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BENIGN NEGLECT OF COVENANT VIOLATIONS: BLISSFUL BANKING OR IGNORANT MONITORING?

STEFANO COLONNELLO, MICHAEL KOETTER and MORITZ STIEGLITZ*

Theoretically, bank's loan monitoring activity hinges critically on its capitalization. To proxy for monitoring intensity, we use changes in borrowers' investment following loan covenant violations, when creditors can intervene in the governance of the firm. Exploiting granular bank-firm relationships observed in the syndicated loan market, we document substantial heterogeneity in monitoring across banks and through time. Better capitalized banks are more lenient monitors that intervene less with covenant violators. Importantly, this hands-off approach is associated with improved borrowers' performance. Beyond enhancing financial resilience, regulation that requires banks to hold more capital may thus also mitigate the tightening of credit terms when firms experience shocks. (JEL G21, G32, G33, G34)

I. INTRODUCTION

Loan monitoring and screening qualify banks as information producers and informed lenders. Numerous empirical determinants of monitoring have been explored, ranging from loan characteristics to business cycle conditions (Becker, Bos, and Roszbach forthcoming; Cerqueiro, Ongena, and Roszbach 2016; Gustafson, Ivanov,

and Meisenzahl forthcoming). But bank funding received little attention despite being a potentially crucial supply-side driver of monitoring. We fill this void by studying empirically banks' monitoring activity conditional on their capital (and debt) structure.

The relationship between a bank's reliance on equity capital and monitoring activity over loans is ex ante ambiguous. Equity may induce more intense monitoring if it mitigates moral hazard problems that entail too little effort by banks to exert scrutiny due to limited liability and reliance on deposit funding (Allen, Carletti, and Marquez 2011). Such a problem is mitigated by market discipline inducing banks to hold equity capital, which typically exceeds minimum regulatory requirements. Several other theoretical papers also predict a positive link between bank capitalization and monitoring intensity (Coval and Thakor 2005; Holmstrom and Tirole 1997; Jayaraman and Thakor 2014; Mehran and Thakor 2011). More generally, the "equity monitoring hypothesis" (Schwert 2018)

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ABBREVIATIONS

BoA: Bank of America
CCM: Center for Research in Security Prices/
Compustat Merged
OLS: Ordinary Least Squares
PPE: Property, Plant, and Equipment
RDD: Regression Discontinuity Design
ROA: Return on Assets
SCAP: Supervisory Capital Assessment Program
SNC: Shared National Credit Program

posits that bank capital alleviates the moral hazard problems inherent to the banking business by giving managers more “skin in the game” and thus motivating them to screen and monitor borrowers more diligently.

Alternatively, equity may reduce the bank’s incentives to monitor and intervene in the governance of the borrowing firm. 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 sufficiently large equity cushion to absorb increased capital requirements. We are not aware of formal theories that formulate exactly this “equity buffer hypothesis”, but a similar conjecture is put forward by Chava and Roberts (2008). This argument mirrors the one developed by Chodorow-Reich and Falato (2018): a tougher bank’s stance may reflect not only borrowers’ but also the bank’s declining financial health. By the same token, a better capitalized lender will be more lenient during borrowers’ distress.

We evaluate these two alternative hypotheses using the U.S. syndicated loan market as a laboratory. Syndicated loans are a primary source of funding for U.S. corporations, with a volume of \$2.4 trillion in 2017 (Sufi 2007).¹ Given pervasive reforms pertaining to capital and liquidity regulation (Hancock and Dewatripont 2018), we focus on relating monitoring intensity to bank funding structure measures in general and the role played by regulatory capital in particular.

Following Chava and Roberts (2008) we link syndicate banks to U.S. corporations to measure bank monitoring between 1994 and 2012. They show that borrowing firms cut investment after covenant violations because creditors intervene with the management of borrowers. Covenant violations provide a useful setting to study bank monitoring because they trigger a transfer of control rights from shareholders to creditors.

We document substantial cross-sectional and time-series variation in bank monitoring. Risk-adjusted Tier 1 capital ratios exhibit a statistically significant and large relationship with our monitoring metric. Better capitalized banks adhere to a more lenient monitoring stance towards troubled borrowers, which is associated with improved borrower performance.

Well-capitalized banks appear to permit borrowers the pursuit of value-increasing projects also when they violate a covenant. The result that better capitalized banks adhere to a “hands-off” approach after covenant violations contradicts the argument that equity favors monitoring by giving bankers more “skin in the game.” Instead, larger equity buffers seem to permit banks to smooth negative shocks of borrowers and avoid to constrain corporate investment policy. Improved borrower performance points, in turn, to an efficiency-enhancing role of bank equity rather than to a lender distraction story. Whereas it is commonplace to considerable monitoring a desirable activity, it can also be too much of a good thing. Carletti (2004) shows theoretically under which circumstances banks monitor borrower too much. Hence, a lack of equity capital may induce banks to demand inefficient investment cuts, a form of excessive monitoring.

To support a causal interpretation of this result, we exploit a quasi-experiment that provides plausibly exogenous variation in bank equity capital. The Supervisory Capital Assessment Program (SCAP or stress test) of 2009 forced a number of U.S. banks to issue equity immediately after the publication of results. We use this episode as a positive unanticipated shock to bank capitalization. The increase in equity induced banks to keep a looser monitoring stance in the years after the stress test. Thus, regulatory equity appears to “buffer” shocks and allows a benign treatment of covenant violators.

Another important facet of funding structure is the composition of its debt. Existing theories focus on the distinction between deposits and other forms of debt. Calomiris and Kahn (1991) and Diamond and Rajan (2001) argue that the threat of bank runs by depositors disciplines bankers. Therefore, banks relying heavily on deposit funding would have more incentives to monitor in our context (the “fragility monitoring hypothesis” in Schwert 2018).² The same economic mechanism may be at work for banks highly exposed to rollover risk on the wholesale short-term funding market.

