Bank Interventions and Trade Credit: Evidence from Debt Covenant Violations

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Abstract

This study examines the consequences of conflicts between creditors. Using the setting of debt covenant violations, I employ a regression discontinuity design to identify the effect of banks' interventions on their borrowers' trade credit. The results show that trade credit experiences a substantial decline when banks intervene in the borrowing firm following covenant violations. The decline is mitigated by the presence of dependent suppliers and exacerbated by banks' incentives to exercise control rights. Such externalities are reflected in the loan-contract design. Borrowing firms sign less restrictive loan contracts when they rely more on trade credit or trade creditors.

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I Introduction

A firm usually has multiple creditors. Since their cash flow claims on the borrower are all fixed payments, creditors have a common interest in mitigating the risk-taking of the borrower (Diamond (1984); Fama (1990); Beatty, Liao and Weber (2012)). However, when there are insufficient cash flows to cover claims from all creditors, conflicts of interest can unavoidably arise. Under such circumstances, the distribution of cash flows will depend on the allocation of control rights among creditors. The bankruptcy literature discusses creditor conflicts during borrowers' reorganization and bankruptcy process (Bulow and Shoven (1978); Franks and Nyborg (1996); Jiang, Li and Wang (2012); Li and Wang (2016)). This paper, on the other hand, examines conflicts between the banks and trade creditors of borrowing firms in the situation of technical default (debt covenant violation). Unlike bankruptcy status, technical defaults occur frequently, even by firms that are not financially distressed (Dichev and Skinner (2002)). Thus the implications of this study apply to a larger set of firms in the economy.

The research questions of the current study are as follows. When a bank's control rights become disproportionately strong among all creditors following a debt covenant violation, will trade creditors cut trade credit supply? Is the cost of creditor conflicts, if any, reflected in the ex ante design of debt contracts?

Trade credit is consistently considered to be one of the most important sources of external finance for firms of all sizes. According to the U.S. Flow of Funds Accounts, as of September 2012, accounts payable is three times the value of bank loans and fifteen times the value of commercial paper on the aggregate balance sheet of nonfinancial U.S. businesses (Barrot (2016)). In my sample, the median ratio of accounts payable to total debts is 39%. Trade credit can substitute for bank credit in liquidity shock (Biais and Gollier (1997); Petersen and Rajan (1997); Nilsen (2002); Cunat (2007); Garcia-Appendini and Montoriol-Garriga (2013)) and is considered to be an important factor in sales and procurement operations (Ng, Smith and Smith (1999); Klapper, Laeven and Rajan (2012)).

Despite its importance, trade credit is relatively less protected than other debt claims. While most banks and bondholders are protected by debt covenants when borrowers perform poorly, only a small proportion of trade credit contracts contain a covenant.[1] Debt covenants are an important tool for allocating control rights among creditors. For example, if a borrowing firm violates a loan covenant, control rights are transferred to banks. The banks can start renegotiating the contract terms and intervene in the borrower's policy. This is usually done by threatening to accelerate principal repayments, which requires the borrower to immediately pay off the unpaid balance of the loan principal.[2] While the majority of public bond contracts contain cross-acceleration provisions that can help them share the accelerated repayments (Beatty et al. (2012)), trade credit contracts seldom contain such clauses. Therefore,

[1] Even for *long-term* supply contracts, the proportion of contracts including a covenant is less than half (Costello (2013)).

[2] When a company violates a debt covenant on long-term debt, the company must reclassify the debt as short term, so long as the lender has the right to demand the immediate repayment of the loan. The bank can also initiate bankruptcy proceedings, in which debts are automatically accelerated. In this case, the trade creditors' claims are jeopardized as well because they are the lowest priority for getting paid among all creditors (Chen (2005)).

after a loan covenant violation, trade credit claims are subject to the threat of enhanced control rights of the bank. To protect ongoing and future claims, trade creditors are expected to provide less trade credit to the violating borrower.

On the other hand, bank interventions could also have positive externalities on trade creditors. Most of the time, repayment accelerations after covenant violations are not enforced, but are used as a threat to force renegotiations of firms' policies (Denis and Wang (2014)). Existing studies have shown that banks monitor the borrower after covenant violations, which leads to better corporate governance practice (Nini, Smith and Sufi (2012); Ferreira, Ferreira and Mariano (2018)) and more transparent financial reporting policies (Tan (2013)). Although banks always act in their own interest in these processes, such as by restricting borrowers' access to other credit (Roberts and Sufi (2009)), their monitoring activities can improve borrowers' performance in the long run, and, thus, are largely in line with trade creditors' interests. If such benefits are sufficient to outweigh the cost resulting from banks' stronger cash flow claims, delegating monitoring to the bank is optimal for trade creditors. Under this circumstance, trade creditors are expected to extend more trade credit to the violating borrower. Therefore, whether trade creditors would extend more or less trade credit to the covenant-violating borrower is ultimately an empirical question.

I examine changes in trade credit around covenant violations using a regression discontinuity (RD) design. Since the violation of covenants depends on whether a financial ratio falls below (or exceeds) a predetermined threshold, crossing the threshold represents a discrete change in bank control. As long as the borrower cannot precisely control the accounting variable, the assignment of bank control can be seen as random for borrowers close to the threshold. This strategy helps isolate the effect of bank interventions from unobserved changes caused by financial distress or industry-wide shocks that simultaneously determine covenant violations and trade credit.

I conduct the RD design using a sample of nonfinancial firms whose loan covenant information is available in Dealscan. I find that, on average, borrowing firms experience a more than 10% reduction in their trade credit in the quarter following covenant violations. The result holds for both linear and polynomial functional forms, for both nonparametric and parametric estimations, for multiple bandwidth selections, and for different samples of covenant violations (Dealscan sample and the sample of violations disclosed in SEC filings). The evidence is consistent with the creditor conflict hypothesis: Suppliers provide less trade credit when banks obtain additional control rights that could adversely affect suppliers' claims.

Extant studies have found that covenant violations lead to a significant decline in violating firms' investments (Chava and Roberts (2008)) and leverage (Roberts and Sufi (2009)). This raises a concern that the reduction in trade credit is a consequence of the fact that the firm is scaling down after the violation. That is, my result can be driven by violating firms' demand rather than trade creditors' supply. However, my measure of trade credit is scaled by average purchase costs (COGS), which can largely account for the reduction in economic activity (Love, Preve and Sarria-Allende (2007);

Garcia-Appendini and Montoriol-Garriga (2013)). The interpretation is that for the same level of capital input, trade credit financing is lower after a covenant violation than prior to the violation. To further mitigate the concern, I perform cross-sectional tests that show that the reduction in trade credit following covenant violations is not driven by a contemporaneous drop of corporate investments or leverage, which is inconsistent with the demand interpretation.

To substantiate the supply effect interpretation, I further show that the documented effect of covenant violations covaries closely with the incentives of suppliers and banks. First, suppliers whose sales largely rely on their customers have dedicated customer-specific investments and are expected to maintain the trade credit supply when customers experience a liquidity shock (Wilner (2000); Cunat (2007)). I find that the reduction in trade credit following covenant violations is mitigated by the presence of such dependent suppliers. Second, banks suffering payment defaults by some borrowers have the incentive to enhance their contingent control rights with nondefaulting borrowers, even though the two groups of borrowers are in distinct industries (Murfin (2012)). I exploit this lender-specific shock that are exogenous to nondefaulting borrowers' fundamentals, and find that nondefaulting firms that experience such an "unrelated default" would suffer a larger drop in trade credit following a covenant violation. Third, a substantial amount of unpaid loans incentivizes banks to exercise their control rights and meanwhile poses a great threat to trade creditors' claims, increasing the likelihood of trade creditors cutting credit supply. Indeed, I find that when the ratio of bank credit (loans) to trade credit is greater, covenant violations and associated bank interventions impose a more pronounced effect on trade credit.

Losing trade credit, as a consequence of bank-supplier conflicts, is surely costly due to the important role played by trade credit (Petersen and Rajan (1997); Cunat and Garcia-Appendini (2012)). An additional cost associated with bank interventions is the expected loss borne by trade creditors due to potential nonpayment of trade credit. Since suppliers, as strategic partners, often provide unique and relationship-specific products to their customers, their losses are a significant concern for the customer (Titman (1984); Banerjee, Dasgupta and Kim (2008)).[3] Ex ante, firms should take these costs of bank-supplier conflicts into account in the design of loan contracts. Consistently, I find that firms relying on trade credit or having dependent suppliers will sign a loan contract that minimizes the ex ante probability of covenant violation, as measured by loan contract strictness. Moreover, I show that banks charge a higher interest rate to compensate for their concessions in loan covenants, suggesting a tradeoff between loan pricing terms and nonpricing terms. Further tests show that when suppliers are providing more relationship-specific investments, the strictness reducing effects of trade credit and dependent suppliers are more pronounced. Overall, the ex ante loan contracting results imply that potential conflicts of interest between creditors are an important determinant of loan contract terms.

[3] An annual survey of automakers' supplier relations has consistently shown that automakers with the best rankings of supplier relations "receive the greatest benefit from their suppliers in a variety of areas including lower costs, higher quality, and innovation" (*The U.S. Motor Vehicle Industry: Confronting a New Dynamic in the Global Economy*, Congressional Research Service, March 26, 2010).

Recent studies have called for a more detailed examination of the implications of the conflicts of interest among classes of creditors (e.g., Beatty et al. (2012), Li, Purda and Wang (2018)). Whereas prior studies have found that the allocation of creditor control plays a role during borrowers' bankruptcy filings (Li and Wang (2016); Eckbo, Thorburn and Wang (2016)), the current study advances our understanding of creditor conflicts by showing their consequences around technical defaults (Dichev and Skinner (2002)).[4] This cost is sufficiently large such that firms consider it in their ex ante contracting behaviors. Moreover, the setting of debt covenant violations enables me to use an RD design to establish causality, which is a difficult task in the literature on creditor conflicts.

This paper is also related to the line of literature examining the role of banks' control rights concerning borrowers. The majority of recent studies support the monitoring role played by banks after covenant violations (Nini et al. (2012); Tan (2013); Vashishtha (2014); Ferreira et al. (2018); among others). This study deviates from these studies by documenting an indirect cost of bank interventions—a reduction in trade credit. This finding can help us explain why some firms rely less on bank loans and more on other financing sources and how debt covenants can be put in place to encourage more conservative behaviors of managers who care about dependent suppliers.

In a related study, Roberts and Sufi (2009) document that firms reduce leverage after covenant violations. Their measure of total debt does not include trade credit.

[4] Jorion and Zhang (2009) and Hertzel, Li, Officer and Rodgers (2008) document the costs of a customer's/supplier's financial distress on the trading counterparty. This study, however, focuses on the effect of the bank's control rights.

