Does contracting efficiency strengthen or weaken information efficiency?

The spill-over effect of debt covenant tightness on equity mispricing

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Does Contracting Efficiency Strengthen or Weaken Information Efficiency? The Spill-over Effect of Debt Covenant Tightness on Equity Mispricing

Abstract: We examine the spill-over effect of debt covenant tightness on equity mispricing. Although covenants aim to improve debt contracting efficiency, they could induce two opposing effects on borrowers' equity market information efficiency. While tight covenants aggravate lenders' dissemination of borrowers' private information to the equity market, they also provide incentives for borrowers to avoid covenant violations through earnings manipulation. Consistent with a trade-off between debt contracting efficiency and equity market information efficiency, we provide evidence that borrowers experience a larger increase in their post-earnings-announcement drift after issuance of loans with tighter covenants. This effect is more pronounced when borrowers have greater incentives to avoid covenant violations or if they increase their earnings management after loan issuance. Our study implies that tight covenants can generate negative unintended consequences that trade off the contracting and valuation roles of financial statement information between debt and equity markets.

Keywords: Debt covenant; syndicated loan; equity valuation; market information efficiency

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Introduction

We evaluate whether debt contracting efficiency exerts a spill-over effect that influences equity market information efficiency by examining whether cross-sectional variations in the probability of debt covenant violation (henceforth PDCV) affect changes in post-earnings-announcement drift (henceforth PEAD) of the corporate borrowers' share prices around the issuance of syndicated loan contracts. Debt and equity are both important sources of external capital for firms (DeAngelo & Roll, 2015; Graham, Leary, & Roberts, 2015). The focus of academic literature in the former case is on contracting efficiency since the rights of lenders and monitoring of borrowers are mainly based on contracts (Armstrong, Guay, & Weber, 2010; H. B. Christensen, Nikolaev, & Wittenberg-Moerman, 2016; Lambert, 2001); in the latter case, the focus is on information efficiency since the separation of ownership and control affects investors' ability to forecast future firm performance (Brav & Heaton, 2002; Richardson, Tuna, & Wysocki, 2010). However, the extent to which debt market contracting efficiency either contributes to or trades off with equity market information efficiency as an externality is less examined, and existing studies of cross-market effects (Bushman, Smith, & Wittenberg-Moerman, 2010; Ivashina & Sun, 2011) provide only indirect and inconclusive inferences regarding this interesting issue. Unlike previous studies, we address this issue more directly by intersecting two strands of research: (i) the impacts of debt covenant tightness in syndicated loan contracts (Demiroglu & James, 2010; McNichols, 2002), and (ii) the determinants of investors' misevaluation of earnings, which drives PEAD in equity prices (Campbell, Ramadorai, & Schwartz, 2009; Chordia & Shivakumar, 2005).

Debt covenants prevent borrowers from engaging in activities that expropriate wealth from lenders (e.g., debt overhang, claim dilution, risk shifting, underinvestment, and asset substitution) (Jensen & Meckling, 1976; Myers, 1977; Smith & Warner, 1979). Moreover, they transfer bargaining power and control rights to lenders when the borrower's credit quality deteriorates (Aghion & Bolton, 1992; Dewatripont & Tirole, 1994; Hart & Moore, 1988, 1998). Therefore, tight covenants are included in debt contracts as an important protection mechanism, aiming to improve lender welfare and contracting efficiency.

Covenant tightness may exert two offsetting effects on the wider corporate information environment. On the one hand, tight covenants could enrich corporate borrowers' overall information environment. Covenant compliance is monitored through a borrower's disclosure of private financial information to lenders and the extent of disclosure is expected to increase with covenant tightness (LSTA, 2007; Standard & Poor's, 2007). Prior literature documents evidence of lenders disseminating this private information to the equity market (Bushman et al., 2010; Ivashina & Sun, 2011; Massa & Rehman, 2008; Massoud, Nandy, Saunders, & Song, 2011). As such, tight covenants that aim to improve contracting efficiency also strengthen information efficiency and reduce security mispricing in the equity market. In other words, tight covenants not only contribute to protecting lender rights but also benefit equity investors by improving their ability to make security valuation decisions. On the other hand, tight covenants may also exacerbate corporate borrowers' overall information environment when managers engage in earnings management to avoid costly covenant breaches (Dichev & Skinner, 2002; Franz, HassabElnaby, & Lobo, 2014). As such, debt contracting efficiency that arises from covenant tightness is expected to weaken information efficiency and escalate security mispricing in the equity market. In this case, although tight covenants help protect lender rights, they also generate a negative unintended consequence for equity investors by impairing their ability to formulate asset pricing decisions. While existing studies evaluate the

different channels of influences in isolation, they do not assess which of the two competing effects dominate overall. Since there is no a priori prediction, the net impact of debt covenant tightness on equity mispricing is ultimately an empirical question that deserves to be examined, and has implications for firms that rely on both debt and equity financing since they are expected to cater to protecting both investor groups simultaneously.

The identification strategy associated with our research design is as follows. To capture debt covenant tightness, we adopt the PDCV measure developed by Demerjian and Owens (2016), which captures the aggregated probability of covenant violation. Next, to observe equity mispricing, we apply the well-established phenomenon of PEAD in equity prices (Bernard & Thomas, 1989; Richardson et al., 2010), which reflects investors' underreactions or delays in price correction to earnings announcements and is described by Fama (1998) as the "grand-daddy" of all anomalies. Finally, to assess the impact of covenant tightness on equity mispricing, we assess whether the PDCV of syndicated loan contracts either moderates or exacerbates the PEAD effect of borrowers' equity prices by comparing the four quarters immediately before and after loan issuance. Existing studies provide empirical evidence of an inverse relationship between changes in the corporate information environment and PEAD (Hung, Li, & Wang, 2015; Lee, Strong, & Zhu, 2014), and we draw upon this intuition to interpret our findings of the PDCV effect on changes in PEAD around loan issuance. As such, if debt contracting efficiency captured through covenant tightness strengthens (weakens) equity market information efficiency, then we should observe a negative (positive) relationship between PDCV and PEAD.

We acquire the following empirical findings based on a sample of 48,102 firm-quarter observations constructed from 8,348 syndicated loan packages for NYSE, AMEX, or NASDAQ firms from 1993 to 2015. First, we show a significant increase in the PEAD effect in share prices of syndicated loan borrowers in the four quarters following issuance of loans

with higher PDCV, compared to the four quarters before. In other words, we acquire empirical evidence that equity investors' mispricing of earnings information reported by corporate borrowers is exacerbated following the issuance of loans with tighter covenants. This is consistent with the weakening effect dominating the strengthening effect of debt contracting efficiency on equity market information efficiency. Second, we show that the significantly positive relationship between PDCV and PEAD exists mainly (i) in performance-based rather than capital-based covenants, (ii) for borrowers with weaker bargaining power over their lenders, and (iii) among borrowers that increase rather than decrease their earnings management immediately after loan issuance. As such, we obtain empirical evidence that the increase in equity mispricing caused by tighter covenants occurs mainly when borrowers have greater incentives to avoid covenant breaches or apply earnings management to achieve this goal. The consistency across these three sets of conditioning analyses further substantiates our inference that debt covenant tightness exerts a spill-over effect that exacerbates equity mispricing because it incentivizes borrowers to avoid violations through financial reporting manipulations. Our empirical analyses control for various factors identified by the literature that could affect PEAD, and the main findings are robust to controls of actual covenant violations, other loan contract terms, loan purposes, other omitted correlated variables, shorter test periods around loan issuance, and alternative measures of standardized unexpected earnings (SUE).

The implications and contributions of our study are as follows. First, existing literature suggests financial statement information serves two fundamental purposes in capital markets: valuation and contracting (Beyer, Cohen, Lys, & Walther, 2010; Bushman, Engel, & Smith, 2006). By focusing on debt covenants based on accounting numbers and equity investors' mispricing of earnings information, we provide evidence on whether the two roles are complementary or contradictory across the debt and equity markets. Our findings suggest

contracting features that seek to enhance lender protection may induce unintended consequences through firms' financial reporting choices that disadvantage equity investors' valuation objectives. Second, a widely accepted intuition in the existing literature is that debt covenants contribute to mitigating lender-shareholder conflicts (Jensen & Meckling, 1976; Myers, 1977; Smith & Warner, 1979). However, our study demonstrates a reversal effect, in which promoting lender protection through tight covenants leads to negative externalities for shareholders by increasing information asymmetry as a result of managerial efforts to avoid covenant breaches. In other words, we highlight how use of tight covenants to mitigate lender-shareholder conflicts could inflict a hidden information cost on shareholders. At the same time, the consensus in the existing literature is that transfer of wealth away from shareholders and towards lenders mainly occurs after covenants are breached (Chava & Roberts, 2008; H. B. Christensen, Lee, & Walker, 2009; Dichev & Skinner, 2002; Nini, Smith, & Sufi, 2009). Nevertheless, to the extent that security mispricing and information inefficiency indirectly affect shareholders' wealth, our findings suggest tight covenants create a loss for shareholders even before the actual covenant violation.

The remainder of the paper is organized as follows. The next section reviews the literature and develops our testable hypotheses. The section "Research Design" describes the research design and sample, and the section "Empirical Findings" presents the empirical findings. The final section concludes.

Literature Review and Hypothesis Development

The Impact of Debt Covenants on Information Environments

Prior literature points out two offsetting effects of debt covenants on the information environment of corporate borrowers. On the one hand, debt covenants can lead to improvement in the information environment. Specifically, lenders may exploit the private information they

acquire from borrowers during the origination and maintenance processes of debt contracts to trade in the equity market, and therefore disseminate this private information to equity investors. Tight covenants facilitate extraction of such private information. For instance, Bushman et al. (2010) document that price discovery in the secondary loan market is faster for loans subject to financial covenants, particularly earnings-based covenants. In addition, they find that firms with early dissemination of confidential information to syndicated loan lenders also exhibit earlier price discovery in the equity market when institutional investors belong to the syndicate group. This evidence is consistent with insider trading by institutional lenders being sufficiently pervasive to significantly affect the speed of price discovery in the equity market. Similarly, Ivashina and Sun (2011), Massa and Rehman (2008), Massoud et al. (2011), and Park and Wu (2009) also provide evidence on the flow of private information from the debt market to the equity market. Moreover, T. Chen and Martin (2011) and Ergungor, Madureira, Nayar, and Singh (2015) show that private information acquired by lenders is also disseminated to the equity research divisions within financial conglomerates. In particular, they find that bank-affiliated analysts improve their forecast accuracy and outperform their unaffiliated peers after the followed firm borrows from the affiliated bank, and forecast accuracy superiority is more pronounced for loans with financial covenants.

