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Abstract

We find that the number of independent directors on corporate boards increases by approximately 24% following nancial covenant violations in credit agreements. Most of these new directors are linked to creditors. Firms with stronger lending relationships with their creditors appoint more new directors in response to covenant violations than firms without such relationships. Moreover, fi rms that appoint new directors after violations are more likely to issue new equity and decrease CEO cash compensation than fi rms without such appointments. We conclude that a fi rm's board composition, governance, and policies are shaped by current and past credit agreements.

JEL Classification: N/A

Keywords: N/A

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Creditor Control Rights and Board Independence*

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Abstract

We find that the number of independent directors on corporate boards increases by approximately 24% following financial covenant violations in credit agreements. Most of these new directors are linked to creditors. Firms with stronger lending relationships with their creditors appoint more new directors in response to covenant violations than firms without such relationships. Moreover, firms that appoint new directors after violations are more likely to issue new equity and decrease CEO cash compensation than firms without such appointments. We conclude that a firm's board composition, governance, and policies are shaped by current and past credit agreements.

JEL classification: G21, G32, G33, G34
Keywords: Corporate boards, Corporate governance, Covenant violations

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After a loan covenant violation, creditors can use the threat of accelerating loan payments and/or terminating credit agreements to extract concessions from borrowers in exchange for contract renegotiation. In practice, creditors rarely need to carry out such threats; most covenant violations lead to contract renegotiation (Roberts (2015)). Covenant violations enhance creditors' bargaining position in renegotiations, as shown by the empirical literature on the impact of violations on firm policies (e.g., Chava and Roberts (2008), Roberts and Sufi (2009), Nini, Smith, and Sufi (2009, 2012), and Falato and Liang (2016)). This literature describes such an improvement in creditors' bargaining power as an increase in "creditor control rights."[1]

How do creditors use their so-called control rights after violations? The threat to curtail access to credit, if credible, may be useful in obtaining immediate concessions, such as a reduction in investment and borrowing (e.g., Chava and Roberts (2008), and Roberts and Sufi (2009)) or even the ouster of a CEO (Nini, Smith, and Sufi (2012)). However, such threats are unlikely to suffice if the goal is to induce proactive and long-lasting changes in policies, such as policies requiring board initiative and monitoring, which require time and effort to implement. In addition, cross-default and cross-acceleration provisions in credit agreements can temper creditors' ability to extract concessions from borrowers.

In this paper, we show that covenant violations trigger changes that have profound effects on a firm's governance. Such governance changes in turn magnify the effect of violations on firm policies, particularly those that require the board to behave proactively. By changing governance, violations can thus have an effect on policies many years after a violation event, implying that current and past credit agreements have a long-lasting impact on a firm's governance.

Our main finding is that covenant violations lead to the appointment of new independent directors. These new directors are not replacements for outgoing directors, implying that board size increases to accommodate the new directors. The effect of violations on the number of independent directors is sizable: Our baseline specification shows that a covenant violation leads to a 24% increase in the number of independent directors (net of departures). Our analysis supports the hypothesis that covenant violations cause changes in board composition.

Figure 1 illustrates our main finding using the raw data. This figure plots the evolution

[1]The term *control rights* is used informally; a creditor has no legal rights to control the borrower following a covenant violation.

of the ratio of independent to non-independent directors (annual cross-sectional averages) in the four years before and after a covenant violation. The figure shows a clear increase in board independence in the years following a violation. Although striking, this evidence is only suggestive. The positive trend in board independence after violations could be explained by other factors, such as omitted variable or sample selection bias.

Our main empirical strategy is adapted from Chava and Roberts (2008), who study the effect of debt covenant violations on investment. To address the possibility that omitted variables could create the appearance of a relationship between financial weakness and board independence, we restrict the sample to include only observations within a narrow interval around the threshold that triggers a covenant violation, while simultaneously saturating the model with fixed effects, polynomials of the distance to covenant thresholds, and other control variables (such as market and accounting performance). This minimizes the concern that the evidence is explained by firms increasing board independence in response to poor financial performance.

We discuss a number of potential concerns and alternative explanations, including the possibility that firms manipulate accounting variables, the existence of firm-specific or aggregate trends, survivorship biases, and measurement issues. Overall, there is little reason to doubt a causal interpretation of the effect of violations on board independence.

This finding raises a number of questions: Who are these directors? Are they related to creditors? If so, how are they related? We show that post-violation directors are similar to ordinary directors in all but one respect: Directors appointed following violations are much more likely to hold positions in other firms that borrow from the same banks. The appointment of directors with such connections to creditors explains 75% of the effect of violations on board independence. We also find that many of these connections arise through syndicate members (lead arrangers or other participants) of the credit agreement that triggered the violation. These findings suggest that creditors influence the appointment of new directors.

Additional evidence corroborates such an interpretation. Intuitively, we expect a creditor to have more leverage over borrowers for whom cross-default and cross-acceleration provisions are less important. In line with this interpretation, we find that the effect of violations on board composition is stronger for firms that regularly borrow from the same banks and for firms that have one large loan. The effect is also economically stronger for more serious violations

(i.e., those that are not renegotiated or waived before the need to report them to the SEC), suggesting that not all types of violations have the same consequences.

What do these new directors do? Firms that appoint new directors after violations are more likely to change certain policies that require board initiative. Such firms are more likely to raise new equity through seasoned equity offerings (SEOs) than firms that violate covenants but do not change their boards. This finding suggests that creditor influence over the board is complementary to new equity issues. Although new equity issues clearly benefit creditors, creditors may find it difficult to influence such decisions through a threat to curtail access to credit immediately after a violation. SEOs require a long and costly process, in which timing plays an important role. A change in the board may be required to guarantee that firms keep their promises to raise new equity in a timely fashion.

We also find that the structure of CEO compensation changes after violations. After violations, but without changes to the board, CEOs experience an increase in cash bonuses that roughly compensates them for a reduction in the value of equity-based compensation. Because this substitution requires the use of scarce cash resources, it is unlikely to please creditors. However, this trend is reversed in firms that appoint new independent directors after violations: Cash bonuses fall and equity-based pay increases more than in firms with no board change. In short, newly appointed independent directors are less likely to approve increases in cash pay to compensate for the decline in the value of CEO stock holdings, which is another reason for creditors to care about board composition.

Although we show that increased creditor control following covenant violations causes changes in board structure, the evidence does not indicate whether creditors explicitly intervene in corporate governance issues. It is true that creditors trigger the process that leads to board changes by declaring a covenant in breach. However, the process that follows could nevertheless largely be in the hands of management or large shareholders who argue for changes in board composition. What we show instead is that the evidence fits a narrative in which creditors benefit from board changes: New directors are more likely to have links to creditors, reformed boards are more likely to adopt some creditor-friendly policies, and new appointments are more likely to happen when creditors are more influential.

Our paper makes several contributions to the literature. Our results are complementary to

the literature on the effect of loan covenant violations on firm outcomes (Chava and Roberts (2008), Roberts and Sufi (2009), Nini, Smith, and Sufi (2009, 2012), and Falato and Liang (2016)). Rather than being just another study on the effects of covenant violations, our work shows that credit agreements have long-lasting effects on how firm decisions are made. Board composition is a means to an end; new directors can influence firm decisions for many years after their initial appointment. We show that changes in firm policies follow the appointment of new directors and that such policies are typically creditor friendly. In addition, we show that creditor influence appears to have profound effects on shareholder governance, which strengthens the typical (but still unproven) interpretation that changes in firm policies after violations are caused by creditor intervention.

Our findings provide direct evidence of the empirical relevance of models of contingent allocation of control rights (e.g., Aghion and Bolton (1992) and Dewatripont and Tirole (1994)). In these models, creditors acquire enhanced control rights in low cash flow states. Our evidence shows that a consequence of such a change in control rights is the appointment of new "monitors" to the board. The evidence thus suggests that enhanced creditor control rights strengthen the monitoring role of the board.

We also contribute to the board of directors literature. Although the endogenous nature of boards is often acknowledged (e.g., Hermalin and Weisbach (1998)), the literature has been unable to provide credible causal estimates of the effect of firm characteristics on board structure. It has also been difficult to identify firm-level variables that have an economically (rather than only statistically) significant effect on board composition (see Ferreira, Ferreira, and Raposo (2011)). In addition, our results help to explain the observed positive relationship between leverage and board independence (Boone, Casares Field, Karpoff, and Raheja (2007), Coles, Daniel, and Naveen (2008), and Linck, Netter, and Yang (2008)). Our evidence shows that leverage may directly affect both board independence and size: Highly leveraged firms are more likely to violate covenants, which may lead to the appointment of new independent directors.

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 evidence that, in negotiated restructurings, banks influence the appointment of directors both directly and through share ownership. Kaplan and Minton (1994) find that

poor financial performance triggers the appointment of former bank directors to the boards of Japanese firms, which indicates that banks actively influence corporate governance. Anderson, Mansi, and Reeb (2004) find a negative association between board independence and the cost of debt, as the presence of independent directors improves the quality of financial accounting reports. Kroszner and Strahan (2001) and Guner, Malmendier, and Tate (2008) study the costs and benefits of the presence of bankers on boards and find evidence of conflicts of interest between creditors and shareholders. In this paper, we show that banks influence board appointments outside bankruptcy, and we provide a causal estimate of the effect of covenant violations on board composition.

Nini, Smith, and Sufi (2012) show that CEO turnover increases after covenant violations. 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, as we show that the effect of covenant violations on board composition is stronger for the subset of firms that *do not* replace their CEOs after a covenant violation. Becker and Stromberg (2012) show that a 1991 change in the law that required boards to consider the interests of creditors in financially distressed firms led to an increase in leverage among affected firms and a reduction in the use of covenants. Their evidence suggests that, as boards become more likely to consider the interests of creditors, covenants become less important. Our findings are broadly consistent with this hypothesis.

1 Institutional Background

Although shareholders formally elect directors in annual meetings, the incumbent board often controls the process of nominating directors. The board's nomination committee usually selects a single set of nominees, who are then included in the company's proxy material for shareholder voting. Most companies do not offer shareholders access to the ballot, meaning that shareholders do not have the right to nominate directors and include them in the company's proxy material. If a group of shareholders wishes to nominate directors, it has to file an alternative proxy, which can be extremely costly. This proxy contest is often a real deterrent to shareholder nominations (Bebchuk (2007)). Despite their costs, proxy contests have recently become more common, and

thus shareholder influence over nominations may have increased (Fos (2016)).

Until 2010, companies had the right to exclude shareholder proposals for proxy access. In 2010, however, the SEC passed a rule mandating shareholder proxy access. Many large companies lobbied against this rule.[2] The lobbying was successful, and the rule was later overturned by the U.S. Court of Appeals for the District of Columbia Circuit. In addition, most boards employed plurality voting rules until 2007. Under plurality voting, directors can be elected with a single vote. Since 2007, majority voting has become increasingly popular. However, even majority voting is not a meaningful hurdle when there are no opponents. Becker and Subramanian (2013) provide a summary of director elections: "It is well known that U.S. director elections are largely a formality: incumbents typically nominate themselves, for elections that are almost always uncontested, and are re-elected with virtual certainty" (p. 1).

In brief, the main hurdle for a director to be elected is thus not the election itself but his/her inclusion on the slate of directors. Thus, to nominate a director, one needs to convince the incumbent board.[3] While small shareholders have virtually no say in director nominations, large investors may have an effect on director nominations by negotiating directly with the CEO and the board. These investors can be either large shareholders or large creditors. Indeed, to the extent that creditors care about board composition, there is no reason to presume that they will have less influence on board nominations than shareholders: In both cases, investors have to negotiate with the incumbent board.

The reasons for creditors to care about board composition are not obvious. Even if creditors can influence board appointments, directors still owe fiduciary duties to shareholders.[4] In addition, excessive and explicit intervention by creditors may force them to owe fiduciary duties to shareholders or, in the case of bankruptcy, make them subject to equitable subordination

[2]For example, IBM Chief Executive Samuel Palmisano wrote in a letter to the SEC: "A highly leveraged corporate raider or hedge fund could easily propose a slate of directors focused on bleeding a company's balance sheet through the payment of special one-time dividends, to the detriment of job-creating investments." (See http://articles.latimes.com/2010/aug/26/business/la-fi-sec-proxy-20100826).

[3]Becker and Subramanian (2013) show that out of the 16,822 candidates for board positions in Russell 3000 companies in 2011, only 26 were proposed by shareholders, while the remaining were proposed by the incumbent directors.

[4]However, depending on the company's charter and state corporate law, a director may also owe fiduciary duties to other stakeholders, such as creditors, employees, customers and the community. For example, in Delaware, directors also owe fiduciary duties to creditors in the vicinity of insolvency (see Becker and Stromberg (2012)).

(i.e., courts may treat their claims as subordinate on equitable grounds). Thus, debt contracts typically do not give creditors explicit rights over board appointments.[5] However, this does not mean that creditors abstain from corporate governance activism. Baird and Rasmussen (2006) and Nini, Smith, and Sufi (2012) argue that creditors' influence on corporate governance is often subtle and exercised behind the scenes, which makes the empirical documentation of their activities challenging.[6]

2 Data

We construct our sample from the non-financial firms in the Investor Responsibility Research Center (IRRC) database, from which we obtain board data. We complement the IRRC data with data on director characteristics from BoardEx. We obtain accounting and segment data from Compustat and stock returns from CRSP. CEO compensation and tenure data are from ExecuComp.

We obtain data on syndicated loans from the DealScan database. DealScan includes information on syndicated loans extended by banks to firms, including loan amounts, maturity, type of loan, syndication, covenants, and pricing. We restrict the sample to loans with information on maturity and spread over the LIBOR (all-in spread drawn), and we eliminate firms with loans for which we do not have any covenant information or that do not include a covenant on the current ratio, net worth, tangible net worth, or debt-to-EBITDA ratio. We merge the loan data with firm and director data using company name, ticker, and loan origination date.[7]

Our main sample uses accounting data from 1994 to 2006 and board data from 1996 to

[5]In our sample of credit agreement contracts, we find no case of provisions that give creditors formal rights to appoint directors. There is, however, direct evidence that creditors do care about control considerations and board composition. Credit contracts often contain *poison puts*, which are covenants that allow for early repayment (at par or at a premium) in the event of significant changes in control. These covenants give creditors some influence over appointments, although they stop short of giving creditors explicit rights over nominations.

[6]According to Nini, Smith, and Sufi (2012), "there is substantial anecdotal evidence that creditors work behind the scenes to affect changes in the way that the company is managed. Although lender liability laws protect equityholders from creditors that directly interfere with the management of the firm, creditors can offer advice to management and the board, quid pro quo, and suggest actions the company can take to maximize the chance of receiving a covenant waiver" (p. 1723).

[7]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.

2008 to allow for lags in our specifications. Data availability determines the beginning of the sample period; before 1996, there are no IRRC board data. The sample period is determined by economic considerations. First, we do not include the period of the recent financial crisis, which led to major changes in bank behavior and regulation, credit conditions, and the financial performance of borrowers. Second, until 2006, "covenant-light contracts" were virtually non-existent, while since 2007, and especially in more recent years, they have rapidly become common, with nearly 40% of all new loans being covenant-light (Becker and Ivashina (2016)). Covenant-light contracts normally have the same number of covenants as covenant-heavy contracts but weaker covenant enforcement. The wide use of covenant-light contracts is thus likely to attenuate the effect of violations on firm policies. Although our baseline sample includes only data from 1994 to 2008, we also run all of our main tests for an extended sample covering the 1994-2014 period.

We identify whether a firm violates any of the covenants in any quarter and how far away the relevant accounting variables are from the covenant threshold. Specifically, for each loan, we first obtain covenant thresholds on the 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, we use the minimum threshold (or the maximum for the debt-to-EBITDA ratio) for each covenant across all active loans in a given quarter. We use Compustat data at a quarterly frequency to compute the accounting variables. If the accounting variable is equal to or below the threshold, there is a (presumed) covenant violation. In the case of the debt-to-EBITDA covenant, a violation occurs if the accounting variable is equal to or above the threshold.

As only few other papers use debt-to-EBITDA covenants (e.g., Demiroglu and James (2010), Denis and Wang (2014), Freudenberg, Imbierowicz, Saunders, and Steffen (2015)), we pay special attention to the construction of this variable. We read a sample of 50 credit agreement contracts of borrowers that experienced covenant violations in our sample. The most common definition of debt is "total consolidated indebtedness," (e.g., consolidated gross debt). In only a few cases does debt exclude subordinated debt (only two cases in our contract sample) or is measured net of cash holdings (no cases found in our contract sample). In Denis and Wang's (2014), total debt is also the most common definition of debt for contracts that establish a debt-

to-EBITDA limit. We assume that total debt is equal to long-term debt plus debt in current liabilities. We measure EBITDA as net income minus extraordinary items, plus income taxes, interest expenses, and depreciation and amortization. For the purpose of covenant calculations, EBITDA is measured over a test period equal to the four most recent fiscal quarters.

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. We call the minimum distance to the threshold across the four covenants the *binding distance*, which is defined as follows:

$$D_{it} \equiv \min_{j,k} \tilde{D}_{itjk}, \text{ where} \tag{1}$$

$$\tilde{D}_{itjk} \equiv \min_{z} \frac{C_{itjk} - T_{itjkz}}{T_{itjkz}}, \tag{2}$$

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); z denotes an active 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; and T_{itjkz} is the threshold for active loan z, covenant type k, in quarter j of year t for firm i. The definition in (1) applies strictly only to the current ratio, net worth, and tangible net worth covenants. For the debt-to-EBITDA covenant, a violation occurs when the accounting variable is above the threshold, and thus $\tilde{D}_{itjk}$ is defined analogously by $T_{itjkz} - C_{itjk}$. We also calculate an alternative measure of distance to threshold—called *tightness*—in which the denominator in equation (2) is the standard deviation of the accounting variable over the full sample period. We use this variable for additional tests later in the paper.

