# Unfriendly Creditors: Debt Covenants and Board Independence*

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**Abstract**

We develop and test a theory of the relation between debt financing and the composition of the board of directors. We argue that, when firms are close to financial distress, shareholders and creditors can have different preferences over the degree of board independence from management: shareholders prefer a management-friendly board, while creditors prefer an unfriendly board. Our model shows that the equilibrium board composition is state contingent: in states of low cash flows, creditors use their enhanced control rights to promote the appointment of independent directors. We empirically test this prediction using data on covenant violations and board composition. We find a net increase in the number of independent directors of roughly two directors in the first two years following a debt covenant violation.

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The existing literature on the role of creditors in corporate governance is for the most part silent about the mechanisms through which creditors can influence governance. For example, in Diamond's (1984) influential model of delegated monitoring, financial intermediaries monitor borrowers to gather information about cash flows. Diamond offers as an example the case of loan covenant violations, after which "*the intermediary monitors the situation and uses the information to re-negotiate the contract with new interest rates and contingent promises*" (p. 395). What is left unexplained is how exactly this monitoring takes place. In this paper, we investigate – theoretically and empirically – one possible mechanism through which creditors monitor borrowers: the composition of the board of directors.

We begin by constructing a model of optimal corporate board composition and capital structure. Although the model is quite simple in many of its assumptions and conclusions, it has at least one surprising implication: in states in which the interests of shareholders and those of creditors diverge, creditors prefer a shareholder-friendlier board, while shareholders prefer a *less* shareholder-friendly board.

Creditors' interest in board composition is not obvious. Even if creditors can influence board appointments, directors still have fiduciary duties towards shareholders.[1] In general, excessive and explicit intervention by creditors can put them in a situation in which they might have a fiduciary duty towards shareholders, or in the case of bankruptcy, activist creditors could be subject to equitable subordination (i.e. courts may treat creditors' claims as subordinate on equitable grounds). Thus, typical debt contracts rarely give explicit rights over board appointments to creditors. Creditors' influence on corporate governance is thus often subtle and behind the scenes (Baird and Rasmussen, 2006; Nini, Smith, and Sufi, 2012), which makes the empirical documentation of such activities more challenging.

Our main empirical finding – which is consistent with the main prediction of the theory – is that board independence increases after a debt covenant violation. Covenant violations imply a net increase in the number of independent directors of approximately two directors. Our analysis strongly supports a causal interpretation of the evidence: debt covenant violations appear to cause significant changes to the composition of boards.

Our empirical analysis is not meant to be a definitive test of the particular theory that we develop here. It is instead a test of a family of theories in which creditors influence governance through new board appointments. Our model is the first formal analysis of one such theory, but alternative theories of creditor influence on governance may have similar

[1] Although in the vicinity of insolvency directors may also consider the interest of creditors (Becker and Strömberg, 2012).

empirical implications, as we discuss later.

Theories of boards normally ignore capital structure considerations. Our contribution to this literature is the introduction of debt and equity financing in an otherwise standard communication model of boards (as in Adams and Ferreira (2007) and Harris and Raviv (2008)). We show that, under an optimal capital structure (in an incomplete contracts framework), the equilibrium board composition is typically state dependent. Furthermore, we show that the optimal contingent board structure can only be implemented if creditors have the right to appoint *independent* (i.e. shareholder-friendly rather than management-friendly) board members in low cash flow states.

The intuition of our model is as follows. We start from the existing theoretical literature, which shows that shareholders may sometimes prefer to hire a "friendly board," i.e. a board that is a weak monitor of the CEO. For example, in Adams and Ferreira (2007) a friendly board facilitates communication with the CEO, in a setting in which both the board and the CEO are endowed with private information. Private information is more relevant when projects are complex and have uncertain payoffs. Our key assumption is that communication between the board and the CEO facilitates the undertaking of complex projects. Creditors however prefer simpler and safer projects, as long as cash flows are sufficient to repay the debt in full. Thus, in states of low expected cash flow, creditors prefer a board that monitors the CEO more intensively, which leads to the choice of safer projects, thus avoiding risk shifting.

In states of low expected cash flow, shareholders often have incentives to take risks that are unwanted by creditors (Jensen and Meckling (1976)). In our model, shareholders engage in risk shifting by choosing a friendly board. Therefore, to increase debt capacity ex ante, shareholders need to commit to an "unfriendly" board in states of low expected cash flow. This commitment can be achieved by transferring the right to appoint board members to creditors in such states. Our theory then predicts that debt contracts must allow creditors to influence board appointments in low cash flow states.[2]

Although debt contracts rarely give explicit rights over board appointments to creditors, in practice creditors become more influential after (or just before) loan covenant violations. A large literature exists that shows significant changes in corporate policies after covenant violations; the usual interpretation of such evidence is that creditors exert direct influence

[2] The optimality of contingent allocation of control rights is a well understood result in the financial contracting literature (Aghion and Bolton (1992); Dewatripont and Tirole (1994)). Our contribution is instead the explicit modeling of one specific mechanism – board monitoring – through which creditors try to influence firm outcomes after acquiring control rights.

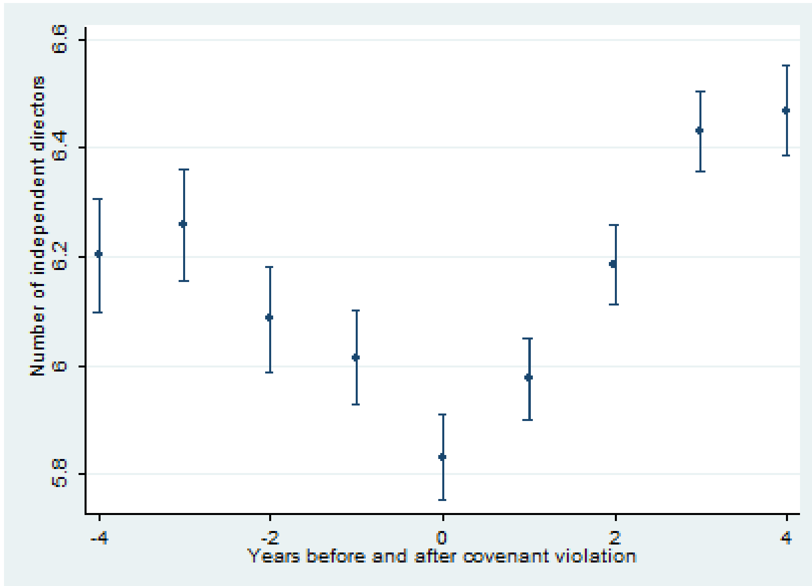


Figure 1: Average Number of Independent Directors before and after a Covenant Violation

on corporate decisions through their threat of accelerating loan payments after covenant violations (recent examples include Chava and Roberts (2008), Roberts and Sufi (2009), and Nini, Smith, and Sufi (2012), among others). Thus, we expect that creditors would use their increased power after (or in the vicinity of) covenant violations to push for an unfriendly board, which communicates less and monitors more. The key testable implication of our theory is that board independence should increase after loan covenant violations.

In our empirical analysis, we find strong evidence that loan covenant violations lead to increases in board independence. Our empirical strategy is adapted from Chava and Roberts (2008), who study the effect of loan covenant violations on corporate investment.

Figure 1 illustrates our main finding in the raw data. It plots the average number of independent directors against the number of years from a loan violation (zero is the year of a violation; the figure shows 95% confidence intervals around the mean). The figure shows that the average number of independent directors falls until the year of a violation, when this trend is sharply reversed.

Although the evidence is suggestive, it raises a number of concerns that we address in detail in the remaining of the paper. Here we provide a brief summary of the main issues and proposed solutions.

The first obvious issue is that the number of non-independent directors might also increase after covenant violations. We show that this is not true; if anything, the average number of non-independent directors decreases slightly following covenant violations. Thus, board independence unambiguously increases after covenant violations.

A second issue is that firms with more independent directors could be more likely to survive and remain in the sample after covenant violations. Similarly, omitted variables could create the appearance of a relation between financial weakness and board independence. In particular, firms may react to poor financial performance by increasing board independence.

We address these concerns in several ways. We use firm fixed-effects in all of our tests, thus we only use within-firm variation in board composition, which rules out the most obvious survivorship bias. We also restrict the sample to include only observations that fall within a narrow interval around the threshold that triggers violation, while at the same time saturating the model with fixed effects and polynomials of measures of the distance to the covenant threshold. Thus, firms on each side of the covenant threshold are very similar in terms of their financial performance and other characteristics, and much of the remaining heterogeneity is captured by the fixed and the measures of the distance to threshold. We find that the effect of covenant violations on board independence is stronger in such subsamples; this average effect can be as large as two new independent directors being appointed within two years of a covenant violation. In contrast, in Figure 1 we see that from Year 0 to Year 4, the average number of independent directors increases from 5.8 to 6.5. Thus, if anything, reducing firm heterogeneity makes the results stronger. In contrast, the effect is weaker (but still economically and statistically significant) in regressions that use the full sample.[3]

Third, our identification strategy requires that, if firms manipulate accounting variables in order to avoid covenant violations, this manipulation should be uncorrelated with firms' propensity to change boards in the near future. The use of fixed effects addresses the concern that some time-invariant firm characteristics may affect both the incentives to manipulate accounting variables and the likelihood of appointing independent directors. We also find that firms on each side of the covenant violation threshold are similar in most observable governance characteristics. In particular, past and current levels of board independence are virtually identical for firms on each side of the covenant threshold, which suggests that past and current levels of board independence are uncorrelated with accounting manipulation (aimed at avoiding covenant violations). Given this evidence, it is unlikely that manipulation is directly affected by the expectation of future changes in board independence.

Fourth, perhaps time trends in board independence could create a spurious relation be-

[3] Bakke and Whited (2013) provide an illustration, in a context similar to the one in this paper, of how estimates using the full sample can differ dramatically from those using a restricted sample.

tween covenant violations and board independence. This could happen mechanically because a violation state often happens one period after a non-violation state. To absorb aggregate trends, we use year dummies. To deal with the possibility of firm-specific trends, we perform a series of falsification tests in which placebo thresholds are set at a fixed distance from the real threshold. We find that our estimated effects are only economically and statistically significant at the real threshold. The placebo tests thus help rule out firm-specific trends as an explanation for the results, as well as other stories of similarly spurious relationships. Given the potentially many identification issues, we believe that the placebo tests provide the strongest support for a causal interpretation of the evidence.

Finally, even if the relation between covenant violations and board independence was causal, the mechanism could be quite different from the one emphasized in our theory. There are two main competing explanations. The first alternative explanation is the possibility that changes in board composition occur only in bankrupt companies. As creditors are often given explicit voting rights in such cases, it is not surprising that they will make changes to the board. In fact, Gilson (1990) documents important changes in the boards of bankrupt companies. We find that bankruptcy cannot explain the evidence: in the subsamples used for our main tests, there is not a single firm that files for bankruptcy within two years of a covenant violation. This is consistent with previous research that show that covenant violations rarely lead to default (e.g. Gopalakrishnan and Parkash (1995); Chava and Roberts (2008) also make this point).

Another competing explanation is that shareholders, and not creditors, are those who wish to increase independence after covenant violations. This explanation, while compatible with the main finding of a causal effect of covenant violations on board independence, is at odds with the wealth of existing evidence of the influence of creditors on corporate outcomes after covenant violations. We also show some evidence directly supporting the bank monitoring mechanism: our results are stronger for firms with more bank debt.

Overall, the evidence that we present provides strong support for the hypothesis that creditors influence the appointment of new independent directors after covenant violations.

Our work is related to a number of studies that focus on the role of creditors on corporate governance. The paper that is the closest to ours is Nini, Smith, and Sufi (2012), who show that CEO turnover increases after loan covenant violations. This evidence "*suggests that creditors also exert informal influence on corporate governance*" (p. 1713). Our evidence complements theirs, as we show that the turnover of independent directors is also a governance mechanism that may be available to creditors. Importantly, our evidence is of a different nature: our results are not explained only by the dismissal of executive directors; our results are strongly driven by the appointment of new independent directors. We also show that our results are stronger for the subset of firms that do not replace their CEOs after a covenant violation. Also related is Becker and Strömberg's (2012) study, which shows that a change in law that required boards to consider the interest of creditors in financially distressed firms increased leverage in affected firms and reduced the use of covenants. There are also important related papers on the costs and benefits of bankers on corporate boards (Kroszner and Strahan (2001); Güner, Malmendier, and Tate (2008)).

Becker and Strömberg (2012) discuss an example of creditor influence on board appointments.[4] In 1991, Pathé Communications obtained a loan from Credit Lyonnais. The loan allowed Pathé Communications to pay its trade creditors and exit from bankruptcy. As part of the loan agreement, Credit Lyonnais and MGM – Pathé Communications' controlling shareholder – signed a corporate governance agreement that limited the power of Pathé's board with respect to its CEO, whose recent appointment was supported by the bank. Later in that year, Credit Lyonnais decided that MGM had breached the corporate governance agreement and then used its contractual rights to remove three directors affiliated with MGM from Pathé's board; these directors' replacements were then nominated by the bank. The dispute ended up in a Delaware court, which ruled in favor of Credit Lyonnais.

The example of Credit Lyonnais illustrates four points that are relevant for this study. First, creditors often choose to monitor borrowers by influencing the appointment of key executives and board members. Second, they do so even in cases in which the borrower is not (yet) insolvent and despite not owning shares in the borrowing firm. Third, creditors force changes to the board after the borrower has breached some condition in the loan contract. Fourth, creditors often support the appointment of new executives and board members who are not closely affiliated with them.

The role of creditors in executive turnover is well understood in the legal literature (Baird and Rasmussen, 2006) and well documented empirically (Nini, Smith, and Sufi, 2012). But the role of creditors in the appointment and replacement of board directors is not yet well understood and documented. This paper aims both to show the empirical importance of creditors in the shaping of corporate boards and to offer tentative explanations for such empirical evidence.

Gilson (1990) was the first to investigate the influence of creditors on board composition.

[4] Our description here follows Becker and Strömberg (2012) and the *Credit Lyonnais v. Pathé Communications* 1991 legal ruling.

He finds direct evidence that banks influence the appointment of directors in negotiated restructurings. This influence is exerted both directly and through share ownership. Our results complement those of Gilson by providing further evidence that bankers influence board appointments *outside bankruptcy states* and by establishing the direction of causation more convincingly.

Our paper also contributes to a now large theoretical literature on corporate boards (Hirshleifer and Thakor (1994, 1998), Hermalin and Weisbach (1998), Raheja (2005), Song and Thakor (2006), Adams and Ferreira (2007), Harris and Raviv (2008), Chemmanur and Fedaseyeu (2010), Chakraborty and Yilmaz (2011), Malenko (2014), Levit and Malenko (2013), Levit (2012)). We differ from this literature by offering a simple model of the joint determination of board structure and capital structure.

# 1 Model

The model is both a simplified version and an extension of Adams and Ferreira (2007). The main innovation is the distinction between equity financing and debt financing.

## 1.1 Setup

**Operations.** A firm needs to raise external finance to cover an initial investment cost $K$. After paying $K$, the firm chooses one investment project. There are two mutually exclusive choices available to the firm. The first option is to do nothing: invest in project $\emptyset$ (the null project). The null project generates zero cash flow for sure. We also call project $\emptyset$ the *simple* project. This project is always available and its identity is known by all players. The second option is to invest in a nonroutine, *complex project*. There is an infinite number of complex projects, but the firm can only undertake one of them. We denote each complex project by a real number $y \in \Re$.

