Observing Unobservables: Identifying Information Asymmetries with a Consumer Credit Field Experiment

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ABSTRACT

We estimate the prevalence of asymmetric information in a consumer credit market using a field experiment methodology derived from theoretical models. A major South African lender issued 58,000 direct mail offers that we randomized along three dimensions: 1) an initial "offer interest rate" issued via direct mail solicitations, 2) a weakly lesser "contract interest rate" revealed to borrowers who respond to the solicitation and agree to the initial offer rate, and 3) a dynamic repayment incentive that extends preferential pricing to borrowers who remain in good standing on their first loan taken at the contract rate. These three randomizations, combined with the large sample (including over 4,000 accepted offers) and complete knowledge of the Lender's information set, enable us to identify the prevalence and impacts of specific types of private information. Specifically, our setup distinguishes adverse selection from moral hazard effects on repayment, and thereby generates unique empirical evidence on the sources and magnitude of asymmetric information. We find evidence of both adverse selection and moral hazard. These effects are large, both economically and statistically, and help explain the prevalence of rationing even in a market that specializes in financing high-risk borrowers at very high rates.

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I. Introduction

Stiglitz and Weiss (1981) launched a cottage industry of theoretical papers on the role of asymmetric information in credit markets. Various models have shown that information frictions can lead to either underinvestment (Willam G. Gale, 1990, R. Glenn Hubbard, 1998) or overinvestment (Ben Bernanke and Mark Gertler, 1990), with attendant real effects at both micro and macro levels. Moreover, different varieties of information problems suggest radically different optimal policy (non)responses (Ben Bernanke and Mark Gertler, 1990).

Despite the proliferation and influence of these theoretical credit market models, empirical evidence on the existence and importance of specific information frictions remain relatively rare and inconclusive. Chiappori and Salanie (2000) find this to be true for contract theory in general, and for tests of adverse selection and moral hazard in particular.[1] Distinguishing between adverse selection and moral hazard is difficult even when precise data on underwriting criteria and clean variation in contract terms are available, as a single interest rate (or insurance contract) might produce independent selection and incentive effects.[2] Hence, even random variation in interest rates (Pierre-Andre Chiappori and Bernard Salanie, 2000) is not sufficient to decompose any reduced-form effect of the interest rate into its structural components.[3]

We test for the presence of distinct types of hidden information using a new methodology that disentangles selection from ex-post incentive effects on repayment, as well as two different types of ex-post incentive effects. Specifically, we designed a market field experiment that was implemented by a South African firm that specializes in making high-interest, unsecured term loans to employed, but primarily poor, consumers who do not have access to the formal banking sector. The experiment identifies information asymmetries by randomizing terms of the loan along three dimensions: first on the interest rate offered on a direct mail solicitation, second on the actual interest rate on the loan contract, and third on the length of the special interest rate (one loan versus all loans for one year).

A stylized example gets to the heart of our methodology: consumers are randomly offered a high or a low interest rate on a direct-mail solicitation. They then decide whether to borrow at the solicitation's "offer" rate. Of those that respond to the high rate, half are randomly given the low interest rate instead (the "contract" rate), while the remaining half continue to receive the high rate (i.e., the contract rate equals the offer rate). Any selection effect can then be identified by considering the sample that received the low contract rate, and comparing the repayment performance of those who responded to the *high offer* interest rate with those who responded to the *low offer* interest rate. This follows from the fact that although everyone in this hypothetical sample was randomly assigned identical contracts, they selected in at varying, randomly assigned rates, so any difference in repayment can be attributed to selection. Similarly, any effect of moral hazard (or more precisely of debt burden), can be identified by considering the sample that responded to the high offer interest rate. We simply compare the repayment performance of those who received the *high contract* interest rate to those who received the *low contract* interest rate. These borrowers selected in identically, but have randomly different interest rates on their contract, and we can thus attribute any difference in repayment to moral hazard or debt burden.

Our approach to estimating the extent and nature of asymmetric information is thus most similar in intent to Edelberg (2003), and in methodology to Ausubel (2003).[4] Edelberg estimates a structural model that

[1] Indeed, the 2001 Nobel Prize Committee's extended citation for pioneering work on asymmetric information did not cite any empirical work on credit markets, while citing six empirical papers on labor markets and four on insurance markets (Pierre-Andre Chiappori and Bernard Salanie, 2003).

[2] (Bank of Sweden, 2001) discuss this problem in the insurance market context.

[3] A related problem is that there may be different types of private information working in different directions; e.g., if there is advantageous selection combined with moral hazard. Finkelstein and McGarry (1999) tackle this problem in the long-term insurance market by combining contract information with supplementary data on insured preferences that is not observable to the insurer.

[4] At least two other papers endeavor to disentangle adverse selection from moral hazard in a credit market. Ahlin and Townsend (1999) do so using contract choice in Thailand, and find evidence consistent with adverse selection. Klonner and Rai (2003) do

attempts to disentangle the effects of adverse selection and one type of moral hazard (in effort) in collateralized U.S. consumer credit markets, and finds evidence consistent with both phenomena. Ausubel uses market experiments conducted by a large American credit card lender to estimate the extent and nature of adverse selection. He does not attempt to account for moral hazard separately, arguing that any such effect must be trivially small over the range of interest rates (800 basis points) contracted on in his data.

We find evidence of both moral hazard and adverse selection. These effects are large, both economically and statistically, and help explain the prevalence of rationing even in a market that specializes in financing high-risk borrowers at very high rates.

The paper proceeds as follows. Section II provides background on the South Africa consumer credit market and our cooperating Lender. Section III details the experimental design and implementation. Section IV presents a formal model of information asymmetries in credit markets. The experimental design allows for direct testing of this model. Section V maps the experimental design and related theory into a specific empirical strategy. Section VI presents the core results, based on the entire sample. Section VII explores heterogeneity and related refinements. Section VIII concludes with a brief discussion of implications, unresolved questions, and related ongoing work.

II. Market and Lender Overview

The consumer credit market in South Africa is distinct from most other developing countries in that there is a large, for-profit industry segment extending "cash loans" to individuals with verifiable employment (Leigh M. Drake and Mark J. Holmes, 1995). These lenders offer small, high-interest, short-term credit with fixed repayment schedules to a "working poor" population estimated to comprise anywhere from 2.5 million to 6.6 million people. Cash lenders arose to substitute for traditional "informal sector" moneylenders following deregulation of the usury ceiling in 1992, and they are regulated by the Micro Finance Regulatory Council (MFRC).

The working poor population lacks the credit history and/or collateralizable wealth needed to borrow from traditional institutional sources such as commercial banks. Loan sizes tend to be small relative to the fixed costs of underwriting and monitoring them, but substantial relative to borrower income; e.g., our cooperating Lender's median loan size of R1000 ($150) is 32% of its median borrower's gross mothly income. Not surprisingly, credit card and mortgage markets are extremely thin in South Africa (and other developing countries) compared to the U.S. Accordingly the cash loan market is important in the aggergate, with outstandings totaling 1% of GDP and 5% of non-mortgage consumer credit (David Porteous, 2003).

Cash loans are very short-term and expensive relative to credit card or mortgage rates in industrialized nations, although their terms compare favorably to informal sector substitutes in South Africa and elsewhere. Cash lenders focusing on the observably high-risk market segment typically make one month term loans at 30% interest *per month*. Lenders targeting observably lower risk segments may charge as little as 3% per month. Note there is essentially no difference between these nominal rates and corresponding real rates, since inflation continues to be relatively low in South Africa (e.g., 10.2% from March 2002-2003, and 0.4% from March 2003-March 2004). Rationing appears prevalent even in the face of these high rates; e.g., the Lender rejects 50% of new loan applicants.

The Lender has been in business since 1978 and is one of the largest micro-lenders in South Africa, with over 150 branches throughout the country. Our experiment took place in a mix of 86 urban and rural branches throughout the provinces of Kwazulu-Natal, Eastern Cape, Western Cape, and Gauteng. All loan underwriting and transactions are conducted face-to-face in the branch network, with the risk assessment technology combining centralized credit scoring with decentralized loan officer discretion.

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so using institutional features of rotating credit associations in India, and find evidence for adverse selection. Other papers estimating the prevalence of private information in credit markets include Calem and Mester (2004), Cressy and Toivanen (1995), Crook (2001), and Drake and Holmes (2002).

The Lender's product offerings are somewhat differentiated from competitors. Unlike many cash lenders, it does not pursue collection or collateralization strategies such as direct debit from paychecks, or physically keeping bank books and ATM cards of clients. The Lender is also unusually transparent in its pricing, with no surcharges, application fees, insurance premiums, etc., added to the cost of the loan. The Lender also has an unusual "medium-term" product niche, with a large concentration of 4-month loans (85%). Most other cash lenders focus on 1-month or 18-month loans.[5] The Lender's standard 4-month rates, absent this experiment, range from 7.75% to 11.75% per month, depending on observable risk.

III. Experimental Design and Implementation

We identify specific types of asymmetric information by integrating the random assignment of interest rates into the day-to-day operations of a consumer lender. This section outlines the experimental design and implementation, describes related data collection, and validates the integrity of the random assignments using several statistical tests. The experiment is implemented in a consumer credit market, but we believe the methodology is a general solution to the problem of disentangling distinct selection and moral hazard effects in a market setting.

The experiment was pilot tested in July 2003, and then fully executed in two waves in September and October 2003. We begin with a brief overview of the experiment, and then describe each step in detail below.

A. Design Overview

First the Lender randomized interest rates attached to "pre-approved," limited-time offers that were mailed to 57,533 former clients with good repayment histories.[6] Two rates were assigned to each client, an "offer rate" (r^o) included in the direct mail solicitation, and a "contract rate" (r^c) that was weakly less than the offer rate and revealed only after the borrower had accepted the solicitation and applied for a loan. For 60% of the clients, the contract rate was identical to the offer rate. Final credit approval (i.e., the Lender's decision on whether to offer a loan after updating the client's information) and maximum loan size were orthogonal to the experimental interest rates by construction. Therefore the two interest rate randomizations enable us to cleanly distinguish selection effects from moral hazard effects, since some clients will select on different interest rates *ex-ante*, but then have identical repayment burdens *ex-post*, while other clients will select on the same rate *ex-ante*, but have different repayment burdens *ex-post.*[7] We also randomly assigned differential "contract rate windows" (W), with some clients receiving the lower rate for one year (W=1) and others obtaining it for just the first loan (W=0). This "dynamic repayment incentive" enables us to test whether access to future financing at preferable rates reduces any moral hazard found in this market. Figure 1 shows the experimental operations, step-by-step.

