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Learning about Competitors: Evidence from SME Lending

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Abstract

We study how small and medium enterprise (SME) lenders react to information about their competitors' contracting decisions. To isolate this learning from lenders' joint reaction to unobserved common shocks to fundamentals, we exploit the staggered entry of lenders into an information sharing platform. Upon joining the platform, lenders adjust their contract terms toward what others are offering. This effect is mediated by market structure: lenders with higher market share or operating in concentrated markets react less to competitors' information. Contract terms and outcomes are thus shaped by the availability of competitor information and not just by borrower or lender fundamentals.

Keywords: learning, information sharing, market structure, corporate loans, SME lending, loan maturity

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Introduction

Information shapes markets and strategic behavior. Indeed, in many settings, information is *dispersed*: market participants do not have full information about their counterparties or their competitors' actions. Strategic and information considerations are thus linked: agents' optimal actions depend on their information about competitors' actions. Recent advances in information technology have attracted considerable attention from academics and policymakers concerned with the effects on competition.[1] In the context of credit markets, information technology has been studied primarily through the lens of learning about *borrowers* through the revelation of their credit records or the collection of soft information. However, there is also an increased scope for learning about *competitors*, which introduces new issues related to competition, information aggregation, and the distribution of loan terms.

Conceptually, the implications of lenders learning about their competitors are largely unresolved. As Vives (2006) notes, "the analysis of information sharing is complex [and] depends on the type of competition, uncertainty as well as on the number of firms." Existing theoretical models imply a wealth of empirical predictions, with considerable disagreement over channels, magnitudes, and even the sign of these effects. The industrial organization literature emphasizes the role of imperfect competition, suggesting that lenders can either mimic rivals, if there are strategic complementarities, or differentiate themselves through product choice. There is also a role for information aggregation, in which rivals' actions partially reveal their private information. Moreover, recent work has shown that the link between information and market outcomes is more complex than previously thought (Murfin and Pratt, 2017a; Liberman et al., 2018; Goldstein and Yang, 2019).

The main contribution of this paper is to address the paucity of empirical research on this issue. The goal is not to distinguish among all existing models but to make progress by providing several credible empirical facts. Questions related to information and imperfect competition are notoriously difficult to study empirically. Indeed, the empirical challenge in estimating the effect of learning about competitors is how to isolate variation in agents' information sets. Specifically, lenders might offer similar terms not because they respond to each other but simply because they respond to the same economic shock.

Our paper addresses this challenge by exploiting a unique setting that permits us to observe a direct shift in information that lenders have about rivals. Specifically, we use micro lending data around the introduction of a commercial credit information sharing platform, PayNet, covering small and medium enterprises (SMEs) in the United States. PayNet

[1] In the words of European Commissioner for Competition Margrethe Vestager, "the future of big data is not just about technology. It's about things like. . . competition." EDPS-BEUC Conference on Big Data and Competition, Brussels, September 29, 2016.

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launched in 2001; since then it has attracted eight of the 10 largest lenders in the market, a group that includes Bank of America, Wells Fargo, PNC, John Deere, IBM, Volvo, and Caterpillar. The platform provides information on contract terms offered by other lenders that was previously not widely available. We exploit the staggered entry of lenders to the platform to estimate the response to competitors and find that lenders adjust their terms toward what others are offering. Moreover, this effect is strongly mediated by the distribution of market shares: the effect is driven by markets with lower levels of concentration and lenders with low market shares. We provide additional evidence that this result cannot be explained by more conventional channels of information sharing, nor by lenders' joining at the same time as shocks unrelated to the platform. Finally, we investigate one important consequence of our findings: matching competitors tends to increase delinquencies during the recent crisis, possibly because of the neglect of future risk.

We document this evidence in the context of maturity dynamics for SMEs' equipment financing contracts from 2001 to 2014. With over $1 trillion of annual volume, equipment financing is a major component of corporate investment, particularly for SMEs. Because of their implications for firms' liquidity and investments, maturity cycles and rollover risk became a concern during the recent crisis and recovery. The Survey of Terms of Business Lending shows that maturity on loans lasting over a year fell by 30% between 2007 and 2010, and Kalemli-Ozcan et al. (2018) document the dramatic effect of rollover risk on firm investment. Moreover, in our context of financing a specific piece of equipment, it is natural to focus on maturity, as it is negotiable, while contract size is largely dictated by the equipment needed. And by design, interest rates are not shared in the platform, just as they are typically not shared in consumer credit bureaus. Finally, there is evidence consistent with oligopolistic competition in this market (Murfin and Pratt, 2017b).

Our empirical strategy addresses key empirical challenges associated with estimating the effect of learning about competitors. Specifically, two lenders can offer similar contracts not because they react to what the other is offering but simply because they react to the same shock to fundamentals. This is a crucial issue because it is plausible that at least some of these fundamentals cannot be observed by the econometrician and therefore cannot be controlled for. To address this challenge, we rely on two features of our setting. First, we exploit lenders joining the platform in a staggered fashion to generate variation in information sets within and across lenders over time. Second, for each borrower-lender relationship, we observe contracts made before and after the lender joins the platform.[2] Our empirical tests

[2] Joining involves an invasive implementation process in which PayNet establishes access to the lenders' IT systems to ensure complete and truthful sharing. PayNet uses shared information to create credit scores and reports for members. Nonmembers cannot access the system or its scores and reports.

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do not take a stand on the direction of the response. The key idea is that, while a lender's terms may track the bureau average before joining, whether they track it relatively *better* or *worse* afterward reveals the sign of the response.

However, this strategy potentially exposes us to two important confounders. First, lenders can respond to information on the platform other than rivals' terms—specifically borrower credit records. There is evidence that this reduction in asymmetric information leads lenders to start lending to borrowers with different characteristics (Liberti et al., 2016, 2018; Foley et al., 2018). To abstract from any change in borrower composition, our tests are conducted within an existing relationship.[3] We look within the *same borrower-lender pair* over a short window around the lender's joining the platform and compare the change in maturity to the change for similar borrowers. Second, the decision to join the platform is voluntary and can therefore depend on factors that could affect maturity, independent of the information revealed by the platform. We leverage our micro-data to show that borrower or lender shocks coinciding with the timing of joining cannot explain our results. Specifically, we conduct within borrower-time tests (Khwaja and Mian, 2008) and show that lenders react to the information driven by *others* joining the platform, which is beyond their control.

In our main specification, we measure average maturity within collateral type-quarter for members and control for contract size, credit history, and contract type as well as borrower-lender relationship and collateral type-year fixed effects. We establish two stylized facts. First, we show that the gap between the maturity offered by a lender and what others in the platform are offering shrinks by 7% after the lender joins. Lenders' terms therefore track the bureau average relatively better after joining, consistent with a partial matching of rivals. Economically, this average effect corresponds to a 10% probability of a six-month change in maturity, therefore substantially affecting rollover risk. Interestingly, the effect is symmetric: sometimes lenders match rivals by increasing maturity, sometimes by shortening it.

A second fact is that the effect of learning about competitors is strongly mediated by the distribution of market shares. Specifically, lenders competing in concentrated markets (measured by the HHI) or having larger market shares react much less or not at all to observing competitor information. This pattern is not sensitive to the manner in which we define market shares and concentration or to using relationship switching rates as an alternative proxy for competitive pressure.

Additional results suggest that the conventional effects of information sharing in credit markets cannot fully account for our findings. For instance, information can trigger "run-like" behavior by creditors and financial distress for firms with multiple lenders (Hertzberg et al., 2011). However, we do not find that lenders shorten their maturity systematically

[3] In principle, the channel we document is nevertheless likely to apply to new borrowers as well.

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upon joining or that the effect is smaller for borrowers with a single relationship (for which the incentives to run are muted).[4] In addition, our findings are not driven by the revelation of a borrower's repayment history. This is interesting because borrower payment histories are the most studied aspect of information sharing and strategic behavior in credit markets (Pagano and Jappelli, 1993) and have recently received credible empirical support (Foley et al., 2018; Sutherland, 2018; Liberti et al., 2018). We emphasize instead a novel channel of learning about competitors, which operates incrementally to more conventional channels.

For robustness, we address several important remaining threats to identification. As discussed above, there could be shocks either to the borrower or lender that coincide with the time of joining the platform and drive maturity independently of observing rivals' offers. On the borrower side, our results hold when comparing contracts made to the same borrower by two lenders with different information sets: one that has joined the platform, the other not. Specifically, we include borrower-time fixed effects (Khwaja and Mian, 2008) and find the lender joining PayNet offers a maturity closer to the bureau average, relative to the other lender in the same period.

On the lender side, joining the platform might coincide with a shift in business model correlated with a propensity to offer specific contract terms. To address this concern, we implement two additional tests that exploit the behavior of *other lenders*. First, we show our result holds within lender-year across different market segments. Specifically, the information coverage in the platform depends on contracts made by other lenders and thus varies by collateral type over time in a way that is not directly driven by the decision to join.[5] Including lender-year fixed effects, we show that the maturity of collateral types with higher coverage tracks the bureau average better than collateral types with low coverage. Second, we isolate large shocks to bureau information arising from new lenders joining and show that incumbent lenders' contract terms better track those of their rivals once this information is available to them. These additional tests support the interpretation that lenders adjust their contract terms in reaction to the information revealed on the platform.

Which economic mechanisms might explain these results? Broadly speaking, there are two (non-exclusive) possibilities. The first is rooted in classic oligopoly models of imperfect competition, in which lenders react to competitors to preserve their market share. The convergence in terms we observe suggests that contract terms are strategic complements and

[4] Although all contracts are formally collateralized, there is still significant default risk. For instance, our sample contains contracts to finance copiers and computers, whose value depreciates quickly, as well as other equipment that is movable and therefore difficult to recover in default.

[5] For example, after a truck captive joins there is a large increase in the platform's coverage of truck contracts but no new contracts for copiers. Thus lenders who had joined before this truck captive experience an information shock for reasons beyond their control (they have no say over the truck captive joining) and only to the extent they participate in the truck market.

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not that lenders choose to endogenously differentiate themselves from competitors by offering a different contract (Shaked and Sutton, 1982). The evidence that this effect is mediated by market share is also consistent with this view: dominant lenders have less incentive to match rivals, as their market share is likely less sensitive to competing offers. Within credit markets, Bebchuk and Goldstein (2011) also argue that strategic complementarities have stronger effects in less concentrated markets.

The second kind of explanation is rooted in information aggregation or other social learning models: rivals' offers reveal private information, which in turn can be used by lenders to adjust their own terms. Note that this information comes from contracts offered to *other* borrowers, which implies that it pertains to market-wide factors, as opposed to just borrower idiosyncratic creditworthiness. In general, the relationship between information aggregation and market structure is model-dependent, but our finding that the competitive fringe reacts more is consistent with the Bustamante and Frésard (2017) model of learning from peers. Overall, the findings of this paper support some existing models over others, although it is challenging to fully discriminate among all alternatives, given that market power or beliefs are not directly observable.

Finally, we investigate a key implication of our learning results. While a full welfare analysis is beyond the scope of this paper, we examine the link between learning from competitors and the incidence of delinquencies during the Great Recession. This episode is revealing in that it consists of a large wave of unexpected defaults. For a group of lenders joining the platform before the Great Recession, we compare the recession-period delinquencies for contracts originated just before versus just after joining. Controlling for collateral type-quarter, region-quarter, and lender fixed effects as well as borrower observables, we find that matching competitors meant an increase in delinquencies. An interpretation in line with our main findings is that lenders neglected future risk, either because of greater competition or because they relied more on shared information at the expense of their own information collection. In general, these findings echo those of Murfin and Pratt (2017a) and Goldstein and Yang (2019) that technologies that increase the availability of competitor information can have unintended consequences.