Complex projects require specialized knowledge; this knowledge is represented by a real number $e \in \Re$. Formally, let $P(y, e)$ denote the payoff associated with a complex project $y$ that requires knowledge of $e$. For simplicity we assume a quadratic technology:

$$P(y, e) = \alpha - (y - e)^2, \qquad (1)$$

where $\alpha > 0$ is non-stochastic. Projects $y \in \Re$ are complicated in the sense that knowledge of $e$ may be difficult to acquire. If the decision-maker learns $e$, the choice that maximizes the firm's payoff is project $y = e$ (we can also think of this as the optimal scale of a project). The prior distribution of $e$ is left unspecified; we denote its mean and variance by $\mu$ and $\sigma^2$, respectively. This technology is similar to that found in many papers in the cheap talk and delegation literature (e.g. Adams and Ferreira, 2007; Dessein, 2002; Harris and Raviv, 2005, 2008, 2011). For the null project, we set $P(\emptyset, e) = 0$.

Firm value also depends on the value of assets in place $A(s)$, where $s \in \{0, 1\}$ is a state variable. Assets in place can take two possible values: $A(1) \equiv A_1 > A_0 \equiv A(0)$. The probability of $A_s$ is given by $p_s \in (0, 1)$. The *total value of the operating assets* is defined as

$$V(s, y, e) = A(s) + P(y, e). \tag{2}$$

For the sake of simplicity and interpretation, we assume that $A_1 \gg A_0 + \alpha$, so that we can unambiguously refer to state $s = 0$ as the low cash flow state.

**Financing.** Firms can issue only two types of securities: equity and (straight) debt. The space of security contracts is exogenously determined. Capital providers are perfectly competitive, risk-neutral and have zero discount rate. Equity holders have residual claims over cash flows and the right to appoint and replace board members.

Debt securities take the form of zero coupon bonds, possibly with covenants. We denote the market value of the debt by $D$ and its face value by $F$. We deviate from the Modigliani-Miller world by assuming that debt has direct costs and benefits. For each dollar raised through debt, the firm receives $1 + t$ dollars in cash, $t > 0$. The most natural interpretation is that debt generates tax shields, which we assume are proportional to the amount of debt issued. That is, $tD$ is the value of the tax shields, which for simplicity we assume can be converted into cash immediately and used to pay for part of the initial investment cost. An alternative interpretation is that transaction costs (due to issuing costs, unmodeled asymmetric information, or mispricings) may make issuing new shares more costly than issuing bonds.

At maturity, if the firm does not have sufficient assets that can be immediately monetized to pay the face value of debt in full, the firm defaults. Default generates direct distress costs. To save on notation we assume that distress costs are so severe that the value of assets under distress is zero. This assumption is without loss of generality; the analysis is basically identical if the value of operating assets falls to strictly positive levels.

In sum, we assume that the capital structure is at least partly determined by traditional trade-off forces: tax shields and financial distress. Importantly, we also assume that: (i) the value of the operating assets at (and immediately prior to) maturity is verifiable and (ii) debt renegotiations are infinitely costly. Assumption (i) implies that the firm cannot default strategically; if the firm is solvent, debt holders can contractually enforce repayment. It also implies that managers cannot steal assets or sell assets to pay dividends before paying out creditors. Assumption (ii) implies that deadweight distress costs will occur with strictly positive probability in any equilibrium with risky debt.

Project decisions are made after the realization of the random state variable $s \in \{0, 1\}$ becomes public known. Variable $e$ may or may not be known when project decisions are made. The value of the existing equity as a function of $(s, y, e)$ is

$$E(s, y, e) = \max\{V(e, s, y) - F, 0\}. \tag{3}$$

If $e$ is not known – a case that we denote by $\tilde{e}$ – the value of equity is given by

$$E(s, y, \tilde{e}) = \max\{\mathbb{E}[V(e, s, y)] - F, 0\}, \tag{4}$$

where $\mathbb{E}$ denotes the expectation over $e$. Notice that the expectation operator is inside the max operator because of our assumption that the value of the operating assets is fully verifiable (and thus can be sold and monetized). In other words, if $e$ is not known at this date, it will only become known at a later date after all investors (shareholders and creditors) have sold the firm's assets (after the end of the game). This assumption also implies that shareholders have no risk-shifting incentives arising from the stochastic nature of $e$.

**Organization.** The firm is founded by an initial set of shareholders who hire a CEO to run the operations. The CEO's preferences are given by:

$$U(s, y, e) = E(s, y, e) + B(s, y, e), \tag{5}$$

that is, the CEO cares about the equity value but also enjoys private benefits $B(s, y, e)$.[5] We assume that private benefits are also quadratic in $y$:

$$B(e, y) = \beta - (y - e - b)^2, \tag{6}$$

with $\beta \geq 0$ and $b > 0$. This implies that the CEO has a preference for projects that are larger than their optimal scales. For the null project, we set $B(\emptyset, e) = 0$. We also make the simplifying assumption that $\alpha \geq \beta$, because otherwise maximizing private benefits with no concern for equity would be more efficient than maximizing equity value with no concern for private benefits.

[5] This reduced-form specification is consistent with situations in which the CEO holds some of the equity (or is compelled to care about equity value for reputational or other reasons) while enjoying private benefits.

We can rewrite the CEO's utility as

$$U(s,y,e) = E(s,y,e) + \mathbf{1}_{(y\neq\emptyset)}\left[\beta - (y-e-b)^2\right], \tag{7}$$

where $\mathbf{1}_{(y\neq\emptyset)}$ is an indicator function that takes the value of 1 if the firm undertakes a complex project. This utility function is a reduced-form approach to model preference misalignment between the CEO and shareholders. The CEO's utility is increasing in the value of equity (the first term on the right-hand side), but also depends on his own bias $b$ in project choice. This reduced-form specification is typical in the communication literature on which this model is based (e.g. Crawford and Sobel, 1982; Adams and Ferreira, 2007; Dessein, 2002; Harris and Raviv, 2005, 2008, 2011, and many others). Here, $U(s,y,e)$ looks superficially different from its counterparts in the related literature, but only because we make a distinction between equity and debt, which is absent from that literature.

Shareholders also appoint a board of directors to monitor and advise the CEO. Board composition may be changed at predetermined dates if shareholders wish to do so. The board can be of two types. With probability $\pi$, the board is fully aligned with shareholders and thus has utility $E(s,y,e)$. With probability $1-\pi$, the board is fully aligned with the CEO and thus has utility $U(s,y,e)$. Shareholders can choose whatever $\pi$ they wish at no cost.[6] In line with Adams and Ferreira (2007), we call $\pi$ the board's *independence level* or *monitoring intensity*, while $1-\pi$ is a measure of *board friendliness*.

Regardless of its type, the board may play an important advising role. If the CEO reports his private information about firm-specific conditions to the board, the board learns the value of $e$ with probability one, in which case it may report this value back to the CEO. Formally, we model the communication game as in Adams and Ferreira (2007). We assume that project $y$ must be chosen from a continuum of sets of projects indexed by another real number $\theta \in [0,1]$. The CEO learns (at no cost) the realization of $\theta$, which we call his *firm-specific information*. The CEO then decides whether to reveal this information to the board. Information is hard and, when revealed, becomes publicly observable by all players.

After receiving the CEO's report, the board invests one unit of its time to gather its own private signal $e$ about the profitability of the project. Our main assumption is that if the board is informed about the relevant set of projects, denoted by the state variable $\theta$, then the board obtains its private information $e$ with probability one. If the board remains uniformed about $\theta$, then it cannot learn $e$.

[6] This is for simplicity only. Nothing substantial changes if the appointment of independent directors is costly.

We interpret the communication stage of our model as follows. The CEO raises an *issue* $\theta$ to the board. For each issue, the board proposes a *solution* $e(\theta)$. Without knowing what the issue is, the board cannot propose any solution, thus the CEO needs to speak first and raise the issue. The CEO has the option of either raising an issue or not raising any issue. In line with Adams and Ferreira (2007), we assume that the CEO cannot be punished for not raising an issue, perhaps because often there are no issues to be raised.

Formally, the board believes that the prior distribution of issues $\theta$ is uniform on the unit interval, $\theta \sim U[0,1]$. Conditional on knowing the realization of $\theta$, the CEO's posterior belief is that $e(\theta)$ also has mean $\mu$ and variance $\sigma^2$. If $\theta \neq \theta'$, then $e(\theta)$ is independent from $e(\theta')$, that is, the solutions to different issues are independently and identically distributed random variables.

**Timing.**

*Date 0* - The firm is established and the shareholders hire a CEO. They also appoint a board of directors with a degree of independence $\pi \in [0,1]$ and choose the capital structure: they raise external funds through a combination of debt and equity and use the proceeds to pay for the initial investment cost $K$.

*Date 1* - All players observe the realization of a signal $s \in \{0,1\}$. The signal is related to the value of assets in place $A(s)$. For simplicity, we assume that, conditional on $s$, $A(s)$ is non-stochastic but fully illiquid at this date (that is, it cannot be converted into cash or sold separately from the firm). The signal is verifiable.

At this date, board composition may be changed, either by shareholders' initiative or by creditors (if they have the rights to do so).

*Date 2* - The CEO learns $\theta \in [0,1]$ and decides whether to raise an issue to the board.

*Date 3* - If an issue is raised, the board learns its own private signal $e$ with probability one. If the CEO does not raise an issue, all players remain uninformed about $e$.

*Date 4* - The board's type is now revealed. With probability $\pi$, the board is fully aligned with shareholders and will thus only approve decisions that maximize the value of equity, conditional of the information available at this date. With probability $1-\pi$, the board colludes with the CEO and the CEO retains his right to choose his preferred project.

At the end of Date 4, the firm is liquidated, assets become liquid, and investors are paid off.

## 1.2 Analysis

### 1.2.1 All-Equity Baseline Model

Consider first the case of an all-equity firm. In this case, $E(s,y,e) = V(s,y,e)$, and the CEO's utility becomes

$$U(s,y,e) = A_s + \mathbf{1}_{(y\neq\emptyset)}\left[\alpha+\beta-(y-e)^2-(y-e-b)^2\right] \tag{8}$$

if $e$ is known and $\mathbb{E}U(s,y,e)$ if $e$ is not known.

Suppose first that $e$ is known and that the CEO has the right to decide on which project to undertake. The optimal complex project is $y_{in,ceo}(e) = e + \frac{b}{2}$ and the CEO's utility is

$$U_{in,ceo} = A_s + \alpha + \beta - \frac{b^2}{2}, \tag{9}$$

where subscript *in* denotes that the CEO is informed (as well as all other players) and subscript *ceo* indicates that the CEO is the decision maker.

From now on we assume that $\alpha + \beta > \frac{b^2}{2}$. This assumption implies that the CEO always prefers the complex project over the null project when $e$ is known. If this assumption does not hold, the CEO will never communicate with the board and the model becomes trivial and uninteresting.

Next, suppose that the CEO has control over the decision but $e$ is unknown. His optimal choice of project is

$$y_{un,ceo} = \begin{cases} \mu + \frac{b}{2} & \text{if } \alpha+\beta \geq \frac{b^2}{2} + 2\sigma^2 \\ \emptyset & \text{if } \alpha+\beta \leq \frac{b^2}{2} + 2\sigma^2 \end{cases}, \tag{10}$$

and his utility is

$$U_{un,ceo} = A_s + \max\left\{\alpha+\beta-\frac{b^2}{2}-2\sigma^2, 0\right\}. \tag{11}$$

Thus, all else constant, the CEO prefers to be informed. As in Adams and Ferreira (2007), $\sigma^2$ can be interpreted as the benefit from board advice. Without communication, the incremental value of a complex project to the CEO is $\alpha + \beta - \frac{b^2}{2} - 2\sigma^2$. Raising an issue to the board leads to a solution, which increases the value of a complex project by exactly $2\sigma^2$.

Now consider the case in which $e$ is known and the CEO faces a shareholder-oriented board. In that case, the board chooses the first-best project $y_{in,sh}(e) = e$ and the CEO's utility becomes

$$U_{in,sh} = A_s + \alpha + \beta - b^2. \tag{12}$$

Finally, if $e$ is unknown, and the board is shareholder-oriented, the board chooses

$$y_{un,sh} = \begin{cases} \mu & \text{if } \alpha > \sigma^2 \\ \emptyset & \text{if } \alpha \leq \sigma^2 \end{cases}, \tag{13}$$

which implies that the CEO's utility is

$$U_{un,sh} = A_s + \begin{cases} \alpha + \beta - b^2 - 2\sigma^2 & \text{if } \alpha > \sigma^2 \\ 0 & \text{if } \alpha \leq \sigma^2 \end{cases}. \tag{14}$$

We now consider the CEO's incentives to reveal his information (i.e. to communicate with the board). For a given level of independence $\pi$, the CEO will reveal his information only if

$$\pi U_{in,sh} + (1-\pi) U_{in,ceo} \geq \pi U_{un,sh} + (1-\pi) U_{un,ceo}. \tag{15}$$

It is straightforward to check that if $\alpha - \sigma^2 > 0$, the revelation constraint is never binding, and the CEO shares his information with the board regardless of the level of board independence. Intuitively, a low $\sigma^2$ means that board advice has a small effect on profits. If $\alpha > \sigma^2$, then lack of communication does not reduce the profitability of complex projects sufficiently to make the null project preferable. In that case, if the CEO does not raise an issue, he simply adds noise to the decision, which reduces his own utility. Thus, to focus on the relevant case, from now on we also assume that $\alpha \leq \sigma^2$.

The CEO's revelation constraint simplifies to

$$\pi \left(A_s + \beta + \alpha - b^2\right) + (1-\pi)\left[A_s + \alpha + \beta - \frac{b^2}{2}\right] \geq A_s. \tag{16}$$

or

$$\pi \leq \frac{2(\alpha+\beta)}{b^2} - 1 \equiv \tilde{\pi}. \tag{17}$$

Notice that if $b^2 \leq \alpha + \beta$, then $\tilde{\pi} \geq 1$ and the CEO always reveals his information regardless of the board's monitoring intensity. In this case, it is trivial to verify that the optimal solution, from the shareholders' perspective, is to set $\pi = 1$, and thus friendly boards are never optimal. To focus on the interesting (i.e. interior) case, we then assume that $b^2 > \alpha+\beta$.

We summarize our parametric assumptions as

**Assumption 1.** $\alpha + \beta \in \left[\frac{b^2}{2}, b^2\right]$ and $\alpha \leq \sigma^2$.

Assumption 1 is necessary for an interior solution;[7] if this assumption does not hold, shareholders always choose a fully independent board. Under Assumption 1, the CEO raises

[7] Note that Assumption 1 also implies $\sigma^2 > \frac{b^2}{2}$.

an issue only if the board is sufficiently friendly, i.e. if the monitoring intensity is less than or equal to $\tilde{\pi} < 1$. The maximum level of monitoring that satisfies the CEO's revelation constraint depends on the ratio between the CEO's benefits from the complex project $\alpha + \beta$ and his bias in project choice $b$. As $\alpha + \beta$ increases, the complex project is more valuable to the CEO and thus the CEO is willing to reveal his information even if the board monitors more intensively. In contrast, as $b$ increases, the CEO puts a higher value on controlling project choice, and thus is less likely to reveal his information to the board.

