The forecasting use of EBITDA covenants by equity investors[*]

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Abstract:

Covenants are traditionally viewed as contractual devices used by banks to monitor borrowers. We examine whether covenants can also be used by equity investors outside of loan contracts to help forecast future borrower performance. Using analysts to proxy for investors' behavior, we find that analysts use the covenant thresholds set on borrower EBITDA to revise their expectations. Specifically, analysts revise their outstanding forecasts *upward* when the forecasts fall below the threshold and *downward* when they are well above it. We also find that EBITDA covenants contain incremental information useful for predicting future borrower performance and the revisions triggered by these covenants result in more accurate forecasts. Beyond analyst forecasts, we also find a reduction in information asymmetry around loans with EBITDA covenants, consistent with the idea that investors can use EBITDA covenants to help predict borrower performance. Overall, we highlight a new and perhaps unintended use of EBITDA covenants by equity investors.

Keywords: bank loans; debt contracts; debt covenants; equity analysts; earnings forecasts; information asymmetry.

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1 Introduction

Bank loans are among the most important and common form of financing to corporations (Eckbo 2007; Erel et al. 2012). At the end of 2018, commercial and industrial loans made by US banks amounted to 2.3 trillion dollars (Board of Governors 2018). When deciding whether to lend to a borrower, banks hold private conversations with management and perform on-site visits to gather information. Simultaneously, borrowers share with banks confidential information and internal forecasts about current and future projects to ensure access to credit (e.g., Smith and Warner 1979; Bradley and Roberts 2004). Private communications help banks and borrowers to determine loan amount, interest rate, and non-price contract terms such as accounting covenants, which are contractual devices used by banks to monitor borrowers (e.g., Rajan and Winton 1995; Dichev and Skinner 2002). A large literature focuses on how accounting numbers in covenants facilitate bank monitoring and assist *stewardship* purposes (Watts and Zimmerman 1986; Shivakumar 2013). An unanswered question is whether accounting information in covenants can also be used for *forecasting and valuing* the borrower by equity investors *outside* of the loan contract. In this paper, we answer this question by examining how investors, who can access the contract details through firm disclosures or commercial databases use covenants set on borrower performance metrics for forecasting and valuation purposes.

Covenants set on borrower performance metrics, especially EBITDA, are common (Leftwich 1983). These so-called performance covenants (e.g., minimum EBITDA requirements) set a prudent threshold so that if borrower performance deteriorates unexpectedly, banks can promptly intervene in borrowers' actions, renegotiate, or both to prevent any losses (Christensen and Nikolaev 2012). Since covenant thresholds reflect the *minimum* level of borrower performance acceptable to banks, investors outside of the loan contract who have different payoff structures and are primarily interested in the *expected* level of borrower performance may not consider the information in covenant thresholds particularly relevant. However, there are reasons for why investors may find the threshold useful. For example, prior studies find that the thresholds are carefully chosen by banks and borrowers based on their private information, and that where the thresholds are set is informative about future changes in borrower performance (e.g., Demiroglu and James 2010; Li, Vasvari, and Wittenberg-Moerman 2016). As such, investors may find the threshold details incrementally useful for their forecasting purposes.

In forming their expectations about a firm's future performance, investors often construct estimates of firm performance under multiple scenarios, for example upside and downside scenarios (e.g., Joos, Piotroski, and Srinivasan 2016). We expect that investors can use the covenant threshold to help assess both the probability of each scenario and the borrower performance forecast under the scenario. Further, how investors use the covenant information is likely to depend on whether their current expectations are above or below the threshold.

For investors whose current expectations are *well above* the minimum EBITDA threshold indicated in the covenant, they are likely to reassign probabilities from the upside to the other, more downside scenarios. Additionally, these investors may also lower their scenario forecasts which are likely to well exceed the threshold. This can happen because, in practice, covenants are set tightly relative to the variation in the underlying variables, meaning that there is a high likelihood that the realized EBITDA will lie relatively close to the covenant threshold (Dichev and Skinner 2002). Banks set covenants tightly because they are characterized by asymmetric payoffs and are particularly concerned about borrower downside risk. To ensure that they can step in promptly to take precautionary actions when borrower performance deteriorates unexpectedly, banks set the minimum EBITDA threshold at a prudent, yet realistic, level. Knowing these practices, investors especially those whose expectations are *well* above the EBITDA threshold are likely to revise their expectations *downward*.

For investors whose current expectations are *below* the minimum EBITDA, they are likely to revise their expectations *upward* because the chance that a borrower will comply with covenants is high. Borrowers seek to comply with covenants because violations can impose costs such as higher interest rates, reductions in loan amount, and new restrictions that limit borrowers' investments and payouts. Empirically, covenants are not violated in the majority of loans (about 70 percent; e.g., Dichev and Skinner 2002), and investors likely take this into account when forming their expectations about borrower performance.

Our main tests focus on how analysts revise their forecasts for borrowers shortly after loans with performance covenants, instead of how equity investors react to loan events. We focus on analysts because we can directly observe their earnings expectations from their forecasts. Hence, we can compare analyst expectations before the loan event with the EBITDA covenant threshold and attribute any subsequent forecast revisions to the use of information in performance covenants. As in prior literature (Richardson, Tuna, and Wysocki 2010; Bradshaw 2011; deHaan, Madsen, and Piotroski 2017), we argue that analyst behaviors provide a reasonable proxy for investor behaviors and thus we can study analyst forecasts to infer investors' use of performance covenants.

To construct our sample, we start with a sample of loans with contract details in DealScan over the period 1995 to 2012.[1] We then identify analysts who follow the borrowers prior to the loan events, and examine how they revise their forecasts shortly (i.e., in the two weeks) after the loan event. To limit any confounding effects, we exclude observations associated with loan

[1] Our analysis stops in 2012 since this is the last available year in the DealScan-Compustat linking tables provided by Chava and Roberts (2008). Similarly, we start our sample in 1995 because of limited DealScan coverage of loans in prior years (e.g., Demerjian 2011).

contracts with concurrent news events (i.e., earnings announcements and management guidance) around the loan event (i.e. the two weeks before and two weeks after the event).

The test includes loan event fixed effects, which allows us to compare across analysts facing the *same* loan contract for the *same* borrower and examine how they revise their forecasts differently depending on their expectations prior to the loan event (i.e., where their forecasts are relative to the EBITDA thresholds). This *within*-loan comparison holds constant all borrower and loan characteristics (e.g., loan terms and market reactions to loan announcements) and allows us to examine whether analysts revise their forecasts using the information in covenant thresholds. Consistent with our hypothesis, we find that analysts are more likely to revise their forecasts *upward* if their forecasts fall *below* the covenant threshold. We also find that analysts are more likely to revise *downward* when their forecasts are well *above* the threshold value.

To strengthen inferences, we exploit cross-sectional variations in borrower and loan covenant characteristics to identify situations where analysts are more likely to find the covenant information useful. If we find that the results are mainly concentrated in these situations, we can then better attribute the revisions shortly after a loan event to analysts' use of performance covenants. Consistent with our hypothesis, we find that results are concentrated in situations where (i) borrower management provide limited information about its firm's future performance, (ii) there is greater uncertainty about future borrower performance, (iii) borrower default risk is high which attracts bank scrutiny and tight covenants, and (iv) the covenant threshold is more informative about changes in future borrower performance. Taken together, the results lend additional support to our hypothesis that performance covenants have incremental information that is useful for forecasting purposes.

We conduct additional tests to further strengthen inferences. First, we expect and find that

EBITDA thresholds are incrementally useful in predicting realized borrower performance in the future, after controlling for what the analysts already knew prior to the loan event as reflected in their outstanding forecasts. Second, we study revised forecasts triggered by loan events and find that they are more accurate than the outstanding forecasts made by the same analyst right before the event. Third, we compare revised forecasts to outstanding forecasts for the same borrower by other analysts who did not revise after the loan event. We find that the revised forecasts are more accurate. Taken together, these results indicate that performance covenant thresholds contain incremental information which allows analysts to improve their predictions of future borrower performance.

Last, while using analysts as a proxy for investors allows us to design more direct and specific tests of our hypotheses, our final test focuses on investors directly and examines changes in information asymmetry shortly around loan contracts with performance covenants. If performance covenants allow investors to better predict future borrower performance, investors will assess lower information asymmetry after these loan events. Using bid-ask spreads and stock illiquidity as information asymmetry measures, our tests support this prediction.

We contribute to the literature by highlighting that performance covenants in loan contracts can be used for forecasting purposes by investors outside of the contracts, which differs from and complements the traditional view that accounting numbers in loan contracts mainly facilitate bank monitoring and serve a stewardship role (e.g., Watts and Zimmerman 1986; Shivakumar 2013; Dyreng, Vashishtha, and Weber 2017). Understanding these issues broadens our knowledge of the nature of accounting numbers in debt contracts. It also highlights a perhaps unintended use of accounting thresholds by non-contracting parties.

More broadly, our study indicates potential information transfers from the debt to equity markets. Prior studies suggest this possibility by studying how equity market participants react to price movements in the credit default swap market (Batta, Qiu and Yu 2016), and to research by debt analysts (e.g., Johnston, Markov and Ramnath 2009; Gurun, Johnston and Markov 2015) or lender-affiliated analysts (Chen and Martin 2011). We add to this line of research by highlighting that loan covenants embed *accounting*-related information privately shared between the contracting parties that equity investors can infer to improve their forecasting. To this end, our study provides a direct example of what accounting information equity investors can learn from debt market participants.

Our study differs from prior studies that examine loan announcements and the information associated with loan covenant choices. For example, previous studies generally find positive stock price reactions to news of loan issuances (e.g., James 1987; Lummer and McConnell 1989). In addition, more recent studies examine the information associated with loan covenant choices and find that where covenant thresholds are set is associated with changes in borrower characteristics in the future (Demiroglu and James 2010; Li et al. 2016). While these are related studies, they do not investigate whether accounting thresholds in loan covenants can help investors and analysts to improve their predictions of future borrower performance.

2 Institutional setting

2.1 Performance covenants and private information

Performance covenants are set on borrower EBITDA metrics. They are a common clause in loan contracts, appearing in more than 60 percent of U.S. loans (Chava and Roberts 2008), especially when borrower earnings better reflects default risk and hence is more contractible

(Christensen and Nikolaev 2012).[2] These covenants (e.g., minimum EBITDA requirements) indicate the minimum values of future borrower performance that banks accept before they will intervene in the lending relationships. While these are minimum acceptable values, they are not very low values. Instead, they are prudently set so that when covenants are violated, borrowers are still relatively healthy and there is still room for banks to intervene to prevent losses.

