

PUBLIC INFORMATION AND COORDINATION: EVIDENCE FROM A CREDIT REGISTRY EXPANSION

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Abstract

This paper provides evidence that lenders to a firm close to distress have incentives to coordinate: lower financing by one lender reduces firm creditworthiness and causes other lenders to reduce financing. To isolate the coordination channel from lenders' joint reaction to new information, we exploit a natural experiment that made lenders' negative private assessments about their borrowers public. We show that lenders, while learning nothing new about the firm, reduce credit in anticipation of the reaction by other lenders to the same firm. The results show that public information exacerbates lender coordination and increases the incidence of firm financial distress.

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Creditors to the same firm have an incentive to coordinate when the firm is close to financial distress. A creditor has less incentive to provide additional financing if it believes that other creditors to the same firm are about to liquidate their claims and potentially disrupt the firm's operations. These coordination incentives may have first order effects on equilibrium credit and default. For example, they can lead to creditor runs whereby each creditor withdraws financing due to the expectation other creditors will do the same, although collectively creditors would be better off by lending. Modern bankruptcy code is designed to alleviate this type of creditor coordination problems in distress (Jackson 1986).

Despite the importance of lender coordination incentives for theory and policy, there is to date no direct evidence of its empirical relevance.¹ The main difficulty in providing such evidence is that the coordination motive can be confounded with the arrival of news. For example, the inability of Bear Sterns and Northern Rock to secure short term financing at the beginning of the 2007-2008 crisis is interpreted as anecdotal evidence of creditor runs (Brunnemeier 2009, Shin 2009). However, it is difficult to ascertain whether the lending shortage occurred because lenders were anticipating each other's withdrawals, or because of the release of news about the deteriorating fundamentals of these financial institutions.

The present paper provides evidence that lenders to the same firm have incentives to coordinate. We exploit a credit market intervention as a natural experiment that resembles key aspects of the following ideal laboratory setting. Two asymmetrically informed banks lend to the same firm. Bank A has bad news about the firm that B does not have. The experiment makes bank A's bad news available to B. Since A learns nothing from B, A's lending will not change in the absence of coordination motives. In contrast, any change in A's lending is due to its anticipation of the expected reaction by B to the bad news.

The natural experiment comes from the expansion of the Public Credit Registry in Ar-

¹Indirect evidence of coordination motives among creditors has been shown by Asquith, Gertner, and Scharfstein (1994) and Brunner and Karhnen (2008), and among investors in mutual funds by Chen, Goldstein, and Jian (2009).

gentina 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 required banks to release information retroactive to January 1998, but its implementation was delayed. As a result we identify three periods in the registry data: 1) a pre-announcement period, when banks reported information to the Central Bank under the presumption it would remain private, 2) an interim period after the reform announcement in April 1998 and before its implementation in July of the same year, during which lenders knew information they reported in the pre-expansion period would become public, but they had not yet received other lenders' information, and 3) a post-expansion period, when banks make lending and reporting decisions having observed the previous reports of other banks.

This setting allows us to replicate the ideal experiment described above. We use the risk ratings banks report *before* the expansion announcement as a proxy for each bank's prior about firm creditworthiness. During the interim period, lenders know their private assessment will become public and have not obtained additional information from the registry. We identify lender coordination incentives by measuring the lending reaction during the interim period.

To estimate the causal effect of the expansion announcement on debt 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 use 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 \$150,000 and \$200,000, relative to borrowers whose pre-expansion debt was between \$200,000 and \$250,000.

We find that lenders react strongly to private information they already possess about a borrower upon the announcement that this information will become public. The reaction occurs only when the information is likely to have a significant effect on the amount of lending by the firm's other creditors. This occurs in our context when a bank possesses bad news about a firm that borrows from multiple banks. During the interim period the registry expansion announcement causes a 19.8% 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 does not decline until the post-expansion period, when another bank's bad rating becomes public.

We also show that the decline in lending during the interim period causes financial distress: firms with a poor rating in the pre-announcement period experience a 5.6 percentage point increase in the default hazard rate during the two-month interim period. The registry expansion announcement has no effect on firm debt or default in the subsample of borrowers with a single lender, for which lender coordination incentives are absent.

We use the post-expansion period to characterize the resulting credit market equilibrium when lender coordination is exacerbated by public information. Firms with initially perfect credit records whose information became public experience, on average, a permanent 6.9% decline in debt. Again, this effect is present only among firms with multiple lenders and are thus susceptible to lender coordination problems. We also find that 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, and are consistent with a stylized model where lenders have incentives

to coordinate.²

The results demonstrate that when lenders have an incentive to coordinate, their actions are more sensitive to public than to private information. This occurs because public information better forecasts the actions of other lenders. This publicity multiplier of information is a feature present in theoretical accounts of creditor runs (Morris and Shin 2004), bank runs (Goldstein and Pauzner 2005), borrower runs (Bond and Rai 2009), currency attacks (Morris and Shin 1998; Hellwig et al. 2006), financial crises (Goldstein 2005), political action (Edmond 2008), monetary policy (Morris and Shin 2002b), and asset price volatility (Ozdenoren and Yuan 2008).

The publicity multiplier is also a practical concern for policymakers in banking regulation, central banking, and securities regulation. 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.”³ Following a similar logic, the Term Auction Facility (TAF) was created in 2007 to allow banks to borrow from the Federal Reserve without being observed by the bank’s other creditors (Cecchetti 2009). The present paper provides the first direct evidence of the publicity multiplier of information outside a laboratory environment (Cornand and Heinemann 2009). We identify the publicity multiplier by demonstrating that a lender’s reaction to public news is larger than its reaction if the *same* news were private. Thus, this paper contributes to

²Information 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).

³Quote 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)

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).

The rest of the paper proceeds as follows. Section I describes both the institutional environment and the data, and provides a brief history of Argentina’s registry expansion. In Section II, 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 III outlines the empirical strategy. Sections IV provides evidence of lender coordination motives and Section V studies the long run effect of public information on creditor coordination. Section VI concludes.

I. 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). In the analysis, any firm with a rating

of 4 or 5 is classified to be in default. 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 exceeds \$200,000 at any time during the prior 12 months.⁴ 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.⁵

⁴Argentina's currency board imposed a one-to-one peg of the local currency to the U.S. dollar throughout the analysis period.

⁵There 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).

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).⁶ 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 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. Two pieces of evidence indicate that, in practice, the transition to the new technology faced delays. 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. Also, a media search produced no mention before July 1998 of the registry expansion. 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

⁶See Central Bank Communication A2686 dated April 14, 1998 (URL: <http://www.bcra.gov.ar>).

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.⁷

II. Framework: Information Sharing and Lender Coordination

We present a stylized theoretical framework motivated by the features of our empirical environment. The purpose of the model is to show that when lenders to the same firm have incentives to coordinate, information sharing will lead creditors to change the way they use their own information when allocating credit. Absent the coordination incentive, creditors in our model will not change their credit supply when information is made public.

A. Set-Up

Two banks lend to the same firm. $L_i \geq 0$ is the amount lent by bank i . The gross interest rate on each loan is fixed and assumed to be $R \geq 1$. To simplify the analysis we assume that banks are unable to adjust this interest rate either due to competition or because doing so will exacerbate moral hazard or adverse selection problems (Stiglitz and Weiss 1981). A loan either pays off RL_i or the firm defaults on the loan in which case it pays zero.

The probability that a bank's loan is repaid is $\theta_i L_j$. The first term in this probability, θ_i , captures heterogeneity in the creditworthiness of each bank's loan. Each bank's θ_i is independently drawn from a probability distribution $f(\theta) \geq 0$ over the positive real numbers.

