working paper

11 14

Banking Relationships and Sell-Side Research

Ozgur E. Ergungor, Leonardo Madureira, Nandkumar Nayar, and Ajai K. Singh

FEDERAL RESERVE BANK OF CLEVELAND

Working Paper 11-14 June 2011

Banking Relationships and Sell-Side Research

Ozgur E. Ergungor, Leonardo Madureira, Nandkumar Nayar, and Ajai K. Singh

This paper examines disclosures by sell-side analysts when their institution has a lending relationship with the firms being covered. Lending-affiliated analysts' earnings forecasts are found to be more accurate relative to forecasts by other analysts but this differential accuracy manifests itself only after the advent of the loan. Despite this increased earnings forecast accuracy, lending-affiliated analysts exhibit undue optimism in their brokerage recommendations and forecasts of long term growth. The optimism exists both before and after the lending commences. The evidence suggests that any insights into the covered firm via the lending relationship are employed by bank analysts in a selective manner. They appear unwilling to compromise on disclosures where ex post accuracy is clearly revealed, possibly to preserve their own personal reputation. However, they are overly optimistic on other disclosures where resolution is less readily verifiable, possibly to promote their lending client's financial standing.

JEL Classification Codes: G21, G24.

Keywords: relationship lending, investment bank relationships, sell-side research, analyst forecast accuracy

Ozgur E. Ergungor is at the Federal Reserve Bank of Cleveland (ozgur.e.ergungor@clev.frb.org), Leonardo Madureira is at Case Western Reserve University (leonardo.madureira@case.edu), Nandkumar Nayar is at Lehigh University (nan2@lehigh.edu), and Ajai K. Singh is at Case Western Reserve University (ajai@case.edu). The authors are grateful to Oya Altınkılıç, Scott Bauguess, Paul Brockman, Mariassunta Giannetti, Kathleen Weiss Hanley, Jennifer Marrieta-Westburg, Kasturi Rangan, and seminar participants at Case Western Reserve University, the U.S. Securities and Exchange Commission, the 2009 FMA Meeting, and the 2010 FIRS Meeting for their comments. They also thank Thomson Financial Services, Inc., for the I/B/E/S data provided as part of a broad academic program to encourage earnings expectation research. Nayar gratefully acknowledges financial support from the Hans Julius Bär Endowed Chair. Part of the work of this paper was completed when Nayar was with the U.S. Securities and Exchange Commission. This study expresses the authors' views and does not necessarily reflect those of the SEC, the Commissioners or other members of the SEC staff. Please address correspondence to Leonardo Madureira, 364 PBL Building, 11119 Bellflower Rd, Cleveland, OH 44106, tel: 216-368-5003, fax: 216-368-6249, e-mail: leonardo.madureira@case.edu.

1. Introduction

This paper examines the intersection of two strands of research; specifically, on disclosures by sell-side research analysts, and the informational advantage that can be gained from banking relationships. Evidence on the informativeness of analyst disclosures has been clearly documented by Frankel, Kothari and Weber (2006). There is also extensive literature that argues for and documents the unique role of financial intermediaries in information production and monitoring of borrowers. Leland and Pyle (1977) argue that financial intermediaries' *raison d'être* is to alleviate information asymmetries in capital markets. Campbell and Kracaw (1980) extend that argument to say that information production is important in conjunction with a portfolio of other services provided by financial intermediaries. Fama (1985) asks, "What's different about banks?", and makes the case that banks have an edge in information production because they provide "inside" loans via access to borrowers' information that has not been publicly revealed. If indeed bank loans are unique and enable privileged access to information, then within the realm of sell-side research, it may be argued that analysts employed by the lending institution (henceforth lending-affiliated analysts) should have an edge over other analysts where information gathering and contextual interpretation is concerned.

Another possibility that banks are "special" pertains to the incentive effects when loans are granted to borrowers. Specifically, when a loan is granted, the bank's own capital is at risk, and the borrower is subjected to increased scrutiny to reduce the lending risk faced by the bank (Billett, Flannery, and Garfinkel; 1995). Within the context of sell side research, an analyst may exert greater effort at analyzing firms with which the bank has a lending relationship due to this "skin in the game" effect. Thus, even if there is no incremental informational advantage provided by bank loans, the extra attention and analysis devoted to a borrower by the lending-affiliated analyst may make her sell-side research more meaningful.

The nature of this sell-side research associated with the lending affiliation is the focus of this paper. We examine (a) whether analysts associated with banks that have lending relationships with the covered firms provide more accurate earnings forecasts (relative to unrelated analysts covering the same firms); and if so, (b) whether this superiority manifests itself in other analyst disclosures. Additionally, we include investment banking/underwriting relationships and, within this more general realm, we explore whether such "related" sell-side research is likely to produce more accurate forecasts. There are arguments (and some evidence) to support claims both for and against the proposition that lending and underwriting relationships provide incentives for more meaningful research by related analysts. These divergent views point to an important empirical question which serves as the motivation underlying our paper.

We find that analysts associated with institutions that have lending relationships with a firm produce more accurate earnings forecasts relative to unrelated analysts. Notably, the improvement in accuracy for lending-affiliated analysts manifests itself only *after* the advent of the loan. This improvement is robust to the usual determinants of forecast accuracy – such as proxies for analysts' skill and experience, bank and firm characteristics – and it is orthogonal to the improvement in accuracy associated with analysts having underwriting relationships with the firm being covered (e.g., Malloy, 2005) and with all-star analysts (Stickel, 1992; Malloy, 2005; Chan et al, 2008; Fang and Yasuda, 2009). Finally, we show that the association between improved accuracy and lending relationship is robust to controlling for the endogeneity of the lending relationship decision.[1]

It must be recognized that accuracy can be empirically measured only when there is a verifiable *ex post* resolution of the measure used in analyst disclosures – which is indeed the case with earnings forecasts versus actual earnings. However, analysts also produce, among other information, firm

[1] The magnitude of the improvement in accuracy that we document is comparable to estimates reported for sell-side research in other contexts, such as the improvements reported for all-star analysts (Fang and Yasuda, 2009) and for analysts in close geographical proximity to their target firms (Malloy, 2005).

recommendations and long term growth rate (*LTG*) estimates, for which *ex post* resolution is less precise because the horizon for those forecasts is not well defined. The resolution is noisy also because the long-term nature of such forecasts makes it more challenging to disentangle other confounding reasons for the specific path of the variable being forecasted. We are interested in examining whether the apparent superiority of the lending-affiliated analysts in producing more accurate earnings forecasts extends to these "less precise" or "less-verifiable" disclosures as well. We show that firm recommendations and LTG forecasts from lending-affiliated analysts are more optimistic than those provided by analysts without a lending affiliation – both before and after the lending relationship starts. We also present evidence, based on the value extracted from firm recommendations, that the excess optimism displayed by lending affiliated analysts is unwarranted.

Affiliated analysts may have better access to information or they may be putting in greater effort after the advent of the bank loan. Either way, the joint evidence of more accurate earnings forecasts but a more optimistic bias in their recommendations, presents an interesting dichotomy. Taken together, our results indicate that lending institutions' sell-side analysts generate more meaningful research on their borrowers but employ this advantage selectively. They appear to provide more accurate research in earnings forecasts, where *ex post* verification is relatively straightforward. This is done probably to enhance their personal and professional reputation as analysts. Conversely, they tend to be overly optimistic about disclosures where accuracy is not easily resolved. Presumably, this unwarranted optimism arises from a desire to maintain/improve the reputation of a borrowing client, or to cozy up to the borrowers' management. Analysts may be willing to compromise in this manner because the probability of suffering a loss in their personal reputation is low, since the optimistic bias in their disclosures is less easily verifiable.

Our findings of increased accuracy of earnings forecasts after the advent of the loan for bank related analysts are consistent with the evidence on the commercial banking relationship literature suggesting that there are advantages to such liaisons. Universal banks can use information collected during commercial lending activities in their investment banking business involving the same client, to the benefit of both parties.[2] It is, therefore, not too surprising that commercial banks' lending relationships seem to play an important role in securing future underwriting business in common stock offerings (Drucker and Puri, 2005, Ljungqvist, Marston and Wilhelm 2006) and debt offers (Yasuda, 2005; Burch et al, 2005). Furthermore, there is evidence that institutional investors who participate in loan renegotiations and subsequently trade in the stock of the same company outperform other investors following the renegotiation (Ivashina and Sun, 2007). Additionally, since banks may suffer losses when borrowers default on loans, lending-affiliated analysts have added incentive to devote extra efforts to scrutinizing borrowers. This in-depth examination of borrowers justifiably should result in more meaningful sell-side research.

Improvements in accuracy are also consistent with the evidence on the securities underwriting side that investment banks possess information about their clients that other traders do not. The informational advantage allows investment banks to provide price support (Schultz and Zaman, 1994; Aggarwal, 2000) and act as the dominant market maker on trading volume (Ellis et al, 2000) and on liquidity and price discovery measures (Madureira and Underwood, 2008).

However, our findings are not obvious *ex ante*. In fact, there are several reasons that may prevent finding any association between sell-side research and banking relationships. For instance, the amount of information collected via the lending relationship may not be meaningful enough to provide a substantial advantage. Further, non-bank institutional investors steadily increased their share in the loan syndication pools from 25 percent (of the total *number* of participants) in mid-1990s to 70 percent

[2] For instance, Puri (1996) and Gande, Puri, Saunders and Walter (1999) document that commercial banks that make loans to firms and also underwrite their securities are able to obtain better prices (lower yields) for their clients' security offerings compared to investment banks. Narayanan, Rangan and Rangan (2004) find that gross spreads are lower for issues underwritten by lending banks relative to investment bank underwritten issues, suggesting economies of scope associated with information production due to lending.

in 2005. Thus, it is also conceivable for bank analysts not to be incentivized to devote as much attention to scrutinizing a borrower. With the advent of loan syndication pools, the risk exposure to banks from their loans declined, in turn allowing these entities to be (a) less meticulous in their information collection/research, and (b) lax in ongoing monitoring of the borrower. The ability to trade the syndicated shares in the secondary loan market may also have had a similar effect on commercial lenders' incentive to collect and process information about the borrower.[3]

On the investment banking side, the Chinese Wall between the underwriting and research departments is a potential barrier to information flows; this would be especially true following the implementation of Regulation Fair Disclosure (Reg FD) in 2000 by the *Securities and Exchange Commission*, which mandates simultaneous dissemination of material information to all investors for publicly traded companies. There is also the possibility that related analysts' estimates could reveal an optimistic bias; Lin and McNichols (1998) and Michaely and Womack (1999) document evidence that affiliated investment bankers' recommendations are significantly more favorable than those of unaffiliated analysts. Moreover, the informational advantage results have also been questioned.[4] In view of such mixed evidence in prior literature, our objective of exploring and documenting the nature of research produced by lending-affiliated analysts addresses an important empirical question.

The rest of the paper is organized as follows. The data description is in Section 2. Section 3 discusses the empirical methods employed in the measurement of earnings forecast accuracy and the attendant results. Robustness checks of these results are in Section 4. In Section 5, we examine less-verifiable disclosures, and whether lending affiliated analysts are unduly optimistic about firms with

[3] We believe, however, that the incentive problems arising from the presence of a secondary market may not be severe in our sample. First, the volume of such trading is modest; it amounted to $145 billion in 2003, which is equivalent to 19% of new originations on the primary market that year and to 9% of outstanding syndicated loan commitments (Blaise, 2004). Second, the secondary market is mostly for distressed debt (Dahiya, Puri and Saunders, 2003).

[4] Kroszner and Rajan (1997) provide contradictory results to Puri (1996). Like Puri, they also examine independent entities versus internal department-underwritten security offers. However, their results show that independent structures provide more credible certification and have significantly lower yields.

which they have a lending relationship. We discuss tests to examine the timing aspects of disclosures by lending affiliated analysts in Section 6. The conclusions appear in Section 7.

2. Data

Our basic sample involves data on loans and on sell-side research. The source for information on loans is the Loan Pricing Corporation (LPC) Dealscan database. This database contains data on origination of loans made to median and large sized companies, including information on the borrower, contractual terms, lead arrangers and, when pertinent, participants of a lending syndicate.

We obtain information on sell-side research outputs from the Institutional Brokers' Estimate System (IBES) database. We use data on analysts' estimates of earnings forecasts, long-term growth (LTG) and firm recommendations. For earnings forecasts, we focus on analysts' forecasts of one-year-ahead annual earnings. For each such forecast, the IBES detail files report information on the broker/analyst issuing the forecast, the issuance date, the value of the forecast and the firm and fiscal year it refers to.[5] We complement the forecast database with information on the actual earnings released by the firm. We then hand-match Dealscan's banks-list to the IBES brokers-list; recognizing when they are part of the same financial institution.

We obtain information on the sample firms' SEOs, particularly the lead underwriters and syndicate members that participated in the firm's equity offerings, from the Securities Data Corporation (SDC) database. These data are used to construct proxies for equity underwriting relationships between a bank and a firm (again, a hand-match is necessary to link SDC data to Dealscan and IBES). We obtain yearly data for all-star analysts from 1988 to 2004 from the

[5] We use the unadjusted files for earnings forecasts and actual earnings. Relying on the usual IBES files, which are adjusted for stock splits, would lead to loss of information. For example, forecasts of 13 and 10 cents per share, when adjusted for a 10-fold stock split, would be recorded on an adjusted basis both as 1 cent per share, hence masking the true dispersion of the forecasts.