On the other hand, debt covenants can also lead to deterioration in the information environment. Covenant violations are meant to be costly for borrowers, with direct consequences of renegotiation costs (e.g., use of lawyers, auditors, and accountants) and stricter contract terms (e.g., increased collateral requirements, more restrictive covenants, and increased interest rates) (Beneish & Press, 1993; Smith, 1993; Sweeney, 1994; Taylor, 2013), as well as indirect consequences associated with changes in firm investment strategies, corporate governance, and accessibility to capital markets (Bhaskar, Krishnan, & Yu, 2017; Chava & Roberts, 2008; Gao, Khan, & Tan, 2017; Nini, Smith, & Sufi, 2012; Roberts & Sufi,

2009). In accordance with positive accounting theory (Watts & Zimmerman, 1986, 1990), the debt covenant hypothesis stipulates that managers have incentives to make financial reporting decisions that help them avoid violating accounting-based covenants, and those incentives increase with the cost and likelihood of violations (Dichev & Skinner, 2002; Holthausen & Leftwich, 1983; Smith & Warner, 1979). In other words, to influence contractual outcomes that depend on accounting numbers, managers have incentives to engage in earnings management, which the literature largely asserts impairs the ability of financial statement information to reflect underlying economic performance (Dechow, Ge, & Schrand, 2010; Healy & Wahlen, 1999; Leuz, Nanda, & Wysocki, 2003; Schipper, 1991). Empirical studies provide supporting evidence that covenant violation avoidance provokes various forms of earnings management behavior. For instance, Sweeney (1994) provides evidence that firms approaching covenant violation are more likely to make income increasing accounting choices in the period leading to the default. DeFond and Jiambalvo (1994) examine a sample of firms associated with covenant violations and observe accruals adjustments in the year preceding the violation. Dichev and Skinner (2002) show that an unusually large number of firms report financial performance that just meets or beats covenant thresholds, consistent with manipulation of accounting numbers to avoid covenant breaches. Roychowdhury (2006) confirms that managers, especially managers of firms associated with outstanding debt, manipulate real activities to avoid reporting losses. Stanley and Sharma (2011) find the likelihood of financial statement misreporting is positively related to bank borrowing. Franz et al. (2014) document a positive relationship between the likelihood of covenant violations and both accruals and real activities earnings management. These empirical findings broadly concur with the survey evidence of Graham, Harvey, and Rajgopal (2005), which reveals that managers are indeed willing to make accounting choices to avoid debt covenant violations, and

there are cross-sectional variations in such incentives depending on the likelihood and cost of the violations.

While existing studies assess the different channels of intended and unintended information consequences associated with debt covenants in isolation, they do not examine the net overall impact of these various channels on corporate borrowers' information environments. In other words, it is not clear whether debt covenants' strengthening or weakening effects on the corporate information environment ultimately prevail. Since equity investors make security valuation decisions by incorporating all sources of price sensitive information, PEAD provides a suitable platform for evaluating the net information consequence of debt covenants.

Impact of the Information Environment on PEAD

Information efficiency in capital markets is a key concern for investors, managers, and regulators because it affects the financial resource allocation that in turn drives the real economy (Kothari, 2001). The efficient market hypothesis (EMH) stipulates that security value should quickly and fully reflect all available price sensitive information (Fama, 1970, 1991). Financial statements provide a key source of such information, catering to investors' demand for information for both valuation and monitoring purposes (Beyer et al., 2010; Bushman et al., 2006). Nevertheless, the existing literature has long and consistently identified a significant delay in share price responses following earnings announcements (Ball & Brown, 1968; Bernard & Thomas, 1989, 1990; Bhushan, 1994; Chordia & Shivakumar, 2005; Dehaan, Madsen, & Piotroski, 2017; Ke & Ramalingegowda, 2005; Narayanamoorthy, 2006; Rendleman, Jones, & Latané, 1982). This phenomenon, which is known as PEAD or earnings momentum, is a blatant contradiction of the EMH prediction since reported earnings are prominent and publicly available information about firm performance; as such, Fama (1998)

suggests that it is the "granddaddy" of all stock return anomalies. The widespread explanation promulgated in the literature regarding the PEAD effect is summarized as follows:

> Investors attempt to forecast a firm's earnings using innovations to current reported earnings, but they underestimate the implications that current earnings have for future earnings. This underreaction generates anomalous returns because prices do not fully reflect all the information contained in current earnings changes. Richardson et al. (2010, pp. 427).

Empirical studies consistently reveal that information environment limitations contribute to the PEAD effect. For instance, Bartov, Radhakrishnan, and Krinsky (2000) provide evidence that PEAD is inversely related to institutional ownership. Ke and Ramalingegowda (2005) show that institutional investors exploit the PEAD and their trading activities accelerate price discovery in the market. Mikhail, Walther, and Willis (2003) document that firms followed by more experienced analysts exhibit less drift. Kimbrough (2005) shows reduced PEAD among firms that supplement earnings announcements with additional disclosures, consistent with less earnings mispricing under greater information transparency. Battalio and Mendenhall (2005) provide evidence that investors who execute small trades appear to react to less sophisticated signals, which do not fully reflect the implication of current earnings changes for future earnings. Shivakumar (2006) also observes that underreaction to earnings surprises is more common among smaller than larger traders. Garfinkel and Sokobin (2006) confirm that PEAD is positively related to the component of volume that is unexplained by prior trading activity, and suggest investors' opinion divergence contributes to their underreactions. Y. Zhang (2008) shows that PEAD is less pronounced when analysts promptly issue forecast revisions following earnings announcements, and interprets this as evidence that analyst responsiveness improves market information efficiency. Lee et al. (2014) document a significant reduction in PEAD following enactment of a series of disclosure regulations in the U.S. intended to strengthen the corporate information environment. Hung et al. (2015) examine an international sample and observe a decline in PEAD following the

mandatory adoption of International Financial Reporting Standards (IFRS), which they argue leads to improvement in financial reporting quality.

Since the inverse relationship between PEAD and market information efficiency is well-established in the literature, it is possible to empirically observe variations in the overall corporate information environment through changes in the PEAD. In other words, analysis of PEAD provides a suitable platform for evaluating the net effect of debt covenants on borrowers' information environments, since equity investors incorporate all sources of price sensitive information. In fact, despite the importance of debt markets for corporate financing and the prevalence of firms that acquire external capital through both debt and equity markets, existing studies on the impact of the information environment on PEAD have not yet considered the cross-market spill-over effect of debt contracting.

Development of Hypotheses

We intersect the two aforementioned sets of literature and formulate our testable hypotheses. Studies on the impact of debt covenants on borrowers' information environments propose two opposing effects. On the one hand, lenders are expected to use the private information they acquire through debt contracting to trade in the equity market and therefore enrich the borrowers' information environment (Bushman et al., 2010; Ivashina & Sun, 2011; Massa & Rehman, 2008; Massoud et al., 2011). To the extent that tight covenants facilitate lenders' acquisition of private information (Allen, Guo, & Weintrop, 2008; LSTA, 2007; Standard & Poor's, 2007), borrowers' information environments are likely to improve with the tightness of debt covenants. Since the PEAD effect in share prices can be moderated by information quality (Hung et al., 2015; Lee et al., 2014), we expect to observe a larger reduction in such phenomena following issuance of debt contracts with tighter covenants. On the other hand, borrowers have incentives to avoid costly covenant violations through earnings

management, which impairs their information environment; these incentives are expected to grow with the tightness of covenants (Dichev & Skinner, 2002; Franz et al., 2014; Graham et al., 2005; Holthausen & Leftwich, 1983; Smith & Warner, 1979). If tight covenants unintentionally exacerbate corporate borrowers' information environments, they would also reduce equity investors' ability to assess the future implications of the current performance reported by such firms. Because the PEAD effect in share prices is inversely related to information quality (Kimbrough, 2005; Y. Zhang, 2008), we should observe a larger increase in this phenomenon following issuance of debt contracts with tighter covenants. However, it is not clear which of these two effects is likely to dominate and what the direction of the net impact of debt covenants on borrowers' information efficiency will be after accounting for these different sources of influence. Therefore, we treat the relationship between covenant tightness and changes in PEAD around the issuance of debt contracts as an empirical question. Our testable hypotheses are stated as follows.

H1a (H1b): There is a larger reduction (increase) in the post-earnings-announcement drift of corporate borrowers' share prices after issuance of debt contracts with tighter covenants.

Research Design

Measurement of PDCV

We adopt the Demerjian and Owens (2016) measure of the aggregate probability of covenant violation to capture a loan contract's covenant tightness. This measure has several advantages compared with alternative proxies for covenant tightness applied in the literature.[1]

[1] Alternative proxies for covenant tightness include other aggregate probability of violation measures (Freudenberg, Imbierowicz, Saunders, & Steffen, 2017; Murfin, 2012; Prilmeier, 2017; Wang, 2017), the distance

First, it specifies covenant definitions and reduces the measurement error of the well-adopted aggregate probability of violation measure developed by Murfin (2012). Dealscan provides information on the general types of covenants included in a loan but does not provide details of how those covenants are defined. As Leftwich (1983) and Li (2010) note, lenders often make adjustments to GAAP numbers when defining financial covenants to incorporate borrower-specific characteristics, rendering covenant tightness measured using GAAP numbers subject to measurement error. Demerjian and Owens (2016) address this problem by using a hand-coded subsample for which actual covenant definitions are observable to determine the best definition for each covenant category. Second, it uses the full set of Dealscan covenants and therefore can capture the overall strictness of a loan's covenants. In contrast, studies that focus on a single covenant at a time, usually a current ratio or net worth covenant, (e.g., Demiroglu & James, 2010; Dichev & Skinner, 2002; Franz et al., 2014) do not accurately capture strictness in deals that use a combination of complementary covenants. For example, a deal may include a lax current ratio covenant but a binding EBITDA (earnings before interest, taxes, depreciation, and amortization) covenant. Third, it incorporates a variety of features that determine the tightness of financial covenants in a loan package, including the number of covenants (*intensity*), distance between the actual values of financial ratios underlying covenants and the covenant thresholds (*slack*), volatility of the underlying financial ratios (*volatility*), and correlations among the underlying financial ratios (*correlation*),[2] giving rise to

between the actual ratio and covenant threshold based on a single type of covenant (Demiroglu & James, 2010; Dichev & Skinner, 2002; Franz et al., 2014), and the number of covenants (H. B. Christensen & Nikolaev, 2012; Costello & Wittenberg-Moerman, 2011; Demerjian, 2011).

[2] A loan package is deemed to have tighter covenants when it includes more covenants, when the actual values of financial ratios underlying covenants are closer to the covenant thresholds, when the underlying financial ratios are more volatile, and when the correlations among the underlying financial ratios are lower.

a more comprehensive measure than those that only focus on a single feature such as intensity (Bradley & Roberts, 2015; Costello & Wittenberg-Moerman, 2011) or slack (Dichev & Skinner, 2002; Franz et al., 2014). Last, but not least, Demerjian and Owens (2016) document that their measure better predicts actual covenant violations than alternative measures.