⁷Note 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 domain of $f(\theta)$ is limited to values that ensure the probability of repayment is not greater than one. Each bank knows its own realization of θ_i . We model the effect of information sharing in the following way. If information is (not) shared then θ_j is (not) also known by bank i . The second term is the amount lent by bank j . This term implies that the probability that bank i will receive its payoff is increasing in the amount of credit extended by bank j . This captures each bank's incentive to coordinate: if one bank lowers the amount it is willing to lend this can disrupt the operations of the firm and hence lower the firm's ability to pay its other loans.

The cost to bank i of extending a loan is $\frac{1}{\delta}L_i^\delta$ and we assume that $\delta > 2$ so as to ensure finite solutions. The increasing marginal cost of lending reflects costs of bearing risk or costs of additional monitoring that are required for larger loans. Each bank chooses its amount of lending simultaneously and we look for a Nash equilibrium in lending choices.

The objective of bank i is

$$\max_{L_i \geq 0} (\theta_i E_i(L_j)) \times L_i R - \frac{1}{\delta} L_i^\delta$$

where $E_i(L_j)$ is bank i 's expectation of the amount that bank j will lend and is formed according to whether information is shared or not. Crucially, when information is not shared $E_i(L_j)$ is a constant determined only by $f(\theta)$ and does not depend on bank i 's realization of θ_i . If banks did not have an incentive to coordinate (e.g. the payment probability was θ_i) then the optimal solution to this problem will be invariant to whether or not information is shared.

B. *Publicity Multiplier*

The optimal choice of lending for bank i is characterized by the following first order condition

$$\theta_i E_i(L_j) R - L_i^{\delta-1} = 0.$$

The first order condition shows that bank i 's optimal level of lending is increasing in the creditworthiness of its loan θ_i and, due to the complementarity, increasing in the expected level of lending of bank j . The publicity multiplier can be understood by examining how the elasticity of the equilibrium level of lending with respect to loan creditworthiness, η , is affected by information sharing. Let L_i^S and L_i^{NS} (η^S and η^{NS}) denote bank i 's equilibrium level of lending (elasticity of lending to θ) with and without information sharing.

If information is not shared then there is only a direct effect of θ_i on L_i through the expected payoff to a dollar of lending. In this case the elasticity of L_i^{NS} with respect to θ_i is

$$\eta^{NS} = \frac{1}{\delta - 1}.$$

When information is shared the same direct effect is present and is amplified by the indirect effect that θ_i has on $E_i(L_j)$. In this case, an increase in θ_i is observed by bank j and will raise bank j 's choice of L_j , due to the incentive to coordinate. This in turn will increase lending by bank i , L_i , and so on. In this case the elasticity of L_i^S with respect to θ_i is

$$\eta^S = \Omega \frac{1}{\delta - 1}, \quad \text{where } \Omega \equiv \frac{1}{1 - \left(\frac{1}{\delta-1}\right)^2} > 1.$$

Here $\Omega > 1$ is the publicity multiplier that magnifies the initial direct effect. In the absence of a coordination motive, there is no reason for banks to change how they use their information to allocate credit when this information becomes public ($\eta^S = \eta^{NS}$). Thus, the empirical implication of coordination motives that we test is whether $\eta^S - \eta^{NS} > 0$. In words, we test in the data whether a bank's lending response to the same private information it holds about a firm's repayment prospects changes once it is announced this private information will become public.

III. Estimation and Descriptive Statistics

We exploit the cross-sectional variation induced by the preexisting \$200,000 eligibility threshold to identify the causal effect of the registry expansion announcement and subsequent release of information. 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 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 (treatment) and control groups by restricting the analysis sample to borrowers whose total debt was close to the \$200,000 threshold. A firm belongs to the treatment (control) group in our sample if it has total debt between \$150,000 and \$200,000 (\$200,000 and \$250,000) during January, February, and March 1998.

Note that the definition of both the treatment and control groups includes bounds from above and below. This assures that the results are not mechanically driven by asymmetries in the sample selection method. Also note that because the data is left-truncated at January 1998, our classification procedure can erroneously classify firms into the treatment group when they actually belong to the control. The misclassification occurs if the firm has total debt above \$200,000 before January 1998, but below that number between January and March. This classification error biases the results towards zero, since it implies that a fraction of the firms in the treatment group is not affected by the registry expansion. For this reason, our results must be interpreted as a lower bound estimate of the effect of public information. We explore the potential magnitude of the attenuation bias in the next section.⁸

Since only firms with a risk rating of 1 and 2 are affected by the registry expansion, we

⁸We thank the Associate Editor for highlighting these issues.

exclude from the sample all firms with a risk rating higher (worse) than 2 in January 1998. The two restrictions exclude firms with poor ratings or that had not obtained credit from the formal financial system before April 1998. Thus, the estimates are obtained from small firms with high expected creditworthiness relative to other borrowers in the population of similar characteristics.

The analysis sample's descriptive statistics prior to expansion announcement, March 1998 cross-section, are shown in Table 1 (Panel A). The subsample includes 1,715 borrowers with an average total debt of \$214,600. The median borrower has two lenders and a high collateral to debt ratio (0.61). Firms in our sample come from a wide range of industries. The most common are agricultural production (37.4%), services (16.5%), manufacturing (16.3%), and wholesale (15.7%).⁹

It is important to emphasize two features of the risk rating distribution in the analysis sample. First, note that banks report ratings of 2 even when they know this will be shared with other banks: 10% of firm-bank relationships in the control group in March 1998 have a rating of 2 (Table 1, panel A). Furthermore these ratings are informative for a firm's true creditworthiness: 14.9% of the relationships that had a 2 reported entered default within the next twelve months as compared to 8.89% 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. Since these ratings are used for prudential regulation, a bank that assigns a rating of 1 to all firms that are not in default would trigger scrutiny by the regulator. 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.

Second, we expect that only the report of 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. Over 93% of

⁹We have no further information on the sample firms beyond what is reported in the credit registry.

the bank-firm pairs in the full analysis sample have a rating of 1 in March 1998. Conditioning on having assigned a rating of 2, the probability that another lender to the same firm assigned a 1 is 70%. This implies that a rating of 1 contains little information in this subsample, and therefore we do not expect banks who have reported a 1 to display a measurable positive reaction when they learn this rating will be made public. We demonstrate with a numerical example in the Appendix that this intuition holds in the context of the theoretical framework of the previous section (see Panel A, Figure A1). For this reason, our main specifications are estimated on the subsample of firms that have at least one rating of 2 before the registry expansion. The descriptive statistics of this subsample are shown in Panel B of Table 1.

A. *Cross-Section*

Table 1 shows separately the descriptive statistics for treatment and control firms. The number of treatment firms is one fourth of the number of control firms. This jump in the borrower distribution is a concern if it is the result of self-selection of firms into the control group along firm characteristics that are related to changes in future credit demand and supply. We explore the differences across the two groups of firms in Figure 1, where we plot the number of firms and their average characteristics by total debt for the March 1998 cross section. The horizontal axis measures firm total debt in \$10,000 bins. Treatment (control) firms are to the left (right) of the \$200,000 threshold, highlighted by the vertical dashed line.

The plot confirms that there is a discrete jump in the density of firms just above the \$200,000 threshold. Despite this jump, the plot shows that firms above and below the threshold have similar collateral-to-debt ratios and risk ratings, both observable proxies for firm credit quality. Since loan contract characteristics and risk ratings capture differences between the treatment and control firms' credit quality that are observed by lenders, the patterns in this plot rule out self-selection along dimensions of credit quality that are unobservable to the econometrician, but observable by the lenders.

There is a discontinuity in firms' debt concentration around the threshold: firms in the

control group tend to concentrate their borrowing from fewer lenders relative to firms in the treatment group. This is a concern if firms with more concentrated debt distort borrowing decisions to put themselves above the threshold. This may occur if firms with more concentrated debt benefit more from having their information shared through the registry because, for example, this reduces informational hold-up by their lenders. This type of self-selection implies that the registry expansion will affect borrowing by control firms: the expansion eliminates the incentives to distort upwards borrowing by control firms and will thus bias the DD estimates upwards. We explore this possibility next.

