# Why does Takeover Vulnerability Matter to Debtholders? [*] [†]

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**Abstract**

Recent work documents that firms that are more vulnerable to takeover have higher borrowing costs. This paper investigates the reasons behind this stylized fact. My results show that firms with few antitakeover defenses face a higher cost of debt because lenders are concerned that takeovers may result in leverage increases. Specifically, I find that takeover vulnerability does not increase loan spreads when the loan deal contains covenants restricting leverage. In order to identify the effect of covenants on spreads, I use two instruments to control for the endogeneity of covenants, which arises from the fact that lenders are more likely to include covenants when lending to riskier firms. My first instrument exploits exogenous supply-side variation in the contracting strictness of the lead-arranger lender, induced by lender-specific factors such as the rate of past defaults suffered by the bank in unrelated loans. My second instrument makes use of the relation between syndicate size and the likelihood that a given loan includes covenants. This instrument exploits exogenous variation in the contribution of the deal to the idiosyncratic risk of the lead bank's loan portfolio. The identifying assumption is that lead banks tend to include covenants in those loans whose risk has a higher correlation with the risk of their existing portfolio, so that they can syndicate a larger share of such loans. Overall, my findings show how debt covenants can successfully resolve agency conflicts between shareholders and debtholders. In the absence of covenants, takeover defenses have opposite effects on the cost of equity and debt capital. Yet this difference disappears when debt deals contain leverage-limiting covenants.

*Keywords:* Loan pricing, covenants, corporate governance, takeover defenses.

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## 1. Introduction

A firm's cost of capital plays a critical role in determining the profitability of its investments. Debt is by far the main source of capital in the United States (Bhojrah and Sengupta 2003), and bank loans constitute well over 50 percent of debt capital even for large, public companies (Houston and James 1996). Thus, factors that influence the cost of bank loans have a first-order effect on firms' investment decisions, whose effects are felt throughout the whole economy.

Shareholder rights are one such factor. A number of recent papers (e.g., Chava, Livdan, and Purnanandam 2009) find a robust positive relation between debt spreads and strong shareholders rights (as measured by a low number of takeover defenses), suggesting that lenders view antitakeover provisions as valuable. This paper contributes to the literature by enhancing our understanding of how takeover defenses create value for debtholders. In particular, I explore the hypothesis that lenders value takeover defenses because they are concerned about takeovers resulting in increases in leverage. Such a concern is consistent with the evidence in Warga and Welch (1993), Ghosh and Jain (2000), and Cremers and Nair (2005), who show that a target firm's leverage tends to increase significantly after a takeover. My analysis builds on the fact that the risk of such leverage increases can be contracted away through the inclusion of covenants in the debt agreement that limit the borrowing firm's leverage.

The empirical results presented here support this hypothesis. Using a panel data set of almost 5,600 bank loans issued to over 1,500 U.S. non-financial, publicly traded firms between 1994 and 2005, I show that, consistent with prior evidence, firms with fewer takeover defenses pay significantly higher loan spreads. Yet this effect disappears when the loan agreement contains at least one of a range of covenants that limit the risk lenders face in the event of a takeover by

restricting increases in the borrower's leverage. In the presence of such leverage-limiting covenants, I find that takeover defenses have an insignificant, negative impact on loan spreads.[1]

The relevance of these results is twofold. First, they shed light on the mechanism through which takeovers increase the riskiness of bank loans. The fact that the positive effect of takeover vulnerability on the cost of debt disappears when the loan agreement contains at least one covenant limiting the borrowing firm's leverage shows that lenders are concerned about takeovers resulting in increases in leverage. Second, my evidence suggests that the improvement in the quality of corporate governance commonly associated with fewer antitakeover provisions does not have a first-order effect in creating value for a firm's debtholders. Debtholders do not seem to reward with significantly lower loan spreads those companies for which the threat of a takeover is a way to minimize agency problems associated with managerial entrenchment, not even when debtholders are protected from the leverage increases often associated with takeovers. This indicates that debtholders are not concerned about an entrenched manager engaging in self-dealing behavior that might lead the firm into bankruptcy, perhaps because bankruptcy is one way an entrenched manager could lose his position (Gilson 1989, 1990).

My empirical analysis faces two possible sources of endogeneity. One concern is that the presence or absence of antitakeover provisions might be correlated with some firm-level risk factor that is observed and priced by lenders but omitted from the analysis, thus causing the estimates to be inconsistent. My finding that the positive effect of takeover vulnerability on the cost of debt disappears when debtholders are protected from leverage increases greatly alleviates this concern, by explicitly showing that this effect is driven by lenders pricing the risk of a leverage-increasing takeover and not by some other omitted risk factor. Further results presented

[1] One reason why not all debt deals include such covenants is that covenants impose restrictions that can be costly for the borrowing firm (or, at least, for its CEO). See, e.g., Chava and Roberts (2008), Nini, Smith, and Sufi (2009).

by Chava, Livdan, and Purnanandam (2009) in a related paper reinforce the claim that the positive effect of takeover vulnerability on loan spreads is not driven by endogeneity. Using a similar sample, Chava et al. study the effect of antitakeover provisions on the cost of debt. They argue that it is unlikely that antitakeover provisions and loan spreads are determined simultaneously, given that most antitakeover provisions were put in place well before the issuance date of the loans in their sample.

A second concern is the endogeneity of covenants. Smith and Warner (1976), among others, view covenants as a mechanism that reduces agency conflicts between debtholders and shareholders (Jensen and Meckling 1976, Myers 1977). Consequently, their theory predicts that covenants should be associated, ceteris paribus, with lower debt spreads. However, the empirical implementation of the ceteris paribus clause is problematic, as it is not possible to observe two versions of a same loan, one with covenants and one without. Furthermore, it is well known that lenders are more likely to include covenants when lending to riskier firms (Malitz 1986). My empirical analysis attempts to control for all firm-specific and loan-specific risk factors for which data is available. Yet it is all but certain that there are some risk factors not observed by the researcher that are observed by the lender, particularly in the case of bank loans because lenders and borrowers often establish a close relationship (Agarwal and Hauswald 2009, Bharath et al. 2009). Arguably, such unobserved risk factors will be correlated both with the decision to include covenants in the loan and with its pricing. This correlation is likely to drive the positive relation between covenants and loan spreads found in standard debt pricing regressions, both in this paper and in previous studies (see, e.g., Bradley and Roberts 2004, Mansi, Maxwell, and Wald 2009), contrary to the theoretical prediction.

I deal with the endogeneity of covenants by instrumenting for the probability that a given loan includes covenants restricting leverage. My two instruments make use of the fact that most bank loans are syndicated, i.e., the loan is jointly funded by several financial institutions (usually, commercial or investment banks). Of all the institutions funding the loan, only a small subgroup, the lead arranger(s), are in charge of drafting the technical details of the loan, including the covenants (Sufi 2007, Ivashina 2009). Consequently, one can expect characteristics of the lead arrangers to have an impact on the covenants included in the loan, but not on its final pricing, which is jointly determined by all the syndicate participants within the context of the competitive U.S. capital market.[2]

In particular, my first instrument exploits plausibly exogenous variation in the contracting style of the lead arranger, which is reflected in the fact that some banks are more likely to include covenants (at the cost of a lower spread) in the loans they manage, after controlling for the riskiness of these loans. The identifying assumption is that, after controlling for risk, the variation in the proportion of loans led by a given bank that include leverage-limiting covenants reflects internal bank contracting policy or *style* that is only related to the spread of the loans through these covenants. Such differences in style might be driven, for instance, by the rate of past (unrelated) defaults suffered by the bank in its loan portfolio. See Murfin (2009) for evidence in this respect.

My second instrument builds on work by Ivashina (2009). In her analysis of the costs of asymmetric information within lending syndicates, Ivashina shows that lead banks tend to syndicate a larger share of those loans that have a higher contribution to the idiosyncratic credit risk of their loan portfolio. Therefore, one can expect lead banks to be more likely to include

[2] My sample is comprised of U.S. public firms, mostly from the S&P 1500, and thus it seems reasonable to assume that these firms have access to a variety of competing sources of capital.

covenants in such loans, thus objectivizing their monitoring and making them more attractive for syndication. Indeed, my analysis reveals a strong positive relation between the lending syndicate size and the presence of leverage-limiting covenants in a loan, suggesting the use of syndicate size as instrument for the probability that a loan contains covenants restricting leverage. In this case, the identifying assumption is that, after controlling for borrowing firm and loan characteristics, the relation between syndicate size and the likelihood that a given loan includes covenants is driven by the contribution of the loan to the idiosyncratic risk of the lead bank's loan portfolio, and thus only affects loan spreads through covenants.

My paper is related to two recent strands of literature: the effects of corporate governance on the cost of debt, and the determination and pricing of debt covenants. Among the papers that examine the effects of corporate governance on debt spreads, two different sets of results emerge. Bhojrah and Sengupta (2003) and Anderson, Mansi, and Reeb (2004) find that firms with a more independent board, a common measure of the quality of *internal* corporate governance, face lower bond yields. In related evidence, Graham, Li, and Qiu (2008) find that firms pay higher loan spreads after financial restatements. On the other hand, Klock, Mansi, and Maxwell (2005), Cremers, Nair, and Wei (2007), and Chava, Livdan, and Purnanandam (2009) all find that firms with fewer antitakeover provisions, usually seen as a measure of the quality of *external* corporate governance, face higher bond and loan spreads. Similarly, Francis et al. (2009) show that bonds issued by firms incorporated in takeover friendly states have higher yield spreads than bonds issued by firms incorporated in states with restrictive antitakeover laws.[3]

[3] An exception is Qui and Yu (2009), who find that firms that are more costly to takeover pay higher bond spreads. They argue that the findings of Klock et al., Cremers et al., and Chava et al. may be driven by the endogeneity of takeover defenses. My findings help alleviate this concern, by explicitly highlighting the channel through which takeover vulnerability affects loan spreads.

