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

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July 2007

*Contact information: dean.karlan@yale.edu, jzinman@dartmouth.edu. We are grateful to the National Science Foundation (SES-0424067, CAREER SES-0547898), and BASIS/USAID (CRSP) for funding research expenses and the Lender for financing the loans. We thank four anonymous referees and the editor, Costas Meghir, for extremely helpful comments. Thanks also to Saku Aura, Abhijit Banerjee, Emek Basker, Gharad Bryan, Michael Carter, Pierre-Andre Chiappori, Habiba Djebbari, Esther Duflo, Amy Finkelstein, Douglas Gale, Jeffrey Kling, Stefan Klonner, Andreas Lehnert, Alessandro Lizzeri, Ted Miguel, Jonathan Morduch, Rohini Pande, Michel Robe, numerous seminar and conference participants, and in particular Chris Udry. Jeff Arnold, Jonathan Bauchet, Lindsay Dratch, Tomoko Harigaya, Kurt Johnson, Karen Lyons, and Marc Martos-Vila provided excellent research assistance. We thank the management and staff of the cooperating Lender for implementing the experimental protocols. Much of this work was completed while Zinman was at the Federal Reserve Bank of New York (FRBNY). We thank FRBNY, and in particular Jamie McAndrews, Jamie Stewart, and Joe Tracy, for supporting this project. The views expressed herein are those of the authors and do not necessarily reflect those of the FRBNY, the Federal Reserve System, the National Science Foundation, or USAID.

Abstract

Information asymmetries are important in theory but difficult to identify in practice. We estimate the presence and importance of adverse selection and moral hazard in a consumer credit market using a new field experiment methodology. We randomized 58,000 direct mail offers issued by a major South African lender along three dimensions: 1) an initial "offer interest rate" featured on a direct mail solicitation; 2) a "contract interest rate" that was revealed only after a borrower agreed to the initial offer rate; and 3) a dynamic repayment incentive that extended preferential pricing on future loans to borrowers who remained in good standing. These three randomizations, combined with complete knowledge of the Lender's information set, permit identification of specific types of private information problems. Our setup distinguishes adverse selection from moral hazard effects on repayment, and thereby generates unique evidence on the existence and magnitudes of specific credit market frictions. We find evidence of moral hazard and weaker evidence for adverse selection. A rough calibration suggests that perhaps 7% to 16% of default is due to asymmetric information problems. Asymmetric information may help explain the prevalence of credit constraints even in a market that specializes in financing high-risk borrowers at very high rates.

1 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 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

[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 (2006) 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.

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 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 financial institution specializing in high-interest, unsecured, fixed-repayment-schedule lending to poor workers. The experiment identifies information asymmetries by randomizing loan pricing along three dimensions: first on the interest rate offered on a direct mail solicitation, second on the actual interest rate on the loan contract, and third on the interest rate offered on future loans.

A stylized example, illustrated in Figure 1, 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.

[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 Armendariz de Aghion and Morduch (2005) for a discussion of this literature.

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) 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 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.

Thus our design creates two experiments: a selection experiment on all individuals who received an offer, and a moral hazard and repayment burden experiment on those who agree to borrow. In both cases these are 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 substantively to Edelberg (2004), and methodologically 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 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 (2006) 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.

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 2. Section 3 lays out the experimental design and implementation. Section 4 provides an informal discussion of how theories of asymmetric information motivate and shape our experimental design, and then a formal model of adverse selection and moral hazard, as well as a mapping of our experimental design to the theoretical model. Section 5 presents the empirical results. Section 6 concludes with some practical and methodological implications.

2 Market and Lender Overview

Our cooperating Lender operated for over 20 years as one of the largest, most profitable micro-lenders in South Africa. 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.

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 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. 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 de-centralized 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.

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 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.

3 Experimental Design and Implementation

3.1 Experimental Design

The experiment was conducted in three waves: July, September and October 2003. In each wave, the Lender sent direct mail solicitations with pre-qualified, limited-time offers to former clients with good repayment histories. We randomly assigned each of the 57.533 clients an "offer rate" (r^o) included in the direct mail solicitation with deadlines ranging from 2 to 6 weeks. The Lender routinely contacted former borrowers via mail but had never promoted specific interest rate offers before this experiment.

The offer interest rate was assigned conditional on the borrower's observable risk category set by the Lender, and bounded above by the Lender's normal rate for each individual's risk category: 11.75 percent, 9.75 percent and 7.75 percent for the high, medium, and low risk categories, respectively. The lower bound for all individuals was the "upmarket" competitor rate of 3.25 percent per month.

5,028 clients applied for a loan under this experiment (a takeup rate of 8.7%). Clients applied 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. The loan application process took at most one hour, typically less. 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.[4] Of the 5,028 applicants, 4,348 (86.5%) were approved by the Lender.

Next, after loan size and maturity were agreed upon, 41% of the sample was chosen randomly and unconditionally to receive a contract interest rate, r^c, lower than the offer interest rate, r^o. This was done by software developed for the purpose of this experiment. The presence and value of the contract interest rate was revealed only after the borrower came into the branch and agreed to borrow at 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 following the revelation of r^c. In theory, endogenizing the loan size 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 loan size 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. For now, we note that allowing loan size to change following the revelation of r^c would push against finding repayment burden effects. We postpone further discussion of this issue until Section 5.6.

