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

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Abstract

Existing theories posit that a bank's monitoring activity over loans crucially hinges on its funding structure and business model. 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 the granular structure of a sample linking syndicate banks to borrowing firms, we observe substantial heterogeneity in monitoring across banks and through time. We uncover the important role of equity capital in shaping bank monitoring intensity. More capitalized banks are more lenient monitors, and this is associated with improved borrowers' performance.

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. The theoretical literature has long investigated under which conditions banks act as effective monitors (see, e.g., Holmstrom and Tirole, 1997; Diamond and Rajan, 2001; Greenwood, Sanchez, and Wang, 2010). More recently, 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 supply-side determinants of monitoring identified as important by existing theories, such as a bank's funding structure or business model.

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 $2.41 trillion in 2017 (see, e.g., Sufi, 2007).[1] Relying on creditors' interventions in the borrowing firms' management following covenant violations as a measure of monitoring intensity, we document the existence of substantial cross-sectional and time-series variation in bank monitoring. Among balance sheet measures of a bank's funding structure (equity capital and debt composition) and business model (scope of services provided, lending technology, and efficiency), only the risk-adjusted Tier 1 ratio capital exhibits a strong relation with the bank's monitoring activity. More capitalized banks keep a more lenient monitoring stance towards troubled borrowers. We find that this lenient stance is linked to improved borrowers' performance, i.e., rather than being inefficiently distracted, well-capitalized banks allow borrowers to keep pursuing value-increasing projects also when they violate a covenant. Apart from Tier 1 capital, other supply-side determinants of monitoring activity emphasized by existing theories appear to have only limited explanatory power with respect to heterogeneity in bank monitoring.

In our empirical analysis, we use a granular dataset linking syndicate banks to US public borrowing firms over the period 1994-2012. To measure bank monitoring, we build on Chava and Roberts (2008), who provide 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. Our granular data structure allows

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

us to tease out bank-time-specific effects from borrowing firms' changes in investment following covenant violations. We rely on these bank-time-specific investment effects as a measure of bank monitoring intensity.

Bank monitoring intensity, while highly heterogeneous through time and across banks, appears to be to a large extent non-cyclical. Interestingly, dispersion in monitoring intensity across banks goes through cycles, which, however, do not seem related to the state of the economy. These findings are hard to reconcile with theories predicting important interactions between monitoring incentives and the business cycle.

We then relate our bank-time-specific monitoring measure to bank balance sheet characteristics, such as equity capital (leverage ratio and risk-adjusted Tier 1 ratio), debt composition (reliance on deposit funding and short-term), scope of activities (size, non-interest income, trading activity), and monitoring technology and efficiency (non-performing assets, profitability, and cost-efficiency). These bank characteristic explain a low fraction of variation in bank monitoring. Moreover, most of them do not exhibit a statistically significant correlation with it. The only bank characteristic for which we find a clear pattern is the risk-adjusted Tier 1 capital ratio. More capitalized banks keep a looser stance after covenant violations, which contradicts the argument that equity favors monitoring by giving bankers more "skin in the game". Rather, equity seems to provide banks with a "buffer" to absorb future negative shocks and allows them not to constrain borrowers' investment policy. We provide evidence that this looser stance is associated with improved borrowers' performance, pointing to an efficiency-enhancing role of equity capital rather than to a lender distraction story. Finally, the role of the bank balance sheet characteristics listed above does not vary meaningfully through the business cycle.

All in all, we conclude that bank heterogeneity in monitoring may be to a large extent driven by supply-side determinants not yet captured by existing theories. Among the bank traits we analyze, only equity capital is an important determinant of bank monitoring. More capitalized banks are less likely to impose inefficient investment cuts on borrowers.

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] Closer to this study is the work of Gustafson et al. (2017), who 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 banks' funding structure and business model for monitoring heterogeneity.

