Losing Control?

The 20-Year Decline in Loan Covenant Restrictions*

Thomas Griffin, Greg Nini, and David C. Smith**

November 2019

Abstract

This paper finds that lenders today rely on less restrictive financial covenants compared to 20 years ago, resulting in a nearly 70% drop in the annual proportion of U.S. public firms reporting a covenant violation. To study this decline, we develop a simple model of optimal covenant design that balances the costs associated with violations that occur when a firm is not in danger of financial distress ("false positives") with the costs of failing to detect a borrower in danger of financial distress ("false negatives"). We present evidence that lenders have eased the restrictiveness of covenants in ways that greatly reduce the ratio of false positives relative to false negatives, including by switching to covenant packages with higher signal-to-noise ratios.

* We thank seminar participants at the Norwegian School of Economics (NHH), OsloMet Business School, UC-Berkeley Workshop on Law and Economics, the Washington University Conference on Corporate Finance, the Drexel Corporate Governance Conference, and the Philly Five conference, as well as Ken Ayotte, Adam Badawi, Øyvind Bøhren, Gabriel Chodorow-Reich, and Larry Halperin for helpful comments. Will Lenhart provided research assistance.

** Griffin (thomas.griffin@villanova.edu) is from the Villanova School of Business, Villanova University; Nini (gpn26@drexel.edu) is from the LeBow College of Business, Drexel University; and Smith (dcs8f@virginia.edu) is from the McIntire School of Commerce, University of Virginia.

1 Introduction

Financial contracting theory motivates covenants as the primary tool that lenders use to protect their interests outside states of payment default (Tirole, 2010). In particular, financial covenants serve as “tripwires” that monitor borrower performance and grant creditors the right to sever lending commitments, recall outstanding debt, and foreclose on collateral if the borrower breaches a contractual threshold. A growing empirical literature documents the widespread use of these covenants in corporate loan agreements and shows that lenders use their bargaining power after a violation to renegotiate terms of the loan contract and influence firm policies.[1]

Against this backdrop, we show that lenders have substantially altered their reliance on financial covenants over the last 20 years. As shown in Figure 1, the average loan package in 2016 for U.S. public firms contained roughly half as many financial covenants as the average loan in 1997, and the covenants that remained were more than twice as slack as those twenty years prior, measured in terms of the borrower’s distance to the covenant threshold at origination. Using newly hand-collected data, we find that the decline in contractual restrictiveness has resulted in a nearly 70% drop in the annual proportion of U.S. public firms that report a covenant violation, as shown in Figure 2.

We examine this decline through the lens of a simple model of optimal covenant design that trades off the costs and benefits of restrictive covenants. Restrictive covenants are beneficial because they are violated frequently, allowing lenders to verify borrowers’ projected ability to repay based on small changes in observable performance (Townsend, 1979; Gale and Hellwig, 1985; Williamson, 1987). Violations provide lenders with the opportunity to catch – and potentially correct – borrower performance declines early and take steps to protect their claim by, among other

[1] See, e.g. Chava and Roberts (2008), Roberts and Sufi (2009), Nini, Smith, and Sufi (2009, 2012), Falato and Liang (2016), Freudenberg, Imbierowicz, Saunders, and Steffen (2017), Chava, Nanda, and Xiao (2017), Ferreira, Ferreira, and Mariano (2017), Balsam, Gu, and Mao (2018), Becher, Griffin, and Nini (2019), and Ersahin, Irani, and Le (2019).

actions, reducing loan commitments and requiring additional collateral. However, restrictive covenants are costly ex-ante because they require lender monitoring and reduce borrower operational flexibility (Smith and Warner, 1979), and costly ex post because violations require renegotiation (Berlin and Mester, 1992; Garleanu and Zwiebel, 2007). In a competitive lending environment, loan parties have the incentive to design optimal covenants that minimize these costs and maximize the benefits.

One way that loan parties can economize on this tradeoff is to use financial covenants that have a high signal-to-noise ratio, such that they are only violated when borrowers have a high risk of payment default. Borrowing terminology from diagnostic testing, a high quality covenant should yield a "true positive" result (a violation) only when a borrower has a high likelihood of financial distress and a "true negative" result (no violation) only when a borrower has a low likelihood of financial distress. Covenants with a low signal-to-noise ratio are likely to result in "false positives," where borrowers violate despite low risk of distress, and "false negatives," where borrowers become distressed without first violating the covenant.

In our theoretical framework, the optimal covenant minimizes the total expected cost of false negative and false positive outcomes. Based on this model, we predict that the optimal covenant threshold will be decreasing (i.e., tighter) in the unconditional probability of distress and increasing (i.e., looser) in the relative costs of false positives to false negatives. Financial covenants that can better discriminate between healthy and unhealthy borrowers – that is, have a higher signal-to-noise ratio – will result in fewer false positive and false negatives, with an ambiguous impact on total violations.

Guided by our model, we use newly collected data to study how well changes in the relative benefits of restrictive covenants and covenant quality explain the patterns we observe. Our findings suggest that lenders have become more adept through time in managing covenants to reduce costly false positives without considerably increasing the frequency of false negatives. For instance, based on an observable measure of expected financial distress, we show that the ratio of false positive violations to false negatives has declined steadily through time. In this sense, while lenders now rely on fewer financial covenants, the covenants appear to be "better" in terms of reducing false positives relative to false negatives.

We quantify this relation further by estimating how ex-ante covenant quality has changed through time. Motivated by our theoretical model, we construct a measure of covenant tightness in the spirit of Murfin (2012) and Demerjian and Owens (2016), who measure the ex-ante probability that a borrower will violate a covenant, conditional on the borrower's covenant package and distance to covenant thresholds. While we also examine the closeness of borrower financials to covenant thresholds, we are interested in measuring covenant quality in terms of the ex-ante probabilities of true positive violation and true negative violations.

We estimate these probabilities assuming that covenants serve as an early warning signal of financial distress, which we measure as the probability of payment default. To do so, we employ a default prediction model using the actual financial covenants in each of nearly 20,000 loan packages and assess how well the contractual thresholds would catch firms that actually default. The process produces an estimate of the covenant threshold, measured as a probability of default at the contractual thresholds, and an estimate of the rates of false positive and true positives. In this way, we can separate the probability of violation into expected false positives, which are costly, from expected true positives, which are beneficial. Based on these measures, we find that the vast majority of the 1997-2016 decline in loan restrictiveness derives from a reduction in expected false positives, reinforcing the idea that most of the decline in restrictiveness through time comes in the form of a relaxation on non-distressed firms.

Importantly, we demonstrate that a clear shift occurs through time in the *types* of covenants utilized in credit agreements toward covenants with higher signal-to-noise ratios. At the start of the sample period, lenders commonly relied on covenants written on quick ratios, current ratios, debt-to-asset, debt-to-equity, and net worth, which measure firm performance based directly on balance sheet figures.[2] By contrast, as of 2016, the predominant financial covenants benchmark credit performance against borrower EBITDA, a proxy for free cash flow. We find the number of financial covenants has declined over time because lenders have dropped balance sheet covenants and kept only covenants based on EBITDA.[3] Moreover, loans with balance sheet covenants create more expected violations because balance covenants generate significantly more false positive violations. In short, the trend toward cash flow covenants allows lenders to better discriminate between borrowers and substantially reduces the expected number of false positive violations.

We also directly characterize features of the changing covenant mechanisms through time by studying the actions taken by lenders in response to covenant violations, as disclosed in borrower SEC filings. We label a covenant violation as a “foot fault” when lenders waive the violation without taking a more consequential action in response to the violation. Consequential actions include interest rate increases, loan commitment reductions, principal repayment requirements, forced capital raises, asset sales, and demands for more collateral. We view a foot fault as a recognition by a lender that a violation was a false positive, in the sense that upon further monitoring, the lender elects to make no major contractual changes in response to the covenant violation. In 1997, foot-fault violations accounted for almost two-thirds of all new covenant violations. By 2016, foot faults represented fewer closer to one-third of new violations. Consistent with cash-flow covenants being more informative, we find that violations of balance sheet

---

[2] The popularity of balance-sheet based covenants is evident in early studies of financial covenants in loan agreements. See, e.g., Beneish and Press, 1993, 1995; Sweeney, 1994; Defond and Jiambalvo, 1994; Dichev and Skinner, 2002.

[3] These covenants include debt/EBITDA, fixed charge coverage, interest coverage, and a minimum level of EBITDA requirement. While the number of cash-flow based covenants have remained constant over our sample period, there is evidence that the components of EBITDA – a non-GAAP concept whose definition can vary by contract – have changed through time, including by more recent use of forward-looking “add-backs” that incorporate projected future savings and synergies into the EBITDA calculation. See, for example, Badawi and de Fontenay (2019).

covenant are more than 40% more likely to be classified as a foot fault than cash-flow covenant violations.

Overall, we interpret our findings as an indication that lenders have adapted through time to select financial covenants that better optimize on the tradeoffs between false positives and false negatives. Combined, our results suggest that the relative costs of false positives have increased through time, inducing lenders optimally to rely on covenants with higher signal-to-noise ratios and more slack in the thresholds of the covenants.

We conduct a variety of analyses to exclude other explanations for the decline in covenant restrictiveness. We find that the trend cannot be explained by factors such as measurement error, changes over time in the composition of syndicated borrowers, or improvements in the overall credit quality of public firms. Instead, the trend appears to be secular in nature and widespread among all types of borrowers.

Moreover, the patterns we observe cannot be explained by recent developments in the supply side of the corporate loan market, such as the rise of non-bank institutional lenders (Ivashina and Sun, 2011; Lim, Minton, and Weisbach, 2014) and the growing popularity of "covenant-lite" term loans that lack traditional financial covenants. (Billett, Elkamhi, Popov, and Pungaliya, 2016; Becker and Ivashina, 2016). While these developments have received substantial media and regulatory scrutiny, Berlin, Nini, and Yu (2019) show that only the institutional "Term Loan B" tranche of a loan package is covenant-lite; lenders are almost always still protected by traditional financial covenants on the revolving (and Term Loan A) components of a loan. We find that even revolver-only loan packages have shifted toward fewer and more slack covenants, a trend that predates the recent credit boom and extends to all types of loan packages and lenders. We uncover no evidence that leveraged loans, loans marketed to institutional investors, or deals backed by private equity sponsors drive our results.

Our paper contributes to the literature studying the design and renegotiation of debt contracts. Our theoretical contribution builds on prior research, which justifies the existence of financial covenants, by modeling factors that determine the optimal covenant threshold (e.g., Aghion and Bolton, 1992; Berlin and Mester, 1992; Garleanu and Zwiebel, 2009). A novel aspect of our framework is that we show that the optimal covenant threshold varies with the ability of the covenant to discriminate between distressed and non-distressed borrowers, as well as the relative costs associated with screening incorrectly. We use these theoretical predictions to develop a novel method for estimating covenant tightness that extends the work of Murfin (2012) and Demerjian and Owens (2016)

Our empirical contribution is to provide a new measure of covenant quality and document the widespread trend toward fewer financial covenants and fewer realized violations. Our combined evidence suggests that the trend primarily reflects a relative increase in the costs of false positives and a shift toward more efficient covenants.

2 Background on Debt Covenants

Covenants have long been recognized as an important component of lending arrangements. The current study material for the chartered financial analyst exams includes covenants as one of the four "Cs" of credit analysis.[4] Smith and Warner (1979) emphasize that covenants are designed to minimize conflicts of interest between lenders and owners and managers of borrowing companies. Whether these conflicts arise due to differences in preferences, differences in the structure of payoffs, or differences in access to relevant information, covenants help ensure that

[4] The four Cs are capacity, collateral, covenants, and character, according to Fundamentals of Credit Analysis, Christopher L. Gootkind (CFA Institute).

firms do not take actions that are detrimental to lenders. Minimizing conflicts of interest expands the ex-ante supply of credit and allows firms access to more credit and/or lower interest rates.

2.1 Theoretical Background

In the incomplete contracting paradigm of Grossman-Hart-Moore, control rights can be allocated to mitigate two financing frictions: moral hazard and information asymmetry. Aghion and Bolton (1992) show that managerial moral hazard can be minimized via state-contingent control rights. By assigning additional control rights to investors when private benefits are likely to lead to smaller financial returns, state-contingent control can increase the amount of income that can be pledged to support borrowing. This feature resembles a standard bank loan, which contains covenants written on financial ratios and transfers control rights to lenders if performance falls below contractual thresholds.

Garleanu and Zwiebel (2009) show that financial covenants facilitate financing when a manager's propensity to pursue private benefits is unobservable. Assuming asymmetric information over future wealth transfers from creditors, the optimal contract allocates strong decision rights to creditors ex-ante to overcome the adverse selection problem and reallocates control rights via ex-post renegotiation. Covenants thus serve two crucial roles: i) to define the circumstances when creditors receive the right to intervene in management, and ii) to prevent managers from taking privately beneficial actions that may reduce the value of lenders' claims (Tirole, 2010).

