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Preface

One important theme in the work of the Center for Global Development is the search for ways to make foreign aid agencies more effective. It is a tough problem because aid agencies are not accountable to the people they aim to serve in aid-receiving countries. One symptom of this lack of accountability, noted by CGD's Evaluation Gap Working Group, is that donors too rarely commission rigorous, independent studies of how the programs they back affect clients. This leaves donors vulnerable to development fads and waste.

CGD non-resident fellow Dean Karlan and his co-authors are exemplars of a growing movement within academia to change that. This paper comes out of a program of work that strives to bring the highest scientific standards to the study of microfinance, an area in which public and private donors are heavily involved. Understanding how microfinance affects clients is not straightforward because there are several possible explanations for why, say, a borrower is doing well compared to her non-borrowing peers. The credit may be helping—or perhaps she only borrowed because she was already well-off. This, and other papers in the series, elucidates cause and effect by performing controlled experiments, in which a few parameters are randomly varied and the effects measured. The result is sharper answers, in specific contexts, to questions such as: How sensitive are potential borrowers to high interest rates? At the margin, does access to credit increase their incomes? Does it empower women? In the solidarity group lending method made famous by the Grameen Bank, wherein small groups of borrowers guarantee each other's loans, is that mutual guarantee the essential glue that holds the system together?

This paper contributes both by giving donors insight into the programs they fund, and, more generally, by demonstrating the value of rigorous impact evaluation.

I. Introduction

Information asymmetries are important in theory. Stiglitz and Weiss (1981) sparked a large theoretical literature on the role of asymmetric information in credit markets that has influenced economic policy and lending practice worldwide (Bebczuk 2003; Armendariz de Aghion and Morduch 2005). Theories show that information frictions and ensuing credit market failures can create inefficiency at both the micro and the macro level, via underinvestment (Mankiw 1986; Gale 1990; Banerjee and Newman 1993; Hubbard 1998), overinvestment (de Meza and Webb 1987; Bernanke and Gertler 1990), or poverty traps (Mookherjee and Ray 2002). Many policies have been put forth to address information asymmetry problems. A better understanding of which information asymmetries are empirically salient is critical for determining optimal remedies, if any. For instance, adverse selection problems should motivate policymakers and lenders to consider subsidies, loan guarantees, information coordination, and enhanced screening strategies. Moral hazard problems should motivate policymakers and lenders to consider legal reforms in the areas of liability and garnishment, and enhanced dynamic contracting schemes.

But information asymmetries are difficult to identify in practice. Empirical evidence on the existence and importance of specific information frictions is relatively thin in general, and particularly so for credit markets (Chiappori and Salanie 2003). 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, conflated selection and incentive effects. For example, a positive correlation between loan default and a randomly assigned interest rate, conditional on observable risk, could be due to adverse selection *ex-ante* (those with relatively high probabilities of default will be more likely to accept a high rate) or moral hazard *ex-post* (because those given high rates have greater incentive to default).[1]

More generally, despite widespread interest in liquidity constraints and their real effects, empirical evidence on the existence of any *specific* credit market failure is lacking. Consequently there is little consensus on the importance of liquidity constraints for individuals.[2] Empirical work typically has examined this issue indirectly,[3] either through accounting exercises which calculate the fixed and variable costs of lending, or by inferring credit constraints by from an agent's ability to smooth consumption and/or income (e.g., Morduch (1994)). Work studying the impact of credit market failures on the real economy tends to take some reduced-form credit constraint as given (e.g., Wasmer and Weil (2004)), or as a hypothesis to be tested (e.g., Banerjee and Duflo (2004)), without evidence of a specific friction that may (or may not) actually produce a sub-optimal allocation of credit. Our work provides a microfoundation for studying the real effects of credit constraints by identifying the presence (or absence) and magnitudes of two specific credit market failures: adverse selection and moral hazard.

We test for the presence of distinct types of hidden information problems using a new experimental methodology that disentangles adverse selection from moral hazard effects on repayment under specific identifying assumptions. The research design was implemented by a South African firm specializing in high-interest, unsecured, fixed-repayment-schedule lending to poor workers. The experiment identifies information asymmetries by randomizing loan pricing

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[1] See Ausubel (1999) for a related discussion of the problem of disentangling adverse selection and moral hazard in a consumer credit market. See Chiappori and Salanie (2000) and Finkelstein and McGarry (forthcoming) for approaches to the analogous problem in insurance markets. Insurance markets have been the subject of relatively active interplay between theoretical and empirical contributions, but recent papers on other markets have also made important strides towards identifying the independent effects of adverse selection and/or moral hazard; see, e.g., Cardon and Hendel (2001) on health insurance, and Shearer (2004) on labor contracts.
[2] The empirical importance of credit market failures for firms is also debated; see, e.g., Hurst and Lusardi (2004) and Banerjee and Duflo (2004).
[3] See Morduch and Armendariz de Aghion (2005) for a discussion of this literature.

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.[4]

A stylized example, illustrated in Figure 1, captures the heart of our methodology. The Lender offers potential borrowers with the same observable risk a high or low interest rate on a direct-mail solicitation (high and low are relative terms: almost all of the experimental rates were actually below the Lender's normal ones). Individuals then decide whether to borrow at the solicitation's "offer" rate. Of those that respond to the high offer rate, half randomly receive 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). Individuals do not know beforehand that the contract rate may differ from the offer rate, and our design produces empirical tests confirming that the contract rate was indeed a surprise.

We identify any selection effect by considering the sample that received the low contract rate, and comparing the repayment behavior of those who responded to the *high offer* interest rate with those who responded to the *low offer* interest rate. This test identifies any selection effect because everyone in this sample was randomly assigned identical contracts, but selected in at varying, randomly assigned rates. Any difference in repayment comes from selection on unobservables.

Similarly, we identify any effect of repayment burden (which includes moral hazard)[5] by considering the sample that responded to the high offer interest rate and comparing the repayment behavior of those who received the *high contract* interest rate with those who received the *low contract* interest rate. These borrowers selected in identically, but ultimately received randomly

[4] 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 management. Although the Lender typically marketed to former clients via direct mail, they had not previously advertised interest rates in the letters.

[5] We define moral hazard as any effect of repayment burden on default that stems from *ex-post* behavioral changes driven by the incentives of the contract. Repayment burden also includes a mechanical wealth or income effect: those with positive (negative) shocks to wealth or income may be more (less) able to repay higher-interest debt. Section IV discusses this in more detail.

different interest rates on their contract. Any difference in default comes from the resulting repayment burden.

Finally, after all terms on the initial loan (loan amount, maturity, and interest rate) are finalized, the Lender announces a randomly assigned price on future loans. Some borrowers receive the contract rate only on their initial loans, while others are eligible to borrow at the contract rate on future loans, provided that they remain in good standing. The latter case explicitly raises the benefits of repaying the initial loan on time in the 98% of cases where the contract rate is less than the Lender's normal rate. Moreover, this "dynamic repayment incentive" does not change the costs of repaying the initial loan, since the initial debt burden is unperturbed. Any correlation between this incentive and default must be driven by *choices*; i.e., by "pure" moral hazard. The response of repayment behavior to the dynamic repayment incentive thus yields our sharpest test for the presence of moral hazard.

Our design creates a selection experiment on all individuals who received an offer. We observe whether the repayment behavior differs for the *pools* of individuals who select on high and low rates.[6] The moral hazard and repayment burden experiments are conducted only on those who borrow. In both cases these are the relevant sample frames from the perspective of a Lender contemplating changes to its pricing strategy.

Our approach to estimating the extent and nature of asymmetric information is most similar in intent to Edelberg (2004), and in methodology to Ausubel (1999). Edelberg estimates a structural model to disentangle the effects of adverse selection and one type of moral hazard (in effort) in collateralized consumer credit markets in the United States. She finds evidence consistent with both phenomena. Ausubel uses market experiments conducted by a large American

[6] Here we examine how contract terms alter the pool of unobservable characteristics of individuals that apply for credit. One could also study whether individuals possess information unobservable to the lender at all. This is a necessary but not sufficient condition for the type of adverse selection that we study. See Section V for more details on the specific construction of selection we are testing, and see Grant and Padula (2006) for an example of the selection question of the latter type.

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 per annum) in his data. Klonner and Rai (2005) is the most similar paper studying a developing country setting. They exploit institutional features of rotating credit associations in India and find evidence of adverse selection.[7]

We find relatively strong evidence of economically significant moral hazard in a South African consumer credit market. We find weaker evidence of repayment burden and adverse selection effects. Moral hazard appears to work in different directions on contemporaneous loan prices (where we find that lower interest rates do not generally improve repayment) and future loan prices (where we find the lower interest rates substantially improve repayment on current loans). The pattern of information asymmetries appears to differ by gender in surprising ways, and with the intensity of the prior relationship with the Lender in intuitive ways. The effects of private information are economically important in the setting we study: a rough calibration suggests that moral hazard explains perhaps 7%-16% default in our sample. Information asymmetries may help explain the prevalence of credit constraints even in a market that specializes in financing high-risk borrowers at very high rates.

The paper proceeds by providing background on South African consumer credit markets and our cooperating Lender in Section II. Section III lays out the experimental design and implementation. Section IV provides an informal discussion of how theories of asymmetric information motivate and shape our experimental design. Section V formally derives our tests of some general theoretical predictions while highlighting the assumptions under which our design can identify the presence or absence of adverse selection, repayment burden, and moral hazard effects. Section VI presents the main empirical results. Section VII discusses interpretation issues

[7] Also see Karlan (2006), which finds evidence of social capital mitigating moral hazard effects. Other papers estimating the prevalence of private information in credit markets include Calem and Mester (1995), Cressy and Toivanen (2001), Crook (2002), Drake and Holmes (1995), and Grant and Padula (2006).

and presents some related results on mechanisms. Section VIII concludes with some practical and methodological implications.

II. Market and Lender Overview

Our cooperating Lender operated for over 20 years as one of the largest, most profitable micro-lenders in South Africa.[8] It competed in a "cash loan" industry segment that offers small, high-interest, short-term, uncollateralized credit with fixed monthly repayment schedules to a "working poor" population. Aggregate outstanding loans in this market segment equal 38% of non-mortgage consumer credit (Department of Trade and Industry South Africa 2003).

Cash loan borrowers generally lack the credit history and/or collateralizable wealth needed to borrow from traditional institutional sources such as commercial banks. Cash loan sizes tend to be small relative to the fixed costs of underwriting and monitoring them, but substantial relative to a typical borrower's income. For example, the Lender's median loan size of R1000 ($150) was 32% of its median borrower's gross monthly income.

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). Cash lenders focusing on the observably highest-risk market segment typically make one-month maturity loans at 30% interest *per month*. Informal sector moneylenders charge 30-100% per month. Lenders targeting observably lower risk segments charge as little as 3% per month.[9]

The cash loan market has important differences and similarities with "traditional" microcredit (e.g., the Grameen Bank, or government or non-profit lending programs). In contrast to our setting, most microcredit has been delivered by lenders with explicit social missions that

[8] The Lender was merged into a large bank holding company in 2005 and hence no longer exists as a distinct entity.
[9] There is essentially no difference between these nominal rates and corresponding real rates. For instance, South African inflation was 10.2% *per year* from March 2002-2003, and 0.4% per year from March 2003-March 2004.

target groups of female entrepreneurs, sometimes in group settings. On the other hand, the industrial organization of microcredit is trending steadily in the direction of the for-profit, more competitive delivery of individual, untargeted credit that characterizes the cash loan market (Robinson 2001; Porteous 2003). This push is happening both from the bottom-up (non-profits converting to for-profits) as well as from the top-down (for-profits expanding into microcredit segments).

Our cooperating Lender's product offerings were somewhat differentiated from competitors. Unlike many cash lenders, it did not pursue collection or collateralization strategies such as direct debit from paychecks, or physically keeping bank books and ATM cards of clients. Its pricing was transparent and linear, with no surcharges, application fees, or insurance premiums added to the cost of the loan. The Lender also had a "medium-maturity" product niche, with a 90% concentration of 4-month loans (Table 1a). Most other cash lenders focus on 1-month or 12+-month loans.[10] The Lender's normal 4-month rates, absent this experiment, ranged from 7.75% to 11.75% per month depending on observable risk, with 75% of clients in the high risk (11.75%) category.

Per standard practice in the cash loan market, essentially all of the Lender's underwriting and transactions were conducted face-to-face in its network of over 100 branches. Its risk assessment technology combined centralized credit scoring with decentralized loan officer discretion. Rejection was prevalent even with a modal rate of 200% APR; the Lender denied 50% of new loan applicants. Reasons for rejection included unconfirmed employment, suspicion of fraud, poor credit rating, and excessive debt burden.[11]

Applicants who were approved often defaulted on their loan obligation, despite facing several incentives to repay. Carrots included decreasing prices and increasing future loan sizes

[10] The Lender also has 1, 6, 12, and 18 month products, with the longer maturities offered at lower rates and restricted to the most observably creditworthy customers.
[11] No regulation prevented the Lender from charging higher rates to price risk.

following good repayment behavior. Sticks included reporting to credit bureaus, frequent phone calls from collection agents, court summons, and wage garnishments. Repeat borrowers had default rates of about 15%, and first-time borrowers defaulted twice as often.

