Equity Market Use of Loan Market Information: Evidence from Analysts' Forecast Revisions around Loan Disclosures*

Carlo M. Gallimberti
carlo.gallimberti@bc.edu
Boston College

Lian Fen Lee
lianfen.lee@ bc.edu
Boston College

Alvis Lo
alvis.lo@ bc.edu
Boston College

Aug 2017

Abstract:

To examine equity market use of loan market information, we study whether and how equity analysts exploit the information embedded in loan contracts to improve their forecasts for borrowing firms. Our evidence reveals that analysts use the information in covenants with an earnings-related component to revise their forecasts and that the accuracy of the revised forecasts improves. Additional analyses suggest that analysts, especially the influential ones whose research gains more investor attention, are more likely to revise their forecasts, providing further evidence that the public equity market learns useful information from the private loan market. Our study has implications for understanding information flows between these two capital markets, highlighting that participants in one market can benefit from the information produced in another.

Keywords: equity analysts; debt contracts; debt covenants; equity market; credit market; private information; forecast errors; bank loans.

JEL Classification: D82, G14, G17, G2

* We thank the Carroll School of Management at Boston College for financial support. We also thank the workshop participants at Boston College and University of California, Berkeley for their useful comments and suggestions, as well as Justin Jin and Molly Pighini for their excellent research assistance.

1. Introduction

Loans and public equity are alternative sources of financing to corporations (e.g., Myers and Majluf 1984). A traditional view is that these are distinct forms of capital, with investors in each market having their own way of resolving information asymmetries. Specifically, lenders rely more on private communications with borrowers to address asymmetric information problems, whereas equity investors rely more on public disclosures. Recently, studies indicate that equity investors can learn from the loan market when assessing a firm's future performance. For example, through participating in syndicated loans, hedge funds and institutional investors learn private information about the borrower in their role as lenders, which allows them to subsequently trade in the equity market before the information gets released to the public (e.g., Massoud et al. 2011; Bushman et al. 2010; Ivashina and Sun 2011). These studies indicate that information can flow from the loan market to the equity market through the trades of *selected* syndicated loan participants. In this paper, we explore a *more widely* accessible channel through which private information from the loan market flows to the public equity market, made available by the SEC requirement on public disclosures of loan contracts. Specifically, we examine *whether* and *how* a group of equity market participants—namely equity analysts—makes use of the information embedded in the disclosed loan contracts in forecasting firms' future performance, broadening our understanding of how information produced in the debt market is used in the equity market.

Banks have a long-standing history of acquiring private information regarding their borrowers (e.g., Fama 1985). They hold private conversations with management, perform on-site visits, collect business plans, request confidential information about future projects, and so on. Banks have an incentive to collect information since they want to minimize the risk of lending to non-creditworthy borrowers, and to make sure that they can correctly price the future risk

borrowers are going to take. Borrowers have incentives to share private information with their banks to access credit at favorable conditions (e.g., Smith and Warner 1979; Bradley and Roberts 2004). Using this information, banks write loan covenants (e.g., minimum EBITDA requirements) to set satisfactory performance values for borrowers and determine price terms according to the risk assessed. As a result, the elements in loan contracts reflect the private information shared by borrowers and verified by their banks, which non-contracting parties (e.g., equity analysts) can use to learn about the borrowers. In the U.S., non-contracting parties can directly observe these contract elements since the SEC mandates the disclosure of loan contracts for public companies.[1] To illustrate, in February 2001, the telecommunications equipment company Lucent entered into a loan contract with a minimum EBITDA covenant.[2] This information may suggest that Lucent's EBITDA is unlikely to fall below the contracted threshold, potentially providing new information to analysts. Our study provides large-sample evidence on such a use of the information embedded in loan contracts by analysts to forecast borrowers' future performance.

We focus on analysts' reactions to loan disclosures instead of equity investors' reactions for several reasons. First, unlike investors whose earnings expectations are not observable, we can directly observe those of analysts through their outstanding forecasts. Hence, we can compare analyst expectations with loan contract elements such as the contracted EBITDA threshold, and attribute any subsequent forecast revisions by the analysts to the information they learn from the contract. Such an examination of analysts' use of the loan contract information speaks to the assimilation of information by investors, as analysts are a fair representation for the "beliefs held by investors in general" (Bradshaw 2011, 10) and "the overall behavior of capital market

[1] Details on the SEC disclosure requirements are contained in section 2.3.
[2] Available at https://www.sec.gov/Archives/edgar/data/1006240/000095012301001854/0000950123-01-001854-index.htm

participants" (Richardson et al. 2010, 423). Moreover, existing studies consider analysts important information intermediaries and show that investors quickly react to their research such as earnings forecasts (e.g., Cornell and Landsman 1989; Stickel 1991; Gleason and Lee 2003; Ivković and Jegadeesh 2004). Therefore, examining analysts' earnings forecasts revisions after loan contract disclosures provides insights into how public equity market participants learn information from the private loan market.

Our first set of tests assesses whether analysts respond to loan disclosures. We identify firms with disclosed loan contracts (i.e., treatment firms) using Dealscan during the period 1995 to 2012. For each analyst following the treatment firm, we identify all *other* firms she covered that did not disclose loan contracts during the *same* time period (i.e., control firms). We then obtain the analyst's outstanding forecasts for both the treatment and control firms prior to the loan disclosure (i.e., loan event), and test if she is more likely to revise her forecast in the two-week period after the loan event for the treatment firms than for the control firms. This difference-in-differences test design allows us to adjust for the analyst's tendency of revising a forecast in the absence of a loan disclosure. Although we expect analysts to use the new information in the mandated loan contracts disclosed, prior studies report that analysts and equity investors in general not always make use of all information available to them (e.g., Sloan 1996, Bradshaw, Richardson and Sloan 2001). Therefore, our research question remains an empirical question.

Results show that analysts are 14% more likely to revise their forecasts for treatment firms subsequent to their loan contract disclosures than for control firms. These results are obtained after we exclude confounding news events (i.e., earnings announcements and management guidance) around the loan disclosure. We also control for residual news events as captured by stock returns and stock volatility, as well as market reactions upon the loan announcement. Our finding provides

initial support that analysts use the new information in the disclosed loan contracts to update their beliefs about borrowers' future earnings and suggests that the mandated *public* disclosure of such contracts facilitates the transmission of *private* information once only known to the contracting parties from the private loan market to the public equity market.

To strengthen the link between analysts' behavior and the information in loan contract disclosures, we perform two additional sets of tests to understand *why* and *how* analysts revise their forecasts after loan contract disclosures. The first set of tests exploits two regulatory changes during our sample period that likely affected how analysts gather and use information in generating their forecasts. First, effective October 2000, Regulation Fair Disclosure (Reg FD) prohibited firms from selectively disclosing information to investors and analysts—but not banks—and hence reduced the amount of private information that analysts can obtain directly from managers. As a result, analysts are likely to rely more on public information (Kross and Suk 2012), including loan contract disclosures, in the post Reg FD period. Thus, we expect a higher likelihood of forecast revisions by analysts following loan disclosures post Reg FD. Second, effective August 2004, additional 8K disclosure requirements issued by the SEC increased the availability and timeliness of loan contract disclosures, making the information more easily accessible to analysts. Consequently, we expect that analysts are more likely to use the loan contract information after the additional 8-K disclosure requirements become effective. We find support for both predictions regarding regulatory changes, providing further evidence that analysts use the information embedded in the loan contracts disclosed

Illuminating the mechanism behind analysts' revisions, the second set of tests delves into the loan contract details, focusing on loans containing covenants explicitly written on future performance (e.g., EBITDA covenants). Analysts can use these covenants to derive the acceptable

EBITDA that the bank expects from the borrower. Falling below such threshold entails costly consequences for the borrowing firm, making the covenant a credible signal of borrowers' (minimum) future performance. Thus, we expect that analysts revise their earnings forecasts *upward* if their forecasts fall below the contracted threshold contained in the EBITDA covenant. Covenants thresholds may also provide guidance on the upper limit of the expected borrowers' performance. Prior studies report that banks are unlikely to set very loose covenants (e.g., Dichev and Skinner 2002). Thus, we expect that analysts whose earnings forecasts (well) exceed such covenant threshold have incentives to revise their existing forecasts downward. The results support our hypothesis and demonstrate one way in which analysts use the information in loan contracts in their forecasting process.

To understand further the transmission of information to equity investors, we study whether analysts whose research triggers more investor attention (i.e., influential analysts) are more likely to respond to loan contract information than other analysts. A positive finding would further support the idea that the information embedded in the loan contracts disclosed gets transmitted to equity investors. Following prior research, we identify All-Star analysts and analysts from larger brokerage firms as influential analysts (e.g., Stickel 1992, 1995; Gleason and Lee 2003). Ex ante, we expect influential analysts to be more likely to use the information in the loan contracts given their higher abilities (e.g., Stickel 1992; Leone and Wu 2007) and better resources, such as support team and timely data (Stickel 1995; Jacob et al. 1999; Clement 1999). Consistent with our expectations, we find that, conditional on the same loan contract, influential analysts are more likely to revise their forecasts following the loan disclosure than other analysts.

Finally, to validate our maintained assumption that the information in loan contracts is useful to equity market participants for forecasting borrowers' future performance, we examine

whether the forecasts revised right after loan disclosures are more accurate than the outstanding forecasts. As expected, we find that the revised forecasts are almost 10 percent more accurate. Importantly, this result does not depend on the time lapse between the prior and revised forecast issue dates, indicating that it is the new information embedded in loan disclosures that allows analysts to improve their predictions of firm future performance.

We contribute to several streams of literature. First, our findings show that analysts, particularly the influential ones, not only use the information in loan disclosures but improve their forecast accuracy when they do so. This suggests that the private information on which borrowers and banks contracted can improve the ability of equity market participants to forecast firm fundamentals. At a more general level, our results indicate that public equity market participants can learn from the loan market about a firm, highlighting a link between these two markets that are traditionally viewed as operating independently in their efforts to resolve information asymmetries. Also, our results indicate that the private information in lending relationships does not only favor the loan participants (Massoud et al. 2011; Bushman et al. 2010; Ivashina and Sun 2011) but, once disclosed, can also benefit other capital market participants, which has implications for understanding the (unintended) economic consequences of private capital transactions and disclosure regulations.