2.2 Financial Covenants

The typical credit agreement contains affirmative, negative, and financial covenants. Affirmative and negative covenants minimize incentive conflicts by contracting directly on certain events, such as the purchase of insurance or the distribution of dividends. While these covenants are ubiquitous in public and private debt contracts, their scope is limited by an inability to contract on all possible contingencies (Smith and Warner, 1979). Financial covenants enable creditors to overcome this hurdle by assigning decision rights based on a verifiable signal. Indeed, financial covenants are often referred to as "tripwires" because they transfer control rights to lenders only when financial ratios drop below contractual thresholds (Smith, 1993; Dichev and Skinner, 2002). Due to high monitoring and renegotiation costs of public debt, these covenants are found predominately in private debt contracts.

Roberts and Sufi (2009) show that more than 95% of private loan agreements contain at least one financial covenant. These covenants are tailored to each borrower and do not appear to be set in a boiler-plate fashion. Freudenberg, Imbierowicz, Saunders, and Steffen (2017) find more than 80 unique financial covenant descriptions in a sample of nearly 5,000 credit agreements. Although highly tailored, many covenants share a similar structure. The most common financial covenants place limits on the borrowing company's leverage (typically measured as debt-to-EBITDA), coverage of period cash obligations (fixed charge or interest), liquidity (current or quick ratio), and net worth. Historically, these covenants have been set tightly, with the average covenant threshold set fairly close to the current ratios for the company (Chava and Roberts, 2009).

2.3 Covenant Violations

The breach of a financial covenant constitutes an event of default and grants lenders the right to immediately sever all lending commitments, recall outstanding debt, and proceed to foreclose on collateral. In practice, lenders typically do not initiate default rights upon a violation, preferring instead to use their bargaining power to renegotiate terms of the loan contract. The renegotiation typically addresses two issues. First, the borrower must "cure" the existing violation to return to compliance with the loan contract. This is often achieved with a "waiver," where the lender formally agrees that the borrower is excused from complying with the covenant for the period of the violation. Second, the loan agreement may be adjusted going forward. Because the borrower's credit quality has deteriorated, financial covenant thresholds are sometimes loosened to avoid repeated violations in subsequent periods. The contract may also be adjusted to impose stricter terms on the borrower; reducing the limit on a line of credit and increasing the interest rate are common changes. The borrower may also be required to provide the lender with additional compensation, such as an amendment fee or warrants to purchase the borrower's stock.

A large body of empirical literature shows that this renegotiation process leads to more conservative investment and financial policies. Specifically, covenant violations are associated with a decline in debt issuance (Roberts and Sufi, 2009), capital investment (Chava and Roberts, 2008), R&D expenditure and patent quantity (Chava, Nanda, and Xiao, 2017; Gu, Mao, Tian 2017), employment (Falato and Liang, 2017), and shareholder payouts (Nini, Smith, and Sufi, 2012). Creditors impose these changes via the negotiation process and contractual tightening. Ferreira, Ferreira, and Mariano (2017) provide evidence of behind-the-scenes negotiation by showing that most new independent directors added after a violation have links to creditors. Becher, Griffin, and Nini (2018) show that creditors tighten the restrictive covenant on acquisitions after a financial covenant violation.

In a large fraction of violations, however, the renegotiation following a financial covenant results in very few or no changes to the lending relationship. For example, Chen and Wei (1993) examine 128 examples of covenant violations during the years 1985-1988 and find that 45% of the violations were waived with no additional changes to loan terms. Chodorow-Reich and Falato (2019) study a larger and more recent sample of violations but examine only reductions in the line of credit following a violation. Even during the 2008 financial crisis, only 37% of firms faced a reduction in their credit line following a violation, meaning that the remaining 63% of violators were permitted to continue borrowing at the same level. In our analysis below, we examine a larger set of potential actions following a violation but still find that roughly one-half of violations are settled with no significant changes to the lending relationship.[5]

3 A Conceptual Framework for Financial Covenants

In contrast to the extant literature on loan covenant violations, which typically studies how loan terms and conditions often change following a violation, we highlight here that loan terms are very often left *unchanged*. In such cases, we infer that the lender determined that the borrower's credit quality had not deteriorated sufficiently to impose further conditions on the borrower.

We draw an analogy with medical tests used to diagnose disease. In the event of a positive medical test, further testing is frequently required to confirm a diagnosis and develop a plan for treatment. In some cases, subsequent testing reveals that the patient does not have the disease and does not need treatment. Because medical tests often provide only a probabilistic assessment of disease likelihood, such "false positives" can be quite common. Financial covenants function similarly; they are tests applied periodically to assess the current credit quality of the borrower, and in the case of a violation, prompt a monitoring and renegotiation period during which the lender gathers more information, diagnoses the current credit health of the borrower, and proposes a treatment if needed. In many cases, however, further monitoring reveals that the violation is a false positive and no treatment occurs.

[5] As an example of a violation with no consequences, SRI/Surgical Express Inc. reported a covenant violation in their March 31, 2006 10-Q, which was filed with the SEC on May 9, 2006. In the 10-Q, SRI writes, "The Company's net loss for the first quarter of 2006 resulted in a funds flow coverage ratio of 2.42, which was below the requirement (2.50) of its credit agreement. Both financial institutions issued a waiver of the requirement for the period ended March 31, 2006 and amended the covenant for the balance of 2006." SRI attached the loan amendment to the 10-Q filing, so we can confirm that no other loan terms were changed by the May 8, 2006 amendment that provided the waiver.

If the monitoring and renegotiation following a violation are costly, then there is a tradeoff in the design of financial covenants.[6] Tight covenants catch a large fraction of borrowers whose credit quality has deteriorated sufficiently to warrant intervention by creditors. But tight covenants are also likely to catch a large fraction of borrowers that, upon further monitoring, are revealed to be relatively healthy and require no further action other than a waiver. The optimal contract balances these considerations, and moreover, lenders will have an incentive to design financial covenants that detect better when firms are truly financially distressed. We explore these tradeoffs more formally in the next section.

3.1 Financial Covenant Thresholds: A Model

We assume that the population of borrowers contains both distressed and non-distressed borrowers, which we separate by the variable $\widetilde{D} = \{D, ND\}$.[7] The true status of a firm is unobservable, but the lender can write financial covenants on a set of accounting values that serve as a diagnostic screening test. We denote the occurrence of a violation by the indicator variable $\tilde{V} = \{0,1\}$.

The accounting metrics are imperfect indicators of distress, so two types of mistakes will occur. First, the covenant can fail to catch a distressed firm, which is the probability of a false negative, Pr $(V = 0|D = 1)$. Second, the covenant can catch a borrower that is not truly distressed, which is the probability of false positives, Pr $(V = 1|D = 0)$. We denote the cost of each of these mistakes as C_{FN} and C_{FP}.

[6] We take the existence of financial covenants as prima facie evidence of the costs of renegotiation. Instead of financial covenants that trigger renegotiation only occasionally, loan agreements could have very short maturities, which would trigger very frequent renegotiation (see, e.g., Berglöf and von Thadden, 1994). We take the ubiquity of longer maturities and financial covenants as support for our assumption that renegotiation costs are important.

[7] From a lender's vantage, a borrower is financially distressed when there is a high probability that the borrower will not meet its expected interest or principal payments.

Using the notation of Murfin (2012), we denote the performance metric used in the financial covenant as r and the contractual threshold as $\bar{r}$, with the convention that a violation occurs if $r > \bar{r}$. We assume that distressed and non-distressed firms have (potentially) different distributions over r, so the probabilities of false negatives and positives are given by $F_D(\bar{r})$ and $[1 - F_{ND}(\bar{r})]$, where $F_D(\cdot)$, $F_{ND}(\cdot)$ are the distribution functions for the accounting variables for distressed and non-distressed firms, respectively. Since $F_D(\bar{r})$ is increasing in $\bar{r}$ and $[1 - F_{ND}(\bar{r})]$ is decreasing in $\bar{r}$, there is a trade-off in setting the covenant threshold; tighter covenants result in fewer false negatives but more false positives.

We denote the expected false negative and positive frequencies as $FN(\bar{r})$ and $FP(\bar{r})$ and assume that covenants are set to minimize the total expected costs

$$(1-\rho)FP(\bar{r})C_{FP} + \rho FN(\bar{r})C_{FN} \,,$$

where ρ is the unconditional probability that a firm is distressed. The first-order condition for the minimization problem yields an intuitive equation that determines the optimal threshold:

$$\frac{(1-\rho)}{\rho}\frac{C_{FP}}{C_{FN}} = -\frac{FN'(r^*)}{FP'(r^*)} = \frac{f_D(r^*)}{f_{ND}(r^*)} \,, \tag{1}$$

where $f_D(\cdot)$ and $f_{ND}(\cdot)$ are the density functions corresponding to $F_D(\cdot)$ and $F_{ND}(\cdot)$. The left-hand side of (1) is the ratio of expected costs of false positives to false negatives; we expect $\frac{C_{FP}}{C_{FN}}$ to be much less than 1 and $\frac{(1-\rho)}{\rho}$ to be much larger than 1. The right-hand side of (1) is a likelihood ratio for the relative probabilities of violation for a distressed and non-distressed borrower, which we denote as $L(r)$.

Equation (1) identifies three important factors that determine the optimal covenant threshold. The first two are straightforward. First, the optimal threshold is decreasing in the unconditional probability of distress, ρ. As the likelihood of financial distress declines, increasing the threshold reduces the frequency of false positives without significantly increasing the rate of false negatives. Second, the optimal threshold is increasing in the relative costs of false positives to false negatives, $C \equiv \frac{c_{FP}}{c_{FN}}$. As the costs of false positives rise relative to the cost of false negatives, increasing the threshold reduces the number of more costly false positives.

With this framework, the probability of a covenant violation is given by:

$$\Pr(V = 1) = (1 - \rho)FP(r^*) + \rho TP(r^*) = (1 - \rho)FP(r^*) + \rho[1 - FN(r^*)] \,. \tag{2}$$

Differentiating (2) with respect to C confirms that an increase in the relative costs of false positives to false negatives will result in a lower rate of covenant violations. Differentiating (2) with respect to ρ shows that an increase in the fraction of distressed firms, ρ, will result in a lower rate of covenant violations as long as the true positive rate is less than the false positive rate, $TP(r^*) > FP(r^*)$, which is a reasonable assumption for standard financial covenants.[8] We state these two conclusions as hypotheses to explain the decrease in violation frequency over time.

H1. The frequency of covenant violations is lower when the underlying fraction of distressed firms is lower.

H2. The frequency of covenant violations is lower when the relative cost of false positive violations to false negative violations is higher.

The third important factor determining the optimal covenant threshold is the ability of the performance metrics to discriminate between distressed and non-distressed firms, as summarized by $L(r)$. $L(r)$ is typically increasing in r, meaning that a larger value of r raises the likelihood

[8] If the false positive rate is above the true positive rate, then an increase in the fraction of distressed firms may result in a lower rate of violations despite the decrease in the optimal threshold. This can happen because the reduction in the fraction of non-distressed firms, which are violating at a higher rate.

that the borrower is distressed rather than not. This is a natural consequence of the metrics used as financial covenants. The unique optimal threshold corresponds to $L(r^*) = \frac{(1-\rho)}{\rho}\frac{C_{FP}}{C_{FN}}$.

A change in the ability of financial covenants to screen different borrowers corresponds to a change in the underlying distribution functions, $F_D(\cdot)$ and $F_{ND}(\cdot)$, which will change the shape of the likelihood function $L(r^*)$. The impact on the optimal covenant threshold is ambiguous, as we illustrate with the following example.

3.2 Optimal Threshold Example

Suppose that the performance metric is normally distributed with a mean that depends on the underlying status of the borrower; $r \sim N(\mu_D, \sigma)$ for distressed firms and $r \sim N(\mu_{ND}, \sigma)$ for non-distressed firms. Under this assumption, the likelihood ratio is given by

$$e^{\frac{1}{2\sigma^2}[(2r-\mu_D-\mu_{ND})(\mu_D-\mu_{ND})]}$$

Denoting the left-hand side of (1) as $R \equiv \frac{(1-\rho)}{\rho}\frac{C_{FP}}{C_{FN}}$, the optimal threshold is given by

$$r^* = \frac{2\sigma^2 \ln(R) + \mu_D^2 - \mu_{ND}^2}{2(\mu_D - \mu_{ND})} \tag{3}$$

If we assume that $\mu_D = \mu + \frac{1}{2}\varepsilon$ and $\mu_{ND} = \mu - \frac{1}{2}\varepsilon$, then (3) simplifies to $r^* = \frac{\sigma^2 \ln(R)}{\varepsilon} + \mu$. In this case, an increase in ε corresponds to a metric that is better able to separate distressed and non-distressed borrowers. The slope of the threshold with respect to ε, $\frac{dr^*}{d\varepsilon}$, depends on the sign of $\ln(R)$. When $R < 1$, which corresponds to high expected costs of false negatives, $(1-\rho)C_{FP} < \rho C_{FN}$, the optimal threshold increases when the test becomes better. In such a case, it is possible to keep the false negative rate low while increasing the threshold to minimize false positives. When $R > 1$, the optimal threshold decreases when the test becomes better, since it is possible to keep the false positive rate low while decreasing the threshold to increase the rate of true positives.

