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Non-competes, Career Concerns, and Debt Covenants*

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Abstract

We study the impact of managers' career concerns from non-competes enforcement on the design of debt covenants in private debt agreements. Using exogenous changes in the enforceability of managers' non-compete clauses over the period of 1992-2004 across states in the United States, we show that borrowers headquartered in U.S. states with strong enforcement of non-compete clauses have fewer debt covenants compared to those headquartered in states with weak enforcement after controlling for reporting quality, risk taking, and firm performance. Our evidence is consistent with the argument that reduced job mobility and enhanced career concerns incentivize managers to ex ante avoid debt covenants that may trigger default and lead to high risk of terminating their current employment. Moreover, the effect of the enforceability of non-competes on debt covenants is more pronounced for managers with limited outside options (i.e., low ability, small network) or firms with strong bargaining power with lenders.

JEL Classification: G21, G31, J41

Keywords: non-compete clauses, career concerns, debt covenants

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I. INTRODUCTION

Non-compete clauses are contractual provisions that forbid exiting managers from working for competing firms for a specified period of time within a certain geographic area (Gilson 1999). They are common practice for upper-level management in publicly traded U.S. firms (Samila and Sorenson 2011). Garmaise (2011) shows that at least 70% publicly traded U.S. firms use non-compete clauses with their top managers.¹ These clauses restrain managers' free movement from one firm to another, limit their outside options, and impose higher job-switching and unemployment costs (Vanko 2002; Marx, Strumsky, and Fleming 2009; Garmaise, 2011), all of which increase managers' career concerns. Such enhanced career concerns have an effect on managerial incentives and in turn on firm policies (e.g., Garmaise 2011; Chen, Zhang, and Zhou 2016). In this paper, we examine the impact of non-compete provisions on the design of debt covenants in private debt agreements.

Financial theories suggest that managerial incentives affect debt contracting (e.g., John and John 1993; Begley and Feltham 1999). Managerial incentives not only include explicit incentives arising from compensation contracts but also implicit ones related to career concerns (Gibbons and Murphy 1992). Career concerns are managers' concerns about the effects of current performance on the labor market's assessment of their ability (e.g., Gibbons and Murphy 1992). They arise frequently because the efforts of managers are unobservable and unfavorable assessment of their ability can have significant adverse effects such as job termination (e.g., Hermalin and Weisbach

¹ Focusing on the 1992-2004 period, Garmaise (2011) used a random sample of 500 Execucomp firms and conducted searches in 10kwizard.com of the 10-K, 10-Q, and other SEC filings of these firms. For 351 (70.2%) of the firms, Garmaise found evidence of noncompetition agreements between the firm and its top executives. He notes that this number likely underestimates the prevalence of non-compete clauses as there is no regulation on the disclosure of such clauses in corporate filings in the U.S. during this period. Bishara, Martin, and Thomas (2015) find even greater usage of such provisions in recent years. One of the potential concerns in our study is our results are driven by the states with a very small percentage of firms that include non-compete clauses in their executive employment contracts. We address this issue in Section 6.

1998; Holmstrom 1982, 1999). Consequently, career concerns may incentivize managers to ex ante avoid debt covenants that could trigger default and negatively affect the labor market's assessment of their ability. Using a quasi-natural experiment of the changes in non-competes enforceability across states (Stuart and Sorenson 2003; Marx, Strumsky, and Fleming 2009; Amir and Lobel 2011; Garmaise 2011; Samila and Sorenson 2011), we examine the causal effect of managers' concerns from non-competes on debt covenants.

In this study, we focus on debt covenants because they are the most relevant terms to a manager's control rights and job security in a debt contract. The violation of debt covenants typically leads to a large negative stock price reaction and difficulty in securing additional financing (Beneish and Press 1993; Roberts and Su 2009a). It also enables the transfer of control rights from managers to debt holders (e.g., Aghion and Bolton 1992; Dewatripont and Tirole 1994). Furthermore, recent studies show that such violation increases shareholder and auditor uncertainty in the form of higher bid-ask spread, return volatility, and audit fees. These are costly to firms because they impair timely price discovery, increase the cost of capital, and create basis risk for issuing new equity or making stock-based acquisitions (e.g., Gao, Khan, and Tan 2017). Taken together, the violation of debt covenants can send a negative signal to the labor market about managers' ability and increase job loss risk (Nini, Smith, and Sufi 2012). Therefore, when managers' career concerns increase due to the strict enforcement of non-competes, managers may have strong incentives to avoid covenant violations by ex ante reducing the presence of debt covenants. Furthermore, we expect that such incentives to be concentrated among managers who are more affected by the exogenous changes in enforcement and among firms with greater bargaining power over the design of debt contract terms.

Our results support above predictions. First, our differences-in-differences estimates show that borrowing firms headquartered in states with strong enforcement have fewer financial and general covenants in private debt agreements, compared to those headquartered in states with weak enforcement. The result holds after controlling for various firm and debt characteristics, as well as firm fixed effects that control for time-invariant factors relevant to the design of debt covenants. This effect is economically significant. The switch from a weak to strong enforcement regime leads to a 10% decrease in the mean value of the number of debt covenants. We also find that the effect of the enforcement of non-competes on debt covenants is weaker for high-ability or well-connected managers who are less concerned with job security, but is stronger for firms holding a greater proportion of the leader bank's total outstanding loans and hence more bargaining power over debt contract terms.

In addition, we show that the effect of the enforceability of non-competes on debt covenants we document is not driven by changes in lenders' expectation about borrowers' fundamentals. Specifically, we find no evidence that the reduction in covenants is caused by firms that experience a decrease in accounting quality ([Costello and Wittenberg-Moerman 2011](#); [Chen, Zhang, and Zhou 2016](#)), a decline in risk taking ([Stuart and Sorenson 2003](#); [Amir and Lobel 2011](#); [Garmaise 2011](#); [Samila and Sorenson 2011](#)), or an increase in profitability ([Holmstrom 1982](#); [Grossman and Hart 1983](#)). Note that although none of these tests is individually able to rule out all possible alternative explanations, taken together, the results suggest that it is unlikely that our main findings are driven by other confounding effects of non-competes enforcement.

As discussed before, we focus on debt covenants because potential debt covenant violation is the mechanism through which managers' job mobility and career concerns are most likely affected by non-competes. In additional analysis, we also examine the effect of the enforceability of non-

competes on debt contract terms other than covenants. We do not find evidence that the strong enforcement of non-competes affects the loan size or the use of collaterals in the debt agreements. However, we find some weak evidence that the increased enforcement of non-competes leads to longer debt maturity, which helps reduce firms' short-term liquidity risk. This result suggests that managers with greater career concerns not only negotiate fewer covenants but also longer maturity with lenders to avoid being removed from their jobs in case of liquidation.

Moreover, borrowing firms that are subject to strong enforcement of non-competes are charged with higher interest rates compared to those that are subject to weak enforcement of non-competes. This result suggests that increased career concerns lead to a trade-off between different contract terms. Specifically, in exchange for fewer debt covenants and longer maturity, managers seem to be willing to sacrifice firms' cash flows and accept higher interest rates. Our results continue to hold if we account for the simultaneous determination of debt contract terms by estimating all contract terms in simultaneous equations ([Asquith, Beatty, and Weber 2005](#)).

Finally, we conduct several robustness tests. First, we show that stricter non-compete enforcement leads to less tight covenants and lower likelihood of covenant violations, consistent with managerial incentives to reduce the job risk from debt covenant violation. Second, our results are robust to dropping firms headquartered in California, which is the largest state in the control group. Third, our results are also robust to the short-window trend analysis that mitigates the concern that debt contract terms may have changed in the years prior to the change in the enforcement of non-competes. Finally, [Vasvari \(2008\)](#) shows that managers' compensation structure affects their risk-taking incentives and in turn has an effect on debt contract terms. To control for the effect of compensation structure on the contract terms, we include various compensation components (e.g., severance packages and stock options) as additional controls. As

a result, the sample size reduces significantly, but the effect of the increased enforcement of non-competes on the number of covenants remains statistically and economically significant.

Our paper makes three contributions to the literature. First, broadly speaking, we contribute to the financial contracting literature. A firm is a nexus of contractual relationships between different stakeholders including managers and debt holders (Jensen and Meckling 1976). While prior literature has focused on the role of compensation-related managerial incentives in debt contracting (e.g., Vasvari 2008; Brockman, Martin, and Unlu 2010), using legal changes to the enforceability of non-compete provisions as plausible exogenous shocks, we provide first evidence on the casual effect of managerial incentives related to labor market mobility and career concerns on the design of debt contract terms.² As such, we add to the understanding of how managers' incentives can impact financial contracting. In particular, prior studies argue that debt covenants play an important role in reducing agency conflicts (e.g., Dessein 2005; Garleanu and Zwiebel 2009). Our finding suggests a caveat to such argument: managers can manipulate the structure of debt contracts to undo the potential benefits of debt covenants in mitigating agency problems.

Second, we also contribute to the debt contracting literature by studying the trade-off between different contract terms. In some studies, all contract terms are set uniformly to reduce the agency cost of debt from lenders' perspectives (e.g., Bharath, Sunder, and Sunder 2008; Chan, Chen, and Chen 2013). For example, Bharath, Sunder, and Sunder (2008) show that firms with poor accounting quality are not only charged with strict price terms (i.e., high interest rates) but are also imposed with stringent non-pricing terms such as collateral and short maturity. We argue that

² More broadly, prior studies of career concerns focus on the effect of manager-specific career concerns (tenure, founder status, and promotion path). Those measures are often correlated with other factors (such as individual ability, agency costs or information asymmetry), which complicates the identification problem. Our identification strategy using plausibly exogenous shocks allows us to draw clearer causal inferences than these prior studies.

managers also influence the design of debt contract terms. In particular, managers trade off the costs and benefits of each debt contract term when negotiating with lenders, which leads to a substitution effect between pricing and non-pricing debt contract terms.

Finally, we add to the growing literature on the effect of managers' non-compete clauses on managers' behavior and corporate policies [see [Bishara and Thomas \(2015\)](#) for a literature review]. Non-competes reduce corporate investment ([Garmaise 2011](#)), and lead to less efficient employer-employee matching ([Marx, Strumsky, and Fleming 2009](#)), more impediment to innovation and entrepreneurship ([Samila and Sorenson 2011](#)), withholding bad news ([Ali, Li, and Zhang 2016](#)) and less reliable earnings reports ([Chen, Zhang, and Zhou 2016](#)). In line with these studies, we show that due to career concerns, managers extend their influence over debt contracting in response to the increased enforcement of non-competes. Managers' willingness to sacrifice firms' cash flows via offering higher interest rates in exchange for fewer debt covenants points to yet another negative consequence associated with managers' non-compete clauses.

The rest of the paper is organized as follows. In Section II, we discuss the institutional background and develop the hypothesis. We describe the data and sample construction in Section III. In Section IV, we present the methodology and the main results. We discuss some additional analyses in Section V. We conclude in Section VI.

II. INSTITUTIONAL BACKGROUND AND HYPOTHESIS DEVELOPMENT

Institutional Background

The legal literature shows that non-competes are effective mechanisms in binding employees to their employers. Non-competes reduce managers' job mobility because they restrict managers' post-employment opportunities and impose legal costs on executives who switch jobs or establish new businesses in similar or related industries after they leave their current employers ([Gilson](#)

1999; Samila and Sorenson 2011). For example, early evidence shows that increased enforcement of noncompetition agreements in Michigan in the 1980s led to a 34% decrease in the mobility of inventors (Marx et al. 2009). Using a sample of publicly listed U.S. firms, Garmaise (2011) also provides evidence that the switch from a low to a high non-compete enforcement regime leads to a 47% reduction in the frequency of a manager finding another job in the same industry.³

Such reduction in job mobility in response to a shift to a tougher enforcement could be explained by following reasons. First, employers become more aware of using non-compete provisions to protect their interests. Prior studies show that the number of employees sued for breaching non-compete clauses has increased significantly in recent years. For example, the number of U.S. court decisions pertaining to non-compete clauses rose 61% from 2002 to 2012. By 2012, there were 760 cases ruled related to the violation of these provisions (Simon and Loten 2013). These numbers are the low bound for the total number of lawsuits involving non-compete clauses since many of these cases are settled out of court.

Second, lawsuits related to the violation of non-competing agreements impose significant financial cost on managers and distract them from their new jobs. For example, Capital One brought a lawsuit against two former employees, John Kanas and John Bohlsen, on charges of violating the non-compete provisions after they became the Chief Executive Officer and Chief Lending Officer of BankUnited. In June 2012, the former employees settled the case with Capital One by agreeing to pay a \$20 million fine, which is about six times their annual salaries for 2011.⁴

³ It is possible for some managers to switch jobs to firms in a different industry, which would not violate non-compete agreements. However, we expect the frequency of job-switching across industries to be low given that managers are usually hired based on their industry expertise. In particular, if a debt covenant violation has occurred during managers' tenure in their previous employment, it would send a negative signal about managers' ability to the labor market, further reducing their cross-industry job mobility.