We do not find evidence that predicting larger exposures to creditor runs induces bankers to exert more monitoring effort. Banks with a more fragile debt structure, that is, characterized by a

1. See <https://www.reuters.com/article/us-uslending-records/u-s-syndicated-lending-topples-records>.

2. Acharya, Mehra, and Thakor (2016) consider both the bright (loan monitoring) and the dark side (risk-shifting) of debt for banks, concluding that this trade-off can lead to multiple equilibria.

higher reliance on deposit or short-term funding, do not monitor their borrowers significantly more intensely after covenant violations.

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

This paper contributes to three strands of the literature. First, it relates to studies on the effect of covenant violations on corporate policies, such as investment (Chava and Roberts 2008), financing (Roberts and Sufi 2009), governance (Nini, Smith, and Sufi 2012), employment (Falato and Liang 2016), and board structure, see Ferreira, Ferreira, and Mariano (2018) for this last point and an overview of this literature. We study (bank) heterogeneity in creditor-induced investment reactions to covenant violations, which we use as a measure of bank monitoring intensity.³ Moreover, we investigate how covenant-violation induced investment reactions relate to changes in performance around the same events. We believe that the *joint* analysis of reactions of corporate policies to violations as opposed to the investigation of single measures in isolation is an important avenue to better understand the role of creditors in the governance of borrowing firms.

Second, we relate to empirical studies linking heterogeneity in bank monitoring to syndicate structure (Sufi 2007), collateral values (Cerqueiro, Ongena, and Roszbach 2016), securitization (Wang and Xia 2014), and business cycle conditions (Becker, Bos, and Roszbach forthcoming). Besides providing an overview of the literature, Gustafson, Ivanov, and Meisenzahl (forthcoming) use confidential regulatory syndicated loan data from the Shared National Credit (SNC) program 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, Plosser and Santos (2016) use expanded SNC data and find that a bank's role in the syndicate does not affect monitoring intensity. According to them, monitoring effort is determined by

the economic exposure of a bank, that is, the absolute value of a bank's individual loan share relative to a bank's size. We contribute to this literature by exploring the role of banks' funding structure for monitoring heterogeneity. Our findings clearly underpin that bank capitalization is a crucial supply-side determinant of monitoring compared to other bank traits, such as the bank's debt structure, business model, and efficiency.

Third, our paper complements the literature that links observable financial health indicators of lenders to borrower actions. Murfin (2012) shows that better capitalized banks design looser covenants. Whereas he considers equity-induced bank heterogeneity in loan contracting, we investigate how capitalization influences bank heterogeneity in responses to covenant violations. The studies most closely related to ours are Chodorow-Reich and Falato (2018) and Acharya et al. (forthcoming). Both use changes in bank balance sheet characteristics during the financial crisis to explain heterogeneity in bank responses to covenant violations. Using SNC data, Chodorow-Reich and Falato (2018) show that during the financial crisis lenders used covenant violations as an opportunity to cut credit exposure that otherwise would have been hard to reduce given loans' high average maturity. Acharya et al. (forthcoming) corroborate the findings of Chodorow-Reich and Falato (2018) using publicly available data on credit lines. These two studies examine one extreme of the whole spectrum of monitoring that we are considering. During a crisis, distressed banks may be less interested in intervening in the borrowing firms' management but rather want to implement lump-sum cuts in their loan book. Our study tests whether bank funding structure explains differences in monitoring looking over the entire business cycle, mitigating external validity concerns. Our results may thus provide guidance to policy-makers interested in designing regulation that brings banks closer to the optimal level of monitoring effort.

II. EMPIRICAL APPROACH

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

A. Bank Monitoring and Covenant Violations

The main goal of our analysis is to study how a bank's monitoring effort correlates with its

3. Roberts (2015) relates renegotiation outcomes after violations to *aggregate* banking sector leverage.

characteristics, insulating their role from that of the borrowing firm's characteristics.

Bank monitoring activity is inherently elusive. Most studies therefore measure it indirectly, assuming that certain features of the bank–borrower relationship (e.g., closer geographical distance or loan concentration among syndicate members) are conducive to more intense monitoring (see, e.g., Sufi 2007). Other, more recent studies take a different approach and look at observable monitoring activities.⁴

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

Financial covenants set limits on accounting-based measures of financial health and performance (e.g., on net worth or current ratio) of borrowing firms. Loan covenants are commonly maintenance-based. Debtors must comply with the limits set in the loan contract at the end of each fiscal quarter (Nini, Smith, and Sufi 2012). A covenant violation constitutes a technical default, after which the creditors can impose the immediate repayment (acceleration) or the termination of the loan. Creditors mostly use the threat of such actions to renegotiate the debt contract and extract concessions from borrowers (Roberts 2015).

According to the theoretical work by Gorton and Kahn (2000) and Berlin and Mester (1992), monitoring entails renegotiating loan terms upon the arrival of new information about the firm's prospects. In their models, covenants and their violation are a mechanism to institutionalize regular renegotiations. After a violation, a lender

can choose to liquidate certain projects of the borrower to prevent risk-taking. This is exactly what we are measuring in the form of restrictions on firm investment. More broadly, Nikolaev (2018) defines monitoring as both acquiring timely information about borrowers and acting upon that information to exert control on management. While monitoring measures such as loan reviews (Plosser and Santos 2016), site visits, and borrower meetings (Gustafson, Ivanov, and Meisenzahl forthcoming) entail only the first part of that definition, our measure incorporates both parts because the lender has to acquire information to detect the violation.

Chava and Roberts (2008) and Nini, Smith, and Sufi (2012) provide both anecdotal and large sample evidence consistent with increased monitoring following covenant violations (e.g., through increased frequency of required compliance reports). Whereas the change in investment policy linked to the resolution of the technical default can reflect a host of bank-side actions (typically changes in loan terms—interest rate, maturity, credit line availability, etc.—that make the borrower more financially constrained), it seems sensible to think that such actions capture also “pure” monitoring.

In sum, covenant violations provide a useful setting to study banks' monitoring activity for 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 providing a more complete picture of the role of creditors in borrowing firms (Nini, Smith, and Sufi 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 feature 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.

