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 a 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 with multiple lenders to avoid creditor runs. Our results suggest that manipulation of credit ratings limits the positive effects of credit registries' information disclosure on credit allocation.
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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 common across the world and have a variety of designs. Lenders are often required to provide not only any negative information about the borrowers, such as delinquencies and defaults, but also non-verifiable information, such as a borrower's rating.[1] Surprisingly, in existing cross-country studies, evidence that the adoption of public credit registries affects the supply of credit is ambiguous (Djankov, McLiesh, and Shleifer, 2007; Peria and Singh, 2014).

We show that banks have incentives to manipulate non-verifiable private information included in their borrower credit ratings before sharing it, and that this limits the positive effects of information sharing on the allocation of credit. 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 banks downgrade their high quality borrowers before disclosing the rating information in order to safeguard their

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[1] With the introduction of the Basel II framework lenders use internal ratings to determine capital requirements. The ratings disclosed in public credit registries are typically external ratings reflecting a borrower's creditworthiness, not the Basel II internal ratings used for capital requirements.

informational monopoly. Banks also tend to upgrade low quality borrowers with multiple lenders to avoid creditors runs, which could impair the borrower's financial situation and ability to repay any loan.

To the best of our knowledge, this is the first paper to highlight that banks' incentives may impair the effectiveness of public credit registries. More in general, we highlight that making information public may negatively affect the quality of information if information disclosure feedbacks on an agent's payoff. This has wider policy implications for the debate on how information should be disseminated in financial markets, which we discuss in the conclusions.

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 a borrower's credit rating, that is, a score capturing the loan officer's judgment of the customer's prospects (including private information) in three different periods: (1) a pre-expansion period, before the reform, when banks reported information to the Central Bank, but expected the credit ratings 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.[2] We can thus ask whether banks abnormally modified their borrowers' credit ratings in the interim period after the reform announcement but before having to share the information with other banks. 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.

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[2] Hertzberg, Liberti and Paravisini (2011) exploit the same setting to provide evidence on the consequences of lender coordination problems on loan amounts.

Existing theories help us to formulate hypotheses on the direction in which banks would have incentives to manipulate the credit ratings of the borrowers. 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 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 creditworthiness to other lenders. These effects have been widely documented in the literature even for firms close to the IPO stage (Schenone, 2010) and are expected to be stronger for borrowers that entertain exclusive relationships with their banks.

Under these conditions, we expect that public 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. Banks' 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. [3] Therefore, they have no incentives to downgrade them. On the contrary, the revelation of negative public information about borrowers may induce a creditor run as highlighted by Corsetti,

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[3]Banks may clearly enjoy rents also on low quality borrowers because of lack of competition in the credit market. However, they do not enjoy an informational rent because low quality borrowers are not hurt from being pooled with other borrowers.

Dasgupta, Morris, and Shin (2004) and Hertzberg, Liberti and Paravisini (2011). Observing a negative rating for a given borrower, lenders may revise upwards the probability that the bank sharing the negative rating will withdraw credit and in response cut their own loans. Thus, in order to avoid a creditor run, banks should have an incentive to upgrade low quality borrowers that have multiple relations with other lenders.

In addition to exploiting these theoretical predictions that differ for different subsamples of borrowers to identify rating manipulation, our empirical setting 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.

Before the 1998 reform of the public credit registry, even though it collected information on all borrowers, the Central Bank shared only information about borrowers whose total outstanding debt was above $200,000 and borrowers in default with other lenders 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. Thus, in April 1998, it was decided that credit information for roughly 540,000 borrowers below the threshold 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 high quality borrowers with total borrowing less than $200,000 in the interim period between the announcement and implementation of the reform, using high quality 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 with an information advantage such as local banks or those that entertain exclusive relationships with the borrowers, which presumably enjoy the highest informational rents, for relationships with opaque borrowers, for which adverse selection is greatest, and for borrowers in states with more bank branches, which are more likely to be poached following a decrease in information asymmetry. Similarly, we test whether non-exclusive lenders upgrade low quality borrowers below the $200,000 threshold during the interim period to a larger extent than those in the control sample.

To abstract from the effects of unobserved borrower heterogeneity, we restrict the analysis to borrowers whose pre-expansion total borrowing was between $150,000 and $200,000 (treatment group) and borrowers whose pre-expansion total borrowing was between $200,000 and $250,000 (control group), who we show to be similar in terms of observable characteristics (excluding total borrowing), and perform a number of robustness tests.

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 entirely driven by local banks, that is, by informationally advantaged banks (Berger et al., 2005). In the same vein, we find that exclusive lenders, who are also expected to have private information on their borrowers, are more likely to downgrade high quality borrowers before sharing their ratings. Opaque borrowers and borrowers in more competitive credit markets are more likely to be downgraded. This is consistent with lenders' desire to protect their informational rents in credit markets in which competitors are more likely to poach customers once information is shared. We also find that banks that are lenders to low

quality borrowers with multiple relationships 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 enhances access to credit.

We also examine the effects of the public credit registry expansion on the affected borrowers' bank relationships. We show that banks update their ratings to take into account other lenders ratings. Consequently, the expansion of the public credit registry facilitates new lending to single lender borrowers with the highest credit ratings. 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 benefit 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 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) and common in public credit registries across the world (Powell et al, 2004; Brown, Jappelli, and Pagano, 2009), 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. Hertzberg, Liberti, and Paravisini (2011) also exploit the same expansion of the Argentinian public credit registry as we do 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.

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 being paid by issuers, credit ratings may overstate issuers' creditworthiness (see, for instance, Bolton, Freixas, and Shapiro, 2012; Efing 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 II 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 lenders. In our setting, the incentives to manipulate may be even stronger as the reputational costs 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 they would unambiguously improve the allocation of credit.

For this reason, governments often sponsor the creation of public credit registries. Public credit registries are databases managed by a government agency,

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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 negative and positive information, including late payments, defaults, and ratings (Brown, Jappelli, and Pagano, 2009). In existing studies, public credit registries appear to have more limited effects on the allocation of credit in comparison to private credit bureaus, in which banks voluntarily share information (Love and Mylenko, 2003; Djankov, McLiesh, and Shleifer, 2007; Peria and Singh, 2014).

We argue that information manipulation may limit the positive effects of public credit registries. Even if regulators audit the information provided to the credit registry, banks may manipulate non-verifiable information on borrowers, such as credit ratings, before reporting it to the credit registry. Not only would this behavior be consistent with banks' incentives, but it would also be hard to prevent for government authorities because the reported information is difficult to verify. Detecting any manipulation of the ratings reported to the registry is likely to be particularly difficult if ratings concern opaque borrowers, such as small entrepreneurial firms.

1.2 Banks' Incentives to Share and Manipulate Information

Since making information about the borrowers public can affect their payoffs, banks have incentives to manipulate hard to verify information, such as credit ratings.

Existing theories allows us to formulate stringent hypotheses on the direction of manipulation. As we discuss below, these predictions starkly differ for different groups of borrowers.

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. The same adverse selection problem that allows banks to charge excessive interest rates prevents high quality borrowers from reacting to a strategic downgrade (if they observe the change in credit rating) as claims of being unfairly rated would not be verifiable by outsiders.

A bank is more likely to have 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 have different information about their borrowers. Thanks to their flat organizational structures, local banks have the highest level of private information about their borrowers (Berger et al., 2005), while foreign banks base their lending decisions on easy to verify, mostly public information (Mian, 2006; Giannetti and Ongena, 2009). Similarly, local banks being specialized in lending to a particular region are expected to have more private information than banks active nation-wide (Berger et al., 2005). We thus expect that the incentives to manipulate ratings and to downgrade high quality borrowers should be stronger for local banks, which have more private information and should have more to lose in terms of informational rents.

Incentives for lenders to low quality borrowers are different. Lenders to low quality borrowers do not enjoy informational rents because the credit risk of these borrowers warrants a high interest rate. Making public negative information about the borrower may lead other lenders to withdraw their loans or to increase the interest rate causing financial distress for the borrower and impairing the value of the loan of all banks, including the one disclosing negative information (Hertzberg, Liberti, and Paravisini, 2011). Thus, if their low quality clients have multiple lenders, banks have an incentive to upgrade the ratings of the borrowers before sharing them.

We expect this mechanism to hinge to a lower extent on the extent of private information of a bank. If lenders interpret a negative credit rating as a signal that the bank issuing the negative rating will restrict or withdraw credit to the borrower, then all other lenders may want to cut their loans right away irrespective of their private information on the borrower. To avoid creditor runs, and the negative feedback

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effects that these may have on their balance sheets, all banks (not only the most informationally advantaged) are expected to strategically upgrade borrowers with multiple lenders.

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 on whether the borrower has multiple banks. Therefore, any evidence supporting the conjecture that lenders manipulate the credit ratings before making them public 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 manipulate ratings before making them public.

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. Its design is typical of public credit registries around the world (Powell, Majnoni, Miller, and Mylenko, 2004; Brown, Jappelli, and Pagano, 2009). Therefore, any conclusions of our study on whether non-verifiable information, such as credit ratings, can be shared without manipulation has broad applicability.

All financial institutions active in Argentina are required to report to the Central Bank the amount of the loan, the amount of collateral pledged, and the rating of 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 rating based on its private assessment of the borrower repayment

prospects. Ratings ranging from 3 to 5 are 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. Therefore, banks have to set aside more capital when assigning ratings of 3 or higher, while granting a rating of 1 or 2 has no implications for capital requirements.[4]

While discretional, 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. Also lenders often attribute different ratings to the same borrower indicating that ratings include private information.[5]

Up to 1998, the Central Bank provided, using monthly magnetic tapes, information on the most recent cross-section of borrowers with a total amount of loans above a $200,000 threshold to financial institutions and credit rating agencies making request of it.[6] The Central Bank also provided information on borrowers with a default rating of 3 or higher regardless of the amount of their loans. In April 1998, the Central Bank announced the switch to CD-ROMs, which substantially lowered information sharing costs and made it feasible to distribute monthly information on all

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[4] Banks commonly use credit ratings even if these are not shared in the credit registry to categorize borrowers' creditworthiness. These credit ratings are often shared in public credit registries and are typically different from the internal credit ratings used to compute capital requirements under Basel II (Brown, Jappelli, and Pagano, 2009; Powell, Majnoni, and Mylenko, 2004). As we discuss in the conclusions, any regulations requiring banks to share the Basel II internal credit ratings in public credit registries would not invalidate the conclusions of our paper, but make them even more relevant.

[5] Figure IA.1 shows the within-borrower standard deviation in credit ratings. Throughout the sample period there is some disagreement indicating that lenders have private information. However, disagreement is greatest when the ratings are private, that is, for treatment borrowers pre-reform. Table IA.1 tests formally that banks update their ratings after observing other lenders' ratings.

[6] The Central Bank aggregated all loans outstanding to a borrower and made the information public if the total loan amount was $200,000 of above.

borrowers even the ones with total loan amount below the $200,000 threshold and with a rating better than 3.

Since lenders could always infer any borrower's amount of loans from other lenders from financial statements and tax returns, and delinquencies and defaults were shared in the registry, the reform made available the credit ratings assigned by existing lenders to non-deliquent borrowers (that is, borrowers with 1 and 2 ratings) with a total amount of loans below the $200,000 threshold.[7]

We thus focus on banks' incentives to manipulate the reported 1 and 2 ratings. 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 maintain their information advantage for some borrowers.

The expansion of the credit registry was announced in April 1998 and implemented in July of the same year. Thus, the ratings of the first quarter of 1998 for borrowers with total borrowing below the $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.

In April 1998, institutions learnt that their ratings would be shared with other existing and potential lenders.[8] However, until July of the same year, they did not observe other banks' ratings. Any systematic changes in the ratings of borrowers with

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[7] Also, borrowers have strong incentives to reveal whether they have other lenders in the contracting phase to decrease their interest rate.

