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Auditor Quality and Debt Covenants

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ABSTRACT: This study examines the impact of auditor quality on financial covenants in debt contracts. We conjecture that high-quality auditors have two related effects on these debt covenants: (i) they encourage fewer and less restrictive covenants by providing assurance to lenders at contract inception and, consequently, (ii) they ensure a lower probability of eventual covenant violations. Consistent with the conjectures, we find that auditor quality is negatively associated with the intensity and tightness of financial covenants. Specifically, high-quality auditors are associated with fewer covenants (especially performance covenants) and less binding covenants. Additionally, we find that auditor quality is negatively associated with the likelihood of covenant violations. In an ancillary test, we provide evidence that high-quality auditors mitigate the detrimental effect of covenant violations on the cost of borrowing. Together, these findings highlight the important role of auditors in debt contracting.

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1. Introduction

Covenants play an important role in mitigating information asymmetry and agency problems in debt contracting (e.g., Jensen and Meckling 1976; Smith and Warner 1979; Smith 1993). However, intensive and strict covenants could be costly for borrowing firms because their violation triggers a negative stock market reaction as well as significant refinancing and restructuring costs (Beneish and Press 1995). For example, Beneish and Press (1993) report that violations trigger refinancing costs of about 1 percent and restructuring costs of about 0.4 percent of a firm's total assets. Surprisingly, given the importance of covenants in debt contracting and the significant costs of covenant violations, few studies examine whether specific monitoring mechanisms (e.g., auditors) can mitigate these costs related to debt covenants.[1] Our paper fills the gap by examining whether high-quality auditors decrease lenders' demand for covenants serving as "trip wires" in debt contracting and, consequently, reduce the likelihood of subsequent covenant violations.[2]

Information asymmetry and the conflicts of interest between security issuers and capital providers engender significant information and agency risks for capital providers (Beatty and Ritter 1986). For example, in the context of debt contracting, these risks are manifested in a variety of moral hazard problems such as asset substitution and claim dilution (Bebchuk 2002). The prior literature argues that auditors, as external monitors, can mitigate both

[1] One exception is Zhang (2008), which shows that conservative accounting increases the probability of covenant violations.

[2] According to prior literature (e.g., Zhang 2008; Kim, Tsui, and Yi 2011b), a significant number of debt covenants are written in terms of thresholds for accounting variables (hereafter, "financial covenants"), while all other nonfinancial covenants, such as restrictions on dividend payment or sales of assets, are generally labelled as general covenants. As in much of the literature we follow, our study focuses on financial covenants.

information and agency problems (see, e.g., Watts and Zimmerman 1983; Francis and Wilson 1988; Balsam, Krishnan, and Yang 2003).

Consistent with this view, Minnis (2011) demonstrates a demand for auditing by borrowers and attendant benefits even when auditing is not mandated. Specifically, Minnis (2011) presents evidence that private firms compiling audited financial statements enjoy a lower cost of debt. In our public firm setting, in which all firms are required to disclose audited financial statements, the corresponding notion is that high-quality auditors mitigate moral hazard and adverse selection problems in debt contracting to a greater extent than low-quality auditors. This risk mitigation may manifest itself in two ways with regard to debt covenants. First, high-quality auditors may be associated with fewer and looser covenants. Second, if high-quality auditors lead to fewer and looser covenants, ceteris paribus, they would also be associated with *lower* likelihood of covenant violations. We refer to these two conjectured effects as the *contracting effect* and the *violation reduction effect*, respectively.

Using a sample of 9,849 loan-year observations from 1996 to 2007, we examine the relation between auditor quality and the structure of financial covenants. Using two traditional proxies for auditor quality (industry expertise and size), we find that auditor quality is negatively related to covenant intensity (the number of financial covenants), covenant tightness (whether financial covenant thresholds are relatively binding), and the use of performance covenants (covenants focusing on current-period profitability and efficiency as defined in Christensen and Nikolaev 2012). Overall, the result shows that high-quality auditors are associated with both fewer and looser covenants in debt contracts. This is consistent with the *contracting effect* of auditors.

Next, using a sample of 35,181 firm-year observations from 1996 to 2007, we find a negative relation between auditor quality and the probability of financial covenant violations. Specifically, the probability of covenant violations is about 1.45 percent lower for firms audited by industry experts and about 4.98 percent lower for firms audited by Big N auditors compared to other firms. Given that the mean value of violation probability is about 14 percent in our sample, these results indicate a reasonably strong effect of high-quality auditors on the likelihood of covenant violations. To mitigate the concern that endogeneity influences the observed negative relation between auditor quality and covenant violations, we apply a variety of empirical methods including the Heckman two-step procedure, analysis of auditor changes, and propensity score matching. Overall, the finding of a negative and significant effect of high-quality auditors on the likelihood of covenant violations is consistent with the *violation reduction effect* of auditors.

Finally, we conduct an ancillary test to further support our arguments. We find that after a first-time covenant violation, banks charge firms a significantly higher loan spread, which is the basis points a borrower pays in excess of the London Interbank Offered Rate (LIBOR) or LIBOR equivalent for each dollar drawn down. However, such a post-violation increase in the loan spread appears to be smaller in firms audited by high-quality auditors. This finding suggests that high-quality auditors play a role in mitigating the adverse effect of covenant violations on corporate borrowing costs and therefore complements our main findings.

Our study makes several contributions. First, it contributes to the literature examining the relation between financial reporting quality and debt contracting. Sloan (2001, 343) notes that, despite the significant role of accounting information in debt contracting, “there has been little research on the role of accounting information in financial contracting.” In response to his influential work, subsequent studies explore how various attributes of accounting information affect the cost of debt.[3] Our study differs in that we focus on how auditor quality influences non-price terms of debt—debt covenants and their violations. Our study sheds light on the significant role of auditors in efficient debt contracting and responds to the call in Armstrong, Guay, and Weber (2010) for examining how the financial reporting environment, including auditor quality, affects the efficiency of debt contracting.

Second, our study contributes to the literature on covenant violations. The importance of covenant violations has been studied in the literature since the early 1990s (e.g., Sweeney 1994). Recent studies (e.g., Nini, Smith, and Sufi 2012) focus on the consequences of covenant violations. In contrast, our paper is the first study examining whether high-quality auditors can reduce the likelihood of covenant violations. We contribute to the literature by linking auditor quality to debt covenants and their violations, thereby providing insight into how external monitors affect debt contracting ex ante, and therefore, the likelihood of covenant violations ex post.

[3] For example, recent studies examine how accrual quality (Bharath, Sunder, and Sunder 2008), conservatism (Ball, Robin, and Sadka 2008), and earnings predictability (Hasan, Park, and Wu 2012) affect debt contracting.

Finally, our study contributes to the literature on auditor quality. Prior studies on auditor quality mainly focus on the impact of auditors on earnings quality (e.g., Becker, DeFond, Jiambalvo, and Subramanyam 1998) and, consequently, on the interest cost of debt financing (e.g., Blackwell, Noland, and Winters 1998). Different from these studies, we contribute by showing that high-quality auditors encourage more favorable debt covenant terms and, thereafter, reduce the probability of covenant violations. Additionally, we provide some evidence that high-quality auditors can mitigate the adverse consequences of covenant violations. Given the significant consequences of covenant violations, our findings complement earlier evidence that high-quality auditors decrease the overall cost of debt.

The rest of the paper is organized as follows. Section 2 reviews the prior literature and explains how auditor quality potentially affects covenant structure and the probability of covenant violations. Section 3 introduces the research design, describes sample construction, and presents summary statistics. Section 4 presents empirical results. Section 5 provides a brief summary and concluding remarks.

2. Background and hypotheses

Debt covenants and their violations

Debt covenants are conditions imposed on the borrower, and they can serve as “trip wires” when lenders face high levels of information and agency risk (e.g., Dichev and Skinner 2002; Frankel, Seethamraju, and Zach 2008; Chava, Kumar and Warga 2010; Demerjian 2014). Prior empirical studies generally show that covenant intensity and tightness are positively related to the level of information or agency risk. For example, Kim et al. (2011b) find that enhanced disclosures due to IFRS adoption alleviate the information risk and mitigate potential agency conflicts, leading to fewer financial covenants; Hasan, Park, and Wu (2012) find that banks are less likely to use financial covenants for firms with higher earnings predictability. Other studies showing that more severe information problems lead to more intensive or tighter covenants include Drucker and Puri (2009), Knyazeva and Knyazeva (2012), and Miller and Reisel (2012).

Additionally, prior studies find results that firm-level governance mechanisms (Ge, Kim, and Song 2012), board monitoring (Fields, Fraser, and Subrahmanyam 2012; Francis, Hasan, Koetter, and Wu 2012), government monitoring (Black, Carnes, Mosebach, and Moyer 2004), and analyst coverage (Francis, Hasan, and Liu 2014) are negatively associated with covenant intensity or tightness.[4] The fact that the various governance mechanisms listed above decrease covenant intensity and tightness suggests that auditor quality may have the same effect.

When firms violate covenants, significant consequences arise in the context of a renegotiated agreement between violating firms and their creditors, such as increased refinancing cost, restructuring cost, and negative stock market reactions (Beneith and Press 1993, 1995). In addition, some recent large-sample studies find that there are other equally significant consequences such as reduced investments (Chava and Roberts 2008), impaired access to financing (Roberts and Sufi 2009), and increased CEO turnover (Nini et al. 2012).[5] We add to this literature by investigating whether auditor quality is associated with less onerous covenants and fewer violations.

Auditor quality

Auditors play important roles as external monitors (e.g., Jensen and Meckling 1976; Becker et al. 1998; Nelson, Elliott, and Tarpley 2002). Francis (2004) provides an extensive review of the auditor quality literature. Early studies in this literature use the size of the audit firm as a proxy for auditor quality, and more recent studies measure

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[4] Ge et al. (2012) conduct tests on a sample of non-U.S. firms from 22 countries during 2003–07 and find that banks impose fewer restrictive covenants in better-governed firms. Fields et al. (2012) and Francis et al. (2012) find that debt contracts include fewer covenants when the borrower's board characteristics are more effective in mitigating agency risk.