Finally, our paper fits in the literature linking lending bank health to borrowers. The studies most closely related to ours are Chodorow-Reich and Falato (2018) and Acharya, Almeida, Ippolito, and Perez-Orive (2016a). Both studies 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 and focusing on credit lines. These two studies are essentially examining 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. Both studies concentrate on establishing a causal relation lenders' and borrowing firms' health. Whereas we abstain from causal inference in this respect, our study has a broader focus and tests whether there are any bank characteristics that consistently explain differences in monitoring using the entire range of available data.

[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.

2 Theoretical background

The theoretical literature has analyzed several bank-level determinants of monitoring effort as well as how such an effort relates to business cycle conditions.

Several theories link monitoring effort to bank characteristics. Relevant characteristics studied in the literature can be grouped in two main categories: The funding structure and the business model of the bank.

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

[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] Huang and Ratnovski (2011), building on the model of Calomiris and Kahn (1991), develop a model

Also the business model of a bank can impact its monitoring activity. Broadly speaking, 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).[7]

One key technological development that has taken place in the banking industry over the last three decades is the transition to the so-called 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 has important implications for monitoring. In a costly-state verification framework, Greenwood et al. (2010) model a bank's monitoring as a function of its technology and the resources allocated to it. Monitoring effectiveness increases in both quantities.

Finally, it is 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 limited.[8] 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 and business model 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.

in which banks rely on short-term wholesale funding besides retail deposits. They show that wholesale financiers have reduced monitoring incentives over the bank, but do not extend the analysis to the monitoring over borrowers by the bank.

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

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

3 Empirical approach

In this section, we start by discussing the economic intuition behind our approach to the measurement of banks' monitoring intensity in the context covenant violations. We then describe the different empirical approaches we follow to measure monitoring intensity and to relate it to banks' characteristics as well as to business cycle conditions.

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, however, is inherently elusive, often taking place behind closed doors. Several studies in the literature thus 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), for instance, 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. However, it is challenging to impute changes in borrowing firms' policies to banks' monitoring actions. To this end, we look at instances in which banks are especially likely to take monitoring actions. More specifically, 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, in line with Bird, Ertan, Karolyi, and Ruchti (2017a) and Bird, Ertan, Karolyi, and Ruchti (2017b).

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, 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. However, in most cases, creditors start a debt renegotiation and use the threat of such actions to extract concessions from borrowers (Roberts, 2015). The concessions extracted by creditors 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 thus covenant violations provide 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 automatically 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

[9] Loan contracts may include also affirmative covenants (requiring the borrower to take specific actions, such as complying with certain regulations) and negative covenants (prohibiting the borrower from taking certain actions, such as asset sales): See Nini et al. (2012) for further institutional details about loan covenants. We only look at financial covenants and, for brevity, in the remainder of the paper we refer to them simply as covenants.

of the technical default can reflect a host of bank-side actions, it seems sensible to think that such actions capture also "pure" monitoring. An inattentive bank monitor is unlikely to cause a substantial contraction in investment, keeping rather a loose stance towards violating firms.

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, 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. In other words, our approach complements that of Gustafson et al. (2017), who have direct measures of monitoring and study their correlation with future loan outcomes (considering also covenant violation). By contrast, our measure of monitoring – or more generally, of bank actions – is indirect, but we pin down plausibly causal estimates of its effects on borrowing firms. However, when we then relate bank monitoring to bank characteristics, the goal is not to establish a causal link. Rather, we test the consistency of observed correlations among equilibrium quantities with those predicted by existing theories.

3.2 Investment and covenant violations

As a preliminary step in our 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 bank heterogeneity (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 tease out 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 if the firm violates any covenant in any of the loans it has taken. 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 control variables $\mathbf{x}_{f,q-1}$ 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 then repeat the same analysis of investment around covenant violations but on a different data structure, which we also use in our main analysis below to tease out heterogeneity in investment responses depending on the bank from which the firm borrowed. We treat each syndicated loan as a set of separate loans, one for each bank in the syndicate, so that the unit of observation is the loan-bank-firm-quarter. We carry out a

[10] As we discuss below, we focus on syndicated loans containing covenants on (tangible) net worth or the current ratio as in Chava and Roberts (2008).