[8] 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 successfully ask for a lower interest rate.

total borrowing 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.[9]

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

2.2 Empirical Framework

The stages of the reform implementation 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 informational rents. To preserve their informational monopoly, banks may thus be inclined to strategically downgrade high quality borrowers before sharing their ratings. We expect this propensity to be predominantly driven by local banks, which are better than other

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[9] The strategic manipulation can achieve the objective of fudging information even if other lenders observed the history of the ratings. In the interim period, at least some downgrades from 1 to 2 might have been driven by the arrival of negative information. Therefore, potential lenders were unable to distinguish between actual and strategic downgrades and to offer downgraded borrowers with a 2 rating loans at the same favorable conditions as to borrowers with a 1 rating. This weakened competition for downgraded borrowers and is consistent with the empirical evidence in Tables 12 that these borrowers did not experience any benefits from the credit registry.

lenders at collecting information on small opaque borrowers and should therefore have a stronger informational advantage.

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 total borrowing above \$200,000 were already public. To the extent that borrowers above and below the \$200,000 threshold are similar, any abnormal downgrades for borrowers below the threshold (the treatment group) than for borrowers above the threshold (the control group) are expected to capture strategic downgrades.

Most of our tests focus on the subsample of borrowers with a single relationship and rating of 1 (or on borrowers with multiple relationships and a rating of 2) without comparing borrowers with single and multiple relationships or different ratings. Thus, differential exposures of single- and multiple-lender borrowers, or of borrowers with different credit ratings, to economic shocks cannot drive our findings.

Our identifying assumption is that, within each group of borrowers, borrowers with total borrowing below and above the \$200,000 threshold are subject to similar shocks. For this reason, to abstract from any effects of borrowers' heterogeneity, which could lead to downgrades, we consider only borrowers that in the pre-period had total outstanding loans between \$150,000 and \$250,000 and perfom robustness around this interval. In what follows, we present extensive evidence corroborating our identifying assumption.

Focusing on the highest quality single-lender borrowers, for which we expect banks to enjoy the highest informational rents and to have the strongest incentives to manipulate ratings downwards, we estimate the following equation:

$$downgrade^{1to2,s}_{i,b,t} = \beta_0 + \beta_1 \times Treated_i + \beta_2 \times Treated_i \times Interim_t + \beta_3 \times Treated_i \times Post_t +$$
$$+\xi_t + \lambda_b + \phi_{b,t} + \varepsilon_{i,t} \hspace{4cm} (1)$$

where $downgrade^{1to2,s}_{i,b,t}$ is a dummy variable that takes a value equal to 1 if a single-lender borrower (s) 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. Consistent 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.

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), bank-time ($\phi_{b,t}$), and borrower industry-time 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 minimum amount of total outstanding loans was below $200,000 January-March 1998 (the pre-announcement period), and was therefore omitted from the public registry prior to the expansion. We cluster errors at the borrower level.[10]

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.

We develop the above framework to test whether some local banks having more private information are more inclined to manipulate than other lenders. We also explore cross-sectional differences across borrowers.

Next, we test whether a rating of 2 is associated with a lower probability of default 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.

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. To test whether there is any evidence of strategic upgrades, 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. If banks indeed manipulate the borrowers' ratings before making them public, we expect that treated borrowers are more likely to be upgraded in the interim period if they have multiple relationships.

3. Sample and Descriptive Statistics

We condition on borrowers that had 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. We assign to the

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treatment group borrowers with total borrowing between $150,000 and $200,000 in the pre-period and to the control group borrowers with total borrowing between $200,000 and $250,000 during the same period.

Table 1 presents descriptive statistics for the sample of treatment and control borrowers. 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 all, treatment, and control borrowers during the pre-period with the aim of validating our identifying assumption. We further sorts on the exclusivity of the lending relationship. Examining treatment and control groups in Panel A, the mean loan for treatment (control) borrowers is for $113,167 ($135,155), with total borrowing of $179,517 ($223,562). Overall, the two groups are similar in terms of collateral, credit rating, number of banks, size (as measured by a categorical variable assigning each borrower to one of six categories based on number of employees), and importantly the proportion of borrowers with exclusive relationships. The proportion of urban borrowers is only slightly higher in the control group.

We find similar evidence that treatment and control groups are comparable (except for lending amounts) for both exclusive and multiple-relationship borrowers in Panels B and C, respectively. Figure 1 shows that the distribution of treatment and control borrowers during the pre-period is remarkably similar across 23 industries, 6 size categories, and 24 states for both exclusive and multiple-relationship borrowers.

In Figure 2 there appear to be more borrowers with debt just above the $200,000 threshold than just below in February 1998. 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 in February 1998 appear remarkably continuous above and below the threshold, suggesting that any manipulation does not lead to uncomparability of the treatment and control samples.[11]

This evidence provides strong support for our identifying assumption that treatment and control borrowers are similar once we subordinate to their ratings and to their single or multiple relationship borrower status.[12]

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 a decrease in 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.

Figure 3 further explores how credit ratings vary around the registry reform. Distinguishing between the single lender subsample (Figure 3A) and the multiple lender subsample (Figure 3B), we plot the proportion of borrowers that have a credit

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[11] Cross-sectional differences are similar in January and March 1998.
[12] Further supporting our identifying assumption, Table IA.3 of the Internet Appendix shows that borrowers become more likely to default in the post period. However, there is no difference in defaults for treatment or control borrowers either in the single or multiple lender sample.

rating of 2. We focus only on borrowers 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 3A, 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 estimate 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 treatment group is downgraded but the difference is not significant either in absolute or relative terms.

Interestingly, the treatment group converges to the control group. This is precisely what we would expect based on our hypothesis because the ratings of borrowers in the control group were already public. Therefore, if the mechanisms we highlight are at work, banks should already have strategically downgraded some of the borrowers above the threshold.

In Figure 3B, 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.

Figure 4 reproduces Figure 3, but considers only local banks. The patterns that emerge are very similar to Figure 3 indicating that the differences before and after the

reform are not driven by an increasing presence of foreign banks, or by local banks becoming more similar to foreign banks.

In the Internet Appendix, we present analogous figures for foreign banks and large domestic banks. As we will discuss in Subsection 4.1, the tendency to strategically downgrade is driven by local banks, while all of banks appear inclined to strategically upgrade multiple lender borrowers.

This evidence is fully consistent with the conjecture that banks manipulate ratings downward to capture good borrowers, and manipulate ratings upward to pre-empt 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.

Figures 3 and 4 also show that 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 evidence that treated high quality borrowers, defined as borrowers which maintain a rating of 1 and total loans below the $200,000 threshold in the period preceding the announcement of the credit registry expansion, are more

likely to be downgraded in the interim period than borrowers in the control sample.[13]

We consider only the subsample of borrowers that in the pre-period entertained a single relationship, for which we expect banks' informational rent, and, consequently, incentives to manipulate ratings before their release, to be stronger.

Treated borrowers with a rating of 1 appear 3 percentage points more likely to be downgraded during the interim period than borrowers in the control sample. This is economically large relative to the unconditional probability of a downgrade of 0.016 in whole sample and 0.011 in the pre-period.

Under the identification assumption that treated borrowers did not receive stronger negative shocks than borrowers with slightly larger loans included in the control sample, this evidence indicates that banks strategically downgrade high quality borrowers to preserve their informational advantage.

In column 2, we test whether the propensity to downgrade high quality borrowers persist in the post-period, once the ratings have been made public. A higher propensity to downgrade treatment borrowers than control borrowers may suggest that the quality of treated borrowers is deteriorating, possibly because of the approaching Argentinian recession, and should be interpreted as evidence against our identification assumption that treatment and control borrowers are similarly exposed to shocks. We find no evidence that banks continue to abnormally downgrade treated borrowers in the post-period.

To provide further evidence that borrowers above and below the threshold are not subject to asymmetric shocks, Figure 5 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

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[13] 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.

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.

Local banks are known to have a stronger informational advantage than other lenders, often under the form of soft information, which is hard to collect in large banks that are active nation-wide and internationally (Berger et al., 2005). Therefore, we expect local banks to have more to lose in terms of informational rents from sharing the private information contained in the credit rating. Consistently with our hypothesis that banks' abnormal propensity to downgrade in the interim period is driven by the desire to protect their informational rents, columns 3 and 4 show that the tendency to downgrade high quality single lender borrowers is entirely due to local banks. Column 4 also shows that this tendency emerges in the interim, but not in the post period, indicating that the customers of local banks are unlikely to be more exposed to shocks than control borrowers.

In column 5, we control for bank-specific shocks and industry specific-shocks by including bank-time and industry-time fixed effects. The results are invariant. The robustness of the results indicates that bank-specific shocks and borrower industry composition or seasonal effects cannot drive our findings.[14] Finally, column 6 shows that our results are unaffected when we use a three month post period, instead of a nine month one as in the earlier specifications.

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

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[14] Table IA.4 in the Internet Appendix further controls for the possibility that treated borrowers are more likely to be in agriculture and relatively more exposed to shocks in the interim period. The triple interaction term Treated $\times$ Agriculture $\times$ Interim is not statistically significant dispelling this concern.

transactional relationships with some of their multiple banks and have a close relationship with a few others.

Table 3 shows that all our findings hold also for borrowers with multiple relationships. That is, we find an abnormal propensity to downgrade high quality borrowers in the interim period (column 1), which is driven by local banks (column 2) and does not emerge in the post-period once the rating becomes public (column 3). As we would expect, the effects are somewhat smaller than in the case in which we focus on single relationship borrowers.

In column 4, we explicitly test the conjecture that the incentives to strategically downgrade borrowers with multiple relationships are weaker. 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.

These results support our conjecture that banks strategically downgrade high quality borrowers below the threshold to preserve their informational rents. In particular, any alternative explanation based on borrowers below the threshold being more exposed to shocks would have to explain why borrowers below the threshold with multiple banks are less likely to be downgraded.

Table 4 further evaluates the merit of the criticism that borrowers below the threshold are more exposed to negative shocks. 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. We find no evidence of that both in the single lender sample (column 1) and in the sample that also includes multiple lender borrowers (column 2).

The rest of Table 4 presents a local sample analysis that further supports our identifying assumptions. While in our benchmark specifications we consider

borrowers with total debt between $150,000 and $250,000, in columns 3 and 4, we reestrimate the specification in column 5 of Table 2 and restrict the interval to borrowers with total debt between $175,000 and $225,000 and between $125,000 and $250,000. It is conforting that the estimates are hardly affected when changing the sample we vary the extent of unobserved heterogeneity. Also, the effect of *Treated* × *Interim* is not statistically significant in the placebo tests presented in columns 5 and 6, where borrowers with loan amounts strictly below and strictly above the cutoff are considered in the estimation.

Table 5 tests whether more opaque borrowers are more likely to be downgraded. If banks' strategic downgrades aim to preserve informational rents, banks should downgrade to a lower extent borrowers with less information asymmetry. Large borrowers are more visible and therefore less informationally captured. Columns 1 and 2 of Table 5 show that indeed strategic downgrades affect large borrowers, defined as firms with employees above the median of 25, to a lesser extent. Urban borrowers may also be more visible to loan officers. To the extent that they face less asymmetric information, their lenders are able to enjoy less informational rents and should have weaker incentives to downgrade them as we find in columns 3 and 4 of Table 5.

In Table 6, we further show that borrowers in areas in which lenders face less competition are less likely to be downgraded. We measure lack of competition as the number of borrowers per branch in a state. In states with a higher number of borrowers per branch, borrowers are less likely to be poached even after the expansion of the credit registry. Incentives to strategically downgrade should therefore be weaker, as we find.

The manipulation of credit ratings would imply that the pool of borrowers with a rating of 2 becomes less likely to default as some high quality borrowers have been downgraded. This is precisely what we find in Panel A of Table 7. Treatment 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 treatment borrowers receiving the same rating in the interim or the post-period. Thus, on average, borrowers with a rating of 2 are better quality after the announcement of the credit registry expansion than in the pre-expansion period.[15] This effect is unlikely to be driven by the business cycles 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. Also, a higher exposure to the Argentinian approaching recession would imply that treatment borrowers with a rating of 2 should be more, not less likely to default.[16]

One may also wonder whether banks just downgrade marginal borrowers, perhaps slightly changing the internal cutoff for different ratings. While this behavior would be consistent with manipulation, it may make the results less striking and also less relevant from a policy point of view. If banks just downgraded marginal borrowers, the pool of borrowers rated 1 should have lower default rates post manipulation because the worse borrowers are now rated 2. In Panel B of Table 7, we find no evidence that this is the case, suggesting that the downgrades affect not only

[15] In Table IA.5 of the Internet Appendix, we also find that treated borrowers that are downgraded in the interim period are not more likely to default than treated 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.