For a given $\pi$, if the CEO and the board communicate with each other, the interim (Date 1) value of equity becomes:

$$E(\pi) = A_s + \alpha - (1 - \pi) \frac{b^2}{4}. \tag{18}$$

Conditional on communication, the equity value is increasing in $\pi$. Thus, if communication is optimal, shareholders choose $\pi = \tilde{\pi}$. If there is no communication, the null project is chosen and the value of equity becomes $A_s$. Shareholders thus prefer a board that communicates with the CEO if and only if

$$E(\tilde{\pi}) - A_s = \alpha - (1 - \tilde{\pi}) \frac{b^2}{4} = \frac{3\alpha + \beta - b^2}{2} \geq 0. \tag{19}$$

We have just proved the following result:

**Proposition 1** ***(Optimal Friendly Boards)*** *Under Assumption 1, in the all-equity case, there is a unique equilibrium with the following properties:*

*(1) if* $3\alpha + \beta \geq b^2$*, then*

- *The optimal board independence level is* $\tilde{\pi} = \frac{2(\alpha+\beta)}{b^2} - 1$*;*
- *The CEO always reveals his private information;*
- *Project* $y = e$ *is chosen with probability* $\tilde{\pi}$ *and project* $y = e + \frac{b}{2}$ *is chosen with probability* $1 - \tilde{\pi}$*.*

*(2) If* $3\alpha + \beta < b^2$*, then*

- *The optimal board independence level is* $\tilde{\pi} = 1$*;*
- *The CEO never reveals his private information;*
- *Project* $\emptyset$ *is chosen with probability* 1*.*

This proposition shows that there are cases in which shareholders prefer a board that is not too independent from management. This occurs when the CEO's bias is not too high (i.e. $b^2 \leq 3\alpha + \beta$), so that the agency cost of a friendly board is offset by the advisory benefits of an informed board. Proposition 1 mirrors Proposition 5 in Adams and Ferreira (2007). The model we present here is however much simpler and more intuitive.

### 1.2.2 Debt and Equity

We now consider the case in which shareholders can choose a combination of debt and equity to fund the initial investment. Although we assume that hybrid securities (e.g. convertible bonds) are not available, this assumption could be justified by the unique tax treatment of debt. That is, our key assumption is that straight debt is the only security that generates tax shields.[8]

Our goal in this section is to find the optimal debt contract. A debt contract specifies an amount $D$ of funds borrowed today (date 0) and a promise to pay back $F$ to creditors at the end of date 4. The debt contract may also contain a covenant. Covenants may be contingent on the signal $s \in \{0, 1\}$. We assume that board control rights can be transferred after covenant violations. As discussed in the introduction, our interpretation is that creditors become more powerful after covenant violations and thus acquire *de facto* control over the board.

We focus initially on a special case in which the optimal debt contract is risk-free. It can be easily shown that the optimal debt contract will indeed be risk-free if the tax benefit of debt, $t$, is positive but sufficiently small. Thus we assume that this is the case. In the appendix, we discuss the possibility of risky debt and show that our main result is unchanged.

We now proceed in four steps. First, we compute the optimal debt contract under the assumption that board independence is set at $\tilde{\pi}$, which is the level of monitoring that maximizes the value of the operating assets (that is, ignoring the direct costs and benefits of debt) whenever communication is optimal. Second, we compute the optimal debt contract under the assumption that $\pi = 1$ after observing $s = 0$. Third, we show that there exist constellations of parameters for which the second contract dominates the first contract. Finally, we show that the second contract cannot be implemented if shareholders keep control

[8]Although in reality convertible bonds may also create tax shields, these benefits tend to be lower than those associated with straight debt. Interest deductability disappears when the option to convert is exercised or forced by the firm (e.g. if there are call provisions). Thus, even if convertibles are possible, they may be suboptimal in our model because of lower tax shields.

of the board at date 1. Then we conclude that a contract that allows creditors to choose $\pi = 1$ after observing $s = 0$ would raise more debt capital and is thus optimal whenever a larger debt capacity is required.

**Step 1.** *Suppose that monitoring is fixed at $\tilde{\pi}$.*

Because debt is risk free, $F = D$. The CEO's utility becomes (recall that there is no default in equilibrium)

$$U(s, y, e) = A_s - D + \mathbf{1}_{(y \neq \emptyset)} \left[ \alpha + \beta - (y - e)^2 - (y - e - b)^2 \right], \tag{20}$$

if $e$ is known and $\mathbb{E}U(s, y, e)$ if $e$ is not known.

If $s = 1$, default cannot occur (because $A_1 \gg A_0 + \alpha$ and debt is risk-free) and the equilibrium identical to that in the all-equity case. Consider now the case in which $s = 0$. To guarantee that debt is indeed risk free, we need that

$$A_0 + \alpha - (y_{in,ceo}(e) - e)^2 \geq D, \tag{21}$$

where $y_{in,ceo}(e)$ is the project chosen by a CEO who knows $e$ if he has control over project choice. Because $y_{in,ceo}(e) = e + \frac{b}{2}$, the no default condition becomes

$$A_0 + \alpha - \frac{b^2}{4} \geq D, \tag{22}$$

in which case the CEO's utility becomes

$$A_0 + \alpha + \beta - \frac{b^2}{2} - D \equiv U^1. \tag{23}$$

We also need to make sure that the CEO prefers this project over the project that would lead to default. If the CEO chooses a project that leads to default, equity is wiped out, and the CEO chooses the project that minimize $(y - e - b)^2$, which leads to the choice of $y = e + b$, and his utility becomes $\beta$. Thus, to avoid default in equilibrium we also need that $U^1 \geq \beta$, which implies

$$A_0 + \alpha - \frac{b^2}{2} \geq D. \tag{24}$$

Only Condition (24) is binding. Thus, default is avoided if and only if

$$D \leq A_0 + \alpha - \frac{b^2}{2} \equiv \tilde{D}_f. \tag{25}$$

We call $\tilde{D}_f$ the firm's *debt capacity* under a friendly board. Because debt is risk free, the optimal debt value conditional on adopting an optimal friendly board is $\tilde{D}_f$. Note that $\tilde{D}_f$

is strictly lower than the minimum cash flow that can be guaranteed in equilibrium, which is $A_0 + \alpha - \frac{b^2}{4}$. To understand why, notice that if the firm promises to repay $A_0 + \alpha - \frac{b^2}{4}$, then the CEO optimally chooses a project that leads to default. Thus, the firm must promise less than its minimum guaranteed cash flow, to make sure that the CEO does not have incentives to distort project choice further away from $e + \frac{b}{2}$.

Intuitively, the firm's debt capacity is increasing in the value of assets in place in the bad state ($A_0$) and in the risk-free component of the project's cash flow ($\alpha$), and decreasing in the CEO's bias ($b$).

It is straightforward to check that if the debt is set at $\tilde{D}_f$, shareholders will indeed choose $\pi = \tilde{\pi}$ at date 1, regardless of $s$.

**Step 2.** *Suppose that monitoring is fixed at $\pi = 1$ after $s = 0$.*

This case is trivial: with no information the board chooses $y = \emptyset$ and the debt capacity is $\tilde{D}_u = A_0$. Thus the optimal debt level is $A_0$.

**Step 3.** *Comparing the two cases.*

If $\tilde{D}_u \leq \tilde{D}_f$, then trivially the optimal choice is to choose $\tilde{D}_f = F$ and a friendly board. If $\tilde{D}_u > \tilde{D}_f$, the unfriendly board case creates larger tax benefits. The incremental benefit from switching from $\tilde{D}_f$ to $\tilde{D}_u$ is

$$t\left(A_0 - A_0 - \alpha + \frac{b^2}{2}\right) = t\left(\frac{b^2}{2} - \alpha\right), \tag{26}$$

which is positive provided that $\tilde{D}_u > \tilde{D}_f$. But switching from $\tilde{D}_f$ to $\tilde{D}_u$ reduces the expected value of the operating assets because complicated projects are better than the null project whenever the board and the CEO communicate. The reduction in the value of operating assets is given by

$$p_0 \left[E\left(\tilde{\pi}\right) - A_s\right] = \frac{p_0}{2}\left(3\alpha + \beta - b^2\right) > 0. \tag{27}$$

Choosing $\tilde{D}_u$ and committing to $\pi = 1$ is optimal if

$$t\left(\frac{b^2}{2} - \alpha\right) \geq \frac{p_0}{2}\left(3\alpha + \beta - b^2\right). \tag{28}$$

**Step 4.** *Shareholders cannot commit to choosing a fully independent board after observing $s = 0$.*

Suppose that debt is $D = A_0 > \tilde{D}_f$. If shareholders choose $\pi = 1$, then their payoff is zero for sure. If they choose instead some $\pi < 1$ such that the CEO communicates, they receive

$\alpha$ with probability $\pi$. Thus the latter is preferable, provided that the CEO communicates. The revelation constraint for the CEO is

$$\pi\left(\beta+\alpha-b^2\right)+(1-\pi)\,\beta\geq 0, \tag{29}$$

which implies that the board will choose $\pi' \equiv \frac{\beta}{b^2-\alpha} < 1$, thus commitment to $\pi = 1$ is not possible if $D = A_0$.[9]

These four steps prove the following result.

**Proposition 2** *Assume that debt is risk-free. The unique equilibrium has the following properties:*

*(1) If* $3\alpha+\beta > b^2$ *and condition (28) holds, then*

- *The equilibrium amount of debt is* $\tilde{D}_u = A_0$.
- *Board independence is contingent on s:* $\pi^* = \tilde{\pi}$ *if* $s = 1$ *and* $\pi^* = 1$ *if* $s = 0$.
- *Contingent board independence requires transferring the right to appoint board members to creditors if* $s = 0$.

*(2) If* $3\alpha+\beta > b^2$ *and condition (28) does not hold, then*

- *The equilibrium amount of debt is* $\tilde{D}_f = A_0 + \alpha - \frac{b^2}{2}$.
- *Board independence is not contingent on s:* $\pi^* = \tilde{\pi}$ *regardless of s.*

*(3) If* $3\alpha+\beta \leq b^2$*, then*

- *The equilibrium amount of debt is* $\tilde{D}_u = A_0$.
- *Board independence is not contingent on s:* $\pi^* = 1$ *regardless of s.*

The main implication of this proposition is that, whenever the debt contract involves a covenant (case 1), board independence increases after a covenant violation. This is the main implication that we test in this paper, so we state it as a corollary:

**Corollary 1** ***(Main result)*** *Board independence increases after loan covenant violations.*

[9] Notice also that $\pi' < \tilde{\pi}$.

## 1.3 What do we learn from the model?

The model delivers an unambiguous prediction (Corollary 1) that we test in this paper. The model can thus be falsified. But alternative theories may also be compatible with the prediction in Corollary 1. As examples, here we consider two simple alternatives (this list is not exhaustive):

**Informal Theory 1** Firms that violate covenants have bad governance (on average). After covenant violations, creditors use their control rights to improve governance. This often includes increasing board independence.

**Informal Theory 2** Firms that violate covenants have bad governance (on average). After covenant violations, shareholders (or perhaps managers) realize the need to improve governance. Shareholders and managers agree to increase the independence of the board, without any input from creditors.

Both of these informal theories predict that covenant violations cause increases in board independence. Thus we do not attempt to test our theory against these alternatives. Our goal is more simply to test the null hypothesis that covenant violations do not affect appointments to the board.

A formal theory is useful if it provides new insights. Here we list four non-trivial insights that our model helps to illustrate:

1. *The model highlights that board independence does not imply good governance.*

2. *The model provides an explicit reason for creditors to care about board appointments.*

3. *The model explains why shareholders and creditors may disagree about the optimal level of independence when the firm is close to financial distress.*

4. *The model shows that creditors may prefer to support the appointment of independent directors even if these directors are fully aligned with shareholders.*

Informal theories 1 and 2 assume that creditors and/or shareholders believe that board independence is always value-enhancing, a belief that has no credible empirical or theoretical support. Literally hundreds of empirical papers on corporate boards have failed to find convincing evidence that board independence is value increasing (see e.g. the review article by Adams, Hermalin and Weisbach, 2012). Many theories of boards emphasize the costs and benefits of board independence (e.g. Adams and Ferreira, 2007; Harris and Raviv, 2008).

Our model highlights that an increase in board independence after creditors acquire control may be due to creditors' preference for conservative investments. It does not imply that the firm had bad governance before the violation. In fact, in our model creditor influence on board appointments may be value-destroying ex post (as it maximizes the market value of debt, which is not the same as maximizing the total value of the firm).

Exactly because they are informal, informal theories 1 and 2 do not explain why creditors care about board independence. Our model instead provides a possible link between capital structure and board structure, which has not been studied before.

Informal Theory 1 is in many aspects indistinguishable from our model. Our model could instead be seen as a particular formalization of Informal Theory 1, but one that does not require board independence to be associated with "good governance."

Informal Theory 2 is a bit of a straw-man argument against the hypothesis that creditors influence board composition. The fact that shareholders may decide to change board composition after covenant violations does not imply that creditors have no influence on this decision. Although in reality only shareholders have the formal rights to appoint directors, creditors can use their contractual rights to accelerate loan repayments to influence such decisions. Informal Theory 2 also leaves unexplained why shareholders would wait until covenants are violated to intervene and change the board. Its assumption that creditors play no formal or informal role in shaping the board is also difficult to reconcile with the existing formal and anecdotal evidence that empowered creditors often push for governance changes (e.g. Gilson (1990); Baird and Rasmussen (2006); Nini, Smith, and Sufi (2012); Becker and Strömberg (2012)).

# 2 Data

We start with nonfinancial firms in the Investor Responsibility Research Center (IRRC) database between 1994 and 2008. We obtain board and governance data from the IRRC for the 1996-2008 period. We obtain accounting and segment data from Compustat and stock returns from CRSP. CEO compensation and tenure data are from ExecuComp.

We then merge this sample with covenant violations data obtained from the Reuters Loan Pricing Corporation's (LPC) DealScan database for the 1990-2007 period, but only consider covenant violations starting in 1994 (the first year for which board composition data are available). The DealScan database contains information on syndicated loans made by banks to firms. It provides information about loan amounts, maturity, type of loan, syndication, covenants, and pricing, among others. We restrict the sample to loans with information on the maturity and the spread over the LIBOR (all-in spread drawn), and we eliminate loans for which we do not have any covenant information or which do not contain a covenant on current ratio, net worth, tangible net worth, and debt-to-EBITDA ratio. We then merge the loan data with firm and director data using company name, ticker, and loan origination date.[10]

We identify whether a firm violates any of the covenants at any point in time, and how far away the relevant accounting variables are from the covenant threshold. More specifically, for each loan, we first obtain covenant thresholds on current ratio, net worth, tangible net worth, and debt-to-EBITDA ratio. We assume that the firm is bound by the covenants at every quarter until the maturity of the loan. Since a firm might have more than one active loan at a given quarter with the same covenants, we use the minimum covenant threshold (maximum for the debt-to-EBITDA ratio) for each covenant across all active loans in a given quarter.

We then use Compustat data at quarterly frequency to compute accounting variables. If the accounting variable is below or equal to the covenant threshold, there is a covenant violation. In the case of the debt-to-EBITDA covenant, there is a violation if the accounting variable is above or equal to the threshold.

Since some of the relevant accounting variables are ratios and others are measured in dollars, we measure the distance to the covenant threshold as a proportion of the threshold. This allows us to compute a unique measure of the distance to the covenant threshold, which is given by the minimum distance to the threshold across the four covenants. We call this variable *binding distance (to threshold)* and it is defined as follows:

$$D_{it} \equiv \min_{j,k} \tilde{D}_{itjk}, \text{ where} \tag{30}$$

$$\tilde{D}_{itjk} \equiv \min_{z} \frac{C_{itjk} - T_{itjkz}}{T_{itjkz}}, \tag{31}$$

where $i$ and $t$ denote firm and year respectively, $j = 1, ..., 4$ denotes a quarter of year $t$, $k = 1, ..., 4$ denotes covenant type (one of the four covenant types), and $z$ denotes an active loan (a firm may have more than one loan with covenants). $T_{itjkz}$ is the threshold for active loan $z$, covenant type $k$, in quarter $j$ of year $t$ for firm $i$. $C_{itjk}$ is the quarterly value of the

[10] It is possible that a new loan is taken to refinance a previous one, but Dealscan provides limited information on whether a loan is a refinancing. We discard past loans that are active when a new loan occurs only if it is clear from Dealscan that the new loan is refinancing a previous loan.

accounting variable relevant for covenant $k$.[11]

In words, for firms not in violation of a covenant, we choose the lowest possible distance to a covenant threshold in a given year, normalized by the value of the threshold. If a firm is in violation of a covenant, we choose instead the highest distance between the accounting variables and the threshold, for those covenants that are breached. A violating firm is defined as a firm that violates one or more covenants in at least one quarter in a given year. For expositional simplicity, we allow $D_{it}$ to assume negative values; "negative distance" denotes cases in which the firm is in a violation state with respect to a least one covenant in at least one quarter of the year.

