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Abstract

We show that banks manipulate the credit ratings of their borrowers before being compelled to share them with competing banks. Using a unique feature on the timing of information disclosure of the Argentinean public credit registry, we disentangle the effect of manipulation from learning of credit ratings. We show that banks downgrade high quality borrowers on which they have positive private information to protect their informational rents. Banks also upgrade low quality borrowers to avoid creditor runs. Our results can explain the limited effectiveness of public credit registries and cast doubt on the use of credit ratings in reducing information asymmetry

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Information Sharing and Rating Manipulation *

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Abstract

We show that banks manipulate the credit ratings of their borrowers before being compelled to share them with competing banks. Using a unique feature on the timing of information disclosure of the Argentinean public credit registry, we disentangle the effect of manipulation from learning of credit ratings. We show that banks downgrade high quality borrowers on which they have positive private information to protect their informational rents. Banks also upgrade low quality borrowers to avoid creditor runs. Our results can explain the limited effectiveness of public credit registries and cast doubt on the use of credit ratings in reducing information asymmetry.

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The diffusion of information technology promises to enable the sharing of larger and finer amounts of information. By reducing information asymmetries between borrowers and banks, and between banks to the same borrower, information sharing is expected to reduce market segmentations and to enhance access to credit for creditworthy borrowers (Padilla and Pagano, 1997).

Information sharing however is also known to increase competition in credit markets (Pagano and Jappelli, 1993; Padilla and Pagano, 1997). For this reason, banks may not necessarily agree to share information about their clients, and private solutions for sharing information, such as credit bureaus, may not emerge. Public credit registries, to which banks *must* provide information, are believed to be a valid substitute (Jappelli and Pagano, 2002).

Public credit registries are mandated databases managed by a government agency, usually the Central Bank, which collect information on the standing of all borrowers in the financial system and make it available to actual and potential lenders. Surprisingly, in existing cross-country studies, evidence that the adoption of public credit registries affects the supply of credit is at best ambiguous (Djankov, McLiesh, and Shleifer, 2007; Peria and Singh, 2014).

We provide evidence that the structure of public credit registries and the mechanism through which information is disclosed may be crucial to yield positive effects on the allocation of credit because banks have incentives to manipulate the information they share about their borrowers. Banks often summarize information on the borrowers using credit ratings, which reflect factual information on the borrower, such as their industry, as well as the loan officer's judgement on the customer's prospects. Using a unique feature on the timing of the information released, provided by the expansion of the public credit registry in Argentina, we show that in order to

safeguard their informational monopoly banks manipulate the credit ratings of their borrowers before sharing them in the credit registry. To the best of our knowledge, this is the first paper to highlight that banks' incentives may impair the effectiveness of public credit registries.

Our tests exploit a unique feature of the reform of the Argentinian public credit registry, enacted in 1998 thanks to the adoption of CD-ROMs, which decreased the cost of distributing large amounts of information. The way this reform was implemented enables us to observe the credit ratings banks attribute to borrowers in three different periods: (1) a pre-expansion period, before the reform, when banks reported information to the Central Bank, but expected the information to remain private; (2) an interim period following the reform announcement in April 1998, but preceding its implementation in July 1998; and (3) a post-expansion period following the implementation of the reform, when information on the borrowers' credit ratings was actually shared.¹ We can thus ask whether after the reform announcement and before having to share the information with other banks, that is, during the interim period, banks abnormally modified their borrowers' credit ratings to safeguard their profits. In this way, by focusing on the interim period, we are able to isolate a bank's manipulation of credit ratings from learning from the disclosure of other banks.

Existing theories help us in formulating hypotheses on the direction in which banks would have incentives to manipulate the credit ratings of the borrowers before sharing them. These hypotheses allow us to test whether banks indeed manipulate ratings to conceal their risk assessment of borrowers from competitors.

The first set of tests we perform build on the influential papers of Sharpe (1990) and Rajan (1992) who show that banks may have an informational monopoly

¹ Hertzberg, Liberti and Paravisini (2011) exploit the same setting in a different context. Their objective is to provide evidence of lender coordination problems and the consequences on loan amounts.

on their high quality clients. High quality borrowers may be “informationally” captured by their lending banks because they face difficulties in conveying information about their superior performance and creditworthiness to other lenders. These effects are expected to be stronger for borrowers that entertain exclusive relationships with their banks.

Under these conditions, we would expect that government credit registries, which force banks to share information about the borrowers, should mitigate adverse selection problems and enhance credit access for high quality borrowers. However, truthfully revealing their positive private information on borrowers might erode banks’ informational rents and ultimately their profits. Their incentives would thus be to downgrade high quality borrowers before sharing information with other banks.

The incentives of banks lending to low quality borrowers with multiple relationships are opposite. First, these banks do not enjoy high informational rents because borrowers are low quality and would have to pay a high interest rate on their loan even in a symmetric information environment. Therefore, they have no incentives to downgrade them. On the contrary, the revelation of negative public information about the borrowers may induce a creditor run as highlighted by Corsetti, Dasgupta, Morris, and Shin (2004) and Hertzberg, Liberti and Paravisini (2011). In order to make a creditor run less likely, banks lending to low quality borrowers with multiple relations should have an incentive to upgrade borrowers on which they have negative private information before making this information public.

Not only the theoretical predictions that differ for different subsamples of borrowers enable us to identify the effects of manipulation on changes in credit ratings, but our empirical setting also allows us to design a difference-in-differences methodology which further mitigates any concerns that our findings may be driven by

aggregate shocks that systematically affect borrowers during the various phases of the reform implementation.

Before the 1998 reform of the public credit registry, banks shared information only about borrowers whose total outstanding debt was above \$200,000 because distributing information for large numbers of small debtors would have been prohibitively costly. However, the adoption of CD-ROMs dramatically reduced the cost of distributing information and eliminated the need for the \$200,000 threshold.² Thus, in April 1998, it was decided that borrower credit information for roughly 540,000 borrowers below the threshold, which was previously privately known by their banks, would be publicly disclosed. The reform was actually implemented in July of the same year.

We can thus test whether banks exhibit an abnormally high propensity to downgrade borrowers with the highest credit rating borrowing less than \$200,000 in the interim period between the announcement of the reform and its actual implementation using borrowers above the \$200,000 threshold as a control sample. We can also test whether the propensity to downgrade high quality borrowers below the threshold is highest for banks that entertain exclusive relationships with the borrowers and that presumably enjoy the highest informational rents.

Similarly, we test whether non-exclusive lenders upgrade low quality borrowers below the \$200,000 threshold to a larger extent than those in the control sample during the interim period.

To abstract from the effects of unobserved heterogeneity, we restrict all our tests to borrowers whose pre-expansion debt was between \$150,000 and \$200,000

² These thresholds are common among public credit registries.

(treated group) and borrowers whose pre-expansion debt was between \$200,000 and \$250,000 (control group).

Our tests provide unambiguous evidence that banks manipulate borrowers' credit ratings in the interim-period before making them public. We show that banks downgrade their high quality borrowers before sharing their credit ratings with other banks and that this tendency is more pronounced for banks that are exclusive lenders, that is, for banks that are expected to have more private information. In the same vein, we find that the banks that manipulate the borrowers' credit ratings are domestic or local. We do not observe a similar tendency to manipulate for informationally disadvantaged banks, such as foreign banks. We also find that banks that are multiple lenders to low quality borrowers have a tendency to upgrade them before making their ratings public as is consistent with a desire to avoid a creditor run.

As a consequence of rating manipulation, ratings become less informative after the announcement of the public credit registry extension. Borrowers that were rated as the safest were less likely to default before the reform; borrowers that were rated as relatively riskier are instead less likely to default after the reform. Consequently, ratings manipulation might limit the extent to which public information shared in a registry might enhance access to credit.

We also examine the effects of the public credit registry expansion on the affected borrowers' bank relationships. The expansion of the public credit registry appears to facilitate new lending to single lender borrowers with the highest credit ratings, at the expense of relationship banks. Importantly, single lender borrowers that have been strategically downgraded do not enjoy these benefits. The number of bank relationships of multiple lender borrowers also increases, which implies that informationally disadvantaged intermediaries that provide smaller transactional loans

may have benefited from the credit registry. These results suggest that notwithstanding the rating manipulation, the credit registry enhanced access to credit by reducing adverse selection for the most informationally disadvantaged lenders. However, borrowers that were downgraded in the interim period do not appear to reap the benefits of the credit registry expansion indicating that the strategic downgrades are effective in preserving banks' informational rents.

This paper belongs to a growing literature exploring the impact of credit registries and information sharing on access to credit. Existing papers show that pooling of information about borrowers' credit histories decreases delinquencies (Doblas-Madrid and Minetti, 2013; Sutherland, 2015) and enhances creditworthy borrowers' access to credit (Musto, 2004; Gonzales-Uribe and Osorio, 2014). All these papers consider private credit bureaus and the sharing of information on loan repayments. While the sharing of information on borrower quality is contemplated by existing theories (e.g., Padilla and Pagano, 1997), there is scarce empirical evidence on the effects of this dimension of information sharing. Liberti, Seru and Vig (2015) show that, after the expansion of the Argentinian credit registry, a large international lender made some use of other banks ratings for credit allocation. This is consistent with our findings that less informational advantaged foreign banks extended new credit to affected borrowers. To the best of our knowledge, we are the first to highlight that borrowers manipulate their private information before sharing it, thus limiting the effectiveness of non-voluntary information sharing mechanisms, such as public credit registries.