[16] The conclusion that downgrades are driven by information manipulation rather than by the arrival of new information is also consistent with the finding that banks on average do not change the amount of the loan in the interim period (see Table IA.6 for single lender borrowers and Table IA.7 for multiple lender borrowers).

marginal borrowers but also the best quality borrowers that are rated 1, comfortably above the cutoff.

Taken together, the results in Panel A and B of Table 7 imply that default rates converge for treated borrowers with a rating of 1 and 2 in the period of the credit registry expansion. Put differently, we find that the ratings of treatment borrowers become less informative even after we include bank and borrower controls, and industry, bank and time fixed effects. Thus, 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.

4.2 Strategic Upgrades of Low Quality Borrowers with Multiple Bank Relationships

Lenders to low quality borrowers with multiple lenders may fear that the public revelation of negative information to other lenders will 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 be less likely to be repaid. Therefore, banks may have an incentive to strategically upgrade multiple lender borrowers with a rating of 2.

This is precisely what we find when we estimate the probability that a borrower is upgraded from 2 to 1 in the multiple lender subsample. The results are presented in Table 8. Treatment borrowers with a rating of 2 are more likely to be upgraded in the interim-period in comparison to borrowers in the control sample, whose credit ratings were already public. The magnitude of the effects in column 1 is 8.9 percentage points, large compared to the unconditional propensity to upgrade for

this subsample of borrowers of 0.19 overall and 0.16 in the pre-period. The effect is almost double in column 2 once we include borrower fixed effects.[17] Consistent with our identification assumption, the abnormal propensity to upgrade treatment borrowers with multiple lenders exists in the interim period, but not in the post period.

Our argument that banks strategically upgrade borrowers with multiple lenders to avoid creditor runs does not hinge on lenders' private information. Observing a negative rating, other lenders may revise upwards the probability that the bank with negative rating will withdraw credit. We would therefore expect all banks, not only local banks, to strategically upgrade their borrowers. This is precisely what we find in columns 3 and 5.

Finally, the strategic upgrades are not driven by bank or industry-specific shocks as the results in column 6 are robust to the inclusion of bank-time and industry-time fixed effects. Column 7 further shows that our results are robust when we use a 3 month post period.

Table 9 provides futher robustness tests. 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 what we find in column 1. This is comforting because it further assuages concerns that treated borrowers with a rating of 2 may be subject to positive shocks.

The finding in column 1 is also important to dispel concerns that changes in credit ratings are due to a systematic revision of *all* credit ratings aiming to update information on the borrowers before sharing it with other financial institutions. If the changes in ratings we observe were driven by a mere incorporation of new

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[17] 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.

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 cross-sectional differences are consistent with theoretical predictions and the finding that credit ratings become less informative) indicates that changes in ratings are not driven by systematic revisions, but are instead strategic.

The rest of Table 9 presents a subsample analysis mimicking the one in Table 4. The results in columns 2 to 5 fully support our identifying assumption. In column 2, where we use a smaller set of borrowers around the cutoff, thus limiting unobserved heterogeneity, the coefficient of *Treated × Interim* increases with respect to the benchmark specification in column 6 of Table 8. The contrary is true in column 3, where we use a larger set of borrowers around the cutoff. Also, the effect of *Treated × Interim* is not statistically significant in the placebo tests, presented in columns 4 and 5, where borrowers with loan amounts strictly below and strictly above the cutoff are considered in the estimation.

Consistent with the interpretation that the upgrades of low quality borrowers are strategic, Panel A of Table 10 shows that the subsample of borrowers with multiple lenders and a credit rating of 1 was better quality before the announcement of the reform. Treated borrowers with a rating of 1 have a lower probability of future default before the announcement of the credit registry expansion.[18] Thus, while the subsample of single lender borrowers with a rating of 2 becomes relatively better and the quality of single lender borrowers with a rating of 1 is invariant, the subsample of multiple lender borrowers with a rating of 1 becomes relatively worse.

Panel B of Table 10 explores whether the upgraded borrowers are marginal borrowers. If the borrowers upgraded from the pools of 2s were the relatively better

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[18] Also multiple lenders borrowers upgraded in the interim period are on average as likely to default as borrowers that have not been upgraded and the size of their loans remain unchanged.

ones, the quality of multiple lender borrowers with a rating of 2 should decline post manipulation. We find that the quality of borrowers of multiple lenders with a rating of 2 is unchanged, on average, suggesting once again that strategic upgrades occur across the board.[19]

This evidence parallels the findings on the probability of default in the single borrower sample. There is a convergence in the probability of default of borrowers with 1 and 2 ratings also in the multiple lender subsample as multiple lender borrowers with a rating of 1 were less likely to default in the pre-period. Thus, also in this subsample ratings become less informative.

Taken jointly, our findings are fully consistent with the banks' incentives to manipulate information with the ultimate goal of maximizing their net wealth and are hard to explain with shocks affecting the treated borrowers but not the control borrowers. For our results to be driven by negative shocks, it would have to be that single lender treatment borrowers with an initial rating of 1 are more exposed to negative shocks than control borrowers, while the contrary would have to hold for multiple lender treatment borrowers with an initial rating of 2. This seems implausible given that treatment and control are based on the same total outstanding loan threshold in the two subsamples.

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, credit ratings become less informative once it is known that they will be shared with other banks. This implies that only garbled

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[19] Only the quality of single lenders borrowers, with a rating of 2 improves after the reform, as shown in Panel A of Table 7.

information is revealed. Is this information sufficiently informative to affect bank-borrower relationships and the structure of loan contracts?

Table 11 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 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) and the number of relationships (column 2) do not change.

However, following the credit registry expansion, the fraction of debt provided by local banks to treatment borrowers decreases (column 3), and the fraction of debt provided by foreign banks increases (column 4). 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 3 and 4 in Panel A of Table 11 imply that the credit registry expansion provided greater access to foreign funding, and that treated borrowers shifted debt away from local lenders.

In columns 3 and 4, 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 5 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. If the credit registry mitigates this friction, 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 6) or greater than 1 (column 7) 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 2). 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 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.

While overall the credit registry expansion may have advantaged transaction banks at the expense of relationship banks, the strategic downgrades appear to have been effective in preserving some of the rents of relationship banks. This is evident in

Table 12, 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 10. 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 rating manipulation impacts the allocation of credit.[20]

5. Broader Implications and Conclusions

We provide evidence that banks manipulate credit ratings before being compelled to share them by the extension of a public credit registry. Our findings are important for the design of public credit registries. Policymakers often use these tools for supervision as well as for attempting to limit asymmetric information between lenders and between borrowers and lenders. We highlight that making positive, non-verifiable information about the borrowers public to other lenders may affect negatively the quality of information available for supervision.

Our findings are even more relevant in the light of a debate on the design of public credit registries that has accompanied the implementation of Basel II. While the credit ratings banks share with other lenders in public credit registries are typically not the internal credit ratings used for Basel II capital requirements, the possibility of

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[20] 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.

sharing internal credit ratings has been widely discussed (see, for instance, Powell et al 2004 and Hertig 2006).

Our results imply that such regulations could reinforce the incentives to report manipulated ratings in a way that varies over the business cycles. To the extent that fears of creditor runs are stronger when capital requirements are binding and lending standards are expected to tighten, regulations requiring lenders to share borrowers' internal ratings would strengthen banks' incentives to strategically upgrade borrowers with multiple lenders.

In normal times, banks are willing to hold levels of capital well above regulatory minimums (Flannery and Rangan, 2008; Gropp and Heider, 2008; Allen, Carletti, and Marquez, 2011). Even if the ratings shared in the registry were the same as those used for Basel II, banks would still have an incentive to strategically downgrade good borrowers if the capital requirements are not binding. This is most likely during good times, when competitors are likely to expand their loan portfolio and competition in the credit market is stronger.

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. We show that ratings are manipulated in a situation in which the rater is the lender. This result has implications for alternative models to disseminate information in financial markets that have been considered after the financial crisis.

Following the Dodd Frank Act, insurance companies obtain regulator-paid expected loss assessments from PIMCO and Blackrock to compute their capital requirements. If PIMCO or Blackrock increase the expected loss assessment, insurance companies are likely to sell in the same way as creditors may withdraw

their loans after observing a not so good rating. Since PIMCO and Blackrock invest themselves in the securities they assess, an increase in the expected loss assessment would prompt sales and feedback negatively on the valuations of their holdings. Our results imply that this alternative model to disseminate information is not immune from conflicts of interest because any agents will behave strategically if they expect information disclosure to feedback on their profits. An important area of research would be to understand whether the incentives to manipulate we highlight are at work outside the banking industry and public credit registries.

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Figure 1
The Distribution of Treatment and Control Borrowers Across Observable Characteristics Before the Credit Reform Expansion

These plots show the cross-sectional kernel density distributions for observable characteristics across treatment and control borrowers during the period before the credit registry reform announcement from January 1998 till March 1998 (pre-period). The three time-invariant observable characteristics are: industry classification, size and geographical location. The plots are for all treatment and control borrowers in single lending relationships (left-hand side) and multiple lending relationships samples (right-hand side). 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. Industry Classification includes 23 unique industries following the Argentinean federal tax administration authority (*Administración Federal de Ingresos Públicos (AFIP)*). We use the classification according to the economic activities defined in *Clasificación de Actividades Económicas* (CLAE Number 883). The size measure is a categorical variable from 1 (lowest) to 6 (highest) measured by the borrower's number of employees. The geographical location corresponds to one of the 24 states where the borrower is legally incorporated. The source of information for size and geographical location is the Argentinean federal tax administration authority (AFIP). We also perform two-sample Kolmogorov-Smirnov tests for the equality of distributions for each of the observable characteristics across treatment and control borrowers in the single lending relationships and the multiple lending relationships samples. The distributions across treatment and control borrowers for each of the observable characteristic are not statistically different from each other.

Industry Classification
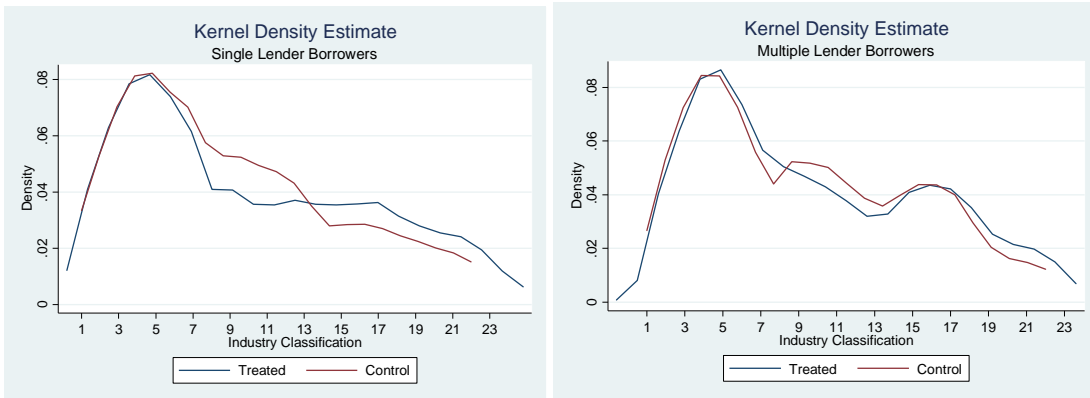
Size
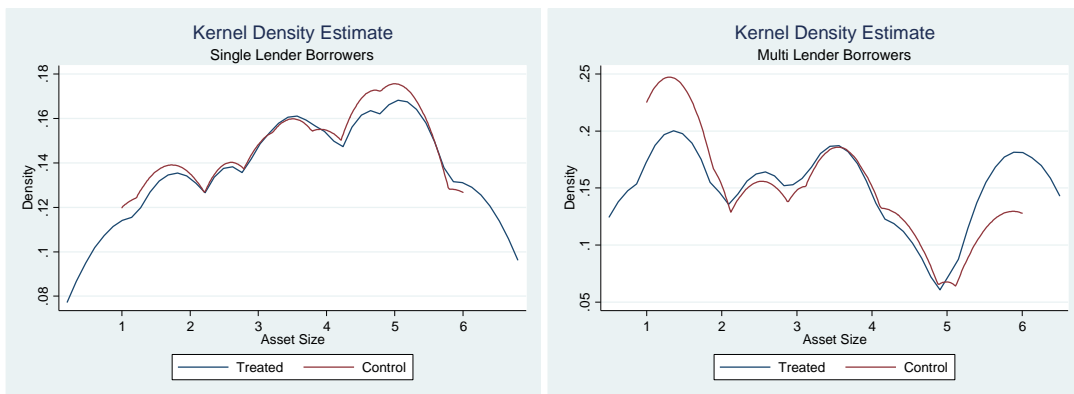
Geographical Location (State)
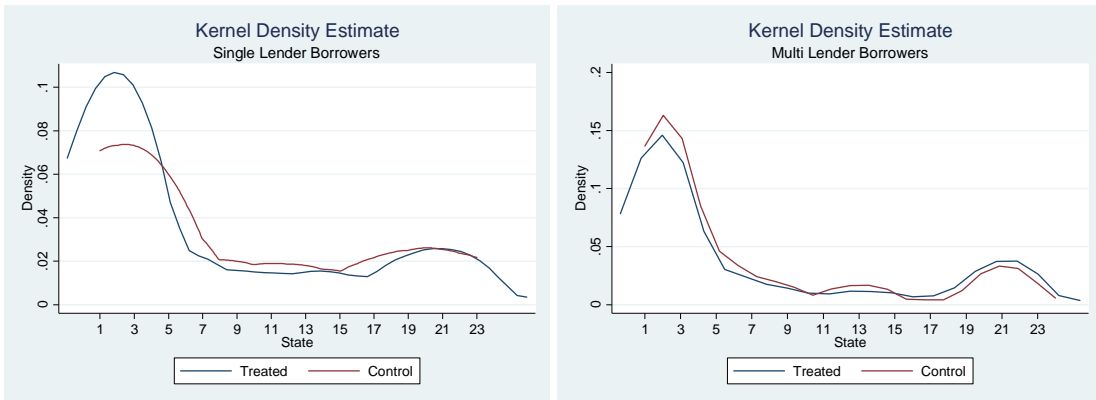
Figure 2
The Distribution of Borrowers by Total Debt Before the Credit Reform Expansion

