

# PUBLIC INFORMATION AND COORDINATION: EVIDENCE FROM A CREDIT REGISTRY EXPANSION

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## Abstract

When agents have incentives to coordinate, their actions are more sensitive to public than to private information. We provide evidence of this publicity multiplier among creditors to a common borrower, by exploiting a government intervention that disclosed credit information for all borrowers with debt below \$200,000. Lenders with a negative assessment of a firm reduce lending upon the announcement that their private assessment will become common knowledge and before they receive other lenders' private information. On average, making information public increases defaults, causes a permanent decline in debt, and results in firms borrowing from fewer lenders.

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# I. Introduction

When agents have an incentive to coordinate, their actions are more sensitive to public than to private information because the former better forecasts the actions of others.<sup>1</sup> This publicity multiplier of information is a feature present in theoretical accounts of creditor runs, bank runs, currency attacks, financial crises, political action, monetary policy, and asset price volatility.<sup>2</sup> This multiplier is also a practical concern for policymakers in banking regulation, central banking, and securities regulation.<sup>3</sup> For example, IndyMac Bancorp’s bank run in June 2008 immediately followed the public release of letters by Senator Charles Schumer (Banking Committee) commenting on the health of the financial institution. In response, regulatory agencies emphasized that regulators do not publicly comment on the financial condition of open operating institutions because “it can erode public confidence, mislead depositors and investors, and cause unintended consequences, including depositor runs and panic stock trades.”<sup>4</sup> Despite the importance of the publicity multiplier for theory and policy, there is to date no evidence of its empirical relevance. The main difficulty in providing such evidence is the absence of a counterfactual: identifying the publicity multiplier requires comparing an agent’s reaction to public news to her reaction if the *same* news were private.<sup>5</sup>

The present paper provides evidence of the publicity multiplier of information in the context of a bank credit market. Credit markets provide an appropriate empirical setting for

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<sup>1</sup>Angeletos and Pavan (2004, 2007); Carlsson and van Damme (1993); Cornand and Heinemann (2008); Goldstein and Pauzner (2005); Morris and Shin (1998, 2002a, 2002b, 2005, 2007); Morris, Shin, and Tong (2006); and Svensson (2006).

<sup>2</sup>The publicity multiplier of information is discussed explicitly for creditor runs (Morris and Shin 2004), political action (Edmond 2008), monetary policy (Morris and Shin 2002b), and asset prices (Ozdenoren and Yuan 2008). As emphasized by Morris and Shin (2002a), it is a general feature of any interaction where agents have an incentive to coordinate and possess private information. Hence it is present in theoretical accounts of bank runs (Goldstein and Pauzner 2005), currency attacks (Morris and Shin 1998; Hellwig et al. 2006), and financial crises (Goldstein 2005).

<sup>3</sup>See, for example, Woodford (2005).

<sup>4</sup>Quote from John Reich, director of the Office of Thrift Supervision (see news article “Regulators to Schumer on IndyMac: Please shut up”, [http://latimesblogs.latimes.com/money\\_co/2008/07/sen-charles-e-s.html](http://latimesblogs.latimes.com/money_co/2008/07/sen-charles-e-s.html))

<sup>5</sup>In a laboratory setting, Heinemann et al. (2004) examine the effect of changing the degree of common information to test the global games unique equilibrium existence conditions. Chen et al. (2008) show that bad past performance has a stronger effect on investor decisions when mutual fund investors have an incentive to coordinate due to asset illiquidity.

this study because lenders to the same firm have an incentive to coordinate when a borrower is close to financial distress. A creditor has less incentive to provide additional liquidity to a firm if it believes that other creditors are about to liquidate their claims and potentially disrupt its operations. The relevance of these incentives is highlighted by the fact that modern bankruptcy code is designed to alleviate creditor coordination problems in distress (Jackson 1986).<sup>6</sup>

We exploit a particular credit market intervention as a natural experiment to identify the publicity multiplier: the expansion of the Public Credit Registry in Argentina in 1998. Public credit registries are government-managed databases of borrowers' credit information in a financial system. Registries exist in 71 countries and often mandate borrower level information sharing across banks (Djankov, McLiesh, and Shleifer 2007). The Argentine registry reform in 1998 publicly disclosed borrower credit information for 540,000 firms and individuals that was previously privately known by their lenders. The reform was driven by technological improvements that lowered the cost of distributing information. Before April 1998 information was shared only for borrowers whose total outstanding debt was above \$200,000 to reduce the cost of distributing information for large numbers of small debtors. The adoption of CD-ROMs eliminated the need for this threshold.

The reform made public information retroactive to January 1998, but its implementation was delayed. As a result we identify three periods in the credit registry data: a pre-announcement period, an interim period, and a post-expansion period. During the pre-announcement period, banks reported information to the Central Bank under the presumption it would remain private. During the interim period that followed the reform announcement in April 1998, lenders knew information they reported in the pre-expansion period would become public, but they had not yet received other lenders' information. This interim period allows us to measure whether the anticipated publicity induces changes in lending outcomes that can be explained by information banks had already reported. We use this

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<sup>6</sup>To document this force empirically, Asquith, Gertner, and Scharfstein (1994) show that distressed firms with more dispersed creditors find it harder to restructure out of court. Brunner and Karhnen (2008) show that German banks of distressed firms form pools prior to bankruptcy to mitigate coordination problems.

period to assess whether publicity elicits an additional response to information. During the post-expansion period, banks made reporting and lending decisions having observed the previous reports of other banks. We use this period to characterize the resulting credit market equilibrium with public information. To provide a counterfactual for the time series evolution of debt and other credit outcomes, we exploit the fact that the reform did not affect borrowers with more than \$200,000 in debt before April 1998. By focusing on firms close to and on either side of the threshold, we obtain difference-in-differences (DD) estimates that control for aggregate shocks to credit outcomes. All our reported results are drawn by comparing the changes in outcomes pre- and post-registry expansion for borrowers whose pre-expansion debt was between \$175,000 and \$200,000, relative to borrowers whose pre-expansion debt was between \$200,000 and \$225,000.

We find that lenders react strongly to their private information about a borrower upon the announcement that their information will become public. The reaction occurs only when the information is likely to affect another lender's beliefs about the same firm. In our context, this occurs when a bank possesses bad news about a firm that borrows from multiple banks.<sup>7</sup> During the interim period the registry expansion announcement causes a 15% decline in a firm's debt with lenders that had rated it a poor risk in the pre-announcement period. In contrast, those same firms' debt with lenders that assigned them a good rating in the pre-announcement period does not decline in the interim period. Debt with these lenders drops sharply after the interim period ends, when another bank's bad rating becomes public. We find a similar pattern in defaults: the default hazard rate increases by 13 percentage points during the interim period if a bank had assigned a poor rating to the firm in the pre-announcement period.

Additional results show that the registry expansion has first order effects on long-run credit outcomes that are consistent with a stylized model where lenders have incentives to coordinate. Firms whose information became public experience, on average, a permanent 8%

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<sup>7</sup>In the sample, 92.8% of the relationships (bank-firm pairs) have a risk rating of 1 (best) at the sample's beginning (January 1998). Furthermore, conditioning on having assigned a rating of 2, the probability is 85.4% that another lender to the same firm assigned it a 1.

decline in debt. The decline is due to a reduction in the likelihood of receiving new funding, consistent with banks' diminished incentives to provide interim liquidity needs. These effects are present only among firms with multiple lenders and are thus susceptible to lender coordination problems. These firms concentrate borrowing from fewer creditors after the registry expansion, potentially reducing the likelihood of coordination problems in the long run (Corsetti et al. 2004). These long-run and cross sectional patterns are difficult to reconcile with standard asymmetric information interpretations of the findings.<sup>8</sup>

Our paper relates to a broad literature that studies the effect of disclosure and transparency, particularly in credit markets. The costs and benefits of public information in environments with coordination have been discussed in recent theory papers (Morris and Shin 2002b, 2005, 2007; Angeletos and Pavan 2004, 2007; Morris et al. 2006; Woodford 2005; Svensson 2006; Cornand and Heinemann 2008). Although the welfare predictions are mixed in theory, transparency is a widely promoted policy recommendation for developing credit markets, and several papers provide empirical support for the benefits of mandated disclosure of information to investors (Bushee and Leuz 2005, Greenstone et al. 2006, Musto 2004, Simon 1989).

The results are relevant for academic and policy research on the potential effects of public credit registries. Existing empirical evaluations find a positive cross-country correlation between the existence of a credit registry and the aggregate level of lending (Jappelli and Pagano 2002; and Djankov, McLiesh, and Shleifer 2007). More recently, credit reporting has been found to have a negligible effect on borrower incentives in a laboratory environment (Brown and Zehnder 2007), but shown to generate efficiency gains for a microfinance lender in Guatemala (Janvry, McIntosh, and Sadoulet 2008). We show that a registry can increase the sensitivity of lending decisions to credit information, which can lead existing creditworthy

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<sup>8</sup>Information sharing will lead to more lending in the long run if it reduces adverse selection or moral hazard (Stiglitz and Weiss 1981), reduces hold-up by a privately informed banks (Rajan 1992), or reduces firm liquidity risk by lowering the costs of switching lenders (Detragiache, Garella, and Guiso 2000). And it will affect debt of single lender firms if it reduces bank monitoring incentives (Petersen and Rajan 1995, Rajan 1992), lowers firm reputational incentives (Padilla and Pagano 2000), or reveals hidden firm debt (Parlour and Rajan 2001, Bisin and Guaitoli 2004).

borrowers to obtain less credit in equilibrium. Our results are drawn from small firms that already have access to bank credit, so we do not draw conclusions regarding aggregate credit outcomes.

The rest of the paper proceeds as follows. Section II describes both the institutional environment and the data, and provides a brief history of Argentina’s registry expansion. In Section III, we build a stylized framework motivated by the empirical experiment to show how information sharing will impact the coordination game between creditors to the same firm. Section IV outlines the empirical strategy for identifying the effect of information sharing on credit outcomes. Section V presents the empirical results, and Section VI concludes.

## II. Empirical Setting

### A. *The Credit Registry prior to 1998*

Argentina’s public credit registry, established in 1991, is a database containing credit information on every firm and individual that obtains credit from the formal financial system. Since the registry’s inception, all formal financial institutions are required to submit to the Central Bank monthly reports that include the following information on each of its borrowers: total outstanding debt, amount of collateral pledged, and a rating reflecting the borrower’s creditworthiness and repayment status. The rating is an integer ranging from 1 to 5, where 1 represents the lowest default risk. Banks can exercise discretion in assigning ratings of 1 and 2 based on their private assessment of the borrower’s repayment prospects. Lenders are required to assign a rating of 3 to borrowers whose assessed potential default risk is high and also when the borrower has interest payments in arrears in excess of 90 days or requires principal refinancing. Ratings of 4 and 5 are mechanically determined by the repayment status of the borrower (i.e., more than 180 days in arrears, bankruptcy filings, collateral seized). Since each bank must report borrower level information, the data in the registry aggregates the entire set of loans, collateral and repayment status of each borrower with every lender.

Prior to 1995, the Central Bank of Argentina used the registry purely for the purpose of banking supervision. Outside the Central Bank and the Banking Supervision Agency, the information in the registry was only available aggregated at the bank level in quarterly financial reports. In 1995 the Central Bank granted financial institutions access to borrowers' full current credit record (debt, collateral, rating with each lender) for a subset of borrowers. A borrower's information was shared across financial institutions if 1) the borrower received a rating of 3 or higher by any bank during the prior 24 months or 2) the borrower's total debt outstanding added across all institutions exceeded \$200,000 at any time during the prior 12 months. Minimum borrowing limits for debtor eligibility in information sharing are a common feature of public credit registries due to the considerable costs of processing information for large numbers of small debtors. Of the 37 public credit registries surveyed in Miller (2003), 26 established minimum loan size cutoffs for information sharing.

Only financial institutions and credit rating companies were granted access to the registry data. Institutions that requested borrower level information received a monthly magnetic tape containing the most recent cross section of borrowers. Information reported to the Central Bank was shared with a typical delay of 3 months, i.e., the credit information for January 1998 became available in April 1998. Outside of the public credit registry, lenders could not formally ascertain how much total debt a borrower owed other financial institutions.<sup>9</sup>

### *B. CR-ROM Adoption in 1998*

In May 1998 the Central Bank switched to a low-cost technology for distributing the registry information (CD-ROMs).<sup>10</sup> The resulting lower information sharing costs made the \$200,000 threshold obsolete, and the Central Bank virtually eliminated it by sharing information for every borrower with a total debt above \$50. The elimination of the threshold was implemented retroactively to January 1998. Because the policy change was not announced until April, banks' lending and reporting decisions during the first three months of 1998 were

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<sup>9</sup>There is no secondary market for loans in Argentina. This means there is no price that can aggregate the private signals of different investors as in Angeletos and Werning (2006).

<sup>10</sup>See Central Bank Communication A2686 dated April 14, 1998 (URL: <http://www.bcra.gov.ar>).

plausibly made under the expectation that the information reported to the Central Bank would remain private.