[5] Most of these studies examine the consequences of covenant violations. Two recent studies examine the determinants of covenant violation probability. Zhang (2008) finds that borrowers with more conservative accounting practices are more likely to violate debt covenants following a negative price shock, and that creditors require lower interest rates when lending to such firms. Francis, Hasan, and Sun (2011) examine the relation between CEO compensation and covenant violations. They find that CEO compensation *delta* reduces the likelihood of violations while *vega* increases it.

auditor quality using industry expertise. Prior literature has provided overwhelming evidence that high-quality auditors decrease information risks. For example, Big N or industry-expert auditors are associated with lower discretionary accruals (Becker et al. 1998; Balsam et al. 2003), higher disclosure quality (Dunn and Mayhew 2004), and lower likelihood of restatements (Chin and Chi 2009).

Although the majority of the literature focuses on the role of auditors in reducing information risk, certain influential papers such as Watts and Zimmerman (1983) and Jensen and Meckling (1976) highlighted the role of auditors in decreasing agency costs, restraining malfeasance by managers, and deterring expropriation. In the same vein, Francis and Wilson (1988, 663) state that "auditing is widely viewed as a means of reducing agency costs." Thus, high-quality auditors reduce agency risks as well as information risks for firms. Because of this and because information and agency risks affect the pricing of debt as well as the design of debt covenants (e.g., Graham, Li, and Qiu 2008; Hasan, Hoi, Wu, and Zhang 2014), we conjecture that auditor quality has a significant impact on various aspects of debt contracting including covenants.[6]

Auditor quality and debt covenants

Auditor quality could potentially affect the intensity and tightness of covenants in conflicting ways. One may argue that lenders are more likely to use financial covenants because high-quality auditors increase the reliability of accounting information. For example, Costello and Wittenberg-Moerman (2011) provide evidence that lenders use fewer financial covenants when a firm experiences a material internal control weakness (ICW).[7] This line of

[6] Prior empirical studies (e.g., Blackwell et al. 1998; Mansi, Maxwell, and Miller 2004; Pittman and Fortin 2004; Minnis 2011) have shown that auditor quality has a significant impact on the interest cost of debt financing. Specifically, these studies suggest that high-quality auditors facilitate debt contracting and help borrowing firms reduce the interest cost of debt financing. However, none of these studies examine whether high-quality auditors influence nonprice terms of bank loans, which is the focus of our study.

[7] However, Kim et al. (2011a) find that lenders impose more financial covenants on ICW firms than on non-ICW firms.

argument implies that high-quality auditors are associated with more intensive and stringent covenants at loan inception.

However, as discussed earlier, the majority of prior studies suggest that the factors alleviating information or agency risks act as *substitutes* for debt covenants. The auditing literature indicates that high-quality auditors decrease information risks (e.g., Becker et al. 1998; Dunn and Mayhew 2004) and act as independent monitors in reducing agency costs, restraining malfeasance by managers, and deterring expropriation (e.g., Francis and Wilson 1988; Fan and Wong 2005). This naturally suggests that high-quality auditors, who reduce information and agency risks for lenders, could *decrease* lenders' demand for intensive or stringent covenants. We label this the *contracting effect* of auditors.

Auditor quality and covenant violations

We conjecture that high-quality auditors could have two effects on the likelihood of debt covenant violations. The previously described *contracting effect* suggests that high-quality auditors help borrowing firms reduce the intensity and tightness of covenants. Because less-binding covenants lead to a lower probability of covenant violations (Watts and Zimmerman 1990), the contracting effect predicts a *negative* relation between auditor quality and the probability of covenant violations. We call this the *violation reduction effect* of auditors.

On the other hand, high-quality auditors may influence the probability of covenant violations by discouraging earnings manipulation. Based on the assumption that covenant violations are costly, earlier studies argue that managers make reporting decisions to avoid violating covenants. These studies (e.g., DeFond and Jiambalvo 1994; Sweeney 1994) detect a tendency on the part of firms to manipulate earnings in situations where such manipulation could avoid imminent covenant violations. It is also well known that high-quality auditors deter earnings management (e.g., Becker et al. 1998; Krishnan 2003). One would therefore expect that high-quality auditors increase the probability of covenant violations by constraining earnings management.[8]

[8] We acknowledge that the violation reduction effect and the deterrence of earnings management effect of auditors on the likelihood of covenant violations may not take place simultaneously and therefore are not mutually exclusive.

All told, the net effect of high-quality auditors on the likelihood of covenant violations depends on the relative strengths of the two aforementioned, opposite effects. We however consider the *violation reduction effect* to be a first-order effect, and expect an overall negative relation between auditor quality and violation probability. Thus, our tests on this conjecture would also shed light on the economic significance of the *contracting effect* of high-quality auditors.

3. Research design, sample selection, and summary statistics

The effect of auditor quality on debt covenant structure

We use the following ordinary least squared (OLS) regression model to investigate the effect of auditor quality on the intensity of debt covenants:

$$
\begin{aligned}
\textit{Financial covenant intensity}_{it} = {} & \beta_0 + \beta_1\,\textit{Auditor industry expertise}_{it\text{-}1} + \beta_2\,\textit{ln(Sales)}_{it\text{-}1} + \beta_3\,\textit{Leverage}_{it\text{-}1} \\
& + \beta_4\,\textit{ROA}_{it\text{-}1} + \beta_5\,\textit{Cash holding}_{it\text{-}1} + \beta_6\,\textit{Tangibility}_{it\text{-}1} \\
& + \beta_7\,\textit{Capital expenditure}_{it\text{-}1} + \beta_8\,\textit{Earnings volatility}_{it\text{-}1} \\
& + \beta_9\,\textit{Z-score}_{it\text{-}1} + \beta_{10}\,\textit{Dividend}_{it\text{-}1} + \beta_{11}\,\textit{Senior debt rating}_{it\text{-}1} \\
& + \beta_{12}\,\textit{Institutional ownership}_{it\text{-}1} + \beta_{13}\,\textit{Prior violation}_{it\text{-}1} \\
& + \beta_{14}\,\textit{ln(Spread)}_{it} + \beta_{15}\,\textit{ln(Loan size)}_{it} + \beta_{16}\,\textit{ln(Loan maturity)}_{it} \\
& + \beta_{17}\,\textit{Syndication}_{it} + \textit{Inverse Mills ratio}_{it\text{-}1} + \varepsilon_{it}\,. \qquad (1)
\end{aligned}
$$

We estimate a similar model, replacing *Financial covenant intensity* with *Financial covenant tightness*. We also estimate (1) with *Big N* as an alternative to *Auditor industry expertise*. The models describe the covenant structure for a specific loan facility *i* that a firm borrowed in year *t*. The test variables are as follows. *Financial covenant intensity* is defined as the number of financial covenants in a loan contract (e.g., Chava et al. 2010; Demiroglu and James 2010). We follow Demiroglu and James (2010) to construct the covenant tightness measure using the two covenants based on the debt-to-EBITDA ratio and the current ratio.[9] Specifically, a current ratio covenant for a loan

[9] We follow Demiroglu and James (2010) to define a tight current ratio covenant. We place firms with similar current ratios at loan inception into clusters. Within each cluster, we compute the cluster median value of the covenant threshold choices. Then, a covenant with a threshold greater than the cluster median value is defined as a tight covenant. Please refer to Figure 1 of Demiroglu and James (2010) for a visual illustration of their approach used to define covenant tightness.

is defined as tight if its threshold is *above* the corresponding cluster's median value; in contrast, because lower thresholds for the covenant based on debt-to-EBITDA ratio indicate stricter covenants, a debt-to-EBITDA covenant is tight if its threshold is *below* the corresponding cluster's median value. The variable *Financial covenant tightness* equals one for a loan if there is at least one tight covenant (out of the two covenants) and zero otherwise. [10]

The key independent variables, *Auditor industry expertise* and *Big N*, are two common proxies for auditor quality. A significant and negative coefficient (β_1) of the auditor quality variables would support the contracting effect of auditors. Following prior studies (e.g., Graham et al. 2008), we control for various firm characteristics as well as loan characteristics (e.g., loan spread, loan size, and loan maturity). Please refer to Appendix 1 for detailed variable definitions. We also include the *Inverse Mills ratio* calculated from the first stage of the two-step Heckman procedure (discussed in Appendix 2) as an additional control to mitigate the potential self-selection problem. In addition, we employ two-digit Standard Industrial Classification (SIC) industry dummies, year dummies, audit opinion dummies, loan type and loan purpose dummies to control for the potential industry, year, audit opinion, loan type and purpose effects.

The effect of auditor quality on the likelihood of covenant violations

We estimate the following logit regression models to investigate the effect of auditor quality on the likelihood of covenant violations, with an analogous specification substituting *Big N* for *Auditor industry expertise*:

$$\begin{aligned} Violation_{it} = {} & \beta_0 + \beta_1\, Auditor\ industry\ expertise_{it\text{-}1} + \beta_2\, ln(Sales)_{it\text{-}1} + \beta_3\, Leverage_{it\text{-}1} \\ & + \beta_4\, ROA_{it\text{-}1} + \beta_5\, Cash\ holdings_{it\text{-}1} + \beta_6\, Tangibility_{it\text{-}1} + \beta_7\, Capital\ expenditure_{it\text{-}1} \\ & + \beta_8\, Earnings\ volatility_{it\text{-}1} + \beta_9\, Z\text{-}score_{it\text{-}1} + \beta_{10}\, Dividend_{it\text{-}1} \\ & + \beta_{11}\, Senior\ debt\ rating_{it\text{-}1} + \beta_{12}\, Institutional\ ownership_{it\text{-}1} \\ & + \beta_{13}\, Prior\ violation_{it\text{-}1} + Inverse\ Mills\ ratio_{it\text{-}1} + \varepsilon_{it}; \qquad (2) \end{aligned}$$

where the dependent variable, *Violation*, equals one if a firm has at least one violation of financial covenant in a year and zero otherwise. Our main interest is in the coefficient of the auditor quality variable (β_1). A negative coefficient would be consistent with high-quality auditors lowering the likelihood of covenant violations. Following prior

[10] Our results hold if we define the covenant tightness dummy using three popular covenants (namely, covenants based on the debt-to-EBITDA ratio, the interest coverage ratio, and the fixed charge ratio).

studies (e.g., Zhang 2008), we control for several firm characteristics related to the probability of covenant violations. We also include the *Inverse Mills ratio* from the selection model (discussed in Appendix 2). In addition, we control for two-digit SIC industry dummies, year dummies, and audit opinion dummies.