This figure shows the cross-sectional distribution of the number of borrowers for different levels of total debt and some average salient observable 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; (d.) Upgrades; (e.) Urban, and (f.) Large = Size is equal or greater to size 3 (> 25 employees). Urban and Large are indicator variables equal to one if the borrower is located in the capital city of each state, and the borrower size is equal or greater to 3, respectively
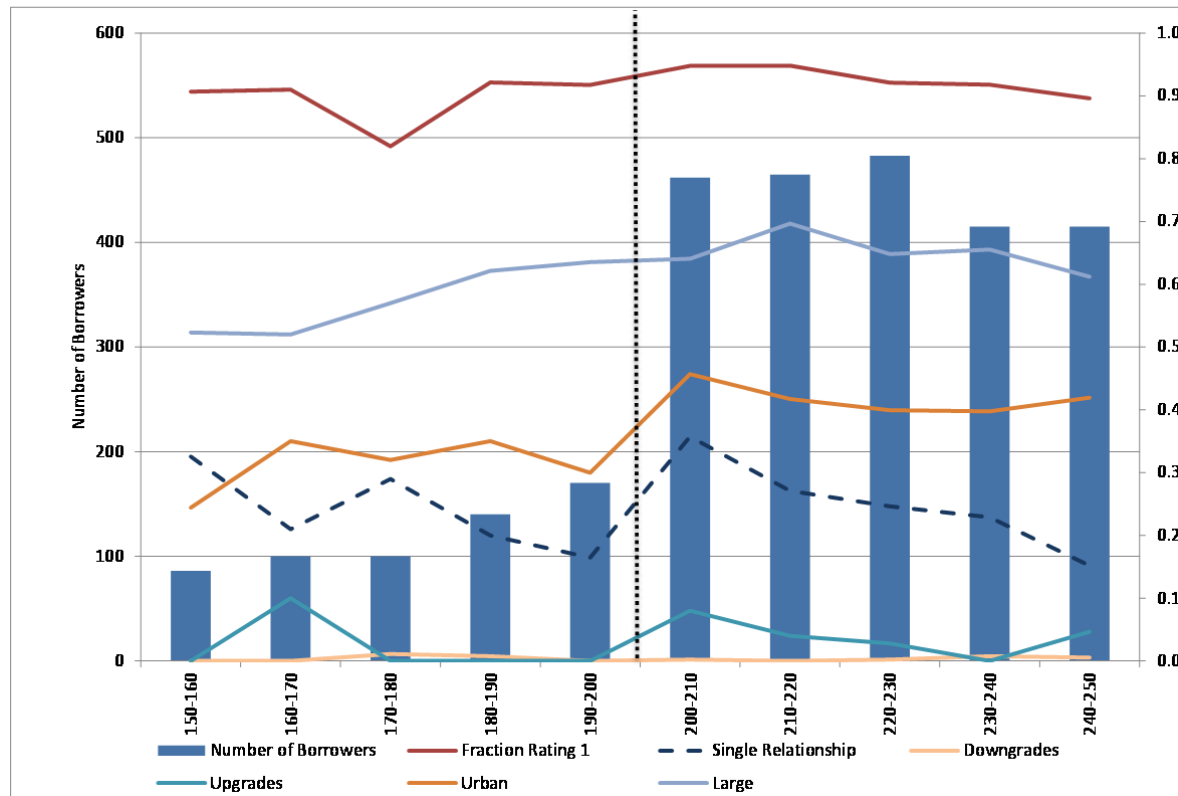
Figure 3
Upgrades and Downgrades Around the Credit Reform Expansion

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). We track the bank-borrower relationships that existed as of January 1998 for borrowers that did not become delinquent or default in the pre, interim, and 3-month post period. The unit of observation is at the borrower-bank-month level.
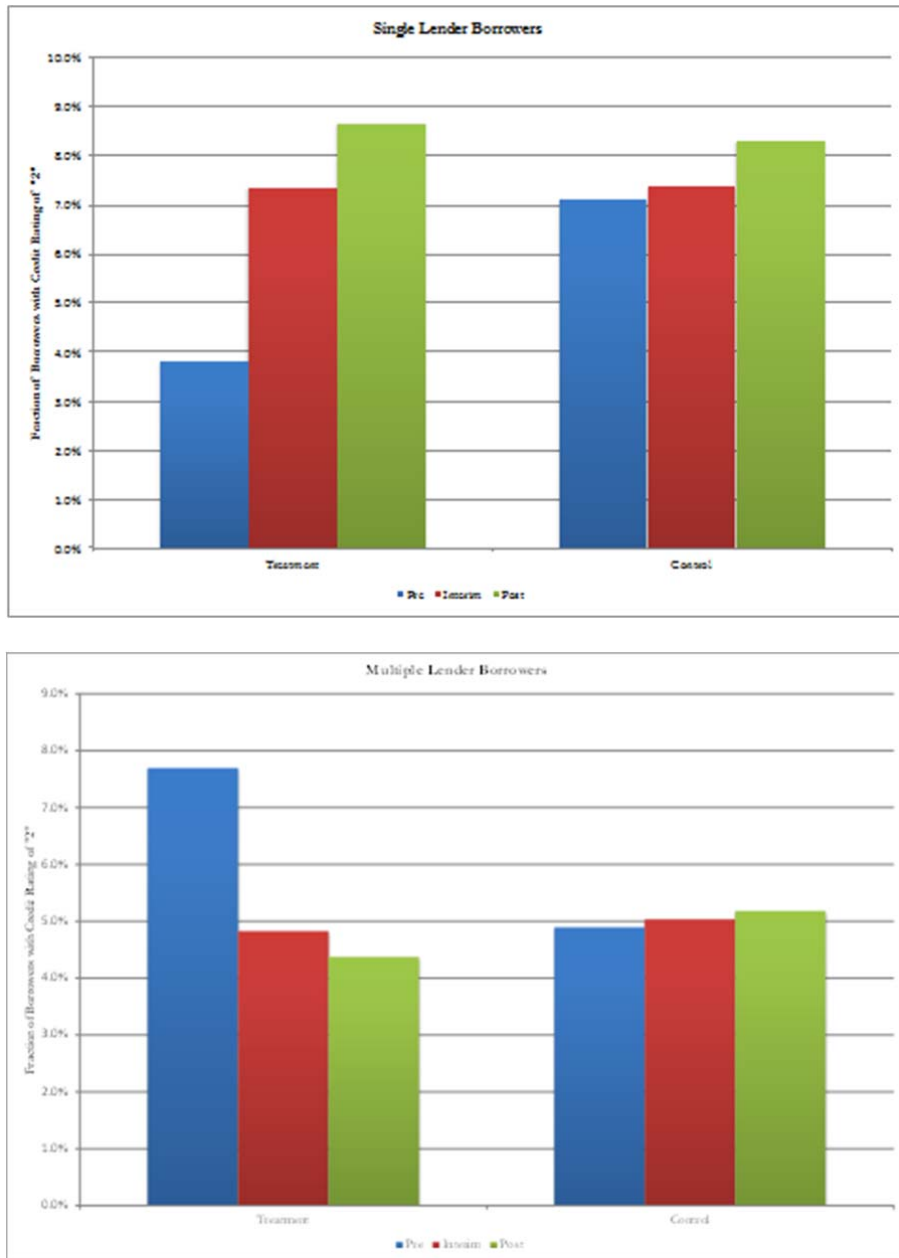
Figure 4
Upgrades and Downgrades Around the Credit Reform Expansion: Local Banks

These figures show the fraction of borrowers with a Rating = 2 for single lending relationship borrowers and multiple lending relationship borrowers, respectively, considering lending relationships with local banks only. 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). We track the bank-borrower relationships that existed as of January 1998 for borrowers that did not become delinquent or default in the pre, interim, and 3-month post period. The unit of observation is at the borrower-bank-month level.
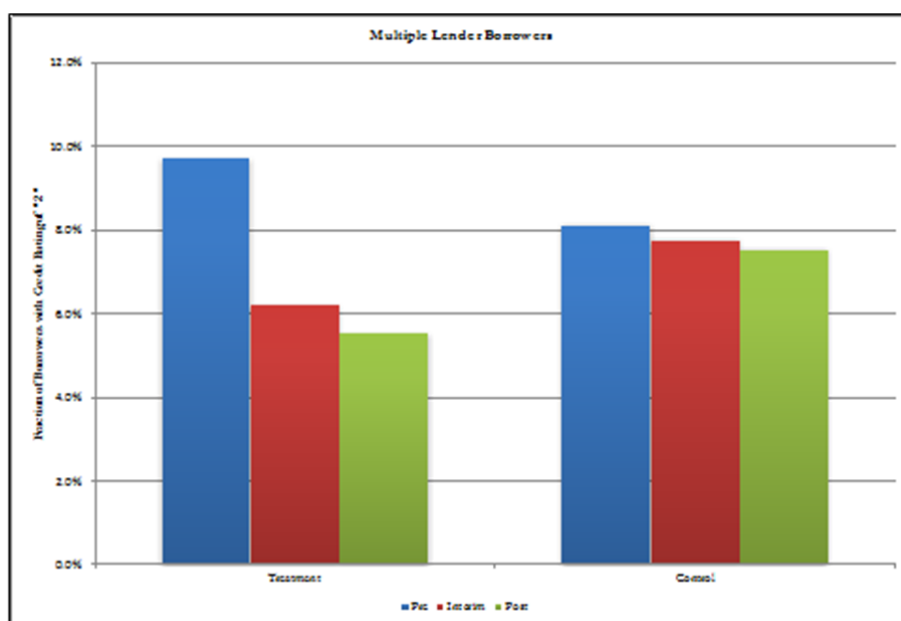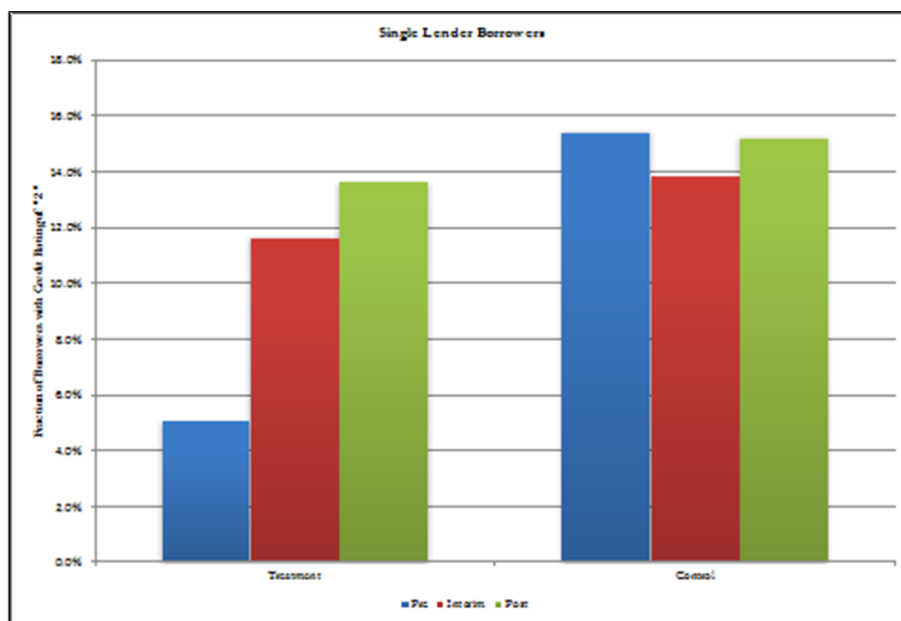
Figure 5
Conditional Estimates of Strategic Downgrades by Month for Single Lenders