B. Time-Series

Our main identification assumption is that the debt of firms affected by the expansion and in the control group would have evolved in a similar manner in the absence of the registry expansion. Figure 2 (Panel A) shows evidence that validates this assumption for our main analysis sample: firms with multiple lenders before the registry expansion and at least one rating of 2. It plots the time series of the average (log) debt of treatment and control firms, during the four months before the expansion announcement, the two-month interim period, and the four months after the registry expansion. Consistent with the identification assumption, average debt of treatment and control firms follow parallel trends before the expansion announcement. After the announcement, control firms' debt continues the pre-announcement trend, while treatment firms' debt declines. Figure 2, Panel B, shows the same plot for the subsample of firms with a single lender and a rating of 2 before the registry expansion announcement. It shows that debt of treatment and control firms with a single lender evolve in parallel both before and after the registry.

Two preliminary observations can be derived from these figures. First, the registry expansion does not appear to affect the total debt of control firms. This observation rules out the possibility raised above, that firm debt is distorted upwards to self-select into the control group. This suggests that the discrete jump in debt concentration observed in Figure 1 is

caused by the publicity of information. We corroborate this in Section V.

Second, the debt of treatment firms appears to decline during the interim period, suggesting that the *announcement* of the registry expansion affects lending and that this announcement was not anticipated. Further, the decline occurs only for firms that have multiple lenders before the announcement. Both of these observations, which we corroborate later with the DD estimates, are consistent with the coordination model derived in the previous section: making private information public, holding the information available constant, affects lending when a firm has multiple lenders with incentives to coordinate.

C. *Difference-in-Differences Specification*

The previous evidence establishes that credit outcomes of control firms represent a valid counterfactual for those of the treatment firms, and provides the rationale for a DD estimation based on the following borrower fixed-effects specification:

$$\ln(Debt_{it}) = \alpha_i + \xi_t + \gamma_{Interim} \cdot Treat_i \times Interim_t + \gamma_{Post} \cdot Treat_i \times Post_t + \varepsilon_{it}. \quad (1)$$

The dependent variable is the (log) debt of firm i at month t . The right-hand side includes firm fixed effects, which capture any time-invariant unobserved heterogeneity across borrowers, and calendar month dummies, which account for all macroeconomic and aggregate shocks that affect credit demand or supply. The dummy $Treat_i$ is equal to one if firm i is in the treatment group (i.e., the firm’s information becomes public after July 1998 due to the registry expansion). The specification includes the interactions between this variable and 1) a dummy equal to one during the interim period, $Interim_t$ (i.e., after the announcement of the registry expansion but before information in the registry becomes public), and 2) a dummy equal to one during the 12 months after the registry expansion, $Post_t$ (i.e., after information becomes public).

The coefficient on the first interaction, $\gamma_{Interim}$, is the DD estimate of the effect of making

information public on debt, holding available firm information constant. It measures how the difference between log debt of treatment and control firms changes during the interim period relative to the pre-announcement period. The coefficient on the second interaction, γ_{Post} , represents the DD estimate of the average effect of the actual registry expansion on debt during the 12 months after the expansion.

Specification (1) is estimated on an 18-month panel (4 pre-announcement, 2 interim, 12 post-expansion period months). All standard errors below are estimated allowing for clustering at the firm level to account for serial correlation in outcomes. Excluded for brevity, all the results are robust to narrowing the analysis sample to treatment (control) firms with debt between \$175,000 and \$200,000 (\$200,000 and \$225,000).¹⁰

IV. Results: Lender Coordination and Publicity Multiplier

We identify lender coordination motives and the publicity multiplier of information on the subsample of firms that had a rating of 2 assigned by at least one of its lenders before the registry expansion announcement. We analyze the subsample of firms with perfect credit records and the full sample in the next section.

A. Lending Response to Information

Table 2 shows the estimated effects of the registry announcement on debt using specification (1). We use three different dependent variables: (log) debt with bank(s) that assigned a rating of 2 during the pre-expansion period, (log) debt with banks that assigned only ratings of 1 during the same period, and (log) total firm debt.

¹⁰In the On-Line Appendix we show that narrowing the range of the analysis sample results in similar 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.

The expansion announcement has a significant and immediate negative effect on bank lending to firms with multiple lenders during the interim period, when the registry information has not yet become public. The point estimates indicate that a firm's debt with banks that assigned it a rating of 2, i.e., banks that possess bad news about the firm before the announcement, declines 19.8% during the interim period (Table 2, column 1). In contrast, the announcement's effect on debt with banks that did not possess bad news is statistically insignificant during this period (Table 2, column 2). Only after the information becomes public does debt with these banks drop significantly by 11.5%. The combined effect implies that total firm debt declines by 13.5% on average during the 12 months following the registry expansion (Table 2, column 3).

These findings are consistent with lender coordination motives and 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. The decline in lending occurs after the expansion is announced, but before information actually becomes public, which implies that the decline is not due to the arrival of additional information. There is no similar decline in lending by banks that had assigned a rating of 1. This is consistent with our claim that a rating of 1 contains very little information relative to the prior for our sample firms. Thus, a bank that assigned a rating of 1 does not anticipate any reaction from other lenders. This suggests that the immediate decline in lending after the registry expansion announcement by banks that assigned a rating of 2 occurs in anticipation of other lenders' reactions when the bad news becomes common knowledge. This anticipation is confirmed by the fact that debt by banks that assigned a rating of 1 drops after the bad news is released (Table 2, column 2).

We test directly whether the expansion announcement during the interim period has a different effect on lending by banks with good and bad news in the pre-expansion period. To do so we add a third difference to the DD specification (1): the change in lending by banks that assigned a rating of 2 relative to those that assigned a rating of 1. This leads to the

following differences-in-differences-in-differences specification:

$$\begin{aligned} \ln(Debt_{ijt}) = & \alpha_{ij} + \xi_{jt} + Treat_i \times \delta_t + \gamma_{Interim_Bank2} \cdot Treat_i \times Interim_t \times BankAssigned2_j \\ & + \gamma_{Post_Bank2} \cdot Treat_i \times Post_t \times BankAssigned2_j + \varepsilon_{ijt}. \end{aligned} \quad (2)$$

The dependent variable is the (log) debt of firm i at month t with the banks that assigned rating j (1 or 2) during the pre-expansion period. Note that the estimation panel contains two observations per firm each month, which results from stacking the firm-month panels used to estimate columns 1 and 2 of Table 2. The specification includes: 1) dummies for every firm-rating pair, α_{ij} , which accounts for within-firm differences in lending levels across banks with bad and good news, 2) dummies for every rating-time pair, ξ_{jt} , which accounts for any aggregate shocks that affect differentially lending by banks with good and bad news, and 3) an interaction between the treatment dummy and time dummies, which account for aggregate shocks that affect differentially borrowing by treatment and control firms.

The right-hand side variable of interest is the triple interaction between the treatment dummy, a dummy equal to one during the interim period, and a dummy equal to one if the banks assigned a rating of 2. The coefficient on this dummy is the proportional change in lending by banks that assigned a 2 relative to those that assigned a 1, before and after the expansion announcement and between the treatment and control firms. It measures the effect of the registry expansion announcement on the share of lending by banks that assigned a rating of 2 relative to banks that assigned a rating of 1 to the same firm and it is reported in Table 3. The point estimates indicate that lending by banks with bad news declines by 21.4% relative to lending by banks with good news to the same firm during the interim period, and the decline is statistically significant at the 5% level. In terms of the framework in Section II, the results imply that the elasticity of a bank's loan supply to bad news is larger when the bad news is public (or $\eta^S - \eta^{NS} > 0$).