III. Experimental Design and Implementation

We seek to identify the presence or absence of distinct classes of asymmetric information problems by incorporating the random assignment of interest rates into the day-to-day operations of a lender. The experiment was pilot-tested in July 2003, and then fully executed in two additional waves launched in September and October 2003. The methodology was implemented in a consumer credit market but is applicable to other market settings as well.

This section begins with an overview of the methodology and some key statistical results validating adherence to its experimental protocols. Then it describes each design and operational step in detail.

A. Design Overview

First the Lender randomized three interest rates related to "pre-qualified," limited-time offers that were mailed to 57,533 *former* clients with good repayment histories (see III-C for details). Information asymmetries may be less prevalent among former clients than new clients if hidden type is revealed through the lending relationship (Elyasiani and Goldberg 2004). Hence there is reason to expect that a lender faces more adverse selection among new clients (those who have not previously done business with the firm). . The Lender tried addressing this possibility by sending solicitations to 3,000 individuals from a mailing list purchased from a consumer database. Only one person from this list borrowed. Another list was purchased from a different vendor, and 5,000 letters were sent without randomized interest rates. Only two people responded. The Lender had no previous experience with direct mail solicitation to *new* clients, and concluded that the lack of response was due to low-quality (fraudulent or untargeted) lists from the consumer database firms, or to consumer unfamiliarity with receiving a solicitation from a firm they have not done business with in the past. In general, unsolicited direct mail is not common in South Africa, but individuals are accustomed to receiving mail from firms with which they do business (e.g., the Lender mails solicitations and monthly statements to prior and existing clients). We explore the importance of the prior relationship by examining the interaction between borrowing history and asymmetric information, in our sample of prior borrowers, in Section VII-A-iii.

We assigned each client an "offer rate" (r^o) included in the direct mail solicitation, 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, and a dynamic repayment incentive (D) that was revealed only after all loan terms had been finalized (see III-D for details). 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 maturity offered to the client, were orthogonal to the experimental interest rates by construction (see III-E for details). We tracked repayment behavior using the Lender's administrative data (see III-F for details), and correlated defaults with the randomized interest rates to estimate the prevalence of specific information asymmetries (see Sections V and VI for details).

Informally, r^o and r^c help distinguish selection effects from repayment burden effects. Some clients will select on different interest rates *ex-ante*, but then have identical repayment burdens *ex-post*. Other clients will select on the same rate *ex-ante*, but have different repayment burdens *ex-post.*[12]

The dynamic repayment incentive helps identify pure moral hazard (see Section IV for a discussion of the different varieties of moral hazard that may be relevant in our setting). Clients receiving the incentive (D=1) were eligible to receive r^c, which was a discounted rate in 98% of the

[12] As detailed in Section IV, we define "repayment burden" as the reduced-form combination of several underlying moral hazard parameters and a income effect.

cases, on all future loans taken within the next year. The discount embodied in r^c, and hence D=1, was substantial: 350 basis points off the monthly rate on average. Moreover, the Lender's prior data suggested that, conditional on borrowing once, a client would borrow again within a year more than half the time. Eligibility to borrow at r^c on future loans was made conditional on the borrower repaying their current loan on schedule. Clients not receiving the incentive (D=0) obtained r^c for just the first loan (which had only a 4-month maturity in 80% of the cases). D=1 thus provided randomly assigned favorable pricing on *future* borrowing, but did not shift the cost of repaying the borrower's *initial* loan taken at r^c. D therefore creates a test of whether a marginal *incentive*-- access to future financing at preferable rates-- promotes better *choices* that lead to repayment. Clients were informed of D by the branch manager only after all paperwork had been completed and all other terms of the loan were finalized. Figure 2 shows the experimental operations, step-by-step.

B. Adherence to the Experimental Design

Our design's ability to identify separately any effects of *ex-ante* and *ex-post* asymmetric information problems relies on the surprise revelation of r^c and D (see Section V for formal derivations). Consequently, we developed operations software to tightly control and monitor the underwriting and processing of loan applications. The design also permits statistical tests of whether operational protocols were followed. Table 2, Column 4 corroborates that the borrower application decisions were indeed "blind" to the contract rate r^c. Specifically, r^c is uncorrelated with the application decision, and this is reassuring because the prospective client should not have known anything about r^c when deciding whether to apply.[13] Column 5 shows that the Lender's credit decision was indeed uncorrelated with the surprise rates; i.e., the probability that an

[13] On the other hand, the application decision is negatively correlated with offer rate, indicating a downward-sloping demand curve with respect to price (Karlan and Zinman 2005).

application is rejected does not vary significantly with either r^c or D. This corroborates that loan officers could not access the surprise rates in making their credit supply decisions.

C. Sample Frame

The sample frame consisted of all individuals from 86 predominantly urban branches who had borrowed from the Lender within the past 24 months, were in good standing, and did not have a loan outstanding in the thirty days prior to the mailer.[14] Tables 1a and 1b present 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 assigns prior borrowers into "low," "medium," and "high" risk categories, and this determines the borrower's loan pricing and maturity options under normal operations. The Lender did not typically ask clients why they seek a loan but added a short survey at the end of the application process. Borrowers use proceeds for a variety of different investment and consumption smoothing activities. The most common appear to be education, housing, paying off other debt, events, and food and clothing (Table 1b). But these tabulations are merely suggestive, as the survey was administered to a small (25%) and nonrandom sample of clients, and the nonresponse rate was high.

D. The Randomizations

As noted above, we assigned each client 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 (D=1) or one loan (D=0). The r^o and r^c distributions each ranged from 11.75 percent per *month* to 3.25 percent per month. r^o and r^c were assigned, conditional on the borrower's observable risk category, within a predefined range bounded above by the Lender's normal rate for

[14] The sample frame includes branches and clients from four of South Africa's nine provinces: Kwazulu-Natal, Eastern Cape, Western Cape, and Gauteng.

that risk category (11.75 percent, 9.75 percent and 7.75 percent for the high, medium, and low risk categories, respectively) and below by "upmarket" competitor rates (3.25 percent).[15] 41% of the sample was chosen randomly and unconditionally to receive $r^c<r^o$. Figure 3 shows the distribution of offer and contract rates for these cases, and Appendix Table 2 shows the cell sizes for each $r^c<r^o$ cross-tab. At the time of the randomization, we verified that the assigned rates were uncorrelated with other known information, such as credit report score. Table 2 shows that the randomizations were successful, *ex-ante*, in this fashion. The prevalence of significant correlations between the randomly assigned interest rates and other variables (3 out of 45 cases), conditional on the observable risk category, is what one would expect to occur by chance.[16]

The dynamic repayment incentive was randomized at the branch level during the pilot and the second wave of the experiment, with prospective borrowers from 14 branches assigned D=0, and from 10 branches assigned D=1. In the third wave, D was assigned at individual level.[17]

E. The Offer and Loan Application Process

The Lender mailed solicitations featuring the offer rate to 57,533 former clients. Each letter had a randomly assigned deadline by which the individual had to respond in order to obtain r^o. The deadline ranged from 2 weeks to 6 weeks, and was assigned independently of the interest

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

[16] Columns 1 and 2 show that the offer rate and contract interest rates are not statistically significantly correlated with any of the independent variables. Column 3 shows that the dynamic repayment incentive was statistically significantly correlated with three out of fifteen independent variables: number of months since the last loan, the number of prior loans and age. Including controls for the (statistically significant) independent variables in the primary specifications does not change the estimates of the effect of the dynamic incentive on default.

[17] 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 D on a case-by-case basis. Once the Lender was more comfortable with the experimental design, this was relaxed for the third (and largest) wave of offers.

rate randomizations.[18] The Lender routinely contacted former borrowers via mail but had never promoted specific interest rate offers before this 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 performed the following tasks: a) they updated observable information (current debt load, external credit report, and employment information) and decided whether to offer *any* loan based on their updated risk assessment; b) they decided the maximum loan size for which applicants qualified at the normal interest rate; and c) they decided the longest loan maturity for which applicants qualified at the normal interest rate. Each loan supply decision was made "blind" to the experimental rates; i.e., the credit, loan amount, and maturity length decisions were made as if the individual were applying to borrow at the normal rate dictated by her observable risk class.

5,028 clients applied for a loan under this experiment (a takeup rate of 8.7%), and of those 4,348 (86.5%) were approved. The loan application process took at most one hour, typically less. There were no instances of someone applying for the loan, being approved, and then not taking out the loan. This fact further corroborates that the contract rate and dynamic repayment incentive were surprises; i.e., that borrowers made application decisions with reference to the offer rate only, and not in expectation of a lower r^c or D.

In determining maximum loan size, the Lender relied on a debt service ratio: the monthly payment of a loan could not exceed a certain percentage of a borrower's net monthly income. A lower interest rate normally would 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

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[18] The solicitations also incorporated randomized decision frames and cues, inspired by findings from marketing and psychology literatures, and designed to estimate the impact of "behavioral" effects on consumer demand. These randomizations were orthogonal to the pricing randomizations examined here, by construction, and are discussed in related research (Bertrand, Karlan, Mullainathan, Shafir and Zinman 2005).

hazard with respect to the interest rate). In order to mitigate this potential confound, the maximum allowable loan size was calculated based on the *normal*, not experimental, interest rates.

Once loan size and maturity were agreed upon, the software then revealed r^c, which was less than or equal to 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.

Due to operational constraints, clients were then 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 repayment burden effects (since a lower r^c strengthens repayment incentives *ceterus paribus*, but might induce choice of a higher L that weakens repayment incentives). In practice, however, only about 3% of borrowers who received $r^c < r^o$ changed their loan demand after r^c was revealed.[19] For now, we note that allowing L to change following the revelation of r^c would push against finding repayment burden effects. We postpone further discussion of this issue until Section VII-B.

Finally, the software informed the loan officer whether the individual's r^c was valid for one year (47% of borrowers obtained D=1) or for one loan (53% obtained D=0).

F. Default Outcomes

In principle, a measure of default should summarize the true economic cost of lending. In practice the true cost is very difficult to measure because of uncertainty and fixed costs in originating,

[19] Many had already decided to borrow the maximum amount offered by the Lender, and as such had no ability to re-optimize. Letter recipients *did* exhibit significant interest rate elasticities with respect to the offer interest rate on both the extensive (likelihood of borrowing anything) as well as the intensive (amount borrowed) margins (Karlan and Zinman 2005).

monitoring, and collections.[20] Given these difficulties, the Lender lacked a summary statistic for default, and instead relied on a range of proxies for true costs (this is common practice). Consultation with the Lender suggested focusing on three measures: (1) Monthly Average Proportion Past Due (the average default amount in each month divided by the total debt burden); (2) Proportion of Months in Arrears (the number of months with positive arrearage divided by the number of months in which the loan was outstanding); and (3) Account in Collection Status (typically, the Lender considered a loan in collection status if there are three or more months of payments in arrears). Table 1a presents summary statistics on these default measures.

IV. Theoretical Overview

This section starts by outlining some shared features of theoretical models that motivate our experimental design. We then discuss specific varieties of hidden type (selection) and hidden action (moral hazard) models that seem most relevant for the consumer credit market studied in this paper. Section V then formally derives the conditions under which our empirical strategy identifies distinct selection, repayment burden, and moral hazard effects.

A. Asymmetric Information Models: Motivating Features

Above we discussed how our methodology can be used to test for distinct effects of adverse selection and moral hazard in a credit market. Our tests are based on a prediction shared by most models of adverse selection and moral hazard: an information asymmetry will produce a positive correlation between *ex-post* risk (e.g., default) and the interest rate, conditional on observables (Freixas and Rochet 1997; Ghosh, Mookherjee and Ray 2001). Intuitively, this property holds when higher prices induce borrowers to make unobservable choices — *ex-ante* and/or *ex-post* — that reduce the likelihood of repayment. Consequently, higher interest rates

[20] These difficulties are evident in the wide variety and complexity of accounting rules regarding risky and delinquent loans, and in the substantial amount of resources and expertise devoted to "activity-based costing".

produce more defaults, even after we condition on the Lender's risk assessment. Two similar papers on credit markets also base their tests of information asymmetries on a positive correlation property (Ausubel 1999; Klonner and Rai 2005). The insurance analog of this property — a positive correlation between claims and coverage — has been the workhorse of a large empirical literature (Chiappori, Julien, Salanie and Salanie forthcoming).

