Public Information and Coordination: Evidence from a Credit Registry Expansion

Andrew Hertzberg Jose Maria Liberti Daniel Paravisini*

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Abstract

When many creditors lend to the same borrower each faces a potential coordination problem. Suspension of finance by one creditor can disrupt the operations of the firm and lower the value of other creditors' claims. We study a change in the Argentine public credit registry to provide evidence that these problems are exacerbated when credit market information is shared. In April 1998 the registry was expanded to include borrowers with less than $200,000 in total debt. We identify the effect of public information by studying the change in lending outcomes on borrowers just below the $200,000 cut-off using borrowers above the cut-off as a control group. Firms affected by the expansion experience a decline in lending and an increase in default rates. These effects come from firms who borrow from multiple creditors and are hence subject to potential coordination failures. Firms appear to mitigate their exposure to coordination problems by increasing the concentration of their borrowing across banks: we show that the correlation across lending decisions of different banks to the same firm increases after the registry expansion, and decreases after lending becomes concentrated.

*Columbia Business School; DePaul University, and Columbia Business School and BREAD respectively. E-mails: a-hertzberg@kellogg.northwestern.edu, jliberti@depaul.edu, and dp2239@columbia.edu. We wish to thank seminar participants at Columbia Business School, Kellogg School of Management, and DePaul University. All errors are our own.

I. Introduction

It has long been recognized that credit market outcomes are affected by the presence of private information. First, borrowers may be constrained when they have information about their creditworthiness that lenders do not possess (Stiglitz and Weis (1981), Jaffe and Russell (1976), Myers and Majiluf (1984)). A primary role of financial intermediaries is to acquire information to remedy this problem (Petersen Rajan (1994)). This creates a second source of private information in credit markets – an asymmetry of information between lenders regarding the creditworthiness of a borrower. Recent theoretical work has argued that the absence of common information in environments where coordination among agents is important can have important equilibrium implications (see for example Carlsson and Van Damme 1993 and Morris and Shin 2002a). Credit markets are one of the most natural applications of this theory. When many creditors lend to the same borrower they typically each posses some degree of private information and face a potential coordination problem. Suspension of finance by one creditor can disrupt the operations of the firm and lower the value of other creditors' claims.[1] Morris and Shin (2004) apply this framework to credit markets and argue that increased public information can exacerbate the incidence of creditor runs. However to date no empirical evidence exists to demonstrate that this theoretical prediction has first order consequences in credit markets. Answering this question has important lessons for understanding the role of public information in credit markets and related public policy questions. Of the 129 countries surveyed by Djankov, McLiesh and Shleifer (2007) in 2003, 71 were found to have some form of public credit registry whereby borrower information from private lenders is collected and shared by a government agency. More broadly transparency of information is a widely promoted policy recommendation for developing credit markets.[2]

In this paper we measure the effect of information sharing in a credit market. We exploit as a natural experiment the expansion of the public credit registry operated by the Central Bank of Argentina in April 1998. This expansion is uniquely suited to measuring the effect of information

[1] The coordination problem between creditors has been studied theoretically, for example Dewatripont and Maskin (1995), Bolton and Scharfstein (1996), Bris and Welch (2005). Modern bankruptcy code is designed to alleviate these problems in distress (Jackson (1986)). To document this force empirically it has been shown that distressed firms with more dispersed creditors find it harder to restructure out of court (Asquith Gertner Scharfstein (1994)). Brunner and Karhnen (forthcoming) show that German banks of distressed firms form pools prior to bankruptcy to mitigate coordiantion problems.

[2] See for example Glennerster and Shin (2004) and Glennerster and Shin (forthcoming). One of the concerns voiced by policy makers is the possibilitiy that public registries will create over-reaction to shared information (see for example Miller (2003) p.42).

sharing on the incidence of creditor coordination failures for two reasons.

First, the expansion created time series and cross-section variation that allows us to empirically identify the effect of the information sharing. Prior to April 1998 information was shared only for borrowers whose total outstanding debt was above $200,000. Technological improvements lowered the cost of storing and distributing information and eliminated the need of such a threshold, originally put in place to reduce the cost of distributing information for large numbers of small debtors.[3] In April 1998 the minimum eligibility limit declined to $50. The registry disclosed information back to January 1998, allowing us to see lending outcomes for three months before the expansion took effect. And second, the registry expansion involved disclosure of each bank's debt outstanding and risk rating of each of their borrowers (default information for all borrowers was always shared). The shared information provides a credible signal of each bank's assessment of the future creditworthiness of each borrower, thus providing potentially important information which each bank can use to coordinate their actions.

Our first results are drawn by comparing the changes in outcomes before and after the registry expansion, for all borrowers whose lending was between $175,000 and $200,000 prior to the expansion with those of all borrowers whose lending was between $200,000 and $225,000 (the control). The results show that firms for whom information became shared experience, on average, a decrease in lending of 9.5% in the 12 months after the expansion. This lending was accompanied by an increase in the cumulative increase in default probability during the three months after the registry expansion of 5.8 percentage points. These results are striking considering that information asymmetries are the usual culprit behind financing constraints and rationing in credit markets. Also, the results are consistent with the hypothesis that information sharing increases the incidence of creditor runs. First, the incidence of creditor runs predicts that banks will be unwilling to provide additional finance to meet a firm's short term liquidity needs. This will lead directly to the empirical observation of lower debt amounts and more defaults. In addition, banks will lend less in anticipation of an increased probability of a run in the future.

Having established a causal relationship between information sharing, lending, and default, we provide evidence that increased coordination failures are the primary channel behind this effect. We do this, first, by exploring whether the cross-section implications of this mechanism are borne in

[3] Minimum borrowing limits for debtor eligibility in information sharing are a common feature of public credit registries around the world due to the considerable costs of processing information for large numbers debtors. Of the 37 public credit registries surveyed in Miller (2003) 26 had minimum loan size cutoffs.

the data. Coordination failures are only possible for firms that have multiple lenders. We repeat our differences-in-differences analysis separately for the subgroup of firms who have one and multiple banks prior to the registry expansion. Our results show that the reduction in lending and increased default are experienced only by firms who have multiple creditors. Firms who borrow from only one bank experience no change in their level of lending or their incidence of default relative to the control group.

These cross-sectional results suggest that the role of public information in exacerbating creditor runs was the key force triggered by the registry expansion. Other potential explanations for our primary lending and default results are that being forced to share information may erode a bank's incentive to monitor (Rajan 1992, Petersen and Rajan 1995) or may limit a firm's incentive to work hard to maintain a good reputation (Padilla and Pagano 2000). However, neither of these theories predicts that information sharing should only have negative consequences on firms who borrow from multiple banks.

The previous cross-sectional pattern can also occur if information sharing reveals that some firm's were engaging in lending that they had not revealed to other creditors. We present evidence to suggest that the discovery of hidden lending was not a first order effect of the expansion. First, contrary to our findings, this account would predict an increase in lending for firms who are revealed to only have one lender since their banks receive the good news that they are not being deceived. Further we exploit the \$200,000 cut-off as a source of cross-sectional variation in the potential for hidden lending. Since a total debt of \$200,000 makes a firm's credit records visible through the registry, a bank that lends \$199,000 to a firm prior to the expansion could be assured that any degree of hidden lending is no more than \$999. Conversely, if the bank lends \$150,000 then it faced potential hidden lending up to \$49,999. By this account we would expect to see a more positive lending response for firms (with only one lender) who were receiving credit further below the cut-off. We find no evidence of such differential effect further suggesting that hidden lending is not a primary driver of our results.

We then explore whether firms adjust their lending arrangements in a way consistent with an increase in the likelihood of creditor runs. Since coordination failures stem from a collective action problem, firms can limit their vulnerability by lowering the number of banks they lend from and increasing their concentration of lending across banks.[4] We find strong evidence to support this

[4] Corsetti et al (2004) show theoretically, in the context of a currency attack, that the presence of an agent with large market share can reduce the incidence of coordination failures.

hypothesis. In the 12 months after the expansion, firms affected by the registry expansion borrowed from 10.4% fewer lenders, and increased the fraction of borrowing from their top lender by 8.7%.

To provide additional evidence that information sharing led to increase coordination failures we measure the change in the correlation of lending decisions by different banks to the same firm. We show that this correlation becomes higher by 15 percentage points relative to the control group in the 2 months after information is shared. By way of contrast, we show that a simple Bayesian framework absent coordination motives, would predict a decrease in this correlation. One year after the registry expansion this correlation is no longer statistically different between the control and treatment group. We interpret this as suggesting that the increased concentration in bank lending over the 12 months was effective in mitigating coordination failures. Moreover, this result provides further evidence that our results are not primarily driven by free-riding in monitoring. If this were the case we would expect this to be further exacerbated by the concentration of lending as small stakes lenders free ride on the information they receive from the large stakes lender.

Our paper contributes to a number of related research areas. First, there a number of recent theoretical papers have stressed the importance of public information in settings where coordination among agents is important. Carlsson and Van Damme (1993) and Morris and Shin (2002a) show that when each agent has sufficient private information, relative to what is known publicly, the typical multiple equilibrium which arise in these contexts disappears. This framework has been applied to coordination games such as bank runs (Goldstein and Pauzner 2005), currency attacks (Morris and Shin 1998), and creditor runs (Morris and Shin (2004)).[5]

The central intuition can be understood with the following example. Suppose that two creditors have provided finance to a firm and hold separate collateralized claims on half its assets each. The firm's assets are more productive when used together. This creates the basic motive for coordination between each creditor. If the firm has an additional interim liquidity shock each creditor must decide whether or not to extend additional finance or whether to refuse to rollover the loan and seize its pledged collateral. Each creditor will base its decision on i) its assessment of the fundamental value

[5] In a series of related papers the costs and benefits of public information in environments with coordination has been argued by several theory papers (Morris and Shin 2002b; Angeletos and Pavan 2004, 2007; Morris et al 2006; Svenson 2006). To date, the only existing empirical test of these ideas is provided by an experimental evidence in Heinemann et al (2004). They find that increased public information tends to slightly reduce coordination failures. Relatedly Musto (2004) studies the effect of deletion of bankruptcies from personal credit histories and finds that, in the long term this leads to increased bankruptcy. A recent paper by Chen et al (2008) shows that runs may be an important force in the mutual fund industry although they do not discuss the role of public information in exacerbating these problems.

of the firm and ii) whether or not it anticipates that the other creditor will also rollover its loan. Publicly released bad news about the firm's prospects will have two effects. First it will lower each bank's assessment of the fundamental value of the firm. Second, because this information is common knowledge it will lower each bank's assessed probability that the other will extend credit. This second effect creates a publicity multiplier and implies that firms become more vulnerable to a creditor run when news about their creditworthniess is made public.

