Financial Shocks and Corporate Investment Activity: The Role of Financial Covenants*

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Abstract:

We examine whether economic shocks to credit institutions differentially affect the use and strictness of different accounting-based covenants in debt contracts, and whether these effects represent a channel through which shocks to lenders propagate to the real sector. To capture lender-specific shocks, we use variation in payment defaults experienced by lenders outside the borrower's region and industry. We find that lenders respond to payment defaults by shifting towards performance-based covenants (and away from capital-based covenants), and by increasing the strictness of performance covenants. In turn, these changes in covenants constrain future investments among relationship borrowers. We also find that subsequent to contract initiation, lender-specific shocks affect corporate investment. Overall, our results suggest that credit-supply frictions influence the type and strictness of covenants in debt contracts, and that financial covenants represent a channel through which shocks to lenders are transmitted to the nonfinancial sector.

Keywords: Covenants, debt contracting, financial market frictions.

JEL Classification: M4, G32.

1. Introduction

Accounting information plays an important role in capital markets by facilitating contracts between capital providers and firms with profitable investment opportunities (Watts and Zimmerman 1986). Debt contracts are a salient manifestation of this role because they frequently rely on elaborate covenants that allocate control rights between borrowers and lenders contingent on accounting performance. While it is established in the literature that state-contingent control allocations improve contract efficiency and enable cheaper access to finance, the costs of including additional accounting-based covenants into debt contracts are not well understood. In this paper, we study a cost to borrowers associated with the use of financial covenants. Specifically, we investigate whether the choice of accounting-based covenants and their strictness represents a mechanism through which idiosyncratic shocks to credit institutions transmit to the corporate sector and interfere with investment activity.

A large body of literature in economics and finance suggests that financial sector shocks propagate to the corporate sector.[1] However, there is limited evidence on the mechanisms through which the behavior of borrowers is impacted by shocks to their lenders. Drawing on incomplete contracts theory, we argue that creditor control rights play a role in the transmission of lender-specific shocks into the corporate sector. We argue that for two non-mutually exclusive reasons, lenders experiencing adverse economic shocks impose stricter control rights on borrowers. First, an adverse shock to the lender's loan portfolio may reveal information about the lender's own ability to assess creditworthiness (Murfin 2012).[2] In order to protect themselves, lenders that learn from a negative portfolio shock about their lower ability to detect creditworthy borrowers are more

[1] See, e.g., Bernanke and Gertler (1990), Holmstrom and Tirole (1997), Dell'Ariccia, Detragiache, and Rajan (2008), Becker and Ivashina (2014).

[2] As in Broecker (1990), the ability to assess creditworthiness refers to the bank's capability to determine the ability of a potential creditor to repay credit.

likely to demand the inclusion of stricter control rights. They can use these additional control rights to force future renegotiations as they learn more information about the borrower. Second, an adverse shock to a loan portfolio is expected to shift the preferences of the lender towards greater control over borrowers, which in turn allows lenders to proactively manage the risk associated with their loan portfolio. Both reasons suggest that a covenants package can enable lenders to pursue their private objectives, which are not necessarily aligned with contract efficiency considerations.

The arguments above suggest that covenant-package composition, in addition to being optimally chosen as a function of the agency and information problems, is also determined by lender-specific (supply-side) factors. This variation in covenants, which is exogenous from the borrower's standpoint, likely influences borrower decisions. An increase in control by lenders, independent of debt-related agency conflicts, is likely to reduce incentives to invest in projects with long-term, uncertain payoffs. The prospect of creditor-intervention weakens borrowers' incentives to pursue long-term uncertain projects, because possible covenant violations will require costly concessions and potential project shutdown (Dewatripont and Tirole, 1994; Roberts and Sufi, 2009; Nini, Smith and Sufi, 2012).

To provide evidence on whether the composition of the covenant package is a function of lenders' portfolios and whether the composition ultimately influences the corporate sector's investment decisions, we need a proxy for lender-specific shocks that is independent of the borrower's fundamentals. We construct this proxy by tracing lender-specific shocks to individual borrowers in a way that is orthogonal to the borrower's fundamentals. In particular, we follow Murfin (2012) and use recent payment defaults that are experienced by the lead arranger outside the borrower's industry and geographic region.

In line with prior studies, we characterize the covenant package in terms of (i) its reliance on capital (balance-sheet) vs. performance (income-statement) covenants (Demerjian 2011, Christensen and Nikolaev 2012), and (ii) in terms of covenant strictness, measured as the probability of violating either type of covenant (Demerjian and Owens 2016; Demerjian, Owens, and Sokolowski 2019). Christensen and Nikolaev (2012) argue that the primary role of capital covenants is to align the interest of the contracting parties ex ante, whereas performance covenants facilitate the transfer of control rights ex post, i.e., they allow creditors to proactively intervene in the borrower's decisions.[3] Because lenders who experience negative shocks to their loan portfolios have incentives to proactively manage their portfolio downside risks, we expect lenders to increase the frequency and strictness of performance covenants when issuing new loans. In contrast, capital covenants primarily work via ex ante incentive alignment and reduce lenders' direct influence over managerial decisions after the loan is initiated, which means that lenders are less likely to use them following a shock. We expect both of these effects to be present mostly among borrowers that rely on relationships with their lenders, i.e., borrowers that find switching to another lender to be costly.

We begin our analysis by examining how the structure of covenant packages in new debt contracts varies with lenders' recent default experiences. In line with Murfin (2012), we find a positive association between lender-specific shocks and the overall use of covenants. However, when we distinguish between capital and performance covenants, the results differ. Consistent with our hypothesis, we find a positive association between recent payment defaults in a lender's portfolio and the frequency and strictness of performance covenants in subsequent loan contracts. In contrast, payment defaults exhibit a negative association with the frequency of capital covenants and no statistically significant association with capital-covenant strictness. In support of our

[3] Evidence in Christensen and Nikolaev (2012); Dyreng, Vashishtha, and Weber (2017); Hollander and Verriest (2016); and Honigsberg et al. (2015) is broadly consistent with this hypothesis.

hypothesis, we find that the association between the frequency/strictness of performance covenants and payment defaults is only significant for borrowers with limited outside options. Overall, our findings suggest that lenders respond to adverse economic shocks by contracting in ways that enable them to exert more influence on borrowers over the loan term.

Having established the effect of lender shocks on covenant composition and strictness, we next examine whether covenants are a mechanism through which lender-specific shocks transmit to the corporate sector. In support of this mechanism, we show that an increase in the frequency and strictness of performance covenants—explained by lender default experiences in other regions and industries and not by changes in borrower fundamentals—reduces research and development (*R&D*) and capital expenditures (*Capex*) amongst the relationship borrowers. Additional analysis suggests that some, but not all, of this effect occurs because R&D intensive firms are less likely to access the syndicated lending market when their lender is subject to an adverse shock.

Given that we argue that performance covenants enable lenders to interfere with borrower's decisions ex post, we also study whether the effect of covenant composition on borrowers' investments is present after the initial contracting when lenders experience a new financial shock. Specifically, we examine the cross-sectional relationship between lender-specific shocks and corporate investments when existing covenants are in effect. Under the null hypotheses, covenants are optimally chosen to address the borrower's agency and information problems, and unanticipated lender-specific shocks do not influence the borrower's investment decisions. The alternative hypothesis, however, is that the more extensive use of performance covenants, combined with a lender-specific shock, results in a higher likelihood of lender interference. This implies that covenants already in place expose borrowers to lenders' financial shocks. For example, borrowers will anticipate that a possible covenant violation will likely have more severe

consequences when the lender's portfolio is distressed, and hence the borrower should choose a more conservative investment policy to reduce the likelihood of covenant violations. Consistent with this hypothesis, we find that when debt contracts rely on performance covenants, relationship borrowers reduce investments in response to payment defaults experienced by their lenders. This evidence triangulates the *ex ante* effect detected above and further supports the hypothesis that covenants are one mechanism through which lender-specific shocks propagate to the corporate sector.

Our paper makes several contributions to the literature. First, prior literature documents the importance of economic differences among covenants and shows that the choice of covenants is a function of agency conflicts and firm characteristics (Demerjian 2011, Christensen and Nikolaev 2012, Armstrong, Gallimberti, and Tsui 2019). We build on this evidence by showing that the composition of covenant packages also depends on credit supply-side factors such as lenders' private preferences. Our study complements Murfin (2012), who shows the importance of supply-side factors in explaining the overall use of covenants. However, we document that this effect depends critically on the type of covenants and the contracting role they play.

Second, it is well documented that accounting-based covenants affect corporate investment ex post, i.e., following a covenant violation and renegotiation (Beneish and Press 1993; Chava and Roberts 2008; Roberts and Sufi 2009a,b; Sufi 2009; Nini, Smith, and Sufi 2012; Denis and Wang 2014). However, it is not well understood whether the *ex ante* choice of financial covenants for reasons unrelated to the fundamentals of the borrower affects investment absent covenant violations. Nini, Smith, and Sufi (2009) document that ex ante contractual restrictions on the amount of investment (negative covenants) are often binding ex post. However, they do not study the *ex ante* role of financial covenants (accounting-based covenants) in determining investment

policy. Furthermore, the studies above conclude that covenants influence investment policy in response to agency conflicts between borrowers and lenders, and that covenant violations warrant reduced investment in response to borrowers' financial difficulties. In contrast, we show that the private preferences of lenders, expressed via the use of covenants, have an important effect on corporate investments for reasons unrelated to the economics of the borrower. Our findings are in line with predictions from the theory of incomplete contracts, specifically, that control allocations have both a benefit and a cost side.

Third, we contribute to the literature in economics that examines the impact of financial market shocks on the real economy (e.g., Bernanke and Gertler 1990, Holmstrom and Tirole 1997, Dell'Ariccia, et al. 2008, Becker and Ivashina 2014, Chodorow-Reich and Falato 2017). Our paper complements these studies by providing evidence on the role of accounting-based covenants in the transmission of bank's shocks to the real economy.

Overall, our study suggests that although accounting-based covenants play a central role in addressing agency conflicts and improving credit market access, covenants also entail a cost to borrowers associated with lenders' use of the control rights. This cost could help explain why debt contracts include relatively few covenants.

2. Prior Research and Empirical Predictions

Our study connects two largely independent literatures in corporate finance and macroeconomics. These literatures form the basis of our predictions as to how lender-specific shocks can influence the use of covenants and corporate investment, as we discuss next.

First, a number of studies show that covenants influence borrowers' investments through violation or renegotiation (Beneish and Press 1993; Chava and Roberts 2008; Roberts and Sufi

2009a,b; Sufi 2009; Nini, Smith, and Sufi 2012; Denis and Wang 2014).[4] For example, Chava and Roberts (2008) and Nini et al. (2012) show that covenant violations lead to a reduction in subsequent investments by the borrower. These studies suggest that covenants allow lenders to discipline underperforming borrowers, presumably in response to ineffective management or agency problems, and limit underperforming borrowers' investment. In support of this view, Nini et al. (2012) document improvements in firms' performance after creditor interventions.

While the efficiency role of financial covenants is well established, it is also plausible that creditors sometimes take advantage of covenants in order to interfere with a borrower's decisions for reasons not explained by the borrower's fundamentals (and hence unexplained by the risk of expropriating wealth from lenders) but rather explained by the lenders' private objectives. This view builds on the theory of incomplete contracts (Grossman and Hart 1986, Aghion and Bolton 1992), which predicts that control allocations trade-off opportunistic behavior on both sides of the contract. This theory also predicts that in some cases, lenders will inefficiently liquidate or curtail profitable investments.