B. Investment and Covenant Violations

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

4. Gustafson, Ivanov, and Meisenzahl (2020) look at banks' meetings with borrowers and on-site visits. Cerqueiro, Ongena, and Roszbach (2016) and Becker, Bos, and Roszbach (2019) measure monitoring as the frequency of borrowers or collateral reviews. Plosser and Santos (2016) infer monitoring activity from changes to banks' internal borrower ratings.

commonly observed in the literature (Chava and Roberts 2008).

The borrowing firm's treatment status (violating vs. nonviolating) exhibits a discontinuity with respect to the distance between the observed accounting ratio and the contractual covenant threshold. We exploit this discontinuity for identification purposes in a RDD at the firm-quarter level in the spirit of Chava and Roberts (2008) to isolate the effect of financing frictions on investment as follows⁵:

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

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

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

where $z_{f,q-1}$ is the observed value of the accounting measure restricted by the covenant and $z_{f,q-1}^0$ is the most binding covenant threshold contained in any of the firm's outstanding syndicated loan contracts. In this firm-quarter-level analysis, $v_{f,q-1}$ equals one if the firm violates any covenant in any of the outstanding loans. For a given accounting measure, the *relative* distance $(z_{f,q-1} - z_{f,q-1}^0)/z_{f,q-1}^0$ is defined with respect to the tightest covenant threshold across the different outstanding loans at a given point in time. Thus, the assignment variable is the relative distance between the actual accounting measure and the threshold. Hence, a violation is not more severe simply because the level of the accounting measure and the corresponding threshold are relatively high to begin with.

We control for a vector of covariates $\mathbf{x}_{f,q-1}$ including Tobin's q , the contemporaneous cash flow, and the natural logarithm of total assets of the borrowing firm. We use a second-order polynomial of the relative distance of the different accounting measures from the tightest covenant

threshold to specify a vector of smooth functions $\mathbf{p}_{f,q-1}$ (Gelman and Imbens 2018). 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). Firm (γ_f) and time (γ_q) fixed effects absorb time-invariant differences in investment policy across borrowing firms and macroeconomic conditions. Error terms $\epsilon_{f,q}$ are clustered at the firm-level.

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

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

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

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

where the difference relative to the firm-quarter-level indicator (2) lies in the covenant threshold $z_{l,q}^0$, which is now loan-specific.⁶ 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. As before, we only observe borrowing firms' investment at the firm-quarter-level and the notation $I_{l,b,f,q}$ reflects the repetitive

5. This analysis is a sharp RDD because of the deterministic assignment rule into treatment and non-treatment. A caveat is that banks and firms can renegotiate the contract in anticipation of a violation. See Denis and Wang (2014) on firm policies after renegotiations outside of actual covenant violations.

6. Thus, we do not need to focus on the tightest covenant. Time-subscripts indicate dynamic covenant thresholds. 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.

nature of our data structure. Because of this feature, we use two-way clustering by bank and time in the error term $\epsilon_{l,b,f,q}$ in line with Schwert (2018).⁷

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

We follow Chava and Roberts (2008) and estimate both specifications (1) and (3) without firms that never violate any covenant, but deviate slightly 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.⁸ Second, we do not consider covenant violations as events that happen right at the beginning of a loan's lifetime. This approach allows us to improve comparability in terms of covenant design within our sample of loans by excluding those loans that are characterized by very strict covenants. Third, once a firm violates a covenant for the first time for a given loan, we require at least four quarters without a violation before we code another breach as a "new violation" in the same spirit as Nini, Smith, and Sufi (2012). In this way, we aim to capture instances in which there is an actual transfer of control rights from shareholders to creditors. Unreported tests show the (in)sensitivity of the main results vis-à-vis monitoring coefficients obtained after accounting for covenant violations satisfying different combinations of these sample restrictions. Results are available upon request.

C. Heterogeneous Effects of Covenant Violations across Banks

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

7. We estimate specifications with rich sets of fixed effects by means of the Stata package REGHDFE, which implements the estimator proposed by Correia (2016).

8. 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.

First, we use the variables defined as above to estimate the RDD specification:

$$(5) \quad I_{l,b,f,q} = \alpha \cdot v_{l,q-1} + \sum_b \sum_y \beta_{b,y} \cdot v_{l,q-1} \times \gamma_{b,y} \\ + \eta \mathbf{x}_{f,q-1} + \zeta \mathbf{p}_{l,q-1} + \gamma_{b,y} \\ + \gamma_f + \gamma_q + \gamma_e + \epsilon_{l,b,f,q}.$$

Relative to Equation (3), Equation (5) interacts $v_{l,q-1}$ with bank-year fixed effects ($\gamma_{b,y}$).⁹ The parameters of interest are $\beta_{b,y}$, which gauge the time-varying component of bank-specific treatment effects of covenant violations on investment.

In the second step, we specify the estimated coefficients $\hat{\beta}_{b,y}$ as the dependent variables to study the relationship between $\hat{\beta}_{b,y}$ and bank funding structure, controlling for bank's business model traits. The bank-year panel specification to estimate is:

$$(6) \quad \hat{\beta}_{b,y} = \psi + \theta \mathbf{\Gamma}_{b,y-1} + v_{b,y},$$

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

9. Ideally, we would interact $v_{l,q-1}$ with bank-quarter fixed effects rather than bank-year fixed effects. Yet small banks experience only very few covenant violations in a specific quarter. This can lead to situations where all covenant violations on loans extended by a small bank in a given quarter are happening for loans that were syndicated together with other, larger banks in our sample. In those cases, it is problematic to disentangle the role of small banks from that of large players in the market. Therefore, we cannot estimate many bank-quarter-specific violation coefficients. To alleviate this issue, we interact $v_{l,q-1}$ with less granular fixed effects at the bank-year level.

Whereas the first-step RDD estimates plausibly allow for causal inference on the (bank-time-specific) treatment effect of covenant violations on investment, the second step provides only correlations. As pointed out by Chodorow-Reich and Falato (2018) in a similar setting, to interpret $\Gamma_{b,y-1}$ 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 nonrandom matching (Schwert 2018).