B. Sample Frame

The sample frame consisted of all individuals from 86 branches who had borrowed from the Lender within the past 24 months and were in good standing, but who did not have a loan outstanding in the thirty days prior to the mailer.[8] Table [1] presents summary statistics on the sample frame and the sub-sample of clients who obtained a loan at r^c by applying before the deadline on their mailer. Most notably, clients differ in observable risk as assessed by the Lender. The Lender groups prior borrowers into "low", "medium", and "high" risk categories, and this summary statistic determines the borrower's loan pricing

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[5] The Lender also has 1, 6, 12, and 18 month products, with the longer terms offered at lower rates and restricted to the most observably creditworthy customers.

[6] Limiting the sample to past clients may stack the deck against finding hidden information, if information is revealed over time through lender-client interactions (i.e., the lending "relationship" that is oft-discussed in banking and finance literatures; see, e.g, [(cite)]). We return to this issue below.

[7] As discussed in Section V, what is often described as a "moral hazard" effect in a credit market context is more accurately defined as a reduced-form "repayment burden" effect that is a combination of several underlying moral hazard parameters and a wealth effect.

[8] This was done because many clients take a new loan out immediately after repaying the prior, the Lender did not want to crowd-out this business they would receive regardless of the offer.

and term options, under both normal and experimental operations. The Lender does not typically ask clients why they seek a loan, but the experimental protocol included a survey that indicates the following self-reported uses: consumption smoothing (food, clothes, rent, Christmas, personal-- 45%), education (21%), payoff other debt (13%), home improvements (11%), vehicle (6%), and funeral (4%).

C. The Randomization

Each client was assigned three random variables: an offer interest rate (r^o), a contract interest rate (r^c), and a binary variable for whether the contract rate is valid for one year (W=1) or one loan (W=0). Since we were constrained by the Lender to impose upper bounds at its standard rates, which vary only by observable risk under its standard (nonexperimental) operating procedure, our randomization program established a target distribution of interest rates, and randomly assigned each individual to a rate based on the target distribution, conditional on observable risk. [Appendix Appendix Table 1] shows the resulting r^o and r^c distributions conditional on the three observable risk categories. Note that rates varied from 3.25 percent per *month* to 11.75 percent per month.[9] 41% of the sample was chosen randomly and unconditionally to receive $r^c<r^o$ [Table 2—switch ordering]. Following the randomization we verified manually that the assigned rates were uncorrelated with other known information, such as credit report score. [Table 3—switch ordering] shows that the randomizations were successful, *ex-ante*, in this fashion; i.e., conditional on the observable risk category, the r^o and r^c are uncorrelated with other observable characteristics.

Lastly, each individual was assigned to receive r^c either for one full year (W=1), or for only the first loan (W=0). In the first wave of the randomization, this randomization was conducted at the branch level, such that 14 branches were assigned to "one loan" r^c window, and 10 branches were assigned to "one year" r^c window. In the second wave, this randomization was done at the individual level.[10]

D. The Offer and Loan Application Process

The Lender mailed solicitations to 57,533 former clients.[11] Each letter had a deadline by which the individual had to respond. The deadline ranged from 2 weeks to 6 weeks, and is discussed in related research (Marianne Bertrand et al., 2004). The Lender routinely mails teasers to former borrowers but had never promoted specific interest rate offers before the experiment.

Clients accepted the offer by entering a branch office and filling out an application in person with a loan officer. Loan applications were taken and assessed as per the Lender's normal underwriting procedures. Specifically, loan officers a) updated observable information and decided whether to offer *any* loan based on their updated risk assessment; b) decided the maximum loan size to supply to accepted applicants; and c) decided the longest loan term to supply to accepted applicants. Each decision was made "blind" to the experimental rates, with strict operational controls ensuring that loan officers instead used the Lender's *standard* rates in any debt service calculations. To determine loan size, the Lender has a specific affordability formula: the allowable monthly payment can be no more than a certain percentage of net income. A lower interest rate would thus allow for a larger loan. A larger loan might then generate a debt burden effect, which could cause a higher default rate. In order to avoid this potential confound, the maximum allowable loan size was established based on the *normal*, not experimental, interest rates. 4,348 clients obtained loans before their assigned deadline, a 7.6% takeup rate.

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[9] Note these are "add-on" rates, where interest is charged upfront over the original principal balance, rather than over the declining balance. As such the effective interest rates (APRs) ranged from [] to [].We adopt the cash loan market's convention of presenting rates in add-on, monthly form unless otherwise noted.

[10] The dynamic repayment incentive randomization was done initially at the branch level because operations personnel at the Lender were concerned that it would be complicated to communicate W on a case-by-case basis. Once the branches were more comfortable with the experimental design, this was relaxed for the larger, second wave of offers.

[11] The solicitations also incorporated randomized framing manipulations, inspired by findings from marketing and psychology literatures, that were designed to the impact of these "behavioral" effects on consumer demand ((The Department of Trade and Industry South Africa) The DTI, 2003).

The contract rate r^c, was kept secret from both the branch manager and borrower until after the manager approved the loan application and a loan amount was established.[12] Special operations software was developed to facilitate and control this process, and we verify that this condition held in practice by showing that offer takeup decision varied with the r^o but not r^c ([Table 2, column 4]). Once the loan terms were decided, the software then revealed the contract interest rate (r^c), which was equal to or less than the offer rate (r^o). If the rates were the same, no mention was made of the second rate. If the contract interest rate was lower than the offer interest rate, the branch manager told the client that the interest rate from the computer was in fact lower. Branch managers were instructed to present this as simply what the computer dictated, not part of a special promotion, and not due to anything particular to the client.

Clients were permitted to adjust their desired loan size L following the revelation of r^c. In theory endogenizing L in this fashion has implications for identifying moral hazard effects (since a lower r^c strengthens repayment incentives *ceterus paribus*, but might induce choice of a higher L that weakens repayment incentives), as discussed below. But in practice only 10% of borrowers changed their loan demand after r^c was revealed (Dean Karlan and Jonathan Zinman, 2004).[13]

Finally, the software informed the branch manager whether the individual's r^c was valid for one year (W=1) or for one loan (W=0). 47% of experimental loans obtained W=1, conditional on repayment performance. W=1 provides a dynamic repayment incentive that is used to help understand the importance of access to future favorable financing as an incentive to repay current debt.

E. Tracking Repayment

Monitoring repayment behavior is a simple matter of extracting data from the Lender's normal loan tracking system. Default is prevalent by any measure [Table 1].

IV. Theoretical Model

A. Theoretical Overview

We begin by discussing the specific models of private information that motivate our experimental setup, and then turn to a formal derivation of the hypotheses our experiment is designed to test.

As a general point let us begin by noting that whereas many field experiments are analyzed strictly in reduced-form, our setup was also designed with a structural approach in mind. In particular we can test explicitly whether the Stiglitz-Weiss (2004) *adverse selection* model (hereafter "SW") or the de Meza and Webb (1981) *advantageous selection* model (hereafter "DW") describes any selection on unobservables occurring in our setting. We refer to DW selection as advantageous, *a la* (David de Meza and David Webb, 1987), since it implies that a Lender with higher rates will attract a safer pool of borrowers, all else equal. Our research design also produces explicit tests for the presence of different varieties of incentive effects considered in private information models, including different types of moral hazard.

More specifically, r^o can produce either adverse or advantageous selection, depending on the relationship between borrower risk and return. If risk, defined from the Lender's perspective as the probability of default, and return are positively correlated, then SW implies that higher rates induce unobservably less risky borrowers to drop out of the applicant pool. Thus under adverse selection repayment and profitability would decrease in r^o as we move away from the initial equilibrium. If risk and return are negatively correlated, then DW implies that higher rates induce unobservably riskier borrowers to drop

[12] There are several reasons to implement the contract rate assignment "double-blind". Most importantly, we did not want the contract rate to contaminate any selection effects (by influencing either credit approval, or the applicant's decision whether to accept the loan offer). The double blind device also elicits two points on the credit demand curve for each consumer who received $r^c<r^o$ ((Marianne Bertrand, Dean Karlan, Sendhil Mullainathan, Eldar Shafir and Jonathan Zinman, 2004).
[13] On the other hand, project clients *did* exhibit significant interest rate elasticities with respect to r^o on both the extensive (takeup) and intensive margins (Dean Karlan and Jonathan Zinman, 2004)

out of the applicant pool.[14] Thus under advantageous selection thus repayment and profitability would increase in r^o as we move away from equilibrium.[15] This holds if borrowers, instead of differing in their risk to the Lender (as in SW), differ in intrinsic quality; i.e., if the return distribution of a better borrower first-order stochastically dominates that of a worse borrower. Such a return distribution seems plausible in a consumer credit context; e.g, if borrowers enjoy the same gross "returns" to borrowing broadly defined, but have (unobservably) different probablities of repaying (due to varying access to employment opportunities, family resources, etc.)

In the absence of compelling empirical evidence on the existence of either form of selection in comparable markets, our priors on the existence, direction, and importance of any selection effect should be agnostic.[16] The institutional features of the Lender's market reinforce this uncertainty. On one hand, several aspects of the contracting environment appear consistent with the presence of selection effects (rationing/redlining; and a very limited menu of contracts, including prohibitively high costs of taking fixed assets as collateral); on the other hand, the Lender has a fairly sophisticated risk assessment model, and the applicant pool may have already revealed their types in previous transactions with the Lender (see [Section VII]).

The second randomly assigned interest rate, r^c, identifies the reduced-form impact of "repayment burden" via a combination of several underlying structural parameters of interest. Repayment burden incentives operate through the borrower's project management and repayment choices. Project management choices are defined as those that impact returns. Higher interest rates will produce moral hazard in *project choice* (conditional on effort) if borrowers prefer mean-preserving spreads in project returns under limited liability (Lawrence M. Ausubel, 1991). Similarly, higher interest rates reduce effort (conditional on project choice), by producing *debt overhang* that reduces borrower returns in successful states (Joseph E. Stiglitz and Andrew Weiss, 1981). Repayment choice simply refers to the fact that *voluntary default*, conditional on project returns, becomes more attractive under limited enforcement as repayment burden increases (Jonathan Eaton and Mark Gersovitz, 1981, Parikshit Ghosh et al., 2000). In contrast, the *income effect* of repayment burden has nothing to do with choice; it works mechanically, by simply increasing the probability that a borrower with uncertain cash flow will be unable to repay. Note that each of these hypothesized incentive and income effects works in the same direction — a higher repayment burden decreases the probability of repayment.