Our study relates to prior literature studying equity market reactions to loan announcements (e.g., James 1987; Lummer and McConnell 1989). James (1987), for instance, finds that loan issuances are associated with positive market returns while bond issuances are not, and concludes that such a difference in market reactions arises because equity investors value the close monitoring by banks over their borrowers. Unlike these studies, our focus is not specifically on bank monitoring, but rather on how public equity market participants can learn about the private

information embedded in the disclosed loan contract terms. Our research design explicitly controls for analyst forecast revisions arising from market reactions to the loan announcements, to help alleviate the concern that our results are capturing the "good news" about banks funding rather than the information transmitted from the loan contract details to the equity market.[3]

Our study also relates to Demiroglu and James (2010). Studying the information content of loan covenants, they show that variations in the choice of tight covenants across different loans are associated with different future borrower characteristics.[4] While we are also interested in the information content of loan contracts, we focus on equity market *use* of such information by examining analysts' forecast revisions around loan disclosures. Because we can observe how different analysts react differently to the disclosure of the same loan according to their earnings expectations, we illuminate how the public equity market uses the information produced in the private loan market, highlighting information flows between these two markets.

Finally, we add to the analyst literature by showing that analysts' forecasts incorporate the information embedded in loan contracts. Our results provide concrete examples of the inputs analysts use and how they use the information, shedding light on the process by which analysts generate their forecasts and hence complementing research that looks into the "black box" of financial analysts (e.g., Brown et al. 2015).

2. Institutional setting and hypotheses development

2.1 Loan contracts and private information

[3] As further discussed in the results section, we find that less than 50% of the revisions are upward revisions, suggesting that we are unlikely to be capturing the positive loan announcement effect reported by other studies (e.g., James 1987; Lummer and McConnell 1989).

[4] Demiroglu and James find that borrowers with tight covenants are associated with future increases in the covenant variable (e.g., current ratio) and future reductions in debt issuances and debt to EBITDA. They also find that covenant tightness is *not* related to future EBITDA (e.g., Table 6). They state that covenant tightness is associated with borrowers' future changes because of monitoring by banks and/or the signaling of information by borrowers through their covenant choices (see p. 3702).

Loans represent a main form of financing (Drucker and Puri 2007). Prior studies suggest that more than 70 percent of debt among U.S. corporations is contracted through loans (Houston and James 1996). The Loan Syndication and Trading Association (LSTA) manual offers a good overview of the most common elements and standardized legal structure of loan contracts (Wight et al. 2009). These contracts are complex legal documents, easily more than 60 pages long, with many provisions and clauses. In practice, virtually all loan contracts share the same structure and are written using similar clauses. Other than this "boilerplate" legal component, loan contracts contain terms that are tailored to each borrower, such as interest rate, borrowed amount, covenant structure, and so on. To specify these terms, banks screen their borrowers using information from both public sources (e.g., through press releases) and private sources (e.g., private interactions with the management).[5] Specifically, banks receive supplemental information from borrowers, such as business plans, internal sales forecasts, and potential changes in the cost structure, which they use to tailor the loan terms in each loan agreement for each specific borrower. As a result, loan terms reflect information that may go beyond the public information currently available to investors in the public capital markets.

By studying these loan contract terms, non-contracting parties such as analysts or other investors may infer the private information about the borrower. As a case in point, on February 22, 2001, the telecommunications equipment company Lucent announced a loan contract with a minimum EBITDA covenant. On February 28, a UBS analyst reported that "Lucent filed an 8-K yesterday 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". The analyst also noted that "This is

[5]Armstrong et al. (2010) provide an overview of recent accounting studies investigating the relation between loan contractual features and borrowers' information.

the first time that we have any type of financial guidance for Lucent since the 1Q01 earnings call".[6] Lucent had to disclose the loan contract due to the SEC disclosure requirement (see the next section). We conjecture that market participants may use the information embedded in the loan contract. For example, they might infer that Lucent's EBITDA is unlikely to fall below the contracted minimum threshold and update their expectations about the company future performance accordingly.

Covenants are likely to be rich in information, as they represent future performance values that banks expect borrowers to have. Covenants in loan contracts are mainly set up either as "trip wires"—i.e., early signals of default based on EBITDA measures—or to align interests between debtholders and shareholders (Christensen and Nikolaev 2012). Typical trip-wire covenants include minimum EBITDA, debt over EBITDA or interest coverage, while alignment covenants are set up on debt-to-equity ratio or net worth. As trip-wire covenants contain an earnings related component, we expect that the information contained in these covenants is particularly useful to analysts in forecasting firms' future earnings. Among these covenants, minimum EBITDA likely enables analysts to make the most direct inference about future earnings. For covenants that contain earnings together with other financial statement components, analysts can estimate the other components within reasonable boundaries to back out the implicit EBITDA. This is especially true for covenants such as debt to EBITDA, given that firm leverage is highly persistent over time (Lemmon et al. 2008). In contrast, we expect analysts to rely less on alignment covenants (e.g., net worth) to infer firms' future performance, since they are not directly based on earnings.

Other elements of the loan contracts, besides covenants, contain private information about borrowing firms, too. Bharath et al. (2009) show how banks adjust loan spread, maturity, collateral,

[6] As reported by Nikos Theodosopoulos, at UBS Warburg, entitled "Notes on 8-K, New Financial Guidance", February 28, 2001.

and amount as a function of their private information about the borrower. For instance, riskier borrowers likely have a higher interest rate (e.g., Merton 1974). By observing the interest rate, analysts might update their beliefs about the borrower's future prospect. However, unlike the case of EBITDA, we cannot observe analysts' ex ante expectations of interest rates, which makes it difficult for us to assess how they would use the loan contract information on this aspect in revising their forecasts. Hence, we will mainly focus our analysis on covenants that are written on future EBITDA values.

2.2 Analysts as information intermediaries

An extensive literature documents that investors respond to analysts' research (e.g., Cornell and Landsman 1989; Stickel 1991; Gleason and Lee 2003; Ivković and Jegadeesh 2004), consistent with the long-standing view that analysts play an important role as information intermediaries in the capital market.[7] Examining how analysts use and process information can therefore help us understand how information is transmitted to capital market participants. Specific to our context, we investigate how information about borrowers embedded in loan contracts is revealed to equity market participants such as analysts.

While analyzing information is a central activity for analysts, studies have characterized the way analysts analyze information as a "black box" (e.g., Bradshaw 2011; Brown et al. 2015), given that little is known about what information analysts use and how they process it in generating their research. We provide insights into the black box by examining whether analysts incorporate in their research the private information embedded in loan contracts. Unlike studies showing that

[7] Some studies (e.g., Altınkılıç and Hansen 2009; Altınkılıç et al. 2013) challenge this information role of analysts by documenting that the intraday returns for analysts' recommendation and earnings forecast revisions are insignificant. However, recent studies suggest that the insignificant returns around analysts' revisions could be attributed to other factors and provide evidence that analysts help facilitate information discovery (e.g., Bradley et al. 2014; Li et al. 2015).

analysts revise their forecasts after firms' earnings announcements or issuance of earnings guidance (e.g., Brown and Rozeff 1979; Waymire 1986; Jennings 1987; Stickel 1989), our focus on analysts' response to loan contract information allows us to assess whether they incorporate the information produced in the private loan market. Such a focus enables us to speak to potential information flows between the private and public capital markets.

2.3 Mandatory disclosure of loan contracts

Firms typically announce their loans to the market through press releases (e.g., James 1987). For example, Pantry Inc. issued a press release announcing the signing of a senior secured credit facility on December 30, 2005. While the press release contains the loan announcement, it does not specify most of the contract details (e.g., the covenant terms). Such details are likely to contain information relevant to investors and analysts, explaining why the Securities Exchange Commission (SEC) requires firms to disclose all the material information in loan contracts. Currently, firms signing material definitive agreements need to file a form 8-K under Item 1.01 within four business days after the event,[8] in which they are required to provide a summary of the material terms and conditions of the agreement, or attach the agreement itself, as commonly done in practice (see e.g., Nini et al. 2009).[9] In compliance with this requirement, on January 3, 2006, Pantry Inc. filed an 8-K containing the whole 145-page loan contract agreement.[10] We are interested in whether analysts revise their forecasts using the information in the loan contract disclosures, rather than simply as a consequence of the loan announcement. Accordingly, our identification strategy captures analysts' revisions beyond any potential response to loan

[8] See https://www.sec.gov/about/forms/form8-k.pdf.
[9] The SEC encourages firms to provide the agreement as an exhibit in their 8-K filings and this view is reiterated by legal experts, likely to reduce litigation risk. See examples from Morrison Foerster: https://media2.mofo.com/documents/faq-form-8-k.pdf, and from WilmerHale: https://www.wilmerhale.com/uploadedFiles/Shared_Content/Editorial/Publications/Documents/WilmerHale-Form-8-K-Guide-October-2014.pdf.
[10] https://www.sec.gov/Archives/edgar/data/915862/000119312506000486/0001193125-06-000486-index.htm.

announcements, as explained later in Section 3. Also as discussed later in Section 2.4, the SEC modified the requirements on the disclosure of loan contracts over our sample period, which we exploit to strengthen inferences.

2.4 Hypotheses development

We start by examining whether analysts revise their forecasts after a loan is disclosed. Ex ante, it is not clear whether analysts use the information in loan contracts to guide their forecasts. Previous studies show that analysts do not fully incorporate the public information released by firms (e.g., DeBondt and Thaler 1990; Abarbanell and Bernard 1992; Bradshaw et al. 2001) or from other sources, such as tax law changes (Plumlee 2003) or earnings announcements by firms in the same industry (Ramnath 2002). Part of the reason may be related to the fact that they already have private information about the firm through selective access to managers (e.g., Ivković and Jegadeesh 2004; Gintschel and Markov 2004; Chen and Matsumoto 2006).[11] Given this selective access, analysts might find the information embedded in loan contracts redundant, and thus not worth the additional effort uncovering it. This is especially so because information in loan contracts needs to be continuously monitored and processed over time, which could be onerous if the analyst lacks resources such as time, a support team, and easy access to timely data. However, if analysts decide to invest in understanding the main features of loan contracts, they could potentially expand their information set. Based on this discussion, we formulate our first hypothesis in alternate form as follows:

H1: Analysts are more likely to revise their forecasts for firms disclosing a loan contract compared to other firms in their portfolio that do not disclose a loan during the same time period.

[11] Studies suggest that analysts and managers interact in private meetings (Soltes 2014; Bushee et al. 2016), investors conferences (Green et al. 2014) and analyst/investor days (Kirk and Markov 2016).

