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Do Bank-Affiliated Analysts Benefit from Lending Relationships?

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ABSTRACT

This paper investigates whether private information from lending activities improves the forecast accuracy of bank-affiliated analysts. Using a matched sample design, matching by affiliated bank or borrower, we demonstrate that the forecast accuracy of bank-affiliated analysts increases after the followed firm borrows from the affiliated bank. We also find that the increase in forecast accuracy is more pronounced for borrowers with greater information asymmetry and bad news, and for deals with financial covenants. Last, we find that the informational advantage of bank-affiliated analysts exists only when the affiliated banks serve as lead arrangers, not merely as participating lenders. Overall, our evidence suggests that information flows from commercial banking to equity research divisions within financial conglomerates.

1. *Introduction*

Since the 1990s, the financial industry in the United States has seen a wave of mergers and acquisitions. Much of the consolidation has been spurred by the relaxation of the Glass-Steagall provisions in the late 1980s and peaked after the passage of the Gramm-Leach-Bliley Act (GLBA) in 1999. GLBA repealed the Glass-Steagall Act of 1933, which had separated commercial banking from investment banking and other types of security

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dealing businesses. This financial industry consolidation resulted in conglomerates that not only provide commercial lending service, but also engage in securities dealing and market making businesses. As is well known, commercial banks, thanks to their lending activities, have superior information about borrowers (e.g., Fama [1985], James [1987], Petersen and Rajan [1994]).

In this paper, we investigate whether banks' information advantage benefits their affiliated security analysts by helping them make more accurate earnings forecasts. Specifically, we ask two questions: First, do earnings per share (EPS) forecasts by bank-affiliated analysts for borrowers become more accurate after loan initiation? Second, if so, do we see cross-sectional variation in the improvement of forecast accuracy that is related to borrower characteristics and deal structure?

We investigate these questions using a sample of bank loans and analyst forecasts for the period 1994 to 2007. For purposes of our analysis, we define *conglomerate analysts* as security analysts who are affiliated with a commercial bank within a financial conglomerate. A conglomerate analyst can issue earnings forecasts for firms that borrow from the affiliated bank and firms that do not borrow from the affiliated bank. The former are referred to as *conglomerate forecasts*. We employ a difference-in-difference research design in the paper. Specifically, we make a pre- and post-loan comparison in the accuracy of conglomerate forecasts relative to benchmark forecasts issued by the same analyst for firms that do not borrow from the affiliated bank. We also compare conglomerate forecasts made during the pre- and post-loan period to benchmark forecasts made by nonconglomerate analysts who follow the same borrower.

We document four main findings. First, the accuracy of conglomerate forecasts increases after a firm borrows from the affiliated bank, and this increase is both statistically and economically significant. Relative to benchmark forecasts, conglomerate analysts reduce annual EPS forecast error by 7 cents, which is about one-sixth of the average EPS forecast error in our sample. This result is robust to various model specifications and controls. Second, the increase in the accuracy of conglomerate forecasts after loan initiation is more pronounced for: (1) borrowers with high information asymmetry—that is, those characterized by small size and high standard deviation of analyst EPS forecasts—and for (2) deals with financial covenants and high bank ownership. Third, the informational advantage of conglomerate analysts is concentrated among borrowers with bad news and high credit risk. Fourth, the informational advantage for conglomerate analysts exists only when conglomerates serve as lead arrangers, not merely as participating lenders. Taken together, our results suggest that there is information spillover from the commercial lending divisions to the equity research divisions of financial conglomerates and that bank-affiliated analysts benefit from this information spillover via more accurate forecasts. Although information sharing is beneficial to financial conglomerates, it is not without controversy, particularly when much of the superior information comes from ongoing correspondence between borrowers and banks.[1] In recent years, regulators and market participants have expressed concerns that the spillover of private information into the public domain might breach confidentiality agreements between lenders and issuers and, more importantly, could lead to illegal trading (Standard and Poor's [2008]). Banks have tried to address this concern by establishing limits to the flow of information among different parts of a financial conglomerate, that is, erecting so-called Chinese Walls. Analysts, along with public trading and sales desks that they are associated with, work on the public side of the wall and are therefore not supposed to receive private information. Our findings suggest that, despite the purported existence of Chinese Walls, financial analysts still have access to superior information from lending relationships and exploit this access to improve their forecast accuracy.

In this regard, our study is closely related to several recent papers that investigate the information flow from the lending arm to other divisions of financial conglomerates or sometimes even to outside parties. Acharya and Johnson [2007], for example, provide evidence consistent with the use of private information by informed banks in the credit default swap market. Similarly, Massa and Rehman [2008] show that a subset of bank-affiliated mutual funds benefits from information sharing within a financial conglomerate. Ivashina et al. [2009] find that the probability of a borrower being a target increases when both the acquirer and the target have the same lender, suggesting information sharing between the bank and potential acquirers. Our study adds to this stream of literature by identifying a new channel of information sharing within financial conglomerates and documenting the impact of this information sharing on analyst forecast accuracy.

Our study also relates to several recent papers that document that institutional investors as syndicate lenders trade on borrowers' private information acquired from the loan market (Bushman, Smith, and Wittenberg-Moerman [2010], Ivashina and Sun [2010], Massoud et al. [2011]). Our findings suggest that borrowers' private information can also be capitalized by lender-affiliated security analysts to improve their forecast accuracy.

Our study also adds to the security analyst literature. A considerable amount of literature examines the impact of investment banking ties or brokerage firm affiliation on the properties of analyst forecasts and recommendations (e.g., Lin and McNichols [1998], Michaely and Womack [1999], Bradley, Jordan, and Ritter [2003], and Cowen, Groysberg, and Healy [2006]). However, little research focuses on how commercial banking ties affect the incentives and performance of financial analysts. We fill this void by documenting that bank-affiliated analysts increase forecast accuracy after the initiation of lending relationships between followed firms

[1] The superior information may include material private information such as financial projections and plans for mergers or acquisitions.

and affiliated banks. The accompanying increase in forecast accuracy could enhance the analysts' job security and attract more investment advisory business and trading volume to financial conglomerates.

Finally, our study has implications for policy makers. First, it speaks to the debate regarding the reform of the banking industry in the 1990s, particularly on the repeal of Glass-Steagall Act of 1933. So far, much of the evidence regarding the effects of the repeal has focused on the consequences of housing lending and underwriting under the same roof (e.g., Puri [1996], Gande et al. [1997], Gande, Puri, and Saunders [1999], and Roten and Mullineaux [2002]). Our study, together with Acharya and Johnson [2007], Ivashina et al. [2009], and Massa and Rehman [2008], suggests that conflicts of interest may arise not only when banks combine lending with underwriting but also when they engage in other types of securities businesses such as equity research, trading, and investment advisory service. Second, the U.S. Securities and Exchange Commission (SEC) implemented Regulation Fair Disclosure (FD) on October 23, 2000, to prohibit firms from privately disclosing value-relevant information to select securities markets professionals without simultaneously disclosing the same information to the public. Although private communications between lenders and borrowers are exempt from Regulation FD, sharing of private information with analysts violates the spirit of the regulation and may warrant regulators' attention.

The rest of the paper is organized as follows. Section 2 develops testable hypotheses. Sections 3 and 4 discuss sample selection and research methodology, respectively. Section 5 presents empirical results and additional analyses. Section 6 discusses sensitivity tests, and section 7 concludes.

2. *Literature Review and Hypotheses Development*

2.1 BACKGROUND ABOUT THE REGULATION CHANGES IN THE BANKING INDUSTRY

The Glass-Steagall Act of 1933 prohibited commercial banks from underwriting and dealing in corporate securities. In the 1980s, the banking industry sought to repeal Glass-Steagall. In 1987, the Federal Reserve permitted banks to establish Section 20 subsidiaries to engage in underwriting or dealing "ineligible" securities. However, Section 20 subsidiaries are subject to a substantial set of firewalls that limit information, resource, and financial linkage between them and their parent holding companies as well as their commercial banking affiliates. The Federal Reserve also limited to 5% of the subsidiary's total revenue the amount of revenue that a Section 20 subsidiary could derive from bank "ineligible" activities. The limit was raised to 10% in 1989 and then to 25% in 1996. On November 26, 1999, Congress passed the GLBA, which effectively repealed the Glass-Steagall Act. The GLBA expands the options available for a financial conglomerate to engage in securities underwriting and dealing activities by creating the financial holding company (FHC). Securities subsidiaries of FHCs do not face the revenue constraints of Section 20 and are subject to far fewer firewall constraints.

Over the course of the 1990s and early 2000s, commercial banks acquired or merged with investment banks and brokerage firms on a massive scale. In the United States alone, about 10% of brokerage firms tracked by First Call (referred to as financial conglomerate analysts in this article) were affiliated with commercial banks through the same parent holding companies between 1994 and 2007.

2.2 HYPOTHESES DEVELOPMENT

Prior literature on financial intermediation highlights that banks have superior information about borrowers that is not available to other market participants. Rajan [1992], for example, shows that lending relationships generate valuable information including "soft data" such as insights on the competence of management. Stein [2002] echoes this point and argues that the unique characteristic of small-business lending is that banks rely on the "soft data" generated by the lending relationship. Petersen and Rajan [1994] similarly show that lending relationships reduce the asymmetric information problem between the firm and its lender, with the positive effect of expanding the availability of credit to firms. Chemmanur and Fulghieri [1994] argue that banks have an incentive to spend resources obtaining private information and monitoring their borrowing firms' activities. Doing so enables them to evaluate whether to liquidate the firm or renegotiate its loans when a firm undergoes financial distress.

Given that most of the information obtained from lending activities is nonpublic and material, it is not surprising that regulators have long required banks to erect "Chinese Walls" to limit the flow of this information within financial conglomerates. However, evidence from prior studies suggests that Chinese Walls may not be totally effective in preventing information spillover from the lending division to other divisions of a financial conglomerate. Massa and Rehman [2008] find that lending relationships affect portfolio allocation of bank-affiliated funds in a way that is consistent with bank-affiliated funds exploiting superior information acquired from the lending side of their parent company. Ivashina et al. [2009] also document information spillover: they find that the probability of a borrower being a target increases when both the acquirer and the target borrow from the same lender, suggesting information sharing between the lender and the acquirer.

We hypothesize that information spillover may also exist from the lending division to the equity research division within financial conglomerates. Inside information is valuable to financial analysts as they have a strong incentive to forecast accurately. Keane and Runkle [1998, p. 769] note that "... financial analysts' livelihoods depend on the accuracy of their forecasts ..." Furthermore, Hong, Jeffrey, and Solomon [2000] show that inaccurate earnings forecasts threaten security analysts' careers. Consequently, when a brokerage firm is affiliated with a financial conglomerate, its research analysts would have strong motivation to obtain private information from the lending division about any borrower for which they make forecasts.

Accurate forecasts can also benefit conglomerates by generating more trading volume and attracting more investment advising business; Alford and Berger [1999], for example, document a positive relation between brokerage commissions and analyst forecast accuracy. Such benefits can, to some extent, disincentivize financial conglomerates from strictly enforcing their Chinese Walls.[2]

Of course, any benefits reflected in more accurate equity research need to be balanced against potential litigation and reputational costs associated with breaching the Chinese Walls. However, we argue that during our sample period, the benefits from information spillover were likely to exceed potential costs for at least three reasons. First, insider trading can be difficult to prove in court.[3] Insider trading generally requires proof of breaching a duty either based on a relationship or a confidentiality agreement. Furthermore, evidence of insider trading is seldom straightforward. Instead, it tends to be circumstantial and subject to inference and interpretation.[4] Second, although borrowers may bring cases against banks if they suspect that the banks misuse the confidential lending information, they may have little incentive to do so when their capital providers are on the giving end and research analysts (rather than their competitors or potential suitors) are on the receiving end. Dass and Massa [2009] show that close banking relationships increase borrowers' firm value through banks' active monitoring, which further reduces borrowers' motive to sue conglomerates. Third, due to the lack of staff at the SEC, the regulator rarely brings enforcement actions against banks for misusing lending information in equity research. These lines of argument lead to our first hypothesis, formally stated as follows:

H1: The accuracy of conglomerate forecasts increases after loan initiation.

[2] Note that information spillover from the lending division to the equity division does not necessarily diminish the opportunities for financial conglomerates to capitalize their information advantage through proprietary trading because they can time the release of analyst forecasts and proprietary trading.

[3] For example, recently, a federal judge dismissed a high-profile insider-trading case against a Deutsche Bank salesman and a hedge-fund trader, against whom the SEC brought enforcement action on the account that they shared confidential information about a leveraged buyout deal and then traded credit swaps based on that information. In dismissing the case, the U.S. District Judge John Koeltl wrote that, "While the SEC attempts to attribute nefarious content to those calls through circumstantial evidence, there is, in fact, no evidence to support this inference and ample evidence that undercuts the SEC's theory that the defendants engaged in insider trading." (Hurtado and Weidlich [2010]).

