

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. We randomized 58,000 direct mail offers issued by a major South African lender along three dimensions: 1) the initial "offer interest rate" appearing on the direct mail solicitations; 2) a weakly lesser "contract interest rate" revealed to the over 4,000 borrowers who responded to the solicitation and agreed 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 and complete knowledge of the Lender's information set, permit identification 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) sparked a literature of theoretical papers on the role of asymmetric information in credit markets that has influenced policy and practice worldwide. Theory suggests that information frictions could have harmful real consequences at both the micro and the macro level, via underinvestment (Gale, 1990, Hubbard, 1998) or overinvestment (Bernanke and Gertler, 1990). In developing countries in particular, information asymmetries are presumed to contribute to credit constraints for the poor.

Yet empirical evidence on the existence and importance of specific information frictions remains relatively thin. Chiappori and Salanie (2003) 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) may produce independent selection and incentive effects.²

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. Specifically, we designed a market field experiment that was implemented by a South African firm specializing in making high-interest, unsecured term loans to poor workers. The experiment identifies information asymmetries by randomizing loan pricing 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 interest rate offered on future loans.

A stylized example, illustrated in Figure 1, gets to the heart of our methodology. Potential borrowers with the same observable risk are randomly offered a high or a low interest rate on a

¹ The 2001 Nobel Prize Committee's 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 (Sweden, 2001).

² See Ausubel (1999) and Chiappori and Salanie (2000) for related discussions of this problem in the credit and insurance market contexts, respectively.

direct-mail solicitation. Individuals then decide whether to borrow at the solicitation's "offer" rate. Of those that respond to the high rate, half are randomly given a new lower "contract" interest rate, while the remaining half continue to receive the high rate (i.e., their contract rate equals the offer rate). Any selection effect is 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 is attributable to selection on unobservables. Similarly, any effect of moral hazard (or more generally of repayment burden) is identified by considering the sample that responded to the high offer interest rate, and comparing the repayment performance of those who received the *high contract* interest rate with those who received the *low contract* interest rate. These borrowers selected in identically, but have randomly different interest rates on their contract, and any difference in repayment is attributable to repayment burden.³

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 (1999).⁴ Edelberg estimates a structural model to disentangle the effects of adverse selection and one type of moral hazard (in effort) in collateralized U.S. consumer credit markets. She 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.

³ We define moral hazard as the portion of any effect of debt burden on repayment that stems from *incentives*. Repayment burden also has a wealth or income effect component, stemming from shocks. Please see Section IV.

⁴ At least three other papers endeavor to disentangle adverse selection from moral hazard in a credit market. Karlan (2004) finds evidence of social capital mitigating moral hazard effects. Ahlin and Townsend (2003) uses contract choice in Thailand, and finds evidence consistent with adverse selection. Klöner and Rai (2004) uses institutional features of rotating credit associations in India, and finds evidence for adverse selection. Other papers estimating the prevalence of

We find evidence of moral hazard among male borrowers and adverse selection among female borrowers. 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 details how specific theoretical models motivated the design. Section V maps the experimental design and related theory into a specific empirical strategy. Section VI presents the empirical results. Section VII 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 (Porteous, 2003). 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 loan borrowers generally lack the credit history and/or collateralizable wealth needed to borrow from traditional institutional sources such as commercial banks. 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). Aggregate cash loan market outstandings total 1% of GDP and 5% of non-mortgage consumer credit (The DTI, 2003).

Cash loan sizes tend to be small relative to the fixed costs of underwriting and monitoring them, but substantial relative to borrower income. For example, our cooperating Lender’s median

private information in credit markets include Calem and Mester (1995), Cressy and Toivanen (2001), Crook (2002), and Drake and Holmes (1995).

loan size of R1000 (\$150) is 32% of its median borrower's gross monthly income. 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 highest-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. There is essentially no difference between these nominal rates and corresponding real rates.⁵ Rationing is 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 of its 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. 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. Its pricing is unusually transparent and linear, with no surcharges, application fees, or insurance premiums 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.⁶ The Lender's standard 4-month rates, absent this experiment, range from 7.75% to 11.75% per month, depending on observable risk.

⁵ For instance, South African inflation was 10.2% from March 2002-2003, and 0.4% from March 2003-March 2004.

⁶ 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.

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 methodology is implemented in a consumer credit market, but is applicable to other market settings as well.

The experiment was pilot tested in July 2003, and then fully executed in two additional waves launched 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.⁷ 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 the loan size and term offered to the client were orthogonal to the experimental interest rates by construction.⁸ 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*.⁹

⁷ 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., through the lending “relationship” that is studied in banking and finance literatures, see Elyasiani and Goldberg (2004)). We return to this issue in Section VII.

⁸ Table 2 verifies that these orthogonality conditions held in practice; i.e., credit approval, loan size offered, and term offered were all uncorrelated with the randomly assigned interest rates.

⁹ As detailed in Section IV, we define “repayment burden” as the reduced-form combination of several underlying moral hazard parameters and a wealth effect.

We also randomly assigned differential “contract rate windows” (W), with some clients eligible to receive r^c on all future loans taken within the next year ($W=1$), conditional on repayment performance, and others obtaining r^c for just the first loan ($W=0$). Since r^c was less than the Lender’s standard rate in 97% of the cases,¹⁰ $W=1$ provides a “dynamic repayment incentive” that enables us to test whether access to future financing at preferable rates reduces any moral hazard found in this market. Figure 2 shows the experimental operations, step-by-step. Figure 3 shows a scatter plot of the offer rate against the contract rate for all individuals (41% of sample) who were assigned $r^c < r^o$.

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.¹¹ 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 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%).

¹⁰ The Lender assumed all of the revenue and repayment risk from these pricing changes. Some implementation and operational costs were shared with the authors; e.g., training and project and management.

¹¹ The thirty day restriction was imposed because many clients take a new loan out immediately after repaying their prior one, and the Lender did not want to inefficiently subsidize these loans.

C. *The Randomizations*

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 would be valid for up to 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.¹² Rates varied from 3.25 percent per *month* to 11.75 percent per month.¹³ 41% of the sample was chosen randomly and unconditionally to receive $r^c < r^o$ (Table 2). Following the randomization we verified that the assigned rates were uncorrelated with other known information, such as credit report score. Table 3 shows that the randomizations were successful, *ex-ante*, in this fashion, i.e., conditional on the observable risk category with r^o and r^c 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 pilot and the second 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 third wave, this randomization was done at the individual level.¹⁴

¹² Appendix Table 1 shows the resulting r^o and r^c distributions conditional on the three observable risk categories.

¹³ Note these are “add-on” rates, where interest is charged upfront over the original principal balance, rather than over the declining balance. We adopt the cash loan market’s convention of presenting rates in add-on, monthly form.

¹⁴ 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.

D. *The Offer and Loan Application Process*

The Lender mailed solicitations featuring the offer rate to 57,533 “pre-approved” former clients.¹⁵ Each letter had a deadline by which the individual had to respond in order to obtain r^o . The deadline ranged from 2 weeks to 6 weeks, and is discussed in related research (Bertrand, Karlan, Mullainathan, Shafir and Zinman, 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 accepted applicants qualified for; and c) decided the longest loan term accepted applicants qualified for. 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 cannot exceed a certain percentage of net income. A lower interest rate would thus allow for a larger loan. A larger loan might then generate a repayment burden effect, which could cause a higher default rate (and bias against finding moral hazard with respect to the interest rate). In order to mitigate 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.

The contract rate r^c , was kept secret from both the loan officer and borrower until after the manager approved the loan application and a loan amount and term were established.¹⁶ Special

¹⁵ 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 (Bertrand et al., 2004).

¹⁶ 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 $r^c < r^o$ (Karlan and Zinman, 2004).

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 r^c , which was weakly less than r^o . If the rates were the same, no mention was made of the second rate. If $r^c < r^o$, the loan officer told the client that the actual interest rate was in fact lower than the initial offer. Loan officers were instructed to present this as simply what the computer dictated, not as part of a special promotion or anything particular to the client.

Clients then 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 (Karlan and Zinman, 2004).¹⁷

Finally, the software informed the loan officer 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 (see Table 1).

¹⁷ 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 (Karlan and Zinman, 2004).

IV. Theoretical Overview

We begin by discussing the specific models of private information that motivate our experimental setup, and then describe how these models map into the experimental design. The Theory Appendix provides a more formal derivation.

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 (1981) *adverse selection* model (hereafter “SW”) or the de Meza and Webb (1987) *advantageous selection* model (hereafter “DW”) describes any selection on unobservables occurring in our setting. We refer to DW selection as advantageous, *a la* (de Meza and Webb, 2001), 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^0 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^0 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 out of the applicant pool.¹⁸ Thus under advantageous selection, repayment and profitability would increase in r^0 as we move away from equilibrium.¹⁹ This holds if borrowers, instead of differing in their risk to the Lender (as in SW), differ in intrinsic quality, e.g., if the return distribution of a better borrower first-order stochastically dominates that

¹⁸ Klöner and Rai (2004) 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.