However, alternative theories suggest a negative correlation may occur. In the case of *ex-ante* effects, an advantageous selection model predicts a negative correlation between interest rate and default. In the case of *ex-post* incentives, the positive correlation property is generated by models with one lender or multiple identical lenders. It may not hold under nonexclusive contracting. Consequently, part B of this section also discusses setups where individuals are borrowing from multiple sources, either explicitly (e.g., a loan from another financial institution), or implicitly (e.g., delaying payment of a utility bill). In these cases, a negative correlation between interest rate and default may prevail due to borrower incentives for prioritizing the repayment of relatively expensive debt.

A final general note on the theoretical models that motivate our experimental design: although the theoretical literature on information asymmetries has often used entrepreneurial credit as its motivating examples, its insights apply equally well to consumption loan markets. There are several reasons for this. First, the line between entrepreneurial "investment" and consumption "smoothing" is rarely evident for small, closely-held businesses. Money is fungible. Empirical evidence from Bangladesh microfinance finds, for example, that consumption smoothing is a key factor in expanding access to credit *to entrepreneurs* (Menon 2003). More generally, asymmetric information problems as applied to risky "projects" have natural and close analogs for consumption loan borrowers.[21] Just as entrepreneurs may pool with respect to interest rates based on unobservably fixed characteristics about the return structure of their "project" that impact loan repayment, individuals may pool with respect to interest rates based on unobservably fixed personality traits (e.g., trustworthiness) or behaviors (e.g., probability of incurring bad shocks) that impact repayment. Similarly, if entrepreneurs may unobservably change effort levels or repayment choices in response to interest rates, individuals may change their effort in activities such as maintaining wage employment, or securing alternative sources of cash in the event of a bad shock. And of course individuals may also default strategically.

B. Varieties of Relevant Asymmetric Information Problems

We are interested in three broad classes of asymmetric information problems: ex-ante *selection* on unobservables (or *hidden type* problems that manifest in differential pooling at different interest rates), ex-post *incentive effects* (or *hidden action* problems), and unobserved *income effects* that influence repayment behavior mechanically. Within these broad classes, there are several varieties of private information problems potentially relevant in our setting.

In the *hidden type* class of models, consumer lending has a natural analog to selection on unobservable "project" risk that may characterize lending to firms. In the case of consumer lending, this is selection on *ex-ante* cash flow risk more generally. Here adverse selection *a la* Stiglitz and Weiss occurs if high interest rates attract those with unobservably lower probabilities of repaying the loan for any number of reasons. This could be due to standard project risk, since there may be entrepreneurial activity financed with "consumption" loans, and/or it could be due to employment or household instability (e.g., higher likelihood to incur shocks to job, marital, and health status), relatively poor access to family or community resources, or general dishonesty *a la*

[21] These analogs are borne out by the fact that the bulk of empirical literature testing for asymmetric information in lending focuses on consumption or unrestricted loan markets; e.g., Ausubel (1991), Ausubel (1999), Calem and Mester (1995); Edelberg (2004); Klonner and Rai (2005); Grant and Padula (2006).

Jaffee and Russell (1976). Under each variety adverse selection occurs if unobservably riskier borrowers are more willing to borrow at relatively high rates because they are less likely to incur the financing cost by paying back the loan. In our setting this would produce a positive correlation between the offer interest rate and the default rate, holding constant the other two randomly assigned rates, under the identifying assumptions derived in Section V below. On the other hand, *advantageous selection* models, *à la* DeMeza-Webb (1987; 2001), predict a negative relationship between the interest rate and default.

Our empirical tests cannot distinguish between the different varieties of adverse and advantageous selection. Instead we will test for the *net* impact of selection on unobservables.

The *hidden action* class of models also has natural consumer credit analogs to moral hazard by firms. One variety of models concerns moral hazard in *effort*: here, higher interest rates discourage productive activity by reducing borrower returns in successful states. This is also known as the *debt overhang* effect (Ghosh, Mookherjee and Ray 2000). If productive activity would increase the probability that the borrower generates sufficient cash flow for loan repayment, it follows that higher interest rates produce higher default rates under the identifying assumptions detailed in Section V below. In the consumer case, the relevant effort may not relate to a firm production function, but rather to the borrower's effort to retain or obtain employment, to tap alternative sources of cash in the event of a bad shock, or to manage consumption in order to retain sufficient funds for loan repayment. Another variety concerns moral hazard via *voluntary default*. These models consider incentives for default even when the agent has the ability to repay. Default becomes more attractive under *limited enforcement* as the interest rate increases, with the realistic assumption that penalties are concave in the amount owed (Eaton and Gersovitz 1981; Ghosh and Ray 2001). Again this would imply that higher interest rate contracts lead to higher rates of default.

This result applies equally to individuals and firms (and indeed to sovereign entities), and provides motivation for dynamic incentive schemes.[22]

On the other hand, a setting with multiple lenders could produce a *negative* correlation between the contract rate and default. The moral hazard models described above typically assume *exclusive contracting* between a borrower and a single lender (Bisin and Guaitoli 2004). In our setting, however, and in most other markets, borrowers can access multiple, distinct sources of credit (both formal and informal). The relatively small literature on *nonexclusive contracting*— e.g., Parlour and Rajan (2001)— suggests that the Lender may generate *worse* repayment by contracting at lower rates with borrowers holding expensive outside debt. The notion is that a borrower with existing, relatively expensive obligations, and an ability to repay only one, may benefit from repaying the more expensive— i.e., the outside— obligation and defaulting on her new obligation with the Lender. Concretely, consider Ms. Smith, a borrower with a debt to the Lender that can be fulfilled at marginal cost r^c, and an outside obligation that can be fulfilled at marginal cost r^{out}, with $r^c < r^{out} < r^n$, where r^n is the Lender's normal rate for Ms. Smith's observable risk classification. Note that this outside obligation need not be a formal loan (it could be a utility bill, or a responsibility to support a family member). At time t, Ms. Smith *expects* to fulfill both obligations at t+1 but lacks the ability or incentive to stay current on both. So she pays down the obligation with the highest shadow cost per unit of time. At $r^c < r^{out}$ this means paying down the outside obligation; at $r^c > r^{out}$ this means paying down the Lender's loan. Hence, multi-lateral contracting can produce moral hazard that is *advantageous* for lenders charging higher rates. Under our design this would manifest as a negative correlation between the contract rate and the

[22] We doubt that moral hazard via choices that impact "project" returns, a la Bester and Hellwig (1987) on technology choice, or Stiglitz and Weiss (1981) on project choice, are salient here. We surmise as such even though borrowers may have a choice among several consumption smoothing or investment "projects", broadly defined, because in most cases cash flows from wage employment will be the primary source of a borrower's ability to repay the loan. The fact that very few borrowers re-optimize their loan size upon revelation of a "pleasant surprise" contract rate is consistent with this supposition.

default rate. There is no such channel working through the dynamic repayment incentive, since D does not change current debt burdens.

Identifying moral hazard effects is often complicated by another *ex-post* interest rate effect, a simple *income effect*. If there are binding liquidity constraints — e.g., such that a borrower who incurs a bad shock truly has no means of securing cash to repay her debt to the Lender — then, holding loan amount constant, a higher interest rate mechanically increases the repayment amount. This reduces the probability that a borrower with uncertain cash flow will be *able* to repay. Although this effect is not due to moral hazard when liquidity constraints bind — since the borrower effectively has no *choice* related to loan repayment — an income effect still may prevent a profit-maximizing lender from using interest rates to clear the market. Section V will show that any income effect would manifest as a positive correlation between the contract interest rate and the default rate under our experiment. Section V will also show how we can use the randomly assigned dynamic repayment incentive to help isolate "pure" moral hazard; i.e., a pure incentive effect that is not confounded by any income effect.

However, as with selection, our design does not identify any particular hidden action problem. For example, we can not measure the relative importance of debt overhang versus that of voluntary default. Our design can only identify the *net* effect on default of *ex-post* incentives provided by interest rates.

V. Empirical Strategy and Identification

This section derives the specific assumptions necessary to identify the presence or absence of adverse selection, repayment burden, and moral hazard under our experimental design.

A. Identification of Adverse Selection Using Differential Offer Interest Rates

We start by defining the selection effect as the difference in expected repayment by high-risk and low-risk types, given the same contract. We define type as the *unobserved* component of default risk, and for expositional simplicity assume that all potential borrowers have the same observable risk. Then formally:

$$(1)\quad \Delta^{AS} = E(Y(q^{H}, r^{c})) - E(Y(q^{L}, r^{c}))$$

where $Y(q^{H}, r^{c})$ is default, equal to one if a high-risk type individual (q^{H}) defaults given the contract interest rate (r^{c}) and zero otherwise, and likewise for $Y(q^{L}, r^{c})$. In other words, given the same contract, what is the difference in expected default between high-risk types and low-risk types? If (1) is positive, we have adverse selection: high-risk types default more frequently than low-risk types.[23] Note that effort does not appear in this definition. This follows the classic Stiglitz-Weiss formulation of adverse selection occurring on a *fixed* individual characteristic that is known to the agent and is unobservable to the lender.

The expectation of default is conditional on the distribution of funds available for repayment, hence (1) can be written as:

$$(2)\quad \Delta^{AS} = \int_{-\infty}^{\infty} Y(\pi, r^{c}) f_{q^{H}}(\pi) d\pi - \int_{-\infty}^{\infty} Y(\pi, r^{c}) f_{q^{L}}(\pi) d\pi ,$$

where $Y(\pi, r^{c})$ represents default, defined as an indicator function equal to one if the individual defaults on their loan, given π and r^{c}, where π is the financial outcome after borrowing and r^{c} is the contract interest rate. $f_{q^{L}}(\pi)$ is the distribution of π (financial outcome after borrowing) for low-risk types, whereas $f_{q^{H}}(\pi)$ is the distribution of π for high-risk types. The difference between these two distributions is the essence of the selection effect.

[23] Conversely, if (1) is negative we have advantageous selection a la DeMeza and Webb. More on this in Section VII-A.

Of course the point of adverse selection models is that type is not observed, in this case by neither the Lender nor the econometrician. Equation (2) above is not observed because we do not know who is q^H and who is q^L. We seek to identify the presence of adverse selection by examining the repayment behavior of the *pools* of individuals that agree to borrow at high and low interest rates.

We can however observe the following:

$$(3) \quad \hat{\Delta}^{AS} = E(Y(q \mid r_H^o, r_L^c)) - E(Y(q \mid r_L^o, r_L^c))$$

Note that (3) can be re-written as follows:

$$(4) \quad \hat{\Delta}^{AS} = \begin{array}{c} \{\Pr(q^H \mid r_H^o, r_L^c) E(Y(q^H, r_L^c)) + [1 - \Pr(q^H \mid r_H^o, r_L^c)] E(Y(q^L, r_L^c))\} \\ - \\ \{\Pr(q^H \mid r_L^o, r_L^c) E(Y(q^H, r_L^c)) + [1 - \Pr(q^H \mid r_L^o, r_L^c)] E(Y(q^L, r_L^c))\} \end{array}$$

We now make the first of two identifying assumptions. First, the contract rate, given the offer rate, influences neither the decision by the borrower to borrow nor the decision by the Lender to lend (AS-1). This assumption can be written as:

(AS-1): $\Upsilon(r^o, q, r^c) = \Upsilon(r^o, q)$,

where $\Upsilon(r^o, q, r^c)$ is an indicator function equal to one if an individual applies for a loan and the Lender approves the loan application, given the individual's offer interest rate, type (unobservable to the Lender), and contract rate. Empirically, this assumption has two implications for our experiment. First, it requires that applicants did *not* know that the contract rate could be lower than the offer rate when deciding to apply. Our design makes the contract rate a surprise, and as discussed above, Table 2 Column 4 corroborates that the contract rate indeed did not affect the probability of an individual applying. Second, AS-1 requires that the Lender did not condition its approval of loans on the contract interest rate, but rather only considered information observable to it from the application and credit record of the individual. Our design built this restriction into the experimental operations, and Table 2 Column 5 shows that the Lender's decision to reject is indeed uncorrelated with the contract interest rate.

When AS-1 holds (4) simplifies to:

$$(5) \quad \hat{\Delta}^{AS} = \begin{array}{c} \{\Pr(q^H \mid r_H^o)E(Y(q^H, r_L^c)) + [1 - \Pr(q^H \mid r_H^o)]E(Y(q^L, r_L^c))\} \\ - \\ \{\Pr(q^H \mid r_L^o)E(Y(q^H, r_L^c)) + [1 - P(q^H \mid r_L^o)]E(Y(q^L, r_L^c))\} \end{array}.$$

The second identification assumption is

(AS-2): $\Pr(q^H \mid r_H^o) = 1$.