Last, 47% of clients were randomly assigned and informed of a dynamic incentive (B) in which clients received the same low contract interest rate on all future loans for one year as long as they remained in good standing with the Lender.[5] The average discount embodied in r^c, and hence B, was substantial: an average of 350 basis points

[4]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 hazard with respect to the interest rate). For this reasons, the maximum allowable loan size was calculated based on the normal, not experimental, interest rates.

[5]For operational reasons, the dynamic repayment incentive was randomized at the branch level during the first and second wave of the experiment, and at the individual level for the third wave.

off the monthly rate. 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. Clients not receiving the dynamic incentive obtained r^c for just the first loan (which had only a 4-month maturity in 80% of the cases). Clients were informed of B 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.

3.2 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. 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.

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 5.8.

3.3 Integrity of the Experimental Design

First, we verify the orthogonality of various demographic variables to the randomized variables. Table 2, Columns 1-3 show 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.

Second, the experimental design, to interpret it as we do, requires that the revelation of r^c and B were indeed a surprise (IA-1 in the theoretical model and identification section). 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 borrower application decisions were indeed "blind" to the contract rate r^c by showing that r^c is uncorrelated with the application decision. This is reassuring because the prospective client should not have known anything about r^c when deciding whether to apply. Table 2, Column 5 shows that the Lender's credit decision was indeed uncorrelated with the surprise rates; i.e., the probability that an application is rejected does not vary significantly with either r^c or B. This corroborates that loan officers could not access the surprise rates in making their credit supply decisions.

Furthermore, 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 B.

3.4 Default Outcomes

We tracked repayment behavior using the Lender's administrative data.

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, monitoring, and collections. 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.

We also create summary index tests that aggregate across these three measures of default in order to address the problem of multiple inference, following Kling, Liebman and Katz (2007).

4 The Theoretical Model and Identification Strategy

4.1 Theoretical Overview

Most models of adverse selection and moral hazard share a common prediction: 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 produce more defaults, even after one conditions 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 2006). 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 (de Meza and Webb, 1987; 2001). 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 (Bisin and Guaitoli 2004; Parlour and Rajan 2001), in which individuals borrowing from multiple sources choose, e.g., to pay down the highest interest rate obligation first.

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 clear for small, closely-held businesses. Money is fungible. Empirical evidence from Bangladesh microfinance finds, for example, that consumption smoothing is a key factor in demand for credit by entrepreneurs (Menon 2003). More generally, asymmetric information problems as applied to risky "projects" have natural and close analogs for consumption loan borrowers.

For *hidden type* models, for example, consumers may know their overall "type", in the sense that they know the likelihood of having sufficient cash to repay their loan. Lenders do not know this, just as they do not know which "project" an entrepreneur's investment is. Hence 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 if the untargeted loan will be used for entrepreneurial activity, 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 Jaffee and Russell (1976).

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 2001). 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 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.

4.2 Model Overview

To organize ideas we provide a model which clarifies the meaning of adverse selection and moral hazard in this context, and then discuss how the experimental design allows us to test separately for the presence of adverse selection and moral hazard. Our goal is not to put forward new theory which incorporates both adverse selection and moral hazard and discusses their interplay (e.g., see Chassagnon and Chiappori, 1997), but rather to detail precisely what is meant by each in this context. Models with similar features can be found in many sources; for example, Bardhan and Udry (1999).

We discuss seven effects that interest rates may produce on borrow behavior under asymmetric information:

Effect 1: Individuals that have a higher level of unobservable risk are more likely to take out loans at higher offer rates and less likely to repay those loans ("adverse selection").

Effect 2: Individuals that have a higher level of unobservable risk put less effort into ensuring the success of their project ("adverse selection").

Effect 3: A given fixed set of borrowers exert less effort at higher contract interest rates than at lower contract interest rates ("*ex-ante* moral hazard").

Effect 4: A given fixed set of borrowers is more likely to default voluntarily at higher contract interest rates than at lower contract interest rates ("*ex-post* moral hazard").

Effect 5: A given fixed set of borrowers, at a fixed level of effort, is less likely to have sufficient funds to repay debt at higher interest rates than at lower interest rates ("income effect").

Effect 6: A given fixed set of borrowers exerts less effort as the cost of default decreases, holding constant the contract interest rate ("*ex-ante* moral hazard").

Effect 7: A given fixed set of borrowers is more likely to default voluntarily as the cost of default decreases, holding constant the contract interest rate ("*ex-post* moral hazard").

We consider effects 1 and 2 to be "Adverse Selection." Effect 1 is traditionally thought of as adverse selection in a credit market, motivated by studies such as Stiglitz and Weiss (1981). Effect 2 requires more discussion. An immediate reaction is that because the word "effort" is used Effect 2 should fall under the rubric of moral hazard. However, the selection process is a necessary step in order to generate Effect 2: it is produced by the effect of the offer rate on the composition of types that agree to borrow. It is also true that if effort were contractible, then the effect would not occur, and the only selection effect would be Effect 1.

We cannot test these seven effects individually. The experimental design will estimate Effects 1 and 2 together (labeled "adverse selection" and identified via the offer interest rate), Effects 3, 4 and 5 together (labeled "repayment burden" and identified via the contract interest rate), and Effects 6 and 7 together (labeled "moral hazard" and identified via the future contract interest rate conditional on successful repayment of the current loan).

