Sharing Information in the Credit Market: Contract-Level Evidence from U.S. Firms

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Abstract

We investigate the impact of lenders' information sharing on firms' performance in the credit market using contract-level data from a major U.S. credit bureau. The staggered entry of lenders into the bureau, the richness of the data set (28,000 loans and leases extended to roughly 4,000 businesses), and the small and medium size of borrowing firms offer a suitable natural experiment to identify the effect of lenders' improved access to information. In line with the predictions of Pagano and Jappelli (1993) and Padilla and Pagano (1997, 2000), we find that information sharing reduces firms' delinquencies on loans and leases, and that this effect is more pronounced for informationally opaque and risky businesses. The results also document that information sharing induces creditors to grant smaller loans and to demand more guarantees. Thus, information sharing appears to improve firms' repayment performance but not necessarily leads financiers to loosen their lending standards.

JEL Codes: D82, G21.

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1 Introduction

In recent decades, a broad consensus has formed that most credit market failures (such as credit rationing) are attributable to information asymmetries between lenders and borrowers. Although the advances in information and communications technology are enhancing lenders' ability to acquire information on customers, the credit frictions stemming from poor information are likely to persist into the future.[1] A response to these problems that is gaining ground is the development of institutional arrangements through which lenders share their proprietary information one with another. An example of such arrangements are public credit registers and private credit bureaus, "informational intermediaries" that collect data on credit histories of firms and consumers and make them available to their members upon request. Credit registers and bureaus are numerous in developed countries (Jappelli and Pagano, 2002) and their presence is becoming pervasive in emerging and transition economies (Brown, Jappelli and Pagano, 2009).[2]

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[1] See, e.g., Petersen and Rajan (2002) for more on this issue.

[2] Conducting a survey of 43 countries, Jappelli and Pagano (2002) show that before 1950 less than 5 percent of the countries had a private credit bureau while in 2000 over 60 percent had one bureau at least.

The benefits and costs of lenders' information sharing have been the object of increasing attention in the last twenty years or so. The conclusions reached by the theoretical literature suggest that lenders' information sharing generally improves the performance of borrowers, although the results are not always clear-cut. The seminal work of Pagano and Jappelli (1993) demonstrates that the information on the past behavior of borrowers made available by a credit bureau facilitates lenders' screening of credit applicants. Furthermore, lenders' information sharing can discipline entrepreneurs (Padilla and Pagano, 2000) and deter lenders from holding up borrowers (Padilla and Pagano, 1997), thus stimulating entrepreneurial effort. These mechanisms imply that the entry of a lender into a credit bureau should trigger an improvement in the repayment performance of its borrowers, such as a reduction in their delinquency rate. However, it is also acknowledged that the availability of information from other lenders can induce financiers to grant credit to riskier firms, resulting into more severe delinquency (Pagano and Jappelli, 1993). Clearly, establishing the impact of lenders' information sharing and gauging its magnitude can not only yield valuable insights into the functioning of the credit market, especially the consequences of information asymmetries, but also produce important normative implications. For instance, policy makers debate the optimal design of public credit registers and whether private credit bureaus should be subsidized.

The objective of this paper is to shed new light on the impact of information sharing on firms' performance in the credit market exploiting unique and unusually rich contract-level data from a U.S. credit bureau. The testing ground of our analysis consists of the equipment finance industry. At the end of 2000, PayNet Inc., a company engaged in providing predictive analytics for credit risk management, launched the "Payment Information Network", a credit bureau aimed at serving this industry. PayNet applied a standard principle of reciprocity: it requested equipment finance companies interested in pooling information to release data on their own customers in exchange for the right to retrieve credit reports on credit applicants. In the years that followed the inception of the bureau at the end of 2000, several major equipment finance companies underwent the process necessary to join the credit bureau. This involved an up-front investment in technological infrastructure, negotiating an agreement with PayNet administrators, addressing legal issues arising from the disclosure of clients' confidential information, and developing a protocol for processing the information made available by the bureau. Importantly for our purposes, lenders completed these procedures at different speeds and, hence, joined the PayNet credit bureau on different dates: for instance, the 15 lenders investigated in this paper became members of the bureau in a period spanning from August 2001 to March 2004. Exploiting the staggered entry of lenders into the credit bureau as a natural experiment, we can isolate the contribution of the entry of a lender on the performance of its credit contracts from changes in the conditions of the economy and of the industry.

Our data consist of a random sample of the PayNet Information Network containing about 28,000 loans and leases extended by 15 major equipment finance companies to roughly 4,000 businesses. The data set includes information on the performance of equipment finance contracts originated both before and after lenders' entry into the credit bureau. This information comprises several measures of the rates of delinquency and foreclosure, such as the number of days payments were late and the frequency of mild or serious delinquencies. The data also inform us on the date of entry of each lender into the bureau, thus allowing us to exploit the staggered entry of lenders as a natural experiment. Finally, the sample offers details on the characteristics of firms and contractual arrangements. Most borrowers are small businesses, which is ideally suited for our purposes. In fact, small businesses are supposedly informationally opaque because they are not monitored by rating agencies or the financial press (Petersen and Rajan, 1994). Therefore, the benefits from joining a credit bureau should be particularly strong for lenders financing such firms.

Consistent with the predictions of Pagano and Jappelli (1993) and Padilla and Pagano (1997, 2000), we find that, after controlling for lender and firm attributes and for macroeconomic effects, the entry of lenders into the credit bureau reduced the incidence of delinquencies and foreclosures on their contracts. This result is robust to using a broad array of measures of contract performance as well as alternative econometric specifications. The effects are also sizeable: for instance, a lender that joined the bureau experienced a drop of 13 percent (at the sample mean) in the average number of days a borrower was late on its payments. In further tests, we take a closer look at the mechanism through which information pooling affected borrowers' performance and uncover evidence that the impact on serious delinquencies was more pronounced for small and low-rating businesses. This result is in line with expectations and reveals that sharing information on informationally opaque (e.g., small) firms or high risk (low rating) ones is particularly helpful to lenders. An additional question that our data help address is whether the availability of more accurate information leads financiers to loosen or tighten their lending standards. We document that their entry into the credit bureau induced creditors to grant smaller loans or leases and to demand more guarantees, both signals of tighter lending standards. This finding could match the prediction of Bennardo, Pagano and Piccolo (2008) that, absent information sharing arrangements, firms easily obtain large and non-guaranteed loans from multiple financiers, while this is no longer feasible when the financiers become aware of firms' debt exposure to other lenders. In conjunction with the results on borrowers' delinquency, this could also hint that lenders rather than borrowers gained the most from the bureau.

The plan of the paper is as follows. In the next section, we relate the analysis to the prior literature. Section 3 presents the setting of the empirical analysis, describing the equipment finance industry as well as the credit bureau under scrutiny. In Section 4, we draw testable hypotheses from the extant theoretical studies. Section 5 details the empirical methodology and the data. In Sections 6 and 7, we present the main results. Section 8 contains additional tests that explore the impact of information sharing on the form of financial arrangements. Section 9 concludes.

2 Prior Literature

This paper relates to three strands of literature. The first strand theoretically investigates the role of information sharing in the credit market.[3] Pagano and Jappelli (1993) demonstrate that the information conveyed by other financiers allows a lender to better screen credit applicants. They also show that, by mitigating the adverse selection to which safe borrowers are exposed, information pooling may lead to an increase in the volume of lending.[4] Padilla and Pagano (2000) find that information sharing encourages entrepreneurs to exert effort because any delinquency will be disclosed to several lenders. Bennardo, Pagano and Piccolo (2008) study an environment where a firm can borrow from multiple banks and lending by each of them raises its default risk. This, in turn, can cause credit rationing, excessive borrowing or non-competitive lending rates. In such an environment, banks' information sharing about the firm's leverage can reduce delinquency rates and ease the firm's access to credit. Shortly, we shall elaborate on these

[3] A large theoretical literature explores the role of financial intermediaries in screening and monitoring borrowers (see, e.g., Diamond, 1984, and, for a detailed review, Gorton and Winton, 2003).

[4] Pagano and Jappelli (1993) also argue that the affiliation to a credit bureau allows a lender to obtain more accurate information about credit applicants but reduces its informational advantage vis-à-vis competitors.

theoretical models and their predictions.

The second stream of studies investigate the consequences of information exchange empirically. Most of these studies rely on aggregate, cross-country data and explore whether the presence of public credit registers and private credit bureaus in a country is associated with a different development of the credit market and a different tightness of credit constraints. Jappelli and Pagano (2002) consider a panel of 43 countries and document that in countries with more credit registers default rates are lower and bank credit is more abundant. Examining a set of 129 countries, Djankov, McLiesh and Shleifer (2007) obtain that especially in poor countries the presence of credit registers positively correlates with the ratio of private sector credit to GDP and that this ratio rises following the introduction of registers. Love and Mylenko (2003) use cross-country, firm-level data and obtain that the presence of credit registers and bureaus in a country lowers the perceived incidence of financing constraints, with this effect being differentially stronger for small and young firms.[5] While insightful, the analyses that employ aggregate data in cross-country empirical settings are exposed to measurement and endogeneity issues. For example, in such settings it is problematic to control for country specific institutions and laws that affect credit market performance.

Probably because of a dearth of data, very few studies provide microeconometric evidence on the implications of lenders' information sharing. Kalberg and Udell (2003) describe the functioning of a private business information exchange and examine whether the credit performance score compiled by this information exchange helps predict the probability of firms' failure. Luoto, McIntosh and Wydick (2007) focus on the provision of microfinance to solidarity groups and poor households in a developing country. Using branch-level data from a microfinance institution (MFI), they estimate that the introduction of a credit bureau in Guatemala improved the average performance of loans in the branches. Brown and Zehnder (2007) offer experimental evidence that a credit register motivates borrowers to repay loans. They implement an experimental credit market and show that if borrowers and lenders interact only once the credit market will collapse, possibly because lenders fear that borrowers will default. The introduction of a credit register in this context raises repayment rates and the amount of credit granted. The contribution of our study to the empirical literature is unique in its kind in that we can exploit the natural experiment generated by the staggered entry of lenders into a credit bureau in conjunction with rich contract-level data on U.S. businesses.

3 Institutional Background and Data

In this section, we highlight the main features of the equipment finance industry and the credit bureau investigated in the empirical analysis.

3.1 The Industry

Equipment finance plays a critical role in allowing firms to expand their capital stock. Equipment finance companies provide firms in all industries with a variety of financing products for acquiring and employing plants, equipment and software. According to the Equipment Leasing and Finance Association (ELFA), the trade association that serves as the advocate for the equipment finance industry to the U.S. federal government, loans, leases and other financial instruments offered by equipment finance companies account for over 50 percent of the nonresidential investment made

[5] Galindo and Miller (2001) obtain similar results exploring the impact of public credit registers and private credit bureaus in over 30 countries.

by U.S. businesses, non-profits and government agencies each year. Moreover, according to ELFA, roughly 80 percent of U.S. firms use equipment finance to fund their operations. In addition to originating loans, leases, and other contracts, equipment finance companies participate in primary and secondary market financing activities and facilitate to customers the access to services such as management of physical assets, advice on technology updates, and help with disposal of old equipment.

Financial services companies include commercial banks, independent finance and leasing companies, captive finance companies, diversified financial services companies, as well as broker/packagers and investment banks.[6] Depending on the amount due, equipment finance contracts are classified into small-ticket, when less than a quarter of a million dollars, middle-ticket, when between a quarter of a million and five million, and big-ticket when over five million dollars. Traditionally, the lease has been the most commonly used type of contract. In a lease, the equipment finance company (lessor) owns the equipment and rents it to the customer firm (lessee), which may have the option to buy the equipment during or after the term of the lease. Recently, however, other contracts have risen in importance and, according to ELFA, they now amount to over 50% of the total. These contract varieties include, among others, loans, in which the customer firm owns the equipment; conditional sales, which are essentially leases in which, after the term, the lessor transfers the title to the lessee; and financing leases, which are leases in which the customer has control of the asset for a large portion of its useful life, and also has many of the benefits and costs of ownership, such as maintenance, taxes and insurance. Loans are the most used among these alternative contractual arrangements.[7]

ELFA tracks the pattern of activity of the equipment finance industry by conducting a survey of prominent equipment finance companies.[8] Between 1998 and 2007, on average the net portfolio of the surveyed companies increased by approximately 4.5 percent per year. Originations of new loans fluctuated between a 25% drop in 2000 and a 22.8% rise in 2004. Delinquencies, expressed as a fraction of outstanding receivables, remained fairly stable during this period, with slight declines in 2001 and 2005.

3.2 Data Description

Our source of data is the PayNet "Payment Information Network" database. PayNet is a major company that provides credit risk management tools for business lending and, as stated on its official website, is "designed with the objectives of reducing operating expenses, increasing approval rates, controlling delinquencies, and reducing both actual write-offs and reserving requirements". PayNet provides to commercial lenders and credit card issuers credit ratings on commercial loans, leases and other financial obligations for thousands of privately held small- and medium-sized businesses. At the end of 2000, in partnership with the Equipment Leasing and

[6] ELFA reports that its members finance the acquisition of all types of capital equipment and software such as manufacturing and mining machinery and equipment, vessels and containers, construction and off-road equipment, medical technology and equipment, commercial and corporate aircraft, trucks and transportation equipment, business, retail and office equipment, IT equipment and software. In general, the transportation industries - rail, truck, marine and aircraft - are the most likely to seek external finance for their investment in equipment. For example, in 2006 businesses in truck transportation externally financed almost 80% of their investment.

[7] A loan allows to acquire, use and eventually own equipment after investing in a downpayment. While in a lease the equipment serves as collateral, a loan may require the borrower to pledge assets. In a loan, the user, as owner, bears the risk of equipment obsolescence and devaluation whereas in a lease the lessor bears this risk.

[8] This information is extracted from the ELFA's Monthly Leasing and Finance Index (MLFI-25) for the 2005-2007 period and from the Quarterly Performance Indicators Report (PIR) for the 1998-2004 period. These surveys track the performance of the same companies over time so that they provide a fairly reliable trend analysis.

Finance Association, PayNet launched a credit bureau to serve the equipment finance industry (the "Payment Information Network"). This bureau was the first of its kind in the industry and thus represented a major breakthrough for lenders' ability to share information. Absent credit bureaus, financiers have to either acquire costly information or rely on informal contacts among local managers or loan officers (Jappelli and Pagano, 2006). Before the inception of the "Payment Information Network", equipment finance companies interested in pooling information could resort to very few small credit bureaus that - as we shall elaborate below - only provided trade information on very short-term debt obligations (e.g., the payment of utility bills). Such limited information sharing arrangements were allegedly of little help for lenders' monitoring and screening of borrowers.[9]

In the years following the inception of PayNet, several major equipment finance companies took the steps necessary to join the bureau. This involved an up-front investment in a technological facility, solving the legal issues associated with the disclosure of confidential information on customers, reaching an agreement with PayNet administrators, and developing a protocol to process the credit reports made available by the bureau. In the documents accompanying the introduction of the bureau, the administrators of PayNet stressed that the up-front technological setup was more problematic and time consuming for some equipment finance companies than for others because "a certain percentage of the industry's players have custom systems, which requires a unique [technological] solution." Indeed, a major problem for some institutions was "getting IT resources to do the extractions (of credit reports)". Crucially for the objectives of this paper, because of these different technological and legal hurdles, lenders completed the procedures necessary for becoming part of the bureau at different speeds and ended up joining the bureau on different dates. We thus have strong reasons to believe that lenders' different dates of entry reflect the different technological and legal problems they faced in joining the bureau and not factors endogenous to the performance of borrowers or lenders' expectations regarding the benefits of the bureau. A further element that corroborates this view is that the Equipment Leasing and Finance Association exerted strong pressure on all major equipment financiers to enter the bureau as soon as possible to enhance the comprehensiveness of the bureau. Therefore, it is very likely that the time needed for the technological, infrastructural and legal setup was the binding constraint in determining the date of lenders' entry.

Like other private credit bureaus, PayNet is a "closed" network that operates on a principle of reciprocity: a lender gains access to information in the bureau only after supplying its own information. Specifically, when they join the bureau, lenders release information on their customers to the bureau. In exchange for this, they gain the right to retrieve from PayNet reports on credit applicants, subject to the payment of very small fees.[10] A credit report compiled by the bureau includes "black" or "negative" information on the applicant's past credit delinquencies and defaults and "white" or "positive" information on some of the applicant's characteristics, such as its PayNet credit rating, number of employees, level of indebtedness, and patterns of repayments.[11] Although in the United States loans and leases for business purposes are not

[9] In a 2001 article in the ELT magazine of ELFA ("Better credit data, better credit decisions"), an expert of the Equipment Leasing Association of America defined the PayNet bureau as a "breakthrough source of more relevant lessee credit history".

[10] PayNet reports are delivered in two ways, PayNet On-Line and PayNet Direct. Members enter the PayNet On-Line site through a login/password-protected section. The member queries for the applicant and chooses from a menu of products. The report is then delivered to the screen for review and printing. In case multiple companies are identified from the inquiry, they will all be displayed to the member. PayNet data can also be sent directly to the credit-scoring process through PayNet Direct, a CPU-to-CPU link.