In other words, low-risk types do not apply for a loan at high-offer interest rates.[24] AS-2 implies that (5) simplifies further to:

$$(6) \quad \hat{\Delta}^{AS} = \begin{array}{c} \{E(Y(q^H, r_L^c))\} \\ - \\ \{\Pr(q^H \mid r_L^o)E(Y(q^H, r_L^c)) + [1 - \Pr(q^H \mid r_L^o)]E(Y(q^L, r_L^c))\} \end{array}$$

The second term in (6) is a convex combination of expected default of high-risk types at contract rate r_L^c and low-risk types at the same contract rate r_L^c. Therefore, (6) is positive if and only if (1) is positive

Our experimental design permits direct estimation of (6); it is observable, and equal to the difference between the average default rates of borrowers who received the high offer rate and

[24] The weaker assumption that high-risk types are more likely than low-risk types to borrow at high interest rates-- $\Pr(q^H|r^o_H) > \Pr(q^H|r^o_L)$, rather than $\Pr(q^H|r^o_H) = 1 > \Pr(q^H|r^o_L)$-- leads to the same qualitative conclusions at the cost of slightly messier notation. These assumptions may not hold under multi-lateral contracting if unobserved risk is driven by outside debt holdings and, e.g., low rates attract those with unobservably heavier debt burdens. More generally AS-2 will not hold by definition if unobservable selection is advantageous a la DeMeza-Webb (1987; 2001) rather than adverse (more on this possibility in Section VII-A).

those that received the low offer rate.[25] Therefore, under AS-1 and AS-2 the positive correlation property holds and we can identify whether adverse selection exists; i.e., whether the sign of (1) is positive.

B. Identification of Repayment Burden Through Differential Contract Interest Rates

We now detail the identification strategy for repayment burden. Recall from the discussion above that any repayment burden effect is potentially comprised of two conceptually distinct components: an income effect (defined as the *mechanical* impact of higher interest rates in a world with imperfectly observed liquidity constraints), and moral hazard (defined as unobserved *ex-post choices* that are influenced by interest rates). The contract rate can be used to identify the repayment burden effect, but does not allow us to separately identify the income and moral hazard effects. In section C below we detail how the dynamic repayment incentive does permit identification of "pure" moral hazard.

We formally define the *repayment burden* effect as:

$$(7)\quad \Delta^{RB} = E(Y(r_H^c, e(r_H^c))) - E(Y(r_L^c, e(r_L^c))),$$

where again Y is default. Here default is a function of both the contract rate directly (the income effect component), and the effort exerted given the contract rate (the moral hazard component). As discussed in Section IV, effort should be construed quite generally to include willingness to use available funds for repayment. Equation (7) implicitly conditions on the offer rate r^o— more on this below. Thus, the repayment burden effect is the difference in expected default between high-interest rate loans and low-interest rate contracts, *for a given individual*. Our design allows us to identify this effect in a single cross-section by randomizing across observably identical individuals.

[25] The first bracketed term in (6) is the default rate for borrowers who received the high offer rate and the low contract rate. The second bracketed term is the default rate for borrowers who received low offer rate and the low contract rate.

Note again that (7) includes two components: an unambiguously (weakly) positive income effect, and an ambiguously-signed moral hazard effect. The moral hazard effect is positive under traditional exclusive contracting models where higher effort is induced by lower contract interest rates. However, it may be negative under the nonexclusive contracting scenario discussed earlier, wherein individuals minimize expected interest costs by paying the higher cost obligation first.

In formally deriving our identification strategy we limit the analysis to those who agreed to borrow at high offer interest rates (since there was no ability to lower the rates on the already-low offer interest rates). If only high-risk types agree to borrow at the higher offer interest rate (as assumed in AS-2 above), this implies that the repayment burden experiment is conducted only on high-risk individuals. This limitation in interpretation is mitigated by two facts: first, the "high" rate is in fact equal or *less* than the Lender's normal interest rate, and everyone in our sample frame has borrowed at the normal rate in the recent past. Second, we actually have continuous, not binary, variation in interest rate. Almost no individuals had offer rates at the floor (3.25%), hence almost all contracts had some room to receive a lower contract than offer interest rate. Together these mitigating factors suggest that we are conducting the repayment burden experiment on individuals of interest to lenders in this market.

We do not observe (7), but under our experimental design we do observe:

$$(8) \quad \hat{\Delta}^{RB} = E(Y(r_H^c, e(r_H^o, r_H^c))) - E(Y(r_H^c, e(r_H^o, r_L^c))).$$

To reiterate, the contract rate enters both directly (the income effect component) and indirectly (the moral hazard component). Given that everyone in (8) has the same offer rate, any differences in effort are due to the contract rate. This means that (8) reduces to:

$$(9) \quad \hat{\Delta}^{RB} = E(Y(r_H^c, e(r_H^c))) - E(Y(r_L^c, e(r_L^c))),$$

This is identical to (7) and observable: it is simply the difference in default rates between borrowers with high and low contract rates.[26]

C. Identification of Moral Hazard using a Dynamic Repayment Incentive

In order to separate moral hazard from repayment burden, we exploit a third margin of random variation in interest rates. Recall that the "dynamic repayment incentive" (D), detailed in Section III, promised continued discounted rates on future loans to a borrower assigned D=1, provided that the borrower repaid her initial loan on time.

We define any moral hazard that is alleviated by D as:

(10) $\Delta^{MH} = E(Y(e(r_H^f)) - E(Y(e(r_L^f))$

where Y still measures default on the initial loan, r_L^f is the low contract interest rate available on *future* loans (i.e., D=1) conditional on successful repayment of the initial loan, and r_H^f is the high contract rate analog for *future* loans (i.e., D=0). Equation (10) implicitly conditions on the offer rate and the current contract rate.

To simplify the exposition, we limit the sample frame to borrowers with low current contract rates. Then moral hazard is defined as the difference in expected default between those who receive high *future* contract interest rates and those who receive low *future* contract interest rates (conditional on successfully repaying the current loan).

Similar to adverse selection and repayment burden, note that we actually observe:

(11) $\hat{\Delta}^{MH} = E(Y(r_L^c, r_H^f, r^o, e(r^o, r_L^c, r_H^f))) - E(Y(r_L^c, r_L^f, r^o, e(r^o, r_L^c, r_L^f)))$

[26] Using the full continuous variation in randomly assigned rates requires the following identifying assumption to get from (8) to (9) : $\Pr(e^H \mid r_H^o, r^c) = \Pr(e^H \mid r_L^o, r^c)$. This seems reasonable in our setting, as it merely implies that once the contract rate was revealed the offer rate had no effect on the *ex-post* effort exerted by the individual.

As in the repayment burden case, we now see immediately that any differences in effort in (11) are driven by differences in the future contract interest rate. Thus (11) reduces to:

(12) $\hat{\Delta}^{MH} = E(Y(r_H^f, e(r_H^f))) - E(Y(r_L^f, e(r_L^f)))$ [27]

We can now separate moral hazard from the direct income effect under the highly plausible assumption that the *future* contract interest rate does not affect the ability to repay the *current* loan except via its inducement of high or low effort, i.e.: [28]

(MH-1): $Y(r_H^f, e^H(r_H^f)) = Y(e^H(r_H^f))$.

This allows us to simplify (12) to:

(13) $\hat{\Delta}^{MH} = E(Y(e(r_H^f))) - E(Y(e(r_L^f)))$,

which is exactly the same as (10). Therefore the difference in default rates between those with D=1 and those with D=0 identifies any moral hazard that is alleviated by the dynamic repayment incentive.

VI. Main Empirical Results

A. Comparison of Means: Table 3

The identification strategies derived above classify rates into "high" and "low" groups, conditional on observable risk, *a la* Figure 1. We implement this empirically by setting cutoffs at the median experimental rates for each observable risk category. Table 3 presents mean comparisons using this method for each of the three default measures described in Section III-F.

[27]Using the full continuous variation in randomly assigned rates requires the following identifying assumption to get from (11) to (12): $\Pr(e^H \mid r_H^o, r^c) = \Pr(e^H \mid r_L^o, r^c)$. This is precisely the same assumption as in the repayment burden case; again, it seems reasonable given that it implies only that, once the contract rate is revealed, the offer rate does not influence effort.

[28] We cannot prove the validity of this identifying assumption with our experimental data, except to note that for this to be violated one would need to be able to sell the right to the future contract interest rate, and that no such formal market exists. Moreover the terms of the dynamic incentive were not put in writing by the Lender, making any such forward-sale unlikely even in an informal market transaction.

Net selection on unobservables is estimated on the sub-sample of borrowers receiving low contract rates by calculating (6): the difference between the average repayment performance of borrowers receiving high offer rates and those receiving low offer rates. The results are presented in the top panel of Table 3, in Columns 1-3. The significant difference in the Average Monthly Proportion Past Due across the two groups is consistent with adverse selection, as is the equally large but statistically insignificant difference in Account in Collection Status. The difference in Proportion of Months in Arrears is small and statistically insignificant.

The repayment burden effect is estimated on the sub-sample of borrowers receiving high offer rates by calculating (9): the difference between the average repayment performance of borrowers receiving high contract rates and those receiving low contract rates. The results are presented in the top panel of Table 3, in Columns 4-6. The large and significant difference in the Proportion of Months in Arrears across the two groups is consistent with a repayment burden effect, but there is no evidence of the effect on the other two measures of default.

Moral hazard is estimated on the sub-sample of those receiving low current contract rates by calculating (13): the difference between the average repayment performance of borrowers receiving no dynamic repayment incentive and those receiving one. Columns 7-9 of the top panel show large, significant differences in all three measures of default. These results indicate that a substantial amount of moral hazard was alleviated by the conditional promise of discounted rates on future borrowing.

We discuss the translation of our point estimates into economic magnitudes in the next two sub-sections.

B. Econometric Specification: Table 4

The means comparisons of Table 3 are the simplest method for identifying information asymmetries using our experimental design, but they neglect the information provided by continuous random variation in the offer and contract rates. Consequently, we implement OLS[29] analogs to our means comparisons by estimating the relationship between default and a randomly assigned interest rate, conditional on observable risk and holding the other rates constant:

(14) $Y_i = \alpha + \beta_o r_i^o + \beta_c r_i^c + \beta_d D_i + \chi X_i + \varepsilon_{ib}$

Here *i* indexes borrowers, and β_o, β_c, and β_d are the estimates of the selection effect, the repayment burden effect, and the dynamic incentive effect, respectively. *X* includes the Lender's summary measure of observable risk (since the randomizations conditioned only on this variable) and dummies for the month in which the offer letter was sent (since separate interest rate randomizations were conducted for each of the three "waves" of mailers). The error term, ε_{ib}, allows for clustering at the branch level, *b*. We estimate (14) on the entire sample of 4,348 individuals who obtained a loan under this experiment.

Table 4 presents the results for several different specifications of (14). In all cases, the interest rate units are in monthly percentage points (e.g., 7.50 for 7.50% per month). Results on the offer and contract rate variables therefore capture the effect of a one percentage point (100 basis point) increase in the monthly rate on the default measure listed in the super-column heading.

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. The point estimate is indeed positive in every case, but we find no statistically significant evidence of adverse selection.

[29] Tobits and probits (not reported) produce qualitatively identical results.

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 income effects. We find mostly positive coefficients that are statistically insignificant. The one marginally significant result (column 3) implies that a 400 basis point cut would reduce the average number of months in arrears by 13%.[30]

Row 3 of Table 4 (Columns 1, 3, 5, 7, 8 and 9) presents results on D, the dynamic repayment incentive variable. As with the simple means comparisons, every result points to significant moral hazard. D's effect is large, with the incentive producing decreases in the various default measures ranging from 1.1 to 1.9 percentage points in the OLS specifications. These magnitudes imply that D=1 clients defaulted 7 to 16 percent less often than the mean borrower. Columns 2, 4 & 6 show that D's effect is increasing in and driven by the size of the discount on future loans, as each 100 basis point decrease in the price of future loans reduces default by 4% in the full sample. The last row of the table shows that D and the size of the discount are jointly significant in all 3 specifications.

C. Magnitude Calculations Comparing Observables and Unobservable Effects

We now explore the relative importance of private versus public information in determining default. In doing so we focus exclusively on the role of moral hazard, since we find relatively weak evidence of adverse selection and repayment burden. We estimate the proportion of defaults that are due to moral hazard by comparing the raw default rates of high-risk and low-risk borrowers (Table 1a), and estimating how much of these differences are due to the incentive effects provided by variation in interest rates (versus the observable portion that caused the Lender to label them as high-risk and low-risk borrowers). Table 1a shows that the average high-risk borrower obtained a

[30] Coefficient * 4 / mean outcome = 0.007 * 4 / 0.219 = 13%

contract rate that was 200 basis points higher than the average low-risk borrower. Recall that the average discount provided by the dynamic repayment incentive was 350 basis points.