Our final sample contains 597 firms and 2,801 firm-year observations. For this sample, we find that 75.9% of the firms violate a covenant at least once during the sample period (453 firms), and 60% of the firm-year observations correspond to a covenant violation event (1,669 firm-year observations). We also find that 5% of the firm-year observations (139 firm-year observations) correspond to a change from a non-violation status in one year to a violation status in the subsequent year.

Our procedure for defining violations overstates the actual number of violations because we do not consider covenant threshold renegotiations. Denis and Wang (2013) show that covenant thresholds are often renegotiated when firms are close to the threshold and also that being close to the threshold increases the probability of covenants being relaxed. In their sample, about 50% of the contracts would be in violation if the original covenants were not relaxed. This number can explain our relatively high number of violations – 60% of the firm-year observations.[12]

As Denis and Wang (2013) also show, debt covenant renegotiations in firms that would be in violation are associated with changes in investment and financial policies. Their results suggest that creditors become more influential when a firm is sufficiently close to a violation, so that, without renegotiation, the firm would almost surely be in violation. Our measure of "violations" also includes cases in which violations did not occur because covenant limits were renegotiated. We want to include such cases because debt renegotiation is exactly the mechanism through which we expect creditors to influence firm choices. As a robustness check, we also consider a different sample in which only covenant violations filed with the

[11] For the debt-to-EBITDA covenant, $\tilde{D}_{itjk}$ is defined analogously by multiplying $C_{itjk} - T_{itjkz}$ in the numerator of the right-hand side of (31) by $-1$.

[12] Denis and Wang's (2013) unit of analysis is a contract, while ours is a firm-year. Firms in our sample often have more than one debt contract and it suffices to be in violation of one contract for a firm to be considered in violation according to our criteria.

SEC are included; we describe this sample below.

Table 1 presents descriptive statistics of the main variables used in our study from the DealScan sample. Table A.1 in the Appendix presents a detailed definition of the variables. For a director to qualify as independent, he must not be an employee, a former executive, or a relative of a current corporate executive of the company. In addition, the director must not have any business relations with the company.

The descriptive statistics in Table 1 are comparable to those in other studies using covenant data. The average firm has assets valued at \$3.54 billion, which is a bit higher than in Nini, Smith, and Sufi (2009) (\$3.26 billion) and Denis and Wang (\$2.68 billion). The statistics for the board variables are similar to those in other studies using IRRC data. The average board size (9.15) and the proportion of independent directors (6.39/9.15 = 0.70) are both slightly less than in Ferreira, Ferreira and Raposo (2011) – 9.82 and 0.75 respectively.

The median of the binding distance variable is negative, reflecting the fact that a violation occurs in 60% of the firm-year observations. The minimum and the maximum for the distance variable are quite extreme; e.g. the maximum distance in the sample is 9.44 (i.e. more than nine times the threshold value away from a violation), which is one order of magnitude larger than the distance in the 90th percentile (0.85). Even if these observations are not statistical outliers, it makes little economic sense to use them to estimate the effects of crossing a covenant threshold. Thus we mainly use subsamples that trim out observations that are very far from the threshold.

Table 2 shows descriptive statistics for variables $C_{itjk}$, $T_{itjkz}$ and $\tilde{D}_{itjk}$. The average current ratio covenant threshold is 1.41; 3.7% of the firm-year observations (105) correspond to a violation of this covenant. Violations of net worth and tangible net worth are more frequent: 20% (560) and 8.2% (231) of firm-year observations, respectively. The average value for the debt-to-EBITDA covenant threshold is 3.49. This is the covenant that firms are less likely to comply with, as 57% (1,597) of the firm-year observations correspond to a violation of this covenant.

Compared to Chava and Roberts (2008) and Nini, Smith and Sufi (2012), we have a higher fraction of firms and firm-year observations (in their case firm-quarter observations) with covenant violations. Chava and Roberts (2008) report that 37% of the firms violate the current ratio covenant and 31% violate the net worth (and tangible net worth) covenant, which corresponds to 15% and 14% of the observations, respectively. Nini, Smith and Sufi (2012) report that 40.5% of firms are in violation and 6.9% of observations are associated with a covenant violation, but they use SEC 10-Q and 10-K filings, rather than DealScan, to identify violations.

There are three main reasons that explain the differences between our descriptive statistics and those in Chava and Roberts (2008) and Nini, Smith and Sufi (2012). First, our sample is different from those in these related studies. Second, unlike Chava and Roberts (2008), we include in our analysis an additional covenant: the debt-to-EBITDA covenant. Third, due to the nature of our research question, we use annual instead of quarterly data. Thus in our case it is enough that a firm violates a covenant in just one quarter to be classified as a violating firm for the whole year.

As in Chava and Roberts (2008), we infer violations from threshold data and their respective accounting variables. This procedure may lead to coding and other unintended errors, as well as possibly overstating the number of actual violations (e.g., it could be that some of these covenants have been made redundant by other contracts that we do not observe; see e.g. Denis and Wang (2013)). It is unclear in what direction such errors would bias our results. In particular, the debt-to-EBITDA variable can be quite noisy, as it may vary across contracts according to the definition of debt (see Chava and Roberts (2008)). We simply assume that debt is equal to total debt (long-term debt and debt in current liabilities). In Denis and Wang's (2013) sample, this is the most common definition of debt for contracts that establish a debt-to-EBITDA limit. Because debt-to-EBITDA is the most frequent covenant in our sample, we effectively face an efficiency trade-off: using this variable substantially increases the variation in the sample, but it also adds noise. But ignoring this variable, given its ubiquity, would also introduce noise. We believe that there is no reason to expect that the measurement error in the debt-to-EBITDA covenant would bias our results in the direction of our predictions. In the robustness sections, we also run our main tests without this variable and we obtain qualitatively similar findings.

As an additional robustness test, we use an alternative data set (obtained from Amir Sufi's website), which was constructed using information from the 10-Q and 10-K filings on the SEC Edgar website. Using an algorithm described in Nini, Smith, and Sufi (2012), they identify financial covenant violations for a large number of publicly traded firms. They construct an indicator variable of whether or not the firm reports a violation of a financial covenant during the corresponding quarter.

The database of covenant violations reported in SEC filings is also compiled at quarterly frequency. As before, we define a violating firm as one that violates a covenant in at least one quarter at a given year. The sample that results from merging the covenant violation data with our initial sample contains 1,213 firms and 7,348 firm-year observations. This sample has the advantages of using only violations filed with the SEC and of being much larger. The main disadvantage is that we cannot calculate the distance to violation.

From this sample, we calculate that 20.3% of the firms violate a covenant at least once during the sample period (246 firms), and 5.5% of the firm-year observations correspond to a covenant violation event (402 firm-year observations). Table 3 presents descriptive statistics of the variables used in our study for the SEC sample. The descriptive statistics in Table 3 are comparable to those in Table 1.

# 3 Graphical Analysis

Figure 1 (in the introduction) illustrates our main result. To construct this figure, we use all firm-years to calculate annual cross-sectional averages of the number of independent directors, where years are measured in "event time" (i.e., zero is the year in which a violation occurs). We find that the number of independent directors increases significantly following a covenant violation. The effect is not immediate and persists for four years. This is expected because usually directors can only be replaced at regular intervals of no less than one year, and often up to three years, as is the case of firms with staggered-board provisions in their charters.[13]

The magnitude of this effect appears small at first glance, but it is quite large compared to the effects usually found in the board literature.[14] As we will show later, once we control for other confounding effects, these estimates become much larger.

[13]The replacement of directors is regulated by the state corporate law and the company's charter and bylaws. Normally, directors can only be replaced once a year at the shareholder meetings (sometimes once in every three years, if the board is "staggered"), and new appointments have to be nominated well in advance of the annual meetings. Such rules often imply a significant lag between the decision to appoint a new director and its actual implementation. For a detailed description of the rules governing the appointment of board directors, see Ferreira, Kershaw, Kirchmaier, and Schuster (2012).

[14]In virtually all regressions of board independence on firm characteristics in the existing literature, the economic significance of the estimated effects are almost always small. For example, Boone, Field, Karpoff, and Raheja (2007) report that a one-standard deviation increase in firm size is associated with a 1.79 percentage point increase in board independence (measured as the fraction of independent directors over the size of the board), which corresponds to about an one-tenth increase in the number of independent directors (for an average board size of seven directors). The economic effect of other important determinants of board independence (e.g., firm age, number of business segments, CEO tenure and ownership) is similar. Ferreira, Ferreira, and Raposo (2011) conjecture that these small magnitudes come from a combination of measurement errors due to imperfect proxies for the fundamental variables and heterogeneity in how firms change boards as the environment changes.

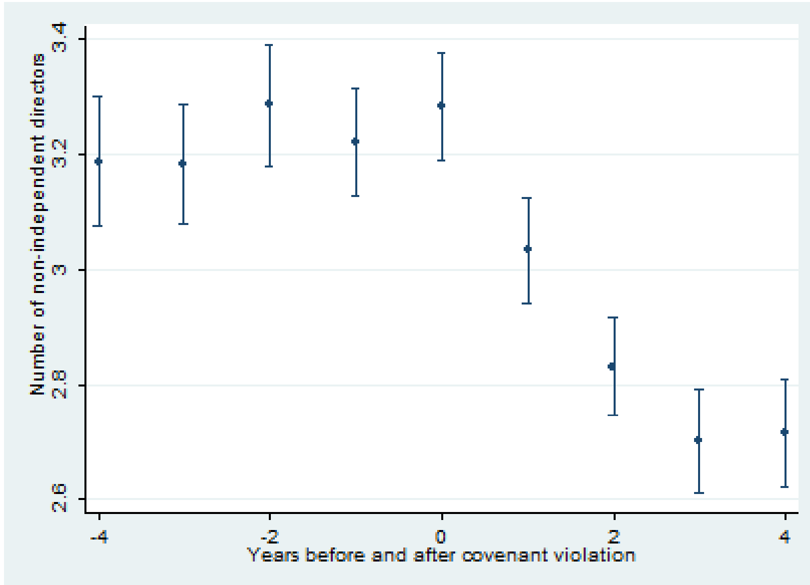


Figure 2: Non-Independent Directors before and after Covenant Violations

Figure 2 shows the how the number of non-independent directors evolves in event time. The average number of non-independent directors shows no clear trend in the years leading to a violation, and drops from 3.3 in Year 0 to 2.7 in Year 4. The combined evidence in Figures 1 and 2 shows that board independence unambiguously increases after a covenant violation.

One problem with our analysis so far is that firms enter and exit the sample, and sample selection criteria (of which survivorship is an example) may be correlated with the time-distance to a violation. For example, suppose that firms with more independent directors are more likely to survive after a covenant violation. Then the higher average levels of independence after a violation could be due to firms with low independence levels dropping out of the sample. To address this issue, Figures 3 and 4 show the number of independent and non-independent directors using only observations from firms that remain in the sample for nine years around a violation. We see the same pattern as before. Confidence intervals are wider because the sample is smaller, but, economically, the effects are even slightly larger. The evidence suggests that board independence increases after a firm violates a loan covenant.

We now consider how far away a given accounting variable is from the value in which a violation is triggered. We use the measure of the distance to violation that is explained in the data section. Figure 5 plots nonparametric regressions of the number of independent

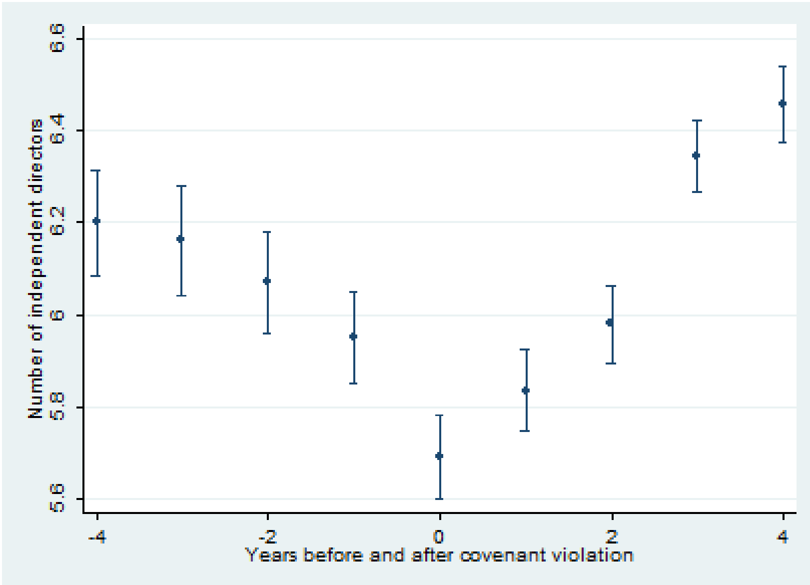


Figure 3: Independent Directors before and after Covenant Violations: Stable Sample

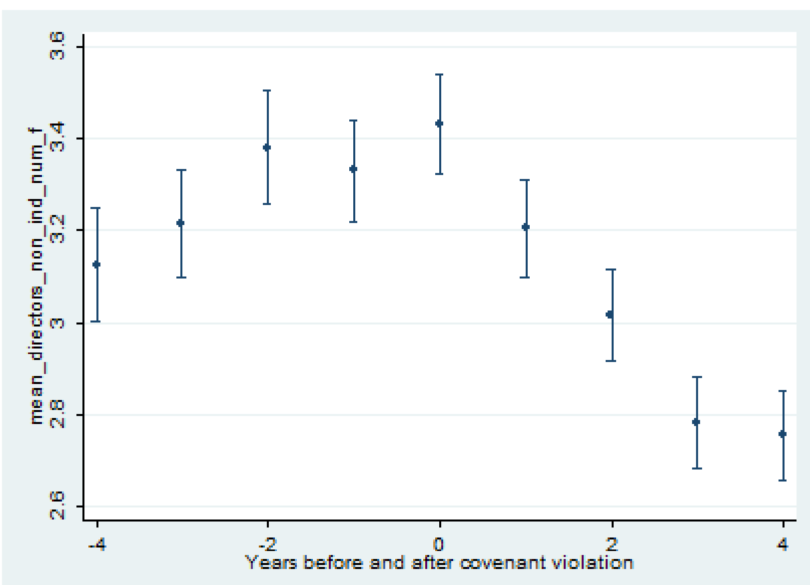


Figure 4: Non-Independent Directors before and after Covenant Violations: Stable Sample

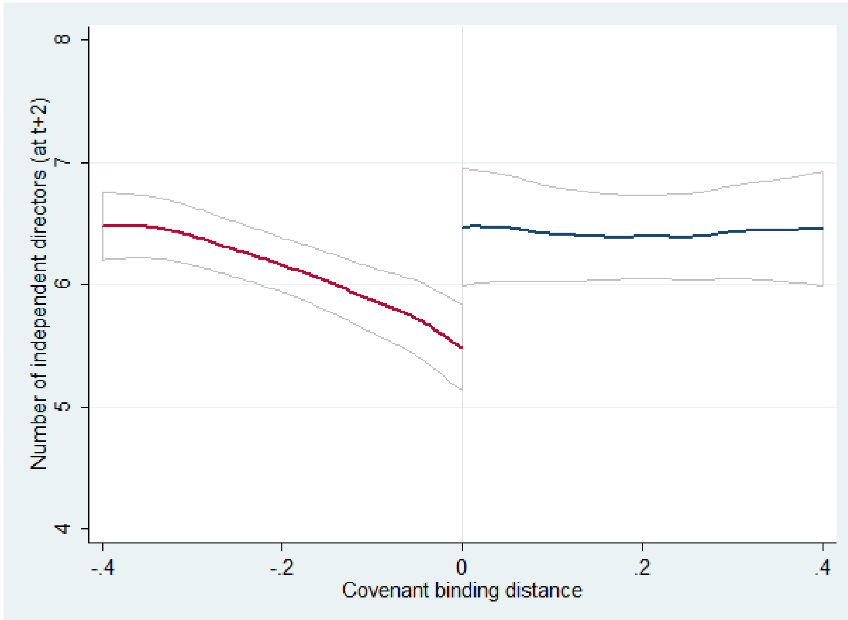


Figure 5: Nonparametric regression of number of independent directors on binding distance

directors on (the negative of) the distance to the covenant threshold. We run separate regressions for each side of the threshold. We measure the dependent variable at year $t+2$ as the board composition cannot be immediately adjusted. The thick lines are fitted regression lines and the thin lines represent 95% confidence intervals. The regression is performed using an Epanechnikov kernel with a bandwidth of 0.1. Negative values on the x-axis represent a non-violation and positive values represent a violation. Only observations in the interval $[-0.4, 0.4]$ are shown.