Sample selection and summary statistics

In the previous two subsections, we discuss equations (1) and (2) used to test our main research questions: (i) whether high-quality auditors influence the structure of financial covenants, and (ii) whether high-quality auditors reduce the likelihood of covenant violations. The first equation is estimated using 9,849 loan-level observations, and the second equation is estimated using 35,181 firm-year observations. There are three main data sources: COMPUSTAT (financial and auditor quality variables); Nini et al. (2012) dataset (financial covenant violations); and DealScan (loan information). The sample used for the aforementioned first equation (9,849 loans) makes use of all three sources while the sample used for the second equation (35,181 firm-years) uses the first two. Thus, our sample construction process starts with the latter sample first and then moves on to the former.

Specifically, we start with all firm-year observations in the COMPUSTAT annual database. To obtain debt covenant violation information, we use the data provided by Nini et al. (2012) who collect violations of financial covenants in private debt agreements for the universe of COMPUSTAT nonfinancial firms (SIC code outside of 6000 to 6999) from 1996 to 2007 by extracting information from SEC 10-K and 10-Q filings.[11] Note that Nini et al. (2012) search for violations from both quarterly and yearly financial statements. To avoid double-counting—the same violation may be reported in consecutive quarters—we set *Violation* equal to one if a firm has one or more violations in a certain year and zero otherwise. We merge this annual covenant-violation data with COMPUSTAT annual data that include auditor characteristic and firm financial variables. Because it is only meaningful to examine covenant violations for firms with leverage, we delete observations with zero leverage. The resulting sample has 35,181 firm-year observations for 8,042 unique firms from 1996 to 2007. In this sample, 85.61 percent of firm-year

[11] For more details concerning the construction of the violation dataset, please refer to "Data Appendix for Creditor Control Rights, Corporate Governance, and Firm Value" in Nini et al. (2012). As the SEC does not require firms to disclose exactly which covenant has been violated, the data of Nini et al. (2012) do not contain information about the types of covenants violated.

observations (30,118 out of the 35,181 firm-year observations) have zero covenant violations, which is consistent with the finding in the prior literature (e.g., Chava and Roberts 2008). Next, we merge the above sample with the DealScan database, which provides comprehensive coverage of U.S. loan facilities. This merge results in a sample with information pertaining to 9,849 loans and their related firm-year financial variables.[12]

We present the summary statistics for these two samples in Table 1. In panel A, we report the summary statistics for the full sample of 35,181 firm-year observations, the 30,118 firm-year observations without violations, and the 5,063 firm-year observations with violations. The full-sample summary statistics indicate that the majority of firm-years (about 74 percent) are associated with Big N auditors. We note that industry experts are associated with about 39.6 percent of firm-years. The distributions of our main test variables are comparable to those reported in prior studies. For example, Francis, Maydew, and Sparks (1999) report that about 72 percent of firm-years are associated with Big N auditors; and Francis et al. (2004) report that about 35 percent of firm-years are associated with industry experts. The mean value of *Leverage* is around 0.36. The mean values of *Z-score*, *Tangibility*, and *Capital expenditure* are around 1.156, 0.29, and 0.06, respectively. Among the firm-year observations in this sample, 12.5 percent pay dividends and 5.8 percent have debt ratings available in the COMPUSTAT database. Institutional investors hold approximately half of firms' shares in our sample. Also, 29.5 percent of firm-years have prior violations.

The summary statistics for the violation and nonviolation subsamples indicate that the nonviolation subsample is associated with higher-quality auditors. The nonviolation subsample also has greater *Sales*, lower *Leverage*, greater *Z-score*, greater *Tangibility*, and higher *Institutional ownership*. We note that the violation and nonviolation subsamples do not have a significant difference in *ROA* (0.012 versus 0.011). This is consistent with Zhang (2008), who finds a statistically insignificant correlation between ROA and the violation dummy.

Panel B of Table 1 presents the summary statistics for a variety of loan characteristics for the sample of 9,849 loans used to estimate equation (1). This equation controls for several firm characteristics, so panel B also presents the summary statistics for these firm-characteristic variables. We find that the loans on average have 1.766

[12] We winsorize continuous variables at the 1 percent and the 99 percent levels in both samples.

financial covenants, consistent with prior studies (e.g., Kim, Song, and Zhang 2011a; Knyazeva and Knyazeva 2012; Demerjian 2014). Christensen and Nikolaev (2012) classify financial covenants into two categories: capital covenants and performance covenants. Capital covenants require that shareholders maintain enough capital and rely on information about sources and uses of capital (i.e., balance sheet information). In contrast, performance covenants rely on current-period profitability and efficiency indicators formulated in terms of income statement (or cash flow statement) information alone or in combination with balance sheet data (e.g., debt-to-cash flow ratio). Of the 1.766 financial covenants, about 69 percent are performance covenants (*P-covenants*) and the rest are capital covenants (*C-covenants*). We find that 19.9 percent of loans contain at least one tight covenant. Panel B also shows that the average loan spread is about 201 basis points, the mean loan size is $641 million, and the average loan maturity is 45 months. The firm characteristics reported in panel B are comparable to those reported in panel A for the 35,181 firm-year sample.

4. Empirical results

Auditor quality and covenant structure

In Table 2, panel A, model 1 presents results for industry expertise, and model 2 presents results for auditor size. In both models, the coefficient of the auditor quality variable is negative and statistically significant. For example, in model 2 of panel A, the coefficient of *Big N* is –0.054 with a *t*-value of –2.43. This means that there are 0.054 fewer financial covenants in firms audited by Big N auditors compared to firms audited by non-Big N auditors. Turning to control variables, the coefficients of firm-level and loan-level control variables are generally consistent with those reported in prior studies such as Graham et al. (2008). For example, firms with greater firm size and higher cash holding are less likely to use covenants. We find that *Inverse Mills ratio* is statistically significant, validating inclusion of the correction for self-selection bias.

Similar conclusions are obtained concerning covenant tightness. Table 2, panel B shows that the coefficient of the auditor quality variable is negative and statistically significant. For example, in model 1 the coefficient of *Auditor industry expertise* is –0.105 with a *z*-value of –2.10. The marginal effect indicates that the probability of using tight covenants is 2.29 percent lower for firms with industry expert auditors compared to firms without industry expert auditors. The results are consistent with the conjecture that the presence of high-quality auditors is associated with fewer covenants as well as reduced covenant tightness.

To provide further evidence of the contracting effect, we test a corollary of our main argument by examining separately two types of financial covenants: capital covenants and performance covenants. As mentioned earlier, capital covenants focus on capital adequacy, while performance covenants focus on current-period profitability and efficiency. Christensen and Nikolaev (2012) argue that performance covenants better address information and agency problems by acting as trip wires or timely indicators of distress.

In the context of our study, since high-quality auditors reduce information and agency risks for lenders, we expect high-quality auditors to have a more pronounced and negative effect on the usage of performance covenants. Evidence in Table 3 supports this expectation. Specifically, we estimate equation (1) separately for the number of performance covenants (models 1 and 2), capital covenants (models 3 and 4), and the ratio of performance covenants to total covenants (models 5 and 6). We find that the coefficient of auditor quality variables is significantly negative in performance-covenant regressions (models 1-2 and 5-6), while it is not significant in capital-covenant regressions (models 3-4).

Overall, the results in Tables 2 and 3 indicate that high-quality auditors are associated with less restrictive financial covenant terms (fewer covenants overall, looser covenants, and fewer performance covenants). This implies that lenders face a lower level of information and agency risk when high-quality auditors are present, leading them to lower their demand for intensive and stringent covenants. These results support the contracting effect.

Auditor quality and the likelihood of covenant violations

The likelihood of covenant violations

As discussed in the hypothesis section, the finding that high-quality auditors are associated with less intensive and stringent financial covenants suggests that high-quality auditors likely have an overall *negative* effect on the likelihood of covenant violations. Therefore, a test of the relation between auditor quality and violation probability can shed light on the economic significance of the contracting effect in the sense that such an effect is significant enough to offset the potential increase in covenant violations resulting from high-quality auditors constraining earnings management.

We estimate equation (2) to examine this issue and report results in Table 4. In model 1, we find that the coefficient of *Auditor industry expertise* is −0.143 with a *z*-value of −3.52. In model 2, the coefficient of *Big N* is −0.373 with a *z*-value of −7.55. These results are economically meaningful. For example, the calculation of marginal effects shows that the probability of covenant violations is about 1.45 percent lower for firms audited by industry experts compared to other firms and about 4.98 percent lower for firms audited by Big N auditors compared to other firms. When compared to the mean value of violation probability of about 14 percent, these results indicate a reasonably strong effect of high-quality auditors on the likelihood of covenant violations.

Turning to controls, we find significant coefficients for most control variables with the signs consistent with predictions. We note that the coefficients of *Inverse Mills ratio* are statistically significant. This justifies the correction for self-selection bias. Consistent with prior studies (e.g., Zhang 2008), violations appear more likely for smaller and more leveraged firms. Also, violations are positively related to earnings volatility and prior violation, and negatively linked to the Altman *Z*-score, institutional ownership, and capital expenditure. Our interpretation is that violations are generally related to inherent operational, financial, and governance-related risks. To the extent that these variables capture firms' inherent risks, the negative relation between auditor quality and covenant violations is less likely to be driven by firms' inherent risks. Finally, we note that our models have pseudo R^2 of around 0.11, comparable to other studies (e.g., Zhang 2008).

Endogeneity in covenant violation tests

In previous analyses, we mitigate self-selection bias by using the Heckman two-step procedure. However, the covenant violation tests are likely affected by different endogeneity issues (e.g., reverse causality); therefore, we employ two additional econometric approaches in this section. First, we examine auditor changes to partially mitigate the reverse causality concern. Second, we use a propensity-score matching analysis (Lawrence, Minutti-

Meza, and Zhang 2011) to address the concern that certain firm characteristics influence both the likelihood of covenant violations and auditor choice inducing a negative relation between the two variables.