Our solution is to conduct two quasi-experiments within the second-step estimation. To test the implications of bank equity and funding fragility for monitoring intensity, we exploit plausibly exogenous shocks to (a) equity capital resulting from the U.S. banks’ assessment in the SCAP stress test of 2009 and (b) exposure to bank runs following changes in the deposit insurance coverage around the world, respectively. These experiments scrutinize if the baseline correlation analysis between bank monitoring and funding structure supports a causal interpretation. We provide more details in Section C.

Two caveats concerning the two-step approach remain. First, whereas we cluster standard errors by bank in Equation (6), the dependent variable $\hat{\beta}_{b,y}$ is generated, which may require further corrections of standard errors because of measurement error (Dumont et al. 2005; Feenstra and Hanson 1999; Gawande 1997). Assuming that the measurement error ($\hat{\beta}_{b,y} - \beta_{b,y}$) is uncorrelated with the error term $v_{b,y}$, the ordinary least squares (OLS) estimator $\hat{\theta}$ is consistent, but suffers from inflated standard errors, possibly leading to an under-rejection of the null hypothesis of nonsignificance (Roberts and Whited 2013).¹⁰

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

Appendix Section 5 (Supporting information) presents a one-step approach addressing both caveats, which is less flexible although to study bank monitoring behavior. Therefore, we report in the remainder results from the two-step procedure.

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

III. DATA

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

A. Data and Sample Selection Procedure

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

We use bank quarterly balance sheet data from Compustat Banks, supplemented with Bankscope if information are missing for the 20 most active lenders. Syndicated loan and bank data are combined using the link file made available by Michael Schwert (2018). As a result, we focus on the 103 most active banks on the U.S. syndicated loan market, of which 87 are covered by Compustat Banks. Unlike most of the literature, we sample all syndicate members and not only lead banks. Macroeconomic data are retrieved from the Federal Reserve Economic Data, St. Louis Federal Reserve Bank.

The sample starts in 1994, which is the first year when Dealscan provides sufficiently comprehensive information about covenants (Chava and Roberts 2008). The sample runs until 2012, which is the last year covered by the Dealscan-CCM link file of Michael Roberts. We focus on Dealscan loans containing covenants on (tangible) net worth or the current ratio as in Chava and Roberts (2008) and build a matched quarterly panel of firms, which are assumed to be subject to a given covenant up to the maturity date of the corresponding loan. We identify covenant violations by testing if the observed (tangible) net worth or current ratio complies with the contractual threshold. This approach might result in some false positives, but enables us to measure the distance between the accounting quantity and

the covenant threshold to enhance identification in the RDD.

We treat each syndicated loan as a number of separate loans to gauge heterogeneous bank behavior, that is, a loan deal of a given borrowing firm with n different banks enters as n separate bank-firm deals. As in Schwert (2018), deal-bank-firm triplets are the panel unit of analysis to study quarterly covenant violations as opposed to firm-quarter level violations in Chava and Roberts (2008).

B. Variable Construction and Summary Statistics

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

To explain variation in monitoring intensity, we employ a host of bank characteristics contained in the vector $\Gamma_{b, y-1}$ of the second-step specification (6).¹¹ 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), noninterest income over total revenue (i.e., the reliance on nontraditional banking services), and assets held for trading scaled by total assets (i.e., the involvement in trading activities) relate to the range of activities the bank operates in. To proxy for the monitoring technology and overall efficiency of the bank, we specify nonperforming assets-to-total assets, net income-to-total assets, and the cost-to-income ratio. Table A.1 (Supporting information) provides the list of 51 banks for which all of these variables are

available for at least 1 year and can thus be included in the sample for the second-step estimation. These 51 banks still capture a large fraction of the market, namely 57.3% of all deals extended by our sample banks, calculated on the facility-level as in De Haas and Van Horen (2013) (64.7% of the total credit).

Finally, we measure U.S. macroeconomic conditions by using an indicator variable for National Bureau of Economic Research recessions, the National Financial Conditions Index, and the Chicago Fed National Activity Index.

Table 1 shows summary statistics for firm variables in and outside covenant violations (panel A and panel B, respectively), bank characteristics (panel C) and selected deal loan characteristics (panel D). Covenant violating firms exhibit lower investment, cash flows, and ROA than other firms. They are also smaller and more levered. On average, the loan syndicates in our sample comprise 5.21 institutions, and 95% of deals include at least one revolver loan, arguably a monitoring intensive credit type. 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 Table A.2 (Supporting information).

IV. INVESTMENT AND COVENANT VIOLATIONS

As a building block for our subsequent tests on bank heterogeneity, it is important to verify that we obtain the well-known result of a reduction in investment due to covenant violations (Chava and Roberts 2008; Nini, Smith, and Sufi 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 Figure A.1 (Supporting information) we implement manipulation tests of the running variables based on the smooth local polynomial density estimator of Cattaneo, Jansson, and Ma (2019), 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).

11. A caveat is the neglect of syndicate loan shares. Studies using publicly available datasets highlight the role of the lead arranger's loan share (see, e.g., Lee and Mullineaux 2004; Sufi 2007). But administrative data yields mixed evidence on whether the syndicate role (Gustafson, Ivanov, and Meisenzahl 2020) or rather participants' economic exposure (Plosser and Santos 2016) are key to explaining monitoring intensity.

TABLE 1
Summary Statistics

Panel A: Firm Characteristics in Covenant Violation Quarters						
	<i>N</i>	Mean	<i>SD</i>	<i>P25</i>	Median	<i>P75</i>
Tobin's <i>q</i>	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						
	<i>N</i>	Mean	<i>SD</i>	<i>P25</i>	Median	<i>P75</i>
Tobin's <i>q</i>	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	1,591.436	68.696	185.826	529.535
Tangible net worth	19,930	605.627	1,581.507	68.544	184.858	527.159
Panel C: Bank Characteristics						
	<i>N</i>	Mean	<i>SD</i>	<i>P25</i>	Median	<i>P75</i>
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
Noninterest 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
Nonperforming 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						
	<i>N</i>	Mean	<i>SD</i>	<i>P25</i>	Median	<i>P75</i>
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

Notes: This table shows summary statistics for our sample of U.S. 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 A.1 (Supporting information). Panel D reports summary statistics for syndicated loans. Refer to Table A.2 (Supporting information) for variable definitions.

