EXPERIMENTAL EVIDENCE ON THE "CREDIT RATIONING" THEORY

Sergio H. Lence*

Credit markets are probably the markets most heavily regulated and intervened upon by governments all over the world. For example, U.S. regulations explicitly prohibit commercial banks from engaging in many otherwise legal activities (e.g., there are legal limits to the amount a bank may lend to any one borrower) (Spong). The significant size of federally-assisted lending in the U.S., which exceeded $4 trillion between 1980 and 1987 (Gale), is illustrative of the extent of government intervention in credit markets. Such heavy government involvement in credit markets is due, at least in part, to the belief that poorly-functioning credit markets are a major obstacle to an economy's well-being.

Credit rationing is a major credit market imperfection. A formal theory of credit rationing was first advanced by Stiglitz and Weiss and further developed by many others. The great interest in this area of research is evidenced by the fact that Stiglitz and Weiss' study has been among the most cited articles in economics in recent years.[1] The article by Stiglitz and Weiss constitutes a cornerstone of contemporary models in macroeconomics (e.g., Azariadis and Smith), development economics (e.g., Basu), finance (e.g., Thakor), public economics (e.g., Innes), and regional economics (e.g., Hughes), among other fields in economics.

Despite the significant theoretical developments, there has not been a parallel empirical work and, to the best of this author's knowledge, no tests of the theory of credit rationing have been performed. Therefore, the present research is aimed at starting to fill this empirical gap in our current understanding of credit markets.

---

*Associate Professor, Department of Economics, Iowa State University, Ames.

[1]The Social Science Citation Index has often shown more than 50 articles citing Stiglitz and Weiss in a single year.

The distinctive contributions of the present study to the literature are twofold. First, it shows how to set up laboratory experiments specifically aimed at examining the workings of credit markets. Second, tests of the theory of credit rationing are performed employing data gathered at laboratory sessions conducted under the advocated experimental design.

A Simple Theoretical Model of Credit Rationing

The theoretical model underlying the experimental design is a simplified version of the one advanced by Stiglitz and Weiss. Succinctly, the basic economy consists of $N \geq 2$ risk-neutral entrepreneurs and of numerous risk-neutral banks. Each entrepreneur belongs to one of $\Theta \leq N$ entrepreneurial types, with the number of entrepreneurs of type θ being denoted by N_θ, so that $\sum_{\theta=1}^{\Theta} N_\theta = N$. Each entrepreneur of the θth type can invest in a single project that either suceeds and yields R_θ^s with probability p_θ, or fails and yields R_θ^f with probability $(1 - p_\theta)$. Projects associated with different entrepreneurial types differ in their outcomes, but all projects have the same expected outcome $\overline{R}$. That is,

$$(1.1) \quad \overline{R} = R_\theta^s\, p_\theta + R_\theta^f\, (1 - p_\theta),\ \theta = 1, 2, \ldots, \Theta.$$

Without loss of generality, it will be assumed throughout that $R_1^s \leq R_2^s \leq \ldots \leq R_\Theta^s$, so that $p_1 \geq p_2 \geq \ldots \geq p_\Theta$. In other words, entrepreneurs with a greater θ index are "riskier," in the sense that they have a greater probability of failure (although they have higher profits if they succeed).

Each entrepreneur has zero wealth, and must invest the indivisible amount $F > 0$ to realize his project. To make the problem interesting, it is assumed that $F > R_\theta^f$. Also, for simplicity and without loss of generality, F is set equal to one throughout the remainder of the analysis.

Therefore, an entrepreneur must borrow \$1 from a bank to pursue his project. In return, the entrepreneur must promise to pay \$(1 + *r*) back to the bank if the project succeeds, and to surrender the project's outcome to the bank if that outcome is less than \$(1 + *r*). In other words, *r* is the contractual interest rate.

By investing in a project, the random net profits of a θ-type entrepreneur are $\tilde{\pi}_\theta(r) = [R_\theta^s - (1 + r)]$ with probability p_θ, and $\tilde{\pi}_\theta(r) = 0$ with probability $(1 - p_\theta)$. Hence, his expected profits are

$$(1.2) \quad \bar{\pi}_\theta(r) = [R_\theta^s - (1 + r)]\, p_\theta.$$

But a risk-neutral θ-type entrepreneur will invest in a project only if he expects to profit from it ($\bar{\pi}_\theta(r) \geq 0$). Therefore, the maximum interest rate at which he is willing to borrow (r_θ*) is:

$$(1.3) \quad r_\theta^* = R_\theta^s - 1.$$

It follows from (1.3) that $r_1^* \leq r_2^* \leq \ldots \leq r_\Theta^*$, because $R_1^s \leq R_2^s \leq \ldots \leq R_\Theta^s$. That is, the riskier the entrepreneurial type (i.e., the smaller his probability of success), the greater the interest rate at which the entrepreneur is willing to borrow. This is the "adverse selection" effect that drives the credit rationing results; by charging higher interest rates, banks cause the "safer" entrepreneurs to exit the market.

The aggregate demand for loans (L^D) denotes the total amount of funds that entrepreneurs are willing to borrow at any given interest rate. The demand curve L^D is derived from (1.3) and is depicted in Figure 1. The demand curve L^D is a monotonically nonincreasing function of the contractual interest rate (*r*). This occurs because the higher the contractual interest rate, the greater the number of entrepreneurs who do not to want borrow, as borrowing at a higher interest rate will lead to negative expected profits. Furthermore, the aforementioned adverse selection effect means that only the riskier entrepreneurs (those with a small probability of success) are willing to borrow at high interest rates.

A bank's loan at interest rate r to a θ-type entrepreneur yields a random revenue of $\tilde{\rho}_\theta(r)$ = $(1 + r)$ with probability p_θ, and $\tilde{\rho}_\theta(r) = c(R_\theta^f) < (1 + r)$ with probability $(1 - p_\theta)$. The function $[R_\theta^f - c(R_\theta^f)] > 0$ represents the cost incurred by the bank to verify that the project has failed (Gale and Hellwig). Costly state verification (i.e., costly bankruptcy) yields debt, as opposed to equity, the preferred contractual arrangement between banks and entrepreneurs in our setting (Black and de Meza). Therefore, a bank's expected revenue from a loan at interest rate r to an entrepreneur of type θ equals:

(1.4) $\bar{\rho}_\theta(r) = (1 + r)\, p_\theta + c(R_\theta^f)\ (1 - p_\theta)$ if $r \le r_\theta^*$, $\bar{\rho}_\theta(r) = 0$ if $r > r_\theta^*$.

