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Benign Neglect of Covenant Violations: Blissful Banking or Ignorant Monitoring?*

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Abstract

Theoretically, bank's loan monitoring activity hinges critically on its capitalization. To proxy for monitoring intensity, we use changes in borrowers' investment following loan covenant violations, when creditors can intervene in the governance of the firm. Exploiting granular bank-firm relationships observed in the syndicated loan market, we document substantial heterogeneity in monitoring across banks and through time. Better capitalized banks are more lenient monitors that intervene less with covenant violators. Importantly, this hands-off approach is associated with improved borrowers' performance. Beyond enhancing financial resilience, regulation that requires banks to hold more capital may thus also mitigate the tightening of credit terms when firms experience shocks.

JEL Classification: G21, G32, G33, G34

Keywords: Bank Monitoring, Covenant Violations, Syndicated Loans, Business Cycle

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1 Introduction

Loan monitoring is the activity that qualifies banks as information producers and thus informed lenders. Several studies have explored empirically the determinants of bank monitoring, ranging from loan characteristics to business cycle conditions (Cerqueiro, Ongena, and Roszbach, 2016; Gustafson, Ivanov, and Meisenzahl, 2017; Becker, Bos, and Roszbach, 2018). Yet, relatively little attention has been devoted to the role of bank funding structure, a prominent supply-side determinant of monitoring identified by existing theories (e.g., Holmstrom and Tirole, 1997; Diamond and Rajan, 2001).

We fill this gap by using the US syndicated loan market as a laboratory. This market is a primary source of funding for US corporations, with a volume of $2.4 trillion in 2017 (Sufi, 2007).[1] Against the backdrop of pervasive reforms pertaining to capital and liquidity regulation (Hancock and Dewatripont, 2018), our primary interest is to relate monitoring intensity to bank funding structure measures in general and the role played by regulatory capital in particular.

To measure bank monitoring, we build on Chava and Roberts (2008) and employ a granular dataset that links syndicate banks to US corporations over the period 1994-2012. Chava and Roberts (2008) show evidence of creditors' intervention in borrowing firms' management following covenant violations, as witnessed by the investment cuts experienced by those firms. Covenant violations provide a useful setting to study bank monitoring because they trigger a transfer of control rights from shareholders to creditors, who can then play a more active role in the firm.

We document substantial cross-sectional and time-series variation in bank monitoring and find that risk-adjusted Tier 1 capital ratios exhibit a statistically significant and large relation with this bank monitoring metric. Better capitalized banks adhere to a more lenient monitoring stance towards troubled borrowers, which is also associated with improved borrower performance. Rather than being inefficiently distracted, well-capitalized banks appear to permit borrowers the pursuit of value-increasing projects also when they violate a covenant. The result that better capitalized banks pursue more of a "hands-off" approach to monitoring after covenant violations contradicts the argument that equity favors monitoring by giving bankers more "skin in the game". Instead, larger equity buffers seem to permit banks to smooth negative shocks experienced by borrowers and allow them not to constrain corporate investment policy. The improved borrowers'

[1] See https://www.reuters.com/article/us-uslending-records/u-s-syndicated-lending-topples-records-in-2017-idUSKBN1ED2NO.

performance points, in turn, to an efficiency-enhancing role of equity capital rather than to a lender distraction story.

To support a causal interpretation of this result, we exploit a quasi-experiment that provides plausibly exogenous variation in bank equity capital. The Supervisory Capital Assessment Program (SCAP) of 2009, i.e., the US stress test, forced a number of banks to issue equity immediately after the publication of results. We use this equity issuance episode as a positive unanticipated shock to bank capitalization. We find that the increase in equity induced banks to keep a looser monitoring stance in the years after the stress test, in line with the role of regulatory equity to "buffer" shocks and allow a benign treatment of borrowers that violate covenants.

By contrast, our empirical results do not support theories predicting that larger exposures to creditor runs induces bankers to exert more monitoring effort. Banks with a more fragile debt structure, i.e., characterized by a higher reliance on deposit or short-term funding, do not monitor their borrowers significantly more intensely after covenant violations. To investigate potential causal interpretations about the relationship between debt structure fragility and monitoring intensity, we consider changes to the coverage of deposit insurance schemes around the world. We argue that reforms that increase deposit insurance coverage represent negative country-level shocks to banks' exposures towards depositor runs, because such policies lower monitoring incentives by reducing the disciplining effect of the threat of bank runs. In line with the baseline correlation analysis, we do not find any evidence in support of this hypothesis.

We conclude that, in the context of covenant violations, well-capitalized banks are more patient monitors that are less likely to impose inefficient investment cuts on borrowers. This result complements existing theories (e.g., Holmstrom and Tirole, 1997), which focus on bankruptcy rather than on covenant violations (i.e., technical defaults). In contrast to bankrupt firms, covenant violators appear to be sufficiently healthy to survive certain shocks, which increases the probability that heavy-handed creditor interventions after violations turn out to be value-destroying.

This paper contributes to three strands of the literature. First, it relates to a wide array of studies on the effect of covenant violations on corporate policies, such as, among others, investment (Chava and Roberts, 2008), financing (Roberts and Sufi, 2009), governance (Nini, Smith, and Sufi, 2012), employment (Falato and Liang, 2016) and board structure (Ferreira, Ferreira, and Mariano, 2018).[2] We study (bank) heterogeneity in creditor-induced investment reactions to covenant violations, which we use as a measure

[2]See Ferreira et al. (2018) for a recent overview of this literature.

of bank monitoring intensity.[3]

Second, our work relates to empirical studies linking heterogeneity in bank monitoring to syndicate structure (Sufi, 2007), collateral values (Cerqueiro et al., 2016), securitization (Wang and Xia, 2014), and business cycle conditions (Becker et al., 2018).[4] Related to our study, Gustafson et al. (2017) use confidential regulatory syndicated loan data from the Shared National Credit Program (SNC) to show that higher lead arranger shares, shorter loan maturities, private borrowers, and a smaller number of covenants lead to higher monitoring effort. By contrast, using an expanded version of SNC data, Plosser and Santos (2016) find that a bank's role in the syndicate does not affect monitoring intensity. According to them, what determines monitoring effort is the economic exposure of a bank, i.e., the absolute value of a bank's individual loan share relative to a bank's size. We contribute to this literature by exploring the role of banks' funding structure for monitoring heterogeneity.

Finally, our paper fits in the literature that links observable financial health indicators of lenders to borrower actions. The studies most closely related to ours are Chodorow-Reich and Falato (2018) and Acharya, Almeida, Ippolito, and Perez-Orive (2016a). Both use changes in bank balance sheet characteristics during the financial crisis to explain heterogeneity in bank responses to covenant violations. Using SNC data, Chodorow-Reich and Falato (2018) show that during the financial crisis lenders used covenant violations as an opportunity to cut credit exposure that otherwise would have been hard to reduce given loans' high average maturity. Acharya et al. (2016a) corroborate the findings of Chodorow-Reich and Falato (2018) using publicly available data on credit lines. These two studies examine one extreme of the whole spectrum of monitoring that we are considering. During a crisis, distressed banks may be less interested in intervening in the borrowing firms' management but rather want to implement lump-sum cuts in their loan book. Our study tests whether bank funding structure explains differences in monitoring looking over the entire business cycle.

2 Theoretical background

The theoretical literature has analyzed how monitoring effort relates to the bank's funding structure and business cycle conditions.

The funding structure of a bank pertains to the relative weight of equity and debt

[3] Roberts (2015) looks at heterogeneity in renegotiation outcomes following violations and relates it to *aggregate* banking sector leverage.

[4] See Gustafson et al. (2017) for an overview of the literature.

in its capital structure, and to its debt composition. There are several theoretical papers suggesting that bank equity capital might induce bank heterogeneity in monitoring (Holmstrom and Tirole, 1997; Coval and Thakor, 2005; Meh and Moran, 2010; Allen, Carletti, and Marquez, 2011; Mehran and Thakor, 2011; Jayaraman and Thakor, 2014). Schwert (2018) calls this the "equity monitoring hypothesis". Essentially, these studies argue that bank capital alleviates the moral hazard problem between the managers of the bank, and its investors. Bank capital raises managers' "skin in the game", thus incentivizing them to screen and monitor borrowers more diligently.

A different view on the role of equity, which we label "equity buffer hypothesis", is that equity reduces the bank's incentives to monitor and intervene in the governance of the borrowing firm. The intuition is that less capitalized banks may face *binding* increased capital charges if borrowers become troubled and have thus an incentive to monitor them closely. By contrast, a well-capitalized bank may not need to restrict borrowers' action set through monitoring, because it has a large enough equity cushion to absorb increased capital requirements. Although we are not aware of formal theories formulating exactly this prediction, a similar conjecture is put forward by Chava and Roberts (2008).

Another important facet of a bank's funding structure is the composition of its debt. Existing theories focus on the distinction between deposits and other forms of debt. For instance, Calomiris and Kahn (1991) and Diamond and Rajan (2001) argue that bank fragility, i.e., the threat of bank runs by depositors, disciplines bankers. In our context, this would suggest that banks highly reliant on deposits would have more incentives to monitor (this is named the "fragility monitoring hypothesis" by Schwert, 2018).[5] Whereas deposits are nowadays to a large extent insured and thus less exposed to bank runs, the same economic mechanism may be at work for banks highly exposed to rollover risk (for instance, on the repo market).[6]

It is also possible that bank monitoring intensity varies with macroeconomic conditions. A strand of the literature studies fluctuations in banks' credit standards through the business cycle. Ruckes (2004) argues that banks have less incentives to screen borrowers in upturns because the pool of loan applications is of high quality. The reverse argument holds in downturns. Mariathasan and Zhuk (2018) develop a similar argument in a rational inattention framework where loan officers' time to spend on each loan is lim-

[5] Acharya, Mehran, and Thakor (2016b) consider both the bright (loan monitoring) and the dark side (risk-shifting) of debt for banks, concluding that this can lead to multiple equilibria.

[6] In Huang and Ratnovski (2011), banks rely on short-term wholesale funding and retail deposits. They show that wholesale financiers have reduced incentives to monitor the bank. The implications for the monitoring of borrowers by the bank remain unclear though.

ited.[7] Martinez-Miera and Repullo (2017) show how monitoring incentives differ between booms and busts due to fluctuations in real interest rates and the aggregate supply of savings. The state of the business cycle, besides being important per se, can also interact with the bank's funding structure in shaping monitoring incentives. For instance, a bank may take advantage of its equity capital buffer exactly in recessions and be able to exert effective monitoring even during those periods.

While not the subject of this study, a number of theoretical studies show the importance of the bank's business model of a bank for its monitoring activity. The business model of a bank speaks to the mix of services it provides and to its technology (or more generally, its efficiency). The range of services offered by a bank has the potential to affect its monitoring activity through a diversion of resources to other, potentially more profitable business segments than traditional lending. Such a "distraction" argument can be found in the literature on rational inattention (Sims, 2003), with applications also to the case of bank monitoring (Mariathasan and Zhuk, 2018).[8]

A key technological development in the banking industry during the last three decades is the emergence of the originate-to-distribute business model, which substantially affected the servicing of loans. For instance, Parlour and Plantin (2008) argue that the presence of a secondary loan market may reduce banks' incentives to monitor. Parlour and Winton (2013) develop a framework in which banks can use either loan sales or credit derivatives to manage credit risk, which can instead lead to excessive monitoring over riskier loans. More generally, bank technology and efficiency have important implications for monitoring. In a costly-state verification framework, Greenwood, Sanchez, and Wang (2010) model a bank's monitoring as a function of its technology and the resources allocated to it. Monitoring effectiveness increases in both quantities.

Therefore, we specify below empirical proxies to gauge the differences in banks' business models as a confounding factor and enhance the isolation of funding structure effects on monitoring intensity.

3 Empirical approach

We explain the economic intuition why and the empirical methods how we measure monitoring intensity in the context of covenant violations before relating it to bank traits.

[7]Whereas Ruckes (2004) and Mariathasan and Zhuk (2018) study loan screening, it seems natural to extend the argument to monitoring effort.

[8]As pointed out above, however, the focus of Mariathasan and Zhuk (2018) is on time-varying bank attention throughout the business cycle.

3.1 Bank monitoring and covenant violations

The main goal of our analysis is to study how a bank's monitoring effort correlates with its characteristics, insulating their role from that of the borrowing firm's characteristics. At the same time, we aim to study whether and how bank characteristics interact with the macroeconomic environment in shaping bank monitoring effort.

Bank monitoring activity is inherently elusive. Most studies therefore measure it indirectly, assuming that certain features of the bank-borrower relationship (e.g., closer geographical distance or loan concentration among syndicate members) are conducive to more intense monitoring (see, e.g., Sufi, 2007). Another strand of the literature builds bank-level proxies for monitoring activity based on salary expenses and loan portfolio characteristics (Coleman, Esho, and Sharpe, 2006; Bhat and Desai, 2017). Other, more recent studies take a different approach and look at observable monitoring activities. Gustafson et al. (2017) are among the first to directly measure monitoring over syndicated loans from SNC by looking at banks' meetings with borrowers and on-site visits as well as at the frequency of information requests to the borrower (e.g., financial statements). Similarly, using confidential data from one large Swedish bank, Cerqueiro et al. (2016) and Becker et al. (2018) measure monitoring by looking at the bank's frequency of reviews of borrowers or collateral. Ono and Uesugi (2009) follows a similar approach using Japanese business loan data. Plosser and Santos (2016) infer monitoring activity from changes to banks' internal ratings of borrowers using SNC syndicated loan data.

These approaches focus either on specific loan characteristics linked to monitoring effort (e.g., the lead bank's share in syndicated loans) or on specific monitoring actions (e.g., collateral reviews). We follow a different route and reverse engineer banks' monitoring intensity starting from the effect of their actions on borrowing firms' policies. A main challenge is to impute changes in borrowing firms' policies to banks' monitoring actions. Our approach is to consider events when banks are likely to take monitoring actions. In line with Bird, Ertan, Karolyi, and Ruchti (2017), we use changes in borrowing firms' investment policy around violations of financial covenants contained in syndicated loan contracts as a proxy for banks' monitoring intensity.

Financial covenants set limits on accounting-based measures of financial health and performance (e.g., on net worth or current ratio) of borrowing firms.[9] In loan contracts,

[9]Loan contracts may include also affirmative covenants (requiring the borrower to take specific actions,

these covenants are commonly maintenance-based, i.e., the borrowing firm must comply with the limits set in the loan contract at the end of each fiscal quarter (Nini et al., 2012). A covenant violation constitutes a technical default. Upon such an event, the creditors can impose the immediate repayment (acceleration) or the termination of the loan. In most cases, creditors use the threat of such actions to renegotiate the debt contract and extract concessions from borrowers (Roberts, 2015). These concessions typically pertain to loan terms and, most importantly for our purposes, to lenders' monitoring intensity.

