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ARTICLE

What's my style? Supply-side determinants of debt covenant inclusion

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Abstract

We examine the supply-side determinants of debt covenants included in loan agreements. Controlling for borrower characteristics, we find evidence that the covenants that lead arranger banks include in new contracts persist into future contracts for at least 3 years. We document that this covenant style effect is smaller when borrowers have recently violated a debt covenant or when the loan issue amount is large, and it is larger when the costs of contracting are highest and when a borrower provides collateral. We also find that the covenant style effect decreases following changes in a bank's CEO or CFO. Overall, our evidence is consistent with lenders' covenant preferences arising from strategic cost-benefit analysis informed from prior lending experiences and being related to lender expertise in negotiating, monitoring and enforcing covenants.

KEYWORDS
banks, debt contracts, debt covenants, supply-side determinants

JEL CLASSIFICATION
G10, G21, M41

1 | INTRODUCTION

Banks develop expertise from their prior lending experience in monitoring borrowers' financial activities in order to mitigate conflicts of interests between managers and creditors arising from asymmetric information (e.g., Diamond, 1984). In loan contracts, covenants have classically been viewed as a method of controlling agency problems by restricting managerial behavior (e.g., Jensen & Meckling, 1976; Smith, 1993; Smith & Warner, 1979). Covenants can align the interests of the contracting parties *ex-ante* and serve as "trip wires" and reallocate decision rights ex-post (e.g., Bozanic, 2016; Chava et al., 2010; Demerjian, 2011; Dichev & Skinner et al., 2002).

Financial contracting models (e.g., Gale & Hellwig, 1985; Townsend, 1979; Williamson, 1986) suggest both lenders and the borrowers will consider their own costs and benefits when setting contracts. However, research to date generally provides evidence that covenant design (intensity and tightness) is associated with the level of information asymmetry or agency risks, and thus they are determined based on various borrower characteristics (e.g., Black et al., 2004; Bonsall & Miller, 2017; Bradley & Roberts, 2015; Chen et al., 2016; Costello & Wittenberg-Moerman, 2011; Drucker & Puri, 2009; Hollander & Verriest, 2016; Ma et al., 2019; Prilmeier, 2017). Less is known about the supply-side, or lender-based, determinants of debt covenants, which may be in part because of data limitations. A few notable exceptions are Murfin (2012), who provides evidence that lenders increase the strictness of the financial covenants included in their debt contracts after suffering payment defaults in their own loan portfolios, holding borrowing credit quality constant, and Demerjian et al. (2021), who show that loans from lenders with lower regulatory capital issue loans with lower financial covenant strictness. While prior studies investigate how borrowers' management and individual loan officers can influence debt financing terms (see e.g., Bonsall et al., 2017; Bushman et al., 2021), in this study, we extend the literature on the supply-side determinants of debt covenants included in loan agreements.[1] Specifically, we empirically investigate how banks' preference for covenants influences the future loan contracts they design as well as factors that influence the reliance of banks on their "covenant style."

Including covenants in debt contracts are costly to lenders because they are required to expend time and effort to monitor covenants over the maturity of a loan and to renegotiate contracts after a covenant is violated. Furthermore, Denis and Wang (2014) show that debt covenants are frequently renegotiated, even in the absence of any covenant violation. Beyond covenants, lenders have other tools at their disposal, such as interest spread, the requirement of collateral and loan maturity, to use when designing contracts. Different lenders may have different preferences or abilities for using covenants as a tool for screening and monitoring, based on their business strategy, organizational structure and staff composition.[2] If some banks believe that they possess expertise in negotiating, monitoring and enforcing certain covenants, they may prefer to include these covenants in debt contracts more frequently than other banks. In other words, the net benefits of including financial covenants are higher for some banks compared to others, and thus these banks will include more covenants in their loan contracts.

Additionally, banks may develop expertise over time-related to covenant use because of the sheer number of loans that they have issued in the past, which have included covenants. For example, banks may learn how to efficiently perform due diligence related to determining appropriate covenant ratio levels, given borrower characteristics. Extensive experience with a certain covenant may also allow banks to develop expertise in deciding how to respond to specific types of debt covenant violations, with covenant-specific expertise allowing a bank to quickly and efficiently renegotiate a loan contract after a covenant violation.[3] These arguments suggest that banks may have a general preference for financial covenant use, related to their cost-benefit analysis of using different loan terms when designing contracts, and this preference may affect future contract design from the supply (lender) side, controlling for borrower characteristics. In summary, over time we argue that experience and preferences may give rise to a lender-specific debt contracting "style."[4]

To analyze the effect of banks' current preference on the design of future debt contracts, which we term "covenant style," we use a sample of private loans and construct an annual measure of a lead bank's covenant preference. We find that controlling for borrower characteristics, the preference for including covenants in the recent loans of a lead bank

[1] Relatedly, Herpfer (2019) provides evidence that strong personal relationships between individual bankers and borrowers are associated with lower interest spreads.

[2] For example, Berger (2017) find that banks are less likely to collect audited financial statements from firms in industries and geographic regions in which they have more portfolio exposure, consistent with lenders developing expertise over time through experience.

[3] While a bank is legally able to call to maturity a loan in the event of a debt covenant violation, in actuality lenders respond in many different ways. Common responses to violation involve renegotiating different aspects of the loan agreement, and the most common response to covenant violation is to waive the violation (Dichev & Skinner, 2002).

[4] Our research question explores lender-level preferences that motivate debt contract design, and these preferences may develop over time through company and manager culture, lender-wide trainings and lender-specific experience. Our intuition is similar to that stemming from prior studies documenting that individual managers have a personal style (see e.g., Bamber et al., 2010; Ge et al., 2011).

has predictive power for the covenants that will be included in subsequent loan contracts and that this effect persists for at least 3 years.[5] Specifically, we find that loans issued by a bank that, on average, included one more financial covenant in all of their loans in the last year, relative to other banks, will include 0.54 more financial covenants than other banks in the following year, controlling for borrower characteristics.[6] We argue that this covenant style is related to lead banks' expertise in negotiating, monitoring and enforcing covenants, and it is a direct consequence of banks attempting to minimize their costs related to debt contract design.

In order to isolate the effect of supply from demand and rule out the alternative explanation that the effect is driven by the matching between a certain bank and certain clients, we include firm and firm-year fixed effects in our regressions. Empirically, we use loans issued to a borrower from other *different* banks as the control group, which produces a strong identification strategy and controls for unobservable borrower characteristics. In these tests, we show that the "style" effect still exists, providing us more confidence that what we document is coming from the supply side, and it cannot be fully explained by a systematic matching between banks and clients or by borrower characteristics.

The use of the persistent "covenant style" reduces the screening, monitoring and renegotiating costs of contracts for lenders. Providing further strength to our interpretation of our findings, we perform a series of cross-sectional tests to investigate the conditions under which the persistence of "covenant style" varies. In our first set of cross-sectional tests, we consider the effect of bank size and the presence of collateral on "covenant style." For small banks with limited resources, the costs of contracting are highest, and they are more likely to rely on the "covenant style" in order to control their costs. When collateral is provided by a borrower, reducing lenders' downside in the event of default, a custom-tailored debt contract is less valuable and banks will be more likely to rely on their preferred covenant style. Consistent with both of these predictions, we find that the covenant style effect is larger for small banks and in the presence of collateral.

In a second set of cross-sectional tests, we examine the persistence of the covenant style for large loans and when borrowers have recently violated a debt covenant. Banks typically subject larger loans to additional monitoring and review in order to comply with credit exposure requirements imposed by regulators and/or board committees (Minnis & Sutherland, 2017). Therefore, for larger loans, banks will more carefully design loan contracts and will decrease their reliance on covenant style. Additionally, when borrowers have recently experienced a covenant violation, banks will be more likely to increase their level of due diligence before the loan issuance and will be more likely to custom-tailor a contract for a borrower, decreasing reliance on covenant style in setting contracts. In these cases, the benefits of efficiency are offset by the downside costs of inadequately controlling for a borrower's risk. We find evidence consistent with both of these predictions. Together, these cross-sectional tests add to the plausibility of our assertion that banks have a preference for including certain covenants in their loan contracts, but we also find that banks rationally deviate from this preference in the presence of additional risk factors.

In further tests, we attempt to examine how the preference for including financial covenants affects other contract terms. We find that banks' preference for including financial covenants is associated with lower interest spreads, shorter loan maturities and a lower likelihood of a collateral requirement, consistent with the notion that banks balance the costs and benefits of using different tools for designing loan contracts. We also find that covenant style is associated with an increased likelihood that a borrower will violate a debt covenant over the life of its loan. The increase in the likelihood of violation is consistent with an increase in the number of covenants leading generally to more violations, perhaps an unsurprising result. However, these covenant violations will trigger more debt

[5] We construct our measure of "style" by modeling the determinants of financial covenants, annually, with the inclusion of a lead arranger bank fixed effect in the prediction model. We then use the coefficient of each lead arranger bank as our measure of "covenant style" for that bank for the next year. If there is more than one lead arranger in a loan deal, we take the average of the style effect for all the lead arrangers. More details related to the construction of our measure can be found in Section 3.