Expression (1.4) implies that, if it were possible, banks would charge the contractual interest rate r_θ^* to θ-type entrepreneurs. This is true because doing so maximizes the expected profits from lending to such entrepreneurs ($0 = \bar{\rho}_\theta(r > r_\theta^*) < \bar{\rho}_\theta(r < r_\theta^*) < \bar{\rho}_\theta(r = r_\theta^*)$). But if banks cannot distinguish among different entrepreneurial types, banks will charge the same interest rate to all loan applicants. In this instance, a bank's expected revenue per loan made is given by the weighted average of (1.4):

(1.5) $$E[\bar{\rho}_\theta(r)] = \frac{\sum_{\theta=j}^{\Theta}[(1+r)\, p_\theta + c(R_\theta^f)(1-p_\theta)]\, N_\theta}{\sum_{\theta=j}^{\Theta} N_\theta},$$

where j is such that $r_{j-1}^* < r \le r_j^*$, and $E(\cdot)$ denotes the expectation with respect to the population of entrepreneurs willing to borrow at the interest rate r.

For each loan made, a bank incurs a cost to obtain the funds that are loaned. Furthermore, that cost increases with the total number of loans made by the banking sector as a whole (L^S). Hence, a bank's cost of funds for each loan made is represented by the monotonically nondecreasing function $i(L^S) \geq 0$, with $i'(L^S) \geq 0$. If the banking sector is competitive, risk-neutral banks will compete until their expected profits are driven to zero, so that

$$(1.6) \quad E[\bar{\rho}_\theta(r)] - i(L^S) = 0.$$

From (1.6), the aggregate supply of loans by banks is obtained as $L^S = i^{-1}\{ E[\bar{\rho}_\theta(r)] \}$, where $i^{-1}(\cdot)$ denotes the inverse function of $i(\cdot)$.

The aggregate loan supply schedule (L^S) may be backward bending, as illustrated in Figure 1. This is in contrast to typical supply schedules for other goods/services (except labor), which are monotonically nondecreasing. The loan supply schedule in Figure 1 is backward bending because higher interest rates induce the "safest" entrepreneurs to exit the market. Hence, at high interest rates only the "riskiest" entrepreneurs are willing to borrow. Loan supply is zero at loan prices between 120 and 155, because lenders expect a loss by lending at such prices. The reason for this is that, even though loan prices between 120 and 155 are relatively high, they are not high enough to offset the very low probability of success of the pool of loan applicants left at such prices.

As a result of the backward-bending supply curve, it is possible for loan market equilibrium to be characterized by "credit rationing." In Figure 1, the credit rationing equilibrium occurs at loan prices between 47 and 75. When loan prices are between 47 and 75, there are 10 loans demanded but only 8 loans supplied, which implies a "credit rationing" of 2 loans. Loan prices between 47 and 75 constitute an equilibrium, because banks have no incentive to deviate

from such loan prices.[2] If banks were to increase loan prices above 75, they would give up profits. This is true because the "safer" entrepreneurs would exit the market due to the high loan prices, thereby leaving a "riskier" pool of loan applicants which yields smaller expected profits per loan made (see (1.5)). If banks reduced loan prices below 47, they would give up profits as well. This is true because the price reduction does not increase the pool of loan applicants.

Given the supply and demand schedules shown in Figure 1, an alternative outcome is the "market clearing" equilibrium which is attained at loan prices between 190 and 205. This outcome may be termed an equilibrium, because at such loan prices the number of loans supplied (2) equals the number of loans demanded (2), so that there is no "credit rationing." However, the present theoretical model indicates that the "credit rationing" equilibrium will prevail over the "market clearing" equilibrium. This is true because at the "credit rationing" equilibrium banks maximize their profits per loan and the number of loans made; hence, banks have no incentives to charge loan prices greater than 75.

Observationally Distinguishable Entrepreneurs

In the previous model, banks charged a single interest rate to all borrowers because it was impossible for banks to distinguish among potential borrowers. A realistic extension consists of allowing for Γ groups of observationally distinguishable entrepreneurs. In this instance, it is necessary to index the variables in the previous model to identify the entrepreneurial group to which they refer. For example, for each observationally distinguishable group γ ($\gamma = 1, 2, \ldots, \Gamma$),

[2]Within the 47-75 interval, banks increase (entrepreneurs reduce) their expected profits by increasing the loan prices charged (paid). Actual loan price locations within the 47-75 interval will depend on the relative negotiating power of banks vis-à-vis entrepreneurs.

there is a total of $N(\gamma)$ entrepreneurs and $\Theta(\gamma)$ types (each of them denoted by $\theta(\gamma)$), and group γ's expected project outcome is $\overline{R}(\gamma)$.

Since banks can distinguish among the Γ borrower groups, they can taylor an interest rate for each group (i.e., charge $r(\gamma)$ to group γ). A bank's expected revenue from a loan made to an entrepreneur of group γ who is of type $\theta(\gamma)$ at interest rate $r(\gamma)$ equals $E\{\overline{\rho}_{\theta(\gamma)}[r(\gamma)]\}$ (see (1.5)). To maximize profits, banks will lend first to the observationally distinguishable group that yields the greatest expected revenues per loan. Only after the pool of loan applicants from such a group is exhausted will banks lend to the entrepreneurs from the observationally distinguishable group that yields the next greatest expected revenues per loan, and so on. As a result, if banks' cost of funds ($i(L^S)$) is sufficiently high, some of the observationally distinguishable entrepreneurial groups do not receive loans at any interest rate $r(\gamma)$. That is, some entrepreneurial groups are "redlined."

Institutional Setting

Hellwig argues that the equilibrium outcome in markets with adverse selection may depend on the institutional setting. For example, the equilibrium outcome in a market in which banks move first (i.e., banks offer loans that entrepreneurs may or may not accept) need not be the same as the outcome obtained when entrepreneurs move first (i.e., entrepreneurs ask for loans that banks may or may not decide to fund). In contrast, the preceding theoretical model is not explicit about the precise meaning of "competition." That is, the institutional setting is implicitly irrelevant to obtain Stiglitz and Weiss' credit rationing results. Therefore, it seems appropriate to investigate empirically whether the institutional setting matters or not for a credit market equilibrium.

Testable Hypotheses

The main objective of the present study is to shed light on some of the fundamental implications of the theory of credit rationing. More specifically, the analysis focuses on the following two key propositions from such theory, which can be proven using the credit market model presented in the previous section:

- Proposition 1. Under certain conditions, in a market where all loan applicants look *identical*, some of them receive loans and others do not, and the latter do not receive loans *even if they offer to pay a higher interest rate*.
- Proposition 2. Under certain conditions, *identifiable groups* of individuals cannot obtain loans *at any interest rate* with a given supply of credit, even though they do with a larger credit supply.

In addition, given the ambiguity of the theory with regard to the institutional setting, the analysis also investigates the following proposition:

- Proposition 3. The institutional setting does not affect market behavior.

The testable hypothesis designed to provide information about the empirical validity of propositions 1 through 3 are discussed in the next subsections.

