Public Information and Coordination: Evidence from a Credit Registry Expansion

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Abstract

When agents have incentives to coordinate, actions are more sensitive to public than to private information because it is a better forecast of the actions of others. We provide evidence of this publicity multiplier among creditors to a common borrower. A coordination problem arises because each creditor has less incentive to rollover financing if it believes other creditors will liquidate their claims and potentially disrupt operations. For identification we exploit a technological change in Argentina's Public Credit Registry in 1998 that led to the disclosure of debt and rating information for firms with less than $200,000 in total debt. Comparing firms either side of this threshold, we show that lenders who already had a negative assessment of a firm reduce lending upon announcement that private assessments will become common knowledge. The decline occurs only if the firm has other creditors, and before the lender receives any additional information. On average, making information public causes a permanent decline in debt and an immediate increase in defaults.

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I. Introduction

What is the effect of making information *public* in an environment where agents have an incentive to coordinate? Recently an extensive theoretical literature has provided an answer to this question: actions are more sensitive to public than private information because it is a better forecast of the actions of others.[1] The publicity multiplier of information is a feature present in recent theoretical accounts of creditor runs, bank runs, currency attacks, financial crises, political action, monetary policy, and asset price volatility.[2]

The present paper provides the first empirical evidence of this publicity multiplier of common information in coordination games.[3] We do this in the context of bank lending because it perfectly captures the two key features that characterize the environment studied in this literature. First, banks have both public and private information about borrower credit worthiness. Second, when a borrower is close to financial distress each of its creditors has an incentive to coordinate their actions. A creditor has less incentive to rollover financing or inject additional liquidity if it believes that other creditors are about to liquidate their claims, potentially disrupting operations.[4]

The fundamental reason that no empirical support for this force has been provided to date is that it is generally impossible in practice to compare an agent's reaction to public news to her reaction if the same news were private. The present paper exploits an expansion of the Public Credit Registry in Argentina to provide such a counterfactual and isolate the publicity effect of information on credit market outcomes. We find evidence that creditors are more sensitive to information when it is public and that, as a result, public information can lead to less credit, more defaults and more concentrated lending.

Public Credit Registries are government managed databases of credit information on borrowers in a

[1] Angeletos and Pavan (2004, 2007a, 2007b),Carlsson and van Damme (1993), Conrad and Heinemann (2008), Goldstein and Pauzner (2005), Morris and Shin (1998, 2002a, 2002b, 2005, 2007), Morris, Shin, and Tong (2006), Svenson (2006), and Woodford (2005).

[2] The publicty multiplier of information is discussed explicity for creditor runs (Morris and Shin 2004), political action (Edmond 2008a, 2008b), 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 coordiante and posses private information and hence is present in theoretical accounts of bank runs (Goldstein and Pauzner 2005), currency attacks (Morris and Shin 1998), and financial crises (Goldstein 2005).

[3] 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.

[4] Modern bankruptcy code is designed to alleviate creditor coordination problems in distress (Jackson 1986). 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.

financial system. Registries exist in 71 countries and often mandate borrower level information sharing across banks (Djankov, McLiesh, Shleifer (2007)). The Argentine registry reform in 1998 is uniquely suited to measuring the effect of public information for several reasons. First, the reform made public borrower credit information that was previously privately known by their lenders. This change affected 540,000 firms and individuals and was not related to firm specific changes in creditworthiness. The reform was driven by technological improvements that lowered the cost of distributing information. Prior to 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.

Second, the reform made public information retroactive to January 1998, but its implementation was delayed. As a result, lenders knew their information would become public but had not yet received information from other lenders during an interim period after the reform announcement in April 1998. This interim period allows us to plausibly measure whether a change in the anticipated publicity of information affects lending outcomes.

Finally, the reform did not affect borrowers with more than $200,000 in debt prior to April 1998, providing a plausible counterfactual. By focusing on firms close and on either side of the threshold, we can obtain difference-in-differences (DD) estimates that control for aggregate shocks to credit outcomes in the time series. All our reported results are drawn by comparing the changes in outcomes before and after the registry expansion for borrowers whose debt was between $175,000 and $200,000 prior to the expansion, relative to those of borrowers whose debt was between $200,000 and $225,000 (the control).

We find that the announcement of the registry expansion causes a sharp decline in a firm's debt if a lender already possessed bad news about it. Debt with the lender that had assigned a poor risk rating to a firm prior to the announcement drops by 15% the month after the announcement. This immediate decline occurs only if the firm also obtained credit from other lenders prior to the announcement, and persists even if the registry expansion reveals later that the other lenders had assigned better ratings to the same firm. We find a similar pattern in defaults: if a bank had assigned a poor rating to the firm prior to the announcement, the default hazard rate increases by 13 percentage points the month after the announcement. The findings imply that the anticipation of private bad news becoming observed by other lenders has an immediate negative effect on firm lending outcomes and induces financial distress.

The results represent stark evidence that information has a publicity multiplier in credit markets.

Additional evidence indicates that the registry expansion had first order effects on average credit outcomes that are consistent with lender coordination motives.[5] Firms whose information became public experience an 8% decrease in lending that persists 24 months after the registry expansion announcement. The fact that public information has a negative long run effect on debt is difficult to reconcile with standard asymmetric information interpretations.[6] In addition, this effect is present only among firms with multiple lenders, which is at odds with interpretations based on reduced bank monitoring incentives (Petersen and Rajan (1995), Rajan (1992)), lower firm incentives to work hard to maintain a good reputation (Padilla and Pagano 2000), or hidden firm debt (Parlour and Rajan 2001, Bisin and Guaitoli 2004). The evidence also indicates that the decline in average firm debt is due to a permanent drop in the likelihood of receiving new funding, consistent with bank's diminished incentives to cover interim liquidity needs. Finally, we find that firms concentrate their borrowing from fewer creditors after the registry expansion, which can potentially reduce the likelihood of facing coordination problems in the long run (Corsetti et. al. 2004).

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 several recent theory papers (Morris and Shin 2002b; 2005; 2007; Angeletos and Pavan 2004; 2007a; 2007b; Morris et. al. 2006; Woodford 2005; Svenson 2006, Conrad and Heinemann 2008). Transparency of information is a widely promoted policy recommendation for developing credit markets (for example Glennerster and Shin 2004; forthcoming). Several papers have provided empirical support for the benefits of mandated disclosure that increases the amount of information available to investors (Bushee and Leuz 2005; Chow 1983; Greenstone et. al. 2006; Musto 2004; Simon 1989). Our setting is distinct from existing work since it allows us to focus on the effect of making common knowledge information which creditors already possess.

The present paper is also relevant for academic and policy research on the potential effects of public credit registries.[7] Our results speak directly to the concerns raised by policy makers regarding

[5] Section III provides a theoretical framework where we show that information sharing can affect the unconditional probability that a bank provides interim liquidity.

[6] For example, information sharing may increase access to credit due to reduced adverse selection or moral hazard (Stiglitz and Weiss 1981), reduce hold-up by a 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.

[7] For theoretical papers on credit registries see Pagano and Jappelli (1993), Vercammen (1995), Padilla and Pagano (1997), and Padilla and Pagano (2000).

a potential 'over-reaction' of credit markets to shared creditor information (Miller 2003). We show that a registry can increase the sensitivity of lending decisions to credit information. We do not draw conclusions regarding overall credit outcomes since the empirical strategy allows us to measure a local effect of information sharing on small to medium sized borrowers. Existing empirical evaluations of credit registries based on cross-country analysis find a positive correlation between the existence of a credit registry and the aggregate level of lending (Jappelli and Pagano 2002, and Djankov, McLiesh, and Shleifer 2007). Janvry, McIntosh, and Sadoulet (2008) show that credit bureaus generate large efficiency gains for a microfinance lender in Guatemala.

The rest of the paper proceeds as follows. Section II describes the institutional environment, the data and provides a brief history of the registry expansion in Argentina. 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. Sections V and VI present the empirical results and Section VII concludes.

II. Empirical Setting and Identification of Coordination Motives

A. The Credit Registry prior to 1998

Argentina's public credit registry, established in 1991, is a database that contains 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 produce monthly reports to the Central Bank that include the following information on each of its borrowers: total debt outstanding, 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 the assignment of 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, but also when the borrower has interest payments in arrears in excess of 90 days, or requires principal refinancing. Ratings of 4 and 5 are mostly mechanically determined by the repayment status of the borrower (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 one of its lenders.

Prior to 1995, the Central Bank of Argentina used the registry purely for the purpose of banking supervision. The information in the registry was only available outside the Central Bank 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. For example the credit information for January 1998 would be shared in April 1998. Outside of the public credit registry lenders could not formally ascertain how much total debt a borrower owed other financial institutions.

B. CR-ROM Adoption in 1998

Beginning in May 1998 the Central Bank switched to a low cost technology for distributing the registry information (CD-ROMs).[8] The resulting lower information sharing costs made obsolete the $200,000 threshold, 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 announced in April, banks' lending and reporting decisions during the first three months of 1998 were plausibly made under the expectation that the

[8] Central Bank Comunicado A2679 dated April 1, 1998 proposed the termination of the availability of the magnetic tapes and the set-up of an on-line consulting system for individual credit registry searches (URL: http://www.bcra.gov.ar). Meanwhile, Central Bank Comunicado A2686 dated April 14, 1998 informed the public that the complete latest credit registry would be available on the 20s of each month via compact disc technology (CDs) at a cost of $10 (US Dollars). The release of the first CD was scheduled for May 20, 1998. Finally, Comunicado A2697 of May 5, 1998 explained both the criteria and type of information to be included in the CD.

information reported to the Central Bank would remain private.

The first CD-ROM, officially released on May 20th 1998, in principle would contain the January 1998 credit record for virtually every borrower in the financial system. In practice, the transition to the new technology faced delays and the first CD-ROM contained 26.7% of the registry entries for January (33.8% of the total loans). The information was backfilled in subsequent CD-ROMs. As a result of the implementation delays, the data in the registry corresponding to the first three months of 1998 became fully available in October 1998. This implies that during the five months after the announcement of the registry expansion banks made lending decisions knowing that the data in the registry would become available, but with no or limited access to the data itself.

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 prior to 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, whose debt represents 11% of the $67 billion dollars of total debt outstanding from the banking sector.[9]

C. Identification of Publicity Multiplier

The registry expansion is well suited for assessing empirically the implications of public information for lender coordination. A key identification problem is how to distinguish the effect of *public* information on credit outcomes, from the effect of the same information when it is *private*. The ideal laboratory experiment entails a firm that borrows from two lenders, A and B, each with private information about the firm's creditworthiness. The experiment would exogenously make bank A's private information observable by bank B. Since firm creditworthiness and lender A's total information about it are constant, 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. Such change in outcomes would

[9] The banking industry in Argentina during 1998 was characterized by growth, consolidation, and foreign capital entry (Calomiris and Powell 2000; Goldberg, Gades, and Kinney 2000). Total deposits grew by 18.6%, and total loans to the private sector (non-government) by 12% during 1998. 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.

represent direct evidence of the publicity multiplier of information due to coordination incentives.[10]

The registry expansion provides a natural experiment that resembles in key aspects this ideal one. Since we are able to see ratings assigned before the expansion was announced, we can measure if sharing leads a bank to show an additional reaction to the same information it possessed prior to the expansion. Our empirical setting departs from this ideal experiment in that information sharing is symmetric: bank A also learns bank B's private information. This implies that changes in the firm's outcomes with lender A will include the coordination motive and the effect of the new information obtained from bank B. We identify the coordination motive in this setting in two ways.

First, we exploit the fact that there is a period after the expansion announcement during which banks know information will be shared but information is still private. We expect bank A in our example to change its lending behavior in anticipation of B's reaction to the poor rating in the future. Only the coordination effect of public information in this example may happen before information becomes available through the registry. Thus, coordination motives can be identified by measuring the causal effect of the announcement of the registry expansion on credit outcomes, and before the information is released.

