Unfriendly Creditors: Debt Covenants and Board Independence*

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Abstract

We develop and test a theory of the relation between capital structure and the composition of the board of directors. Our model shows that board composition should become more management-unfriendly when a firm is close to financial distress. Empirical tests of this prediction indicate that the number of independent directors increases by roughly 30% following a loan covenant violation. The evidence suggests that board composition is an important channel through which creditors monitor borrowers.

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The 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 (1984) offers as an example the case of loan covenant violations, after which "the intermediary monitors the situation and uses the information to renegotiate the contract with new interest rates and contingent promises" (Diamond (1984), p. 395). What is left unexplained is how exactly such monitoring takes place.

We investigate theoretically and empirically one channel through which creditors monitor borrowers: the composition of the board of directors. The reasons for creditors to intervene in board composition are not obvious. Even if creditors can influence board appointments, directors still owe fiduciary duties to shareholders.[1] In general, excessive and explicit intervention by creditors may force them to owe fiduciary duties to shareholders as well or, in the case of bankruptcy, make them subject to equitable subordination (i.e., courts may treat their claims as subordinate on equitable grounds). Thus, typical debt contracts rarely give creditors explicit rights over board appointments. Creditors' influence on corporate governance is thus often subtle and behind the scenes (Baird and Rasmussen (2006) and Nini, Smith, and Sufi (2012)), which makes the empirical documentation of their activities challenging.

We construct a model of optimal corporate board composition and capital structure. As in previous works on contingent allocation of control rights (e.g., Aghion and Bolton (1992) and Dewatripont and Tirole (1994)), in our model the equilibrium debt contract gives creditors control rights in states of low cash flow. Our contribution is the explicit modeling of board monitoring as a channel through which creditors try to influence firm outcomes after acquiring control rights.

Theories of boards normally ignore capital structure considerations. It is our contribution to introduce debt and equity financing to 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

[1] If a firm is near insolvency, directors may also consider the interest of creditors (Becker and Stromberg (2012)).

can be implemented only if creditors have the right to appoint *independent* (i.e., management-unfriendly) board members in low cash flow states.

The intuition is as follows. We start with the theoretical literature, which shows that shareholders may prefer to elect a "friendly " board, i.e., a board that is a weak monitor of the Chief Executive Officer (the CEO). For example, in Adams and Ferreira (2007) a friendly board facilitates communication with the CEO, in a model 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.[2]

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 prompts the choice of safer projects. In such states, shareholders often have incentives to take risks that are unwanted by creditors (Jensen and Meckling (1976)). Shareholders engage in risk shifting by choosing a management-friendly board. Therefore, to increase debt capacity, shareholders need ex ante to commit to an "unfriendly" board in states of low expected cash flow. They can do this by transferring the right to appoint board members to creditors. Our theory then predicts that debt contracts must allow creditors to influence board appointments in low cash flow states.[3]

While debt contracts rarely give creditors explicit rights over board appointments, in practice creditors become more influential after (or just before) loan covenant violations. Chava and Roberts (2008), Roberts and Sufi (2009), and Nini, Smith, and Sufi (2012) among others show that there are significant changes in corporate policies after covenant violations. The usual interpretation of such evidence is that creditors can exert direct influence on corporate decisions through the threat of accelerating loan payments after covenant violations. Thus, we expect creditors to use their increased power at the time of covenant violations to push for a management-unfriendly board, one that communicates less and monitors more. The key testable implication of our theory is that board independence should increase after loan

[2]Evidence consistent with this assumption can be found in Kang, Liu, Low, and Zhang (2014).

[3]The optimality of contingent allocation of control rights is a well understood result in the financial contracting literature (Aghion and Bolton (1992) and Dewatripont and Tirole (1994)). Our contribution is instead the explicit modeling of one channel through which creditors monitor borrowers.

covenant violations.

Our main empirical finding is that boards become more independent after a debt covenant violation. A covenant violation implies a 30% net increase in independent directors, or approximately two directors for the typical firm. Our analysis strongly supports a causal interpretation of the evidence, that is, covenant violations appear to cause significant changes to board composition. We also find that newly appointed directors are 50% more likely than incumbent directors to have links to creditors. Such evidence strongly supports the hypothesis that creditors influence changes in board composition.

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

Our main empirical strategy is adapted from Chava and Roberts (2008), who study the effect of debt covenant violations on investment. Interpretation of evidence raises a number of concerns that we address in detail later. Here we provide a summary of the main issues.

A first issue is that firms with more independent directors might 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. To address these concerns, we restrict the sample to include only observations within a narrow interval around the threshold (e.g., the specified minimum net worth) that triggers a covenant violation, while at the same time saturating the model with fixed effects and polynomials of measures of the distance to the covenant threshold.

Second, 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 firm incentives to manipulate accounting variables and the likelihood that independent directors would be appointed. We also find that firms on each side of the covenant threshold are similar in most observable corporate governance characteristics. Past and current levels of board independence, for example, 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.

Third, it is possible that time trends in board independence could create a spurious relation between covenant violations and board independence. To absorb aggregate trends, we use year fixed effects. To deal with the possibility of firm-specific trends, we perform a series of falsification tests using placebo covenant thresholds. We find that our estimated effects are economically and statistically significant only 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 spurious correlations. Given the many identification issues, the placebo tests strengthen the causal interpretation of the evidence.

Finally, even if the relation between covenant violations and board independence is causal, the channel could be quite different from the one envisioned in our theory. There are two competing explanations. The first is the possibility that changes in board composition occur only in the case of bankruptcy. 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 shows that covenant violations rarely lead to default (e.g., Gopalakrishnan and Parkash (1995) and Chava and Roberts (2008)).

The other 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 that covenant violations have a causal effect on board independence, is at odds with considerable evidence of the influence of creditors on corporate outcomes after covenant violations. We also show evidence that our results are stronger for more bank-dependent firms.

To investigate the creditor intervention hypothesis further, we collect data on the characteristics of independent directors who are appointed within two years of a covenant violation. We find that 65% of the newly appointed directors hold positions in other firms that also borrow from the same banks. This number is about 50% more (or 22 percentage points higher) than that of a baseline sample of matched directors. This evidence strongly suggests that there are indirect links between newly appointed directors and incumbent creditors, which provides additional support for the creditor intervention hypothesis.

Our work is related to a number of studies that focus on the role of creditors on corporate governance. Gilson (1990) was the first to investigate the influence of creditors on board composition. He finds direct evidence that in negotiated restructurings banks influence the appointment of directors both directly and through share ownership. Our results complement Gilson (1990) by providing further evidence that bankers influence board appointments *outside bankruptcy states* and by establishing the direction of causation more convincingly.

The work 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. Our evidence is of a different nature, however; our results are stronger for the subset of firms that *do not* replace their CEOs after a covenant violation.

Anderson, Mansi, and Reeb (2004) argue that creditors may prefer more independent boards because the presence of independent directors is likely to improve the quality of financial accounting reports. Consistent with this "unfriendly " creditor hypothesis, they show evidence of a negative association between board independence and the cost of debt. Kroszner and Strahan (2001) and Guner, Malmendier, and Tate (2008) also study the costs and benefits of the presence of bankers on corporate boards.

Also related is Becker and Stromberg (2012) study, which shows that a 1991 change in the 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. In 1991, Pathé Communications obtained a loan from Credit Lyonnais. The loan allowed Pathé 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 had been supported by the bank. Later 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 when the borrower is not (yet) insolvent and even when they do not own shares in the borrower. Third, creditors can force changes to the board after the borrower has breached some condition in the loan contract. Finally, creditors often support the appointment of board members who are not closely affiliated with them, but instead are professionals who are "trusted" by the banks.

Our work also contributes to a large theoretical literature on corporate boards. Examples include Hirshleifer and Thakor (1994), Hirshleifer and Thakor (1998), Hermalin and Weisbach (1998), Raheja (2005), Song and Thakor (2006), Adams and Ferreira (2007), Harris and Raviv (2008), Chemmanur and Fedaseyeu (2012), Chakraborty and Yilmaz (2013), Levit (2012), Levit and Malenko (2013), and Malenko (2014). 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 we introduce 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 from two mutually exclusive choices. 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 non-routine *complex project*. There are an infinite number of complex projects, but the firm can undertake only one of them. We denote each complex project by a real number $y \in \Re$. Complex projects require specialized knowledge, which we represent 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:

$$P(y, e) = \alpha - (y - e)^2, \tag{1}$$

where $\alpha > 0$ is non-stochastic. Projects $y \in \Re$ are complex 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 μ and σ^2, respectively. This technology is similar to that found in the cheap talk and delegation literature (e.g., Adams and Ferreira (2007), Dessein (2002), Harris and Raviv (2005), Harris and Raviv (2008), and Harris and Raviv (2010)). 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 determined exogenously. Capital providers are perfectly competitive and risk-neutral, and have a 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 (whether issuing costs, unmodeled asymmetric information, or mispricing) may make issuing new shares more costly than financing through loans.

The firm defaults if it does not have sufficient assets that can be immediately monetized to pay the face value of debt in full at maturity. Default generates direct distress costs. To simplify 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 is always strictly positive.