Testable Hypotheses of Proposition 1

According to the theoretical model with no observationally distinguishable loan applicants, credit rationing may occur under certain conditions. That is, in a market where all loan applicants look identical, some of them receive loans and others do not, and the latter do not receive loans even if they offer to pay a higher interest rate. The theoretical model's results may be tested by means of working hypothesis P1.1:

- *Working hypothesis P1.1.* Assume that the theoretical model is parameterized so as to yield a rationing equilibrium. Then, the prices and quantites traded in the corresponding experimental implementation are the same as the prices and quantities traded predicted by the theoretical model.

It may be argued that the system under study is so complex that a statistical rejection is almost assured (Noussair, Plott, and Riezman), even though important aspects of the empirical results may closely conform to the theoretical predictions. For this reason, we also perform a "comparative statics" test by means of working hypothesis P1.2:

- *Working hypothesis P1.2.* Assume that the theoretical model is initially parameterized so as to yield a rationing equilibrium, and that the theoretical supply of funds is increased to obtain a new rationing equilibrium with the same prices but greater quantity traded than in the original parameterization. Then, in the corresponding experimental implementation, the increase in the supply of funds will leave prices unchanged but will cause quantities traded to increase.

The test based on working hypothesis P1.2 is less stringent than the one based on working hypothesis P1.1, because the latter entails levels, whereas the former only involves changes.

Testable Hypothesis of Proposition 2

In the presence of observationally distinguishable entrepreneurial groups, the theoretical model indicates that some entrepreneurial groups may be "redlined." Let the Γ entrepreneurial groups be ordered according to banks' maximum expected profits per loan made to each of such groups (i.e., let $\gamma_i > \gamma_j$ if $\max_{r(\gamma_i)} E\{\bar{\rho}_{\theta(\gamma_i)}[r(\gamma_i)]\} > \max_{r(\gamma_j)} E\{\bar{\rho}_{\theta(\gamma_j)}[r(\gamma_j)]\}$). Then, the working hypothesis corresponding to the "redlining" result is P2.1:

- *Working hypothesis P2.1.* Assume that there are observationally distinguishable groups, and that the theoretical model is parameterized so that in equilibrium one of the entrepreneurial groups γ_j is rationed but not redlined. Then, in the corresponding experimental implementation, all of the entrepreneurs in group γ_{j-1} receive loans.

Testable Hypotheses of Proposition 3

In developing the theoretical model, no specific references were made to the institutional setting. For example, no explicit assumption was made about the market being characterized by posted offers (i.e., banks offering loans that entrepreneurs may or may not accept) or by posted bids (i.e., entrepreneurs asking for loans that banks may or may not decide to fund). Similarly, the theoretical model is not explicit about the amount of information that entrepreneurs have about the functioning of the market. For example, the model does not assume that entrepreneurs know how bankers (or even other entrepreneurs) make their lending (borrowing) decisions. Hence, the model's credit rationing results are implicitly independent of the institutional setting. The empirical validity of such conclusions may be tested by means of working hypotheses P3.1 and P3.2:

- *Working hypothesis P3.1.* Market equilibrium outcomes are the same under posted offers as under posted bids.
- *Working hypothesis P3.2.* Market equilibrium outcomes do not depend on whether participants are "informed" or "uninformed." Informed (uninformed) participants means that

entrepreneurs are (not) told of the problem the bankers face in making their decision, and vice versa.[3]

Experimental Methods and Procedures

The fundamental reason for the existence of rationing equilibrium is that lenders know less about borrowers than the latter know about themselves. Furthermore, different degrees of lender information about borrowers may lead to different results (e.g., contrast the situation in which all of the loan applicants look the same with that in which there is more than one observationally distinguishable entrepreneurial group). Hence, in any test of the credit rationing theory it is crucial to control strictly for the information available to market participants. Because the best way to accomplish such a strict control is by using laboratory conditions (Brandts and Holt, Davis and Holt, Plott, Roth, Smith), experimental sessions were conducted to systematically collect credit market data.

The data analyzed in the present study were obtained from 14 laboratory experiments. The experimental design specifically addressed two issues of potential relevance for the outcomes of the experiments; namely, unbiasedness and risk attitudes.

With regard to unbiasedness, in laboratory settings it is important to avoid biases due to experimental subjects perceiving that some behavioral patterns are considered either "correct" or "incorrect" (Davis and Holt, Ch. 1). This issue is highly pertinent to the present context, because words such as "loans" or "borrowers" are likely to carry strong moral and/or ethical connotations, and induce subjects to behave differently than if the terms "objects" or "buyers" are used instead.

---

[3]In practice, the "informed" setting is accomplished by giving the same game instructions to all market participants at the beginning of the experimental session, and then telling them what kind of player (i.e., lender or borrower)

Hence, credit language (e.g., loan, borrower, lender) was discarded in favor of a more neutral wording (e.g., object, buyer, seller) for experimental purposes.

The theoretical credit markets introduced previously are isomorphic to markets where sellers (lenders) may supply an object (loan) to buyers (borrowers) at a price (interest rate), but the "net price" or actual amount of money received by sellers from a sale (loan repayment) depends on the specific buyer they sell to. Buyers who are willing to bid high prices (i.e., the "riskiest" loan applicants) are the ones that yield the lowest "net price" for sellers. This isomorphism allowed us to write the laboratory instructions without ever referring to credit language, and to conduct the experiments without subjects realizing that they were actually participating in credit markets.

Experiments were performed with sellers located in different rooms than buyers. This was done not only to better control for the information available to sellers and buyers (i.e., "informed" vs. "uninformed" settings), but also to control for behavioral biases. For example, "risky" buyers might have felt embarrased to bid high prices if buyers had been able to identify them personally. Also to control for biases, experiments involving observationally distinguishable buyer groups were performed using colors to identify such groups (e.g., buyers could belong to the "blue" group or to the "red" group).

Another important issue addressed in the experimental design was the risk attitudes of experimental subjects. Because the theoretical model assumes risk-neutral decision makers, experimental subjects were induced to behave in a risk-neutral manner. In the case of buyers, this was accomplished by simply setting their payoffs equal to their expected values. That is, the

---

they are. In contrast, laboratory experiments with "uninformed" participants are those in which individuals are provided only the instructions corresponding to the kind of player that they are.

payoff to a buyer of type θ who bought an object at price $(1 + r)$ was nonrandom and equal to $\bar{\pi}_{\theta}(r) = [R_{\theta}^{s} - (1 + r)]\, p_{\theta}$ (see expression (1.2)).

The strategy used to induce risk neutrality upon sellers was quite different from that used upon buyers. The reason for this was that, for the experiments to represent the theoretical model, it was crucial that sellers not know the buyer type to whom each of them sold until after making the sale. Risk neutrality upon sellers was induced by adopting the two-stage lottery procedure advocated by Berg et al. That is, a seller's random payoff from a sale was denominated in terms of tickets to a lottery to be conducted in a second stage.