[6] We also conduct our analysis using Murfin's (2012) covenant strictness measure, and our inferences do not change. In those tests, we find that banks that issue loans with covenant strictness one standard deviation above their peers will issue loans in the following year with covenants strictness that is 0.33 standard deviations above peer loans, controlling for borrower characteristics. We report these results in Table 10 and discuss them in more depth in Section 5. We also document that the covenant style effect does not differ between performance and capital covenants. We report this test in Table 8.

renegotiation, and we argue banks' expertise in financial covenants makes them more willing and able to efficiently (i.e., at low cost) renegotiate contracts following violations.

Last, we examine how covenant style is influenced by changes in the CEO or CFO of lead arranger banks. We predict that new CEOs and CFOs will make changes to debt contracting guidelines based on their preferences and expertise, including changes that will influence debt covenant usage in contracts. Consistent with our prediction, we find that in the year after a new CEO or CFO starts their position, the covenant style decreases. This result is consistent with style being, at least partially, attributable to individual manager preferences.

Our study partially fills the gap in the literature related to the supply-side determinants of debt contract terms, specifically for banks' preference for financial covenants. Even though theory work clearly suggests that optimal contracts will reflect the preferences of both lenders and borrowers (e.g., Gale & Hellwig, 1985; Townsend, 1979; Williamson, 1986), with the notable exception of Murfin (2012), prior empirical research has primarily focused on the demand-side of debt contract design or the various characteristics of borrowers that influence debt contract terms (e.g., Black et al., 2004; Bonsall & Miller, 2017; Bradley & Roberts, 2015; Chen et al., 2016; Costello & Wittenberg-Moerman, 2011; Drucker & Puri, 2009; Hollander & Verriest, 2016; Prilmeier, 2017). Murfin (2012) finds that lenders write tighter contracts than their peers, holding borrower characteristics constant, after suffering payment default in their loan portfolios, providing some of the first empirical evidence that banks' exhibit preferences in debt contract design that are not related to borrower characteristics. Demerjian et al. (2021) show that loans from lenders with lower regulatory capital include financial covenants with lower strictness. Our results are consistent with recent findings by Bushman (2021) who find that individual loan officers exert more influence over covenant design than other loan contract terms, such as loan spread.

Generally, we provide evidence that some banks exhibit a style when including financial covenants in their loan contracts, and we argue that this style is a direct result of banks attempting to minimize their costs associated with debt contract design. Strengthening our argument, our cross-sectional tests provide evidence of several intuitive conditions that influence banks reliance on covenant style versus tailor-fitting debt contracts given the characteristics of borrowers. By considering the general supply side of covenants' determinants, we complement both theoretical and empirical studies and provide a fuller picture of the covenant use. In so doing, our findings add to the understanding of the economic determinants of the structure of debt agreements as called for in Skinner (2011).

In the next section, we develop our hypotheses. We describe the sample selection procedures and variables used in this study in Section 3. Section 4 presents the empirical results of our hypotheses, and Section 5 presents the results of additional analyses. The summary and conclusions are provided in Section 6.

2 | BACKGROUND AND HYPOTHESIS DEVELOPMENT

Syndicated loans represent an important source of financing for corporations and a major service provided by banks (Roberts, 2015). Banks play a critical role in reducing information asymmetries and moral hazard problems in the lending process since they are specialist in monitoring borrowers' financial activities to mitigate conflicts of interests between managers and creditors arising from asymmetric information (e.g., Diamond, 1984). Because of the repeat nature of the syndicated loan market, banks are eager to maintain strong relationships with borrowers while also appropriately managing risk. The ability of a bank to conduct screening and monitoring may increase a banks reputation in the lending market and be a valuable competitive advantage.

Banks have many tools to deal with the information risk or agency risk in the lending process. They can charge a high spread to compensate it or demand additional collateral in the deal. Of these methods, debt covenants have classically been viewed as a method of controlling agency problems implicit in the lender–borrower relationship by restricting managerial behavior (Jensen & Meckling, 1976; Myers, 1977; Smith & Warner, 1979). The covenants limit a manager's ability to opportunistically expropriate wealth from debt holders when a firm is in economic distress (Jensen & Meckling, 1976; Smith & Warner, 1979). They can work as tripwires that advise the lenders of poor financial performance by the borrower (Chava et al., 2010; Demerjian, 2011; Dichev & Skinner, 2002) and thus lead to a renegotiation of contract terms or some other forms of control transfer. An increased scope for renegotiation increases contracting efficiency and minimizes opportunistic behavior (Christensen et al., 2016).

Significant consequences arise when firms violate covenants and trigger a renegotiated agreement between violating firms and their creditors, such as negative stock market reactions (Beneish & Press, 1993; Stice, 2018), reduced investments (Chava & Roberts, 2008), impaired access to financing (Roberts & Sufi, 2009), employment cuts (Falato & Liang, 2016) and increased CEO turnover and independent director appointments (Ferreira et al., 2018; Nini et al., 2012). Even though covenants provide lenders with the valuable option to renegotiate contracts and covenant violations serve as tripwires that allow creditors to step in and influence firm policies, they also bring cost to banks. The renegotiation process is time-consuming since the violation of a covenant does not automatically trigger bankruptcy procedures and both lender and borrower usually want to keep the firm out of costly bankruptcy. Renegotiated covenants are more often loosened instead of tightened than the initial ones (Chava & Roberts, 2008; Denis & Wang, 2014; Garleanu & Zwiebel, 2009; Nini et al., 2012; Roberts, 2015; Roberts & Sufi, 2009; Smith, 1993). Thus the cost or the ease of renegotiation should be considered when designing the optimal intensity and tightness of covenants (Garleanu & Zwiebel, 2009). If banks are not good at dealing with these cases, they may avoid using covenants.[7]

Different lenders might have different abilities in using covenants as a tool of screening and monitoring, according to its business strategy, organizational structure and staff composition.[8] For example, El-Gazzar and Pastena (1990, 1991) show that insurance companies typically impose more covenants (as well as more slack at initiation) than banks. Loan officers who engage in loan *prospecting*, *screen* new loan applications and *monitor* existing loans are affected by bank-specific economic incentives (Berg et al., 2014; Cole et al., 2015; Hertzberg et al., 2010; Qian et al., 2015;) and social characteristics (Fisman et al., 2017). Loan officers at lower tiers are typically responsible for collecting information about borrowers and transmitting this information to managers (Stein, 2002), and banks balance the communication cost between loan officers and loss of valuable soft information regarding the banks' borrowers when setting the degree of delegation (Agarwal & Hauswald, 2010; Skrastins & Vig, 2018; Stein, 2002).

However, prior studies examining the design of covenants usually focus on borrowers' characteristics (e.g., Black et al., 2004; Chen et al., 2016; Costello & Wittenberg-Moerman, 2011; Drucker & Puri, 2009; Hollander & Verriest, 2016; Prilmeier, 2017) and generally neglect lender characteristics and preferences. Murfin (2012) explicitly examines covenant design from the supply side, or in other words, from the perspective of banks. He reports that US banks apply tighter covenants than their peers to equivalent borrowers following payment defaults in the loan portfolios of the lead arranger of a syndicated loan (the lead bank responsible for providing due diligence before and monitoring after loan issuance), consistent with lenders updating their beliefs about their screening ability based on default experience and adjusting contracts accordingly, with defaults on recent loans perceived as being more relevant than those on older loans. As it stands, much less is known about the factors that drive debt contract design, and specifically the choice of debt covenants, coming from the perspective of lenders.

Given that banks will use covenants to monitor firms after the loan issuance (e.g., Chava & Roberts, 2008; Nini et al., 2009; 2012; Roberts & Sufi, 2009), the cost of monitoring covenants will be considered by banks when they design loan contracts. One possible way to decrease monitoring costs is to use the same types of covenants in all contracts. For example, Kahan and Klausner (1997) argue that the common use of contract terms, or boilerplate contracts, can create learning and network externalities. For example, a bank may know that it has expertise in a certain type of covenant (e.g., financial covenants generally, or the max debt-to-EBITDA covenant specifically) and, therefore, prefers to use these types of covenants for similar borrowers. Alternatively, a bank may intentionally acquire expertise related

[7] Publicly traded bonds tend to have much more dispersed ownership, decreasing the ease of renegotiation. That is one explanation for why bank loans have more and tighter covenants than publicly traded bonds (see Bolton & Scharfstein, 1996).

[8] Wang and Xia (2014) use BB-rated and B-rated borrower sample to analyze the strictness of loan covenants. They find that banks active in securitization impose looser covenants on borrowers.

to specific types of covenants in order to achieve debt contracting efficiency. In other words, we anticipate that banks will include similar covenants in the contracts in both similar and dissimilar borrowers because of their developed expertise in negotiating, monitoring and renegotiating certain covenants.