Definition (1) implies that a violation event is a firm-year observation in which the firm violates at least one covenant in at least one quarter of the year. For expositional simplicity, we allow D_{it} to assume negative values; a firm-year observation that displays "negative distance" is a violation event.[8]

[8]Because EBITDA may assume values that are close to zero or even negative, the debt-to-EBITDA ratio becomes meaningless in such cases. Thus, we replace negative values with a debt-to-EBITDA ratio equal to its 99th percentile in the sample of positive EBITDA observations. The results show little sensitivity to how such cases are treated. In particular, the results are similar if all negative EBITDA observations are dropped.

Our final (baseline) sample covers 597 firms and 2,801 firm-year observations. For this sample, we find that 51% of the firms violate a covenant at least once during the sample period (305 firms), and 24% of the firm-year observations include a violation event (675 firm-year observations).[9] Because a violation event requires a covenant violation in only one quarter of the year, the number of observations in violation is mechanically inflated relative to studies that use quarterly data. At a quarterly frequency, only 16% of the observations in our sample are violation events.

Our procedure for defining violations may overstate the actual number of violations because we do not consider covenant threshold renegotiations. Roberts (2015) shows that credit agreements are renegotiated on average every nine months, often outside violation events. Denis and Wang (2014) show that covenant thresholds are often renegotiated when firms are close to the threshold. In their sample, approximately 50% of contracts would be in violation if the original covenants had not been relaxed. Their results suggest that creditors gain more influence when a firm is close enough to a violation that, without renegotiation, the firm would almost certainly trigger the covenant. Our measure of "violations" includes cases in which violations did not occur as a consequence of threshold renegotiations. We choose to retain such cases because renegotiation is one of the possible mechanisms through which creditors influence firm choices.

As in Chava and Roberts (2008) and Falato and Liang (2016), we infer violations from threshold and accounting data. This procedure may lead to coding and other 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). We may also misstate the number of actual violations because banks may waive covenants and because the accounting numbers, such as earnings-based measures and net worth, used in credit agreements may differ from those reported on financial statements. In sum, there are a number of possible sources of measurement errors, although we see no a priori reason to suspect that such errors would bias the results toward our hypothesis.

The debt-to-EBITDA variable can be noisy, as it may vary across contracts depending on how debt is defined. Because debt-to-EBITDA is the most frequent covenant in our sample,

[9]For comparison, Falato and Liang (2016), who also use data at an annual frequency, find that 21% of their firm-year observations include a violation event.

we face a trade-off: Using this variable substantially increases the variation in the sample, but it also adds noise. In the robustness section, we also run our main tests without this variable.

To minimize concerns about measurement errors, in Subsection 4.5, we consider an alternative definition of violations, which includes only covenant violations registered with the SEC. This definition has the advantage of eliminating many of the concerns above. There are, however, two disadvantages: We thereby obtain a severely reduced sample size, and we may miss many renegotiated violations. Our results, however, appear stronger when we consider only registered violations, which suggests that, if anything, measurement errors in our original definition of violations work against our hypothesis.

Table 1 presents descriptive statistics of each variable in our main sample. Table A.1 in the Appendix provides variable definitions and data sources. The median of the binding distance is 0.30. The minimum and the maximum of the distance are quite extreme. For example, the minimum distance in the sample is -7.36 (more than seven times the threshold that triggers violation), which is one order of magnitude larger than the 10th percentile (-0.63). 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. Our empirical approach guarantees that such extreme values have no effect on our results, since we use (discontinuity) subsamples that exclude observations that are far from the threshold.

As our sample is constructed mainly by the intersection of three data sources (Compustat, IRRC, and DealScan), it is instructive to consider how the sample selection procedure affects the sample and the types of firms included in our study. Relative to studies that use covenant data from DealScan such as Chava and Roberts (2008), our sample is smaller for two reasons: the need to match data with the IRRC sample and the use of annual versus quarterly data. Table IA.1 in the Internet Appendix presents a comparison of the averages of each variable across data sources.[10] This comparison reveals that firms in our sample are substantially larger than those in both the Compustat and the DealScan samples, which is expected because IRRC collects data for S&P 1,500 companies only. Consistent with this fact, our sample has fewer

[10] As Compustat is the primary source for all accounting information, we define the restricted samples by their intersection with Compustat. Thus, the DealScan sample is defined as all observations in Compustat for which we could find data on covenants in the DealScan database. Similarly, the IRRC sample contains all firm-year observations for which data are available in both Compustat and IRRC.

covenant violations (24%) than the DealScan sample (34%). However, our sample firms are on average smaller than those in the IRRC sample. This is because larger firms are less likely to have syndicated loans with restrictive covenants.[11] In contrast, sample selection has virtually no effect on average board characteristics. If anything, our sample has slightly smaller and more independent boards than the IRRC sample, but such differences are not meaningful.[12]

Table IA.2 in the Internet Appendix reports descriptive statistics for the value of the accounting variable (C_{itjk}), threshold (T_{itjkz}), binding distance ($\tilde{D}_{itjk}$), and tightness for each covenant type (at a quarterly frequency). The average current ratio is 2.04, while the corresponding average threshold is significantly lower at 1.41. The average net worth and tangible net worth are significantly higher than their corresponding thresholds. The debt-to-EBITDA is the covenant with the lowest absolute distance to the threshold. The average debt-to-EBITDA is 3.20, while the corresponding average threshold is only slightly higher at 3.49. We conclude that, as expected, the average firm is not violating any covenant.

Table IA.3 in the Internet Appendix presents covenant tightness at loan origination and the number and frequency of violations for our sample (at a quarterly frequency), as well as comparable statistics for the sample in Chava and Roberts (2008). Our sample shows a lower fraction of observations with covenant violations than that of Chava and Roberts (2008). They report that 15% of their firm-quarter observations correspond to a violation of the current ratio covenant and 14% to a violation of the net worth (and tangible net worth) covenant, while we report 9% and 5%, respectively. This is expected since our sample is smaller and contains larger firms on average due to the use of board data. Conditional on the presence of covenants, however, the covenant characteristics are similar. In Chava and Roberts's sample, the values for covenant tightness at origination are 1.09 (current ratio) and 0.68 (net worth and tangible net worth), while in our sample the values are 1.44 (current ratio), 0.58 (net worth), and 0.65 (tangible net worth).

[11] Despite the restriction imposed by the IRRC data, our firms are not substantially larger on average ($3.5 billion in assets) than those in other studies using loan covenant data, such as Nini, Smith, and Sufi (2009) ($3.3 billion) and Denis and Wang (2014) ($2.8 billion).

[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. The statistics for the board variables are also similar to those in other studies using IRRC data (e.g., Ferreira, Ferreira, and Raposo (2011)).

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. We start by clarifying our terminology. We define the "pure" (in the sense of "uncontaminated") effect of a covenant violation as the effect that a violation would have while holding financial performance and other confounding factors constant. Violations typically occur when financial performance deteriorates; thus, board changes after violations may be a joint consequence of the effect of the violation and the decline in performance around the violation event. The main empirical challenge is to isolate the pure effect of a violation from the effect of financial performance and other confounding factors. This is our main goal.

Following the previous literature (e.g., Chava and Roberts (2008), Roberts and Sufi (2009)), we call the pure effect of a covenant violation *an increase in creditor control rights*, where control rights refer to informal power that creditors have over the firm in negotiations. Although this terminology is somewhat ambiguous, we think it is useful because it highlights that a pure covenant violation (as we define above) is simply a change in the outside options of both parties. Should negotiation break down after a violation, the creditor typically has the right to exercise the threat of terminating the credit agreement and requesting repayment of the loan. Controlling for financial performance and other factors, a violation can affect firm outcomes only because creditors have the right to make threats that were not possible before the violation. This does not mean that creditors actually use their enhanced control rights to obtain concessions from the firm. It could be that management or large shareholders encourage changes in policies in response to increased creditor control (i.e., in response to creditors' potential to make threats), even absent any indication that creditors favor a particular policy. We call creditors' actual use of explicit or implicit threats to obtain changes in policies *creditor intervention*. Thus, creditor control rights and creditor intervention are distinct concepts.

Our main goal is to show that an increase in creditor control rights caused by covenant violations leads to the appointment of new directors. While we do not provide direct evidence that creditor intervention causes the appointment of new directors, our secondary goal is to

analyze the mechanisms in greater detail. We present some evidence consistent with creditor intervention being one of the mechanisms through which board changes occur.

To establish the pure effect of violations on board changes, the most serious difficulty is the possibility of a spurious correlation between violations and board independence due to omitted variables or sample selection issues (e.g., survivorship bias). Another issue is reverse causality: Expectations of future changes in board composition affect the current likelihood of covenant violations. We argue below that reverse causality is difficult 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 primarily on results obtained in discontinuity subsamples constructed using narrow windows around the threshold. However, this approach is arguably not sufficient for addressing firm heterogeneity in our particular application. There are at least four challenges to apply a standard regression discontinuity design to our problem:

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

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

(3) The use of ratios as "running" variables. To understand this problem, consider, for example, the debt-to-EBITDA variable. Most of the variation in this variable comes from its denominator because earnings vary more than debt. Because debt-to-EBITDA is a convex function of EBITDA, for a given amount of variation in EBITDA, this ratio will vary more when it is initially low than when it is initially high. Thus, 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 that are 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 one that is not as tight. An additional issue

arises because covenant thresholds are endogenously chosen (Gârleanu and Zwiebel (2009) and Demiroglu and James (2010)).

To address these concerns, we proceed as follows. First, we use firm fixed effects, which address the most obvious selection problems and time-invariant omitted variables. Second, we perform balancing tests that show that observable firm characteristics are either similar on both sides or fully "explained" by the distance to threshold variable. Third, we control for the distance to a violation threshold and a long list of time-varying firm variables, including measures of market and operating performance. Finally, if spurious correlations 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 placebo tests aimed at detecting jumps in board independence at other points near the actual covenant thresholds.

3.2 Empirical Model

Our baseline specification is given by

$$\ln y_{it} = \beta v_{it-2} + \sum_{p=1}^{P} \left[\gamma_{p0} + \gamma_{p1} v_{it-2}\right] D_{it-2}^{p} + \alpha_t + f_i + \boldsymbol{\delta}\mathbf{x}_{it-2}' + \varepsilon_{it}, \tag{3}$$

where y_{it} is either the number of independent directors or the number of non-independent directors; v_{it} is an indicator variable that takes the value of one if firm i violates 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 $\mathbf{x}_{it}$ is a vector of control variables. Our default option is to cluster standard errors by firm; we obtain similar standard errors when we cluster by industry or industry-year.

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 the interpretation of the results, the tables also present the marginal effects of a violation evaluated at the sample average of y_{it}: $\partial y_{it}/\partial v_{it-2} = \beta\overline{y}$.

We consider either the number of independent directors or the number of non-independent

directors as the outcome variable, not the ratio between them or the ratio of independents to board size. We choose this approach because it is more informative and general than focusing on ratios; we can always calculate the effect on the ratio from the effects on the levels. In particular, ratios do not indicate what happens to board size after violations, while our approach allows us to infer changes in both the proportion of different types of directors and the total number of directors. In the robustness section, we also present results in which y_{it} is the fraction of independent directors on the board.

We lag all explanatory variables by two years because changes in board structure occur slowly. Board directors can be replaced only at regular intervals of no less than one year at annual shareholder meetings and often up to three years in the case of firms with staggered-board provisions in their charters. Moreover, new directors have to be nominated well in advance of annual meetings. State corporate law and a firms's charter regulate the appointment of directors. These rules often imply a significant lag between the decision to appoint a new director and its actual implementation.[13] Note also that we use fiscal years, not calendar years; thus, even in the unusual case in which changes occur within one or two months after a violation, they will typically be recorded with a lag of one year (unless a special shareholder meeting is called, which is rare). Finally, we note that, because board turnover is typically low, the effect of violations on appointments is cumulative: The effect in two years is (approximately) the sum of year 1 and year 2 appointments. Later, we present estimates using alternative lags.

As is typical in regression discontinuity designs, the sample includes only those observations for which the absolute value of the binding distance is less than h (the bandwidth). 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 sample that includes 665 observations (24% of the full sample).[14] The standard deviation of the binding distance is 1.45 (see Table 1); thus, one unit of binding distance is equivalent to 0.69 of a standard deviation. Therefore, the $h = 0.4$ bandwidth is roughly

[13]Of course, there are also situations in which appointments can be made quickly, such as when directors resign or when a new space can be created and temporarily filled until the next formal election.

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

equivalent to 0.28 of a standard deviation.

The standard 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 almost equally balanced sample size on each side of the threshold. Table IA.4 in the Internet Appendix shows that the samples on each side of the threshold for the baseline bandwidth ($h = 0.4$) are not balanced. The split between $v_{it} = 0$ and $v_{it} = 1$ is approximately 68% and 32%, respectively. One possible reason that observations cluster on one side of the threshold is the choice of an insufficiently narrow bandwidth. Table IA.4 also shows that the samples become more balanced as we narrow the bandwidth. In particular, with $h = 0.2$ (approximately 14% of a standard deviation) the split is 54%-46%, which appears fairly random. This suggests that our choice of bandwidth is the likely cause of the sample imbalance. The trade-off we face is that narrower bandwidths improve sample balance but reduce sample size. Because one might be instinctively skeptical of estimates from subsamples containing only 10% or less of the full sample, we choose to focus on the relative large sample defined by $h = 0.4$ and check the robustness of the results to larger and smaller bandwidth choices.[15]

Another possible reason for sample imbalance is manipulation: Firms may manipulate earnings to avoid crossing the threshold. Although sample balance does not appear to be an issue for sufficiently low h's, we cannot a priori rule out manipulation or other similar sample selection concerns, such as survivorship bias.[16] We thus 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, we ensure that our results are driven by firms that are on both sides of the threshold, which is particularly useful for addressing survivorship bias. This comes at the cost of 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.

[15]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 explicitly concerns the effect of the threshold on the outcome variable (board independence).

[16]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)).

The combination of fixed effects and the use of observations near the threshold mitigates concerns about omitted variables. With fixed effects, our key identification assumption is that the expectation of an imminent increase in board independence does not make firms less likely to manipulate earnings to avoid covenant violations. Although we cannot test this assumption, it is plausible. However, as is the case with any identification assumption, it may be invalid.[17]

3.3 Discontinuity Sample: Descriptive Statistics

Table 2 presents average values for each variable on each side of the threshold for the discontinuity sample with the baseline bandwidth ($h = 0.4$). We find that narrow violators have significantly higher leverage than narrow non-violators. This is a mechanical result; leverage directly affects the variable that defines a violation. There are no statistically significant differences in the other firm characteristics. In particular, board characteristics – past, current, and future – are similar on both sides of the threshold.

Table IA.5 in the Internet Appendix reports the same comparison for the complement of the discontinuity sample. There are many economically and statistically significant differences, including firm size, leverage, number of segments, market-to-book, volatility, free cash flow, return on assets, and CEO tenure. This suggests that the bandwidth choice of $h = 0.4$ controls for observable characteristics, including those related to the financial position of the firm.

Panel A of Table IA.6 in the Internet Appendix presents summary statistics for the discontinuity sample ($h = 0.4$). Compared to the full sample statistics in Table 1, firms in the the discontinuity sample are smaller (average value of assets \$2.7 billion) and more levered (31%). They are also more likely to violate covenants (32%). These differences are unsurprising; by definition, the discontinuity sample contains only observations that are close to the violation threshold. All other variables in Table IA.6 appear similar to those in the full sample. For completeness, Panel B presents summary statistics of all observations that are not in the discontinuity sample.

[17]Note that our approach does not require manipulation to be nonexistent or random. Our analysis remains valid if manipulation is related to time-invariant firm characteristics or to changing characteristics included in our regressions.

4 Empirical Results

4.1 Graphical Analysis

Figure 1 depicts our finding, making it clear that we do not need sophisticated econometrics to uncover it. In this section, we complement this graphical evidence with nonparametric results.

Panel A of Figure 2 plots estimates of nonparametric regressions of the number of independent directors on (the negative of) the binding distance. To facilitate the visualization, we reverse the convention in definition (1), such that—in the figures only—negative values on the x-axis represent a non-violation and positive values represent a violation. The figure shows only observations in the interval $[-0.4, 0.4]$. We run separate regressions for each side of the threshold. To be consistent with the regression model in (3), we measure the dependent variable at year $t+2$. The thick lines are fitted regression lines, and the thin lines are 95% confidence intervals. The regression uses an Epanechnikov kernel with a bandwidth of 0.05.

Figure 2 shows a clear discontinuity at the threshold. The average number of independent directors increases by approximately 0.8 after a violation. Figure 2 also shows that the number of independent directors declines as the firm approaches a violation threshold, jumps upward at the threshold, and then resumes its decline thereafter. Although we have no reason to predict such a pattern, we note that the relationship between the number of independent directors and the binding distance appears similar on both sides of the threshold.[18]

Figure 2 also clarifies the reason that the average number of independent directors at $t+2$ is roughly the same on both sides of the threshold (see Table 2): The relationship between the number of independent directors and the binding distance is non-monotonic.