According to the theoretical work by Gorton and Kahn (2000) and Berlin and Mester (1992), monitoring entails renegotiating loan terms upon the arrival of new information about the firm's prospects. In their models, covenants and their violation are a mechanism to institutionalize regular renegotiations. After a violation, a lender can choose to liquidate certain projects of the borrower to prevent risk-taking. This is exactly what we are measuring in the form of restrictions on firm investment. More broadly, Nikolaev (2018) defines monitoring as both acquiring timely information about borrowers and acting upon that information to exert control on management. While monitoring measures such as loan reviews (Plosser and Santos, 2016), site visits, and borrower meetings (Gustafson et al., 2017) entail only the first part of that definition, our measure incorporates both parts since the lender has to acquire information to detect the violation.

Chava and Roberts (2008) and Nini et al. (2012) provide both anecdotal and large sample evidence consistent with increased monitoring following covenant violations (e.g., through increased frequency of required compliance reports). In line with an increase in monitoring, Ferreira et al. (2018) show that the number of independent directors serving on the board of borrowing firms increases following violations, and most of them have ties with lending banks. Whereas the change in investment policy linked to the resolution of the technical default can reflect a host of bank-side actions, it seems sensible to think that such actions capture also "pure" monitoring.

All in all, covenant violation provide a useful setting to study banks' monitoring activity for mainly three reasons. First, they give a specific channel through which creditors can intervene in the governance of the borrowing firm, namely a formal transfer of control rights from shareholders to creditors. Second, covenant violations are widespread and involve also relatively healthy firms, thus allowing the researcher to have a more complete picture of the role of creditors in borrowing firms (Nini et al., 2012). Third,

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such as complying with certain regulations) and negative covenants (prohibiting the borrower from taking certain actions, such as asset sales). Nini et al. (2012) provides further institutional details about loan covenants. We consider only financial covenants and refer to them for brevity as covenants.

the management of borrowing firms' only has limited ability (and incentives) to manipulate the firm's accounting ratios to avoid covenant violations (Roberts and Sufi, 2009). This and the discrete nature of covenant violation around the covenant threshold lend themselves to a regression discontinuity design (RDD), commonly used in the literature starting from Chava and Roberts (2008), which we discuss below more in detail.

Our approach complements Gustafson et al. (2017), who measure monitoring more directly and study correlations with future loan outcomes, including covenant violations. Our measure of monitoring – or more generally, of bank actions – may be more indirect, but we provide plausibly causal estimates of monitoring effects on borrowing firms.

3.2 Investment and covenant violations

As a preparatory analysis, we study the behavior of violating firms' investment around covenant violations without conditioning on the lender. The goal is to link our core analysis on observable differences in bank funding structure described below to the contraction in investment commonly observed in the literature (Chava and Roberts, 2008).

The borrowing firm's treatment status (violating vs. non-violating) exhibits a discontinuity with respect to the distance between the observed accounting ratio and the contractual covenant threshold. Such a discontinuity can be exploited for identification purposes in a RDD to isolate the effect of financing frictions on investment. We first implement a RDD at the firm-quarter level in the spirit of Chava and Roberts (2008) specified as follows:

$$I_{f,q} = \alpha \cdot v_{f,q-1} + \boldsymbol{\eta} \mathbf{x}_{f,q-1} + \boldsymbol{\zeta} \mathbf{p}_{f,q-1} + \gamma_f + \gamma_q + \epsilon_{f,q}, \tag{1}$$

where f and q denote the borrowing firm and the (quarterly) period, respectively. $I_{f,q}$ is the firm's investment rate. The treatment variable is the firm-quarter-level covenant violation indicator $v_{f,q-1}$ defined as

$$v_{f,q-1} = \begin{cases} 1 & \text{if } z_{f,q-1} - z^0_{f,q-1} < 0 \text{ for any covenant in loans of firm } f \\ 0 & \text{otherwise,} \end{cases} \tag{2}$$

where $z_{f,q-1}$ is the observed value of the accounting measure restricted by the covenant and $z^0_{f,q-1}$ is the most binding covenant threshold contained in any of the firm's outstanding syndicated loan contracts.[10] In this firm-quarter-level analysis, $v_{f,q-1}$ is equal to one

[10] We focus on covenants on (tangible) net worth or the current ratio as in Chava and Roberts (2008).

if the firm violates any covenant in any of the outstanding loans. For a given accounting measure, the *relative* distance $(z_{f,q-1} - z^0_{f,q-1})/z^0_{f,q-1}$ is defined with respect to the tightest covenant threshold across the different outstanding loans at a given point in time.

We control for a vector of covariates $\mathbf{x}_{f,q-1}$ including Tobin's q, the contemporaneous cash flow, and the natural logarithm of total assets of the borrowing firm. We also control for a vector of smooth functions $\mathbf{p}_{f,q-1}$ of the relative distance of the different accounting measures from the tightest covenant threshold. The inclusion of $\mathbf{p}_{f,q-1}$ improves the identification of the treatment effect α around the discontinuity and captures any information these distance measures may convey about the firm's growth prospects (Falato and Liang, 2016). We include firm (γ_f) and time (γ_q) fixed effects to absorb time-invariant differences in investment policy across borrowing firms and macroeconomic conditions, respectively. We allow for firm-level clustering in the error term $\epsilon_{f,q}$.

We repeat the analysis of investment around covenant violations, but treat each syndicated loan as a set of separate loans, one for each bank in the syndicate. The unit of observation is the loan-bank-firm-quarter, so that we can focus on the heterogeneity in investment responses depending on the bank from which the firm borrowed. We use this setting in our main analysis below and execute a RDD specified as follows:

$$I_{l,b,f,q} = \alpha \cdot v_{l,q-1} + \boldsymbol{\eta}\mathbf{x}_{f,q-1} + \boldsymbol{\zeta}\mathbf{p}_{l,q-1} + \gamma_{b,y} + \gamma_f + \gamma_q + \gamma_e + \epsilon_{l,b,f,q}, \tag{3}$$

where l, b, and y denote the syndicated loan deal, the lending bank, and the year, respectively. We add bank-year $(\gamma_{b,y})$ and fiscal quarter (γ_e) fixed effects to control for time-varying heterogeneity in investment across different banks' borrowers outside covenant violations and seasonality, respectively. The treatment variable is the loan-quarter-level covenant violation indicator $v_{l,q-1}$ defined as

$$v_{l,q-1} = \begin{cases} 1 & \text{if } z_{f,q-1} - z^0_{l,q-1} < 0 \text{ for any covenant in loan } l \\ 0 & \text{otherwise,} \end{cases} \tag{4}$$

where the difference relative to the firm-quarter-level indicator (2) lies in the covenant threshold $z^0_{l,q}$, which is now loan-specific.[11] In this setting, $v_{l,q-1}$ is equal to one if the firm violates any of the covenants contained in a given loan. Analogously to (1), we include a

[11] In other words, we do not need to focus on the tightest covenant in this setting. The time-subscript indicates the possibility that covenant thresholds are dynamic. Current ratio thresholds might increase over time and net worth thresholds might increase with net income. As in Chava and Roberts (2008), we linearly interpolate initial and final covenant thresholds over the life of the loan.

vector of smooth functions $\mathbf{p}_{l,q-1}$ of the relative distance between the different accounting measures and the loan-level covenant-threshold. Note that, as above, we only observe borrowing firms' investment at the firm-quarter-level and the notation $I_{l,b,f,q}$ reflects the repetitive nature of our data structure. Because of this feature, we use two-way clustering by bank and time in the error term $\epsilon_{l,b,f,q}$ in line with Schwert (2018).

In both specifications (1) and (3), the parameter α captures the treatment effect. The RDD allows us to identify the treatment effect as long the error terms ($\epsilon_{f,q}$ or $\epsilon_{l,b,f,q}$) do not exhibit the same discontinuity with respect to the threshold distance as the treatment variable (Falato and Liang, 2016).

We follow Chava and Roberts (2008) and estimate both specifications (1) and (3) without firms that never violate any covenant. However, we slightly deviate from Chava and Roberts (2008) in the definition of the sample of violating firms and of the violation indicator ($v_{f,q-1}$ or $v_{l,q-1}$). First, we remove loans for which the firm is in violation in all quarters of their lifetime.[12] Second, we do not consider covenant violations as events that happen right at the beginning of a loan's lifetime. This approach allows us to improve comparability in terms of covenant design within our sample of loans by excluding those loans that are characterized by very strict covenants. Third, once a firm violates a covenant for the first time for a given loan, we require at least four quarters without a violation before we code another breach as a "new violation" in the same spirit as Nini et al. (2012). In this way, we aim to capture instances in which there is an actual transfer of control rights from shareholders to creditors.

3.3 Heterogeneous effects of covenant violations across banks

The RDD specifications described so far do not capture heterogeneity across banks in borrowing firms' investment changes in the wake of covenant violations. We pursue two alternative approaches to augment specification (3) to study bank heterogeneity in terms of capitalization, funding structure, and business models.

[12]In our sample, 35.8% of all loans are violated at least once. Of these, roughly 18.5% (or 6.6% of our sample) are violated in all quarters of their lifetime.

3.3.1 Two-step approach

The first approach consists of two steps. First, we estimate the RDD specification:

$$I_{l,b,f,q} = \alpha \cdot v_{l,q-1} + \sum_b \sum_y \beta_{b,y} \cdot v_{l,q-1} \times \gamma_{b,y} \tag{5}$$

$$+ \boldsymbol{\eta}\mathbf{x}_{f,q-1} + \boldsymbol{\zeta}\mathbf{p}_{l,q-1} + \gamma_{b,y} + \gamma_f + \gamma_q + \gamma_e + \epsilon_{l,b,f,q}. \tag{6}$$

All the variables are defined as above. Relative to equation (3), equation (5) also interacts $v_{l,q-1}$ with bank-year fixed effects ($\gamma_{b,y}$).[13] We are interested in the set of parameters denoted as $\beta_{b,y}$, which gauge the time-varying component of bank-specific treatment effects of covenant violations on investment.

The estimated coefficients $\hat{\beta}_{b,y}$ constitute the dependent variable in the second step of the analysis. We study the relationship between $\hat{\beta}_{b,y}$ and and bank funding structure, controlling for several other characteristics related to the bank's business model. To this end, we estimate the following specification over a bank-year panel:

$$\hat{\beta}_{b,y} = \psi + \boldsymbol{\theta}\boldsymbol{\Gamma}_{b,y-1} + \upsilon_{b,y}, \tag{7}$$

where $\boldsymbol{\Gamma}_{b,y-1}$ is a vector of bank characteristics at annual frequency capturing funding structure through the level of equity capital (leverage ratio, risk-adjusted Tier 1 capital ratio) and debt composition (deposits and short-term funding), as well as the bank's business model through the scope of activities (non-interest income, trading activity, and bank size) and technology and efficiency (non-performing assets, net income, and cost-to-income ratio) of the bank. All variables in $\boldsymbol{\Gamma}_{b,y-1}$ are measured as of the last quarter of the year and lagged by one year. We first estimate univariate regressions for each of the bank characteristics contained in $\boldsymbol{\Gamma}_{b,y-1}$ and then a multivariate regression for the entire vector of covariates. In additional tests, we also interact $\boldsymbol{\Gamma}_{b,y-1}$ with measures of macroeconomic conditions to investigate how the role of different bank characteristics varies throughout the business cycle.

Whereas the first-step RDD estimates plausibly allow for causal inference on the (bank-time-specific) treatment effect of covenant violations on investment, the second step provides only correlations. Indeed, as pointed out by Chodorow-Reich and Falato

[13]Ideally, we would interact $v_{l,q-1}$ with bank-quarter fixed effects rather than bank-year fixed effects. Yet small banks experience only very few covenant violations in a specific quarter, so that their investment responses are too correlated with the investment responses of large players in the market. Therefore, we cannot estimate many bank-quarter-specific violation coefficients.

(2018) in a similar setting, to interpret $\mathbf{\Gamma}_{b,y-1}$ estimates causally, we would need to have "as good as random" matching between borrowers and banks. Unlike Chodorow-Reich and Falato (2018), we do not focus on the years around the Great Recession to achieve such a condition, thus we are left with arguably non-random matching (Schwert, 2018).

Our solution is to carry out two quasi-experiments within the second-step estimation. To test the theoretical implications of bank equity and funding fragility for monitoring intensity, we exploit plausibly exogenous shocks to (i) equity capital resulting from the US banks' assessment in the SCAP stress test of 2009 and (ii) exposure to bank runs following changes in the deposit insurance coverage around the world, respectively. These two experiments aim at verifying whether the baseline correlation analysis between bank monitoring and funding structure supports a causal interpretation. We provide more details in Section 6.1.3.

Two caveats concerning the two-step approach remain. First, whereas we cluster standard errors by bank in equation (7), the dependent variable $\hat{\beta}_{b,y}$ is generated, which may require further corrections of standard errors because of measurement error (Gawande, 1997; Feenstra and Hanson, 1999; Dumont, Rayp, Thas, and Willemé, 2005).[14] Assuming that the measurement error $(\hat{\beta}_{b,y} - \beta_{b,y})$ is uncorrelated with the error term $\upsilon_{b,y}$, the OLS estimator $\hat{\boldsymbol{\theta}}$ is consistent, but suffers from inflated standard errors, possibly leading to an under-rejection of the null hypothesis of non-significance (Roberts and Whited, 2013).[15]

Second, by construction the sample size in the second step is substantially smaller than in the second step, which limits statistical power and may entail an under-rejection of the null hypothesis of non-significance.

3.3.2 One-step approach

To address the shortcomings of the two-step approach, we also implement a one-step procedure which (i) does not suffer from the issues linked to generated variables, (ii) relies on the entire sample of observations. In particular, we estimate this RDD specification:

$$I_{l,b,f,q} = \alpha \cdot \upsilon_{l,q-1} + \boldsymbol{\theta} \cdot \upsilon_{l,q-1} \times \mathbf{\Gamma}_{b,q-1} + \boldsymbol{\eta}\mathbf{x}_{f,q-1} + \boldsymbol{\zeta}\mathbf{p}_{l,q-1} + \gamma_{b,q} + \gamma_f + \gamma_e + \epsilon_{l,b,f,q}, \quad (8)$$

where $\mathbf{\Gamma}_{b,q-1}$ is a vector of bank time-varying traits defined as in equation (7), but measured at quarterly frequency and $\gamma_{b,q}$ are bank-by-quarter fixed effects. We cluster standard errors by bank and time and are interested in the vector of coefficients $\boldsymbol{\theta}$.