[4] We thank Hillary Sale, a law professor at Washington University in St. Louis and corporate lawyer, for discussion and insights relating to the legal issues involved in insider trading.

When information asymmetry between firms and outsiders is large, banks are likely to expend more resources and effort in screening and monitoring these firms due to the lack of publicly available information. For this reason, a lending relationship is likely to generate more private information for opaque firms compared with transparent firms. Consistent with this view, Petersen and Rajan [1994], Berger and Udell [1995], and Bharath et al. [2007] argue that durable relationships between lenders and borrowers can attenuate information asymmetry for smaller firms. Slovin, Johnson, and Glascock [1992] find that, for small firms, both loan initiation and renewal are associated with positive abnormal returns when announced, while for large firms, neither initiation nor renewal has significant excess returns. Based on this line of reasoning, we posit that lending relationships have a greater impact on the accuracy of conglomerate forecasts when borrower information asymmetry is high. The second hypothesis is stated formally as follows:

H2: The accuracy of conglomerate forecasts increases more after loan initiation for borrowers with high information asymmetry.

Because creditors' claims are more sensitive to bad news than good news (Smith [1979]), banks are naturally more alert about potential deteriorations in borrowers' financial positions. In addition, loan contracts often contain mechanisms (i.e., financial covenants) through which bad news is revealed to lenders on a more timely basis than good news. Consistent with these arguments, Acharya and Johnson [2007] show that the information revealed by the credit default swap market is asymmetric and consists mainly of bad news. Allen, Guo, and Weintrop [2008] find that the information about earnings is reflected in loan prices four to five weeks prior to public earnings announcements and the preannouncement price movement is more pronounced for firms with bad news. Based on these arguments, we posit that the information effect of lending relationships on the accuracy of conglomerate analyst forecasts is more pronounced when firms experience bad news. Our third hypothesis is formally stated as follows:

H3: The accuracy of conglomerate forecasts increases more after loan initiation for borrowers with bad news.

When a loan contract imposes financial covenants, borrowers are required to provide syndicate lenders timely covenant reports that often preempt information relevant to loan pricing in upcoming quarterly earnings releases (Allen, Guo, and Weintrop [2008]). Furthermore, lenders are more likely to include financial covenants in loan contracts for more informationally opaque borrowers (Standard and Poor's [2008], Bradley and Roberts [2004], Chava, Kumar, and Warga [2010], Bushman, Smith, and Wittenberg-Moerman [2010]). Both arguments suggest that syndicate lenders have a greater information advantage when a loan contract includes financial covenants. Therefore, our fourth hypothesis is stated as follows:

H4: The accuracy of conglomerate forecasts increases more after loan initiation when a loan includes financial covenants.

The majority of the loans issued during our sample period and covered by Dealscan are syndicated loans. An important feature of a loan syndicate is that the lead arranger and the other participating banks play different roles and receive different information. Lead arrangers establish and maintain a relationship with the borrower by taking on the primary role of information collection and monitoring. In contrast, the other participants rarely directly negotiate with the borrower, maintaining an arm's-length relationship and communicating through the lead arranger (Sufi [2007]). Bushman and Wittenberg-Moerman [2009] note that soft information collected by the lead arranger in the process of screening and monitoring the borrower is not available to uninformed investors. Because soft information is costly to process, the lead arranger may have an incentive not to disclose it to other syndicate participants in order to retain an information advantage. Because the possession of information advantage by banks is the key argument underlying our previous hypotheses, we restrict attention to single-lender loans or syndicated loans where financial conglomerates act as lead arrangers when testing H1–H4.

One could argue that every member of a syndicate has the same right to the information gathered by the lead arrangers and that this information is actively shared electronically (Acharya and Johnson [2007]). Therefore, participating lenders can have private information about a borrower that resembles what the lead arranger has. If this is true, the forecast accuracy of financial conglomerates that act as participating lenders may also increase after a loan initiation. Given the two-sided argument regarding the information advantage of the participating lenders, we state the next hypothesis in the null form.

H5: The forecast accuracy of a financial conglomerate does not increase after loan initiation when a conglomerate acts as a participating bank in a syndicated loan.

3. Data and Sample Construction

3.1 SAMPLE BANKS AND THEIR SECURITIES SUBSIDIARIES

We are interested in instances where a firm borrows from a financial conglomerate and is covered concurrently by an analyst affiliated with that financial conglomerate. We start our sample selection with a list of financial conglomerates and their securities subsidiaries obtained from the Federal Reserve Board.[5] There are 85 securities subsidiaries, associated with 62 financial conglomerates, as of December 2008. We then manually match these securities subsidiaries with broker names in First Call's Broker ID file.

[5] http://www.federalreserve.gov/generalinfo/subsidiaries/

We identify 32 matches. These 32 securities subsidiaries, along with the 27 financial conglomerates that they are affiliated with, constitute our initial list of sample banks. Panel A of appendix A provides a list of these financial conglomerates and their securities subsidiaries.

Many of the sample banks and their securities subsidiaries are products of mergers and acquisitions throughout the 1990s and beyond. This adds at least two complications to our data construction: first, when measuring lending relationships between a bank and its borrowers, we need to account for lending relationships inherited via acquisitions. Second, some securities subsidiaries were acquired by their parent banks during our sample period. In these cases, we need to ensure that the potential information sharing between a securities subsidiary and its parent bank's lending unit did not start until the acquisition was completed. To tackle these complications, we use the Securities Data Company (SDC) Mergers and Acquisitions database to identify all merger/acquisition transactions involving our sample banks or their predecessors that were completed between 1991 and 2008.[6] Based on the SDC data, we then construct a merger history tree for each of our sample banks, focusing on those transactions that would have implications for our data construction. Figure 1 provides an illustration of the merger history tree using Bank of America as an example. We refer back to this example when we later explain how the aforementioned two complications are dealt with based on the merger history compiled in this step.

3.2 BANK LOANS

Bank loans are obtained from the Loan Pricing Corporation's (LPC) Dealscan database. The database provides detailed information on syndicated loans and single-lender loans, including lenders' identities, loan sizes and maturities, facility start and end dates, etc. We focus on loans initiated by the sample banks between January 1, 1994, and December 31, 2007, to nonfinancial firms (excluding firms identified with two-digit SIC codes 60 through 70). The cutoff date of December 31, 2007, is chosen because we require at least one earnings forecast to be issued after a loan inception and EPS forecasts data from the First Call end in 2008. We choose January 1, 1994, as the beginning date of loan initiation for two reasons: first, most financial conglomerates were formed in the 1990s; second, the coverage of First Call was not stable until 1993.

To account for the lending relationship inherited from acquired banks, we assume that acquiring banks assume the lending relationships of their targets when a merger/acquisition is completed.[7] Besides the above

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[6] The reason we start searching mergers and acquisitions deal announcements in the financial industry from 1991 is to ensure the accuracy of identifying conglomerate analysts. Our sample analyst forecasts start from 1993 and it usually takes about one or two years to complete an acquisition.

[7] For example, Bank of America acquired FleetBoston on October 27, 2003 as illustrated in figure 1. If there are any outstanding loan deals between FleetBoston and its borrowers as

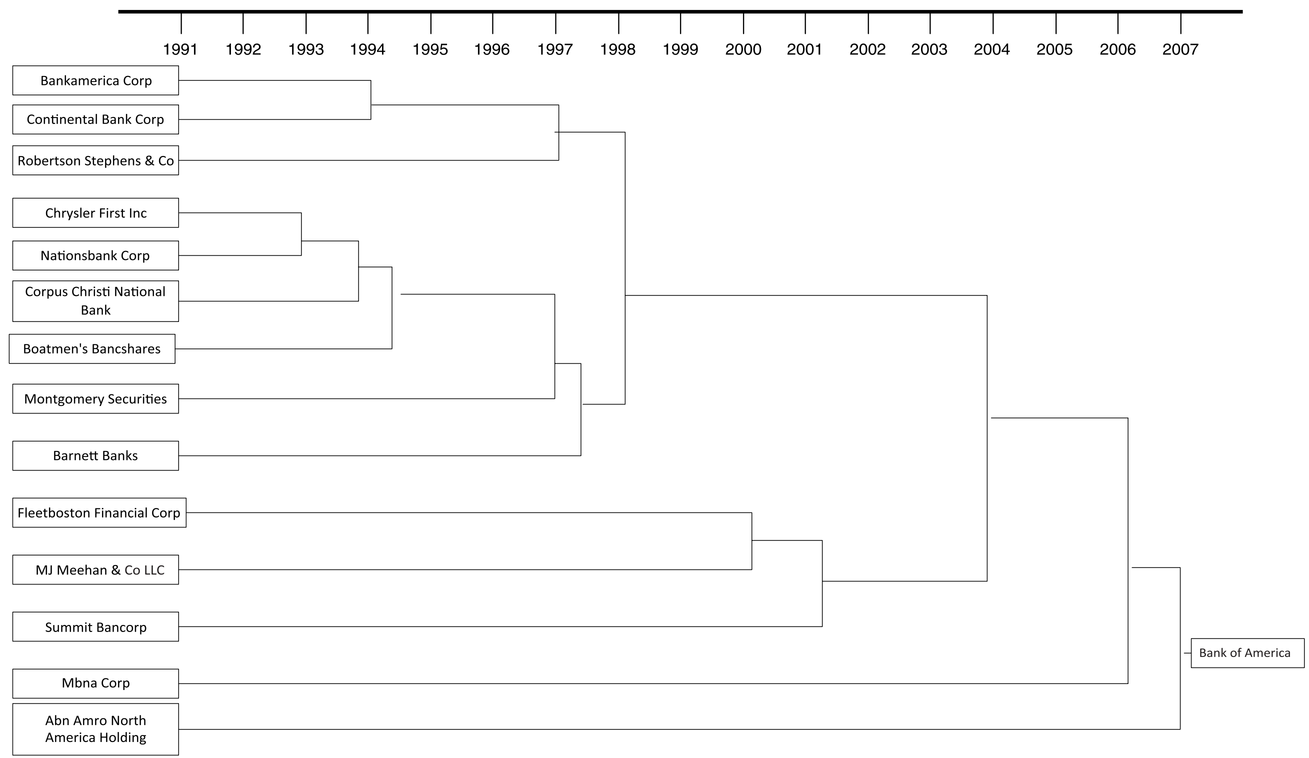

FIG. 1.—Merger history tree of Bank of America. Figure 1 depicts the mergers and acquisitions conducted by Bank of America as an illustration for the development of financial conglomerates consolidating brokers and commercial banks over 1990s and 2000s around the repeal of the Gramm-Leach-Bliley Act.

requirements, a deal-lender facility is included in our sample if it also satisfies the following criteria: (1) borrowers are public nonfinancial U.S. companies and their financial information is available in the Compustat annual database,[8] (2) it is the first lending relationship between the lender and the borrower since 1990 or the maturity date of the previous loan and the starting date of the current loan for the same lender-borrower pair are at least one year apart,[9] (3) the lending bank is either the sole lender or the lead arranger in a syndicated loan, [10,11] and (4) the lending bank owns at least 10% of a loan.[12]

3.3 ANALYSTS FORECASTS DATA

We obtain analysts' annual EPS forecast data from the First Call Historical Database.[13] The actual earnings are also collected from First Call to be consistent with forecasts in the treatment of nonrecurring items. For each deal-lender facility identified in the previous step, we gather conglomerate analyst forecasts (if any) for the borrowing firm made during the one-year period prior to and after a loan initiation date.[14] We delete those loan deals

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of October 27, 2003, we assume that this relationship is inherited by Bank of America and set October 27, 2003 as the loan initiation date for all the outstanding loans. This assumption is based on the belief that the affiliated analysts of acquiring banks will not have access to the private information of acquired banks' borrowers until the completion of acquisitions.

[8] We thank Michael Roberts for providing Dealscan-Compustat link data. For details on the construction and usage of the data, please see Chava and Roberts [2008].

[9] We impose this requirement because these deals are more likely to generate a substantial amount of new information for lending banks and there is less ambiguity in attributing the information effect to the current loan rather than the previous one.

[10] We classify a lender as a lead arranger if the lender's role is not participant, technical, packager, or secondary investor.

[11] In testing H5, we require that financial conglomerates are participant lenders instead of lead arrangers. All other sample selection procedures remain the same. Specifically, we require a sample deal with a financial conglomerate as a participating lender and this deal is either the first deal or a deal whose start date is at least one year apart from the maturity date of the prior deal for the same lender-borrower pair. Note that the affiliating bank can be a lead arranger or a participating lender in the prior deal. Furthermore, all sample deals are still required to have at least one lead arranger with 10% ownership because participant lenders rely on information provided by lead arrangers (Jones, Lang, and Nigro [2005]) and higher loan ownership increases lead arrangers' incentive to acquire private information to monitor borrowers. Then, we obtain earnings forecasts for both affiliated analysts and matched benchmarks (matched by affiliating banks or borrowing firms) and form the broker-constant sample and the firm-constant sample, respectively.