The macroeconomic research from which we draw studies the effect of credit-supply-side factors on firms' investment decisions. This literature provides evidence that shocks to the financial sector lead to a reduction in the supply of credit, which then transmits to the corporate sector by constraining companies' investments (e.g., Bernanke and Gertler 1990, Holmstrom and Tirole 1997, Becker and Ivashina 2014, Chodorow-Reich and Falato 2017). Specifically, the literature shows that an exogenous tightening of credit standards prevents profitable investment projects from being undertaken when companies are dependent on lending relationships to access

[4] This literature has focused on the relationship between the economics of the borrower and contracting at initiation or renegotiation. In analyzing the relationship between lender economics and contract structure, we follow studies like Saavedra (2017), who link syndicate structure to debt contract terms, and Bozanic, Loumioti, and Vasvari (2018), who analyze the influence of collateralized loan obligations on debt contract terms.

capital. However, this literature has yet to examine whether the use of accounting-based covenants represents a channel through which lender-specific shocks propagate to the corporate sector, and whether this transmission depends on covenant type or composition.

Drawing on the two literatures discussed above, it is plausible that debt covenants are a mechanism by which the credit supply-side factors can interfere with a borrower's investments. To explore this, we draw on Murfin (2012), who shows that the strictness of the loan contract varies as a function of plausibly exogenous shocks to the lender's portfolio. In other words, covenant strictness cannot be completely explained by the borrower's economic fundamentals but banks set stricter covenants on new loans after experiencing payment defaults on other loans in their portfolios, even when the defaults occur in a different geographic region and industry.

Unlike Murfin (2012), who treats all covenants as similar, we hypothesize that lenders' preferences for control have important implications for the choice of covenant package composition. We focus on the distinction between capital and performance covenants, following Christensen and Nikolaev (2012), Demerjian (2011), and Demerjian et al. (2019), who argue that there are significant differences between the contractual roles of the two covenant types. Capital covenants incentivize the borrower to pursue value maximization (e.g., to refrain from asset substitution) as lenders play a more passive role. In contrast, performance covenants tightly monitor firms' key performance indicators, allowing lenders to proactively exercise control in response to performance fluctuations.

We expect that lenders that experienced adverse shocks to their loan portfolios prefer covenant packages that give them more control over borrowers. A loan portfolio deterioration makes control more valuable for two not mutually exclusive reasons. First, an adverse economic shock may reveal information to lenders about their ability to assess creditworthiness (Murfin

2012). When lenders learn about their own inability to determine the quality of a borrower ex ante, they are more likely to negotiate covenants that give them more control over the borrower ex post. In other words, covenants which give lenders more control over the borrower ex post can substitute for ex ante ability to assess creditworthiness. Second, an adverse shock to a loan portfolio should shift lenders' preferences towards increased control because it allows them to proactively manage the increased risks associated with their loan portfolio, particularly in cases where adverse economic conditions persist or reoccur. For example, lenders can reduce deterioration in their portfolio by exercising control over individual loans to minimize borrowers' risky activities (likely correlated across loans), such as R&D, new investments, and acquisitions and/or to encourage borrowers to repay or restructure risky loans.[5]

For both of these reasons, we expect that lender-specific shocks shift lenders' preferences towards more control, regardless of the scope for opportunistic behavior by borrowers. Performance covenants which tightly monitor firms' key performance indicators are a potential way to accomplish this. In contrast, the ex ante alignment of incentives via capital covenants assigns a less active role to creditors after loan initiation. Accordingly, we predict that lenders experiencing defaults in their loan portfolios shift away from incentive alignment (i.e., capital covenants) and towards increased control (i.e., performance covenants), effectively substituting ex post monitoring for ex ante incentive alignment. In other words, we expect lenders to respond to portfolio shocks by including *more* and *tighter* performance covenants in subsequent loans. This hypothesis is formally stated as follows:

[5] From an institutional perspective, the bank's central office that has authority in lending decisions across markets and monitors the ex post performance of loans for the entire portfolio is the *central credit committee* (see Ranson 1995). This committee must offer final approval before a loan officer can commit to lending. It follows that, given the relatively small number of large transactions in the syndicated loan market, recent payment defaults are particularly informative for the terms of new approvals.

H1: The likelihood of including more frequent and tighter performance covenants in debt contracts increases in the number of payment defaults the lead arranger recently experienced.

If the above hypothesis is true and lender preferences are a determinant of covenant package composition, it is likely that the variation in covenants alters the borrowers' investment decisions. We expect that increased use/strictness of performance covenants unrelated to the borrower's fundamentals is likely to impose a constraint on firms' investments or reduce the borrower's incentives to take on risky projects (due to the increased likelihood of a covenant violation). More formally, we hypothesize the following:

H2: Borrowers reduce capital expenditures and R&D spending in response to a shift towards performance covenants that is unexplained by their fundamentals.

Together, these two hypotheses suggest that performance covenants represent a channel through which idiosyncratic shocks to lenders are transmitted to the corporate sector.

3. Measurement, Sample Construction, and Summary Statistics

3.1. Measuring lender-specific shocks

Our analysis requires a measure of lender-specific shocks independent from the borrower's fundamentals. Following Murfin (2012), we measure lender-specific shocks as the number of payment defaults in the lead arranger's loan portfolio occurring outside the borrower's industry and region.[6] We focus on payments instead of technical defaults because we need economically significant defaults that are likely to affect lender behavior. Hence, for each lead arranger, we count the number of borrowers that the S&P's Compustat Rating Database reports as in default or

[6] Credit institutions' recent default experience is used as a measure of shocks to lender behavior in Berger and Udell (2004); Chava and Purnanandam (2011); and Gopalan, Nanda, and Yerramilli (2011).

selective default during the 90 days leading up to the negotiation of a new loan.[7] This gives us a count of all defaults for a given lead arranger.

We assign this default count to each new loan contract in the Loan Pricing Corporation Dealscan database (hereafter, Dealscan). To avoid capturing the geography- or industry-specific economic conditions of the borrower, we exclude payment defaults that the lead arranger experiences in the borrower's geographic region and industry.[8] For example, for a retail company in Illinois, the measure counts only defaults by non-retail firms outside of Illinois, omitting the defaults of all retail firms regardless of location and the defaults of all Illinois firms regardless of industry. This measure allows us to examine whether a default by a high-tech firm in Massachusetts, for example, affects how a new debt contract is written for a retail company in Illinois through their common lender.

Using this procedure, for each loan contract in Dealscan we obtain a measure of financial shocks to the lead arranger, which is plausibly exogenous to the current borrowers' characteristics and the economic conditions in which they operate. We term this measure "*Defaults different Region and SIC*."

To supplement our default-based measure of financial shocks, we use an alternative measure based on significant changes in Tier 1 Capital in order to capture a broader type of shock to lenders' financial health. To construct this shock variable, we first gather quarterly Tier 1 data

[7] In constructing our defaults count measure, we need to make assumptions about the contracting timing. Rhodes (2000) documents that the terms of a syndicated loan are negotiated far in advance of the legal start date reported in Dealscan. In particular, it usually takes a month to receive a mandate, while the documentation process takes 2 months. To account for this time lag, and consistent with Murfin (2012), we use the date three months prior to the Dealscan reported start date as the contracting date. Moreover, Murfin (2012) shows that lenders are most sensitive to defaults that occur in the 90 days immediately prior to the negotiation of a new loan, as opposed to the 180 or 360 days prior to contracting. Accordingly, we use the 90-day period in our analysis. Nevertheless, our results are robust to using a window of 180 days (not tabulated) or 360 days (see, e.g., Tables 8 and 9).

[8] Within the United States and Canada, the geographic region is the state and province, respectively. All other domiciles are classified as one international region. We measure industry at the two-digit SIC code level.

from the Compustat Bank dataset and calculate the change in the lender's Tier 1 Ratio over the most recent quarter ending at least 90 days before the loan becomes active.[9] We then calculate the annual percentile of these changes and set *Shock to Tier 1* equal to -1 if changes in Tier 1 Capital Ratio are at or below the 25th percentile, equal to 0 if changes in Tier 1 Capital Ratio are greater than the 25th percentile and less than or equal to the 75th percentile, and equal to 1 if changes in Tier 1 Capital Ratio are above the 75th percentile of the annual distribution.

3.2. Main variables of interest

We obtain annual data on borrower characteristics from the WRDS Compustat Database and data on loan characteristics from the Dealscan database.

As the dependent variables in our investment analysis, we use research and development expenses scaled by total revenues (*R&D*), and capital expenditures scaled by the book value of total assets (net of capital expenditures) (*Capex*).

We quantify the use and composition of financial covenants in several ways. First, we count the total number of covenants in a loan contract (*Number of Covenants*) to measure covenant intensity. To measure covenant composition, we separately count performance covenants (*P-Covenants*) and capital covenants (*C-Covenants*).[10] Finally, for a single measure of the covenant package composition, we calculate the fraction of performance covenants: *Covenants Ratio* = *P-Covenants*/(*P-Covenants*+*C-Covenants*). In constructing these variables, we do not replace the missing values for covenants on Dealscan with zeros (e.g., Demerjian 2011).

To measure the strictness of financial covenants, we first obtain the simulated probabilities of covenant violation developed in Demerjian and Owens (2016) and Demerjian et al. (2019) from

[9] Consistent with our measure of defaults, in constructing *Shock to Tier 1,* we make the same assumption about the contracting timing (i.e., we use the date three months prior to the Dealscan reported start date as the contracting date; see Rhodes 2000, and Murfin 2012).

[10] The complete list of P- and C-covenants is presented in Appendix A.

the Edward Owens website. Next, we use three measures: *Prob-Violation*, the simulated probability that the firm violates at least one covenant in the first quarter after the loan is initiated (Demerjian and Owens 2016); *Prob-Violation P-Cov*, the simulated probability that the firm violates at least one performance covenant in the first quarter after the loan is initiated; and *Prob-Violation C-Cov*, the simulated probability that the firm violated at least one capital covenant in the first quarter after the loan is initiated (Demerjian et al. 2019).

3.3. Control variables

Our analysis includes a number of firm-level, contract-level, and macro-economic control variables. Control variables for borrower characteristics include: borrower size, measured as the natural logarithm of the market value of total assets (the sum of the market value of equity and the book value of debt) (*Size*); borrower profitability, measured as the return on assets, computed as income before extraordinary items scaled by average total assets over the last two years (Sloan 1996) (*ROA*); borrower growth opportunities, measured as the Book-to-Market ratio, which we define as the book value of total assets scaled by total debt plus the market value of equity (Skinner 1993) (*Book to Market*); borrower leverage, measured as long-term debt scaled by the sum of the market value of equity and the book value of debt (Dichev and Skinner 2002) (*Leverage*); and borrower risk, measured as separate indicators for the borrower's S&P senior long-term debt rating (*Borrower Rating*), and the Altman's Z-score index (Altman 1977) (*ZScore*). Finally, we include the borrower's dividends yield (*Dividends*).