¹⁹ de Meza and Webb (2000) shows that advantageous selection can persist in equilibrium if moral hazard prevents lenders from raising interest rates to clear the market.

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 probabilities of repaying (due to varying access to employment opportunities, family resources, etc.) One limitation of our setup is that if there are heterogeneous selection effects, such that if some borrowers select adversely and others advantageously, then the effect of r^0 will obscure the true magnitude of selection on unobservables. We explore the possibility of concurrent, offsetting selection effects in Section VII.

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 are agnostic.²⁰ 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 (prevalent rationing 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 complex 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 (Stiglitz and Weiss, 1981). Similarly, higher interest rates reduce effort (conditional on project choice), by producing *debt overhang* that

²⁰ There is potentially a third type of selection based on private information, a “lemons” effect, that is unlikely to be important in our setting. As described in Ausubel (1991) 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. But survey evidence on pricing practices in the cash loan market suggests strongly that lenders as a rule do not make price concessions, even for good customers. Note that while the lemons effect is commonly

reduces borrower returns in successful states (Ghosh et al., 2000). Repayment choice simply refers to the fact that *voluntary default* (conditional on project returns) becomes more attractive under limited enforcement as repayment burden increases (Eaton and Gersovitz, 1981, Ghosh and Ray, 2001). 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 work 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 may not — supplementary data collection suggests the possibility of “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.

Given that we find a significant reduced-form effect of repayment burden, 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 . Second, recall that we randomly assigned repeat contracting opportunities at preferential, experimental rates, conditional on previous repayment performance. This provides an additional,

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.

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 flows and project outcomes.

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 by 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; and $r^c \leq r^o$ 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 the interest rates were randomized 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 monthly past due as a percentage of original principal (“default rate”); 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 the Theory Appendix shows formally:

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

The base specification is a linear model estimated using OLS:

$$(2) \mathbf{Y}_i = \alpha + \beta_o r_i^o + \beta_c r_i^c + \chi \mathbf{X}_i + \varepsilon_{ib}$$

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 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) and only one contract rate r^c , but several offer rates. Then the impact of r^c would be captured in the intercept:

$$(3) \mathbf{Y}_i = \alpha_c + \beta_o^3 r_i^o + \varepsilon_{ib}$$

And β_o^3 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:

$$(4) \mathbf{Y}_i = \boldsymbol{\alpha} + \boldsymbol{\beta}_o^4 \mathbf{r}_i^o + \boldsymbol{\delta} \mathbf{R}_i^c + \boldsymbol{\varepsilon}_{ib}$$

Where \mathbf{R}^c is a vector of indicator variables for each individual contract rate, and $\boldsymbol{\delta}$ is a vector of their coefficients. Now $\boldsymbol{\delta}$ captures the repayment burden effect (inferred via, e.g., a joint significance test) and $\boldsymbol{\beta}_o^4$ 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:

$$(5) \mathbf{Y}_i = \boldsymbol{\alpha} + \boldsymbol{\gamma} \mathbf{R}_i^o + \boldsymbol{\beta}_c^5 \mathbf{r}_i^c + \boldsymbol{\varepsilon}_{ib}$$

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. We can include an estimate of the impact of this incentive on repayment by simply adding the indicator W to any specification.

VI. Empirical Results

A. *Overview of Base Specification*

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).²¹ 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

²¹ 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 (as discussed in Section III). 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.

default rate in units of South African currency and all outcomes measured as of July 2004.²² 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. Aggregate Results

Row 1 of Table 4 presents estimates of β_o , the response of repayment behavior to the offer rate. This coefficient identifies any net 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).²³ The point estimate here implies a 0.7% 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. This is significant, given a mean default rate of 15% and a normal range of rates of 400 basis points, the selection effect assuming linearity equals 2.8% points of default, which is 19% of the observed default.

Row 2 of Table 4 presents estimates of β_c , the response of repayment behavior to the contract rate. This coefficient identifies any effect of repayment burden, with $\beta_c > 0$ indicating some combination of moral hazard and wealth effects. Similar to the adverse selection results, we find consistently positive effects, but typically not significant statistically.

²² \$1= R6.1 on the repayment data pull date.

²³ Appendix Table 3 highlights 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., (Ausubel, 1999). 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 2 shows that estimating (2) on this sub-sample produces results that are roughly equal to our identified estimates of moral hazard. Column 1 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.

Results on W , the rate window variable, deliver robust evidence of moral hazard. 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 1.5 percentage points to 3.1 percentage points (relative to a mean 15% default rate). Table 8 shows that this effect is stronger as the discount for future loans is larger. This robustness check verifies that by raising the benefit of repaying a loan (by providing better future borrowing opportunities), individuals are less likely to commit moral hazard on current loans.

Having observed 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. 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. Moreover, the validity of these results is questionable since the data quality on project choice may be relatively low.²⁴ 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.

²⁴ 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.

Table 4 Columns 7 (8) presents the results where the offer (contract) rate is included as a linear term, and the contract (offer) rate is included non-parametrically. These results are not significant statistically.

C. Disaggregating by Gender

Table 5 shows results disaggregated by gender. Here, we find significant and robust evidence of moral hazard (but not adverse selection) for males, and adverse selection (but not moral hazard) for females. The issue of differential response to interest rates by gender is of particular interest to development economists and microfinance practitioners, given that microcredit initiatives often target women, in large part because females are considered more likely to repay loans.²⁵

Columns 1 through 6 in Table 5 show the results for males. Both experimental instruments for moral hazard, the contract rate and the offer window length (W), are significant and robust determinates of default. However, the offer rate is actually signed negatively (but not significant statistically). Table 8 Column 3 examines whether the value of offer length window (measured as the reduction from the normal lending rate) predicts repayment, or whether effect is non-linear from simply having the promotion valid for longer. The effect is indeed linear in the magnitude of the future benefit, supporting the basic idea that this experimental variable is directly altering the benefit of repayment, and hence the value of the lending relationship, to the client.

For females, Table 5 Columns 7-12 show the reverse: the offer rate is positive and significant, indication of adverse selection, yet the moral hazard instruments (contract rate and offer window length) are insignificant statistically. This result is robust to all the different measures of default, and the magnitude is economically important: a 100 basis point drop in the interest rate indicates a 1.2% point reduction in arrearage (Column 7), and a 1.3% point reduction

²⁵ Although the reasons for this are unclear, the anecdotal evidence suggests that women are weakly more likely to repay microcredit loans. Some microfinance organizations report more stark differences than others do.

in the probability of the debt going into collection (Column 9). Given a mean of 15%, these are significant changes in expected default rates.

Next, we examine whether the gender difference can be explained by household demographics, such as number of dependants, marital status, education or age. Table 7 shows that these gender differences are not explained by the behavior of any of these sub-categories. By interacting each of the experimental variables with these demographic variables, we can observe whether the adverse selection or moral hazard effects are stronger (or weaker) for any of these categories. They are not; the effect remains on the straight term for each of the experimental variables.

Figures 6 through 9 present the results graphically, by gender. Each of these is a kernel regression of the coefficient on the indicator variables from a specification similar to Table 4 Columns 7 and 8, except disaggregated by gender. The y-axis represents the coefficient of each of the coefficients from a semi-parametric regression in which one rate is included as a linear term, and the other rate (the rate on the x-axis in the kernel regression) is included as a set of indicator variables. For females, the offer rate is upward sloping, whereas the graph for the contract rate is virtually flat (and noisy). Similarly, in support of the parametric findings, the graph for males is upward sloping for the contract rate, but flat for the offer rate.

D. Disaggregating by Income

Another possible explanation for the estimated lack of selection effects in the aggregate 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 (and presumably are aware of no alternatives at similar terms). On the other hand, 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, that adverse selection is mitigated by advantageous selection). The interaction term for r^o and the interaction term $\text{income} * r^o$ is jointly significant for the “bad account” and the “proportion of months in arrears” outcome.

However, again, by disaggregating by gender we find that this effect is driven by females. Column 4 of Table 6 shows the same specification for females, and the coefficient on the offer rate is now larger, with a negative coefficient on the interaction term. Although similarly signed for men, neither is significant statistically. This suggests that women exhibit evidence of information asymmetries with respect to selection, whereas men do not. However, note that it is not as simple as women being sensitive to rates and men not. See Karlan and Zinman (2004) for an analysis of demand for credit, in which we find that both men and women are responsive (although women are more elastic than men).

E. Observable Determinants of Default and Selection on Observables

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, 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-squared in Tables 4 and 9).

Since the Lender does not use the full information available to make credit decisions, we can examine whether this remaining information provides further evidence of selection being

determined by “riskiness” (in this case observable, but not used). Table 10 presents this further robustness check. First, we predict the past due amount (from Table 9), and then we regress the binary decision to apply for a loan on the offer interest rate and an interaction term of the offer rate times the predicted past due amount. Indeed, we find for women that riskier women are less elastic with respect to the interest rate. Men, on the other hand, do not exhibit this selection effect.

F. 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 of transacting, private information becomes public.²⁶ 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^0 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.