How should we interpret this relation between better external corporate governance and higher cost of debt? Prior work argues that the threat of a takeover aligns the incentives of the managers of a company with those of their shareholders, implying that takeover defenses decrease the value of a company to its shareholders (Gompers, Ishii, and Metrick 2003, Bebchuk, Cohen, and Ferrell 2009). However, takeovers also present an inherent risk to debtholders, because they may lead to higher leverage (e.g., Ghosh and Jain 2000), and this risk may dwarf any potential benefits of improved governance from the point of debtholders. My results provide direct evidence of this interpretation, by showing that the association between takeover defenses and lower loan spreads disappears when lenders are contractually protected against increases in leverage through covenants.[4] As hypothesized by Smith and Warner (1976), debt covenants appear to be successful in reducing agency conflicts between shareholders and debtholders. In the absence of covenants, takeover defenses have opposite effects on the cost of equity and debt capital. Yet this difference disappears when debt deals contain leverage-limiting covenants.

This paper is the first to account for the endogeneity of covenants when analyzing the effect of corporate governance on the cost of debt, and this turns out to be crucial to identify the interaction effect of covenants and takeover vulnerability. This endogeneity is well understood in the literature, and several papers have attempted to account for it when simultaneously analyzing the choice and price effect of covenants.[5] These include Bradley and Roberts (2004), Goyal (2005), and Reisel (2009), who follow a Heckman (1979) self-selection approach. My GMM instrumental variables strategy has the advantage of not relying on functional form assumptions

[4] Cremers et al. (2007), Chava et al. (2009), and Francis et al. (2009) already provide some evidence in this respect. However, the focus of these authors' analysis is not on covenants, and therefore they do not account for their endogeneity.

[5] A number of empirical papers analyze the choice of covenants, leaving aside their price effect. These include Malitz (1986), Nash, Netter, and Poulsen (2003), and Billett, King, and Mauer (2007). In a recent paper, Chava, Kumar, and Waga (2009) analyze the relation between bond covenants and managerial agency.

for identification (Angrist and Pischke 2008, p. 191), and makes use of two instruments (based on the lead arranger) that are grounded in the institutional characteristics of the syndicated loan market.

The remainder of the paper is organized as follows. Section 2 develops the hypotheses; section 3 describes the data and variables used in the analysis; the empirical strategy and results are presented in section 4; and section 5 concludes.

## 2. Hypothesis Development

One of the defining characteristics of a public corporation is the separation between ownership, in the hands of the shareholders, and control, which the shareholders delegate to the management team (Fama and Jensen 1983).[6] This separation can give rise to agency problems between the shareholders, who are interested in the maximization of the cash-flows that they receive from the company, and the CEO, who may maximize the private benefits that he extracts from the firm (Jensen and Meckling 1976).[7]

Corporations can use a number of mechanisms in order to minimize managerial agency problems, which fall under the umbrella of what is commonly known as corporate governance. One salient mechanism is the board of directors, which is formally responsible for selecting and monitoring the CEO on behalf of shareholders. Another prominent factor in aligning the incentives of the CEO and the shareholders can be the threat of a takeover. This is particularly important when the board of directors lacks independence to properly monitor the CEO and allows him to run the firm inefficiently for his own private benefit. In this case, a group of external investors can buy a majority stake in the firm at a price that reflects its inefficient

[6] Henceforth I will refer to the management team as the CEO, even though one is usually a strict subset of the other.
[7] There are a variety of reasons why the private benefits of the CEO may not coincide with those of the shareholders: differences in time horizon or risk-aversion (e.g., Haubrick 1994), non-pecuniary benefits (e.g., Yermack 2006), etc.

management, force a change in its board of directors in order to replace the CEO with a more efficient manager,[8] and eventually sell its stake at a higher price reflecting the efficiency gains, thus turning a profit.

The ability of an external raider to successfully and profitably complete this process crucially depends on the target firm not having a number of provisions in its statutes or by-laws that prevent or significantly delay at least one of the key steps of the takeover process. Among the most popular of such provisions, known as takeover defenses, are those that stagger the terms of directors, limit shareholders' ability to meet or act, or dilute a potential raider's stake (Gompers, Ishii, and Metrick 2003). Therefore, the presence of these takeover defenses will determine the extent to which the takeover threat is an effective mechanism to prevent the CEO from self-dealing.

Gompers, Ishii, and Metrick (2003) and Bebchuk, Cohen, and Ferrell (2009), among others, provide evidence that takeover defenses decrease the value of a firm for its shareholders, consistent with the previous argument. However, the effect of takeover defenses on debtholders' value might be different, given the different nature of their claims: debtholders' interest in a firm is capped by the face value of the debt, while shareholders retain all the upside potential of the firm (e.g., Myers 1977). The value of debtholders' claims depends on the risk of the firm, namely, the probability that the firm will go bankrupt and default on these claims and the recovery value of the assets in case of default (e.g., Merton 1974). Therefore, debtholders' assessment of takeover defenses will depend on how these defenses affect their perceived risk of the borrowing firm.

[8] Strictly speaking, it is not necessary to change the CEO. It suffices to ensure that the new board forces him to run the company efficiently.

Several empirical studies have found that a target firm's leverage increases significantly after a takeover (e.g., Cook and Martin 1991, Warga and Welch 1993, Ghosh and Jain 2000, Cremers and Nair 2005), thus increasing its probability of default. Therefore, takeover defenses can be seen as alleviating this *leverage* risk. On the other hand, takeover defenses might entrench the CEO, thus increasing his capacity to inefficiently manage the firm, and potentially increasing the *agency* risk that the firm will eventually go bankrupt. Which effect dominates is an empirical question, and several papers (Klock, Mansi, and Maxwell 2005, Cremers, Nair, and Wei 2007, and Chava, Livdan, and Purnanandam 2009) conclude that the takeover risk dwarfs any potential effect of the agency risk, and thus takeover defenses are associated with lower debt spreads.

There is a fundamental difference between the risk induced by leverage increases associated with takeovers and the agency risk induced by managerial self-dealing. Agency risk is inherently non-contractible, while leverage increases can be restricted in a debt contract. This is commonly done through the inclusion of covenants, which require the borrower to fulfill certain conditions (e.g., maintain leverage below a certain threshold) or which forbid the borrower from undertaking certain actions (e.g., issue new debt). Violations of covenants are usually referred to as "technical defaults." Upon breaching a covenant, control rights shift to the creditor, who can demand immediate repayment of the loan or use this as a threat to extract other concessions from the borrower (Chava and Roberts 2008, Nini, Smith, and Sufi 2009).

Therefore, leverage-limiting covenants offer a unique opportunity to empirically capture the channel through which takeover vulnerability affects the risk perceived by debtholders. If debtholders' main concern in the event of a takeover is an increase in the target's firm leverage, then one would expect that the positive effect of antitakeover provisions on debt spreads

disappears when the loan contains at least one covenant placing restrictions on the leverage of the borrowing form.[9] This leads to the formulation of the following testable hypothesis:

**Hypothesis 1:** In loans that contain at least one leverage-limiting covenant, the degree of takeover vulnerability of the borrowing firm does not have a positive effect on the loan spread.

Assuming that Hypothesis 1 holds and leverage-limiting covenants are effective in offsetting the risk that takeovers imply for debtholders, one can gain further understanding of how debtholders perceive antitakeover provisions by analyzing the effect of these provisions on loan spreads, conditional on the loan containing at least one leverage-limiting covenant. If debtholders value the threat of a takeover as a way to mitigate the agency risk imposed by an entrenched CEO engaging in self-dealing, then one would expect a negative effect of antitakeover provisions on the spreads of loans that contain covenants restricting leverage-increasing takeovers. This leads to the following hypothesis:

**Hypothesis 2:** In loans that contain at least one leverage-limiting covenant, the degree of takeover vulnerability of the borrowing firm has a negative effect on the loan spread.

Rejection of Hypothesis 2 might indicate that debtholders do not view an entrenched manager as a factor that increases the risk that a firm will go bankrupt. This may reflect the fact that bankruptcy often results in lenders forcing the replacement of senior managers (Gilson 1989, 1990), who might even lose their liability insurance (Eitel 2000), and therefore managers have strong incentives to avoid bankruptcy. This is also consistent with the evidence in Bertrand and Mullainathan (2003), who find that entrenched managers tend to prefer enjoying the "quiet life" rather than engaging in risky empire building behavior.

[9] There are several ways of formulating such restrictions: limits on the leverage ratio, tangible net worth, debt to cash flow ratio, etc.

## 3. Data and Variable Definitions

### *3.1 Sample Construction*

Each observation in my sample corresponds to a unique syndicated or sole-lender loan deal to a non-financial U.S. public corporation denominated in U.S. dollars, issued between 1994 and 2005. Panel A in Table 1 shows the time distribution of these loan deals.