Taking a concrete example, we estimate how much of the raw difference in the Average Monthly Proportion Past Due between high-risk and low-risk clients (9 percentage points) is driven by the fact that low-risk clients face better incentives to repay. So we take the default response to the dynamic repayment incentive as estimated in Table 3 (alternately we could use the OLS point estimate in Table 4), scale the average size of the incentive (350 basis points) by the average contract rate difference between high- and low-risks (200 basis points), and divide by the raw difference in default rates: ((200/350)*.015)/.09 = 10%.[31] This estimate suggests that 10% of default is due to moral hazard, with the other 90% due to observable differences in risk. Using the OLS coefficient on D in Table 4 (0.11) instead of the simple difference in means produces an estimate of 8%. Repeating the calculation using the means difference or the OLS coefficient for the other two default measures yields estimates ranging from 7% to 16%.[32]

VII. Interpretation: Heterogeneity and Mechanisms

Tables 3 and 4 show fairly robust evidence of moral hazard, but only weak evidence of a repayment burden and of adverse selection. This section discusses two critical issues in interpreting these results— identification and external validity— and presents some additional evidence related to mechanisms underlying the main results.

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[31] As a robustness check, we used the Lender's normal risk-based pricing difference (400 basis points) and its risk manager's estimate of normal differences in raw default rates. The results are qualitatively similar.

[32] Note that we are only estimating the moral hazard effect from the presence, not magnitude, of the dynamic repayment incentive. From Table 4 Columns 2, 4 and 6, the coefficient on the magnitude of the dynamic incentive is only statistically significant in one of three specifications. Thus we use the more precise estimate on the effect of presence of the incentive to estimate the importance of the incentive relative to the sorting done by the Lender on observable information.

A. Interpreting the Offer Rate Results

i. Offsetting Selection Effects?

Does the absence of a robust positive correlation between default and the offer rate indicate the absence of adverse selection, or the violation of identification assumption AS-2 (that higher rates actually attract unobservably riskier types)? Heterogeneity in unobservable selection could obscure the presence of selection of unobservables by producing offsetting selection effects on the offer rate. Some (pools of) borrowers may select adversely, producing a positive correlation, while other borrowers select advantageously, producing an offsetting negative correlation. This is an empirically important point, since asymmetric information problems may produce inefficiencies even when they cancel out on net (Finkelstein and McGarry 2006). Lacking a clean test for offsetting effects, we explored whether there was any evidence that the offer rate coefficient switches signs across different demographic groups (e.g., adverse selection for relatively low-income borrowers but advantageous selection for relatively high-income borrowers). We found no evidence suggesting that this occurs.

ii. Gender Differences

Our exploration of heterogeneity in selection effects did reveal one notable source of heterogeneity: the presence of adverse selection in the sample of female borrowers, and its absence among male borrowers (Table 3 and Table 5). This finding is interesting because many microcredit initiatives in developing countries target women. Of course, the pattern may be due to some omitted variable rather than gender *per se*. An imperfect test of this confound is to estimate whether the gender effect persists after conditioning on all available demographic information (age, income, years at employer, education, number of dependents, credit score, marital status, and home ownership) and the interactions of these variables with the randomly assigned interest rates. Table

6 presents some of these results, and the *Female*Offer Rate* column gives an indication of what we find: the gender effect does persist after adding controls for observable demographics. However, some other omitted variable may be driving the results.[33]

iii. External Validity and the Power of Repeated Transactions

External validity issues often temper the generalizability of empirical results, and this is especially true of our attempt to identify the presence or absence of adverse selection on a sample of successful prior borrowers. Adverse selection is typically thought of as impinging most severely on a lender's ability to price risk for unknown (i.e., truly marginal) borrowers. In contrast, our sample may have already revealed itself to be comprised of "good types" by repaying successfully on prior loans. More generally, the premise is that in the process of transacting, private information eventually becomes public over time. If this holds, then frequent borrowers are less likely to have private information that they can exploit, *ex-ante* and/or *ex-post,* and consequently affect repayment behavior.

We explore the possibility that transaction history reduces asymmetric information problems, within our sample of prior borrowers, by testing whether the repayment response to the randomly assigned interest rates varies with the number of prior loans the borrower has taken from the Lender. If private information is revealed over time, then contract terms (in this case interest rates) should have less influence on default. In other words, when all information is public, default will be independent of the randomly assigned interest rates (barring the income effect discussed earlier), and driven instead by bad shocks or realizations.

Table 7 shows that defaulting by frequent prior borrowers is indeed less responsive to the offer and contract rates. We tested this by adding a prior loans main effect and its interaction with

[33] A brief phone survey of 374 of the Lender's former borrowers, including 61 defaulters, found no differential incidence of bad shocks across genders.

an interest rate to equation (14). The interaction term is negative and significant for the offer rate (Column 1) and the contract rate (Column 2), but not for the dynamic repayment incentive (Column 3). The interaction between the offer rate and borrowing history is large; e.g., it eliminates 43% of adverse selection (as measured by the offer rate main effect) at the mean number of prior loans (4.3) in the full sample. Thus, selection is indeed relatively more adverse among those borrowers with whom the Lender is least familiar. Similarly, the repayment burden effect is worse for relatively unfamiliar borrowers.

These results are consistent with information revelation reducing certain information asymmetries over time; i.e., with lending relationships (and dynamic contracting) having a causal effect on the reduction of adverse selection and repayment burden effects.

B. Interpreting the Contract Rate Results

As discussed in Sections III-E and IV, interpreting the contract rate result may be complicated by two factors. First, its reduced-form repayment burden effect is actually composed of two distinct underlying components: an income effect and moral hazard. These components potentially work in offsetting directions, if the standard moral hazard effect (higher rates produce weaker incentives for repayment) is reversed due to nonexclusive contracting, as discussed in Section IV. Second, the experimental implementation could not entirely prevent endogeneity of loan amount and maturity with respect to the contract rate. Some borrowers were given the opportunity to select larger loan amounts and longer maturities following the revelation of a lower contract rate, and this could in principle bias against finding a repayment burden effect on the contract rate. We now discuss these two issues in turn.

First, note that advantageous moral hazard stemming from nonexclusive contracting could reconcile our qualitatively different results on the contract rate and dynamic repayment incentive.

The results on the dynamic repayment incentive suggest that borrowers do respond strongly to incentives; i.e., there is moral hazard (in this case, as traditionally defined in exclusive contracting models). This implies that we might expect to find moral hazard operating through the contract rate as well. If there is traditional moral hazard, it should reinforce the income effect and produce a strong positive correlation between the contract rate and default. Yet, we find only weak evidence of a significant positive correlation. This could be because moral hazard operates advantageously, operating through the nonexclusive contracting channel, and hence pushes against the income effect component. These offsetting components— a positive correlation between the contract rate and default produced by the income effect, and a negative correlation produced by borrowers prioritizing repayment of relatively expensive outside obligations— could explain why we find little evidence of a repayment burden effect.

An alternative interpretation is that both the income effect and the incentives provided by the contract rate are relatively small. This reconciles the contract rate and dynamic repayment results by noting that the two types of incentives— discounts on current and future loans— are qualitatively different. The current discount provides a discount with certainty, *unconditional on loan repayment*. If defaulting is relatively cheap for the borrower due to limited enforcement and/or the limited value of future access to credit at *normal* rates, then the repayment burden effect is likely to be relatively small (in the absence of an income effect). The future contract interest rate, on the other hand, is a direct incentive to repay since the future interest rate is lower *only if* the borrower repays the current loan without arrearage. As shown in Section III-A, the discounted future interest rate is large on average (350 basis points), and obtained with high probability.

The second issue, endogeneity of the loan amount and maturity with respect to the contract rate, does not seem to be borne out by the data. It is true, as noted in Section III-E, that borrowers who had not already agreed to borrow the maximum amount offered by the loan officer were allowed to re-optimize following the revelation of a lower contract rate. A lower contract rate might induce more borrowing on the intensive margin via loan amount and/or maturity (Karlan and Zinman 2005), thereby pushing against finding traditional moral hazard effect with respect to the contract rate. The potential confound stems from the fact that the lower contract rate improves repayment incentives only *ceteris paribus*; if loan amount and/or maturity increases as a result of the lower rate, this weakens repayment incentives. But the data suggest that only 3% of borrowers receiving a lower contract rate re-optimized. This low frequency is driven in large part by supply constraints; many borrowers had already decided to borrow the maximum amount and maturity offered by the Lender, and *supply* decisions did not change following the revelation of the contract rate. Two econometric approaches help confirm that endogeneity did not contaminate the contract results in practice. One adds control variables for loan size and maturity to the specifications presented in Table 4. The results do not change (not shown). Nor does adding branch fixed effects to control for any differences in experimental implementation change the results. An alternative approach is to instrument for total repayment burden (evaluated separately at the offer and contract rates) using the randomly assigned interest rates. The instrumental variables results are qualitatively similar to those obtained with OLS (a positive, significant contract rate effect on Proportion of Months in Arrears, nothing on the other default variables, results not shown).

In all then, it seems likely that the contract rate results are explained either by offsetting income and advantageous moral hazard effects, or by a relatively weak income effect coupled with relatively weak incentives provided by the contract rate.

C. Interpreting the Dynamic Repayment Incentive Results

Again, the sharp increase in current repayment induced by the dynamic repayment incentive indicates pure moral hazard. D did not change current debt burden, only the incentive to repay. The striking thing here is that D had such a large effect even in the presence of the Lender's pre-existing repeat contracting scheme. We discuss this more in the Conclusion.

D. Is the Lender Assessing Risk Efficiently?

A final question is whether the Lender faced asymmetric information problems due to its own inefficiency in assessing risk; i.e., was there readily observable information that the Lender could and should have used to price risk, but did not? For example, although the law prohibits underwriting based on gender, the Lender could change its weighting of prior borrowing history and related interactions, per Table 7. However, we must keep in mind that, on balance, we find little evidence of adverse selection in the full sample. This suggests that alternative tests of risk assessment efficiency on this sample should find that the Lender can do little else to predict default based on *ex-ante* observables. Table 8 shows that this is indeed the case. It presents results from a model of default on observables, *conditional on the Lender's assessment of observable risk*. We estimate the model after adding several additional observables to equation (14). Although several of the observed variables are independent predictors of default, adding observables beyond the summary statistic generates only small improvements in the overall explanatory power of the models (as measured by the adjusted R-squareds; compare to Table 4).

This does not rule out the possibility that the Lender used information inefficiently when screening out clients (rather than pricing risk), and/or when lending at its normal range of rates. It merits repeating, however, that the Lender was relatively profitable and long-lived compared to its competitors.

VIII. Conclusion

We develop a new market field experiment methodology that disentangles adverse selection from moral hazard under plausible identifying assumptions. The experiment was implemented on a sample of successful prior borrowers by a for-profit lender in a high-interest, high-risk consumer loan market in South Africa. The experiment produces evidence of significant moral hazard, and weaker evidence of adverse selection. This study has both methodological and practical motivations.

Practically, as discussed in the introduction, identifying the existence and prevalence of any adverse selection and moral hazard is important because of the preponderance of credit market interventions that *presuppose* credit rationing arising from these asymmetric information problems. Adverse selection and moral hazard are the theoretical microfoundations that have motivated the microfinance movement to fight poverty and promote growth by expanding access to credit. Billions of dollars of subsidies, and countless other resources, have been allocated to such efforts.

As such, the theory and practice of microcredit is far ahead of the empirical evidence. To craft optimal policies and business strategies we need answers to at least three key questions: (1) Which models of information asymmetries (if any) accurately describe existing markets? (2) What lending practices are effective at mitigating information asymmetries? (3) What are the welfare implications of resolving information asymmetry problems in credit markets?

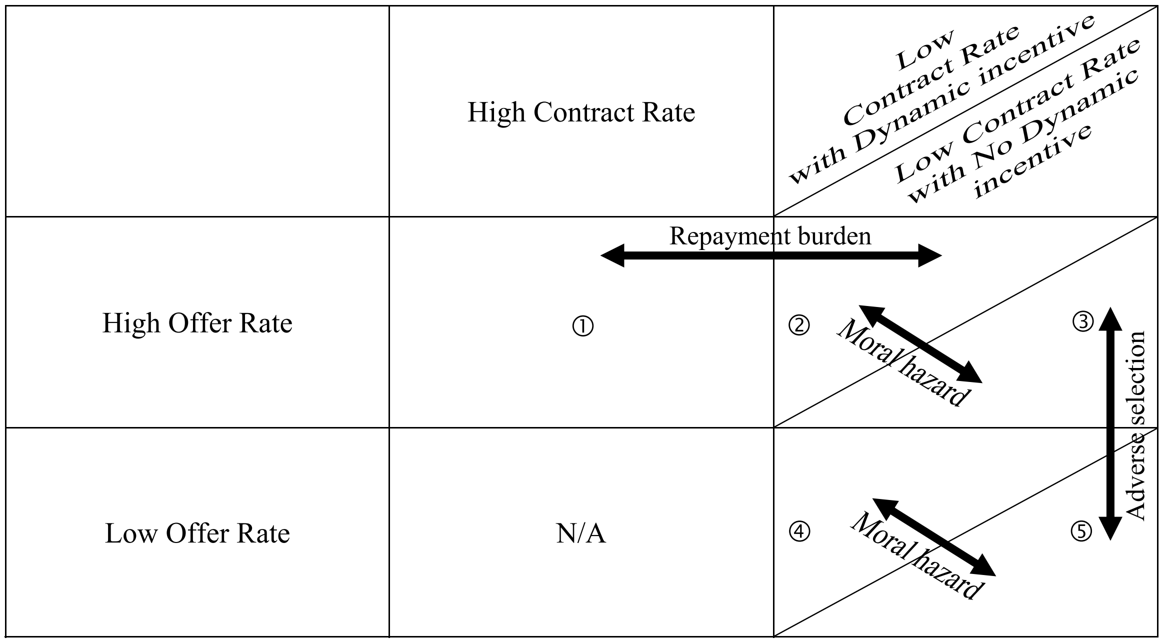
Our paper makes inroads on the first question only, and hence does not lead directly to a policy prescription. It is not advisable to extrapolate our findings to other markets and settings without further study. We note simply that this paper provides uniquely clean and direct evidence of a specific asymmetric information problem in a credit market. Again, this type of evidence is the first piece of several that would be needed to rigorously justify and refine welfare-improving credit market innovations and interventions. We believe that there are particularly strong motivations for implementing similar designs on samples of the types of truly marginal (e.g. first-time) borrowers that are often the focus of microcredit initiatives. Such studies would help address the question of whether moral hazard is more endemic than adverse selection, and whether adverse selection prevents credit markets from clearing marginal borrowers.