Neither the expansion announcement nor the actual release of registry information has

a significant effect on debt of firms with a single lender before the expansion announcement (Table 2, column 4). This 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. The results rule out, for example, that lending declines because information sharing creates incentives to free ride on the information collected by other banks.

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 in the post-expansion period. This interpretation is at odds with the fact that the announcement of the registry expansion affects outcomes during the interim period. This 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

The rationale for lender coordination is that lower financing by one lender reduces firm creditworthiness when the firm is close to financial distress. We thus expect firm default probabilities to increase as a result of making bad news public, which we test next. The default specification compares the default hazard rate of firms affected by the registry expansion to

the hazard rate of control firms in a manner analogous to specification (1):

$$1[Default_{it} = 1 | Default_{it-1} = 0]_{it} = \xi'_t + \lambda_{Interim} \cdot Treat_i \times Interim_t + \lambda_{Post} \cdot Treat_i \times Post_t + \zeta_{it} \quad (3)$$

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 treatment firms and dummies equal to one during the interim period and during the 12 months after the registry expansion. The estimated coefficients on these interaction terms are shown in Table 4.

Firms experience a sharp and immediate increase in the probability of default: the hazard rate of defaults on any debt increases by 5.6 percentage points during the 2-month interim period (Table 4, column 1). Using the control firms' default hazard rate as a baseline for comparison, this point estimate implies that registry expansion announcement causes the monthly hazard rate to increase by 107% to 10.8% during these two months. 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. The default hazard also increases by 3 percentage points on average during the 12 month post-expansion period. Both estimates are significant only at the 10% level. The registry expansion has no statistically significant effect on the default probability of firms with a single lender (Table 4, column 2).

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.

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.

C. Placebo Tests

We perform placebo tests to verify that the sample selection procedure of firms into the treatment and control groups does not mechanically produce the above results. In the first placebo test, we shift forward by 12 months both the sample selection and the specification estimation. A firm belongs to the placebo treatment (control) group in our sample if it has total debt between \$150,000 and \$200,000 (\$200,000 and \$250,000) during January, February, and March 1999 instead of 1998. Similarly, the placebo pre-expansion, interim, and post-expansion periods are defined as if the registry expansion was announced in April 1999 instead of April 1998. Table 5, Panel A, shows the estimated parameters of specification (1) using this placebo setting. None of the DD estimates is significant in these tests.

We can also exploit this placebo setting to obtain a rough estimate of the fraction of firms that we classify into the treatment group that belong to the control (due to the left-truncation of the sample in January 1998). Of the 2,313 in the placebo treatment group, 1,212 (52.4%) had total debt above \$200,000 between January and December 1998. Under the assumption that the same misclassification ratio occurred in our analysis sample, the actual effect of public information on debt and default hazards are twice the estimates presented in Tables 2, 3, and 4. However, because the dynamics of debt growth change after the registry expansion, it is difficult to ascertain whether this provides a good approximation of the attenuation bias introduced by misclassification.

The stark difference in the number of firms in the treatment and control groups observed in Table 1 is not present in the placebo sample (2,313 in the placebo treatment, 2,435 in the placebo control). This suggests that the discrete jump in the firm distribution around \$200,000 observed in Figure 1 is caused by the discontinuity in the information sharing rule at this threshold. Consistent with interpretation, we show in the next section that information sharing affects average lending levels.

We perform a second placebo test where we shift upwards by \$100,000 the threshold around which we define the samples. A firm belongs to the placebo treatment (control) group in our sample if it has total debt between \$250,000 and \$300,000 (\$300,000 and \$350,000) during January, February, and March 1998. Again, none of the estimated parameters is significant when using this placebo sample (Table 5, Panel B). Overall, the results on both placebo tests allows us to rule out that the results in this section are driven by sample selection or seasonality.

V. Coordination and Public Information in the Long Run

We have documented that firms with multiple lenders that had bad news released at the time of the registry expansion experience a decline in lending. This decline begins during the interim period due to lenders' coordination motives, which produce a publicity multiplier of information. In theory, the credit outcomes of firms who had only good news released at the time of the registry expansion can be affected in the long run. For example, average lending will decline if the registry expansion increases firms' vulnerability to coordination failures upon the future arrival of bad public news. On the other hand, public information may increase average lending by reducing information asymmetries. On balance, the direction of the effect on average lending is ambiguous. This ambiguity also arises in the context our theoretical framework in the Appendix (see Figure A1, panel B). This section estimates the

ongoing effect of the registry expansion on debt by looking at firms with initially perfect credit records. In addition, we explore whether lenders' increased ability to coordinate under the new information environment leads to firms concentrating borrowing with fewer lenders.

A. Debt and Default

Table 6, column 1, shows the estimated coefficients of specification (1) over the subsample of firms with multiple lenders and a perfect credit record before the registry expansion (worst rating by any bank is a 1). Focusing on this subsample assures that any observed effect of the registry expansion on lending is not due to the stock of information revealed at the time of the expansion.

The DD point estimates indicate that average firm debt during the year after the registry expansion is 5.4% lower than in the pre-announcement period, but the difference is only significant at the 10% confidence level. The registry expansion does not have a statistically significant effect on the average debt level of firms with a perfect credit record and a single lender (Table 6, column 2). The results suggest that firms are more vulnerable to creditor runs after the registry expansion. However, we find no statistically significant effect of the registry expansion on the default hazard rate of firms with initially perfect credit records (Table 6, columns 3 and 4).

Information sharing will, in principle, improve the precision of each creditors' assessment of a borrower's creditworthiness. However, this mechanism is difficult to reconcile with a permanent reduction in average credit. An increase in information about creditworthiness would reduce either adverse selection or moral hazard (Jaffee and Russell 1976, Stiglitz and Weiss 1981), reduce holdup by privately informed banks (Rajan 1992), or reduce firm liquidity risk by lowering the costs of switching lenders (Detragiache, Garella, and Guiso 2000). All these interpretations imply that information sharing leads to more lending in equilibrium.

B. Debt Concentration

The consequences of lender coordination can be avoided by concentrating debt from fewer lenders. Brunner and Karhnen (2008) provide indirect evidence of this by showing that German banks of distressed firms form pools prior to bankruptcy. In the context of currency attacks, Corsetti et al. (2004) show theoretically 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) on the subsample of firms with multiple lenders before April using debt concentration ($DebtHHI$) as the dependent variable. The point estimate indicates that average debt concentration is 0.014 higher after the registry expansion than during the pre-announcement period (Table 7, column 1). This represents a 1.7% increase relative to the pre-announcement period mean. Debt concentration increases gradually over the year after the expansion: the average effect on concentration is significant only during the second half of the year (Table 7, column 2). This result is consistent with the cross-sectional patterns in debt concentration observed before the registry expansion (Figure 1). The concentration increase appears to be driven by an increase in the fraction of debt that firms obtain from their main lender, rather than by a decline in the number of lending relationships (Table 7, columns 3 through 6).

The findings indicate that lending arrangements respond endogenously to the increased ability of banks to coordinate 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 a year after the registry expansion, which suggests that this effect is only partially al-

leviated. The results also suggest that limiting lender coordination failures is a first order force in the trade-off firms face when choosing the concentration of creditors (Dewatripont and Maskin (1995), Bolton and Scharfstein (1996), and Bris and Welch (2005)).

VI. Conclusion

We provide evidence that creditors to the same firm have incentives to coordinate their lending decisions when the firm is close to financial distress, by showing that these decisions become more sensitive to information when it is public. 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 public is identified by comparing firms affected by the expansion (total lending between \$150,000 and \$200,000) with comparable firms not affected by it (lending between \$200,000 and \$250,000). Lending with a bank that possessed bad news about a firm's creditworthiness falls 19.8% 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 5.6 percentage point increase in the monthly hazard rate of default during the two-month period that follows the expansion announcement. On average, information sharing leads firms to concentrate borrowing from fewer creditors.