Specifically, the PDCV measure of Demerjian and Owens (2016) is computed as follows. To begin, for each loan package, they obtain the actual values of the financial ratios that underlie all its covenants, using the borrower's most recent quarterly data prior to the loan initiation date (R_{actual}). They then match the borrower firm with a set of Compustat firms based on year, size, and profitability. For each matched firm, they calculate the quarterly change in all financial ratios that underlie the loan's covenants ($C = R_q / R_{q-1}$). Next, they simulate the borrower's one-quarter-ahead financial ratios using the borrower's actual current ratios and the change in financial ratios of a randomly drawn firm-quarter observation from the matched firms ($R_{predict} = R_{actual} * C_{random}$). They then compare the predicted financial ratios with the loan covenant thresholds. This PDCV measure equals 1 if any of the covenants is violated and 0 otherwise. They repeat the simulation 1,000 times, randomly drawing (with replacement) a new match firm-quarter observation in each iteration. Finally, this PDCV measure is calculated as the total number of iterations where a violation is indicated divided by 1,000 ($PDCV = \sum_{i=1}^{1000} PDCV_i /1000$). Compared with the parametric approach of Murfin (2012), the nonparametric approach of Demerjian and Owens (2016) is more flexible and minimizes the loss of observations.[3]

Hypotheses Test

To test the prediction in hypothesis H1a vs. that in hypothesis H1b, we implement the following regression analysis:

[3] See Demerjian and Owens (2016) for a more detailed description of how their PDCV measure is calculated.

$$
\begin{aligned}
CAR_{iq} = {} & a_0 + a_1 SUE_{iq} + a_2 PDCV_{iq} + a_3 POST_{iq} \\
& + a_4 SUE_{iq} \times PDCV_{iq} + a_5 POST_{iq} \times SUE_{iq} + a_6 POST_{iq} \times DCV_{iq} \\
& + a_7 POST_{iq} \times SUE_{iq} \times PDCV_{iq} + \sum_{j=1}^{J} \beta_j CONTROLS_{iqj} + \varepsilon_{iq}
\end{aligned} \tag{1}
$$

where the dependent variable *CAR* is the size-adjusted stock return for firm *i* accumulated over the drift window, which is between +2 days after the earnings announcement of quarter *q* and +1 day after the earnings announcement of quarter *q*+1. The return is calculated as the difference between the daily raw returns and daily benchmark returns of the same CRSP size decile and exchange index (NYSE/AMEX or NASDAQ) the firm belongs to at the beginning of the calendar year. We incorporate delisting returns using the method described in Beaver, McNichols, and Price (2007). *SUE* is standard unexpected earnings measured as the difference between actual EPS and the consensus forecast among analysts' expectations reported in the 90-day period prior to the earnings announcement date, deflated by the stock price at the end of the quarter. This follows the well-established approach of the PEAD literature (e.g., Abarbanell & Bernard, 1992; Livnat & Mendenhall, 2006). *PDCV* is covenant tightness of Demerjian and Owens (2016), which is based on the aggregate probability of covenant violation. *POST* is an indicator variable that is equal to 1 if the firm-quarter observation belongs to the four quarters immediately after loan issuance (i.e., q+1 to q+4), and 0 if it belongs to the four quarters before loan issuance (i.e., q-5 to q-2). We exclude one quarter immediately before loan issuance (i.e., q-1) since negotiation of syndicated loan contracts may affect firm disclosure before the loan active date.[4]

In Equation (1), the coefficient of *SUE* (a_1) captures equity mispricing or the delay in share price response following earnings surprise news, and is expected to be significantly positive in the presence of the well-documented PEAD effect. The coefficient of *SUE* × *PDCV*

[4] Our main results are unaffected if we include quarter q-1.

(a_4) captures the incremental PEAD effect in equity prices before loan issuance for borrower firms subject to tighter covenants in their later loan contracts, and the coefficient of $SUE \times PDCV \times POST$ (a_7) identifies the impact of PDCV on changes in the PEAD effect of borrower firms' equity prices from before to after loan issuance. If the tightness of debt contracts strengthens (weakens) borrower firms' equity market information efficiency after loan issuance, as hypothesis H1a (H1b) predicts, then we should observe a significantly negative (positive) a_7.

Equation (1) essentially seeks to determine whether and how covenant tightness leads to abnormal changes in equity mispricing by comparing the PEAD effect in borrowers' share prices over multiple quarters around contract inception. The four pre-loan quarters are used to establish a reliable norm for the level of equity mispricing in the information environment before the presence of tight covenants. The four post-loan quarters are used to capture any immediate, prolonged, or delayed changes in equity mispricing in the information environment after the presence of tight covenants. Although the *PDCV* measure we apply technically estimates the probability of covenant violation during the first quarter after loan initiation and may change afterward, we believe it is necessary and appropriate to examine the PEAD effect over a four-quarter period around loan issuance[5], because we do not expect the potential impact of lenders' private information dissemination or borrowers' incentives to avoid covenant violations to immediately and systematically diminish or reverse after just one quarter. Furthermore, even if such impacts occur only one quarter after loan issuance, their influence on the information set available to equity investors may not be isolated to a single quarter to

[5] Demerjian and Owens (2016) also provide evidence that their PDCV measure can predict actual covenant violations within the 4-quarter period after loan initiation.

the extent that investors use past information to identify benchmarks when formulating their expectations.[6]

We include a number of control variables identified in the previous literature that are likely to affect the PEAD effect. Narayanamoorthy (2006) shows that accounting conservatism can lead to differences in PEAD patterns, and we control for this effect using the C_score of Khan and Watts (2009). Bartov et al. (2000) find that PEAD and institutional ownership are negatively correlated. Therefore, we control for the percentage of equity ownership held by institutional investors (*Instown*), measured at the closest day to fiscal-quarter end. Following Mendenhall (2002), we include dollar trading volume (*Vol*) by multiplying the closing stock price by the total number of shares from day -272 to day -21 relative to the earnings announcement day. We also control for other variables that could influence firm information environments. Firm size (*Size*) is measured as the firm's market capitalization at the end of the fiscal quarter. Analyst following (*Anf*) is the number of analysts following the firm at the end of the fiscal quarter. Stock price (*Price*) is the average stock price during the last week before the earnings announcement day. Earnings persistence (*EP*) is estimated as the first-order serial correlation of seasonally differenced earnings over the prior 20 quarters (L. Zhang, 2012). Negative earnings surprise (*Badnews)* is an indicator variable that equals 1 if the unexpected earnings are negative, and 0 otherwise. *4thqtr* is an indicator variable that equals 1 if the observation is from the fourth fiscal quarter and 0 otherwise.

Due to concerns about outliers and nonlinearities in the returns-earnings relationship, we follow prior research, and transform *SUE* and other continuous control variables into decile ranks and convert them to range between 0 and 1 (e.g., Bartov et al., 2000; Frederickson & Zolotoy, 2016; Hirshleifer, Lim, & Teoh, 2009; Livnat & Mendenhall, 2006). Specifically, for

[6] We also show in a later robustness test that our empirical results hold even when we examine shorter windows around loan initiation.

each calendar quarter, we rank *SUE* and each continuous control variable into deciles ranging from 0 to 9, and then scale each decile by 9 to obtain values between 0 and 1.[7] Therefore, the coefficient of each continuous control variable can be interpreted as the abnormal return earned by a hedge portfolio that is long in the highest *SUE* decile and short in the lowest *SUE* decile. All variables used in our analyses are defined in Table 1.

[Insert Table 1 here]

Sample Selection

We first collect a loan sample based on loan packages obtained from the Demerjian and Owens (2016) covenant tightness dataset, which includes 16,485 loans with financial covenants issued to U.S. public firms from 1994 to 2014.[8] The starting point is restricted by the availability of covenant data. As Chava and Roberts (2008) note, covenant data is scant prior to 1994. We remove 22 loans that no longer exist in the updated version of Dealscan.[9] The final loan sample includes 16,463 loans issued to 5,398 borrowers. We then collect the accounting and return data from Compustat and CRSP, respectively. We start with all firm-quarters available in Compustat from 1993 to 2015 to match with the sample period of the loan data, as four quarters of firm data, both pre- and post-loan issuance, are required. Next, we merge the Compustat accounting data with the CRSP return data and I/B/E/S analyst data. We exclude firm-quarter observations with share prices of less than $1, market capitalization of less than $5 million, and those with insufficient data for calculating the variables used in our main analyses. This leads to a sample of 385,184 firm-quarter observations involving 12,438

[7] We do not transform *PDCV* since it is already a probability figure between 0 and 1.

[8] A loan package may include multiple facilities. Our study is based on packages rather than facilities because all facilities in a loan package adopt the same set of covenants.

[9] We use the December 2016 WRDS version.

firms. Finally, we merge the loan sample with the firm sample using the Dealscan_Compustat_Link_31 Aug 2012 file of Chava and Roberts (2008). We also require firms to have at least one available observation in both the pre- and post-loan issuance periods. The final merged sample consists of 48,102 firm-quarter observations linked to 8,348 loan packages issued to 2,749 borrowers. The sample selection process is described in Table 2.

[Insert Table 2 here]

Table 3 presents the frequency of each covenant type in our sample. Among the performance covenants, debt to EBITDA covenants are the most common, with 54.31% of loans in our sample containing such a covenant. Fixed charge coverage and interest coverage covenants also appear frequently, with 33.46% and 38.78%, respectively, of loans including these covenants. Among capital covenants, leverage ratio and net worth covenants are relatively popular. A leverage ratio covenant is included in 24.37% of our sample loans and 35.29% have a net worth covenant (i.e., net worth and tangible net worth covenants). The frequency of covenant types in our sample is similar to that reported in Prilmeier (2017) and Rhodes (2016).

[Insert Table 3 here]

Empirical Findings

Summary Statistics and Correlation Analysis

Table 4 reports the summary statistics of the variables used in our main tests. The distributions of *CAR* and *SUE* are similar to those reported in prior research (e.g., Frederickson & Zolotoy, 2016; Livnat & Mendenhall, 2006). Specifically, the mean (median) of *CAR* is 0.007 (0.010) and both the mean and median of *SUE* are close to zero. On average, our sample loans have a 28.7% likelihood of any covenant violation in the quarter immediately after the

loan issuance, a 23.0% likelihood of a performance covenant violation, and an 8.2% likelihood of a capital covenant violation. These figures are consistent with the argument in H. B. Christensen and Nikolaev (2012) and Wang (2017) that performance covenants are more likely to trigger covenant violations than capital covenants. The distributions of firm size, trading volume, and stock price are skewed and widely dispersed. Specifically, *Size* has a mean, median, and standard deviation of $5,116.413 million, $1,330.351 million, and $14,418.374 million, respectively; *Vol* has a mean, median, and standard deviation of $8,246.305 million, $1,784.705 million, and $23,460.573 million, respectively; and *Price* has a mean, median, and standard deviation of 89.831, 20.284, and 4,335.340, respectively.[10]

[Insert Table 4 here]

Table 5 presents the correlation analysis. The Pearson (Spearman) correlations are reported above (below) the diagonal. The correlation between *SUE* and *CAR* is significantly positive. In general, the three PDCV measures (i.e., *PDCV*, *PDCV_PCOV*, and *PDCV_CCOV*) are associated with other variables in a way that reflects tighter covenants for firms with higher information uncertainty. For example, *PDCV* is significantly positively correlated with *Badnews* and significantly negatively correlated with *Size*, *Anf*, *Vol*, *Price*, *Instown,* and *4fthqtr*. Since the correlation coefficients among the independent variables are mostly less than 0.7, multicollinearity should not be a concern in this study (Lind, Marchal, & Wathen, 2005).