[14] This case differs from that of generated regressors considered by Murphy and Topel (1985).

[15] With a slight abuse of notation, we denote both the OLS estimator and the actual estimate as $\hat{\beta}_{b,y}$.

The main disadvantage of this approach relative to the two-step procedure is that it directly assumes the same relationship between bank actions after technical defaults and $\mathbf{\Gamma}_{b,q-1}$ for all banks and periods in the sample. By contrast, in the two-step procedure we make this assumption only in the second step, whereas the first step allows us to capture also that part of bank heterogeneity in technical default that is not explained by the vector of bank characteristics $\mathbf{\Gamma}_{b,q-1}$.

4 Data

We describe data sources, sample selection, variable construction, and summary statistics.

4.1 Data and sample selection procedure

We use data on syndicated loans, borrowing firms, lending banks, and macroeconomic conditions. Syndicated loan data is from the Thomson-Reuters' Loan Pricing Corporation DealScan (Dealscan) database. We use quarterly accounting and stock price data about US public firms from the the Center for Research in Security Prices/Compustat merged (CCM) database, excluding financial institutions and utilities. We drop firm-quarters with missing information about sales, number of shares outstanding, stock price, and calendar date. We also drop firm-quarters for which net property, plant, and equipment (PPE) is below \$1M, for which leverage is zero, or for which the market (book) leverage lies outside of the unit interval. We match them to the syndicated loans using the link file provided by Michael Roberts, which builds on the sample of Chava and Roberts (2008).

We use bank quarterly balance sheet data from Compustat Banks, supplemented with Bankscope if information are missing.[16] We link syndicated loans to balance sheet data of lending banks using the Dealscan-Compustat Banks-Bankscope link file made available by Michael Schwert.[17] As a result, we focus on the 103 most active banks on the US syndicated loan market, of which only 87 are covered by Compustat Banks. Unlike most of the literature, we sample all syndicate members and not only lead banks. Finally, we retrieve data on US macroeconomic variables from the Federal Reserve Economic Data (FRED), St. Louis Federal Reserve Bank.

The sample starts in 1994, which is the first year when Dealscan provides sufficiently comprehensive information about covenants (Chava and Roberts, 2008). The sample

[16] We use Bankscope only for the 20 most active lenders with missing information (Citi, BNP Paribas, and National City) to avoid losing an important source of variation in monitoring.

[17] This link file is used in Schwert (2018) and available at https://sites.google.com/site/mwschwert/.

runs until 2012, which is the last year covered by the Dealscan-CCM link file of Michael Roberts. We focus on Dealscan loans containing covenants on (tangible) net worth or the current ratio and build a matched quarterly panel of firms, which are assumed to be subject to a given covenant up to the maturity date of the corresponding loan. We identify covenant violations by testing if the observed (tangible) net worth or current ratio complies with the contractual threshold.While this approach may result in some false positives, it allows us to measure the distance between the accounting quantity and the covenant threshold, which enhances identification in the RDD.[18]

We treat each syndicated loan as a number of separate loans to gauge heterogenous bank behavior, i.e., a loan deal of a given borrowing firm with n different banks enters as n separate bank-firm deals.[19] As in Schwert (2018), deal-bank-firm triplets are the panel unit of analysis. Put differently, we consider covenant violations on the deal-bank-firm-quarter level as opposed to firm-quarter level violations Chava and Roberts (2008) .

4.2 Variable construction and summary statistics

In our analysis, we rely on borrowing firm-level and bank-level time-varying characteristics. Concerning borrowing firms' variables, investment is defined as capital expenditures over last quarter's PPE. Tobin's q is defined as total assets minus book equity plus market capitalization scaled by total assets. Cash flow is defined as income before extraordinary items plus depreciation and amortization over last quarter's PPE. We use the natural logarithm of total assets as a proxy for firm size. Return on assets (ROA) is defined as income before extraordinary items scaled by total assets.

To explain variation in monitoring intensity, we employ a host of bank characteristics contained in the vector $\mathbf{\Gamma}_{b,y-1}$ of the second-step specification (7). The leverage ratio (common equity/assets) and the risk-adjusted Tier 1 capital ratio capture the bank's level of equity capital. Deposits-to-total assets and short-term funding-to-total assets speak to the composition of its debt. The natural logarithm of total assets (i.e., bank size), non-interest income over total revenue (i.e., the reliance on non-traditional banking services) and assets held for trading scaled by total assets (i.e., the involvement in trading activities) relate to the range of activities the bank operates in. To proxy for the monitoring technology and overall efficiency of the bank, we specify non-performing

[18]At the cost of unobserved distances from covenant thresholds, one may use alternatively violations reported in SEC filings, i.e., not only at those linked to Dealscan loans (Roberts and Sufi, 2009).

[19]We refer to a loan deal simply as a "deal", which is a package of facilities in Dealscan.

assets-to-total assets, net income-to-total assets, and the cost-to-income ratio. Table 1 provides the list of 51 banks for which all of these variables are available for at least one year and can thus be included in the sample for the second-step estimation. These 51 banks still capture a large fraction of the market, namely 57.3% of all deals (64.7% of the total credit) extended by our sample banks.[20]

Finally, we measure US macroeconomic conditions by using an indicator variable for National Bureau of Economic Research (NBER) recessions, the National Financial Conditions Index (NFCI), and the Chicago Fed National Activity Index (CFNAI).

Table 2 shows summary statistics for firm variables in and outside covenant violations (Panel A and Panel B, respectively), bank characteristics (Panel C) and selected deal characteristics (Panel D). As we would expect, covenant violating firms exhibit lower investment, cash flows, and ROA than other firms. They are also smaller and more levered. On average, the loan syndicates in our sample comprise 5.23 institutions. All firm and bank variables are winsorized at the 1st and 99th percentile. All monetary variables are expressed in millions of 2010 dollars. We provide detailed variable definitions in Appendix Table A.1.

5 Investment and covenant violations

As a building block for our subsequent tests on bank heterogeneity, it is important to verify that we obtain the well-known result of a reduction in investment due to covenant violations (Chava and Roberts, 2008; Nini et al., 2012).

The use of an RDD relies on the assumption that the running variable (i.e., the accounting ratio regulated by a covenant in our case) cannot be manipulated. This assumption is unlikely to be violated in our setting. As discussed extensively by Chava and Roberts (2008), lending relationships are valuable and firms are reluctant to risk their relationship and general reputation by manipulating their books. Nonetheless, in Appendix Figure A.1 we implement manipulation tests of the running variables based on the smooth local polynomial density estimator of Cattaneo, Jansson, and Ma (2017), who build on the approach of McCrary (2008). Reassuringly, we cannot reject the null hypothesis of no manipulation for any of the three accounting measures (net worth, tangible net worth, and current ratio). All figures clearly suggest that there is no discontinuity around the threshold (of zero relative distance).

[20] We calculate these figures on the facility-level following De Haas and Van Horen (2013) to assign deal shares among facility members.

Given this RDD validity check, Table 3 reports estimates of regression specifications studying the effect of covenant violations on borrowing firms' investment, without conditioning on the lending bank. In columns 1 and 2, we use the same firm-quarter data structure of Chava and Roberts (2008) and estimate equation (1). Reassuringly, we find a statistically significant reduction in investment linked to covenant violations. Column 1 focuses on the period 1994-2005 – the same used by Chava and Roberts (2008) – and the magnitude of the change in investment is consistently also the same, i.e., -0.8%.[21] In column 2, where we extend the analysis to the entire sample period 1994-2012, the magnitude of the effect is only slightly smaller.

In columns 3 and 4, we resort to our repetitive deal-bank-firm-quarter data structure and estimate equation (3). We still find a decline in investment following covenant violations, which is, however, statistically insignificant at conventional levels. The magnitude of the reduction over the deal-bank-firm-quarter data structure declines and ranges between -0.3% and -0.2%. This result is arguably a mechanic effect, which reflects that firms with multiple deals outstanding may be in violation of covenants for multiple deals at the same time. For instance, consider a firm with two deals outstanding (deal 1 and deal 2), both containing a covenant on the current ratio (with thresholds at 175% and 150%, respectively). Assume that the firm's current ratio goes down to 170% in period t, which violates deal 1's covenant. After t, the firm's current ratio continues declining and in period $t+2$ reaches 145%, which breaches also deal 2's covenant. The first transfer of control rights to creditors happens at time t, so that we are most likely to observe the sharpest reduction in investment between t and $t+1$, whereas the effect of the second violation between $t+2$ and $t+3$ is arguably milder. In addition, columns 3 and 4 include bank-year fixed effects, which may also absorb part of the effect of covenant violations.

The estimated unconditional effect of covenant violations may mask important heterogeneity in the course of action followed by different lenders. We thus augment equation (3) by interacting the covenant violation indicator with bank fixed effects. Figure 1, where we plot the estimated coefficients for such interaction terms, confirms the existence of substantial heterogeneity in borrowing firms' investment reactions across banks. Some banks, like Fleet Bank and Comerica, are significantly stricter than Bank of America (BoA), which is the most active lender in our sample and serves as the reference bank. Other banks, like Deutsche Bank, are significantly more lenient.

Given this prima facie evidence, we study next heterogenous investment effects across banks and time, i.e., our proxy for bank monitoring intensity.

[21]See column 7 of Table V (Panel A) of Chava and Roberts (2008).

6 Heterogeneous effects of covenant violations across banks

The granular deal-bank-firm-quarter data structure allows us to scrutinize heterogeneity in monitoring and its relationship with bank funding structure and business cycle conditions by using the two methods described above.

6.1 Two-step approach

The two-step approach consists of (i) a first step in which we isolate heterogeneous effects of covenant violations on investment across lending banks and time, and (ii) a second step in which we correlate these effects with bank funding structure and business cycle conditions (controlling for several other bank time-varying characteristics).

6.1.1 First step

To tease out bank-induced heterogeneity in borrowers' investment response to violations through time, we estimate specification (5) in column 1 of Table 4. In this way, we obtain a vector of bank-year-specific coefficients that capture (heterogeneous) monitoring effects, namely $\hat{\beta}_{b,y}$. These coefficients measure the difference in the violation effect relative to the reference group, namely deals by BoA in 2003.[22]

An F-test of joint significance rejects the null hypothesis that our monitoring effects $\hat{\beta}_{b,y}$ are equal to zero. In terms of economic significance, these effects exhibit an interquartile range of $0.0175 - (-0.0071) = 0.0246$, which is roughly $0.025/0.057 = 44\%$ of the mean investment rate in the regression sample. Thus, these simple tests suggest that bank heterogeneity in monitoring is both statistically and economically important.

Columns 2 and 3 repeat the same exercise, but specify ROA and Tobin's q as dependent variables, respectively. These specifications provide us with bank-year-specific effects of covenant violations on borrowing firms' accounting performance and market value: $\hat{\beta}^{ROA}_{b,y}$ and $\hat{\beta}^{q}_{b,y}$.[23] The F-tests corroborates the existence of an important degree of heterogeneity across bank-years. Below, we explore the correlation of $\hat{\beta}^{ROA}_{b,y}$ and $\hat{\beta}^{q}_{b,y}$ with our monitoring measure $\hat{\beta}_{b,y}$.

[22]We choose BoA as the reference bank, because it is the most active bank in terms of number and volume of deals (see Table 1), which renders it a well-populated benchmark. The reference year 2003 is the one with most observations in our sample. We do not report the coefficient estimate for the violation indicator in Table 4, because that would only provide information on reactions to covenants in the reference bank-year, which is devoid of interest per se.

[23]In column 3, we remove Morgan Stanley from the estimation sample because it produces an outlier in the bank-year effect on Tobin's q, which reduces the bank sample size from 90 to 89.

Given the large size of the vector $\hat{\beta}_{b,y}$ obtained from the specification shown in column 1 of Table 4, we provide a visual analysis in Figure 2 rather than tabulating all the bank-year monitoring coefficients. In total, we are able to estimate 640 coefficients.[24] The left graph of Figure 2 shows how the number of estimated coefficients is distributed in time. All in all, we do not obtain a balanced bank-year panel of monitoring coefficients for the second-step analysis. Indeed, several banks drop out of the sample early due to M&A activity: For instance, Bank One was purchased by JPMorgan (JPM) in 2004. Other banks only exhibit covenant violations as of the late 1990s.

The right graph of Figure 2 shows the empirical density of the bank monitoring coefficients. While the distribution peaks at 0%, we still observe a substantial degree of heterogeneity. Most coefficients lie roughly in the $[-5\%, +4\%]$ range. The heavy right tail is partially explained by the negative investment effect of -1% of covenants in our reference bank-year (BoA 2003). We obtain a very similar result (slightly shifted towards the left) when using the second most active lender in our sample (JPM) as the reference bank rather than BoA. Indeed, the correlation between the monitoring coefficients $\hat{\beta}_{b,y}$ estimated using BoA and JPM as the reference bank exhibit perfect correlation.

To further explore bank heterogeneity, in Figure 3 we visualize the distribution of the monitoring coefficients year-by-year through box plots. Heterogeneity across banks is not just an artefact of changes in business cycle conditions over our sample. The resulting variation in bank monitoring coefficients within each single year is substantial. Annual distributions reflect what we observe over the entire sample, i.e., a right-skewed distribution with a median slightly above zero. Nonetheless, time-series variation matters, as witnessed by fluctuations in both the central tendency (median) and dispersion (interquartile range) of our monitoring coefficients.

Overall, our first-step estimates point to a substantial degree of heterogeneity in banks' monitoring intensity following covenant violations.

6.1.2 Second step

We now link the heterogeneity in monitoring documented in the first step to banks' funding structure in general and capitalization in particular, which existing theories identify as important determinants of bank monitoring activities, conditional on observable bank

[24] Roughly 4% of all possible coefficients cannot be estimated due collinearity issues. We define these bank-years as missing and add them to the estimation sample for Table 5. Moreover, we drop those bank-years where there is at least one quarter in which the bank did not have any outstanding loans. Lastly, we drop the bank City National, a small regional bank, because it produces an outlier in 2000.

characteristics and business cycle conditions.

Sample selection. Before studying the correlation between the estimated monitoring coefficients and bank traits, we investigate those bank-years for which we are not able to estimate a coefficient.[25] Appendix Table A.2 lists those instances, which are clustered in the early sample years when Dealscan's coverage is more sparse.

The lack of a coefficient may signal statistical issues (e.g., for those banks with relatively few deals like Huntington National Bank and Bank of Hawaii, it is also relatively unlikely to observe a violation in a given firm-year that does not coincide with violations on larger banks' loans as well), but also deeper selection issues, especially concerning a bank's preferences in terms of covenant design (Murfin, 2012). Indeed, heterogeneity in banks' behavior in technical default may stem from heterogeneous monitoring incentives as well as from ex ante differences in the presence and tightness of covenants, which determine the likelihood of observing a technical default in first place.

