Debt Covenant Violations and Trade Credit*

Zilong Zhang†

July 20, 2014

Abstract

This study examines the relationship between senior and junior creditors through the lens of debt covenant violations. I find that covenant violations cause the trade credit (i.e., accounts payable) to decline in a sharp and persistent manner. The decline is larger when the violating firm has poor creditworthiness and higher level of short-term debt. Further, suppliers whose sales largely depend on their customers suffer deteriorating operating performance and lower stock returns after customers' covenant violations. This negative effect is exacerbated by the presence of larger loans outstanding and higher costs of switching customers. Overall, the findings suggest and quantify the real costs imposed by senior creditors' control rights (e.g., rights of accelerating debt repayments) on junior creditors.

JEL classification: G21, G30, G32

Keywords: Debt covenants, Trade credit, Creditor control rights, Acceleration of debt repayments, Financial distress, Stakeholders, Delegated monitoring

*I thank Vidhan Goyal, Sudipto Dasgupta, David Denis, Pengjie Gao, Kai Li, Yupeng Lin, Laura Liu, Xuewen Liu, Abhiroop Mukherjee, Kasper Nielsen, Rik Sen, Wei Wang, Fan Yu (discussant), and participants at CKGSB-SAIF Summer Institute of Finance Conference and FMA Asian Conference Doctoral Consortium for helpful comments. All errors are my own.

†Hong Kong University of Science and Technology, Clear Water Bay, Kowloon, Hong Kong. Tel.: +852 5319-5519. zilongzhang@connect.ust.hk

1 Introduction

Firms often have multiple classes of lenders with varying seniority. While in general the interests of these different lenders are aligned and delegating monitoring to a certain class of lenders can be cost effective (Diamond, 1984), there are cases, such as bankruptcy, in which conflicts of interest between senior and junior lenders arise. Junior creditors are often sacrificed when senior creditors with control rights pursue their own interest and make suboptimal liquidation decisions (Ayotte and Morrison, 2009; Bulow and Shoven, 1978; Franks and Nyborg, 1996). For instance, senior creditors (banks) can provide debtor-in-possession financing after the borrower files for Chapter 11, through which they gain power against junior creditors to protect their loans. Recent studies have called for a more detailed examination on the costs and benefits of creditors' control rights in the presence of multiple classes of cash flow claimants (e.g., Li and Wang, 2014; Beatty et al., 2012).

This paper examines the causes and consequences of conflicts of interest between banks and trade creditors after their common borrower violates loan covenant(s). Aside from bankruptcy, debt covenant violation represents a scenario under which different classes of creditors may have conflicting claims. After a borrower violates certain debt covenant(s), the debt contract typically enables banks to take one of the following actions: renegotiating the debt contract terms; accelerating loan repayment;[1] initiating legal bankruptcy proceedings. By implementing or threatening to implement one of these actions, banks are able to influence borrowers' management, which is usually referred to as banks having certain "control rights" after their borrowers' covenant violations (see, for example, Denis and Wang, 2014; Chava and Roberts, 2008; Roberts and Sufi, 2009; Nini et al.,

[1]Acceleration of loans requires the borrower to immediately pay off the unpaid balance of the loans' principal. When a company violates a debt covenant on a long-term debt, the company must reclassify the debt as short-term, as long as the lender has the right to call the immediate repayment of the loan.

2012; Gu et al., 2013; Tan, 2013; Ferreira et al., 2014). Banks can actively monitor the borrower using such control rights, which lowers the monitoring costs of other creditors; in other words, banks act as a delegated monitor that benefits other creditors (Diamond, 1984; Fama, 1990; Beatty et al., 2012). On the other hand, however, banks' accelerating loan repayments or initiating legal bankruptcy proceedings can jeopardize junior creditors' claims on borrower's cash flows, since in either case cash flows are supposed to go to senior creditors ahead of junior creditors.[2] Therefore, debt covenant violations provide a unique setting to test the relationship between senior and junior creditors.

There are two reasons for focusing on trade creditors' response to covenant violations. First, trade creditors are vulnerable to banks' control rights when borrower performance deteriorates. In extreme cases such as borrower bankruptcy, most of the time trade creditors have the lowest seniority among all creditors (Chen, 2005). Before bankruptcy (e.g., after covenant violation), if banks decide to trigger acceleration of debt repayments, while other junior creditors such as public bond holders can use cross-acceleration provisions to protect their own claims (Beatty et al., 2012), trade creditors do not have any contractual tools to prevent banks from grabbing cash flows ahead of themselves. Trade credit has to be delayed if cash flows left over after loan acceleration are insufficient to pay off accounts payable. For suppliers, especially those whose sales are largely dependent on their customers, delay in payment of trade credit can cause substantial costs. For instance, during the 2007-2008 financial crisis, the liquidity crunch caused by the nonpayment of Detroit's Big Three automakers put many auto suppliers on the brink of failure in just one year; some of them could not even get adequate funding to pursue Chapter 11 bankruptcy.[3] Therefore, to isolate themselves from customers' potential delay in repayment, trade cred-

[2]Debts are automatically accelerated if bankruptcy is filed.

[3]The suppliers group estimated that payments from the Big Three to auto parts manufacturers decreased to $2.4 billion in March 2009, from a monthly average of $8.4 billion in the fourth quarter of 2008. – "The Auto Industry's Other Crisis", *Businessweek*, 13 March 2009.

itors either need to reduce their exposure by extending less credit, or have to strengthen their claims by directly revising the credit contract terms.

Second, trade creditors are one of the most important creditors of a firm, and hence their welfare would have important value implications for the borrower. On average, trade credit accounts for a significant proportion of firms' external financing,[4] and is arguably a substitute for bank credit (Garcia-Appendini and Montoriol-Garriga, 2013; Love et al., 2007; Burkart and Ellingsen, 2004). Moreover, for the buyers, trade credit not only offers working capital financing, it also provides liquidity insurance (Cunat, 2007; Wilner, 2000), product quality guarantee (Klapper et al., 2012; Ng et al., 1999), and savings in transaction costs (Ferris, 1981; Banerjee et al., 2005).

I empirically examine the average effect of covenant violations on trade credit. The baseline results show that on average firms experience a substantial reduction in their accounts payable after covenant violations, and the reduction is persistent for two years. I perform a difference-in-differences (DID) analysis to control for unobserved trend that is correlated with covenant violations and trade credit, and a discontinuity design with borrowers close to violation thresholds to further mitigate the endogeneity concern. The findings are robust across different approaches, and thus suggest that after covenant violations, the increased prospect of banks exercising control rights drives away trade creditors. Furthermore, I find that the reduction of trade credit after covenant violations is larger for borrowers with poorer creditworthiness and with more short-term debts outstanding, which are consistent with the idea that the effect of covenant violations on trade cred-

[4]In my sample, the median ratio of accounts payable to total debts is 38%. During the 1990s, trade credit financing has accounted for an average $1.5 trillion of the book value of all assets of U.S. corporations and has represented approximately 2.5 times the combined value of all new public debt and primary equity issues during a given year in the US (Ng et al., 1999).

it is increasing with the probability of banks accelerating debt repayments (reasons are discussed in Section 2).

Next, I examine the value implications of covenant violations on trade creditors. For suppliers whose sales largely depend on one or two major customers, it is difficult to quickly switch to another customer and avoid the costs arising from banks' strengthened control rights. Therefore, on average the firm value of such "dependent" suppliers should be negatively impacted after covenant violations. Consistently, I find that after their customers' violations of debt covenants, dependent suppliers suffer from declining sales, deteriorating operating performance, reduced investment, and lower long-run stock returns. The negative impact on operating performance is more pronounced when the customer has larger loans outstanding (such that less is left over for trade creditors after loan acceleration) and when it is more costly for the supplier to switch to another customer.

Overall, the findings of this paper suggest that senior creditors' control rights, via debt covenant violations, impose significant costs on junior creditors. The primary reason is that the control rights enable senior creditors to grab cash flows ahead of junior creditors more conveniently, such as through loan acceleration, or, in the extreme, liquidating the borrower.

This study is related to the existing studies examining the effect of financial distress along the supply chain. Supplier value is found to be negatively affected by their customers' bankruptcy filings (Jorion and Zhang, 2009; Hertzel et al., 2008); as a response, many suppliers extend less trade credit (Garcia-Appendini and Montoriol-Garriga, 2014). My paper deviates from these studies and thus contributes to the literature in two aspects. First, while all the existing studies are investigating cases in which a default or bankruptcy is imminent, this paper examines covenant violation and argues that the negative effect of

customer's financial distress starts to emerge well before bankruptcy. Second, the senior creditors' enhanced control rights are the key driving force that triggers the early response by trade creditors, which is largely omitted by the afore-mentioned empirical studies but has been suggested by theoretical models (Bulow and Shoven, 1978; Franks and Nyborg, 1996).

Furthermore, this paper contributes to the growing literature that examines the costs and benefits of creditor control rights in the presence of multiple classes of creditors (see, for example, Li and Wang, 2014; Beatty et al., 2012; Eckbo et al., 2014). To my best knowledge, this is the first study that quantifies the effect of creditor conflicts on junior creditors' firm value. In addition, I also provide evidence for the value implications on the borrower.[5] The loss of external finance (trade credit), especially when the borrower has poor credit quality, would cause financial difficulty of the borrower and impact its real economic activities. In this sense, this paper also adds to the literature studying the indirect costs of financial distress. Knowing ex ante that debt contracts impose significant consequences, managers may decide to rely less than they otherwise would on debt financing, which provides a potential explanation for debt conservatism (Graham, 2000).[6]

The remainder of the paper proceeds as follows. Section 2 develops the main hypotheses. Section 3 describes the data and summary statistics. Section 4 discusses the empirical strategy and presents the results. Endogeneity problems are discussed in the same section. Section 6 concludes.

[5]In the internet appendix, I show that the reduction in trade credit caused by covenant violations leads to less inventory, less investment, lower sales, lower ROA, and worse long-run stock price performance.
[6]For a discussion of the literature on debt conservatism, see Frank and Goyal (2008).

2 Hypotheses

Debt covenant violations bring borrower and bank to the negotiation table and place the bank in a position of influence: debt contracts enable the bank to decide whether to modify or waive restrictions or to demand immediate repayment. Through renegotiation or threatening to accelerate repayment, the bank is able to influence the management of the borrower (Denis and Wang, 2014; Chava and Roberts, 2008; Roberts and Sufi, 2009; Nini et al., 2012; Gu et al., 2013; Tan, 2013; Ferreira et al., 2014).

However, banks' enhanced control power not only affects the borrower, it also has potential externalities to other creditors. For lenders whose claims are diffusely held and costly to renegotiate, delegating monitoring to "credible specialists", such as bank, can mitigate agency problems, enhance borrower's firm value, and thus preserve lenders' collective claims (Diamond, 1984; Fama, 1990; Beatty et al., 2012; Nini et al., 2012). On the other hand, however, covenant violation signals an increased probability of banks demanding immediate loan repayment and even liquidating the borrower, either of which would allow banks to grab borrower's cash flows ahead of other creditors. This negative externality is especially pronounced for junior creditors who have little control power to protect their cash flow rights, such as trade creditors. While public bond holders can use cross-acceleration or cross-default provisions to protect their claims, trade creditors are not granted with such contractual rights. Therefore, trade creditors would be most vulnerable to banks' post-violation control rights; the costs brought by covenant violations to trade creditors are likely to exceed the benefits of delegated monitoring, causing them to extend less trade credit. The null hypothesis is thereby as below.

Hypothesis 1 *On average, firms' trade credit declines after covenant violations.*

Trade creditors' response should be more pronounced with increased prospect of banks exercising their control rights. Borrower's creditworthiness or ability to recover from the liquidity shock is arguably a determinant of banks' decision to request immediate repayment. First, poor creditworthiness indicates lower ability of generating future cash flows for banks and trade creditors to split; banks thus have the incentive to secure their principal by demanding immediate repayment. Trade creditors that have dispersed holdings and thus little power to renegotiate against banks would probably suffer from a delay or default of repayment. Second, poor credit quality signals a higher likelihood of going bankrupt, in which debt repayments are automatically accelerated and senior creditors are paid off first; junior creditors' claims are impaired as long as borrower's liquidation value is insufficient to pay off all debt liabilities.