Our results are relevant for understanding 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: Theoretical Framework

A. Derivation of Publicity Multiplier

Each bank solves

$$\max_{L_i \geq 0} \theta_i L_i E_i(L_j) R - \frac{1}{\delta} L_i^\delta$$

where $E_i(L_j)$ is formed in equilibrium using the information available to bank i . The first order condition gives the optimal level of lending for any θ_i and $E_i(L_j)$

$$L_i = [\theta_i R E_i(L_j)]^{\frac{1}{\delta-1}}.$$

The second order condition to ensure that this solution is a maximum is

$$SOC : -c(\delta - 1) L_i^{\delta-2} < 0$$

which must always hold since we assume $\delta > 2$.

To compute the elasticity of bank i 's equilibrium level of lending absent information sharing L_i^{NS} with respect to θ_i , totally differentiate the first order condition

$$\frac{dL_i^{NS}}{d\theta_i} = \left(\frac{1}{\delta - 1} \right) \frac{1}{\theta_i} [\theta_i R E_i (L_j^{NS})]^{\frac{1}{\delta-1}}.$$

This makes use of the fact that when information is not shared $\frac{\partial E_i(L_j^{NS})}{\partial \theta_i} = 0$. To convert this to an elasticity:

$$\eta^{NS} = \frac{dL_i^{NS}}{d\theta_i} \frac{\theta_i}{L_i^{NS}} = \left(\frac{1}{\delta - 1} \right).$$

With information sharing, totally differentiating bank i 's first order condition gives

$$\frac{dL_i^S}{d\theta_i} = \left(\frac{1}{\delta - 1} \right) \frac{1}{\theta_i} [\theta_i R L_j^S]^{\frac{1}{\delta-1}} + \left(\frac{1}{\delta - 1} \right) \frac{1}{L_j^S} [\theta_i R L_j^S]^{\frac{1}{\delta-1}} \frac{\partial L_j^S}{\partial L_i^S} \frac{dL_i^S}{d\theta_i}.$$

The second term in this expression takes into account that, when information is public, raising θ_i will increase the lending of bank j and hence makes it optimal for bank i to lend more due to the complementarity. Using

$$\frac{\partial L_j^S}{\partial L_i^S} = \left(\frac{1}{\delta - 1} \right) \frac{1}{L_i^S} [\theta_i R L_i^S]^{\frac{1}{\delta-1}}$$

and rearranging gives

$$\frac{dL_i^S}{d\theta_i} = \frac{\left(\frac{1}{\delta-1} \right) L_i^S}{1 - \left(\frac{1}{\delta-1} \right)^2 \theta_i}.$$

Hence the elasticity of L_i^S with respect to θ_i with information sharing is

$$\eta^S = \frac{\left(\frac{1}{\delta-1} \right)}{1 - \left(\frac{1}{\delta-1} \right)^2}.$$

B. Additional Implications: Binary Types

To derive additional empirical implications of this framework we assume the density function $f(\theta)$ is a binary distribution. The creditworthiness of each loan can be either high or low: $\theta_i \in \{\theta_h, \theta_l\}$ ($\theta_h > \theta_l > 0$) with probability p or $1 - p$ respectively.

B-1. No Information Sharing

Suppose that bank i does not know θ_j . This equilibrium will be defined by two equilibrium levels of lending depending only on bank i 's possible payoff. Call these L_h and L_l . They are

characterized by the following two equations:

$$\begin{aligned} L_h &= [\theta_h R [pL_h + (1-p)L_l]]^{\frac{1}{\delta-1}} \\ L_l &= [\theta_l R [pL_h + (1-p)L_l]]^{\frac{1}{\delta-1}} \end{aligned}$$

Solving these simultaneously gives

$$L_l = (R)^{\frac{1}{\delta-2}} \theta_l^{\frac{1}{\delta-1}} \left[p\theta_h^{\frac{1}{\delta-1}} + (1-p)\theta_l^{\frac{1}{\delta-1}} \right]^{\frac{1}{\delta-2}}$$

and

$$L_h = (R)^{\frac{1}{\delta-2}} \theta_h^{\frac{1}{\delta-1}} \left[p\theta_h^{\frac{1}{\delta-1}} + (1-p)\theta_l^{\frac{1}{\delta-1}} \right]^{\frac{1}{\delta-2}}.$$

B-2. Information Sharing

When information is shared there are four possible equilibrium levels of lending: L_{hh} , L_{hl} , L_{lh} , and L_{ll} . The first subscript represents the creditworthiness of bank i 's loan (θ_i) and the second represents the creditworthiness of bank j 's loan (θ_j). Using bank i 's first order condition and using symmetry we have

$$L_{hh} = [\theta_h R L_{hh}]^{\frac{1}{\delta-1}}.$$

Manipulating gives that

$$L_{hh} = [\theta_h R]^{\frac{1}{\delta-2}}.$$

By a symmetric argument we also have that

$$L_{ll} = [\theta_l R]^{\frac{1}{\delta-2}}.$$

The equilibrium lending decisions when each bank faces a different payoff are characterized by the following two simultaneous equations:

$$\begin{aligned} L_{hl} &= [\theta_h R L_{lh}]^{\frac{1}{\delta-1}} \\ L_{lh} &= [\theta_l R L_{hl}]^{\frac{1}{\delta-1}} \end{aligned}$$

Solving simultaneously we have

$$L_{hl} = (R)^{\frac{1}{\delta-2}} \theta_h^{\frac{\delta-1}{\delta(\delta-2)}} \theta_l^{\frac{1}{\delta(\delta-2)}}.$$

and

$$L_{lh} = (R)^{\frac{1}{\delta-2}} \theta_l^{\frac{\delta-1}{\delta(\delta-2)}} \theta_h^{\frac{1}{\delta(\delta-2)}}.$$

B-3. Numerical Solution

We present numerical solutions to establish the sign and relative magnitude of the quantities predicted by the model. In our sample, the unconditional probability that a bank assigns a high rating is 90% (Table 1, Panel A). Thus we assume $p = 90\%$. We use $\theta_h = 1$, $\theta_l = 0.25$ and normalize $R = 1$. These assumptions are not qualitatively important for our numerical results. We study the model for a range of values for δ because changing this parameter alters one of the predicted effects.

B-4. Publicity Multiplier with Binary Types

In our empirical analysis we decompose the publicity multiplier into the effect of information sharing on lenders who have high and low initial assessment of the creditworthiness of their loans. The average percentage change in lending for a bank with either of these assessments is

$$\begin{aligned}\% \Delta L_h &= \frac{pL_{hh} + (1-p)L_{hl} - L_h}{L_h} \\ \% \Delta L_l &= \frac{pL_{lh} + (1-p)L_{ll} - L_l}{L_l}.\end{aligned}$$

These quantities are shown in Panel A of Figure A1. The expected change in lending for a bank with a high (low) assessment of creditworthiness is positive (negative). Note however that the magnitude of the effect is approximately 10 times larger for a bank with a low assessment. This comes from the fact that the unconditional prior on creditworthiness p is 90% and hence revealing bad news has a much larger impact on the actions of the rival bank. This is why in our empirical analysis we focus on the effect of revealing bad news and do not expect to see a measurable publicity multiplier for banks who have assigned a rating of 1 prior to the registry expansion.

