Public Information and Coordination: Evidence from a Credit Registry Expansion

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Abstract

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

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I. Introduction

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

The present paper provides evidence of the publicity multiplier of information in the

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

[2] The publicty multiplier of information is discussed explicity for creditor runs (Morris and Shin 2004), political action (Edmond 2008), monetary policy (Morris and Shin 2002b), and asset prices (Ozdenoren and Yuan 2008). As emphasized by Morris and Shin (2002a), it is a general feature of any interaction where agents have an incentive to coordinate and possess private information. Hence it is present in theoretical accounts of bank runs (Goldstein and Pauzner 2005), currency attacks (Morris and Shin 1998; Hellwig et al. 2006), financial crises (Goldstein 2005) and borrower runs (Bond and Rai 2009).

[3] See, for example, Woodford (2005).

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

[5] In a laboratory setting, Heinemann, Nagel and Ockenfels (2004) examine the effect of changing the degree of common information to test the global games unique equilibrium existence conditions. Cornand and Heinemann (2009) provide experimental evidence that players in a coordination game give additional weight to information when it is public.

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

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

The reform made public information retroactive to January 1998, but its implementation was delayed. As a result we identify three periods in the credit registry data: a pre-announcement period, an interim period, and a post-expansion period. During the pre-announcement period, banks reported information to the Central Bank under the presumption it would remain private. During the interim period that followed the reform announcement in April 1998, lenders knew information they reported in the pre-expansion period would become public, but they had not yet received other lenders' information. This interim period allows us to measure whether the anticipated publicity induces changes in lending outcomes that can be explained by information banks had already reported. We use this period to assess whether publicity elicits an additional response to information. During the post-expansion period, banks made reporting and lending decisions having observed the previous reports of other banks. We use this period to characterize the resulting credit market equilibrium with public information. To provide a counterfactual for the time series evolution of debt and other credit outcomes, we exploit the fact that the reform did not affect borrowers with more than $200,000 in debt before April 1998. By focusing on firms close to and on either side of the threshold, we obtain difference-in-differences (DD) estimates that control for aggregate shocks to credit outcomes. All our reported results are drawn by comparing the changes in outcomes pre- and post-registry expansion for borrowers whose pre-expansion debt was between $175,000 and $200,000, relative to borrowers whose pre-expansion debt was between $200,000 and $225,000.

We find that lenders react strongly to their private information about a borrower upon the announcement that their information will become public. The reaction occurs only when the information is likely to affect another lender's beliefs about the same firm. In our context, this occurs when a bank possesses bad news about a firm that borrows from multiple banks.[6] During the interim period the registry expansion announcement causes a 15% decline in a firm's debt with lenders that had rated it a poor risk in the pre-announcement period. In contrast, those same firms' debt with lenders that assigned them a good rating in the pre-announcement period does not decline in the interim period. Debt with these lenders drops sharply after the interim period ends, when another bank's bad rating becomes public. We find a similar pattern in defaults: the default hazard rate increases by 13 percentage points during the interim period if a bank had assigned a poor rating to the firm in the pre-announcement period.

Additional results show that the registry expansion has first order effects on long-run credit outcomes that are consistent with a stylized model where lenders have incentives to coordinate. Firms whose information became public experience, on average, a permanent 8% decline in debt. The decline is due to a reduction in the likelihood of receiving new fund-

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

ing, consistent with banks' diminished incentives to provide interim liquidity needs. These effects are present only among firms with multiple lenders and are thus susceptible to lender coordination problems. These firms concentrate borrowing from fewer creditors after the registry expansion, potentially reducing the likelihood of coordination problems in the long run (Corsetti et al. 2004). These long-run and cross sectional patterns are difficult to reconcile with standard asymmetric information interpretations of the findings.[7]

Our paper relates to a broad literature that studies the effect of disclosure and transparency, particularly in credit markets. The costs and benefits of public information in environments with coordination have been discussed in recent theory papers (Morris and Shin 2002b, 2005, 2007; Angeletos and Pavan 2004, 2007; Morris et al. 2006; Woodford 2005; Svensson 2006; Cornand and Heinemann 2008). The implication in our context is that making information public can lower welfare if it causes banks to underweight their private information. In opposition to this, increased public information can limit the probability with which a bank lends based on an incorrect assessment of the actions of other banks. Although we cannot measure the net welfare implications, our evidence confirms the central mechanism: banks place additional weight on information when it is made public. Thus, this paper contributes to the ongoing policy debate on the consequences of transparency and mandated disclosure of information to investors (see Bushee and Leuz 2005, Greenstone et al. 2006, Musto 2004, Simon 1989).

By measuring the publicity multiplier our paper also provides evidence of complimentarities in bank lending decisions for firms that are close to distress. Other evidence of this force has been documented by Asquith, Gertner, and Scharfstein (1994) who 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

[7] Information sharing will lead to more lending in the long run if it reduces adverse selection or moral hazard (Stiglitz and Weiss 1981), reduces hold-up by a privately informed banks (Rajan 1992), or reduces firm liquidity risk by lowering the costs of switching lenders (Detragiache, Garella, and Guiso 2000). And it will affect debt of single lender firms if it reduces bank monitoring incentives (Petersen and Rajan 1995, Rajan 1992), lowers firm reputational incentives (Padilla and Pagano 2000), or reveals hidden firm debt (Parlour and Rajan 2001; Bisin and Guaitoli 2004; Bennardo, Pagano, and Piccolo 2009).

to mitigate coordination problems. In the context of mutual funds, Chen Goldstein, and Jian (2009) show that bad past performance has a stronger effect on mutual fund investor decisions when they have an incentive to coordinate due to asset illiquidity.

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

II. Empirical Setting

A. The Credit Registry prior to 1998

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

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

Only financial institutions and credit rating companies were granted access to the registry data. Institutions that requested borrower level information received a monthly magnetic tape containing the most recent cross section of borrowers. Information reported to the Central Bank was shared with a typical delay of 3 months, i.e., the credit information for January 1998 became available in April 1998. Outside of the public credit registry, lenders could not formally ascertain how much total debt a borrower owed other financial institutions.[8]

B. CD-ROM Adoption in 1998

In May 1998 the Central Bank switched to a low-cost technology for distributing the registry information (CD-ROMs).[9] The resulting lower information sharing costs made the $200,000 threshold obsolete, and the Central Bank virtually eliminated it by sharing information for every borrower with a total debt above $50. The elimination of the threshold was implemented retroactively to January 1998. Because the policy change was not announced until April, banks' lending and reporting decisions during the first three months of 1998 were

[8] There is no secondary market for loans in Argentina. This means there is no price that can aggregate the private signals of different investors as in Angeletos and Werning (2006).

[9] See Central Bank Communication A2686 dated April 14, 1998 (URL: http://www.bcra.gov.ar).

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

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

Our empirical analysis uses the monthly data from the public registry released through CD-ROMs. The sample period starts in January 1998 and covers the universe of borrowers (firms and individuals) with more than $50 of debt with the formal banking sector in Argentina. On March 1998, the month before the announcement of the switch to CD-ROMs and virtual elimination of the threshold, the registry contains information for 566,416 borrowers in 966,513 bank-borrower lending relationships. The registry expansion increased the number of borrowers with publicly shared credit information by 540,000 firms and individuals; their debt represents 11% of the $67 billion dollars of total outstanding debt from the banking sector.[10]

C. Identifying the Publicity Multiplier

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

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

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

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

III. Framework: Information Sharing and Coordination

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

A. Setup

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

The true profitability of the project, θ, is uncertain and is distributed normally with mean μ_0 and precision τ_0. This distribution is common knowledge to both banks and the entrepreneur all of which are assumed to begin with symmetric information. 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 information sharing is mandated. This assumption follows from the fact that information in a credit registry is a subset of the information banks possess about the profitability of their borrowers.

After the signals are released, each bank can choose whether to roll over its loan to the entrepreneur or to liquidate the loan and receive L from selling the collateral. This roll-over decision can be interpreted more broadly to capture a scenario where the banks are deciding whether to inject additional funds to cover an interim liquidity shock to the firm. We distinguish between the two interpretations empirically in Section V. The banks' payoffs are determined by the following simultaneous move game:[11]

Action	Roll Over$_b$	Liquidate$_b$
Roll Over$_a$	θ, θ	$\theta - K, L$
Liquidate$_a$	$L, \theta - K$	L, L

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

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

A formal analysis of the model is presented in the Appendix. Our focus here is to use that analysis to highlight how information sharing can alter a bank's roll-over decision. If bank i's posterior expectation of θ is greater than $L + K$ (less than L), then it will optimally

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

choose to roll over (liquidate) its loan, regardless of what it expects the other bank to do. However, if bank i's expectation of θ is between L and $K + L$, then its optimal action will depend on what it expects the other bank will do. In this range, bank i will optimally choose to roll over its loan only if it assesses the probability that the other bank will also roll over is sufficiently high. The unique equilibrium strategy of each bank is to roll over its loan if and only if its posterior belief is above some cutoff level $\overline{\mu}$.[12]

When bank i's posterior is in this intermediate range, it will use all available information to form an assessment of bank j's posterior and hence the probability that j will roll over its loan. Bank i's expectation of j's posterior is a weighted average of their shared common information (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.