The impact of a change in test quality on the violation frequency is also ambiguous. Differentiating (2) with respect to ε yields

$$\frac{d\,\Pr(V=1)}{d\varepsilon} = (1-\rho)\left[FP^{'}(r^*)\frac{dr^*}{d\varepsilon} + \frac{dFP(r^*)}{d\varepsilon}\right] - \rho\left[FN^{'}(r^*)\frac{dr^*}{d\varepsilon} + \frac{dFN(r^*)}{d\varepsilon}\right]$$

Substituting (1) and simplifying yields:

$$\frac{d\,\Pr(V=1)}{d\varepsilon} = \frac{dr^*}{d\varepsilon}(1-\rho)FP^{'}(r^*)(1+\frac{C_{FP}}{C_{FN}}) + \left[(1-\rho)\frac{dFP(r^*)}{d\varepsilon} - \rho\frac{dFN(r^*)}{d\varepsilon}\right]. \quad (4)$$

The right part of (4) represents the direct impact of changing the test; a better test will have fewer false positives and false negatives, so both $\frac{dFP(r^*)}{d\varepsilon}$ and $\frac{dFN(r^*)}{d\varepsilon}$ will be negative. Reducing the false positive rate will lead to fewer violations, but a smaller false negative rate will increase the number of violations; the net effect depends on the shape of the distributions and the size of ρ and will be negative if $(1-\rho)\frac{dFP(r^*)}{d\varepsilon} < \rho\frac{dFN(r^*)}{d\varepsilon}$. In the case of normally distribute tests, this simplifies to the condition that $L(r^*) < \frac{1-\rho}{\rho}$. given the condition (1), this will be true as long as $C_{FN} > C_{FP}$, which again is a reasonable assumption for covenant violations.

The left part of (4) captures the endogenous response of the threshold to the change in the underlying test. Since $FP^{'}(r^*) < 0$, the sign of this term is determined by the sign of $\frac{dr^*}{d\varepsilon}$. In the case when $\frac{dr^*}{d\varepsilon} > 0$, which happens when $R < 1$ in this example, a better test results in a higher threshold, which further reduces the probability of violation. $R < 1$ corresponds to the condition that $(1-\rho)C_{FP} < \rho C_{FN}$, which implies that $C_{FN} > C_{FP}$. So a sufficient condition for an improvement in test quality is that the unconditional expected costs of false negatives exceed the expected costs of false positives, which we state as our third hypothesis.

H3. If the unconditional expected costs of false negatives exceeds the expected costs of false positives, $R = \frac{(1-\rho)C_{FP}}{\rho C_{FN}} < 1$, then the frequency of covenant violations is lower when the underlying test becomes better.

We end by noting that, in the case when the optimal threshold decreases with ε, which happens when $R > 1$, the endogenous threshold response tends to increase the expected number of violations. However, the overall expected number of violations may still decrease if the term $\left[(1-\rho)\frac{dFP(r^*)}{d\varepsilon} - \rho\frac{dFN(r^*)}{d\varepsilon}\right]$ is sufficiently negative. In this case, covenant thresholds will appear "tighter" as the test improves, but the overall number of violations may still decrease because of a sharp drop in the expected number of false positives.

4 Sample Selection and Summary Statistics

We conduct our analyses using two datasets. First, we collect loan-level data to study the evolution of debt contracts over the sample period, 1997-2016. Second, we assemble firm-year data to analyze covenant violations over the same period. The following sections describe the construction of these datasets and provide summary statistics.

4.1 Loan Sample

We begin by extending to 2016 the public company dataset used by Nini, Smith, and Sufi (2012) over the years 1997-2008. We examine all nonfinancial U.S. firm-quarter observations in Compustat that can be matched to a corresponding 10-Q or 10-K SEC filing in EDGAR.[9] To facilitate a match and ensure consistency, we employ the same filters as Nini, Smith, and Sufi (2012). Specifically, we require non-missing total assets, total sales, common shares outstanding,

[9] We begin with this sample to ensure consistency with the covenant violation sample.

closing share price, and calendar quarter of the observation, and drop firms with average book assets of less than $10 million in real 2000 dollars. These filters yield a sample of 288,390 firm-quarter observations that we use to construct the variables, as described in Appendix 1.

To construct the loan sample, we begin with our filtered sample of 288,390 firm-quarter observations and merge in all loan packages from Dealscan using the Chava and Roberts (2008) link file. Since this file ends in 2012, we use a fuzzy name match and extensive hand-checking to update the link table through 2016. For each package, we use accounting data from the most recent quarter-end after origination, requiring the fiscal quarter-end date to be less than 100 days after the loan origination date. This matching process leaves us with a sample of 20,189 loan packages originated between 1997 and 2016. We then remove packages without a senior bank loan that is syndicated in the U.S. and denominated in dollars. After dropping packages without completed status, we are left with a sample of 18,131 loans from 4,771 firms between 1997 and 2016.

Dealscan provides loan information at both the package and facility level. Packages (deals) contain one or more facilities (tranches) that are governed by the same credit agreement. Our unit of analysis is the loan package because covenants are typically set at the package level and apply to all facilities in the loan agreement. However, pricing, maturity, and other loan characteristics are only available at the facility level. We aggregate these variables to the deal level by using the mean spread, maximum maturity, and constructing indicators that equal one if at least one of the underlying facilities are secured or have performance pricing. Panel A of Table 1 provides descriptive statistics for the loan sample. Overall, our summary statistics are similar to those reported in the prior literature.

Dealscan provides data on financial covenants that we aggregate and measure according to the definitions provided in Demerjian and Owens (2016). Figure 1 shows the time series of the mean number of covenants and a measure of the tightest covenant level, computed as the number of standard deviations between the contractual threshold and the borrower's level at origination. The figure shows that the average number of covenants decreased from nearly three covenants at the beginning of the sample to less than two covenants in recent years. Concomitantly, the tightest covenant remaining in the package has become looser over time.

4.2 Covenant Violation Data

To construct a comprehensive sample of covenant violations by U.S. public corporations, we rely on information reported in quarterly financial statements. Regulation S-X requires "any breach of covenant …, which … existed at the date of the most recent balance sheet being filed and which has not been subsequently cured, [to] be stated in the notes to the financial statements" (CFR § 210.4-08). Further, "[i]f a default or breach exists but … has been waived for a stated period of time beyond the date of the most recent balance sheet being filed, …" Regulation S-X requires the firm to "… state the amount of the obligation and the period of the waiver" (CFR § 210.4-08). Due to this regulation, we can identify all covenant violations regardless of whether they are outstanding or were cured by a waiver.

Nini, Smith, and Sufi (2012) collect reported violations from nearly the universe of 10-K/10-Q filings on EDGAR from 1996 to 2008 using a text-search algorithm and manual inspection.[10] This dataset, provided online by the authors, indicates whether a firm reports a violation in the SEC filing associated with each fiscal quarter.[11] We extend this dataset through 2016 using the same text-search algorithm and manual coding procedure.

[10] The Securities and Exchange Commission (SEC) did not require electronic filing for all firms until the second quarter of 1996.

[11] See the data appendix in Nini et al. (2012) for details on the sample selection and text-search algorithm.

In order to minimize problems from seasonality and measurement error, we aggregate the quarterly data to the firm-year level.[12] We create an annual violation indicator for each firm-year that denotes whether the firm reported a violation during the any of the four quarters of the year. We use the fourth calendar quarter of each firm-year, so that each firm-year observation is measured at the same point in time. We also aggregate the quarterly Compustat variables and drop some observations with missing firm variables. The resulting sample consists of 66,589 firm-year observations from 8,499 firms between 1997 and 2016. Panel B of Table 1 provides descriptive statistics for the firm-year sample. Overall, our summary statistics are similar to those reported in the prior literature.

Column 3 of Table 1 displays the number of firms that reported a covenant violation during each calendar year of our sample. The number of violators falls dramatically from a peak of 758 in 2001 to only 122 in 2016. Figure 2 plots the time series of the frequency that firms report a covenant violation. The figure shows that the frequency of violations has decreased substantially over time. Since the recent financial crisis, the rate of violation has averaged about 6% per year, which is one-half the rate of the lowest year prior to 2005.

To ensure that the downward trend shown in Table 1 and Figure 2 is not due to biases in our data, we consider two alternative measures of violations. First, we examine the Roberts and Sufi (2009) covenant violation dataset provided online by Michael Roberts. The Roberts and Sufi (2009) text-search algorithm examines a larger set of SEC filings but uses a smaller set of search terms. On net, the procedure identifies fewer violations. Column (2) displays an even stronger downward trend through 2011, which is when the dataset ends. The similarity between columns

[12] As reported in Nini, Sufi, and Smith (2012), firms report violations more frequently in 10-Ks relative to 10-Qs because firms often summarize the experience of the entire year in annual reports. Moreover, aggregating to the firm-year minimizes the likelihood that our coding procedure fails to identify a violation, since we would have to miss for four consecutive quarters.

(1) and (2) confirm that the trend is not unique to our hand-collection procedure. However, since both measures use violations reported in financial statements, it could be the case that the trend is due to changes in reporting behavior rather than changes in violation frequency.

To rule out this alternative explanation, we use the methodology of Chava and Roberts (2008) to impute violations from observed accounting ratios and covenant thresholds. For each firm-quarter with a loan outstanding in Dealscan, we determine violation status by observing whether the current ratio, total net worth, or tangible net worth observed in Compustat falls below the contractual threshold in Dealscan.[13] The trend in column (3) confirms that the decline in covenant violations is not driven by changes in reporting. In fact, the number of firms that violate one of the three covenants examined by Chava and Roberts (2008) drops to single digits in the latter part of the sample.[14]

5 The time trend in covenant strictness

To explore in more depth the mechanisms behind the drop through time in covenant violations, we define and construct a new measure of covenant strictness.

5.1 Measuring covenant strictness

Motivated by the model in Section 3, two natural measures of the quality of a financial covenant are the probability that the covenant generates a true positive $\Pr(V = 1 | D = 1)$ and the

[13] Following Chava and Roberts (2008), we linearly interpolate dynamic covenant thresholds, drop loans that appear to be in violation at origination, and, in the case of overlapping loans, define the relevant package to be the tighter of the two unless the latter deal corresponds to a refinancing.

[14] In percentage terms, the decline in violations is larger in columns (2) and (3) than in column (1). Comparing total violations in 2001 and 2002 with the total in 2010 and 2011, the drop is 74% in column (1) and 86% in column (2). Comparing total violations in 2001 and 2002 with the total in 2015 and 2016, the drop is 81% in column (1) and 92% in column (3).

probability that the covenant creates a false positive $\Pr(V = 1|D = 0)$.[15] A better quality covenant will have higher true positive rate and a lower false positive rate. We begin by constructing empirical measures of these quantities using the observed covenant packages in Dealscan and the historical distribution of defaults in the sample of firms in Compustat.

As in Murfin (2012), our measures require that we form a comparable estimate of the covenant threshold $\bar{r}$ across different covenant packages. To do so, we estimate a default probability as a function of the set of accounting variables provided by the covenants in a loan package. The fitted default probability provides an index of covenant "restrictiveness" that also has the natural economic interpretation of an estimated default probability measured at the covenant thresholds in the loan package. Specifically, for each loan l with covenant package r_l, we estimate a logistic model of default as in Campbell, Hilscher, and Szilagyi (2008):

$$\Pr(Default_{i.t+1}) = \frac{1}{1+\exp\left(-\alpha-\beta_l r_{i,t}\right)} \tag{5}$$

using the full sample of firm-years in Compustat, denoted by *i* and *t*. We measure distress using data on corporate bankruptcies gathered from Compustat, CRSP, the UCLA-LoPucki Bankruptcy Database, and Audit Analytics. We use accounting ratios measured as of year *t* to forecast bankruptcy over the subsequent year $(t + 1)$. The explanatory variables include only the accounting ratios used in the covenant package r_l, so the estimated model varies across loans.

With the loan-specific estimates $\widehat{\beta_l}$, we calculate a violation threshold as the fitted probability of default at the contractual accounting thresholds:

[15] Murfin (2012) measures the strictness of a financial covenant as the ex-ante probability of violation. In our framework, this measure includes the probability of both true positives and false positives, $\Pr(V = 1) = \rho\Pr(V = 1|D = 1) + (1 - \rho)\Pr(V = 1|D = 0)$.

$$Threshold_l = \Pr(\widehat{Default_l}) = \frac{1}{1 + \exp\,(-\hat{\alpha} - \widehat{\beta_l} r_l)}$$

An estimate of the violation threshold can be computed for each sample loan and provides an easily interpretable measure that can be compared across loans. As in Section 3, a higher threshold corresponds to a higher value of $\bar{r}$ and a less restrictive covenant package.

We next measure the expected rate of true positives and true negatives based on the full set of firms in Compustat, which serves as the empirical distribution from which compute expectations. For each firm-year, we compute the fitted default probability using the coefficient estimates and denote the fitted probability $PD_{i,t} = \frac{1}{1+\exp\,(-\hat{\alpha}-\widehat{\beta_l} r_{i,t})}$. We estimate the probability of a violation as the fraction of firm-years with $PD_{i,t}$ larger than the estimated threshold, $Threshold_l$. We label firm *i* as a "true positive" if $PD_{i,t} > Threshold_l$ and the firm defaults at *t+1* ($Default_{i.t+1} = 1$), and a "false positive" if $PD_{i,t} > Threshold_l$ and the firms does not default at *t+1* ($Default_{i.t+1} = 0$). Following this procedure across the entire set of Compustat firms generates a large sample of firms that can be categorized through time into one of four groups as follows:

	$Default = 0$	$Default = 1$
$PD < Threshold$	True Negative	False Negative
$PD > Threshold$	False Positive	True Positive

For each loan, we compute the empirical frequency of each outcome to form our measures of expected rates of true and false positives.