Prior studies suggest that, in practice, covenants are set tightly relative to the variation in the underlying variables, meaning that there is a high likelihood that the realized values of the covenant variables during the loan term will lie relatively close to the covenant thresholds (Dichev and Skinner 2002). Prior studies (e.g., Smith 1993) also suggest that it is optimal for banks to have tight covenants, for example, with the thresholds set just below the expected value. In that case, the covenants would not be violated under expected business conditions. But when unexpected deteriorations happen, the covenants would be quickly violated which allows banks to swiftly intervene and take precautionary actions to protect their interests. Because of these reasons, performance covenants are viewed as "trip wires" and serve as a primary contractual device that allows banks to maintain close monitoring over their borrowers (e.g., Rajan and Winton 1995; Dichev and Skinner 2002; Christensen and Nikolaev 2012).

Common consequences from covenant violations include (i) bank reviews and waivers, (ii) modifications to loan contracts in terms of higher interest rates, reductions in loan amount, and new restrictions that limit borrower investments and payouts, or (iii) in the more extreme cases, terminations of the loan (Beneish and Press 1993; Dichev and Skinner 2002; Roberts and Sufi

[2] Besides performance covenants, loan contracts often include other accounting covenants which are set on debt-to-equity ratio or net worth. These covenants, known as capital covenants, are mainly used to align banks' interests with those of shareholders and are more likely to be used for borrowers who have financial flexibility (Christensen and Nikolaev 2012). We do not focus on capital covenants because they are not directly based on earnings measures. As a result, it may be hard for investors to use them directly to improve their forecasts of borrowers' future earnings.

2009; Nini, Smith, and Sufi 2012; Ozelge and Saunders 2012). In addition, prior studies find that covenant violations and subsequent bank intervention are associated with increased turnovers of borrower top management (Nini et al. 2012; Ozelge and Saunders 2012). Because of the costly consequences, borrowers have strong incentives to avoid covenant violations.[3] Yet, Dichev and Skinner (2002) find that violations still occur in approximately 30 percent of loans, consistent with covenants having tight thresholds and serving as trip wires.

To specify covenant thresholds, banks collect information on borrowers from both public sources (e.g., SEC filings) and private sources (e.g., private interactions with the management), paying special attention to borrower downside risk. Meanwhile, borrowers provide banks with supplemental information, such as confidential business plans, internal sales forecasts, and future changes in cost structure, whose reliability will be evaluated and verified by banks. As a result, the thresholds reflect both the borrower's and the bank's private information concerning future changes in borrower performance.[4] Prior studies confirm this observation. For example, Demiroglu and James (2010) suggest that because of the costly consequences from covenant violations, borrowers are unlikely to agree to thresholds that are set close to their current performance if they do not expect their *future* performance to improve so that they will readily comply with the covenants. Consistent with this argument, the authors find that thresholds set close to the covenant variable computed using recent financials are positively associated with future improvements in that variable. Li et al. (2016) examine loans with thresholds that change over the loan duration (e.g., increasing minimum EBITDA requirements) and find that the trend in how the

[3] Dichev and Skinner (2002, 1096) explain that even for borrowers who can obtain a waiver of violation, they seek to avoid violations and the associated bank reviews. This is because "any review of the firm's operations by outsiders is likely to be costly – in terms of managerial time, the need to generate updated financial reports, and the need for management to ex-plain and justify its forecasts and strategy–and something managers prefer to avoid."

[4] In addition, covenant thresholds are likely to reflect factors such as bargaining powers of borrowers and banks because thresholds are negotiated values.

thresholds change reflects future changes in borrower performance.

Overall, performance covenants are a common contractual device that facilitates bank monitoring of borrowers, and prior research finds that that the EBITDA thresholds reflect contracting parties' private information concerning future borrower prospects. What is less well understood is whether and how performance covenants can help non-contracting parties such as public equity investors to forecast future borrower performance.

2.2 Public availability of loan contract details

The vast majority of publicly traded companies in the U.S. have bank loans, and loan contracts and updates are publicly available. On average, each loan has a maturity of less than four years (Bharath et al. 2011) and a typical contract is modified about five times before maturity (Roberts 2015).[5] This suggests that loan contract information frequently arise, which makes it a potentially relevant information source for investors.

Investors can access loan contract information in at least two ways. First, investors can use commercial databases such as Thomson-Reuters' LPC DealScan. DealScan collects loan pricing and contract details mainly through public borrower disclosures and sometimes supplements the information collection using their own contacts (Chava and Roberts 2008). Market participants can access the loan contract details in real time through the DealScan web-based platform.[6]

Investors can also use borrowers' public disclosures filed with the SEC to obtain loan contract details, especially after August 2004.[7] Item 1.01 of the 8-K filing requires firms entering

[5] Roberts (2015) notes that many loan contract changes are not due to borrowers' financial distress but to their desire to relax the previously contracted ratios. For example, a plan to issue debt could lead the borrower's debt-to-EBITDA ratio to go beyond its contracted threshold, so the borrower will try to renegotiate with the banks before the issuance.
[6] See https://www.loanpricing.com/products/loanconnectordealscan/.
[7] Prior to August 2004, there was no specific requirement that firms entering into loan contracts have to make a disclosure. However, Item 5 of the 8-K filing requirements recommended disclosure of unspecified events considered important by the firm which included loan events (Carter and Soo 1999).

into a material definitive agreement (including a loan contract) to file a Form 8-K describing the agreement within four business days after the event.[8] Firms are encouraged, but not required, to file the agreement as an exhibit to the Form 8-K, which firms commonly follow in practice to avoid litigation risk due to omitted disclosures.

We are interested in whether and how investors exploit loan contract details for forecasting purposes. We differ from studies documenting that 8-K disclosures elicit trades among investors (Carter and Soo 1999; Lerman and Livnat 2010) because our focus is on whether the earnings-based thresholds in performance covenants, which happen to be disclosed through 8-Ks, can be used by equity investors to predict future borrower performance.[9]

3 Hypotheses development

3.1 Investors' use of performance covenants

Covenants are conventionally viewed as contracting mechanisms set in place to regulate the relation between borrowers and banks (Rajan and Winton 1995; Dichev and Skinner 2002; Christensen and Nikolaev 2012). As discussed in Section 2.1, an EBITDA covenant threshold represents a prudent level of future borrower performance that banks accept before they will intervene and take precautionary corrective actions. For investors, if they hold the view that covenants are merely used by banks to monitor borrowers, they may not think of performance covenants as having useful information for forecasting purposes.

An alternative view is that performance covenants are rich in information and can aid investor forecasting. The EBITDA thresholds do not only reflect borrower private information of

[8] See https://www.sec.gov/about/forms/form8-k.pdf. Firms can also satisfy the disclosure requirement by describing the agreement in their Form 10-Q or 10-K if the Form is filed within four business days after the loan event.
[9] Prior studies focus on a variety of events that trigger 8-K disclosures (e.g., mergers and acquisitions, CEO departure) and use movements in stock price and volume to provide evidence that these disclosures are informative on average.

future performance and operational changes, but are also certified by an informed third party (the bank) who has performed due diligence on the borrower and has a vested financial interest. Thus, the information embedded in performance covenants likely extends beyond publicly available information and can be useful to equity investors in forming their forecasts of future borrower performance. Moreover, given banks' asymmetric payoff structures, they specialize in assessing borrower downside risks and have been shown to be superior to equity investors in this task (e.g., Batta et al. 2016). Consequently, investors may perceive the covenant threshold to be particularly informative when assessing a borrower's potential downside. For those investors who are able and willing to process the additional information in EBITDA thresholds, some can benefit by using it to confirm their forecasts of future borrower performance whereas others can use it to revise and improve their forecasts. We provide anecdotal evidence and additional explanations to these arguments below.

Anecdotally, on February 28, 2001, an UBS analyst covering Lucent Technologies reported that Lucent had filed an 8-K "which detailed the loan terms for the completed $4.5 billion 364-day credit facility." The analyst explained that he generated new estimates of Lucent earnings "as conditions of the loan require Lucent to meet certain EBITDA requirements." Interestingly, the analyst also noted that "[t]his is the first time that we have any type of *financial guidance* for Lucent since the 1Q01 earnings call" [italic added]. This anecdote indicates that investors might get hints about future borrower performance from performance covenants.

Because of the uncertainty surrounding business factors that can affect future performance, investors are unlikely to have a precise estimate of future borrower performance. More often, they generate a range of possible performance forecasts using different business factor assumptions and then form an expected estimate of future performance using these forecasts. Joos et al. (2016)

discuss how in practice, investors generate forecasts that reflect the base, downside, and upside scenarios according to their assessments about the conditions supporting each scenario and the probability of each scenario occurring. We expect that investors can use the EBITDA threshold in the covenant to help with these assessments. First, as discussed in section 2.1, since covenants are set tightly relative to the variation in the underlying variables, there is a high likelihood that the realized EBITDA will be relatively close to the covenant threshold. Anticipating that, investors can use the threshold to reassess the probability of the earnings scenarios and the forecasts under the scenarios. Second, because banks are characterized by their asymmetric payoffs and tend to focus on the borrower's downside, investors likely perceive banks to be superior in assessing borrower downside risks and will take that into account especially when reassessing the downside scenarios. We expect that these reassessments depend on whether investors' current expectations are above or below the threshold.

For investors whose current expectations are well above the EBITDA threshold, they are likely to perceive their expectations to be "too high" given that there is a low likelihood that the realized earnings outcome is well above the threshold.[10] As a result, investors are likely to decrease the probabilities of the upside occurring and correspondingly increase the probabilities of the other, more downside scenarios. Additionally, investors whose downside scenario forecasts exceed the threshold may also lower their forecasts under these scenarios, if investors consider banks to be superior in analyzing downside risks and think that the threshold reflects earnings outcomes

[10] To help to see that the probability of an earnings outcome that is well above the threshold is relatively low, we first compare the actual EBITDA in the first fiscal year end after the loan contract to the minimum EBITDA inferred from the covenant threshold. We find that about 50 percent of the borrowers in our sample report an actual EBITDA that is at most 1.5 times of the minimum EBITDA in the covenant threshold, and about 75 percent of the borrowers report an actual EBITDA that is at most 2.7 times of the minimum EBITDA. This indicates that the probability of an earnings outcome that is well above the threshold (e.g., more than 4 times of the threshold) is relatively low.

deemed plausible by banks under downside scenarios.[11] Both actions would lead investors to revise their expected earnings forecasts downward. For example, suppose investors currently estimate a borrower's future EBITDA to fall within the range of $9 to $15 million, with a current expected forecast of $12 million. Suppose further that a minimum EBITDA covenant of $3 million is imposed on the borrower by its bank. As long as investors reassign the probabilities of earnings outcomes from the higher end of the range (e.g., $14 - 15 million) to the lower end (e.g., $9 - 10 million), or they lower the estimates at the lower end from $9 to say, $8 million, the updated expected value will likely be lower than the current expected value of $12 million. This would then result in investors revising their EBITDA estimates downward.