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 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, we allow for 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).

As Chava and Roberts (2008), in the estimation of both specifications (1) and (3) we exclude 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 code as such those covenant violations that happen right at the beginning of a loan's lifetime. This allows us to improve comparability

[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.

[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.

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 triggers a covenant for the first time for a given loan, we require at least four quarters without a violation before we code another violation 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. The purpose of all these adjustments is to restrict the attention to firms that enter a certain syndicated loan and after some time violate one covenant contained in the contract.

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 (i.e., technical default). We now present two alternative approaches to augment specification (3) and study bank heterogeneity, which lies at the core of our analysis. The ultimate aim is to analyze bank-time-specific borrowers' investment responses to covenant violations.

3.3.1 Two-step approach

The first approach consists of two steps. In the first step, we estimate the following 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 allow us to pin down 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 for our second-step analysis. In particular, we study the relation between $\hat{\beta}_{b,y}$ and several bank characteristics that existing theories identify as important determinants of monitoring intensity. To this

[13] Ideally, we would interact $v_{l,q-1}$ with bank-quarter fixed effects rather than bank-year fixed effects. However, often times small banks experience only very few covenant violations in a specific quarter, so that investment responses linked to them are too correlated with the investment responses linked to big players in the market. As a result, the estimation procedure cannot estimate many bank-quarter-specific violation coefficients.

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 characteristics capturing the level of equity capital (leverage ratio, risk-adjusted Tier 1 capital ratio), debt composition (deposits and short-term funding), 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 at annual frequency. 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 arguably provides simple correlations. Indeed, as pointed out by Chodorow-Reich and Falato (2018) in a similar setting, to interpret $\boldsymbol{\Gamma}_{b,y-1}$ 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). However, the second step does not aim to provide causal estimates. Our goal is rather to verify if observed correlations between equilibrium quantities (bank monitoring vs. bank characteristics) line up with those predicted by existing theories.

There are two caveats about our two-step approach. First, whereas in equation (7) we cluster standard errors by bank, 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 has 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. This, in turn, can lead to limited statistical power and to an

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

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

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:

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

where $\boldsymbol{\Gamma}_{b,q-1}$ is a vector of bank time-varying characteristics (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. Again, as in the second-step in specification (7), we are interested in the vector of coefficients $\boldsymbol{\theta}$.

The main disadvantage of this approach relative to the two-step procedure is that it directly assumes the same relation between bank actions after technical defaults and $\boldsymbol{\Gamma}_{b,q-1}$ 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 $\boldsymbol{\Gamma}_{b,q-1}$.

4 Data

In this section, we describe our data sources, the selection of the sample, 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. We obtain syndicated loan data from the Thomson-Reuters' Loan Pricing Corporation DealScan (Dealscan) database. We use quarterly accounting and stock price data about US public firms from the CRSP-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 made available by Michael Roberts, which builds on the sample of Chava and Roberts (2008).

We use bank quarterly balance sheet data from Compustat Banks. We supplement Compustat Banks with Bankscope if some information is missing.[16] We link syndicated loans to information about the balance sheet 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 retain all syndicate members (and not only lead banks) in our sample. Finally, we retrieve data on US macroeconomic variables from Federal Reserve Economic Data (FRED), St. Louis Federal Reserve Bank.

The sample starts in 1994, namely the first year in which Dealscan provides fairly complete information about covenants (Chava and Roberts, 2008). We end the sample period in 2012 because this is the last year covered by the Dealscan-CCM link file of Michael Roberts. In line with Chava and Roberts (2008), 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 looking at whether the observed (tangible) net worth or current ratio complies with the contractual threshold.[18] While this approach may give back several false positives, it allows us to measure the distance between the accounting quantity and the covenant threshold, which helps ameliorate identification in the RDD.[19]

To infer heterogeneity in bank behavior, we treat each syndicated loan as a number of separate loans, i.e., a loan deal of a given borrowing firm with n different banks enters as n separate bank-firm deals.[20] Similarly to Schwert (2018), the deal-bank-firm triplet is the panel unit in our main analysis. Or to say it differently, while the dataset of

[16]More precisely, we resort to Bankscope only for the 20 most active lenders with missing information (namely Citi, BNP Paribas, and National City) to avoid losing an important source of variation in monitoring, whereas we leave missing information from smaller lenders as such.