The nonparametric results show clear evidence of an increase in the number of independent directors following a violation, but these results are subject to some concerns. One specific concern is that a small number of firms that experience multiple violations could explain the estimated effects. To address this concern, we define a *first violation* indicator as

$$v'_{it} = \{1 \text{ if } v_{it} = 1; 0 \text{ if } v_{is} = 0 \text{ for all } s < t; \text{ missing otherwise}\}. \tag{4}$$

[18] As an alternative approach, we construct figures of estimated regression lines of second-order polynomials on each side of the threshold. These figures have a similar shape to that in Figure 2.

That is, v'_{it} considers only the first violation event experienced by firm i. After such an event, we assume that the firm never returns to a non-violation state.[19] Panel B of Figure 2 replicates Panel A using the first violation indicator. We find that, if anything, the discontinuity appears more pronounced in this sample; the implied effect is approximately 1.2 directors.

Finally, Figure IA.1 in the Internet Appendix plots estimates of the effect of violations on the number of non-independent directors. Covenant violations appear to reduce the average number of non-independent directors, but there is substantial overlap between the two confidence intervals, suggesting that the effect is statistically less precise (in addition to being economically less important) than that for the number of independent directors. This is indeed confirmed by the parametric analysis below.

Overall, the graphical evidence supports the hypothesis that boards become more independent after covenant violations. The ratio between independent and non-independent directors increases in the years following a violation. In addition, 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, however, weaker evidence that covenant violations affect the number of non-independent directors, suggesting that any increase in independence is driven primarily by new appointments.

4.2 Primary Results

Table 3 reports our primary results. The dependent variable is the logarithm of the number of independent directors. Column (1) of Panel A reports the estimate of β from a (local) regression that 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 24% in the number of independent directors. This implies an increase of $0.24 \times 6.4 = 1.5$ independent directors, evaluated at the (full) sample average of the number of independent directors.[20] This effect is

[19] A variation of this definition in which all "zeros" after the first "one" are replaced with "ones" is also possible. However, under such a definition, there are cases in which a firm in "violation" could have a positive binding distance, which is nonsensical. We choose the current definition to avoid those cases.

[20] As expected, this result is driven primarily by firms with lower board independence. For firms with a below-median number of independent directors, the estimated β is 0.33 ($t = 2.96$), while for those with above-median independence, the estimated β is 0.07 and statistically insignificant.

approximately twice the effect in Figure 2, which suggests that the inclusion of firm and year fixed effects amplifies the effect of violations on board independence. The estimated effect is also economically important and much larger than those documented in most of the empirical literature on boards (see the discussion in Ferreira, Ferreira, and Raposo (2011)).[21]

The specification in column (2) includes a long list of control variables: operating performance (return on assets), growth opportunities (market-to-book), firm size (assets), leverage, firm age, number of business segments, R&D-to-assets ratio, stock return volatility, free cash flow, governance index (Gompers, Ishii, and Metrick (2003)), and CEO ownership and tenure. All of these variables are lagged by two years. To save space, we do not report the coefficients of the control variables. We find that neither market-to-book nor return on assets appears to be negatively related to board appointments. Although return on assets enters negatively, its coefficient is neither economically meaningful (-0.78) nor statistically significant ($t = -1.16$). A one-standard-deviation decrease in ROA (-0.08) implies a less than 1% increase in the number of independent directors. Surprisingly, market-to-book enters positively, but it is statistically insignificant ($t = 1.55$) and economically small: For the average firm, a 60% increase in market-to-book (equivalent to one standard deviation) leads to an 8% increase in the number of independent directors. Among the control variables, only (log) firm age ($0.19, t = -1.96$) and (log) number of segments ($0.11, t = 2.27$) display statistically significant coefficients.

The most important conclusion from column (2) is that the estimated β is virtually identical to that in column (1), which suggests that omitted variables are unlikely to explain our results. While these firm characteristics may be jointly determined with the expectation of future changes in board composition, it is reassuring that the inclusion of these variables does not seem to affect the estimates in an economically meaningful way. We confirm the irrelevance of these firm characteristics by replicating the regression in (1) using firm characteristics as dependent variables. These are "balancing tests," as in Falato and Liang (2016). Table IA.7 in the Internet Appendix reports a summary of these results. We find that covenant violations

[21] 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, Casares 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 the fraction of independent directors, which corresponds to an approximately one-tenth increase in the number of independent 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.

do not appear to have an economically or statistically significant (contemporaneous) effect on any of the firm characteristics used in our analysis. In particular, there is no pure effect of violations on the financial performance or governance variables. This indicates that violations cannot explain contemporaneous differences in firm characteristics, after controlling for the binding distance and firm and year fixed effects. Violations may still affect the *future* value of some of these variables, as the related literature reports and as we also show later.

As an alternative means of controlling for time-invariant unobserved firm heterogeneity, in columns (3) and (4), we estimate our model using first differences. We find that the estimated β is larger at 0.30 and 0.27. Finally, for comparison, we also estimate the same regressions without firm fixed effects, including industry (two-digit SIC) fixed effects. In columns (5) and (6), the estimated β is 0.32 and 0.23, respectively. Thus, firm fixed effects do not appear to affect the estimates significantly, especially after the introduction of firm-level controls.

Panel B shows results using two alternative definitions of the covenant violation dummy. The first definition is the *first violation* indicator, as defined in equation (4). This variable considers only the first violation episode for each firm (i.e., we assume that the firm never returns to a non-violation state). Using this variable addresses the concern that changes from $v_{it-1} = 0$ to $v_{it} = 1$ may not be symmetric to changes from $v_{it-1} = 1$ to $v_{it} = 0$; while the former leads to a covenant violation, the latter does not (necessarily) reverse an earlier violation.

The second definition follows Nini, Smith, and Sufi (2012). We define a *new violation* as a violation event that follows a non-violation event. That is, we drop all firm-year observations such that $v_{it} = 1$ and $v_{it-1} \neq 0$. Nini, Smith, and Sufi (2012) argue that new violations "represent the first opportunity for creditor intervention and thus provide the cleanest identification of the effect of violations on corporate behavior" (p. 1724).[22]

In columns (1)-(3), which use the first violation indicator, the estimated β rises to 0.34, that is, a substantially higher marginal effect of 2.2 new directors (evaluated at the sample mean). This estimate is also remarkably stable across methods. In columns (4)-(6), which use the new violation indicator, the estimated β ranges from 25% (fixed effects) to 38% (OLS). We conclude

[22]Our definition of new violations is almost identical to that of Nini, Smith, and Sufi (2012), who require a new violation to be preceded by four quarters of no violation. Our definition is slightly stricter; we also require at least four quarters of no violation, but in some cases our definition also discards violations preceded by four to six quarters of no violation. The only reason for this difference is that we use annual data, while they use quarterly data.

that our results are not driven by multiple or "stale" violations.

Table 4 replicates the regression analysis above using the logarithm of the number of non-independent 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. However, this effect is statistically and economically weak. In addition, the estimated β is not robust across different specifications and definitions. Comparing Table 3 with Table 4 reveals that the number of new appointments is two to three times larger than the number of insider departures. Thus, the new outside directors are typically not replacements for resignations by insiders; board size increases after covenant violations.

Overall, we find robust evidence of an economically important effect of covenant violations on board independence. The appointment of new directors following covenant violations explains most of this effect. By contrast, there is no evidence of a similar increase in the number of non-independent directors. Thus, board independence unambiguously increases following violations. The joint evidence from Tables 3 and 4 shows that newly appointed directors are not replacements for departing directors.

4.3 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) advise against using polynomials of order higher than 2. Polynomials of order 2 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, we also experiment with different polynomial orders and bandwidth choices, as recommended by Roberts and Whited (2013).

Table 5 reports the estimates of β for a combination of six different bandwidths ($h = 0.3$ to 0.5 and the full sample) and polynomial orders (1 to 5), using the logarithm of the number of independent directors as the outcome variable. We do not include other firm-level characteristics as controls, but the results are similar when we include them.

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 0.07 and statistically insignificant. With our preferred specification (order 2), the estimate is 0.24. For polynomials of order 3 or higher, the estimated β ranges between 0.20 and 0.30. Choosing the narrowest bandwidth ($h = 0.3$) reduces the number of observations by almost half. The point estimate of β is approximately the same (0.22) as that for the baseline bandwidth. Although the confidence intervals are wider, which is expected because of the smaller sample size, all estimated effects are statistically significant. Larger bandwidths ($h = 0.45$ or $h = 0.5$) lead to slightly lower point estimates of β for polynomials of orders 1 and 2, but polynomial order has little impact on β for orders of 3 or higher. We conclude that the effect of covenant violations on the number of independent directors is robust to polynomial order and bandwidth choice.

An alternative to local regressions is global regressions with high-order polynomials. While this approach is considered inferior to local regressions by some authors (e.g., Imbens and Kalyanaraman (2012) and Gelman and Imbens (2014)), for completeness, we report (in column (6)) the estimates from global regressions. The global regression results are consistent with the hypothesis that board independence increases after covenant violations, but such results underscore the limitations of this approach. Global regressions require high-order polynomials, unless there are a priori reasons to assume that the relationship between the outcome variable and the running variable is smooth. However, high-order polynomials create a number of issues (Gelman and Imbens (2014)). One issue is that estimates are often sensitive to the polynomial order. We find that, for lower-order polynomials (orders 1 to 4), the estimated β is positive but small and only statistically significant for order 1. For polynomials of order equal to or higher than 5 (untabulated), the estimated β is always statistically significant, although generally lower than that estimated with local regressions.

4.4 Discontinuity-based Exogeneity Tests

Firm fixed effects address the problem of time-invariant omitted variables, and the large number of firm controls further mitigates concerns about time-varying omitted variables. Nevertheless, we cannot completely exclude the possibility that time-varying omitted variables explain the relationship between covenant violations and board independence. For example, there could be firm-specific trends or cycles that appear to coincide with covenant violation events.

Under mild assumptions, we can formally test for omitted variables by means of a series of placebo tests. Following Caetano (2015), we interpret our tests as discontinuity-based exogeneity tests. Consider the following model:

$$\ln y_{it} = \beta_d v_{it-2}^d + \gamma_1 D_{it-2} + \gamma_2 D_{it-2}^2 + v_{it-2}^d \left(\gamma_3 D_{it-2} + \gamma_4 D_{it-2}^2\right) + \alpha_t + f_i + u_{it}, \tag{5}$$

$$v_{it-2}^d = \begin{cases} 1 & \text{if } D_{it-2} \leq d \\ 0 & \text{if } D_{it-2} > d \end{cases}. \tag{6}$$

That is, if $d = 0$, v_{it-2}^0 equals the real threshold indicator, v_{it-2}. All other $d \neq 0$ define "fake" or "placebo" thresholds. Formally, we perform a series of tests for the null $\mathbb{H}_0 : \beta_d = 0$ against the alternative $\mathbb{H}_1 : \beta_d \neq 0$, for a set of $d \in [-h, h]$. That is, we run the same regressions as before, after replacing the true threshold v_{it-2} with a fake threshold v_{it-2}^d, $d \neq 0$.

Under the assumption that the true relationship between y_{it} and D_{it-2} is continuous (plus a few additional regularity assumptions; see Caetano (2015)), a rejection of the null $\beta_d = 0$ implies that D_{it-2} is not (locally) exogenous at d; this rejection indicates that there exists at least one omitted variable that creates a discontinuity at point $D_{it-2} = d$.[23]

To implement these tests, we first create eight different fake thresholds that are equally distant from one another. These placebo thresholds lie in the interval defined by $d \in [-0.4, 0.4]$, which includes the real threshold. Each d is 0.1 units away from an adjacent threshold. To facilitate comparison with our previous results, we implement such tests using the analog of equation (3) instead of equation (5): For each placebo threshold, we redefine the binding distance variable such that it becomes centered at the new threshold. We then redefine the discontinuity sample accordingly and estimate the number of independent directors regression in column (1) of Table 3 for each placebo threshold.[24]

Table 6 shows the results.[25] For all values of $d \neq 0$, we cannot reject the null that $\beta_d = 0$ at the 5% significance level (the null is rejected at 10% only for $d = 0.3$, but the estimated effect is

[23]Our placebo test can be interpreted as a parametric version of Caetano (2015) exogeneity tests without instruments. She shows that such tests only have nontrivial power for alternatives in which an omitted variable creates a discontinuity in the distribution of unobservables. The test is not meant to rule out omitted variables (exogeneity is the null) but rather to detect cases in which omitted variables are likely.

[24]If we were to not use the analogous model as before, our estimates would not be comparable. The results are similar when we use equation (5).

[25]The results including firm-level controls are virtually identical.

negative and economically small at -0.06). Furthermore, most estimates are economically close to zero, with magnitudes in the range $[-0.06, 0.11]$, and display changes in sign that follow no particular pattern. By contrast, the estimated effect at the true threshold is statistically and economically strong at $\beta_0 = 0.24$.

We believe that these placebo tests provide the strongest evidence in favor of a causal interpretation of our findings. In the presence of fixed effects, the main source of endogeneity is (time-varying) omitted variables. Our placebo tests fail to detect such omitted variables at values of the forcing variable that differ from the true covenant violation threshold.

4.5 Possible Mismeasurement of Covenant Violations

Are the estimates sensitive to our measure of covenant violations? We address this question by considering a different definition of covenant violations: violations that are registered with the SEC, as in Roberts and Sufi (2009) and Nini, Smith, and Sufi (2012). Henceforth, we refer to this variable as *registered violations* and to our original variable as *implied violations*. The registered violation variable is constructed using information from the SEC's 10-Q and 10-K filings.[26] Nini, Smith, and Sufi (2012) use an algorithm to identify financial covenant violations in credit agreements for publicly traded firms. They construct an indicator variable of whether the firm reports a violation of a financial covenant during each quarter.

A limitation of the registered violation measure is that we do not know which covenant is responsible for a reported violation. Therefore, to measure the binding distance, we need to infer from accounting data which covenant has been violated. This procedure reduces the sample size and may create other forms of measurement errors. We thus consider four different ways of using registered violations.

First, we use registered violations to eliminate "false negatives," which we define as cases in which we observe a registered violation but not an implied violation. We drop all firm-year observations for which (1) there is no implied violation but there is a registered violation in one of the previous four quarters or (2) we do not have data on registered violations. This procedure eliminates 75 observations from the discontinuity sample, or 11% of that sample. We expect this correction to improve measurement quality because a false negative is hard evidence of

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

mismeasurement.[27] Table 7 reports the results in columns (1) (without firm-level controls) and (2) (with firm-level controls). We find that correcting for false negatives has no effect on the estimates: The number of independent directors increases by 24% after a covenant violation.[28]

Second, we use registered violations to eliminate "false positives," which are cases in which we have an implied violation but find no registered violation in the current or following year. Eliminating false positives is a more controversial procedure than eliminating false negatives. False positives will often occur when a violation is waived or renegotiated before the need to report it. Thus, false positives could indicate a less serious violation but one that could nonetheless affect board composition. Dropping all false positives eliminates 257 observations from the discontinuity sample, or 39% of that sample. False positives are quite frequent; just over 80% of all implied violations are not registered. This suggests that renegotiation and the waiving of covenants are frequent occurrences (Roberts (2015) and Denis and Wang (2014)).

Columns (3) and (4) of Table 7 report the results using only registered violations (i.e., after correcting for false positives). We find that using only registered violations significantly increases the estimated β: The number of independent directors increases by 49% after a violation. Due to a significant reduction in sample size, this effect is less precisely estimated, but it is still statistically significant at the 10% level. A larger effect when using only registered violations is somewhat expected; registered violations are likely to be the most serious violations and thus more likely to have consequences for borrowers.

Third, we simultaneously correct for both false negatives and false positives . This eliminates 293 observations from the discontinuity sample, or 44% of that sample. Columns (5) and (6) report the results. The estimated β is 0.5 and statistically significant at the 10% level.

Finally, we can also simply replace the implied violation measure with the registered violation measure, without attempting to infer which covenant is associated with an observed registered violation. Under this approach, we cannot calculate the binding distance, and thus, we also cannot define the discontinuity sample. The best we can do here is to work with the full sample and control for accounting variables that may be used in credit agreements.

[27] A false negative does not imply that we made errors when constructing the implied violation measure; a likely reason for a false negative is the occurrence of a violation of a covenant not included in our list.

[28] Table IA.8 in the Internet Appendix shows that estimates are close to zero when we estimate the placebo tests in Table 6 using this sample of registered violations.

We report the full sample analysis in the Internet Appendix. The sample that results from merging the registered violation data with the IRRC data yields 1,296 firms and 8,514 firm-year observations. Table IA.9 in the Internet Appendix presents descriptive statistics of the variables in our study using this sample. Figure IA.2 in the Internet Appendix replicates Figure 1 with this alternative sample. We find that the evolution of the ratio of independent to non-independent directors around a covenant violation is similar to that in Figure 1. In fact, the two figures are noticeably similar, clearly showing that the ratio of independent to non-independent directors increases following a violation.

Next, following Roberts and Sufi (2009) and Nini, Smith, and Sufi (2012), we estimate a "quasi-discontinuity" specification:

$$\ln y_{it} = \beta v_{it-2} + \boldsymbol{\delta} \mathbf{h}\left(\mathbf{x}_{it-2}\right) + \alpha_t + f_i + \varepsilon_{it}, \tag{7}$$

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. We include third-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.[29] Table IA.10 in the Internet Appendix reports the estimates of equation (7). All specifications produce similar estimates. The semi-elasticity of the number of independent directors to covenant violations is approximately 4%. The size of the effects, especially compared to those in the discontinuity samples when we use registered violations only, suggests that controlling for the distance to a violation substantially increases the estimates. When we use the number of non-independent directors as the dependent variable, we find a negative effect of covenant violations, but as before, the effect is statistically insignificant.