⁴ Note that we do not rely on the assumption that executives themselves pay the settlement costs. In some cases, the perspective employers help pay such settlement costs. Regardless of who pays, the higher costs eventually reduce the labor market mobility because of higher job-switching costs (e.g., Marx et al. 2009; Garmaise 2011). Based on the

In addition, BankUnited was restricted until January 2013 from expanding its business to the New York area where Capital One has a major presence.

Furthermore, the enforcement of the non-competes can also have an indirect effect on managers who are subject to these provisions through potential employers that are looking to recruit new managers. This is because those potential employers may also avoid recruiting managers with non-competes due to the potential extra cost of paying managers until their non-compete provisions expire ([Simon and Loten 2013](#)).

Taken together, non-competes limit managers' outside employment opportunities and impose high job-switching costs on managers ([Vanko 2002](#)), thereby significantly increasing managers' career concerns. Although non-competes are widely used in executive employment contracts, the enforcement of these provisions varies from state to state. For example, California is historically known for its lax enforcement of non-compete clauses. [Fallick, Fleischman, and Rebitzer \(2006\)](#) show that California has employee mobility that is 56% higher than other states in the computer industry. In fact, legal scholars posit that the weak enforcement of non-compete clauses may have contributed to the diffusion of knowledge and personnel in the information technology industry and thereby play a crucial role in the rise of Silicon Valley (e.g., [Gilson 1999](#); [Samila and Sorenson 2011](#)). Further, the state laws pertaining to non-compete clauses have also changed over time. For example, in 1996, Florida replaced its law governing non-competes with a new law. The new law rests on the presumption that there is injury to the firm whenever a non-compete clause is violated, which strengthens the firm's ability of obtaining an injunction that blocks its former employee from working for another firm ([Garmaise 2011](#)).

prior findings, we argue that, all else equal, executives in states with more strict enforcement of non-competes are faced with less mobile labor market and therefore have stronger incentives to minimize their risk of job dismissal.

Hypothesis Development

We posit that the enforceability of managers' non-compete clauses have significant effects on managers' incentives to increase their job security, which in turn affects the negotiation between managers and lenders in setting the initial debt contract terms, in particular debt covenants. Prior studies show that managers do influence debt structure and contractual terms in the loan initiation process (e.g., [Dennis and Sharpe 2005](#); [Hackbarth, Hennessy, and Leland 2007](#); [Niesche 2012](#)). Stronger non-compete enforcement reduces job mobility and increases managers' career concerns, which alter their incentives to contract with lenders. [Marx, Strumsky, and Fleming \(2009\)](#) and [Garmaise \(2011\)](#) show that executive mobility and managers' outside options decrease sharply when non-compete clauses become binding at the state level. The reduced outside options in turn put pressure on managers to retain their current jobs (e.g., [Ali, Li, and Zhang 2016](#); [Chen, Zhang, and Zhou 2016](#)). As the cost of job-switching and dismissal increases with the enforceability of non-compete clauses, managers would have incentives to avoid events that may give rise to a negative assessment of their ability, which would in turn increase their likelihood of being discharged or further constrain their limited outside opportunities. These career concerns can lead managers to ex ante avoid debt covenants that could trigger default and threaten their current employment.

We focus on debt covenants because they play an important role in reducing the agency conflicts between borrowers and lenders ([Jensen and Meckling 1976](#)). Furthermore, incomplete contracting theories suggest that debt covenants serve as contractual tools to transfer the control right from borrowers to lenders when borrowers financially underperform (e.g., [Aghion and Bolton 1992](#); [Dewatripont and Tirole 1994](#)). The violation of debt covenants can trigger the bankruptcy of borrowers and the loss of executive jobs. Even if a debt covenant violation does not lead to firm

default/bankruptcy, it can still limit managers' power since lenders can obtain the control right of borrowing firms and influence firm policies (e.g., [Roberts and Su 2009a, 2009b](#); [Nini, Smith, and Sufi 2012](#); [Falato and Liang 2017](#)). For example, [Nini, Smith, and Sufi \(2012\)](#) show that creditors play an active role in corporate governance in response to the violation of debt covenants. In particular, they document that debt covenant violations are followed by an immediate increase in CEO turnover. Similarly, [Ozelge and Saunders \(2012\)](#) show that the power of lending banks in forcing CEO turnovers can be due in large part to covenant violations. The increase in a CEO's replacement probability is twice larger when a borrowing firm violates its loan covenants than when it does not.

In addition, even if CEOs are not replaced, debt covenant violations can provide a negative signal on their managerial ability to the labor market, which further narrows their limited outside options. [Chava and Roberts \(2008\)](#) and [Nini, Smith, and Sufi \(2009\)](#) find that shareholders bear substantial direct and indirect costs when firms violate debt covenants (i.e., technical defaults). For example, [Roberts and Sufi \(2009a\)](#) show that firms experience an increase in interest cost and a decrease in the availability of credit following debt covenant violations. Consequently, managers who are subject to strict enforcement of non-competes have strong incentives to minimize the presence of covenants in debt contracts ex ante. As such, they can avoid debt covenant violations that could lead to a negative assessment of their ability by the labor market or even the termination of their current employment.

Nevertheless, the reduction in the number of debt covenants could also be affected indirectly by managers' career concerns. For example, [Chen, Zhang, and Zhou \(2016\)](#) find that managers with greater career concerns report better financial performance by adopting an aggressive accounting policy. Many covenants are based on financial ratios, such as the interest coverage

ratio. If increased non-compete enforceability leads managers to distort the quality of accounting numbers, debt holders may in turn rely less on the financial information reported by firms and include fewer accounting-based covenants in debt agreements (e.g., [Costello and Wittenberg-Moerman 2011](#)).⁵

Taken together, we hypothesize that stricter non-compete enforcement, which reduces job mobility and increases managers' career concerns, can lead to fewer covenants in debt contracts.⁶ However, the non-competes may also affect debt covenants through other channels such as the riskiness of firms' cash flows and corporate performance, which influence debt holders' incentive to include debt covenants. We discuss and conduct empirical analyses to rule out these alternative channels in Section IV.

III. DATA AND SAMPLE CONSTRUCTION

Our initial sample consists of all U.S. companies in Compustat from year 1992 to 2004, which coincides with the period of [Garmaise's \(2011\)](#) enforceability index for all jurisdictional regions of the U.S. [Garmaise \(2011\)](#) constructed this state-level enforceability index and the changes in enforceability using 12 questions proposed by [Malsberger \(2004\)](#) regarding the details of non-compete laws in the U.S. The enforceability index captures the strength of the legal enforcement of non-compete clauses, with higher values indicating stricter enforcement. To ascertain a firm's jurisdictional area, we retrieve information on the location of each firm's headquarters from

⁵ Alternatively, the upward earnings manipulation reduces the likelihood of accounting-based covenants being violated, which in turn reduces managers' incentives to minimize the presence of such covenants ex ante.

⁶ Although general covenants set restrictions on corporate activities that are directly decided by managers (e.g., asset selling, dividend issuance) and thus are less likely to be violated, managers would want to keep these operating options open, especially when the firms are close to financial covenants violation. Therefore, we expect that stricter non-competes enforcement incentivizes managers to reduce both financial and general covenants.

Compact Disclosure.⁷ We exclude firms with missing data in Compustat, CRSP, I/B/E/S, Thomson Reuters, and DealScan. The final sample consists of 14,838 loan facilities issued by 3,648 firms.

Garmaise (2011) identifies three states that amended their non-compete enforcement laws during the 1992-2004 period. Specifically, Florida increased its enforceability of non-competes in 1996 following a legislative act (*Fla. Sess. Law Serv. Ch. 96-257*); Texas loosened the enforceability of non-competes after its Supreme Court ruled on *Light v. Cellular Co.* (Texas, 1994); the Louisiana Supreme Court reduced the non-compete enforceability in the case *SWAT 24 Shreveport Bossier v. Board* in 2001, but later the state legislature retightened non-compete enforceability in 2003 (*La. Sess. Law Serv. Act. 428*). These changes have been explored in several other studies (e.g., Garmaise 2011; Samila and Sorenson 2011; Acharya, Baghai, and Subramanian 2013; Belenzon and Schankerman 2013).

Of the 14,838 loan facilities in our sample, 2,357 loan facilities (15.88%) are issued by firms located in one of these three states, which experienced a shift in non-compete enforcement policy. Therefore, 15.88% of loan facilities are treated, while 84.12% of loan facilities belong to the control group. Table 1 presents the distribution of enforceability index and loan facilities by state. As shown, the enforceability index varies significantly across states and years. For example,

⁷ We assume that a company's headquarters state is where non-compete lawsuits would be filed because of *lex loci contractus* ("law of the place where the contract is made) principle in labor and employment laws (e.g., Pollard 2014). Consistent with this, the headquarters state is the most commonly chosen state for law provision of non-compete lawsuits. To provide more support for this assumption, we manually check through the CEO employment contracts of S&P 500 firms to look for the choice of law provision. We find that 79% of the firms choose the state of the company's headquarters as the governing state. Nonetheless, we acknowledge that there is some controversy as to which state's law should apply in enforcing non-competes when a lawsuit occurs, as shown by previous case law (e.g., *Advanced Bionics v. Medtronic* 2002; *Aspect Software Inc. v. Barnett* 2011; *In re Autonation Inc.*, 2007). However, we believe that this uncertainty would only add noise and bias against what we find.

California has an enforceability index of 0, while Florida has an enforceability index of 9 from 1996 until 2004.

IV. EMPIRICAL DESIGN AND MAIN RESULTS

Empirical Design

To examine the effect of non-competing enforcement on the number of debt covenants, we estimate the model below following [Bertrand and Mullainathan \(2003\)](#) and [Garmaise \(2011\)](#):

$$Y_{i,j,t} = \alpha_0 + \alpha_1 IncreaseEnforce_{s,t} + \beta X_{i,j,t} + \gamma_j + \sigma_i + \omega_t + \varepsilon_{i,t}. \quad (1)$$

The basic unit of the empirical analysis is a loan facility, also referred to as a facility or tranche in *Dealscan*. The dependent variable is the number of debt covenants (*TotalCov*), including both general and financial covenants (*GenCov* and *FinCov*). Since the number of debt covenants is a count variable, we use Poisson estimation in the analyses. However, our results are robust to using OLS estimation.

The independent variable of interest, *IncreaseEnforce*, captures shifts in state laws governing the enforcement of non-compete provisions and their subsequent effects on the number of debt covenants. Specifically, *IncreaseEnforce* takes the value of 1 for firms located in Florida in the 1997-2004 period, -1 for firms located in Texas in the 1995-2004 period, and firms located in Louisiana in the 2002-2003 period. For all other firm-year observations, *IncreaseEnforce* is equal to 0.

As discussed in the previous section, two of the changes in non-compete enforceability (Texas in 1994 and Louisiana in 2001) were led by court rulings, and the other two (Florida in 1996 and Louisiana again in 2003) by legislative changes. Although legislative events may result from firms' lobbying activities or may be anticipated by managers, we do not find evidence that

treatment firms start to change their debt contract terms in the years prior to the amendments, or relocate to other states with lower enforceability in the years surrounding the two amendments in Florida and Louisiana. Furthermore, we searched for and found no contemporaneous legal changes in Florida and Louisiana related to employee contracts or executive compensation. These findings provide further support for our setting.

We also add firm and year fixed effects (σ_i and ω_t) to all regressions. The firm fixed effects control for any time invariant firm-level factors that affect the contract terms, while the year fixed effects account for common time variant factors. This research design essentially represents a difference-in-differences approach in which firms with headquarters in the states that have not experienced a change of non-compete enforceability in a given year serve as the control group for firms headquartered in states that have experienced the change of non-compete enforceability in that year (e.g., [Bertrand and Mullainathan 2003](#); [Garmaise 2011](#)). The coefficient α_1 is our difference-in-difference estimate, which captures the average effect of the non-compete enforceability change on the treatment group relative to the control group. Specifically, we predict a negative coefficient on *IncreaseEnforce* (α_1) if an increase in non-compete enforceability incentivizes managers to lower the number of debt covenants.

Furthermore, we follow [Graham, Li, and Qiu \(2008\)](#) and include a set of variables to control for firm, loan, and macroeconomic characteristics that influence the design of debt contract terms. For example, we include firm size (*Size*), market-to-book ratio (*MB*), leverage ratio (*LEV*), firm profitability (*Prof*), cash flow volatility (*CFVol*), tangible assets (*Tang*), default risk (*Distress*), and earnings quality (*AbsAM*). We also control for various loan characteristic such as loan type (*InstitutionalInvestor*), loan size (*LoanSize*), loan maturity (*Maturity*), syndication size (*NumLender*), performance pricing (*PPindicator*), whether the loan is secured (*Collateral*), and

loan purposes (γ). In addition, to control for macroeconomic factors, we include monthly credit spread (*CreditSpread*) and term spread (*TermSpread*) for the month prior to the loan issuance, as well as state-level GDP growth of a firm's headquarter (*ChgGDP*).

Because the enforcement of non-compete provisions varies by state, we cluster standard errors by states to account for heterogeneity and within-state correlation. By doing so, we mitigate potential concerns due to time-varying correlations in omitted variables that could systematically influence firms within each headquarter state (Bertrand and Mullainathan 2003; Bertrand, Duflo, and Mullainathan 2004). All continuous variables are winsorized at the 1% and 99% percentiles. Data definitions and measurement details for all variables are reported in the Appendix.