Figure 5 shows a clear discontinuity at the threshold: the average number of independent directors increases by one director after a violation. Consistent with Figures 1 and 3, in which the number of independent directors shows a negative trend in the years leading to a violation, Figure 5 shows that the number of independent directors also declines as the firm approaches a violation threshold. Although our model is too simple to generate predictions on how independence in regions close to a covenant violation should behave, we note that the logic of our model is consistent with a decrease in independence just prior to violations. In our model, shareholders have stronger risk-shifting incentives in low cash flow states and thus would like to reduce board independence even further in those states (this is shown formally in Step 4 of the proof of Proposition 2). Thus, we would expect to see lower levels of board independence exactly in those states of low cash flows but in which control is still in the hands of shareholders, that is, in regions that approach the covenant threshold from

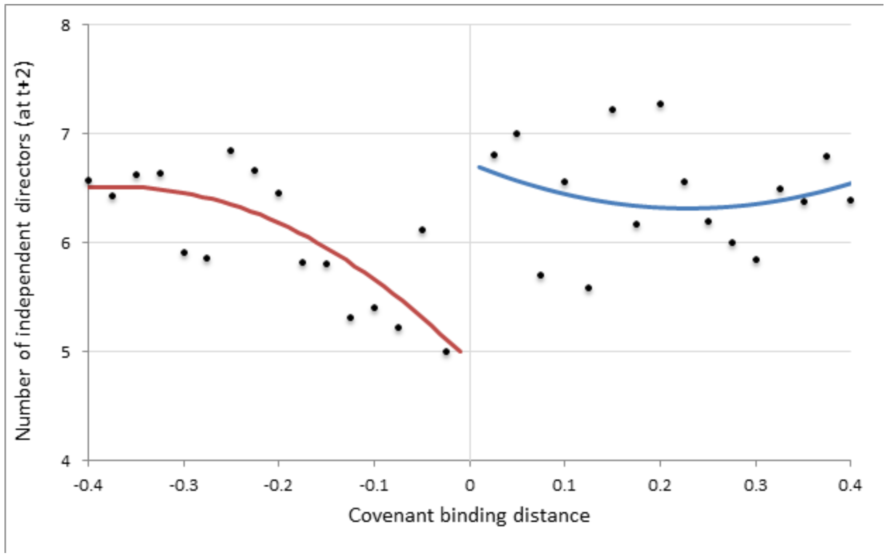


Figure 6: Independent directors: Second-order polynomials and averages for bins of size 0.025

the left.

Figure 6 shows the estimated regression lines of second-order polynomials at each side of the threshold. These regressions use only observations in an interval of 40 percentage points around the threshold ($D_{it} \in [-0.4, 0.4]$). This approach is the closest one to the more rigorous analysis that we present in the next section. As we discuss later, all results are robust to the choice of polynomial order and to the size of the interval around the threshold. Each circle represents the average value of the outcome variable for bins of size 0.025 each. Figure 6 suggests a clear discontinuity at the threshold. A covenant violation appears to increase the number of independent directors by almost two directors.

Figures 7 and 8 replicate the previous two figures for the number of non-independent directors. In Figure 7, a covenant violation appears to reduce the average number of non-independent directors by 0.5. There is a (small) overlap between the two confidence intervals, suggesting that this effect is less statistically precise than the one for the number of independent directors. Figure 8 suggests, as before, that if anything the number of non-independent directors decreases after a violation, but the evidence for a discontinuity at the threshold is weak. This is indeed confirmed in the parametric analysis below: the results for independent directors are more statistically precise than those for non-independent directors.

Overall, the graphical evidence supports the hypothesis that independence increases after covenant violations. There are more independent directors in the years following a violation.

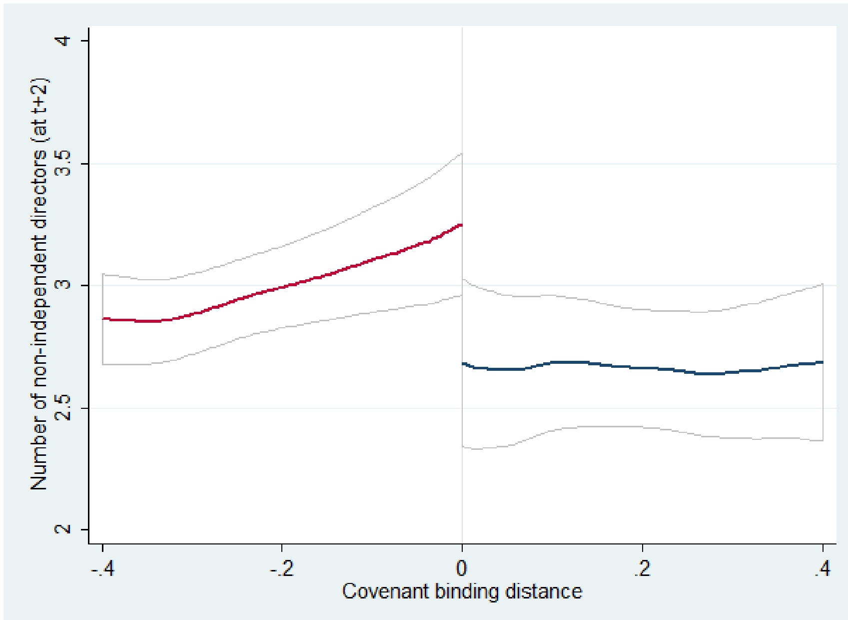


Figure 7: Nonparametric regression of number of non-independent directors on binding distance

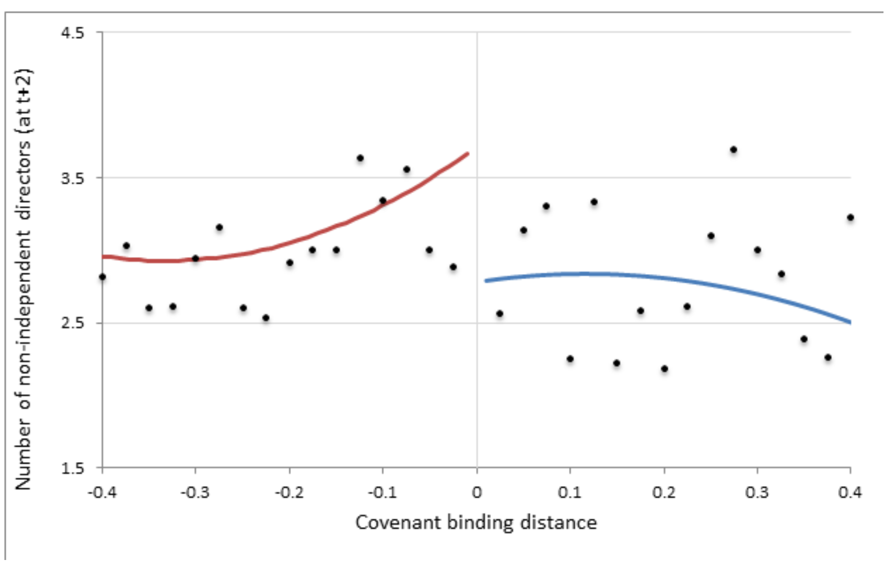


Figure 8: Non-independent directors: Second-order polynomials and averages for bins of size 0.025

There are more independent directors in firms that violate covenants by small margins than in non-violating firms that are close to covenant thresholds. The evidence here seems very strong. There is somewhat weaker evidence consistent with fewer non-independent directors in years following a violation and in firms that violate covenants by small margins.

# 4 Main empirical results

## 4.1 Empirical challenges

Our goal is to estimate the average effect of a covenant violation on board composition, for those firms that have loans with restrictive covenants. The most serious difficulty is the possibility that a spurious relationship between covenant violations and board independence is explained by (i) omitted variables or (ii) sample selection issues (e.g. survivorship biases). A third issue – reverse causality – is less plausible: it requires that expectations of future changes to board composition affect the current likelihood of covenant violations. We argue below that this is hard to reconcile with the evidence that current and past board composition is unrelated to covenant violations.

To reduce firm heterogeneity around covenant thresholds, we focus mostly on results obtained in small subsamples constructed as narrow windows around the threshold. As in typical regression discontinuity designs, we also control for high-order polynomials of the distance to threshold, which here works as the running (or "forcing") variable. However, this approach is arguably not sufficient for dealing with firm heterogeneity in our particular application. There are at least four issues that potentially invalidate the application of a traditional regression discontinuity design to our problem:

*1) Sample selection.* The probability of firms exiting or entering the subsamples around the threshold may be correlated with board composition.

*2) Violations may directly affect the distance to threshold.* After covenant violations, if a firm takes actions that improve the underlying accounting variables, the firm may exit the violation subsample quickly, creating an unbalanced distribution of observations on either side of the threshold.

*3) The use of ratios as running variables.* To understand this problem, consider for example the debt-to-EBITDA variable. Most of the variation of this variable comes from its denominator, because earnings vary more than total debt levels. Because debt-to-EBITDA is a convex function of EBITDA, for the same amount of variation of EBITDA, this ratio will vary more when it is initially low than when it is initially high. Thus, by construction, observations in violation of this covenant are more likely to be far from the threshold than observations that are not in violation. This mechanical effect means that any narrow window that is symmetric around the threshold is more likely to contain observations not in violation than observations in violation.

4) *Covenant thresholds are different for different firms.* Although we normalize all covenant thresholds to make them comparable across firms, the underlying thresholds are different for different firms. Thus, the effects of violating a covenant might differ across firms, because the breach of a tight covenant might have different implications than the breach of a not-so-tight one. A further complication arises also because covenant thresholds are endogenously chosen. For evidence on these issues, see Demiroglu and James (2010).

To address these potential problems, we proceed as follows. First, we use firm-fixed effects in all of our specifications. Firm-fixed effects address the most obvious selection problems as well as time-invariant omitted variables. Because of the fixed effects, we prefer to interpret our analysis not as a regression discontinuity design, but as an event study or a before-after analysis, in which the proximity to the event threshold is used (in a nonlinear fashion) to control for firm heterogeneity.

Second, we show that, although the observations are indeed more likely to cluster on one side of the threshold, most observable firm characteristics are either similar on both sides or fully "explained" by the distance to threshold variable. In contrast, future levels of board independence are affected by the covenant violation status and not fully explained by the distance to threshold.

Finally, if spurious relationships are created by omitted variables that may jump discontinuously, but not always exactly at the covenant thresholds, we would expect to find similar results for at least some thresholds that do not coincide with the actual threshold. Such placebo tests provide the strongest evidence in favor of a causal interpretation of our findings.

## 4.2 Empirical model

Our most general specification is given by

$$y_{it} = \beta v_{it-2} + \sum_{p=1}^{P} \left[ \gamma_{p0} + \gamma_{p1} v_{it-2} \right] D_{it-2}^{p} + \alpha_t + f_i + \boldsymbol{\delta} \mathbf{x}_{it-2}' + \varepsilon_{it}, \tag{32}$$

where $y_{it}$ is either the logarithm of the number of independent directors or the logarithm of the number of non-independent directors, $v_{it}$ is an indicator variable that takes the value of one if firm $i$ is in a violation state in year $t$ (i.e. $v_{it} = 1$ if $D_{it} \leq 0$), $\sum_{p=1}^{P} \left[\gamma_{p0} + \gamma_{p1} v_{it}\right] D_{it}^{p}$ is a polynomial of order $P$ of the distance to threshold, where coefficients $\gamma_{p0}$ and $\gamma_{p0} + \gamma_{p1}$ can differ on the left and right hand sides of the threshold, $\alpha_t$ denotes year fixed effects, $f_i$ denotes firm fixed effects, and $\mathbf{x}_{it-2}$ is a vector of covariates. Standard errors are clustered by firm. All explanatory variables are lagged two years as changes in board structure occur slowly. As some of the covariates in $\mathbf{x}_{it-2}$ might nevertheless be endogenous, our main focus is on specifications without $\mathbf{x}_{it-2}$. The coefficient of interest is $\beta$.

We initially consider two intervals around the threshold (the *windows*): a narrow interval of 40% ($D_{it-2} \in [-0.4, 0.4]$), which we call *Subsample 1*, and a wider interval of 80% ($D_{it-2} \in [-0.8, 0.8]$), which we call *Subsample 2*. Subsample 1 is small; it contains 332 observations (12% of the full sample). Subsample 2 contains 884 observations (32% of the full sample).[15] We postpone to Subsection 4.5 the discussion of the robustness of the results to alternative window widths.[16]

## 4.3 Preliminary analysis: Descriptive statistics

Table 4 presents average values for some selected variables on each side of the threshold, for Subsample 1 (Panel A) and Subsample 2 (Panel B). In Subsample 1 (the narrow window), we find that the average number of independent directors at $t + 2$ is about 0.62 larger in firms that violate a covenant at $t$ than in firms that do not violate a covenant at $t$. Similarly, the average number of non-independent directors at $t + 2$ for firms that violate a covenant is about 0.44 lower than that of those firms that do not violate a covenant. Thus, covenant violations appear to be related to higher board independence in the future. We note that this effect does not appear to be a "fixed effect": the average number of past and current independent directors is virtually the same on either side of the threshold. The same applies to the number of past and current non-independent directors.

Panel B shows results for Subsample 2 (the wide window). The differences are qualitatively similar, but economically and statistically weaker. This is to be expected: as we move

[15] We drop observations from firms that appear in these samples in only one year; the reported number of observations thus includes only observations that are not fully "explained" by firm fixed effects.

[16] Bakke and Whited (2013) rightly warn against generalizing from estimated effects that are present in only a small proportion of the sample. Although such effects may have strong internal validity, they may only apply to a small set of firms. This is not a concern in our application, however, because the effects predicted by our theory only apply to firms that cross the threshold. In other words, we do not use a threshold event as an identification tool for testing a broader theory (e.g. cash flows and investment); our theory is explicitly about the effect of the threshold on the outcome variable.

away from the threshold, the confounding effects of other variables are likely to become more important.