To the extent that academics, practitioners, and policymakers are interested in building on our findings, we suggest two particular directions. One is refining dynamic contracts to alleviate moral hazard. The powerful effect of the dynamic repayment incentive (Tables 3 and 4), and the findings hinting that private information is revealed through the course of lending relationships (Table 7), suggest that there may be profitable and welfare-enhancing opportunities to refine dynamic contracting schemes. Our setting suggests that this is worth exploring even where successful lenders are already using repeat play to strengthen borrower repayment incentives. The second is a re-examination of gender issues with respect to credit market failures. Microcredit initiatives are often designed to remedy both information asymmetries and gender discrimination, but there has been little examination of whether information problems vary by gender and how this may influence these dual objectives. Our results suggest that adverse selection is only a problem among pools of female borrowers, but further studies will be needed to test whether and why this pattern prevails in other markets.

On a methodological level, this paper demonstrates how experimental methodologies can be implemented, in market settings, to answer questions of theoretical interest (Banerjee, Bardhan, Basu, Kanbur and Mookherjee 2005; Duflo 2005). Field experiments need not be limited to program evaluation. Introducing several dimensions of random variation in contract terms enabled us to move beyond reduced-form treatment effects, and toward testing theoretical predictions. This approach has value to firms weighing investments in screening, monitoring, and/or enforcement, and to academics interested in testing and refining theories of asymmetric information. Our specific design is replicable— indeed, we are currently implementing a similar experiment in a private hospitalization insurance market in the Philippines— and a growing number of projects points to the general feasibility of partnering with firms to implement field experiments and study questions of mutual interest.

More generally, our work highlights the value of interplay between theoretical and empirical work in contract theory. Any individual theory of adverse selection or moral hazard in a credit market makes several specific, strong assumptions that breed doubts about its applicability and generalizability to any particular credit market. Consequently, uncovering the actual nature and practical implications (if any) of asymmetric information problems in credit markets will require theoretical as well as empirical progress. Salanie (2005) lauds the "constant interaction between theory and empirical studies" (p. 221) that has characterized the closely related literature on insurance markets. Comparably intense interactions would deepen our understanding of credit markets.

Figure 1. Basic Intuition Behind the Experimental Design



Section V formally derives our identification strategy and related assumptions. This figure provides some basic intuition behind our strategy of using three dimensions of random variation in interest rates to identify the presence or absence of specific asymmetric information problems. The actual experiment generated continuous variation in two of the three rates (offer and contract), conditional on observable risk. Here for expositional purposes we label each assigned rate either “high” or “low” based on the median experimental rate for the borrower’s observable risk category. This highlights that our methodology:

- Identifies adverse selection by focusing on those who borrow at the low contract rates, and comparing the repayment behavior of those who select in at high offer rates (cells 2 and 3 in the diagram) with those who select in a low offer rates (cells 4 and 5). If there is adverse selection then default will be lower in cells 4 and 5.
- Identifies moral hazard by focusing on those who borrow at low contract rates, and comparing the repayment behavior of those who received the dynamic repayment incentive (cells 2 and 4 in the diagram) with those who did not (cells 3 and 5). If the dynamic repayment incentive alleviates moral hazard then default will be lower in cells 2 and 4.
- Identifies repayment burden by focusing on those who select in at high offer rates, and comparing the repayment behavior of those who borrow at high contract rates (cell 1 in the diagram) with those who borrow at low contract rates (cells 2 and 3 in the diagram). If there is a repayment burden effect then default will be lower in cells 2 and 3.

Figure 2: Operational Steps of Experiment

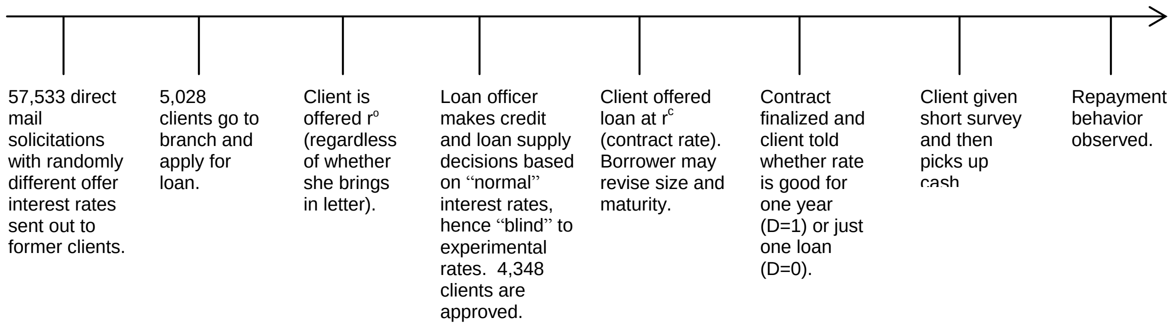
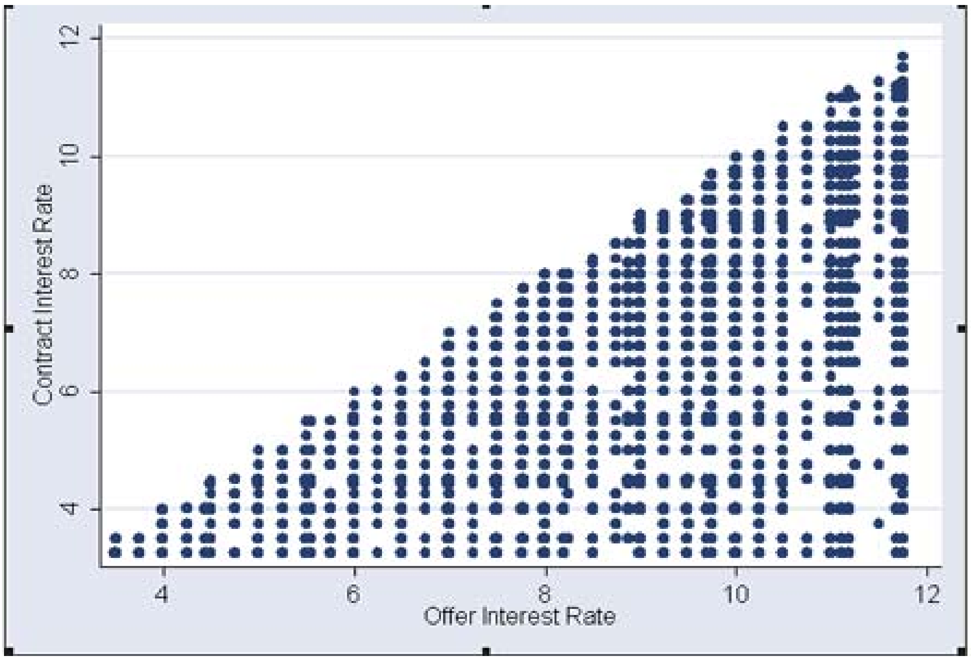


Figure 3: Scatter plot of Contract versus Offer Interest Rates on 4 Month Loans



Plot includes only the 41% of borrowers that received a contract rate less than the offer rate.

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Table 1a. Summary Statistics for Sample Frame, Borrowers, and Other Sub-Samples of Interest

						Lender-Defined Risk Category		
	All	*Borrowed*	*Female Borrowed*	*Male Borrowed*	*Did Not Borrow*	*High Risk*	*Medium Risk*	*Low Risk*
A. Full Sample								
# of months since last loan	10.3	5.9	6.0	5.8	10.6	12.7	2.8	2.8
	(6.9)	(5.8)	(5.8)	(5.8)	(6.8)	(6.1)	(1.7)	(1.6)
Size of last loan prior to project (Rand)	1116.4	1156.0	1161.4	1150.9	1113.1	1086.4	1176.5	1229.7
	(829.9)	(825.7)	(798.2)	(851.6)	(830.2)	(785.2)	(878.4)	(994.5)
# of prior loans with the lender	4.3	4.9	4.8	4.9	4.2	3.6	5.7	6.6
	(3.9)	(4.2)	(4.2)	(4.2)	(3.8)	(3.5)	(4.2)	(4.3)
Maturity of last loan prior to project								
1 or 2 months	1,656	132	54	78	1,524	1,407	93	156
	2.88%	3.04%	2.53%	3.52%	2.87%	3.26%	1.50%	1.92%
4 months	53,296	3,939	1,926	2,013	49,357	40,687	5,658	6,951
	92.64%	90.59%	90.30%	90.88%	92.80%	94.18%	91.17%	85.54%
6 months	2,030	223	123	100	1,807	887	369	774
	3.53%	5.13%	5.77%	4.51%	3.40%	2.05%	5.95%	9.52%
12 months	551	54	30	24	497	220	86	245
	0.96%	1.24%	1.41%	1.08%	0.93%	0.51%	1.39%	3.02%
Number of Observations	57,533	4,348	2,133	2,215	53,185	43,201	6,206	8,126
B. Randomized Variables								
Offer Interest Rate	7.88	7.18	7.16	7.22	7.94	8.10	7.20	5.73
	(2.42)	(2.30)	(2.32)	(2.29)	(2.42)	(2.48)	(1.85)	(1.36)
Contract Interest Rate	7.08	6.53	6.46	6.58	7.12	7.29	6.56	5.28
	(2.42)	(2.26)	(2.25)	(2.27)	(2.42)	(2.52)	(1.87)	(1.34)
Proportion Receiving Rate for One year (vs. one loan)	0.43	0.47	0.47	0.47	0.43	0.46	0.47	0.48
	(0.50)	(0.50)	(0.50)	(0.50)	(0.49)	(0.50)	(0.50)	(0.50)
Proportion Receiving a Contract Rate < Offer Rate	0.41	0.40	0.40	0.40	0.41	0.41	0.39	0.39
	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)
C. Default Measure								
Monthly Average Past Due Amount		152.56	131.10	173.21		180.13	224.49	57.40
		(359.28)	(337.39)	(378.09)		(404.86)	(408.52)	(181.67)
Monthly Avg Past Due Amount, Proportion of Principal		0.09	0.08	0.11		0.12	0.13	0.03
		(0.21)	(0.19)	(0.23)		(0.24)	(0.24)	(0.11)
Proportion of Months With Some Arrearage		0.22	0.20	0.24		0.25	0.32	0.10
		(0.29)	(0.28)	(0.30)		(0.31)	(0.31)	(0.19)
Account is in Collection (3+ months arrears)		0.12	0.10	0.14		0.14	0.17	0.04
		(0.32)	(0.30)	(0.33)		(0.35)	(0.38)	(0.19)
Number of Observations	57,533	4,348	2,133	2,215	53,185	2,090	941	1,317

Standard deviations are in parentheses. Money amounts in South African Rand, ~7.5 Rand = US $1 at the time of the experiment. Please see Section III-D of the text for more details on the randomized variables. Please see Section III-F for more details on the default measures.