Table 2 presents the summary statistics of all variables used in Model (1). Consistent with prior studies (e.g., Graham, Li, and Qiu 2008), loan contracts on average have 4.6 covenants, including 3.1 general covenants and 1.5 financial covenants. The average loan spread is 191 bps over LIBOR, while the average maturity is 41 months. Furthermore, the average loan size is about 23% of total assets.

Main Results

We present the main results in Table 3. We regress the number of total covenants on *IncreaseEnforce* and loan/firm characteristics that could influence the number of debt covenants in column (1). In column (2), we include additional state-level and country-level macroeconomic conditions that could be correlated with debt covenants to the regression. The results show that the estimated coefficient on *IncreaseEnforce* is significant at the 1% level and is fairly stable across columns (1) and (2), ranging from -0.091 to -0.099. In terms of economic significance, the result in column (2) for the incidence ratio from the Poisson regression suggests that an increase in the enforceability of non-compete provisions leads to a 9.1% decrease in the number of total covenants.

Since the average total number of covenants for treatment firms during the low enforceability period is about 5, the increase in the enforceability decreases about 0.5 covenants.

We next decompose the total covenants into general and financial covenants. General covenants prevent the firm from engaging in certain activities, such as paying dividends or selling core assets, while financial covenants require the borrowing firm to ensure that certain accounting ratios remain above or below pre-specified thresholds. The results in columns (3) and (4) show the estimated effects on general and financial covenants, respectively. The coefficient estimate on *IncreaseEnforce* is negative and statistically significant for both general and financial covenants. In terms of economic significance, the increase in the enforceability of non-competes leads to a decrease of about 10% in the number of general covenants and 8% in the number of financial covenants. This economic magnitude is comparable to the effect of financial restatement on covenant intensity.⁸ Specifically, [Graham, Li, and Qian \(2008\)](#) show that the number of covenants increases from an average of 6.9 in a pre-restatement loan to 7.6 in a post-restatement loan. Therefore, the effect of the enforceability of non-competes on the debt covenants is economically significant. Overall, the results in Table 3 are consistent with the argument that due to career concerns, managers try to avoid covenant violations by minimizing the presence of debt covenants.

To provide further evidence that career concerns incentivize managers to negotiate fewer debt covenants, we next examine whether the effect of the enforceability of non-competes varies across different types of managers and depends on the negotiation power between managers and lenders. Specifically, we first explore whether managers with higher ability (or larger networks) are less affected by the enforcement of non-compete clauses. The intuition is that higher ability managers

⁸ Using the raw value of [Garmaise \(2011\)](#) enforcement index, we find an increase in enforceability index from 0 to 12; the corresponding incidence ratio indicates a decrease of 49%, 54%, and 44% in total covenants, general covenants and financial covenants, respectively.

or managers with larger networks are less replaceable or have more external opportunities, rendering non-compete constraints less binding.

We define *HighAbility*, which equals to one if the manager ability score developed by [Demerjian, Lev, and McVay \(2012\)](#) is above the sample median *before* the debt contract is originated, and zero otherwise. We interact *HighAbility* with *IncreaseEnforce* in Model (1). Panel A of Table 4 shows the corresponding results. The coefficient estimate on *IncreaseEnforce*×*HighAbility* is significantly positive in column (1), which suggests that managers with higher ability have fewer career concerns and thereby have lower incentives to decrease the number of debt covenants. We further decompose debt covenants into general and financial covenants. The results in columns (2) and (3) show that the interaction effect between *HighAbility* and *IncreaseEnforce* is mainly driven by general covenants.

Furthermore, we define *BigNetwork*, which equals to one if the CEO network size from the BoardEx database is above the median *before* the debt contract is originated, and zero otherwise. We include the interaction between *BigNetwork* and *IncreaseEnforce* in the regression. Panel B of Table 4 presents the corresponding results. The coefficient estimate on the interaction term is positive and statistically significant, suggesting that managers with bigger networks have fewer incentives to reduce the presence of debt covenants. Taken together, these findings are in line with the argument that non-compete clauses are less binding for higher ability or better connected managers, and in turn the effect of the enforceability of non-competes on the number of debt covenants is weaker for these managers.

Next, we examine whether the effect of the enforceability of non-competes varies across managers' ability to exert influence over the design of debt contract terms. [Dennis and Sharpe \(2005\)](#) show that firms with more bargaining power have an advantage in influencing the contract

terms in private debt agreements. In particular, when the deal size is large, managers have great power over setting initial debt contract terms (Niesche 2012). If so, we would expect the effect of the enforceability of non-competes on the number of debt covenants to be stronger for managers who have more bargaining power with lenders. We follow Niesche (2012) and use the size of the loan to proxy for managers' bargaining power. The intuition is that the larger the loan, the more important it is for lenders. Specifically, we construct an indicator variable *BigLoan*, which equals to one if the loan amount as the percentage of the total debt of the leader bank is above sample median during the loan initiation year, and zero otherwise. This measure captures how important a borrower is to a leader bank, which translates into greater bargaining power for managers.⁹ We then interact *BigLoan* with *IncreaseEnforce* in the regression. Panel C of Table 4 reports the corresponding results. The coefficient estimates on the interaction term are significantly negative across columns (1) to (3). These results are consistent with the notion that managers with stronger bargaining power are more able to decrease the number of covenants when they face greater career concerns.

Alternative Explanations

As mentioned in Section II, the negative relation between the number of debt covenants and the enforceability of non-competes may also be consistent with alternative channels.. First, managers with greater stronger career concerns opportunistically report better financial outcomes by applying aggressive accounting policies (e.g., Chen, Zhang, and Zhou 2016). If increased non-compete enforceability leads managers to aggressively report inflated earnings, debt holders may

⁹ Measuring for the borrower's bargaining power in empirical research is inherently challenging because it is a classic example for a latent variable. The ideal solution would be to compare the ex ante preferences of both contracting parties (bank, borrower) with the actual outcome of the loan negotiation process. Borrower bargaining power would be present if the contracted loan terms are closer to the borrower's ex ante preferences than to the bank's initial offer. Unfortunately, there are no reliable data on ex ante preferences about lending terms available for research. Therefore, we consider other possible measures of borrower bargaining power.

rely less on the financial information reported by borrowing firms and include fewer covenants (e.g., [Costello and Wittenberg-Moerman 2011](#)), especially accounting-based covenants (“change in accounting policy channel”).

Second, strict enforcement of non-competes impedes entrepreneurial activities ([Stuart and Sorenson 2003](#); [Samila and Sorenson 2011](#)) and incentivizes managers to shy away from firm-specific capital investments such as R&D ([Amir and Lobel 2011](#); [Garmaise 2011](#)). The decline in entrepreneurial activities and firm investment reduces firms’ risk-taking behavior, which may lead to more stable cash flows. Debt holders care about the downside risk and prefer less volatile cash flows. As a consequence, debt holders may agree to fewer covenants in the debt agreements (“change in risk-taking channel”).

Third, strong enforcement of non-competes helps firms protect their human capital and trade secrets, and retain their competitive advantage, which leads them to obtain a return higher than the industry average and puts firms in better financial positions to meet debt obligations. Debt holders in turn may be willing to grant favorable contract terms in the form of fewer covenants and/or lower interest rates to these borrowing firms (“change in firm performance channel”). The change in the firm performance channel is also consistent with a managerial effort story. For example, [Grossman and Hart \(1983\)](#) argue that career concerns provide strong incentives for managers to exert effort. The strict non-compete enforcement increases managers’ career concerns and thereby induces managers to work harder to improve firm performance. As a result, debt holders agree to less restrictive contract terms in the debt agreements.

We find that these alternative channels are unlikely to explain all of our results. First, for the change in accounting policy channel, the cross-sectional results of managers’ ability and network size show that managers’ incentives to lower the number of debt covenants does not have a

significant impact on financial covenants. This result is inconsistent with [Costello and Wittenberg-Moerman \(2011\)](#), who argue that debt holders include fewer accounting-based covenants if borrowers' financial reporting quality deteriorates. Nevertheless, to further rule out this alternative channel, we directly examine whether borrowing firms experiencing a decrease (an increase) in accounting quality are more likely to have fewer (more) debt covenants when the enforcement of non-competes becomes stronger (weaker). Specifically, we create an indicator variable, *ChgAQ*, which equals to one if accounting quality deteriorates (improves) for firm-years with an increase (or decrease) in the enforceability of non-competes, and zero otherwise. We follow [Kothari, Leone, and Wasley \(2005\)](#) and measure accounting quality as performance-adjusted discretionary accruals. Note that an increase (or a decrease) in discretionary accruals from year $t-2$ to year $t-1$ indicates a decrease (or an increase) in accounting quality *before* the debt contract is originated. Accordingly, our design is not affected by a looking-ahead bias because lenders are able to observe the change in accounting quality (*ChgAQ*) following the change in enforcement of non-compete provision but before they negotiate the terms in a debt contract with managers. We then interact *ChgAQ* with *IncreaseEnforce* in the regression. If the change in accounting policy channel is true, the interaction term should be negatively significant. Panel A of Table 5 reports the estimation results. We do not find that the effect of non-compete enforceability on the number of debt covenants is more negative for firms experiencing changes in accounting quality.

Second, we examine the change in the risk-taking channel by testing whether firms experiencing a decrease (an increase) in risk taking are more likely to have fewer (more) debt covenants when non-compete enforcement becomes stronger (weaker). Specifically, we create an indicator variable *ChgRisk*, which is equal to one if cash flow volatility decreases (increases) for firm-years with an increase (or decrease) in the enforceability of non-competes and zero otherwise.

Cash flow volatility is measured as the standard deviation of quarterly cash flows from operations over the previous four fiscal years scaled by total assets. An increase (or a decrease) in cash flow volatility from year $t-2$ to year $t-1$ indicates an increase (or decrease) in the risk taking of a firm *before* the debt contract is initiated. Similarly, lenders are able to observe the change in risk taking of a firm (*ChgRisk*) following the change in enforcement of non-compete provision but before they negotiate the terms in a debt contract with managers. We include the interaction term $IncreaseEnforce \times ChgRisk$ in the regression. If the change in the risk-taking channel is true, the interaction term would be negatively significant. The results are reported in Panel B of Table 5. The coefficient estimates on the interaction term between the change in enforceability and the change in operating risk are not significant across all columns. This result suggests that the change in the risk-taking story does not explain our results.

Finally, we examine the change in the firm performance channel by testing whether firms experiencing an increase (a decrease) in profitability are more likely to have fewer (more) debt covenants when non-compete enforcement becomes stronger (weaker). Similarly, we use an indicator variable *ChgProf*, which is equal to one if the ratio of EBITDA and total assets increases (decreases) for firm-years with an increase (or decrease) in the enforceability of non-competes and zero otherwise. An increase (or a decrease) in profitability from year $t-2$ to year $t-1$ indicates an increase (or a decrease) in the financial performance of a firm *before* the debt contract is originated. Again, lenders are able to observe the change in firm performance (*ChgProf*) following the change in enforcement of non-compete provision but before they negotiate the terms in a debt contract with managers. We include the interaction term $IncreaseEnforce \times ChgProf$ in the regression. If the change in firm performance channel is true, the interaction term would be negatively significant.

We report the results in Panel C of Table 5. We fail to find the evidence to support this alternative channel.

V. ADDITIONAL ANALYSES

Effect of Non-compete Provisions on Other Loan Contract Terms

In this sub-section, we examine the effect of the enforceability of non-competes on other loan contract terms. Column (1) of Table 6 reports the results pertaining to the impact of non-compete enforceability on debt maturity, where we control for variables that could correlate with maturity in the regression. The coefficient on *IncreaseEnforce* is positive and statistically significant, indicating that after an increase of the enforceability, the maturity of loans issued by the firms is approximately 5% (1.8 months) longer. This result is consistent with the conjecture that managers use longer maturity to reduce short-term liquidity risk and alleviate career concerns arising from the enforcement of non-compete provisions.

Column (2) of Table 6 reports the results of the effect of the enforceability of non-competes on the loan spread. The dependent variable *LnCostofBorrow* is measured as the natural logarithm of all-in spread (AIS) drawn, which is the amount the borrower pays in basis points over LIBOR or LIBOR equivalent for each dollar drawn. Interestingly, we find that the effect of non-compete enforcement on the loan spread is positive and statistically significant. In terms of economic significance, a one unit increase in the enforcement of non-compete clauses leads to a 4% increase in the loan spread relative to the average loan spread of treatment firms during the low enforceability period. Lenders ensure that they obtain a certain risk premium from borrowing firms through a package of contract terms including covenants and interest rate. Since covenants play an important role in reducing the agency costs of debt (Jensen and Meckling 1976), lenders would naturally ask for compensation in exchange for fewer debt covenants. For instance, lenders may

demand high interest rates to compensate the risk for their investment.¹⁰ Hence, managers' career concerns due to the enforcement of non-compete clauses lead to a substitution effect between different debt contract terms. Specifically, managers forgo cash flows in the form of interest rates in exchange for fewer restrictive debt covenants to avoid being dismissed from their current employment.