The typical regression discontinuity design implies that observations around the threshold are (as good as) random, thus if the threshold window is sufficiently narrow, we should expect an equally balanced sample size on each side of the threshold. Table 4 shows that the samples on each side of the threshold are not balanced. The split between $v_{it} = 0$ and $v_{it} = 1$ is about 67%-33% in both Subsample 1 and Subsample 2.

One possible reason why observations cluster on one side of the threshold is manipulation: firms may manipulate earnings to avoid crossing the threshold. This is a problem if earnings manipulation is related to board independence.[17] Other (perhaps more realistic) reasons for the observed imbalance are those previously discussed in Subsection 4.1.

Table 4 shows evidence consistent with more profitable and valuable firms being more likely to stay in the sample after a covenant violation: firms in the violation sample have higher market to book ratios, ROA and free cash flow ratios. The differences are statistically significant for these three variables in both panels. However, as we discuss later, the distance to violation variable captures most of the observed heterogeneity in these variables, with the sole exception of the free cash flow variable: Free cash flows increase because capital expenditures fall after covenant violations, as previously documented by Chava and Roberts (2008). Thus, differences in free cash flows on either side of the threshold are not due to firm heterogeneity; instead, they are more likely another consequence of covenant violations.

We note that there are no statistically significant differences in the other firm characteristics, with the exception of CEO tenure.[18] In particular, violating firms are similar to non-violating firms in the number of past and current independent and non-independent directors. Thus, although there is some evidence of sample selection on measures of firm performance, there is no evidence of sample selection on board characteristics.

We use the panel nature of our data to mitigate concerns about the nonrandom nature of the subsamples to the right and to the left of the threshold. By including firm fixed effects in all of our regressions, we ensure that our results are driven by those firms that are found on both sides of the threshold. This comes at some loss of external validity: our results are valid only for those firms that can be observed both in state $v_{it} = 0$ and in state $v_{is} = 1$, where $s \neq t$. This may be a nonrandom sample of firms; e.g. firms that manipulate

[17] Chava and Roberts (2008) provide various arguments and tests suggesting that accounting manipulation to avoid covenant violations is both unlikely and difficult to implement (see also Roberts and Whited, 2013).

[18] Nini, Smith and Sufi (2012) find that CEO turnover increases after covenant violations, which can explain why CEO tenure is lower in the violation sample.

earnings aggressively may have a low probability of entering this sample and influencing our estimates. But the combination of fixed effects and the use of observations near the threshold goes a long way toward mitigating concerns about omitted variables. With fixed effects, our key identification assumption is that firms' propensity to manipulate earnings to avoid covenant violations is not correlated with their expectations of an imminent increase in board independence. Although we cannot test this assumption, it is certainly a plausible and not too demanding assumption. It is important to note that we do not need manipulation to be random. Manipulation could be related to time invariant firm characteristics (or to changing characteristics included in our regressions).

## 4.4 Main Results

Table 5 reports our main results. In Column (a) we present the estimate of $\beta$ with the log of the number of independent directors as the dependent variable. This regression uses Subsample 1. The regression includes firm fixed effects, year dummies, and a second-order polynomial on each side of the discontinuity. The estimated $\beta$ is positive and statistically significant. A covenant violation leads to an increase of $0.3 \times 6.4 = 1.9$ independent directors, evaluated at the (full) sample average of the number of independent directors of 6.4.[19] This effect is about three times larger than the one found in Panel A of Table 4, which suggests that the inclusion of controls (firm fixed effects, time dummies, and the binding distance) makes the effect of covenants on independence appear more pronounced. We also note that these effects are economically important: firms in our sample have 6.4 independent directors on average, thus a change of 2 directors is not trivial. This is in contrast to most of the empirical literature on boards, where the economic effects of variables that influence board composition are usually quite small (see the discussion in Ferreira, Ferreira, and Raposo (2011)).

In Column (b) we include a long list of potential covariates in the regression: firm size, firm age, leverage, number of business segments, market-to-book ratio, R&D-to-assets ratio, free cash flow, return on assets, the GIM governance index, and CEO ownership and tenure. All these variables are lagged by two years relative to the dependent variable. The estimated $\beta$ is virtually identical to that in Column (a), which suggests that omitted variables are unlikely to be a concern. One potential concern here is that these firms characteristics

[19] This is a local average effect and thus expected to be valid only for those firms in our sample (and others like them) that can be found at both sides of the threshold. These firms are (i) heavy users of bank debt with covenants and (ii) likely to remain solvent and financially healthy after covenant violations.

may be jointly determined with the expectation of future changes in board composition. It is nevertheless reassuring that the inclusion of these variables does not seem to affect the estimates in an economically meaningful way. As an alternative approach to understanding the potential impact of these control variables, in Subsection 4.6 we investigate the direct effect of covenant violations on these variables.

Another potential problem with our empirical strategy is that, by using the variable $v_{it}$ in (32), we are implicitly assuming that changes from $v_{it} = 0$ to $v_{it} = 1$ affect $y_{it}$ in the same way that changes from $v_{it} = 1$ to $v_{it} = 0$ do. This assumption is unreasonable because, while the former leads to a covenant violation, the latter does not reverse an earlier violation. Our interpretation of the results would be under suspicion if the results were mostly driven by changes from 1 to 0. To address this issue, in Column (c) we replace variable $v_{it}$ with

$$\widetilde{v}_{it} = \{1 \text{ if } v_{it} = 1; 0 \text{ if } v_{is} = 0 \text{ for all } s \leq t; \text{ missing otherwise}\}. \qquad (33)$$

That is, $\widetilde{v}_{it}$ only considers changes from $v_{it} = 0$ to $v_{it} = 1$. We find a larger estimate for $\beta$ (0.41) and a substantially larger marginal effect of 2.62 new directors (evaluated at the sample mean). This evidence reinforces our interpretation. The evidence is consistent with a substantial restructuring of the board after covenant violations, as they typically lead to the (net) addition of two or more independent directors.[20]

For the sake of comparison, we also estimate the same regressions without firm fixed effects, in which case we include two-digit SIC dummies to control for industries (the results are not tabulated). Our estimates for Columns (a) - (c) are 0.45 (t-stat 2.98), 0.30 (t-stat 2.63), and 0.35 (t-stat 2.55), respectively. Thus, the firm fixed effects do not appear to affect the estimates significantly, especially once we introduce firm-level controls (as in Column (b)). In the rest of the paper, we only present the fixed-effects estimates, as these are free from the most obvious sample selection and omitted variables biases, but we note that none of our fixed-effects estimates significantly differ from the estimates obtained by pooled OLS with two-digit SIC dummies.

In Columns (d)-(f) we replicate the previous regressions using the logarithm of the number of non-independent directors as the dependent variable. We find that covenant violations also increase board independence by reducing the number of non-independent directors on

[20] To deal with the potential asymmetry between changes in each direction, we also considered two additional variations of the violation dummy. The first one drops only the first two observations after a change from $v = 1$ to $v = 0$. With this variable, the estimated $\beta$ is 0.40 (t-stat 2.48 and 277 observations). The second definition drops all observations (both zeros and ones) after a first change from $v = 1$ to $v = 0$. With this variable, the estimated $\beta$ is 0.38 (t-stat 2.16 and 233 observations).

the board of directors. As before, we find that the inclusion of firm-level controls does not have a meaningful effect on the estimates of $\beta$ and that using $\widetilde{v}_{it}$ leads to stronger results. The marginal effects on the number of non-independent directors range from 0.6 to 1.4 fewer directors, evaluated at the sample average of 2.8.

Table 6 replicates the regressions in Table 5 using Subsample 2 (observations within 80% of the threshold). This is a larger subsample, which accounts for about one third of the full sample. This larger sample may be justified because it is not uncommon for accounting variables to oscillate by more than 40% of the covenant threshold in one year. Thus, we can have a much larger number of firms changing from 0 to 1 at least once in the sample. The drawback is that, in a larger sample, nonlinearities are more pronounced. We now use two fifth-order polynomials of the distance to threshold (one on each side) in an attempt to capture any nonlinearities in the relation between the assignment variable (the accounting variables) and the outcome variable.

For the number of independent directors, we find results that are very similar in magnitude and statistical precision to those found in Subsample 1. For the number of non-independent directors, the estimates all have the correct sign, but their magnitudes and statistical precision are smaller than those using Subsample 1. We conclude that the evidence that independence increases because of the appointment of new directors is much stronger – economically and statistically – than the evidence that non-independent directors are fired after covenant violations. We discuss this point further in Subsection 4.5.

For completeness, Table 7 presents the results obtained with the full sample. A larger sample allows us to use polynomials of a higher order to obtain more reliable estimates. We now use a polynomial of order 10 on each side of the discontinuity. We still find effects that are statistically significant for the number of independent directors. The magnitude of this effect is about one director. As in Table 6, there is no statistically reliable effect of covenant violations on the number of non-independent directors.

We summarize our main findings as follows: (1) There is strong evidence that covenant violations lead to an increase in the number of independent directors; these effects are economically important; (2) there is weak evidence that covenant violations lead to a decrease in the number of non-independent directors; and (3) the estimated effects appear economically stronger when using observations near to the covenant threshold.

## 4.5 Robustness to Polynomial Order and Window Width

Roberts and Whited (2013) recommend experimenting with different polynomial orders and window widths to check whether the results are robust. In Table 8 we report the estimates of $\beta$ for a combination of different window widths and polynomial orders, using the (log of the) number of independent directors as the outcome variable. To allow for more flexibility in polynomial choice, we do not include other firm-level characteristics, but the results are very similar if we include them; as we can see from Tables 5 to 7, firm characteristics do not seem to affect much the estimate of $\beta$. We consider all polynomials of order 1 to 12. We also include two new subsamples with large window widths: Subsample 3 ($D_{it-2} \in [-4.5, 4.5]$) and Subsample 4 ($D_{it-2} \in [-8.5, 8.5]$). These new subsamples come very close to covering the whole sample and are thus better viewed as "quasi-full samples."[21]

Consider first Subsample 1 (the narrowest window). With a polynomial of order 1, the estimated $\beta$ is 0.14 and statistically significant. With our preferred specification (order 2), this effect becomes 0.3. For higher polynomials, this effect is no longer sensitive to the order of the polynomial: the estimate ranges between 0.29 to 0.36. Subsample 2 shows a very similar pattern: once the polynomial order is sufficiently high (in this case, order 4), the estimated effect gets close to 0.3 and then it is no longer significantly affected by the order of the polynomial.

In Subsample 3, which includes about 90% of the observations, the estimated $\beta$ increases almost monotonically with the polynomial order, and only reaches values close to 0.3 for a polynomial of order 12. Both Subsample 4 (95% of the observations) and the full sample are very similar, and the estimated effects become stronger as the polynomial order increases, but never become as high as 0.3.

We conclude that the effect of covenant violations on the number of independent directors is very robust. Higher order polynomials tend to make this effect stronger, which suggests the existence of a real discontinuity, rather than some steeply-sloped relation between the assignment variable and the outcome variable. The economic magnitude of this effect is stable across sample sizes; it falls a little only when we include observations that are very far from the threshold, and which account for about 10% of the sample.[22]

[21] We also tried narrow windows from $[-0.3, 0.3]$ to $[-1, 1]$ and obtained very similar results to those in Subsamples 1 and 2. There is nothing special about our chosen windows.

[22] We have also replicated Table 8 with two-digit SIC dummies instead of firm fixed effects (results not tabulated). Overall, the results are very similar, with a few differences. In Subsamples 1 and 2, the estimated $\beta$, although always positive, displays more (non-monotonic) variation with polynomial order. In Subsample 1, the estimated $\beta$ is statistically significant for polynomials of order 1, 2, 3 and 5, and the point estimates

As Tables 5 to 7 show, the estimated $\beta$ for non-independent directors is not robust to window width. It should come as no surprise that this effect is also not robust to polynomial order. As an example, the estimated effect in Table 5 becomes much smaller and statistically insignificant if a polynomial of order 3 is used instead of order 2. Overall, a replication of Table 8 (results not tabulated) shows that most point estimates are negative, but economically small and statistically insignificant.

## 4.6 Covenant violations and other firm characteristics

In this subsection we consider the effects on covenant violations on additional firm characteristics. For each (non-board) characteristic in Table 4, we replicate the full set of regressions as in Table 8. Here we report just a summary of these results.

We find that, unlike in Table 4, covenant violations do not appear to have a significant effect on ROA and market-to-book once fixed effects, year dummies and the distance to default variables are included in the regressions. The estimated effects for these two variables are always economically small and statistically insignificant for all polynomial orders and window widths.

In contrast, the estimated effect of covenant violations on the free cash flow variable ranges from 0.03 to 0.05 in most specifications using Subsamples 1 and 2, and these effects are often statistically significant. Because ROA seems to be unaffected by covenant violations, the effect on free cash flow must be due to a negative effect of violations on capital expenditures. In fact, we find that covenant violations appear to have a negative effect of one or two percentage points on capital expenditures over assets (these effects are statistically stronger in Subsample 1, for polynomials of order 3 to 6).

The book value of assets also appears to be larger for firms in violation. This is more likely to be a selection effect; larger firms and firms with more tangible assets may be more likely to remain in our sample after a covenant violation.

For all other variables, we do not find any effect that is robust across specifications. The vast majority of estimated effects are statistically weak.

We conclude that there are only two firm characteristics (among the set of variables that

vary from 0.12 (order 10) to 0.45 (order 2). In Subsample 2, the estimated $\beta$ is statistically significant for all polynomial orders, with the exception of orders 5 and 6, and the point estimates vary from 0.11 (order 1) to 0.38 (order 11). Subsamples 3 and 4 and the full sample display on average larger point estimates than the ones obtained in fixed-effects regressions, with statistically significant (at 1%) point estimates close to 0.3 for all polynomials of order 6 or higher.

we consider) that appear to be related to covenant violations: free cash flow and the book value of assets. The effect on the first variable is explained by a fall in capital expenditures. The most likely explanation is that covenant violations reduce capital expenditures, as documented in previous studies (e.g. Chava and Roberts (2008)). The effect on the book value of assets suggests that sample selection is not random. Thus, "controlling" for firm size may be important. The results from the previous sections suggest however that controlling for firm size has negligible results on the estimated effects of violations on board independence. In fact, in the regressions in Tables 5 to 6, firm size does not seem to be robustly related to the appointment of new directors.

## 4.7 Placebo tests

To check for firm-specific trends or other confounding effects that appear to coincide with a covenant violation, we perform a series of tests with placebo thresholds. To do so, we create 10 different fake thresholds that are equally distant from each other. These placebo thresholds lie in the interval defined by $D_{it-2} \in [-0.5, 0.5]$, which includes the real threshold. For each placebo threshold, we first redefine the binding distance variable so that it becomes centered at the new threshold, and then we redefine Subsamples 1 and 2 accordingly. We run the exactly same regressions as in Tables 5 and 6 for each of the placebo thresholds.

We summarize the results in Figures 9 (Subsample 1) and 10 (Subsample 2). We present results using the specification in Column (a) of Tables 5 and 6 (no firm level controls).[23] On the $x$-axis we list the placebo thresholds, with 0.00% denoting the true threshold. The $y$-axis measures the estimated $\beta$. The vertical lines represent 95% confidence intervals. Clearly, we find that only the real threshold produces an estimate that is both economically and statistically significant (at 5%) and robust across samples.

We believe that these placebo tests provide the strongest evidence in favor of a causal interpretation of our findings. One key concern is that the number of independent directors may display naturally occurring discontinuities, either because board independence may display firm-specific trends (aggregate trends should be captured by the year dummies) or because there could be omitted variables related to board composition, and these omitted variables may also jump discontinuously. If either of these possibilities were true, we would expect to see a few other discontinuities in Figures 8 and 9. These figures suggest instead that we can only find a robust effect at the "real" threshold.