Table 1b. Summary Statistics

	Full Sample	Female	Male	Female Borrowed	Male Borrowed
A. Client Characteristics					
Female, proportion	0.48	1	0	1	0
	(0.50)	(0)	(0)	(0)	(0)
Married, proportion	0.44	0.37	0.50	0.39	0.52
	(0.50)	(0.48)	(0.50)	(0.49)	(0.50)
# of dependents	1.59	1.53	1.64	1.82	1.97
	(1.74)	(1.62)	(1.85)	(1.61)	(1.87)
Age	41.25	42.03	40.55	41.74	40.10
	(11.53)	(11.89)	(11.14)	(11.38)	(10.82)
Education (# of years, estimated from occupation)	6.78	7.23	6.36	7.45	6.53
	(3.32)	(3.45)	(3.14)	(3.51)	(3.19)
Monthly gross income at last loan (000's Rand)*	3.42	3.26	3.56	3.39	3.45
	(19.66)	(2.63)	(27.05)	(2.19)	(2.07)
Home bond, proportion	0.07	0.07	0.06	0.08	0.06
	(0.25)	(0.25)	(0.24)	(0.26)	(0.24)
External credit score	551.35	544.23	557.82	547.77	571.69
	(215.64)	(210.22)	(220.27)	(203.20)	(204.22)
No external credit score, proportion	0.12	0.11	0.12	0.11	0.10
	(0.32)	(0.32)	(0.33)	(0.31)	(0.30)
Months at Employer	93.82	90.42	96.92	93.34	96.86
	(88.01)	(82.55)	(92.59)	(82.33)	(88.53)
# of Observations	57533	27387	30146	2133	2215
B. Loan Characteristics					
Amount of last loan prior to experiment	1116.36	1122.87	1110.44	1161.37	1150.86
	(829.90)	(844.42)	(816.46)	(798.21)	(851.56)
Maturity of last loan prior to experiment	4.06	4.09	4.03	4.15	4.07
	(1.00)	(1.01)	(1.00)	(1.16)	(1.09)
# of prior loans with the lender	4.26	4.22	4.29	4.83	4.90
	(3.86)	(3.82)	(3.90)	(4.20)	(4.26)
# of months since the last loan	10.26	10.21	10.31	5.98	5.82
	(6.88)	(6.84)	(6.92)	(5.78)	(5.82)
Internal credit score when new borrower	29.66	32.59	26.99	32.97	27.40
	(8.75)	(8.53)	(8.06)	(8.38)	(8.22)
# of Observations	57533	27387	30146	2133	2215
C. Self-Reported Loan Usage					
School				24.2%	13.6%
Housing (mostly renovations)				12.6%	9.8%
Payoff other debt				10.9%	11.1%
Family/Event				5.7%	8.1%
Consumption				5.6%	7.1%
Transport				4.1%	7.6%
Funeral/Medical				3.8%	4.4%
Durable				2.3%	1.0%
Business/Other Investment				2.3%	2.7%
Misc/unreported				28.7%	34.6%
# of Observations				690	775

* Standard deviations are in parentheses. Gross income at time of last loan is missing for participants from pilot phase. Age, gender and other demographic information also missing for <10 observations. Number of observations reported is the total number, irrespective of missing data. Usage sample size is low relative to takeup due to reluctance of loan officers to administer survey (the Lender does not typically ask applicants about intended usage, and if anything emphasizes that it does not ask such questions). Reported "Consumption" uses are primarily food (39%) and clothing (23%); "Family/Events" are largely Christmas (45%) expenses; "School" is largely the fees required for children to attend; "Misc" is largely borrowers declining to specify (88%).

Table 2. Experimental Integrity Checks and Observable Selection

OLS

					Sample Restricted to Applied = 1
Dependent variable:	Contract Rate (1)	Offer Rate (2)	Rate Valid for One Year (versus One Loan) (3)	Applied=1 (4)	Rejected = 1 (5)
Female	0.009	0.028	-0.002		
	(0.022)	(0.021)	(0.004)		
Married	0.017	0.022	0.004		
	(0.022)	(0.021)	(0.004)		
External credit score	-0.000	-0.000	0.000		
	(0.000)	(0.000)	(0.000)		
No External credit score	-0.017	-0.006	0.016		
	(0.093)	(0.091)	(0.016)		
Internal credit score	-0.001	-0.002	0.000		
	(0.001)	(0.001)	(0.000)		
Log (Size of last loan prior to project)	-0.017	-0.003	-0.004		
	(0.017)	(0.017)	(0.003)		
Maturity of last loan prior to project	-0.010	-0.011	-0.001		
	(0.011)	(0.010)	(0.002)		
# of prior loans with the lender	0.003	0.003	0.001**		
	(0.003)	(0.003)	(0.001)		
Gross income	-0.001	-0.000	0.000		
	(0.001)	(0.000)	(0.000)		
Years at Employer	0.000	0.001	-0.000		
	(0.002)	(0.002)	(0.000)		
Mean education	0.002	-0.002	-0.000		
	(0.003)	(0.003)	(0.001)		
# of dependants	0.002	-0.005	0.000		
	(0.007)	(0.006)	(0.001)		
Age	-0.000	-0.001	-0.000*		
	(0.001)	(0.001)	(0.000)		
Home bond	0.053	0.028	0.011		
	(0.041)	(0.040)	(0.007)		
# of months since last loan	-0.001	-0.001	-0.001***		
	(0.002)	(0.002)	(0.000)		
Offer Interest Rate				-0.003***	
				(0.001)	
Contract Interest Rate				0.000	-0.001
				(0.001)	(0.002)
Dynamic Repayment Incentive					-0.014
					(0.012)
Constant	7.700***	8.369***	0.228***	0.081***	0.334***
	(0.297)	(0.292)	(0.051)	(0.005)	(0.075)
Observations	57339	57339	57339	57533	5028
Joint F-Test	0.87	0.96	0.01		
R-squared	0.10	0.14	0.37	0.04	0.09

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors in parentheses. Columns 1 through 3 test whether the randomized variables are correlated with information observable before the experiment launch. For column 3, if the dormancy variable is omitted the F-test is 0.21. Column 4 shows that the decision to borrow by the client was affected by the Offer Interest Rate, but not the Contract Interest Rate, hence verifying the internal controls of the experimental protocol. Column 5 shows that the decision by the branch manager to reject applicants was not predicted by the contract interest rate or the dynamic repayment incentive. Column 5 sample frame includes only those who applied for a loan. Regressions include controls for lender-defined risk category, month of offer letter and branch.

Table 3. Identifying Adverse Selection, Repayment Burden, and Moral Hazard: Comparison of Means

		Selection Effects			Repayment Burden Effects			Moral Hazard Effects		
		High Offer, Low Contract	Low Offer, Low Contract	t-stat: diff≠0	High Offer, High Contract	High Offer, Low Contract	t-stat: diff≠0	No Dynamic Incentive, Low Contract	Dynamic Incentive, Low Contract	t-stat: diff≠0
Full Sample		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Average Monthly Proportion Past Due	0.102	0.082	1.90*	0.105	0.102	0.23	0.094	0.079	1.94**
		(0.009)	(0.004)		(0.006)	(0.009)		(0.006)	(0.005)	
	Proportion of Months in Arrears	0.211	0.202	0.72	0.244	0.211	2.38**	0.217	0.188	2.70***
		(0.011)	(0.006)		(0.008)	(0.011)		(0.008)	(0.008)	
	Account in Collection Status	0.123	0.101	1.50	0.139	0.123	0.99	0.118	0.092	2.16**
		(0.013)	(0.007)		(0.009)	(0.013)		(0.008)	(0.008)	
	# of observations	625	2087		1636	625		1458	1254	
Female										
	Average Monthly Proportion Past Due	0.101	0.067	2.42**	0.089	0.101	-0.85	0.078	0.071	0.65
		(0.013)	(0.005)		(0.007)	(0.013)		(0.007)	(0.007)	
	Proportion of Months in Arrears	0.209	0.181	1.55	0.221	0.209	0.64	0.194	0.180	0.97
		(0.02)	(0.008)		(0.011)	(0.02)		(0.010)	(0.010)	
	Account in Collection Status	0.121	0.082	1.88*	0.107	0.121	-0.65	0.102	0.078	1.57
		(0.019)	(0.008)		(0.121)	(0.019)		(0.011)	(0.011)	
	# of observations	307	1047		779	307		724	630	
Male										
	Average Monthly Proportion Past Due	0.103	0.099	0.30	0.120	0.103	1.05	0.111	0.087	1.97**
		(0.013)	(0.007)		(0.008)	(0.013)		(0.009)	(0.008)	
	Proportion of Months in Arrears	0.213	0.223	-0.51	0.264	0.213	2.60***	0.240	0.197	2.77***
		(0.016)	(0.009)		(0.011)	(0.016)		(0.011)	(0.011)	
	Account in Collection Status	0.126	0.120	0.26	0.168	0.126	1.87*	0.134	0.107	1.48
		(0.019)	(0.010)		(0.013)	(0.019)		(0.013)	(0.012)	
	# of observations	318	1040		857	318		734	624	

"High" is defined as above the median offer rate for that risk category. This is equal to 7.77% for high risk clients, 7.50% for medium risk clients and 6.00% for low risk clients. Sample sizes vary due to exclusions motivated by the formal derivation of our identification strategy, please see Section V for details. The column headings indicate which rate cells are included in any given analysis. T-tests assume unequal variances across columns.

Table 4. Identifying Adverse Selection, Repayment Burden, and Moral Hazard: OLS on the Full Sample

OLS

Dependent Variable:	*Monthly Average Proportion Past Due*		*Proportion of Months in Arrears*		*Account in Collection Status*	
	(1)	(2)	(3)	(4)	(5)	(6)
Offer Rate (Selection)	0.004	0.004	0.002	0.002	0.007	0.007
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)
Contract Rate (Repayment Burden)	-0.000	-0.002	0.007*	0.003	0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)
Dynamic Repayment Incentive Dummy (Moral Hazard)	-0.011*	0.003	-0.016**	0.013	-0.019**	0.000
	(0.005)	(0.011)	(0.008)	(0.018)	(0.009)	(0.019)
Dynamic Repayment Incentive Size (Moral Hazard)		-0.004		-0.008**		-0.005
		(0.003)		(0.004)		(0.004)
Constant	0.079***	0.094***	0.139***	0.171***	0.069***	0.090***
	(0.014)	(0.019)	(0.025)	(0.027)	(0.024)	(0.028)
Observations	4348	4348	4348	4,348	4348	4348
Adjusted R-squared	0.04	0.04	0.11	0.11	0.03	0.03
Mean of dependent variable	0.09	0.09	0.22	0.22	0.12	0.12
Prob(both Dynamic Incentive variables = 0)		0.08*		0.01***		0.05**

* significant at 10%; ** significant at 5%; *** significant at 1%. Each column presents results from a single model estimated using the base OLS specification (equation 14). Tobits and probits (not reported) produce qualitatively identical results. Robust standard errors in parentheses are corrected for clustering at the branch level. “Offer Rate” and “Contract Rate” are in monthly percentage point units (7.00% interest per month is coded as 7.00). “Dynamic Repayment Incentive” is an indicator variable equal to one if the contract interest rate is valid for one year (rather than just one loan) before reverting back to the normal (higher) interest rates. "Dynamic Repayment Incentive Size" interacts the above indicator variable with the difference between the Lender's normal rate for that individual's risk category and the experimentally assigned contract interest rate. All models include controls for lender-defined risk category and month of offer letter. Adding loan size and maturity as additional controls does not change the results. A positive coefficient on the Offer Rate variable indicates adverse selection, a positive coefficient on the Contract Rate variable indicates a reduced-form repayment burden effect, and a negative coefficient on the Dynamic Repayment Incentive variable indicates moral hazard that is alleviated by the dynamic pricing incentive.

Table 5. Identifying Adverse Selection, Repayment Burden, and Moral Hazard by Gender

OLS

	Male			Female		
Dependent Variable:	*Monthly Average Proportion Past Due*	*Proportion of Months in Arrears*	*Account in Collection Status*	*Monthly Average Proportion Past Due*	*Proportion of Months in Arrears*	*Account in Collection Status*
	(1)	(2)	(3)	(4)	(5)	(6)
Offer Rate	-0.002	-0.004	0.001	0.010***	0.008*	0.013**
	(0.004)	(0.005)	(0.007)	(0.003)	(0.005)	(0.005)
Contract Rate	0.005	0.014***	0.010	-0.005	-0.001	-0.009
	(0.003)	(0.005)	(0.007)	(0.004)	(0.005)	(0.006)
Dynamic Repayment	-0.014	-0.025**	-0.020	-0.007	-0.006	-0.017
Incentive Indicator	(0.009)	(0.012)	(0.015)	(0.008)	(0.012)	(0.012)
Constant	0.108***	0.178***	0.092**	0.050***	0.097***	0.043
	(0.025)	(0.040)	(0.043)	(0.015)	(0.026)	(0.027)
Observations	2215	2215	2215	2133	2133	2133
R-squared	0.05	0.12	0.04	0.05	0.10	0.04

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors in parentheses are corrected for clustering at the branch level. Results reported here are estimated using the base OLS specification (equation 14) on samples split by gender. The specification includes controls for lender-defined risk category and month of offer letter. Adding loan size and maturity as additional controls does not change the results. Using tobit or probit instead of OLS produces qualitatively similar results.

Table 6: Heterogeneity by Gender, or by Other Demographics?