The release of the first CD-ROM with the entire cross section of records for January 1998 was scheduled for May 20th. Several pieces of evidence indicate that, in practice, the transition to the new technology faced delays. First, the CD-ROM labeled “January 1998” contains only 26.7% of the actual total registry entries (33.8% of the total lending) in January. The information was backfilled in subsequent CD-ROMs, and the complete data for January 1998 became available with the “July 1998” CD-ROM release. Second, a media search produced no mention before July 1998 of the registry expansion. Finally, the data show that the lending decisions of different banks to the same firm become strongly correlated in July 1998, an indication of a common reaction to the release of a stock of news (see Section A and Table A1 in the Appendix). This suggests that the actual release of information occurred no sooner than July 1998. Thus, during the three months after the announcement of the registry expansion, banks knew the data in the registry would become available but had no access to it yet.

Our empirical analysis uses the monthly data from the public registry released through CD-ROMs. The sample period starts in January 1998 and covers the universe of borrowers (firms and individuals) with more than \$50 of debt with the formal banking sector in Argentina. On March 1998, the month before the announcement of the switch to CD-ROMs and virtual elimination of the threshold, the registry contains information for 566,416 borrowers in 966,513 bank-borrower lending relationships. The registry expansion increased the number of borrowers with publicly shared credit information by 540,000 firms and individuals; their debt represents 11% of the \$67 billion dollars of total outstanding debt from the banking

sector.<sup>11</sup>

### *C. Identifying the Publicity Multiplier*

To isolate the publicity multiplier, one must distinguish the effect of public information on credit outcomes from the effect of the *same* information when it is private. Consider the ideal laboratory experiment, which entails a firm in good standing that borrows from two lenders, A and B, each with private information about the firm's creditworthiness. Suppose only lender A possesses bad news about the firm, and thus assigned it a poor rating. The experiment exogenously makes bank A's private information observable by bank B. This intervention does not change the firm's creditworthiness or lender A's total information about the firm. Thus, any observed change in lending outcomes between lender A and the firm must result from lender A's expectation of B's reaction to the new information. In particular, if A expects B to withdraw financing when it observes the bad rating assigned by A and thus increase the firm's likelihood of distress, then A may withdraw credit in anticipation of this reaction. As we show formally in the next subsection, such a reaction by A represents direct evidence of the publicity multiplier of information due to coordination incentives.

The registry expansion provides a natural experiment that resembles key aspects of this ideal one. We use the ratings banks reported before the expansion announcement as a proxy for each bank's prior about firm creditworthiness. Using this proxy we can identify firms that have multiple lenders, and for which at least one lender has a bad assessment and one lender has a good assessment. Upon the announcement of the registry expansion, lenders know their private assessment will become public, but they will have not yet obtained any additional information from the registry. In line with the example, the publicity multiplier implies that

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<sup>11</sup>Note that the elimination of the threshold did not change the amount of information possessed by the Central Bank or the regulatory agency within it. Also, banking regulation rules and enforcement were not changed during 1998. The banking industry in Argentina during 1998 was characterized by growth, consolidation, and foreign capital entry (Calomiris and Powell 2000; Goldberg, Dages, and Kinney 2000). During 1998, total deposits grew by 18.6%, and total loans to the private sector (nongovernment) by 12%. The number of financial institutions declined from 134 in January 1998 to 117 two years later. The percentage of total bank lending controlled by foreign financial institutions, 35% in January 1998, increased to almost 50% by the end of 1999.

the firm’s debt with the bank that has the bad prior will decline after the registry expansion is announced. Thus, the multiplier can be identified by measuring the causal effect of the announcement of the registry expansion on credit outcomes before the information becomes common knowledge. In Section IV, we discuss in detail the estimation of the causal effect of the registry on credit outcomes.

### III. Framework: Information Sharing and Coordination

We present a stylized theoretical framework motivated by the features of our empirical environment. Our goal is to show that—due to the incentive to coordinate—information sharing can alter the way a bank reacts to the same piece of information. We also use our framework to show how making information public can alter the unconditional probability with which a firm receives financing.

#### A. Setup

Consider an entrepreneur who has obtained bank financing to purchase two complimentary assets. To study the effect of information sharing, we focus on the case where the entrepreneur has raised the finance from two separate banks. Each bank holds only one of the two assets as collateral for its loan. Each bank’s lending contract allows it to roll over or liquidate its loan. All agents are risk neutral, and the entrepreneur has no wealth of her own.

Both banks and the entrepreneur share an initial common prior about the uncertain true profitability of the project,  $\theta$ , that is distributed normally with mean  $\mu_0$  and precision  $\tau_0$ . This initial common prior is based on all publicly available information about the project, such as knowledge about the industry, the entrepreneur’s audited past financial statements, and knowledge about whether the entrepreneur has defaulted in the past.

Each bank  $i = a, b$  receives two independent signals  $s_i$  and  $x_i$  about the profitability of the loan. The first signal is  $s_i = \theta + \varepsilon_i$  where  $\varepsilon_i$  is an iid noise term distributed normal with

mean zero and precision  $\tau_\varepsilon$ . This signal represents the information that is potentially shared through the credit registry. To capture this we represent no information sharing in our model as a case where each  $s_i$  is privately observed by bank  $i$ . Conversely, information sharing corresponds to the case where the signals  $s_a$  and  $s_b$  are publicly observed. The second signal is  $x_i = \theta + \omega_i$  where  $\omega_i$  is an iid noise term distributed normal with mean zero and precision  $\tau_\omega$ . This signal is always privately observed by each bank whether or not information sharing is mandated. This assumption follows from the fact that information in a credit registry is a subset of the information banks possess about the profitability of their borrowers.

After the signals are released, each bank can choose whether to roll over its loan to the entrepreneur or to liquidate the loan and receive  $L$  from selling the collateral. This roll-over decision can be interpreted more broadly to capture a scenario where the banks are deciding whether to inject additional funds to cover an interim liquidity shock to the firm. We distinguish between the two interpretations empirically in Section V. The banks' payoffs are determined by the following simultaneous move game:<sup>12</sup>

Action	Roll Over <sub>b</sub>	Liquidate <sub>b</sub>
Roll Over <sub>a</sub>	$\theta, \theta$	$\theta - K, L$
Liquidate <sub>a</sub>	$L, \theta - K$	$L, L$

If a bank rolls the loan over, its payoff, net of any funds it injects to roll over the loan, is increasing in the true profitability of the project  $\theta$ . Maintaining an ongoing lending relationship by rolling a loan over is more valuable for more profitable projects. If one bank liquidates its claim, then this will disrupt the firm's operations and lower the expected payoff to the other bank. This occurs because the two assets are complementary; hence liquidating one lowers the value of the other. The cost of this disruption is captured by  $K$  and creates a desire for each bank to coordinate its actions with those of the other bank.

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<sup>12</sup>The first (second) element in each cell refers to  $a$ 's ( $b$ 's) payoff.

## B. *Equilibrium Roll-Over Decisions and Information Sharing*

A formal analysis of the model is presented in the Appendix. Our focus here is to use that analysis to highlight how information sharing can alter a bank's roll-over decision. If bank  $i$ 's posterior expectation of  $\theta$  is greater than  $L + K$  (less than  $L$ ), then it will optimally choose to roll over (liquidate) its loan, regardless of what it expects the other bank to do. However, if bank  $i$ 's expectation of  $\theta$  is between  $L$  and  $K + L$ , then its optimal action will depend on what it expects the other bank will do. In this range, bank  $i$  will optimally choose to roll over its loan only if it assesses the probability that the other bank will also roll over is sufficiently high. The unique equilibrium strategy of each bank is to roll over its loan if and only if its posterior belief is above some cutoff level  $\bar{\mu}$ .<sup>13</sup>

When bank  $i$ 's posterior is in this intermediate range, it will use all available information to form an assessment of bank  $j$ 's posterior and hence the probability that  $j$  will roll over its loan. Bank  $i$ 's expectation of  $j$ 's posterior is a weighted average of their shared common prior belief (formed using  $\mu_0$  and any public signals) and  $i$ 's posterior. This is the channel through which public information has a magnified effect on each bank's actions. Public information helps  $i$  forecast the action of  $j$  over and above its role in forming  $i$ 's own posterior belief.

Absent information sharing, each bank has a fixed cutoff posterior above which it will chose to roll over its loan (Figure 1, Panel A). In this case, the information each bank receives is only used to adjust its posterior. With information sharing, the cutoff strategy that each bank follows is a function of the common prior that is formed using publicly released information (see Figure 1, Panel A). If the shared information is bad news, and hence the common prior is low, bank  $i$  will use a high cutoff strategy (close to  $L + K$ ), because the pessimistic public information implies that  $j$  is likely to have a low posterior and hence liquidate its loan. This high cutoff is further reinforced by the knowledge that  $j$  is also using a high cutoff and so on.

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<sup>13</sup>If  $\theta \in (L + K, L)$  and its true value is common knowledge, then the game has multiple equilibria. We assume that the private information each bank possesses (which has at least a precision of  $\tau_\omega$ ) is sufficiently large so as to ensure that the unique equilibria concept pioneered by Carlsson and van Damme (1993) and Morris and Shin (1998) applies in our setting with and without information sharing. The specific restriction this places on parameters is given by condition (3) in the Appendix. This restriction ensures that the unique equilibrium strategy of each bank is characterized by a cutoff rule whereby it will roll over the loan if and only if its posterior belief about  $\theta$  is above some critical level.

The same argument applies symmetrically for good news. In the Appendix we show that each bank's equilibrium cut-off is strictly decreasing in the common public prior (formed using  $s_a$  and  $s_b$ ). By the same logic, the cutoff strategy that each bank uses is strictly decreasing in its own shared signal:  $\frac{\partial \bar{\mu}}{\partial s_i} < 0$ . Holding all else constant, when bank  $i$  shares bad news ( $s_i < \mu_0$ ), its expectation that the other bank will roll over declines. This highlights the publicity multiplier of information. A piece of information will have the same effect on bank  $i$ 's posterior whether or not it is shared. However, only when the information is made public does it also alter the bank's cutoff strategy.

This leads to the following empirical prediction. If a bank shares bad news, it will raise its equilibrium cutoff and thus, on the margin, will display an additional reaction to the same news that it already possessed privately. On average, a bank that held bad news before the expansion should reduce lending when it learns that other banks will see this information.

Information sharing can affect the unconditional probability that a bank will roll over its loan. The direction of this effect depends on whether the initial common prior  $\mu_0$  is high or low. Suppose that  $\mu_0$  is high. Absent information sharing, bank  $i$  will assign a high probability that its rival will roll over its own loan. As a result  $i$  will use a low cutoff rule ( $\bar{\mu}$  close to  $L$ ). This is the case represented in Figure 1, Panel A. Since the optimal cutoff each bank uses cannot fall below  $L$ , the first order effect of information sharing will be to create the possibility that bad news is released publicly and leads each bank to apply a stricter cutoff rule. Thus when  $\mu_0$  is high, information sharing will result in a decrease in the ex-ante probability that a bank rolls over its loan. A symmetric argument applies in reverse when the initial common is low.

Figure 1 (Panel B) formalizes this intuition by showing how each bank's unconditional probability of liquidating its loan is affected by information sharing. If the initial common prior is high (low) then information sharing increases (decreases) this probability. In our empirical setting, we will test whether information sharing causes an increase or decrease in the average lending level. Although the model predicts that both are possible, our analysis sample comprises firms with prior access to credit, good credit ratings (2 or better), and an

unconditional default probability below 4%, which implies that it is reasonable to presume that the initial common prior for these firms is high. Under this assumption, the model predicts that information sharing will increase the probability of liquidation and reduce average lending. Ultimately, however, this remains a question we leave for our empirical analysis.

## IV. Estimation and Descriptive Statistics

To identify the causal effect of the registry expansion announcement and subsequent release of information, we exploit the cross-sectional variation induced by the preexisting \$200,000 eligibility threshold. The registry expansion affects firms with less than \$200,000 of total debt at any time prior to April 1998. However, this effect will be confounded in the time series with the potential influence of other contemporaneous aggregate shocks. We use the firms with total debt above \$200,000 prior to April 1998, plausibly unaffected by the policy change, to construct a counterfactual.

Taking advantage of the high density of firms with total debt around \$200,000, we control nonparametrically for differences in total debt across the affected and control groups by restricting the analysis sample to borrowers whose total debt was always between \$175,000 and \$225,000 before April 1998. Since only firms with a risk rating of 1 and 2 were affected by the registry expansion, we exclude all firms with a risk rating higher than 2 in January 1998 from the sample. These restrictions exclude firms with poor ratings or that had not obtained credit from the formal financial system before April 1998. Thus, the estimates will be valid for small firms with high expected creditworthiness relative to other borrowers of similar characteristics. The panel descriptive statistics between January 1998 and April 1999 of this subsample are shown in Table 1 (Panel A). The subsample includes 1,006 borrowers with an average total debt of \$205,600 and a median of two lenders over the 16 month period starting in January 1998. Our main empirical strategy to identify the publicity multiplier involves measuring the effect of disclosing bad news about a firm that a bank possesses privately before the registry expansion. Thus, in some specifications, we focus on the subsample of firms that

have at least one rating of 2 before the registry expansion. The descriptive statistics of this subsample of firms are shown in Panel B of Table 1.