[Insert Table 5 here]

Covenant Tightness and Changes in PEAD Around Loan Issuance

Table 6 presents the regression results of the impact of PDCV on PEAD. In Column (1), we regress *CAR* on *SUE* and the control variables, and document a significantly positive

[10] The summary statistics are based on the raw variables before transforming them to the range of 0 to 1.

coefficient of *SUE* (coeff. = 0.077, t-stat. = 4.09), indicating the well-established phenomenon of PEAD in equity prices is present in our sample and cannot be fully explained by the control variables. In Column (2), we include the interaction between *PDCV* and *SUE* in the regression, and the coefficient of *SUE* × *PDCV* is significantly positive (coeff. = 0.018, t-stat. = 1.88). While this preliminarily suggests that equity mispricing is more severe under tighter covenants, it is also possible that the finding is driven by some firm characteristics that simultaneously affect both covenant tightness and the borrower information environment. To address this concern, in Column (3), our identification strategy provides a direct test of hypothesis H1a vs. hypothesis H1b by comparing the conditioning effect of PDCV on PEAD from the pre- to post-loan issuance period. The coefficient of *Post* × *SUE* × *PDCV* is positive and significant at the 5% level (coeff. = 0.045, t-stat. = 2.41), showing that the larger PEAD effect in equity prices for borrower firms subject to tighter covenants in their later loan contracts increases further after loan issuance. Economically, one can earn an incremental 3% in abnormal returns from PEAD in the post-loan issuance period compared to the pre-loan issuance period for firms with tighter covenants.[11] This result is consistent with debt covenant tightness exerting a cross-market spill-over effect to exacerbate equity investors' mispricing of earnings information reported by corporate borrowers, lending support to hypothesis H1b but contrary to H1a. In other words, while tight covenants aim to improve debt contracting efficiency, they also induce the unintended consequence of reduced equity market information efficiency. In terms of control variables, we show that the PEAD effect shrinks with higher institutional ownership, which is consistent with the findings of Bartov et al. (2000). We also observe that PEAD is moderated for negative unexpected earnings and for a firm's fourth fiscal quarter. The

[11] This incremental abnormal return is estimated by the linear combination of *Post* × *SUE* + *Post* × *SUE* × *PDCV* (coeff. = 0.030, t-stat. = 1.89, not reported in table 6).

coefficients of the interaction terms between *SUE* and other control variables are not significant at conventional levels.

[Insert Table 6 here]

Conditional Tests

To strengthen our inference, we perform three sets of conditioning analyses. First, we distinguish between covenants that are performance- versus capital-based. H. B. Christensen and Nikolaev (2012) and Wang (2017) suggest that performance-based or income statement covenants serve as trip wires that lead to covenant violations and consequently transfer control rights from shareholders to creditors, whereas capital-based or balance sheet covenants promote interest alignment between creditors and shareholders. Because performance-based covenants are more likely to trigger violations, they provide greater incentives for borrowers to avoid covenant violations through earnings management. As such, if PDCV indeed exacerbates PEAD by inducing managers' earnings management activities, then the impact should be greater under performance-based covenants.

To test this prediction, we substitute *PDCV* in Equation (1) with *PDCV_PCOV* and *PDCV_CCOV* separately. *PDCV_PCOV* is based on performance covenants and includes cash interest coverage, debt service coverage, fixed charge coverage, interest coverage, debt to EBITDA, senior debt to EBITDA, and EBITDA covenants. *PDCV_CCOV* is based on capital covenants and includes debt to equity, debt to tangible net worth, leverage ratio, senior leverage, current ratio, quick ratio, net worth, and tangible net worth covenants.[12] Both

[12] Beyond the 15 covenant types included in our study, Dealscan also includes a few other covenant types, such as capital expenditure, loan to value, long-term investment to net worth, net debt to assets, total debt to tangible net worth, the equity to assets ratio, and net worth to total assets. These covenants are omitted because they are either not accounting-based or appear very rarely (i.e., each in less than 0.05% of the loans in Dealscan).

measures follow H. B. Christensen and Nikolaev (2012) and Demerjian and Owens (2016). If the impact of PDCV on PEAD for performance covenants is more pronounced than for capital covenants, as we predict, then a_7 should be greater and more significant with *PDCV_PCOV* than with *PDCV_CCOV*.

Consistent with our prediction, the results in Table 7, Column (1), show that when PDCV is measured as *PDCV_PCOV*, the coefficient of *Post* × *SUE* × *PDCV* is significantly positive (coeff. = 0.049, t-stat. = 2.43). In stark contrast, Column (2) shows that when PDCV is measured as *PDCV_CCOV*, the coefficient of *Post* × *SUE* × *PDCV* is statistically insignificant (coeff. = 0.013, t-stat. = 0.42). This finding suggests the conditioning effect of covenant tightness on changes in PEAD from before to after loan issuance occurs only for performance covenants and not for capital covenants.

[Insert Table 7 here]

Second, the cost of covenant violation is likely to be affected by the relative bargaining power of borrower firms and lenders in subsequent renegotiations. Covenant breaches are expected to be costlier for borrowers with weaker bargaining power, and, as such, this group should have greater incentives to avoid breaches (Bird, Ertan, Karolyi, & Ruchti, 2017; K. C. W. Chen & Wei, 1993; Dichev & Skinner, 2002; HassabElnaby, 2006). Thus, if PDCV indeed exacerbates PEAD by inducing managers to engage in earnings management activities, then the impact should be greater among borrowers with weaker bargaining power.

To test this prediction, we partition the sample according to the borrower firm's bargaining power, estimate Equation (1) separately using each subsample, and compare the results for a_7 across the different subsamples using an F-test. We adopt five proxies to capture the borrower firm's bargaining power: firm size (*Size*), profitability (*ROA*), financial constraints (*Financial Constraint*), past borrowings (*New Borrower*), and the number of

lenders involved in the syndicated loan (*Lender No.*). Corporate borrowers that are smaller, less profitable, and more financially constrained have lower creditworthiness and are therefore likely to (i) incur more severe consequences if they violate the covenants (e.g., lenders accelerate the loan instead of granting a waiver) (Bird et al., 2017; K. C. W. Chen & Wei, 1993; Dichev & Skinner, 2002; HassabElnaby, 2006), and (ii) have more difficulty accessing alternative sources of external financing if they lose their current loan. Borrowers that obtain funding from the syndicated loan market for the first time are also disadvantaged in their bargaining power due to greater information asymmetry and a lack of reputational capital (Diamond, 1989). Syndicated loan borrowers that face more lenders are subject to greater pressure and cost during renegotiation (Bolton & Scharfstein, 1996; Gilson, John, & Lang, 1990). As such, we expect these five types of firms to be in less favorable bargaining positions against their lenders. Across the whole sample, we classify firms with size and profitability among the lower (higher) tercile, financial constraints and the number of lenders among the higher (lower) tercile, and without (with) a prior borrowing record in the syndicated loan market in the weaker (stronger) bargaining power subsample. If the effect of PDCV on PEAD is more prominent among borrowers with weaker than stronger bargaining positions against their lenders, as we predict, then a_7 should be more pronounced for the weaker bargaining power subsample than the stronger subsample.

Table 8 provides the test results. In four out of the five proxies for borrowers' bargaining power, we acquire evidence that supports our prediction. For instance, the analyses reveal that the coefficient of *Post* × *SUE* × *PDCV* is significantly greater among borrowers that are (i) smaller in size (Panel A, F-test p-value = 0.015), (ii) lower in profitability (Panel B, F-test p-value = 0.027), more intense in financial constraints (Panel C, F-test p-value = 0.061),

and (iv) without a previous borrowing history (Panel D, F-test p-value = 0.009).[13] We also observe that the coefficient of *Post* × *SUE* × *PDCV* is significantly positive only in the higher lender number subsample (Panel E, coeff. = 0.072, t-stat. = 2.27), although its difference with the lower lender number subsample does not reach statistical significance at conventional levels. Overall, these findings are consistent with a weaker bargaining position making borrowers more reluctant to enter contract renegotiations and therefore more motivated to avoid covenant violations through earnings manipulation. In other words, strengthening debt contracting efficiency through tight covenants is more likely to incur the unintended consequence of reducing equity market information efficiency among borrowers with weaker bargaining power.

[Insert Table 8 here]

Third, we directly distinguish between borrowers that increase or decrease their earnings management from the pre- to post-loan issuance periods. If PDCV indeed exacerbates PEAD by inducing managers to engage in earnings management activities, then the impact should be greater among borrowers that actually do increase their earnings management.

To test this prediction, we split the sample according to the borrower firm's changes in earnings management, estimate Equation (1) separately within each subsample, and compare

[13] Panels C and D of Table 8 indicate that the coefficient of SUE, or the average PEAD effect prior to loan issuance, is statistically insignificant among firms with fewer financial constraints and with previous borrowing history, respectively. In the case of Panel C, this observation is consistent with the inverse association between financial constraints and financial reporting quality (Kurt, 2017; Linck, Netter, & Shu, 2013), which potentially reduces equity mispricing among less constrained firms. In the case of Panel D, the magnitude of the coefficient of SUE is similar for firms without and with a previous borrowing history (i.e., 0.076 and 0.075, respectively), and as such, the lack of statistical significance in the latter subsample is likely caused by the greater standard errors due to the lower number of observations.

the results on a_7 using an F-test. We consider both accruals and real earnings management. To measure accruals earnings management, we adopt the modified Jones model (Dechow, Sloan, & Sweeney, 1995), while we use the Roychowdhury (2006) models to measure real earnings management. Both accruals and real earnings management measures are adjusted for the quarterly setting following Collins, Pungaliya, and Vijh (2017). We also sum the two to generate an overall earnings management measure. We classify firms in the decrease (increase) subsample if the change in their average earnings management from the pre- to post-loan issuance period is negative (positive). If the effect of PDCV on PEAD is more evident among borrowers with an increase in earnings management, as we predict, then a_7 for the increase subsample should be more pronounced than for the decrease subsample.

