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Credit Reporting, Relationship Banking, and Loan Repayment

How does information sharing between lenders affect borrowers repayment behavior? We show—in a laboratory credit market—that information sharing increases repayment rates, as borrowers anticipate that a good credit record improves their access to credit. This incentive effect of information sharing is substantial when repayment is not third-party enforceable and lending is dominated by one-shot transactions. If, however, repeat interaction between borrowers and lenders is feasible, the incentive effect of credit reporting is negligible, as bilateral banking relationships discipline borrowers. Information sharing nevertheless affects market outcome by weakening lenders' ability to extract rents from relationships.

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IN CREDIT MARKETS, borrowers typically have more information about their investment opportunities, their own character and their prior indebtedness than lenders. This asymmetry of information gives rise to selection problems for lenders and potential moral hazard of borrowers, which may lead to a rationing of credit (Stiglitz and Weiss 1981). In many countries problems of asymmetric information are aggravated by the fact that loan contracts are costly to enforce (Levine 1998, Jappelli, Pagano, and Bianco 2005).

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One response to asymmetric information and costly enforcement in the credit market is information sharing between lenders about the characteristics and behavior of their borrowers. Theoretical models suggest that information sharing can reduce adverse selection in markets where borrowers approach different lenders sequentially (Pagano and Jappelli 1993). Moreover, information sharing can also have a strong disciplining effect on borrowers. The model of Diamond (1989) suggests that a public credit registry can motivate borrowers to choose agreed projects. Further models show that information sharing can discipline borrowers into exerting high effort in projects (Vercammen 1995, Padilla and Pagano 2000) and repaying loans (Klein 1992).

A recent survey by the World Bank shows that institutionalized information sharing, i.e., credit reporting through private credit bureaus or public credit registries, now exists in over 100 countries worldwide (World Bank 2006).[1] In the United States, where credit reporting is most prevalent, over 3 million credit reports are issued every day (Hunt 2005). In recent years, many developing and transition economies have also introduced credit registries or fostered credit bureaus in the hope of boosting credit growth (Miller 2003). Giving the strong growth of credit reporting worldwide and the high hopes which policymakers place in such institutions, there is a need for empirical evidence which examines how credit reporting affects the performance of the financial sector.

In this paper, we use experimental methods to examine how credit reporting affects loan repayment and credit market performance. We examine an experimental credit market in which loan repayment is not third-party enforceable. We first implement a market in which there is no opportunity for information sharing between lenders. We then implement an identical market but with a stylized public credit registry which collects and disburses credit information to lenders. By comparing repayment behavior and credit volumes between the two markets we can identify the impact of a credit registry on credit market performance.

We contribute to the empirical literature on information sharing in two ways. First, this is the only study we know of which examines the impact of information sharing on borrower behavior. Several authors have shown that credit reports do reduce the selection costs of lenders by allowing them to more accurately predict loan defaults (Barron and Staten 2003, Kallberg and Udell 2003, Powell et al. 2004, Luoto, McIntosh, and Wydick 2007). The disciplining effect of information sharing on borrower behavior has, however, not yet been studied.[2] This is by no means surprising, giving that with field data it is difficult to identify whether an individual borrower has behaved differently than he would have done without the presence of a credit registry.

1. Public credit registries are created by public authorities and typically run by central banks. It is usually mandatory for all supervised financial institutions to submit information to a public credit registry. In return, the same institutions are entitled to receive credit reports based on information available in the registry. Private credit bureaus are typically set up by banking associations or private entrepreneurs to facilitate voluntary information sharing between lenders.

2. Jappelli and Pagano (2002) show that loan defaults, measured by country risk indicators, are lower in countries where credit registries and bureaus are more developed. However, this result can obviously arise from better selection of borrowers rather than from actually disciplining them to repay.

The second contribution of our paper is that, in contrast to existing studies, we can directly identify how credit reporting affects credit market performance. Current evidence on the relation between information sharing and credit market performance relies on cross-country comparisons using aggregate or firm-level data. Jappelli and Pagano (2002) and Djankov, McLiesh, and Shleifer (2007) show that aggregate bank credit to the private sector is higher in countries where information sharing is more developed. Analyses of firm-level survey data (Galindo and Miller 2001, Love and Mylenko 2003, Brown, Jappelli, and Pagano 2007) further show that access to bank credit is easier in countries where credit bureaus or registries exist. These studies cannot clearly identify the direction of causality between information sharing and credit volume. After all, theory suggests that information sharing will emerge where lenders benefit more from them (Pagano and Jappelli 1993), and this is certainly the case where the credit volume is higher. Thus a positive correlation between credit reporting and credit market performance may arise simply because credit bureaus are more likely to emerge in countries where current lending is vibrant or an expansion of credit activity is expected. By applying experimental methods, our study allows us to circumvent this endogeneity issue and identify how the exogenous introduction of a credit registry affects credit market performance.

The impact of credit reporting on repayment behavior should depend on the presence of alternative disciplining mechanisms. One alternative disciplining mechanism is relationship banking. Theoretical models suggest that implicit contracts between lenders and borrowers, i.e., banking relationships, can motivate high effort and timely repayments (Boot and Thakor 1994). Empirical studies confirm that some credit market segments (in particular, small business lending) are pervaded by relationship banking and that these relationships improve the access of potential borrowers to credit (Petersen and Rajan 1994, Elsas and Krahnen 1998). Experimental studies (Brown, Falk, and Fehr 2004, Fehr and Zehnder 2005) also confirm that long-term relationships are a powerful disciplinary device. In credit markets dominated by repeated interactions (e.g., working capital loans), information sharing may therefore not be required to discipline borrowers. In contrast, in credit markets dominated by short-term interactions (e.g., consumer credit markets when borrower mobility is high), borrowers may only be motivated to repay if they know that, due to credit reporting, their current behavior is observable by other lenders. In this paper, we examine how the impact of credit reporting on repayment is related to the presence of relationship banking. We conduct our experiment for two credit market environments. In one environment information conditions prevent repeated interaction between borrowers and lenders so that all lending transactions are inherently one-off. In the second environment, information conditions are such that lenders can choose to trade with the same borrower repeatedly and banking relationships can emerge endogenously.

Our results indicate that the impact of credit reporting on repayment behavior and credit market performance is highly dependent on the potential for relationship banking. When bilateral relationships are not feasible, the credit market essentially collapses in the absence of credit reporting. As repayments are not third-party enforceable, many borrowers default and lenders cannot profitably offer credit contracts. The

introduction of a credit registry in this environment greatly enhances the performance of the credit market. The availability of information on past repayment behavior allows lenders to condition their offers on the borrowers' reputation. As borrowers with a good track record receive better credit offers, all borrowers have a strong incentive to sustain their reputation by repaying their debt. As a consequence a well functioning credit market is established in which a large percentage of the available gains from trade is realized.

When relationship banking is feasible, credit reporting has no such effect on market performance. In this environment, the market participants solve the moral hazard problem related to repayment even in the absence of a credit registry. By repeatedly interacting with the same borrower, lenders establish long-term relationships that enable them to condition their credit terms on the past repayments of their borrower. As only a good reputation leads to attractive credit offers from the incumbent lender, borrowers have strong incentives to repay. The disciplining effect of these banking relationships is sufficiently strong so that the introduction of a credit registry only slightly improves credit market performance. Nevertheless, even when relationship banking is feasible, a credit registry does affect market outcome. First, the credit market is less dominated by specific borrower–lender relations, as these are no longer necessary to enforce repayment. Second, by improving the information available to "outside" lenders, a credit registry reduces the ability of incumbent lenders to extract rents from relationships.

The plan of the paper is as follows. Section 1 presents our experimental design and Section 2 the corresponding predictions. Section 3 presents our results. Section 4 concludes.

1. EXPERIMENTAL DESIGN

Our objective is to study how the repayment behavior of borrowers in a competitive credit market is affected by information sharing among lenders. We therefore implement a simple lending game in which loan repayment is not third-party enforceable, and embed this game in a competitive trading environment.

1.1 Experimental Credit Market

Our lending game is based on the trust game introduced by Berg, Dickhaut, and McCabe (1995). In this sequential, two-player game the first-mover has an endowment which he can keep or transfer to the second-mover. If the second-mover receives a transfer from the first-mover he earns an income which exceeds the value of that transfer. The second-mover then decides how to share his income with the first-mover by choosing the size of a return-transfer. In a very stylized way the trust game captures the basic features of a credit transaction when borrowers have riskless investment opportunities, but loan repayments are not enforceable. The extension of credit (first-mover transfer) then generates positive gains from trade. However, lending is risky because borrowers (second-movers) can maximize their short-term

profits by keeping all earned income to themselves. One-shot and repeated trust games have been studied intensively in the experimental literature (for an extensive review see, e.g., Camerer 2003, p. 83 ff., p. 446 ff.). Our experimental design implements a version of the trust game that differs in at least two fundamental aspects from the previous literature. First, in order to measure the impact of information sharing on repayment behavior, we implement a repeated trust game in which we exogenously vary the lenders' information on borrowers' past repayment decisions. Second, as we want to study repayment behavior in a market environment, we allow lenders and borrowers to endogenously choose their trading partners in a competitive trading environment. To the best of our knowledge, both of these features have not been experimentally investigated before.