It is important to emphasize that reporting bad news, which in our analysis sample entails reporting a rating of 2, will have a significant effect on other lenders' priors about a firm's creditworthiness. The reason is that the most likely rating that a bank assigned to a firm is a 1, both unconditionally and conditioning on the firm having at least one rating of 2. More than 92% of the bank-firm pairs in the full analysis sample have a rating of 1 in January 1998. Conditioning on having assigned a rating of 2, the probability that another lender to the same firm assigned a 1 is 85.4%.

The main identification assumption is that lending outcomes of firms affected by the expansion and those in the control group would have evolved in a similar manner in the absence of the registry expansion. Aggregate shocks plausibly have the same effect on the time series of credit outcomes of firms to either side of the \$200,000 threshold. However, firms above and below the \$200,000 threshold are different, by definition, because the credit information of firms in the control group is already public. Information sharing is likely to affect both observable and unobservable firm characteristics related to credit outcomes. For example, the information content of risk ratings is different for firms in the control group, since lenders of these firms observe with a 3 month lag other banks' ratings and lending levels before assigning their own ratings. It is unlikely for this reason that the identification assumptions hold unconditionally.

Figure 2 shows evidence that suggests these assumptions hold after conditioning on pre-existing means and trends of the outcome variable of interest, both in the full analysis sample and the subsample of firms with at least one rating of 2. Panel A of Figure 2 plots the time series of median debt for firms affected by the expansion and control firms. Both series have pre-April 1998 means and trends removed throughout. There is no change in the median debt evolution of firms in the control group after the registry expansion. This holds both in the full sample and for firms with at least one rating equal to 2 before the expansion. The same is true for the average firm debt concentration, measured as the HHI of a firm's debt across

all its lenders (Figure 2, Panel B). This suggests that the registry expansion did not affect the credit outcomes of the control group, regardless of their pre-expansion credit rating. This observation rules out some types of borrower self-selection into the control group that would induce an upward bias in the DD estimates. Suppose that firms endogenously choose higher levels of total debt to make their credit records public through the registry. These control group firms would reduce their total debt after the elimination of the threshold that would be measured as a relative increase in total debt in the affected group by the DD estimate.

A different type of self-selection can occur if borrowers or lenders have incentives to prevent credit information from becoming public and choose debt levels below the \$200,000. This is an issue if selection below the threshold is correlated with firm credit quality. To explore this we plot in Figure 3 the distribution of firms and average firm characteristics by total debt for the March 1998 cross section. We expand the sample to include firms with total debt in the \$100,000 to \$300,000 range so that discontinuities at the \$200,000 threshold can be evaluated relative to the general size distribution of borrowers. The density of firms in the *treatment* group, firms affected by the registry expansion, does not appear to be abnormally high to the left of the \$200,000 threshold. Although we cannot reach definitive conclusions because of the lack of a proper counterfactual for the firm distribution, the plot suggests that there is no stark accumulation of firms below the threshold.

The firm characteristic distribution in Panel B of Figure 3 suggests that firms above and below the threshold in March 1998 are similar in the collateral-to-debt ratio and the fraction with a risk rating of 1, observable proxies for credit quality. Loan contract characteristics and risk ratings should capture differences between the treatment and control firms' credit quality that are observed by lenders. Thus, the patterns in this plot allow us to rule out self-selection of firms to the control group along dimensions of credit quality that are unobservable to the econometrician, but observable by the lenders. The plot also indicates that firms in the control group concentrate their borrowing with fewer lenders than firms in the treatment group. A regression discontinuity analysis in the cross section before the registry expansion (not shown) indicates that the concentration difference is statistically significant. The standard

interpretation of a regression discontinuity estimate would indicate that information sharing induces a significant increase in debt concentration. We corroborate this conclusion later with the DD estimation.

Finally, Figure 2 demonstrates that firm outcomes of the exposed firms were affected by the registry expansion. The median debt of affected and control firms, parallel by construction before the registry expansion, diverge after April when the registry expansion is announced. The median debt of affected firms drops relative to firms in the control group, both unconditionally and conditioning on the pre-expansion risk rating. Average debt concentration and default rates of the firms affected by the registry expansion increase relative to the control group after April 1998 (Figure 2, Panels B and C). These patterns represent strong evidence that the announcement was not anticipated and the registry information was private before the expansion.

The previous evidence establishes that credit outcomes of the control firms represent a valid counterfactual for those of the affected firms after conditioning on means and trends. In addition, our strategy for isolating the publicity multiplier relies on the timing of the effect of the registry expansion announcement on lending outcomes. For that reason, we wish to impose no structure on the time pattern of credit outcomes. These arguments provide the rationale for a DD estimation based on the following specification:

$$\ln(Debt_{it}) = \alpha_i + \xi_t + \delta_i t + \sum_{m=-2}^{12} \gamma_m \cdot PublicApril98_i \cdot I(m = t)_t + \varepsilon_{it} \quad (1)$$

The dependent variable is the (log) debt of firm  $i$  at month  $t$ . To ease interpretation we label April 1998, the last month before information sharing through the registry, as  $t = 0$ . Thus, March (May) 1998 corresponds to  $t = -1$  ( $t = 1$ ). The right-hand side includes firm fixed effects, calendar month dummies and firm specific time trends.  $PublicApril98_i$  is a dummy equal to one if firm  $i$ 's credit information becomes public after April 1998 due to the registry expansion. The coefficient on this dummy represents the log-difference between the average debt of firms affected by the registry expansion and firms in the control group.

*PublicApril98* is interacted with a full set of calendar month dummies. The interaction coefficients represent the log-debt differences across the two groups every month before and after the registry expansion.

The DD estimate of the effect of the of public information on total lending is given by the change in the estimated coefficients,  $\gamma_m$ , before and after April 1998. For example, the effect of public information on total debt one year after the expansion is given by the difference between the coefficient corresponding to March 1999 ( $\gamma_{12}$ ) and the average coefficient between February 1998 and April 1998, the pre-expansion period ( $\gamma_{pre}$ ).

All the results are reported as DD estimates, obtained over the \$175,000 and \$225,000 debt subsample, and using February through April 1998 as the pre-period. Estimates are obtained by first-differencing specification (1) to account for the firm fixed effects. All standard errors of the first-differenced specification are estimated allowing for clustering at the firm level to account for residual serial correlation in outcomes. Although excluded for brevity, the results and conclusions are robust both to widening the analysis sample to include firms with debt between \$150,000 and \$250,000 debt and to excluding April 1998 (the month when the policy change was announced) from the pre-expansion period.<sup>14</sup>

## V. Empirical Results

### A. *Publicity Multiplier*

Our strategy to isolate the publicity multiplier laid out in Sections II and III relies on measuring the lending response to bad news that was private before the registry expansion and becomes public afterwards. We start the analysis with the subsample of firms that had a rating of 2 assigned by at least one of its lenders before the registry expansion announcement (we turn to the full sample estimation below). Table 2 shows the estimated effects of the reg-

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<sup>14</sup>Narrowing the range of the analysis sample results in similar patterns of point estimates, but the statistical significance of some results becomes marginal. The robustness of the results to the choice of the estimation window represents additional evidence that firm selection in the immediate vicinity of the \$200,000 cutoff does not affect the dynamics of debt outcomes after the registry expansion conditional on firm specific trends.

istry announcement on total firm debt, on the debt with the bank(s) that assigned the rating of 2, and on the debt with the banks that assigned a rating of 1, which result from estimating specification (1) on this subsample. Each column shows the estimated parameters  $\gamma_m$ , the DD estimates, and their corresponding standard errors (for brevity, statistical significance is highlighted with asterisks only for the DD estimates).

The expansion announcement has a significant and immediate negative effect on firm debt with banks that assigned a rating of 2, i.e., banks that possess bad news about the firm before the expansion announcement. Debt with these banks declines in excess of 15% the month immediately after the announcement (Table 2, column 2). The negative effect on debt occurs even if other banks had assigned better ratings to the same firms (Table 2, column 3). In contrast, the announcement's effect on debt of the same set of firms but with banks that did not possess bad news is statistically insignificant during the two months after the announcement (Table 2, column 4). These months correspond to the interim period during which the registry information had not yet become public. Only after the information becomes public does debt with these banks drop significantly by 3.9%. Because estimates in columns 3 and 4 (Table 2) are obtained from the same sample of firms, the difference in the estimates is unlikely driven by firm-specific shocks unaccounted for with the DD estimation. Finally, neither the expansion announcement nor the actual release of registry information appears to have a significant effect on debt of firms with a single lender before the expansion announcement (Table 2, column 5).

These findings are consistent with a publicity multiplier of information. The announcement that information will become public causes a bank's lending to respond to bad news it already possesses. Under the publicity multiplier interpretation, the immediate decline in lending after the registry expansion announcement occurs in anticipation of other lenders' reactions when the bad news becomes common knowledge. The decline in lending occurs after the expansion is announced, but before information actually becomes public, thus implying that the reaction is due to the expected effect of the publicity of information and not due to the arrival of additional information. The fact that debt of the same firms with lenders

with no bad news before the registry expansion drops after the information becomes public corroborates that this expectation was rational.

The finding that the registry expansion announcement and actual sharing of information have no effect on firm debt on the subsample of borrowers with a single lender is reassuring, since the incentive for lenders to coordinate is not present for these borrowers. In addition, under the strong assumption that firms with a single lender and those with multiple lenders are affected in the same way by public information through channels other than lender coordination, this finding is inconsistent with alternate interpretations of the results. For example, by mandating information sharing, the registry may reduce banks' incentives to collect information and create incentives to free ride on the information collected by other banks. Reduced incentives to screen and monitor could potentially result in reduced equilibrium lending. However, diminished informational rents will reduce the incentives to lend to all firms, potentially more to firms with a single lender. The results suggest that reduced information collection incentives are not the main force driving the observed effects. The same argument applies to theories that suggest that releasing too much public information will lower a borrower's incentive to work hard to maintain her reputation (Padilla and Pagano 2000).

An alternative channel through which public credit information can cause a decline in lending to firms with multiple lenders is by revealing firms' hidden debt. A bank that is unaware of the number of lenders providing credit to a firm will become informed after the registry expansion. This interpretation is at odds with the fact that the announcement of the registry expansion affects outcomes only for firms with multiple lenders, before revealing any registry information. The debt decline before information is shared suggests that banks are aware that the firm had multiple lenders before the registry expansion. Also, the hidden debt account would predict a debt increase for firms revealed to have a sole lender after the registry expansion. By both accounts, the evidence indicates that hidden debt revelation does not have first order effects on credit outcomes in our empirical setting.

## B. Financial Distress

We now study the effect on the probability of default on the same subsample of firms with at least one rating of 2. The default specification compares the empirical default hazard rate of firms affected by the registry expansion to the hazard rate of control firms in a manner analogous to specification (1). Panel C of Figure 2 suggests that the registry expansion announcement causes a short-term and onetime jump in the cumulative hazard function that cannot be easily captured by a parametric duration model. To impose no structure on the timing and distribution of the effect on the hazard function, we compare empirical hazard rates through the following specification:

$$1[Default_{it} = 1 | Default_{it-1} = 0]_{it} = \xi'_t + \sum_{month=-2}^{12} \lambda_{month} PublicApril98_i \times Dum\_month_t + \zeta_{it} \quad (2)$$

The left-hand side variable is a dummy equal to zero as long as firm  $i$ 's debt is in good standing, turns to one if default happens at time  $t$ , and drops out of the sample afterwards. As in (1), the specification includes time dummies, and the right-hand side variable of interest is the interaction of an indicator variable for firms affected by the registry expansion and calendar month dummies. The estimated interaction coefficients (shown in Table 3) represent the average difference in the default hazard rates across firms affected by the registry expansion and control firms. The DD estimates of the effect on the hazard rate are reported next to each coefficient. We estimate different sets of parameters according to whether a firm defaults on any debt, on debt with the bank that assigned the rating of 2 (i.e., had bad news) or assigned a rating of 1 (i.e., was unaware of the bad news) before the expansion.

The results mirror those on debt. Firms experience a sharp and immediate increase in the probability of default: the hazard rate of defaults on any debt increases by 16 percentage points the month after the expansion announcement (Table 3, column 1). Most of the immediate increase in the hazard rate is due to default on debt with lenders that had the bad news before the expansion announcement (Table 3, columns 2 and 3). Default with

the banks that do not possess bad news before the expansion announcement also increases after the announcement, but the point estimate is not statistically significant. The default hazard with these banks increases significantly by 3.9 percentage points two months after the expansion announcement, when the information in the registry becomes available. As before, the registry expansion has no statistically significant effect on the default probability of firms with a single lender.

The expansion announcement affects the default probability before information is made public. This suggests that the anticipation of bad news becoming common knowledge increases the likelihood of firm financial distress. Financial distress can result if a lender that possesses bad news denies interim liquidity funding necessary for the firm's solvency. The lender may deny funds it would have otherwise provided because it anticipates the response by other lenders when the bad news it possesses becomes public after the registry expansion. We provide evidence later in this section that corroborates this interpretation.