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, the bank and borrower controls, as well as bank fixed effects. Bank controls include log(Total Assets), Return on Assets, Capitalization (%), log(Number of Branches), Non-Performing Loans-to-Total Assets, Deposits-to-Total Assets and Growth on Total Assets, while borrower controls include the log(Debt), State, Size, Industry Classification and Urban.
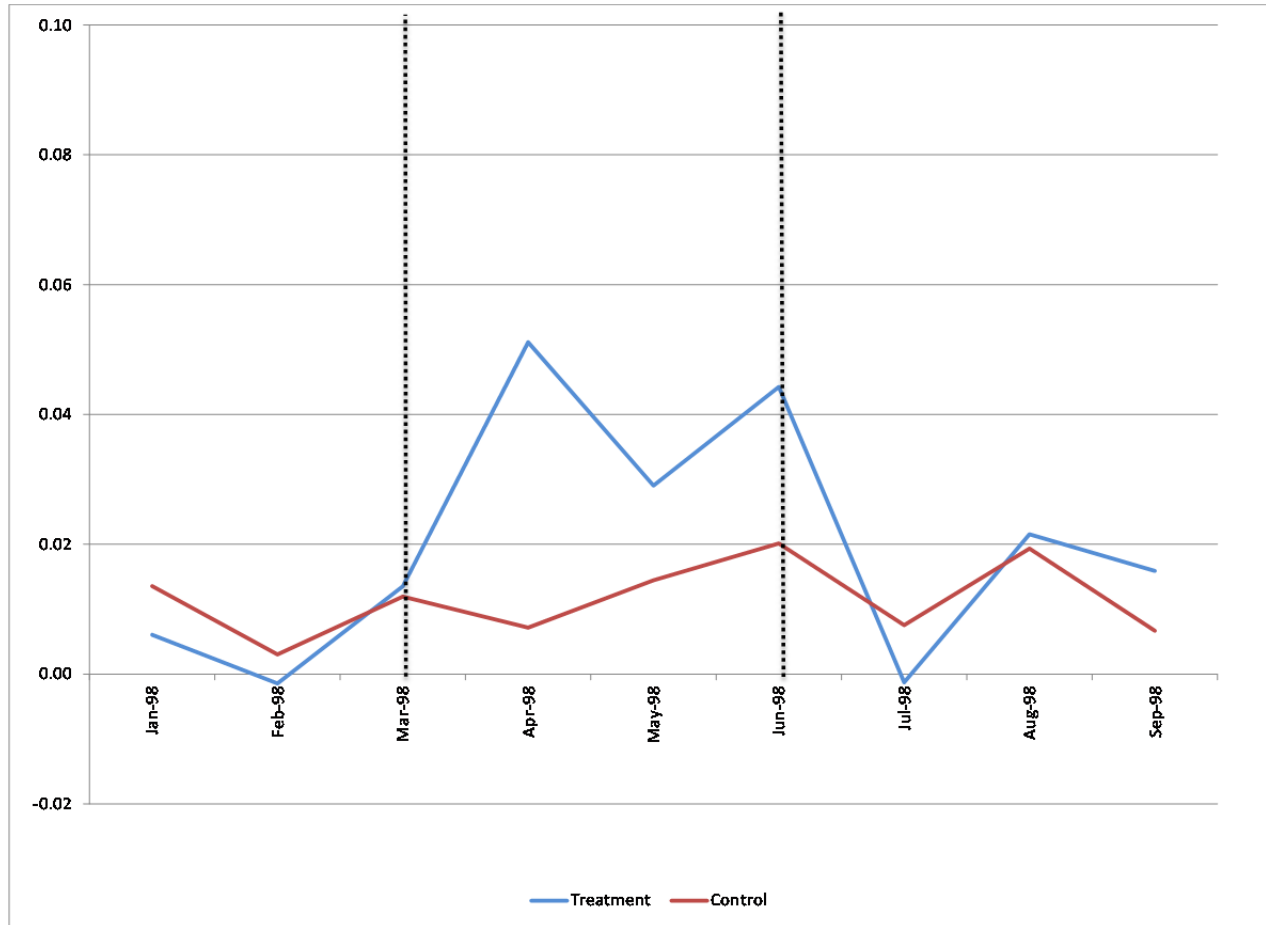
Table 1
Descriptive Statistics

This table presents descriptive statistics for the borrower-bank relationships across treatment and control borrowers during the pre-announcement period only (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. Panel A is divided in three panels following different samples: (A1) for all borrower-bank relationships; (A2) for single relationship borrowers; and, (A.3) for multiple relationship borrowers. Loan Amount is the dollar loan for each borrower-bank relationship; Total Debt is the total dollar amount of loans 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; Credit Ratings are assigned by each lender to a borrower, and are integer between 1 (best) and 5 (worst); Size is an indicator variable equal to one if the borrower size classification is equal or larger than 3 (> 25 employees); and, Urban is an indicator variable equal to one if the borrower is located in the capital city of each state. 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											
		n=8,522				n=1,791				n=6,731	
Loan Amount	130,534	151,450	83,864		113,167	133,000	67,129		135,155	161,300	87,204
Total Debt	214,305	216,700	25,743		179,517	179,700	21,550		223,562	222,500	17,543
Collateral	0.63	1.00	0.48		0.66	1.00	0.47		0.62	1.00	0.49
Single Relationship	0.25	0.00	0.43		0.22	0.00	0.42		0.25	0.00	0.43
Number of Lenders	2.28	2.00	1.21		2.24	2.00	1.20		2.29	2.00	1.21
Credit Rating	1.08	1.00	0.29		1.09	1.00	0.30		1.08	1.00	0.29
Size	3.42	3.00	1.88		3.19	3.00	1.84		3.49	3.00	1.88
Urban	0.40	0.00	0.49		0.31	0.00	0.46		0.42	0.00	0.49
Panel A2: Borrower-Bank Relationship Level Statistics for Single Relationship Borrowers											
		n=2,103				n=402				n=1,701	
Loan Amount	211,759	213,000	24,137		176,147	177,600	16,903		220,175	219,350	16,798
Total Debt	211,759	213,000	24,137		176,147	177,600	16,903		220,175	219,350	16,798
Collateral	0.73	1.00	0.45		0.69	1.00	0.46		0.74	1.00	0.44
Single Relationship	1.00	1.00	0.00		1.00	1.00	0.00		1.00	1.00	0.00
Number of Lenders	1.00	1.00	0.00		1.00	1.00	0.00		1.00	1.00	0.00
Credit Rating	1.09	1.00	0.30		1.08	1.00	0.28		1.09	1.00	0.30
Size	3.71	4.00	1.85		3.67	4.00	1.83		3.72	4.00	1.86
Urban	0.46	0.00	0.50		0.38	0.00	0.49		0.48	0.00	0.50
Panel A3: Borrower-Bank Relationship Level Statistics for Multiple Relationship Borrowers											
		n=6,419				n=1,389				n=5,030	
Loan Amount	103,923	97,700	79,226		94,940	97,100	65,174		106,404	97,950	82,521
Total Debt	215,139	218,000	26,195		180,492	181,100	22,630		224,707	223,850	17,643
Collateral	0.60	1.00	0.49		0.65	1.00	0.48		0.58	1.00	0.49
Single Relationship	0.00	0.00	0.00		0.00	0.00	0.00		0.00	0.00	0.00
Number of Lenders	2.70	2.00	1.11		2.59	2.00	1.13		2.73	2.00	1.10
Credit Rating	1.08	1.00	0.29		1.09	1.00	0.31		1.08	1.00	0.29
Size	3.33	3.00	1.88		3.05	3.00	1.82		3.41	3.00	1.88
Urban	0.38	0.00	0.48		0.30	0.00	0.46		0.40	0.00	0.49

Panel B. Bank Relationships across Sample Periods

	Sample Means			
Period=	All	Pre	Interim	Post
	Panel A: All Borrowers			
	n=46,891			
Loan Amount	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
Single 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
Size	3.41	3.42	3.42	3.40
Urban	0.38	0.40	0.40	0.38
	Panel B: Single Relationship Borrowers			
	n=11,698			
Loan Amount	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
Single 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
Size	3.71	3.71	3.71	3.71
Urban	0.44	0.46	0.46	0.43
	Panel C: Multiple Relationship Borrowers			
	n=35,193			
Loan Amount	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
Single 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
Size	3.31	3.33	3.32	3.29
Urban	0.37	0.38	0.37	0.36

Table 2
Strategic Downgrades of High Quality Borrowers

This table presents the difference-in-differences effect of the credit registry expansion announcement (interim period) and public information (post-expansion 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. The sample is all treatment and control borrowers with a Rating = 1 in the pre-period, and with a single lending relationship in the pre-period. Columns (1) to (5) use a 12-month post-period horizon, while column (6) uses a 3-month post-period horizon. We exclude borrowers that have been downgraded from the sample, explaining why the number of observations is lower than in Table 1. Local is an indicator variable equal to one if the lender is a local bank. The unit of observation is at the borrower-bank-month level. Bank controls include log(Total Assets), Return on Assets, Capitalization (%), log(Number of Branches), Non-Performing Loans-to-Total Assets, Deposits-to-Total Assets and Growth on Total Assets, while borrower controls include the log(Debt), State, Size, Industry Classification and Urban. 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					
Sample Lender	Single					
Period	All					3-month Post
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.004	-0.006	0.002	0.008	0.000	0.004
	(0.004)	(0.007)	(0.004)	(0.007)	(0.004)	(0.005)
Treated × Interim	0.026**	0.036***	-0.004	-0.011	0.002	-0.001
	(0.012)	(0.014)	(0.009)	(0.011)	(0.009)	(0.009)
Treated × Post		0.013		-0.008		
		(0.008)		(0.008)		
Treated × Interim × Local			0.053**	0.064***	0.038*	0.054**
			(0.021)	(0.023)	(0.020)	(0.021)
Treated × Post × Local				0.034		
				(0.024)		
Bank Controls	Yes	Yes	Yes	Yes	No	No
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	No	No
Bank Fixed Effects	Yes	Yes	Yes	Yes	No	No
Industry Fixed Effects	Yes	Yes	Yes	Yes	No	No
Bank-Time Fixed Effects	No	No	No	No	Yes	Yes
Industry-Time Fixed Effects	No	No	No	No	Yes	Yes
No. Observations	9,558	9,558	9,558	9,558	9,558	5,151
R-Sq.	0.03	0.03	0.03	0.03	0.16	0.18