Institutional Investor magazine.[6] Finally, we collect stock price data from CRSP and accounting data from COMPUSTAT.

Table 1 presents summary statistics on the match between the IBES and Dealscan datasets. A loan is of interest to our research only if the loan is provided by a bank that also offers sell-side research and to a firm that appears in the IBES database. We consider that a bank offers sell-side research services in one specific year if there is at least one earnings forecast issued by the bank in that year. Similarly, a firm appears in the IBES database in one specific year if there is at least one earnings forecast issued for the firm in that year.

Panel A of Table 1 has the summary statistics on the Dealscan loans that comply with this restriction. [7] Let's take the year 2000 as an example. Among the originations in Dealscan in that year, 2,658 were loans such that at least one lead arranger was a bank also providing sell-side research services and only 831 of them had a lead arranger also offering sell-side research on that specific borrower. The number of lending relationships created by these originations was slightly higher (979) than the number of loans, given that some loans had more than one lead arranger. Finally, from the point of view of firms, there were about 773 firms in the IBES database that borrowed money in 2000 according to Dealscan while about half of them borrowed from lead arrangers that were also providing them sell-side research services. [8]

[6] We thank Jonathan Clarke for supplying us with the all-star analyst ranking data.

[7] We do not aggregate different tranches of a loan. In other words, by a loan we mean an entry in the Dealscan database with a unique deal number. Therefore, the numbers in Panel A of Table 1 refer to the total number of tranches that were originated in 2000. Since our proxy for a lending relationship is merely whether there was *some* loan outstanding at one point in time, it is irrelevant whether the loan was dispersed in different tranches or not.

[8] The distribution of the loans over time indicates that the data is sparse during the early years of the sample. This is not unexpected. First, the Dealscan coverage has improved over the years. Second, and more important, before the Gramm-Leach-Bliley act was passed in 1999, banks operated under the Glass-Steagall Act of 1933, which precluded banks from combining investment banking and commercial banking activities until 1986. Given that sell-side research services tended to be associated with investment banking, this restriction implied that banks were barred from lending to firms for which they were also offering sell-side services. The lending relationships with sell-side research that indeed show up pre-1999 are an indication of the gradual relaxation of the Glass-Steagall Act that finally led to the passage of the Gramm-Leach-Bliley act in 1999.

Since our interest lies in analyzing forecast performance conditional on the presence of a lending relationship, it is important to assess whether there are differences between the two subsamples – that is, the sample of forecasts with, and forecasts without lending relationships. Panel B of Table 1 provides a first look at this issue. We compare some basic characteristics between firms with, and firms without *affiliated* loans. A firm is considered to have an affiliated loan in one specific year if there is at least one loan originated for the firm in that year with a lead arranger that also was providing sell-side forecasts for that firm in the same year.[9] The dimensions of comparison in Panel B are analyst coverage, firm market size, book-to-market ratio (BE/ME), and age. The first pair of columns corroborates the view that the number of firms with forecasts and loans from the same financial institution is very small in the beginning of the sample; it then increases through time but never reaches more than 20% of the IBES universe. The next set of columns show that the firms with affiliated loans are significantly bigger, covered by more analysts, and (with the exception of the late 1990s) older. On the other hand, there is no significant difference between value and growth firms in the likelihood of having affiliated loans in our sample period.

3. Forecast Accuracy

Our objective is to determine whether the performance of sell-side analysts is affected by the presence of a lending relationship. Paramount to this goal is to define how we measure analyst performance. Sell-side analysts disseminate information about firms in various forms – from reports detailing firm activities that are factual in nature to a myriad of forecasts about the firm's future path, most prominently earnings forecasts, stock recommendations and forecasts of long-term growth and target stock price. Analyst performance in this context refers to the accuracy of the information produced by the analyst. Specifically, for each such forecast, if there is a realization of the variable

[9] That is, in each year, the subsample of IBES firms with affiliated loans in Panel B corresponds to the data presented in the 6^{th} column in Panel A. Sample size differences, if any, in Panel B are due to missing data from CRSP and COMPUSTAT matches.

being forecasted, we can determine the performance of the analyst by comparing the forecast against its actual realization.

Our initial focus in this study is on analysts' one-year-ahead annual earnings-per-share (EPS) forecasts.[10] Our original data point is an analyst, *i*, issuing a forecast of what the earnings will be for a firm, *j*, in some fiscal year *t*.[11] Moreover, the analyst can update/revise her own forecast, or even cancel it, at any point before the firm releases the actual earnings, which means that a fourth dimension *k* (i.e., the forecast date) needs to be added to our specification. Therefore, our original data point is based on four dimensions: an analyst, *i*, a firm, *j*, a fiscal year, *t*, and the identification of the specific forecast date, *k*, among the potentially many forecasts issued by that analyst.

Each such forecast will have its own performance measure and we want to compare this performance across different characteristics – most notably whether a lending relationship exists between the forecaster's employer and the firm referenced in the forecast. Our research design is intended to serve a predictive role: having in hand a set of forecasts at some specific point in time, we design a model that predicts how to pick the "best" forecast among them. This specific point in time can be, for example, the day marking the end of the fiscal year. In this case, the set of forecasts will be the forecasts that were *outstanding* at the end of the fiscal year – where outstanding for an analyst, *i*, refers to the last forecast or revision of forecast that was issued (and not cancelled) by that analyst prior to the end of the fiscal year. By using only the most recent forecast by an analyst for a firm and a fiscal year, we are able to evaluate analysts with the same forecast horizon, and we eliminate the *k* dimension of our data points; our sample now refers to the forecasts $F_{i,j,t}$: for a firm *j* and some fiscal year *t*, it includes the outstanding forecasts by analyst *i* at that point in time.

[10] The comparison is straightforward in forecasts of earnings forecasts because the *ex post* realization of the forecasts becomes public knowledge at a very precise point in time – i.e., when the firm releases the actual earnings number at the end of the fiscal period which the forecast refers to. In Section 5, we also explore firm recommendations and long term growth rate forecasts, which are less precisely verifiable analyst disclosures.

[11] When not specified, we use the subscript *i* to refer interchangeably to the analyst and to the bank or broker employing the analyst. Sometimes, though, a distinction is required when dealing with the analyst or with the bank – e.g., an analyst but not its bank can be an all-star.

Therefore, for some cross-sectional cut-off point such as the day the fiscal year ends, we can collect all outstanding forecasts $F_{i,j,t}$, compute a performance measure from those forecasts, and run the regression model

$$Performance_{i,j,t} = \gamma_0 Relationship_{i,j,t} + \beta_X X_{i,j,t} \quad ,$$

where the left-hand side variable is the performance measure and the right-hand side includes the lending relationship proxy and other determinants of performance – represented by $X_{i,j,t}$ (discussed in the next Section). In order to facilitate the predictive nature of this model, we restrict each right-hand side variable to be public knowledge at the time the forecasts are collected.

The dependent variable in the model – the performance measure – is derived from the comparison between analysts' earnings forecasts and the actual earnings number. The computation of this performance measure follows next. First, assume we have a set of I outstanding forecasts $F_{i,j,t}$ that were issued by analysts i=1,...,I for some firm j regarding that firm's earnings for fiscal year t. Our measure of absolute performance for a forecast $F_{i,j,t}$ is simply its absolute error,

$$FERROR_{i,j,t} = | F_{i,j,t} - A_{j,t} | ,$$

where $A_{j,t}$ is firm j's actual earnings number for fiscal year t. An analyst performs well when her forecast error is small, with zero forecast error denoting perfect forecasting. When comparing two different analysts, we can infer the better analyst by simply finding the smallest forecast error between them. Therefore, this absolute measure of performance appears adequate to examine the performance of a set of analysts *at the level of a specific firm and for a specific fiscal year*.

Using the absolute forecast error as a proxy for performance becomes challenging when comparing forecasts across different firms and different periods. This is because the distribution of forecast errors differs across firms and across time. In other words, the absolute measure of performance, when applied to multiple firms and periods, can depend strongly on the characteristics of firm j and period t. Instead of trying to identify all such characteristics, we follow Hong, Kubik and Solomon (2000) and the more recent literature (e.g., Hong and Kubik, 2003; Loh and Mian, 2006; Ljungqvist, Marston and Wilhelm, 2006) in computing a measure of relative performance that normalizes accuracy at the level of the firm and the time period.

The normalized measure is computed from ranking all the forecast errors at the level of the firm, j, and fiscal year, t. The ranking proceeds such that the best forecast (i.e., smallest forecast error) has a $rank_{i,j,t}$ of 1, the 2nd best a rank of 2 and so on, up to the worst forecast having a rank equal to n, where n is the number of outstanding forecasts.[12] Therefore, a rank of 1 identifies the best forecaster at the level of some specific firm and specific earnings period – and by construction this identification does not depend on firm or period characteristics. Conversely, the identification of the worst forecast is still dependent on a firm characteristic – the number of outstanding forecasts: a rank of 3 denotes the worst forecaster for a firm having 3 outstanding forecasts but just the median forecaster for a firm having 5 outstanding forecasts. To avoid this problem, we scale the ranking to the number of outstanding forecasts, and create a score measure defined as:

$$Score_{i,j,t} = 100 - 100 * \left[\frac{rank_{i,j,t} - 1}{n - 1} \right].$$

The *Score* is thus a measure of accuracy performance normalized to be between 0 (the worst forecast) and 100 (the best forecast), with 50 being the average score amongst analysts following a firm in some fiscal year.

3.1 Univariate Results

[12] If two analysts have the same forecast error, they are assigned the average of the ranks they would receive in a simple sorting of the forecast errors.

Panel A of Table 2 presents summary statistics on the sample of forecasts used in our subsequent analyses. Recall that our research design compares forecasts that are outstanding at the same point in time with reference to the earnings period it refers to. For the data presented in Table 2 (and for most of the later tables) this reference point is 90 days before that fiscal year's actual earnings announcement. Our starting point, thus, includes the forecasts $F_{i,j,t}$ identified by the triple (broker *i*, firm *j*, year *t*); for each firm, *j*, and each fiscal year, *t*, between 1993 and 2004, we collect the forecast for fiscal year *t* by broker *i* for firm *j* that was outstanding 90 days before that year's actual earnings announcement.

To eliminate trivial cases, we further restrict the sample to only include forecasts for firm *j* and fiscal year *t* if there were outstanding forecasts for that firm and that specific fiscal year from at least two different brokers at the reference point discussed above. We call this the "All Earnings" sample and note that among this huge dataset, there are firms for which their forecasts are not made by any analyst with a lending relationship. Since the focus of our study is on the lending relationship, we then reduce this raw sample based on whether a lending relationship existed at the time the forecast was issued. More specifically, a lending relationship is a dummy equal to 1 if at the time $F_{i,j,t}$ was issued, there was an outstanding loan for firm *j* for which bank *i* was a lead arranger. We then define a "Lending Earnings" sample with the earnings for which at least one outstanding forecast had such lending relationship dummy set to 1.[13]

The presence of a lending relationship is associated with higher performance scores: the average score is 52.31 for the sample in which the bank issuing the forecast had also been a lead arranger for an active loan for that same firm, compared to an average score of 49.94 for the sample in which such lending relationship is not present. A similar picture emerges when looking at an alternative measure of forecast accuracy. Specifically, for each forecast, we define a dummy variable,

[13] A close look at the "All Earnings" sample corroborates the data in Table 1. The vast majority of the sample, (330,662 out of 338,031 observations or 97.82%) is composed of forecasts without a lending relationship as we have defined it.

“error below consensus”, that identifies whether its forecast error is smaller than the consensus forecast error amongst all outstanding forecasts. The results show that forecasts with lending relationships have forecast errors that are more likely to be smaller than the consensus forecast error when compared to forecasts without lending relationships (43.6% versus 41.2%).

Table 2 shows a positive association between the presence of a lending relationship and the ability to forecast annual earnings. We are cognizant of the fact that these are merely univariate inferences. In fact, lending-affiliated forecasts tend to be correlated with many other determinants of forecast accuracy[14] – found under the category of “General characteristics” in Panel A of Table 2. Forecasts with lending relationships are more recent, bolder, more likely to come from all-stars and bigger brokers, and from brokers that had also been lead underwriters in stock offerings (SEOs) prior to the issuance of that forecast. Finally, lending forecasts come from more experienced analysts.[15]

We further stratify the “All Earnings” sample with respect to underwriting relationships. In the “UWR Earnings” subsample, we include only fiscal year-end earnings announcements such that at least one forecast has an underwriting relationship and at least one other forecast does not have an underwriting relationship. We recognize an underwriting relationship is present for a forecast $F_{i,j,t}$ if at the time the forecast was issued the broker *i* had been a lead underwriter for an SEO by firm *j* in the

[14] Better forecasts have been shown to come from all-stars (Stickel, 1992; Malloy, 2005; Chan et al, 2008) and bigger brokerage houses (Malloy, 2005; Chan et al, 2008). Regarding brokerage houses that had been equity issuance underwriters for the firm being researched, the literature presents mixed results. While some present improvements in accuracy (Malloy, 2005; Jacob et al, 2008), others show no such effect (Dugar and Nathan, 1995; Lin and McNichols, 1998; Malmendier and Shanthikumar, 2009). Forecast accuracy is also associated with forecast age: accuracy increases as one approaches the earnings announcement (Ivkovic and Jegadeesh, 2004), and decreases with forecast age (Clement, 1999; Malloy, 2005). Clement and Tse (2005) show that bold forecasts are more accurate than herding forecasts. Their measure of boldness, measured at the time the forecast is issued, is a dummy equal to 1 if the newly issued forecast is above both the analyst’s prior forecast and the consensus, or else below both. Another potential driver of performance is analyst experience, both in terms of how long s/he has been issuing forecasts, and how long s/he has been issuing forecasts for that specific firm (e.g., Mikhail et al., 1997; Clement, 1999). Proxies for experience are the number of days since her/his first forecast (for any firm, and for that firm in particular) appears in IBES.

