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Creditor Control Rights and Board Independence

DANIEL FERREIRA, MIGUEL A. FERREIRA, and BEATRIZ MARIANO*

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 have links to creditors. Firms that appoint new directors after violations are more likely to issue new equity, and to decrease payout, operational risk, and 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.

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), Falato and Liang (2016)). This literature describes such an improvement in creditors' bargaining power as an increase in "creditor control rights."[1]

*Daniel Ferreira is at the London School of Economics, CEPR, and ECGI. Miguel A. Ferreira is at the Nova School of Business and Economics, CEPR, and ECGI. Beatriz Mariano is at the Cass Business School, City, University of London. We thank Michael Roberts (the Editor), the Associate Editor, two anonymous referees, Andres Almazan, Per Axelson, Ilona Babenko, Laurent Bach, Tom Bates, Bruno Biais, Daniel Carvalho, Geraldo Cerqueiro, Jonathan Cohn, Andrew Ellul, Cesare Fracassi, Diego Garcia, Jarrad Harford, Jay Hartzell, Thomas Hellmann, Leonid Kogan, Yrjo Koskinen, Kai Li, Chen Lin, Laura Lindsey, Daniel Metzger, Walter Novaes, Daniel Paravisini, Enrico Perotti, Alessio Saretto, Clemens Sialm, Stephan Siegel, Laura Starks, and Margarita Tsoutsoura; participants at the European Finance Association Annual Meeting and University of British Columbia Winter Finance Conference; and seminar participants at Arizona State University, EIEF, ESCP, IE Business School, Hong Kong University of Science and Technology, London School of Economics, Nanyang Technological University, National University of Singapore, PUC-Rio, Queen Mary University, Singapore Management University, University of British Columbia, University of Cambridge, University of New South Wales, Universitat Pompeu Fabra, University of Technology–Sidney, University of Texas–Austin, and University of Sydney for helpful comments. Miguel Ferreira acknowledges financial support from the European Research Council. We have read the *Journal of Finance*'s disclosure policy and have no conflicts of interest to disclose.

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

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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 loan covenants on firm policies, particularly those policies that require the board to behave proactively. By changing governance, covenant violations can thus affect firm policies many years after the event, implying that current and past credit agreements have a long-lasting impact on a firm's governance.

Our main finding is that firms tend to appoint new independent directors to their boards following covenant violations. The new directors typically do not replace outgoing directors, which implies that board size increases as new directors are appointed. We call a covenant breach an *implied covenant violation* because a registered violation may not occur if a firm obtains a covenant waiver through renegotiation. We examine implied rather than registered covenant violations because renegotiation is one of the mechanisms through which loan covenants can affect firm choices. The effect of implied covenant violations on the number of independent directors is sizable: in our baseline specification, a violation leads to a 24% increase in the number of independent directors. Our results support the hypothesis that covenant violations lead to changes in board composition.

Our work is related to a number of studies that focus on the impact of creditors and credit agreements on corporate governance. Gilson (1990) is 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 credit agreements affect board appointments outside bankruptcy, and we provide a causal estimate of the effect of implied 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 available to creditors. However, 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 requires 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 creditors' interests, covenants become less important. Our findings are broadly consistent with this hypothesis.

The finding that loan covenant violations lead to the appointment of new directors to the board raises a number of questions: who are these directors?, are they related to creditors?, and if so, how are they related? We show that postviolation directors are similar to ordinary directors in all but one respect. Specifically, directors appointed following covenant violations are much more likely to hold positions in other firms that borrow from the same banks.

What do these new directors do? We find that firms that appoint new directors after covenant violations are more likely to change firm policies that require board initiative. Such firms are more likely to raise new equity through seasoned equity offerings (SEOs) and to invest than firms that violate covenants but do not change their boards, which suggests that reformed boards are in a better position to address debt overhang problems. In addition, reformed boards appear to take actions that decrease payout and operational risk, which alleviates concerns about risk-shifting problems. We also find that the structure of CEO compensation changes after violations. After violations, in firms that do not change the board, CEOs experience an increase in cash bonuses that roughly compensates them for the reduction in the value of their equity-based compensation. This trend is reversed, however, in firms that appoint new independent directors after violations: cash bonuses fall and equity-based pay increases more than in firms without such appointments.

To summarize, we find that new directors are more likely to have links to creditors and that reformed boards are more likely to adopt creditor-friendly policies. We also show that firms with stronger lending relationships with their creditors appoint more directors in response to violations than firms without such relationships. However, this evidence does not settle the question of 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. But the process that follows could be largely in the hands of management or large shareholders who push for changes in board composition. For example, it could be the case that, to improve its negotiation stance, a firm chooses to hire a director who has experience dealing with a particular bank.

The reasons for creditors to care about board composition are not obvious. Even if creditors can influence board appointments, directors still have a fiduciary obligation to shareholders.[2] In addition, explicit intervention by creditors may force them to have a fiduciary obligation to shareholders or, in the case of bankruptcy, make them subject to equitable subordination (i.e., courts may

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

treat their claims as subordinate on equitable grounds). Thus, debt contracts typically do not give creditors explicit rights over board appointments. However, this does not mean that creditors abstain from corporate governance activism. There is ample anecdotal evidence of lenders demanding changes to board composition as a consequence of credit renegotiations.[3] There are also cases in which a contract renegotiation triggered by a covenant violation is reported together with the appointment of new independent directors, although no explicit link is mentioned.[4] 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 empirically documenting their activities challenging.