Second, we exploit instances where coordination and information effects on credit have opposite signs. Consider in our example the potential effect of the policy change on lending by bank A, assuming that bank A has assigned a rating of 2 (bad risk) and bank B a rating of 1 (good risk) to the same firm prior to the registry expansion. After the expansion bank A: 1) shares its bad rating with B, and 2) learns that B assigned a good rating. Since the vast majority of ratings are 1, the net effect of A and B sharing information reduces their shared common prior belief about the firm's creditworthiness. The lower common prior makes A more pessimistic that B will continue to lend and, as a result, the coordination motive makes A less willing to lend. The information effect, resulting from the fact that A observes a good rating from B, goes in the opposite direction.

In the absence of a coordination motive the firm's debt with bank A will increase after the registry expansion as A updates upwards its assessment. If the coordination effect is large, the firm's debt with bank A will decrease after the registry expansion, as it anticipates B's reaction to the bad rating A has assigned. Thus, one can measure whether coordination motives have a first order effect on debt by looking at the sign of the causal effect of the registry expansion on firm debt with banks that have

[10] Outside a laboratory, it is possible that bank A's incentives to collect information and monitor are diminished after it is forced to share information with B. We will address this concern empirically in the results section.

assigned a bad rating prior to the expansion.

In Section IV we discuss in detail how we measure the causal effect of the registry on outcomes using the time series variation induced by the registry expansion, and the cross sectional variation induced by the pre-existing $200,000 eligibility threshold. We exploit the monthly frequency of the data to study the short term effects after the policy announcement.

III. Framework: Information Sharing and Lender Coordination

We present a stylized theoretical framework motivated by the features of our empirical environment to study how information sharing can affect credit market outcomes. We use the equilibrium concept developed by Carlsson and van Damme (1993) and Morris and Shin (1998, 2002b, 2004) to study how information sharing affects the coordination game between different banks who lend to the same firm. We abstract from many features of a lending relationship both for simplicity and in order to rely on the existence results established in these papers. We assume that banks hold a collateralized debt claim and do not directly consider the contracting frictions that leads to this be the optimal contract. 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 rollover finance.

A. Set-Up

Consider an entrepreneur who has already raised bank finance in order to purchase two complimentary assets required to undertake her project. In order to study the effect of information sharing we focus on the case where the entrepreneur has raised finance for each asset from two separate banks. Each bank holds one of the two assets as collateral for their loan. Each bank's lending contract allows them to choose to rollover or liquidate her loan. All agents are risk neutral and the entrepreneur has no wealth of her own.

All banks and the entrepreneur start the game sharing an initial common prior about the uncertain true profitability of the project, θ, that is distributed normally with mean μ_0 and precision τ_0. This initial common prior is based on all publicly available information about the entrepreneur's project such as knowledge about the industry, audited past financial statements, and knowledge that the entrepreneur has not defaulted in the past. In our empirical context we will study the effect of information sharing on firms who were already receiving loans prior to information sharing and whose rating has been 2 or better with all banks for at least the last 12 months. Hence we are looking at the set of borrowers for whom this initial common belief is likely to be optimistic - i.e. absent any other news the unconditional probability that a loan is liquidated is small. This will be important when we draw empirical implications from the model.

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 ε_i is an iid noise term distributed normal with mean zero and precision τ_ε. 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 ω_i is an iid noise term distributed normal with mean zero and precision τ_ω. This signal is always privately observed by each bank whether or not there is mandated information sharing. This reflects the idea that despite the existence of credit registry banks will continue to hold private information about the profitability of their borrowers.

After the signals are released each bank can choose whether or not to rollover the loan they have extended to the entrepreneur or to liquidate the loan and receive L from selling the collateral. This rollover decision can be interpreted more broadly to capture a scenario where the banks are deciding whether or not to inject additional funds to cover an interim liquidity shock to the firm. We attempt to distinguish between the two interpretations empirically in section V. The banks' payoffs are determined by the following simultaneous move game:[11]

Action	Rollover_b	Liquidate_b
Rollover_a	θ, θ	$\theta - K, L$
Liquidate_a	$L, \theta - K$	L, L

If a bank rolls the loan over, it's payoff net of any funds it injects to rollover the loan, is increasing in the true profitability of the project θ. This reflects the idea that maintaining an ongoing lending relationship by rolling a loan over is more valuable for more profitable projects. If one bank liquidates her claim then this will disrupt the operations of the firm and lower the expected payoff to the other bank. This comes from the fact that the two assets being financed are complementary and hence

[11] The first (second) element in each cell refers to a's (b's) payoff.

liquating 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 their actions with the actions of the other bank.

B. *Equilibrium Rollover 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 rollover decision. If bank i's posterior expectation of θ is greater than $L+K$ (less than L) then she will optimally choose to rollover (liquidate) her loan, regardless of what she expects the other bank to do. However if bank i's expectation of θ is between L and $K+L$ then her optimal action will depend on what she expects the other bank will do. In this range bank i will optimally choose to rollover her loan only if she assesses the probability that the other bank will also rollover is sufficiently high. The unique equilibrium strategy of each bank is to rollover their loan if and only if their posterior belief is above some cutoff level $\overline{\mu}$.[12]

When bank i's posterior is in this intermediate range she will use all available information to form an assessment of bank j's posterior and hence the probability that j will rollover her loan. Bank i's expectation of j's posterior is a weighted average of their shared common prior belief (formed using μ_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. This can be seen by comparing the equilibrium cut-off strategies that both banks adopt with and without information sharing (Appendix Figure A1).

Absent information sharing, each bank has a fixed cut-off posterior above which they will chose to rollover their loan. We show formally in the appendix that i's cutoff strategy is unaffected by the signals she receives when information is not shared: $\frac{\partial \overline{\mu}}{\partial s_i} = 0$. In this case the information each bank receives is only used to adjust their posterior.

With information sharing the cut-off strategy that each bank follows is a function of the information that is publicly released. Figure A1 draws the cut-off strategy for different levels of the common prior that is formed using the shared signals s_a and s_b. If the shared information is positive, and hence the common prior is high, bank i will use a low cutoff strategy (close to L) because the optimistic public

[12] 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 precision of τ_ω) 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 (5) in the Appendix. This restriction ensures that the unique equilibrium strategy of each bank is characterized by a cut-off rule whereby she will rollover the loan if and only if her posterior belief about θ is above some critical level.

information implies that j is likely to have a high posterior and hence rollover her loan. This low cutoff is further re-enforced by the knowledge that j is also using a low cut-off and so on. Conversely, if bad news is shared thereby lowering the common prior, then each bank will assign a high probability to the other choosing to liquidate. As a result they will use a higher cutoff posterior belief. We establish this formally in the appendix by showing that the equilibrium cut-off that each bank uses 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: $\frac{\partial \overline{\mu}}{\partial s_i} < 0$. 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 alter bank i's posterior whether or not it is shared. However, only when the information is made public does it also alter the cut-off strategy rule that the bank uses.

This leads to two immediate empirical predictions. The first is a stark implication of the publicity multiplier. If a bank shares bad news it will raise it's equilibrium cut-off and thus, on the margin, will display an additional reaction to the same news that it already possessed privately. On average a bank who held bad news prior to the expansion should reduce lending when she learns that other banks will see this information. This effect will persist as long as the shared information on net lowers the resulting shared common belief (i.e. whenever $\frac{1}{2}(s_a + s_b) < \mu_0$). Second, as a result we should see an increased cross sectional incidence of loan liquidations, lending reductions and liquidity induced default following the registry expansion for firms who have bad news shared through the registry.

Information sharing can affect the unconditional probability that a bank will rollover its loan. The direction of this effect depends on whether the initial common prior μ_0 is high or low. Suppose that μ_0 is high. Absent information sharing bank i will assign a high probability to her rival rolling over her loan. As a result i will use a low cutoff rule ($\overline{\mu}$ close to L). This is represented in Panel A of Figure A2. 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 lead each bank to apply a stricter cutoff rule. Thus when μ_0 is high information sharing will result in a decrease in the ex-ante probability that a bank rolls over her loan. The same logic applies in reverse when the initial common is low, as represented in Panel B of Figure A2. Here information sharing creates the possibility that good news is shared publicly which would lead each bank to apply a less strict cutoff lower.

Figure A3 formalizes this intuition by showing how each bank's unconditional probability of liquidating their 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 level of lending. Although the model predicts that both are possible our analysis sample is comprised of firms with access to credit, good credit ratings, 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. Identification and Estimation Methods

To identify the impact of public information on credit market outcomes we exploit the time series variation induced by the registry expansion, and the cross sectional variation induced by the preexisting $200,000 eligibility threshold. We expect to observe the impact of public information on the time series of debt levels and other outcomes after the registry expansion for firms that had debt below $200,000 at any time prior to April 1998. However, this effect will be confounded with the potential influence of other contemporaneous aggregate shocks. We construct a counterfactual using firms with total debt above $200,000 prior to April 1998, plausibly unaffected by the policy change. Changes in lending outcomes of firms affected by the expansion relative to those of the control group provide the causal effect of information sharing.

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. To make the two groups of firms comparable in observable characteristics, we restrict the time series analysis to borrowers whose total debt was always between $175,000 and $225,000 before April 1998. Note that this restriction excludes firms that have not obtained credit from the formal financial system at all prior to April 1998, which precludes us from studying the effect of public information on access to credit. We also exclude from the control group all firms with a risk rating higher than 2 in January 1998. Only firms with a risk rating of 1 and 2 were affected by the registry expansion, so this restriction selects control firms with comparable observable expected creditworthiness. The panel descriptive statistics between January 1998 and April 1999 of this subsample are shown in Table 1. 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.

To validate the identification strategy we plot in panel A of Figure 1 the time series of median debt for the firms affected by the expansion and control firms. Both series have pre-April 1998 means and trends removed. Two observations arise from the plot that are consistent with the identification assumption. First, there is no change in the median debt evolution of firms in the control group after the registry expansion. The same is true for the average firm debt concentration, measured as the HHI of a firm's debt across all its lenders (panel B). This suggests that the registry expansion did not affect the credit outcomes of the control group.

This observation rules out some types of self selection of borrowers into the control group that would induce an upward bias in the DD estimates. For example, suppose firms endogenously choose higher levels of total debt to make their credit records public through the registry. These firms in the control group would reduce their total debt after the elimination of the threshold that would be measured as relative increase in total debt in the affected group by the DD estimate.

We verify that self selection of poor quality firms under the threshold to avoid information sharing is also not a concern in this empirical context. Panel A of Figure 2 shows that the density of firms in the *treatment* group, those whose information was not shared in March 1998, does not show a sharp increase to the left of the $200,000 threshold prior to the registry expansion.[13] Also we show in panel B that firms above and below the threshold in March 1998 are similar in observable proxies for credit quality (collateral to debt ratio and fraction with the lowest risk rating of 1). The only observable difference across the two groups is in debt concentration: firms in the control group concentrate their borrowing with fewer lenders. A regression discontinuity analysis in the cross section prior to the registry expansion (not shown) leads to the conclusion that information sharing induces a significant increase in debt concentration, a conclusion that we corroborate later with the DD approach.

The second observation from Figure 1 that is relevant for the identification assumptions is that

[13] Note there are firms with total debt below $200,000 in March 1998 that are assigned to the control group. These firms had total debt above $200,000 either in January or February 1998, and thus had their information shared prior to the registry expansion.

median debt of the two groups of firms, parallel by construction prior to the registry expansion, diverges after April when credit information is made public. The median debt of affected firms drops relative to firms in the control group. Similarly, average debt concentration and default rates of the firms affected by the registry expansion increase relative to the control group after April 1998 (Figure 1, panels B and C). These patterns represent strong evidence that, conditioning on pre-existing differences in means and trends, credit outcomes of the control firms represent a valid counterfactual for lending outcomes of the affected firms. It also represents preliminary evidence that the registry expansion induced a permanent decline in firm total debt and a permanent increase in debt concentration. The cumulative hazard plot indicates that information sharing induces an immediate and short lived increase in the default probability.