To summarize, we assume that the capital structure is at least partly determined by traditional trade-off forces: tax shields and costly financial distress. We also assume that: (1) the value of the operating assets at (and immediately prior to) maturity is verifiable, and (2) debt renegotiations are infinitely costly. Assumption (1) 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 them to pay dividends before paying creditors. Assumption (2) 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 publicly known. Variable e may or may not be known when project decisions are made. The value of the current equity as a function of (s, y, e) is

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

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

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

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 that they can be sold and monetized). In other words, if e is not known at this date, it will become known only at a much 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 $B(s,y,e)$.[4] 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 $\alpha \geq \beta$; otherwise maximizing private benefits with no concern for equity would be more efficient than maximizing equity value with no concern for 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 a 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 increases with the value of equity (the first term on the right-hand side of equation (7), 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),

[4]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.

Harris and Raviv (2005), Harris and Raviv (2008), Harris and Raviv (2010), 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 that is not made in 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. The board can be of two types. With probability π, 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 π they wish at no cost.[5] Following Adams and Ferreira (2007), we call π 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 advisory role. If the CEO reports private information about firm-specific conditions to the board, the board learns the value of e with probability 1, in which case it may report this value 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 θ, which we call *CEO 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 θ, it obtains its private information e with probability 1. If the board remains uniformed about θ, it cannot learn e.

We interpret the communication stage of our model as follows. The CEO raises an *issue* θ 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, so 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. As in 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.

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

Formally, the board believes that the prior distribution of issues θ is uniform on the unit interval, $\theta \sim U[0,1]$. Conditional on knowing the realization of θ, the CEO's posterior belief is that $e(\theta)$ also has mean μ and variance σ^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, the assets 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 revealed. With probability π, the board is fully aligned with shareholders, and it approves only decisions that maximize the value of equity, conditional on the information available at this date. With probability $1-\pi$, the board colludes with the CEO, and the CEO retains the right to choose a preferred project.

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

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 are 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. The CEO's optimal choice of project is

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

with utility

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

Thus, all else constant, the CEO prefers to be informed. As in Adams and Ferreira (2007), σ^2 can be interpreted as the benefit gained by 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 with the board leads to a solution, which increases the value of a complex project by exactly $2\sigma^2$.

Now suppose 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 private information (i.e., to communicate with the board). For a given level of independence π, the CEO will reveal such 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 private information with the board whatever the level of board independence. Intuitively, a low σ^2 means that board advice has a negligible effect on profits. If $\alpha > \sigma^2$, then lack of communication does not reduce the profitability of complex projects enough to make the null project preferable. In that case, if the CEO does not raise an issue, this simply adds noise to the decision, which reduces the CEO's 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 private 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; if this assumption does not hold, shareholders always choose a fully independent board.[6] Under Assumption 1, the CEO raises 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 the CEO's bias in project choice b. As $\alpha + \beta$ increases, the complex project is more valuable to the CEO, encouraging the disclosure of private information even if the board monitors the CEO more intensively. As b increases, the CEO puts a higher value on controlling project choice, and thus is less likely to reveal private information to the board.

For a given π, 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 increases with π. 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 proven the following result:

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

(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 private information; and*

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

- *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 private information; and*
- *Project $\emptyset$ is chosen with probability 1.*

This proposition shows that sometimes 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 management-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, however, is much simpler and more intuitive.

1.2.2 Debt and Equity

We next 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 (such as 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.[7]

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 include a covenant, which 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.

[7] While convertible bonds may also create tax shields, the benefits are not as great as those associated with straight debt. Interest deductibility disappears when the option to convert is exercised or forced (e.g. if there are call provisions). Furthermore, for the same amount of funds raised, convertible bonds have lower coupons than straight debt. Thus, even if convertibles are possible, they may be suboptimal in our model because of lower tax shields.

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 Internet Appendix, we discuss the possibility of risky debt and show that our main result is unchanged.

We proceed in four steps. First, we compute the optimal debt contract assuming 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 assuming that one could increase board independence to $\pi = 1$ after observing $s = 0$. Third, we show that in certain cases the second contract dominates the first contract. Finally, we show that the second contract cannot be implemented if shareholders keep control 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 greater 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 is identical to that in the all-equity case. Consider now that $s = 0$. To guarantee that debt is indeed risk-free, we require 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 the CEO 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 fully wiped out, and the CEO chooses the project that minimizes $(y - e - b)^2$, which leads to the choice of $y = e + b$, and CEO utility becomes β. Thus, to avoid default in equilibrium we also require 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 increases with the value of assets in place in low cash flow states (A_0) and in the risk-free component of the project's cash flow (α), and declines with the CEO's bias (b).

It is straightforward to check that if the value of 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 makes larger tax benefits possible. 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 complex 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

$$p_0\left[E\left(\tilde{\pi}\right) - A_0\right] = \frac{p_0}{2}\left(3\alpha + \beta - b^2\right) > 0. \tag{27}$$

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 α with probability π. 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 shareholders will choose $\pi' \equiv \frac{\beta}{b^2 - \alpha} < 1$. Thus commitment to $\pi = 1$ is not possible if $D = A_0$.[8]

These four steps prove:

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

Proposition 2 *There is a unique risk-free debt equilibrium with properties as follows:*

(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* $s = 0$*; and*
- *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* $\tilde{D}_f = A_0 + \alpha - \frac{b^2}{2}$*; and*
- *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* $\tilde{D}_u = A_0$*; and*
- *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)*** *Boards become more independent after loan covenant violations.*

1.3 What Do We Learn from the Model?

The model delivers an unambiguous prediction (Corollary 1). 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 of them (this list is not exhaustive):

Informal Theory 1 Firms that violate covenants have poor 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 poor 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 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. There are 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 a firm is close to financial distress.*

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

Informal theories 1 and 2 assume that creditors or shareholders believe that board independence is always value-enhancing, a belief that has no credible empirical or theoretical support. Many empirical papers on corporate boards have failed to find convincing evidence that board independence is always value increasing (see the review article by Adams, Hermalin, and Weisbach (2010)). The evidence instead suggests that board independence may have positive or negative effects on firm value (e.g., Duchin, Matsusaka, and Ozbas (2010) and Nguyen and Nielsen (2010)), which is consistent with many theories of boards that emphasize the costs and benefits of board independence (e.g., Adams and Ferreira (2007) and Harris and Raviv (2008)).

Our model highlights that greater board independence after creditors acquire control may be due to creditors' preference for conservative investments. It does not imply that the firm suffered from poor 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 that 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 formal and anecdotal evidence that empowered creditors often push for governance changes (e.g., Gilson (1990), Baird and Rasmussen (2006), Becker and Stromberg (2012) and Nini, Smith, and Sufi (2012)).

Finally, although we cannot rule out the shareholder intervention hypothesis, we also present direct evidence for the creditor intervention hypothesis, which is inconsistent with this particular version of Informal Theory 2 where creditors do not influence board composition.

2 Data

We start with non-financial 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 syndicated loans data obtained from the DealScan database for the 1994-2008 period. The DealScan database provides information on syndicated loans made by banks to firms, including loan amounts, maturity, type of loan, syndication, covenants, and pricing, among others. We restrict the sample to loans with information on maturity and spread over the LIBOR (all-in spread drawn), and we eliminate loans for which we do not have any covenant information or that do not include a covenant on current ratio, net worth, tangible net worth, or debt-to-EBITDA ratio. We then merge the loan data with firm and director data using company name, ticker, and loan origination date.[9]

We identify whether a firm violates any of the covenants at any 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 in every quarter until maturity. Since a firm might have more than one active loan in 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 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 (presumed) 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)*, 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

[9]It is possible that a new loan may be taken to refinance a previous one, but Dealscan provides limited information on whether a loan is a refinancing. We exclude 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.

loan (a firm may have more than one loan with covenants). C_{itjk} is the quarterly value of the accounting variable relevant for covenant k. T_{itjkz} is the threshold for active loan z, covenant type k, in quarter j of year t for firm i.[10]

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 greatest distance between the accounting variables and the threshold, for those covenants that are breached. A violating firm is defined as a firm that violates at least one covenant in at least one quarter in a given year. For expositional simplicity, we allow D_{it} to assume negative values; "negative distance" means that 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 covers 597 firms and 2,801 firm-year observations. For this sample, we find that 76% of the firms violate a covenant at least once during the sample period (453 firms), and 60% of the firm-year observations include a covenant violation event (1,669 firm-year observations).

Our procedure for defining violations overstates the actual number of violations because we do not consider covenant threshold renegotiations. Denis and Wang (2014) show that covenant thresholds are often renegotiated when firms are close to the threshold, and also that nearing the threshold makes it more likely that covenants will be relaxed. In their sample, about 50% of contracts would be in violation if the original covenants had not been relaxed. This number can explain our relatively high number of violations – 60% of the firm-year observations.[11]

Denis and Wang (2014) show that debt covenant renegotiations in firms that would be in violation are associated with changes in investment and financial policies. Their results suggest that creditors gain more influence when a firm is close enough to a violation, so that, without renegotiation, the firm would almost surely trigger the covenant. Our measure of "violations" also includes cases in which violations did not occur because covenant thresholds were renegotiated. We want to include such cases because debt renegotiation is exactly the mechanism through which we expect creditors to influence firm choices. In a robustness check,

[10]For the debt-to-EBITDA covenant, $\tilde{D}_{itjk}$ is defined analogously by the symmetric of $C_{itjk} - T_{itjkz}$.