Our paper is also related to the literature which studies the effect of information sharing in credit markets. Due to their prominence around the world several theory papers have suggested potential effects of information sharing.[6] The existing empirical evidence relies on country level analysis and finds a positive correlation between the existence of a credit registry and the level of lending ((Jappelli and Pagano (2002) and Djankov, McLiesh and Shleifer (2007)). Our analysis differs from this previous work in a number of key ways. First, the registry expansion provides us with a natural control group against which to compare and identify the causal effect of the expansion. Cross country studies are limited by the concern that the presence of a registry is endogenous at the country level – most notably that an increased reliance on financial intermediation could lead to the adoption of a registry. A second key difference is that our firm and relationship level data allows us to measure cross-sectional differences on the impact of information sharing thus providing evidence of the channel through which information sharing impacts outcomes.

The rest of the paper proceeds as follows. Section II describes the institutional environment, the data and provides a brief history of the registry expansion in Argentina. Section III outlines our empirical strategy for identifying the effect of information sharing in credit markets. Section IV presents our primary results studying the impact of information sharing on lending, default and lender concentration. Section IV studies cross-sectional differences in the impact of the registry expansion and shows how the correlation of bank lending decision is affected by the expansion. Section V provides a brief conclusion.

[6] See for example Pagano and Jappelli (1993), Vercammen (1995), Padilla and Pagano (1997), and Padilla and Pagano (2000).

II. Setting and Data: The Credit Registry in Argentina

Starting in 1991 the Central Bank of Argentina operated a public credit registry named the Central de Deudores del Sistema Financiero. From that time on, all financial institutions in Argentina were required to report individual loan level information about all of their borrowers to the Central Bank each month. For each borrower a lender is required to report the total amount of lending outstanding, the amount of collateral the borrower currently has pledged to the lender, and a rating of the borrower's creditworthiness. The credit rating is expressed as a number from 1 to 5 where a higher number indicates a higher risk of default. Ratings of 1, 2, and 3 are made at the bank's discretion using their private assessment of the borrower's future repayment prospects. Ratings of 4 and 5 are mechanically determined by the status of lending with the borrower. If the borrower has missed a repayment by more than 90 days they are assigned a 4. A 5 is assigned to borrowers whose loans have been written-off. All information is reported to the Central Bank at the borrower level and hence aggregates the entire set of loans that a borrower may have with any one lender.

Prior to 1995 the Central Bank of Argentina collected this information purely for the purpose of banking supervision. Starting in that year the Central Bank began to share the information it received for borrowers whose total lending outstanding summed across all institutions was over $200,000.[7] In addition, borrowers with a risk rating of 3 or higher with any bank had their information shared going forward until their worst rating improved to 2 or 1 for at least two years. The registry reports the outstanding lending, value of pledged collateral and credit rating for each borrower with each of their banks. Information was provided to all financial institutions and a few selected credit rating companies, if requested, in the form of a monthly magnetic tape which contained the latest cross-section of data. Information reported to the central bank is shared with a delay of typically 3 months. So for example the credit information for January 1998 would be shared in April 1998.

We study the effect of information sharing by exploiting a natural experiment created by a change in the information available in the registry that occurred in 1998. In April of that year the Central Bank changed the technology with which credit information was distributed. To lower distribution costs credit information was now shared electronically through the central bank's website and compact discs.[8] Access to information to both of these mediums was granted to the general public. The central

[7] A borrower whose lending was over $200,000 in month t and fell below that threshold in month t+1 would continue to have her information shared for the next twelve months (*i.e.* until t+13).

[8] Central Bank Comunicado A2679 dated April 1, 1998 proposed the termination of the availability of the magnetic

bank took advantage of the reduced cost of disseminating information by expanding the set of borrowers for whom credit information was shared. The threshold of $200,000 in total borrowing was lowered to $50. This change was retroactive so that in April 1998 the credit information for January 1998 of all borrowers with total lending over $50 was shared even though this data was reported under the expectation that it was not going to be shared. This provides us with three months (January, February, March) of credit data for borrowers with total lending under $200,000 before The data covers the universe of all borrowers (firms and individuals) with a relationship with the formal intermediation sector in Argentina.they were affected by the policy change.

Our empirical analisys uses the monthly data from the public registry. The sample period starts in January 1998, when information becomes public. On the month prior to the public registry expansion, March 1998, the registry contains information for 566,416 borrowers in 966,513 bank-borrower lending relationships. The registry expansion increases the number of borrowers with publicly shared credit information by 540,000 firms and individuals, whose debt represents 11% of the $67 billion dollars of total debt outstanding from the banking sector.

Outside of the public credit registry there was no formal way for a lender to ascertain how much credit a borrower owed other financial institutions. From talking to loan officers and informal conversations with senior managers of financial institutions in Argentina we know that common practice was to ask borrowers for a list of all outstanding credit at the time of a loan application or when credit was being renewed. This list could be checked by personally contacting other banks. Hiding credit obligations from a bank is fraud and is punishable in court. There is no dedicated collateral registry in Argentina. However when collateral is pledged in the form of real estate or motor vehicles the public registry of ownership serves as an effective collateral record since in these cases the deeds of these assets are handed over to the creditor or the lien is attached to the deed itself. For other assets a creditor cannot determine if the same asset has been pledged to another lender. In the event of default all banks are treated equally (in proportion to their claim) and hence a lender cannot use seniority to prevent dilution of its claim.

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

III. Identification and Estimation Methods

To identify the impact of public information on credit market outcomes, we exploit the time series variation induced by the registry expansion, and the cross sectional variation induced by the pre-existing $200,000 eligibility threshold. We expect to observe the impact of public information on the time series of debt levels and other outcomes after the registry expansion for firms that had debt below $200,000 prior to April 1998. However, this effect will be confounded with the potential effect of other contemporaneous aggregate shocks. To identify the effect of information sharing we use the firms with total debt above $200,000 prior to April 1998, who were unaffected by the registry expansion, as a control group. The control group serves as a counterfactual for the time series evolution of the lending outcomes of the firms affected by 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. By measuring changes in lending outcomes of firms affected by the expansion relative to those of the control group we can estimate the causal effect of sharing credit information.

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. Since firms above and below the $200,000 threshold have, by definition, different levels of total debt, it is likely they also differ in other important characteristics related to other outcomes. To make the two groups comparable in this dimension we restrict the time series analysis to borrowers whose total debt was always between $175,000 and $225,000 before April 1998. We also exclude from the control group all firms with a risk rating higher than 2 prior to the registry expansion, since information for all firms with such a rating were already shared through the registry prior to April. Only firms with a risk rating of 1 and 2 were affected by the registry expansion, and restricting the control group makes the two groups plausibly comparable in expected creditworthiness.[9]

Panel A of Figure 1 shows the time series of median debt for the firms affected by the expansion and control firms in this subsample. Both series have the pre-April 1998 means and trends removed. Several observations arise from the plot that validate our identification assumption. First, the median debt of firms affected by the expansion and of control firms evolve in parallel prior to the registry

[9] The raw data in the registry contains the cross section of all active relationships between a borrower and the formal financial sector, and the stock of defaulted relationships during the previous five years. For example, every amount outstanding defaulted and never repaid prior to January 1998 appears as in default in the first cross section in our panel. For that reason, 34.2% of the 78,293 observations in the January 1998 cross section appear in default. We will limit our analysis to include those firms that are not in default when they enter our sample in January 1998.

expansion. The same is true for the average debt concentration, measured as the debt HHI (Panel B of Figure 1).[10] Second, the debt of the two groups of firms starts to diverge after April when credit information is made public. The median debt of affected firms drops relative to firms in the control group. These patterns represent strong evidence that, conditioning on pre-existing differences in means and trends, lending outcomes of the control group of firms represent a valid counterfactual for lending outcomes of the affected firms. In addition, the fact that the median debt of firms in the control group presents no observable change after April 1998 rules out the possibility that the credit outcomes of this group were affected by the registry expansion.[11]

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

$$\ln(Debt_{it}) = \alpha_i + \xi_t + \delta_i t + \sum_{month=2}^{16} [\gamma_{month} Public\_Apr98_i \times Dum\_month_t] + \varepsilon_{it} \qquad (1)$$

The dependent variable is the (log) debt of firm i at month t. The right-hand side includes firm fixed effects, calendar month dummies and firm specific trends. The variable of interest is a dummy equal to one if firm i's credit information becomes public after April 1998 due to the registry expansion ($Public\_Apr98_i$). The coefficient on this dummy represents the log-difference between the average debt of firms affected by the registry expansion and firms in the control group. $Public\_Apr98$ is interacted with a full set of calendar month dummies, labeled consecutively starting from January 1998 ($t = 1$). The interaction represents the log-debt difference across the two groups every month before and after the registry expansion. The magnitude of the change in the estimated coefficients before and after April 1998 represents the difference-in-difference estimator of the effect of public information on total lending. For example, one estimate of 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 and March 1998 ($\gamma_{16} - \gamma_3$). In Figure 1, the difference is normalized to zero before the expansion, and drops to -11 in March 1999. This suggests that public information reduces by \$11,000 the median debt of affected firms, after accounting for other shocks to credit outcomes. Since statistics

[10] For any firm, debt HHI is the sum of the square fraction of debt from each lender, and represents a measure of debt concentration. If a firm borrows from a single bank then debt HHI is equal to one, and if it borrows equal amounts from two lenders debt HHI is 0.5.

[11] This can occur, for example, due to diminished incentives for self selection. Self-selection of firms above the threshold can result if firms chose to increase the amount of total debt prior to the expansion to ensure that their credit history became shared across banks. Once the eligibility threshold was removed, these firms face diminished incentives to increase debt above \$200,000. This type of self selection would induce an upward bias in the difference-in-difference estimator.

are estimated on the subsample of firms with debt between \$175,000 and \$225,000 prior to April, the effect represents approximately a 5.5% decline in debt ($\gamma_{16} - \gamma_3 = 0.055$ in specification (1)).