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), Falato and Liang (2016)). 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.

Our findings also provide direct evidence of the empirical relevance of models of contingent allocation of control rights (e.g., Aghion and Bolton (1992), 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)). Our results help explain the observed positive relationship between leverage and board independence (Boone et al. (2007), Coles, Daniel, and Naveen (2008), Linck, Netter, and Yang (2008)). Our evidence shows that leverage can 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.

[3] For example, a forbearance agreement between BMO Harris Bank and Quadrant 4 System Corporation required that "the Company appoint(ed) three new directors who were acceptable to the Board and to BMO." Similarly, after failing to comply with its financial covenants and other contractual obligations, RCS Capital Corporation entered an agreement with its lenders that required "the appointment of an independent director reasonably acceptable to such lenders." See the Internet Appendix, which is available in the online version of the article on the *Journal of Finance* website, for more details on these and other examples.

[4] See, for example, the case of Hooper Holmes in the Internet Appendix.

I. Data

To construct our sample, we start with the nonfinancial 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. We restrict the sample to loans with information on maturity and spread over 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 firm's current ratio, net worth, tangible net worth, or debt-to-EBITDA ratio.

Our main sample uses accounting data from 1994 to 2006 and board data from 1996 to 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). Economic considerations determine the sample period. First, we do not include the period of the recent financial crisis, which led to major changes in bank behavior, regulation, credit market conditions, and the financial performance of borrowers. Second, while "covenant-light contracts" were virtually nonexistent prior to 2006, 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 enforcement. The wide use of covenant-light contracts is thus likely to attenuate the effect of violations on firm policies. Our baseline sample therefore focuses only on data from 1994 to 2008. In the Internet Appendix, however, we rerun all of our main tests for an extended sample covering the 1994 to 2014 period.

For each loan, we first obtain covenant thresholds on the firm's current ratio, net worth, tangible net worth, and debt-to-EBITDA ratio. We assume that the firm is bound by the covenants each 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 quarterly Compustat data to compute the accounting variables. If the accounting variable is less than or equal to the threshold, we say there is an implied covenant violation. In the case of the debt-to-EBITDA covenant, an implied covenant violation occurs if the accounting variable is greater than or equal to the threshold.

Since some of the relevant accounting variables are ratios and others are measured in dollars, we measure the distance to the covenant threshold as a proportion of the threshold. We call the minimum distance to the threshold across the four covenants the *binding distance*, which is given as follows:

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

where

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

i and t denote firm and year, respectively, $j = 1, ..., 4$ denotes a quarter in 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 of covenant type k, in quarter j of year t for firm i. Equation (1) applies strictly only to the current ratio, net worth, and tangible net worth covenants. For the debt-to-EBITDA covenant, $\tilde{D}_{itjk}$ 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.

Equation (1) implies that an implied covenant violation is a firm-year observation in which the firm breaches at least one covenant threshold 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 an implied covenant violation.[5]

Our final (baseline) sample covers 597 firms and 2,801 firm-year observations. For this sample, we find that 51% of firms have at least one covenant violation during the sample period (305 firms), and 24% of firm-year observations include a violation (675 firm-year observations).[6] Because a covenant violation requires a violation in only one quarter of the year, the number of violation observations 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.

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 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 covenant threshold and that, without renegotiation, the firm would almost certainly trigger the covenant. 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

[5] Because EBITDA may assume values that are close to zero or even negative, the debt-to-EBITDA ratio can become meaningless in such cases. We therefore replace negative values with a debt-to-EBITDA ratio equal to its 99$^{\text{th}}$ 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.

[6] 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.

possible sources of measurement errors, although we see no a priori reason to suspect that such errors would bias the results toward finding a positive effect of covenant violations on board independence.

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, we face a trade-off: using this variable substantially increases the variation in the sample, but it also adds noise. As only a few other papers use debt-to-EBITDA covenants (e.g., Demiroglu and James (2010), Denis and Wang (2014), Freudenberg et al. (2017)), we pay special attention to the construction of this variable. We read a sample of 50 credit agreement contracts of borrowers who 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, debt excludes subordinated debt or it is measured net of cash holdings. In Denis and Wang (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 (over a test period equal to the four most recent fiscal quarters).

To minimize concerns about measurement errors, in Section E, we consider an alternative definition of violations. Here, we include only covenant violations registered with the SEC. This definition has the advantage of eliminating many of the concerns above. However, it has also two disadvantages: we obtain a severely reduced sample size, and we may miss many renegotiated violations. Notwithstanding, our results appear stronger when we consider only registered violations. This finding suggests that, if anything, measurement errors in our original definition of violations work against finding a positive effect of violations on board independence.

Table I presents descriptive statistics for each variable in our main sample. The Appendix provides variable definitions and data sources. The median of the binding distance is 0.30. The minimum and 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 10^{th} percentile (-0.63). Even if these observations are not statistical outliers, it makes little economic sense to use them to estimate the effects of breaching 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. Compared 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.I in the Internet Appendix compares the averages of each variable

Table I

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 loan 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 1 if the firm violates at least one out of four types of 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. The Appendix presents variable definitions. Financial ratios are winsorized at the bottom and top 1% levels.