B-5. Average Lending

The model also demonstrates that the unconditional average level of lending can be affected by making information public. The percentage change in average lending as a result of information sharing is defined as

$$\% \Delta L_{average} = \frac{[p^2 L_{hh} + p(1-p)(L_{hl} + L_{lh}) + (1-p)^2 L_{ll}] - [pL_h + (1-p)L_l]}{[pL_h + (1-p)L_l]}$$

The effect on average lending is shown in Panel B of Figure A1. The model's prediction depends on the curvature of the cost function. If $\delta \leq 3.32$ ($\delta > 3.32$) then the model predicts that average lending will increase (decrease) with information sharing. Since we do not have a good estimate for the value of δ we leave the direction of this effect as an open question for our empirical analysis.

Figure 1

Borrower Distribution and Characteristics, Cross-Section by Total Debt in March 1998

The plots represent the cross section distribution of treatment and control firms whose highest (worst) risk rating during the pre-announcement period is a 2. Treatment (control) firms are those with total debt between \$150,000 and \$200,000 (\$200,000 and \$250,000) during the pre-announcement period (January to March 1998). The vertical line emphasizes the \$200,000 threshold for information sharing. Left-hand side axis: number of firms in each \$10,000 bin. Right-hand side axis: average firm characteristics in each \$10,000 bin. 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.

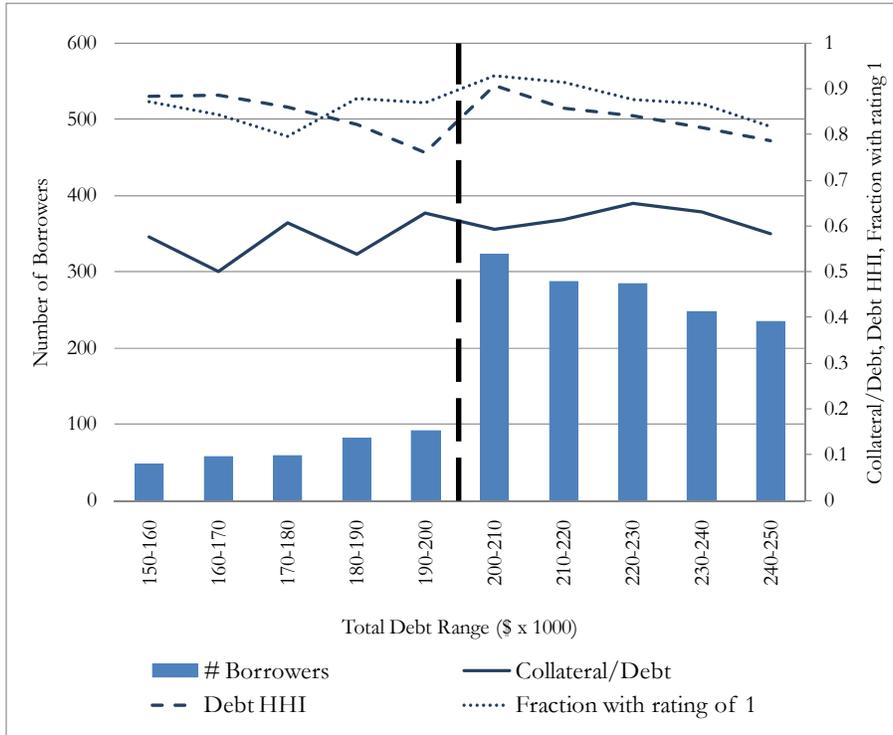
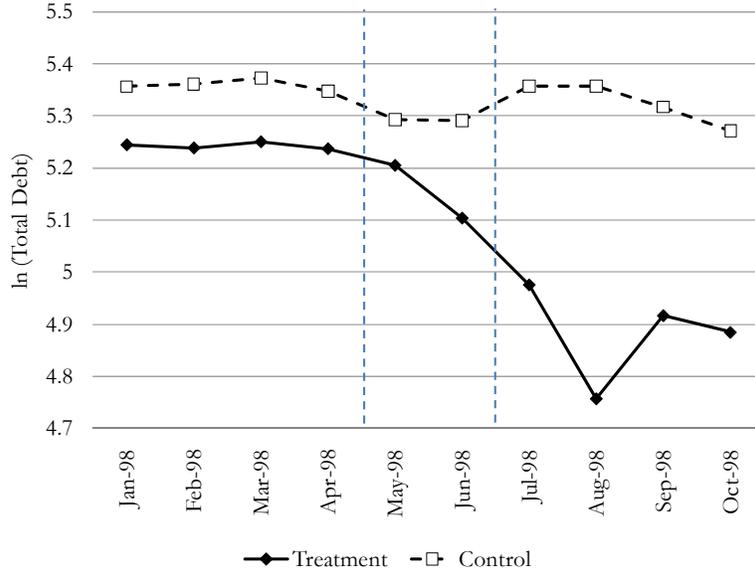


Figure 2

Average (log) Total Debt, Treatment and Control Firms with at Least one Rating = 2

The plots represent the time series of log bank debt for treatment and control firms that received at least one rating of 2 between January and March 1998. Treatment (control): firms with total debt between \$150,000 and \$200,000 (\$200,000 and \$250,000) in January and March 2008. The vertical lines enclose the interim period after the registry expansion announcement and before the actual information sharing through registry takes place.