Again, the lack of empirical evidence and the market's institutional framework interact to produce agnostic priors on the significance and magnitude of each of these four components of repayment burden. *Project choice* may be relatively limited (compared to say a pure commercial loan market), or then again not — supplementary data collection suggests the possibility of substantial "hidden" investment in entrepreneurial projects, and reveals cross-sectional variation in the deployment of funds consistent with a range of consumption smoothing and human capital investment opportunities. *Debt overhang* might also be less salient in a consumer rather than commercial credit setting, but then again the relevant effort in this case might be related to maintaining one's wage employment, or to obtaining credit from the informal sector in the event of a negative outcome. *Voluntary default* might be mitigated by reputational effects (repeat contracting opportunities) and aggressive (if imperfect) enforcement, but to what extent? The size of the *income effect* depends critically on the variance of borrower cash flows, which is unknown.

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[14]. Klonner and Rai (2001) provides a clear comparison of the Stiglitz-Weiss and de Meza-Webb models of selection, and find some empirical evidence consistent with the former but not the latter.

[15] de Meza and Webb (2004) shows that advantageous selection can persist in equilibrium if moral hazard prevents lenders from raising interest rates to clear the market.

[16] There is potentially a third type of selection based on private information, a "lemons" effect, that we believe is unlikely to be important in our setting. As described in Ausubel (2000) and elsewhere, given a setting with competitive bargaining and the presence of private information generated from lending relationships, a single deviating lender would find that reducing rates attracts *ex-ante* unobservably worse repayment risks, since competing lenders will match the rate reduction only for the better risks. Our prior is that the lemons effect is unimportant for the Lender, since survey evidence on pricing practices suggests strongly that lenders in this market simply do not make price concessions, even for good customers. Nevertheless we plan to take the possibility of a lemons effect seriously by examining whether the reduced-form selection effect varies by outside debtholding status (observed via credit bureau data) at the time of application (in the next draft). Note that while the lemons effect is commonly described as *adverse* selection, in our setting it is analogous to *advantageous* selection in the sense that reducing interest rates decreases profitability on the margin in either case.

Of course, given that we find a significant reduced-form effect of repayment burden, then distinguishing among the structural channels is important. Several approaches are feasible. First, we have collected data on utilization of loan proceeds and use this to estimate whether *project choice* varies with r^c in the next draft. Second, recall that we randomly assigned repeat contracting opportunities at preferential, experimental rates, conditional on previous repayment performance. This provides an additional, marginal incentive to repay, helping us to distinguish incentive from income effects. Identifying specific income and debt overhang effects would require additional data collection on cash flow histories, project outcomes, employment history, and other borrowing.

B. The Model

We now show more formally how our research design maps into classic theoretical models of private information, and thereby permits identification of specific adverse selection and repayment burden effects.

Assume a Lender implementing our experiment is faced with loan applicants who have identical observable characteristics but may be heterogeneous with respect to unobservable information.[17] These characteristics *q* are not observable to the Lender, but known to the applicant. Let *q* be continuous, and bounded below by zero: $Q = \{q, q \in [0, \infty)\}$. *q* impacts but does not wholly determine the "success" of the borrower's project, which is defined discretely. (The success/fail framework is most intuitive in a pure commercial credit market, but also applies to a consumer credit setting. Here we can think of success as the *ability* to repay out of realized cash flows, whether these flows be from entrepreneurial activity, wage income, or other financing sources.) Borrowers succeed with probability $p(\upsilon, q, e)$ and fail with probability 1-*p*, where υ a (macroeconomic) shock common to all borrowers and *e* is effort exerted by the agent. We allow effort to be continuous, $e \in [0,1]$, and assume it imposes a linear cost $\varphi(e) = e$.

We assume that *p* is twice continuously differentiable in effort, and differentiable with respect to the common shock and the unobservable risk *q*. Next we impose the following standard assumptions on the probability structure:

Assumption 1.

$$\frac{\partial p(\upsilon, q, e)}{\partial q} < 0, \frac{\partial p(\upsilon, q, e)}{\partial e} > 0, \frac{\partial p(\upsilon, q, e)}{\partial \upsilon} > 0, \frac{\partial^2 p(\upsilon, q, e)}{\partial e^2} < 0.$$

$$\textit{Inada conditions}: \frac{\partial p(\upsilon, q, 0)}{\partial e} \to \infty, \frac{\partial p(\upsilon, q, 1)}{\partial e} = 0.$$

I.e., *q* is indexed such that it is increasing in risk, the probability of repayment is increasing in the borrower's effort, υ is indexed such that it is increasing in p, and the returns to effort are decreasing.

Next assume for simplicity that the return to the borrower is *R(q)* in the event of success and zero in the event of failure. We assume for the moment that returns are observable and verifiable, thereby abstracting from the possibility of *voluntary default* (Jonathan Eaton and Mark Gersovitz, 1981, Parikshit Ghosh and Debraj Ray, 2001). Then default occurs if and only if the project doesn't succeed under the additional simplifying assumption that:

Assumption 2. $\forall q \in Q: \quad R(q) > (1 + r^c)B.$

Where B is the loan principal amount demanded and assumed to be identical across all applicants. This is a relatively innocuous assumption in our setting, since: a) loan supply is orthogonal to interest rates in our experiment by construction; b) we can condition on the actual principal amount borrowed in our empirical implementation.[18]

[17] In our setting, the empirical counterpart to the homogeneity assumption is considering each of the client categories separately.
[18] We also surpress the impact of wealth on repayment here. One can think of this as a zero-wealth assumption (which seems reasonable in the population under consideration), or of wealth being subsumed in unobservable information *p*.

So a borrower repays in full if her project succeeds and repays nothing if her project fails, and note that the Lender's "risk" is inversely related to *p* and therefore increasing in *q*.

We now show that the assumption on the relationship between risk (to the Lender) and returns (to the borrowers) is critical to identifying any selection effects of interest rates.

SW shows that *adverse selection* results if this relationship is positive. Formally,

Assumption 3a. (SW) $\forall q, q' \in Q: \quad p(\upsilon, q, e)R(q) = p(\upsilon, q', e)R(q') = C(\upsilon, e).$

Where C is a constant. The equation states simply that expected returns to the borrower are constant—projects that yield high returns in the successful state have low probabilities of success. We show below that this condition will indeed produce adverse selection in our setting.

DW shows that *advantageous selection* results if risk and returns are negatively correlated. Formally:

Assumption 3b. (DW). $\forall q, q' \in Q: \quad q > q' \Leftrightarrow p(\upsilon, q, e)R < p(\upsilon, q', e)R.$

This will hold, e.g., if borrowers differ only in the probability of project success, but not in project payoff conditional on success. More generally, we show below that in our setting 3b. implies that raising the interest rate discourages *low* quality borrowers on the margin, thereby improving the average composition of the borrower pool via *advantageous selection*.

We solve for selection and moral hazard effects by focusing on the borrower's problem. Define the borrower's expected profit (after the effort choice is made) as:

$$E(\pi) = p(\upsilon, q, e)[R(q) - (1 + r^c)B] - e$$

Where we assume, quite plausibly, that borrowers are price takers (see footnote []). We can normalize the borrower's outside option to zero so that she applies for the loan if her expected profit is positive. Then the lender's expected profit is:

$$E(\pi^B) = (1 + r^c)B\int_0^\infty 1_{apply}\, p(\upsilon, q, e)dF(q) - B\int_0^\infty 1_{apply}[1 - p(\upsilon, q, e)]dF(q)\,,$$

Where *F(q)* is the distribution of unobserved risk types with a well-defined density function *f(q),* and the indicator function takes the value of one if the agent applies for the loan and zero otherwise. Below we ignore the Lender's problem, however, since in our setting the interest rate is not a choice variable, and the variables the Lender does control (loan supply on the extensive and intensive margins) are orthogonal to the rate by construction. Therefore we can assume (without loss of generality) that applicants are approved by the Lender.[19]

Accordingly we return to the borrower's problem and begin by solving the model backwards; i.e., conditional on the borrower deciding to apply, she decides upon the repayment effort after learning r^c. Note the interest rate that the agent takes into account is the contract rate, not the offer rate (in the case where they differ). Therefore, holding q and υ fixed, the agent solves

$$\max_e \quad p(\upsilon, q, e)[R(q) - (1 + r^c)B] - e$$

Given our set of assumptions, the optimization program yields a unique interior solution for each value of q (and υ) and is characterized by the following first-order condition :

$$\tilde{e} = e(\upsilon, q, r^c): \quad \frac{\partial p(\upsilon, q, \tilde{e})}{\partial e} = \frac{1}{R(q) - (1 + r^c)B}.$$

[19] In practice, []% of applicants were approved in the experiment. More generally, one can think of any rejected borrowers as being *observably* differentiated— and recall that our model conditions on observable information.

Proposition 1. The level of effort chosen is inferior to the first-best value (that is, when effort is observable and verifiable). Moreover, note that $\partial\tilde{e}/\partial r^c < 0.$ This is the *debt overhang* version of moral hazard effect-- the higher the interest rate, the less the optimal effort since the agent only receives a positive profit in case of success (i.e., the profit function is convex). Proof is in the Appendix. Note this formulation focuses on moral hazard in effort rather than in *project choice,* a la Stiglitz and Weiss (2001).

The next step in solving the game is to study the decision to apply for the loan, which is made using the offer rate, r^o. (Recall from Section III that borrowers are not aware that there might be a distinct r^c when they are deciding whether to apply for the loan.) Define the marginal applicant as the one who has expected profits of exactly zero. That is,

$$\hat{q}: \quad p(\upsilon,\hat{q},e(r^o))[R(\hat{q})-(1+r^o)B]-e(r^o)=0.$$

Proposition 2. If assumption 3a holds, the agent applies for a loan if $q \geq \hat{q}.$ Moreover, an infinitesimal increase in r^o increases the marginal borrower's *q*, $\partial\hat{q}/\partial r^o > 0.$ (There is no effect on effort since effort is endogenous and the marginal effect is zero by the envelope theorem). Therefore when the offer rate increases the marginal applicant is riskier; i.e., the safer borrowers choose not to apply, creating a pool that is riskier on average. This is the classic adverse selection effect *a la* SW. If instead Assumption 3b holds, the agent applies if $q \leq \tilde{q}$. In this case $\partial\tilde{q}/\partial r^o < 0,$; i.e., increasing the offer rate decreases the marginal applicant's *q*, and the applicant pool becomes less risky on average. This is advantageous selection *a la* DW. (See Appendix for the proofs.)