The default results suggest that the observed effect on lending documented in the previous section is driven by a change in credit supply. Under the assumption that firms bear substantial costs of financial distress (see for example, Almeida and Philippon 2007), it is unlikely that firms would voluntarily reduce their demand for credit so far as to induce an immediate increase in default. The evidence on financial distress also imply that the documented decline in bank financing cannot be easily substituted for other sources of financing by the firms in our sample. This suggests that the publicity multiplier of information may affect not only credit market outcomes but also real investment.

### *C. Unconditional Effect of Information Sharing*

So far we have restricted the analysis to the subsample of firms for which banks possessed some private bad news before the registry expansion. We turn now to investigate whether lending outcomes become more sensitive to news made public after the expansion. It is, however, difficult to isolate the change in sensitivity to news caused by the publicity multiplier from that caused by the direct effect of new information. For example, a firm's downgrade

from a risk rating of 1 to 2 should be accompanied by a larger decline in lending after the registry information becomes publicly available for two reasons: first, because of the publicity multiplier of information, and second, because ratings assigned after the expansion contain information made public through the registry and are potentially more precise signals of creditworthiness due to information aggregation.<sup>15</sup>

Therefore, we focus on the average effect of the registry expansion on credit outcomes without conditioning on post-expansion risk ratings (using specifications (1) and (2)). If the registry expansion increases firms' vulnerability to coordination failures upon the future arrival of bad public news, we expect average lending to decline unconditionally in our study sample. The rationale, as discussed in Section III, is that the sample includes firms with low unconditional default probabilities. The initial common priors on firm creditworthiness for the firms in our analysis sample are plausibly high. Under these priors, information sharing increases the liquidation probability and reduces average lending. In addition, even though both treatment and control firms have their information shared through the registry after the expansion, firms in the treatment group are more vulnerable to lender coordination failures because they borrowed from more lenders before the registry expansion (we return to the rationale for this in the final subsection).

Column 1 of Table 4 shows the estimated coefficients of specification (1) over the full subsample and the DD estimates relative to the pre-expansion period. The DD point estimates indicate that average firm debt declines by 10% around nine months after the registry expansion and remains at the lower level thereafter.<sup>16</sup> The sharp immediate decline in lending observed in the subsample of firms with a rating of 2 is not observed in the full sample. This confirms that the lending results in the previous subsection are induced by the anticipation of the bad news becoming public.

The observed permanent decline in lending after the registry expansion is unlikely to be related to the stock of information revealed at the time of the registry expansion. The

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<sup>15</sup>This provides an additional rationale for performing our analysis so far on ratings assigned by banks before the expansion announcement.

<sup>16</sup>We confirm in unreported estimations that the decline persists two years post-expansion.

finding is consistent with the hypothesis that firms with perfect credit records become more vulnerable to coordination failures upon the arrival of public bad news. The permanent unconditional effect on the equilibrium lending is smaller in magnitude than the immediate effect of revealing a stock of bad news, but it is economically significant and pertains to most borrowers who have no pre-expansion indications of poor performance on their credit history.

Information sharing will, in principle, improve the creditors' assessment of each borrower's creditworthiness. However, this mechanism is hard to reconcile with the observed permanent reduction in average credit. An increase in information about creditworthiness would reduce either adverse selection or moral hazard (Jaffee and Russell 1976, Stiglitz and Weiss 1981), reduce holdup by privately informed banks (Rajan 1992), or reduce firm liquidity risk by lowering the costs of switching lenders (Detragiache, Garella, and Guiso 2000). Contrary to our findings, all these interpretations would result in more lending in equilibrium. The lender coordination framework discussed in Section III provides a plausible rationale for the negative effect of public information on equilibrium debt.

We perform two additional tests to validate this interpretation of the results. First, columns 2 and 3 of Table 4 confirm that the average permanent effect of public information estimated over the full sample is driven solely by the decline in debt of firms with multiple lenders before the expansion. Only these firms are susceptible to lender coordination issues. The DD estimates indicate that total debt of firms with multiple lenders declines by 10% to 14% nine to twelve months after the registry expansion. There is no significant effect on lending to firms that had a single lender before the expansion. As mentioned earlier, this cross-sectional heterogeneity is inconsistent with public information destroying banks' monitoring incentives or firm managers' incentives. Second, we perform placebo tests to verify that the sample selection does not mechanically produce the results in Table 4. Specification (1) is estimated assuming that the registry expansion was announced in April 1999 instead of April 1998, and assuming that the cutoff rule was applied at \$300,000 (Appendix Table A2). The samples were selected using the analysis sample's criteria (total debt in a \$50,000 window around the cutoff during three months before announcement and borrowers with a rating of

1 or 2). No DD estimate is significant in these tests.

The registry expansion announcement has a statistically significant effect on defaults of the full firm sample. The point estimate in column 4 of Table 4 indicates that the monthly default hazard rate increases by 2.6 percentage points on average the month after the registry announcement. Columns 5 and 6 of Table 4 corroborate that this increase accrues solely to firms that had multiple lenders before the expansion announcement. There is also evidence that the default hazard rate is permanently higher twelve months after the registry expansion for firms with multiple lenders, but the point estimates are marginally significant.

#### *D. Effect on Debt Growth Distribution*

When lenders have incentives to coordinate, public information can cause the documented changes in average firm debt because it leads banks to either withdraw credit (less likely to roll over loans) or stop providing new funds (less likely to cover firm's interim liquidity needs). Thus, the publicity multiplier has distinct distributional implications on debt growth. Fewer loans rolled over will lead to more frequent sharp debt declines, which will increase the mass of the left tail of the debt growth distribution. Fewer interim liquidity loans will reduce the likelihood of sharp increases in debt, which will reduce the mass on the right tail of the loan growth distribution. This section tests these distributional predictions.

We use a quantile regression model to explore how the tails of the debt growth distribution are affected by the registry expansion after April 1998. For this analysis, debt growth is defined as the percentage monthly change in debt between two consecutive months. The bottom rows of Table 5 show quantiles of this measure over the subsample of firms with multiple lenders and obtained over the pre-April period. The 5th (95th) percentile of debt growth is -20.1% (25.5%), indicating frequent and substantial month-to-month debt increases and decreases in the sample.

As before, we use firms in the control group to build a counterfactual for the debt growth distribution. We estimate the difference between percentage debt growth quantile  $\tau$  for firms affected by the expansion and firms in the control group for every month  $m$ ,  $\psi_m$ , where

months are labeled as in all previous specifications relative to April 1998. Table 5 presents the estimated  $\psi_m$  for the 5th, 10th, 50th, 90th, and 95th percentiles, as well their change relative to the pre-April period ( $\psi_m - \psi_{pre}$ ).<sup>17</sup>

The estimates indicate no systematic change in the 5th or 10th quantiles of the debt growth distribution after the registry expansion (Table 5, columns 1 and 2). This indicates that the registry expansion does not induce sharp declines in lending. Conversely, there is a substantial drop in the 90th and 95th percentiles of debt growth (Table 5, columns 4 and 5). The point estimates indicate that the 95th percentile of debt growth of the affected firms declines 30 to 40 percentage points during the three months after the expansion. The pre-April debt growth 95th percentile of the affected firms is 41%, which suggests that the announcement of information sharing virtually eliminates the likelihood of receiving additional financing during the interim period. The decline in the 95th percentile remains at 23 percentage points a year after the expansion.

These results suggest that public information substantially decreases the likelihood of firms receiving additional interim financing in this empirical context. Absent evidence of changes in other quantiles of the debt growth distribution, including the median (Table 5, column 3), this decline in access to new financing potentially explains the entire decline in average debt and provides a rationale for the immediacy of the decline. It also suggests that the accompanying increase in defaults is driven by a reduction in financing that was necessary for the firm to remain solvent. The fact that the results are still economically and statistically significant a year after the registry expansion also suggest that the reduced willingness to provide liquidity is a permanent feature of the new credit market equilibrium

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<sup>17</sup>We exploit the fact that quantile treatment effects on the marginal outcome distribution are simple differences between quantiles of the marginal distributions of potential outcomes (Firpo 2007). The estimated monthly quantile differences  $\psi_{\tau_m}$  in our application minimize the weighted check functions of the residuals of the following specification:

$$\frac{Debt_{it} - Debt_{it-1}}{Debt_{it-1}} = \left[ \delta_t + \sum_{m=-2}^{12} \psi_{\tau_m} \cdot Public\_Apr98_i \cdot I(m = t)_t \right] - u_{it}$$

Although a quantile is a nonlinear function, we obtain the pre-period quantile as the average quantile between February and April for consistency with the other estimates in the paper. The results are robust to estimating a debt growth quantile over the whole pre-period.

with public information.

### *E. Debt Concentration*

Our analysis so far has ignored the potential endogenous reaction of the structure of lending arrangements to the new information environment. Because of the registry expansion, lenders become more sensitive to bad news; thus firms are less likely to receive interim liquidity injections and become more likely to default. In theory, the consequences of lender coordination can be avoided by concentrating debt from fewer lenders. In the context of currency attacks, Corsetti et al. (2004) show that the presence of an agent with large market share can reduce the incidence of coordination failures. In practice, firms would need to balance the benefits of avoiding lender coordination problems with the costs of concentrating borrowing from fewer lenders (i.e., due to holdup). Although we cannot measure the costs and benefits involved in this trade-off directly, the increased incidence of coordination problems due to the registry expansion will likely increase the optimal debt concentration.

To explore this hypothesis, we estimate specification (1) using as dependent variables the log number of lenders ( $\#Lenders$ ), debt concentration ( $DebtHHI$ ), and the fraction of debt with the main lender ( $\%TopLender$ ). The estimated coefficients over the subsample of firms with multiple lenders before April are shown in Table 6. The DD estimates indicate that the average firm borrowed from 10.5% fewer banks and increased the fraction of debt with the main lender by 8.3% a year after the registry expansion. These changes induced an increase of 0.11 in the HHI of debt concentration across different lenders. These results are consistent with the cross-sectional patterns in debt concentration observed before the registry expansion (panel B, Figure 2). Both findings indicate that lending arrangements respond endogenously to the increased coordination induced by the publicity of information, leading to the concentration of firm borrowing from fewer banks. This endogenous response very likely mitigates the equilibrium effect of public information on debt and defaults. We still observe reduced debt and more defaults a year after the registry expansion, which suggests that firms face large potential costs when their borrowing is concentrated in few lenders. It

also suggests that limiting coordination failures is a first order force in the trade-off firms face when choosing how many creditors to borrow from. The trade-off studied in Dewatripont and Maskin (1995), Bolton and Scharfstein (1996), and Bris and Welch (2005) is affected by the degree to which information is common knowledge.

## VI. Conclusion

We provide evidence of the publicity multiplier of information among creditors who have an incentive to coordinate their actions. We exploit a natural policy experiment created by the expansion of a public credit registry in Argentina in April 1998. The timing of the expansion allows us to measure how credit outcomes are affected when a bank learns that its private information will be shared with a firm's other creditors and before it actually obtains information from these creditors. The effect of making information common knowledge is identified by comparing firms affected by the expansion (total lending between \$175,000 and \$200,000) with comparable firms not affected by it (lending between \$200,000 and \$225,000). Lending with a bank that possessed bad news about a firm's creditworthiness falls 15% when it is announced this information will be public. This effect is only present for firms that borrow from multiple banks. The same firms experience a simultaneous 13 percentage point increase in the monthly hazard rate of default the month after the expansion is announced. On average, information sharing has a first order and permanent negative effect on the average level of lending.

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# A Appendix

## A. *Timing of the Information Release*

Our empirical approach to distinguish the effect of the publicity of information from the effect of more information after the registry expansion hinges on the interim period created by the administrative implementation delays. Both quantitative data from the information content of the CD-ROM releases and qualitative information from press releases indicate that the registry information did not become available before July 1998. We now corroborate in the data that information release was in fact delayed. We also estimate the approximate timing of the release.

The lending decisions by different banks to the same firm will become strongly correlated when the stock of information in the registry becomes public, as banks react to the new common signal. We look in the time series for an abnormally high correlation across banks' lending decisions to identify the timing of the information release. We obtain a proxy for these correlations for each month by estimating an OLS regression of the (log) debt of firm  $i$  with bank  $j$  at time  $t$  on the (log) debt of the same firm  $i$  with all other lenders excluding  $j$  at time  $t$ . To control for potential aggregate shocks, we use firms in the control groups as a counterfactual, which leads to the following specification:

$$\ln(Debt_{ijt}) = \alpha_{ij} + \delta_t + \tau_i t + \sum_{m=-2}^{12} \beta_{1\_m} \ln(TDebt_{i(-j)t}) \times Dum\_m t + \sum_{m=-2}^{12} \beta_{2\_m} \ln(TDebt_{i(-j)t}) \times PublicApril98_i \times Dum\_m t + \omega_{ijt} \quad (3)$$

The dependent variable is the debt by firm  $i$  with bank  $j$  at month  $t$ . On the right hand side is the log of the total debt of firm  $i$  with all other lenders except  $j$  at time  $t$ ,  $TDebt_{i(-j)t} = \sum_{s \neq j}^{n_{it}-1} Debt_{ist}$ . The coefficients on this variable,  $\beta_{1\_m}$ , are proportional to the contemporaneous partial correlation of debt across the lenders of the same firm in month  $m$ . The coefficient on the interaction with *PublicApril98*,  $\beta_{2\_m}$ , measures the difference in this correlation between firms affected by the registry expansion and the control group. The difference-in-differences (DD) estimate of the effect of the registry expansion on lending correlation is given by the difference in the interaction coefficients before and after April 1998. The standard errors allow for clustering at the firm level to account for the mechanical correlation across different observations for the same firm in the regression estimation. We estimate by first differencing over two months to reduce the noise inherent in monthly lending changes. Estimation requires restricting the sample to firms that borrow from multiple banks. We include a firm in the sample if it had debt from more than one bank in March 1998, before the expansion announcement.