OLS

Dependent Variable: Monthly Average Percentage Past Due

Demographic Control Variable:	*Married*		*Dependents in Household*		*Educated*		*Age*		*Log(Monthly Gross Income)*		*Tenure at Employment*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Experimental Variables												
Offer Rate	0.023	-0.252	0.089	0.176	0.079	-0.213	0.282	-0.341	2.700	-0.716	0.122	0.082
	(0.435)	(0.537)	(0.432)	(0.530)	(0.402)	(0.410)	(1.162)	(1.325)	(2.338)	(4.218)	(0.456)	(0.495)
Contract Rate	0.415	0.716	0.482	0.660	0.260	0.557	0.269	0.652	-0.968	1.852	0.404	0.442
	(0.393)	(0.508)	(0.446)	(0.527)	(0.414)	(0.440)	(1.098)	(1.283)	(2.707)	(4.600)	(0.465)	(0.523)
Dynamic Repayment Incentive Indicato	-1.158	-0.706	-1.098	-1.920	-0.878	-1.081	-1.280	0.934	7.378	3.457	-1.165	-0.691
	(1.160)	(1.510)	(1.237)	(1.434)	(1.028)	(1.252)	(2.678)	(4.049)	(8.692)	(12.814)	(1.145)	(1.465)
Female	-2.985	-3.095	-2.558	-1.021	-2.215	-2.652	-1.887	-5.296	-2.821	-30.918	-2.667	-2.298
	(1.939)	(2.585)	(1.980)	(3.110)	(1.886)	(2.631)	(1.914)	(7.409)	(1.926)	(28.386)	(1.875)	(3.073)
Demographic Variable (see column heading)	-1.838	-2.040	-0.036	0.427	-1.761	-2.487	-0.172	-0.223	-0.001	-2.020	-0.015	-0.013
	(1.952)	(2.854)	(0.536)	(0.741)	(2.432)	(3.909)	(0.105)	(0.157)	(1.669)	(3.019)	(0.012)	(0.018)
*Female * Experimental Variables*												
Female * Offer Rate	0.887*	1.369**	0.834*	0.637	0.902*	1.534**	0.763*	1.951	0.890**	6.945	0.807*	0.891
	(0.456)	(0.632)	(0.460)	(0.661)	(0.480)	(0.604)	(0.455)	(1.916)	(0.445)	(6.117)	(0.447)	(0.749)
Female * Contract Rate	-1.042**	-1.575**	-1.029**	-1.440**	-1.138**	-1.783***	-0.977**	-1.782	-1.040**	-6.318	-0.967**	-1.047
	(0.476)	(0.718)	(0.497)	(0.678)	(0.482)	(0.640)	(0.486)	(1.979)	(0.474)	(7.129)	(0.479)	(0.748)
Female * Dynamic Repayment Incentive	0.813	-0.037	0.896	2.732	1.077	1.554	0.701	-3.491	0.603	8.148	0.730	-0.290
	(1.350)	(2.143)	(1.343)	(2.052)	(1.351)	(1.903)	(1.336)	(5.867)	(1.353)	(14.026)	(1.328)	(2.363)
*Demographic Control Variable * Experimental Variables*												
Demographic Variable * Offer Rate	-0.135	0.415	-0.046	-0.084	-0.400	0.626	-0.008	0.008	-0.343	0.079	-0.002	-0.001
	(0.540)	(0.796)	(0.122)	(0.164)	(0.625)	(0.853)	(0.026)	(0.030)	(0.289)	(0.522)	(0.003)	(0.003)
Demographic Variable * Contract Rate	0.195	-0.397	-0.009	-0.124	0.748	-0.279	0.006	-0.003	0.183	-0.166	0.001	0.001
	(0.511)	(0.788)	(0.141)	(0.177)	(0.583)	(0.776)	(0.026)	(0.031)	(0.325)	(0.561)	(0.003)	(0.003)
Demographic Variable * Dynamic Repayment Incentive	-0.577	-1.442	-0.224	0.162	-1.577	-1.017	-0.002	-0.056	-1.077	-0.592	-0.002	-0.007
	(1.211)	(1.897)	(0.353)	(0.431)	(1.307)	(2.104)	(0.061)	(0.092)	(1.042)	(1.530)	(0.006)	(0.009)
*Female * Demographic Control Variable*												
Female * Demographic Variable		0.305		-1.217		1.155		0.083		3.457		-0.004
		(3.234)		(1.138)		(4.167)		(0.167)		(3.460)		(0.023)
*Female * Demographic Control Variable * Experimental Variables*												
Female * Demographic Variable * Offer Rate		-1.079		0.111		-1.755		-0.029		-0.748		-0.001
		(0.951)		(0.274)		(1.080)		(0.044)		(0.758)		(0.006)
Female * Demographic Variable * Contract Rate		1.181		0.277		1.777		0.020		0.654		0.001
		(1.033)		(0.292)		(1.196)		(0.045)		(0.882)		(0.005)
Female * Demographic Variable * Dynamic Repayment Incentive		1.797		-0.968		-1.049		0.102		-0.937		0.011
		(2.652)		(0.654)		(2.713)		(0.125)		(1.672)		(0.015)
Constant	10.161***	10.236***	8.917***	8.252***	9.608***	9.821***	14.984***	17.066**	9.240	25.704	10.281***	10.122***
	(2.476)	(2.791)	(2.542)	(2.986)	(2.240)	(2.546)	(5.136)	(7.222)	(13.856)	(25.009)	(2.642)	(3.133)
Observations	4317	4317	4317	4317	4348	4348	4348	4348	4348	4348	4348	4348
R-squared	0.05	0.05	0.05	0.05	0.05	0.05	0.06	0.06	0.05	0.05	0.06	0.06

* significantat 10%; ** significantat 5%; *** significantat 1%. Each column presents results from a single OLS regression on a version of equation (14). Robust standard errors in parentheses are corrected for clustering at the branch level. "Educated" is a binary indicator for the top 25% in years of education, predicted by the client's occupation. Regressions include controls for lender-defined risk category and month of offer letter. Adding loan size and maturity as additional controls does not change the results. The dependent variable here is defined in percentage point terms, not proportions, and hence equals 100x the variable used in other tables.

Table 7: Are Information Asymmetries Less Severe for Clients with More Frequent Borrowing History?

OLS

Dependent Variable: Monthly Average Proportion Past Due

Sample:	*All*		
	(1)	(2)	(3)
Offer Rate	0.008**	0.004	0.004
	(0.003)	(0.003)	(0.003)
Contract Rate	0.000	0.004	0.000
	(0.003)	(0.003)	(0.003)
Dynamic Repayment Incentive Indicator	-0.011*	-0.011*	-0.013
	(0.006)	(0.006)	(0.010)
# of prior loans with the lender	0.001	0.000	
	(0.002)	(0.001)	
Offer Rate*# of prior loans	-0.001***		
	(0.000)		
Contract Rate*# of prior loans		-0.001***	
		(0.000)	
Rate Valid for One Year*# of prior loans			0.001
			(0.001)
Constant	0.078***	0.083***	0.105***
	(0.018)	(0.017)	(0.014)
Observations	4317	4317	4317
R-squared	0.05	0.05	0.05

* significant at 10%; ** significant at 5%; *** significant at 1%. Each column presents results from a single OLS regression on a version of equation (14). Robust standard errors in parentheses are corrected for clustering at the branch level. Regressions include controls for lender-defined risk category and month of offer letter. Adding controls for loan size and maturity does not change the results.

Table 8 Observable Determinants of Default and Assessment Efficiency

OLS

Dependent Variable:	*Monthly Average Proportion Past Due*		*Proportion of Months in Arrears*		*Account in Collection Status*	
	(1)	(2)	(3)	(4)	(5)	(6)
Offer Rate	-0.001		-0.003		0.003	
	(0.003)		(0.005)		(0.006)	
Contract Rate	0.005		0.014***		0.010	
	(0.003)		(0.005)		(0.007)	
Dynamic Repayment Incentive Indicator	-0.017*		-0.024**		-0.022	
	(0.010)		(0.012)		(0.016)	
Female * Offer Rate	0.007*		0.008		0.007	
	(0.004)		(0.006)		(0.007)	
Female * Contract Rate	-0.009**		-0.015**		-0.017**	
	(0.005)		(0.007)		(0.008)	
Female * Dynamic Repayment Incentive	0.008		0.014		0.003	
	(0.013)		(0.018)		(0.021)	
Female	-0.015	-0.021***	-0.005	-0.035***	0.033	-0.029**
	(0.019)	(0.007)	(0.026)	(0.010)	(0.027)	(0.012)
Log(loan size)	-0.026***	-0.026***	0.013*	0.013*	0.004	0.004
	(0.005)	(0.005)	(0.007)	(0.007)	(0.008)	(0.008)
Age	0.000	0.000	0.002	0.001	0.002	0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(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)
Years at Employer	-0.001	-0.001	-0.001**	-0.001**	-0.002*	-0.002*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Gross Income	0.003	0.003	-0.007*	-0.007*	-0.006	-0.005
	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)
Education (predicted by occupation)	-0.001	-0.001	-0.001	-0.002	-0.002	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
# of Dependents	-0.001	-0.001	0.000	0.000	-0.006*	-0.006**
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
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)
No External Credit Score	-0.097***	-0.100***	-0.244***	-0.251***	-0.075*	-0.082*
	(0.035)	(0.034)	(0.049)	(0.049)	(0.045)	(0.044)
Internal Credit Score at First-Time Application	-0.001*	-0.001*	-0.001*	-0.001**	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Married	0.002	0.003	0.005	0.005	0.014	0.015
	(0.007)	(0.007)	(0.009)	(0.009)	(0.012)	(0.012)
Home Bond	0.010	0.009	0.014	0.012	0.041*	0.038*
	(0.014)	(0.014)	(0.021)	(0.022)	(0.023)	(0.022)
# of prior loans with the lender	-0.003***	-0.003***	-0.005***	-0.005***	-0.004***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
# of months since last loan	0.004***	0.004***	0.004**	0.004**	0.005***	0.005***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Constant	0.466***	0.488***	0.412***	0.486***	0.277***	0.368***
	(0.069)	(0.068)	(0.087)	(0.080)	(0.100)	(0.089)
Observations	4348	4348	4348	4348	4348	4348
R-squared	0.0886	0.0862	0.1570	0.1520	0.0711	0.0660
Adjusted r-squared	0.0808	0.0796	0.1497	0.1459	0.0631	0.0593

* significant at 10%; ** significant at 5%; *** significant at 1%. Each column presents results from a single OLS regression on a version of equation (14). Robust standard errors in parentheses are corrected for clustering at the branch level.

Appendix Table 1. Frequency of Monthly Offer and Contract Interest Rates

	Low Risk Clients				Medium Risk Clients				High Risk Clients			
	Offer Interest Rate		*Contract Interest Rate*		*Offer Interest Rate*		*Contract Interest Rate*		*Offer Interest Rate*		*Contract Interest Rate*	
	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent
3.25%	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: Cross-Tabulation of Individual Cell Sizes for Monthly Offer and Contract Interest Rates

Monthly Offer Interest Rate	Monthly Contract Interest Rate: 3.00	3.50	4.00	4.50	5.00	5.50	6.00	6.50	7.00	7.50	8.00	8.50	9.00	9.50	10.00	10.50	11.00	11.50	Total
3.00	1,971	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1,971
3.50	442	1,809	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2,251
4.00	154	628	2,256	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	3,038
4.50	78	239	417	1,291	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2,025
5.00	38	178	308	294	1,464	0	0	0	0	0	0	0	0	0	0	0	0	0	2,282
5.50	41	192	353	353	360	2,270	0	0	0	0	0	0	0	0	0	0	0	0	3,569
6.00	16	49	82	93	96	143	774	0	0	0	0	0	0	0	0	0	0	0	1,253
6.50	31	145	198	237	273	359	132	2,358	0	0	0	0	0	0	0	0	0	0	3,733
7.00	24	149	211	254	260	362	148	477	2,889	0	0	0	0	0	0	0	0	0	4,774
7.50	26	111	199	198	233	330	71	475	397	3,083	0	0	0	0	0	0	0	0	5,123
8.00	9	54	84	95	101	124	41	165	132	181	1,431	0	0	0	0	0	0	0	2,417
8.50	10	63	98	107	110	156	41	211	224	267	128	2,080	0	0	0	0	0	0	3,495
9.00	19	55	98	87	113	147	27	225	176	217	124	233	2,140	0	0	0	0	0	3,661
9.50	10	44	77	91	98	142	32	213	161	215	104	252	188	2,282	0	0	0	0	3,909
10.00	5	37	85	91	103	112	33	183	141	199	100	219	186	201	2,328	0	0	0	4,023
10.50	10	28	62	41	57	70	26	129	87	124	55	140	125	104	123	1,584	0	0	2,765
11.00	15	42	61	81	99	102	29	150	121	177	90	196	177	189	170	138	2,495	0	4,332
11.50	10	21	46	31	50	68	24	117	81	102	61	120	129	93	111	83	106	1,659	2,912
Total	2,909	3,844	4,635	3,344	3,417	4,385	1,378	4,703	4,409	4,565	2,093	3,240	2,945	2,869	2,732	1,805	2,601	1,659	57,533

Interest rates rounded down to nearest 50 basis points.