[15] The differences in performance measures are all significant, using a t-test of the null that the measures are the same between the two samples. The differences in firm characteristics are also significantly different, with the exception of the measures of scope of coverage. We prefer not to emphasize these differences, given their univariate nature. The results are available upon request.

two-year period prior to the earnings announcement date.[16] The patterns of the examined variables closely mimic the results obtained when categorizing the sample with respect to lending relationships: forecasts with underwriting relationship are associated with better measures of forecast performance, and they are also likely to be more recent, bolder forecasts, and they are more likely to come from all-stars and from bigger brokers, and from brokers with lending relationships.

Panel B of Table 2 provides a time series view of the frequency of forecasts with relationships (lending and underwriting relationships, and whether the forecast is issued by an all-star analyst). While the presence of forecasts from all-star analysts and forecasts with underwriting relationships seem stable through time, the fraction of forecasts with lending relationships is very small in the earlier years of the sample. This is consistent with the evidence on fewer loans available with affiliated sell-side research that was presented in Table 1. For example, with respect to the "Lending Earnings" sample, only 6.8% of the forecasts in 1993 have lending relationships, compared to roughly double that percentage for the years after 1999.

3.2 Regression Results

We now turn to regression methodologies in order to examine the association between forecast performance and lending relationship after controlling for other known and potential determinants of forecast performance. The basic model is

$$Score_{i,j,t} = \gamma_0 Lending_{i,j,t} + \beta_X X_{i,j,t} \quad , \tag{1}$$

where $Score_{i,j,t}$ is the specific measure of forecasting performance, and $Lending_{i,j,t}$ is a dummy variable for the lending relationship being examined. The control variables, $X_{i,j,t}$, are the bank, analyst, and forecasts' characteristics from Table 2 that were also deemed to be determinants of forecast performance. These are the all-star dummy, brokerage size, forecast age, scope of coverage,

[16] There are many alternatives in defining underwriting relationships. Bradley, Jordan and Ritter (2008), for example, discuss similarities between the roles of lead underwriters and co-managers. Our results are robust to including the co-managers in the definition of the underwriting relationship. We could also proxy the relationships by underwriting roles in initial public offerings (IPOs). Again, results are robust to this alternative.

analyst experience, coverage length, and forecast boldness. We transform some of the variables to make it suitable for use in a regression specification. For variables that are integers – brokerage size and forecast age – we use the log transformation. Other variables might present some time series patterns or dispersion across different firms that can confound results of a pooled regression. For example, analyst experience and coverage length are mechanically smaller in the earlier part of the sample when IBES coverage was limited. We normalize these variables (denoted by Var in the equation below) at the level of firm and earnings period to be between 0 and 1, using the transformation

$$Normalized\ \mathrm{Var}_{i,j,t} = \frac{\mathrm{Var}_{i,j,t} - \underset{j,t}{Min}\ \mathrm{Var}}{\underset{j,t}{Max}\ \mathrm{Var} - \underset{j,t}{Min}\ \mathrm{Var}}$$

We apply this normalization to the variables experience, scope of coverage and coverage length. The remaining variables – all-star and bold dummies – are indicator variables and thus do not require any transformation.

Panel A of Table 3 reports the results of regressions using the cross-section of outstanding forecasts as of 90 days prior to the earnings announcement for the "Lending Earnings" sample. [17] Models I and II report results of running regression equation 1 above. Model I reports pooled OLS results and model II reports Fama-MacBeth (1973) style coefficients from yearly regressions. The results corroborate the inferences from Table 2. Better scores are associated with bigger brokerage

[17] We employ multivariate analyses to help disentangle the effect of lending relationship from other determinants of forecast performance. For this purpose, we do not employ the "All Earnings" sample. As mentioned before, the vast majority of forecasts in the "All Earnings" sample are for pairs of firms and fiscal years for which no forecast with a lending relationship is present (about 83% of the observations). If we use this sample to analyze the effect of lending on accuracy, any such effect (even if it is significant for all of the 17% of earnings forecast observations where the firms being covered have at least one forecast with a lending relationship) will be swamped by the remainder of the sample without a lending relationship. In other words, the effect of the lending relationship will be obscured if we use the "All Earnings" sample since it is largely composed of forecasts without this effect. Moreover, given that we want to examine the relative performance of lending versus non-lending forecasts, the vast majority of the "All Earnings" sample does not help us in this task. In order to address this issue, we can focus on lending versus non-lending forecasts by using the subsample of earnings for which at least one forecast has a lending relationship and at least one forecast does not have such a lending relationship. This is our "Lending Earnings" sample.

houses and with younger and bold forecasts. All-star analysts also are better forecasters, and analysts that cover more stocks tend to provide worse forecasts. Results on analyst experience are mixed: while analyst's overall experience does not seem to matter for performance, the length of time the analyst has been covering that specific firm is important (although not robust to the Fama-MacBeth specification). Most importantly, after controlling for these other determinants of performance, the presence of a lending relationship is associated with a significant improvement in the score measure of approximately 1.73 (t-statistic of 4.50) for the pooled OLS model and 2.49 (t-statistic of 5.56) for the Fama MacBeth model.

To demonstrate further robustness of the association between forecast performance and the lending relationship, we run a logistic regression to examine the association between the lending relationship and the likelihood of the forecast having its "error below consensus". The results, shown in model III of Panel A of Table 3, corroborate the inferences from the prior two models using the score measure. In particular, the coefficient on the lending relationship dummy is significantly positive. Its odds-ratio (not reported in the table) suggests that the odds of finding the forecast error below the consensus forecast error are 13% higher when the forecast is issued in the presence of a lending relationship.

Are the inferences regarding the association between lending relationships and forecasting performance robust to using cross-sections of the outstanding forecasts at different points in time? Panel B of Table 3 examines this issue by rerunning model 1 with cross-sections of outstanding forecasts collected at different vintages relative to the earnings announcement day, from 180 days prior to the earnings announcement, up to the exact earnings announcement day. The coefficient on the lending relationship dummy is always significantly positive, and decreases only slightly as the timing approaches the earnings announcement day. Given the resilience of the results, for the rest of the paper we focus only on the cross-section of the outstanding forecasts collected 90 days prior to the earnings announcement day.

Before proceeding further, we note that we did not control for the presence of an underwriting relationship in those regressions. Given that an underwriting relationship might also be a source of informational advantage and some forecasts are issued with both relationships present, it might be the case that the lending dummy could be just a proxy for the underwriting relationship. We tackle both issues by rerunning the pooled OLS regression for score on different samples and including both relationship dummies. The results are shown in Table 4. The first sample is the one used in Table 3 (models I and II, "Lending Earnings" sample), and the second sample includes earnings for which at least one forecast was issued with an underwriting relationship (models III and IV, "UWR Earnings" sample).

In models I and II, the lending relationship dummy is highly significant and reflects the results previously discussed for Table 3. We also note that the underwriting dummy variable is insignificant in this sample (model II). However, in the "UWR Earnings" sample, the results are reversed. Specifically, the underwriting dummy, UWR, indicates an improvement of 2.19 in the performance score (t-statistic of 4.79) when the forecast is issued by a broker that has had an underwriting relationship with the firm, while the lending relationship dummy is insignificant.

These results suggest that the lending relationship and the underwriting relationship represent two different, separate and orthogonal effects. Namely, the lending relationship dummy, while significant in the "Lending Earnings" regression, is not significant in the "UWR Earnings" regression, and vice-versa. That these effects are each present separately in their own respective samples but not in the other reflects the fact that the intersection of these relationship dummies is limited. Recall from the summary statistics in Table 2 that only 7.1% of the forecasts with lending relationships also have an underwriting relationship.[18] It is telling that the coefficient estimate of the lending relationship dummy in Model I is similar in magnitude to the coefficient estimate of the UWR dummy in Model III.[19] Specifically, the improvement in forecast accuracy obtained via lending is similar to the improvement obtained through underwriting activities (e.g., Malloy; 2005 and Jacob et al; 2008).

4. Robustness of Improved Forecast Accuracy

In this section, we examine the robustness of our results by looking at partitions of the overall sample. We then try to uncover whether the association between lending relationship and forecasting performance is also mirrored by other characteristics of forecast leadership. We also examine the lending relationship as an endogenous choice of the bank issuing the forecast. Finally, we discuss the magnitude of the improvement in accuracy associated with a lending relationship.

4.1 Sorting by Firm Characteristics and Sample Period

A potential issue with the results seen so far is whether they are dependent on firm characteristics, or on the time period. Recall that the basic regression model does not include firm characteristics given that the dependent variable is normalized at the level of firm and earnings period. However, it is possible that our results do not extend to – or differ for – earnings reports from some types of firms. For example, it might be the case that the information advantage coming from having a lending relationship is more valuable for smaller firms, for which information asymmetry is more pronounced. Besides, the dispersion in the score measure is higher for earnings reported by firms with lower coverage: Analysts are more likely to have extreme performance measures, either 0 or a 100, if they cover firms with fewer forecasts, so the overall results on the score regressions might be dependent on the coverage level. Regarding sampling period, recall that the sample of lending

[18] In results not reported in the paper, we also construct an all-star earnings forecast sample. In this sample, the all-star dummy variable is highly significantly associated with score performance whereas the UWR and lending relationship dummies are not significant. These results are available upon request. Taken together with the results of Table 4, they suggest that the three effects – all-star, UWR, and lending – could be associated with different and separate dimensions of forecast accuracy.

[19] The difference in the estimated coefficients is insignificant.

relationships is rather thin in the earlier part of the sample, which raises the question whether the pooled results simply reflect the power of the lending relationship in the later part of the sample. On the other hand, the later part of sample coincides with the adoption of the Regulation Fair Disclosure (Reg FD), which curtailed privileged communication between firm managers and analysts (Gomes et al., 2004). If fear of litigation also curtailed the information flowing from the firm to a bank via a lending relationship, we should see the association between lending relationship and forecasting performance subsiding in the later part of the sample.

Therefore, in Table 5 we break our sample based on firm characteristics and on sub-periods. Each cell in the table shows the coefficient on the lending relationship dummy from running model I of Panel A in Table 3 on a specific subsample that is identified by some firm characteristic (identified by the row heading) and some period (identified by the column heading). The firm characteristics are the level of analyst coverage, firm size, and book-to-market ratio: low coverage (high coverage) firms are the ones with number of analysts following the firm in that year below (above) the median number of analysts amongst all firms in that year; small (big) firms are the ones with market value below (above) the median market value amongst all firms in that year; and value (growth) firms are the ones with book-to-market above (below) the median book-to-market amongst all firms in that year. The breakdown on period is between earnings before Reg FD (earnings announcement date prior or equal to year 2000) and earnings after Reg FD (earnings announcement date after 2000).

The results (first 2 columns of Table 5) show that the lending relationship effect is generally robust to subsamples of firms based on coverage, firm size, and book-to-market ratio when the whole sample period is used. Regarding the pre- and post-FD periods (middle and right hand side of Table 5, respectively), the lending coefficient is significantly positive for both periods when all firms are included. A more nuanced picture emerges, though, when the two dimensions are combined. The lending relationship effect seems smaller for the post-FD period, and tends to vanish for the sample of small and low-coverage firms. Finally, when value versus growth samples are combined with pre- and post-FD periods, the lending relationship effects seems much more robust for the sample of growth firms: the lending relationship leads to better forecasting performance for growth firms both pre- and post-FD, but is not associated with performance for the sample of value firms in either sub-period.

4.2 Timeliness and Forecast Frequency

Since analysts can issue and update their forecasts at any time, there is always the possibility that some analysts will herd after higher quality forecasts: If these analysts recognize that some analyst, A, with special attributes (skills, privileged access to information, etc.) will issue better forecasts, they will be better off updating their own forecasts – thus herding – after A issues her forecast. The results from Section 3 suggest that lending (and underwriting) relationships lead to higher quality forecasts. We ask in this section whether this higher quality is recognized by the overall community of analysts in terms of whether there is herding after forecasts that are issued by these "related" analysts.