Our experimental credit market involves 17 participants. These participants are randomly assigned to the role of borrowers and lenders at the beginning of a session. Ten subjects are in the role of lenders and seven subjects are in the role of borrowers. Each session lasts for 20 periods and the roles of subjects are fixed for the whole session.[3]

At the beginning of every period each lender is endowed with 50 capital units. A lender has two opportunities to make use of his endowment. He can either invest the endowment in an endowment-storing technology or he can use the endowment to extend credit to a borrower. The first stage of each period is a continuous one-sided auction, in which lenders and borrowers can seal credit contracts. The lenders are the contract makers; i.e., they alone can make credit offers to the borrowers, who themselves cannot apply for credit. When making a credit offer the lender has to specify four items: the size of the loan (k), the requested repayment ($\tilde{r}$), the set of market participants who can observe the offer, and which borrowers are authorized to accept the offer. Lenders can freely decide how they want to split their endowment between the endowment-storing technology and a credit offer; i.e., the loan size k can be picked from the set $\{5, 10, 15, \ldots, 50\}$. The set for the requested repayment $\tilde{r}$ is given by $\{5, 10, 15, \ldots, 100\}$. There are two types of credit offers: public credit offers and private credit offers. A private credit offer is only addressed to one specific borrower. It cannot be seen or accepted by other borrowers and is also not visible to other lenders. A public offer is always shown to all borrowers and all other lenders. However, even with public offers the lender must specify which borrowers are authorized to accept the offer. He can choose or exclude borrowers at will.[4] During the auction a lender can make as many public and private offers as he wants. However,

3. There are two reasons why we choose a fixed number of periods instead of a random stopping time: First, the fixed number of periods makes sure that we have sessions of comparable length for all our treatments. Thus, differences in behavior across treatments cannot be due to different learning opportunities or other time-dependent effects. Second, the finite time horizon also provides us with within-subject evidence for the relevance of reputation effects. While a random stopping time provides constant reputational incentives, a finite time horizon implies that reputational concerns are strong in the early periods but fade towards the end of the experiment. As a consequence the same subjects are exposed to varying intensities of reputation effects.

4. This implementation of public offers is designed to capture public announcements of credit conditions by banks, who can always choose not to extend credit to some clients on these terms.

each lender can only conclude one credit contract per period. As soon as a borrower accepts an offer of a given lender, a contract is concluded and all other outstanding offers made by this lender disappear from the market and can no longer be accepted by other borrowers. Each borrower can accept at most one contract per period so that our credit market implements an excess supply of credit.

Borrowers are endowed with 5 capital units in each period. At the second stage of a period borrowers automatically earn an investment income that is twice the size of this endowment and their borrowed capital, $2(5 + k)$. At the third stage of a period, borrowers who received a loan decide whether they want to make the repayment requested by the lender ($r = \tilde{r}$) or not repay at all ($r = 0$). Partial repayments are not possible.[5]

At the end of each period, each lender is informed about his borrower's repayment decision, profits are calculated and all market participants get to know their own and their partner's payoffs for the period. Payoff functions, the number of lenders and borrowers and the number of trading periods are common knowledge. The monetary payoffs of the market participants per period are calculated as follows:

$$\text{Payoff of lender:} \quad \pi = 50 = k + r$$
$$\text{Payoff of borrower:} \quad v = 2(5 + k) - r.$$

1.2 Treatments

Our goal is to study how credit reporting affects borrowers' repayment choices and credit market performance. In order to do so we first implement our credit market without any opportunity for information sharing between lenders. We then implement the same credit market with an exogenous credit reporting mechanism, which collects and disburses information on past repayment behavior of borrowers. In the treatments with credit reporting all lenders have free access to a credit report at the beginning of every period. The report lists, for each borrower and all past periods, whether the borrower received a loan and whether he repaid it.

Our credit reporting mechanism is a stylized version of a public credit registry. The main feature of a public credit registry is that it is mandatory for (at least supervised) financial institutions to contribute information to its records. In contrast, private credit bureaus are based on voluntary information sharing between lenders. In our experiment all lenders must submit information about their lending activity to the credit reporting institution in each period. In the following we therefore refer to it as the "credit registry."[6] In reality, public credit registries (and private credit bureaus) differ strongly in the range of lending activities which they cover, and the range of information they collect and distribute on each credit activity. The coverage of public

5. In reality some borrowers obviously become delinquent without fully defaulting. However, due to the deterministic nature of investment earnings in our design we exclude partial repayments.

6. As we are only interested in the impact of information sharing, rather than its emergence we do not consider voluntary information sharing on which private credit bureaus are founded. For an experimental analysis of voluntary information sharing in a competitive credit market see Brown and Zehnder (2007).

TABLE 1
EXPERIMENTAL TREATMENTS

	Relationship banking	
	Not possible (Random ID)	Possible (Fixed ID)
Credit reporting		
Yes	RID–CR	FID–CR
No	RID–NO	FID–NO

credit registries is affected by the range of financial institutions which must submit information, the types of loans which are included (commercial credit/consumer credit) and the size of the threshold above which loans are included (Miller 2003). The range of information collected per credit transaction can vary between default information only (referred to as "negative" or "black" information in the literature) to detailed information on outstanding and past repaid loans ("positive" or "white" information). Further, real-world public credit registries differ in the time frame for which they distribute this information.[7] Our credit registry provides full coverage of all credit activities in our experiment by collecting and disbursing information on each individual loan from each prior period. The information distributed by our credit registry on each credit transaction is limited. Our credit reports tell lenders which borrowers received a loan in which period, and whether these loans were repaid or not. Moreover, our registry provided complete historical information on each borrower, as each lender can review the full repayment history of each individual borrower at any time. However, the registry does not provide information on loan sizes and interest rates (requested repayments) of the credit transactions in question. Thus, our credit registry disburses both negative and positive information, but the detail of this information is (like for most real-life credit registries) limited.[8]

Table 1 provides an overview of our experimental treatments. Treatments which include credit reporting are called "CR" treatments (for credit reporting). Treatments without credit reporting are called "NO" treatments (for no credit reporting).

The impact of credit reporting on borrowers' repayment behavior may depend on the feasibility of alternative disciplining mechanisms in the credit market. Obviously, there will be no impact of information sharing on repayment behavior if loan repayment is perfectly enforceable at no cost by a third party. However, even when a

7. Public credit registries and private credit bureaus often restrict the historical information provided, due to administrative costs or consumer protection laws. Brown, Jappelli, and Pagano (2007) show that in transition countries 8 out of 12 public credit registries and 8 out of 9 private credit bureaus provide information on lending activities for longer than 2 years.

8. We chose to exclude information on loan sizes from the credit reports in our experiment in order to simplify the information provided to lenders. In contrast to some public credit registries our registry also does not collect information on interest rates which lenders demand from borrowers. However, even where public credit registries do collect information on interest rates, they do not provide to other lenders in credit reports, but rather use this information to facilitate bank supervision.

third party is absent, repayment may be implicitly enforced through bilateral banking relationships (Boot and Thakor 1994). For this reason, we are particularly interested in how the impact of credit reporting depends on the degree of relationship banking in a credit market. In reality, the feasibility of relationship banking in a credit market varies, depending on how mobile borrowers are and how diverse their funding needs are, compared to the product and geographical specialization of lenders. If borrowers are highly mobile and lenders are geographically specialized, banking relationships will be difficult to maintain. On the other hand, if mobility of borrowers is low or lenders are universal banks with country-wide coverage relationships are simple to maintain.

We examine the impact of credit reporting for market conditions with varying feasibility of relationship banking. In order to study the range of the impact that credit reporting may have on borrower behavior we implement our CR–NO comparison for two border cases: in one market condition relations are not feasible at all, while in the other condition borrowers and lenders can always continue relationships if they want to. Our first condition makes it impossible for lenders to interact repeatedly with a particular borrower by randomly assigning identification numbers (IDs) to borrowers and lenders in each new period. This procedure guarantees that no market participant can identify his former trading partners at the beginning of a period and therefore intentional repeated offers by lenders to borrowers are ruled out. We henceforth call treatments with this environment Random ID (RID) treatments. Our second environment involves a market in which lenders and borrowers have the opportunity to engage in long-term relationships. Repeated interaction with the same trading partner is possible because subjects have fixed IDs for the entire experimental session. Consequently, lenders can offer credit to the same borrower (i.e., to the same ID number) in consecutive periods and, if the borrower accepts these offers, a long-term relationship is established. In the following we call treatments with this environment Fixed ID (FID) treatments.

1.3 Procedures

In total we conducted 20 experimental sessions, five for each of our four treatments. We had 17 subjects in each session, which makes a total of 340 participants. All experimental subjects were volunteers. They were all participating for the first time in such an experiment, and each participant could only participate in one session (i.e., each subject experienced only one of the treatments). All participants were students at the University of Zurich or the Swiss Federal Institute of Technology Zurich (ETH). The computerized experiment was programmed and conducted with the experimental software z-Tree (Fischbacher 2007). A session lasted approximately 90 minutes. Subjects received a show-up fee of 10 Swiss francs (CHF) and 1 additional franc for every 20 points earned during the experiment. On average subjects earned 55 Swiss francs (CHF 1.3 $\approx$ 1 USD in January 2006).