Another factor that affects banks' decision to accelerate loans is borrower's debt maturity structure. The reason is that invoking an acceleration clause costs banks future interest income: once a bank demands immediate principal repayment, it loses the right to receive future interest payments that would have come due had the loan been paid off normally. Long-term debts (those with a longer remaining life) normally carry more future interest payments than short-term debts. If at the covenant violation there is a substantial proportion of debts that have relatively long remaining life, creditors would be discouraged to demand immediate repayment of loans. The conflicts of interest, in this case, would be less severe and supplier firms are more likely to stay with the borrower.

The second hypothesis thus rests on the cross-sectional variation of the likelihood of banks exercising control rights.

Hypothesis 2 *On average, the reduction of trade credit after covenant violations is more pronounced for borrowers with lower credit quality and for borrowers with more short-term debts.*

Some suppliers are highly dependent on their customer who generates a major proportion of their sales. Once such a customer violates a debt covenant, keeping providing trade credit puts their (dependent suppliers') future sales under the risk of default due to senior creditors' debt acceleration or liquidation decisions; meanwhile, it is very difficult for dependent suppliers to liquidate their stakes all at once since it triggers major costs to exit an important bilateral trading relationship and rebuild another by making new customer-specific investments. In other words, once a major customer violates debt covenant, a dependent supplier has to trade off the expected loss from debt acceleration and the cost of switching to a new customer, both of which are not avoidable. Therefore, covenant violations are expected to negatively impact the firm value of dependent suppliers. This is our third hypothesis.

Hypothesis 3 *On average, customers' covenant violations have a negative effect on dependent suppliers' operating performance and long-run stock returns.*

Since dependent suppliers have to trade off the expected cost of loan acceleration and the cost of switching customers, an increase in either cost is likely to exacerbate the negative impact of covenant violations on suppliers. Expected cost of loan acceleration can be measured by the total amount of loan principal. Holding the probability of loan acceleration constant, the larger the loan principal, the more cash flows that would be diverted to the bank ahead of suppliers. To measure switching costs, I exploit the idea that switching costs are higher for suppliers producing specialized products, since it is difficult for them to redeploy investments that are specific to major customers (Banerjee

et al., 2008). Following Titman and Wessels (1988), I use R&D to sales ratio and SG&A to sales ratio to proxy for product uniqueness. The fourth hypothesis, regarding the two major costs facing dependent suppliers after covenant violations, is formalized as follows.

Hypothesis 4 *On average, the negative effect of covenant violations on dependent suppliers' performance is more pronounced for suppliers faced with larger balance of outstanding loans and higher costs of switching customer.*

3 Data and Summary Statistics

The sample construction begins with all Compustat firm-quarter observations from 1996 to 2008, excluding financial firms (SIC codes 6000-6999). The sample begins in 1996 because I rely on the information provided by Nini et al. (2012) on covenant violations, which starts from 1996 when electronic filing first became mandatory for all SEC-registered firms. Nini et al. (2012) identify the occurrence of covenant violations directly from 10-K and 10-Q Securities and Exchange Commission (SEC) filings based on a text-search algorithm,[7] and make the collected data set available on Amir Sufi's website. I refer interested readers to the Data Appendix of Nini et al. (2012) for more details on the search algorithm and data information. To construct my sample, I merge this covenant-violation dataset with Compustat firm-quarter observations. Because after the 1990s public debt rarely includes financial covenants (Begley and Freedman, 2004),[8] our sample of covenant violations are mainly those contained in bank loans, or syndicated loans with at least one lead bank.

[7]SEC requires that any breach of the covenant shall be stated in the notes to the financial statements (SEC, 1988), and has reinforced this requirement in recent interpretations (SEC, 2003).

[8]Begley and Freedman (2004) find that less than 5% of the public debt agreements contain financial covenants for their sample of 1990s bond contracts.

To gauge the trade credit a firm receives, I use accounts payable scaled by cost of goods sold. This measure can proxy for the extent to which a firm uses trade credit to finance its purchase costs.[9] I also use an alternative measure of purchase cost, the quarter-end COGS adjusted for within-quarter changes of inventory, and find almost identical results (not reported). Following Nini et al. (2012), to explicitly control for the accounting ratios on which financial covenants are written (which are also correlated with accounts payable a firm can receive), I construct a set of variables called "covenant controls", which include: operating cash flow scaled by average assets, the leverage ratio, the ratio of interest expense to average assets, the ratio of net worth to total assets, the current ratio, and the market-to-book ratio. Other controls include logarithm of book assets, tangibility, cash holding, ROA, and net capital expenditure. All variables used in this study are formally defined in the Appendix. I require all these variables to be non-missing and thus end up with 173764 firm-quarter observations. To mitigate the impact of outliers on my analysis, I winsorize all variables at the 5th and 95th percentiles, though my results remain quantitatively similar if I winsorize at the 1st and 99th percentiles.

Table 1 presents the summary statistics. The outcome variable, Accounts Payable divided by COGS, has an average of 0.71 and a median of 0.46, suggesting that firms finance a large proportion of their costs of goods by trade credit. Accounts payable scaled by adjusted COGS have an identical mean value and a slightly smaller median (0.45). The average size (in terms of book assets) of my sample firms is \$1553 million. Most of our measures have highly comparable statistics with those in Roberts and Sufi (2009), for

[9]Note that Accounts Payable/COGS also proxies for days payable outstanding, which is Accounts Payable/(COGS/365) and measures the average number of days a company takes to pay its suppliers. Thus the results would also imply the credit terms required by trade creditors: a reduction in AP/COGS implies suppliers demanding fewer days to repay trade credit. Though this interpretation is consistent with the main argument of this paper that trade creditors become more concerned about their junior claims and extend less trade credit to the borrower, I conduct robustness tests using total sales as an alternative denominator, and find very similar results (reported in internet appendix).

example, Cash, Tangibility, Leverage, Market/Book, Cash flow, Interest expenses, and Net worth. For measures not reported in Roberts and Sufi (2009), such as Current ratio, the statistics are very similar with Gu et al. (2013).[10]

[Table 1 is here]

4 Empirical Results

This section first explains the empirical strategies used in this study, and proceeds by presenting the baseline results. Then I discuss possible endogeneity issues and conduct corresponding strategies to address them. Furthermore, the results are shown to be stronger when banks are more likely to exercise their control rights. In the end, I examine the value implications of covenant violations on trade creditors.

4.1 Empirical Strategies

To disentangle the effect of covenant violations from changes in trade credit that would have otherwise occurred around the covenant violation, I first use quasi-discontinuity regressions following Roberts and Sufi (2009) and Nini et al. (2012), which exploit the discontinuity created at the point of violation. To be specific, covenant violations depend on whether a known variable is below or above a pre-specified threshold, regardless of the distance to the threshold. This rule arbitrarily creates a discontinuous treatment in the neighborhood of a known cutoff, and thus resembles a randomized trial.[11] As long as the trade credit (or unobserved variables that affect trade credit), as a function of variables

[10] My sample has a comparable sample size with Roberts and Sufi (2009), but much larger than Gu et al. (2013)

[11] Renegotiations may change the initial cutoff and its local randomness, which would be discussed below.

on which covenants are written, does not exhibit the same exact discontinuity at the violation threshold, we are able to identify the effect of a discrete violation on the trade credit. This assumption is valid as long as the following conditions hold: (1) suppliers, in the absence of financial covenants, would not have chosen the exact same threshold to determine the trade credit policy, and (2) managers of the borrower would not have chosen the same cutoff point to determine how much trade credit to borrow. The possible breakdown of these two conditions are discussed in the endogeneity section.

Our regressions employ a large sample of violating and non-violating firms in a dynamic model of trade credit, and include as right-hand side variables a covenant violation indicator variable along with linear, nonlinear, and step functions of the covenant controls (underlying variables on which covenants are written). By flexibly controlling for these variables, the impact of a covenant violation is identified by the discontinuity occurring at the covenant threshold. The estimation of regressions is specified as follows:

$$\begin{aligned} AccountsPayable_{i,t} =& \alpha_0 + \beta_0 Violation_{i,t-1} + \beta_1 X_{i,t-1} + \beta_2 Z_{i,t} + \beta_3 Z_{i,t-1} \\ &+ \beta_4 HighOrder_{i,t-1} + \eta_j + \gamma_t + \nu_\tau + \epsilon_{i,t} \end{aligned} \tag{1}$$

where $AccountsPayable$ is accounts payable scaled by the cost of goods sold, $Violation$ is an indicator variable that equals 1 for a new financial covenant violation, which is defined as a violation not preceded by any violation during the last four quarters,[12] η_j is industry fixed effects, γ_t is fiscal-quarter fixed effects, ν_τ is calendar-quarter fixed effects, X is a vector of trade credit control variables,[13] Z is a vector of covenant control variables:

[12]Following the literature, I mainly focus on new violations, because they represent the initial signal and the first opportunity for creditors to intervene, and thereby provide the cleanest identification of the effect of violations on trade credit. However, as a reference, I also provide evidence on an alternative definition of violations in the baseline results.

[13]Both the contemporaneous and the lagged term of CAPEX are included to account for the change of investment activities due to covenant violations.

net worth to assets ratio, current ratio, interest expense to assets ratio, market to book, operating cash flow to assets ratio, and leverage ratio, and *HighOrder* is the second and third power of the covenant controls. The key coefficient of interest is β_0.

To address the concern that the functional form in equation (1) is misspecified and ensure the robustness of my findings, I employ a propensity-score matching method following Nini et al. (2012). In particular, I use a first-stage probit regression to estimate the likelihood that a firm violates a covenant, and then match violating firms to non-violating firms based on their fitted violation probability, i.e., the "propensity score". The second stage measures differences in outcomes between violating firms and their propensity score-matched firms. As the validity of discontinuity design lies critically in the accuracy of model specification, such matching estimators allow us to control for a variety of observable traits between the two samples in a highly nonlinear and flexible fashion (Roberts and Whited, 2011).

More importantly, the propensity-score matching algorithm ensures that the treated firms (violating firms) and control firms (matched non-violating firms) have parallel trends before violations, such that we can estimate the effect of covenant violations using a difference-in-differences (DID) approach (conducted in section 4.4).

Finally, I conduct a discontinuity design that focuses on firms that fall in the narrow band around the initial covenant thresholds (also referred to as "covenant limits"). Since violating and non-violating firms within the close neighborhood of initial covenant thresholds are more likely to be homogenized, i.e. the treatment is unlikely to be related to unobserved firm characteristics, this discontinuity design helps us establish the causal effect of covenant violations more cleanly.

4.2 Baseline Results: Covenant Violations and Trade Credit

I begin the analysis by graphically illustrating the relationship between violations and accounts payable. Figure 1 presents unconditional means and medians for accounts payable in event time, where time zero is the year of a new violation.[14] In addition to COGS, I also use total sales as the scaling variable to show the robustness of the trend. The graphs show that before covenant violation, both measures of accounts payable are increasing, consistent with the findings of Molina and Preve (2012) and Garcia-Appendini and Montoriol-Garriga (2014) that firms at initial stages of financial distress use more trade credit to substitute alternative sources of finance. However, immediately following the covenant violation, the increasing trend of accounts payable is reversed, and the decline is persistent for at least 3 years after the violation.

Next, I formally examine the effect of covenant violations on trade credit by employing the quasi-discontinuity approach, i.e., by estimating regression (1). Results are presented in Table 2. I alternate the model specification by excluding and including the second and third power of covenant controls. Coefficients on the lagged violation indicator are the key estimates of interest. As a reference, I alternatively define *Violation* as any violations (not necessarily new violations) that occurred during the sample period for each firm, and report the results in the last two columns.

[Table 2 is here]

The coefficient on the lagged new violation indicator is -0.045 and increased to -0.050 after adding higher-order controls, both statistically significant at all conventional levels. The interpretation is that, on average, a violation that occurred a quarter before

[14] In order to clearly identify the effect of a violation, I require that firms not experience another violation in the 7-year window surrounding the event. Although this requirement restricts the analysis to a subsample, the following regression analysis examines the entire sample, including all violations.

would lead to a 4.5% to 5.0% reduction in accounts payable relative to industry mean. The contemporaneous indicators of new violation have smaller coefficients than lagged ones, suggesting an incremental effect immediately *after* the violation. Regressions using indicators of any violations have quantitatively similar results with those using indicators of new violations. I will only report the results for new violation indicators henceforth. The result indicates that, on average, covenant violations have a negative effect on the trade credit, consistent with the notion that banks' stronger control rights create conflicts of interest between creditors.