Brief Description of a Generic Experimental Session

Each experimental session involved between 14 and 18 subjects, who were drawn from the population of students at Iowa State University taking economics principles classes and who had no previous experience participating in experimental markets. Each experimental session took approximately two hours to complete. During each session, sellers and buyers participated in 15 trading periods of about 4 minutes each. The item being traded was named "object." During the experiment, all earnings were expressed in "experimental yen" (¥). At the end of the experiment, earnings in experimental yen were converted into U.S. dollars at a prespecified exchange rate. Subjects were paid $10 plus any additional money earned during the experiment. On average, subjects earned between $20 and $25 in total for participating in the experimental session.

In each period, each buyer was allowed to purchase up to one object. If a buyer purchased the object, he received a prespecified "value" for that period and his earnings were computed as Earnings = Price × Value. If the buyer did not purchase in the period, then his earnings were zero for that period. Earnings from each period were added up to obtain the buyer's cumulative earnings at the end of the experiment.

For sellers, each period consisted of three stages: (1) a trading stage; (2) a revelation stage; and (3) a lottery stage.

1. Trading Stage: In each period's trading stage, each seller was allowed to sell up to one object. The seller incurred a prespecified "cost" for that period if he sold the object. If the seller did not sell the object, he did not incur such cost.
2. Revelation Stage: In each period's revelation stage, the buyer "type" to whom each seller sold an object at the trading stage was revealed. Given this buyer "type," each seller converted the price at which he sold into a "final price" that depended on the specific buyer he had sold to (sellers were provided a conversion table for this purpose). The final price was used to calculate the number of tickets to a lottery that determined the seller's likelihood of winning ¥100 or ¥0. If the seller sold his object in the trading stage, then his lottery tickets were calculated as Lottery Tickets = Final Price × Cost. If the seller did not sell his object at the trading stage, he did not incur the Cost, and his Lottery Tickets equalled a prespecified number greater than zero.
3. Lottery Stage: In each period's lottery stage, a "cutoff level" was determined by drawing a random number between 000 and 999. The cutoff level was used to determine the sellers' earnings. A seller's Earnings = ¥100 if his lottery tickets were greater than or equal to the cutoff level, and his Earnings = ¥0 otherwise. Earnings from each period were added up to obtain the seller's cumulative earnings at the end of the experiment.

Results and Discussion

A total of 17 laboratory sessions were conducted, but only data pertaining to the last 14 sessions are analyzed below. Data from the first three sessions were discarded because the sole purpose of such sessions was to gain information about the experimental design and to calibrate the model's parameters. Of the 14 sessions with usable data, 12 sessions were conducted to collect data about propositions 1 and 3 simultaneously, and the other two sessions were used to obtain information about propositions 2 and 3 simultaneously. For expositional clarity, results from the two set of experiments are discussed separately in the following subsections.

Experiments Related to Propositions 1 and 3

The twelve experimental sessions related to propositions 1 and 3 involved the three alternative market scenarios reported in the top six rows of Table 1. For example, under scenario S the equilibrium price is within the interval [47, 75] and the quantity traded is 8 if the equilibrium is characterized by rationing. In contrast, if the equilibrium under scenario S is characterized by market clearing, the equilibrium price is within the interval [190, 205] and the quantity traded is 2. Graphically, the three alternative market scenarios are shown in Figures 1 (scenario S), 2 (scenario S1), and 3 (scenario S2).

For each market scenario, experiments were conducted under two alternative institutions, posted offers (O) vs. posted bids (B), and two alternative information sets available to market participants, informed players (I) vs. uninformed players (U). The institution/information setting yielded an array of 4 experiments (BI, BU, OI, OU) for each of the 3 market scenarios, for a total of 12 experiments.

Table 1. Theoretical equilibria and actual outcomes from alternative scenarios and institutions.

	Scenario	Loan Price $(1 + r)$	Quantity of Loans Traded	Excess Demand for Loans
Theoretical Equilibrium:				
Rationing	S	47-75	8	2
Rationing	S1	103-120	6	3
Rationing	S2	103-120	4	5
Theoretical Equilibrium:				
Market clearing	S	190-205	2	0
Market clearing	S1	149-170	4	0
Market clearing	S2	190-205	2	0
Actual outcome:				
Posted bid, informed players	S	25.7	7.71	
Posted bid, informed players	S1	79.8	5.74	
Posted bid, informed players	S2	30.7	3.84	
Wald test statistic (p-value)		$\chi_3^2 = 148.20$ $(p < 10^{-5})$	$\chi_3^2 = 2.33$ $(p = 0.51)$	
Actual outcome:				
Posted bid, uninformed players	S	67.8	7.79	
Posted bid, uninformed players	S1	97.2	6.02	
Posted bid, uninformed players	S2	43.1	3.56	
Wald test statistic (p-value)		$\chi_3^2 = 105.23$ $(p < 10^{-5})$	$\chi_3^2 = 4.82$ $(p = 0.19)$	
Actual outcome:				
Posted offer, informed players	S	64.6	7.57	
Posted offer, informed players	S1	129.2	5.33	
Posted offer, informed players	S2	115.6	3.47	
Wald test statistic (p-value)		$\chi_3^2 = 3.62$ $(p = 0.30)$	$\chi_3^2 = 11.99$ $(p = 0.007)$	
Actual outcome:				
Posted offer, uninformed players	S	85.8	7.93	
Posted offer, uninformed players	S1	125.7	5.90	
Posted offer, uninformed players	S2	107.1	3.47	
Wald test statistic (p-value)		$\chi_3^2 = 6.19$ $(p = 0.10)$	$\chi_3^2 = 5.96$ $(p = 0.11)$	

Multiple regression results are summarized in Table 2. The dummy variable "initial trading periods" equals 1 for trading periods 1 through 12, and 0 for trading periods 13 through 15. This dummy variable is used to control for participants' learning about the trading environment throughout the session. The "initial trading periods" dummy affects loan prices significantly, as the Wald test statistic corresponding to the joint hypothesis:

H_0: $\alpha_T = \alpha_{T\times O} = \alpha_{T\times I} = \alpha_{T\times S1} = \alpha_{T\times S2} = 0$; H_A: H_0 not true;

equals $\chi^2_5 = 141.72$, which is substantially greater than the 1 percent critical value of $\chi^2_{5,0.01} = 15.09$. In contrast, the quantity of loans is not affected by the "initial trading periods" variable, as the test statistic for the joint hypothesis:

H_0: $\beta_T = \beta_{T\times O} = \beta_{T\times I} = \beta_{T\times S1} = \beta_{T\times S2} = 0$; H_A: H_0 not true;

equals $\chi^2_5 = 1.61$, which has a p-value of 90 percent.