We conclude that the effect of covenant violations on board independence does not depend on our particular measure of covenant violations. We also find that, when using registered violations in the discontinuity sample, the estimated effects are economically stronger (but statistically weaker) than those obtained with implied violations, indicating that more serious violations have stronger consequences for board composition.

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

4.6 Robustness

We first consider Poisson regressions that take into account the count nature of the dependent variable (number of independent directors). These regressions assume that y_{it} is independently Poisson distributed with conditional mean equal to

$$E\left[y_{it} \mid v_{it-2}, D_{it-2}, \alpha_t, f_i\right] = \exp\left\{\beta v_{it-2} + \sum_{p=1}^{P}\left[\gamma_{p0} + \gamma_{p1} v_{it-2}\right] D_{it-2}^{p} + \alpha_t + f_i\right\}. \tag{8}$$

Parameter β is again a semi-elasticity, and thus, it can be directly compared to the previous estimates. We report the results in column (1) of Table 8. The Poisson regression yields an estimate of β that is just slightly lower (19%) than those from log-linear regressions.

Second, we consider the possibility that director turnover is simply a consequence of CEO turnover. Nini, Smith, and Sufi (2012) show that violations lead to more CEO turnover. Thus, it is possible that new CEOs bring new directors to the board. If this is the case, the effect of covenants on board independence 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 years in which a CEO is replaced and in the two years thereafter. Table 8, column (2), reports the results. If anything, the estimated effect is stronger, at 30%, when using a sample of firms without CEO turnover. Table IA.11 in the Internet Appendix presents estimates using alternative samples of firms without CEO turnover.

Third, we consider an alternative measure of the covenant violation indicator excluding debt-to-EBITDA covenants. This is likely to add noise to our estimates (debt-to-EBITDA violations are associated with 84% of the violations in our full sample). Table 8, column (3), reports the results. The estimate remains qualitatively similar (18%) to those in Table 3, but it is statistically weaker. We conclude that including debt-to-EBITDA covenants is important for estimating the effects of violations with precision, but we find qualitatively similar results even when we ignore debt-to-EBITDA covenants. In column (4), we add interest coverage covenants to our list of covenants (i.e., our binding distance variable now considers five different covenants). Adding new covenants changes the definition of the discontinuity sample because the number and the types of covenants affect the calculation of the binding distance. The introduction of

interest coverage covenants has only a minor impact on the estimated β, which is now 0.20 and statistically significant. Table IA.12 in the Internet Appendix replicates all specifications in Table 3 when including the interest coverage covenant.

Fourth, we consider alternative sample periods. In columns (5) and (6), we divide the sample into observations before and after the Sarbanes-Oxley Act (SOX) of 2002, which, among other things, mandated more independent boards. We find strikingly similar estimates for both the pre- and post-SOX subsamples (20% and 21%, respectively). Despite the significant reduction in sample size, the effects remain statistically significant. In column (7), we extend our sample to include observations after 2008, up to 2014. We replicate our main specification using the extended sample and find an estimated β of 0.17 (t-statistic of 3.57). Adding observations from 2009 to 2014 has a small impact on the magnitude of the estimated effects, but overall, the estimated effects are similar and remain statistically significant. Table IA.13 in the Internet Appendix replicates all specifications in Table 3 when using the extended sample.

Finally, in column (8) we replace the number of independent directors with the ratio of the number of independent directors to board size. We find that covenant violations reduce the fraction of independent directors by 16%, which is equivalent to 11 percentage points when evaluated at the sample average (70%). Table IA.14 in the Internet Appendix provides additional estimates using the fraction of independent directors as the outcome variable.

The Internet Appendix reports the results of additional robustness checks. Table IA.15 shows estimates of the regression in equation (3) using either one or three lags instead of two lags. Using one lag, the effects are economically weaker but can still be detected, being statistically significant in some but not all specifications. This is expected because the process of appointing directors may take time. When using three lags, the effects are similar to those obtained with two lags and are typically economically and statistically significant. However, the three-lag effects are less statistically precise because of the reduction in sample size.

For our baseline results, we adopt a conservative criterion to determine which observations to retain in the discontinuity sample. According to this criterion, an observation is retained only if, for each quarter of the year, the quarterly binding distance falls inside the interval. Table IA.16 shows that our results are robust to a less stringent criterion in which we only require the annual binding distance to lie within the interval.

5 Mechanisms and Consequences

Who are the directors appointed after covenant violations? Which firms appoint new directors after loan covenant violations? What happens after the appointment of these directors? This section provides some answers to these questions, thus helping us better understand the causes and consequences of board changes after covenant violations.

5.1 Who are the directors appointed after covenant violations?

To answer this question, we collected 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 $v_{it-1} = 0$ to $v_{it} = 1$). We identify 226 directors for which current and past employment data (in publicly listed firms) are available from the BoardEx database.

We consider three different control groups. For the first, we match each new director to a randomly chosen independent director who joined the board in a non-violation year (to maximize the number of matches, we consider the two years before the first violation).[30] With this matching criterion, we match only 129 directors. Panel A of Table 9 presents sample averages of the characteristics of new directors and directors in the control group. We find that newly appointed directors are not substantially different from directors in the control group in most characteristics.

We also use directors' employment information to investigate whether there may be *indirect* links to banks (the variable *bank connection*). We consider a director to be connected to a bank if the director holds a position (board or non-board) in a firm that borrows from the same bank. To measure these connections, we consider links via banks (lead arrangers or other participants) in outstanding syndicated loans. Of the 129 new directors, 109 work for firms for which we are able to obtain syndicated loan data. We find that 75% of the new directors have connections to their firms' current banks, while only 40% of the control group have connections to current banks. The difference between the two groups – 35% – is statistically significant, with a t-statistic of 5.93.

We also construct a variation of the bank connection variable, in which we consider only

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

connections through banks in the syndicate of the loan contract for which a violation occurs. We find that 69% of the new directors are connected to the banks of a syndicated loan with a recent covenant violation (i.e., 92% of all connections occur via banks of the loan contract that triggered the violation). In the control group, however, only 31% of the directors have connections to the banks in the syndicate of the loan for which a violation occurs. The difference is 38%, with a t-statistic of 6.84.

We construct two alternative control groups. In the first of these groups, we match each new director to a randomly chosen independent director retained by the same firm for at least two years after the first violation. Panel B reports the average director characteristics using this alternative control group. This choice of control group – retained directors – is a conservative one. If creditors do indeed influence board composition, they may support the retention of connected directors after a violation. This control group allows us to match a higher number of directors (223). Compared to this control group, newly appointed directors are younger, more likely to have a finance-related degree, and more likely to have past experience in a financial role. In addition, the difference between the two groups in terms of connections to current banks is 21% and statistically significant (t-statistic of 4.58). For connections via a bank in the syndicate of the loan for which a violation occurs, the difference is 17% (t-statistic of 3.56).

Panel C reports the average director characteristics using a second alternative control group. The control group now includes independent directors who are members of the board in the two years before the first violation and remained on the board for at least two years after the violation. This control group allows us to match an even higher number of directors (226). The results are consistent with those in Panels A and B. The difference between the two groups in terms of connections to current banks is 32% (t-statistic of 6.31).

We conclude that new directors appointed after violations are likely to have connections to creditors. The large majority of these connections occur through the banks of loans for which there is a violation. These connections are unlikely to be chance events; connections via banks in syndicated loans are infrequent in the control groups.

5.2 Do bank connections explain the effect of violations on board appointments?

The results in the previous subsection do not tell us how much of the effect of covenant violations on board appointments is explained by connected directors. To perform this analysis, we need data on potential bank connections for *all* independent directors in our sample, not just for the newly appointed directors. We match our sample with BoardEx and use the procedure explained above to classify independent directors as either connected or unconnected to banks (lead arrangers or other participants) in the loan syndicate. Because of the matching, our sample becomes smaller. Another issue that arises is that IRRC and BoardEx often do not agree on the classification of directors as either independent or not. Whenever there is a conflict between the two, we choose the IRRC classification, which appears to be more stringent and accurate than the BoardEx classification.

With these data, we estimate the regression in equation (2) using as the outcome variable either the logarithm of one plus the number of *connected* independent directors or the logarithm of one plus the number of *unconnected* independent directors. Table 10 shows the results. Column (1) shows our preferred specification (the analog of column (1) in Table 3 with firm and year fixed effects and no control variables). A covenant violation increases the number of connected independent directors by 18%. This point estimate and that in Table 3 (24%) jointly imply that connections with banks explain 75% (=18/24) of the effect of violations on board independence. Columns (2) and (3) show that our findings are robust to different specifications.[31] By contrast, columns (4)-(6) show that unconnected directors explain a negligible fraction of the effect of violations on board appointments; the effect is economically small (5%) and statistically insignificant.

5.3 Which firms appoint new directors after covenant violations?

Not all firms are likely to respond to loan covenant violations in the same way. Whether firms experience major or minor board changes after violations depends on the reason that

[31] These numbers are only approximate for two reasons: (1) in Table 10, we use $\ln(1 + y_{it})$ to avoid taking the log of zero (this is not an issue in any of the previous tables because the number of independent directors is always strictly positive), and (2) the basis for applying the percentages is different across the two tables.

such changes occur. For example, if creditors (directly or indirectly) promote board changes, we expect to find large effects among firms that have closer relationships with their creditors. In contrast, if banks are indifferent to board composition, the effect of violations on board composition should be independent of the identity of creditors. To test for the hypothesized differential board responses, we expand the specification in equation (3) by interacting the covenant violation indicator with a particular proxy and examining the effect of each proxy in a separate regression. The dependent variable is the logarithm of the number of independent directors, and the regressions include firm and year fixed effects (as in column (1) of Table 3).

Panel A of Table 11 presents the results for the lending relationship proxies. We first consider the impact of relationship lending on the effect of violations on board appointments. For each loan in our sample, we first identify the lead arranger and then count the number of past term loans that a borrower has obtained from the same lead arranger. We then create an indicator of whether the borrower has at least two historical lending relationships with its current lead arranger (*past loans* ≥ 2; 302 observations) and an indicator of whether the borrower has no historical lending relationship or only one (*past loans* < 2; 363 observations). Column (1) shows that the estimated β is 32% for borrowers with more historical lending relationships and just 13% (statistically insignificant) for borrowers with no such relationships. The difference—19% —is statistically significant at the 10% level. Column (2) shows similar estimates when we include firm-level control variables.

In columns (3) and (4), we use all loans (i.e., term loans and credit lines) to count the number of past loans that a borrower has obtained from the same lead arranger. We then create an indicator of whether the borrower has at least five historical lending relationships with its current lead arranger (*past loans* ≥ 5; 227 observations) and an indicator of whether the borrower has fewer than five historical lending relationships (*past loans* < 5; 438 observations). We again find a larger violation effect for the group of borrowers with more historical lending relationships. The effect for the group of borrowers with fewer historical lending relationships is smaller, but the difference between the two groups is not statistically significant.

Firms without credit ratings are more dependent on bank debt, which makes banks more powerful in negotiations at the time of covenant violations. Furthermore, in the absence of public debt markets and ratings agency monitoring, creditor monitoring of unrated firms may

be more important. These reasons suggest a stronger effect of violations on the appointment of new directors in unrated firms. However, unrated firms may find it difficult to recruit new directors, as these firms are likely to be more opaque and have a weaker financial position. Columns (5) and (6) present the estimated coefficients for *rated firms* (406 observations) and *unrated firms* (259 observations). We find that the violation effect is stronger for unrated firms than for rated firms. The difference is economically sizable (23% with control variables), but it is not statistically significant at conventional levels.

Creditors' bargaining power after violations is tempered by cross-default and cross-acceleration provisions because such provisions reduce creditors' incentives to declare a borrower in default. This is, however, less of a concern if the borrower's other loans are relatively small, that is, if a single loan constitutes most of the borrower's debt. To construct a proxy for debt concentration, we create an indicator of whether the firm needs to repay at least one large loan (*large loan*; 450 observations) and an indicator for all other cases (*small loan*). We consider a loan to be large if the ratio of the loan amount to total assets at origination is above the median. Panel B of Table 11, columns (1) and (2), presents the results. We find that the effect is stronger for the large loan group than that for those cases with no such loans. The 17% difference between the two groups (in column (1)) is statistically significant at the 10% level.

Finally, in Panel B of Table 11, columns (3)-(6), we consider the effect of covenant slack at loan origination on the impact of violations on board independence. The evidence in Demiroglu and James (2010) motivates this analysis. They show that firms with tighter covenants at origination experience less significant changes in investment and debt issuance after violations. A possible explanation is that borrowers are more likely to agree to tight covenants when they expect violations to have little impact on investment and financial policy. A similar logic may apply to our setup: Managers and incumbent directors might be more reluctant to agree to tight covenants if they expect creditors to use violations to force changes in board structure.

We measure covenant tightness by the binding distance and tightness at origination, as described in Section 2.[32] Using either measure, we find that the effect of violations on board appointments is stronger in those firms with less tight covenants at origination (*high binding*

[32]Demiroglu and James (2010) use a more sophisticated definition of tightness, in which they group firms by clusters of covenant threshold levels, and within each cluster, firms are sorted by their covenant choices. Tight covenants are those more restrictive than the median for each cluster.

distance and *high tightness* groups). The differences between the two groups are economically significant at 16% and 20% in columns (4) and (6) with control variables but not sufficiently precise to be statistically significant at conventional levels.

Overall, the evidence shows that the impact of violations on board appointments is economically stronger in firms for which we would expect more creditor intervention after violations. These are firms that regularly borrow from the same banks, firms that have one large loan, firms without credit ratings, and firms with less tight covenants at loan origination.

5.4 What happens after new directors are appointed?

In this section, we examine what happens when new directors are appointed following violations. We identify all first violations in the $h = 0.4$ subsample; we focus on first violations to avoid using overlapping observations for the same firm. We then create a subsample of all firms that experience a first violation. In this subsample, we create a *new appointment* dummy that takes a value of one if there is an increase in the number of independent directors between year 0 (when a violation occurs) and year 2 (two years after a violation). We consider years -3, -2, and -1 as the period before the violation and years 2, 3, and 4 as the period after the violation.

We estimate the following regression:

$$y_{it} = \alpha a_{it} + \gamma n_i a_{it} + \alpha_t + f_i + \delta x_{it} + \varepsilon_{it}, \tag{9}$$

where y_{it} is a firm outcome; a_{it} is an indicator that takes a value of one for years 2 to 4 after firm i experiences a first violation (i.e., a_{it} is the *after* indicator); n_i is the *new appointment* dummy; α_t is a year fixed effect; f_i is a firm fixed effect; and x_{it} is a measure of firm size (the logarithm of assets).[33] Note that the new appointment dummy for the period before the violation is absorbed by the firm fixed effects, and the *after* indicator is defined in event-time and thus not absorbed by the year fixed effect. The interpretation of coefficient γ is similar to that of a difference-in-differences estimator, except that the "treatment" here—an increase in board independence—is certainly endogenous, which means that the estimated γ should not be interpreted as a causal effect.

[33] We keep the model parsimonious because we have a small sample.

Table 12 shows the results. Panel A studies financial and investment policies after covenant violations. Column (1) shows that *investment*—measured by capital expenditures scaled by lagged property, plant and equipment—decreases in years 2 to 4 after a violation. This result is similar to that in Chava and Roberts (2008), but the horizon is different: While Chava and Roberts (2008) estimate the effects one quarter ahead of a violation, our results suggest that investment rates remain low for a number of years after a violation. The -0.07 coefficient on the *after* dummy variable implies that, for firms that do not appoint new directors in the post-violation years, the annual investment rate is 7% (of capital) lower than that in the pre-violation years. For firms that appoint new directors, there are no economically or statistically significant differences in investment rates before and after the violation; the estimated effect is $-0.07 + 0.08 = 0.01$, which is not statistically significant.

Column (2) shows the estimate for *net debt issues* scaled by lagged assets. The estimate is qualitatively similar to that in Roberts and Sufi (2009), but our results are for a longer horizon. We find that debt issuance decreases less in firms that appoint new directors, but the difference between the two groups is not statistically significant. Column (3) shows that *net equity issues* (scaled by lagged assets) increase in years 2 to 4 after a violation. This increase is more pronounced in firms that appoint new independent directors: Annual net equity issuance is 4% higher in firms that appoint new directors after a violation than in firms with no such appointments; the difference is statistically significant at the 10% level. Column (4) measures the effect of violations on equity issuance using *SEO proceeds* (scaled by lagged assets). We find a significant increase in SEO activity in firms that appoint new directors following violations.

In sum, although the evidence here is only suggestive, it indicates more intense equity issuance activity in firms that appoint new directors after covenant violations. Such activity is compatible with the finding that investment in those firms falls by less than that in firms without changes in board independence.[34]

Panel B studies CEO compensation after covenant violations. The dependent variables are the logarithm of *total pay*, *salary*, *bonus*, *options* grants, and *stock* grants. Columns (1) and (2) show that both total pay and salary do not seem to change significantly after violations.

[34]Table IA.17 in the Internet Appendix presents estimates of a variation of equation (9) in which we collapse the data into two periods: before and after covenant violation. We obtain estimates similar to those in Table 12.

Column (3) shows that cash bonuses increase in the years after a violation for firms that do not appoint new independent directors, while bonuses actually decrease for firms that appoint new directors. In contrast, column (4) shows that the value of options grants decreases after a violation, but this decrease is much less pronounced in firms that appoint new directors.