My data fall into three main categories: bank loans, including loan spreads, covenants, and borrower ratings; borrowing firm accounting variables; and firm-specific measures of takeover vulnerability. Data on bank loans are obtained from the Dealscan database distributed by the Loan Pricing Corporation.[10] The source of firms' balance sheet information is Standard and Poor's Compustat annual files. Finally, I use data from RiskMetrics to construct firm-level measures of takeover vulnerability.[11]

At the moment, there is no common unique firm identifier in Compustat and Dealscan. In order to match companies in both datasets, I use a matching file constructed by Chava and Roberts (2008).[12] Following Bharath et al. (2009), I use only accounting information that is publicly available at the time a loan deal is closed by requiring that the balance sheet information belongs to a fiscal period that ended at least 90 days before the date of loan activation.

Syndicated loans can be structured in several tranches, also called facilities (different facilities within the same deal might differ, for instance, in size or maturity). The average loan deal in my sample consists of 1.4 facilities with a median of 1. The identity of lenders, syndicate structure, and general contract terms such as covenants are typically determined at the deal level, thus

[10] Chava and Roberts (2004, 2008) provide a detailed discussion of the Dealscan database.
[11] Within the years of my sample, RiskMetrics data is only available for 1993, 1995, 1998, 2000, 2002, and 2004. Gompers, Ishii, and Metrick (2003) point out that this data is very persistent, therefore I follow standard practice and use the previously available data until a new update is available.
[12] I am grateful to Michael Roberts for providing me with the matching file. See Chava and Roberts (2008) for details on the matching procedure.

being identical for all the facilities of a same deal. Given that covenants play a prominent role in my analysis, I use a loan deal as unit of observation. Consequently, for deals with multiple facilities, I follow Ivashina (2009) and look at the characteristics of the largest facility that starts at the loan initiation.

After merging Dealscan, Compustat, and RiskMetrics, I am left with a sample of 5,593 loan deals to 1,523 non-financial U.S. public companies issued between 1994 and 2005 for which all variables used in my analysis are available. This sample is of similar size and characteristics to that used by Chava, Livdan and Purnanandam (2009).[13] The biggest drop in sample size is attributable to the requirement that firms are covered by RiskMetrics, which mainly focuses on S&P 1500 companies. Chava et al. argue that this does not induce a selection bias beyond the fact that the analysis focuses on U.S. public corporations.[14]

*3.2 Variable Definitions*

The dependent variable throughout my loan pricing analysis is the loan spread, for which I follow standard practice and use the All-in Drawn Spread variable from Dealscan. All-in Drawn Spread is measured in basis points and is defined by Dealscan as the total annual cost, including a set of fees and fixed spread, paid over LIBOR[15] for each dollar used under the loan commitment. Given that the spread is highly skewed to the right, I follow Chava et al. and use its logarithm.

My main explanatory variables of interest are a variable capturing each borrowing firm's degree of takeover vulnerability and an indicator variable identifying those loans that contain at

[13] Note, however, that their analysis is at the facility instead of at the deal level.
[14] Dealscan also contains data on loans to private firms, which differ substantially from public corporations. However, the corporate governance problems that are at the heart of my analysis are specific to public companies.
[15] The London Interbank Offered Rate (or LIBOR) is a daily reference rate based on the interest rates at which banks borrow unsecured funds from other banks in the London wholesale money market.

least one leverage-limiting covenant. In my baseline analysis, I construct the takeover vulnerability variable using the Gompers, Ishii, and Metrick's (2003) G index, which measures the number of takeover defenses that a firm has, on a scale from 0 to 24. In order to capture takeover vulnerability, I invert the G index and normalize it so that it lies between 0 and 1 (that is, *Takeover Vulnerability* = (24 – G)/24). In robustness checks, I use two other measures of takeover vulnerability based on the ATI index, introduced by Cremers and Nair (2005), and the E index, introduced by Bebchuk, Cohen, and Ferrell (2009). Panel B in Table 1 details the distribution of the three takeover indices used in the analysis.

The *Covenant* indicator variable is intended to identify those loan deals that contain at least one covenant limiting leverage or new debt issuance of the borrowing firm. In particular, *Covenant* equals 1 if the deal contains a covenant limiting one of the following: leverage ratio, debt to cash flow, senior debt to cash flow, debt to tangible net worth, debt to equity, tangible net worth, or net worth. In addition, I also set *Covenant* equal to 1 if the loan deal contains a debt sweep covenant that requires the borrower to use at least 75% of the proceeds of excess debt issuance to pay down the loan. In robustness analysis, I use an alternative definition of *Covenant* that includes covenants that restrict interest payments: fixed charge coverage, debt service coverage, interest coverage, or cash interest coverage.[16] As shown in Table 1, Panel D, *Covenant* equals 1 for 54 % of the deals in my sample when the baseline definition is used, and 57 % when covenants that restrict interest payments are included.

My two instruments are defined and discussed in Section 4.2 below. The definitions of the other explanatory variables used to control for firm and loan characteristics that may impact the risk of a loan and thus its pricing follow standard practice; they are shown in Table 1, Panel D,

[16] I do not include these covenants in my baseline definition of *Covenant* because their violation might be avoided even if new debt is issued by, for instance, using adjustable interest rates.

along with descriptive statistics.[17] All regressions include Fama-French industry fixed effects, as well as indicator variables with the S&P credit rating of the borrowing firm at deal close. The distribution of these ratings is detailed in Table 1, Panel C. I also include year dummies in all my regressions to capture macroeconomic risk.

## 4. Empirical Strategy and Results

### *4.1 Empirical Strategy*

The aim of this paper is to analyze how the effect of takeover vulnerability on loan spreads varies depending on whether the loan deal contains at least one covenant restricting the leverage of the borrowing firm. I therefore estimate the following model:

$$\begin{aligned} Loan\ Spread_{it} = {} & \alpha_t + \beta\ Takeover\ Vuln._{it} + \gamma\ Covenant_{it} + \delta\ Takeover\ Vuln._{it} \times Covenant_{it} + \\ & + \varphi\ Firm\ Characteristics_{it} + \theta\ Loan\ Characteristics_{it} + \varepsilon_{it} \end{aligned} \quad (1)$$

The main coefficients of interest are $\beta$, which captures the base effect of takeover vulnerability on loan spreads when the deal contains no leverage-limiting covenants, and $\delta$, which captures the differential effect of takeover vulnerability on spreads for deals containing at least one leverage-limiting covenant. In terms of this model, my Hypothesis 1 of no positive effect of takeover vulnerability on loan spreads for deals with leverage-limiting covenants can be expressed as $\beta + \delta \leq 0$, while Hypothesis 2 of that effect being negative implies that $\beta + \delta < 0$.

A direct estimation of equation (1) is bound to run into endogeneity problems. As argued previously, both a firm's takeover vulnerability and the presence of leverage-limiting covenants are potentially endogenous. Chava, Livdan, and Purnanandam (2009), using a sample similar to mine, analyze the endogeneity of the takeover vulnerability variable and conclude that it is not

[17] I do not include facility size as a regressor in my analyses because it is highly correlated with firm size. Results are similar if it is included.

likely to be a source of bias. They claim that the fact that antitakeover provisions in their sample were typically adopted well before the date of loan origination minimizes the concern that antitakeover provisions and loan spreads might be determined simultaneously. The interaction results presented below further alleviate this concern. Indeed, the fact that the positive effect of takeover vulnerability on loan spreads disappears when the loan deal contains at least one leverage-limiting covenant makes it harder to argue that such effect is induced by some omitted variable correlated with takeover vulnerability. Rather, this suggests that this positive effect reflects lenders' concern about takeovers resulting in leverage increases.

The endogeneity of covenants, on the other hand, poses a serious problem. Given two identical loan contracts to the same borrowing firm, one with covenants (leverage-limiting or of any other type) and the other without, it must be the case that the contract with covenants has a lower spread than the contract without covenants.[18] By addressing the agency conflicts between debtholders and shareholders (see, e.g., Smith and Warner 1976), covenants decrease the risk faced by lenders, who are thus willing to accept a lower spread in exchange for covenants. Conversely, covenants are costly for borrowers because they limit their actions (see, e.g., Chava and Roberts 2008), and thus borrowers will only agree to their inclusion in the loan contract if, in exchange, they receive a lower spread.[19] Unfortunately, in reality we can never observe two versions of the same contract, one with covenants and the other without, nor can we assume covenants to be randomly assigned across contracts. Rather, the empirical evidence shows that lenders tend to include more covenants when lending to riskier firms (see, e.g., Malitz 1986).

[18] The spread will be strictly lower except for those cases in which covenants might be redundant and thus immaterial.

[19] There are other terms of the loan that could be negotiated in exchange for covenants, but in this argument I am comparing two identical contracts except for their covenants and, potentially, their spreads.

As mentioned earlier, even though I attempt to control for all firm-specific and deal-specific risk factors for which data is available, it is very likely that some firm-specific risk factors omitted from the analysis are actually observed by lenders, particularly in the case of bank loans, in which close relationships between lenders and borrowers are common (Agarwal and Hauswald 2009, Bharath et al. 2009). Such omitted risk factors will be correlated both with the decision to include covenants in the loan deal and with the spread charged on the loan. This correlation likely drives the positive association between covenants and loan spreads found in standard debt pricing regressions, both in this paper (Table 3, Equation 1) and in previous studies (see, e.g., Bradley and Roberts 2004, Mansi, Maxwell, and Wald 2009).