We perform all the time series analysis on the \$175,000 and \$225,000 debt subsample. The conclusions of the next section are robust to choosing a narrower range around \$200,000, although doing so reduces the precision of the parameter estimates. The full panel descriptive statistics in this subsample are shown in Table 1. All standard errors are estimated allowing for clustering at the firm level. To produce consistent estimates of the standard errors under residual serial correlation in outcomes, we also use a specification with all the observations before and after registry expansion collapsed into one observation as suggested in Bertrand, Mullainathan and Duflo (2004). The collapsed specification is defined in growth rates to eliminate the debt level component α_i from specification (1), and includes a firm dummy to control for firm specific growth rates:

$$[\ln(Debt_{iApril98}) - \ln(Debt_{iJan98})] - [\ln(Debt_{iApril99}) - \ln(Debt_{iMay98})] = \delta_i' + \beta Public\_Apr98_i + \varepsilon_i' \quad (2)$$

The left-hand side variable represents the change in the growth rate of debt of firms i between the four months prior and the twelve months after the registry expansion (we also consider specifications with shorter periods of 4 and 8 months after the expansion). The right-hand side variable of interest is $Public98\_Apr98_i$ as before, and its coefficient (β) is the difference-in-difference estimator of the effect of public information on the *debt growth rate* during the year after the registry expansion. Continuing the previous example, the difference in debt growth rates across the affected and control firms was normalized to zero prior to the registry expansion. The median debt of the firms affected by the expansion declines by \$7,000 during the twelve months after the expansion (from Figure 1), which implies a negative growth rate of 3.5% over the year and a difference-in-difference estimate of $\beta = 0.035$.

IV. Public Information, Equilibrium Lending and Default

The visual comparison of the outcomes of firms affected by the registry expansion and those in the control group suggests that public information reduces the equilibrium level of debt. Table 2

shows the estimated coefficients of specification (1), which represent the differences between the (log) debt of firms affected by the registry expansion and control firms every month between February 1998 and March 1998. The estimates in Column 1, over the full subsample of firms, confirm the patterns in Figure 1. The point estimates show no significant time series change prior to April 1998. The difference between the point estimates for March 1999 and March 1998 ($\gamma_{16} - \gamma_3$) is -0.095, significant at 1% confidence level. This implies that publicly sharing information through the registry reduces total debt by 9.5% in a year.

Monthly point estimates are noisy, and the standard error estimates are potentially inconsistent under serial correlation in specification (1). The growth specification (2) addresses these concerns, and the estimated coefficients are shown in panel A of Table 3. The results confirm that making public credit information reduces debt growth. The point estimates indicate that the debt growth declines by 5.1 percentage points over the four months after the registry expansion, and 3.7 percentage points over the entire year, both significant at the 1% level of confidence.

The differences-in-differences estimates confirm the patterns observed in Figure 1. Publicly sharing information on a firm's debt outstanding and creditworthiness reduce the equilibrium amount of borrowing. To confirm that the observed patterns are due to information in the credit registry, we look at the influence of the firm ratings prior to the expansion on our estimates. We divide firms in two groups according to whether they received a risk rating higher than one (the lowest risk) by any lender prior to the registry expansion or not. The estimated parameters of specification (1) over the two groups are plotted in panel A of Figure 2 to facilitate their comparison. Firms that do not have a perfect credit rating credit record at the time their information is publicly shared experience a larger decline in total debt. The point estimates indicate that information sharing causes a fall of 23.3% in total debt during the first six months after the expansion ($\gamma_9 - \gamma_3 = 0.233$ in column 3 of Table 2). The estimates of the growth specification (2) corroborate that debt growth drops by 6.4 percentage points during the first four months (panel C of Table 3). This evidence indicates that releasing bad news about a borrower has a sharp and immediate negative effect on credit.

Firms with unblemished credit records at the time of the expansion also experience a decline in total debt after their credit information is shared. The decline, however, is smaller in magnitude and gradual over time. The estimated parameters imply a 9% drop in total debt between during the year after the expansion (column 2 of Table 2). The difference-in-difference estimates from the

growth specification (panel B of Table 3) corroborate that the average debt for this group of firms declines steadily throughout the year following the information release. These results indicate that public information has a persistent impact on debt growth beyond the initial revelation of news. This permanent effect on the equilibrium lending behavior is smaller than the immediate effect of revealing bad news, but it is economically significant and pertains to the majority of borrowers who have no pre-expansion indications of poor performance on the credit history.

We turn to estimate the effect of public credit information on default rates. To validate the identification assumptions of the differences-in-differences strategy we plot the cumulative default hazard rates for the borrowers affected by the registry expansion and those in the control group in our subsample (top panel of Figure 3). The cumulative hazard rate represents the fraction of the borrower-bank relationships that is in default at month t, conditional on being in good standing in $t = 1$ (January 1998). We observe a pattern similar to those in Figure 1: the fraction of firms that default in both groups of firms is similar up to April 1998, and increases for borrowers affected by the registry expansion thereafter. The cumulative hazard rate of borrowers affected by the registry expansion increases relative to the control group between April and July 1998 ($t \in [4, 7]$) and remains parallel afterwards. This suggests that public information has a sharp and potentially temporary impact on the hazard default rate soon after its release.

We proceed by comparing the empirical default hazard rates through a variation of specification (1):[12]

$$1[Default_{ijt} = 1|Default_{ijt-1} = 0]_{ijt} = \delta_{jt} + \sum_{month=2}^{16} \lambda_{month} Public\_Apr98_i \times Dum\_month_t + \zeta_{ijt} \quad (3)$$

The left-hand side variable is a dummy equal to zero as long as the relationship between firm i with bank j is in good standing, turns to one if default happens at time t, and drops out of the sample afterwards. The right-hand side, as before has an indicator variable for firms affected by the registry expansion interacted with calendar month dummies. The interaction coefficients represent the average difference in the default hazard rates across firms affected by the registry expansion and control firms. The relationship level specification allows us to include bank-month dummies to control for supply

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

side effects (for example, due to shocks to bank balance sheets).

The differences-in-differences estimate of the effect of public information on the default hazard rate is given by the time-series changes of the interaction coefficient. The default pattern in Figure 2 suggests that these coefficients remain unchanged prior to the registry expansion, increase during the four months after the expansion, and decline back to the pre-expansion level afterwards. The estimates, plotted in the bottom panel of Figure 3 confirm this. They imply that the registry expansion induces an increase in the monthly default hazard rate of 1.3 to 2.6 percentage points between April and June 1998 (column 1 of Table 4). The implied cumulative increase in default probability during the three months after the registry expansion is 5.8 percentage points (significant at the 5% confidence level). The estimates suggest that the registry expansion does not have a statistically significant effect on the default hazard rate after three months.

In the cross-section, the point estimates indicate a cumulative increase in default probability is 8.5 percentage points for firms with at least one risk rating of 2 prior to the registry expansion (column 3 of Table 4). The cumulative increase is 3.6 percentage points on the subsample of firms with a perfect credit rating. Although the default estimates on the subsamples are noisy and only marginally significant at standard levels, these are in line with the results on total debt, suggesting that the effect of the registry expansion is larger in magnitude on the subsample of firms with a less than perfect credit record at the time of the expansion.

V. Evidence on Lender Coordination Problems

The release of public information causes on average an immediate decline in the equilibrium amount of lending and increase in the probability of default. It also causes a permanent decline in future credit, even for borrowers with spotless credit records at the time of the information release. These results are striking considering that information asymmetries are the usual culprit behind financing constraints and rationing in credit markets. Also, these results are consistent with the hypothesis that information sharing increases the incidence of creditor runs. First, the incidence of creditor runs predicts that banks will be unwilling to provide additional finance to meet a firm's short term liquidity needs. This will have a direct negative impact on the observed average amount of financing and increase default rates. In addition, banks will tend to lend in anticipation of an increased probability of a run in the future. The present section is devoted to exploring whether coordination problems across lenders or other

economic forces consistent with the negative impact of public information on credit can explain the results.

A. Evidence from the Cross Section

A-1. Number of Lenders

Coordination problems can occur only among borrowers that obtain credit from multiple lenders. We estimate again specification (1) over the subsamples of firms with multiple lenders and a single lender prior to April 1998 (columns 4 and 7 of Table 2). The results show that the average effect of public information estimated over the full sample are driven by the decline in debt of firms with multiple lenders prior to the expansion. The difference between the point estimates for March 1999 and March 1998 ($\gamma_{16} - \gamma_3$) is -0.1 for the multiple lender subsample (significant at 5% level) and 0.007 for the single lender subsample (not significant). The magnitude, sign and statistical significance of the effect estimate is again corroborated by the difference-in-difference estimates using the growth specification (2) (columns 4 through 9 of Table 6).

Consistent with the coordination failure interpretation, these cross sectional results confirm that making credit information public has a negative impact on firms with multiple lenders and no effect on firms with a single lender. In addition, the fact that information sharing has no impact on single lender firms is inconsistent with an interpretation based on reduced incentives to monitor. The registry mandates sharing the information that banks possesses about a borrower with other lenders, which may reduce the banks' incentives to collect the information in the first place and lead to lower equilibrium levels of lending. However, this effect should be stronger when the bank is the sole lender of the firm and informational rents are higher. Our results suggest that reduced information collection incentives are not the main force driving the negative effect of public information on debt. 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)).

A-2. Hidden Lending

If firms engage in hidden lending we may expect to see a reduction in the supply of credit to firms with multiple lenders at the time this is made public. A bank that is unaware of the number of lenders a firm is obtaining credit from will become informed after the registry expansion. As a result, the

bank will revise upward its expectation on the creditworthiness of firms that have a single lender ex post, and potentially downwards that of firms with multiple lenders. For hidden lending to explain the decline in debt of firms with multiple lenders, we should be able to observe in the data a positive effect of the registry on firms with a single lender.

In our chosen subsample between \$175,000 and \$225,000, however, a firm with a single borrower can have a limited amount of hidden lending. This limit is given by the distance to the \$200,000 threshold. Suppose for example that a bank lends \$190,000 to a firm whose information is not shared through the registry prior to April 1998. The bank may not know whether the firm is obtaining credit from other banks, but it knows that the total amount of credit from other banks is smaller than \$10,000. Otherwise, the information of the firm would appear in the registry.