Hertzberg, Liberti and Paravisini (2011) also exploit the same expansion of the Argentinian public credit registry and show that, before the public release of information, fearing a creditor run, lenders strategically decreased their credit

exposure to low quality borrowers with multiple bank relations. We highlight that lenders strategically manipulated the information they released, a complementary strategic effect of the public release of information, which is important for an effective design of public credit registries and of the methods in which information can be credibly released.

Our findings are also important for the growing literature exploring the role of credit ratings in the financial system and the incentives of credit rating agencies. Existing literature has highlighted that because of competition among credit rating agencies and conflicts of interest arising from credit agencies providing other services to issuers, credit ratings may overstate issuers' creditworthiness (see, for instance, Bolton, Freixas and Shapiro, 2012; Efung and Hau, 2015; Griffin, Nickerson and Tang, 2013). Others have shown that banks may understate credit risk in order to be able to securitize (Rajan, Seru and Vig, 2010; 2015) or lower their Basel III capital requirements (Carey, 2002; Plosser and Santos, 2014; Begley, Purnanandam, and Zheng, 2015). We highlight that incentives to manipulate ratings exist also when ratings do not impact capital requirements because the ratings affect credit market competition through informational rents and potential loan losses in case of multiple banks. In such a setting, the incentives to manipulate may be even stronger as the reputational effects of poor quality credit ratings are limited.

1. Theoretical Background

1.1 Information Sharing

Existing literature highlights that information sharing among banks produces two types of effects (Padilla and Pagano, 1997). On the one hand, it reduces the cost of granting credit because it tends to diminish information asymmetries between

borrower and lender. By limiting problems arising from asymmetric information and moral hazard, this effect of information sharing unambiguously enhances bank profits.

On the other hand, information sharing reduces information asymmetries between banks and stimulates harsher competition between lenders. This negative effect of information sharing on banks' informational rents tends to reduce bank profits. Thus, banks have an incentive to voluntarily share information on their borrowers through credit bureaus only if the first effect prevails on the second, leading to a positive effect of information sharing on bank profits. Private credit bureaus do not emerge otherwise, even though by decreasing adverse selection and moral hazard problems, they unambiguously improve the allocation of credit.

Also for this reason, governments often sponsor the creation of public credit registries. Public credit registries are databases managed by a government agency, usually the Central Bank, which collects information on the standing of borrowers in the financial system and makes it available to actual and potential lenders. A crucial feature of public credit registries is that they compel lenders to share information about their borrowers.

Public credit registries can have a variety of structures. Some collect only limited information on outstanding loans of large borrowers and focus on banking supervision. Others distribute extensive information including late payments, defaults, and ratings. In existing cross-country studies, evidence that the adoption of public credit registries affects the supply of credit is at best ambiguous (Love and Mylenko, 2003; Djankov, McLiesh, and Shleifer, 2007; Peria and Singh, 2014).

We argue that public credit registries yield limited benefits when banks are forced to provide information and would not do so voluntarily because they are

concerned about preserving their informational rents. In this case, even if the accuracy of the information reported is routinely verified, banks may manipulate information on the borrowers before reporting it to the credit registry. Not only would this behavior be consistent with the banks' incentives, but it would also be hard to prevent for government authorities especially if banks are expected to report hard to verify information on the borrowers' riskiness through credit ratings, besides information on past defaults and delays in payments.

Detecting any manipulation of the ratings reported to the registry is likely to be particularly difficult if the ratings concern small entrepreneurial firms about which limited information is available through financial statements and other sources. As we explain below, borrowers' opaqueness may also exacerbate banks' incentives to manipulate.

1.2 Banks' Incentives to Share and Manipulate Information

It follows from the influential papers of Sharpe (1990) and Rajan (1992) that banks have weaker incentives to share information about borrowers on which they have positive private information. High quality borrowers face an adverse selection problem if they approach outside lenders, who expect many low quality borrowers, rejected by their previous banks, to approach them. This adverse selection problem creates an informational rent for current lenders and allows lending banks to charge interest rates higher than the ones that would prevail in a competitive environment with no asymmetric information.

A bank is more likely to obtain private information about a high quality customer if it entertains an exclusive relationship. The informational monopoly is otherwise at least partially eroded by the fact that many lenders to the same borrower

are likely to observe the same information. Supporting the idea that firms with multiple relations are less subject to an informational lock in from their lenders, Ioannidou and Ongena (2010) show that these firms obtain smaller cuts in interest rates if they manage to switch to an outside bank. Furthermore, Ongena and Smith (2001) and Farinha and Santos (2002) find that borrowers with multiple relationships entertain shorter relationships with their banks, suggesting that they are less likely to be locked in these relationships. These findings indicate that multiple relationships soften hold-up problems. Therefore, exclusive lenders should have stronger incentives to manipulate downwards the ratings of high quality borrowers before sharing information in the credit registries.

Existing literature also highlights that different types of banks may have different information about their borrowers. For instance, foreign banks are often believed to have less information about their borrowers (Mian, 2006; Giannetti and Ongena, 2009). We thus expect that the incentives to manipulate the ratings should be stronger for domestic banks, which having more private information should have more to lose in terms of informational rents. Similarly, local banks that are specialized in lending to a particular region may be expected to have more private information than larger national banks (Berger et al., 2005). We thus expect local banks to downgrade high quality borrowers to a larger extent than other banks before joining the credit registry to protect their informational rents.

The lenders to low quality borrowers do not enjoy informational rents because these borrowers risk is such that they warrant a higher interest rate. If these banks know that their low quality clients have multiple lenders, they have an incentive to upgrade the ratings of the borrowers before sharing them. This follows from the fact that making public negative information about the borrowers may lead the other

banks to withdraw their loans or increasing the interest rate causing financial distress for the borrowers and impairing the value of the loan of all banks, including the one disclosing negative information (Hertzberg, Liberti and Paravisini, 2011). To protect the value of their loans, banks may thus want to upgrade low quality borrowers with multiple relationships.

Importantly, the incentives to manipulate ratings are opposite for banks involved in relationships with relatively high and low quality borrowers. They also differ depending on the type of bank and whether the borrower has multiple banks. Therefore, any evidence supporting the above conjecture would be hard to explain using omitted factors, asymmetric shocks or mechanisms involving the systematic review and update of ratings before their release. Below, we describe an institutional context that lends itself naturally to test whether banks indeed manipulate ratings before joining public credit registries.

2. Institutional Setting and Empirical Implementation

2.1 The Credit Registry and Its Reform

Argentina's public credit registry was established in 1991 and covers every firm and entrepreneur that obtains credit from a financial institution. Since inception, all financial institutions were required to report to the Central Bank the amount of the loan, the amount of collateral pledged, and the rating for each borrower.

Ratings are provided as an integer ranging from 1 to 5, where 1 represents highest creditworthiness. The bank has full discretion in assigning the borrower a 1 or a 2 based on its private assessment of the borrower repayment prospects. Ratings ranging from 3 to 5 are more mechanically determined based on the borrower's repayment status. The bank has to assign a rating of 3 if the borrower has been

delinquent for more than 90 days or, more in general, if the borrower is considered to have high default risk. Ratings of 4 and 5 are assigned to borrowers that have been delinquent for over 180 days and which have had collateral seized or are in bankruptcy. These borrowers can be considered to be in default.³

While discretionary, 1 and 2 ratings are informative. In our sample, borrowers with a rating of 2 have a 21% probability of default over the subsequent 6-months, significantly higher than the 3.6% default rate of borrowers with a rating of 1. Consistent with our conjecture that banks with private information have an incentive to muddle waters and make credit rating less informative, the difference in the unconditional default probability for treated borrowers with ratings of 2 and 1 goes from 16.4% to 11% after the registry expansion.

We consider information on the borrower's repayment status easier to verify and focus on banks' incentives to manipulate the reported 1 and 2 ratings, on which the bank has full discretion. Also, granting a rating of 1 or 2 has no implications for capital requirements, while banks have to set aside more capital when assigning worse ratings, raising the cost of downgrading borrowers. Thus, banks should have scope to alter the 1 and 2 ratings subject to the constraint of not raising suspicions with the Central Bank, which monthly audits a random sample of each bank's portfolio. In this way, banks can muddle waters and increase the level of asymmetric information for some borrowers.

Since inception, the credit registry imposed the sharing of information on borrowers with loans above a \$200,000 threshold. Also, information on borrowers with a default rating of 3 or higher was shared in the registry regardless of the amount of their loans. Up to April 1998, this information was shared using monthly magnetic

³ Banks commonly use credit ratings even if these are not shared in the credit registry to categorize borrowers' creditworthiness. In fact, in the absence of information sharing there may be more incentives to have accurate credit ratings because the strategic effects we highlight are not at work.

tapes using the most recent cross-section of borrowers to financial institutions and credit rating agencies making request of it.

In May 1998, the Central Bank switched to CD-ROMs. This substantially lowered information sharing costs and made feasible to distribute monthly information on all loans even the ones below the \$200,000 threshold pertaining to borrowers with a rating better than 3.