The auditor-change test exploits three types of auditor changes: increasing quality (from non-Big N to Big N or from non-expert to expert), decreasing quality (from Big N to non-Big N or from expert to non-expert), and lateral (a change to another auditor in the same quality level). Auditor changes that increase quality are captured by *Change to Big N* and *Change to Experts*. Changes that decrease quality are instead captured by *Change to Non-Big N* and *Change to Non-Experts*. We use the dummy variable, *Post*, to denote observations following an auditor change. Using these variables, we estimate the following logit regression model, with an analogous specification substituting *Experts* / *Non-Experts* for *Big N* / *Non-Big N*:

$$\begin{aligned} Violation_{it} = {} & \beta_0 + \beta_1\, Post_{it} + \beta_2\, Post_{it} \times Change\ to\ Big\ N_{it} + \beta_3\, Post_{it} \times Change\ to\ Non\ Big\ N_{it} \\ & + \beta_4\, ln(Sales)_{it\text{-}1} + \beta_5\, Leverage_{it\text{-}1} + \beta_6\, ROA_{it\text{-}1} + \beta_7\, Cash\ holdings_{it\text{-}1} \\ & + \beta_8\, Tangibility_{it\text{-}1} + \beta_9\, Capital\ expenditure_{it\text{-}1} + \beta_{10}\, Earnings\ volatility_{it\text{-}1} \\ & + \beta_{11}\, Z\text{-}score_{it\text{-}1} + \beta_{12}\, Dividend_{it\text{-}1} + \beta_{13}\, Senior\ debt\ rating_{it\text{-}1} \\ & + \beta_{14}\, Institutional\ ownership_{it\text{-}1} + \beta_{15}\, Prior\ violation_{it\text{-}1} \\ & + Inverse\ Mills\ ratio_{it\text{-}1} + \varepsilon_{it}. \end{aligned} \quad (3)$$

The coefficient of *Post* captures the benchmark effect of a lateral auditor change. The coefficients of the interaction terms reflect the incremental probability of a violation for firms that increase or decrease auditor quality relative to firms with a lateral auditor change. Consistent with the violation reduction effect, we expect the coefficients of *Post*×*Change to Big N* or *Post* ×*Change to Experts* and *Post*×*Change to Non-Big N* or *Post*×*Change to Non-Experts* to be negative and positive, respectively. As in previous tests, we include the *Inverse Mills ratio* to control for self-selection bias (please see the last paragraph of Appendix 2 for details). In addition, we control for two-digit SIC industry dummies, year dummies, and audit opinion dummies.

We apply the following two filters to increase the possibility that the auditor at contract inception is also the auditor at violation. First, we restrict our sample to firms with only one auditor change in the entire sample period.[13] Second, because the typical debt contract has a maturity of three years (Nini et al. 2012), we discard firms

[13] In robustness tests, we expand our sample to include firms with multiple auditor changes in our sample period. For these firms, we define the auditor change variables based on the last occurrence of an auditor change. This

with covenant violations within two years following the auditor change.[14] Defining our data in this manner, we obtain 2,269 firms with a single auditor change during the sample period. Among these firms, for *Big N* as the auditor quality measure, the numbers of firms with quality-increasing, quality-decreasing, and lateral auditor changes are 50, 549, and 1,670, respectively.[15] For industry expertise as the auditor quality measure, the analogous numbers are 367, 469, and 1,433. The sample used to estimate equation (3) consists of 11,454 firm-year observations from these 2,269 firms.

Table 5, panel A reports results of the auditor-change analysis. Consistent with our conjecture, model 1 shows that the coefficient of the interaction term, *Post*×*Change to Big N*, is significantly negative and the coefficient of the interaction term, *Post*×*Change to Non-Big N*, is significantly positive. This indicates that in firms switching from a non-Big N auditor to a Big N auditor (a Big N auditor to a non-Big N auditor), there is a significantly lower (higher) probability of covenant violations compared to firms making a lateral change. Similarly, in model 2, the coefficient of *Post*×*Change to Experts* is significantly negative while the coefficient of *Post*×*Change to Non-Experts* is significantly positive. Thus, increasing auditor quality leads to a lower likelihood of financial covenant violations, while decreasing auditor quality has an opposite effect.

[Insert Table 5]

Next, we report the propensity score analysis in which we identify matching pairs audited by high-quality auditors and low-quality auditors in the following steps. First, we estimate the following logit regression for each year: the dependent variable is coded one if a firm is audited by a high-quality auditor in a given year and zero

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alternative procedure has the advantage of a slightly larger sample but has the disadvantage that multiple auditor changes possibly contaminate the result. We find our results are qualitatively unchanged.

[14] In a robustness test, we also delete firms with violations in the third year following the auditor change and find our results hold.

[15] There are two potential explanations as to why the number of firms changing from Big N to non-Big N auditors is significantly larger than the number of firms changing from non-Big N auditors to Big N auditors. First, fewer than 30 percent of sample firms are audited by non-Big N auditors. Since there are fewer firms engaging non-Big N auditors in the first place, it is reasonable that there are fewer switches from non-Big N to Big N auditors. Second, the greater number of firms changing from Big N to non-Big N auditors is possibly driven by the emergence of second-tier auditors in the United States.

otherwise; the independent variables include all the firm-level control variables in the auditor quality selection model, that is equation (A.1) in Appendix 2. This generates a predicted probability of being audited by a high-quality auditor for each observation. Second, we match, without replacement, each treatment firm (a firm audited by a high-quality auditor) with a matching firm (a firm audited by a low-quality auditor) having the closest propensity score, using a caliper—the difference in the predicted probabilities between treatment and control observations—of 10 percent. This procedure results in two matched samples: 21,466 firm-year observations from 10,733 matched pairs audited by Big N and non-Big N auditors and 11,200 firm-year observations from 5,600 matched pairs audited by industry-expert and nonexpert auditors.

Using the matched samples, we rerun our baseline equation (2). Table 5, panel B reports the results. For brevity, we only display the main test variables. We find that the coefficients of both auditor quality variables remain negative and statistically significant. These results further mitigate the endogeneity concern and corroborate the earlier evidence.

Other robustness checks

We conduct a variety of robustness checks. For brevity, we do not tabulate these results, but they are available upon request. First, we address the possibility that violations reported in two consecutive years may actually reflect a single violation event by setting *Violation* to zero in the second year. We rerun tests using this alternate measure and find that the coefficients of the two auditor quality variables continue to be significantly negative.

Second, we address the possibility that the auditor has changed between contract inception and violation (creating noise in results) by excluding firms that change auditors during the sample period so that only firms *without* any auditor change are analyzed. This ensures that our estimate of the effect of auditors on the probability of covenant violations reliably indicates the effect of the same auditor and is not contaminated by an auditor change between loan contract inception and covenant violation. We continue to find that the coefficients of both auditor quality variables are negative and statistically significant.

Third, we address concerns that governance variables have been omitted by incorporating additional controls for corporate governance mechanisms relating to compensation structure and board independence. Using data from COMPUSTAT ExecuComp and RiskMetrics Directors databases, we define two new variables. *Fixed salary* is the ratio of CEO total cash salary to total compensation, which is inversely related to pay-for-performance sensitivity (Francis et al. 2011). *Board independence* equals the number of outside directors divided by the total number of directors, which captures the quality of the board of directors. We add these variables to our baseline regression equation (2). Although adding these two controls significantly reduces our sample size, we still find that auditor quality is significantly and negatively related to the likelihood of financial covenant violations.

Subsample analyses: Firms with high default risk

As discussed in the hypothesis section, the relation between auditor quality and the likelihood of covenant violations depends on the relative strengths of the violation reduction effect and auditors' deterrence of earnings management. Overall, from our results, it appears the former outweighs the latter. However, it is possible that for certain firms with higher likelihood of managing earnings (e.g., firms with high default risk), a stronger deterrence of earnings management could result in a less negative (or even a positive) relation between auditor quality and the likelihood of covenant violations. We examine this prediction in this section.

We use the Altman *Z*-Score and the Ohlson *O*-Score as alternate measures of a firm's default risk. We first define firms with high default risk as those with a below-median *Z-score* or an above-median *O-score*. We create a dummy variable, *High default risk*, to indicate firms with high default risk. We modify our baseline regression equation (2) by adding this *High default risk* dummy and its interaction term with auditor quality. We are interested not only in the coefficient of auditor quality, but also in the interaction variable. A positive coefficient of the interaction variable would suggest that the negative overall effect of auditor quality on covenant violations is *weaker* among firms with greater default risk.

We estimate this modified regression equation (2) using the sample of 35,181 firm-year observations. In Table 6, panel A (panel B) reports results when a firm's default risk is measured by *Z-score* (*O-score*). We find that the coefficients of the auditor quality variables (*Auditor industry expertise* and *Big N*) remain negative and statistically significant. More importantly, we find that the coefficients of the interaction terms, *Auditor industry expertise*×*High default risk* and *Big N*×*High default risk*, are all significantly positive. The results are consistent with our prediction that there is a stronger deterrence of earnings management effect among high defaultrisk firms attenuating the overall negative relation between auditor quality and the likelihood of covenant violations. Nevertheless, even in this subsample, the overall effect of auditor quality on the likelihood of covenant violations is negative.[16]

Can auditor quality mitigate covenant violation costs?

Prior studies show that when covenants are violated, significant costs are borne by the violating firms (e.g., Beneish and Press 1993; Nini, Smith, and Sufi 2009). In this section, we first confirm these costs in terms of increased loan spreads and then show that high-quality auditors help alleviate these costs.

We use DealScan data to examine how new borrowing cost changes after covenant violations. We measure bank loan cost using loan spread, which is defined as the basis points a borrower pays in excess of the London Interbank Offered Rate (LIBOR) or LIBOR equivalent for each dollar drawn down. To examine whether covenant violations influence a firm's cost of bank debt, we compare the spreads of loans obtained by this firm before a first-time covenant violation with the spreads of loans after the violation. We focus on first-time violations for two reasons. First, it allows us a relatively clear definition of the previolation period and the postviolation period. Second, prior studies show that first-time violations are more significant events than subsequent violations (e.g., Roberts and Sufi 2009).