Table 9 reports the test results. Panel A shows that the coefficient of *Post* × *SUE* × *PDCV* is significantly greater for firms that increase their overall earnings management than for those that decrease their overall earnings management (F-test p-value = 0.070). In Panels B and C, we separately consider accruals and real earnings management. Panel B presents results similar to those in Panel A, showing that the coefficient of *Post* × *SUE* × *PDCV* is significantly greater for firms that increase their accruals earnings management than for those that decrease their accruals earnings management (F-test p-value = 0.037). Panel C also reveals a significantly positive coefficient of *Post* × *SUE* × *PDCV* only for firms that increase their real earnings management, although its difference is not statistically significant in the subsample of firms that decrease their real earnings management.[14] In general, these findings

[14] Panels A and C in Table 8 indicate the coefficient of SUE, or the average PEAD effect prior to loan issuance, is statistically insignificant among the subsample of firms that increase their overall and real earnings management, respectively. To the extent that these subsamples include firms with substantially lower earnings management prior to loan issuance, it is possible we observe less PEAD or earnings mispricing before loan issuance.

suggest that the increase in PEAD after issuance of loans with tighter covenants is more pronounced among firms that increase their earnings management. They further strengthen our identification strategy by providing a more direct substantiation of our inference that the weakening effect of PDCV on borrower firms' equity market information efficiency can be attributed to the incentive to avoid covenant violations through earnings manipulation.

[Insert Table 9 here]

Robustness Tests

Table 10 addresses the potential confounding effect associated with actual covenant violation. There is a concern that our covenant tightness measure predicts actual covenant violation and it is covenant violation that drives our results. Prior literature has documented that after covenant violations, firms reduce disclosure (T. E. Christensen, Pei, Pierce, & Tan, 2015; Vashishtha, 2014), which may increase PEAD. To address this concern, we include an additional control variable (*Viol*) in Equation (1) that indicates whether an actual covenant violation has been disclosed in the 10-K/10-Q SEC filings of a specific firm-quarter.[15] Table 10 shows the coefficient of *Post* × *SUE* × *PDCV* remains positive and significant at the 5% level after controlling for actual covenant violations. The coefficient of *SUE* × *Viol* is also positive but does not reach significance at conventional levels. These results indicate our main findings are independent of actual covenant violations, consistent with the inference that tight covenants have a negative impact on the information efficiency of borrower firms even before an actual violation occurs.

[Insert Table 10 here]

[15] We obtain the covenant violation data from Professor Amir Sufi's website, available at http://faculty.chicagobooth.edu/amir.sufi/data.html. Since this dataset only covers fiscal years 1996-2007, our sample period is shortened and the sample size is reduced.

Table 11 controls for other loan contract terms that could also exert confounding effects.[16] Apart from covenant tightness, existing studies suggest other loan contract terms can also influence a corporate borrower's information environment. For example, Khurana and Wang (2015) show that debt maturity could affect firms' accounting conservatism. Frankel, Kim, Ma, and Martin (2011) document that a collateral requirement based on borrowers' accounts receivable can induce changes in borrowers' accounting recognition policies. To address this concern, in Table 11, we add a number of controls of other loan contract terms to Equation (1), including the loan amount (*Amount*), maturity (*Maturity*), inclusion of performance pricing provisions (*PPP*), and inclusion of collateral requirements (*Secured*).[17, 18] This robustness test reveals that the coefficient of *Post* × *SUE* × *PDCV* remains statistically significant after controlling for these additional loan terms. In other words, the impact of covenant tightness on PEAD we observe is incremental to the influence of other loan contract terms. The coefficients of the interaction terms involving these additional loan terms are largely insignificant, except for *Post* × *SUE* × *Maturity*, where we document a significantly positive coefficient.

[Insert Table 11 here]

[16] These loan contract terms were not controlled in the main analyses to avoid sample size reduction due to data availability.

[17] The controls of other loan contract terms do not include interest spreads since prior studies (e.g., Bradley & Roberts, 2015; Reisel, 2014) provide evidence that covenant tightness has a significant impact on interest spreads.

[18] These additional loan terms are specified at the facility level instead of the package level. Since our study is based on packages, we must aggregate the terms of the individual facilities in the package to construct package-level terms. Specifically, *Amount* is the total amount of all facilities in the package, *Maturity* is the longest maturity across all facilities in the package, and *PPP*/*secured* equals 1 if at least one of the facilities in the package contains performance pricing provisions/collateral requirements.

Table 12 controls for loan purposes. Some firm events funded by the loans may impair the firm's information environment and, at the same time, lead to tighter covenants. Thus, there is a concern that the increase in PEAD around issuance of loans with tighter covenants is caused by such firm events rather than the tightness of covenants. To address the potential effects of these events, we control for loan purposes. Table 12 shows that the coefficient of *Post* × *SUE* × *PDCV* remains qualitatively unchanged after including loan purposes as additional control variables, confirming that our main findings are not driven by firm events funded by the loans. Among the 10 identified loan purposes (i.e., acquisition line, LBO/MBO, takeover, debt repayment, corporate purposes, working capital, commercial paper backup, debtor-in-procession, recapitalization, and others), only debtor-in-procession has a significant impact on the change in PEAD.

[Insert Table 12 here]

Table 13 presents a placebo test to alleviate the concern regarding other remaining omitted correlated variables. If some unobservable firm event causes both an increase in PEAD and tighter covenants, we should be able to observe an increase in PEAD for firms with tighter covenants before and/or after loan issuance, instead of just around loan issuance (unless the event has to take place at the same time as loan issuance, that is, is funded by the loan, but we have controlled for such event in Table 12). We further divide the pre- and post-loan issuance periods into earlier and later sub-periods, and test the changes in PEAD from the earlier to the later pre-loan issuance period (i.e., q-5 & q-4 versus q-3 & q-2) and from the earlier to the later post-loan issuance period (i.e., q+1 & q+2 versus q+3 & q+4), respectively. As reported in Table 13, we do not observe a significant increase in PEAD for firms with tighter covenants either before or after loan issuance, indicating the increase only takes place around loan

issuance, therefore mitigating the concern that our main findings are driven by some unobservable omitted correlated variables. Figure 1 provides a graphic depiction of changes in PEAD around loan issuance that juxtaposes the findings of the tests of hypothesis H1a vs hypothesis H1b in Table 6 and the placebo test in Table 13.

[Insert Table 13 here]

[Insert Figure 1 here]

Table 14 examines the impact of PDCV on PEAD across shorter time horizons after loan issuance. As previously mentioned, our main analyses compare this impact up to four quarters around loan issuance, but our measure of PDCV technically only captures the probability of covenant violation during the first quarter after loan initiation. Although we do not expect the impact of covenant tightness on equity mispricing to occur exactly and only in that quarter, it would also strengthen our inference to observe this impact more immediately after loan initiation. Indeed, we observe that the coefficient of *Post* × *SUE* × *PDCV* is significantly positive throughout the first (coeff. = 0.061, t-stat. = 1.81), second (coeff. = 0.065, t-stat. = 2.39), and third (coeff. = 0.038, t-stat. = 1.73) quarters after loan issuance.[19] The robustness tests here and our main findings jointly confirm that the increase in equity mispricing due to tight covenants occurs consistently throughout and up to four quarters after loan issuance.

[Insert Table 14 here]

[19] When estimated for the first one quarter, this coefficient is similar in magnitude but lower in significance level compared to the first two quarters; this is possibly due to the higher level of standard errors caused by the lower number of observations in this analysis.

Table 15 applies alternative measures of SUE. In the main analyses, following existing literature (e.g., Livnat & Mendenhall, 2006; L. Zhang, 2012), we calculate SUE based on analyst consensus forecasts. However, two alternative measures of SUE based on rolling seasonal random walk models (*SUE_SRW* and *SUE_SRWex*) are also frequently adopted in the literature (e.g., Battalio & Mendenhall, 2005; Bernard & Thomas, 1990; Livnat & Mendenhall, 2006). In Table 15, we show that the coefficient of *Post* × *SUE* × *PDCV* remains significantly positive even when we apply these alternative SUE measures. As such, our main results are robust to different ways of determining earnings surprise news.

[Insert Table 15 here]

Summary and Conclusion

This study examines whether and how debt covenant tightness influences equity mispricing among corporate borrowers with syndicated loans, and, in doing so, determines whether debt contracting efficiency generates a cross-market spill-over effect that influences equity market information efficiency. Although tight covenants facilitate lenders' acquisition and dissemination of private information, which enriches borrowers' information environments, they could also motivate borrowers to avoid covenant breaches through manipulation of reported performance, which deteriorates borrowers' information environments. Our empirical evidence reveals a significant increase in equity mispricing following issuance of loans with tight covenants. In other words, while tight covenants aim to promote debt contracting efficiency, they induce the unintended consequence of greater equity mispricing, which in turn implies a reduction in equity market information efficiency. Further conditioning analyses suggest our main finding is more pronounced (i) when the covenants are performance-based instead of capital-based, (ii) among borrowers with weaker rather than stronger bargaining power over lenders, and (iii) when borrowers increase rather than decrease

their earnings manipulation after loan issuance. These conditioning effects strengthen our inference that the increase in equity mispricing following tight covenants is indeed attributed to borrowers' incentives to avoid covenant breaches by managing earnings.

Our study contributes to the literature in the following ways. First, while existing studies tend to evaluate the issues of debt contracting efficiency and equity market information efficiency separately, we intersect the two literatures and document a cross-market and trade-off effect between them. Second, although existing literature often highlights that financial statement information plays two important roles in facilitating resource allocation in the capital market, that is, valuation and contracting, we provide evidence that these two roles can conflict. Third, while debt covenants are assumed to mitigate lender-shareholder conflicts, our findings imply that tight covenants may inflict hidden information costs on shareholders and induce potential and indirect wealth transfer away from them even before a covenant is breached due to escalation of equity mispricing. Fourth, although previous studies document both the intended and unintended information consequences of debt contracting, they tend to evaluate the different channels of influences in isolation, while we provide empirical evidence of their net effect on an overall basis by examining equity mispricing. Last but not least, while past literature confirms that security mispricing such as PEAD can be influenced by changes in the information environment from various sources such as corporate disclosure, analyst research, and regulations, we document the impact of a spill-over effect from the debt market.