This reform of the credit registry was announced in April 1998 and implemented in July of the same year. This implies that the ratings of the first quarter of 1998 for borrowers with loans below \$200,000 threshold are the ones that financial institutions reported to the Central Bank when they did not observe other banks' ratings and did not expect other banks to ever observe their assessment of the borrowers. These ratings capture the financial institutions' private information. Since they were expected to remain exclusive knowledge of the Central Bank, they do not capture the strategic behavior of financial institutions.

Starting from April 1998, institutions learnt that their ratings would be shared with other existing and potential lenders.⁴ However, until July of the same year, they did not observe other banks' ratings. Any systematic changes in the ratings of borrowers with loans below the \$200,000 threshold occurring during this three-month period, to which we refer below as interim period, can only be driven by the anticipation that other banks will observe the ratings. Systematic changes cannot be explained by the effect of learning from other financial institutions' assessment of the borrower's creditworthiness, as information had not yet been shared.

Clearly, shocks may affect borrowers and lead to changes in the banks' risk assessment. For this reason, below, we design an empirical methodology that allows

⁴ Borrowers may learn their own ratings when they ask for a new loan. However, if they are informationally captured they cannot ask for an upgrade to the same extent that they cannot ask for a lower interest rate.

us to abstract from the effects of shocks, learning, and borrower unobserved heterogeneity.

2.2 Empirical Framework

The stages in which the reform was implemented and the different theoretical predictions for banks' incentives to manipulate the ratings of borrowers with single and multiple relationships and high and low quality borrowers allow us to identify the effect of information sharing on rating manipulation.

We start by considering the effect of the announcement of the reform on the borrowers of highest credit quality that entertain a single bank relationship. As argued in Section 1, under these conditions, banks are likely to enjoy large informational rents. To preserve their informational monopoly, banks may thus be inclined to strategically downgrade high quality borrowers before sharing their ratings. Thus, we should observe a higher probability of a downgrade for borrowers with single bank relationships during the interim period than for other borrowers.

We then extend the sample to high quality borrowers with single and multiple relations and test that the propensity to downgrade is indeed lower for borrowers that have multiple relationships and are therefore less informationally captured by their lending banks.

To control for the fact that high quality borrowers may be subject to negative shocks that lead to downgrades, we consider that the ratings of borrowers with loans above \$200,000 were already public (the control group). Thus, the strategic downgrades should be more likely for borrowers with loans below the \$200,000 threshold (the treated group) than for the control group if any strategic manipulation of the ratings is indeed occurring.

We focus on the subsample of borrowers with a single relationship (s) and rating of 1. These are the highest quality borrowers on which we expect banks to enjoy the highest informational rents and to have the strongest incentives to manipulate the rating downwards. We estimate the following equation:

$$\begin{aligned} \text{downgrade}_{i,b,t}^{1to2,s} = & \beta_0 + \beta_1 \times \text{Treated}_i + \beta_2 \times \text{Treated}_i \times \text{Interim}_t + \beta_3 \times \text{Treated}_i \times \text{Post}_t + \\ & + \xi_t + \lambda_b + \phi_{b,t} + \varepsilon_{i,t} \end{aligned} \quad (1)$$

where $\text{downgrade}_{i,b,t}^{1to2,s}$ is a dummy variable that takes a value equal to 1 if a borrower that had received the highest credit rating of 1 from bank b during month $t-1$ is downgraded by bank b to a rating of 2 during month t . Consistently with the theoretical predictions, this variable is defined only for borrowers with a rating of 1 at $t-1$, is equal to zero if the credit rating is 1 at t , becomes 1 if the credit rating is 2 at t , and drops out of the sample afterwards.

Since downgrades from 2 to 3 or worse capture delinquencies and defaults and are mechanically determined without scope for manipulation, we will use them to construct a placebo test.

In all specifications, we include a full set of time effects using month dummies (ξ_t) and test the robustness of our findings to the inclusion of bank (λ_b) and bank-time ($\phi_{b,t}$) fixed effects. The dummy variables Interim_t and Post_t take value equal to one, respectively, in the periods April-June 1998 and July 1998-June 1999. The omitted category is the period preceding the reform announcement, which goes from January 1998 to March 1998. The dummy Treated_i takes a value equal of one if borrower i has a loan below \$200,000 in the pre-reform announcement period, and was therefore omitted from the public registry prior to the expansion.

To further abstract from any effects of borrowers' heterogeneity, which could lead to downgrades, we consider only borrowers that during January, February and March 1998 had loans between \$150,000 and \$250,000. We cluster errors at the borrower level.

If banks indeed manipulate ratings to preserve their informational monopoly, we expect that $\beta_2 > 0$. We do not necessarily expect an analogous effect in the post-period, when ratings have already become public, or for downgrades from 2.

We develop the above framework to test whether some banks that may have accumulated more private information are more inclined to manipulate in a way that is consistent with existing literature.

To further evaluate whether the changes in ratings are associated with real changes in borrowers' conditions, we consider changes in borrowers' indebtedness and ex post defaults. We restrict these tests to borrowers with a single lender, who are the most likely to have been strategically downgraded because lenders have larger informational rents. First, we explore whether the size of the loan to single relationship treated borrowers varies in the interim period using a framework analogous to the one described in equation (1), except for that we also include borrower fixed effects. Any changes in loan size in the interim period could be associated with changes in creditworthiness and would go against the hypothesis that banks strategically manipulate their ratings.

Next, we test whether strategic downgrades affect the informativeness of the rating. Specifically, we examine if a rating of 2 becomes less likely to predict defaults for treated borrowers (in comparison to the control sample) after the public credit registry has been announced. If banks indeed manipulate the rating of high quality borrowers downwards, we expect borrowers with a rating of 2 to be more likely to

default before the announcement, when the rating was more likely to reflect the bank's negative information.

Finally, we perform a number of placebo tests for which we expect no differential effect for treated borrowers in the interim period. We introduce these additional tests in Section 4 where we describe the results.

The theories described in Section 1 also imply that fearing a creditor run, banks may have an incentive to upgrade low quality borrowers with multiple relationships.⁵ To test whether there is any evidence of manipulation, we estimate a model analogous to (1) where the dependent variable is $upgrade_{i,b,t}^{2to1,m}$, which is defined only for multiple relationship borrowers with a rating of 2 and that takes value one if the borrower is upgraded. Also in this case, we do not consider ratings below 2 as these cannot be manipulated.

If banks indeed manipulate the borrowers' ratings before making them public, we expect that treated borrowers, whose rating is about to be disclosed for the first time, are more likely to be upgraded in the interim period if they have multiple relationships.

3. Sample and Descriptive Statistics

As explained above, we exploit the credit registry inclusion threshold prior to the expansion to examine how the credit registry expansion affects the release of information. Prior to the expansion, all borrowers with total borrowing more than \$200,000 and/or borrowers in default with at least one lender were included in the registry. Post expansion, all borrowers were included. Thus, we compare the release

⁵ Similarly to Hertzberg, Liberti, and Paravisini (2011), these tests rely on the assumption that lenders know which borrowers have multiple relationships even before the expansion of the credit registry. We think that this assumption is natural because by revealing to have other lenders borrowers can decrease their interest rate.

of information for borrowers with total borrowing less than \$200,000 (the treatment borrowers) with the counterfactual of borrowers with total borrowing greater than \$200,000 (the control borrowers). Further, to limit the effects of unobserved heterogeneity we examine only those borrowers whose debt was close to the threshold. The treatment (control) group comprises borrowers with total debt between \$150,000 and \$200,000 (\$200,000 and \$250,000).

Table 1 presents descriptive statistics for the sample of treatment and control borrowers. We condition on borrowers that had existing relationships with banks in January 1998 and then track these borrowers through the three-month pre-period, the three-month-interim period, and the twelve-months post registry expansion. The sample includes 1,764 borrowers, of which 702 have an exclusive relationship with a bank in the pre-period, and 2,865 bank-borrower relationships. Of the 1,764 borrowers, 389 (1,375) are treatment (control) borrowers.

Panel A of Table 1 presents descriptive statistics for the full sample period for all, treatment, and control borrowers, and further sorts on the exclusivity of the lending relationship. Examining treatment and control groups in Panel A, the mean debt contract for treatment (control) borrowers is for \$112,672 (\$130,301), with total borrowing of \$200,973 (\$226,683). Overall, the two groups are similar in terms of collateral, credit rating, number of banks, and importantly the proportion of borrowers with exclusive relationships. We find similar evidence that treatment and control groups are comparable (except for lending amounts) for both exclusive and multiple-relationship borrowers.

Figure 1 shows how some salient borrower characteristics vary for the borrowers included in our sample in February 1998.⁶ There appear to be more

⁶ Cross-sectional differences are similar in January and March 1998.

borrowers with debt just above the \$200,000 threshold than just below. This may indicate that borrowers try to obtain at least \$200,000 of total debt to have visible credit ratings. Importantly, however, any ability to manipulate does not appear to be precise as some salient average characteristics of the borrowers appear remarkably continuous above and below the threshold, suggesting that any manipulation does not lead to uncomparability of the treatment and control samples.

In Panel B of Table 1, we provide descriptive statistics on bank-relationships in the pre, interim, and post-reform periods. There are a few striking observations that relate to our study. First, borrowers' credit ratings worsen on average subsequently to the credit registry's expansion. However, the credit ratings of multiple borrowers worsen by less than for borrowers with exclusive lenders, with an increase of the rating of 0.18 for multiple relationship borrowers in comparison to 0.33 for borrowers with an exclusive lender.