Overall, the evidence suggests a narrative in which CEO compensation is tilted toward cash bonuses—and away from options and stock—in firms that do not appoint new directors. In contrast, firms with newly appointed directors experience a decrease in cash bonuses and a much smaller decline in options grants. The evidence is consistent with the hypothesis that reformed boards following violations are more likely to favor equity-based compensation over cash-based compensation.

6 Conclusion

We show that credit agreements have consequences for the composition of boards of directors. While the evidence does not show whether creditors explicitly intervene in corporate governance issues, it is consistent with a narrative in which creditors benefit from those governance changes. We find that, after covenant violations, a large number of newly appointed directors have connections to creditors; these connected directors explain most of the estimated effects. In addition, the effect of violations on board appointments is more pronounced in firms with stronger lending relationships, firms that are more dependent on bank loans, and firms with less tight covenants at loan origination.

Our results show that current and past credit agreements can have long-lasting effects on a firm's governance structure. In the years after a covenant violation, firms with newly appointed independent directors issue more equity and invest more than those firms that do not reform their boards. Firms with new board appointments also have a different CEO compensation structure in the years following a violation: They are less likely to increase cash bonuses to compensate for a decrease in the value of equity-based compensation. Since boards are responsible for approving investments, equity issuances, and CEO compensation, these changes in firm policies are consistent with the hypothesis that more independent boards actively favor policies that are beneficial (not only) to creditors in the post-violation period.

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Table 1: Summary Statistics

This table presents mean, standard deviation, minimum, 10th percentile, median, 90th percentile, maximum, number of observations, and number of firms for each variable. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) firms from 1994 to 2008 for which syndicated loans data are available from DealScan. Financial industries are omitted (SIC codes 6000-6999). Board and governance data are 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 (CRSP). Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. Binding distance is the relative distance between the actual accounting variable and the corresponding covenant threshold. 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 pctile	Median	90th pctile	Maximum	Number of obs.	Number of firms
Number of independent directors	6.39	2.11	1.00	4.00	6.00	9.00	15.00	2,801	597
Number of non-independent directors	2.76	1.65	1.00	1.00	2.00	5.00	13.00	2,801	597
Ratio of indep. to non-indep. directors	3.45	2.58	0.10	1.00	2.67	8.00	13.00	2,801	597
Number of directors	9.15	2.13	4.00	7.00	9.00	12.00	19.00	2,801	597
Number of connected directors	3.14	2.20	0.00	0.00	3.00	6.00	11.00	2,663	571
Number of non-connected directors	2.80	1.75	0.00	1.00	3.00	5.00	10.00	2,663	571
Firm size ($ millions)	3,542	11,324	43	368	1,231	7,144	270,634	2,801	597
Leverage	0.25	0.16	0.00	0.02	0.25	0.45	0.87	2,801	597
Firm age	22.56	17.42	1.00	6.00	17.00	42.00	81.00	2,801	597
Number of segments	2.88	1.91	1.00	1.00	3.00	6.00	10.00	2,801	597
Market-to-book	1.88	1.15	0.62	1.04	1.54	3.00	8.89	2,801	597
R&D	0.02	0.04	0.00	0.00	0.00	0.06	0.37	2,801	597
Stock return volatility	0.38	0.20	0.12	0.19	0.34	0.63	1.74	2,801	597
Free cash flow	0.09	0.08	-0.79	0.01	0.09	0.18	0.36	2,801	597
Return on assets	0.15	0.08	-0.66	0.07	0.14	0.25	0.44	2,801	597
Governance index	9.33	2.63	3.00	6.00	9.00	13.00	17.00	2,801	597
CEO ownership	0.02	0.05	0.00	0.00	0.00	0.05	0.30	2,801	597
CEO tenure	7.51	7.51	0.00	1.00	5.00	17.00	49.00	2,801	597
Covenant violation	0.24	0.43	0.00	0.00	0.00	1.00	1.00	2,801	597
Binding distance	0.07	1.45	-7.36	-0.63	0.30	0.92	4.14	2,801	597

Table 2: Averages for Violation and Non-Violation Groups - Sample within Bandwidth

This table presents sample averages of board composition and firm characteristics for observations with no covenant violation and observations with at least one covenant violation. A covenant violation occurs if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth ($h = 0.4$).

	No violation (1)	Violation (2)	Difference (1)-(2)	t-statistic
Number of independent directors (2 leads)	6.34	6.41	-0.08	-0.40
Number of independent directors (2 lags)	5.98	5.94	0.04	0.18
Number of independent directors (1 lag)	5.99	5.89	0.10	0.47
Number of independent directors	5.97	6.03	-0.06	-0.31
Number of non-independent directors (2 leads)	2.94	2.95	-0.01	-0.07
Number of non-independent directors (2 lags)	3.36	3.59	-0.23	-1.30
Number of non-independent directors (1 lag)	3.32	3.53	-0.22	-1.31
Number of non-independent directors	3.22	3.39	-0.17	-1.05
Firm size ($ millions)	2,553	3,051	-498	-1.28
Leverage	0.29	0.35	-0.06	-5.03
Firm age	23.98	21.95	2.03	1.38
Number of segments	2.96	3.03	-0.06	-0.38
Market-to-book	1.47	1.48	-0.01	-0.24
R&D	0.02	0.02	0.00	0.11
Stock return volatility	0.37	0.38	-0.01	-0.95
Free cash flow	0.07	0.07	0.00	1.02
Return on assets	0.13	0.13	0.00	0.54
Governance index	9.45	9.33	0.12	0.57
CEO ownership	0.03	0.03	0.00	-0.55
CEO tenure	8.24	7.30	0.94	1.54
Number of observations	454	211		
Number of firms	192	121		
Fraction of observations in violation		0.32		
Fraction of firms in violation		0.55		

Table 3: Regression of Number of Independent Directors

This table presents estimates of firm fixed effects, first differences and ordinary least squares (OLS) panel regressions of the logarithm of the number of independent directors. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. The firm-level control variables are firm size (log), leverage, 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. All explanatory variables are lagged two years. Panel A presents estimates using all covenant violations, and Panel B presents estimates using the first covenant violation or new violations for each firm. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth ($h = 0.4$). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

Panel A: All Violations

	Firm fixed effects		First differences		OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.24***	0.25***	0.30***	0.27***	0.32***	0.23***
	(3.47)	(3.66)	(3.37)	(3.21)	(3.30)	(2.68)
Marginal effects (at mean)	1.53	1.60	1.92	1.73	2.04	1.47
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm level controls	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	No	No	No	No
Industry fixed effects	No	No	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.176	0.249	0.137	0.167	0.301	0.497
Number of observations	665	665	472	472	665	665
Number of firms	222	222	214	214	222	222

Panel B: First and New Violations

	First violations			New violations		
	Firm FE	First diff.	OLS	Firm FE	First diff.	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.34***	0.34***	0.34***	0.25***	0.35***	0.38***
	(3.20)	(2.75)	(2.88)	(2.68)	(3.22)	(3.01)
Marginal effects (at mean)	2.17	2.17	2.17	1.60	2.24	2.43
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	No	No	Yes	No	No
Industry fixed effects	No	Yes	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.163	0.161	0.378	0.190	0.184	0.317
Number of observations	522	350	522	502	357	502
Number of firms	188	179	188	175	165	175

Table 4: Regression of Number of Non-Independent Directors

This table presents estimates of firm fixed effects, first differences and ordinary least squares (OLS) panel regressions of the logarithm of the number of non-independent directors. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. The firm-level control variables are firm size (log), leverage, 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. All explanatory variables are lagged two years. Panel A presents estimates using all covenant violations, and Panel B presents estimates using the first covenant violation or new violations for each firm. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth ($h = 0.4$). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

Panel A: All Violations

	Firm fixed effects		First differences		OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	-0.21**	-0.21**	-0.19	-0.19	-0.13	-0.09
	(-2.41)	(-2.45)	(-1.44)	(-1.49)	(-0.97)	(-0.75)
Marginal effects (at mean)	-0.58	-0.58	-0.52	-0.52	-0.36	-0.25
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	No	No	No	No
Industry fixed effects	No	No	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.245	0.285	0.163	0.176	0.389	0.452
Number of observations	665	665	472	472	665	665
Number of firms	222	222	214	214	222	222

Panel B: First and New Violations

	First violations			New violations		
	Firm FE	First diff.	OLS	Firm FE	First diff.	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	-0.35***	-0.33**	-0.12	-0.40***	-0.19	-0.19
	(-2.80)	(-2.04)	(-0.71)	(-3.42)	(-1.37)	(-1.17)
Marginal effects (at mean)	-0.97	-0.91	-0.33	-1.10	-0.52	-0.52
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm level controls	No	No	No	No	No	No
Firm fixed effects	Yes	No	No	Yes	No	No
Industry fixed effects	No	Yes	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.247	0.269	0.394	0.310	0.185	0.436
Number of observations	522	350	522	502	357	502
Number of firms	188	179	188	175	165	175

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

This table presents estimates of firm fixed effects panel regressions of the logarithm of the number of independent directors. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. All explanatory variables are lagged two years. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth (h). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

	Polyn. order	Bandwidth					Full sample
		$h = 0.3$	$h = 0.35$	$h = 0.4$	$h = 0.45$	$h = 0.5$	
		(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	1st	0.12*	0.11**	0.07	0.05	0.04	0.03**
		(1.67)	(2.08)	(1.57)	(1.21)	(1.13)	(2.15)
Covenant violation	2nd	0.22**	0.19**	0.24***	0.15***	0.14***	0.02
		(2.35)	(2.54)	(3.47)	(2.76)	(2.97)	(0.96)
Covenant violation	3rd	0.36***	0.28***	0.20**	0.23***	0.21***	0.02
		(2.75)	(2.87)	(2.37)	(3.12)	(2.94)	(1.06)
Covenant violation	4th	0.46***	0.31**	0.30***	0.23**	0.23***	0.04
		(2.82)	(2.49)	(2.82)	(2.54)	(2.80)	(1.36)
Covenant violation	5th	0.41**	0.42***	0.28**	0.28**	0.21**	0.06*
		(2.48)	(2.70)	(2.16)	(2.59)	(2.12)	(1.76)
Firm fixed effects		Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects		Yes	Yes	Yes	Yes	Yes	Yes
R^2		0.226	0.166	0.152	0.164	0.182	0.191
Number of observations		346	503	665	813	976	2,801
Number of firms		129	176	222	255	292	597

Table 6: Regression of Number of Independent Directors - Placebo Test

This table presents estimates of firm fixed effects panel regressions of the logarithm of the number of independent directors. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. All explanatory variables are lagged two years. The estimates are shown using different distances to the real threshold, which is set at zero. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth (h). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

	Distance to real threshold								
	−0.4	−0.3	−0.2	−0.1	0.0	0.1	0.2	0.3	0.4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Covenant violation	0.11	0.10	0.05	0.03	0.24***	-0.06	0.01	-0.06*	-0.01
	(0.55)	(1.00)	(0.41)	(0.33)	(3.47)	(-1.22)	(0.19)	(-1.66)	(-0.23)
Marginal effects (at mean)	0.70	0.64	0.32	0.19	1.53	-0.38	0.06	-0.38	-0.06
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.213	0.270	0.232	0.147	0.176	0.187	0.194	0.181	0.182
Number of observations	104	151	245	430	665	883	1,068	1,109	1,128
Number of firms	45	64	97	155	222	272	316	321	325

Table 7: Regression of Number of Independent Directors - SEC-Dealscan Matched Sample

This table presents estimates of firm fixed effects panel regressions of the logarithm of the number of independent directors. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to EBITDA) during the year in at least one quarter. The firm-level control variables are firm size (log), leverage, 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. All explanatory variables are lagged two years. Columns (1) and (2) drop observations in case the covenant violation dummy is zero but there is a covenant violation according to the SEC's 10-Q or 10-K filings. Columns (3) and (4) drop observations in case the covenant violation dummy is one but there is no covenant violation according to the SEC's 10-Q or 10-K filings. Columns (5) and (6) drop observations in both cases. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth ($h = 0.4$). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.24***	0.24***	0.49*	0.49*	0.51*	0.50*
	(3.35)	(3.64)	(1.86)	(1.93)	(1.72)	(1.76)
Marginal effects (at mean)	1.53	1.53	3.13	3.13	3.26	3.19
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm level controls	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.174	0.258	0.253	0.317	0.241	0.323
Number of observations	590	590	408	408	372	372
Number of firms	203	203	146	146	135	135

Table 8: Regression of Number of Independent Directors - Robustness

This table presents estimates of firm fixed effects (columns (2)-(8)) panel regressions of the logarithm of the number of independent directors. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. All explanatory variables are lagged two years. Column (1) presents estimates of a Poisson regression. Column (2) presents estimates of a regression excluding observations in the year in which the CEO is replaced as well as the two years after the CEO turnover. Column (3) presents estimates of a regression in which debt-to-EBITDA covenants are excluded from the definition of covenant violations. Column (4) presents estimates of a regression in which interest coverage covenants are included in the definition of covenant violations. In columns (5) and (6) the sample period is the pre-SOX period (1994-2002) and post-SOX period (2003-2008) respectively. In column (7) the sample period is extended to 1994-2014. Column (8) presents estimates of a regression in which the dependent variable is the logarithm of the ratio of the number of independent directors to board size. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth ($h = 0.4$). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

	Poisson regression	Excluding CEO turnover	Excluding Debt-to-EBITDA	Including interest coverage	Pre-SOX	Post-SOX	Sample 1994-2014	Board independence
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Covenant violation	0.19***	0.30***	0.18*	0.20***	0.20**	0.21**	0.17***	0.16***
	(3.57)	(2.62)	(1.75)	(2.92)	(2.25)	(2.17)	(3.57)	(2.97)
Marginal effects (at mean)	1.21	1.92	1.15	1.30	1.28	1.34	1.15	0.11
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2		0.233	0.199	0.192	0.091	0.221	0.232	0.215
Number of observations	665	360	402	655	294	291	1,008	665
Number of firms	222	132	143	217	108	112	300	222

Table 9: Characteristics of Independent Directors Appointed after Covenant Violations

This table reports sample averages of the characteristics of new independent directors appointed in the two years after a firm first violates a covenant and a matched control group of independent directors. To construct the control group, a new director is matched to a randomly-chosen independent director in the same firm. In Panel A the control group includes independent directors who joined the board in the two years before the first violation. In Panel B the control group includes independent directors retained by the firm for at least two years after the first violation. In Panel C the control group includes independent directors who are members of the board in the two years before the first violation and remained in the board for at least two years after the first violation. Director characteristics are from the BoardEx database. Refer to Table A1 in the Appendix for variable definitions.

Panel A: Control Group - Directors who joined the board before violation

	New directors	Control group	Difference	t-statistic	Number of obs.
Male	0.91	0.86	0.05	1.30	129
Age	55.83	54.55	1.28	1.42	129
MBA	0.18	0.16	0.02	0.33	129
Financial education	0.25	0.26	-0.02	-0.31	129
Audit or finance committee	0.55	0.65	-0.10	-1.65	129
Past audit or finance committee	0.46	0.33	0.12	1.99	129
Past financial role	0.21	0.16	0.05	0.95	129
Financial firm connection	0.21	0.12	0.09	1.94	129
Financial firm board member	0.14	0.12	0.02	0.39	129
Number of board positions	1.99	1.83	0.16	0.54	129
Number of past board positions	1.33	1.02	0.32	1.58	129
Bank connection	0.75	0.40	0.35	5.93	109
Bank connection - violation	0.69	0.31	0.38	6.84	109

Panel B: Control Group - Directors who remained in the board after violation

	New directors	Control group	Difference	t-statistic	Number of obs.
Male	0.92	0.88	0.04	1.41	223
Age	55.97	60.20	-4.23	-5.97	223
MBA	0.20	0.13	0.07	1.90	223
Financial education	0.26	0.14	0.12	3.18	223
Audit or finance committee	0.59	0.63	-0.04	-0.77	223
Past audit or finance committee	0.47	0.40	0.07	1.40	223
Past financial role	0.20	0.11	0.09	2.42	223
Financial firm connection	0.22	0.22	0.00	-0.12	223
Financial firm board member	0.15	0.19	-0.04	-1.00	223
Number of board positions	1.96	2.20	-0.24	-1.18	223
Number of past board positions	1.23	1.57	-0.34	-1.96	223
Bank connection	0.71	0.50	0.21	4.58	171
Bank connection - violation	0.65	0.48	0.17	3.56	171

Panel C: Control Group - Directors who joined the board before violation and remained in the board

	New directors	Control group	Difference	t-statistic	Number of obs.
Male	0.92	0.87	0.05	1.65	226
Age	55.75	59.27	-3.52	-5.01	226
MBA	0.20	0.12	0.08	2.46	226
Financial education	0.27	0.17	0.11	2.83	226
Audit or finance committee	0.59	0.59	0.00	0.00	226
Past audit or finance committee	0.46	0.32	0.14	2.94	226
Past financial role	0.21	0.12	0.09	2.45	226
Financial firm connection	0.23	0.22	0.01	0.24	226
Financial firm board member	0.16	0.19	-0.03	-0.75	226
Number of board positions	2.00	2.46	-0.46	-1.60	226
Number of past board positions	1.27	1.35	-0.08	-0.48	226
Bank connection	0.72	0.40	0.32	6.31	174
Bank connection - violation	0.65	0.33	0.32	6.31	174