Theory work clearly predicts that the optimal contract will reflect the preferences of both the lender and the borrower (e.g., Gale & Hellwig, 1985; Townsend, 1979; Williamson, 1986). If the choice and structure of covenants are determined solely based on firm characteristics on a case-by-case basis, then we would expect that banks' preferences do not matter, which is what is typically assumed in the literature implicitly. However, if the monitoring cost to banks is considered, and if banks do have their own preferred style of covenants, then we would expect to see a lingering effect of covenant use for different types of firms within a specific bank's portfolio, even after controlling for borrower characteristics. Motivated by this intuition and contracting theory, we predict that the types and number of covenants used in recent loans by a bank will be associated with those in future loans. Expressed formally:

> **H1**: *The preference for including financial covenants in prior loans is associated with the covenants included by lead arrangers in current loans, indicating that banks have a "covenant style."*

A variety of factors are likely to influence the effect of "covenant style" and the inclusion of covenants included in loan contracts, and "covenant style" itself is likely to change over time. Additionally, recent borrower defaults, the resources available to banks and loan characteristics are all likely to affect the relation between banks' style and covenant inclusion. We do not construct formal hypotheses for these additional tests, but we investigate the influence of each of these factors on "covenant style" and report the results as additional tests.

3 | RESEARCH DESIGN AND SAMPLE SELECTION

3.1 | Measure of the supply effect: bank covenant preference

A "covenant style" suggests that lenders' preference for including covenants will be persistent. Before we examine the effects of banks' "covenant style" on debt covenant inclusion in loan contracts, we first construct a measure of lenders' "covenant preference". Following related studies in this area, we focus our primary analysis on the intensity of financial covenant inclusion (i.e., the number of covenants included), but we also test the strictness of included covenants as a robustness check in additional analyses. Our empirical strategy for developing our measure employs the following research design at the lead arranger-firm-facility level:

$$\begin{aligned} FinancialCovenants = {} & \beta_1 Determinants\ of\ Financial\ Covenants + \beta_i Controls \\ & + \beta_j Lead\ Arranger\ Fixed\ Effects + \varepsilon, \end{aligned} \tag{1}$$

where *Financial Covenants* is the number of financial covenants included in the contracts of a lead arranger's loans issued within the last year. We annually estimate the determinants of financial covenants with lead arranger bank fixed effects included in the regression. We focus our analyses on lead arranger banks because these are the primary parties that negotiate loan contracts with borrowers on behalf of all participating syndicate members. The fixed effect regression coefficients are individual bank-specific constants, which we take as the supply effect measure for that particular bank in the following year, labeled as a lead bank's "covenant preference" (*Bank Covenant Preference*).[9] This measure

[9] If there is more than one lead arranger in a loan deal, we treat these deals as separate observations in order to calculate the coefficients for each lead arranger.

captures the preference of a given bank to include financial covenants, relative to other banks, controlling for other known characteristics of borrowers and the relationship between the bank and the borrowers. We view this preference as arising from a strategic cost-benefit analysis that is formed from prior lending experiences. The direction of the preference is not clear *ex-ante* because a bank's prior experience and/or expertise could lead to financial covenant preferences in either direction. Moreover, it is reasonable to assume that banks would have different strategies, and therefore banks' style will likely vary across banks.

While we are unable to predict a direction for covenant preference, we do expect an effect to be present. To test this, we examine whether the lenders' estimated coefficients are significant, not necessarily the direction of the coefficient. A coefficient of zero would indicate that a given bank has no difference in their preference for including financial covenants in debt contracts compared to the other benchmark banks.[10] A positive (negative) coefficient indicates a larger (smaller) preference to include financial covenants in debt contracts. Following the literature (e.g., Bird et al., 2018), bank-years with estimated fixed effects that are not significant at traditional levels are assigned an "effect" of zero.[11,12]

3.2 | Research design

After constructing *Bank Covenant Preference*, we investigate its effect on the covenants that lead arranger banks include in the loans they issue over the following year. If banks only consider borrower-specific factors, then *Bank Covenant Preference* will have no predictive power after controlling for other determinants of covenant use in loan contracts, and we will observe no style of lenders' covenant usage. Empirically, we estimate the following model:

$$FinancialCovenants = \alpha + \beta_1 Bank\ CovenantPreference + \beta_i CONTROLS\ + \varepsilon, \tag{2}$$

where *Financial Covenants* is the number of financial covenants used in a bank's new loans. We expect β_1 to be significantly positive if banks have a covenant usage "style."[13] If banks design loan contracts on a case-by-case basis, solely considering borrower-specific characteristic, then there will be no persistence in covenant usage preference, and we will observe an insignificant β_1.

Importantly, we empirically examine the effect of *Bank Covenant Preference* after controlling for the other known determinants of financial covenants. We select control variables similar to those in prior studies on the determinants of covenants in debt contracting (Beatty et al., 2002; Bradley & Roberts, 2015; Costello & Wittenberg-Moerman, 2011; Coyne & Stice, 2018; Graham et al., 2008; Sufi, 2007). Specifically, we include loan interest rate as a control variable (*Interest Rate*) because agency theory on debt covenants predicts a negative relation between loan spread and the use of covenants (Jensen & Meckling, 1976; Smith & Warner, 1979). We control for loans to institutional investors (*Institutional Investor*) because relative to bank loans, they have a higher information symmetry with the borrower. We also control for revolving loans (*Revolver*) because these loans typically have a lower riskiness to lenders than term loans.

[10] Kang and Zhuang (2019) provide evidence that non-bank lenders do not appear to improve their monitoring expertise based after defaults from their portfolio of borrowers. This finding may indicate that covenant preferences will be stronger (weaker) for bank (non-bank) lenders.

[11] In an untabulated robustness test, we alternatively use the coefficient of the estimated fixed effects even if it is statistically insignificant with no change to our inferences.

[12] The mean of the estimated effect is approximately 0.46, with a standard deviation of 1.08. Approximately 60% of the bank-years in our sample are set as zero, either because the coefficients estimated from the regression are zero or because they are insignificant at the traditional levels (and thus treated as zero).

[13] If there is more than one lead arranger in a loan deal, we average the effect from each lead arranger.

We control for loan size because larger loans are typically less risky and face lower price and non-price loan terms (Beatty et al., 2002; Booth, 1992). We control for the existence of performance pricing provisions because they reduce adverse selection and moral hazard costs for lenders (Asquith et al., 2005). We also control for the relationship between banks and firms because a prior lending relationship may also affect the use of financial covenants in any new deal. We control for firm size because small firms have greater information asymmetry and higher default risk (Bharath et al., 2007). We include a number of controls related to financial distress found in the prior literature: leverage, market-to-book, profitability, tangibility, sales growth, cash flow volatility, and *Z*-score (Graham et al., 2008). Last, we control for other contracting devices available to lenders: loan maturity, the inclusion of collateral and loan purpose fixed effects. We also include different combinations of year, industry and firm fixed effects.[14] To mitigate the influence of outliers, we winsorize all continuous variables at the top and bottom 1% of their respective distributions. Standard errors are heteroskedasticity robust, and we also adjust standard errors using two-way firm and year clustering in all regressions.

3.3 | Data sources and sample selection

The data come from several sources. We obtain data on private loans from Dealscan for the period from 1995 to 2016 because data in Dealscan may not be complete before 1995.[15] Dealscan is provided by the Loan Pricing Corporation and provides detailed loan-specific contract information. Following prior literature, we exclude loans borrowed by financial institutions and utilities because they are subject to different regulations related to debt financing that other borrowers do not face (Nikolaev, 2010). Control variable data comes from Dealscan and Compustat. Our sample includes all US-dollar private loans issued by US publicly traded companies that have non-missing loan covenant and control variable data. Our final sample consists of 30,932 loans to 4943 borrowers during the period 1995 to 2016.[16]

Panel A of Table 1 reports the annual distribution of loans. Over our 22-year sample period the number of firms per year range from 1212 in 1996 to 135 in 2016. The number of loans over the sample period also exhibits a substantial range from 2024 in 1996 to 210 in 2016. The data also seem to suggest some cyclicality in the number of loan issues over time. This pattern is consistent with tighter monetary policy and poor bank performance making access to bank loans more difficult.

3.4 | Descriptive statistics

Panel B of Table 1 reports the descriptive statistics for our sample. The loans in our sample have a mean (median) of 1.68 (2.000) financial covenants, 219.58 (200.00) basis point interest spread, 48.90 (60.00) months loan maturity and provide collateral 58% of the time. *Bank Covenant Preference* has a mean (median) value of 0.76 (0.79) with a standard deviation of 0.86, indicating a large variation in bank styles. Panel C of Table 1 provides a correlation matrix. The correlation between *Bank Covenant Preference* and *Financial Covenants* is 0.12 and is statistically significant. This univariate result provides some preliminary evidence consistent with our hypotheses. Many of the control variables are significantly correlated with the inclusion of financial covenants. As expected, the number of included financial covenants is positively correlated with whether a loan is an institutional loan, the presence of performance pricing provisions, loan maturity, the likelihood that collateral is provided, profitability and sales growth; and it is negatively correlated with firm size, tangibility and cash flow volatility.

[14] All variables are defined in the Appendix.

[15] In a robustness test, we confirm that our results do not change across our tests if we start our sample period in 1990 or 1991 (untabulated).

[16] Our dataset ends in the middle of 2016, reducing the number of observations for this final year.