Second, while exclusive relationships become less common post reform, the average number of banks increases less for borrowers with an exclusive relationship than for borrowers with multiple relationships, consistent with exclusive lenders protecting their informational rents. Third, exclusive relationship borrowers exhibit the largest increase in total debt even though the overall increase in borrowing was modest following the reform.

We explore further how credit ratings vary around the registry reform by presenting univariate tests in Figure 2. Distinguishing between the single lender subsample (Figure 2A) and the multiple lender subsample (Figure 2B), we plot the proportion of borrowers that have a credit rating of 2. We focus only on those borrowers that are not in default, in order to capture the discretionary nature of the rating process, and track borrowers in the three-month pre-period, the three-month

interim period, and the three-month post-reform period. In Figure 2A, the treatment borrowers with an exclusive lending relationship exhibit significant downgrades in the interim period: 3.5 percent of the treatment borrowers are downgraded compared with only one-quarter of a percent of the control borrowers. The difference-in-differences of 3.3 percent is economically and statistically significant (at the 10%-level). In the post-reform period, approximately one percent of the exclusive lender sample is downgraded but the difference is not significant either in absolute or relative terms. Interestingly, the proportion of borrowers with a rating of 2 in the control sample is larger. Since the ratings of borrowers in the control group were already public, this suggests that the banks had already strategically downgraded some of these borrowers.

In Figure 2B, we focus on the borrowers with multiple relationships. The treatment borrowers exhibit significant upgrades in the interim period: 2.9 percent of the treatment borrowers are upgraded while 0.1 percent of the control borrowers are downgraded. The difference-in-differences of 3.0 percent is economically and statistically significant (at the 5%-level). There is no evidence of significant upgrade or downgrade activity in the post-reform period. Also in this case, the proportion of treatment borrowers with a rating of 2 appears to converge to the proportion of borrowers with a rating of 2 in the control sample.

This evidence is fully consistent with the conjecture that banks manipulate ratings downward to capture good borrowers, and manipulate ratings upward to preempt runs on riskier borrowers. Since lenders strategically downgrade some borrowers and strategically upgrade others, the manipulation does not give rise to systematic rating inflation or deflation and may therefore not to be easily detected by regulators.

Figure 2 also shows that the treatment borrowers with an exclusive relationship appear healthier than comparable control borrowers, and that treatment borrowers with multiple relationships are riskier than comparable control borrowers in the pre-reform period. The separation on risk is consistent with adverse selection models of credit rationing (Stiglitz and Weiss, 1981). Banks that are unable to evaluate borrowers offer a loan contract that is acceptable only to riskier borrowers, while healthier borrowers contract exclusively to overcome information asymmetry.

4. Results

4.1 Strategic Downgrades of High Quality Borrowers

Table 2 provides unambiguous evidence that high quality borrowers, defined as the borrowers that had a rating of 1 in the period preceding the announcement of the credit registry expansion, with loans below the \$200,000 threshold are more likely to be downgraded in the interim period than borrowers in the control sample.⁷ Under the identification assumption that the treated borrowers did not receive stronger negative shocks than the borrowers that are included in the control sample because of slightly larger loans, this evidence indicates that banks strategically downgrade high quality borrowers to preserve their informational advantage.

In columns 1 to 3, we consider only the subsample of borrowers with a single relationship, for which we expect the banks' informational rent, and consequently the incentives to manipulate ratings before their release, to be stronger. Borrowers with an initial rating of 1 appear 3 percentage points more likely to be downgraded than borrowers in the control sample during the interim period. This is a relatively large

⁷ Once a borrower has been downgraded we exclude it from the sample, explaining why the number of observations in Table 2 is lower than in Table 1.

number because the unconditional probability of a downgrade for this subsample of borrowers is 0.16 in the whole sample and 0.11 in the pre-period.

This result is qualitatively and quantitatively robust when we absorb unobserved heterogeneity including an increasing larger set of fixed effects. In column 1, we include time effects, bank fixed effects, the natural logarithm of debt, and control for changes in bank characteristics by including the natural logarithm of total assets and return on assets.

In column 2, we test whether the propensity to downgrade high quality borrowers persist also in the post-period, once the ratings have been made public. In the single lender subsample, the bank cannot learn new information when the information is shared. Thus, a higher propensity to downgrade would suggest that the quality of treated borrowers is deteriorating and should be interpreted as evidence against strategic downgrades. We find no evidence that banks continue to abnormally downgrade treated borrowers in the post-period.⁸ Finally, in column 3, we include bank-time fixed effects to control for bank-specific shocks that may affect downgrades.

To provide evidence that borrowers above and below the threshold are not subject to asymmetric shocks, Figure 3 shows the dynamic effects of downgrades. It plots the coefficient estimate, obtained including the same controls as in column 1, of the banks' propensity to downgrade treated borrowers in each month. The propensity of high quality borrowers with single lenders to be downgraded is remarkably similar in the pre- and post-periods. All differences are concentrated in the interim-period as our hypothesis would predict.

⁸ This also suggests that banks do not use downgrades to punish borrowers once the ratings become public.

While we expect banks to have higher informational rents on borrowers with which they entertain exclusive relations, also lenders to borrowers with multiple relationships may have private information, because borrowers may entertain transactional relationships with some of their multiple banks and have a close relationship with only one of their banks. Column 4 shows that our finding holds even if we include all relationships (including the ones of the borrowers with multiple relationships) in the sample. As we would expect, the effect is somewhat smaller than in the case in which we focus on single relationship borrowers.

In column 5, we explicitly test the conjecture that the incentives to strategically downgrade borrowers are weaker for banks that lend to borrowers with multiple relationships. This conjecture is confirmed by the fact that the triple interaction term between $Treated_i \times Interim_t \times Ln(\#Banks)$ is negative and significant, while the coefficient on $Treated_i \times Interim$ is of similar magnitude to the one presented in columns 1-3.

These results support our conjecture that banks strategically downgrade high quality borrowers below the threshold. In particular, any alternative explanation would have to explain why borrowers below the threshold with multiple banks are less likely to be downgraded. Nevertheless, in the rest of Table 2, we reconsider the possibility that a negative shock may cause the downgrade of borrowers below the threshold. If a negative shock indeed explained our findings, we would expect to see downgrades also to ratings that have less scope for manipulation, such as downgrades from 1 to 3, 4, and 5. Both in the sample of single relationships and in the whole sample, we find no evidence of that (columns 6 and 7).

Table 3 tests whether some categories of banks strategically downgraded borrowers to a larger extent in the interim-period. We examine three types of banks:

foreign banks, which are international banks funded with foreign capital; domestic banks, which are Argentinian banks funded with domestic capital; and local banks, which are a subset of domestic banks that focus on a geographical area, such as a state. Focusing on the single bank sample, in which we expect the incentives to strategically downgrade to be stronger, we find that domestic banks (column 1) and local banks (columns 2 and 3), but not other banks, strategically downgrade high quality borrowers in the interim period. These banks are the ones that are generally thought to have more private information on the borrowers, often under the form of soft information, which is harder to collect in large and international banks. Thus, the finding that domestic and local banks are the ones that downgrade high quality borrowers is consistent with the interpretation that banks' abnormal propensity to downgrade in the interim period is driven by the desire to protect their informational rents.

We find no evidence that the downgrades disproportionately affect entrepreneurs, which we identify as unincorporated businesses in column 3 of Table 3. Thus, the effect we highlight appears pervasive and not limited to the most opaque borrowers without financial statements.

The conclusion that downgrades are driven by information manipulation rather than by the arrival of new information is also consistent with the finding in Table 4 that banks on average do not change the amount of the loan in the interim period. We estimate the within-borrower effect of the registry expansion announcement (column 1) and public information (column 2) on the level of credit at the borrower-bank relationship level on the intensive margin. In column 3, we identify the effect for those borrowers that experience a downgrade and similarly find that banks do not

modify the level of credit for treatment borrowers.⁹ The results indicate that the loans of borrowers with single lenders that are more likely to have been strategically downgraded change to the same extent as for borrowers in the control sample suggesting that on average the downgrades have not been driven by the arrival of new information, but are rather strategic.

The manipulation of credit ratings would imply that the ratings become less informative. This is precisely what Table 5 implies. Borrowers with a credit rating of 2 in the period preceding the announcement of the credit registry expansion are more likely to default in the following 6 or 12 months than borrower receiving the same rating in the interim or the post-period. Thus, on average, borrowers with a rating of 2 are better after the announcement of the credit registry expansion than in the pre-expansion period.¹⁰ This effect is unlikely to be driven by the business cycle because it is present only for treated borrowers, that is, for the borrowers whose credit ratings become public for the first time, not for the borrowers with slightly bigger loans.

In addition, this result indicates that the downgrades cannot be driven by the incorporation of more precise information in the ratings before their public release, because in that case ratings should become more, not less precise in predicting defaults.

Overall, these findings fully support the conjecture that banks manipulate the credit ratings in order to protect their informational rents.

⁹ Consistently with the interpretation that the changes in ratings were not information driven, we also find that the loan collateralization did not change in the interim period on average and for the downgraded borrowers.

¹⁰ In results that we do not report for brevity, we also find that borrowers that are downgraded in the interim period are not more likely to default than borrowers with a rating of 1 that were not downgraded, further supporting our interpretation that most of the downgrades of treated borrowers in the interim period were not driven by the arrival of new information.