4.3 The Model

Our model incorporates both adverse selection and moral hazard with respect to effort and operates under three standard assumptions. We also make two empirical identification assumptions (IA-1 and IA-2) that are required for the experimental design to map to the theoretical model and separate adverse selection from moral hazard.

Each individual has an opportunity to invest in a project but requires financing of 1 to do so. Let r^c be the interest due on the loan contract; although normally endogenous, our experimental design assigns r^c exogenously. As discussed earlier, we refer to "project" here in a broad sense that includes household as well as entrepreneurial activities. If the project succeeds, it returns Y, and if it fails it returns 0. The probability of success is a function of the project risk type, θ_i, and the effort put forth by the borrower, e. Both risk type and effort are observable to the borrower but unobservable to the Lender. So the probability of success is denoted by $\pi(\theta_i, e)$, and the probability of failure is $1 - \pi(\theta_i, e)$. We denote the state in which the product is successful g, and the state in which it is not successful b.

We make the following standard assumptions regarding project returns:

Assumption 1: $Y(\theta_i) > 1 + r^c$ for all $\theta_i \in [\theta_L, \theta_U]$, if the project succeeds, the loan can be repaid.

Assumption 2: $\frac{\partial \pi(\theta_i, e)}{\partial e} > 0$ and $\frac{\partial \pi(\theta_i, e)}{\partial \theta_i} < 0$, higher effort ($e$) and lower risk type ($\theta_i$) increase the likelihood of the project succeeding (π).

Assumption 3: $\pi(\theta_i, e)Y(\theta_i) = \overline{Y}(e)$ for all $\theta_i \in [\theta_L, \theta_U]$, all types, θ_i, have the same expected project return.

Assumption 3 implies that projects with a higher θ_i are "riskier" in terms of second order stochastic dominance. This follows Stiglitz and Weiss (1981) and will allow us to sign the direction of the combined selection effects (Effect 1 and Effect 2) described above.

The steps of the model follow the experimental design:

1. The Lender offers individuals the opportunity to borrow at randomly different interest rates (the offer interest rate, r^o).

2. Individuals decide whether to borrow or not at the offer interest rate. Individuals know both the riskiness of their project, θ_i, and the effort, e, they intend to put forth.

3. The Lender then randomly lowers the interest rate for some of the borrowers from the offer interest rate, r^o, to the contract interest rate, r^c. The Lender also randomizes r^f, the interest rate on future loans conditional on repaying this loan successfully. r^f is set to either r^c or to a "normal" non-experimental interest rate which is higher than r^c for 98% of clients, and equal to r^c for the other 2%.

4. The borrower then decides how much effort, $e \in [\underline{e}, \bar{e}]$, to put forth. We assume that effort is costly and causes a disutility equal to the amount of effort. The cost of defaulting, denoted C_i, is specific to the state ($i = g, b$) and is a function of both lost benefits from defaulting (such as access to the future interest rate, r^f, offered to successful borrowers by this Lender, or reduced access to loans from other lenders due to a bad credit bureau record), and explicit costs of defaulting (such as legal expenses, stress, and stigma). Limited liability implies that $C_b(r^f) < C_g(r^f)$, but we explore the implications of relaxing this assumption below.

5. Lastly, the state of the world is realized and the project either succeeds or fails. We innocuously simplify the exposition by assuming that if the project succeeds, there is no voluntary default. Thus the model does not predict Effects 4 and 7. Empirically these effects may be present. However, the randomizations of the contract interest rate r^c and the dynamic incentive $C_i(r^f)$ do not allow us to distinguish between *ex-ante* moral hazard effects and the *ex-post* moral hazard effects (i.e., Effects 3 and 4 are empirically indistinguishable, as are Effects 6 and 7). Thus the implications of this experiment are on moral hazard generally, not specifically on *ex-ante* moral hazard or *ex-post* moral hazard.

Since the Lender's decisions (r^o, r^c, r^f) are set exogenously by the experiment, we can focus on the borrower's optimization problem. We break the problem into three stages. In stage 1, the borrower decides whether to take out a loan at a repayment amount of $1 + r^o$. In stage 2, the borrower decides on how much effort e to exert, after being "surprised" by a separate contract interest rate r^c and future interest rate r^f. After stage 2 and before stage 3, the state of nature is revealed. In stage 3, the borrower repays the loan if there are sufficient funds to do so (i.e., we assume no voluntary default). We consider the decision problem backwards.

Stage 3 In stage 3, a borrower with risk type θ_i and effort level e repays the loan iff:

$$Y(\theta_i) \geq 1 + r^c, \tag{1}$$

the project succeeds and by assumption 1 yields sufficient funds to repay the loan, and

$$C_g(r^f) \geq 1 + r^c, \tag{2}$$

the benefits of repaying are higher than the cost of repaying. We assume throughout that equation (2) holds and consequently everyone pays if they are able.

Stage 2 For any r^c such that (2) holds, the borrower chooses effort to solve:

$$\max_{e\in[\underline{e},\bar{e}]} \pi(\theta_i, e)((Y(\theta_i) - 1 - r^c + C_b(r^f)) - e - C_b(r^f) \tag{3}$$

Assuming that π is concave in effort we have the usual comparative static that effort is decreasing in r^c (Effect 3) and is increasing in C_b (Effect 6).