We can now tie our propositions regarding the selection effects of r^o and the moral hazard effect of r^c directly to an empirical outcome of interest, the probability of default. According to the model, the expected probability of default, once r^c is known and effort is chosen, can be expressed as:

$$E(defprob)=\int_{\hat{q}}^{\infty}[1-p(\upsilon,q,\tilde{e})]\frac{f(q)dq}{1-F(\hat{q})}.$$

Proposition 3. The marginal effect of r^o on the default probability captures the effect of selection. It is easy to show that if Assumption 3a holds:

$$\frac{\partial E(defprob)}{\partial r^o}=\frac{\partial\hat{q}}{\partial r^o}\frac{f(q)}{1-F(q)}\int_{\hat{q}}^{\infty}[1-p(\upsilon,q,e)]\frac{f(q)dq}{1-F(\hat{q})}-\frac{\partial\hat{q}}{\partial r^o}\frac{f(q)}{1-F(q)}[1-p(\upsilon,\hat{q},e)]>0.$$

The proof is a direct application of proposition 2. If instead Assumption 3b holds then the effect of a marginal change in the offer rate on the estimated probability of default has a negative sign.

On the other hand the marginal effect of r^c will capture the moral hazard effect,

$$\frac{\partial E(defprob)}{\partial r^c}=-\int_{\hat{q}}^{\infty}\frac{\partial p(\upsilon,q,\tilde{e})}{\partial e}\frac{\partial\tilde{e}}{\partial r^c}\frac{f(q)dq}{1-F(\hat{q})}>0.$$

The result is again immediate since, by proposition 1, $\partial\tilde{e}/\partial r^c < 0.$

Incorporating the dynamic repayment incentive W is a straightforward application of models designed to explore the *voluntary default* version of moral hazard (Jonathan Eaton and Mark Gersovitz, 1981, Joseph E. Stiglitz and Andrew Weiss, 1981). Please see the Theory Appendix for the derivation.

V. Empirical Strategy

We now present the empirical strategy used to test the theoretical model and interpret the results of the experiment. Recall that we identify any selection and repayment burden effects by randomly assigning separate offer and contract interest rates to potential borrowers ("borrowers"), conditional on observable risk, and then estimating the relationship between loan repayment and these rates. Abstracting from functional form considerations for the moment, our basic empirical model takes the form:

(1) $Y_i = f(r_i^o, r_i^c, W_i, X_i)$

where i indexes borrowers. Y is an outcome of interest, namely a measure of repayment, as detailed below. r^o is the rate offered on the "pre-qualified" mail solicitation; $r^c =< r^o$ is the rate actually contracted upon loan approval. W is the randomly assigned contract rate window, with W=1 if r^c is valid for up to one year assuming successful repayment, and W=0 if r^c applies to one loan only. X always includes the Lender's summary measure of observable risk (since all random assignments were conditional on this measure), and may also include other readily observable characteristics that the Lender *could* use for screening.

In testing for effects of asymmetric information, the first-order outcome of interest is loan repayment, which can be measured in several different ways. We focus on three particular measures: average past due amount since takeup ("late amount"); average months delinquent since takeup ("months late"); and a binary variable for whether the account is in collection or charged off ("bad account") ([Table 1] shows some related summary statistics.) These measures were chosen in consultation with the Lender as proxies for the credit risk, collection costs, and bad debt incurred by the firm.

We exploit the random variation in r^o and r^c to identify any effects of selection and repayment burden on these outcomes. Specifically, as Section IV shows formally:

- r^o identifies the selection effect conditional on r^c-- with $dY/dr^o>0$ if there is adverse selection, and $dY/dr^o<0$ if there is advantageous selection
- r^c identifies the repayment burden effect conditional on r^o — with $dY/dr^c>0$ if there is such an effect.

To fix ideas and entertain functional form considerations, consider our favored ("base") specification, a linear model estimated using OLS:

(2) $Y_i = \alpha + \beta_o r_i^o + \beta_c r_i^c + \chi X_i + \varepsilon_i$

i again indexes borrowers, and β_o and β_c are now the estimates of the selection and repayment burden effects, respectively. (For expositional simplicity we momentarily ignore W, the contract rate window.) X need include only the Lender's summary measure of observable risk, since the only conditions imposed on the interest rate randomizations were tied directly to this variable. ε_{ib} is the error term, with standard errors corrected for clustering at the branch level, b. The model is estimated on the takeup sample of 4,348 observations, since, of course, these are the only project clients for whom we observe repayment behavior.

To motivate the linear parameterization consider the following thought exercise. Say we had only one observable risk category (or, equivalently, estimated equation (2) separately by category), only one contract rate r^c, but several offer rates. Then the impact of r^c would be captured in the intercept:

(3) $Y_i = \alpha_c + \beta^3{}_o r_i^o + \varepsilon_i$

And r^o again identifies the selection effect. Its coefficient is indexed by the equation number simply to highlight the fact that we do not constrain coefficients to be equal across different specifications.

Now we can account for the actual variation in r^c by allowing each contract rate to have its *own* intercept:

(4) $Y_i = \alpha + \beta^4{}_o r_i^o + \delta R_i^c + \varepsilon_i$

Where R^c is a vector of indicator variables for each individual contract rate, and δ is a vector of their coefficients. Now δ captures the repayment burden effect (inferred via, e.g., a joint significance test) and r^o again identifies the selection effect.

We next do the same analysis, but with a linear term for the contract rate, and a vector of indicator variables for the offer rate:

(5) $Y_i = \alpha + \gamma R_i^o + \beta^5{}_c r_i^c + \varepsilon_i$

Lastly, recall that r^c is weakly less than the Lender's standard interest rate r. Accordingly when $r^c < r$ and W=1, the borrower has a dynamic repayment incentive. In other words, a random subset of project borrowers (those with W= 1) are granted the opportunity to borrow multiple times at lower-than-standard rates, provided that they remain in good standing on their previous loan. In all specifications above, we can include an estimate of the impact of this incentive on repayment.

VI. Empirical Results: Core

A. Overview of Base Specification and Results

Our primary analysis estimates equation (2) using ordinary least squares, tobit, or probit on the "pooled" sample containing all three observable risk categories (Table 4, columns 1-6).[20] We also estimate equations (4) and (5), the semi-parametric versions of equation (2) (Table 4, columns 7 and 8). The dependent variables are the three repayment measures described above, with the late amount in units of South African currency and all outcomes measured as of July 2004.[21] In all specifications, the interest rate units are in monthly percentage points (e.g., 7.50 for 7.50% per month), and we report marginal effects where applicable. Results on interest rate variables therefore capture the effect of a one percentage point (100 basis point) increase in the monthly rate. In aggregated data, we find evidence of moral hazard but little suggestion of any selection on unobservables.

B. Evidence on Selection

The first row of results in Table 4 presents estimates of β_o, the response of repayment behavior to the offer rate. Recall that this coefficient identifies any selection on unobservables, with $\beta_o>0$ indicating adverse selection, and $\beta_o<0$ indicating advantageous selection. We find no robust evidence in either direction, although there is one marginally significant result showing adverse selection (column 6). The point estimate here implies a 6% increase in the probability an account ends up "bad" (in collection or charged off) for every 100 basis point increase in the monthly interest rate. Given the range of interest rates used in this experiment (850 basis points) and typically offered by this lender (400 basis points), the economic magnitude of an adverse selection effect of this size would be substantial.

In the next section, we will employ further tests which do find evidence of adverse selection on subsamples, such as women, and for those with lower income.

C. Evidence on Repayment Burden and Moral Hazard

The second row of results in Table 4 presents estimates of β_c, the response of repayment behavior to the contract rate. Recall that this coefficient identifies any effect of repayment burden, with $\beta_c>0$ indicating some combination of moral hazard and wealth effects. We find robust evidence of a large, significant effect on the late amount, with the coefficients showing increases of R11 to R18 (7% to 12%) per 100 basis point increase in the interest rate, depending on the estimator. On the other hand we find little evidence of a significant effect on the other repayment measures.

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[20] We also include the log of loan size, and loan term, to address the possibility of endogenous response following the revelation of r_i^c (see discussion on p. []). The results are robust to excluding these variables. Nor do results change if we include branch fixed effects to control for any differences in experimental implementation and/or the mechanical influence of varying mailer dates (staggered by groups of branches) on repayment measures.

[21] $1 US = [] on the repayment data pull date.

Results on W, the rate window variable, deliver robust evidence of moral hazard. Recall that clients with W=1 face a marginal *incentive* to repay— if they maintain good standing with the Lender they are eligible to borrow at r^c for up to a year. Since r^c is almost always lesser than the Lender's standard rate (in 98% of the cases, with a 350 basis point discount on average), we parameterize W discretely to simplify interpretation. The effect of W is large and significant across the board, with the incentive producing decreases in default ranging from 7% to 26% of mean levels in the sample.

Having found evidence of moral hazard, we now attempt to shed some light on which of the many different, specific types of moral hazard may be at work here. First, we measure project choice directly with a survey administered by the loan officer, and then regress project choice categories on the interest rates using equations (2), (4), and (5). Although there is some evidence that project choice responds to the contract rate (the likelihood of paying school fees decreases with the rate, while durable purchases increase with the rate, results not shown), it is difficult in this setting to classify common projects by *ex-ante* risk in a way that maps neatly into moral hazard.[22] Moreover, the validity of these results is questionable, since the data quality on project choice may be relatively low.[23] A second relevant finding is that very few clients (10%) increased their loan size following the revelation of $r^c < r^o$, which is consistent with inertia in project choice and a relatively large role for debt overhang or voluntary default.

D. Comparison to the Standard "One-Rate Method"

Columns 9 and 10 of Table 4 highlight the value of the two-tiered randomization strategy. Prior work on private information has used single contracts to identify the existence of private information and relied on various assumptions in attempting to disentangle adverse selection from moral hazard; see, .e.g., (Parikshit Ghosh and Debraj Ray, 2001). In our setting, this method is equivalent to having offer and contract rates that are equivalent. We had 2,619 such loans in our takeup sample, and column 10 shows that estimating (2) on this sub-sample produces results that are roughly equal to our identified estimates of moral hazard. Column 9 repeats the same exercise, but uses the entire takeup sample and includes only the offer rate, ignoring the contract rate. The result is similar. Recall that our two-step randomization permits identification of whether this type of reduced-form relationship is due to adverse selection and/or moral hazard. Thus, merely having a randomized offer interest rate, in our setting, would have falsely led to a conclusion of adverse selection.