[11] Some credit bureaus only provide information about arrears and defaults while others inform on characteristics of borrowing firms such as assets and liabilities, guarantees, history of repayments and debt maturity structure.

subject to the same privacy regulation as consumer loans, lenders systematically provide privacy notifications to businesses. Therefore, we expect that the borrowing firms in our sample were fully aware of the possible use of information on their repayment history in a credit bureau. A thornier issue is whether the firms in the sample quickly became aware of the affiliation of lenders to the bureau after entry occurred. We believe that this was the case. In fact, the bureau was a major breakthrough in the equipment finance industry and was widely publicized. Furthermore, all lenders in our sample are major institutions and plausibly receive an ample press coverage. If one adds to this that equipment financing constitutes a large portion of the external finance of firms, it is likely that borrowing firms kept track of such a major change in the industry. As it will become clear shortly, firms' awareness of lenders' entry into the bureau matters for some of the mechanisms though which information sharing can impact firms' repayment performance.

The PayNet bureau has a number of appealing features. First, like other private credit bureaus and unlike public credit registers, it offers a complete coverage of the single leases and loans previously issued to an applicant, rather than consolidated information on an applicant's credit history.[12] Second, unlike private credit bureaus directly owned by lenders, it is independently managed by a third party and, hence, it is less exposed to problems of free riding among lenders, such as the risk that a lender utilizes the information provided by other members without releasing its own data in return. A third characteristic of the bureau is its reliability. The accuracy of its data is ensured by rigorous checking mechanisms and by strict norms that punish lenders with the exclusion from the use of the database in the event of misreporting or falsification of information.[13] PayNet filters data at multiple processing points prior to their filing in the live database and flags suspect data for review by its data consultants. PayNet also uses the most sophisticated search algorithms for firm identification based on a hybrid of proprietary and off-the-shelf matching tools. An additional feature of the bureau is its comprehensive coverage of different categories of contracts. Because of lack of robust data on small businesses prior to the inception of the PayNet bureau, commercial lenders in the equipment finance industry had often to rely on short-term payment histories to make medium- and long-term financing decisions. For instance, frequently a credit bureau reported to a lender how an applicant had paid her light bills, utility bills, or overnight express bills, all very short-term obligations which are poor predictors of repayment performance for equipment loans, leases and other such contracts. The PayNet bureau covers contracts of all maturities, thus allowing firms to make medium- and long-term lending decisions based on comparable credit rather than on "net-30-days" payment data.

4 Theoretical Predictions

The main focus of this paper is the impact of lenders' information sharing on borrowers' repayment performance. In addition, we shall investigate how lenders' information exchange shapes contractual terms. The theoretical literature has identified four channels whereby the information exchange that takes place through a credit bureau can impact lending outcomes and arrangements.

i. Adverse selection. According to Pagano and Jappelli (1993), the access to information provided by other financiers allows a lender to evaluate the credit worthiness of potential customers more effectively. This helps the lender enhance the average quality of its pool of borrowers, mit-

[12] Public credit registers generally collect information on loans above a certain size threshold.

[13] PayNet has a dispute resolution process that ensures compliance with state and federal credit-reporting laws. For a theoretical analysis of the incentives of a lender to behave opportunistically and provide false information to a credit bureau see, e.g., Semenova (2008).

igating arrears and defaults. In Pagano and Jappelli (1993), this is modeled as the consequence of a lender possessing more information about firms that operate in its local market than about firms in other markets. In particular, assume that a bank receives credit applications from businesses that are located in the area of a second bank. In this case, the affiliation of both banks to a credit bureau will allow the first bank to learn which among these applicants are less likely to default. In their study, Pagano and Jappelli (1993) also demonstrate that ex ante it is ambiguous whether, after the formation of a bureau, credit will only be granted to higher quality applicants. In fact, exactly because they gain access to better information, banks could choose to take more risks. In this case, while the repayment performance of old customers could improve after lenders join the bureau, the average repayment performance could remain unaltered or even worsen.

ii. Lenders' moral hazard. A second mechanism through which lenders' information sharing can affect borrowers' performance is the hold-up channel. Padilla and Pagano (1997) develop a model in which the performance of a loan depends on the quality of the entrepreneur and on her effort. Initially, each bank possesses private information on the quality of an entrepreneur. As in Rajan (1992), after extending a loan to an entrepreneur a bank can exploit its private information on her quality and threaten to withhold credit to extract rents from her (hold-up). Anticipating that the returns of her effort will be at least partially appropriated by her bank, the entrepreneur has then less incentives to exert effort ex ante. In turn, this worsens her repayment performance. Banks can tackle this incentive problem by committing ex ante to sharing one with another their proprietary information about entrepreneurs' quality. Expecting this information pooling, entrepreneurs will be reassured that no hold up will be possible and will step up their effort, lowering delinquency rates. In sum, by promoting competition among lenders and mitigating the risk of hold up, a credit bureau can stimulate entrepreneurial effort, reducing delinquencies.[14]

iii. Entrepreneurs' moral hazard. A third channel through which a credit bureau can affect lending outcomes is by imposing discipline on borrowers. In Padilla and Pagano (2000), lenders' information sharing induces entrepreneurs to exert effort because they "perform for a broader audience", that is, if they are delinquent on their contractual obligations, their misconduct will be disclosed to more lenders. Thus, in this context information sharing mitigates entrepreneurs' moral hazard.[15] However, Padilla and Pagano (2000) also underscore that this effect weakens if lenders pool information on borrowers' characteristics in addition to information on delinquencies. In this case, a high quality entrepreneur who has been delinquent on past contracts knows that anyway her high quality will be disclosed to lenders, regardless of whether her credit history is good or bad.

iv. Over − indebtedness. The last way whereby information sharing can improve repayment performance is by revealing to lenders the debt exposure of an applicant (Bennardo, Pagano and Piccolo, 2008). In fact, a firm will not be able to accumulate excessive debt by borrowing from several lenders without them realizing it. Therefore, by limiting firms' indebtedness, lenders' information exchange can mitigate the incidence of delinquencies.

To recapitulate, as argued by Jappelli and Pagano (2006), combining the four channels just discussed, one can conclude that, looking at an individual borrower, lenders' information sharing should unambiguously improve contract performance. In contrast, the predictions concerning the average delinquency rate are ambiguous because the extension of credit to a larger number of low

[14] Padilla and Pagano (1997) also examine the impact of the availability of information on entrepreneurs' past defaults. On the one hand, "forgetting" a default dilutes the related punishment, worsening ex-ante incentives. On the other hand, it improves an entrepreneur's ex-post reputation, letting her obtain credit in more circumstances.

[15] Diamond (1991) puts forth a model in which firms establish a good reputation as borrowers to obtain less expensive credit in the future.

quality applicants implied by the adverse selection channel could outweigh the positive impact implied by the other channels. With regard to the impact of information sharing on the volume of credit, the implications of the theory are definitely ambiguous (Jappelli and Pagano, 2006). For example, the lower risk of inducing adverse selection could push lenders to grant credit more easily whereas the revelation of any excess leverage accumulated by applicants could lead lenders to curtail their supply of credit.

5 Evidence from Contract Performance

In the first part of the empirical analysis, we carry out tests that collapse the repayment history of each contract into one observation, such as the average or maximum delinquency rate over the life of the contract. This has the cost of neglecting information on the internal dynamics of contracts, complicating the way we can control for macroeconomic conditions. However, it has the important benefit that we do not need to exclude early contracts from our tests. In fact, before the first quarter of 2000 we lack period-by-period information on the payments made to lenders and only have information on the average performance of contracts. We first discuss the empirical methodology. After that, we detail the measurement of the variables and present sample summary statistics.

5.1 Empirical Model

The baseline empirical model we estimate is

$$Bad_{ijt} = c + \alpha Information_{jt} + \sum_{j} \gamma_j D_j^L + \sum_{t} \delta D_t^P \left[+ \sum_{i} \beta_i D_i^F \right] + u_{ijt}. \tag{1}$$

The dependent variable bad_{ijt} is a measure of the (poor) performance of the contract extended to firm i by lender j at time t. Delinquencies carry several important costs for lenders such as lost interests, opportunity cost of principal, legal fees and expenses, and related costs.[16] Therefore, they are extensively used to assess the quality of lenders' portfolios (for an analysis of the equipment finance industry, see Murtagh, 2005) and to construct indicators of the credit performance of businesses (Berger and Udell, 2003). In addition to a broad variety of measures of delinquency, our data set includes some information on contract foreclosure and litigation, as well as borrowers' bankruptcy. Therefore, in the analysis we will also perform tests employing such information to construct the dependent variable. However, as we shall extensively explain in Section 6.6 when discussing such measures, using indicators of foreclosure and litigation entails several problems due to the very low frequency of foreclosure and litigation, measurement issues, and the legal treatment of bankruptcy. Therefore, indicators of delinquency will be our preferred metrics of contract performance. As our primary indicators, we use the natural logarithm of (one plus) the average number of days a payment was past due during the contract (*average DPD*) and the natural log of (one plus) the maximum number of days past due (*maximum DPD*).[17] However, one could argue that the cost of delinquencies may not be proportional to the payment delay (for instance, two mild 45-day delinquencies may imply a lower cost than a serious 90-day

[16] The decision to foreclose a delinquent loan or lease depends upon several elements. Financiers tend to refrain from foreclosing a delinquent loan or lease until it becomes evident that it cannot be recovered. In documents describing the day-to-day practice of equipment financiers, we found that the rule of thumb of most financiers is that they do not begin foreclosure proceedings until a loan or lease is at least 90 days delinquent.

[17] See also Table A.I for a description of the variables.

delinquency). Therefore, we also split late payments according to their severity and consider the log of (one plus) the number of times during the life of the contract that a payment was more than 30 days past due, the log of (one plus) the number of times that a payment was more than 60 days past due, and the log of (one plus) the number of installments that were over 90 days past due (*number of times DPD over* 30, *over* 60, *over* 90). To verify whether the effects of information sharing operate only on the intensive margin, we further complement these measures of contract performance with three dichotomous variables that respectively take on the value of one if at least once during the contract a payment was late (*delinquency*), at least once a payment was over 30 days late (*moderate or serious delinquency*), and at least once a payment was over 90 days late (*serious delinquency*). On the right hand side of (1), we include a constant c, a variety of controls, and our key variable of interest, a time-varying treatment variable ($information$) that takes on the value of one if the contract was originated no sooner than the date lender j joined the bureau, and 0 otherwise. Control variables include lender dummies D_j^L, reflecting differences in delinquency rates due to lender specific time-invariant characteristics, and time dummies D_t^P, controlling for macroeconomic conditions. In addition, we experiment both with specifications that include firm dummies D_i^F, capturing time-invariant firm attributes, and with specifications that do not include firm fixed effects but in which standard errors are clustered at the firm level. Since the life of a contract spans various periods and we are treating each contract as a single observation, we experiment with different specifications of the time controls. In one specification, time effects are determined by the origination date of the contract, in another by the mid-date of the contract, and in a third one by its closing date. In the first specification, for instance, D_t^P takes on the value of one if the origination date of the contract is in period t. Also, in our baseline specification we consider a period to be a year, but we verify that the results carry through when a period is defined as a quarter. Due to their potential endogeneity to outcomes, we initially exclude contract-level controls (such as the size of the contract) from the model. In fact, unobserved factors could drive both delinquencies and the choice of contract terms. However, we are aware that, especially in the case of the number of late payments, maturity can be a determinant because this number may tend to increase with the passage of time. Therefore, in our baseline results we also display regressions in which we control for scheduled contract maturity or actual contract duration. A worthwhile observation about such regressions is that in the literature it is often claimed that the maturity of a contract closely matches the nature of the equipment purchased or leased. In turn, the nature of the equipment is likely to reflect the technological needs of the borrowing firm so that overall we expect endogeneity problems to be mild when inserting contract maturity in the regressions. In additional tests, we also augment the specification with further contract attributes and we allow for time-varying lender characteristics by interacting lender dummies with time dummies.

Our non-dichotomous dependent variables constitute a corner solution response, taking on the value of zero for a non-trivial share of the population and being otherwise distributed over positive values. To account for this, we estimate the model in (1) both by ordinary least squares (OLS) and by Tobit. As explained by Wooldridge (2008), the OLS method provides a good approximation to the conditional expectation of the dependent variable, especially for values of the covariates close to the mean values. However, OLS estimates also present some drawbacks. First, the predicted values of the dependent variable can be negative. Second, the partial effect of any explanatory variable expressed in levels is constrained to be constant. Third, since its distribution piles up at zero, the dependent variable cannot have a conditional normal distribution, so that inference would only have an asymptotic justification. Complementing the OLS regressions with Tobit estimates allows to assuage these concerns.

5.2 Measurement and Sample Properties

Our data set consists of a random sample of the portfolios of 15 major equipment finance companies that gradually joined the PayNet bureau after its inception. The data set comprises detailed information on firm and especially contract performance and characteristics. Table I, Panels A to D, reports summary statistics. The total number of borrowing firms in the sample is 3,815. The firms are located in all U.S. states, the District of Columbia and Puerto Rico and span the majority of two digit SIC codes. Of the 2133 firms that report their SIC code, about 15 percent operate in construction, slightly more than 11 percent in manufacturing, 17 percent in transportation, 18 percent in wholesale or retail trade, and 28 percent in services. While we do not have demographic information for all firms, for the 1757 businesses for which the number of employees in 2007 is available, the average number of employees in that year was approximately 131 and the median was 8. Firm age on the start date of a contract, available for 2,140 firms, was 16 years on average, with a median of 12 years. Revenues in 2007, which are available for 1,620 firms, had an average of 1,140,000,000 dollars and a median of 750,000 dollars. And firm quality, as measured by the borrower's PayNet rating when the contract was originated, is available for 3486 firms and equalled 55 (out of 100) at the median and 54 at the average. These ratings are in the ballpark of those of all PayNet members: we obtained documentation that reveals that in 2001, for instance, the average rating of PayNet members across all new originations was 56. To better grasp their magnitude, we compared the demographic statistics for the firms in our sample with those for the pooled 1998 and 1993 waves of the National Survey of Small Business Finance (NSSBF) conducted by the U.S. Board of Governors of the Federal Reserve System and the Small Business Administration.[18] On average, the businesses in the pooled NSSBF waves are 15 years old (with a median of 12) and have 30 employees (with a median of 6). Thus, the businesses in our sample are slightly larger than those in the NSSBF, although they are still small- or medium-sized. As mentioned earlier, the small and medium size of the firms in the database is appropriate for our purposes. In fact, public information on small businesses is not readily available because they are not covered by bond rating agencies or the financial press (Petersen and Rajan, 1994). Therefore, the affiliation to a credit bureau is likely to convey to a lender information that could not be obtained otherwise.

At the contract level, the data set provides information on 28,623 deals originated by 15 lenders (Table 1, Panel B reports the percentages of contracts originated by the various lenders).[19] Most of the lenders hold a portfolio of contracts diversified across industrial sectors, with three out of the 15 lenders having more than half of their portfolio concentrated in one sector.[20] Panels C and D display summary statistics for contract origination dates, maturities, sizes, and for the measures of contract performance. The oldest contracts in the sample were originated as early as the fourth quarter of 1986, while the latest were originated in the second quarter of 2007. However, origination dates are not uniformly distributed over the sample period, as the large majority of them (28,320) are in 1995 or later. Regarding the information available to lenders at the time of origination, about 59% of the contracts in our sample were originated after the lender joined PayNet. The average (over contracts) of the average (over the life of a contract) of number of days past due is 6.2, while the average (over contracts) of the maximum (over the life of a contract) of the number of days past due is 29.5. The average number of times a payment was more than 30 days past due is 1.62, more than 60 days past due about 0.47, and over 90 days

[18] Several studies of the U.S. credit market use the NSSBF (see, e.g., Petersen and Rajan, 2002 and 1994).

[19] All the lenders except for two grant more than 500 contracts, one has 494 and one 71; the lender with the largest share of contracts grants 4,843.

[20] Two lenders appear somewhat specialized in transportation while another is especially active in construction.

past due 0.33. In almost 90 percent of cases, we know whether there is equipment related to the contract. In practically all of these contracts, there is indeed equipment that is being leased or purchased, with the most common equipment types being trucks, construction equipment, and copy machinery. Contracts have a guarantor in approximately 35% of cases, while in 55% of deals there is no guarantor (in the remaining 10% of cases we lack this information). With regard to contract maturity, the average scheduled term is 44 months and the median 47, whereas both the median and the average actual durations (computed using the differences between the actual closing date of the contract and its origination date) are between 31 and 32 months. Contract size, as given by the total amount to be paid back to the lender, is on average equal to 102,633 dollars, and the median is 43,813.43 dollars.[21] Thus, most contracts are small. In fact, about 93% of contracts are referred to as “small-ticket”, that is, under 250,000 dollars, and all of the remaining contracts, except for 33, are “medium-ticket” since they are below 5,000,000 dollars.

6 Estimation Results

In discussing the empirical findings, we start with presenting the core results. Afterward, we enrich the baseline specification and carry out several extensions.