Given this RDD validity check, Table 2 reports estimates of regression specifications studying the effect of covenant violations on borrowing firms' investment, without conditioning on the lending bank. In columns 1 and 2, we use the same firm-quarter data structure of Chava and Roberts (2008) and estimate Equation (1). Reassuringly, we find a statistically significant reduction in investment linked

to covenant violations. Column 1 focuses on the period 1994–2005—the same used by Chava and Roberts (2008)—and the estimated magnitude of the change in investment of −0.8% is consistent (column 7 of their Table V [panel A]). Column 2 extends the analysis to the entire sample period 1994–2012, yielding an effect that is only slightly smaller in magnitude.

TABLE 2
Investment and Covenant Violations

Dependent Variable	Investment			
	(1)	(2)	(3)	(4)
Violation (firm)	-0.008*** (-3.38)	-0.007*** (-3.15)		
Violation (deal)			-0.003 (-1.54)	-0.002 (-1.02)
Tobin's q (firm)	0.022*** (5.86)	0.022*** (6.81)	0.019*** (7.70)	0.022*** (8.72)
Cash flow (firm)	0.004 (1.03)	0.006** (2.00)	0.010*** (2.73)	0.009*** (2.77)
ln(Assets) (firm)	-0.007 (-1.52)	-0.009** (-2.46)	-0.012*** (-3.66)	-0.015*** (-4.06)
Default distance (NW)	-0.000 (-1.05)	-0.000 (-0.97)	0.001 (1.15)	0.001 (1.08)
Default distance (CR)	0.009** (2.56)	0.008*** (2.70)	0.016*** (3.82)	0.006 (1.03)
Default distance (CR) ²	-0.001*** (-3.21)	-0.000*** (-2.99)	-0.003*** (-3.21)	0.000 (0.13)
Default distance (NW) ²	0.000 (1.01)	0.000 (0.93)	-0.000 (-0.64)	-0.000 (-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

Notes: 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 Table A.2 (Supporting information) for variable definitions.

In columns 3 and 4, we resort to our repetitive deal-bank-firm-quarter data structure and estimate Equation (3). We still find a decline in investment following covenant violations, which is, however, statistically insignificant at conventional levels. The magnitude of the reduction over the deal-bank-firm-quarter data structure declines and ranges between -0.3% and -0.2% . This result is arguably a mechanic effect, which reflects that firms with multiple deals outstanding may be in violation of covenants for multiple deals at the same time. Consider, for example, 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

declines to 170% in period t , which violates deal 1's covenant. After t , the firm's current ratio declines further and reaches 145% in period $t + 2$, thus breaching 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 $t + 1$. The effect of the second violation between $t + 2$ and $t + 3$ is, in turn, 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 study next heterogeneous

investment effects across banks and time, that is, our proxy for bank monitoring intensity.

V. HETEROGENEOUS EFFECTS OF COVENANT VIOLATIONS ACROSS BANKS

The granular deal-bank-firm-quarter data structure allows us to scrutinize heterogeneity in monitoring and its relationship with bank funding structure and business cycle conditions. Our two-step approach consists of (a) a first step to isolate heterogeneous effects of covenant violations on investment across lending banks and time, and (b) a second step to correlate these effects with bank funding structure and business cycle conditions (controlling for other bank time-varying characteristics).

A. First Step

To tease out bank-induced heterogeneity in borrowers' investment response to violations through time, we estimate specification (5) in column 1 of Table 3. In this way, we obtain a vector of bank-year-specific coefficients that capture (heterogeneous) monitoring effects, namely $\hat{\beta}_{b,y}$. These coefficients measure the difference in the violation effect relative to the reference group, namely deals by Bank of America (BoA) in 2003.¹²

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

Columns 2 and 3 show results where we specify ROA and Tobin's q as dependent variables to obtain bank-year-specific effects of covenant violations on borrowing firms' accounting performance and market value: $\hat{\beta}_{b,y}^{ROA}$ and $\hat{\beta}_{b,y}^q$. The F -tests corroborates the existence of an important degree of heterogeneity across bank-years.

12. BoA is the reference bank, because it is most active in terms of number and volume of deals (Table A.1, Supporting information). Likewise, the reference year 2003 has most observations. Choosing the second-most active bank, JPM, leaves results intact. We do not report the coefficient estimate for the violation indicator in Table 3, because it provides only reaction information in the reference bank-year, which is devoid of interest per se.

TABLE 3
Investment, ROA, Tobin's q , and Covenant Violations

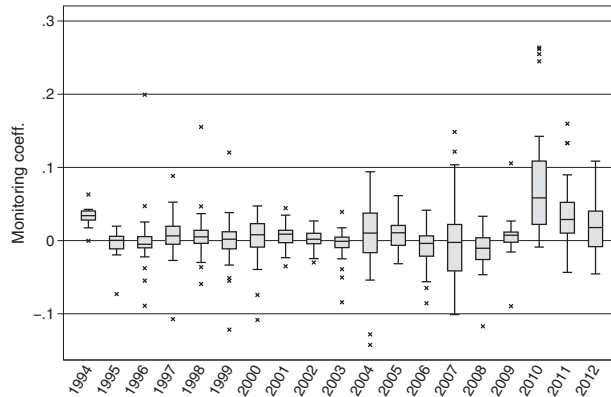
Dependent Variable	Investment (1)	ROA (2)	Tobin's q (3)
Violation \times Bank-year	Yes	Yes	Yes
FE			
F -test (statistic)	4,213.138***	3,879.250***	544.110***
F -test (p -value)	0.000	0.000	0.000
Tobin's q (firm)	0.022*** (7.59)	0.005*** (2.75)	
Cash flow (firm)	0.009** (2.34)	0.111*** (17.91)	0.153*** (3.82)
ln(Assets) (firm)	-0.015*** (-3.50)	-0.002 (-1.09)	-0.189*** (-4.99)
Default distance (NW)	0.000 (0.38)	-0.000 (-0.05)	0.032** (2.33)
Default distance (CR)	0.007 (1.02)	-0.003 (-0.53)	0.037 (0.97)
Default distance (NW) ²	0.000 (-0.10)	0.000 (0.95)	-0.001* (-1.87)
Default distance (CR) ²	0.000 (0.01)	0.000 (0.20)	-0.000 (-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	$\hat{\beta}_{b,y}$	$\hat{\beta}_{b,y}^{ROA}$	$\hat{\beta}_{b,y}^q$
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

Note: 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 Table A.2 (Supporting information) for variable definitions.