I then follow Roberts and Sufi (2009) to check whether the effect covenant violations is persistent. The regression specifications of Table 3 are identical to the specifications reported in columns (1) and (2) of Table 2, but include covenant violation indicators for four quarters and eight quarters after covenant violations, respectively. The sample size is smaller than that in Table 2 due to the simultaneous inclusion of lags. To save space, coefficients of variables other than the violation indicators are omitted.

[Table 3 is here]

Columns (1) and (2) present the results when including violation indicators for the past four quarters; column (2) additionally controls for 2nd and 3rd power of covenant controls. The results show that accounts payable drop continuously in the four quarters after the covenant violation, and all the coefficients remain statistically significant. In the last two columns, I simultaneously include violation indicators up to eight lags, and the results show that all eight indicators have negative and significant coefficients, indicating that the effect of covenant violations on trade credit persists for at least two years.

4.3 Propensity-score Matching Approach

To check the robustness of the baseline results, I re-estimate the effect of covenant violations using a propensity-score matching approach. The propensity-score approach allows us to control for a variety of observable traits between the two samples in a highly nonlinear and flexible fashion (Roberts and Whited, 2011).

To implement the propensity-score matching approach, I first estimate the probability of a firm violating a covenant as a function of firm size, calendar quarter fixed effects, fiscal quarter fixed effects, industry fixed effects, and the full set of covenant control variables, higher order covenant controls, and lagged covenant controls. Second, based on the estimated coefficients I compute the propensity score for each firm-quarter, and then find each violating firm a nearest neighbor non-violating firm in terms of propensity scores. If a firm from the non-violating group is matched with more than one violating firm, we keep the pair for which the distance between the two firms' propensity scores is the smallest. We end up with 3743 unique pairs of matched firms.

I then estimate the regression model (1) using propensity-score matched sample, and report the results in Table 4. Columns (1) and (2) include only violation indicators of periods t and $t-1$. After using the propensity-score matched sample, the estimates of the violation effect have larger magnitude than the baseline specification using the whole sample. The average decline of trade credit in the first quarter after the covenant violation is 8.6% relative to industry mean (the first two columns), and the coefficient is highly significant. Columns (3) and (4) include the violation indicators for four quarters after the covenant violation. The results show that the reduction is present and significant for all four post-violation quarters, and the magnitude of reduction shows a monotonically decreasing trend over the four quarters (especially after controlling for the higher-order

covenant controls). These findings are consistent with the baseline results, and suggest the adverse effect of covenant violations are very persistent, and gradually decays as time goes by.

[Table 4 is here]

In columns (5) and (6), as a further attempt to control for the unobserved attributes at the firm level that are related to both covenant violation and trade credit, I estimate regressions with firm fixed effects. The estimated coefficients on the first three violation dummies are negative and statistically significant. Their magnitude is around 0.03, which is smaller than those in the industry fixed-effects specifications, suggesting that covenant violations on average cause the trade credit of an individual firm to shrink by 3% relative to the firm-level average.

4.4 Difference-in-differences Approach

In Figure 1 we observe an increasing trend, and then a reversal, of accounts payable around covenant violations. It is possible that this reversal is caused by borrowers' financial distress, such that suppliers are affected by the contagion effect along the supply chain rather than the banks' control rights (Jorion and Zhang, 2009; Hertzel et al., 2008). This contagion channel implies that covenant violations coincide with a discontinuous change in borrower's distress risk. Although this is not consistent with the observation that covenant violation is hardly an indicator of real financial distress or filling for bankruptcy (Dichev and Skinner, 2002; Gopalakrishnan and Parkash, 1995), we cannot completely rule it out; it is still possible that some unobserved factors are causing covenant violations and trade credit declines at the same time (e.g., industry-wide shocks that cause shift of business focus).

To mitigate this concern, I estimate a difference-in-differences (DID) regression that exploits a control group with a parallel trend of trade credit. The control group consists of firms matched by propensity scores, the same approach as in section 4.3. These firms are those that barely pass the covenant threshold, and thus should be inherently similar to the violating firms. Figure 2 plots the median values of Accounts Payable/COGS around the new covenant violations for violating firms and matched firms, respectively. Quarter 0 means the fiscal quarter within which a new violation occurs. Before quarter 0, the median values for both groups of firms are trending up in a paralleled pattern; after the covenant violation, the median accounts payable of non-violating firms decline as well, but not as quickly as that of the violating firms. This reversal trend of control group can capture the exact effect we want to strip from our estimate: it can be highly attributable to unobserved changes caused by financial distress or industry-wide shocks that simultaneously determine covenant violations and trade credit. In other words, to the extent that changes in the trade credit of control firms represent the "true" trend the treated firms could have otherwise exhibited, the DID estimate can capture the unique effect of covenant violations on suppliers.

[Figure 2 and Table 5 is here]

The following regression model is estimated as a formal DID analysis.

$$\begin{aligned} AccountsPayable_{i,t} =& \alpha_0 + \beta_0 PostViolation_{i,t} + \beta_1 X_{i,t-1} + \beta_2 Z_{i,t} + \beta_3 Z_{i,t-1} \\ &+ \beta_4 HighOrder_{i,t-1} + \eta_i + \gamma_t + \nu_\tau + \epsilon_{i,t} \end{aligned} \tag{2}$$

where *PostViolation* is an indicator variable that equals one if a borrower is in its post-violation period, and zero otherwise. *PostViolation* is defined as zero for all firm-quarter observations of matched non-violating firms. Firm fixed effects (η_i) and fiscal and calendar

quarter fixed effects (γ_t and ν_τ) are controlled for to absorb the unobserved time-invariant firm characteristics and quarter-specific common shocks. β_0 is the DID estimate.

Estimation results are presented in Table 5. *PostViolation* dummy is defined as 4 quarters after covenant violations in the first two column and as 8 quarters after violations in the last two columns. The results indicate that during the 4 quarters after the covenant violation, on average a violating firm loses 3.4% more trade credit than the matched (non-violating) firm, controlling for high-order of lagged covenant variables. The results for 8 quarters after the covenant violation are statistically weaker, but still show that a violating firm has significantly lower trade credit than the control firm during the post-violation period. Overall, the DID analysis in this section confirms that after covenant violations, the trade credit of violating firms experiences a significant drop relative to that of non-violating firms, consistent with Hypothesis 1.

4.5 Regression Discontinuity with Dealscan Sample

This section isolates the analysis to a sample of loans for which we know the covenant thresholds, as well as any changes in those thresholds. The preceding results using the entire sample may suffer from model misspecification problem even though a polynomial function of trade credit is used to control for possible nonlinearity. Fortunately, once we focus on the subsample of firm-quarter observations that are "close" to the covenant threshold, the linearity assumption becomes increasingly mild. In addition, inferring covenant violations from observed cutoff points mitigates the concern that some covenant violations may not be captured by the text-search algorithm. Another advantage, which is especially important to the current study, is that borrowers just above and just below the violation threshold are very similar in terms of the degree of financial distress,

thus alleviating the concern that my findings are simply driven by the costs imposed by borrowers' financial distress.

Since violations of initial covenant thresholds are usually waived or revised (Denis and Wang, 2014), I focus on loan covenants that have available information about the changes in thresholds. This limits our attention to the following ten types of financial covenants: minimum interest coverage, minimum current ratio, minimum quick ratio, minimum fixed charge coverage, maximum debt to EBITDA, maximum debt to equity, maximum debt to tangible net worth, maximum leverage ratio, maximum CAPEX, and minimum EBITDA.[15] Calculations of these financial ratios closely follows Murfin (2012), and are displayed in Appendix.

Next, I compute the relative distance between the accounting variable and the corresponding covenant threshold, and restrict the sample to quarterly observations of non-financial firms that are close to violating thresholds in terms of the relative distance. The choice of this narrow window surrounding the thresholds, or, the bandwidth, is based on the formula of Silverman (1986), who identifies a robust measure of the optimal bandwidth for a unimodal distribution. The absolute value of relative distance for each observation is set to be smaller or equal to the optimal bandwidth. To ensure the robustness of the estimates, I also use several alternative choices of bandwidth.

The main variable of interest, $NewViolation$, is equal to one if the relative distance is negative and zero otherwise. I estimate a linear regression model of accounts payable on lagged $NewViolation$ dummy, the same control variables as in column (1) of Table 2, and firm and quarter fixed effects. Alternative functional form is examined as well

[15]Dealscan also specifies the thresholds for "minimum debt service coverage" and "maximum senior debt to EBITDA". However, since the definitions for debt service and senior debt are ambiguous, I exclude these two types from the sample. Networth covenants, due to the unavailability of threshold revision information, is excluded from the sample too.

by adding second and third power of control variables. Table 6 presents the estimation results. Due to the sample selection procedure, the sample size declines dramatically (the number of observations using optimal bandwidth accounts for less than 2% of the original sample size). The results from linear regression and polynomial model show that the decline in trade credit in the quarter immediately after the covenant violation is 6.1% and 6.6%, respectively, with statistical significance at 5% level. The economic magnitude of the estimates are larger than that in Table 4, the propensity-score matching approach. Further, I choose several arbitrary bandwidths and find that the choice of bandwidth have little effect on the results. As a demonstration, I report the estimates by using bandwidths of 0.20 and 0.30 on either side of the threshold (in columns (3) to (6)), which show quantitatively similar estimates with those under optimal bandwidth choice.

[Table 6 is here]

Overall, the regression discontinuity design using a narrow band surrounding the covenant thresholds generates consistent evidence with regression model using the entire sample, and thus confirms the conjecture that bank control rights after covenant violations cause trade credit to decline.

4.6 Manipulation and Selection Problems

The premise of a valid regression discontinuity design is that it is impossible for individuals to precisely manipulate the assignment variable, which in our case is the financial ratio on which the covenant is written. This condition would break down if either the financial ratio can be manipulated or the covenant threshold can be selected. The former case is normally known as managerial earnings management, and the latter case

occurs when banks selectively revise the covenant thresholds. This section discusses and addresses these two problems separately.

4.6.1 Managerial Earnings Management

Accounting literature argues that since technical defaults are costly, firms tend to manage earnings in order to avoid possible future debt covenant violations (Watts and Zimmerman, 1986; DeFond and Jiambalvo, 1994; Dichev and Skinner, 2002). This argument appears to weaken the validity of my empirical strategy in that firms on the two sides of the threshold are very different in terms of manager opportunism or preference that can also determines the trade credit.

To address this concern, I first incorporate into all regressions (using both the entire sample and the narrow-band sample) measures of abnormal accruals, which are used by the accounting literature to proxy for manipulation strategies, and show similar estimates of the impact of covenant violations on trade credit (reported in internet appendix). Second, I show that manipulating financial ratios precisely at the covenant threshold is difficult. One advantage of regression discontinuity design is its mild assumption about manipulation: as long as the assignment variable cannot be manipulated precisely, the treatment resembles a local randomized experiment (Lee and Lemieux, 2010).

[Figure 3 is here]

Borrowers will cluster on one side of the cutoff point if the manager can manipulate to the extent of staying "just above" the required minimum cutoff (or "just below" the maximum cutoff). To show the nonexistence of such a discontinuity, I plot the density distribution of borrower-quarter observations for different subsamples based on their respective financial ratios on which covenants are written. My focus is on loan covenants

whose financial ratios are associated with either income or investment variables that are more subject to manager's discretion.[16] In Figure 3, we can observe that none of the six density plots shows discontinuity in the narrow band surrounding the covenant threshold, consistent with the nonexistence of accurate manipulation of financial ratios.

There are several reasons for the nonexistence of manipulation when firms are close enough to covenant thresholds. First, as suggested by Chava and Roberts (2008) and Roberts and Sufi (2009), CFOs are required to submit periodic covenant compliance reports that discuss the computation of each financial covenant in great detail, which leaves little room for any subtle manipulation at the threshold, not to mention that lead banks of syndicated loans usually have significant experience in originating and monitoring loans and are well aware of possible accounting manipulations. Second, accounting manipulation, once revealed, usually involves substantial reputation loss. On one hand, lending process is a repeated game, any borrower deception is at the expense of future borrowing opportunities. On the other hand, market imposes huge penalties on financial misconduct. Karpoff et al. (2008) find that for each dollar that a firm misleadingly inflates its market value, on average, it loses this dollar when its misconduct is revealed, plus an additional $3.08.