Our controls for loan characteristics include: *Maturity*, measured as the number of months to maturity at contract initiation; *Loan Size*, measured as the natural logarithm of the loan amount in US dollars; *Secured*, an indicator for whether the loan is secured; *Number of Lenders,* measured as the number of non-lead arranger banks participating in the loan; and *Loan Purpose*, which we

measure with separate indicators for the following categories reported in Dealscan: leverage buyout, corporate purposes, working capital debt, debt repayment, CP backup, or other. Finally, in order to control for macroeconomic conditions at loan initiation, we obtain data on the quarterly growth of the gross domestic product (*Quarterly GDP Growth*) from the Bureau of Economic Analysis. See Appendix A for further information on how we construct the variables used in the analyses.

3.4. Sample construction

To construct our sample of lenders and borrowers, we merge syndicated loan information from Dealscan and Compustat using the link file provided by Sudheer Chava and Michael Roberts (Chava and Roberts 2008), and distributed by the Wharton Research Data System. From Dealscan, we collect information regarding lender-borrower relationships and syndicated loan terms. We obtain financial information about borrowers from WRDS Compustat.

Our measure of payment defaults requires knowledge of the lead banks. We use the banks names reported in Dealscan and trace them to their parent institution. When a regional branch (e.g., JP Morgan Chase Illinois) or a business-banking sector (e.g., Wells Fargo Securities) is listed as the lender of record, we combine the regional office or business-banking sector under the parent's bank name (in these cases, JP Morgan Chase and Wells Fargo, respectively). We repeat this process for all subsidiaries of the 200 largest banks reported in Compustat (which jointly account for the vast majority of all US corporate lending). Furthermore, in the event of a merger and acquisition, we mark the acquired bank as independent until the year of acquisition, only aggregating the acquired and acquiring bank under the same parent's name after the acquisition is finalized. Our final sample contains 21,458 observations from 1996 to 2013.[11]

[11] Due to missing test and control variables, the number of reported observations in the tables differs.

3.5. Summary statistics

The summary statistics for our sample are reported in Table 1. The first two rows report statistics for our measure of negative shocks to lenders. Our measure of financial shocks to the lender, *Defaults different Region and SIC*, is right skewed with a mean of 2.21, which is similar to Murfin (2012).[12] The indicator variable *Shock to Tier 1* has a mean close to zero (-0.07) and a standard deviation of 0.70, which suggests balance between positive and negative shocks to lender health.

The next four rows report statistics for loan contract characteristics. The average maturity of loans in our sample is 48 months, the average loan size is 18.95 (log of dollars), 67 percent of loans are secured, and, on average, syndicates consist of 10 (non-lead) banks.

The next eight rows report the summary statistics for borrower characteristics. The characteristics of the borrowers in our sample are similar to those reported in Christensen and Nikolaev (2012). In particular, average R&D expenses are 2 percent of the total revenue, and capital expenditures are 8 percent of total assets. In terms of *size*, *ROA*, *market to book*, *leverage*, *Z-score*, and *dividends,* borrowers are also similar to those in the sample from Christensen and Nikolaev (2012).

The remaining seven rows of Table 1 report the summary statistics for our covenant variables. 50 percent of our sample firms have 3 covenants per loan, with the average *Covenants Ratio* equal to 0.73. This indicates that the average package includes more performance than capital covenants. The last three rows of Table 1 report summary statistics for our measures of the

[12] In particular, Murfin (2012) reports summary statistics for defaults during the 90 days prior to the activation of a new loan, including those occurring in the borrower's SIC and geographic region. His data show a mean default count of 1.51 and a 90th percentile of 5. However, our sample period is shorter than Murfin's. In fact, if we consider the whole sample period and include the defaults in the borrowers' industries and geographic regions, we have a mean default count of 1.46 and a 90th percentile of 5, which is similar to Murfin (2012).

probability of covenant violation. Similar to the sample provided by Demerjian and Owens (2016) and Demerjian et al. (2019), the means of these variables suggest that the probability that firms will violate at least one covenant in the first quarter after the loan is initiated is 41 percent, that the probability of violating a performance covenant is 35 percent, and that the probability of violating a capital covenant is 11 percent.

To assess the comparability of our covenant count variables—and the probabilities of covenant violation—to prior research, in Table 2 we report the correlations among these variables. As expected, *P-* and *C-Covenants* are positively correlated with the *Number of Covenants* in loan contracts. More importantly, *P-Covenants* and *C-Covenants* are negatively correlated (–0.38), consistent with Christensen and Nikolaev (2012). We also document a negative correlation between *P-Covenants* and *Prob-Violation C-Cov* (and find similarly for *C-Covenants* and *Prob-Violation P-Cov*). These correlations suggest that contracting parties use performance and capital covenants in different ways, depending on the contracting circumstances, which is consistent with the assumptions underlying our Hypothesis 1.

4. Results

4.1. Lender idiosyncratic shocks and the choice among financial covenants

In this subsection, we provide evidence on the use and composition of financial covenants when lenders experience recent defaults on loans issued in different geographic regions and industries (Hypothesis 1). We run the following regression:

$$Covenants_{i,t} = \beta_0 + \beta_1 Defaults\ different\ Region\ and\ SIC_{i,t} + \beta_2 Controls_{i,t} + \tau_t + \lambda_l + \rho_r + \epsilon_{i,t}, \quad (1)$$

where *Covenants* are one of the following: the total number of covenants in loan contract *i* entered into at time *t* (*Number of Covenants*), the number of performance covenants (*P-Covenants*), the

number of capital covenants (*C-Covenants*), the composition of covenants (*Covenants Ratio*), the probability of violating any covenant (*Prob-Violation*), the probability of violating a performance covenant (*Prob-Violation P-Cov*), or the probability of violating a capital covenant (*Prob-Violation C-Cov*). *Defaults different Region and SIC* is our measure of financial shocks, defined in Section 3.1. *Controls* denote the observable characteristics of the loan contract, and the macroeconomic environment that are likely associated with both covenants and financial shocks to the lender, but that are unrelated to the hypothesis we are testing. *Controls* include *Maturity*, *Loan Size*, *Secured*, *Number of Lenders*, *Zscore*, and *Quarterly GDP Growth*.[13] We also control for year, loan type, and borrower rating fixed effects, denoted as τ_t, λ_l, and ρ_r, respectively. Year fixed effects control for time trends in contracting practices that may be determined by the business cycle or changes in the standard setting (Demerjian 2011, Christensen and Nikolaev 2017). Loan-type fixed effects control for the purpose of the loan and address the concern that treatment and control borrowers may differ in their motives for accessing the loan market. In other words, loan-type fixed effects allow us to compare a borrower that, for example, is financing a leverage buyout with another borrower that is also financing a leverage buyout. Finally, borrower-rating fixed effects allow us to compare two borrowers with the same level of risk (see Section 3.3 and Appendix A for further information on the construction of the control variables). We cluster standard errors by the borrower.

Table 3 reports the results of estimating equation (1). The main explanatory variable of interest is *Defaults different Region and SIC*. Column (1) shows that *Number of Covenants* has a positive and statistically significant relation with past defaults from different geographical areas

[13] In our analyses, controls for loan characteristics are measured at the date of loan initiation as reported by Dealscan, whereas borrower characteristics are measured at the beginning of the fiscal year when the loan is negotiated in order to control for the most recent firm characteristics available at the time the loan is negotiated. Finally, *Quarterly GDP Growth* is measured at the loan initiation quarter in order to control for macroeconomic conditions at loan initiation.

and industries. Columns (2)-(4) provide evidence on Hypothesis 1 with *P-Covenants*, *C-Covenants*, and *Covenants Ratio* as the dependent variables, respectively. For *P-Covenants*, Column (2), we find that the coefficient on *Defaults different Region and SIC* is positive and statistically significant at the 1 percent level, whereas for *C-Covenants*, Column (3), the coefficient is negative and significant at the 5 percent level. Consistent with these results, the coefficient on *Defaults different Region and SIC* is positive and statistically significant when the dependent variable is *Covenants Ratio*, Column (4). The coefficients on the control variables are consistent with prior literature: *Maturity*, *Secured*, and *Zscore* are associated with an increased number of covenants. The associations between the statistically significant control variables and *C-Covenants* vs. *P-Covenants* have opposite signs, in line with Christensen and Nikolaev (2012).

Turning attention to covenant strictness, Column (5) indicates a positive relation between the probability of violating any covenant (*Prob-Violation*) and *Defaults different Region and SIC* that is significant at the 5 percent level. Columns (6) and (7) examine the effect of lenders' defaults on the probability of violating performance and capital covenants, respectively. Consistent with Hypothesis 1, we find a positive and statistically significant relation between *Defaults different Region and SIC* and *Prob-Violation P-Cov*, whereas the coefficient on *Prob-Violation C-Cov* is negative and statistically insignificant.

Overall, the results presented in Table 3 are consistent with Hypothesis 1. Specifically, lenders who experience adverse shocks to their loan portfolios initiate loans with more performance covenants, and set these tighter, while reducing the use of capital covenants. This evidence indicates that lenders' private preferences influence the structure of covenant packages in a way that, subsequent to a negative lender shock, grants lenders more control over borrowers.

4.1.1 Alternative control variables and fixed effect structures

Recall that we exclude defaults in the borrower's region and industry in order to avoid capturing the borrower's geography- or industry-specific risk. As an alternative way to alleviate concerns that factors correlated with the economics of the borrower are driving our results, we augment equation (1) with a more comprehensive fixed-effect structure and additional control variables, and evaluate the sensitivity of the results reported in Table 3.

Table 4, Panel A reports the results from the analysis when we include five additional control variables intended to capture borrower characteristics: *Size*, *ROA*, *Book to Market*, *Leverage*, and *Dividends* (all defined in Section 3.2 and Appendix A). We include these control variables because they capture additional variation in contracting practices (Christensen and Nikolaev 2012). For this analysis, we focus on five different dependent variables: *P-Covenants* (Column 1), *C-Covenants* (Column 2), *Covenants Ratio* (Column 3), *Prob-Violation P-Cov* (Column 4), and *Prob-Violation C-Cov* (Column 5). We find that the coefficients on *Default different Region and SIC* across the alternative specifications are very similar to those reported in Table 3.

In Table 4 Panel B, we focus on concerns related to potential omitted variables correlated with the borrower's industry.[14] For brevity, we only report results with the number and probability of violating performance-covenants as the dependent variables. In Columns (1) and (4), we control for the total defaults experienced by the lender in the borrower's region and industry, *Defaults same Region and SIC*. In both cases, we find that the coefficient on *Defaults same Region and SIC*

[14] Another possible concern is that the number of defaults in the 90 days prior to a new loan also reflects a banking macro-economic shock. Murfin (2012) performs a placebo test (which we do not replicate, for brevity) in which he uses the random reassignment of contracts to placebos (lenders active in the contract year that were not lead arrangers on the current transaction) and repeats the experiment 500 times. The placebo banks fail to report positive and significant coefficients on their recent default experience. This lends additional support to our lender-specific shock measure and supports the hypothesis that it is the lender's own defaults that matter during contracting.

is insignificant, and that the coefficient on *Defaults different Region and SIC* does not materially change as a result of adding the new variable (compared to Table 4 Panel A, Columns (1) and (4)). The lack of a statistically significant coefficient on *Defaults same Region and SIC* suggests that the effect is not driven by lenders' local portfolio considerations. Next, we control for potential industry-specific risks by adding industry fixed effects (Columns (2) and (5)), and industry-by-year fixed effects (Columns (3) and (6)) that control for time-variant and -invariant industry characteristics. In both these alternative specifications, the coefficients on *Defaults different Region and SIC* are statistically significant at the 1 percent level.