[17]This is the link file used in Schwert (2018) and can be retrieved from Michael Schwert's personal webpage: https://sites.google.com/site/mwschwert/.

[18]See Section 3.2 for further details.

[19]The alternative would be to look at the universe of covenant violations reported in SEC filings, i.e., not only at those linked to Dealscan loans (Roberts and Sufi, 2009). Such an approach, however, does not allow the researcher to observe the distance from the covenant threshold.

[20]Note that in the empirical analysis, we refer to a loan deal simply as a "deal". A deal in Dealscan is a package of facilities. We disregard the facility level by only looking at the intersection of lenders for the small share of deals with unequal syndicate structures across facilities.

Chava and Roberts (2008) is on the firm-quarter level, our dataset is on the deal-bank-firm-quarter level. Consequently, our covenant violations are on the deal-quarter level, whereas violations in Chava and Roberts (2008) are on the firm-quarter level.

4.2 Variable construction and summary statistics

In our analysis, we rely on borrowing firm-level and bank-level time-varying characteristics. Concerning borrowing firm 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 look at non-performing 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 57.30% of all deals (64.72% of the total credit) extended by our sample banks.[21]

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

[21] To calculate these figures, we go back to the facility-level and use the procedure from De Haas and Van Horen (2013) to assign deal shares among facility members.

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 reduction in investment linked 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).

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 consistently the magnitude of the change in investment is also the same, i.e., -0.8%.[22] In columns 2, where we extend the analysis to our 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 be-

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

tween -0.3% and -0.2%. However, this is arguably a mechanic effect due to the fact 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 triggers deal 1's covenant. After t, the firm's current ratio continues declining and in period $t+2$ reaches 145%, which triggers 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), the most active lender in our sample which we use as the reference bank. Other banks, like Deutsche Bank, are significantly more lenient.

Given this prima facie evidence, we now move to the study of heterogeneity of the investment effect across banks and time, i.e., our proxy for bank monitoring intensity, which we relate to bank time-varying characteristics.

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 relation with bank characteristics 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 tease out 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 characteristics and business cycle conditions, which speak to existing theories on the determinants of bank monitoring.

6.1.1 *First step*

To tease out bank-induced heterogeneity in borrowers' investment response to violations through time, in column 1 of Table 4 we estimate specification (5). In this way, we obtain a vector of bank-year-specific coefficients capturing such heterogeneous effects in monitoring, 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.[23]

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 is $0.0175 - (-0.0071) = 0.0246$, 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 using 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 $\hat{\beta}^{q}_{b,y}$.[24] Again, looking at F-tests, there appears to be 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 $\hat{\beta}_{b,y}$.

Given the large size of the vector $\hat{\beta}_{b,y}$ obtained through the specification in column 1 of Table 4, rather than tabulating all the bank-year monitoring coefficients, we provide a visual analysis in Figure 2. In total, we are able to estimate 640 coefficients.[25] 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. Furthermore, other banks only start experiencing covenant violations in the late 1990s, possibly because Dealscan is less well-filled in the early years of our sample.

[23] 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 makes it a well-populated benchmark. The reference year, i.e., 2003, is the one with most observations in our sample (together with 2002, which, however, follows a NBER recession). Note that in Table 4 we do not report the coefficient estimate for the violation indicator, because that would only provide information on reactions to covenants in the reference bank-year, which is devoid of interest per se.

[24] 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.

[25] 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.