4.2 Strategic Upgrades of Low Quality Borrowers with Multiple Bank Relationships

Lenders to low quality borrowers do not enjoy informational rents. Therefore, they have no incentives to downgrade their clients. The banks lending to low quality clients with multiple lenders may instead fear that the public revelation of negative private information to the other banks may cause a creditor run and the ultimate default of the borrower. If this occurred, the bank revealing negative information would suffer because the loan would not be repaid as a result of the other banks' behavior. Therefore, banks may have an incentive to hide negative information on borrowers with multiple lenders.

Put differently, we expect that banks lending to borrowers with a discretionary negative rating of 2 will have an incentive to strategically upgrade them if these receive credit from multiple banks. This is precisely what we find when we estimate the probability that a borrower is upgraded, similarly to equation (1), for multiple lender borrowers. The results are presented in Table 6. Low quality borrowers with a rating of 2 appear to be more likely to be upgraded in the interim-period. This result is robust as going from column 1 to 4 we include an increasing number of controls as we did in Table 2.¹¹ Absorbing unobserved heterogeneity in fixed effects appears to increase the probability that treated borrowers are upgraded in the interim period in comparison to borrowers in the control sample, whose credit ratings were already public.

If the upgrades are indeed driven by fear that the revelation of negative information may induce a creditor run, we should not observe a similar effect for borrowers with which the bank entertains an exclusive relationship. This is precisely

¹¹ We include borrower fixed effects in the multiple lender sample to mitigate the concern that our results are due to a handful of borrowers being downgraded by multiple lenders.

what we find in column 5. This is comforting because it makes it unlikely that the upgrades are driven by a positive shock affecting low quality borrowers.

The finding in column 5 is also important to assuage any concerns that the changes in credit ratings we observe may just be due to a systematic revision of *all* credit ratings aiming to update information on the borrowers before sharing the information with other financial institutions. If the changes in ratings we observe were driven by a mere incorporation of new information, we should observe that banks also upgrade some borrowers with whom they entertain single relationships. The fact that this is not the case (together with our earlier evidence that the cross-sectional predictions are consistent with theoretical predictions and the finding that credit ratings become less informative) indicates that the changes in ratings we observe are not driven by systematic revisions, but are instead strategic.

Consistent with the interpretation that the upgrades of low quality borrowers are due to strategic manipulation of information, in Table 7, we do not find any change in the bank credit exposure to borrowers with multiple relationships in the interim-period, when the ratings change.¹² Thus, the rating changes are unlikely to be driven by the arrival of new information on borrowers' indebtedness.

Furthermore, Table 8 shows that in the subsample of borrowers with multiple lenders a credit rating of 1 was more informative about the low probability of a future default before the expansion of the credit registry.¹³ Thus, while the subsample of single lender borrowers with a rating of 2 becomes relatively better, the subsample of multiple lender borrowers with a rating of 1 becomes relatively worse. This evidence is hard to explain with shocks affecting the treated borrowers but not the control sample because we would need a negative shock affecting high quality treated

¹² Also in this case we find no evidence that the collateralization of the loan changes.

¹³ Also multiple lenders borrowers upgraded in the interim period are on average as likely to default as borrowers that have not been upgraded.

borrowers and a positive shock affecting low quality treated borrowers. The evidence is instead fully consistent with the banks' incentives to manipulate information with the ultimate goal of maximizing their net wealth.

4.3 Implications of Information Sharing on Bank-Borrower Relationships

So far we have shown that banks strategically manipulate the ratings they can discretionally assign to borrowers before sharing them in a way that is consistent with their incentives. As a consequence, public credit ratings are less informative than internal credit ratings, which were not expected to be shared with other banks. This implies that only garbled information is revealed. Is this information sufficiently informative to affect bank-borrower relationships and the structure of loan contracts?

Table 9 provides some evidence on the effects of information sharing on the structure of bank-borrower relationships. We estimate how the release of public information affects the total debt and the composition of bank relationships on the intensive and extensive margins in the post-period. For borrowers in the treatment and control groups, we examine the number of bank relationships, usage of collateral, and composition of debt by bank type across all relationships, not just those that existed prior to the registry expansion. Panel A concentrates on borrowers that had a single lender at the beginning of the sample period. The within-borrower estimation results show that on average the total debt (column 1), the fraction of loans secured with collateral (column 2) and the number of relationships (column 3) do not change.

However, following the credit registry expansion, the fraction of debt provided by local banks to treatment borrowers decreases (column 4), and the fraction of debt provided by foreign banks increases (column 5). Prior to the expansion of the credit registry, on average, treatment borrowers sourced 63% of loans from local banks, while control borrowers, whose information was already public, sourced just

41%. In contrast, treatment borrowers sourced just 16% of debt from foreign banks, while control borrowers sourced 30%. Post expansion, foreign banks provided treatment borrowers 18% and control borrowers 29% of their debt. Consequently, the results in columns 4 and 5 in Panel A of Table 9 imply that credit registry expansion provided greater access to foreign funding, and that treated borrowers shifted debt away from local lenders.

In columns 4 and 5, we omit borrower fixed effects as there is very little within-borrower variation in the composition of lenders over our short time window. Nonetheless, we find similar results in column 6 when we estimate the within-borrower effect of public information on local bank lending.

One of the greatest frictions to new bank entry is adverse selection. This friction is aggravated for transaction lenders, such as foreign banks in our setting. The credit registry expansion mitigates this friction by offering foreign banks past payment data for potential borrowers. Therefore, we should observe that the decrease in local bank lending (and increase in foreign lending) should be concentrated in borrowers with the best credit ratings. Consistent with this, we split the sample of single lender borrowers based on whether the borrower has a credit rating of 1 (column 7) or greater than 1 (column 8) and find that the shift in borrowing away from local lenders is concentrated in the best borrowers.

Panel B concentrates on borrowers that had multiple bank relationships at the beginning of the sample period. It appears that these borrowers are able to start new relationships (column 3). Possibly because the level of information asymmetry was lower for borrowers that engaged multiple banks already in the pre-period, it does not appear that foreign banks take great advantage of the public credit registry, as the

share of loans provided by foreign banks and by local banks does not change post-registry expansion.

These results appear to indicate that the public credit registry increases bank competition for lending to borrowers not previously included in the registry. The competition takes the form of more credit from foreign banks for borrowers with single lenders. The number of bank relationships of multiple-lender borrowers also increases suggesting that banks are willing to provide a smaller share of credit.

While overall the credit registry expansion may have advantaged transaction banks at the expense of relationship banks in lending to the most opaque borrowers, the strategic downgrades appear to have been effective in preserving some of the rents of relationship banks. This is evident in Table 10, which reproduces the results in Panel A of Table 9 for the subsample of treated borrowers that had a rating of 1 in the pre-period. The interaction term $Downgraded_i \times Post_t$ shows that for borrowers that have been strategically downgraded in the interim-period the effects are opposite to the ones in Panel A of Table 9. Downgraded borrowers do not start new relationships at the same rate as non-downgraded borrowers explaining why we find no effect in the number of new relationships in Panel A of Table 9. Strategically downgraded borrowers become more, not less, dependent on local banks and receive less credit from foreign banks after their ratings become public, indicating that the rating manipulation impacts the allocation of credit.¹⁴

5. Conclusions

¹⁴ Focusing on borrowers with multiple relationships and a rating of 2 in the pre-period we find that the total loans of borrowers that have not been upgraded in the interim period decrease in the post-period suggesting that a creditor run indeed occur for these borrowers, as Hertzberg, Liberti and Paravisini (2011) find. This finding also suggests that strategic upgrades achieve their objective of avoiding a creditor run.

We provide evidence that banks manipulate the ratings of their clients before being compelled to share them by the extension of a public credit registry. Our findings can explain the puzzling evidence in existing literature that public credit registries, differently from private credit bureaus in which banks voluntarily share information, have a limited effect on the allocation of credit.

Our findings are also important for the growing debate on the use of credit ratings following the 2007-2009 financial crisis. Existing literature indicates the limitations arising from the issuer-pays model, which allows an issuer to shop for the best credit rating, and that rating agencies have conflict of interests arising from the fact that they are able to sell other services to borrowers. We show that ratings are manipulated in a situation in which the rater is the lender. In this case, the rater has clearly strong incentives to acquire information. However, the fact that it competes with other lenders or that by revealing information may start a creditor run leads it not to truthfully reveal the information. The fact that ratings can be manipulated in so different contexts cast doubts on the use of credit ratings to reduce asymmetric information in the financial system.

Our findings have also important implications for the design of public credit registries and suggest that positive, non verifiable information about the borrowers cannot be credibly shared.

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Figure 1
The Distribution of Borrowers by Total Debt Before the Credit Reform Expansion

This figure shows the cross-sectional distribution of the number of borrowers for different levels of total debt and some average salient characteristics of the borrowers in February 1998. The plot is for all the treatment and control borrowers in the sample. Treatment (control) borrowers are those with total debt between \$150,000 and \$200,000 (\$200,000 and \$250,000) during the period before the credit registry reform announcement from January 1998 till March 1998. The left hand-side axis plots the Number of Borrowers per \$10K bin. The right-hand side axis plots some average borrower characteristics also per \$10K bin including the fractions of borrowers with (a.) Rating = 1; (b.) Single Lending Relationship; (c.) Downgrades, and, (d.) Upgrades.