To make sure that all participants fully understand the decision process and the payment structure of the game, each subject had to read a detailed set of instructions

before a session was started. An English version of our German instructions is available in Brown and Zehnder (2006). The experimental instructions were framed in a credit market language. The reason why we chose a context-specific and not a neutral framing was that the experiment was relatively complex. In complex experiments a completely neutral language bears the danger that subjects create their own (potentially misleading) interpretation of the decision environment. Thus, the context-specific framing gives us control over what our participants have in mind. In our view, this not only reduces noise but also increases the external validity of the experiment.[9] After reading the instructions participants had to pass a test with control questions. No session started before all subjects had correctly answered all control questions. Additionally, there were two practice periods before an actual session was started in order to make the participants familiar with the bidding procedures. In both practice periods, subjects only went through the offering stage of a period, i.e., there were no repayment choices and subjects could not earn money in the practice periods.

2. PREDICTIONS

Under the assumption of common knowledge of rationality and selfishness of all market participants, the predictions for each of our four treatments are straightforward. Since repayments are not enforceable, a borrowers best response in the stage game is to never repay a loan. Lenders, anticipating this behavior, will never offer credit. Consequently, the credit market collapses in the stage game equilibrium. As our experiment lasts for a finite number of periods, a simple backward induction argument ensures that the stage game equilibrium is played in every period of the game. The different treatment conditions do not affect this prediction. If lenders are certain that all borrowers are selfish, neither public information on past repayment behavior of borrowers (RID–CR, FID–CR treatments) nor the possibility to establish long-term relationships (FID–NO, FID–CR treatments) can overcome this inefficient outcome.

Empirical evidence suggests, however, that not all people will simply maximize monetary payoffs in our experiment. It has been shown that, in a wide range of economic settings, the behavior of some people is also driven by social motives (for an overview see, e.g., Fehr and Schmidt 2002). Especially important for our purposes is the experimental evidence on the "trust game" described in the previous section. In his survey of experimental evidence for the trust game Camerer (2003) shows that even in anonymous, one-shot transactions many second-movers do make substantial return transfers. It appears that many second-movers feel a moral obligation to repay the first-mover for his initial transfer or are willing to reciprocate a first-mover's

9. Some experimenters argue that a context-specific framing distorts incentives as it provides the subjects with notions of how they "should behave." However, in this experiment we are only interested in the behavioral differences across treatments. We do not make any inferences from observed levels of variables (e.g., repayment rates or interest rates). As we do not see any reasons why the context-specific framing should create different notions of how to behave across treatments, this problem is not relevant for our analysis.

risky decision which benefits them. Recent research by Karlan (2005) shows that the behavior of second-movers in the trust game extends to their behavior in real-life financial decisions, suggesting that the observed "social motives" are by no means an artifact of laboratory experiments.

The evidence from the trust game therefore suggests that in our experiment social motives could lead some borrowers to repay loans because they would otherwise suffer from a bad conscience, or because they would like to reciprocate the decision of lenders to lend to them. In the following, we examine our four treatments under the assumption that the behavior of some (non-distinguishable) borrowers display such social motives. We assume that "social" borrowers are conditionally reciprocal: they are willing to meet their repayment obligations ($r = \tilde{r}$) even in a one-shot situation, as long as the interest rate requested by the lender $\frac{(\tilde{r}-k)100}{k}$ does not exceed a personal threshold value. We assume that the remaining borrowers are selfish in the sense that they never repay loans in a one-shot situation.

2.1 Predictions for the RID Treatments

In the RID–NO treatment, lenders have no information on the prior behavior of any particular borrower in the market. This treatment essentially implements a series of one-shot interactions so that each period can be analyzed as a one-period game. In such a game, selfish borrowers never repay their debt, while "social" borrowers repay as long as they are offered fair financing conditions. Under these conditions the provision of credit can only be profitable for lenders if there is a substantial fraction of social borrowers. If, in contrast, there are only few social borrowers the credit market collapses and all lenders fully invest their capital into the endowment-storing technology. In the Appendix, we examine optimal lending behavior in the RID–NO treatment depending on the share of social borrowers and the degree of their social preferences. Proposition A1 of the Appendix summarizes our findings, and suggests that under reasonable assumptions about the degree of social preferences[10] at least two-thirds of borrowers would need to be social to guarantee the existence of a functioning credit market. Existing experimental evidence for trust games suggests that this condition is unlikely to be satisfied (Camerer 2003). Accordingly, we predict that the credit market will collapse in our RID–NO treatment.

In the RID–CR treatment, lenders receive a credit report at the beginning of each period stating, for each borrower and each prior period, whether the borrower concluded a credit contract and whether he repaid his debt. In contrast to the RID–NO treatment, lenders in the RID–CR can therefore condition their credit offers (whether to offer credit, the credit size, and the desired repayment) on the borrowers' past repayment behavior. If selfish borrowers anticipate this conditionality of loan offers they have a strong incentive to hide their type and imitate the behavior of social borrowers. Repaying a loan is the only way for selfish borrowers to build up a reputation as a social type and thereby obtain access to profitable future credit offers of lenders. In the

10. We assume that social borrowers are never willing to repay if the requested repayment of the lender implies that the borrower is left with less than half of the gains from trade.

Appendix, we show that this mechanism can sustain an equilibrium in which a substantial credit volume is provided, even in cases where the share of social borrowers is such that the credit market would collapse in the RID–NO treatment.[11]

Proposition A2 in the Appendix describes the following equilibrium behavior of lenders and borrowers: In all periods lenders strictly condition their credit offers on the borrowers' past repayment behavior; i.e., they make only credit offers to borrowers who have never defaulted in the past. In a first phase of the RID–CR treatment this motivates all selfish borrowers to repay loans out of reputational concerns and accordingly lenders extend the maximal credit volume. During this "pooling" phase, selfish, and social borrowers behave identically and therefore no information about the borrowers' types is revealed. In later periods, reputational incentives decline and repayment rates fall as selfish borrowers begin to default with a positive probability. In this second phase, the aggregate credit volume begins to fall as those borrowers who defaulted in prior periods receive no further loans and those who repaid receive only loans with non-maximal credit sizes. Furthermore, competition among lenders implies that credit offers are such that all gains from trade go to the borrowers and lenders make zero profits throughout the experiment.

Based on the above considerations we state the following hypothesis for our RID treatments:

Hypothesis RID Treatments: In the RID–CR treatment, lenders condition credit offers and terms on the information available in the credit registry about the prior repayment behavior of borrowers. This creates reputation incentives in the RID–CR treatment inducing a significantly higher repayment rate of borrowers than in the RID–NO treatment. Consequently, the credit volume extended by lenders is also significantly higher than in the RID–NO treatment. The repayment rate and credit volume in the RID–CR treatment converge, however, to that of the RID–NO treatment toward the end of the experiment.

2.2 Predictions for the FID Treatments

In the FID–NO treatment, there is no credit registry, so lenders do not have information on the behavior of all borrowers in all prior periods. However, due to fixed ID's, lenders do have information on past behavior of those borrowers with whom they themselves have traded in prior periods. Thus, in contrast to the RID–NO treatment, lenders can reward known borrowers with good repayment histories by offering them attractive contract renewals. If repayment guarantees access to profitable loans from incumbent lenders, selfish borrowers may also be motivated to repay. In the Appendix, we show that there is an equilibrium in the FID–NO treatment in which

11. The assumption that there are two non-distinguishable types of borrowers implies that we analyze a finitely repeated game with incomplete information. Such games are usually characterized by a large number of equilibria (Fudenberg and Maskin 1986). It is not our objective to provide a complete formal analysis of our experimental game in the Appendix. We rather prove that there are perfect Bayesian equilibria in which the reputation mechanisms intuitively described in this section ensure that a functioning credit market exists.

endogenously formed banking relationships ensure the provision of a substantial credit volume even in the case where the fraction of social borrowers is insufficient to guarantee the existence of a credit market in the RID–NO treatment.

The equilibrium behavior of lenders and borrowers derived in Proposition A3 of the Appendix can be described as follows: In the first period, all lenders make a competitive offer and try to conclude a contract with a borrower. Those lenders who succeed in concluding a contract with a borrower in the first period subsequently establish a long-term relationship with their incumbent borrower. As long as the incumbent borrower repays, they renew his contract in every period by making him a private offer. Lenders who could not conclude a contract in the first period invest their capital in the endowment-storing technology and remain outside the credit market. They do not try to enter the market by making competitive offers to borrowers in relationships with other lenders because they believe that such contract offers would only attract selfish borrowers. As outside lenders do not contest the market, lenders who have established a relationship with a borrower can exert a certain market power and "hold-up" their borrower. By making offers that just satisfy the conditions under which social borrowers repay, they can skim off part of the gains from trade in their relationship. Of course, in the first period lenders anticipate that they will earn a rent if they manage to establish a relationship. Competition among lenders therefore implies that they are prepared to make losses in the first period in order to gain access to the rents earned in a relationship. Within relationships, the conditional contract renewals of incumbent borrowers, in combination with the fact that outside lenders are not willing to offer credit, motivates selfish borrowers to perfectly imitate the repayment behavior of social borrowers in a first phase of the game. As lenders make profits in these periods they maximize their income by extending maximum credit amounts. During this "pooling" phase of the experiment, no additional information about the types of borrowers is revealed and the lenders' beliefs remain constant at the initial level. When the end of the game draws near, however, lenders are only willing to renew their contracts if they obtain additional information on the borrowers' types. Therefore, in this phase, selfish borrowers start defaulting with positive probabilities, thus ensuring that lenders can update their beliefs and remain willing to renew their contracts. However, as defaulting borrowers no longer receive credit offers and as lenders start to lower the size of their loans, the extended credit volume decreases towards the end of the game.