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 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 artifact of changes in business cycle conditions over our sample, as there appears to be substantial variation in bank monitoring coefficients within each single year, which reproduces 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

Given the heterogeneity documented in the first step, we now link it it to observable bank characteristics and business cycle conditions, which existing theories identify as important determinants of bank monitoring activities.

Before studying the correlation between the estimated monitoring coefficients and such covariates, we investigate those bank-years for which we are not able to estimate a coefficient.[26] Appendix Table A.2 provides a list of those instances, which are clustered in the early years of the sample, 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

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

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 for each of them a binary variable equal to one 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 non-interest income 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. 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.

Given this backdrop, we implement the second step of our two-step 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 $\mathbf{\Gamma}_{b,y-1}$, while the model in column 11 includes the entire vector of bank characteristics, and the model in column 12 only those characteristics that are significant on a stand-alone basis (Tier 1, total assets, total assets, non-interest income, non-performing assets, and net income). Only for Tier 1 and non-performing assets we find a statistically significant relation 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.[27]

Whether increased bank leniency linked to Tier 1 capital and non-performing assets is efficient or a symptom of distraction by bank monitors is an empirical question. We thus study how the bank interventions captured by the coefficients in $\hat{\beta}_{b,y}$ correlate with the borrowing firms' performance around the same covenant violation events. More specifically, in Table 7 we study 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 4.[28] In column 1, we uncover a positive and significant relation between $\hat{\beta}_{b,y}$ and $\hat{\beta}^{ROA}_{b,y}$. While this may seem at odds with the positive effect of covenant violations on ROA shown by Nini et al. (2012), this 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.[29] 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 are more lenient. This is also 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.

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.

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

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

[29] Note that in unreported regressions relying on 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, in line with Nini et al. (2012).

To further explore the business cycle properties of bank monitoring, in Table 8 we augment specification (7) with interactions between the bank variables in $\mathbf{\Gamma}_{b,y-1}$ and macroeconomic indicators (NBER recessions, NFCI, CFNAI). 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 a average of zero and positive (negative) values indicate tighter (looser) financial conditions. CFNAI (column 3) measures aggregate economic activity in the US. It has a average of zero and 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 relation with Tier 1 capital is not significantly different at different stages of the business cycle. This in line with the finding above that bank monitoring is to a large extent non-cyclical and inconsistent with theories predicting an important role for the business cycle.

Because in the second step we have coarse data and a relatively small sample, in Table 9 we reduce the dimension of the problem to capture overall bank quality and explain variation in monitoring across banks. Besides including Tier 1 capital – the only bank variable providing consistent results across different tests –, in columns 1, 2, and 3 we define 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, respectively. None of these "bad bank" measures exhibits a significant correlation with our bank monitoring coefficients, whereas Tier 1 retains a positive and statistically significant coefficient.

Overall, the results of the second step only provide evidence in favor of the equity buffer hypothesis. Better capitalized banks act more leniently towards violating firms, allowing them to invest at higher rates. This lenient stance is linked to improved performance by borrowing firms, pointing to its efficiency rather than to distraction or shirking of managers and loan officers of well-capitalized banks.

6.2 One-step approach

The lack of support for most existing theories 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 bias us against finding statistically significant correlations. We address these concerns through the one-step approach.

Table 10 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 0.20 as in Chava and Roberts (2008).[30] Column 3 repeats the regression from column 1 over the discontinuity sample. The starkest result is again the positive and significant interaction between risk-adjusted Tier 1 ratio and covenant violations, in line with the equity buffer argument. 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.

All in all, also the one-step approach yields support the role of equity in mitigating banks' monitoring responses to covenant violations.

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 and business model 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 violation, 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.

[30] 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.