The figures reported in Table 2 suggest that the number of loans made is unaffected by whether the institution is posted bids or posted offers, but that loan prices are significantly affected by them. This intuition is partly confirmed by the Wald test corresponding to the following hypotheses:

Price: H_0: $\alpha_O = \alpha_{T\times O} = \alpha_{O\times I} = \alpha_{O\times S1} = \alpha_{O\times S2} = 0$; H_A: H_0 not true.

Quantity: H_0: $\beta_O = \beta_{T\times O} = \beta_{O\times I} = \beta_{O\times S1} = \beta_{O\times S2} = 0$; H_A: H_0 not true.

The corresponding test statistics are $\chi^2_5 = 276.44$ and $\chi^2_5 = 13.68$, respectively. Thus, the null joint hypothesis is strongly rejected for prices, and (not) rejected for quantities at the 5 (1) percent significance level. Therefore, working hypothesis P3.1 (i.e., that outcomes are the same under posted offers as under posted bids) is strongly rejected by the experimental data.

Table 2. Multiple regression results corresponding to propositions 1 and 3.

Dependent Variable	Loan Price $(1 + r)$	Quantity of Loans Made
Explanatory variables:		
Intercept	$\hat{a} = 67.8^{**}$ (4.6)[a]	$\hat{\beta} = 7.79^{**}$ (0.22)
Initial trading periods	$\hat{a}_T = 33.6^{**}$ (4.8)	$\hat{\beta}_T = 0.01$ (0.23)
Posted offer	$\hat{a}_O = 18.0^{**}$ (5.1)	$\hat{\beta}_O = 0.14$ (0.23)
Informed players	$\hat{a}_I = -42.2^{**}$ (5.1)	$\hat{\beta}_I = -0.08$ (0.23)
Scenario S1	$\hat{a}_{S1} = 29.4^{**}$ (5.7)	$\hat{\beta}_{S1} = -1.77^{**}$ (0.26)
Scenario S2	$\hat{a}_{S2} = -24.7^{**}$ (6.6)	$\hat{\beta}_{S2} = -4.23^{**}$ (0.26)
Initial trading periods × Posted offer	$\hat{a}_{T\times O} = -25.4^{**}$ (4.7)	$\hat{\beta}_{T\times O} = -0.08$ (0.20)
Initial trading periods × Informed players	$\hat{a}_{T\times I} = 12.2^{**}$ (4.7)	$\hat{\beta}_{T\times I} = -0.06$ (0.20)
Initial trading periods × Scenario S1	$\hat{a}_{T\times S1} = -15.9^{**}$ (5.4)	$\hat{\beta}_{T\times S1} = -0.04$ (0.25)
Initial trading periods × Scenario S2	$\hat{a}_{T\times S2} = 4.7$ (6.2)	$\hat{\beta}_{T\times S2} = 0.23$ (0.25)
Posted offer × Informed players	$\hat{a}_{O\times I} = 21.0^{**}$ (3.8)	$\hat{\beta}_{O\times I} = -0.29$ (0.16)
Posted offer × Scenario S1	$\hat{a}_{O\times S1} = 10.5^{*}$ (4.3)	$\hat{\beta}_{O\times S1} = -0.27$ (0.20)
Posted offer × Scenario S2	$\hat{a}_{O\times S2} = 46.0^{**}$ (4.9)	$\hat{\beta}_{O\times S2} = -0.23$ (0.20)
Informed players × Scenario S1	$\hat{\alpha}_{I\times S1} = 24.8^{**}$ (4.3)	$\hat{\beta}_{I\times S1} = -0.20$ (0.20)
Informed players × Scenario S2	$\hat{a}_{I\times S2} = 29.8^{**}$ (4.9)	$\hat{\beta}_{I\times S2} = 0.37$ (0.20)
R^2	0.408	0.908
Number of observations	1,025	180

* (**) Significantly different from zero at the 5 (1) percent level based on the two-tailed t-statistic.

[a]Numbers between parentheses below coefficient estimates denote the respective standard deviations.

Similarly, working hypothesis P3.2 (i.e., that outcomes do not depend on whether players are informed or uninformed) is strongly rejected as well, because the following null hypotheses are strongly rejected:

Price: H_0: $\alpha_I = \alpha_{T\times I} = \alpha_{O\times I} = \alpha_{I\times S1} = \alpha_{I\times S2} = 0$; H_A: H_0 not true.

Quantity: H_0: $\beta_I = \beta_{T\times I} = \beta_{O\times I} = \beta_{I\times S1} = \beta_{I\times S2} = 0$; H_A: H_0 not true.

The associated Wald tests yield test statistics of χ_5^2 = 102.06 and χ_5^2 = 18.28, respectively, thereby indicating a strong rejection of the null.

Because of the significant effect of the trading period and of institutional arrangements on loan prices, the following tests of working hypotheses P1.1 and P1.2 refer specifically to point estimates of the outcomes for the last three periods of trading in each experiment (periods 13 through 15) obtained by means of the multiple regressions reported in Table 2, for each of the four institutional settings (BI, BU, OI, and OU).

Working hypothesis P1.1 (i.e., that observed prices and traded quantitities are the same as predicted by the theoretical rationing equilibrium model) may be tested as follows:

H_0: Loan price at S is between 47 and 75, loan price at S1 is between 103 and 120, and loan price at S2 is between 103 and 120.

H_A: H_0 not true.

Although this hypothesis test only involves prices, its rejection implies rejection of working hypothesis P1.1. Point estimates of prices for scenarios S, S1, and S2 are reported in Table 1 and depicted in Figures 1, 2, and 3, respectively. Only four of the 12 point estimates lie inside the respective hypothesized intervals (S-BU, S-OI, S2-OI, and S2-OU). Five of the 12 point estimates lie below the respective hypothesized intervals (S-BI, S1-BI, S2-BI, S1-BU, and S2-BU), and three lie above (S1-OI, S-OU, and S1-OU). The reported Wald test statistics indicate a strong rejection of the null joint hypothesis that the price point estimates for scenarios S, S1, and S2 lie inside their respective intervals in the two posted-bid (B) settings. In contrast, in the two posted-offer (O) settings, the null hypothesis cannot be rejected according to the Wald test statistics.

Table 1 and Figures 1 through 3 also suggest that posted-offer prices tend to be systematically greater than posted-bid prices. Furthermore, the difference between posted offer and posted bid prices seems systematically greater for the informed setting. The corresponding hypothesis tests indicate that both results are statistically significant.

An alternative way of testing working hypothesis P1.1 is:

H_0: Quantity traded at S = 8, quantity traded at S1 = 6, and quantity traded at S2 = 4.

H_A: H_0 not true.