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

This table presents the difference-in-differences effect of the registry expansion announcement (interim period) and public information (post-expansion 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, from Rating = 1 to Rating = 2. The sample is all treatment and control borrowers with a single and multiple lending relationships and with a rating of 1 in the pre-period. The unit of observation is at the borrower-bank-month level. Local is an indicator variable equal to one if the lender is a local bank; log(#Banks) is the log of the total number of bank-borrower relationships. We omit from reporting the coefficient estimates of Treated $\times$ Local in columns (2), (3) and (4). Borrower controls include log(Debt), State, Size, Industry Classification and Urban. 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			
Sample Lender	All			
	(1)	(2)	(3)	(4)
Treated	-0.000	0.001	-0.000	0.003
	(0.002)	(0.003)	(0.003)	(0.004)
Treated $\times$ Interim	0.015***	0.001	0.003	0.016
	(0.005)	(0.006)	(0.007)	(0.011)
Treated $\times$ Interim $\times$ Local		0.026**	0.040***	0.022**
		(0.011)	(0.015)	(0.011)
Treated $\times$ Post			0.002	
			(0.004)	
Treated $\times$ Post $\times$ Local			0.015	
			(0.010)	
log(#Banks)				-0.002
				(0.002)
Treated $\times$ log(#Banks)				-0.016*
				(0.009)
Borrower Controls	Yes	Yes	Yes	Yes
Bank-Time Fixed Effects	Yes	Yes	Yes	Yes
Industry-Time Fixed Effects	Yes	Yes	Yes	Yes
No. Observations	35,269	35,269	35,269	35,269
R-Sq.	0.08	0.08	0.08	0.08

Table 4
Robustness: Strategic Downgrades of High Quality Borrowers

This table presents a series of robustness tests of the difference-in-differences effect of the registry expansion announcement (interim period) on downgrade hazard rates by bank-type, estimated using OLS. The dependent variable, Downgrade, is from Rating = 1 to Rating = 3, 4 or 5 in columns (1) and (2) and from Rating = 1 to Rating = 2 in columns (3) to (6). The sample is all treatment and control borrowers with a Rating = 1 in the pre-period, and with a single lending relationship in the pre-period in columns (1) and (3) to (6), and with single and multiple lending relationship in the pre-period in column (2). 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 columns (1) and (2). Treatment (control) borrowers are those with total debt between $175,000 and $200,000 or $125,000 and $200,000 ($200,000 and $225,000 or $200,000 and $275,000) during the pre-announcement period (January to March 1998) in columns (3) and (4), respectively. Using only treated borrowers, in column (5): treatment (control) borrowers are those with total debt between $100,000 and $150,000 ($150,000 and $200,000) during the pre-announcement period (January to March 1998). Using only control borrowers, in column (6): treatment (control) borrowers are those with total debt between $200,000 and $250,000 ($250,000 and $300,000) during the pre-announcement period (January to March 1998). The unit of observation is at the borrower-bank-month level. We omit from reporting the coefficient estimates of Treated × Local in all columns. Borrower controls include the log(Debt), State, Size, Industry Classification and Urban. 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 3, 4 or 5		Downgrades From 1 to 2			
Sample Lender	Single	All	Single			
			Robustness			
	Placebo: Default of Treatment and Control	Placebo: Default of Treatment and Control	Cutoff: $175K-$225K	Cutoff: $125K-$275K	Treatment Only ($100K-$150K is Treated; $150K-$200K is Control)	Control Only ($200K-$250K is Treated; $250K-$300K is Control)
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.003	-0.001	-0.003	-0.002	0.025	-0.002
	(0.002)	(0.001)	(0.005)	(0.003)	(0.017)	(0.006)
Treated × Interim	0.005	0.004	-0.002	0.002	-0.018	-0.007
	(0.003)	(0.003)	(0.008)	(0.007)	(0.034)	(0.011)
Treated × Interim × Local	-0.023	-0.014	0.037*	0.034**	0.022	0.000
	(0.016)	(0.008)	(0.020)	(0.017)	(0.032)	(0.016)
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
No. Observations	9,558	35,269	5,678	12,863	3,820	12,655
R-Sq.	0.19	0.08	0.22	0.13	0.22	0.15

Table 5
Cross-Sectional Borrower 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 borrower-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 and with a rating of 1 in the pre-period. The unit of observation is at the borrower-bank-month level. Characteristic is Large Borrowers (columns (1) and (2)) or Urban Borrowers (columns (3) and (4)). Large Borrowers is an indicator variable equal to one if the borrower size classification is equal or larger than 3 (> 25 employees); Urban is an indicator variable equal to one if the borrower is located in the capital city of each state; Local is an indicator variable equal to one if the lender is a local bank. We omit from reporting the coefficient estimates of Treated × Characteristic, Interim × Characteristic in all columns; Treated × Local, Interim × Local, Treated × Local × Characteristic, Interim × Local × Characteristic in columns (2) and (4). Borrower controls include log(Debt), State, Industry Classification and Urban in columns (1) and (2) and include log(Debt), Size, State and Industry Classification in columns (3) and (4). 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			
Sample Lender	Single			
	Borrower Characteristic			
	Large Borrowers		Urban Borrowers	
	(1)	(2)	(3)	(4)
Treated	-0.010	-0.009	0.002	-0.008
	(0.007)	(0.006)	(0.006)	(0.006)
Treated × Interim	0.058**	-0.001	0.043**	0.001
	(0.024)	(0.012)	(0.018)	(0.011)
Treated × Interim × Characteristic	-0.048*	0.003	-0.047**	0.000
	(0.028)	(0.013)	(0.021)	(0.017)
Treated × Interim × Local		0.077**		0.056**
		(0.032)		(0.026)
Treated × Interim × Characteristic × Local		-0.066*		-0.070*
		(0.041)		(0.037)
Borrower Controls	Yes	Yes	Yes	Yes
Bank-Time Fixed Effects	No	Yes	Yes	Yes
Industry-Time Fixed Effects	No	Yes	Yes	Yes
No. Observations	9,558	9,558	9,558	9,558
R-Sq.	0.16	0.16	0.16	0.16

Table 6
Competition and Strategic Downgrades of High Quality Borrowers

This table presents the difference-in-differences effect of the registry expansion announcement (interim period) on downgrade hazard rates by lender-type and degree of competition, 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 and with a rating of 1 in the pre-period. The unit of observation is at the borrower-bank-month level. Number of borrowers per branch at the state level measures the degree of competition. #Borrowers/Branch. is the ratio of the total number of borrowers over the total number of branches for each lender at the state level; Local is an indicator variable equal to one if the lender is a local bank. Bank controls include log(Total Assets), Return on Assets, Capitalization (%), log(Number of Branches), Non-Performing Loans-to-Total Assets, Deposits-to-Total Assets and Growth on Total Assets, while borrower controls include the log(Debt), State, Size, Industry Classification and Urban. 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		
Sample Lender	Single		
	(1)	(2)	(3)
Treated	0.004	0.003	-0.000
	(0.004)	(0.004)	(0.004)
Treated × Interim	0.026**	0.026**	0.004
	(0.012)	(0.012)	(0.009)
Treated × Interim× #Borrowers/Branch	-0.029**	-0.030*	-0.029*
	(0.014)	(0.016)	(0.016)
Treated × Interim × Local			0.035*
			(0.020)
Bank Controls	Yes	No	No
Borrower Controls	Yes	Yes	Yes
Time Fixed Effects	Yes	No	No
Bank Fixed Effects	Yes	No	No
Industry Fixed Effects	Yes	No	No
Bank-Time Fixed Effects	No	Yes	Yes
Industry-Time Fixed Effects	No	Yes	Yes
No. Observations	9,558	9,558	9,558
R-Sq.	0.03	0.16	0.16

Table 7
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). In Panel A, the sample is all treatment and control borrowers with a single lending relationship in the pre-period and with a Rating = 2 in the current period. In Panel B, the sample is all treatment and control borrowers with a single lending relationship in the pre-period and with a Rating = 1 in the current period. The unit of observation is at the borrower-bank-month level. Bank controls include log(Total Assets), Return on Assets, Capitalization (%), log(Number of Branches), Non-Performing Loans-to-Total Assets, Deposits-to-Total Assets and Growth on Total Assets, while borrower controls include the log(Debt), State, Size, and Urban. Standard errors are clustered at the borrower level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

	Panel A	
Dependent Variable	Default	
Sample Lender	Single and Rating = 2	
	(1)	(2)
Default Horizon	6 Mths	12 Mths
Treated	-0.014	-0.084
	(0.102)	(0.122)
Treated × Pre	0.273**	0.175*
	(0.109)	(0.100)
Bank Controls	Yes	Yes
Borrower Controls	Yes	Yes
Time Fixed Effects	Yes	Yes
Bank Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
No. Observations	768	435
R-Sq.	0.34	0.43

	Panel B	
Dependent Variable	Default	
Sample Lender	Single and Rating =1	
	(1)	(2)
Default Horizon	6 Mths	12 Mths
Treated	0.001	0.009
	(0.021)	(0.034)
Treated × Pre	0.017	0.010
	(0.030)	(0.028)
Bank Controls	Yes	Yes
Borrower Controls	Yes	Yes
Time Fixed Effects	Yes	Yes
Bank Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
No. Observations	7,387	4,238
R-Sq.	0.15	0.25

Table 8
Strategic Upgrades of Low Quality Borrowers by Multiple Lenders

This table presents the difference-in-differences effect of the registry expansion announcement (interim period) and public information (post-expansion 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 pre-period for borrowers with multiple lending relationships in the pre-period. Columns (1) to (6) use a 12-month post-period horizon, while column (7) uses a 3-month post-period horizon. Local is an indicator variable equal to one if the lender is a local bank. The unit of observation is at the borrower-bank-month level. Bank controls include log(Total Assets), Return on Assets, Capitalization (%), log(Number of Branches), Non-Performing Loans-to-Total Assets, Deposits-to-Total Assets and Growth on Total Assets, while borrower controls include the log(Debt), State, Size, Industry Classification and Urban. 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						
Sample Lender	Multiple						3-month Post
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	-0.030						
	(0.022)						
Treated × Interim	0.089*	0.152**	0.209**	0.162*	0.222***	0.147**	0.180**
	(0.051)	(0.064)	(0.071)	(0.094)	(0.111)	(0.061)	(0.074)
Treated × Post			0.096		0.103		
			(0.064)		(0.099)		
Treated × Interim × Local				-0.034	-0.041		
				(0.130)	(0.148)		
Treated × Post × Local					-0.014		
					(0.125)		
Bank Controls	Yes	Yes	Yes	Yes	Yes	No	No
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	No	No
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	No	No
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	No	No
Borrower Fixed Effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Time Fixed Effects	No	No	No	No	No	Yes	Yes
Industry-Time Fixed Effects	No	No	No	No	No	Yes	Yes
No. Observations	2,365	2,365	2,365	2,365	2,365	2,365	1,240
R-Sq.	0.11	0.36	0.36	0.36	0.36	0.66	0.59

Table 9
Robustness: Strategic Upgrades of Low Quality Borrowers by Multiple Lenders

This table presents a series of robustness tests of the difference-in-differences effect of the registry expansion announcement (interim period) on upgrade hazard rates, estimated using OLS. The dependent variable, Upgrade, is from Rating = 2 to Rating = 1 (best). The sample is all treatment and control borrowers with a Rating = 2 in the pre-period, and with a single lending relationship in the pre-period in columns (1), and with multiple lending relationship in the pre-period in columns (2) to (5). 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 column (1). Treatment (control) borrowers are those with total debt between $175,000 and $200,000 or $125,000 and $200,000 ($200,000 and $225,000 or $200,000 and $275,000) during the pre-announcement period (January to March 1998) in columns (2) and (3), respectively. Using only treated borrowers, in column (4): treatment (control) borrowers are those with total debt between $100,000 and $150,000 ($150,000 and $200,000) during the pre-announcement period (January to March 1998). Using only control borrowers, in column (5): treatment (control) borrowers are those with total debt between $200,000 and $250,000 ($250,000 and $300,000) during the pre-announcement period (January to March 1998). The unit of observation is at the borrower-bank-month level. Borrower controls include the log(Debt), State, Size, and Urban. 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				
Sample Lender	Single	Multiple			
			Robustness		
	Placebo: Upgrade of Single Lender Borrowers	Cutoff: $175K-$225K	Cutoff: $125K-$275K	Treatment Only ($100K-$150K is Treated; $150K-$200K is Control)	Control Only ($200K-$250K is Treated; $250K-$300K is Control)
	(1)	(2)	(3)	(4)	(5)
Treated	-0.017				
	(0.032)				
Treated × Interim	-0.004	0.182**	0.090*	-0.016	0.011
	(0.035)	(0.090)	(0.049)	(0.089)	(0.044)
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Borrower Fixed Effects	No	Yes	Yes	Yes	Yes
Bank-Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Industry-Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
No. Observations	987	1,199	4,089	1,569	4,310
R-Sq.	0.67	0.76	0.59	0.69	0.59

Table 10
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). In Panel A, the sample is all treatment and control borrowers with a multiple lending relationships in the pre-period and with a Rating = 1 in the current period. In Panel B, the sample is all treatment and control borrowers with a multiple lending relationships in the pre-period and with a Rating = 2 in the current period. Bank controls include log(Total Assets), Return on Assets, Capitalization (%), log(Number of Branches), Non-Performing Loans-to-Total Assets, Deposits-to-Total Assets and Growth on Total Assets, while borrower controls include the log(Debt), State, Size, and Urban. 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.