We contend that analysts' forecast revisions are prompted by private information embedded in public loan contract disclosures. If this is the case, then we expect that analysts are more likely to revise their forecasts after a loan disclosure when their access to private information through contacts with managers becomes limited. To strengthen inferences, we test this prediction using two regulatory changes during our sample period. First, as discussed earlier, starting from October 2000, Regulation Fair Disclosure (Reg FD) prohibits firms from selectively disclosing material information to individuals with the purpose of levelling the playing field for all equity market participants. Kross and Suk (2012) document that analysts rely more on public disclosures such as earnings announcements, management earnings forecasts and conference calls after Reg FD, concluding that Reg FD limits analysts' access to private information from managers. This regulation however does not apply to banks who can still acquire private information from borrowing firms in setting loan contract terms. Thus, we conjecture that this regulatory change could increase the usefulness of the information embedded in loan contracts to analysts, and test whether they are more likely to revise their forecasts following loan disclosures after Reg FD is implemented. Second, starting from August 2004, the SEC tightened the requirements on public disclosure of loan contracts. Specifically, while the current rule requires a "description of the material terms and conditions of the agreement or amendment that are material to the registrant", the old requirement only encouraged disclosure of unspecified events considered important by the firm such as acquiring loans (Carter and Soo 1999). If Reg FD represents a change in the usefulness of the information in the loan contract, this second regulatory change ("New 8K Disclosure") represents a change in the availability of such information. Accordingly, similar to the earlier case, we test for an increase in analysts' revision activity following loan disclosures after the implementation of New 8K Disclosure.

Following our first hypothesis on whether forecast revisions are more likely after a loan contract has been disclosed, we investigate which of the loan features trigger such revisions. By shedding light on the mechanism behind the revisions, we can better understand the analyst's analysis process and the nature of information that is transmitted and assimilated in the capital markets. We can also increase our confidence that any results related to H1 is not due to analyst reactions to loan announcements, but rather to the loan information content. Recall from Section 2.1 that we focus on trip-wires covenants (aka performance covenants, see Christensen and Nikolaev 2012), as they are written on future EBITDA which contains information about future earnings that analysts are interested in forecasting. Analysts can compare the earnings estimates in their forecasting models with the implicit minimum EBITDA included in the loan contracts.[12] The minimum EBITDA in loan contracts represents a lower bound estimate of borrowers' performance acceptable to the bank. If borrowers fall below these minimum values, they will have to start a costly renegotiation process with their bank (e.g., Nini et al. 2012). Because of these costly consequences, analysts are likely to infer that it is highly unlikely for companies to fall below the contracted minimum EBITDA. Accordingly, they are likely to revise their earnings forecast upward if their current estimates are "too low" compared to the EBITDA implicit in the loan contract. At the same time, analysts whose estimates are "too high" compared to the ones implicit in the loan contract are unlikely to revise upward. They actually might use the contracted minimum EBITDA in the loan to revise their forecasts downward. Prior studies show that banks set covenants tightly, meaning that the borrowers' actual future performance is unlikely to be too far from the threshold used in the covenant (e.g., Dichev and Skinner 2002). Hence, for analysts

[12] Many analysts forecast net earnings rather than EBITDA, but EBITDA is a needed intermediate component to compute the net earnings. Therefore, even if analysts do not disclose their EBITDA forecasts, they are likely to have this information and can compare it with the loan contract's EBITDA.

whose forecasts are too high relative to the contracted covenant threshold, they have incentives to revise their forecasts downward to minimize ex post forecasting errors.[13] This leads us to our hypothesis:

H2: Analysts are more likely to revise their earnings forecasts upward (downward) if their expected EBITDA is below (well above) the minimum EBITDA implicit in the loan contract.

Although we expect analysts to use EBITDA covenants as a benchmark for their forecasts, extracting information from the EBITDA covenants implicit in loan contracts can be challenging. First, although the majority of loan contracts have at least one EBITDA covenant (Christensen and Nikolaev 2012), less than 10% of loan contracts specify a "minimum EBITDA" covenant (Demerjian and Owens 2016). The majority of EBITDA covenants are set relative to either firm leverage or interest expenses. Therefore, additional assumptions on the capital structure of the firm are needed to back out the minimum EBITDA implicit in the loan contract. Moreover, the definition of EBITDA in the loan contract might be different from the one used by the analyst (e.g., Leftwich 1983). If feasible, the reconciliation then might be time consuming or noisy. Because of these reasons, analysts might decide not to use EBITDA covenants to guide their predictions, making our prediction an empirical question.

After examining the information in the loan contract that analysts might use to revise their forecasts, we study whether influential analysts are more likely to respond to loan contract information than other analysts. A positive finding would support the transmission of information from the private loan market to public equity market, as previous studies document that these

[13] To illustrate, if the minimum EBITDA covenant requires an EBITDA of $300 million, analysts whose estimates falling below that threshold will likely revise upward. In contrast, analysts who were too optimistic (e.g., with a forecast of over $1 billion in EBITDA) will likely revise downwards, as they know that banks are unlikely to set such loose covenants.

analysts can trigger investor attention and generate the strongest price reactions. [14] We identify influential analysts as those with a "All-Stars" status or those working at larger brokerage houses, consistent with existing research (e.g., Stickel 1992, 1995; Gleason and Lee 2003). Ex ante, we expect that conditional on the same loan disclosure, influential analysts are more likely than other analysts to use the information in it for two reasons. First, they have greater ability (e.g., Stickel 1992; Leone and Wu 2007), making it less costly for them to uncover implicit information embedded in loan disclosures. Second, they have access to better resources, which helps them process the information in complex legal documents such as loan contracts. These reasons lead us to the following hypotheses:

H3: Influential analysts are more likely than non-influential analysts to revise their earnings forecasts for firms disclosing a loan contract.

So far, we have looked at analysts' revisions assuming that the information embedded in the loan contracts disclosed disclosure is useful for predicting firms' future performance. To validate our assumption, we examine analysts' forecast accuracy. If the information contained in the loan contracts is new and useful to equity market participants, the forecast accuracy of analysts revising after the loan contract is disclosed should improve. At the same time, a positive finding would also help us understand why analysts have incentives to look into these loan contracts in the first place, as several studies document that analyst forecast accuracy is associated with job turnover and career prospects (Mikhail et al. 1999; Hong and Kubik 2003; Groysberg et al. 2011). We discuss these supplemental tests in Section 4.3.

[14] For example, studies document that investors react more strongly to forecast revisions by analysts ranked as All-Star than those by unranked analysts (e.g., Stickel 1992; Gleason and Lee 2003). Furthermore, Gleason and Lee (2003) find that the post-revision drift subsequent to forecast revisions by All-Stars is smaller, suggesting that "the price adjustment process is faster and more complete" for these analysts. Other studies suggest that recommendation changes by analysts from larger brokerage firms have greater immediate impact on investors (Womack 1996) and generate stronger market reactions (Stickel 1995).

3. Are analysts using information in disclosed loan contracts?

3.1. Research design

To test whether analysts revise their forecasts after a loan contract is disclosed, we use an event study approach and examine whether analysts issue a revision immediately after a company discloses a loan. We use fourteen days as our event window to measure analysts' forecast revisions. SEC requires firms to disclose loan contracts within four business days from the loan start. We find that the firms in our sample conform to this requirement.[15] We allow an additional week for the analysts to update their forecasts, which suggests a two-week event window.[16]

To illustrate, suppose Coca Cola discloses a loan contract on Aug 27, 2002, and it is followed by Gregory Marshal at Goldman Sachs. We measure whether Gregory updates his forecasts for Coca Cola (our treatment firm) in the fourteen days immediately after Aug 27. To minimize the risk that Gregory updates his forecasts due to other events, we make sure that Coca Cola does not announce earnings or earnings guidance in the two weeks before or immediately after the loan announcement. To strengthen the link between the potential revision and the loan disclosed on August 27, we also ensure that Coca Cola does not issue other loans in this period. Finally, to control for other events affecting Coca Cola that we might not be aware of, we include the standard deviation and the absolute value of its cumulative stock returns in the fourteen days prior to August 27. These controls are appropriate for events that would influence stock price, which are events that would likely trigger analysts' revisions.[17]

[15] We randomly select a sample of 200 firms announcing loans from our sample and compare the loan dates in Dealscan with the dates in which the loan contracts are disclosed through EDGAR. On average, we find that firms disclose their loan contracts 3.2 days after the loan dates.

[16] Prior research has used a similar event window to study analysts' revisions following other events of interest. For example, Barron et al. (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] For instance, previous studies find significant market reaction upon or before the loan inception through press releases (e.g., Billett et al. 1995; Gande and Saunders 2012).

To tighten the identification, we also consider Gregory's tendency to revise his forecasts absent a loan disclosure. Suppose Gregory also follows other companies such as Pepsi, which did not have a loan in the two weeks following August 27. We use these other firms as control firms and ensure that they do not experience any significant corporate events as described above for Coca Cola. Our focus is on whether Gregory is more likely to issue a forecast revision for Coca Cola than for, say, Pepsi.

This research design mitigates the effects of unobservable confounding factors such as time trends and analyst characteristics. First, by comparing forecast revisions for firms followed by the same analysts, we keep the analysts' characteristics, such as their ability and tendency to revise, constant. Second, by comparing forecast revisions happening at the same time, we control for any macroeconomic news that could potentially trigger revisions unrelated to the loan disclosure.

Figure 1 summarizes the research design. To measure the effect of a loan disclosure (i.e., the event) on analysts' revisions for Borrower A (treatment firm), we compare how more likely Analyst 1 is to issue a forecast revision for Borrower A than for other firms that she follows (i.e., control firms) after the event. We perform the same procedure described for Analyst 1 for each of the other analysts following Borrower A. We then repeat the same exercise for each loan event in our sample and compute the average treatment effect across all loans disclosed and all analysts. Operationally, we estimate the following linear probability model:

$$Revision_{t,j,i} = \alpha_0 + \alpha_1 Loan\ disclosed_{i,t} + fixed\ effects_{t,j} + X_{i,t}\alpha + \varepsilon_{t,j,i} \quad [1]$$

where *t* is the loan disclosed, *j* is the analyst, and *i* is the firm followed by the analyst. *Revision* is an indicator variable that takes the value of one if the analyst issues a revision for the firm within the two weeks after the loan contract is disclosed, and zero otherwise. *Loan disclosed* is an indicator variable equal to one for firms disclosing loan contracts in our event window, and zero

otherwise. We include analyst × loan-event fixed effects so that the coefficient estimates are identified within each loan event and within firms followed by the same analyst. Therefore, the coefficient on *Loan disclosed* (α_1) captures how more likely an analyst is to issue a forecast revision for a firm disclosing a loan than for other firms followed by the *same analyst* at the *same time*. A positive value of the coefficient on *Loan disclosed* (α_1) is consistent with H1. *X* is a vector of firm level control variables. To control for extraordinary events that might trigger a revision besides loan disclosure, we include firms' stock returns and standard deviation of their returns in the fourteen days prior to the loan disclosure.