[23]The figures for the regressions with firm-level controls are virtually identical to Figures 9 and 10 and are omitted for brevity.

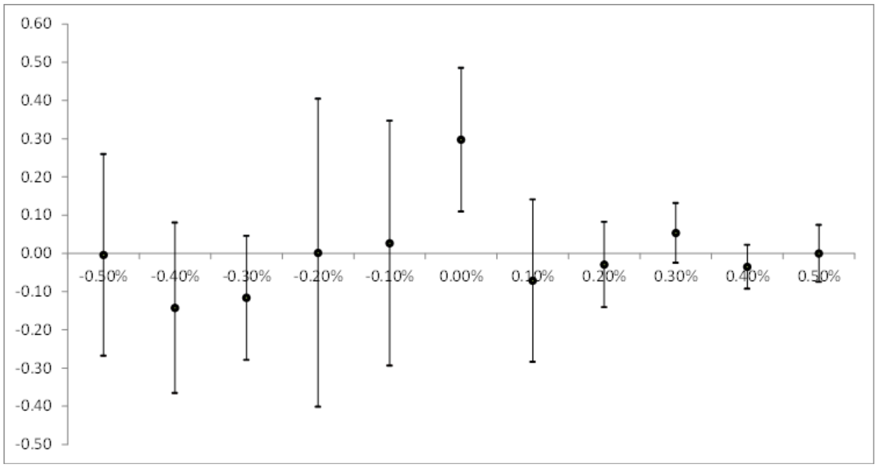


Figure 9: Placebo tests: Estimated effect of placebo violation on the (log) of number of independent directors (Subsample 1; real violation at 0.00%)

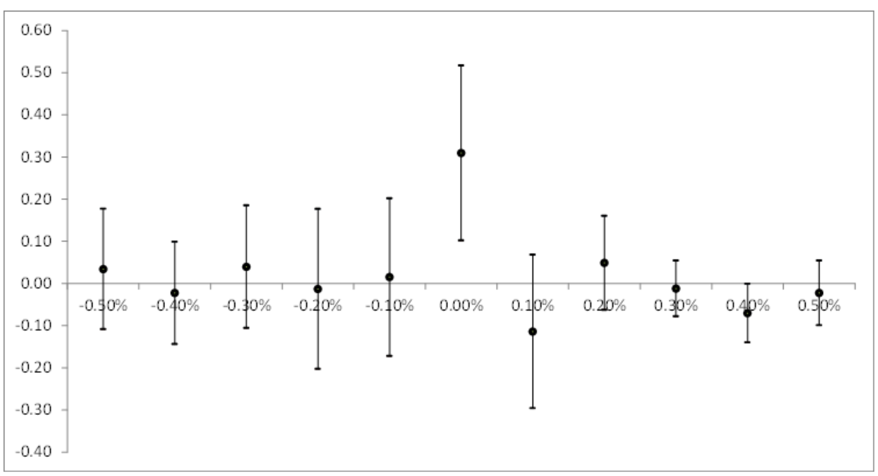


Figure 10: Placebo tests: Estimated effect of placebo violation on the (log) of number of independent directors (Subsample 2; real violation at 0.00%)

# 5 Further Robustness and Additional Evidence

In this section, we first present a number of further robustness tests and then we present some additional evidence.

As robustness tests, we first consider Poisson regressions which take into account the count nature of the data. These regressions assume that $y_{it}$ is independently Poisson distributed with conditional mean

$$E\left[y_{it} \mid v_{it-2}, D_{it-2}, \alpha_t, f_i\right] = \exp\left\{\beta v_{it-2} + \sum_{p=1}^{P}\left[\gamma_{p0} + \gamma_{p1} v_{it-2}\right] D_{it-2}^{p} + \alpha_t + f_i\right\}. \qquad (34)$$

Parameter $\beta$ is a semi-elasticity, which can be directly compared to those estimated using OLS and $\log y$. We report the results in the first three columns of Table 9. Poisson regressions yield marginal effects that are just a bit lower than those from log-linear regressions.

Next we consider the possibility that director turnover is just a consequence of CEO turnover. Nini, Smith and Sufi (2012) show that covenant violations lead to more CEO turnover. Thus it is possible that new CEOs bring new directors to the board. If this is the case, the effect of covenants on board independence is still causal, but perhaps less interesting because it could simply be a side effect of another result that has already been documented in the literature. To address this possibility, we drop from the sample all observations in which the CEO is replaced in the two years following a violation. We report the results in Columns (d) to (f) in Table 9. If anything, the estimated effects are a bit stronger when using only data from firms without CEO turnover.

Finally, we consider an alternative measure of the covenant violation dummy that does not use debt-to-EBITDA covenants. This measure requires us to ignore the existence of debt-to-EBITDA covenants in the definition of the violation dummy, which is likely to add noise to our estimates. We report our results in Columns (g) to (i) in Table 9. We still find estimates that are qualitatively similar to those in Tables 5 to 7, but statistically weaker. We conclude that the debt-to-EBITDA covenant is important for estimating the effects, but also that qualitatively similar results are found even if the debt-to-EBITDA covenant is ignored.

In an attempt to understand the mechanism better, we estimate heterogeneous effects that can vary across firms depending on the importance of bank debt (To be added).

Next, we use a different definition of covenant violations – from Nini, Smith and Sufi (2012) – which consider only those violations registered with the SEC. We again find support for a positive relation between covenant violations and board independence (To be added)

Finally, we present descriptive characteristics of directors appointed after covenant violations (To be added).

# 6 Conclusions

TBW

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# A Appendix: Risky Debt

The assumption that the optimal debt contract must be riskless makes the characterization of the equilibrium contract simpler. But this assumption is not necessary for the main result. Here we prove that, in the general case, whenever covenants are needed, board independence (weakly) increases after violations. Thus, because our goal is to study the relation between covenant violations and independence, restricting the analysis to (equilibrium) riskless debt is without loss of generality.

Suppose that an equilibrium debt contract implies that, conditional on $s_0$, the firm defaults with some strictly positive probability. We now prove a series of results.

**Lemma 1** *In any equilibrium in which debt is risky, the probability of default conditional on $s_0$ is either 1 or $1 - \pi_F$, where*

$$\pi_F \equiv \frac{\beta - \max\{A_0 - F, 0\}}{b^2 - \alpha - F + A_0}.$$

**Proof.** If debt is risky, either default occurs for sure if the state $s_0$ or it occurs only if the cash flow is low. If monitoring is $\pi > \pi_F$ then the CEO does not communicate and the cash flow is $A_0$. Thus default occurs with probability 1 if debt is risky and $\pi > \pi_F$. If there is communication, default only occurs in the state in which the CEO has control. If $\pi \leq \pi_F$ the CEO communicates with the board, and the optimal monitoring intensity is $\pi_F$. Thus default occurs with probability $1 - \pi_F$. ■

Intuitively, the equilibrium probability of default is positively related to the face value of the debt $F$. A higher $F$ means that the equity component of the CEO's utility is less valuable, thus the CEO is less willing to share information with the board to increase equity value. In order for the communication constraint to be met, the optimal monitoring level needs to be lower, which increases the probability that the CEO will be in control and thus the probability of default.

**Lemma 2** *Consider an equilibrium in which the face value of debt is $F$. If the equilibrium probability of default conditional on $s_0$ is $1 - \pi_F$, then there exists a payoff-equivalent equilibrium in which shareholders retain board control rights in state $s_0$.*

**Proof.** Suppose not. Then it must be that shareholders would choose $\pi \neq \pi_F$ if they retained board control.

Trivially, shareholders would never strictly prefer $\pi < \pi_F$ to $\pi_F$ because communication is achieved at $\pi_F$.

If $F > A_0$, choosing any $\pi > \pi_F$ would lead to certain default and zero equity value, and thus this action is dominated by $\pi_F$.

If $F < A_0$ and shareholders choose $\pi > \pi_F$, default does not happen, which means that creditors would also prefer $\pi > \pi_F$. Thus in this case an equilibrium with $1 - \pi_F > 0$ cannot happen under either shareholder control or creditor control. ■

**Lemma 3** *Consider an equilibrium in which the face value of debt is $F > A_0$ and default occurs with probability 1 conditional on $s_0$. Then there exists a payoff-equivalent equilibrium in which creditors gain board control rights in state $s_0$.*

**Proof.** Suppose not. Then creditors would prefer some $\pi < 1$ so that default no longer occurs with probability 1. But then shareholders also cannot commit to $\pi = 1$ because shareholders are always strictly better off when the probability of default is lower than 1. Thus $\pi = 1$ cannot be sustained in equilibrium. ■

These three results jointly imply that, if debt is risky, three possibilities may occur following $s = 0$: (i) creditors are given board control rights, in which case they choose $\pi = 1$, (ii) shareholders retain control rights and choose $\pi_F < 1$, or (iii) both shareholders and creditors agree to a level of monitoring $\pi_F < 1$ and the allocation of control is irrelevant.

Only under Case (iii) board independence may decrease after a covenant violation. But because the same outcome could also be implemented in a contract without covenants, this case has limited empirical appeal. In Case (iii), shareholder control and creditor control are equally likely (from a theoretical perspective) to be chosen in equilibrium, thus this case makes no directional prediction concerning covenant violations and board independence. In contrast, case (i) unambiguously predicts that board independence should increase after covenant violations.

## Table 1
## Summary Statistics: Main Variables

This table reports summary statistics for the main variables used in the regressions. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) firms from 1994 to 2007, with matched data on syndicated loans from DealScan. Financial industries are omitted (SIC codes 6000-6999). Board and governance data are taken from the IRRC database. Executive compensation data are from ExecuComp. Accounting and segment data are from Compustat. Stock return data are from the Center for Research in Security Prices. The covenant violation dummy is equal to 1 if the firm violates at least one out of the 4 covenants during the year in at least one quarter and equal to 0 otherwise. The binding distance is defined in the text. Refer to Table A1 in the Appendix for the remaining variable definitions. Financial ratios are winsorized at the bottom and top 1% level.

| | Mean | Standard deviation | Minimum | 10th percentile | Median | 90th percentile | Maximum | Obs. |
|---|---|---|---|---|---|---|---|---|
| Number of independent directors | 6.39 | 2.11 | 1.00 | 4.00 | 6.00 | 9.00 | 15.00 | 2801 |
| Number of non-independent directors | 2.76 | 1.65 | 1.00 | 1.00 | 2.00 | 5.00 | 13.00 | 2801 |
| Number of directors | 9.15 | 2.13 | 4.00 | 7.00 | 9.00 | 12.00 | 19.00 | 2801 |
| Firm size | 3541.82 | 11323.65 | 43.45 | 368.25 | 1231.07 | 7144.07 | 270634.00 | 2801 |
| Leverage | 0.25 | 0.16 | 0.00 | 0.02 | 0.25 | 0.45 | 0.87 | 2801 |
| Firm age | 22.56 | 17.42 | 1.00 | 6.00 | 17.00 | 42.00 | 81.00 | 2801 |
| Number of segments | 2.88 | 1.91 | 1.00 | 1.00 | 3.00 | 6.00 | 10.00 | 2801 |
| Market-to-book | 1.88 | 1.15 | 0.62 | 1.04 | 1.54 | 3.00 | 8.89 | 2801 |
| R&D | 0.02 | 0.04 | 0.00 | 0.00 | 0.00 | 0.06 | 0.37 | 2801 |
| Stock return volatility | 0.38 | 0.20 | 0.12 | 0.19 | 0.34 | 0.63 | 1.74 | 2801 |
| Free cash flow | 0.09 | 0.08 | -0.79 | 0.01 | 0.09 | 0.18 | 0.36 | 2801 |
| Return on assets | 0.15 | 0.08 | -0.66 | 0.07 | 0.14 | 0.25 | 0.44 | 2801 |
| Governance index (GIM) | 9.33 | 2.63 | 3.00 | 6.00 | 9.00 | 13.00 | 17.00 | 2801 |
| CEO ownership | 0.02 | 0.05 | 0.00 | 0.00 | 0.00 | 0.05 | 0.30 | 2801 |
| CEO Tenure | 7.51 | 7.51 | 0.00 | 1.00 | 5.00 | 17.00 | 49.00 | 2801 |
| Covenant violation dummy | 0.60 | 0.49 | 0.00 | 0.00 | 1.00 | 1.00 | 1.00 | 2801 |
| Binding Distance | -1.44 | 3.64 | -24.43 | -3.93 | -0.52 | 0.85 | 6.45 | 2801 |

**Table 2**

**Summary Statistics: Covenant Data**

This table reports summary statistics for the four covenant types used in this paper: current ratio, net worth, tangible net worth, and debt-to-EBITDA. It shows descriptive statistics for the accounting variables, the binding distance to threshold, and the value for the thresholds. Accounting variables are computed for all the quarterly values that correspond to firm-year observations in the final sample. The thresholds are computed as follows: at each quarter, we take the minimum (maximum for debt-to EBITDA) threshold for each covenant across all outstanding loans (in case there are several loans with the same covenant) and omit observations with no threshold data. The binding distance is defined in the text. Current ratio is defined as current assets over total liabilities. (Tangible) Net worth is defined as (tangible) assets minus total liabilities. Debt-to-EBITDA is total debt (long term debt plus debt in current liabilities) over earnings before interest, taxes, depreciation and amortization.

| | Mean | Standard deviation | Minimum | 10th percentile | Median | 90th percentile | Maximum | Obs. |
|---|---|---|---|---|---|---|---|---|
| Current ratio | 2.06 | 1.51 | 0.08 | 0.90 | 1.76 | 3.33 | 32.34 | 11114 |
| Net worth (in million) | 1351.16 | 5467.93 | -2612.00 | 156.08 | 514.35 | 2505.12 | 160000.00 | 11199 |
| Tangible net worth (in million) | 1330.70 | 5337.74 | -2612.00 | 154.62 | 508.98 | 2471.59 | 152914.00 | 11107 |
| Debt-to-EBITDA | 13.20 | 814.25 | -32229.22 | 0.21 | 6.58 | 17.56 | 76338.00 | 10443 |
| Binding distance (Current ratio) | 0.48 | 0.59 | -0.65 | -0.08 | 0.33 | 1.13 | 3.16 | 241 |
| Binding distance (Net worth) | 1.58 | 12.26 | -2.96 | 0.07 | 0.41 | 1.43 | 238.25 | 1106 |
| Binding distance (Tangible net worth) | 14.45 | 130.02 | -64.42 | 0.08 | 0.62 | 2.57 | 2003.15 | 656 |
| Binding distance (Debt-to-EBITDA) | -2.87 | 8.66 | -187.18 | -4.99 | -1.62 | 0.62 | 6.77 | 1920 |
| Threshold (Current ratio) | 1.41 | 0.45 | 0.50 | 1.00 | 1.30 | 2.00 | 3.00 | 808 |
| Threshold (Net worth) in million | 901.22 | 6178.71 | 4.70 | 100.00 | 300.00 | 1150.00 | 180562.50 | 3731 |
| Threshold (Tangible net worth) in million | 526.16 | 2165.90 | -5.00 | 55.00 | 230.96 | 1000.00 | 43950.00 | 2154 |
| Threshold (Debt-to-EBITDA) | 3.49 | 1.09 | 0.55 | 2.25 | 3.25 | 5.00 | 11.00 | 7042 |

**Table 3**

**Summary Statistics: SEC Sample**

(To be added)

## Table 4
## Sample Averages for Violation and Non-Violation Samples

This table reports sample averages for selected variables for firm-year observations with no covenant violation and for firm-year observations with at least one covenant violation. Panel A uses Subsample 1, defined as those observations for which the binding distance is 40 percentage points away from zero. Panel B uses Subsample 2, defined as those observations for which the binding distance is 80 percentage points away from zero. Statistically significant differences (of at least 10%) are boxed.