[16] Specifically, in model 1 of Table 6, panel A, we find that the overall effect of *Auditor industry expertise* on the violation probability for high-default-risk firms is –0.087 (=0.149 – 0.236) and it is statistically insignificant. In model 2 of panel A, the overall effect of *Big N* for high-default-risk firms is –0.254 (=0.292 – 0.546) and it is significant at the 1 percent level. When we use *O-score* as the proxy for default risk in panel B, we find that overall effect of *Auditor industry expertise* on the violation probability for high-default-risk firms is –0.064 (=0.128 – 0.192) in model 1, and it is significant at the 10 percent level. The overall effect of *Big N* for high-default-risk firms is –0.153 (=0.418 – 0.571) in model 2, and it is significant at the 1 percent level.

As we intend to compare loan spreads for the pre- and postviolation periods, we require the sample firms to have at least one new loan during both the pre- and postviolation periods. The final sample contains 1,728 firms with 5,728 loans, 2,827 (2,901) of which are initiated before (after) the firm's first-time covenant violation.[17] We construct a dummy variable, *Post (First violation)*, which equals one if the firm obtained the loan facility after its first-time violation and zero otherwise.

To demonstrate that violations increase the loan spread, we follow Graham et al. (2008) and Hasan et al. (2014) to estimate the following equation:

$$\begin{aligned} Loan\ (Spread)_{it} = {} & \beta_0 + \beta_1\, Post\ (First\ violation)_{it\text{-}1} + \beta_2\, ln(Sales)_{it\text{-}1} + \beta_3 Leverage_{it\text{-}1} + \beta_4 Tangibility_{it\text{-}1} \\ & + \beta_5\, Cash\ holdings_{it\text{-}1} + \beta_6\, ROA_{it\text{-}1} + \beta_7\, M/B_{it\text{-}1} + \beta_8\, Sales\ growth_{it\text{-}1} \\ & + \beta_9\, Earnings\ volatility_{it\text{-}1} + \beta_{10}\, Z\text{-}Score_{it\text{-}1} + \beta_{11}\, ln(Loan\ size)_{it} \\ & + \beta_{12}\, ln(Loan\ maturity)_{it} + \beta_{13}\, Syndication_{it} + \varepsilon_{it}\,, \end{aligned} \tag{4}$$

where *ln(Loan Spread)* is the natural logarithm of loan spread. The key independent variable is the *Post (First violation)* dummy. We control for the effects of firm size, leverage, asset structure, cash holding, profitability, growth potential, earnings volatility, and default probability. We also control for the effects of loan size, loan maturity, and loan syndication. Lastly, we include dummy variables to control for loan purposes, loan types, debt rating, year effects, and two-digit SIC industry effects in the regression models.

Table 7, model 1 presents estimates of equation (4). The coefficient of *Post (First violation)* is positive and statistically significant at the 1 percent level, indicating that firms experience significant increases in the cost of new bank borrowing after a first-time covenant violation. The results are also economically significant; the value of

[17] We discard loans initiated in the year of the first-time covenant violation. In a robustness test, we use an alternative sample that limits loan observations to a five-year period surrounding the first-time covenant-violation events in both the pre- and the postevent windows. We find that the results are more significant in this sample.

0.131 for the coefficient of *Post (First violation)* dummy can be interpreted as equaling a 13 percent increase in the loan spread after a first-time violation, which in turn implies a $1 million increase in annual interest cost.[18]

[Insert Table 7]

Next, we examine whether high-quality auditors mitigate these costs by using a difference-in-differences method implemented in the following steps. First, to avoid a confounding effect due to auditor changes, we restrict the sample to firms without any auditor changes during our sample period. Second, we separate sample firms that experience the first-time covenant violations into treatment firms (firms audited by high-quality auditors) and control firms (firms audited by low-quality auditors). Third, we apply a one-to-one propensity score match method to pair each treatment firm with a control firm with similar firm characteristics. Specifically, we estimate the propensity score by running the following logit regression: the dependent variable is the auditor quality variable (i.e., *Auditor industry expertise* or *Big N*) and the independent variables are the ones used in the selection model discussed in Appendix 2. Fourth, we merge these matched pairs of firms with data from the DealScan database to obtain the corresponding loan-year observations.[19] The procedure results in two matched samples: 1,324 loan observations from matching based on *Auditor industry expertise* and 520 loan observations from matching based on *Big N.*

Using these two matched samples, we estimate a modified specification of equation (4) in which we add an auditor quality variable and its interaction variable with *Post (First violation)*. We are particularly interested in the coefficient of the interaction variable because it captures the change in the bank loan spread for treatment firms (firms audited by high-quality auditors) net of the change for matching firms (firms audited by low-quality auditors).

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[18] In the sample used to estimate equation (4), the mean loan size is $334 million, and the average loan spread is 223 basis points, implying that the first-time covenant violation triggers $1 million (0.968 = 334×0.0223×13%) in annual interest costs.

[19] Since we intend to perform a difference-in-differences estimation we require both treatment and match firms in each matched pair to have at least one loan-facility in both the pre- and postviolation periods.

Table 7, models 2 and 3 report results of the above analysis. We find that the coefficients of *Post (First violation)* remain significantly positive, indicating an increased cost of a new bank loan borrowing after first-time covenant violation. In addition, the coefficients of the interaction variables are negative and statistically significant, suggesting that although banks charge a greater loan spread after covenant violations, firms audited by high-quality auditors experience a significantly smaller increase in loan spreads after a covenant violation. A related and interesting question is whether firms audited by high-quality auditors actually bear higher interest costs following a covenant violation. By simple calculation, we find that the coefficient of *Post (First violation)* for high-quality firms is 0.062 (=0.195 − 0.133) in model 2 and −0.060 (=0.132 − 0.192) in model 3. Our tests show that both values are statistically insignificant, suggesting that banks do not charge firms audited by high-quality auditors a greater loan spread after covenant violations.

Overall, the results in Table 7 imply that (i) covenant violations are associated with significantly higher borrowing costs, and (ii) high-quality auditors mitigate this effect. These findings paint a picture of how high-quality auditors affect lenders' reactions to covenant violations and help mitigate the negative impact of violations on borrowers.

5. Conclusion

Debt, especially private debt, provides the bulk of new financing to U.S. firms. In much of this debt, creditors seek protection and recourse through the use of covenants. In addition, research shows that the violations of debt covenants are significant events: they result in significant restructuring and refinancing costs, hinder corporate investments, force changes in senior management, and also have a direct impact on shareholders (e.g., reduced shareholder payouts). The purpose of this study is to explore the effects of auditor quality on various issues concerning covenants, including the intensity and tightness of financial covenants, the probability of covenant violations, and the consequence of violations on loan spread.

Based on evidence from the prior literature on auditor quality, we argue that high-quality auditors improve debt contracting by mitigating information risk and agency problems. The presence of high-quality auditors should decrease risks faced by creditors and therefore reduce the intensity and tightness of covenants in debt contracts (the *contracting effect*). In turn, this reduces the probability of covenant violations (the *violation reduction effect*).

We first present evidence on the contracting effect. We show that high-quality auditors are negatively associated with covenant intensity, covenant tightness, and the usage of performance covenants. Consistent with this result, we provide evidence that high-quality auditors reduce the likelihood of covenant violations. We find this relation to be quite robust and invariant to alternative empirical specifications. In an ancillary test, we find that covenant violations impose significantly greater interest costs on borrowing firms; however, the presence of high-quality auditors mitigates this effect. Thus, in the main as well as in the ancillary test, we show that high-quality auditors play a significant role in debt contracting.

Our findings highlight the beneficial role of high-quality auditors in debt contracting. We therefore respond to the call in Armstrong et al. (2010) for examining how the financial reporting environment, including auditor quality, affects the efficiency of debt contracting. In addition, most studies in the covenant violation literature focus on consequences of violations and provide evidence that violating firms incur significant costs, while our focus is on the impact of auditor quality on the probability of violations. Our findings have important implications for borrowing firms that generally have incentives to obtain more favorable loan contract terms and to reduce the probability of covenant violations.

Appendix 1
Variable definitions and data sources

Auditor variables (Data source: COMPUSTAT)	
Auditor industry expertise	Following Casterella, Francis, Lewis, and Walker (2004) and Dunn and Mayhew (2004), we define auditor industry expertise as an indicator variable equal to one if the client's audit firm audits at least 20 percent of sales in the client's two-digit SIC-code industry, and zero otherwise
Big N	One if a firm is audited by a Big N auditor, and zero otherwise. Big N auditors are defined as auditors with the audit code (COMPUSTAT item AU) between 1 and 8
Post	One if a firm-year observation is after auditor changes, and zero otherwise
Change to Big N	One if a non-Big N firm switches to a Big N auditor when it changes its auditor and zero otherwise
Change to Experts	One if a non-expert firm switches to an industry expert when it changes its auditor and zero otherwise
Change to Non- Big N	One if a Big N firm switches to a non-Big N auditor when it changes its auditor and zero otherwise
Change to Non-Experts	One if a firm audited by industry expert switches to a non-expert when it changes its auditor and zero otherwise
Audit opinion	The auditor's opinion on a company's financial statements. 0 is Unaudited; 1 is Unqualified; 2 is Qualified; 3 is No opinion; 4 is Unqualified with additional language; and 5 is Adverse opinion

Debt covenant violation (Data source: Amir Sufi's website)	
Violation	One if a firm has at least one debt covenant violation in a certain year and zero otherwise
Prior violation	One if the year is after the first debt covenant violation during the sample period and zero otherwise

Bank loan (Data source: DealScan)	
Financial covenant intensity	Total number of financial covenants in a loan facility
Financial covenant tightness	We follow Demiroglu and James (2010) to construct the covenant tightness measure using the two covenants based on the debt-to-EBITDA ratio and the current ratio respectively. Specifically, a current ratio covenant for a loan is defined as tight if its threshold is above the corresponding cluster's median value; in contrast, because lower thresholds for the covenant based on debt-to-EBITDA ratio indicate stricter covenants, a debt-to-EBITDA covenant is tight if its threshold is below the corresponding cluster's median value. *Financial covenant tightness* equals one for a loan if there is at least one tight covenant (out of the two covenants) and zero otherwise
P-covenants	The number of performance covenants. According to Christensen and Nikolaev (2012), performance covenants are based on (1) cash interest coverage ratio, (2) debt service coverage ratio, (3) level of EBITDA, (4) fixed charge coverage ratio, (5) interest coverage ratio, (6) ratio of debt to EBITDA, and (7) ratio of senior debt to EBITDA.
C-covenants	The number of capital covenants. According to Christensen and Nikolaev (2012), capital covenants are based on (1) quick ratio, (2) current ratio, (3) debt-to-equity ratio, (4) loan-to-value ratio, (5) ratio of debt to tangible net worth, (6) leverage ratio, (7) senior leverage ratio, and (8) net worth requirement
P/(P+C) ratio	The ratio of P-covenants to the sum of P-covenants and C-covenants
Spread	The loan spread is defined as the basis points a borrower pays in excess of the London Interbank Offered Rate (LIBOR) or LIBOR equivalent for each dollar drawn down
Loan size	Total dollar amount of a loan facility