Our method is inspired by the approach of Demerjian and Owens (2016), who estimate the Murfin (2012) covenants strictness measure, $\Pr(\tilde{r} > \bar{r})$, by simulating from the empirical distribution of realizations of $\tilde{r}$ among Compustat firms. We modify the logic of Murfin (2012) along two dimensions. First, unlike Murfin (2016), we assess the probability of violation using firm data at the time the covenant test would be conducted rather than at the time the loan is originated. Second, we separate the probability of violation into true positives and true negatives. We use realized defaults as our measure of the true underlying status of the firm, but the method can be modified to accommodate other choices.

We also compute the Murfin (2012) measure of covenant strictness, $\Pr(V = 1)$, using the approach followed by Demerjian and Owens (2016). The probability of violation is estimated based on the borrower's covenant package, the borrower's current level of accounting variables, and an estimate of the probability distribution for changes in the relevant accounting variables.[16] The probability of violation increases with the number and tightness of covenants included in a loan agreement, adjusted for the variance and covariance of the corresponding accounting ratios. Over our sample, the average loan contains 2.5 covenants with the tightest threshold 0.60 standard deviations away from the corresponding accounting ratio at origination.

5.2 Trends in covenant quality

Since our method for assessing covenant quality requires estimating a logit model for the probability of default, we limit the sample to covenant packages using common sets of ratios. We drop any covenant packages used in fewer than 100 packages, which leaves the remaining sample with nearly 50 unique packages covering more than three-quarters of the total sample. The most common package includes a restriction on debt-to-EBITDA and a fixed charge coverage ratio; this package is used in about 15% of deals.

[16] Murfin (2012) and Demerjian and Owens (2016) differ in how they estimate the probability of violation. Murfin (2012) assumes a multivariate normal cumulative distribution and estimates the covariance matrix associated with quarterly changes in accounting ratios. Demerjian and Owens (2016) simulate from the empirical distribution of quarterly changes in accounting ratios. The two approaches produce similar estimates, so we present only the Demerjian and Owens (2016) measure because the non-parametric estimation is calculable for a larger sample of loans. We thank Justin Murfin and Edward Owens for sharing their code to construct these estimates.

Table 2 provides some summary statistics for our measures of covenant quality. Across our full sample, the mean estimated threshold probability of default is 3.6%, which corresponds to about the 75th percentile of the estimated probability of default, based on estimating the probability of default using a full set of accounting variables. The estimated thresholds are positively skewed, so the expected rate of violations exceeds 25%; the average total positive rate is 39%. For comparison, the last column provides that estimated probability of default from Demerjian and Owens (2016).

Table 2 also shows the split by number of covenants in the package. As expected, increasing the number of covenants from one to two increases the rate of violations. The true positive rate increases from 63% to 73%. The "cost" of this increase is a small increase in the false positive rate from 31% to 33%. However, adding a third covenant increases the true positive rate only from 73% to 80%, while the false positive rate jumps from 33% to 47%, an increase that is nearly double the increase in the true positive rate. This simple exercise highlights how increasing covenant restrictiveness by adding an additional covenant to a package influences the tradeoff between true positives and false positives. Our results indicate that after including two covenants, the benefit versus cost of adding an additional covenant swings disproportionately towards false positives.

Figure 3 reports the time series average of our measures of covenant quality. Panel A reports the annual mean level of the estimated threshold. Consistent with a decline in covenant restrictiveness through time, the mean fitted default probability at observed covenant thresholds increases over the sample period from slightly below 3.5% to slightly above 4.0%. If we estimate the probability of default using the full set of covenant variables, this is an increase from about the 75th percentile to the 80th percentile of the distribution of estimated default probabilities.

Absent any changes in the types of ratios used in covenant packages, we would expect about five percentage points fewer violations. Panel B, however, shows that the expected number of violations, termed "positive rate" in the graph, falls considerably more, closer to 20 percentage points. Panel B also reports the mean probability of violation based on the Demerjian and Owens (2016) method, which shows a very similar time series trend.

Panel C helps explain why the decrease in expected violations is considerably larger than what would be expected based on the change in thresholds shown in Panel A. The figure shows the annual mean expected rate of false positives and false negatives, based on the historical distribution of realized defaults. The figure shows a marked drop in false positives and only a slight drop in true positives. If we compare the years 2010-2016 with the period 1997-2002, the expected probability of violation falls by about 17 percentage points. The decrease in expected false positives is of a similar magnitude, but the decrease in expected true positives is only seven percentage points. Such a differential is possible only if the financial ratios underlying covenants changed over time to better allow lenders to discriminate between borrowers. We provide further evidence of this channel in the next section.

5.3 Trends in covenant types

Figure 4 compares the annual fraction of loans containing a financial covenant that uses only balance sheet figures with loans that contain a covenant constructed using a measure of EBITDA. Specifically, we classify covenants as “cash flow” if they incorporate EBITDA and “balance sheet” otherwise (Note that a loan covenant package can include both balance sheet and cash flow covenants, so the figure reflects that double counting). Across the whole sample, the most common covenant package includes a limit on debt-to-EBITDA (often termed a “leverage ratio” in the credit agreements) and a minimum “fixed charge” coverage ratio, both of which are cash-flow covenants.[17] Debt-to-assets is the most common balance sheet covenant, but restrictions on net worth (or tangible net worth) are also quite common.

Figure 4 shows a marked change over time in the types of accounting variables used as financial covenants. At the beginning of our sample, balance sheet covenants are more common than cash flow covenants, and roughly two-thirds of loans contain at least one balance sheet item. By the end of the sample, however, only one-quarter of loans have a balance sheet related covenant. Conversely, more than 70% of loans contain a cash-flow based covenant.

Table 3 shows that the movement to cash-flow covenants has important implications for the quality of financial covenants. The table summarizes our measure of covenant quality for the subset of loans with a balance sheet covenant and with a cash flow covenant. The table shows that balance sheet covenants are set with a much higher threshold than cash flow covenants and have significantly more expected violations, because the rate of false positives is much higher. Indeed, the table suggests that cash flow based covenants dominate balance sheet covenants in terms of quality because cash flow covenants catch more true positives and fewer false positives than balance sheet covenants. We infer that the trend over time shown in figure 4 represents a rational move towards financial covenants that are better able to identify truly distressed firms.

Combining inferences from Tables 2 and 3, it is straightforward to see why the number of covenants could decline over time without consequential downside consequences (i.e., in terms of more false negatives). By switching from balance sheet to cash flow covenant and lender can rely

[17] A fixed charge ratio measures the ability for a company’s per period EBITDA, often less cash taxes and capital expenditures, to cover “fixed charges” such as interest, required principal payments, and rent payments.

on fewer but higher quality covenants to balance the tradeoff between false positives and false negatives.

6 Trends in reported violations

We further explore the role of covenants to discriminate between distressed and non-distressed firms by examining the nature of firms that actually violate a financial covenant. The downward trend in reported covenant violations indicates that lenders are either failing to catch truly distressed borrowers (i.e., more false negatives), reducing unnecessary violations by firms that are in relatively good health (i.e., fewer false positives), or a combination of these factors. While a decline in false positives could be caused by a general loosening of covenant restrictions without any concomitant improvement in covenant quality, a decline in false positives relative to false negatives should indicate an overall improvement in the quality of the covenants being put to use.

6.1 Realized false positives and false negatives

We begin by examining directly how the decline in covenant violations over time impacted the ability of lenders to detect financial distress. The extant literature on covenant violations has shown that covenants can act as "tripwires" that allow lenders to catch distress early and turn firm performance around before default. In this section, we examine how the decline trend in covenant violations has impacted this function of covenants.

Similar to Section 5, we analyze the likelihood of distress conditional on the occurrence of a violation, but now study *realized* violations rather than expectations of violations based on contractual thresholds. We begin by estimating the probability of default at the time of a violation to form an observable proxy for distress that classifies violations as true or false positives. To estimate default probabilities, we employ a Merton (1974) distance-to-default model, following the methodology in Bharath and Shumway (2008). For each firm-year in our Compustat panel, we estimate the probability of default and classify firms as distressed if they are in the top decile of default probability, using the full distribution of firm-years.

Figure 5 summarizes the time series of the distribution of estimated default probabilities for covenant violators. The figure shows the 10th, 50th, and 90th percentiles of the distribution each year. Figure 5 shows that the distribution of default probabilities at violation has remained quite stable over time even as covenant violations have declined. If we interpret the probability of default at violation as a composite measure of covenant thresholds, with higher default probabilities indicating less restrictive thresholds, then Figure 5 indicates firms violating covenants appear to be no more distressed than firms that violated in years prior, consistent with very little change in the underlying threshold for violation. This holds true at all percentiles of the distribution, including the most distressed violators.

Figure 6 uses the realized violations and estimates of distress to measure aggregate false positives and false negatives through time. For this figure, we classify a firm as distressed if it is in the 90th percentile of the distribution of Bharath-Shumway default probabilities and non-distressed otherwise, and plot the annual fraction of non-distressed firms that violate a covenant – a measure of total false positives violations – and the annual fraction of distressed firms that do not violate a covenant – a measure of total false negatives. The figure shows that false positive misclassifications have fallen by half over the sample period while false negatives show no discernible increase over time. The evidence in Figure 6 appears most consistent with loan packages containing cash flow covenants and a perhaps a slightly higher threshold. Most importantly, we see no evidence of a large increase in the fraction of distressed firms escaping the monitoring of lenders as violations have decreased through time.

6.2 The role of foot faults

As a final measure of the ability of covenants to discriminate between distressed and non-distressed firms, we examine the actions that lenders take in response to a covenant violation. Continuing with the medical test analogy, if covenant quality has improved through time, we should expect to see a greater proportion of "treatments" – that is, consequential actions taken by lenders following a covenant violation – and a decline in the frequency of lenders simply waving the covenant violation because of a false positive.

Following the breach of a covenant threshold, loan parties must agree on how to "cure" the violation. As an event of default, a covenant violation grants lenders a right to stop lending commitments, demand immediate repayment of the loan, and foreclose on collateral if the loan is secured. These rights provide the lender with substantial bargaining power to impose conditions on the borrower (i.e., "treat the disease") that mitigate risks to the lender's claim. These conditions can include an increase in the interest rate, a reduction to the loan commitment, partial repayment of principal, and a requirement for an asset sale or raising of additional capital. Alternatively, lenders can simply waive the violation with no additional consequences. When a lender waives a violation without taking further consequential action, we refer to the violation as a "foot fault."

In order to examine borrower consequences following a violation, we hand-collect data on lender actions by reading 10-K/Q's for a sample of 1,755 new covenant violations by 1,452 firms over the 1997-2016 sample period. We form this sample as the intersection of firms in our Compustat panel and Dealscan samples, which reduces the cost of collecting the data by limiting the sample size and concentrates the sample on firms most likely to have loans with covenants.

For each instance of a "new" violation – meaning that the firm was not in violation during the previous four quarters – we read the corresponding SEC filing, including attached exhibits if necessary, to determine the resolution of the violation. We code whether the violation resulted in an amended loan contract that did any one of the following: (i) raised the interest rate, (ii) reduced the loan commitment, (iii) required repayment of outstanding loan balances, and/or (iv) forced an asset or capital raising. Violations not requiring any of these actions are deemed a foot fault.[18]

Across our full sample, we find that 55% of new violations are foot faults, implying that more than a majority of violations in the sample are resolved with very little adverse consequence to the borrower. We interpret foot faults as direct indications by the lender that the violation was a false positive because the lender chooses “no treatment” in the wake of the violation. Panel A of Table 4 provides summary statistics for borrower characteristics split by whether the violation is classified as a foot fault or not. The statistics show a clear pattern that lenders condition their negotiation based on the riskiness of the borrower. Borrowers facing more severe consequences following a violation have lower cash flow, higher leverage, less liquidity, and a lower market value.

Figure 7a shows the time series of new violations and new violations excluding foot faults. In this figure, the plot that excludes foot faults (i.e., includes only consequential violations) corresponds to the frequency of true positive violations. The plot shows that once we remove foot faults, the time series of consequential violations shows virtually no downward trend over time; in other words, while violations have dropped substantially through time, the frequency of true positives have declined by a much smaller amount. To see this different, Figure 7b plots the proportion of foot fault to total new violations (using two-year averages to reduce variability) and shows that the frequency of foot faults declined from nearly 60% of violations prior to 2003 to less than 40% of violations since 2010. Given the previous results that violators do not seem to be more distressed

[18] The range of actions that lenders can take following a violation is quite broad. However, in our reading of SEC filing for covenant violators, we observed only very few cases in which a reasonable reading of the outcome would suggest that the firm faced adverse consequences, but the firm faced none of the four outcomes we code. Our claim is not that these four are an exhaustive list of consequences but rather the lack of one of these is a very good indicator that the firm faced no other consequences. For this reason, we believe that our classification of foot faults is measured with very little error.

upon violation, the most likely explanation is that covenants are screening borrowers better for true distress, so a smaller fraction of violations in more recent periods are foot faults.