We can also consider whether effort will depend on risk type, θ_i. The first order condition is:

$$\frac{\partial\pi(\theta_i, e)}{\partial e}(Y(\theta_i) - 1 - r^c + C_b(r^f)) = 1 \tag{4}$$

Making use of assumption 3, we then implicitly define optimal effort as $\hat{e}$, which is a function of r^c, $C_b(r^f)$, and θ_i.

$$\frac{(1 - \bar{Y}'(\hat{e}))Y(\theta_i)}{\bar{Y}'(\hat{e})} = C_b(r^f) - 1 - r^c. \tag{5}$$

Note that equation (5) implies that $\hat{e}(r^c, C_b(r^f), \theta_i)$, optimal effort at the contract interest rate, must be a decreasing function of θ_i. This is Effect 2 listed above: high-risk types are not only riskier (Effect 1), but also put in less effort implicitly as a consequence of the lower probability of success. Note, however, that the sign of this effect is driven by assumption 3 (that risk is a mean preserving spread).

Stage 1 An individual decides to take up the offer if the expected return from her project, given expected optimized effort at the offer interest rate, $\hat{e}(r^o, C_b(r^f), \theta_i)$, is greater than her next-best option (set to zero for simplicity). That is, the individual borrows from the Lender if and only if

$$\pi(\theta_i, \hat{e}(r^o, C_b(r^f), \theta_i))(Y(\theta_i) - 1 - r^o + C_b(r^f)) - \hat{e}(r^o, C_b(r^f), \theta_i) - C_b(r^f) \geq 0 \tag{6}$$

where $\hat{e}(r^o, C_b(r^f), \theta_i)$ is the optimal level of effort for an individual with project type θ_i that borrows and expects to pay the offer interest rate, r^o.

If we assume that $C_b(r^f) < 1 + r^o$ then the left-hand side of (6) is increasing in riskiness, θ_i. To see this, note that the envelope theorem implies that the increase in

θ_i has no indirect effect through effort. Assumption 3 then implies that the only effect of increasing θ_i comes through the term $\pi(\theta_i)$, which has a negative first derivative by assumption 2. Consequently, for a given r^o, either all borrowers will take out a loan, or there will be a separation with those with a higher θ_i taking a loan. We define the implicit function $\underline{\theta}(r^o)$ as the θ_i below which individuals, offered interest rate r^o, do not borrow, i.e. the θ_i at which equation (6) equals zero. The implicit function theorem implies that:

$$\frac{d\underline{\theta}(r^o)}{dr^o} > 0. \tag{7}$$

This will produce Effect 1 listed above.

If $C_b(r) > 1 + r^o$, which is implied by $C_b(r)C_g(r)$, we would get the opposite result. That is, increasing the interest rate would lead to less risk in the borrower pool - advantageous selection. The classic adverse selection result relies heavily on the asymmetry of borrower default costs across states. While the empirical prevalence of limited liability gives the asymmetry assumption some appeal, there may be cases in which it does not hold. Our empirical results, will shed light on the plausibility of the asymmetry assumption. Finally it is worth noting how this affects our identification. Under assumption 3 we have established that selection and effort tend to move default in the same direction. That is, if $C_b(r^f) < 1+r^o$ increasing r^o leads to a riskier pool of clients that also exert less effort, while the opposite is true if $C_b(r^f) > 1+r^o$, therefore, under the assumptions of the model we are always able too sign the direction of the selection effect and therefore can say whether we observe adverse or advantageous selection.

4.4 Relationship to Experimental Design

Now we relate the above model directly to our experimental design.

4.4.1 Adverse Selection (offer interest rate)

Adverse selection comes from the pooling effect(s) the Lender encounters when the interest rate offered influences the average θ_i of those who agree to borrow. Stage 1 of the model relies on the offer interest rate (r^0), not the contract interest rate (r^c), to generate the composition effects. Econometrically, this implies that we need an identification assumption specific to the experimental design:

IA-1: The borrower decides whether to borrow, and the Lender decides whether to lend, before r^c and r^f are revealed to either, and after r^o is revealed to both. Furthermore, the borrower does not anticipate that there might be an r^c that is lower than r^o.

Key evidence that this held in our experimental design is that zero individuals applied for a loan at r^o and then chose not to borrow after learning r^c. This assumption is also defended empirically in Table 2, Column 4; r^c does not predict take-up, whereas r^o does and in Table 2, Column 6 which shows that r^c does not predict rejection by the Lender. Thus, as implied by IA-1, $\underline{\theta}$ is a function of r^o alone, and not r^c.