[12] Criteria 3 and 4 are imposed to make sure that lending banks have incentives to gather necessary information on the borrowing firms, which is the primary condition for testing the first four hypotheses. Similar criteria are imposed in prior studies (e.g., Massa and Rehman [2008], Mora and Sowerbutts [2008]).

[13] If an analyst issues multiple annual EPS forecasts with different forecasting period ends on the same day, we keep the one with the closest forecasting period end.

[14] Following Massa and Rehman [2008], who examine holdings of bank-affiliated mutual funds over a six-month period prior to and after a loan initiation, we choose a one-year period to investigate analyst earnings forecasts. Furthermore, forecasts issued in the year before loan

where conglomerate analysts provide no forecasts for the borrowing firms during the one-year period either before or after the loan initiation day.

Table 1 summarizes our sample selection process in detail. After imposing the above selection criteria, our final loan sample contains 418 unique deal-lender facilities and 382 unique loan facilities. During our sample period (1994 through 2007), there are 24,988 unique loan facilities in the LPC database where borrowers are Compustat nonfinancial firms and 7,109 unique loan facilities with loan ownership data available. The total loan value of these 7,109 deals amounts to US$1.65 trillion. Of these, 4,396 (US$1.41 trillion) deals have at least one lead arranger with an equity research division and 1,257 (US$0.64 trillion) deals have at least one lead arranger whose analysts issue conglomerate forecasts. Thus, our final sample of 382 unique loan facilities, which amounts to $US0.26 trillion, is equivalent to 40% of the value of the loans with conglomerate forecasts and ownership data available that are more relevant for our study. While economically significant, our results need to be interpreted with caution with regard to the representativeness of our findings for all banks issuing conglomerate forecasts.

4. *Methodology*

4.1 MATCHED SAMPLE DESIGN

We are interested in whether private information generated by lending activities helps financial-conglomerate analysts improve their forecast accuracy. Part of our research design involves comparing the accuracy of conglomerate forecasts in the post-loan-initiation period to that in the pre-loan-initiation period. However, such pre-/post- comparison may be confounded by changes in lender or borrower characteristics or both or a general trend in forecast accuracy. To mitigate these confounding factors, we employ a matched sample design and construct two sets of benchmark forecasts to compare with the conglomerate forecasts.

The first set of benchmark forecasts, defined as the broker-constant sample, consists of forecasts made by the same conglomerate analysts for matching nonborrowing firms. A set of matching nonborrowing firms is identified for the borrowing firm in a deal-lender facility in the following way. First, we identify all nonborrowing firms followed by the analysts in the same financial conglomerate during the loan initiation year that are within the same industry (measured by two-digit SIC code) as the borrowing firm. Nonborrowing firms are defined as firms in the Compustat universe that do not borrow from any of the 27 financial conglomerate banks listed in panel A of appendix A. We further require that the selected nonborrowing firms be followed by the bank-affiliated analysts during both the pre- and

initiation will not be affected by the information obtained from previous lending relationship due to the requirement of one-year separation between two adjacent loans.

TABLE 1
Sample Selection

	Broker-Constant Sample	Firm-Constant Sample
Sample selection:		
No. of unique syndication deal/facility matched to Compustat	24,988	24,988
No. of syndication deal/facility lenders matched to Compustat	192,308	192,308
No. of deal-lenders initiated after 1999 for which lead arrangers have greater than 10% ownership and for which lenders are not participant, technical, packager, and secondary investors	7,777	7,777
No. of deal-lenders with lead arrangers who are financial conglomerates	3,384	3,384
No. of first deal–lenders or deal-lenders whose facility start dates are at least three years away from the maturity date of the lender's previous deal with financial conglomerates	418	418
No. of first deal–lenders	345	345
No. of non–first deal–lenders having facility start date after facility end date of previous deal	73	73
No. of deal-lenders successfully matched by the broker (the borrower)	418	376
Final sample:		
No. of forecasts from First Call issued by financial conglomerate analysts during the three years before and after the first loan initiation	4,509	4,041
No. of forecasts issued by the same analysts for firms who did not borrow from a financial conglomerate over the sample period	19,363	16,627
No. of analyst forecasts with nonmissing Compustat control variables	23,872	20,668
Unique loans in the sample	382	376
Unique loan deal lenders	418	376
Unique firms in the sample	1,151	302
Unique firms borrowing from financial conglomerates	310	302
Unique firms not borrowing from financial conglomerates	841	–
Unique brokers	16	120
Unique financial conglomerate broker	16	16
Unique nonfinancial conglomerate brokers	16	104

Table 1 describes the sample selection procedure from Dealscan for syndicated loans, First Call for analyst forecasts, and Compustat for financial variables.

post-loan initiation periods and have financial data available from Compustat. Among the firms that satisfy these criteria, we choose five with the closest total assets to the borrowing firm at the fiscal year-end prior to loan initiation as the matching firms.[15] This matching yields 1,868 unique matched

[15] Lee [1997], Lyon, Barber, and Tsai [1999], and Chan, Ikenberry, and Lee [2004]

pairs for the 418 deal-lender facilities previously identified. The loan initiation year of the borrowing firm is hypothetically assigned to the matching nonborrowing firm. As shown in table 1, conglomerate analysts issued a total of 19,363 unique forecasts for the matching nonborrowing firms and 4,509 unique forecasts for the borrowing firms during the one-year period prior to and after loan initiation. These 23,872 unique forecasts constitute our broker-constant sample. This sample has the advantage of controlling for the impact of brokerage characteristics and any general trend in analyst forecast accuracy.

The second set of benchmark forecasts, defined as the firm-constant sample, consists of forecasts made by matching nonconglomerate analysts for the same borrowing firms. Specifically, for each deal-lender facility, we first identify all nonconglomerate analysts that follow the same borrowing firm during both the pre- and post-loan initiation periods. Among these analysts, we choose the five analysts with the closest number of firms being followed (in the deal initiation year) compared with the conglomerate analyst.[16,17] We find matching nonconglomerate analysts for 376 out of the 418 deal-lender facilities previously identified. These nonconglomerate analysts issued a total of 4,041 unique forecasts for the borrowing firms during the two-year period surrounding the loan initiation, while conglomerate analysts issued a total of 16,627 unique forecasts. These 20,668 forecasts comprise our firm-constant sample. The advantage of this matching procedure is that it controls for correlated omitted borrower characteristics and any general trend that may affect analyst forecast accuracy.

4.2 REGRESSION MODELS FOR LENDING RELATIONSHIP AND CONGLOMERATE ANALYST FORECAST ACCURACY

Our first set of tests investigates whether the accuracy of conglomerate forecasts improves relative to that of benchmark forecasts after loan initiation. We estimate the following OLS model for the broker-constant sample and the firm-constant sample, respectively. To control for invariant firm/broker characteristics and year-specific shocks that may affect analyst forecast accuracy, we include firm and year fixed effects for the broker-constant sample and broker and year fixed effects for the firm-constant sample in all estimations. In addition, heteroskedasticity-consistent standard errors clustered at the firm level are used to derive p-values.

$$ERROR = \beta_0 + \beta_1 POST + \beta_2 CONGLOMERATE * POST + \gamma\, CONTROLS + \varepsilon \tag{1}$$

suggest that using one control firm leads to noisy point estimates. Because our paper focuses on testing corporate finance theory, the noise from low-power methods is of primary concern. Therefore, we follow their suggestion and use multiple control firms to reduce the noise in single benchmark and increase the power of our tests.

[16] Our results are qualitatively similar when each deal-lender facility is matched with one or three nonborrowing firm(s) for the broker-constant sample and one or three nonconglomerate analyst(s) for the firm-constant sample.

[17] For borrowers with fewer than five analysts following, we use all these analysts as benchmarks for the conglomerate analyst in the firm-constant sample.

where *ERROR* is measured as the absolute difference between an analyst forecast and actual earnings deflated by the stock price at the beginning of the forecast month (Mikhail, Walther, and Willis [1999]). *POST* is a dummy variable that equals 1 if a forecast is issued during the post-loan-initiation period and 0 otherwise. Recall that nonborrowing firms assume the loan initiation date from their matched borrowing firms in the broker-constant sample. *CONGLOMERATE* is a dummy variable that equals 1 for conglomerate forecasts and 0 for benchmark forecasts. Due to the way that we constructed our benchmark samples, there is no variation in *CONGLOMERATE* within a firm for the broker-constant sample and no variation in *CONGLOMERATE* within an analyst for the firm-constant sample. Consequently, firm fixed effects and broker fixed effects subsume the estimation of *CONGLOMERATE* for both samples.[18,19] Although the coefficient on *CONGLOMERATE* cannot be estimated in our fixed effects regressions due to its time-invariant nature, the estimation of our main variable of interest, *POST* $*$ *CONGLOMERATE* is not affected by the inclusion of firm/broker fixed effects since *CONGLOMERATE* is interacted with *POST*, which changes over time. Based on H1, we expect the coefficient on *POST* $*$ *CONGLOMERATE* to be negative (i.e., $\beta_2 < 0$).

CONTROLS includes a set of firm characteristics and forecast characteristics identified in the prior research that are associated with forecast accuracy. First, firm characteristics include log market value (*LOGMKT*), market-to-book ratio (*MB*), the probability of loss (*PLOSS*), and a dummy for earnings increase *(EPSUP)*. We expect larger firms, and firms with lower *MB*, lower probability of losses, and earnings increases to have more accurate earnings forecasts. Second, we include the number of analysts following a firm (*LOGNUMANALYST*) in year t as another control variable. On the one hand, prior research finds that consensus forecast accuracy is positively associated with the number of analysts following a firm (Alford and Berger [1999]). To the extent that consensus forecast accuracy reflects the individual analyst ability, it implies a negative relation between the number of analysts following and forecast error. On the other hand, Bhushan [1989] argues that investor demand for analyst coverage is greater for firms with greater share price volatility, because the potential investor gains from firm-specific information are greater for these firms. If analyst coverage is positively related to stock price volatility as Bhushan argues, we may instead observe a positive association between the number of analysts following and forecast error because greater price volatility can lead

[18] The inability to estimate the coefficients on the time-invariant variables has long been recognized as a disadvantage of the fixed effects model. As Greene [2008, p. 194] points out, "this lack of identification is the price of the robustness of the specification to unmeasured correlation between the common effects and the exogenous variables."

19 All our results (except for the forecast optimism test based on the broker-constant sample) are robust to the exclusion of firm or broker fixed effects, where the coefficient on *CONGLOMERATE* can be separately estimated.

to larger forecast errors. Third, many prior papers identify the age of forecasts as being negatively associated with forecast accuracy (e.g., Brown et al. [1987], O'Brien [1988], Clement [1999]). We use *HORIZON*, measured as the number of days between the forecast date and the earnings announcement date, to control for this effect. We also control for the number of months for which an analyst has been following a firm before the current year (*LOGEXP*) and expect it to be negatively associated with forecast error (Mikhail, Walther, and Willis [1997], Clement [1999]). The effect of analyst learning on forecast accuracy could diminish with experience. Therefore, we include a square term of experience and expect a positive coefficient on this term. All firm-level control variables are measured at the end of the fiscal year prior to the issuance of an EPS forecast. A more detailed description of these variables and their measurement is provided in appendix B. All continuous variables are winsorized at the 1% and 99% levels.

H2 predicts that increases in the accuracy of conglomerate forecasts are more pronounced for borrowers with high information asymmetry. To test this hypothesis, we employ two measures of information asymmetry used in the previous literature. The first measure is total market value of equity measured at the fiscal year prior to loan initiation (*LOGMKT*); the second measure is the standard deviation of analyst annual earnings forecasts made in the fiscal year prior to loan initiation (*STD DEV*). Using these two measures, we estimate model 1 for the two subsamples partitioned based on the sample median of *LOGMKT* or *STD DEV*, respectively.

If H2 holds, we would expect the coefficient on *CONGLOMERATE* $*$ *POST* to be more pronounced for the subsample of small firms (i.e., firms with *LOGMKT* below the corresponding sample median) and the subsample of firms with a high standard deviation of analyst earnings forecasts (i.e., firms with *STD DEV* above the corresponding sample median).