The above arguments are less likely to apply to investors whose current expectations are just immediately above the EBITDA threshold. Given that there is a high likelihood that the realized EBITDA will be relatively close to the threshold, these investors are less likely to revise their expectations.

For investors whose current expectations are below the minimum EBITDA threshold, similar arguments apply.[12] They would perceive their expectations to be "too low" given that banks would intervene to prevent borrower performance from getting worse when unexpected deteriorations trigger covenant violations. Investors infer a low probability that borrower performance will fall below the covenant threshold also because borrowers seek to avoid violations and covenants are not violated in the majority of loans (about 70 percent; e.g., Dichev and Skinner

[11] Whether investors would increase or decrease their forecasts in the downside scenario depends on whether the forecasts are above or below the threshold. Thus, we do not center our argument on changing the downside forecasts. Nevertheless, for investors whose current expectations (i.e. base scenario forecasts) are well above the threshold, their downside scenario forecasts are likely to be above the threshold. In that case, we expect these investors, on average, to lower their downside scenario forecasts.

[12] Recall that banks set the thresholds at a prudent level so that when unexpected deteriorations trigger covenant violations, banks can intervene quickly to avoid the worsening of the situation. Because thresholds are at a prudent (i.e., not a very low) level, some investors may hold a current expectation that is below the thresholds.

2002).[13] Consequently, investors are likely to either increase their forecasts in the downside scenarios or lower the probabilities assigned to the downside occurring, both of which would move their estimates for the base scenario upward. Therefore, we expect investors whose current estimates are below the minimum EBITDA to revise their earnings estimates upward.

One challenge of investigating how investors use performance covenants is that their earnings estimates are not observable. Following prior literature (Richardson et al. 2010; Bradshaw 2011; deHaan et al. 2017), we study the reactions of analysts, whose earnings estimates are directly observable through their forecasts. In our context, we can investigate whether analysts with different outstanding forecasts before the loan event react *differently* to the *same* EBITDA covenant, and attribute any subsequent forecast revisions by the analysts to their use of information from the covenant. We expect analysts to revise (i) upward if their forecasts are below the threshold and (ii) downward if their forecasts are above it.[14] Our hypothesis is:

H1: Analysts are more likely to revise their forecasts *upward* if their expected EBITDA is *below* the minimum EBITDA threshold in the loan contract, and they are more likely to revise *downward* if their expected EBITDA is well *above* the minimum EBITDA.

One might argue that analysts might use covenant details for other purposes such as assessing a borrower's probability of covenant violation, not for forecasting purposes. Or, analysts may simply react to other analysts' forecasts around loan events and not to covenant information. Section 6 details these alternative arguments and explains why they are unlikely to provide an alternative explanation.

To better attribute the revisions shortly after a loan event to analysts' use of information in the performance covenants, we further exploit cross-sectional variations in both borrower and loan

[13] Consistent with this argument, we find that in about 70 percent of the loan events in our sample, the actual EBITDA in the first fiscal year end after the loan contract exceeds the minimum EBITDA inferred from the covenant threshold.
[14] Prior studies find that both analyst EBITDA estimates and the loan contract EBITDA tend to exclude transitory items (Bradshaw 2011; Dyreng et al. 2017), which makes the two constructs comparable.

covenant characteristics to identify situations where analysts are more likely to revise their forecasts according to H1. Specifically, we examine situations where analysts are more likely to find the information in performance covenants useful, (e.g., when borrower management provides limited information about its firm's future performance, when the covenant threshold is particularly informative about borrower performance changes in the future, etc.). Section 4.4 provide the details of these tests.

3.2 Are performance covenants useful to investors in predicting future borrower performance?

H1 is developed on the premise that EBITDA covenant thresholds contain information that helps analysts to predict future borrower performance. Analysts who are able and willing to process this additional piece of information can then use it in conjunction with their existing information to either confirm or improve their forecasts. If this argument is valid, we expect that EBITDA thresholds are incrementally useful in predicting future borrower performance after controlling for analysts' information set as summarized in their outstanding forecasts before the loan event. Our hypothesis is:

H2a: In predicting future borrower performance, EBITDA covenant thresholds contain incremental information beyond what is reflected in analysts' outstanding forecasts before the loan contract.

To further examine whether analysts use performance covenants in conjunction with their existing information to improve their forecasts, we conduct two additional tests. First, we study whether revising analysts experience any improvement in forecast accuracy. If analysts learn more about borrowers' future earnings from performance covenants, their revised forecasts should become more accurate. We test the following hypothesis:

H2b: Analyst forecasts revised after loans with performance covenants are more accurate compared to the outstanding forecasts before the loan contract.

Second, we compare the accuracy of forecasts by analysts who revised after the loan contract versus those who did not revise (i.e., those who must believe that the information in performance covenants is either not useful or already reflected in their outstanding forecasts). If performance covenants contain information that is useful for forecasting, analysts who revise their forecasts using the information should be more accurate than non-revising analysts.[15] Our hypothesis is:

H2c: Forecasts by analysts who revised are more accurate than forecasts by analysts who did not revise after loans with performance covenants.

Finally, we extend our analysis to examining investors in general and investigate changes in information asymmetry among investors around loan contracts with performance covenants. Prior studies suggest that increased disclosures can lead to reductions in information asymmetry (e.g., Leuz and Verrecchia 2000). Coller and Yohn (1997) find that information asymmetry decreases after management issue earnings guidance to help investors assess future performance. If performance covenants provide incrementally useful information and help investors predict future borrower performance, investors would likely assess lower information asymmetry following loan contracts with performance covenants. We therefore test the following hypothesis:

H2d: There is a decrease in information asymmetry following loans with performance covenants.

4 H1 – to what extent and how do analysts use the minimum EBITDA thresholds in performance covenants?

4.1 Research design

To test whether and how analysts use performance covenants when revising their forecasts, we examine revisions of EPS forecasts after a borrower issues a loan with performance covenants.

[15] Empirically, within non-revising analysts, we cannot distinguish between analysts who did not consider the performance covenant information versus those who did and used the information to confirm but not revise their forecasts. The presence of confirming analysts would make it harder for us to detect a difference in forecast accuracy between the revising and non-revising analysts.

We focus on revisions of EPS forecasts because they are the most common form of forecasts issued by analysts on I/B/E/S, whereas other forms of forecasts including EBITDA forecasts are much less common. We expect analysts to revise EPS forecasts in light of performance covenants because EBITDA is a necessary component to predict EPS.

To better attribute analyst forecast revisions to loan events, we focus on revisions made relatively shortly (i.e., two weeks) after the events. The two-week period takes into account the SEC requirement that firms disclose loan contracts within four business days after they enter into the contracts, and allows sufficient time for analysts to react to the information.[16] We further make sure that there are no concurrent information events (i.e., earnings announcements and management earnings guidance) in the two weeks before and two weeks after the loan event that can trigger analyst revisions.[17]

To design our test, we focus on how analysts facing the *same* loan contract but with different pre-loan earnings expectations revise their forecasts differently. For example, suppose that a loan contract specifies a minimum EBITDA covenant threshold of $3 million for firm A. Suppose further that firm A has three outstanding analyst forecasts of $2, $5, and $12 million. In this case, although all analysts receive the same information (e.g., good news that the borrower is able to obtain a loan to fund a project), we do not expect the same reaction from all analysts. Rather, we expect that the analyst with a forecast of $2 million is more likely to revise upward whereas the analyst with a forecast of $12 million is more likely to revise downward. One of the main advantages of this *within*-loan test is that our analysis holds all loan characteristics constant,

[16] Prior research has used a similar event window to study analysts' revisions following other events of interest. For example, Barron, Byard, and Kim (2002) use a 10-day event window when studying earnings announcements and Baginski and Hassell (1990) use a two-week event window when studying management earnings forecasts.
[17] In Section 4.3, we further exclude revisions made after loan events that are accompanied with concurrent filings of a 10-Q or 10-K, even though prior studies do not find significant market reactions to these filings (e.g., Stice 1991).

including loan terms, market reactions to the loan announcement, as well as other loan characteristics unobservable to us such as the timing of the loan contract disclosures or the propensity of firms to include performance covenants.

Operationally, we run the following linear probability model using analysts who revise their forecasts shortly after the borrowers they follow issue loans with performance covenants:

$$Revision\ Up_{i,j} = \alpha_0 + \alpha_1 Below\ Analyst_{i,j} + \ Loan\ event\ fixed\ effects + \varepsilon_{i,j} \quad [1]$$

where *i* refers to the loan event, and *j* to the analyst. *Revision Up* takes the value of one if the analyst who revises her forecast in the two weeks after the loan event revises upward, and zero otherwise. Our independent variable of interest, *Below Analyst*, takes the value of one if the EBITDA forecasted by the analyst prior to the loan event is below the EBITDA threshold in the loan contract, and zero otherwise. [18] H1 predicts that if analysts find the information in performance covenants incrementally useful and their forecasts are below the covenant threshold, they are likely to revise their forecasts upward. Comparatively, analysts whose forecasts are above the covenant threshold (i.e. Indicator variable *Below Analyst* = 0) are less likely to move their estimates upward. Thus, we expect coefficient α_1 to be positive.

To construct the EBITDA threshold in the loan contract, we use the value stated in the minimum EBITDA covenant, when available. For contracts without such a covenant, we compute the minimum EBITDA threshold by using the "Max Debt to EBITDA" covenant and loan amount in the contract, as well as firms' financial information. For example, for a $200 million loan contract with a Max Debt to EBITDA covenant of 4 and the firm's outstanding debt is $1 billion,

[18] Specifically, we compare the analyst's annual forecast with the covenant threshold to the variable. The use of annual forecasts in the comparisons is akin to the approach in Demerjian and Owens (2016), who compare annualized earnings numbers with covenant thresholds in DealScan when assessing covenant slack. Moreover, the choice of studying annual forecasts is common among the forecasting literature (e.g., Baginski and Hassell 1990; Bamber, Barron, and Stober 1999; Barron, Byard, and Kim 2002; Gleason and Lee 2003).

the minimum EBITDA threshold would be equal to \$300 million (= 1,000 + 200 / 4).[19]

For analysts' EBITDA forecasts, we infer them from their EPS forecasts.[20] We first multiply the forecasted EPS by the number of outstanding shares to compute the forecasted earnings. Then, we add back interest, taxes and depreciation and amortization from the most recent financial statement available before the loan event to approximate the forecasted EBITDA. Our calculation follows our reading of loan contracts, which often define EBITDA the way we just described.[21]

Our objective is to examine whether analysts' responses to the same loan contract differ depending on their expectations of EBITDA prior to the loan event. Hence, we include loan event fixed effects to ensure that comparisons between analysts are made within the same loan. Furthermore, given that all revising analysts for each loan event face the same loan contract details for the same borrower, it is unnecessary to include controls for loan and borrower characteristics.