Switching to other loan terms, we do not find evidence that the enforceability of non-competes affects the size of a loan or whether a loan is collateralized. Columns (3) and (4) of Table 6 present the corresponding results. Overall, the results in this sub-section are consistent with the argument that managers with limited outside options have strong incentives to retain their jobs. To achieve so, it is probable that they are willing to pay higher interest costs in order to minimize default risk via fewer covenants and longer maturity.¹¹

Simultaneous Equation Analysis

The results in Section 5.1 show that managers with greater career concerns are willing to pay higher interest costs in order to minimize default risk via fewer covenants and longer maturity. One potential concern is that the associated loan terms may be endogenous. Covenants, loan spread, and maturity, for example, may be simultaneously determined in a debt contract. To verify that such potential simultaneity does not affect our findings, we estimate a system of three equations where the total number of covenants, the loan spread, and maturity are simultaneously determined. We follow prior studies and use syndicate relationship (*LPRel*) and reputable arranger

¹⁰ Note that, however, the increase in interest rate cannot be explained by the alternative channels of borrowers' lower risk or better future performance after an increase in non-compete enforcement.

¹¹ We acknowledge that higher interest rate reduces firms' cash flow and makes it more difficult for managers to achieve earnings targets. Thus, managers trade off between the costs of debt covenant violation and higher interest rate. Whether the cost of debt covenant violation exceeds the cost of higher interest rate is an empirical question. Our results suggest that the cost of higher interest rate is perceived less important by managers compared to the cost of debt covenant violation.

(*HighRepLeader*) to instrument the number of covenants, the average prior rate (*AvgLnCostOfBorrow*) to instrument loan spreads, and asset maturity (*LnAssetMaturity*) to instrument the loan maturity (Bharath et al. 2011; Costello and Wittenberg-Moerman 2011).

Table 7 reports the corresponding results. In the first stage, as expected, we find a positive and significant association between loan spreads and the averaged prior rate (column (1)), a significantly positive association between loan maturity and asset maturity (column (2)), and a significantly negative association between the number of covenants and the syndicate relationship (columns (3)-(5)). In the second stage, we find that the variable *IncreaseEnforce* remains statistically and economically significant in the *LnCostOfBorrow* and *TotalCov* specifications, after controlling for the endogeneity of loan terms (columns (6) and (8)). These results also hold for general and financial covenants separately (untabulated). Nevertheless, the coefficient estimate on *IncreaseEnforce* is no longer statistically significant when loan maturity is the dependent variable.

Robustness Analysis

In this section, we discuss whether our findings are robust to using alternative variable of interest or alternative research designs.

The design of debt covenants not only involves how many covenants to include in a loan (covenant intensity) but also how tightly to set these covenants (covenant tightness). Therefore, in addition to examining the effect of non-compete enforcement on the number of covenants, we also investigate its influence on the tightness of debt covenants in this section. We follow Murfin (2012) and focus on financial covenants to measure covenant tightness as follows. For a given financial covenant, we define *Slack* as the absolute value of the difference between the financial covenant threshold and the actual value of the covenant variable at the end of last fiscal quarter prior to the

loan's origination date, divided by the standard deviation of the corresponding financial variable (estimated over the 20 preceding quarters). We next obtain *Mean Slack (Max Slack)* as the mean (max) value of *Slack* across the different covenants that are included in the contract. The lower the value of *Mean Slack (Max Slack)*, the tighter the covenants are. We then re-estimate equation (1) using the dependent variable of *Mean Slack (Max Slack)*. Panel A of Table 8 reports the results. We show that *Slack* is positively associated with *Increaseinforce* across both two measures, suggesting that covenants become less tight for the borrowers in the states occurring an increase in the enforcement of non-compete provisions. This result is consistent with our finding that the increased non-compete enforceability leads managers to ex ante avoid the violation of debt covenants.

As a complementary analysis, we also examine whether debt covenants are less likely to be violated ex post in response to stricter enforcement of non-competes. Specifically, we use financial covenant violations from [Nini et al. \(2012\)](#) and find that firms from the states with stricter enforcement of non-compete provisions are less likely to violate the covenants (untabulated).¹² This suggests that managers succeed in lowering the risk of debt covenant violation when they are subject to greater career concerns.

Second, in the main analysis, the basic unit of observation is a loan facility since the majority of debt contract terms are determined at the facility level. Debt covenants, however, are usually determined at the package level. To ensure the robustness of our results, we complement the facility-level covenant test in the main analysis with the package level covenant test. Specifically,

¹² Financial covenant violation data from [Nini et al. \(2012\)](#) is only available starting from 1996, during which there is only one exogenous change of non-compete enforcement (in Louisiana). To mitigate the concern on weak testing power of our difference-in-differences estimates, we conduct a cross-sectional test using the *level* of [Gaimaise's \(2011\)](#) enforcement index from 1992-2004 and examine whether firms from the states with stricter enforcement of non-compete provisions are less likely to violate the covenants.

we focus on the largest facility for each loan package and re-run our model (1). We report the results in Panel B of Table 8. We find that the negative relation between the enforceability of managers' non-compete provisions and debt covenants continues to hold across all covenants.

Third, as shown in Table 1, firm-year observations from California represent the largest group in the sample. [Bishara and Thomas \(2015\)](#) show that there are only a small proportion of California firms that use non-compete clauses. To the extent that some managers might not have any non-compete agreements in their employment contracts, the changes in enforcement of non-competes should not have any effect on these managers. However, given the popularity of non-compete agreements, we believe such countering effect is likely minimal. Even if it is commonplace, it should only bias against us finding a significant effect of non-compete enforcement on the design of debt covenants. Nevertheless, to ensure that our results are not sensitive to the inclusion of firms located in California, we remove these firms from the sample. We present the results in Panel C of Table 8. The coefficient estimates on *IncreaseEnforce* stay negative and statistically significant across all covenants. The fact that our results are robust to excluding observations in California mitigates the concern that our findings are driven by the states with a very small percentage of firms that include non-compete clauses in their executive employment contracts.

Fourth, although the difference-in-difference design in [Bertrand and Mullainathan \(2003\)](#) and [Garmaise \(2011\)](#) helps us to exploit the plausibly exogenous changes in the enforceability of non-compete provisions, there are still some limitations. For example, our difference-in-difference effects could be biased if we compare the mean from a very long window after a law change to the mean from a short window before a law change. Relatedly, as it is not able to precisely identify the timing of the effect, it is possible that legislative events may result from firms' lobbying activities or may be anticipated by managers. As such, firms may start to change their debt contract

terms in the years prior to the law change, which violates the central assumption of the difference-in-difference methodology—parallel trend assumption.

To address these concerns, following [Gormley and Matsa \(2011\)](#), we replace the indicator variable *IncreaseEnforce* with two indicator variables for pre-law change period (*Pre1*), exact year of law change (*Pre0*), and two interaction terms of *IncreaseEnforce* with indicators for the years after the law change (*Post1* and *Post2*). We present the corresponding results in Panel D of Table 8. The sample size is smaller because we exclude observations outside of the [-3, +3] years of the law change and focus on the effect of the change in non-compete enforceability in a shorter window. We find insignificant coefficients on *Pre1* across three columns but significant coefficients on both *IncreaseEnforce*×*Post1* and *IncreaseEnforce*×*Post2* in most of cases. Thus, we do not find that firms start to change their debt contract terms in the years prior to the law changes, suggesting that the parallel trend assumption central to the interpretation of our difference-in-differences estimates holds. In addition, our results also suggest that it usually takes one or more years for the changes in non-compete enforceability to affect the number of debt covenants in loan contracts. In sum, this finding suggests that non-compete enforceability exerts a long and gradual effect on debt covenants.

Finally, managerial incentives arising from executive compensation may affect the design of debt contract terms (e.g., [Vasvari 2008](#); [Brockman, Martin and Unlu 2010](#)). We also acknowledge the possibility that some executives may be compensated for agreeing not to compete in the form of a higher severance package ([Schwab and Thomas 2006](#)). However, such arrangement should bias against finding our results of a negative association between non-compete enforceability and debt covenant inclusion. Thus, our results are unlikely to be driven by alternative compensation schemes such as severance packages. Nevertheless, to ensure that the effect of enforceability of

non-compete provisions on debt covenants is not driven by compensation-related incentives, we further control for the different components of executive compensation, including bonuses, options, and other compensation (e.g., severance package). Panel E of Table 8 shows that the negative relation between the enforceability of non-competes and covenants remains robust to the inclusion of compensation-related variables.

VI. CONCLUSION

In this paper, we examine the impact of the exogenous change in the enforceability of non-compete provisions on the design of debt covenants. We find that the increase in enforceability leads to a reduction in debt covenants in newly issued loans. These results are consistent with the argument that managers try to avoid debt covenant violations as a response to their enhanced career concerns. Furthermore, we find that borrowing firms subject to stronger enforcement of non-competes are also charged with higher interest rates in the new loans. These results suggest that lenders demand compensation for fewer covenants in the form of a higher cost of debt. Taken together, managers' career concerns lead to a trade-off between pricing and non-pricing terms in debt contracts.

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Appendix: Variable Definitions

Variable of interest	
<i>IncreaseEnforce</i>	A three-level, {-1, 0, +1}, measure of non-compete enforceability changes.
Loan specific variables	
<i>GenCov</i>	The number of general covenants included in the loan agreement.
<i>FinCov</i>	The number of financial covenants included in the loan agreement.
<i>TotalCov</i>	Total number of general covenants and financial covenants.
<i>LoanSize</i>	Amount borrowed, scaled by total assets.
<i>Maturity</i>	The number of months between the facility's issue date and the loan maturity date.
<i>Collateral</i>	An indicator variable equal to 1 if the loan is backed by collateral, and zero otherwise.
<i>InstitutionalInvestor</i>	An indicator variable is equal to 1 if the loan's type is term loan B, C, or D (institutional term loans), and zero otherwise.
<i>Revolver</i>	An indicator variable is equal to 1 if the loan is a revolver, and zero otherwise.
<i>NumLender</i>	Number of participants in the loan syndicate, including the arranger.
<i>PPindicator</i>	An indicator variable is equal to 1 if the loan contract includes a performance pricing provision, and zero otherwise.
<i>Loan purpose effect</i>	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.
<i>High lender number</i>	An indicator variable is equal to 1 if <i>NumLender</i> is above the industry median, and zero otherwise.
<i>HighRepLeader</i>	An indicator variable taking the value of 1 if the loan is syndicated by one of the top six arrangers, based on the arrangers' market share in the primary loan market in the year prior to entering into a loan contract; If there are multiple arrangers, we take the highest market share across the arrangers involved in the loan transaction.
<i>LPRel</i>	The syndicate-specific reputation of the arranger, measured in terms of the previous arranger-participant relationships. For every syndicate participant, the number of previous relationships between the lead arranger and the participant is deflated by the total number of deals syndicated by the arranger (the estimation is performed over a five-year period preceding the loan's issuance); The syndicate-specific measure is estimated as the averaged relationship measure across all syndicate participants.
<i>CostOfBorrow</i>	The cost of borrow is based on the All-in-Drawn-Spread measure reported by DealScan.
<i>LnCostOfBorrow</i>	The natural log value of <i>CostOfBorrow</i> .
<i>AvgLnCostOfBorrow</i>	The average <i>LnCostOfBorrow</i> for all loans originated in the syndicated loan market (and recorded in DealScan) over the six-month period prior to entering into the loan contract.
<i>Slack</i>	Covenant slack, measured as the absolute value of the difference between the financial covenant threshold and the actual value of the covenant variable at the end of last fiscal quarter prior to the loan's origination date, divided by the standard deviation of the corresponding financial variable (estimated over the 20 preceding quarters)
<i>Mean Slack (Max Slack)</i>	The mean (max) value of <i>Slack</i> across the different covenants that are included in a contract.

Borrower-specific variables

<i>Size</i>	Natural logarithm of total assets, estimated in the year prior to entering into a loan contract.
<i>MB</i>	Market value of equity over book value of equity in the year prior to entering into a loan contract.
<i>LEV</i>	Long-term debt divided by total assets, estimated in the year prior to entering into a loan contract.
<i>Prof</i>	EBIDTA divided by total assets, estimated in the year prior to entering into a loan contract.
<i>CFVol</i>	Standard deviation of quarterly cash flows from operations over previous four fiscal years, scaled by total assets.
<i>Tang</i>	Net PPE divided by total assets, estimated in the year prior to entering into a loan contract.
<i>Distress</i>	An indicator variable equal to 1 if the Z-score in the year prior to entering into a loan contract is smaller than 2.675; Z-score is estimated following Zmijewski (1984) .
<i>AbsAM</i>	Absolute abnormal accruals calculated as the residual of a cross-sectional version of the modified Jones' (1991) model for each (two-digit) SIC industry and year.
<i>CreditSpread</i>	The difference between AAA corporate bond yield and BAA corporate yield for the month prior to the loan issuance.
<i>TermSpread</i>	The difference between the 10-year Treasury yield and the 2-year Treasury yield for the month prior to the loan issuance.
<i>ChgGDP</i>	Percentage change in the state's real gross domestic product for year <i>t</i> .