Furthermore, H3 and H4 predict that increases in the accuracy of conglomerate forecasts are more pronounced for borrowers with bad news and for loans with financial covenants. To test the bad news hypothesis, we partition the broker-constant sample (the firm-constant sample) into two subgroups based on positive or negative stock return of a borrower in the year that an EPS forecast is issued, where the annual stock return is cumulative abnormal stock return over a fiscal year. To test the covenant hypothesis, we adopt a similar approach and partition the broker-constant sample (the firm-constant sample) into two subgroups based on whether a loan contains a financial covenant. We then rerun model 1 for both subgroups under each partition. If these two hypotheses hold, the coefficient on *CONGLOMERATE* $*$ *POST* should be greater for the subgroup of firms with bad news and for the subgroup of loans with financial covenants.

H5 tests whether forecast accuracy increases after loan initiation for financial conglomerates that act as participant lenders. To test this hypothesis, we construct a parallel broker-constant sample and firm-constant sample for participant lenders as described in footnote 11 and reestimate model 1 for the new samples.

TABLE 2
Summary Statistics

Variable	*N*	Mean	Std Dev	Lower Quartile	Median	Upper Quartile
Panel A: Broker-constant sample						
ERROR	23,872	0.014	0.028	0.001	0.004	0.013
CONGLOMERATE	23,872	0.189	0.391	0.000	0.000	0.000
POST	23,872	0.508	0.500	0.000	1.000	1.000
MKT ($million)	23,872	9,316	19,624	997	2,907	8,780
ASSETS ($million)	23,872	8,949	21,527	879	2,623	8,150
MB	23,872	3.288	3.189	1.689	2.512	4.051
LEVERAGE	23,872	0.258	0.192	0.117	0.249	0.370
PLOSS	23,872	0.133	0.121	0.052	0.092	0.173
EPSUP	23,872	0.571	0.495	0.000	1.000	1.000
NUMANALYST	23,872	11.229	5.971	6.650	10.366	15.133
STD DEV	20,477	0.093	0.126	0.025	0.049	0.105
HORIZON (days)	23,872	297.688	195.488	156.000	273.000	366.000
EXPERIENCE (months)	23,872	59.256	47.202	20.000	48.000	87.000
BADNEWS	23,872	0.555	0.497	0.000	1.000	1.000
INVEST	23,872	0.373	0.484	0.000	0.000	1.000
BANK SHARE (%)	4,509	24.912	21.395	12.000	16.027	27.000
FINCOVENANT	4,509	0.624	0.484	0.000	1.000	1.000
NCOVENANT	4,509	1.213	1.152	0.000	1.000	2.000
Panel B: Firm-constant sample						
ERROR	20,668	0.012	0.028	0.001	0.003	0.010
CONGLOMERATE	20,668	0.196	0.397	0.000	0.000	0.000
POST	20,668	0.510	0.500	0.000	1.000	1.000
MKT ($million)	20,668	10,537	19,024	1,263	3,675	11,007
ASSETS ($million)	20,668	8,277	12,826	1,186	3,283	9,878
MB	20,668	3.561	3.480	1.845	2.615	4.129
LEVERAGE	20,668	0.275	0.171	0.163	0.260	0.367
PLOSS	20,668	0.104	0.092	0.045	0.075	0.133
EPSUP	20,668	0.574	0.494	0.000	1.000	1.000
NUMANALYST	20,668	11.998	6.027	7.667	10.960	15.230
STD DEV	17,694	0.075	0.100	0.023	0.043	0.088
HORIZON (days)	20,668	301.071	193.106	161.000	277.000	369.000
EXPERIENCE (months)	20,668	72.105	53.983	24.000	59.000	118.000
BADNEWS	20,668	0.534	0.499	0.000	1.000	1.000
INVEST	20,668	0.504	0.500	0.000	1.000	1.000
BANK SHARE (%)	4,041	24.757	21.563	12.000	16.000	25.000
FINCOVENANT	4,041	0.624	0.484	0.000	1.000	1.000
NCOVENANT	4,041	1.224	1.165	0.000	1.000	2.000

Panels A and B present summary statistics for all variables in the empirical analysis based on the broker-constant sample and the firm-constant sample, respectively. All variables are defined in appendix B.

5. *Empirical Results*

5.1 SUMMARY STATISTICS

Table 2 reports the summary statistics for the main variables for the broker-constant sample and the firm-constant sample. The summary statistics for all variables are similar across the two samples, so we only focus on the broker-constant sample. Analyst forecast errors (*ERROR*) are right skewed with a mean of 0.014 and a median of 0.004, comparable with that reported in Mikhail, Walther, and Willis [1999]. As a result of our research design, the number of benchmark forecasts (81.1% of the broker-constant sample) is slightly less than five times that of conglomerate forecasts (18.9% of the sample). On average, bank-affiliated analysts issued similar numbers of forecasts in the one-year period before and after the loan initiation. Two size variables are also highly skewed: the mean (median) market value (*MKT*) is 9,316 (2,907) millions and the mean (median) asset size (*ASSETS*) is 8,949 (2,623) millions.[20] The mean (median) *MB* is 3.29 (2.51). The average predicted probability of loss (*PLOSS*) for the broker-constant sample is about 13.3%, while 57.1% of observations report an earnings increase (*EPSUP)* during the fiscal year prior to analyst forecasts. There are, on average, 11 analysts following the sample firms over the course of a fiscal year *(NUMANALYST)* and the average forecast horizon (*HORIZON*) and forecast experience (*EXPERIENCE*) are 298 days and 59.3 months, respectively. Finally, about 37.3% of forecasts are issued for investment grade firms *(INVEST)* and about 55.5% of firm-years experience negative abnormal return *(BADNEWS)* during the forecast year.

Regarding deal characteristics, the mean (median) bank ownership of a loan is 24.9% (16%). About 62.4% of deals have at least one financial covenant. Conditional on the existence of a financial covenant, the average number of financial covenants in the sample is 1.21.

Table 3 presents the correlation matrix for the variables used in our analyses for the broker-constant sample (panel A) and the firm-constant sample (panel B). Pearson (Spearman) correlations are displayed in the lower (upper) diagonal. *CONGLOMERATE* is negatively correlated with *ERROR* in the broker-constant sample and positively correlated with *ERROR* in the firm-constant sample. In general, larger firms and firms with more growth opportunities, more analyst following, lower leverage, better performance, and smaller standard deviation of analyst EPS forecasts have more accurate analyst forecasts. All of these results are consistent with our predictions. In addition, analysts with more forecasting experience for a firm provide more accurate forecasts. Analyst forecasts are also more accurate for investment grade firms than for noninvestment grade firms.[21]

5.2 UNIVARIATE RESULTS

In table 4, we compare properties of conglomerate forecasts with those of benchmark forecasts in the pre- and post-loan initiation periods. For

[20] Compared to Sufi [2007] and Ball, Bushman, and Vasvari [2008], our sample firms are much larger. This is expected given that all our sample banks are large financial conglomerates and therefore are more likely to have large borrowers.

[21] Note that the high correlation between *LOGMKT* and *LOGNUMANALYST* is of concern and may cause biased coefficient estimates. To address this concern, we check variance inflation factors (VIF) for these variables, based on model 1, and the VIF for *LOGMKT* is 3.11, which is much lower than 10. Therefore, multicollinearity is not likely to be a problem in interpreting the results.

TABLE 3
Correlation Matrix

Panel A: Broker-constant sample (N = 23,872)

Variable	*ERROR*	*CONGLO-MERATE*	*POST*	*LOG-MKT*	*MB*	*PLOSS*	*EPSUP*	*LOGNUM ANALYST*	*HORI-ZON*	*EXP*	*STD DEV*	*LEV-ERAGE*	*BAD-NEWS*	*INVEST*
ERROR		−0.029	0.009	−0.230	−0.289	0.215	−0.131	−0.085	0.376	−0.036	0.415	0.174	0.081	−0.164
CONGLOMERATE	−0.021		−0.020	0.035	0.018	−0.091	0.010	−0.003	0.020	0.038	−0.046	0.096	−0.014	0.113
POST	0.018	−0.020		0.016	−0.018	−0.010	0.003	0.027	−0.011	0.114	0.050	0.002	−0.001	−0.002
LOGMKT	−0.233	0.028	0.013		0.310	−0.661	0.090	0.655	0.009	0.416	0.023	−0.073	−0.213	0.619
MB	−0.161	0.030	−0.026	0.232		−0.161	0.131	0.166	0.019	0.006	−0.363	−0.269	−0.176	0.088
PLOSS	0.264	−0.084	−0.002	−0.556	−0.076		−0.087	−0.304	−0.007	−0.354	0.078	0.033	0.190	−0.585
EPSUP	−0.121	0.010	0.003	0.090	0.075	−0.070		0.027	0.017	0.030	−0.116	−0.088	−0.147	0.042
LOGNUMANALYST	−0.118	0.001	0.033	0.664	0.090	−0.285	0.023		−0.010	0.284	0.057	−0.155	−0.066	0.301
HORIZON	0.221	0.019	−0.010	0.013	0.018	0.002	0.021	−0.004		0.001	−0.054	−0.006	−0.006	−0.009
EXP	−0.045	0.030	0.162	0.377	−0.032	−0.286	0.024	0.294	0.016		0.095	−0.024	−0.064	0.286
STD DEV	0.386	−0.059	0.021	0.061	−0.205	0.113	−0.097	0.046	−0.032	0.057		0.273	0.030	0.043
LEVERAGE	0.203	0.082	0.002	−0.118	−0.178	0.204	−0.092	−0.124	−0.002	−0.057	0.164		0.038	0.107
BADNEWS	0.111	−0.014	−0.001	−0.223	−0.145	0.181	−0.147	−0.083	−0.012	−0.052	0.013	0.043		−0.097
INVEST	−0.172	0.113	−0.002	0.605	0.055	−0.445	0.042	0.307	−0.008	0.250	0.015	0.049	−0.097	

(*Continued*)

TABLE 3—*Continued*

Panel B: Borrowing firm-constant sample ($N = 20{,}668$)

Variable	*ERROR*	*CONGLO-MERATE*	*POST*	*LOG-MKT*	*MB*	*PLOSS*	*EPSUP*	*LOGNUM ANALYST*	*HORI-ZON*	*EXP*	*STD DEV*	*LEV-ERAGE*	*BAD-NEWS*	*INVEST*
ERROR		0.034	−0.011	−0.260	−0.297	0.187	−0.121	−0.098	0.355	−0.060	0.392	0.129	0.110	−0.217
CONGLOMERATE	0.018		−0.027	−0.042	−0.025	0.023	−0.004	−0.058	0.002	−0.091	0.019	0.039	0.005	−0.027
POST	−0.001	−0.027		0.021	−0.003	0.005	0.011	0.001	−0.012	0.077	0.041	0.004	−0.003	−0.012
LOGMKT	−0.270	−0.048	0.014		0.438	−0.631	0.125	0.639	0.062	0.477	−0.091	−0.006	−0.171	0.646
MB	−0.175	−0.013	−0.004	0.344		−0.254	0.129	0.213	0.038	0.123	−0.353	−0.189	−0.117	0.249
PLOSS	0.251	0.032	0.019	−0.581	−0.184		−0.074	−0.309	−0.042	−0.375	0.035	−0.118	0.099	−0.555
EPSUP	−0.146	−0.004	0.011	0.126	0.071	−0.091		0.070	0.028	0.065	−0.136	−0.159	−0.182	0.065
LOGNUMANALYST	−0.139	−0.066	0.001	0.643	0.124	−0.342	0.071		0.007	0.343	−0.005	−0.105	−0.063	0.369
HORIZON	0.150	0.003	−0.013	0.061	0.028	−0.042	0.029	0.005		0.008	−0.053	0.002	−0.012	0.028
EXP	−0.072	−0.069	0.110	0.427	0.073	−0.348	0.045	0.350	0.018		0.094	0.039	−0.098	0.380
STD DEV	0.338	0.017	0.052	−0.141	−0.152	0.218	−0.093	−0.013	−0.038	0.042		0.298	0.063	−0.039
LEVERAGE	0.189	0.040	0.006	−0.090	−0.110	0.158	−0.160	−0.135	0.001	−0.014	0.196		0.007	0.061
BADNEWS	0.144	0.005	−0.003	−0.170	−0.072	0.124	−0.182	−0.066	−0.010	−0.073	0.090	0.021		−0.096
INVEST	−0.217	−0.027	−0.012	0.629	0.198	−0.465	0.065	0.374	0.026	0.328	−0.093	0.012	−0.096	

Panels A and B reports Pearson and Spearman correlations among variables used in the empirical analysis below and above the diagonal, respectively. All variables are defined in appendix B. Values above 0.012 are statistically significant at the 0.10 level, values above 0.015 are statistically significant at the 0.05 level, and values above 0.02 are statistically significant at the 0.01 level. Values above 0.014 are statistically significant at the 0.10 level, values above 0.017 are statistically significant at the 0.05 level, and values above 0.02 are statistically significant at the 0.01 level.