Taken together, the results based on the alternative specifications (reported in Table 4) do not change our conclusion in Table 3 that lenders' private preferences affect covenant package structure.

4.1.2. The role of relationship lending

To provide further evidence on Hypothesis 1, we run the analysis specified in equation (1) on the subsample where we expect the effect to be more pronounced. We expect lenders' recent default experiences to significantly affect the contracting terms in new loans specifically for borrowers that have limited opportunities to borrow from other lenders, i.e., have an established relationship with one lender. In the absence of financial frictions (e.g., informational asymmetries), borrowers would refuse tighter covenants motivated by the lender's portfolio considerations by threatening to switch to another lender. However, prior studies suggest that borrowers often establish long-term relationships with their lender (Petersen and Rajan 1994, 1995; Cole 1998). While relationship lending reduces informational asymmetries between the borrower and lender, it also gives the lender an informational advantage over its competitors; this makes it costly for the borrower to switch to a new, less informed lender (Rajan 1992). These switching costs allow

current lenders to extract rents from their borrowers as competition among the lending group is limited (Petersen and Rajan 1994, 1998, Cole 1998). As a result, we expect the effect of lender-specific shocks on contracting practices to be stronger for borrowers that have limited outside options, i.e., rely on ongoing relationships with a small group of lenders.

Table 5 presents the results of estimating equation (1) when we partition the sample into subsamples of borrowers with a low vs. a high number of prior transactions with different lenders (referred to as the number of relationships). We classify borrowers as having a low (high) number of lender relationships if they have worked with less than (more than) the sample median number of lenders in the four transactions before contract initiation (Petersen and Rajan 1994, 1995; Murfin 2012).[15] Low number of past relationships proxies for limited outside opportunities and hence for the presence of relationship lending. For this analysis, we use the specification with the comprehensive set of control variables (as in Table 4, Panel A) and, for brevity, only report results with *P-Covenants, Covenants Ratio*, and the probability of violating a P-Covenant (*Prob-Violation P-Cov*) as the dependent variables.

Columns (1), (3), and (5) show that the associations between *Default different Region and SIC* and *P-Covenants*, *Covenants Ratio*, and *Prob-Violation P-Cov*, respectively, are statistically significant for borrowers with established lending relationships, i.e., who have fewer transactions with different lenders. The coefficients of interest are approximately double those reported in Table 4. In contrast, the coefficients on *Default different Region and SIC* for borrowers with a higher number of past lending relationships are statistically insignificant in all three cases (Columns (2), (4), and (6)). The differences between the two coefficients (for high vs. low number of relationships) are statistically significant.

[15] The current loan is excluded from the lender count to limit concerns that the subsamples were determined endogenously.

Overall, the results in Table 5 are consistent with lender-specific shocks only affecting relationship borrowers (i.e., borrowers with more limited outside options) who find it costly to switch lenders and thus are more likely to agree to the imposition of additional performance covenants. Our results are also consistent with prior work which suggests that relationship borrowers suffer from reduced credit availability following lender shocks (see, e.g., Khwaja and Mian 2008, Chava and Purnanandam 2011, Chodorow-Reich and Falato 2017).

4.2. An alternative measure of lender-specific shocks

Up to this point, our findings suggest that lenders adjust their choice of financial covenants following defaults on loans in their portfolio. While our empirical design is able to isolate a lender-specific shock that is plausibly exogenous to the fundamentals of the borrower, it likely sacrifices the generalizability to a broader (maybe less exogenous) shock. To complement our prior tests, we also examine an alternative, broader measure of lender-specific shocks. Instead of measuring shocks based only on payment defaults, we expand the definition of lender-specific shocks to include significant changes in the tier 1 capital ratio. We then estimate the following regression for both relationship and non-relationship borrowers:

$$Covenants_{i,t} = \beta_0 + \beta_1 Shock\ to\ Tier\ 1_{i,t} + \beta_2 Controls_{i,t} + \lambda_l + \rho_r + \nu_s \times \tau_t + \epsilon_{i,t}, \quad (2)$$

where *Covenants*, as in Table 5, are one of the following: the number of performance covenants (*P-Covenants*), the covenant mix (*Covenants Ratio*), or the probability of violating a performance covenant (*Prob-Violation P-Cov*) in loan contract *i* entered into at time *t*. *Shock to Tier 1* is our proxy for a lender-specific shock to their Tier 1 capital ratio, described in Section 3.1. *Controls* indicates the use of the same set of control variables as in Table 4 Panel A. As before, we control for loan-type and borrower-rating fixed effects, denoted by λ_l, and ρ_r, respectively. However, we

now add fixed effects for the interactions between borrower's region (state) indicators and year, indicated by the term $\nu_s \times \tau_t$. Given the construction of our treatment variable, these interactions absorb time-variant and time-invariant characteristics for each region where borrowers operate.[16] These fixed effects enable us to compare two borrowers in the same state—one with a lender that suffers a significant reduction in Tier 1 capital and one that does not. To correct for serial correlation, we cluster standard errors by borrower.

Results are reported in Table 6. In line with our prior findings and consistent with Hypothesis 1, we find evidence that lenders shift towards performance covenants when their own financial condition deteriorates. Specifically, in Columns (1), (3), and (5), we find that a large negative change in the tier 1 capital ratio during the quarter prior to the negotiation of a new loan is positively associated with both the number and the probability of violating performance covenants for relationship borrowers.[17] Furthermore, in line with our evidence in Table 5, the effect is not statistically significant for non-relationship borrowers (Columns (2), (4), and (6)).

In sum, these findings reinforce our conclusions reported in Tables 3, 4, and 5, and provide further support for Hypothesis 1. Lenders who experience adverse shocks to their loan portfolios and/or a significant financial downturn initiate loans that use performance covenants more frequently as compared to lenders who do not experience adverse shocks. Moreover, these lenders set performance covenants more tightly around the borrower's performance indicators. This

[16] While we have controlled for the economic trends of the borrower's state by relying on state-level default activity outside that state, the different structure of our treatment variable here does not allow us to do so. As a result, we introduce this alternative fixed-effects structure to achieve the same goal.

[17] Results for column (1) are statistically significant at the 10 percent level when using one-tailed statistical significance levels. Results in columns (3) and (5) are significant at the 5 percent and 10 percent levels in two-tailed tests.

evidence is consistent with lenders' private objectives shifting the composition of covenant packages to gain more control over borrowers.[18]

4.3. Covenant composition and corporate investments

We expect the shift in the number of performance covenants (driven by lenders' portfolio considerations and not by the borrower's economic conditions) to change a borrower's investment incentives, consistent with Hypothesis 2. In this section, we examine whether an increase in performance covenants related to lender's shocks leads to a reduction in the borrower's capital and R&D expenditures.

The main challenge to conducting this analysis is that covenants are not randomly assigned to debt contracts. For example, a borrower with a high level of R&D or Capex could arguably appear riskier and would therefore have debt contracts with more and tighter performance covenants than would (*ceteris paribus*) a borrower with a low R&D or Capex level have. As a result, a *naïve* estimation of the effect of performance covenants on investments would be biased upward, i.e., would be biased against finding a negative association between performance covenants and investments.

To obtain an unbiased estimate of the effect of performance covenants on borrower investments, we use variation in performance covenants driven by changes in lender-specific preferences, which is plausibly exogenous to the economics of the borrower. In a two-stage research design, we use the lead arranger's recent default experience outside of the borrower's region and industry (*Defaults different Region and SIC*) as an instrumental variable for the use of

[18] Demerjian et al. (2019) argue that lenders with lower levels of regulatory capital (Tier 1 Ratios) issue loans with looser covenants in order to avoid spurious covenant violations which could further lower regulatory capital. We believe that we examine a distinct economic phenomenon. In our case, we see firms tightening covenants as they learn about deficiencies in their own abilities to assess creditworthiness (i.e., when they experience precipitous declines in regulatory capital), while Demerjian et al. (2019) examine the ways in which lenders with low average levels of regulatory capital use loan terms to avoid further declines in their regulatory capital.

performance covenants in a borrower's contract, *P-Covenants* and *Prob-Violation P-Cov*.[19] The identifying assumption is that the number of defaults a lead arranger experiences outside the borrower's region and industry is orthogonal to the borrower's characteristics and the economic conditions in which the borrower operates (Murfin 2012). Importantly, the use of a two-stage regression allows us to document whether performance covenants represent a channel through which idiosyncratic shocks to lenders propagate to the real sector, which is our Hypothesis 2. In other words, this empirical approach enables us to isolate the variation in corporate investments that is related to the change in the covenant package caused by lender-specific shocks.

In the first stage, we regress measures of the use of performance covenants, *P-Covenants* and *Prob-Violation P-Cov*, on lender-specific shocks (see equation 1). Because, as we have shown previously, this instrument is only relevant for relationship-based loans, we limit the analysis to a subsample of firms that rely on lending relationships. The results are reported in Table 5, Columns (1) and (5), and indicate that *Defaults different Region and SIC* is a relevant instrument for *P-Covenants* and *Prob-Violation P-Cov*, respectively (see Section 4.1.2. for discussion).[20] In the second stage, we examine the following model on the subsample of relationship borrowers:

$$\log(Investment)_{i,t_n} = \beta_0 + \beta_1 \widehat{P\text{-}Covenants}_{\iota,t} + \beta_2 Controls_{i,t} + \tau_t + \lambda_l + \rho_i + \epsilon_{i,t}, \quad (3)$$

[19] We argue that the number of performance covenants and the probability of violating these covenants are two measures of the same underlying economic construct, which we have discussed in section 2. The number of defaults in a lead arranger's portfolio is also used as an instrumental variable in a current working paper of Spyridopoulos (2016). However, while we use the lender's default as a plausibly exogenous shock to the covenant choice (P-covenants vs. C-covenants) and the probability of P-covenants violation (i.e., strictness), studying the effect of performance covenants on a borrower's investment strategy, Spyridopoulos (2016), motivated by an agency framework, uses the lender's default as an instrument for overall contract strictness (similar to Murfin 2012) and studies the effect of covenant strictness on firm performance.

[20] To address concerns that lender defaults is a weak instrument for *P-Covenants*, at the bottom of Table 5, we report the *F*-test for the null hypothesis that the coefficient on the first-stage instrument is zero, and at the bottom of Table 7, we report the *F*-test for the excluded IV. Both tests indicate that *Defaults different Region and SIC* is not a weak instrument.

where the dependent variable, log(*Investment*), is either the logarithm of a borrower's *R&D* or *Capex* for each of the periods from *t* to *t+3*, where *t* is the end of the loan's origination year, and where $\widehat{P\text{-}Covenants}$ are predicted values from the first-stage regression models. We use the same fixed effect structure and set of control variables as in Table 4 Panel A and cluster standard errors at the borrower level. Our main coefficient of interest is β_1, which captures the effect of *P-Covenants* or *Prob-Violation P-Cov* on a borrower's investments. Consistent with Hypothesis 2, we expect β_1 to be negative for both *Capex* and *R&D*.