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Table 1
Definition and Measurement of Variables

Variables	Definition and Measurement	Source
4thqtr	An indicator variable equal to 1 if the firm-quarter observation belongs to the fourth fiscal quarter of the firm, and 0 otherwise.	Compustat
Amount	Total loan amount.	Dealscan
Anf	Number of analysts following the firm at the end of the fiscal quarter.	I/B/E/S
Badnews	An indicator variable equal to 1 if the unexpected earnings are negative, and 0 otherwise.	I/B/E/S
CAR	Abnormal stock return accumulated from two days after an earnings announcement for quarter t through one day after the earnings announcement for quarter t+1. Abnormal return is the raw return minus the benchmark return of the same CRSP size decile and same CRSP exchange index (NYSE/AMEX or NASDAQ) the firm belongs to.	CRSP
C_score	A firm-specific conservatism measure developed by Khan and Watts (2009).	Compustat
EP	Earnings persistence calculated as the first-order serial correlation of the seasonally differenced earnings estimated over the past 20 quarters.	Compustat
Financial Constraint	Index of financial constraints based on Whited and Wu (2006). The coefficients are taken from the fourth column of Table 1 of Whited and Wu (2006): -0.091*CF + 0.062*DIVPOS + 0.021*TLTD – 0.044*LNTA + 0.102*ISG – 0.035*SG where CF is the ratio of cash flow (*cheq*) to total assets (*atq*); DIVPOS is an indicator that equals 1 if the firm pays cash dividends (*dvp+dvc*); TLTD is the ratio of long-term debt (*dlttq*) to total assets; LNTA is the natural log of total assets; ISG is the firm's 3-digit SIC industry code; SG is firm sales (*saleq*) growth.	Compustat
Instown	Institutional ownership, calculated as the percentage of total shares outstanding held by institutional investors, measured at the closest day relative to end of the fiscal quarter.	Thomson 13f
Lender no.	Total number of lenders in the loan syndicate, including both lead arrangers and participant lenders.	Dealscan
Maturity	Loan maturity in months.	Dealscan
New Borrower	An indicator variable equal to 1 if the borrowing firm has not had any loan in the syndicated loan market prior to loan issuance.	Dealscan
PDCV	An aggregated measure of the probability of debt covenant violation as described in Demerjian and Owens (2016), calculated at the initiation of each loan agreement.	http://faculty.washington.edu/pdemerj/data.html
PDCV_CCOV	*PDCV* calculated based on capital covenants. Capital covenants include debt to equity, debt to tangible net worth, leverage ratio, senior leverage, current ratio, quick ratio, net worth, and tangible net worth covenants.	http://faculty.washington.edu/pdemerj/data.html
PDCV_PCOV	*PDCV* calculated based on performance covenants. Performance covenants include cash interest coverage, debt service coverage, fixed charge coverage, interest coverage, debt to EBITDA, senior debt to EBITDA, and EBITDA covenants.	http://faculty.washington.edu/pdemerj/data.html

Post	An indicator variable equal to 1 if the firm-quarter observation belongs to the four quarters immediately after loan issuance, and 0 if it belongs to the four quarters before loan issuance.	Dealscan
PPP	An indicator variable equal to 1 if the loan contains a performance pricing provision, and 0 otherwise.	Dealscan
Price	Average stock price of the seven trading days before the earnings announcement day.	CRSP
ROA	Return on assets calculated as operating income before depreciation (*oibdpq*) divided by average assets (*atq*). We adjust data items reported on a cumulative basis to reflect quarterly values.	Compustat
Secured	An indicator variable equal to 1 if the loan contains collateral requirements, and 0 otherwise.	Dealscan
Size	Market capitalization (*prccq*cshoq*) at the end of the fiscal quarter.	Compustat
SUE	Standardized unexpected earnings measured as earnings minus expected earnings, deflated by the stock price at the end of the quarter. Earnings is the actual EPS. Expected earnings is the mean analyst forecast of EPS during the 90-day period before earnings disclosure. Both actual and forecast earnings are spilt-unadjusted from the detailed I/B/E/S file.	I/B/E/S
SUE_SRW	Standardized unexpected earnings measured as earnings minus expected earnings, deflated by the stock price at the end of the quarter, following a rolling seasonal random walk model. Earnings is basic EPS (*EPS*) before extraordinary items (*epspxq*). Expected earnings is EPS for the same quarter in the prior year (adjusted for stock splits through the current quarter).	Compustat
SUE_SRWex	Standardized unexpected earnings measured as earnings minus expected earnings, deflated by the stock price at the end of the quarter, following a rolling seasonal random walk model. Earnings is basic EPS (*EPS*) before extraordinary items (*epspxq*). Expected earnings is EPS for the same quarter in the prior year (adjusted for stock splits through the current quarter). Earnings figures exclude special items (*spiq*). When special items are excluded, *spiq* is multiplied by 0.65 and scaled by the number of shares (*cshoq*) used to calculate EPS.	Compustat
Viol	An indicator variable equal to 1 if the borrower firm discloses an actual covenant violation in its 10-K/10-Q SEC filings in a specific quarter, collected by Nini et al. (2009).	http://faculty.chicagobooth.edu/amir.sufi/data.html
Vol	Trading volume measured by multiplying the closing stock price by the number of shares traded from days [-272, -21] relative to the earnings announcement day.	CRSP

This table provides definitions of all variables used in our analyses. We transform all continuous variables except *PDCV*, *PDCV_PCOV*, and *PDCV_CCOV* into scaled decile rank values.

Table 2
Sample Selection

	No. of loans	No. of firms	No. of firm-quarters
Loan Sample			
Loan packages obtained from the Demerjian and Owens (2016) covenant tightness dataset	16,485		
Less: loans no longer in existence in the updated version of Dealscan	(22)		
Final	16,463	5,398	
Firm Sample			
All firm-quarters available in Compustat for 1993–2015			835,721
Less: firm-quarters that cannot be merged with CRSP and I/B/E/S			(182,501)
Less: firm-quarters with share prices less than $1, and market capitalization less than $5 million			(183,115)
Less: firm-quarters with insufficient data			(84,921)
Final		12,438	385,184
Merged Sample			
Merge the loan sample with the firm sample, retaining four quarters' firm data both pre and post-loan issuance			52,479
Less: firms without at least one available observation in both the pre-loan and post-loan issuance periods			(4,377)
Final	8,348	2,749	48,102

This table presents the sample selection process for the sample used in our main analyses.

Table 3
Frequency of Performance and Capital Covenant Types

	Percent
Performance Covenants	
Cash interest coverage	1.03
Debt service coverage	4.91
Fixed charge coverage	33.46
Interest coverage	38.78
Debt to EBITDA	54.31
Senior debt to EBITDA	8.16
EBITDA	5.69
Capital Covenants	
Debt to equity	0.68
Debt to tangible net worth	7.32
Leverage ratio	24.37
Senior leverage	0.19
Current ratio	8.33
Quick ratio	2.56
Net worth	20.38
Tangible net worth	14.91

This table presents the frequency of performance and capital covenants included in our sample.

Table 4
Summary Statistics

Variable	N	Mean	Std. Dev.	25th Pctl.	Median	75th Pctl.
CAR	48,102	0.007	0.209	-0.083	0.010	0.104
SUE	48,102	-0.000	0.024	-0.000	0.000	0.002
PDCV	48,102	0.287	0.387	0.004	0.053	0.642
PDCV_PCOV	48,102	0.230	0.364	0.000	0.017	0.286
PDCV_CCOV	48,102	0.082	0.229	0.000	0.000	0.030
C_score	48,102	0.046	0.079	0.004	0.041	0.077
Size (in millions $)	48,102	5,116.413	14,418.374	404.689	1,330.351	4,140.286
Anf	48,102	6.590	5.635	2.000	5.000	9.000
Vol (in millions $)	48,102	8,246.305	23,460.573	406.286	1,784.705	6,819.244
Price	48,102	89.831	4,335.340	11.588	20.284	33.413
Instown	48,102	0.547	0.312	0.339	0.634	0.799
EP	48,102	0.020	0.236	-0.122	0.012	0.161
Badnews	48,102	0.326	0.469	0.000	0.000	1.000
4thqtr	48,102	0.229	0.420	0.000	0.000	0.000

This table presents the summary statistics. All variables are defined in Table 1.

Table 5
Correlation Matrix

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
(1) *CAR*		0.042***	-0.005	-0.014***	0.005	0.025***	-0.037***	-0.019***	0.000	-0.098***	-0.001	0.000	-0.015***	0.052***
(2) *SUE*	0.047***		-0.026***	-0.029***	0.000	0.002	0.001	-0.013***	0.017***	0.004	0.015***	-0.006	-0.798***	0.000
(3) *PDCV*	-0.020***	-0.034***		0.815***	0.444***	0.379***	-0.417***	-0.195***	-0.352***	-0.320***	-0.102***	-0.003	0.074***	-0.016***
(4) *PDCV_PCOV*	-0.028***	-0.038***	0.872***		0.042***	0.362***	-0.397***	-0.194***	-0.342***	-0.316***	-0.096***	-0.014***	0.066***	-0.021***
(5) *PDCV_CCOV*	-0.001	-0.006	0.481***	0.057***		0.155***	-0.210***	-0.107***	-0.168***	-0.095***	-0.073***	0.018***	0.023***	-0.001
(6) *C_score*	0.024***	0.004	0.298***	0.297***	0.090***		-0.715***	-0.411***	-0.671***	-0.482***	-0.151***	-0.002	0.103***	0.021***
(7) *Size*	-0.034***	-0.001	-0.288***	-0.283***	-0.090***	-0.715***		0.583***	0.883***	0.575***	0.196***	-0.019***	-0.092***	0.020***
(8) *Anf*	-0.020***	-0.014***	-0.112***	-0.125***	-0.013***	-0.412***	0.584***		0.611***	0.291***	0.184***	0.005	-0.009*	-0.106***
(9) *Vol*	-0.005	0.016***	-0.241***	-0.241***	-0.067***	-0.671***	0.883***	0.613***		0.430***	0.232***	-0.002	-0.103***	0.020***
(10) *Price*	-0.091***	0.003	-0.256***	-0.268***	-0.052***	-0.482***	0.575***	0.291***	0.430***		0.198***	-0.005	-0.090***	0.011**
(11) *Instown*	-0.009**	0.014***	-0.096***	-0.111***	-0.006	-0.150***	0.195***	0.180***	0.230***	0.197***		0.008*	-0.045***	0.011**
(12) *EP*	-0.002	-0.006	-0.011**	-0.014***	0.001	-0.002	-0.019***	0.006	-0.002	-0.005	0.007		0.005	-0.003
(13) *Badnews*	-0.020***	-0.797***	0.077***	0.076***	0.026***	0.103***	-0.092***	-0.009*	-0.103***	-0.090***	-0.044***	0.005		-0.001
(14) *4fthqtr*	0.054***	0.000	-0.009*	-0.012***	0.003	0.021***	0.020***	-0.104***	0.020***	0.011**	0.012***	-0.003	-0.001	

This table presents the correlation coefficients. All variables except the PDCV and dummy variables are transformed into decile ranks within each calendar quarter, and coded from 0 to 1. Spearman (Pearson) correlation coefficients are presented above (below) the diagonal. All variables are defined in Table 1. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 6
PDCV and Changes in PEAD Around Loan Issuance (Test of Hypothesis H1a vs Hypothesis H1b)