[11]Denis and Wang (2014) 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.

we also consider a different sample that includes only covenant violations filed with the SEC.

Table 1 presents descriptive statistics of the main variables in our study using the DealScan sample. Table A.1 in the Appendix presents a detailed definition of the variables. The descriptive statistics are comparable to those in other studies using loan covenant data (e.g., Nini, Smith, and Sufi (2009) and Denis and Wang (2014)). The average firm has assets of $3.54 billion. The statistics for the board variables are similar to those in other studies using IRRC data. The average board size is 9.15, with 70% (= 6.39/9.15) independent directors, measures that are similar to those in other studies of boards (e.g., Ferreira, Ferreira, and Raposo (2011)).[12]

The median of the binding distance 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 6.45 (more than nine times the threshold value away from a violation), which is one order of magnitude higher 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 use subsamples that exclude observations that are 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% (561) and 8.2% (231) of firm-year observations, respectively. The average value for the debt-to-EBITDA covenant threshold is 3.5. This is the covenant that firms are less likely to comply with, as 57% (1,598) of the observations correspond to a violation of this covenant.

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

[12] To qualify as independent, a director must not be an employee, a former executive, or a relative of a current corporate executive of the company. In addition, the director must have no business relations with the company.

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 theirs, mainly because the use of board data restricts the sample size. 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 violating for the whole year.

As in Chava and Roberts (2008), we infer violations from threshold and accounting data. This procedure may lead to coding and other unintended errors, as well as possible overstatement of 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). 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 depending on how debt in defined. We simply assume that debt is equal to total debt (long-term debt and debt in current liabilities). In Denis and Wang's (2014) 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. 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. We also run our main tests without this variable and obtain qualitatively similar findings.[13]

3 Methodology

3.1 Empirical Challenges

Our goal is to estimate the average effect of a covenant violation on board composition, conditional on firms having loans with restrictive covenants. The most serious difficulty is the possibility of a spurious correlation between covenant violations and board independence if

[13] In the Internet Appendix, we present results using only violations that are registered with the SEC and show that our main result is unchanged.

there are omitted variables or sample selection issues (e.g., survivorship biases). Another issue – reverse causality – is less plausible. It requires that expectations of future changes in 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 using narrow windows around the threshold. 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 a subsample 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 "forcing" (or "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 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 likely to be farther 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 include observations not in violation than observations in violation.

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

To address these potential problems, we proceed as follows. First, we use firm fixed effects in all the 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 proximity to the event threshold is used 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. Future levels of board independence are, on the contrary, 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. To address this issue, we perform a series of placebo tests aimed at detecting "jumps" in board independence at other points near the actual covenant thresholds.

3.2 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 has violated a covenant 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 γ_{p0} and γ_{p1} can differ on the left- and right-hand sides of the threshold; α_t is a year fixed effect; f_i is a firm fixed effect; and $\mathbf{x}_t$ 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. Board directors can be replaced only at regular intervals of no less than one year and often up to three years, as occurs in firms with staggered-board provisions in their charters.[14] As some of the covariates in $\mathbf{x}_{it}$ might nevertheless be endogenous, our main specification does not include $\mathbf{x}_{it}$.

The coefficient of interest is β. Given the log-linear specification, β is a semi-elasticity, and thus has a simple interpretation: β is the percentage change in y_{it} due to a covenant violation. To facilitate interpretation, the tables also present the marginal effects of a covenant violation evaluated at the sample average of y_{it}: $\partial y_{it}/\partial v_{it-2} = \beta\overline{y}$.

As in regression discontinuity designs, we discard observations that are more than h (the bandwidth) away from the threshold (which is normalized to zero). We do not use a theoretically-motivated bandwidth selection criterion (for example, Imbens and Kalyanaraman (2012)) because some of the necessary assumptions are unlikely to hold in our application. We choose instead an ad hoc narrow bandwidth ($h = 0.4$) as the baseline, which generates a subsample that includes 332 observations (12% of the full sample).[15] We later check the robustness of the results to bandwidth choice.[16]

4 Empirical Results

4.1 Preliminary Analysis: Descriptive Statistics

Table 3 presents average values for some selected variables on each side of the threshold, for the subsample associated with the baseline bandwidth ($h = 0.4$). The average number of independent directors at $t + 2$ is about 0.62 higher 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

[14]The replacement of directors is regulated by state corporate law and a company's charter. Normally, directors can be replaced only once a year at shareholder meetings (sometimes once every three years, if directors have staggered terms), and new directors have to be nominated well in advance of 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 various rules governing the appointment of board directors, see Ferreira, Kershaw, Kirchmaier, and Schuster (2012).

[15]We drop observations from firms that appear in this subsample 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 (2012) 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 apply to only 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.

directors at $t+2$ for firms that violate a covenant is about 0.44 lower than in 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," because 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.

Table 3 shows evidence that may indicate that more profitable firms and firms with higher market valuation are more likely to stay in the sample after a covenant violation. Firms in the violation sample have higher market-to-book, return on assets and free cash flow. The differences are statistically significant for these three variables. As we discuss later, however, the distance to violation variable captures most of the observed heterogeneity in these variables. There are no statistically significant differences in the other firm characteristics.

The typical regression discontinuity design implies that observations around the threshold are (as good as) random. Thus, if the bandwidth is sufficiently narrow, we should expect an equally balanced sample size on each side of the threshold. Table 3 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%. 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] We later discuss other reasons for the observed imbalance.

We use the panel structure of our data to mitigate concerns about the non-random nature of the subsamples to the right and to the left of the threshold. By including firm fixed effects in all the regressions, we ensure that our results are driven by firms that are on both sides of the threshold. This comes at some loss of external validity; that is, 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 non-random sample of firms, because, for example, firms that manipulate earnings aggressively may have a low probability to be in this sample and influence our estimates.

The combination of fixed effects and the use of observations near the threshold (the bandwidth is $h = 0.4$) goes a long way toward mitigating concerns about omitted variables. With

[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)).

fixed effects, our key identification assumption is that firms' propensity to manipulate earnings to avoid covenant violations is not correlated with firms' expectations of an imminent increase in board independence. Although we cannot test this assumption, it is a plausible one.[18]

4.2 Primary Results

Table 4 reports our primary results. Column (1) reports the estimate of β with the log of the number of independent directors as the dependent variable. The (local) regression includes firm fixed effects, year fixed effects, and a second-order polynomial of the binding distance on each side of the discontinuity. The estimated β is positive and statistically significant. A covenant violation leads to an increase of 30% in the number of independent directors. This implies 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 the effect in Table 3, which suggests that the inclusion of controls (firm fixed effects, time dummies, and the binding distance) amplifies the effect of covenants on independence. The estimated effect is also economically important. Firms in our sample have 6.4 independent directors on average, so a change of two directors is not trivial. This effect contrasts with most findings documented by the empirical literature on boards, which shows small economic effects of variables that influence board composition (see the discussion in Ferreira, Ferreira, and Raposo (2011)).[20]

The specification in column (2) includes a long list of potential covariates: firm size, leverage, firm age, number of business segments, market-to-book, R&D-to-assets ratio, stock return volatility, free cash flow, return on assets, governance index (Gompers, Ishii, and Metrick (2003)), and CEO ownership and tenure. All these variables are lagged by two years relative to the dependent variable. The estimated β is virtually identical to that in column (1), which

[18]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).

[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 heavy users of bank debt with covenants and likely to remain solvent and financially healthy after covenant violations.

[20]In virtually all regressions of board independence on firm characteristics in the literature, the economic significance of the estimated effects is low. 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.

suggests that omitted variables are unlikely to be a concern. One potential concern here is that these firm characteristics 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.

Another potential problem with our empirical strategy is that, by using variable v_{it}, we are implicitly assuming that changes from $v_{it-1} = 0$ to $v_{it} = 1$ are similar to changes from $v_{it-1} = 1$ to $v_{it} = 0$. This assumption is unreasonable because, while the former leads to a covenant violation, the latter does not reverse (necessarily) an earlier violation. Our interpretation of the results would be suspect if the results were mostly driven by changes from 1 to 0. To address this issue, in column (3) we replace variable v_{it} with

$$v'_{it} = \{1 \text{ if } v_{it} = 1; 0 \text{ if } v_{is} = 0 \text{ for all } s \leq t; \text{ missing otherwise}\}. \tag{33}$$

That is, v'_{it} considers only changes from $v_{it-1} = 0$ to $v_{it} = 1$. The β rises to 0.41, that is, an effect of 41%, and a substantially higher marginal effect of 2.6 new directors (evaluated at the sample mean). This finding reinforces our interpretation. The evidence is consistent with the hypothesis that covenant violations precedes substantial board restructuring, as they typically lead to the (net) addition of two or more independent directors.[21]

For comparison, we also estimate the same regressions without firm fixed effects, including two-digit SIC dummies to control for industry (the results are not tabulated). Our estimates for columns (1)-(3) are 0.45 (t-statistic 2.98), 0.30 (t-statistic 2.75), and 0.29 (t-statistic 1.99), respectively. Thus, firm fixed effects do not appear to affect the estimates significantly, especially when we introduce firm-level controls. Henceforth we present only the fixed-effects estimates, as these are free from the most obvious sample selection and omitted variables biases (none of our fixed-effects estimates substantially differs from the estimates obtained by pooled OLS with industry fixed effects).