Table 10: Regression of Number of Connected and Non-Connected Directors

This table presents estimates of firm fixed effects, first differences and ordinary least squares (OLS) panel regressions of the logarithm of one plus the number of connected directors or unconnected directors. Connected directors are those that have a board or non-board position in another firm with outstanding loans that have at least one bank (lead arranger or other participant) in common with the firm's current banks. Non-connected directors include all other cases. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to EBITDA) during the year in at least one quarter. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth ($h = 0.4$). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

	Number of connected directors			Number of non-connected directors		
	Firm FE	First diff.	OLS	Firm FE	First diff.	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.18**	0.33***	0.33**	0.05	0.05	0.10
	(2.26)	(2.74)	(2.40)	(0.60)	(0.39)	(0.83)
Marginal effects (at mean)	0.75	1.37	1.37	0.19	0.19	0.38
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	No	No	Yes	No	No
Industry fixed effects	No	Yes	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.060	0.168	0.281	0.133	0.119	0.257
Number of observations	623	439	623	623	439	623
Number of firms	207	199	207	207	199	207

Table 11: Regression of Number of Independent Directors - Cross-Sectional Variation

This table presents estimates of firm fixed effects panel regressions of the logarithm of the number of independent directors. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. The specification interacts an indicator for a group of firms with the covenant violation dummy and control variables (firm size, leverage, firm age, number of segments, market-to-book, R&D, stock return volatility, free cash flow, return on assets, governance index, CEO ownership, and CEO tenure). Panel A reports the estimates for the lending relationship proxies. The past loans ≥ 2 group includes firms that had at least two historical lending relationships (only term loans) with the lead arranger in the current loan syndicate and the past loans < 2 group includes all other cases. The past loans ≥ 5 group includes firms that had at least five historical lending relationships (all loans) with the lead arranger in current loan syndicate and the past loans < 5 group includes all other cases. The rated firm group consists of firms with a S&P credit rating and the unrated firm group includes all other cases. Panel B reports the estimates for the loan repayment and covenant slack proxies. The large and small loan groups include firms with ratio of loan amount (at origination) to assets above and below the median. The high and low binding distance groups include firms with binding distance at origination above and below the median. The high and low tightness groups include firms with tightness at origination above and below the median. All explanatory variables are lagged two years. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth ($h = 0.4$). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

Panel A: Lending Relationships

	(1)	(2)	(3)	(4)	(5)	(6)
Past loans ≥ 2	0.32***	0.30***				
	(4.41)	(3.43)				
Past loans < 2	0.13	0.17				
	(1.40)	(1.49)				
Past loans ≥ 5			0.30***	0.25***		
			(3.15)	(2.87)		
Past loans < 5			0.18**	0.22**		
			(2.25)	(2.23)		
Unrated firms					0.26**	0.39***
					(2.27)	(2.61)
Rated firms					0.23***	0.16**
					(2.95)	(2.19)
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm level controls	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.204	0.305	0.199	0.308	0.188	0.289
Number of observations	665	665	665	665	665	665
Number of firms	222	222	222	222	222	222

Panel B: Loan Repayment and Covenant Slack

	(1)	(2)	(3)	(4)	(5)	(6)
Large loan	0.27***	0.26***				
	(3.49)	(2.83)				
Small loan	0.10	0.16				
	(1.07)	(1.52)				
High binding distance			0.35**	0.38**		
			(2.15)	(2.21)		
Low binding distance			0.23***	0.22***		
			(3.24)	(3.09)		
High tightness					0.23**	0.33**
					(2.13)	(2.44)
Low tightness					0.19***	0.13**
					(2.63)	(2.07)
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm level controls	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.179	0.275	0.185	0.285	0.207	0.338
Number of observations	658	658	665	665	665	665
Number of firms	222	222	222	222	222	222

Table 12: Regression of Investment, Financing and CEO Compensation

This table presents estimates of regressions of investment, financing and CEO compensation around the time of covenant violations. A covenant violation occurs if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. All regressions include firm size (log) as control variable. New appointment is a (treatment group) dummy variable that takes a value of one if there is an increase in the number of independent directors between year 0 and year 2. After is a dummy variable that takes a value of one in the post-violation period. Panel A presents estimates in which the dependent variable is capital expenditures (scaled by lagged property, plant and equipment), net debt issues, net equity issues or SEO proceeds (all scaled by lagged total assets). Panel B presents estimates in which the dependent variable is the logarithm of CEO total pay, salary, bonus, value of option grants (grant-date Black-Scholes value) or value of restricted stock grants (grant-date fair value). The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes years -3, -2, and -1 before the violation, and years 2, 3, and 4 after the violation. Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

Panel A: Investment and Financing

	Investment	Net debt issues	Net equity issues	SEO proceeds
	(1)	(2)	(3)	(4)
New appointment × After	0.08	0.03	0.04*	0.03**
	(1.60)	(1.09)	(1.77)	(1.99)
After	-0.07*	-0.09**	0.03	0.01
	(-1.92)	(-2.11)	(1.26)	(0.23)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
R^2	0.132	0.100	0.045	0.056
Number of observations	697	697	697	697
Number of firms	118	118	118	118

Panel B: CEO Compensation

	CEO Total pay (log)	CEO Salary (log)	CEO Bonus (log)	CEO Option (log)	CEO Stock (log)
	(1)	(2)	(3)	(4)	(5)
New appointment × After	0.13	-0.04	-0.64***	0.51**	0.05
	(1.28)	(-0.82)	(-2.62)	(2.00)	(0.13)
After	-0.22	0.07	0.36*	-0.77**	-0.76**
	(-1.33)	(0.96)	(1.72)	(-2.35)	(-2.10)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
R^2	0.218	0.301	0.184	0.136	0.395
Number of observations	660	663	485	413	227
Number of firms	118	118	117	110	80

Figure 1: Ratio of Independent to Non-Independent Directors

This figure shows the cross-sectional average and 95% confidence interval of the ratio of independent to non-independent directors in the four years before and after a covenant violation. A covenant violation occurs if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan.

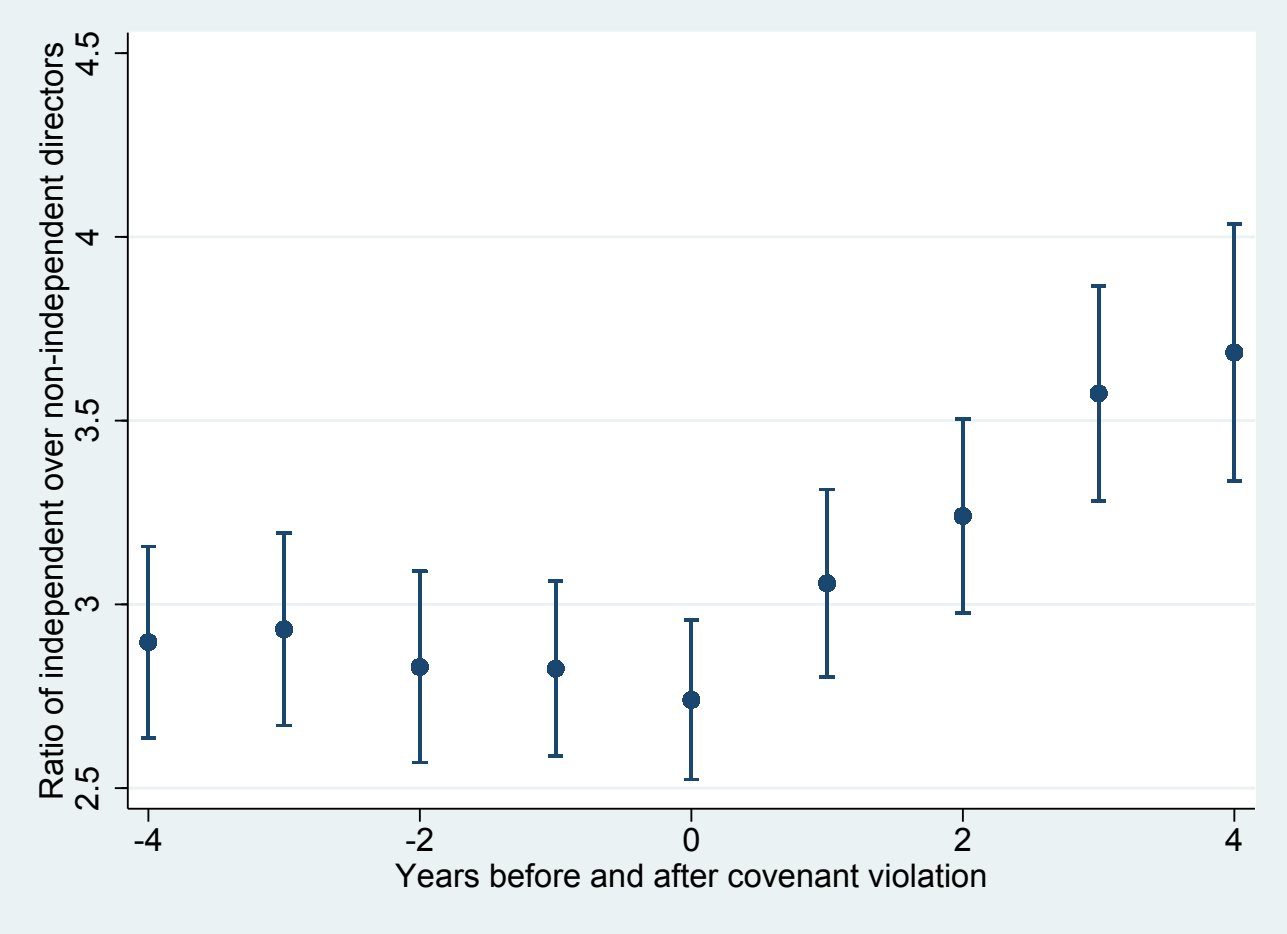
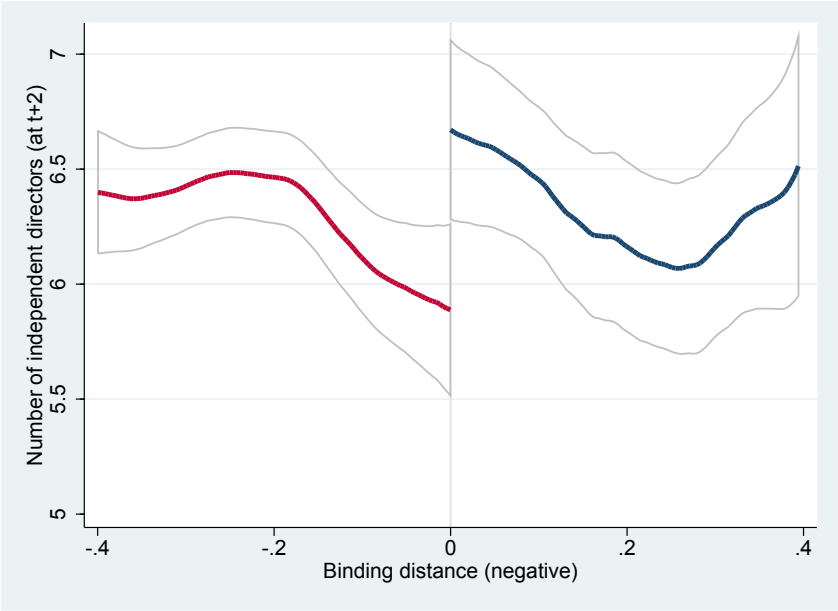


Figure 2: Number of Independent Directors and Binding Distance to Covenant Threshold

This figure shows nonparametric regression estimates of the number of independent directors (two years after violation) on the relative binding distance to the covenant threshold. A covenant violation occurs if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. Panel A presents estimates using all covenant violations, and Panel B presents estimates using only the first covenant violation for each firm. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan.

Panel A: All Violations



Panel B: First Violations

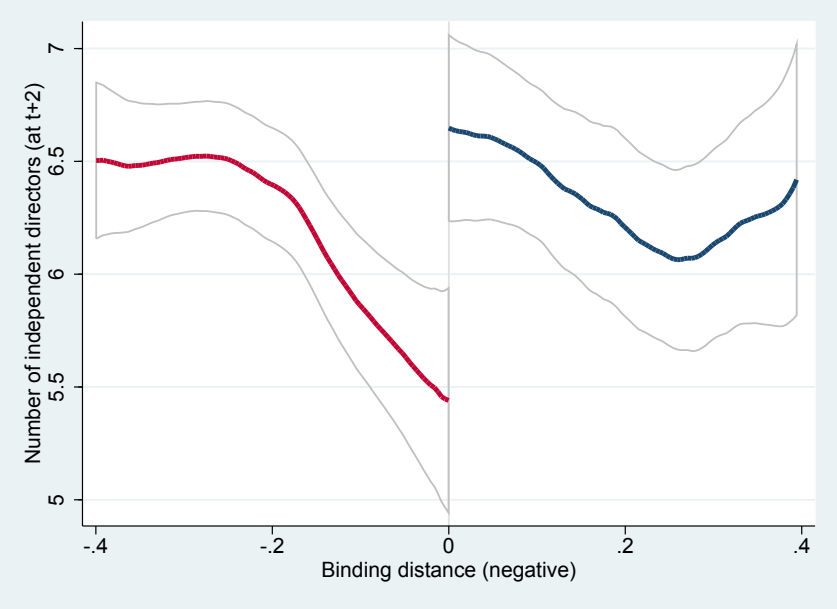


Table A1: Variable Definitions

Variable	Definition
Number of independent directors	Number of board members that are independent directors (IRRC).
Number of non-independent directors	Number of board members that are non-independent directors (IRRC).
Number of directors	Number of board members (IRRC).
Number of connected directors	Number of board members that have a board or non-board position in another firm with outstanding loans that have at least one bank (lead arranger or other participant) in common with the firm's current banks (BoardEx).
Number of non-connected directors	Number of board members that do not have a board or non-board position in another firm with outstanding loans that have at least one bank (lead arranger or other participant) in common with the firm's current banks (BoardEx).
Covenant violation	Dummy variable that takes a value of one if the firm violates at least one out of four covenant (current ratio, net worth, tangible net worth and debt-to-EBITDA) during the year in at least one quarter, and zero otherwise (DealScan).
Current ratio	Ratio of current assets to current liabilities in each quarter (Compustat ACTQ / LCTQ).
Net worth	Total assets minus total liabilities in each quarter in $ millions (Compustat ATQ − LTQ).
Tangible net worth	Tangible assets minus total liabilities in each quarter in $ millions (Computstat ACTQ + AOQ + PPENTQ − LTQ).
Debt-to-EBITDA	Ratio of total debt (long-term debt plus debt in current liabilities) to earnings before interest, taxes, depreciation, and amortization (sum of four most recent fiscal quarters) (Compustat (DLTTQ + DLCQ) / (NIQ − XIQ + TXTQ + XINTQ + DPQ)).
Interest coverage	Ratio of earnings before interest, taxes, depreciation, and amortization to interest expenses (sum of four most recent fiscal quarters) (Compustat (NIQ − XIQ + TXTQ + XINTQ + DPQ) / XINTQ).
Firm size	Total assets in $ millions (Compustat AT).
Leverage	Ratio of total debt (long-term debt plus debt in current liabilities) 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 (total assets plus market value of equity minus book value of equity) to total assets (Compustat (AT + CSHO × PRCC_F − CEQ) / AT).
R&D	Ratio of research and development expenditures to total assets (Compustat XRD / AT).
Stock return volatility	Standard deviation (annualized) of returns estimated with daily stock returns (CRSP).
Free cash flow	Ratio of earnings before interest, taxes, depreciation, and amortization minus capital expenditures to total assets (Compustat (EBITDA − CAPX) / AT).
Return on assets	Ratio of earnings before interest, taxes, depreciation, and amortization to total assets (Compustat EBITDA / AT).
Governance index	Governance index of Gompers, Ishii, and Metrick (2003), which is based on 24 antitakeover provisions (IRRC).

Variable	Definition
Investment	Ratio of capital expenditures to lagged net property, plant and equipment (Compustat CAPEX / PPENT).
Net debt issues	Ratio of long term net debt issues (issuance minus reduction of debt) proceeds to lagged total assets (Compustat (DLTIS − DLTR) / AT).
Net equity issues	Ratio of net equity proceeds (issuance minus purchases of stock) to lagged total assets (Compustat (SSTK − PRSTKC) / AT).
SEO proceeds	Ratio of SEO proceeds (SDC New Issues) to lagged total assets (Computstat AT).
CEO total pay	Total CEO compensation in $ thousands (Execucomp TDC1).
CEO salary	CEO salary in $ thousands (Execucomp SALARY).
CEO bonus	CEO bonus in $ thousands (Execucomp BONUS).
CEO options	Value of option grants to the CEO based on grant-date Black-Scholes value in $ thousands (Execucomp OPTION_AWARDS_BLK_VALUE).
CEO stock	Value of restricted stock grants to the CEO based on grant-date fair value in $ thousands (Execucomp STOCK_AWARDS_FV).
CEO ownership	Number of shares held by the CEO divided by number of shares outstanding (ExecuComp).
CEO tenure	Number of years since the date the director became CEO (ExecuComp).
Male	Dummy variable that takes a value of one if a director is male, and zero otherwise (BoardEx).
Age	Age when director joins the board (BoardEx).
MBA	Dummy variable that takes a value of one if a director holds an MBA when he joins the board, and zero othwerwise (BoardEx).
Financial education	Dummy variable that takes a value of one if a director has a financial education when he joins the board, defined as a degree in the field of economics, accounting, finance, management, and zero otherwise (BoardEx).
Audit or finance committee	Dummy variable that takes a value of one if a director is a member of the finance or audit committees, and zero otherwise (BoardEx).
Past audit or finance committee	Dummy variable that takes a value of one if a director has been a member of the finance or audit committee based on past work experience, and zero otherwise (BoardEx).
Past financial role	Dummy variable that takes a value of one if a director has held a financial role (CFO, finance director, treasury, accountant) based on past work experience, and zero otherwise (BoardEx).
Financial firm connection	Dummy variable that takes a value of one if a director has held a position in a financial firm (SIC 6000-6999) based on past work experience, and zero otherwise (BoardEx).
Financial firm board member	Dummy variable that takes a value of one if a director has held a board position in a financial firm (SIC 6000-6999) based on past work experience, and zero otherwise (BoardEx).
Number of board positions	Number of board positions held by a director (BoardEx).
Number of past boards positions	Number of board positions a director has held based on past work experience (BoardEx).
Bank connection	Dummy variable that takes a value of one if a director has a board or non-board position in another firm with outstanding loans that have at least one bank (lead arranger or other participant) in common with the firm's current banks (BoardEx).
Bank connection - violation	Dummy variable that takes a value of one if a director has a board or non-board position in another firm with outstanding loans that have at least one bank (lead arranger or other participant) in common with the firm's banks in the syndicate of the loan for which a violation occurs (BoardEx).