Absent information sharing, each bank has a fixed cutoff posterior above which it will chose to roll over its loan (Figure 1, Panel A). In this case, the information each bank receives is only used to adjust its posterior. With information sharing, the cutoff strategy that each bank follows is a function of the common prior that is formed using publicly released information (see Figure 1, Panel A). If the shared information is bad news, and hence the common prior is low, bank i will use a high cutoff strategy (close to $L + K$), because the pessimistic public information implies that j is likely to have a low posterior and hence liquidate its loan. This high cutoff is further reinforced by the knowledge that j is also using a high cutoff and so on. The same argument applies symmetrically for good news. In the Appendix we show that each bank's equilibrium cut-off is strictly decreasing in the common public prior (formed using s_a and s_b). By the same logic, the cutoff strategy that each bank uses is strictly decreasing in its own shared signal: $\frac{\partial \overline{\mu}}{\partial s_i} < 0$. Holding all else constant, when bank i shares bad news

[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 a 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 (3) in the Appendix. This restriction ensures that the unique equilibrium strategy of each bank is characterized by a cutoff rule whereby it will roll over the loan if and only if its posterior belief about θ is above some critical level.

($s_i < \mu_0$), its expectation that the other bank will roll over declines. This highlights the publicity multiplier of information. A piece of information will have the same effect on bank i's posterior whether or not it is shared. However, only when the information is made public does it also alter the bank's cutoff strategy.

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

Information sharing can affect the unconditional probability that a bank will roll over its loan. The direction of this effect depends on whether average firm creditworthiness, μ_0, is high or low relative to $L + K$. Suppose that μ_0 is high. Absent information sharing, bank i will assign a high probability that its rival will roll over its own loan. As a result i will use a low cutoff rule ($\overline{\mu}$ close to L). This is the case represented in Figure 1, Panel A. If bad news is released publicly this will lead each bank to apply a stricter cutoff rule and lend less in expectation. The effect of having bad news shared will on average outweigh the opposite effect of having good news shared because the optimal cutoff each bank uses cannot fall below L and hence good news will have a much smaller effect on the cutoff rule that each bank applies. Thus when μ_0 is high, information sharing will result in a decrease in the ex-ante probability that a bank rolls over its loan. A symmetric argument applies in reverse when average firm creditworthiness is low.

Figure 1 (Panel B) formalizes this intuition by showing how each bank's unconditional probability of liquidating its loan is affected by information sharing. If average firm creditworthiness is high (low) then information sharing increases (decreases) this probability. In our empirical setting, we will test whether information sharing causes an increase or decrease in the average lending level. Although the model predicts that both are possible, our analysis sample comprises firms with prior access to credit, good credit ratings (2 or better), and an unconditional default probability below 4%, which implies that it is reasonable to presume that average creditworthiness these firms is high. Under this assumption, the model predicts that information sharing will increase the probability of liquidation and reduce average lending. Ultimately, however, this remains a question we leave for our empirical analysis.

IV. Estimation and Descriptive Statistics

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

Taking advantage of the high density of firms with total debt around $200,000, we control nonparametrically for differences in total debt across the affected and control groups by restricting the analysis sample to borrowers whose total debt was always between $175,000 and $225,000 before April 1998. Since only firms with a risk rating of 1 and 2 were affected by the registry expansion, we exclude all firms with a risk rating higher than 2 in January 1998 from the sample. These restrictions exclude firms with poor ratings or that had not obtained credit from the formal financial system before April 1998. Thus, the estimates will be valid for small firms with high expected creditworthiness relative to other borrowers of similar characteristics. Descriptive statistics for the March 1998 cross-section (prior to expansion announcement) of this subsample are shown in Table 1 (Panel A). The subsample includes 1,006 borrowers with an average total debt of $203,300 in March. The median borrower has one lender and a high collateral to debt ratio (0.83). The firms in our sample come from a wide range of industries. The most common are agricultural production (34.9%), services (19.5%), wholesale (18.7%), and manufacturing (13.8%).[13]

Our main empirical strategy to identify the publicity multiplier involves measuring the

[13] We do not have further information on the firms in our sample beyond what is reported in the credit registry.

effect of disclosing bad news about a firm that a bank possesses privately before the registry expansion. Thus, in some specifications, we focus on the subsample of firms that have at least one rating of 2 before the registry expansion. The descriptive statistics of this subsample of firms are shown in Panel B of Table 1.

It is important to emphasize that only reporting bad news, which in our analysis sample entails reporting a rating of 2, will have a significant effect on other lenders' priors about a firm's creditworthiness. The reason is that the most likely rating that a bank assigned to a firm is a 1, both unconditionally and conditioning on the firm having at least one rating of 2. More than 92% of the bank-firm pairs in the full analysis sample have a rating of 1 in January 1998. Conditioning on having assigned a rating of 2, the probability that another lender to the same firm assigned a 1 is 85.4%. Therefore we do not expect to see banks who have reported a 1 to display a measurable positive reaction when they learn this will be made public because this rating contains very little information.

Observe that banks report ratings of 2 even when they know this will be shared with other banks. Panel A of Table 1 shows that 10 percent of control firm bank relationships in March 1998 were assigned a rating of 2. Furthermore these ratings are informative for a firm's true creditworthiness: 27.3% of the relationships that had a 2 reported entered default within the next twelve months as compared to 6.1% for relationships that had a 1 reported in March 1998. There are several reasons for a bank to report a rating of 2 even when it knows this will be observed by a firm's other lenders. First, these ratings are used for prudential regulation and it would arouse further scrutiny by the central bank if all relationships not in default were rated 1. In addition, the credible threat of being publicly rated 2 provides incentives to borrowers to avoid actions which lower the value of an outstanding loan.

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

Figure 2 shows evidence that suggests these assumptions hold after conditioning on pre-existing means and trends of the outcome variable of interest in the full analysis sample.[14] Panel A of Figure 2 plots the time series of median debt for firms affected by the expansion and control firms. Both series have pre-April 1998 means and trends removed throughout. There is no change in the median debt evolution of firms in the control group after the registry expansion. The same is true for the average firm debt concentration, measured as the HHI of a firm's debt across all its lenders (Figure 2, Panel B). This suggests that the registry expansion did not affect the credit outcomes of the control group, regardless of their pre-expansion credit rating. This observation rules out some types of borrower self-selection into the control group that would induce an upward bias in the DD estimates. Suppose that firms endogenously choose higher levels of total debt to make their credit records public through the registry. These control group firms would reduce their total debt after the elimination of the threshold that would be measured as a relative increase in total debt in the affected group by the DD estimate.

A different type of self-selection can occur if borrowers or lenders have incentives to prevent credit information from becoming public and choose debt levels below the \$200,000. This is an issue if selection below the threshold is correlated with firm credit quality. To explore this we plot in Figure 3 the distribution of firms and average firm characteristics by total debt for the March 1998 cross section. We expand the sample to include firms with total debt in the \$100,000 to \$300,000 range so that discontinuities at the \$200,000 threshold can be evaluated

[14] Figure IA.1 in the internet appendix shows these assumptions also hold for the subsample of firms with at least one rating of 2.

relative to the general size distribution of borrowers. The density of firms in the treatment group does not appear to be abnormally high to the left of the $200,000 threshold. Although we cannot reach definitive conclusions because of the lack of a proper counterfactual for the firm distribution, the plot suggests that there is no stark accumulation of firms below the threshold.

The firm characteristic distribution in Panel B of Figure 3 along with the descriptive statistics for treatment and control firms in Table 1 suggests that firms above and below the threshold in March 1998 are similar in the collateral-to-debt ratio and the fraction with a risk rating of 1, observable proxies for credit quality. Loan contract characteristics and risk ratings should capture differences between the treatment and control firms' credit quality that are observed by lenders. Thus, the patterns in this plot allow us to rule out self-selection of firms to the control group along dimensions of credit quality that are unobservable to the econometrician, but observable by the lenders. The plot also indicates that firms in the control group concentrate their borrowing with fewer lenders than firms in the treatment group. A regression discontinuity analysis in the cross section before the registry expansion (see Internet Appendix) indicates that the concentration difference is statistically significant. The standard interpretation of a regression discontinuity estimate would indicate that information sharing induces a significant increase in debt concentration. We corroborate this conclusion later with the DD estimation.