4.2 Sample selection

To form our sample of loan events, we start with all available loan events in Dealscan that we can match with CRSP and Compustat over the period 1995 to 2012 (21,811 loan events).[22] We exclude observations that contain multiple loan events for the same firm on the same day to ensure

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[19] We obtain similar results when we use only contracts with minimum EBITDA covenants, although our sample is significantly smaller.

[20] Some analysts provide EBITDA forecasts but the data availability is limited. Thus, our tests do not use these forecasts to define *Below Analyst*. For a small sample where analysts issued both EBITDA and EPS forecasts, we find a high correlation (92 percent) between the EBITDA forecasts and the EBITDA estimates we impute using the EPS forecasts.

[21] For example, in the credit agreement between Aceto Corp. and JPMorgan Chase Bank dated as of December 31, 2010 (p.10), EBITDA for any period is defined as "Consolidated Net Income (or consolidated net loss) for such period, plus the sum, without duplication, of (a) Consolidated Interest Expense, (b) depreciation and amortization expenses or charges, (c) all income taxes [...] in accordance with Generally Accepted Accounting Principles." See https://www.sec.gov/Archives/edgar/data/2034/000118811211000025/ex10-1.htm

[22] Our analysis stops in 2012 since this is the last available year in the DealScan-Compustat linking tables provided by Chava and Roberts (2008). Similarly, we start our sample in 1995 since DealScan coverage of loan elements in prior years is scarce (e.g., Demerjian 2011).

we can attribute analyst revisions to the details of a particular loan contract (20,789 loan events left). We also exclude (i) loan events with concurrent information events (i.e., earnings announcements and management earnings guidance) in the two weeks before and after the loan event, and (ii) loan events where we cannot find a positive match with I/B/E/S where we obtain analyst data (8,363 loan events left). Finally, we require that the loan events have contract details available from DealScan so that we can obtain the minimum EBITDA covenant threshold (4,764 loan events left).

For each loan event, we identify all analysts who revise their forecasts for the borrower within two weeks after the event. First, we identify analysts with an outstanding annual earnings forecast prior to the loan event, where outstanding forecasts are the ones issued no more than 180 days prior to the loan event (e.g., Jegadeesh and Kim 2010; Lee and Lo 2016).[23] Then, for each analyst with an outstanding earnings forecast, we determine if the analyst has a revised forecast for the same fiscal period in the two weeks after the event. Overall, we are able to find revising analysts for 1,878 loan events (about 39 percent of the 4,764 available loan events),[24] resulting in 4,108 loan-analyst observations.

To help implement the within-loan tests described in Section 4.1, which compare across revising analysts of the same loan event, we retain only those analysts for whom we can find at least one other revising analyst to compare with. Our final sample consists of 2,833 loan-analyst observations, and there are 818 loan events and 1,700 unique analysts underlying this sample. The

[23] Our analyses focus on how analysts use covenant information to revise their near-term annual forecasts because analysts seldom report long-term forecasts (e.g., among all available forecasts from I/B/E/S for our sample borrowers before the loan events, just about one percent of the forecasts are for four years ahead, i.e., the horizon of a typical loan contract). This data issue also prevents us from studying loans with changing thresholds over the loan duration (Li et al. 2016) and assess whether analysts use the thresholds towards the end of the loan to revise their forecasts about the borrower's long-term future.

[24] As a comparison, prior literature indicates that for significant information events such as management earnings guidance, about 50 percent of the events receive a forecast revision by analysts in the 2 weeks after the event (for example, see Jennings 1987, Table 2 Panel C for the weeks T=0 and T=1).

sample construction details are summarized in Table 1 Panel A. Table 1 Panel B provides descriptive statistics. The majority (i.e., 70 percent) of the sample analysts have pre-loan earnings forecasts that are above the threshold indicated in the covenant (variable *Below Analyst* = 0.30). This is expected because the threshold captures the minimum value of borrower performance that banks accept, and it is likely that many analysts have an expectation greater than that value. At the same time, 30 percent of the analysts have pre-loan earnings forecasts that are below the minimum threshold. The non-trivial percentage is consistent with the idea that the threshold is set at a reasonably prudent (i.e., not very low) level such that when the covenant is violated, there is still room for banks to intervene to prevent losses. Within revising analysts, about half of them revise upward after the loan event (i.e., mean value of *Revision Up* = 0.51). It appears that analysts do not necessarily interpret loan events as conveying positive news, and that the direction of revision could be associated with an analyst's pre-loan expectation relative to the covenant threshold. For completeness, Panel B also provide basic loan and borrower characteristics at the loan-analyst level. In untabulated tests, we assess whether the borrowers in the 818 loan events included in our sample are different from other borrowers available from DealScan (i.e., those in the 8,363 loan events reported in Table 1 Panel A). We find that our sample borrowers are generally larger firms, presumably because we focus on firms with greater analyst following. However, earnings performance as captured by firm ROA, and the volatilities of earnings and operating cash flows are largely similar between the two groups of borrowers. These results help reduce concerns that our sample is unique along the dimensions of firm performance predictability and forecasting.

4.3 Results of H1

Table 2 reports the main results. We cluster standard errors at the analyst level, as forecasts made by the same analyst can be correlated with each other. In column (1) of Panel A, the coefficient on *Below Analyst* is positive and significant (coefficient = 0.192, t-statistic = 2.60), consistent with H1 that, facing the same loan, analysts whose EBITDA forecasts fall below the contracted minimum threshold are 38 percent more likely to revise upward than analysts whose forecasts are above the threshold.[25]

To ensure that the results are robust to an alternative fixed effect structure, we use industry and year fixed effects instead of loan event fixed effects. Unlike the previous test where we compare across analysts within the same loan event, this alternative specification compares across analysts from different loan events and assesses whether *Below Analysts* are more likely to revise upward upon observing covenant thresholds than other analysts. To control for the effects of differences in loan and borrower characteristics on analyst revisions, we add to this specification common characteristics such as firm size, firm performance, stock price movements around the loan event, and important loan characteristics (e.g., loan size and spread). The results are in column (2) of Panel A. The coefficient on *Below Analyst* remains positive and significant (coefficient = 0.052, t-statistic = 1.99), and the inferences remain unchanged.

We conduct additional tests assessing whether analysts whose forecasts are below ("Below Analysts") and those whose forecasts are above ("Above Analysts") the covenant threshold revise their forecasts according to H1. We keep *Revision Up* as the dependent variable and create two independent variables to separately identify analysts belonging to the Below or Above Analyst group. Specifically, we compute the relative distance between the analysts' EBITDA forecasts and the loan contract EBITDA. The first variable (*Abs distance Below Analyst*) takes the absolute value of the distance if it is negative and zero otherwise. This variable captures how far the analyst forecast is below the covenant threshold for Below Analysts. The second variable (*Abs distance*

[25] From Table 1, Panel B, 51 percent of analysts in our sample revise upward. Therefore, the coefficient in column (1) shows an increase in probability of upward revision of 38 percent (i.e., 0.192 / 0.51).

Above Analyst) is equal to the distance if it is positive and zero otherwise. This variable captures how far the analyst's forecast is above the covenant threshold for Above Analysts. On average, the EBITDA forecasts by Above Analysts are 4.2 times of the minimum EBITDA in the loan contract, which suggest relatively optimistic forecasts by these analysts given that most of the borrowers in our sample report an actual EBITDA in the first year after the loan event that is at most 2.7 times of the EBITDA threshold (see footnote 10). For the Below Analysts, their forecasts are about 35 percent lower than the minimum EBITDA in the loan.

Table 2 Panel B reports the results. The coefficient on *Abs distance Below Analyst* is positive and significant, suggesting that analysts are more likely to revise upward when their EBITDA forecast is further below the covenant threshold. The coefficient on *Abs distance Above Analyst*, however, is negative and significant, suggesting that analysts are less likely to revise upward when their forecast is further above the covenant threshold. Since we restrict our analyses to analysts who revise their forecasts, this also suggests that the analysts whose EBITDA forecasts are further above the loan contract threshold are more likely to revise *downward*.

We also note that the absolute size of the coefficient on *Abs distance Below Analyst* is about 20 times smaller than that of the coefficient on *Abs distance Above Analyst* (untabulated F-test=23.66). This indicates that a relatively small difference below the covenant EBITDA threshold is enough to trigger upward revisions but a much larger difference above the threshold is needed to trigger downward revisions with the same probability. In other words, analysts need to be *well* above the EBITDA threshold before revising downward. Therefore, Above Analysts appear to understand that the EBITDA threshold is set prudently by the bank and do not simply adjust their expectations downward when their forecasts are not too much above the threshold. However, when their expectations are well above the threshold, they seem to understand that covenants are generally set tightly and use the incremental information to revise their estimates downward.

We conduct an untabulated test to confirm that our inferences are not confounded by the concurrent filings of Form 10-Q or 10-K around loan events. Using filing dates from SEC EDGAR, we find that there are 10-K or 10-Q filings in the two weeks before or two weeks after the loan event for about 19 percent of our sample. While prior research does not find significant market reactions to these filings (Stice 1991; Li and Ramesh 2009), we exclude these observations from the test and find that our inferences remain unchanged.

Overall, the results in Table 2 are consistent with H1 that analysts move their forecasts towards the EBITDA threshold, providing direct insights into analyst use of the incremental information in performance covenants to guide their predictions of future borrower performance.

4.4 Cross-sectional tests

To strengthen inferences, we examine cross-sectional variations in borrower and loan covenant characteristics to identify situations where analysts are more likely to find the covenant information useful. If we find that the results are mainly concentrated in these situations, we can then better attribute the revisions shortly after a loan event to analysts' use of performance covenants. Our first set of tests examines how analyst revisions after a loan event vary with borrower characteristics.

We expect analysts to have stronger incentives to use covenant details as alternative information sources when there is scant indication from borrower management about its firm's future. To test this, we split the sample based on whether the borrower issued management earnings guidance and repeat the test in Table 2 Panel B. Table 3 Panel A columns (1) and (2) report the findings. Consistent with our expectation, we find that our main results are concentrated in the subsample where analysts did not receive guidance from management.