During the two months after the announcement of the registry expansion, there is no change in the correlation across lending decisions of different banks to the same firm (Table A1). In July 1998, the DD point estimates indicate that this correlation increases by 16.1 percentage points in July 1998, three months after the registry expansion announcement. The estimate is similar in sign and magnitude (18.7) when estimated using debt by other

banks lagged one month to eliminate the mechanical correlation across observations for the same firm at month  $t$  (Table A1, column 2). This represents a tenfold increase of the average lending correlation across banks in the entire sample (1.56%).

The fact that there is no significant change in the lending decisions across banks to the same firm in the first two months after the expansion announcement is consistent with our account that no information was shared during this interim period. The heightened correlation in July indicates the timing of the release of a substantial amount of information. These findings corroborate that any observed change in bank lending decisions and credit outcomes during the interim period after the announcement and before July 1998 must be due to the anticipated reaction of other lenders to the actual information release.

## B. Solution of Theoretical Model

We briefly characterize the equilibrium strategies of each bank. The basic solution method and existence results are directly analogous to the two player game studied in Morris and Shin (2002a), which establishes that each agent will employ a simple cutoff strategy when choosing its action. The generic solution with and without information sharing can be characterized as a game where each bank has a common prior (this includes any information that is shared) that  $\theta$  is distributed  $N(\mu^{com}, (\tau^{com})^{-1})$ . Let  $\mu_i^{post}$  denote bank  $i$ 's expected value of  $\theta$  after receiving all information and let  $\tau^{priv}$  denote the precision of any private information that each bank receives. Let  $\bar{\mu}$  denote the equilibrium cutoff that each bank follows. By symmetry this will be the same for each bank.

Begin by considering bank  $i$ 's belief about bank  $j$ 's posterior. Bank  $j$ 's posterior will be

$$\mu_j^{priv} = \frac{\tau^{com} \mu^{com} + \tau^{priv} \chi_j^{priv}}{\tau^{com} + \tau^{priv}}$$

where  $\chi_j^{priv}$  is the private signal that  $j$  receives. Since  $i$  does not observe  $\chi_j^{priv}$ , this forms the basis for  $i$ 's uncertainty about  $j$ 's posterior belief. Since  $\chi_j^{priv}$  is an unbiased estimate of  $\theta$ ,  $i$ 's expectation of  $\chi_j^{priv}$  is  $\mu_i^{post}$ . Accordingly, bank  $i$ 's expectation of bank  $j$ 's posterior belief is

$$E_i(\mu_j^{post} | \mu^{com}, \mu_i^{post}) = \frac{\tau^{com} \mu^{com} + \tau^{priv} \mu_i^{post}}{\tau^{com} + \tau^{priv}}.$$

Moreover  $i$ 's uncertainty about  $j$ 's posterior can be calculated by noting that  $j$ 's posterior belief is

$$\frac{\tau^{com} \mu^{com}}{\tau^{com} + \tau^{priv}} + \frac{\tau^{priv}}{\tau^{com} + \tau^{priv}} (\theta + e_j^x)$$

where  $e_j^x$  is the mean zero noise in  $j$ 's private information. Note that from  $i$ 's perspective the first term in this expression is a known constant, and hence  $i$ 's uncertainty about  $j$ 's posterior belief is drawn from  $i$ 's remaining uncertainty about  $\theta$  and  $e_j^x$ . Hence we can write the standard deviation of  $i$ 's belief about  $j$ 's posterior as

$$\sigma = \frac{\tau^{priv}}{\tau^{com} + \tau^{priv}} \sqrt{(\tau^{com} + \tau^{priv})^{-1} + (\tau^{priv})^{-1}}.$$

Bank  $i$  will choose to roll over its loan if the expected payoff is at least as large as  $L$ , i.e.,

if and only if

$$\mu_i^{post} - K \Pr(\mu_j^{post} < \bar{\mu} | \mu^{com}, \mu_i^{post}) \geq L.$$

Since bank  $i$ 's belief about  $j$ 's posterior is normally distributed, we have that

$$\Pr(\mu_j^{post} < \bar{\mu} | \mu^{com}, \mu_i^{post}) = \Phi \left( \frac{\bar{\mu} - \frac{\tau^{com} \mu^{com} + \tau^{priv} \mu_i^{post}}{\tau^{com} + \tau^{priv}}}{\sigma} \right)$$

where  $\Phi$  is the cumulative density of the standard normal distribution. Bank  $i$  will optimally choose to roll over if and only if

$$\mu_i^{post} - K \Phi \left( \frac{\bar{\mu} - \frac{\tau^{com} \mu^{com} + \tau^{priv} \mu_i^{post}}{\tau^{com} + \tau^{priv}}}{\sigma} \right) \geq L$$

and hence the equilibrium cut-off strategy must correspond to the posterior belief for which this holds with equality. Hence the equilibrium cutoff strategy,  $\bar{\mu}$ , is characterized by the following equation:

$$\bar{\mu} = K \Phi \left( \frac{\tau^{com} (\bar{\mu} - \mu^{com})}{\tau^{priv} \sqrt{(\tau^{com} + \tau^{priv})^{-1} + (\tau^{priv})^{-1}}} \right) + L. \quad (4)$$

Following the results established in Morris and Shin (2002a), the coordination game is guaranteed to have a unique equilibrium if the slope of the right-hand side in  $\bar{\mu}$  is always less than one. A cumulative normal reaches its maximal slope at zero, and hence a sufficient condition to ensure uniqueness is that

$$\left( \frac{\tau^{com}}{\tau^{priv}} \right) \left[ (\tau^{com} + \tau^{priv})^{-1} + (\tau^{priv})^{-1} \right]^{-\frac{1}{2}} \leq \frac{\sqrt{2\pi}}{K}. \quad (5)$$

For all simulated results, we will look only at parameters where this condition holds, so as to be able to make unique predictions about the effect of information sharing. This condition amounts to requiring that the precision of private information is sufficiently large relative to any public information and hence will be most constraining under information sharing.

This generic analysis can be applied to the coordination problem between banks with and without information sharing in the following way. Without information sharing

$$\mu^{com} = \mu_0, \tau^{com} = \tau_0, \tau^{priv} = \tau_\varepsilon + \tau_\omega.$$

Similarly, with information sharing

$$\mu^{com} = \frac{\tau_0 \mu_0 + \tau_\varepsilon (s_a + s_b)}{\tau_0 + 2\tau_\varepsilon}, \tau^{com} = \tau_0 + 2\tau_\varepsilon, \tau^{priv} = \tau_\omega.$$

The effect of  $\mu^{com}$  on  $\bar{\mu}$  can be obtained by implicitly differentiating (4) to give:

$$\frac{\partial \bar{\mu}}{\partial \mu^{com}} = \frac{-K\Omega\phi(\Omega(\bar{\mu} - \mu^{com}))}{1 - K\Omega\phi(\Omega(\bar{\mu} - \mu^{com}))} < 0 \quad (6)$$

$$\text{where } \Omega \equiv \frac{\tau^{com}}{\tau^{priv} \sqrt{(\tau^{com} + \tau^{priv})^{-1} + (\tau^{priv})^{-1}}} > 0$$

and  $\phi(\cdot) > 0$  is the density function of the standard normal. Note that the sign of  $\frac{\partial \bar{\mu}}{\partial \mu^{com}}$  is ensured to be negative since, by construction, the uniqueness condition (5) guarantees that  $1 - K\Omega\phi(\Omega(\bar{\mu} - \mu^{com})) > 0$ . Using this we have that with information sharing:

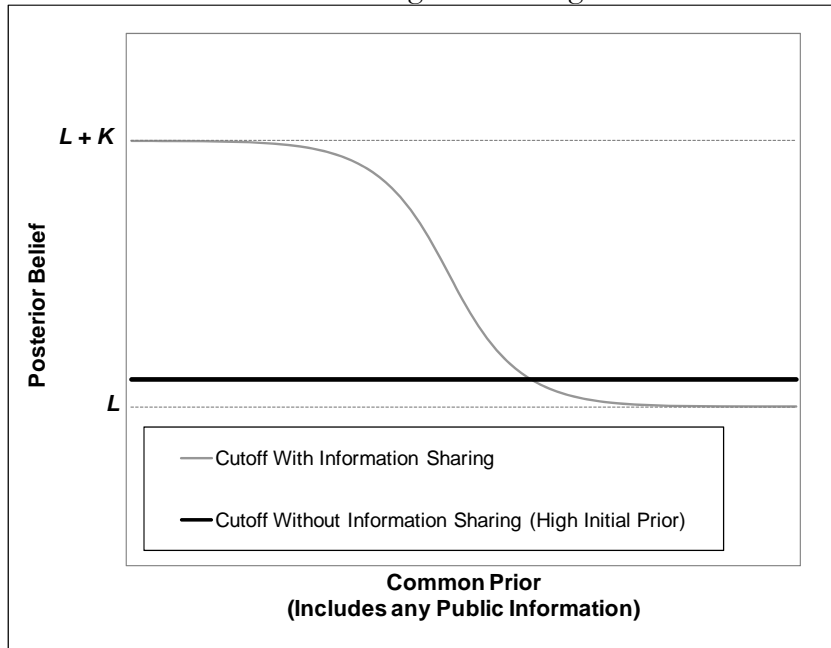
$$\frac{\partial \bar{\mu}}{\partial s_i} = \frac{\partial \mu^{com}}{\partial s_i} \frac{\partial \bar{\mu}}{\partial \mu^{com}} = \left( \frac{\tau_\varepsilon}{\tau_0 + 2\tau_\varepsilon} \right) \left( \frac{-K\Omega\phi(\Omega(\bar{\mu} - \mu^{com}))}{1 - K\Omega\phi(\Omega(\bar{\mu} - \mu^{com}))} \right) < 0.$$

Without information sharing,  $\frac{\partial \mu^{com}}{\partial s_i} = 0$ , and hence the cutoff  $\bar{\mu}$  is unaffected by  $s_i$  in this case.

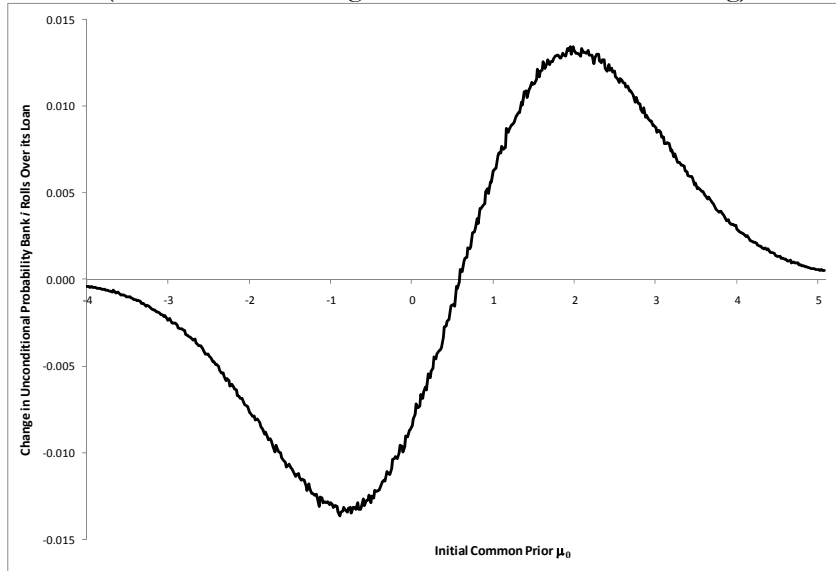
In the paper, we discuss results drawn with this characterized solution using a numerical simulation. We use the following simulation parameters (unless indicated otherwise):  $\mu_0 = 2$ ,  $\tau_0 = 0.4$ ,  $\tau_\varepsilon = 1$ ,  $\tau_\omega = 1$ ,  $K = 0.2$ ,  $L = 0.3$ , and  $\alpha = 0.4$ . All data points are generated using 1,000,000 simulations of the game.