The previous evidence provides the rationale for a differences-in-differences estimation based on the following specification:

$$\ln(Debt_{it}) = \alpha_i + \xi_t + \delta_i t + \sum_{m=-2}^{12} \gamma_m . Public\_Apr98_i . I(m = t)_t + \varepsilon_{it} \tag{1}$$

The dependent variable is the (log) debt of firm i at month t. To ease interpretation we label April 1998, the last month prior to information sharing through the registry, as $t = 0$. Thus, March 1998 corresponds to $t = -1$, May 1998 corresponds to $t = 1$, and so on. The right-hand side includes firm fixed effects, calendar month dummies and firm specific time trends. The variable of interest is a dummy equal to one if firm i's credit information becomes public after April 1998 due to the registry expansion ($Public\_Apr98_i$). 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. $Public\_Apr98$ is interacted with a full set of calendar month dummies. The interaction represents the log-debt difference 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, γ_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 (γ_{12}) and the average coefficient between February and April, the pre-expansion period (γ_{pre}).

We can obtain an approximation of the expected magnitude of the DD estimate from Figure 1. The median debt difference across the affected and the control firms drops by \$11,000 between the pre-period and March 1999. Since the average firm has a total debt of $200,000 in the sample this represents a 5.5% decline in debt, which corresponds to a DD estimate of $\gamma_{12} - \gamma_{pre} = 0.055$ in specification (1).

All the results in the next section are reported as differences-in-differences 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 eliminate 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 to choosing a narrower range around $200,000 and alternate definitions of the pre-period.

V. Debt and Default Results

A. Average Debt and Publicity Multiplier

Before focusing on the publicity multiplier, we look at the average (unconditional) effect of information sharing on credit outcomes. Column 1 of Table 2 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 8% to 12% within a year of the registry expansion. The estimates confirm the patterns observed in Figure 1. Publicly sharing information on a firm's debt outstanding and creditworthiness reduce the equilibrium amount of borrowing.

We perform two placebo tests to verify that the sample selection does not mechanically produce the results in Table 2. Specification (1) is estimated assuming that the registry expansion was announced exactly one year after the actual announcement (Appendix Table A1), and assuming that the cut-off rule was applied at $300,000 (Appendix Table A2). The samples were selected using the same criteria than the analysis sample (total debt in a $50,000 window around cutoff during three months prior to announcement, borrowers with rating of 1 or 2). None of the DD estimates is significant in these tests.

To examine whether the decline in debt is transitory, we expand the sample period until April 2000, two years after the registry expansion. Table 3 shows the DD estimates of the effect of public information on debt after 15, 18, 21 and 24 months. The estimates over the full subsample (column 1) indicate that firms affected by the expansion have a total debt that is 7.7% lower than firms in the control group two years after the information in the public registry became publicly available. This suggests that public information had a permanent effect on average firm debt.

Information sharing will in principle improve the assessment that creditors can make about the credit worthiness of each borrower. This mechanism is potentially consistent with the short run decline in lending, if additional information allows lenders to identify the poorest quality borrowers, but it is hard to reconcile with the observed permanent reduction in average credit. For example, an increase in information about credit worthiness would reduce adverse selection or moral hazard (Jaffee and Russell 1976, Stiglitz and Weiss 1981), reduce hold-up by a 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 can provide a rationale for the negative effect of public information on equilibrium debt. We demonstrated that increasing the amount of public information could lower lending on average if the initial common prior about each firm's creditworthiness was high.

We identify the publicity multiplier of information by implementing empirically the arguments laid out in sections II and III. To summarize, we look at whether debt with banks that possess bad news about a firm drops in anticipation of this news becoming public after the registry expansion announcement. We begin by separating firms in two groups according on whether they had a perfect credit record (ratings by all banks equal to 1) or not prior to the registry expansion. The estimated parameters from specification (1) and the DD estimates for each subsample are shown in columns 2 and 3 of Table 2. Firms whose worse rating across all lenders is a 2 at the time of the expansion announcement experience a large and immediate decline in total debt. The DD estimates indicate that average debt declines between 19% and 25% within five months of the registry expansion for these firms. Consistent with the publicity multiplier in our framework, making public bad news about a borrower has a sharp and immediate negative effect on credit in this context.

To isolate the publicity multiplier of bad news we focus on lending by the banks that already had assigned a rating of 2 prior to the registry expansion. For this we exploit the fact that we observe debt at the bank-firm relationship level and repeat the previous estimation over the subsample of firms with less than perfect credit records, but using as the dependent variable the debt of firm i with with the bank (or banks) that assigned the rating of 2 in March 1998.

The DD estimates, reported in column 4 of Table 2, indicate that debt by banks that already possessed the bad news declines between 30% and 40% within 5 months after the registry expansion. More importantly, the decline in lending is significant in May 1998, before the information in the registry is made publicly available. In column 5 we corroborate that these results hold when the firm's other lenders assigned a rating of 1 prior to April, and the expected direct information effect on lending is positive.

These results represent strong evidence that the publicity of bad news can cause a decline in the equilibrium amount of lending. For emphasis, recall that these estimates are obtained from the comparison of firms either side of the $200,000 threshold, all with less than perfect credit records prior to the expansion. Debt with banks that assigned the poorest rating prior to the expansion declines sharply for the firms whose information becomes public, even if these banks received good news from other firm creditors afterwards. Further, the point estimates suggest that lending drops when the registry expansion is announced and before credit information is actually revealed. This indicates that the decline in lending occurs in anticipation of the effect of the publicity of information, and not due to information revealed through the registry itself. These results are consistent with the coordination effect of public information: banks that assigned the poor rating reduce lending in anticipation of the reaction of other lenders to the bad news. The coordination effect is economically significant and has first order consequences on equilibrium lending, since it dominates the potential effect of receiving good news.

The results over the subsample of firms with perfect credit records prior to the expansion show no evidence of a sharp decline in lending (column 2 of Table 2). The DD estimates imply that the total debt of these firms declines between 9% and 11% a year after the registry expansion. This decline in unlikely to be related to the stock of positive information that is revealed at the time of the registry expansion. The finding is consistent with the hypothesis that firms with perfect credit records are made more vulnerable to coordination failures at the arrival of bad public news in the future. Firms affected by the registry expansion are more vulnerable to lender coordination failures than firms in the control group because they borrow from more lenders and have less concentrated debt before the registry expansion. This permanent effect on the equilibrium lending is smaller than the immediate effect of revealing a stock of bad news, but it is economically significant and pertains to the majority of borrowers who have no pre-expansion indications of poor performance on the credit history.

B. Default Hazards

We estimate the effect of public information on defaults by comparing the empirical default hazard rates of the firms affected by the registry expansion and the control firms through a variation of specification (1):[14]

$$1[Default_{ijt} = 1 | Default_{ijt-1} = 0]_{ijt} = \delta_{jt} + \sum_{month=-2}^{12} \lambda_{month} Public\_Apr98_i \times Dum\_month_t + \zeta_{ijt} \quad (2)$$

The left-hand side variable is a dummy equal to zero as long as the relationship between firm i with bank j is in good standing, turns to one if default happens at time t, and drops out of the sample afterwards. The relationship level specification allows us to include bank-month dummies to control for supply side effects.[15] As before, 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 are shown in Table 4 (column 1) and 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 and the cumulative effect are reported next to each coefficient. These indicate that the registry expansion induced a 2.2 percentage point increase in the default hazard during the month after the expansion, and a cumulative 3.7 percentage point increase in defaults over the three months after the expansion. Columns 2 and 3 of Table 4 look at the cross sectional variation according to the worst risk rating of the firm prior to the registry expansion. The DD estimates indicate that the observed average increase in the default hazard occurs solely through firms that had obtained a rating of 2. These firms experience a 13.1 percentage point increase in the default hazard rate during the month after the registry expansion.

The results indicate that the announcement of the registry expansion causes a sharp increase in the default hazard for firms with some indication of poor credit risk in their ratings. This increase occurs in May when the information in the registry is not yet 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 it would have otherwise

[14] We choose this approach because parametric duration models cannot capture the short term and localized nature of the effect of the expansion that appears in the data.

[15] Recent empirical research shows that positive (Paravisini 2008) and negative (Khwaja and Mian 2008) shocks to bank balance sheets in developing countries have significant effects on lending outcomes.

provided, since it anticipates the response by other lenders to the bad news after information in the registry becomes public. We provide evidence in the next section that corroborates this interpretation. The overall findings are consistent with the implications of the publicity multiplier of information.

The estimated cumulative effect increases to 25 percentage points six months after the registry expansion announcement. This suggests that the revelation of the information in the registry itself has an additional effect on firm default probabilities, although this direct effect of information is hard to distinguish from the publicity multiplier in this environment.

VI. Additional Implications of the Publicity Multiplier

In this section we show that additional implications of the publicity multiplier of information are borne out in the data. We assess whether the reduction in lending is coming from liquidations or reduced liquidity injections, and examine if lending arrangements endogenously respond to increased public information. We demonstrate that other possible effects of information sharing are not first order in explaining the observed average effects of the registry expansion.

A. Multiple Lenders

Coordination problems can occur only among borrowers that obtain credit from multiple lenders. We explore whether the measured average effect of the registry expansion is heterogeneous across firms along this dimension. We estimate the debt and default specifications (1) and (2) separately over the subsamples of firms with multiple lenders and a single lender prior to April 1998 (Table 5). Although firms with a single and multiple bank relationships will differ along several and potentially unobserved dimensions, the DD estimation compares firms affected by the expansion with firms in the control group within each subsample of firms. The results show that the average effect of public information estimated over the full sample is driven solely by the decline in debt of firms with multiple lenders prior to the expansion. The DD estimates indicate that total debt of firms with multiple lenders declines by 10.1% to 16.7% nine to twelve months after the registry expansion. There is no significant effect on lending to firms with a single lender prior to the expansion. The default specifications show similar patterns. The sharp increase in the default hazard rate the month after the registry expansion announcement occurs solely for firms with multiple lenders.

These findings are inconsistent with a free-riding interpretation of the observed negative effects of information sharing on average credit outcomes. By mandating information sharing the registry may reduce banks' incentives to collect information in the first place, and creates 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, and potentially more to firms that have a single relationship. The results suggest that reduced information collection incentives are not the main force driving the average effect of public information on debt and defaults. 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)). The publicity of information reduces equilibrium lending significantly only for firms with multiple lenders among which creditor coordination issues may arise.

An alternative channel through which the registry expansion can cause a decline in lending to firms with multiple lenders is by revealing that firms had 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 any information in the registry is revealed. Also, the hidden debt account would predict a debt increase for firms that are revealed to have a sole lender after the registry expansion. By both accounts, the cross sectional heterogeneity of the effect of the registry expansion announcement is inconsistent with hidden debt revelation.

B. Debt Growth Distribution

When lenders have incentives to coordinate, public information can cause large changes in firm debt either because it leads banks to withdraw credit (less likely to roll over loans) or stop providing new funds (less likely to cover firm's interim liquidity needs). Either channel is consistent with the average negative effect of information sharing on lending documented so far. It is possible to distinguish these channels empirically since each has distinct distributional implications not captured by average debt. 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.

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 the purposes of this analysis debt growth is defined as the percentage monthly change in debt between two consecutive months. The bottom rows of Table 6 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 τ for firms affected by the expansion and firms in the control group for every month m, ψ_{τ_m}, where months are labeled as in all previous specifications relative to April 1998. Table 6 presents the estimated ψ_{τ_m} for the 5th, 10th, 50th, 90th, and 95th percentiles, as well their change relative to the pre-April period.[16]

The estimates indicate that there is no systematic change in the 5th or 10th quantiles of the debt growth distribution after the registry expansion (columns 1 and 2). This indicates that sharp declines in lending did not become more likely after the registry expansion. On the other hand, the estimates indicate that there was a sharp decline in the 90th and 95th percentiles (columns 4 and 5). The point estimates indicate that the 95th percentile of debt growth of the affected firms drops 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 information sharing virtually eliminated the likelihood of receiving additional financing during these months. 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 that firms receive additional interim financing in this empirical context. Given that there is no evidence of changes in other quantiles of the debt growth distribution, including the median (column 3), this decline in access to new financing potentially explains the entire decline in average debt, and also provides a rationale for the immediacy of the decline.