We also expect that analysts are more likely to seek multiple sources of information when there is greater uncertainty about a borrower's future performance. Thus, we split the sample based on the median value of borrower stock return volatility measured before the loan event. For borrowers with relatively high return volatility, we expect uncertainty about their future is likely higher and analysts are more likely to use performance covenants as an additional source of information to guide their forecasts. Our results in Table 3 Panel A columns (3) and (4) show that the results are mainly detected within the subsample where borrower return volatility is high.

We also split the sample based on whether the borrower's credit rating is investment grade or not. As discussed earlier, analysts likely perceive banks to be better at assessing downside risks and are likely to use EBITDA covenants to assess the borrower's downside scenarios. Analysts likely find covenant details particularly useful for borrowers with higher default risk because these borrowers attract banks' scrutiny and tight covenants. Thus, we expect that in this subsample of borrowers, there is a greater use of covenant information by analysts to help predict borrower future performance. Consistent with this expectation, in Table 3 Panel A Columns (5) and (6), we see that Above Analysts revise their forecasts downward only in the subsample of borrowers with high default risk.

Second, with respect to loan covenant characteristics that affect how analysts use threshold information to help revise their forecasts, we consider the impact of where the threshold is set relative to the borrower's current performance. Prior studies find that thresholds set close to the covenant variable computed using recent financials are positively associated with future improvements in the covenant variable (Demiroglu and James, 2010). This is because borrowers are unlikely to agree to thresholds that are set close to their current performance if they do not expect their future performance to improve so that they will readily comply with the covenants.[26] We expect that in this situation, Above Analysts have weaker incentives to revise downward even if their estimates are well above the threshold. To begin with, Above Analysts are relatively more optimistic about the borrower's future performance. If they see a threshold that is set close to the borrower's current performance and expect future borrower improvements, these analysts would have reasons to not revise downward. In contrast, Above Analysts have stronger incentives to revise downward to come closer to the threshold if they see the threshold is set relatively far from the borrower's current performance, which prior research finds to be associated with deteriorations in future borrower performance (Demiroglu and James 2010). For Below Analysts, because of the costly consequences of covenant violations for borrowers, we expect these analysts to perceive a low likelihood that future borrower performance would fall below the covenant threshold regardless of whether the threshold is set close or far from the borrower's current performance.

Table 3 Panel B reports the results. Consistent with our expectations, we find significant results for Above Analysts' use of covenant details to revise downward mainly when the covenant threshold is set relatively far from the borrower's current performance, whereas Below Analysts' use of covenant details to revise upward does not seem to depend on where the threshold is set.

5 Do performance covenants provide useful information for predicting future borrower performance?

This section discusses the analyses in relation to H2a – H2d. Our interest is to understand whether performance covenants provide investors with incremental information, which improves

[26] Alternatively, Demiroglu and James (2010) suggest that thresholds set close to the covenant variable based on recent financials can discipline borrowers to improve the covenant variable to avoid covenant violations in the future. Regardless of the explanations, we expect that thresholds set close to the borrower's current performance are associated with improvements in future borrower performance.

their forecasts of future borrower performance and lower information asymmetries.

5.1 *H2a – evidence from incremental usefulness of performance covenant thresholds in predicting future borrower performance*

To test whether performance covenants contain *incremental* information that helps analysts predict borrower performance, we assess whether the covenant threshold is associated with the realized value of future borrower performance, after controlling for what analysts already knew before the loan event. Operationally, we estimate the following regression model:

$$Actual\ EPS_{i,j} = \alpha_0 + \alpha_1 Loan\ contract\ Min\ EBITDA\ per\ share_i + \alpha_2 Outstanding\ EPS\ forecast_{i,j} + fixed\ effects + \varepsilon_{i,j} \quad [2]$$

where *i* refers to the loan event, and *j* to the analyst. *Actual EPS* is realized borrower earnings per share for which the analyst forecasted before the loan event. *Loan contract Min EBITDA per share* is the contracted EBITDA threshold divided by the number of outstanding shares. We scale the EBITDA threshold to make it on a per-share basis to be consistent with *Actual EPS*. We control for each analyst's information set before the loan event using *Outstanding EPS forecast* which is the analyst's outstanding estimate before the loan event. *Fixed effects* is a vector of industry and year fixed effects and we include them as general controls for industry and time trends.[27] H2a predicts that the coefficient on *Loan contract Min EBITDA per share* (α_1) is significant.

We start with the sample used in Tables 2 and 3. After excluding observations without available data for *ActualEPS* from I/B/E/S, we are left with 2,687 observations. Table 4 reports the results. We cluster standard errors at the firm level because EPS values for the same firm can

[27] We cannot use loan event fixed effects in the this test because our variables of interest, *ActualEPS* and *Loan contract Min EBITDA per share*, are constant across analyst observations within the same loan event. Using loan event fixed effects would not leave us any variation in these variables and prevent us from estimating Eq. [2].

be correlated with each other.[28] Column (1) shows that *Loan contract Min EBITDA per share* is positively associated with *Actual EPS* (t-stat = 2.18) suggesting that the EBITDA threshold contains information beyond what analysts knew about future borrower performance before the loan event. To the extent that other terms in the loan contract are determined jointly with the EBITDA threshold and these terms provide incremental information in forecasting future earnings, we control for the loan characteristics included in Table 2. Table 4 column (2) shows that our results are robust to these controls.

To make sure that the incremental information comes from the EBITDA threshold and not the number of outstanding shares (i.e., the scaler), we repeat our estimation including both the EBITDA threshold and the number of shares as separate regressors, after controlling for *Estimated EPS*. The results (untabulated) continue to show a significant positive relation between EBITDA threshold and *Actual EPS* (t-stat 3.41).

5.2 H2b – evidence from changes in analyst forecast accuracy before and after loan events

If the performance covenant threshold is incrementally useful in predicting future borrower performance, then the revised forecasts triggered by the threshold information will likely be more accurate than the outstanding forecast made by the same analyst prior to the loan event. To test this hypothesis, we estimate the following OLS regression:

$$Difference\ accuracy_{i,j} = \alpha_0 + Loan\ event\ fixed\ effects + \varepsilon_{i,j} \qquad [3]$$

where *i* refers to the loan event, and *j* to the analyst. *Difference accuracy* is the accuracy of the revised forecast minus the accuracy of the outstanding forecast made by the same analyst before the loan contract. We define forecast accuracy as forecast error multiplied by –1, where forecast

[28] This clustering structure is different from the previous tests where we study analyst revisions and cluster standard errors at the analyst level. When we cluster at the analyst level in the current test instead of at the firm level, we find higher significance among the coefficients of interest, consistent with the idea that clustering at the analyst level likely overstates the significance of the estimates as it does not control for within-firm autocorrelation.

error is defined as the absolute value of the difference between the analyst's estimate and the actual realized borrower earnings, divided by the borrower's stock price measured two days before the forecast date. A higher resulting number corresponds to greater forecast accuracy and thus, a higher value of *Difference accuracy* corresponds to an improvement in accuracy for the revised forecast.

By examining how an analyst's forecast accuracy improves over her *own* forecast immediately before the loan event, we mitigate the effects of analyst-specific characteristics (e.g., ability) on forecast accuracy. To minimize the effects of loan and borrower characteristics on forecast accuracy (e.g., certain loans or borrowers might provide additional information beyond covenant thresholds that can help analysts to improve their forecasts), we include loan event fixed effects so that the specific effects unique to a particular loan for a particular borrower on analyst forecast accuracy are mitigated.[29] Our variable of interest is the intercept, i.e., coefficient α_0, which captures the average change in the accuracy of each analyst's forecast before and after a loan event. We expect α_0 to be positive, consistent with H2b that the revised forecast after the loan event is more accurate than the forecast before.

We begin with the sample of 2,833 revisions used in Table 2. After excluding observations without data to compute *Difference accuracy,* we are left with 2,472 observations. Table 5 reports the results. We cluster standard errors at the analyst level, as accuracy of forecasts by the same analyst can be correlated with each other. Consistent with performance covenants providing analysts with useful information, Column (1) shows that the coefficient on α_0 is positive and significant (coefficient = 0.002, t-statistic = 4.15), indicating an improvement in analyst forecast

[29] We do not include controls for loan and borrower characteristics in this test because these controls cannot be estimated with the inclusion of loan fixed effects. To see this, suppose two analysts revise their forecasts after loan event A. Both analysts face the same loan contract for the same borrower. Because there are no variations in loan and borrower characteristics across the analyst observations within each loan event, the inclusion of loan event fixed effects makes the controls of these characteristics not estimable.

accuracy right after the loan event for revising analysts. Given that the average absolute value of forecast accuracy pre-loan in our sample is –0.040 (untabulated), the improvement in forecast accuracy of about 5 percent (= 0.002/0.040) is relevant.

One potential concern is that as time passed from when the outstanding forecast was issued to when the revised forecast occurs, analysts gathered additional information that is not sourced from the performance covenant and that allows them to improve forecast accuracy. This concern is somewhat mitigated since we have already excluded loan events with concurrent confounding information events (e.g., management earnings forecasts) from our analyses (See Section 4.1). Nevertheless, we conduct additional subsample analyses to alleviate this concern. Columns (2), (3), and (4) present results for subsamples where the revised forecast is less than 90, 60, and 15 days apart from the forecast before the loan event respectively. The results remain similar, suggesting that the time between the outstanding forecast and its revision does not affect our inferences.

5.3 H2c – evidence from difference in forecast accuracy between revising and non-revising analysts

To test H2c, we compare the accuracy of forecasts by analysts who revised versus those who did not revise after the loan event. For revising analysts, we use their revised forecasts right after the loan event while for non-revising analysts, we use their outstanding forecasts prior to the loan event. Since non-revising analysts did not update their forecasts after the loan event, they must believe that the information in performance covenants is either not useful or already reflected in their outstanding forecasts. Thus, we can treat non-revising analysts' outstanding forecasts prior to the loan event as their forecasts right after the loan event and compare them with the forecasts by revising analysts. We estimate the following regression:

$$Forecast\ accuracy_{i,j} = \alpha_0 + \alpha_1 Revising\ analysts_{i,j} + Loan\ event\ fixed\ effects + \varepsilon_{i,j} \qquad [4]$$

where *i* refers to the loan event, and *j* to the analyst. *Forecast Accuracy* is defined as in Section 5.2. A higher resulting number corresponds to greater forecast accuracy. We include loan event fixed effects to ensure we estimate how more accurate the forecasts by revising analysts are compared to the forecasts by non-revising analysts for the *same* loan event. Since revising and non-revising analysts for the same loan event face the same loan contract for the same borrower, it becomes unnecessary for us to include controls for loan and borrower characteristics in the test.