4.6.2 Banks' Selection on Covenant Thresholds

Borrowers' covenant violations often trigger renegotiations with banks, which can lead to revisions of covenant thresholds. Revisions are not random. Banks can repeatedly discover new information through renegotiations, based on which they selectively decide the new covenant threshold (Garleanu and Zwiebel, 2009). In this case, the occurrence of

[16] Graham, Harvey, and Rajgopal (2005) suggest that investment is more subject to manager manipulation than accounting statements. Excluding the subsample of CAPEX covenants has little effect on our results.

eventual covenant violation is confounded with some key information about the borrower, which may meanwhile determine borrower's trade credit.

This is not a major problem for my identification strategy as long as the borrowers just above and just below the revised covenant thresholds are similar. That is, banks cannot set such a perfect cutoff point that slightly falling below the required minimum value signals a major fundamental difference, which is a feasible assumption since even the borrower itself is not able to do so. Moreover, SEC requires that all public firms file detailed credit agreements and report any material changes to these credit agreements (Denis and Wang, 2014); suppliers thus are able to observe the changes in covenant thresholds and do not have to wait until the occurrence of covenant violation to respond.

Despite the above arguments, I design two tests to mitigate the concern of the selection problem. First, I explicitly control for observed determinants of banks' decision to relax or tighten the covenant thresholds. Denis and Wang (2014) find that changes in covenant limits are determined by borrowers' investment opportunity and expected profitability. I incorporate, into my model, these two factors and their interaction terms with firms' corresponding distance to covenant thresholds. Coefficients on the interaction terms indicate the incremental effects of the two determinants when firms are approaching the thresholds. The results from these additional regressions are quantitatively similar to our previous estimates and are not tabulated here.

Second, to control for unobserved information acquired by banks when selecting the thresholds, I estimate a Heckman selection model. The first step uses a probit model in which the dependent variable equals one if the firm violates a debt covenant in a certain fiscal quarter and zero otherwise. Banks' geographic proximity to the borrower and loan concentration ratio (Herfindahl index) are used as instruments in the first-stage regression, since they reflect the information advantage of banks but are not directly related to trade

credit. The second step uses the sample of violating firms and estimates an OLS regression with the inverse Mill's ratio, estimated from the first step, as an explanatory variable. Findings from this Heckman selection model consistently show that once the borrower newly violates debt covenant(s), trade credit would experience a significant decline. The detailed procedure and results are presented in internet appendix.

4.7 Cross-sectional Analyses

As discussed in the section of hypotheses, conflicts of interest caused by banks' control right (loan acceleration and liquidation) are likely to be the main channel through which covenant violations impact trade creditors. If this argument is valid, the reduction in trade credit should be larger when banks are more likely to exercise their control rights. I test this hypothesis in this section. All regressions are estimated using the propensity-score matched sample, with the full set of controls including the high-order covenant covariates.

Hypothesis 2 argues that when the borrower has poorer credit quality or a larger balance of short-term debt, bank is more likely to accelerate loans or force liquidation. I use book value of assets, presence of investment-grade rating, and distance-to-default to proxy for credit quality, and the ratio of short-term liabilities to total debt value for debt maturity structure. Investment-grade rating refers to long-term credit rating above $BBB-$ (inclusive). Distance-to-default (DtD) is estimated using the Merton (1974) model which views equity as a call option on the value of the firm with a strike price equal to the face value of the firm's debt. All proxies are measured at the end of last quarter.

The sample is split into two groups based on each measure of credit quality and debt maturity structure. Firms with book value of assets larger than the sample median, with an investment-grade rating, and with DtD larger than the sample median are defined as

those with better creditworthiness; and firms whose short-term debt value divided by total debt value is larger than the sample median are defined as those with more short-term debt claims. At last, I estimate equation (1) in these separate subsamples.

[Table 7 is here]

Table 7 reports the results. I omit the coefficients on control variables for the ease of presentation. Coefficients on lagged violation indicator is negative and statistically significant only for firms with smaller book assets, without an investment-grade rating, and with small DtD. I test whether the differences between the estimates of subsamples are different from zero, and find that all differences are statistically significant. These results are consistent with the hypothesis that the reduction in trade credit is larger when banks are more likely to exercise their control rights.

Results based on debt maturity structure, on the other hand, show that both groups of firms experience significant drop in trade credit. However, the magnitude of coefficient is much smaller when more short-term debt claims are present, and the difference in coefficients are statistically significant at all conventional levels. This evidence is consistent with the idea that more short-term debts imply a higher likelihood of loan acceleration, which leads to a larger decline of trade credit.

4.8 Covenant Violations and Suppliers' Firm Value

To examine the value implications of covenant violations on trade creditors, I test Hypothesis 3 that dependent suppliers' firm value is negatively affected by their customers' covenant violations. The primary reason is that the dependent supplier has to trade off

the expected loss from debt acceleration and the cost of switching to a new customer once a major customer violates debt covenant, and they cannot avoid both.

4.8.1 Operating Performance of Suppliers

I first investigates the response of dependent suppliers' operating performance and capital expenditures to their customers' covenant violation. Faced with the threat of loan acceleration and borrower bankruptcy, suppliers are expected to cut off trade credit and relationship-specific investment, and transfer their sales to another customer. Since it is costly and takes time to sell products to a new customer, dependent suppliers would suffer from declining sales and have to cut back physical investment.[17]

To identify firm's dependent suppliers, I use the dataset containing supplier-customer link for Compustat firms between 1980 and 2004. Cohen and Frazzini (2008) collect this dataset from Compustat segment file using a phonetic string matching algorithm and a hand-match process. This supplier-customer data are featured by principal customers, as firms are required to report only customers representing more than 10% of their total sales.[18] In other words, all suppliers reporting a customer in this sample are dependent suppliers.

Using this dataset, I link all the dependent suppliers to my sample firms, and then aggregate for each supplier the number of covenant violations by all its customers at each quarter end. In this manner, I obtain 14372 supplier-quarter observations with customer violation information. Next, I define *Customer Viol.* as an indicator variable that equals one if at least one customer violated a debt covenant in the last quarter, and zero oth-

[17] A premise of this argument is that it is also difficult to sell products to current customer by receiving cash, which is reasonable considering that the customer is in violation of debt covenant and faced with senior creditors' eager demand for cash repayments.

[18] Prior to 1997, Regulation SFAS No. 14 governed segment disclosure. SFAS No. 131, issued by the FASB in June 1997, has been effective for fiscal years beginning after December 15, 1997.

erwise. Similarly, *Cust. New Viol.* equals one if at least one customer newly violated a debt covenant in the last quarter and zero otherwise, where "new violation" is a violation not proceeded by any violation in prior four quarters. Then I regress supplier's operating performance measures (sales and ROA) and investment on the indicator variable together with other control variables. Requiring the availability of control variables further reduces the sample size. Supplier fixed effects are included to absorb the effect of unobserved supplier traits on both customer covenant violations and supplier performance.

[Table 8 is here]

Panel A of Table 8 reports the results. One quarter after the covenant violation(s) by their customer(s), suppliers' total sales decline by around 9.9%; if the violation is new in the past four quarters, the decline is larger, by 10.4%. Since in my sample an average customer accounts for 17.6% percent of its supplier's total sales, the results imply, on average, a more than 50% reduction in trading volume between the supplier and the violating customer.[19] This is economically a very large estimate. However, relationship-specific and customer characteristics omitted in the regression could bias the estimate upward. Below I further control for relationship fixed effects and customer characteristics in addressing endogeneity problems, and the estimate is much smaller. Panel A also shows that suppliers' ROA will decline at the rate of 0.3% one quarter after the customer violation(s), and the reduction is larger for new violations (0.5%).

I also examine the implication on supplers' investment policy. Panel A of Table 8 shows that supplier's capital expenditures decline after a new violation from its customer, and the reduction is around 0.5% of book assets and statistically significant. This evidence is consistent with the idea that faced with declining sales from the existing customer and

[19]There are 7 cases where violations are from multiple customers in a single quarter, which will slightly bias the estimates upward.

the costs of switching to the next, a dependent supplier have to tailor their production capacity by cutting down capital expenditures.

Endogeneity problems could bias our estimates. The business of suppliers and that of their customers are closely tied to each other, thus are exposed to some common shocks. For example, poor investment opportunities in the customer's industry are likely to cause the customer to violate debt covenants, and meanwhile lead to poor performance of the supplier whose sales largely depend on that industry. To attenuate the concern that some unobservable attributes could bias our results, I further incorporate the following controls in the regression: (1) (customer's) industry-quarter fixed effects that control for the industry wide shock; (2) supplier-customer pair fixed effects, which control for the time-invariant characteristics of the trading relationship; and (3) customer's covenant controls that absorb customer's performance relative to the covenant violation threshold. Standard errors are clustered at the pair level and the results are reported in Panel B of Table 8. Compared with the results in Panel A, while the estimates of violation's effect on supplier's change in sales are smaller in magnitude, which indicate a 6% to 8% decline, those on ROA and capital expenditures are quantitatively similar.

Therefore, findings in this section suggest that covenant violations and associated conflicts of interest have a negative real effect on violating firms' suppliers, which does not appear to be driven by endogeneity.

4.8.2 Long-run Stock Returns of Suppliers

Stock return performance can generate implications on shareholder wealth, thus I explore the impact of customers' covenant violations on suppliers' stock returns. I use a calendar-time portfolio approach proposed by Mitchell and Stafford (2000). Recently, Nini

et al. (2012) apply the same approach in analysing the effect of debt covenant violations by estimating the monthly abnormal portfolio returns of violating and non-violating firms, respectively.

I construct two portfolios of suppliers. *Costumer Vol.* supplier portfolio is created by buying all stocks whose customers experience a covenant violation at the beginning of the month following the report of the violation and holding the stocks in a value-weighted portfolio for k months before selling the stocks, where k is a pre-specified holding period. I examine holding periods of 12, 24, and 48 months, respectively. The portfolio is formed starting in July 1996 and rebalanced monthly to add new qualified suppliers each month and drop suppliers at the end of their holding period. The *No Costumer Vol.* supplier portfolio is constructed in the same way except for buying stocks with non-violating customers.

[Table 9 is here]

Next, I regress value-weighted monthly portfolio excess return on Fama-Frech-Carhart four-factors, and report the average monthly abnormal return α over different holding periods in Table 9. The premise is that the level of α measures the long-run stock return performance of the portfolio. Suppliers with violating customers have lower average monthly abnormal returns throughout all holding period specifications. The return difference peaks when the holding period is 12 months, and then decreases monotonically with the length of holding period.

Consistently, the long-short portfolio, long in the *No Costumer Vol.* supplier portfolio and short in the *Costumer Vol.* supplier portfolio, shows an average monthly abnormal return of 0.60% for 12-month holding period, and 0.16% for 48-month holding period. In annualized terms, the corresponding returns are 7.5% and 2.0% per year, respectively.

These results suggest that covenant violations of a major customer generate loss for the dependent suppliers in terms of their equity valuation.

Taken together, the operating performance and stock return results are consistent with Hypothesis 3 that the conflicts of interest between bank lenders and trade creditors destroy value for trade creditors when a large proportion of their sales depends on the major customers.

4.8.3 Cross-sectional Analyses for Suppliers' Value Destruction

According to Hypothesis 4, an increase in either the expected loss of loan acceleration or the switching costs is likely to exacerbate the negative impact of covenant violations on suppliers. I test this hypothesis by exploiting the cross-sectional variation in each of the two major costs.

The expected cost of loan acceleration is proxied by the size of total loans. *Ceteris paribus*, an increase in the amount of outstanding loans means more cash flows to be taken away by the bank once the loan is accelerated, thus jeopardizing other junior creditors' claims. I obtain information of loan size from the Dealscan database, and merge it with all customers in my sample. Then the sample is divided into two groups, based on the median value of loan to total debt ratio. Regression model with pairwise fixed effects is estimated on separate subsamples to investigate whether the negative impact on suppliers is more pronounced when the customer has more loans outstanding prior to violation.

Switching costs are higher for suppliers producing specialized products, since it is difficult for them to redeploy investments that are specific to major customers (Banerjee et al., 2008). Following Titman and Wessels (1988), I use R&D to sales ratio and SG&A to sales ratio to proxy for product uniqueness. First, firms that sell products with close

substitutes are likely to do less research and development since their innovations can be more easily duplicated. Second, firms with relatively unique products are expected to spend more in promoting and selling their products, which are reflected in SG&A expenses. I divide the sample by the median level of these two measures, and estimate the regression model with pairwise fixed effects on separate subsamples to show the cross-sectional difference in supplers' performance.