TABLE 1 Sample distribution and descriptive statistics

Panel A: Annual sample distribution					
Year	Freq.	(%)	Year	Freq.	(%)
1995	1557	5.03	2006	1609	5.20
1996	2024	6.54	2007	826	2.67
1997	1854	5.99	2008	628	2.03
1998	1708	5.52	2009	999	3.23
1999	1684	5.44	2010	1342	4.34
2000	1710	5.53	2011	1134	3.67
2001	1842	5.95	2012	1382	4.47
2002	1728	5.59	2013	1214	3.92
2003	1829	5.91	2014	1201	3.88
2004	1752	5.66	2015	1129	3.65
2005	1570	5.08	2016	210	0.68
			Total	30,932	100

Panel B: Descriptive statistics						
Variable	*N*	Mean	Std. dev.	P25	P50	P75
Financial Covenants	30,932	1.68	1.56	0.00	2.00	3.00
Bank Covenant Preference	30,932	0.76	0.86	0.00	0.79	1.41
Interest Spread	30,932	219.58	143.25	117.75	200.00	300.00
Institutional Investor	30,932	0.11	0.32	0.00	0.00	0.00
Revolver	30,932	0.58	0.49	0.00	1.00	1.00
Loan Size	30,932	18.63	1.68	17.62	18.83	19.81
PP Index	30,932	0.45	0.50	0.00	0.00	1.00
Maturity	30,932	3.72	0.67	3.58	4.09	4.09
Collateral	30,932	0.58	0.49	0.00	1.00	1.00
Relationship	30,932	0.44	0.50	0.00	0.00	1.00
Size	30,932	6.93	1.94	5.61	6.93	8.28
Leverage	30,932	0.27	0.22	0.10	0.24	0.38
Market-to-Book	30,932	1.75	0.99	1.13	1.44	1.98
Profitability	30,932	0.12	0.10	0.08	0.12	0.17
Tangibility	30,932	0.32	0.23	0.12	0.25	0.47
Sales Growth	30,932	0.19	0.48	−0.01	0.08	0.23
Cash Flow Volatility	30,932	0.03	0.03	0.01	0.02	0.03
Z-Score	30,932	1.62	1.41	0.83	1.65	2.45

(Continues)

TABLE 1 (Continued)

Panel C: Correlation matrix

		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
1	*Financial Covenants*																	
2	*Bank Covenant Preference*	0.12																
3	*Interest Spread*	0.04	−0.09															
4	*Institutional Investor*	0.01	−0.06	0.29														
5	*Revolver*	0.06	0.05	−0.20	−0.42													
6	*Loan Size*	−0.21	−0.08	−0.33	0.11	−0.01												
7	*PP Index*	0.44	0.10	−0.25	−0.16	0.18	0.13											
8	*Maturity*	0.06	0.00	0.08	0.25	0.10	0.15	0.11										
9	*Collateral*	0.32	0.00	0.47	0.29	−0.05	−0.30	0.03	0.19									
10	*Relationship*	−0.09	0.05	−0.13	0.05	−0.02	0.29	−0.03	0.02	−0.11								
11	*Size*	−0.35	−0.15	−0.28	0.06	−0.10	0.76	−0.02	0.05	−0.37	0.28							
12	*Leverage*	0.00	−0.04	0.16	0.20	−0.13	0.16	−0.04	0.15	0.13	0.12	0.18						
13	*Market-to-Book*	−0.03	0.05	−0.16	−0.05	−0.01	0.00	0.01	−0.05	−0.11	−0.02	−0.08	−0.15					
14	*Profitability*	0.03	0.06	−0.27	0.01	0.00	0.21	0.13	0.10	−0.18	0.06	0.13	0.01	0.30				
15	*Tangibility*	−0.04	0.01	−0.05	−0.02	0.02	0.13	0.01	−0.01	−0.07	0.04	0.17	0.23	−0.16	0.07			
16	*Sales Growth*	0.11	0.11	0.01	−0.01	−0.01	−0.08	0.04	0.01	0.08	−0.02	−0.13	0.01	0.16	−0.06	0.00		
17	*Cash Flow Volatility*	0.05	−0.01	0.12	−0.07	0.08	−0.28	−0.04	−0.12	0.14	−0.11	−0.38	−0.20	0.08	−0.12	−0.19	−0.09	
18	*Z-Score*	0.02	0.07	−0.29	−0.09	0.08	0.09	0.13	0.03	−0.17	0.01	0.02	−0.30	0.09	0.55	−0.18	−0.08	0.06

Note: Panels A and B present the annual sample distribution and descriptive statistics for the variables used in the analyses, respectively. Panel C presents the Pearson correlation matrix. Correlation coefficients in bold indicate significance at the 0.05 level or better. See the Appendix for variable definitions.

4 | EMPIRICAL RESULTS

4.1 | The effect of bank covenant preference on financial covenant inclusion

Table 2 presents the effect of covenant preference on the inclusion of financial covenants in a lead arrangers loan issuances. We regress the number of financial covenants included in a loan deal on *Bank Covenant Preference*, constructed using a lead arranger's loans issued in the prior year, and a set of control variables. Our hypothesis predicts that banks have a preferred template for including loan covenants and that the covenants included in a bank's recent deals will have predictive power in the covenants included in the bank's next year's deal. We present four sets of results. The first column reports results with year fixed effects only. Column 2 reports results with industry (Fama–French 48) and year fixed effects. Column 3 reports results with firm fixed effects that attempt to control for omitted borrower characteristics. Column 4 reports results with firm times year fixed effects.[17] By comparing deals already lent to a client (client-year) from different banks, we impose a strong identification strategy and control for unobservable borrower characteristics, which help to isolate the effect of supply from demand and rule out the alternative explanation that the effect is driven by the matching between certain bank and certain clients.

The coefficient on *Bank Covenant Preference* is positive and statistically significant across all the specifications in Table 2, consistent with our hypothesis and the univariate results reported in Table 1. For example, Column 2 reports that *Bank Covenant Preference* increases the number of financial covenants included in bank's next loan by 0.54, suggesting that on average, loans lead by a bank that includes one more financial covenants in all last year lending than other banks will include 0.54 more financial covenants than loans lead by other banks, given the borrower's characteristics.

The specification in Column 4 of Table 2 includes firm-year fixed effects. This specification accounts for unobservable firm characteristics as well as unobservable time-varying shocks to the borrower's creditworthiness and demand for debt financing. These results provide additional comfort that our inferences are warranted.

Many of the other included control variables are statistically significant. For example, *Financial Covenants* are positively associated with the number of performance pricing provisions, loan maturity and profitability and negatively associated with firm size and market-to-book. Our results also hold when we cluster at both the firm and bank levels.

4.2 | The effect of bank covenant preference over time

We next examine the effect of *Bank Covenant Preference* over time. The intuition is that because staff, market conditions and technology all change over time, the effect of a banks *Supply Effect* decrease over time. We use the following design to examine the effect over time:

$$FinancialCovenants = \alpha + \beta_1 Bank\ Covenant\ Preference_{t-1} + Bank\ Covenant\ Preference_{t-2} + Bank\ CovenantPreference_{t-3} + \beta_4 Bank\ Covenant\ Preference_{t-4} + \beta_i Controls\ + \varepsilon. \quad (3)$$

We present the results of the style effect over time in Table 3. The results are consistent with our predictions, with a monotonic decrease in the coefficient on *Bank Covenant Preference* over the 4 years we examine. Interestingly, *Bank Covenant Preference* does have predictive power out to 3 years, consistent with a lingering effect that becomes insignificant 4 years after the construction of *Bank Covenant Preference*.

[17] For brevity, we generally only discuss the year and industry fixed effects specifications in the text (Column 2) and report these fixed effects in our cross-sectional tests, but our results are consistent across all specifications.

TABLE 2 The effect of bank covenant style on the use of covenants

Variables	*Financial Covenants* (1)	(2)	(3)	(4)
Bank Covenant Preference	**0.55*****	**0.54*****	**0.47*****	**0.04*****
	(17.43)	**(17.15)**	**(12.52)**	**(4.53)**
Interest Spread	0.00	0.00	−0.00	−0.00***
	(1.22)	(1.05)	(−0.27)	(−4.06)
Institutional Investor	0.13***	0.12***	0.10***	0.16***
	(3.92)	(3.69)	(3.55)	(6.41)
Revolver	−0.03*	−0.02	−0.02	−0.02
	(−1.68)	(−1.17)	(−1.13)	(−1.57)
Loan Size	−0.01	−0.00	0.03***	−0.04***
	(−0.43)	(−0.21)	(2.82)	(−5.07)
PP Index	1.13***	1.13***	1.06***	1.10***
	(48.39)	(48.36)	(42.82)	(65.07)
Maturity	0.04**	0.03*	0.04**	−0.03***
	(2.16)	(1.88)	(2.26)	(−2.76)
Collateral	0.68***	0.68***	0.67***	0.70***
	(25.05)	(24.63)	(21.77)	(33.51)
Relationship	0.12***	0.12***	0.08***	0.02
	(5.80)	(5.85)	(4.07)	(1.52)
Size	−0.15***	−0.15***	−0.11***	
	(−13.60)	(−12.98)	(−3.53)	
Leverage	0.00	−0.03	−0.18*	
	(0.01)	(−0.39)	(−1.87)	
Market-to-Book	−0.06***	−0.07***	−0.02	
	(−4.82)	(−5.23)	(−1.14)	
Profitability	1.08***	0.95***	0.33	
	(6.59)	(5.59)	(1.36)	
Tangibility	−0.21***	−0.06	−0.06	
	(−3.81)	(−0.84)	(−0.35)	
Sales Growth	0.07***	0.08***	0.09***	
	(2.62)	(3.09)	(2.65)	
Cash Flow Volatility	−3.16***	−2.39***	−2.52***	
	(−6.32)	(−4.55)	(−2.89)	
Z-Score	−0.03***	−0.01	−0.03	
	(−2.66)	(−0.61)	(−1.00)	
Loan purpose fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No
Industry fixed effects	No	Yes	No	No