Columns (4)-(6) present estimates using the logarithm of the number of non-independent

[21]To deal further with potential asymmetry between changes in each direction, we also consider 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 β is 0.40 (t-statistic 2.48, 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 β is 0.38 (t-statistic 2.16, 233 observations).

directors as the dependent variable. The estimates show that covenant violations also increase board independence by reducing the number of non-independent directors on boards of directors. As before, we find that the inclusion of firm-level controls does not have a meaningful effect on the estimates of β and that using 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.

4.3 Robustness to Polynomial Order and Bandwidth Choice

There is no generally accepted criterion for choosing the polynomial order in regression discontinuity designs. Although the use of high-order polynomials is common in the literature, Gelman and Imbens (2014) strongly advise against using polynomials of order higher than 2. Polynomials of order 2 may also have additional attractive properties. Calonico, Cattaneo, and Titiunik (2014) show that, under certain conditions, one can adjust for the bias of a local-linear estimator by constructing confidence intervals based on the local-quadratic estimator. Although these are compelling reasons to choose a second-order polynomial as the baseline, common sense also suggests experimenting with different polynomial orders and bandwidth choices, as also recommended by Roberts and Whited (2013).

Table 5 reports the estimates of β for a combination of three different bandwidths (0.3 to 0.5) and polynomial orders (1 to 3), using the (log of the) number of independent directors as the outcome variable. We do not include other firm-level characteristics, but the results are very similar if we do include them. In most cases, firm characteristics do not much affect the estimate of β.

Consider first the choice of polynomial order. For the baseline bandwidth ($h = 0.4$) and with a polynomial of order 1 (i.e., a local-linear regression), the estimated β is 0.14 and is statistically significant. With our preferred specification (order 2), this effect becomes 0.3. For polynomials of order 3 or higher (orders higher than 3 are not reported in the table for brevity), the effect is no longer sensitive to the order of the polynomial.[22] Choosing a narrower bandwidth ($h = 0.3$) reduces by half the number of observations. The point estimate for β is generally

[22] When we estimate β with polynomials of order higher than 3 (up to order 12) we find statistically significant estimates ranging from 0.29 and 0.36.

higher than that for the baseline bandwidth, for all polynomial orders. Although the confidence intervals are wider, which is expected because of the smaller sample size, all estimated effects are statistically significant. A larger bandwidth ($h = 0.5$) leads to slightly lower point estimates for β for polynomials of order 1 and 2, but polynomial order has little impact on β for orders 3 or higher (not reported).[23] We conclude that the effect of covenant violations on the number of independent directors is fairly robust to polynomial order and bandwidth choice.

An alternative to local regressions is to use global regressions with high-order polynomials. Such an approach is considered inferior to local regressions by some authors (e.g., Imbens and Kalyanaraman (2012) and Gelman and Imbens (2014)). Nevertheless, for completeness, we report a summary of the results obtained through global regressions, but omit the tables.

The global-regression results are also consistent with the hypothesis that board independence increases after covenant violations, but such results also underscore the limitations of the global-regression approach. Using high-order polynomials is necessary in global regressions, unless there are good a priori reasons to assume that the relation between the outcome variable and the forcing variable is smooth. But high-order polynomials create a number of issues, as explained by Gelman and Imbens (2014). One issue is that estimates are often sensitive to polynomial order. We find that, for lower-order polynomials (orders 1 to 4), the estimated β is positive, small and statistically insignificant. For polynomials of order equal or higher than 5, the estimated β is statistically significant, although generally lower than those estimated with local regressions. The estimated β tends to increase with polynomial order until order 7, after which it stabilizes at the 0.13 to 0.16 range and becomes robust to polynomial order (we consider all polynomial orders up to 21). Thus, the effect estimated with global regressions is about half of that estimated with local regressions.

Table 6 shows that, unlike the case of independent directors, the estimated β for non-independent directors is not robust to polynomial order or to bandwidth choice. Although most point estimates are negative, they are often economically minor and statistically insignificant.

We conclude that the evidence that board independence increases because of the appointment of new directors is more robust and much stronger, both economically and statistically,

[23] In untabulated results, we also find that larger bandwidths (0.6 to 0.8) produce estimates that are always statistically significant for polynomials of order 2 or higher, ranging from 0.12 to 0.31. Results for $h = 0.2$ are also very similar, both in economic and statistical significance, but the sample becomes very small.

than the evidence that non-independent directors leave after covenant violations. However, if anything, there are slightly fewer non-independent directors following covenant violations. Thus, board independence unambiguously increases after covenant violations.

4.4 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 using placebo thresholds. We create ten 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 the discontinuity subsamples accordingly. We run the same regressions as in Table 4 for each placebo threshold.

We summarize the results in Figure 1, using the specification in column (1) of Table 4.[24] The placebo thresholds are on the x-axis, where 0.00% denotes the true threshold. The y-axis measures the estimated β. 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 the 5% level.

We believe that these placebo tests provide the strongest evidence in favor of a causal interpretation of our findings. One key concern is that there may be naturally occurring discontinuities in the number of independent directors, 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 other discontinuities in Figure 1. This figure suggests instead that we can only find a robust effect at the "real" threshold.

[24]The figures for the regressions with firm-level controls are virtually identical to Figure 1.

4.5 Covenant Violations and Other Firm Characteristics

To evaluate the effects of covenant violations on firm characteristics, for each (non-board) characteristic in Table 3, we replicate the full set of regressions as in Table 5. We report a summary of these results, but omit the tables.

We find that covenant violations do not appear to have a significant effect on ROA, free cash flow, and market-to-book when we include firm fixed effects, year fixed effects and the distance to default variables in the regressions. The estimated effects for these three variables are always economically small and statistically insignificant for all polynomial orders and bandwidths.

In contrast, firms in violation tend to be older and larger. This is more likely to be a selection effect; larger and older firms 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 we consider) that appear to be related to covenant violations: firm age and size. This suggests that sample selection is not random. Thus, "controlling" for firm age and size may be important. Our results in general, however, suggest that controlling for these variables has negligible impact on the estimated effects of violations on board independence. In fact, in the regressions in Table 4, firm age and size do not seem to be robustly related to the appointment of new directors.

4.6 Graphical Analysis

Here we present nonparametric evidence that covenant violations affect board independence. Panel A of Figure 2 plots estimates of nonparametric regressions of the number of independent directors on (the negative of) the distance to the covenant threshold. We run separate regressions for each side of the threshold. As in the previous regressions, we measure the dependent variable at year $t + 2$. 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 2 shows a clear discontinuity at the threshold. The average number of independent directors increases by approximately one after a violation. Figure 2 also shows that the number of independent directors declines as the firm approaches a violation threshold. Although our model is too simple to generate predictions on how board independence in regions close to a covenant violation should behave, we note that the logic of our model is consistent with a reduction in independent directors 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 when control is still in the hands of shareholders; that is, in regions that approach the covenant threshold from the left.[25]

Panel B of Figure 2 replicates Panel A for the number of non-independent directors. A covenant violation appears to reduce the average number of non-independent directors by 0.5. Consistent with the parametric analysis, there is a (small) overlap between the two confidence intervals, suggesting that this effect is less statistically precise.

Figure 3 presents the evolution of the ratio of independent to non-independent directors over time. To construct this figure, we use all firm-years to calculate annual cross-sectional averages of this ratio, where years are measured in "event time" (i.e., zero is the year in which a violation occurs). We find that the ratio of independent to non-independent directors increases following a covenant violation. The effect is not immediate and peaks at year 3.[26]

Overall, the graphical evidence supports the hypothesis that boards become more independent after covenant violations. There are more independent directors in firms that violate covenants by small margins than in non-violating firms that are close to covenant thresholds. There is somewhat weaker evidence consistent with fewer non-independent directors in firms that violate covenants by small margins. Finally, the ratio between independent and non-independent directors increases in the years following a violation.

[25] As an alternative approach, we also construct figures of estimated regression lines of second-order polynomials on each side of the threshold. This figure has a similar shape to that in Figure 2, and the implied discontinuity is now of about two directors.

[26] In event time, the number of independent directors seems to decline in the years prior to a violation and then increase after a violation. The number of non-independent directors exhibits the opposite behavior.

4.7 Further Robustness

In further robustness tests, we first consider Poisson regressions that take into account the count nature of the dependent variable. 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 β is again a semi-elasticity, so it can be directly compared to the previous estimates. We report the results in column (1) of Table 7. The Poisson regression yields an estimate for β that is just a bit lower than those from log-linear regressions.

Next we consider the possibility that director turnover is simply 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 a board. If this is the case, the effect of covenants on board independence could still be causal but perhaps less interesting, because this would 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 column (2) of Table 7. If anything, the estimated effect is a bit stronger when using only a sample of 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 ignores debt-to-EBITDA covenants in the definition of the violation dummy, which is likely to add noise to our estimates (a debt-to-EBITDA violation happens in 57% of the observations in our sample). We report our results in column (3) of Table 7. The estimate remains qualitatively similar to those in Table 5, but it is 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.