Thus, to test whether the registry expansion has a positive effect on firms with a single lender we must look at firms farther away from the threshold. To do this we estimate the following variation of specification (2) over the subsample of firms that: 1) were affected by the registry expansion, 2) have debt between \$100,000 and \$200,000 in March, and 3) have a single lender prior to March:

$$[\ln(Debt_{iApril98}) - \ln(Debt_{iJan98})] - [\ln(Debt_{iApril99}) - \ln(Debt_{iMay98})] =$$
$$\alpha + \delta_i' + \beta_1 D\_150\_175_i + \beta_2 D\_125\_150_i + \beta_3 D\_100\_125_i + \varepsilon_i'$$

The right-hand side variables $D\_150\_175_i$, $D\_125\_150_i$, $D\_100\_125_i$ are dummies equal to one if firm i has debt between \$150,000 and \$175,000, \$125,000 and \$150,000, and \$100,000 and \$125,000 in March 1998 respectively. The coefficients represent the change in debt growth before and after the registry expansion for affected firms in each of these debt intervals, relative to those in the \$175,000 to \$200,000 debt range (not statistically different from zero). Under the hidden information hypothesis, the firm's potential hidden debt is larger as its debt prior to the registry expansion is lower. Thus, this hypothesis implies that $\beta_3 > \beta_2 > \beta_1$. The point estimates, shown in Table 5, are not ordered according to this prediction. All the estimates are not statistically different from zero, which indicates that the registry expansion did not have an effect on the debt growth of firms with a single lender, regardless of their distance to the \$200,000 threshold. This evidence confirms that hidden lending is not a major force driving the lending results.

B. Response to Coordination Failures: Lending Concentration

If public information exacerbates lender coordination problems, we expect borrowers to mitigate these problems by lowering the number of banks from which they lend and increasing the concentration of their lending across banks. This idea is related to Corsetti et. al. (2004) who show that the presence of an agent with large market share can reduce the incidence of coordination failures in the context of currency attacks. We test these hypotheses with specification (1) using number of lenders ($\#Lenders$), debt concentration ($DebtHHI$), and the fraction of debt with the main lender ($\%TopLender$) as dependent variables. The estimated coefficients over the subsample of firms with multiple lenders prior to April are shown in Table 7 and Figure 4. The point estimates indicate that between March 1998 and March 1999, firms affected by the registry expansion borrowed from 10.4% fewer lenders, and increased the fraction of borrowing from their top lender by 8.7%. These changes induced an increase of 0.12 in the HHI of debt concentration across borrowers. Both the plotted results in Figure 4 and the difference-in-difference estimates suggest that debt concentration may have not achieved a new steady state a year after the registry expansion. If this is the case, the difference-in-difference point estimates may underestimate the overall long run effect of the registry expansion on debt concentration. The results indicate that within the same firm, public information caused a substantial decline in the amount of debt held with lenders that had smaller stakes in the firm prior to the registry expansion.

C. Creditor Runs: Within-Firm Lending Correlation

We have shown that information sharing results in reduced lending and an increase in default for firms who borrow from multiple banks. We have argued that these patterns arise because increased public information exacerbates the possibility of a creditor run. The key intuition is that a creditor is less willing to extend additional credit if she predicts that other creditors will be attempting to liquidate their own claims on the firm. If unfavorable information about a creditor is released publicly it not only lowers each bank's assessment of the firm's fundamental creditworthiness but also indicates that other creditors will be more likely to foreclose. Central to this story is the idea that shared information has a multiplier effect because it increases the ability of each creditor to ensure she extends additional credit only when other banks are also providing liquidity.

We look for this effect directly by testing whether the registry expansion increased the contem-

poraneous correlation of changes in lending from different banks to the same firm. As before, we measure this change relative to the correlation in lending to firms in our control group. We estimate the following relationship level specification:

$$\begin{aligned} \ln\left(Debt_{ijt}\right) &= \alpha_{ij} + \delta_t + \tau_i t + \sum_{m=2}^{16} \beta_{1\_m} \ln\left(TDebt_{i(-j)t}\right) \times Dum\_m_t + \\ & \sum_{m=2}^{16} \beta_{2\_m} \ln\left(TDebt_{i(-j)t}\right) \times Public\_Apr98_i \times Dum\_m_t + \omega_{ijt} \end{aligned} \tag{4}$$

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}, measures the contemporaneous correlation of debt across the lenders of the same firm in month m. The coefficient on the interaction with $Public\_Apr98$, β_{2_m}, is the difference-in-difference estimate of the effect of the registry expansion on this contemporaneous correlation. We estimated the standard errors allowing for clustering at the firm level to account for the mechanical correlation across different observations for the same firm in the regression estimation. We estimate by first differencing over two months to reduce the noise inherent in monthly lending changes.[13]

The estimated β_{2_m} are reported in Table 7 and plotted in panel A of Figure 5. Prior to information being shared in April 1998, the correlation was not different between the firms affected by the expansion and those in the control group. The registry expansion resulted in a statistically significant increase in this correlation. The point estimates indicate that the lending correlation across different banks increases on average by 15 percentage points after the registry expansion. This is consistent with our hypothesis that additional public information increases the degree to which creditors were able to align their lending decisions with other banks. Note that this coincides with the period when information sharing produces a dramatic increase in the hazard rate of default and is thus consistent with our hypothesis that these were creditor runs.

To understand the observed increase in bank correlation it is useful to compare these results to a counterfactual benchmark where banks have no incentive to coordinate and simply use the registry to obtain information about the creditworthiness of each borrower. We briefly outline such a model in

[13] We obtain qualitatively similar results using monthly changes in lending, but the estimates are noisy.

Appendix A assuming that each bank sets lending as an increasing function of its assessment of firm creditworthiness. We run the same regression on data simulated from this simple Bayesian learning framework. These estimates are shown in panel B of Figure 5 and stand in stark contrast to our estimations. At the time information is first shared the contemporaneous correlation in lending sharply decreases and then settles at a new lower level. This decrease occurs because, absent strategic considerations, each bank uses public information to correct errors in their assessment of firm creditworthiness. If for example, shared information indicates that bank A's assessment of a firm's creditworthiness is higher than bank B's then both will update their assessment in the opposite direction. A will reduce its assessment of the firm's creditworthiness and similarly B will raise its assessment.[14] Our empirical results indicate that banks are not simply using information in the registry to correct their assessment of each firm's creditworthiness. Instead, it appears they are using the information to increase the degree to which lending decisions are coordinated.

The results reported in panel A of Figure 5 indicate that 5 months after information is shared, the within firm correlation in lending has returned to the pre-registry expansion level (relative to the control group). This suggests that bank's motive to coordinate their actions falls over this period. Note that this is accompanied by the increase in lending concentration that we documented above. Our interpretation is that increased lender concentration is successful in lowering a firm's vulnerability to creditor runs.

VI. Conclusion

We provide evidence that information sharing exacerbates coordination problems in credit markets. We demonstrate this by exploiting a natural policy experiment created by the expansion of a public credit registry in Argentina in April 1998. The effect of information sharing is identified by comparing firms who were affected by the expansion (total lending between \$175,000 and \$200,000) with comparable firms who were not affected by the change (lending between \$200,000 and \$225,000). We find that information sharing lowers the amount of lending a firm receives and increases the hazard rate of default. Consistent with our hypothesis that information sharing exacerbates coordination failures we show that these effects are only present for firms that borrow from multiple banks. Further we show

[14] This effect will be most pronounced when information is first shared as a stock of information is released through other banks' level of lending.

that firms endogenously react to limit these coordination problems by increasing the concentration of their lending. Consistent with this we find that the correlation in lending decisions is higher immediately after the expansion but returns to its previous level 12 months later once firms have significantly concentrated their lending.

We offer some caution in using our results to draw an overall evaluation of credit registries around the world. Our primary caveat stems from the fact that we are only able to identify the effect of information sharing on medium sized firms, with total debt close to $200,000 at the time of the registry expansion. It may be that smaller or new borrowers experienced effects different to those which we are able to study using this natural experiment. In addition, as the survey in Miller (2003) indicates, there is substantial heterogeneity across public credit registries. We therefore view our results as providing detailed evidence of one important effect of information sharing in credit markets.

Finally, our results also relate to the literature which studies the optimal number of creditors. Our analysis suggests that one important force is that firms may limit their number of creditors so as to prevent coordination problems. Bolton and Scharfstein (1996) have argued that there may be ex-ante benefits from ex-post coordination problems with creditors at the time of default. We do not see direct evidence of that ex-ante benefit in our setting. However it may be that the heightened probability of a creditor run may serve to discipline some borrowers ex-ante. More generally, our natural experiment does not provide evidence on the forces that lead a borrower to choose multiple creditors.

Appendix: Bayesian Learning and Information Sharing

In this appendix we build a stylized model to study the effect of information sharing in an environment where banks have no coordination motive. Instead we suppose that a bank's objective is to use all available information to assess a borrower's fundamental credit worthiness. We abstract from the details of lending and assume that each bank chooses to lend an amount that is increasing linearly in its assessment of a firm's creditworthiness.

Suppose there are 2 banks, A and B, lending to the same firm. Time is discrete and indexed by t. Denote the creditworthiness of the firm in period t by θ_t. This is not directly observed by either bank. The firm's creditworthiness evolves stochastically over time so that:

$$\theta_t = \theta_{t-1} + \varepsilon_t$$

where $\varepsilon_t \tilde{} N\left(0, \sigma_\varepsilon^2\right)$. Assume that the initial state of the firm's creditworthiness, θ_1, is drawn from a mean zero normal distribution with variance σ_1^2. Each bank learns about the firm's credit worthiness by receiving a private signal each period. Bank i's signal in period t is:

$$x_t^i = \theta_t + \eta_t^i$$

where $\eta_t^i \tilde{} N\left(0, \sigma_i^2\right)$ for $i = A, B$. Assume that all signal noise is independent across time and across banks. Each bank attempts to optimally adjust the level of lending it sets each period L_t^i for $i = A, B$ with noisy control. In particular, the level of lending in each period is a linear function of the banks' current posterior belief about the firm's type:

$$L_t^i = \Gamma^i E\left(\theta_t | I_t^i\right) + w_t^i$$

where: Γ^i is a positive publicly known constant; $w_t^i \tilde{} N\left(0, \sigma_{wi}^2\right)$ reflects all other stochastic shocks to the actual realized level of lending; and, I_t^i is bank i's information set at t. For simplicity assume that w_t^i is independent across time and banks.