In the FID–CR treatment, the presence of a credit registry implies that lenders have information not only on the behavior of their own prior borrowers, but on all borrowers in the game. As a consequence, the "credit reporting" equilibrium derived for the RID–CR treatment and described in detail in Proposition A2 of the Appendix also applies for the FID–CR treatment. Even in the presence of a credit registry, though, the relationship equilibrium described for the FID–NO treatment in Proposition A3 in the Appendix can also be sustained in the FID–CR treatment. In the Appendix (Propositions A2 and A3) we show that the "credit reporting" equilibrium and the "relationship banking" equilibrium can yield identical repayment rates and practically identical credit volumes. Thus market performance in the FID–CR treatment

can be similar to that in the FID–NO, regardless of which equilibrium type arises. However, market structure and distribution of surplus will differ between the FID–CR and FID–NO treatments if the "credit reporting" equilibrium emerges in the FID–CR. As discussed above, long-term relationships are not necessary to sustain this equilibrium and would therefore be observed less frequently than in the FID–NO. Moreover, lenders who establish relationships earn quasi-rents in the FID–NO treatment while in the "credit reporting" equilibrium all lenders earn zero profits in all periods. Based on the above considerations we make the following hypothesis for our FID treatments:

Hypothesis FID Treatments: Repayment rates and credit volume are identical in the FID–NO and FID–CR treatments: both display high repayment rates and credit volumes in an initial phase. Toward the final period, however, some borrowers start to default and credit volumes decrease. In the FID–CR, the disciplining of borrowers is less reliant on relationship lending, so long-term relationships may be less frequent. Moreover, the presence of a credit registry implies that in the FID–CR it will be more difficult for lenders to extract profits from relationships than in the FID–NO.

3. RESULTS

3.1 Random ID Treatments

In this section we examine the impact of credit reporting in a market where there is no alternative device to motivate loan repayment. In particular, bilateral relationships are prevented due to the random assignment of ID numbers to all lenders and borrowers in each period. Table 2 presents summary statistics for our two corresponding treatments, the RID–CR and RID–NO. For each treatment the table presents mean statistics across all five sessions, as well as the range of results per sessions.

TABLE 2

SUMMARY STATISTICS: RANDOM ID TREATMENTS

Treatment	RID–CR			RID–NO		
	Mean	Max	Min	Mean	Max	Min
Repayment rate	0.80	0.87	0.70	0.28	0.39	0.16
Realized contracts (%)	93.7	96.4	90.7	58.7	78.6	40.0
Credit size	41.0	44.1	36.3	22.8	27.2	18.3
Credit volume (%)	76.8	83.7	65.9	26.6	36.4	15.6
Interest rate (%)	33.3	48.1	24.6	33.1	38.4	29.9
Payoff lender	50.6	52.7	47.7	44.0	45.4	42.2
Payoff borrower	47.6	52.4	39.5	31.8	39.3	24.4

NOTES: The table presents summary statistics for the RID–CR and RID–NO treatments. For each statistic the table presents the mean value across the five sessions of each treatment. In addition, it presents the highest mean value (Max) and lowest mean value (Min) of an individual session. *Repayment rate* is the share of credit transactions in which the borrower made the desired repayment of the lender. *Realized contracts* is the percentage of potential contracts (7 per session and period) which were realized. *Credit size* is the mean size of credit extended in realized credit transactions. *Credit volume* is the percentage of the potential credit volume ($7 \times 50 = 350$ per session and period) which was realized. *Interest rate* is the mean of the calculated interest rate per transaction: $\frac{(\text{Desired repayment} - \text{Credit size})}{\text{Credit size}} \times 100$. *Payoff lender* is the payoff per lender and period. *Payoff borrower* is the payoff per borrower and period.

Our results display strong differences in market outcome between the RID–CR and RID–NO treatments. As predicted, the repayment rate of borrowers is substantially higher in the RID–CR than in the RID–NO treatment. On average 80% of all loans are repaid in the RID–CR treatment, while only 28% of loans are repaid in the RID–NO. Moreover, looking at session level outcomes we see that in the RID–CR the mean repayment rate exceeds 70% in all sessions, while in the RID–NO treatment no session displays a repayment rate higher than 40%. Based on a comparison of session averages a Mann–Whitney test suggests that the difference in repayment rates between the two treatments is statistically significant.[12]

Table 2 further shows a substantial difference in lending activity between the RID–CR and RID–NO treatments. In the RID–CR, 94% of all potential lending contracts are realized, and the average credit size is 41 (out of 50 possible) points. In the RID–NO, by contrast, only 59% of potential contracts are realized, while the average credit size is only 23 points. These results imply that 77% of the potential credit volume is realized in the RID–CR treatment, while only 27% of the potential credit volume is realized in the RID–NO. Session level results further show that the realized credit volume is higher in every session of the RID–CR than in any session of the R–NO. A Mann–Whitney test based on session averages confirms that credit volume is significantly higher in the RID–CR than in the RID–NO treatment.[13]

While credit market efficiency differs substantially between the RID–CR and RID–NO treatments, the distribution of gains from trade is more similar. Table 2 shows that the mean interest rate demanded by lenders is practically identical in the two treatments. In our design the interest rate in percentage can be calculated as: $\frac{(\text{Desired repayment} - \text{Credit size}) \times 100}{\text{Credit size}}$. While interest rates do vary more across sessions in the RID–CR than in the RID–NO, there does not seem to be a significant difference in the level of interest rates.[14] In both treatments, all gains from trade are entirely reaped by the borrowers. Lenders earn average payoffs that are very close to their outside option of 50 points per period. In contrast, borrowers earn average payoffs which well exceed their outside option of 10 points per period. Looking at differences across treatments we find that borrowers earn substantially more in the RID–CR than RID–NO treatment because gains from trade are higher in the RID–CR. A comparison of session averages suggests that the difference in borrower payoffs between

12. We conduct a Mann–Whitney test using mean repayment rates per session as observations. The five sessions of the RID–CR treatment display repayment rates of 87, 85, 81, 80, and 70%, respectively. In the RID–NO treatment, the five sessions have repayment rates of 39, 30, 29, 26, and 16%, respectively. A one-sided test thus cannot reject the hypothesis that repayment is more frequent in the RID–CR treatment ($p = 0.004$).

13. We conduct a Mann–Whitney test using realized credit volume per session as observations. In the RID–NO treatment, the five sessions display a credit volume (measured in percentage of the total potential volume) of 36, 29, 29, 24, and 16%, respectively. In the RID–CR treatment, the credit volume per session was 84, 81, 78, 76, and 66%, respectively. A Mann–Whitney test thus yields a p-coefficient of $p = 0.004$.

14. We conduct a Mann–Whitney test using mean interest rates per session as observations. In the RID–NO treatment, the five sessions display a mean interest rate of 38.4, 34.7, 32.0, 30.4, and 29.9%, respectively. In the RID–CR treatment, the mean interest rate per session was 48.1, 35.2, 30.1, 28.4, and 24.6%, respectively. A two-sided test thus yields a p-coefficient of $p = 0.69$.