3.2. *Sample selection*

Our sample begins with loans issued by firms on Dealscan. Following prior studies (e.g., Gande and Saunders 2012), we use the loan starting date as the loan announcement date. We gather information on individual analyst earnings forecasts from I/B/E/S U.S. Detail file. For each loan event, we identify all outstanding annual earnings forecasts by analysts following the borrowing firm before the loan announcement.[18] Based on prior research, we consider an analyst's forecast to be outstanding if it was issued no more than 180 days prior to the loan announcement (e.g., Jegadeesh and Kim 2010; Lee and Lo 2016). For each analyst with an outstanding earnings forecast prior to the loan announcement, we determine if she has a revised forecast for the same fiscal period in the two weeks following the loan announcement. We also obtain variables required to construct analyst characteristics and other analyst forecasts such as cash flow forecasts and EBITDA forecasts from I/B/E/S. We use Dealscan to collect additional loan information, such as loans' covenant details, that we use in additional tests. We collect financial and stock related

[18] Our choice of annual forecasts follows prior literature (e.g., Baginski and Hassell 1990; Bamber et al.1999; Barron et al. 2002; Gleason and Lee 2003).

information from Compustat and CRSP.[19] Finally, we use the information on First Call and Compustat to exclude firms issuing earnings guidance and announcement, respectively, around our event window.

3.3. Summary statistics

Our sample period spans the years 1995 to 2012, during which we are able to match 17,927 loans (i.e., our treatment event) with available information on our main variable of interest. Each firm issuing a loan (our treatment firms) is followed by, on average, nine analysts. At the same time, each of these analysts follows, on average, eleven other firms not concurrently issuing a loan (our control firms). Therefore, our final sample includes about 167,000 treatment observations (i.e., 17,927 × 9) and 1.7 million control observations (i.e., 17,927 × 9 × 11). Appendix B summarize the sampling structure.

Table 1 reports the main summary statistics for our treatment and control firms and for our overall sample. Panel A reports that more than 40 per cent of the treatment firms receive a forecast revision during our event window. As a comparison, prior literature reports that about 50 per cent of firms receive a forecast revision by analysts after management guidance (e.g., Jennings 1987).[20] While the statistic in Table 1 Panel A is lower than that in prior studies, the two numbers are not too far apart. Considering that management guidance is one of the most important information disclosures for firms and is at times coupled with earnings announcement which is also an important event, the descriptive statistic in Table 1 Panel A is consistent with anecdotal evidence that analysts pay attention to loans. Notably, only 46 percent of these revisions are upward

[19] We use the linking table in Chava and Roberts (2008) to match information between Dealscan, Compustat, and CRSP.

[20]Table 2 Panel C in Jennings (1987) shows that most of the consensus analysts' forecast revisions happen from the week of (T=0) to four weeks (T=4) after management earnings guidance. Because we examine forecast revisions in the 14-day period after the loan announcements, the distributions for T=0 and T=1 are most comparable. Data from columns (-,0) and (+,0), which represent cases where there are no analyst forecast revisions after management guidance, indicate that only about 50% of firms receive a revision in the two-week period.

revisions, providing initial evidence that analysts interpret the information in the loan contract disclosed differently. We study this issue further in Section 4.1.

Panels B and C present descriptive statistics for our treatment and control firms, respectively. In addition to our controls for contemporaneous residual news events (i.e., *Cumulative returns* and *SD returns*), we also present descriptive statistics for common firm characteristics such as size, market-to-book, performance and leverage. These characteristics are measured using the most recent financial statements before the loan event. The treatment firms are smaller than the control firms, have lower market-to-book, ROA, and higher leverage and stock returns (both in absolute value and in their standard deviations) before the loan event. Given that these common firm characteristics are associated with firm information environment and prior research shows that firm information environment affects analysts' forecasting activity (e.g., Lang and Lundholm 1996), we control for the firm characteristics in all of our tests.

Finally, Panel D presents the sample characteristics at the *firm-analyst* level, which we use in our analyses. On average, nine percent of analysts following the firms in our sample issue a forecast revision during our two-week event window.

3.4. Main results

Table 2, Panel A reports the estimation results for equation [1]. We cluster standard errors at the analyst-level, as estimates for different firms by the same analysts are likely to be correlated.[21] In column (1), the coefficient of interest (α_1) is positive (0.013) and significant (t-stat = 16.21), consistent with H1, posing that analysts are more likely to revise their estimates for a firm that discloses a loan contract than for other firms they follow. In terms of economic

[21] Because our fixed effects structure requires multiple observations for each loan announcement-analyst duple, the sample in Table 2 is slightly smaller than that in Table 1.

significance, they are 14% more likely to revise for loan disclosing firms (= 0.013/0.09, where 0.09 is the average revision rate at the firm-analyst level as reported in Table 1, Panel C).

Columns (2) and (3) show that our results are robust to controls for contemporaneous news events as proxied by stock returns variables and firm characteristics. For example, these controls capture the market reaction to loans announced before their starting date, usually through press releases, increasing our confidence that the analysts' reaction is due to the content of the loan contract disclosed, rather than its announcement. In case the loan is announced only at its inception, Column (4) also includes the abnormal stock returns, in absolute terms, around the loan inception date as an additional control. This specification provides further reassurance that market reactions to loan announcements do not fully explain analysts' revisions around loan disclosures. Results are robust in all specifications.

Our findings in Table 2 Panel A are consistent with the idea that analysts use the private information embedded in loan disclosures to revise their forecasts. To strengthen the link between the information content of the loan and analysts' revising behavior, we further examine how analysts' use of the information varies with (i) their reliance on and (ii) the availability of public disclosures. Panel B reports the results. As discussed in section 2, because analysts are more reliant on public disclosures after Reg FD, we predict that the likelihood of them revising their forecasts following the loan contract disclosures is higher in the post-Reg FD period. To test our conjecture, we interact our main coefficient of interest (α_1) with an indicator variable that equals one after the adoption date of Reg FD (October 2000). We limit our sample period up to August 2004 when the New 8K Disclosure requirements became effective so that the impact of other regulatory changes would be minimized. Column (1) reports the results. Consistent with our conjecture, the interacted coefficient on *Loan disclosed* × *Reg FD* is positive and significant, indicating that post Reg FD

analysts are more likely to revise their forecasts following a loan disclosure. This result is in line with the idea that analysts use the private information embedded in the loan disclosure to supplement the decrease in their access to management after Reg FD.

Column (2) reports the results for the tests exploiting the New 8K Disclosure requirements in August 2004. As discussed in Section 2, this regulatory change made the loan disclosures more readily available to the public. We expect that analysts are more likely to revise their forecasts following loan disclosures after this regulatory change. To test our conjecture, we interact our main coefficient of interest (α_1) with an indicator variable that equals one after the New 8K disclosure requirements are in place. To sidestep the confounding effect of Reg FD, we limit our sample period to years when Reg FD was already implemented. Column (2) shows that the coefficient on *Loan disclosed* × *New 8K Disclosure* is positive and significant, consistent with our conjecture that the treatment effect is more pronounced after the disclosure of loan contracts became more readily available.

Finally, as a robustness check and for completeness, Column (3) presents results for the whole sample period including both regulatory changes. In this case, the coefficient of *Loan disclosed* × *Reg FD* represents the incremental change in the likelihood of revisions, net of the effect of *New 8K Disclosure* on revisions. Similarly, the coefficient of *Loan disclosed* × *New 8K Disclosure* represents the incremental change in the likelihood of revisions, net of the effect of *Reg FD*. Consistent with the results in the first two columns, both coefficients are positive and significant, and have similar magnitude. Overall, the results indicate that analysts' use of the information in loan contracts varies predictably with their reliance on and the availability of public disclosures.

4. Further insights into analysts' use of loan contract information

4.1. How do analysts use loan contract information to revise their forecasts?

In developing H2, we posit that analysts could use the information embedded in trip- wires covenants to guide their own estimates. If the EBITDA forecasted by analysts falls below the minimum EBITDA implicit in the loan contract, analysts are likely to increase their earnings forecasts. Conversely, if their forecast is (well) above the loan minimum EBITDA, they will likely revise downwards. In this section, we perform tests of two predictions arising from H2. First, if analysts find the information embedded in trip-wires covenants particularly useful, they are more likely to revise their forecasts after disclosures of loan contracts that have such covenants than after contracts without them. Second, for analysts revising their forecasts after disclosures of loan contracts that have trip-wire covenants, we expect them to compare their own estimates with the ones implicit in the covenants.

Table 3 reports test results for the first prediction. Since we focus on how analysts react differently to loan contracts with or without trip-wire covenants, we exclude control observations (about 1.7 million) and focus solely on analysts' coverage of firms with loans disclosed. . In addition, our test requires information on loan contract details to be available on Dealscan (e.g., loan spread and covenant details, etc), which excludes about 70 percent of the remaining sample, leaving us with 62,102 loan-analysts observations.[22] Appendix B contain further details on the sample used.

To focus on the variation between contracts with different loan elements while keeping other firm, analysts and macro characteristics constant, we add firm, analysts, and time fixed effects. We also include the vector of firm control variables used in the previous tests. The variable

[22] Consistent with prior studies (e.g., Christensen and Nikolaev 2012; Ball et al. 2015), we exclude contracts that are reported as having no covenants in Dealscan, since the absence of covenants is likely to be more indicative of data unavailability than actual absence of covenants.

of interest, *Trip wire covenants*, measures how more likely analysts are to revise their forecasts when the loan disclosed contains one additional trip wire covenant. As expected, its coefficient is positive and significant, indicating that loan contracts containing trip wire covenants trigger more analyst revisions.[23] In contrast, it does not seem that analysts are more likely to revise their forecasts when the loan contains alignment covenants, presumably because these covenants are not directly written on earnings-related components and hence reveal less information about the borrower's future performance. The coefficient on *Alignment covenants* is not significant.