Trade credit is different from other debt claims due to its in-kind nature and close association with product market relationships (Burkart and Ellingsen (2004); Klapper et al. (2012)). As such, trade credit often stimulates relationship-specific investment from the suppliers (Smith (1987)). And in terms of amount, trade credit is at least as important as any other debt claims (Ng et al. (1999); Cunat and Garcia-Appendini (2012)). Therefore, trade credit is worth an independent examination.

The remainder of the paper unfolds as follows. Section II develops the main hypotheses. Section III describes the data and summary statistics. Section IV introduces RD design as the main identification strategy. Section V reports the results regarding the response of trade credit to bank interventions and a battery of robustness tests is implemented. Section VI discusses the ex ante choice of loan contract structure given the findings in preceding sections. Section VII concludes.

II Hypotheses

Banks obtain contingent control rights upon their borrowers' covenant violations. These rights put banks in a position of influence: They determine whether to modify or waive restrictions or to demand immediate repayment (Chen and Wei (1993)). By exercising these rights, banks are able to intervene with a wide range of borrowers' policies (e.g., Chava and Roberts (2008); Roberts and Sufi (2009); Nini et al. (2012), Gu, Mao and Tian (2017), Denis and Wang (2014); Vashishtha (2014); Falato and Liang

(2016)). Since loans are repaid in a fixed amount in good states but bear the loss in bad states, banks have a strong incentive to monitor their clients and mitigate excessive risk-taking. They would act on this position of enhanced control following covenant violations and actively engage in the corporate governance of the violating client (Tan (2013); Ferreira et al. (2018)). This monitoring role is documented to improve the long-run performance of the borrower in the post-violation period (Nini et al. (2012)). The risk mitigation and performance improvement of the borrowing firms are largely in line with trade creditors' interest. Therefore, to save monitoring costs and protect their claims in the long run, trade creditors could optimally delegate monitoring to the controlling banks (Diamond (1984); Fama (1990); Beatty et al. (2012)). In this case, I expect trade creditors to increase their provision of trade credit in the covenant-violating borrower.

On the other hand, bank interventions could impose substantial negative externalities on trade creditors, as there are no protections for trade creditors' debt claims. Covenant violations allow banks to grab the borrower's cash flows before other creditors can. Unlike public bond contracts, trade credit contracts rarely contain such contractual rights as cross-acceleration or cross-default provisions to protect their claims. Even worse, trade creditors have the lowest priority of getting repaid during a borrower's bankruptcy proceedings (Chen (2005)) and thus are subject to substantial losses once the bank decides to push the firm into bankruptcy procedures. Additionally, it is difficult for trade creditors to avoid the conflicts with banks by quickly switching to another customer, since they have to redeploy relationship-specific investment (Titman (1984); Banerjee et al. (2008)). Thus, trade creditors would be most vulnerable to banks' control rights and resultant interventions. They are expected to cut trade credit supply after their customers' covenant violations.

Therefore, the reaction of trade credit to covenant violations depends on the comparison between the benefits and costs of bank interventions and is not clear ex ante. The main hypothesis is thereby stated in a null form.

Hypothesis 1 *Bank interventions on a borrower following the borrower's covenant violations have no effect on the borrower's trade credit.*

If bank interventions cause trade credit to decline, the cost incurred to the borrowing firm could be substantial. First, based on the existing findings of the literature, the importance of trade credit to a firm is without question (Cunat and Garcia-Appendini (2012)). Second, a trade creditor (supplier) is a long-term trade partner that provides relationship-specific investment to its customer. Trade creditors' welfare is thus a major concern of the customer (Titman (1984); Banerjee et al. (2008)). Therefore, if bank interventions following covenant violations can impose costs on trade creditors and cause a reduction in trade credit supply, the borrowing firm should try to avoid such costs ex ante. Hypothetically, this can be reflected in the design of loan contract terms. If a borrower relies more on trade credit or cares more about its supplier, the borrower should prefer a loan contract that contains looser loan covenants, such that the probability of bank interventions is lower. The second hypothesis is thus as follows.

Hypothesis 2 *A loan contract will have lower contract strictness (smaller probability of covenant violations) if the borrowing firm relies more on trade credit or has one or more dependent suppliers.*

III Data and Summary Statistics

To perform a regression discontinuity (RD) design, I rely on the information on covenant types and covenant thresholds provided by Loan Pricing Corporation's Dealscan database. The basic unit of observation in Dealscan is a loan, also referred to as a facility. Usually, more than one facility is grouped into a package, and the information concerning covenants is reported at the package level. To be included in my sample, a facility must have nonmissing covenant information, be initiated after January 1, 1996, and have more than three years' life.[5] I then match these facilities with the quarterly observations of corresponding borrowing firms recorded in Compustat between 1996 and 2008. Firms incorporated outside the United States and financial firms are excluded (SIC codes 6000–6999). I also exclude the following firms that are subject to data integrity problems: firms that are recorded to have negative total book assets, total sales, market value or book value of equity, accounts payable, and cost of goods sold; firms with a leverage ratio that is either negative or greater than 1; and firms with accounts payable that are greater than total book assets.

[5] I exclude observations before 1996 because the coverage of loans in Dealscan is fairly limited before the mid-90s (Chava and Roberts (2008)).

RD design requires an accurate input of the running variable, which, in the context of loan covenants, is the distance of firms' actual financial ratio from the corresponding covenant threshold. However, the computations of financial ratio using Compustat items could differ from the definitions actually used by banks. Demerjian and Owens (2016) collect the actual calculation methods in loan contracts and generate the measurement error of computing financial ratios using Compustat information for each type of covenant. Drawing on their study, I include covenant types only when their accuracy rate based on Compustat is higher than 80%. Six types of loan covenant survive this selection procedure (the number in parenthesis indicates the accuracy rate): minimum EBITDA (97.40%), minimum current ratio (95.40%), maximum debt to EBITDA (91.00%), maximum senior debt to EBITDA (89.40%), minimum senior leverage (86.80%), and maximum leverage ratio (84.50%).[6] Definitions of these covenant variables closely follow Demerjian and Owens (2016) and are displayed in the Appendix. All flow variables are annualized (rolling four quarters) for both income statement and statement of cash flow variables.

Next, I compute the distance from the underlying financial ratio to the corresponding covenant threshold (DISTANCE) as follows:

$$\text{DISTANCE} = \begin{cases} \frac{r'-r}{\sigma} & \text{for covenants specifying a minimum limit} \\ -\frac{r'-r}{\sigma} & \text{for covenants specifying a maximum limit} \end{cases} \tag{1}$$

[6] My main finding is robust to using up to nine most accurate types of loan covenant. I do not include the net worth covenant since information on stock issuance escalators—fraction of stock issuances on which the net worth threshold adjusts—is missing from Dealscan.

where r' is the actual financial ratio, $\underline{r}$ is the covenant threshold, and σ is the sample standard deviation of r'.[7] A negative DISTANCE means a covenant violation; and the smaller the DISTANCE, the closer the borrower is to a violation (when DISTANCE> 0) or the more severely the covenant is violated (when DISTANCE< 0).

The main dependent variable, TRADE_CREDIT, is defined as the ratio of accounts payable to cost of goods sold (AP/COGS). This ratio captures the fraction of total purchase costs that are financed by trade credit, or, firms' reliance on trade credit. Moreover, by scaling accounts payable by a flow variable, I control for the reduction in economic activity that is commonly associated with covenant violations (Garcia-Appendini and Montoriol-Garriga (2013)). For robustness, I also use two alternative measures of COGS, the average COGS of the last four quarters and the quarter-end COGS adjusted for within-quarter changes in inventory. All my results hold for these two alternative measures (results are unreported but available upon request). I also construct several covariates and their lagged values: the logarithm of book assets (ln(ASSET)), cash holding (CASH), return on assets (ROA), market-to-book ratio (MTB), net worth (NET_WORTH), capital expenditure (CAPEX), leverage ratio (LEVERAGE), and absolute abnormal accruals (AB_ACC). I require nonmissing values

[7] To ensure the accuracy of the measurement, I follow Chava and Roberts (2008) to make the following adjustment to the thresholds. First, when firms have multiple loans that overlap, I define the relevant covenant to be the tightest (the one with the smallest DISTANCE) unless it corresponds to a refinancing loan, in which case I define the relevant covenant to be that specified by the refinancing regardless of whether or not it is the tightest. Second, for covenants that change over time, I linearly interpolate the covenant thresholds over the life of the loan. Loans whose initial cutoff is more than 100 times the final cutoff or the other way around are excluded from the sample. Third, for post-origination amendments to covenants that are caused by renegotiations outside technical default, I extract the amendment information from Dealscan and adjust the affected covenant threshold on the amendment date. Finally, I drop loans that appear to be in violation within the first year following the loan initiation date.

for TRADE_CREDIT, DISTANCE, all covariates, and their lagged values. The final sample consists of 14,670 firm-quarter observations.

Panel A of Table 1 presents the summary statistics for the sample. The formal definitions of all variables are presented in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. The outcome variable, TRADE_CREDIT, has an average of 51.0% and a median of 38.6%, suggesting that firms finance a large proportion of their costs of goods by trade credit. In addition, the observations have a mean (median) book assets of $1,904 million ($536 million), a mean (median) ROA of 3.6% (3.5%), a mean (median) market-to-book ratio of 1.39 (1.10), a mean (median) cash holding of 8.4% (3.9%), a mean (median) net worth of 49.5% (48.4%), a mean (median) leverage of 24.4% (24.1%), and a mean (median) capital expenditure of 6% (4.6%).

[Table 1 is here]

IV Empirical Strategy

A Methodology

I use a regression discontinuity (RD) design to disentangle the effect of bank interventions from changes in trade credit that would have otherwise occurred around the covenant violation. The treatment in an RD design is determined by whether the running variable falls short of (or exceeds) a prespecified threshold. This rule arbitrarily creates a discrete change in treatment in the neighborhood of a known cutoff and, thus, resembles a "locally" randomized trial. In the context of a loan covenant, the running variable is the distance of a known financial ratio from the corresponding covenant threshold. The status of bank control rights changes going from one side of the threshold to the other. As long as trade credit (or unobserved variables that affect trade credit), as a function of variables on which covenant thresholds are written, does not exhibit the same discrete change at the covenant threshold, it is possible to identify the effect of bank interventions on trade credit.