Panel A. Firms with Multiple Lenders before Expansion Announcement



Panel B. Firms with a Single Lender before Expansion Announcement

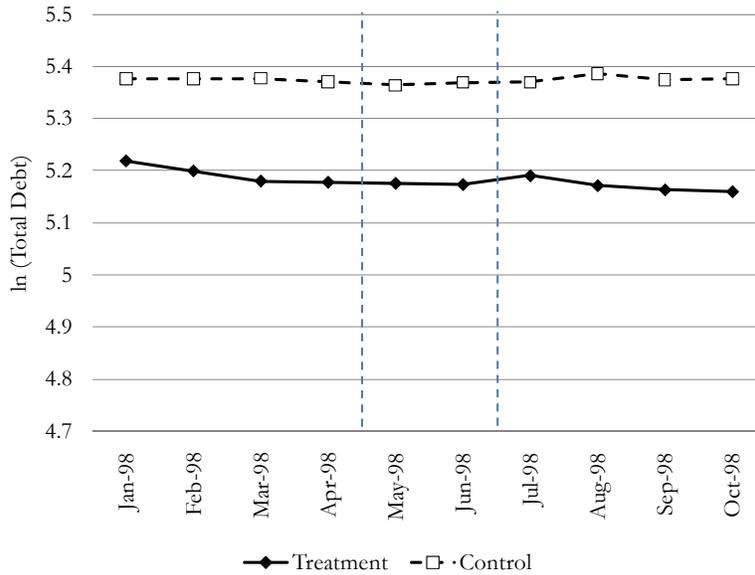


Table 1

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

Summary statistics of the firm cross section in our sample of treatment and control firms whose highest (worst) risk rating during the pre-announcement period is a 2. Treatment (control) firms are those with total debt between \$150,000 and \$200,000 (\$200,000 and \$250,000) during the pre-announcement period (January to March 1998). Firm debt HHI is the sum of the squared fractions of debt from each lender. We define the lead bank as 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). A rating of 1 (2) represents a firm in good standing with no (some) potential repayment problems. 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: Firms with rating of 2 or better before the Registry expansion announcement									
	n = 1,715			n = 337			n = 1,378		
	Firm level statistics								
Total debt ('000)	214.6	217.0	22.8	178.7	181.2	14.3	223.4	222.1	14.3
Number of lenders	1.88	2.00	1.06	1.89	2.00	1.13	1.88	2.00	1.04
Debt concentration (HHI)	0.84	1.00	0.22	0.83	1.00	0.23	0.85	0.99	0.21
Fraction debt from lead bank	0.88	1.00	0.17	0.88	1.00	0.18	0.89	1.00	0.17
Collateral/Debt	0.61	0.78	0.40	0.57	0.71	0.39	0.61	0.80	0.40
Average risk rating	1.10	1.00	0.31	1.12	1.00	0.32	1.10	1.00	0.31
Std. Dev. of same firm ratings	0.13	0.00	0.32	0.16	0.00	0.35	0.12	0.00	0.31
	Firm-Bank relationship level statistics								
	n = 3,228			n = 636			n = 2,592		
Debt ('000)	114.0	114.3	88.0	94.7	93.1	71.1	118.8	119.7	91.1
Risk rating	1.10	1.00	0.37	1.12	1.00	0.39	1.10	1.00	0.36
Panel B: Firms with at least one rating of 2 before the Registry expansion announcement									
	n = 208			n = 49			n = 159		
	Firm level statistics								
Total debt ('000)	216.0	221.8	25.7	177.0	176.6	13.8	228.0	229.2	13.9
Number of lenders	2.18	2.00	1.21	2.18	2.00	1.38	2.18	2.00	1.16
Debt concentration (HHI)	0.79	0.90	0.24	0.78	0.90	0.24	0.79	0.90	0.24
Fraction debt from lead bank	0.85	0.95	0.19	0.84	0.95	0.18	0.85	0.95	0.19
Collateral/Debt	0.67	0.81	0.35	0.66	0.80	0.35	0.67	0.81	0.35
Average risk rating	1.61	1.50	0.45	1.55	1.33	0.53	1.63	1.50	0.43
Std. Dev. of same firm ratings	0.53	0.58	0.39	0.46	0.58	0.42	0.55	0.58	0.37
	Firm-Bank relationship level statistics								
	n = 454			n = 107			n = 347		
Debt ('000)	99.0	72.0	85.1	81.0	67.1	67.6	104.5	74.7	89.2
Risk rating	1.50	1.00	0.63	1.44	1.00	0.65	1.51	1.00	0.62

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

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) estimated with OLS:

$$\ln(\text{Debt}_{it}) = \alpha_i + \xi_t + \gamma_{\text{Interim}} \text{Treat}_i \times \text{Interim}_t + \gamma_{\text{Post}} \text{Treat}_i \times \text{Post}_t + \varepsilon_{it}$$

Treatment (control) firms are those with total debt between \$150,000 and \$200,000 (\$200,000 and \$250,000) during the pre-announcement period (January to March 1998). Sample: firms 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 3 are estimated over the subsample of firms with multiple lenders, and column 4 on the subsample with a single lender, before the expansion announcement. Dependent variables: (log) debt of borrower i at time t with the banks that assigned the worst rating (column 1), with the banks that assigned the best rating (column 2), and with all lenders (columns 3 and 4). 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 dummies equal to one during the interim period and post-expansion period. Coefficients γ_{Interim} and γ_{Post} are the DD estimates of the effect of the registry expansion announcement and actual expansion on debt. Standard errors are heteroskedasticity robust and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Subsample by # of Lenders before April	Multiple Lenders			Single Lender
	ln(Debt from Banks w/ Rating = 2 _{it})	ln(Debt from Banks w/ Rating = 1 _{it})	ln(Debt _{it})	ln(Debt _{it})
Dependent Variable	(1)	(2)	(3)	(4)
Effect on Debt Level – Interim Period (2 months)	-0.198* (0.103)	-0.058 (0.065)	-0.075 (0.078)	-0.013 (0.131)
Effect on Debt Level – Post Expansion (12 months)	-0.356** (0.148)	-0.115** (0.057)	-0.135*** (0.045)	0.000 (0.180)
Firm Fixed Effects and Month Dummies	Yes	Yes	Yes	Yes
Observations (Firm-Month)	2,381	2,368	2,546	993
Clusters (Firms)	151	150	151	57
R-squared	0.914	0.671	0.581	0.556

Table 3
Effect of Registry Expansion on (log) Debt,
Subsample of Firms with at Least One Rating of 2 before Expansion

Difference-in-differences (DD) effect of the registry expansion announcement (interim period) and public information (post-expansion period) on how the share of lending by banks that assigned a rating of 2 changes relative to the share by banks that assigned a rating of one to the same firm, using specification (2) estimated with OLS:

$$\ln(\text{Debt}_{ijt}) = \alpha_{ij} + \xi_{jt} + \delta_{(\text{Treat}_i=1)_t} + \gamma_{\text{Interim-Bank2}} \text{Treat}_i \times \text{Interim}_t \times \text{BankAssigned2}_j + \gamma_{\text{Post-Bank2}} \text{Treat}_i \times \text{Post}_t \times \text{BankAssigned2}_j + \omega_{ijt}$$

Treatment (control) firms are those with total debt between \$150,000 and \$200,000 (\$200,000 and \$250,000) during the pre-announcement period (January to March 1998). Sample: firms with multiple lenders, 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). Dependent variables: (log) debt of borrower i at time t with bank that assigned rating j . Right-hand side variable of interest: triple interaction between a dummy equal to one if borrower i was in the treatment group, dummies equal to one during the interim period and post-expansion period, and a dummy equal to one in the lender assigned a rating of 2. Coefficients $\gamma_{\text{Interim-Bank2}}$ and $\gamma_{\text{Post-Bank2}}$ are the DD estimates. Standard errors are heteroskedasticity robust and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable:	ln(Debt _{ijt})
Effect on Debt by Banks that Assigned Rating = 2– Interim Period (2 months) (Treat × Interim × BankAssigned2)	-0.214** (0.106)
Effect on Debt by Banks that Assigned Rating = 2 – Post Expansion (12 months) (Treat × Post × BankAssigned2)	-0.319** (0.158)
Firm × Bank Fixed Effects	Yes
Bank × Month Dummies	Yes
Treat × Month Dummies	Yes
Observations (Firm-Month-Bank)	4,749
Clusters (Firms)	151
R-squared	0.921

Table 4
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 (3) estimated with OLS:

$$I[Default_{it} = 1 | Default_{it-1} = 0]_{it} = \xi'_{it} + \lambda_{Interim} \cdot Treat_i \times Interim_t + \lambda_{Post} \cdot Treat_i \times Post_t + \zeta_{it}$$

Treatment (control) firms are those with total debt between \$150,000 and \$200,000 (\$200,000 and \$250,000) during the pre-announcement period (January to March 1998). Sample: firms 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 (2) estimated over the subsample of firms with multiple lenders (a single lender) before the expansion announcement. Dependent variable: conditional default of borrower i at time t with any bank. Coefficients $\lambda_{Interim}$ and λ_{Post} are the DD estimates of the effect of the registry expansion announcement and actual expansion on default hazards. Standard errors are heteroskedasticity robust and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable:	Default Hazard: 1 if Firm in Default at t , and not at $t-1$	
	Multiple Lenders	Single Lender
Subsample:	(1)	(2)
Interim Period		
Effect on Default Hazard Rate – Interim Period (2 months)	0.056* (0.030)	-0.008 (0.028)
Effect on Default Hazard Rate – Post Expansion (12 months)	0.030* (0.016)	0.033 (0.022)
Month Dummies	Yes	Yes
Observations (Firm-Month)	2,546	993
Clusters (Firms)	151	57
R-squared	0.104	0.057

Table 5
Placebo Tests

The placebo tests replicate the difference-in-differences (DD) estimations on (log) debt from Tables 2 and 4 over different samples. Placebo 1: Estimates where sample is constructed as if registry expansion announcement occurred in April 1999, one year after actual expansion. Treatment (control) firms are those with total debt between \$150,000 and \$200,000 (\$200,000 and \$250,000) between January to March 1999 (instead of 1998). Sample: firms whose highest (worst) risk rating during this is a 2, and with at least one rating of 2 (firms with only good ratings excluded).

Placebo 2: Estimates where sample is constructed as if registry cutoff rule is at \$300,000 before the expansion, instead of \$200,000. Treatment (control) firms are those with total debt between \$250,000 and \$300,000 (\$300,000 and \$350,000) during the pre-announcement period (January to March 1998). Sample: firms 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).