**Figure 1**  
**Cutoff Strategies and Propensity to Liquidate**

Panel A. Bank Cutoff Strategies and a High Initial Common Prior

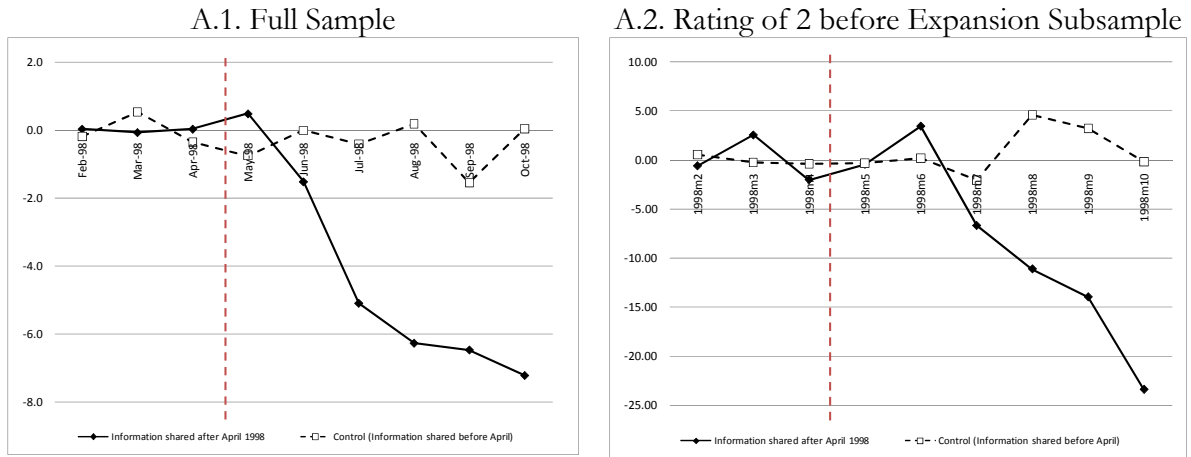


Panel B. Information Sharing and the Ex-ante Probability that a Loan is Liquidated (Information sharing minus no information sharing)

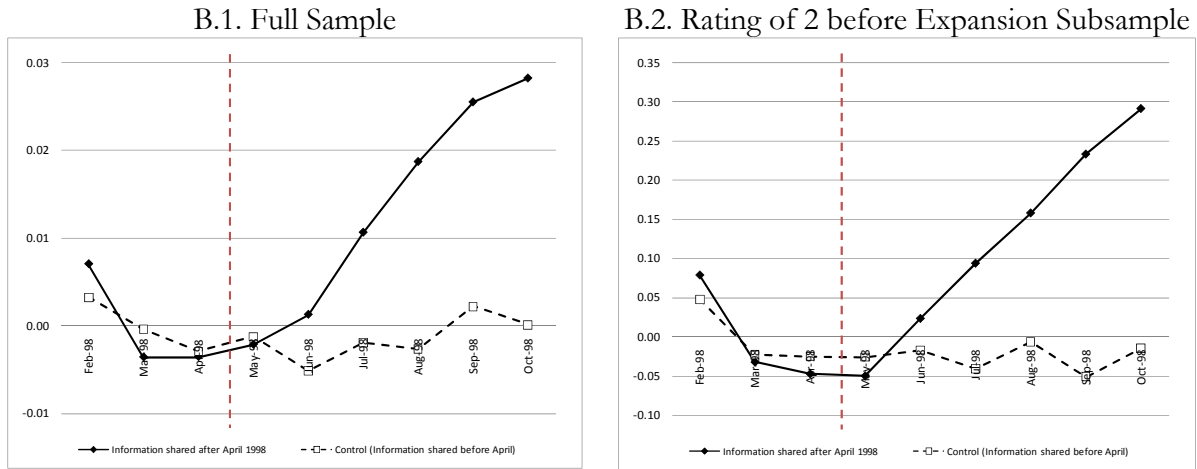


**Figure 2**  
**Firm Characteristics by Month, Affected by the Registry Expansion and Control**

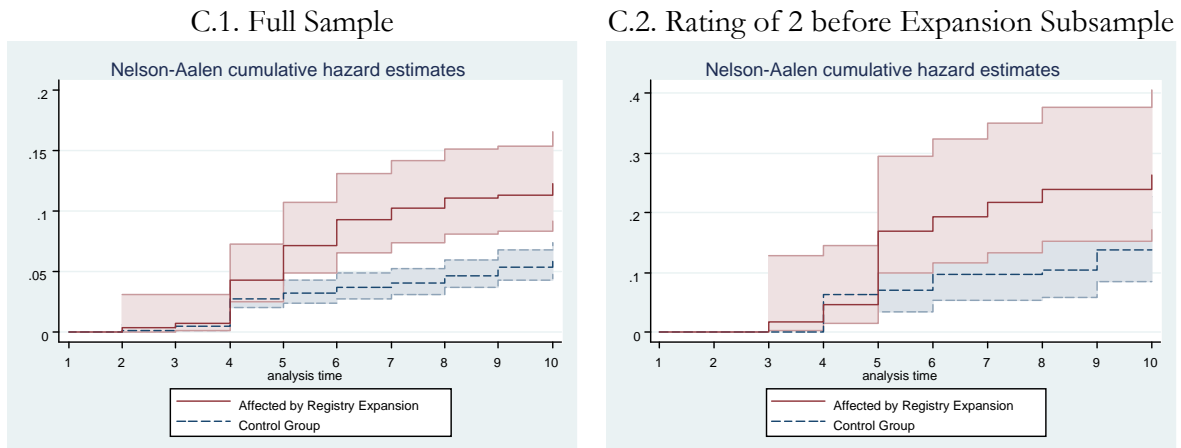
Panel A. Median Debt, aggregate pre-1998 mean/trend removed from entire series



Panel B. Firm Debt HHI, aggregate pre-1998 mean/trend removed from entire series

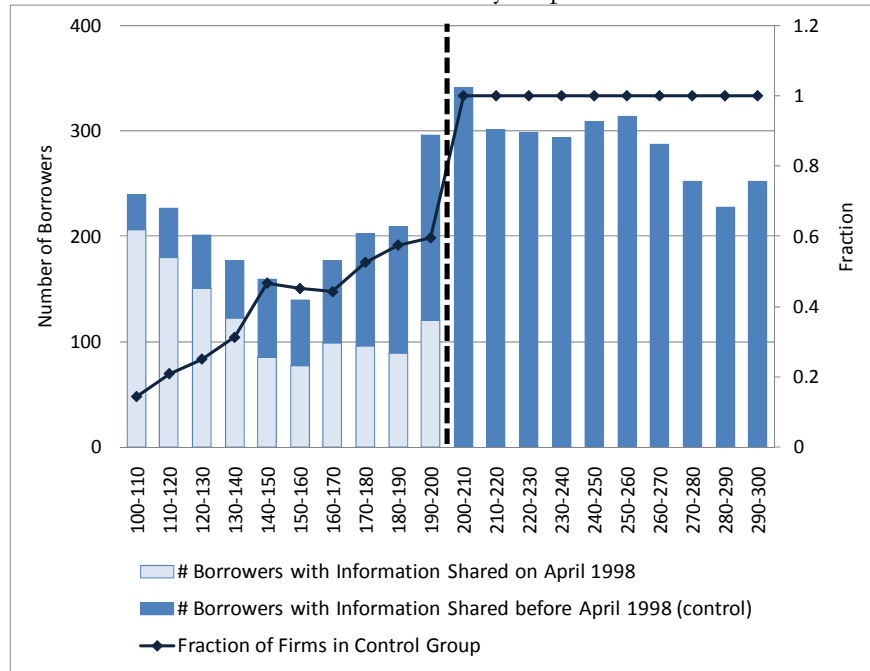


Panel C. Cumulative Default Hazard (control firms: treat=0)

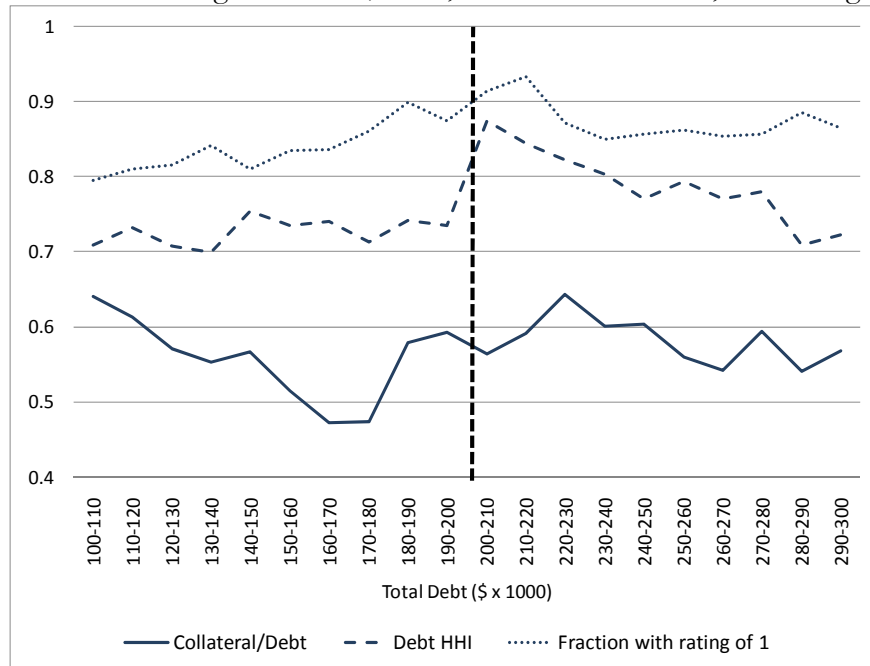


**Figure 3**  
**Borrower Distribution and Characteristics by Total Debt in March 1998**

Panel A. Number of Borrowers Affected by Expansion and in Control Group



Panel B. Average Collateral/Debt, Debt Concentration, and Rating



**Table 1**  
**Panel Descriptive Statistics, January 1998 to April 1999**  
 Firms with total debt between \$175,000 and \$225,000 and risk ratings of 1 and 2 before April 1998  
 (1,006 firms)

Variable	mean	sd	p50	min	max	N
<b>Panel A: All Firms</b>						
Firm level statistics						
Total debt ('000)	205.6	295.4	193.2	0.1	10,240	17,321
Total collateral ('000)	117.2	104.2	127.2	0	4,391	17,321
Number of lenders	1.95	1.15	2	1	9	17,321
Debt concentration (HHI)	0.85	0.21	0.99	0.20	1	17,321
Fraction debt from lead bank	0.89	0.16	1.00	0.23	1	17,321
Collateral/Debt	0.60	0.40	0.76	0	1.00	17,321
Average risk rating	1.27	0.71	1.00	1.00	5.00	17,321
In default (with any bank)	0.064					17,321
Std. Dev. of same firm ratings (*)	0.24	0.53	0.00	0	2.83	9,513
Relationship level statistics						
Debt ('000)	105.5	177.7	80.9	0	7,103	33,756
Collateral ('000)	60.1	88.4	5.5	0	4,332	33,756
Risk rating	1.2	0.7	1	1	6	33,756
In default	0.039					33,756
<b>Panel B: Firms with at least one rating of 2 before April 1998</b>						
Firm level statistics						
Total debt ('000)	184.1	67.9	188.5	0.6	520	1,684
Total collateral ('000)	119.2	74.7	125.8	0	351	1,684
Number of lenders	2.12	1.30	2	1	8	1,684
Debt concentration (HHI)	0.84	0.21	0.97	0.28	1	1,684
Fraction debt from lead bank	0.89	0.16	0.98	0.34	1	1,684
Collateral/Debt	0.64	0.37	0.77	0	1.00	1,684
Average risk rating	1.88	1.05	1.50	1.00	5.00	1,684
In default (with any bank)	0.182					1,684
Std. Dev. of same firm ratings (*)	0.60	0.68	0.58	0	2.83	1,010
Relationship level statistics						
Debt ('000)	86.7	84.2	51.0	0.1	519	3,575
Collateral ('000)	56.1	72.5	8.3	0	267	3,575
Risk rating	1.8	1.2	1	1	6	3,575
In default	0.127					3,575

(\*) Only firm-month observations where firms have debt with multiple lenders

**Table 2**  
**Publicity Multiplier: Effect of Registry Expansion on (log) Debt,**  
**Subsample of Firms with at Least One Rating of 2 before Expansion**

Sample: Firms with total debt between \$175,000 and \$225,000 before April 1998. The dependent variable: (log) debt of borrower  $i$  at time  $t$ . Right-hand side variable of interest: interaction between a dummy equal to one if borrower  $i$  had total debt below \$200,000 before April and a month dummy. Coefficients  $\gamma_m$  represent the monthly (log) debt of firms with total debt below \$200,000 before April, relative to firms with total debt above \$200,000 (control). Robust standard errors clustered at the borrower level. Difference-in-differences (DD) estimates obtained by subtracting from each coefficient  $\gamma_m$  the average coefficients in the pre-expansion period,  $\gamma_2$ ,  $\gamma_{-1}$ , and  $\gamma_0$  (February through April 1998). Statistical significance of DD estimates based on Wald test of null that the difference is equal to zero. \*, \*\*, and \*\*\* indicate test statistically significant at the 10%, 5%, and 1% level.