Next we consider the derivative of expected default with respect to r^o, and irrespective of r^c (by IA-1), in the presence of adverse selection:

$$\frac{d}{dr^o}\left[\frac{1}{1-G(\underline{\theta}(r^o))}\int_{\underline{\theta}(r^o)}^{\infty} 1-\pi(\theta_i, \widehat{e}(r^c, C_b(r^f), \theta_i))g(\theta_i)d\theta_i\right] > 0, \tag{8}$$

where $g(\cdot)$ is the density of θ_i in the overall population, and $G(\cdot)$ is the cumulative distribution of $g(\cdot)$. We know that equation (8) is positive because of equation (7), and because the marginal θ_i ($= \underline{\theta}(r^o)$) has a lower probability of default than all other θ who borrow:

$$1-\pi(\underline{\theta}, e(r^c, C_b(r^f), \underline{\theta})) < 1-\pi(\theta_i, e(r^c, C_b(r^f), \theta_i)) \qquad \forall \theta_i > \underline{\theta}. \tag{9}$$

Equation (9) makes vivid the two adverse selection effects listed at the beginning of this section. First, since higher r^o implies a pool of individuals with higher average θ_i, the average project is more risky, and thus, holding effort fixed, default increases (Effect 1). This comes through the first arguments of the π functions in (9). Second, again due to the increase in the average θ_i that borrows as r^o increases, equation (5) implies that the average effort put forth of those who borrow decreases (Effect 2). This comes through the second arguments, $e(\cdot)$, in the π functions in (9).

Econometrically, we estimate β_o from the following specification:

$$1-\pi_i = \alpha + \beta_o r_o + \beta_c r_c + \beta_b C + X_i + \epsilon_i, \tag{10}$$

where X_i is a set of control variables for conditions of the randomization: the month of the solicitation (one of three months) and the lender-defined risk category based on observable characteristics (one of three categories). By controlling for r_c and C,[6] (10) estimates the sign of (8), holding effort constant except as effort changes due to the change in composition of $\theta_i' s$ through Effect 2 noted above.

4.4.2 Repayment Burden (contract interest rate)

The model also helps to interpret the relationship between default and the contract rate. Consider two individuals who have the same offer interest rate, r^0. Based on the model and IA-1, these individuals have the same expected θ_i. Then, one individual receives a lower contract rate, r^c, than the other individual.

A higher contract rate may increase default through three effects (Effects 3-5 listed above). Effect 3 results from equation (3): higher r^c reduces effort, and thereby the reduces the probability of success and loan repayment. Effects 4 (voluntary default) and 5 (income effect) are assumed away by our model, since in stage 3 all individuals repay their loans if able to do so, and since by assumption 1 successful projects always yield sufficient returns to repay. We emphasize this because we do not wish to overstate the theoretical interpretation of the effect of the contract rate on default under our design.

We refer to the sum of these three effects as "repayment burden," defined algebraically as:

$$\frac{d}{dr^c}\left[\frac{1}{1-G(\underline{\theta}(r^o))}\int_{\underline{\theta}(r^o)}^{\infty} 1-\pi(\theta_i,\widehat{e}(r^c,C_b(r^f),\theta_i))g(\theta_i)d\theta_i\right] > 0 \qquad (11)$$

because $\frac{d\pi}{de}\frac{de}{dr^c} < 0$. This is parallel to equation (8). IA-1 shows that we can hold $\underline{\theta}$ constant by controlling for r^o. So econometrically, we return to equation (10) and now β_c estimates the repayment burden effect.

4.4.3 Moral Hazard (dynamic repayment incentive)

Finally, we consider the effect of an increase in C on default (recall that in the experiment, $C$is increased after the loan contract is agreed upon by randomly informing some individuals that the lower interest rate will apply on future loans conditional on

[6]Note that C is not directly randomized, but rather r^f is randomly assigned to be either low ($= r^c$) or high (= the normal non-experimental rate). C is a function of r^f as well as other uncontrolled factors that influence the benefit of retaining good status with the Lender. Also, controlling for r^c semi-parametrically– rather than linearly as we show here– produces qualitatively similar results.

successful repayment of their current loan). We see in equation (3) that an increase in B will lead to an increase in effort. Also note that, while outside the model, an increase in C will dissuade some borrowers from defaulting voluntarily. So an increase in C may reduce default by reducing moral hazard (Effects 4 and 7).

To empirically identify C's effect on default we need a second identification assumption specific to the experimental design:

IA-2: $V(r^f) < 1 + r^c$,

where $V(r^f)$ is the market value of the option to borrow at r^f, the future contract interest rate. This identification assumption requires that the borrower must not be able to repay the loan even if the project fails by selling $V(r^f)$ for $1 + r^c$. This is a realistic assumption given the lack of a market for options to borrow from the Lender. Also note that the future contract interest rate does not influence the cash required to pay the current loan. Thus if there is moral hazard with respect to the future contract interest rate we will find:

$$\frac{d}{dC}\left[\frac{1}{1 - G(\underline{\theta}(r^o))}\int_{\underline{\theta}(r^o)}^{\infty} 1 - \pi(\theta_i, e(r^c, C, \theta_i))g(\theta_i)d\theta_i\right] < 0 \tag{12}$$

because $\frac{d\pi}{de}\frac{de}{dC} > 0$. Assumption IA-1 allows us to estimate the comparative static of B on π and assume $\underline{\theta}$ to be constant, and IA-2 allows us to assume that there are no wealth effects from C. Econometrically, moral hazard is then identified by the coefficient β_b from equation (10).

5 Empirical Results

5.1 Comparison of Means: Table 3

First we present the simplistic analysis that returns to the framework described in the introduction and 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 3.4.

Net selection on unobservables is estimated on the sub-sample of borrowers receiving low contract rates by calculating 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 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 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 below.