We bolster this conclusion by examining the rate of foot faults by type of covenant. Panel B of Table 4 reports that 68% of violations of balance sheet covenants are classified as foot faults compared to only 48% of violations of cash flow covenants. As shown in Figure 4, the observed decline in covenant restrictiveness derives almost entirely from a switch away from balance sheet covenants – a trend which begins after the credit crises of 2002 and accelerates after 2008. Combined with the evidence in Table 4, it seems that loan parties have adapted to higher quality covenants that reduce the incidence of false positives without large increases in false negatives.

7 Alternative explanations

In this final section, we consider alternative explanations for the observed pattern in covenant restrictiveness through time. None of the alternative explanations can account for the time trends and we include this section for robustness.

7.1 The role of realized outcomes

We first investigate whether the downward trend in violations could be explained by changes through time in borrower characteristics. Specifically, we address whether covenant violations have declined simply because firms have performed better through time. To do this, we estimate a violation probability model

$$\Pr\left(Violation_{i,t}\right) = f\left(CovenantControls_{i,t}\right) \tag{6}$$

where i denotes a firm, t denotes a year, $Violation_{i,t}$ is an indicator of a reported violation, and $Covenant\ Controls_{i,t}$ includes these four performance ratios: return on assets

(EBITDA/assets), market leverage (total debt/market value of assets), interest expense (interest expense/assets), and a current ratio (current assets/current liabilities).

We estimate the parameters in (6) using data through 2003 and then, based on the 1997-2003 parameter estimates, forecast realized violations for the full sample period. If the model parameters are stable across time, the forecasts of realized violations should closely match actual realized violations. The results are summarized in Figure 8, which reports actual violations and the sample average predicted probability of a violation based on the logit model and the estimated parameters from column (2) in Table 5. The figure shows that the model fits quite well during the estimation period, capturing the increase in violations around the recession in the early 2000s, despite no year indicators in the model. Subsequent to the estimation period, however, predicted violations remain close to 15% per year and rise and fall around the time of financial crisis. The dramatic difference between predicted violations and realized violations suggests that the fall in realized violations is not due to any trend in the firm-level performance variables.

Instead, the fall in realized violations is best explained by a change in the model that relates firm outcomes to violations. Columns (2) and (3) in Table 4 reported estimated marginal effects of a violation based on (1) for the sample during years 2004-2010 and 2011-2016, which are the latter two-thirds of the full period. For each of the variables, the estimated marginal effects move closer to zero, in some cases dramatically so. The most notable change is for interest expense, which has a statistically insignificant relationship with violations during the last part of our sample.

Column (1) of Table 4 presents estimates of model (1) that include a full set of year dummy variables, in addition to *CovenantControls*. The estimated parameters, along with confidence intervals, are plotted in the bottom panel of Figure 8. The pattern mimics the pattern of reported violations, again suggesting that the fall in realized violations cannot be explained by changes in firm outcomes.

A final inference from Table 4 is that the trend downward in violations seems to happen prior to the years 2011-2016. During the last third of our sample, we cannot reject the hypothesis that the year dummy variables are equal, as reported in the bottom row of Table 4. Whatever the underlying cause for the change in the model, it began prior to the 2011-2016 period.

7.2 Changes in sample composition

We next ask whether the downward trend in realized violations could be due to changes in the composition of firms in our sample. Doidge, Kahle, Karolyi, and Stulz (2018) document a large decline in the number of public U.S. companies during our sample period. The remaining public firms are older and larger and perhaps less likely to face tight covenants than the set of public firms around in the earlier part of the sample period.

We examine four firm characteristics that have been shown to be correlated with realized covenant violations: firm size, measured by total assets; firm age, measured by the number of annual observations in Compustat; a measure of market valuation, proxied by the market-to-book ratio; and the credit rating of the borrower. We term these variables *FirmCharacteristics*. Firm size and age show clear evidence of a trend; the sample of public firms has become significantly larger and older during our sample period. There has also been a trend upward in the number of firms with a credit rating, although the increase has been primarily in firms with a speculative-grade rating. Market-to-book ratios show no obvious trend.

Table 5 reports estimated marginal effects based on model (6) with dummy variables indicating the periods 1997–2003 and 2011–2016, with 2004-2010 the omitted category. Specification (1) includes no controls, specification (2) includes *CovenantControls*, and specification (3) adds *FirmCharacteristics*. In each of case, the dummy indicating the latter period is significantly smaller than the dummy for the earlier period. We summarize the trend in the row labelled "Relative change," which reports the estimated decrease in violation probability as a percentage of the average violation frequency during the years 1997 – 2003, labelled "1997 – 2003 avg." This standardization permits an easier comparison across groups with very different unconditional violation probabilities. The estimated fall in reported violations varies from -75% with no controls to -38% with full controls. Realized outcomes and changes in the composition of the sample account for roughly one-half of the decline in realized violations. Nevertheless, we conclude that the trend in reported violations is not entirely due to changes in the composition of the sample of public firms or a series of positive realized outcomes.

7.3 Trends across loan types

Becker and Ivashina (2016) show that 70 percent of leveraged loans issued in 2015 lacked traditional financial covenants and argue that the rise of these covenant light, or "cov-lite", loans is driven by the influx of non-traditional lenders with high renegotiation costs, such as CLOs and mutual funds. Policymakers and the business press alike have highlighted the potential danger of such loans. Indeed, the Federal Reserve issued supervisory guidance in March 2013 on highly-leveraged loans and Bloomberg recently speculated that weak protections in these loans might lead to the next crisis.[19]

In Figure 9, we plot trends in covenant strictness across loan types to examine whether our results merely emphasize the rise of cov-lite leveraged loans or if the trends identify a broader change in the corporate loan market. The figure shows that the decline in ex-ante covenant strictness is pervasive across deal types. We find no evidence that leveraged loans, loans marketed to institutional investors, or deals backed by private equity sponsors drive our results. In fact, we find that that the trend is equally strong among revolver-only packages, which are typically held

[19] See https://www.federalreserve.gov/supervisionreg/srletters/sr1303a1.pdf and https://www.bloomberg.com/opinion/articles/2018-09-17/subprime-corporate-loans-could-spark-the-next-financial-crisis

by banks rather than non-bank institutional investors. This evidence is striking because Berlin, Nini, and Yu (2018) show that, despite the rise of cov-lite term loans, lenders are almost always protected by traditional financial covenants in the revolver.

7.4 Changes in other loan terms

One potential explanation for less restrictive covenant strictness is that it accompanies a secular increase in the supply of credit. To the extent that borrowers value less restrictive covenants, increased competition among credit supplies could loosen the restrictiveness of covenants, with lenders willing to accept lower expected returns from weaker covenant protections. Of course, an increase in supply would likely result in lower interest rates, as borrowers would also value cheaper credit. We examine the evolution of credit spreads to shed some light on whether an increase in credit supply can explain the loosening of covenants.

Figure 10 explores the evolution of loan spreads, maturity, and collateral in our sample of loans. We include loan maturity and collateral to understand better potential changes in loan supply and pricing through time. The figure shows that although loan spreads fell sharply from their peak during the crisis, they have remained relatively high in recent years. Particularly compared to the credit boom prior to the financial crisis, average loan spreads do not seem low relative to historical standards. To the extent that a large shift in the supply of credit would result in lower loan spreads, we do not see supporting evidence that the decrease in covenant restrictions is due to an increase in credit supply.

The trends in average maturity and covenant usage also suggest that loans have been made riskier over time. In recent years, the shortest maturity facility in a deal has been about 5 months, on average. This is only slightly longer than the typical maturity in 2005-2007 but much longer than the typical maturity during the earlier part of the sample. There is also a notable trend away from loans being secured, which appears to have accelerated after the crisis.

Table 6 examines whether these changes in loan terms remain evident after controlling for changes in borrowers' credit risk and other firm characteristics. We estimate regressions of the form in equation (2) using the natural log of the spread on the loan, the natural log of the loan maturity, and an indicator that the loan is secured by collateral. Other than the use of collateral, the inference from the regressions is similar to those from Figure 11. Compared with the beginning of the sample period, loan spreads are significantly larger at the end of the sample, and the average loan maturity is substantially longer. The likelihood of a loan being secured seems to increase after controlling for borrower characteristics.

8 Conclusion

This paper presents a novel empirical finding – that financial covenants in loan agreements of U.S. public firms have become considerably less restrictive over the last 20 years – to understand how loan covenants evolve in response to time-varying market forces. We model covenant design as a tradeoff between setting the covenant threshold tight, causing violations even when the borrower is not in danger of financial distress (a false positive) and setting the covenant loose, failing to cause violations even when the borrower is on the path to financial distress (a false negative). One intuitive implication of our model is that covenants will be optimally set so that expected violations will decline as the relative cost of false positives increases. Further, our model predicts that financial covenants that can better discriminate between borrowers will result in fewer false positive and false negative violations.

Our initial empirical findings – observing a broad decline in covenant violations between 1997 and 2016 – suggest that the relative costs of false positives have risen through time. Using proxies that allow us to separate firms according to whether they are financially distressed at the time of a covenant violation, we show that the most of the observed decline through in covenant violations can be explained by a substantial drop in false positives (violations by firms not in financial distress).

Moreover, our results indicate that the quality of covenants have improved through time. For instance, even as the frequency of false positives have fallen through time, the frequency of false negatives – which could be expected to increase with looser covenants – shows no upward trend. Similarly, while the proportion of “foot-fault” violations has declined through time, consequential violations in which the lender takes actions to mitigate the risk of payment default have decreased only slightly over time. We show that at least part of the decline in foot faults derives from the fact that lenders have reduced their use of balance sheet covenants in favor of EBITDA-based covenants, which appear to be more informative predictors of financial distress.

We interpret our findings as evidence that lenders have adapted through time to select financial covenants and financial covenant thresholds that better optimize on the tradeoffs between false positives and false negatives.

9 References

Acharya, Viral and Matthew Richardson. “Causes of the Financial Crisis.” *Critical Review* 21 (2009): 195-210.

Aghion, Philippe, and Patrick Bolton. “An incomplete contracts approach to financial contracting.” *The Review of Economic Studies* 59.3 (1992): 473-494.

Axelson, Ulf, Tim Jenkinson, Per Strömberg, and Michael Weisbach. “Borrow Cheap, Buy High? The Determinants of Leverage and Pricing in Buyouts.” *Journal of Finance* 68 (2013): 2223-2267.

Bean, Charles, Christian Broda, Takatoshi Ito, and Randal Kroszner, “Low for Long? Causes and Consequences of Persistently Low Interest Rates.” *Geneva Reports on the World Economy* 17 (2015). Available at https://www.cimb.ch/uploads/1/1/5/4/115414161/geneva_report17_2.pdf.

Becher, David A., Thomas Griffin, and Greg Nini. “Congruence in Governance: Evidence from Creditor Monitoring of Corporate Acquisitions.” Available at SSRN 3046910 (2018).

Becker, Bo, and Victoria Ivashina. “Reaching for Yield in the Bond Market.” *Journal of Finance* 70 (2015): 1863-1902.

Becker, Bo, and Victoria Ivashina. “Covenant-light contracts and creditor coordination.” Available at SSRN 2871887 (2017).

Berlin, Mitchell, Greg Nini, and Edison Yu. “Concentration of control rights in leveraged loan syndicates.” Available at SSRN 2960757 (2018).

Berglöf, Eric, and E. L. von Thadden. “Short-Term versus Long-Term Interests: Capital Structure with Multiple Investors.” *Quarterly Journal of Economics* 109 (1994): 1055–1084

Bernanke, Ben S., and Vincent Reinhart. “Conducting Monetary Policy at Very Low Short-Term Interest Rates.” *American Economic Review* 94 (2004): 85-90.

Chava, Sudheer, Vikram Nanda, and Steven Chong Xiao. “Lending to innovative firms.” *The Review of Corporate Finance Studies* 6.2 (2017): 234-289.

Dell’Ariccia, Giovanni, Deniz Igan, and Luc Laeven. “Credit booms and lending standards: Evidence from the subprime mortgage market.” *Journal of Money, Credit, and Banking* 44 (2012): 367-384.

Demerjian, Peter R., and Edward L. Owens. “Measuring the probability of financial covenant violation in private debt contracts.” *Journal of Accounting and Economics* 61.2-3 (2016): 433-447.

Dichev, Ilia D., and Douglas J. Skinner. “Large–sample evidence on the debt covenant hypothesis.” *Journal of Accounting Research* 40.4 (2002): 1091-1123.

Doidge, C., Kahle, K. M., Karolyi, G. A., & Stulz, R. M. “Eclipse of the public corporation or eclipse of the public markets?” *Journal of Applied Corporate Finance* 30.1 (2018): 8-16.

Ersahin, Nuri, Rustom M. Irani, and Hanh Le. “Creditor control rights and resource allocation within firms.” Available at SSRN 2689866 (2018).