6.1 Preliminary Tests

The inception of the PayNet bureau offers a clean natural experiment so that we do not expect endogeneity problems to affect our results. However, to further assuage concerns, we perform some preliminary exogeneity tests. One hypothesis we can test is whether the timing of entry of lenders into the bureau was correlated with transitory shocks to the performance of their portfolios before entry (that is, there is an “Ashenfelter dip” in our data). Finding no such correlation would suggest that lenders did not join the bureau on different dates because they were motivated by a different evolution of the performance of their portfolios. We carry out two tests (the results are not tabulated to conserve space). In a first test, we regress the average delinquencies (number of days contracts are past due) for a lender in a given quarter prior to entry on quarter dummies and on dummies capturing 1, 2, 3, and 4 quarters before the lender’s entry into the bureau. The estimated coefficients on the pre-entry time dummies are statistically insignificant. The second test consists of a Spearman test, which computes the degree of association between the ranking of lenders by entry date and the ranking of lenders by average delinquencies in the quarter before entry. This test also supports independence between lenders’ time of entry and the pre-entry performance of their portfolios. All in all, these results further support the idea that lenders’ entry was driven by exogenous factors.

6.2 Baseline Results

In the estimations that follow, we consistently restrict attention to contracts that were closed as of the second quarter of 2007 (the last quarter in our sample). In fact, we need to account for the possibility that the impact of information is greater at some point during the life span of a contract and, if that is the case, including contracts that are still active at the end of the sample could bias our results. In any case, at the end of this section, we will verify that the results are robust to including contracts still open at the end of the sample period. Once we

[21] The total amount reflects what the firm has to repay, not the amount borrowed. Therefore, we cannot distinguish interests from principal and test the impact of information sharing on the cost of borrowing.

remove open contracts, we are left with 17,216 deals for which we know the average number of days past due and 21588 contracts for which we know the maximum number of days past due and the number of times payments were late (more than 30, more than 60, or more than 90 days). In both these sub-samples, approximately half of the contracts are originated after lenders joined the bureau. In Table II, column 1, we present the results of a simple linear probability model where the dependent variable is a *delinquency* dummy which takes on the value of one if during the contract the borrower missed at least one scheduled payment, and zero otherwise. In addition to the treatment variable $information$, the model includes firm dummies, lender dummies, and time dummies. The $information$ dummy negatively affects the probability of delinquency, suggesting that the entry of a lender into the bureau improved the performance of its portfolio. In the other columns of Table II, we display the results of the baseline estimates where we regress the (log of the) average number of days past due and the (log of the) maximum number of days past due lender dummies, time dummies, and the $information$ dummy (with some specifications estimated without firm fixed effects but with standard errors clustered at the firm level and other specifications including firm fixed effects). In all the tables throughout the analysis, we report the OLS and Tobit coefficient estimates. Since the OLS model is likely to give a good estimate of the average effects (Wooldridge, 2008), and to avoid cluttering the tables, we omit instead the partial effects computed from the Tobit model but we discuss them in the text (details are available from the authors). In Table III, we augment the specifications by enlisting the scheduled maturity or actual duration of the contract (in months) as a control variable. In Table IV, we use three additional measures of delinquency (the logs of the number of times payments were more than 30, 60 or 90 days past due) as dependent variables (considering both the specifications with and without contract maturity or duration as a control). In the same table, we also perform tests using as dependent variables two dichotomous indicators of contract performance that also downplay the role of mild delinquencies: the first (*moderate or serious delinquency*) is a dummy that takes on the value of one if at least once the borrower was late on a payment more than 30 days, and zero otherwise; the second (*serious delinquency*) is a dummy that takes on the value of one if at least once the borrower was late on a payment more than 90 days, and zero otherwise. Finally, in Table IV we also perform tests using three indicators of contract foreclosure and litigation (full details on these variables and the related tests are in Section 6.6).

Let us first consider the regressions in which we omit firm fixed effects. All standard errors are heteroskedasticity-robust and clustered at the firm level. We obtain a consistently negative effect of the treatment variable on all our measures of delinquency. Looking at the effect on the average number of days past due over the life of the contract (columns 2 and 3 in Table II), in the OLS regressions the estimated coefficient on the treatment variable ranges from -0.223 to -0.320 when we do not control for contract scheduled maturity or actual duration. When we insert maturity or duration, as in columns 1 and 2 of Table III, the results remain virtually unchanged: for instance, relying on the start-date of the contract to account for time effects and controlling for scheduled maturity, the estimated coefficient on information equals -0.144. The regressions including firm fixed effects confirm that the treatment variable has a statistically and economically significant negative effect on our dependent variables, whether we control for contract maturity or duration or not.[22] For example, when delinquency is measured as the average number of days past due, we exclude contract maturity, and we enlist time dummies based on the mid-date of the contract, the coefficient on $information$ is significant at the 1

[22] Clearly, in these regressions the delinquency of firms with one contract is entirely explained by the firm dummies.

percent level and takes on the value of -0.115 (see Panel B, column 4, in Table II). For the average contract, whose payments are 6.2 days past due on average, this implies that lender entry into the bureau makes the average days past due fall from 6.2 to 5.4, or about 13 percent. Results are qualitatively similar for all the other metrics of delinquency: in the baseline regression the coefficient on $information$ is generally negative, statistically significant at conventional levels and the implied magnitude of the effect is sizeable (regardless of whether we include firm fixed effects or not and whether we construct time dummies based on the origination date, mid-date or closing date of the contract). Regarding other variables, it shall be noted that the scheduled maturity and actual duration of the contract appear to have a positive effect on the measures of delinquency. Moreover, some lender dummies are statistically significant, suggesting that some financiers systematically experience lower delinquency rates than others.

While contracts in the data set were originated as early as 1986, 99 percent of the origination dates are clustered between 1995 and 2007. This could raise concerns about the representativeness of the earliest contracts in the sample. To address these concerns, in Table V we report (a sample of) the point estimates obtained by dropping the oldest contracts. Specifically, we rerun the regressions of Tables II and IV first removing the oldest 1% of contracts, which were originated prior to November 1994, and then the oldest 5% of contracts, which were originated before November 1997. The results are robust to these exercises, with the coefficient on the treatment variable retaining its statistical and economic significance. A robustness check in a similar vein consists of including in the sample the contracts that were still active as of the second quarter of 2007 (the last quarter of the sample). The results, reported in Table V, Panels D-F, are robust to this exercise too: for example, when we use the average days past due as the dependent variable, insert the contract scheduled maturity and use the contract start date to construct time dummies, the estimated coefficient on the treatment variable is significant at the 1 percent level and takes on the value of -0.084 (see Panel D, column 3).

6.3 Contract Attributes

In the baseline specification, we have deliberately avoided enlisting contract characteristics as control variables, with the only exception of contract scheduled maturity or actual duration. One rationale for this exclusion is that such characteristics could be determined by - and, hence, be endogenous to - the expected performance of the contract. Thus, inserting them can be a source of bias. Nevertheless, it is useful to verify the robustness of our results to the inclusion of further contract attributes. The data set provides information on the size of contracts (expressed in dollars), the kind of equipment purchased or leased, the type of deals (loans, leases, etc.), the presence of a guarantor and its type, and the payment frequency.[23] The results obtained adding these controls to the baseline specification are displayed in Table VI.[24] The finding that the treatment variable negatively affects delinquency carries through to the augmented specifications.

[23] Possible types of equipment are agricultural, auto, bus, construction equipment, computers, copy equipment, forks, medium size trucks, manufacturing, no equipment, retail, telecommunications, trucks, unknown, vending machines, waste processing. For guarantor, the options are yes, no, corporate, personal, both, and unknown. For payment frequency, the options are montly, quarterly, semiannually, anually, or other. Types of contract include conditional sale, loan, lease purchase, revolver account, rental lease, true lease, and unknown. A conditional sale differs from a lease in that the lessor has no expectation of return of the equipment and has no residual value at risk at the end of the term. In a true lease, the lessee may return the equipment at the end of the term. Hence, the lessor retains significant residual value and tax advantages. An equipment loan usually requires a downpayment and finances the remaining cost of the equipment. A true lease finances 100 percent of the value of the equipment.

[24] There are less observations than in baseline regressions because information on some characteristics is unavailable for some contracts.

Regarding the newly inserted covariates, a noteworthy result is that the size of the deal does not appear to explain the variation in delinquencies. In general, the dummies for the various types of equipment do not exhibit a consistent pattern: changes in the way we measure delinquency affect the significance of these dummies. The effect that appears most robust across specifications is the positive correlation between the dummy for copy equipment and the delinquency rate. When we examine the dummies for the various types of contracts, rental leases, loans, and conditional sales appear to be associated with somewhat lower default rates than other contracts. Neither the presence of a guarantor nor the periodicity of payments seem to have a role in determining contract performance.

The robustness of our results to the inclusion of all these contract attributes is reassuring. In fact, although the metrics of delinquency used in the tests are standard in the literature, one could remain concerned that they imperfectly reflect the impact of information sharing on borrowers' performance, especially because they do not incorporate the size of the installments on which borrowers are delinquent. The finding that controlling for size, maturity and payment frequency of the deal does not alter the results assuages such possible concerns. In untabulated tests, we further reestimate the regressions by splitting the sample at the median size and duration of contracts and obtain that $information$ negatively affects delinquency rates in all sub-samples. The result we obtain for contracts shorter than the median duration (i.e., less than 31 months) is noteworthy because it suggests that we are properly controlling for macroeconomic effects in our tests. In fact, it is likely that for short-term maturities the time dummies we insert in the regressions adequately capture conditions of the aggregate economy during contract lifetime. Indeed, to further mitigate concerns about the way we control for time effects, we also rerun the regressions by adopting a finer partition of the sample according to the duration of contracts. Specifically, we consider contracts that have duration below two years, between two and four years, and above four years, and find that consistently across these subsamples the treatment variable negatively affects delinquency (see the alternative specifications in Table IX, Panel A). The results of the baseline specifications are also confirmed when we partition the sample according to whether the contract has a guarantor or not.[25] Finally, we reestimate the regressions after excluding in turn leases and loans from the sample. In addition to the earlier mentioned differences, leases also differ from loans because debtors that have filed for chapter 11 bankruptcy protection can choose whether to reject or assume leases (according to Section 365(d)(4) of the Bankruptcy Code). This could induce a different propensity to be delinquent on leases and a different impact of information sharing on repayment behavior. However, we find that the results carry through if we restrict attention to leases or loans.

6.4 Timing Issues

In the baseline specifications, our definition of the treatment variable is based on the premise that lenders' information sharing affects contract performance if and only if information is available before the origination of the contract. However, one could conjecture that even for contracts that are active when the lender joins the bureau the arrival of additional information could improve borrowers' repayment performance. As it can be gleaned from our review of the theoretical literature, this may occur because the news that the lender has joined the bureau could discipline the borrower. In fact, the borrower would now be aware that any delinquency on her contractual

[25] When we employ the number of times past due as the dependent variable, the impact of $information$ is stronger for non-guaranteed contracts. When we use average days past due the estimated coefficients are instead very similar for guaranteed and non-guaranteed contracts.

obligations will be recorded by the bureau, influencing her ability to obtain credit in the future (see, e.g., Padilla and Pagano, 2000). Clearly, whether this scenario is plausible depends on whether relevant choices are made during the life of a contract that can still significantly affect its performance or whether, instead, the choices that really matter are made before the origination of a contract; it also depends on whether firms with contracts active at the time of lenders' entry into the bureau became quickly aware of this entry.

To account for these issues, we reestimate the regressions with an alternative definition of the $information$ dummy. In particular, we experiment with defining the treatment variable as a dummy that equals one if the contract ended after lender entry into the bureau and was originated no earlier than 1 month, 3 months, 6 months, 1 year or 2 years before the date of entry of the lender into the bureau. The results, reported in Table VII, essentially carry through after these changes in the construction of the treatment variable, but, expectedly, the estimated coefficient tends to drop in magnitude and progressively lose statistical significance as we move back the threshold date. For instance, if we consider the average days past due as the dependent variable, we estimate a coefficient on $information$ of -0.14 when we shift the threshold date back by 6 months, -0.125 when we shift it back by one year, and a non-significant coefficient when we shift the threshold date back by two years (see Panel A).

An argument that goes in a direction opposite of that just discussed is that not only entry into the bureau could have no impact on contracts already active but also that it could affect borrowers' performance with a small lag. For example, although lenders had time to prepare before their entry into the bureau, the reader may still conjecture that a lender may need to refine the protocol to process the new information after its entry. In Table VII, we then reestimate the regressions by redefining $information$ as a dummy that takes on the value of one if the contract was originated no earlier than 1 month, 3 months, 6 months, 1 year or 2 years after the lender's entry date, and zero otherwise. Clearly, we do not expect lags in the effect of information sharing to be that long, so that, when we move ahead the threshold date, say, by six or more months, the test progressively becomes less and less about lagged effects and more a placebo test in which we use fake years of the treatment. The results are in line with expectations. Typically, we detect the strongest and most significant effects of (our proxy for) information sharing when we employ either the baseline specification or the specification in which the treatment variable equals one for contracts originated at least one month after the lender's entry (with the effect progressively weakening as the threshold date is pushed further ahead). This suggests that the affiliation to the credit bureau tends to improve borrowers' repayment performance either as soon as information becomes available or with a short lag.

6.5 Sorting by Firm Attributes

A possible way to better identify the effect of lenders' information pooling on borrowers' performance is to test whether this effect is stronger for firms that are reputed to be more informationally opaque or riskier. Small and young firms are allegedly informationally opaque because they lack an established track record accessible by lenders (Petersen and Rajan, 1994); in turn, firms with low credit ratings are a natural candidate for risky businesses. In light of these considerations, we now reestimate the empirical model by sorting the firms according to their size, age, and rating. We first restrict attention to the sub-sample of firms for which we have demographic information and partition this sub-sample on the basis of firm size and age. The results are reported in Table VIII. In Panel A, we experiment by dropping contracts granted to firms with over 100 or over 500 employees. In line with our null hypothesis, the results reveal that the impact of *information* on serious delinquencies is generally stronger for the sub-samples of small- and medium-sized firms than for the full sub-sample for which we have information on employees. For example, when we exclude contract maturity, we use the mid-point of the contract to control for time effects and we treat the log number of times over 60 days past due as our dependent variable, entry into the bureau translates into a drop of this number by 0.072 for businesses with no more than 100 employees versus a drop by 0.047 for all firms (see Panel A, column 6).[26] Next, in Panel B we sort the firms according to their age. In this case, the evidence is less compelling. We find that the drop in delinquencies induced by lenders' entry into the bureau is somewhat stronger for younger firms when delinquencies are measured by the maximum number of days past due. For instance, the estimated coefficient on the treatment variable equals -0.505 for the sub-sample of firms with age less or equal to the median (11 years) and -0.443 for all the firms whose age is reported. However, we cannot draw the same conclusion when capturing delinquencies with other metrics (indeed, the conclusion appears to be reverted when we use the number of times past due as the dependent variable).

We also learn interesting lessons when we sort the firms according to their rating (Panel C). The coefficients on the treatment variable estimated in the whole sample of firms for which we know ratings tend to be smaller (in absolute value) than those estimated for the businesses with below the median rating (55 out of one hundred) or below the 25th percentile (30), when the dependent variable is the number of times over 30, 60, or 90 days past due.[27] Collectively, these results tend to support the conjecture that the impact of lenders' information exchange on borrowers' performance is stronger for riskier firms.

6.6 Foreclosure and Litigation

As anticipated, the data set provides some information on foreclosure, litigation, and restructuring of contracts, or on the bankruptcy of borrowers. There are three major problems associated with this information. First of all, for the contracts for which this information is available, the frequency of foreclosure, litigation, and restructuring is low. Hence, the estimates obtained using such events to construct the dependent variable are probably unreliable. Second, treating such events in isolation encounters measurement issues. In fact, in the database these events are treated as mutually exclusive, so that a firm that first was involved in a litigation procedure and subsequently experienced a foreclosure of its loan or lease is reported as a "foreclosure". Third, there are several, hardly comparable events that fall under the broad category of contract foreclosure, litigation, and restructuring, such as whether the lender collected collateral, or whether it initiated a legal action against the borrower or was forced to extend the contract, or whether the lease was considered to be in "good standing" or not. In addition, as discussed previously, the U.S. Bankruptcy Code allows firms that declare bankruptcy to assume or reject leases. All these features complicate the definition of a composite variable that captures comparable instances of foreclosure, litigation, or workout.

In spite of these limitations, we may learn some insights from reestimating the baseline regressions using indicators of "litigation, foreclosure and restructuring" as dependent variables. As a first measure, we construct a dummy variable that takes on the value of one if the lender declares that it wrote off or extended the loan or lease, or initiated a legal action against the borrower, or it repossessed or collected the borrower's collateral, or that the lease was in "bad

[26] Clearly, the reader should interpret these results with caution because, unlike for firm ratings and age, we observe the size of firms only at the end of the sample (2007).