E. Observable vs. Unobservable Predictors of Default

Table 9 adds several additional observables to estimates using equation (2) in order to address several related questions about the role of observables in predicting default. First, we find that the Lender's summary statistic for observable risk does not in fact completely summarize the role of observables, at least over the range of interest rates used in our experiment. Regardless of specification, several readily observed variables help predict default, including credit scores and the number of prior transactions with the Lender. Second, however, we find that adding observables beyond the summary statistic generates only slight improvements in the overall explanatory power of the model (as measured by the adjusted R-squareds in Tables 4 and 9). Finally, we can use the estimates in Table 4 and 9 to help calibrate the importance of private information in predicting default, relative to the importance of public information. For example, estimating our base specfication (with only the summary statistic for observable risk, but none of the other variables shown in Table 9) for late amount produces a significant contract coefficient that is 11% of the magnitude of the estimated mean difference between low and high risk clients. Over the Lender's typical range of interest rates (400 basis points), this implies that private information might be almost half (44%) as important as observable risk in predicting default.

VII. Empirical Results: Heterogeneity and Refinements

[22]

[23] Project choice is borrower-reported, in contrast to every other outcome of interest in this paper (which is tracked and reported by the Lender), and was collected for a possibly nonrandom sample of applicants due to some reluctance by loan officers to administer the survey.

The pooled results presented thus far may obscure hetereogeniety that bear on the interpretation of the results in important ways. In particular, hetereogeneity may obscure the full importance of private information on several margins.

A. External Validity and the Power of Repeated Transactions

One reason why we find little evidence of adverse selection could be that our sample of prior borrowers has already revealed their types to the Lender; i.e., in the process transacting, private information becomes public.[24] We explore this possibility indirectly, and within-sample, by exploring whether offer rate effect varies with the number of prior loans. Table 11 shows that this is indeed the case; adding a prior loans main effect and interaction with r^o to equation (2) produces negative and significant interaction term. This implies that, indeed, selection is relatively more adverse for those borrowers with whom the Lender is least familiar.

B. Internal Validity: Offsetting Selection Effects

Another possible explanation for the estimated lack of selection effects could be that our setup obscures concurrent, offsetting adverse and advantageous selection effects. This might be the case, for example, if those with higher income are more likely to respond to higher interest rate loans because they perceive themselves as more likely to be able to repay such loans. Lower income individuals see the monthly payments required on the high interest loans and do not borrow. This is testable by interacting the offer interest rate with income. Table 6, Columns 1-3 suggest that indeed these effects might be counterbalancing each other: the coefficient on r^o is positive, indicating adverse selection, and the coefficient on the interaction term of r^o and income is negative, indicating that for higher income individuals, the offer interest rate is less predictive of default (or, the adverse selection is mitigated by advantageous selection). The interaction term for r^o and the interaction term income*r^o is jointly significant for the "bad account" and the "proporation of months in arrears" outcome.

C. Observable, not Useable: Gender and Race

Certain observables can not actually be used for screening and contracting; e.g., anti-discrimination laws in South Africa and elsewhere preclude consideration of gender and race in underwriting. Accordingly we test whether unusable public information may create, exacerbate, or conceal private information problems by splitting the sample on gender and race. The issue of differential response to interest rates by gender is of particular interest to development economists and microfinance practictioners, given that microcredit initiatives often target women, in large part because the fairer sex is perceived to be less prone to moral hazard [cite]. And indeed, Table 5 shows that the pooled moral hazard results are driven entirely by males. However, the table also shows that females may pose adverse selection problems (at least as measured by months late and bad account variables), suggesting that the inability to screen on gender creates the equivalent of private information. Table 7 shows that the gender differences do not appear to be driven by differences in household composition or other observable characterstics across male v. female borrowers.

In all, the results in this section highlight the importance of allowing for heterogeneity when evaluating the impacts of private information. We find results that are consistent with adverse selection among borrowers with no prior history with the Lender, and strong evidence suggesting that information asymmetries vary by gender.

VIII. Conclusion

This paper develops a new market field experiment methodology for disentangling adverse selection from moral hazard effects. We implement the experiment in a South African consumer credit market, and find strong evidence of moral hazard (among males) and some evidence of adverse selection (among females).

[24] We sought to include clients with no prior relationship with the Lender by extending 3,000 offers to names obtained from a mailing list; unfortunately, the list turned out to be fraudulent.

We also find that our two-tiered randomization matters in practice; the standard one-rate identification strategy produces results that easily could be misinterpreted.

Much other work remains to be done, of course. Our data and identification seem ripe for testing theoretical models of optimal contract design, using the parameter estimates presented here to help identify the microfoundations of the credit market under consideration. For example, given the observed constraints on the set of feasible contracts (e.g., the prohibitively high cost of taking collateral; the legal prohibition of screening on gender or race), the estimated elasticities of borrowing and repayment with respect to interest rates, and the estimated importance of specific informational frictions, one should be able to derive the optimal contract and compare it to what actually prevails in the market. Conversely, a contract theory approach (e.g., taking the observed contracts as optimal) might provide a complementary way of putting structure on the parameter estimates in a way that helps identify and distinguish specific different varieties of private information and their equilibrium effects.

Our data and methodology also seem conducive to refining econmetric applications. The selection effects documented in this paper may be suited for refining the use of observable selection to control for unobservable selection, *a la* (Lawrence M. Ausubel, 1999) with application to credit scoring and/or the estimation of information asymmetries and demand elasticities.

The policy and practical implications of these findings are mixed. The lack of adverse selection on average suggests that the Lender possesses effective screening devices, at least for its prior borrowers, even at the extremely high interest rates prevaling in this market; indeed, a companion paper finds that marketing techniques may complement traditional risk assessment in helping to screen applicants [Bertrand et al., 2004]. On the other hand, adverse selection may well be important in a population we do not observe in this experiment— first-time borrowers. The prevalence of moral hazard points to the possibility of productive public or private investments in enforcement technologies, information sharing, and dynamic pricing schemes. It is important to note, however, that the mere presence information asymmetries, coupled with evident rationing, is not sufficient motivation for interventions designed to expand access to credit (no matter how well-run). The merit of such efforts depends critically on the structure of borrower returns, which helps motivate our upcoming project that will randomly "deration" marginal borrowers and follow-up with household surveys designed to measure decision-making and a broad range of impacts.

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Theory Appendix

Proof of Proposition 1.

To show that the effort level is lower than the first-best level of effort, first note than in a first-best setting where effort is observable and verifiable to all parties the first-order condition reads

$$\frac{\partial p(\upsilon, q, e^*)}{\partial e} = \frac{1}{R(q)}$$

Clearly the right-hand side of this first-order condition is smaller than the one for which effort is unobservable which makes the optimal effort level larger due to decreasing returns in effort.

To show the moral hazard effect, $d\tilde{e}/dr^c < 0,$ totally differentiate the first-order condition to obtain

$$\frac{d\tilde{e}}{dr^c} = \frac{B}{\frac{\partial^2 p}{\partial e^2}\left[R-(1+r^c)B\right]^2} < 0,$$

since the returns to effort are decreasing. *Q.E.D.*

Proof of Proposition 2.

Assume assumption 2a holds. First note that the expected borrower's profit once the offer rate is announced is

$$E(\pi(r^o)) = C(\upsilon, \tilde{e}, q) - p(\upsilon, \tilde{e}, q)(1+r^o)B - \tilde{e},$$

which is zero for the marginal applicant $\hat{q}$ by definition. Since the expected profits are increasing in *q* only applicants with *q's* higher than $\hat{q}$ are going to have nonnegative expected profits, thus they form the pool of applicants.

By totally differentiating the marginal applicant condition we find that

$$\frac{d\hat{q}}{dr^o} = \frac{pB}{-(1+r^o)B\frac{\partial p(\upsilon, \hat{q}, \tilde{e})}{\partial q}} > 0,$$

since *p* is decreasing in unobservable risk *q*. If instead assumption 3b holds the steps are the same although the signs are the opposite. In particular expected profits are decreasing in *q* and so only safer projects than the marginal *q*, $\tilde{q}$,apply. On the other hand, the total differentiation now gives

$$\frac{d\tilde{q}}{dr^o} = \frac{pB}{\left[R-(1+r^o)B\right]\frac{\partial p(\upsilon, \tilde{q}, \tilde{e})}{\partial q}} < 0. \text{ Q.E.D.}$$

The Alleviation of Moral Hazard through Dynamic Incentives

Consider now a variation of the benchmark model in which, after the realization of returns, the borrower asks for financing for one additional period. If the borrower has repaid her debt in the first period then the Lender may offer a second period rate r^s_2 which is lower than the rate offered in case the borrower defaulted in the first period; i.e., $r_2^{\,s} < r_2^{\,f}$. Basically, the Lender offers a reward for repayment. In our case $r_2^s = r^c < r_2^f$ if W=1 and $r^c<r^f$, where r^f is the Lender's standard rate. Recall that we assign W randomly (along with r^c and r^o), so we can again ignore the Lender's problem. Solving for the borrower's choice backwards, we see that the agent faces a static problem in period 2, since that is the last stage. The first-order conditions for the optimal choice of effort are

$$\hat{e}_2^s: \quad p'(\hat{e}_2^s) = \frac{1}{R-(1+r_2^s)B}$$

$$\hat{e}_2^f: \quad p'(\hat{e}_2^f) = \frac{1}{R-(1+r_2^f)B}$$

Note that since $r_2^s < r_2^f$ then $\hat{e}_2^s > \hat{e}_2^f$.

At the beginning of the period the borrower chooses the first period effort, e_1, taking the optimal valued for e_2 into account. Now the borrower's expected profit in the first period is

$$E(\pi) = p(e_1)\left[R-(1+r_1)B + p(\hat{e}_2^s)\left(R-(1+r^s{}_2)B\right)\right] + (1-p(e_1))\left[p(\hat{e}_2^f)\left(R-(1+r_2{}^f)B\right)\right] - e.$$

Note that the first-order condition now reads

$$p'(\hat{e}_1) = \frac{1}{R-(1+r_1)B+\left\{\left[p(\hat{e}_2^s)-p(\hat{e}_2^f)\right]R-\left[p(\hat{e}_2^s)(1+r^s{}_2)-p(\hat{e}_2^f)(1+r_2^f)\right]B\right\}}.$$

Here we can see that a reduction in interest rates conditional on repayment increases the effort made by the borrower, compared to a benchmark in which there is no discount conditional on 'good behavior'. This is so since the third summand in the denominator of the first-order condition is positive. As effort increases, the expected probability of default should decrease. This tells us that the pool of borrowers subject to a discount in case of repayment should have lower probabilities of default compared to borrowers who are not offered such a discount, other things equal. Significant differences in default probabilities between both groups are indicative of a moral hazard problem related to repeated borrowing (see Eaton and Gersowitz (2002); or Ghosh, Mookherjee, and Ray (1981, Parikshit Ghosh, Dilip Mookherjee and Debraj Ray, 2000) for a more detailed reference).