To address this endogeneity problem, I instrument for the probability that a loan deal contains leverage-limiting covenants with two instruments: the risk-adjusted proportion of deals led by the lead arranger that include covenants restricting leverage[20] and the number of lenders in the syndicate (in log). The first-stage equation is the following linear probability model:[21]

$$\begin{aligned} Covenant_{it} = \alpha_t + \beta\ Proportion\ Deals\ w.\ Covenants_{it} + \gamma\ \log(Number\ Lenders)_{it} + \\ +\varphi\ Firm\ Characteristics_{it} + \phi\ Loan\ Characteristics_{it} + \varepsilon_{it} \end{aligned} \tag{2}$$

In the second stage, I estimate equation (1) by GMM using the *Proportion Deals w. Covenants* and the log(*Number Lenders*) as excluded instruments, as well as the fitted probability of the first stage interacted with the *Takeover Vulnerability* variable as instrument for the interaction term $Covenant \times Takeover\ \ Vulnerability$. Results are almost indistinguishable if the second-stage estimation is done by two-stage least squares or limited information maximum likelihood. I report heteroskedasticity-consistent standard errors clustered at the firm level for all my first and second-stage regressions.

[20] See section 4.2 for a precise definition.
[21] Here *Firm Characteristics* include also *Takeover Vulnerability*.

I use a linear probability model in the first stage, instead of a non-linear binary choice model (e.g., probit or logit), to avoid identification in the second stage arising from the first-stage functional form assumption, which I cannot test.[22] Also, note that the first-stage model does not need to be correctly specified for the estimates of the second stage to be consistent, given that the consistency of the second stage does not depend on the correct specification of the first (Angrist and Pischke 2008, p. 190).

*4.2 Instruments Justification*

I use two different instruments to account for the endogeneity of the presence of covenants in a loan deal. Both instruments are based on the fact that most bank loans are syndicated, that is, they are jointly funded by several financial institutions (the median syndicate in my sample consists of 8 different lenders, with a mean of 10.1). Yet only a small subgroup of the syndicate members, the lead arranger(s), are responsible for drafting the technical details of the loan, such as covenants (Sufi 2007, Ivashina 2009).[23] Thus, we can expect characteristics of these lead arrangers to influence the covenants included in a loan deal. The generic exclusion assumption is that, conditional on the other covariates, the lead arrangers will have no impact on the loan spread other than through covenants. The exclusion restriction is likely satisfied because the spread is jointly determined by all the members of the syndicate operating in the context of the highly competitive U.S. market for capital, rather than by the lead arranger.[24]

[22] Reisel (2009) follows a different strategy and models the choice and pricing of debt covenants using a Heckman-style (1979) self-selection model. However, the consistency of the self-selection model does depend on the normality assumption, and it has been shown that its parameter estimates are surprisingly sensitive to this assumption (Greene 2003, p. 789).
[23] The median loan in my sample has 1 lead arranger, and the mean is 1.39.
[24] Note that my sample is comprised of public companies, which have the option to issue equity or resort to other banks or the bond market for financing.

My first instrument is based on the premise that different lead banks may have different contracting styles, with some of them being more likely to include covenants in loan deals at the expense of negotiating a lower spread. This supply-side variation in contract strictness is supported by the evidence presented by Murfin (2009), who shows that banks write tighter contracts than their peers after suffering defaults to their own loan portfolios, even when defaulting borrowers are in different industries and geographic regions than the current borrower. In order to empirically capture the contracting style of each lead arranger bank, I use the proportion of deals that include covenants restricting leverage out of all loan deals led by a given bank.[25] Figure 1a shows how this proportion varies across the 117 lead banks in my sample.

A potential concern with this identification strategy is that variation in the proportion of deals with covenants might reflect the fact that some banks specialize in lending to riskier firms. In such case, this proportion would be correlated with the loan spreads, rendering it invalid as an instrument. In order to account for this, I regress the proportion of deals with covenants for the 117 banks in my sample on the average risk of the loans led by each of the banks, as captured by the S&P credit ratings of their borrowing firms. For every deal in my sample, I define my first instrument as the residual of this regression for the lead arranger of that deal.[26] Figure 1b shows the distribution of these residuals across the lead arranger banks in my sample; the residuals can be interpreted as the contracting style of each lead bank. Note that the figure reflects wide variation in the contracting styles of the lead banks, with an important mass of banks that seem more likely to include covenants in the deals they lead than one would expect according to the riskiness of these loans.

[25] I group all banks that lead less than 4 deals in my sample as "Other", since I do not have enough observations to capture their contracting style.

[26] When the loan has more than one lead arranger, I define the instrument as the average of the residuals of the different lead arrangers.

My second instrument is based on an instrument developed by Ivashina (2009) to study the costs of asymmetric information within lending syndicates. This instrument is built on the notion that lead banks are not fully diversified and so are exposed to idiosyncratic credit risk. Ivashina shows that lead banks tend to syndicate a larger share of those loans that have a higher contribution to the credit risk of their loan portfolio, e.g., when the borrowing firm belongs to an industry group whose default probability is highly correlated with that of other loans in the loan portfolio. In order to facilitate the syndication of such loans, we can expect lead banks to be more likely to include covenants in them, thus objectivizing and standardizing their monitoring and reducing future moral hazard problems within the syndicate. This suggests a positive relation between the probability of a loan deal containing covenants and the size of the lending syndicate, which would allow me to use syndicate size as instrument for the probability that a loan deal contains covenants restricting leverage.

The identifying assumption in this case is that, after controlling for borrowing firm and deal characteristics, the relation between syndicate size and the likelihood that a given loan includes covenants is driven by the contribution of the loan to the idiosyncratic risk of the lead bank portfolio, and thus only affects loan spreads through covenants. The empirical results presented below support this assumption, alleviating concerns that syndicate size is correlated with the generic risk of the borrowing firm.[27]

Equation 1 in Table 2 shows the results of estimating the first-stage equation (2). Both instruments have a positive and highly significant effect in this first stage, with *t*-statistics over 5 and an *F*-statistic of their joint significance over 27. In view of this, the weakness of my

[27] Note that it is not obvious that there should be any relation between the size of the syndicate and the generic risk of the borrowing firm. On one hand, a larger syndicate allows better diversification of risky loans, but it also makes more difficult their monitoring and exacerbates asymmetric information and moral hazard problems within the syndicate.

instruments should not be a concern (see, e.g., Stock, Wright, and Yogo 2002). The remaining estimates (including the S&P credit rating indicators, not shown for brevity) are broadly consistent with the notion that lenders tend to include covenants when lending to riskier firms.[28]

Equations 2 to 4 in Table 2 show reduced form estimates of the effect of my instruments on loan spreads. The results indicate that both my instruments have a very significant negative effect on loan spreads, which is consistent with my identifying assumption that the instruments affect loan spreads because they correlate with the likelihood that the loan deals include leverage-limiting covenants. These reduced form results are reassuring, given that I expect to find a negative relation between covenants and loan spreads in the second stage. As Angrist and Krueger (2001) note, if one cannot see the causal relation of interest in the reduced form, it is probably not there.

*4.3 Main Results*

Table 3 contains the main results of the paper. In Equation 2, I investigate the effect of takeover vulnerability on loan spreads, without controlling for covenants. Consistent with Chava, Livdan, and Purnanandam (2009), I find that takeover vulnerability has a positive effect on spreads, which is both economically and statistically significant. A one standard deviation increase in the degree of a firm's takeover vulnerability (equivalent to removing 2.6 takeover provisions) is associated with a 2.3% increase in its loan spreads.

My aim is to understand the reasons behind this positive impact of takeover vulnerability on loan spreads. To this effect, Equations 3 to 6 report the results of estimating regression (1) above, where the effect of takeover vulnerability is allowed to vary depending on whether the loan deal

[28] The positive effect of market-to-book on the probability of including leverage-limiting covenants in the loan deal suggests that these covenants are less costly for growth firms, which are more likely to use equity to finance themselves in the future.

contains leverage-limiting covenants. Equation 3 shows the results of estimating regression (1) by OLS, without accounting for the endogeneity of covenants. This yields a non-significant positive effect of covenants on loan spreads, contrary to the theoretical prediction. The interaction effect of takeover vulnerability and covenants is also poorly estimated.

In Equations 4 to 6, I account for the endogeneity of covenants by estimating regression (1) by GMM. My baseline results are presented in Equation 4, in which I use the full set of excluded instruments. The effect of takeover vulnerability on deals without leverage-limiting covenants is now estimated to be more than twice as large as the average effect estimated in Equation 2: a one standard deviation increase in takeover vulnerability is associated with a 6.2% increase in the spread of loans without covenants. However, this positive effect of takeover vulnerability on loan spreads disappears when the deal contains covenants that protect the lenders against leverage increases: in such case, the point estimate of the effect of takeover vulnerability on spreads is a non-significant -0.14.[29]

Given that I have one more instrument than endogenous variables, in Equation 4 I can perform a Hansen test of overidentifying restrictions. The results of the test do not reject the validity of my instruments (*p*-value 0.80). In addition, the results presented in Equations 5 and 6 provide related support for the validity of my instruments. Equation 5 is similar to Equation 4 but with syndicate size included also as regressor, thus leaving the risk-adjusted proportion of deals with covenants as the only source of GMM identification. Reassuringly, syndicate size is highly insignificant (*p*-value 0.80), with the other estimates basically unchanged. Analogously, in Equation 6 I leave syndicate size to drive the identification and introduce the risk-adjusted proportion of deals with covenants as regressor; this is also highly insignificant (*p*-value 0.81)

[29] This results from adding up the coefficients of *Takeover Vulnerability* (0.54) and *Takeover Vulnerability x Covenant* (-0.68) estimated in Equation 4.

and its inclusion has no effect on the remaining estimates. These results further validate my instruments by providing evidence that the instruments relate to loan spreads only through covenants, as indicated by the fact that their strong reduced form effect captured in Table 2 becomes insignificant when covenants are included in the analysis.