5 Investigating the Mechanism

Do creditors use their enhanced control rights after loan covenant violations to influence the appointment of new directors? Although the evidence is certainly consistent with this interpretation, we cannot rule out an alternative hypothesis that shareholders voluntarily change board composition, without any intervention from creditors. Even if creditors play an active role in governance, finding direct evidence of such an activity is difficult. Here we investigate whether there is suggestive evidence for the "creditor intervention hypothesis."

5.1 Bank Dependence

Bank monitoring is more relevant for firms that, for whatever reason, use more bank loans than other forms of public debt. Thus, if our findings are explained by large creditors – banks in particular – which force changes to the composition of the board, we should expect our results to be stronger for firms that are more bank-dependent.

We first measure bank dependence using the ratio of the amount of bank loans outstanding to total debt. The "high bank debt" dummy variable equals one when the ratio is above the median. The "low bank debt" dummy variable equals one when the ratio is below the median. We then replicate the regressions in Table 4 including interactions of the explanatory variables (except year and firm fixed effects) with the high and low bank debt dummy variables.

Table 8 reports the results. We report results without and with firm-level controls. We find, in both specifications, a higher estimated β for firms with high bank dependence than for firms with low bank dependence. The difference $\beta(high) - \beta(low)$ is, however, not statistically significant.

An alternative proxy for bank dependence is loan size. We create a large-loan dummy that equals one when a firm needs to repay a large loan. A loan is considered large if the ratio of the loan amount at origination to total assets is above the sample median.

We report the results in the last two columns of Table 8. The estimated β for firms with at least one outstanding large loan is higher than for firms with no such loans. The difference $\beta(high) - \beta(low)$ ranges from 32 percentage points to 42 percentage points, and is statistically significant in both specifications.

5.2 Director Characteristics

Who are the directors appointed after covenant violations? To answer this question, we collect additional data on all newly appointed independent directors within two years after a firm first violates a covenant (i.e., the first time that we observe a change from $v = 0$ to $v = 1$). We initially identify 181 such directors whose information is available from BoardEx. Of these, for 144 we have information about current or past employment histories in another publicly listed firm. Panel A of Table 9 presents descriptive statistics for these directors.

To construct a baseline control group, we match each new director to a randomly chosen independent director retained by the same firm.[27] We could match only 133 directors. Panel B reports the characteristics of the control group. In short, we find that newly appointed directors are not much different from retained directors.

An interesting finding is that 63% of the newly appointed independent directors are immediately assigned to audit or finance committees, which suggests that the majority of them have been recruited to work on financial issues. There is, however, limited evidence that such directors are directly linked to creditors. Only 22% of the new directors have direct employment links (current or past) to financial firms, and the vast majority of these links are not with the creditors of the firm.

We also use directors' employment information to investigate whether there may be *indirect* links to creditors. We consider a director to be indirectly linked to a bank if the director holds a position in a firm that borrows from the same bank. Of the 133 new directors with full data, 116 currently work (in board or non-board positions) for firms for which we also have syndicated loan data. Using this subsample, we then look for links with the creditors of the firm. We find that 65% of the new directors have links to their firms' current creditors (not shown in the table). In contrast, only 43% of the comparison group have outside links to current creditors. The difference between the two groups – 22 percentage points – is statistically significant, with a t-statistic of 3.86.

Our choice of control group – retained directors – is a conservative one. If creditors do indeed influence board composition, they may have supported the appointment of linked directors even before a violation occurred. Furthermore, such directors would be more likely to remain on the

[27] The selection criterion is alphabetical; we choose the first director in alphabetical order.

board after a violation. In fact, directors in the control group are not much different in most characteristics (with the exception that they have fewer financial positions in other firms). Thus, our finding that new directors are roughly 50% more likely to have indirect links to current creditors strongly suggests that creditors have some influence in the appointment of new directors.

6 Conclusion

Our evidence shows that creditors influence the composition of boards of directors of borrowers. Firms tend to appoint new independent directors after the violation of loan covenants. This response to covenant violations is stronger for bank-dependent firms. We also show that newly appointed directors are significantly more likely to have links to creditors than are incumbent directors.

We develop a theory that helps with interpretation of the evidence. The model illustrates one particular reason why creditors may care about board independence: Independent directors may favor more conservative projects when firms are close to financial distress. Our theory also shows that creditor influence on board composition is not necessarily ex post efficient (even when it is ex ante efficient). Increasing board independence is not synonymous with improvement in corporate governance, but it could often be the best option available to creditors.

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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) non-financial firms from 1994 to 2008, 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 equals one if the firm violates at least one out of the four covenants during the year in at least one quarter, and equals zero otherwise. The binding distance is defined in the text. Refer to Table A1 in the Appendix for 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	2,801
Number of non-independent directors	2.76	1.65	1.00	1.00	2.00	5.00	13.00	2,801
Ratio of indep.-to-non-indep. directors	3.45	2.58	0.10	1.00	2.67	8.00	13.00	2,801
Number of directors	9.15	2.13	4.00	7.00	9.00	12.00	19.00	2,801
Firm size	3,542	11,324	43	368	1,231	7,144	270,634	2,801
Leverage	0.25	0.16	0.00	0.02	0.25	0.45	0.87	2,801
Firm age	22.56	17.42	1.00	6.00	17.00	42.00	81.00	2,801
Number of segments	2.88	1.91	1.00	1.00	3.00	6.00	10.00	2,801
Market-to-book	1.88	1.15	0.62	1.04	1.54	3.00	8.89	2,801
R&D	0.02	0.04	0.00	0.00	0.00	0.06	0.37	2,801
Stock return volatility	0.38	0.20	0.12	0.19	0.34	0.63	1.74	2,801
Free cash flow	0.09	0.08	-0.79	0.01	0.09	0.18	0.36	2,801
Return on assets	0.15	0.08	-0.66	0.07	0.14	0.25	0.44	2,801
Governance index	9.33	2.63	3.00	6.00	9.00	13.00	17.00	2,801
CEO ownership	0.02	0.05	0.00	0.00	0.00	0.05	0.30	2,801
CEO tenure	7.51	7.51	0.00	1.00	5.00	17.00	49.00	2,801
Covenant violation dummy	0.60	0.49	0.00	0.00	1.00	1.00	1.00	2,801
Binding distance	-1.44	3.64	-24.43	-3.93	-0.52	0.85	6.45	2,801

Table 2: Summary Statistics - Covenant Data

This table reports summary statistics for the current ratio, net worth (in millions of dollars), tangible net worth (in millions of dollars), and debt-to-EBITDA, which are used to defined covenant violations. 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 over earnings before interest, taxes, depreciation, and amortization. Accounting variables are computed for every quarter that correspond to firm-year observations in the sample. The table also shows summary statistics for the value for the covenant thresholds and the binding distance to the threshold in quarters with syndicated loans that contain covenant thresholds. 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.

	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	11,114
Net worth	1,351	5,468	-2,612	156	514	2,505	160,000	11,199
Tangible net worth	1,331	5,338	-2,612	155	509	2,472	152,914	11,107
Debt-to-EBITDA	13.20	814.25	-32,229	0.21	6.58	17.56	76,338	10,443
Threshold - current ratio	1.41	0.45	0.50	1.00	1.30	2.00	3.00	808
Threshold - net worth	902	6,182	5	100	300	1,150	180,563	3,727
Threshold - tangible net worth	525	2,174	-5	55	230	1,000	43,950	2,138
Threshold - debt-to-EBITDA	3.50	1.10	0.55	2.25	3.25	5.00	11.00	6,586
Binding distance - current ratio	0.64	0.67	-0.35	0.03	0.48	1.44	3.31	808
Binding distance - net worth	1.08	2.48	-0.29	0.14	0.52	1.65	19.03	3,727
Binding distance - tangible net worth	13.40	108.26	-0.83	0.14	0.73	2.96	1,019.7	2,138
Binding distance - debt-to-EBITDA	-1.23	2.37	-13.57	-3.28	-0.97	0.89	7.11	6,586

Table 3: Sample Averages for Violation and Non-Violation Samples

This table reports sample averages of board structure and firm characteristics for firm-year observations with no covenant violation and observations with at least one covenant violation. *, **, *** indicate significance at the 10%, 5%, and 1% levels.

	No violation (1)	Violation (2)	Difference (2)-(1)	t-statistic
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	2,011	2,550	539	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	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	

Table 4: Regression of Number of Independent Directors and Non-Independent Directors

Estimates of firm fixed effects panel regressions of the number of independent directors (log) and non-independent directors (log) are shown. The bandwidth around the covenant threshold (which is normalized to zero) is $h = 0.4$. 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, CEO ownership, and CEO tenure. 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.

	Number of indep. directors (log)			Number of non-indep. directors (log)		
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.30***	0.29***	0.41**	-0.23***	-0.24**	-0.52***
	(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
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm level controls	No	Yes	Yes	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	332	332	260	332	332	260

Table 5: Regression of Number of Independent Directors - Different Polynomial Orders and Bandwidths

Estimates of firm fixed effects panel regressions of the number of independent directors (log) are shown. Each column uses a different bandwidth around the covenant threshold (which is normalized to zero). Each row uses a polynomial of a different order. 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.