We explore the herding possibility using the measure of forecast herding developed by Cooper, Day, and Lewis (2001). They identify a lead analyst by comparing the release times of forecasts by the other analysts (following the same stock in the same fiscal year) in the periods preceding and following each forecast by that analyst. More specifically, their leader-follower ratio (LFR) is constructed as follows. For each of the *K* forecasts by analyst *i* that were issued for firm *j* with respect to fiscal year *t*, we compute the number of days required to generate the two forecasts preceding ($t\_bef_{i,j,t,k,1}$ and $t\_bef_{i,j,t,k,2}$) and the two forecasts following ($t\_aft_{i,j,t,k,1}$ and $t\_aft_{i,j,t,k,2}$) each of these forecasts; these measures are summarized into the leader-follower ratio for the analyst as

$$LFR_{i,j,t} = \sum_{k=1}^{K}(t\_bef_{i,j,t,k,1} + t\_bef_{i,j,t,k,2}) / \sum_{k=1}^{K}(t\_aft_{i,j,t,k,1} + t\_aft_{i,j,t,k,2}),$$

where higher values of LFR imply analysts with a stronger leadership role.

We examine the LFR using a pooled OLS regression model similar to the one used for the score measure. Panel A of Table 6 presents some summary statistics for the LFR measure. The mean LFR is higher for analysts with lending relationships. More important, the statistic emphasizes the skewness of the LFR measure. For that reason, we use the log of LFR measure as the dependent variable in the regression. As control variables, we adopt a subset of the control variables from the score regression. Notice that our LFR measure is not computed for each individual forecast but rather for the set of forecasts issued by a broker/analyst *i* for a firm *j* in one specific earnings period. For that reason, control variables that are specific to each forecast (the bold dummy, and forecast age) are not used. For other control variables that still need a point of reference for measurement, such as analyst experience, we adopt the earnings announcement day as the reference point.

Models I and II of Panel B in Table 6 suggest that most significant determinants of forecasting performance are also drivers of forecast leadership: bigger banks, all-star analysts, and analysts with a smaller basket of equities to analyze are more likely to be leaders. Different to what happens with forecasting performance, analyst experience is associated negatively with leadership. Finally, when looking at the relationship dummies, an underwriting relationship is significantly associated with a higher LFR, but a lending relationship is not. The results suggest that, even as lending and underwriting relationships lead to improvements in forecasting performance that are similar, only underwriting relationships are recognized by the community of analysts for herding purposes whereas lending relationships are not.

We now ask whether the frequency with which analysts issue forecasts is explained by the relationships that impact forecasting performance. The frequency is defined as the number of forecasts that broker *i* issues for firm *j* with respect to fiscal year *t*. While forecast frequency tends to be correlated with forecast age (i.e., the more often you issue forecasts, the more likely it will be that your outstanding forecast is young) which is an important determinant of the score in the regressions in

Table 3, it is not clear that the presence of a relationship would drive the analyst to forecast more frequently. It might be the case that such analysts simply provide better forecasts, even if they are less or as frequent as their peers' forecasts. We attempt to answer this question by running a regression explaining the log of forecast frequency (again, the frequency measure is highly skewed, as showed in Panel A of Table 6). Results are shown in models III and IV of Panel B in Table 6. They show no association between lending relationship and forecast frequency, while a strongly positive association exists for the underwriting relationships.

4.3 The Endogeneity of the Lending Relationship

A potential problem with the regression specification in equation 1 is that the lending dummy is endogenous; that is, a bank decides whether to lend money to the firm for which it also provides earnings forecasts. This endogeneity raises the possibility that both the lending decision and the forecasting performance are associated with some other factor not included in our model – the omitted variable bias. In this section we address this possibility by explicitly accounting for the endogeneity of the lending relationship.

We address this endogeneity by applying the Heckman treatment effects model (Maddala, 1983, pp. 117- 122) – or dummy endogenous variable model – to the regression on forecasting performance. The Heckman treatment effects model analyzes the effect of an endogenously chosen binary variable (the treatment; in this case whether or not there is a lending relationship with a firm) on another continuous variable (the forecasting performance). The treatment effects model is an extension of the standard Heckman selection model. The difference between them is that in the treatment effects model there is no censoring of the data, i.e., the outcome is observed for everyone in the sample, the ones that were under the "treatment" and the ones that were not. The model is implemented through a two-step procedure, with the output of the decision or first-stage equation being used to construct a correction factor – the equivalent to the so-called Mills ratio in the selection model – that is added in the main regression. We derive the following model:

$$\begin{aligned} Score_{i,j} &= \gamma_0 Lending_{i,j} + \beta_{X,1} X_{i,j} + \beta_Y Y_{i,j} \\ Lending_{i,j} &= \beta_{X,2} X_{i,j} + \beta_{X,3} Z_{i,j} \end{aligned} \tag{2}$$

The Heckman treatment endogenizes the lending dummy by first running a probit on whether there is a lending relationship for a pair made of analyst *i* and firm *j* (the regression on lending) and then adjusting the second stage OLS (the score regression) for the Mills ratio. Notice also that in the regression model (2) the subscript *t* is not present – as compared to the regression model (1). This indicates that, instead of pooling the data from all years, we run the Heckman model each year, later aggregating results using the Fama-MacBeth (1973) style coefficients. This allows for the coefficients in the decision equation to be year-specific.

As in every selection model, the issue of an "instrumental variable" is important, that is, one needs a regressor that determines the first-stage regression but is not correlated with the error of the second stage regression. This is represented by the set of independent variables $Z_{i,j}$ in the decision equation. Technically we do not need them here, since the first-stage regression is non-linear, but the absence of instrumental variables can lead to substantial collinearity between the correction factor and the other regressors. Therefore, we adopt two such instrumental variables. First, we use a dummy for whether the bank has any loan (not for that firm) issued in that year; it identifies the commercial banks, but should not affect accuracy beyond broker size – which is another regressor in the second stage regression. The second instrument refers to a possible industry bias. It measures, for a pair (bank *i*, firm *j*), the fraction of firms in the same industry as *j* (but not including *j*) that have some loan outstanding from bank *i*. The idea is that it helps predict whether a firm has a lending relationship with a bank by looking at other firms in the same industry. This will be the case if banks lend with some sort of industry concentration.[20] On the other hand, it is not clear why that bias on loans would lead to better forecasting ability beyond the other drivers already included in the second stage regression.

Table 7 presents the Fama-MacBeth (1973) coefficients from running the endogenous model yearly. Panel A shows the first-stage regression with the determinants of the lending relationship. As predicted, both instrumental variables are important determinants of the lending relationship. Also, a lending relationship is associated with banks having an underwriting relationship with the firm.[21] Perhaps more surprisingly, bank size does not impact the lending decision very significantly (though this might be due to the high correlation between bank size and whether the bank has any loan outstanding) and the presence of an all-star analyst does not increase the odds of a lending relationship. Panel B of Table 7 reports the second-stage regression. The potential OLS bias from not treating the endogeneity can be analyzed by comparing this second-stage regression with the single OLS model in Table 3. Since Fama-MacBeth coefficients are used here, we compare them with the Fama-MacBeth coefficients from the OLS model, represented by model II of Panel A in Table 3. The lending relationship effect seems even more prominent for the model with the endogenous specification: the lending coefficient goes up to about 7.21, compared to 2.49 for the simple OLS model. Moreover, the Mills ratio is significantly negative, attesting that the selection issue is a problem and that the lending effects are understated when using simple OLS. Therefore, if anything, not treating for endogeneity just provides more conservative estimates of the effects of the presence of a lending relationship

[20] A concentration of loans in some industries is suggested by the data. We analyze for each bank and each year the fraction of firms being covered by the bank that also receives some loans from the bank in that year. If banks lent to firms randomly, we would see the distribution of borrowers across industries mimic exactly the industry distribution of firms receiving sell-side services. The alternative is that banks concentrate lending in some industries. To analyze this concentration, we compare, for each bank and each year, the Herfindahl index based on the fraction of borrowers in each 2-digit SIC industry with the Herfindahl index based on the fraction of covered firms in each of those industries. The Herfindahl index for the lending concentration is higher than the Herfindahl index for the coverage concentration for more than 90% of the bank-year observations. Results are available upon request.

[21] We acknowledge that the underwriting relationship can also be endogenous, though its decision is more likely to come also from the firm side. Nevertheless, the qualitative results in Table 7 do not change if the underwriting relationship dummy is removed from the first-stage regression.

between a bank and a firm on the forecasting performance of the sell-side analyst employed by that bank and issuing forecasts for the firm.

4.4 The Magnitude of the Improvement in Accuracy via Lending

Thus far, the improvement in *Score*, our proxy variable for accuracy as estimated through the coefficient of the lending variable, has values ranging from 1.73 (in the pooled OLS model), to 2.49 (in the Fama-MacBeth model), up to 7.21 (after correcting for the endogeneity of the lending decision). Dividing these number by 50 (the average score), we get estimates of the magnitude of improvements in the analyst's accuracy score ranging from 3.46% to 4.98% up to a 14.42%.[22] These estimates are on par with the economic significance that has been reported for improvements in analyst accuracy in other setups. For example, Fang and Yasuda (2009) find a significant 4.47% accuracy difference between all-star analysts and other sell side analysts, while Malloy (2005) reports a significant 2.77% difference for analysts in close geographical proximity of their covered firms relative to their more distantly located counterparts.

5. Less-Verifiable Analyst Disclosures

5.1 Are Less-Verifiable Disclosures by Lending Affiliated Analysts Different?

Besides earnings forecasts, analysts also make other disclosures about the firms they cover – amongst then estimates of long term growth (*LTG*) and firm recommendations. These disclosures have in common the fact that they are not verifiable to the same degree of precision as earnings forecasts. The absence of a clear and unambiguous resolution could tempt analysts to bias their disclosures to "cozy up" to the management of firms they cover without unduly risking their personal reputation. Consistent with this logic, Lin and McNichols (1998) report a bias in firm recommendations, but not in

[22] An idea about the importance of a forecast error improvement can be obtained by assuming that the price-earnings ratio for a given firm is fixed, let's say, P/E=*k*. That is, at the time of the earnings announcement E, the equilibrium stock price will be P=E**k*. What a lending coefficient of, let's say, 2.49 tells us is that, other things equal, a price estimate based on a forecast with a lending relationship reduces the average error of this estimate by 4.98%.

earnings forecasts.[23] With respect to these less-verifiable disclosures, what is of specific interest to our study is whether a lending relationship exacerbates or diminishes the bias in these disclosures.

To examine any bias or optimism that an analyst introduces into her less-verifiable disclosures, we collect firm recommendations and *LTG* forecasts from the IBES database.[24] Each sample point includes a recommendation (or *LTG* forecast), identified by the triple (broker *i*, firm *j*, year *t*). Specifically for each firm, *j*, and each fiscal year, *t*, between 1993 and 2004, we collect the recommendation (or *LTG* forecast) by broker *i* for firm *j* that was outstanding *D*=90 days before that year's actual earnings announcement. From these data, we create a measure of relative optimism. For recommendations, we employ a variable, *OptimisticREC*, which is a dummy variable equal to 1 whenever the recommendation is more optimistic than the median recommendation among all outstanding recommendations for firm, *j*, and fiscal year, *t*, available *D* = 90 days before that year's actual earnings announcement. Similarly for *LTG* forecasts, we define a dummy, *OptimisticLTG*, which indicates a more optimistic *LTG* forecast than the median forecast for that firm.[25,26]

Table 8 contains summary statistics for the *OptimisticREC* and the *OptimisticLTG* variables, together with several other characteristics from Table 2. In Panel A for firm recommendations, we note that percentage of firms where *OptimisticREC* is equal to 1 is a little higher for analysts with lending relationships (29.8%) than for analysts without a lending relationship (28.2%) who follow the same

[23] Extending their insight, we believe that biases may manifest not only in firm recommendations, but also in forecasts of *LTG* because of the reduced resolution associated with such disclosures.

[24] Ljungqvist, Malloy, and Marston (2009) show that the IBES recommendation files cut in the period 2002-2004 have problems related to errors in data handling. The IBES files we used were downloaded in 2009 and should be free from these data problems.

[25] Note that the firm recommendations variable in the IBES database spans the range from 1 to 5, with 1 being the most superior rating. Thus, when the *OptimisticREC* dummy variable is 1, the recommendation data point in the database for that lending analyst has a numerical value smaller than the median recommendation value for that firm.

[26] We can also define score variables based on recommendations and *LTG* projections. Running OLS regressions on these score measures yields results that are qualitatively similar with respect to the inferences obtained with the *OptimisticREC* and *OptimisticLTG* variables (results available upon request). Notice, though, that using a score variable for recommendations might not provide a good representation of their distribution since recommendations are stored as discrete variables.

firms. The evidence in Panel B related to *LTG* forecasts also suggests a similar interpretation as we noted earlier for stock recommendations (37.1% versus 35.8%).

We next report regression-based tests on the variables mentioned earlier and these results are provided in Table 9. The tests consist of logistic regressions using *OptimisticREC* and *OptimisticLTG* as dependent variables and independent variables to measure the presence of a lending relationship as well as various control variables. The net takeaway from Table 9 is that for both *OptimisticREC* and *OptimisticLTG*, the positive and statistically significant coefficient of the lending dummy variable, in the presence of various control variables, suggests that the presence of a lending relationship is associated with more optimism in firm recommendations and *LTG* forecasts.