The coefficient estimates of the other covariates included in the analysis remain largely unchanged across all specifications reported in Tables 3 and 4, and they are consistent with the notion that riskier firms pay higher loan spreads. Also, note that my instrumental variables approach is successful in capturing a negative effect of covenants on loan spreads, as predicted by the theory.

The empirical results presented in Table 3 support Hypothesis 1: I find that takeover vulnerability has no positive effect on loan spreads when the loan deal contains leverage-limiting covenants. This is consistent with the interpretation that lenders perceive takeover vulnerability as a risk because they are concerned about takeovers resulting in leverage increases. The reason why not all loan contracts contain covenants is that covenants can be costly for borrowers (e.g., Chava and Roberts 2008), and thus some borrowers may prefer to pay a higher spread to compensate lenders for their increased risk rather than have them included in their loan deal.

The results in Table 3 do not provide much support for Hypothesis 2. Not even when lenders are protected against the increases in leverage associated with takeovers do they offer a significantly lower spread to borrowing firms with fewer takeover defenses. This is consistent with the notion that debtholders, unlike shareholders, do not perceive takeover vulnerability as increasing their valuation of the firm. One potential explanation is that, while an entrenched CEO can divert resources from the firm for his own benefit, thus destroying shareholder value, it is never in his interest to divert so much as to cause the firm to go bankrupt, which is the ultimate

concern of debtholders, given that CEOs often lose their position in case of bankruptcy (Gilson 1989, 1990).

*4.4 Robustness Checks*

In Table 4, I analyze the sensitivity of my results to alternative definitions of the takeover vulnerability measure and of leverage-limiting covenants. For each specification, Panel A shows the first-stage estimates of regression (2), while Panel B contains the GMM second-stage estimates with my full set of instruments as well as the results of estimating the second-stage by OLS. In the first robustness check, I construct the takeover vulnerability measure using Cremers and Nair's (2005) ATI index, which is based on only three types of takeover defenses: the presence of a staggered board, of a preferred blank check ("poison pill"), and of restrictions on shareholder voting to call special meetings or act through written consent. In the second specification, I use Bebchuk, Cohen, and Ferrell's (2009) E index, which is based on six takeover defenses: staggered board, limitation on amending bylaws, limitation on amending the charter, supermajority requirement to approve a merger, golden parachute, and poison pill. The results obtained with these two alternative indices are consistent with the baseline results obtained with the Gompers, Ishii, and Metrick's (2003) G index. When using the ATI index, a one standard deviation increase in takeover vulnerability is associated with a significant 4.5% increase in loan spreads when the loan deal contains no covenants restricting leverage; when such covenants are present, takeover vulnerability has an insignificant negative effect on spreads. If the E index is used instead, I find that a one standard deviation increase in takeover vulnerability is associated with a significant 3.7% increase in loan spreads when the loan deal contains no leverage-limiting covenants; this effect becomes negative but insignificant in the presence of such covenants.

In the third specification, I return to the G index to measure takeover vulnerability, but use a definition of the *Covenant* variable that includes covenants that restrict interest payments as leverage-limiting covenants. With this new specification, I obtain very similar results to those obtained in Table 3 with my baseline definition of *Covenant*. Also, note that the first-stage results and the Hansen test of overidentifying restrictions support the strength and validity of my instruments in all three specifications, respectively.

## 5. Conclusions

In this paper, I investigate the reasons behind the positive effect of takeover vulnerability on loan spreads documented by a number of previous studies (most recently Chava, Livdan, and Purnanandam 2009). My main contribution is to empirically show that this effect reflects lenders' concerns that takeovers could result in increases in leverage. My findings indicate that takeover vulnerability has no positive effect on loan spreads when lenders are protected from leverage increases through covenants.

My results build on the notion that the risk of leverage-increasing takeovers can be averted by including covenants in the loan deal that restrict leverage increases. To my knowledge, mine is the first paper to account for the endogeneity of such covenants when analyzing the effect of takeover vulnerability on the cost of debt, and this control for endogeneity turns out to be crucial to identifying the causes behind this effect. My analysis is based on two instruments that exploit exogenous variation in the contracting style of the lead-arranger lender and in the idiosyncratic risk induced by the deal on the lead lender's loan portfolio. These instruments might prove useful to future researchers analyzing the choice and pricing of debt covenants.

My findings shed light on the channel through which takeovers increase the risk of bank loans, thus alleviating concerns that the effect of takeover vulnerability on spreads might be

driven by an omitted variable correlated with takeover defenses. They also show how debt covenants can successfully resolve agency conflicts between shareholders and debtholders: the difference in the direction of the effect that shareholder rights have on the cost of equity[30] and debt capital disappears in the presence of covenants. In addition, by analyzing the effect of takeover defenses on loan spreads when lenders are protected from leverage-increasing takeovers via covenants, I can study whether debtholders value takeover vulnerability as a governance mechanism aimed at alleviating agency problems induced by managerial entrenchment, in the same way shareholders do. I do not find significant evidence that debtholders are concerned about managerial entrenchment, unlike shareholders, reflecting the different nature of the claims that debtholders and shareholders hold on the cash flows generated by the firm.

[30] See, e.g., Gompers, Ishii, and Metrick (2003), Bebchuk, Cohen, and Ferrell (2009).

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**Figure 1a.**

Figures 1a and 1b illustrate the source of identification underlying one of my two instruments for the presence of covenants in loan deals: variation in the proportion of deals that contain leverage-limiting covenants out of all loans led by a given bank, presumably due to differences in the contracting style of the banks. The unit of observation is a bank or lending institution that acts as lead arranger (or co-lead arranger) for at least four of the 5,593 loan deals in my sample. This leaves me with 117 different lending institutions, including "Other", which groups all banks that lead less than four deals in my sample (I group these banks because I do not have enough observations to capture their contracting style). Figure 1a shows the Epanechnikov kernel density of the proportion, with the width of the density window around each point set equal to 0.05.

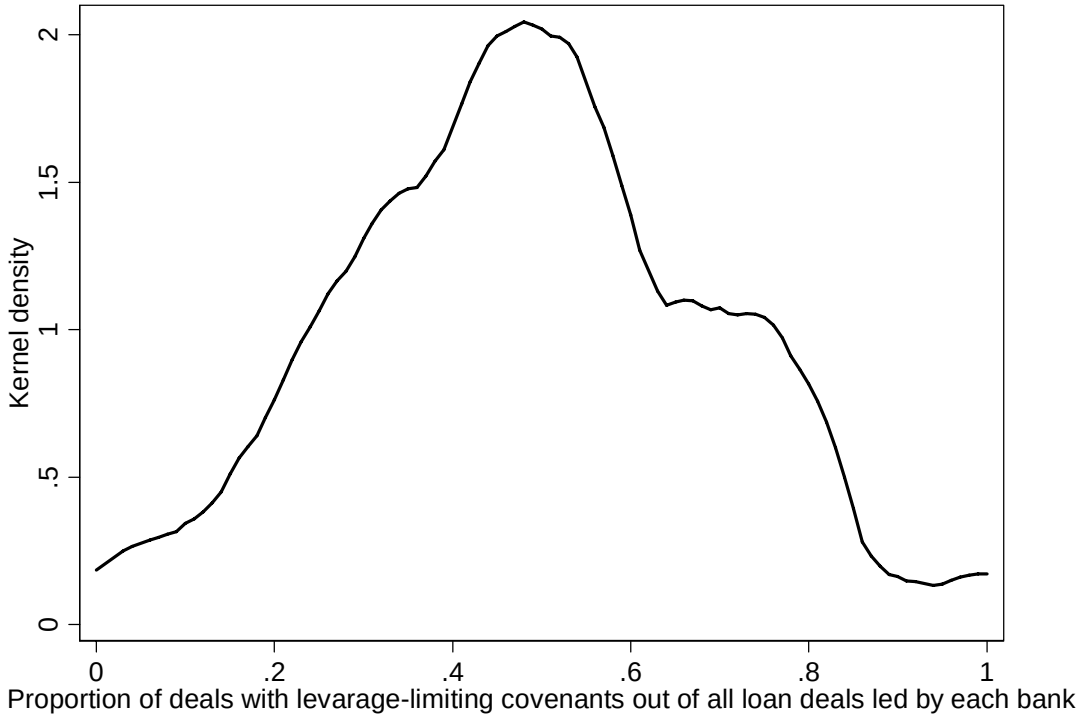
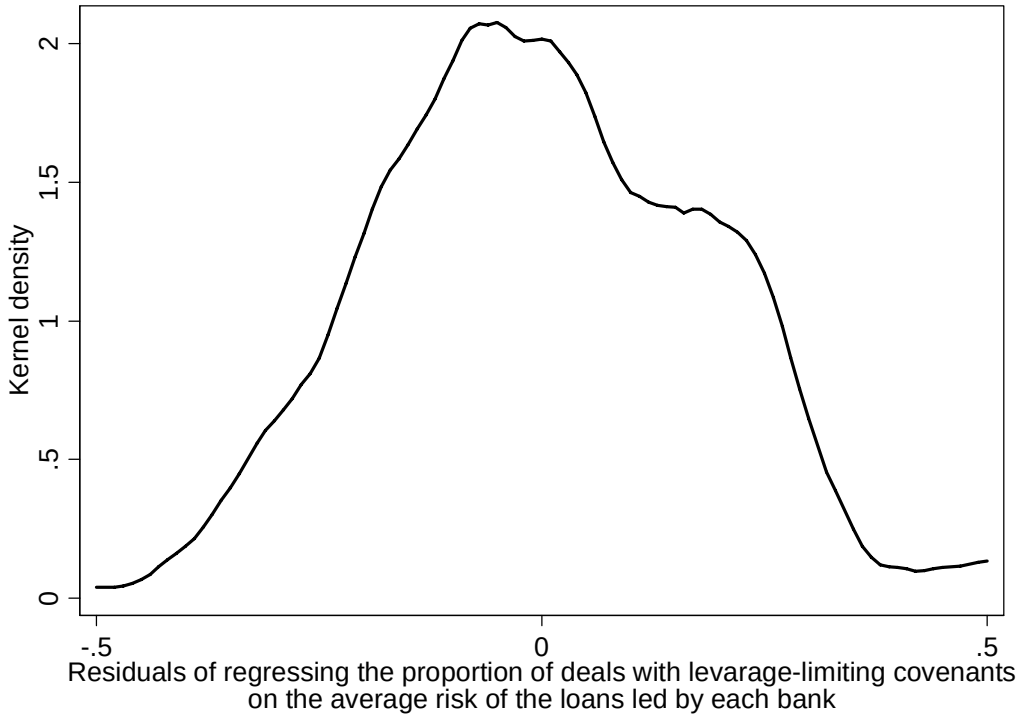