Related Works

This paper relates to a growing empirical literature studying how information and interactions across lenders affect credit market outcomes. Murfin and Pratt (2017a) study comparable pricing in the syndicated loan market. They find that past transactions impact new transaction pricing but a failure to account for the overlap in information across loans leads

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to pricing mistakes. While our data lacks the power to trace out paths of influence as they do, we nevertheless find suggestive evidence of learning about competitors leading to more frequent delinquencies during the Great Recession. Hertzberg et al. (2011) provide clean evidence on the role of public information in credit market coordination. Lenders react strongly to the public revelation of information they already possess about a borrower. This publicity effect triggers "run-like" behavior by creditors and financial distress for firms with multiple lenders. By comparison, we study the effect of observing information about other lenders and find evidence of a channel independent of creditor runs. Kang et al. (2019) study the introduction of loan-level reporting requirements for the ECB repo borrowers that mandate the disclosure of all contract terms, including prices. In a very different environment from ours, they find convergence for price and non-price contract terms within banks.

In credit card markets, Liberman et al. (2018) study the equilibrium effects of information deletion on the allocation of credit and risk, while Foley et al. (2018) show the impact of the information environment on competition. Compared to these works and much of the earlier literature on information sharing in credit markets, we study learning about competitors as opposed to sharing information about borrowers. We also contribute to the literature that studies the drivers of loan terms and specifically maturity. Hertzberg et al. (2018) provide evidence from an online consumer lending platform, showing that loan maturity can be used to screen borrowers based on their private information. In the auto loan market, Argyle et al. (2017) show that borrowers display a demand for maturity and target low monthly repayments, while Argyle et al. (2018) find that loan maturity impacts the pricing of cars.

The literature on information sharing and credit bureaus is vast, including works by Jappelli and Pagano (2006), Doblas-Madrid and Minetti (2013), Sutherland (2018), Liberti et al. (2018), Giannetti et al. (2017), Brown et al. (2009), Kovbasyuk and Spagnolo (2018), and Balakrishnan and Ertan (2017), as is the literature studying the role of information in lending markets more broadly (Hertzberg et al. (2010), Liberti et al. (2016), Hauswald and Marquez (2003), Liberti (2017), Berger et al. (2017) and Ryan and Zhu (2018)). Finally, an extensive literature has studied the role played by public firms and public markets in diffusing information (Sockin and Xiong (2015), Chen et al. (2006), Foucault and Fresard (2014), Dessaint et al. (2018), Kurlat and Veldkamp (2015), Veldkamp (2006), Leary and Roberts (2014), Bustamante and Frésard (2017), and Badertscher et al. (2013)). In contrast, we study private credit markets for which no centralized price exists, making information technology the primary channel of information diffusion.

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1 Equipment Financing and PayNet

1.1 The PayNet Platform

Our data comes from PayNet, an information sharing platform focusing on the U.S. equipment finance market and SMEs. Borrowers in this market seek loans and leases for an array of assets, including agricultural, construction, manufacturing, medical, office, and retail equipment as well as computers, copiers, and trucks. Lenders include banks, manufacturers ("captives"), and independent finance companies.[6] Since PayNet's 2001 launch it has attracted eight of the 10 largest lenders in the market, as well as several hundred others as members. Like other credit bureaus, PayNet operates on the principle of reciprocity: members must share information, and only members can purchase the credit files, credit scores, and default probability products offered. PayNet gathers its data by directly connecting into lenders' IT systems, ensuring that the information shared is comprehensive, reliable, and timely. PayNet has developed these products using 24 million contracts for over $1.6 trillion in transactions collected from members.

Prior to PayNet, lenders generally had access to very limited information about new borrowers and other lenders. Competing data providers, such as Experian, offered limited (and rarely timely) information about trade liabilities, which were much smaller than the typical equipment contract. Public UCC filings documented the existence of a contract but did not detail whether the borrower paid on time or the terms received. Thus PayNet provided equipment finance lenders with a source of timely contract-level information about a borrower's ability to service similar liabilities and details on previous contracts it received. This development was particularly relevant for small borrowers, who typically lacked audited financial statements or public information about their creditworthiness (Berger et al., 2017).[7] Although PayNet does not allow lenders to mine its data (e.g., by accessing all credit files for a given industry or zip code), lenders can observe how their counterparts contract. During the frequent process of accessing individual credit files, they can see the terms other lenders are providing or have provided a given firm in the past. PayNet's data collection and verification process is further detailed by Doblas-Madrid and Minetti (2013) and the online appendix of Sutherland (2018).

Crucially, unlike many consumer credit bureaus, the platform includes detailed information about contracts offered by competitors. Figure 1 illustrates the detailed information available exclusively to PayNet members. The figure displays a snapshot of a (fictitious)

6 Murfin and Pratt (2017b) provide an explanation for the existence of captives in equipment financing.

7 We are not aware of any alternative to PayNet in which small borrowers can voluntary share information with lenders in this market. See Glode et al. (2018) for a theoretical analysis.

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borrower's credit file accessible on the platform in return for a fee. While the first page of the credit file contains a summary of past payments as well as the borrower's state, industry, and age (omitted), subsequent pages reveal the terms of past and current contracts offered by all lenders members of PayNet. In the example of Figure 1, the borrower had two lenders and five contracts in total. For each contract, the maturity, amount, and delinquency status are detailed.

However, similar to other credit bureaus (e.g., the consumer bureaus in the United States), PayNet does not collect or distribute interest rate information and takes care that it is not easily identifiable, for fear of potential collusion. On the one hand, this choice is revealing and supports our hypothesis that information about competitors can have large effects on credit market outcomes. On the other hand, it means that we cannot trace directly the pricing implications of our hypothesis in this setting.

1.2 Sample

We construct our sample from the quarterly credit files of 20,000 borrowers randomly chosen from PayNet's database. The files contain detailed information for each of the borrower's current and past contracts with PayNet members. This information includes the contract's amount, maturity, payment frequency, collateral type, contract type, and delinquency status as well as the borrower's state, industry, and age. The data set provides a constant identifier for borrowers and lenders, which we use to track contracting over time. One limitation is that we cannot match lenders and borrowers to external data with this identifier. Importantly, also note that while we have a large amount of information about lenders' contract choices, we cannot observe the universe of contracts in the bureau. This implies that an estimate of the average of rivals' contract terms, although unbiased, is measured with error. Such measurement error can in general reduce the statistical significance of our results. A final limitation of not observing the universe of lenders and contracts is limited statistical power to study in detail the propagation of the effect across lenders and markets.

Our research question focuses on estimating the effect of observing competitors' contract terms on one's own contract terms. We therefore restrict the sample of contracts used for our main analysis to a relatively short window around the lender joining PayNet. We include contracts originated between the four quarters before to four quarters after the lender joins the bureau. We only study lenders with at least one contract before and one contract after joining the bureau in the given collateral type. This sample selection has little effect on the distribution of loan terms in the population.

Sample statistics: Table 1 describes the lenders and borrowers that meet our regres-

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sion sample requirements described below. We have 2,076 unique borrowers and 44 unique lenders involved in 8,194 credit relationships with 54,290 contracts. Relationships can span multiple contracts, because a borrower's needs for capital grow over time, and old assets depreciate and new ones with updated features are released. The typical borrower maintains two relationships; though because borrowers occasionally switch lenders, we observe more relationships across the full sample period. Lenders on average maintain 94 relationships; this understates their true scope, given we only observe a random snapshot of their clients. Borrowers maintain multiple relationships, in part because lenders can specialize by collateral type. A given firm may, for example, require both computers and forklifts and can access different lenders to finance each. The average lender is exposed to just over six collateral types and the average borrower to 1.7. Table A.1 illustrates the distribution of collateral types in the sample. The five most common collateral types are copiers, trucks, construction and mining equipment, computers, and agricultural equipment.

Oligopolistic competition: As in other credit markets for big-ticket items (cars, real estate, etc.), borrowers in the equipment financing market transact at regular intervals and search for and negotiate with lenders. For this reason, these markets tend not be defined by a single market-clearing price (Argyle et al., 2018). At the same time, relationships are prevalent, and lenders can exercise some degree of market power, with the degree of competition affecting borrowers (Rice and Strahan, 2010). Nevertheless, market power likely varies across market segments, consistent with the evidence in Murfin and Pratt (2017b), Mian and Smith Jr. (1992), or Bodnaruk et al. (2016). Defining market segments as census district (henceforth "region")-collateral type pairs, the median probability that a new contract is issued with a previous lender is 70%, the 25th percentile is 55%, and the 75th percentile is 92%.[8] The median number of lenders in each segment is 12, with an interquartile range of 5 to 31.

1.3 Contract Terms

Table 2 describes the terms for the typical contract in our regression sample. The median contract size is $20,300, with an average of $101,000. The median maturity is 37 months from origination; the average is 44.3 months. Eighty-one percent of contracts are some form of lease (including true leases, conditional sales, and rental leases) while the remaining 19% are loans.[9] The overwhelming majority of contracts require fixed monthly payments. The

[8] Throughout this paper, we use the term "region" to refer to one of the nine census divisions, described at https://www2.census.gov/geo/pdfs/maps-data/maps/reference/us _regdiv.pdf

[9] The borrower's choice between a lease or a loan can depend on many considerations, including cost, tax or financial reporting treatment, different services offered under each contract type, the borrower's credit risk and liquidity, and obsolescence risk. For our purposes, these contracts function similarly. In the context

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level of these contract terms are broadly similar before and after a lender joins the platform, although these levels are affected by changes in lender and borrower composition over time.

Our analyses study contract maturity, for two reasons. First, maturity impacts firms' liquidity and investments. During the Great Recession, maturity on loans lasting over a year fell by 30% between 2007 and 2010 before recovering slowly (Survey of Terms of Business Lending). Figures 3 and 4 show that contracts in our sample also display considerable time variation throughout the business cycle. Kalemli-Ozcan et al. (2018) provide extensive evidence that short maturities and rollover risk were responsible for a large share of the drop in firm investment during the recent recession. Milbradt and Oehmke (2015) also argue that loan maturity has real effects by distorting firms' decisions toward inefficiently short-term investments. Second, maturity is an important variable to allocate risk and credit. In our context of financing a specific piece of equipment, contract size is largely dictated by the equipment needed, but short maturity protects the lender against a deterioration in the borrower's financial position. Indeed, the corporate finance literature has shown that, in the presence of frictions, non-price loan terms are key to credit access, maturity being a prominent example (see Tirole (2010) for a summary). Since maturity and prices are not perfect substitutes, studying contract maturity is relevant, even though interest rates are not reported in our data.[10]

Moreover, maturity choices appear to be far from mechanical and display substantial unexplained variation in the cross-section of borrowers and lenders over our sample period. The raw standard deviation is 17 months, a little less than half of the sample mean. Table A.2 in the appendix shows that only about a third of this variation can be explained by collateral type, year, and borrower-lender fixed effects. In the analysis below, we analyze the dispersion in contract terms by computing, for each contract, the gap between its maturity and the bureau's average maturity (excluding the lender's own contracts) for that collateral type in the previous quarter. The median gap in our sample is 11 months, which is a substantial fraction of the underlying variation in maturity choice.

1.4 Lender Participation in PayNet

When a lender joins PayNet, it gains access to information about others' contracts but must share information about its own contracts, *including past contracts*. This is enforced through PayNet's direct access into lenders' IT systems and extensive audit and testing procedures.

of captive financing, Murfin and Pratt (2017b) highlight the fundamental similarities of leases and loans.

[10] Hertzberg et al. (2018) document that demand for maturity is heterogeneous in consumer credit markets and that maturity can be used screen applicants. We abstract from screening by focusing on repeat borrowers.

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This back-fill requirement is crucial to our empirical design: We can observe contracts made before and after the lender joins. This allows us to study changes in contracting between the same firm and lender during a relatively short window around the lender joining PayNet.