TABLE 4

Univariate Results of the Association between Financial Conglomerate, Loan Initiation, and Analyst Forecast Error

Panel A: Broker-constant sample (N = 23,872)

Variable	*CONGLOMERATE* *PRE* (1)	*CONGLOMERATE* *POST* (2)	Matching Sample *PRE* (3)	Matching Sample *POST* (4)	Difference (1) − (2)	Difference (3) − (4)	Difference (1) − (3)	Difference (2) − (4)
ERROR (mean)	0.0133	0.0123	0.0136	0.0151	**0.0011**	**−0.0015**	−0.0003	**−0.0028**
ERROR (median)	0.0039	0.0038	0.0041	0.0043	0.0001	**−0.0002**	−0.0002	**−0.0005**
MKT ($million)	9,392	10,480	8,883	9,452	**−1,088**	**−569**	509	**1,028**
ASSETS ($million)	7,692	8,236	8,822	9,520	**−544**	**−698**	**−1,130**	**−1,284**
MB	3.614	3.355	3.312	3.175	**0.259**	**0.138**	**0.302**	**0.181**
PLOSS	0.109	0.115	0.139	0.136	**−0.006**	0.002	**−0.030**	**−0.021**
EPSUP	0.566	0.596	0.570	0.567	**−0.030**	**0.003**	**−0.004**	**0.030**
NUMANALYST	11.187	11.332	11.025	11.410	**−0.146**	**−0.385**	**0.162**	**−0.078**
STD DEV	0.074	0.082	0.095	0.099	−0.007	−0.004	**−0.021**	−0.017
HORIZON (days)	305.859	302.590	298.611	293.823	3.270	**4.788**	7.249	**8.767**
EXPERIENCE (months)	58.695	66.957	53.830	62.840	**−8.262**	**−9.009**	**4.865**	**4.118**
N	2,311	2,198	9,438	9,925				

(*Continued*)

TABLE 4—*Continued*

Panel B: Firm-constant sample (N = 20,668)

Variable	*CONGLOMERATE* *PRE* (1)	*CONGLOMERATE* *POST* (2)	Matching Sample *PRE* (3)	Matching Sample *POST* (4)	Difference (1) − (2)	(3) − (4)	(1) − (3)	(2) − (4)
ERROR (mean)	0.0123	0.0102	0.0130	0.0160	**0.0022**	**−0.0029**	−0.0007	**−0.0058**
ERROR (median)	0.0040	0.0039	0.0034	0.0034	**0.0001**	0.0001	**0.0006**	**0.0005**
MKT ($million)	9,938	11,534	9,129	10,695	**−1,596**	**−1,566**	810	**840**
ASSETS ($million)	7,999	9,723	8,916	11,187	**−1,724**	**−2,271**	**−917**	**−1,464**
MB	3.752	3.340	3.477	3.080	**0.412**	**0.397**	**0.275**	**0.259**
PLOSS	0.103	0.096	0.127	0.121	**0.007**	0.006	**−0.024**	**−0.024**
EPSUP	0.565	0.624	0.579	0.586	**−0.059**	**−0.007**	**−0.015**	**0.038**
NUMANALYST	11.658	12.464	11.367	12.274	**−0.806**	**−0.907**	**0.291**	**0.190**
STD DEV	0.206	0.205	0.222	0.207	0.000	0.015	**−0.016**	−0.001
HORIZON (days)	310.863	303.394	301.887	291.970	7.469	9.917	8.976	**11.424**
EXPERIENCE (months)	57.977	81.501	52.335	73.719	**−23.523**	**−21.384**	**5.642**	**7.781**
N	4,015	3,998	20,074	20,469				

Table 4 reports univariate comparison in variables used in the empirical analysis between conglomerate forecasts and their benchmark forecasts in the pre- and post-loan period based on the broker-constant sample in panel A and the firm-constant sample in panel B. Mean difference is reported for all variables except that the difference in *ERROR* is reported for both mean and median. Independent t-test is used to test mean difference and Wilcoxon signed rank test is used to test median difference. The difference is boldfaced with statistical significance at the .10 level or better. All variables are defined in appendix B.

the broker-constant sample (panel A of table 4), the mean conglomerate forecast error is 0.0133 in the pre-loan-initiation period, which is not statistically different from the mean benchmark forecast error (0.0136) during the same period. In contrast, after loan initiation, the conglomerate forecast error decreases by 0.0011 to 0.0123, while the benchmark forecast error increases by 0.0015 to 0.0151. Both changes are statistically significant. Looking at the medians, conglomerate forecast error decreases from 0.0039 to 0.0038 after loan initiation. However, the change is not statistically significant based on Wilcoxon sign-rank test. The median benchmark forecast error increases from 0.0041 to 0.0043 after loan initiation, and the increase is statistically significant at 5% level.

Turning to the firm-constant sample (panel B of table 4), the mean conglomerate forecast error decreases significantly from 0.0123 before loan initiation to 0.0102 after loan initiation at the 0.01 level, while the benchmark forecast error increases significantly from 0.0130 before loan initiation to 0.0160 after loan initiation. When focusing on medians, the error of conglomerate forecasts decreases from 0.0040 before loan initiation to 0.0039 after loan initiation, and the decrease is statistically significant at the 10% level based on Wilcoxon sign-rank test. By comparison, the median benchmark forecast errors remain unchanged after loan initiation. Taken together, the univariate evidence from both the broker-constant sample and the firm-constant sample is consistent with the prediction of H1 that the accuracy of conglomerate forecasts improves relative to benchmark forecasts after loan initiation.

Table 4 also provides information on the control variables for conglomerate forecasts and benchmark forecasts. Underscoring the importance of controlling for these variables in the multivariate analysis, most control variables are significantly different across conglomerate forecasts and benchmark forecasts or across the pre- and post-loan initiation periods.

5.3 MULTIVARIATE REGRESSION RESULTS

5.3.1. The Association Between Conglomerate Analyst Forecast Accuracy and Loan Initiation. Table 5 presents regression results for model 1. The estimated results for the broker-constant sample are reported in columns 1 and 2 of table 5. The coefficient on *POST* is insignificant, suggesting little change in benchmark forecasts accuracy after loan initiation. In contrast, the coefficient on *CONGLOMERATE* ∗ *POST* is negative and statistically significant at less than the 5% level ($\beta_3 = -0.002$, $p = 0.012$). This is consistent with H1 in that the accuracy for conglomerate forecasts increases after loan initiation relative to that of benchmark forecasts made by the same analysts for nonborrowing firms.

Helping to put the above estimates in economic perspective, the mean stock price of the broker-constant sample is $34. Thus, a 0.002 reduction in forecast error relative to the benchmark forecasts translates to about 7 cents of improvement in forecast accuracy, which is not only statistically, but also economically, significant. Another way to evaluate the materiality

TABLE 5

Multivariate Results of the Association Between Conglomerate Forecast Accuracy and Loan Initiation

$$ERROR = \beta_0 + \beta_1 POST + \beta_2 CONGLOMERATE * POST + \gamma\, CONTROLS + \varepsilon \qquad (1)$$

		Broker-Constant Sample		Firm-Constant Sample	
Variable	Predicted Sign	Coeff. Est. (1)	p-value (2)	Coeff. Est. (3)	p-value (4)
INTERCEPT	?	−0.022	< 0.001	0.014	< 0.001
POST	?	0.000	0.560	0.000	0.619
CONGLOMERATE* * *POST	−	**−0.002**	**0.012**	**−0.002**	**0.017**
LOGMKT	−	−0.006	< 0.001	−0.004	< 0.001
MB	+	0.000	0.047	−0.001	< 0.001
PLOSS	+	0.065	< 0.001	0.041	< 0.001
EPSUP	−	−0.004	< 0.001	−0.006	< 0.001
LOGNUMANALYST	?	0.004	< 0.001	0.003	< 0.001
HORIZON	+	0.010	< 0.001	0.006	< 0.001
LOGEXP	−	0.002	< 0.001	−0.001	0.054
$(LOGEXP)^2$	+	0.000	0.007	0.000	< 0.001
Firm fixed effect		Yes			
Broker fixed effect				Yes	
Year fixed effect		Yes		Yes	
N		23,872		20,668	
Adj. R^2 (%)		53.83		19.75	

Table 5 presents results of testing the relation between conglomerate forecast accuracy and loan initiation based on the broker-constant sample and the firm-constant sample, respectively. Hypothetically, in the broker-constant sample, a loan deal for the borrower is assigned for its matching nonborrowers, who assume the same loan initiation date. Firm and year fixed effects are included in the model for the broker-constant sample and broker and year fixed effects are included in the model for the firm-constant sample. Robust standard errors clustered at the firm level are used to derive p-values. All variables are defined in appendix B. Variables of interest are boldfaced.

of the above estimate is to compare it with the mean forecast error for the overall sample. The mean *ERROR* in the pre-loan period is 0.013 for the broker-constant sample, as shown in table 4. Thus, a 0.002 reduction in forecast error represents nearly a one-sixth improvement in forecast accuracy relative to an average forecast.

The estimated coefficients for the control variables are largely consistent with our predictions: larger firms as well as firms with lower *MB*, lower probabilities of losses, and earnings increases are associated with more accurate forecasts. In addition, greater analyst following leads to larger forecast errors, which is consistent with the argument in Bhushan [1989]. Analyst forecasts are more accurate when they are issued closer to earnings announcement dates. The coefficient on analyst experience is inconsistent with our predictions for the broker-constant sample but consistent for the firm-constant sample.[22] Finally, the coefficient on the square term of

[22] However, the estimate of this coefficient is sensitive to the inclusion of firm fixed effects. When we reestimate model 1 for the broker-constant sample without controlling for firm fixed effects, the coefficient on analyst experience has the predicted sign.

analyst experience is positive and consistent with our predictions for both the broker-constant sample and the firm-constant sample.

The estimated results for the firm-constant sample—reported in columns 3 and 4 of table 5—are both quantitatively and qualitatively similar to those based on the broker-constant sample. The accuracy of benchmark forecasts does not change after loan initiation, as indicated by the insignificant coefficient before *POST*. More importantly, the coefficient on *CONGLOMERATE* $*$ *POST* is negative and statistically significant at less than the 5% level, ($\beta_2 = -0.002$, $p = 0.017$), suggesting that the accuracy of conglomerate forecasts improves relative to benchmark forecasts after loan initiation.

Overall, the results in table 5 corroborate the univariate evidence presented in table 4 and provide support for H1: regardless of the benchmark forecasts used, the accuracy of conglomerate forecasts significantly improves after loan initiation relative to benchmark forecasts.

5.3.2. The Association of Conglomerate Analyst Forecast Accuracy with Loan Initiation and Borrower Information Asymmetry. Table 6 reports the estimated results for tests of H2 for the broker-constant sample and the firm-constant sample using firm size (panel A) and the standard deviation of analyst forecasts (panel B) as proxies for information asymmetry, respectively. Because the estimated results are qualitatively similar across the two samples, we focus our discussion on the results estimated from the broker-constant sample. The coefficient on C*ONGLOMERATE* $*$ *POST* is significantly negative for the subsample of small firms ($\beta_2 = -0.002$, $p = 0.030$) and the subsample of firms with the standard deviation of analyst forecasts above the sample median ($\beta_2 = -0.005$, $p < 0.001$), while it is indistinguishable from 0 for the subsample of large firms and the subsample of firms with the standard deviation of analyst forecasts below the sample median. Therefore, the evidence supports H2 in that the improvement in the accuracy of conglomerate forecasts after loan initiation relative to benchmark forecasts is more pronounced for borrowers with poorer information environment.

5.3.3. The Association of Conglomerate Analyst Forecast Accuracy with Loan Initiation and the Type of News. H3 predicts that the information effect of a lending relationship on conglomerate forecast accuracy is stronger for borrowers experiencing bad news. The estimated results are summarized in table 7. Consistent with our prediction, the coefficient on *CONGLOMERATE* $*$ *POST* is negative and statistically significant for firms with bad news (columns 3, 4, 7, and 8). In contrast, the same coefficient is not distinguishable from 0 for firms with good news (columns 1, 2, 5, and 6).