**Figure 1b.**

In this figure, the proportion of deals that contain leverage-limiting covenants out of all loans led by a given bank has been risk-adjusted, by regressing it on a set of indicator variables capturing the average risk of the loans led by each bank. In particular, I use 7 indicator variables (and no constant), one for each of the following rating classes: AAA or AA; A; BBB; BB; B; CCC to D; Not Rated. For each bank, the indicators capture the share of loan deals led by the bank that are issued to borrowing firms with credit ratings in each of the previous classes. These indicators are jointly highly significant (*F* statistic is 135.99), and the regression estimates are consistent with the notion that banks lending to riskier firms are morel likely to include covenants in the loan deals. The graph shows the Epanechnikov kernel density of the residuals of such regression, with the width of the density window around each point set equal to 0.05.



## Table 1. Sample description.

This table describes the sample of loan deals that I use to study why takeover vulnerability affects the cost of bank loans. Each observation corresponds to a unique syndicated or sole-lender loan deal issued to a U.S. public corporation and denominated in U.S. dollars between 1994 and 2005. As is customary, I exclude from the sample loans issued to financial institutions (SIC 6000-6999). For a loan deal to be part of my sample, I require the following: the loan needs to be recorded in the Dealscan database, from where I obtain the loan characteristics; the borrowing firm needs to be recorded in the Compustat annual files, so that I can retrieve its accounting variables; and the borrowing firm needs to be recorded in the RiskMetrics' Governance database, from where I obtain the data needed to construct the takeover vulnerability measures. At the moment, there is no common unique firm identifier in Compustat and Dealscan. In order to match companies in both datasets, I use a matching file constructed by Chava and Roberts (2008), which matches company names and loan origination dates from Dealscan to company names and corresponding active dates in the CRSP historical header file. I ensure that each loan deal is matched to the latest publicly available accounting information of the borrowing firm by requiring that the accounting data correspond to a fiscal year that ended at least 90 days before the date of loan activation. I match borrowing firms in Compustat and RiskMetrics through their CUSIP numbers. Within the years of my sample, RiskMetrics data is only available for 1993, 1995, 1998, 2000, 2002 and 2004. Given that this data are highly persistent, I follow standard procedure and use the previously available data until a new update is available. Note that syndicated loans can be structured in several tranches, also called facilities. The average loan deal in my sample consists of 1.39 facilities with a median of 1. The identity of lenders, syndicate structure, and general contract terms such as covenants are typically determined at the deal level, thus being identical for all the facilities of a same deal. Given that covenants play a key role in my analysis, I use as unit of observation a loan deal. Consequently, for deals with multiple facilities, I follow Ivashina (2009) and look at the characteristics of the largest facility that starts at the loan initiation. I have 5,593 loan deals for which all variables used in my analyses are available, corresponding to 1,523 different firms. Panel A shows the time distribution of these deals. Panels B and C show the number of takeover provisions of the borrowing firms according to three well-known indices and their S&P rating for senior debt at the time of loan activation, respectively. All the regressions that follow include firm rating fixed effects. I combine AAA and AA+ ratings, as well as CCC+ to D, to avoid having rating classes with less than 40 deals. NR stands for Not Rated (or Not Available). Panel D contains the definition of all variables used in my analysis (using Compustat item identifiers when appropriate), as well as their mean, standard deviation (in italics underneath the mean) and median. The modified Altman Z score is a modified version of the Z score that does not include leverage (Graham et al. 1998). All-in spread drawn measures the amount the borrower pays in basis points over LIBOR for each dollar drawn down., adding up the spread of the loan with any annual fees (or facility fee) associated with it. Leverage-limiting covenants is an indicator variable set equal to 1 if the loan deal contains at least one covenant that restricts any of the following: leverage ratio, senior debt to cash flow, debt to cash flow, debt to tangible net worth, debt to equity, tangible net worth, net worth; or a debt sweep covenant that requires the borrower to use at least 75% of the proceeds of excess debt issuance to pay down the loan. When covenants that limit interest coverage are included as leverage-limiting covenants, I set the covenant indicator equal to if the deal contains at least one of the previous covenants or at least one covenant that restricts any of the following: fixed charge coverage, debt service coverage, interest coverage or cash interest coverage. All firm characteristics, spread and facility size variables are winsorized 1% in each tail to reduce the impact of outliers.

### Panel A. Number of loan deals per year.

| 1994 | 1995 | 1996 | 1997 | 1998 | 1999 | 2000 | 2001 | 2002 | 2003 | 2004 | 2005 | Total |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 284 | 298 | 332 | 372 | 396 | 399 | 564 | 558 | 578 | 583 | 643 | 586 | 5,593 |

**Panel B. Takeover indices and the number of loan deals.**

Gompers, Ishii and Metrick (2003) - G index

| G ≤ 4 | G = 5 | G = 6 | G = 7 | G = 8 | G = 9 | G = 10 | G = 11 | G = 12 | G = 13 | G = 14 | G ≥ 15 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 161 | 267 | 349 | 567 | 713 | 765 | 810 | 739 | 482 | 405 | 195 | 140 |

Cremers and Nair (2005) - ATI index

| ATI=0 | ATI=1 | ATI=2 | ATI=3 |
|---|---|---|---|
| 362 | 1,779 | 2,244 | 1,208 |

Bebchuk, Cohen and Ferrell (2009) - E index

| E = 0 | E = 1 | E = 2 | E = 3 | E = 4 | E = 5 | E = 6 |
|---|---|---|---|---|---|---|
| 602 | 1,074 | 1,424 | 1,524 | 810 | 156 | 3 |

**Panel C. Firm S&P rating for senior debt at loan deal close.**

| ≥ AA+ | AA | AA– | A+ | A | A– | BBB+ | BBB | BBB– |
|---|---|---|---|---|---|---|---|---|
| 41 | 79 | 97 | 187 | 397 | 304 | 388 | 601 | 418 |
| BB+ | BB | BB– | B+ | B | B– | ≤ CCC+ | NR | |
| 243 | 286 | 293 | 191 | 95 | 67 | 92 | 1,814 | |

**Panel D. Variables definition and descriptive statistics.**

| | Definition (*Compustat items*) | Mean<br>*Std. Dev.* | Median |
|---|---|---|---|
| **Firm characteristics (source: Compustat)** | | | |
| Total Assets (year 2000 millions $) | *at*, deflated using annual GDP deflator | 5,696.3<br>*9,051.0* | 1,914.0 |
| Firm size (log) | ln (*at*, in year 2000 millions $) | 7.669<br>*1.433* | 7.557 |
| Leverage (book) | (*dltt* + *dlc*) / *at* | 0.289<br>*0.172* | 0.287 |
| Cash flow | *oibdp* / *at* | 0.141<br>*0.078* | 0.133 |
| Market-to-Book | (*prcc_f* x *cshpri* + *lt*) / *at* | 1.765<br>*1.039* | 1.432 |
| Altman Z-score (modified) | 3.3(*pi*/*at*) + 1.0(*sale*/*at*) + 1.4(*re*/*at*) + 1.2(*wcap*/*at*) | 1.817<br>*1.169* | 1.755 |
| Tangibility | *ppent* / *at* | 0.368<br>*0.226* | 0.319 |
| **Takeover measures (source: RiskMetrics)** | | | |
| Gompers, Ishii and Metrick (G index) | (24 – G index) / 24 | 0.607<br>*0.110* | 0.625 |
| Cremers and Nair (ATI index) | (3 – ATI index) / 3 | 0.411<br>*0.287* | 0.333 |
| Bebchuk, Cohen and Ferrell (E index) | ( 6 – E index) / 6 | 0.627<br>*0.216* | 0.667 |