	Mean	Standard Deviation	Minimum	10th Percentile	Median	90th Percentile	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 nonindependent directors	2.76	1.65	1.00	1.00	2.00	5.00	13.00	2,801	597
Ratio of independent to nonindependent 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 nonconnected 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

across data sources.[7] This comparison reveals that firms in our sample are substantially larger than those in both the Compustat and DealScan samples, which is to be expected because IRRC collects data for S&P 1500 companies only. Consistent with this fact, our sample has fewer covenant violations than the DealScan sample (24% versus 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.[8] 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.[9]

Table IA.II 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. Average net worth and tangible net worth are significantly higher than their corresponding thresholds. The debt-to-EBITDA covenant has 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.III 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 contains a lower fraction of observations with covenant violations than that of Chava and Roberts (2008). Specifically, they find that 15% of their firm-quarter observations are associated with a violation of the current ratio covenant and 14% with a violation of the net worth (and tangible net worth) covenant, while in our sample, these figures are 9% and 5%, respectively. These differences are 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 (2008) sample, the average 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 average values are 1.44 (current ratio), 0.58 (net worth), and 0.65 (tangible net worth).

[7] 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 can 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.

[8] 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).

[9] 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)).

II. Methodology

A. Empirical Challenges

Our goal is to estimate the average effect of an implied 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 violation as the effect that a violation would have while holding financial performance and other confounding factors constant. The main empirical challenge is to isolate the pure effect of a violation from the effect of financial performance and other confounding factors.

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 the informal power that creditors have over the firm in negotiations. 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 the case that management or large shareholders encourage changes in policies in response to increased creditor control rights (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 leads to the appointment of new directors, our secondary goal is to analyze the mechanisms in greater detail.

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 appropriate for addressing firm heterogeneity in our particular application. There are at least four challenges to applying 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), 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 control for the distance to a violation threshold and for a long list of time-varying firm variables, including measures of market and operating performance. Third, 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. 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 designed to detect jumps in board independence at other points near the actual covenant thresholds.

B. Empirical Model

Our baseline specification is given by

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

where y_{it} is either the number of independent directors or the number of nonindependent directors, v_{it} is an indicator variable that takes the value of 1 if firm i breaches a covenant threshold in year t (i.e., $v_{it} = 1$ if $D_{it} \leq 0$), $\sum_{p=1}^{P} [\gamma_{p0} + \gamma_{p1} v_{it}] D_{it}^{p}$ is a polynomial of order P of the distance to threshold, where the coefficients γ_{p0} and γ_{p1} can differ on the left- and right-hand sides of the threshold, α_t and f_i represent year and firm fixed effect respectively, 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 semielasticity and thus has a simple interpretation as the percentage change in y_{it} due to a violation. To facilitate 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 the number of independent directors or the number of nonindependent 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. First, we can always calculate the effect on the ratio from the effects on the levels. More importantly, 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. There are three reasons to expect a lag between the first covenant violation and changes to the board. First, the date of a covenant violation (actual or implied) may indicate the start of negotiations between the firm and its lenders. Such negotiations may result in future agreements, such as new credit or forbearance agreements. Such agreements may then require (formally or informally) the appointment of new directors to the board. The lag between an initial covenant violation and a follow-up agreement that requires board changes can be substantial. In the Internet Appendix, we describe an example of explicit creditor intervention (Peekay Boutiques Inc., Auburn, WA, United States) in which lenders demand the appointment of new board members in a contract signed two years after the first violation. There can also be lags between an agreement and the date when new directors are appointed (see the case of Quadrant 4 System Corporation in the Internet Appendix). And even when changes do occur shortly after a violation, they may be recorded with a lag of one year, if the appointment is effective as of the next fiscal year (see the case of RCS Capital Corporation, in which an appointment occurs only five days after the agreement but in a new fiscal year).

Second, directors can normally 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. Typically, new directors have to be nominated well in advance of annual meetings. State corporate law and a firm's charter regulate the appointment of directors. These rules may imply a significant lag between the decision to appoint a new director and its actual implementation.[10]

Finally, we note that because board turnover is typically low, the effect of violations on appointments is cumulative: the effect in two years is

[10] Of course, there are also situations in which appointments can be made quickly, such as when directors resign or when a new position is created and temporarily filled until the next formal election (e.g., Arena and Ferris (2007)).

(approximately) the sum of year 1 and year 2 appointments. In the Internet Appendix, 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 instead choose an ad hoc narrow bandwidth ($h = 0.4$) as the baseline, which generates a sample that includes 665 observations (24% of the full sample).[11] The standard deviation of the binding distance is 1.45 (see Table I). Thus, one unit of binding distance is equivalent to 0.69 of a standard deviation. The $h = 0.4$ bandwidth is therefore roughly 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.IV 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.IV 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% to 46%, which appears fairly random. This suggests that our choice of bandwidth is likely the 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.

Another possible reason for sample imbalance is manipulation: firms may manipulate earnings to avoid breaching the threshold. Although sample balance does not appear to be an issue for sufficiently low h, we cannot a priori rule out manipulation or other similar sample selection concerns, such as survivorship bias.[12] We thus use the panel structure of our data to mitigate concerns about the nonrandom nature of the subsamples to the right and 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

[11] 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.

[12] 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)).

both in state $v_{it} = 0$ and in state $v_{is} = 1$, where $s \neq t$—this may be a nonrandom sample of firms.