[16] 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 ψ_{τ_m} in our application minimize the weighted check functions of the residuals of the following specification:

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

Although a quantile is a non-linear 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.

C. *Within-Firm Lending Correlation*

We have argued that the publicity multiplier is driven by banks using common information to better align their actions with others. We test whether the contemporaneous lending decisions across banks to the same firm become more positively correlated after the registry expansion, a direct implication of enhanced coordination. The differences-in-differences estimate of the effect on within-firm changes in debt correlation is given by the following relationship level specification:

$$\begin{aligned} \ln\left(Debt_{ijt}\right) \quad = \quad & \alpha_{ij} + \delta_t + \tau_i t + \sum_{m=2}^{16} \beta_{1\_m} \ln\left(TDebt_{i(-j)t}\right) \times Dum\_m_t + \\ & \sum_{m=-2}^{12} \beta_{2\_m} \ln\left(TDebt_{i(-j)t}\right) \times Public\_Apr98_i \times Dum\_m_t + \omega_{ijt} \end{aligned} \tag{3}$$

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, β_{1_m}, measure the contemporaneous correlation of debt across the lenders of the same firm in month m. The coefficient on the interaction with $Public\_Apr98$, β_{2_m}, measures the difference in this correlation between firms affected by the registry expansion and the control group. As before, the 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 allowing 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.

Prior to the announcement of the registry expansion there is no time series change in the estimated coefficients, shown in Table 7, which validates the identification assumptions. The DD point estimates indicate that the lending correlation across different banks increases on average by 16.1 percentage points during the three months following the registry expansion (18.7 when debt by other banks is lagged one month (column 2)). This represents a tenfold increase of the average lending correlation across banks in the entire sample (1.56%). The increase in correlation is short lived and begins two months after the expansion announcement, when the information in the registry becomes public.

The timing of the correlation increase corroborates the earlier analysis in two ways. First, the fact that there is no significant change in the first two months after information sharing was announced confirms that little information was actually shared in this interim period. This is the period during which banks are changing their actions in anticipation of the response from other banks. The heightened correlation in July confirms that banks were correct in anticipating that information they shared would induce a coordination driven response from other banks.

D. Debt Concentration

Our findings show that the incentive to coordinate leads banks to respond more to public information. As a result of the registry expansion, lenders become more sensitive to bad news, firms are less likely to receive interim liquidity injections, and become more likely to default. In theory, firms can avoid the consequences of lender coordination problems by concentrating their 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.

To explore whether the registry expansion affects debt concentration across lenders in a way consistent with this interpretation, 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 prior to April are shown in Table 8. 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 prior to the registry expansion (panel B of Figure 2). Both findings indicate that firms respond endogenously to the increased coordination induced by the publicity of information by concentrating debt from fewer banks.

VII. Conclusion

We provide evidence of the publicity multiplier of information among creditors who have an incentive to coordinate their actions. We demonstrate this by exploiting 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 the private information it possesses will be shared with a firm's other creditors. The effect of making information common knowledge is identified by comparing firms who were affected by the expansion (total lending between \$175,000 and \$200,000) with comparable firms who were not affected by the change (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 even though the bank has not learned anything else about the firm. 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.

We find that the structure of lending arrangements endogenously reacts to information sharing whereby firms concentrate their lending across fewer banks. This suggests that limiting coordination failures is a first order force in the trade-off firms face when choosing how many creditors to borrow from. This paper shows that this 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.

A Appendix

We briefly characterize the equilibrium strategies that each bank will apply at $t = 2$ for an entrepreneur who borrows from two banks. The basic solution method and existence results are directly analogous to the two player game studied in Morris and Shin (2002) which establishes that each agent will employ a simple cut-off strategy when choosing their 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 θ is distributed $N(\mu^{com}, (\tau^{com})^{-1})$. Let μ_i^{post} denote bank i's expected value of θ after receiving all information and let τ^{priv} denote the precision of any private information that each bank receives. Let $\overline{\mu}$ denote the equilibrium cut-off 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 χ_j^{priv} is the private signal that j receives. Since i does not observe χ_j^{priv} this forms the basis for i's uncertainty about j's posterior belief. Since χ_j^{priv} is an unbiased estimate of θ, i's expectation of χ_j^{priv} is μ_i^{post}. Accordingly, bank i's expectation of bank j's posterior belief is

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

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

$$\frac{\tau^{com}\mu^{com}}{\tau^{com}+\tau^{priv}}+\frac{\tau^{priv}}{\tau^{com}+\tau^{priv}}\left(\theta+e_j^{\chi}\right)$$

where e_j^{χ} 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 θ and e_j^{χ}. 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 rollover her loan if the expected payoff is at least as large as L, i.e. if and only if

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

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

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

where Φ is the cumulative density of the standard normal distribution. Bank i will optimally choose to rollover if and only if

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

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

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

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

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

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

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

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

$$\begin{aligned} \frac{\partial\overline{\mu}}{\partial\mu^{com}} &= \frac{-K\Omega\phi\left(\Omega\left(\overline{\mu} - \mu^{com}\right)\right)}{1 - K\Omega\phi\left(\Omega\left(\overline{\mu} - \mu^{com}\right)\right)} < 0 \\ \text{where } \Omega &\equiv \frac{\tau^{com}}{\tau^{priv}\sqrt{\left(\tau^{com} + \tau^{priv}\right)^{-1} + \left(\tau^{priv}\right)^{-1}}} > 0 \end{aligned} \tag{6}$$

and $\phi(\cdot) > 0$ is the density function of the standard normal. Note that the sign of $\frac{\partial\overline{\mu}}{\partial\mu^{com}}$ is ensured to be negative since, by construction, the uniqueness condition (5) guarantees that $1 - K\Omega\phi\left(\Omega\left(\overline{\mu} - \mu^{com}\right)\right) >$

0. Using this we have that with information sharing:

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

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

The results we discuss in the paper are drawn using 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.

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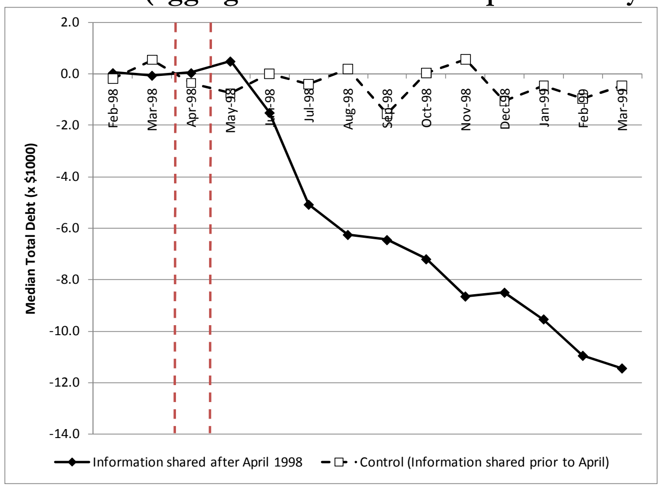
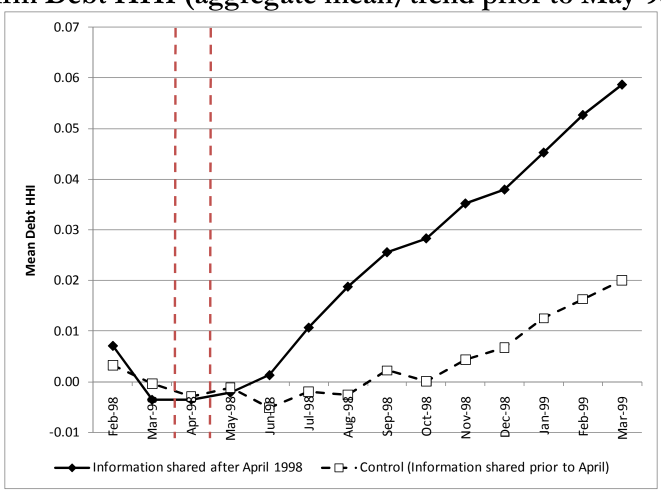
Figure 1
Median Debt, Average Debt Concentration and Default Hazard by Month, for Firms Affected by the Registry Expansion and Control Firms

Panel A. Median Debt (aggregate mean and trend prior to May-98 removed)



Panel B. Firm Debt HHI (aggregate mean/trend prior to May-98 removed)



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

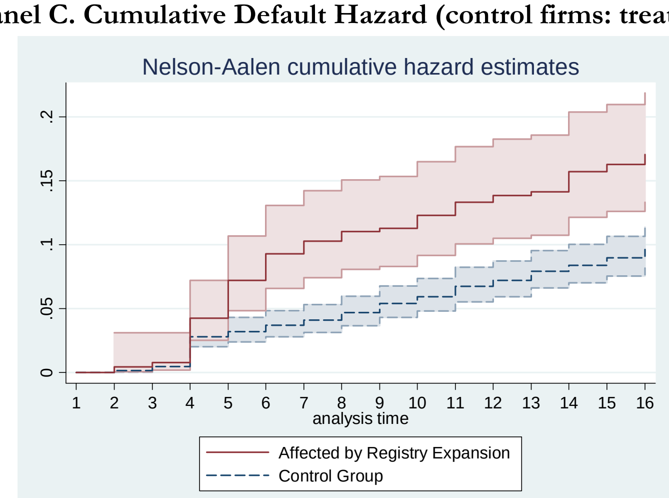
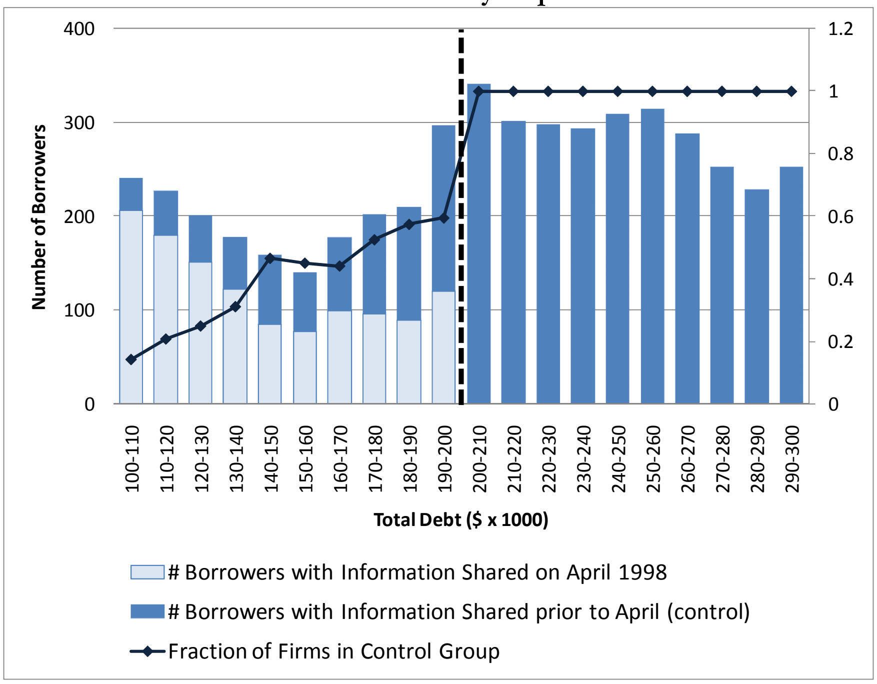
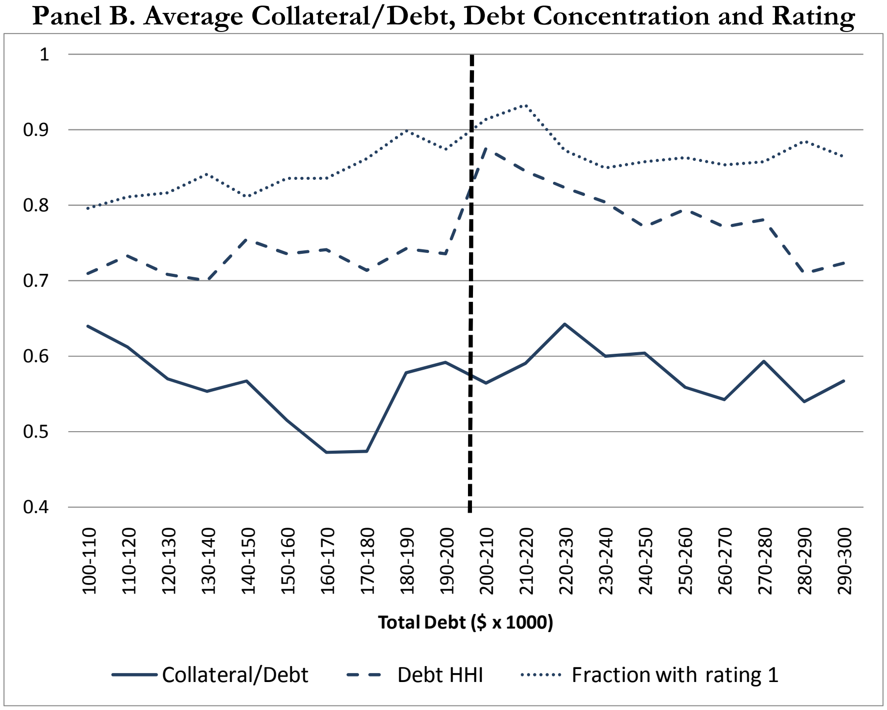