Again, this hypothesis test only involves quantities, but its rejection implies rejection of working hypothesis P1.1. Point estimates of the quantities traded in the last three periods are shown in Table 1, and represented pictorially in Figures 1 through 3. In only one (S1-BU) out of the 12 experiments do the point estimates exceed the number of trades predicted by the rationing equilibrium. The Wald test statistics indicate that the null hypothesis should be rejected in only one (OI) of the four scenarios, at standard levels of significance.

The experiments were designed to yield easily testable comparative statics results to discriminate between the "credit rationing" and the "market clearing" equilibria. This is useful because it is important to know whether the "credit rationing" equilibrium is rejected because the actual equilibrium favored in the experiments is "market clearing." Note that in going from scenario S to scenario S1, the "credit rationing" equilibrium implies an increase in price (from the interval [47, 75] to the interval [103, 120]) and a decrease in the quantity traded (from 8 to 6). In contrast, if "market clearing" equilibrium prevails, switching from scenario S to scenario S1 should cause a decrease in price (from the interval [190, 205] to the interval [149, 170]) and an increase in the quantity traded (from 2 to 4).

In terms of the regression models of Table 2, "market clearing" can be rejected in favor of "credit rationing" if the point estimate of price at the three final periods for scenario S1 minus the point estimate of price for scenario S is significantly positive. Such a difference equals 54.1, 29.4, 64.6, and 39.9 for the BI, BU, OI, and OU settings, respectively. The associated *t* statistics equal 9.50, 5.16, 11.26, and 7.04, respectively, indicating that in all instances the price differences are significantly greater than zero.

Alternatively, "market clearing" can be rejected in favor of "credit rationing" if the point estimate of last-period quantity for scenario S1 minus the one for scenario S is significantly negative. The point estimates of this difference are –1.97, –1.77, –2.23, and –2.03 for the BI, BU, OI, and OU settings, respectively. In all four settings the differences are significantly smaller than zero, as the associated *t* statistics are 7.46, 6.70, 8.47, and 7.71. Both price and quantity comparative statics strongly reject the "market clearing" equilibrium in favor of the "credit rationing" equilibrium, confirming the visual intuition provided by Figures 1 through 3.

Working hypothesis P1.2 (i.e., that under rationing equilibrium in the theoretical model, an increase in the supply of funds increases observed quantities traded but does not affect observed prices) may be tested by comparing the price outcomes for scenarios S1 and S2. Scenarios S1 and S2 are the same, except that the supply of funds is greater for the former than for the latter. Therefore, working hypothesis P1.2 can be tested as follows:

H_0: $\Delta P \equiv$ loan price under S1 – loan price under S2 = 0.

H_A: H_0 not true.

Point estimates of ΔP (*t* statistics) for the three final periods are 49.1 (7.13), 54.1 (7.82), 13.6 (1.94), and 18.6 (2.66) for BI, BU, OI, and OU, respectively. Hence, ΔPs are greater than zero at standard significance levels for BI, BU, and OU, and barely nonsignificant for OI. Based on this evidence, the "credit rationing" equilibrium can be rejected.

It is worth noting that the above rejection of the "credit rationing" model is not due to the "market clearing" model being favored. This is evident because, by the design of experiments S1 and S2, one should obtain ΔP < 0 if the actual outcome were the "market clearing" equilibrium. Under the "market clearing" equilibrium, S1 prices [103-120] are smaller than S2 prices [190-205]. Given that the point estimates of ΔP are all positive and three of them significantly so, it is clear that the "market clearing" equilibrium is rejected even more strongly than the "credit rationing" equilibrium.

Experiments Related to Propositions 2 and 3

Two laboratory sessions (one with institutional setting BI and the other with OI) involving two observationally distinguishable borrower groups were conducted with the purpose of gathering data to test working hypotheses P2.1 and P3.1. The scenario analyzed is summarized in Table 3. The aggregate loan demand schedule is identical to that of scenario S in Table 1, but the crucial difference now is that borrowers are classified into two observationally distinguishable groups (i.e., $\Gamma = 2$). Lenders' maximum expected profits per loan made to group 2 borrowers are greater than lenders' maximum expected profits per loan made to group 1 borrowers. Therefore, if the "redlining" equilibrium held, group 1 loan applicants should receive loans only if all four of the applicants from group 2 receive loans. The alternative "naïve rationing" equilibrium reported in Table 3 is obtained under the (highly unlikely) assumption that lenders do not care about which

Table 3. Theoretical equilibria and actual outcomes from alternative scenarios and institutions in the presence of observationally distinguishable groups.

	Scenario	Observationally Distinguishable Entrepreneurial Group	Loan Price $(1 + r)$	Quantity of Loans Traded	Excess Demand for Loans
Theoretical equilibrium:					
Redlining	S	$\gamma = 1, N_1 = 6$	50-75	4	2
Redlining	S	$\gamma = 2, N_2 = 4$	7-96	4	0
Theoretical equilibrium:					
Naïve rationing	S	$\gamma = 1, N_1 = 6$	47-75	4.8[a]	1.2[a]
Naïve rationing	S	$\gamma = 2, N_2 = 4$	47-75	3.2[a]	0.8[a]
Theoretical equilibrium:					
Market clearing	S	$\gamma = 1, N_1 = 6$	190-205	2	0
Market clearing	S	$\gamma = 2, N_2 = 4$	-	0	0
Actual outcome:					
Posted bid, informed players	S	$\gamma = 1, N_1 = 6$	10.8	4.30	
Posted bid, informed players	S	$\gamma = 2, N_2 = 4$	61.8	2.03	
Actual outcome:					
Posted offer, informed players	S	$\gamma = 1, N_1 = 6$	53.3	5.03	
Posted offer, informed players	S	$\gamma = 2, N_2 = 4$	85.2	2.97	

[a]On average.

observationally distinguishable group a loan applicant belongs to. Finally, the "market clearing" equilibrium is characterized by a loan price at which aggregate loan demand is equal to aggregate loan supply.

Results from the multiple regressions are reported in Table 4. There are strong indications that prices as well as quantities in early trading periods were quite different from

Table 4. Multiple regression results corresponding to propositions 2 and 3.