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.[13]

C. Discontinuity Sample: Descriptive Statistics

Table II 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 nonviolators. 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.V 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 (ROA), and CEO tenure.

Panel A of Table IA.VI in the Internet Appendix presents summary statistics for the discontinuity sample ($h = 0.4$). Compared to the full-sample statistics in Table I, firms in 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.VI appear similar to those in the full sample. For completeness, Panel B presents summary statistics for all observations that are not in the discontinuity sample.

III. Empirical Results

A. Graphical Analysis

Figure 1 illustrates our main finding using the raw data. This figure plots the evolution of the ratio of independent to nonindependent directors (annual cross-sectional averages) in the four years before and after an implied covenant violation. As can be seen, there is a clear increase in board independence

[13] 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.

Table II

Averages for Violation and Nonviolation 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 types of 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) nonfinancial firms from 1994 to 2008 for which syndicated loan 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 ($h = 0.4$).

	Noviolation (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 nonindependent directors (2 leads)	2.94	2.95	−0.01	−0.07
Number of nonindependent directors (2 lags)	3.36	3.59	−0.23	−1.30
Number of nonindependent directors (1 lag)	3.32	3.53	−0.22	−1.31
Number of nonindependent 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		

in the years following a violation. Figure 1 thus shows that we do not need sophisticated econometrics to uncover our main finding.

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 nonviolation 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

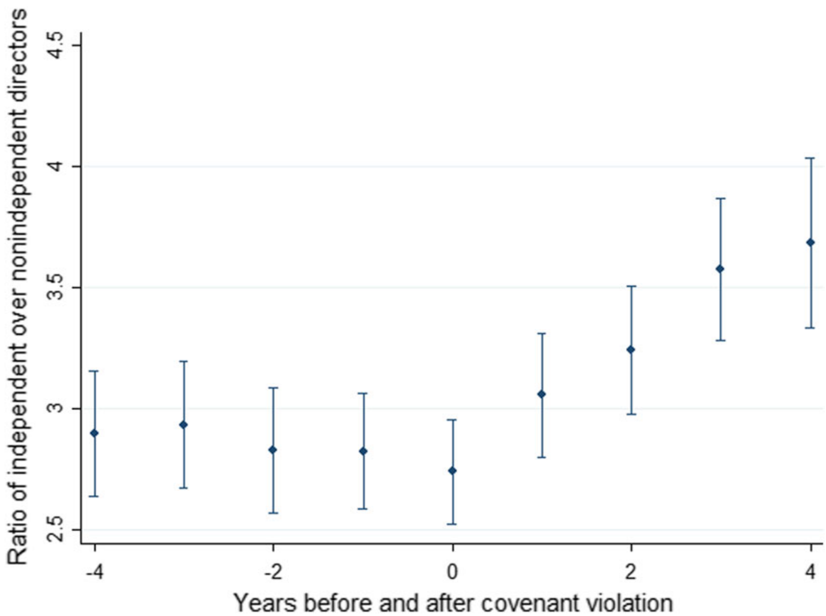

Figure 1. Ratio of independent to nonindependent directors. This figure shows the cross-sectional average and 95% confidence interval of the ratio of independent to nonindependent directors in the four years before and after a covenant violation. A covenant violation occurs if the firm violates at least one out of four types of 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) nonfinancial firms from 1994 to 2008 for which syndicated loan data are available from DealScan. (Color figure can be viewed at wileyonlinelibrary.com)

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 binding distance appears similar on both sides of the threshold.

The nonparametric results provide 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