Table 1: Summary Statistics

	All	Borrowed	Did Not Borrow	Internal Client Category: High Risk	Internal Client Category: Medium Risk	Internal Client Category: Low Risk
A. Full Sample						
# of months since last loan	10.259	5.899	10.616	12.727	2.836	2.813
	(6.879)	(5.801)	(6.838)	(6.139)	(1.650)	(1.644)
Size of last loan prior to project	1116.358	1156.015	1113.116	1086.394	1176.534	1229.7
	(829.899)	(825.744)	(830.162)	(785.200)	(878.430)	(994.506)
# of prior loans with the lender	4.26	4.899	4.207	3.62	5.682	6.579
	(3.861)	(4.221)	(3.826)	(3.476)	(4.182)	(4.345)
Term of last loan prior to project						
1 or 2 months	1,656	132	1,524	1,407	93	156
	2.88%	3.04%	2.87%	3.26%	1.50%	1.92%
4 months	53,296	3,939	49,357	40,687	5,658	6,951
	92.64%	90.59%	92.80%	94.18%	91.17%	85.54%
6 months	2,030	223	1,807	887	369	774
	3.53%	5.13%	3.40%	2.05%	5.95%	9.52%
12 months	551	54	497	220	86	245
	0.96%	1.24%	0.93%	0.51%	1.39%	3.02%
Number of Observations	57,533	4,348	53,185	43,201	6,206	8,126
B. Default Measure						
Monthly Average Past Due Amount		152.555		180.131	224.489	57.397
		(359.279)		(404.864)	(408.526)	(181.672)
Monthly Average # of Payments in Arrears		0.434		0.528	0.625	0.147
		(0.899)		(1.020)	(0.990)	(0.444)
Account is in Collection		0.118		0.144	0.174	0.038
		(0.323)		(0.351)	(0.380)	(0.191)
Number of Observations		4,348		2,090	941	1,317

Standard deviations are in parentheses

Table 2: Randomization Validation

OLS

Dependent Variable	Contract Rate	Offer Rate	Year Long Rate	Drop Foreseen?
	(1)	(2)	(3)	(4)
Female	0.002	0.015	-0.002	
	(.019)	(0.020)	(0.003)	
Internal credit score	-0.000	-0.000	-0.000	
	(0.000)	(0.000)	(0.000)	
Married	0.012	0.009	0.005	
	(0.020)	(0.021)	(0.004)	
Log (Size of last loan prior to project)	-0.014	-0.002	-0.003	
	(0.016)	(.017)	(0.003)	
Term of last loan prior to project	-0.010	-0.011	-0.000	
	(0.011)	(.010)	(0.002)	
Offer Rate				-0.003***
				(0.001)
Contract Rate				0.000
				(0.001)
Constant	7.598***	8.248***	0.219***	0.068***
	(0.275)	(0.272)	(0.049)	(0.004)
Observations	57,533	57,533	57,533	57,533
R-squared	0.1	0.14	0.37	0.03

Standard errors in parentheses

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

Table 3: Summary of Randomization Variables

		High Risk	Medium Risk	Low Risk
Offered Interest Rates				
	Min	3.25	3.25	3.25
	Max	11.75	9.75	7.75
	Mean	8.36	7.22	5.83
Contract Interest Rates				
	Min	3.25	3.25	3.25
	Max	11.75	9.75	7.75
	Mean	7.48	6.55	5.38
Proportion Receiving Rate for One year (vs. one loan)		43.10	44.00	42.40
Proporation Receiving a Contract Rate < Offer Rate		43.90	44.40	43.50

Table 4: Primary Results: Disentangling Selection on Unobservables from Moral Hazard

	OLS			Tobit		Probit	OLS (semi-parametric)		OLS	
Dependent Variable	Monthly Average Past Due	Proportion of Months in Arrears	Account in Collection Status	Monthly Average Past Due	Proportion of Months in Arrears	Account in Collection Status	Monthly Average Past Due	Monthly Average Past Due	Monthly Average Past Due	Monthly Average Past Due
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Offer Rate (AS)	1.563	0.002	0.007	1.085	0.003	0.007*	-0.054	Indicator	11.619***	13.806***
	(3.897)	(0.004)	(0.005)	(7.003)	(0.007)	(0.004)	(4.248)	Variables	(2.795)	(3.414)
Contract Rate (MH)	11.991***	0.006*	0.001	17.676***	0.010	0.001	Indicator	11.295***		
	(3.377)	(0.003)	(0.005)	(6.267)	(0.007)	(0.004)	Variables	(3.665)		
Rate Valid for One Year (vs one loan)	-22.784**	-0.016*	-0.019**	-39.582**	-0.031**	-0.019**	-22.538**	-22.489**	-22.884**	-15.472
	(11.429)	(0.008)	(0.009)	(17.347)	(0.016)	(0.008)	(11.048)	(11.108)	(11.395)	(16.160)
Constant	-684.767***	0.032	0.104	-1,311.023***	-0.379***		-596.363***	-673.463***	-677.253***	-844.924***
	(98.248)	(0.062)	(0.063)	(179.779)	(0.139)		(106.690)	(89.034)	(98.744)	(131.366)
Observations	4,348	4,348	4,348	4,348	4,348	4,348	4,348	4,348	4,348	2,619
R-squared	0.09	0.11	0.04				0.10	0.10	0.09	0.10

Standard errors, in parentheses, Significant at 10%; ** significant at 5%; *** significant at 1%. "Offer Rate" and "Contract Rate" are in monthly percentage point units (7.00% interest per month is coded as 7.00). The "Rate Valid for One Year" is an indicator variable equal to one if the contract interest rate is valid for one year (rather than one loan) before reverting back to the normal (higher) interest rates.

Controls for length of loan, log of amount of loan, lender-defined risk category, and month of offer letter.
Standard errors corrected for clustering at the branch level.

Table 5: Disentangling Selection on Unobservables from Moral Hazard, by Gender

Sample Restriction	Male						Female					
	OLS			Tobit		Probit	OLS			Tobit		Probit
Dependent Variable	Monthly Average Past Due	Proportion of Months in Arrears	Account in Collection Status	Monthly Average Past Due	Proportion of Months in Arrears	Account in Collection Status	Monthly Average Past Due	Proportion of Months in Arrears	Account in Collection Status	Monthly Average Past Due	Proportion of Months in Arrears	Account in Collection Status
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Offer Rate (AS)	-3.999	-0.004	0.001	-9.238	-0.008	0.001	7.177	0.008*	0.013**	11.960	0.014	0.011***
	(5.946)	(0.005)	(0.007)	(9.995)	(0.010)	(0.007)	(5.035)	(0.004)	(0.005)	(9.183)	(0.009)	(0.004)
Contract Rate (MH)	18.489***	0.014***	0.010	27.543***	0.021**	0.009	4.655	-0.002	-0.009	5.672	-0.004	-0.007*
	(5.038)	(0.005)	(0.007)	(9.137)	(0.010)	(0.006)	(5.016)	(0.005)	(0.006)	(8.859)	(0.009)	(0.004)
Rate Valid for One Year (vs one loan)	-27.866*	-0.025**	-0.020	-58.905**	-0.049**	-0.020	-16.470	-0.006	-0.018	-16.392	-0.008	-0.017
	(15.043)	(0.012)	(0.014)	(25.377)	(0.023)	(0.014)	(15.424)	(0.012)	(0.012)	(26.232)	(0.025)	(0.011)
Constant	-760.738***	0.075	0.048	-1337.340	-0.262		-617.691***	-0.033	0.146*	-1315.260	-0.578***	
	(129.935)	(0.101)	(0.098)	(232.997)	(0.203)		(104.236)	(0.066)	(0.082)	(228.431)	(0.177)	
Observations	2,215	2,215	2,215	2,215	2,215	2,215	2,133	2,133	2,133	2,133	2,133	2,133
R-squared	0.10	0.12	0.04				0.07	0.10	0.04			

Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%.

Controls for length of loan, log of amount of loan, lender-defined risk category, and month of offer letter.
Standard errors corrected for clustering within branch.

Table 6: Disentangling Adverse and Advantageous Selection

OLS

Dependent Variable	Monthly Average Past Due	Proportion of Months in Arrears	Account in Collection Status	Monthly Average Past Due	Proportion of Months in Arrears	Account in Collection Status
	(1)	(2)	(3)	(4)	(5)	(6)
Offer Rate (AS)	-36.301	0.036*	0.075***	2.350	0.002	0.008
	(31.034)	(0.021)	(0.028)	(4.710)	(0.004)	(0.006)
Contract Rate (MH)	11.971***	0.007**	0.002	12.556***	0.007**	0.002
	(3.405)	(0.003)	(0.005)	(3.421)	(0.004)	(0.005)
Rate Valid for One Year (vs one loan)	-22.966**	-0.015*	-0.017*	-21.919*	-0.015*	-0.018**
	(11.436)	(0.008)	(0.009)	(11.358)	(0.008)	(0.009)
Net Income	-39.651	-0.012	0.014			
	(25.361)	(0.018)	(0.023)			
Offer Rate*Net Income	4.824	-0.004	-0.009**			
	(4.145)	(0.003)	(0.004)			
Years at Employer				-2.609	-0.003*	-0.003
				(2.210)	(0.002)	(0.002)
Offer Rate*Years at Employer				-0.148	-0.000	-0.000
				(0.346)	(0.000)	(0.000)
Constant	-386.296*	-0.016	-0.168	-709.551***	0.011	0.081
	(199.938)	(0.145)	(0.180)	(105.062)	(0.064)	(0.066)
Observations	4,348	4,348	4,348	4,348	4,348	4,348
R-squared	0.09	0.12	0.04	0.09	0.12	0.04

Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%.

Controls for length of loan, log of amount of loan, lender-defined risk category, and month of offer letter.
Standard errors corrected for clustering within branch.