[Table 10 is here]

Table 10 reports the results. Control variables are the same as in Table 8 and are hereby omitted. It is shown that costumers' covenant violations negatively impact suppliers sales and ROA only when the outstanding loans take a larger proportion of total debt, consistent with the hypothesis that the negative impact of covenant violations is exacerbated by a larger expected cost of loan acceleration. The last two panels show that the negative effect of covenant violations on supplier performance is stronger when the supplier has higher R&D to sales ratio or higher SG&A to sales ratio, implying that suppliers with specialized products are more vulnerable to customers' covenant violations, as it is more costly for them to switch to another customer. All the differences of coefficients between subsamples are tested and are shown to be statistically different from zero.

5 Additional Tests: Borrower Firm Value and Bank's Ex Ante Response

I have shown that the banks' control rights are costly for trade creditors and lead to a reduction in trade credit. However, trade credit is an important source of external finance, especially when borrowers are experiencing negative liquidity shock (Cunat, 2007; Wilner,

2000; Garcia-Appendini and Montoriol-Garriga, 2013). Additionally, firms would suffer from increasing costs of bank finance after covenant violations (Freudenberg et al., 2013; Roberts and Sufi, 2009), making alternative source of finance, such as trade credit, more valuable. Therefore, the loss of trade credit after covenant violations is expected to incur costs to borrowers by impacting their real economic activities, unless they have financial slack.

In internet appendix, I examine how violation-induced reduction in trade credit affects borrowers' sales, inventory, investment, and ROA. The results show that violating firms that experience a larger violation-induced drop[20] in trade credit would cut back inventory, reduce physical investment, and suffer from contracted total sales and lower return on assets (ROA) during the two quarters after the covenant violation. Moreover, the negative effect of customer's covenant violation on net capital expenditures and ROA is exacerbated by the violating firm's degree of financial constraint (proxied by firm size, Whited-Wu index, and credit rating).

I also examine the effect of trade credit reduction on borrower firms' long-run stock return performance by using the calendar-time portfolio approach. Violating firms are divided into a group with larger violation-induced drop in trade credit and the other with smaller violation-induced drop. The result, reported in internet appendix, demonstrates that the former portfolio consistently under-performs the latter across various specifications of holding period, which also suggests that the value improvement effect of creditor control rights (documented by Nini et al., 2012) is only present when the drop in trade credit is small.

[20]The violation-induced drop in trade credit is a difference-in-differences estimate of Accounts Payable/COGS, and thus is less subject to endogeneity issue. Please see the details in the internet appendix.

Given the value implications of covenant violations and corresponding creditor control rights, an interesting question is, what is the implication for bank's ex ante response? In a lending relationship, suppose that bank rationally assesses how costly its ex post control rights are to both the supplier and the borrower, would the bank ex ante allow looser debt covenants or tolerate more rounds of renegotiations when an important dependent supplier is at stake? To conduct an empirical examination on this question, I measure the tightness of the loan covenant by "initial covenant slack", which is computed as the absolute value of the relative distance between actual financial ratio and the initial covenant threshold. Next, I conduct a package-level regression of initial covenant slack on an indicator of the presence of a dependent supplier at the beginning of the quarter, together with other borrower characteristics (same as in Table 2), covenant covariates, year and borrower industry fixed effects.[21] The result (not reported) shows that on average the pre-existence of a dependent supplier would increase the initial covenant slack by 4% (statistically significant at 10% level), which supports the idea that banks indeed take into account the consequences of their own control rights and ex ante adjust their choices in a rational manner.[22]

6 Conclusion

The value implications of creditors control rights in the presence of multiple classes of creditors have rarely been examined in empirical studies. This paper examines this issue by utilizing the setting of debt covenant violations, in which extra control rights are obtained by senior creditors. The findings indicate that suppliers respond to their

[21] To conduct package-level regression, I keep only borrower-quarter observations in which a lending package and corresponding covenants start to be effective.

[22] In equilibrium, it is possible that banks would compensate their "leniency" on covenant tightness by asking for concurrent tightening of other loan terms, which deserves further investigation.

customers' covenant violations by reducing trade credit in a sharp and persistent manner, and the reduction is larger when banks are more likely to exercise the control rights (e.g., accelerating loan repayments or liquidating the borrower). Importantly, suppliers whose sales largely depend on their customers suffer from poor operating performance and low equity valuation after their customers' covenant violations. These results support the idea that conflicts of interest between classes of creditors, caused by senior creditors' control rights, impose real costs on junior creditors.

References

Ayotte, Kenneth M., and Edward R. Morrison, 2009, Creditor control and conflict in chapter 11, *Journal of Legal Analysis* 1, 511–551.

Banerjee, Shantanu, Sudipto Dasgupta, and Yungsan Kim, 2005, Buyer-supplier relationships and trade credit, working paper.

Banerjee, Shantanu, Sudipto Dasgupta, and Yungsan Kim, 2008, Buyersupplier relationships and the stakeholder theory of capital structure, *The Journal of Finance* 63, 2507–2552.

Beatty, Anne, Scott Liao, and Joseph Weber, 2012, Evidence on the determinants and economic consequences of delegated monitoring, *Journal of Accounting and Economics* 53, 555 – 576.

Begley, Joy, and Ruth Freedman, 2004, The changing role of accounting numbers in public lending agreements, *Accounting Horizons* 18, 81–96.

Bulow, Jeremy I., and John B. Shoven, 1978, The bankruptcy decision, *The Bell Journal of Economics* 9, pp. 437–456.

Burkart, Mike, and Tore Ellingsen, 2004, In-kind finance: A theory of trade credit, *American Economic Review* 94, 569–590.

Chava, Sudheer, and Michael R. Roberts, 2008, How does financing impact investment? the role of debt covenants, *The Journal of Finance* 63, 2085–2121.

Chen, Yeh-Ning, 2005, Debt seniority and the lenders' incentive to monitor: Why isn't trade credit senior?, working paper.

Cohen, Lauren, and Andrea Frazzini, 2008, Economic links and predictable returns, *The Journal of Finance* 63, 1977–2011.

Cunat, Vicente, 2007, Trade credit: Suppliers as debt collectors and insurance providers, *Review of Financial Studies* 20, 491–527.

DeFond, Mark L., and James Jiambalvo, 1994, Debt covenant violation and manipulation of accruals, *Journal of Accounting and Economics* 17, 145 – 176.

Denis, David J., and Jing Wang, 2014, Debt covenant renegotiations and creditor control rights, *Journal of Financial Economics* 113, 348 – 367.

Diamond, Douglas W., 1984, Financial intermediation and delegated monitoring, *The Review of Economic Studies* 51, 393–414.

Dichev, Ilia D., and Douglas J. Skinner, 2002, Large sample evidence on the debt covenant hypothesis, *Journal of Accounting Research* 40, 1091–1123.

Eckbo, B. Espen, Karin S. Thorburn, and Wei Wang, 2014, How costly is corporate bankruptcy for top executives?, working paper.

Fama, Eugene F., 1990, Contract costs and financing decisions, *The Journal of Business* 63, pp. S71–S91.

Ferreira, Daniel, Miguel Ferreira, and Beatriz Mariano, 2014, Unfriendly creditors: Debt covenants and board independence, working paper.

Ferris, J. Stephen, 1981, A transactions theory of trade credit use, *The Quarterly Journal of Economics* 96, pp. 243–270.

Frank, Murray Z., and Vidhan K. Goyal, 2008, *Handbook of Corporate Finance: Empirical Corporate Finance*, volume 2, chapter Trade-off and Pecking Order Theories of Debt (Amsterdam, Elsevier/North-Holland).

Franks, Julian R., and Kjell G. Nyborg, 1996, Control rights, debt structure, and the loss of private benefits: the case of the uk insolvency code, *Review of Financial Studies* 9, 1165–1210.

Freudenberg, Felix, Bjorn Imbierowicz, Anthony Saunders, and Sascha Steffen, 2013, Covenant violations and dynamic loan contracting, working paper.

Garcia-Appendini, Emilia, and Judit Montoriol-Garriga, 2013, Firms as liquidity providers: Evidence from the 2007-2008 financial crisis, *Journal of Financial Economics* 109, 272 – 291.

Garcia-Appendini, Emilia, and Judit Montoriol-Garriga, 2014, Trade credit and financial distress, working paper.

Garleanu, Nicolae, and Jeffrey Zwiebel, 2009, Design and renegotiation of debt covenants, *Review of Financial Studies* 22, 749–781.

Gopalakrishnan, V., and Mohinder Parkash, 1995, Borrower and lender perceptions of accounting information in corporate lending agreements, *Accounting Horizons* 9, 1326.

Graham, John R., 2000, How big are the tax benefits of debt?, *The Journal of Finance* 55, 1901–1941.

Gu, Yuqi, Connie X. Mao, and Xuan Tian, 2013, Creditor interventions and firm innovation: Evidence from debt covenant violations, working paper.

Hertzel, Michael G., Zhi Li, Micah S. Officer, and Kimberly J. Rodgers, 2008, Inter-firm linkages and the wealth effects of financial distress along the supply chain, *Journal of Financial Economics* 87, 374 – 387.

Jorion, Philippe, and Gaiyan Zhang, 2009, Credit contagion from counterparty risk, *The Journal of Finance* 64, 2053–2087.

Karpoff, Jonathan M., D. Scott Lee, and Gerald S. Martin, 2008, The cost to firms of cooking the books, *Journal of Financial and Quantitative Analysis* 43, 581–611.

Klapper, Leora, Luc Laeven, and Raghuram Rajan, 2012, Trade credit contracts, *Review of Financial Studies* 25, 838–867.

Lee, David S., and Thomas Lemieux, 2010, Regression discontinuity designs in economics, *Journal of Economic Literature* 48, 281355.

Li, Kai, and Wei Wang, 2014, Creditor control through loan-to-loan and loan-to-own, working paper.

Love, Inessa, Lorenzo A. Preve, and Virginia Sarria-Allende, 2007, Trade credit and bank credit: Evidence from recent financial crises, *Journal of Financial Economics* 83, 453–469.

Merton, Robert C, 1974, On the pricing of corporate debt: The risk structure of interest rates, *The Journal of Finance* 29, 449–470.

Mitchell, Mark L, and Erik Stafford, 2000, Managerial decisions and long-term stock price performance, *The Journal of Business* 73, 287–329.

Molina, Carlos A., and Lorenzo A. Preve, 2012, An empirical analysis of the effect of financial distress on trade credit, *Financial Management* 41, 187–205.

Murfin, Justin, 2012, The supply-side determinants of loan contract strictness, *The Journal of Finance* 67, 1565–1601.

Ng, Chee K., Janet Kiholm Smith, and Richard L. Smith, 1999, Evidence on the determinants of credit terms used in interfirm trade, *The Journal of Finance* 54, pp. 1109–1129.

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

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

Roberts, Michael R., and Tony Whited, 2011, *Endogeneity in empirical corporate finance*, volume 2 of *Handbook of the Economics of Finance* (Amsterdam: Elsevier).

Silverman, Bernard W., 1986, *Density Estimation for Statistics and Data Analysis* (Chapman & Hall, London).