Loan maturity	Months to maturity of a loan facility
Syndication	One if there is more than one lender in a loan facility and zero otherwise
Rating	S&P senior debt rating for bank loans as reported in DealScan
Loan type	Distribution method of a loan facility
Loan purpose	Primary purpose of a loan facility

Firm characteristics (Data source: COMPUSTAT)	
Sales	Total sales of a firm
Leverage	(Long term debt + debt in current liabilities)/total assets
ROA	EBITDA/total assets
Cash holding	Cash and short-term investments/total assets
Tangibility	Net property, plant and equipment/total assets
Capital expenditure	Capital expenditure/total assets
Earnings volatility	The standard deviation of quarterly earnings in the previous five years
Dividend	One if there is any dividend payout in a certain year and zero otherwise.
Senior debt rating	One if there is any debt rating in COMPUSTAT and zero otherwise

Firm characteristics (Data source: Thomson Reuters Ownership Database)	
Institutional ownership	The proportion of a company's shares held by institutional investors

Additional variables used in the tests	
Segments	The number of geographic segments in a firm
Herfindahl index	Computed by summing the squared market shares within each three-digit SIC industry
Z-score	Modified Altman's (1968) *Z*-score = (1.2 working capital + 1.4 retained earnings + 3.3 EBIT + 0.999 sales)/total assets. We follow Graham et al. (2008) to use this modified *Z*-score, which does not include the ratio of market value of equity to book value of total debt, because a similar term, market-to-book (M/B), enters our regressions as a separate control variable
O-score	Ohlson's (1980) O-score$_t$ = –1.32 – 0.407 (log (total assets$_t$)) + 6.03 (total liabilities$_t$ /total assets$_t$) – 1.43 (working capital$_t$ /total assets$_t$) + 0.076 (current liabilities$_t$ /current assets$_t$) – 1.72 (One if total liabilities$_t$ > total assets$_t$, zero otherwise) – 0.521 (net income$_t$ – net income$_{t-1}$)/(|net income$_t$ | + |net income$_{t-1}$ |))
Post (First violation)	One if the firm obtained the loan facility after its first-time violation and zero if the firm obtained the loan facility before the violation
M/B	Market-to-book ratio is measured as market value of equity, scaled by book value of equity
Sales growth	The percentage growth rate of sales from two years prior to the year immediately before the year of loan inception

Appendix 2
Self-selection of auditor quality and the Heckman procedure

Key tests in our study involve estimations of equations (1) and (2). Both equations have auditor quality as the main independent variable. Since firms may select auditors based on considerations that may also affect covenant structure or the probability of covenant violation, we need to address the problem of selection bias. We do so using the Heckman (1979) two-step procedure. We estimate the auditor selection equation as the first step to obtain the *Inverse Mills ratio*, which is then inserted in our test equations in the second step. Below, we display the selection model and results from the first stage. Specifically, we estimate the following regression:

$$\begin{aligned} \textit{Auditor industry expertise}_{it} = {} & \beta_0 + \beta_1 \textit{Herfindahl index}_{it} + \beta_2 \textit{Segments}_{it} + \beta_3 \textit{ln(Sales)}_{it} \\ & + \beta_4 \textit{Leverage}_{it} + \beta_5 \textit{ROA}_{it} + \beta_6 \textit{Cash holdings}_{it} \\ & + \beta_7 \textit{Tangibility}_{it} + \beta_8 \textit{Capital expenditure}_{it} \\ & + \beta_9 \textit{Earnings Volatility}_{it} + \beta_{10} \textit{Z-score}_{it} + \beta_{11} \textit{Dividend}_{it} \\ & + \beta_{12} \textit{Senior debt rating}_{it} + \beta_{13} \textit{Institutional ownership}_{it} \\ & + \beta_{14} \textit{Prior violation}_{it} + \textit{Industry dummies} \\ & + \textit{Year dummies} + \textit{Audit opinion dummies} + \varepsilon_{i,t}; \qquad \text{(A.1)} \end{aligned}$$

and an analogous specification with *Big N* as the dependent variable in place of *Auditor industry expertise*. Please note that, although most of the independent variables are firm characteristics included in the second-step regressions, two of them (i.e., instrumental variables) are not included in the second-step regressions. These instrumental variables, *Segments* (the number of geographic segments) and *Herfindahl index* (the sum of the squared market shares within each three-digit SIC industry), capture the scope and complexity of auditing and should therefore be correlated with auditor quality variables. However, they can be excluded from the second-step regressions because prior studies do not provide evidence that these variables directly affect the likelihood of covenant violations. We examine this condition and find that it is met. Specifically, in an untabulated test, we include the instruments as additional control variables in the test model and find that they are uncorrelated with the dependent variable.

Table A.1 reports the results of the selection model. The results indicate that both instrumental variables are significantly and positively related to auditor quality variables. The results also indicate that firms with larger size, lower leverage, and lower *Z*-score are more likely to choose high-quality auditors. These results are largely consistent with those reported in previous studies (e.g., Chaney, Jeter, and Shivakumar 2004; Chang, Dasgupta, and

Hilary 2009; Lennox, Francis, and Wang 2012). We note that most of the control variables are more statistically significant in the regression of *Big N*, which may potentially explain why the regression of *Big N* has a higher R^2 value. We then compute the *Inverse Mills ratio* and include it in equations (1) and (2).[20]

Please note that we modify our selection model in the auditor-change tests (see Table 5). Specifically, we estimate the probability that a firm will switch to a high-quality auditor by regressing *Change to Big N* or *Change to Experts* on the *changes* in the set of independent variables we used earlier in the auditor quality selection model. For brevity, we do not report this estimation, but results are available from the authors on request.

[20] Following the suggestion of Lennox et al. (2012), we conduct sensitivity analysis on the first-step model specification. In an alternative model, we include several additional independent variables (e.g., asset growth, stock return volatility, and a dummy capturing litigation risk). We find that our main findings are robust to this sensitivity test.

TABLE A.1
Heckman first-step regressions

Variables	(1) *Auditor industry expertise*	(2) *Big N*
Instrumental variables		
Herfindahl index	0.410***	0.415*
	(3.15)	(1.92)
Segments	0.012***	0.021***
	(4.14)	(5.48)
Control variables		
ln(Sales)	0.197***	0.390***
	(24.20)	(28.93)
Leverage	-0.200***	-0.341***
	(-5.60)	(-7.59)
ROA	0.021	0.068*
	(0.69)	(1.90)
Cash holding	-0.153	-0.203*
	(-1.45)	(-1.92)
Tangibility	0.126*	-0.042
	(1.83)	(-0.48)
Capital expenditure	0.468***	0.859***
	(3.12)	(5.13)
Earnings volatility	0.054***	0.280***
	(2.62)	(8.45)
Z-score	-0.022***	-0.040***
	(-3.75)	(-5.61)
Dividend	-0.012	-0.202***
	(-0.39)	(-4.70)
Senior debt rating	0.266***	0.554***
	(5.88)	(7.00)
Institutional ownership	0.163***	0.464***
	(2.93)	(6.70)
Prior violation	-0.107***	-0.243***
	(-3.95)	(-7.32)
Industry, year and auditor opinion effects	Y	Y
Observations	35,181	35,181
Pseudo R^2	0.141	0.354

Notes:
This table presents the results of the first-step regression model (A.1). The sample consists of 35,181 firm-year observations from 1996 to 2007. The dependent variables are Auditor industry expertise and *Big N* in columns 1 and 2, respectively. The independent variables include *Segments*, *Herfindahl index*, *ln(Sales)*, *Leverage*, *ROA*, *Cash holding*, *Tangibility*, *Capital expenditure*, *Earnings volatility*, Z-score, *Dividend*, *Senior debt rating*, *Institutional ownership*, and *Prior violation*. We control for year effect, two-digit SIC industry effect, and auditor opinion effect. Variable definitions are in Appendix 1. The standard errors are clustered at the firm level. Values of the robust *z*-statistics are in parentheses. Significance at the 10 percent, 5 percent, and 1 percent levels are indicated by *, **, and ***, respectively.

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TABLE 1
Summary statistics

Panel A: Summary statistics for the sample of 35,181 firm-year observations

	Full sample						*Violation*=0		*Violation*=1		
Variables	*N*	Mean	SD	P25	P50	P75	Mean	SD	Mean	SD	Difference
Violation	35,181	0.144	0.351	0.000	0.000	0.000					
Auditor industry expertise	35,181	0.396	0.489	0.000	0.000	1.000	0.406	0.491	0.349	0.477	0.057***
Big N	35,181	0.743	0.436	0.000	1.000	1.000	0.754	0.431	0.682	0.465	0.072***
ln(Sales)	35,181	4.847	2.642	3.222	5.001	6.658	4.892	2.752	4.580	1.823	0.312***
ROA	35,181	0.012	0.175	0.001	0.030	0.086	0.012	0.196	0.011	0.156	0.001
Leverage	35,181	0.358	0.529	0.098	0.256	0.431	0.352	0.545	0.397	0.421	-0.045***
Cash holding	35,181	0.074	0.112	0.000	0.054	0.114	0.076	0.110	0.064	0.117	0.012***
Tangibility	35,181	0.287	0.239	0.093	0.201	0.427	0.289	0.241	0.282	0.228	0.007***
Capital expenditure	35,181	0.061	0.081	0.018	0.037	0.072	0.063	0.082	0.051	0.073	0.012**
Earnings volatility	35,181	0.453	0.602	0.116	0.243	0.515	0.436	0.586	0.556	0.677	-0.120***
Z-score	35,181	1.156	1.626	0.876	1.200	1.876	1.378	1.892	0.933	1.423	0.445***
Dividend	35,181	0.125	0.325	0.000	0.000	0.000	0.166	0.372	0.091	0.289	0.075***
Senior debt rating	35,181	0.058	0.244	0.000	0.000	0.000	0.081	0.273	0.042	0.201	0.039***
Institutional ownership	35,181	0.495	0.278	0.291	0.497	0.712	0.506	0.281	0.435	0.241	0.071***
Prior violation	35,181	0.295	0.456	0.000	0.000	1.000	0.249	0.432	0.567	0.495	-0.318***