Other Variables

<i>HighAbility</i>	An indicator variable set to 1 if managerial ability score (Demerjian et al., 2012) of a CEO is greater than the sample median, and zero otherwise.
<i>BigNetwork</i>	An indicator variable set to 1 if the network size of a CEO from <i>BoardEx</i> database is greater than the sample median, and zero otherwise.
<i>BigLoan</i>	An indicator variable set to 1 if the deal amount, as the percentage of the total debt amount of the leader bank during the year, is above sample median, and zero otherwise.
<i>ChgAQ</i>	An indicator variable set to 1 if accounting quality deteriorates (improves) before the debt contract for firm-years with an increase (or decrease) in enforceability of non-competes, and zero otherwise. Accounting quality is measured as performance adjusted discretionary accrual calculated following Kothari et al. (2005) . An increase (or a decrease) in discretionary accrual from year <i>t-2</i> to year <i>t-1</i> indicates a decrease (or an increase) in accounting quality before the debt contract.
<i>ChgRisk</i>	An indicator variable set to 1 if cash flow volatility decreases (increases) before the debt contract for firm-years with an increase (or decrease) in enforceability of non-competes, and zero otherwise. Cash flow volatility is measured as the operating cash flow of the four quarters during the fiscal year. An increase (or a decrease) in cash flow volatility from year <i>t-2</i> to year <i>t-1</i> indicates an increase (or a decrease) in risk taking of a firm before the debt contract.
<i>ChgProf</i>	An indicator variable set to 1 if profitability increases (decreases) before the debt contract for firm-years with an increase (or decrease) in enforceability of non-competes, and zero otherwise. Profitability is measured as EBIDTA divided by total assets. An increase (or a decrease)

	in profitability from year $t-2$ to year $t-1$ indicates an increase (or a decrease) in profitability of a firm before the debt contract.
<i>LnAssetMaturity</i>	Asset maturity, measured as the weighted average of current assets divided by the cost of goods sold, and Net PPE divided by depreciation and amortization; These ratios are weighted by the relative size of current assets and Net PPE (Barclay, Marx, and Smith, 2003).
<i>Pre1</i>	An indicator variable for observations in treatment states (i.e., Florida, Texas, or Louisiana) one year prior to the enforceability change, and zero otherwise.
<i>Pre0</i>	An indicator variable for observations in treatment states (i.e., Florida, Texas, or Louisiana) in the year of the enforceability change, and zero otherwise.
<i>Post1</i>	An indicator variable for observations treatment states (i.e., Florida, Texas, or Louisiana) one year after the enforceability change, and zero otherwise.
<i>Post2</i>	An indicator variable for observations in treatment states (i.e., Florida, Texas, or Louisiana) two or more years after the enforceability change, and zero otherwise.
<i>Bonus</i>	CEO's annual bonus compensation as a proportion of total compensation.
<i>OtherComp</i>	CEO's other annual compensation as a proportion of total compensation.
<i>OpGrnt</i>	Number of options granted to the CEO during the year divided by the firm's total common shares outstanding.
<i>OpEx</i>	Number of exercisable options held by the CEO at year-end divided by the firm's total common shares outstanding.
<i>OpUnex</i>	Number of unexercisable options (excluding annual option grants) held by the CEO at year-end divided by the firm's total common shares outstanding.
<i>Owner</i>	Shares owned by the CEO divided by the firm's total common shares outstanding.

TABLE 1
Non-Compete Enforceability Index by State

State	Enforceability index	# of Obs.	State	Enforceability index	# of Obs.
Alabama	5	139	Missouri	7	322
Alaska	3	12	Montana	2	9
Arizona	3	94	Nebraska	4	103
Arkansas	5	177	Nevada	5	133
California	0	1,836	New Hampshire	2	105
Colorado	2	327	New Jersey	4	628
Connecticut	3	448	New Mexico	2	9
Delaware	6	65	New York	3	1,177
DC	7	41	North Carolina	4	280
Florida 1992-06/1996	7	166	North Dakota	0	313
Florida 07/1996-2004	9	467	Ohio	5	687
Georgia	5	477	Oklahoma	1	117
Hawaii	3	8	Oregon	6	124
Idaho	6	51	Pennsylvania	6	624
Illinois	5	782	Rhode Island	3	53
Indiana	5	267	South Carolina	5	69
Iowa	6	149	South Dakota	5	6
Kansas	6	78	Tennessee	7	259
Kentucky	6	111	Texas 1992-06/1994	5	1,305
Louisiana 1992-06/2001, 07/2003-2004	4	114	Texas 07/1994-2004	3	279
Louisiana 07/2001-06/2003	0	26	Utah	6	59
Maine	4	7	Vermont	5	17
Maryland	5	130	Virginia	3	259
Massachusetts	6	647	Washington	5	185
Michigan	5	331	West Virginia	2	22
Minnesota	5	468	Wisconsin	3	232
Mississippi	4	44	Wyoming	4	0

This table presents the enforceability index and our sample distribution by state. The non-compete enforceability index is obtained from [Garmaise \(2011\)](#). The sample consists of 14,838 observations (3,648 firms) from 1992 through 2004. The states with changes of enforceability index are in bold.

TABLE 2
Summary Statistics

	<i>N</i>	Mean	Std. Dev.	Q1	Median	Q3
<i>TotalCov</i>	14,838	4.577	4.202	0.000	4.000	7.000
<i>GenCov</i>	14,838	3.111	3.102	0.000	3.000	5.000
<i>FinCov</i>	14,838	1.469	1.462	0.000	1.000	3.000
<i>CostOfBorrow</i>	14,838	191.455	129.931	75.000	175.000	275.000
<i>Maturity</i>	14,838	41.000	23.827	20.000	36.000	60.000
<i>LoanSize</i>	14,838	0.230	0.238	0.074	0.155	0.300
<i>Collateral</i>	14,838	0.539	0.498	0.000	1.000	1.000
<i>InstitutionalInvestor</i>	14,838	0.055	0.228	0.000	0.000	0.000
<i>Revolver</i>	14,838	0.607	0.488	0.000	1.000	1.000
<i>NumLender</i>	14,838	6.939	8.518	1.000	4.000	10.000
<i>Mppindicator</i>	14,838	0.443	0.497	0.000	0.000	1.000
<i>Size</i>	14,838	6.061	1.923	4.648	5.970	7.393
<i>MB</i>	14,838	2.989	3.210	1.247	2.059	3.410
<i>LEV</i>	14,838	0.222	0.174	0.069	0.206	0.341
<i>Prof</i>	14,838	0.125	0.103	0.084	0.129	0.177
<i>CFVol</i>	14,838	0.034	0.030	0.016	0.026	0.041
<i>Tang</i>	14,838	0.312	0.220	0.143	0.256	0.436
<i>Distress</i>	14,838	0.435	0.496	0.000	0.000	1.000
<i>AbsAM</i>	14,838	0.078	0.085	0.022	0.050	0.101
<i>CreditSpread</i>	14,838	0.808	0.221	0.640	0.750	0.860
<i>TermSpread</i>	14,838	0.001	0.016	-0.008	0.003	0.011
<i>ChgGDP</i>	14,838	0.056	0.024	0.041	0.054	0.072

This table presents the summary statistics for the total sample of 14,838 facilities. All continuous variables are winsorized at the 1% and 99% percentiles. All variables are defined in the Appendix.

TABLE 3
Enforcement of Non-compete Clauses and Covenants

	(1)	(2)	(3)	(4)
	<i>TotalCov</i>	<i>TotalCov</i>	<i>GenCov</i>	<i>FinCov</i>
<i>IncreaseEnforce</i>	-0.099*** (-2.59)	-0.091*** (-2.66)	-0.104*** (-3.45)	-0.078* (-1.91)
<i>InstitutionalInvestor</i>	0.221*** (10.15)	0.222*** (10.19)	0.213*** (10.16)	0.246*** (9.37)
<i>Revolver</i>	-0.025** (-2.46)	-0.025** (-2.43)	-0.042*** (-3.71)	0.011 (1.06)
<i>LoanSize</i>	0.013 (0.49)	0.014 (0.54)	0.072** (2.33)	-0.114*** (-3.10)
<i>Maturity</i>	-0.000 (-0.17)	-0.000 (-0.11)	0.000 (0.89)	-0.001 (-1.01)
<i>NumLender</i>	0.013*** (10.64)	0.013*** (10.57)	0.012*** (10.77)	0.013*** (8.00)
<i>PPindicator</i>	0.574*** (24.34)	0.574*** (24.59)	0.573*** (24.14)	0.582*** (19.65)
<i>Collateral</i>	0.632*** (23.50)	0.632*** (23.63)	0.698*** (23.23)	0.498*** (17.44)
<i>Size</i>	-0.007 (-0.23)	-0.006 (-0.20)	0.038 (1.09)	-0.107** (-2.56)
<i>MB</i>	0.000 (0.04)	0.000 (0.01)	0.001 (0.25)	-0.001 (-0.35)
<i>LEV</i>	-0.106 (-1.06)	-0.107 (-1.06)	-0.171 (-1.50)	0.042 (0.39)
<i>Prof</i>	0.207 (0.96)	0.206 (0.94)	0.058 (0.24)	0.475** (2.17)
<i>CFVol</i>	-0.905 (-1.36)	-0.900 (-1.36)	-0.668 (-0.92)	-1.321 (-1.41)
<i>Tang</i>	0.035 (0.26)	0.027 (0.21)	0.136 (1.03)	-0.187 (-1.08)
<i>Distress</i>	0.065** (2.35)	0.064** (2.33)	0.077*** (2.62)	0.040 (1.29)
<i>AbsAM</i>	0.071 (0.70)	0.066 (0.64)	0.089 (0.80)	0.001 (0.01)
<i>CreditSpread</i>		0.098 (1.48)	0.112 (1.40)	0.059 (0.94)
<i>TermSpread</i>		-0.446 (-0.93)	-0.221 (-0.41)	-0.963* (-1.89)
<i>ChgGDP</i>		0.871* (1.71)	0.516 (0.92)	1.588*** (2.61)
Loan purpose fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Pseudo R ²	0.488	0.488	0.469	0.400
No. of Obs.	12,953	12,953	12,660	12,150

This table presents the results from the estimation of the Poisson regression model for debt covenants. In columns (1) to (3), we regress the number of total covenants, number of general covenants, and number of financial covenants on the non-compete enforceability change variable and control variables, respectively. All continuous variables are winsorized at the 1% and 99% percentiles. All variables are defined in the Appendix. Intercepts are not reported. The numbers in parentheses are z-statistics, based on the Huber-White sandwich estimate of variances and adjusted for clustering by state. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

TABLE 4
Cross-sectional Tests

Panel A: CEO Ability and Impacts of Non-compete clauses			
	(1)	(2)	(3)
	<i>TotalCov</i>	<i>GenCov</i>	<i>FinCov</i>
<i>IncreaseEnforce</i>	-0.119** (-2.47)	-0.143*** (-3.56)	-0.084 (-1.37)
<i>IncreaseEnforce×HighAbility</i>	0.063* (1.72)	0.085*** (3.24)	0.014 (0.22)
<i>HighAbility</i>	0.008 (0.41)	0.000 (0.01)	0.031 (1.23)
<i>InstitutionalInvestor</i>	0.221*** (9.74)	0.211*** (9.70)	0.245*** (9.16)
<i>Revolver</i>	-0.024** (-2.31)	-0.040*** (-3.49)	0.011 (0.96)
<i>LoanSize</i>	0.022 (0.84)	0.082*** (2.62)	-0.110*** (-3.04)
<i>Maturity</i>	-0.000 (-0.24)	0.000 (0.77)	-0.001 (-1.12)
<i>NumLender</i>	0.012*** (10.56)	0.012*** (10.73)	0.013*** (8.02)
<i>PPindicator</i>	0.576*** (25.49)	0.575*** (25.36)	0.584*** (19.94)
<i>Collateral</i>	0.638*** (23.45)	0.704*** (23.50)	0.503*** (16.81)
<i>Size</i>	-0.007 (-0.24)	0.038 (1.09)	-0.110*** (-2.62)
<i>MB</i>	0.001 (0.22)	0.001 (0.31)	0.000 (0.08)
<i>LEV</i>	-0.113 (-1.14)	-0.181 (-1.61)	0.042 (0.39)
<i>Prof</i>	0.222 (1.02)	0.087 (0.37)	0.471** (2.08)
<i>CFVol</i>	-0.841 (-1.37)	-0.586 (-0.87)	-1.265 (-1.38)
<i>Tang</i>	0.048 (0.32)	0.142 (1.01)	-0.129 (-0.63)
<i>Distress</i>	0.070** (2.55)	0.083*** (2.84)	0.046 (1.49)
<i>AbsAM</i>	0.027 (0.26)	0.051 (0.46)	-0.040 (-0.31)
<i>CreditSpread</i>	0.092 (1.42)	0.100 (1.27)	0.067 (1.07)
<i>TermSpread</i>	-0.379 (-0.77)	-0.170 (-0.30)	-0.871* (-1.71)
<i>ChgGDP</i>	0.844* (1.68)	0.479 (0.87)	1.557** (2.56)
Loan purpose fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Pseudo R ²	0.487	0.468	0.399
No. of Obs.	12,804	12,515	12,014