Internet Appendix for "Creditor Control Rights and Board Independence"

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Table IA.1: Comparison with DealScan, IRRC and Compustat Samples

This table presents mean, number of observations, and number of firms for each variable. Our sample consists of annual observations on Investor Responsibility Research Center (IRRC) firms from 1994 to 2008 for which syndicated loans data are available from DealScan. Financial industries are omitted (SIC codes 6000-6999). The table also presents summary statistics for the samples of DealScan firms, IRRC firms and Compustat firms from 1994 to 2008. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. Refer to Table A1 in the Appendix for variable definitions. Financial ratios are winsorized at the bottom and top 1% level.

	Our sample			DealScan sample			IRRC sample			Compustat sample		
	Mean	Number of obs.	Number of firms	Mean	Number of obs.	Number of firms	Mean	Number of obs.	Number of firms	Mean	Number of obs.	Number of firms
Number of independent directors	6.39	2,801	597	-	-	-	6.54	9,201	1,574	-	-	-
Number of non-independent directors	2.76	2,801	597	-	-	-	2.89	9,201	1,574	-	-	-
Ratio of indep. to non-indep. directors	3.45	2,801	597	-	-	-	3.43	9,201	1,574	-	-	-
Number of directors	9.15	2,801	597	-	-	-	9.43	9,201	1,574	-	-	-
Firm size ($ millions)	3,542	2,801	597	1,534	11,021	2,603	5,078	9,201	1,574	1,394	52,447	8,665
Leverage	0.25	2,801	597	0.27	11,021	2,603	0.23	9,201	1,574	0.22	52,447	8,665
Firm age	22.56	2,801	597	14.18	11,021	2,603	26.58	9,201	1,574	13.51	52,447	8,665
Number of segments	2.88	2,801	597	2.28	11,021	2,603	2.77	9,201	1,574	1.90	52,447	8,665
Market-to-book	1.88	2,801	597	1.77	11,021	2,603	2.05	9,201	1,574	2.17	52,447	8,665
R&D	0.02	2,801	597	0.02	11,021	2,603	0.03	9,201	1,574	0.06	52,447	8,665
Stock return volatility	0.38	2,801	597	0.50	11,021	2,603	0.36	9,201	1,574	0.58	52,447	8,665
Free cash flow	0.09	2,801	597	0.05	11,021	2,603	0.09	9,201	1,574	-0.04	52,447	8,665
Return on assets	0.15	2,801	597	0.12	11,021	2,603	0.15	9,201	1,574	0.02	52,447	8,665
Governance index	9.33	2,801	597	-	-	-	9.28	9,201	1,574	-	-	-
CEO ownership	0.02	2,801	597	-	-	-	0.02	9,201	1,574	-	-	-
CEO tenure	7.51	2,801	597	-	-	-	7.43	9,201	1,574	-	-	-
Covenant violation	0.24	2,801	597	0.34	11,021	2,603	-	-	-	-	-	-
Binding distance	0.07	2,801	597	-0.08	11,012	2,603	-	-	-	-	-	-

Table IA.2: Covenant Data - Quarterly Frequency

This table presents mean, standard deviation, minimum, 10th percentile, median, 90th percentile, maximum, number of observations, and number of firms for covenant data. Threshold is the minimum current ratio, net worth and tangible net worth and maximum debt-to EBITDA specified for each covenant across all outstanding loans in each quarter (omitting observations with no threshold data). Binding distance is the relative distance between the actual accounting variable and the corresponding covenant threshold. Tightness is the distance between the actual accounting variable and the corresponding covenant threshold divided by the firm-specific standard deviation of the accounting variable over the full sample period. The sample consists of quarterly observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan.

	Mean	Standard deviation	Minimum	10th pctile	Median	90th pctile	Maximum	Number of obs.	Number of firms
Current ratio	2.04	1.25	0.29	0.90	1.76	3.33	11.90	11,114	597
Net worth ($ millions)	1,351	5,468	-2,612	156	514	2,505	160,000	11,199	597
Tangible net worth ($ millions)	1,331	5,344	-2,612	155	508	2,478	152,914	11,081	597
Debt-to-EBITDA	3.20	4.88	0.00	0.12	1.87	5.95	35.71	10,446	597
Threshold - current ratio	1.41	0.45	0.50	1.00	1.30	2.00	3.00	808	597
Threshold - net worth ($ millions)	902	6,182	5	100	300	1,150	180,563	3,727	597
Threshold - tangible net worth ($ millions)	531	2,186	5	55	232	1,000	43,950	2,112	597
Threshold - debt-to-EBITDA	3.49	1.10	0.55	2.25	3.25	5.00	11.00	6,587	597
Binding distance - current ratio	0.64	0.67	-0.35	0.03	0.48	1.44	3.31	808	597
Binding distance - net worth	1.08	2.48	-0.29	0.14	0.52	1.65	19.03	3,727	597
Binding distance - tangible net worth	1.66	5.28	-0.83	0.14	0.73	2.63	111.99	2,112	597
Binding distance - debt-to-EBITDA	0.16	1.20	-6.49	-0.42	0.44	0.95	1.00	6,587	597
Tightness - current ratio	1.42	1.20	-1.14	0.07	1.29	3.11	4.43	808	597
Tightness - net worth	0.82	0.68	-0.56	0.15	0.70	1.65	3.35	3,727	597
Tightness - tangible net worth	0.91	0.73	-0.77	0.16	0.78	1.83	3.65	2,112	597
Tightness - debt-to-EBITDA	1.00	2.55	-3.35	-0.42	0.40	3.37	18.85	6,587	597

Table IA.3: Covenant Data - Quarterly Frequency

This table presents average initial covenant tightness, number of observations, and fraction of covenant violations based on the current ratio, net worth, tangible net worth, and debt-to-EBITDA. Covenant violation is a dummy variable that takes a value of one if the firm violates a covenant during a quarter. Initial tightness is the distance between the actual accounting variable and the corresponding covenant threshold at loan origination divided by the firm-specific standard deviation of the accounting variable over the full sample period. Our sample consists of quarterly observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The table compares our sample statistics with those in Chava and Roberts (2008).

	Our sample			Chava and Roberts (2008)		
	Initial Tightness	Number of obs.	Fraction of violations	Initial Tightness	Number of obs.	Fraction of violations
Current ratio	1.44	808	0.09	1.09	5,428	0.15
Net worth	0.58	3,727	0.05	0.68	13,021	0.14
Tangible net worth	0.65	2,138	0.04	0.68	13,021	0.14
Debt-to-EBITDA	0.83	6,587	0.19	-	-	-
All covenants	0.53	9,721	0.16	-	-	-

Table IA.4: Covenant Data - Annual Frequency

This table presents the fraction of covenant violations and number of violations based on the current ratio, net worth, tangible net worth, and debt-to-EBITDA. Current ratio is current assets over current liabilities. Net worth is assets minus total liabilities. Tangible net worth is tangible assets minus total liabilities. Debt-to-EBITDA is total debt over earnings before interest, taxes, depreciation, and amortization. Covenant violation is a dummy variable that takes a value of one if the firm violates a covenant (at least one out of four covenants in the case of all covenants) during the year in at least one quarter. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth (h).

	Bandwidth $h = 0.2$		Bandwidth $h = 0.3$		Bandwidth $h = 0.4$		Full sample	
	Fraction of viol.	Number of viol.	Fraction of viol.	Number of viol.	Fraction of viol.	Number of viol.	Fraction of viol.	Number of viol.
Current ratio	0.28	5	0.30	14	0.27	22	0.14	33
Net worth	0.16	9	0.11	19	0.10	30	0.07	81
Tangible net worth	0.24	7	0.15	10	0.10	14	0.06	39
Debt-to-EBITDA	0.57	30	0.41	88	0.34	147	0.30	570
All covenants	0.46	50	0.37	129	0.32	211	0.24	675

Table IA.5: Averages for Violation and Non-Violation Groups - Sample outside Bandwidth

This table presents sample averages of board composition and firm characteristics for observations with no covenant violation and observations with at least one covenant violation. A covenant violation occurs if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes observations outside the bandwidth ($h = 0.4$).

	No violation (1)	Violation (2)	Difference (1)-(2)	t-statistic
Number of independent directors (2 leads)	6.41	6.36	0.05	0.44
Number of independent directors (2 lags)	5.95	5.94	0.01	0.12
Number of independent directors (1 lag)	5.97	5.98	-0.01	-0.10
Number of independent directors	6.02	6.03	-0.01	-0.08
Number of non-independent directors (2 leads)	2.74	2.54	0.20	2.47
Number of non-independent directors (2 lags)	3.23	3.00	0.23	2.35
Number of non-independent directors (1 lag)	3.19	2.94	0.24	2.61
Number of non-independent directors	3.13	2.89	0.23	2.54
Firm size ($ millions)	3,403	5,234	-1,831	-1.93
Leverage	0.20	0.34	-0.13	-15.67
Firm age	22.34	22.23	0.11	0.12
Number of segments	2.75	3.17	-0.42	-4.28
Market-to-book	2.14	1.51	0.62	13.38
R&D	0.02	0.02	0.00	-1.34
Stock return volatility	0.37	0.45	-0.09	-7.18
Free cash flow	0.11	0.05	0.05	11.29
Return on assets	0.17	0.10	0.07	16.77
Governance index	9.27	9.39	-0.11	-0.84
CEO ownership	0.02	0.02	0.00	1.67
CEO tenure	7.64	6.40	1.24	3.37
Number of observations	1,672	464		
Number of firms	495	241		
Fraction of observations in violation		0.22		
Fraction of firms in violation		0.44		

Table IA.6: Summary Statistics for Violation and Non-Violation Groups

This table presents mean, standard deviation, minimum, 10th percentile, median, 90th percentile, maximum, number of observations, and number of firms for each variable. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample in Panel A includes observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth ($h = 0.4$). The sample in Panel B includes observations outside the bandwidth.

Panel A: Sample within Bandwidth $h = 0.4$

	Mean	Standard deviation	Minimum	10th pctile	Median	90th pctile	Maximum	Number of obs.	Number of firms
Number of independent directors	6.36	2.30	1.00	3.00	6.00	9.00	14.00	665	222
Number of non-independent directors	2.95	1.84	1.00	1.00	2.00	6.00	10.00	665	222
Ratio of indep. to non-indep. directors	3.42	2.72	0.10	0.75	2.67	8.00	13.00	665	222
Number of directors	9.31	2.11	5.00	7.00	9.00	12.00	19.00	665	222
Firm size ($ millions)	2,711	4,374	73	346	1,269	6,153	35,525	665	222
Leverage	0.31	0.15	0.00	0.09	0.33	0.49	0.74	665	222
Firm age	23.34	17.50	1.00	7.00	18.00	43.00	81.00	665	222
Number of segments	2.98	2.08	1.00	1.00	3.00	6.00	10.00	665	222
Market-to-book	1.47	0.57	0.70	0.96	1.37	2.10	6.24	665	222
R&D	0.02	0.03	0.00	0.00	0.00	0.04	0.24	665	222
Stock return volatility	0.37	0.18	0.12	0.19	0.33	0.59	1.67	665	222
Free cash flow	0.07	0.05	-0.22	0.01	0.08	0.13	0.26	665	222
Return on assets	0.13	0.06	-0.17	0.07	0.12	0.20	0.41	665	222
Governance index	9.42	2.66	3.00	6.00	9.00	13.00	15.00	665	222
CEO ownership	0.03	0.06	0.00	0.00	0.00	0.09	0.30	665	222
CEO tenure	7.94	7.92	0.00	1.00	6.00	17.00	49.00	665	222
Covenant violation	0.32	0.47	0.00	0.00	0.00	1.00	1.00	665	222
Binding distance	0.06	0.18	-0.39	-0.22	0.10	0.27	0.38	665	222

Panel B: Sample outside Bandwidth $h = 0.4$

	Mean	Standard deviation	Minimum	10th pctile	Median	90th pctile	Maximum	Number of obs.	Number of firms
Number of independent directors	6.40	2.05	1.00	4.00	6.00	9.00	15.00	2,136	546
Number of non-independent directors	2.70	1.58	1.00	1.00	2.00	5.00	13.00	2,136	546
Ratio of indep. to non-indep. directors	3.47	2.54	0.17	1.00	2.58	8.00	13.00	2,136	546
Number of directors	9.10	2.13	4.00	6.00	9.00	12.00	17.00	2,136	546
Firm size	3,801	12,725	43	374	1,226	7,605	270,634	2,136	546
Leverage	0.23	0.16	0.00	0.01	0.23	0.43	0.87	2,136	546
Firm age	22.31	17.40	1.00	6.00	17.00	42.00	81.00	2,136	546
Number of segments	2.84	1.85	1.00	1.00	3.00	5.00	10.00	2,136	546
Market-to-book	2.00	1.25	0.62	1.07	1.64	3.31	8.89	2,136	546
R&D	0.02	0.04	0.00	0.00	0.00	0.07	0.37	2,136	546
Stock return volatility	0.39	0.21	0.12	0.19	0.34	0.65	1.74	2,136	546
Free cash flow	0.10	0.09	-0.79	0.01	0.09	0.19	0.36	2,136	546
Return on assets	0.16	0.09	-0.66	0.07	0.15	0.26	0.44	2,136	546
Governance index	9.30	2.63	3.00	6.00	9.00	13.00	17.00	2,136	546
CEO ownership	0.02	0.05	0.00	0.00	0.00	0.05	0.30	2,136	546
CEO tenure	7.37	7.38	0.00	1.00	5.00	17.00	48.00	2,136	546
Covenant violation	0.22	0.41	0.00	0.00	0.00	1.00	1.00	2,136	546
Binding distance	0.07	1.66	-7.36	-1.17	0.43	0.99	4.14	2,136	546

Table IA.7: Regression of Firm Characteristics

This table presents estimates of firm fixed effects panel regressions of firm size (log), leverage, 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, CEO tenure, and investment. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. All explanatory variables are contemporaneous. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth ($h = 0.4$). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

	Covenant violation	
	Coefficient	t-statistic
Firm size	-0.0031	-0.19
Leverage	-0.0033	-0.05
Firm age	0.0030	0.13
Number of segments	-0.0792	-0.83
Market-to-book	0.0273	0.70
R&D	0.0020	0.76
Stock return volatility	0.0353	1.09
Free cash flow	0.0026	0.25
Return on assets	0.0006	0.06
Governance index	-0.0082	-0.06
CEO ownership	0.0052	0.85
CEO tenure	-0.1890	-0.19
Investment	-0.0020	-0.38
2nd order polynomial	Yes	
Firm fixed effects	Yes	
Year fixed effects	Yes	
Number of observations	665	
Number of firms	222	

Table IA.8: Regression of Number of Independent Directors - Placebo Test using SEC-DealScan Matched Sample

This table presents estimates of firm fixed effects panel regressions of the logarithm of the number of independent directors. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. All explanatory variables are lagged two years. The estimates are shown using different distances to the real threshold, which is set at zero. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample drops observations in case the covenant violation dummy is zero but there is a covenant violation according to the SEC's 10-Q or 10-K filings. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth (h). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

	Distance to real threshold								
	−0.4	−0.3	−0.2	−0.1	0.0	0.1	0.2	0.3	0.4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Covenant violation	0.19	0.02	0.11	0.03	0.24***	-0.04	0.01	-0.04	-0.01
	(1.00)	(0.19)	(0.80)	(0.35)	(3.35)	(-0.87)	(0.15)	(-0.97)	(-0.37)
Marginal effects (at mean)	1.21	0.13	0.70	0.19	1.53	-0.26	0.06	-0.26	-0.06
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.209	0.220	0.205	0.150	0.174	0.177	0.202	0.185	0.197
Number of observations	94	139	220	381	590	778	958	983	1,008
Number of firms	41	59	87	142	203	248	292	291	295