Table 7, Panel A reports the results from the second-stage IV regression estimated using GMM.[21] In Columns (1)-(4) [(5)-(8)], the dependent variable is the natural log of *R&D* [*Capex*]. As predicted, we document a negative and statistically significant effect for *P-Covenants* on both *R&D* and *Capex*. For *R&D*, the negative effect of *P-Covenants* starts after loan origination and persists until three years after issuance, though the relationship is only significant in a one-tailed test in the third year. For *Capex*, the negative effect of *P-Covenants* persists up to three years after the loan initiation. Our estimates suggest that exogenously adding a performance covenant to a loan contract constrains future R&D and Capex by about 2 and 4 percent, respectively.

For comparison, we estimate corresponding OLS specifications of the relationship between *P-Covenants* and the set of investment variables. Results are reported in Appendix B (Table A1, Panel A). The OLS results mirror those reported in Table 7 and, consistent with our expectations, are a lower economic magnitude, suggesting that performance covenants tend to be included in contracts where borrowers' unobserved characteristics are positively correlated with investments.

[21] We use a GMM specification to better correct for bias caused by endogenous explanatory variables.

This is consistent with a positive association between borrower risk and performance covenants, as we discuss in the beginning of this subsection.[22]

Next, Table 7, Panel B reports the results from the second-stage IV GMM regression for the relation between investment and the probability of violating performance covenants (*Prob-Violation P-Cov*). In Panel B, Columns (1)-(4) [(5)-(8)], the dependent variable is the natural log of *R&D* [*Capex*]. Consistent with Hypothesis 2, we also observe a negative effect for *Prob-Violation P-Cov* on both *R&D* and *Capex* (although the results are somewhat weaker for *R&D*). The negative effect of *Prob-Violation P-Cov* on *R&D* starts in the year following loan origination and persist for three years, though the relationship is only statistically significant in years *t+1* (10 percent level in a two-tailed test) and *t+2* (10 percent level in a one-tailed test). In the case of *Capex*, the statistically significant negative effect of *Prob-Violation P-Cov* starts after loan origination and persists for up to three years following debt issuance.

As before, we compare the IV estimates in Table 7 Panel B to those based on OLS specifications. Results are reported in Appendix B (Table A1, Panel B). The OLS estimates on the variables of interest are also negative and statistically significant. As in Panel A, they are smaller in magnitude than the results for the IV regressions (see the above discussion for why OLS results are biased toward zero).

Overall, the results in Table 7 are consistent with an increase in performance covenants and their strictness (not explained by a borrower's fundamentals) reducing the amount of R&D and Capex. Nevertheless, we caution the reader of the general drawback of instrument-based estimation, namely, that the treatment effect is only estimated for the subset of borrowers for which the instrument is relevant. To the extent that there is heterogeneity in the treatment effect, our

[22] For example, a borrower with a high level of R&D or Capex could arguably appear riskier and would therefore have debt contracts with more performance covenants than a borrower with low levels of *R&D* or *Capex* (*ceteris paribus*).

estimates can only be interpreted as the *local average treatment effect* (LATE) of performance covenants on borrowers that respond to the instrument (i.e., *compliers*; see Imbens and Angrist, 1994). However, we argue that this local effect (i.e., the effect of performance covenants motivated by idiosyncratic shocks to lenders) is important in its own right and constitutes evidence on Hypothesis 2. Overall, our results suggest that performance covenants are a channel through which idiosyncratic shocks to lenders are propagated to the corporate sector.

4.4. Changes in borrower composition

There are two possible economic mechanisms that can explain why performance covenants reduce corporate investments. First, in response to the heavier use of performance covenants, borrowers can alter their investment policies. Alternatively, the shift in contracting terms may cause changes to the composition of borrowers in the lender's portfolio. The borrower composition effect may occur if the impact of performance covenants on investment is costlier to borrowers with a higher level of desired investments, which incentivizes them to refrain from borrowing instead of agreeing to a more extensive use of covenants.

To test whether borrower composition effects occur, we create a panel of lender portfolio observations at the borrower's industry-state-year level and examine the association between defaults in lenders' portfolios and the fraction of borrowers that engages in R&D for loans initiated over the next calendar year. Specifically, we estimate the following OLS fixed effects regression:

$$No.R\&D\ Firms/All\ Firms_{i,j,z,t+1} = \beta_0 + \beta_1 Defaults_{i,j,z,t} + \lambda_j + \tau_t + \rho_i + \epsilon_{i,t}, \qquad (4)$$

where *No. R&D Firms/All Firms* is the ratio of loans issued to borrowers with R&D expenditures over the total number of loans issued by lender *i* to firms in industry *j* and region *z* in each calendar year *t+1*, where *t* is the loan's origination year. *Defaults* is the total number of defaults in the loan portfolio of lender *i* that are not in industry *j* or region *z* in each calendar year *t*. In all specifications,

to control for time trends in contracting practices, industry-specific risk and lender characteristics, we add three alternative sets of fixed effects: (1) industry fixed effects, λ_j, which control for time-invariant industry characteristics; (2) year fixed effects, τ_t, which control for arbitrary time trends; and (3) lender fixed effects, ρ_i, which control for time-invariant lender characteristics such as lending preferences. In one specification, we also include industry-by-year fixed effects ($\lambda_j \times \tau_t$), which control for time-variant and -invariant industry characteristics. The coefficient on *Defaults*, β_1, captures the borrower composition effect. A negative β_1 coefficient is expected if the fraction of borrowers with R&D decreases in response to shocks in lenders' portfolios.

The estimates for equation (4) are reported in Table 8. In all specifications, we find that the coefficient on *Defaults* is negative and statistically significant at the 1 percent level, which supports the borrower composition effect. However, the magnitude of this coefficient is rather small (–0.0142; the unconditional mean of *No. R&D Firms/All Firms* is 0.328 and the standard deviation is 0.463) and not economically different across the three different fixed-effect specifications. The findings suggest that after a lender experiences defaults in loan portfolio, the proportion of new borrowers with positive R&D expenditures is slightly reduced (e.g., a default will reduce the ratio of *No. R&D Firms/All Firms* from 32.8 percent to 31.4 percent). These findings indicate that borrower selection occurs, but is moderate.

4.5. Defaults in lenders' portfolios and investments by existing borrowers

So far, our analysis has examined whether plausibly exogenous variation in performance covenants in new loans affects corporate investment ex ante. In this section, we use cross-sectional variation in the existing contracts to examine whether the covenants in place change the borrower's

exposure to lender-specific shocks.[23] We expect that when an existing contract relies more extensively on performance covenants, lenders have more control over the borrower. As shown above, lenders can exploit this control when experiencing an unanticipated financial shock in order to manage the riskiness of their portfolio. For example, lenders could take advantage of covenant violations to influence investment policies in a way that would reduce exposure to credit risk (e.g., Chava and Roberts 2008; Roberts and Sufi 2009a,b). Even outside of a covenant violation, borrowers subject to performance covenants should anticipate that possible covenant violations will have a higher cost, and may change their investment policies to reduce the likelihood of future covenant violations. Importantly, examining the contracts already in place before the financial shock isolates potential borrower selection effects because the borrower composition effects can occur only for new contracts (unless the contracting parties anticipated the lender-specific shocks, which seems implausible).

To test whether lender-specific shocks affect borrowers when contracts are in place, we allow the effect of future, lender-portfolio shocks on borrowers' investments to vary with cross-sectional variation in performance covenants. Specifically, we estimate the following OLS fixed effects regression for relationship borrowers partitioned on the use of performance covenants:

$$\log(Investment)_{i,t+2} = \beta_0 + \beta_1 Defaults_{i,t+1} + \beta_2 Controls_{i,t} + \tau_t + \lambda_i + \rho_i + \epsilon_{i,t}, \quad (5)$$

where the dependent variable is either the logarithm of *R&D* or *Capex* for borrower *i* in year *t*+2, where *t* is the loan's origination year. *Defaults* are defined as the annual count of defaults outside borrower *i*'s geographic region and industry in the lender's portfolio in year *t*+1. To capture cross-sectional variation in the number of performance covenants that are independent from *Defaults*,

[23] This alternative identification strategy does not rely on an IV approach and addresses the general limitation of IV designs that the exclusion restriction cannot be directly tested.

we measure *Defaults* at $t + 1$, which is the end of one calendar year after the loan's origination year (t). In other words, we avoid capturing defaults that may determine the number of performance covenants included in the loan contract. We use the same set of control variables as in Table 4 Panel A, and as previously, we cluster standard errors at the borrower level.

In Table 9 we report results for the pooled sample (Columns (1) and (4)), the subsample of contracts with performance covenants (Columns (2) and (5)), and the subsample of contracts without performance covenants (Columns (3) and (6)). In line with Hypothesis 2, borrowers should anticipate that covenant violations will likely have more severe consequences after their lender experiences a shock. Hence, we expect β_1 to be negative for borrowers with *P-covenants* in place (Columns (1), (2), (4), and (5)). Table 9 indicates that borrower investment in both R&D and Capex is lower when the lead arranger experiences payment defaults after loan's initiation (Columns (1) and (4) of Table 9). We also find that the effect of lender-specific shocks is more (less) pronounced and statistically significant (insignificant) for contracts that rely (do not rely) on performance covenants. This is in line with our expectations.

Overall, although the economic significance is smaller for borrowers with existing contracts than for those with new contracts (see Table 7),[24] these results reconcile with our arguments in Section 2 and triangulate the findings from the two-stage design, lending further support to the hypothesis that lender-specific portfolio shocks that are unrelated to borrower fundamentals can affect the borrower's investment decisions.

[24] As expected, the results in Table 9 are economically weaker than those for the two-stage estimation reported in Table 7. In the analysis of Table 7, borrowers arguably face an additional economic cost when they sign a loan that includes an additional P-covenant that is orthogonal to the borrower's fundamentals. On the contrary, in the analysis of Table 9, we show that some borrowers may perceive that defaults in their lender's portfolio will not have immediate, severe consequences on their own operations. Hence, the effect documented in Table 9 is consistent with that documented in Table 7, though economically weaker.

5. Conclusion

Accounting information plays an important role in facilitating contracts between lenders and borrowers. While it is well established that accounting-based covenants improve contract efficiency and enable access to capital, the costs to borrowers associated with the use of these covenants are not well understood. We focus on one such cost, specifically, that lenders can use covenants to pursue their own private objectives. We argue that in addition to the agency and information problems, which are commonly known to determine the optimal use of covenants, lenders' portfolio considerations influence their choice of covenant package. We further argue that covenants can act as a transmission mechanism through which idiosyncratic shocks to lenders propagate to the corporate sector.

Our main identification strategy exploits lender-specific shock, namely, recent payment defaults that the lead arranger experiences outside a borrower's region and industry. Such shocks are arguably exogenous to the borrower's economic conditions, but are still expected to influence lenders' preferences of financial covenants. We find that lenders respond to recent payment default shocks by changing the composition of covenants towards performance-based covenants and away from capital-based covenants, as well as increase the strictness of performance covenants. Consistent with our hypotheses, we show that this effect is driven by borrowers that rely on relationship-based lending and cannot be attributed to borrowers' economic fundamentals. Our analysis of corporate investment decisions reveals that an increase in the number and strictness of performance covenants constrains the borrowers' future capital expenditures and their research and development activities. Overall, our findings suggest that financial covenants play a role in propagating lender-specific shocks to the corporate sector.