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

The previous evidence establishes that credit outcomes of the control firms represent a valid counterfactual for those of the affected firms after conditioning on means and trends. In addition, our strategy for isolating the publicity multiplier relies on the timing of the effect of the registry expansion announcement on lending outcomes. For that reason, we wish to impose as little structure on the time pattern of credit outcomes as possible. These arguments provide the rationale for a DD estimation based on the following specification:

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

The dependent variable is the (log) debt of firm i at month t. To ease interpretation we label April 1998, the last month before information sharing through the registry, as $t = 0$. Thus, March (May) 1998 corresponds to $t = -1$ ($t = 1$). The right-hand side includes firm fixed effects, calendar month dummies and firm specific time trends. $Treat_i$ is a dummy equal to one if firm i's credit information becomes public after April 1998 due to the registry expansion. The coefficient on this dummy represents the log-difference between the average debt of firms affected by the registry expansion and firms in the control group. $Treat$ is interacted with a full set of calendar month dummies. The interaction coefficients represent the log-debt differences across the two groups every month before and after the registry expansion. Our specification is designed to measure the effect of information sharing on the level of lending to a firm. The use of firm specific time trends means that we are likely to underestimate any permanent effect on the growth rate of lending.

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 1998 and April 1998, the pre-expansion period (γ_{pre}).

All the results are reported as DD estimates, obtained over the \$175,000 and \$225,000 debt subsample, and using February through April 1998 as the pre-period. Estimates are obtained by first-differencing specification (1) to account for the firm fixed effects. The estimated γ_m coefficients are reported in the Internet Appendix. All standard errors of the first-differenced specification are estimated allowing for clustering at the firm level to account for residual serial correlation in outcomes. Although excluded for brevity, the results and conclusions are robust both to widening the analysis sample to include firms with debt between \$150,000 and \$250,000 debt and to excluding April 1998 (the month when the policy change was announced) from the pre-expansion period.[15]

V. Empirical Results

A. *Publicity Multiplier*

Our strategy to isolate the publicity multiplier laid out in Sections II and III relies on measuring the lending response to bad news that was private before the registry expansion during the interim period. We start the analysis with the subsample of firms that had a rating of 2 assigned by at least one of its lenders before the registry expansion announcement (we turn to the full sample estimation below). Table 2 shows the estimated effects of the registry announcement on total firm debt, on the debt with the bank(s) that assigned the rating of 2, and on the debt with the banks that assigned a rating of 1, which result from estimating specification (1) on this subsample.

The expansion announcement has a significant and immediate negative effect on firm debt during the interim period, during which the registry information had not yet become public. Debt with banks that assigned a rating of 2, i.e., banks that possess bad news about the firm before the announcement, declines in excess of 15% the month immediately after the announcement (Table 2, column 2). The negative effect on debt occurs even if other banks had assigned better ratings to the same firms (Table 2, column 4). In contrast, the

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

announcement's effect on debt of the same set of firms but with banks that did not possess bad news is statistically insignificant during the two months after the announcement (Table 2, column 5). Only after the information becomes public does debt with these banks drop significantly by 26.9%. Because estimates in columns 4 and 5 (Table 2) are obtained from the same sample of firms, the difference in the estimates is unlikely driven by firm-specific shocks unaccounted for with the DD estimation. Finally, neither the expansion announcement nor the actual release of registry information appears to have a significant effect on debt of firms with a single lender before the expansion announcement (Table 2, column 6).[16]

These findings are consistent with a publicity multiplier of information. The announcement that information will become public causes a bank's lending to respond to bad news it already possesses. Under the publicity multiplier interpretation, the immediate decline in lending after the registry expansion announcement occurs in anticipation of other lenders' reactions when the bad news becomes common knowledge. The decline in lending occurs after the expansion is announced, but before information actually becomes public, thus implying that the reaction is due to the expected effect of the publicity of information and not due to the arrival of additional information. The fact that debt of the same firms with lenders with no bad news before the registry expansion drops after the information becomes public corroborates that this expectation was rational.

The finding that the registry expansion announcement and actual sharing of information have no effect on firm debt on the subsample of borrowers with a single lender is reassuring, since the incentive for lenders to coordinate is not present for these borrowers. In addition, under the strong assumption that firms with a single lender and those with multiple lenders are affected in the same way by public information through channels other than lender coordination, this finding is inconsistent with alternate interpretations of the results. For example, by mandating information sharing, the registry may reduce banks' incentives to collect information and create incentives to free ride on the information collected by other banks.

[16] The long run effects are imprecisely estimated (see Internet Appendix) due to the small sample size in the specifications estimated on firms with a rating of 2 prior to the announcement. We discuss the long run effects in the full subsample of firms below.

Reduced incentives to screen and monitor could potentially result in reduced equilibrium lending. However, diminished informational rents will reduce the incentives to lend to all firms, potentially more to firms with a single lender. The results suggest that reduced information collection incentives are not the main force driving the observed effects. The same argument applies to theories that suggest that releasing too much public information will lower a borrower's incentive to work hard to maintain her reputation (Padilla and Pagano 2000).

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

B. Financial Distress

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

$$1[Default_{it} = 1|Default_{it-1} = 0]_{it} = \xi'_t + \sum_{m=-2}^{12} \lambda_m.Treat_i.I(m = t)_t + \zeta_{it} \qquad (2)$$

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

The results mirror those on debt. Firms experience a sharp and immediate increase in the probability of default: the hazard rate of defaults on any debt increases by 16 percentage points the month after the expansion announcement (Table 3, column 1). This immediate effect indicates that the increase in defaults must come from firms who are unable to make principal payments since missing interest payments would be reported with a delay. Most of the immediate increase in the hazard rate is due to default on debt with lenders that had the bad news before the expansion announcement (Table 3, columns 2 and 4). Default with the banks that do not possess bad news before the expansion announcement also increases after the announcement, but the point estimate is not statistically significant. The default hazard with these banks increases significantly by 3.9 percentage points two months after the expansion announcement, when the information in the registry becomes available. As before, the registry expansion has no statistically significant effect on the default probability of firms with a single lender.

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

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

C. Unconditional Effect of Information Sharing

So far we have restricted the analysis to the subsample of firms for which banks possessed some private bad news before the registry expansion. We turn now to investigate whether lending outcomes become more sensitive to news made public after the expansion. It is, however, difficult to isolate the change in sensitivity to news caused by the publicity multiplier from that caused by the direct effect of new information. For example, a firm's downgrade from a risk rating of 1 to 2 should be accompanied by a larger decline in lending after the registry information becomes publicly available for two reasons: first, because of the publicity multiplier of information, and second, because ratings assigned after the expansion contain information made public through the registry and are potentially more precise signals of creditworthiness due to information aggregation.[17]

[17] This provides an additional rationale for performing our analysis so far on ratings assigned by banks before the expansion announcement.

Therefore, we focus on the average long run effect of the registry expansion on credit outcomes without conditioning on post-expansion risk ratings (using specifications (1) and (2)). If the registry expansion increases firms' vulnerability to coordination failures upon the future arrival of bad public news, we expect average lending to decline unconditionally in our study sample. As discussed in Section III, the effect of having bad news released through the registry will outweigh the average effect of having good news released because the sample is comprised of firms with low unconditional default probabilities (μ_0 is high). In addition, even though both treatment and control firms have their information shared through the registry after the expansion, firms in the treatment group are more vulnerable to lender coordination failures because they borrowed from more lenders before the registry expansion (we return to the rationale for this in the final subsection).

Column 1 of Table 4 shows the estimated coefficients of specification (1) over the full subsample and the DD estimates relative to the pre-expansion period. The DD point estimates indicate that average firm debt declines by 10% around nine months after the registry expansion and remains at the lower level thereafter.[18] The sharp immediate decline in lending observed in the subsample of firms with a rating of 2 is not observed in the full sample. This confirms that the lending results in the previous subsection are induced by the anticipation of the bad news becoming public.

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

Information sharing will, in principle, improve the creditors' assessment of each borrower's creditworthiness. However, this mechanism is hard to reconcile with the observed permanent

[18] We confirm in unreported estimations that the decline persists two years post-expansion.

reduction in average credit. An increase in information about creditworthiness would reduce either adverse selection or moral hazard (Jaffee and Russell 1976, Stiglitz and Weiss 1981), reduce holdup by privately informed banks (Rajan 1992), or reduce firm liquidity risk by lowering the costs of switching lenders (Detragiache, Garella, and Guiso 2000). Contrary to our findings, all these interpretations would result in more lending in equilibrium. The lender coordination framework discussed in Section III provides a plausible rationale for the negative effect of public information on equilibrium debt.