5.3.4. The Association of Conglomerate Analyst Forecast Accuracy with Loan Initiation and Existence of Financial Covenants. H4 conjectures that the accuracy of financial conglomerate forecasts increases more when a loan deal contains financial covenants. Table 8 reports the test results for this hypothesis. For deals with financial covenants, the coefficient on our main variable of

TABLE 6

The Association of Conglomerate Forecast Accuracy with Loan Initiation and Borrower Information Asymmetry

Panel A:

$$ERROR = \beta_0 + \beta_1 POST + \beta_2 CONGLOMERATE * POST + \gamma\, CONTROLS + \varepsilon \quad (1)$$

		Broker-Constant Sample				Firm-Constant Sample			
		SMALL FIRM = 1		*SMALL FIRM* = 0		*SMALL FIRM* = 1		*SMALL FIRM* = 0	
Variable	Predicted Sign	Coeff. Est. (1)	*p*-value (2)	Coeff. Est. (3)	*p*-value (4)	Coeff. Est. (5)	*p*-value (6)	Coeff. Est. (7)	*p*-value (8)
INTERCEPT	?	−0.023	0.008	−0.007	0.379	0.047	<0.001	0.015	<0.001
POST	?	0.002	0.002	−0.001	0.002	0.002	0.005	−0.001	0.023
CONGLOMERATE * POST	−	**−0.002**	**0.030**	**−0.001**	**0.425**	**−0.003**	**0.039**	**0.000**	**0.949**
LOGMKT	−	−0.011	<0.001	−0.005	<0.001	−0.010	<0.001	−0.004	<0.001
MB	+	0.001	<0.001	0.000	0.080	−0.002	<0.001	0.000	<0.001
PLOSS	+	0.049	<0.001	0.043	0.001	0.038	<0.001	−0.002	0.767
EPSUP	−	−0.006	<0.001	−0.002	<0.001	−0.009	<0.001	−0.003	<0.001
LOGNUMANALYST	−	0.004	0.013	0.002	0.049	0.003	0.009	0.003	<0.001
HORIZON	+	0.014	<0.001	0.007	<0.001	0.009	<0.001	0.004	<0.001
LOGEXP	−	0.002	0.029	−0.001	0.134	−0.001	0.202	−0.002	0.006
$(LOGEXP)^2$	+	0.000	0.024	0.000	0.419	0.000	0.047	0.000	<0.001
Firm fixed effect		Yes		Yes					
Broker fixed effect						Yes		Yes	
Year fixed effect		Yes		Yes		Yes		Yes	
N		11,928		11,944		10,271		10,397	
Adj. R^2 (%)		58.95		42.80		26.07		21.31	

(*Continued*)

TABLE 6—*Continued*

Panel B:

$$ERROR = \beta_0 + \beta_1 POST + \beta_2 CONGLOMERATE * POST + \gamma\, CONTROLS + \varepsilon \quad (1)$$

		Broker-Constant Sample				Firm-Constant Sample			
		HIGH STD DEV = 1		*HIGH STDD EV* = 0		*HIGH STD DEV* = 1		*HIGH STD DEV* = 0	
Variable	Predicted Sign	Coeff. Est. (1)	*p*-value (2)	Coeff. Est. (3)	*p*-value (4)	Coeff. Est. (5)	*p*-value (6)	Coeff. Est. (7)	*p*-value (8)
INTERCEPT	?	−0.034	0.003	−0.002	0.736	0.037	<0.001	0.004	0.185
POST	?	0.000	0.706	0.000	0.652	0.002	0.072	0.000	0.694
CONGLOMERATE * POST	−	**−0.005**	**< 0.001**	**0.001**	**0.202**	**−0.005**	**0.007**	**−0.001**	**0.266**
LOGMKT	−	−0.008	<0.001	−0.004	<0.001	−0.007	<0.001	−0.001	<0.001
MB	+	0.000	0.967	0.000	0.039	−0.001	<0.001	0.000	0.445
PLOSS	+	0.101	<0.001	0.015	0.200	0.044	<0.001	0.012	0.001
EPSUP	−	−0.006	<0.001	−0.002	<0.001	−0.010	<0.001	−0.001	<0.001
LOGNUMANALYST	−	0.005	0.013	0.002	0.173	0.004	<0.001	0.000	0.981
HORIZON	+	0.015	<0.001	0.005	<0.001	0.010	<0.001	0.004	<0.001
LOGEXP	−	0.004	0.010	0.000	0.303	−0.002	0.355	0.000	0.311
$(LOGEXP)^2$	+	0.000	0.029	0.000	0.507	0.000	0.084	0.000	0.876
Firm fixed effect		Yes		Yes					
Broker fixed effect						Yes		Yes	
Year fixed effect		Yes		Yes		Yes		Yes	
N		10,237		10,240		7,911		9,783	
Adj. R^2(%)		56.29		45.09		27.63		16.20	

Table 6 presents results of testing the relation between the change in conglomerate analyst forecast accuracy after loan initiation and borrower information asymmetry as measured by firm size and the standard deviation of analyst annual earnings forecasts, based on the broker-constant sample and the firm-constant sample, respectively. *SMALLFIRM* is a dummy variable equal to 1 if total market value of equity measured at the fiscal year prior to loan initiation (*LOGMKT*) is below the sample median, and equal to 0 otherwise; *HIGH STD DEV* is a dummy variable equal to 1 if the standard deviation of analyst annual earnings forecasts made in the fiscal year prior to loan initiation (*STD DEV*) is above the sample median, and equal to 0 otherwise. Hypothetically, in the broker-constant sample, a loan deal for the borrower is assigned for its matching nonborrowers, who assume the same loan initiation date. Firm and year fixed effects are included in the model for the broker-constant sample and broker and year fixed effects are included in the model for the firm-constant sample. Robust standard errors clustered at the firm level are used to derive *p*-values. All variables are defined in appendix B. Variables of interest are boldfaced.

TABLE 7

The Association of Conglomerate Forecast Accuracy with Loan Initiation and the Type of News

$$ERROR = \beta_0 + \beta_1 POST + \beta_2 CONGLOMERATE * POST + \gamma\, CONTROLS + \varepsilon \quad (1)$$

		Broker-Constant Sample				Firm-Constant Sample			
		BADNEWS = 0		*BADNEWS* = 1		*BADNEWS* = 0		*BADNEWS* = 1	
Variable	Predicted Sign	Coeff. Est. (1)	*p*-value (2)	Coeff. Est. (3)	*p*-value (4)	Coeff. Est. (5)	*p*-value (6)	Coeff. Est. (7)	*p*-value (8)
INTERCEPT	?	−0.028	0.003	0.003	0.766	0.000	0.818	0.025	<0.001
POST	?	−0.001	0.001	0.001	0.001	−0.001	0.104	0.000	0.802
CONGLOMERATE * POST	−	**−0.001**	**0.380**	**−0.002**	**0.080**	**−0.001**	**0.477**	**−0.003**	**0.017**
LOGMKT	−	0.000	0.867	−0.011	<0.001	−0.002	<0.001	−0.006	<0.001
MB	+	0.000	0.067	0.000	0.072	0.000	0.714	−0.001	<0.001
PLOSS	+	0.049	0.007	0.058	<0.001	0.017	<0.001	0.042	<0.001
EPSUP	−	0.001	0.065	−0.007	<0.001	0.002	<0.001	−0.011	<0.001
LOGNUMANALYST	?	0.000	0.882	0.007	<0.001	0.002	<0.001	0.005	<0.001
HORIZON	+	0.009	<0.001	0.011	<0.001	0.006	<0.001	0.007	<0.001
LOGEXP	−	0.001	0.301	0.001	0.115	0.000	0.450	−0.004	0.005
$(LOGEXP)^2$	+	0.000	0.133	0.000	0.186	0.000	0.091	0.001	<0.001
Broker fixed effect		Yes		Yes					
Firm fixed effect						Yes		Yes	
Year fixed effect		Yes		Yes		Yes		Yes	
N		10,622		13,250		9,638		11,030	
Adj. R^2 (%)		51.67		59.90		15.47		27.35	

Table 7 presents results of testing the relation between conglomerate analyst forecast accuracy and loan initiation for borrowers with bad news and borrowers without bad news based on the broker-constant sample and the firm-constant sample, respectively. Hypothetically, in the broker-constant sample, a loan deal for the borrower is assigned for its matching nonborrowers, who assume the loan initiation date. Bad news are defined as dummy variable equal to 1 if a firm has negative cumulative annual abnormal returns, and 0 otherwise. Firm and year fixed effects are included in the model for the broker-constant sample and broker and year fixed effects are included in the model for the firm-constant sample. Robust standard errors clustered at the firm level are used to derive *p*-values. All variables are defined in appendix B. Variables of interest are boldfaced.

TABLE 8

The Association of Conglomerate Forecast Accuracy with Loan Initiation and Existence of Financial Covenant

$$ERROR = \beta_0 + \beta_1 POST + \beta_2 CONGLOMERATE * POST + \gamma\, CONTROLS + \varepsilon \quad (1)$$

		Broker-Constant Sample				Firm-Constant Sample			
		FCOVENANT = 1		*FCOVENANT* = 0		*FCOVENANT* = 1		*FCOVENANT* = 0	
Variable	Predicted Sign	Coeff. Est. (1)	*p*-value (2)	Coeff. Est. (3)	*p*-value (4)	Coeff. Est. (5)	*p*-value (6)	Coeff. Est. (7)	*p*-value (8)
INTERCEPT	?	0.009	0.266	−0.016	0.458	0.011	0.048	0.003	0.892
POST	?	0.000	0.551	0.001	0.436	0.000	0.698	−0.001	0.744
CONGLOMERATE * POST	−	**−0.003**	**0.005**	**−0.001**	**0.285**	**−0.002**	**0.060**	**−0.001**	**0.143**
LOGMKT	−	−0.007	<0.001	−0.006	0.012	−0.003	<0.001	−0.006	0.021
MB	+	0.000	0.009	0.000	0.529	0.000	<0.001	−0.001	0.079
PLOSS	+	0.067	<0.001	0.077	0.013	0.025	<0.001	0.049	0.010
EPSUP	−	−0.004	<0.001	−0.003	0.013	−0.005	<0.001	−0.008	0.013
LOGNUMANALYST	−	0.003	0.021	−0.001	0.854	0.004	<0.001	0.001	0.795
HORIZON	+	0.010	<0.001	0.010	<0.001	0.005	<0.001	0.008	<0.001
LOGEXP	−	0.002	0.007	0.001	0.338	−0.002	0.012	0.001	0.569
$(LOGEXP)^2$	+	0.000	0.009	0.000	0.564	0.000	0.011	0.000	0.519
Firm fixed effect		Yes		Yes					
Broker fixed effect						Yes		Yes	
Year fixed effect		Yes		Yes		Yes		Yes	
N		15,361		8,511		12,863		7,805	
Adj. R^2 (%)		59.02		54.42		16.45		34.60	

Table 8 presents results of testing the relation between conglomerate analyst forecast accuracy and loan initiation for loans with financial covenants and loans without financial covenants based on the broker-constant sample and the firm-constant sample, respectively. Hypothetically, in the broker-constant sample, a loan deal for the borrower is assigned for its matching nonborrowers, who assume the same covenant specification. Firm and year fixed effects are included in the model for the broker-constant sample and broker and year fixed effects are included in the model for the firm-constant sample. Robust standard errors clustered at the firm level are used to derive *p*-values. All variables are defined in appendix B. Variables of interest are boldfaced.

TABLE 9

Multivariate Results of the Association Between Conglomerate Forecast Accuracy and Loan Initiation When Conglomerate Analysts Are Participant Lenders

$$\text{ERROR} = \beta_0 + \beta_1\text{POST} + \beta_2\text{CONGLOMERATE} * \text{POST} + \gamma\text{CONTROLS} + \varepsilon \quad (1)$$

Variable	Predicted Sign	Broker-Constant Sample Coeff. Est.	Broker-Constant Sample p-value	Firm-Constant Sample Coeff. Est.	Firm-Constant Sample p-value
INTERCEPT	?	0.004	0.729	0.006	0.460
POST	?	0.001	0.002	0.000	0.856
CONGLOMERATE * POST	**?**	**0.000**	**0.964**	**0.001**	**0.415**
LOGMKT	−	−0.004	0.000	−0.003	0.013
MB	+	0.000	0.929	0.000	0.185
PLOSS	+	0.057	0.001	0.062	0.000
EPSUP	−	−0.003	0.000	−0.003	0.037
LOGNUMEST	?	0.008	0.000	0.005	0.082
HORIZON	+	0.009	0.000	0.008	0.000
LOGEXP	−	0.000	0.468	−0.001	0.401
$(LOGEXP)^2$	+	0.000	0.022	0.000	0.106
Firm fixed effect		Yes			
Broker fixed effect				Yes	
Year fixed effect		Yes		Yes	
N		33,779		25,973	
Adj. R^2(%)		48.65		17.36	

Table 9 presents results of testing the relation between conglomerate analyst forecast accuracy and loan initiation for financial conglomerates as participant lenders based on the broker-constant sample and the firm-constant sample, respectively. Hypothetically, in the broker-constant sample, a loan deal for the borrower is assigned for its matching nonborrowers, who assume the same covenant specification. Firm and year fixed effects are included in the model for the broker-constant sample and broker and year fixed effects are included in the model for the firm-constant sample. Robust standard errors clustered at the firm level are used to derive p-values. All variables are defined in appendix B. Variables of interest are boldfaced.

interest, *CONGLOMERATE * POST* is significantly negative, ($\beta_2 = -0.003$, $p = 0.005$ for the broker-constant sample (columns 1 and 2); $\beta_2 = -0.002$, $p = 0.060$ for the firm-constant sample (columns 5 and 6)). Conversely, for deals without financial covenants, the coefficient on *CONGLOMERATE * POST* is not distinguishable from 0 (columns 3, 4, 7, and 8). This provides support for H4 in that the accuracy of financial conglomerate forecasts increases more when a deal contains financial covenants. These results are also consistent with the findings in prior studies that the existence of financial covenants enables lenders to have more timely access to firms' nonpublic information (Bushman, Smith, and Wittenberg-Moerman [2010], Massoud et al. [2010]).