Below, we explore the correlation of $\hat{\beta}_{b,y}^{ROA}$ and $\hat{\beta}_{b,y}^q$ with our monitoring measure $\hat{\beta}_{b,y}$.

Given the large size of the vector $\hat{\beta}_{b,y}$ obtained from the specification shown in column 1 of Table 3, we provide a visual analysis in Figure A.2 (Supporting information) rather than tabulating all the bank-year monitoring coefficients. In total, we estimate 640 coefficients and the left graph of Figure A.2 (Supporting information) shows their distribution over time. Note that we do not obtain a balanced bank-year panel of monitoring coefficients for the second-step analysis. One reason is that several banks drop out

FIGURE 1
Distribution of Bank Monitoring Through Time



Note: 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.

of the sample early due to M&A activity, such as Bank One that was purchased by JPMorgan in 2004. Other banks only exhibit covenant violations as of the late 1990s. The right graph of Figure A.2 (Supporting information) shows the empirical density of the bank monitoring coefficients. Whereas the distribution peaks at 0%, we observe a substantial degree of heterogeneity.

To further explore bank heterogeneity, in Figure 1 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 the sample period. The resulting variation in bank monitoring coefficients within each single year is substantial. Annual distributions reflect what we observe over the entire sample, that is, 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.

B. Second Step

Next, we link the heterogeneity in monitoring documented in the first step to banks' funding structure in general and capitalization in particular given that the latter plays a central role

in many theoretical models of bank monitoring activities.

We implement the second step of the approach by estimating specification (6) and report coefficient estimates in Table 4. Columns 1–10 report univariate specifications for each of the bank characteristics contained in $\Gamma_{b,y-1}$, whereas the model in column 11 includes the entire vector of bank characteristics. The model in column 12 features only bank traits that exhibit univariate significance (Tier 1, total assets, noninterest income, nonperforming assets, and net income). Only for Tier 1, size, and nonperforming assets we find a statistically significant relationship with $\hat{\beta}_{b,y}$ in each specification.

The positive link between $\hat{\beta}_{b,y}$ and Tier 1 capital brings further support to the equity buffer hypothesis, whereas 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.¹³

13. Table A.3 (Supporting information) shows the bank-years without coefficient estimates from specification (5). Endogenous covenant design (Murfin 2012) may, inter alia, determine a lack of observed violations for a given bank-year leading to a missing coefficient estimate. In Table A.4 (Supporting information), we explore how the absence of such an estimate relates to bank observable characteristics. In line with the equity buffer argument, the association between the availability of $\hat{\beta}_{b,y}$ coefficient and Tier 1 capital is negative.

TABLE 4
Monitoring and Bank Characteristics

Dependent Variable	$\hat{\beta}_{h,y}$											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Leverage	-0.030 (-0.38)										-0.013 (-0.11)	
Tier 1		0.443*** (4.67)									0.410*** (3.03)	0.375*** (3.46)
Deposits			-0.011 (-0.76)								-0.008 (-0.20)	
Short-term funding				0.031 (1.50)							-0.014 (-0.30)	
In(Assets)					0.002** (2.04)						0.002 (1.15)	0.003* (1.88)
Noninterest income						-0.015* (-1.70)					-0.017 (-1.48)	-0.012 (-1.30)
Trading							0.002 (0.15)				0.003 (0.12)	
Nonperforming assets								1.302*** (6.18)			0.702** (2.52)	0.802** (2.57)
Net income									-1.259* (-1.69)		0.331 (0.30)	0.194 (0.28)
Cost-to-income										0.005 (0.50)	-0.001 (-0.07)	
Constant	0.010 (1.57)	-0.033*** (-3.88)	0.015 (1.53)	0.006*** (3.25)	-0.017 (-1.47)	0.015*** (3.58)	0.008*** (4.39)	-0.002 (-0.60)	0.011*** (4.53)	0.004 (0.67)	-0.049 (-1.05)	-0.061*** (-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.												
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

Note: This table reports estimates from the second-step OLS specification (6) over a 1994–2012 bank-year panel, where the dependent variable is our bank monitoring measure $\hat{\beta}_{h,y}$. $\hat{\beta}_{h,y}$ 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 1 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 Table A.2 (Supporting information) for variable definitions.

Also nonperforming assets correlate positively with $\hat{\beta}_{b,y}$, which suggests that banks with a worse screening technology are less strict as monitors since a higher $\hat{\beta}_{b,y}$ corresponds to looser monitoring. In contrast, our estimates do not support theories emphasizing the fragility of banks' funding to explain monitoring efforts.

The positive and statistically significant coefficient of the Tier 1 ratio is robust to (a) controlling for overall bank quality by means of indicator variables reflecting poor accounting ratios exhibited by a bank (Table A.5, Supporting information), (b) controlling for nonlinearities in the effect of regulatory ratios (Table A.6, Supporting information), and (c) controlling for bank activity in the syndicated loan market (Table A.7, Supporting information).