Following the RD literature (e.g., Hahn, Todd and Van der Klaauw (2001); Imbens and Lemieux (2008)), I use nonparametric local linear regressions as the main test strategy. To alleviate any model misspecification concern, I also consider nonparametric local quadratic regressions and parametric regressions with high-order running variables (Lee and Lemieux (2010)). The local linear regression model, estimated on a small bandwidth around the threshold, is as follows:[8]

$$\begin{aligned} \text{TRADE\_CREDIT}_{i,t} =& \alpha_0 + \beta_0 \text{VIOLATION}_{i,t-1} + \beta_1 \text{DISTANCE}_{i,t-1} + \\ & \beta_3 \text{VIOLATION}_{i,t-1} \times \text{DISTANCE}_{i,t-1} + \epsilon_{i,t} \qquad (2) \\ & s.t. -h \leq \text{DISTANCE} \leq h \end{aligned}$$

where i indexes firms and t indexes quarters. TRADE_CREDIT is accounts payable scaled by the cost of goods sold. VIOLATION is an indicator variable that equals 1 if

[8]Note that local linear regressions are not estimated using ordinary least squares but by using kernel-weighted least squares on either side of the cutoff.

the firm violates a covenant (DISTANCE<0), and 0 otherwise (DISTANCE>0). VIOLATION×DISTANCE accounts for the possibility that the regression parameter differs between the two sides of the covenant threshold. β_0 quantifies the discontinuous effect of bank interventions on borrowers' trade credit.

The choice of bandwidth, h, involves a tradeoff between bias and efficiency. A large bandwidth improves estimation efficiency by including more observations, but can result in a biased estimate. In this study, the baseline bandwidth relies on a mean-squared-error optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014) (CCT bandwidth). I use rectangular kernel as the baseline kernel weighting function, and experiment with two other kernel choices: triangular and Epanechnikov. For robustness, I perform the main analysis using different multiples of the baseline bandwidth and an alternative bandwidth proposed by Imbens and Kalyanaraman (2012) (IK bandwidth).

B Identifying assumptions

The validity of RD design relies on two identifying assumptions: (1) firms that fall just above and just below the covenant threshold should be comparable along various dimensions, and (2) firms in the vicinity of the threshold could not precisely manipulate their financial ratios to narrowly avoid a covenant violation. These two conditions jointly guarantee the local randomness of the treatment assignment. I test these two assumptions separately.

The first condition requires the continuity of firm characteristics around the covenant threshold. To examine this condition, I conduct regression discontinuity plots for ex ante covariates that could determine trade credit. In Figure 1, the x-axis is the distance between the financial ratio and the corresponding threshold (DISTANCE). The dots represent the average covariates in each of twenty equally spaced bins on either side of the threshold. The fitted curves are based on local linear regressions with a 95% confidence interval around the fitted value.[9] Each graph uses the CCT bandwidth for local linear regression of the corresponding variable. For all covariates—ln(ASSET), MTB, ROA, CASH, NET_WORTH, CAPEX, LEVERAGE, and AB_ACC—the two fitted lines on the two sides of the threshold are close to each other and there is a clear overlap of the 95% confidence intervals. The RD estimates of these covariates are all statistically insignificant (reported in Table IA.1 of Internet Appendix). The continuity of covariates around the cutoff allows me to estimate equation (2) without controlling for these covariates while causing little bias in the estimation for β_0 (Imbens and Lemieux (2008)).

[Figure 1 is here]

The second condition requires the continuity of the density of the running variable around the threshold. To test this, I follow the methodology proposed by McCrary (2008). A jump in the density at the threshold is indicative of managerial manipulations. Figure 2 shows a plot of the test result. The x-axis is the distance from

[9] The results are robust to fitted curves based on quadratic functions.

the threshold and the y-axis represents the density of the running variable. The solid line depicts the fitted density function with a 95% confidence interval. The discontinuity estimate of the density of the running variable is statistically insignificant (Wald statistic is 0.574). Therefore, I cannot reject the null hypothesis that the density at the threshold is continuous. In other words, the McCrary test result suggests that managers do not significantly manipulate their financial ratios when they get close enough to the threshold.[10] In fact, to the extent that absolute abnormal accruals measure managerial earnings manipulation (Dechow and Dichev (2002)), the continuity of absolute abnormal accruals around the cutoff (in Figure 1) provides reassuring evidence for the McCrary test result.

[Figure 2 is here]

V Bank Interventions and Trade Credit

A Baseline Results

In this section, I show the effect of bank interventions on borrowers' trade credit using a regression discontinuity design. I first use graphical analysis by presenting the

[10] Accounting literature argues that since technical defaults are costly, firms tend to manage earnings to avoid possible future debt covenant violations (Watts and Zimmerman (1986); DeFond and Jiambalvo (1994); Dichev and Skinner (2002)). Instead of arguing against that literature, I merely suggest that it is extremely difficult for managers to precisely manipulate the financial ratios when the distance to the covenant threshold is *sufficiently small* (I use the CCT bandwidth of estimating equation (2) with rectangular kernel but the result is robust to the use of other kernel weighting functions and the IK bandwidth). This is consistent with the argument by Chava and Roberts (2008) and Roberts and Sufi (2009).

RD plots of trade credit. In Figure 3, I plot the mean values of trade credit against the running variable, lagged DISTANCE. I use both linear functions and quadratic polynomial functions to fit the data points and plot the 95% confidence intervals around the fitted value. The upper two graphs use the CCT bandwidth with rectangular kernel weights, and the lower graphs use the IK bandwidth with rectangular kernel as well. As shown in all graphs, there is a clear discontinuity of trade credit around the cutoff. Firms that are just below the covenant threshold, and thus subject to bank interventions, experience a distinct drop in their trade credit.

[Figure 3 is here]

Table 2 quantifies the graphical findings in Figure 3 by reporting the discontinuity estimates. I first perform local linear regressions by estimating equation (2). Again, the optimal bandwidth is based on Calonico et al. (2014). I report results using a rectangular, a triangular, and an Epanechnikov kernel, respectively. The results consistently show negative and statistically significant coefficients on VIOLATION. For example, when using the rectangular kernel weighting function, the RD estimate is -0.070, suggesting a 7-percentage-point average decline in violating firms' trade credit. Since the mean value of trade credit of the sample is 0.51, a 0.07 decline represents a 13.7% reduction of trade credit for an average firm in the sample. This is an economically significant amount. When using triangular and Epanechnikov kernels, the magnitudes of estimate are even larger (-0.074 and -0.078, respectively).

I then use a more flexible functional form by implementing local quadratic polynomial regressions. Note that changing the functional form also changes the length of the optimal bandwidth (Calonico et al. (2014)), resulting in a different number of observations. Consistent with the results from local linear regressions, the RD estimates using quadratic regressions are negative and statistically significant: the magnitude ranging from -0.078 to -0.088. Based on the above two sets of results, we can reject the null hypothesis (Hypothesis 1), which states that covenant violations have no effect on trade credit. Provided that the RD design is a valid identification strategy, this evidence is consistent with the interpretation that bank interventions cause the violating borrowers' trade credit to decline.

[Table 2 is here]

Note that AP/COGS is also a proxy for days payable outstanding, which is computed as AP/$\frac{\text{COGS}}{\text{number of days}}$. Days payable outstanding measures the average number of days a company takes to pay its suppliers. Thus a reduction in AP/COGS can also be interpreted as suppliers' demanding fewer days for borrowers to pay the amount owed. This interpretation is consistent with the main argument of this paper that trade creditors become more concerned about their claims and extend less trade credit to the borrower.[11]

[11] I also implement a test directly examining the net days specified in trade credit contracts (days within which the buyer has to pay the amount owed). Any decline in net days as a contract term is likely due to suppliers' demand—the supply-side effect—rather than customers'. To obtain the information of net days, I implement a textual analysis on all the material contracts (Exhibit 10) reported in SEC 8-K and 10-K filings and successfully obtain 524 supply contracts. After matching them to Compustat-Dealscan firms, I get net days information for 21 covenant-violating firm-years and 15 nonviolating firm-years (out

In addition to the above RD analyses, I also perform a graphical analysis that shows the dynamic trend of trade credit for violating and nonviolating firms in the narrow band around the cutoff. I use the CCT bandwidth of estimating equation (2) to gauge the narrow band. I define the violation quarter as the one in which the violating and nonviolating firms appear in the chosen bandwidth. Then I obtain trade credit data in the prior two quarters and the following two quarters for these two groups of firms, and plot their average trade credit separately during the five-quarter window around the violation quarter. Therefore, this is in fact a difference-in-differences analysis. As shown in Figure 4, violating firms and nonviolating firms have a similar (and parallel) level of average trade credit before the violation quarter but, starting from the violation quarter, violating firms experience a sharp decline in trade credit relative to nonviolating firms.[12] This is consistent with the results documented above.

[Figure 4 is here]

B Alternative Bandwidths

To ensure that the results are not sensitive to the selection of bandwidth, I use several alternative bandwidths and redo my main analyses. First, I arbitrarily set the

of 67 successful matches). I examine the difference in net days between the two groups, and find that the mean value of net days for the violating group is 12.1 days and that for the nonviolating group is 23.5 days. The difference is statistically significant ($p = 0.027$), suggesting that suppliers require violating customers to pay the full amount in fewer days.

[12] The difference in means of trade credit in the first post-violation quarter is around 0.033. Note that the means of the two groups are computed using all firms falling in the CCT bandwidth on either side of the cutoff, thus differing from the formal RD estimates in Table 2, which measure the exact jump at the cutoff.

bandwidth for estimation to be 50%, 75%, 125%, and 150% of the CCT bandwidth, respectively. For each bandwidth specification, I first estimate nonparametric linear regressions and then quadratic regressions. For brevity, I report only the results using a rectangular kernel. Table 3 presents the results. For both linear and quadratic specifications, the discontinuity estimates are negative and statistically significant for each multiple of the CCT bandwidth.

Second, I conduct the main analysis using the optimal bandwidth devised by Imbens and Kalyanaraman (2012) (IK bandwidth). Based on my sample, the IK bandwidth is wider than the CCT bandwidth for local linear regressions and is similar to the CCT bandwidth for quadratic polynomial regressions. The estimation results are highly consistent throughout. When local linear regressions are estimated, the selection of IK bandwidth yields discontinuity estimates ranging from -0.063 to -0.066. When local quadratic regressions are estimated, the estimation magnitudes are larger, ranging from -0.081 to -0.084. All coefficients are highly significant at conventional significance levels.

[Table 3 is here]

C Robustness Checks

Given the continuity of the covariates around the cutoff point, it is not necessary to control for these covariates in the RD design (Imbens and Lemieux (2008)). However, for robustness, I follow the approach suggested by Lee and Lemieux (2010) and absorb the impact of these covariates in the estimates. Specifically, I first conduct an OLS regression of trade credit on lagged values of ln(ASSET), MTB, ROA, CASH, NET_WORTH, CAPEX, LEVERAGE, and AB_ACC, then I apply the nonparametric estimations on the residual trade credit obtained from OLS estimation. The results are reported in Panel A of Table 4. Both linear and quadratic regressions generate RD estimates that are negative and highly significant, with magnitudes (-0.085 to -0.097) slightly larger than the corresponding baseline estimates. In Panel B of Table 4, I use the same approach to absorb the effect of industry fixed effects and quarter fixed effects: Instead of controlling for the covariates in the first step, I control for industry (based on Fama-French 48 classification) and fiscal quarter dummies.[13] The results consistently show that covenant violations lead to a significant drop in firms' trade credit. The magnitudes (-0.060 to -0.072) are slightly smaller than the corresponding baseline estimates.