[27] We do not detect differences across sub-samples when using the average or maximum number of days past due.

standing", or at least once incurred into serious delinquency (over 90 days past due).[28] When constructing this first measure (*foreclosure and litigation w/o bankruptcy*), we exclude cases in which the borrowing firm declared bankruptcy. In fact, as discussed earlier, the Bankruptcy Code leaves to firms the freedom to reject or assume leases. The second measure of foreclosure and litigation (*foreclosure and litigation w/ bankruptcy*) is defined analogously to the first one but we treat bankruptcy like other bad events. Finally, the third measure (*foreclosure and litigation w/o extension*) is constructed treating bankruptcy like other bad events but coding a value of zero when contracts were in "bad standing" or were extended (that is, the cases that, among those listed above, are arguably the least similar to foreclosure and litigation). Clearly, there is some arbitrariness in the choice of the events that should be bundled together in our composite measure of foreclosure and litigation. However, this is not a source of major concern because each of these various events is rare and in addition the findings appear to be robust across different definitions of the composite variable. The results, displayed in Table IV, columns 12-14, are consistent with those for delinquencies in that they point to a negative impact of the treatment variable on foreclosure and litigation, regardless of the way the latter is defined. For example, using the first measure, inserting firm fixed effects in the regression, and using time dummies based on the contract start date, we estimate that the entry of a lender into the bureau reduced the probability of foreclosure and litigation by approximately 1.8% (see column 12, Panel A). Using instead the third measure, that is, treating contract workouts and "bad lease" status differentially, the estimated reduction in the probability of foreclosure and litigation induced by a lender's entry into the bureau was 2.5%.

6.7 Other Specifications

In a further robustness check - reported in Table IX, Panel B - we experiment by including the interactions between lender dummies and time dummies in the regressions. These interaction terms can capture time-varying characteristics of lenders. The results are essentially robust to the inclusion of these terms. In a final robustness check (not tabulated), we also experiment with an alternative specification of the dependent variables. In particular, we normalize the average number of days past due and the maximum number of days past due by the number of days between installments. In fact, one could wonder whether the pecuniary loss associated with a given number of days in arrear is larger when the number of days between payments is lower. It is far from clear whether this should be the case as the larger number of days between repayments could be offset by the fact that less frequent payments are associated with larger installments. Furthermore, the large majority of contracts in our sample have monthly payment periodicity, so that our transformations are effectively equivalent to a mere rescaling. Nonetheless, to allay concerns, we rerun our baseline regressions with the following modified dependent variables: the logarithm of the ratio of average number of days past due to the number of days between installments and the logarithm of the maximum number of days past due to the number of days between installments. Results are robust to these alternative specifications, and are in fact very similar to those of the baseline regressions (the estimates are not tabulated). For example, using the specification in Table II based on the mid-point of the contract, the coefficient for the impact of the *information* dummy on the adjusted measure of average days past due is -0.1204 and is statistically significant (p-value below 0.01). Similarly, the coefficient for the adjusted measure

[28] In the industry practice, most procedures that can eventually lead to foreclosure are started after the borrower has been in arrear for 90 days. Therefore, having in mind a broad notion of litigation that accounts for both formal and informal disputes, we also include serious delinquencies (over 90 days) in our measure.

of maximum days past due is -0.4203, also significant at the 1 percent level.[29]

6.8 Additional Issues

A point that deserves closer scrutiny regards the actual use of the PayNet database by lenders. PayNet members pay a very low fee to retrieve information about a credit seeker. This could raise some concerns. First, it could be that for some contracts the lender did not use the database to make its lending decisions. However, if there is a bias, this would be towards finding no effect of the affiliation to the bureau on contract performance so that we do not risk overestimating the impact of lenders' information sharing. Second, the reader could wonder whether the decision to retrieve a credit report could be influenced by the benefit expected from using the report. However, the fees requested to retrieve reports are very small. Moreover, these fees are typically pre-paid as lenders buy the right to retrieve a given number of PayNet reports at the beginning of the year. These features make it very unlikely that the use of a credit report is endogenous to the perceived quality of the single borrower.

The reader may also wonder whether some firms could have the tendency to "substitute" delinquencies on equipment finance contracts with delinquencies on other categories of loans (which we do not observe in our data set). However, even if some borrowers behaved this way this would not diminish the relevance of our results. In fact, it would indicate that sharing information in equipment finance allows lenders to alleviate moral hazard or perform effective screening of firms in this financing activity, so that indeed information sharing has a beneficial impact. This would suggest that, to minimize the risk that borrowers associate their better credit performance on equipment loans and leases with a worse performance on other segments of loans, financiers should render their information sharing more comprehensive.[30] A second consideration is that, as observed earlier, equipment investment and financing constitute a large portion of the total expenditures and debt of firms, respectively. Thus, a borrower's margins for substituting delinquencies on equipment loans or leases with delinquencies on other contractual obligations are relatively limited.[31] This is especially true if we account for the size of the firms in our sample together with the lumpy nature of equipment investment. For example, the purchase of a truck is likely to absorb a sizeable portion of the operating expenses of a small transportation company with, say, one or two dozens of employees.

Finally, one could be concerned about possible changes in the way payments were made during this period and wonder whether the observed reductions in payment delays could somewhat reflect the introduction of more efficient payment methods. However, such a structural shock in the payment technology would affect all lenders more or less at the same time and would be then picked up by the time dummies. Moreover, to further assuage concerns we checked whether new payment methods were introduced in the years of the sample and did not find evidence of such a structural change in the equipment finance industry.

7 Evidence from Contract Dynamics

In the tests of the previous section, we have treated each contract as a single observation and investigated the effect of lenders' information pooling on the average or maximum rate of delin-

[29] Results are similar when time dummies are constructed using the contract origination or maturity date.

[30] This discussion especially refers to the impact of information sharing on banks' profitability while in this paper we investigate how information sharing impacts firms' and lenders' behavior.

[31] The large share of total firm debt accounted by equipment financing also suggests that the impact of a change in borrowers' performance on the profitability of lenders is likely to be important.

quency over the life of the contract or on the occurrence of foreclosures and litigations. This has allowed us to exploit the size of our data set in full. The drawback of this approach is that, by collapsing all the information on the performance of a contract into a unique observation, we may lose precision in controlling for the impact on contract performance of time-varying factors, such as macroeconomic conditions. This could be especially problematic for very long-term contracts, during which macroeconomic conditions can change more substantially. In this section, we perform alternative tests in which we use information at quarterly frequency on delinquencies occurring during the life of contracts. This approach entails a loss of observations because before the first quarter of 2000 we only have information on the average performance of contracts and do not observe the (lack of) repayment on a quarter-by-quarter basis. Thus, we can carry out the tests of this section only on a subset of our sample.

7.1 Empirical Models and Results

Our methodological approach broadly parallels that followed in the previous section. We construct measures of delinquency for each contract on a quarterly basis and test whether the affiliation of the lender to the bureau affects these measures. The data set provides information on 147,935 contract-quarter observations. In the sub-sample, the average number of days a payment on a contract was past due in a quarter is 5.87. In Table X, Panel A, we take a first glance at the impact of lenders' information exchange by simply reestimating the model in (1) with contract-quarter number of days past due as the dependent variable (and accordingly redefining the time dummies). Once again, we are primarily interested in the impact of the treatment variable $information$, which in the specification of column 2 takes on the value of one if the contract was originated after the entry of the lender into the bureau, and zero otherwise. The coefficient has the expected negative sign and is statistically significant at the 1 percent level. The estimated impact implies that joining the bureau would have resulted into a decline by 2.7 in the number of days past due in a quarter.

The model we have just estimated is fraught with several problems. A first issue is that over the course of a contract the delinquency of a borrower in a quarter could be correlated to her delinquency in the previous quarter(s). This suggests that lenders' information sharing could also influence the persistence of delinquencies during the contract. To account for this, we next turn to estimate the following model

$$Delinq_{ijt} = \left(\begin{array}{c} c + \rho Delinq_{ijt-1} + \alpha Information_{jt} + \zeta(Information_{jt} Delinq_{ijt-1}) + \\ + \sum_j \gamma_j D_j^L + \sum_t \delta_t D_t^P \left[+ \sum_i \beta_i D_i^F \right] + u_{ijt} \end{array} \right), \quad (2)$$

where $delinq_{ijt}$ is the number of days past due in quarter t in a contract between firm i and lender j, $delinq_{ijt-1}$ is the lagged dependent variable, D_j^L is a lender fixed effect, D_t^P is a quarter time dummy, and, in a first specification, $information_{jt}$ is an indicator variable taking on the value of one if the lender was affiliated to the bureau when the contract was originated, zero otherwise. As in the previous section, we experiment both with specification that include firm dummies (D_i^F) and with specifications without firms fixed effect but with standard errors clustered at the firm level. In model (2), the ζ coefficient on the interaction term between the lagged dependent variable and the treatment variable reflects the impact of lenders' information sharing on the persistence of delinquencies during the contract. Panel B of Table X reports estimation results (see column 2). The estimated value of α is significantly negative suggesting that the affiliation to the bureau reduces arrears throughout the life of a contract. Moreover, the estimated coefficient on the lagged dependent variable is positive and significant, indicating that the more a borrower has been delinquent in the previous quarter the more she tends to be delinquent in the current quarter. Specifically, suppose that a borrower has been delinquent 5.87 days in the previous quarter (which is the sample mean of days past due). All else equal, relative to not being delinquent at all in the previous quarter, this borrower will on average be 1.04 additional days past due in the current quarter. Most interestingly, this number drops to 0.98 after a lender joins the bureau: in fact, the coefficient ζ on the interaction term takes on the value of -0.0148 (and is statistically significant at the 1% level of confidence). Thus, the affiliation to the credit bureau also appears to reduce the persistence of delinquencies over the life of a contract. In column 1 of the same panel, we reestimate the model allowing the affiliation of a lender to the bureau to have an impact even if the contract was already active at the time of the treatment. Precisely, we now define $information$ as a dummy that takes on the value of one if the contract-quarter observation is after the lender's entry into the bureau, and zero otherwise. The insights we draw from the estimates are essentially unaltered, although the statistical significance on the treatment variable tends to drop. In particular, the estimated coefficient for $information$ is -0.0161 (not significant at conventional levels), whereas the estimated coefficient on the interaction term equals -0.0412 and is significant at the 1 percent level. In untabulated results, we also add contract-level controls to the specification, obtaining similar estimates.

There are still two fundamental problems associated with the empirical model in (2). The model hinges on the assumption that any heterogeneity across contracts is fully captured by lenders' affiliation to the bureau as well as by any contract attributes added to the specification. Clearly, the reader could remain concerned that we are omitting contract-level factors that are relevant in determining delinquencies. Allowing for an individual contract effect in (2) is tantamount to thinking that there are two sources of persistence in a contract: the individual, unobservable effect and the state dependence through the $delinq_{ijt-1}$ term. As shown by Wooldridge (2008), for example, putting an individual contract effect into the error term is a source of inconsistency of the ordinary least square estimates if this effect is correlated with other regressors. To tackle this issue, we can eliminate the unobservable, individual effect by taking the first difference of (2), that is

$$\Delta Delinq_{ijt} = \left(\begin{array}{c} \rho \Delta Delinq_{ijt-1} + \gamma \Delta Information_{jt} + \\ \zeta \Delta (Information_{ijt} Delinq_{ijt-1}) + \sum_t \delta_t D_t^P + \Delta u_{ijt} \end{array} \right). \tag{3}$$

Given the presence of a lagged dependent variable in the original model, however, estimating the transformation in (3) by ordinary least squares still produces inconsistent estimates. This occurs because the first-differenced error term Δu_{ijt} is correlated with the first-differenced lagged dependent variable $\Delta delinq_{ijt-1}$. Although the size of the inconsistency can be small for a sufficiently large time span of the series, we cannot neglect this issue.[32] Therefore, we resort to an instrumental variable approach. Arellano and Bond (1991) propose a generalized method of moments (GMM) to estimate a first-difference model such as (3). They demonstrate that using lags of both the dependent and independent variables as instruments delivers consistent and asymptotically efficient parameter estimates, even if the panel is unbalanced as in our case. The Arellano and Bond (1991) estimator has a number of appealing features. Compared with other GMM estimators (such as the one proposed by Anderson and Hsiao, 1981), the coefficient estimates are more efficient because the estimator uses all the available lags of the right-hand-side variables as instruments.[33] Moreover, the estimator allows us to account for the possible endogeneity of variables different from $delinq_{ijt-1}$. Finally, the estimator can handle a flexible

[32] The OLS estimates of (3) are asymptotically consistent (Wooldridge, 2008).

[33] Anderson and Hsiao (1981) propose the second lag of the level and the first difference of this lag as instruments.

error covariance matrix, with possible serial correlation and heteroscedasticity. Before turning to the regression output, a caveat is in order regarding the first-differenced $\Delta information_{jt}$ term in (3). This term is meaningful only when we allow the entry of a lender into the bureau to have an impact on contracts already originated. In contrast, in the specifications in which we only allow entry to exert a role in contracts not yet originated we are unable to disentangle the effect of $\Delta information_{jt}$ (which would have the constant value of zero). In these specifications, we can only estimate the impact of lenders' information sharing on the persistence of delinquencies during contracts, as captured by the ζ coefficient on the interaction term.

The estimation results obtained using the Arellano and Bond approach are displayed in Panel D of Table X. Consider the case in which we do not allow affiliation to the bureau to have an effect on contracts already open at the time of entry into the bureau (column 2). Under this specification, the coefficient on the lagged dependent variable takes on the positive value of 0.262 and is significant at the 1 percent level. Most importantly, affiliation to the bureau appears to reduce the persistence of delinquencies: the interaction term has a negative coefficient of -0.668, also significant at the 1 percent level. Let us now turn to the specification in which we allow information to affect ongoing contracts. The estimates, displayed in column 1 of Panel D, show again that the coefficient on the $\Delta(information_{ijt}delinq_{ijt-1})$ interaction term is negative and statistically significant while the coefficient on $\Delta information_{jt}$ has a positive sign. Summing the two terms at the sample mean of $delinq_{ijt-1}$, the overall effect of entry into the bureau on delinquencies is negative, although not large. We put forth two possible interpretations for the weaker result we obtain when we allow for an effect on contracts already open. A first possibility is that firms that have already reached an agreement with a lender could be less aware of the lender's entry into the bureau than firms that negotiate an agreement after the lender's entry. There is, however, a more intriguing interpretation stemming from the theoretical literature. The main beneficial effect of information sharing on ongoing contracts should operate through tighter discipline on borrowers: once a borrower becomes aware that her lender is part of a bureau, she will know that any additional delinquency on her contractual obligations will be recorded by the bureau and alter her future chances to obtain credit. Padilla and Pagano (2000) predict that this effect tends to disappear if lenders also pool "white" information on borrowers' characteristics, such as their ratings. In this case, a high quality entrepreneur knows that her quality will be disclosed to lenders anyway, regardless of whether her credit history is good or bad. Since the PayNet bureau reports the rating of borrowers (besides their credit histories) the finding that the effect of information sharing gets diluted if we also allow for an impact on ongoing contracts may then be in line with this argument of Padilla and Pagano (2000).

7.2 Duration Analysis

The availability of detailed information on contract histories allows us to further investigate the performance of loans with a duration analysis. In particular, in line with previous studies, we are interested in testing whether lenders' information sharing impacts the time to the occurrence of the first delinquency during the contract. We evaluate the risk of delinquency using a proportional hazard model and treat contracts in which delinquency does not occur as censored observations.[34] In this setting, the hazard rate $h(t)$ is defined as the probability that the lender faces the first delinquency on the contract at time t, conditional on never having faced a delinquency on the contract prior to that time. In the proportional hazard framework, the (instantaneous) hazard

[34] We do not distinguish between the event of a borrower paying off the loan or lease according to the amortization schedule and the event of the borrower paying it off with an early prepayment.

function takes the form

$$h(t; X) = e^{X\beta} h_0(t) \tag{4}$$

where $h_0(t)$ is the "baseline hazard" (that obtains when all covariates are set to zero), $X(t)$ is a vector of covariates that multiplicatively shift the baseline hazard (without affecting the underlying shape of the hazard function), and β is the vector of parameters to be estimated. In our context, holding everything else constant, the ratio between the predicted hazard for a contract extended after the lender's entry into the bureau and the predicted hazard for a contract extended before entry measures the impact of bureau affiliation on the risk of contract delinquency. Given the proportional hazards assumption, this hazard ratio is independent of time.[35] To analyze the effect of the covariates on the hazard function we use the Cox model. This is a semi-parametric method in that no assumption about the form of the hazard function is made (Greene, 2003).[36] As the Cox model assumes that the hazard function is continuous while our data are quarterly (in which some delinquencies occur at the same survival time or quarter), we adopt the Breslow (1974)'s method for handling tied delinquencies in the calculation of the log partial likelihood and residuals.

The estimates of the Cox model are displayed in Panel C of Table X. The treatment variable $information$ shifts the hazard rate in the expected manner. In particular, contracts extended after lenders joined the bureau have significantly lower hazard rates than contracts extended before entry. In the specification with firm dummies, affiliation to the bureau lowers the hazard rate by 22 percent; if we drop firm dummies and cluster standard errors at the firm level, the estimated shift in the hazard rate amounts to about 29 percent. The estimates also reveal that some lenders have systematically higher hazard rates than others (hazard ratios for lender dummies are not tabulated to conserve space). Overall, the duration analysis thus confirms the findings in the rest of the paper that lenders' information sharing improves borrowers' performance.