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

In Table 5, we examine the correlation between $\hat{\beta}_{b,y}$ and $\hat{\beta}_{b,y}^{ROA}$ ($\hat{\beta}_{b,y}^q$), the bank-year specific violation effect on ROA (Tobin's q) also obtained from the estimations in Table 3.¹⁴ In column 1, we uncover a positive and significant relationship between $\hat{\beta}_{b,y}$ and $\hat{\beta}_{b,y}^{ROA}$. This result may seem at odds with the positive effect of covenant violations on ROA shown by Nini, Smith, and Sufi (2012), but it can actually be reconciled with their findings. 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.¹⁵ To the best of our knowledge, we are the first to show that the positive effect of covenant violations on performance is driven by those instances in which the lending banks act in a more lenient fashion regarding their intervention behavior. This inference is corroborated by the positive—although insignificant—relationship between $\hat{\beta}_{b,y}$ and Tobin's q $\hat{\beta}_{b,y}^{ROA}$ in column 3.

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

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

TABLE 5
Bank Monitoring over Investment and
Performance of Borrowing Firms

Dependent Variable	$\hat{\beta}_{b,y}^{ROA}$ (1)	$\hat{\beta}_{b,y}^q$ (2)
$\hat{\beta}_{b,y}$	0.097** (2.51)	0.856 (0.93)
Leverage	-0.004 (-0.08)	0.286 (0.32)
Tier 1	0.075 (1.48)	0.625 (0.90)
Deposits	-0.017 (-1.41)	0.106 (0.51)
Short-term funding	-0.022 (-1.50)	-0.250 (-0.86)
ln(Assets)	0.000 (0.46)	0.014 (0.96)
Noninterest income	-0.007 (-0.79)	0.064 (0.81)
Trading	-0.010 (-0.84)	0.413* (1.93)
Nonperforming assets	-0.251 (-1.50)	4.140 (1.60)
Net income	0.952 (1.31)	5.224 (0.77)
Cost-to-income	0.010 (1.23)	0.156 (1.38)
Constant	-0.003 (-0.17)	-0.493 (-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

Note: This table reports estimates from a modified second-step OLS specification (5) over a 1994–2012 bank-year panel. The dependent variable is either $\hat{\beta}_{b,y}^{ROA}$ or $\hat{\beta}_{b,y}^q$. $\hat{\beta}_{b,y}^{ROA}$ ($\hat{\beta}_{b,y}^q$) 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 1 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 Table A.2 (Supporting information) for variable definitions.

All in all, these results point to the efficiency of banks' leniency after covenant violations.

This result suggests that banks reacting strictly to violations pursue an inefficient solution, at least from the perspective of the borrowers. In light of the result on Tier 1, a possible explanation is that these banks are constrained in

their choice set due to their relatively low capitalization. Rather than opting for the course of action maximizing borrowing firms' value, they chose to impose investment restrictions to protect their short-term claim on a borrower's cash flow. In other words, their action can be seen as an example of excessive monitoring.

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

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

Additional tests show that the bank monitoring measure does not correlate with the state of the business cycle (Figure A.3 and Table A.8 in Supporting information). Likewise, the baseline results on the role of equity and debt structure for monitoring are robust to using a one-step procedure that does not suffer from the econometric issues discussed in Section C (Figure A.4 and Tables A.9–A.11 in Supporting information).

C. Quasi-Experimental Evidence

We use the 2009 U.S. SCAP stress test to draw causal inference on the equity monitoring hypothesis versus the equity buffer narrative. On May 7, 2009, the Federal Reserve Board (the Board) released the results of its first stress test after the financial crisis (the SCAP) for the 19 largest U.S. banks. Ten banks were identified to

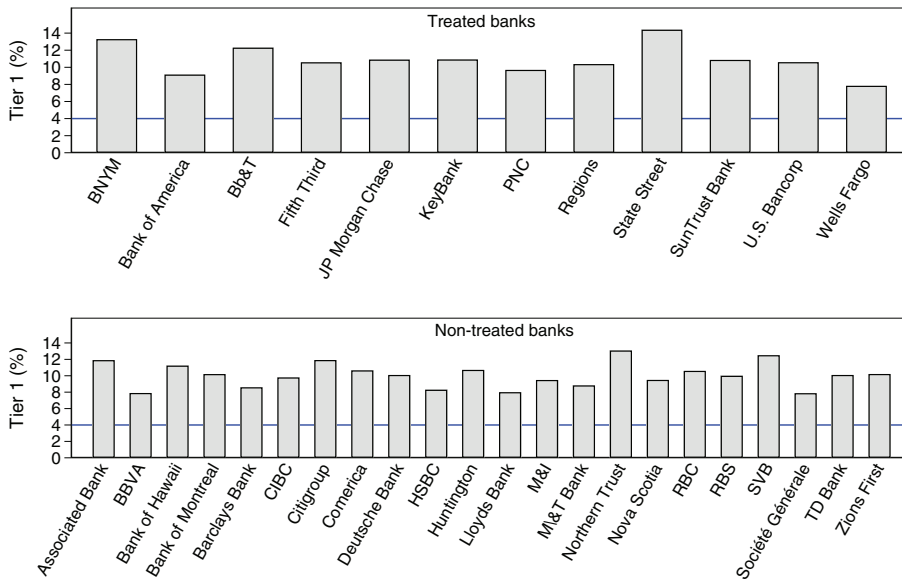
have severe capital shortfalls, ranging from \$0.6 to \$33.9 billion. The results induced 14 banks to issue equity in the 3-month window around the publication of results. Importantly, as noted by Greenlaw et al. (2012) affected banks were not issuing capital in the 3 months before the publication. According to Morgan, Peristiani, and Savino (2014) the size of each bank's capital shortfall identified in the SCAP was not anticipated by market participants. Thus, we interpret this equity issuance as a plausibly exogenous increase in Tier 1 capital. We use issuance in the 3 months after the publication of the stress test scaled by 2008 total assets as our treatment intensity indicator.

Figure 2 shows that there was no clearly discernible difference in terms of Tier 1 capitalization as of the end of 2008 across treated banks (i.e., those that issued equity in the 3 months after the SCAP) and nontreated banks. The Board based its stress test on criteria that were not known *ex ante* and not tightly linked to Tier 1 capital, which arguably explains why markets did not anticipate the SCAP results. Reassuringly, the treated and nontreated group appear to be heterogeneous in terms of business model, both comprising a mix of global and more regional banks.