To further ensure the internal validity of my baseline results, I perform a set of placebo tests. The principle is, if the covenant threshold is arbitrarily placed somewhere other than the actual threshold, we should observe no discontinuity of the outcome variable. To check this, for each covenant type of each firm-quarter observation, I arbitrarily move the actual covenant threshold up or down by a certain percentage (i.e., create a pseudo-threshold that equals a certain multiple of the actual covenant threshold), then I compute the pseudo-distance by replacing the actual threshold, $\underline{r}$, in

[13] I do not consider firm fixed effects for estimation. As suggested by Appel, Gormley and Keim (2016), in an RD design in which only a limited fraction of sample firms switches treatment status during the sample period, using firm fixed effects leads to misleading inferences. See McKinnish (2008) and Gormley and Matsa (2014) for more detailed arguments.

equation (1) by the pseudo-threshold. Finally, I apply the nonparametric estimations using the lagged pseudo-distance as the running variable. The results are presented in Panel C of Table 4. For 50%, 75%, 125%, and 150% of the original threshold value, none of the estimated coefficients is statistically significant and all of them have a fairly small magnitude. The results suggest that the discontinuous reduction in trade credit can hardly occur unless the firm truly violates a loan covenant.

[Table 4 is here]

D Heterogeneous Effect of Covenant Violations on Trade Credit

This study argues that the negative effect of covenant violations on trade credit is due to a cut in the trade credit supply by violating firm's trade creditors. However, a likely alternative explanation is that the reduction in trade credit is a consequence of the fact that the firm is scaling down after the violation. Other papers have shown evidence that firms do scale down by reducing investments (Chava and Roberts (2008); Nini et al. (2012)) and leverage (Roberts and Sufi (2009)) following a covenant violation. Thus the reduction in trade credit can be driven by demand rather than supply.

The cross-sectional tests in this section provide implications for the economic mechanism underlying the main result, which in turn helps me disentangle the supply-side effect from the demand side. On the one hand, I show that the decline in trade credit following covenant violations is unlikely caused by a contemporaneous decline in violating firms' demand. On the other hand, I find that the response of trade credit to covenant violations varies predictably with the supply-side incentives. Specifically, the reduction in trade credit is mitigated by the presence of dependent suppliers while exacerbated by banks' incentive to intervene and the resultant conflicts once banks intervene.

I first investigate whether the main finding is driven by a contemporaneous drop in investments or leverage following covenant violations. A demand-side effect would predict a larger drop of trade credit for firms that cut capital expenditures or reduce leverage. I thus split the sample based on whether a firm experiences an increase or a decrease in its CAPEX (LEVERAGE) from the prior quarter to the current quarter, then estimate equation (2) on each subsample using the CCT bandwidth. The results are presented in Table 5, columns (1) and (2) for CAPEX and (3) and (4) for LEVERAGE, respectively. While the coefficient for firms experiencing a CAPEX drop is indeed negative and significant, the coefficient for firms with a CAPEX increase is also significantly negative and the magnitude difference is quite small. Similarly, the coefficients for firms with and without a LEVERAGE drop are both negative and statistically significant, and in fact, the one for firms with a LEVERAGE increase is larger. The results indicate that the reduction in trade credit following a covenant violation similarly occurs in each group, not necessarily dependent on the change in investment or leverage. Therefore, the documented trade credit reduction following covenant violations cannot be fully attributed to firms' declining demand as represented by firm investment and leverage.

I proceed to investigate how suppliers' characteristics, banks' incentives, and supplier-bank conflicts mitigate or intensify the effect of covenant violations on trade credit. First, if suppliers' sales are largely dependent on the violating firm, they are less likely to cut trade credit following the firm's covenant violations. The trade credit literature has argued that it is sensible for a dependent supplier to provide liquidity insurance to its important trade partner that experiences temporary liquidity shock (Wilner (2000); Cunat (2007)). Moreover, for a dependent supplier, the cost of switching to a new customer could outweigh the expected cost of staying with the current violating borrower, since the supplier has to rebuild customer-specific investments. Based on the information from FactSet, I define a firm as having a dependent supplier if the firm is reported as an important customer of a supplier. Then I partition the sample based on whether the firm has at least one dependent supplier, and estimate local linear regressions with the CCT bandwidth on the two subsamples, separately. Columns (5) and (6) of Table 5 report the estimates. While both coefficients are negative, only the one without dependent suppliers is statistically significant. The magnitude of the group with dependent suppliers is much smaller, just half the magnitude of the other group. This is consistent with dependent suppliers' maintaining the supply of trade credit and mitigating the effect of bank interventions.

Second, I utilize lender-specific shocks that are uncorrelated with borrowing firms' demand. When banks suffer payment defaults by certain borrowers, they become more conservative in screening and monitoring other nondefaulting borrowers, even though the defaulting and nondefaulting borrowers are in different industries. Murfin (2012) finds that this lender-specific shock induces a strong incentive on the part of banks to enhance contingent control rights. Using the same setting, I define a firm in my sample as having an "unrelated default" in a quarter if its lead banks suffered from payment defaults in the prior quarter by borrowers in different industries. While strongly correlated with banks' propensity of exercising control rights, an unrelated default is exogenous to the violating firm's fundamentals ex ante. In columns (7) and (8) of Table 5, the subsample RD analysis shows that the trade credit cut following covenant violations is significantly larger when the firm experiences an unrelated default (-0.117 versus -0.005).

Finally, I quantify the magnitude of potential creditor conflicts by directly using the value of cash flow claims held by the two parties, that is, the ratio of bank loans to trade credit. A substantial amount of outstanding bank debt incentivizes banks to exercise their control rights, and meanwhile poses a great threat to trade creditors' claims. Therefore, if bank credit is large relative to trade credit, suppliers have the incentive to reduce their exposure to banks' strengthened control rights following customers' covenant violations, resulting in a greater reduction in trade credit. To test the above arguments, I divide the sample by the median level of lagged loan-to-trade-credit ratio, then estimate equation (2) on the two subsamples separately. Estimated coefficients are reported in columns (9) and (10) of Table 5. The decline in trade credit is much larger for the group with a higher loan-to-trade-credit ratio (-0.125) than for the other group (-0.018), consistent with my conjecture.

Jointly, the findings in this section suggest that the trade credit cut following covenant violations is not driven by demand-side changes but is reliably associated with supply-side incentives. Therefore, the findings highlight the underlying mechanism and the economic interpretation of the main result: It is banks' control rights and resultant conflicts of interest between creditors that lead to the trade credit reduction.

[Table 5 is here]

E Parametric Analyses

To offer additional reassurance about the robustness of my baseline estimates, I perform a conventional parametric analysis. Following Chava and Roberts (2008), I implement parametric regressions using observations falling within a narrow band around the covenant threshold. The bandwidth selection is based on the optimal estimator from the corresponding nonparametric analyses: I use both CCT and IK bandwidths for local linear regressions to gauge the selection of the narrow band.

The estimation results are presented in Table 6. In columns (1) to (6) I use the CCT bandwidth and in columns (7) to (10), the IK bandwidth. Column (1) presents the result from a linear regression model that controls for lagged DISTANCE and its interaction with VIOLATION, industry fixed effects, and quarter fixed effects. The negative and statistically significant coefficient on VIOLATION suggests that after a covenant violation, on average, trade credit declines by 0.085. Column (2) estimates a polynomial regression by additionally controlling for the quadratic term of lagged DISTANCE (to account for potential nonlinearity) and yields a similar estimate (-0.093).

Corporate investment and leverage are known to be affected by covenant violations. To ensure that the coefficient estimate on VIOLATION for trade credit is not biased by the effect of investment and leverage, I perform the regression with and without controlling for CAPEX and LEVERAGE and compare the results. I include all covariates except CAPEX and LEVERAGE in columns (3) and (4), while additionally control for CAPEX and LEVERAGE in columns (5) and (6). The estimation results show that the coefficient estimates with and without controlling for the two variables are similar: They are negative, statistically significant, and are similar in magnitude, but those controlling for CAPEX and LEVERAGE are slightly larger.

[Table 6 is here]

I repeat all the above tests using the IK bandwidth except for the specifications in columns (1) and (2) (to save space). The results in columns (7) to (10) show that, while the observations are significantly more than those in the first six columns, the coefficient estimates on VIOLATION remain quite similar. The coefficient estimates range from -0.085 to -0.092 when using the IK bandwidth, and the same specifications using the CCT bandwidth generate estimates between -0.085 and -0.098. Overall, parametric analyses in this section generate consistent findings with the baseline results.

To check the external validity of my main findings, I conduct another parametric analysis using an alternative sample of covenant violations, namely, covenant violations disclosed in SEC filings (Nini et al. (2012)). To estimate the effect of covenant violations on trade credit, I employ a quasi-RD design analogous to that in Nini et al. (2012) and a difference-in-differences approach. The results, presented in Section 2 of Internet Appendix, show that covenant violations lead to a sizable drop in violating firms' trade credit.

VI Trade Credit, Dependent Suppliers, and Loan Contract Design

A Baseline Results

The findings thus far suggest that covenant violations cause trade credit to decline, which is likely due to the conflicts of interest between banks and suppliers when banks acquire the control right after a covenant violation. This indirect cost of covenant violations should be factored into the ex ante design of the loan contract if borrowers rationally expect the conflicts. In particular, a borrower that cannot afford the consequence of losing trade credit (or hurting the supplier) would optimally choose a loan contract that has looser debt covenants, that is, a contract with a smaller ex ante probability of covenant violations.

To examine how the importance of trade credit or dependent suppliers impacts the ex ante loan contracting, I conduct a loan-level regression of contract strictness on TRADE_CREDIT and dependent supplier indicator (DEP_SUPPLIER) separately. Both measured prior to the contracting date, TRADE_CREDIT proxies for a firm's reliance on trade credit and DEP_SUPPLIER measures the importance of the supplier. A dependent supplier contributes relationship-specific investments but is vulnerable to a bank's control right due to a high switching cost.