8 Other Tests: Lending Standards

In addition to information on repayment histories, the data set provides details on features of the contracts such as their size and whether the contracts are guaranteed or not. Thus, besides its impact on borrowers' repayment performance, we can test the effect of lenders' information pooling on these contract terms. The first characteristic we consider is whether the contract is backed by a guarantor (which can be personal, corporate, or both). In the sample, about 30 percent of the contracts are guaranteed. We construct a dummy variable that takes on the value of one if the contract is guaranteed, and zero otherwise, and replace the dependent variable in (2) with this *guarantor* dummy. The estimates obtained using a linear probability model reveal that the treatment variable has a positive effect on the probability the contract has a guarantor (see Table XI, Panel A).[37] The estimated coefficient is 0.180 and is significant at the 5 percent level of confidence; interestingly, this effect appears to weaken somewhat if we restrict attention to risky (low rating) and informationally opaque (small) firms (see columns 2-4). The second characteristic we examine is the *size* of the deal. The regressions in the top panel of Table XI

[35] Under the proportional hazard assumption, the unspecified baseline hazard drops out of the partial likelihood, which can then be maximized with standard methods.

[36] For a discussion of the Cox model, see, e.g., Greene (2003). Key assumptions of the model are that the survival prospects remain constant over time and that the probability that an individual is censored is unrelated to the probability she suffers a "failure" event.

[37] When we use the contract origination date to construct time dummies, this effect is still positive but not statistically significant. When we use the maturity date, the effect is positive and significant.

suggest that entry into the bureau negatively affects the (natural logarithm) of the deal size (when using the mid-date of the contract to control for time effects, the coefficient takes on the value of -0.111 and is significant at the 1 percent level).

Collectively, the estimates thus suggest that entry into the credit bureau induces financiers to tighten their lending standards, offering smaller and better guaranteed contracts. Although the theoretical literature has not explicitly modelled the effect of information sharing on these contract characteristics we can put forth some interpretations. Pagano and Jappelli (1993) show that, after accessing information provided by other financiers, a lender can have the incentive to grant larger and possibly riskier (e.g., less guaranteed) loans. In other words, lending standards could become looser. However, Bennardo, Pagano and Piccolo (2008) stress that once a lender gains access to the information provided by other financiers it could realize that the borrower is over-leveraged. This could lead the lender to reduce the amount of credit and, if we follow this line of reasoning, apply tighter lending standards, such as requiring more guarantees. Our results thus offer suggestive evidence in support of the predictions of Bennardo, Pagano and Piccolo (2008). Naturally, alternative explanations for our findings can be proposed. For instance, it is reasonable that third parties are more willing to guarantee a contract when they know that the lender can monitor and discipline the borrower, as is the case if the lender has access to the accurate information contained in a bureau. In fact, in such a case the guarantors expect that the borrower will default on her contractual obligations - and, hence, they will become liable - with a lower probability. Disentangling the contribution of different mechanisms to the link between information sharing and the form of financing arrangements is, however, beyond the scope of this paper.

9 Conclusions

In this paper, we have investigated the consequences of lenders' information sharing using unique contract-level data from a credit bureau that serves the U.S. equipment finance industry. The staggered entry of creditors into the bureau, in conjunction with the small and medium size of borrowing firms, offer a suitable natural experiment to identify the effect of creditors' improved access to information. Consistent with extant theories (especially Pagano and Jappelli, 1993, and Padilla and Pagano, 1997 and 2000), we have uncovered evidence that lenders' information exchange has a beneficial impact on the repayment behavior of firms, reducing the incidence of delinquencies and foreclosures of loans and leases (for example, with a drop in the average number of days a contract is in arrear ranging from 10% to 15% depending on specifications). This effect appears to be stronger for firms that are reputed to be less informationally transparent (such as small firms) and riskier (e.g., with lower credit ratings). We have also found that the affiliation to the credit bureau induces financiers to grant smaller and shorter-term loans and to demand more guarantees. This indicates that information sharing improves borrowers' performance but not necessarily leads financiers to loosen credit standards. These findings may support the prediction of recent theoretical work (e.g., Bennardo, Pagano and Piccolo, 2008) that after lenders join a credit bureau they become better aware of the debt exposure of their clients and apply more stringent criteria for granting credit.

The analysis leaves open a number of interesting questions. An aspect that we have not been able to address in this paper is the impact of information exchange on firms' debt structure. For example, several studies have recently explored the choice between single and multiple borrowing in environments with scarce information. Studying the implications of information sharing arrangements for this choice could yield new insights into the role of multiple borrowing. We leave this and other issues for future research.

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Table A.I
Variable Definitions

Variable	Description
Average DPD	log(1+average over the life of the contract of the number of days past due)
Maximum DPD	log(1+maximum over the life of the contract of the number of days past due)
No. of Times Over 30 DPD	log(1+number of times where days past due exceeded 30)
No. of Times Over 60 DPD	log(1+number of times where days past due exceeded 60)
No. of Times Over 30 DPD	log(1+number of times where days past due exceeded 90)
Delinquency	Dummy equal to one if Maximum DPD is positive, zero otherwise
Moderate or Serious Delinquency	Dummy equal to one if No. of Times Over 30 DPD is positive, zero otherwise
Serious Delinquency	Dummy equal to one if No. of Times Over 90 DPD is positive, zero otherwise
Foreclosure and Litigation (w/o Bankruptcy)	Dummy equal to one if Number of times over 90 DPD is positive, there is bankruptcy, collateral reposession, writeoff, collection, extension, legal action, bad lease status, or loss, zero otherwise
Foreclosure and Litigation (w/ Bankruptcy)	Dummy equal to one if Number of times over 90 DPD is positive, there is bankruptcy, collateral reposession, writeoff, collection, extension, legal action, bad lease status, or loss, zero otherwise
Foreclosure and Litigation (w/o Extension)	Dummy equal to one if Number of times over 90 DPD is positive, there is bankruptcy, collateral reposession, writeoff, collection, legal action, bad lease status, or loss, zero otherwise
Current DPD	log(1+number of days past due as of the current quarter)
Information	In basic specifications, dummy equal to one if the contract origination date is after the lender's entry into the bureau, zero otherwise (for alternative
Contract Size	log(total amount to be repaid by the firm over the contract life)
Scheduled Maturity	log(scheduled contract maturity, in months)
Guarantor	Dummy equal to one if the contract has a guarantor (personal and/or corporate), zero otherwise
Firm Age	Months since the inception of the firm
Firm Rating	Credit rating assigned by Paynet to the firm (as of the origination date of the contract)

Table I
Summary Statistics

This table reports firm-level (Panel A) and contract-level (Panel C) summary statistics for the sample as well as the percentages of contracts by lender (Panel B). The table also displays summary statistics for contracts started before (Panel C) and after (Panel D) lenders' entry into the bureau. The total number of firms is 3,815, but firm revenue (in dollars), number of employees and age are available for 1613, 1757, and 2140 firms, respectively. Revenue and number of employees are reported as of 2007. Rating and age are reported as of the start date of the contract. The total number of contracts is 28,623. Contract scheduled maturity and actual duration are expressed in months, total amount is measured in dollars and reflects the total amount the firm has to repay, not the amount borrowed. Average DPD and Maximum DPD are, respectively, the average and the maximum number of days past due during the contract. No. of times DPD over 30, over 60, and over 90 are the number of times the contract was over 30 days past due, over 60 days past due and over 90 days past due. Delinquency is a dummy that takes on the value of one if during the contract at least once a payment was over 30 days past due, and zero otherwise; moderate or serious delinquency (serious delinquency) is a dummy that takes on the value of one if during the contract at least once a payment was over 60 (90) days past due, and zero otherwise. foreclosure and litigation w/o bankruptcy is a dummy variable that takes on the value of one if the lender declares that she wrote off or extended the loan or lease, or initiated a legal action against the borrower, or she repossessed or collected the borrower's collateral, or that the lease was in "bad standing", or at least once incurred into serious delinquency (over 90 days past due). Foreclosure and litigation w/ bankruptcy is defined analogously but also takes on the value of one if the borrowing firm declared bankruptcy. Foreclosure and litigation w/o extension is constructed similarly to foreclosure and litigation w/ bankruptcy but takes on the value of zero when the contract was in "bad standing" or was extended.

Panel A - Firm-Level Summary Statistics

	Revenue	No. of Employees	Age	Rating
No. of Observations	1,613	1,757	2,140	3,486
Mean	114,000,000	131	16.09	44.80
Standard Deviation	778,000,000	1,635	15.65	23.96
25th Percentile	238,000	2	5.60	25.00
Median	750,000	8	11.70	42.50
75th Percentile	3,000,000	26	21.10	62.50

Panel B - Number of Contracts by Lender

Lender	1	2	3	4	5	6	8	9	10	11	12	13	14	15	Total
Number	1,603	1,231	3,234	716	1,116	2,118	4,843	1,603	4,694	2,662	494	1,403	2,118	71	28,623
Percentage	5.6	4.3	11.3	2.5	3.9	7.4	16.9	5.6	16.4	9.3	1.7	4.9	7.4	0.2	100.0

Panel C - Contract-Level Summary Statistics

	Start Date	Scheduled Maturity (months)	Actual Duration (months)	Contract Size ($)	Average DPD	Maximum DPD	No. of Times DPD Over 30	No. of Times DPD Over 60	No. of Times DPD Over 90
No. of Observations	28,622	28,622	21,588	28,622	17,216	21,588	28,623	28,623	28,623
Mean	9/13/2002	44	31.22	102,633	6.2	29.5	1.62	0.47	0.33
Standard Deviation	1000 days	16.84	16.66	404,012	21.06	51.78	4.68	2.03	2.06
25th Percentile	12/14/2000	36	18.03	16,728	0	0	0	0	0
Median	12/20/2002	47	31.8	43,813	0	10	0	0	0
75th Percentile	9/27/2004	60	41.13	92,504	3	32	1	0	0

	Delinquency	Moderate or Serious Delinquency	Serious Delinquency	Foreclosure and Litigation W/o Bankruptcy	Foreclosure and Litigation W/ Bankruptcy	Foreclosure and Litigation W/o Extension	Guarantor
Observations	21,588	21,588	21,588	21,588	21,588	21,588	24,868
Mean	0.601	0.390	0.096	0.118	0.123	0.120	0.346
Standard Deviation	0.490	0.488	0.295	0.322	0.328	0.325	0.476
25th Percentile	0	0	0	0	0	0	0
Median	1	0	0	0	0	0	0
75th Percentile	1	1	0	0	0	0	1

Type of Contract

	Conditional Sale	Loan	Lease Purchase	Rental Lease	Revolver Account	True Lease	Unknown	Total
Number	4,649	12,357	813	848	459	8,974	522	28,622
Percentage	16.2	43.2	2.8	3.0	1.6	31.4	1.8	100

Equipment Type

	Number	Total $ Amount	Actual Duration	Percentage	Lease
Agricultural	1,005	78,795	29.04	37	28
Automobile	242	30,359	28.42	34	25
Buses and Motor Coaches	658	127,589	32.13	35	12
Construction	5,885	111,271	29.18	36	19
Computer	1,535	108,131	30.73	32	48
Copy and Fax	2,350	21,475	37.04	34	88
Forklift	639	76,630	25.00	31	43
Medium/Light Duty Truck	1,668	66,608	33.00	30	24
Manufacturing	1,000	193,980	33.92	30	47
Office	170	94,330	30.15	39	40
Retail	1,011	62,177	33.75	38	35
Telecommunications	342	91,139	33.24	38	32
Truck	7,633	109,926	31.38	36	12
Vending and Restaurant	503	26,726	39.00	44	17
Waste and Refuse Handling	78	916,463	28.11	23	46
Unknown	3,416	77,939	31.85	32	57
None	487	73,540	19.8	30	1
Total	28,622	102,633	31.22	35	31

Payment Frequency

	Number	Percentage
Monthly	25,340	88.5
Quarterly	173	0.6
Semiannual	2,717	9.5
Annual	369	1.3
Other	23	0.1
Total	28,622	100.0

Table I (cont.)

Panel D - Contract-Level Summary Statistics before Entry

	Start Date	Scheduled Maturity (months)	Contract Size ($)	Average DPD	Maximum DPD	No. of Times DPD Over 30	Over 60	Over 90
Observations	11,599	11,599	11,599	8,817	11,449	11,599	11,599	11,599
Mean	1/28/2000	44.736	107,482	7.807	34.307	2.689	0.768	0.563
Standard Deviation	731 days	16.21	368,123	24.772	57.661	6.232	2.695	2.875
25th Percentile	2/26/1999	36	16,869	0	0	0	0	0
Median	7/26/2000	48	46,276	0	19	0	0	0
75th Percentile	6/6/2001	60	93,051	5	39	3	0	0

	Delinquency	Moderate or Serious Delinquency	Serious Delinquency	Foreclosure and Litigation W/o Bankruptcy	W/ Bankruptcy	W/o Extension	Guarantor
Observations	11,449	11,449	11,449	11,449	11,449	11,449	10,356
Mean	0.668	0.450	0.106	0.127	0.131	0.126	0.315
Standard Deviation	0.471	0.496	0.308	0.333	0.337	0.333	0.465
25th Percentile	0	0	0	0	0	0	0
Median	1	0	0	0	0	0	0
75th Percentile	1	1	0	0	0	0	1

Panel E - Contract-Level Summary Statistics after Entry

	Start Date	Scheduled Maturity (months)	Contract Size ($)	Average DPD	Maximum DPD	No. of Times DPD Over 30	Over 60	Over 90
Observations	17,203	17,203	17,203	8,399	10,139	10,139	10,139	10,139
Mean	6/27/2004	43.037	99,329	4.504	24.139	1.515	0.463	0.308
Standard Deviation	515 days	17.222	426,719	16.118	43.596	3.786	1.755	1.575
25th Percentile	5/25/2003	36	16,626	0	0	0	0	0
Median	5/25/2004	42	41,829	0	2	0	0	0
75th Percentile	8/12/2005	60	92,186	1	31	1	0	0

	Delinquency	Moderate or Serious Delinquency	Serious Delinquency	Foreclosure and Litigation W/o Bankruptcy	W/ Bankruptcy	W/o Extension	Guarantor
Observations	10,139	10,139	10,139	10,139	10,139	10,139	14,512
Mean	0.525	0.322	0.085	0.107	0.113	0.112	0.367
Standard Deviation	0.499	0.467	0.279	0.309	0.317	0.316	0.482
25th Percentile	0	0	0	0	0	0	0
Median	1	0	0	0	0	0	0
75th Percentile	1	1	0	0	0	0	1

Table II
Information Sharing and Contract Performance

This table reports coefficient estimates and associated standard errors (in parentheses) for the impact of bureau affiliation on contract delinquencies. Delinquency is an indicator variable taking on the value of one if there was at least one late payment during the contract, and zero otherwise. Average Days Past Due (DPD) and Maximum Days Past Due (DPD) are respectively the average and the maximum number of days payments were past due during the contract (both measured in logs). Information is a dummy variable that takes on the value of one if the contract began after lender entry into the PayNet bureau, and zero otherwise. All the regressions include lender dummies and time dummies. For both the Average DPD and the Maximum DPD, we estimate four models: ordinary least squares (OLS) and Tobit without firm fixed effects and with heteroskedasticity-robust standard errors clustered at the firm level, and OLS and Tobit with firm fixed effects. Panels represent different ways to construct time dummies. Time dummies in the Start Date panel equal one if a contract starts in a given year, and zero otherwise, and similarly for Mid-Date and Close Date. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% level, respectively. For all the regressions, the table also reports the adjusted R-squared (pseudo R-squared if Tobit). In the regressions, n.a. appears when the Tobit did not converge.