| Panel A – Subsample 1 (-0.4,0.4) | Mean | | Difference | T-stat |
|---|---|---|---|---|
| | No violation | Violation | | |
| Number of independent directors (2 leads) | 6.01 | 6.63 | -0.62 | -2.36 |
| Number of non-independent directors (2 leads) | 3.19 | 2.75 | 0.44 | 2.38 |
| | | | | |
| Number of independent directors (2 lags) | 5.59 | 5.60 | -0.01 | -0.03 |
| Number of independent directors (1 lag) | 5.52 | 5.77 | -0.25 | -0.92 |
| Number of independent directors | 5.57 | 5.91 | -0.34 | -1.31 |
| Number of non-independent directors (2 lags) | 3.65 | 3.78 | -0.13 | -0.52 |
| Number of non-independent directors (1 lag) | 3.73 | 3.51 | 0.22 | 1.00 |
| Number of non-independent directors | 3.60 | 3.36 | 0.23 | 1.10 |
| | | | | |
| Firm size | 2010.98 | 2549.97 | -539.00 | -1.00 |
| Leverage | 0.22 | 0.22 | 0.00 | -0.16 |
| Firm age | 23.63 | 22.44 | 1.19 | 0.65 |
| Number of segments | 2.95 | 2.76 | 0.19 | 0.78 |
| Market-to-book | 1.55 | 1.89 | -0.35 | -3.34 |
| R&D | 0.02 | 0.02 | 0.00 | 0.22 |
| Stock return volatility | 0.38 | 0.38 | 0.01 | 0.30 |
| Free cash flow | 0.06 | 0.11 | -0.04 | -4.30 |
| Return on assets | 0.13 | 0.18 | -0.06 | -5.76 |
| Governance index (GIM) | 9.05 | 8.82 | 0.23 | 0.81 |
| CEO ownership | 0.03 | 0.03 | 0.00 | 0.19 |
| CEO Tenure | 9.21 | 7.79 | 1.42 | 1.56 |
| | | | | |
| Number of observations | 218 | 114 | 104 | |

| Panel B – Subsample 2 (-0.8,0.8) | Mean | | Difference | T-stat |
|---|---|---|---|---|
| | No violation | Violation | | |
| Number of independent directors (2 leads) | 6.23 | 6.50 | -0.27 | -1.72 |
| Number of non-independent directors (2 leads) | 2.98 | 2.81 | 0.17 | 1.42 |
| | | | | |
| Number of independent directors (2 lags) | 5.82 | 5.81 | 0.01 | 0.06 |
| Number of independent directors (1 lag) | 5.80 | 5.88 | -0.08 | -0.49 |
| Number of independent directors | 5.88 | 6.00 | -0.12 | -0.79 |
| Number of non-independent directors (2 lags) | 3.39 | 3.47 | -0.08 | -0.55 |
| Number of non-independent directors (1 lag) | 3.45 | 3.30 | 0.15 | 1.13 |
| Number of non-independent directors | 3.38 | 3.20 | 0.18 | 1.36 |
| | | | | |
| Firm size | 2270.41 | 2364.09 | -93.68 | -0.33 |
| Leverage | 0.20 | 0.21 | -0.01 | -0.91 |
| Firm age | 22.97 | 21.21 | 1.76 | 1.55 |
| Number of segments | 2.81 | 2.61 | 0.20 | 1.47 |
| Market-to-book | 1.86 | 2.11 | -0.25 | -2.74 |
| R&D | 0.02 | 0.02 | 0.00 | 0.65 |
| Stock return volatility | 0.37 | 0.36 | 0.01 | 0.85 |
| Free cash flow | 0.09 | 0.11 | -0.02 | -2.64 |
| Return on assets | 0.15 | 0.18 | -0.02 | -3.39 |
| Governance index (GIM) | 9.18 | 8.93 | 0.25 | 1.36 |
| CEO ownership | 0.02 | 0.02 | 0.00 | -0.38 |
| CEO Tenure | 8.19 | 7.06 | 1.14 | 2.19 |
| | | | | |
| Number of observations | 596 | 288 | 308 | |

## Table 5

## Regression of (Log) Number of Independent Directors and Number of Non-independent Directors (Subsample 1 - 40% discontinuity sample)

Estimates of firm fixed effects panel regressions of the number of independent and non-independent board directors are shown in columns (a)-(c) and (d)-(e), respectively. Firm level controls are leverage, firm size (log), firm age (log), number of segments (log), market-to-book (log), R&D, stock return volatility, free cash-flow, return on assets, governance index (GIM), CEO ownership, and CEO tenure. All regressions include year fixed effects, firm fixed effects, and polynomials of the distance to violation. Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% levels.

| | (Log) Number of Independent Directors | | | (Log) Number of Non-Independent Directors | | |
|---|---|---|---|---|---|---|
| | (a) | (b) | (c) | (d) | (e) | (f) |
| Covenant violation | 0.30*** | 0.29*** | 0.41** | -0.23*** | -0.24** | -0.52*** |
| t-stat | (3.13) | (3.21) | (2.35) | (-2.67) | (-2.48) | (-3.08) |
| *Marginal effects (at means)* | 1.92 | 1.85 | 2.62 | -0.63 | -0.66 | -1.43 |
| | | | | | | |
| Only changes from 0 to 1? | no | no | yes | no | no | yes |
| Firm level controls | no | yes | yes | no | yes | yes |
| $2^{nd}$ order polynomial | yes | yes | yes | yes | yes | yes |
| Firm fixed effects | yes | yes | yes | yes | yes | yes |
| Year dummies | yes | yes | yes | yes | yes | yes |
| Observations | 332 | 332 | 260 | 332 | 332 | 289 |

## Table 6

## Regression of (Log) Number of Independent Directors and Number of Non-independent Directors (Subsample 2 - 80% discontinuity sample)

Estimates of firm fixed effects panel regressions of the number of independent and non-independent board directors are shown in columns (a)-(c) and (d)-(e), respectively. Firm level controls are leverage, firm size (log), firm age (log), number of segments (log), market-to-book (log), R&D, stock return volatility, free cash-flow, return on assets, governance index (GIM), CEO ownership, and CEO tenure. All regressions include year fixed effects, firm fixed effects, and polynomials of the distance to violation. Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% levels.

| | (Log) Number of Independent Directors | | | (Log) Number of Non-Independent Directors | | |
|---|---|---|---|---|---|---|
| | (a) | (b) | (c) | (d) | (e) | (f) |
| Covenant violation | 0.31*** | 0.28*** | 0.36** | -0.03 | -0.02 | -0.19 |
| t-stat | (2.94) | (3.06) | (2.12) | (-0.27) | (-0.16) | (-1.11) |
| *Marginal effects (at means)* | 1.98 | 1.79 | 2.30 | -0.08 | -0.06 | -0.52 |
| | | | | | | |
| Only changes from 0 to 1? | no | no | yes | no | no | yes |
| Firm level controls | no | yes | yes | no | yes | yes |
| $5^{th}$ order polynomial | yes | yes | yes | yes | yes | yes |
| Firm fixed effects | yes | yes | yes | yes | yes | yes |
| Year dummies | yes | yes | yes | yes | yes | yes |
| Observations | 884 | 884 | 682 | 884 | 884 | 682 |

## Table 7

## Regression of (Log) Number of Independent Directors and Number of Non-independent Directors (full sample)

Estimates of firm fixed effects panel regressions of the number of independent and non-independent board directors are shown in columns (a)-(c) and (d)-(e), respectively. Firm level controls are leverage, firm size (log), firm age (log), number of segments (log), market-to-book (log), R&D, stock return volatility, free cash-flow, return on assets, governance index (GIM), CEO ownership, and CEO tenure. All regressions include year fixed effects, firm fixed effects, and polynomials of the distance to violation. Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% levels.

| | (Log) Number of Independent Directors | | | (Log) Number of Non-Independent Directors | | |
|---|---|---|---|---|---|---|
| | (a) | (b) | (c) | (d) | (e) | (f) |
| Covenant violation | 0.15*** | 0.14*** | 0.22*** | -0.04 | -0.05 | -0.17* |
| t-stat | (2.65) | (2.66) | (2.72) | (-0.61) | (-0.66) | (-1.76) |
| *Marginal effects (at means)* | 0.96 | 0.89 | 1.41 | -0.11 | -0.14 | -0.47 |
| | | | | | | |
| Only changes from 0 to 1? | no | no | yes | no | no | yes |
| Firm level controls | no | yes | yes | no | yes | yes |
| $10^{th}$ order polynomial | yes | yes | yes | yes | yes | yes |
| Firm fixed effects | yes | yes | yes | yes | yes | yes |
| Year dummies | yes | yes | yes | yes | yes | yes |
| Observations | 2,801 | 2,801 | 2,443 | 2,801 | 2,801 | 2,443 |

## Table 8

## Regression of (Log) Number of Independent Directors on Covenant Violations: Different Polynomial Orders and Subsamples

Estimates of firm fixed effects panel regressions of the number of independent board directors are shown. Each column uses a different subsample, defined by different windows around the covenant threshold (which is normalized to zero). In Columns 1 to 4, the interval *-x, x* means that the binding distance is *x* percentage points away from zero. Each row uses a polynomial of a different order. Preferred specifications (i.e. those reported in Tables 5 to 7) are boxed. All regressions include year fixed effects and firm fixed effects. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% levels.

| | Polynomial order | (1)<br>-0.4,0.4 | (2)<br>-0.8,08 | (3)<br>-4.5,4.5 | (4)<br>-8.5,8.5 | (5)<br>Full sample |
|---|---|---|---|---|---|---|
| Covenant violation | 1st | 0.14** | 0.06 | 0.01 | 0.03 | 0.03 |
| t-stat | | (2.50) | (1.51) | (0.66) | (1.10) | (1.45) |
| Covenant violation | 2nd | 0.30*** | 0.12** | 0.05** | 0.02 | 0.01 |
| t-stat | | (3.13) | (2.58) | (2.00) | (0.90) | (0.70) |
| Covenant violation | 3rd | 0.29** | 0.20*** | 0.04 | 0.05* | 0.04 |
| t-stat | | (2.30) | (2.97) | (1.26) | (1.79) | (1.54) |
| Covenant violation | 4th | 0.30** | 0.27*** | 0.08* | 0.07* | 0.04 |
| t-stat | | (2.05) | (3.00) | (1.91) | (1.92) | (1.47) |
| Covenant violation | 5th | 0.34* | 0.31*** | 0.10** | 0.07* | 0.08** |
| t-stat | | (1.96) | (2.94) | (2.15) | (1.74) | (2.02) |
| Covenant violation | 6th | 0.35* | 0.30*** | 0.16*** | 0.11** | 0.10** |
| t-stat | | (1.85) | (2.64) | (3.07) | (2.33) | (2.44) |
| Covenant violation | 7th | 0.34* | 0.28** | 0.16*** | 0.13** | 0.14*** |
| t-stat | | (1.79) | (2.21) | (2.66) | (2.58) | (3.01) |
| Covenant violation | 8th | 0.36* | 0.27** | 0.19*** | 0.17*** | 0.15*** |
| t-stat | | (1.92) | (1.97) | (2.76) | (2.88) | (2.90) |
| Covenant violation | 9th | 0.31* | 0.26* | 0.24*** | 0.21*** | 0.16*** |
| t-stat | | (1.76) | (1.97) | (3.27) | (3.40) | (2.84) |
| Covenant violation | 10th | 0.31* | 0.30** | 0.22*** | 0.20*** | 0.15*** |
| t-stat | | (1.75) | (2.13) | (3.14) | (3.35) | (2.65) |
| Covenant violation | 11th | 0.32* | 0.26* | 0.21*** | 0.19*** | 0.13** |
| t-stat | | (1.91) | (1.71) | (3.23) | (3.23) | (2.37) |
| Covenant violation | 12th | 0.32* | 0.30** | 0.27*** | 0.17*** | 0.16*** |
| t-stat | | (1.92) | (2.40) | (3.71) | (2.78) | (3.01) |
| Observations | | 332 | 884 | 2,495 | 2,648 | 2,801 |

## Table 9
## Robustness Checks: Poisson Regressions, CEO Turnover and debt-to EBITDA

Estimates of firm fixed effects panel regressions of the number of independent board directors are shown. Columns (a) to (c) show results of Poisson regressions. Columns (d) to (f) show results of regressions after dropping observations in which the CEO is replaced in one or two years after a covenant violation. Columns (g) to (i) show results of regressions in which debt-to-EBITDA covenants are ignored in the definition of covenant thresholds and distance to violation. All regressions include year fixed effects, firm fixed effects, and polynomials of the distance to violation. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% levels.

| | (Log) Number of Independent Directors | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Poisson regressions | | | No CEO turnover | | | No debt-to-EBITDA | | |
| | (a) | (b) | (c) | (d) | (e) | (f) | (g) | (h) | (i) |
| Covenant violation | 0.24*** | 0.28*** | 0.11*** | 0.38*** | 0.36*** | 0.15*** | 0.18* | 0.14** | 0.02 |
| t-stat | (3.34) | (3.33) | (2.60) | (3.95) | (3.53) | (2.60) | (1.75) | (1.98) | (0.42) |
| *Marginal effects (at means)* | 1.53 | 1.79 | 0.70 | 2.43 | 2.30 | 0.96 | 1.15 | 0.89 | 0.13 |
| | | | | | | | | | |
| Subsample | 1 | 2 | full | 1 | 2 | full | 1 | 2 | full |
| Firm level controls | no | no | no | no | no | no | no | no | no |
| Polynomial order | 2 | 5 | 10 | 2 | 5 | 10 | 2 | 5 | 10 |
| Firm fixed effects | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Year dummies | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Observations | 332 | 884 | 2800 | 304 | 809 | 2422 | 403 | 1006 | 1761 |

## Table A.1

## Variable Definitions

| Variable | Definition |
|---|---|
| Number of independent directors | Number of board members that are independent directors (IRRC). |
| Number of non- independent directors | Number of board members that are non-independent directors (IRRC). |
| Covenant violation dummy | Dummy variable that takes the value of one if there are a covenant violation based on covenant slack (current ratio, net worth, tangible net worth and debt-to-EBITDA), and zero otherwise (DealScan). |
| Number of directors | Number of board members (IRRC). |
| Firm size | Market capitalization in $ millions (Compustat: CSHO x PRCC_F). |
| Leverage | Ratio of total debt to total assets (Compustat: (DLTT + DLC) / AT). |
| Firm age | Number of years since the stock inclusion in the CRSP database. |
| Number of segments | Number of business segments in which firm operates (Compustat). |
| Market-to-book | Ratio of market value of assets to book value of total assets (Compustat: (AT + CSHO x PRCC_F - CEQ) / AT). |
| R&D | Ratio of research and development expenditures to book value of assets (Compustat: XRD / AT) |
| Stock return volatility | Stock return standard deviation (annualized) estimated with daily stock returns (CRSP). |
| Free cash flow | Ratio of operating income before depreciation minus capital expenditures to total assets (Compustat: (EBITDA - CAPX) / AT). |
| Return-on-assets | Ratio of operating income before depreciation to total assets (Compustat: EBITDA / AT). |
| Governance index (GIM) | Governance index of Gompers, Ishii, and Metrick (2003), which is based on 24 antitakeover provisions (IRRC). |
| CEO ownership | Number of shares held by the CEO divided by number of shares outstanding (EXECUCOMP). |
| CEO tenure | Number of years since the date the director became CEO (EXECUCOMP). |
| Net worth | Ratio of total assets minus total liabilities to total assets (Compustat: (AT - LT) / AT) |
| Current ratio | Ratio of current assets to current liabilities (Compustat: AC/ LC) |