G. Magnitude Calculations Comparing Observables and Unobservable Effects

We 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. The Lender uses the three-tiered risk category to determine interest rates. On average, the high risk clients have 11.7 percentage points less default than the low risk clients. The moral hazard effect, over the 400 basis point spread between the normal rate provided to high versus low risk clients, predicts a 4.8 percentage point decrease in default rates for me (Table 5 Column 1). For women, the adverse selection effect also predicts a 4.8 percentage point reduction in default rate (Table 5 Column 7). Hence, 41% of the reduction in default rates from high to low risk clients can be explained not by

²⁶ 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.

the observable difference, but rather by the lower interest rate mitigating the information asymmetry problem.

VIII. Conclusion

We develop a new market field experiment methodology that disentangles 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 evidence of adverse selection among females. The effects we observe are significant, both economically and statistically. Although the results differ by gender, the overall findings provide clean empirical evidence of significant information asymmetries in consumer credit markets.

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 prevailing in this market. Related work explores whether marketing techniques can complement traditional risk assessment in helping to screen applicants (Bertrand, Karlan, Mullainathan, Shafir and Zinman, 2004). On the other hand, adverse selection may be obscured by concurrent advantageous selection, and/or be more important among populations we do not observe in this experiment; principally, 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 of 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. For this reason, we are currently conducting a new experiment in which we randomly “duration” marginal borrowers and follow-up with household surveys designed to document use of funds, as well as measure a broad range of impacts.

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Theory Appendix

Here is a more formal derivation of how our research design maps into classic theoretical models of private information, and thereby permits identification of unobservable selection and moral hazard 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.²⁷ 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 $p(q, e)$ and fail with probability $1-p$, where 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. $\frac{\partial p(q, e)}{\partial q} < 0, \frac{\partial p(q, e)}{\partial e} > 0, \frac{\partial^2 p(q, e)}{\partial e^2} < 0.$

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

²⁷ In our setting, the empirical counterpart to the homogeneity assumption is considering each of the client categories separately.

I.e., q is indexed such that it is increasing in risk, the probability of repayment is increasing in the borrower's effort, 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* (Eaton and Gersovitz, 1981, Ghosh and Ray, 2001). Then default occurs if and only if the project doesn't succeed under the additional simplifying assumption that:

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

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.²⁸

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.

Stiglitz and Weiss (1981) shows that *adverse selection* results if this relationship is positive.

Formally,

²⁸ We also suppress 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 .

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

Where C is a constant. The equation states 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.

De Meza and Webb (1987) shows that *advantageous selection* results if risk and returns are negatively correlated. Formally:

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

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 return (after the effort choice is made) as:

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

Where we assume, quite plausibly, that borrowers are price takers (see footnote 19). We ignore the Lender’s problem 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.²⁹

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 fixed, the agent solves

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

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

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

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 return in case of success (i.e., the return function is convex). Proof is at the end of this Appendix. Note this formulation focuses on moral hazard in effort rather than in *project choice* a la SW.

The next step in solving the borrower's problem is to examine 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 returns of exactly zero. That is,

²⁹ In practice, 84% of applicants were approved in the experiment. More generally, one can think of any rejected

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

Proposition 2. If the SW 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 no effect through effort since it 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 the DW 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 the end of this Appendix for 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:

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

Proposition 3. The marginal effect of r^o on the default probability captures the effect of selection.

If the SW assumption holds, then:

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

borrowers as being *observably* differentiated— and recall that our model conditions on observable information.

The proof is a direct application of proposition 2. If instead the DW assumption 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,

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

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

Incorporating the dynamic repayment incentive W is an application of models designed to explore the *voluntary default* version of moral hazard (Eaton and Gersovitz, 1981, Ghosh and Ray, 2001). Consider 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 will offer a second period rate r_2^s which is lower than the rate offered in the case where the borrower defaulted in the first period; i.e., $r_2^s < r_2^f$. Basically, the Lender offers a reward for repayment (see Eaton and Gersowitz (1981); or Ghosh, Mookherjee, and Ray (2000) for a more detailed reference).

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

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

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

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

The borrower chooses the first period effort, e_1 , taking the optimal value for e_2 into account. Now the borrower's expected return in the first period is

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

Note that the first-order condition now reads

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

The third summand in the denominator of the first-order condition is positive (as effort increases, the expected probability of default should decrease), so a reduction in interest rates conditional on repayment will increase borrower effort compared to a benchmark in which there is no discount conditional first-period repayment.

Proof of Proposition 1.

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

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

The right-hand side of this first-order condition is smaller than the one for which effort is unobservable, making 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

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

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

Proof of Proposition 2.

Assume the SW assumption (3a) holds. Recall that the marginal borrower's return once the offer rate is announced is by definition. Since the expected returns are increasing in q only applicants with q 's higher than marginal borrower's \hat{q} are going to have nonnegative expected returns. Accordingly these borrowers with $q > \hat{q}$ form the pool of applicants.

Totally differentiating the marginal applicant condition yields:

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

since p is decreasing in unobservable risk q . If instead the DW assumption (3b) holds the steps are the same but the signs are the opposite. In particular expected returns are decreasing in q and so only safer projects than the marginal q apply. In this case the total differentiation produces:

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

Figure 1: Stylized Depiction of Experimental Design

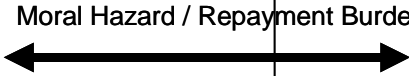
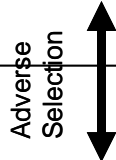
	High Contract Rate	Low Contract Rate
High Offer Rate		
Low Offer Rate	N/A	

Figure 2: Operational Steps of Experiment

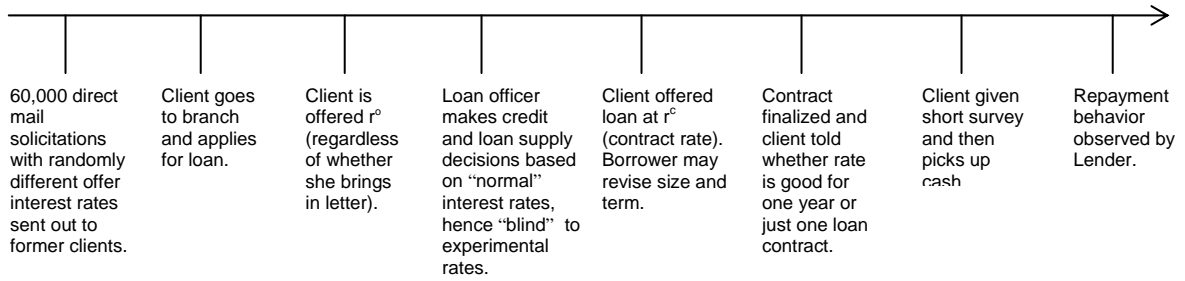


Figure 3: Scatter plot of Contract versus Offer Interest Rates on 4 Month Loans

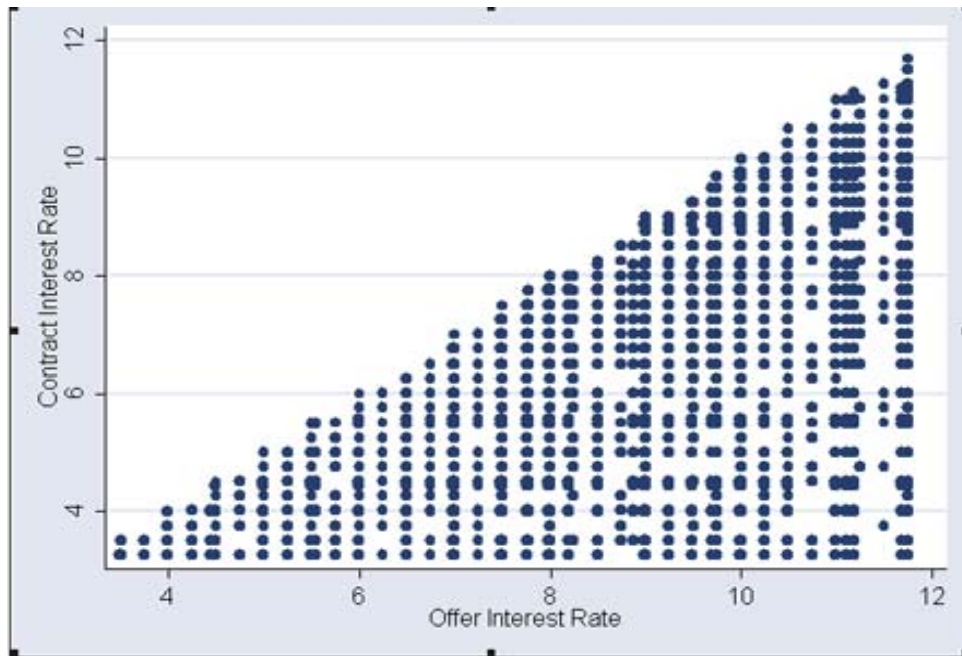
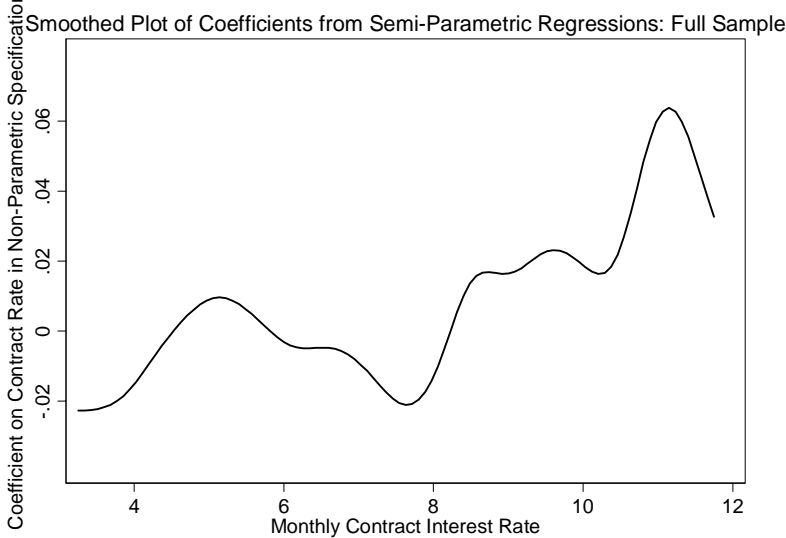