We simulate this model numerically using an OLS regression with 15 lags of all available information to estimate $E\left(\theta_t | I_t^i\right)$ for each bank. We set simulation time so that in April 1998 bank's can begin to see the three month lagged level of lending, L_{t-3}^{-i} set by the other bank. We set:

$$\sigma_1 = 25, \sigma_\varepsilon = 1, \sigma_A = \sigma_B = 1, \Gamma^A = \Gamma^B = 1, \sigma_{wA} = \sigma_{wB} = 2$$

and simulate the pattern of lending that evolves for each bank 200,000 times. Using this simulated lending data we run the following cross-sectional regression at each time t:

$$L_{k,t}^A - L_{k,t-2}^A = \beta_0^t + \beta_1^t \left(L_{k,t}^B - L_{k,t-2}^B\right) + \eta_{k,t}$$

across each 200,000 simulated firms indexed by k. The β_1^t coefficients are plotted in panel B of Figure 5.

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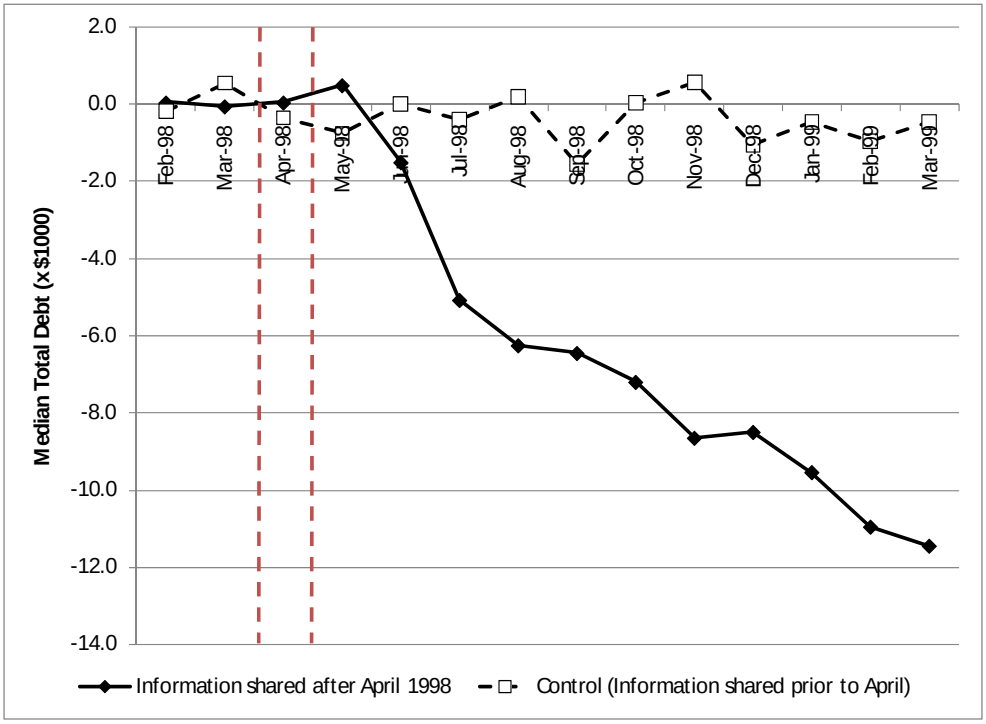
Figure 1
Time Series of Selected Firm Characteristics,
for Firms Affected by the Registry Expansion and Control Firms
Means and Trends prior to Registry Expansion Removed

Panel A: Median Debt



Panel B: Average Debt HHI (Debt Concentration)

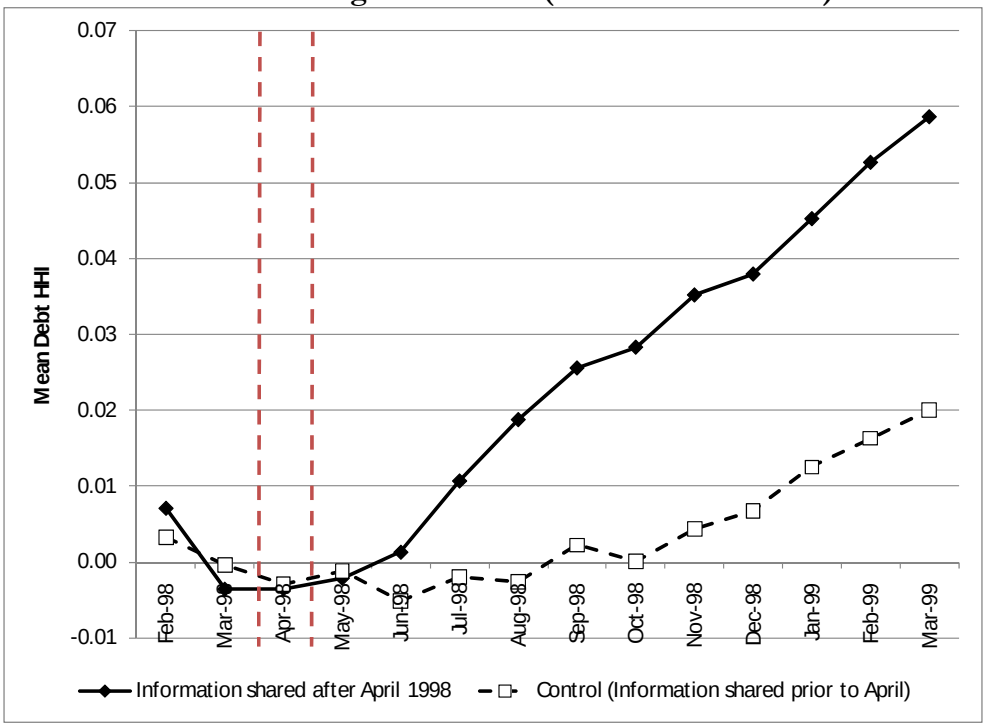
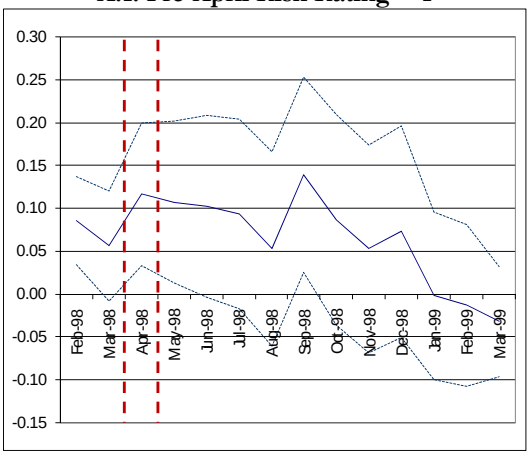


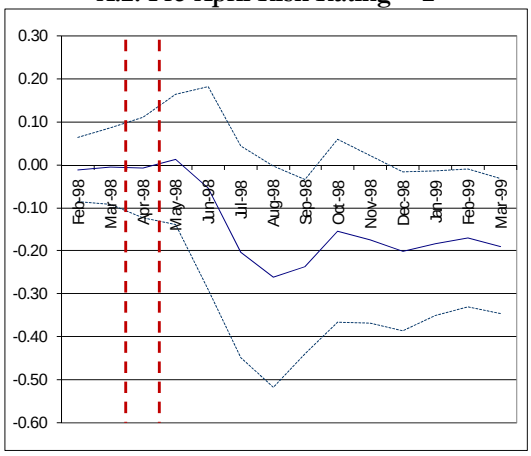
Figure 2
Difference of (log) Debt between Firms Affected by Expansion and Control Firms