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

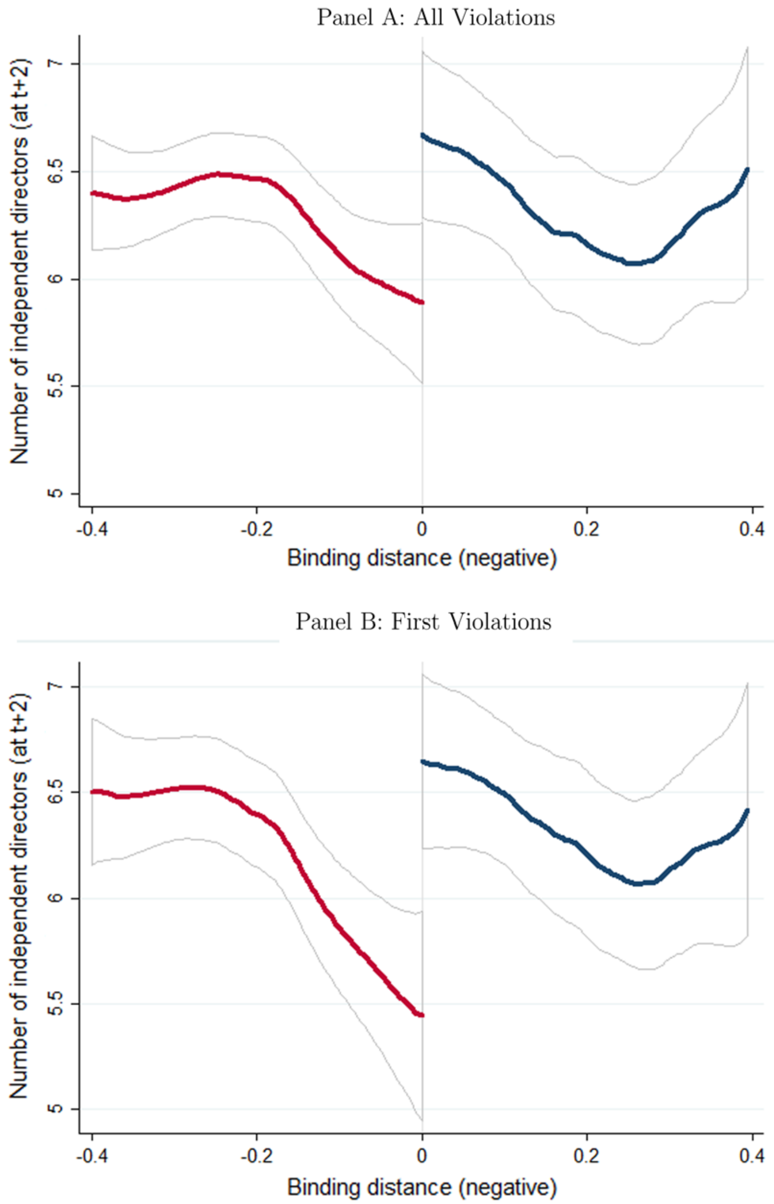

Figure 2. Number of independent directors and binding distance to covenant threshold. This figure shows nonparametric regression estimates of the number of independent directors (two years after violation) on the relative binding distance to the covenant threshold. A covenant violation occurs if the firm violates at least one out of four types of 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) nonfinancial firms from 1994 to 2008 for which syndicated loan data are available from DealScan. (Color figure can be viewed at wileyonlinelibrary.com)

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 nonviolation state. Panel B of Figure 2 replicates Panel A using the first violation indicator. We find that, if anything, the discontinuity is 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 nonindependent directors. Covenant violations appear to reduce the average number of nonindependent directors, but the effect is statistically less precise (in addition to economically less important) than that for the number of independent directors. We confirm this result in the parametric analysis below.

B. Primary Results

Table III 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. An implied covenant violation leads to a 24% increase 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.[14] This effect is 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 Ferreira, Ferreira, and Raposo (2011)).[15]

The specification in column (2) includes a long list of control variables: operating performance (ROA), 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 two years. To save space, we do not report the coefficients on the control variables.[16] We find that neither market-to-book nor ROA appears to be negatively related to board appointments. Although ROA enters negatively,

[14] 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.

[15] 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 et al. (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 CEO ownership) is similar.

[16] Table IA.VII in the Internet Appendix reports the coefficients on the control variables.

Table III

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 1 if the firm violates at least one out of four types of 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) nonfinancial firms from 1994 to 2008 for which syndicated loan 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 ($h = 0.4$). The Appendix presents variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

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
Second-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 (1)	First Differences (2)	OLS (3)	Firm FE (4)	First Differences (5)	OLS (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
Second-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	0.163	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

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 affect the estimates in an economically meaningful way. We confirm the irrelevance of these firm characteristics by replicating the regression in column (1) using firm characteristics as dependent variables. These are "balancing tests," as in Falato and Liang (2016). Table IA.VIII in the Internet Appendix summarizes these results. We find that implied covenant violations do not appear to have an economically or statistically significant (contemporaneous) effect on any of the firm characteristics used in our analysis. This indicates that violations cannot explain contemporaneous differences in firm characteristics, after controlling for 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 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 (implied) violation episode for each firm (i.e., we assume that the firm never returns to a nonviolation 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 nonviolation 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).

In columns (1) to (3), which use the first violation indicator, the estimated β rises to 0.34, which implies 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) to (6), which use the new violation indicator, the estimated β ranges from 0.25 (fixed effects) to 0.38 (OLS). We conclude that our results are not driven by multiple or "stale" violations.

Table IV replicates the regression analysis above using the logarithm of the number of nonindependent directors as the dependent variable. The estimates show that violations also increase board independence by reducing the number of nonindependent 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 Tables III and IV reveals that the number of new appointments is two to three times larger than the number of insider departures. Thus, new outside directors are typically not replacements for resignations by insiders; rather, board size increases after violations.

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

C. 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 V 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

Table IV
Regression of Number of Nonindependent 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 nonindependent directors. *Covenant violation* is a dummy variable that takes a value of 1 if the firm violates at least one out of four types of 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) nonfinancial firms from 1994 to 2008 for which syndicated loan 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 ($h = 0.4$). The Appendix presents variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

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
Second-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	0.245	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 (1)	First Diff. (2)	OLS (3)	Firm FE (4)	First Diff. (5)	OLS (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
Second-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 V

Regression of Number of Independent Directors: Polynomial Order and Bandwidth

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 1 if the firm violates at least one out of four types of 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) nonfinancial firms from 1994 to 2008 for which syndicated loan 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). The Appendix presents variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

		Bandwidth					
	Polynomial Order	$h = 0.3$ (1)	$h = 0.35$ (2)	$h = 0.4$ (3)	$h = 0.45$ (4)	$h = 0.5$ (5)	Full Sample (6)
Covenant violation	First	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	Second	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	Third	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	Fourth	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	Fifth	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

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 effect on β for orders of 3 or higher. We conclude that the effect of 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), Gelman and Imbens (2014)), for completeness, we report the estimates from global regressions in column

(6). The global regression results are consistent with the view 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 5 or higher, the estimated β is always statistically significant, although generally lower than that estimated with local regressions.