I follow the recent development of the literature to measure loan contract strictness. Murfin (2012) pioneers the area by proposing a strictness measure that incorporates the number of covenants, the initial slack of these covenants, and the covariance between the financial ratios underlying these covenants. Employing a more flexible estimation approach, Demerjian and Owens (2016) construct a new measure of contract strictness that incorporates Murfin's logic, includes more covenant categories, and, importantly, addresses the measurement error problem. In the current study, I use Demerjian and Owens' (2016) measure to quantify the ex ante probability of covenant violation (denoted as STRICTNESS). Using Murfin's measure does not alter our empirical results though, as the two measures are highly correlated.

The sample construction begins with all loans in Dealscan that have a nonmissing contract strictness measure and loan spread measure (AISD).[14] Then I match these loans with quarterly observations of corresponding nonfinancial borrowers in Compustat between 1996 and 2008. Following Murfin (2012), control variables include loan characteristics obtained from Dealscan (ln(MATURITY), ln(LOAN_SIZE), ln(LENDER_COUNT), SECURED, and loan type dummies), firm characteristics prior to contracting (ln(ASSET), ZSCORE, RATING, and MTB) and covenant controls (LEVERAGE, NET_WORTH, CURRENT_RATIO, and INTEREST_EXPENSE). I also control for the VIOLATION_SEC dummy (Nini et al. (2012)), which can proxy for the prior enforcements of material covenant violations by lead arrangers that could affect the forthcoming loan contracting (Bird, Ertan, Karolyi and Ruchti (2017)). I require nonmissing information on these variables and on TRADE_CREDIT and DEP_SUPPLIER. The final sample for estimation consists of 7,024 loan-level observations. The summary statistics of the sample are reported in Panel B of Table 1. The mean value of contract strictness is 0.414, suggesting that the ex ante probability of violating a covenant for an average firm is 41.4%. The average loan spread of the sample is around 2%. The average trade credit of the current sample (57.9%) is greater than that of the RD sample (51%).

The regression results are presented in Table 7. To overcome the omitted variable problem, I include both borrower fixed effects to control for unobserved, time-invariant

[14] AISD (all-in-spread drawn) measures the interest rate spread on a loan (over LIBOR) plus any associated fees in originating the loan. Thus, AISD is an all-inclusive measure of loan price (Bharath, Dahiya, Saunders and Srinivasan (2007)). I divide AISD by 100 to express it in percentage points.

firm characteristics and industry-quarter fixed effects to account for industry-wide economic shocks. Column (1) shows that the coefficient on TRADE_CREDIT is -0.039 and statistically significant. The estimate suggests that a one-standard-deviation change in TRADE_CREDIT (0.684) explains 6.4% of a one-standard-deviation change in STRICTNESS (0.417). In column (2), the negative and statistically significant coefficient on DEP_SUPPLIER suggests that having one or more dependent suppliers would reduce the covenant violation probability by 5.5%, which is a 13.3% decline for an average firm in the sample (the average STRICTNESS of the sample is 41.4%). These results imply that when signing loan contracts, borrowers (and lenders) take into account the effect of bank interventions on trade credit and dependent suppliers and, accordingly, adjust the covenant tightness.

[Table 7 is here]

I then study whether the loan spread impounds borrower's concession in nonpricing terms due to the reliance on trade credit or dependent suppliers. Since banks are more vulnerable to managerial agency problems when looser debt covenants are set, they are likely to require a higher risk premium for such a problem. Consistent with this argument, the result in column (3) of Table 7 shows a positive and statistically significant coefficient on TRADE_CREDIT (0.167, $p < 0.001$). The estimate suggests that a one-standard-deviation change in TRADE_CREDIT (0.684) explains 9.3% of a one-standard-deviation change in AISD (1.227). In column (4), DEP_SUPPLIER also has a positive and significant coefficient (0.128, $p = 0.062$), the magnitude suggesting that having one or more dependent suppliers would increase the loan spread by 0.128%, which is a 6.4% increase for an average firm in the sample (the average AISD is 2.00%). The results imply that borrowers relying more on trade credit and dependent suppliers are willing to substitute high interest rates for restrictive covenants.

However, why are higher interest rates preferred to stricter loan covenants? The answer lies in the fact that debt covenant violations usually happen in bad times. Thus, given the risk aversion of firm managers, the risk-adjusted probabilities of violations (bank interventions) are considerably larger than objective probabilities. Consequently, the risk-adjusted present value of violation costs is higher than the non-risk-adjusted value. Put simply, risk-averse managers will care more about covenant violations than is suggested by risk-free valuations. Ex ante, such managers would find a higher interest rate to be more acceptable than more restrictive covenants. This argument is similar to Almeida and Philippon (2007), who argue that risk-adjusted distress cost is much higher than is implied by historical default probabilities, because financial distress is more likely to occur in bad times.

Overall, the ex ante loan contracting results support Hypothesis 2—when signing a debt contract, the borrowing firm indeed takes into account the potential negative effect of banks' interventions on trade creditors (and their willingness to provide trade credit) and chooses a loan contract that minimizes this effect. This evidence is consistent with Demiroglu and James (2010) in that the selection of loan contract takes into account the ex post outcomes of covenant restrictiveness.

B Cross-Sectional Tests

The above empirical approach can suffer from omitted variable bias, namely, an unobservable demand-side factor potentially drives the association between trade credit (dependent suppliers) and contract terms. Due to the lack of an exogenous variation in the supply of trade credit (or the presence of dependent suppliers), it is difficult to conclude that the loan contracting results are purely driven by the supply-side effect. But I perform several cross-sectional analyses, which generate findings that substantiate the supply-side effect but are inconsistent with demand effect interpretations.

First, I investigate how heterogeneities on the supply side that are not directly related to borrowers' fundamentals affect the association between trade credit and loan strictness. Specifically, I examine how the results are affected when the supplier conducts more unique or relationship-specific investments. The prediction is that, if the suppliers have invested more relationship-specific capital or have produced more unique products for the borrower, on the one hand, it is harder for these suppliers to switch customers and potential bank interventions have more bite on the suppliers; on the other hand, the borrower would rely more on such suppliers and care more about the potential cost imposed on them (Titman (1984); Titman and Wessels (1988); Banerjee et al. (2008)). Therefore, ex ante, borrowers are more likely to sign a loan contract that minimizes the probability of bank interventions by choosing a less restrictive contract. Following the literature (e.g., Titman and Wessels (1988); Raman and Shahrur (2008)), I employ two proxies for suppliers' investment specificity: the mean value of suppliers'

R&D/sales (SUP_RD) and the mean value of suppliers' SG&A/sales (SUP_SGA), both measured in the year prior to contracting. In Panel A of Table 8, I construct interaction variables TRADE_CREDIT×SUP_RD, DEP_SUPPLIER×SUP_RD and separately include them in the contract design regressions, together with SUP_RD itself. When contract strictness is the dependent variable, the coefficients on both TRADE_CREDIT×SUP_RD and DEP_SUPPLIER×SUP_RD are negative and significant, suggesting that suppliers' investment specificity enhances the strictness reducing effect of trade credit and dependent suppliers. When loan spread is the dependent variable, the coefficients on both TRADE_CREDIT×SUP_RD and DEP_SUPPLIER×SUP_RD are positive and significant, implying a tradeoff between the nonpricing and pricing terms. Furthermore, I similarly perform the cross-sectional tests using SUP_SGA, and again find that the coefficients on both TRADE_CREDIT×SUP_SGA and DEP_SUPPLIER×SUP_SGA are negative and statistically significant when contract strictness is the dependent variable. When loan spread is the dependent variable, the coefficients on these two interaction terms are positive but statistically insignificant.

Second, drawing on the theories of trade credit and loan contracting, the most likely demand-side effect that potentially explains the ex ante loan contracting result is borrowers' growth opportunities. A higher level of trade credit or the presence of dependent suppliers is associated with more potential growth opportunities. Since restrictive covenants lead to more constraints or interventions on investment (Chava and

Roberts (2008)), firms with more investment opportunities will optimally choose a contract with less restrictive covenants (Demiroglu and James (2010)). Thus, these firms are willing to trade off the pricing term for a more favorable nonpricing term, resulting in looser covenants and a higher spread. If this is the case, the correlation between trade credit (or dependent suppliers) and loan contract strictness should be even more negative when borrowers have more growth opportunities. To formally test this, I use two proxies for firms' investment opportunities, market-to-book ratio (MTB) and the quarterly growth rate of total sales (SALE_GROWTH), both measured in the quarter prior to contracting. I am interested in the coefficients on TRADE_CREDIT×MTB, DEP_SUPPLIER×MTB, TRADE_CREDIT×SALE_GROWTH, and DEP_SUPPLIER×SALE_GROWTH when these terms are individually included in the contract strictness regression as well as in the AISD regression. The regression results are reported in Panel B of Table 8. None of the coefficients are statistically significant, except the coefficient on DEP_SUPPLIER×SALE_GROWTH in the strictness regression. However, the positive sign suggests that the contract is tighter when there are more growth opportunities, inconsistent with the investment opportunity argument.

Taken together, the results show that supplier-side characteristics, which are not directly linked to borrowers' demand, are largely attributable to the results of loan contract terms. Therefore, the evidence is more consistent with supply-side effects than it is with demand-side effects.

[Tables 8 is here]

VII Conclusion

A loan contract will allocate contingent control rights to the bank following a borrower's covenant violation, which may impose negative externalities on other creditors. Using an RD design, this study documents robust evidence of a sizable trade credit cut after a covenant violation, consistent with bank interventions adversely affecting trade creditors' claims. Further findings suggest that the trade credit cut is mitigated when the violating borrower has dependent suppliers and becomes deeper when the conflicts between banks and suppliers are likely to be severe.

An optimal loan contract should take the costs into consideration, making the tripwires of bank interventions sensitive to the importance of trade credit and the supplier-customer relationship. Consistently, I find that if a borrower relies more on trade credit or has dependent suppliers who have dedicated relationship-specific investments, it would sign a less restrictive loan contract, that is, a contract that has a smaller ex ante probability of bank intervention. Overall, my results demonstrate that creditor conflicts can have significant consequences for a firm and should be considered in financial contracts.

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Figure 1: Regression Discontinuity (RD) Plots of Firm Characteristics

The figure plots the average covariates around the covenant threshold. The x-axis is the relative distance of financial ratio to the corresponding threshold (DISTANCE). Each dot represents the average covariate for firms within one of 20 equally spaced bins on either side of the threshold. The fitted lines are based on local linear regressions with a 95% confidence interval around the fitted value. Bandwidth selection is based on Calonico, Cattaneo and Titiunik (2014) (CCT bandwidth) with rectangular kernel weights.