Panel A - Start Date

	Delinquency (Yes/No)	Average DPD				Maximum DPD			
	OLS	OLS	Tobit	OLS	Tobit	OLS	Tobit	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Information	-0.051***	-0.320***	-0.796***	-0.114***	-0.332***	-0.262**	-0.442**	-0.167***	-0.3261
	(0.015)	(0.083)	(0.170)	(0.033)	(0.067)	(0.128)	(0.191)	(0.052)	n.a.
Firm Fixed Effects	Yes	No	No	Yes	Yes	No	No	Yes	Yes
No. of Observations	21,588	17,216	17,216	17,216	17,216	21,588	21,588	21,588	21,588
Adjusted R-sq (Pseudo R-sq if Tobit)	0.421	0.107	0.049	0.645	0.378	0.130	0.042	0.5	0.237

Panel B - Mid-Date

	Delinquency (Yes/No)	Average DPD				Maximum DPD			
	OLS	OLS	Tobit	OLS	Tobit	OLS	Tobit	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Information	-0.127***	-0.223***	-0.668***	-0.115***	-0.353***	-0.547***	-0.910***	-0.472***	-0.774***
	(0.011)	(0.073)	(0.154)	(0.026)	(0.053)	(0.107)	(0.171)	(0.040)	(0.059)
Firm Fixed Effects	Yes	No	No	Yes	Yes	No	No	Yes	Yes
No. of Observations	21,588	17,216	17,216	17,216	17,216	21,588	21,588	21,588	21,588
Adjusted R-sq (Pseudo R-sq if Tobit)	0.372	0.112	0.051	0.648	0.379	0.547	0.041	0.511	0.238

Panel C - Close Date

	Delinquency (Yes/No)	Average DPD				Maximum DPD			
	OLS	OLS	Tobit	OLS	Tobit	OLS	Tobit	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Information	-0.152***	-0.314***	-0.865***	-0.144***	-0.407***	-0.716***	-1.191***	-0.541***	-0.883***
	(0.009)	(0.058)	(0.128)	(0.020)	n.a.	(0.082)	(0.138)	(0.031)	(0.046)
Firm Fixed Effects	Yes	No	No	Yes	Yes	No	No	Yes	Yes
No. of Observations	21,588	17,216	17,216	17,216	17,216	21,588	21,588	21,588	21,588
Adjusted R-sq (Pseudo R-sq if Tobit)	0.37	0.110	0.051	0.650	0.383	0.128	0.041	0.517	0.240

Table III
Basic Results Controlling for Contract Maturity

This table reports coefficient estimates and associated standard errors (in parentheses) for the impact of bureau affiliation on contract delinquencies after controlling for actual contract duration (Panels A-C) and scheduled maturity (Panels D-F). Average Days Past Due (DPD) and Maximum Days Past Due (DPD) are respectively the average and the maximum number of days payments were past due during the contract (both measured in logs). Information is a dummy variable that takes on the value of one if the contract began after lender entry into the PayNet bureau, and zero otherwise. Actual duration is the actual length of the contract while scheduled maturity is the planned one. All the regressions include lender dummies and time dummies. For both dependent variables, we estimate four models: ordinary least squares (OLS) and Tobit without firm fixed effects and with heteroskedasticity-robust standard errors clustered at the firm level, and OLS and Tobit with firm fixed effects. Panels represent different ways to construct time dummies. Time dummies in the Start Date panel equal one if a contract starts in a given year and zero otherwise, and similarly for Mid-Date and Close Date. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% level, respectively. For all the regressions, the table also reports the adjusted R-squared (pseudo R-squared if Tobit). In the regressions, n.a. appears when the Tobit did not converge.

Panel A - Start Date

	Average DPD				Maximum DPD			
	OLS	Tobit	OLS	Tobit	OLS	Tobit	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information	-0.321***	-0.803***	-0.120***	-0.357***	-0.256**	-0.432***	-0.177***	-0.337***
	(0.083)	(0.095)	(0.033)	(0.067)	(0.123)	(0.091)	(0.051)	(0.075)
Actual Duration	-0.0001	0.006***	0.005***	0.012***	0.122***	0.022***	0.018***	0.028***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Firm Fixed Effects	No	No	Yes	Yes	No	Yes	Yes	Yes
No. of Observations	17,216	17,216	17,216	17,216	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.107	0.049	0.648	0.379	0.138	0.045	0.524	0.244

Panel B - Mid-Date

	Average DPD				Maximum DPD			
	OLS	Tobit	OLS	Tobit	OLS	Tobit	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information	-0.218***	-0.536***	-0.049*	-0.182	-0.263***	-0.398***	-0.173***	-0.296***
	(0.070)	(0.079)	(0.027)	n.a.	(0.102)	(0.075)	(0.042)	(0.062)
Actual Duration	0.0002	0.006***	0.004***	0.010	0.013***	0.023***	0.017***	0.027***
	(0.001)	0.001	(0.001)	n.a.	(0.002)	(0.001)	(0.001)	(0.001)
Firm Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
No. of Observations	17,216	17,216	17,216	17,216	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.112	0.052	0.649	0.381	0.139	0.044	0.525	0.245

Panel C - Close Date

	Average DPD				Maximum DPD			
	OLS	Tobit	OLS	Tobit	OLS	Tobit	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information	-0.336***	-0.847***	-0.104***	-0.312***	-0.454***	-0.731***	-0.257***	-0.427***
	(0.059)	(0.065)	(0.022)	(0.047)	(0.085)	(0.062)	(0.035)	(0.051)
Actual Duration	-0.001	0.001	0.002***	0.004***	0.011***	0.019***	0.014***	0.022***
	(0.001)	(0.002)	(0.0005)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Firm Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
No. of Observations	17,216	17,216	17,216	17,216	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.110	0.051	0.651	0.383	0.135	0.043	0.525	0.245

Table III (cont.)

Panel D - Start Date

	Average DPD				Maximum DPD			
	OLS	Tobit	OLS	Tobit	OLS	Tobit	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information	-0.311***	-0.776***	-0.112***	-0.331	-0.244**	-0.414**	-0.163***	-0.321***
	(0.080)	(0.161)	(0.033)	n.a.	(0.123)	(0.0185)	(0.051)	(0.076)
Scheduled Maturity	0.007***	0.020***	0.003***	0.008	0.013***	0.021***	0.010***	0.015***
	(0.002)	(0.004)	(0.0005)	n.a.	(0.002)	(0.004)	(0.001)	(0.001)
Firm Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
No. of Observations	17,216	17,216	17,216	17,216	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.116	0.054	0.646	0.377	0.142	0.046	0.515	0.239

Panel E - Mid-Date

	Average DPD				Maximum DPD			
	OLS	Tobit	OLS	Tobit	OLS	Tobit	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information	-0.144**	-0.464***	-0.090***	-0.297	-0.406***	-0.677***	-0.391***	-0.654***
	(0.068)	(0.145)	(0.026)	n.a.	(0.106)	(0.168)	(0.040)	(0.060)
Scheduled Maturity	0.007***	0.019***	0.003***	0.006	0.013***	0.021***	0.009***	0.013***
	(0.002)	(0.004)	(0.0005)	n.a.	(0.002)	(0.004)	(0.001)	(0.001)
Firm Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
No. of Observations	17,216	17,216	17,216	17,216	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.121	0.056	0.648	0.380	0.141	0.045	0.516	0.24

Panel F - Close Date

	Average DPD				Maximum DPD			
	OLS	Tobit	OLS	Tobit	OLS	Tobit	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information	-0.219***	-0.638***	-0.122***	-0.360	-0.565***	-0.951***	-0.468***	-0.778***
	(0.054)	(0.119)	(0.020)	n.a.	(0.083)	(0.137)	(0.032)	(0.047)
Scheduled Maturity	0.007***	0.018***	0.002***	0.004	0.012***	0.018***	0.007***	0.010***
	(0.002)	(0.004)	(0.0005)	n.a.	(0.002)	(0.004)	(0.001)	(0.001)
Firm Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
No. of Observations	17,216	17,216	17,216	17,216	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.120	0.056	0.651	0.383	0.138	0.044	0.519	0.241

Table IV
Information Sharing and Contract Performance - Other Measures of Performance

This table reports coefficient estimates and associated standard errors (in parentheses) for the impact of bureau affiliation on contract delinquencies. Here, we measure delinquencies with an indicator variable (moderate or severe delinquency) taking on the value of one if during the contract at least one payment was over 30 days past due, and zero otherwise, as well as with an indicator (severe delinquency) which takes on the value of one if during the contract at least one payment was over 90 days past due, and zero otherwise. We also measure delinquencies as the number of times over 30 days past due, over 60 days past due and over 90 days past due (all in logs). Information is a dummy variable that takes on the value of one if the contract began after lender entry into the PayNet bureau, and zero otherwise. Actual duration is the actual length of the contract while scheduled maturity is the planned one. All the regressions include lender dummies and time dummies. For all the three non-dichotomous dependent variables, we estimate ordinary least squares (OLS) and Tobit models without firm fixed effects and with heteroskedasticity-robust standard errors clustered at the firm level as well as an OLS model with firm fixed effects. In panels A-C, we display results from regressions that do not include contract maturity or duration. In Panels D-F (G-I), we display results controlling for actual contract duration (scheduled contract maturity). Panels also represent different ways to construct time dummies. Time dummies in the Start Date panels equal one if the contract starts in a given year, and zero otherwise, and similarly for Mid-Date and Close Date. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% level, respectively. The table also reports the adjusted R-squared (pseudo R-squared if Tobit).

Panel A - Start Date

	Moderate or Severe Delinquency (Yes/No)	Severe Delinquency (Yes/No)	Number of Times DPD								
				Over 30			Over 60			Over 90	
	OLS	OLS	OLS	OLS	Tobit	OLS	OLS	Tobit	OLS	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Information	-0.026*	-0.022**	-0.105	-0.005	-0.063	-0.137	0.031**	0.060	-0.082*	-0.041***	-0.644*
	(0.014)	(0.009)	(0.063)	(0.023)	(0.147)	(0.052)	(0.015)	(0.268)	(0.050)	(0.014)	(0.356)
Firm Fixed Effects	Yes	Yes	No	Yes	No	No	Yes	No	No	Yes	No
No. of Observations	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.453	0.457	0.097	0.576	0.044	0.063	0.552	0.042	0.038	0.453	0.037

Panel B - Mid-Date

	Moderate or Severe Delinquency (Yes/No)	Severe Delinquency (Yes/No)	Number of Times DPD								
				Over 30			Over 60			Over 90	
	OLS	OLS	OLS	OLS	Tobit	OLS	OLS	Tobit	OLS	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Information	-0.094***	-0.017***	-0.232***	-0.206***	-0.502***	-0.052	-0.047***	-0.125	-0.045	-0.043***	-0.272
	(0.011)	(0.006)	(0.013)	(0.018)	(0.115)	(0.035)	(0.011)	(0.179)	(-0.033)	(0.011)	(0.249)
Firm Fixed Effects	Yes	Yes	No	Yes	No	No	Yes	No	No	Yes	No
No. of Observations	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.454	0.457	0.093	0.576	0.042	0.063	0.552	0.042	0.040	0.454	0.040

Panel C - Close Date

	Moderate or Severe Delinquency (Yes/No)	Severe Delinquency (Yes/No)	Number of Times DPD								
				Over 30			Over 60			Over 90	
	OLS	OLS	OLS	OLS	Tobit	OLS	OLS	Tobit	OLS	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Information	-0.100***	-0.013**	-0.309***	-0.230***	-0.691***	-0.824***	-0.053***	-0.316**	-0.039*	-0.033***	-0.311
	(0.009)	(0.006)	(0.036)	(0.014)	(0.092)	(0.025)	(0.009)	(0.132)	(0.022)	(0.008)	(0.186)
Firm Fixed Effects	Yes	Yes	No	Yes	No	No	Yes	No	No	Yes	No
No. of Observations	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.455	0.458	0.092	0.580	0.040	0.059	0.552	0.039	0.040	0.455	0.038

Table IV (cont.)

Panel D - Start Date

	Moderate or Severe Delinquency (Yes/No)	Severe Delinquency (Yes/No)	Number of Times DPD								
				Over 30			Over 60			Over 90	
	OLS	OLS	OLS	OLS	Tobit	OLS	OLS	Tobit	OLS	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Information	-0.028**	-0.023***	-0.007	-0.010	-0.050	-0.013	0.029**	0.068	-0.082*	-0.042***	-0.641*
	(0.014)	(0.009)	(0.060)	(0.022)	(0.140)	(0.051)	(0.015)	(0.262)	(0.050)	(0.014)	(0.352)
Actual Duration	0.003***	0.001***	0.007***	0.010***	0.123***	0.002**	0.003***	0.005	0.0005	0.003***	0.002
	(0.0002)	(0.0001)	(0.001)	(0.0003)	(0.003)	(0.001)	(0.0002)	(0.003)	(0.0004)	(0.0002)	(0.004)
Firm Fixed Effects	Yes	Yes	No	Yes	No	No	Yes	No	No	Yes	No
No. of Observations	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.461	0.460	0.109	0.648	0.047	0.064	0.557	0.042	0.038	0.457	0.037

Panel E - Mid-Date

	Moderate or Severe Delinquency (Yes/No)	Severe Delinquency (Yes/No)	Number of Times DPD								
			Over 30			Over 60			Over 90		
	OLS	OLS	OLS	OLS	Tobit	OLS	OLS	Tobit	OLS	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Information	-0.032***	0.002	-0.061	-0.039**	-0.154	-0.011	0.009	0.049	-0.040	-0.007	-0.254
	(0.012)	(0.007)	(0.048)	(0.018)	(0.115)	(0.036)	(0.012)	(0.183)	(0.034)	(0.011)	(0.260)
Actual Duration	0.004***	0.001***	0.008***	0.010***	0.016***	0.002***	0.003***	0.008**	0.0002	0.002***	0.001
	(0.0002)	(0.0001)	(0.001)	(0.0003)	(0.002)	(0.0006)	(0.0002)	(0.003)	(0.0004)	(0.0002)	(0.004)
Firm Fixed Effects	Yes	Yes	No	Yes	No	No	Yes	No	No	Yes	No
No. of Observations	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.462	0.459	0.109	0.593	0.047	0.065	0.556	0.043	0.040	0.457	0.040

Panel F - Close Date

	Moderate or Severe Delinquency (Yes/No)	Severe Delinquency (Yes/No)	Number of Times DPD								
				Over 30			Over 60			Over 90	
	OLS	OLS	OLS	OLS	Tobit	OLS	OLS	Tobit	OLS	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Information	-0.037***	-0.006	-0.131***	-0.058***	-0.317***	-0.036	0.005	-0.080	-0.026	0.003	-0.258
	0.010)	(0.006)	(0.039)	(0.015)	(0.099)	(0.028)	(0.010)	(0.147)	(0.025)	(0.009)	(0.215)
Actual Duration	0.003***	0.001***	0.007***	0.009***	0.016***	0.002***	0.003***	0.010***	0.0005	0.002***	0.002
	(0.0002)	(0.006)	(0.001)	(0.0003)	(0.002)	(0.001)	(0.0002)	(0.003)	(0.0004)	(0.0002)	(0.004)
Firm Fixed Effects	Yes	Yes	No	Yes	No	No	Yes	No	No	Yes	No
No. of Observations	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.461	0.459	0.106	0.593	0.045	0.061	0.556	0.040	0.040	0.457	0.038

Table IV (cont.)

Panel G - Start Date

	Moderate or Severe Delinquency (Yes/No)	Severe Delinquency (Yes/No)	Number of Times DPD								
			Over 30			Over 60			Over 90		
	OLS	OLS	OLS	OLS	Tobit	OLS	OLS	Tobit	OLS	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Information	-0.026***	-0.022**	-0.002	-0.003	-0.045	-0.009	0.032**	0.090	-0.080	-0.040***	-0.607*
	(0.014)	(0.009)	(0.061)	(0.023)	(0.141)	(0.051)	(0.015)	(0.026)	(0.049)	(0.014)	(0.340)
Scheduled Maturity	0.001***	0.001***	0.006***	0.006***	0.013***	0.003***	0.003***	0.015***	0.002***	0.002***	0.019***
	(0.0002)	(0.0001)	(0.001)	(0.0003)	(0.002)	(0.001)	(0.0002)	(0.003)	(0.0005)	(0.0002)	(0.005)
Firm Fixed Effects	Yes	Yes	No	Yes	No	No	Yes	No	No	Yes	No
No. of Observations	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.454	0.458	0.109	0.581	0.048	0.072	0.556	0.048	0.041	0.455	0.043

Panel H - Mid-Date

	Moderate or Severe Delinquency (Yes/No)	Severe Delinquency (Yes/No)	Number of Times DPD								
			Over 30			Over 60			Over 90		
	OLS	OLS	OLS	OLS	Tobit	OLS	OLS	Tobit	OLS	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Information	-0.082***	-0.009	-0.162***	-0.157***	-0.348***	-0.013	-0.019*	0.063	-0.024	-0.029***	-0.062
	(0.011)	(0.006)	(0.048)	(0.018)	(0.115)	(0.035)	(0.011)	(0.176)	(0.033)	(0.011)	(0.244)
Scheduled Maturity	0.001***	0.001***	0.006***	0.006***	0.014***	0.003***	0.003***	0.017***	0.002***	0.001***	0.020***
	(0.0002)	(0.0001)	(0.001)	(0.0003)	(0.002)	(0.0005)	(0.0002)	(0.003)	(0.0005)	(0.0002)	(0.005)
Firm Fixed Effects	Yes	Yes	No	Yes	No	No	Yes	No	No	Yes	No
No. of Observations	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.455	0.458	0.106	0.582	0.046	0.073	0.555	0.049	0.044	0.456	0.045

Panel I - Close Date

	Moderate or Severe Delinquency (Yes/No)	Severe Delinquency (Yes/No)	Number of Times DPD								
			Over 30			Over 60			Over 90		
	OLS	OLS	OLS	OLS	Tobit	OLS	OLS	Tobit	OLS	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Information	-0.090***	-0.004	-0.231***	-0.182***	-0.516***	-0.036	-0.023**	-0.075	-0.125	-0.019**	-0.032
	(0.009)	(0.005)	(0.038)	(0.014)	(0.095)	(0.026)	(0.009)	(0.132)	(0.023)	(0.009)	(0.188)
Scheduled Maturity	0.001***	0.001***	0.006***	0.005***	0.013***	0.004***	0.003***	0.018***	0.002***	0.001***	0.022***
	(0.0002)	(0.0001)	(0.001)	(0.0003)	(0.003)	(0.001)	(0.0002)	(0.003)	(0.0005)	(0.0002)	(0.004)
Firm Fixed Effects	Yes	Yes	No	Yes	No	No	Yes	No	No	Yes	No
No. of Observations	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588	21,588
Adj R-sq (Pseudo R-sq if Tobit)	0.456	0.459	0.104	0.584	0.044	0.069	0.556	0.046	0.045	0.456	0.045

Table V

Information Sharing and Contract Performance - Robustness Tests

This table reports coefficient estimates and associated standard errors (in parentheses) for the impact of bureau affiliation on contract delinquencies excluding, in Panels A-C, the oldest 1 percent or 5 percent of contracts (respectively, contracts started before November 1994 and before November 1997) or including, in Panels D-E, contracts still open as of the last quarter of the sample (2007Q2). Average Days Past Due (DPD) and Maximum Days Past Due (DPD) are respectively the average and the maximum number of days payments were past due during the contract (both measured in logs). Information is a dummy variable that takes on the value of one if the contract began after lender entry into the PayNet bureau, and zero otherwise. Actual duration is the actual length of the contract while scheduled maturity is the planned one. All the regressions include lender dummies and time dummies. For all the dependent variables, we estimate ordinary least squares (OLS) regressions with firm fixed effects. Panels also represent different ways to construct time dummies. Time dummies in the Start Date panels equal one if a contract starts in a given year and zero otherwise, and similarly for Mid-Date and Close Date. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% level, respectively. The table also reports the adjusted R-squared.