Table 5 shows coefficient estimates from a linear probability model analogous to equation (7), where the dependent variable is an indicator equal to one if $\hat{\beta}_{b,y}$ is non-missing for a given bank-year and zero otherwise. To keep sample size constant, we set missing variables to zero and include a binary variable equal to one for each of them if the corresponding variable is missing and zero otherwise. In columns 1 to 10, we present estimates of univariate regressions on each of the bank characteristics in $\mathbf{\Gamma}_{b,y-1}$, which capture the bank's funding structure and business model. In column 11, the specification comprises the entire vector of bank covariates but it can only explain 10.6% of the variation in the dependent variable. In column 12, we include only those variables that are individually significant (Tier 1, deposits, total assets, total assets, trading activity, and cost-to-income ratio). Only Tier 1, total assets, and the cost-to-income ratio retain statistical significance across all specifications.

The negative association between the presence of a $\hat{\beta}_{b,y}$ coefficient and Tier 1 supports the equity buffer argument. More capitalized banks are able to absorb larger shocks on risky loans and are thus potentially more prone to design loose covenants or to extend covenant-lite loans, which translates in a lower probability of observing a monitoring coefficient. Interestingly, the effect of a plain leverage ratio is instead not statistically different from zero. This result is consistent with Dermine (2015), who shows theoretically that it is the uncertainty about the value of bank assets, and hence risk-adjusted capitalization, which might trigger bank runs. Our results support the notion that ample

[25] Note that $\beta_{BoA,2003}$ is missing, because BoA-2003 constitutes our reference group in equation (5).

risk-adjusted capital provides banks with the ability to be patient with borrowers who violate covenants. The positive coefficient linked to bank size and cost-to-income ratio is consistent with monitoring being increasing in the resources devoted to it and in the quality of the bank's technology.

The role of bank funding structure and other characteristics. Against this backdrop, we implement the second step of the approach by estimating specification (7). We report coefficient estimates in Table 6. Columns 1 to 10 report univariate specifications for each of the bank characteristics contained in $\mathbf{\Gamma}_{b,y-1}$, whereas the model in column 11 includes the entire vector of bank characteristics. The model in column 12 features only bank traits that exhibit univariate significance (Tier 1, total assets, non-interest income, non-performing assets, and net income). Only for Tier 1, size, and non-performing assets we find a statistically significant relationship with $\hat{\beta}_{b,y}$ in each specification.

The positive link between $\hat{\beta}_{b,y}$ and Tier 1 capital brings further support to the equity buffer hypothesis, while it does not line up with the equity monitoring hypothesis. More capitalized banks – for which increased capital requirements stemming from violations are less likely to bind – appear to be more lenient towards violating firms, allowing them to invest more. Also non-performing assets correlate positively with $\hat{\beta}_{b,y}$, which suggests that banks with a worse screening technology are less strict as monitors.[26]

The data used in the second step is coarse and the sample is relatively small. Therefore, we reduce the dimension of the problem to capture overall bank quality and explain variation in monitoring across banks in Table 7. Besides including Tier 1 capital – the only bank variable providing consistent results across different tests –, we define a "bad" bank in columns 1, 2, and 3 if its mean non-performing assets, non-interest income, and cost-to-income ratio is in the top quartile of the distribution of mean bank values, respectively. None of these "bad bank" measures exhibits a significant correlation with our bank monitoring coefficients. Tier 1 ratios, in turn, retain a positive and statistically significant coefficient.

Whether increased bank leniency – linked, for instance, to Tier 1 capital – is efficient or a symptom of distraction by bank monitors is an empirical question. We thus study how bank interventions captured by the coefficients in $\hat{\beta}_{b,y}$ correlate with the borrowing firms' performance around the same covenant violation events.

In Table 8, we examine the correlation between $\hat{\beta}_{b,y}$ and $\hat{\beta}^{ROA}_{b,y}$ ($\hat{\beta}^{q}_{b,y}$), the bank-year specific violation effect on ROA (Tobin's q) also obtained from the estimations in Table

[26]Note that a higher $\hat{\beta}_{b,y}$ corresponds to looser monitoring.

4.[27] In column 1, we uncover a positive and significant relationship between $\hat{\beta}_{b,y}$ and $\hat{\beta}^{ROA}_{b,y}$. This result may seem at odds with the positive effect of covenant violations on ROA shown by Nini et al. (2012), but it can actually be reconciled with their findings. Indeed, they document a negative (positive) effect of covenant violations on investment (performance), but they do not regress the violation-related adjustment in investment on the violation-related adjustment in performance.[28] To the best of our knowledge, we are the first to show that the positive effect of covenant violations on performance is driven by those instances in which the lending banks act in a more lenient fashion regarding their intervention behavior. This inference is corroborated by the positive – although insignificant – relation between $\hat{\beta}_{b,y}$ and Tobin's q $\hat{\beta}^{ROA}_{b,y}$ in column 3. All in all, these results point to the efficiency of banks' leniency after covenant violations.

This result seems to imply that banks reacting strictly to violations pursue an inefficient solution, at least from the perspective of the borrowers. In the light of our result on Tier 1, one could argue that these banks are constrained in their choice set due to their relatively low capitalization. Rather than opting for the course of action maximizing borrowing firms' value, they chose to impose investment restrictions to protect their short-term claim on a borrower's cash flow. In other words, their action can be seen as an example of excessive monitoring.

The idea of excessive monitoring might seem counterintuitive at first sight. As noted by Pagano and Röell (1998), researchers in corporate finance usually think about settings in which principals provide too little monitoring due to free-riding. However, by taking the viewpoint of the firm's owner, there can be circumstances where monitoring is actually excessive. Specifically, Pagano and Röell (1998) and Burkart, Gromb, and Panunzi (1997) show how shareholders' overmonitoring can be detrimental to firm value by disincentivizing managers from showing initiative and finding new investment projects. Specific to the case of monitoring by banks, Besanko and Kanatas (1993) and Carletti (2004) illustrate that in certain principal-agent settings banks can be monitoring excessively, only maximizing their own utility at the expense of the borrowers. Another strand of theoretical literature providing insights into inefficient bank interventions is that on financial contracting as a means to alleviate liquidation bias in distress (e.g., Gennaioli and Rossi, 2013).

[27] Since $\hat{\beta}_{b,y}$ is a generated regressor, we adjust standard errors following Bertrand and Schoar (2003).

[28] In unreported results based on the the firm-quarter data structure of Chava and Roberts (2008), we also find a positive and significant effect of violations on the borrowing firms' ROA, which is perfectly in line with Nini et al. (2012).

Bank monitoring throughout the business cycle. Next, we study the role of business cycle conditions in shaping monitoring activity. We begin by visualizing the dynamics of monitoring coefficients $\hat{\beta}_{b,y}$ alongside recession periods and CFNAI in Figure 4. The left (right) graph plots the mean (standard deviation) of the monitoring coefficients. The non-cyclical behavior of the average monitoring intensity (except for the spike in 2010-11) – as witnessed by its insignificant correlation of 26.57% with CFNAI – does not lend support to theories predicting countercyclical patterns in monitoring incentives because of the procyclical nature of loan quality applications (Ruckes, 2004) or because of rational inattention in expansions (Mariathasan and Zhuk, 2018). Also the standard deviation of monitoring intensity is non-cyclical with an insignificant correlation with CFNAI of 12.41%. Interestingly, such a standard deviation appears to go through cycles, which are however non-synchronous (or even unrelated) with the cycle of the economy. This finding is hard to reconcile with existing theories.

To further explore the business cycle properties of bank monitoring, we augment specification (7) with interactions between the bank variables in $\mathbf{\Gamma}_{b,y-1}$ and macroeconomic indicators (NBER recessions, NFCI, CFNAI) in Table 9. Given that we use annual data, the indicator for NBER recessions (column 1) is equal to one if the first month of the year is in recession, and zero otherwise. NFCI (column 2) measures conditions on US capital markets and the banking system. It has an average of zero and positive (negative) values indicate tighter (looser) financial conditions. CFNAI (column 3) measures aggregate economic activity in the US. It is on average equal to zero. Positive (negative) values indicate growth above (below) trend. None of the bank characteristics in $\mathbf{\Gamma}_{b,y-1}$ interacts meaningfully with the business cycle. Also the positive and significant relationship with Tier 1 capital does not vary significantly over the business cycle. This finding is in line with the earlier outcome that bank monitoring is to a large extent non-cyclical and inconsistent with theories predicting an important role for the business cycle.

Overall, the second-step results clearly support the equity buffer hypothesis. Better capitalized banks are more benign monitors of covenant violating firms. This monitoring style is associated with improved borrower performance, pointing to its efficiency rather than to distraction or shirking of managers and loan officers of well-capitalized banks.

6.1.3 Quasi-experimental evidence

We implement two quasi-experiments to scrutinize if the correlation results on capitalization, funding structure, and monitoring lend themselves to a causal interpretation. First, we use the 2009 US SCAP stress test to conduct causal inference on the equity monitoring hypothesis (vs. our equity buffer story). Then, we use deposit insurance coverage reforms to test the funding fragility hypothesis.

SCAP stress test. On May 7, 2009, the Federal Reserve Board (the Board) released the results of its first stress test after the financial crisis (the SCAP) for the 19 largest US banks. Ten banks were identified to have severe capital shortfalls ranging from $0.6 billion to $33.9 billion. The results induced 14 banks to issue equity in the three month window around the publication of results. Importantly, as noted by Greenlaw, Kashyap, Schoenholtz, and Shin (2012) affected banks were not issuing capital in the three months before the publication. According to Morgan, Peristiani, and Savino (2014) the size of each bank's capital shortfall identified in the SCAP was not anticipated by market participants. Thus, the equity issuance can be interpreted as a plausibly exogenous increase in Tier 1 capital. We use issuance in the three months after the publication of the stress test scaled by 2008 total assets as our treatment intensity indicator.

Figure 5 shows that there was no clearly discernible difference in terms of Tier 1 capitalization as of the end of 2008 across treated banks (i.e., those that issued equity in the three months after the SCAP) and non-treated banks. The Board based its stress test on criteria that were not known ex ante and not tightly linked to Tier 1 capital, which arguably explains why markets did not anticipate the SCAP results. Reassuringly, the treated and non-treated group appear to be heterogeneous in terms of business model, both comprising a mix of global and more regional banks.

Table 10 shows the results of a difference-in-difference analysis where we interact the SCAP treatment measure indicator with either year-indicators for the years 2010, 2011, and 2012 or a cumulative post-period indicator that is equal to one starting in 2010. In addition, we control for bank-level total TARP equity injections scaled by 2007 total assets to account for selection into treatment, as well as for the bank characteristics in $\mathbf{\Gamma}_{b,y-1}$.[29] Across a range of specifications involving different sample restrictions and pre- and post-periods, we find a positive and significant effect of equity issuance activity linked to the SCAP on monitoring intensity.[30] The positive effect of such equity shocks work in the same direction as Tier 1 capital in the baseline correlation analysis and corroborates

[29] Note that the timing, forced nature, and special conditions associated with the TARP capital injections preclude us from using them as a shock to Tier 1 equity (Calomiris and Khan, 2015).

[30] In unreported results that are available upon request, we re-estimate all specifications from Table 10 without the treatment year 2009. The results remain qualitatively unchanged and most specifications retain statistical significance.

the equity buffer story.

Deposit insurance coverage reforms. We turn next to reforms changing deposit insurance coverage to obtain plausibly exogenous variation in banks' exposure to bank runs. An increased insurance coverage translates into a lower probability of depositor runs and allows us to conduct causal inference on the fragility monitoring hypothesis, which postulates that bankers should monitor less intensely in such circumstances.

We combine information from Demirgüç-Kunt, Kane, and Laeven (2005), Demirgüç-Kunt, Kane, and Laeven (2014), and Schich (2009) on reforms increasing deposit insurance coverage for the country-years in our second-step sample. In total, we rely on 10 single-country reforms and the 2011 EU-wide increase in deposit insurance coverage. We construct a recursive reform index similar to the employment protection reform index by Simintzi, Vig, and Volpin (2014). Our reform index starts at zero for all country-years in the sample. In each year with an increase in deposit insurance coverage, the respective country indicator increases by 1, thus showing the running sum of coverage reforms.

In column 1 of Table 11, we employ this variable in a regression with our baseline bank characteristics and country indicators. We do not find any statistically significant effect of coverage reforms on monitoring intensity.

To use bank-level variation in funding fragility, we then focus on US banks and take a closer look at the 2008 Emergency Economic Stabilization Act (EESA), which increased deposit insurance coverage from \$100,000 to \$250,000 per depositor. In line with Lambert, Noth, and Schüwer (2017), we obtain information on the bank-level change in insured deposits (scaled by total assets) around the EESA. Because our sample of US banks is considerably smaller than that of Lambert et al. (2017), we rely on a different definition of treatment and control group. We assign a bank to the treatment group if its change in insured deposits is above the 75th percentile, and to the the control group otherwise.[31]

In column 2 of Table 11, we interact the treatment indicator with a post-EESA indicator equal to one from 2009 onwards. The positive effect seems to lend support to the funding fragility story. However, in column 3, where we use a more narrow time window, the effect becomes insignificant.[32] In column 4, we obtain a similar result implementing the EESA experiment over the entire sample of banks and controlling for the country-level

[31] Using only banks below the 25th percentile as the control group as in Lambert et al. (2017) leaves the results qualitatively unchanged.

[32] In unreported regressions with year indicators, we show that the positive effect comes entirely from the years 2011 and 2012 and is thus likely to be a spurious correlation unrelated to the 2008 increase in deposit insurance coverage.

reform indicator.

Overall, the quasi-experimental setting lends no support to the funding fragility hypothesis, reinforcing the result from the baseline analysis.

6.2 One-step approach

The lack of support for the funding fragility hypothesis stemming from our two-step approach should be interpreted with caution. As pointed out in Section 3.3.1, the second-step estimates may suffer from (i) measurement error in the dependent variable (which is generated) and (ii) limited statistical power. Both forces generate a bias against finding statistically significant correlations. We address these concerns through the one-step approach.

Table 12 shows coefficient estimates from the one-step specification (8) for investment over the granular deal-bank-firm-quarter data structure. In column 1, we use all banks in our sample. In column 2, we focus again on the banks used in column 11 of Table 6. We then define a "discontinuity sample" as those deal-bank-quarter observations for which the absolute value of the relative distance between the accounting variable – (tangible) net worth or current ratio – and the corresponding covenant threshold is less than 0.2 as in Chava and Roberts (2008).[33] Column 3 repeats the regression from column 1 over the discontinuity sample. The most consistent result is again the positive and significant interaction between risk-adjusted Tier 1 ratio and covenant violations, in line with the equity buffer argument. The proxy for banks' exposure to bank runs – the asset share of short-term funding – remains in turn insignificant. As such, evidence supporting the funding fragility hypotheses is not primarily driven by econometric concerns associated with the two-stage baseline approach. We also find a negative and significant correlation with non-interest income, which goes against the intuition that more diversified banks may pay less to troubled borrowing firms.