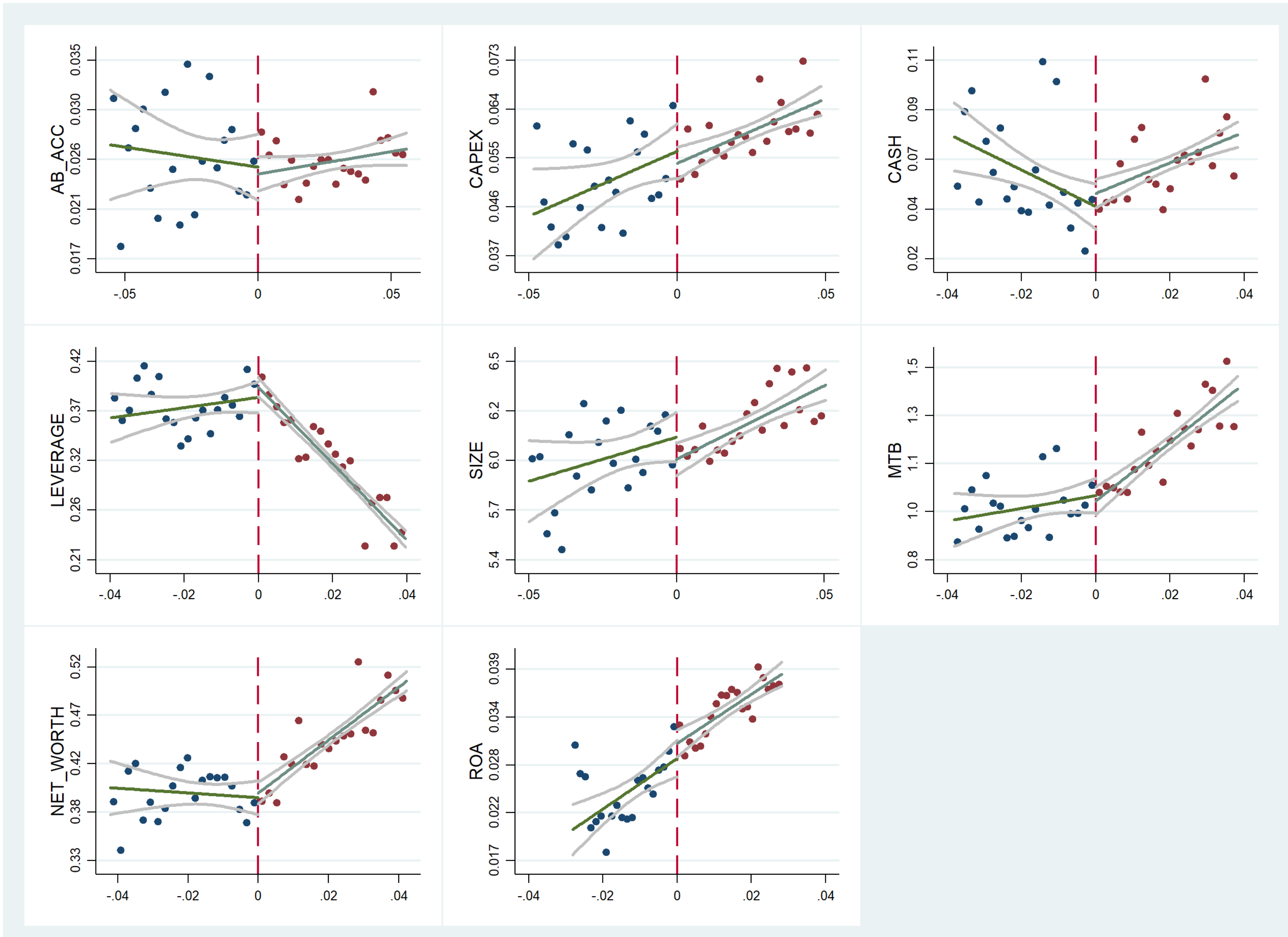

Figure 2: Density Distribution of the Distance from the Covenant Threshold

The figure shows the test result based on McCrary (2008). I plot the density of the running variable in a chosen bandwidth around the covenant threshold (CCT bandwidth of estimating equation (2) with rectangular kernel weights). The x-axis represents the distance between the financial ratio and the corresponding threshold (DISTANCE) and the y-axis represents the density of the running variable. The solid line depicts the fitted density function of the running variable with a 95% confidence interval.

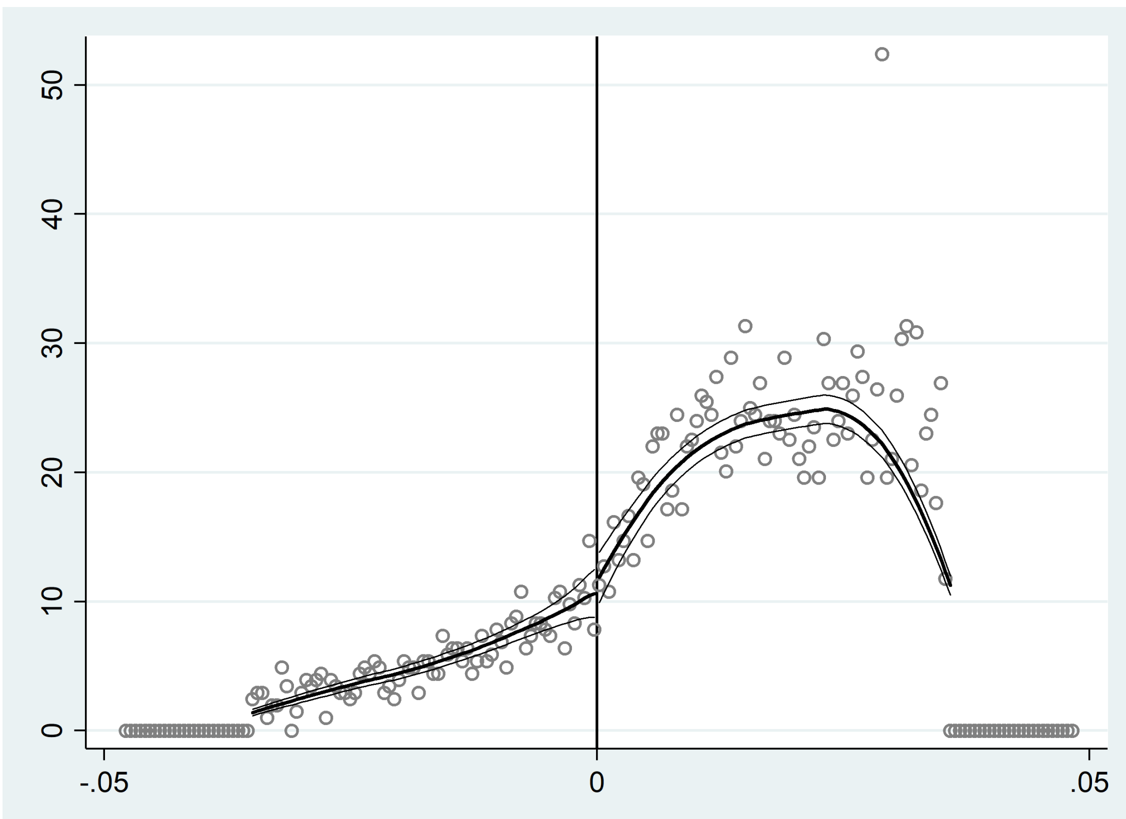

Figure 3: RD Plots: Bank Interventions and Trade Credit

This figure plots the average trade credit against the running variable. The x-axis is the lagged DISTANCE, which is divided into 20 bins on either side of the threshold and each dot represents the average trade credit for a bin. The fitted curves are based on linear regressions (left graphs) and quadratic regressions (right graphs), separately, with a 95% confidence interval around the fitted value. The upper two graphs use CCT bandwidth and the lower two use IK bandwidth (Imbens and Kalyanaraman (2012)).

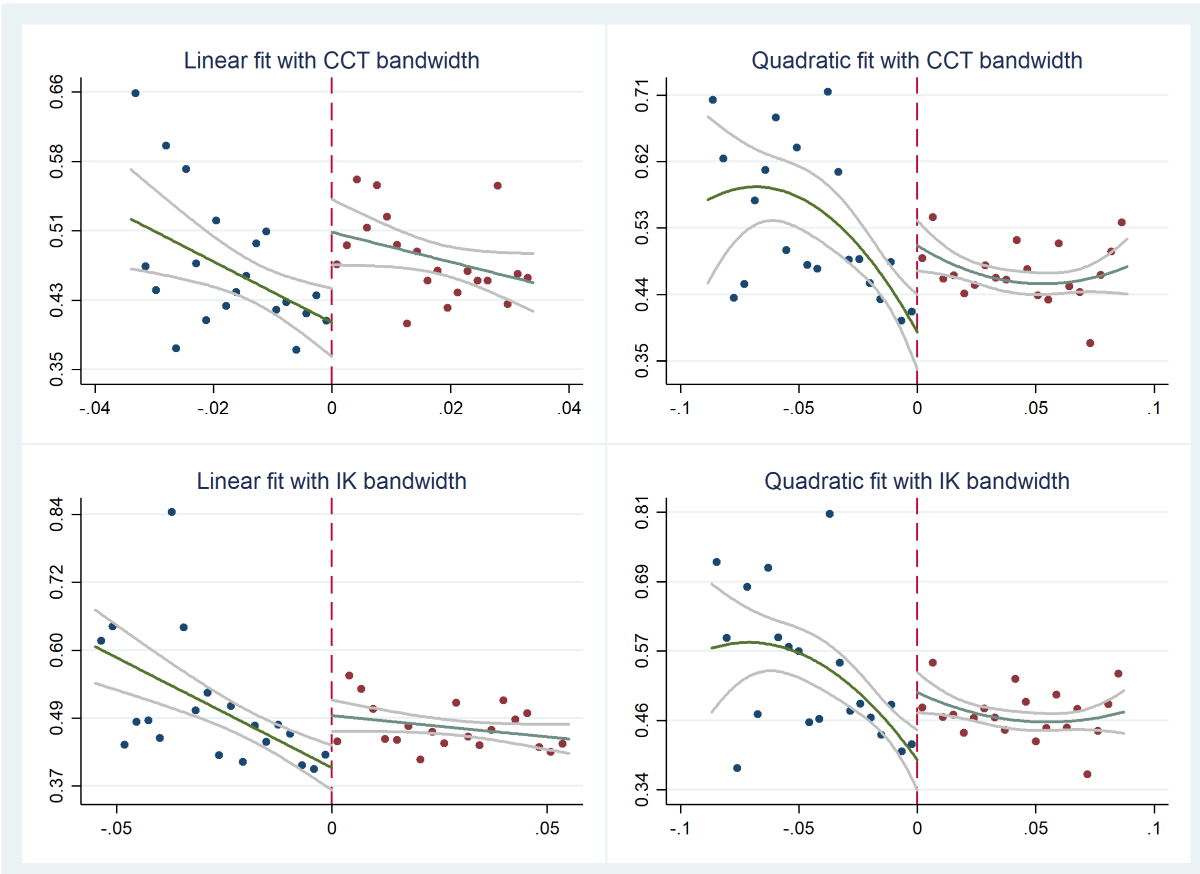

Figure 4: Difference-in-Differences Plot for Firms Surrounding the Covenant Threshold

This figure presents the time series of average trade credit of violating firms and nonviolating firms in the narrow band around the covenant threshold. The x-axis represents the relative quarters to the quarter in which the two groups of firms fall into the narrow band around the threshold. I use the CCT bandwidth of estimating equation (2) to gauge the narrow band.