Panel A: All Firms

A.1. Pre-April Risk Rating = 1

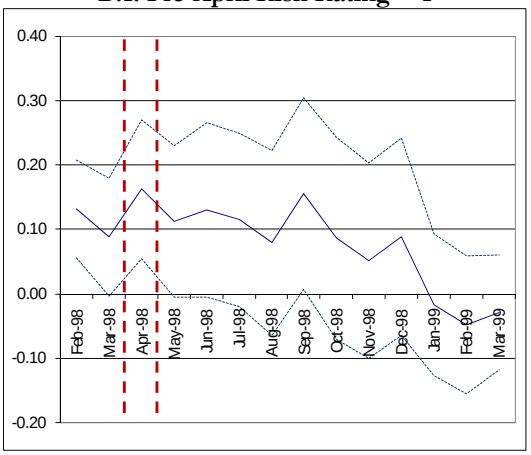


A.2. Pre-April Risk Rating = 2

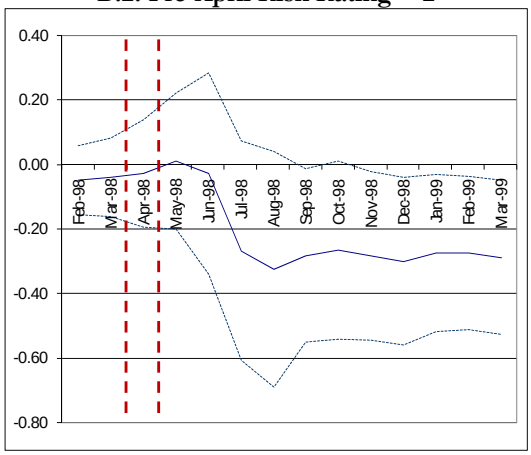


Panel B: Firms with Multiple Lenders prior to April

B.1. Pre-April Risk Rating = 1

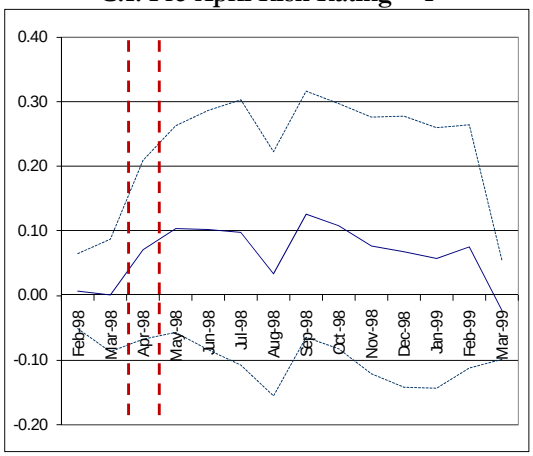


B.2. Pre-April Risk Rating = 2



Panel C: Firms with Single Lender prior to April

C.1. Pre-April Risk Rating = 1



C.2. Pre-April Risk Rating = 2

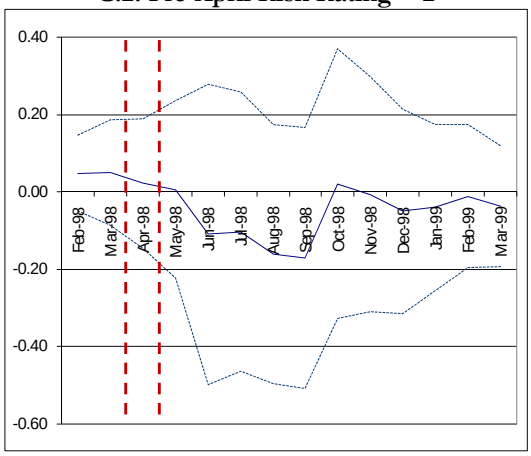
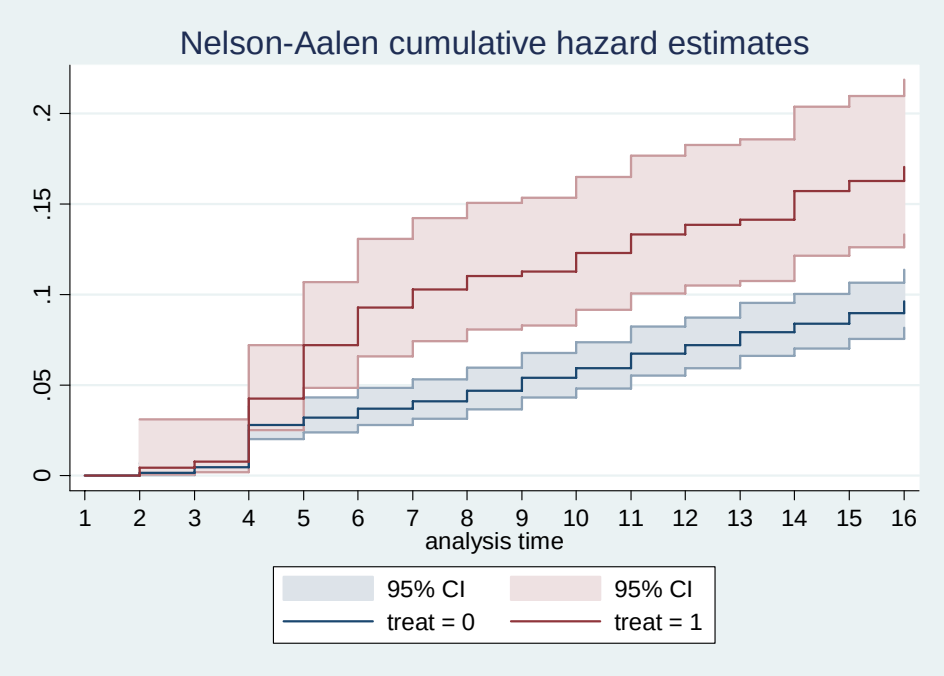


Figure 3
Effect of Information sharing on Default Rates

Panel A: Cumulative Default Hazard Rate,
Firms Affected by the Registry Expansion (treat=1) and Control Firms (treat=0)
Analysis time = 1 corresponds to January 1998



Panel B: Default Hazard Rate Difference between Firms Affected by Expansion and Control Firms

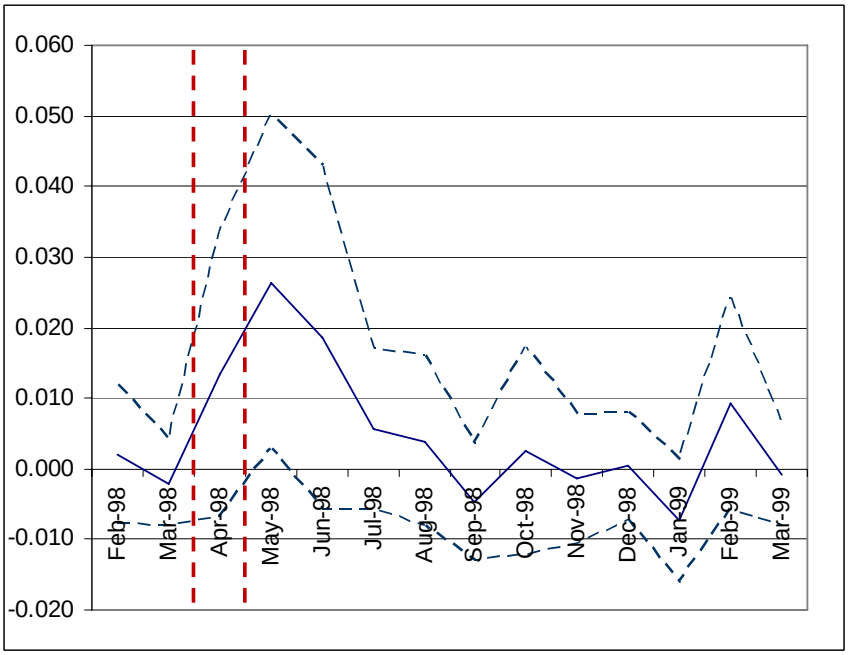
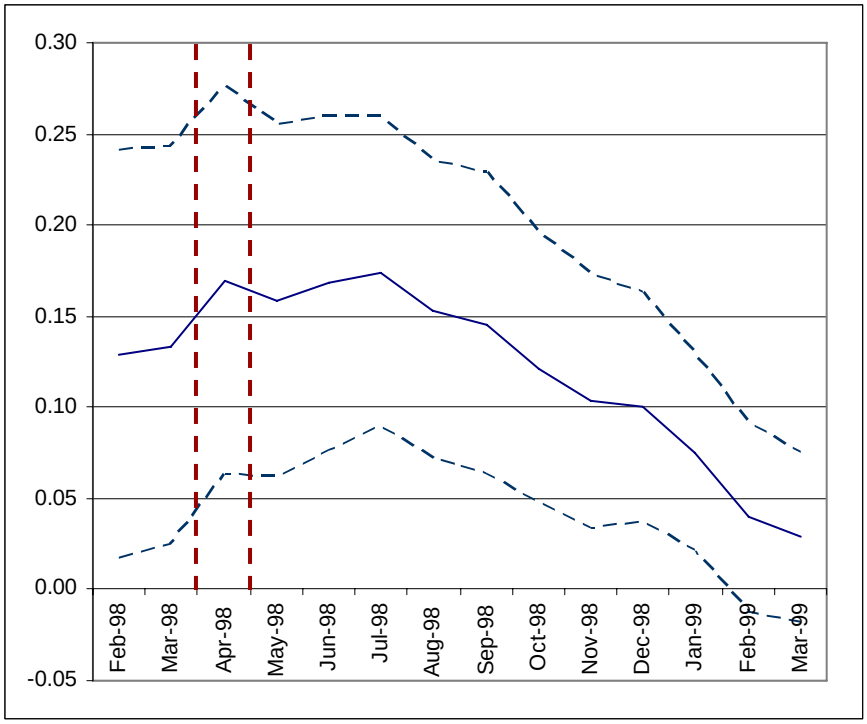


Figure 4
Difference of Debt Concentration Measures between Firms Affected by Expansion and Control Firms, Firms with Multiple Lenders Prior to April

Panel A: log Number of Lenders



Panel B: Debt HHI

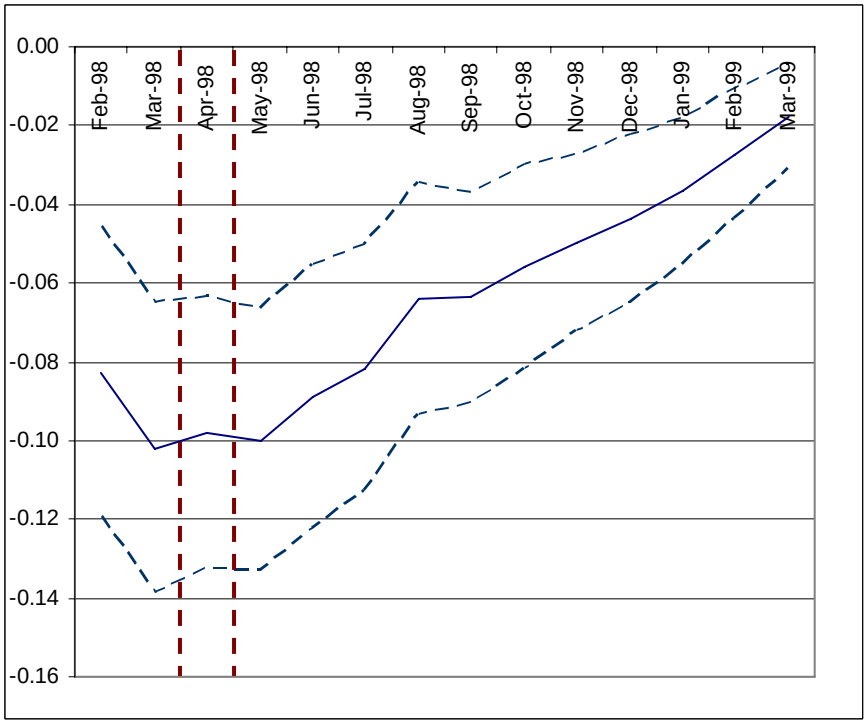
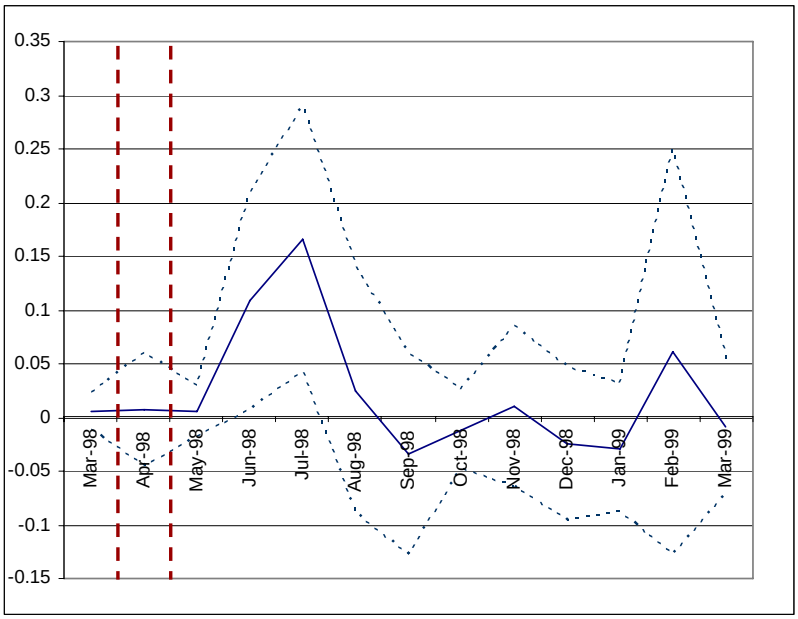