5.2 Econometric Specification: Table 4

Table 4 presents estimates from the empirical model derived and detailed in Section 4.4. In each case we estimate equation (10) on the entire sample of 4,348 individuals who obtained a loan under this experiment. Each specification includes the Lender's summary measure of observable risk (since the randomizations conditioned only on this variable) and indicator variables 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 allows for clustering at the branch level. The specifications vary only in how they measure default and whether the dynamic incentive is identified as a binary variable or binary and continuous variable. Columns 1-6 estimate the effects of the randomly assigned interest rates on default using individual default measures. Columns 7 and 8 use a method for reducing the number of empirical tests when there are multiple outcome measures: aggregate the measures by standardizing them into a summary index (Kling, Liebman and Katz, 2007). The results are interpreted as the average effect of the interest rate on default, in standard deviation units. Column 9 uses seemingly unrelated regression (SUR) to test the joint null hypothesis that a given interest rate coefficient is zero for all three default measures.

Row 1 of Table 4 presents estimates of β_o, the effect of the offer rate on default. This coefficient identifies any net selection on unobservables, with $\beta_o > 0$ indicating adverse selection. The point estimate is indeed always positive, and the implied magnitudes are economically substantial; e.g., the β_o of 0.007 in Column 5 translates into a 6% increase in default for a 100 basis point increase in the offer rate. But we find no statistically significant evidence of adverse selection in any of the individual outcome or summary index specifications (Columns 1-8). The SUR model does indicate a significant effect however, with a p-value of 0.015 (Column 9).

Row 2 of Table 4 presents estimates of β_c, the effect of the contract rate on default. This coefficient identifies any effect of repayment burden, with $\beta_c > 0$ indicating some combination of moral hazard and income effects. All but one of the estimates in Columns 1-8 imply economically small effects that are not significantly different from zero. The one marginally significant result (Column 3) implies that a 100 basis point cut would reduce the average number of months in arrears by 3%. SUR finds a marginally significant effect, with a p-value of 0.083 (Column 9).

Row 3 of Table 4 presents estimates of β_b,the effect of the dynamic repayment incentive on default. Nearly every specification points to economically and statistically significant moral hazard. Columns 1, 3, and 5 imply that clients assigned the dynamic incentive defaulted an estimated 7 to 16 percent less than the mean. The summary index test also finds a large and significant effect. Columns 2, 4 & 6 show that $B's$ 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 about 4% in the full sample. The last row of the table shows that B and the size of the discount are jointly significant in all specifications, including the summary index test (Column 8). The SUR p-values shown in Column 9 are close to significant for the binary specification (Columns 1, 3 & 5) and significant for the binary and continuous specification (Columns 2, 4 & 6).

5.3 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 more robust evidence for moral hazard than for adverse selection or 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 how much is due to the observable information used by the Lender to classify them as high-risk and low-risk). Table 1a shows that the average high-risk borrower obtained a 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)*0.015)/0.09 = 10%. 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 B 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%.

5.4 Interpretation: Heterogeneity and Mechanisms

Tables 3 and 4 show fairly robust evidence of moral hazard effects, but weaker evidence of repayment burden and adverse selection effects. 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.

5.5 Interpreting the Offer Rate Results

5.5.1 Offsetting Selection Effects?

Heterogeneity in unobservable selection could obscure the presence of selection on 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.

5.5.2 Gender Differences

Our exploration of heterogeneity in selection effects did reveal one notable pattern: 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 target women. Of course, the pattern may be due to some omitted variable rather than gender *per se*. An imperfect test of this potential 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 on the interactions of these demographic variables with the randomly assigned interest rates. Table 6 presents estimates from specifications of this approach for each demographic variable (Columns 1-6) as well as with all demographic variables at once (Column 7). The row of interest is “Female*Offer Rate.” The results on this variable are consistent with the interpretation that our results are in fact driven by gender *per se*, and not by any *observable* demographics that are correlated with gender. However, we can not rule out that some omitted variable is driving the results, and we cannot speak to the root cause of this gender effect.

5.5.3 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 default 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 an interest rate to equation (10). 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.

5.6 Interpreting the Contract Rate Results

Interpreting the contract rate result may be complicated by two factors. First, since the repayment burden effect is the combination of income and moral hazard effects, as discussed above, a null effect could be a result of offsetting effects, rather than the absence of both. Second, the experimental implementation did 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 discuss these two issues in turn.

If the contract interest rate generates a moral hazard effect, it should reinforce any income effect and produce a 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, through the nonexclusive contracting channel, and hence offsets the income effect. These offsetting effects— 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. The discounted future interest rate is large on average (350 basis points), and obtained with high probability. We have no way of distinguishing empirically between these interpretations of the contract interest rate coefficient.

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 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, 2007), 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 (not shown) do not change. 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.

5.7 Interpreting the Dynamic Repayment Incentive Results

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

5.8 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 (10). 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.

6 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-risk South African consumer loan market. The results indicate significant moral hazard, with weaker evidence for adverse selection. The study has both methodological and practical motivations.

Practically, 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 development community to try to expand access to credit to fight poverty and promote growth. Billions of dollars of subsidies and investments 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 address the questions 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 direction 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 targeting 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, and a growing number of projects point to the general feasibility of researchers 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. 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.