In sum, results from the one-step approach support the inference drawn on the basis of the baseline specification. Larger equity buffer mitigate banks' monitoring responses to covenant violations, and we find no evidence in support of the funding fragility story.

[33]The optimal bandwidth criterion by Imbens and Kalyanaraman (2012) would suggest almost the same bandwidth, namely 0.203. We obtain similar results with a bandwidth of 0.4 as Ferreira et al. (2018): See Appendix Table A.3.

7 Conclusion

Loan monitoring is a key activity of banks as informed lenders. Several theories link the intensity and effectiveness of such an activity to bank funding structure as well as to the state of the business cycle.

This paper studies heterogeneity in monitoring across banks in the context of syndicated loans to US firms. Making use of a granular data structure linking lending banks to borrowing firms, we extract a bank-time specific measure of monitoring intensity. More specifically, we measure monitoring by looking at bank intervention in borrowers' management after covenant violations, as proxied by firms' changes in investment policy.

Using our measure of monitoring, we document the existence of substantial heterogeneity in monitoring both across banks and through time. We find that equity capital is an important determinant of bank monitoring incentives. Well-capitalized banks, which are better able to absorb negative shocks on their loan portfolio, keep a looser stance towards borrowing firms. This looser stance, rather than being distortive, is linked to improved borrowers' performance.

To move closer to causal inference, we investigate banks' monitoring responses towards exogenous shocks to their regulatory equity capital during the Supervisory Capital Assessment Program (SCAP) of 2009. This exercise confirms the inferences based on correlations quantified in the regression analysis. In contrast, neither reduced-form regression analyses nor global and US-only changes to deposit insurance schemes that represent exogenous shifts in depositor discipline indicate support for theories emphasizing the role of banks' funding structures to explain monitoring efforts.

Against the backdrop of ongoing regulatory changes that pertain to risk-adjusted capital requirements, leverage ratios, and liquidity buffers to insure banks against sudden re-financing stops, it is important to note that our results clearly corroborate the importance of risk-weighted capital buffers. Only larger Tier 1 capital buffers entail that banks pursue a more benign monitoring style, which in turn appears to enable financial intermediaries to better bolster shocks experienced by their borrowers that result in covenant violations.

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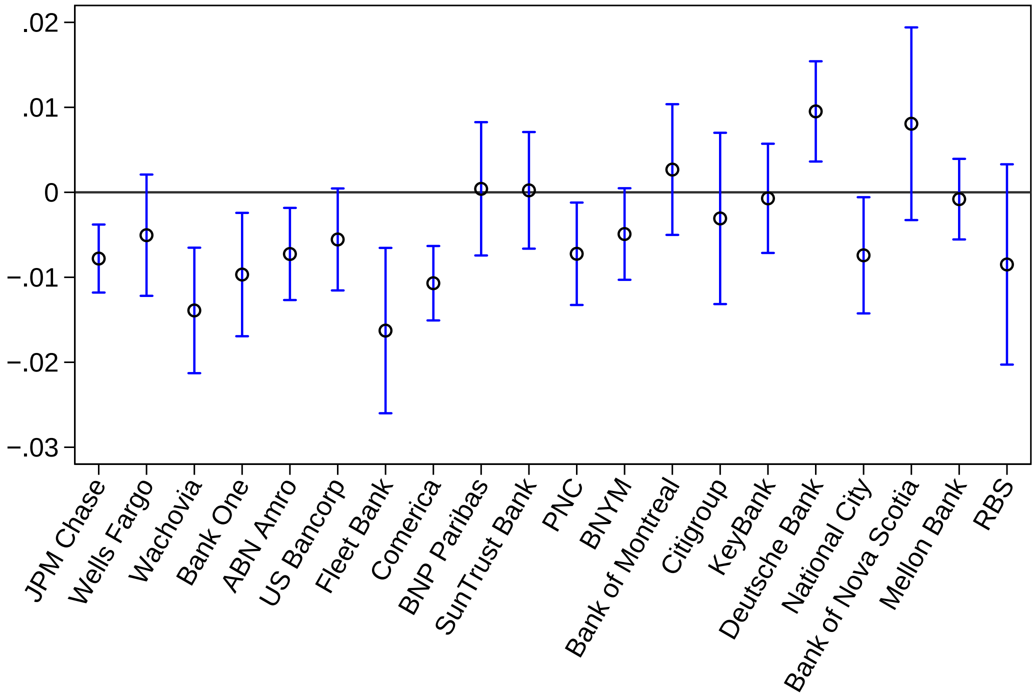

Figure 1: Distribution of time-invariant of monitoring across banks
This figure visualizes coefficient estimates $\hat{\beta}_b$ (with 95% confidence intervals) across banks. $\hat{\beta}_b$ are estimated coefficients from this specification: $I_{l,b,f,q} = \alpha \cdot v_{l,q-1} + \sum_b \beta_b \cdot v_{l,q-1} \times \gamma_b + \boldsymbol{\eta}\mathbf{x}_{f,q-1} + \boldsymbol{\zeta}\mathbf{p}_{l,q-1} + \gamma_b + \gamma_f + \gamma_q + \gamma_e + \epsilon_{l,b,f,q}$. The specification is estimated over the bank sample in Table 1 between 1994 and 2012, but for readability the coefficient estimates are reported only for the 20 most active lenders in our sample. BoA is the reference bank.

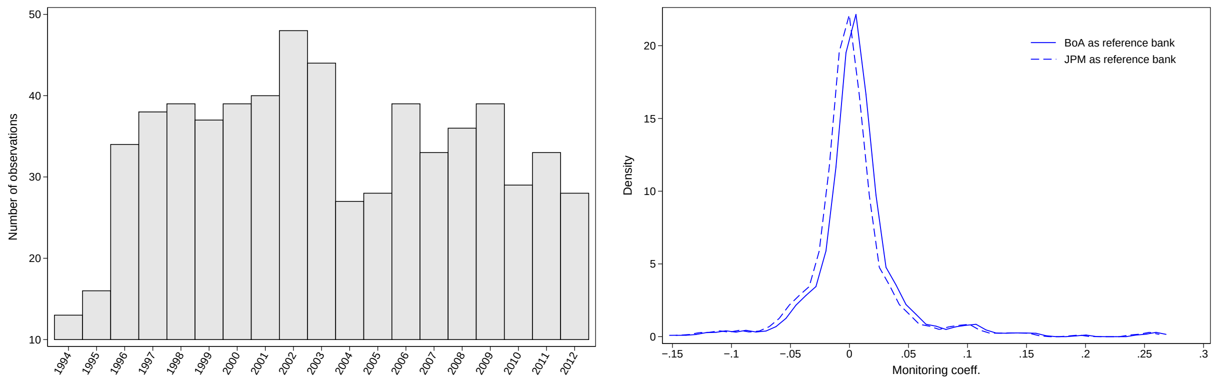

Figure 2: Distribution of bank monitoring
This figure visualizes the distribution of our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. The left graph shows the number of available observations in each year between 1994 and 2012. The right graph plots the density of $\hat{\beta}_{b,y}$ using BoA (solid line) and JPM (dashed line) as the reference bank.

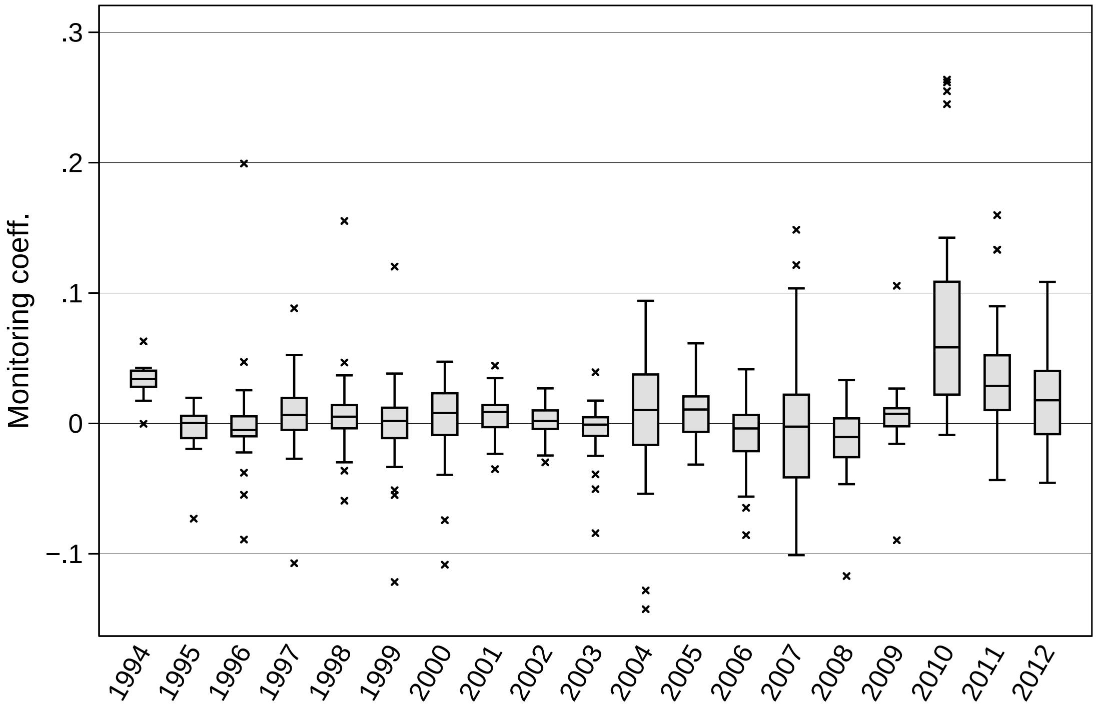

Figure 3: Distribution of bank monitoring through time
This figure visualizes the distribution of our bank monitoring measure $\hat{\beta}_{b,y}$ in each year of our 1994-2012 bank-year sample through box plots. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy.

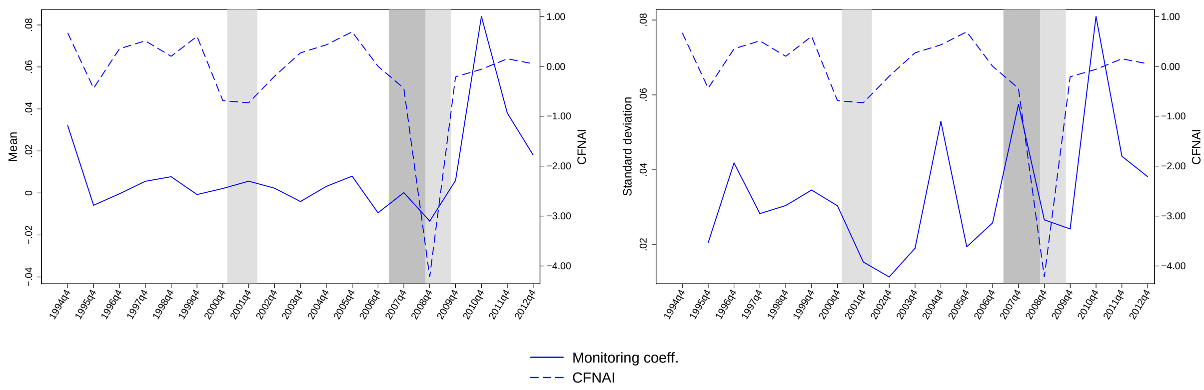

Figure 4: Bank monitoring through the business cycle
This figure visualizes the mean (left graph) and the standard deviation (right graph) of our bank monitoring measure $\hat{\beta}_{b,y}$ in each quarter between 1994 and 2012. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. Business cycle are measured by means of CFNAI (dashed line), NBER recessions (shaded in light grey), and the early phase of the Great Recession before Lehman Brothers' bankruptcy as defined by (shaded in dark grey, defined as in Kahle and Stulz, 2013).

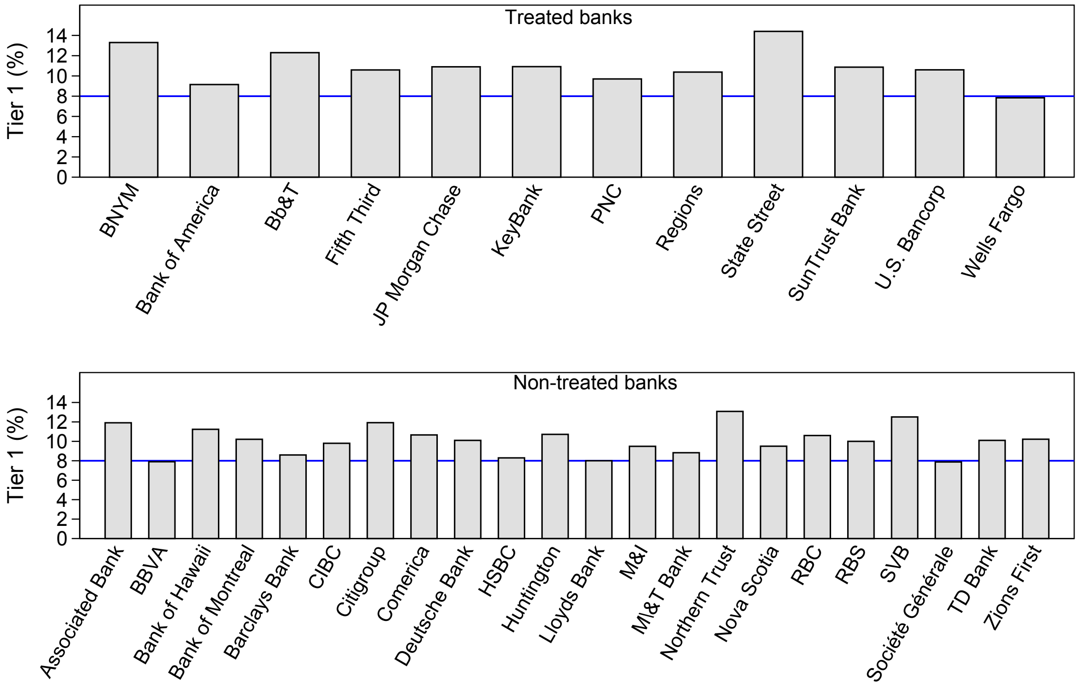

Figure 5: Risk-adjusted Tier 1 capital ratio before the SCAP stress test of 2009
This figure visualizes risk-adjusted Tier 1 capital ratio for treated (top graph) and non-treated (bottom graph) banks before the SCAP of 2009. The bar charts show the regulatory Tier 1 capital ratio at the end of 2008 together with the minumum capital requirement of 8% (horizontal blue line). Treated banks are those banks that issued equity in the three month-window around the SCAP stress test in May 2009.