Figure 2
Borrower Distribution and Characteristics by Total Debt in March 1998

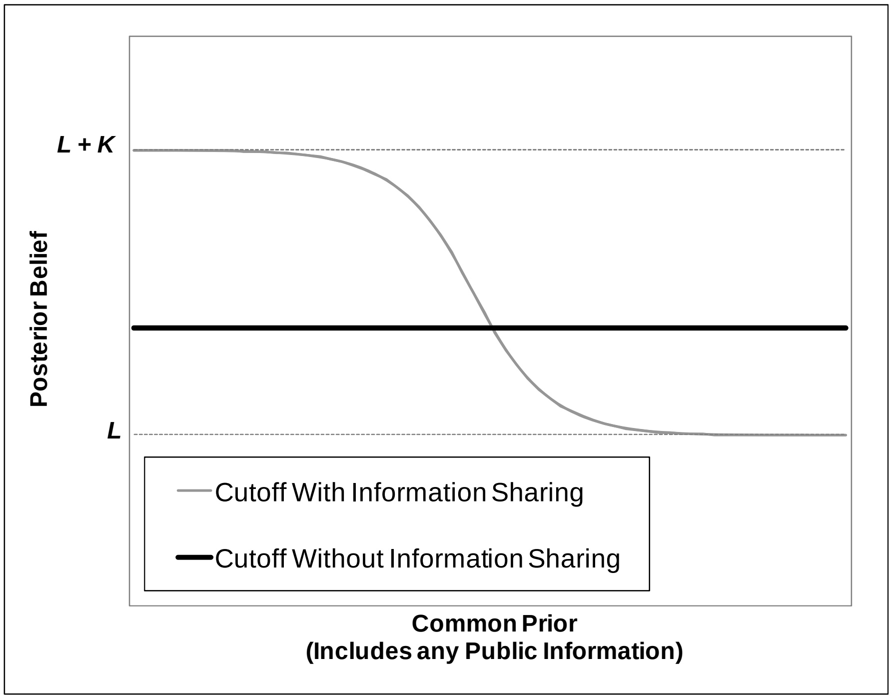
Panel A. Number of Borrowers affected by Expansion and in Control Group



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

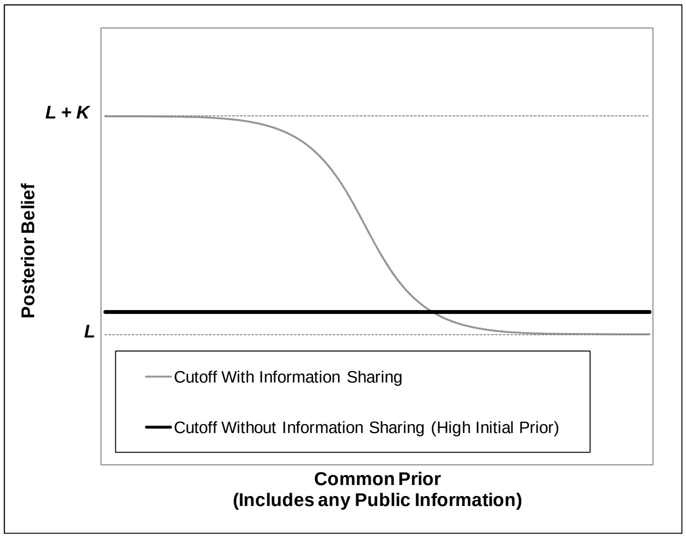


Appendix Figure A1
Bank Cut-off Strategies and Information Sharing

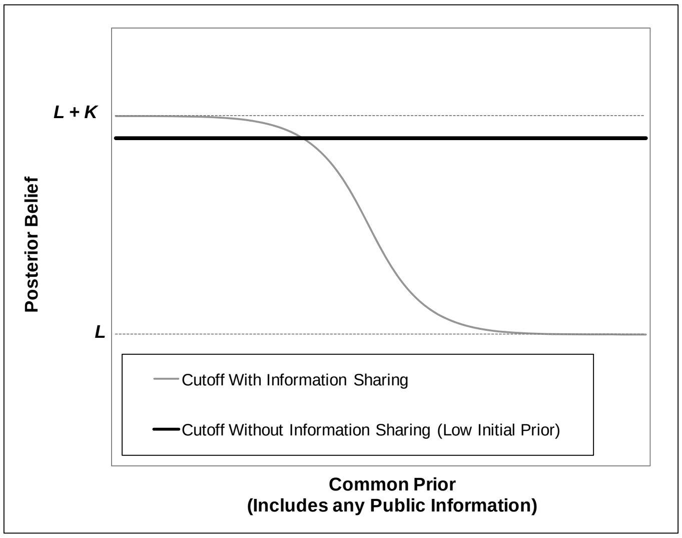


Appendix Figure A2
Bank Cut-off Strategies and the Initial Common Prior

Panel A
High Initial Common Prior



Panel B
Low Initial Common Prior



Appendix Figure A3
Information Sharing and the Ex-ante Probability that a Loan is Liquidated
(Information sharing minus no information sharing)

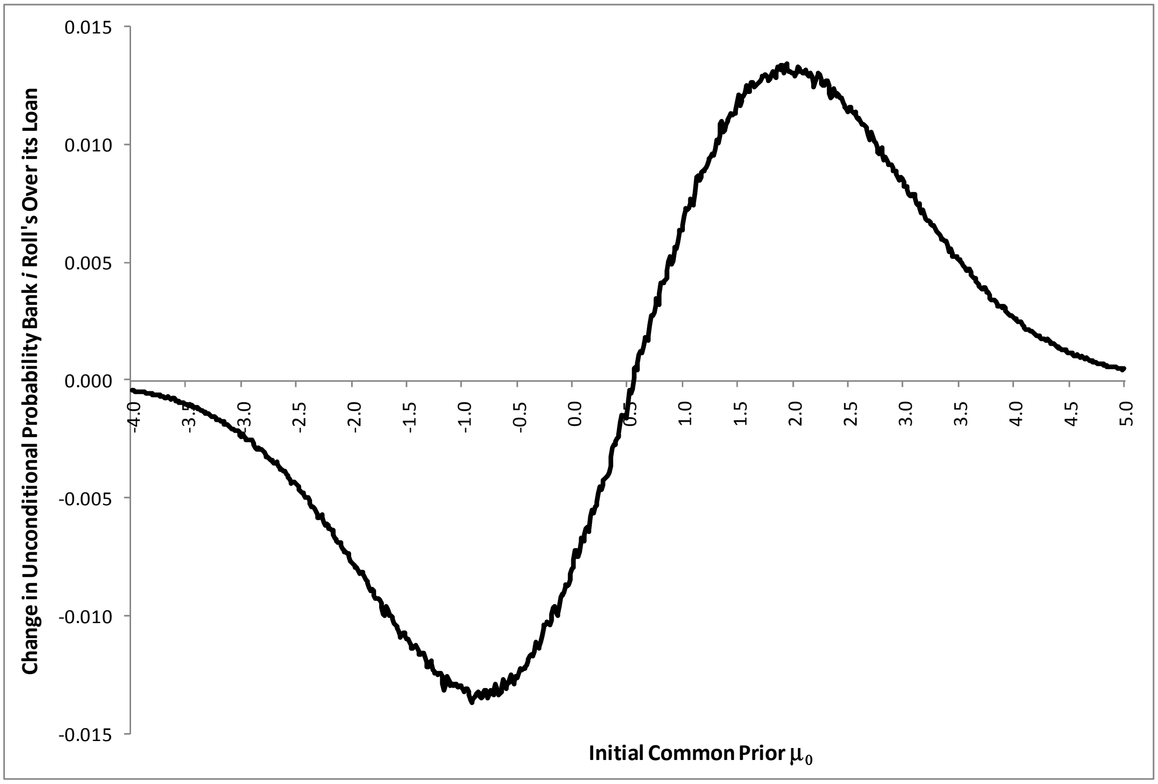


Table 1
Panel Descriptive Statistics, January 1998 to April 1999
Firms with total debt between $175,000 and $225,000 and risk ratings of 1 and 2 before April 1998 (1,006 firms)

Variable	**mean**	**sd**	**p50**	**min**	**max**	**N**
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
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

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

Table 2
Effect of Public Information on (log) Debt, Difference-in-Difference Estimation

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. Estimates are obtained after first differencing, and include firms fixed effects and month dummies. The reported coefficients represent the monthly (log) debt of firms with total debt below $200,000 prior to April, relative to firms with total debt above $200,000 (control). Robust standard errors clustered at the borrower level. Difference-in-difference (DD) estimates are obtained by subtracting to each coefficient γ_m the average coefficients in the pre-expansion period, γ_{-2}, γ_{-1} , and γ_0 (February through April 1998). Statistical significance of 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.