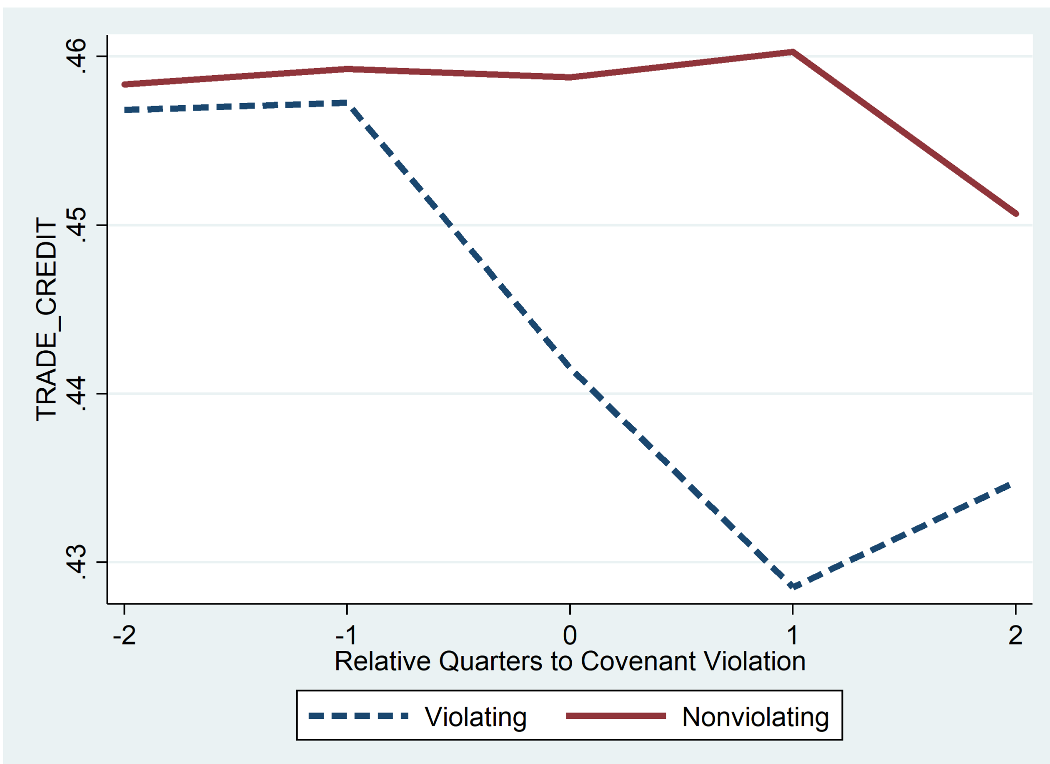

Table 1: Summary Statistics

Panel A presents summary statistics (means, standard deviations, 25th percentile, medians, and 75th percentile) for a sample of nonfinancial firms whose loan covenant thresholds can be identified using the Dealscan database. The sample is an unbalanced panel of 14,670 firm-quarters between 1996 and 2008. Panel B presents the summary statistics of the sample of loan contract strictness, which consists of 7,024 loans borrowed by 2,053 nonfinancial firms from 1996 to 2008. Variable definitions appear in the Appendix.

Panel A. Summary Statistics of the RD Sample

	N	Mean	Std. Dev.	25th percentile	Median	75th percentile
TRADE_CREDIT	14,670	0.510	0.520	0.246	0.386	0.569
VIOLATION	14,670	0.112	0.315	0.000	0.000	0.000
DISTANCE	14,670	0.540	1.029	0.023	0.058	0.256
ASSET ($ million)	14,670	1904.18	4256.96	175.34	536.41	1466.78
ROA	14,670	0.036	0.025	0.023	0.035	0.049
MTB	14,670	1.392	0.943	0.780	1.097	1.670
CASH	14,670	0.084	0.107	0.014	0.039	0.112
AB_ACC	14,670	0.028	0.030	0.008	0.018	0.036
NET_WORTH	14,670	0.495	0.180	0.366	0.484	0.621
LEVERAGE	14,670	0.244	0.166	0.113	0.241	0.358
CAPEX	14,670	0.060	0.049	0.027	0.046	0.078

Panel B. Summary Statistics of the Loan Contract Strictness Sample

	N	Mean	Std. Dev.	25th percentile	Median	75th percentile
STRICTNESS	7,024	0.414	0.417	0.026	0.188	0.937
AISD (%)	7,024	2.003	1.227	1.000	2.000	2.750
MATURITY	7,024	3.697	0.654	3.526	4.043	4.094
LOAN_SIZE ($ million)	7,024	275.94	431.43	40.00	125.00	300.00
SECURED	7,024	0.343	0.475	0.000	0.000	1.000
LENDER_COUNT	7,024	9.069	9.263	3.000	6.000	12.000
ZSCORE	7,024	1.936	2.255	0.808	1.507	2.490
RATING	7,024	5.702	6.195	0.000	0.000	11.000
ASSET ($ million)	7,024	2,654.51	5,639.57	213.14	675.40	2,039.15
MTB	7,024	1.365	1.007	0.808	1.099	1.592
TRADE_CREDIT	7,024	0.579	0.684	0.269	0.418	0.646
DEP_SUPPLIER	7,024	0.173	0.379	0.000	0.000	0.000

Table 2: RD Estimates of the Effect of Bank Interventions on Trade Credit

This table presents the RD estimates for trade credit at the covenant threshold. Each cell represents the coefficient on VIOLATION for each specification. The discontinuity estimates are reported separately for local linear regressions and local quadratic regressions. I use the optimal bandwidth proposed by Calonico et al. (2014) for different kernel weighting methods. Robust bias-corrected standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
Linear regressions	-0.070***	-0.074***	-0.078***
	(0.026)	(0.027)	(0.027)
Observations	3,985	5,431	4,962
Quadratic regressions	-0.088***	-0.080***	-0.078***
	(0.030)	(0.029)	(0.029)
Observations	9,279	9,548	9,408
Kernel	Rectangular	Triangular	Epanechnikov

Table 3: RD Estimates using Alternative Bandwidths

This table shows estimates of the RD design using several alternative bandwidths. Panel A uses different multiples of the optimal bandwidth based on Calonico et al. (2014). Panel B uses the optimal bandwidth proposed by Imbens and Kalyanaraman (2012). Discontinuity estimates from both local linear regressions and local quadratic regressions are reported. Robust bias-corrected standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Multiples of CCT Bandwidth

	(1)	(2)	(3)	(4)
CCT bandwidth ×	50%	75%	125%	150%
Linear regressions	-0.094**	-0.092***	-0.065**	-0.062**
	(0.042)	(0.035)	(0.029)	(0.025)
Observations	1,981	2,961	4,963	5,897
Quadratic regressions	-0.093**	-0.074**	-0.124***	-0.095***
	(0.040)	(0.035)	(0.029)	(0.026)
Observations	5,207	7,598	9,801	10,112
Kernel	Rectangular	Rectangular	Rectangular	Rectangular

Panel B. IK Bandwidth

	(1)	(2)	(3)
Linear regressions	-0.066***	-0.063***	-0.063***
	(0.025)	(0.024)	(0.024)
Observations	6,327	7,902	7,457
Quadratic regressions	-0.084***	-0.083***	-0.081***
	(0.033)	(0.030)	(0.031)
Observations	9,237	9,688	9,562
Kernel	Rectangular	Triangular	Epanechnikov

Table 4: Robustness Checks

This table shows robustness checks for the RD design. In Panel A (B), I first conduct an OLS regression of TRADE_CREDIT on ln(ASSET), MTB, ROA, CASH, NET_WORTH, CAPEX, LEVERAGE, and AB_ACC (industry and quarter dummies), then apply nonparametric estimations on the residual trade credit obtained from OLS estimation. In Panel C, I create an arbitrary covenant threshold that is equal to a certain multiple of the actual threshold, then apply nonparametric estimations using the pseudo-threshold. Robust bias-corrected standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Controlling for Covariates

	(1)	(2)	(3)
Linear regressions	-0.088***	-0.085***	-0.088***
	(0.030)	(0.029)	(0.029)
Observations	4,197	5,439	5,021
Quadratic regressions	-0.097***	-0.090***	-0.087***
	(0.034)	(0.032)	(0.032)
Observations	7,859	9,344	8,954
Kernel	Rectangular	Triangular	Epanechnikov

Panel B. Controlling for Fixed Effects

	(1)	(2)	(3)
Linear regressions	-0.064***	-0.060***	-0.063***
	(0.024)	(0.023)	(0.023)
Observations	4,168	5,627	5,193
Quadratic regressions	-0.072***	-0.063**	-0.060**
	(0.027)	(0.027)	(0.027)
Observations	7,890	9,474	9,237
Kernel	Rectangular	Triangular	Epanechnikov

Panel C. Placebo Tests

	(1)	(2)	(3)	(4)
Actual threshold ×	50%	75%	125%	150%
Linear regressions	0.018	-0.002	0.049	0.020
	(0.018)	(0.018)	(0.043)	(0.041)
Quadratic regressions	0.016	-0.020	0.022	-0.017
	(0.019)	(0.024)	(0.044)	(0.045)
Kernel	Rectangular	Rectangular	Rectangular	Rectangular

Table 5: Heterogeneous Treatment Effects on Trade Credit

This table presents estimates of local linear regressions of trade credit using subsamples partitioned by whether the firm cuts CAPEX, whether the firm cuts LEVERAGE, whether the firm has one or more dependent suppliers, whether an unrelated borrower of the common bank had a default in the prior quarter, and the median of loan-to-trade-credit ratio. All regressions rely on CCT bandwidth and rectangular kernel for estimation. Variable definitions appear in the Appendix. Robust bias-corrected standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	CAPEX drop		LEVERAGE drop		Dependent supplier		Unrelated borrower default		Loan/(Trade Credit)	
	Yes	No	Yes	No	Yes	No	Yes	No	Low	High
Linear regressions	-0.070**	-0.066*	-0.055**	-0.078**	-0.038	-0.078**	-0.117***	-0.005	-0.018	-0.125***
	(0.033)	(0.039)	(0.027)	(0.036)	(0.056)	(0.034)	(0.035)	(0.045)	(0.027)	(0.047)
Observations	2,284	2,167	2,702	2,693	1,259	3,349	1,982	2,487	4,225	2,264