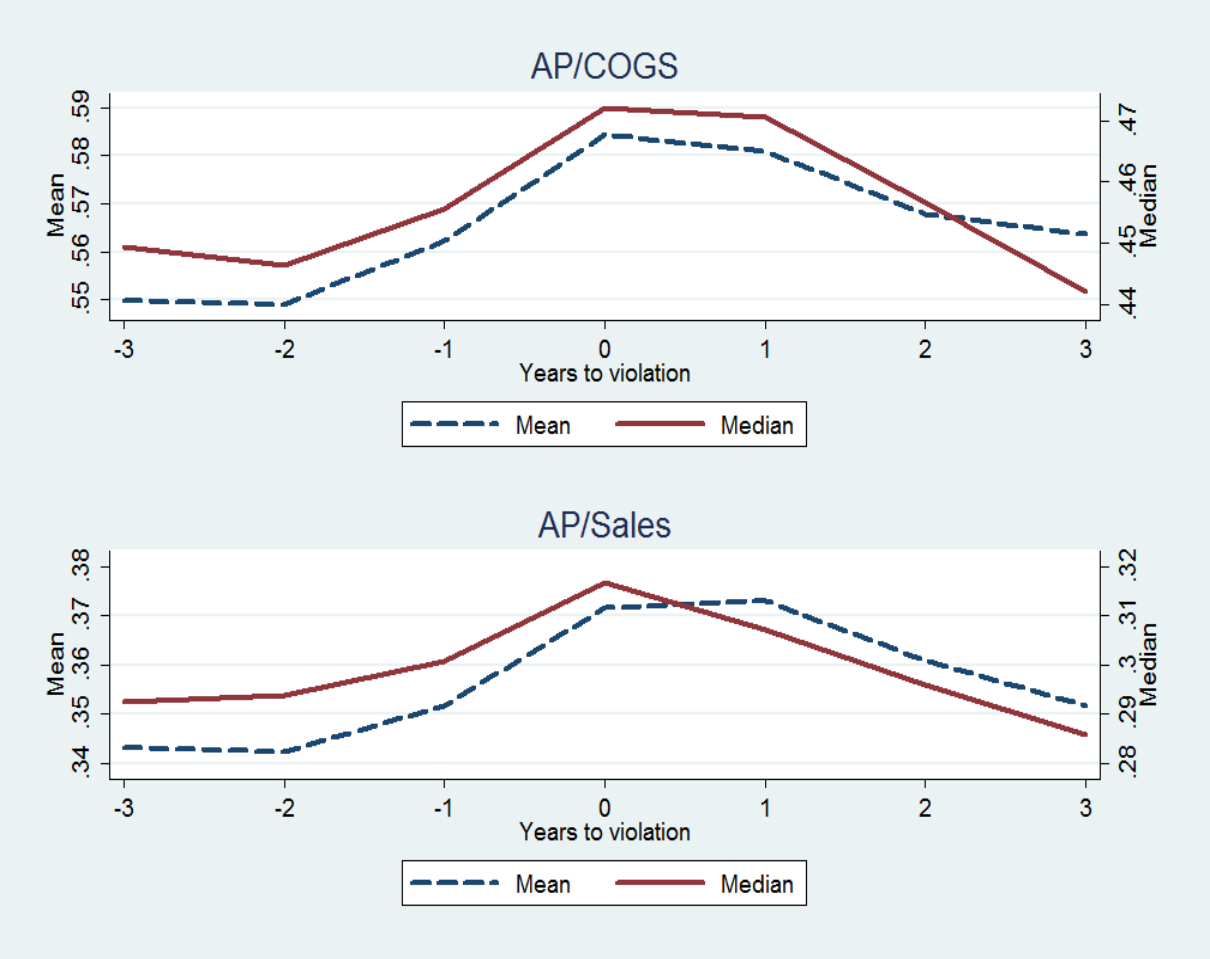
Tan, Liang, 2013, Creditor control rights, state of nature verification, and financial reporting conservatism, *Journal of Accounting and Economics* 55, 1 – 22.

Titman, Sheridan, and Roberto Wessels, 1988, The determinants of capital structure choice, *The Journal of Finance* 43, pp. 1–19.

Watts, R.L., and J.L. Zimmerman, 1986, *Positive accounting theory*, Contemporary topics in accounting series (Englewood Cliffs, NJ: Prentice Hall).

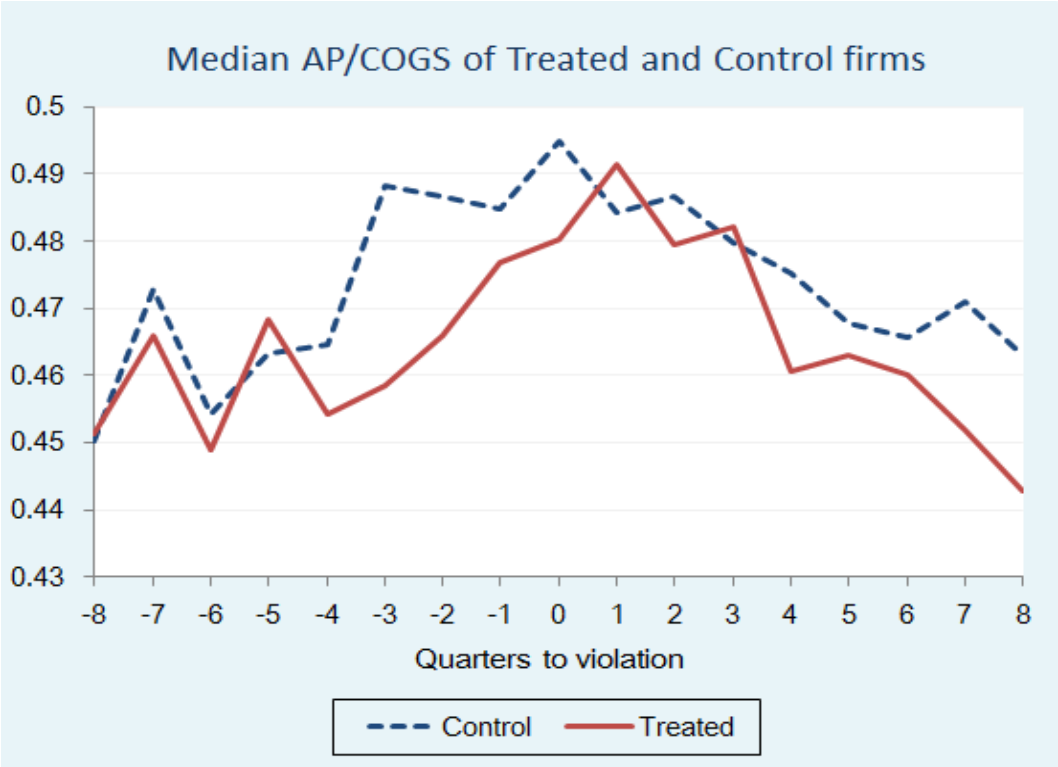
Wilner, Benjamin S., 2000, The exploitation of relationships in financial distress: The case of trade credit, *The Journal of Finance* 55, 153–178.

Figure 1: **Accounts Payable around Covenant Violations**



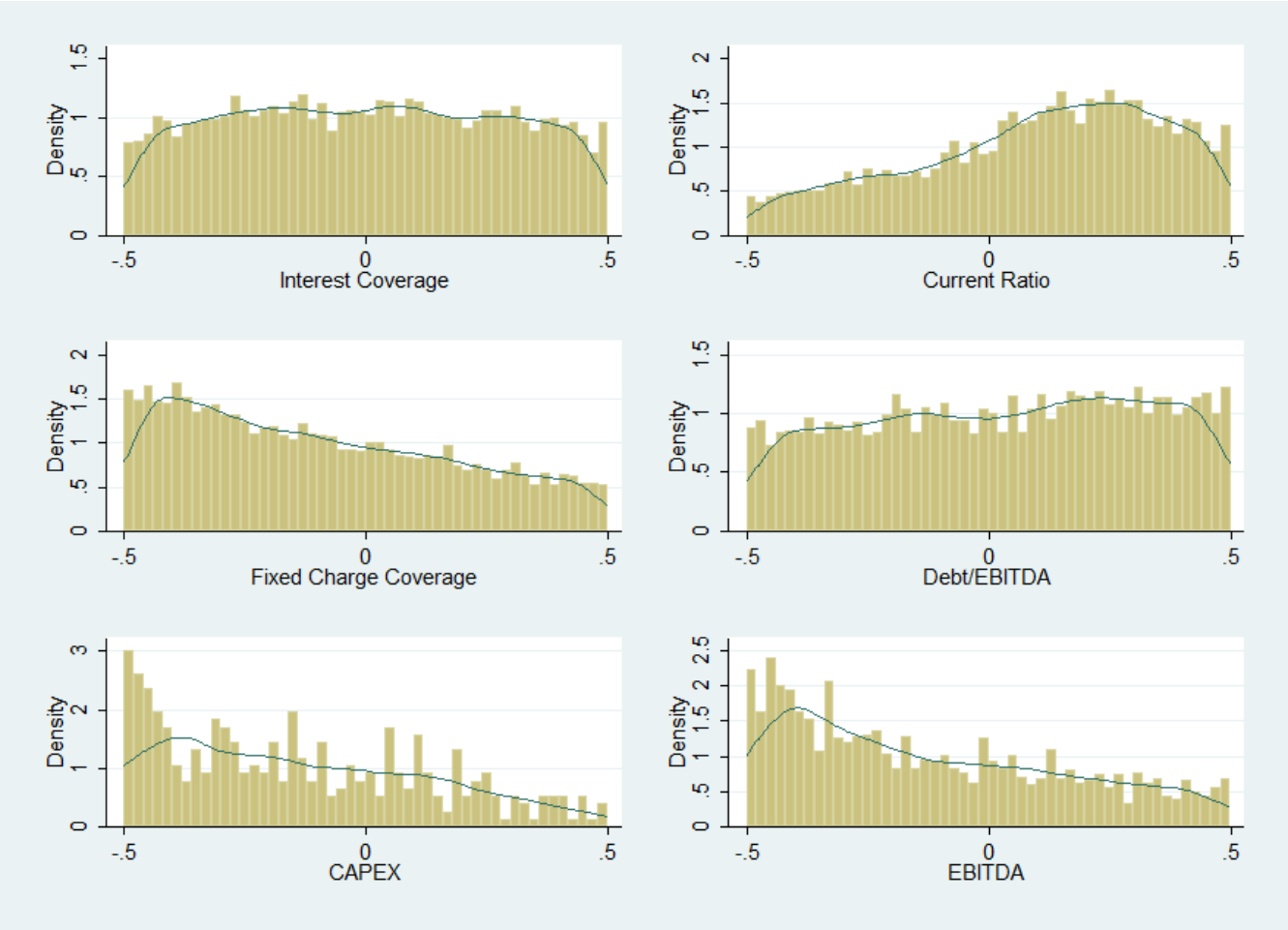
This figure presents means and medians for measures of accounts payable around a new financial covenant violation. A new violation is a violation by a firm that has not violated in the previous four quarters. Year 0 denotes a fiscal year during which a new violation occurs.

Figure 2: **Accounts Payable of Violating Firms and Matched Firms**



This figure presents medians of Accounts Payable/COGS for treated firms (violating firms) and control firms (matched non-violating firms) around a new financial covenant violation. A new violation is a violation by a firm that has not violated in the previous four quarters. Quarter 0 denotes a fiscal quarter during which a new violation occurs.

Figure 3: **Density Distribution of Borrowers around the Covenant Threshold**



The vertical axis indicates the density, and the horizontal axis indicates the relative distance to the covenant threshold. For example, for the current ratio sample, I calculate the distance as: (Actual current ratio−Threshold current ratio)/Threshold current ratio.

Table 1: **Summary Statistics**

This table presents summary statistics - averages, 25th percentile, medians, and 75th percentile - for the unbalanced panel of 10258 firms from 1996 to 2008. Variable definitions appear in Appendix A.