In column (2), we control for other loan elements such as loan spread that likely reflect private information about the borrower (e.g., Bharath et al. 2009). Some of the control variables are significant. For instance, analysts are more likely to revise their forecasts when the loans have higher spreads and higher dollar amounts but are less likely to do so for revolving loans.[24] Our inferences remain the same after adding these controls.

Our second test examines whether analysts increase (decrease) their earnings forecasts if their EBITDA forecasted prior to the loan announcement is below (above) the minimum EBITDA implicit in the loan contract. We focus on observations where (i) the analysts revise their forecasts and (ii) the loan contracts have the EBITDA threshold information. Recall that about 9 percent of analysts in our sample revise their forecasts (see Table 1, Panel C), which gives us an initial sample of 5,589 observations (9% × 62,102 observations from Table 3, column 2). Out of this initial sample, EBITDA threshold is present in the loan contract for 4,040 observations. Within these observations, analysts report explicit EBITDA forecasts and cash flows forecasts for only 681

[23] On average, loan contracts in our sample have 1.4 trip wire covenants. Results are similar when we use an indicator variable instead of a discrete variable.

[24] Revolving loans are line of credits repeatedly extended over time, with similar loan terms and covenant structure (e.g., Sufi 2009) which therefore are less likely to convey new information.

observations, which represent the observations available for our test.[25] Appendix B contain further details on the sample used.

In the regression, the dependent variable, *RevisionUp*, takes the value of one if the analyst revises upward her existing forecast, and zero otherwise. Our independent variable of interest, *[Analyst EBITDA < minimum EBITDA loan]*, takes the value of one if the outstanding EBITDA forecasted by the analyst is below the EBITDA threshold in the loan contract, and zero otherwise. In later tests we also use the distance between analysts' forecasted EBITDA and the EBITDA in the loan contract. To construct the EBITDA threshold, we use the value stated in the minimum EBITDA covenant, when available. For contracts without such covenant, we compute the minimum EBITDA threshold by using the "Max Debt to EBITDA" covenant and firms' financial information.[26] For example, for a loan contract with a Max Debt to EBITDA covenant of 4, loan amount of $300 million, and the firm's most recent outstanding debt of $1 billion, the minimum EBITDA threshold would be equal to $325 million(= [1,000 + 300] / 4).[27]

Our objective is to examine whether analysts' responses to the same loan contract differ depending on their expectations of EBITDA prior to the loan disclosure. Hence, we include loan contract fixed effects to ensure the comparisons between analysts are made within the same loan. Furthermore, given that the coefficient of interest is identified within loan contract, all loan (and firm) characteristics are held constant, making the inclusion of loan and firm control variables unnecessary. However, the cost of this identification strategy is that we lose 295 observations that have no variation in the dependent variable within each loan contract, since these cases are

[25] Prior literature has used cash flow per share (CPS) forecasts in I/B/E/S as analysts' forecasts of operating cash flows (e.g., DeFond and Hung 2003; Call et al. 2009; Lee 2012).
[26] If the "Max Debt to EBITDA" covenant is also unavailable, we use the "Max Senior Debt to EBITDA".
[27] We obtain similar results when we use only contracts with minimum EBITDA covenants, although our sample is significantly smaller.

absorbed by the fixed effects.[28] Panel A of Table 4 reports the results. The coefficient on Indicator [*Analyst EBITDA < minimum EBITDA loan*] in column 1 is positive and significant, suggesting that, facing the same loan, analysts whose EBITDA forecasts fall below the minimum threshold in the loan contract are 50 percent more likely to revise upward than analysts whose forecasts are above this number.[29]

It is possible that an analyst's EBITDA construct differs from the construct used in the loan contract, which results in the analyst's forecasted EBITDA prior to the loan announcement to be lower than the minimum EBITDA in the loan contract. In this case, we will capture differences in analysts' definitions of EBITDA rather than their expectations of earnings. We do not see any obvious reason why analysts with a low forecast due to differing definition of EBITDA would be more likely to revise upward upon loan disclosures. Nevertheless, we use analyst cash flow forecasts as an alternative definition of EBITDA to assess the robustness of our results. Column (2) reports the results which are consistent with the results in Column (1).

Another potential concern is the small sample size due to the limited availability of EBITDA and cash flow forecasts by individual analysts. To ease this concern, we attempt to expand our sample by constructing analysts' EBITDA forecasts using their EPS forecasts. We first multiply the forecasted EPS by the number of outstanding shares to compute the forecasted earnings. Then, we add back income taxes, interest expenses and depreciation and amortization from the most recent financial statement available before the analysts' revision to approximate the forecasted EBITDA.[30] To the extent that these items do not change much over time, our imputed

[28] If we run our regression without fixed effects, i.e., exploiting all of the 681 observations in our sample, our inference is unchanged.
[29] On average, 49% of analysts in our sample revise upward. Therefore, the coefficient in column 1 shows an increase in probability of upward revision of 50% (i.e., 0.25 / 0.49).
[30] We consider either the tax dollar amount or the effective tax rate to compute the estimated taxes. Results are similar in both cases.

EBITDA would be close to the one forecasted by the analysts.[31] Consistent with this argument, Panel B reports that the correlation between the EBITDA forecasts in I/B/E/S and our imputed EBITDA estimates is high (around 90%). In Panel C, we report the regression results using the imputed EBITDA estimates as the proxy for analysts' EBITDA forecasts before the loan announcement. The sample size is almost ten times larger and the inference from Panel A remains unchanged, which increases the generalizability of our results.

The coefficients in Table 4 confirm H2 that analysts' revisions depend on their expectations relative to the loan contracts. We conduct additional tests to verify that our results are driven by both the analysts falling below and above the loan covenant threshold (*below analysts* and *above analysts*, respectively). We split our independent variable into two different variables depending on which group the analysts belong to. Specifically, we compute the relative distance between the analysts' forecasts and the implicit EBITDA covenant threshold, and create two variables depending on the sign of the distance. The first variable (*Abs distance if [Analyst EBITDA < minimum EBITDA loan]*) takes the absolute value of the distance if the distance is negative and the value of zero otherwise. This variable captures how far the analyst forecast is below the covenant threshold, for *below analysts*. The second variable (*Abs distance if [Analyst EBITDA > minimum EBITDA loan]*) is equal to the distance if it is positive and zero otherwise. This variable captures how far the analyst forecast falls above the covenant threshold, for *above analysts*. On average, the EBITDA forecasts by *above analysts* are 1.8 times higher than the minimum EBITDA in the loan contract. Compared to the EBITDA forecasts by *below analysts*, the minimum EBITDA in the loan contract is about 18 percent higher.

[31] Nevertheless, it is possible that we measure the analysts' expectation with error. We do not expect this error, however, to necessarily bias the results in our favor. To create such bias, the use of financial statement data should lead us to underestimate the analysts' EBITDA when the analysts revise upward their forecasts immediately after the contract disclosure. We cannot think of a reasonable scenario where this is the case.

Panel D reports the results. Both the analysts' groups behave in the predicted way: the further below (above) the estimate of the analyst, the more likely she is to revise upward (downward). Interestingly, the absolute size of the coefficients is different (p-value < .01), indicating that small differences below the covenant EBITDA threshold are likely to trigger upward revision but much larger (about eight times) differences above such threshold are needed to trigger downward revisions with the same probability. In other words, analysts need to be well above the loan threshold EBITDA before revising downward.

Overall, the results in Table 4 suggest that analysts compare their own forecasts with the embedded loan contract information in determining how they revise their estimates, which illuminates a mechanism through which the loan contract information impacts analysts' prediction of future firm performance.

4.2. Are influential analysts more likely to revise their forecasts following loan disclosures than other analysts?

As discussed in section 2, we test whether influential analysts are among those who revise their forecasts using the information in disclosed loan contracts. A positive finding is consistent with private information in loan contracts being transmitted to equity market participants, as influential analysts trigger investors' attention and the strongest market reactions (e.g., Stickel 1992; Gleason and Lee 2003). In estimating the differential probability of analysts' revisions, we include loan contract fixed effects to capture whether facing the same loan contract, influential analysts respond differently relative to other analysts. As in the case of Table 4, this fixed effects structure excludes firms that are followed by only one analyst when they disclose their loan contracts and makes the use of firm and loan controls unnecessary. Given that our treatment sample is made of about 167,000 observations (see section 3.3 and Appendix B), after excluding

observations with only one analyst, our sample consists of about 164,000 observations. Appendix B contain further details on the sample used.

Table 5 reports the results. In column (1), we proxy for influential analyst using *Analyst is All-star* which is an indicator variable that takes the value of one if the analyst is ranked as an All-Star by Institutional Investor magazine at the time of the contract disclosure, and zero otherwise. Consistent with H3, the coefficient on *Analyst is All-star* is positive and significant, suggesting that All-Star analysts are more likely to use the information in the loan contract to revise their earnings forecasts than other analysts. In column (2), we proxy for influential analysts using the size of the brokerage firm the analyst works for at the time of the loan contract, where brokerage firm size is defined as the natural logarithm of the number of analysts the firm employs. Consistent with H3, the coefficient on *Size of the brokerage house* is positive and significant, indicating that analysts employed at larger brokerage houses are more likely to revise their forecasts following loan disclosures. Taken together, our evidence suggests that influential analysts contribute to transmitting the information from disclosed loan contracts to the equity market.

4.3. Is the information embedded in loan contracts useful in predicting future firm performance?

To validate our maintained assumption that the information contained in loan contracts is useful to equity market participants, we examine the change in analysts' forecast accuracy following loan disclosures. As discussed in Section 2.4, if the loan contracts contain useful information to analysts, the revised forecasts would be more accurate than the analysts' outstanding forecasts prior to the loan disclosures. To test this prediction, we estimate a regression model with forecast accuracy as the dependent variable. We measure accuracy using forecast errors, namely, the absolute value of the difference between the analysts' estimate and the realized

firm earnings, divided by the firm's stock price measured two days before the forecast date. Lower forecast errors correspond to higher forecast accuracy. Our variable of interest (*After*) is an indicator variable that takes the value of one if the forecast is made in the two weeks after the loan disclosure (our event window), and zero otherwise. We include loan contract × analyst fixed effects since we want to compare the accuracy of forecasts by the same analyst before and after the same loan disclosure. Therefore, our test holds analyst characteristics (e.g., their ability) and loan characteristics (e.g., interest rate) constant and we do not need to include the corresponding controls. The use of loan × analyst fixed effects also makes it unnecessary to include firm-level controls because loans are firm and time specific.