D. 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 violation events.

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

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

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

If $d = 0$, v^0_{it-2} equals the real threshold indicator, v_{it-2}; all other $d \neq 0$ constitute "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 $d \in [-h, h]$. We therefore run the same regressions as before, after replacing the true threshold v_{it-2} with a fake threshold v^d_{it-2}, $d \neq 0$.

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 $D_{it-2} = d$.[17]

[17] Our placebo test can be interpreted as a parametric version of Caetano (2015) exogeneity tests without instruments. She shows that such tests have nontrivial power only 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.

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 III for each placebo threshold.

Table VI shows the results. 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 the 10% level only for $d = 0.3$, but the estimated effect is 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.

E. Possible Mismeasurement of Covenant Violations

Are the estimates sensitive to our measure of covenant violations? To address this question, we consider an alternative definition of covenant violations. Specifically, we classify as covenant violations those that are registered with the SEC, as in Roberts and Sufi (2009) and Nini, Smith, and Sufi (2012). We refer to this set of violations as *registered violations*. The registered violation variable is constructed using information from 10-Q and 10-K filings with the SEC.[18] 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 that captures 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

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

Table VI

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 1 if the firm violates at least one out of four types of 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 of Investor Responsibility Research Center (IRRC) nonfinancial firms from 1994 to 2008 for which syndicated loan 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). The Appendix presents variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	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
Second-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 VII

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 1 if the firm violates at least one out of four types of 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 if the covenant violation dummy is zero but there is a covenant violation according to the 10-Q or 10-K filings with the SEC. Columns (3) and (4) drop observations if the covenant violation dummy is one but there is no covenant violation according to the 10-Q or 10-K filings with the SEC. Columns (5) and (6) drop both sets of observations. The sample consists of annual observations of Investor Responsibility Research Center (IRRC) nonfinancial firms from 1994 to 2008 for which syndicated loan 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 ($h = 0.4$). The Appendix presents variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	(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
Second-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

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 mismeasurement. Table VII 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 24% after a covenant violation.[19]

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 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

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

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 (Denis and Wang (2014), Roberts (2015)).

Columns (3) and (4) of Table VII 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 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 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.

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.X in the Internet Appendix presents descriptive statistics for the variables in our study using this sample. Figure IA.2 in the Internet Appendix replicates Figure 1 for this alternative sample. We find that the evolution of the ratio of independent to nonindependent 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 nonindependent directors increases following a violation.

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

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

where $\mathbf{h}(\mathbf{x}_{it-2})$ 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, ROA, interest expense-to-assets ratio, net worth-to-assets ratio, and cash-to-assets ratio. Table IA.XI in the Internet Appendix reports the estimates of equation (7). All specifications produce similar estimates. The semielasticity 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 nonindependent 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.

F. Robustness

Table IA.XII in the Internet Appendix reports the results of several robustness tests: (1) Poisson regressions, (2) regressions that exclude CEO turnover events, (3) regressions that exclude debt-to-EBITDA covenant, (4) regressions adding interest coverage covenants, (5) regressions that split the sample into two periods, before and after the Sarbanes-Oxley Act (SOX), (6) regressions that extend the sample to include observations after 2008, up to 2014, and (7) regressions that use the ratio of independent directors to board size as the outcome variable. Tables IA.XIII to IA.XX in the Internet Appendix report additional robustness checks such as using different lag structures, controlling for past stock returns, and using different criteria to determine which observations are retained in the discontinuity sample.

IV. Mechanisms and Consequences

A. Who Are the Directors Appointed after Covenant Violations?

We use directors' employment information to investigate whether there may be (indirect) links to banks. We classify a director as connected to a bank if the director holds a position (board or nonboard) 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. In the full sample, we find that 53% of all directors are connected to current banks. Of these connections, 88% happen through lead arrangers.

We estimate the regression in equation (3) 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 VIII shows the results. Column (1) reports our preferred specification (the analog of column (1) in Table III with firm and year fixed effects and no control variables). An implied covenant violation increases the number of connected independent directors by 18%. Columns (2) and (3) show that our

Table VIII

Regression of Number of Connected and Nonconnected 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 nonboard 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. *Nonconnected directors* include all other cases. *Covenant violation* is a dummy variable that takes a value of 1 if the firm violates at least one out of four types of 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 of Investor Responsibility Research Center (IRRC) nonfinancial firms from 1994 to 2008 for which syndicated loan 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 ($h = 0.4$). The Appendix presents variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	Number of Connected Directors			Number of Nonconnected Directors		
	Firm FE (1)	First Differences (2)	OLS (3)	Firm FE (4)	First Differences (5)	OLS (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
Second-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

findings are robust to different specifications. By contrast, columns (4) to (6) show that unconnected directors explain a negligible fraction of the effect of violations on board appointments with the effect economically small (5%) and statistically insignificant.[20]

The results in Table VIII show that violations explain the increase in the number of directors with indirect links to current banks. Given that about half of all directors have such indirect links, this finding is perhaps unsurprising. A relevant question is thus whether directors appointed outside violation events also have such connections. In other words, are directors appointed following violations more likely to have indirect links to banks than those appointed outside violation events?