Another key feature of our setting is that lenders join in a staggered pattern over the sample period. This variation offers two benefits. First, the platform information is not publicly revealed: in the same period, some lenders have access to it, while others competing in the same market do not. This within market-period, across-lender variation allows us to distinguish the effects of the new information from other events affecting lenders or borrowers in a given year. Second, the information revealed to entrants by the platform varies over time as a function of what *other lenders* are offering. Indeed, lenders often specialize by collateral type; therefore the bureau coverage across collateral types evolves in a nonsystematic pattern. Thus members regularly experience shocks to the information coverage in their markets driven by other lenders, which is by construction outside of their control.[11] We leverage these additional sources of variation in our main specification and robustness tests.

Table 3 shows the variation in entry timing for lenders meeting our sample criteria described in Section 3. Lenders join in all years between 2002 and 2014 except one. While large lenders tend to join earlier than small ones, in most years, a variety of lenders join. At the same time, joining PayNet is voluntary, and the timing of joining the platform is not randomly assigned. In section 2.6 below, we perform a series of tests to ensure that results are not driven by lender or borrower shocks coinciding with the timing of joining. Note also that Liberti et al. (2018) study in detail the decision to join PayNet and show that a key driver of lenders' joining is access to new markets, but our tests are performed exclusively within an existing relationship. Note also that our sample of lenders differs by design that of Liberti et al. (2018) in that, given our purpose, we impose a different event window and sample requirements, as described in Section 2.4.

2 Learning About Competitors

2.1 An Illustrative Framework

How do lenders react to observing their competitors' contract terms? Credibly answering this question presents some empirical challenges. To help explain our empirical approach, we sketch a simple illustrative framework in which lenders have dispersed information about their borrowers as well as their competitors. We use the model to describe the effect of

[11] Figure A.1 in the online appendix shows there is considerable time variation in the volume of contracts in the bureau across collateral types.

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joining the platform on contract maturity, as well as how we empirically account for some important confounders. However, because our data is silent on these issues, we do not explicitly provide microfoundations for the market game nor for the joint optimization of maturity and pricing. The main text is limited to notation and key ideas, while the appendix contains more details.[12]

A lender's optimal contract terms depend on two factors: (i) fundamentals, such as borrower credit risk and the lender's risk tolerance, and (ii) the lender's competitors' terms, due to imperfect competition. Lenders have access to some public information as well as private signals about both factors. We can decompose lender l's choice of maturity m to firm f, which is part of a group of similar firms g, linearly as follows:

$$m_l^f = \underbrace{m_0^g}_{\text{public information}} + \underbrace{\mathbb{E}[\phi^g|I_l]}_{\text{borrower fundamentals}} + \underbrace{\alpha\mathbb{E}[m_{-l}^g|I_l]}_{\text{competitors' terms}} + \underbrace{\eta_{lf}}_{\text{idiosyncratic to relationship}}$$

Fundamentals are denoted by ϕ^g and competitors' terms by m_{-l}^g. The degree to which lenders care directly about their competitors' terms is denoted by α and summarizes the nature and degree of competition faced by the lender for this borrower. Given that borrowers plausibly value longer maturity, the probability that a contract offer is accepted increases in the lender's own term m_l and decreases in rivals' terms m_{-l}. This would naturally lead to strategic complementarities: the optimal maturity choice m_l^* increases with the lender's belief of its rivals' offers ($\alpha > 0$). On the other hand, the industrial organization literature has also raised the possibility that rivals choose to differentiate themselves through product choice as in Shaked and Sutton (1982) ($\alpha < 0$). Our empirical test does not take a stand on the direction of the response.

Finally, the idiosyncratic term η_{lf} includes borrower characteristics, news about its creditworthiness, or shocks to the lender's balance sheet that affect its propensity to lend.

Crucially, lenders are uncertain about both fundamentals and their competitors' actions. Before joining PayNet, lenders have two sources of information: (1) public information about fundamentals or competitors' terms that can be gleaned from, for instance, forecasts of local and national economic conditions or industry reports, summarized in $m_0 = (m_0^\phi, m_0^m)$, and (2) private signals $s_l = (s_l^\phi, s_l^m)$, reflecting the lender's own effort to determine the appropriate contract maturity.

After joining the platform, lenders can also observe an additional signal: the average terms offered by competitors ($\bar{m}^g$) to similar borrowers.[13] In equilibrium, the maturity choice

[12] The mathematical notation borrows from canonical "beauty contest" models exemplified by Morris and Shin (2002). Note, however, that we use it for a different purpose and the underlying economics and microfoundations differ.

[13] Concretely, lenders can learn about others' terms by purchasing individual credit files from PayNet. This

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depends on the information available to the lender at the time. Before joining, lenders put some weight on their own private signals, depending on their respective precision. After joining, lenders place less weight on their own private signals and place some weight on the bureau average.

This paper's investigation is empirical. Questions related to information and imperfect competition are notoriously difficult to study, and our focus is on providing novel, credible evidence on how lenders react to learning about competitors. Predictions from theoretical models are complex and often ambiguous, as emphasized by Vives (2006). Our contribution is to estimate the sign and magnitude of this reaction in our setting as well as testing whether it is driven by important features of the environment, specifically market structure. Nevertheless, it is useful to discuss potential channels at play, although fully distinguishing among them is beyond the scope of this paper.

Lenders can react to information $\bar{m}^g$ about their competitors through two broad, non-exclusive channels, as can be seen from the decomposition above. First, there can be a direct strategic effect: a change in contract terms helps preserve or grow market shares (*oligopoly channel*). This effect comports with modern industrial organization models focusing on imperfect competition with incomplete information. Intuitively, the market for financing equipment is not centralized, and not all lenders offer the same contract terms in equilibrium. Instead, buyers search for good deals, and lenders' choice of terms is driven by attracting or retaining borrowers. The profit-maximizing contract terms balance a higher probability that a contract is accepted with a lower profit margin on that contract. Learning about competitors is valuable because it helps solve this trade-off optimally.[14] The sign of the effect, however, is ambiguous: it depends whether strategic complementarities or the differentiation motives dominate.

Second, there can be an indirect inference effect: competitors' actions partly reveal their private signals, which are informative about fundamentals, such as credit risk or borrower demand in the economy (*information aggregation channel*). Note the differences from the previous channel. The oligopoly channel emphasizes that lenders care about others' actions per se, while here lenders care because of what they represent: maturities partially reveal competitors' private information that was used to make this choice. As opposed to learning about a specific borrower from its payment history, information aggregation postulates that

makes it unlikely they can learn the entire distribution of competitors' terms or that they can leak this information easily.

[14] In the language of classical IO models, the optimal price set by a producer equates inverse residual demand elasticity with the profit margin. Learning about competitors leads to a better estimate of this elasticity. The same logic applies to maturity choice, in a joint model of lending in which interest rates and maturity are not perfect substitutes.

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lenders use the bureau information to extrapolate to other similar borrowers (e.g., with respect to size, sector, or collateral type). The rational expectations version of this effect is canonical in the context of financial markets (Hellwig, 1980) but has been much less explored within credit markets. Importantly, at this stage, we want to include under this broad channel other social learning models that are less "rational" or "efficient" in nature, such as information cascades or naive herding (Murfin and Pratt, 2017a) as well as rational models with endogenous information acquisition. We include these different forces under the "information aggregation" label. Section 3 will re-examine all these channels in the light of our main empirical findings and discuss the potential efficiency implications.

2.2 Empirical Approach

The main identification threat in isolating the effect of learning about competitors is the existence of unobserved common shocks. Maturity choices are naturally correlated across agents, due to public information m_0 as well as private signals $\{s_l\}$, independent of the information revealed by the bureau. Then lenders might start offering certain terms at the same time, not because they respond to each other but simply because they react to the same news about fundamentals. The main contribution of our empirical strategy is to specifically account for these unobserved common components.

To address this challenge, we exploit the time dimension associated with the lender joining PayNet. Joining leads to a shift in the lender's information set. Importantly, lenders join in a staggered fashion: an individual lender gaining access to PayNet does not coincide with a release of public information to all lenders. Our main specification measures how maturity changes within a relationship over a short window around the lender joining. While a lender's terms may track the bureau average before joining, we ask whether they track it relatively *better* or *worse* afterward.

Figure 2 provides a graphical illustration of this idea, focusing on the case of convergence for simplicity. Because of common shocks, the lender's terms are correlated with competitors' even before joining the platform. However, they track the bureau average relatively better after joining. This would be consistent with lenders mimicking competitors. A divergence in terms would generate the opposite pattern, with lenders' terms generally tracking the bureau worse after joining. In the data, we can follow lender-borrower relationships over time, including the time before the lender joined the platform. We can also observe rivals' offers before as well after the lender joins. This allows us to test this prediction directly within a fixed effect regression framework.

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2.3 Addressing Confounders

By construction, our empirical strategy is not confounded by the existence of a number of factors: public information unobservable to econometrician m_0, other sources of information outside of the platform s_l, or idiosyncratic loan terms η_{lf}. Indeed, all of these forces exist in the framework above, and our tests based on comparing before and after joining are valid, independent of the sequence of realization of any of these shocks. This is the main advantage of our approach.

However, a necessary assumption for identification is that the precision or dispersion of these other shocks is constant, within fixed effects groups, around the time lenders join. While this assumption is considerably weaker than assuming that no such shocks exist, it cannot be taken for granted. Specifically, the identification strategy creates the possibility of two important confounders. Specifically, lenders' responses might be driven by (i) information in the platform other than rivals' offers, namely the revelation of the borrower's repayment history, or (ii) by shocks unrelated to the platform information but whose timing coincides with the decision to join. We take both concerns seriously and design our main specification as well as additional tests to address them as best we can.

Revelation of borrower past repayment history: Contract terms offered to a borrower can be influenced by what a lender learns from the borrower's PayNet *credit file*. Note, however, that we restrict attention to lending to previous borrowers, for which the credit file is not necessarily informative. For instance, we show in additional tests that our main result holds for borrowers with a single relationship, for which the credit file carries no additional information.

Other shocks correlated with joining PayNet: The decision to join the platform is voluntary and can therefore depend on a number of factors that could affect maturity, independent of the information revealed by the bureau. On this front, note that Liberti et al. (2018) show that lenders joining PayNet are motivated by a desire to enter new markets. However, our main test is exclusively within existing markets. In addition, in Section 2.6, we show that borrower or lender shocks coinciding with the timing of joining cannot explain our results by conducting a series of additional within borrower-time tests (Khwaja and Mian, 2008) as well as tests leveraging the decision of other lenders to join PayNet.

2.4 Main Specification and Findings

We design our main specification to answer the following question: does the contract maturity for the same borrower match the lender's rivals' maturities better after the lender joins the bureau? For each contract, the dependent variable is a measure of the "gap" $|m_i^* - \bar{m}|$ between

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the maturity offered by the lender and what rivals are offering for similar transactions. The variable of interest is a "Post Joining" dummy, equal to 0 for contracts issued before joining PayNet and 1 for those issued after. A negative coefficient on $\delta_{post} < 0$ implies that lenders react to the bureau information by offering terms more similar to those of competitors. Importantly, we can account for heterogeneous deviations from average maturity by including a series of granular fixed effects.

Specifically, the main specification estimates the following fixed effect regression:

$$\log |m_{lfc,t} - \overline{m_{c,t-1}}| = \delta_{post} + \eta_{lf} + \alpha_t + \nu_{contract} + \varepsilon_{lfc,t} \tag{1}$$

The unit of observation is a contract signed between firm f and lender l to finance a piece of equipment. The sample includes contracts originated between the four quarters before to four quarters after the lender joins the bureau. Because our predictions concern the intensive margin, we only study lenders with at least one contract before and one contract after joining the bureau in the given collateral type.

The dependent variable is the log of absolute value of the gap between the contract maturity at origination and the bureau average maturity for that collateral type in the previous quarter $\overline{m_{c,t-1}}$, excluding the lender's own contracts.[15] We show robustness to using different measures of rivals' offers below. Importantly, recall that our data set is constructed from a random sample of 20,000 borrowers' quarterly credit files. We therefore cannot observe the universe of contracts in the bureau, and this power concern restricts how finely we can measure rivals' offers.