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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

Dependent variable:	Contract Rate (1)	Offer Rate (2)	Rate Valid for One Year (versus One Loan) (3)	Applied=1 (4)	Sample Restricted to Applied = 1: 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*		*Standardized Index of Three Default Measures*		*SUR: p-value*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Offer Rate (Selection)	0.004	0.004	0.002	0.002	0.007	0.007	0.017	0.016	0.015** (Columns 1, 3 & 5)
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.013)	(0.013)	
Contract Rate (Repayment Burden)	0.000	-0.002	0.007*	0.003	0.001	-0.001	0.009	-0.001	0.083* (Columns 1, 3 & 5)
	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)	(0.012)	(0.014)	
Dynamic Repayment Incentive Dummy (Moral Hazard)	-0.011*	0.003	-0.016**	0.013	-0.019**	0.000	-0.058**	0.022	0.132 (Columns 1, 3 & 5)
	(0.005)	(0.011)	(0.008)	(0.018)	(0.009)	(0.019)	(0.025)	(0.053)	0.078* (Columns 2, 4 & 6)
Dynamic Repayment Incentive Size (Moral Hazard)		-0.004		-0.008**		-0.005		-0.022*	
		(0.003)		(0.004)		(0.004)		(0.013)	
Constant	0.079***	0.094***	0.139***	0.171***	0.069***	0.090***	-0.119*	0.420***	
	(0.014)	(0.019)	(0.025)	(0.027)	(0.024)	(0.028)	(0.071)	(0.138)	
Observations	4348	4348	4348	4348	4348	4348	4348	4348	
Adjusted R-squared	0.04	0.04	0.11	0.11	0.03	0.03	0.07	0.07	
Mean of dependent variable	0.09	0.09	0.22	0.22	0.12	0.12	0.06	0.06	
Prob(both Dynamic Incentive variables = 0)		0.08*		0.01***		0.05**		0.02**	

* significant at 10%; ** significant at 5%; *** significant at 1%. Each column presents results from a single model estimated using the base OLS specification. 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. For Columns (7) and (8), we created an index of the three measures by calculating the mean of the standardized value (relative to the low offer and contract interest rate group, standardized at mean zero, standard deviation one) of each of the three measures of default.

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*	*Standardized Index of Three Default Measures*	*SUR: p-value Cols 1,2&3=0*	*Monthly Average Proportion Past Due*	*Proportion of Months in Arrears*	*Account in Collection Status*	*Standardized Index of Three Default Measures*	*SUR: p-value Cols 4,5&6=0*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Offer Rate	-0.002	-0.004	0.001	-0.007	0.621	0.010***	0.008*	0.013**	0.040**	0.038
	(0.004)	(0.005)	(0.007)	(0.018)		(0.003)	(0.005)	(0.005)	(0.016)	
Contract Rate	0.005	0.014***	0.010	0.036**	0.044	-0.005	-0.001	-0.009	-0.020	0.171
	(0.003)	(0.005)	(0.007)	(0.017)		(0.004)	(0.005)	(0.006)	(0.017)	
Dynamic Repayment	-0.014	-0.025**	-0.020	-0.076*	0.191	-0.007	-0.006	-0.017	-0.039	0.497
Incentive Indicator	(0.009)	(0.012)	(0.015)	(0.040)		(0.008)	(0.012)	(0.012)	(0.036)	
Constant	0.108***	0.178***	0.092**	0.002		0.050***	0.097***	0.043	-0.246	
	(0.025)	(0.040)	(0.043)	(0.127)		(0.015)	(0.026)	(0.027)	(0.073)	
Observations	2215	2215	2215	2215		2133	2133	2133	2133	
R-squared	0.05	0.12	0.04	0.07		0.05	0.10	0.04	0.07	

* 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. For Columns (4) and (9), we created an index of the three measures by calculating the mean of the standardized value (relative to the low offer and contract interest rate group, standardized at mean zero, standard deviation one) of each of the three measures of default.