Panel B: Summary statistics for the sample of 9,849 loan-year observations

Variables	N	Mean	SD	P25	P50	P75
Financial covenant intensity	9,849	1.766	1.510	0.000	2.000	3.000
Financial covenant tightness	9,849	0.199	0.399	0.000	0.000	0.000
P-covenants	9,849	1.212	1.161	0.000	1.000	2.000
C-covenants	9,849	0.554	0.746	0.000	0.000	1.000
P/(P+C) ratio	6,593	0.685	0.308	0.500	0.667	1.000
Spread	9,849	200.979	151.183	75.000	175.000	275.000
Loan size	9,849	641	144	75	225	675
Loan maturity	9,849	45.345	24.678	24.000	48.000	60.000
Syndication	9,849	0.927	0.260	1.000	1.000	1.000
ln(Sales)	9,849	6.774	1.949	5.461	6.756	8.121
ROA	9,849	0.014	0.210	-0.008	0.044	0.092
Leverage	9,849	0.378	0.260	0.218	0.348	0.489
Cash holding	9,849	0.080	0.088	0.027	0.069	0.117
Tangibility	9,849	0.329	0.233	0.136	0.280	0.484
Capital expenditure	9,849	0.063	0.075	0.023	0.041	0.073
Earnings volatility	9,849	0.607	0.677	0.184	0.369	0.715
Z-score	9,849	1.195	1.777	0.603	1.238	2.090
Dividend	9,849	0.210	0.407	0.000	0.000	0.000
Senior debt rating	9,849	0.186	0.389	0.000	0.000	0.000
Institutional ownership	9,849	0.574	0.242	0.498	0.498	0.762
Prior violation	9,849	0.341	0.474	0.000	0.000	1.000

Notes:
Panel A presents summary statistics for the sample of 35,181 firm-year observations for 8,042 unique firms from 1996 to 2007, which is used to examine the relation between auditor quality and the likelihood of covenant violations. Specifically, panel A presents the summary statistics for the full sample of 35,181 firm-year observations, the 30,118 firm-year observations without violations (i.e., *Violation* = 0), and 5,063 firm-year observations with violations (i.e., *Violation* = 1). The last column of panel A reports the differences in mean values for the subsamples without and with violations. Significance in the differences at the 10 percent, 5 percent, and 1 percent levels are indicated by *, **, and ***, respectively. Panel B presents the summary statistics for the sample of 9,849 loans during 1996-2007, which is used to examine the relation between auditor quality and debt covenant characteristics. Variable definitions are in Appendix 1.

TABLE 2
Auditor quality and financial covenant intensity and tightness

Panel A: Auditor quality and financial covenant intensity

Variables	Pred. Sign	(1) *Financial covenant intensity*	(2) *Financial covenant intensity*
Auditor industry expertise	-	-0.027**	
		(-2.13)	
Big N	-		-0.054**
			(-2.43)
ln(Sales)	-	-0.145***	-0.156***
		(-9.01)	(-18.20)
Leverage	+	0.051	0.045
		(1.63)	(1.61)
ROA	-	0.082**	0.085**
		(2.25)	(2.34)
Cash holding	-	-0.061	-0.033
		(-0.75)	(-0.41)
Tangibility	-	0.017	0.037
		(0.38)	(0.86)
Capital expenditure	-	-0.193*	-0.217*
		(-1.66)	(-1.96)
Earnings volatility	+	0.053***	0.065***
		(4.99)	(6.37)
Z-score	-	-0.060***	-0.058***
		(-11.28)	(-11.52)
Dividend	+	-0.006	0.014
		(-0.34)	(0.83)
Senior debt rating	-	-0.061**	-0.106***
		(-2.13)	(-4.12)
Institutional ownership	-	-0.122***	-0.071**
		(-3.66)	(-2.26)
Prior violation	+	0.047***	0.060***
		(3.04)	(4.32)
ln(Spread)	+	0.087***	0.078***
		(8.04)	(7.20)
ln(Loan size)	+	0.085***	0.090***
		(11.97)	(12.63)
ln(Loan maturity)	+	0.030**	0.026**
		(2.25)	(1.97)
Syndication	-	-0.045*	-0.051*
		(-1.68)	(-1.90)
Inverse Mills ratio	?	-0.241**	-0.433***
		(-2.04)	(-7.78)
Industry, year, auditor opinion, loan type and loan purpose effects		Y	Y
Observations		9,849	9,849
R^2		0.202	0.204

Panel B: Auditor quality and financial covenant tightness

Variables	(1) *Financial covenant tightness*	(2) *Financial covenant tightness*
Auditor industry expertise	-0.105**	
	(-2.10)	
Big N		-0.399***
		(-4.42)
Control variables	Y	Y
Industry, year, auditor opinion, loan type and loan purpose effects	Y	Y
Observations	9,849	9,849
Pseudo R^2	0.105	0.107

Notes:
This table presents OLS regressions of covenant intensity (in panel A) and logit regressions of covenant tightness (in panel B). The regressions are based on the sample of 9,849 loans during 1996-2007. Variable definitions are in Appendix 1. We control for year effect, two-digit SIC code industry effect, auditor opinion effect, loan type effect, and loan purpose effect. All firm-level independent variables are measured with a one-year lag. Values of the robust *t*-statistics and *z*-statistics are in parentheses. For brevity, panel B only reports the results of key independent variables, the number of observations, and the R^2 values. Significance at the 10 percent, 5 percent, and 1 percent levels are indicated by *, **, and ***, respectively.

TABLE 3
Auditor quality and the use of performance and capital covenants

Variables	(1) *P-covenants*	(2) *P-covenants*	(3) *C-covenants*	(4) *C-covenants*	(5) *P/(P+C) ratio*	(6) *P/(P+C) ratio*
Auditor industry expertise	-0.030***		0.005		-0.018**	
	(-2.67)		(0.63)		(-2.47)	
Big 4		-0.067***		-0.010		-0.030**
		(-3.22)		(-0.60)		(-2.42)
ln(Sales)	-0.146***	-0.136***	-0.039***	-0.057***	-0.029***	-0.004
	(-10.40)	(-18.31)	(-3.57)	(-9.92)	(-2.97)	(-0.70)
Leverage	0.198***	0.171***	-0.140***	-0.124***	0.223***	0.193***
	(7.23)	(7.02)	(-6.60)	(-6.57)	(12.20)	(12.07)
ROA	0.099***	0.102***	0.001	0.002	0.075***	0.076***
	(3.14)	(3.24)	(0.06)	(0.10)	(3.49)	(3.50)
Cash holding	0.005	0.017	-0.133**	-0.113**	0.016	-0.001
	(0.07)	(0.25)	(-2.46)	(-2.10)	(0.37)	(-0.02)
Tangibility	-0.026	0.009	0.097***	0.091***	-0.055**	-0.032
	(-0.68)	(0.23)	(3.29)	(3.14)	(-2.18)	(-1.32)
Capital expenditure	-0.360***	-0.336***	0.148*	0.104	-0.252***	-0.191***
	(-3.58)	(-3.49)	(1.91)	(1.39)	(-3.69)	(-2.91)
Earnings volatility	0.043***	0.050***	0.035***	0.041***	0.001	0.005

	(4.62)	(5.64)	(4.86)	(6.02)	(0.14)	(0.79)
Z-score	-0.061***	-0.057***	-0.021***	-0.023***	-0.023***	-0.020***
	(-13.21)	(-12.93)	(-6.02)	(-6.77)	(-7.41)	(-6.66)
Dividend	-0.001	0.019	-0.006	-0.001	0.003	0.007
	(-0.10)	(1.32)	(-0.53)	(-0.13)	(0.28)	(0.76)
Senior debt rating	-0.080***	-0.105***	-0.032	-0.058***	-0.036**	-0.014
	(-3.23)	(-4.73)	(-1.64)	(-3.38)	(-2.22)	(-0.99)
Institutional ownership	-0.150***	-0.117***	-0.024	-0.052**	-0.051***	-0.069***
	(-5.14)	(-4.31)	(-1.06)	(-2.49)	(-2.73)	(-3.85)
Prior violation	0.050***	0.053***	0.012	0.023**	0.017*	0.006
	(3.70)	(4.37)	(1.11)	(2.40)	(1.92)	(0.76)
ln(Spread)	0.064***	0.054***	0.046***	0.044***	-0.030***	-0.030***
	(6.78)	(5.80)	(6.34)	(6.04)	(-4.59)	(-4.64)
ln(Loan size)	0.090***	0.095***	0.010**	0.011**	0.024***	0.025***
	(14.54)	(15.37)	(2.19)	(2.36)	(5.30)	(5.53)
ln(Loan maturity)	0.059***	0.055***	-0.027***	-0.027***	0.055***	0.054***
	(5.05)	(4.73)	(-2.97)	(-3.06)	(7.08)	(6.98)
Syndication	0.071***	0.069***	-0.133***	-0.138***	0.122***	0.128***
	(3.04)	(2.98)	(-7.37)	(-7.68)	(8.34)	(8.72)
Inverse Mills ratio	-0.430***	-0.466***	0.095	-0.078**	-0.277***	-0.102***
	(-4.20)	(-9.63)	(1.20)	(-2.09)	(-3.99)	(-2.89)
Industry, year, auditor opinion, loan type and loan purpose effects	Y	Y	Y	Y	Y	Y
Observations	9,849	9,849	9,849	9,849	6,593	6,593
R^2	0.207	0.212	0.199	0.199	0.262	0.261

Notes:
This table presents OLS regressions of the use of performance and capital covenants. The regressions are based on the sample of 9,849 loans during 1996-2007. The dependent variables include the number of performance covenants (*P-covenants*), the number of capital covenants (*C-covenants*), and *P/(P+C) ratio* (the ratio of *P-covenants* to the sum of *P-covenants* and *C-covenants*). Variable definitions are in Appendix 1. We control for year effect, two-digit SIC code industry effect, auditor opinion effect, loan type effect, and loan purpose effect. All firm-level independent variables are measured with a one-year lag. Values of the robust *t*-statistics and *z*-statistics are in parentheses. For brevity, panel B only reports the results of the key independent variables, the number of observations, and the R^2 values. Significance at the 10 percent, 5 percent, and 1 percent levels are indicated by *, **, and ***, respectively.