Subsample: Max Risk Rating Prior to April	2		2 (at least one 1)				2 (single lender)			
Dependent Variable	ln(Debt <sub>it</sub> )		ln(Debt by Banks w/ Rating = 2 <sub>it</sub> )		ln(Debt by Banks w/ Rating = 2 <sub>it</sub> )		ln(Debt by Banks w/ Rating = 1 <sub>it</sub> )		ln(Debt <sub>it</sub> )	
	(1)	(2)	(3)	(4)	(5)					
	$\gamma_m$	DD ( $\gamma_m - \gamma_{Pre}$ )	$\gamma_m$	DD ( $\gamma_m - \gamma_{Pre}$ )	$\gamma_m$	DD ( $\gamma_m - \gamma_{Pre}$ )	$\gamma_m$	DD ( $\gamma_m - \gamma_{Pre}$ )	$\gamma_m$	DD ( $\gamma_m - \gamma_{Pre}$ )
Information Public after Apr-98 × Dum_1998_05 ( $\gamma_1$ )	0.014 (0.077)	0.020 (0.061)	-0.302 (0.133)	-0.155** (0.069)	-0.34 (0.205)	-0.226** (0.096)	0.112 (0.138)	0.021 (0.077)	-0.015 (0.119)	-0.032 (0.061)
Information Public after Apr-98 × Dum_1998_06 ( $\gamma_2$ )	-0.053 (0.119)	-0.047 (0.111)	-0.441 (0.207)	-0.294* (0.174)	-0.502 (0.285)	-0.388* (0.231)	0.066 (0.170)	-0.024 (0.139)	-0.052 (0.134)	-0.069 (0.095)
Information Public after Apr-98 × Dum_1998_07 ( $\gamma_3$ )	-0.202 (0.124)	-0.196* (0.118)	-0.563 (0.214)	-0.416** (0.185)	-0.601 (0.286)	-0.487** (0.241)	-0.061 (0.175)	-0.151 (0.148)	-0.079 (0.126)	-0.096 (0.085)
Information Public after Apr-98 × Dum_1998_10 ( $\gamma_6$ )	-0.152 (0.107)	-0.146** (0.112)	-0.303 (0.149)	-0.155 (0.144)	-0.422 (0.215)	-0.309 (0.189)	-0.22 (0.159)	-0.310** (0.146)	0.001 (0.142)	-0.016 (0.174)
Information Public after Apr-98 × Dum_1999_01 ( $\gamma_9$ )	-0.182 (0.085)	-0.175 (0.087)	-0.244 (0.122)	-0.097 (0.128)	-0.402 (0.201)	-0.288 (0.198)	-0.291 (0.136)	-0.381*** (0.140)	0.017 (0.120)	0.000 (0.151)
Information Public after Apr-98 × Dum_1999_04 ( $\gamma_{12}$ )	-0.088 (0.062)	-0.081** (0.069)	-0.191 (0.114)	-0.043 (0.136)	-0.306 (0.202)	-0.192 (0.231)	-0.137 (0.100)	-0.227 (0.146)	-0.012 (0.066)	-0.029 (0.105)
First Differenced Estimation	Yes		Yes		Yes		Yes		Yes	
Firm Fixed Effects and Trends	Yes		Yes		Yes		Yes		Yes	
Month Dummies	Yes		Yes		Yes		Yes		Yes	
Observations (Firm-Month)	1,654		1,585		993		993		501	
Clusters (Firms)	95		94		69		69		36	
R-squared	0.12		0.15		0.21		0.22		0.23	

**Table 3**  
**Publicity Multiplier: Effect of Registry Expansion on Default Hazard Rate,**  
**Subsample of Firms with at Least One Rating of 2 before Expansion**

Sample: Firms with total debt between \$175,000 and \$225,000 and multiple lenders before April 1998. The table shows the results of the OLS estimation of specification (2) over the subsamples of firms with the maximum risk rating of 2 before April 1998. Each coefficient represents a difference in a monthly default hazard rate between firms affected by the expansion and control firms. Robust standard errors clustered at the borrower level. Difference-in-differences (DD) estimates are obtained by subtracting from each coefficient  $\lambda_m$  the average coefficients in the pre-expansion period,  $\lambda_{-2}$ ,  $\lambda_{-1}$ , and  $\lambda_0$  (February through April 1998). The cumulative effect is the sum of all the DD estimates up to month  $m$ . Statistical significance of the DD estimates and cumulative effects based on Wald test of null that linear combination of regression coefficients is equal to zero. \*, \*\*, and \*\*\* indicate test statistically significant at the 10%, 5%, and 1% level.

Susample: Max Risk Rating Before April	2		2 (at least one 1)				2 (single lender)			
	Default with any bank		Default with bank w/ Rating = 2		Default with bank w/ Rating = 2		Default with bank w/ Rating = 1		Default with any bank	
	(1)	(2)	(3)	(4)	(5)					
Dependent Variable: 1 if relationship in default at t, not in default at t-1	$\lambda_m$	DD ( $\lambda_m - \lambda_{Pre}$ )	$\lambda_m$	DD ( $\lambda_m - \lambda_{Pre}$ )	$\lambda_m$	DD ( $\lambda_m - \lambda_{Pre}$ )	$\lambda_m$	DD ( $\lambda_m - \lambda_{Pre}$ )	$\lambda_m$	DD ( $\lambda_m - \lambda_{Pre}$ )
Short Run:										
Information Public after Apr-98 × Dum_1998_05 ( $\lambda_1$ )	0.193 (0.071)	0.168*** (0.065)	0.166 (0.062)	0.111* (0.052)	0.136 (0.071)	0.122* (0.066)	0.052 (0.058)	0.086 (0.070)	-0.137 (0.206)	0.033 (0.089)
Information Public after Apr-98 × Dum_1998_06 ( $\lambda_2$ )	0.055 (0.063)	0.030 (0.052)	0.025 (0.052)	-0.030 (0.033)	-0.018 (0.057)	-0.032 (0.045)	0.064 (0.056)	0.097 (0.063)	-0.198 (0.197)	-0.028 (0.053)
Information Public after Apr-98 × Dum_1998_07 ( $\lambda_3$ )	0.109 (0.059)	0.084* (0.046)	0.115 (0.058)	0.060 (0.044)	0.034 (0.048)	0.020 (0.026)	0.005 (0.006)	0.039** (0.019)	-0.052 (0.211)	0.118 (0.103)
Information Public after Apr-98 × Dum_1998_10 ( $\lambda_6$ )	0.118 (0.063)	0.093* (0.053)	0.123 (0.061)	0.069 (0.050)	0.118 (0.078)	0.104 (0.075)	-0.004 (0.018)	0.029 (0.023)	-0.147 (0.193)	0.023 (0.038)
Information Public after Apr-98 × Dum_1999_01 ( $\lambda_9$ )	0.098 (0.063)	0.073 (0.053)	0.053 (0.048)	-0.002 (0.028)	0.004 (0.054)	-0.010 (0.043)	0.076 (0.065)	0.109 (0.072)	-0.147 (0.193)	0.023 (0.038)
Information Public after Apr-98 × Dum_1999_04 ( $\lambda_{12}$ )	0.073 (0.044)	0.048* (0.025)	0.073 (0.044)	0.018 (0.020)	0.038 (0.044)	0.024 (0.025)	0.000 (0.001)	0.033* (0.019)	-0.137 (0.192)	0.033 (0.039)
Month Dummies	Yes		Yes		Yes		Yes		Yes	
Observations (Firm-Month)	1,654		1,585		993		993		501	
Clusters (Firms)	95		94		69		69		36	
R-squared	0.21		0.18		0.18		0.25		0.18	

**Table 4**  
**Unconditional Effect of Registry Expansion on Credit Outcomes**

Sample: Firms with total debt between \$175,000 and \$225,000 before April 1998. Dependent variables: (log) debt of borrower  $i$  at time  $t$ , default hazard of firm  $i$  at month  $t$ . Right-hand side variable of interest: interaction between a dummy equal to one if borrower  $i$  had total debt below \$200,000 before April and a month dummy. The  $\gamma_m$  ( $\lambda_m$ ) represent the difference in log debt (hazard rate) between the firms affected by registry expansion and control firms in month  $m$ . Robust standard errors clustered at the borrower level. Difference-in-differences (DD) estimates are obtained by subtracting from each coefficient the average coefficients in the pre-expansion period (February through April 1998). Statistical significance of the DD estimates based on Wald test of null that difference is equal to zero. \*, \*\*, and \*\*\* indicate test statistically significant at the 10%, 5%, and 1% level, respectively.

Dependent Variable	ln(Debt <sub>it</sub> )						1 if relationship in default at t with any bank, not in default at t-1					
	All		Multiple Lenders		Single Lender		All		Multiple Lenders		Single Lender	
	(1)	(2)	(3)	(4)	(5)	(6)						
	$\gamma_m$	DD ( $\gamma_m - \gamma_{Pre}$ )	$\gamma_m$	DD ( $\gamma_m - \gamma_{Pre}$ )	$\gamma_m$	DD ( $\gamma_m - \gamma_{Pre}$ )	$\lambda_m$	DD ( $\lambda_m - \lambda_{Pre}$ )	$\lambda_m$	DD ( $\lambda_m - \lambda_{Pre}$ )	$\lambda_m$	DD ( $\lambda_m - \lambda_{Pre}$ )
Information Public after Apr-98	0.091	0.02	0.09	-0.004	0.084	0.054	0.015	0.026*	0.041	0.043**	-0.012	0.001
× Dum_1998_05 ( $\gamma_1$ )	(0.041)	(0.024)	(0.050)	(0.028)	(0.068)	(0.042)	(0.022)	(0.014)	(0.024)	(0.019)	(0.037)	(0.022)
Information Public after Apr-98	0.078	0.006	0.104	0.01	0.057	0.027	0.027	0.038**	0.055	0.057***	-0.006	0.006
× Dum_1998_06 ( $\gamma_2$ )	(0.049)	(0.036)	(0.062)	(0.048)	(0.085)	(0.060)	(0.024)	(0.017)	(0.024)	(0.022)	(0.037)	(0.023)
Information Public after Apr-98	0.046	-0.025	0.054	-0.04	0.05	0.02	0.011	0.022	0	0.002	0.036	0.048
× Dum_1998_07 ( $\gamma_3$ )	(0.051)	(0.041)	(0.064)	(0.053)	(0.091)	(0.065)	(0.022)	(0.014)	(0.016)	(0.010)	(0.045)	(0.034)
Information Public after Apr-98	0.049	-0.023	0.033	-0.06	0.076	0.045	0.002	0.013	0.008	0.01	0.012	0.024
× Dum_1998_10 ( $\gamma_6$ )	(0.054)	(0.050)	(0.070)	(0.066)	(0.084)	(0.067)	(0.021)	(0.013)	(0.020)	(0.015)	(0.039)	(0.026)
Information Public after Apr-98	-0.027	-0.099**	-0.05	-0.144**	0.025	-0.005	-0.007	0.004	0.001	0.003	-0.008	0.004
× Dum_1999_01 ( $\gamma_9$ )	(0.043)	(0.045)	(0.050)	(0.061)	(0.084)	(0.066)	(0.020)	(0.011)	(0.019)	(0.014)	(0.033)	(0.015)
Information Public after Apr-98	-0.009	-0.081**	-0.008	-0.101**	-0.002	-0.032	0.012	0.023	0.028	0.03*	0.012	0.024
× Dum_1999_04 ( $\gamma_{12}$ )	(0.023)	(0.034)	(0.029)	(0.048)	(0.033)	(0.040)	(0.023)	(0.016)	(0.022)	(0.018)	(0.042)	(0.032)
First Differenced Estimation	Yes		Yes		Yes							
Firm Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes	
Firm Specific Trends	Yes		Yes		Yes							
Month Dummies	Yes		Yes		Yes		Yes		Yes		Yes	
In sample after default?							No		No		No	
Observations (Firm-Month)	16,859		8,686		8,173		14,346		7,234		7,112	
Clusters (Firms)	1,006		505		501		1,006		505		501	
R-squared	0.11		0.12		0.10		0.18		0.18		0.17	

**Table 5**  
**Effect of Registry Expansion on Debt Growth Distribution**

Sample: Firms with total debt between \$175,000 and \$225,000 and multiple lenders before April 1998. The table shows the results of a quantile regression of monthly percentage debt growth of firm  $i$  at month  $t$  on interactions between a dummy equal to one if borrower  $i$  had total debt below \$200,000 before April and month dummies. Bootstrapped standard errors are reported (400 repetitions). The difference between each quantile after April 1998 and the average quantile in the pre-expansion period (February through April 1998) is reported next to each coefficient. Statistical significance is based on Wald test of null that linear combination of quantiles is equal to zero. \*, \*\*, and \*\*\* indicate test statistically significant at the 10%, 5%, and 1% level, respectively.