The parameter of interest is the coefficient δ_{post}. To control for heterogeneous deviations from average maturity, we add a series of fixed effects. η_{lf} is a borrower-lender fixed effect that accounts for idiosyncratic time-invariant maturity at the relationship level, including industry and regional variation. Given that lenders join at different times, we include time fixed effects α_t to allow for aggregate time series patterns in maturity. Finally, we include controls $\nu_{contract}$ for each of the three contract size categories, whether the contract is classified as a lease or a loan, and each borrower risk category based on prior delinquencies.[16]

To lend support to the empirical strategy, Table A.3 in the appendix reports pre-trends for contract terms before joining the platform. For the entire distribution of loan size and

[15] Excluding the lender's own contract and using a lag addresses the mechanical aspects of the reflection problem of Manski (1993). Exploiting the timing of joining PayNet accounts for the existence of common shocks, as explained in detail above.

[16] Specifically, the three contract size categories are: small ticket (below $250k), medium ticket (between $250k and $5M), and big ticket (above $5M). The three delinquencies categories are: no missed payments, missed payments 90 or fewer days late, and default or missed payments over 90 days late, all measured over the last three years.

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maturity, there is virtually no difference a quarter before joining, relative to a year prior to joining. The distribution of the gap relative to the bureau average also does not display any particular trend. The lack of pre-trends also assuages concerns about survivorship bias. Because our Post Joining variable captures the passage of time, it is identified only for borrowers with a contract before and after their lender joins PayNet. While in principle the passage of time (i.e., survival) can be correlated with borrower characteristics, we do not see this pattern prior to joining. In fact, the dynamic coefficient plots in Figure 5 below show that the change in maturity happens on impact, the quarter after the lender joins PayNet.

Table 4 presents the main result of estimating Equation 1. It shows that, upon joining PayNet, the gap between a lender's maturity and the bureau average falls by 6% to 7% in absolute value. This effect reveals that observing new information about competitors leads lenders to offer maturities closer to what others are offering. The effect is virtually unchanged whether we use quarter, year, or collateral-year fixed effects to account for aggregate time variation. Table 5 shows that the effect appears to be symmetric in that maturity itself does not change on average, only the gap relative to rivals changes. Column 2 confirms that lenders adjust terms in both directions.

Economically, this effect on borrowers is likely to be sizeable. While the the average effect implies only a one-month change in contract maturity, it masks a large effect on rollover risk for some borrowers. Indeed, it corresponds to a 10% probability of a six-months or larger change in contract maturity, as suggested by unreported regressions that use six-months bins of contract maturity.[17] Moreover, for the subset of borrowers with fully amortizing contracts, a back of the envelope calculation suggests a 2% change in monthly payments, equivalent to a 2 percentage point change in APR.[18]

Table 6 shows that our finding is robust to a number of alternative specifications, both in terms of economic magnitude and statistical significance. To account for heterogeneous shocks to collateral types across regions, column 1 calculates the bureau average by collateral type-region-quarter, instead of collateral type-quarter, and yields a similar estimate. Column 2 shows that our results are unchanged if we drop contracts originated during the crisis years of 2008–2009. Column 3 shows that our results are not driven by small collateral types with fewer than 100 observations, for which the bureau average is likely measured with a significant amount of error.

[17] Contract maturities are often multiples of six months.

[18] For example, the median contract is for $20,000 and 37 months, which corresponds to a $678 payment per month. Reducing maturity to 36 months increases monthly payments to $693, roughly comparable to increasing the interest rate from 15% to 17% ($698). Since we cannot directly observe interest rates nor any embedded options in our data, this calculation relies on Schalheim and Zhang (2017)'s estimate of the mean annualized interest rate of 15% during this period.

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We then perform two placebo tests. First, in column 4, we calculate the bureau average using contracts from one year ago instead of current contracts. We expect lenders to react less to stale information. Second, in column 5, we calculate the bureau average using an unrelated collateral type, based on the relatedness measure introduced in Liberti et al. (2018). For both placebo tests, we find null results. Moreover, unreported results show that the effect is not tied to whether the lender joins early or late in our sample period.

We make three comments regarding the implications of this main finding. First, recall that our data does not allow us to trace the pricing implications of this effect, as interest rates are not shared in the platform. If prices were shared, lenders might react to this information as well. Generally, since interest rates and maturity are not perfect substitutes, a change in maturity can have real effects. For instance, Argyle et al. (2018) provide evidence that maturity impacts the pricing of cars in auto loan markets. Undoing a maturity change by increasing rates could also turn borrowers away or increase their debt burden and credit risk. Second, note that for econometric reasons our tests is restricted to existing borrower-lender relationships. However, in principle this effect would apply to new borrowers as well. For example, better information about competitors' offers is particularly valuable when trying to poach borrowers. Nevertheless, cleanly isolating this effect for new borrowers is particularly challenging. Finally, it is plausible that the change in lender behavior upon joining PayNet in turn affects other lenders, implying that there can be knock-on effects that propagate and amplify the initial effect, either through competition or learning. For instance, Murfin and Pratt (2017a) document pricing mistakes by tracing out "paths of influence" across syndicated loans and show how these mistakes are propagated across time in this market. However, the fact that we do not observe the universe of lenders and contracts limits our ability to study propagation in detail in our setting.

2.5 The Role of Market Structure

A natural question to ask is whether the effect of learning about competitors is mediated by market structure. For instance, the oligopoly channel suggests that market power is a key driver of incentives to match rivals. Indeed, lenders in a dominant position and whose market share is less sensitive to competitors face little pressure to respond to what others are offering.

To investigate this, we construct different measures capturing the distribution of market shares.[19] Across markets, we first measure concentration according to HHI. We define a "market" either at the collateral type-contract size category level or at the collateral type-

[19] Direct measures of market power are hard to obtain outside a fully structural model. Therefore we interpret our results with caution.

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contract size category-region level, because lenders might compete locally or nationally. To alleviate concerns that local market concentration is directly affected by information sharing, we compute market concentration at the beginning of 2001, before PayNet was introduced. There is considerable variation in concentration across market segments: moving from the 25th to the 75th percentile of the distribution implies a 0.15-0.20 increase in the HHI. We also use relationship switching rates as an alternative measure of market competitiveness. Some market segments see more relationship switching than others, presumably because of their unique degree of product differentiation, specialization, or other switching costs. Finally, we also construct a within-market across-lender measure that flags lenders that are among the five largest in a collateral type-region-quarter. This classification allows us to distinguish between dominant lenders and the competitive fringe. Table A.4 in the appendix shows summary statistics for these measures.

Table 7 shows that our learning results are strongly mediated by market structure. All measures of market structure point in the same direction. The first two columns show that the effect is driven by markets with low levels of concentration. In these less concentrated markets, the gap between the lender's maturity and the bureau average falls by 8% after joining, while it is statistically unchanged in markets with high concentration levels. Column 3 confirms these findings by showing that the effect is driven by markets with high relationship switching rates. Finally, Column 4 suggests the same interpretation: lenders in the competitive fringe are more responsive to information about their competitors, although the distinction is statistically weaker in this specification. While the point estimate is two times larger for the competitive fringe than for the top five lenders, the p-value of the coefficient difference is 0.32.

Figure 5 illustrates the full dynamics of the effect across subsamples with high and low market concentrations, respectively. Each panel plots the coefficients of a version of Equation 1 in which each quarter before and after joining has its own dummy variable. The omitted category is the quarter prior to joining and is labeled as time zero. For this graph, we extend the window to two years around joining PayNet. The left panel shows that, in the most-concentrated markets, the gap between a lender's terms and the bureau average is unaffected by joining. The right panel paints a different picture for the least-concentrated markets. After joining, there is a significant and persistent fall in the gap, implying that lenders adjust their terms toward what others are offering. The gradual reduction in the gap is intuitive: because lenders cannot mine the database, it takes time to aggregate and use the information about rivals contained in individual credit files. It is also possible that the effect grows over time, as lenders learn from each other sequentially. Statistically, there are no significant pre-trends, although the data is somewhat noisy four quarters prior to joining.

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2.6 Other Shocks Coinciding with the Lender Joining PayNet

Joining PayNet is voluntary and not randomly assigned. Therefore we cannot exclude the possibility that our results are due to factors other than the bureau information that drives both the decision to join and maturity choices. Relatedly, note that access to new markets is the key driver of lenders' joining PayNet (Liberti et al., 2018). However, our main test is exclusively within existing markets: it includes lender-borrower fixed effects and is restricted to lenders with contracts in a given collateral type before and after joining. Note also that Table A.3 and Figure 5 reveal no discernible pre-trends in our dependent variable prior to joining. Nevertheless, we leverage the granularity of our data and conduct a number of robustness tests to directly address this threat to identification.

Accounting for Borrower Shocks: On the borrower side, we exploit the fact that, in a given period, some lenders to the same borrower have access to the platform, while others do not. We can use this across-lender variation to distinguish the effects of the new information from other events affecting a given borrower in a given year. Specifically, we include borrower-year fixed effects for the subset of borrowers with multiple lenders:

$$\log|m_{lfc,t} - \overline{m_{c,t-1}}| = \delta_{post} + \eta_{lf} + \zeta_{ft} + \nu_{contract} + \varepsilon_{lfc,t} \tag{2}$$

Table 8 shows the results of this extended specification. As before, the gap between a lender's maturity and the bureau average falls after joining in competitive market segments but is unchanged in others. The coefficient reflects the reduction in the gap after joining, relative to other lenders of the firm in the post period. This more stringent specification alleviates the concern that results are driven by shocks to borrower demand or creditworthiness that coincide with the lender's decision to join PayNet.

Accounting for Lender Shocks: On the lender side, joining PayNet might coincide with a business model shift, which is potentially correlated with the propensity to offer specific contract maturities. To address this concern, we design two additional tests that exploit the behavior of *other lenders*. Specifically, the information coverage in the bureau depends on contracts originated by others and thus varies by collateral type over time in a way that is not directly driven by one's own decision to join. For example, after lenders join, they have no control over how the bureau's membership or collateral market coverage evolves. Any given year could see non-systematic changes in bureau coverage across collateral types based on who else joins, and these coverage changes affect the precision of the bureau average.

In the first test, we leverage this variation driven by other lenders to check whether our result holds within lender-year across different collateral types. We can ask whether

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the maturity of collateral types with higher coverage tracks the bureau average better than collateral types with low coverage. Concretely, we augment Equation 1 by adding two elements:

$$\log |m_{lfc,t} - \overline{m_{c,t-1}}| = \delta_{post} * Volume_{c,t-1} + \eta_{lf} + \xi_{lt} + \nu_{contract} + \varepsilon_{lfc,t} \tag{3}$$

First, the main coefficient of interest is now the Post×Volume interaction, where Volume is defined as the number of open contracts in the bureau of the same collateral type as of the previous quarter.[20] Second, we include a lender-year fixed effect ξ_{lt} that absorbs any change in lenders' supply that is constant across collateral types within a year. Panel A of Table 9 shows the results for this extended specification. The estimated coefficients are consistent with our main finding. For a given lender joining in a specific quarter, the maturity of collateral types with higher coverage tracks the bureau average better than collateral types with low coverage and only so in the most-competitive market segments. Columns 3 and 4 also include borrower-year fixed effects for robustness and arrive at the same results.

In the second test, we ask whether lenders react to large information shocks due to others joining PayNet. We implement this test in three steps. First, for each lender, we identify its primary collateral type—the one that lender most frequently finances. Second, for each lender, we identify an event quarter *after the lender joined* when the bureau experiences the largest increase in contract coverage for the primary collateral type. Although some lenders will share primary collateral types, their staggered joining results in different event quarters. Third, for each lender, we estimate a variant of Equation 1 around the event quarter, where the Post dummy is now defined relative to each lender's event quarter. Panel B of Table 9 shows the results for this alternative specification. Consistent with our interpretation that lenders react to information about competitors contained in the platform, contract maturities are closer to rivals' average following a large information inflow after the lender has joined PayNet. Overall, these additional results alleviate the concern that our main findings are purely driven by factors behind the decision to join.