Figure 4: Non-Parametric Selection Effects, Full Sample



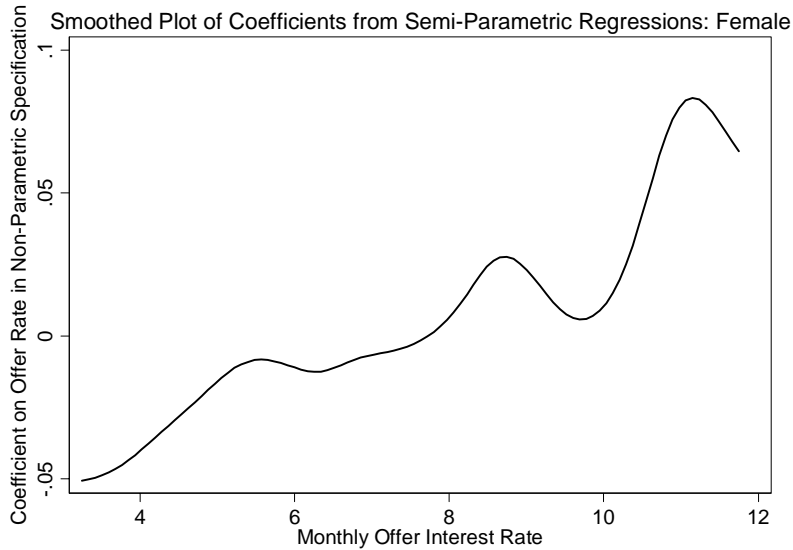
This graph shows the coefficients on the indicator variables (y-axis) for each individual Offer Interest Rate (x-axis), after smoothing. Table 4, Column 8 reports the results from this specification. This graph can be interpreted as the selection effect, with an upward slope indicating adverse selection.

Figure 5 : Non-Parametric Debt Burden Effects, Full Sample



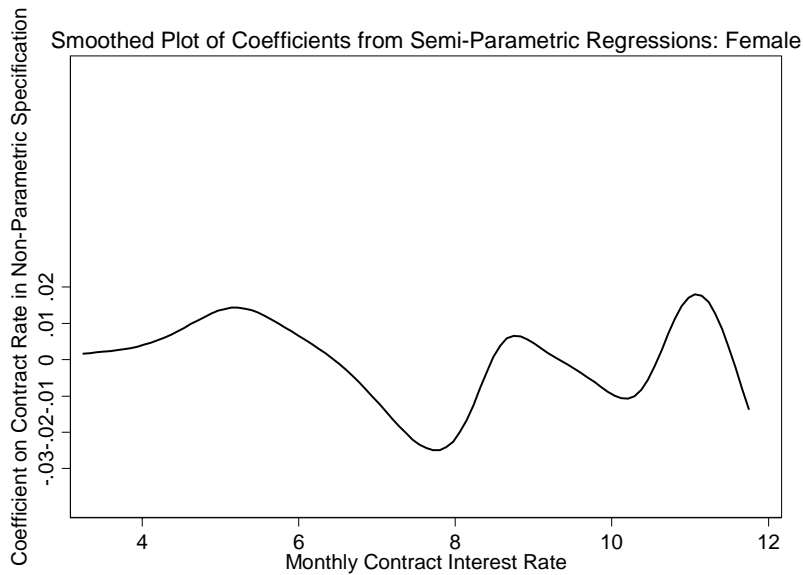
This graph shows the coefficients on the indicator variables (y-axis) for each individual Contract Interest Rate (x-axis), after smoothing. Table 4, Column 8 reports the results from this specification. This graph can be interpreted as the moral hazard/debt burden effect, with an upward slope indicating moral hazard.

Figure 6 : Non-Parametric Selection Effects, Female



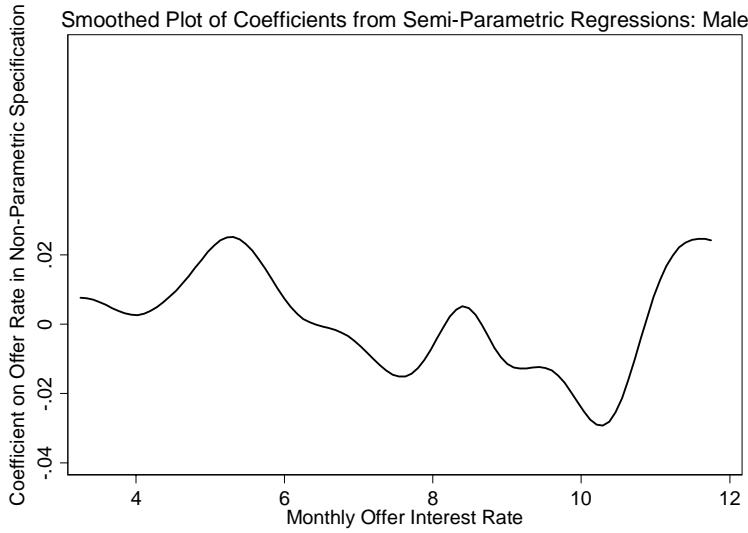
This graph shows the coefficients on the indicator variables (y-axis) for each individual Offer Interest Rate (x-axis), after smoothing, and for females only. Table 4, Column 8 reports the results from this specification but for the entire sample. The female-only version of this particular specification is not shown in the tables. This graph can be interpreted as the selection effect, with an upward slope indicating adverse selection.

Figure 7 : Non-Parametric Debt Burden Effects, Female



This graph shows the coefficients on the indicator variables (y-axis) for each individual Contract Interest Rate (x-axis), after smoothing, and for females only. Table 4, Column 8 reports the results from this specification but for the entire sample. The female-only version of this particular specification is not shown in the tables. This graph can be interpreted as the moral hazard/debt burden effect, with an upward slope indicating moral hazard.

Figure 8 : Non-Parametric Selection Effects, Male



This graph shows the coefficients on the indicator variables (y-axis) for each individual Offer Interest Rate (x-axis), after smoothing, and for males only. Table 4, Column 8 reports the results from this specification but for the entire sample. The male-only version of this particular specification is not shown in the tables. This graph can be interpreted as the selection effect, with an upward slope indicating adverse selection.

Figure 9 : Non-Parametric Debt Burden Effects, Male



This graph shows the coefficients on the indicator variables (y-axis) for each individual Contract Interest Rate (x-axis), after smoothing, and for males only. Table 4, Column 8 reports the results from this specification but for the entire sample. The male-only version of this particular specification is not shown in the tables. This graph can be interpreted as the moral hazard/debt burden effect, with an upward slope indicating moral hazard.