Dependent Variable	Loan Price $(1 + r)$	Quantity of Loans Made
Explanatory variables:		
Intercept	$\hat{\varphi} = 10.8\ (9.4)^{a}$	$\hat{\phi} = 4.30^{**}\ (0.44)$
Initial trading periods	$\hat{\varphi}_T = 45^{**}\ (11)$	$\hat{\phi}_T = -1.04^{*}\ (0.48)$
Posted offer	$\hat{\varphi}_O = 42^{**}\ (13)$	$\hat{\phi}_O = 0.73\ (0.54)$
Entrepreneurial group 2	$\hat{\varphi}_{y2} = 51^{**}\ (15)$	$\hat{\phi}_{y2} = -2.27^{**}\ (0.54)$
Initial trading periods × Posted offer	$\hat{\varphi}_{T\times O} = 7\ (14)$	$\hat{\phi}_{T\times O} = -0.25\ (0.55)$
Initial trading periods × Entrepreneur. group 2	$\hat{\varphi}_{T\times y2} = -46^{**}\ (14)$	$\hat{\phi}_{T\times y2} = 2.08^{**}\ (0.55)$
Posted offer × Entrepreneur. group 2	$\hat{\varphi}_{O\times y2} = -19\ (11)$	$\hat{\phi}_{O\times y2} = 0.20\ (0.44)$
R^2	0.324	0.350
Number of observations	209	60

* (**) Significantly different from zero at the 5 (1) percent level based on the two-tailed t-statistic.
[a]Numbers between parentheses below coefficient estimates denote the respective standard deviations.

those at later trading periods. This is true because the test statistics for the joint hypotheses:

H_0: $\varphi_T = \varphi_{T\times O} = \varphi_{T\times\gamma 2} = 0$; H_A: H_0 not true;

H_0: $\phi_T = \phi_{T\times O} = \phi_{T\times\gamma 2} = 0$; H_A: H_0 not true;

are $\chi^2_3 = 33.29$ and $\chi^2_3 = 14.81$, respectively, which are well in excess of the 1 percent critical value ($\chi^2_{3,0.01} = 11.34$).

With regard to the institutional arrangement, the hypotheses:

Price: H_0: $\varphi_O = \varphi_{T\times O} = \varphi_{O\times\gamma 2} = 0$; H_A: H_0 not true;

Quantity: H_0: $\phi_O = \phi_{T\times O} = \phi_{O\times\gamma 2} = 0$; H_A: H_0 not true;

yield test statistics of $\chi^2_3 = 55.62$ and $\chi^2_3 = 8.73$, respectively, compared to a critical value of $\chi^2_{3,0.05} = 7.82$ ($\chi^2_{3,0.01} = 11.34$) at the 5 (1) percent level of significance. Thus, as this was the case under no observationally distinguishable groups, (a) prices are significantly different under posted bids than under posted offers, and (b) the number of loans made is significantly different under posted bids than under posted offers at the 5 percent significance level, but not at the 1 percent level. Therefore, working hypothesis P3.1 (i.e., that outcomes are the same under posted offers as under posted bids) is also strongly rejected by data obtained under observationally distinguishable groups.

In the present experimental setting, working hypothesis P2.1 means that, if the "credit rationing" model is true, group 1 loan applicants should receive loans ($Q_1 > 0$) only if all four of group 2 loan applicants receive loans ($Q_2 = 4$). This condition is equivalent to (a) $Q_1 = 0$ if $Q_2 < 4$, and (b) $Q_1 \geq 0$ if $Q_2 = 4$. Hence, working hypothesis 2 can be tested as follows:

H_0: $(4 - Q_2)\, Q_1 = 0$; H_A: $(4 - Q_2)\, Q_1 > 0$.

The test statistics (p-values) are $\chi_1^2 = 20.44$ ($p = 0.00001$) and $\chi_1^2 = 5.89$ ($p = 0.015$) for the posted bid and the posted offer scenarios, respectively, leading to a strong rejection of the null hypothesis for posted bids, and to rejection (nonrejection) at the 5 (1) percent significance level for posted offers.

Summary and Conclusions

An experimental design of laboratory sessions specifically aimed at examining the workings of credit markets is developed. A major feature of the advocated design is the tight control of experimental subjects' information about the market. Experiments are designed to avoid likely biases in participants' behavior induced by the use of credit market terminology (e.g., "loans" or "borrowers") with potential strong moral and/or ethical connotations. In addition, for consistency with the assumptions from the theoretical model of credit rationing, the experiments are set up to elicit risk-neutral behavior from participants.

Statistical analysis of the experimental data strongly rejects the credit rationing hypothesis for most scenarios studied. There is significant evidence that the institutional setting greatly influences the market outcomes. In particular, posted offer prices tend to be higher than posted bid prices, and such a difference increases with the participants' information about the market. More importantly, the evidence against the credit rationing hypothesis is much weaker (or even nonexistent) under posted offers than under posted bids.

For modeling purposes, an important implication of the present study is that the "credit rationing" framework is more tenable to represent real-world credit markets better characterized as posted-offer markets than posted-bid markets. Interestingly enough, perhaps the most conspicuous credit markets of all, those for bank loans, can be approximated reasonably well as posted-offer markets.

When interpreting the present empirical findings, however, there are at least two issues to consider. First, in economic systems as complex as the one being studied, it is very easy to statistically reject the benchmark model(s) (Noussair, Plott, and Riezman). In this regard, future empirical work should assess the economic, as opposed to statistical, relevance of the departures from the "credit rationing" predictions. Second, the most obvious competing model, "market clearing," is rejected far more strongly than the "credit rationing" paradigm. That is, at the present time it is not clear that there exists a better alternative to the "credit rationing" theory, even if the latter is rejected empirically. It seems fair to argue that this issue should occupy a prominent place in the future research agenda on the theory of credit markets.

References

Azariadis, C., and B. D. Smith. "Financial Intermediation and Regime Switching In Business Cycles." *American Economic Review* 88(1998):516-536.

Basu, K. "Limited Liability and the Existence of Share Tenancy." *Journal of Development Economics* 38(1991):203-220.

Berg, J., L. A. Daley, J. W. Dickhaut, and J. R. O'Brien. "Controlling Preferences for Lotteries On Units of Experimental Exchange." *Quarterly Journal of Economics* 101(1986):281-306.

Black, J., and D. de Meza. "The Nature of Credit-Market Failure." *Economics Letters* 46(1994):243-249.

Brandts, J., and C. A. Holt. "An Experimental Test of Equilibrium Dominance in Signaling Games." *American Economic Review* 82(1992):1350-1365.

Davis, D. A., and C. A. Holt. *Experimental Economics*. Princeton NJ: Princeton University Press, 1993.

DeBondt, W. F. M., and R. H. Thaler. "Financial Decision Making In Markets and Firms: A Behavioral Perspective." In Jarrow, R. A., V. Maksimovic, and W. T. Ziemba, Editors, *Finance, Handbooks In Operations Research and Management Science* 9, 385-410. Amsterdam: New Holland, 1995.

Innes, R. "Investment and Government Intervention In Credit Markets When There Is Asymmetric Information." *Journal of Public Economics* 46(1991):347-381.

Gale, D., and M. Hellwig. "Incentive-Compatible Debt Contracts: The One-Period Problem." *Review of Economic Studies* 52(1985):647-663.

Gale, W. G. "Economic Effects of Federal Credit Programs." *American Economic Review* 81(1991):133-152.

Hellwig, M. "Some Recent Developments In the Theory of Competition In Markets with Adverse Selection." *European Economic Review* 31(1987):319-325.

Hughes, M. "General Equilibrium of A Regional Economy with a Financial Sector - Part II: A Simple Behavioral Model." *Journal of Regional Science* 32(1992):19-37.