3 Interpreting the Findings and Implications

3.1 Conventional Channels of Information Sharing

The previous section provides robust evidence that lenders react to learning about their competitors. In this section, we put this result into perspective with more conventional

[20] We omit the level effect of Volume in the regression equation for brevity.

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channels of information sharing in credit markets. We do not claim that these channels are not at play in general; in fact, previous work using PayNet data suggests some of them are operating in our setting (Doblas-Madrid and Minetti (2013), Sutherland (2018), Liberti et al. (2018)). We argue only that our findings cannot be fully explained by these conventional channels.

Revelation of Credit History: A key role of credit bureaus is to create credit files that reduce information asymmetries between lenders and borrowers. The revelation of borrowers' payment histories affects the amount of credit and contract terms. Part of this channel works through a change in the composition of borrowers: worse borrowers are screened out or offered harsher terms, while better borrowers receive better offers (Foley et al., 2018). However, by design, our tests keep the composition of borrower-lender pairs constant by including relationship fixed effects. The effect we document is therefore a change in maturity within a relationship. The revelation of credit histories can affect an existing relationship if a borrower has multiple lenders. Accessing the bureau can reveal negative information to the lender that the borrower tried to keep secret previously.

If this channel were driving our result, we expect that it would be smaller or absent for borrowers with (1) a good credit history and (2) a single relationship, because for them the credit file would contain no new information.[21] However, Table 10 reveals that there is no significant difference in the effect for borrowers with bad credit records or single relationship borrowers.

Creditor Runs: Alternatively, lenders can react to observing others' terms due to the fear of a creditor run.[22] For instance, Hertzberg et al. (2011) illustrates the effect of information sharing on lender coordination. In the context of maturity choice, Brunnermeier and Oehmke (2013) emphasize the risk of a "maturity rat race," in which new lenders offer short maturities in an effort to front-run existing creditors. In general, these incentives to run lead to strategic complementarities in maturity choice that could explain a convergence in maturities after joining the bureau. Although all contracts are formally associated with a specific piece of equipment, there is still significant default risk. Nevertheless, several pieces of evidence speak against an explanation based on run-like behavior of creditors. First, recall from Table 5 that lenders do not shorten their maturities systematically upon joining: lenders adjust their terms toward what others are offering, in both directions. Second, the aforementioned findings in Table 10 contradict a run interpretation; the effect is equally

[21] It may be news that the borrower does not have a relationship with any other lender. Nevertheless, we would expect this piece of news to be substantially less informative than a full credit history.

[22] More broadly, a number of papers have emphasized the role of information in explaining run-like behavior, such as Morris and Shin (1998), Bebchuk and Goldstein (2011), Goldstein et al. (2011), Goldstein and Pauzner (2005).

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strong for borrowers with good credit records or with a single relationship for which the incentives to run are muted.

3.2 Learning about Competitors: Revisiting the Channels

The illustrative framework presented in Section 2.1 suggests two potential broad, non-exclusive channels of learning about competitors: the oligopoly channel and the information aggregation channel. We revisit them in light of the our evidence. Overall, the evidence above support some existing channels over others. Nevertheless, it is challenging to fully discriminate among all alternative models, given that market power or beliefs are not directly observable.

Oligopolistic Competition: under this channel, lenders respond to competitors' offers to preserve or grow their market share. Interestingly, industrial organization models can disagree on the sign of the effect. Indeed, lenders might try to preserve the demand for their product by matching rivals' terms or by trying to differentiate themselves (Shaked and Sutton, 1982). Our evidence of lenders adjusting toward what others are offering is consistent with the first view, i.e., that maturity choices are strategic complements in our setting. In general, this fact can have important implications, because it is well known that strategic complementarities help propagate shocks throughout the economy (Angeletos and Lian, 2016). Strategic complementarities are also crucial to determining the total effect of lifting barriers to entry, as they dictate the strength of the response of incumbents to entrants contesting the market.

Moreover, our findings that the effect is mediated by market structure are in line with this view. Lenders in a dominant position and whose market share is less sensitive to competitors face little pressure to respond to what others are offering. Conceptually, lenders' "market power" should predict the strength of the effect.

Ideally, we would also use data on applications to measure directly how the take-up rate of a lender's offer depends on rivals' maturity, as in Argyle et al. (2018). Unfortunately, PayNet does not collect data on applications, and we can only measure actual market shares. Actual market shares are not enough to test the channel directly, because they are determined in equilibrium. For example, a producer might reduce its price to try to attract demand, but its rivals have incentives to adjust their own price as well. Market shares might therefore not change significantly, although market participants are reacting strongly to each other.

Information Aggregation: There can also be an inference effect: competitors' actions partly reveal their private signals, which are informative about fundamentals such as credit risk or borrower demand in the economy. As opposed to learning about a specific borrower's

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credit file, this channel postulates that lenders look at the bureau information to extrapolate to similar borrowers. The rational expectations version of this effect has been studied extensively, but at this stage, other social learning models, such as information cascades or naive herding (Murfin and Pratt, 2017a), are equally plausible. The unifying theme is that learning about competitors reduces the lender's reliance on its own private information.

We cannot directly measure lenders' beliefs, so instead we ask how this class of models squares with our cross-sectional evidence. Conceptually, the information aggregation channel does not suggest a clear-cut prediction with respect to the role of market structure. The main reason is that the canonical models tend to be cast in terms of a competitive financial market or through a sequence of decision-making problems. Recent work has incorporated elements of strategic behavior, and a consensus has yet to emerge (Vives, 2011; Bernhardt and Taub, 2015; Rostek and Weretka, 2015). Our finding that the competitive fringe reacts more strongly is, however, consistent with the model of learning from peers in Bustamante and Frésard (2017).

To provide additional evidence, we compare the behavior of specialist lenders relative to others joining the platform. Although this is an imperfect proxy for differences in information, the idea is that specialist lenders may have more-precise private information and thus put less weight on others' terms when deciding what to offer.[23] We include five definitions of lender specialization, with the intent of capturing lenders that have expertise in a market segment. The first two define specialization as the number of quarters since the lender's first contract originated in this collateral type or collateral type-region category. The next two define a lender as a specialist for a specific collateral type if that collateral type is either the most common or one of the top three originated by that lender. Finally, we define a lender as a specialist for a collateral type if that collateral type constitutes at least 30% of its lending portfolio. If information aggregation explains our main results, then specialists should adjust their terms relatively less upon observing others' terms. However, the specialist interaction is typically small, of the wrong sign, and insignificant, as displayed in Table A.5 in the appendix. While this is only a cross-sectional prediction, we do not find direct evidence in favor of this information channel.

3.3 Implications

Our learning results go beyond conventional mechanisms of the effects of information sharing. Interestingly, across many markets the rise in big data and algorithm developments is making learning about competitors increasingly easier. The debate on the effect of information

[23] Stroebel (2016), Kurlat and Stroebel (2015), and Loutskina and Strahan (2011) also exploit heterogeneity in expertise in the context of real estate markets.

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sharing on market behavior has therefore resurfaced recently. Our findings speak, in a novel way, to the interaction between information and market competition that has been emphasized in the literature (Vives, 2006; Jappelli and Pagano, 2000).

The economic forces at play are subtle. On the one hand, information from competitors could facilitate collusion. On the other, pooling information can be beneficial: it can improve production efficiency or remove barriers to competition. Similarly, having access to more information can backfire if "mistakes" are propagated as opposed to corrected when information is shared. For instance, Murfin and Pratt (2017a) document in detail how the use of comparables leads to pricing mistakes in the syndicated loan market. Goldstein and Yang (2019) argues that in general the market-quality implications of information disclosure are subtle and can crowd out the production of private information.

To relate these questions to our setting, we examine one consequence of learning about competitors by studying delinquencies during the Great Recession. This is an interesting episode, as it led to wave of defaults that was difficult to predict. Broadly speaking, there are two potential, not mutually exclusive, channels that could increase delinquencies. First, enhanced competition can lead lenders to neglect risk as they compete aggressively to preserve their market share today. Second, reliance on hard information, such as credit reports and scores, exposes lenders to significant losses caused by negative shocks that are not anticipated by the hard information. Rajan et al. (2015) document this phenomenon in the market for securitized subprime mortgages during this period.[24]

We exploit the staggered timing of lenders' joining and study how contracts originated prior to the crisis performed during it. Specifically, for each lender joining between 2005 and 2007, we study the 2008-2009 performance of contracts originated shortly before joining, compared to contracts originated shortly after joining. Our assumption, based on our prior tests, is that lenders do more firm-specific screening before joining and rely more on shared information after and react to what rivals are offering. In addition to lender fixed effects, our tests include indicators for the quarter of origination for each collateral type and the quarter of origination for each borrower region. These last controls ensure that our results are not driven by lending to different cohorts with differential (and potentially region-specific) default risk.

Table 11 shows that contracts originated just after the lender joined experienced more crisis-period delinquencies than the contracts originated by the same lender just before. Specifically, the post-join contracts experienced approximately 0.3 more quarters of delinquency from 2008 to 2009 than the pre-join contracts. One interpretation is that a desire to

[24] More generally, this is related to the Lucas critique (Lucas, 1983). See also Farboodi et al. (2018) for a recent discussion of how the use of information by the stock market can deviate from the social optimal.

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match competitors can backfire if lenders overlook fundamental sources of risk.

Admittedly, this is not the only possible explanation, and although our data cannot reject alternatives with absolute confidence, we offer additional pieces of supporting evidence. First, in line with our prior results, we also find that the delinquency increase is entirely driven by markets with low levels of market concentration, as shown in columns 2 and 3. Second, we identify states with the largest drop in housing prices during the recession, where a substitution from screening to mimicking should result in worse contract outcomes.[25] Even after controlling for region x origination quarter fixed effects, columns 4 and 5 shows more delinquencies for post-joining contracts only in large housing price drop states. Additional results support this interpretation (not tabulated for brevity): we find a reduction in the average gap between the lender's contract maturity and rivals' maturity after joining PayNet, but this decline is most pronounced for contracts ending up delinquent. And lenders do not seem to target riskier borrowers after joining. On the contrary, if anything, borrowers' credit records improve, consistent with the canonical information effect of credit bureaus (Doblas-Madrid and Minetti, 2013). Accordingly, we find that the effect is large for existing borrowers.

Because the set of lenders joining PayNet a few years before the Great Recession instead of in other periods is small and potentially selected, we take this evidence as suggestive. Nevertheless, it supports the idea that incentives to match competitors behind contract design can have a cost if they lead to the neglect of fundamental risk.

4 Conclusion

This paper estimates the effect of learning about competitors on the behavior of market participants. This contrasts with the conventional channel of learning about borrowers that has been emphasized in credit markets. We document contract-level evidence of this effect in the context of maturity dynamics for SME equipment financing contracts, using micro-data from the introduction of an information sharing platform. The platform provides details of previous and current contracts and not simply current payment status or debt balances. We exploit the staggered timing of lenders joining the platform to estimate the effects of learning about competitors. We find that, upon joining, lenders adjust their terms toward what others are offering. Crucially, we address two key confounders: unobserved common shocks to fundamentals and the endogenous timing of joining the bureau. We also show that the effect is strongly mediated by market structure and that mimicking competitors

[25] Housing crisis states are defined as those with a greater than 30% housing price decline from peak, according to the FHFA index (14 states).

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can backfire if it leads lenders to neglect fundamental risk.

These results illuminate the interaction between information and market competition in credit markets. Learning about competitors is likely becoming easier, given the rise of large pooled databases and improvements in data mining, in credit markets and beyond. While we find a greater convergence between rivals in our setting, the sign, magnitudes, and channel can vary in other markets. Indeed, a number of markets are characterized by a large degree of horizontal differentiation or populations of unsophisticated consumers, facing endogenously complex products. The implications for consumer welfare, production efficiency, and policy design are important open questions and further empirical research on these topics is warranted.