	Panel A	
Dependent Variable	Default	
Sample Lender	Multiple and Rating = 1	
	(1)	(3)
Default Horizon	6 Mths	12 Mths
Treated	0.020	0.032
	(0.015)	(0.022)
Treated × Pre	-0.033**	-0.025*
	(0.016)	(0.015)
Bank Controls	Yes	Yes
Borrower Controls	Yes	Yes
Time Fixed Effects	Yes	Yes
Bank Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
No. Observations	23,539	13,355
R-Sq.	0.07	0.11

	Panel B	
Dependent Variable	Default	
Sample Lender	Multiple and Rating = 2	
	(1)	(2)
Default Horizon	6 Mths	12 Mths
Treated	0.015	0.017
	(0.046)	(0.072)
Treated × Pre	-0.025	-0.070
	(0.066)	(0.077)
Bank Controls	Yes	Yes
Borrower Controls	Yes	Yes
Time Fixed Effects	Yes	Yes
Bank Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
No. Observations	1,700	924
Adj. R-Sq	0.178	0.15

Table 11
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-period, respectively. Dependent variables are the Log(Debt), 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 (5) include all borrowers, column (6) includes borrowers that maintain a Rating =1 with all lenders, and column (7) includes those borrowers with at least one Rating > 1. Borrower controls include the State, Size, Industry Classification and Urban in column (1) and log(Total Debt), State, Size, Industry Classification and Urban in columns (2)-(7). 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	Single Lender						
	All Borrowers					Rating = 1	Rating > 1
Dependent Vaiable	og(Total Debt)	Log(#Banks)	% Local	% Foreign	% Local	% Local	% Local
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated × Post	0.021	-0.017	-0.032**	0.026*	-0.015*	-0.017*	-0.005
	(0.064)	(0.025)	(0.015)	(0.015)	(0.009)	(0.010)	(0.005)
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower Fixed Effects	Yes	Yes	No	No	Yes	Yes	Yes
Industry-Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Observations	11,698	11,698	11,698	11,698	11,698	9,737	1,961
R-Sq.	0.57	0.68	0.44	0.24	0.99	0.99	0.99

Sample	Multiple Lender						
	All Borrowers					Rating = 1	Rating > 1
Dependent Vaiable	Log(Total Debt)	Log(#Banks)	% Local	% Foreign	% Local	% Local	% Local
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated × Post	0.009	0.044**	-0.006	0.009	-0.008	-0.006	-0.002
	(0.038)	(0.018)	(0.010)	(0.011)	(0.008)	(0.008)	(0.023)
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower Fixed Effects	Yes	Yes	No	No	Yes	Yes	Yes
Industry-Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Observations	18,838	18,838	18,838	18,838	18,838	16,534	2,304
R-Sq.	0.58	0.79	0.411	0.231	0.97	0.97	0.98

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Table 12
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-period. Dependent variables are the Log(Debt), 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 State, Size, Industry Classification and Urban in column (1) and log(Total Debt), State, Size, Industry Classification and Urban in columns (2)-(4). 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	Single Lender			
	Treatment Borrowers w/Rating = 1 in the Pre-Period			
Dependent Vaiable	Log(Total Debt)	Log(#Banks)	% Local	% Foreign
	(1)	(2)	(3)	(4)
Downgraded × Post	0.055	-0.130*	0.021**	-0.017*
	(0.176)	(0.070)	(0.010)	(0.009)
Borrower Controls	Yes	Yes	Yes	Yes
Borrower Fixed Effects	Yes	Yes	No	No
Industry-Time Fixed Effects	Yes	Yes	Yes	Yes
No. Observations	2,092	2,092	2,092	2,092
R-Sq.	0.57	0.77	0.98	0.96

Internet Appendix:
Information Sharing and Rating Manipulation

Mariassunta Giannetti, José Liberti, and Jason Sturgess

Figure IA.1
Within-Borrower Standard Deviation in Credit Ratings

The figure presents the average within-borrower standard deviation of credit ratings for borrowers with multiple lending relationships. The standard deviation of credit ratings is estimated for each borrower in each month across lenders, and then this within-borrower standard deviation is averaged across borrowers. 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 the three months after information about the borrowers has been made public (post-expansion period). The figures compare the standard deviation of credit ratings of the pre and interim periods, when information was private, with the post-period, when information was public, for treatment and control borrowers. 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).
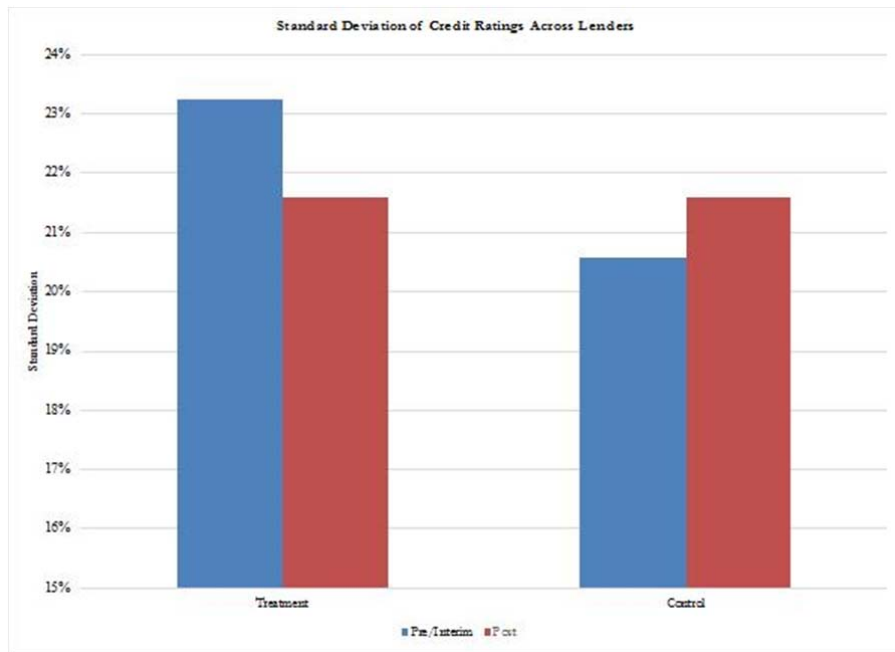
Figure IA.2

Upgrades and Downgrades Around the Credit Reform Expansion: Large Domestic Banks

These figures show the fraction of borrowers with a Rating = 2 for single lending relationship borrowers and multiple lending relationship borrowers, respectively, considering lending relationships with large domestic banks only. 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). We track the bank-borrower relationships that existed as of January 1998 for borrowers that did not become delinquent or default in the pre, interim, and 3-month post period. The unit of observation is at the borrower-bank-month level.
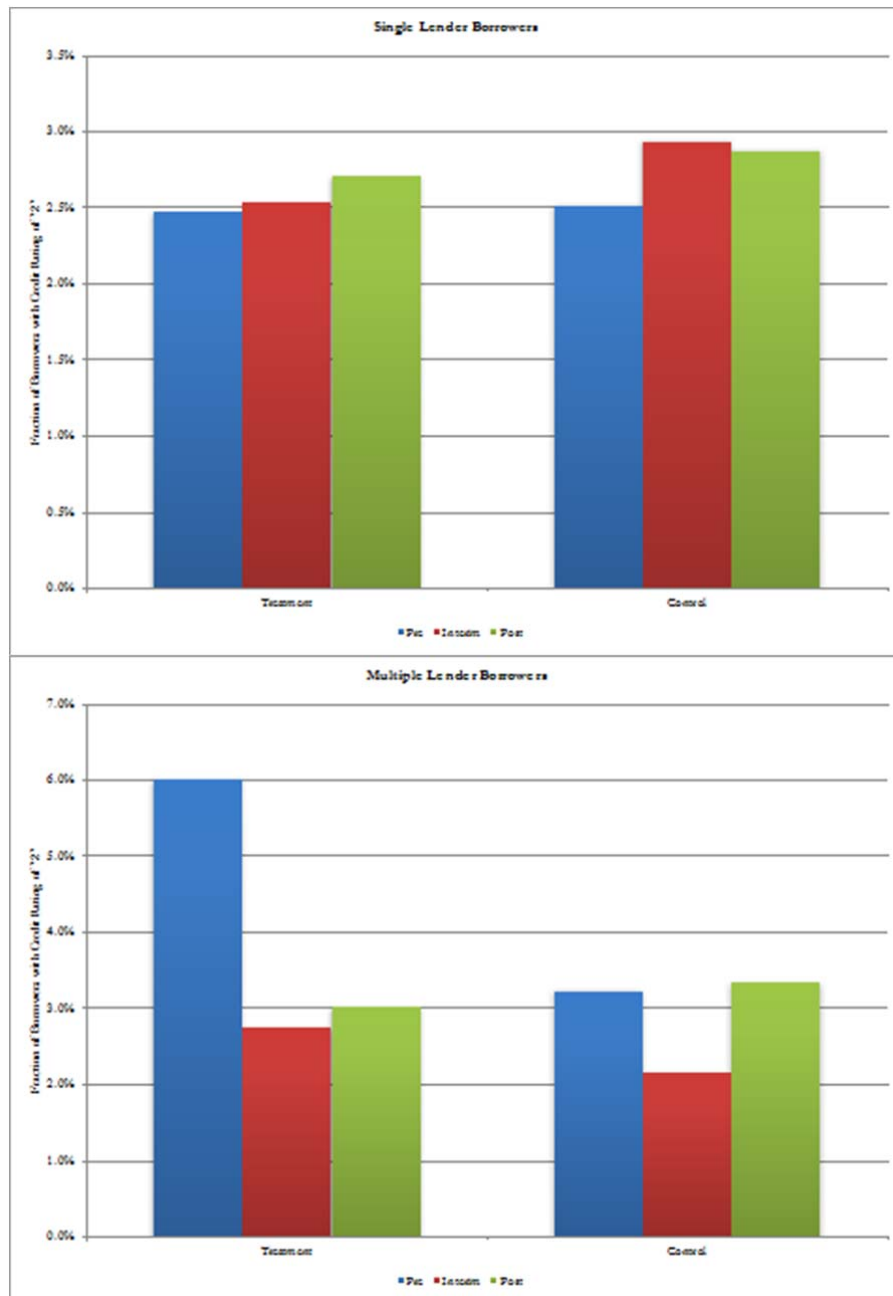
Figure IA.3
Upgrades and Downgrades Around the Credit Reform Expansion: Foreign Banks