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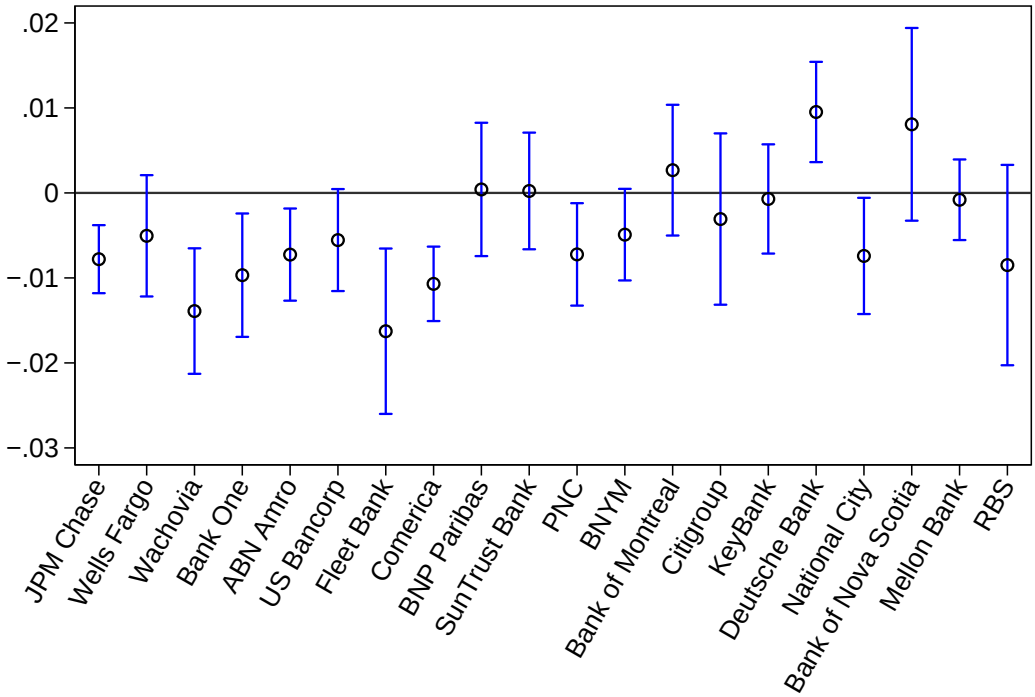

Figure 1: Distribution of time-invariant of monitoring across banks
This figure visualizes coefficient estimates $\hat{\beta}_b$ (with 95% confidence intervals) across banks. $\hat{\beta}_b$ are estimated coefficients from this specification: $I_{l,b,f,q} = \alpha \cdot v_{l,q-1} + \sum_b \beta_b \cdot v_{l,q-1} \times \gamma_b + \boldsymbol{\eta}\mathbf{x}_{f,q-1} + \boldsymbol{\zeta}\mathbf{p}_{l,q-1} + \gamma_b + \gamma_f + \gamma_q + \gamma_e + \epsilon_{l,b,f,q}$. 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.

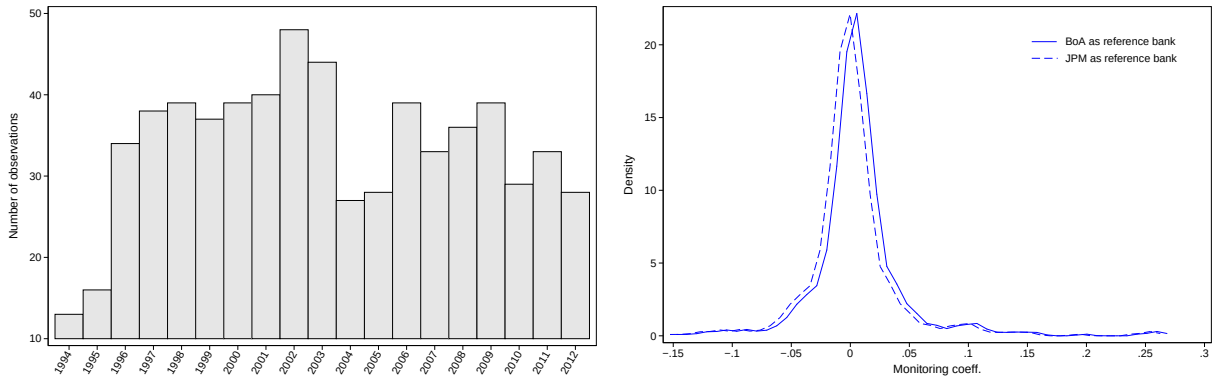

Figure 2: Distribution of bank monitoring
This figure visualizes the distribution of our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing 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.