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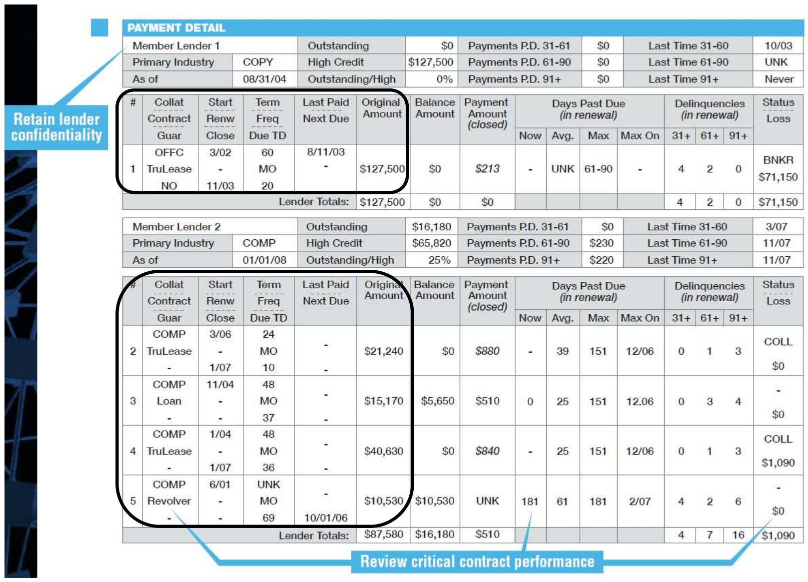
Figures and Tables



PAYMENT DETAIL

Member Lender 1		Outstanding	$0	Payments P.D. 31-61	$0	Last Time 31-60	10/03
Primary Industry	COPY	High Credit	$127,500	Payments P.D. 61-90	$0	Last Time 61-90	UNK
As of	08/31/04	Outstanding/High	0%	Payments P.D. 91+	$0	Last Time 91+	Never

#	Collat / Contract / Guar	Start / Renw / Close	Term / Freq / Due TD	Last Paid / Next Due	Original Amount	Balance Amount	Payment Amount (closed)	Days Past Due (in renewal) Now	Avg.	Max	Max On	Delinquencies (in renewal) 31+	61+	91+	Status / Loss
1	OFFC / TruLease / NO	3/02 / - / 11/03	60 / MO / 20	8/11/03 / -	$127,500	$0	*$213*	-	UNK	61-90	-	4	2	0	BNKR / $71,150
				Lender Totals:	$127,500	$0	$0					4	2	0	$71,150

Member Lender 2		Outstanding	$16,180	Payments P.D. 31-61	$0	Last Time 31-60	3/07
Primary Industry	COMP	High Credit	$65,820	Payments P.D. 61-90	$230	Last Time 61-90	11/07
As of	01/01/08	Outstanding/High	25%	Payments P.D. 91+	$220	Last Time 91+	11/07

#	Collat / Contract / Guar	Start / Renw / Close	Term / Freq / Due TD	Last Paid / Next Due	Original Amount	Balance Amount	Payment Amount (closed)	Days Past Due (in renewal) Now	Avg.	Max	Max On	Delinquencies (in renewal) 31+	61+	91+	Status / Loss
2	COMP / TruLease / -	3/06 / - / 1/07	24 / MO / 10	- / -	$21,240	$0	*$880*	-	39	151	12/06	0	1	3	COLL / $0
3	COMP / Loan / -	11/04 / - / -	48 / MO / 37	- / -	$15,170	$5,650	$510	0	25	151	12.06	0	3	4	- / $0
4	COMP / TruLease / -	1/04 / - / 1/07	48 / MO / 36	- / -	$40,630	$0	*$840*	-	25	151	12/06	0	1	3	COLL / $1,090
5	COMP / Revolver / -	6/01 / - / -	UNK / MO / 69	- / 10/01/06	$10,530	$10,530	UNK	181	61	181	2/07	4	2	6	- / $0
				Lender Totals:	$87,580	$16,180	$510					4	7	16	$1,090

Figure 1: Past Contract Terms in PayNet Credit File

Note: This figure illustrates the type of detailed information contained in a borrower credit file. Contract terms are highlighted.

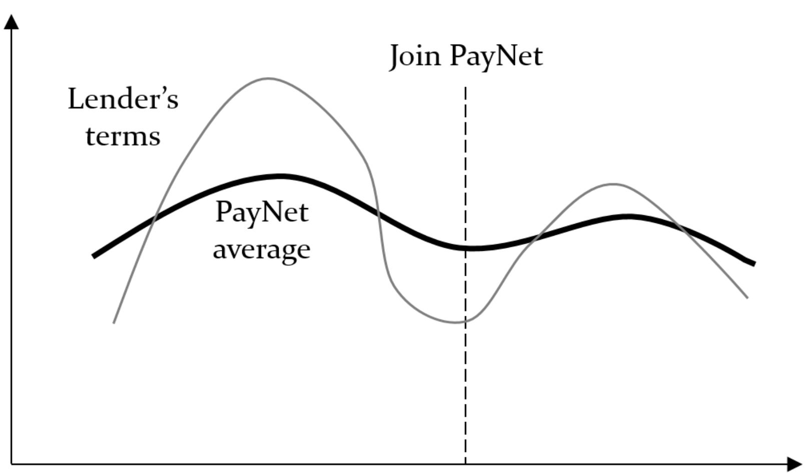


Figure 2: Empirical Strategy: Illustration

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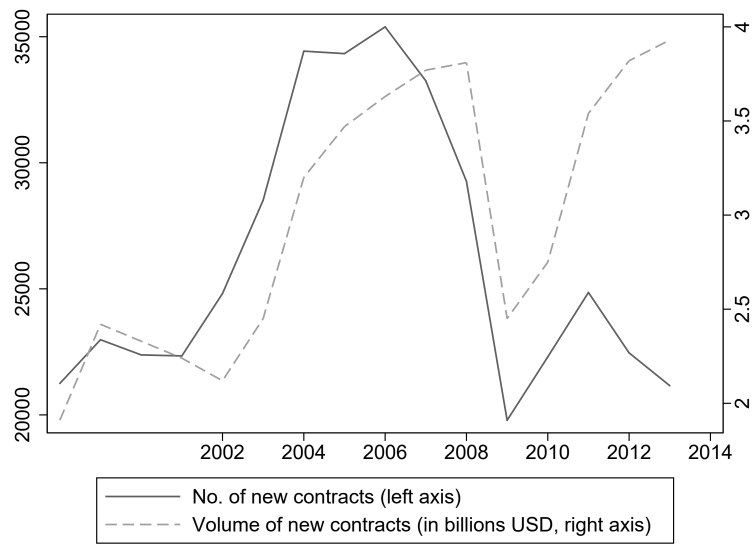

Figure 3: Origination of Contracts in PayNet

Note: This figure displays the distribution of contract originations by origination year for our random sample of PayNet data. The sample includes all contracts in our data.

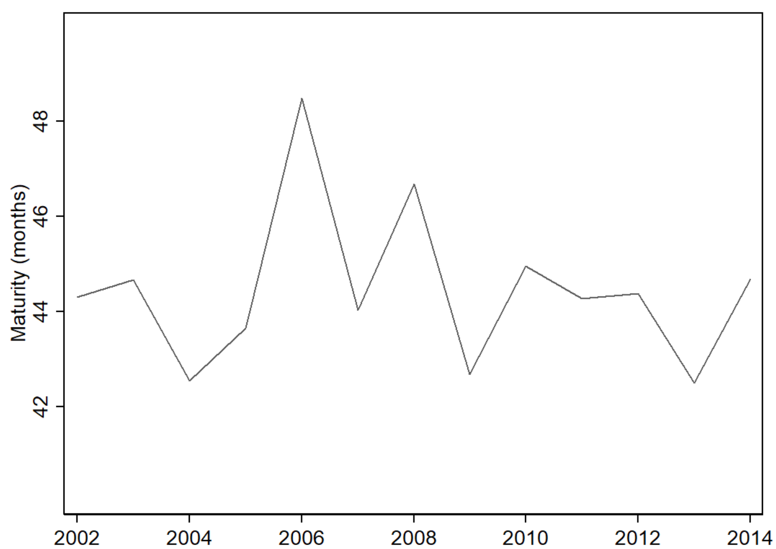

Figure 4: Contract Maturity by Origination Year

Note: This figure displays the average maturity of the contracts in our regression sample according to origination year.

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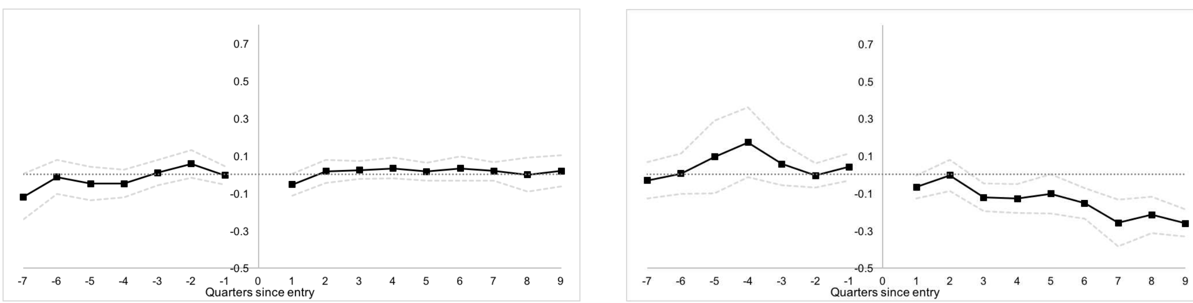
Figure 5: Joining PayNet and Contract Maturity by Market Concentration: Dynamic Coefficients Plot



(a) High HHI

(b) Low HHI

Note: This figure plots the coefficients from estimating a piecewise version of Equation (1), using event quarter indicators. For this plot, we extend the Table 4 sample to include contracts originated between the eight quarters before to eight quarters after the lender joins the bureau. The dashed lines plot 90% level confidence intervals. The sample is split according to the median HHI of the collateral type-region-contract size category measured at the contract level. The unit of observation is contract. The dependent variable is the log absolute value of the gap between the contract maturity and the bureau average maturity for that collateral type in the previous quarter (excluding the lender's own contracts).

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Table 1: Sample Description

No. of borrowers	2,076
No. of lenders	44
No. of relationships	8,194
No. of contracts	54,290
No. of collateral types	23
No. of relationships per lender	94.0
No. of relationships per borrower	2.0
No. of collateral types per lender	6.1
No. of collateral types per borrower	1.7

Note: This table presents summary statistics for the borrowers and lenders in our Table 4 regression sample.

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Table 2: Contract Characteristics

	All Contracts				Post Joining=1				Post Joining=0			
Contract Characteristics	N	Mean	Median	SD	N	Mean	Median	SD	N	Mean	Median	SD
Loan size (thousands $)	54,290	101	20.3	593	37,333	104	20.7	589	16,957	93	19.7	605
Maturity (months)	54,290	44.3	37	17	37,333	44.5	39	17	16,957	43.8	37	16
Lease (indicator)	54,290	0.81	1	0.39	37,333	0.81	1	0.39	16,957	0.82	1	0.39
Monthly repayment (indicator)	51,568	0.91	1	0.28	35,410	0.90	1	0.29	16,158	0.92	1	0.26
|Maturity gap| (months)	54,290	13.9	11.3	12.8	37,333	14.0	11.4	13.5	16,957	13.5	11.1	11.4

Note: This table presents summary statistics of the terms for the contracts in our Table 4 regression sample. The unit of observation is contract.