Our variable of interest, *Revising analysts*, is an indicator variable that takes the value of one if the forecast is made by revising analysts, and zero otherwise. As predicted in H2c, if performance covenants contain useful information for forecasting future borrower performance, analysts who revise their forecasts using the information should be more accurate than non-revising analysts. Therefore, we expect the coefficient on *Revising analysts* to be positive.

To construct the sample for this test, we add to the sample of revising analysts in previous tests all non-revising analysts who had an outstanding forecast for the same borrower prior to the loan event, resulting in 10,167 observations. Table 6 presents the results. We cluster standard errors at the analyst level. Consistent with H2c, column (1) shows that the coefficient on *Revising Analyst* is positive and significant (coefficient = 0.012, t-statistic = 7.81). In terms of economic significance, given that the average forecast accuracy for non-revising analysts is –0.045 (untabulated), revising analysts have a relatively higher forecast accuracy than non-revising analysts of about 27 percent (= 0.012/0.045).

Columns (2) to (4) show that our results remain similar if we compare forecasts by revising analysts to only those outstanding forecasts by non-revising analysts that were issued shortly (i.e., less than 90, 60, or 15 days) before the loan event. Thus, learning over time by analysts unrelated to the loan event is unlikely to explain the results.

5.4 H2d – evidence from changes in borrower information asymmetry upon loan events

H2d tests how information asymmetry changes shortly around loans with performance covenants. We use bid-ask spread as our information asymmetry measure. Coller and Yohn (1997) find that information asymmetry, as proxied by bid-ask spread, decreases after firms provide management earnings forecasts to help investors predict future firm performance. If performance covenants provide information that is incrementally useful for investors to estimate future borrower performance, we expect that bid-ask spreads of borrower stocks would decrease after the loan event. We estimate the following regression:

$$Information\ asymmetry_{k,t} = \alpha_0 + \alpha_1 After\ PCov\ loan_{k,t} + \beta_1 Mkt\ value_{k,t} + \beta_2 Share\ turnover_{k,t} + Loan\ event\ fixed\ effects + \varepsilon_{k,t} \quad [5]$$

where *k* refers to the borrower and *t* refers to the day in the test window (i.e., two weeks before until the two weeks after the loan event). *Information asymmetry* is the difference between the bid and ask closing prices of the borrower stock divided by the midpoint, and is computed for each day in the test window. To correct for measure skewness, we take the natural logarithm of *Information asymmetry*. *After PCov loan* is an indicator variable equal to one for days in the two weeks after the loan event, and zero for days before the event. The coefficient on *After PCov loan*, α_1, captures the change in information asymmetry in the two weeks after the loan event with performance covenants from the two weeks before the loan event. H2d predicts a negative α_1, indicating that borrowers experience a decrease in information asymmetry.

Since we want to examine how information asymmetry changes shortly around the *same* loan event, we include loan event fixed effects to focus on within-loan-event changes. The fixed effects and the use of a short test window also mitigate the need for us to include firm-level controls such as governance structure, and information environment and financial reporting quality, which can affect information asymmetries but which hardly change shortly around the same loan event.

We include relevant controls used by previous studies on information asymmetry such as firm market value and shares turnover that change daily and therefore would not be captured by our fixed effect structure (e.g., Christensen, Hail, and Leuz 2016). Similar to the earlier tests, this test excludes loan events with concurrent information events during the test window to prevent any confounding effects.

To strengthen our inferences that the reduction in information asymmetry is attributable to performance covenants, rather than to other changes around loan events such as stock returns around loan announcements as reported in prior studies (e.g., James 1987), we further conduct a difference-in-differences analysis using the following model:

$$Information\ asymmetry_{k,t} = \alpha_0 + \alpha_1 After\ PCov\ loan_{k,t} + \alpha_2\ After\ loan\ with\ no\ PCov_{k,t} + \beta_1 Mkt\ value_{k,t} + \beta_2 Share\ turnover_{k,t} + \ Loan\ event\ fixed\ effects + \varepsilon_{k,t} \quad [6]$$

where *After loan with no PCov* is an indicator variable equal to one for days in the two-week window after loans with no performance covenants, and zero for days before the loan. Essentially, this analysis uses loan contracts with no performance covenants as a control group. If changes around loan contracts are mechanically related to information asymmetry, such changes would likely affect all loans including those that do not have performance covenants. Therefore, we use the differential change for loans with performance covenants to assess the change in information asymmetry due to investors' use of the covenant information. Such a differential change is captured by the difference between α_1 and α_2, which H2d predicts to be negative.

The initial sample starts with 21,811 from Table 1, Panel A. After restricting our sample to loans with performance covenants and firms with the necessary information to compute *Information asymmetry*, we are left with 8,806 loan events, which provides us 165,513 firm-day observations for our baseline test described in Equation (5). Table 7 Column (1) reports the results. We cluster standard errors at the firm level, since bid-ask spreads for the same firm across different days might be correlated with each other. The table shows that the coefficient on *After PCov loan* is negative and significant (coefficient = -0.011, t-statistic = -2.82), consistent with H2d that there is a reduction in information asymmetry after loans that have performance covenants.

To perform the difference-in-differences test described in Equation (6), we add to the sample in Column (1) all loans on DealScan that do not report performance covenants. Column (2) reports the results. The coefficient on *After PCov loan* is still negative in this specification and significant, whereas the coefficient on *After loan with no PCov* is less negative. Furthermore, F-test confirms that the coefficient on *After PCov loan* is more negative, consistent with H2d that there is a reduction in information asymmetry after loans with performance covenants.

We conduct robustness checks, including (i) using an alternative proxy for information asymmetry, (ii) perform additional tests that mitigate concerns from loan announcement returns, and (iii) exclude loan events with 10Q or 10K filings during the test window.

First, similar to prior studies (e.g., Balakrishnan et al. 2014; Christensen, Hail, and Leuz 2016), we use stock illiquidity as an alternative measure of information asymmetry. The computation of stock illiquidity follows Amihud's (2002) measure, which captures the price impact for a given level of trading volume and which prior studies show to decrease when there are more information disclosures. We expect that borrower stock illiquidity decreases after loans with performance covenants when investors can use the information in the thresholds to help predict borrower performance. Table 7 Columns (3) and (4) report the results. Similar to Columns (1) and (2), we find that the coefficient on *After PCov loan* remains negative and significant.

As discussed before, our difference-in-differences tests comparing how information asymmetry changes differ between different types of loans mitigate concerns that loan announcement returns might confound our results. To further address this issue, we conduct two untabulated tests. First, we follow prior research and use DealScan loan active date as loan announcement date (Gande and Saunders 2012). We exclude the three-day loan announcement period from the test window and re-estimate the change in bid-ask spread. Our inferences remain unchanged. Second, we split loan events with performance covenants into two types; loans with higher than median loan announcement returns, and loans with lower than median loan announcement returns. We find similar results (i.e., a significant decrease in information asymmetry) even after dropping the loans with the biggest (i.e., among median) loan announcement reactions, suggesting that our results are not driven by the loan news but rather the presence and content of the performance covenant. Overall, it does not seem that loan announcement returns reported in prior studies confound our inferences. Finally, while prior research does not find significant market reactions to the filings of Form 10Q or 10K (Stice 1991; Li and Ramesh 2009), we repeat our tests after removing loan events with these filings during the test period, and our inferences remain unchanged.

Taken together, the findings in section 5 help bolster the underlying premise that the information in performance covenants is useful to investors to form their estimates of future borrower performance. The results suggest that the information in performance covenant threshold is incrementally useful in predicting future performance. Moreover, analysts and investors can use the information in performance covenants to guide their estimates of future borrower performance, leading to improved predictions of borrower performance and lower information asymmetry.

6 Potential alternative explanations for analyst revisions predicted in H1

6.1 Using performance covenants to assess the probability of covenant violation

We consider the possibility that analysts use performance covenants not for forecasting purposes but to determine a borrower’s probability of covenant violation. If so, analysts would compare the minimum EBITDA threshold with their earnings expectations prior to the loan event to determine if the borrower is likely to violate the covenant. When analysts' earnings expectations are *below* the covenant threshold, they would determine that the borrower is likely to violate the covenant and revise their forecasts *downward* because violations can negatively affect borrower earnings. Comparatively, when analysts' expectations are *above* the covenant threshold, they would determine that the borrower is likely to comply with the covenant and have *no reason to revise* their expectations. Our results do not provide support for these predictions.

6.2 Analyst reaction to other analysts' forecasts

It is possible that some analyst revisions after a loan event are not directly triggered by the information in the covenant threshold per se, but by initial revised forecasts made by other analysts after the same loan event. To the extent that the initial forecast revisions are triggered by information in the covenant threshold, we can still attribute the subsequent herding forecast revisions to analyst use of performance covenants.

Another alternative explanation for our findings is that analysts are converging towards a consensus forecast and not necessarily revising their forecasts using the covenant information. However, it is unclear why analysts would wait until a loan event to converge to consensus when they had all the opportunities to converge to consensus updates before the loan event. Specifically, consensus continuously updates when there is a forecast issued by an analyst. For revising analysts especially those who are among the first to issue the pre-loan event forecasts (e.g., those issuing a forecast 90 days before the loan event and revising the forecast only right after the event), they could have revised their forecasts every time when there was a subsequent forecast by another analyst, and needed not wait until the loan event. However, because we observe that these analysts did not make their revisions until right after the loan event, we believe that they are using the information in performance covenants, not consensus, to improve their forecasts. To confirm that our results hold when we study only these analysts, for each loan event, we retain only the revising analysts who issued the first two pre-loan event forecasts and drop all other revising analysts whose pre-loan event forecasts came subsequently. We then repeat the main tests for H1 reported in Table 2 Panel B and find the results remain robust.

7 Conclusion

We examine whether and how equity investors use performance covenants in loan contracts to help forecast future borrower performance. Performance covenants act as “trip wires”, by allowing banks to monitor borrowers and to take corrective actions when needed (e.g., Christensen and Nikolaev 2012). When setting performance covenant thresholds, borrowers and banks agree on their expectations of acceptable future borrower performance. We argue that investors outside of loan contracts can use performance covenant thresholds to help revise their expectations of future borrower performance.

We use equity analysts to proxy for investor behavior and examine how analysts use performance covenants to help revise their forecasts. We find that analysts tend to revise their forecasts upward when the forecasts fall below the performance covenant threshold in the loan contract, and downward when the forecasts are well above the covenant threshold. We also find that performance covenants provide incremental information in predicting future borrower performance, and the revised forecasts triggered by performance covenants are more accurate than (i) the forecasts made by the same analyst right before the loan event or (ii) the outstanding forecasts made by other analysts who do not revise after the loan event. Moreover, we find a decrease in information asymmetry after loan contracts with performance covenants. Overall, our findings highlight a new and perhaps unintended use of performance covenants by investors for forecasting purposes, which helps broadens our understanding of the nature of accounting numbers in debt contracts.