(Continues)

TABLE 2 (Continued)

Variables	*Financial Covenants* (1)	(2)	(3)	(4)
Firm fixed effects	No	No	Yes	No
Firm*year fixed effects	No	No	No	Yes
Constant	0.88***	1.37***	0.24	1.22***
	(4.26)	(4.14)	(0.83)	(8.49)
Observations	30,932	30,932	30,932	30,932
R-squared	0.424	0.429	0.340	0.285

Note: Table 2 presents the results from the estimation of the following model: $FinancialCovenants = \alpha + \beta_1 BankCovenantPreference + \beta_i CONTROLS + \varepsilon$.
We regress the number of financial covenants on *Bank Covenant Preference*, loan- and firm-specific control variables. All variables are defined in the Appendix. Firm-specific financial variables are winsorized at the top and bottom 1%. Regressions include loan purpose, year and other fixed effects. Standard errors are heteroskedasticity robust and clustered at the firm level. *Z*-statistics are reported in parentheses.
***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

5 | ADDITIONAL ANALYSES

We next conduct a variety of cross-sectional tests to validate the robustness of our basic results. Additionally, these further tests allow us to identify factors that strengthen or weaken the effect of a bank's covenant style on debt contract design.

5.1 | Bank covenant preference after a recent debt covenant violation

We first examine whether the covenant style effect weakens when a borrower has experienced a technical default on a prior loan in the last year. The default data is from Nini, Smith, and Sufi (2012) and is at firm quarter level.[18] Our prediction is that a borrower's technical default will lead banks to pay more attention to their case and custom-tailor a loan contract given the characteristics of the borrower, relying less on their preferred covenant style. In short, we expect the covenant style effect to become weaker after a default. We use the following design to examine the effect of defaults on a style where *Default* is a dummy variable equal to one if the borrower has experienced a technical default on a loan in the last year and zero otherwise:

$$\begin{aligned} FinancialCovenants = {} & \alpha + \beta_1 Bank\ Covenant\ Preference + \beta_2 Default \\ & + \beta_3 Bank\ Covenant\ Preference * Default + \beta_i CONTROLS\ + \varepsilon. \end{aligned}$$

where we expect β_3 to be negative.

Table 4 presents results consistent with our prediction. The coefficient on the interaction between *Bank Covenant Preference* and *Default* is negative and statistically significant, consistent with banks expending more resources to tailor contracts to borrowers, which recently violated a covenant. For these borrowers, the benefits to customized contract design are likely highest and banks are willing to forego some of the benefits of uniform contracting.

[18] We thank Amir Sufi and his coauthors for making these data available at: https://faculty.chicagobooth.edu/amir.sufi/.

TABLE 3 The effect of bank covenant preference on the use of covenants: Trend analysis

	Financial Covenants		
Variables	(1)	(2)	(3)
Bank Covenant Preference$_{t-1}$	**0.47*** **	**0.45*** **	**0.46*** **
	(12.63)	**(11.07)**	**(10.56)**
Bank Covenant Preference$_{t-2}$	**0.30*** **	**0.28*** **	**0.28*** **
	(9.82)	**(8.53)**	**(7.98)**
Bank Covenant Preference$_{t-3}$		**0.12*** **	**0.13*** **
		(3.70)	**(3.56)**
Bank Covenant Preference$_{t-4}$			**0.03**
			(0.94)
Facility-level controls	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes
Loan purpose fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes
Constant	1.43***	1.43***	1.38***
	(4.27)	(4.22)	(4.02)
Observations	30,094	29,378	28,619
R-squared	0.436	0.437	0.439

Note: Table 3 presents the results from the estimation of the following model:
$FinancialCovenants = \alpha + \beta_1 BankCovenantPreference_{t-1} + \beta_2 BankCovenantPreference_{t-2.}$
$+ \beta_3 BankCovenantPreference_{t-3} + \beta_4 BankCovenantPreference_{t-4} + \beta_i CONTROLS + \varepsilon.$
We regress the number of financial covenants on *Bank Covenant Preference* in the previous several years, loan- and firm-specific control variables. All variables are defined in the Appendix. Firm-specific financial variables are winsorized at the top and bottom 1%. Regressions include loan purpose, year and other fixed effects. Standard errors are heteroskedasticity robust and clustered at the firm level. *Z*-statistics are reported in parentheses.
***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

5.2 | Bank covenant preference for big banks, large loans, and in the presence of collateral

We next examine the difference in covenant style between big and small banks. We expect that small banks will be more likely to keep a covenant contract style because they will be more constrained by monitoring costs given that they possess fewer resources. Stated in other words, large banks will be less susceptible to a specific covenant style because they will have more employees, research and in-house expertise, which they can deploy across a wider variety of borrowers' loans. We define 20 banks with the largest market share (in client number) as *Big Bank* and define the rest as small ones. We present results consistent with this intuition in Table 5. Specifically, the coefficient on the interaction between *Bank Covenant Preference* and *Big Bank* is negative and statistically significant.

In Table 6, we examine how the effect of covenant style varies with the other contract terms included in the loan. If a loan's size is small, banks may not care about it as much because of the lower risk in dollar terms and will be more likely to include their standard covenants. On the other hand, if the loan size is large, then banks will be more careful to perform due diligence on the borrower and tailor-fit an appropriate set of covenants given a borrower's characteristics. Similarly, if a loan contract includes collateral, then banks may be more protected given the smaller loss they face

TABLE 4 The effect of bank covenant preference on the use of covenants: Interaction with covenant violation (default)

	Financial Covenants	
Variables	**(1)**	**(2)**
Bank Covenant Preference	0.60***	0.59***
	(16.22)	(15.82)
Default	0.25***	0.25***
	(5.59)	(5.64)
Bank Covenant Preference* Default	**−0.09*** **	**−0.09*** **
	(−2.77)	**(−2.69)**
Facility-level controls	Yes	Yes
Firm-level controls	Yes	Yes
Loan purpose fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Industry fixed effects	No	Yes
Constant	0.34	0.49
	(1.44)	(1.11)
Observations	20,136	20,136
R-squared	0.433	0.441

Note: Table 4 presents the results from the estimation of the following model: $FinancialCovenants = \alpha + \beta_1 BankCovenantPreference + \beta_2 Default + \beta_3 BankCovenantPreference * Default + \beta_i CONTROLS + \varepsilon$
We regress the number of financial covenants on *Bank Covenant Preference*, default, interaction term, loan- and firm-specific control variables. All variables are defined in the Appendix. Firm-specific financial variables are winsorized at the top and bottom 1%. Regressions include loan purpose, year and other fixed effects. Standard errors are heteroskedasticity robust and clustered at the firm level. *Z*-statistics are reported in parentheses. $***$, $**$ and $*$ denote significance at the 1%, 5% and 10% levels, respectively.

in the event of default. This may lead banks to rely more on standard covenants. To test these predictions, we estimate the following model:

$$\begin{aligned} FinancialCovenants &= \alpha + \beta_1 Bank\ Covenant\ Preference + \beta_2 Large\ Loan\ \ (Collateral) \\ &+ \beta_3 Bank\ Covenant\ Preference * Large\ Loan\ \ (Collateral) + \beta_i Controls\ + \varepsilon. \end{aligned} \tag{5}$$

The results presented in Table 6 are consistent with our predictions—large loans are associated with a smaller covenant style effect, while the presence of collateral increases the covenant effect.

5.3 | Robustness tests

We next perform a series of robustness tests to further validate the robustness of our main results. There may be other borrower characteristics that influence the inclusion of financial covenants in debt contracts that are not captured by our control variables. Hence, we further include a control variable for the number of financial covenants included in a borrower's last deal in order to control for a trend or preference from the demand (borrower) side. We present these results in Column 1, Panel A, of Table 7. In Columns 2 and 3, we re-run the regression of our main model using a Tobit

TABLE 5 The influence of the supply effect on the use of covenants: Based on bank size

	Financial Covenants	
Variables	**(1)**	**(2)**
Bank Covenant Preference	0.54***	0.53***
	(16.72)	(16.42)
Big bank	−0.11***	−0.10***
	(−4.57)	(−4.51)
Bank Covenant Preference* Big Bank	**−0.05***	**−0.05****
	(−1.93)	**(−1.99)**
Facility-level controls	Yes	Yes
Firm-level controls	Yes	Yes
Loan purpose fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Industry fixed effects	No	Yes
Constant	0.79***	1.24***
	(3.72)	(3.64)
Observations	30,388	30,388
R-squared	0.423	0.429

Note: Table 5 presents the results from the estimation of the following model: $FinancialCovenants = \alpha + \beta_1 BankCovenantPreference + \beta_2 BigBank + \beta_3 BankCovenantPreference * BigBank + \beta_i CONTROLS + \varepsilon$.
We regress the number of financial covenants on *Bank Covenant Preference*, Big Bank, interaction term loan- and firm-specific control variables. *Big Bank* is equal to one if the lead arranger is one of the top 20 largest lead arrangers in the market and zero otherwise. All other variables are defined in the Appendix. Firm-specific financial variables are winsorized at the top and bottom 1%. Regressions include loan purpose, year and other fixed effects. Standard errors are heteroskedasticity robust and clustered at the firm level. *Z*-statistics are reported in parentheses.
***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

model and a Poisson model, and we find similar results.[19]

Drucker and Puri (2009) as well as Ball et al. (2015) find that some Dealscan loan observations that report no financial covenants are actually data errors and that covenants are in fact included in these loans. However, to the extent that loans without covenants represent covenant-lite loans, removing them will throw out useful information for testing our hypothesis (e.g., reducing the use of covenants from 2 to 0 will not be captured if we drop "zero covenant" observations). Following Costello and Wittenberg-Moerman (2011) and Cohen et al. (2021), we code loans with missing covenant information in Dealscan as having no covenants and include them in our sample.