Appendix A: Variable Definitions

Variables	Description
Lender's shocks:	
Defaults different Region and SIC	The number of payment defaults in a lead arranger's loan portfolio that are outside the borrower's industry and region. We measure this variable by counting the number of borrowers that the S&P Compustat's Rating Database reports as in default or selective default during the 90 days leading up to a new loan.
Defaults same Region and SIC	The number of payment defaults in a lead arranger's loan portfolio within the borrower's industry and region (used in Table 4 Panel B only).
Defaults	The annual number of payment defaults in a lead arranger's loan portfolio outside of the borrower's industry and region (used in Tables 8 and 9 only).
Shock to Tier 1	Indicates quarterly changes in Tier 1 Capital Ratio, measured on the quarter leading up to a new loan. This variable is equal to -1 if quarterly changes are at or below the 25th percentile, is equal to 0 if quarterly changes are greater than the 25th percentile and less than or equal to the 75th percentile, and is equal to 1 if quarterly changes are above the 75th percentile of the annual distribution.
Loan Characteristics:	
Maturity	The number of months remaining to maturity at contract initiation.
Loan size	The natural logarithm of the loan amount in US dollars.
Secured	An indicator equal to 1 if the loan is secured and 0 otherwise.
Number of lenders	The number of non-lead arranger banks participating in the loan.
Loan type	Separate indicators for the following loan categories reported in DealScan: leverage buyout, corporate purposes, working capital debt, debt repayment, CP backup, or others.

Borrower Characteristics:

R&D	Research and development expenses scaled by total revenues.
Capex	Capital expenditures scaled by the book value of total assets.
Size	The natural logarithm of the book value of total assets.
ROA	Return on assets computed as income before extraordinary items scaled by average total assets over the last two years.
Book-to-Market	The book value of total assets scaled by total debt plus the market value of equity.
Leverage	Long-term debt scaled by the market value of total assets.
Zscore	The Altman's Z-score index, computed as 3.3 pretax operating income/total assets + sales/total assets + 1.4 retained earnings/total assets + 1.2 (current assets − current liabilities)/total assets + 0.6 market value of equity/total liabilities.
Dividends	Dividend yield computed as the ratio of common dividends to the market value of equity.
Borrower rating	The borrower S&P senior, long-term debt rating.

Covenant Variables:

Number of Covenants	The total number of covenants in a loan contract.
Number of C-Covenants	Capital covenants (C-Covenants), defined as the sum of (1) Quick ratio, (2) Current ratio, (3) Debt-to-equity ratio, (4) Loan-to-value ratio, (5) Ratio of debt to tangible net worth ratio, (6) Leverage ratio, (7) Senior leverage ratio, and (8) Net Worth requirement.
Number of P-Covenants	Performance covenants (P-Covenants), defined as the sum of (1) Cash interest coverage ratio, (2) Debt service coverage ratio, (3) Level of EBITDA, (4) Fixed-charge coverage ratio, (5) Interest coverage ratio, (6) Ratio of debt to EBITDA, and (7) Ratio of senior debt to EBITDA.
Covenants ratio	The fraction of covenants that are P-covenants, calculated as P-covenants/(P-covenants+C-covenants).

Prob-Violation	We follow Demerjian and Owens (2016) and use the simulated probability of violating at least one covenant during the quarter after initiation. To accomplish this, the financial measures underlying each covenant are simulated and the frequency of violations generated by the forecasts is observed. Data are available from Ed Owens website at https://sites.google.com/site/edowensphd/research.
Prob-Violation P-Cov	We follow Demerjian, Owens, and Sokolowski (2019) and use the simulated probability of violating at least one performance covenant during the quarter after initiation. Performance covenants are defined as in Christensen and Nikolaev (2012), data is available from Ed Owens website at https://sites.google.com/site/edowensphd/research.
Prob-Violation C-Cov	We follow Demerjian, Owens, and Sokolowski (2019) and use the simulated probability of violating at least one capital covenant during the quarter after initiation. Capital covenants are defined as in Christensen and Nikolaev (2012), data is available from Ed Owens website at https://sites.google.com/site/edowensphd/research.
Other Variables:	
No. R&D Firms/All Firms	The ratio of loans issued to borrowers with R&D over the total number of loans issued by lender *i* to firms in industry *j* and US state *z* in each calendar year *t* (used in Table 7 only).
Quarterly GDP growth	The quarterly growth of the gross domestic product.

Continuous variables are winsorized at the 1st and 99th percentiles, and count variables are winsorized at the 99th percentile.

Appendix B: Additional Analyses

Panel A: Ordinary Least Squares

	Dependent Variable							
	Log Research and Development (*R&D*)				Log Capital Expenditures (*Capex*)			
	$R\&D_{(t)}$	$R\&D_{(t+1)}$	$R\&D_{(t+2)}$	$R\&D_{(t+3)}$	$Capex_{(t)}$	$Capex_{(t+1)}$	$Capex_{(t+2)}$	$Capex_{(t+3)}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Effect:								
P-Covenants	-0.0046***	-0.0046***	-0.0045***	-0.0040***	-0.0103***	-0.0089***	-0.0074***	-0.0068***
	(0.0009)	(0.0009)	(0.0010)	(0.0010)	(0.0019)	(0.0018)	(0.0017)	(0.0018)
Control Variables (Table 4)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects:								
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan type	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower rating	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,850	10,083	9,199	8,424	10,755	9,994	9,130	8,355
R-squared	0.198	0.205	0.195	0.192	0.068	0.064	0.070	0.067

Panel B: Ordinary Least Squares

	Dependent Variable							
	Log Research and Development (*R&D*)				Log Capital Expenditures (*Capex*)			
	$R\&D_{(t)}$	$R\&D_{(t+1)}$	$R\&D_{(t+2)}$	$R\&D_{(t+3)}$	$Capex_{(t)}$	$Capex_{(t+1)}$	$Capex_{(t+2)}$	$Capex_{(t+3)}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Effect:								
Prob-Violation P-Cov	-0.0161***	-0.0152***	-0.0142***	-0.0148***	-0.0178***	-0.0182***	-0.0155***	-0.0140***
	(0.0018)	(0.0018)	(0.0019)	(0.0019)	(0.0037)	(0.0036)	(0.0033)	(0.0033)
Control Variables (Table 4)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects:								
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan type	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower rating	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,652	8,951	8,153	7,463	9,571	8,872	8,088	7,399
R-squared	0.200	0.208	0.197	0.199	0.065	0.066	0.071	0.069

For comparison with results in Table 7 Panel A, Table A.1 Panel A reports results for the OLS fixed-effects regression model of P-Covenants and borrowers' future investments in a sample of relationship borrowers. Robust standard errors are reported in parentheses and are clustered at the borrower level. The superscripts ***, **, and * indicate two-tailed statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively. For comparison with results in Table 7 Panel B, Table A.1 Panel B reports results for the OLS fixed-effects regression model of *Prob-Violation P-Cov* and borrowers' future investments in a sample of relationship borrowers. Robust standard errors are reported in parentheses and are clustered at the borrower level. The superscripts ***, **, and * indicate two-tailed statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively.

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Table 1: Descriptive Statistics

	Mean	Sd	P25	P50	P75
Lender's shocks:					
Defaults different Region and SIC	2.2055	3.8281	0.0000	0.0000	3.0000
Shock to Tier 1	-0.0728	0.6986	-1.0000	0.0000	0.0000
Loan Characteristics:					
Maturity	47.9396	23.2204	33.1765	48.0000	60.0000
Loan size	18.9511	1.7765	17.7275	19.1124	20.2124
Secured	0.6670	0.4713	0.0000	1.0000	1.0000
Number of lenders	9.6103	10.5530	2.0000	6.0000	13.0000
Borrower Characteristics:					
R&D	0.0207	0.0510	0.0000	0.0000	0.0137
Capex	0.0764	0.1020	0.0236	0.0439	0.0843
Size	6.9204	1.9915	5.4916	6.9012	8.3139
ROA	0.0197	0.1213	-0.0034	0.0381	0.0762
Book-to-Market	0.4506	1.4085	0.2634	0.4691	0.7463
Leverage	0.1422	0.1889	0.0094	0.1123	0.2493
Zscore	2.1362	1.5665	1.2126	2.0348	2.9801
Dividends	0.0085	0.0171	0.0000	0.0000	0.0101
Covenant Variables:					
Number of Covenants	2.7158	1.1693	2.0000	3.0000	3.0000
C-Covenants	0.5903	0.6785	0.0000	0.0000	1.0000
P-Covenants	1.7021	0.9878	1.0000	2.0000	2.0000
Covenants ratio	0.7290	0.3358	0.5000	1.0000	1.0000
Prob-Violation	0.4087	0.4215	0.0170	0.1710	0.9360
Prob-Violation P-Cov	0.3518	0.4145	0.0000	0.0850	0.8750
Prob-Violation C-Cov	0.1091	0.2642	0.0000	0.0000	0.0440

Table 1 presents summary statistics for the variables used in the analysis. For each variable, we report the mean, the standard deviation, the first quartile, the median, and the third quartile of the distribution. We obtain loan characteristics from Dealscan and borrower characteristics from Compustat. We restrict the sample to Dealscan observations that link to Compustat. Contracts without covenant information are excluded. Deals with multiple credit facilities are aggregated and considered at the deal level. Our final sample contains 21,465 observations from 1996 to 2013. All variables are defined in Appendix A.

Table 2: Correlation Matrix

	Number of Covenants	*P-Covenants*	*C-Covenants*	*Covenants ratio*	*Prob-Violation*	*Prob-Violation P-Cov*
P-Covenants	0.6695	1				
C-Covenants	0.2564	-0.3824	1			
Covenants ratio	0.0972	0.6585	-0.8274	1		
Prob-Violation	0.3591	0.2671	*0.0138*	0.1312	1	
Prob-Violation P-Cov	0.3431	0.3512	-0.1236	0.2644	0.8991	1
Prob-Violation C-Cov	0.2054	-0.0762	0.3461	-0.2483	0.4403	0.1135

Table 2 presents the Pearson correlation statistics for individual covenant types. Coefficients are statistically significant at the 1 percent level, except for the coefficients indicated in italics, which are significant at the 5 percent level. Performance covenants (*P-Covenants*) are defined as the sum of (1) Cash-interest coverage ratio, (2) Debt-service coverage ratio, (3) Level of EBITDA, (4) Fixed-charge coverage ratio, (5) Interest coverage ratio, (6) Ratio of debt to EBITDA, and (7) Ratio of senior debt to EBITDA. Capital covenants (*C-covenants*) are defined as the sum of (1) Quick ratio, (2) Current ratio, (3) Debt-to-equity ratio, (4) Loan-to-value ratio, (5) Ratio of debt to tangible net worth, (6) Leverage ratio, (7) Senior leverage ratio, and (8) Net Worth requirement. *Covenants ratio* is defined as P-covenants divided by the sum of P- and C-covenants. *Prob-Violation* is the probability that at least one covenant is violated in the first quarter after contract initiation. *Prob-Violation C-Cov* is the probability that at least one capital covenant is violated in the first quarter after contract initiation. *Prob-Violation P-Cov* is the probability that at least one performance covenant is violated in the first quarter after contract initiation. All data on the probabilities of violation is from Ed Owens' website: https://sites.google.com/site/edowensphd/research.