TABLE 3

REPAYMENT HISTORY AND CREDIT ACCESS: RANDOM ID TREATMENTS

	RID–CR			RID–NO		
	(1) Contract	(2) Credit size	(3) Interest rate	(4) Contract	(5) Credit size	(6) Interest rate
Prior repayment rate	1.256**	20.474**	−20.18*	−0.083	0.914	−3.009
	(3.54)	(4.61)	(4.43)	(0.23)	(0.60)	(0.93)
Period 6–10	0.455	4.768*	−2.075	−0.414*	−5.463	2.744
	(1.62)	(4.57)	(1.34)	(2.05)	(1.24)	(0.64)
Period 11–15	0.415	5.958**	−5.023*	−0.874**	−8.045	−2.338
	(1.23)	(5.69)	(2.94)	(5.85)	(2.75)	(0.42)
Period 16–20	−0.984**	1.000	18.582	−1.303**	−7.502*	−10.798
	(9.34)	(0.61)	(0.81)	(5.05)	(3.03)	(1.91)
Constant	0.804	20.166*	48.9**	0.892**	26.287**	35.328**
	(1.70)	(3.42)	(15.43)	(2.95)	(12.68)	(9.40)
Observations	663	621	621	665	376	376
R^2		0.18	0.02		0.07	0.02

NOTES: The table reports regression estimates using credit contract data per borrower and period from the RID–CR and RID–NO treatments. Column (1) and (4) report probit estimates for *Contract* (probability of sealing a contract). Column (2) and (5) report OLS estimates for *Credit size* (size of credit received) using data only for borrowers who sealed a credit contract in a particular period. Column (3) and (6) report OLS estimates for *Interest rate* $\frac{(\text{Desired repayment}-\text{Credit size})}{\text{Credit size}} \times 100$, again using data only for borrowers who sealed a credit contract in a particular period. All six regressions estimate coefficients for the following explanatory variables: *Prior repayment* rate is the ratio of prior credit transactions in which the borrower made the repayment desired by the lender. *Period 6–10* is a dummy variable for all transactions that took place in periods 6–10. *Period 11–15* is a dummy variable for all transactions that took place in periods 11–15. *Period 16–20* is a dummy variable for all transactions that took place in periods 16–20. In all regressions the T-statistics reported in parentheses are based on standard errors adjusted for clustering at session level. *indicates significance at the 5% level; **indicates significance at the 1% level.

treatments is highly significant.[15] Lenders also earn slightly more in the RID–CR treatment, where on average they break even, than in the RID–NO treatment where they make slight losses. A comparison of session averages suggests that the difference in lender payoffs between treatments is significant.[16] The fact that all gains from trade are reaped by the borrowers confirms our prediction that the excess supply of credit in our experiment would lead to a highly competitive credit market.

The summary statistics displayed in Table 2 support our hypothesis that the presence of the credit registry encourages loan repayment in the RID–CR treatment because borrowers anticipate that their future access to credit depends on their repayment history. Table 3 presents regression results confirming that borrowers' access to credit in the RID–CR treatment is strongly dependent on their prior repayment behavior. In our experiment, lenders can condition three aspects of credit offers on a borrowers repayment history: whether to offer a contract at all, which credit size to offer, and

15. We conduct a Mann–Whitney test using mean period profits of borrowers per session as observations. In the RID–NO treatment, the five sessions display mean borrower profits of 39.3, 34.5, 31.1, 29.8, and 24.4 per period, respectively. In the RID–CR treatment the five sessions display mean borrower profits of 52.4, 52.2, 47.1, 46.5, and 39.5 per period, respectively. A one-sided test thus yields a p-coefficient of $p = 0.004$.

16. We conduct a Mann–Whitney test using mean period profits of lenders per session as observations. In the RID–NO treatment, the five sessions display mean lenders profits of 45.4, 45.3, 44.4, 42.9, and 42.2 per period, respectively. In the RID–CR treatment the five sessions display mean lenders profits of 52.7, 52.4, 50.5, 49.8, and 47.7 per period, respectively. A one-sided test thus yields a p-coefficient of $p = 0.004$.

which interest rate to demand. We expect that in the RID–CR treatment borrowers with better repayment records are more likely to receive credit, receive larger loans, and pay lower interest rates. In contrast, in the RID–NO treatment where information conditions prevent conditional contract offers, we should find that access to credit and the cost of funds are independent of a borrower's past behavior. In order to test these hypotheses, Table 3 examines the credit conditions of borrowers in each period of our RID–CR and RID–NO treatments. Column (1) of Table 3 reports the results of a probit regression relating a borrower's probability of sealing a credit contract in the RID–CR treatment to his personal repayment history. The dependent variable in this column is a dummy variable "Contract," which is 1 if the borrower seals a credit contract in period t and 0 otherwise. We relate this dummy variable to a borrower's "Prior repayment rate," which measures the share of previous loans which he repaid. We control for time effects by including three dummy variables "Period 6–10," "Period 11–15," and "Period 16–20," which are 1 only for observations within the respective phase of the experiment. Since observations within a session may not be independent, the t-statistics reported in parentheses in Table 3 (and in all other regressions below) are based on robust standard errors, adjusted for clustering at the session level. The positive and significant coefficient on "Prior repayment rate" in column (1) shows that in the RID–CR treatment borrowers with good credit records are more likely to obtain credit. Column (2) and column (3) relate the credit conditions of those borrowers who did receive credit in the RID–CR treatment to their prior repayment behavior. In column (2), we report results for "Credit size," while in column (3) we report results for the "Interest rate" charged by lenders. The coefficient of "Prior repayment rate" in both columns confirms our predictions; borrowers with good credit histories receive larger loans and pay lower interest rates. The results reported in columns (1) through (3) of Table 3 demonstrate that lenders in the RID–CR make extensive use of information available from the credit registry in this treatment. They condition their loan offers strongly on the prior repayment behavior of borrowers. By doing so they create strong incentives for borrowers to repay loans at least in the early phases of the experiment. Such reputation incentives are not present in the RID–NO treatment, where the access to credit and the cost of funds are not conditioned on prior repayment behavior. Columns (4) through (6) repeat our regression analysis using data from the RID–NO treatment. Not surprisingly, in this treatment a borrower's prior repayment rate has no significant impact on his probability of obtaining credit, the size of this loan or the interest rate.

Our results so far show that credit reporting creates strong reputation incentives for even selfish borrowers to repay loans in the RID–CR treatment. This explains why the average repayment rate in the RID–CR is almost three times higher than in the RID–NO treatment. However, even if the credit registry in the RID–CR treatment does discipline borrowers to repay loans, we expect that the repayment rate will fall towards the end of the experiment. Remember that the value of a good credit record declines towards the end of our experiment, due to the finite horizon of 20 periods. We therefore expect that selfish borrowers who repay in earlier periods due to reputation concerns will default in the final periods. Indeed, Figure 1 shows that toward the end

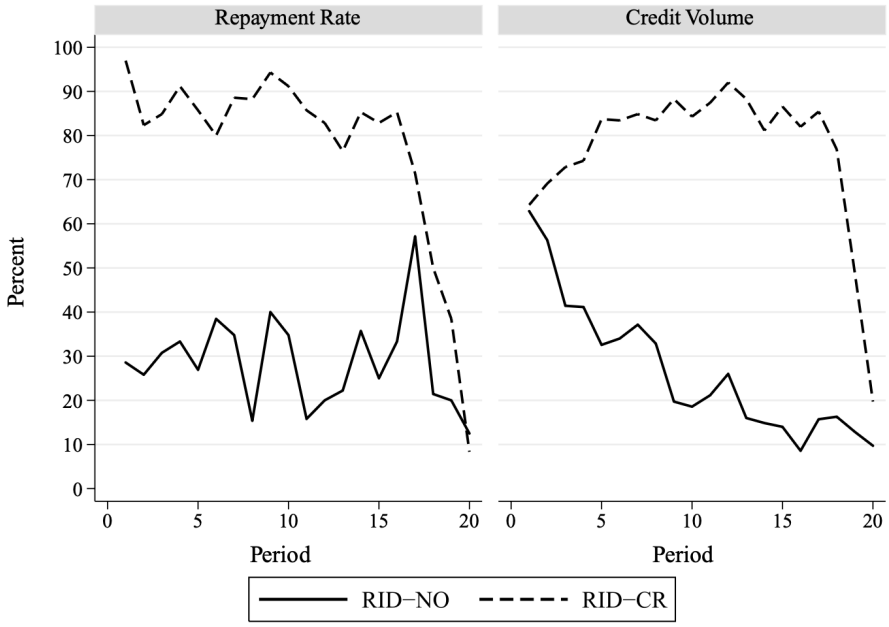

FIG. 1. Repayment Rate and Credit Volume: Random ID Treatments.

of the RID–CR treatment loan repayments drop substantially. While 86% of all loans are repaid in periods 1–15, this falls to less than 50% in the last five periods of the RID–CR treatment. In contrast, the repayment rate in the RID–NO treatment hovers around 30% for the entire duration of the experiment. As predicted, in the final periods of the experiment the repayment rate in the RID–CR treatment converges to that of the RID–NO treatment. These findings support our conjecture that high repayment rates in earlier phases of the RID–CR treatment are due to the reputation effects of the credit registry.