To mitigate concerns that this data quality issue is affecting our inferences, we first re-run our main test on the sample of non-zero financial covenant loans (but use the style measure estimated using the total sample) and find similar results (untabulated). Second, we also use the sample of non-zero financial covenant loans to estimate the style coefficients and use it in the new analyses. Panel B of Table 7 shows these results under different specifications. Again our results are similar. Together, these two tests give us comfort that our results are not an artifact of the data resulting from how we code missing data fields.

[19] Our inference are also not affected by adding measures of borrowers' earnings quality (untabulated).

TABLE 6 The effect of bank covenant preference on the use of covenants: Based on debt contract terms

	Financial Covenants			
	(1)	(2)	(3)	(4)
Bank Covenant Preference	0.55***	0.54***	0.48***	0.47***
	(17.50)	(17.21)	(11.87)	(11.53)
Large Loan	0.03	0.02		
	(0.90)	(0.77)		
Bank Covenant Preference * Large Loan	**−0.06*** **	**−0.06** **		
	(−2.61)	**(−2.55)**		
Collateral	0.68***	0.68***	0.18***	0.18***
	(25.06)	(24.67)	(4.37)	(4.21)
Bank Covenant Preference * Collateral			**0.09*** **	**0.10*** **
			(3.47)	**(3.48)**
Facility-level controls	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes
Loan purpose fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	No	Yes	No	Yes
Constant	0.81***	1.33***	0.99***	1.43***
	(6.00)	(4.68)	(4.34)	(3.92)
Observations	30,932	30,932	30,932	30,932
R-squared	0.424	0.429	0.424	0.429

Table 6 presents the results from the estimation of the following model:
$FinancialCovenants = \alpha + \beta_1 BankCovenantPreference + \beta_2 LargeLoan(Collateral)$
$+ \beta_3 BankCovenantPreference * LargeLoan(Collateral) + \beta_i CONTROLS + \varepsilon.$
We regress the number of financial covenants on *Bank Covenant Preference*, *Large Loan (Collateral)*, interaction term loan- and firm-specific control variables. *Large Loan* is one if the size of the loan is larger than the annual median and zero otherwise. All other variables are defined in the Appendix. Firm-specific financial variables are winsorized at the top and bottom 1%. Regressions include loan purpose, year and other fixed effects. Standard errors are heteroskedasticity robust and clustered at the firm level. *Z*-statistics are reported in parentheses.
***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

5.4 | The effect of bank covenant preference for capital and performance covenants

In order to further understand which financial covenants are driving our results, we break up the covenant style effect into performance and capital covenants (Christensen & Nikolaev, 2012). It could be the case that one type of these covenants drives a banks preference for certain covenants. We re-run equation (1) using performance and capital covenants as the dependent variable in the previous year's estimation to generate performance and capital covenant *Bank Covenant Preference* and put it in the equation (2) regression. In Table 8, we find a similar covenant style effect for both performance and capital covenants.

TABLE 7 The effect of bank covenant preference on the use of covenants: Robustness tests

Panel A: Other estimation methods			
	Financial Covenants		
Variables	**(1) Covenants from borrower's last deal**	**(2) Tobit**	**(3) Poisson**
Bank Covenant Preference	**0.42*****	**0.73*****	**0.28*****
	(15.18)	**(16.92)**	**(15.71)**
Financial Covenants$_{t-1}$	0.41***		
	(60.19)		
Facility-level controls	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes
Loan purpose fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes
Constant	0.69**	0.83	0.11
	(2.40)	(1.54)	(0.54)
Observations	27,523	30,932	30,932
R-squared	0.570		

Panel B: Non-zero sample				
	Financial Covenants			
Variables	**(1)**	**(2)**	**(3)**	**(4)**
Bank Covenant Preference	**0.54*****	**0.53*****	**0.40*****	**0.02*****
	(16.73)	**(16.26)**	**(10.43)**	**(3.07)**
Facility-level controls	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes
Loan purpose fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No
Industry fixed effects	No	Yes	No	No
Firm fixed effects	No	No	Yes	No
firm*year fixed effects	No	No	No	Yes
Constant	1.55***	1.88***	2.15***	3.78***
	(7.39)	(6.61)	(7.62)	(24.68)
Observations	18,807	18,807	18,807	18,807
R-squared	0.351	0.364	0.218	0.049

Note: Table 7 presents the results from the estimation of the following model: $FinancialCovenants = \alpha + \beta_1 BankCovenantPreference + \beta_i CONTROLS + \varepsilon$.

We regress the number of financial covenants on *Bank Covenant Preference*, loan- and firm-specific control variables. All variables are defined in the Appendix. We focus on the non-zero covenants in Column 1, include number of financial covenants in the last deal in Column 2, use a Tobit model in Column 3 and use a Poisson model in Column 4. Firm-specific financial variables are winsorized at the top and bottom 1%. Regressions include loan purpose, year and industry fixed effects. Standard errors are heteroskedasticity robust and clustered at the firm level. *Z*-statistics are reported in parentheses. $***$, $**$ and $*$ denote significance at the 1%, 5% and 10% levels, respectively.

TABLE 8 The effect of bank covenant preference on the use of covenants: Performance and capital covenants

	P-covenants			C-covenants		
	(1)	(2)	(3)	(4)	(5)	(6)
Performance Covenant Preference	**0.54*** **	**0.52*** **	**0.40*** **			
	(16.70)	**(16.23)**	**(10.08)**			
Capital Covenant Preference				**0.59*** **	**0.58*** **	**0.38*** **
				(17.71)	**(17.18)**	**(9.85)**
Facility-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan purpose fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	No	Yes	No	No	Yes	No
Firm fixed effects	No	No	Yes	No	No	Yes
Constant	−0.37**	0.19	−0.55**	0.72***	0.62***	0.59***
	(−2.20)	(0.75)	(−2.35)	(8.57)	(6.29)	(6.13)
Observations	30,932	30,932	30,932	30,932	30,932	30,932
R-squared	0.382	0.396	0.293	0.199	0.224	0.132

Note: Table 8 presents the results from the estimation of the following model:
$PerformanceCovenants(CapitalCovenants) = \alpha + \beta_1 BankPerformance(Capital)CovenantPreference + \beta_i CONTROLS + \varepsilon.$
We regress the number of performance or capital covenants on performance or capital covenant *Bank Covenant Preference*, loan- and firm-specific control variables. P-covenant is the performance covenants that include (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. C-covenant is the capital-covenants that include: (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. The *Bank Covenant Preference* calculation is similar to financial covenants but based on performance or capital covenants. All variables are defined in the Appendix. Firm-specific financial variables are winsorized at the top and bottom 1%. Regressions include loan purpose, year fixed effects and other fixed effects. Standard errors are heteroskedasticity robust and clustered at the firm level. *Z*-statistics are reported in parentheses.
***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

5.5 | The effect of bank covenant preference on future borrower defaults

We further check the consequence of the bank's preference to add more financial covenants. If there is no difference in the probability of future default for different preferences to add more financial covenants, financial covenants should not be used for the reason that these banks have a relative advantage in monitoring as we argued. Table 9 presents the results from the estimation of the following model:

$$Future\ Default = \alpha + \beta_1 Bank\ Covenant\ Preference + \beta_i Controls\ + \varepsilon, \tag{6}$$

where *Future Default* is equal to one if a borrower violates a debt covenant over a loan's maturity and zero otherwise. We regress *Future Default*, acquired from the Nini et al. (2012) database discussed earlier, on *Bank Covenant Preference* at year *t*, loan- and firm-specific control variables. In contrast to prior tests in which we use a bank's previous year's coefficient to measure their covenant effect, here we use the current year's coefficient in order to capture the covenant style effect that the current loan creates.