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Appendix

Abs 3-day returns $_{around\ loan}$	Absolute value of cumulative buy-and-hold abnormal returns in the three-day window around the loan entry date. We compute abnormal returns by subtracting the value-weighted market returns from the stock returns.
Abs distance Above Analyst	The absolute value of the distance between the analyst's EBITDA forecast and the loan contract EBITDA if the analyst's EBITDA forecast exceeds the loan contract EBITDA, and zero otherwise.
Abs distance Below Analyst	The absolute value of the distance between the analyst's EBITDA forecast and the loan contract EBITDA if the analyst's EBITDA forecast is below the loan contract EBITDA, and zero otherwise.
Actual EPS	Firm actual EPS corresponding to the analysts' forecasts, as reported by I/B/E/S.
After loan with no PCov	An indicator variable that takes the value of one in the two weeks after the loan event if the loan does not include a performance covenant, and zero otherwise.
After PCov loan	An indicator variable that takes the value of one in the two weeks after the loan event if the loan includes a performance covenant, and zero otherwise. Performance covenants include the level of EBITDA, debt-to-EBITDA ratio, senior debt to EBITDA ratio, cash interest and debt service coverage ratios, interest coverage ratio (Christensen and Nikolaev 2012).
Abs returns $_{before\ loan}$	Absolute cumulative stock returns from 14 calendar days before the loan entry date to 1 day before.
Below Analyst	An indicator variable that takes the value of one if the EBITDA forecasted by the analyst before the loan event is lower than the minimum EBITDA threshold in the loan contract.
Bid-Ask Spread	The natural logarithm of daily quoted percentage bid ask-spread, computed as the difference between the two closing prices divided by the midpoint.
Collateral	An indicator variable that equals to one if the loan contract contains a secured facility, and zero otherwise.
Difference accuracy	The difference in the accuracy of analyst's post-loan contract forecast and pre-loan outstanding forecast.
Dividend restriction	An indicator variable that takes the value of one if the loan contains a dividend restriction clause and zero otherwise.
Firm leverage	Sum of debt in current liabilities and long-term debt, divided by total assets.
Firm EBITDA over total assets	Operating income before depreciation, divided by total assets.
Firm size	The natural logarithm of the firm total assets.
Forecast accuracy	The absolute value of the difference between the analyst's estimate and the realized firm earnings, divided by the firm's stock price measured two days before the forecast date. We multiply this number by -1 so that a higher resulting number corresponds to greater forecast accuracy.
Loan contract Min EBITDA per share	Minimum EBITDA threshold reported in the loan covenant divided by the firm number of outstanding shares. For the computation of the Minimum EBITDA threshold, see Section 4.1.
Loan maturity (months)	The package maturity, measured in months.

Loan size (ln)	The natural logarithm of the package dollar amount.
Loan spread	The weighted average spread of each facility within each loan package, using each loan facility dollar amount as weights.
Market to book	Sum of market value of equity and the book value of debt, divided by total assets.
Mkt value (USD Mil)	Market value of the borrower, computed as number of shares outstanding times the share price, in USD million.
Nr performance pricing provisions	The sum of the performance pricing restrictions in the facilities included in each loan contract.
Nr sweep provisions	The sum of the sweep provisions in the facilities included in each loan contract.
Outstanding EPS forecasts	Analyst EPS forecasts, as reported by I/B/E/S.
Revising analysts	An indicator variable that takes the value of one if the forecast is made by revising analysts, and zero otherwise.
Revision Up	An indicator variable that equals to one if the analyst revises her forecast upward, and zero otherwise.
SD returns $_{before\ loan}$	Standard deviation of stock returns from 14 calendar days before the loan entry date to 1 day before.
Share turnover (000)	Daily share turnover, in thousands, computed as the dollar value of the number of shares traded divided by the dollar value of the number of shares outstanding.
Stock Illiquidity	Amihud (2002)'s stock illiquidity measure, computed as: $\textit{Stock Illiquidity}_{i,d} = \ln\left(1 + \frac{\lvert R_{i,d}\rvert}{DVol_{i,d}} \times 10^7\right)$ where $R_{i,d}$ is the daily return for firm *i* on day *d*. $DVol_{i,d}$ is the daily dollar volume for firm *i* on day *d*.

Table 1: Sample construction and descriptive statistics

This table presents descriptive statistics for our sample, which includes 2,833 loan-analyst observations associated with 818 loan events from 1995 through 2012. Panel A reports the sample construction, and Panels B reports descriptive statistics.

Panel A: Sample selection

	# of loan events	**# of loan-analyst obs.**
Loan packages with information on covenants in DealScan and available matches with Compustat	21,811	n.a.
Sample without multiple loan packages on the same day	20,789	n.a.
Sample with (i) no earnings releases and management guidance in the two weeks before or two weeks after the loan event and (ii) available matches in I/B/E/S	8,363	69,303
Sample with available information to compute the minimum EBITDA covenant threshold and analyst EBITDA	4,764	38,420
Sample with revising analysts	1,878	4,108
Final sample with at least two revising analysts per loan contract	**818**	**2,833**

Panel B: Sample characteristics at the loan-analyst level

	N	**Mean**	**SD**	**p25**	**p50**	**p75**
Revision Up	2,833	0.51	0.50	0.00	1.00	1.00
Below Analyst	2,833	0.30	0.46	0.00	0.00	1.00
Firm size	2,833	7.73	1.60	6.68	7.62	8.80
Market to book	2,833	1.94	1.18	1.23	1.60	2.25
Firm leverage	2,833	0.29	0.19	0.16	0.28	0.40
Firm EBITDA over total assets	2,833	0.15	0.09	0.09	0.14	0.19
Abs returns $_{\text{before loan}}$	2,833	0.08	0.08	0.03	0.06	0.11
SD returns $_{\text{before loan}}$	2,825	0.03	0.02	0.01	0.02	0.03
Abs 3-day returns $_{\text{around loan}}$	2,821	0.03	0.04	0.01	0.02	0.04
Dividend restriction	2,833	0.74	0.44	0.00	1.00	1.00
Nr performance pricing provisions	2,833	1.24	1.02	1.00	1.00	2.00
Nr sweep provisions	2,833	1.36	1.81	0.00	0.00	3.00
Loan size (US$/Mil)	2,833	953	1,290	200	500	1,080
Loan maturity (months)	2,814	49.38	20.61	36.00	54.56	60.00
Loan spread	2,833	183.91	116.17	97.50	175.00	250.00
Collateral	2,833	0.59	0.49	0.00	1.00	1.00

Table 2: How analysts use the EBITDA thresholds in performance covenants?

Panel A – Regression result, indicator variable

This table presents results from the tests that assess how analysts with different EBITDA expectations revise their forecasts differently when facing the same loan contract. The tests estimate the following linear probability model:

$$Revision\ Up_{i,j} = \alpha_0 + \alpha_1 Below\ Analyst_{i,j} + Loan\ event\ fixed\ effects + \varepsilon_{i,j}$$

for each loan event *i* and analyst *j*. *Revision Up* is an indicator variable that takes the value of one when the revising analyst revises her forecast upward, and zero otherwise. *Below Analyst* is an indicator variable that takes the value of one if the EBITDA forecasted by the analyst before the loan event is lower than the minimum EBITDA threshold in the loan contract. In column (1), we include loan event fixed effects to capture how analysts with different initial forecasts react differently to the same minimum EBITDA value in the loan contract. These fixed effects absorb loan- and firm-level characteristics and hence we do not include them as controls in the model. In column (2), we include borrower industry and year fixed effects, together with a vector of firm- and contract-level controls. Variables are defined in the Appendix. Standard errors are calculated based on clustering by analyst. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	Revision Up	
	(1)	(2)
Below Analyst	**0.192*** **	**0.052** **
	(2.60)	**(1.99)**
Firm size		-0.004
		(-0.35)
Market to book		0.012
		(1.19)
Firm leverage		0.105
		(1.63)
Firm EBITDA over total assets		0.031
		(0.23)
Abs returns $_{\text{before loan}}$		-0.086
		(-0.58)
SD returns $_{\text{before loan}}$		-1.386*
		(-1.69)
Abs 3-day returns $_{\text{around loan}}$		-0.678**
		(-2.23)
Dividend restriction		-0.020
		(-0.78)
Nr performance pricing provisions		-0.015
		(-1.35)
Nr sweep provisions		0.003
		(0.34)
Loan size (ln)		0.004
		(0.33)
Loan maturity (months)		-0.001**
		(-2.09)
Loan spread		-0.000*
		(-1.84)
Collateral		0.103***
		(3.63)
Loan event FE	Yes	No
Industry and Year FE	No	Yes
Observations	2,833	2,768
Adjusted R-squared	0.409	0.081

Table 2: How analysts use the EBITDA thresholds in performance covenants? (Continued)

Panel B – Regression result, continuous variable

This table presents results from estimating the following linear probability model:

$$Revision\ Up_{i,j} = \alpha_0 + \alpha_1 Abs\ distance\ Below\ Analyst_{i,j} + \alpha_2 Abs\ distance\ Above\ Analyst_{i,j} + Loan\ event\ fixed\ effects + \varepsilon_{i,j}$$

for each loan event *i*, and analyst *j*. *Revision Up* is an indicator variable that takes the value of one when the revising analyst revises her forecast upward, and zero otherwise. We create two variables to separately identify analysts (i) whose EBITDA forecasts fall below the loan covenant threshold ("Below Analyst") versus (ii) analysts whose EBITDA forecasts stay above the covenant threshold ("Above Analyst"). Specifically, we compute the distance between the analysts' EBITDA forecasts and the loan contract EBITDA. The first variable (*Abs distance Below Analyst*) takes the absolute value of the distance if it is negative and zero otherwise. This variable captures how far the analyst forecast is below the covenant threshold for Below Analysts. The second variable (*Abs distance Above Analyst*) is equal to the distance if it is positive and zero otherwise. This variable captures how far the analyst forecast is above the covenant threshold for Above Analysts. We include loan event fixed effects to capture how analysts with different initial forecasts react differently to the same minimum EBITDA value in the loan contract. These fixed effects absorb loan- and firm-level characteristics and hence we do not include them as controls in the model. Variables are defined in the Appendix. Standard errors are calculated based on clustering by analyst. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	Revision Up
	(1)
Abs distance Below Analyst	**0.746***** **(4.87)**
Abs distance Above Analyst	**-0.032**** **(-2.27)**
Firm- and loan-level controls	Not Applicable
Loan event FE	Yes
Observations	2,833
Adjusted R-squared	0.418