A regression analysis confirms the strong impact of the credit registry on repayment rates in the RID–CR treatment. Table 4 presents the results of a probit analysis of borrowers repayment decisions in the RID–CR (column 1) and RID–NO (column 2), controlling for credit conditions and the phase of the experiment in which the credit transaction takes place. Our dependent variable is a dummy variable "Repayment," which is 1 if a borrower repaid and 0 if he defaulted. We control for the size of loans and the interest rate by including the variables "Credit size" and "Interest rate." We also control for time effects by including 3 dummy variables "Period 6–10," "Period 11–15," and "Period 16–20," which are 1 only for observations within the respective phase of the experiment. Our regression analysis identifies a negative time effect on repayment behavior in the RID–CR treatment. The coefficients of "Period 11–15" and "Period 16–20" are both negative and significant, with the latter more pronounced than the former. This result confirms our conjecture that the presence of a credit registry creates reputation incentives to repay loans in earlier periods of

TABLE 4
REPAYMENT BEHAVIOR: RANDOM ID TREATMENTS

	(1) RID–CR	(2) RID–NO
Interest rate	−0.046**	−0.013**
	(2.95)	(5.61)
Credit size	−0.011	−0.008
	(1.06)	(0.91)
Period 6–10	−0.021	0.06
	(0.12)	(0.41)
Period 11–15	−0.459**	−0.34
	(2.83)	(1.45)
Period 16–20	−1.296**	−0.199
	(6.48)	(0.60)
Constant	3.226**	0.126
	(3.56)	(0.48)
Observations	656	411

NOTES: The table reports probit estimates of the repayment choice per borrower and period for the RID–CR and RID–NO treatments. The dependent variable in both columns is a dummy variable that is 1 only if the borrower chooses to make the desired repayment of the lender. Both regressions estimate coefficients for the following explanatory variables: *Interest rate*: $\frac{(\text{Desired repayment}-\text{Credit size})}{\text{Credit size}} \times 100$. *Credit size* is size of credit received. *Period 6–10* is a dummy variable for all transactions that took place in periods 6–10. *Period 11–15* is a dummy variable for all transactions that took place in periods 11–15. *Period 16–20* is a dummy variable for all transactions that took place in periods 16–20. In both regressions the *T*-statistics reported in parentheses are based on standard errors adjusted for clustering at session level. *indicates significance at the 5% level; **indicates significance at the 1% level.

the RID–CR treatment. We find no corresponding time effect on repayment rates in the RID–NO treatment. The negative and significant coefficient on "Interest rate" in both columns suggests that the "fairness" of a credit offer does affect the probability of repayment. Lenders who demand higher interest rates are less likely to be repaid in both treatments. The insignificant coefficient of "Credit size" in both treatments suggests, in contrast, that repayment behavior does not vary with the volume of credit received.

Figure 1 shows that the fall in repayment rates in the final periods of the RID–CR treatment is mirrored by a substantial decline in lending activity. The figure reports the realized credit volume per period as a percentage of the maximum credit volume for the RID–CR and RID–NO treatments.[17] In the RID–CR treatment the total volume of credit rises from 64% in period 1 to 92% in period 12 and remains above 80% until period 17. As predicted, credit volume then falls in the final periods of the RID–CR treatment. Surprisingly, the RID–NO treatment starts off with a similar credit volume to that of the RID–CR. However, in this treatment lending activity falls rapidly, declining to less than 30% of its potential from period 9 onward. Again, confirming our predictions we find that the lending volume in the RID–CR treatment converges to that of the RID–NO in the final periods of the experiment.

Regression results also confirm that lenders anticipate the fall in repayment incentives in the RID–CR treatment over time. Looking back to Table 3 we see that

17. As the maximum loan size was 50 units and seven loans were possible in each period, the maximum credit volume per period in a session was 350 units.

borrowers in the RID–CR treatment are less likely to receive credit in the final five periods of the experiment than beforehand, even controlling for their credit history (see negative coefficient of "Period 16–20" in column (1) of Table 3).

Our results in this section suggest that, in the absence of alternative disciplining devices, credit reporting can strongly motivate borrowers to repay loans. Lenders use the information available from the credit registry to condition lending terms on borrowers prior repayment behavior. By doing so they generate strong reputation incentives for borrowers to repay loans at least in early phases of the experiment. High repayment rates make it feasible for lenders to extend high credit volumes, despite the fact that repayment is not third-party enforceable. Strong competition among lenders implies that all surplus generated by credit reporting is reaped by borrowers.

3.2 FID Treatments

In this section, we examine the impact of credit reporting on repayment behavior when there is an alternative mechanism to motivate repayment: bilateral banking relationships. Our predictions suggest that in this environment credit reporting may not necessarily enhance repayment incentives and credit market volume. However, it might alter the structure of trade by reducing the prevalence of bilateral relations, and also limit the ability of lenders to hold up borrowers in bilateral relations.

Table 5 displays summary statistics for our two treatments in which bilateral relationships are feasible due to fixed identities of all borrowers and lenders throughout the experiment: the FID–CR and FID–NO treatments. The table presents treatment means as well as the variation of results across sessions. Our results show only negligible differences in market outcome between the two treatments. The repayment rate of

TABLE 5

SUMMARY STATISTICS: FIXED ID TREATMENTS

Treatment	FID–CR			FID–NO		
	Mean	Max	Min	Mean	Max	Min
Repayment rate	0.79	0.86	0.72	0.74	0.79	0.67
Realized contracts (%)	93.9	98.6	89.3	91.6	97.9	85.0
Credit size	42.2	43.3	41.5	40.3	42.6	37.8
Credit volume (%)	79.3	82.4	75.6	73.9	81.4	68.6
Interest rate (%)	29.2	36.8	23.1	26.0	31.4	21.5
Payoff lender	49.9	52.4	47.8	48.5	49.2	47.7
Payoff borrower	49.8	51.7	47.7	49.1	51.9	46.7
Contract renewal ratio	0.39	0.55	0.22	0.48	0.56	0.35

NOTES: The table presents summary statistics for the FID–CR and FID–NO treatments. For each statistic the table presents the mean value across the five sessions of each treatment. In addition, the table presents the highest mean value (Max) and lowest mean value (Min) of an individual session. *Repayment rate* is the share of credit transactions in which the borrower made the desired repayment of the lender. *Realized contracts* is the percentage of potential contracts (7 per session and period) which were realized. *Credit size* is the mean size of credit extended in realized credit transactions. *Credit volume* is the percentage of the potential credit volume ($7 \times 50 = 350$ per session and period) which was realized. *Interest rate* is the mean of the calculated interest rate per transaction: $\frac{(\text{Desired repayment} - \text{Credit size})}{\text{Credit size}} \times 100$. *Payoff lender* is the payoff per lender and period. *Payoff borrower* is the payoff per borrower and period.

borrowers is very high in both the FID–CR (79%) and the FID–NO (74%) treatment, with little variation across sessions in either treatment. Our results also show little differences in lending activity between the FID–CR and FID–NO treatments. In the FID–CR treatment, 94% of all potential lending contracts are realized and the average credit size is 42, while in the RID–NO 92% of potential contracts are realized, with an average credit size of 40 points. Interest rates and the distribution of gains from trade are also very similar in the FID–CR and FID–NO treatments. The mean interest rate demanded by lenders is only slightly higher in the FID–CR (29%) than the FID–NO (26%). Similarly to our results for the random ID treatments, we find that in both the FID–CR and FID–NO treatments all gains from trade are reaped by the borrowers. Lenders earn average payoffs which are very close to their outside option of 50. In contrast, borrowers earn average payoffs which well exceed their outside option of 10 points per period. This result confirms that the excess supply of credit in our experiment did induce a highly competitive credit market. Mann–Whitney tests based on session averages suggest that the slight differences in repayment rates, credit volume and interest rates between the FID–CR and FID–NO treatments are not statistically significant.[18]

The high repayment rates presented in Table 5 suggest that the incentives for borrowers to repay loans are strong in both the FID–CR and FID–NO treatments. Indeed, our data show that reputation incentives were very strong in both treatments, as lenders conditioned their loan offers on past repayment behavior. Table 6 replicates our regression analysis from Table 3 relating borrowers' access to credit to their prior repayment rate, now using data from the FID–CR and FID–NO treatments. Again, we examine the impact of a borrower's "Prior repayment rate" on his probability of sealing a "Contract," the "Credit size" he gets, and the "Interest rate" he pays. Columns (1–3) show results using data from the FID–CR treatment, while columns (4–6) show results for the FID–NO treatment. In all regressions the main explanatory variable is a borrower's "Prior repayment rate," which measures the share of previous loans which he repaid. We control for time effects in all regressions by including three dummy variables "Period 6–10," "Period 11–15," and "Period 16–20," which are 1 only for observations within the respective phase of the experiment. The significant coefficient of "Prior repayment rate" in all columns shows that in both the FID–CR and the FID–NO treatment borrowers with good credit records are more likely to obtain credit, receive larger loans and pay lower interest rates.

18. We conduct a Mann–Whitney test using mean repayment rates per session as observations. The five sessions of the FID–CR treatment display repayment rates of 86, 82, 78, 76, and 72%, respectively. In the FID–NO treatment the five sessions have repayment rates of 79, 77, 76, 72, and 67%, respectively. A two-sided test thus cannot reject the hypothesis that repayment equally frequent in the two treatments ($p = 0.22$). We further conduct a Mann–Whitney test using realized credit volume per session as observations. In the FID–CR treatment the five sessions display a credit volume (measured in percentage of the total potential volume) of 82, 82, 80, 76, and 76%, respectively. In the FID–NO treatment, the credit volume per session was 81, 78, 72, 69, and 69%, respectively. A two-sided test thus yields a p-coefficient of $p = 0.158$. We finally conduct a two-sided test using mean interest rates per session as observations. In the FID–CR treatment the five sessions display a mean interest rate of 37, 35, 28, 24, and 23%, respectively. In the FID–NO treatment, the mean interest rate per session was 31, 26, 26, 25, and 22%, respectively. A two-sided test thus yields a p-coefficient of $p = 0.55$.