Table 6 shows the results. To be included in the sample we require the analyst to have a revision after the loan is disclosed, which brings our sample to about 32,000 observations.[32] Column 1 shows that the coefficient on *After* is negative and significant, indicating that the forecasts revised right after the loan disclosures are more accurate than the forecasts issued before the disclosures (i.e., the forecast error is lower). The value of the average forecast error in our sample is 0.035, suggesting that forecasts made after the loan is disclosed are 9% more accurate.

Given that the median number of days between the outstanding forecast prior to the loan event and its revision afterwards is 50, one potential concern is that analysts might have gathered more information that is not related to the loan disclosure and that allows them to generate more accurate forecasts. To alleviate this concern, columns 2, 3, and 4 present results for a subset of observations where the revised forecast is less than 30, 15, and 7 days apart from the outstanding

[32] About 17,927 firms have a loan contract and each firm, on average, is followed by 9 analysts (see Table 1, Panel A), which yields about 167,000 observations at the loan-analyst level. We further require the analysts to have a revision after the loan disclosure (about 10% of the sample), which yields about 16,000 observations. We compare these revised forecasts to the analysts' outstanding forecasts before the loan disclosure, which gives us about 32,000 observations (16,000 × 2). Appendix B contain further details on the sample used.

forecast before the loan event respectively. The magnitude of the coefficient does not decrease across columns (if anything it seems to increase), suggesting that the time between the forecast and its revision does not affect our inferences.

Overall, our results suggest that the information in the loan contract is useful to equity market participants to predict future firm performance. Simultaneously, given that analyst forecast accuracy is associated with the analyst's job turnover and career prospects (Mikhail et al. 1999; Hong and Kubik 2003; Groysberget al. 2011), our finding provides an explanation for why analysts may have the incentives to look into the disclosed loan contracts in the first place.

5. Conclusion

We investigate whether and how equity market participants use the private information embedded in the terms of loan contracts mandatorily disclosed according to SEC rules. Our analyses focus on equity analysts, a sophisticated class of equity market participants whose earnings expectations are directly observable through their outstanding forecasts before the loan contract disclosures. After controlling for other events that might trigger a revision and for analysts' general forecasting tendency (i.e., by using other firms followed by the same analysts at the same time the loan is disclosed as a control group), we find that analysts are 14 percent more likely to revise their forecasts when a loan is disclosed. Our analyses also reveal that analysts use the information in covenants with an earnings-related component to guide their forecast revisions, which improves their forecast accuracy. Overall, these results suggest that loan contracts contain information about borrowers' future performance that is useful to equity investors. Reinforcing the transmission of the private information embedded in loan contract to the equity market, we further find that analysts, particularly the influential ones whose research triggers greater market reactions, are more likely to revise their forecasts after loan disclosures.

Our work has implications for understanding the information flows between the private loan market and the public equity market, which are traditionally viewed as operating independently in their efforts to resolve information asymmetries. Our findings indicate that the private information shared in lending relationships does not only favor the loan participants (e.g., Massoud et al. 2011; Bushman et al. 2010; Ivashina and Sun 2011), but once disclosed, can also benefit other capital market participants. Importantly, we illuminate a disclosure channel through which such private lending information gets transmitted to the wider public, which has implications for understanding the (unintended) economic consequences of private capital transactions and disclosure regulations. Finally, by highlighting the information component of loan contract disclosures, we contribute to studies on how the market reacts to loan announcements (e.g., James 1987) and on the information contained in loan covenant structure (e.g., Demiroglu and James 2010).

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Appendix A: Variable definitions

Firm size	The natural logarithm of the market value of firm equity
Market to book	Sum of market value of equity and the book value of debt, divided by total assets
Firm leverage	Sum of debt in current liabilities and long-term debt, divided by total assets
Firm ROA	Operating income before depreciation, divided by total assets
Absolute cumulative return	Absolute cumulative stock returns from 14 calendar days before the loan disclosure date to 1 day before the loan disclosure date
Absolute buy-and-hold abnormal returns around loan inception	Absolute value of the buy-and-hold abnormal return in the three-day window around the loan inception date. We compute abnormal returns by subtracting the value-weighted market returns from the stock returns.
SD returns	Standard deviation of stock returns from 14 calendar days before the loan disclosure date to 1 day before the loan disclosure date
Revision	An indicator variable equal to one if analysts revise their annual forecasts within the event window and zero otherwise
Loan disclosed	An indicator variable that takes the value of one if the firm discloses a loan contract and zero otherwise
Trip wire covenants	The number of the following covenants in each loan contract (at the package level): level of EBITDA, debt-to-EBITDA ratio, senior debt to EBITDA ratio, cash interest and debt service coverage ratios, interest coverage ratio.
Alignment covenants	The number of the following covenants in each loan contract (at the package level): debt-to-equity ratio, loan-to-value ratio, the ratio of debt to tangible net worth, leverage and senior leverage ratios, and net worth requirements quick and current ratios
Capex restriction	An indicator variable that equals to one if the loan contract contains a capital expenditure restriction and zero otherwise
Dividend restriction	An indicator variable that equals to one if the loan contract contains a dividend restriction and zero otherwise
# of performance pricing	The sum of the performance pricing restrictions in the facilities included in each loan contract
# of sweeps	The sum of the sweep provisions in the facilities included in each loan contract
Package revolver	An indicator variable that equals to one if the loan contract contains a revolving facility and zero otherwise
Loan size	The natural logarithm of the loan dollar amount
Package maturity	The loan maturity, measured in months
Loan spread	The natural logarithm of the weighted average spread of each facility, using each loan facility dollar amount as weights

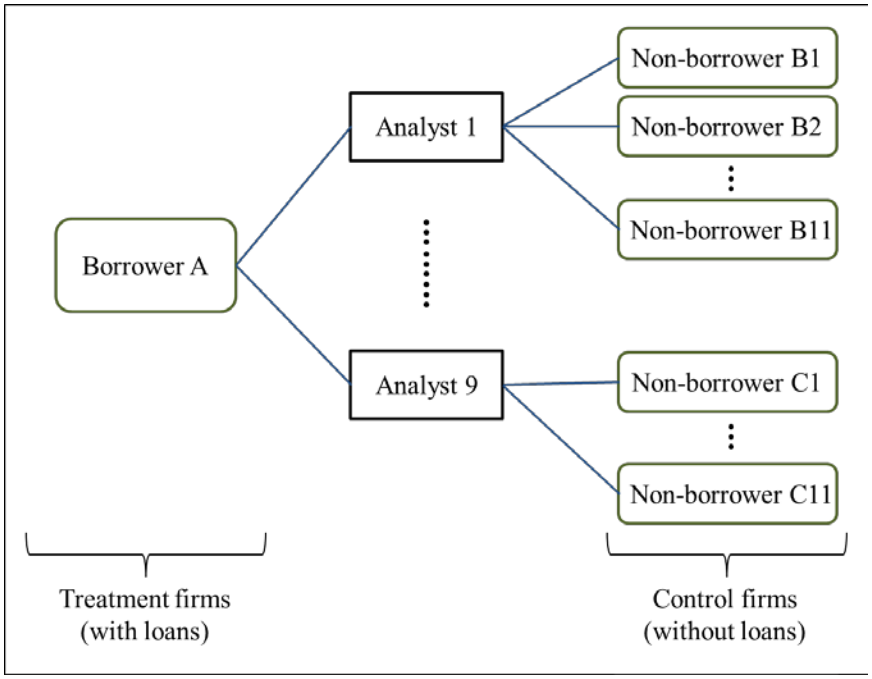
Collateral	An indicator variable that equals to one if the loan contract contains a secured facility and zero otherwise
Reg FD	An indicator variable that equals to one if the loan contract is disclosed after 23 October 2000
Reg 8K	An indicator variable that equals to one if the loan contract is disclosed after 23 August 2004
Indicator [Analyst EBITDA < minimum EBITDA loan]	An indicator variable that takes the value of one if the EBITDA forecasted by the analyst is lower than the EBITDA implicit in the loan contract (see section 4.1 for details)
Indicator [Analyst EBITDA > minimum EBITDA loan]	An indicator variable that takes the value of one if the EBITDA forecasted by the analyst is higher than the EBITDA implicit in the loan contract (see section 4.1 for details)
Analyst EBITDA	Analysts' EBITDA forecast
Analyst EBITDA (CF)	Operating cash flow forecasted by the analyst
Analyst EBITDA (EPS, tax exp)	Implicit EBITDA forecasted by the analyst obtained by multiplying the forecasted EPS by the number of outstanding shares and adding back income taxes, interest expenses and depreciation and amortization from the most recent financial statement available before the analysts' revision
Analyst EBITDA (EPS, tax ratio)	Implicit EBITDA computed as *Analyst EBITDA (EPS, tax exp)* with the exception of using the effective tax rate to compute the estimated taxes instead of the reported tax dollar amount
Forecast error	The absolute value of the difference between the forecasted and the realized earnings per share, divided by the stock price 2 days before the forecast.
Revision Up	An indicator variable that equals to one if the analyst's revision is increasing (i.e., the new forecasted earnings are higher) and zero if the revision is decreasing (i.e., the new forecasted are lower)
Analyst is All Star	An indicator variable that takes the value of one if the analyst is ranked as an All-Star by Institutional Investor magazine at the time of the contract disclosure
Size of the brokerage house	The natural logarithm of the number of analysts working at the same analyst's brokerage house

Appendix B: Sample construction

Sample selection to test H1

We start with 17,927 firms disclosing a loan contract, each followed by (on average) 9 analysts, for a total of 167 thousands treatment observations (i.e., 17,927 × 9). Each analyst follows, on average, other 11 firms, besides firms issuing a loan. Therefore, our control sample is about 11 times bigger than our treatment sample, yielding about 1.7 million control observations (i.e., 17,927 × 9 × 11). Figure B1 provides an illustration of our data structure for a typical loan disclosure event.