| **Loan deal characteristics (source: Dealscan)** | | | |
|---|---|---|---|
| All-in Spread Drawn | in basis points over LIBOR | 122.656<br>*108.377* | 87.500 |
| Log (All-in Spread Drawn) | ln (All-in Spread Drawn, in basis points over LIBOR) | 4.441<br>*0.882* | 4.472 |
| Leverage-limiting covenants?<br>(not including limits on interest coverage) | yes = 1, no = 0 | 0.542<br>*0.498* | 1 |
| Risk-adj. proportion of deals w. covenants in deals w. same lead bank (not including int. cov) | residual of regression of prop. of deals with covenants on average risk of deals | 0.021<br>*0.097* | 0.034 |
| Leverage-limiting covenants?<br>(including limits on interest coverage) | yes = 1, no = 0 | 0.571<br>*0.495* | 1 |
| Risk- adj. proportion of deals w. covenants in deals w. same lead bank (including inter. cov.) | residual of regression of prop. of deals with covenants on average risk of deals | 0.026<br>*0.099* | 0.034 |
| Number lenders in syndicate | I report here statistics for level, use log in analysis | 10.113<br>*8.928* | 8 |
| Maturity - in months | I report here statistics for level, use log in analysis | 37.951<br>*23.937* | 36.533 |
| Deal number of facilities | I report here statistics for level, use log in analysis | 1.394<br>*0.757* | 1 |
| Facility size (year 2000 millions $) | deflated using annual GDP deflator | 390.41<br>525.97 | 207.31 |
| | | | |
| No. observations (deals) | | 5,593 | |
| No. firms | | 1,523 | |

## Table 2. Why does takeover vulnerability matter to debtholders? First-stage and reduced form estimates.

Column 1 of this table presents the results of a linear regression with the dependent variable being an indicator set equal to one if a loan deal contains leverage-limiting covenants (not including limitations on interest coverage). This is the first-stage equation used to account for the endogeneity of covenants in the IV results presented in Table 3. The main regressors of interest are two, which will be used as instruments in the second stage: The first instrument is the risk-adjusted proportion of deals with covenants out of all deals with the same lead arranger. This is defined as the residual of a bank-level regression of the proportion of deals led by each bank that have leverage-limiting covenants on the average risk of the loans led by each bank, where risk is captured by a set of indicator variables with the S&P credit rating (without notches) of the borrowing firms. This variable intends to capture exogenous variation in the contracting style of each lead arranger, which might effect technical details of the contract such as covenants but not its spread. The second variable of interest is the number of lenders in the syndicate (in log). This variable intends to capture exogenous shifts in credit risk of the lead bank's loan portfolio, which might induce the lead bank to introduce more covenants in loans with a higher effect on its idiosyncratic risk to ensure that it will be able to syndicate a larger share of these loans. I report the results of an *F* test of the joint significance of these two regressors. All other variables are defined in Table 1. Columns 2 to 4 present reduced-form estimates of the effect of my instruments and the other control variables on loan spreads (in log). All regressions include an intercept and a battery of fixed effects (year; Fama-French industry; S&P rating; loan type and purpose; and indicators for senior, secured and performance pricing deals), which are not reported to conserve space. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. I use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

| | First-stage estimates | Reduced-form estimates | | |
|---|---|---|---|---|
| ***Dependent variable:*** | **Leverage-limiting covenants?** | **Log (All-in spread drawn, in basis points over LIBOR)** | | |
| | (1) | (2) | (3) | (4) |
| Risk-adj. proportion of deals w. covenants in deals w. same lead | 0.366*** | -0.267*** | | -0.261*** |
| | *0.070* | *0.098* | | *0.098* |
| Number lenders in syndicate (log) | 0.036*** | | -0.033*** | -0.032*** |
| | *0.007* | | *0.010* | *0.010* |
| Takeover Vulnerability | -0.044 | 0.208*** | 0.205** | 0.204** |
| | *0.056* | *0.079* | *0.079* | *0.079* |
| Firm size (log) | -0.028*** | -0.130*** | -0.120*** | -0.120*** |
| | *0.007* | *0.010* | *0.010* | *0.010* |
| Leverage (book) | -0.082* | 0.321*** | 0.325*** | 0.332*** |
| | *0.044* | *0.062* | *0.062* | *0.062* |
| Cash flow | -0.010 | -0.775*** | -0.773*** | -0.751*** |
| | *0.113* | *0.162* | *0.162* | *0.162* |
| Market-to-Book | 0.017** | -0.057*** | -0.057*** | -0.057*** |
| | *0.007* | *0.010* | *0.010* | *0.010* |
| Altman Z-score (modified) | 0.011 | -0.055*** | -0.056*** | -0.054*** |
| | *0.008* | *0.011* | *0.011* | *0.011* |
| Tangibility | -0.048 | -0.038 | -0.047 | -0.042 |
| | *0.041* | *0.056* | *0.056* | *0.056* |
| Maturity - months (log) | -0.012 | -0.132*** | -0.124*** | -0.124*** |
| | *0.013* | *0.020* | *0.020* | *0.020* |
| Deal number of facilities (log) | 0.093*** | 0.133*** | 0.140*** | 0.140*** |
| | *0.015* | *0.021* | *0.020* | *0.020* |

| | | | | |
|---|---|---|---|---|
| *F* Test: Proportion & # lenders = 0 | 27.8*** | n/a | n/a | 10.1*** |
| Year & Industry fixed effects? | yes | yes | yes | yes |
| Firm S&P rating fixed effects? | yes | yes | yes | yes |
| Loan type & purpose fixed effects? | yes | yes | yes | yes |
| | | | | |
| $R^2$ | 52.1% | 75.7% | 75.7% | 75.8% |
| *F* test: all coefficients = 0 | 116.4*** | 190.9*** | 190.5*** | 188.7*** |
| No. observations (deals) | 5,593 | 5,593 | 5,593 | 5,593 |
| No. firms | 1,523 | 1,523 | 1,523 | 1,523 |

**Table 3. Why does takeover vulnerability matter to debtholders? Second-stage estimates.**

In this table, I investigate whether and why takeover vulnerability affects the pricing of bank loans. The dependent variable is the All-in Spread Drawn, in log to account for its skewness. Equations 1 and 2 show OLS results of the level effect of leverage-limiting covenants and takeover vulnerability on loan spreads, respectively. Equation 3 allows the effect of takeover vulnerability to vary depending on whether the loan deal contains leverage-limiting covenants (not including limitations on interest coverage), while also accounting for the level effect of these covenants. Equation 4 accounts for the potential endogeneity of the covenant variable (loans to riskier firms are more likely to include covenants), using as instruments the risk-adjusted proportion of deals with covenants out of all deals with the same lead arranger and the number of lenders in the syndicate (in log). I also include as an instrument the first-stage predicted probability of a deal containing covenants (see Table 2) interacted with takeover vulnerability, to instrument for the interaction of covenants and takeover vulnerability. In Equation 5, I include as regressor the number of lenders in the syndicate, and thus the risk-adjusted proportion of deals with covenants becomes the only source of identification (analogously in Equation 6). IV regressions are estimated by GMM; results are very similar using 2SLS or LIML. Variables are defined in Table 1. All regressions include an intercept and a battery of fixed effects (year; Fama-French industry; S&P rating; loan type and purpose; and indicators for senior, secured and performance pricing deals), which are not reported to conserve space. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. I use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

| ***Dependent Variable:*** | **Log (All-in Spread Drawn, in basis pts. over LIBOR)** | | | | | |
|---|---|---|---|---|---|---|
| | OLS (1) | OLS (2) | OLS (3) | IV (4) | IV (5) | IV (6) |
| Takeover Vulnerability | | 0.209*** | 0.278** | 0.543*** | 0.533*** | 0.548*** |
| | | *0.079* | *0.119* | *0.158* | *0.161* | *0.161* |
| Leverage-limiting Covenants | 0.059*** | | 0.134 | -0.368* | -0.319 | -0.416 |
| | *0.020* | | *0.082* | *0.207* | *0.278* | *0.289* |
| Takeover Vulnerability x Covenant | | | -0.120 | -0.680*** | -0.659*** | -0.695*** |
| | | | *0.131* | *0.203* | *0.217* | *0.215* |
| Firm size (log) | -0.129*** | -0.129*** | -0.129*** | -0.145*** | -0.143*** | -0.146*** |
| | *0.010* | *0.010* | *0.010* | *0.012* | *0.016* | *0.012* |
| Leverage (book) | 0.313*** | 0.314*** | 0.316*** | 0.264*** | 0.269*** | 0.259*** |
| | *0.062* | *0.062* | *0.062* | *0.073* | *0.074* | *0.077* |
| Cash flow | -0.799*** | -0.798*** | -0.798*** | -0.744*** | -0.744*** | -0.745*** |
| | *0.163* | *0.162* | *0.162* | *0.189* | *0.186* | *0.193* |
| Market-to-Book | -0.055*** | -0.057*** | -0.057*** | -0.043*** | -0.044*** | -0.042*** |
| | *0.010* | *0.010* | *0.010* | *0.012* | *0.012* | *0.013* |
| Altman Z-score (modified) | -0.060*** | -0.058*** | -0.058*** | -0.045*** | -0.046*** | -0.045*** |
| | *0.012* | *0.011* | *0.011* | *0.013* | *0.013* | *0.014* |
| Tangibility | -0.044 | -0.043 | -0.039 | -0.071 | -0.069 | -0.074 |
| | *0.056* | *0.056* | *0.056* | *0.065* | *0.064* | *0.067* |
| Maturity - in months (log) | -0.133*** | -0.132*** | -0.132*** | -0.134*** | -0.133*** | -0.134*** |
| | *0.020* | *0.020* | *0.020* | *0.023* | *0.023* | *0.024* |
| Deal number of facilities (log) | 0.127*** | 0.132*** | 0.126*** | 0.214*** | 0.209*** | 0.220*** |
| | *0.020* | *0.021* | *0.020* | *0.032* | *0.038* | *0.040* |
| Risk-adj. proportion of deals w. covenants in deals w. same lead | | | | *Instrum.* | *Instrum.* | 0.043 *0.175* |
| Number lenders in syndicate (log) | | | | *Instrum.* | -0.004 *0.017* | *Instrum.* |
| Hansen test of overid. restr. (*p*-val.) | n/a | n/a | n/a | 0.801 | n/a | n/a |