Table 7: Disentangling Selection on Unobservables from Moral Hazard, by Gender and Demographics

Dependent Variable: Monthly Average Past Due, OLS

Sample Restriction	Male				Female			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Offer Rate (AS)	1.586	3.200	-8.469	23.463	10.346	8.821	14.354*	7.148
	(8.680)	(8.551)	(5.846)	(23.016)	(6.882)	(6.753)	(7.481)	(19.584)
Contract Rate (MH)	15.701*	18.975**	19.376***	-0.570	1.029	-1.303	-9.822	10.769
	(9.310)	(7.945)	(5.724)	(24.550)	(6.682)	(7.789)	(7.547)	(20.319)
Rate Valid for One Year (vs one loan)	-18.050	-45.181**	-32.696**	13.415	-10.234	-2.728	-0.249	-64.253
	(19.291)	(21.686)	(16.102)	(57.619)	(22.072)	(27.335)	(15.521)	(64.603)
Married	29.016				-21.728			
	(45.517)				(51.830)			
Offer Rate*Married	-11.429				-6.837			
	(11.351)				(10.412)			
Contract Rate*Married	5.796				8.905			
	(12.470)				(10.153)			
Rate Valid for One Year*Married	-19.606				-11.152			
	(29.352)				(31.003)			
# of Dependants		12.700				-16.728		
		(11.762)				(17.945)		
Offer Rate*# of Dependants		-3.082				-0.659		
		(2.155)				(2.468)		
Contract Rate*# of Dependants		-1.105				3.547		
		(2.427)				(3.812)		
Rate Valid for One Year*# of Dependants		7.965				-7.238		
		(6.222)				(10.231)		
Educated			-79.553				-59.303	
			(60.807)				(41.848)	
Offer Rate*Educated			15.133				-16.323	
			(14.600)				(12.164)	
Contract Rate*Educated			-3.217				30.089**	
			(13.315)				(14.511)	
Rate Valid for One Year*Educated			16.012				-27.708	
			(28.613)				(28.785)	
Age				-0.680				-1.834
				(1.985)				(2.035)
Offer Rate*Age				-0.682				-0.025
				(0.495)				(0.420)
Contract Rate*Age				0.483				-0.131
				(0.566)				(0.414)
Rate Valid for One Year*Age				-1.026				1.169
				(1.350)				(1.334)
Constant	-780.536***	-784.193***	-715.667***	-765.135***	-625.139***	-601.195***	-572.397***	-538.138***
	(137.073)	(134.887)	(133.256)	(159.409)	(106.349)	(115.811)	(116.293)	(151.882)
Observations	2,198	2,198	2,215	2,215	2,119	2,119	2,133	2,133
R-squared	0.11	0.11	0.11	0.11	0.08	0.08	0.08	0.08
Prob (Offer rate = interaction term = 0)	0.44336	0.21321	0.30343	0.14848	0.28868	0.31654	0.16188	0.42386
Prob (Contract rate = interaction term = 0)	0.00043	0.00488	0.00135	0.00015	0.43110	0.46474	0.10819	0.50062
Prob (Year long offer= interaction term = 0)	0.16498	0.10985	0.12946	0.12393	0.53521	0.36194	0.52798	0.54888

Standard errors corrected for clustering within branch. * significant at 10%; ** significant at 5%; *** significant at 1%. "Educated" is a binary indicator for the top 25% in years of education, predicted by the client's occupation. Controls for length of loan, log of amount of loan, lender-defined risk category, and month of offer letter. Standard errors corrected for clustering within branch.

Table 8: Dynamic Incentives: Effect of Making Promotiona Rate Valid for One Year versus One Loan

Dependent Variable: Monthly Average Past Due, OLS

Sample Restriction		Low Risk	Medium Risk	High Risk
	(1)	(2)	(3)	(4)
Offer Rate (AS)	1.479	8.468	0.978	0.834
	(3.919)	(8.307)	(9.749)	(4.836)
Contract Rate (MH)	9.392**	-4.913	1.837	14.643**
	(4.436)	(8.858)	(15.101)	(5.596)
Rate valid for one year, Indicator Variable	-2.756	-3.524	40.409	-35.773
	(25.514)	(24.347)	(56.392)	(44.330)
Rate valid for one year, # of points below normal rate	-5.606	-5.353	-11.391	-1.228
	(5.174)	(6.797)	(14.860)	(7.314)
Constant	-663.840***	-247.287***	-682.447***	-859.472***
	(93.758)	(69.573)	(205.856)	(145.430)
Observations	4,348	1,317	941	2,090
Adjusted R-squared	0.0864	0.0189	0.0844	0.0672
Prob(both yearlong variables = 0)	0.0176	0.0753	0.7460	0.1015

Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: Observable Determinanst of Default

OLS

Dependent Variable	Monthly Average Past Due		Proportion of Months in Arrears		Account in Collection Status		Monthly Average Past Due					
Sample Restriction							Low Risk		Medium Risk		High Risk	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Offer Rate (AS)	2.024		0.002		0.007		7.699		1.017		1.082	
	(4.010)		(0.003)		(0.005)		(8.415)		(10.504)		(5.095)	
Contract Rate (MH)	10.759***		0.006*		0.001		-1.231		8.841		13.889***	
	(3.491)		(0.003)		(0.005)		(8.251)		(11.844)		(4.096)	
Rate Valid for One Year	-24.419**		-0.017**		-0.022**		-15.643		5.957		-42.770**	
(vs one loan)	(11.384)		(0.008)		(0.009)		(11.334)		(22.663)		(21.276)	
Log(loan size)	105.399***	104.295***	0.005	0.005	-0.003	-0.004	39.420***	39.203***	154.190***	154.868***	121.800***	118.861***
	(14.204)	(13.795)	(0.009)	(0.008)	(0.010)	(0.010)	(10.774)	(10.855)	(27.145)	(27.224)	(23.406)	(22.980)
Female	-37.437***	-38.401***	-0.033***	-0.034***	-0.028**	-0.028**	-8.419	-8.517	-46.050	-45.108	-53.517***	-56.025***
	(12.975)	(13.014)	(0.010)	(0.010)	(0.012)	(0.012)	(11.506)	(11.375)	(33.098)	(33.409)	(19.584)	(18.890)
Age	-10.309***	-9.871***	-0.004*	-0.003	-0.003	-0.003	1.598	1.905	-22.449**	-21.992**	-10.179*	-9.631*
	(3.707)	(3.670)	(0.002)	(0.002)	(0.003)	(0.003)	(2.782)	(2.800)	(9.513)	(9.400)	(5.158)	(5.047)
Age squared	0.095***	0.091**	0.000	0.000	0.000	0.000	-0.017	-0.019	0.217**	0.214**	0.089*	0.083*
	(0.036)	(0.035)	(0.000)	(0.000)	(0.000)	(0.000)	(0.026)	(0.026)	(0.100)	(0.099)	(0.050)	(0.049)
Years at Employer	-2.077**	-2.031**	-0.001*	-0.001*	-0.001	-0.001	-0.862	-0.812	-3.304	-3.208	-2.159	-2.131
	(0.968)	(0.962)	(0.001)	(0.001)	(0.001)	(0.001)	(0.895)	(0.889)	(2.227)	(2.227)	(1.607)	(1.628)
Gross Income	1.136	1.177	-0.001*	-0.001	-0.000	-0.000	-0.156	-0.141	-0.156	-0.109	1.982	2.040
	(1.028)	(1.029)	(0.000)	(0.000)	(0.000)	(0.000)	(0.345)	(0.341)	(0.702)	(0.689)	(1.594)	(1.599)
Predicted # of Years of	-1.851	-1.981	-0.002	-0.002	-0.002	-0.002	-2.087	-2.230	-1.328	-1.500	-0.716	-0.854
Education	(1.983)	(1.975)	(0.001)	(0.001)	(0.002)	(0.002)	(1.413)	(1.448)	(4.185)	(4.068)	(3.225)	(3.242)
# of Dependants	0.266	-0.128	0.001	0.001	-0.005*	-0.005*	-3.457	-3.363	-2.981	-3.211	4.204	2.796
	(3.361)	(3.278)	(0.003)	(0.003)	(0.003)	(0.003)	(3.139)	(3.109)	(8.212)	(8.136)	(5.917)	(5.745)
External Credit Score	-0.240**	-0.256**	-0.000***	-0.000***	-0.000	-0.000*	-0.164**	-0.169**	-0.279	-0.301	-0.245*	-0.261*
	(0.098)	(0.097)	(0.000)	(0.000)	(0.000)	(0.000)	(0.069)	(0.068)	(0.190)	(0.192)	(0.145)	(0.143)
No External Credit Score	-153.602**	-162.448**	-0.245***	-0.251***	-0.075*	-0.081*	-111.625**	-115.141**	-185.285	-198.256	-147.672	-156.598*
	(63.062)	(62.438)	(0.049)	(0.049)	(0.045)	(0.044)	(46.187)	(45.913)	(119.688)	(120.131)	(94.656)	(93.374)
Internal Credit Score	-0.518	-0.644	-0.001	-0.001*	-0.002**	-0.002***	-1.400**	-1.477**	-0.589	-0.563	-0.428	-0.682
	(0.659)	(0.663)	(0.000)	(0.000)	(0.001)	(0.001)	(0.700)	(0.709)	(1.766)	(1.788)	(0.986)	(0.969)
Married	8.723	8.869	0.009	0.009	0.018	0.018	2.563	2.131	23.963	23.223	6.873	7.900
	(11.374)	(11.426)	(0.009)	(0.009)	(0.011)	(0.012)	(12.035)	(11.945)	(26.023)	(26.218)	(19.693)	(19.474)
Home Bond	39.128	36.874	0.015	0.013	0.041*	0.039*	34.522	33.977	50.054	48.309	28.922	24.728
	(30.378)	(30.174)	(0.022)	(0.022)	(0.023)	(0.022)	(34.291)	(34.022)	(50.816)	(51.099)	(51.333)	(50.560)
# of prior loans with the	-5.070***	-5.021***	-0.005***	-0.005***	-0.004***	-0.004***	-2.591**	-2.632**	-3.290	-3.441	-8.214***	-7.934***
lender	(1.121)	(1.137)	(0.001)	(0.001)	(0.001)	(0.001)	(1.164)	(1.168)	(3.104)	(3.128)	(1.819)	(1.818)
# of months since last loan	5.081***	5.191***	0.004**	0.004**	0.005***	0.005***	-0.865	-0.811	-2.123	-2.380	5.751***	5.791***
	(1.689)	(1.678)	(0.002)	(0.002)	(0.002)	(0.002)	(3.477)	(3.533)	(9.454)	(9.441)	(2.049)	(2.000)
Constant	-148.795	-59.869	0.467***	0.522***	0.406***	0.463***	-132.283	-104.686	57.305	123.329	-189.733	-91.507
	(139.614)	(130.747)	(0.097)	(0.090)	(0.107)	(0.103)	(112.700)	(102.732)	(271.918)	(263.716)	(190.477)	(174.790)
Observations	4,317	4,317	4,317	4,317	4,317	4,317	1,310	1,310	935	935	2,072	2,072
R-squared	0.1242	0.1180	0.1601	0.1562	0.0709	0.0673	0.0480	0.0438	0.1454	0.1434	0.1101	0.0992
Adjusted r-squared	0.1164	0.1108	0.1526	0.1493	0.0627	0.0596	0.0272	0.0252	0.1199	0.1208	0.0974	0.0877

Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. # of years of education is predicted by the occupation.