TABLE 4 (cont'd)

Panel B: CEO Network and Impacts of Non-compete Clauses			
	(1)	(2)	(3)
	<i>TotalCov</i>	<i>GenCov</i>	<i>FinCov</i>
<i>IncreaseEnforce</i>	-0.298** (-2.53)	-0.211* (-1.71)	-0.544*** (-4.42)
<i>IncreaseEnforce×BigNetwork</i>	0.607*** (3.74)	0.724*** (3.68)	0.204 (0.52)
<i>BigNetwork</i>	0.186* (1.84)	0.218** (2.16)	0.093 (0.68)
<i>InstitutionalInvestor</i>	0.302*** (5.83)	0.302*** (5.60)	0.309*** (5.40)
<i>Revolver</i>	0.024 (0.99)	0.017 (0.71)	0.039 (1.32)
<i>LoanSize</i>	0.217 (1.13)	0.233 (1.20)	0.175 (0.80)
<i>Maturity</i>	0.000 (0.02)	-0.001 (-0.50)	0.002 (1.43)
<i>NumLender</i>	0.013*** (4.08)	0.014*** (4.34)	0.012*** (3.41)
<i>PPindicator</i>	0.711*** (9.32)	0.700*** (9.09)	0.743*** (9.24)
<i>Collateral</i>	0.678*** (7.88)	0.706*** (8.09)	0.642*** (6.38)
<i>Size</i>	0.122 (1.35)	0.158 (1.38)	0.024 (0.21)
<i>MB</i>	0.006 (0.66)	0.006 (0.66)	0.003 (0.33)
<i>LEV</i>	-1.038*** (-2.68)	-1.271*** (-2.86)	-0.393 (-1.06)
<i>Prof</i>	-0.146 (-0.21)	-0.222 (-0.30)	0.172 (0.22)
<i>CFVol</i>	-2.466 (-0.76)	-1.999 (-0.56)	-3.666 (-1.21)
<i>Tang</i>	0.469 (0.96)	0.546 (1.12)	0.269 (0.43)
<i>Distress</i>	-0.023 (-0.25)	0.011 (0.10)	-0.094 (-0.76)
<i>AbsAM</i>	0.900** (2.15)	1.021** (2.47)	0.575 (1.03)
<i>CreditSpread</i>	0.117 (0.77)	0.075 (0.45)	0.217 (1.42)
<i>TermSpread</i>	-0.796 (-0.82)	-0.645 (-0.63)	-1.114 (-1.08)
<i>ChgGDP</i>	-0.313 (-0.25)	-0.502 (-0.39)	0.318 (0.24)
Loan purpose fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Pseudo R ²	0.487	0.468	0.399
No. of Obs.	1,691	1,665	1,612

TABLE 4 (cont'd)

Panel C: Loan Size and Impacts of Non-Compete Clauses			
	(1)	(2)	(3)
	<i>TotalCov</i>	<i>GenCov</i>	<i>FinCov</i>
<i>IncreaseEnforce</i>	-0.065** (-2.22)	-0.077*** (-3.12)	-0.055 (-1.34)
<i>IncreaseEnforce×BigLoan</i>	-0.061** (-2.12)	-0.067** (-2.15)	-0.054** (-2.43)
<i>BigLoan</i>	0.019 (1.00)	0.004 (0.20)	0.051* (1.88)
<i>InstitutionalInvestor</i>	0.223*** (10.23)	0.213*** (10.24)	0.248*** (9.40)
<i>Revolver</i>	-0.026** (-2.48)	-0.043*** (-3.68)	0.010 (0.93)
<i>LoanSize</i>	0.003 (0.12)	0.068** (2.08)	-0.137*** (-3.54)
<i>Maturity</i>	-0.000 (-0.07)	0.000 (0.92)	-0.001 (-0.98)
<i>NumLender</i>	0.013*** (10.63)	0.012*** (10.78)	0.013*** (8.12)
<i>PPIndicator</i>	0.574*** (24.59)	0.572*** (24.15)	0.584*** (19.73)
<i>Collateral</i>	0.631*** (23.61)	0.698*** (23.37)	0.494*** (17.00)
<i>Size</i>	-0.008 (-0.24)	0.037 (1.06)	-0.110*** (-2.64)
<i>MB</i>	0.000 (0.04)	0.001 (0.27)	-0.001 (-0.32)
<i>LEV</i>	-0.106 (-1.07)	-0.172 (-1.51)	0.044 (0.41)
<i>Prof</i>	0.205 (0.93)	0.051 (0.21)	0.486** (2.17)
<i>CFVol</i>	-0.904 (-1.37)	-0.674 (-0.93)	-1.304 (-1.40)
<i>Tang</i>	0.037 (0.28)	0.143 (1.08)	-0.173 (-1.01)
<i>Distress</i>	0.064** (2.31)	0.076*** (2.60)	0.039 (1.27)
<i>AbsAM</i>	0.069 (0.69)	0.091 (0.82)	0.009 (0.08)
<i>CreditSpread</i>	0.092 (1.38)	0.110 (1.36)	0.045 (0.70)
<i>TermSpread</i>	-0.434 (-0.91)	-0.221 (-0.41)	-0.920* (-1.78)
<i>ChgGDP</i>	0.892* (1.76)	0.528 (0.95)	1.629*** (2.67)
Loan purpose fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Pseudo R ²	0.488	0.469	0.400
No. of Obs.	12,953	12,660	12,150

This table presents the results from the estimation of the impacts of CEO ability, CEO network size, and loan size on the influence of non-compete clauses on debt covenants. In columns (1) to (3), we regress number of total covenants, number of general covenants and number of financial covenants on non-compete enforceability change variable, high CEO ability indicator (big CEO network size indicator, or big loan size indicator) and their interaction term, and control variables, respectively. In Panel A, a firm-year is classified as of high CEO ability if the CEO ability score is above the sample median. CEO ability score is obtained from [Demerjian et al. \(2012\)](#). In Panel B, a firm-year is classified as of big CEO network size if the CEO network size is above the sample median. In Panel C, a firm-year is classified as a big loan size if the deal amount, as the percentage of the total debt amount of the leader bank during the year, is above the sample median. All continuous variables are winsorized at the 1% and 99% percentiles. All variables are defined in the Appendix. Intercepts are not reported. The numbers in parentheses are *t*-statistics, based on the Huber-White sandwich estimate of variances and adjusted for clustering by state. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

TABLE 5
Alternative Channels

Panel A: Accounting Quality and Impacts of Non-compete Clauses			
	(1)	(2)	(3)
	<i>TotalCov</i>	<i>GenCov</i>	<i>FinCov</i>
<i>IncreaseEnforce</i>	-0.142*** (-2.68)	-0.155*** (-3.34)	-0.063** (-2.28)
<i>IncreaseEnforce</i> × <i>ChgAQ</i>	0.086** (2.27)	0.091* (1.90)	0.084** (2.52)
<i>InstitutionalInvestor</i>	0.215*** (9.82)	0.208*** (9.52)	0.347*** (15.38)
<i>Revolver</i>	-0.024** (-2.36)	-0.039*** (-3.62)	0.015 (1.21)
<i>LoanSize</i>	0.024 (0.82)	0.089** (2.44)	-0.256*** (-6.30)
<i>Maturity</i>	-0.000 (-0.14)	0.000 (0.90)	0.000 (1.01)
<i>NumLender</i>	0.012*** (9.74)	0.012*** (10.04)	0.009*** (7.22)
<i>PPIndicator</i>	0.582*** (23.99)	0.577*** (23.02)	0.664*** (20.57)
<i>Collateral</i>	0.615*** (24.25)	0.681*** (24.46)	0.434*** (17.30)
<i>Size</i>	-0.015 (-0.50)	0.033 (0.88)	-0.141*** (-18.02)
<i>MB</i>	-0.003 (-0.72)	-0.001 (-0.37)	-0.009*** (-2.92)
<i>LEV</i>	-0.115 (-1.02)	-0.191 (-1.43)	0.227*** (4.20)
<i>Prof</i>	0.223 (1.02)	0.083 (0.35)	0.648*** (5.02)
<i>CFVol</i>	-0.802 (-1.23)	-0.727 (-0.91)	-1.867*** (-5.59)
<i>Tang</i>	-0.041 (-0.29)	0.049 (0.35)	-0.165*** (-3.64)
<i>Distress</i>	0.083*** (2.65)	0.098*** (2.96)	0.037* (1.95)
<i>AbsAM</i>	0.141 (1.27)	0.189 (1.57)	-0.095 (-0.79)
<i>CreditSpread</i>	0.059 (0.84)	0.079 (0.94)	0.094 (1.34)
<i>TermSpread</i>	-0.390 (-0.75)	-0.253 (-0.46)	-0.866* (-1.72)
<i>ChgGDP</i>	0.907* (1.71)	0.369 (0.63)	1.716*** (2.93)
Loan purpose fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Pseudo R ²	0.540	0.522	0.306
No. of Obs.	11,669	11,407	13,432

TABLE 5 (Cont'd)

Panel B: Risk Taking and Impacts of Non-compete Clauses			
	(1)	(2)	(3)
	<i>TotalCov</i>	<i>GenCov</i>	<i>FinCov</i>
<i>IncreaseEnforce</i>	-0.125***	-0.126***	-0.138***
	(-4.16)	(-4.54)	(-3.56)
<i>IncreaseEnforce</i> × <i>ChgRisk</i>	0.053	0.035	0.090***
	(1.59)	(0.72)	(6.48)
<i>InstitutionalInvestor</i>	0.220***	0.208***	0.248***
	(10.01)	(9.96)	(9.41)
<i>Revolver</i>	-0.023**	-0.040***	0.013
	(-2.31)	(-3.88)	(1.11)
<i>LoanSize</i>	0.018	0.076**	-0.109***
	(0.67)	(2.26)	(-3.10)
<i>Maturity</i>	-0.000	0.000	-0.001
	(-0.10)	(0.93)	(-1.07)
<i>NumLender</i>	0.013***	0.012***	0.013***
	(10.40)	(10.69)	(7.72)
<i>PPIndicator</i>	0.573***	0.567***	0.591***
	(23.50)	(22.14)	(19.53)
<i>Collateral</i>	0.628***	0.695***	0.493***
	(23.42)	(23.24)	(16.20)
<i>Size</i>	-0.001	0.042	-0.097**
	(-0.02)	(1.13)	(-2.20)
<i>MB</i>	0.001	0.002	-0.001
	(0.18)	(0.39)	(-0.20)
<i>LEV</i>	-0.090	-0.161	0.072
	(-0.94)	(-1.46)	(0.67)
<i>Prof</i>	0.183	0.021	0.486**
	(0.80)	(0.08)	(2.16)
<i>CFVol</i>	-0.904	-0.708	-1.217
	(-1.39)	(-0.95)	(-1.27)
<i>Tang</i>	-0.017	0.078	-0.207
	(-0.12)	(0.57)	(-1.09)
<i>Distress</i>	0.055**	0.069**	0.028
	(1.98)	(2.28)	(0.95)
<i>AbsAM</i>	0.079	0.108	-0.005
	(0.69)	(0.90)	(-0.03)
<i>CreditSpread</i>	0.106	0.130	0.050
	(1.57)	(1.60)	(0.75)
<i>TermSpread</i>	-0.365	-0.120	-0.933*
	(-0.79)	(-0.23)	(-1.89)
<i>ChgGDP</i>	0.768	0.388	1.532**
	(1.44)	(0.67)	(2.28)
Loan purpose fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Pseudo R ²	0.513	0.495	0.430
No. of Obs.	12,312	12,035	11,543

TABLE 5 (Cont'd)

Panel C: Firm Performance and Impacts of Non-compete Clauses			
	(1)	(2)	(3)
	<i>TotalCov</i>	<i>GenCov</i>	<i>FinCov</i>
<i>IncreaseEnforce</i>	-0.121*** (-5.50)	-0.143*** (-6.19)	-0.090*** (-2.63)
<i>IncreaseEnforce</i> × <i>ChgProf</i>	0.034 (1.00)	0.056*** (2.58)	-0.010 (-0.14)
<i>InstitutionalInvestor</i>	0.221*** (10.22)	0.212*** (10.21)	0.245*** (9.27)
<i>Revolver</i>	-0.024** (-2.43)	-0.042*** (-3.75)	0.012 (1.13)
<i>LoanSize</i>	0.014 (0.56)	0.073** (2.32)	-0.115*** (-3.27)
<i>Maturity</i>	0.000 (0.05)	0.000 (1.09)	-0.001 (-0.99)
<i>NumLender</i>	0.013*** (10.51)	0.012*** (10.71)	0.013*** (7.94)
<i>PPIndicator</i>	0.573*** (25.90)	0.571*** (24.94)	0.582*** (20.54)
<i>Collateral</i>	0.630*** (24.49)	0.696*** (23.88)	0.496*** (17.83)
<i>Size</i>	-0.009 (-0.27)	0.037 (1.03)	-0.112*** (-2.58)
<i>MB</i>	0.000 (0.03)	0.001 (0.28)	-0.001 (-0.35)
<i>LEV</i>	-0.082 (-0.80)	-0.144 (-1.24)	0.060 (0.54)
<i>Prof</i>	0.171 (0.76)	0.009 (0.04)	0.471** (2.19)
<i>CFVol</i>	-0.970 (-1.44)	-0.763 (-1.04)	-1.356 (-1.45)
<i>Tang</i>	-0.016 (-0.11)	0.093 (0.67)	-0.232 (-1.30)
<i>Distress</i>	0.061** (2.26)	0.073** (2.54)	0.038 (1.24)
<i>AbsAM</i>	0.061 (0.60)	0.094 (0.85)	-0.021 (-0.17)
<i>CreditSpread</i>	0.112 (1.63)	0.125 (1.53)	0.076 (1.14)
<i>TermSpread</i>	-0.387 (-0.79)	-0.188 (-0.34)	-0.843 (-1.60)
<i>ChgGDP</i>	0.862* (1.70)	0.513 (0.92)	1.558** (2.54)
Loan purpose fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Pseudo R ²	0.492	0.472	0.403
No. of Obs.	12,857	12,566	12,055