Table IA.9: Summary Statistics - SEC Sample

This table presents mean, standard deviation, minimum, 10th percentile, median, 90th percentile, maximum, number of observations, and number of firms for each variable. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) firms from 1994 to 2008. Financial industries are omitted (SIC codes 6000-6999). Covenant violation is a dummy variable that takes a value of one if the firm reports a covenant violation in SEC's 10-Q or 10-K filings. 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 pctile	Median	90th pctile	Maximum	Number of obs.	Number of firms
Number of independent directors	6.63	2.30	1.00	4.00	7.00	10.00	22.00	8,514	1,296
Number of non-independent directors	2.91	1.75	1.00	1.00	3.00	5.00	13.00	8,514	1,296
Ratio of indep. to non-indep. directors	3.46	2.70	0.08	0.83	2.50	8.00	15.00	8,514	1,296
Number of directors	9.54	2.37	4.00	7.00	9.00	13.00	28.00	8,514	1,296
Firm size ($ millions)	5,340	15,336	38	336	1,624	13,779	697,239	8,514	1,296
Leverage	0.24	0.16	0.00	0.01	0.24	0.43	0.89	8,514	1,296
Firm age	27.54	19.80	1.00	7.00	24.00	59.00	81.00	8,514	1,296
Number of segments	2.81	1.94	1.00	1.00	3.00	6.00	10.00	8,514	1,296
Market-to-book	2.01	1.45	0.53	1.07	1.56	3.38	27.09	8,514	1,296
R&D	0.03	0.05	0.00	0.00	0.00	0.10	0.66	8,514	1,296
Stock return volatility	0.36	0.20	0.11	0.16	0.31	0.60	2.24	8,514	1,296
Free cash flow	0.09	0.09	-0.92	0.00	0.09	0.18	0.36	8,514	1,296
Return on assets	0.15	0.08	-0.67	0.07	0.14	0.25	0.43	8,514	1,296
Governance index	9.37	2.63	2.00	6.00	9.00	13.00	17.00	8,514	1,296
CEO ownership	0.02	0.05	0.00	0.00	0.00	0.05	0.34	8,514	1,296
CEO tenure	7.38	7.44	0.00	1.00	5.00	17.00	55.00	8,514	1,296
Covenant violation	0.05	0.22	0.00	0.00	0.00	0.00	1.00	8,514	1,296

Table IA.10: Regression of Number of Independent Directors - SEC Sample

This table presents estimates of firm fixed effects panel regressions of the logarithm of the number of independent directors. Covenant violation is a dummy variable that takes a value of one if the firm reports a covenant violation in SEC's 10-Q or 10-K filings. The firm-level control variables are firm size (log), leverage, 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. All explanatory variables are lagged two years. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008. Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

	All violations		First violations		New violations	
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation - SEC	0.04**	0.04**	0.04*	0.04*	0.03*	0.03*
	(2.34)	(2.53)	(1.81)	(1.93)	(1.77)	(1.77)
Marginal effects (at mean)	0.27	0.27	0.27	0.27	0.20	0.20
Polyn. and covenant quintile indicators	Yes	Yes	Yes	Yes	Yes	Yes
Firm level controls	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.173	0.198	0.169	0.194	0.172	0.198
Number of observations	8,514	8,514	7,741	7,741	8,337	8,337
Number of firms	1,296	1,296	1,223	1,223	1,294	1,294

Table IA.11: Regression of Number of Independent Directors - Excluding CEO Turnover

This table presents estimates of firm fixed effects panel regressions of the logarithm of the number of independent directors. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. The firm-level control variables are firm size (log), leverage, 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. All explanatory variables are lagged two years. Columns (1) and (2) present estimates of regressions excluding observations in the two years after the CEO is replaced. Columns (3) and (4) present estimates of regressions excluding observations in the year in which the CEO is replaced and a violation occurred as well as the two years after the CEO turnover. Columns (5) and (6) present estimates of regressions excluding observations in the two years after the CEO is replaced and a violation occurred. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth ($h = 0.4$). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.28**	0.30***	0.24***	0.25***	0.24***	0.26***
	(2.49)	(2.80)	(2.87)	(3.27)	(2.83)	(3.08)
Marginal effects (at mean)	1.60	1.92	1.53	1.60	1.53	1.66
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm level controls	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	No	No	No	No	No	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.248	0.346	0.216	0.292	0.199	0.270
Number of observations	411	411	541	541	576	576
Number of firms	150	150	185	185	195	195

Table IA.12: Regression of Number of Independent Directors: Including Interest Coverage Covenant

This table presents estimates of firm fixed effects, first differences and ordinary least squares (OLS) panel regressions of the logarithm of the number of independent directors. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of five covenants (current ratio, net worth, tangible net worth, debt-to-EBITDA, interest coverage) during the year in at least one quarter. The firm-level control variables are firm size (log), leverage, 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. All explanatory variables are lagged two years. Panel A presents estimates using all covenant violations, and Panel B presents estimates using the first covenant violation or new violations for each firm. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth ($h = 0.4$). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

Panel A: All Violations

	Firm fixed effects		First differences		OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.20***	0.21***	0.30***	0.28***	0.24**	0.18**
	(2.92)	(3.05)	(3.50)	(3.53)	(2.53)	(2.06)
Marginal effects (at mean)	1.30	1.36	1.95	1.82	1.56	1.17
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm level controls	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	No	No	No	No
Industry fixed effects	No	No	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.192	0.253	0.146	0.181	0.303	0.503
Number of observations	655	655	469	469	655	655
Number of firms	217	217	217	217	211	211

Panel B: First and New Violations

	First violations			New violations		
	Firm FE	First diff.	OLS	Firm FE	First diff.	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.30***	0.36***	0.26**	0.18**	0.32***	0.31**
	(3.18)	(3.13)	(2.26)	(2.15)	(3.10)	(2.46)
Marginal effects (at means)	1.95	2.33	1.69	1.17	2.08	2.01
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	No	No	Yes	No	No
Industry fixed effects	No	Yes	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.158	0.175	0.386	0.209	0.204	0.332
Number of observations	514	347	514	482	349	482
Number of firms	188	181	188	166	158	166

Table IA.13: Regression of Number of Independent Directors - Sample Period 1994-2014

This table presents estimates of firm fixed effects, first differences and ordinary least squares (OLS) panel regressions of the logarithm of the number of independent directors. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. The firm-level control variables are firm size (log), leverage, 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. All explanatory variables are lagged two years. Panel A presents estimates using all covenant violations, and Panel B presents estimates using the first covenant violation or new violations for each firm. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2014 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth ($h = 0.4$). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

Panel A: All Violations

	Firm fixed effects		First differences		OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.17***	0.16***	0.14**	0.13**	0.20***	0.14**
	(3.57)	(3.54)	(2.47)	(2.42)	(2.95)	(2.46)
Marginal effects (at mean)	1.15	1.08	0.95	0.88	1.36	0.95
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm level controls	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	No	No	No	No
Industry fixed effects	No	No	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.232	0.275	0.110	0.138	0.290	0.483
Number of observations	1,008	1,008	740	740	1,008	1,008
Number of firms	300	300	286	286	300	300

Panel B: First and New Violations

	First violations			New violations		
	Firm FE	First diff.	OLS	Firm FE	First diff.	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.25***	0.22**	0.27***	0.18**	0.16*	0.25***
	(3.11)	(2.23)	(2.80)	(2.49)	(1.92)	(2.89)
Marginal effects (at means)	1.69	1.49	1.83	1.22	1.08	1.69
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	No	No	Yes	No	No
Industry fixed effects	No	Yes	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.235	0.144	0.367	0.268	0.141	0.304
Number of observations	720	498	720	744	553	744
Number of firms	244	229	244	235	223	235

Table IA.14: Regression of Fraction of Independent Directors

This table presents estimates of firm fixed effects, first differences and ordinary least squares (OLS) panel regressions of the logarithm of the ratio of the number of independent directors to the number of non-independent directors (Panel A) and the ratio of the number of independent directors to board size (Panel B). Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. The firm-level control variables are firm size (log), leverage, 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. All explanatory variables are lagged two years. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth ($h = 0.4$). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

Panel A: Ratio of Independent Directors to Non-Independent Directors

	Firm fixed effects		First differences		OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.44***	0.46***	0.49**	0.46**	0.44**	0.32*
	(3.19)	(3.30)	(2.59)	(2.52)	(2.22)	(1.78)
Marginal effects (at mean)	1.52	1.59	1.69	1.59	1.52	1.11
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm level controls	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	No	No	No	No
Industry fixed effects	No	No	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.252	0.304	0.150	0.165	0.376	0.501
Number of observations	665	665	472	472	665	665
Number of firms	222	222	214	214	222	222

Panel B: Ratio of Independent Directors to Board Size

	Firm fixed effects		First differences		OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.16***	0.17***	0.16**	0.15**	0.18**	0.13*
	(2.97)	(3.21)	(2.24)	(2.11)	(2.39)	(1.96)
Marginal effects (at mean)	0.11	0.12	0.11	0.10	0.13	0.09
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm level controls	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	No	No	No	No
Industry fixed effects	No	No	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.215	0.294	0.151	0.171	0.344	0.483
Number of observations	665	665	472	472	665	665
Number of firms	222	222	214	214	222	222

Table IA.15: Regression of Number of Independent Directors - Alternative Lags

This table presents estimates of firm fixed effects, first differences and ordinary least squares (OLS) panel regressions of the logarithm of the number of independent directors. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to EBITDA) during the year in at least one quarter. All explanatory variables are lagged one year in columns (1)-(3) and three years in columns (4)-(6). The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non- financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The sample includes only those observations in which the absolute value of the relative binding distance to the covenant threshold is less than the bandwidth ($h = 0.4$). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

	One year lag			Three years lag		
	Firm FE	First diff.	OLS	Firm FE	First diff.	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.04	0.13**	0.17**	0.18**	0.12	0.26***
	(0.67)	(2.20)	(2.07)	(2.52)	(0.99)	(2.81)
Marginal effects (at mean)	0.26	0.83	1.09	1.15	0.77	1.66
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	No	No	Yes	No	No
Industry fixed effects	No	Yes	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.252	0.100	0.315	0.153	0.166	0.337
Number of observations	743	547	743	535	373	535
Number of firms	246	235	246	181	173	181

Table IA.16: Regression of Number of Independent Directors - Annual Binding Distance

This table presents estimates of firm fixed effects, first differences and ordinary least squares (OLS) panel regressions of the logarithm of the number of independent directors. Covenant violation is a dummy variable that takes a value of one if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. The firm-level control variables are firm size (log), leverage, 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. All explanatory variables are lagged two years. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. Panel A includes only those observations in which the absolute value of the relative annual binding distance to the covenant threshold is less than the bandwidth ($h = 0.3$). Panel B includes only those observations in which the absolute value of the relative annual binding distance to the covenant threshold is less than the bandwidth ($h = 0.2$). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

Panel A: Bandwidth $h = 0.3$

	Firm fixed effects		First differences		OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.19***	0.20***	0.22***	0.23***	0.18**	0.14**
	(3.62)	(3.93)	(2.63)	(2.85)	(2.19)	(2.06)
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm level controls	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	No	No	No	No
Industry fixed effects	No	No	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.218	0.277	0.129	0.167	0.278	0.455
Number of observations	872	872	637	637	872	872
Number of firms	268	268	261	261	268	268

Panel B: Bandwidth $h = 0.2$

	Firm fixed effects		First differences		OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Covenant violation	0.22***	0.26***	0.27**	0.24**	0.19**	0.13
	(3.13)	(3.56)	(2.24)	(2.08)	(2.08)	(1.63)
2nd order polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Firm level controls	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	No	No	No	No
Industry fixed effects	No	No	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.176	0.243	0.160	0.194	0.348	0.533
Number of observations	473	473	348	348	473	473
Number of firms	166	166	164	164	166	166

Table IA.17: Regression of Investment, Financing and CEO Compensation - Collapsed Observations

This table presents estimates of regressions of investment, financing and CEO compensation around the time of covenant violations. A covenant violation occurs if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. All regressions include firm size (log) as control variable. New appointment is a (treatment group) dummy variable that takes a value of one if there is an increase in the number of independent directors between year 0 and year 2. After is a dummy variable that takes a value of one in the post-violation period. Panel A presents estimates in which the dependent variable is capital expenditures (scaled by lagged property, plant and equipment), net debt issues, net equity issues or SEO proceeds (scaled by lagged total assets). Panel B presents estimates in which the dependent variable is the logarithm of CEO total pay, salary, bonus, value of option grants (grant-date Black-Scholes value) or value of restricted stock grants (grant-date fair value). The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan. The data is collapsed into two periods: before violation (years -3, -2, and -1) and after violation (years 2, 3, and 4). Refer to Table A1 in the Appendix for variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels.

Panel A: Investment and Financing

	Investment	Net debt issues	Net equity issues	SEO proceeds
	(1)	(2)	(3)	(4)
New appointment × After	0.07	0.02	0.04*	0.03*
	(1.33)	(0.98)	(1.74)	(1.76)
After	-0.09**	-0.05***	-0.02	-0.02*
	(-2.36)	(-3.34)	(-1.38)	(-1.89)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
R^2	0.118	0.101	0.038	0.071
Number of observations	236	236	236	236
Number of firms	118	118	118	118

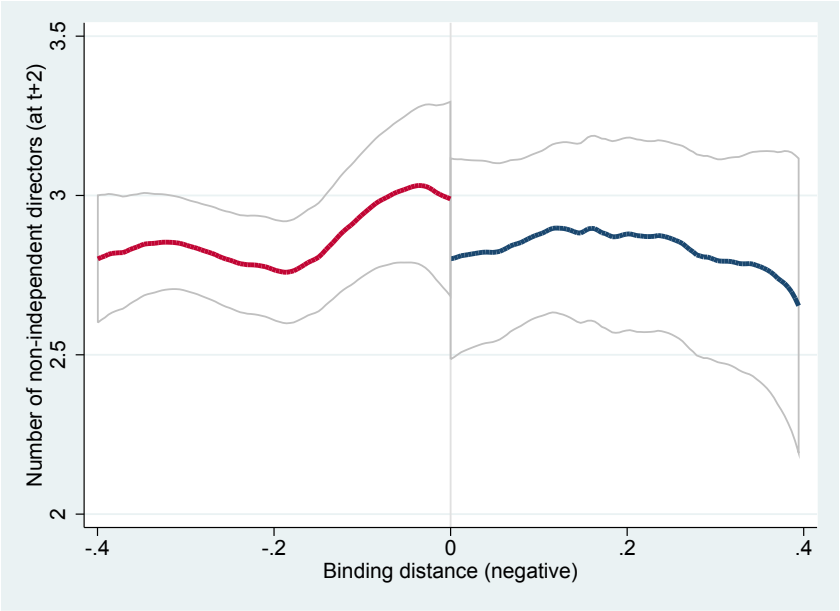
Panel B: CEO Compensation

	CEO Total pay (log)	CEO Salary (log)	CEO Bonus (log)	CEO Option (log)	CEO Stock (log)
	(1)	(2)	(3)	(4)	(5)
New appointment × After	0.13	-0.03	-0.26	0.64**	0.10
	(1.08)	(-0.57)	(-1.10)	(2.27)	(0.27)
After	0.14	0.14***	0.29*	-0.49*	0.73**
	(1.47)	(3.57)	(1.90)	(-1.82)	(2.49)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
R^2	0.326	0.461	0.146	0.095	0.472
Number of observations	233	233	206	185	115
Number of firms	118	118	117	110	80

Figure IA.1: Number of Non-Independent Directors and Binding Distance to Covenant Threshold

This figure shows nonparametric regression estimates of number of non-independent directors (two years after violation) on the relative binding distance to the covenant threshold. A covenant violation occurs if the firm violates at least one out of four covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. Panel A presents estimates using all covenant violations, and Panel B presents estimates using only the first covenant violation for each firm. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008 for which syndicated loans data are available from DealScan.

Panel A: All Violations



Panel B: First Violations

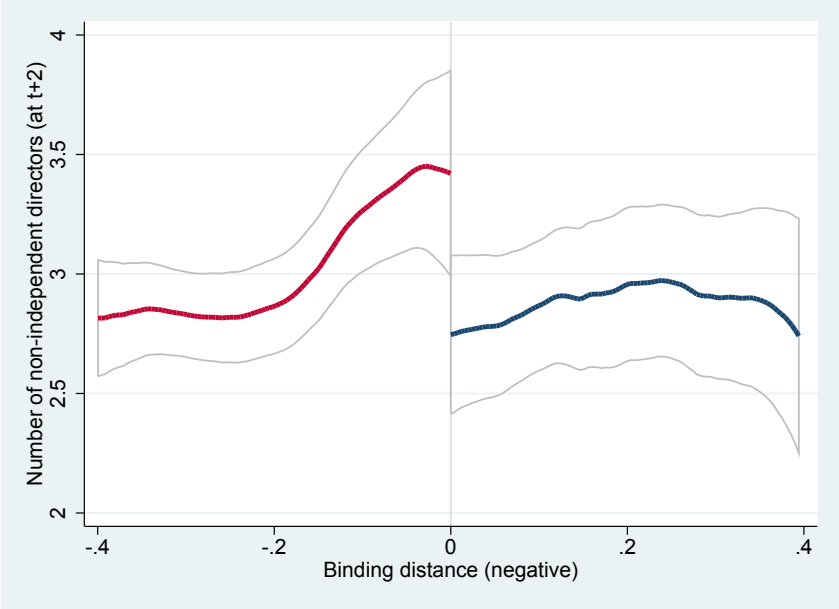


Figure IA.2: Ratio of Independent to Non-Independent Directors - SEC Sample

This figure shows the cross-sectional average and 95% confidence interval of the ratio of independent to non-independent directors in the four years before and after a covenant violation. A covenant violation occurs if the firm reports a financial covenant violation in SEC's 10-Q or 10-K filings. The sample consists of annual observations on Investor Responsibility Research Center (IRRC) non-financial firms from 1994 to 2008.