Table 6 shows the results of a difference-in-difference analysis. We interact the SCAP treatment measure indicator with year-indicators for the years 2010, 2011, and 2012 or a cumulative post-period indicator that is equal to one starting in 2010. We also control for bank-level total TARP equity injections scaled by 2007 total assets to account for selection into treatment, as well as for bank characteristics in $\Gamma_{b, y-1}$. Across a range of specifications involving different sample restrictions and pre- and post-periods, we find a positive and significant effect of equity issuance activity linked to the SCAP on monitoring intensity. The positive effect of such equity shocks work in the same direction as Tier 1 capital in the baseline correlation analysis and corroborates the equity buffer narrative.

Finally, we exploit plausibly exogenous shocks to banks' exposure to runs, both on the deposit and on the wholesale funding market. Specifically, we specify in the vein of the SCAP analysis above three indicators of funding fragility: exposure to the reform of deposit insurance taken from Lambert, Noth, and Schüwer (2017), substantial co-syndication with Lehman Brothers, and large exposures to the subprime residential mortgage market. Tables

FIGURE 2
Risk-Adjusted Tier 1 Capital Ratio Before the SCAP Stress Test of 2009



Note: This figure visualizes risk-adjusted Tier 1 capital ratio for treated (top graph) and nontreated (bottom graph) banks before the SCAP of 2009. The bar charts show the regulatory Tier 1 capital ratio at the end of 2008 together with its minimum threshold of 4% (horizontal blue line). Treated banks are those banks that issued equity in the 3-month window around the SCAP stress test in May 2009.

A.12–A.14 (Supporting information) corroborate the absence of evidence that bank funding fragility matters for monitoring.

VI. LIMITATIONS

This paper is one of the first attempts to empirically quantify how funding structure of banks impacts their monitoring activity. Although covenant violations provide a unique and useful setting to insulate the effect of bank actions on borrowing firms' governance, our empirical design suffers from some drawbacks on which we elaborate in this section.

First, our proxy for bank monitoring may entail nontrivial measurement errors. Besides the issues related to generated variables discussed in Section C and addressed in Appendix Section 5 (Supporting information), covenant violations indeed trigger various bank reactions (such as changes to loan terms) together with enhanced monitoring. Although a dynamic loan renegotiation process is inherent to covenant design and

constitutes a form of monitoring by itself (Denis and Wang 2014; Smith 1993), changes in investment due to changes in loan terms should be ideally filtered out. But originations and renegotiations cannot be adequately distinguished in Dealscan (see Roberts 2015), which makes such an exercise difficult. Thus, we have to assume that cross-firm differences in investment adjustment following violations are entirely ascribable to cross-bank differences in monitoring effort.

A second issue pertains to selection effects, which relate to contract design at origination as well as to renegotiations of covenants taking place before they are actually breached (Denis and Wang 2014). Controlling for the borrowing firm's financial policies through a one-step procedure as in Table A.11 (Supporting information) ameliorates this problem. But we cannot rule out that the sample is biased, for example towards those violations entailing smaller costs for borrowers. At the same time, Appendix Section 2 (Supporting information) confirms that the availability of our monitoring measure, which depends on observing enough covenant violations for a

TABLE 6
The SCAP Quasi-Experiment

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

Note: This table reports estimates from the second-step OLS specification (6) augmented with a difference-in-differences exercise based on the publication of SCAP stress test results on May 7, 2009. The dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. Explanatory variables include *Affected* (SCAP) (defined as the bank-specific equity issuance after the publication of SCAP results scaled by 2008 total assets) and its interactions with year-specific or cumulative post period indicators for the treatment period, *TARP* (defined as total TARP take-up scaled by 2007 total assets), and lagged time-varying bank characteristics $\Gamma_{b,y-1}$. Information on the sample period/selection and standard error clustering is indicated below. Specifications including also non-U.S. banks control for a U.S. bank indicator and its interactions with post-SCAP indicators. The t statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Table A.2 (Supporting information) for variable definitions.

given firm-bank-year triplet, depends on bank characteristics. The latter may also determine the type and the strictness of the covenants negotiated at origination.

Third, two important innovations in loan origination became established over the time span of our sample: nonbank lending and covenant-lite loans. Chernenko, Erel, and Prilmeier (2019) show that nonbank lenders rely less on financial covenants. Biswas, Ozkan, and Yin (2019) confirm this finding, but also document that nonbank lenders make extensive use of covenants restricting capital expenditures. By contrast, the rise in covenant-lite term loans did apparently not induce a major shift in covenant design, as banks continue to impose traditional covenants in loan packages through credit line facilities

(Berlin, Nini, and Yu 2020). Nonetheless, both nonbank lending and covenant lites are arguably important for our setting. Alas, Becker and Ivashina (2016) note that the reporting quality for the cov-lite indicator in Dealscan is poor and the differentiation between maintenance-based covenants (cov-heavy) and incurrence-based (cov-light) is hindered by several intermediate cases.

We believe that all three issues are of relevance. However, the robustness tests that can be conducted given the available data bear only limited indication that they are of first-order importance to the qualitative inference that better capitalized banks take a more lenient monitoring stance. At the same time, future research to scrutinize the sensitivity of this main result based

on more detailed data in a more rigorous fashion seems warranted.

VII. CONCLUSION

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

This paper studies heterogeneity in monitoring across banks in the context of syndicated loans to U.S. 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 analyzing banks' interventions in borrowers' management after covenant violations, which we approximate by firms' changes in investment policy.

This monitoring measure reveals the existence of substantial heterogeneity in monitoring both across banks and over time. The results clearly indicate 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 is linked to improved borrowers' performance instead of being distortive.

To move closer to causal inference, we investigate banks' monitoring responses towards exogenous shocks to their regulatory equity capital during the SCAP of 2009. This exercise confirms the inferences based on correlations quantified in the regression analysis.

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

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Appendix S1. Supporting information