Figure 5
Contemporaneous Correlation of Debt across Lenders of Same Firm

Panel A: Estimated Effect of Registry on Linear Projection of Own Lending on other Banks' Lending to the Same Firm



Panel B: Simulated Effect of Registry under Bayesian Learning (no Coordination Failure)

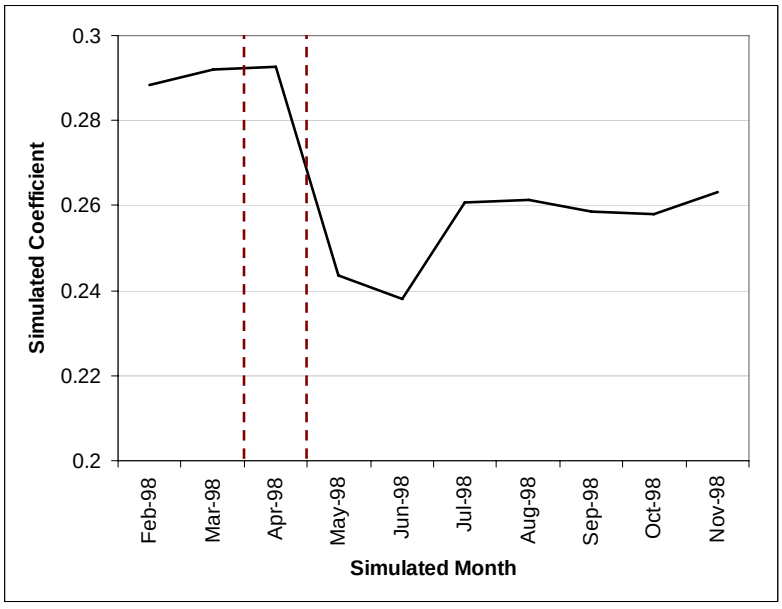


Table 1
Panel Descriptive Statistics, January 1998 to April 1999
Firms with total debt between $175,000 and $225,000 before April 1998 (1,814 firms)

variable	mean	sd	p50	min	max	N
Firm level statistics						
Total debt	183.4	198.1	174.6	0.1	9,862	31,244
Number of lenders	1.97	1.15	2	1	10	31,244
Debt concentration (hhi)	0.83	0.21	0.98	0.17	1	31,244
Fraction debt from lead bank	0.88	0.17	0.99	0.23	1	31,244
Total collateral	108.7	91.5	114.9	0	4,391	31,244
Collateral/Debt	0.60	0.39	0.75	0	1.00	31,244
Average risk rating	1.85	1.23	1.00	1.00	6.00	31,244
Std. Dev. of same firm ratings	0.54	0.78	0.00	0	2.83	17,600
Fraction in Private Bureau	0.48	0.50	0.00	0	1.00	31,244
Relationship level statistics						
Debt	93.0	129.0	76.0	0	7,103	61,604
Collateral	55.1	77.2	10.7	0	4,332	61,604
Risk rating	1.7	1.2	1	1	6	61,604
In default	0.139	0.35	0	0	1	61,604
Fraction in Private Bureau	0.487	0.50	0	0	1	61,604

Table 2
Monthly Average Difference in (log) Debt between Borrowers Affected by Registry Expansion and Control Group

Sample: Firms with total debt between $175,000 and $225,000 before April 1998. The dependent variable is the (log) debt of borrower *i* at time *t*. The right hand side variable of interest is the interaction between a dummy equal to one if borrower *i* had total debt below $200,000 before April (*Treated*) and a month dummy. Estimates are obtained after first differencing, and include firms fixed effects and month dummies. The reported coefficients represent the monthly (log) debt of firms with total debt below $200,000 prior to April (treatment), relative to firms with total debt above $200,000 (control), after controlling for unobserved bank-firm relationship heterogeneity and aggregate month specific shocks. Robust standard errors clustered at the borrower level. *, **, and *** indicate point estimate statistically significant at the 10%, 5% and 1% level.

Dependent Variable	$\ln(Debt_{it})$								
Firm Sample	All			Multiple Lenders			Single Lender		
Max Risk Rating Prior to April	1 or 2	1	2	1 or 2	1	2	1 or 2	1	2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dum_1998_02 x Public_Apr98	0.069***	0.086***	-0.011	0.026	0.052	-0.036	0.093*	0.09	0.109*
	(0.022)	(0.026)	(0.038)	(0.038)	(0.056)	(0.044)	(0.050)	(0.061)	(0.062)
Dum_1998_03 x Public_Apr98	0.046*	0.056*	-0.003	-0.007	0.009	-0.04	0.131**	0.125*	0.161*
	(0.028)	(0.033)	(0.045)	(0.051)	(0.071)	(0.050)	(0.060)	(0.072)	(0.082)
Dum_1998_04 x Public_Apr98	0.099***	0.116***	-0.006	0.097	0.114	0.038	0.244***	0.259**	0.179*
	(0.035)	(0.042)	(0.060)	(0.063)	(0.085)	(0.087)	(0.092)	(0.111)	(0.091)
Dum_1998_05 x Public_Apr98	0.091**	0.107**	0.014	0.092	0.103	0.081	0.277***	0.293**	0.231*
	(0.041)	(0.048)	(0.077)	(0.071)	(0.094)	(0.099)	(0.103)	(0.124)	(0.134)
Dum_1998_06 x Public_Apr98	0.078	0.102*	-0.053	0.057	0.085	-0.027	0.257**	0.277*	0.163
	(0.049)	(0.054)	(0.119)	(0.073)	(0.084)	(0.180)	(0.119)	(0.143)	(0.147)
Dum_1998_07 x Public_Apr98	0.046	0.093*	-0.202	-0.026	0.04	-0.236	0.244*	0.270*	0.118
	(0.051)	(0.056)	(0.124)	(0.079)	(0.083)	(0.211)	(0.130)	(0.157)	(0.148)
Dum_1998_08 x Public_Apr98	0.002	0.053	-0.260**	-0.099	-0.034	-0.293	0.128	0.141	0.061
	(0.054)	(0.058)	(0.130)	(0.086)	(0.083)	(0.250)	(0.118)	(0.143)	(0.138)
Dum_1998_09 x Public_Apr98	0.086*	0.139**	-0.236**	-0.044	0.009	-0.213	0.18	0.202	0.059
	(0.051)	(0.058)	(0.102)	(0.065)	(0.070)	(0.160)	(0.124)	(0.150)	(0.147)
Dum_1998_10 x Public_Apr98	0.049	0.087	-0.152	-0.141*	-0.12	-0.207	0.177	0.201	0.041
	(0.054)	(0.062)	(0.107)	(0.077)	(0.091)	(0.156)	(0.122)	(0.148)	(0.152)
Dum_1998_11 x Public_Apr98	0.015	0.053	-0.173*	-0.159**	-0.136*	-0.243*	0.16	0.182	0.089
	(0.053)	(0.062)	(0.099)	(0.069)	(0.083)	(0.130)	(0.128)	(0.157)	(0.105)
Dum_1998_12 x Public_Apr98	0.032	0.073	-0.201**	-0.113*	-0.088	-0.216*	0.15	0.179	0.012
	(0.053)	(0.063)	(0.094)	(0.066)	(0.080)	(0.120)	(0.137)	(0.168)	(0.088)
Dum_1999_01 x Public_Apr98	-0.027	-0.002	-0.182**	-0.104*	-0.087	-0.180*	0.185	0.227	-0.004
	(0.043)	(0.050)	(0.085)	(0.059)	(0.074)	(0.094)	(0.140)	(0.172)	(0.067)
Dum_1999_02 x Public_Apr98	-0.033	-0.013	-0.170**	-0.105*	-0.097	-0.156**	0.207	0.258	-0.013
	(0.041)	(0.048)	(0.081)	(0.056)	(0.072)	(0.078)	(0.152)	(0.187)	(0.064)
Dum_1999_03 x Public_Apr98	-0.049*	-0.032	-0.190**	-0.107**	-0.094*	-0.174**	0.138	0.174	-0.033
	(0.029)	(0.033)	(0.079)	(0.045)	(0.055)	(0.077)	(0.093)	(0.113)	(0.067)
Dum_1999_04 x Public_Apr98	-0.009	0.003	-0.088	-0.059*	-0.052	-0.089	0.149*	0.182*	0.015
	(0.023)	(0.026)	(0.062)	(0.035)	(0.043)	(0.065)	(0.087)	(0.107)	(0.030)
Dum_1999_05 x Public_Apr98	-0.013	-0.007	-0.066	-0.041**	-0.044*	-0.029	0.109	0.138	-0.02
	(0.017)	(0.020)	(0.048)	(0.019)	(0.024)	(0.036)	(0.077)	(0.094)	(0.022)
First Differenced	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,859	15,205	1,654	13,172	11,484	1,688	13,773	12,566	1,207
R-squared	0.07	0.07	0.09	0.07	0.08	0.08	0.06	0.06	0.12

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

Sample: Firms with total debt between $175,000 and $225,000 before April 1998. The dependent variable is the change in (log) debt of borrower *i* with bank *j* before and after April 1998. The pre-April average is calculated over January through April. The post-April average is calculated over the 4, 8 and 12 consecutive months beginning in May 1998, depending on the specification. Reported is the coefficient on a dummy equal to one if borrower *i* had total debt below $200,000 before April (*Treated*). Robust standard errors clustered at the borrower level. *, **, and *** indicate point estimate statistically significant at the 10%, 5% and 1% level.