	Polyn. order	(1) Bandwidth $h = 0.3$	(2) Bandwidth $h = 0.4$	(3) Bandwidth $h = 0.5$
Covenant violation	1st	0.22**	0.14**	0.09*
		(2.32)	(2.50)	(1.85)
Covenant violation	2nd	0.31**	0.30***	0.23***
		(2.01)	(3.13)	(3.45)
Covenant violation	3rd	0.41**	0.29**	0.32***
		(2.14)	(2.30)	(3.04)
Firm level controls		No	No	No
Firm fixed effects		Yes	Yes	Yes
Year fixed effects		Yes	Yes	Yes
Observations		166	332	478

Table 6: Regression of Number of Non-Independent Directors - Different Polynomial Orders and Bandwidths

Estimates of firm fixed effects panel regressions of the number of non-independent directors (log) are shown. Each column uses a different bandwidth around the covenant threshold (which is normalized to zero). Each row uses a polynomial of a different order. 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.

	Polyn. order	(1) Bandwidth $h = 0.3$	(2) Bandwidth $h = 0.4$	(3) Bandwidth $h = 0.5$
Covenant violation	1st	-0.22**	-0.10	-0.03
		(-2.59)	(-1.45)	(-0.30)
Covenant violation	2nd	-0.06	-0.23***	-0.12
		(-0.46)	(-2.67)	(-1.39)
Covenant violation	3rd	0.06	-0.05	-0.11
		(0.30)	(-0.50)	(-0.97)
Firm level controls		No	No	No
Firm fixed effects		Yes	Yes	Yes
Year fixed effects		Yes	Yes	Yes
Observations		166	332	478

Table 7: Regression of Number of Independent Directors - Robustness

Estimates of firm fixed effects panel regressions of the number of independent directors (log) are shown. Column (1) shows estimates of a Poisson regression. Column (2) shows estimates of a regression excluding observations in which the CEO is replaced in one or two years after a covenant violation. Column (3) shows estimates of a regression in which debt-to-EBITDA covenants are ignored in the definition of covenant thresholds and distance to violation. The bandwidth around the covenant threshold (which is normalized to zero) is $h = 0.4$. 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.

	(1)	(2)	(3)
Covenant violation	0.24***	0.38***	0.18*
	(3.34)	(3.95)	(1.73)
Marginal effects (at means)	1.53	2.43	1.15
2nd order polynomial	Yes	Yes	Yes
Firm level controls	No	No	No
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	332	304	268

Table 8: Regression of Number of Independent Directors - Effect of Bank Dependence

Estimates of firm fixed effects panel regressions of the number of independent directors (log) are shown. The high (low) bank debt dummy variable equals one for those cases with ratio of the amount of bank loans outstanding to total debt above (below) the median. The large (small) loan dummy variable equals one for those cases with ratio of the loan amount at origination to total assets above (below) the median. 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, CEO ownership, and CEO tenure. Refer to Table A1 in the Appendix for variable definitions. The bandwidth around the covenant threshold (which is normalized to zero) is $h = 0.4$. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% levels.

	(1)	(2)	(3)	(4)
Covenant violation × High bank debt	0.32***	0.37***		
	(3.11)	(2.74)		
Covenant violation × Low bank debt	0.29*	0.22*		
	(1.94)	(1.81)		
Covenant violation × Large loan			0.41***	0.51***
			(4.34)	(3.50)
Covenant violation × Small loan			0.09	0.09
			(0.66)	(0.69)
β (High) - β (Low)	0.03	0.15	0.32**	0.42**
2nd order polynomial	Yes	Yes	Yes	Yes
Firm level controls	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	332	332	332	332

Table 9: Characteristics of Independent Directors Appointed after Covenant Violations

Panel A reports summary statistics of the characteristics of independent directors appointed within two years after a firm first violates a covenant. Panel B reports summary statistics for a control group of retained directors. To construct the control group, we match each new director to a randomly-chosen independent director retained by the same firm. Director characteristics are collected from the BoardEx database. Refer to Table A1 in the Appendix for variable definitions.

Panel A: New Directors

	Mean	Standard deviation	Minimum	10th percentile	Median	90th percentile	Maximum	Obs.
Male	0.89	0.32	0	0	1	1	1	133
Age	56.1	7.07	40	46	57	65	76	133
MBA	0.17	0.38	0	0	0	1	1	133
Finance field of study	0.23	0.42	0	0	0	1	1	133
Audit or finance committee	0.63	0.48	0	0	1	1	1	133
Other audit or finance committee	0.47	0.50	0	0	0	1	1	133
Other financial role	0.22	0.41	0	0	0	1	1	133
Financial firm connection	0.22	0.41	0	0	0	1	1	133
Financial firm board member	0.18	0.39	0	0	0	1	1	133
Number of current boards	1.96	2.71	0	0	1	4	25	133
Number of past boards	1.48	1.90	0	0	1	4	10	133

Panel B: Matched Control Retained Directors

	Mean	Standard deviation	Minimum	10th percentile	Median	90th percentile	Maximum	Obs.
Male	0.84	0.37	0	0	1	1	1	133
Age	60.20	8.37	38	50	61	71	85	133
MBA	0.17	0.38	0	0	0	1	1	133
Finance field of study	0.14	0.35	0	0	0	1	1	133
Audit or finance committee	0.56	0.50	0	0	1	1	1	133
Other audit or finance committee	0.47	0.50	0	0	0	1	1	133
Other financial role	0.08	0.28	0	0	0	0	1	133
Financial firm connection	0.23	0.42	0	0	0	1	1	133
Financial firm board member	0.20	0.40	0	0	0	1	1	133
Number of current boards	2.14	1.97	0	0	2	4	10	133
Number of past boards	1.52	1.85	0	0	1	4	9	133

Figure 1: Placebo Test

This figure shows estimates of effect of placebo violation on the number of independent directors (log). The real violation is set at 0.00%.

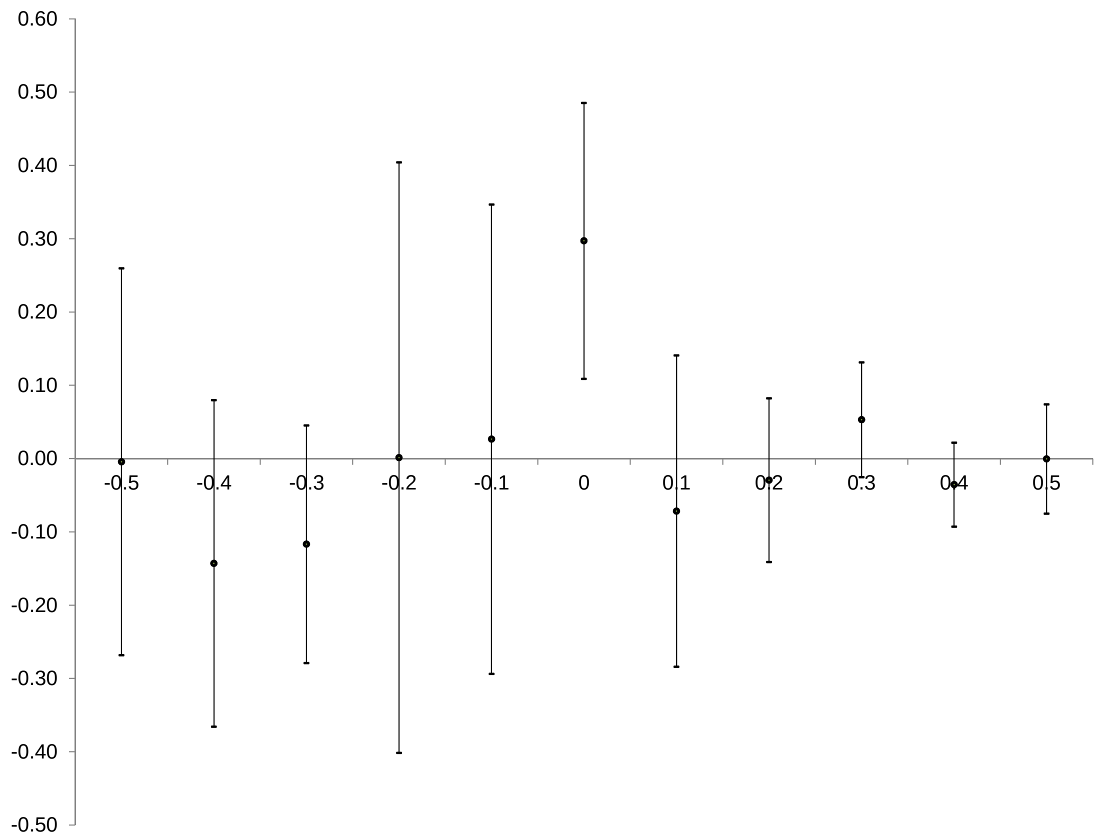
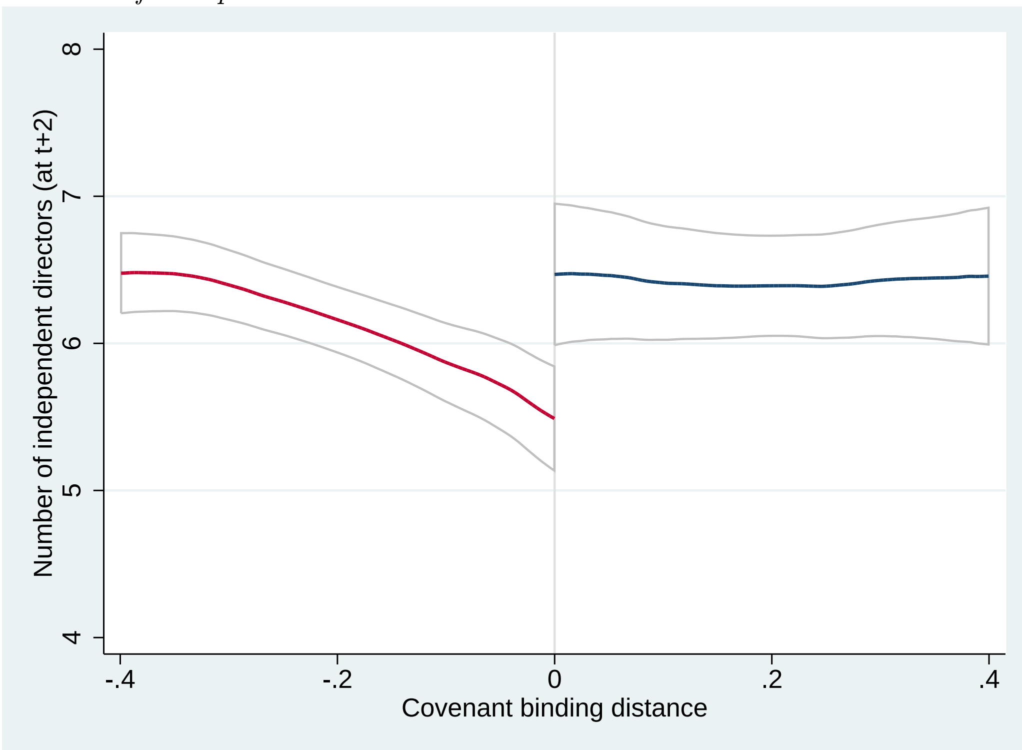