Table 3: Covenants and Lender's Recent Defaults

Dependent variable:	Covenant Counts				Probability of Covenant Violation		
	Number of Covenants	*P-Covenants*	*C-Covenants*	*Covenants ratio*	*Prob-Violation*	*Prob-Violation P-Cov*	*Prob-Violation C-Cov*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment Effect:							
Defaults different Region and SIC	0.0126***	0.0170***	-0.0050**	0.0039**	0.0033**	0.0047***	-0.0005
	(0.0035)	(0.0034)	(0.0021)	(0.0012)	(0.0013)	(0.0013)	(0.0008)
Control Variables:							
Maturity	0.0056***	0.0067***	-0.0017***	0.0012***	-0.0004	0.0001	-0.0007***
	(0.0009)	(0.0009)	(0.0005)	(0.0003)	(0.0003)	(0.0003)	(0.0002)
Loan Size	-0.0263	0.1014***	-0.0911***	0.0480***	-0.0144**	0.0012	-0.0249***
	(0.0176)	(0.0157)	(0.0137)	(0.0060)	(0.0054)	(0.0061)	(0.0043)
Secured	0.3471***	0.3085***	-0.1214***	0.0868***	0.1351***	0.1213***	0.0403***
	(0.0458)	(0.0504)	(0.0302)	(0.0182)	(0.0160)	(0.0165)	(0.0087)
Number of Lenders	0.0016	0.0023	0.0008	-0.0005	0.0001	-0.0006	0.0011
	(0.0022)	(0.0022)	(0.0019)	(0.0008)	(0.0008)	(0.0009)	(0.0007)
Zscore	0.0323***	0.0170**	0.0013	0.0104***	-0.0547***	-0.0446***	-0.0212***
	(0.0087)	(0.0085)	(0.0055)	(0.0031)	(0.0037)	(0.0039)	(0.0022)
Quarterly GDP Growth	0.0107	0.0057	-0.0002	-0.0005	-0.0014	-0.0009	0.0006
	(0.0073)	(0.0075)	(0.0038)	(0.0020)	(0.0028)	(0.0028)	(0.0019)
Fixed Effects:							
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan type	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower rating	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21,458	20,891	20,891	20,891	18,429	18,429	18,429
R-squared	0.239	0.262	0.239	0.249	0.200	0.167	0.099
F-Test (p-value)	0.000	0.000	0.016	0.002	0.009	0.000	0.514

Table 3 presents results for the OLS fixed-effects regression model described in equation 1. In column (1), we regress the *Number of Covenants* on our treatment effect (*Defaults different Region and SIC*). In column (2), we regress the number of *P-Covenants* on *Defaults different Region and SIC*. In column (3), we regress the number of *C-Covenants* on *Defaults different Region and SIC*. In column (4), we regress the *Covenants Ratio* on *Defaults different Region and SIC*. In column (5), we regress the *Prob-Violation* on *Defaults different Region and SIC*. In column (6), we regress the *Prob-Violation P-Cov* on *Defaults different Region and SIC*. In column (7), we regress the *Prob-Violation C-Cov* on *Defaults different Region and SIC*. We control for: *Maturity, Loan size, Secured, Number of Lenders, Zscore,* and *Quarterly GDP growth*. We include fixed effects for *Year*, *Loan type,* and *Borrower rating*. All variables are defined in Appendix A. At the bottom of the table, we report the results of the (p-value) F-test for the coefficient on the treatment effect. Robust standard errors are reported in parentheses and are clustered at the borrower level. The superscripts ***, **, and * indicate two-tailed statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively. † indicates one-tailed significance at the 10 percent level.

Table 4: Covenants, Lender's Recent Defaults, and Alternative Control Variables

Panel A: Alternative Borrower-level Control Variables

Dependent variable:	Covenant Counts			Probability of Covenant Violation	
	P-Covenants	*C-Covenants*	*Covenants ratio*	*Prob-Violation P-Cov*	*Prob-Violation C-Cov*
	(1)	(2)	(3)	(4)	(5)
Treatment Effect:					
Defaults different Region and SIC	0.0148***	-0.0045**	0.0035**	0.0045***	-0.0005
	(0.0033)	(0.0019)	(0.0012)	(0.0012)	(0.0008)
Additional Control Variables:					
Size	-0.1336***	0.0503**	-0.0330***	-0.0507***	0.0010
	(0.0138)	(0.0181)	(0.0061)	(0.0061)	(0.0075)
ROA	0.5901***	0.3543***	0.0048	-0.3787***	-0.0894**
	(0.1148)	(0.0782)	(0.0390)	(0.0577)	(0.0421)
Book to Market	-0.0369***	0.0278***	-0.0136***	-0.0013	0.0049**
	(0.0079)	(0.0041)	(0.0020)	(0.0040)	(0.0022)
Leverage	0.3206***	-0.0554	0.0349	0.0321	0.0006
	(0.0798)	(0.0479)	(0.0242)	(0.0346)	(0.0223)
Dividends	-4.6406***	2.3242***	-1.5787***	-0.1874	0.2062
	(0.8643)	(0.5553)	(0.3313)	(0.3530)	(0.2546)
Control variables also included in Table 3:					
Maturity	0.0061***	-0.0017***	0.0011***	0.0003	-0.0006**
	(0.0009)	(0.0005)	(0.0003)	(0.0003)	(0.0002)
Loan size	0.1832***	-0.1262***	0.0699***	0.0358***	-0.0247**
	(0.0178)	(0.0216)	(0.0077)	(0.0062)	(0.0083)
Secured	0.2222***	-0.0772**	0.0615***	0.0893***	0.0417***
	(0.0492)	(0.0296)	(0.0180)	(0.0166)	(0.0106)
Number of lenders	0.0044**	0.0002	-0.0001	-0.0001	0.0010
	(0.0020)	(0.0015)	(0.0007)	(0.0008)	(0.0006)
Zscore	0.0066	-0.0196**	0.0132***	-0.0254***	-0.0176***
	(0.0104)	(0.0070)	(0.0037)	(0.0048)	(0.0029)
Quarterly GDP Growth	0.0057	-0.0001	-0.0004	-0.0016	0.0004
	(0.0072)	(0.0037)	(0.0020)	(0.0027)	(0.0019)
Fixed Effects:					
Year	Yes	Yes	Yes	Yes	Yes
Loan type	Yes	Yes	Yes	Yes	Yes
Borrower rating	Yes	Yes	Yes	Yes	Yes
Observations	20,671	20,671	20,671	18,260	18,260
R-squared	0.293	0.254	0.267	0.187	0.100
F-Test (p-value)	0.000	0.020	0.004	0.000	0.554

Table 4: Continued

Panel B: Alternative Industry-level Control Variables

Dependent variable:	*P-Covenants*			*Prob-Violation P-Cov*		
	Defaults in same Region and Industry	Industry Fixed Effects	Industry*Year Fixed Effects	Defaults in same Region and Industry	Industry Fixed Effects	Industry*Year Fixed Effects
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect:						
Defaults different Region and SIC	0.0152***	0.0135***	0.0130***	0.0046***	0.0045***	0.0047***
	(0.0036)	(0.0030)	(0.0026)	(0.0013)	(0.0012)	(0.0012)
Defaults same Region and SIC	-0.0122	—	—	-0.0034	—	—
	(0.0176)			(0.0064)		
Control Variables (Table 4 Panel A)	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects:						
Year	Yes	Yes	No	Yes	Yes	No
Loan type	Yes	Yes	Yes	Yes	Yes	Yes
Borrower rating	Yes	Yes	Yes	Yes	Yes	Yes
Industry	No	Yes	No	No	Yes	No
*Industry*Year*	No	No	Yes	No	No	Yes
Observations	20,671	20,671	20,671	18,260	18,260	18,260
R-squared	0.293	0.346	0.403	0.187	0.214	0.272
F-Test (p-value)	0.000	0.000	0.000	0.000	0.000	0.000

Table 4 presents results for a robustness test to the OLS fixed-effects regression model described in equation 1. In Panel A, we expand the Murfin (2012) model (Eq. 1) by adding additional controls for borrower characteristics. In particular, we include controls for: *Size, ROA, Book to Market, Leverage, Zscore,* and *Dividends*. All variables are defined in Appendix A. In Panel B, we regress *P-covenants* (columns (1)-(3)) and *Prob-Violation P-Cov* (columns (4)-(6)) on *Defaults different Region and SIC*. Consistent with Table 4 Panel A, we control for *Size, ROA, Book to Market, Leverage, Zscore, Dividends, Maturity, Loan size, Secured, Number of lenders, Quarterly GDP growth.* In addition to the controls reported in Table 4 Panel A, in column (1) of Panel B, we control for the number of defaults in the industry and region of the current borrower. In column (2), we also include *industry-fixed effects*, while in column (3), we *interact industry fixed effects* with *year fixed effects*. Columns (4)-(6) repeat this analysis with *Prob-Violation P-Cov*. At the bottom of the table we report the results of the (p-value) F-test for the coefficient on the treatment effect. Robust standard errors are reported in parentheses and are clustered at the borrower level. The superscripts ***, **, and * indicate two-tailed statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively.

Table 5: Covenants, Lender's Recent Defaults, and Borrower-Lender Relationships

Dependent variable:	*P-Covenants*		*Covenants ratio*		*Prob-Violation P-Cov*	
Sample:	Low no. of Relationships	High no. of Relationships	Low no. of Relationships	High no. of Relationships	Low no. of Relationships	High no. of Relationships
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect (Instrument):						
Defaults different Region and SIC	0.0256***	0.0024	0.0064***	0.0007	0.0084***	0.0019
	(0.0053)	(0.0033)	(0.0017)	(0.0013)	(0.0020)	(0.0015)
Test Low = High no. of Relationships (p-value)	0.000		0.007		0.007	
Control Variables (Table 4)	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects:						
Year	Yes	Yes	Yes	Yes	Yes	Yes
Loan type	Yes	Yes	Yes	Yes	Yes	Yes
Borrower rating	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,225	9,446	11,225	9,446	9,715	8,545
R-squared	0.271	0.381	0.274	0.274	0.138	0.257
F-Test (p-value)	0.000	0.471	0.000	0.604	0.000	0.193

Table 5 presents results for the OLS fixed-effects regression model described in equation 1. Unlike our earlier analyses, we estimate the breadth of lender relationships available to borrowers. In particular, we count the number of banks that have lent to a given borrower (going back up to four transactions), and split the borrowers into samples where the number of lenders used in the prior four transactions is less than (Columns 1, 3, and 5) or greater than (Columns 2, 4, and 6) the median. Columns (1) and (2) report results for the effect of *Defaults different Region and SIC* on *P-covenants*, Columns (3) and (4) report results for the effect of *Defaults different Region and SIC* on the *Covenants ratio*, and Columns (5) and (6) report results for the effect of *Defaults different Region and SIC* on *Prob-Violation P-Cov*. Consistent with Table 4 Panel A, we control for *Size, ROA, Book to Market, Leverage, Zscore, Dividends, Maturity, Loan size, Secured, Number of lenders, Quarterly GDP growth*, and we include fixed effects for *Year, Loan type,* and *Borrower rating*. All variables are defined in Appendix A. (Note that Column 1 (Column 5) represents the first-stage regression for the analysis reported in Table 7 Panel A (Table 7 Panel B)). We report the results (p-value) of seemingly unrelated estimation tests of the hypothesis that the treatment effect differs between samples. We also report the results of the (p-value) F-test for the coefficient on the treatment effect. Robust standard errors are reported in parentheses and are clustered at the borrower level. The superscripts ***, **, and * indicate two-tailed statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively.