	Mean	25th	Median	75th
Accounts Payable/COGS	0.71	0.28	0.46	0.79
Accounts Payable/COGS$_{adj}$	0.71	0.26	0.45	0.80
Accounts Receivable/Sales	0.64	0.39	0.60	0.83
Assets ($ M)	1553	19.52	101.6	543.5
CashHolding	0.20	0.02	0.09	0.31
ROA	-0.01	-0.02	0.02	0.04
Tangibility	0.26	0.07	0.18	0.38
CAPEX	0.03	0.00	0.01	0.02
Leverage	0.25	0.02	0.19	0.39
Market/Book	2.38	0.84	1.35	2.60
CashFlow	-0.02	-0.03	0.01	0.03
InterestExp.	0.01	0.00	0.00	0.01
NetWorth	0.44	0.29	0.49	0.71
CurrentRatio	2.66	1.11	1.89	3.29

Table 2: **Baseline Regressions: Covenant Violations and Trade Credit**

The sample consists of firm-quarter observations between 1996 and 2008. The dependent variable is Accounts Payable/COGS. Variable definitions appear in Appendix A. All specifications include industry indicator variables, calendar year-quarter indicator variables and fiscal quarter indicator variables. t-statistics (reported in parentheses) are based on standard errors adjusted for within-firm correlation. ***, **, and * indicate statistics significance at 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
New Violation(t-1)	-0.045***	-0.050***		
	(-4.148)	(-4.667)		
New Violation(t)	-0.037***	-0.028***		
	(-3.534)	(-2.708)		
Violation(t-1)			-0.038***	-0.052***
			(-4.601)	(-6.249)
Violation(t)			-0.037***	-0.040***
			(-4.750)	(-5.137)
Size(t-1)	-0.026***	-0.018***	-0.026***	-0.018***
	(-6.957)	(-4.909)	(-7.082)	(-5.063)
Tangibility(t-1)	-0.031	-0.087**	-0.033	-0.090***
	(-0.882)	(-2.525)	(-0.932)	(-2.595)
CashHolding(t-1)	0.124***	0.157***	0.121***	0.154***
	(3.426)	(4.388)	(3.344)	(4.298)
ROA(t-1)	-1.077***	-1.002***	-1.071***	-0.979***
	(-10.700)	(-9.426)	(-10.647)	(-9.201)
CAPEX	-0.117	-0.097	-0.120	-0.101
	(-1.579)	(-1.315)	(-1.618)	(-1.379)
CAPEX (t-1)	0.126*	0.106	0.127*	0.108
	(1.843)	(1.574)	(1.854)	(1.595)
Leverage	-0.061*	-0.040	-0.063*	-0.042
	(-1.717)	(-1.089)	(-1.764)	(-1.146)
Market/Book	-0.002	-0.007***	-0.003	-0.007***
	(-0.949)	(-2.833)	(-1.147)	(-2.965)
CashFlow	-0.310***	-0.251***	-0.313***	-0.254***
	(-6.414)	(-5.134)	(-6.472)	(-5.199)

Table 2 Continued

InterestExp.	1.629*	0.854	1.715*	0.930
	(1.777)	(0.921)	(1.870)	(1.004)
NetWorth	-0.210***	-0.210***	-0.214***	-0.214***
	(-7.229)	(-7.042)	(-7.363)	(-7.178)
CurrentRatio	-0.048***	-0.048***	-0.048***	-0.048***
	(-20.468)	(-20.507)	(-20.521)	(-20.591)
Leverage(t-1)	-0.174***	0.274*	-0.168***	0.287*
	(-4.755)	(1.864)	(-4.599)	(1.959)
Market/Book(t-1)	0.022***	0.086***	0.021***	0.082***
	(9.668)	(6.050)	(9.466)	(5.765)
CashFlow(t-1)	-0.159**	-0.574***	-0.168**	-0.637***
	(-2.391)	(-4.171)	(-2.521)	(-4.597)
InterestExp.(t-1)	3.025***	-17.294***	3.050***	-17.532***
	(3.904)	(-3.670)	(3.940)	(-3.725)
NetWorth(t-1)	-0.050	0.067	-0.046	0.070
	(-1.633)	(1.422)	(-1.507)	(1.477)
CurrentRatio(t-1)	0.008***	-0.318***	0.008***	-0.322***
	(2.795)	(-15.544)	(2.702)	(-15.726)
High-order controls	No	Yes	No	Yes
Adj. R-sq	0.222	0.239	0.222	0.239
N. of Obs.	172,805	172,805	172,805	172,805

Table 3: **Long-Run Effect of Covenant Violations**

The sample consists of firm-quarter observations between 1996 and 2008. The dependent variable is Accounts Payable/COGS. Variable definitions appear in Appendix A. All specifications include industry indicator variables, calendar year-quarter indicator variables and fiscal quarter indicator variables. t-statistics (reported in parentheses) are based on standard errors adjusted for within-firm correlation. ***, **, and * indicate statistics significance at 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
New Violation(t-1)	-0.044***	-0.049***	-0.046***	-0.050***
	(-3.950)	(-4.413)	(-3.747)	(-4.036)
New Violation(t-2)	-0.048***	-0.051***	-0.046***	-0.048***
	(-4.372)	(-4.599)	(-3.810)	(-3.963)
New Violation(t-3)	-0.041***	-0.039***	-0.043***	-0.041***
	(-3.549)	(-3.409)	(-3.503)	(-3.337)
New Violation(t-4)	-0.048***	-0.041***	-0.046***	-0.039***
	(-4.116)	(-3.581)	(-3.660)	(-3.125)
New Violation(t-5)			-0.057***	-0.052***
			(-4.332)	(-3.933)
New Violation(t-6)			-0.049***	-0.042***
			(-3.660)	(-3.113)
New Violation(t-7)			-0.051***	-0.039***
			(-3.707)	(-2.825)
New Violation(t-8)			-0.056***	-0.043***
			(-4.279)	(-3.304)
Other controls	Yes	Yes	Yes	Yes
High-order controls	No	Yes	No	Yes
Adj. R-sq	0.227	0.244	0.231	0.248
N. of Obs.	156,108	156,108	126,304	126,304

Table 4: **Baseline Regressions using Propensity-Score Matched Sample**

The sample consists of firm-quarter observations between 1996 and 2008, in which the violating firms are matched with non-violating firms based on their propensity scores of covenant violation. To obtain propensity scores, I first estimate the probability of a firm violating a covenant as a function of firm size, calendar quarter fixed effects, fiscal quarter fixed effects, industry fixed effects, and the full set of covenant control variables, higher order covenant controls, and lagged covenant controls. Covenant controls are the six variables: operating cash flow scaled by average assets, the leverage ratio, the ratio of interest expense to average assets, the ratio of net worth to total assets, the current ratio, and the market-to-book ratio. Higher order covenant controls refers to the second and third power of the covenant control variables; lagged covenant controls refers to the 4-quarter lagged level of the covenant control variables. Second, based on the estimated coefficients I calculate the propensity score for each firm-quarter, and then find each violating firm a nearest neighbor non-violating firm in terms of the propensity score. The second stage regression uses the matched sample. The dependent variable is Accounts Payable/COGS. Variable definitions appear in Appendix A. All specifications include industry indicator variables, calendar year-quarter indicator variables and fiscal quarter indicator variables. t-statistics (reported in parentheses) are based on standard errors adjusted for within-firm correlation. ***, **, and * indicate statistics significance at 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
New Violation(t)	-0.065***	-0.053***				
	(-4.305)	(-3.631)				
New Violation(t-1)	-0.086***	-0.086***	-0.092***	-0.092***	-0.028**	-0.030**
	(-5.391)	(-5.423)	(-5.341)	(-5.364)	(-2.133)	(-2.282)
New Violation(t-2)			-0.084***	-0.083***	-0.031**	-0.034**
			(-4.724)	(-4.708)	(-2.364)	(-2.533)
New Violation(t-3)			-0.080***	-0.077***	-0.031**	-0.034**
			(-4.414)	(-4.338)	(-2.083)	(-2.291)
New Violation(t-4)			-0.082***	-0.076***	-0.020	-0.022
			(-4.430)	(-4.118)	(-1.308)	(-1.436)
Industry FE	Yes	Yes	Yes	Yes	No	No
Firm FE	No	No	No	No	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
High-order controls	No	Yes	No	Yes	Yes	Yes
Adj. R-sq	0.253	0.270	0.256	0.273	0.629	0.631
N. of Obs.	37,938	37,938	36,569	36,569	36,569	36,569

Table 5: **Difference-in-differences Regressions using Propensity-Score Matched Sample**

The sample consists of firm-quarter observations between 1996 and 2008, in which the violating firms are matched with non-violating firms based on their propensity scores of covenant violation. Propensity score math procedure follows from Table 4. The DID regression uses the matched sample. The dependent variable is Accounts Payable/COGS. PostViolation equals one for violating firms that are in their post-violation period (4 quarters and 8 quarters after violations, respectively). Variable definitions appear in Appendix A. All specifications include firm indicator variables, calendar year-quarter indicator variables and fiscal quarter indicator variables. t-statistics (reported in parentheses) are based on standard errors adjusted for within-firm correlation. ***, **, and * indicate statistics significance at 1%, 5%, and 10% level, respectively.

	(1)	(2)	(1)	(2)
Post-violation(4 quarters)	-0.031***	-0.034***		
	(-2.843)	(-3.115)		
Post-violation(8 quarters)			-0.031*	-0.031*
			(-1.780)	(-1.879)
Other controls	Yes	Yes	Yes	Yes
High-order controls	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Adj. R-sq	0.621	0.623	0.604	0.608
N.of Obs.	37,674	37,674	32,810	32,810

Table 6: **Discontinuity Design using Dealscan Sample**

This table estimates "local" discontinuity design using a sample of non-financial firms whose loan covenant thresholds can be identified by Dealscan database during 1996 to 2008. To be included in the sample, a borrower must have information available in Dealscan about whether the covenant is revised or not. The dependent variable is Accounts Payable/COGS. Variable definitions appear in Appendix A. All specifications include firm and quarter fixed effects. t-statistics (reported in parentheses) are based on standard errors adjusted for within-firm correlation. ***, **, and * indicate statistics significance at 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Optimal Bandwith		Bandwith=0.20		Bandwith=0.30	
New Violation(t-1)	-0.061**	-0.066**	-0.045*	-0.052**	-0.058**	-0.062**
	(-2.442)	(-2.549)	(-1.781)	(-2.003)	(-2.050)	(-2.110)
Size(t-1)	-0.042	-0.041	-0.025	-0.027	-0.038	-0.034
	(-0.869)	(-0.908)	(-0.689)	(-0.804)	(-0.906)	(-0.844)
Tangibility(t-1)	-0.017	0.005	0.016	0.019	0.256	0.242
	(-0.054)	(0.016)	(0.056)	(0.068)	(0.945)	(0.912)
CashHolding(t-1)	0.107	0.119	0.108	0.129	-0.006	-0.016
	(0.669)	(0.728)	(0.755)	(0.874)	(-0.041)	(-0.104)
ROA(t-1)	-0.389	-0.579	-0.747	-0.936*	-1.444**	-1.638**
	(-0.727)	(-1.129)	(-1.459)	(-1.871)	(-2.084)	(-2.293)
CAPEX(t-1)	0.241	0.229	0.529	0.447	1.311	1.286
	(0.279)	(0.276)	(0.687)	(0.602)	(1.417)	(1.392)
CAPEX	1.382*	1.397*	0.924	0.919	0.197	0.109
	(1.787)	(1.825)	(1.234)	(1.235)	(0.166)	(0.093)
Leverage(t-1)	0.141	0.636	0.068	0.625	0.016	0.181
	(0.615)	(1.295)	(0.345)	(1.428)	(0.072)	(0.376)
Market/Book(t-1)	0.009	0.017	0.011	0.074	0.040**	0.175
	(0.571)	(0.103)	(0.724)	(0.468)	(2.089)	(0.953)
CashFlow(t-1)	-0.051	0.207	0.356	0.674	1.031*	1.231
	(-0.124)	(0.307)	(0.938)	(1.097)	(1.782)	(1.564)
InterestExp.(t-1)	-5.602	-42.88**	-5.330	-32.61**	-2.809	-7.263
	(-0.898)	(-2.445)	(-1.053)	(-2.330)	(-0.579)	(-0.408)
NetWorth(t-1)	0.033	0.385	-0.063	0.496	0.095	0.098
	(0.141)	(0.637)	(-0.307)	(0.886)	(0.434)	(0.187)
CurrentRatio(t-1)	-0.020	-0.094	-0.019	-0.164	-0.005	-0.132
	(-1.332)	(-0.583)	(-1.409)	(-1.014)	(-0.327)	(-0.716)
High-order controls	No	Yes	No	Yes	No	Yes
Adj. R-sq	0.826	0.828	0.831	0.832	0.756	0.757
N.of Obs.	2,966	2,966	3,419	3,419	5,191	5,191

Table 7: **Cross-sectional Analyses of Trade Credit Reduction**

The sample consists of firm-quarter observations between 1996 and 2008, in which the violating firms are matched with non-violating firms based on their propensity scores of covenant violation. The matching procedure is described in Table 4. The dependent variable is Accounts Payable/COGS. "Small" ("Large") denotes the group of firms whose book assets are smaller (larger) than the sample median. "Rating" denotes the group of firms with a long-term rating above $BBB-$ and "No rating" denotes the group of firms with a rating below $BBB-$ or without a rating. Distance to Default is estimated using the Merton (1974) model which views equity as a call option on the value of the firm with a strike price equal to the face value of the firm's debt. ST Debt Ratio is short-term liabilities divided by total debt value. Variable definitions appear in Appendix A. All specifications include industry indicator variables, calendar year-quarter indicator variables and fiscal quarter indicator variables. t-statistics (reported in parentheses) are based on standard errors adjusted for within-firm correlation. ***, **, and * indicate statistics significance at 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Firm Size		Credit Rating		Distance to Default		ST Debt Ratio	
	Large	Small	Rating	No Rating	Large	Small	Low	High
New Violation(t-1)	-0.020	-0.110***	0.005	-0.093***	-0.002	-0.075***	-0.042**	-0.090***
	(-1.150)	(-4.396)	(0.159)	(-5.283)	(-0.075)	(-3.653)	(-1.995)	(-3.996)
New Violation(t)	-0.014	-0.063***	0.026	-0.061***	0.001	-0.072***	-0.033*	-0.065***
	(-0.826)	(-2.720)	(0.828)	(-3.803)	(0.027)	(-3.749)	(-1.697)	(-3.049)
All other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
All FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-sq	0.300	0.243	0.288	0.266	0.311	0.284	0.330	0.261
N. of Obs.	19,510	18,596	7,852	30,254	10280	16918	17,906	16,856

Table 8: **Customers' Covenant Violations and Supplier's Operating Performance**

This table presents the estimates of the impact of customers' covenant violations on operating performance and investment. The sample consists of supplier-quarter observations during 1996 to 2004. *Customer Violation*(*New Violation*) is an indicator variable that equals one if at least one customer (newly) violated a debt covenant in the last quarter, and zero otherwise. The dependent variables are ln(Sales), return on assets (ROA), and capital expenditure (CapEx). Variable definitions appear in Appendix A. Panel A include firm indicator variables, calendar year-quarter indicator variables and fiscal quarter indicator variables; standard errors are adjusted for within-firm correlation. Panel B controls for customer industry-quarter fixed effects, customer-supplier pair fixed effects, and customer covenant controls; standard errors are adjusted for within-pair correlation. ***, **, and * indicate statistics significance at 1%, 5%, and 10% level, respectively.