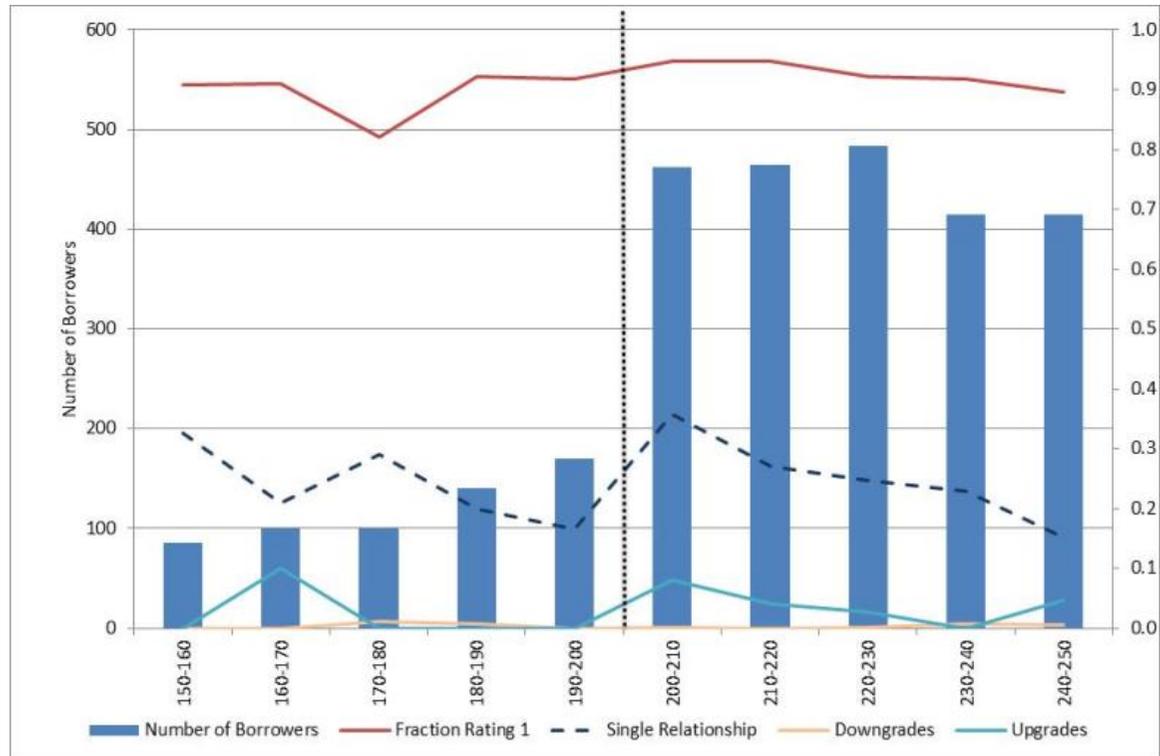


Figure 2

Upgrades and Downgrades around the Credit Reform Expansion

These figures show the fraction of borrowers with a Rating = 2 for single lending relationship borrowers and multiple lending relationship borrowers, respectively. Statistics are shown for three months prior to the credit registry expansion announcement (pre-period), during a period in which the credit registry expansion has been announced, but information about the borrowers has not yet been made public (interim period), and after information about the borrowers has been made public (post-expansion period). The unit of observation is at the borrower-bank-month level.

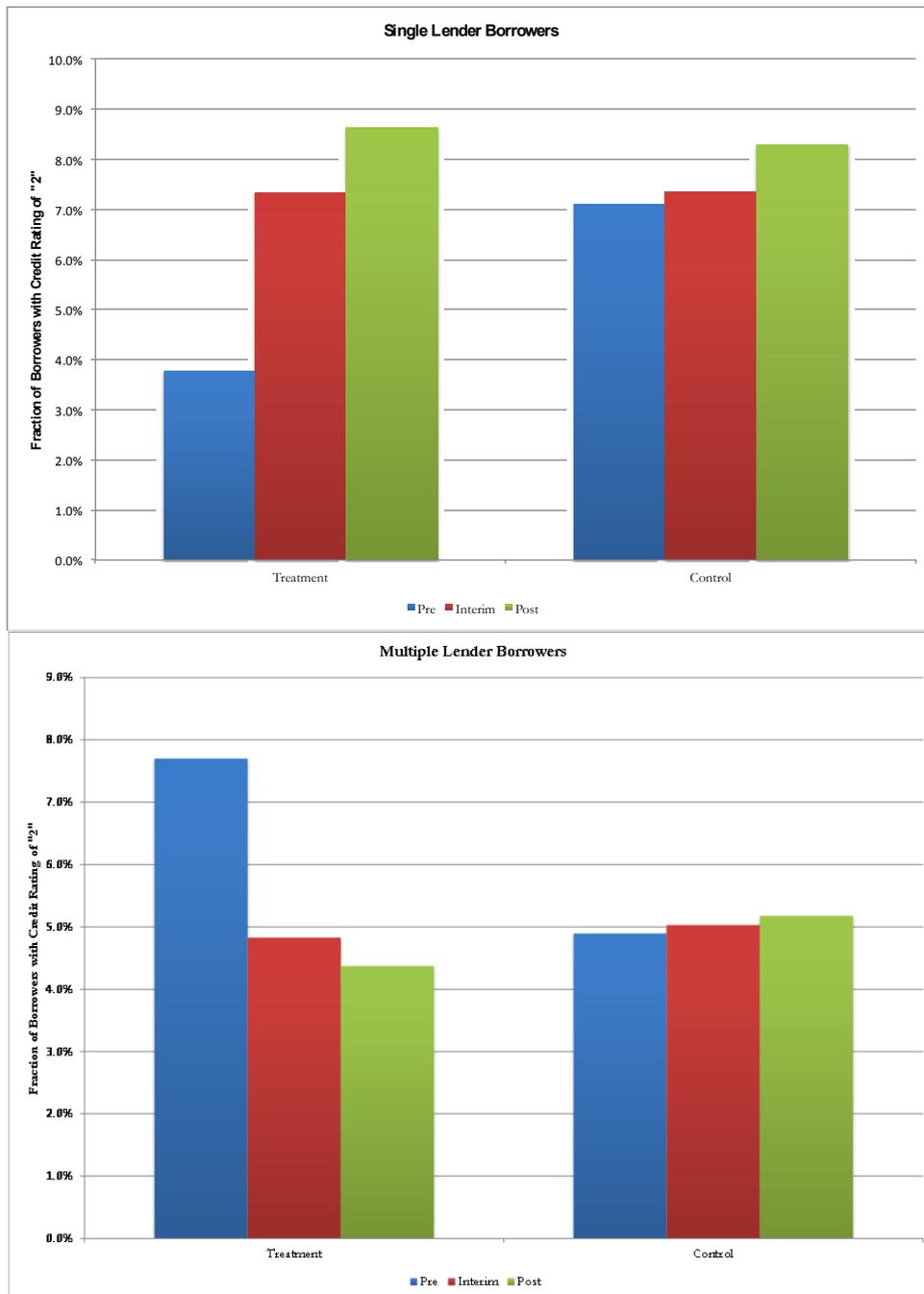


Figure 3
Conditional Estimates of Strategic Downgrades by Month for Single Lenders

This figure reports single relationship lenders' propensity to downgrade high quality borrowers by month. The parameter estimates reported are for the coefficient of the interaction between the *Treated_i* dummy and monthly dummies for each month from January 1998 to September 1998. The parameter estimates are obtained by controlling for the *Treated_i* dummy, each of the monthly dummies, all the banks' and borrowers' controls as well as bank fixed effects. Bank controls include the log(Total Assets) and the Return on Assets, while borrower controls include the log(Debt).