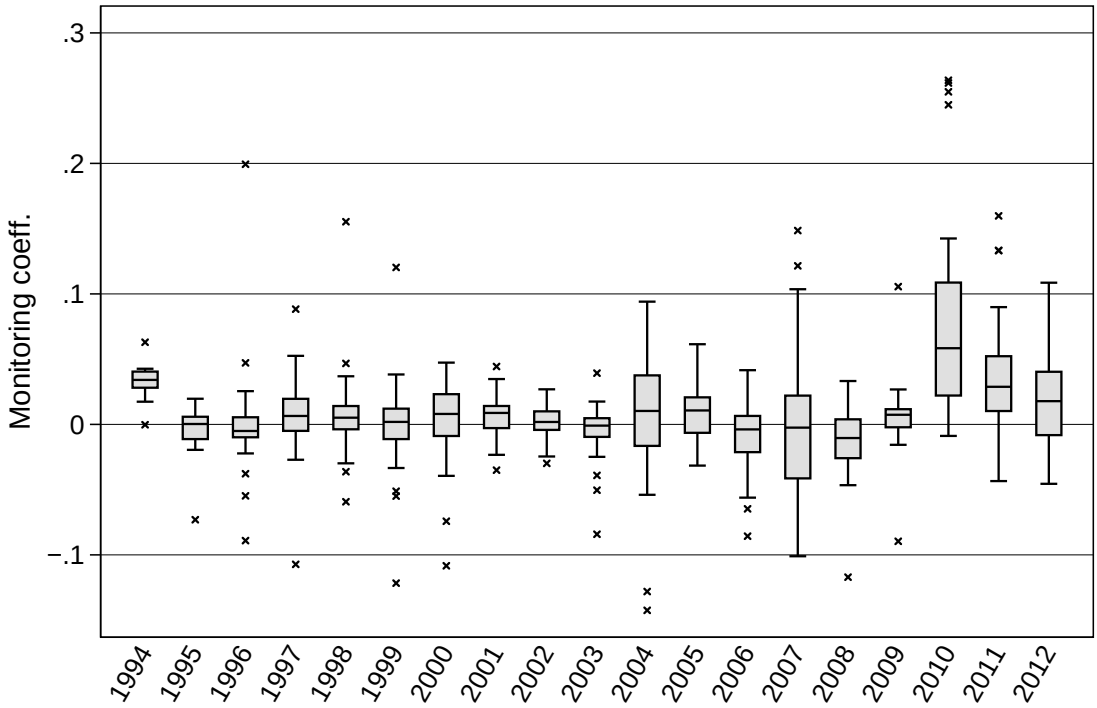

Figure 3: Distribution of bank monitoring through time
This figure visualizes the distribution of our bank monitoring measure $\hat{\beta}_{b,y}$ in each year of our 1994-2012 bank-year sample through box plots. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and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