These figures show the fraction of borrowers with a Rating = 2 for single lending relationship borrowers and multiple lending relationship borrowers, respectively, considering lending relationships with foreign banks only. 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). There are not treated borrowers with exclusive single lending relationships with foreign banks. We track the bank-borrower relationships that existed as of January 1998 for borrowers that did not become delinquent or default in the pre, interim, and 3-month post period. The unit of observation is at the borrower-bank-month level.
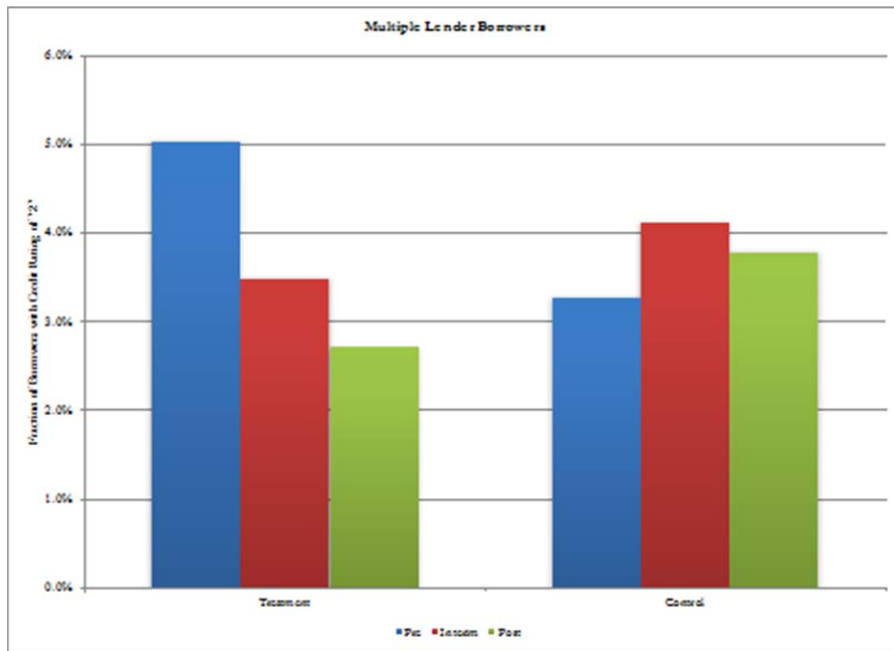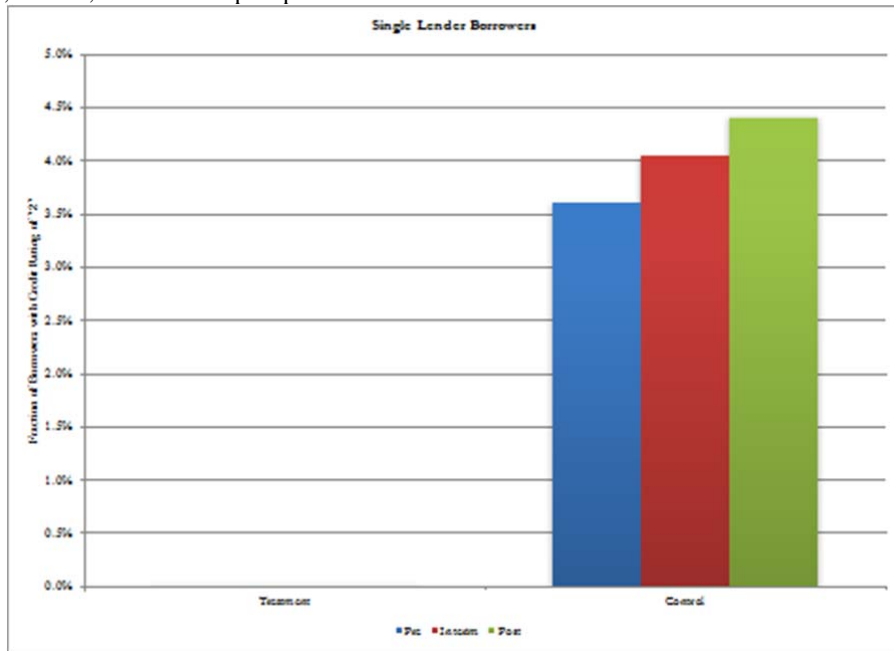
Table IA.1
Information Sharing and Convergence in Credit Ratings

This table examines the convergence of credit ratings in the post-reform period. 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 a single (multiple) lending relationship in the pre-period, in columns (1) and (2) ((3) to (6)). Tests examining the single lending relationship examine new relationships; tests examining the multiple lending relationship sample examine borrowers with at least two relationships in the pre-period and borrowers that are not in default. The dependent variable, Credit Rating, is the credit rating for borrower i from bank b in month t. Mean Other Credit Rating is the mean of the observed credit ratings from all other banks $-b$ lending to borrower i in month $t-1$. The unit of observation is at the borrower-bank-month level. Borrower controls include the log(Debt), State, Size, and Urban. Standard errors are clustered at the borrower level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable	Credit Rating (t)					
Sample	Single		Multiple			
	(1)	(2)	(3)	(4)	(5)	(6)
Credit Rating (t-1)			0.388***		0.385***	0.385***
			(0.058)		(0.059)	(0.059)
Mean Other Credit Rating (t-1)	0.101*	0.144**		0.052***	0.033***	0.038***
	(0.057)	(0.070)		(0.007)	(0.006)	(0.012)
Mean Other Credit Rating (t-1) × Treated		-0.421***				-0.037**
		(0.131)				(0.017)
Mean Other Credit Rating (t-1) × Post × Treated		0.354**				0.034*
		(0.158)				(0.019)
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Controls	Yes	Yes	No	No	No	No
Time Fixed Effects	Yes	Yes	No	No	No	No
Bank-Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Industry-Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Observations	404	404	29,921	29,921	29,921	29,921
R-Sq.	0.17	0.20	0.33	0.09	0.40	0.40

Table IA.2
Replicating the Main Results: Clustering at Bank Level

This table replicates the main results from Table 2 (Strategic Downgrades of High Quality Borrowers) and Table 9 (Strategic Upgrades of Low Quality Borrowers by Multiple Lenders) clustering at the bank level, rather than at the borrower level. The table presents the difference-in-differences effect of the credit registry expansion announcement (interim period) on downgrade (upgrade) hazard rates in columns (1) and (2) ((3) and (4)), 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) and (2); Upgrade, is from Rating = 2 to Rating =1 in columns (3) and (4). The sample is all treatment and control borrowers with a Rating = 1 in the pre-period, and with a single lending relationship in the pre-period, in columns (1) and (2). The sample is all treatment and control borrower-bank relationships with a Rating = 1 in the pre-period for borrowers with multiple lending relationships in the pre-period, in columns (3) and (4). Local is an indicator variable equal to one if the lender is a local bank. The unit of observation is at the borrower-bank-month level. Borrower controls include the log(Debt), State, Size, Industry Classification and Urban. Standard errors are clustered at the bank level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable	Downgrades From 1 to 2		Upgrades From 2 to 1	
Sample Lenders	Single		Multiple	
Period	All			
	(1)	(2)	(3)	(4)
Treated	0.002	0.000		
	(0.007)	(0.004)		
Treated × Interim	0.024***	0.002	0.147**	0.440***
	(0.006)	(0.005)	(0.067)	(0.131)
Treated × Interim × Local		0.037***		-0.386***
		(0.009)		(0.143)
Borrower Controls	Yes	Yes	Yes	Yes
Borrower Fixed Effects	No	No	Yes	Yes
Bank-Time Fixed Effects	Yes	Yes	Yes	Yes
Industry-Time Fixed Effects	Yes	Yes	Yes	Yes
No. Observations	9,558	9,558	2,365	2,365
R-Sq.	0.16	0.16	0.66	0.66

Table IA.3
Evolution of Defaults Over Time

This table examines the evolution of defaults in the 3-month period after the credit expansion announcement (post-period) relative to the interim period. 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 a single (multiple) lending relationship in the pre-period, in columns (1) to (3) and columns (4) to (6), respectively. The dependent variable, Default, is an indicator variable equal to one if the borrower is in default, i.e.: has a rating greater than or equal to 3. The unit of observation is at the borrower-bank-month level. Bank controls include log(Total Assets), Return on Assets, Capitalization (%), log(Number of Branches), Non-Performing Loans-to-Total Assets, Deposits-to-Total Assets and Growth on Total Assets, while borrower controls include the log(Debt), State, Size, and Urban. Standard errors are clustered at the borrower level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Sample Lender	Single			Multiple		
	Default			Default		
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.021**	0.018**		0.006	0.006	
	(0.009)	(0.009)		(0.004)	(0.004)	
Treated		-0.013	-0.010		-0.004	-0.004
		(0.016)	(0.016)		(0.008)	(0.008)
Treated × Post		0.018	0.011		-0.001	-0.002
		(0.020)	(0.020)		(0.009)	(0.010)
Bank Controls	Yes	Yes	No	Yes	Yes	No
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	No	Yes	Yes	No
Industry Fixed Effects	Yes	Yes	No	Yes	Yes	No
Bank-Time Fixed Effects	No	No	Yes	No	No	Yes
Industry-Time Fixed Effects	No	No	Yes	No	No	Yes
Observations	4,041	4,041	4,041	12,051	12,051	12,051
R-Sq.	0.18	0.18	0.22	0.06	0.06	0.09

Table IA.4
Borrower's Specialization: Agriculture

This table presents the difference-in-differences effect of the credit registry expansion announcement (interim period) on downgrade and upgrade hazard rates in columns (1) and (2), respectively, estimated using OLS and controlling for borrowers' specialization in agriculture. 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 column (1), and Upgrade, is from Rating = 2 to Rating =1 in column (2). The sample is all treatment and control borrowers with a Rating = 1 in the pre-period, and with a single lending relationship in the pre-period, in column (1). The sample is all treatment and control borrower-bank relationships with a Rating = 1 in the pre-period for borrowers with multiple lending relationships in the pre-period, in column (2). Agriculture is an indicator variable equal to one if the borrower is in the agriculture business as per the Industry Classification. The unit of observation is at the borrower-bank-month level. Borrower controls include the log(Debt), State, Size, and Urban. 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	Upgrades From 2 to 1
Sample Lender	Single	Multiple
	(1)	(2)
Treated	0.006	
	(0.005)	
Treated × Interim	0.021*	0.173**
	(0.013)	(0.082)
Treated × Agriculture × Interim	0.009	-0.058
	(0.025)	(0.127)
Borrower Controls	Yes	Yes
Borrower Fixed Effects	No	Yes
Bank-Time Fixed Effects	Yes	Yes
Industry-Time Fixed Effects	Yes	Yes
No. Observations	9,558	2,365
R-Sq.	0.16	0.66

Table IA.5
Differential Defaults of Downgraded and Non-Downgraded Treated Borrowers

This table presents the difference-in-differences effect of downgrades of treatment borrowers in the interim period on default hazard rates. 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 and a Rating = 1 in the pre-period. The sum of the coefficients *Downgraded* and *Treated × Downgraded* along with the p-value of the Wald test F-Statistic for the hypothesis that the sum of the coefficients is zero are presented in the bottom two rows. In both columns (1) and (2) we cannot reject the hypothesis that the effect of downgrades of treatment borrowers in the interim period on future default is zero. The unit of observation is at the borrower-bank-month level. Borrower controls include the log(Debt), State, Size, and Urban. Standard errors are clustered at the borrower level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable	Default	
Sample =	Single Lender	
	(1)	(2)
Default Horizon	6 Mths	12 Mths
Treated	-0.021	-0.019
	(0.013)	(0.018)
Downgraded	0.134**	0.212**
	(0.059)	(0.090)
Treated × Downgraded	-0.108*	-0.144*
	(0.063)	(0.080)
Borrower Controls	Yes	Yes
Bank-Time Fixed Effects	Yes	Yes
Industry-Time Fixed Effects	Yes	Yes
No. Observations	6,477	3,226
R-Sq.	0.26	0.24
Downgraded + Treated × Downgraded	0.03	0.07
p-value	0.64	0.20

Table IA.6
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-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 × Downgrade and Treated × Interim, in column (3). Standard errors are clustered at the borrower level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Dependent Variable	Log(Debt)		
Sample Lenders	Single		
	(1)	(2)	(3)
Treated × Interim	0.027	0.015	0.026
	(0.042)	(0.021)	(0.043)
Treated × Post		-0.016	
		(0.064)	
Treated × Interim × Downgrade			0.001
			(0.093)
Borrower Fixed Effects	Yes	Yes	Yes
Bank-Time Fixed Effects	Yes	Yes	Yes
Industry-Time Fixed Effects	Yes	Yes	Yes
No. Observations	11,698	11,698	11,698
R-Sq.	0.49	0.65	0.65

Table IA.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-period. The unit of observation is at the borrower-bank-month level. We omit from reporting the coefficient estimates of Treated × Upgrade and Interim × Upgrade in column (3). 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) | | |
| Sample Lenders | Multiple | | |
	(1)	(2)	(3)
Treated × Interim	0.019	-0.004	0.038
	(0.030)	(0.033)	(0.028)
Treated × Post		-0.030	
		(0.050)	
Treated × Interim × Upgrade			0.079
			(0.257)
Borrower Fixed Effects	Yes	Yes	Yes
Bank-Time Fixed Effects	Yes	Yes	Yes
Industry-Time Fixed Effects	Yes	Yes	No
No. Observations	35,193	35,193	35,193
R-Sq.	0.36	0.36	0.36