Table 6: Parametric Regressions

This table presents estimates of parametric regressions using firm-quarters falling in a chosen bandwidth around the covenant threshold. Columns (1) to (6) use the CCT bandwidth for nonparametric local linear regression of trade credit with rectangular kernel. Columns (7) to (10) use the IK bandwidth for nonparametric local linear regression of trade credit with rectangular kernel. Standard errors clustered by firm are reported in parentheses. I alter the regression specification by including or excluding the quadratic term of the lagged DISTANCE. All regressions control for industry and quarter fixed effects. Variable definitions are presented in the Appendix. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	CCT bandwidth						IK bandwidth			
VIOLATION	-0.085**	-0.093**	-0.085**	-0.092**	-0.093**	-0.098**	-0.085**	-0.086**	-0.089**	-0.092**
	(0.038)	(0.038)	(0.038)	(0.039)	(0.038)	(0.038)	(0.036)	(0.038)	(0.036)	(0.038)
DISTANCE	-1.320	-4.750	-2.107	-5.171	-3.435**	-5.445	-0.963	-1.193	-1.677	-2.207
	(1.618)	(3.569)	(1.487)	(3.544)	(1.487)	(3.497)	(1.001)	(2.430)	(1.033)	(2.349)
VIOLATION×DISTANCE	-1.171	5.191	0.472	6.153	1.643	5.374	-1.687	-1.260	-1.214	-0.234
	(2.547)	(6.703)	(2.420)	(6.522)	(2.385)	(6.365)	(1.786)	(4.328)	(1.825)	(4.215)
ln(ASSET)			0.013	0.012	0.017	0.017	0.022*	0.022*	0.027**	0.027**
			(0.015)	(0.015)	(0.015)	(0.015)	(0.013)	(0.013)	(0.013)	(0.013)
ROA			-0.377	-0.376	-0.002	-0.003	-1.113*	-1.113*	-0.980	-0.979
			(0.833)	(0.833)	(0.815)	(0.815)	(0.663)	(0.663)	(0.640)	(0.640)
MTB			0.043	0.043	0.043	0.043	0.040*	0.041*	0.032	0.032
			(0.030)	(0.030)	(0.030)	(0.030)	(0.023)	(0.023)	(0.023)	(0.023)
CASH			0.809**	0.810**	0.667**	0.667**	0.559**	0.559**	0.453**	0.453**
			(0.324)	(0.324)	(0.300)	(0.300)	(0.237)	(0.237)	(0.226)	(0.226)
AB_ACC			-0.158	-0.161	-0.452	-0.453	0.107	0.106	-0.105	-0.107
			(0.312)	(0.312)	(0.316)	(0.316)	(0.292)	(0.291)	(0.289)	(0.287)
NET_WORTH			-0.114	-0.112	-0.587***	-0.584***	-0.148	-0.148	-0.467***	-0.468***
			(0.126)	(0.126)	(0.173)	(0.174)	(0.114)	(0.114)	(0.175)	(0.175)
LEVERAGE					-0.627***	-0.624***			-0.435**	-0.436**
					(0.181)	(0.181)			(0.190)	(0.189)
CAPEX					0.832***	0.831***			1.083***	1.083***
					(0.308)	(0.308)			(0.276)	(0.276)
DISTANCE^2	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.213	0.213	0.245	0.245	0.262	0.262	0.195	0.195	0.211	0.211
Observations	3,985	3,985	3,985	3,985	3,985	3,985	6,327	6,327	6,327	6,327

Table 7: Loan Contracting and Reliance on Trade Credit and Dependent Suppliers

Results from loan-level regressions of loan contract terms on TRADE_CREDIT and DEP_SUPPLIER, respectively. STRICTNESS comes from Demerjian and Owens (2016) and measures the ex ante probability of covenant violation. AISD comes from Dealscan database and measures loan spread. Covenant controls include borrower's LEVERAGE, NET_WORTH, CURRENT_RATIO, and INTEREST_EXPENSE. Industry classifications are based on Fama-French 48 industries. Loans of financial firms are excluded. Variable definitions appear in the Appendix. Standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	STRICTNESS		AISD	
TRADE_CREDIT	-0.039***		0.167***	
	(0.012)		(0.042)	
DEP_SUPPLIER		-0.055***		0.128*
		(0.020)		(0.068)
AISD	0.022***	0.022***		
	(0.005)	(0.005)		
STRICTNESS			0.255***	0.249***
			(0.056)	(0.056)
ln(MATURITY)	0.010	0.010	-0.086**	-0.085**
	(0.011)	(0.011)	(0.037)	(0.037)
ln(LOAN_SIZE)	-0.013**	-0.013**	-0.119***	-0.120***
	(0.005)	(0.005)	(0.017)	(0.017)
SECURED	0.026	0.027	0.091	0.090
	(0.017)	(0.017)	(0.058)	(0.058)
ln(LENDER_COUNT)	0.017**	0.017**	-0.038	-0.037
	(0.007)	(0.007)	(0.025)	(0.025)
ZSCORE	-0.015**	-0.016**	0.077***	0.079***
	(0.007)	(0.007)	(0.024)	(0.024)
RATING	-0.003	-0.003	-0.019***	-0.020***
	(0.002)	(0.002)	(0.007)	(0.007)
ln(ASSET)	0.037**	0.041***	-0.125**	-0.129**
	(0.016)	(0.016)	(0.052)	(0.053)
MTB	-0.024*	-0.023	-0.290***	-0.293***
	(0.014)	(0.014)	(0.048)	(0.048)
VIOLATION_SEC	0.049**	0.048**	0.226***	0.231***
	(0.021)	(0.021)	(0.071)	(0.072)
Covenant controls	Yes	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes	Yes
Borrower firm FE	Yes	Yes	Yes	Yes
Industry×Quarter FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.742	0.742	0.725	0.724
Observations	7,024	7,024	7,024	7,024

Table 8: Cross-Sectional Analyses of Loan Contract Design

The table presents cross-sectional analyses on the association between TRADE_CREDIT and loan contract terms. Loans of financial firms are excluded. Other controls include all the control variables (including various fixed effects) that appear in Table 7. Variable definitions appear in the Appendix. Standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	STRICTNESS				AISD			
Panel A: Suppliers' Investment Specificity								
TRADE_CREDIT×SUP_RD	-0.386***				4.251***			
	(0.144)				(1.385)			
DEP_SUPPLIER×SUP_RD		-0.403***				1.687*		
		(0.127)				(0.945)		
TRADE_CREDIT×SUP_SGA			-0.186**				0.061	
			(0.091)				(0.122)	
DEP_SUPPLIER×SUP_SGA				-0.665***				0.169
				(0.187)				(0.202)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.900	0.900	0.899	0.900	0.766	0.764	0.759	0.760
Observations	1,224	1,224	1,224	1,224	1,224	1,224	1,224	1,224
Panel B: Borrowers' Growth Opportunities								
TRADE_CREDIT×MTB	0.007				-0.030			
	(0.011)				(0.036)			
DEP_SUPPLIER×MTB		-0.013				0.092		
		(0.026)				(0.090)		
TRADE_CREDIT×SALE_GROWTH			-0.014				0.014	
			(0.013)				(0.045)	
DEP_SUPPLIER×SALE_GROWTH				0.140**				-0.318
				(0.065)				(0.223)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.737	0.738	0.738	0.738	0.715	0.714	0.715	0.714
Observations	7,024	7,024	7,024	7,024	7,024	7,024	7,024	7,024

Appendix: Definitions of Variables

This appendix describes the definitions of variables used in this study in terms of Compustat data mnemonics.

Dependent Variables

- AISD: Interest rate spread on a loan (over LIBOR) plus any associated fees in originating the loan.
- STRICTNESS: Loan contract strictness, or, the ex ante probability of covenant violation, from Demerjian and Owens (2016).
- TRADE_CREDIT: $apq/cogsq$

Control Variables

- AB_ACC: The absolute value of discretionary accruals, estimated from the performance-adjusted modified cross-sectional Jones model (Dechow and Dichev (2002)).
- ASSET: The book value of total assets, atq
- CAPEX: $capxy$ adjusted for fiscal quarter accumulation divided by the lagged net property, plant, and equipment ($ppentq$).
- CASH: $cheq/atq$
- DEP_SUPPLIER: A dummy variable that equals 1 if the firm has a dependent supplier and 0 otherwise.
- DISTANCE: The distance of the actual financial ratio to the covenant threshold in a fiscal quarter. DISTANCE equals $\frac{r'-\underline{r}}{\sigma}$ for covenants limiting a minimum threshold and $-\frac{r'-\underline{r}}{\sigma}$ for those limiting a maximum threshold, where r' is the actual financial ratio, $\underline{r}$ is the covenant threshold, and σ is the standard deviation of r'.
- LENDER_COUNT: The number of lenders in a fiscal quarter.
- LOAN_SIZE: The loan amount in millions of U.S. dollars.
- MTB: $(prccq * cshoq + tdq + pstkq - txditcq)/atq$
- MATURITY: The stated maturity of a loan in months.
- RATING: Credit ratings based on S&P long-term debt ratings. The highest notch, AAA, is assigned a value of 22, the lowest notch, D, is assigned a value of 1, and nonrated borrowers are assigned a value of 0.
- ROA: $oibdpq/atq$
- SALE_GROWTH: Borrower's quarterly growth rate of total sales ($saleq$) before the loan contracting date.

- SECURED: A dummy variable that equals 1 for loans that are secured and 0 otherwise.
- SUP_RD: The mean value of suppliers' $xrd/sale$ before the loan contracting date.
- SUP_SGA: The mean value of suppliers' $xsga/sale$ before the loan contracting date.
- VIOLATION: A dummy variable that equals 1 if the firm violates a covenant (DISTANCE<0), and 0 otherwise (DISTANCE>0).
- VIOLATION_SEC: A dummy variable that equals 1 when a firm reports a covenant violation in SEC filings in a fiscal quarter and 0 otherwise.
- ZSCORE: $1.2*((actq-lctq)/atq)+1.4*(req/atq)+3.3*(oibdpq/atq)+0.6*(prccq*cshoq/ltq)+0.999*(saleq/atq)$

Covenant Variables

- CURRENT_RATIO: $actq/lctq$
- Debt to EBITDA: $(dlcq+dlttq)/oibdpq$
- EBITDA: $oibdpq$
- INTEREST_EXPENSE: $xintq/atq$
- LEVERAGE: $(dlcq+dlttq)/atq$
- NET_WORTH: $(atq-ltq)/atq$
- Senior Debt to EBITDA: $(dlcq+dlttq-ds)/oibdpq$
- Senior Leverage: $(dlcq+dlttq-ds)/atq$