TABLE 6

REPAYMENT HISTORY AND CREDIT ACCESS: FIXED ID TREATMENTS

	FID–CR			FID–NO		
	(1) Contract	(2) Credit size	(3) Interest rate	(4) Contract	(5) Credit size	(6) Interest rate
Prior repayment rate	2.671**	24.507**	−32.495*	3.378**	20.914**	−23.363*
	(9.33)	(10.51)	(4.03)	(7.55)	(6.00)	(2.93)
Period 6–10	−0.625	2.422*	−2.861	−0.456	4.178**	1.074
	(1.56)	(2.96)	(2.16)	(1.06)	(4.98)	(0.84)
Period 11–15	−0.651*	4.608**	−5.186**	−0.982*	4.333**	0.432
	(2.14)	(7.42)	(4.89)	(2.20)	(7.33)	(0.18)
Period 16–20	−1.821**	3.213	−5.207	−1.964**	3.684*	0.029
	(4.83)	(1.65)	(1.97)	(4.28)	(3.92)	(0.01)
Constant	0.682	19.624**	59.489**	0.364	21.294**	43.736**
	(1.65)	(7.00)	(17.04)	(1.51)	(7.18)	(7.80)
Observations	665	622	622	665	606	606
R^2		0.27	0.13		0.21	0.11

NOTES: The table reports regression estimates using credit access data per borrower and period from the FID–CR and FID–NO treatments. Column (1) and (4) report probit estimates for *Contract* (probability of sealing a contract). Column (2) and (5) report OLS estimates for *Credit size* (size of credit received) using data only for borrowers who sealed a credit contract in a particular period. Column (3) and (6) report OLS estimates for *Interest rate* $\frac{(\text{Desired repayment}-\text{Credit size})}{\text{Credit size}} \times 100$, again using data only for borrowers who sealed a credit contract in a particular period. All six regressions estimate coefficients for the following explanatory variables: *Prior repayment* rate is the ratio of prior credit transactions in which the borrower made the repayment desired by the lender. *Period 6–10* is a dummy variable for all transactions that took place in periods 6–10. *Period 11–15* is a dummy variable for all transactions that took place in periods 11–15. *Period 16–20* is a dummy variable for all transactions that took place in periods 16–20. In all regressions the T-statistics reported in parentheses are based on standard errors adjusted for clustering at session level. *indicates significance at the 5% level; **indicates significance at the 1% level.

Even if borrowers have strong reputation incentives to repay loans in the FID–CR and FID–NO treatments, these incentives should wear off toward the end of the experiment. We therefore expect a significant decline in repayment rates in both treatments in the final phase of the experiment. Figure 2 shows that this is indeed the case. The repayment rate in both treatments is around 80% from the beginning of the experiment until period 17. In the final three periods, we then see a significant fall in repayment rates to below 50%. This decline in repayment rates is mirrored by a substantial drop in the volume of extended by lenders. Figure 2 shows that prior to period 18 well above 70% of the potential credit volume per period was extended. The credit volume then declines rapidly and reaches just 30% in the final period.

Table 7 provides a detailed analysis of the repayment behavior of borrowers in the fixed ID treatments, replicating our analysis of the Random ID treatments in Table 4. The table presents the results of a probit analysis of borrowers repayment decisions in the FID–CR (column 1) and the FID–NO (column 2), controlling for credit conditions and the phase of the experiment in which the credit transaction takes place. The dependent variable is a dummy variable "Repayment," which is 1 if a borrower repaid and 0 if he defaulted, and we control for the size of loans and the interest rate by including the variables "Credit size" and "Interest rate." We again control for time effects by including three dummy variables "Period 6–10," "Period 11–15," and "Period 16–20," which are 1 only for observations within the respective phase of the experiment. T-statistics reported in parentheses are again based on robust standard errors and adjusted for clustering at session level. We find a negative and

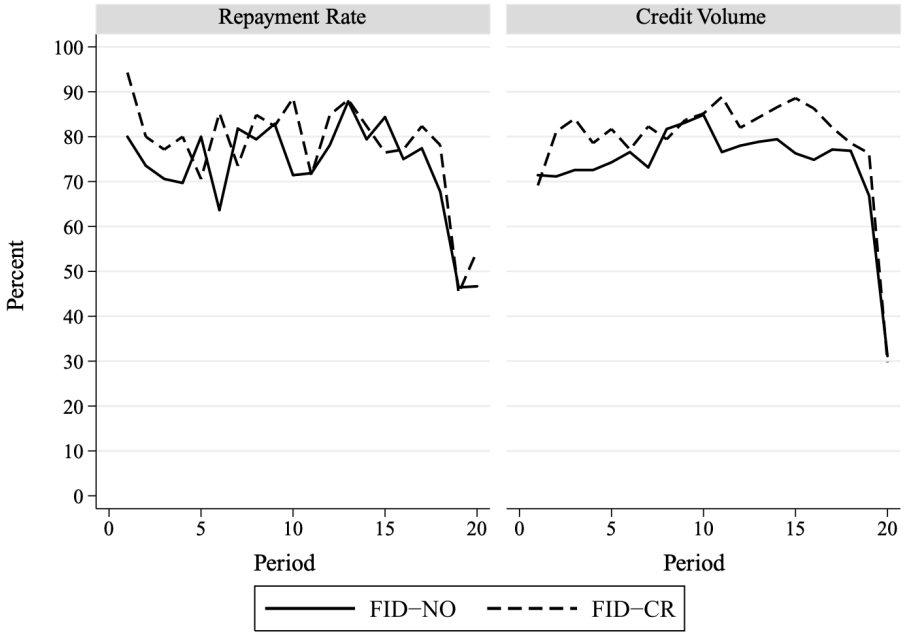

FIG. 2. Repayment Rate and Credit Volume: Fixed ID Treatments.

TABLE 7

REPAYMENT BEHAVIOR: FIXED ID TREATMENTS

	(1) FID–CR	(2) FID–NO
Interest rate	−0.02**	−0.032**
	(4.04)	(4.98)
Credit size	−0.006	0.019**
	(0.85)	(2.81)
Period 6–10	0.085	0.03
	(0.82)	(0.18)
Period 11–15	−0.042	0.152
	(0.35)	(0.63)
Period 16–20	−0.415*	−0.427*
	(2.12)	(2.54)
Constant	1.79**	0.85
	(3.53)	(1.92)
Observations	657	641

NOTES: The table reports probit estimates of the repayment choice per borrower and period for the FID–CR and FID–NO treatments. The dependent variable in both columns is a dummy variable that is 1 only if the borrower chooses to make the repayment desired by the lender. Both regressions estimate coefficients for the following explanatory variables: *Interest rate*: $\frac{(\text{Desired repayment}-\text{Credit size})}{\text{Credit size}} \times 100$. *Credit size* is size of credit received. *Period 6–10* is a dummy variable for all transactions that took place in periods 6–10. *Period 11–15* is a dummy variable for all transactions that took place in periods 11–15. *Period 16–20* is a dummy variable for all transactions that took place in periods 16–20. In both regressions the *T*-statistics reported in parentheses are based on standard errors adjusted for clustering at session level. *indicates significance at the 5% level; **indicates significance at the 1% level.

significant coefficient on the variable "Period 16–20" in both columns, showing that borrowers were less likely to repay a given loan in the final phase of the experiment in both treatments. This result supports our hypothesis that the high repayment rates in the two treatments are due the reputation incentives created by bilateral relations (FID–NO) and the presence of a credit registry (FID–CR).

Table 7 also displays an interesting discrepancy between repayment behavior in the FID–CR and FID–NO treatments. In the FID–CR treatment the probability of repaying a loan was independent of the agreed loan size. In the FID–NO treatment, however, our results suggest that the repayment rate is significantly dependent on the loan size offered to the borrower. In this treatment the coefficient on "Credit size" is positive and highly significant. These results suggest that credit reporting does alter repayment incentives even when bilateral relations are feasible. Without a credit registry borrowers will only repay loans if the relation with their *current* lender is valuable to them. The borrower is thus more likely to repay the higher his expected earnings from future loan contracts from this lender, which depend on future loan sizes and interest rates. If the current loan size is an indicator of future loan sizes then it is rational for the borrower to condition his repayment choice on the credit size. In contrast, the presence of the credit registry in the FID–CR treatment creates incentives to repay loans irrespective of the current credit size. The reason for this is that the credit registry, as implemented in our experiment, only provides partial information to future prospective lenders. The registry only reports the prior repayment behavior of borrowers, but not the conditions of the loans which they repaid or defaulted upon. Given that a borrower's public reputation is dependent on his repayment behavior, there are strong incentives to repay even small loans in the FID–CR treatment. Indeed the borrower should be particularly concerned about his public reputation if he is not interested in pursuing a bilateral relationship with his current lender. The reasoning above would also imply that borrowers in the FID–CR treatment would also condition their repayment choice less on the interest rate than borrowers in the FID–NO treatment. This is indeed the case. In both treatments we find a negative and significant coefficient on "Interest rate" suggesting that the "fairness" of a credit offer affects the probability of repayment. However, in the FID–CR treatment the impact of the interest rate on repayment behavior is substantially weaker than in the FID–NO treatment. This result again shows that the repayment incentives of borrowers are altered by the presence of credit reporting.