Dependent Variable	$\ln(Debt_{it})$						$\ln(\text{Debt by Banks w/ Rating} = 2_{it})$			
Sub-Sample: Max Risk Rating Prior to April	1 or 2		1		2		2		2 (at least one 1)	
	(1)	DD (γ_m-γ_{Pre})	(2)	DD (γ_m-γ_{Pre})	(3)	DD (γ_m-γ_{Pre})	(4)	DD (γ_m-γ_{Pre})	(5)	DD (γ_m-γ_{Pre})
Information Public after Apr-98	0.069		0.086		-0.011		-0.05		0.013	
× Dum_1998_02 (γ_{-2})	(0.022)		(0.026)		(0.038)		(0.102)		(0.162)	
Information Public after Apr-98	0.046		0.056		-0.003		-0.117		-0.078	
× Dum_1998_03 (γ_{-1})	(0.028)		(0.033)		(0.045)		(0.129)		(0.207)	
Information Public after Apr-98	0.099		0.116		-0.006		-0.274		-0.276	
× Dum_1998_04 (γ_0)	(0.035)		(0.042)		(0.060)		(0.133)		(0.207)	
Information Public after Apr-98	0.091	0.020	0.107	0.021	0.014	0.020	-0.302	-0.155**	-0.34	-0.226**
× Dum_1998_05 (γ_1)	(0.041)	(0.024)	(0.048)	(0.026)	(0.077)	(0.061)	(0.133)	(0.069)	(0.205)	(0.096)
Information Public after Apr-98	0.078	0.006	0.102	0.016	-0.053	-0.047	-0.441	-0.294*	-0.502	-0.388*
× Dum_1998_06 (γ_2)	(0.049)	(0.036)	(0.054)	(0.036)	(0.119)	(0.111)	(0.207)	(0.174)	(0.285)	(0.231)
Information Public after Apr-98	0.046	-0.025	0.093	0.007	-0.202	-0.196*	-0.563	-0.416**	-0.601	-0.487**
× Dum_1998_07 (γ_3)	(0.051)	(0.041)	(0.056)	(0.040)	(0.124)	(0.118)	(0.214)	(0.185)	(0.286)	(0.241)
Information Public after Apr-98	0.002	-0.070	0.053	-0.033	-0.26	-0.254**	-0.575	-0.428**	-0.642	-0.528**
× Dum_1998_08 (γ_4)	(0.054)	(0.047)	(0.058)	(0.048)	(0.130)	(0.127)	(0.213)	(0.190)	(0.292)	(0.257)
Information Public after Apr-98	0.086	0.014	0.139	0.053	-0.236	-0.230	-0.398	-0.251*	-0.417	-0.303
× Dum_1998_09 (γ_5)	(0.051)	(0.046)	(0.058)	(0.050)	(0.102)	(0.097)	(0.156)	(0.134)	(0.224)	(0.188)
Information Public after Apr-98	0.049	-0.023	0.087	0.001	-0.152	-0.146**	-0.303	-0.155	-0.422	-0.309
× Dum_1998_10 (γ_6)	(0.054)	(0.050)	(0.062)	(0.055)	(0.107)	(0.112)	(0.149)	(0.144)	(0.215)	(0.189)
Information Public after Apr-98	0.015	-0.056	0.053	-0.033	-0.173	-0.167	-0.268	-0.120	-0.37	-0.256
× Dum_1998_11 (γ_7)	(0.053)	(0.048)	(0.062)	(0.054)	(0.099)	(0.102)	(0.136)	(0.132)	(0.207)	(0.184)
Information Public after Apr-98	0.032	-0.040	0.073	-0.013	-0.201	-0.195**	-0.288	-0.141	-0.405	-0.291
× Dum_1998_12 (γ_8)	(0.053)	(0.048)	(0.063)	(0.055)	(0.094)	(0.097)	(0.131)	(0.129)	(0.207)	(0.194)
Information Public after Apr-98	-0.027	-0.099**	-0.002	-0.088*	-0.182	-0.175	-0.244	-0.097	-0.402	-0.288
× Dum_1999_01 (γ_9)	(0.043)	(0.045)	(0.050)	(0.053)	(0.085)	(0.087)	(0.122)	(0.128)	(0.201)	(0.198)
Information Public after Apr-98	-0.033	-0.104**	-0.013	-0.099*	-0.17	-0.163**	-0.162	-0.015	-0.333	-0.220
× Dum_1999_02 (γ_{10})	(0.041)	(0.044)	(0.048)	(0.052)	(0.081)	(0.081)	(0.121)	(0.131)	(0.204)	(0.214)
Information Public after Apr-98	-0.049	-0.120***	-0.032	-0.118***	-0.19	-0.183**	-0.175	-0.028	-0.338	-0.224
× Dum_1999_03 (γ_{11})	(0.029)	(0.039)	(0.033)	(0.046)	(0.079)	(0.079)	(0.120)	(0.132)	(0.205)	(0.224)
Information Public after Apr-98	-0.009	-0.081**	0.003	-0.083**	-0.088	-0.081**	-0.191	-0.043	-0.306	-0.192
× Dum_1999_04 (γ_{12})	(0.023)	(0.034)	(0.026)	(0.040)	(0.062)	(0.069)	(0.114)	(0.136)	(0.202)	(0.231)
First Differenced Estimation	Yes		Yes		Yes		Yes		Yes	
Firm Fixed Effecs	Yes		Yes		Yes		Yes		Yes	
Firm Specific Trends	Yes		Yes		Yes		Yes		Yes	
Month Dummies	Yes		Yes		Yes		Yes		Yes	
Observations (Firm-Month)	16,859		15,205		1,654		1,585		971	
Clusters (Firms)	1,006		911		95		94		79	
Adjusted R-squared	0.01		0.01		0.02		0.05		0.11	

Table 3
Long Run Effect of Public Information on (log) Debt, Difference-in-Difference Estimation

Sample: Firms with total debt between $175,000 and $225,000 before April 1998. Reports the difference-in-difference (DD) estimates and standard errors of the effect of public information on total debt. Based on specification (1) estimated over the extended sample period from January 1998 to April 2000, with robust standard errors clustered at the borrower level. 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.

Dependent Variable	$\ln(Debt_{it})$		
Max Risk Rating prior to April	1 or 2	1	2
	(1)	(2)	(3)
DD estimate: effect after 15 months (γ_{15} - $\gamma_{Feb\text{-}Apr\ Avg}$)	-0.129 **	-0.122 **	-0.072
	(0.059)	(0.122)	(0.102)
DD estimate: effect after 18 months (γ_{18} - $\gamma_{Feb\text{-}Apr\ Avg}$)	-0.092 *	-0.088 *	0.017
	(0.051)	(0.059)	(0.080)
DD estimate: effect after 21 months (γ_{21} - $\gamma_{Feb\text{-}Apr\ Avg}$)	-0.057	-0.054	-0.015
	(0.043)	(0.050)	(0.064)
DD estimate: effect after 24 months (γ_{24} - $\gamma_{Feb\text{-}Apr\ Avg}$)	-0.077 *	-0.108 **	0.070
	(0.044)	(0.054)	(0.055)
First Differenced Estimation	Yes	Yes	Yes
Firm Fixed Effecs	Yes	Yes	Yes
Firm Specific Trends	Yes	Yes	Yes
Month Dummies	Yes	Yes	Yes
Observations (Firm-Month)	26,394	23,759	2,635
Clusters (Firms)	1,006	911	95
Adjusted R-squared	0.01	0.01	0.00

Table 4

Effect of Information Sharing on Default Hazard Rate

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 prior to April 1998 equal to 1 and 2. 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-difference (DD) estimates are obtained by subtracting to each coefficient β_m the average coefficients in the pre-expansion period, β_{-2}, β_{-1} , and β_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.

Dependent Variable	1 if relationship in default at t, not in default at t-1								
Sub-Sample: Max Risk Rating Prior to April	1 or 2			1			2		
	(1)	DD (β_m-β_{Pre})	Cummul. Effect	(2)	DD (β_m-β_{Pre})	Cummul. Effect	(3)	DD (β_m-β_{Pre})	Cummul. Effect
Information Public after Apr-98	0.002			0.003			0		
× Dum_1998_02 (β_{-2})	(0.005)			(0.006)			0.000		
Information Public after Apr-98	-0.002			-0.003			0.017		
× Dum_1998_03 (β_{-1})	(0.003)			(0.001)			(0.016)		
Information Public after Apr-98	0.013			0.018			-0.040		
× Dum_1998_04 (β_0)	(0.010)			(0.011)			(0.033)		
Information Public after Apr-98	0.026	0.022*		-0.001	-0.007		0.123	0.131**	
× Dum_1998_05 (β_1)	(0.012)	(0.013)		(0.004)	(0.006)		(0.049)	(0.053)	
Information Public after Apr-98	0.019	0.014	0.036*	0.019	0.013	0.007	0.002	0.01	0.140**
× Dum_1998_06 (β_2)	(0.012)	(0.011)	(0.021)	(0.015)	(0.013)	(0.016)	(0.002)	(0.013)	(0.059)
Information Public after Apr-98	0.006	0.001	0.037*	0.002	-0.004	0.003	0.02	0.028	0.168**
× Dum_1998_07 (β_3)	(0.006)	(0.006)	(0.022)	(0.005)	(0.005)	(0.016)	(0.018)	(0.023)	(0.072)
Information Public after Apr-98	0.004	-0.001	0.037	0.000	-0.006	-0.003	0.026	0.034	0.202**
× Dum_1998_08 (β_4)	(0.006)	(0.008)	(0.025)	(0.004)	(0.007)	(0.019)	(0.039)	(0.045)	(0.103)
Information Public after Apr-98	-0.005	-0.009	0.028	-0.002	-0.008	-0.011	0.000	0.008	0.210*
× Dum_1998_09 (β_5)	(0.004)	(0.006)	(0.027)	(0.004)	(0.007)	(0.022)	(0.000)	(0.014)	(0.113)
Information Public after Apr-98	0.002	-0.002	0.026	-0.002	-0.007	-0.019	0.034	0.042	0.251*
× Dum_1998_10 (β_6)	(0.007)	(0.009)	(0.033)	(0.005)	(0.007)	(0.026)	(0.029)	(0.037)	(0.141)
Information Public after Apr-98	-0.001	-0.006	0.02	-0.005	-0.011*	-0.029	-0.003	0.005	0.256*
× Dum_1998_11 (β_7)	(0.005)	(0.007)	(0.037)	(0.002)	(0.006)	(0.031)	(0.038)	(0.034)	(0.141)
Information Public after Apr-98	0.000	-0.004	0.016	0.002	-0.004	-0.033	-0.016	-0.009	0.247*
× Dum_1998_12 (β_8)	(0.004)	(0.006)	(0.042)	0.002	(0.008)	(0.037)	(0.020)	(0.024)	(0.151)
Information Public after Apr-98	-0.007	-0.012*	0.004	-0.009	-0.015***	-0.047	0.018	0.025	0.273
× Dum_1999_01 (β_9)	(0.004)	(0.006)	(0.046)	(0.003)	(0.005)	(0.041)	(0.027)	(0.030)	(0.167)
Information Public after Apr-98	0.009	0.005	0.009	0.001	-0.005	-0.052	0.039	0.047*	0.320*
× Dum_1999_02 (β_{10})	(0.008)	(0.010)	(0.053)	(0.006)	(0.009)	(0.048)	(0.028)	(0.025)	(0.176)
Information Public after Apr-98	-0.001	-0.005	0.003	-0.003	-0.009*	-0.061	-0.021	-0.013	0.306
× Dum_1999_03 (β_{11})	(0.004)	(0.004)	(0.054)	(0.001)	(0.005)	(0.053)	(0.032)	(0.040)	(0.197)
Information Public after Apr-98	0.001	-0.003	0.000	0.002	-0.004	-0.066	0.000	0.008	0.314
× Dum_1999_04 (β_{12})	(0.004)	(0.005)	(0.057)	(0.005)	(0.006)	(0.056)	(0.000)	(0.014)	(0.209)
Relationship in sample after default	No			No			No		
Bank x month dummies	Yes			Yes			Yes		
Clusters (Banks)	99			98			51		
Observations (Firm-Bank-Month)	34,878			31,521			3,357		
Adjusted R-squared	0.06			0.07			0.25		

Table 5
Effect of Public Information on (log) Debt and Default Hazard Rates, Cross Section Heterogeneity by Number of Lenders

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 the relationship of firm i with bank j 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 reported coefficients represent the difference in log debt or hazard rate between the firms affected by registry expansion and control firms. Robust standard errors clustered at the borrower level. Difference-in-difference (DD) estimates are obtained by subtracting to 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.