Control dummies included for Lender's 13-category risk level.

Table 10: Selection on Observable Information

Dependent Variable: "Applied for Loan", Probit

Sample Restriction			female	male		female	male
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Offer Rate (AS)	-0.003***	-0.004***	-0.005***	-0.002	-0.004***	-0.005***	-0.002
	(0.000)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Low Risk	0.113***	0.100***	0.097***	0.105***	0.100***	0.094***	0.107***
	(0.007)	(0.008)	(0.014)	(0.010)	(0.008)	(0.015)	(0.010)
Medium Risk	0.111***	0.116***	0.115***	0.117***	0.116***	0.116***	0.117***
	(0.006)	(0.006)	(0.009)	(0.009)	(0.006)	(0.010)	(0.009)
Fitted values		-0.088**	-0.109*	-0.046	-0.086**	-0.128*	-0.026
		(0.042)	(0.065)	(0.062)	(0.043)	(0.068)	(0.062)
Offer Rate* Predicted Past Due		0.004	0.009	-0.002	0.004	0.010	-0.004
		(0.005)	(0.007)	(0.008)	(0.005)	(0.007)	(0.007)
High Gross					-0.004	0.011	-0.017
					(0.009)	(0.012)	(0.012)
Offer Rate*High Gross Income					0.001	-0.001	0.002
					(0.001)	(0.002)	(0.001)
Observations	48,852	48,852	23,284	25,568	48,852	23,284	25,568

Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%.

"High Gross Income" equals to 1 if gross income above median in sample.

First wave, 4339 observations, omitted because income variable missing.

Marginal values reported for coefficients in probit specifications.

Table 11: Do Unobservable Selection Effects Diminish for More Frequent Borrowers?

OLS

	Monthly Average Past Due			Proportion of Months in Arrears			Account in Collection Status		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Offer Rate (AS)	6.934	1.431	1.467	0.007	0.002	0.002	0.013**	0.007	0.007
	(4.791)	(3.807)	(3.814)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Contract Rate (MH)	12.274***	17.871***	12.305***	0.007*	0.011***	0.007*	0.002	0.008	0.002
	(3.346)	(3.583)	(3.332)	(0.004)	(0.004)	(0.003)	(0.005)	(0.005)	(0.005)
Rate Valid for One Year (vs one loan)	-22.311*	-22.187*	-22.833	-0.016*	-0.016*	-0.020	-0.020**	-0.020**	-0.030**
	(11.337)	(11.350)	(17.761)	(0.008)	(0.008)	(0.013)	(0.009)	(0.009)	(0.014)
# of prior loans with the lender	2.417	1.755		0.001	-0.000		0.004*	0.002	
	(2.529)	(2.013)		(0.002)	(0.002)		(0.003)	(0.002)	
Offer Rate*# of prior loans	-1.268***			-0.001***			-0.002***		
	(0.362)			(0.000)			(0.000)		
Contract Rate*# of prior loans		-1.293***			-0.001***			-0.001***	
		(0.341)			(0.000)			(0.000)	
Rate Valid for One Year*# of prior loans			0.162			0.001			0.002
			(1.940)			(0.002)			(0.001)
Constant	-700.053***	-696.264***	-661.862***	0.027	0.032	0.061	0.086	0.096	0.134**
	(99.700)	(98.674)	(96.662)	(0.063)	(0.062)	(0.060)	(0.065)	(0.064)	(0.063)
Observations	4,317	4,317	4,317	4,317	4,317	4,317	4,317	4,317	4,317
R-squared	0.09	0.09	0.09	0.12	0.12	0.12	0.04	0.04	0.04

Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%.

Controls for length of loan, log of amount of loan, lender-defined risk category, and month of offer letter.
Standard errors corrected for clustering within branch.

Appendix Table 12. Frequency of Monthly Interest Rates Initially Offered and Final Interest Rates

	High Frequency Clients				Medium Frequency Clients				Low Frequency Clients			
	Offer Interest Rate		Contract Interest Rate		Offer Interest Rate		Contract Interest Rate		Offer Interest Rate		Contract Interest Rate	
	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent
3.25%	142	1.80	293	3.71	94	1.53	172	2.81	585	1.36	1016	2.36
3.49%	272	3.44	339	4.29	110	1.79	135	2.20	756	1.76	934	2.17
3.50%	261	3.30	381	4.82	116	1.89	163	2.66	538	1.25	928	2.16
3.75%	31	0.39	41	0.52	18	0.29	26	0.42	53	0.12	80	0.19
3.99%	344	4.35	554	7.01	103	1.68	223	3.64	751	1.74	1394	3.24
4.00%	197	2.49	332	4.20	98	1.60	142	2.32	524	1.22	843	1.96
4.25%	40	0.51	61	0.77	22	0.36	29	0.47	59	0.14	69	0.16
4.44%	203	2.57	373	4.72	78	1.27	213	3.47	492	1.14	1215	2.82
4.49%	381	4.82	315	3.99	139	2.27	136	2.22	773	1.80	864	2.01
4.50%	175	2.21	286	3.62	99	1.62	146	2.38	589	1.37	823	1.91
4.75%	45	0.57	39	0.49	22	0.36	29	0.47	59	0.14	76	0.18
4.99%	193	2.44	369	4.67	116	1.89	210	3.43	713	1.66	1346	3.13
5.00%	276	3.49	325	4.11	118	1.92	164	2.68	548	1.27	807	1.87
5.25%	44	0.56	48	0.61	19	0.31	26	0.42	67	0.16	77	0.18
5.49%	333	4.21	382	4.83	145	2.37	235	3.83	711	1.65	1328	3.09
5.50%	421	5.33	406	5.14	97	1.58	143	2.33	603	1.40	758	1.76
5.55%	283	3.58	262	3.32	80	1.31	120	1.96	513	1.19	659	1.53
5.75%	45	0.57	55	0.70	20	0.33	27	0.44	74	0.17	92	0.21
5.99%	477	6.04	395	5.00	208	3.39	254	4.14	711	1.65	1172	2.72
6.00%	394	4.99	310	3.92	115	1.88	138	2.25	585	1.36	766	1.78
6.25%	47	0.59	48	0.61	23	0.38	24	0.39	74	0.17	80	0.19
6.50%	379	4.80	359	4.54	123	2.01	199	3.25	608	1.41	1284	2.98
6.75%	417	5.28	333	4.21	147	2.40	197	3.21	566	1.31	901	2.09
6.99%	446	5.64	295	3.73	227	3.70	189	3.08	773	1.80	902	2.10
7.00%	423	5.35	286	3.62	201	3.28	194	3.16	854	1.98	880	2.04
7.25%	385	4.87	260	3.29	199	3.25	203	3.31	832	1.93	1026	2.38
7.49%	553	7.00	335	4.24	256	4.18	212	3.46	1013	2.35	977	2.27
7.50%	349	4.42	224	2.84	194	3.16	165	2.69	844	1.96	821	1.91
7.75%	345	4.37	195	2.47	180	2.94	161	2.63	903	2.10	1026	2.38
7.77%	0	0.00	0	0.00	199	3.25	136	2.22	821	1.91	716	1.66
7.99%	0	0.00	0	0.00	220	3.59	156	2.54	1025	2.38	927	2.15
8.00%	0	0.00	0	0.00	167	2.72	159	2.59	886	2.06	825	1.92
8.19%	0	0.00	0	0.00	233	3.80	165	2.69	1019	2.37	825	1.92
8.25%	0	0.00	0	0.00	25	0.41	28	0.46	73	0.17	78	0.18
8.50%	0	0.00	0	0.00	212	3.46	160	2.61	824	1.91	976	2.27
8.75%	0	0.00	0	0.00	35	0.57	23	0.38	81	0.19	76	0.18
8.88%	0	0.00	0	0.00	218	3.56	152	2.48	800	1.86	846	1.97
8.99%	0	0.00	0	0.00	258	4.21	172	2.81	1041	2.42	811	1.88
9.00%	0	0.00	0	0.00	210	3.43	126	2.05	873	2.03	752	1.75
9.25%	0	0.00	0	0.00	213	3.47	140	2.28	884	2.05	863	2.01
9.49%	0	0.00	0	0.00	292	4.76	163	2.66	1158	2.69	877	2.04
9.50%	0	0.00	0	0.00	37	0.60	27	0.44	89	0.21	81	0.19
9.69%	0	0.00	0	0.00	230	3.75	134	2.19	1198	2.78	889	2.07
9.75%	0	0.00	0	0.00	214	3.49	114	1.86	885	2.06	722	1.68
9.99%	0	0.00	0	0.00	0	0.00	0	0.00	1237	2.87	885	2.06
10.00%	0	0.00	0	0.00	0	0.00	0	0.00	1252	2.91	874	2.03
10.25%	0	0.00	0	0.00	0	0.00	0	0.00	1272	2.96	886	2.06
10.49%	0	0.00	0	0.00	0	0.00	0	0.00	1484	3.45	955	2.22
10.50%	0	0.00	0	0.00	0	0.00	0	0.00	1274	2.96	829	1.93
10.75%	0	0.00	0	0.00	0	0.00	0	0.00	92	0.21	72	0.17
10.99%	0	0.00	0	0.00	0	0.00	0	0.00	1384	3.22	894	2.08
11.00%	0	0.00	0	0.00	0	0.00	0	0.00	1383	3.21	855	1.99
11.11%	0	0.00	0	0.00	0	0.00	0	0.00	1343	3.12	798	1.85
11.19%	0	0.00	0	0.00	0	0.00	0	0.00	1491	3.46	864	2.01
11.25%	0	0.00	0	0.00	0	0.00	0	0.00	104	0.24	77	0.18
11.50%	0	0.00	0	0.00	0	0.00	0	0.00	98	0.23	71	0.16
11.69%	0	0.00	0	0.00	0	0.00	0	0.00	1426	3.31	829	1.93
11.75%	0	0.00	0	0.00	0	0.00	0	0.00	1374	3.19	745	1.73
Total	7,901	100	7,901	100	6,130	100	6,130	100	43,042	100	43,042	100

Figure 1. Timeline of the Experimental Operations