Table 3: Lender Entry to PayNet

	Lenders	Lender size quartile			
Year		Q1	Q2	Q3	Q4
2002	2				2
2003	1			1	
2004	9	1	1	2	5
2005	2	1			1
2006	2	1			1
2007	4	1		3	
2008	4	1	3		
2009	3		2		1
2010	0				
2011	4		3		1
2012	7	1	2	4	
2013	6	5		1	
Total	44	11	11	11	11

Note: This table displays the year of joining PayNet for lenders in our Table 4 regression sample according to the size of the lender. Lender size is measured according to total credit upon joining the bureau.

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Table 4: Joining PayNet and Contract Maturity: Main Specification

	Log | gap |			
	(1)	(2)	(3)	(4)
Post Joining	-0.069**	-0.069**	-0.067**	-0.059**
	[-2.30]	[-2.34]	[-2.12]	[-2.30]
Year FE	Yes	Yes	No	No
Lender-Borrower FE	Yes	Yes	Yes	Yes
Quarter FE	No	No	Yes	No
Collateral-Year FE	No	No	No	Yes
Controls	No	Yes	Yes	Yes
N	54,290	54,290	54,290	54,290
Adj. R-squared	0.521	0.522	0.524	0.524

*Note: This table displays the regression results from estimating Equation 1. The unit of observation is contract. The sample includes contracts originated between the four quarters before to four quarters after the lender joins the bureau. We study only lenders with at least one contract before and one contract after joining the bureau in the given collateral type. The dependent variable is the log absolute value of the gap between the contract maturity and the bureau average maturity for that collateral type in the previous quarter (excluding the lender's own contracts). Controls include indicators for contract size categories, leases, and the borrower's risk category. Standard errors are clustered by lender, and t-statistics are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.*

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Table 5: Joining PayNet and Contract Maturity: Symmetry

	(1) Log maturity	(2) Log $\lvert$gap$\rvert$
Post Joining	0.024	
	[1.16]	
Post $\times$ Positive Gap_{t-1}		-0.103*
		[-1.68]
Post $\times$ Negative Gap_{t-1}		-0.055*
		[1.91]
Year FE	Yes	Yes
Lender-Borrower FE	Yes	Yes
Controls	Yes	Yes
N	54,290	54,290
Adj. R-squared	0.666	0.522

Note: This table displays the regression results from estimating a modified version of Equation 1. The unit of observation is contract. In column (1), the dependent variable is log contract maturity. In column (2), the dependent variable is the log absolute value of the gap between the contract maturity and the bureau average maturity for that collateral type in the previous quarter (excluding the lender's own contracts). Positive Gap_{t-1} *and Negative* Gap_{t-1} *are defined based on the last contract in the relationship before the lender joins PayNet. Controls include indicators for contract size categories, leases, and the borrower's risk category. Standard errors are clustered by lender, and t-statistics are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.*

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Table 6: Robustness

	Bureau average by collateral type-quarter-region	Drop crisis period	Drop small collateral types	Bureau average for previous year	Bureau average for unrelated collateral type
	(1)	(2)	(3)	(4)	(5)
Post Joining	–0.047**	-0.078**	-0.071**	-0.042	0.059
	[-1.82]	[-2.52]	[-2.38]	[-1.03]	[0.42]
Year FE	Yes	Yes	Yes	Yes	Yes
Lender-Borrower FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
N	53,231	51,011	54,136	41,540	22,484
Adj. R-squared	0.510	0.515	0.522	0.553	0.477

*Note: This table displays the regression results from estimating variations of Equation 1. Column (1) calculates the bureau average within collateral type-quarter-region, instead of within collateral type-quarter. Column (2) drops observations during the crisis period, defined as 2008 to 2009. Column (3) drops collateral types with fewer than 100 observations. Column (4) calculates the bureau average from four quarters ago instead of one quarter ago. Column (5) uses the bureau average for an unrelated collateral type, chosen as the median of the relatedness measure defined by Liberti et al. (2018). The unit of observation is contract. Controls include indicators for contract size categories, leases, and the borrower's risk category. Standard errors are clustered by lender, and t-statistics are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.*

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Table 7: Joining PayNet and Contract Maturity: Split by Market Structure

	Log | gap |			
	(1)	(2)	(3)	(4)
	Collateral-Region-Contract Size HHI	Collateral-Contract Size HHI	Switching Rate	Top 5 Lender
Post Joining × High HHI	-0.030	-0.036		
	[-0.93]	[-1.01]		
Post Joining × Low HHI	-0.116***	-0.104***		
	[-2.91]	[-3.93]		
Post Joining × High Switching			-0.115***	
			[-4.28]	
Post Joining × Low Switching			-0.022	
			[-0.58]	
Post Joining × Top 5				-0.063**
				[-2.11]
Post Joining × Not Top 5				-0.101**
				[-2.20]
Lender-Borrower FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	53,305	54,101	54,290	54,290
Adj. R-squared	0.523	0.522	0.523	0.523

*Note: This table displays the regression results from estimating an augmented version of Equation 1 that considers various market structure measures. In columns 1 and 2, market structure is defined according to the median HHI of the collateral type-region-contract size category and collateral type-contract size category, respectively. Column 3 uses the relationship switching rate, defined as the fraction of relationships in the market last quarter that no longer exist this quarter. Column 4 uses an indicator for whether the lender is among the five largest in this particular collateral type-region-quarter combination. The unit of observation is contract. The dependent variable is the log absolute value of the gap between the contract maturity and the bureau average maturity for that collateral type in the previous quarter (excluding the lender's own contracts). Controls include indicators for contract size categories, leases, and the borrower's risk category. Standard errors are clustered by lender, and t-statistics are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.*

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Table 8: Accounting for Borrower Shocks

	Log |gap|	
	(1)	(2)
	High HHI	Low HHI
Post Joining	0.048	-0.044*
	[0.89]	[-1.79]
Borrower-Year FE	Yes	Yes
Lender-Borrower FE	Yes	Yes
Controls	Yes	Yes
N	17,615	18,175
Adj. R-squared	0.523	0.561

*Note: This table displays the regression results from estimating Equation 2. In addition to our Table 4 sample restrictions, these tests are also limited to borrowers with at least two outstanding relationships. The unit of observation is contract. The dependent variable is the log absolute value of the gap between the contract maturity and the bureau average maturity for that collateral type in the previous quarter (excluding the lender's own contracts). Controls include indicators for contract size categories, leases, and the borrower's risk category. Standard errors are clustered by lender, and t-statistics are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.*

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Table 9: Accounting for Lender Shocks

Panel A: Volume Tests

	Log |gap|			
	(1)	(2)	(3)	(4)
	HHI		HHI	
	High	Low	High	Low
Post*Volume	-0.002	-0.011*	0.002	-0.008**
	[-0.59]	[-1.67]	[0.38]	[-2.09]
Lender-Year FE	Yes	Yes	Yes	Yes
Lender-Borrower FE	Yes	Yes	Yes	Yes
Borrower-Year FE	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	26,142	27,163	17,607	18,163
Adj. R-squared	0.553	0.574	0.525	0.560

Panel B: Other Lenders' Entry Tests

	Log |gap|
	(1)
Post Large Info Shock	-0.064***
	[-2.88]
Year FE	Yes
Lender-Borrower FE	Yes
Controls	Yes
N	30,498
Adj. R-squared	0.482

*Note: Panel A displays the regression results from estimating Equation 3. Volume is defined as the number of contracts in the bureau of the same collateral type in the previous quarter. The sample in columns 1-4 is split according to the median HHI of the collateral type-region-contract size category measured at the contract level. Panel B displays the regression results from estimating a variant of Equation 1 in which the post dummy is defined with respect to when the lender experiences a large information shock for its primary collateral type after it has joined the bureau. In both panels, the unit of observation is contract. The dependent variable is the log absolute value of the gap between the contract maturity and the bureau average maturity for that collateral type in the previous quarter (excluding the lender's own contracts). Controls include indicators for contract size categories, leases, and the borrower's risk category. Standard errors are clustered by lender, and t-statistics are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.*

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Table 10: Joining PayNet and Contract Maturity: Borrower Heterogeneity

	(1)	(2)
	Log |gap|	Log |gap|
Post Joining	-0.055**	-0.085***
	[-2.12]	[-2.66]
Post Joining × Single Relationship	-0.070	
	[-1.13]	
Post Joining × Past 90+ Days Delinquency		0.036
		[1.22]
Collateral-Year FE	Yes	Yes
Lender-Borrower FE	Yes	Yes
Controls	Yes	Yes
N	54,290	54,290
Adj. R-squared	0.545	0.545

*Note: This table displays the regression results from estimating Equation 1 by borrower type. The interaction in column (1) flags borrowers with one lender at the time of contract origination. The interaction in column (2) flags borrowers whose worst delinquency in the previous three years exceeds 90 days. The unit of observation is contract. The dependent variable is the log absolute value of the gap between the contract maturity and the bureau average maturity for that collateral type in the previous quarter (excluding the lender's own contracts). Controls include indicators for contract size categories, leases, and the borrower's risk category. Standard errors are clustered by lender, and t-statistics are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.*

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Table 11: Joining PayNet and Delinquencies during 2008-2009 Crisis

	Number of quarters delinquent in 2008-2009				
	(1) All contracts	(2) High HHI market	(3) Low HHI market	(4) Housing crisis states	(5) Other states
Post Joining	0.299**	-0.430	0.501**	0.594***	0.113
	[2.54]	[-1.60]	[2.73]	[3.41]	[0.73]
Lender FE	Yes	Yes	Yes	Yes	Yes
Collateral type-quarter FE	Yes	Yes	Yes	Yes	Yes
Region-quarter FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
N	3,236	1,676	1,485	1,324	1,912
adj. R-sq	0.211	0.230	0.246	0.247	0.232

*Note: This table shows the effect of joining PayNet on delinquencies during the crisis. The sample is restricted to (1) lenders joining between 2005 and 2007 and (2) contracts originated no later than 2006 and still open in 2008-2009. The unit of observation is contract. HHI is the credit-weighted Herfindahl-Hirschman Index for the market, measured in 2001, before the bureau's inception. Housing crisis states are defined as those states with a greater than 30% housing price decline from peak, according to the FHFA index (14 states). Standard errors are clustered by lender, and t-statistics are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.*

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Online Appendix

Illustrative Theoretical Framework

Assume the following information structure:

$$\begin{pmatrix} s_l^{\phi} \\ s_l^{m} \end{pmatrix} = \begin{pmatrix} \phi \\ m_{-l} \end{pmatrix} + \begin{pmatrix} \epsilon_l^{\phi} \\ \epsilon_l^{m} \end{pmatrix}$$

and $\begin{pmatrix} \phi \\ m_{-l} \end{pmatrix} \sim N(0, \Sigma)$ and $\begin{pmatrix} \epsilon_l^{\phi} \\ \epsilon_l^{m} \end{pmatrix} \sim N(0, \Sigma_e)$, with Σ and Σ_e diagonal for simplicity. In this section, we only solve analytically for the case in which lenders' adjust their terms toward what other are offering (i.e., complementarities are strong enough), as this is the canonical case studied in the literature.