5.2 Is the Optimism by Lending Affiliated Analysts Warranted?

We discussed in Table 9 how the presence of a lending relationship is associated with more optimism in less-verifiable disclosures. There are at least two possible explanations for this phenomenon. First, analysts with a lending relationship may have access to private information about the borrower, or may have analyzed the borrower more intensely because of the incentive effects, and consequently, might be justifiably more optimistic than other analysts. Alternately, lending-related analysts might be strategically distorting their view of the firm towards a more optimistic tone in order to curry favor with the firm managers or to enhance the reputation of the borrower and, in turn, shore up the image of their loan portfolio. In the first case, the excess optimism is warranted, while in the second case it reveals a bias on the analyst's part.

We now try to get a sense of whether the excess optimism inherent in recommendations issued in the presence of lending relationship is warranted. For this, we compare the value of recommendations coming from analysts with a lending relationship, which we denote as "lending recommendations," against the value of recommendations from their peers. Our examination employs the returns obtained from portfolios formed based on the investment advice of the recommendations.

More specifically, we follow the methodology in Barber, Lehavy and Trueman (2007) to construct portfolios based on recommendations and analyze whether those portfolios earn excess returns. Two basic portfolios are formed for each sample: (1) a portfolio *U* has stocks that were upgraded to buy or strong buy; and (2) a portfolio *D* has stocks that were downgraded to sell or strong sell. We then form long-short portfolios based on these individual portfolios.

To understand how each portfolio is constructed, take portfolio *U*, for example. When an analyst upgrades a stock towards a buy or strong buy, the stock is added to portfolio *U* at the close of the next trading day (by waiting one day, we make sure we take care of situations when a recommendation is issued after the close of the market). The stock is then kept in the portfolio until the *same* broker issues a recommendation that negates her previous investment advice (e.g., when the broker downgrades the stock, or the broker issues a stopped record for that firm) or at the end of a fixed holding window (in the data reported here, 90 days after the recommendation is issued), whichever comes first. This is to take care of instances where recommendations might become stale, or where an analyst gives up coverage without an explicit stopped record. (Results using various termination windows, such as 30 days, 60 days, or 180 days yield the same qualitative inferences reported herein.) After repeating this procedure for all recommendations of interest, we are left with a sample of which stocks are part of one or the other portfolio (*U* or *D*) each day.

We then compute daily returns for each portfolio. We assume equal dollar investment in each recommendation – for example, every time a stock enters portfolio *U*, the investor buys $1 of the stock and implements a buy-and-hold strategy on the stock as long as it is part of portfolio *U*. Under this strategy, the portfolio return at date *t* is

$$\frac{\sum_{s=1}^{n_t} x_{st} R_{st}}{\sum_{i=1}^{n_t} x_{st}}$$

where R_{st} is the gross return on stock S at time t, n_t is the number of stocks in the portfolio at day t, and x_{st} is the compounded daily return on stock S from the time it enters the portfolio until t-1. This procedure yields a time series of daily returns on each portfolio. We then report three measures of risk-adjusted returns: (1) the average excess return over the market portfolio, (2) the intercept from the Fama-French-Carhart 4-factor model (in-sample alpha); and (3) the average intercept from the same 4-factor model when we allow the factor loadings change over time (out-of-sample alpha).[27]

We form portfolios *U* and *D* for the sample of lending recommendations and for the sample of their peer recommendations. A subtle question is what to consider as a peer recommendation, that is, what exactly is the sample of recommendations *without* a lending relationship. It seems unfair to compare the lending recommendations simply to all the recommendations without a lending relationship – since these two samples might be substantially different, in terms of stocks being followed, the number of recommendations, etc. Our approach instead is to compare lending recommendations to the recommendations that are issued for the same firms around the same time by analysts without such lending relationship. More specifically, the sample of *non-lending* recommendations is defined as follows: when analyst *i* issues a recommendation for stock *j* at day *t*, and *i* has a lending relationship with *j*, we consider as *non-lending* all other recommendations issued for stock *j* around day *t* from analysts that do not have a lending relationship with *j*. (Results reported here are based on a 90-day window around the lending recommendation day.)

Table 10 presents our results. For the sample of lending recommendations, the portfolio of upgrades *U* has abnormal returns (excess returns, in-sample alpha, and out-of-sample alpha) that are

[27] The estimation of the out-of-sample alphas follows an approach similar to Brennan, Chordia, and Subrahmanyam (1998) and Chordia, Subrahmanyam, and Anshuman (2001). For each day *t* in our sample period, we first regress the daily excess returns of each portfolio on the Fama-French-Carhart four factors during the preceding 3 years of daily data thus obtaining the estimated factor loadings of that portfolio. We compute the out-of-sample alpha of the portfolio for that day as the realized excess return of the portfolio less the expected excess return calculated from the realized returns on the factors and the estimated factor loadings.

indistinguishable from zero. On the other hand, the portfolio of downgrades *D* yields significantly negative returns. Had one followed the advice to (short) sell from lending recommendations, a significant 11 basis points a day (using the out-of-sample alpha measure) of abnormal performance would have resulted.[28] For the sample of non-lending recommendations the results are reversed. That is, for the same sample of firms recommended by affiliated analysts, an investor would have earned significantly positive risk-adjusted returns (excess, in-sample, and out-sample) from portfolio *U* but not anymore with portfolio *D*.

The asymmetric performance results discussed above suggest an optimistic bias in lending recommendations. First, with respect to upgrades, unwarranted optimism by lending affiliated analysts indicate that more stocks are upgraded than justified. As a result, the population of upgrades by lending affiliated analysts will naturally contain a larger proportion of false positives (i.e., undeserved upgrades). Due to this high fraction of false positives, upgrades by lending affiliated analysts do not produce excess performance. Corroborating this view, Panel C of Table 10 shows that portfolio that goes long on upgrades from non-lending recommendations and goes short on upgrades from lending recommendations yields significantly positive abnormal returns.

On the other hand, for downgrades towards sell, the optimistic bias by lending analysts diminishes the desire to downgrade a stock in the normal course of events. Consequently, when a downgrade is indeed provided by a lending analyst, it is probably made only (reluctantly) when there is little doubt that the future performance of the covered firm will be negative. Thus, the population of stocks downgraded by lending affiliated analysts, for covered firms to which loans have been granted, will contain very few false positives. It is indeed this sample that exhibits abnormally negative stock price performance. (Again, a long-short portfolio based on downgrades reveals the superior investment

[28] Even if portfolio *D* is formed in only 1,652 days out of a universe of 2,709 days, and we assume holding a cash position over the days when the portfolio is not formed, that still amounts to about 6.7 basis points average daily abnormal return over the entire period of 2,709 days.

value from lending downgrades.) Taken together, the evidence on buy and sell recommendations supports the hypothesis of unwarranted optimism by lending affiliated analysts.

6. Timing Aspects of Analyst Disclosures

The results thus far point to two phenomena associated with disclosures by lending affiliated analysts: (1) An a*ccuracy phenomenon*, wherein for the clearly verifiable earnings forecasts, analysts with a lending relationship display superior accuracy relative to their peers, and (2) an *optimism phenomenon*, whereby, for the less-verifiable disclosures of firm recommendations and LTG forecasts, analysts with a lending relationship appear to be associated with more (and, given our results on the value of recommendations, unwarranted) optimistic views relative to their peers. We next explore the timing aspects related to these phenomena. Specifically, are these phenomena apparent prior to, or only after the advent of the lending arrangement? These tests are crucial to refine the link between the loan and the accuracy/optimism of analysts.

To implement our statistical tests, we employ a dummy variable, *Before Lending*, associated with the analyst output (be it a firm recommendation $R_{i,j,t}$, a long-term projected growth rate, $LTG_{i,j,t}$, or an earnings forecast $F_{i,j,t}$) that equals 1 when two conditions are met: (1) if at the time the analyst output was issued, there was no outstanding loan for firm *j* for which the analyst's bank *i* was a lead arranger, and (2) if a new loan for firm *j* for which the analyst's bank *i* was a lead arranger is provided in the following year. Our results for the estimations with the observations prior to the loan being granted are shown in Models I through III of Table 11 and our discussion below of these results focuses on the variable of interest, the *Before lending* dummy variable.[29]

In model I, the dependent variable in the OLS regression is *Score*, which measures the accuracy of the analyst's forecast of earnings per share. The insignificant coefficient on the *Before lending*

[29] Notice that in this case the regression does not have a predictive role, as one of the right-hand side variables, *Before Lending*, is not necessarily known by the investors at the cross-sectional point in time when the other information is collected.

dummy variable for model I indicates that the analyst whose institution subsequently grants a loan to the covered firm is not more accurate, on average, compared to her peers before the advent of the loan. This evidence suggests that prior to grant of the loan, the analyst does not enjoy superior forecasting ability. In the logistic regressions of models II and III, the dependent variables are the optimism measures, *OptimisticREC* and *OptimisticLTG*, respectively. For both models, the coefficient of the *Before Lending* dummy variable is significantly positive, suggesting that the analyst at the institution that subsequently enters into a loan with the covered firm is significantly more optimistic than her peers even before the loan is granted.

We then augment the data used in models I through III of Table 11 with data from the post-loan period, including in the regressions the *Lending* dummy variable that was used earlier in the paper. Models IV through VI of Table 11 present the results of these tests. In model IV, which examines accuracy of earnings forecasts, the coefficient of the *Before lending* dummy variable continues to be statistically insignificant, while the coefficient of the *Lending* dummy is significantly positive: Lending-affiliated analysts improve forecast accuracy *after* the advent of the loan but not before, suggesting that the improvement can thus be attributed to the lending relationship – either the lending affiliated analyst has better access to information or is researching the client firm more intensely.

With respect to analyst optimism, models V and VI in Table 11 indicate that the coefficients of the *Before Lending* and *Lending* dummy variables are both positive and statistically significant. In fact, within each model, their point estimates are very similar to one another. This suggests that analysts with lending institutions are, on average, optimistic in their non-verifiable disclosures (long-term projected growth rate (*LTG*) and broker recommendations) prior to the advent of the loan, and continue in that vein afterwards as well.

7. Conclusion

There is extensive literature which argues that commercial banks have privileged access to a firm's private information as a consequence of their lending arrangements (e.g., Bhattacharya and Chiesa, 1995; Rajan and Winton, 1995) or have greater monitoring incentives and capabilities than other participants (Billett, Flannery and Garfinkel, 1995). Prior research suggests that significant benefits accrue to the lender from relationship banking. Arguably the lenders' better insight into the borrowers' business activity gives it an informational edge. Further, the lender has greater incentive to analyze the borrower due to the risk inherent in the loans granted to the borrower. In this paper, we examine this issue from a new, previously unexplored, perspective. We examine how successfully "related" sell-side analysts with lending ties are able to analyze the borrower.

We provide evidence that the presence of a lending relationship between a bank and a firm is associated with more accurate forecasts of earnings per share for that firm. In more refined tests that examine the timing aspects of this superior accuracy, we find that accuracy is significantly improved following the advent of the loan. Importantly, this improved accuracy is orthogonal to the more accurate forecasts reported for analysts with underwriting relationships and in forecasts associated with all-star analysts. Furthermore, analysts with an underwriting connection (and all-star analysts) appear to be "leaders" in the sense of having other analysts herding on their forecasts. On the other hand, we find that analysts with a lending connection are not regarded as leaders. Thus, while a similar improvement in accuracy is associated with both lending and underwriting relationships, only analysts with the latter relationship lead the herd and their forecasts are followed by the community of analysts. Perhaps that is a tacit recognition that sell-side research tends to be more intrinsically linked to underwriting business than to the bank's commercial lending department.

Our examination of earnings forecasts reveals evidence consistent with arguments in prior literature that relationship banking is "different". Specifically, we are the first to show that lending affiliated analysts produce more accurate forecasts of earnings per share after the advent of the loan. This evidence demonstrates that there are significant effects associated with commercial lending liaisons. This improved accuracy could arise from an inside view into the borrower via the loan process, or through extra effort exerted in analyzing the borrower because the bank has capital at risk in the loan.

Interestingly, lending affiliated analysts do not exploit this advantage to make their other less-verifiable disclosures superior to those of their peers. The overall evidence on the two types of disclosures indicates that lending analysts use their research edge selectively. It appears that analysts specifically use the informational advantage gained via the lending relationship to achieve improved accuracy of forecasts for which there is clear *ex post* resolution, thus enhancing their personal reputation and human capital. However, for the less-verifiable disclosures (LTG forecasts and firm recommendations), lending analysts are willing to compromise on the accuracy of their estimates and tend be unduly optimistic.

There are two possible inter-related reasons for this. First, by being unduly optimistic, they could be currying favor from management of the borrower firm to obtain future lending business. Second, by promoting the financial standing of the firm to whom their institution has lent money, they are enhancing the image of the loan portfolio of their employer, thereby enabling reductions in risk-based capital requirements. Since these disclosures cannot be easily verified *ex post*, the lending bank's analyst does not bear the cost of being unduly optimistic but realizes the benefits mentioned earlier. This selective use of the research edge gained through the lending relationship is a new result in the literature on sell side research and the banking arena.

References

Aggarwal, R. 2000. Stabilization Activities by Underwriters After Initial Public Offerings. *Journal of Finance* 55(3): 1075-1103.