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

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

D. *Effect on Debt Growth Distribution*

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

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

As before, we use firms in the control group to build a counterfactual for the debt growth distribution. We estimate the difference between percentage debt growth quantile τ for firms affected by the expansion and firms in the control group for every month m, ψ_m, where months are labeled as in all previous specifications relative to April 1998. Table 5 presents the estimated ψ_m for the 5th, 10th, 50th, 90th, and 95th percentiles, as well their change relative to the pre-April period ($\psi_m - \psi_{pre}$).[19]

[19] 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:

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

The estimates indicate no systematic change in the 5th or 10th quantiles of the debt growth distribution after the registry expansion (Table 5, columns 1 and 2). This indicates that the registry expansion does not induce sharp declines in lending. Conversely, there is a substantial drop in the 90th and 95th percentiles of debt growth (Table 5, columns 4 and 5). The point estimates indicate that the 95th percentile of debt growth of the affected firms declines 30 to 40 percentage points during the three months after the expansion. The pre-April debt growth 95th percentile of the affected firms is 41.1%, which suggests that the announcement of information sharing virtually eliminates the likelihood of receiving additional financing during the interim period. The decline in the 95th percentile remains at 23 percentage points a year after the expansion.

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

E. Debt Concentration

Our analysis so far has ignored the potential endogenous reaction of the structure of lending arrangements to the new information environment. Because of the registry expansion, lenders become more sensitive to bad news; thus firms are less likely to receive interim liquidity injections and become more likely to default. In theory, the consequences of lender

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

coordination can be avoided by concentrating debt from fewer lenders. In the context of currency attacks, Corsetti et al. (2004) show that the presence of an agent with large market share can reduce the incidence of coordination failures. In practice, firms would need to balance the benefits of avoiding lender coordination problems with the costs of concentrating borrowing from fewer lenders (i.e., due to holdup). Although we cannot measure the costs and benefits involved in this trade-off directly, the increased incidence of coordination problems due to the registry expansion will likely increase the optimal debt concentration.

To explore this hypothesis, we estimate specification (1) using as dependent variables the log number of lenders ($\#Lenders$), debt concentration ($DebtHHI$), and the fraction of debt with the main lender ($\%TopLender$). The estimated coefficients over the subsample of firms with multiple lenders before April are shown in Table 6. The DD estimates indicate that the average firm borrowed from 10.5% fewer banks and increased the fraction of debt with the main lender by 8.3% a year after the registry expansion. These changes induced an increase of 0.11 in the HHI of debt concentration across different lenders. These results are consistent with the cross-sectional patterns in debt concentration observed before the registry expansion (panel B, Figure 2). Both findings indicate that lending arrangements respond endogenously to the increased coordination induced by the publicity of information, leading to the concentration of firm borrowing from fewer banks. This endogenous response very likely mitigates the equilibrium effect of public information on debt and defaults. We still observe reduced debt and more defaults a year after the registry expansion, which suggests that firms face large potential costs when their borrowing is concentrated in few lenders. It also suggests that limiting coordination failures is a first order force in the trade-off firms face when choosing how many creditors to borrow from. The trade-off studied in Dewatripont and Maskin (1995), Bolton and Scharfstein (1996), and Bris and Welch (2005) is affected by the degree to which information is common knowledge.

VI. Conclusion

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

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

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A Appendix

A. Timing of the Information Release

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

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

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

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}, are proportional to the contemporaneous partial correlation of debt across the lenders of the same firm in month

m. The coefficient on the interaction with $PublicApril98$, β_{2_m}, measures the difference in this correlation between firms affected by the registry expansion and the control group. The difference-in-differences (DD) estimate of the effect of the registry expansion on lending correlation is given by the difference in the interaction coefficients before and after April 1998. The standard errors allow for clustering at the firm level to account for the mechanical correlation across different observations for the same firm in the regression estimation. We estimate by first differencing over two months to reduce the noise inherent in monthly lending changes. Estimation requires restricting the sample to firms that borrow from multiple banks. We include a firm in the sample if it had debt from more than one bank in March 1998, before the expansion announcement.

During the two months after the announcement of the registry expansion, there is no change in the correlation across lending decisions of different banks to the same firm (Table A1). In July 1998, the DD point estimates indicate that this correlation increases by 16.1 percentage points in July 1998, three months after the registry expansion announcement. The estimate is similar in sign and magnitude (18.7) when estimated using debt by other banks lagged one month to eliminate the mechanical correlation across observations for the same firm at month t (Table A1, column 2). This represents a tenfold increase of the average lending correlation across banks in the entire sample (1.56%).

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

B. Solution of Theoretical Model

We briefly characterize the equilibrium strategies of each bank. The basic solution method and existence results are directly analogous to the two player game studied in Morris and Shin (2002a), which establishes that each agent will employ a simple cutoff strategy when choosing its action. The generic solution with and without information sharing can be characterized as a game where each bank has a common prior (this includes any information that is shared) that 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 cutoff that each bank follows. By symmetry this will be the same for each bank.

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

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

where 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

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

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

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

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 roll over 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 cutoff 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 (2002a), 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

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

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

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

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:

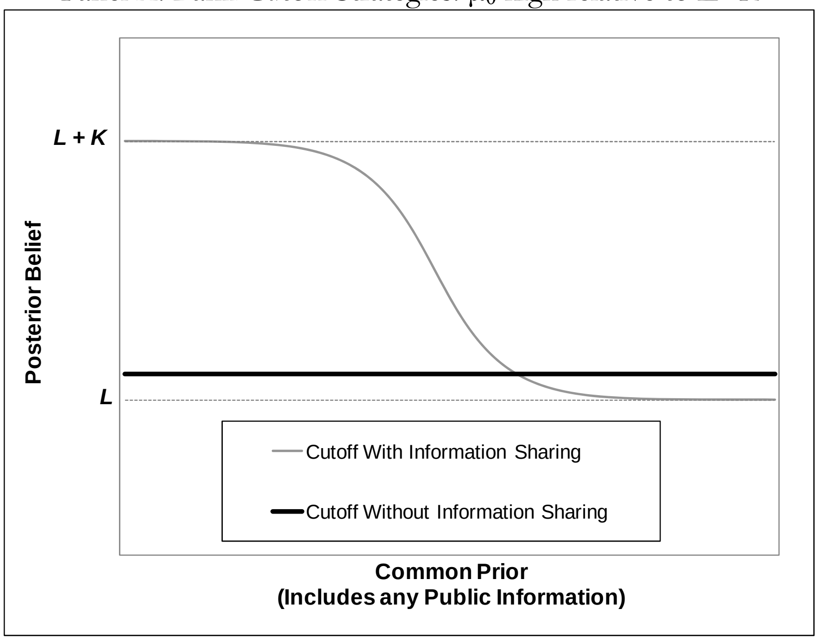
$$\frac{\partial \overline{\mu}}{\partial s_i} = \frac{\partial \mu^{com}}{\partial s_i} \frac{\partial \overline{\mu}}{\partial \mu^{com}} = \left(\frac{\tau_\varepsilon}{\tau_0 + 2\tau_\varepsilon}\right) \left(\frac{-K\Omega\phi\left(\Omega\left(\overline{\mu} - \mu^{com}\right)\right)}{1 - K\Omega\phi\left(\Omega\left(\overline{\mu}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

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.

Figure 1
Cutoff Strategies and Propensity to Liquidate

Both panels are drawn using 1,000,000 simulations of the model with the following parameters: K=0.5, L= 0.3, τ_0=0.4, τ_ε=1,and τ_ω=1. Panel A is drawn using μ_0=2 and Panel B is drawn using values of μ_0 between -4 and 5. Panel A plots the equilibrium cutoff that each bank will use: they will choose to rollover their loan if their posterior belief is above the cutoff (y-axis). The common prior, on the x-axis in Panel A is formed using Bayes rule to forecast θ using all available public information. With information sharing this is a weighted average of μ_0, s_a, and s_b. Without information sharing this is simply μ_0. Panel B as the ex-ante probability that bank *i* liquidates her loan with information sharing less the ex-ante probability that bank *i* liquidates her loan without information.