[20] Table IA.XXI in the Internet Appendix reports estimates of the regressions in Table VIII, columns (1) to (3), for the number of connected independent directors through lead arrangers and other participants in the loan syndicate. We find that the results are economically stronger (in terms of marginal effects) when we measure connections through lead arrangers than through other participants.

Table IX

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. The control group includes independent directors who joined the board in the two years before the first violation. Director characteristics are from the BoardEx database. The Appendix presents variable definitions.

	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

To answer this question, we collect additional data on all newly appointed independent directors within two years after a firm first violates a covenant (i.e., the first time that we observe a change from $v_{it-1} = 0$ to $v_{it} = 1$). We identify 226 directors for which current and past employment (in publicly listed firms) data are available from the BoardEx database.

To create the control group, we match each new director to a randomly chosen independent director who joined the board in a nonviolation year (to maximize the number of matches, we consider the two years before the first violation). With this matching criterion, we match only 129 directors. Of these 129 new directors, 109 work for firms for which we are able to obtain syndicated loan data. Table IX 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. The main exception is the bank connection variable. We find that 75% of the directors appointed after implied violations 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.[21]

We also construct a variation of the bank connection variable in which we consider only connections through banks in the syndicate of the loan contract

[21] Table IA.XII in the Internet Appendix reports the results using two alternative control groups.

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.

In sum, we find that implied covenant violations increase the number of directors with links to the firm's current banks, and that directors appointed after violations are significantly more likely to have connections to banks than directors appointed outside these events. These results indicate that those with power to influence director nominations believe that, following violations, connected directors are particularly beneficial to their interests. However, the evidence cannot tell us whether the main beneficiaries are creditors, managers, or shareholders.

B. What Happens after New Directors Are Appointed?

In this section, we examine what happens when new directors are appointed following violations. We begin by identifying all first violations in the $h = 0.4$ subsample and creating a subsample of firms that experience a first violation. Using this subsample, we create both a *New appointment* dummy, which takes a value of 1 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), and an *After* dummy, which takes a value of 1 for years 2, 3, and 4 as the period after the violation; we define years −3, −2, and −1 as the period before the violation.

Next, we estimate the regression

$$y_{it} = \eta a_{it} + \beta n_i a_{it} + \alpha_t + f_i + \delta x_{it} + \varepsilon_{it}, \tag{8}$$

where y_{it} is a firm outcome, a_{it} is the *After* dummy (takes a value of 1 for years 2 to 4 after firm i experiences a first violation), n_i is the *New appointment* dummy, α_t and f_i are year and firm fixed effects, respectively, and x_{it} is a measure of firm size (the logarithm of assets).[22] Note that the *New appointment* dummy for the period before the violation is absorbed by the firm fixed effects, while the *After* dummy is defined in event time and thus not absorbed by the year fixed effects. The interpretation of coefficient β 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 β should not be interpreted as a causal effect.

Table X reports the results. Panel A examines investment, financial, and payout 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

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

Table X

Regression of Firm Policies

This table presents estimates of regressions of investment, financing, payout, volatility, and CEO compensation around the time of covenant violations. A covenant violation occurs if the firm violates at least one out of four types of covenants (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter. The firm-level control variable is firm size (log). *New appointment* is a (treatment group) dummy variable that takes a value of 1 if there is an increase in the number of independent directors between year 0 (the violation year) and year 2. *After* is a dummy variable that takes a value of 1 in the postviolation 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, SEO proceeds, changes in dividends (all scaled by lagged total assets), and changes in the standard deviation (annualized) of return on assets (ROA) over the last eight quarters. 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 of Investor Responsibility Research Center (IRRC) nonfinancial firms from 1994 to 2008 for which syndicated loan 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. The Appendix presents variable definitions. Robust t-statistics adjusted for firm-level clustering are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Panel A: Investment, Financing, Payout, and Volatility

	Investment (1)	Net Debt Issues (2)	Net Equity Issues (3)	SEO Proceeds (4)	Dividends (5)	Volatility of ROA (6)
New appointment × After	0.081	0.028	0.037*	0.035**	−0.002**	−0.007**
	(1.60)	(1.09)	(1.77)	(1.99)	(−2.51)	(−2.01)
After	−0.066*	−0.086**	0.034	0.007	0.000	−0.002
	(−1.92)	(−2.11)	(1.26)	(0.23)	(0.41)	(−0.24)
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.132	0.100	0.045	0.056	0.090	0.087
Number of observations	697	697	697	697	678	652
Number of firms	118	118	118	118	118	118

Panel B: CEO Compensation

	CEO Total Pay (log) (1)	CEO Salary (log) (2)	CEO Bonus (log) (3)	CEO Option (log) (4)	CEO Stock (log) (5)
New appointment × After	0.134	−0.041	−0.641***	0.510**	0.051
	(1.28)	(−0.82)	(−2.62)	(2.00)	(0.13)
After	−0.220	0.065	0.365*	−0.774**	−0.760**
	(−1.33)	(0.96)	(1.72)	(−2.35)	(−2.10)
Firm-level controls	Yes	Yes	Yes	Yes	Yes
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

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 implies that, for firms that do not appoint new directors in the postviolation years, the annual investment rate is 7% (of capital) lower than that in the previolation 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 with the difference 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. Column (5) measures the effect on payout using *Dividends* (scaled by lagged assets). We find a significant decrease in dividends in firms that appoint new directors following violations. Column (6) shows that operational risk—measured by the annualized standard deviation of ROA over the last eight quarters (*Volatility of ROA*)—significantly decreases in firms that appoint new directors following violations.[23]

In sum, although the evidence here is only suggestive, it points to more intense equity issuance, investment activity, and dividend cuts in firms that appoint new directors after covenant violations than in firms with no such appointments. In addition, the newly appointed directors appear to take actions that reduce operational risk. While some of these policies are likely to benefit both creditors and shareholders, we note that the dividend cuts and risk reductions are more likely to benefit creditors (see Becker and Stromberg (2012) for similar arguments).