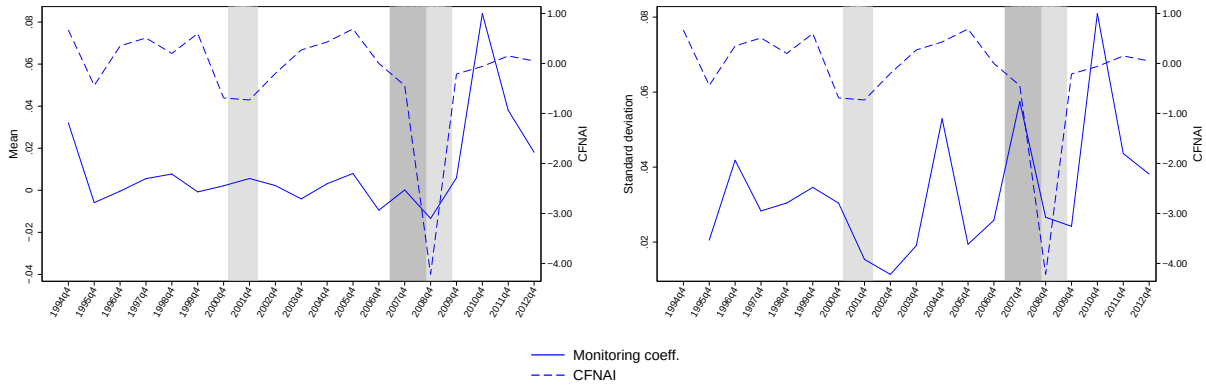

Figure 4: Bank monitoring through the business cycle
This figure visualizes the mean (left graph) and the standard deviation (right graph) of our bank monitoring measure $\hat{\beta}_{b,y}$ in each quarter between 1994 and 2012. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (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).

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: 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 8: 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 9: 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 10: 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?"

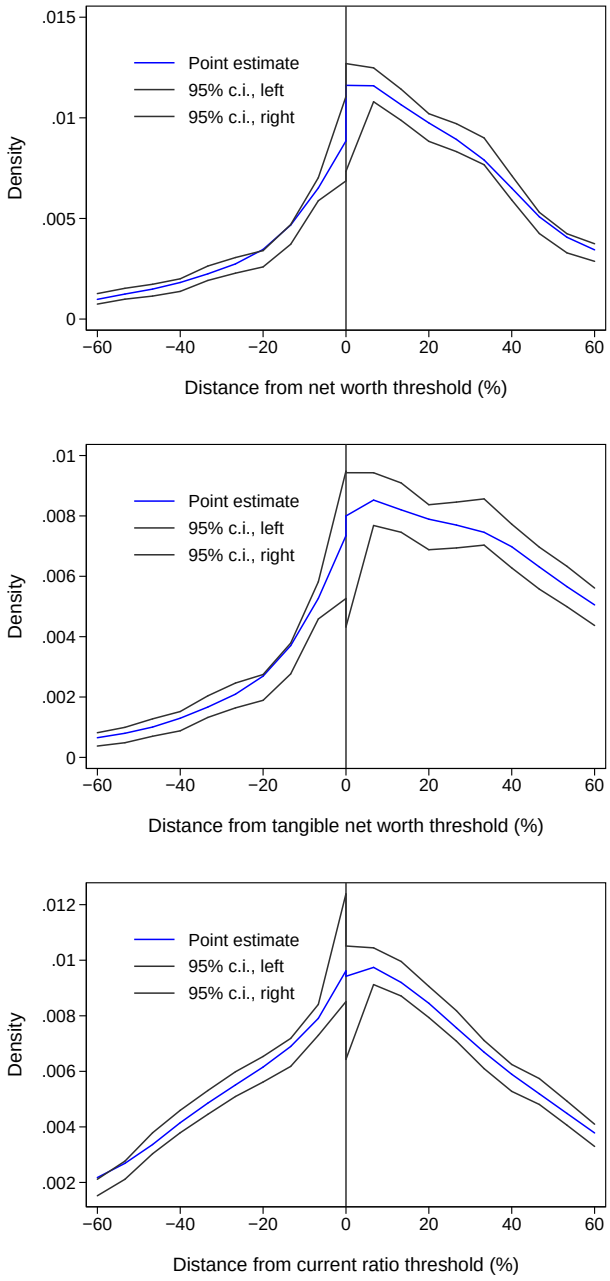

Figure A.1: Manipulation tests
This figure shows a density plot of the relative distance of a firm's accounting variable in a given quarter to the respective covenant threshold in the loans in our sample. The top graph shows the density plot for net worth covenants. The middle graph shows tangible net 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.

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, 20002
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