Dependent Variable	ln(Average Debt Post-April98$_i$) - ln(Average Debt Pre-April98$_i$)								
Sub-Sample	All			Multiple Lenders			Single Lender		
Post period (months)	4	8	12	4	8	12	4	8	12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. All Firms									
Public_Apr98	-0.051***	-0.036**	-0.037***	-0.062***	-0.047**	-0.047**	-0.035	-0.02	-0.023
	(0.017)	(0.014)	(0.014)	(0.023)	(0.019)	(0.018)	(0.023)	(0.020)	(0.022)
Bank Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	994	994	994	502	502	502	492	492	492
R-squared	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
Panel B. Subsample: Maximum Pre-Apri 1998 Risk Rating = 1									
Public_Apr98	-0.045**	-0.037**	-0.041**	-0.057**	-0.049**	-0.054**	-0.03	-0.02	-0.025
	(0.019)	(0.017)	(0.017)	(0.025)	(0.022)	(0.022)	(0.027)	(0.024)	(0.026)
Bank Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	899	899	899	443	443	443	456	456	456
R-squared	0.01	0.01	0.01	0.01	0.01	0.02	0.01	0.01	0.01
Panel C. Subsample: Maximum Pre-Apri 1998 Risk Rating = 2									
Public_Apr98	-0.064**	-0.022	-0.004	-0.051*	-0.028	-0.001	-0.071	-0.013	-0.008
	(0.032)	(0.022)	(0.020)	(0.030)	(0.030)	(0.027)	(0.048)	(0.032)	(0.027)
Bank Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	95	95	95	59	59	59	36	36	36
R-squared	0.05	0.01	0.01	0.08	0.01	0.01	0.05	0.01	0.01

Table 4
Effect of Information Sharing on Default Hazard Rate

Sample: Firms with total debt between $175,000 and $225,000 and multiple lenders before April 1998. The table shows the results of the OLS estimation of specification (3), first differenced, and over the subsamples of firms with the maximum risk rating prior to April 1998 equal to 1 and 2. multiple lenders and a single lender. Each coefficient represents a difference in a monthly default hazard rate between firms affected by the expansion and control firms.

Dependent Variable	Dum = 1 if relationship defaults at t, and has not defaulted until t-1		
Max Risk Rating Prior to April	1 or 2	1	2
	(1)	(2)	(3)
Dum_1998_02 x Public_Apr98	0.002	0.003	0
	(0.005)	(0.006)	0.000
Dum_1998_03 x Public_Apr98	-0.002	-0.003**	0.017
	(0.003)	(0.001)	(0.016)
Dum_1998_04 x Public_Apr98	0.013	0.018*	-0.040
	(0.010)	(0.011)	(0.033)
Dum_1998_05 x Public_Apr98	0.026**	-0.001	0.123**
	(0.012)	(0.004)	(0.049)
Dum_1998_06 x Public_Apr98	0.019	0.019	0.002
	(0.012)	(0.015)	(0.002)
Dum_1998_07 x Public_Apr98	0.006	0.002	0.02
	(0.006)	(0.005)	(0.018)
Dum_1998_08 x Public_Apr98	0.004	0	0.026
	(0.006)	(0.004)	(0.039)
Dum_1998_09 x Public_Apr98	-0.005	-0.002	0
	(0.004)	(0.004)	0.000
Dum_1998_10 x Public_Apr98	0.002	-0.002	0.034
	(0.007)	(0.005)	(0.029)
Dum_1998_11 x Public_Apr98	-0.001	-0.005**	-0.003
	(0.005)	(0.002)	(0.038)
Dum_1998_12 x Public_Apr98	0	0.002	-0.016
	(0.004)	(0.004)	(0.020)
Dum_1999_01 x Public_Apr98	-0.007*	-0.009***	0.018
	(0.004)	(0.003)	(0.027)
Dum_1999_02 x Public_Apr98	0.009	0.001	0.039
	(0.008)	(0.006)	(0.028)
Dum_1999_03 x Public_Apr98	-0.001	-0.003**	-0.021
	(0.004)	(0.001)	(0.032)
Dum_1999_04 x Public_Apr98	0.001	0.002	0.000
	(0.004)	(0.005)	0.000
Dum_1999_05 x Public_Apr98	0.003	-0.001	0.033
	(0.007)	(0.003)	(0.031)
Relationship in sample after default	No	No	No
Bank x month dummies	Yes	Yes	Yes
Observations	34,878	31,521	3,357
R-squared	0.06	0.07	0.25

Table 5
Hidden Information? Effect of Public Information on Single Lender Firm (log) Debt by Closeness to the $200,000 Threshold

Sample: Firms affected by the registry expansion with total debt between $100,000 and $200,000 before April 1998. The dependent variable is the change in (log) debt of borrower *i* with bank *j* before and after April 1998. The pre-April average is calculated over January through April. The post-April average is calculated over the 4, 8 and 12 consecutive months beginning in May 1998, depending on the specification. Reported is the coefficient on a dummy equal to one if borrower *i* had total debt between $150,000-$175,000 and $125,000-$150,000, and $100,000-$125,000 in March 1998. The coefficients represent the magnitude of the difference-in-differences estimate of public information on debt growth, by distance to the $200,000 threshold. Robust standard errors clustered at the borrower level. *, **, and *** indicate point estimate statistically significant at the 10%, 5% and 1% level.

Dependent Variable	ln(Average Debt Post-April98$_i$) - ln(Average Debt Pre-April98$_i$)		
Post period (months)	4	8	12
	(1)	(2)	(3)
Panel A. All Firms			
D_150_175	0.018	0.010	0.005
	(0.034)	(0.026)	(0.027)
D_125_150	0.001	-0.007	-0.014
	(0.042)	(0.037)	(0.037)
D_100_125	-0.028	-0.050	-0.049
	(0.054)	(0.052)	(0.051)
Bank Dummies	Yes	Yes	Yes
Observations	194	194	194
R-squared	0	0.01	0.01

Table 6
Monthly Average Difference in (log) Number of Lenders and Debt Concentration between Borrowers Affected by Registry Expansion and Control Group

Sample: Firms with total debt between $175,000 and $225,000 and multiple lenders before April 1998. The dependent variables are the (log) number of lenders, the debt HHI, and the fraction of debt with the main lender, of firm i at month t. The right-hand side variable of interest is the interaction between a dummy equal to one if borrower *i* had total debt below $200,000 before April and a month dummy. Estimates are obtained after first differencing, and include month dummies. The reported coefficients represent the average difference of the outcome variable of firms with total debt below $200,000 prior to April, relative to control firms, after controlling for unobserved firm heterogeneity and aggregate shocks. Robust standard errors clustered at the borrower level. *, **, and *** indicate point estimate statistically significant at the 10%, 5% and 1% level.

Dependent Variable	$\#Lenders_{it}$	$DebtHHI_{it}$	$\%TopLender_{it}$
	(1)	(2)	(3)
Dum_1998_02 x Public_Apr98	0.119**	-0.106***	-0.083***
	(0.06)	(0.03)	(0.02)
Dum_1998_03 x Public_Apr98	0.124**	-0.129***	-0.098***
	(0.06)	(0.03)	(0.02)
Dum_1998_04 x Public_Apr98	0.169***	-0.116***	-0.089***
	(0.05)	(0.03)	(0.02)
Dum_1998_05 x Public_Apr98	0.158***	-0.111***	-0.085***
	(0.05)	(0.03)	(0.02)
Dum_1998_06 x Public_Apr98	0.168***	-0.093***	-0.070***
	(0.05)	(0.03)	(0.02)
Dum_1998_07 x Public_Apr98	0.174***	-0.085***	-0.067***
	(0.04)	(0.02)	(0.02)
Dum_1998_08 x Public_Apr98	0.153***	-0.062***	-0.046**
	(0.04)	(0.02)	(0.02)
Dum_1998_09 x Public_Apr98	0.146***	-0.063***	-0.044***
	(0.04)	(0.02)	(0.02)
Dum_1998_10 x Public_Apr98	0.122***	-0.057***	-0.046***
	(0.04)	(0.02)	(0.02)
Dum_1998_11 x Public_Apr98	0.103***	-0.050***	-0.038***
	(0.04)	(0.02)	(0.01)
Dum_1998_12 x Public_Apr98	0.100***	-0.046***	-0.037***
	(0.03)	(0.02)	(0.01)
Dum_1999_01 x Public_Apr98	0.075***	-0.040***	-0.033***
	(0.03)	(0.01)	(0.01)
Dum_1999_02 x Public_Apr98	0.039	-0.026**	-0.024**
	(0.03)	(0.01)	(0.01)
Dum_1999_03 x Public_Apr98	0.029	-0.012	-0.011
	(0.02)	(0.01)	(0.01)
Dum_1999_04 x Public_Apr98	0.032*	-0.007	-0.007
	(0.02)	(0.01)	(0.01)
Dum_1999_05 x Public_Apr98	0.014	0.002	0.003
	(0.01)	(0.01)	(0.01)
First Differenced	Yes	Yes	Yes
Month dummies	Yes	Yes	Yes
Observations	8,686	8,686	8,686
R-squared	0.22	0.12	0.10

Table 7
Effect of Information Sharing on Lending Coordination

Sample: Firms with total debt between $175,000 and $225,000 and multiple lenders before April 1998. The table shows the results of the OLS estimation of specification (4) in the paper (after first differencing to account for relationship specific heterogeneity). The dependent variable is (log) debt of firm i with bank j at month t. The right hand side variable of interest is the (log) total debt of firm *i* with all banks except *j*. The variable is also interacted with a dummy equal to one if borrower *i* had total debt below $200,000 before April and calendar month dummies. The specification includes bank-month interaction dummies and controls for common time trends in the treatment and control groups. Robust standard errors clustered at the borrower level. *, **, and *** indicate point estimate statistically significant at the 10%, 5% and 1% level.

Dependent Variable	$Tdebt_{i(-j)t}$
	(1)
Debt x Dum_1998_02 x Public_Apr98	0.002
	(0.00)
Debt x Dum_1998_03 x Public_Apr98	0.005
	(0.00)
Debt x Dum_1998_04 x Public_Apr98	0.007
	(0.03)
Debt x Dum_1998_05 x Public_Apr98	0.006
	(0.01)
Debt x Dum_1998_06 x Public_Apr98	0.109**
	(0.05)
Debt x Dum_1998_07 x Public_Apr98	0.166***
	(0.06)
Debt x Dum_1998_08 x Public_Apr98	0.025
	(0.06)
Debt x Dum_1998_09 x Public_Apr98	-0.035
	(0.05)
Debt x Dum_1998_10 x Public_Apr98	-0.011
	(0.02)
Debt x Dum_1998_11 x Public_Apr98	0.01
	(0.04)
Debt x Dum_1998_12 x Public_Apr98	-0.025
	(0.04)
Debt x Dum_1999_01 x Public_Apr98	-0.029
	(0.03)
Debt x Dum_1999_02 x Public_Apr98	0.061
	(0.10)
Debt x Dum_1999_03 x Public_Apr98	-0.008
	(0.03)
Debt x Dum_1999_04 x Public_Apr98	0.043
	(0.04)
Debt x Dum_1999_05 x Public_Apr98	0.009
	(0.03)
First Differenced	Yes
Month-Debt x Dummies	Yes
Firm specific trends	Yes
Firm Fixed Effects	Yes
Bank-Month dummies	Yes
Observations	20,306
R-squared	0.11