Falato, Antonio, and Nellie Liang. “Do creditor rights increase employment risk? Evidence from loan covenants.” *The Journal of Finance* 71.6 (2016): 2545-2590.

Fama, Eugene F., and Kenneth R. French. “Disappearing dividends: changing firm characteristics or lower propensity to pay?” *Journal of Financial Economics* 60.1 (2001): 3-43.

Ferreira, Daniel, Miguel A. Ferreira, and Beatriz Mariano. "Creditor control rights and board independence." The Journal of Finance 73.5 (2017): 2385-2423.

Freudenberg, Felix, Björn Imbierowicz, Anthony Saunders, and Sascha Steffen, “Covenant violations and dynamic loan contracting.” *Journal of Corporate Finance* 45 (2017): 540-565.

Garleanu, Nicolae, and Jeffrey Zwiebel. “Design and renegotiation of debt covenants.” *The Review of Financial Studies* 22.2 (2008): 749-781.

Gu, Yuqi, Connie X. Mao, and Xuan Tian. “Banks’ Interventions and Firms’ Innovation: Evidence from Debt Covenant Violations.” *The Journal of Law and Economics* 60.4 (2017): 637-671.

Maddaloni, Angela, and José-Luis Peydró. “Bank Risk-taking, Securitization, Supervision, and Low Interest Rates: Evidence from the Euro-area and the U.S. Lending Standards.” *Review of Financial Studies* 24.6 (2011): 2121-2165.

Murfin, Justin. “The supply-side determinants of loan contract strictness.” *The Journal of Finance* 67.5 (2012): 1565-1601.

Nini, Greg, David C. Smith, and Amir Sufi. “Creditor control rights and firm investment policy.” *Journal of Financial Economics* 92.3 (2009): 400-420.

Nini, Greg, David C. Smith, and Amir Sufi. “Creditor control rights, corporate governance, and firm value.” *The Review of Financial Studies* 25.6 (2012): 1713-1761.

Rajan, Raghuram G. “Has Finance Made the World Risker?” *European Financial Management* 12 (2006): 499-533.

Roberts, Michael R., and Amir Sufi. “Control rights and capital structure: An empirical investigation.” *The Journal of Finance* 64.4 (2009): 1657-1695.

Ruckes, Martin. “Bank Competition and Credit Standards.” *Review of Financial Studies* 17 (2004): 1073-1102.

Smith, Clifford. “A perspective on violations of accounting based debt covenants.” *Accounting Review* 68.2 (1993): 289-303.

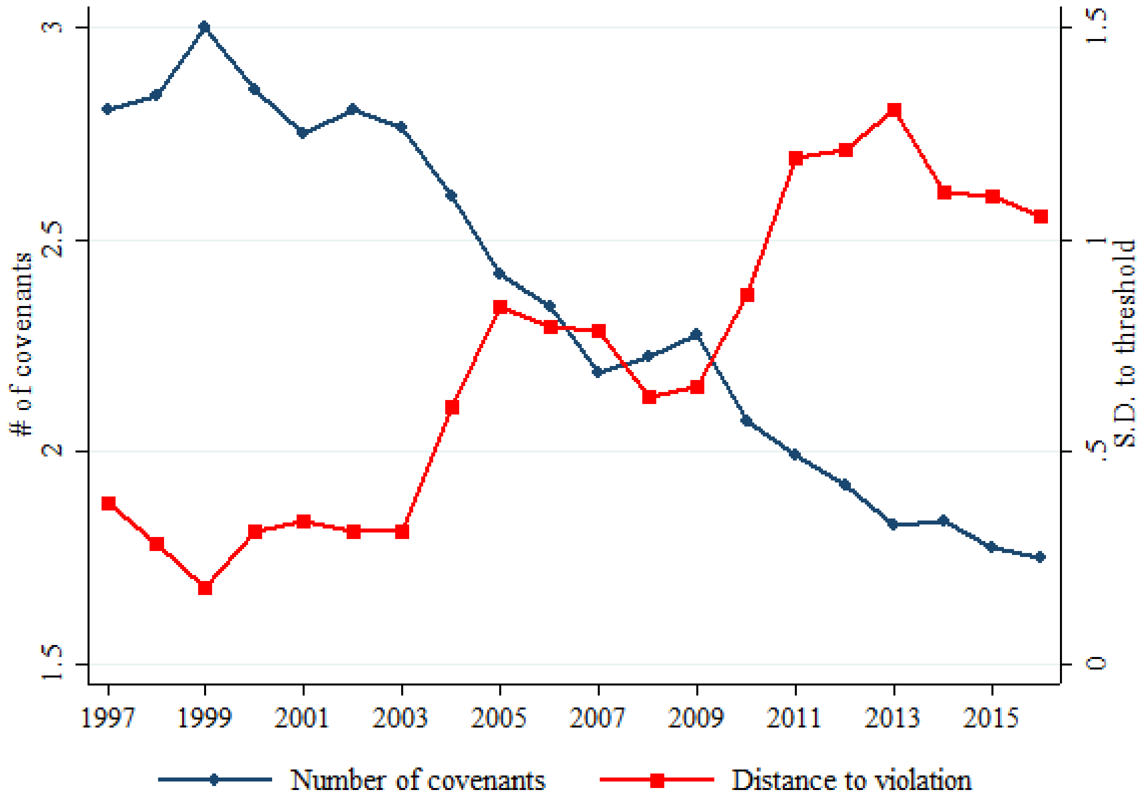
Smith Jr, Clifford W., and Jerold B. Warner. “On financial contracting: An analysis of bond covenants.” *Journal of Financial Economics* 7.2 (1979): 117-161.

Stein, Jeremy C. “Overheating in Credit Markets: Origins, Measurement, and Policy Responses.” Federal Reserve Board Speech (2013). Available at https://www.federalreserve.gov/newsevents/speech/stein20130207a.htm.

Tirole, Jean. *The Theory of Corporate Finance*. Princeton University Press (2010).

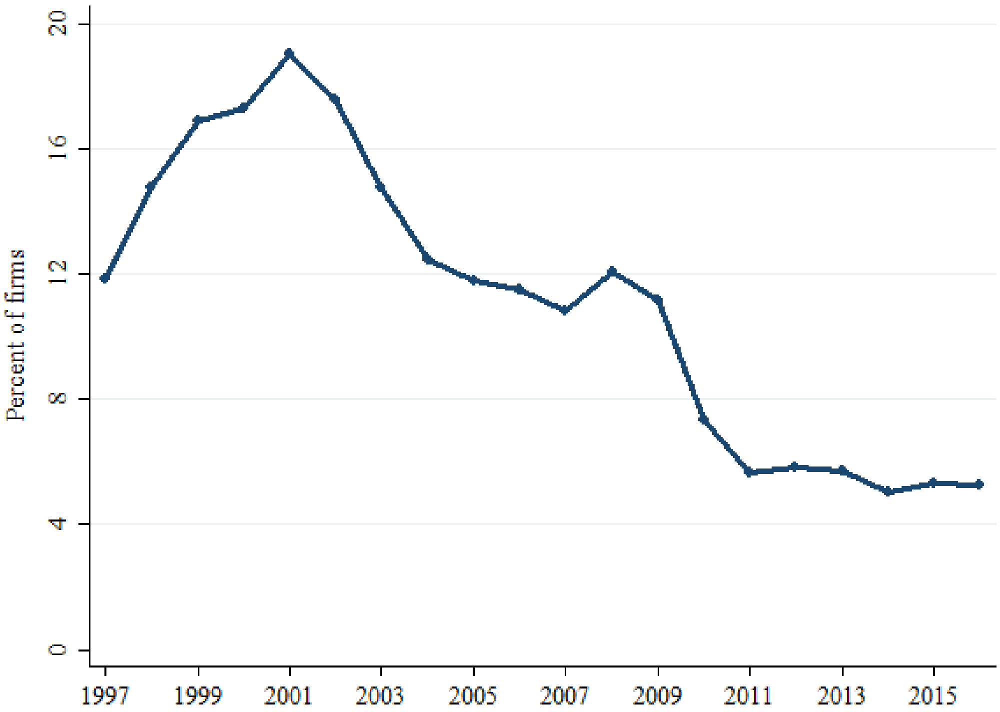
10 Figures and Tables

Figure 1. Financial covenants in corporate loan agreements



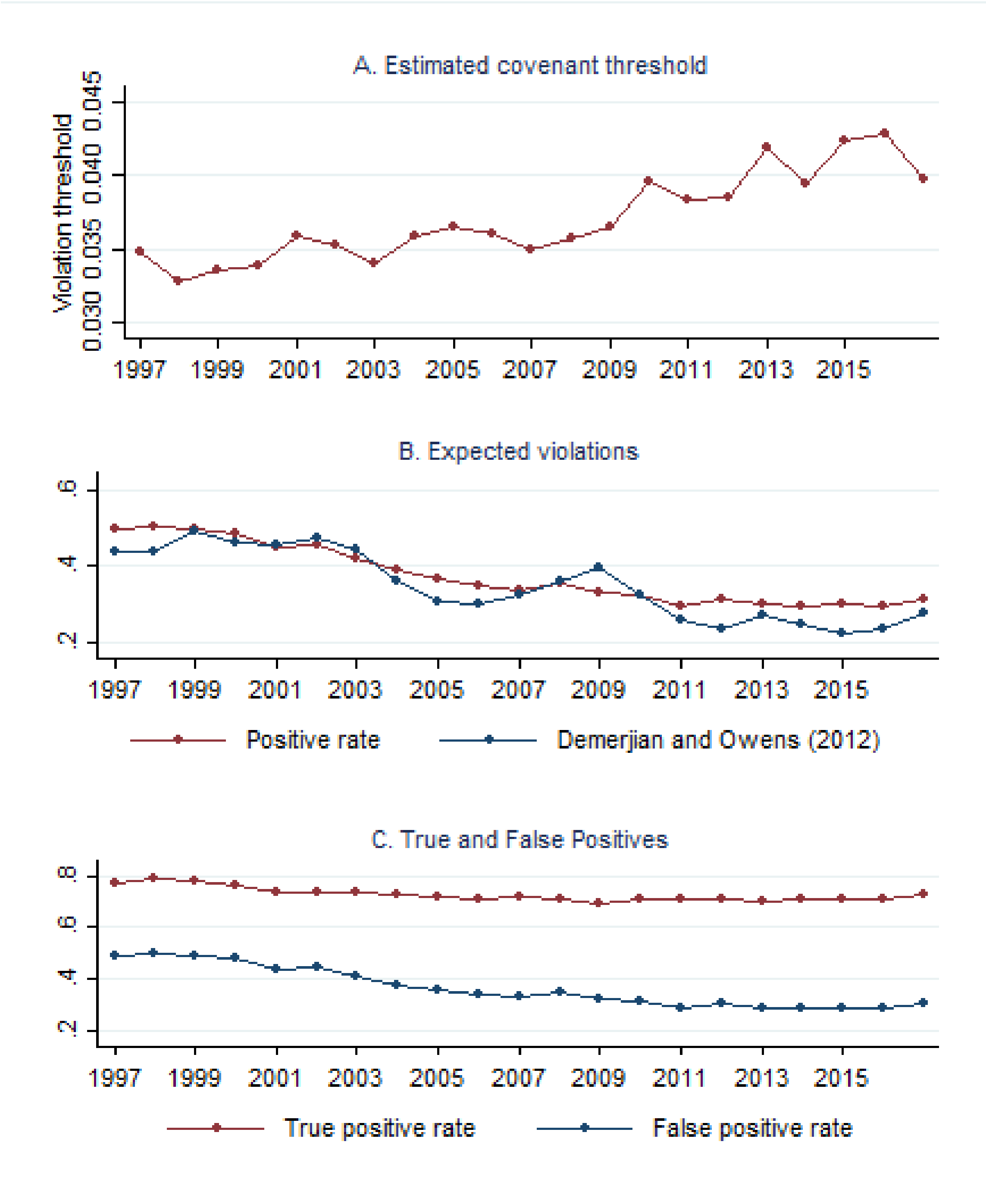
Note: The figure plots the annual mean number of covenants (blue line, measured on left axis) and the annual mean number of standard deviations to violation for the tightest covenant (red line, measured on right axis) at contract origination. The sample is a large set of loans in DealScan issued to U.S. nonfinancial firms in Compustat.