Panel A: Bank Cutoff Strategies: μ_0 high relative to L+K



Panel B: Information Sharing and the Ex-Ante Probability that a Loan is Liquidated

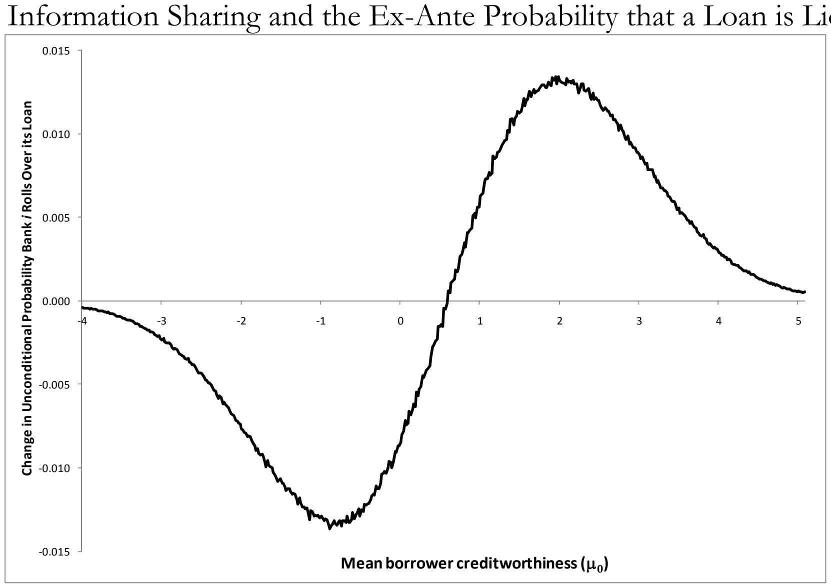
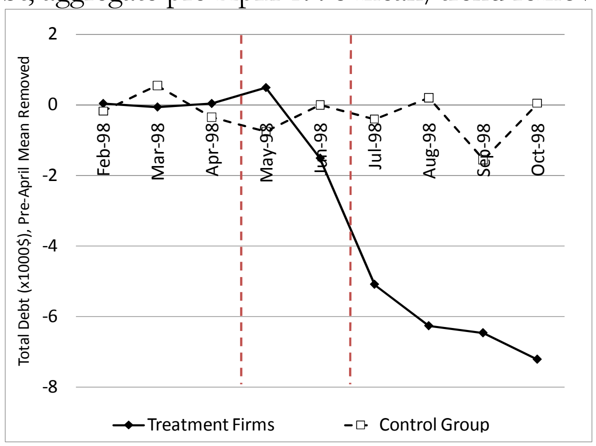


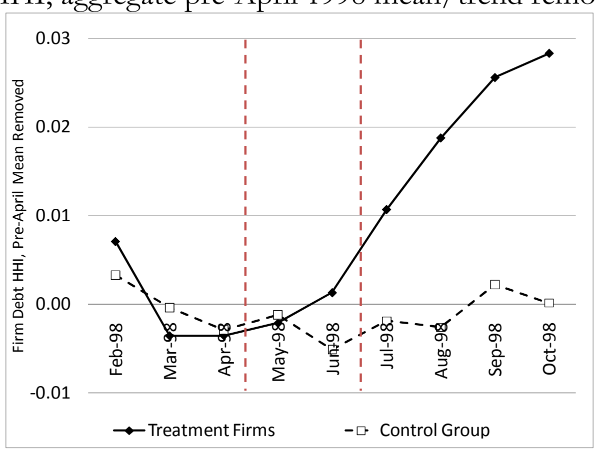
Figure 2
Firm Characteristics by Month, Treatment and Control Groups

The plots represent the time series of firm statistics for treatment and control firms. Treatment (control): firms whose information was not (was) shared before the registry expansion. Mean and trend of median debt (Panel A) and average debt HHI (Panel B) estimated during the pre-announcement period (January through April 1998) have been removed to ease interpretation. The vertical lines enclose the interim period after the registry expansion announcement and before the actual information sharing took place.

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



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



Panel C. Cumulative Default Hazard (Nelson-Aalen Estimates))

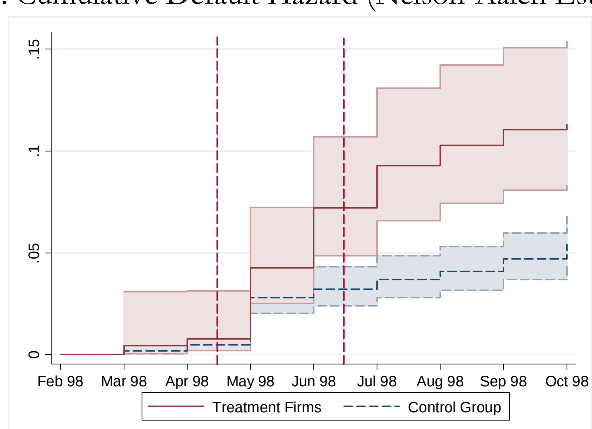
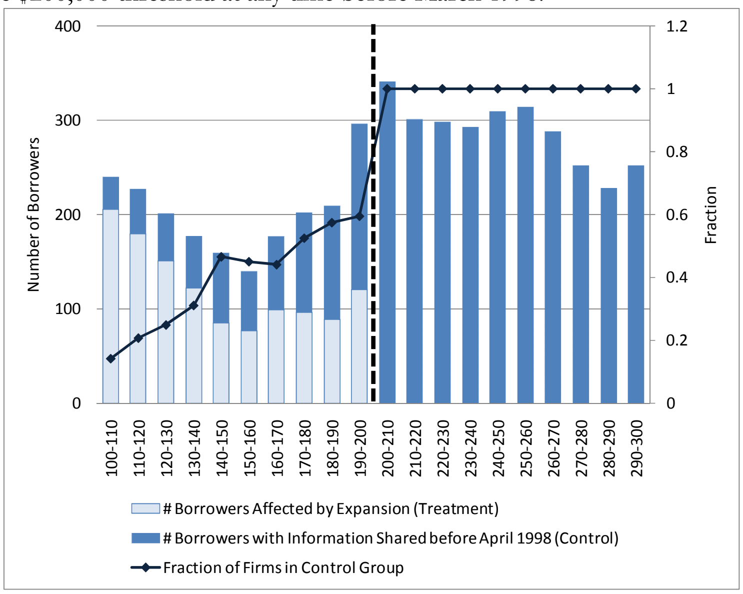


Figure 3
Borrower Distribution and Characteristics by Total Debt in March 1998

The plots represent the cross section distribution of firms by average total debt in March 1998, one month before the registry expansion announcement, for the sample of firms with a rating of 1 or 2 during the pre-announcement period. The vertical line emphasizes the $200,000 threshold for information sharing.

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

Number of firms in each $10,000 bin between $100,000 and $300,000 in the treatment and control groups (and fraction in the control group). Treatment (control): firms whose information was not (was) shared before the registry expansion. Mean and trend of median debt (Panel A) and average. Firms in the control group below the $200,000 threshold are firms that were above the $200,000 threshold at any time before March 1998.



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

Average firm characteristics in each $10,000 bin between $100,000 and $300,000 in the treatment and control groups. Characteristics shown: 1) collateral posted to total outstanding debt ratio, 2) firm debt concentration measured as the HHI of debt across all lenders for the same firm, 3) fraction of the firms with a risk rating equal to one.

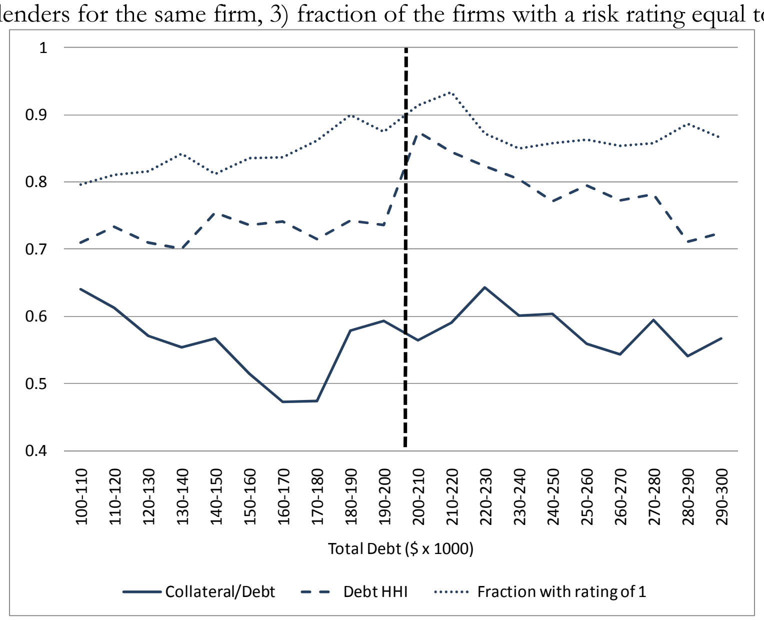


Table 1

Descriptive Statistics, March 1998 Cross Section (before Expansion Announcement)

Summary statistics of the cross section of firms and firm-bank relationships in our sample in 1998: firms with total debt between \$175,000 and \$225,000 and risk ratings of 1 and 2 before April 1998. There are 1,006 (1,786) firms (relationships) in the sample, 160 (349) in the treatment group and 846 (1,437) in the control. For Panel B there are 95 (186) firms (relationships) in the sample, 31 (70) in the treatment group and 64 (116) in the control. We provide three measures of debt concentration: number of lenders with which the firm has a positive amount of debt outstanding, firm debt HHI (sum of the squared fractions of debt from each lender), and fraction of debt from the lender that provides the largest amount of credit. Risk ratings are assigned by each lender to a firm, and are integer between 1 (best) and 5 (worst), although only firms with ratings of 1 and 2 are in the sample. A rating of 1 represents a firm in good standing with no potential repayment problems. A rating of 2 represents a firm with some (not severe) potential repayment problems. A rating of 3 is assigned to borrowers whose assessed potential default risk is high, or when the borrower has interest payments in arrears in excess of 90 days or requires principal refinancing. Ratings of 4 and 5 are mechanically determined by the repayment status of the borrower (i.e., missed principal repayment, more than 180 days in arrears, bankruptcy filings, collateral seized). For firms with multiple lenders, we report the standard deviation of the ratings assigned by different lenders to the same firm.