Standard errors are heteroskedasticity robust and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable:	$\ln(\text{Debt}_{it})$	$\ln(\text{Debt from Banks w/ Rating} = 2_{it})$	Default Hazard: 1 if Firm in Default at t , and not at $t-1$
	(1)	(2)	(3)
A. Placebo 1: Assuming Registry Expansion Occurred in 1999			
Placebo Effect – Interim Period (2 months)	0.020 (0.031)	-0.001 (0.033)	0.007 (0.017)
Placebo Effect – Post Expansion (12 months)	-0.027 (0.022)	-0.036 (0.055)	0.002 (0.011)
Firm Fixed Effects, Month Dummies	Yes	Yes	Yes
Observations (firm-months)	11,272	10,696	8,081
Clusters (firms)	666	666	659
R-squared	0.547	0.925	0.088
B. Placebo 2: Assuming Pre-Expansion Debt Cutoff at \$300,000			
Placebo Effect – Interim Period (2 months)	0.034 (0.081)	0.057 (0.076)	-0.017 (0.034)
Placebo Effect – Post Expansion (12 months)	-0.029 (0.052)	-0.007 (0.122)	0.007 (0.023)
Firm Fixed Effects, Month Dummies	Yes	Yes	Yes
In Sample after Default	Yes	Yes	No
Observations (firm-months)	2,258	2,124	1,725
Clusters (firms)	134	134	134
R-squared	0.513	0.920	0.103

Table 6
Effect of Registry Expansion on Credit Outcomes of Firms with Perfect Credit Records

Estimates the same difference-in-differences (DD) effects as in tables 2 and 4, but over the subsample of firms with perfect credit records before the registry expansion announcement. Treatment (control) firms are those with total debt between \$150,000 and \$200,000 (\$200,000 and \$250,000) during the pre-announcement period (January to March 1998). Sample: firms with multiple lenders, whose highest (worst) risk rating during the pre-announcement period is a 1 (firms with a bad rating with any bank excluded). Standard errors are heteroskedasticity robust and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable	ln(Debt _{it})		Default Hazard: 1 if Firm in Default at t , and not at $t-1$	
	Multiple	Single	Multiple	Single
Subsample: # lenders pre-expansion	(1)	(2)	(3)	(4)
Effect on Dependent Variable – Interim Period (2 months)	-0.0455 (0.0591)	0.0468 (0.0472)	0.0026 (0.0047)	-0.0068 (0.0074)
Effect on Dependent Variable – Post Expansion (1 to 12 months)	-0.0543* (0.0281)	0.0550 (0.0604)	0.0033 (0.0038)	-0.0037 (0.0068)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Month Dummies	Yes	Yes	Yes	Yes
In sample after default?	Yes	Yes	No	No
Observations (Firm-Month)	17,639	9,160	13,370	9,073
Clusters (Firms)	891	616	891	616
R-squared	0.540	0.512	0.183	0.167

Table 7
Effect of Registry Expansion on Firm Debt Concentration

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

$$y_{it} = \alpha_i + \xi_t + \gamma_{Interim} Treat_i \times Interim_t + \gamma_{Post} Treat_i \times Post_t + \varepsilon_{it}$$

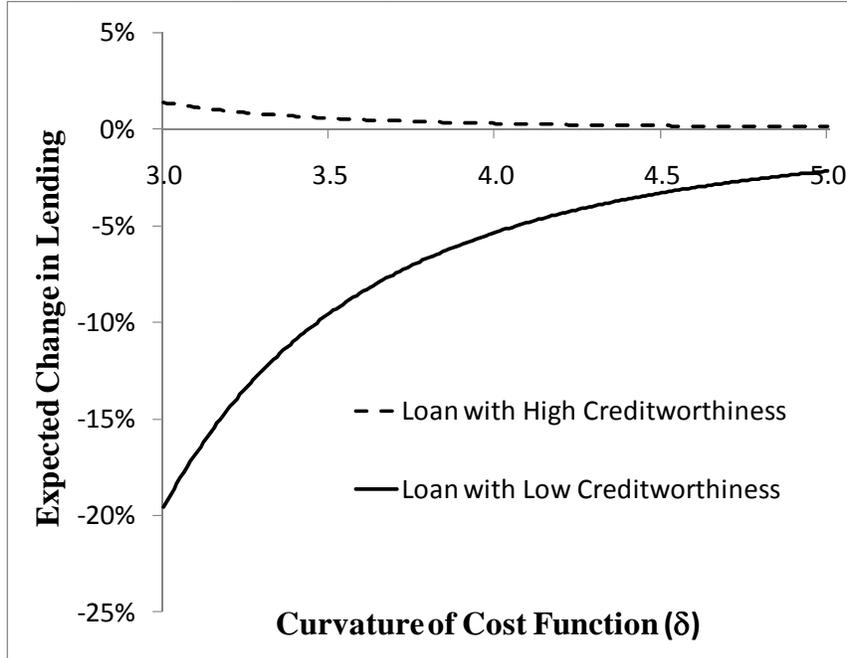
Treatment (control) firms are those with total debt between \$150,000 and \$200,000 (\$200,000 and \$250,000) during the pre-announcement period (January to March 1998). Sample: firms that during the pre-announcement period 1) have multiple lenders and 2) whose highest (worst) risk rating during the pre-announcement period is a 2, and with at least one rating of 2 (firms with only good ratings excluded). The dependent variables are the debt HHI, the (log) number of lenders, 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 dummies equal to one during the interim period and post-expansion period (specifications 2, 4, and 6 include separate dummies for the first and second halves of the post-expansion period). Coefficients $\gamma_{Interim}$ and γ_{Post} are the DD estimates of the effect of the registry expansion announcement and actual expansion on debt. Standard errors are heteroskedasticity robust and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable	Debt HHI _{it}		ln(#Lenders _{it})		Fraction by Top Lender _{it}	
	(1)	(2)	(3)	(4)	(5)	(6)
Effect on Dependent Variable – Interim Period (2 months)	-0.0021 (0.0043)	-0.0021 (0.0043)	0.0092 (0.0075)	0.0092 (0.0075)	-0.0021 (0.0041)	-0.0027 (0.0040)
Effect on Dependent Variable – Post Expansion (1 to 12 months)	0.0148* (0.0078)		-0.0196 (0.0148)		0.0122* (0.0074)	
Effect on Dependent Variable – Post Expansion (1 to 6 months)		0.0094 (0.0070)		-0.0120 (0.0128)		0.0071 (0.0066)
Effect on Dependent Variable – Post Expansion (7 to 12 months)		0.0194** (0.0092)		-0.0261 (0.0175)		0.0153* (0.0086)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Month Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations (Firm-Month)	17,577	17,577	17,577	17,577	17,577	17,577
Clusters (Firms)	1,042	1,042	1,042	1,042	1,042	1,042
R-squared	0.826	0.826	0.784	0.784	0.817	0.817

Appendix Figure A1 Numerical Solution of Theoretical Framework

All panels are drawn using the following parameter assumptions: $p=0.9$, $\theta_h=1$, $\theta_l=0.25$, and $R = 1$. Panel A plots the publicity multiplier conditional on the creditworthiness (θ_h or θ_l) privately observed by the bank prior to information sharing. For each type this is the expected change in lending as a result of information sharing as a fraction of the level of lending without sharing. These are defined in the Appendix as $\% \Delta L_h$ and $\% \Delta L_l$. Panel B plots the unconditional percentage change in the average amount of lending done by each bank as a result of information sharing. This is defined in the Appendix as $\% \Delta L_{average}$.

Panel A: Expected Change in Lending Conditional on Bank's Private Information



Panel B: Effect of Information Sharing on Average Lending