Panel A. Baseline Results

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Sales)		ROA		CAPEX	
Customer Violation	-0.099***		-0.003**		-0.002	
	(-3.582)		(-2.065)		(-1.068)	
Cust. New Violation		-0.104**		-0.005*		-0.005**
		(-2.234)		(-1.889)		(-2.423)
Size(t-1)	0.656***	0.656***	0.002***	0.002***	0.002	0.002
	(14.694)	(14.682)	(2.596)	(2.616)	(1.140)	(1.146)
Tangibility(t-1)	0.207	0.209	-0.001	-0.001	-0.053***	-0.053***
	(0.780)	(0.786)	(-0.234)	(-0.224)	(-5.450)	(-5.451)
CashHolding (t-1)	-0.338***	-0.335***	-0.007***	-0.007***		
	(-3.155)	(-3.137)	(-2.802)	(-2.768)		
ROA(t-1)	1.093***	1.100***				
	(5.573)	(5.612)				
CAPEX	0.982***	0.985***	0.013	0.013		
	(3.153)	(3.175)	(1.224)	(1.227)		
Leverage(t-1)	-0.497***	-0.498***	-0.006	-0.006	-0.009**	-0.009**
	(-4.089)	(-4.096)	(-1.413)	(-1.423)	(-2.334)	(-2.343)
Market/Book(t-1)	0.018***	0.018***	0.000***	0.000***	0.000**	0.000**
	(3.549)	(3.555)	(5.442)	(5.454)	(2.550)	(2.552)
CashFlow(t-1)	-0.017	-0.018	0.040***	0.040***		
	(-0.226)	(-0.247)	(13.615)	(13.630)		
NetWorth (t-1)	-0.411***	-0.410***	-0.005	-0.005		
	(-3.546)	(-3.535)	(-1.506)	(-1.496)		
CurrentRatio(t-1)	-0.011***	-0.011***	0.000	0.000		
	(-2.922)	(-2.938)	(0.115)	(0.112)		
CashFlow	3.006***	3.005***	0.456***	0.456***	0.006	0.006
	(14.258)	(14.238)	(80.076)	(80.072)	(0.622)	(0.621)
Adj. R-sq	0.979	0.979	0.861	0.860	0.758	0.758
N. of Obs.	8,828	8,828	8,857	8,857	11,142	11,142

Table 8 Continued

Panel B. Addressing Omitted Variable Bias

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Sales)		ROA		CAPEX	
Customer Violation	-0.079***		-0.004**		-0.001	
	(-2.674)		(-2.506)		(-0.396)	
Cust. New Violation		-0.063*		-0.005*		-0.004**
		(-1.764)		(-1.821)		(-2.004)
Size(t-1)	0.488***	0.487***	-0.002*	-0.002*	-0.001	-0.001
	(12.512)	(11.374)	(-1.683)	(-1.712)	(-0.298)	(-0.298)
Tangibility(t-1)	-0.006	-0.005	0.003	0.003	-0.043***	-0.043***
	(-0.032)	(-0.024)	(0.451)	(0.454)	(-3.969)	(-3.966)
CashHolding (t-1)	-0.386***	-0.385***	-0.001	-0.001		
	(-4.328)	(-4.207)	(-0.297)	(-0.289)		
ROA(t-1)	0.239**	0.243				
	(2.036)	(1.494)				
CAPEX	0.412	0.410	0.010	0.010		
	(1.265)	(1.004)	(0.781)	(0.773)		
Leverage(t-1)	-0.343***	-0.344***	0.008	0.008	-0.011*	-0.011*
	(-2.872)	(-2.961)	(1.366)	(1.367)	(-1.940)	(-1.940)
Market/Book(t-1)	0.045***	0.045***	0.001***	0.001***	0.002***	0.002***
	(5.849)	(5.730)	(2.817)	(2.808)	(4.640)	(4.633)
CashFlow(t-1)	0.646***	0.636***	0.122***	0.121***		
	(4.644)	(4.367)	(17.600)	(17.537)		
NetWorth (t-1)	-0.291**	-0.290**	-0.001	-0.001		
	(-2.434)	(-2.300)	(-0.152)	(-0.143)		
CurrentRatio(t-1)	-0.007	-0.007	-0.001***	-0.001***		
	(-1.148)	(-1.127)	(-2.889)	(-2.884)		
CashFlow	2.579***	2.576***	0.426***	0.426***	0.004	0.004
	(13.710)	(7.231)	(66.311)	(66.277)	(0.420)	(0.413)
Customer characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Industry-quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Pair FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-sq	0.983	0.983	0.878	0.878	0.817	0.817
N. of Obs.	7,525	7,525	7,547	7,547	9,325	9,325

Table 9: **Customers' Covenant Violations and Suppliers' Stock Price Performance**

This table reports calendar-time abnormal returns of supplier portfolios measured against a four-factor return model over the period July 1996 through March 2008. *(No) Customer Viol.* portfolio is a value-weighted portfolio of suppliers with at least one (without any) customer violating a debt covenant before a quarter end. The portfolio is rebalanced monthly to add the qualified violating firms, and each stock can stay in the portfolio for no more than k months. We estimate time-series regressions of the portfolio excess returns on the Fama-French and Carhart four factors: $R_{p,t} - r_{f,t} = \alpha_p + b_p(R_{m,t} - r_{f,t}) + s_p(SMB_t) + h_p(HML_t) + u_p(UMD_t) + e_{p,t}$. The regression intercepts α_p, which measure the average monthly abnormal return for portfolio p over the holding period, are reported in the table (corresponding t statistics are presented in parentheses). ***, **, and * indicate statistics significance at 1%, 5%, and 10% level, respectively.

	k=12	k=24	k=48
Customer Viol.	0.230	0.522	0.657
	(0.39)	(1.19)	(1.70)*
No Customer Viol.	0.832	0.748	0.821
	(3.84)***	(3.90)***	(4.98)***
Long No Viol.	0.602	0.227	0.164
& Short Viol.	(0.99)	(0.50)	(0.42)

Table 10: **Cross-sectional Analyses of Supplier Performance**

Subsample analyses of the impact of customers' covenant violations on suppliers' operating performance. The sample consists of supplier-quarter observations during 1996 to 2004. *Customer Viol.*(*New Viol.*) is an indicator variable that equals one if at least one customer (newly) violated a debt covenant in the last quarter, and zero otherwise. The dependent variables are ln(Sales) and return on assets (ROA). Variable definitions appear in Appendix A. Panel A include firm indicator variables, calendar year-quarter indicator variables and fiscal quarter indicator variables; standard errors are adjusted for within-firm correlation. Panel B controls for customer industry-quarter fixed effects, customer-supplier pair fixed effects, and customer covenant controls; standard errors are adjusted for within-pair correlation. ***, **, and * indicate statistics significance at 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Low Loan/Total Debt				High Loan/Total Debt			
	ln(Sales)		ROA		ln(Sales)		ROA	
Customer Viol.	-0.015		-0.002		-0.084*		-0.006**	
	(-0.353)		(-0.723)		(-1.795)		(-2.223)	
Cust. New Viol.		0.041		0.003		-0.141*		-0.014***
		(0.491)		(0.643)		(-1.680)		(-2.992)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pair FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-sq	0.986	0.986	0.869	0.869	0.982	0.982	0.874	0.874
N.of Obs.	2,069	2,069	2,081	2,081	1,945	1,945	1,948	1,948

Table 10 Continued

	Low R&D/Sales				High R&D/Sales			
	ln(Sales)		ROA		ln(Sales)		ROA	
Customer Viol.	-0.049		-0.000		-0.122**		-0.008***	
	(-1.450)		(-0.066)		(-2.575)		(-3.477)	
Cust. New Viol.		0.010		-0.003		-0.125*		-0.007*
		(0.242)		(-0.843)		(-1.679)		(-1.759)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pair FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-sq	0.983	0.983	0.824	0.824	0.982	0.982	0.888	0.888
N.of Obs.	3,693	3,693	3,702	3,702	3,832	3,832	3,833	3,833
	Low SG&A/Sales				High SG&A/Sales			
	ln(Sales)		ROA		ln(Sales)		ROA	
Customer Viol.	0.000		-0.000		-0.122**		-0.006**	
	(0.006)		(-0.154)		(-2.508)		(-2.400)	
Cust. New Viol.		0.018		-0.003		-0.138**		-0.006*
		(0.776)		(-1.246)		(-2.305)		(-1.665)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pair FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-sq	0.988	0.988	0.805	0.805	0.952	0.951	0.860	0.860
N.of Obs.	3,174	3,174	3,190	3,190	3,409	3,409	3,408	3,408

Appendix A. Variable definitions

Accounts Payable/COGS: apq/cogsq

Accounts Payable/Purchase: apq/(cogsq -(invtq - lag(invtq)))

Accounts Receivable/Sales: rectq/saleq

Size: ln (atq)

ln(Sales): ln (saleq)

Leverage: (dltcq + dlttq) /atq

Tangibility: ppentq/atq

Cash: cheq/atq

Net Capital Expenditure (CAPEX): capxq/lag(ppentq)

Net Worth: seqq/atq

Cash Flow: (ibq + dpq)/lag(atq)

Market/Book: (prccq×cshoq + tdq + pstkq - txditcq)/atq

ROA: oibdpq/atq

Current Ratio: actq/lctq

Interest Expense: xintq/lag(atq)

Interest Coverage: oibdpq/xintq

Quick Ratio: (actq - invtq)/lctq

Fixed Charge Coverage: oibdpq/(xintq + dlcq)

Debt/EBITDA: (dltcq + dlttq)/oibdpq

Debt/Equity: (dltcq + dlttq)/(prccq×cshoq)

Debt/Tangible Net Worth: (dltcq + dlttq)/(actq + ppentq + aoq - ltq)

Credit Rating: indicator that equals one if the firm have a long-term rating above BBB-, and zero otherwise.

Distance to Default (DtD): The estimation procedure is based on the Merton (1974) model, which views equity as a call option on the value of the firm with a strike price equal to the face value of the firm's debt. In particular, V_A and σ_A are estimated by solving the following two equations: (1) $V_E = V_A N(d_1) - e^{-rT} F N(d_2)$, (2) $\sigma_E = \frac{V_A}{V_E} N(d_1)\sigma_A$, where V_E is the market value of firm equity, V_A is the firm value (market value of asset), $N(.)$ is the cumulative function of standard normal distribution, $d_1 = \frac{ln(V_A/F)+(r+0.5\sigma_A^2)T}{\sigma_A\sqrt{T}}$, $d_2 = d_1 - \sigma_A\sqrt{T}$, σ_A^2 is asset volatility, F is the face value of debt, r is the risk-free rate, and T is the time to maturity of debt. Then the distance to default can be calculated as: $DtD = \frac{ln(V_A/F)+(\mu-0.5\sigma_A^2)T}{\sigma_A\sqrt{T}}$, where μ is an estimate of the expected annual return of the firm's assets.