Dependent Variable Debt Growth Quantile	(Debt <sub>it</sub> - Debt <sub>it-1</sub> ) / Debt <sub>it-1</sub>									
	5%		10%		50%		90%		95%	
	(1)	(2)	(3)	(4)	(5)	$\psi_m$	$\psi_m - \psi_{Pre}$	$\psi_m$	$\psi_m - \psi_{Pre}$	
Information Public after Apr-98 × Dum_1998_05 ( $\gamma_1$ )	0.025 (0.115)	0.040 (0.120)	-0.016 (0.024)	0.009 (0.031)	0.004 (0.005)	0.004 (0.007)	-0.015 (0.054)	-0.200*** (0.072)	-0.148 (0.139)	-0.399** (0.160)
Information Public after Apr-98 × Dum_1998_06 ( $\gamma_2$ )	-0.039 (0.126)	-0.025 (0.130)	-0.002 (0.077)	0.023 (0.080)	0.007 (0.004)	0.007 (0.006)	0.009 (0.040)	-0.177*** (0.062)	-0.066 (0.068)	-0.317*** (0.101)
Information Public after Apr-98 × Dum_1998_07 ( $\gamma_3$ )	-0.097 (0.143)	-0.082 (0.149)	-0.046 (0.063)	-0.021 (0.067)	-0.004 (0.007)	-0.004 (0.008)	0.011 (0.039)	-0.175*** (0.060)	-0.060 (0.107)	-0.311** (0.126)
Information Public after Apr-98 × Dum_1998_10 ( $\gamma_6$ )	-0.013 (0.068)	0.001 (0.080)	-0.017 (0.040)	0.008 (0.043)	0.004 (0.005)	0.004 (0.007)	-0.074 (0.052)	-0.260*** (0.071)	-0.111 (0.089)	-0.362*** (0.123)
Information Public after Apr-98 × Dum_1999_01 ( $\gamma_9$ )	0.038 (0.070)	0.052 (0.077)	0.006 (0.024)	0.031 (0.031)	0.000 (0.006)	0.000 (0.007)	-0.001 (0.039)	-0.186*** (0.060)	-0.117 (0.089)	-0.368*** (0.114)
Information Public after Apr-98 × Dum_1999_04 ( $\gamma_{12}$ )	0.068 (0.044)	0.082 (0.058)	0.044 (0.025)	0.069** (0.032)	0.007 (0.005)	0.006 (0.007)	0.018 (0.038)	-0.168*** (0.060)	0.018 (0.163)	-0.233 (0.181)
Month Dummies	Yes		Yes		Yes		Yes		Yes	
Observations (Firm-Month)	8,686		8,686		8,686		8,686		8,686	
Pre-April Quantiles	5%		10%		50%		90%		95%	
All firms	-0.201		-0.119		-0.004		0.130		0.255	
Affected firms	-0.231		-0.159		-0.003		0.276		0.411	
Control firms	-0.201		-0.115		-0.005		0.080		0.186	

**Table 6**  
**Effect of Registry Expansion on Firm Debt Concentration**

Sample: Firms with total debt between \$175,000 and \$225,000 and multiple lenders before April 1998. The dependent variables are the (log) number of lenders, the debt HHI, and the fraction of debt with the main lender, of firm  $i$  at month  $t$ . The right-hand side variable of interest is the interaction between a dummy equal to one if borrower  $i$  had total debt below \$200,000 before April and a month dummy. Estimates are obtained after first differencing and include month dummies. The reported coefficients represent the average difference of the outcome variable of firms with total debt below \$200,000 before April, relative to control firms, after controlling for unobserved firm heterogeneity and aggregate shocks. Difference-in-differences (DD) estimates are obtained by subtracting from each coefficient  $\gamma_m$  the average coefficients in the pre-expansion period,  $\gamma_2$ ,  $\gamma_{-1}$ , and  $\gamma_0$  (February through April 1998). Statistical significance of the DD estimates based on Wald test of null that difference is equal to zero. Robust standard errors clustered at the borrower level. \*, \*\*, and \*\*\* indicate point estimate statistically significant at the 10%, 5% ,and 1% level, respectively.

Dependent Variable	ln(#Lenders <sub>it</sub> )		DebtHHI <sub>it</sub>		%TopLender <sub>it</sub>	
	(1)		(2)		(3)	
	$\gamma_m$	DD ( $\gamma_m - \gamma_{Pre}$ )	$\gamma_m$	DD ( $\gamma_m - \gamma_{Pre}$ )	$\gamma_m$	DD ( $\gamma_m - \gamma_{Pre}$ )
Firm Information Public after Apr-98	0.158	0.021	-0.111	0.006	-0.085	0.005
× Dum_1998_05 ( $\gamma_1$ )	(0.049)	(0.021)	(0.025)	(0.010)	(0.020)	(0.009)
Firm Information Public after Apr-98	0.168	0.031	-0.093	0.024**	-0.07	0.020*
× Dum_1998_06 ( $\gamma_2$ )	(0.047)	(0.025)	(0.025)	(0.012)	(0.020)	(0.011)
Firm Information Public after Apr-98	0.174	0.037	-0.085	0.032***	-0.067	0.023*
× Dum_1998_07 ( $\gamma_3$ )	(0.043)	(0.028)	(0.024)	(0.014)	(0.019)	(0.012)
Firm Information Public after Apr-98	0.122	-0.015	-0.057	0.060***	-0.046	0.044***
× Dum_1998_10 ( $\gamma_6$ )	(0.038)	(0.031)	(0.019)	(0.017)	(0.016)	(0.014)
Firm Information Public after Apr-98	0.075	-0.062	-0.04	0.077***	-0.033	0.057***
× Dum_1999_01 ( $\gamma_9$ )	(0.027)	(0.041)	(0.014)	(0.020)	(0.012)	(0.016)
Firm Information Public after Apr-98	0.032	-0.105**	-0.007	0.110***	-0.007	0.083***
× Dum_1999_04 ( $\gamma_{12}$ )	(0.017)	(0.046)	(0.008)	(0.025)	(0.008)	(0.019)
First Differenced Estimation	Yes		Yes		Yes	
Firm Fixed Effects	Yes		Yes		Yes	
Firm Specific Trends	Yes		Yes		Yes	
Month Dummies	Yes		Yes		Yes	
Observations (Firm-Month)	8,686		8,686		8,686	
Clusters (Firms)	505		505		505	
R-squared	0.22		0.17		0.17	

**Table A1**  
**Timing of the Information Release: Effect of Registry Expansion on Correlation across Lending Decisions to Same Firm**

Sample: Firms with total debt between \$175,000 and \$225,000 and multiple lenders before April 1998. The table shows the results of the OLS estimation of specification (1) in the Appendix (after first differencing to account for relationship specific heterogeneity). The dependent variable is (log) debt of firm  $i$  with bank  $j$  at month  $t$ . The right hand-side variable of interest is the (log) total debt of firm  $i$  with all banks except  $j$ . The variable is also interacted with a dummy equal to one if borrower  $i$  had total debt below \$200,000 before April and calendar month dummies. The specification includes bank-month interaction dummies and controls for common time trends in the treatment and control groups. Difference-in-differences (DD) estimates are obtained by subtracting from each coefficient  $\gamma_t$  the average coefficients in the pre-expansion period,  $\gamma_{-2}$ ,  $\gamma_{-1}$ , and  $\gamma_0$  (February through April 1998). Statistical significance of the DD estimates based on Wald test of null that difference is equal to zero. Robust standard errors are clustered at the borrower level. \*, \*\*, and \*\*\* indicate point estimate statistically significant at the 10%, 5%, and 1% level, respectively.

Dependent Variable	ln(Total Debt with Banks other than $j_{it}$ )		ln(Total Debt with Banks other than $j_{it+1}$ )	
	(1)	(2)	(1)	(2)
	$\gamma_m$	DD ( $\gamma_m - \gamma_{Pre}$ )	$\gamma_m$	DD ( $\gamma_m - \gamma_{Pre}$ )
Debt $_{ijt}$ × Information Public after Apr-98 × Dum_1998_05 ( $\gamma_1$ )	0.006 (0.012)	0.002 (0.014)	0.008 (0.01)	0.008 (0.016)
Debt $_{ijt}$ × Information Public after Apr-98 × Dum_1998_06 ( $\gamma_2$ )	0.109 (0.052)	0.104 (0.099)	0.072 (0.09)	0.072 (0.084)
Debt $_{ijt}$ × Information Public after Apr-98 × Dum_1998_07 ( $\gamma_3$ )	0.166 (0.063)	0.162*** (0.062)	0.188 (0.07)	0.187*** (0.073)
Debt $_{ijt}$ × Information Public after Apr-98 × Dum_1998_08 ( $\gamma_4$ )	0.025 (0.058)	0.02 (0.059)	0.059 (0.05)	0.059 (0.052)
Debt $_{ijt}$ × Information Public after Apr-98 × Dum_1998_09 ( $\gamma_5$ )	-0.035 (0.048)	-0.039 (0.049)	-0.005 (0.05)	-0.005 (0.050)
Debt $_{ijt}$ × Information Public after Apr-98 × Dum_1998_10 ( $\gamma_6$ )	-0.011 (0.019)	-0.016 (0.021)	-0.02 (0.03)	-0.02 (0.031)
First Differenced Estimation (2 months)	Yes		Yes	
Debt x Month Dummies	Yes		Yes	
Firm specific trends	Yes		Yes	
Firm Fixed Effects	Yes		Yes	
Bank-Month dummies	Yes		Yes	
Observations (firm-bank-months)	20,306		20,306	
Clusters (firms)	495		495	
Adjusted R-squared	0.04		0.04	

**Table A2: Placebo Tests**

Placebo 1: Estimates assuming registry expansion occurred in March 1999, one year after actual expansion. Sample: Firms with total debt between \$175,000 and \$225,000 between January and March 1999. Based on specification (1) (coefficients omitted) estimated over the sample period from January 1999 to April 2000. Placebo 2: Estimates using fake registry cutoff Rule at \$300,000. Sample: Firms with total debt between \$275,000 and \$325,000 before April 1998. Estimation based on specification (1), coefficients omitted.

Statistical significance of the difference-in-differences (DD) estimates based on Wald test of null that difference is equal to zero. Robust standard errors clustered at the borrower level. \*, \*\*, and \*\*\* indicate point estimate statistically significant at the 10%, 5%, and 1% level, respectively.

Dependent Variable	ln(Debt <sub>it</sub> )											
	Placebo 1: Assuming Expansion Occurred in 1999						Placebo 2: Assuming Pre-Expansion Debt Cutoff at \$300,000					
	All			Multiple Lenders			All			Multiple Lenders		
Firm Sample by # of lenders before Apr	1 or 2	1	2	1 or 2	1	2	1 or 2	1	2	1 or 2	1	2
Max Risk Rating before April	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
DD estimate: effect after 1 month ( $\gamma_1 - \gamma_{Feb-Apr Avg}$ )	0.022 (0.011)	0.024 (0.012)	0.017 (0.021)	0.024 (0.012)	0.028 (0.013)	0.008 (0.026)	-0.006 (0.030)	-0.002 (0.034)	-0.035 (0.030)	-0.004 (0.033)	-0.002 (0.038)	-0.017 (0.041)
DD estimate: effect after 2 months ( $\gamma_2 - \gamma_{Feb-Apr Avg}$ )	0.021 (0.013)	0.022 (0.014)	0.021 (0.022)	0.022 (0.014)	0.026 (0.016)	0.008 (0.027)	-0.009 (0.033)	0.002 (0.037)	-0.082 (0.045)	-0.007 (0.038)	0.003 (0.043)	-0.074 (0.061)
DD estimate: effect after 3 months ( $\gamma_3 - \gamma_{Feb-Apr Avg}$ )	0.001 (0.014)	0.004 (0.016)	-0.010 (0.026)	0.005 (0.017)	0.012 (0.019)	-0.024 (0.031)	0.044 (0.046)	0.065 (0.052)	-0.104 (0.060)	0.056 (0.057)	0.077 (0.065)	-0.077 (0.081)
DD estimate: effect after 6 months ( $\gamma_6 - \gamma_{Feb-Apr Avg}$ )	-0.029 (0.018)	-0.031 (0.020)	-0.014 (0.027)	-0.017 (0.020)	-0.015 (0.023)	-0.028 (0.031)	0.051 (0.052)	0.082 (0.057)	-0.160 (0.137)	-0.022 (0.062)	0.015 (0.068)	-0.256 (0.143)
DD estimate: effect after 9 months ( $\gamma_9 - \gamma_{Feb-Apr Avg}$ )	-0.008 (0.019)	-0.010 (0.022)	-0.003 (0.029)	0.012 (0.023)	0.009 (0.026)	0.019 (0.035)	0.048 (0.044)	0.045 (0.048)	0.063 (0.109)	0.017 (0.061)	0.018 (0.066)	-0.005 (0.146)
DD estimate: effect after 12 months ( $\gamma_{12} - \gamma_{Feb-Apr Avg}$ )	0.003 (0.016)	-0.002 (0.018)	0.024 (0.033)	0.012 (0.018)	0.006 (0.020)	0.035 (0.042)	0.001 (1.000)	-0.004 (1.000)	0.022 (1.000)	-0.014 (1.000)	-0.013 (1.000)	-0.041 (1.000)
First Differenced Estimation	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects and Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations (firm-months)	83,306	70,019	13,287	60,691	50,143	10,548	22,447	19,456	2,991	12,498	10,734	1,764
Clusters (firms)	4,769	4,022	747	3,424	2,835	589	1,335	1,162	173	724	623	101
R-squared	0.16	0.16	0.16	0.16	0.16	0.16	0.17	0.17	0.17	0.17	0.17	0.19