Table 3: Cross-sectional tests on the use of EBITDA thresholds in performance covenants

This table presents results from estimating the following linear probability model:

$$Revision\ Up_{i,j} = \alpha_0 + \alpha_1 Abs\ distance\ Below\ Analyst_{i,j} + \alpha_2 Abs\ distance\ Above\ Analyst_{i,j} + Loan\ event\ fixed\ effects + \varepsilon_{i,j}$$

for each loan event i, and analyst j. We use the same variables as in Table 2 but partition the sample according to borrower characteristics (Panel A) and contract characteristics (Panel B). The subsamples in Panel A, Columns 1 and 2, include firms without and with, respectively, outstanding management forecasts, the subsamples in Columns 3 and 4 include firms with high (above median) and low (below median) borrower return volatility measured before the loan event, while the subsamples in Columns 5 and 6 include firms with S&P credit ratings that are below and above investment grade, respectively. The subsamples in Panel B are partitioned based on how far the borrower's current performance before the loan contract is relative to the threshold. We first measure the distance using the absolute difference between the borrower's current performance and the threshold scaled by the threshold. If the distance is below the sample median, we define the borrower's current performance as close to the threshold. Column (1) reports the results for this subsample. If the distance is above the sample median, we define the borrower's current performance as far from the threshold. Column (2) reports the results for this subsample. We include loan event fixed effects to capture how analysts with different initial forecasts react differently to the same *minimum EBITDA* value in the loan contract. These fixed effects absorb loan- and firm-level characteristics and hence we do not include them as controls in the model. Variables are defined in the Appendix. Standard errors are calculated based on clustering by analyst. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

Panel A - Cross-sectional test on borrower characteristics

	Management forecasts		**Return volatility**		**Investment grade**	
	(1)	(2)	(3)	(4)	(5)	(6)
	No	Yes	High	Low	Below	Above
Abs distance Below Analyst	0.740***	1.061	0.725***	0.811**	0.810***	1.698***
	(4.79)	(0.74)	(4.25)	(2.43)	(3.42)	(2.74)
Abs distance Above Analyst	**-0.034****	**0.125**	**-0.038****	**-0.027**	**-0.047*****	**-0.093**
	(-2.36)	**(1.18)**	**(-2.33)**	**(-1.34)**	**(-3.14)**	**(-1.02)**
Loan event FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,505	328	1,412	1,413	1,085	799
Adjusted R-squared	0.415	0.442	0.407	0.428	0.360	0.440

Panel B - Cross-sectional test on contract characteristics

	(1) *Threshold set relatively **close** to borrower current performance*	(2) *Threshold set relatively **far** to borrower current performance*
Abs distance Below Analyst	0.716***	1.377*
	(4.62)	(1.86)
Abs distance Above Analyst	**-0.019**	**-0.054*****
	(-1.13)	**(-3.59)**
Firm- and loan-level controls	Not applicable	Not applicable
Loan event FE	Yes	Yes
Observations	1,419	1,414
Adjusted R-squared	0.357	0.474

Table 4: Explanatory power of loan EBITDA covenants

This table presents results from estimating the following model:

$$Actual\ EPS_{i,j} = \alpha_0 + \alpha_1 Loan\ contract\ Min\ EBITDA\ per\ share_i + \alpha_2 Outstanding\ EPS\ forecast_{i,j} + FE + \varepsilon_{i,j}$$

for each loan event *i*, and analyst *j*. *Loan contract EBITDA per share* is the EBITDA threshold indicated in the loan contract (see Section 4.1 for details) divided by the number of outstanding shares. *Estimated EPS* is the existing estimates of revising analysts *before* the loan contract (i.e., pre-revision). Loan controls are the same as those included in Table 2 Panel A. *FE* is a vector of industry and year fixed effects. Variables are defined in the Appendix. Standard errors are calculated based on clustering by firm. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	Actual EPS	
	(1)	(2)
Loan contract Min EBITDA per share	**0.048****	**0.048****
	(2.18)	**(2.15)**
Outstanding EPS forecast	0.808***	0.776***
	(11.48)	(10.87)
Loan controls	No	Yes
Industry and year FE	Yes	Yes
Observations	2,690	2,678
Adjusted R-squared	0.769	0.778

Table 5: Changes in analyst forecast accuracy upon loan contracts with performance covenants

This table presents results from the tests that assess how more accurate the revised forecast after a loan event is compared to the forecast made by the same analyst immediately before the event. The tests estimate the following OLS model:

$$Difference\ accuracy_{i,j} = \alpha_0 + Loan\ event\ fixed\ effects + \varepsilon_{i,j}$$

for each loan event i, and analyst j. *Difference accuracy* is the difference in the accuracy of each analyst's post-loan contract forecast and pre-loan contract forecast. The coefficient α_0 represents the change in the accuracy of each analyst's forecast pre- and post-loan contract. We define the accuracy of each analyst's forecast as –1 × the analyst's forecast errors, where errors are defined as the absolute value of the difference between the analyst's estimate and the realized firm earnings, divided by the firm's stock price measured two days before the forecast date. A higher resulting number corresponds to greater forecast accuracy. We include loan event fixed effects, which allow us to estimate how more accurate the forecast made after the loan is compared to the forecast made by the same analyst immediately before the loan. These fixed effects absorb loan- and borrowing firm-level characteristics and hence we do not include them as controls in the model. Variables are defined in the Appendix. Column (1) reports the results for the overall sample, independent of how many days passed between the original forecast and the revision, while the other columns restrict the sample to observations with a shorter time gap between the original forecast and the revision. Standard errors are calculated based on clustering by analyst. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	Forecast accuracy			
	(1)	(2)	(3)	(4)
	All	Less than 90 days	Less than 60 days	Less than 15 days
Difference accuracy	**0.002*** (4.15)**	**0.002*** (4.15)**	**0.001* (1.78)**	**0.004*** (4.08)**
Firm- and loan-level controls	Not applicable	Not applicable	Not applicable	Not applicable
Loan event FE	Yes	Yes	Yes	Yes
Observations	2,472	2,161	1,548	123
Adjusted R-squared	0.169	0.223	0.068	0.868

Table 6: Forecast accuracy of revising vs. non-revising analysts

This table presents results from the tests that assess how more accurate the forecasts by revising analysts after a loan event are compared to the forecasts made by the non-revising analysts immediately before the event. The tests estimate the following OLS model:

$$Forecast\ accuracy_{i,j} = \alpha_0 + \alpha_1 Revising\ analysts_{i,j} + Loan\ event\ fixed\ effects + \varepsilon_{i,j}$$

for each loan event *i*, analyst *j*. We define the accuracy of each analyst's forecast as –1 × the analyst's forecast errors, where errors are defined as the absolute value of the difference between the analyst's estimate and the realized firm earnings, divided by the firm's stock price measured two days before the forecast date. A higher resulting number corresponds to greater forecast accuracy. *Revising analysts* is an indicator variable that takes the value of one if the forecast is made by revising analysts, and zero otherwise. We include loan event fixed effects, which allow us to estimate how more accurate the forecasts by revising analysts are compared to forecasts by non-revising analysts for the same borrower after the same loan event. These fixed effects absorb loan- and borrowing firm-level characteristics and hence we do not include them as controls in the model. Variables are defined in the Appendix. Column (1) reports the results for the overall sample, independent of how many days passed between the forecast and the loan contract, while the other columns restrict the sample to observations with a shorter time gap between the forecast and the loan contract. Standard errors are calculated based on clustering by analyst. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	Forecast accuracy			
	(1)	(2)	(3)	(4)
	All	Less than 90 days	Less than 60 days	Less than 15 days
Revising analysts	**0.012*** (7.81)**	**0.012*** (7.64)**	**0.012*** (7.37)**	**0.014*** (5.32)**
Firm- and loan-level controls	Not applicable	Not applicable	Not applicable	Not applicable
Loan event FE	Yes	Yes	Yes	Yes
Observations	10,167	9,121	7,963	3,547
Adjusted R-squared	0.877	0.882	0.886	0.885

Table 7: Changes in borrower information asymmetry upon loan events with performance covenants

This table presents results from the tests that assess how borrower information asymmetry changes after a loan event with performance covenants. The tests estimate the following OLS model:

$$Information\ asymmetry_{k,t} = \alpha_0 + \alpha_1 After\ PCov\ loan_{k,t} + \beta_1 Mkt\ value_{k,t} + \beta_2 Share\ turnover_{k,t} + Loan\ event\ fixed\ effects + \varepsilon_{k,t}$$

where *k* refers to the borrower and *t* refers to the day in the test window (i.e., two weeks before or two weeks after the loan event). We measure information asymmetry using the borrower's bid-ask spread (i.e., the difference between the firm bid and ask price, divided by the mid-point) in Columns (1) and (2), and the Amihud's (2002) stock illiquidity measure in Columns (3) and (4). In Columns (1) and (3) we limit our sample to loans with performance covenants. In Columns (2) and (4) we include loans with no performance covenants as a control group. Accordingly, we define *After PCov loan* as an indicator variable that takes the value of one in the two weeks after the loan event if the loan includes a performance covenant. We define *After loan with no PCov* similarly for loans with no performance covenants. We also test the difference between the coefficients on *After PCov loan* and *After loan with no PCov* and report the results in the table. We estimate the above equation using loan event fixed effects, which ensures the comparison of information asymmetry is made before and after the same loan event and holds constant the loan- and firm-level characteristics. Hence we do not include these characteristics as controls in the model. All variables are defined in the Appendix. Standard errors are calculated based on clustering by firm. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively. We use one-sided tests in the F-test because H2d is a directional hypothesis.

	Bid-Ask Spread		**Stock Illiquidity**	
	(1)	(2)	(3)	(4)
After PCov loan	**-0.011*** **	**-0.012*** **	**-0.010*** **	**-0.010*** **
	(-2.82)	**(-3.00)**	**(-3.24)**	**(-3.29)**
After loan with no PCov		-0.005*		-0.002
		(-1.69)		(-0.99)
Mkt value (USD Mil)	-0.062***	-0.033***	-0.011***	-0.004***
	(-5.18)	(-6.64)	(-4.88)	(-7.03)
Share turnover (000)	-2.175***	-1.933***	-10.789***	-10.339***
	(-5.85)	(-8.22)	(-29.73)	(-37.33)
F-test				
H_0: After PCov loan = After loan with no PCov		1.84*		4.9***
Firm- and loan-level controls	Not Applicable	Not Applicable	Not Applicable	Not Applicable
Loan event FE	Yes	Yes	Yes	Yes
Observations	165,513	407,913	164,114	404,545
Adjusted R-squared	0.858	0.773	0.618	0.472