Panel A - Start Date

	Drop 1% Oldest					Drop 5% Oldest				
	Average DPD	Maximum DPD	No. of Times DPD Over30	Over 60	Over 90	Average DPD	Maximum DPD	No. of Times DPD Over30	Over 60	Over 90
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Information	-0.107***	-0.166***	-0.009	0.029**	-0.034***	-0.085***	-0.152***	-0.107	0.027*	-0.037***
	(0.033)	(0.051)	(0.023)	(0.014)	(0.013)	(.033)	(0.051)	(0.022)	(0.014)	(0.014)
Observations	17,055	21,301	21,301	21,301	21,301	16,280	20,156	20,156	20,156	20,156
Adj R-sq	0.649	0.626	0.585	0.561	0.461	0.662	0.623	0.608	0.582	0.477

Panel B - Mid Date

	Drop 1% Oldest					Drop 5% Oldest				
	Average DPD	Maximum DPD	No. of Times DPD Over30	Over 60	Over 90	Average DPD	Maximum DPD	No. of Times DPD Over30	Over 60	Over 90
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Information	-0.115***	-0.475***	-0.209***	-0.047***	-0.043***	-0.117***	-0.483***	-0.213***	-0.053***	-0.045***
	(0.025)	(0.040)	(0.017)	(0.011)	(0.011)	(0.025)	(0.039)	(0.017)	(0.011)	(0.011)
Observations	17,055	21,301	21,301	21,301	21,301	16,280	20,156	20,156	20,156	20,156
Adj R-sq	0.651	0.520	0.586	0.561	0.462	0.664	0.540	0.610	0.582	0.478

Panel C - Close Date

	Drop 1% Oldest					Drop 5% Oldest				
	Average DPD	Maximum DPD	No. of Times DPD Over30	Over 60	Over 90	Average DPD	Maximum DPD	No. of Times DPD Over30	Over 60	Over 90
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Information	-0.148***	-0.549***	-0.233***	-0.054***	-0.034***	-0.153***	-0.556***	-0.237***	-0.058***	-0.036***
	(0.019)	(0.030)	(0.014)	(0.009)	(0.008)	(0.020)	(0.031)	(0.013)	(0.009)	(0.008)
Observations	17,055	21,301	21,301	21,301	21,301	16,280	20,156	20,156	20,156	20,156
Adj R-sq	0.654	0.525	0.590	0.562	0.463	0.667	0.545	0.615	0.583	0.480

Table V (cont.)

Panel D - Startdate

	Average				Maximum						No. of Times DPD				
		DPD			DPD			Over30			Over 60			Over 90	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Information	-0.086***	-0.083***	-0.084***	-0.168	-0.157***	-0.161***	0.040*	0.042**	0.041**	0.039***	0.035***	0.036***	-0.040***	-0.039***	-0.039***
	(0.031)	(0.031)	(0.031)	(0.103)	(0.048)	(0.048)	(0.020)	(0.020)	(0.020)	(0.011)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)
Actual Duration		0.004***			0.013***			0.004***			0.001***			0.001***	
		(0.0005)			(0.0007)			(0.0003)			(0.0001)			(0.0001)	
Scheduled Maturity			0.002***			0.006***			0.002***			0.002***			0.001***
			(0.0005)			(0.001)			(0.0003)			(0.0002)			(0.0001)
Observations	27,274	27,255	27,274	28,620	28,601	28,620	28,623	28,603	28,622	28,623	28,603	28,622	28,623	28,603	28,622
Adj R-sq	0.438	0.440	0.439	0.416	0.424	0.417	0.533	0.537	0.534	0.530	0.532	0.532	0.450	0.452	0.450

Panel E - Middate

	Average				Maximum						No. of Times DPD				
		DPD			DPD			Over30			Over 60			Over 90	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Information	-0.219***	-0.153***	-0.204***	-0.559***	-0.288***	-0.517***	-0.235***	-0.146***	-0.209***	-0.052***	-0.023**	-0.036***	-0.039***	-0.021***	-0.031***
	(0.022)	(0.023)	(0.023)	(0.035)	(0.036)	(0.0007)	(0.014)	(0.015)	(0.015)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)
Actual Duration		0.004***			0.016***			0.005***			0.002***			0.001***	
		(0.0004)			(0.0007)			(0.0003)			(0.0002)			(0.0002)	
Scheduled Maturity			0.002***			0.006***			0.004***			0.002***			0.001***
			(0.0004)			(0.0007)			(0.0003)			(0.0002)			(0.0002)
Observations	27,274	27,255	27,274	28,620	28,601	28,620	28,623	28,603	28,622	28,623	28,603	28,622	28,623	28,603	28,622
Adj R-sq	0.438	0.441	0.483	0.408	0.422	0.409	0.546	0.553	0.549	0.533	0.536	0.536	0.452	0.454	0.453

Panel F - Closedate

	Average				Maximum						No. of Times DPD				
		DPD			DPD			Over30			Over 60			Over 90	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Information	-0.237***	-0.170***	-0.221***	-0.625***	-0.321***	-0.583***	-0.263***	-0.163***	-0.236***	-0.098***	-0.032***	-0.044***	-0.064***	-0.016**	-0.025***
	(0.018)	(0.020)	(0.019)	(0.028)	(0.031)	(0.029)	(0.012)	(0.013)	(0.012)	(0.007)	(0.008)	(0.007)	(0.006)	(0.007)	(0.007)
Actual Duration		0.003***			0.015***			0.005***			0.002***			0.001***	
		(0.0004)			(0.0007)			(0.0003)			(0.0002)			(0.0001)	
Scheduled Maturity			0.002***			0.005***			0.003***			0.002***			0.001***
			(0.0005)			(0.001)			(0.0003)			(0.0002)			(0.0002)
Observations	27,274	27,255	27,274	28,620	28,601	28,620	28,623	28,603	28,622	28,623	28,603	28,622	28,623	28,603	28,622
Adj R-sq	0.440	0.442	0.441	0.411	0.422	0.412	0.544	0.550	0.546	0.529	0.535	0.535	0.451	0.455	0.454

Table VI
Information Sharing and Contract Performance - Other Robustness Tests

This table reports coefficient estimates and associated standard errors (in parentheses) for the impact of bureau affiliation on delinquencies after controlling for contract attributes. Average Days Past Due (DPD) and Maximum DPD are respectively the average and the maximum number of days payments were late during the contract (both measured in logs). Information is a dummy variable equal to one if a contract began after lender entry into the PayNet bureau, and zero otherwise. Contract amount is measured in millions of dollars, and maturity in months. All other characteristics are captured by dummies which equal one if the contract has the specified property, and zero otherwise. All the regressions include lender dummies and time dummies. To conserve space, the table only reports results obtained using contract mid-dates to construct time dummies. For each dependent variable, we report four models: Two ordinary least squares (OLS) with firm dummies for two sets of controls, and, for the smaller set of controls, two Tobits, one without firm dummies but with heteroskedasticity-robust standard errors clustered at the firm level, and one with firm dummies. ***, ** and * indicate significance at the 1%, 5%, and 10% level, respectively. For all the regressions, the table also reports the adjusted R-squared (pseudo R-squared if Tobit). In the regressions, n.a. appears when the Tobit did not converge.

	Average DPD				Maximum DPD			
	OLS	OLS	Tobit	Tobit	OLS	OLS	Tobit	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information	-0.094***	-0.084***	-0.518***	-0.306	-0.377***	-0.383***	-0.712***	-0.624
	(0.027)	(0.026)	(0.141)	n.a.	(0.043)	(0.040)	(0.172)	n.a.
Contract Size	-0.002	-0.001	-0.002***	-0.007	-0.004	0.004	-0.0011*	-0.008
	(0.002)	(0.002)	(0.0002)	n.a.	(0.003)	(0.003)	(0.0006)	n.a.
Scheduled Maturity	0.003***	0.0025***	0.022***	0.007	0.010***	0.008***	0.022***	0.014
	(0.0005)	(0.0005)	(0.003)	n.a.	(0.001)	(0.001)	(0.003)	n.a.
Guarantor	-0.013	0.332	0.136*	-0.018	-0.022	-0.057	0.087	-0.028
	(0.017)	(0.207)	(0.074)	n.a.	(0.026)	(0.25)	(0.08)	n.a.
Loan	-0.062	-0.335***	-0.029	-0.088	-0.108	-0.416***	-0.190	-0.158
	(0.043)	(0.071)	(0.230)	n.a.	(0.068)	(0.118)	(0.254)	n.a.
True Lease	-0.080**	-0.190***	0.622***	0.238	-0.101	-0.197*	0.736***	0.169
	(0.039)	(0.063)	(0.208)	n.a.	(0.062)	(0.106)	(0.213)	n.a.
Conditional Sale	-0.065	-0.285***	0.737***	-0.029	-0.059	-0.329***	0.683***	0.003
	(0.045)	(0.070)	(0.220)	n.a.	(0.071)	(0.115)	(0.216)	n.a.
Monthly Freq.		-0.254				-1.093		
		(0.765)				(1.361)		
Quarterly Freq.		-0.445				-1.057		
		(0.769)				(1.362)		
Annual Freq.		-0.474				-1.468		
		(0.769)				(1.365)		
Equipment Type	No	Yes	No	No	No	Yes	No	No
Other Contract Attributes	No	Yes	No	No	No	Yes	No	No
Firm Fixed Effects	Yes	Yes	No	Yes	Yes	Yes	No	Yes
No. of Observations	14,932	14,932	14,932	14,932	18,798	18,798	18,798	18,798
Adj R-sq (Pseudo R-sq if Tobit)	0.664	0.672	0.065	0.388	0.664	0.518	0.050	0.242

Table VII
Information Sharing and Contract Performance - Timing Issues

This table reports coefficient estimates and associated standard errors (in parentheses) for the impact of bureau affiliation on contract performance allowing affiliation to become effective either with a lead (i.e., on contracts already open) or with a lag (i.e., on contracts starting at least some time after the lender entry into the bureau). For example, column -2y (+3m) displays estimated coefficients for a dummy variable that is equal to one if the contract start date is no earlier than the lender entry date date minus two years (plus three months) and the close date is after lender entry, and zero otherwise. For all the dependent variables, we estimate ordinary least squares (OLS) regressions with firm fixed effects, lender dummies and time dummies. In Panel A, we construct time dummies using contract start dates. Panels B and C show estimates when we construct time dummies using mid-dates and close dates. Average Days Past Due (DPD) and Maximum Days Past Due (DPD) are respectively the average and the maximum number of days payments were past due during the contract (in logs). The other measures of delinquencies are the number of times over 30 DPD and over 90 DPD (all in logs). *, ** and *** denote statistical significance at the 10%, 5% and 1% level, respectively.

						Panel A - Start Date					
	Impact on Open Contracts						Lagged Impact on Contracts				
	-2y	-1y	-6m	-3m	-1m	0	+1m	+3m	+6m	+1y	+2y
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Average DPD	0.071**	-0.068**	-0.114***	-0.078**	-0.011***	-0.011***	-0.137***	-0.081**	-0.055*	-0.014	0.093**
	(0.312)	(0.031)	(0.031)	(0.031)	(0.032)	(0.033)	(0.033)	(0.033)	(0.030)	(0.032)	(0.036)
Maximum DPD	0.373***	-0.059	-0.093*	-0.050	-0.131***	-0.167***	-0.200***	-0.153***	-0.050	-0.065	0.086
	(0.048)	(0.049)	(0.050)	(0.048)	(0.050)	(0.052)	(0.052)	(0.052)	(0.048)	(0.051)	(0.058)
No. of Times over 30 DPD	0.193***	0.030	0.016	0.033	-0.003	-0.005	-0.030	-0.0001	0.069***	0.070***	0.020
	(0.021)	(0.022)	(0.022)	(0.022)	(0.022)	(0.023)	(0.023)	(0.023)	(0.022)	(0.023)	(0.026)
No. of Times over 90 DPD	0.023*	-0.034***	0.001	0.013	-0.030**	-0.041***	-0.028**	-0.004	0.039***	0.042***	0.062***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.013)	(0.014)	(0.016)
						Panel B - Mid-Date					
	Impact on Open Contracts						Lagged Impact on Contracts				
	-2y	-1y	-6m	-3m	-1m	0	+1m	+3m	+6m	+1y	+2y
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Average DPD	-0.047	-0.125***	-0.140***	-0.101***	-0.118***	-0.115***	-0.125***	-0.084***	-0.060*	-0.002	-0.097***
	(0.029)	(0.025)	(0.025)	(0.025)	(0.026)	(0.026)	(0.025)	(0.025)	(0.025)	(0.025)	(0.029)
Maximum DPD	-0.036	-0.375***	-0.408***	-0.390***	-0.448***	-0.472***	-0.487***	-0.454***	-0.385***	-0.314***	-0.243***
	(0.045)	(0.039)	(0.039)	(0.039)	(0.040)	(0.040)	(0.040)	(0.039)	(0.040)	(0.041)	(0.047)
No. of Times over 30 DPD	-0.067***	-0.188***	-0.196***	-0.182***	-0.205***	-0.206***	-0.218***	-0.194***	-0.135***	-0.118***	-0.098***
	(0.020)	(0.017)	(0.017)	(0.017)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.021)
No. of Times over 90 DPD	-0.024***	-0.053***	-0.026**	-0.014	-0.038***	-0.043***	-0.033***	-0.016	0.013	0.009	0.030**
	(0.012)	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.013)
						Panel C - Close Date					
	Impact on Open Contracts						Lagged Impact on Contracts				
	-2y	-1y	-6m	-3m	-1m	0	+1m	+3m	+6m	+1y	+2y
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Average DPD	-0.115***	-0.166***	-0.171***	-0.146***	-0.148***	-0.144***	-0.149***	-0.119***	-0.097***	-0.050**	0.034
	(0.024)	(0.020)	(0.020)	(0.019)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.023)
Maximum DPD	-0.205***	-0.461***	-0.500***	-0.490***	-0.525***	-0.541***	-0.549***	-0.528***	-0.482***	-0.428***	-0.374***
	(0.037)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.037)
No. of Times over 30 DPD	-0.200***	-0.260***	-0.260***	-0.250***	-0.230***	-0.230***	-0.236***	-0.219***	-0.179***	-0.161***	-0.148***
	(0.015)	(0.012)	(0.012)	(0.012)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.017)
No. of Times over 90 DPD	-0.032***	-0.048***	-0.027***	-0.019***	-0.030***	-0.033***	-0.027***	-0.018**	-0.002	0.007	0.006
	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.010)

Table VIII

Information Sharing and Contract Performance - Subsampling by Firm Attributes

This table reports coefficient estimates and associated standard errors (in parentheses) for the impact of bureau affiliation on contract delinquencies obtained by sorting firms according to their number of employees (Panel A), age (Panel B), and rating (Panel C). The number of employees is reported as of 2007, while age and rating refer to the time when the contract began. Since information on employees and age is missing for a portion of our sample, we rerun the basic regressions including only contracts of firms for which these characteristics are known, and report these results under the label 'All'. In Panel A, the label '<=100' refers to estimates from regressions including only firms with 100 employees or less, and similarly for the label '<=500'. In Panel B, the label '<=5.58' refers to regressions including a contract only if the firm is in the bottom quartile of age (as of the contract start date), and '<=11.68' to contracts of firms below the median age as of the contract start date. In Panel C, estimates in columns labeled 'All' refer to contracts such that the firm rating on the contract start date was available, and 30 and 55 represent the ratings at the 25th percentile and at the median. Average Days Past Due (DPD) and Maximum Days Past Due (DPD) are respectively the average and the maximum number of days payments were past due during the contract (both measured in logs). Information is a dummy variable that takes on the value of one if the contract began after lender entry into the PayNet bureau, and zero otherwise. All ordinary least squares (OLS) and Tobit regressions include firm fixed effects, lender dummies and time dummies. In the regressions, n.a. appears when the Tobit did not converge.