Dependent Variable	$\ln(Debt_{it})$				1 if relationship in default at t, not in default at t-1			
Sub-Sample: by # lenders prior to April	Multiple Lenders		Single Lender		Multiple Lenders		Single Lender	
	(1)	DD (γ_m-γ_{Pre})	(2)	DD (γ_m-γ_{Pre})	(3)	DD (β_m-β_{Pre})	(4)	DD (β_m-β_{Pre})
Information Public after Apr-98	0.092		0.017		-0.004		0.022	
× Dum_1998_02 (γ_{-2})	(0.032)		(0.025)		(0.002)		(0.016)	
Information Public after Apr-98	0.059		0.011		-0.002		0.014	
× Dum_1998_03 (γ_{-1})	(0.039)		(0.037)		(0.002)		(0.018)	
Information Public after Apr-98	0.129		0.063		0.018		-0.02	
× Dum_1998_04 (γ_0)	(0.046)		(0.058)		(0.012)		(0.004)	
Information Public after Apr-98	0.09	-0.004	0.084	0.054	0.032	0.028**	0.009	0.003
× Dum_1998_05 (γ_1)	(0.050)	(0.028)	(0.068)	(0.042)	(0.014)	(0.014)	(0.018)	(0.016)
Information Public after Apr-98	0.104	0.01	0.057	0.027	0.019	0.015	0.018	0.012
× Dum_1998_06 (γ_2)	(0.062)	(0.048)	(0.085)	(0.060)	(0.016)	(0.014)	(0.014)	(0.015)
Information Public after Apr-98	0.054	-0.04	0.05	0.02	-0.004	-0.008	0.045	0.039
× Dum_1998_07 (γ_3)	(0.064)	(0.053)	(0.091)	(0.065)	(0.004)	(0.006)	(0.025)	(0.025)
Information Public after Apr-98	0.012	-0.081	-0.007	-0.037	-0.002	-0.007	0.035	0.03
× Dum_1998_08 (γ_4)	(0.069)	(0.063)	(0.084)	(0.067)	(0.004)	(0.006)	(0.033)	(0.033)
Information Public after Apr-98	0.089	-0.004	0.078	0.048	-0.001	-0.005	-0.015	-0.021***
× Dum_1998_09 (γ_5)	(0.068)	(0.062)	(0.084)	(0.068)	(0.004)	(0.007)	(0.006)	(0.007)
Information Public after Apr-98	0.033	-0.06	0.076	0.045	-0.001	-0.005	0.024	0.018
× Dum_1998_10 (γ_6)	(0.070)	(0.066)	(0.084)	(0.067)	(0.007)	(0.008)	(0.022)	(0.022)
Information Public after Apr-98	0	-0.093	0.042	0.012	-0.007	-0.011**	0.019	0.014
× Dum_1998_11 (γ_7)	(0.067)	(0.062)	(0.085)	(0.067)	(0.003)	(0.006)	(0.022)	(0.022)
Information Public after Apr-98	0.032	-0.061	0.035	0.005	0	-0.004	0.003	-0.003
× Dum_1998_12 (γ_8)	(0.067)	(0.064)	(0.088)	(0.070)	(0.002)	(0.006)	(0.023)	(0.024)
Information Public after Apr-98	-0.05	-0.144**	0.025	-0.005	-0.011	-0.015**	-0.004	-0.009***
× Dum_1999_01 (γ_9)	(0.050)	(0.061)	(0.084)	(0.066)	(0.006)	(0.007)	(0.003)	(0.002)
Information Public after Apr-98	-0.074	-0.168***	0.045	0.015	0.005	0.001	0.033	0.027
× Dum_1999_02 (γ_{10})	(0.048)	(0.059)	(0.079)	(0.063)	(0.007)	(0.010)	(0.027)	(0.026)
Information Public after Apr-98	-0.059	-0.153***	-0.031	-0.061	0	-0.004	-0.014	-0.020***
× Dum_1999_03 (γ_{11})	(0.041)	(0.054)	(0.037)	(0.051)	(0.005)	(0.005)	(0.004)	(0.005)
Information Public after Apr-98	-0.008	-0.101**	-0.002	-0.032	0.003	-0.001	0.004	-0.002
× Dum_1999_04 (γ_{12})	(0.029)	(0.048)	(0.033)	(0.040)	(0.005)	(0.007)	(0.012)	(0.013)
First Differenced Estimation	Yes		Yes					
Firm Fixed Effects	Yes		Yes					
Firm Specific Trends	Yes		Yes					
Month Dummies	Yes		Yes					
Relationship in sample after default					No		No	
Bank x month dummies					Yes		Yes	
Observations (Firm-Month)	8,686		8,173					
Clusters (Firms)	505		501					
Observations (Firm-Bank-Month)					23,839		11,039	
Clusters (Banks)					93		72	
Adjusted R-squared	0.02		0.00		0.07		0.12	

Table 6
Effect of Information Sharing 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.

Dependent Variable	$(Debt_{it} - Debt_{it-1}) / Debt_{it-1}$									
Debt Growth Quantile	5%		10%		50%		90%		95%	
	(1)	$(\psi_m-\psi_{Pre})$	(2)	$(\psi_m-\psi_{Pre})$	(3)	$(\psi_m-\psi_{Pre})$	(4)	$(\psi_m-\psi_{Pre})$	(5)	$(\psi_m-\psi_{Pre})$
Information Public after Apr-98	0.033		-0.002		0.011		0.312		0.327	
× Dum_1998_02 (γ_{-2})	(0.040)		(0.022)		(0.010)		(0.043)		(0.045)	
Information Public after Apr-98	-0.088		-0.086		-0.016		0.107		0.102	
× Dum_1998_03 (γ_{-1})	(0.043)		(0.030)		(0.010)		(0.054)		(0.057)	
Information Public after Apr-98	0.011		0.013		0.006		0.138		0.324	
× Dum_1998_04 (γ_0)	(0.091)		(0.043)		(0.006)		(0.121)		(0.210)	
Information Public after Apr-98	0.025	0.040	-0.016	0.009	0.004	0.004	-0.015	-0.200***	-0.148	-0.399**
× Dum_1998_05 (γ_1)	(0.115)	(0.120)	(0.024)	(0.031)	(0.005)	(0.007)	(0.054)	(0.072)	(0.139)	(0.160)
Information Public after Apr-98	-0.039	-0.025	-0.002	0.023	0.007	0.007	0.009	-0.177***	-0.066	-0.317***
× Dum_1998_06 (γ_2)	(0.126)	(0.130)	(0.077)	(0.080)	(0.004)	(0.006)	(0.040)	(0.062)	(0.068)	(0.101)
Information Public after Apr-98	-0.097	-0.082	-0.046	-0.021	-0.004	-0.004	0.011	-0.175***	-0.060	-0.311**
× Dum_1998_07 (γ_3)	(0.143)	(0.149)	(0.063)	(0.067)	(0.007)	(0.008)	(0.039)	(0.060)	(0.107)	(0.126)
Information Public after Apr-98	-0.166	-0.152	-0.128	-0.103	-0.005	-0.006	0.005	.-0.181***	0.001	-0.250**
× Dum_1998_08 (γ_4)	(0.110)	(0.117)	(0.066)	(0.067)	(0.007)	(0.009)	(0.046)	(0.066)	(0.072)	(0.105)
Information Public after Apr-98	0.021	0.035	0.039	0.064	0.015	0.014	0.118	-0.067	0.073	-0.178
× Dum_1998_09 (γ_5)	(0.132)	(0.137)	(0.054)	(0.057)	(0.005)	(0.009)	(0.049)	(0.068)	(0.368)	(0.377)
Information Public after Apr-98	-0.013	0.001	-0.017	0.008	0.004	0.004	-0.074	-0.260***	-0.111	-0.362***
× Dum_1998_10 (γ_6)	(0.068)	(0.080)	(0.040)	(0.043)	(0.005)	(0.007)	(0.052)	(0.071)	(0.089)	(0.123)
Information Public after Apr-98	0.017	0.031	-0.013	0.012	0.003	0.003	-0.039	-0.224***	-0.050	-0.301**
× Dum_1998_11 (γ_7)	(0.117)	(0.124)	(0.052)	(0.055)	(0.006)	(0.008)	(0.058)	(0.074)	(0.131)	(0.153)
Information Public after Apr-98	0.138	0.152*	0.037	0.062**	0.008	0.007	0.037	-0.149**	-0.05	-0.301
× Dum_1998_12 (γ_8)	(0.072)	(0.080)	(0.023)	(0.029)	(0.006)	(0.008)	(0.037)	(0.059)	(0.074)	(0.103)
Information Public after Apr-98	0.038	0.052	0.006	0.031	0.000	0.000	-0.001	-0.186***	-0.117	-0.368***
× Dum_1999_01 (γ_9)	(0.070)	(0.077)	(0.024)	(0.031)	(0.006)	(0.007)	(0.039)	(0.060)	(0.089)	(0.114)
Information Public after Apr-98	0.014	0.029	-0.015	0.011	-0.002	-0.003	-0.031	-0.216***	-0.028	-0.279***
× Dum_1999_02 (γ_{10})	(0.055)	(0.069)	(0.032)	(0.038)	(0.005)	(0.007)	(0.029)	(0.057)	(0.061)	(0.094)
Information Public after Apr-98	0.006	0.021	0.005	0.030	0.000	-0.001	0.007	-0.178***	-0.03	-0.281
× Dum_1999_03 (γ_{11})	(0.046)	(0.058)	(0.035)	(0.040)	(0.005)	(0.007)	(0.032)	(0.056)	(0.208)	(0.224)
Information Public after Apr-98	0.068	0.082	0.044	0.069**	0.007	0.006	0.018	-0.168***	0.018	-0.233
× Dum_1999_04 (γ_{12})	(0.044)	(0.058)	(0.025)	(0.032)	(0.005)	(0.007)	(0.038)	(0.060)	(0.163)	(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 7
Effect of Information Sharing on Lending Coordination

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 (4) in the paper (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-difference (DD) estimates are obtained by subtracting to each coefficient γ_i the average coefficients in the pre-expansion period, γ_{-2}, γ_{-1}, and γ_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.

Dependent Variable	ln(Total Debt with Banks other than j_{ijt})		ln(Total Debt with Banks other than j_{ijt+1})	
	(1)	DD (γ_m-γ_{Pre})	(2)	DD (γ_m-γ_{Pre})
Debtijt × Information Public after Apr-98	0.002		-0.001	
× Dum_1998_02 (γ_{-2})	(0.003)		(0.01)	
Debtijt × Information Public after Apr-98	0.005		0.005	
× Dum_1998_03 (γ_{-1})	(0.003)		(0.00)	
Debtijt × Information Public after Apr-98	0.007		-0.004	
× Dum_1998_04 (γ_0)	(0.027)		(0.04)	
Debtijt × Information Public after Apr-98	0.006	0.002	0.008	0.008
× Dum_1998_05 (γ_1)	(0.012)	(0.014)	(0.01)	(0.016)
Debtijt × Information Public after Apr-98	0.109	0.104	0.072	0.072
× Dum_1998_06 (γ_2)	(0.052)	(0.099)	(0.09)	(0.084)
Debtijt × Information Public after Apr-98	0.166	0.162***	0.188	0.187***
× Dum_1998_07 (γ_3)	(0.063)	(0.062)	(0.07)	(0.073)
Debtijt × Information Public after Apr-98	0.025	0.02	0.059	0.059
× Dum_1998_08 (γ_4)	(0.058)	(0.059)	(0.05)	(0.052)
Debtijt × Information Public after Apr-98	-0.035	-0.039	-0.005	-0.005
× Dum_1998_09 (γ_5)	(0.048)	(0.049)	(0.05)	(0.050)
Debtijt × Information Public after Apr-98	-0.011	-0.016	-0.02	-0.02
× Dum_1998_10 (γ_6)	(0.019)	(0.021)	(0.03)	(0.031)
Debtijt × Information Public after Apr-98	0.01	0.006	0.034	0.033
× Dum_1998_11 (γ_7)	(0.038)	(0.039)	(0.07)	(0.071)
Debtijt × Information Public after Apr-98	-0.025	-0.029	0.000	0.000
× Dum_1998_12 (γ_8)	(0.037)	(0.037)	(0.04)	(0.040)
Debtijt × Information Public after Apr-98	-0.029	-0.033	-0.028	-0.028
× Dum_1999_01 (γ_9)	(0.030)	(0.031)	(0.03)	(0.031)
Debtijt × Information Public after Apr-98	0.061	0.056	-0.002	-0.002
× Dum_1999_02 (γ_{10})	(0.096)	(0.097)	(0.09)	(0.087)
Debtijt × Information Public after Apr-98	-0.008	-0.013	0.001	0.001
× Dum_1999_03 (γ_{11})	(0.032)	(0.034)	(0.17)	(0.175)
Debtijt × Information Public after Apr-98	0.083	0.079*	0.088	0.088**
× Dum_1999_04 (γ_{12})	(0.044)	(0.045)	(0.04)	(0.045)
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 8
Effect of Public Information 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 prior to April, relative to control firms, after controlling for unobserved firm heterogeneity and aggregate shocks. Difference-in-difference (DD) estimates are obtained by subtracting to each coefficient γ_m the average coefficients in the pre-expansion period, γ_{-2}, γ_{-1} , and γ_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.