Sample	**All**			**Treatment Firms**			**Control Firms**		
	mean	median	sd	mean	median	sd	mean	median	sd
Panel A: All Firms									
				Firm level statistics					
Total debt ('000)	203.3	204.4	12.5	189.3	190.9	8.2	204.8	205.6	12.0
Number of lenders	1.70	1.00	0.94	2.40	2.00	1.29	1.62	1.00	0.87
Debt concentration (HHI)	0.90	1.00	0.17	0.72	0.69	0.22	0.92	1.00	0.15
Fraction debt from lead bank	0.93	1.00	0.13	0.80	0.81	0.18	0.94	1.00	0.12
Collateral/Debt	0.61	0.83	0.41	0.55	0.65	0.38	0.62	0.85	0.42
Average risk rating	1.10	1.00	0.37	1.18	1.00	0.39	1.10	1.00	0.37
Std. Dev. of same firm ratings (*)	0.14	0.00	0.39	0.21	0.00	0.41	0.13	0.00	0.39
				Relationship level statistics					
Debt ('000)	119.9	157.4	88.1	78.8	54.0	68.4	126.4	180.7	89.1
Risk rating	1.11	1.00	0.46	1.22	1.00	0.57	1.10	1.00	0.44
Panel B: Firms with at least one rating of 2 before April 1998									
				Firm level statistics					
Total debt ('000)	203.5	204.4	12.9	185.7	183.2	7.8	207.5	207.7	10.2
Number of lenders	2.04	2.00	1.14	2.70	2.00	1.95	1.89	2.00	0.83
Debt concentration (HHI)	0.86	0.98	0.19	0.74	0.73	0.24	0.88	0.98	0.17
Fraction debt from lead bank	0.90	0.99	0.14	0.82	0.84	0.19	0.92	0.99	0.12
Collateral/Debt	0.66	0.80	0.38	0.68	0.71	0.26	0.65	0.84	0.40
Average risk rating	1.67	1.50	0.56	1.65	1.75	0.55	1.68	1.50	0.56
Std. Dev. of same firm ratings (*)	0.61	0.71	0.54	0.69	0.79	0.52	0.59	0.64	0.56
				Relationship level statistics					
Debt ('000)	100.0	77.1	87.9	68.8	34.8	70.3	109.8	141.0	90.9
Risk rating	1.65	1.00	0.85	1.81	2.00	0.88	1.60	1.00	0.83

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

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

Estimated difference-in-differences (DD) effect of the registry expansion announcement (interim period) and public information (post-expansion period) on (log) debt levels, using specification (1):

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

Sample: firms with total debt between $175,000 and $225,000 before April 1998, and whose highest (worst) risk rating during the pre-announcement period is a 2, and with at least one rating of 2 (firms with only good ratings excluded). Columns 1 through 5 are estimated over the subsample of firms with multiple lenders, and column 6 on the subsample with a single lender, before the expansion announcement. Dependent variables: (log) debt of borrower *i* at time *t* with all banks (columns 1, 3 and 6), debt with the banks that assigned the worst rating (columns 2 and 4), and debt with the banks that assigned the best rating (column 5). Right-hand side variable of interest: interaction between a dummy equal to one if borrower *i* was in the treatment group (information not shared before registry expansion), and a month dummy. Coefficients γ_t represent the monthly (log) debt of firms in the treatment group relative to firms in the control (reported in Internet Appendix for brevity). DD estimates are obtained by subtracting from each coefficient γ_t 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 the difference is equal to zero. *, **, and *** indicate test statistically significant at the 10%, 5%, and 1% level.

Subsample: Highest (Worst) Risk Rating before April	2		2 (at least one 1)			2 (single lender)
Dependent Variable	ln($Debt_{it}$)	ln(Debt from Banks w/ Rating = 2 $_{it}$)	ln($Debt_{it}$)	ln(Debt from Banks w/ Rating = 2 $_{it}$)	ln(Debt from Banks w/ Rating = 1 $_{it}$)	ln($Debt_{it}$)
	(1)	(2)	(3)	(4)	(5)	(6)
Interim Period						
DD Estimate: Effect on Debt Level	0.020	-0.155**	0.019	-0.226**	0.021	-0.032
on 05-98 (γ_1-γ_{Pre})	(0.061)	(0.069)	(0.066)	(0.096)	(0.077)	(0.061)
DD Estimate: Effect on Debt Level	-0.047	-0.294*	-0.009	-0.388*	-0.024	-0.069
on 06-98 (γ_2-γ_{Pre})	(0.111)	(0.174)	(0.139)	(0.231)	(0.139)	(0.095)
Post-Expansion (Short Run)						
DD Estimate: Effect on Debt Level	-0.196*	-0.416**	-0.233	-0.487**	-0.151	-0.096
on 07-98 (γ_3-γ_{Pre})	(0.118)	(0.185)	(0.180)	(0.241)	(0.148)	(0.085)
DD Estimate: Effect on Debt Level	-0.254**	-0.428**	-0.351*	-0.528**	-0.269*	0.036
on 08-98 (γ_4-γ_{Pre})	(0.127)	(0.190)	(0.201)	(0.257)	(0.161)	(0.208)
First Differenced Estimation	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects and Trends	Yes	Yes	Yes	Yes	Yes	Yes
Month Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations (Firm-Month)	1,654	1,585	993	993	993	501
Clusters (Firms)	95	94	69	69	69	36
R-squared	0.12	0.15	0.11	0.21	0.22	0.23

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

Estimated difference-in-differences (DD) effect of the registry expansion announcement (interim period) and public information (post-expansion period) on default hazard rates, using specification (2):

$$1\left[Default_{it}=1\,|\,Default_{it-1}=0\right]_{it}=\xi_t+\sum_{m=-2}^{12}\lambda_m.Treat_i.I(m=t)_t+\zeta_{it}$$

Sample: firms with total debt between $175,000 and $225,000 before April 1998, and whose highest (worst) risk rating during the pre-announcement period is a 2, and with at least one rating of 2 (firms with only good ratings excluded). Columns 1 through 5 are estimated over the subsample of firms with multiple lenders, and column 6 on the subsample with a single lender, before the expansion announcement. Dependent variables: conditional default of borrower *i* at time *t* with any bank (columns 1, 3 and 6), default with the banks that assigned the worst rating (columns 2 and 4), and default with the banks that assigned the best rating (column 5). Each λ_t represents the difference in monthly default hazard rate between treatment (affected by registry expansion) and control firms (reported in Internet Appendix for brevity). DD estimates obtained by subtracting from each coefficient λ_t the average coefficients in the pre-expansion period, λ_{-2}, λ_{-1}, and λ_0 (February through April 1998). Statistical significance of the DD estimates 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.