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

OLS

Dependent Variable: Monthly Average Percentage Past Due

Demographic Control Variable(s):	*Married*	*Number of Dependents in Household*	*Educated*	*Age*	*Log of Monthly Gross Income*	*Tenure at Employment*	*All*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Experimental Variables							
Offer Rate	0.023	0.089	0.079	0.282	2.700	0.122	2.404
	(0.435)	(0.432)	(0.402)	(1.162)	(2.338)	(0.456)	(3.274)
Contract Rate	0.415	0.482	0.260	0.269	-0.968	0.404	0.613
	(0.393)	(0.446)	(0.414)	(1.098)	(2.707)	(0.465)	(4.110)
Dynamic Repayment Incentive Indicator	-1.158	-1.098	-0.878	-1.280	7.378	-1.165	4.842
	(1.160)	(1.237)	(1.028)	(2.678)	(8.692)	(1.145)	(12.209)
Female	-2.985	-2.558	-2.215	-1.887	-2.821	-2.667	-1.375
	(1.939)	(1.980)	(1.886)	(1.914)	(1.926)	(1.875)	(1.984)
Demographic Variable (see column heading)	-1.838	-0.036	-1.761	-0.172	-0.001	-0.015	all
	(1.952)	(0.536)	(2.432)	(0.105)	(1.669)	(0.012)	
*Female * Experimental Variables*							
Female * Offer Rate	0.887*	0.834*	0.902*	0.763*	0.890**	0.807*	0.834*
	(0.456)	(0.460)	(0.480)	(0.455)	(0.445)	(0.447)	(0.489)
Female * Contract Rate	-1.042**	-1.029**	-1.138**	-0.977**	-1.040**	-0.967**	-1.182**
	(0.476)	(0.497)	(0.482)	(0.486)	(0.474)	(0.479)	(0.493)
Female * Dynamic Repayment Incentive	0.813	0.896	1.077	0.701	0.603	0.730	0.914
	(1.350)	(1.343)	(1.351)	(1.336)	(1.353)	(1.328)	(1.424)
*Demographic Control Variable * Experimental Variables*							
Demographic Variable * Offer Rate	-0.135	-0.046	-0.400	-0.008	-0.343	-0.002	all
	(0.540)	(0.122)	(0.625)	(0.026)	(0.289)	(0.003)	
Demographic Variable * Contract Rate	0.195	-0.009	0.748	0.006	0.183	0.001	all
	(0.511)	(0.141)	(0.583)	(0.026)	(0.325)	(0.003)	
Demographic Variable * Dynamic Repayment Incentive	-0.577	-0.224	-1.577	-0.002	-1.077	-0.002	all
	(1.211)	(0.353)	(1.307)	(0.061)	(1.042)	(0.006)	
Constant	10.161***	8.917***	9.608***	14.984***	9.240	10.281***	11.328
	(2.476)	(2.542)	(2.240)	(5.136)	(13.856)	(2.642)	(15.060)
Observations	4317	4317	4348	4348	4348	4348	4317
R-squared	0.05	0.05	0.05	0.06	0.05	0.06	0.07

* 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. "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

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* (1)	(2)	*Proportion of Months in Arrears* (3)	(4)	*Account in Collection Status* (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

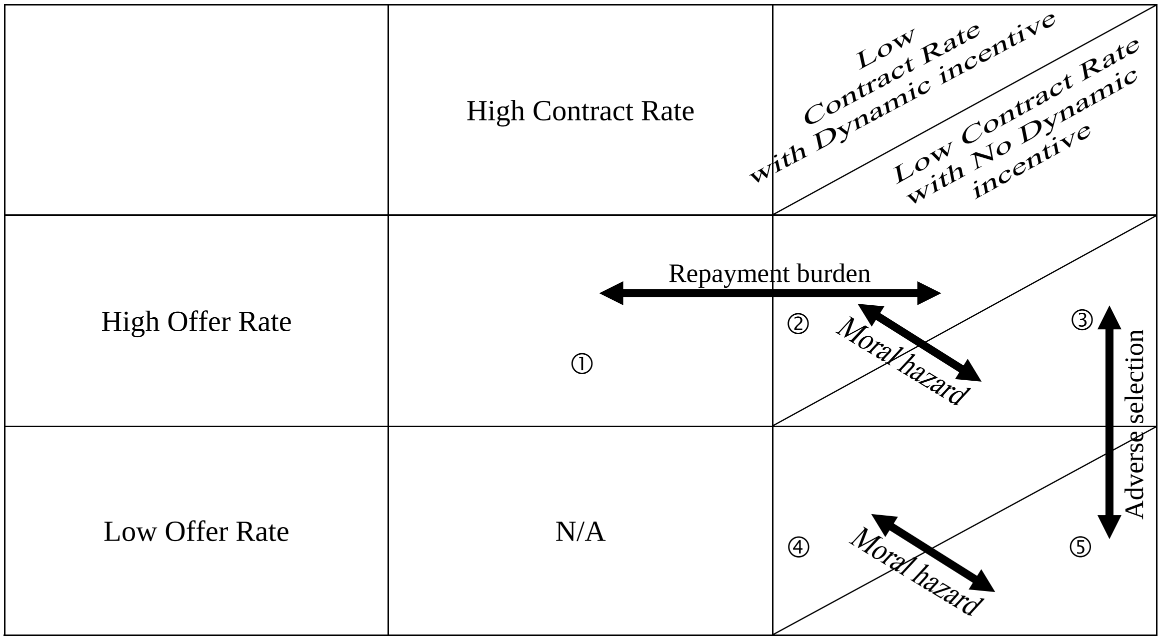
	Low Risk Clients				Medium Risk Clients				High Risk Clients			
	Offer Interest Rate		*Contract Interest Rate*		*Offer Interest Rate*		*Contract Interest Rate*		*Offer Interest Rate*		*Contract Interest Rate*	
	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent
3.25%	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.

Figure 1. Basic Intuition Behind the Experimental Design



This figure provides some basic intuition for our experimental design and identification strategy. We can identify adverse selection by estimating whether loan repayment is worse for those with the same contract but who agreed to borrow at different rates: thus compare the high offer rate groups (cells 2 and 3 in the diagram) to the low offer rate groups (cells 4 and 5), but only for those who received the low contract rate. We can identify moral hazard by estimating for those with the low contract rate whether loan repayment is worse for those who did not receive the dynamic repayment incentive (cells 3 and 5) than for those who did (cells 2 and 4). We can identify repayment burden effects by estimating whether for those who agree to borrow at high rates, loan repayment is worse for those whose rate remains high for the contract (cell 1) than for those whose rate is lowered to the low contract rate (cells 2 and 3).

Figure 2: Operational Steps of Experiment