TABLE 9 The effect of bank covenant preference on future defaults

Variables	Future Default (1)	(2)	(3)
Bank Covenant Preference	**0.20*****	**0.19*****	**0.30*****
	(3.64)	**(3.47)**	**(3.74)**
Interest Spread	0.00***	0.00***	0.00***
	(11.15)	(10.80)	(5.29)
Institutional Investor	−0.12	−0.13*	−0.09
	(−1.64)	(−1.81)	(−0.83)
Revolver	0.06	0.07	0.13*
	(1.53)	(1.62)	(1.77)
Loan Size	−0.06**	−0.05	0.05
	(−2.06)	(−1.61)	(1.33)
PP Index	0.04	0.03	0.05
	(0.66)	(0.56)	(0.67)
Maturity	0.52***	0.53***	1.40***
	(11.01)	(10.96)	(23.20)
Collateral	0.30***	0.32***	-0.06
	(4.84)	(5.14)	(−0.64)
Relationship	0.07	0.07	0.08
	(1.24)	(1.21)	(1.19)
Size	−0.15***	−0.17***	0.34***
	(−3.76)	(−4.85)	(4.04)
Leverage	0.38**	0.34**	0.23
	(2.29)	(2.04)	(1.08)
Market-to-Book	−0.10***	−0.07**	0.06
	(−2.90)	(−2.23)	(1.07)
Profitability	−1.53***	−1.53***	−2.08***
	(−4.00)	(−3.92)	(−3.65)
Tangibility	0.08	0.28	0.56
	(0.47)	(1.28)	(1.17)
Sales Growth	0.05	0.06	0.23***
	(0.99)	(1.23)	(2.79)
Cash Flow Volatility	−0.51	−0.68	0.77
	(−0.46)	(−0.57)	(0.35)
Z-Score	−0.01	−0.01	0.15***
	(−0.29)	(−0.38)	(2.77)
Loan purpose fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Industry fixed effects	No	Yes	No

(Continues)

TABLE 9 (Continued)

	Future Default		
Variables	**(1)**	**(2)**	**(3)**
Firm fixed effects	No	No	Yes
Constant	−2.13***	−16.89***	
	(−4.63)	(−2.87)	
Observations	20,867	20,867	8651
R-squared	0.149	0.161	0.398

Note: Table 9 presents the results from the estimation of the following model: $FutureDefault = \alpha + \beta_1 BankCovenantPreference + \beta_i CONTROLS + \varepsilon$.

We regress *Future Default* on *Bank Covenant Preference* at year *t*, loan- and firm-specific control variables. *Future Default* is a dummy variable that is one if there is a covenant violation (default) within the loan maturity and zero otherwise. Recall that we use the previous year's coefficient to measure supply effect in all prior regressions; here, we use the current year's coefficient to measure supply effect since that is the effect the loan is affected when setting. All other variables are defined in the Appendix. Firm-specific financial variables are winsorized at the top and bottom 1%. Regressions include loan purpose, year fixed effects and other fixed effects. Standard errors are heteroskedasticity robust and clustered at the firm level. *Z*-statistics are reported in parentheses.

***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

In Table 9, we document that covenant style is associated with an increased likelihood that a borrower will violate a loan covenant over the maturity of the loan. Note that observing a larger probability of future default with a high preference to add more financial covenants do not necessarily mean financial covenants are used for the reason that these banks have a relative advantage in monitoring. This is just a necessary condition check for the argument.

5.6 | The effect of bank covenant strictness preference on future covenant strictness

We also investigate the covenant effect using covenant strictness. Covenants, in particular restrictive covenants, reflect banks' commitment to monitoring (Callahan et al., 2019; Demiroglu & James, 2010; Garleanu & Zwiebel, 2009; Rajan & Winton 1995). If covenants are used, as we argue, because banks have a relative advantage in monitoring them, then we would also observe a similar effect using a measure of covenant strictness. In order to test this, we estimate the following regression in order to determine whether this effect exists for covenants strictness:

$$Covenant\ Strictness = \alpha + \beta_1 StrictnessPreference + \beta_i CONTROLS\ + \varepsilon, \tag{7}$$

where the covenant's strictness is the distance between the actual covenant threshold value before the contract and the contracted covenant value as stated in the contract following Demerjian and Owens (2016). *Strictness Preference* is defined similarly to *Bank Covenant Preference*, but we use the covenant's strictness as the dependent variable in the previous year's estimation.

We present the results in Table 10. The coefficient on *Strictness Preference* is significantly positive, consistent with our prior results. This test provides further evidence of the existence of a lender-driven preference for covenant usage, in contrast to most prior studies focused on borrower-driven demand.[20]

[20] In an untabulated rest, we also find that the effect of *Strictness Preference* decreases when the borrowers' financial health is better.

TABLE 10 The effect of bank covenant preference on covenant strictness

	Covenant Strictness		
Variables	**(1)**	**(2)**	**(3)**
Strictness Preference	**0.15***** **(3.93)**	**0.14***** **(3.76)**	**0.07***** **(2.69)**
Interest Spread	0.00*** (11.79)	0.00*** (10.98)	0.00*** (8.15)
Institutional Investor	−0.01 (−0.85)	−0.00 (−0.33)	0.01 (1.22)
Revolver	0.02*** (3.00)	0.02** (2.56)	0.01* (1.68)
Loan Size	−0.01*** (−2.59)	−0.02*** (−3.94)	−0.01*** (−3.74)
PP Index	0.02** (2.31)	0.02** (2.38)	0.03*** (3.72)
Maturity	0.00 (0.41)	0.00 (0.45)	0.02*** (3.27)
Collateral	0.09*** (7.34)	0.09*** (6.90)	0.06*** (5.75)
Relationship	−0.02*** (−2.86)	−0.01** (−2.53)	−0.00 (−0.14)
Size	0.31*** (10.32)	0.31*** (10.70)	0.18*** (8.17)
Leverage	−0.04*** (−6.89)	−0.04*** (−7.06)	−0.04*** (−6.34)
Market-to-Book	−0.78*** (−10.46)	−0.75*** (−10.22)	−0.84*** (−12.68)
Profitability	0.01 (0.25)	−0.07* (−1.93)	−0.15*** (−3.25)
Tangibility	0.02 (1.45)	0.01 (0.50)	−0.02*** (−2.77)
Sales Growth	0.74*** (3.29)	0.36 (1.55)	−0.04 (−0.18)
Cash Flow Volatility	0.00 (0.25)	−0.01* (−1.71)	−0.04*** (−7.31)
Z-Score	0.15*** (3.93)	0.14*** (3.76)	0.07*** (2.69)
Loan purpose fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Industry fixed effects	No	Yes	No

(Continues)

TABLE 10 (Continued)

Variables	Covenant Strictness (1)	(2)	(3)
Firm fixed effects	No	No	Yes
Constant	0.48***	0.68***	0.66***
	(5.92)	(4.10)	(8.10)
Observations	14,075	14,075	14,075
R-squared	0.259	0.286	0.406

Table 10 presents the results from the estimation of the following model:. $CovenantStrictness = \alpha + \beta_1 StrictnessPreference + \beta_i CONTROLS + \varepsilon$.
We regress *Covenant Strictness* on *Strictness Preference* at year *t*, loan- and firm-specific control variables. *Strictness Style* is similarly defined as *Bank Covenant Style*, but we use covenant strictness (from Demerjian & Owens, 2016) as the dependent variable in the previous year's estimation. All other variables are as defined in the Appendix. Firm-specific financial variables are winsorized at the top and bottom 1%. Regressions include loan purpose, year, firm and other fixed effects. Standard errors are heteroskedasticity robust and clustered at the firm level. *Z*-statistics are reported in parentheses.
***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

5.7 | The effect of bank covenant preference on other contract terms

We argue that banks are more likely to add more covenants in debt contracts because they have a relative advantage in setting and monitoring them. Given that there are several contracting terms from which banks can choose, it may be that banks that prefer covenants will be less likely to use other terms, relative to their peers. We investigate this possibility by examining the effect of covenant style on other loan contract terms and report the results in Table 11. We find that a bank's preference for including more covenants is associated with lower interest spreads, shorter loan maturities and a lower likelihood of requiring collateral. These findings are consistent with banks considering monitoring costs when designing loan contracts and being willing to offer more favorable loan terms in exchange for adding the covenants they have a relative advantage in monitoring.