Subsample: Highest (Worst) Risk Rating before April	2		2 (at least one 1)			2 (single lender)
Dependent Variable: 1 if relationship in default at *t*, not in default at *t-1*	Default with any bank	Default with bank w/ Rating = 2	Default with any bank	Default with bank w/ Rating = 2	Default with bank w/ Rating = 1	Default with any bank
	(1)	(2)	(3)	(4)	(5)	(6)
Interim Period						
DD Estimate: Effect on Default Hazard	0.168***	0.111*	0.093	0.122*	0.086	0.033
on 05-98 (λ_1-λ_{Pre})	(0.065)	(0.052)	(0.089)	(0.066)	(0.070)	(0.089)
DD Estimate: Effect on Default Hazard	0.030	-0.030	-0.028	-0.032	0.097	-0.028
on 06-98 (λ_2-λ_{Pre})	(0.052)	(0.033)	(0.053)	(0.045)	(0.063)	(0.053)
Post-Expansion (Short Run)						
DD Estimate: Effect on Default Hazard	0.084*	0.060	0.118	0.020	0.039**	0.118
on 07-98 (λ_3-λ_{Pre})	(0.046)	(0.044)	(0.102)	(0.026)	(0.019)	(0.103)
DD Estimate: Effect on Default Hazard	0.110*	0.049	0.148	0.061	0.026	0.148
on 08-98 (λ_4-λ_{Pre})	(0.064)	(0.048)	(0.114)	(0.074)	(0.022)	(0.114)
Month Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations (Firm-Month)	1,654	1,585	993	993	993	501
Clusters (Firms)	95	94	69	69	69	36
R-squared	0.21	0.18	0.21	0.18	0.25	0.18

Table 4
Unconditional Effect of Registry Expansion on Credit Outcomes

Estimated difference-in-differences (DD) effect of the registry expansion announcement (interim period) and public information (post-expansion period) on (log) debt levels using specification (1) (columns 1 through 3), and default hazard rates using specification (2) (columns 4 through 6). Sample: firms with total debt between $175,000 and $225,000 before April 1998, and whose highest (worst) risk rating during the pre-announcement period is a 2. The sample is the same as in Tables 2 and 3, but also includes firms with only good ratings (rating=1) before the registry expansion. We report coefficients every quarter during the post-expansion period for brevity, and as in Tables 2 and 3, the estimated coefficients from which the DD estimates are obtained are shown in the Internet Appendix. Statistical significance of the DD estimates based on Wald test of null that difference is equal to zero. *, **, and *** indicate test statistically significant at the 10%, 5%, and 1% level, respectively.

Dependent Variable	$\ln(Debt_{it})$			1 if relationship in default at t with any bank, not in default at t-1		
Subsample: # lenders pre-expansion	All	Multiple Lenders	Single Lender	All	Multiple Lenders	Single Lender
	(1)	(2)	(3)	(4)	(5)	(6)
Interim Period						
DD Estimate: Effect on Dependent	0.02	-0.004	0.054	0.026*	0.043**	0.001
Variable on 05-98 (λ_1-λ_{Pre})	(0.024)	(0.028)	(0.042)	(0.014)	(0.019)	(0.022)
DD Estimate: Effect on Dependent	0.006	0.01	0.027	0.038**	0.057***	0.006
Variable on 06-98 (λ_2-λ_{Pre})	(0.036)	(0.048)	(0.060)	(0.017)	(0.022)	(0.023)
Post-Expansion (Long Run)						
DD Estimate: Effect on Dependent	-0.025	-0.04	0.02	0.022	0.002	0.048
Variable on 07-98 (λ_3-λ_{Pre})	(0.041)	(0.053)	(0.065)	(0.014)	(0.010)	(0.034)
DD Estimate: Effect on Dependent	-0.023	-0.06	0.045	0.013	0.01	0.024
Variable on 10-98 (λ_6-λ_{Pre})	(0.050)	(0.066)	(0.067)	(0.013)	(0.015)	(0.026)
DD Estimate: Effect on Dependent	-0.099**	-0.144**	-0.005	0.004	0.003	0.004
Variable on 01-99 (λ_9-λ_{Pre})	(0.045)	(0.061)	(0.066)	(0.011)	(0.014)	(0.015)
DD Estimate: Effect on Dependent	-0.081**	-0.101**	-0.032	0.023	0.03*	0.024
Variable on 04-99 (λ_{12}-λ_{Pre})	(0.034)	(0.048)	(0.040)	(0.016)	(0.018)	(0.032)
First Differenced Estimation	Yes	Yes	Yes			
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Specific Trends	Yes	Yes	Yes			
Month Dummies	Yes	Yes	Yes	Yes	Yes	Yes
In sample after default?				No	No	No
Observations (Firm-Month)	16,859	8,686	8,173	14,346	7,234	7,112
Clusters (Firms)	1,006	505	501	1,006	505	501
R-squared	0.11	0.12	0.10	0.18	0.18	0.17

Table 5
Effect of Registry Expansion on Debt Growth Distribution

Estimated effect of the registry expansion announcement (interim period) and public information (post-expansion period) on debt growth rate quantiles ψ_τ, that minimize the weighted check functions of the residuals of the following specification:

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

Sample: firms with total debt between $175,000 and $225,000 before April 1998, and whose highest (worst) risk rating during the pre-announcement period is a 2. Dependent variable: percentage total debt growth of firm *i* at time *t*. A ψ_τ is estimated for every month, and represents the difference in the τ-th percentile of debt growth at month t between the treatment and the control firms (reported in the Internet Appendix for brevity). The reported estimate is the difference between each quantile ψ_τ after April 1998 and the average quantile in the pre-expansion period (February through April 1998). We report estimated differences every quarter during the post-expansion period for brevity. Statistical significance is based on Wald test of null that linear combination of quantiles is equal to zero (based on bootstrapped standard errors with 400 repetitions for ψ_τ). *, **, and *** indicate test statistically significant at the 10%, 5%, and 1% level, respectively. The unconditional debt growth quantiles for the pre-expansion sample are reported at the bottom of the table.

Dependent Variable	$(Debt_{it} - Debt_{it-1}) / Debt_{it-1}$				
Debt Growth Quantile	5%	10%	50%	90%	95%
	(1)	(2)	(3)	(4)	(5)
Interim Period					
DD Estimate: Effect Debt Growth Quantile	0.040	0.009	0.004	-0.200***	-0.399**
on 05-98 (ψ_1-ψ_{Pre})	(0.120)	(0.031)	(0.007)	(0.072)	(0.160)
DD Estimate: Effect Debt Growth Quantile	-0.025	0.023	0.007	-0.177***	-0.317***
on 06-98 (ψ_2-ψ_{Pre})	(0.130)	(0.080)	(0.006)	(0.062)	(0.101)
Post-Expansion (Long Run)					
DD Estimate: Effect Debt Growth Quantile	-0.082	-0.021	-0.004	-0.175***	-0.311**
on 07-98 (ψ_3-ψ_{Pre})	(0.149)	(0.067)	(0.008)	(0.060)	(0.126)
DD Estimate: Effect Debt Growth Quantile	0.001	0.008	0.004	-0.260***	-0.362***
on 10-98 (ψ_6-ψ_{Pre})	(0.080)	(0.043)	(0.007)	(0.071)	(0.123)
DD Estimate: Effect Debt Growth Quantile	0.052	0.031	0.000	-0.186***	-0.368***
on 01-99 (ψ_9-ψ_{Pre})	(0.077)	(0.031)	(0.007)	(0.060)	(0.114)
DD Estimate: Effect Debt Growth Quantile	0.082	0.069**	0.006	-0.168***	-0.233
on 04-99 (ψ_{12}-ψ_{Pre})	(0.058)	(0.032)	(0.007)	(0.060)	(0.181)
Month Dummies	Yes	Yes	Yes	Yes	Yes
Observations (Firm-Month)	8,686	8,686	8,686	8,686	8,686
Pre-Expansion Quantiles	5%	10%	50%	90%	95%
All firms	-0.201	-0.119	-0.004	0.130	0.255
Affected firms	-0.231	-0.159	-0.003	0.276	0.411
Control firms	-0.201	-0.115	-0.005	0.080	0.186

Table 6
Effect of Registry Expansion on Firm Debt Concentration

Estimated difference-in-differences (DD) effect of the registry expansion announcement (interim period) and public information (post-expansion period) on debt concentration, using specification (1):

$$y_{it} = \alpha_i + \xi_t + \delta_i t + \sum_{m=-2}^{12} \gamma_m .Treat_i I(m=t)_t + \varepsilon_{it}$$

Sample: Sample: firms with total debt between $175,000 and $225,000 before April 1998, and whose highest (worst) risk rating during the pre-announcement period is a 2, 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. Right-hand side variable of interest: interaction between a dummy equal to one if borrower *i* was in the treatment group (information not shared before registry expansion), and a month dummy. Coefficients γ_t represent the difference in the debt concentration between treatment and control firms in month *t* (reported in Internet Appendix for brevity). DD estimates are obtained by subtracting from each coefficient γ_t the average coefficients in the pre-expansion period, γ_{-2}, γ_{-1} , and γ_0 (February through April 1998). We report estimated DD every quarter during the post-expansion period for brevity. Statistical significance of DD estimates based on Wald test of null that the difference is equal to zero. *, **, and *** indicate test statistically significant at the 10%, 5%, and 1% level.