Table 1: Summary Statistics

	<i>All</i>	<i>Borrowed</i>	<i>Did Not Borrow</i>	<i>Observable Risk Category</i>		
				<i>High Risk</i>	<i>Medium Risk</i>	<i>Low Risk</i>
A. Full Sample						
# of months since last loan	10.3 (6.9)	5.9 (5.8)	10.6 (6.8)	12.7 (6.1)	2.8 (1.7)	2.8 (1.6)
Size of last loan prior to project	1116.4 (829.9)	1156.0 (825.7)	1113.1 (830.2)	1086.4 (785.2)	1176.5 (878.4)	1229.7 (994.5)
# of prior loans with the lender	4.3 (3.9)	4.9 (4.2)	4.2 (3.8)	3.6 (3.5)	5.7 (4.2)	6.6 (4.3)
Term of last loan prior to project						
1 or 2 months	1,656 2.88%	132 3.04%	1,524 2.87%	1,407 3.26%	93 1.50%	156 1.92%
4 months	53,296 92.64%	3,939 90.59%	49,357 92.80%	40,687 94.18%	5,658 91.17%	6,951 85.54%
6 months	2,030 3.53%	223 5.13%	1,807 3.40%	887 2.05%	369 5.95%	774 9.52%
12 months	551 0.96%	54 1.24%	497 0.93%	220 0.51%	86 1.39%	245 3.02%
Number of Observations	57,533	4,348	53,185	43,201	6,206	8,126
B. Randomized Variables						
Offer Interest Rate	7.88 (2.42)	7.18 (2.30)	7.94 (2.42)	8.36 (2.43)	7.20 (1.85)	5.80 (1.40)
Contract Interest Rate	7.08 (2.42)	6.52 (2.26)	7.12 (2.42)	7.47 (2.49)	6.55 (1.89)	5.38 (1.34)
Proportion Receiving Rate for One year (vs. one loan)	0.43 (0.50)	0.47 (0.50)	0.43 (0.49)	0.43 (0.50)	0.44 (0.50)	0.42 (0.49)
Proportion Receiving a Contract Rate < Offer Rate	0.41 (0.49)	0.40 (0.49)	0.41 (0.49)	0.42 (0.49)	0.41 (0.49)	0.38 (0.49)
C. Default Measure						
Monthly Average Past Due Amount		152.56 359.28		180.13 404.86	224.49 408.53	57.40 181.67
Monthly Avg Past Due Amount, Proportion of Principal		0.122 (0.277)		0.154 (0.315)	0.169 (0.309)	0.037 (0.129)
Monthly Average # of Payments in Arrears		0.434 (0.899)		0.528 (1.020)	0.625 (0.990)	0.147 (0.444)
Account is in Collection (3+ months arrears)		0.039 (0.194)		0.046 (0.210)	0.058 (0.235)	0.013 (0.116)
Number of Observations		4,348		2,090	941	1,317

Standard deviations are in parentheses

Table 2: Randomization & Experimental Procedure Validation

<i>Dependent Variable</i>	<i>Year Long</i>			
	<i>Contract Rate</i>	<i>Offer Rate</i>	<i>Rate=1</i>	<i>Borrowed=1</i>
	(1)	(2)	(3)	(4)
Female	0.002 (0.019)	0.018 (0.020)	-0.001 (0.003)	
External credit score	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	
Married	0.017 (0.020)	0.020 (0.021)	0.005 (0.004)	
Log (Size of last loan prior to project)	-0.016 (0.016)	-0.005 (0.017)	-0.004 (0.003)	
Term of last loan prior to project	-0.011 (0.011)	-0.011 (0.010)	-0.001 (0.002)	
Offer Rate				-0.003*** (0.001)
Contract Rate				0.001 (0.001)
Constant	7.645*** (0.280)	8.320*** (0.278)	0.216*** (0.051)	0.068*** (0.004)
Observations	57339	57339	57339	57533
R-squared	0.10	0.14	0.37	0.04

Standard errors in parentheses

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

Columns 1 through 3 test whether the randomized variables are correlated with information observable before the experiment launch. Column 4 shows that the decision to borrow was effected by the Offer Interest Rate, but not the Contract Interest Rate, hence verifying the internal controls of the experimental protocol.

Risk level, wave and branch dummies included in all specifications.

Table 3: Disentangling Selection on Unobservables from Debt Burden Effects: Comparison of Means

	Moral Hazard / Debt Burden Effects			Selection Effects		
	High Offer, High Contract	High Offer, Low Contract	t-stat diff<>0	High Offer, Low Contract	Low Offer, Low Contract	t-stat diff<>0
Full Sample						
Average Monthly Past Due	182.14 (9.90)	137.91 (13.63)	2.63***	137.91 (13.63)	133.75 (7.18)	0.27
Average Monthly Proportion Past Due	0.146 (0.007)	0.128 (0.012)	1.30	0.128 (0.012)	0.102 (0.005)	2.04**
Proportion of Months in Arrears	0.503 (0.024)	0.434 (0.036)	1.60	0.434 (0.036)	0.379 (0.018)	1.35
Account in Collection Status	0.046 (0.005)	0.0384 (0.008)	0.99	0.0384 (0.008)	0.0335 (0.004)	1.50
# of observations	1636	625		625	2087	
Female						
Average Monthly Past Due	157.18 (14.00)	124.51 (16.10)	1.53	124.51 (16.10)	113.64 (9.49)	0.58
Average Monthly Proportion Past Due	0.123 (0.010)	0.126 (0.016)	-0.17	0.126 (0.016)	0.082 (0.006)	2.51***
Proportion of Months in Arrears	0.429 (0.032)	0.415 (0.051)	0.24	0.415 (0.051)	0.311 (0.022)	1.87*
Account in Collection Status	0.033 (0.006)	0.036 (0.011)	-0.65	0.036 (0.011)	0.030 (0.005)	1.88*
# of observations	779	307		307	1047	
Male						
Average Monthly Past Due	204.83 (13.93)	150.84 (21.83)	2.08**	150.84 (21.83)	154.00 (10.75)	-0.13
Average Monthly Proportion Past Due	0.166 (0.011)	0.129 (0.016)	1.89*	0.129 (0.016)	0.121 (0.008)	0.41
Proportion of Months in Arrears	0.571 (0.036)	0.451 (0.052)	1.90*	0.451 (0.052)	0.446 (0.028)	0.07
Account in Collection Status	0.058 (0.008)	0.041 (0.011)	1.87*	0.041 (0.011)	0.038 (0.006)	0.26
# of observations	857	318		318	1040	

In this stylized demonstration of the results, "high" was defined as above the median offer rate for that risk category. This was equal to 7.77% for high risk clients, 7.50% for medium risk clients and 6.00% for low risk clients.

T-tests assume unequal variances across groups.

Table 4: Full Sample Results: Disentangling Selection on Unobservables from Moral Hazard

<i>Dependent Variable</i>	OLS			Tobit		Probit	OLS (semi-parametric)	
	<i>Monthly Average Proportion Past Due</i>	<i>Proportion of Months in Arrears</i>	<i>Account in Collection Status</i>	<i>Monthly Average Proportion Past Due</i>	<i>Proportion of Months in Arrears</i>	<i>Account in Collection Status</i>	<i>Monthly Average Proportion Past Due</i>	<i>Monthly Average Proportion Past Due</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Offer Rate (AS)	0.004 (0.003)	0.002 (0.004)	0.007 (0.005)	0.004 (0.006)	0.003 (0.007)	0.007* (0.004)	0.003 (0.003)	Indicator Variables
Contract Rate (MH)	0.005 (0.003)	0.006* (0.003)	0.001 (0.005)	0.008 (0.006)	0.010 (0.007)	0.001 (0.004)	Indicator Variables	0.003 (0.003)
Rate Valid for One Year (vs one loan)	-0.015** (0.007)	-0.016* (0.008)	-0.019** (0.009)	-0.030** (0.012)	-0.031** (0.016)	-0.019** (0.008)	-0.015** (0.007)	-0.014** (0.007)
Constant	0.260*** (0.068)	0.032 (0.062)	0.104 (0.063)	0.013 (0.128)	-0.379*** (0.139)		0.273*** (0.071)	0.254*** (0.067)
Observations	4,348	4,348	4,348	4,348	4,348	4,348	4348	4,348
R-squared	0.09	0.11	0.04				0.07	0.07

Standard errors corrected for clustering at the branch level, and 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 are included. A positive coefficient on the Offer Rate variable should be interpreted as evidence of adverse selection, whereas a positive coefficient on either the Contract Rate or the Rate Valid for One Year variables should be interpreted as a moral hazard effect on ex-post behavior.

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

<i>Dependent Variable</i>	Male						Female					
	OLS			Tobit		Probit	OLS			Tobit		Probit
	<i>Monthly Average Proportion Past Due</i>	<i>Proportion of Months in Arrears</i>	<i>Account in Collection Status</i>	<i>Monthly Average Proportion Past Due</i>	<i>Proportion of Months in Arrears</i>	<i>Account in Collection Status</i>	<i>Monthly Average Proportion Past Due</i>	<i>Proportion of Months in Arrears</i>	<i>Account in Collection Status</i>	<i>Monthly Average Proportion Past Due</i>	<i>Proportion of Months in Arrears</i>	<i>Account in Collection Status</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Offer Rate (AS)	-0.003 (0.004)	-0.004 (0.005)	0.001 (0.007)	-0.008 (0.008)	-0.008 (0.010)	0.001 (0.007)	0.012*** (0.004)	0.008* (0.004)	0.013** (0.005)	0.017** (0.007)	0.014 (0.009)	0.011*** (0.004)
Contract Rate (MH)	0.012*** (0.004)	0.014*** (0.005)	0.010 (0.007)	0.019** (0.008)	0.021** (0.010)	0.009 (0.006)	-0.003 (0.004)	-0.002 (0.005)	-0.009 (0.006)	-0.003 (0.007)	-0.004 (0.009)	-0.007* (0.004)
Rate Valid for One Year (vs one loan)	-0.019 (0.012)	-0.025** (0.012)	-0.020 (0.014)	-0.043** (0.021)	-0.049** (0.023)	-0.020 (0.014)	-0.012 (0.010)	-0.006 (0.012)	-0.018 (0.012)	-0.014 (0.018)	-0.008 (0.025)	-0.017 (0.011)
Constant	0.328*** (0.122)	0.075 (0.101)	0.048 (0.098)	0.121 (0.191)	-0.262 (0.203)		0.175** (0.081)	-0.033 (0.066)	0.146* (0.082)	-0.131 (0.183)	-0.578*** (0.177)	
Observations	2215	2215	2215	2,215	2,215	2,215	2133	2133	2133	2133	2133	2133
R-squared	0.06	0.12	0.04				0.06	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: Concurrent Adverse and Advantageous Selection?