Barber. B., R. Lehavy and B. Trueman. 2007. Comparing the Stocks Recommendation Performance of Investment Banks and Independent Research Firms. *Journal of Financial Economics* 85: 490-517.

Bhattacharya, S. and G. Chiesa. 1995. Proprietary Information, Financial Information and Research Incentives. *Journal of Financial Intermediation* 4: 328-357.

Billett, M. T., M. J. Flannery, and J. A. Garfinkel, 1995. The Effect of Lender Identity on a Borrowing Firm's Equity Return. *Journal of Finance* 50: 699-718.

Blaise, G. 2004. The Syndicated Loan Market: Structure, Development and Implications. *BIS Quarterly Review* December.

Bradley, D., B. Jordan and J. Ritter. 2008. Analyst Behavior Following IPOs: The "Bubble Period Evidence. *Review of Financial Studies* 21: 101-33.

Brennan, M., T. Chordia and A. Subrahmanyam. 1998, Alternative Factor Specifications, Security Characteristics, and The Cross-Section of Expected Stock Returns. *Journal of Financial Economics* 49: 345–373.

Burch, T. R., V. Nanda and V. Warther. 2005. Does it Pay to be Loyal? An Empirical Analysis of Underwriting Relationships and Fees. *Journal of Financial Economics* 77(3): 673-699.

Campbell, T. S. and W. A. Kracaw. 1980. Information Production, Market Signalling and the Theory of Financial Intermediation, *Journal of Finance* 35 (4): 863-882.

Chan, L. K. C., D. Ikenberry, J. Lakonishok, S. Lee. 2008. Are Analysts All Alike? Identifying Earnings Forecasting Ability. *Journal of Investment Management* 6, no. 2: 4-22.

Chordia, T., A. Subrahmanyam, and V. Anshuman. 2001, Trading Activity and Expected Stock Returns. *Journal of Financial Economics* 59: 3–32.

Clement, M., 1999. Analyst forecast accuracy: Do Ability, Resources, and Portfolio Complexity Matter? *Journal of Accounting and Economics* 27: 285-304.

Clement. M. B., S. T. Tse. 2005. Financial Analyst Characteristics and Herding Behavior in Forecasting. *Journal of Finance* 60: 307-341.

Cooper, R., T. E. Day, C. M. Lewis. 2001. Following the Leader: A Study of Individual Analysts' Earnings Forecasts. *Journal of Financial Economics* 61: 383-416.

Dahiya, S., M. Puri and A. Saunders. 2003. Bank Borrowers and Loan Sales: New Evidence on the Uniqueness of Bank Loans. *Journal of Business* 76(4): 563-582.

Diamond, D. 1984. Financial Monitoring and Delegated Monitoring. *Review of Economic Studies* 51: 393-414.

Diamond, D. 1991. Monitoring and Reputation: The Choice between Bank Loans and Directly Placed Debt. *Journal of Political Economy* 91(4): 689-721

Drucker, S. and M. Puri. 2005. On the Benefits of Concurrent Lending and Underwriting. *Journal of Finance* 60(6): 2763-2799.

Dugar, A., S. Nathan. 1995. The Effects of Investment Banking Relationships on Financial Analysts' Earnings Forecasts and Investment Recommendations. *Contemporary Accounting Research* 17, 1-32.

Ellis, K., R. Michaely and M. O'Hara. 2000. When the Underwriter is the Market Maker: An Examination of Trading in the IPO Aftermarket. *Journal of Finance* 55(3): 1039-1074.

Fama, E. 1985. What's Different about Banks. *Journal of Monetary Economics* 15: 29-39.

Fang, L., and A. Yasuda. The Effectiveness of Reputation as a Disciplinary Mechanism in Sell-side Research. *Review of Financial Studies* 22, 3735 - 3777.

Fama, E., J. MacBeth. 1973. Risk, Return, and Equilibrium: Empirical Tests. *Journal of Political Economy* 81: 607-636.

Frankel, R., S. P. Kothari, and J. Weber. 2006. Determinants of the Informativeness of Analyst Research. *Journal of Accounting and Economics* 41, 29 - 54.

Gadanecz, B. 2005. The Syndicated Loan Market: Structure, Development and Implications. *BIS Quarterly Review* December: 75-89.

Gande, A., M. Puri, A. Saunders and I. Walter. 1997. Bank Underwriting of Debt Securities: Modern Evidence. *Review of Financial Studies* 10: 1175-1202.

Gomes, A., G. Gorton, L. Madureira. 2004. SEC Regulation Fair Disclosure, Information, and the Cost of Capital. *Journal of Corporate Finance* 13: 300-334.

Hong, H., J. Kubik. 2003. Analyzing the Analysts: Career Concerns and Biased Earnings Forecasts. *Journal of Finance* 58: 313-351.

Hong, H., J. Kubik and A. Solomon. 2000. Security Analyst's Career Concerns and Herding of Earnings Forecasts. *RAND Journal of Economics*. 31: 121-144.

Ivashina, V. and Z. Sun. 2007. Institutional Stock Trading on Loan Market Information. Harvard Business School Working paper.

Ivkovic, Z., N. Jegadeesh. 2004. The Timing and Value of Forecast and Recommendation Revisions *Journal of Financial Economics* 73: 433-463.

Jacob, J., S. Rock, and D. Weber. 2008. Do Non-Investment Bank Analysts Make Better Earnings Forecasts? *Journal of Accounting, Auditing, and Finance* 23 (Winter): 23-60.

Kroszner, R. S. and R. G. Rajan. 1997. Organization Structure and Credibility: Evidence from Commercial Bank Securities Activities before the Glass-Steagall Act. *Journal of Monetary Economics* 39(3): 475-516.

Leland, H. E. and D. H. Pyle. 1977. Information Asymmetries, Financial Structure, and Financial Intermediation. *Journal of Finance* 32(2): 371-387.

Lin, H. and M. F. McNichols. 1998. Underwriting relationships, analysts' earnings forecasts and investment recommendations. *Journal of Accounting and Economics* 25: 101-127.

Ljungqvist, A., F. Marston and W. J. Wilhelm Jr. 2006. Competing for Securities Underwriting Mandates: Banking Relationships and Analyst Recommendations. *Journal of Finance* 61(1): 301-340.

Ljungqvist, A., C. Malloy, and F. Marston. 2009. Rewriting History, *Journal of Finance* 64: 1935-1960.

Loh, R. K., and G. M. Mian. 2006. Do accurate earnings forecasts facilitate superior investment recommendations? *Journal of Financial Economics* 80:455-483.

Maddala, G. 1983. *Limited-Dependent and Qualitative Variables in Econometrics*. Cambridge University Press, New York.

Madureira L. and S. Underwood 2008. Information, Sell-Side Research and Market Making, *Journal of Financial Economics* forthcoming.

Malmendier, U. and D. Shanthikumar, 2009. Do Security Analysts Speak in Two Tongues? Working Paper.

Malloy, C. J. 2005. The Geography of Equity Analysis. *Journal of Finance* 60: 719-754.

Michaely, R., and K. Womack. 1999. Conflicy of Interest and the Credibility of Underwriter Analyst Recommendations. *Review of Financial Studies* 12: 653-686.

Mikhail, M. B., B. R. Walther, R. H. Willis. 1997. The Development of Expertise: Do Security Analysts Improve Their Performance with Experience? *Journal of Accounting Research* 35: 131-57.

Narayanan, R. P., K. P. Rangan and N. K. Rangan. 2004. The Role of Syndicate Structure in Bank Underwriting. *Journal of Financial Economics* 72(3): 555-580.

Puri, M. 1996. Commercial Banks in Investment Banking: Conflict of Interest or Certification Role? *Journal of Financial Economics* 40: 373-401.

Rajan, R. G. and A. Winton. 1995. Covenants and collateral as incentives to monitor. *Journal of Finance* 50: 1113-1146.

Schultz, P. and M. A. Zaman. 1994. Aftermarket Support and Underpricing of Initial Public Offerings. *Journal of Financial Economics* 35(2): 199-219.

Stickel, E. S. 1992. Reputation and Performance Among Security Analysts. *Journal of Finance* 47: 1811-1836.

Yasuda, A. 2005. Do Bank Relationships Affect the Firm's Underwriter Choice in the Corporate-Bond Underwriting Market. *Journal of Finance* 60(3): 1259-1292.

Table 1. Loans and IBES Samples: Summary Statistics

This table presents summary statistics for the sample of loans and firms receiving loans. Panel A describes the match between LPC loans and the IBES database. The first column lists the number of loans that are initiated by banks that also have sell-side research in the same year. A bank is considered to have sell-side research services when there is at least one annual earnings forecast issued by that bank (for any firm) in the IBES database. The second column further restricts the first column by requiring that at least one lead arranger of the loan be offering sell-side research in that same year for the firm receiving the loan. The third column counts the number of lending relationships derived from the loans in column 1, where one lending relationship is identified by a lead arranger of the loan also offering sell-side research services for the firm receiving the loan. The fourth column counts the number of firms that were available in IBES (IBES firms) who also received loans in that year. The fifth column counts the number of IBES firms that (1) receive a loan and (2) also receive sell-side services from one of the loan's lead arrangers. Panel B further describes the firms in the fifth column by comparing their characteristics with the IBES firms that do not receive loans in the same year. The dimensions of comparison are analyst coverage, firm market size, book-to-market ratio (BE/ME), and age. Analyst coverage is the number of analysts covering the firm during the year. Market size is the market value of equity (in $ billions) at the end of the fiscal year. BE/ME is the ratio of book equity to market equity, where book equity and market equity are the last data available in COMPUSTAT prior to the end of the fiscal year. Book equity is the book value of equity plus deferred taxes and investment tax credit, minus the book value of preferred stock (proxied by – where available – redemption, liquidation, or par value). Firm age is the number of years from the date the firm is first listed in CRSP to the end of the fiscal year. Numbers in italics represent significant differences at the 1% confidence level.

Panel A: The match between Dealscan loans, IBES firms, and sell-side coverage

Year	# loans initiated by banks with sell-side research	# loans with affiliated sell-side research	# lending relationships	# IBES firms receiving loans	# IBES firms with affiliated lending relationships
1990	70	9	9	25	7
1991	132	40	44	45	21
1992	80	12	12	39	9
1993	158	48	57	57	15
1994	268	33	36	85	18
1995	248	61	86	103	36
1996	548	105	129	217	60
1997	836	282	418	328	170
1998	1,026	326	466	342	184
1999	1,972	652	901	564	285
2000	2,658	831	979	773	304
2001	2,419	904	1,195	766	372
2002	2,214	1,033	1,370	769	439
2003	2,089	921	1,251	767	426
2004	2,334	1,091	1,446	893	510

Table 1. (*Continued*)

Panel B: Characteristics of IBES firms receiving loans

Year	#obs		Analyst coverage		Market size		BE/ME		Firm age	
	with loan	w/out loan	with loan	w/out loan	with loan	w/out loan	with loan	w/out loan	with loan	w/out loan
1993	15	3,818	*22.53*	*7.21*	*8.39*	*0.21*	0.55	0.54	*36.87*	*14.50*
1994	18	4,192	*16.83*	*6.77*	*1.46*	*0.19*	0.38	-0.13	*21.39*	*13.89*
1995	35	4,464	*15.63*	*6.67*	*1.83*	*0.22*	0.47	0.56	16.00	13.42
1996	59	5,057	*14.32*	*6.21*	*1.04*	*0.23*	0.42	0.48	16.42	12.51
1997	164	5,168	*11.20*	*5.96*	*0.79*	*0.24*	0.40	0.57	13.39	12.19
1998	176	5,093	*10.44*	*6.24*	*0.93*	*0.23*	0.29	0.54	13.81	12.51
1999	277	4,782	*13.16*	*6.23*	*1.79*	*0.28*	0.52	0.60	*19.71*	*12.36*
2000	293	4,463	*14.00*	*6.01*	*2.88*	*0.26*	0.55	2.06	*22.87*	*12.11*
2001	361	3,777	*12.81*	*5.66*	*4.60*	*0.33*	0.58	0.55	*27.24*	*12.69*
2002	424	3,550	*12.30*	*5.87*	*3.21*	*0.30*	0.61	0.67	*26.26*	*13.41*
2003	415	3,467	*13.58*	*6.12*	*3.87*	*0.47*	0.52	0.54	*26.63*	*14.19*
2004	485	3,500	*12.67*	*6.03*	*3.94*	*0.56*	0.42	0.45	*26.43*	*14.11*