Figure 2: Reported covenant violations



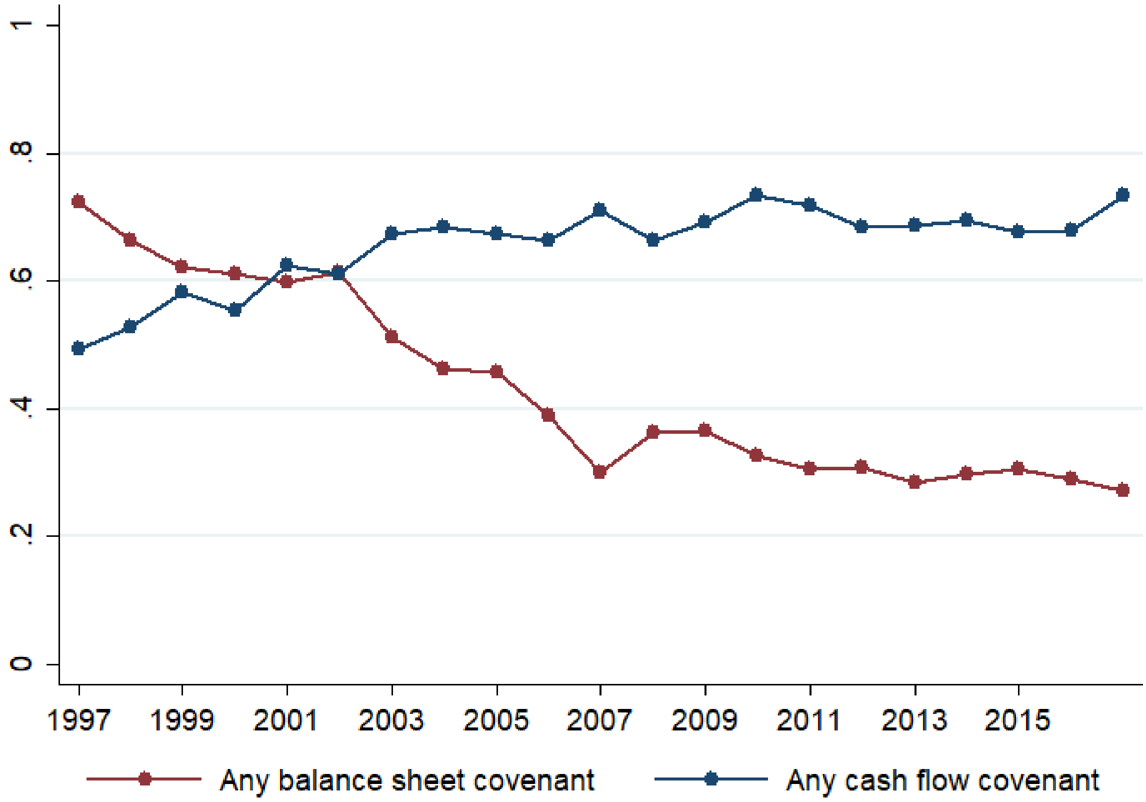
Note: This figure displays the annual percent of firms that report a financial covenant violation in a 10-K or 10-Q filing between 1997 and 2016. The sample consists of 66,589 firm-year observations from 8,499 U.S. nonfinancial firms that can be matched to EDGAR and have data available in Compustat.

Figure 3. Ex ante covenant quality



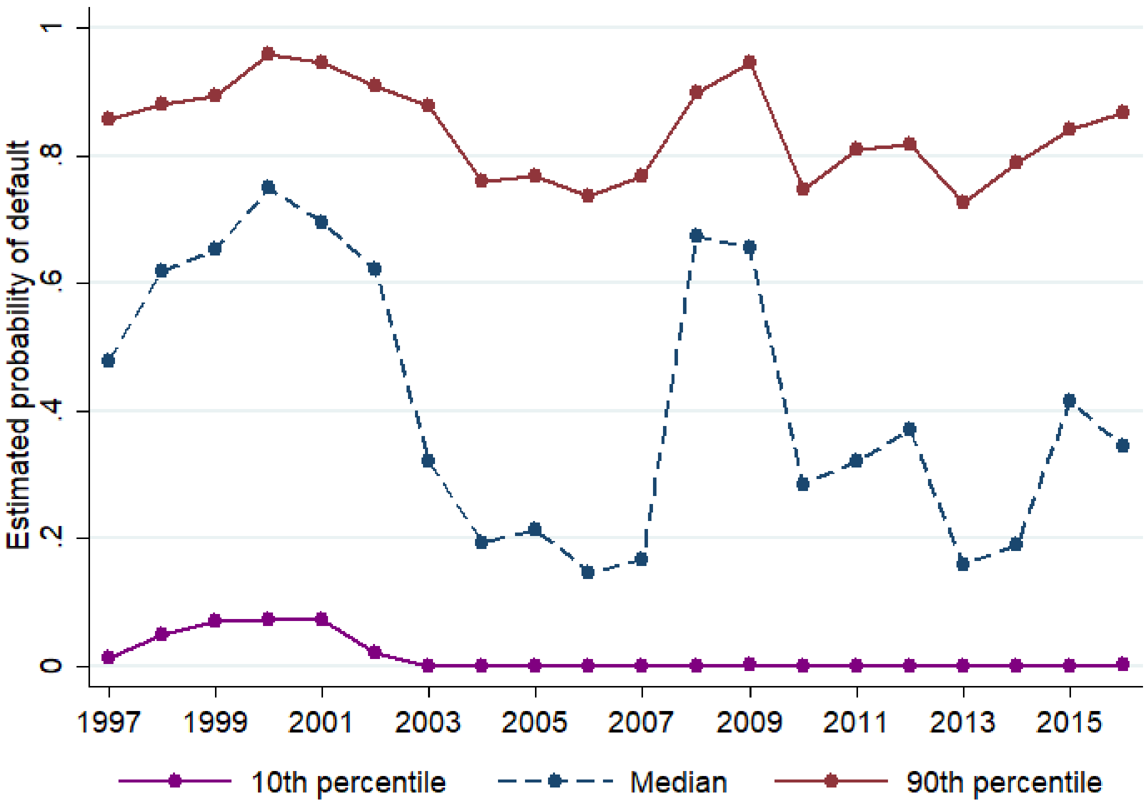
Note: The figures plot time series of ex ante covenant quality. Panel A reports the annual mean covenant threshold, measured as the probability of default at the contractual threshold. Panel B reports the annual mean expected rate of true positives plus false positives, termed "positive rate." The positive rate is the fraction of firm-years exceeding the covenant threshold. Panel B also plots the annual probability of violation from Demerjian and Owens (2012). Panel C reports the expected rates of true positive and true negative. The expected true positive is the fraction of firm-years that subsequently default that exceed the covenant threshold, and the expected false positive rate is the fraction of firm-years that do not default that exceed the covenant threshold.

Figure 4. Balance sheet and cash flow financial covenants



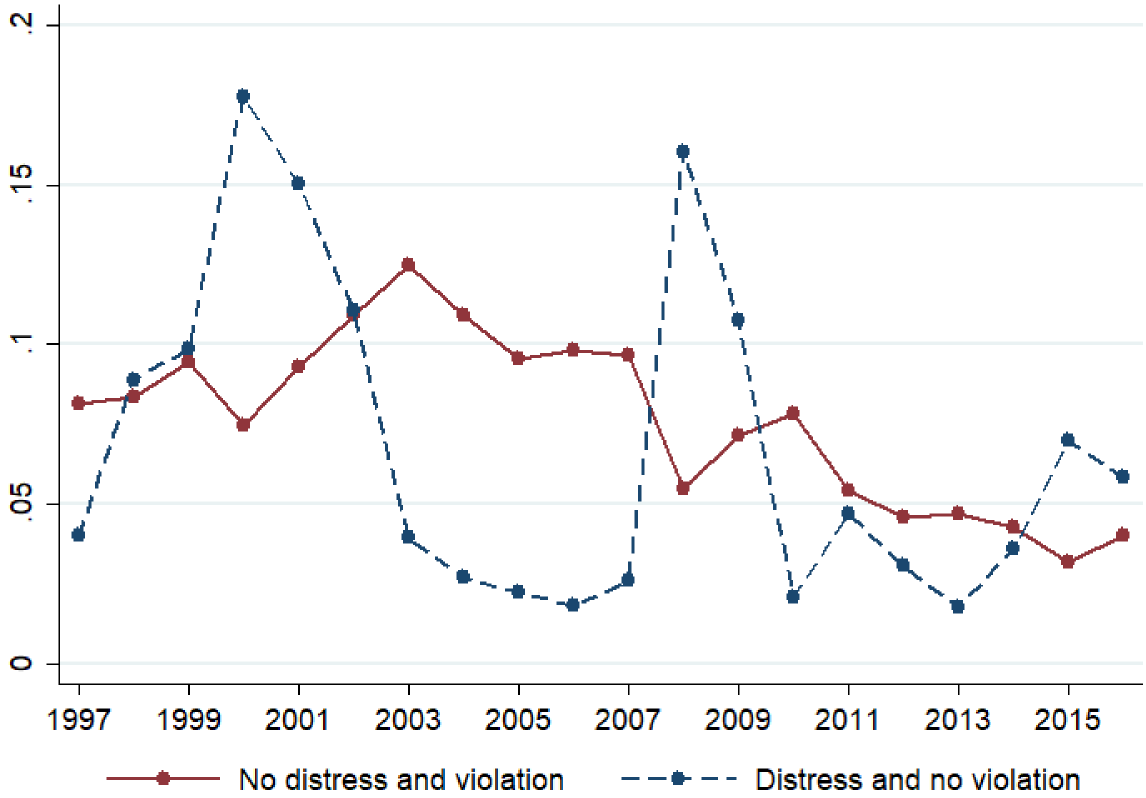
Note: The figure plots the annual mean fraction of loans that contain a financial covenant based on a balance sheet item (red) and based on a cash flow item (blue). We classify covenants as cash flow if they are written on EBITDA and balance sheet otherwise. The groups are not mutually exclusive.

Figure 5. Probability of default conditional on covenant violation



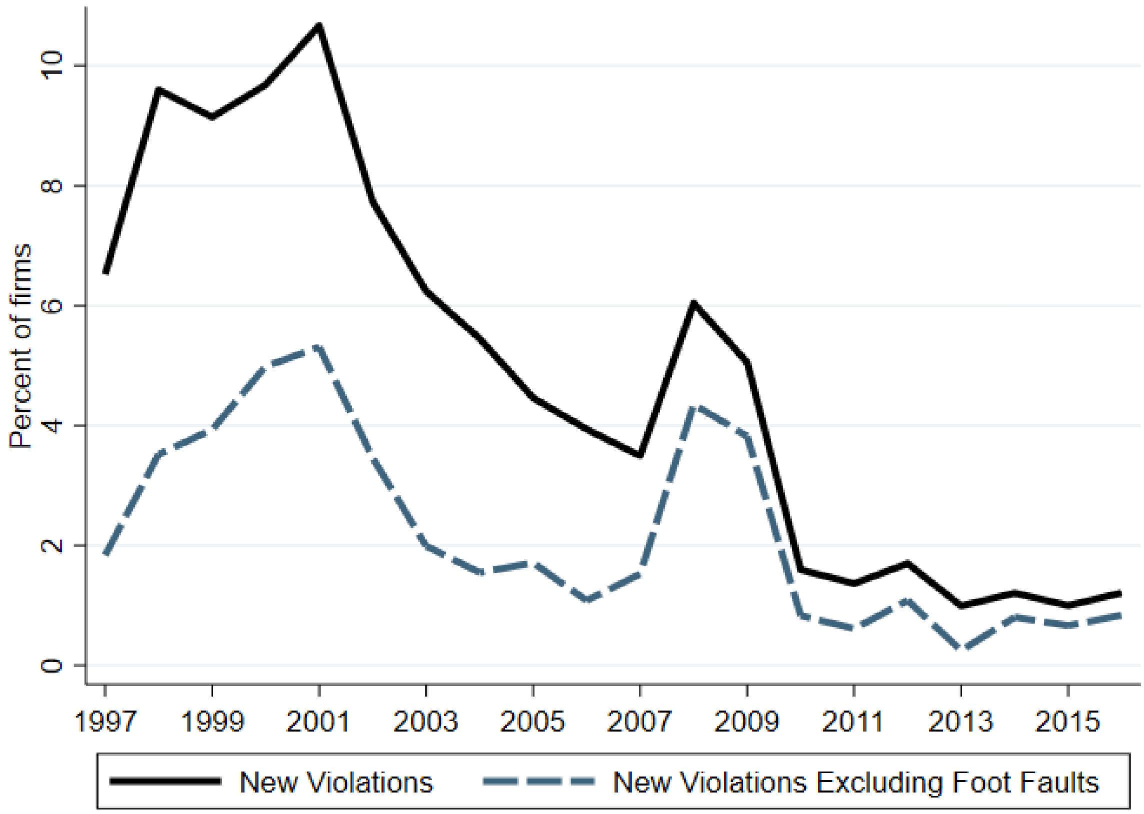
Note: The figure displays the percentiles of the distribution of the annual estimated probability of default for firms that reported a covenant violation in a 10-K or 10-Q filing between 1997 and 2016. The probability of default is based on Bharath and Shumway (2008).