Dependent Variable	$\ln(\#Lenders_{it})$		$DebtHHI_{it}$		$\%TopLender_{it}$	
	(1)	DD estimate (γ_m-γ_{Pre})	(2)	DD estimate (γ_m-γ_{Pre})	(3)	DD estimate (γ_m-γ_{Pre})
Firm Information Public after Apr-98	0.119		-0.106		-0.083	
× Dum_1998_02 (γ_{-2})	(0.057)		(0.029)		(0.022)	
Firm Information Public after Apr-98	0.124		-0.129		-0.098	
× Dum_1998_03 (γ_{-1})	(0.056)		(0.028)		(0.022)	
Firm Information Public after Apr-98	0.169		-0.116		-0.089	
× Dum_1998_04 (γ_0)	(0.054)		(0.026)		(0.020)	
Firm Information Public after Apr-98	0.158	0.021	-0.111	0.006	-0.085	0.005
× Dum_1998_05 (γ_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 (γ_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 (γ_3)	(0.043)	(0.028)	(0.024)	(0.014)	(0.019)	(0.012)
Firm Information Public after Apr-98	0.153	0.016	-0.062	0.055***	-0.046	0.044***
× Dum_1998_08 (γ_4)	(0.041)	(0.029)	(0.022)	(0.016)	(0.018)	(0.013)
Firm Information Public after Apr-98	0.146	0.009	-0.063	0.054***	-0.044	0.046***
× Dum_1998_09 (γ_5)	(0.042)	(0.031)	(0.020)	(0.016)	(0.017)	(0.013)
Firm Information Public after Apr-98	0.122	-0.015	-0.057	0.060***	-0.046	0.044***
× Dum_1998_10 (γ_6)	(0.038)	(0.031)	(0.019)	(0.017)	(0.016)	(0.014)
Firm Information Public after Apr-98	0.103	-0.034	-0.05	0.067***	-0.038	0.052***
× Dum_1998_11 (γ_7)	(0.035)	(0.033)	(0.017)	(0.017)	(0.014)	(0.014)
Firm Information Public after Apr-98	0.1	-0.037	-0.046	0.071***	-0.037	0.053***
× Dum_1998_12 (γ_8)	(0.032)	(0.036)	(0.016)	(0.019)	(0.013)	(0.015)
Firm Information Public after Apr-98	0.075	-0.062	-0.04	0.077***	-0.033	0.057***
× Dum_1999_01 (γ_9)	(0.027)	(0.041)	(0.014)	(0.020)	(0.012)	(0.016)
Firm Information Public after Apr-98	0.039	-0.098***	-0.026	0.091***	-0.024	0.066***
× Dum_1999_02 (γ_{10})	(0.026)	(0.041)	(0.012)	(0.021)	(0.011)	(0.017)
Firm Information Public after Apr-98	0.029	-0.108***	-0.012	0.105***	-0.011	0.079***
× Dum_1999_03 (γ_{11})	(0.024)	(0.044)	(0.010)	(0.023)	(0.009)	(0.018)
Firm Information Public after Apr-98	0.032	-0.105**	-0.007	0.110***	-0.007	0.083***
× Dum_1999_04 (γ_{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	
Adjusted R-squared	0.17		0.12		0.12	

Table A1
Placebo Test: 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. 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.

Dependent Variable	$\ln(Debt_{it})$					
Firm Sub-Sample: by # lenders prior to April	All			Multiple Lenders		
Max Risk Rating prior to April	1 or 2	1	2	1 or 2	1	2
	(1)	(2)	(3)	(4)	(5)	(6)
DD estimate: effect after 1 month (γ_1 - $\gamma_{Feb\text{-}Apr\ Avg}$)	0.022	0.024	0.017	0.024	0.028	0.008
	(0.011)	(0.012)	(0.021)	(0.012)	(0.013)	(0.026)
DD estimate: effect after 2 months (γ_2 - $\gamma_{Feb\text{-}Apr\ Avg}$)	0.021	0.022	0.021	0.022	0.026	0.008
	(0.013)	(0.014)	(0.022)	(0.014)	(0.016)	(0.027)
DD estimate: effect after 3 months (γ_3 - $\gamma_{Feb\text{-}Apr\ Avg}$)	0.001	0.004	-0.010	0.005	0.012	-0.024
	(0.014)	(0.016)	(0.026)	(0.017)	(0.019)	(0.031)
DD estimate: effect after 4 months (γ_4 - $\gamma_{Feb\text{-}Apr\ Avg}$)	-0.009	-0.011	0.008	-0.003	0.002	-0.022
	(0.016)	(0.018)	(0.027)	(0.018)	(0.021)	(0.030)
DD estimate: effect after 5 months (γ_5 - $\gamma_{Feb\text{-}Apr\ Avg}$)	-0.019	-0.021	-0.006	-0.011	-0.009	-0.021
	(0.018)	(0.020)	(0.029)	(0.020)	(0.022)	(0.035)
DD estimate: effect after 6 months (γ_6 - $\gamma_{Feb\text{-}Apr\ Avg}$)	-0.029	-0.031	-0.014	-0.017	-0.015	-0.028
	(0.018)	(0.020)	(0.027)	(0.020)	(0.023)	(0.031)
DD estimate: effect after 7 months (γ_7 - $\gamma_{Feb\text{-}Apr\ Avg}$)	-0.016	-0.018	0.001	0.002	0.005	-0.010
	(0.019)	(0.022)	(0.027)	(0.022)	(0.025)	(0.033)
DD estimate: effect after 8 months (γ_8 - $\gamma_{Feb\text{-}Apr\ Avg}$)	-0.042	-0.047	-0.002	-0.024	-0.027	0.003
	(0.029)	(0.032)	(0.030)	(0.021)	(0.024)	(0.035)
DD estimate: effect after 9 months (γ_9 - $\gamma_{Feb\text{-}Apr\ Avg}$)	-0.008	-0.010	-0.003	0.012	0.009	0.019
	(0.019)	(0.022)	(0.029)	(0.023)	(0.026)	(0.035)
DD estimate: effect after 10 months (γ_{10} - $\gamma_{Feb\text{-}Apr\ Avg}$)	-0.017	-0.021	0.000	0.006	0.002	0.021
	(0.019)	(0.022)	(0.029)	(0.022)	(0.025)	(0.035)
DD estimate: effect after 11 months (γ_{11} - $\gamma_{Feb\text{-}Apr\ Avg}$)	0.000	-0.004	0.023	0.011	0.005	0.033
	(0.018)	(0.020)	(0.026)	(0.020)	(0.023)	(0.032)
DD estimate: effect after 12 months (γ_{12} - $\gamma_{Feb\text{-}Apr\ Avg}$)	0.003	-0.002	0.024	0.012	0.006	0.035
	(0.016)	(0.018)	(0.033)	(0.018)	(0.020)	(0.042)
First Differenced Estimation	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effecs	Yes	Yes	Yes	Yes	Yes	Yes
Firm Specific Trends	Yes	Yes	Yes	Yes	Yes	Yes
Month Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations (firm-months)	83,306	70,019	13,287	60,691	50,143	10,548
Clusters (firms)	4,769	4,022	747	3,424	2,835	589
Adjusted R-squared	0.06	0.06	0.06	0.06	0.06	0.06

Table A2
Placebo Test: Estimates Using Fake Registry Cut-Off 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 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.

Dependent Variable	$\ln(Debt_{it})$					
Firm Sub-Sample: by # lenders prior to April	All			Multiple Lenders		
Max Risk Rating prior to April	1 or 2	1	2	1 or 2	1	2
	(1)	(2)	(3)	(4)	(5)	(6)
DD estimate: effect after 1 month ($\gamma_1 - \gamma_{Feb\text{-}Apr\ Avg}$)	-0.006	-0.002	-0.035	-0.004	-0.002	-0.017
	(0.030)	(0.034)	(0.030)	(0.033)	(0.038)	(0.041)
DD estimate: effect after 2 months ($\gamma_2 - \gamma_{Feb\text{-}Apr\ Avg}$)	-0.009	0.002	-0.082	-0.007	0.003	-0.074
	(0.033)	(0.037)	(0.045)	(0.038)	(0.043)	(0.061)
DD estimate: effect after 3 months ($\gamma_3 - \gamma_{Feb\text{-}Apr\ Avg}$)	0.044	0.065	-0.104	0.056	0.077	-0.077
	(0.046)	(0.052)	(0.060)	(0.057)	(0.065)	(0.081)
DD estimate: effect after 4 months ($\gamma_4 - \gamma_{Feb\text{-}Apr\ Avg}$)	0.070	0.102	-0.146	0.063	0.082	-0.062
	(0.054)	(0.061)	(0.078)	(0.059)	(0.066)	(0.098)
DD estimate: effect after 5 months ($\gamma_5 - \gamma_{Feb\text{-}Apr\ Avg}$)	0.067	0.113	-0.236	0.019	0.054	-0.205
	(0.056)	(0.063)	(0.093)	(0.061)	(0.068)	(0.121)
DD estimate: effect after 6 months ($\gamma_6 - \gamma_{Feb\text{-}Apr\ Avg}$)	0.051	0.082	-0.160	-0.022	0.015	-0.256
	(0.052)	(0.057)	(0.137)	(0.062)	(0.068)	(0.143)
DD estimate: effect after 7 months ($\gamma_7 - \gamma_{Feb\text{-}Apr\ Avg}$)	0.043	0.054	-0.037	-0.017	0.011	-0.202
	(0.049)	(0.052)	(0.137)	(0.061)	(0.066)	(0.153)
DD estimate: effect after 8 months ($\gamma_8 - \gamma_{Feb\text{-}Apr\ Avg}$)	0.024	0.041	-0.091	-0.025	0.008	-0.243
	(0.046)	(0.050)	(0.121)	(0.062)	(0.067)	(0.160)
DD estimate: effect after 9 months ($\gamma_9 - \gamma_{Feb\text{-}Apr\ Avg}$)	0.048	0.045	0.063	0.017	0.018	-0.005
	(0.044)	(0.048)	(0.109)	(0.061)	(0.066)	(0.146)
DD estimate: effect after 10 months ($\gamma_{10} - \gamma_{Feb\text{-}Apr\ Avg}$)	0.023	0.016	0.063	-0.007	-0.010	-0.007
	(0.037)	(0.040)	(0.101)	(0.050)	(0.054)	(0.135)
DD estimate: effect after 11 months ($\gamma_{11} - \gamma_{Feb\text{-}Apr\ Avg}$)	0.010	0.004	0.043	-0.016	-0.014	-0.044
	(0.035)	(0.038)	(0.093)	(0.049)	(0.053)	(0.123)
DD estimate: effect after 12 months ($\gamma_{12} - \gamma_{Feb\text{-}Apr\ Avg}$)	0.001	-0.004	0.022	-0.014	-0.013	-0.041
	(1.000)	(1.000)	(1.000)	(1.000)	(1.000)	(1.000)
First Differenced Estimation	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effecs	Yes	Yes	Yes	Yes	Yes	Yes
Firm Specific Trends	Yes	Yes	Yes	Yes	Yes	Yes
Month Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations (firm-months)	22,447	19,456	2,991	12,498	10,734	1,764
Clusters (firms)	1,335	1,162	173	724	623	101
Adjusted R-squared	0.07	0.07	0.07	0.07	0.07	0.09