Table 2. Forecasts: Univariate Statistics

This table reports summary statistics for the main samples of forecasts used in the paper. Panel A presents aggregated statistics and Panel B yearly summary statistics. The sample includes annual earnings forecasts $F_{i,j,t}$, identified by the triple (broker *i*, firm *j*, year *t*); for each firm *j* and each fiscal year *t* between 1993 and 2004, we collect the forecast for fiscal year *t* by broker *i* to firm *j* that was outstanding *D*=90 days before that year's actual earnings announcement. We present three sets of summary statistics based on further restrictions on the data. The *All earnings* sample includes all earnings for which at least two outstanding forecasts are available. The *Lending earnings* sample includes earnings such that at least one outstanding forecast presents a lending relationship, recorded as *Lending*=1; *Lending* is a dummy equal to 1 if at the time $F_{i,j,t}$ was issued there was an outstanding loan for firm *j* for which bank *i* was a lead arranger. The *UWR earnings* sample includes earnings such that at least one outstanding forecast had the variable *UWR*=1; *UWR* is a dummy equal to 1 if at the time $F_{i,j,t}$ was issued the broker *i* had been a lead underwriter for an SEO from firm *j* in the two-year period prior to the issuance date. *All star* is a dummy equal to 1 if the analyst issuing $F_{i,j,t}$ was awarded all star status by Institutional Investor magazine in the previous year. *Brokerage size* is the number of firms being covered by broker *i* in the year *t*. *Forecast* age is the number of days forecast $F_{i,j,t}$ has been outstanding since its issuance. *Scope of coverage* is the number of firms being covered by the analyst that issued $F_{i,j,t}$. *Analyst experience* is the number of days since that analyst first appeared in the IBES database. *Coverage length* is the number of days since the analyst that issued $F_{i,j,t}$ first issued a forecast for firm *i*. *Bold* is a dummy equal to 1 if $F_{i,j,t}$ was above both the analyst's prior forecast and the consensus, or else below both. To compute the *score* for a forecast $F_{i,j,t}$, we first rank the forecast errors $FERROR_{i,j,t}=|F_{i,j,t}- A_{i,t}|$ of all outstanding forecasts for firm *j* and fiscal year *t* available *D* days before that year's actual earnings announcement, where $A_{j,t}$ is firm *j*'s actual earnings number for fiscal year *t*. The ranking proceeds such that the best (smallest) forecast error has a $rank_{i,j,t}$=1, the 2nd best a rank of 2 and so on, up to the worst forecast having a rank equal to *n*=the number of outstanding forecasts; the score for some specific forecast is obtained by scaling this rank measure as $score_{i,j,t}=100-100*(rank_{i,j,t} – 1)/(n – 1)$. The *Error below consensus* (*Error equal consensus*) is a dummy equal to 1 is a dummy equal to 1 whenever the forecast error $FERROR_{i,j,t}$ is below (equal to) the median forecast error amongst all outstanding forecasts for firm *j* and fiscal year *t* available *D* days before that year's actual earnings announcement.

Panel A: Aggregated summary statistics

	Sample: All earnings		Sample: Lending earnings		Sample: UWR earnings	
	Lending	Non- Lending	Lending	Non-Lending	UWR	Non-UWR
# forecasts	7,369	330,662	7,369	52,620	6,090	37,401
Performance						
Error below consensus (mean)	43.6%	41.2%	43.6%	39.4%	45.1%	40.1%
Error equal consensus (mean)	18.3%	17.1%	18.3%	19.1%	17.6%	17.9%
Score (mean)	52.31	49.94	52.31	49.62	52.78	49.52
General characteristics						
Lending (mean)	100.0%	0.0%	100.0%	0.0%	8.5%	2.2%
UWR (mean)	7.1%	1.7%	7.1%	1.3%	100.0%	0.0%
All star (mean)	37.0%	17.4%	37.0%	20.6%	38.3%	15.4%
Brokerage size (# firms, median)	993	379	993	517	957	381
Forecast age (# days, median)	19	30	19	21	25	28
Analyst experience(# days, median)	2,194	2,075	2,194	2,118	2,396	1,899
Coverage length (# days, median)	834	739	834	867	601	574
Scope of coverage (# firms, median)	13	13	13	13	14	13
Bold (mean)	72.1%	69.0%	72.1%	70.3%	73.9%	69.1%

Table 2. (*Continued*)

Panel B: The distribution of relationships over time

	Sample: All earnings				Sample: Lending earnings				Sample: UWR earnings			
Year	#	% lending	% UWR	% all star	#	% lending	% UWR	% all star	#	% lending	% UWR	% all star
1993	26,346	0.2%	1.4%	23.9%	898	6.8%	1.2%	29.6%	3,315	0.3%	10.9%	25.6%
1994	27,558	0.2%	1.4%	24.7%	857	6.9%	1.1%	31.2%	2,951	0.3%	12.7%	25.0%
1995	28,549	0.3%	1.3%	21.8%	1,218	7.3%	1.8%	26.9%	2,920	0.5%	13.1%	23.0%
1996	30,313	0.4%	1.8%	16.3%	1,693	8.0%	1.9%	18.2%	3,871	0.7%	13.9%	16.7%
1997	31,358	0.9%	1.9%	15.4%	2,833	10.4%	2.0%	18.1%	4,234	1.6%	14.0%	16.2%
1998	31,376	1.9%	1.6%	16.3%	4,764	12.7%	1.5%	20.6%	3,676	2.7%	13.8%	19.1%
1999	31,389	2.6%	1.5%	17.0%	6,719	12.1%	1.6%	21.4%	3,685	4.2%	12.6%	20.2%
2000	29,791	2.9%	1.7%	15.3%	7,268	12.0%	1.6%	20.3%	4,153	3.3%	12.5%	15.6%
2001	25,796	4.0%	2.0%	16.6%	7,880	13.1%	2.0%	23.2%	3,906	4.4%	13.4%	15.4%
2002	24,380	4.6%	2.4%	16.7%	7,811	14.2%	2.6%	24.6%	3,484	6.4%	16.9%	18.7%
2003	25,307	4.2%	2.2%	16.1%	8,635	12.4%	2.1%	24.6%	3,603	5.6%	15.7%	17.1%
2004	25,868	4.7%	2.6%	14.5%	9,413	13.0%	2.4%	22.4%	3,693	6.1%	18.2%	14.5%

Table 3. Lending Relationships: Regressions

This table presents results of regressing measures of accuracy of forecasts for annual earnings. The observations are annual earnings forecasts $F_{i,j,t}$, identified by the triple (broker *i*, firm *j*, year *t*); for each firm *j* and each fiscal year *t* between 1993 and 2004, we collect the forecast for fiscal year *t* by broker *i* to firm *j* that was outstanding *D* days before that year's actual earnings announcement. Panel A reports results using D=90 while panel B relies on different values of *D*. We further constrain the sample to include only earnings such that at least one outstanding forecast presents a lending relationship (the *Lending sample* from Table 2). The dependent variable, in models I and II of panel A and the regressions in panel B, is the accuracy score for the forecast $F_{i,j,t}$. To compute the score for a forecast $F_{i,j,t}$, we first rank the forecast errors $FERROR_{i,j,t}=|F_{i,j,t}- A_{i,t}|$ of all outstanding forecasts for firm *j* and fiscal year *t* available *D* days before that year's actual earnings announcement, where $A_{i,t}$ is firm *i*'s actual earnings number for fiscal year *t*. The ranking proceeds such that the best (smallest) forecast error has a $rank_{i,j,t}$=1, the 2^{nd} best a rank of 2 and so on, up to the worst forecast having a rank equal to *n*=the number of outstanding forecasts; the score for some specific forecast is obtained by scaling this rank measure as $Score_{i,j,t}=100-100*(rank_{i,j,t} – 1)/(n – 1)$. The dependent variable in model III of panel A is a dummy equal to 1 whenever the forecast error $FERROR_{i,j,t}$ is below the median forecast error amongst all outstanding forecasts for firm *j* and fiscal year *t* available *D* days before that year's actual earnings announcement. *Lending* is a dummy equal to 1 if at the time $F_{i,j,t}$ was issued there was an outstanding loan for firm *j* for which bank *j* was a lead arranger. *All star* is a dummy equal to 1 if the analyst issuing $F_{i,j,t}$ was awarded all star status by Institutional Investor magazine in the previous year. *Brokerage size* is the normalized measure of the number of firms being covered by broker *i* in the year *t*. *Forecast age* is the normalized number of days forecast $F_{i,j,t}$ has been outstanding since its issuance. *Scope of coverage* is the normalized number of firms being covered by the analyst that issued $F_{i,j,t}$. Analyst experience is the normalized number of days since that analyst first appeared in the IBES database. *Coverage length* is the normalized number of days since the analyst that issued $F_{i,j,t}$ first issued a forecast for firm *i*. *Bold* is a dummy equal to 1 if $F_{i,j,t}$ was above both the analyst's prior forecast and the consensus, or else below both. Model I of panel A and all models in panel B present results of simple OLS regression; model II of panel A presents average parameter values from running yearly Fama and MacBeth (1973) cross-sectional regressions; and model III of panel A presents results of a probit regression. The reported R^2s and number of observations for model II are the time-series averages of the yearly cross-sectional regression measures. The t-statistics are shown in brackets.

Panel A: Accuracy regressions

	Dep=Score		Dep=Error below consensus
	I: OLS	II: FM	III: Logistic
Intercept	48.81	52.01	0.00
	[60.50]	[23.99]	[0.00]
Lending	1.73	2.49	0.13
	[4.50]	[5.56]	[22.92]
All star	1.30	2.27	0.06
	[4.00]	[2.99]	[6.19]
Brokerage size	0.56	0.36	0.02
	[4.67]	[1.93]	[7.27]
Forecast age	-3.82	-4.23	-0.21
	[-44.96]	[-12.60]	[1203.47]
Scope of coverage	-3.62	-2.87	-0.19
	[-8.54]	[-3.58]	[41.32]
Analyst experience	0.27	-0.44	0.04
	[0.58]	[-0.53]	[1.92]
Coverage length	1.05	1.23	0.04
	[2.42]	[1.44]	[1.55]
Bold	2.56	3.10	0.14
	[9.48]	[4.74]	[55.95]
# obs	59,132	4,928	59,132
R^2	3.91%	6.00%	3.30%

Table 3. (*Continued*)

Panel B: OLS regression on score based on different cross-sections

	Cross-section established *D* days prior to the actual earnings announcement				
	D=180	*D*=90	*D*= 60	*D*= 30	*D*=0
Intercept	52.18	48.81	56.95	56.13	55.35
	[61.61]	[60.50]	[63.84]	[63.75]	[63.90]
Lending	1.68	1.73	1.76	1.45	1.40
	[4.12]	[4.50]	[4.59]	[3.80]	[3.69]
All star	1.79	1.30	1.50	1.35	1.18
	[5.24]	[4.00]	[4.59]	[4.16]	[3.66]
Brokerage size	0.26	0.56	0.42	0.31	0.31
	[2.04]	[4.67]	[3.48]	[2.56]	[2.62]
Forecast age	-3.72	-3.82	-5.12	-4.48	-4.13
	[-39.81]	[-44.96]	[-39.29]	[-36.50]	[-34.85]
Scope of coverage	-2.99	-3.62	-2.85	-2.54	-3.26
	[-6.71]	[-8.54]	[-6.75]	[-6.06]	[-7.83]
Analyst experience	-0.63	0.27	0.00	-0.34	-0.47
	[-1.30]	[0.58]	[0.00]	[-0.74]	[-1.03]
Coverage length	0.69	1.05	0.79	1.23	1.63
	[1.50]	[2.42]	[1.82]	[2.85]	[3.79]
Bold	3.39	2.56	2.02	1.29	0.72
	[11.87]	[9.48]	[7.53]	[4.88]	[2.76]
# obs	55,083	59,132	59,720	60,858	61,485
R^2	3.51%	3.91%	2.84%	2.35%	2.15%

Table 4. OLS Regressions on Score for Different Samples

This table presents results of OLS regressions on measures of accuracy of forecasts for annual earnings. The observations are annual earnings forecasts $F_{i,j,t,}$ identified by the triple (broker *i*, firm *j*, year *t*); for each firm *j* and each fiscal year *t* between 1993 and 2004, we collect the forecast for fiscal year *t* by broker *i* to firm *j* that was outstanding *D*=90 days before that year's actual earnings announcement. Further sampling restrictions are established as follows. The *Lending earnings* sample includes earnings such that at least one outstanding forecast presents a lending relationship (i.e., one $F_{i,j,t}$ with *Lending*=1). The *UWR earnings* sample includes earnings such that at least one outstanding forecast had the variable *UWR*=1. UWR is a dummy equal to 1 if at the time $F_{i,j,t}$ was issued, the broker *i* had been a lead underwriter for an SEO from firm *j* in the two-year period prior to the earnings announcement date. The dependent variable in each regression is the accuracy score for the forecast $F_{i,j,t}$, as defined in Table 2. The other independent variables are as defined in Table 3. The t-statistics are shown in brackets.