Table 1: Bank sample
This table shows the syndicated loan market share of the 51 banks in our second-step sample, i.e., those with all bank variables contained in $\mathbf{\Gamma}_{b,y-1}$ from equation (7) available in at least one year, which can thus be used to estimate such a specification.

	Deals		Volume	
Bank name	Number	Share (%)	$B	Share (%)
Bank of America	1,174	6.045	72.487	7.216
JP Morgan Chase	873	4.495	69.693	6.938
Wells Fargo	662	3.409	28.338	2.821
Wachovia (active until 2008)	593	3.053	32.402	3.226
Bank One Corp (active until 2004)	562	2.894	27.498	2.737
ABN Amro Bank (active until 2007)	428	2.204	25.748	2.563
U.S. Bancorp	411	2.116	21.073	2.098
Fleet Bank, later Fleet Boston (active until 2004)	389	2.003	19.976	1.989
Comerica	379	1.951	18.489	1.841
BNP Paribas	376	1.936	26.043	2.592
SunTrust Bank	368	1.895	19.048	1.896
PNC	347	1.787	15.764	1.569
BNYM	340	1.751	22.498	2.240
Bank of Montreal	338	1.740	19.176	1.909
Citigroup	323	1.663	31.200	3.106
KeyBank	277	1.426	13.743	1.368
Deutsche Bank	263	1.354	26.019	2.590
National City (active until 2008)	249	1.282	9.650	0.961
Bank of Nova Scotia	243	1.251	11.122	1.107
Mellon Bank (active until 2007)	222	1.143	13.021	1.296
Royal Bank of Scotland	205	1.056	13.731	1.367
Wachovia (old, active until 2000)	159	0.819	11.557	1.150
Société Générale	150	0.772	11.584	1.153
Royal Bank of Canada	148	0.762	8.365	0.833
Northern Trust	138	0.711	6.320	0.629
Barclays Bank	132	0.680	12.962	1.290
Fifth Third Bancorp	129	0.664	4.864	0.484
SVB	127	0.654	1.447	0.144
JP Morgan (active until 2000)	119	0.613	11.564	1.151
HSBC	117	0.602	10.716	1.067
BBVA	104	0.536	4.542	0.452
TD Bank	102	0.525	3.074	0.306
Compass Bank	75	0.386	3.012	0.300
Hibernia National Bank	64	0.330	2.801	0.279
Regions	56	0.288	2.313	0.230
CIBC	52	0.268	1.625	0.162
State Street	50	0.257	2.048	0.204
AmSouth Bank	45	0.232	1.785	0.178
Huntington National Bank	44	0.227	1.173	0.117
M&T Bank	42	0.216	1.796	0.179
Bb&T Bank	37	0.191	1.224	0.122
Zions First National	34	0.175	1.509	0.150
Bank of Hawaii	31	0.160	1.518	0.151
Provident Bank (active until 2004)	29	0.149	0.656	0.065
Commerce Bank (active until 2008)	27	0.139	0.894	0.089
SouthTrust Bank (active until 2004)	23	0.118	0.732	0.073
M&I Bank (active until 2011)	21	0.108	0.897	0.089
Lloyds Bank	18	0.093	1.124	0.112
Bank of the West	16	0.082	0.634	0.063
Associated Bank	14	0.072	0.584	0.058
First Merit Bank	4	0.021	0.078	0.008
Total (all 51 lenders)	11,129	57.304	650.113	64.717

Table 2: Summary statistics
This table shows summary statistics for our sample of US borrowing firms (from CCM), banks (from Compustat Banks and Bankscope) and syndicated loans (Dealscan) over the period 1994-2012. Panel A reports summary statistics for firm-quarters that are in covenant violation. Panel B reports summary statistics for firm-quarters that are not in covenant violation. To favor comparability with the other firm-level variables, (tangible) net worth is expressed in millions of 2010 dollars. Panel C reports summary statistics for the lending banks reported in Table 1. Panel D reports summary statistics for syndicated loans. Refer to Appendix Table A.1 for variable definitions.

Panel A: Firm characteristics in covenant violation quarters

	N	Mean	SD	P25	Median	P75
Tobin's q	1,324	1.424	0.884	0.971	1.181	1.554
Cash flow	1,215	-0.178	0.641	-0.126	0.016	0.066
Investment	1,306	0.061	0.078	0.016	0.035	0.075
ROA	1,323	-0.038	0.078	-0.049	-0.009	0.008
ln(Assets)	1,324	5.532	1.453	4.465	5.431	6.451
Leverage	1,324	0.358	0.208	0.194	0.347	0.510
Current ratio	1,319	1.424	1.002	0.846	1.177	1.783
Net worth	1,324	220.138	512.525	20.659	61.768	189.398
Tangible net worth	1,319	220.573	513.415	20.596	61.738	189.486

Panel B: Firm characteristics outside covenant violation quarters

	N	Mean	SD	P25	Median	P75
Tobin's q	20,014	1.667	1.072	1.058	1.340	1.867
Cash flow	18,289	0.091	0.341	0.034	0.077	0.163
Investment	19,500	0.070	0.077	0.026	0.049	0.087
ROA	20,013	0.005	0.034	0.001	0.010	0.019
ln(Assets)	20,014	6.072	1.538	4.939	6.010	7.118
Leverage	20,014	0.257	0.174	0.116	0.245	0.370
Current ratio	19,933	2.381	1.706	1.434	1.985	2.785
Net worth	20,014	610.402	1591.436	68.696	185.826	529.535
Tangible net worth	19,930	605.627	1581.507	68.544	184.858	527.159

Panel C: Bank characteristics

	N	Mean	SD	P25	Median	P75
Leverage	2,626	0.076	0.023	0.062	0.079	0.092
Tier 1	2,565	0.097	0.021	0.080	0.092	0.110
Deposits	2,635	0.640	0.117	0.600	0.655	0.708
Short-term funding	2,438	0.047	0.053	0.005	0.029	0.075
ln(Assets)	2,644	11.699	1.494	10.586	11.510	12.815
Non-interest income	2,213	0.462	0.164	0.347	0.435	0.552
Trading	2,235	0.058	0.098	0.001	0.009	0.091
Non-performing assets	2,436	0.007	0.006	0.003	0.005	0.008
Net income	2,640	0.003	0.002	0.002	0.003	0.004
Cost-to-income	2,213	0.641	0.135	0.559	0.618	0.691

Panel D: Loan characteristics

	N	Mean	SD	P25	Median	P75
Facility amount ($M)	4,596	210.009	490.525	13.840	55.975	201.186
Deal amount ($M)	4,596	322.322	761.214	26.875	92.369	298.646
All-in-drawn spread (b.p.)	4,314	202.311	117.423	120.000	200.000	275.000
Syndicate size	4,592	5.229	6.589	1.000	2.000	7.000

Table 3: Investment and covenant violations
This table reports estimates from RDD specifications for investment of borrowing firms around covenant violations. The sample in odd (even) columns covers the period 1994-2005 (1994-2012). The dependent variable is the borrowing firm's investment rate. The explanatory variables include the binary (0/1) covenant violation indicator, firm time-varying characteristics, and polynomials of distance measures from the covenant threshold. All independent variables are lagged by one quarter, except for *Cash flow (firm)*, which is contemporaneous with investment. Columns 1 and 2 report estimates of specification (1) over a firm-quarter data structure. Columns 3 and 4 report estimates of specification (3) over a deal-bank-firm-quarter data structure. Standard errors are clustered as indicated below. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Investment			
	(1)	(2)	(3)	(4)
Violation (firm)	-0.008***	-0.007***		
	(-3.38)	(-3.15)		
Violation (deal)			-0.003	-0.002
			(-1.54)	(-1.02)
Tobin's q (firm)	0.022***	0.022***	0.019***	0.022***
	(5.86)	(6.81)	(7.70)	(8.72)
Cash flow (firm)	0.004	0.006**	0.010***	0.009***
	(1.03)	(2.00)	(2.73)	(2.77)
ln(Assets) (firm)	-0.007	-0.009**	-0.012***	-0.015***
	(-1.52)	(-2.46)	(-3.66)	(-4.06)
Default distance (NW)	-0.000	-0.000	0.001	0.001
	(-1.05)	(-0.97)	(1.15)	(1.08)
Default distance (CR)	0.009**	0.008***	0.016***	0.006
	(2.56)	(2.70)	(3.82)	(1.03)
Default distance (CR)^2	-0.001***	-0.000***	-0.003***	0.000
	(-3.21)	(-2.99)	(-3.21)	(0.13)
Default distance (NW)^2	0.000	0.000	-0.000	-0.000
	(1.01)	(0.93)	(-0.64)	(-0.54)
Firm FE	Yes	Yes	Yes	Yes
Bank-year FE	No	No	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Fiscal quarter FE	No	No	Yes	Yes
Observations	6,170	7,811	24,687	36,216
Adjusted R^2	0.381	0.364	0.461	0.416
Number of banks	-	-	87	91
Mean dep. var.	0.065	0.065	0.055	0.057
Unit of observation	Firm-quarter	Firm-quarter	Deal-bank-firm-quarter	Deal-bank-firm-quarter
Clustering	Firm	Firm	Bank-quarter	Bank-quarter
Sample selection	All banks	All banks	All banks	All banks
Sample period	1994-2005	1994-2012	1994-2005	1994-2012

Table 4: Investment, ROA, Tobin's q, and covenant violations

This table reports estimates from RDD specifications for investment, ROA and Tobin's q of borrowing firms around covenant violations. The sample covers the period 1994-2012 and has a deal-bank-firm-quarter structure. The explanatory variables include the binary (0/1) covenant violation indicator, firm time-varying characteristics, and polynomials of distance measures from the covenant threshold. All independent variables are lagged by one quarter, except for *Cash flow (firm)*, which is contemporaneous with the dependent variable. Column 1 reports estimates of the first-step specification (5) for borrowing firms' investment. Columns 2 and 3 are based on the same specification but using ROA and Tobin's q as dependent variable, respectively. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Investment	ROA	Tobin's q
	(1)	(2)	(3)
Violation × Bank-year FE	Yes	Yes	Yes
F-test (statistic)	4213.138***	3879.250***	544.110***
F-test (p-value)	0.000	0.000	0.000
Tobin's q (firm)	0.022***	0.005***	
	(7.59)	(2.75)	
Cash flow (firm)	0.009**	0.111***	0.153***
	(2.34)	(17.91)	(3.82)
ln(Assets) (firm)	-0.015***	-0.002	-0.189***
	(-3.50)	(-1.09)	(-4.99)
Default distance (NW)	0.000	-0.000	0.032**
	(0.38)	(-0.05)	(2.33)
Default distance (CR)	0.007	-0.003	0.037
	(1.02)	(-0.53)	(0.97)
Default distance (NW)^2	0.000	0.000	-0.001*
	(-0.10)	(0.95)	(-1.87)
Default distance (CR)^2	0.000	0.000	-0.000
	(0.01)	(0.20)	(-0.05)
Violation	Yes	Yes	Yes
Bank-year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes
Fiscal quarter FE	Yes	Yes	Yes
Summary statistics:	$\hat{\beta}_{b,y}$	$\hat{\beta}^{ROA}_{b,y}$	$\hat{\beta}^{q}_{b,y}$
Mean	0.008	0.008	0.025
Standard deviation	0.040	0.152	0.262
Observations	36,195	36,390	36,206
Adjusted R^2	0.422	0.668	0.676
Number of banks	90	90	89
Mean dep. var.	0.057	0.001	1.450
Clustering	Bank-quarter	Bank-quarter	Bank-quarter
Sample selection	All banks	All banks	All banks
Sample period	1994-2012	1994-2012	1994-2012

Table 5: Availability of a monitoring estimate and bank characteristics

This table reports estimates from linear probability models over a 1994-2012 bank-year panel, where the dependent variable is an indicator variable equal to one if $\hat{\beta}_{b,y}$ is non-missing for bank b in year y, and zero otherwise. $\hat{\beta}_{b,y}$, our bank monitoring measure, is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. The list of bank-years for which $\hat{\beta}_{b,y}$ is missing is provided in Appendix Table A.2. The explanatory variables include bank time-varying characteristics. For each of these variables, (i) we set missing observations to zero, and (ii) add to the specification a binary variable equal to one if the corresponding variable is missing and zero otherwise (denoted as *Missing variable FE*). All independent variables are lagged by one year. Standard errors are clustered as indicated below. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Non-missing $\hat{\beta}_{b,y}$											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Leverage	-1.604										1.628	
	(-1.41)										(1.12)	
Tier 1		-3.428***									-3.192**	-3.036**
		(-2.63)									(-2.35)	(-2.35)
Deposits			-0.751***								-0.134	-0.223
			(-4.29)								(-0.53)	(-1.01)
Short-term funding				0.780							0.035	
				(1.63)							(0.09)	
ln(Assets)					0.081***						0.089***	0.068***
					(5.34)						(3.61)	(2.75)
Non-interest income						0.314					0.095	
						(1.61)					(0.65)	
Trading							0.678**				-0.138	-0.144
							(2.58)				(-0.49)	(-0.50)
Non-performing assets								-0.633			-4.320	
								(-0.16)			(-1.08)	
Net income									-9.497		-5.119	
									(-0.84)		(-0.40)	
Cost-to-income										0.291**	0.276*	0.235**
										(2.02)	(1.81)	(1.99)
Constant	0.843***	1.048***	1.201***	0.690***	-0.247	0.606***	0.683***	0.730***	0.752***	0.559***	-0.217	0.223
	(10.97)	(8.31)	(11.05)	(17.64)	(-1.30)	(5.76)	(17.02)	(17.28)	(16.58)	(4.84)	(-0.44)	(0.52)
Missing variable FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	908	908	908	908	908	908	908	908	908	908	908	908
Adjusted R^2	0.035	0.035	0.061	0.028	0.086	0.037	0.033	0.015	0.029	0.035	0.106	0.100
Number of banks	91	91	91	91	91	91	91	91	91	91	91	91
Mean dep. var.	0.687	0.687	0.687	0.687	0.687	0.687	0.687	0.687	0.687	0.687	0.687	0.687
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Sample selection	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks
Sample period	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012