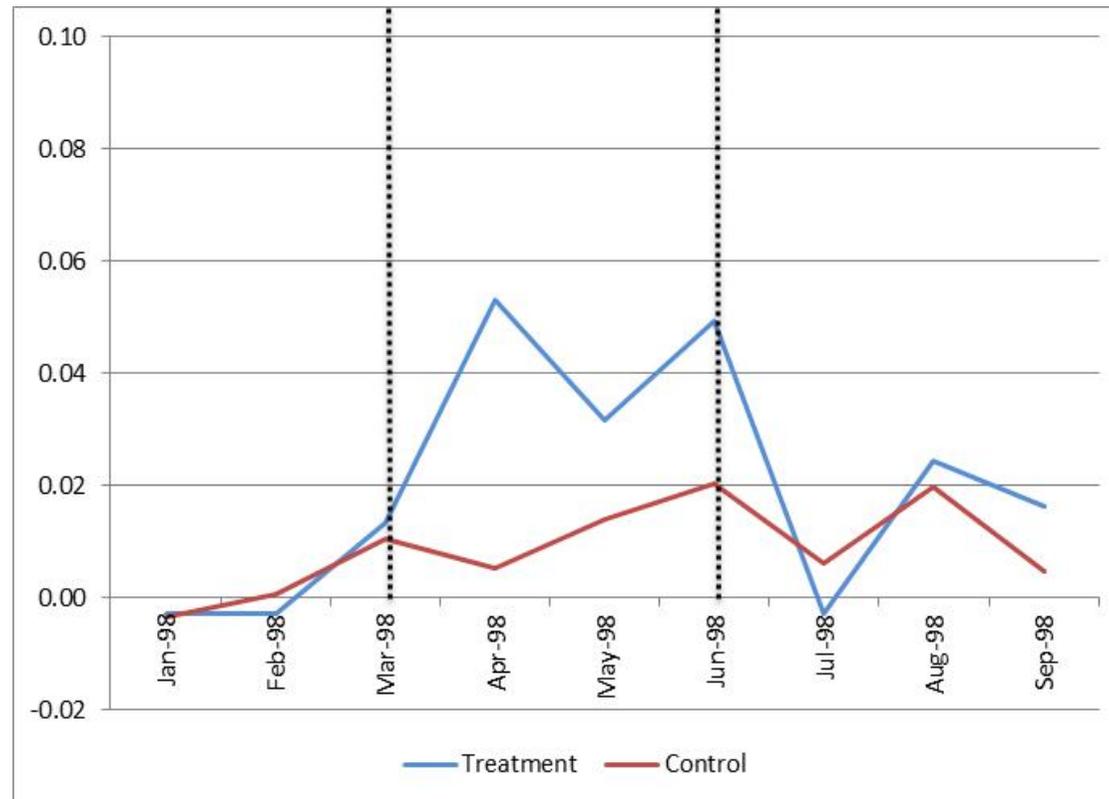


Table 1
Descriptive Statistics

This table presents descriptive statistics for the borrower-bank relationships across treatment and control borrowers (Panel A), and for the pre, interim, and post-credit reform expansion periods (Panel B). Treatment (control) borrowers 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 registry expansion announcement (the interim period) covers April to June 1998, and the post-expansion period, when information has been made public, includes the twelve months from July 1998 onwards. Debt is the dollar loan for each borrower-bank relationship; Total Debt is the total dollar loan for each borrower across all bank relationships; Collateral is an indicator variable equal to one if the loan is secured with collateral; Single Relationship is an indicator variable equal to one if the borrower has just one bank relationship; Number of lenders is the number of lenders the borrower has; and Credit Ratings are assigned by each lender to a borrower, and are integer between 1 (best) and 5 (worst). A rating of 1 (2) represents a borrower in good standing with no (some) potential repayment problems, while a rating greater than 2 represents a degree of default according to specified criteria.

Panel A. Characteristics of Single and Multiple Bank-Relationships

	All			Treatment			Control		
	Mean	Median	Std Dev	Mean	Median	Std Dev	Mean	Median	Std Dev
Panel A1: Borrower-Bank Relationship Level Statistics: All Periods									
	n=46,891			n=10,054			n=36,837		
Debt	126,521	128,300	99,375	112,673	115,483	92,133	130,301	133,600	100,934
Total Debt	221,312	209,600	199,159	200,973	178,250	327,643	226,863	216,367	145,084
Collateral	0.65	1.00	0.48	0.68	1.00	0.47	0.64	1.00	0.48
Single Relationship	0.24	0.00	0.43	0.24	0.00	0.43	0.24	0.00	0.43
Number of Lenders	2.61	2.00	1.46	2.60	2.00	1.44	2.62	2.00	1.47
Credit Rating	1.24	1.00	0.71	1.26	1.00	0.73	1.23	1.00	0.70
Panel A2: Borrower-Bank Relationship Level Statistics for Single Relationship Borrowers									
	n=11,698			n=2,334			n=9,364		
Debt	198,213	201,700	91,753	165,443	167,200	97,539	206,381	207,800	88,387
Total Debt	221,834	204,342	315,975	221,248	170,700	647,804	221,981	210,400	141,998
Collateral	0.75	1.00	0.43	0.72	1.00	0.45	0.75	1.00	0.43
Single Relationship	0.83	1.00	0.38	0.84	1.00	0.36	0.82	1.00	0.38
Number of Lenders	1.23	1.00	0.62	1.26	1.00	0.83	1.23	1.00	0.55
Credit Rating	1.32	1.00	0.83	1.37	1.00	0.88	1.31	1.00	0.82
Panel A3: Borrower-Bank Relationship Level Statistics for Multiple Relationship Borrowers									
	n=35,193			n=7,720			n=27,473		
Debt	102,691	89,600	89,909	96,719	87,700	84,162	104,369	90,250	91,390
Total Debt	221,138	211,800	140,229	194,844	181,950	113,192	228,527	218,800	146,087
Collateral	0.61	1.00	0.49	0.67	1.00	0.47	0.60	1.00	0.49
Single Relationship	0.04	0.00	0.20	0.05	0.00	0.22	0.04	0.00	0.19
Number of Lenders	3.07	3.00	1.37	3.01	3.00	1.34	3.09	3.00	1.38
Credit Rating	1.21	1.00	0.65	1.23	1.00	0.68	1.20	1.00	0.65

Panel B. Bank Relationships across Sample Periods

Period=	Sample Means			
	All	Pre	Interim	Post
Panel A: All Borrowers n=46,891				
Debt	126,521	130,534	129,646	124,540
Total Debt	221,312	214,305	221,520	223,234
Collateral	0.65	0.63	0.64	0.65
Exclusive Relationship	0.24	0.30	0.25	0.22
Number of Lenders	2.61	2.28	2.58	2.72
Credit Rating	1.24	1.08	1.17	1.30
Panel B: Single Relationship Borrowers n=11,698				
Debt	198,213	211,759	206,249	192,235
Total Debt	221,834	211,759	212,983	227,066
Collateral	0.75	0.73	0.74	0.75
Exclusive Relationship	0.83	1.00	0.91	0.76
Number of Lenders	1.23	1.00	1.13	1.33
Credit Rating	1.32	1.09	1.19	1.42
Panel C: Multiple Relationship Borrowers n=35,193				
Debt	102,691	103,923	103,883	102,020
Total Debt	221,138	215,139	224,391	221,959
Collateral	0.61	0.60	0.61	0.62
Exclusive Relationship	0.04	0.07	0.02	0.04
Number of Lenders	3.07	2.70	3.07	3.18
Credit Rating	1.21	1.08	1.16	1.26

Table 2
Strategic Downgrades of High Quality Borrowers

This table presents the difference-in-differences effect of the credit registry expansion announcement (interim period) on downgrade hazard rates, estimated using OLS. Treatment (control) borrowers 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 dependent variable, Downgrade, is from Rating = 1 to Rating = 2 in columns (1) to (5) and Rating = 1 to Rating > 3 (or worse) in columns (6) and (7). The sample is all treatment and control borrowers with a Rating = 1 in the prior period, and with a single lending relationship in the pre-period in columns (1) to (3) and column (6), and all such borrowers in columns (4), (5), and (7). We exclude borrowers that have been downgraded from the sample, explaining why the number of observations is lower than in Table 1. The unit of observation is at the borrower-bank-month level. We omit from reporting in column (5) the coefficient estimates of Treated \times Log (# Banks) and Interim \times Log (# Banks). Bank Controls include the log(Total Assets) and the Return on Assets, while Borrower Controls include the log(Debt). Standard errors are clustered at the borrowers level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable	Downgrades From 1 to 2					From 1 to 3, 4 or 5	
	Single			All		Single	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.004 (0.004)	-0.005 (0.006)	0.004 (0.004)	0.001 (0.002)	0.000 (0.004)	-0.002 (0.003)	-0.001 (0.001)
Treated \times Interim	0.027** (0.012)	0.036*** (0.014)	0.025** (0.012)	0.015*** (0.005)	0.030*** (0.010)	-0.009 (0.009)	-0.004 (0.004)
Treated \times Post		0.011 (0.008)					
Log(#Banks)					-0.003 (0.002)		
Treated \times Interim \times Log (#Banks)					-0.019** (0.009)		
Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Time Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes
No. Observations	9,558	9,558	9,558	35,269	35,269	9,558	35,269
R-Sq	0.02	0.02	0.11	0.07	0.07	0.16	0.07

Table 3**Cross-Sectional Differences in Incentives to Strategically Downgrade**

This table presents the difference-in-differences effect of the registry expansion announcement (interim period) on downgrade hazard rates by bank-type, estimated using OLS. Treatment (control) borrowers 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 dependent variable, Downgrade, is from Rating = 1 to Rating = 2. The sample is all treatment and control borrowers with a single lending relationship in the pre-expansion period and with a rating of 1 in the prior period. The unit of observation is at the borrower-bank-month level. We omit from reporting the coefficient estimates of Treated in columns (1) to (3), and Treated \times Domestic, Treated \times Local and Treated \times Entrepreneurs in columns (1), (2) and (3), respectively. Bank Controls include the log(Total Assets) and the Return on Assets, while Borrower Controls include the log(Debt). Standard errors are clustered at the borrower level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable	Downgrades from 1 to 2		
	Single		
Sample Lender	(1)	(2)	(3)
Treated \times Interim	0.002 (0.004)	0.002 (0.004)	-0.006 (0.008)
Treated \times Interim \times Domestic	0.028* (0.015)	-0.010 (0.013)	-0.010 (0.014)
Treated \times Interim \times Local		0.052** (0.024)	0.043** (0.024)
Treated \times Interim \times Entrepreneurs			0.032 (0.027)
Bank Controls	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes
Bank-Time Fixed Effects	Yes	Yes	Yes
No. Observations	9,558	9,558	9,558
R-Sq	0.11	0.11	0.11