<i>Dependent Variable</i>	OLS				
	<i>Monthly Average Proportion Past Due</i>	<i>Proportion of Months in Arrears</i>	<i>Account in Collection Status</i>	<i>Monthly Average Proportion Past Due</i>	<i>Monthly Average Proportion Past Due</i>
	All (1)	All (2)	All (3)	Female (4)	Male (5)
Offer Rate (AS)	0.025 (0.023)	0.036* (0.019)	0.047* (0.026)	0.036 (0.030)	0.008 (0.036)
Contract Rate (MH)	0.005 (0.003)	0.007* (0.003)	0.001 (0.005)	-0.003 (0.004)	0.012*** (0.004)
Rate Valid for One Year (vs one loan)	-0.015** (0.007)	-0.015* (0.008)	-0.018** (0.009)	-0.012 (0.010)	-0.017 (0.012)
Log(Gross Income)	0.010 (0.019)	-0.007 (0.016)	-0.008 (0.022)	0.025 (0.022)	-0.018 (0.032)
Offer Rate*Log(Gross Income)	-0.003 (0.003)	-0.004* (0.002)	-0.005 (0.003)	-0.003 (0.004)	-0.001 (0.004)
Constant	0.145 (0.155)	-0.059 (0.132)	-0.006 (0.165)	-0.009 (0.202)	0.379 (0.284)
Observations	4348	4348	4348	2133	2215
Prob(OR = OR*GI = 0)	0.20	0.16	0.09	0.01	0.70
R-squared	0.06	0.12	0.04	0.06	0.06

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.

The Prob(OR = OR*GI = 0) row, 2nd from the bottom, reports the t-test for joint significant of the Offer Rate variable and the Offer Rate interacted with the log of gross income.

Standard errors corrected for clustering within branch.

Table 7: Disentangling Selection on Unobservables from Moral Hazard, by Gender & Demographic

Dependent Variable: Monthly Average Proportion Past Due, OLS

Sample Restriction	Male				Female			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Offer Rate (AS)	-0.005 (0.007)	-0.001 (0.007)	-0.005 (0.005)	-0.007 (0.017)	0.015*** (0.005)	0.010** (0.005)	0.018*** (0.006)	0.018 (0.020)
Contract Rate (MH)	0.014** (0.007)	0.015** (0.007)	0.012** (0.005)	0.015 (0.017)	-0.006 (0.006)	-0.006 (0.005)	-0.012** (0.006)	-0.005 (0.021)
Rate Valid for One Year (vs one loan)	-0.009 (0.019)	-0.024 (0.019)	-0.016 (0.016)	0.006 (0.050)	-0.013 (0.015)	0.006 (0.018)	0.003 (0.014)	-0.040 (0.051)
Married	-0.019 (0.036)				-0.018 (0.029)			
Offer Rate*Married	0.004 (0.010)				-0.006 (0.008)			
Contract Rate*Married	-0.004 (0.010)				0.008 (0.008)			
Rate Valid for One Year*Married	-0.019 (0.024)				0.005 (0.022)			
# of Dependents		0.008 (0.009)				-0.015 (0.012)		
Offer Rate*# of Dependents		-0.001 (0.002)				0.001 (0.003)		
Contract Rate*# of Dependents		-0.002 (0.002)				0.002 (0.003)		
Rate Valid for One Year*# of Dependents		0.001 (0.005)				-0.010 (0.007)		
Educated			-0.028 (0.049)				-0.004 (0.033)	
Offer Rate*Educated			0.007 (0.010)				-0.014 (0.010)	
Contract Rate*Educated			-0.003 (0.009)				0.019* (0.011)	
Rate Valid for One Year*Educated			-0.007 (0.026)				-0.027 (0.021)	
Age				-0.003 (0.002)				-0.002 (0.001)
Offer Rate*Age				0.000 (0.000)				-0.000 (0.000)
Contract Rate*Age				-0.000 (0.000)				0.000 (0.000)
Rate Valid for One Year*Age				-0.001 (0.001)				0.001 (0.001)
Constant	0.334*** (0.123)	0.315** (0.123)	0.340** (0.129)	0.403*** (0.141)	0.170** (0.083)	0.189** (0.083)	0.191** (0.081)	0.258** (0.106)
Observations	2198	2198	2215	2215	2119	2119	2133	2133
R-squared	0.06	0.07	0.06	0.07	0.06	0.06	0.06	0.07
Prob (Offer rate = interaction term = 0)	0.73047	0.70613	0.57949	0.78188	0.01243	0.01094	0.00671	0.03083
Prob (Contract rate = interaction term = 0)	0.02272	0.03910	0.03323	0.01848	0.59052	0.54258	0.12625	0.89408
Prob (Year long offer= interaction term = 0)	0.15955	0.21168	0.26762	0.21656	0.56654	0.19122	0.28050	0.52240

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 Promotional Rate Valid for One Year versus One Loan

Dependent Variable: Monthly Average Proportion Past Due, OLS

	<i>Sample Restriction</i>	<i>Full Sample</i>	<i>Female</i>	<i>Male</i>
		(1)	(2)	(3)
Offer Rate (AS)		0.004 (0.003)	0.012*** (0.004)	-0.004 (0.005)
Contract Rate (MH)		0.002 (0.004)	-0.001 (0.005)	0.005 (0.005)
Rate valid for one year, Indicator Variable		0.001 (0.015)	-0.024 (0.016)	0.031 (0.023)
Rate valid for one year, # of points below normal rate		-0.005 (0.004)	0.003 (0.003)	-0.014** (0.006)
Constant		0.278*** (0.071)	0.162* (0.083)	0.381*** (0.123)
Observations		4348	2133	2215
Adjusted R-squared		0.0532	0.0505	0.0559
Prob(both yearlong variables = 0)		0.0419	0.3246	0.0410