Table 6: Monitoring and bank characteristics

This table reports estimates from the second-step OLS specification (7) over a 1994-2012 bank-year panel, where the dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. The explanatory variables include bank time-varying characteristics. All independent variables are lagged by one year. Standard errors are clustered as indicated below. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	$\hat{\beta}_{b,y}$											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Leverage	-0.030										-0.013	
	(-0.38)										(-0.11)	
Tier 1		0.443***									0.410***	0.375***
		(4.67)									(3.03)	(3.46)
Deposits			-0.011								-0.008	
			(-0.76)								(-0.20)	
Short-term funding				0.031							-0.014	
				(1.50)							(-0.30)	
ln(Assets)					0.002**						0.002	0.003*
					(2.04)						(1.15)	(1.88)
Non-interest income						-0.015*					-0.017	-0.012
						(-1.70)					(-1.48)	(-1.30)
Trading							0.002				0.003	
							(0.15)				(0.12)	
Non-performing assets								1.302***			0.702**	0.802**
								(6.18)			(2.52)	(2.57)
Net income									-1.259*		0.331	0.194
									(-1.69)		(0.30)	(0.28)
Cost-to-income										0.005	-0.001	
										(0.50)	(-0.07)	
Constant	0.010	-0.033***	0.015	0.006***	-0.017	0.015***	0.008***	-0.002	0.011***	0.004	-0.049	-0.061***
	(1.57)	(-3.88)	(1.53)	(3.25)	(-1.47)	(3.58)	(4.39)	(-0.60)	(4.53)	(0.67)	(-1.05)	(-2.91)
Observations	523	495	526	503	526	418	453	468	526	418	310	363
Adjusted R^2	-0.001	0.065	-0.000	0.001	0.006	0.003	-0.002	0.061	0.004	-0.002	0.094	0.109
Number of banks	66	64	67	63	67	56	66	62	67	56	51	52
Mean dep. var.	0.008	0.007	0.008	0.008	0.008	0.007	0.008	0.008	0.008	0.007	0.008	0.008
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Sample selection	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks
Sample period	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012

Table 7: Monitoring and bank quality

This table reports estimates from OLS regressions over a 1994-2012 bank-year panel, where the dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. The explanatory variables include Tier 1 and "bad bank" indicators. A bank as "bad" if its mean non-performing assets, non-interest income, and cost-to-income ratio is in the top quartile of the distribution of mean bank values in columns 1, 2, and 3, respectively. Standard errors are clustered as indicated below. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	$\hat{\beta}_{b,y}$		
	(1)	(2)	(3)
Tier 1	0.430***	0.471***	0.475***
	(4.26)	(4.74)	(4.84)
Bad bank	0.003	-0.001	0.001
	(0.69)	(-0.39)	(0.37)
Constant	-0.033***	-0.036***	-0.037***
	(-3.68)	(-4.04)	(-4.14)
Bad bank measure	Non-performing assets > Q3	Non-interest income > Q3	Cost-to-income > Q3
Observations	477	435	426
Adjusted R^2	0.059	0.076	0.077
Number of banks	64	63	63
Mean dep. var.	0.007	0.007	0.008
Clustering	Bank	Bank	Bank
Sample selection	All banks	All banks	All banks
Sample period	1994-2012	1994-2012	1994-2012

Table 8: Bank monitoring over investment and performance of borrowing firms

This table reports estimates from a modified second-step OLS specification (7) over a 1994-2012 bank-year panel. The dependent variable is either $\hat{\beta}^{ROA}_{b,y}$ or $\hat{\beta}^{q}_{b,y}$. $\hat{\beta}^{ROA}_{b,y}$ ($\hat{\beta}^{q}_{b,y}$) is the estimated coefficient from a modified first-step specification (5) that captures the bank-time specific effect of covenant violations on the borrowing firm's ROA (Tobin's q) instead of the effect on its investment. The explanatory variables include bank time-varying characteristics and our monitoring measure, $\hat{\beta}_{b,y}$ from the original first-step specification (5). All independent variables are lagged by one year except for $\hat{\beta}_{b,y}$ which is contemporaneous with the dependent variables. Standard errors are clustered as indicated below are adjusted for the fact that $\hat{\beta}_{b,y}$ is a generated regressor following Bertrand and Schoar (2003). The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	$\hat{\beta}^{ROA}_{b,y}$	$\hat{\beta}^{q}_{b,y}$
	(1)	(2)
$\hat{\beta}_{b,y}$	0.097**	0.856
	(2.51)	(0.93)
Leverage	-0.004	0.286
	(-0.08)	(0.32)
Tier 1	0.075	0.625
	(1.48)	(0.90)
Deposits	-0.017	0.106
	(-1.41)	(0.51)
Short-term funding	-0.022	-0.250
	(-1.50)	(-0.86)
ln(Assets)	0.000	0.014
	(0.46)	(0.96)
Non-interest income	-0.007	0.064
	(-0.79)	(0.81)
Trading	-0.010	0.413*
	(-0.84)	(1.93)
Non-performing assets	-0.251	4.140
	(-1.50)	(1.60)
Net income	0.952	5.224
	(1.31)	(0.77)
Cost-to-income	0.010	0.156
	(1.23)	(1.38)
Constant	-0.003	-0.493
	(-0.17)	(-1.61)
Observations	310	310
Adjusted R^2	0.013	0.047
Number of banks	51	51
Mean dep. var.	0.0001	0.0526
Clustering	Bank	Bank
Sample selection	All banks	All banks
Sample period	1994-2012	1994-2012

Table 9: Monitoring, bank characteristics, and business cycle conditions
This table reports estimates from the second-step OLS specification (7) augmented with interactions with business cycle measures over a 1994-2012 bank-year panel, where the dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. The explanatory variables include bank time-varying characteristics and their interactions with business cycle measures. All independent variables are lagged by one year. Standard errors are clustered as indicated below. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	$\hat{\beta}_{b,y}$		
	(1)	(2)	(3)
Leverage	0.074	-0.031	-0.032
	(0.56)	(-0.25)	(-0.24)
Tier 1	0.258*	0.437***	0.449***
	(2.00)	(3.22)	(2.91)
Deposits	0.013	-0.010	-0.016
	(0.28)	(-0.24)	(-0.35)
Short-term funding	0.025	-0.017	-0.012
	(0.51)	(-0.38)	(-0.23)
ln(Assets)	0.000	0.003	0.003
	(0.19)	(1.54)	(1.20)
Non-interest income	-0.010	-0.025*	-0.023
	(-0.69)	(-1.77)	(-1.49)
Trading	0.022	-0.009	-0.013
	(0.51)	(-0.30)	(-0.39)
Non-performing assets	1.007**	0.590	0.570
	(2.37)	(1.66)	(1.46)
Net income	-0.215	-0.201	-0.311
	(-0.18)	(-0.16)	(-0.22)
Cost-to-income	0.008	0.009	0.009
	(0.51)	(0.61)	(0.56)
Business cycle	-0.145	-0.049	-0.001
	(-1.63)	(-0.96)	(-0.06)
Business cycle × Leverage	-0.360	0.069	-0.029
	(-1.46)	(0.72)	(-0.57)
Business cycle × Tier 1	0.802**	-0.001	0.104
	(2.66)	(-0.01)	(1.36)
Business cycle × Deposits	-0.022	0.027	-0.009
	(-0.35)	(0.89)	(-0.67)
Business cycle × Short-term funding	-0.014	-0.016	-0.000
	(-0.15)	(-0.44)	(-0.02)
Business cycle × ln(Assets)	0.008*	0.002	0.000
	(1.82)	(0.96)	(0.14)
Business cycle × Non-interest income	-0.018	0.005	-0.002
	(-0.61)	(0.50)	(-0.36)
Business cycle × Trading	-0.008	0.010	-0.012
	(-0.11)	(0.51)	(-1.14)
Business cycle × Non-performing assets	0.068	0.101	0.097
	(0.10)	(0.29)	(0.53)
Business cycle × Net income	5.432**	-0.112	-0.516
	(2.24)	(-0.10)	(-0.90)
Business cycle × Cost-to-income	0.007	-0.010	-0.000
	(0.30)	(-1.18)	(-0.01)
Constant	-0.046	-0.060	-0.051
	(-0.89)	(-1.27)	(-0.95)
Business cycle measure	NBER recession	NFCI	CFNAI
Observations	310	310	310
Adjusted R^2	0.119	0.077	0.076
Number of banks	51	51	51
Mean dep. var.	0.008	0.008	0.008
Clustering	Bank	Bank	Bank
Sample selection	All banks	All banks	All banks
Sample period	1994-2012	1994-2012	1994-2012

Table 10: The SCAP quasi-experiment

This table reports estimates from the second-step OLS specification (7) augmented with a difference-in-differences exercise based on the publication of SCAP stress test results on May 7, 2009. The dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. Explanatory variables include *Affected (SCAP)* (defined as the bank-specific equity issuance after the publication of SCAP results scaled by 2008 total assets) and its interactions with year-specific or cumulative post period indicators for the treatment period, *TARP* (defined as total TARP take-up scaled by 2007 total assets), and lagged time-varying bank characteristics $\mathbf{\Gamma}_{b,y-1}$. Information on the sample period/selection and standard error clustering is indicated below. Specifications including also non-US banks control for a US bank indicator and its interactions with post-SCAP indicators. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	$\hat{\beta}_{b,y}$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2010 × Affected (SCAP)	4.110***						
	(2.79)						
2011 × Affected (SCAP)	3.563**						
	(2.05)						
2012 × Affected (SCAP)	2.586						
	(0.88)						
Post (SCAP)		0.077***	0.055***	0.045***	0.020***	0.041***	0.006
		(5.10)	(6.22)	(6.30)	(2.89)	(3.09)	(0.57)
Post (SCAP) × Affected (SCAP)		4.313***	3.718***	3.425**	3.256*	2.597*	2.593*
		(3.09)	(3.45)	(2.04)	(1.90)	(1.79)	(1.82)
TARP	-0.021	-0.075	-0.035	-0.042	-0.031	0.391	0.227
	(-0.13)	(-0.47)	(-0.22)	(-0.24)	(-0.20)	(1.38)	(0.89)
Main interaction terms	Yes	Yes	Yes	Yes	Yes	Yes	Yes
US-post SCAP interactions	Yes	Yes	Yes	Yes	No	Yes	No
Bank characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	310	269	292	310	236	130	78
Adjusted R^2	0.206	0.153	0.169	0.172	0.132	0.185	0.117
Number of banks	51	51	51	51	37	34	22
Mean dep. var.	0.008	0.005	0.007	0.008	0.006	0.019	0.018
Mean *Affected (SCAP)*	0.003	0.003	0.003	0.003	0.004	0.003	0.004
Number of treated banks	12	12	12	12	12	11	11
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Sample selection	All banks	All banks	All banks	All banks	US-banks	All banks	US-banks
Sample period	1994-2012	1994-2010	1994-2011	1994-2012	1994-2012	2007-2012	2007-2012

Table 11: The deposit insurance quasi-experiment
This table reports estimates from the second-step OLS specification (7) augmented with the quasi-experimental deposit insurance exercise. The dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. *Deposit insurance reform* is defined as the running sum of deposit insurance coverage reforms, starting with 0 in the first year for all countries and adding 1 for each increase in coverage. *Affected (EESA)* is an indicator equal to one if a bank's change in insured deposits over total assets induced by the 2008 EESA reform is above the 75th percentile among US banks, and zero otherwise. Column 1 reports estimates obtained over the entire sample using *Deposit insurance reform*. Column 2 interacts *Affected (EESA)* with *Post (EESA)* focusing on US banks from Lambert et al. (2017), where *Post (EESA)* is an indicator equal to one for the period 2009-2012. Column 3 considers a time window of two years around EESA, where the pre- and post-period are defined as 2007-2008 and 2009-2010, respectively. Column 4 combines the specifications of columns 1 and 2. All specifications include country fixed effects (the reference country is the US) and lagged time-varying bank characteristics $\mathbf{\Gamma}_{b,y-1}$. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	$\hat{\beta}_{b,y}$			
	(1)	(2)	(3)	(4)
Deposit insurance reform	0.002			0.001
	(0.61)			(0.26)
Affected (EESA)		-0.014	-0.002	-0.007
		(-1.58)	(-0.12)	(-1.17)
Post (EESA)		0.008	-0.018	0.005
		(1.18)	(-0.79)	(0.62)
Post (EESA) $\times$ Affected (EESA)		0.039**	0.005	-0.016
		(2.44)	(0.23)	(-1.20)
Country FE	Yes	Yes	Yes	Yes
Bank characteristics	Yes	Yes	Yes	Yes
Observations	310	170	46	310
Adjusted R^2	0.162	0.144	-0.047	0.164
Number of banks	51	22	17	51
Mean dep. var.	0.008	0.006	0.011	0.008
Mean *Affected (EESA)*	0.755	0.235	0.283	.
Number of treated banks	36	5	4	37
Clustering	Bank	Bank	Bank	Bank
Sample selection	All banks	Lambert et al. (2017)	Lambert et al. (2017)	All banks
Sample period	1994-2012	1994-2012	2007-2010	1994-2012

Table 12: Monitoring and bank characteristics (alternative approach)
This table reports estimates from the one-step RDD specification (8) for investment of borrowing firms around covenant violations. The sample covers the period 1994-2012 and has a deal-bank-firm-quarter structure. The dependent variable is the borrowing firm's investment rate. The explanatory variables include the binary (0/1) covenant violation indicator, its interaction with bank time-varying characteristics, firm time-varying characteristics, and polynomials of distance measures from the covenant threshold. All independent variables are lagged by one quarter, except for *Cash flow (firm)*, which is contemporaneous with investment. In column 1, the sample includes all banks in our dataset. In column 2, the sample of banks includes the banks from the estimation sample of column 11 of Table 6 (also listed in Table 1). In column 3, the (discontinuity sample) includes those firm-quarters with an absolute distance from the (tangible) net worth or current ratio covenant threshold below 0.2. Standard errors are clustered as indicated below. The *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Investment		
	(1)	(2)	(3)
Violation	-0.024	-0.026	-0.046
	(-0.77)	(-0.81)	(-1.29)
Viol. × Leverage	-0.078	-0.061	0.011
	(-1.23)	(-0.97)	(0.14)
Viol. × Tier 1	0.324***	0.331***	0.812***
	(2.91)	(3.01)	(4.46)
Viol. × Deposits	0.001	0.001	-0.021
	(0.04)	(0.02)	(-1.24)
Viol. × Short-term funding	0.002	0.004	-0.000
	(0.09)	(0.15)	(-0.01)
Viol. × ln(Assets)	0.001	0.001	-0.001
	(0.38)	(0.48)	(-0.64)
Viol. × Non-interest income	-0.018*	-0.019*	-0.028**
	(-1.92)	(-1.97)	(-2.42)
Viol. × Trading	0.006	0.007	-0.008
	(0.25)	(0.30)	(-0.36)
Viol. × Non-performing assets	0.312	0.307	0.571
	(1.10)	(1.02)	(1.32)
Viol. × Net income	0.560	0.596	0.969
	(0.76)	(0.78)	(0.77)
Viol. × Cost-to-income	-0.004	-0.005	0.023
	(-0.45)	(-0.54)	(1.48)
Tobin's q (firm)	0.025***	0.025***	0.013**
	(6.94)	(6.98)	(2.20)
Cash flow (firm)	0.010**	0.009**	0.013*
	(2.56)	(2.52)	(1.86)
ln(Assets) (firm)	-0.011***	-0.011***	-0.000
	(-2.77)	(-2.73)	(-0.04)
Default distance (NW)	0.000	0.000	0.005
	(0.31)	(0.31)	(0.85)
Default distance (CR)	0.008	0.008	0.019
	(1.23)	(1.22)	(0.78)
Default distance (NW)^2	0.000	0.000	-0.000
	(0.42)	(0.47)	(-0.49)
Default distance (CR)^2	-0.000	-0.000	-0.016
	(-0.12)	(-0.10)	(-1.12)
Firm FE	Yes	Yes	Yes
Bank × Quarter FE	Yes	Yes	Yes
Fiscal quarter FE	Yes	Yes	Yes
Observations	18,881	18,419	4,137
Adjusted R^2	0.415	0.415	0.558
Number of banks	63	50	52
Mean dep. var.	0.056	0.056	0.051
Clustering	Bank-quarter	Bank-quarter	Bank-quarter
Sample selection	All banks	Table 1's banks	Discontinuity (< 0.2)
Sample period	1994-2012	1994-2012	1994-2012

Appendix for

"Benign Neglect of Covenant Violations: Blissful Banking or Ignorant Monitoring?"