| | | | | | | |
|---|---|---|---|---|---|---|
| Year & Industry fixed effects? | yes | yes | yes | yes | yes | yes |
| Firm S&P rating fixed effects? | yes | yes | yes | yes | yes | yes |
| Loan type & purpose fixed effects? | yes | yes | yes | yes | yes | yes |
| | | | | | | |
| $R^2$ | 75.6% | 75.7% | 75.7% | n/a | n/a | n/a |
| $F$ test: all coefficients = 0 | 195.2*** | 192.7*** | 190.1*** | 129.5*** | 133.7*** | 122.2*** |
| No. observations (deals) | 5,593 | 5,593 | 5,593 | 5,593 | 5,593 | 5,593 |
| No. firms | 1,523 | 1,523 | 1,523 | 1,523 | 1,523 | 1,523 |

## Table 4. Robustness checks.

In this table, I analyze the sensitivity of the results presented in Tables 2 and 3 to alternative definitions of the takeover vulnerability measure (columns 1 and 2) and of leverage-limiting covenants (columns 3). In particular, in column 1 the takeover vulnerability measure is based on the ATI index, introduced by Cremers and Nair (2005), while in column 2 it is based on the E index, introduced by Bebchuk et al. (2009). In column 3, I return to my baseline measure of takeover vulnerability using Gompers et al.'s (2003) G index, but I change the covenant measure to include also covenants that place restrictions on the borrowing firm's interest coverage. Panels A and B present the first and second-stage estimates, respectively. In panel B, for each specification, I report the results of an OLS regression next to the IV estimates. Except for covenants and takeover vulnerability, all other variables are defined as in Tables 2 and 3. IV regressions are estimated by GMM using my full set of instruments (the sources of identification are the risk-adjusted proportion of deals with covenants out of all deals with the same lead arranger and the number of lenders in the syndicate); results are very similar using 2SLS or LIML. All regressions include an intercept and a battery of fixed effects (year; Fama-French industry; S&P rating; loan type and purpose; and indicators for senior, secured and performance pricing deals). Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. I use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

## Panel A. First-stage estimates.

| | First-stage estimates | | |
|---|---|---|---|
| ***Dependent Variable:*** | **Leverage-limiting covenants? (not including limits on interest coverage - baseline)** | | **(including limits on interest cover.)** |
| *Takeover Vulnerability measure:* | Cremers and Nair | Bebchuk et al. | Gompers et al. |
| | (1) | (2) | (3) |
| Risk-adj. proportion of deals w. covenants in deals w. same lead | 0.366*** | 0.366*** | 0.244*** |
| | *0.070* | *0.070* | *0.064* |
| Number lenders in syndicate (log) | 0.036*** | 0.037*** | 0.043*** |
| | *0.007* | *0.007* | *0.007* |
| Takeover Vulnerability | -0.012 | 0.008 | -0.010 |
| | *0.021* | *0.029* | *0.053* |
| Firm size (log) | -0.028*** | -0.028*** | -0.030*** |
| | *0.007* | *0.007* | *0.006* |
| Leverage (book) | -0.080* | -0.080* | -0.063 |
| | *0.044* | *0.045* | *0.041* |
| Cash flow | -0.012 | -0.009 | -0.049 |
| | *0.113* | *0.113* | *0.104* |
| Market-to-Book | 0.017** | 0.016** | 0.024*** |
| | *0.007* | *0.007* | *0.006* |
| Altman Z-score (modified) | 0.012 | 0.012 | 0.009 |
| | *0.008* | *0.008* | *0.008* |
| Tangibility | -0.047 | -0.047 | -0.065* |
| | *0.041* | *0.041* | *0.037* |
| Maturity - in months (log) | -0.012 | -0.012 | -0.018 |
| | *0.013* | *0.013* | *0.013* |
| Deal number of facilities (log) | 0.093*** | 0.093*** | 0.074*** |
| | *0.015* | *0.015* | *0.014* |
| *F* Test: Proportion & # lenders = 0 | 27.8*** | 27.9*** | 26.1*** |
| Year & Industry fixed effects? | yes | yes | yes |
| Firm S&P rating fixed effects? | yes | yes | yes |

| | | | |
|---|---|---|---|
| Loan type & purpose fixed effects? | yes | yes | yes |
| $R^2$ | 52.1% | 52.1% | 57.2% |
| *F* test: all coefficients = 0 | 116.2*** | 115.4*** | 168.3*** |
| No. observations (deals) | 5,593 | 5,593 | 5,593 |
| No. firms | 1,523 | 1,523 | 1,523 |

## Panel B. Second-stage estimates.

| | Second-stage estimates | | | | | |
|---|---|---|---|---|---|---|
| ***Dependent Variable:*** | **Log (All-in Spread Drawn, in basis points over LIBOR)** | | | | | |
| *Covenant measure:* | not including limits on interest coverage | | | | including limits on interest coverage | |
| *Takeover Vulnerability measure:* | Cremers and Nair (1) | | Bebchuk et al. (2) | | Gompers et al. (3) | |
| | OLS | IV | OLS | IV | OLS | IV |
| Takeover Vulnerability | 0.095** | 0.152*** | 0.035 | 0.169** | 0.256** | 0.547*** |
| | *0.041* | *0.056* | *0.058* | *0.081* | *0.123* | *0.164* |
| Leverage-limiting Covenants | 0.079*** | -0.727*** | 0.058 | -0.634*** | 0.139 | -0.442** |
| | *0.028* | *0.206* | *0.045* | *0.196* | *0.085* | *0.225* |
| Takeover Vulnerability x Covenants | -0.046 | -0.171** | 0.002 | -0.233** | -0.081 | -0.616*** |
| | *0.046* | *0.074* | *0.065* | *0.106* | *0.136* | *0.196* |
| Firm size (log) | -0.128*** | -0.144*** | -0.129*** | -0.146*** | -0.128*** | -0.144*** |
| | *0.010* | *0.012* | *0.010* | *0.012* | *0.010* | *0.012* |
| Leverage (book) | 0.311*** | 0.262*** | 0.315*** | 0.264*** | 0.317*** | 0.274*** |
| | *0.061* | *0.073* | *0.062* | *0.073* | *0.061* | *0.073* |
| Cash flow | -0.789*** | -0.746*** | -0.794*** | -0.746*** | -0.797*** | -0.789*** |
| | *0.163* | *0.189* | *0.163* | *0.189* | *0.161* | *0.190* |
| Market-to-Book | -0.056*** | -0.042*** | -0.056*** | -0.043*** | -0.059*** | -0.037*** |
| | *0.010* | *0.012* | *0.010* | *0.012* | *0.010* | *0.012* |
| Altman Z-score (modified) | -0.061*** | -0.047*** | -0.060*** | -0.046*** | -0.059*** | -0.046*** |
| | *0.012* | *0.013* | *0.012* | *0.013* | *0.011* | *0.013* |
| Tangibility | -0.044 | -0.080 | -0.044 | -0.082 | -0.036 | -0.090 |
| | *0.056* | *0.064* | *0.056* | *0.064* | *0.056* | *0.066* |
| Maturity - in months (log) | -0.131*** | -0.134*** | -0.132*** | -0.135*** | -0.132*** | -0.137*** |
| | *0.020* | *0.023* | *0.020* | *0.023* | *0.020* | *0.024* |
| Deal number of facilities (log) | 0.127*** | 0.215*** | 0.126*** | 0.214*** | 0.125*** | 0.203*** |
| | *0.020* | *0.032* | *0.020* | *0.032* | *0.020* | *0.030* |
| Hansen test of overid. restr. (*p*-val.) | n/a | 0.767 | n/a | 0.794 | n/a | 0.508 |
| Year & Industry fixed effects? | yes | yes | yes | yes | yes | yes |
| Firm S&P rating fixed effects? | yes | yes | yes | yes | yes | yes |
| Loan type & purpose fixed effects? | yes | yes | yes | yes | yes | yes |
| $R^2$ | 75.7% | n/a | 75.7% | n/a | 75.8% | n/a |
| *F* test: all coefficients = 0 | 191.4*** | 130.3*** | 192.7*** | 132.0*** | 192.7*** | 126.2*** |
| No. observations (deals) | 5,593 | 5,593 | 5,593 | 5,593 | 5,593 | 5,593 |
| No. firms | 1,523 | 1,523 | 1,523 | 1,523 | 1,523 | 1,523 |