Table 6: Covenants, Lender's Recent Shock to Tier 1 Capital, and Borrower-Lender Relationships

Dependent variable:	*P-Covenants*		*Covenants ratio*		*Prob-Violation P-Cov*	
Sample:	Low no. of Relationships	High no. of Relationships	Low no. of Relationships	High no. of Relationships	Low no. of Relationships	High no. of Relationships
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect (Instrument):						
Shock to Tier 1	-0.0318†	-0.0109	-0.0220**	-0.0104	-0.0205*	-0.0081
	(0.0230)	(0.0192)	(0.0076)	(0.0070)	(0.0122)	(0.0098)
Test Low = High no. of Relationships (p-value)	0.4875		0.1329		0.431	
Control Variables (Table 4)	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects:						
*Year*BorrowerState*	Yes	Yes	Yes	Yes	Yes	Yes
Loan type	Yes	Yes	Yes	Yes	Yes	Yes
Borrower rating	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,239	5,400	5,239	5,400	4,495	4,906
R-squared	0.362	0.453	0.337	0.333	0.264	0.321
F-Test (p-value)	0.166	0.570	0.004	0.140	0.093	0.410

Table 6 presents results for the OLS fixed-effects regression model described in equation 2. We use a different measure of lender's shock, *Shock to Tier 1*, which measures quarterly changes in the Tier 1 Capital Ratio. Similar to the analysis in Table 5, we estimate the breadth of lender relationships available to borrowers. In particular, we count the number of banks that have lent to a given borrower (going back up to four transactions) and split the borrowers into samples where the number of lenders used in the prior four transactions is less than (Columns 1, 3, and 5) or greater than (Columns 2, 4, and 6) the median. Columns (1) and (2) report results for the effect of *Shock to Tier 1* on *P-covenants*, Columns (3) and (4) report results for the effect of *Shock to Tier 1* on the *Covenants ratio*, and Columns (5) and (6) report results for the effect of *Shock to Tier 1* on the *Prob-Violation P-Cov*. Consistent with Table 4 Panel A, we control for *Size, ROA, Book to Market, Leverage, Zscore, Dividends, Maturity, Loan size, Secured, Number of lenders, Quarterly GDP growth*, and we include fixed effects for *Borrower rating* and *Loan type*. Unlike our prior analysis, we control for the local economic conditions of the borrower by introducing fixed effects for *Borrower State* interacted with *Year*. All variables are defined in Appendix A. We report the results (p-value) of seemingly unrelated estimation tests of the hypothesis that the treatment effect differs between samples. We also report the results of the (p-value) F-test for the coefficient on the treatment effect. Robust standard errors are reported in parentheses and are clustered at the borrower level. The superscripts ***, **, and * indicate two-tailed statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively. † indicates one-tailed significance at the 10 percent level.

Table 7: The Effect of Covenants on Investments in Relationship Loans

Panel A: II-Stage GMM IV Regression

	Dependent Variable							
	Log Research and Development (*R&D*)				Log Capital Expenditures (*Capex*)			
	$R\&D_{(t)}$	$R\&D_{(t+1)}$	$R\&D_{(t+2)}$	$R\&D_{(t+3)}$	$Capex_{(t)}$	$Capex_{(t+1)}$	$Capex_{(t+2)}$	$Capex_{(t+3)}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Effect:								
P-Covenants	-0.0180*	-0.0226**	-0.0204**	-0.0154†	-0.0520**	-0.0392**	-0.0593**	-0.0441**
	(0.0095)	(0.0091)	(0.0099)	(0.0106)	(0.0187)	(0.0168)	(0.0201)	(0.0203)
Control Variables (Table 4)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects:								
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan type	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower rating	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,850	10,083	9,199	8,424	10,755	9,994	9,130	8,355
R-squared	0.197	0.204	0.194	0.192	0.065	0.062	0.071	0.067
F-Test excluded IV	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Panel B: II-Stage GMM IV Regression

	Dependent Variable							
	Log Research and Development (*R&D*)				Log Capital Expenditures (*Capex*)			
	$R\&D_{(t)}$	$R\&D_{(t+1)}$	$R\&D_{(t+2)}$	$R\&D_{(t+3)}$	$Capex_{(t)}$	$Capex_{(t+1)}$	$Capex_{(t+2)}$	$Capex_{(t+3)}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Effect:								
Prob-Violation P-Cov	-0.0273	-0.0413*	-0.0455†	-0.0364	-0.1389**	-0.1375**	-0.2134**	-0.1549**
	(0.0234)	(0.0231)	(0.0297)	(0.0321)	(0.0515)	(0.0516)	(0.0792)	(0.0751)
Control Variables (Table 4)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects:								
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan type	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower rating	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,652	8,951	8,153	7,463	9,571	8,872	8,088	7,399
R-squared	0.188	0.197	0.187	0.188	0.066	0.066	0.075	0.071
F-Test excluded IV	0.000	0.000	0.001	0.004	0.000	0.000	0.001	0.004

Table 7 continued

Table 7 Panel A presents results for the second-stage of the IV/GMM fixed-effects regression model for the effect of *P-covenants* on borrowers' future investments with a sample of relationship borrowers (see equation 3). We measure borrowers' future investments with R&D and Capex from the end of the loan-origination year up to the following third year. We estimate the first-stage effect of *Defaults different Region and SIC* (IV) on *P-Covenants* (X) in Table 5 Column (1). In the second-stage regressions reported in Panel A of Table 7, we estimate the effect of *P-covenants* on (log of) *R&D* (Columns from 1 to 4) and *Capex* (Columns from 5 to 8). Consistent with Table 4 Panel A, we control for *Size, ROA, Book to Market, Leverage, Zscore, Dividends, Maturity, Loan size, Secured, Number of lenders, Quarterly GDP growth*, and we include fixed effects for *Year, Loan type,* and *Borrower rating*. Table 7 Panel B presents results for the second-stage of the IV/GMM fixed-effects regression model for the effect of *Prob-Violation P-Cov* on borrowers' future investments with a sample of relationship borrowers (see equation 3). We measure borrowers' future investments with R&D and Capex from the end of the loan origination year up to the following third year. We estimate the first-stage effect of *Defaults different Region and SIC* (IV) on the *Prob-Violation P-Cov* (X) in Table 5 Column (5). In the second-stage regressions reported in Panel B of Table 7, we estimate the effect of *Prob-Violation P-Cov* on (log of) *R&D* (Columns from 1 to 4) and *Capex* (Columns from 5 to 8). Consistent with Table 4 Panel A, we control for *Size, ROA, Book to Market, Leverage, Zscore, Dividends, Maturity, Loan size, Secured, Number of lenders, Quarterly GDP growth*, and we include fixed effects for *Year, Loan type,* and *Borrower rating*. All variables are defined in Appendix A. At the bottom of the table we report the results of the (p-value) F-test for the test of excluded IV, and the R-squared from the reduced form. Robust standard errors are reported in parentheses and are clustered at the borrower level. The superscripts ***, **, and * indicate two-tailed statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively. † indicates one-tailed significance at the 10 percent level.

Table 8: Borrower Composition and Defaults in Lenders' Portfolios

Dependent Variable:	*No. R&D Firms/All Firms* $_{(t+1)}$		
	(1)	(2)	(3)
Treatment Effect:			
Defaults (t)	-0.0157***	-0.0173***	-0.0142***
	(0.0042)	(0.0044)	(0.0042)
Fixed Effects:			
Year	Yes	No	Yes
Industry	Yes	No	Yes
*Industry*Year*	No	Yes	No
Lender	No	No	Yes
Observations	46,504	46,504	46,504
R-squared	0.586	0.620	0.595
F-Test (p-value)	0.000	0.000	0.001

Table 8 presents results for the OLS fixed effects regressions model described in equation 4. The model examines the effect of lenders' recent defaults on future borrower composition in a lender's portfolio. The dependent variable measures the ratio of the number of loans to borrowers with R&D over the total number of loans a lender issues in a specific industry, US state, and year. The independent variable, *Defaults*, measures the total number of defaults in a lender portfolio for each calendar year, but in US states and industries different from the borrower's. Controls are fixed effects for year and *2SIC industry* (Column 1), the interaction of *Year* and *2SIC industry* (Column 2), *Year, 2SIC industry,* and *lender* (Column 3). All variables are defined in Appendix A. At the bottom of the table, we report results for the (p-value) *F*-test of the coefficient on the treatment effect. Robust standard errors are reported in parentheses and—given the nature of the panel used in this analysis—are clustered at the intersection between lender, US state, and 2SIC industry. This is arguably the most conservative clusterization. In a further analysis (non-tabulated), we cluster standard errors at the lender level and results are statistically significant at the 1 percent level for all specifications. The superscripts ***, **, and * indicate two-tailed statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

Table 9: Defaults in Lenders' Portfolios and Investments by Existing Borrowers

Dependent variable:	Log Research and Development (Dep.: $R\&D_{(t+2)}$)			Log Capital Expenditures (Dep.: $Capex_{(t+2)}$)		
Sample:	All Contracts	P-Cov. > 0	P-Cov. = 0	All Contracts	P-Cov. > 0	P-Cov. = 0
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect:						
Defaults (t+1)	-0.0002**	-0.0001*	-0.0003	-0.0002*	-0.0002**	0.0003
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0002)
Test 'P-Cov. > 0' = 'P-Cov. = 0' (p-value)		0.369			0.024	
Control Variables (Table 4)	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects:						
Year	Yes	Yes	Yes	Yes	Yes	Yes
Loan type	Yes	Yes	Yes	Yes	Yes	Yes
Borrower rating	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,517	8,451	1,066	9,448	8,389	1,059
R-squared	0.203	0.149	0.387	0.064	0.059	0.168
F-Test (p-value)	0.002	0.053	0.163	0.072	0.024	0.166

Table 9 presents results for an OLS fixed effects regression model of the relationship borrowers described in equation 5. This model estimates the effect of the lender's defaults on the borrower's future investments conditional on the type of covenants in place in the borrower's existing contract. We repeat this analysis for all the contracts in our sample (Columns (1) and (4)), for contracts with P-covenants (Columns (2) and (5)), and for contracts without P-covenants (Columns (3) and (6)). To capture cross-sectional variation in the number of P-covenants that is independent from *Defaults*, we measure *Defaults* at t+1, which is the end of the calendar year subsequent to the loan's origination year (t). By doing so, we limit concerns that the subsamples of contracts with and without P-covenants were determined endogenously (i.e., we avoid capturing defaults that may determine the number of P-covenants included in the loan contract). Columns (1) to (3) examine the effect of the lender's defaults on the next year's (log of) *R&D*, while Columns (4) to (6) examine the effect of the lender's defaults on next year's (log of) *Capex*. Consistent with Table 4 Panel A, we control for *Size, ROA, Book to Market, Leverage, Zscore, Dividends, Maturity, Loan size, Secured, Number of lenders,* and *Quarterly GDP growth*; we also include fixed effects for *Year, Loan type,* and *Borrower rating*. All variables are defined in Appendix A. Robust standard errors are reported in parentheses and are clustered at the borrower level. The superscripts ***, **, and * indicate two-tailed statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.