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

Table 9: Adding Observable Determinants of Default

Dependent Variable Sample Restriction	OLS											
	Monthly Average Proportion Past Due		Proportion of Months in Arrears		Account in Collection Status		Monthly Average Proportion Past Due					
	(1)	(2)	(3)	(4)	(5)	(6)	Low Risk		Medium Risk		High Risk	
	(7)	(8)	(9)	(10)	(11)	(12)						
Offer Rate (AS)	0.004 (0.003)		0.002 (0.003)		0.007 (0.005)		0.008 (0.007)		0.008 (0.010)		0.003 (0.004)	
Contract Rate (MH)	0.004 (0.003)		0.006* (0.003)		0.001 (0.005)		-0.005 (0.007)		-0.003 (0.010)		0.008** (0.004)	
Rate Valid for One Year (vs one loan)	-0.017** (0.007)		-0.017** (0.008)		-0.021** (0.009)		-0.003 (0.007)		-0.001 (0.019)		-0.030** (0.015)	
Log(loan size)	-0.029*** (0.008)	-0.030*** (0.008)	0.005 (0.008)	0.004 (0.008)	-0.003 (0.010)	-0.004 (0.010)	-0.010 (0.007)	-0.011 (0.007)	-0.040* (0.020)	-0.040* (0.020)	-0.037** (0.014)	-0.039*** (0.014)
Female	-0.027*** (0.009)	-0.027*** (0.009)	-0.034*** (0.010)	-0.035*** (0.010)	-0.029** (0.012)	-0.029** (0.012)	-0.014* (0.008)	-0.014* (0.008)	-0.013 (0.026)	-0.012 (0.026)	-0.047*** (0.015)	-0.048*** (0.015)
Age	0.000 (0.002)	0.000 (0.002)	0.001 (0.002)	0.001 (0.002)	0.002 (0.002)	0.002 (0.002)	0.001 (0.001)	0.001 (0.001)	0.002 (0.005)	0.002 (0.005)	0.001 (0.002)	0.001 (0.002)
Age squared	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000* (0.000)	-0.000* (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Years at Employer	-0.001 (0.001)	-0.001 (0.001)	-0.001** (0.001)	-0.001** (0.001)	-0.002* (0.001)	-0.002* (0.001)	-0.001* (0.001)	-0.001* (0.001)	-0.003 (0.002)	-0.003 (0.002)	-0.001 (0.001)	-0.001 (0.001)
Gross Income	0.004 (0.007)	0.004 (0.007)	-0.007* (0.004)	-0.007* (0.004)	-0.005 (0.004)	-0.005 (0.004)	-0.002 (0.002)	-0.001 (0.002)	-0.006 (0.005)	-0.006 (0.005)	0.010 (0.012)	0.010 (0.012)
Predicted # of Years of Education	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.002 (0.001)	-0.002 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.003 (0.003)	-0.003 (0.003)	0.001 (0.002)	0.001 (0.002)
# of Dependants	-0.001 (0.003)	-0.001 (0.003)	0.000 (0.002)	0.000 (0.002)	-0.006** (0.003)	-0.006** (0.003)	-0.003 (0.002)	-0.002 (0.002)	-0.001 (0.008)	-0.001 (0.008)	0.002 (0.005)	0.001 (0.005)
External Credit Score	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000* (0.000)	-0.000* (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000* (0.000)	-0.000* (0.000)	-0.000* (0.000)	-0.000* (0.000)
No External Credit Score	-0.121*** (0.046)	-0.127*** (0.045)	-0.252*** (0.049)	-0.257*** (0.049)	-0.079* (0.045)	-0.084* (0.044)	-0.121*** (0.035)	-0.124*** (0.035)	-0.119 (0.101)	-0.124 (0.098)	-0.130* (0.071)	-0.135* (0.070)
Internal Credit Score	-0.001* (0.001)	-0.001* (0.001)	-0.001* (0.000)	-0.001** (0.000)	-0.002*** (0.001)	-0.002*** (0.001)	-0.001* (0.000)	-0.001* (0.000)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
Married	0.004 (0.009)	0.004 (0.009)	0.006 (0.009)	0.006 (0.009)	0.014 (0.012)	0.015 (0.012)	0.000 (0.008)	0.001 (0.008)	0.025 (0.023)	0.025 (0.023)	-0.004 (0.015)	-0.002 (0.015)
Home Bond	0.013 (0.018)	0.011 (0.018)	0.013 (0.021)	0.011 (0.022)	0.039* (0.022)	0.037* (0.022)	0.012 (0.018)	0.012 (0.018)	0.012 (0.030)	0.011 (0.030)	0.008 (0.034)	0.005 (0.034)
# of prior loans with the lender	-0.004*** (0.001)	-0.004*** (0.001)	-0.005*** (0.001)	-0.005*** (0.001)	-0.004*** (0.001)	-0.004*** (0.001)	-0.001** (0.001)	-0.001** (0.001)	-0.005* (0.003)	-0.005* (0.003)	-0.007*** (0.001)	-0.006*** (0.002)
# of months since last loan	0.005*** (0.001)	0.005*** (0.001)	0.004** (0.002)	0.004** (0.002)	0.005*** (0.002)	0.005*** (0.002)	-0.002 (0.003)	-0.002 (0.003)	0.003 (0.008)	0.003 (0.008)	0.005*** (0.002)	0.005*** (0.002)
Constant	0.535*** (0.112)	0.599*** (0.109)	0.359*** (0.088)	0.418*** (0.081)	0.306*** (0.099)	0.366*** (0.092)	0.136* (0.070)	0.162** (0.066)	1.066*** (0.340)	1.105*** (0.342)	0.372*** (0.123)	0.455*** (0.113)
Observations	4348	4348	4348	4348	4348	4348	1317	1317	941	941	2090	2090
R-squared	0.0909	0.0867	0.1587	0.1550	0.0699	0.0665	0.0433	0.0406	0.1061	0.1051	0.0561	0.0475
Adjusted r-squared	0.0829	0.0793	0.1513	0.1481	0.0617	0.0590	0.0225	0.0220	0.0797	0.0817	0.0428	0.0355

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

	<i>All</i>	<i>All</i>	<i>Female</i>	<i>Male</i>	<i>All</i>	<i>Female</i>	<i>Male</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Offer Rate (AS)	-0.003*** (0.000)	-0.004*** (0.001)	-0.006*** (0.002)	-0.002 (0.002)	-0.004*** (0.001)	-0.006*** (0.002)	-0.002 (0.002)
Low Risk	0.113*** (0.007)	0.097*** (0.008)	0.090*** (0.013)	0.107*** (0.010)	0.097*** (0.008)	0.090*** (0.013)	0.106*** (0.010)
Medium Risk	0.111*** (0.006)	0.121*** (0.006)	0.121*** (0.009)	0.121*** (0.008)	0.121*** (0.006)	0.121*** (0.009)	0.122*** (0.008)
Fitted values		-130.704** (57.465)	-213.538** (92.619)	-25.929 (85.933)	-133.613** (57.546)	-214.386** (94.168)	-33.061 (86.813)
Offer Rate* Predicted Past Due Amount		8.170 (6.766)	18.235* (9.992)	-3.076 (10.474)	8.513 (6.712)	18.359* (10.142)	-2.508 (10.588)
High Gross Income					-0.005 (0.008)	0.001 (0.012)	-0.010 (0.011)
Offer Rate*High Gross Income					0.000 (0.001)	0.000 (0.002)	0.001 (0.001)
Observations	48852	52985	25221	27764	52985	25221	27764

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 for non-applicants.

Marginal values reported for coefficients in probit specifications.

Table 11: Do Information Asymmetries Diminish for Clients with More Frequent Borrowing History?

OLS

<i>Dependent Variable</i>	<i>Monthly Average Proportion Past Due</i>			<i>Monthly Average Proportion Past Due</i>			<i>Monthly Average Proportion Past Due</i>		
	Full Sample			Female			Male		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Offer Rate (AS)	0.010*** (0.004)	0.004 (0.003)	0.004 (0.003)	0.018*** (0.005)	0.012*** (0.004)	0.011*** (0.004)	0.001 (0.006)	-0.003 (0.004)	-0.003 (0.004)
Contract Rate (MH)	0.005 (0.003)	0.011*** (0.004)	0.005 (0.003)	-0.002 (0.004)	0.003 (0.005)	-0.003 (0.004)	0.012*** (0.004)	0.017*** (0.005)	0.012*** (0.004)
Rate Valid for One Year (vs one loan)	-0.016** (0.007)	-0.016** (0.007)	-0.020 (0.013)	-0.011 (0.010)	-0.011 (0.010)	-0.011 (0.015)	-0.020 (0.012)	-0.020 (0.012)	-0.031 (0.022)
# of prior loans with the lender	0.003 (0.002)	0.002 (0.002)		0.005** (0.003)	0.003 (0.002)		0.001 (0.004)	0.000 (0.003)	
Offer Rate*# of prior loans	-0.001*** (0.000)			-0.002*** (0.000)			-0.001* (0.001)		
Contract Rate*# of prior loans		-0.001*** (0.000)			-0.001*** (0.000)			-0.001* (0.001)	
Rate Valid for One Year*# of prior loans			0.001 (0.001)			-0.000 (0.001)			0.002 (0.002)
Constant	0.244*** (0.068)	0.250*** (0.066)	0.284*** (0.068)	0.148* (0.083)	0.153* (0.082)	0.191** (0.082)	0.325*** (0.121)	0.330*** (0.120)	0.361*** (0.122)
Observations	4317	4317	4317	2119	2119	2119	2198	2198	2198
R-squared	0.06	0.06	0.06	0.07	0.07	0.06	0.07	0.07	0.07

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 1. Frequency of Monthly Offer and Contract Interest Rates