We study a linear equilibrium, in which the signal from the bureau average is linear in ϕ and m_{-l}: $\bar{m} = a_0 + a_{\phi}\phi + a_m m_{-l} + \bar{\epsilon}$. The rational expectation equilibrium (REE) literature as shown that, in this simple setting, there exists an equilibrium linear in the lender's signals, both before and after joining. In other models, the equilibrium can take different form in general, but in this section, we focus on the linear case as a first-order approximation. Before joining the bureau, lender l offers maturity:

$$m_{l,pre}^{*} = m_0 + \beta_{pre}^{\phi} s_l^{\phi} + \alpha \beta_{pre}^{\phi} s_l^{m} + \eta_{lf}$$

After joining the bureau, lender l offers maturity:

$$m_{l,post}^{*} = m_0 + (\rho^{\phi} + \alpha\rho^{m})(\bar{m} - a_0) + \beta_{post}^{\phi} s_l^{\phi} + \alpha \beta_{post}^{m} s_l^{m} + \eta_{lf}$$

In a simple REE model, these optimal choice are truly linear, while in other models, they can take more general forms. Nevertheless, for the sake of illustration, we focus on the linear case. The weight on the bureau's signal $\rho^{\phi} + \alpha\rho^{m}$ is broken down in two terms to explicitly reflect that it is informative about both ϕ and m_{-l}. In a simple REE model, the vectors of parameters ρ, a, and β are jointly determined and depend on signals' relative precision. In other models, other factors enter. For the sake of our argument, it is sufficient to solve for a in terms of ρ and β. Importantly, it is a common prediction that $\beta_{post} \leq \beta_{pre}$ across a wide class of models: lenders put less weight on their signal after joining the bureau (or equivalently, they collect less information). Although we do not model its micro-foundations, this prediction is an important ingredient of the argument below.

The argument behind the empirical strategy can be formalized as follows (for the conver-

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gence case): the variance of the gap between the lender's maturity choice m_l^* and the bureau average $\bar{m}$ decreases after joining the bureau as long as the information in the bureau is new and relevant ($\rho^\phi + \alpha\rho^m \neq 0$).

To show this, we first solve for a_ϕ and a_m in $\bar{m}$ by aggregating $m^*_{l,post}$ across lenders and identifying the coefficient on ϕ and m_{-l}:

$$\begin{aligned} a_\phi &= \beta^\phi_{post} + (\rho^\phi + \alpha\rho^m)a_\phi \\ a_m &= \alpha\beta^m_{post} + (\rho^\phi + \alpha\rho^m)a_m \end{aligned} \iff \begin{aligned} a_\phi &= \tfrac{\beta^\phi_{post}}{1-(\rho^\phi+\alpha\rho^m)} \\ a_m &= \tfrac{\alpha\beta^m_{post}}{1-(\rho^\phi+\alpha\rho^m)} \end{aligned}$$

Hence $\bar{m} = m_0 + \frac{\beta^\phi_{post}}{1-(\rho^\phi+\alpha\rho^m)}\phi + \frac{\alpha\beta^m_{post}}{1-(\rho^\phi+\alpha\rho^m)}m_{-l} + \bar{\epsilon}$. Substituting in $m^*_{l,post}$:

$$m^*_{l,post} = m_0 + \frac{\beta^\phi_{post}}{1-(\rho^\phi+\alpha\rho^m)}\phi + \frac{\alpha\beta^m_{post}}{1-(\rho^\phi+\alpha\rho^m)}m_{-l} + \beta^\phi_{post}\epsilon^\phi_l + \alpha\beta^m_{post}\epsilon^m_l + (\rho^\phi+\alpha\rho^m)\bar{\epsilon} + \eta_{lf}$$

The tracking error between $m^*_{l,post}$ and $\bar{m}$ after joining the bureau is thus:

$$d_{post} = \beta^\phi_{post}\epsilon^\phi_l + \alpha\beta^m_{post}\epsilon^m_l - (1-\rho^\phi-\alpha\rho^m)\bar{\epsilon} + \eta_{lf}$$

On the other hand, before joining the bureau the tracking error between $m^*_{l,pre}$ and $\bar{m}$ is:

$$d_{pre} = \beta^\phi_{pre}\epsilon^\phi_l + \alpha\beta^m_{pre}\epsilon^m_l - \bar{\epsilon} + \left(\beta^\phi_{pre} - \frac{\beta^\phi_{post}}{1-(\rho^\phi+\alpha\rho^m)}\right)\phi + \left(\alpha\beta^m_{pre} - \frac{\alpha\beta^m_{post}}{1-(\rho^\phi+\alpha\rho^m)}\right)m_{-l} + \eta_{lf}$$

From the last two expressions, it is clear that, as long as the bureau information is informative, the variance of tracking error d is smaller after joining the bureau. Assuming the correlation between ϵ_l and $\bar{\epsilon}$ is negligible:

$$\begin{aligned} V[d_{post}] &= {\beta^\phi_{post}}^2 V[\epsilon^\phi_l] + \alpha^2 {\beta^m_{post}}^2 V[\epsilon^m_l] + (1-\rho^\phi-\alpha\rho^m)^2 V[\bar{\epsilon}] + Var[\eta] \\ V[d_{pre}] &= {\beta^\phi_{pre}}^2 V[\epsilon^\phi_l] + \alpha^2 {\beta^m_{pre}}^2 V[\epsilon^m_l] + V[\bar{\epsilon}] + V[\eta] \\ &\quad + \left(\beta^\phi_{pre} - \frac{\beta^\phi_{post}}{1-(\rho^\phi+\alpha\rho^m)}\right)^2 V[\phi] + \left(\alpha\beta^m_{pre} - \frac{\alpha\beta^m_{post}}{1-(\rho^\phi+\alpha\rho^m)}\right)^2 V[m_{-l}] \end{aligned}$$

Inspecting term by term reveals that the variance drops after joining the bureau (note that $\beta_{post} \leq \beta_{pre}$). Only in the limit case in which the bureau information is not informative is $V[d_{post}] = V[d_{pre}]$, as $\rho^\phi + \alpha\rho^m = 0$ and $\beta_{post} = \beta_{pre}$.

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Supplemental Analysis

Table A.1: Distribution of collateral types

Collateral type	Freq.	Percent
Agricultural	3,410	6.28
Airplane	22	0.04
Automobile	595	1.10
Boat	3	0.01
Bus	128	0.24
Construction and Mining	6,049	11.14
Computer	4,538	8.36
Copier	18,737	34.51
Energy	6	0.01
Forklift	1,520	2.80
Logging	90	0.17
Medium Truck	2,547	4.69
Medical	601	1.11
Manufacturing	1,134	2.09
Office	1,217	2.24
Printing	196	0.36
Railroad	33	0.06
Real Estate	152	0.28
Retail	2,437	4.49
Telephone	2,194	4.04
Truck	8,333	15.35
Vending	237	0.44
Waste	111	0.20
Total	54,290	100.00

Note: This table presents the distribution of collateral types for the contracts in our regression sample. The unit of observation is contract.

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Table A.2: Unexplained Variation in Maturity Choice

Regressors included	Root MSE of maturity residual	R-squared
Collateral Type FE	17.27	0.04
Collateral Type FE + Year FE	17.25	0.05
Collateral Type FE + Year FE + Lender FE	16.17	0.17
Collateral Type FE + Year FE + Lender FE + Borrower FE	13.40	0.52
Collateral Type FE + Year FE + Lender-Borrower FE	10.32	0.76
Collateral Type FE + Year FE + Lender-Borrower FE + Controls	10.18	0.76

Note: This table displays the root mean squared error of a regression of contract maturity (in months) on a combination of fixed effects and controls, using our regression sample from Table 4

Table A.3: Pre-Trends

	One quarter before joining	One year before joining
Loan size ($)		
25th percentile	6,289	5,959
Median	20,241	20,000
75th percentile	67,621	68,852
Maturity (months)		
25th percentile	36	36
Median	37	37
75th percentile	60	60
Absolute gap (months)		
25th percentile	8.94	9.21
Median	12.18	11.59
75th percentile	15.96	15.64

Note: This table displays contract terms prior to the lender joining the bureau, according to when the contracts were originated.

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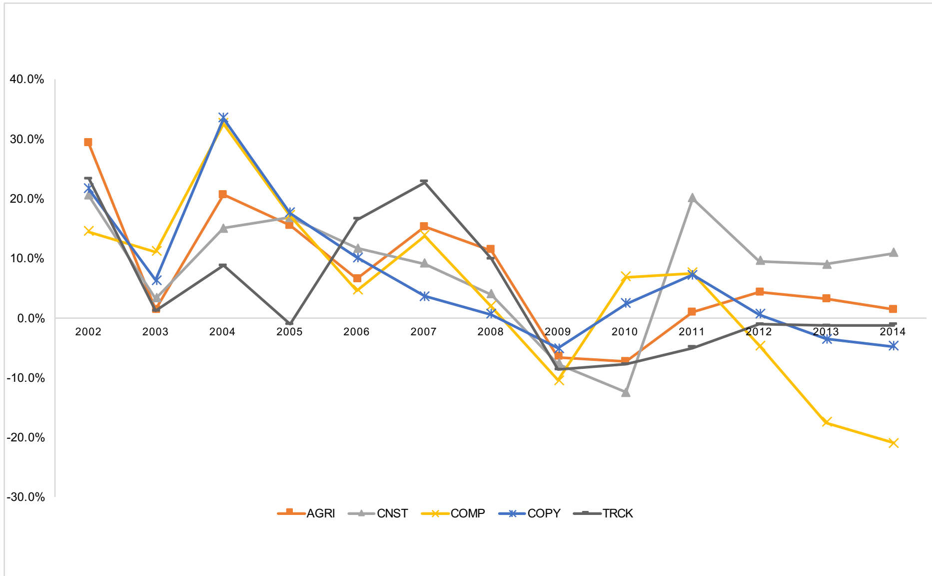

Figure A.1: Annual Growth in Bureau Contracts by Collateral Type

Note: This figure displays the annual growth rate of the number of contracts in the bureau for the five main collateral types: agricultural equipment, construction and mining equipment, computers, copiers, and trucks. The sample includes all contracts in the data.

Table A.4: Market Power Proxies: Summary Statistics

Market Power Proxy	N	Mean	S.D.
HHI for collateral type-contract size-region	53,305	0.34	0.20
HHI for collateral type-contract size	54,101	0.24	0.11
Top 5 lender indicator	54,290	0.48	0.50
Relationship switching rate	53,857	0.027	0.040

Note: This table summarizes competitive features for observations in our regression sample. The unit of observation is contract. HHI is the credit-weighted Herfindahl-Hirschman Index for the market, measured in 2001, before the bureau's inception. Markets are defined as a collateral type-census region-contract size category or collateral type-contract size category combination. The Top 5 indicator is equal to one if the lender is among the five largest in this particular collateral type-region-quarter combination. The relationship switching rate is defined as the fraction of the relationships in the collateral type-region last quarter that end this quarter.

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Table A.5: Joining PayNet and Contract Maturity: Lender Specialization

	Log |gap|				
	(1)	(2)	(3)	(4)	(5)
Specialist definition	Quarters since 1st contract in collateral type	Quarters since 1st contract in collateral type-region	Lender's most common collateral type	In lender's top 3 collateral types	Collateral type >30% of lender's portfolio
Post x Specialist	-0.002	-0.000	-0.045	-0.019	-0.038
	[-0.90]	[-0.14]	[-1.08]	[-0.49]	[-0.79]
Post	-0.050	-0.086	-0.037	-0.062**	-0.040
	[-0.75]	[-1.33]	[-0.78]	[-2.01]	[-0.75]
Specialist	0.017	0.015	0.056	-0.250***	0.076
	[1.19]	[1.56]	[0.51]	[-2.83]	[0.52]
Lender-Borrower FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
N	54,290	54,290	54,290	54,290	54,290
adj. R-sq	0.523	0.524	0.523	0.525	0.523

*Note: This table displays the regression results from augmenting Equation 1 with different specialist lender variables and their interaction with Post. All specifications include year and lender-borrower fixed effects as well as contract controls. Columns (1) and (2) define specialization as the number of quarters since the lender's first contract originated in this collateral type or collateral type-region category. Columns (3) and (4) define a lender as a specialist for a specific collateral type if that collateral type is either its most common or one of its three most common in terms of originations. Column (5) defines a lender as a specialist for a specific collateral type if that collateral type makes up at least 30% of its lending portfolio. The dependent variable is the log absolute value of the gap between the contract maturity and the bureau average maturity for that collateral type in the previous quarter (excluding the lender's own contracts). Controls include indicators for contract size categories, leases, and the borrower's risk category. Standard errors are clustered by lender, and t-statistics are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.*

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