Panel B of Table X presents results on CEO compensation after covenant violations. Columns (1) and (2) show that both *CEO total pay* and *CEO salary* do not seem to change significantly after violations. Column (3) shows that cash bonuses (*CEO bonus*) increase in the years after a violation for firms that do not appoint new independent directors, while they actually decrease for firms

[23] Table IA.XXIII in the Internet Appendix presents estimates of a variation of equation (8) in which we collapse the data into two periods: before and after covenant violation. We obtain estimates similar to those in Table X.

that appoint new directors. By contrast, column (4) shows that the value of *CEO option* grants decreases after a violation, but this decrease is much less pronounced in firms that appoint new directors.

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

The fact that covenant violations have long-lasting effects may appear puzzling since new appointments occur with a lag. However, most lending relationships between banks and firms involve multiple interactions over a long period of time, and thus banks may care about long-lasting effects. Consistent with this reasoning, Table IA.XXIV in the Internet Appendix shows that the effect of violations on board appointments is stronger in firm-bank pairs with repeated relationships. 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.

V. Conclusion

We show that credit agreements have consequences for the composition of boards of directors. We find that covenant violations lead to the appointment of new independent directors. As a consequence, board size increases. A large number of these newly appointed directors have connections to creditors with these connected directors explaining most of the estimated effects.

Our results also show that current and past credit agreements can have long-lasting effects on a firm's governance. In the years after a covenant violation, firms with newly appointed independent directors issue more equity, invest more, pay less dividends, and have less operational risk than those firms that do not reform their boards. This is consistent with firms taking actions to mitigate debt overhang and risk-shifting problems. Firms with new board appointments also have a different CEO compensation structure in the years following a violation: they are more likely to favor equity-based compensation over cash-based compensation. Since boards are responsible for approving investments, equity issuances, dividends, and CEO compensation, these changes in firm policies are consistent with the view that more independent boards actively favor policies that are beneficial to (not only) creditors in the postviolation period.

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Appendix: Variable Definitions

Variable	Definition (Source)
Number of independent directors	Number of board members who are independent directors (IRRC).
Number of nonindependent directors	Number of board members who are nonindependent directors (IRRC).
Number of directors	Number of board members (IRRC).
Number of connected directors	Number of board members who have a board or nonboard 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 nonconnected directors	Number of board members who do not have a board or nonboard 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 1 if the firm violates at least one out of four covenant types (current ratio, net worth, tangible net worth, and debt-to-EBITDA) during the year in at least one quarter, and 0 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 was added to the CRSP database.
Number of segments	Number of business segments in which the 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).

(*Continued*)

Variable Definitions—*Continued*

Variable	Definition (Source)
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).
Stock return	Annual stock return for the fiscal year (CRSP).
Investment	Ratio of capital expenditures to lagged net property, plant, and equipment (Compustat CAPEX/PPENT).
Net debt issues	Ratio of long-term net debt issue proceeds (issuance minus reduction of debt) to lagged total assets (Compustat (DLTIS − DLTR)/AT).
Net equity issues	Ratio of net equity issue 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).
Dividends	Ratio of common dividends to lagged total assets (Compustat DVC/AT).
Volatility of ROA	Standard deviation (annualized) of return on assets over the last eight quarters (Compustat).
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 option	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 director became CEO (ExecuComp).
Male	Dummy variable that takes a value of 1 if a director is male, and 0 otherwise (BoardEx).
Age	Age when director joins the board (BoardEx).
MBA	Dummy variable that takes a value of 1 if a director holds an MBA when he joins the board, and 0 othwerwise (BoardEx).
Financial education	Dummy variable that takes a value of 1 if a director has a financial education, defined as a degree in economics, accounting, finance, or management, when he joins the board, and 0 otherwise (BoardEx).
Audit or finance committee	Dummy variable that takes a value of 1 if a director is a member of the firm's finance or audit committees, and 0 otherwise (BoardEx).
Past audit or finance committee	Dummy variable that takes a value of 1 if a director has been a member of a firm's finance or audit committee based on past work experience, and 0 otherwise (BoardEx).

(*Continued*)

Variable Definitions—*Continued*

Variable	Definition (Source)
Past financial role	Dummy variable that takes a value of 1 if a director has held a financial position (CFO, finance director, treasury, accountant) based on past work experience, and 0 otherwise (BoardEx).
Financial firm connection	Dummy variable that takes a value of 1 if a director has held a position in a financial firm (SIC 6000-6999) based on past work experience, and 0 otherwise (BoardEx).
Financial firm board member	Dummy variable that takes a value of 1 if a director has held a board position in a financial firm (SIC 6000-6999) based on past work experience, and 0 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 1 if a director has a board or nonboard 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 1 if a director has a board or nonboard 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).

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.