Noussair, C. N., C. R. Plott, and R. G. Riezman. "An Experimental Investigation of the Patterns of International Trade." *American Economic Review* 85(1995):462-491.

Plott, C. R. "An Updated Review of Industrial Organization: Applications of Experimental Methods." In Schmalansee, R., and R. D. Willig, Editors, *Handbook of Industrial Organization*, Volume 2, Chapter 19. Amsterdam: New Holland, 1989.

Roth, A. E. "Introduction to Experimental Economics." In Kagel, J. H., and A. E Roth, Editors, *Handbook of Experimental Economics*, Chapter 1. Princeton, NJ: Princeton University Press, 1995.

Smith, Vernon L. "Microeconomic Systems as an Experimental Science." *American Economic Review* 72(1982):923-955.

Spong, K. *Banking Regulation: Its Purposes, Implementation, and Effects, Fourth Edition*. Kansas City: Federal Reserve Bank of Kansas City, 1994.

Stiglitz, J. E., and A. Weiss. "Credit Rationing in Markets with Imperfect Information." *American Economic Review* 71(June 1981):393-410.

Thakor, A. V. "Capital Requirements, Monetary Policy, and Aggregate Bank Lending: Theory and Evidence." Journal of Finance 51(1996):279-324.

Figure 1. Loan supply and demand schedules for scenario S.

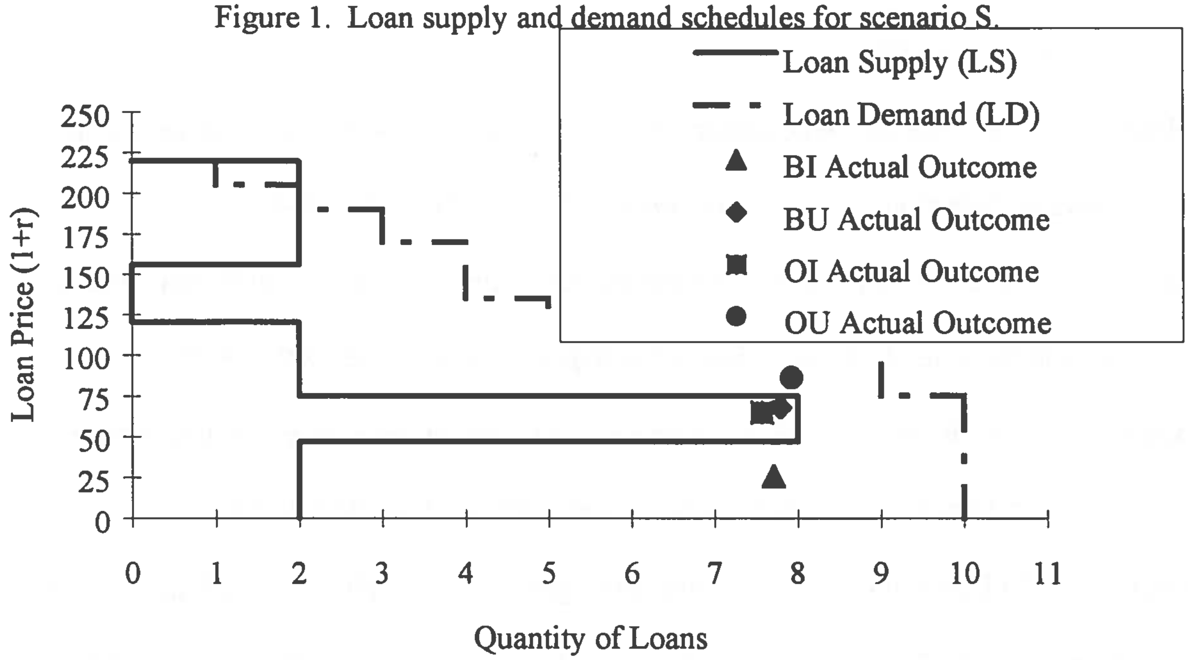

Figure 2. Loan supply and demand schedules for scenario S1.

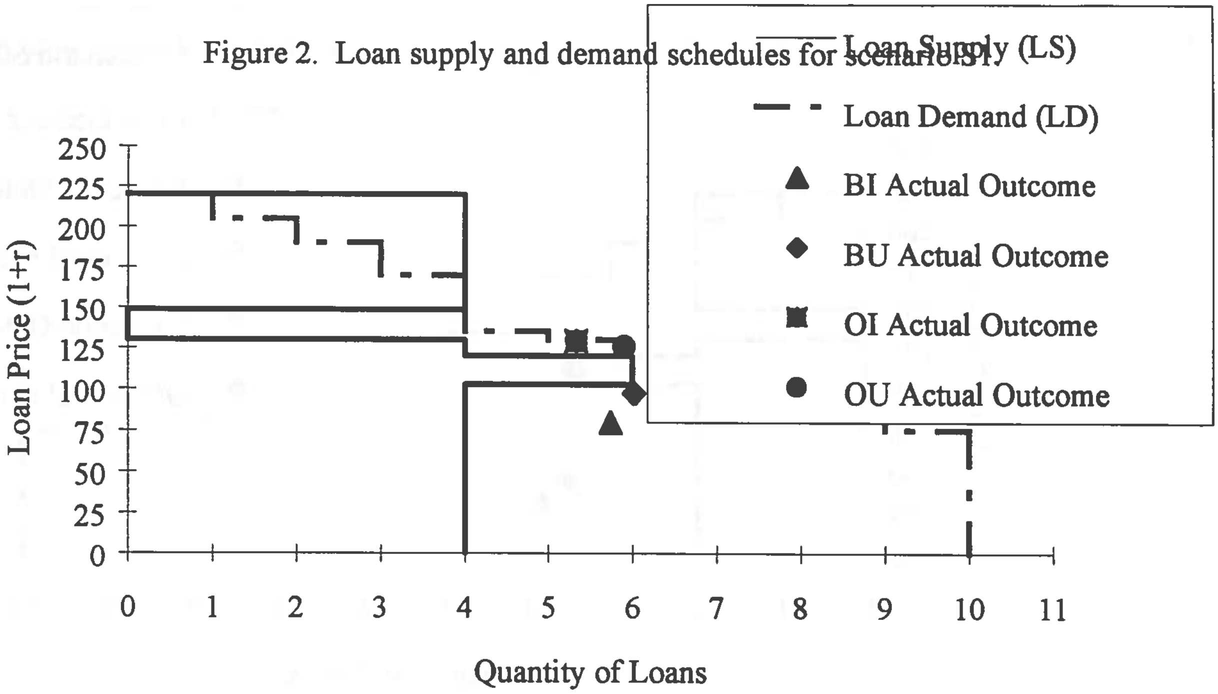

Figure 3. Loan supply and demand schedules for scenario S2.