Figure 2: Number of Directors around Covenant Binding Distance

This figure shows nonparametric regression estimates of number of independent and non-independent directors on the covenant binding distance.

Panel A: Number of Independent Directors



Panel B: Number of Non-Independent Directors

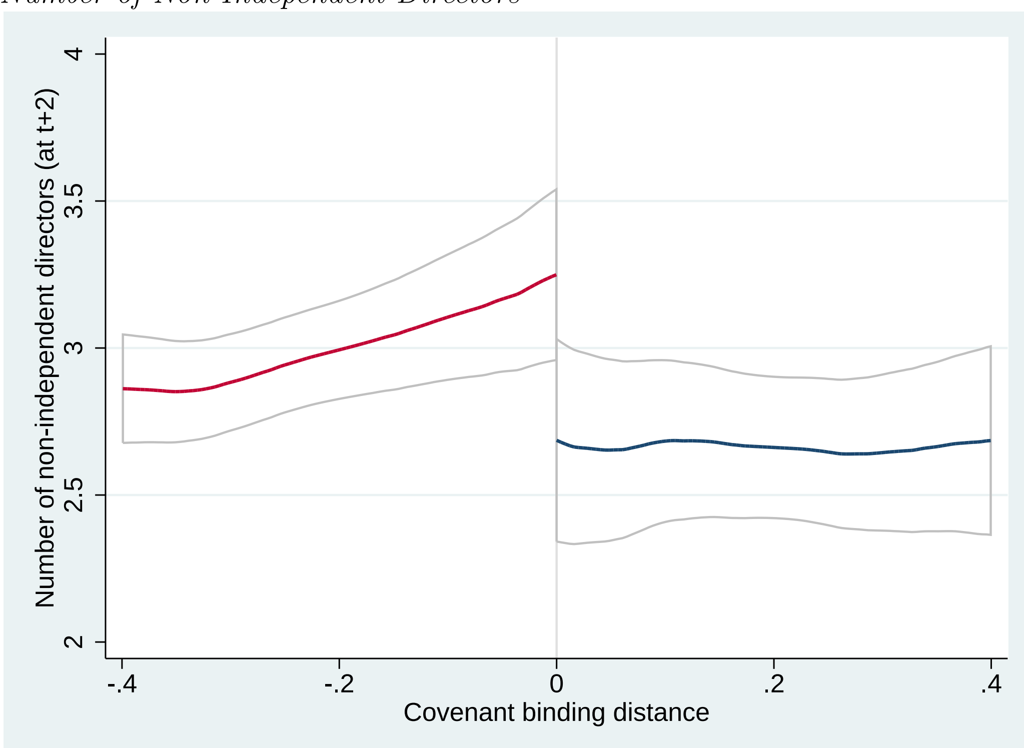


Figure 3: Ratio of Independent to Non-Independent Directors

This figure shows the cross-sectional average of the ratio of independent to non-independent directors before and after covenant violations.

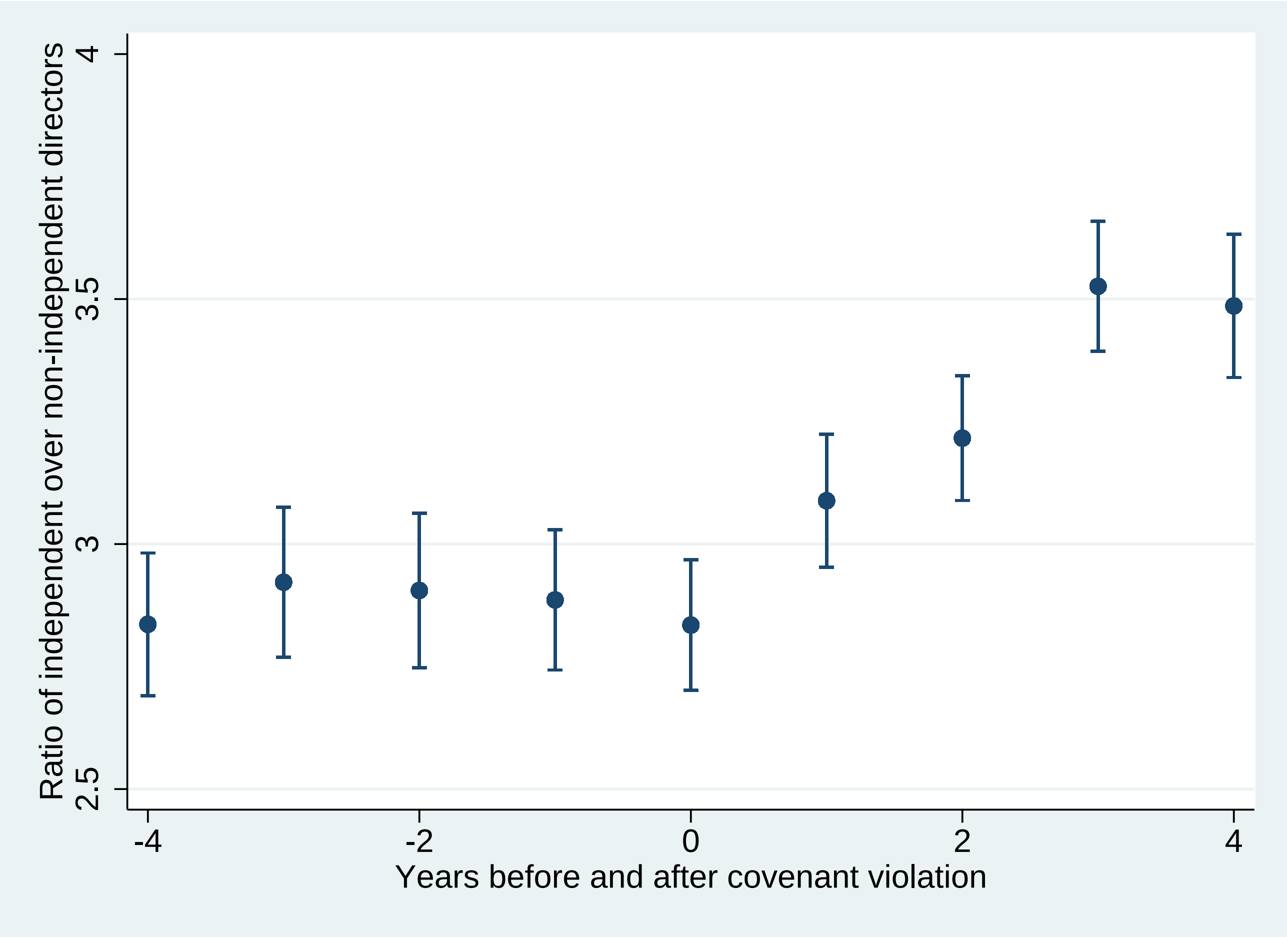


Table A1: Variable Definitions

Variable	Definition
Number of independent directors	Number of board members who are independent directors (IRRC).
Number of non-independent directors	Number of board members who are non-independent directors (IRRC).
Number of directors	Number of board members (IRRC).
Covenant violation dummy	Dummy variable that equals one if there is a presumed covenant violation based on current ratio, net worth, tangible net worth, and debt-to-EBITDA (DealScan).
Firm size	Total assets in millions of dollars (Compustat: AT).
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 × 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	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 incumbent CEO became CEO (EXECUCOMP).
Interest expenses-to-assets	Ratio of interest expenses to total assets (Compustat: XINT / AT).
Net worth-to-assets	Ratio of total assets minus total liabilities to total assets (Compustat: (AT - LT) / AT).
Cash-to-assets	Ratio of cash and short-term investments to total assets (Compustat: CHE/ AT).