TABLE 4
Auditor quality and the probability of covenant violations

Variables	Pred. Sign	(1) *Violation*	(2) *Violation*
Auditor industry expertise	-	-0.143***	
		(-3.52)	
Big N	-		-0.373***
			(-7.55)
ln(Sales)	-	-0.539***	-0.320***
		(-11.14)	(-13.36)
Leverage	+	0.777***	0.684***
		(11.71)	(13.17)
ROA	-	0.154***	0.094*
		(3.29)	(1.88)
Cash holding	-	0.033	-0.115
		(0.12)	(-0.45)
Tangibility	-	0.148	0.473***
		(1.40)	(4.62)
Capital expenditure	-	-4.037***	-3.532***
		(-9.27)	(-8.38)
Earnings volatility	+	0.163***	0.122***
		(5.50)	(3.89)
Z-score	-	-0.068***	-0.041***
		(-6.61)	(-4.51)
Dividend	-	-0.540***	-0.355***
		(-8.37)	(-5.40)
Senior debt rating	-	-0.839***	-0.733***
		(-8.05)	(-7.56)
Institutional ownership	-	-1.397***	-1.273***
		(-15.85)	(-15.19)
Prior violation	+	1.507***	1.375***
		(35.26)	(36.72)
Inverse Mills ratio	?	-3.699***	-2.013***
		(-11.40)	(-15.88)
Industry, year and auditor opinion effects		Y	Y
Observations		35,181	35,181
Pseudo R^2		0.108	0.117

Notes:
This table presents logit regressions of the probability of debt covenant violations. The regressions are based on the sample of 35,181 firm-year observations from 1996 to 2007. Variable definitions are in Appendix 1. We control for year effect, two-digit SIC code industry effect, and auditor opinion effect. All independent variables are measured with a one-year lag. The standard errors are clustered at the firm level. Values of the robust z-statistics are in parentheses. Significance at the 10 percent, 5 percent, and 1 percent levels are indicated by *, **, and ***, respectively.

TABLE 5
Addressing the endogeneity issue

Panel A: Auditor quality changes and subsequent covenant violations

Variables	(1) *Violation*	(2) *Violation*
Post	-0.562***	-0.535***
	(-7.00)	(-6.58)
Post×Change to Big N	-0.367**	
	(-2.27)	
Post×Change to Non-Big N)	0.251**	
	(2.25)	
Post×Change to Experts		-0.219*
		(-1.69)
Post×Change to Non-Experts		0.198*
		(1.81)
ln(Sales)	0.001	-0.257***
	(0.06)	(-5.25)
Leverage	0.095	0.364***
	(1.58)	(4.50)
ROA	0.132	0.123
	(1.52)	(1.41)
Cash holding	-0.211	-0.011
	(-0.54)	(-0.03)
Tangibility	0.715***	0.489**
	(3.07)	(2.06)
Capital expenditure	-3.227***	-3.607***
	(-3.49)	(-3.99)
Earnings volatility	0.239***	0.175***
	(4.82)	(3.34)
Z score	-0.006	0.018
	(-0.40)	(1.06)
Dividend	-0.608***	-0.543***
	(-5.57)	(-4.98)
Senior debt rating	-0.089	-0.286*
	(-0.58)	(-1.91)
Institutional ownership	-0.583***	-0.923***
	(-3.61)	(-5.86)
Prior violation	1.044***	1.249***
	(14.30)	(17.13)
Inverse Mills ratio	-0.605***	-1.651***
	(-4.57)	(-5.54)
Industry, year and auditor opinion effects	Y	Y
Observations	11,454	11,454
Pseudo R^2	0.106	0.108

Panel B: Propensity-score matching analysis

Variables	(1) *Violation*	(2) *Violation*
Auditor industry expertise	-0.118***	
	(-3.17)	
Big N		-0.172***
		(-3.88)
Control variables	Y	Y
Industry, year and auditor opinion effects	Y	Y
Observations	21,466	11,200
Pseudo R^2	0.109	0.123

Notes:

Panel A presents the estimates of equation (3). The sample used in the panel A regressions consists of 11,454 firm-year observations from 2,269 firms experiencing a quality-increasing, quality-decreasing, or lateral auditor change during the sample period. Panel B presents the estimates of equation (2) using propensity-score matched samples. The sample used in Column 1 in panel B consists of 21,466 firm-year observations (10,733 matched pairs audited by Big N and non-Big N auditors), while the sample used in Column 2 consists of 11,200 firm-year observations (5,600 matched pairs audited by industry expert and non-expert auditors). For brevity, panel B reports only the coefficients of key independent variables, the number of observations, and R^2 values. Variable definitions are in Appendix 1. We control for year effect, two-digit SIC code industry effect, and auditor opinion effect. All control variables are measured with a one-year lag. The standard errors are clustered at the firm level. Values of the robust *z*-statistics are in parentheses. Significance at the 10 percent, 5 percent, and 1 percent levels are indicated by *, **, and ***, respectively.

TABLE 6
Default risk subsample analysis
Panel A: High default risk (<median Z-score)

Variables	(1) *Violation*	(2) *Violation*
Auditor industry expertise	-0.236***	
	(-4.15)	
Big N		-0.546***
		(-8.35)
High default risk	0.302***	0.129*
	(5.33)	(1.67)
Auditor industry expertise×*High default risk*	0.149*	
	(1.96)	
Big N×*High default risk*		0.292***
		(3.52)
Control variables	Y	Y
Observations	35,181	35,181
Pseudo R^2	0.109	0.119

Panel B: High default risk (>median O-score)

Variables	(1) *Violation*	(2) *Violation*
Auditor industry expertise	-0.192***	
	(-3.09)	
Big N		-0.571***
		(-7.42)
High default risk	0.692***	0.376***
	(13.26)	(4.73)
Auditor industry expertise×High default risk	0.128*	
	(1.72)	
Big N×High default risk		0.418***
		(4.74)
Control variables	Y	Y
Observations	35,181	35,181
Pseudo R^2	0.121	0.130

Notes:
This table presents the results of the default risk subsample analysis. The analysis is based on 35,181 firm-year observations from 1996 to 2007. We define firms with high default risk as those with below-median Z-score in panel A (or above-median O-score in panel B). We use a dummy variable, *High default risk*, to indicate the firms with high default risk. We modify our baseline regression model (i.e., equation (2)) by adding this *High default risk* dummy and its interaction term with auditor quality variables. For brevity, this table only reports the coefficients of key independent variables. The standard errors are clustered at the firm level. Values of robust *z*-statistics are in parentheses. Significance at the 10 percent, 5 percent, and 1 percent levels are indicated by *, **, and ***, respectively.

TABLE 7
Bank loan spread after first-time covenant violations

Variables	(1) *ln*(*Spread*)	(2) *ln*(*Spread*)	(3) *ln*(*Spread*)
Post (First violation)	0.131***	0.195***	0.132*
	(5.11)	(3.73)	(1.92)
Auditor industry expertise		0.009	
		(0.20)	
Post (First violation)×Auditor industry expertise		-0.133*	
		(-1.90)	
Big N			0.039
			(0.46)
Post (First violation)×Big N			-0.192*
			(-1.89)
Log(Sales)	-0.108***	-0.179***	-0.138**
	(-6.45)	(-6.51)	(-2.12)
Leverage	0.541***	0.566***	0.401***

	(7.95)	(4.90)	(2.83)
Tangibility	-0.022	-0.026	0.003
	(-0.97)	(-1.12)	(0.08)
Cash holding	0.223**	0.037	0.262
	(2.32)	(0.21)	(1.13)
ROA	-0.238***	-0.180	0.090
	(-3.52)	(-1.32)	(0.78)
M/B	-0.006*	-0.016***	-0.007
	(-1.65)	(-2.59)	(-0.79)
Sales growth	0.008	0.034***	0.009
	(1.19)	(2.89)	(0.73)
Earnings volatility	0.017***	0.040***	0.224*
	(3.16)	(3.15)	(1.73)
Z-Score	-0.068***	-0.060***	-0.058**
	(-7.58)	(-3.25)	(-2.35)
ln(Loan size)	-0.036**	0.002	-0.064
	(-2.12)	(0.06)	(-1.62)
ln(Loan maturity)	-0.064***	-0.105***	-0.002
	(-3.36)	(-2.95)	(-0.05)
Syndication	-0.072**	0.013	-0.001
	(-2.39)	(0.19)	(-0.01)
Industry, year, loan purpose, loan type and debt rating effects	Y	Y	Y
Observations	5,728	1,324	520
R^2	0.560	0.670	0.559

Notes:
Model 1 presents the OLS regression results for equation (4). The sample used in model 1 contains 1,728 firms with 5,728 loan observations, 2,827 (2,901) of which are initiated before (after) the firm's first-time covenant violation. model 2 and model 3 present the difference-in-differences analysis results. We first separate sample firms that experience first-time covenant violations into treatment firms (firms audited by high-quality auditors) and control firms (firms audited by low-quality auditors), then apply the one-to-one propensity score matching method to pair each treatment firm with a control firm with similar firm characteristics. Specifically, for the matching method, we estimate a logit regression, where the dependent variable is auditor quality (i.e., *Auditor industry expertise* or *Big N*) and the independent variables are the same as those used in the first-stage regression in Appendix 2. The matching procedure results in two matched samples: 1,324 loan observations from matching based on *Auditor industry expertise* and 520 loan observations from matching based on *Big N*. Using these two matched samples, we estimate a modified version of regression equation (4), in which we add the corresponding auditor quality variable and its interaction variable with *Post (First violation)*. The standard errors are clustered at the firm level. Values of the robust *t*-statistics are in parentheses. Significance at the 10 percent, 5 percent, and 1 percent levels are indicated by *, **, and ***, respectively.