	Dependent variable = *CAR*					
	(1)		(2)		(3)	
Variable	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat
SUE	0.077	4.09***	0.071	3.74***	0.079	4.13***
PDCV			-0.032	-5.27***	-0.022	-2.85***
SUE × PDCV			0.018	1.88*	-0.006	-0.49
Post					0.000	0.00
Post × SUE					-0.015	-2.05**
Post × PDCV					-0.018	-1.62
Post × SUE × PDCV					0.045	2.41**
C_score	-0.022	-2.20**	-0.016	-1.59	-0.015	-1.53
SUE × C_ score	0.020	1.19	0.016	0.99	0.015	0.91
Size	-0.013	-0.79	-0.020	-1.18	-0.020	-1.18
SUE × Size	-0.035	-1.27	-0.032	-1.13	-0.031	-1.12
Anf	-0.001	-0.17	0.002	0.27	0.002	0.22
SUE × Anf	-0.009	-0.60	-0.013	-0.82	-0.012	-0.78
Vol	0.042	2.63***	0.042	2.68***	0.043	2.73***
SUE × Vol	0.016	0.58	0.016	0.59	0.015	0.54
Price	-0.057	-6.64***	-0.061	-7.12***	-0.062	-7.19***
SUE × Price	-0.011	-0.70	-0.007	-0.44	-0.006	-0.38
Instown	0.016	2.31**	0.014	2.11**	0.014	2.10**
SUE × Instown	-0.029	-2.45**	-0.028	-2.37**	-0.028	-2.36**
EP	-0.001	-0.14	-0.002	-0.27	-0.002	-0.29
SUE × EP	-0.002	-0.14	-0.001	-0.06	-0.001	-0.06
Badnews	0.025	4.44***	0.028	4.90***	0.028	4.89***
SUE × Badnews	-0.057	-2.88***	-0.067	-3.41***	-0.066	-3.32***
4fthqtr	0.041	8.04***	0.041	8.06***	0.041	8.08***
SUE × 4fthqtr	-0.030	-3.32***	-0.030	-3.36***	-0.030	-3.34***
Intercept	-0.094	-2.41**	-0.091	-2.13**	-0.094	-2.24**
Year fixed effects	Included		Included		Included	
Observations	48,102		48,102		48,102	
Adj. R^2	0.0211		0.0228		0.0231	

This table presents regression results of the effect of overall covenant tightness on the change in PEAD from before to after loan issuance, controlling for other drift-related variables. All variables except the PDCV and dummy variables are transformed into decile ranks within each calendar quarter, and coded from 0 to 1. All variables are defined in Table 1. The t-statistics are based on standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 7
Performance vs. Capital Covenants

	Dependent variable = *CAR*			
	(1) *PDCV_PCOV*		(2) *PDCV_CCOV*	
Variable	Coefficient	t-stat	Coefficient	t-stat
SUE	0.081	4.25***	0.075	3.95***
PDCV	-0.026	-3.16***	-0.016	-1.25
SUE × PDCV	-0.016	-1.15	0.032	1.46
Post	-0.002	-0.42	-0.005	-1.29
Post × SUE	-0.012	-1.76*	-0.002	-0.24
Post × PDCV	-0.017	-1.41	-0.010	-0.55
Post × SUE × PDCV	0.049	2.43**	0.013	0.42
C_score	-0.015	-1.48	-0.021	-2.12**
SUE × C_ score	0.016	0.98	0.018	1.08
Size	-0.018	-1.08	-0.016	-0.96
SUE × Size	-0.034	-1.20	-0.030	-1.09
Anf	0.001	0.15	-0.001	-0.06
SUE × Anf	-0.012	-0.74	-0.011	-0.74
Vol	0.042	2.69***	0.043	2.70***
SUE × Vol	0.015	0.56	0.014	0.51
Price	-0.063	-7.30***	-0.057	-6.64***
SUE × Price	-0.006	-0.42	-0.011	-0.72
Instown	0.014	2.04**	0.015	2.26**
SUE × Instown	-0.029	-2.40**	-0.029	-2.40**
EP	-0.002	-0.30	-0.001	-0.16
SUE × EP	-0.001	-0.08	-0.002	-0.13
Badnews	0.028	4.88***	0.026	4.50***
SUE × Badnews	-0.065	-3.32***	-0.057	-2.87***
4fthqtr	0.041	8.03***	0.041	8.11***
SUE × 4fthqtr	-0.029	-3.30***	-0.030	-3.38***
Intercept	-0.094	-2.25**	-0.094	-2.39**
Year fixed effects	Included		Included	
Observations	48,102		48,102	
Adj. R^2	0.0239		0.0214	

This table presents regression results of the effect of performance and capital covenant tightness on the change in PEAD from before to after loan issuance, controlling for other drift-related variables. All variables except the PDCV and dummy variables are transformed into decile ranks within each calendar quarter, and coded from 0 to 1. All variables are defined in Table 1. The t-statistics are based on standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 8
Conditional Effect of the Borrower Firm's Bargaining Power

	Dependent variable = *CAR*			
	(1) Low		(2) High	
Variable	Coefficient	t-stat	Coefficient	t-stat
Panel A: *Size*				
SUE	0.067	1.91*	0.108	2.00**
PDCV	-0.015	-1.19	-0.056	-3.77***
SUE × PDCV	-0.026	-1.31	0.036	1.40
Post	0.001	0.05	-0.006	-1.06
Post × SUE	-0.031	-2.06**	-0.000	-0.05
Post × PDCV	-0.044	-2.38**	0.048	2.53**
Post × SUE × PDCV	0.084	2.83***	-0.023	-0.73
Low=High (Chi-squared, p-value)		0.015		
Controls	Included		Included	
Year fixed effects	Included		Included	
Observations	16,077		15,985	
Adj. R^2	0.025		0.025	
Panel B: *ROA*				
SUE	0.053	1.65*	0.129	3.50***
PDCV	-0.012	-0.98	-0.050	-3.21***
SUE × PDCV	-0.020	-0.99	0.055	2.05**
Post	-0.006	-0.77	0.002	0.33
Post × SUE	-0.009	-0.65	-0.012	-1.00
Post × PDCV	-0.033	-1.91*	0.034	1.64
Post × SUE × PDCV	0.057	1.95*	-0.046	-1.25
Low=High (Chi-squared, p-value)		0.027		
Controls	Included		Included	
Year fixed effects	Included		Included	
Observations	16,021		15,933	
Adj. R^2	0.0192		0.0310	
Panel C: *Financial Constraint*				
SUE	0.033	0.82	0.076	2.07**
PDCV	-0.039	-2.56**	-0.014	-1.01
SUE × PDCV	0.047	1.79*	-0.041	-1.84*
Post	-0.002	-0.44	0.002	0.22
Post × SUE	0.002	0.17	-0.031	-1.92*
Post × PDCV	0.014	0.71	-0.030	-1.52
Post × SUE × PDCV	-0.010	-0.29	0.079	2.38**
Low=High (Chi-squared, p-value)		0.061		
Controls	Included		Included	
Year fixed effects	Included		Included	
Observations	15,173		15,086	
Adj. R^2	0.019		0.022	

Table 8
Conditional Effect of the Borrower Firm's Bargaining Power

	Dependent variable = *CAR*			
	(1) Low		(2) High	
Variable	Coefficient	t-stat	Coefficient	t-stat
Panel D: *New Borrower*				
SUE	0.076	3.79***	0.075	1.21
PDCV	-0.028	-3.48***	0.027	0.97
SUE × PDCV	0.006	0.48	-0.124	-2.54**
Post	-0.003	-0.63	0.029	1.61
Post × SUE	-0.010	-1.33	-0.067	-2.19**
Post × PDCV	-0.011	-0.94	-0.080	-2.06**
Post × SUE × PDCV	0.027	1.39	0.210	3.08***
Low=High (Chi-squared, p-value)		0.009		
Controls	Included		Included	
Year fixed effects	Included		Included	
Observations	44,107		3,995	
Adj. R^2	0.023		0.026	
Panel E: *Lender No.*				
SUE	0.074	2.30**	0.075	1.96*
PDCV	-0.036	-2.89***	-0.004	-0.29
SUE × PDCV	-0.008	-0.40	-0.014	-0.59
Post	0.001	0.08	-0.005	-0.77
Post × SUE	-0.019	-1.36	-0.009	-0.92
Post × PDCV	-0.004	-0.20	-0.050	-2.74***
Post × SUE × PDCV	0.045	1.52	0.072	2.27**
Low=High (Chi-squared, p-value)		0.529		
Controls	Included		Included	
Year fixed effects	Included		Included	
Observations	17,588		15,085	
Adj. R^2	0.023		0.024	

This table presents regression results of the effect of overall covenant tightness on the change in PEAD from before to after loan issuance, conditional on the borrower firm's bargaining power, controlling for other drift-related variables. Control variables are the same as in Table 6. The low (high) column refers to the bottom (top) tercile of the sample based on the conditional variable, except in Panel D, where it refers to the condition that *New Borrower* equals 0 (1). The F-test is used to test the statistical significance of the difference in the coefficients of *Post × SUE × PDCV* across the two subsamples, and the p-value is reported. All variables except the PDCV and dummy variables are transformed into decile ranks within each calendar quarter, and coded from 0 to 1. All variables are defined in Table 1. The t-statistics are based on standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 9
Conditional Effect of the Borrower Firm's Change in Earnings Management

	Dependent variable = *CAR*			
	(1) increase		(2) decrease	
Variable	Coefficient	t-stat	Coefficient	t-stat
Panel A: Overall Earnings Management				
SUE	0.055	1.53	0.093	2.55**
PDCV	-0.029	-2.06**	-0.032	-2.25**
SUE × PDCV	-0.001	-0.04	-0.001	-0.04
Post	0.000	0.02	-0.004	-0.38
Post × SUE	-0.037	-2.73***	0.011	0.65
Post × PDCV	-0.039	-1.80*	0.014	0.69
Post × SUE × PDCV	0.087	2.48**	-0.004	-0.12
Low=High (Chi-squared, p-value)		0.070		
Controls	Included		Included	
Year fixed effects	Included		Included	
Observations	14,392		12,786	
Adj. R^2	0.026		0.023	
Panel B: Accruals Earnings Management				
SUE	0.074	2.12**	0.059	1.98**
PDCV	-0.018	-1.39	-0.041	-3.00***
SUE × PDCV	-0.019	-0.88	0.025	1.07
Post	-0.000	-0.06	-0.001	-0.07
Post × SUE	-0.025	-1.96*	-0.002	-0.17
Post × PDCV	-0.037	-1.88*	0.011	0.58
Post × SUE × PDCV	0.091	2.87***	-0.002	-0.06
Low=High (Chi-squared, p-value)		0.037		
Controls	Included		Included	
Year fixed effects	Included		Included	
Observations	15,781		16,797	
Adj. R^2	0.0260		0.0214	
Panel C: Real Earnings Management				
SUE	0.037	1.07	0.137	3.94***
PDCV	-0.024	-1.80*	-0.020	-1.37
SUE × PDCV	-0.005	-0.21	-0.020	-0.82
Post	-0.002	-0.23	0.005	0.49
Post × SUE	-0.035	-2.66***	0.001	0.05
Post × PDCV	-0.046	-2.23**	0.011	0.56
Post × SUE × PDCV	0.082	2.43**	0.008	0.24
Low=High (Chi-squared, p-value)		0.135		
Controls	Included		Included	
Year fixed effects	Included		Included	
Observations	15,851		13,726	
Adj. R^2	0.026		0.023	

