repayment types, but that this selection was (subject to power considerations) un-

successful. We interpret this as evidence that referrers in fact have no useful information – at least that can be extracted with incentives of the size we use. From a contract design perspective, this means that our experiment can be useful in ruling out a whole class of mechanisms, rather than just implying that our particular example is not effective in inducing selection.

Definition 3. *[Referrer Knowledge] We say that referrers have information about repayment type if*

$$E(\theta|\hat{\theta} = \hat{\theta}^H \ \& \ app) > E(\theta|\hat{\theta} = \hat{\theta}^L \ \& \ app). \tag{4}$$

As with the definitions of selection in the main text, this statement is conditional on loan approval: we are interested in whether referrers have information that predicts repayment above and beyond the information that the lender observes.

Inferring whether referrers have information can, but need not, be straightforward. If $S(A) > 0$ it must be that the referrers have information about repayment type. But if $S(A) \leq 0$ it is not immediately clear whether referrers have information. It may be that referrers have information, but that they have not been induced to reveal it. For example, our model allows for the possibility that referrers believe approval types $\hat{\gamma}$ and repayment types $\hat{\theta}$ to be highly correlated. In this case there may be no differential incentive to refer high types in the ex-ante repayment treatment. Alternatively, because $B_A \geq B_R$ referrers in the ex-ante approval treatment may be willing to pay a higher cost λ to make a referral. If λ is correlated with repayment type we may even see $S(A) < 0$, but this would not necessarily show a lack of information. Finally, it could just be that referrers tend to only have one potential referred.

We argue that if $E(\lambda|r) > E(\lambda|a)$, $S(A) = 0$, $S(R) = S(A)$ and $\hat{\sigma}$ is positively correlated with σ, then we can infer that referrers *do not* have information. To see this note that we can write

$$\begin{aligned} S(A) \equiv\ & Pr(\hat{\theta} = \hat{\theta}^H|r)E(\theta|\hat{\theta}^H \ \& \ \text{app}) + (1 - Pr(\hat{\theta} = \hat{\theta}^H|r))E(\theta|\hat{\theta}^L \ \& \ \text{app}) \\ & - \Big(Pr(\hat{\theta} = \hat{\theta}^H|a)E(\theta|\hat{\theta}^H \ \& \ \text{app}) + (1 - Pr(\hat{\theta} = \hat{\theta}^H|a))E(\theta|\hat{\theta}^L \ \& \ \text{app})\Big). \end{aligned}$$

Consequently, if the experiment induces selection in the sense that

$$Pr(\hat{\theta} = \hat{\theta}^H|r) > Pr(\hat{\theta} = \hat{\theta}^H|a), \tag{SELECTION}$$

then $S(A) = 0$ implies that referrers have no information. To complete the argument we need to show that $E(\lambda|r) > E(\lambda|a)$ and $S(R) - S(A) = 0$ imply SELECTION.

The condition $E(\lambda|r) > E(\lambda|a)$, combined with (1) and (2), implies that

$$E\Big(\hat{\gamma}\big(B_R(\hat{\theta} + \sigma e^*(\hat{\sigma})) - c(e^*(\hat{\sigma}))\big)|r\Big) > E\Big(\hat{\gamma}\big(B_R(\hat{\theta} + \sigma e^*(\hat{\sigma})) - c(e^*(\hat{\sigma}))\big)|a\Big). \tag{5}$$

This follows because there must be some expected return to making a high cost referral. Condition (5), combined with the assumption that c is strictly convex, then implies that $E(\hat{\theta} + \sigma e^*(\sigma)|r) > E(\hat{\theta} + \sigma e^*(\sigma)|a)$: that the ex-ante incentive induced selection on the probability of repayment

conditional on anticipated effort. To complete the argument, observe that if $S(R) = S(A)$ then $E(\hat{\theta} + \sigma e^*(\sigma)|r) > E(\hat{\theta} + \sigma e^*(\sigma)|a)$ implies $E(\hat{\theta}|r) > E(\hat{\theta}|a)$ so long as we are willing to make the weak assumption that $\hat{\sigma}$ is positively correlated with σ: that those perceived to be more malleable are, in reality, more malleable.

Finally, it is worth noting that our restriction to two repayment types may be restrictive in the case where $S(A) = 0$. Our experiment provides a limited incentive (B_R) to refer a good repayment type. It may be that this incentive induces SELECTION within a set of low-cost referreds, and that a larger incentive would induce selection across a larger group of potential referreds. For example, it may be that the referrers know people who are high repayment types, but that it is very costly to contact them and make the referral.

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	-0.188**	-0.184**	-0.166**
	(0.097)	(0.084)	(0.084)	(0.068)
Mean in Ex-post approval	0.389	0.206	0.186	0.155
	(0.064)	(0.054)	(0.054)	(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*	-0.115	-0.157*	-0.128*
	(0.107)	(0.084)	(0.091)	(0.076)
Mean in Ex-post approval	0.519	0.226	0.256	0.189
	(0.069)	(0.058)	(0.076)	(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**	-0.127**	-0.120**	-0.115**
	(0.034)	(0.045)	(0.039)	(0.046)
Mean in Ex-post approval	0.389	0.206	0.186	0.155
	(0.064)	(0.054)	(0.054)	(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**	-0.072*	-0.066	-0.098*
	(0.095)	(0.033)	(0.040)	(0.046)
Mean in Ex-post approval	0.519	0.226	0.256	0.189
	(0.069)	(0.058)	(0.076)	(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	0.046	0.035	0.027
	(0.041)	(0.035)	(0.041)	(0.031)
Mean in Ex-Ante Approval	0.389	0.206	0.186	0.155
	(0.064)	(0.054)	(0.054)	(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	0.009	0.018	0.028
	(0.100)	(0.080)	(0.075)	(0.063)
Mean in Ex-Ante Approval	0.258	0.095	0.076	0.047
	(0.054)	(0.037)	(0.039)	(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 6 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.