Dependent Variable	ln(#Lenders$_{it}$)	DebtHHI$_{it}$	%TopLender$_{it}$
	(1)	(2)	(3)
Interim Period			
DD Estimate: Effect on Dependent Variable on 05-98 (γ_1-γ_{Pre})	0.021	0.006	0.005
	(0.021)	(0.010)	(0.009)
DD Estimate: Effect on Dependent Variable on 06-98 (γ_2-γ_{Pre})	0.031	0.024**	0.020*
	(0.025)	(0.012)	(0.011)
Post-Expansion (Long Run)			
DD Estimate: Effect on Dependent Variable on 07-98 (γ_3-γ_{Pre})	0.037	0.032***	0.023*
	(0.028)	(0.014)	(0.012)
DD Estimate: Effect on Dependent Variable on 10-98 (γ_6-γ_{Pre})	-0.015	0.060***	0.044***
	(0.031)	(0.017)	(0.014)
DD Estimate: Effect on Dependent Variable on 01-99 (γ_9-γ_{Pre})	-0.062	0.077***	0.057***
	(0.041)	(0.020)	(0.016)
DD Estimate: Effect on Dependent Variable on 04-99 (γ_{12}-γ_{Pre})	-0.105**	0.110***	0.083***
	(0.046)	(0.025)	(0.019)
First Differenced Estimation	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Firm Specific Trends	Yes	Yes	Yes
Month Dummies	Yes	Yes	Yes
Observations (Firm-Month)	8,686	8,686	8,686
Clusters (Firms)	505	505	505
R-squared	0.22	0.17	0.17

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

Estimated difference-in-differences (DD) effect of the registry expansion on the correlation across lending decisions of different banks to the same firms using the following specification:

$$\ln\left(Debt_{ijt}\right) = \alpha_{ij} + \delta_t + \tau_i t + \sum_{m=-2}^{12} \beta_{1\_m}.\ln\left(TDebt_{i(-j)t}\right).I\left(m=t\right)_t + \sum_{m=-2}^{12} \beta_{2\_m}.\ln\left(TDebt_{i(-j)t}\right).Treat_i.I\left(m=t\right)_t + \varepsilon_{it}$$

Sample: Sample: firms with total debt between \$175,000 and \$225,000 before April 1998, and whose highest (worst) risk rating during the pre-announcement period is a 2, and multiple lenders before April 1998. The dependent variable is the (log) debt by firm *i* with bank *j* at month *t*. The right hand side variable of interest is the log of the total debt of firm *i* with all other lenders except *j* at time *t*, interacted with a dummy equal to one if firm *i* is in the treatment group, and interacted with month dummies. The coefficients β_{2_t} measure the difference in the contemporaneous partial correlation of the changes in debt across all the lenders of firm *i* at month *t* (reported in the Internet Appendix for brevity). We estimate by first differencing over two months to reduce the noise inherent in monthly lending changes. Column 2 shows the estimates when banks lagged one month to eliminate the mechanical correlation across observations for the same firm at month *t*. DD estimates are obtained by subtracting from each coefficient γ_t the average coefficients in the pre-expansion period, γ_{-2}, γ_{-1} , and γ_0 (February through April 1998). We report estimated DD every quarter during the post-expansion period for brevity. Statistical significance of DD estimates based on Wald test of null that the difference is equal to zero. *, **, and *** indicate test statistically significant at the 10%, 5%, and 1% level.

Dependent Variable	ln(Total Debt from Banks other than j_{ijt})	ln(Total Debt from Banks other than j_{ijt+1})
	(1)	(2)
Interim Period		
DD Estimate: Effect on Dependent	0.002	0.008
Variable on 05-98 (γ_1-γ_{Pre})	(0.014)	(0.016)
DD Estimate: Effect on Dependent	0.104	0.072
Variable on 06-98 (γ_2-γ_{Pre})	(0.099)	(0.084)
Post-Expansion		
DD Estimate: Effect on Dependent	0.162***	0.187***
Variable on 07-98 (γ_3-γ_{Pre})	(0.062)	(0.073)
DD Estimate: Effect on Dependent	0.02	0.059
Variable on 08-98 (γ_4-γ_{Pre})	(0.059)	(0.052)
DD Estimate: Effect on Dependent	-0.039	-0.005
Variable on 09-98 (γ_5-γ_{Pre})	(0.049)	(0.050)
DD Estimate: Effect on Dependent	-0.016	-0.02
Variable on 10-98 (γ_6-γ_{Pre})	(0.021)	(0.031)
First Differenced Estimation (2 months)	Yes	Yes
Debt x Month Dummies	Yes	Yes
Firm specific trends	Yes	Yes
Firm Fixed Effects	Yes	Yes
Bank-Month dummies	Yes	Yes
Observations (firm-bank-months)	20,306	20,306
Clusters (firms)	495	495
R-squared	0.04	0.04

Table A2: Placebo Tests

The placebo tests replicate the difference-in-differences (DD) estimations on (log) debt from Table 4 over different samples. Placebo 1: Estimates assuming registry expansion announcement occurred in April 1999, one year after actual expansion. Sample: Firms with total debt between \$175,000 and \$225,000 between January and March 1999, and whose highest (worst) risk rating between January and March 1999 is a 2. Placebo 2: Estimates using fake registry cutoff rule at \$300,000**.** Sample: Firms with total debt between \$275,000 and \$325,000 before April 1998, and whose highest (worst) risk rating during the pre-announcement period is a 2. Statistical significance of the difference-in-differences (DD) estimates based on Wald test of null that difference is equal to zero. Robust standard errors clustered at the borrower level. None of the DD estimates is statistically significant at the standard levels of confidence.

	$\ln(Debt_{it})$											
Sample:	Placebo 1: Assuming Expansion Occurred in 1999						Placebo 2: Assuming Pre-Expansion Debt Cutoff at \$300,000					
# of lenders before April	All			Multiple Lenders			All			Multiple Lenders		
Maximum Risk Rating before April	1 or 2	1	2	1 or 2	1	2	1 or 2	1	2	1 or 2	1	2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Interim Period												
DD Estimate: Effect on Debt Level	0.022	0.024	0.017	0.024	0.028	0.008	-0.006	-0.002	-0.035	-0.004	-0.002	-0.017
on 05-98 (γ_1-γ_{Pre})	(0.011)	(0.012)	(0.021)	(0.012)	(0.013)	(0.026)	(0.030)	(0.034)	(0.030)	(0.033)	(0.038)	(0.041)
DD Estimate: Effect on Debt Level	0.021	0.022	0.021	0.022	0.026	0.008	-0.009	0.002	-0.082	-0.007	0.003	-0.074
on 06-98 (γ_2-γ_{Pre})	(0.013)	(0.014)	(0.022)	(0.014)	(0.016)	(0.027)	(0.033)	(0.037)	(0.045)	(0.038)	(0.043)	(0.061)
Post-Expansion												
DD Estimate: Effect on Debt Level	0.001	0.004	-0.010	0.005	0.012	-0.024	0.044	0.065	-0.104	0.056	0.077	-0.077
on 07-98 (γ_3-γ_{Pre})	(0.014)	(0.016)	(0.026)	(0.017)	(0.019)	(0.031)	(0.046)	(0.052)	(0.060)	(0.057)	(0.065)	(0.081)
DD Estimate: Effect on Debt Level	-0.029	-0.031	-0.014	-0.017	-0.015	-0.028	0.051	0.082	-0.160	-0.022	0.015	-0.256
on 10-98 (γ_6-γ_{Pre})	(0.018)	(0.020)	(0.027)	(0.020)	(0.023)	(0.031)	(0.052)	(0.057)	(0.137)	(0.062)	(0.068)	(0.143)
DD Estimate: Effect on Debt Level	-0.008	-0.010	-0.003	0.012	0.009	0.019	0.048	0.045	0.063	0.017	0.018	-0.005
on 01-99 (γ_9-γ_{Pre})	(0.019)	(0.022)	(0.029)	(0.023)	(0.026)	(0.035)	(0.044)	(0.048)	(0.109)	(0.061)	(0.066)	(0.146)
DD Estimate: Effect on Debt Level	0.003	-0.002	0.024	0.012	0.006	0.035	0.001	-0.004	0.022	-0.014	-0.013	-0.041
on 04-99 (γ_{12}-γ_{Pre})	(0.016)	(0.018)	(0.033)	(0.018)	(0.020)	(0.042)	(1.000)	(1.000)	(1.000)	(1.000)	(1.000)	(1.000)
First Differenced Estimation	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects and Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations (firm-months)	83,306	70,019	13,287	60,691	50,143	10,548	22,447	19,456	2,991	12,498	10,734	1,764
Clusters (firms)	4,769	4,022	747	3,424	2,835	589	1,335	1,162	173	724	623	101
R-squared	0.16	0.16	0.16	0.16	0.16	0.16	0.17	0.17	0.17	0.17	0.17	0.19