	Low Risk Clients				Medium Risk Clients				High Risk Clients			
	<i>Offer Interest</i>		<i>Contract Interest</i>		<i>Offer Interest</i>		<i>Contract Interest</i>		<i>Offer Interest</i>		<i>Contract Interest</i>	
	<i>Rate</i>	<i>Rate</i>	<i>Rate</i>	<i>Rate</i>	<i>Rate</i>	<i>Rate</i>	<i>Rate</i>	<i>Rate</i>	<i>Rate</i>	<i>Rate</i>	<i>Rate</i>	
	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent
3.25%	144	1.77%	304	3.74%	94	1.51%	172	2.77%	586	1.36%	1,017	2.35%
3.49%	281	3.46%	347	4.27%	110	1.77%	135	2.18%	756	1.75%	934	2.16%
3.50%	267	3.29%	393	4.84%	116	1.87%	163	2.63%	540	1.25%	931	2.16%
3.75%	32	0.39%	42	0.52%	18	0.29%	26	0.42%	53	0.12%	80	0.19%
3.99%	367	4.52%	580	7.14%	104	1.68%	229	3.69%	754	1.75%	1,400	3.24%
4.00%	199	2.45%	341	4.20%	99	1.60%	144	2.32%	525	1.22%	845	1.96%
4.25%	40	0.49%	61	0.75%	22	0.35%	29	0.47%	59	0.14%	69	0.16%
4.44%	208	2.56%	380	4.68%	79	1.27%	214	3.45%	494	1.14%	1,220	2.82%
4.49%	399	4.91%	330	4.06%	139	2.24%	136	2.19%	775	1.79%	866	2.00%
4.50%	176	2.17%	288	3.54%	99	1.60%	149	2.40%	591	1.37%	826	1.91%
4.75%	45	0.55%	39	0.48%	22	0.35%	29	0.47%	60	0.14%	77	0.18%
4.99%	202	2.49%	378	4.65%	117	1.89%	211	3.40%	713	1.65%	1,347	3.12%
5.00%	283	3.48%	332	4.09%	119	1.92%	168	2.71%	550	1.27%	809	1.87%
5.25%	45	0.55%	49	0.60%	19	0.31%	26	0.42%	67	0.16%	77	0.18%
5.49%	338	4.16%	387	4.76%	149	2.40%	239	3.85%	712	1.65%	1,330	3.08%
5.50%	426	5.24%	415	5.11%	97	1.56%	144	2.32%	604	1.40%	761	1.76%
5.55%	288	3.54%	267	3.29%	81	1.31%	120	1.93%	513	1.19%	660	1.53%
5.75%	46	0.57%	56	0.69%	20	0.32%	27	0.44%	74	0.17%	92	0.21%
5.99%	495	6.09%	409	5.03%	213	3.43%	259	4.17%	712	1.65%	1,175	2.72%
6.00%	402	4.95%	315	3.88%	118	1.90%	141	2.27%	586	1.36%	766	1.77%
6.25%	49	0.60%	51	0.63%	24	0.39%	25	0.40%	74	0.17%	80	0.19%
6.50%	388	4.77%	377	4.64%	125	2.01%	201	3.24%	611	1.41%	1,286	2.98%
6.75%	422	5.19%	335	4.12%	148	2.38%	198	3.19%	569	1.32%	903	2.09%
6.99%	464	5.71%	308	3.79%	231	3.72%	192	3.09%	775	1.79%	903	2.09%
7.00%	435	5.35%	292	3.59%	201	3.24%	194	3.13%	855	1.98%	881	2.04%
7.25%	399	4.91%	273	3.36%	200	3.22%	205	3.30%	834	1.93%	1,028	2.38%
7.49%	575	7.08%	347	4.27%	260	4.19%	212	3.42%	1,015	2.35%	977	2.26%
7.50%	357	4.39%	229	2.82%	195	3.14%	166	2.67%	849	1.97%	825	1.91%
7.75%	354	4.36%	201	2.47%	181	2.92%	162	2.61%	909	2.10%	1,033	2.39%
7.77%	-	-	-	-	200	3.22%	138	2.22%	825	1.91%	719	1.66%
7.99%	-	-	-	-	224	3.61%	159	2.56%	1,029	2.38%	933	2.16%
8.00%	-	-	-	-	168	2.71%	160	2.58%	891	2.06%	830	1.92%
8.19%	-	-	-	-	235	3.79%	167	2.69%	1,024	2.37%	829	1.92%
8.25%	-	-	-	-	25	0.40%	28	0.45%	74	0.17%	79	0.18%
8.50%	-	-	-	-	215	3.46%	164	2.64%	830	1.92%	984	2.28%
8.75%	-	-	-	-	35	0.56%	23	0.37%	82	0.19%	77	0.18%
8.88%	-	-	-	-	221	3.56%	153	2.47%	805	1.86%	851	1.97%
8.99%	-	-	-	-	263	4.24%	174	2.80%	1,044	2.42%	814	1.88%
9.00%	-	-	-	-	214	3.45%	128	2.06%	877	2.03%	756	1.75%
9.25%	-	-	-	-	218	3.51%	145	2.34%	890	2.06%	867	2.01%
9.49%	-	-	-	-	300	4.83%	170	2.74%	1,162	2.69%	879	2.03%
9.50%	-	-	-	-	37	0.60%	28	0.45%	89	0.21%	82	0.19%
9.69%	-	-	-	-	234	3.77%	137	2.21%	1,201	2.78%	892	2.06%
9.75%	-	-	-	-	217	3.50%	116	1.87%	889	2.06%	727	1.68%
9.99%	-	-	-	-	-	-	-	-	1,242	2.87%	887	2.05%
10.00%	-	-	-	-	-	-	-	-	1,253	2.90%	876	2.03%
10.25%	-	-	-	-	-	-	-	-	1,276	2.95%	892	2.06%
10.49%	-	-	-	-	-	-	-	-	1,494	3.46%	964	2.23%
10.50%	-	-	-	-	-	-	-	-	1,282	2.97%	833	1.93%
10.75%	-	-	-	-	-	-	-	-	93	0.22%	73	0.17%
10.99%	-	-	-	-	-	-	-	-	1,390	3.22%	899	2.08%
11.00%	-	-	-	-	-	-	-	-	1,385	3.21%	857	1.98%
11.11%	-	-	-	-	-	-	-	-	1,345	3.11%	800	1.85%
11.19%	-	-	-	-	-	-	-	-	1,498	3.47%	867	2.01%
11.25%	-	-	-	-	-	-	-	-	104	0.24%	77	0.18%
11.50%	-	-	-	-	-	-	-	-	99	0.23%	72	0.17%
11.69%	-	-	-	-	-	-	-	-	1,431	3.31%	834	1.93%
11.75%	-	-	-	-	-	-	-	-	1,382	3.20%	753	1.74%
Total	8,126	100%	8,126	100%	6,206	100%	6,206	100%	43,201	100%	43,201	100%

Appendix Table 2. Primary Results: Disentangling Selection on Unobservables from Moral Hazard

Sample Restricted to Those Offered $\leq 7.75\%$, the Ceiling for Low Risk Clients

	OLS			Tobit		Probit	OLS (semi-parametric)	
<i>Dependent Variable</i>	<i>Monthly</i>			<i>Monthly</i>			<i>Monthly</i>	
	<i>Average</i>	<i>Proportion</i>	<i>Account in</i>	<i>Average</i>	<i>Proportion</i>	<i>Account in</i>	<i>Average</i>	<i>Monthly</i>
	<i>Proportion</i>	<i>of Months</i>	<i>Collection</i>	<i>Proportion</i>	<i>of Months</i>	<i>Collection</i>	<i>Proportion</i>	<i>Average</i>
	<i>Past Due</i>	<i>in Arrears</i>	<i>Status</i>	<i>Past Due</i>	<i>in Arrears</i>	<i>Status</i>	<i>Past Due</i>	<i>Past Due</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Offer Rate (AS)	0.006 (0.009)	0.008 (0.007)	0.010 (0.012)	0.011 (0.014)	0.013 (0.013)	0.009 (0.012)	-0.004 (0.010)	Indicator Variables
Contract Rate (MH)	0.007* (0.004)	0.007* (0.004)	0.001 (0.006)	0.012* (0.006)	0.012 (0.007)	0.002 (0.006)	Indicator Variables	0.006 (0.004)
Rate Valid for One Year (vs one loan)	-0.017 (0.016)	-0.008 (0.016)	-0.020 (0.017)	-0.021 (0.027)	-0.016 (0.029)	-0.019 (0.017)	-0.015 (0.016)	-0.015 (0.017)
Constant	0.316** (0.157)	-0.023 (0.133)	0.060 (0.162)	0.047 (0.259)	-0.391 (0.255)		0.488*** (0.171)	0.398*** (0.136)
Observations	1762	1762	1762	1762	1762	1760	1762	1762
R-squared	0.02	0.06	0.01				0.05	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 at the branch level.

Appendix Table3. Primary Analysis with Offer Rate Alone (No Contract Rate)

Dependent Variable: Monthly Average Past Due

	All	All	Female	Male	All	All
	Offer Rate \leq 7.75					
	Full Sample	Offer Rate = Contract Rate	Offer Rate = Contract Rate	Offer Rate = Contract Rate	Full Sample	Offer Rate = Contract Rate
	(1)	(2)	(3)	(4)	(5)	(6)
Offer Interest Rate	0.008*** (0.002)	0.010*** (0.003)	0.009** (0.003)	0.010** (0.004)	0.011 (0.008)	0.015 (0.011)
Rate valid for one year (Indicator Variable)	-0.016** (0.007)	-0.008 (0.009)	-0.015 (0.012)	-0.002 (0.014)	-0.018 (0.016)	-0.000 (0.022)
Constant	0.263*** (0.068)	0.151 (0.122)	-0.050 (0.062)	0.238 (0.187)	0.320** (0.158)	0.119 (0.236)
Observations	4348	2619	1280	1339	1762	1007
R-squared	0.06	0.06	0.06	0.06	0.02	0.02

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

This table repeats the primary specification, but ignoring the contract rate. Hence, without the contract rate, this estimation does not disentangle ex-ante selection effects for ex-post incentive effects.

Standard errors corrected for clustering at the branch level, and in parentheses.

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

Dependent Variable: Monthly Average Past Due, IV Approach

	<i>All</i>	<i>Female</i>	<i>Male</i>
	(1)	(2)	(3)
Total Debt Burden, Rand 000s, at Offer Interest Rate	0.069 (0.050)	0.161** (0.062)	-0.022 (0.070)
Total Debt Burden, Rand 000s, at Contract Interest Rate	0.061 (0.056)	-0.049 (0.081)	0.159** (0.070)
Constant	1.711*** (0.349)	1.524*** (0.447)	1.763*** (0.518)
Observations	4348	2133	2215

Robust standard errors in parentheses

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

Two endogenous variables: (1) total debt burden, principal plus interest, on contract and (2) total debt burden, principal plus interest, using the offer interest rate rather than the contract interest rate.

Instrumental variables are (1) offer interest rate and (2) contract interest rate.

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, and in parentheses.