Variable	Definition
Male	Dummy variable that equals one when a director is male.
Age	Age (in years) at which a director is elected to the board.
MBA	Dummy variable that equals one when a director holds an MBA prior to board service.
Finance field of study	Dummy variable that equals one when a director has financial education (CFA or degree with major in economics, accounting, finance, management) prior to board service.
Audit or finance committee	Dummy variable that equals one when a director is a member of the finance or audit committee at the violating firm.
Other audit or finance committee	Dummy variable that equals one when a director is or was a member of the finance or audit committee at another firm.
Other financial role	Dummy variable that equals one when a director holds or held a financial role in another firm (e.g., CFO, finance director, treasurer, accountant).
Financial firm connection	Dummy variable that equals one when a director holds or held a position in a financial firm (SIC codes 6000-6999).
Financial firm board member	Dummy variable that equals one when a director holds or held a board position in a financial firm (SIC codes 6000-6999).
Number of current boards	Number of board positions held by a director.
Number of past boards	Number of board positions held by a director prior to board service.

Internet Appendix for "Unfriendly Creditors: Debt Covenants and Board Independence"

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A Risky Debt

Here we prove that the assumption that the optimal debt contract must be riskless is not necessary for the main result.

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 is $s = 0$ 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 occurs only 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 π_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, so 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 raises 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 π_F because communication is achieved at π_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 π_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 $s = 0$: (1) creditors are given board control rights, in which case they choose $\pi = 1$; (2) shareholders retain control rights and choose $\pi_F < 1$; or (3) both shareholders and creditors agree to a level of monitoring $\pi_F < 1$, and the allocation of control is irrelevant.

Only under Case (3) may board independence decline 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 (3), shareholder control and creditor control are equally likely (from a theoretical perspective) to be chosen in equilibrium, and this case makes no directional prediction concerning covenant violations and board independence. In contrast, case (1) unambiguously predicts that board independence should increase after covenant violations.

B An Alternative Sample

Are the results unique to our particular sample? Are they sensitive to our measure of covenant violations? We address both these questions by using an alternative larger sample and a different definition of covenant violations. Following Roberts and Sufi (2009) and Nini, Smith, and Sufi (2012), we consider only violations that are registered with the SEC.

The sample is constructed using information from the 10-Q and 10-K filings on the SEC Edgar website.[1] Nini, Smith, and Sufi (2012) use an algorithm to 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 each 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 of a given year. The sample that results from merging the covenant violation data with our initial sample yields 1,296 firms and 8,514 firm-year observations. This sample has the advantages of a larger size and of using only violations filed with the SEC. The main disadvantage is that we cannot calculate the distance to violation.

Table IA.1 presents descriptive statistics of the variables in our study using the SEC sample. The descriptive statistics in Table IA.1 are comparable to those in Table 1. Figure IA.1 replicates Figure 4 with this alternative sample. We find that the ratio of independent to non-independent directors over time evolves much like the pattern displayed in Figure 4. In fact, the two figures are noticeably similar, clearly showing that the ratio of independent to non-independent directors increases following a covenant violation.

Next, we run regressions similar to those reported in Table 4. Applying the SEC covenant violation definition has the drawback that we cannot construct a measure of the distance to default. Following Roberts and Sufi (2009) and Nini, Smith, and Sufi (2012), we estimate a "quasi-discontinuity" specification:

$$y_{it} = \beta v_{it-2} + \boldsymbol{\delta}\mathbf{h}\left(\mathbf{x}_{it-2}\right) + \alpha_t + f_i + \varepsilon_{it}, \tag{1}$$

where $\mathbf{h}\left(\mathbf{x}_{it-2}\right)$ denotes a vector of functions of control variables, including those variables on which covenants are written. In some specifications, we include high-order polynomials and quintile indicator variables for each of the following five variables: leverage, return on assets, interest expense-to-assets ratio, net worth-to-assets ratio, and cash-to-assets ratio.[2] As before, we report only results in which the dependent variable y_{it} is the log of the number of independent

[1] The data are available on Amir Sufi's website at http://faculty.chicagobooth.edu/amir.sufi/data.html

[2] The estimates for β are not much sensitive to the functional form of controls. In particular, the choice of polynomial order seems to have limited impact on such estimates.

directors.[3]

Table IA.2 reports the estimates for different variations of regression equation (1). In column (1) we do not include any variable in $\mathbf{h}(\mathbf{x}_{it-2})$. In column (2) we add polynomials and quintile indicators of the covenant-related variables. In column (3) we add additional firm controls, and in column (4) we use v'_{it} instead of v_{it}. All four specifications produce similar estimates. The semi-elasticity of the number of independent directors to covenant violations is about 3% to 4%. The size of these effects, especially compared to those found when discontinuity samples are used, suggests that carefully controlling for the distance to a violation substantially increases precision. Nevertheless, these small effects are statistically significant and stable across specifications, thus confirming that the effect of covenant violations on board independence is not unique to our original sample and to our particular measure of covenant violations.

References

Nini, Greg, David Smith, and Amir Sufi, 2012, Creditor control rights, corporate governance, and firm value, *Review of Financial Studies* 25, 1713–1761.

Roberts, Michael, and Amir Sufi, 2009, Control rights and capital structure: An empirical investigation, *Journal of Finance* 64, 1657–1695.

[3]When we use the log of the number of non-independent directors as the dependent variable, we always find a negative effect of covenant violations but, as before, this effect is often not statistically robust.

Table IA.1: Summary Statistics - SEC Sample

This table reports summary statistics for the main variables in the SEC sample. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which we have information on covenant violations in SEC 10-Q or 10-K filings. 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. 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	6.63	2.30	1.00	4.00	7.00	10.00	22.00	8,514
Number of non-independent directors	2.91	1.75	1.00	1.00	3.00	5.00	13.00	8,514
Ratio of indep.-to-non-indep. directors	3.46	2.70	0.08	0.83	2.50	8.00	15.00	8,514
Number of directors	9.54	2.37	4.00	7.00	9.00	13.00	28.00	8,514
Firm size	5,340	15,336	38	336	1,624	13,779	697,239	8,514
Leverage	0.24	0.16	0.00	0.01	0.24	0.43	0.89	8,514
Firm age	27.54	19.80	1.00	7.00	24.00	59.00	81.00	8,514
Number of segments	2.81	1.94	1.00	1.00	3.00	6.00	10.00	8,514
Market-to-book	2.01	1.45	0.53	1.07	1.56	3.38	27.09	8,514
R&D	0.03	0.05	0.00	0.00	0.00	0.10	0.66	8,514
Stock return volatility	0.36	0.20	0.11	0.16	0.31	0.60	2.24	8,514
Free cash flow	0.09	0.09	-0.92	0.00	0.09	0.18	0.36	8,514
Return on assets	0.15	0.08	-0.67	0.07	0.14	0.25	0.43	8,514
Governance index	9.37	2.63	2.00	6.00	9.00	13.00	17.00	8,514
CEO ownership	0.02	0.05	0.00	0.00	0.00	0.05	0.34	8,514
CEO tenure	7.38	7.44	0.00	1.00	5.00	17.00	55.00	8,514
Interest expenses-to-assets	0.02	0.01	0.00	0.00	0.02	0.03	0.17	8,514
Net worth-to-assets	0.47	0.19	-0.71	0.25	0.45	0.73	0.96	8,514
Cash-to-assets	0.11	0.15	0.00	0.01	0.05	0.32	0.92	8,514
Covenant violation dummy	0.05	0.22	0.00	0.00	0.00	0.00	1.00	8,514

Table IA.2: Regression of Number of Independent Directors - SEC Sample

Estimates of firm fixed effects panel regressions of the number of independent directors (log) are shown. Columns (2)-(4) include third-order polynomials and quintile indicator variables of leverage, ROA, interest expense-to-assets ratio, net worth-to-assets ratio, and cash-to-assets ratio. 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, CEO ownership, and CEO tenure. 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.

	(1)	(2)	(3)	(4)
Covenant violation	0.03*	0.04**	0.04**	0.04*
	(1.81)	(2.34)	(2.53)	(1.93)
Marginal effect (at means)	0.20	0.27	0.27	0.27
Only changes from 0 to 1	No	No	No	Yes
Polynomials and covenant variables quintile indicators	No	Yes	Yes	Yes
Firm level controls	No	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	8,514	8,514	8,514	7,741

Figure IA.1: Ratio of Independent to Non-Independent Directors: SEC Sample

This figure shows the cross-sectional average of the ratio of independent to non-independent directors before and after covenant violations, using the SEC sample.