Table 4**Change in Credit Exposure to Borrowers with Exclusive Relationships**

This table presents the difference-in-differences effect of the registry expansion announcement (interim period) and public information (post-expansion period) on credit exposure (Log Debt). Treatment (control) borrowers 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 dependent variable is the Log(Debt). The sample is all treatment and control borrowers with a single lending relationship in the pre-expansion period. The unit of observation is at the borrower-bank-month level. We omit from reporting the coefficient estimates of Treated in columns (1) to (3) and, Treated \times Downgrade and Treated \times Interim, in column (3). Bank Controls include the log(Total Assets) and the Return on Assets. Standard errors are clustered at the borrower level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable	Log(Debt)		
	Single		
Sample Lender	(1)	(2)	(3)
Treated \times Interim	0.016 (0.038)	0.014 (0.021)	0.031 (0.046)
Treated \times Post		-0.002 (0.060)	
Treated \times Interim \times Downgrade			0.014 (0.109)
Bank Controls	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Borrower Fixed Effects	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes
No. Observations	11,698	11,698	11,698
R-Sq	0.57	0.57	0.57

Table 5
Information Sharing and Credit Rating Informativeness

This table presents the difference-in-differences effect of the credit rating in the pre-announcement period on default hazard rates for borrowers with a Rating = 2, estimated using OLS. Treatment (control) borrowers 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 dependent variable, Default, is an indicator variable equal to one if the borrower defaults in the subsequent 6-month period in column (1) or 12-month period in column (2). The sample is all treatment and control borrowers with a single lending relationship in the pre-expansion period and with a Rating = 2 in the current period. The unit of observation is at the borrower-bank-month level. Bank Controls include the log(Total Assets) and the Return on Assets, while Borrower Controls include the log(Debt). Standard errors are clustered at the borrower level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable Sample Lender	Default Single	
	(1)	(2)
Default Horizon	6 Mths	12 Mths
Treated	0.022 (0.093)	-0.075 (0.135)
Treated × Pre	0.197* (0.116)	0.246* (0.130)
Bank Controls	Yes	Yes
Borrower Controls	Yes	Yes
Time Fixed Effects	Yes	Yes
Bank Fixed Effects	Yes	Yes
No. Observations	768	435
R-Sq	0.21	0.24

Table 6
Strategic Upgrades of Low Quality Borrowers by Multiple Lenders

This table presents the difference-in-differences effect of the registry expansion announcement (interim period) on upgrade hazard rates, estimated using OLS. Treatment (control) borrowers 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 dependent variable, Upgrade, is from Rating = 2 to Rating = 1 (best). The sample is all treatment and control borrower-bank relationships with a Rating = 1 in the prior period for borrowers with multiple lending relationships in the pre-period in columns (1) to (4), and borrowers with a single lender relationship in column (5). The unit of observation is at the borrower-bank-month level. Bank Controls include the log(Total Assets) and the Return on Assets, while Borrower Controls include the log(Debt). Standard errors are clustered at the borrower level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable	Upgrades from 2 to 1				
	Multiple Lenders				Single Lender
	(1)	(2)	(3)	(4)	(5)
Treated	-0.028 (0.020)				0.113 (0.136)
Treated × Interim	0.097** (0.050)	0.132** (0.063)	0.183** (0.072)	0.181** (0.075)	-0.121 (0.139)
Treated × Post			0.086 (0.064)	0.067 (0.071)	-0.131 (0.136)
Bank Controls	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Borrower Fixed Effects	No	Yes	Yes	Yes	No
Bank Fixed Effects	Yes	Yes	Yes	No	No
Bank-Time Fixed Effects	No	No	No	Yes	Yes
No. Observations	2,365	2,365	2,365	2,365	987
Adj. R-Sq	0.08	0.35	0.35	0.36	0.47

Table 7**Change in Credit Exposure to Borrowers with Multiple Relationships**

This table presents the difference-in-differences effect of the registry expansion announcement (interim period) and public information (post-expansion period) on credit exposure. The dependent variable is the Log(Debt). Treatment (control) borrowers 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 sample is all treatment and control borrowers with multiple lending relationships in the pre-expansion period. The unit of observation is at the borrower-bank-month level. We omit from reporting the coefficient estimates of Treated \times Upgrade and Interim \times Upgrade in column (3). Bank Controls include the log(Total Assets) and the Return on Assets. Standard errors are clustered at the borrower level. Standard errors are clustered at the borrower level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable	Log(Debt)		
	Multiple		
	(1)	(2)	(3)
Treated \times Interim	0.041 (0.028)	0.015 (0.030)	0.042 (0.028)
Treated \times Post		-0.034 (0.048)	
Treated \times Interim \times Upgrade			0.088 (0.262)
Bank Controls	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Borrower Fixed Effects	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	No
No. Observations	35,193	35,193	35,193
R-Sq	0.53	0.53	0.53

Table 8
Informativeness of Multiple Lenders Credit Ratings

This table presents the difference-in-differences effect of the credit rating in the pre-announcement period on default hazard rates for borrowers with a credit rating of 1, estimated using OLS. Treatment (control) borrowers 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 dependent variable, Default, is an indicator variable equal to one if the borrower's credit rating worsens in the subsequent 6-month period in column (1) or 12-month period in column (2). The sample is all treatment and control borrowers with a multiple lending relationships in the pre-expansion period and with a Rating = 1 in the current period. Bank Controls include the log(Total Assets) and the Return on Assets, while Borrower Controls include the log(Debt). The unit of observation is at the borrower-bank-month level. Standard errors are clustered at the borrower level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable Sample Lender	Default _t	
	Multiple	
	(1)	(2)
Default Horizon	6 Mths	12 Mths
Treated	0.024 (0.015)	0.036 (0.023)
Treated × Pre	-0.038** (0.016)	-0.026* (0.016)
Bank Controls	Yes	Yes
Borrower Controls	Yes	Yes
Time Fixed Effects	Yes	Yes
Bank Fixed Effects	Yes	Yes
No. Observations	23,527	13,345
Adj. R-Sq	0.05	0.08

Table 9
Information Sharing and Changes in Bank Lending Relationships

This table presents the difference-in-differences effect of public information (post-expansion period) on bank lending-relationships. Treatment (control) borrowers 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). In Panel A and B the sample is all treatment and control borrowers with single and multiple lending relationships in the pre-expansion period, respectively. Dependent variables are the Log(Debt), the fraction of loan relationships that are secured with collateral (% Collateral), the number of lending relationships (Log(#Banks)), the fraction of lending relationships that are with local banks (% Local), which reflect relationship banks, and the fraction of lending relationships that are with foreign banks (% foreign), which reflect transactional banks. The unit of observation is at the borrower-bank-month level. Columns (1) to (6) include all borrowers, column (7) includes borrowers that maintain a Rating =1, and column (8) includes those borrowers with a Rating > 1. Borrower Controls include the Log(Debt). The unit of observation is at borrower-month level. Standard errors are clustered at the borrower level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

		Panel A						
Sample Lender	Single							
Dependent Variable	All Borrowers						Rating = 1	Rating > 1
	Log(Debt)	% Collateral	Log(#Banks)	% Local	% Foreign	% Local	% Local	% Local
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post	0.002 (0.061)	-0.007 (0.023)	-0.007 (0.025)	-0.041** (0.018)	0.033** (0.014)	-0.014* (0.009)	-0.017* (0.010)	-0.006 (0.005)
Borrower Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower Fixed Effects	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Observations	11,698	11,698	11,698	11,698	11,698	11,698	9,737	1,961
R-Sq	0.55	0.63	0.65	0.11	0.03	0.99	0.99	1.00

Panel B

Sample Lender	Multiple							
	All Borrowers						Rating = 1	Rating > 1
	Log(Debt)	% Collateral	Log(#Banks)	% Local	% Foreign	% Local	% Local	% Local
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post	0.020 (0.040)	-0.003 (0.015)	0.038* (0.021)	-0.009 (0.010)	0.013 (0.010)	-0.008 (0.008)	-0.005 (0.008)	-0.009 (0.033)
Borrower Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower Fixed Effects	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Observations	18,838	13,188	18,838	18,838	18,838	18,838	16,534	2,304
R-Sq	0.56	0.77	0.78	0.07	0.04	0.97	0.97	0.98

Table 10
Information Manipulation and Changes in Bank Lending Relationships

This table presents the difference-in-differences effect of public information (post-expansion period) on bank-relationships for those borrowers that were strategically downgraded. Treatment borrowers are those with total debt between \$150,000 and \$200,000 during the pre-announcement period (January to March 1998). The sample is all treatment borrowers with single lending relationships and a Rating = 1 in the pre-expansion period. Dependent variables are the Log(Debt), the fraction of loan relationships that are secured with collateral (% Collateral), the number of relationships (Log(#Banks)), the fraction of lending relationships that are with local banks (% Local), which reflect relationship banks, and the fraction of lending relationships that are with foreign banks (% foreign). The unit of observation is at the borrower-bank-month level. Borrower Controls include the Log(Debt). The unit of observation is at borrower-month level. Standard errors are clustered at the borrower level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Sample Lender	Single				
	Treatment Borrowers w/Rating = 1 in the Pre-Period				
	Log (Debt)	% Collateral	Log(#Banks)	% Local	% Foreign
Dependent Variable	(1)	(2)	(3)	(4)	(5)
Downgraded × Post	0.070 (0.055)	-0.020 (0.043)	-0.128*** (0.027)	0.021** (0.009)	-0.019* (0.010)
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Borrower Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	2,092	2,092	2,092	2,092	2,092
R-squared	0.51	0.84	0.76	0.97	0.97