Panel A - Subsampling by Number of Employees

		Average DPD			Maximum DPD	Over 30 DPD	Over 60 DPD	Over 90 DPD
		OLS	OLS	Tobit	OLS	OLS	OLS	OLS
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Start Date								
All	Information	-0.089**	-0.091**	-0.281***	-0.101	0.062**	0.033*	-0.042**
		(0.041)	(0.041)	(0.086)	(0.064)	(0.029)	(0.018)	(0.017)
	Sched. Mat.		0.005***					
			(0.001)					
<= 100	Information	-0.057	-0.060	-0.204	-0.091	0.052	0.007	-0.058***
		(0.045)	(0.045)	n.a.	(0.072)	(0.032)	(0.020)	(0.020)
	Sched. Mat.		0.006***					
			(0.001)					
<= 500	Information	-0.089**	-0.090**	-0.283***	-0.083	0.052*	0.026	-0.045**
		(0.042)	-0.042	(0.088)	(0.066)	(0.029)	(0.018)	(0.017)
	Sched. Mat.		-0.005***					
			(0.001)					
Mid-Date								
All	Information	-0.085	-0.057*	-0.299	-0.372***	-0.139***	-0.047***	-0.050***
		(0.032)	(0.032)	n.a.	(0.050)	(0.023)	(0.14)	(0.013)
	Sched. Mat.		0.004***					
			(0.004)					
<= 100	Information	-0.083**	0.049	-0.267***	-0.390***	-0.143***	-0.072***	-0.071***
		(0.035)	(0.035)	(0.073)	(0.055)	(0.025)	(0.16)	(0.015)
	Sched. Mat.		0.005***					
			(0.001)					
<= 500	Information	-0.088	-0.57*	-0.306	-0.382***	-0.144***	-0.054***	-0.056***
		(0.032)	(0.033)	n.a.	(0.051)	(0.023)	(0.14)	(0.013)
	Sched. Mat.		0.004***					
			(0.001)					
Close Date								
All	Information	-0.125***	-0.096***	-0.382***	-0.473***	-0.194***	-0.058***	-0.037***
		(0.024)	-0.025	(0.052)	(0.038)	(0.017)	(0.011)	(0.010)
	Sched. Mat.		0.004***					
			(0.001)					
<= 100	Information	-0.121***	-0.086***	-0.376	-0.498***	-0.183***	-0.073***	-0.051***
		(0.027)	-0.028	n.a.	(0.042)	(0.019)	(0.012)	(0.011)
	Sched. Mat.		0.004***					
			(0.001)					
<= 500	Information	-0.120***	-0.091***	-0.372	-0.478***	-0.189***	-0.058***	-0.039***
		(0.025)	(0.026)	n.a.	(0.039)	(0.017)	(0.011)	(0.010)
	Sched. Mat.		0.004***					
			(0.001)					

Panel B - Subsampling by Firm Age

		Average DPD			Maximum DPD	Over 30 DPD	Over 60 DPD	Over 90 DPD
		OLS	OLS	Tobit	OLS	OLS	OLS	OLS
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Start Date								
All	Information	-0.126***	-0.128***	-0.342	-0.168***	0.021	-0.004	-0.075***
		(0.040)	(0.040)	n.a.	(0.065)	(0.029)	(0.019)	(0.017)
	Sched. Mat.		0.005***					
			(0.001)					
<=5.58	Information	-0.056	-0.056	-0.125	-0.333**	0.007	-0.083**	-0.120***
		(0.089)	(0.089)	n.a.	(0.135)	(0.061)	(0.039)	(0.039)
	Sched. Mat.		0.006					
			(0.001)					
<= 11.68	Information	-0.026	-0.039	-0.115	-0.170*	0.062	-0.007	-0.066***
		(0.059)	(0.059)	n.a.	(0.093)	(0.041)	(0.027)	(0.025)
	Sched. Mat.		0.006***					

Table VIII (cont.)

				Mid-Date				
All	Information	-0.130***	-0.094***	-0.380***	-0.443***	-0.188***	-0.087***	-0.086***
		(0.032)	(0.032)	(0.069)	(0.050)	(0.023)	(0.015)	(0.013)
	Sched. Mat.		0.005***					
			(0.001)					
<=5.58	Information	-0.088	-0.045	-0.240	-0.604***	-0.151***	-0.080**	-0.102***
		(0.071)	(0.072)	n.a.	(0.108)	(0.049)	(0.032)	(0.031)
	Sched. Mat.		0.005***					
			(0.002)					
<= 11.68	Information	-0.101**	-0.054	-0.361	-0.505***	-0.140***	-0.044**	-0.052***
		(0.046)	(0.047)	n.a.	(0.073)	(0.032)	(0.021)	(0.020)
	Sched. Mat.		0.006***					
			(0.001)					
				Close Date				
All	Information	-0.158***	-0.122***	-0.440***	-0.522***	-0.213***	-0.083***	-0.058***
		(0.024)	(0.025)	(0.052)	(0.038)	(0.017)	(0.011)	(0.010)
	Sched. Mat.		0.004***					
			(0.001)					
<=5.58	Information	-0.028	0.016	-0.082	-0.493***	-0.096**	-0.061**	-0.085***
		(0.060)	(0.062)	n.a.	(0.091)	(0.041)	(0.027)	(0.026)
	Sched. Mat.		0.004***					
			(0.002)					
<= 11.68	Information	-0.128***	-0.074**	-0.401	-0.578***	-0.155***	-0.044***	-0.041***
		(0.036)	(0.038)	n.a.	(0.057)	(0.025)	(0.016)	(0.016)
	Sched. Mat.		0.005***					
			(0.001)					

Panel C - Subsampling by Firm Rating

				Start Date				
			Average DPD		Maximum DPD	Over 30 DPD	Over 60 DPD	Over 90 DPD
		OLS	OLS	Tobit	OLS	OLS	OLS	OLS
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
All	Information	-0.025	-0.024	-0.076	-0.189***	0.013	0.047***	-0.056***
		(0.036)	(0.036)	n.a.	(0.060)	(0.026)	(0.018)	(0.017)
	Sched. Mat.		0.002***					
			(0.0007)					
<=30	Information	0.120	0.121	0.171	0.030	0.174**	0.127**	0.006
		(0.114)	(0.114)	n.a.	(0.135)	(0.074)	(0.062)	(0.058)
	Sched. Mat.		0.001					
			(0.001)					
<=55	Information	-0.002	0.002	-0.037	-0.164*	0.068	0.068*	-0.027
		(0.067)	(0.067)	n.a.	(0.090)	(0.046)	(0.035)	(0.032)
	Sched. Mat.		0.003***					
			(0.001)					
				Mid-Date				
All	Information	-0.103***	-0.080***	-0.243***	-0.484***	-0.176***	-0.028*	-0.056***
		(0.029)	(0.030)	(0.050)	(0.048)	(0.020)	(0.014)	(0.014)
	Sched. Mat.		0.003***					
			(0.001)					
<=30	Information	-0.081	-0.061	-0.096	-0.458***	-0.400***	-0.170***	-0.121***
		(0.084)	(0.087)	(0.088)	(0.096)	(0.053)	(0.044)	(0.041)
	Sched. Mat.		0.002					
			(0.002)					
<=55	Information	-0.088*	-0.056	-0.125*	-0.407***	-0.250***	-0.096***	-0.074***
		(0.053)	(0.054)	(0.064)	(0.070)	(0.035)	(0.027)	(0.025)
	Sched. Mat.		0.003***					
			(0.001)					
				Close Date				
All	Information	-0.147***	-0.127***	-0.328	-0.551***	-0.186***	-0.029**	-0.039***
		(0.024)	(0.025)	n.a.	(0.040)	(0.017)	(0.012)	(0.012)
	Sched. Mat.		0.003***					
			(0.001)					
<=30	Information	-0.142**	-0.123*	-0.176	-0.523***	-0.399***	-0.158***	-0.113***
		(0.072)	(0.074)	n.a.	(0.082)	(0.045)	(0.037)	(0.035)
	Sched. Mat.		0.002					
			(0.002)					
<=55	Information	-0.121***	-0.089*	-0.187	-0.434***	-0.212***	-0.093***	-0.067***
		(0.045)	(0.047)	n.a.	(0.060)	(0.030)	(0.023)	(0.021)
	Sched. Mat.		0.004***					
			(0.001)					

Table IX
Information Sharing and Contract Performance - Alternative Specifications

This table reports coefficient estimates and associated standard errors (in parentheses) for the impact of bureau affiliation on contract performance using alternative specifications. All the regressions include lender dummies, time dummies and, unless otherwise specified, firm fixed effects. In Panel A, we subsample by contract duration, breaking up the sample into short-duration contracts (less than 2 yrs), medium-duration contracts (2-4 yrs) and long-duration contracts (over 4 yrs). In Panel B, we allow for time-varying characteristics of lenders by including a set of indicator variables which interact lender dummies with year dummies (coefficients not reported). Information is a dummy variable that takes on the value of one if the contract began after lender entry into the bureau, and zero otherwise. For all measures, we run an ordinary least squares (OLS) model, and for the Average DPD measure we also run a Tobit model. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% level, respectively. For all the regressions, the table also reports the adjusted R-squared (pseudo R-squared if Tobit). In the regressions, n.a. appears when the Tobit did not converge.

Panel A - Subsampling by Contract Duration

	Average DPD			Average DPD		
Duration (in months)	< 24	24-48	>48	< 24	24-48	>48
	OLS	OLS	OLS	Tobit	Tobit	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)
Information	-0.396***	-0.040	-0.198**	-0.951	-0.197	-0.451
	(0.060)	(0.040)	(0.086)	n.a.	n.a.	n.a.
No. of observations	6,251	7,983	2,982	6,235	7,996	2,985
Adj R-sq (Pseudo R-Sq if Tobit)	0.727	0.686	0.714	0.470	0.448	0.516

	Maximum DPD			Number of Times DPD Over 30			Over 60			Over 90		
Duration (in months)	< 24	24-48	>48	< 24	24-48	>48	< 24	24-48	>48	< 24	24-48	>48
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Information	-0.436***	-0.295***	-0.151	-0.123***	-0.044	-0.049	-0.026	-0.006	-0.032	-0.064***	-0.049***	-0.034
	(0.098)	(0.066)	(0.124)	(0.033)	(0.029)	(0.067)	(0.023)	(0.018)	(0.042)	(0.022)	(0.018)	(0.036)
No. of Observations	7,526	10,185	3,877	7,526	10,185	3,877	7,526	10,185	3877	7526	10,185	3,877
Adj R-sq	0.571	0.541	0.571	0.647	0.648	0.652	0.624	0.625	0.628	0.557	0.486	0.604

Panel B - Adding Time-by-Lender Controls

	Average DPD	Maximum DPD	Number of Times DPD Over 30	Over 60	Over 90
	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)
Information	-0.125***	-0.536***	-0.249***	-0.078***	-0.055***
	(0.027)	(0.043)	(0.019)	(0.012)	(0.011)
No. of Observations	17,216	21,588	21,588	21,588	21,588
Adj R-squared	0.665	0.524	0.581	0.564	0.467

Table X
Information Sharing and Contract Dynamics

This table reports coefficient or hazard ratio estimates and associated standard errors (in parentheses) for the impact of bureau affiliation on contract delinquencies using contract-quarter observations. In the regressions of Panels A and B, the dependent variable is the number of days past due (measured in logarithms) in a given quarter for a given contract. Panel C presents the results of a duration analysis for the time to occurrence of the first delinquency in a contract. In columns 2, 4, and 6 of Panels A and B and in columns 1 and 2 of Panel C information is a dummy variable that takes on the value of one if the contract began after lender entry into the PayNet bureau, and zero otherwise. In columns 1, 3, and 5 of Panels A and B information is a dummy variable that takes on the value of one if the lender was in the PayNet bureau in the quarter observed, and zero otherwise. The regressions in Panel B include the lagged dependent variable as well as its interaction with the information dummy. Regressions 1, 2, 5, and 6 of Panel A and 1, 2 of Panel B are estimated by ordinary least squares (OLS) with heteroskedasticity-robust standard errors clustered at the firm level. Regression 3 (4) of Panels A and B is estimated by Tobit with heteroskedasticity-robust standard errors clustered at the firm level (respectively, with firm fixed effects). Regressions 5 and 6 in Panel B are estimated with the Arellano and Bond generalized method of moments (GMM) approach. Regressions 1 and 2 of Panel C are estimated with the Cox model; regression 1 is estimated with with heteroskedasticity-robust standard errors clustered at the firm level while regression 2 is estimated with firm fixed effects. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% level, respectively. In Panels A and B, for the Arellano and Bond regressions the table also reports the results of a Wald test while for all the other regressions the table reports the adjusted R-squared (pseudo if Tobit). In Panel C, the table reports the results of an inference test that all model parameters are zero.

Panel A - No Lagged Dependent Variable

	OLS	OLS	Tobit	Tobit	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Information	-0.088 *	-0.142***	-0.182	-0.616***	-0.024 *	-0.772***
	(0.055)	(0.042)	(0.183)	(0.187)	(0.015)	(0.010)
Firm Fixed Effects	No	No	No	No	Yes	Yes
Effect on Open Contracts	Yes	No	Yes	No	Yes	No
No. of Observations	147,935	147,935	147,935	147,935	147,935	147,709
Adj. R-sq (Pseudo R-sq if Tobit)	0.045	0.048	0.029	0.029	0.383	0.463

Panel B - Lagged Dependent Variable

	OLS	OLS	Tobit	Tobit	Arellano and Bond	
	(1)	(2)	(3)	(4)	(5)	(6)
Information	-0.016	-0.031***	-0.519***	-0.412***	0.361***	---
	(0.015)	(0.009)	(0.183)	(0.029)	(0.028)	---
Lagged Dependent	0.397***	0.371***	1.289***	1.376***	0.236***	0.262***
	(0.005)	(0.005)	(0.042)	(0.040)	(0.006)	(0.006)
Interaction	-0.041***	-0.015***	0.260***	0.278***	-0.631***	-0.668***
	(0.005)	(0.005)	(0.044)	(0.039)	(0.007)	(0.008)
Firm Fixed Effects	Yes	Yes	No	No	Yes	Yes
Effect on Open Contracts	Yes	No	Yes	No	Yes	No
No. of Observations	147,709	147,709	147,709	147,709	123,113	123,113
Adj. R-sq (Pseudo R-sq if Tobit)	0.359	0.463	0.151	0.151		
Wald Test					9,990.49	8,972.68

Panel C - Duration Analysis

	Cox Model	Cox Model
	(1)	(2)
Information (Hazard Ratio)	0.707***	0.776***
	(0.067)	(0.040)
Firm Fixed Effects	No	Yes
No. of Observations	28,589	28,589
Inference Test	(Wald) 2,486.5***	(LR) 17,194.85***

Table XI

Information Sharing and Contract Terms

This table reports coefficient estimates and associated standard errors (in parentheses) for the impact of bureau affiliation on contract characteristics (whether there is a guarantor for the contract or not and the contract size). All the regressions include lender, firm, and time controls. Guarantor is a dummy variable that takes on the value of one if there is a guarantor for the contract, zero otherwise. We instead measure contract size as the logarithm of the total dollar amount the firm must pay over the life of the contract. Information is a dummy variable that takes on the value of one if the contract began after lender entry into the PayNet bureau, and zero otherwise. The regressions represent different ways in which time dummies have been constructed (using the start date, mid-date or close date of the contract). In regression (1) of all panels, we use all firms, in regression (2) we restrict attention to firms below the median rating, in regression (3) to firms below the median age, in regression (4) to firms with less than 100 employees. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% level, respectively. For all the regressions, the table also reports the adjusted R-squared.

Panel A - Gurarantor				
	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)
Start Date	All Firms	Rating <=55	Age <= 11	Employees <= 100
Information	0.113	0.065***	0.017	0.002
	(0.012)	(0.021)	(0.023)	(0.018)
No. of Observations	24,868	8,057	7,701	12,511
Adj R-sq	0.597	0.713	0.597	0.555
Mid-Date				
Information	0.180**	-0.036	-0.035*	-0.015
	(0.008)	(0.023)	(0.020)	(0.015)
No. of Observations	24,868	7,799	7,855	12,618
Adj R-sq	0.597	0.340	0.300	0.393
Close Date				
Information	0.024***	0.019	0.017	0.011
	(0.007)	(0.014)	(0.014)	(0.010)
No. of Observations	24,868	8,057	7,701	12,511
Adj R-sq	0.596	0.712	0.597	0.554

Panel B - Contract Size				
	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)
Start Date	All Firms	Rating <=55	Age <= 11	Employees <= 100
Information	-0.564*	-0.198***	0.073	-0.009
	(0.031)	(0.060)	(0.024)	(0.054)
No. of Observations	28,612	9,001	8,902	14,299
Adj R-sq	0.624	0.691	0.631	0.612
Mid-Date				
Information	-0.111***	-0.229***	-0.063	-0.097***
	(0.022)	(0.044)	(0.039)	(0.029)
No. of Observations	28,612	9,001	8,902	14,299
Adj R-sq	0.624	0.693	0.632	0.612
Close Date				
Information	-0.136***	-0.216***	-0.097***	-0.119***
	(0.018)	(0.039)	(0.033)	(0.024)
No. of Observations	28,612	9,001	8,902	14,299
Adj R-sq	0.625	0.695	0.633	0.613