This table presents regression results of the effect of overall covenant tightness on the change in PEAD from before to after loan issuance, conditional on the borrower firm's changes in earnings management from the pre- to post-loan issuance period, controlling for other drift-related variables. Control variables are the same as in Table 6. Overall earnings management in Panel A is the sum of accruals and real earnings management. Accruals earnings management in Panel B is measured using the modified Jones model (Dechow et al., 1995). Real earnings management in Panel C is measured using the Roychowdhury (2006) models. Both accruals and real earnings management measures are adjusted for the quarterly setting following Collins et al. (2017). The F-test is used to test the statistical significance of the difference in the coefficients of *Post × SUE × PDCV* across the two subsamples, and the p-value is reported. All variables except the PDCV and dummy variables are transformed into decile ranks within each calendar quarter, and coded from 0 to 1. All variables are defined in Table 1. The t-statistics are based on standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 10
Control of Actual Covenant Violations (Robustness Tests)

Variable	Dependent variable = *CAR* Coefficient	t-stat
SUE	0.064	2.48**
PDCV	-0.016	-1.60
SUE × *PDCV*	-0.014	-0.84
Post	-0.003	-0.54
Post × *SUE*	-0.010	-0.99
Post × *PDCV*	-0.026	-1.83*
Post × *SUE* × *PDCV*	0.058	2.46**
Viol	-0.025	-4.11***
SUE × *Viol*	0.017	1.63
Controls	Included	
Year fixed effects	Included	
Observations	30,146	
Adj. R^2	0.027	

This table presents regression results of the effect of overall covenant tightness on the change in PEAD from before to after loan issuance, controlling for actual covenant violations (*Viol*) and other drift-related variables. Other control variables are the same as in Table 6. All variables except the PDCV and dummy variables are transformed into decile ranks within each calendar quarter, and coded from 0 to 1. All variables are defined in Table 1. The t-statistics are based on standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 11
Control of Other Loan Contract Terms (Robustness Tests)

	Dependent variable = *CAR*	
Variable	Coefficient	t-stat
SUE	0.092	3.71***
PDCV	-0.016	-1.92*
SUE × PDCV	-0.015	-1.10
Post	0.034	3.00***
Post × SUE	-0.036	-1.81*
Post × PDCV	-0.022	-1.83*
Post × SUE × PDCV	0.051	2.55**
Amount	0.023	1.73*
SUE × Amount	0.007	0.31
Post × Amount	-0.033	-2.37**
Post × SUE × Amount	0.038	1.62
Maturity	0.021	2.16**
SUE × Maturity	-0.025	-1.49
Post × Maturity	-0.035	-2.41**
Post × SUE × Maturity	0.045	1.85*
PPP	0.001	0.14
SUE × PPP	0.014	1.18
Post × PPP	0.000	0.03
Post × SUE × PPP	-0.022	-1.30
Secured	-0.009	-1.39
SUE × Secured	0.007	0.64
Post × Secured	-0.009	-1.00
Post × SUE × Secured	0.006	0.39
Controls	Included	
Year fixed effects	Included	
Observations	42,244	
Adj. R^2	0.023	

This table presents regression results of the effect of overall covenant tightness on the change in PEAD from before to after loan issuance, controlling for loan contract terms and other drift-related variables. Other control variables are the same as in Table 6. All variables except the PDCV and dummy variables are transformed into decile ranks within each calendar quarter, and coded from 0 to 1. All variables are defined in Table 1. The t-statistics are based on standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 12
Control of Loan Purposes (Robustness Tests)

	Dependent variable = *CAR*	
Variable	Coefficient	t-stat
SUE	0.087	2.67***
PDCV	-0.019	-2.50**
SUE × PDCV	-0.009	-0.71
Post	-0.004	-0.15
Post × SUE	0.009	0.19
Post × PDCV	-0.019	-1.70*
Post × SUE × PDCV	0.045	2.40**
Acquisition line	0.020	0.99
SUE × Acquisition line	0.007	0.19
Post × Acquisition line	-0.023	-0.75
Post × SUE × Acquisition line	-0.014	-0.25
LBO/MBO	0.069	1.76*
SUE × LBO/MBO	-0.015	-0.15
Post × LBO/MBO	-0.089	-1.23
Post × SUE × LBO/MBO	0.108	0.73
Takeover	0.027	1.52
SUE × Takeover	0.002	0.05
Post × Takeover	-0.033	-1.22
Post × SUE × Takeover	-0.003	-0.07
Debt repayment	-0.014	-0.81
SUE × Debt repayment	0.026	0.86
Post × Debt repayment	0.006	0.25
Post × SUE × Debt repayment	-0.021	-0.42
Corporate purposes	0.012	0.76
SUE × Corporate purposes	-0.017	-0.62
Post × Corporate purposes	0.009	0.35
Post × SUE × Corporate purposes	-0.023	-0.48
Working capital	0.012	0.70
SUE × Working capital	-0.012	-0.41
Post × Working capital	0.013	0.52
Post × SUE × Working capital	-0.037	-0.77
Commercial paper backup	0.016	0.91
SUE × Commercial paper backup	-0.014	-0.46
Post × Commercial paper backup	0.013	0.50
Post × SUE × Commercial paper backup	-0.030	-0.57
Debtor-in-procession	-0.852	-49.22***
SUE × Debtor-in-procession	1.046	37.99***
Post × Debtor-in-procession	0.932	35.02***
Post × SUE × Debtor-in-procession	-1.354	-28.70***
Recapitalization	0.029	0.66
SUE × Recapitalization	-0.092	-0.65
Post × Recapitalization	0.016	0.25
Post × SUE × Recapitalization	0.025	0.16
Controls	Included	
Year fixed effects	Included	
Observations	48,039	
Adj. R^2	0.026	

This table presents regression results of the effect of overall covenant tightness on the change in PEAD from before to after loan issuance, controlling for loan purposes and other drift-related variables. The loan purpose variables are indicator variables equal to 1 if the primary purpose of the loan is to fund asset acquisition (*Acquisition line*), leveraged buyout/management buyout (*LBO/MBO*), takeover (*Takeover*), debt repayment (*Debt repayment*), corporate purposes (*Corporate purposes*), working capital (*Working capital*), guarantee of an issuer's commercial paper (*Commercial paper backup*), companies during restructuring under corporate bankruptcy law (*Debtor-in-procession*), recapitalization (*Recapitalization*), and 0 otherwise. The classification of loan purposes follows Bharath, Dahiya, Saunders, and Srinivasan

Table 12
Control of Loan Purposes (Robustness Tests)

(2011). Other control variables are the same as in Table 6. All variables except the PDCV and dummy variables are transformed into decile ranks within each calendar quarter, and coded from 0 to 1. All variables are defined in Table 1. The t-statistics are based on standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 13
Placebo Test Subdividing Pre- and Post-loan Issuance Periods into Earlier and Later Periods (Robustness Tests)

	Dependent variable = *CAR*			
	(1) pre-loan issuance		(2) post-loan issuance	
Variable	Coefficient	t-stat	Coefficient	t-stat
SUE	0.093	3.61***	0.057	2.16**
PDCV	-0.036	-3.81***	-0.041	-3.56***
SUE × *PDCV*	0.001	0.08	0.042	2.28**
Later period	0.002	0.30	-0.001	-0.14
Later period × *SUE*	-0.005	-0.47	0.004	0.41
Later period × *PDCV*	0.016	1.10	0.014	0.89
Later period × *SUE* × *PDCV*	-0.016	-0.64	-0.010	-0.40
Controls	Included		Included	
Year fixed effects	Included		Included	
Observations	20,776		27,263	
Adj. R^2	0.033		0.018	

This table presents regression results of the association between overall covenant tightness and changes in PEAD from earlier to later pre-loan issuance periods (i.e., q-5 & q-4 versus q-3 & q-2) and from earlier to later post-loan issuance periods (i.e., q+1 & q+2 versus q+3 & q+4), respectively, controlling for other drift-related variables. Control variables are the same as in Table 6. All variables except the PDCV and dummy variables are transformed into decile ranks within each calendar quarter, and coded from 0 to 1. All variables are defined in Table 1. The t-statistics are based on standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 14
Shorter Periods Before and After Loan Issuance (Robustness Tests)

	Dependent variable = *CAR*					
	(1) One quarter		(2) Two quarters		(3) Three quarters	
Variable	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat
SUE	0.080	2.42**	0.097	3.52***	0.077	3.61***
PDCV	-0.007	-0.51	-0.006	-0.50	-0.015	-1.66*
SUE × *PDCV*	-0.024	-1.00	-0.023	-1.05	-0.008	-0.52
Post	-0.008	-0.98	-0.004	-0.60	0.004	0.83
Post × *SUE*	-0.000	-0.00	-0.011	-0.94	-0.019	-2.29**
Post × *PDCV*	-0.039	-1.92*	-0.035	-2.21**	-0.020	-1.63
Post × *SUE* × *PDCV*	0.061	1.81*	0.065	2.39**	0.038	1.73*
Controls	Included		Included		Included	
Year fixed effects	Included		Included		Included	
Observations	12,740		19,671		34,319	
Adj. R^2	0.026		0.024		0.021	

This table presents regression results of the effect of overall covenant tightness on the change in PEAD from before to after loan issuance, controlling for other drift-related variables. In these tests, we adopt shorter windows, that is, one/two/three quarter(s) before and after loan issuance instead of four quarters. Control variables are the same as in Table 6. All variables except the PDCV and dummy variables are transformed into decile ranks within each calendar quarter, and coded from 0 to 1. All variables are defined in Table 1. The t-statistics are based on standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 15
Alternative Measures of SUE (Robustness Tests)

	Dependent variable = *CAR*			
	(1) *SUE_SRW*		(2) *SUE_SRWex*	
Variable	Coefficient	t-stat	Coefficient	t-stat
SUE	0.058	2.75***	0.060	2.81***
PDCV	-0.020	-2.67***	-0.017	-2.21**
SUE × *PDCV*	-0.011	-0.84	-0.017	-1.29
Post	-0.003	-0.65	-0.002	-0.57
Post × *SUE*	-0.008	-1.09	-0.009	-1.16
Post × *PDCV*	-0.013	-1.16	-0.013	-1.13
Post × *SUE* × *PDCV*	0.033	1.74*	0.032	1.66*
Controls	Included		Included	
Year fixed effects	Included		Included	
Observations	48,129		48,129	
Adj. R^2	0.022		0.022	

This table presents regression results of the effect of overall covenant tightness on the change in PEAD from before to after loan issuance, controlling for other drift-related variables. We adopt alternative measures of *SUE* (*SUE_SRW/ SUE_SRWex*) in these tests. Control variables are the same as in Table 6. All variables except the PDCV and dummy variables are transformed into decile ranks within each calendar quarter, and coded from 0 to 1. All variables are defined in Table 1. The t-statistics are based on standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Figure 1. Graphical depiction of PDCV effect on PEAD