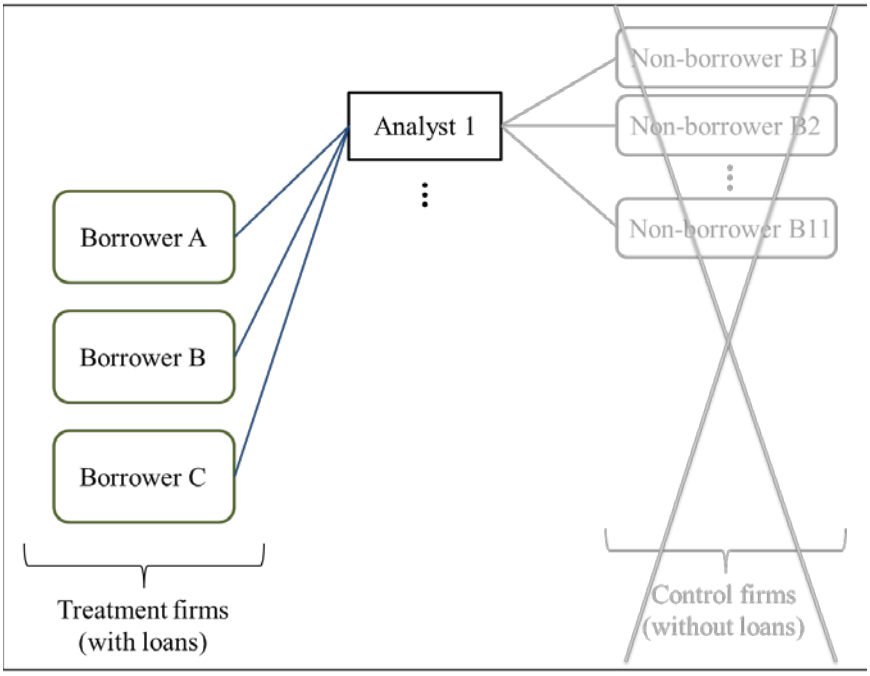
Figure B1 – Sample to test H1



Sample selection to test H2

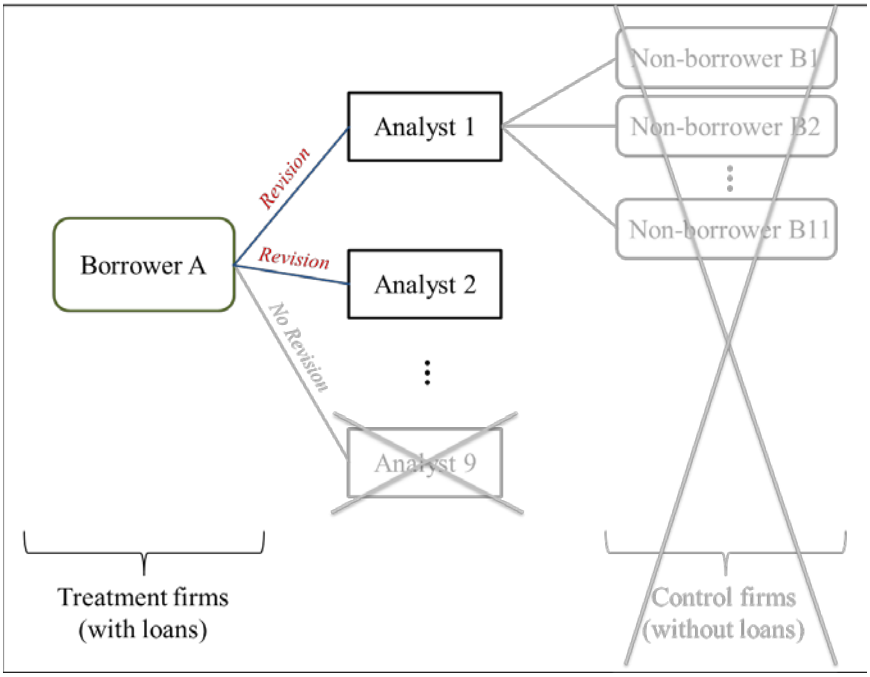
To test H2 (i.e., forecast revision behavior as a function of loan contracts elements) we start with the sample of treatment sample of about 167 thousands observations (i.e., 17,927 loan contracts × 9 analysts). After excluding firms with missing observations on loan contracts (e.g., interest spread, firms reporting no financial covenant), we obtain about 62 thousand observations. These observations represent the sample in Table 3. Figure B2 provides an illustration of our data structure for a typical analyst reviewing multiple loan contracts disclosed.

Figure B2 – Sample to test H2, Table 3



To further test H2, we focus on analysts that did revise their forecasts for the loan disclosing firms and study the direction of such forecast revisions. This reduces our sample from about 62,000 (see above) to about 6 thousand observations (on average, about 9% of analysts revise for loan-disclosing firms, so 9% × 62,000 ~ 6,000 observations). We also need the loan contract to have a minimum EBITDA or debt to EBITDA covenant (about 2/3 of the sample) and analysts to have explicit EBITDA and cash flows forecasts, which leaves us about 700 observations. We also need more than one analyst following each firm, given that we are interested in how analysts with different earnings expectations react differently to the same loan contract. The last restriction gives us about 400 observations (see Table 4, Panel A). To increase our sample size, we estimate an implicit EBITDA starting from the analysts' earnings (EPS) forecasts, which increases our sample from about 400 to almost 3,000 observations (see Table 4, Panel C). Figure B3 provides an illustration of our data structure for a typical loan contract disclosed by a borrower followed by multiple analysts.

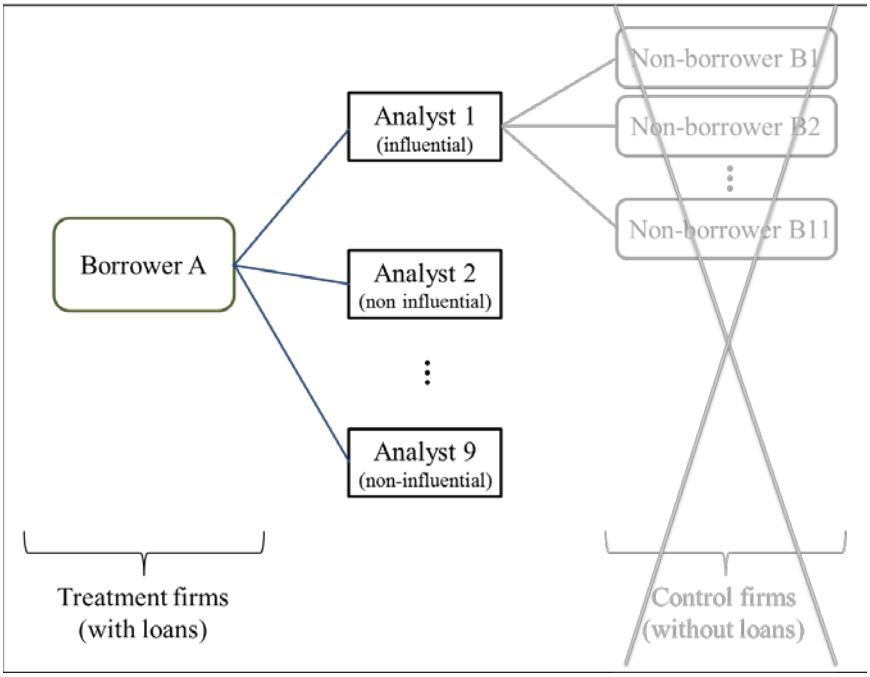
Figure B3 – Sample to test H2, Table 4



Sample selection to test H3

To test the relation between analysts' forecasting behavior and their characteristics, we use the treatment sample (167 thousands observations). Since we are interested in how analysts with different market influences react differently to the same loan contract, we need more than one analyst following each firm, as in the case of H2. This gives us a final sample of about 164 thousands observations (see Table 5). Figure B4 provides an illustration of our data structure for a typical loan contract disclosed by a borrower followed by multiple analysts.

Figure B4 – Sample to test H3



Sample selection to test whether accuracy of the revised forecasts improves

To test analysts' forecast accuracy, we start from our treatment sample (167 thousands observations), and then we require the analysts to have a revision after the loan disclosure (about 10% of the sample), which yields about 16 thousand observations. We compare these revised forecasts to the analysts' outstanding forecasts before the loan disclosures, which gives us about 32 thousand observations (16 × 2). The data structure is similar to the one depicted in Figure B3.

Figure 1 – Identification strategy (timeline)

This figure presents the identification strategy we use in our main and cross-sectional tests. Firm A belongs to the treatment firms group while Firm B belongs to the control firms group, since it does not disclose a loan in the time window although followed by the same analyst *i*.

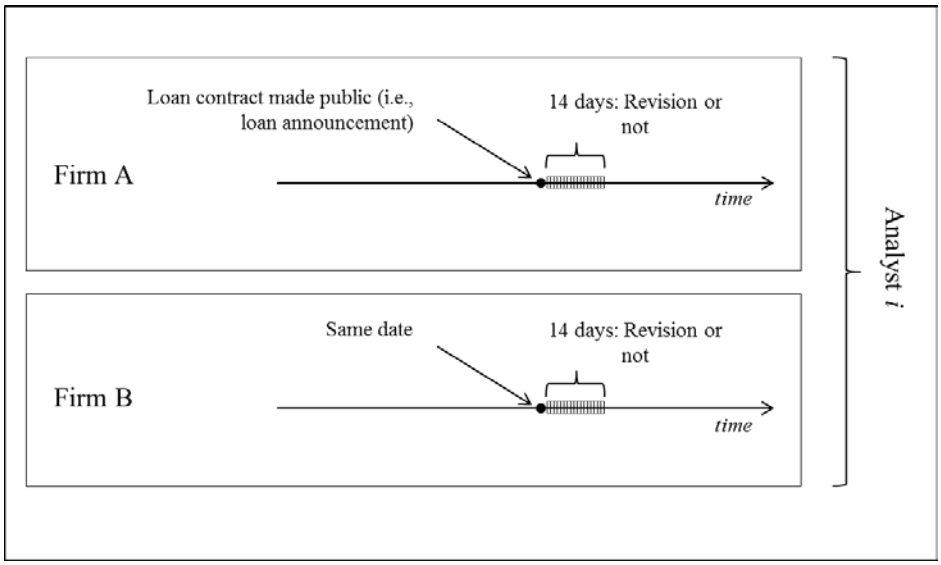


Table 1

This table presents descriptive statistics for the variables that are used in our analysis. The sample consists of 17,927 firms disclosing a loan (i.e., treatment firms) and 796,843 other firms not disclosing a loan but followed by the same analysts (i.e., control firms) from 1995 through 2012. Panel A through C report descriptive statistics at the firm level, while Panel C reports descriptive statistics at the firm-analyst level. The average number of analysts following firms issuing a loan (not issuing a loan) is about 9 (11). Therefore, our final sample includes about 167 thousands treatment observations (i.e., 17,927 × 9) and 1.7 million control observations (i.e., 17,927 × 9 × 11). All variables are defined in the Appendix. The last columns reports the results of a t-test between the mean values of the variables reported in Panel B and C. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