Our predictions suggest that the presence of a credit registry in the FID–CR treatment may lead to a different trading structure than in the FID–NO treatment. We expect that the FID–NO treatment will be pervaded by long-term relationships as bilateral relationships are the key to disciplining borrowers. In contrast, the existence of a credit registry in the FID–CR implies that long-term relationships between particular borrowers and lenders are not necessary to discipline borrowers. We thus predict that there will be fewer relationships in the FID–CR treatment than in the FID–NO treatment. The summary statistics presented in Table 5 show that this is the case. The final line of that table reports the ratio of contracts that are renewed from one period to another, i.e., the share of transactions that involved the same lender–borrower pair

as in the previous period. In the FID–NO treatment this ratio of renewed contracts is 48%. Thus roughly half of all loans made in this treatment involve the same lender and borrower as in the previous period. The table, however, also shows that contract renewals are also quite common even when a credit registry exists. Indeed, nearly 40% of all contracts in the FID–CR treatment are renewed from one period to another. As predicted, the share of renewed contracts is lower in the FID–CR than that in the FID–NO treatment. However, due to the strong variation across sessions displayed in Table 5, this difference is only of borderline significance.[19]

The fact that bilateral relations are so frequent in the FID–CR treatment is surprising. Although lenders have access to a credit registry in this treatment, it seems that they still rely strongly on credit relationships to motivate loan repayment. This finding is less surprising when we compare the information available within a relationship to that available from a credit registry. Within a long-term relationship, lenders typically have much more information about a borrower than they could elicit from a credit report. In our experiment this is also the case. Our credit registry only provided information on whether a borrower repaid a loan or not. Within a relationship, however, the lender had additional information on contract terms (credit size, repayment size) that a lender had accepted and repaid. Our results suggest that this additional information encouraged lenders to maintain relationships with a particular borrower, although they could easily obtain the credit record of each borrower at no cost.

We conclude our results section by investigating the impact of credit reporting on the distribution of gains from trade in bilateral relations. Padilla and Pagano (1997) suggest that information sharing between lenders may mitigate the "hold-up" problem in banking relationships. The presence of a credit registry or credit bureau implies that competitors are better informed about a borrower's quality, and thus limits the potential for incumbent banks to extract all informational quasi-rents generated within a relationship. Given that relationships arise in both our FID–CR and FID–NO treatments, we can test whether the presence of the credit registry in the FID–CR does alter the distribution of gains from trade in relationships. Table 8 reports the results of a regression analysis in which we relate lenders' payoffs per credit transaction to the duration of the relationship in which the credit transaction took place. The dependent variable for all regressions in the table is "Lender Profit," which captures the payoff of a lender per credit transaction in excess of his outside option of 50, i.e., the rent he earns per transaction. Our first explanatory variable, "Short-term," is 1 only for all transactions which take place in relationships with a final duration of less than three periods. Our second explanatory variable, "Long-term," is 1 only for all transactions which take place in relationships with a final duration of at least three periods. As we include both of these dummy variables but no constant to our regression, the two dummies identify the mean rent in short- and long-term relationships. We control for the phase of the experiment in which a transaction takes place by including the

19. In the five sessions of the FID–NO, average renewal rates are 56, 56, 55, 38, and 35%, respectively. In comparison, the five FID–CR sessions have renewal rates of 55, 43, 42, 31, and 22%, respectively. A one-sided Mann–Whitney test using these session averages as observations yields a p-value of $p = 0.11$.

TABLE 8

RELATIONSHIPS AND LENDERS' RENTS IN FIXED ID TREATMENTS

	(1) FID–CR	(2) FID–NO	(3) FID–CR and FID–NO
Short-term	−3.039	−9.31**	−10.295**
	(1.8)	(5.21)	(5.5)
Long-term	5.391*	10.25**	9.106**
	(3.17)	(6.21)	(8.52)
Period 6–10	1.875	−2.763	−0.262
	(2.07)	(1.23)	(0.2)
Period 11–15	−0.402	−2.282	−1.18
	(0.33)	(1.22)	(1.14)
Period 16–20	−7.157	−7.644**	−7.250**
	(2.21)	(4.88)	(4.14)
CR ∗ Short-term			7.967*
			(2.89)
CR ∗ Long-term			−2.886*
			(2.28)
Observations	657	641	1,298
R^2	0.04	0.20	0.12

NOTES: The table reports OLS estimates using payoff data per lender and period from the FID–CR and FID–NO treatments. The dependent variable in all three columns is lender profit per period (lender profit = payoff − 50). Column (1) uses data from the FID–CR treatment, column (2) uses data from the FID–NO treatment, and column (3) uses data from both treatments. Columns (1) and (2) estimate coefficients for the following explanatory variables: *Short-term* is a dummy variable that is 1 only for transactions where the final duration of the relation between a lender and his current borrower is less than three periods. *Long-term* is a dummy variable that is 1 only for transactions where the final duration of the relation between a lender and his current borrower is at least three periods. *Period 6–10* is a dummy variable for all transactions that took place in periods 6–10. *Period 11–15* is a dummy variable for all transactions that took place in periods 11–15. *Period 16–20* is a dummy variable for all transactions that took place in periods 16–20. Column (3) additionally estimates coefficients for the following explanatory variables: *CR ∗ Short-term* is a dummy variable that is 1 only for all short-term transactions that took place in FID–CR treatment. *CR ∗ Long-term* is a dummy variable which is 1 only for all long-term transactions that took place in FID–CR treatment. All three regressions are estimated without constants, and the *T*-statistics reported in parentheses are based on standard errors adjusted for clustering at session level. *indicates significance at the 5% level; **indicates significance at the 1% level.

three dummy variables "Period 6–10," "Period 11–15," and "Period 16–20," which are 1 only for obervations within the respective phase of the experiment. Column (1) reports estimation results for the FID–CR treatment, while column (2) reports results for the FID–NO treatment.

The results in Table 8 show that lenders earn positive rents in both treatments from long-term relations. Our results suggest that on average lenders earn rents of 10.3 points per period from long-term relationships in the FID–NO treatment, and 5.4 points per period in the F–CR treatment. The fact that the coefficient of "Long-term" is lower and less precise in column (1) for the FID–CR treatment suggests that credit reporting does reduce the ability of lenders to extract rents from long-term relationships. In order to test the significance of this result we pool the data from the FID–CR and FID–NO treatments and repeat our regression analysis, including the interaction term "CR ∗ Long-term," which is 1 only for long-term interactions in the FID–CR treatment. We further include the interaction term "CR ∗ Short-term," which is 1 only for short-term interactions in the FID–CR treatment. In this pooled regression the interaction term "CR ∗ Long-term" captures the difference in rents earned by lenders in long-term treatments between treatments. We expect a negative coefficient on this term if the presence of a credit registry reduces lenders' ability to extract profits in bilateral relations. The results reported in column (3) show that the interaction term does yield a significantly negative coefficient. This confirms theoretical predictions

suggesting that information sharing between lenders can mitigate "hold-up" issues in the credit market.

Table 8 further shows that lenders do not earn positive rents from short-term relations in either treatment. Interestingly though, while lenders make significant losses in short-term relations in the FID–NO treatment (9.3 points per period), this does not seem to be the case in the FID–CR treatment. The lower and insignificant coefficient of "Short-term" in column (1) suggests that the credit registry helps lenders to avoid substantial losses in short-term encounters. The difference in earnings from short-term interactions between the treatments is confirmed by the positive and significant interaction term "CR ∗ Short-term" in column (3) of Table 8. These findings suggest a further benefit of information sharing, even when relationship banking is feasible: it helps lenders to reduce losses in one-off transactions by avoiding encounters with borrowers who may not repay their loans.

4. CONCLUSIONS

In this paper, we applied experimental methods to examine the impact of information sharing between lenders on the repayment behavior of borrowers in a competitive credit market. Our results suggest that the impact of credit reporting on repayment behavior and credit market performance depends strongly on the feasibility of relationship banking as an alternative disciplining device. Credit reporting is highly valuable in markets where banking relationships are difficult to establish, for example, due to highly mobile borrowers. In such markets, the disciplining of borrowers to repay loans is strongly dependent on the existence of an institutionalized information-sharing mechanism. By contrast, in markets where relationship banking is prevalent, these relationships may themselves motivate repayment, so that credit reporting has little impact on borrower behavior. However, even when the presence of relationship banking implies that credit reporting does not substantially improve credit market performance, it does alter market structure and the distribution of gains from trade. The presence of an information sharing mechanism implies that the disciplining of borrowers is less reliant on bilateral relations, so that the trading pattern is characterized by fewer long run relationships. Moreover, information sharing implies that the information advantage of incumbent lenders over "outside" lenders about borrowers is reduced, weakening their ability to hold-up borrowers in relationships.

Our methodology and results suggest several avenues of future research. First, experimental methods could be applied to study the endogenous emergence of credit reporting. Theoretical models (Klein 1992, Pagano and Jappelli 1993) suggest that information sharing is more likely to emerge when it is most valuable to lenders. Experimental methods would make it possible to examine this hypothesis by studying the emergence of information sharing in a variety of market environments. Experimental methods could also be applied to study alternative designs of credit bureaus and credit registries. As suggested by theoretical work (Vercammen 1995, Padilla