Figure 6. Realized misclassifications



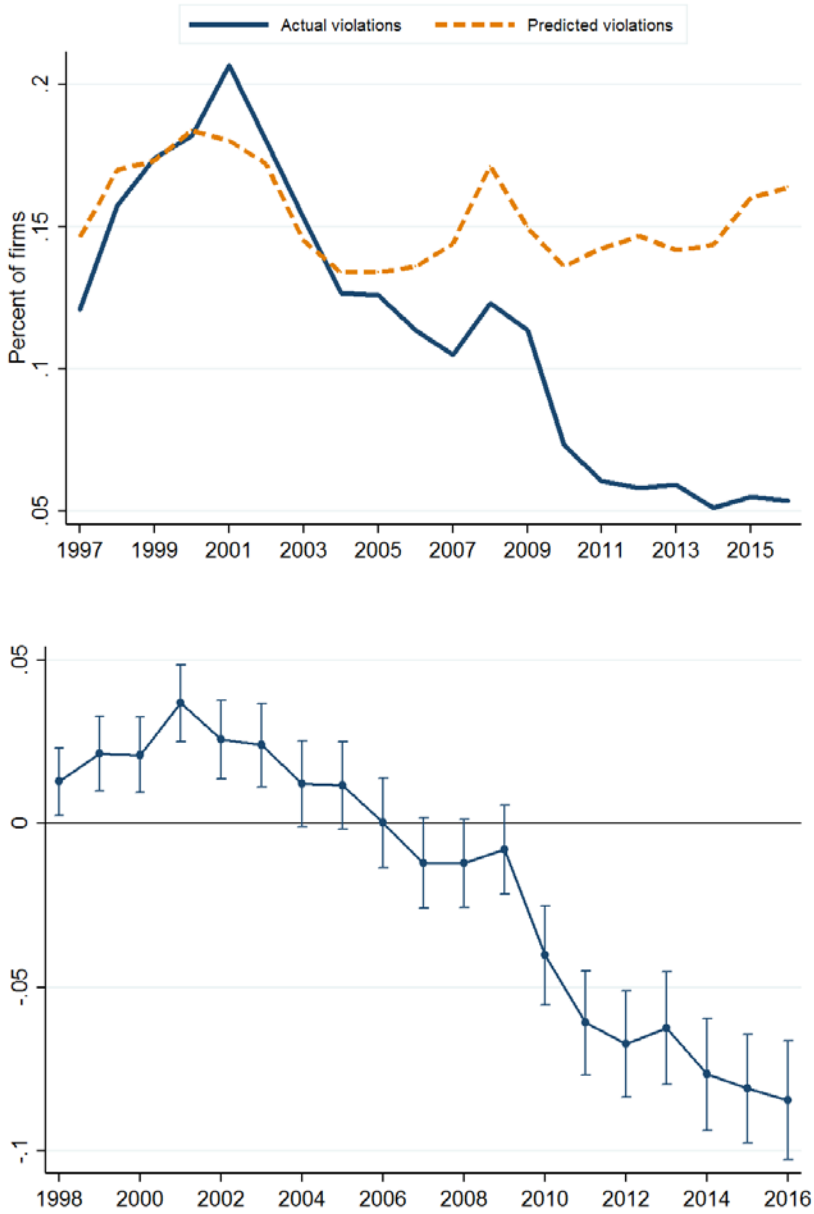
Note: The figure displays the annual fraction of all firms report a financial covenant violation that are classified as non-distressed (red) and the annual fraction of all firms not reporting a financial covenant violation that are classified as distressed (blue). Distressed firms are those with an estimated probability of default above the 90th percentile of the full sample distribution, based on the probability of default from Bharath and Shumway (2008).

Figure 7. Covenant violations with and without foot faults



Note: The figure displays the annual fraction of firms that report a new financial covenant violation (solid black line) and the same fraction excluding foot faults (dashed blue line). We classify a violation as a foot fault if it was resolved through a waiver or an amendment that does not raise interest rates, force repayment, reduce the loan commitment, or force an asset sale/capital raising.

Figure 8. Reported covenant violations and borrower characteristics



Note: These figures display the trend in reported covenant violations after controlling for borrower characteristics. The top panel plots the annual fraction of firms reporting a covenant violation (blue, solid line) and the predicted percent (yellow, dashed line) based on a logistic regression estimated using data from 1997-2003. The bottom panel plots the estimated year fixed effects from the logistic regression that relates the likelihood of a reported covenant violation to covenant control variables. The vertical lines indicate 95% confidence intervals around the point estimates.

Table 1: Sample Description

This table provides summary statistics for our sample of syndicated loans to Compustat firms from 1997 through 2016 (panel A), our sample of firm-years from 1997 through 2016 drawn from Compustat (panel B), and annual counts of covenant violations (panel C). RS (2009) refers to reported violations as produced by Roberts and Sufi (2009), and CR (2008) is based on violations imputed from financial covenants and realized accounting ratios using the methodology of Chava and Roberts (2008).

Panel A: Loan sample summary statistics

	Mean	S.D.	P25	Median	P75	Obs
Prob. of violation	36.90	41.62	1.10	11.15	90.60	10206
Tightest covenant	0.60	1.77	-0.02	0.13	0.76	11396
Number of covenants	2.50	1.14	2.00	2.00	3.00	12069
Deal amount	692.44	1623.72	75.68	256.88	692.57	18131
Spread (bps)	179.12	123.75	87.50	150.00	250.00	15790
Maturity (months)	45.66	23.58	28.00	48.00	60.00	18096
Secured (0/1)	0.70	0.46	0.00	1.00	1.00	13408
Performance pricing (0/1)	0.48	0.50	0.00	0.00	1.00	18131
Number of facilities	1.42	0.79	1.00	1.00	2.00	18131
Inst. term loan (0/1)	0.11	0.31	0.00	0.00	0.00	17560
Revolver only package (0/1)	0.60	0.49	0.00	1.00	1.00	18131

Panel B: Firm-year sample summary statistics

	Mean	S.D.	P25	Median	P75	Obs
Financial covenant violation	0.12	0.32	0.00	0.00	0.00	66589
Operating cash flow / assets	0.03	0.28	0.01	0.10	0.16	61623
Leverage ratio	0.24	0.25	0.02	0.19	0.37	65300
Interest expense / assets	0.02	0.03	0.00	0.01	0.03	54971
Net worth / assets	0.47	0.33	0.32	0.51	0.70	66544
Current ratio	2.95	3.14	1.26	2.00	3.32	65278
Market-to-book ratio	2.09	1.87	1.06	1.46	2.31	66544
Assets ($M)	3517.01	19558.14	70.73	302.32	1426.49	66589
Age	16.66	10.84	8.00	14.00	24.00	66589
Cash / assets	0.21	0.24	0.03	0.11	0.31	66547
Cash flow volatility	0.07	0.11	0.02	0.03	0.07	59060
Investment grade (0/1)	0.12	0.32	0.00	0.00	0.00	66589
Speculative grade (0/1)	0.14	0.35	0.00	0.00	0.00	66589
Unrated (0/1)	0.74	0.44	0.00	1.00	1.00	66589
Rating (0=D ... 21=AAA)	11.09	3.52	8.00	11.00	14.00	16985

Panel C: Covenant violations

Year	Reported violations	RS (2009) Reported violations	CR (2008) Imputed violations
1997	519	238	65
1998	635	296	105
1999	695	296	115
2000	711	313	87
2001	758	307	112
2002	665	230	116
2003	525	193	73
2004	428	137	41
2005	387	138	39
2006	374	116	32
2007	342	117	32
2008	377	127	45
2009	339	98	38
2010	216	50	34
2011	160	25	26
2012	164	.	15
2013	138	.	13
2014	144	.	22
2015	153	.	9
2016	122	.	9

Table 2: Covenant quality

This table reports sample means for the estimated threshold, true positive rate, false positive rate, and total positive rate for the sample of loans from Dealscan. The threshold is the estimated probability of default at the contractual covenant levels, and a positive outcome an estimated default rate above the threshold. True/false positives are based on realized defaults. The probability of violation is computed as in Demerjian and Owens (2012).

		Estimated Rate of			Prob of violation
Number of Covenants	Threshold	True Positives	False Positives	Total Positives	Demerjian and Owens (2012)
1	4.3%	63%	31%	32%	19%
2	3.6%	73%	33%	35%	31%
3+	3.3%	80%	47%	48%	52%
Overall	3.6%	73%	38%	39%	37%

Table 3: Covenant quality by covenant type

This table reports sample means for the estimated threshold, true positive rate, false positive rate, and total positive rate for the sample of loans from Dealscan. The threshold is the estimated probability of default at the contractual covenant levels, and a positive outcome an estimated default rate above the threshold. True/false positives are based on realized defaults. The probability of violation is computed as in Demerjian and Owens (2012). We classify covenants as cash flow if they are written on EBITDA and balance sheet otherwise. The groups are not mutually exclusive.

		Estimated Rate of			Prob of violation
	Threshold	True Positives	False Positives	Total Positives	Demerjian and Owens (2012)
Any balance sheet item	4.5%	68%	48%	48%	35%
Any cash flow item	3.3%	77%	37%	38%	41%

Table 4: Realized violation outcomes

This table presents statistics summarizing the outcome of negotiations following reported covenant violations by U.S. nonfinancial firms in 10-K/Q's between 1997 and 2016. We classify a violation as a foot fault if it was resolved through a waiver or an amendment that does not raise interest rates, force repayment, reduce the loan commitment, or force an asset sale/capital raising. Panel A presents average borrower characteristics for foot fault and non-foot fault violations. Panel B reports foot fault frequencies split by covenant type.

Panel A

	Non-Foot Fault	**Foot Fault**	**p-value**
Operating cash flow / avg. assets	0.007	-0.027	0.002
Leverage ratio	0.316	0.411	0.000
Interest expense / avg. assets	0.026	0.035	0.000
Net worth / asset	0.400	0.315	0.000
Current ratio	1937	1.655	0.000
Market-to-book ratio	1.441	1.205	0.000
CAPX / avg. assets	0.069	0.051	0.000
Net debt issuance / avg assets	0.096	0.045	0.001
Cash / assets	0.088	0.063	0.000

Panel B.

	Foot fault Frequency
Balance sheet covenants	68%
Cash flow covenants	48%
Overall	55%

Table 5: Reported covenant violations and borrower characteristics

This table presents estimates of the marginal effects from logistic regressions that relate the likelihood of a reported covenant violation to covenant control variables. The dependent variable is an indicator that the firm reports a violation during any quarter of the year, and the covenant controls are measured over the contemporaneous year. Specification (1) is estimated over the full sample period, and specifications (2)-(4) over sub-periods. The bottom row reports the p-value from a Wald test that the coefficients on the year fixed effects from 2011-2016 are equal. Standard errors are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

	(1) 1997-2016	(2) 1997-2003	(3) 2004-2010	(4) 2011-2016
ROA	-0.127***	-0.220***	-0.085***	-0.088***
	(0.009)	(0.016)	(0.013)	(0.010)
Market leverage	0.177***	0.306***	0.139***	0.072***
	(0.012)	(0.021)	(0.020)	(0.014)
Interest expense	0.667***	1.227***	0.793***	0.108
	(0.112)	(0.218)	(0.196)	(0.138)
Current ratio	-0.019***	-0.026***	-0.021***	-0.012***
	(0.001)	(0.002)	(0.002)	(0.001)
Observations	49,922	19,061	17,339	13,522
Year FE	Yes	No	No	No
Pseudo R-squared	0.115	0.113	0.0671	0.0644
2011=...=2016	0.130			

Appendix 1: Variable Definitions

This table lists variable definitions and data sources. COMP denotes the Compustat North America Fundamentals Annual File. DS denotes Thomson Reuters LPC DealScan. EDGAR indicates that the data was collected from SEC statements. MR is Michael Roberts' website.

Variable	Source	Description
Loan-level dataset		
Number of covenants	DS	Sum of covenants listed in Financial Covenant and Net Worth Covenant files
Distance to violation	DS&COMP	Distance between contractual threshold and corresponding accounting metric at origination
Prob. of Violation	DS&COMP	Probability that borrower will violate a financial covenant, estimated following Demerjian and Owens (2016)
Murfin (2012) strictness	DS&COMP	Probability that borrower will violate a financial covenant, estimated following Murfin (2012)
Deal amount	DS	Deal amount (in millions) listed in Package file
Spread (bps)	DS	Mean spread of facilities within package
Maturity (month)	DS	Maximum maturity of facilities within package
Secured (0/1)	DS	Indicator equal to one if at least one facility within package is secured
Performance pricing (0/1)	DS	Indicator equal to one if at least one facility within package has performance pricing
Number of facilities	DS	Number of facilities within package
Leveraged loan (0/1)	DS	Indicator equal to one if at least one facility within package is in the leveraged loan market segment
Institutional loan (0/1)	DS	Indicator equal to one if at least one facility within package is in the institutional loan market segment
Sponsored loan (0/1)	DS	Indicator equal to one if at least one facility within package is in the sponsored loan market segment
Revolver only package (0/1)	DS	Indicator equal to one if the package consists of only one facility and that facility is revolver loan-type
Firm-year dataset		
Reported violation	EDGAR	Indicator equal to one if firm reports violating a financial covenant in a quarterly financial statement, collected using the same hand-collection procedure as Nini, Smith, and Sufi (2012)
RS(2009) reported violation	MR	Indicator equal to one if the Roberts and Sufi (2009) dataset identifies a covenant violation
CR(2008) imputed violation	DS&COMP	Indicator equal to one if current ratio, total net worth, or tangible net worth observed in Compustat falls below the contractual threshold in Dealscan, imputed following Chava and Roberts (2008)
ROA	COMP	Rolling four quarter operating income before depreciation scaled by total assets
Leverage ratio	COMP	Long-term debt plus debt in current liabilities, divided by total assets
Interest expense	COMP	Rolling four quarter interest expense scaled by total assets
Current ratio	COMP	Total current assets divided by total current liabilities
Market to book	COMP	Ratio of market value to book value of total assets