Panel A: Percentage of revisions

Number of treatment firms (i.e., firms disclosing a loan)	17,927
Average number of analysts following each treatment firm	9
Number (Percentage) of firms receiving a revision within the 14-days window	7,572 (42.2%)
Out of the 7,572 firms receiving a revision, average percentage of upward revisions	46.3%

Panel B: Firm-level characteristics - treatment firms

	N	Mean	Median	SD	
Firm size	17,927	7.35	7.18	2.02	***
Market to book	17,927	1.72	1.36	1.04	***
Firm leverage	17,927	0.29	0.28	0.20	***
Firm ROA	17,927	0.12	0.11	0.09	***
Absolute cumulative return	17,927	0.07	0.05	0.07	***
SD returns	17,927	0.03	0.02	0.02	***

Panel C: Firm-level characteristics - control firms

	N	Mean	Median	SD
Firm size	796,843	7.41	7.34	1.73
Market to book	796,843	1.86	1.46	1.16
Firm leverage	796,843	0.27	0.26	0.19
Firm ROA	796,843	0.13	0.13	0.09
Absolute cumulative return	796,843	0.07	0.05	0.07
SD returns	796,843	0.02	0.02	0.02

Panel D: Firm-analyst level characteristics - total sample

	N	Mean	Median	SD
Forecast revision indicator (*Revision*)	1,914,410	0.09	0.00	0.29
Firm size	1,914,410	7.91	7.91	1.68
Market to book	1,914,410	1.79	1.41	1.09
Firm leverage	1,914,410	0.28	0.27	0.19
Firm ROA	1,914,410	0.13	0.12	0.08
Absolute cumulative return	1,914,410	0.06	0.04	0.06
SD returns	1,914,410	0.02	0.02	0.01

Table 2

This table presents results from estimating the following model:

$$Revision_{t,j,i} = \alpha_0 + \alpha_1 Loan\ disclosed_{i,t} + fixed\ effects_{t,j} + X_{i,t}\alpha + \varepsilon_{t,j,i}$$

for each loan (package) *i* at date *t*, and for each analyst *j*. *Revision* is a forecast revision indicator and *Loan disclosed* is an indicator that takes the value of 1 if a firm disclosed a loan in our time window. *Fixed effects* is a vector of loan contract × analyst fixed effects and *X* is a vector of control variables. *Panel A* reports the results for our main regression while *Panel B* reports cross-sectional results based on regulatory changes affecting the use and availability of public information. All 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 - Main regression

	(1) Revision	(2) Revision	(3) Revision	(4) Revision
Loan disclosed	**0.013*** **	**0.013*** **	**0.010*** **	**0.009*** **
	(16.21)	**(16.32)**	**(12.45)**	**(11.95)**
Absolute cumulative returns		0.062***	0.067***	0.061***
		(12.31)	(13.33)	(12.09)
SD returns		0.087**	0.378***	0.225***
		(2.55)	(10.25)	(6.10)
Absolute buy-and-hold abnormal returns around loan inception				0.250***
				(21.41)
Firm size			0.007***	0.008***
			(23.57)	(24.31)
Market to book			-0.004***	-0.004***
			(-9.26)	(-9.85)
Firm leverage			0.012***	0.011***
			(4.36)	(4.29)
Firm ROA			0.021***	0.025***
			(3.89)	(4.55)
Observations	1,912,784	1,912,784	1,912,784	1,902,540
Adjusted R-squared	0.248	0.248	0.249	0.250
Loan contract x analyst FE	Yes	Yes	Yes	Yes

Panel B - Regulatory changes affecting the use and availability of public disclosure

	(1) Revision	(2) Revision	(3) Revision
Loan disclosed	0.004***	0.010***	0.004***
	(3.61)	(5.88)	(3.24)
Loan disclosed x Reg FD	**0.007*** **		**0.007*** **
	(3.46)		**(3.37)**
Loan disclosed x New 8K Disclosure		**0.006*** **	**0.005*** **
		(2.79)	**(2.61)**
Controls	Yes	Yes	Yes
Sample period	1995-2004	2000-2012	1995-2012
Observations	1,280,811	992,220	1,912,784
Adjusted R-squared	0.207	0.299	0.249
Loan contract x analyst FE	Yes	Yes	Yes

Table 3

This table presents results from estimating the following model:

$$Revision_{t,j,i} = \alpha_0 + \alpha_1 Loan\ contract\ feature_{i,t} + X_{i,t}\alpha + fixed\ effects_{t,j,i} + \varepsilon_{t,j,i}$$

for each loan (package) *i* at date *t*, and for each analyst *j*. *Revision* is a forecast revision indicator and *Loan contract feature* is a vector of loan characteristics. *X* is a vector of control variables. *Fixed effects* is a matrix of firm, analyst, and time (i.e., month × year) fixed effects. All 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.

	(1) Revision	(2) Revision
Trip wire covenants	**0.009*** **	**0.010*** **
	(2.81)	**(2.69)**
Alignment covenants	**-0.001**	**0.003**
	(-0.34)	**(0.67)**
Capex restriction		-0.011
		(-1.51)
Dividend restriction		-0.008
		(-1.57)
# of performance pricing		0.003
		(1.20)
# of sweeps		0.002
		(0.85)
Package revolver		-0.018***
		(-3.11)
Loan size		0.007**
		(2.33)
Package maturity		-0.000
		(-0.08)
Loan spread		0.008*
		(1.77)
Collateral		0.004
		(0.60)
Firm Controls	Yes	Yes
Observations	64,997	62,102
Adjusted R-squared	0.089	0.090
Firm FE	Yes	Yes
Analyst FE	Yes	Yes
Year x month FE	Yes	Yes

Table 4

This table presents results from estimating the following model:

$$Revision\ Up_{t,j,i} = \alpha_0 + \alpha_1 low\ EBITDA_{i,t} + fixed\ effects_{t,i} + \varepsilon_{t,j,i}$$

for each loan (package) *i* at date *t*, and for each analyst *j*. *Revision Up* is a forecast revision indicator that takes the value of one (zero) when analysts issue positive (negative) forecast revisions and *Low EBITDA* is an indicator that takes the value of 1 if the EBITDA previously forecasted by the analysts is lower that the EBITDA implicit in the loan contract (see section 4.1 for further details). *Fixed effects* is a vector of loan contract fixed effects. *Panel A* reports the results for the sample of analysts with available forecasts on EBITDA (column 1) and operating CF (column 2) while *Panel C* the results for the extended sample of analysts with available EPS forecasts, which we use to compute an implicit EBITDA (see section 4.1 for further details). *Panel B* reports the correlation matrix among the different EBITDA measures used in Panel A and B. All 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 - Small sample regression

	(1) RevisionUp	(2) RevisionUp
Indicator [Analyst EBITDA < minimum EBITDA loan]	**0.251***	
	(1.81)	
Indicator [Analyst EBITDA (CF) < minimum EBITDA loan]		**0.442*** **
		(2.87)
Firms and loan controls	NA	NA
Observations	386	386
Adjusted R-squared	0.284	0.297
Loan contract FE	Yes	Yes

Panel B - Correlation matrix

		(1)	(2)	(3)	(4)
(1)	Analyst EBITDA	1			
(2)	Analyst EBITDA (CF)	0.85	1		
(3)	Analyst EBITDA (EPS, tax exp)	0.94	0.92	1	
(4)	Analyst EBITDA (EPS, tax ratio)	0.87	0.88	0.96	1

Panel C - Extended sample regression

	(1) RevisionUp	(2) RevisionUp
Indicator [Analyst EBITDA (EPS, tax exp) < minimum EBITDA loan]	**0.201****	
	(2.42)	
Indicator [Analyst EBITDA (EPS, tax ratio) < minimum EBITDA loan]		**0.133****
		(1.99)
Firms and loan controls	NA	NA
Observations	2,932	2,932
Adjusted R-squared	0.408	0.408
Loan contract FE	Yes	Yes

Panel D - Extended sample regression for EBITDA (EPS, tax ratio)

	(1) RevisionUp
Abs distance if [Analyst EBITDA < minimum EBITDA loan]	**0.562*****
	(3.20)
Abs distance if [Analyst EBITDA > minimum EBITDA loan]	**-0.067*****
	(-3.11)
Firms and loan controls	NA
Observations	2,932
Adjusted R-squared	0.416
Loan contract FE	Yes

Table 5

This table presents results from estimating the following model:

$$Revision_{t,j,i} = \alpha_0 + \alpha_1 Analysts\ influence_{i,j} + fixed\ effects_{t,j} + \varepsilon_{t,j,i}$$

for each loan (package) *i* at date *t*, and for each analyst *j*. *Revision* is a forecast revision indicator and *Analysts influence* is either analysts' All-star status (column 1) or the size of their brokerage house (column 2). *Fixed effects* is a vector of loan contract fixed effects. All 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.

	(1) Revision	(2) Revision
Analyst is All Star	**0.018***** **(5.96)**	
Size of the brokerage house		**0.006***** **(5.01)**
Firm and loan controls	NA	NA
Observations	164,385	164,382
Adjusted R-squared	0.116	0.116
Loan contract FE	Yes	Yes

Table 6

This table presents results from estimating the following model:

$$Forecast\ error_{t,j} = \alpha_0 + \alpha_1 After_{t,j} + fixed\ effects_{t,j} + \varepsilon_{t,j,i}$$

for each analyst *j* and time *t*. *Forecast error* is the analysts error implicit in the analyst estimate and *After* is an indicator that takes the value of 1 for the analyst forecast revisions following a loan disclosure. Colum 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 shorter time gaps between original forecasts and revisions. *Fixed effects* is a vector of loan contract × analyst fixed effects. All 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.

	(1) *Forecast error*	(2) *Forecast error*	(3) *Forecast error*	(4) *Forecast error*
After	**-0.003***** **(-11.47)**	**-0.003***** **(-5.79)**	**-0.003***** **(-4.34)**	**-0.005**** **(-2.27)**
Time gap between forecasts and revisions	All	Less than 30 days	Less than 15 days	Less than 7 days
Firm and loan controls	NA	NA	NA	NA
Observations	32,340	7,554	1,838	328
Adjusted R-squared	0.955	0.969	0.985	0.974
Loan contract x analyst FE	Yes	Yes	Yes	Yes