This table presents the results from the estimation of the impacts of change in accounting quality, change in risk taking, and change in firm performance on the influence of non-compete clauses on debt covenants. In columns (1) to (3), we regress the number of total covenants, number of general covenants and number of financial covenants on non-compete enforceability change variable, change in accounting quality indicator (change in risk taking indicator, or change in

profitability indicator) and their interaction term, and control variables, respectively. In Panel A, *ChgAQ* is an indicator variable set to 1 if accounting quality deteriorates (improves) before the debt contract for firm-years with an increase (or decrease) in enforceability of non-competes, and zero otherwise. Accounting quality is measured as performance adjusted discretionary accrual calculated following [Kothari et al. \(2005\)](#). An increase (decrease) in discretionary accrual indicates a decrease (increase) in accounting quality. In Panel B, *ChgRisk* is an indicator variable set to 1 if the standard deviation of the operating cash flow of a firm decreases (increases) before the debt contract for firm-years with an increase (a decrease) in the enforceability of non-competes, and zero otherwise. In Panel C, *ChgProf* is an indicator variable set to 1 if the ratio of EBITDA and total assets increases (decreases) before the debt contract for firm-years with an increase (a decrease) in enforceability of non-competes, and zero otherwise. All continuous variables are winsorized at the 1% and 99% percentiles. All variables are defined in the Appendix. Intercepts are not reported. The numbers in parentheses are *t*-statistics, based on the Huber-White sandwich estimate of variances and adjusted for clustering by state. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

TABLE 6
Enforcement of Non-Compete Clauses and Other Contract Terms

	(1) <i>Maturity</i>	(2) <i>LnCostOfBorrow</i>	(3) <i>LoanSize</i>	(4) <i>Collateral</i>
<i>IncreaseEnforce</i>	1.779** (2.20)	0.043*** (2.81)	-0.011 (-1.17)	0.079 (0.30)
<i>InstitutionalInvestor</i>	21.979*** (25.36)	0.206*** (7.75)	0.029*** (3.12)	0.018 (0.15)
<i>Revolver</i>	5.308*** (5.38)	-0.058*** (-5.60)	0.072*** (14.38)	0.074 (1.48)
<i>LoanSize</i>	7.116*** (4.33)	-0.263*** (-9.14)		-0.522*** (-3.61)
<i>Maturity</i>		-0.000 (-0.07)	0.001*** (3.74)	0.004*** (2.77)
<i>TotalCov</i>	0.155 (1.52)	0.015*** (5.34)	0.001 (1.55)	0.303*** (19.23)
<i>NumLender</i>	0.055* (1.84)	-0.004*** (-3.18)	0.004*** (7.40)	-0.007 (-1.44)
<i>PPindicator</i>	4.310*** (6.03)	-0.132*** (-8.19)	0.032*** (5.07)	-0.245** (-2.20)
<i>Collateral</i>	1.499* (1.85)	0.273*** (10.79)	-0.021*** (-2.96)	
<i>Size</i>	1.689** (2.08)	-0.189*** (-15.16)	-0.112*** (-9.13)	-0.562*** (-5.24)
<i>MB</i>	0.140 (1.13)	-0.010*** (-4.03)	0.001 (1.22)	0.020 (0.97)
<i>LEV</i>	-5.117* (-1.86)	0.398*** (4.87)	-0.047 (-1.39)	0.475 (1.23)
<i>Prof</i>	19.600*** (3.95)	-1.123*** (-11.51)	0.114*** (2.80)	-3.219*** (-4.98)
<i>CFVol</i>	9.381 (0.67)	-0.343 (-0.96)	0.190 (1.12)	5.761** (2.30)
<i>Tang</i>	-0.271 (-0.05)	-0.227** (-2.03)	-0.057 (-1.12)	0.150 (0.19)
<i>Distress</i>	0.944 (1.20)	0.150*** (6.74)	-0.029*** (-2.91)	0.404*** (3.42)
<i>AbsAM</i>	-3.182 (-0.77)	0.111 (1.05)	0.058** (2.32)	0.367 (0.94)
<i>CreditSpread</i>	-6.128*** (-3.58)	0.175*** (2.69)	-0.030* (-1.78)	-0.356 (-0.93)
<i>TermSpread</i>	-6.674 (-0.39)	-0.305 (-0.69)	0.065 (0.44)	0.227 (0.11)
<i>ChgGDP</i>	12.270 (0.77)	-0.406 (-1.11)	-0.116 (-1.14)	-2.862 (-1.62)
Loan purpose fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Adj. R ² /Pseudo R ²	0.394	0.800	0.538	0.445
No. of Obs.	14,838	14,838	14,838	6,997

This table presents the results from the estimation of cost of debt, loan size, maturity, and collateral models. In columns (1) to (3), we regress the interest rate, maturity and loan size on non-compete enforceability change variable and control variables. Column (4) is a probit regression estimation of the probability that the lenders require a loan to be secured. All variables are defined in the Appendix. Intercepts are not reported. The numbers in parentheses are *t*-statistics in columns (1) to (3), and *z*-statistics in column (4), based on the Huber-White sandwich estimate of variances and adjusted for clustering by state. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

TABLE 7
Simultaneous Equation Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	<i>LnCostOfBorrow</i>	<i>LnMaturity</i>	<i>TotalCov</i>	<i>GenCov</i>	<i>FinCov</i>	<i>LnCostOfBorrow</i>	<i>LnMaturity</i>	<i>TotalCov</i>
<i>IncreaseEnforce</i>	0.067** (1.99)	0.030 (1.29)	-0.386* (-1.71)	-0.242** (-1.97)	-0.158 (-1.39)	0.078*** (2.82)	0.028 (0.72)	-0.475*** (-2.91)
<i>LnCostOfBorrow</i>							0.086*** (6.07)	1.167*** (19.44)
<i>LnMaturity</i>						0.045*** (6.10)		0.167*** (3.83)
<i>TotalCov</i>						0.034*** (19.61)	0.009*** (3.79)	
<i>AvgLnCostOfBorrow</i>	0.412*** (4.25)					0.394*** (5.80)		
<i>LnAssetMaturity</i>		0.063** (2.10)					0.064*** (3.64)	
<i>HighRepLeader</i>			-0.259** (-2.04)	-0.194** (-2.27)	-0.071 (-1.33)			-0.215*** (-3.32)
<i>LPRel</i>			-0.258 (-0.29)	-0.155 (-0.22)	-0.088 (-0.26)			0.192 (0.30)
<i>InstitutionalInvestor</i>	0.204*** (7.63)	0.657*** (23.64)	1.629*** (9.66)	1.134*** (9.09)	0.516*** (9.68)	0.119*** (6.63)	0.624*** (25.99)	1.285*** (12.23)
<i>Revolver</i>	-0.048*** (-5.62)	0.461*** (21.95)	-0.099* (-1.98)	-0.124*** (-3.60)	0.025 (1.25)	-0.066*** (-6.78)	0.466*** (37.23)	-0.122** (-2.15)
<i>LoanSize</i>	-0.266*** (-6.48)	0.108** (2.49)	-0.538** (-2.42)	-0.407** (-2.41)	-0.131 (-1.29)	-0.252*** (-9.85)	0.136*** (3.83)	-0.248 (-1.64)
<i>NumLender</i>	-0.003* (-1.92)	0.002* (1.79)	0.055*** (7.32)	0.042*** (7.06)	0.013*** (5.79)	-0.005*** (-7.75)	0.001* (1.77)	0.058*** (16.98)
<i>PPIndicator</i>	-0.077*** (-4.22)	0.150*** (7.00)	2.631*** (22.67)	1.804*** (19.70)	0.832*** (19.62)	-0.173*** (-15.35)	0.132*** (8.45)	2.700*** (44.25)
<i>Collateral</i>	0.340*** (12.13)	0.071*** (2.89)	3.538*** (18.66)	2.707*** (18.41)	0.841*** (14.17)	0.217*** (16.37)	0.009 (0.51)	3.131*** (43.52)
<i>Size</i>	-0.211*** (-11.21)	0.051 (1.44)	-0.137 (-0.85)	-0.118 (-0.96)	-0.023 (-0.38)	-0.208*** (-15.78)	0.071*** (3.82)	0.096 (1.23)

<i>MB</i>	-0.017*** (-6.17)	0.007* (1.82)	-0.023 (-0.77)	-0.016 (-0.71)	-0.005 (-0.59)	-0.016*** (-8.48)	0.009*** (3.20)	-0.004 (-0.32)
<i>LEV</i>	0.424*** (4.69)	-0.155 (-1.27)	-0.740 (-1.42)	-0.804** (-2.01)	0.082 (0.34)	0.457*** (9.30)	-0.184*** (-2.69)	-1.211*** (-4.17)
<i>Prof</i>	-1.384*** (-10.22)	0.796*** (2.96)	1.403 (1.28)	0.710 (0.85)	0.681 (1.64)	-1.467*** (-15.68)	0.903*** (6.89)	2.870*** (5.15)
<i>CFVol</i>	0.652 (1.08)	0.218 (0.39)	-1.518 (-0.37)	-0.276 (-0.07)	-1.237 (-0.97)	0.690** (1.96)	0.171 (0.35)	-2.334 (-1.13)
<i>Tang</i>	-0.200 (-1.37)	-0.125 (-0.55)	0.029 (0.02)	-0.001 (-0.00)	0.034 (0.09)	-0.198*** (-2.70)	-0.109 (-1.05)	0.262 (0.61)
<i>Distress</i>	0.134*** (5.33)	0.028 (1.04)	0.500*** (2.75)	0.432*** (3.08)	0.070 (1.17)	0.116*** (8.18)	0.012 (0.63)	0.339*** (4.07)
<i>AbsAM</i>	0.041 (0.36)	0.013 (0.12)	-0.377 (-0.46)	-0.310 (-0.47)	-0.083 (-0.31)	0.053 (0.75)	0.014 (0.14)	-0.422 (-1.02)
<i>CreditSpread</i>	0.179*** (3.06)	-0.132** (-2.09)	0.218 (0.56)	0.080 (0.26)	0.117 (0.78)	0.178*** (4.08)	-0.153** (-2.56)	-0.012 (-0.05)
<i>TermSpread</i>	-0.284 (-0.52)	0.012 (0.02)	-1.207 (-0.46)	0.111 (0.06)	-1.275 (-1.38)	-0.238 (-0.88)	0.049 (0.13)	-0.914 (-0.57)
<i>ChgGDP</i>	-0.147 (-0.34)	-0.187 (-0.42)	0.494 (0.22)	-0.002 (-0.00)	0.651 (0.68)	-0.151 (-0.64)	-0.182 (-0.55)	0.634 (0.45)
Loan purpose fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R ² Pseudo R ²	0.819	0.463	0.755	0.738	0.688	0.521	0.418	0.297
No. of Obs.	9442	9442	9442	9442	9442	9442	9442	9442

This table presents the results from the simultaneous estimation of the interest rate and covenant models. Columns (1) to (4) present the results of the first stage estimation of the interest rate and covenants models, respectively. Columns 5 and 6 present the estimation of the interest rate regression and the total number of covenant regression as a system of two equations. We use 3sls, with the average prior rate (*AvgLnCostOfBorrow*) as our instrument for the interest rate and syndicate relationship (*LPRel*) and reputable arranger (*HighRepLeader*) as our instruments for covenants intensity, and asset maturity (*LnAssetMaturity*) as our instrument for loan maturity. All variables are defined in the Appendix. Intercepts are not reported. The numbers in parentheses are *t*-statistics, based on the Huber-White sandwich estimate of variances and adjusted for clustering by state. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