5.3.5. The Association of Conglomerate Analyst Forecast Accuracy with Loan Initiation When Banks Are Participant Lenders. Table 9 reports the test results of H5 based on the two samples for participant lenders as described in footnote 11. As can be seen, the coefficient on *CONGLOMERATE * POST* is positive but statistically indistinguishable from 0 for both samples, suggesting no information spillover effect after loan initiation. Therefore, consistent with the arguments of Sufi [2007] and Bushman and Wittenberg-Moerman [2009], we do not find that participant lenders have a similar information advantage as lead arrangers.

5.4 ADDITIONAL ANALYSES

5.4.1. The Association of Conglomerate Analyst Forecast Optimism with Loan Initiation. Though our focus is on analyst forecast accuracy, we also investigate how lending relationships affect analyst optimism. Prior studies show that analysts could issue optimistic forecasts to please firms' managers in order to obtain private information to improve forecast accuracy (e.g., Das, Levine, and Sivaramakrishnan [1998], Francis and Philbrick [1993], Lim [2001], and Richardson, Teoh, and Wysocki [2004]). If a lending relationship allows conglomerate analysts access to superior information, then it could mitigate analysts' incentives to issue upward-biased forecasts or optimistic stock recommendations in an attempt to curry favor with firms' managers.

Using both signed earnings forecast error and stock recommendation levels (e.g., "strong buy" and "buy" present optimism compared to "sell" and "hold") to proxy for analyst optimism, we investigate the relation between conglomerate analyst optimism and loan initiation.[23] Overall, we find no evidence that conglomerate forecast optimism decreases after loan initiation. One possible explanation is that, to maintain current or future lending relationships, conglomerate analysts still have incentives to issue optimistic forecasts and recommendations. Such incentives offset the effect of superior information in reducing analysts' optimism.

5.4.2. The Association of Conglomerate Analyst Forecast Accuracy with Loan Initiation and Borrower Default Risk. In addition to the three cross-sectional analyses as posited in H2 to H4, we further investigate whether the information spillover effect varies with a borrower's default risk. When borrowers are highly levered or have low credit ratings, their default risk increases, resulting in high agency costs of debt. In these instances, lenders have greater incentives to collect private information about borrowers to reduce the likelihood of losses. Therefore, we would expect the information spillover effect to be more pronounced for borrowers with high leverage and low credit rating.

Panels A and B of table 10 present the estimation results for the leverage effect and the credit rating effect, respectively. In panel A, the coefficient on $CONGLOMERATE * POST$ is significantly negative for the subsample of firms with above-median leverage ratios while indistinguishable from 0 for

[23] The broker-constant sample and the firm-constant sample for the signed forecast error test are the same as the two main samples used for testing forecast accuracy. The broker-constant sample and the firm-constant sample for stock recommendation test are much smaller than the two main samples ($N = 9{,}882$ and $9{,}652$).

TABLE 10

The Association of Conglomerate Forecast Accuracy with Loan Initiation and Firm Leverage

Panel A:

$$ERROR = \beta_0 + \beta_1 POST + \beta_2 CONGLOMERATE * POST + \gamma\, CONTROLS + \varepsilon \quad (1)$$

		Broker-Constant Sample				Firm-Constant Sample			
	Predicted	*HIGH LEVERAGE* = 1		*HIGH LEVERAGE* = 0		*HIGH LEVERAGE* = 1		*HIGH LEVERAGE* = 0	
Variable	Sign	Coeff. Est. (1)	*p*-value (2)	Coeff. Est. (3)	*p*-value (4)	Coeff. Est. (5)	*p*-value (6)	Coeff. Est. (7)	*p*-value (8)
INTERCEPT	?	0.021	0.043	−0.027	<0.001	0.010	0.011	0.018	<0.001
POST	?	0.000	0.473	0.001	0.040	−0.001	0.349	0.001	0.044
CONGLOMERATE * POST	−	**−0.002**	**0.011**	**−0.001**	**0.452**	**−0.003**	**0.014**	**−0.001**	**0.272**
LOGMKT	−	−0.012	<0.001	−0.005	<0.001	−0.006	<0.001	−0.004	<0.001
MB	+	0.000	0.598	0.000	0.001	−0.001	<0.001	0.000	<0.001
PLOSS	+	0.051	0.001	0.038	<0.001	0.027	<0.001	0.073	<0.001
EPSUP	−	−0.004	<0.001	−0.003	<0.001	−0.006	<0.001	−0.006	<0.001
LOGNUMANALYST	?	0.003	0.078	0.004	<0.001	0.006	<0.001	0.001	0.038
HORIZON	+	0.012	<0.001	0.008	<0.001	0.007	<0.001	0.006	<0.001
LOGEXP	−	0.003	<0.001	0.001	0.104	0.000	0.926	−0.004	0.001
$(LOGEXP)^2$	+	0.000	0.003	0.000	0.452	0.000	0.265	0.001	<0.001
Firm fixed effect		Yes		Yes					
Broker fixed effect						Yes		Yes	
Year fixed effect		Yes		Yes		Yes		Yes	
N		11,937		11,935		10,320		10,348	
Adj. R^2 (%)		54.49		58.42		25.74		21.52	

(*Continued*)

TABLE 10—*Continued*

Panel B:

$$ERROR = \beta_0 + \beta_1 POST + \beta_2 CONGLOMERATE * POST + \gamma\, CONTROLS + \varepsilon \quad (1)$$

		Broker-Constant Sample				Firm-Constant Sample			
		INVEST = 0		*INVEST* = 1		*INVEST* = 0		*INVEST* = 1	
Variable	Predicted Sign	Coeff. Est. (1)	*p*-value (2)	Coeff. Est. (3)	*p*-value (4)	Coeff. Est. (5)	*p*-value (6)	Coeff. Est. (7)	*p*-value (8)
INTERCEPT	?	−0.026	<0.001	−0.007	0.287	0.028	<0.001	0.018	<0.001
POST	?	0.000	0.358	0.000	0.264	0.000	0.668	0.000	0.166
CONGLOMERATE * POST	−	**−0.003**	**0.005**	**−0.001**	**0.164**	**−0.004**	**0.018**	**0.000**	**0.660**
LOGMKT	−	−0.008	<0.001	−0.001	0.052	−0.006	<0.001	−0.002	<0.001
MB	+	0.000	0.055	0.000	0.631	−0.002	<0.001	0.000	0.454
PLOSS	+	0.069	<0.001	−0.038	0.004	0.017	<0.001	0.089	<0.001
EPSUP	−	−0.006	<0.001	−0.001	0.015	−0.012	<0.001	0.000	0.561
LOGNUMANALYST	?	0.002	0.057	0.001	0.440	0.003	0.029	0.000	0.911
HORIZON	+	0.012	<0.001	0.007	<0.001	0.009	<0.001	0.004	<0.001
LOGEXP	−	0.002	0.004	−0.001	0.148	−0.004	<0.001	−0.005	<0.001
$(LOGEXP)^2$	+	0.000	0.020	0.000	0.179	0.001	<0.001	0.001	<0.001
Firm fixed effect		Yes		Yes					
Broker fixed effect						Yes		Yes	
Year fixed effect		Yes		Yes		Yes		Yes	
N		14,971		8,901		10,253		10,415	
Adj. R^2 (%)		55.22		43.49		19.85		31.76	

Table 10, panel A, presents results of testing the relation between conglomerate analyst forecast accuracy and loan initiation for borrowers with high leverage and borrowers with low leverage based on the broker-constant sample and the firm-constant sample, respectively. *HIGHLEVERAGE* is a dummy variable coded as 1 if the borrower's leverage ratio (measured as total debt [Compustat no. 34 + no. 9] scaled by total assets [Compustat no. 6]) is above the sample median, and 0 otherwise. Panel B presents results for borrowers with an investment grade rating and borrowers without an investment grade rating, respectively, based on two samples. A credit rating equal to or above BBB is defined as investment grade rating. Hypothetically, in the broker-constant sample, a loan deal for the borrower is assigned for its matching nonborrowers, who assume the same loan initiation date. Firm and year fixed effects are included in the model for the broker-constant sample and broker and year fixed effects are included in the model for the firm-constant sample. Robust standard errors clustered at the firm level are used to derive *p*-values. All variables are defined in appendix B. Variables of interest are boldfaced.

the subsample of firms with below-median leverage ratios. This is consistent with our expectation that the information spillover effect from the lending division to the equity research division is more pronounced for firms with high credit risk.

In panel B, we estimate model 1 for investment grade firms and noninvestment grade firms separately.[24] For investment grade firms (columns 3, 4, 7, and 8 of the table), the coefficient on *CONGLOMERATE* ∗ *POST* is close to 0 and insignificant for both the broker-constant sample and the firm-constant sample. In contrast, the coefficient on *CONGLOMERATE* ∗ *POST* is significantly negative for noninvestment grade firms based on both the broker-constant sample (columns 1 and 2) and the firm-constant sample (columns 5 and 6). In sum, the evidence is consistent with our prediction that the information spillover effect is more pronounced for high default risk firms.

5.4.3. Forecast Horizon, Loan Characteristics, and Prior Lending Relationship. Prior studies document that analyst forecast accuracy is inversely related to forecast horizon as analysts collect more information when forecast issuance dates are closer to earnings announcements (e.g., Brown et al. [1987], O'Brien [1988], Clement [1999]). One interesting question is whether the information advantage of conglomerate analysts varies with forecast horizon. We measure forecast horizon as the number of days between a forecast issuance and the earnings announcement (*HORIZON*). Then, *HORIZON* is interacted with *CONGLOMERATE* ∗ *POST* in equation (1). Untabulated results show a negative and statistically significant coefficient on this three-way interaction term for the two main samples, suggesting that conglomerate analysts have greater information advantage for longer horizon forecasts, possibly due to less availability of public information and thus more uncertainty about the borrowers when a forecast is issued far away from the earnings announcement.

Though we require all lead banks to have at least 10% ownership of the loans, the extent of information advantage for lead banks can still vary positively with bank ownership. The idea is that banks have greater incentive to engage in information acquisition when they hold a larger fraction of loan shares (Gorton and Pennacchi [1995], Mora and Sowerbutts [2008], Ivashina [2009]). Confirming this conjecture, based on the two main samples, we find that the information effect of lending relationship on the accuracy of conglomerate forecasts is more pronounced for loans with larger bank ownership.

We also examine whether the information effect of bank loans on conglomerate analyst forecasts varies with loan maturity. On the one hand, short-maturity loans could simply indicate greater information asymmetry

[24] The investment grade firms include firms without credit ratings. The results are unaltered when firms without credit ratings are deleted.

between lenders and borrowers (Barnea, Haugen, and Senbet [1980], Leland and Toft [1996]), which predicts a stronger information effect for short-maturity debt based on H2. On the other hand, banks could have less incentive to acquire information for short-maturity loans because debt value is less sensitive to the assets value of borrowers (Barnea, Haugen, and Senbet [1980]). This predicts a weaker information effect for short-maturity debt. Loan maturity is interacted with the test variables in equation (1), and we find no effect of loan maturity on the change of conglomerate forecast accuracy based on the two main samples.

Last, we examine whether the information spillover effect is reduced for borrowers having prior lending relationships with their lenders. By sample construction, as shown in table 1, our sample is dominated by first deals for the same borrower-lender pair (82.5%), which reduces our test power. We create one dummy variable, *PRIOR*, equal to 1 if a borrower had a lending relationship with the current lender in the five years prior to the current deal and 0 otherwise. Then, *PRIOR* is interacted with *CONGLOMERATE* ∗ *POST* in equation (1). Untabulated results show an insignificant coefficient on this three-way interaction term. Therefore, we do not find that prior lending relationship has an impact on the information spillover effect within financial conglomerates.