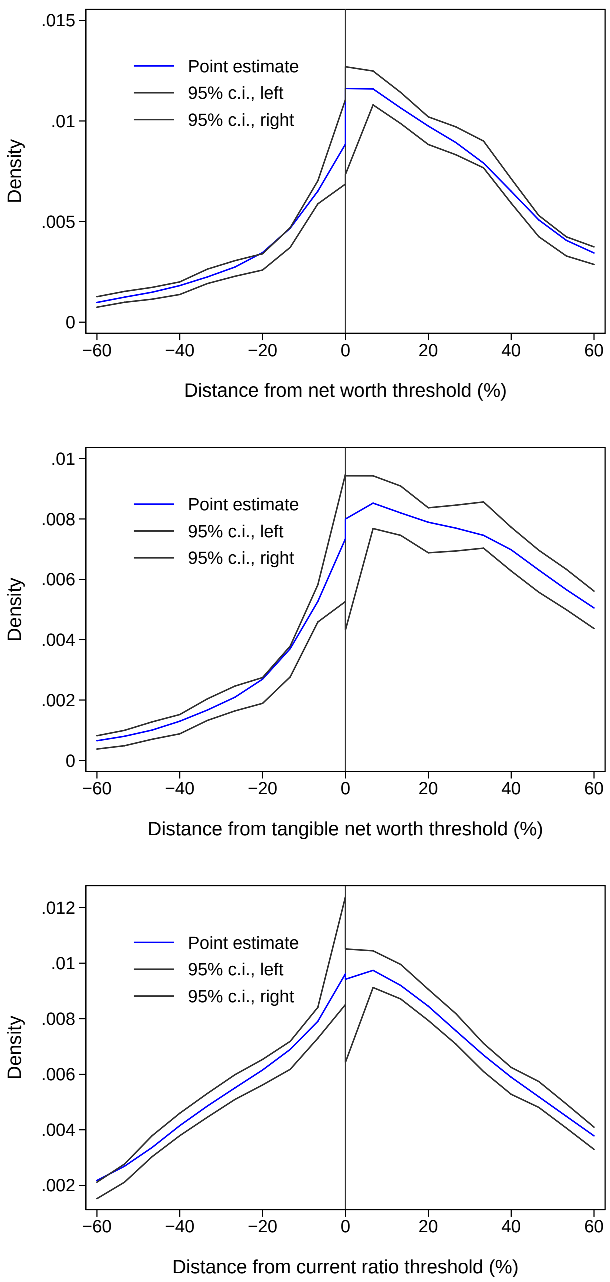

Figure A.1: Manipulation tests
This figure shows a density plot of the relative distance of a firm's accounting variable in a given quarter to the respective covenant threshold in the loans in our sample. The top graph shows the density plot for net worth covenants. The middle graph shows tangible net worth covenants. The bottom graph shows current ratio covenants. The point estimate and the confidence intervals are based on the smooth local polynomial density estimator by Cattaneo et al. (2017) and a bandwidth of 0.2.

Table A.1: Definition of variables

Variable	Databases	Definition
Borrowing firm variables:		
Tobin's q (firm)	CCM	Market value of equity plus book value of debt over book value of asset.s
Cash flow (firm)	CCM	Income before extraordinary items plus depreciation and amortization over last quarter's property plant and equipment.
Investment (firm)	CCM	Capital expenditures over last quarter's property, plant and equipment.
ROA (firm)	CCM	Income before extraordinary items over total assets.
ln(Assets) (firm)	CCM	Natural logarithm of the firm's total assets. Total assets are expressed in millions of 2010 dollars.
Leverage (firm)	CCM	Debt in current liabilities plus long-term debt over total assets.
Current ratio (firm)	CCM	Current assets over current liabilities.
Net worth (firm)	CCM	Total assets minus total liabilities (current liabilities plus long-term debt plus deferred taxes and investment tax credit plus other liabilities).
Tangible worth (firm)	CCM	Current assets plus other assets plus property plant and equipment minus total liabilities.
Loan-related variables:		
Violation	CCM and Dealscan	Indicator equal to one if a firm violates at least one of the covenants specified in the deal. We consider current ratio, net worth and tangible net worth covenants. For details on the type of violations we consider, see chapter A.1.
Default distance current ratio	CCM and Dealscan	The relative distance of a firm's current ratio to the threshold specified in a certain deal. This distance is defined as $(z_{f,q} - z^0_{l,q})/z^0_{l,q}$ where $z_{f,q}$ is the observed value of the accounting measure restricted by the covenant and $z^0_{l,q}$ is the covenant threshold contained in the syndicated loan contract for a specific quarter. The distance is set to zero if the deal is not bound by a current ratio covenant.
Default distance net worth	CCM and Dealscan	The relative distance of a firm's net worth or tangible net worth to the respective threshold specified in a certain deal. The distance is set to zero if the deal is not bound by a net worth covenant.
Violation (firm)	CCM and Dealscan	Indicator equal to one if a firm violates at least one of the covenants specified in any deal in a certain firm-quarter. The deal-level violations are defined as in the case of *Violation*.
Default distance current ratio (firm)	CCM and Dealscan	The relative distance of a firm's current ratio to the most binding covenant in a certain firm-quarter. This distance is defined as $(z_{f,q} - z^0_{f,q})/z^0_{f,q}$ where $z_{f,q}$ is the observed value of the accounting measure restricted by the covenant and $z^0_{f,q}$ is the most binding covenant threshold contained in any active loan contract. The distance is set to zero if the firm is not bound by a current ratio covenant.
Default distance net worth (firm)	CCM and Dealscan	The relative distance of a firm's net worth or tangible net worth to the most binding covenant in a certain firm-quarter. The distance is set to zero if the firm is not bound by a net worth covenant.
Facility amount	Dealscan	The total volume of a certain facility within a loan deal expressed in millions of 2010 dollars.
Deal amount	Dealscan	The total volume of a certain loan deal (package) expressed in millions of 2010 dollars.
All-in-drawn spread	Dealscan	The amount in basis points a borrower pays over the LIBOR for every dollar drawn.
Syndicate size	Dealscan	The number of lenders per loan.
Lending bank variables:		
Leverage	Compustat Banks and Bankscope	Common equity over total assets.
Tier 1	Compustat Banks and Bankscope	Risk-adjusted Tier 1 capital ratio.

(Continued)

Table A.1: – *Continued*

Deposits	Compustat Banks and Bankscope	Total deposits over total assets.
Short-term funding	Compustat Banks and Bankscope	Other short-term borrowings, securities sold under repurchase agreements, and commmercial paper over total assets. (Bankscope: Deposits from banks, repos and cash collaterals and commercial paper over total assets.)
ln(Assets)	Compustat Banks and Bankscope	Natural logarithm of the bank's total assets. Total assets are expressed in millions of 2010 dollars.
Non-interest income	Compustat Banks and Bankscope	Total non-interest income over the sum of net-interest-income and total non-interest income.
Trading	Compustat Banks and Bankscope	Trading/dealing account securities over total assets. (Bankscope: All securities and assets held for trading, excluding derivatives, over total assets.)
Non-performing assets	Compustat Banks and Bankscope	Total non-performing assets (impaired loans for Bankscope) over total assets.
Net income	Compustat Banks and Bankscope	Net income over total assets.
Cost-to-income	Compustat Banks and Bankscope	Total non-interest expense over the sum of net interest income and total non-interest income as defined by Bankscope and FRED's aggregate cost-to-income series for US banks.
Affected (SCAP)	Greenlaw et al. (2012)	Equity issuance in the three months after the publication of the SCAP results on May 7, 2009 (scaled by 2008 total assets).
TARP	US Treasury	Total forced capital injections as part of the TARP program (scaled by 2007 total assets).
Deposit insurance reform	Demirgüç-Kunt et al. (2005), Demirgüç-Kunt et al. (2014) and Schich (2009)	Country-year indicator that starts as 0 in the first year of the sample and increases by 1 for each reform increasing deposit insurance coverage.
Affected (EESA)	Lambert et al. (2017)	Indicator variable equal to 1 if the bank's change in the ratio of insured deposits to total assets induced by the EESA is above the 75th percentile, and zero otherwise.

Table A.2: Missing bank-years

This table lists bank-years for which we are not able to estimate $\hat{\beta}_{b,y}$ in the first-step specification (5) for the banks from the estimation sample of column 11 of Table 6 (also listed in Table 1) – excluding the year 2003 for Bank of America, which is our reference bank-year.

Bank name	Missing years
Bank of America	–
JP Morgan Chase	–
Wells Fargo	1995
Wachovia (active until 2008)	–
Bank One Corp (active until 2004)	1994
ABN Amro Bank (active until 2007)	1994, 1995
U.S. Bancorp	1994, 1995, 1996
Fleet Bank, later Fleet Boston (active until 2004)	1994, 1995
Comerica	1994, 1995, 1996
BNP Paribas	–
SunTrust Bank	1996
PNC	–
BNYM	2010, 2011, 2012
Bank of Montreal	–
Citigroup	1998
KeyBank	1994, 1995, 1996
Deutsche Bank	1994, 1995, 1997
National City (active until 2008)	1994, 1995, 2005
Bank of Nova Scotia	2004
Mellon Bank (active until 2007)	2004, 2005
Royal Bank of Scotland	1998, 1999, 2002, 2003, 2004, 2006
Wachovia (old, active until 2000)	1997
Société Générale	–
Royal Bank of Canada	2004
Northern Trust	1995, 1996, 2004, 2010, 2012
Barclays Bank	1994, 1995, 1996, 1999, 2003, 2004, 2005
Fifth Third Bancorp	1995, 1997, 1998, 1999
SVB	1998, 1999, 2001, 2006, 2011, 2012
JP Morgan (active until 2000)	1994, 1995
HSBC	–
BBVA	–
TD Bank	2004, 2009, 2010, 2012
Compass Bank	1994, 1996, 2000
Hibernia National Bank	1995, 1996, 1997
Regions	1999, 2000, 2001, 2007, 2008, 2010, 2011
CIBC	2004, 2005, 2007, 2008, 2010, 2011, 2012
State Street	1995, 1996, 1997, 2001, 2004, 2005, 2007, 2008, 2010, 2011, 2012
AmSouth Bank	1995, 2001, 2003
Huntington National Bank	1994, 1995, 1997, 1998, 2001, 2004, 2005, 2006, 2007, 2008, 2009, 2010, 2011
M&T Bank	1996, 1997, 2000, 2002, 2004, 2005, 2006, 2007, 2008, 2010, 2011, 2012
Bb&T Bank	2002, 2004, 2006, 2007, 2009, 2010, 2012
Zions First National	1997, 1998, 2000, 2001, 2003, 2004, 2005
Bank of Hawaii	1996, 1997, 2000, 2002, 2003, 2004, 2005, 2006, 2007, 2008, 2010, 2011
Provident Bank (active until 2004)	1994, 1995, 1998
Commerce Bank (active until 2008)	1996, 1997, 1998, 1999, 2003
SouthTrust Bank (active until 2004)	1999, 2000, 2001
M&I Bank (active until 2011)	1999, 2001, 2002
Lloyds Bank	2002, 2003, 2004, 2005, 2008, 2009
Bank of the West	1995, 1996, 1997, 1998, 2000
Associated Bank	2002, 2003, 2004, 2005, 2006, 2007, 2008, 2010, 2012
First Merit Bank	1999, 2000

Table A.3: Monitoring and bank characteristics (alternative approach) – Broader discontinuity sample
This table reports estimates from the one-step RDD specification (8) for investment of borrowing firms around covenant violations. The sample covers the period 1994-2012 and has a deal-bank-firm-quarter structure. The dependent variable is the borrowing firm's investment rate. The explanatory variables include the binary (0/1) covenant violation indicator, its interaction with bank time-varying characteristics, firm time-varying characteristics, and polynomials of distance measures from the covenant threshold. All independent variables are lagged by one quarter, except for *Cash flow (firm)*, which is contemporaneous with investment. The (discontinuity sample) includes those firm-quarters with an absolute distance from the (tangible) net worth or current ratio covenant threshold below 0.4. Standard errors are clustered as indicated below. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Investment
	(1)
Violation	-0.085**
	(-2.18)
Viol. × Leverage	0.051
	(0.91)
Viol. × Tier 1	0.626***
	(4.26)
Viol. × Deposits	0.033
	(1.41)
Viol. × Short-term funding	-0.010
	(-0.42)
Viol. × ln(Assets)	0.001
	(0.64)
Viol. × Non-interest income	-0.028**
	(-2.07)
Viol. × Trading	0.039
	(1.57)
Viol. × Non-performing assets	-0.257
	(-0.66)
Viol. × Net income	0.333
	(0.26)
Viol. × Cost-to-income	0.014
	(0.99)
Firm control variables	Yes
Polynomials	Yes
Firm FE	Yes
Bank × Quarter FE	Yes
Fiscal quarter FE	Yes
Observations	8,352
Adjusted R^2	0.505
Number of banks	59
Mean dep. var.	0.053
Clustering	Bank-quarter
Sample selection	Discontinuity (< 0.4)
Sample period	1994-2012