TABLE 8
Robustness Tests

Panel A: Covenant Slack		
	(1)	(2)
	<i>Mean Slack</i>	<i>Max Slack</i>
<i>IncreaseEnforce</i>	1.192**	2.264**
	(2.19)	(2.61)
<i>InstitutionalInvestor</i>	0.278	0.524
	(1.41)	(1.62)
<i>Revolver</i>	0.096	0.085
	(0.69)	(0.41)
<i>LoanSize</i>	0.224	0.733
	(0.40)	(0.70)
<i>LnMaturity</i>	-0.004	-0.006
	(-0.75)	(-0.59)
<i>NumLender</i>	-0.014	-0.024
	(-1.37)	(-1.33)
<i>PPindicator</i>	-0.061	0.114
	(-0.38)	(0.34)
<i>Collateral</i>	-0.727**	-0.830
	(-2.02)	(-1.20)
<i>Size</i>	-0.259	-0.467
	(-0.86)	(-0.82)
<i>MB</i>	0.032	-0.001
	(0.49)	(-0.01)
<i>LEV</i>	-8.685***	-14.211***
	(-6.51)	(-5.97)
<i>Prof</i>	6.394***	10.312***
	(3.21)	(2.94)
<i>CFVol</i>	-14.436*	-21.287
	(-1.84)	(-1.45)
<i>Tang</i>	3.625	5.738
	(1.24)	(1.09)
<i>Distress</i>	-0.163	-0.324
	(-0.45)	(-0.55)
<i>AbsAM</i>	-0.436	-0.232
	(-0.37)	(-0.10)
<i>CreditSpread</i>	-1.109	-2.005
	(-1.32)	(-1.63)
<i>TermSpread</i>	9.865	9.528
	(1.26)	(0.72)
<i>ChgGDP</i>	1.096	-0.412
	(0.16)	(-0.04)
Loan purpose fixed effects	Yes	Yes
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Pseudo R ²	0.561	0.543
No. of Obs.	7,131	7,131

TABLE 8 (cont'd)

Panel B: Package Level Analysis			
	(1)	(2)	(3)
	<i>TotalCov</i>	<i>GenCov</i>	<i>FinCov</i>
<i>IncreaseEnforce</i>	-0.077** (-2.01)	-0.085** (-2.13)	-0.068* (-1.74)
<i>InstitutionalInvestor</i>	0.273*** (5.45)	0.255*** (5.22)	0.322*** (5.17)
<i>Revolver</i>	0.004 (0.16)	-0.030 (-1.08)	0.066*** (3.25)
<i>LoanSize</i>	0.155*** (4.84)	0.282*** (7.60)	-0.107** (-2.29)
<i>LnMaturity</i>	0.017 (1.03)	0.024 (1.33)	0.008 (0.35)
<i>NumLender</i>	0.015*** (9.94)	0.014*** (9.54)	0.015*** (8.02)
<i>PPindicator</i>	0.667*** (24.60)	0.657*** (24.10)	0.697*** (19.70)
<i>Collateral</i>	0.595*** (19.56)	0.659*** (18.56)	0.462*** (16.86)
<i>Size</i>	-0.010 (-0.33)	0.058 (1.62)	-0.156*** (-4.72)
<i>MB</i>	0.002 (0.42)	0.004 (0.77)	-0.002 (-0.41)
<i>LEV</i>	-0.032 (-0.30)	-0.109 (-0.88)	0.140 (1.44)
<i>Prof</i>	0.079 (0.38)	-0.122 (-0.52)	0.437** (2.29)
<i>CFVol</i>	-1.000 (-1.57)	-0.685 (-0.96)	-1.560* (-1.75)
<i>Tang</i>	0.049 (0.33)	0.135 (0.93)	-0.116 (-0.56)
<i>Distress</i>	0.063** (2.52)	0.085*** (3.23)	0.015 (0.49)
<i>AbsAM</i>	-0.039 (-0.42)	-0.044 (-0.42)	-0.041 (-0.34)
<i>CreditSpread</i>	0.086 (1.41)	0.106 (1.40)	0.041 (0.74)
<i>TermSpread</i>	-0.303 (-0.67)	-0.093 (-0.18)	-0.795* (-1.70)
<i>ChgGDP</i>	1.169** (2.45)	0.943* (1.75)	1.608*** (2.61)
Loan purpose fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Pseudo R ²	0.312	0.278	0.233
No. of Obs.	8,779	8,624	8,233

TABLE 8 (cont'd)

Panel C: Excluding California			
	(1)	(2)	(3)
	<i>TotalCov</i>	<i>GenCov</i>	<i>FinCov</i>
<i>IncreaseEnforce</i>	-0.085**	-0.098***	-0.073
	(-2.22)	(-2.97)	(-1.53)
<i>InstitutionalInvestor</i>	0.224***	0.215***	0.249***
	(8.92)	(8.98)	(8.21)
<i>Revolver</i>	-0.029***	-0.049***	0.012
	(-2.76)	(-4.53)	(0.99)
<i>LoanSize</i>	0.007	0.064*	-0.120***
	(0.25)	(1.88)	(-3.04)
<i>LnMaturity</i>	-0.000	0.000	-0.000
	(-0.03)	(0.65)	(-0.59)
<i>NumLender</i>	0.013***	0.012***	0.013***
	(9.61)	(9.85)	(7.16)
<i>PPindicator</i>	0.584***	0.583***	0.594***
	(24.17)	(23.81)	(18.56)
<i>Collateral</i>	0.635***	0.699***	0.503***
	(21.28)	(20.85)	(15.96)
<i>Size</i>	0.000	0.048	-0.108**
	(0.01)	(1.24)	(-2.26)
<i>MB</i>	0.001	0.002	0.001
	(0.33)	(0.49)	(0.14)
<i>LEV</i>	-0.180*	-0.257**	-0.010
	(-1.89)	(-2.43)	(-0.08)
<i>Prof</i>	-0.000	-0.132	0.255
	(-0.00)	(-0.60)	(1.43)
<i>CFVol</i>	-0.515	-0.350	-0.815
	(-0.81)	(-0.49)	(-0.86)
<i>Tang</i>	-0.046	0.079	-0.287
	(-0.35)	(0.58)	(-1.64)
<i>Distress</i>	0.063**	0.078**	0.037
	(2.04)	(2.35)	(1.04)
<i>AbsAM</i>	0.032	0.042	-0.022
	(0.27)	(0.35)	(-0.15)
<i>CreditSpread</i>	0.119*	0.132	0.080
	(1.66)	(1.53)	(1.17)
<i>TermSpread</i>	-0.663	-0.504	-1.015*
	(-1.35)	(-0.94)	(-1.77)
<i>ChgGDP</i>	0.457	0.193	0.982**
	(1.01)	(0.35)	(2.02)
Loan purpose fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Adj. R ²	0.319	0.282	0.236
No. of Obs.	11,399	11,158	10,685

TABLE 8 (cont'd)

Panel D: Trend Analysis			
	(1)	(2)	(3)
	<i>TotalCov</i>	<i>GenCov</i>	<i>FinCov</i>
<i>Pre1</i>	-0.051	0.020	-0.238
	(-0.47)	(0.47)	(-0.64)
<i>Pre0</i>	-0.029	-0.013	-0.139**
	(-0.67)	(-0.33)	(-2.25)
<i>IncreaseEnforce×Post1</i>	-0.244***	-0.269***	-0.208
	(-3.32)	(-3.04)	(-1.31)
<i>IncreaseEnforce×Post2</i>	-0.188***	-0.167**	-0.306***
	(-3.04)	(-2.19)	(-3.97)
<i>InstitutionalInvestor</i>	0.225***	0.215***	0.251***
	(9.52)	(9.35)	(8.90)
<i>Revolver</i>	-0.018*	-0.037***	0.019*
	(-1.71)	(-2.99)	(1.82)
<i>LoanSize</i>	0.025	0.071*	-0.075*
	(0.83)	(1.93)	(-1.86)
<i>LnMaturity</i>	0.000	0.000	-0.000
	(0.10)	(0.97)	(-0.94)
<i>NumLender</i>	0.013***	0.013***	0.012***
	(9.71)	(9.91)	(7.35)
<i>PPindicator</i>	0.573***	0.571***	0.584***
	(21.72)	(21.73)	(16.96)
<i>Collateral</i>	0.635***	0.703***	0.494***
	(20.72)	(20.38)	(14.95)
<i>Size</i>	-0.005	0.032	-0.090*
	(-0.12)	(0.80)	(-1.80)
<i>MB</i>	0.001	0.001	-0.001
	(0.14)	(0.30)	(-0.14)
<i>LEV</i>	-0.098	-0.162	0.051
	(-0.83)	(-1.21)	(0.39)
<i>Prof</i>	0.165	0.022	0.445
	(0.62)	(0.08)	(1.63)
<i>CFVol</i>	-0.955	-0.906	-0.923
	(-1.32)	(-1.24)	(-0.87)
<i>Tang</i>	0.028	0.143	-0.211
	(0.17)	(0.89)	(-0.98)
<i>Distress</i>	0.059*	0.078**	0.019
	(1.92)	(2.35)	(0.63)
<i>AbsAM</i>	0.066	0.091	0.009
	(0.58)	(0.73)	(0.06)
<i>CreditSpread</i>	0.108	0.117	0.080
	(1.35)	(1.22)	(1.03)
<i>TermSpread</i>	-0.069	0.222	-0.732
	(-0.17)	(0.51)	(-1.33)
<i>ChgGDP</i>	0.645	0.176	1.637**
	(1.10)	(0.29)	(2.22)
Loan purpose fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Adj. R ²	0.556	0.539	0.481
No. of Obs.	11,350	11,072	10,583

TABLE 8 (cont'd)

Panel E: Control for compensation			
	(1)	(2)	(3)
	<i>TotalCov</i>	<i>GenCov</i>	<i>FinCov</i>
<i>IncreaseEnforce</i>	-0.081*	-0.058	-0.158**
	(-1.76)	(-1.10)	(-2.33)
<i>InstitutionalInvestor</i>	0.316***	0.302***	0.364***
	(6.48)	(5.65)	(8.37)
<i>Revolver</i>	0.002	-0.023	0.067***
	(0.14)	(-1.41)	(3.05)
<i>LoanSize</i>	-0.023	0.031	-0.144*
	(-0.38)	(0.54)	(-1.88)
<i>LnMaturity</i>	-0.001**	-0.001	-0.002**
	(-2.26)	(-1.35)	(-2.52)
<i>NumLender</i>	0.012***	0.013***	0.012***
	(8.09)	(8.77)	(5.60)
<i>PPindicator</i>	0.725***	0.708***	0.770***
	(16.48)	(17.62)	(11.66)
<i>Collateral</i>	0.624***	0.694***	0.472***
	(15.35)	(14.95)	(11.09)
<i>Size</i>	-0.146***	-0.122***	-0.205***
	(-3.58)	(-2.76)	(-4.33)
<i>MB</i>	-0.006	-0.007	-0.003
	(-1.21)	(-1.45)	(-0.38)
<i>LEV</i>	0.044	-0.039	0.253
	(0.28)	(-0.25)	(1.26)
<i>Prof</i>	0.131	0.019	0.420
	(0.60)	(0.07)	(1.43)
<i>CFVol</i>	-0.609	-0.074	-1.887
	(-0.57)	(-0.07)	(-1.07)
<i>Tang</i>	-0.181	-0.266	-0.009
	(-0.95)	(-1.38)	(-0.04)
<i>Distress</i>	0.044	0.054	0.019
	(1.06)	(1.22)	(0.39)
<i>AbsAM</i>	0.331**	0.289	0.421*
	(1.97)	(1.63)	(1.90)
<i>CreditSpread</i>	0.051	0.087	-0.041
	(0.39)	(0.60)	(-0.30)
<i>TermSpread</i>	0.025	0.185	-0.304
	(0.04)	(0.28)	(-0.41)
<i>ChgGDP</i>	1.619***	1.387**	2.319***
	(2.70)	(2.06)	(2.87)
<i>Bonus</i>	-0.045	-0.039	-0.063
	(-0.59)	(-0.42)	(-0.66)
<i>OtherComp</i>	0.130	0.192**	-0.031
	(1.20)	(1.99)	(-0.17)
<i>OpGrnt</i>	-0.905	0.611	-5.111*
	(-0.28)	(0.16)	(-1.65)
<i>OpEx</i>	-1.177	-0.645	-2.302
	(-0.55)	(-0.31)	(-0.81)
<i>OpUnex</i>	2.577	5.108	-3.334
	(0.86)	(1.48)	(-1.20)

<i>Owner</i>	0.310 (0.62)	0.151 (0.30)	0.629 (1.12)
Loan purpose fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Adj. R ²	0.333	0.301	0.224
No. of Obs.	5,523	5,489	5,208

This table presents the results from the estimation of the Poisson regression model for debt covenants after controlling for various of compensation components. In Panel A, we regress the covenant slack on non-compete the enforceability change variable and control variables and present the results in columns (1) and (2), respectively. In Panels B-E, we regress the number of total covenants, number of general covenants, and number of financial covenants on non-compete the enforceability change variable and control variables and present the results in columns (1) to (3), respectively. In Panel B, we conduct the package level covenant test by focusing on the largest facility for each loan package. In Panel C, we exclude the observations headquartered in California. In Panel D, we conduct a trend analysis. In Panel E, we include additional controls of various CEO compensation components. All continuous variables are winsorized at the 1% and 99% percentiles. All variables are defined in the Appendix. Intercepts are not reported. The numbers in parentheses are *z*-statistics, based on the Huber-White sandwich estimate of variances and adjusted for clustering by state. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.