6. Sensitivity Tests

6.1 REPORTING REGIME SHIFTS

We examine the sensitivity of our results to alternative reporting regime. The SEC implemented Regulation FD on October 23, 2000, intending to prohibit firms from privately disclosing value-relevant information to select securities markets professionals without simultaneously disclosing that same information to the public. As borrowers can still selectively pass along private information to lending institutions after the passage of Regulation FD, we would expect the information effect predicted in H1 to be stronger after the regulation.

A dummy variable, *FD,* is created to capture the Regulation FD regime shift, and it is coded as 1 if a forecast is issued after October 1, 2000, and 0 otherwise. We then interact *FD* with the test variable in equation (1). Based on both the broker-constant and the firm-constant sample, we do not find that Regulation FD has any impact on the accuracy improvement of conglomerate forecasts in the post-loan-initiation period.

6.2 ESTIMATION ISSUES

6.2.1. Outliers. Though we winsorize analyst forecast errors at the 1% and 99% levels, they are still right skewed for both the broker-constant and the firm-constant samples. To ensure that our results are not driven by outliers, we perform three additional procedures. First, we rerun all our main analyses using median regression. Median regression is more robust in response to large outliers compared to ordinary least squares regression (Koenker [2005]). Second, we use percentage-ranked forecast errors within a firm across the sample period as the dependent variable. Third, we conduct diagnostic analysis of outliers using standardized residuals and Cook's D and delete those outliers exceeding the conventional level of the cutoffs.[25] All results are robust to these additional procedures.[26]

6.2.2. Bad News Subsample and Most Recent Earnings Forecast Subsample. As documented in section 5.3.3, our results mainly concentrate in firms with bad news. In this section, we examine whether the information spillover effect is more pronounced for firms with high information asymmetry for the subsample of firms experiencing bad news. Similar to what we find for the full sample (table 6), when we partition the subsample of bad news firms into large versus small firms and firms with high versus low standard deviation of analyst earnings forecasts, the coefficient on *CONGLOMERATE* ∗ *POST* is only significantly negative for the subsample of small firms and the subsample of firms with high standard deviation of analyst earnings forecasts. Moreover, the magnitudes of this coefficient are stronger for this subsample than those presented based on the full sample.

Furthermore, we rerun model 1 using the most recent analyst earnings forecasts issued within 240 days prior to earnings announcements based on both the broker-constant sample and the firm-constant sample. Untabulated results show that the coefficient on *CONGLOMERATE* ∗ *POST* is negative but statistically indistinguishable from 0. These results are not surprising given our earlier finding, explained in section 5.4.4, that the information spillover effect mostly concentrates in longer horizon analyst forecasts.

6.3 AN ALTERNATIVE EXPLANATION

Our results consistently point to the conclusion that bank-affiliated analysts possess superior information after a loan inception, enabling them to make more accurate EPS forecasts. However, one can argue that a bank's ownership in a loan gives its affiliated analysts the incentive to work harder and that their greater effort yields better forecasts. Yet this alternative explanation seems unlikely. Yu [2007], after all, finds that banks have superior information about borrowers' future earnings, compared with financial analysts, and this finding suggests that the superior information flows from the lending division to the equity research side rather than vice versa. Furthermore, the greater-effort argument predicts a symmetric improvement

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[25] The conventional cutoff values that we used for absolute standardized residuals is 3.5 and $4/n$ (where n is the number of observations) for Cook's D.

[26] Although the economic magnitude of the change in median conglomerate forecast error relative to benchmark forecast error after loan initiation is small based on the univariate test, as shown in table 4, the median regression results suggest that this change is economically significant. For example, the coefficient on *POST* ∗ *CONGLOMERATE* for the broker-constant sample is −0.0007, based on the median regression, which is about one-sixth of the median conglomerate forecast error (0.0039, table 4) in the pre-loan-initiation period.

in conglomerate forecast accuracy with respect to borrowers experiencing good or bad news. Given that we find *asymmetric* improvement in conglomerate forecast accuracy, we conclude that the information argument better explains our results.

7. *Conclusion*

Over the course of the 1990s and the early 2000s, the financial industry witnessed a wave of mergers and acquisitions. As a result, many commercial banks acquired investment banks, brokerage houses, and assets management firms and formed financial conglomerates. Prior studies show that commercial banks have superior information about borrowers and are perceived to be quasi-insiders. The formation of conglomerates created more opportunities for information sharing between lending and security dealing divisions within financial conglomerates. We investigate whether bank-affiliated (conglomerate) analysts obtain private information from lending divisions for the period from 1994 to 2007. Specifically, we test whether the accuracy of conglomerate analysts improves after the followed firm borrows from an affiliated bank.

Using a matched sample design, matching either by affiliated bank or by borrower, we have four key findings. First, the accuracy of conglomerate forecasts increases during the one-year period after a loan inception. Second, the increase in conglomerate forecasts accuracy after a loan inception is more pronounced for borrowers with high information asymmetry and for deals with financial covenants and high bank ownership. Third, the increase in conglomerate forecast accuracy is concentrated in borrowers with negative news and high credit risk. Fourth, the informational advantage for conglomerate analysts exists only when conglomerates serve as lead arrangers, not merely as participating lenders. Collectively, this paper provides evidence that divisions within large financial conglomerates share information and that bank-affiliated analysts benefit from the information spillover. However, because our results are based on a small sample of loan deals, they should be interpreted with caution with regard to the representativeness of our findings for all banks issuing conglomerate forecasts.

Although the information sharing is beneficial from a financial conglomerate's perspective, it seems to underscore regulators' concern that leakage of private lending information to the public domain might breach confidentiality agreements between banks and borrowers and that illegal trading could result (Standard & Poor's [2008]). Amid the financial crisis that started in 2007, large stand-alone U.S. investment banks have disappeared from the banking scene.[27] The universal banking model, which allows financial conglomerates to combine a wide range of financial

[27] Large investment banks cease to exist through bankruptcy (Lehman Brothers), takeovers (of Bear Sterns by JP Morgan and of Merill Lynch by Bank of America), and conversions into commercial banks (JP Morgan and Goldman Sachs).

activities, has emerged as a more desirable structure for a financial institution from the viewpoint of policy makers due to its resilience to adverse shocks (Demirguc-Kunt and Huizinga [2010]). As a consequence, information spillover among different divisions within financial conglomerates is likely to be of greater concern.

APPENDIX A

Panel A: List of conglomerate banks and their securities subsidiaries

Financial Conglomerate (Bank)	Security Subsidiaries
ABN	ABN AMRO Inc.
Bank of America	Banc of America
Bank of Montreal	BMOCM-Canada
Bank of Montreal	BMOCM-US
Bank of Nova Scotia	Scotia Capital
CIBC	CIBC World Mkts
Capital One Bank	Capital One
Citigroup	Citigroup
Commerzbank	Commerzbank
Credit Agricole	Calyon Sec. USA
Credit Suisse Group	Credit Suisse Securities (USA), LLC
Deutsche Bank	DeutscheBankSec
Dresdner	Dresdner US
FRB	FBR & Company
Fortis	Fortis Sec.
HSBC	HSBC
JP Morgan	J.P.Morgan
KeyBank NA	KeyBanc Capital
National Bank of Canada	NB Financial
National City Bank	NatCity Inv.
U.S. Bankcorp	Piper Jaffray
Regions Financial Group	Morgan Keegan
Royal Bank of Canada	RBC Capital Mkt
Royal Bank of Canada	RBC Wealth Mgmt
SunTrust	SunTrust RH
Toronto Dominion	Commerce Cap
Toronto Dominion	TD Newcrest
UBS AG	UBS (US)
UBS AG	UBS Canada
Wachovia	A.G. Edwards
Wachovia	Wachovia Cap
Wells Fargo	Ragen MacKenzie

(*Continued*)

APPENDIX A—*Continued*

Panel B: Conglomerate banks in the broker and firm-constant samples

	N	%	*N*	%
ABN	3	0.72	2	0.53
Bank of America	120	28.71	106	28.19
Bank of Montreal	2	0.48	2	0.53
Bank of Nova Scotia	1	0.24	1	0.27
CIBC	3	0.72	3	0.72
Capital One Bank	1	0.24	1	0.27
Citigroup	89	21.29	70	18.62
Credit Suisse	10	2.39	10	2.66
Deutsche Bank	15	3.59	14	3.72
HSBC	1	0.24	1	0.27
JP Morgan	106	25.36	106	28.19
Regions Financial Group	1	0.24	1	0.27
Royal Bank of Canada	1	0.24	1	0.27
SunTrust	20	4.78	19	5.05
UBS AG	4	0.96	3	0.8
Wachovia	41	9.81	36	9.57
Total	418	100	376	100

Panel C: Lender role in the broker and firm-constant samples

	Broker-Constant Sample		Firm-Constant Sample	
Lender Role	*N*	%	*N*	%
Admin agent	263	62.92	240	63.83
Agent	45	10.77	43	11.44
Arranger	8	1.91	7	1.86
Bookrunner	2	0.48	1	0.27
Documentation agent	6	1.44	6	1.6
Lead manager	1	0.24	1	0.27
Mandated arranger	1	0.24	1	0.27
Syndications agent	92	22.01	77	20.48
Total	418	100	376	100

Appendix A, panel A, reports the list of financial conglomerates contained in Dealscan with the left column being bank holding company and the right column being the broker division. Panel B describes the frequency distribution of financial conglomerates for all loan deals in our broker-constant sample and firm-constant sample, respectively. Panel C reports the frequency distribution of the role of lead arrangers for all loan deals in our broker-constant sample and firm-constant sample, respectively.

APPENDIX B
Variable Definitions

Dependent variables:

ERROR = analyst forecast error, calculated as |annual EPS forecast – actual EPS forecast/price, where price is firm stock price at the beginning of forecast month.

Independent variables:

CONGLOMERATE = 1 if an EPS forecast is issued by a bank-affiliated analyst, and 0 otherwise.

POST = 1 if an EPS forecast is issued after loan initiation, and 0 otherwise.

MKT = market value of equity (Compustat no. 199 × no. 25) measured at the end of the fiscal year prior to an EPS forecast.

SMALLFIRM = dummy variable coded as 1 if *LOGMKT* is below the sample median, and 0 otherwise.

ASSET = total assets (Compustat no. 6).

MB = market value of equity divided by book value of equity (Compustat no. 60) measured at the end of the fiscal year prior to an EPS forecast.

PLOSS = the probability of a firm experiencing loss in the fiscal year prior to an EPS forecast is issued. The probability is predicted using the model with industry fixed effects and industry is based on three-digit SIC code as follows:

$$\begin{aligned}\text{Prob}(\text{loss} = 1) = {} & \text{Industry fixed effects} + \gamma 1 LOGMKT \\ & + \gamma 2\ LAG\_ROA + \gamma 3\ LAG\_CFO + \gamma 4 CFO + \gamma 6 SG \\ & + \gamma 7 DIVDUM + \varepsilon\end{aligned}$$

where *LOGMKT* is natural logarithm of market value of equity, *LAGROA* is returns on assets (Compustat no.18/no. 6) at the end of prior fiscal year; *LAG_CFO* and *CFO* is operating cash flow scaled by total assets (Compustat no. 308/no. 6) measured at the end of the prior fiscal year and the end of the current fiscal year, respectively; *SG* is sales growth (Compustat Δno.12/no. 12); and *DIVDUM* is a dummy variable equal to 1 if a firm pays a dividend, and 0 otherwise.

EPSUP = 1 if a firm experiences an increase in EPS compared with the previous fiscal year, and 0 otherwise, measured at the end of the fiscal year prior to an EPS forecast.

NUMANALYST = average number of analysts following a firm over the course of a fiscal year measured during the fiscal year prior to an EPS forecast. Natural logarithm is taken in the regression.

STD DEV = the average of the standard deviation of analyst annual EPS forecast over the course of a fiscal year measured during the fiscal year prior to an EPS forecast.

HIGH STD DEV = dummy variable coded as 1 if *STD DEV* is above the sample median, and 0 otherwise.

HORIZON = number of days between the time when an EPS forecast is issued and the actual EPS announcement date. Natural logarithm is taken in the regression.

EXPERIENCE = number of months between the time when an analyst started to follow a firm and the time when a specific forecast is issued in year t. Natural logarithm is taken in the regression.

LEVERAGE = total debt (Compustat no. 34 + no. 9), scaled by total assets (Compustat no. 6).

HIGHLEVERAGE = dummy variable coded as 1 if *LEVERAGE* is above the sample median and 0 otherwise.

BADNEWS = dummy variable coded as 1 if cumulative abnormal return in the year that an EPS forecast is issued is negative, and 0 otherwise.

INVEST = dummy variable coded as 1 if a firm has an S&P credit rating equal or above BBB, and 0 otherwise.

BANK SHARE = ownership of a loan by a lead arranger.

FINCOVENANT = dummy variable coded as 1 if a deal contains at least one financial covenant, and 0 otherwise.

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