5.8 | Bank covenant preference after bank CEO and CFO changes

In order to provide further evidence that our results are driven by the supply side of lending relationships, we interact covenant style with an indicator variable, *Bank CEO/CFO Change*, which is equal to one in the year after the lead arranger bank changes its CEO or CFO and zero otherwise. Bamber et al. (2010) document a CEO-specific effect on disclosure style, and we similarly expect that a change in CEOs or CFOs will lead to a decrease in covenant style as the new CEO or CFO implements new guidelines and procedures consistent with their preferences and expertise. Change in top managers (e.g., CEOs/CFOs) might also lead to turnover for other senior lending officers, further changing lending practices and affecting covenant style. We obtain CEO and CFO data from the Execucomp database, limiting our sample to lead arrangers that are publicly listed with CEO and CFO data available. We run the following regression to test our conjecture:

$$\begin{aligned} Financial\ Covenants &= \alpha + \beta_1 Bank\ Covenant\ Preference + \beta_2 Bank\ CEO/CFO\ Change \\ &+ \beta_3 Bank\ Covenant\ Preference * Bank\frac{CEO}{CFO}Change + \beta_i Controls + \varepsilon. \end{aligned} \tag{8}$$

We present the results of this regression in Table 12. The coefficient on the interaction between *Bank Covenant Prefer-*

TABLE 11 The effect of bank covenant preference on other contract terms

Variables	*Interest Spread* (1)	*Maturity* (2)	*Collateral* (3)
Bank Covenant Preference	**−19.73*** **	**−0.05*** **	**−0.19*** **
	(−8.14)	**(−4.34)**	**(−3.74)**
Institutional Investor	53.27***	0.50***	4.05***
	(15.61)	(32.11)	(22.97)
Revolver	−27.27***	0.24***	0.18***
	(−15.20)	(16.47)	(5.14)
Loan Size	−16.21***	0.03***	−0.22***
	(−15.62)	(5.58)	(−7.40)
PP Index	−39.56***	0.15***	0.62***
	(−23.13)	(16.76)	(13.31)
Maturity	−1.66		0.35***
	(−1.00)		(10.07)
Collateral	63.25***	0.12***	
	(28.84)	(10.63)	
Relationship	−10.19***	−0.06***	0.03
	(−6.15)	(−6.65)	(0.72)
Size	−7.93***	−0.01**	−0.42***
	(−7.88)	(−2.51)	(−15.49)
Leverage	64.81***	0.23***	1.92***
	(11.39)	(7.69)	(12.88)
Market-to-Book	−9.95***	−0.02***	−0.19***
	(−9.45)	(−3.82)	(−6.76)
Profitability	−109.69***	0.49***	−3.06***
	(−7.44)	(6.97)	(−7.88)
Tangibility	−8.18	−0.06**	−0.27
	(−1.16)	(−2.04)	(−1.50)
Sales Growth	1.08	0.03***	0.36***
	(0.60)	(2.62)	(6.72)
Cash Flow Volatility	227.83***	−1.59***	7.83***
	(5.72)	(−7.64)	(5.92)
Z-Score	−10.44***	0.02***	−0.17***
	(−9.34)	(3.39)	(−5.63)
Loan purpose fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes
Constant	654.82***	2.66***	6.54***
	(26.07)	(25.36)	(10.57)

(Continues)

TABLE 11 (Continued)

Variables	*Interest Spread* (1)	*Maturity* (2)	*Collateral* (3)
Observations	30,932	30,932	30,932
R-squared	0.520	0.297	0.320

Note: Table 11 presents the results from the estimation of the following model: $Spread(MaturityorCollateral) = \alpha + \beta_1 BankCovenantPreference + \beta_i CONTROLS + \varepsilon$.
We regress *Spread (Maturity or Collateral)* on *Bank Covenant Preference*, loan- and firm-specific control variables. All variables are defined in the Appendix. Firm-specific financial variables are winsorized at the top and bottom 1%. Regressions include loan purpose, year and industry fixed effects. Standard errors are heteroskedasticity robust and clustered at the firm level. *Z*-statistics are reported in parentheses. $***$, $**$ and $*$ denote significance at the 1%, 5% and 10% levels, respectively.

ence and *Bank CEO/CFO Change* is significantly negative, consistent with new CEOs and CFOs changing the "style" of the covenants included in loan contracts. This last test provides additional evidence that the style effect we document is driven by the supply side of debt financing.

5.9 | Caveats

In our study, we focus on analysis on the inclusion of financial covenants by lenders, while controlling for other price and non-price contract terms. Although we follow prior literature in much of our design by considering one contract term and controlling for other terms (e.g., interest spread), these contract outcomes likely are determined simultaneously (Bradley & Roberts, 2015). While some of the contract terms can be set prior to other terms during the contracting process, it is empirically challenging for us to disentangle the effects through a traditional instrumental variable approach. While we acknowledge that this poses a potential limitation of our study and, therefore, interpret the economic magnitude of our estimates with caution, we still feel that our findings provide insights into the corporate financing decisions of firms.

6 | SUMMARY AND CONCLUSION

In this study, we extend the literature on the supply-side determinants of debt covenants included in loan agreements. We provide evidence that lenders themselves have a preference for the covenants that they include in contracts, consistent with lenders having a covenant "style." Specifically, we find that controlling for borrower characteristics, the covenants included in the recent loans of a lender have predictive power for the covenants that will be included in a lender's subsequent loan contracts and that this effect persists for at least 3 years.

We perform a series of additional tests to investigate the factors that affect this covenant style, and we find that covenant style is larger for small banks, for whom the costs of contracting are highest, and when the borrower provides collateral, where lenders' downside is reduced. We also find that the style effect is smaller for borrowers that have recently violated a debt covenant on a prior loan or when the loan issue amount is large, consistent with lenders understanding the tradeoff that they make between efficiencies in contracting and credit risk. We provide evidence that a preference for including more covenants from the supply side (i.e., lender preference driven) is associated with an increased likelihood that a borrower will violate a debt covenant over the life of its loan, a lower interest spread, a shorter maturity and a lower likelihood of a collateral requirement. Last, we document that the covenant style effect decreases in the year after a lead arranger bank changes its CEO or CFO further evidence that covenant style stems, at least in part, from individual manager preferences and expertise.

TABLE 12 The influence of the supply effect on the use of covenants: the effect of top management changes

	Financial Covenants	
Variables	**(1)**	**(2)**
Bank Covenant Preference	0.73***	0.72***
	(10.30)	(10.07)
Bank CEO/CFO Change	0.15**	0.13**
	(2.54)	(2.18)
Bank Covenant Preference* Bank CEO/CFO Change	**−0.15****	**−0.14****
	(−2.21)	**(−1.97)**
Facility level controls	Yes	Yes
Firm level controls	Yes	Yes
Loan propose fixed effect	Yes	Yes
Year fixed effect	Yes	Yes
Industry fixed effect	No	Yes
Constant	0.20	0.91***
	(0.79)	(2.99)
Observations	6694	6694
R-squared	0.338	0.358

Note: Table 12 presents the results from the estimation of the following model:
$FinancialCovenants = \alpha + \beta_1 BankCovenantPreference + \beta_2 BankCEO/CFOchange + \beta_3 BankCovenantPreference * BankCEO/CFOchange + \beta_i CONTROLS + \varepsilon$
We regress the number of financial covenants on *Bank Covenant Preference*, *Bank CEO/CFO Change*, the interaction of these variables and loan- and firm-specific control variables. *Bank CEO/CFO Change* is equal to one in the year after the lead arranger bank changes its CEO or CFO and zero otherwise. All other variables are as defined in the Appendix. Firm-specific financial variables are winsorized at the top and bottom 1%. Regressions include loan purpose, year and other fixed effects. Standard errors are heteroskedasticity robust and clustered at the firm level. *Z*-statistics are reported in parentheses.
***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Overall, we add to the literature by shedding light on the supply-side, or lender side, of loan contract design. A large body of work has explored the borrower characteristics that influence debt contract design. However, far less is known about whether and in what way lenders themselves shape debt contracts based on their preferences, and this study partially fills this gap in the literature.

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APPENDIX

Variable definitions

Variables	Definitions
Financial Covenants	The number of financial covenants included in the loan agreement
Bank Covenant Preference	The coefficients of (lead arranger) banks' fixed effects from a model with financial covenants as the dependent variable and additional relevant control variables included as independent variables. All included control variables are calculated using the prior year's data (t−1). Estimated fixed effects that are not statistically significant are calculated as zero
Interest Spread	The interest rate is the *All-in-Drawn-Spread* measure reported by Dealscan, and it is equal to the number of basis points over LIBOR
Bank CEO/CFO Change	An indicator variable equal to one in the year after the lead arranger changes its CEO or CFO and zero otherwise
Institutional Investor	An indicator variable equal to one if the loan's type is term loan B, C or D (institutional term loans) and zero otherwise
Revolver	An indicator variable is equal to one if the loan is a revolver and zero otherwise
Loan Size	Amount borrowed in millions of dollars
PP Index	An indicator variable that takes the value of one if the loan contract incorporates a performance pricing option and zero otherwise
Maturity	The number of months between the facility's issue date and the loan maturity date
Collateral	An indicator variable that is equal to one if the loan is backed by collateral and zero otherwise
Relationship	An indicator variable equal to one if a borrower and any of the lead arrangers on the deal have had a prior lending relationship during our sample period and zero otherwise
Size	The natural log of total assets, estimated in the year prior to entering into a loan contract
Leverage	Long-term debt divided by total assets, estimated in the year prior to entering into a loan contract
Default	An indicator variable equal to one if the borrower has experienced a technical default on a loan in the last year and zero otherwise
Market-to-Book	Market value of equity plus the book value of debt over total assets in the year prior to entering into a loan contract
Profitability	EBIDTA divided by total assets, estimated in the year prior to entering into a loan contract
Tangibility	Net PPE divided by total assets, estimated in the year prior to entering into a loan contract
Sales Growth	Sales percentage growth
Cash Flow Volatility	Standard deviation of quarterly cash flows from operations over previous four fiscal years, scaled by total assets
Z-Score	Probability of bankruptcy score (Zmijewski, 1984). We exclude the market-to-book component because we include market-to-book in our tests as a separate control variable
Loan Purpose Effect	A series of indicator variables for the purposes of loan facilities in Dealscan, including: corporate purposes, debt repayment, working capital, CP backup, takeover and acquisition line