	Sample: Lending Earnings		Sample: UWR Earnings	
	I	II	III	IV
Intercept	48.81	48.82	50.45	50.42
	[60.50]	[60.44]	[50.67]	[50.56]
Lending	1.73	1.72		-0.39
	[4.50]	[4.44]		[-0.45]
UWR		0.25	2.17	2.19
		[0.28]	[4.76]	[4.79]
All star	1.30	1.30	1.21	1.21
	[4.00]	[3.98]	[2.81]	[2.81]
Brokerage size	0.56	0.56	0.41	0.42
	[4.67]	[4.64]	[2.75]	[2.78]
Forecast age	-3.82	-3.82	-3.93	-3.93
	[-44.96]	[-44.96]	[-36.70]	[-36.69]
Scope of coverage	-3.62	-3.62	-2.00	-2.00
	[-8.54]	[-8.54]	[-4.14]	[-4.14]
Analyst experience	0.27	0.27	0.87	0.86
	[0.58]	[0.58]	[1.80]	[1.79]
Coverage length	1.05	1.05	-1.14	-1.14
	[2.42]	[2.41]	[-2.47]	[-2.47]
Bold	2.56	2.56	3.14	3.14
	[9.48]	[9.47]	[9.38]	[9.38]
# obs	59,132	59,132	42,526	42,526
R^2	3.91%	3.91%	3.69%	3.69%

Table 5. Sorting by Firm and Characteristics and Period

This table analyzes the relationships effects conditional on firm characteristics. The table presents the coefficients on *Lending* and *All star* from running the regression model IV in Table 4 on different subsamples, characterized by the intersection of firm (row headings) and period (column headings) characteristics. Low coverage (high coverage) firms are the ones with number of analysts following the firm in that year below (above) the median number of analysts amongst all firms in that year. Small (big) firms are the ones with market value below (above) the median market value amongst all firms in that year. Value (growth) firms are the ones with book-to-market above (below) the median book-to-market amongst all firms in that year. Earnings before FD (earnings after FD) include all earnings for which the end of the fiscal year was before or equal to (after) 2000.

	All earnings		Earnings before FD		Earnings after FD	
	Coeff on Lending	Coeff on All star	Coeff on Lending	Coeff on All star	Coeff on Lending	Coeff on All star
All firms	1.73	1.30	2.25	1.69	1.50	1.09
	[4.50]	[4.00]	[3.80]	[3.45]	[2.98]	[2.50]
Low coverage	1.24	0.68	1.80	0.43	0.85	0.98
	[2.52]	[1.46]	[2.40]	[0.62]	[1.30]	[1.58]
High coverage	2.17	1.95	2.57	3.11	2.29	1.16
	[3.40]	[4.24]	[2.67]	[4.53]	[2.81]	[1.87]
Small firms	1.41	1.37	1.70	1.00	1.09	1.62
	[2.71]	[2.80]	[2.17]	[1.36]	[1.58]	[2.50]
Big firms	1.62	1.27	2.43	2.27	1.65	0.67
	[2.78]	[2.92]	[2.60]	[3.47]	[2.20]	[1.15]
Growth firms	2.11	1.44	3.03	3.27	1.62	0.76
	[3.74]	[3.09]	[3.47]	[3.38]	[2.20]	[1.21]
Value firms	1.13	1.34	1.25	1.41	1.11	1.32
	[2.05]	[2.85]	[1.48]	[1.97]	[1.57]	[2.10]

Table 6. Analyzing the Leader-Follower Ratio (LFR) and Forecast Frequency

This table analyzes measures of timelines and frequency of analysts' forecasts. The observations are triples (broker *i*, firm *j*, year *t*), such that broker *i* had at least one forecast issued for firm *j* with respect to that firm's annual earnings in year *t*; the sample includes earnings such that at least one forecast presents a lending relationship. The measure of timeliness is the leader-follower ratio (LFR), which is constructed as follows. For each of the *K* forecasts by broker *i* that were issued for firm *j* with respect to fiscal year *t*, we compute the number of days required to generate the two forecasts preceding ($t\_bef_{i,j,t,k,1}$ and $t\_bef_{i,j,t,k,2}$) and the two forecasts following ($t\_aft_{i,j,t,k,1}$ and $t\_aft_{i,j,t,k,2}$) each of these forecasts; these measures are summarized into the leader-follower ratio as

$$LFR_{i,j,t} = \sum_{k=1}^{K}(t\_bef_{i,j,t,k,1} + t\_bef_{i,j,t,k,2}) / \sum_{k=1}^{K}(t\_aft_{i,j,t,k,1} + t\_aft_{i,j,t,k,2}).$$

Forecast frequency is the number of forecasts that broker *i* issued for firm *j* with respect to fiscal year *t*. Panel A shows summary statistics for these measures. Panel B shows results of OLS regressions on the log of the LFR and forecast frequency measures. The independent variables in panel B are as defined in Table 3. The t-statistics are shown in brackets.

Panel A: Summary statistics

	Sample: Lending earnings						Sample: UWR earnings					
	Lending			Non-lending			UWR			Non-UWR		
	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
LFR	1.63	0.01	148	1.50	0.01	140	1.75	0.01	90	1.51	0.01	142
Forecast frequency	8.02	1.00	40	7.52	1.00	56	6.52	1.00	28	6.00	1.00	43

Panel B: OLS regressions

	Dep=log(LFR)		Dep=log(forecast frequency)	
	Sample: Lending Earnings	Sample: UWR Earnings	Sample: Lending Earnings	Sample: UWR Earnings
	I	II	III	IV
Intercept	-0.2876	-0.2641	1.6245	1.5343
	[-14.71]	[-11.51]	[124.78]	[104.81]
Lending	0.0132	0.0036	-0.0042	0.0085
	[1.27]	[0.16]	[-0.60]	[1.13]
UWR	0.0489	0.1081	-0.0251	0.1795
	[2.05]	[9.03]	[-1.58]	[12.35]
All star	0.1335	0.0626	0.1062	0.0536
	[15.21]	[5.62]	[18.14]	[7.52]
Brokerage size	0.0619	0.0548	0.0483	0.0280
	[19.03]	[14.11]	[22.32]	[11.33]
Scope of coverage	-0.0900	0.0057	0.0563	0.0613
	[-7.86]	[0.45]	[7.40]	[7.69]
Analyst experience	-0.0513	-0.0427	-0.1437	-0.1381
	[-4.15]	[-3.39]	[-17.47]	[-17.32]
Coverage length	-0.0207	-0.0186	0.2366	0.3044
	[-1.76]	[-1.56]	[30.26]	[40.32]
# obs	58,678	41,700	59,132	42,529
R^2	1.73%	1.28%	4.20%	5.34%

Table 7. Controlling for the Endogeneity of the Lending Relationship

This table presents average parameter values from running monthly Fama and MacBeth (1973) cross-sectional regressions of accuracy of forecasts for annual earnings. The sample is the same as in Table 3, with the cross-section of forecasts established *D*=90 days prior to the annual earnings announcements, and only earnings (such that at least one outstanding forecast presents a lending relationship) are included. Panel A presents the first-step regression of determinants of the lending relationship that is used for the two-step consistent estimation and panel B reports results of the main regression in the two-step consistent model, equation (X). The dependent variable in panel A is a dummy variable, equal to 1 if at the time $F_{i,j,t}$ was issued there was an outstanding loan for firm *j* for which bank *i* was a lead arranger; the dependent variable in panel B is the Score of $F_{i,j,t}$ as defined in Table 3. The variable "Broker has some loan" is a dummy equal to 1 if broker *i* has arranged at least one loan for an IBES firm in the past year (not necessarily for firm *j*). The variable "Industry bias" measures, for the pair (broker , firm *j*), the fraction of firms in the same 2-SIC industry as *i* (but not including *i*) that have some loans outstanding from the bank associated with broker *i*. The other independent variables are as defined in Table 3. The parameter values are the average of the cross-sectional regressions. In brackets are the Fama-MacBeth (1973) t-statistics. The reported R^2s and number of observations are the time-series averages of the monthly cross-sectional regression measures.

Panel A: First-step regression, determinants of lending		Panel B: Second-step regression, determinants of score	
Intercept	-3.45	Intercept	53.02
	[-8.02]		[24.06]
Brokerage size	0.06	Lending	7.21
	[1.76]		[3.64]
Broker has some loan	1.82	All star	2.13
	[5.15]		[2.75]
Industry bias	21.48	Brokerage size	0.64
	[3.83]		[1.00]
All star	-0.05	Forecast age	-3.32
	[-2.38]		[-6.07]
UWR	0.71	Scope of coverage	0.39
	[7.19]		[0.39]
# obs	4,744	Analyst experience	0.53
R^2	23.2%		[0.71]
		Coverage length	-2.79
			[-3.58]
		Bold	3.10
			[4.76]
		Mills ratio	-3.06
			[-2.58]
		# obs	4,744
		R^2	5.8%

Table 8. Recommendations and Long-Term Projections: Univariate Statistics

This table reports summary statistics for the main samples of non-verifiable disclosures related to firm recommendations and long-term growth projections (*LTG*) used in Tables 8 through 10. Panel A presents aggregated statistics for recommendations and Panel B for *LTG*. Each sample includes recommendations (LTG forecast), identified by the triple (broker *i*, firm *j*, year *t*); for each firm *j* and each fiscal year *t* between 1993 and 2004, we collect the recommendation (LTG forecast) by broker *i* for firm *j* that was outstanding *D*=90 days before that year's actual earnings announcement. The table below pertains to the *Lending earnings* sample which includes earnings such that at least one outstanding recommendation (LTG forecast) presents a lending relationship, recorded as *Lending*=1; *Lending* is a dummy equal to 1 if at the time the recommendation (LTG) was issued there was an outstanding loan for firm *j* for which bank *i* was a lead arranger. *UWR* is a dummy equal to 1 if at the time such recommendation (LTG) was issued the broker *i* had been a lead underwriter for an SEO from firm *j* in the two-year period prior to the issuance date. *All star* is a dummy equal to 1 if the analyst issuing the recommendation (LTG) was awarded all star status by Institutional Investor magazine in the previous year. *Brokerage size* is the number of firms being covered by broker *i* in the year *t*. *Forecast* age is the number of days the recommendation (LTG) has been outstanding since its original issuance. *Scope of coverage* is the number of firms being covered by the analyst that issued the recommendation (LTG). *Analyst experience* is the number of days since that analyst first appeared in the IBES database. *Coverage length* is the number of days since the broker that issued the recommendation (LTG) first issued a forecast for firm *i*. *Bold* is a dummy equal to 1 if the outstanding forecast for the same firm was issued above both the analyst's prior forecast and the consensus, or else below both. *OptimisticREC* is a dummy variable equal to 1 whenever the recommendation is more optimistic than the median recommendation amongst all outstanding recommendations for firm *j* and fiscal year *t* available *D* days before that year's actual earnings announcement. Similarly, *OptimisticLTG* is a dummy variable equal to 1 whenever that analyst's forecast of *LTG* is above the median for that firm.

Panel A: Aggregated summary statistics for Recommendations

	Sample: Lending Earnings	
	Lending	Non-Lending
# recommendations	6,723	42,332
Optimism variable		
OptimisticREC	29.8%	28.2%
General characteristics		
Lending (mean)	100.0%	0.0%
UWR (mean)	7.3%	1.4%
All star (mean)	37.1%	22.4%
Brokerage size (# firms, median)	1,007	562
Forecast age (# days, median)	20	20
Analyst experience(# days, median)	2,172	2,101
Coverage length (# days, median)	819	859
Scope of coverage (# firms, median)	13	13
Bold (mean)	72.0%	71.3%

Panel B: Aggregated summary statistics for Long-Term Growth Projections

	Sample: Lending Earnings	
	Lending	Non-Lending
# LTG projections	4,700	26,992
Optimism variable		
OptimisticLTG	37.1%	35.8%
General characteristics		
Lending (mean)	100.0%	0.0%
UWR (mean)	6.8%	1.4%
All star (mean)	40.7%	26.3%
Brokerage size (# firms, median)	993	618
Forecast age (# days, median)	20	19
Analyst experience(# days, median)	2,353	2,281
Coverage length (# days, median)	917	1,001
Scope of coverage (# firms, median)	13	13
Bold (mean)	74.2%	72.8%

Table 9. Recommendations and Long-Term Growth Projections: Regressions

This table presents results of logistic regressions on measures of relative optimism in recommendations and long-term growth (LTG) projections. The variables are defined in Table 8. The dependent variable, *OptimisticREC*, in model I is a dummy equal to 1 whenever the recommendation is more optimistic than the median recommendation amongst all outstanding recommendations for firm, *j*, and fiscal year, *t*, available *D*=90 days before that year's actual earnings announcement. Similarly, *OptimisticLTG*, is a dummy variable equal to 1 whenever the analyst's forecast of LTG is more optimistic than the median *LTG* forecast for that firm. The independent variables are defined in Table 8. The t-statistics are shown in brackets.

	Dep= *OptimisticREC*	Dep= *OptimisticLTG*
	I	II
Intercept	0.00	-0.41
	[0.00]	[17.93]
Lending	0.25	0.14
	[71.37]	[16.18]
All star	-0.01	-0.08
	[0.18]	[7.76]
Brokerage size	-0.24	-0.12
	[464.69]	[72.79]
Forecast age	0.08	0.11
	[102.08]	[188.67]
Scope of coverage	0.05	0.06
	[1.76]	[2.03]
Analyst experience	0.09	-0.10
	[5.47]	[4.63]
Coverage length	0.06	0.09
	[2.49]	[4.18]
Bold	0.00	-0.04
	[0.03]	[2.18]
# obs	49,055	31,692
R^2	2.00%	1.35%

‘

Table 10. Performance of Portfolios Based on Recommendations

This table reports the statistics on composition and returns for portfolios of upgrades to strong buy and buy and portfolios of downgrades to sell and strong sell. Panel A (Panel B) reports results for portfolios created based on lending (non-lending) recommendations. Panel C reports results from forming zero-cost long-short portfolios combining samples of Panels A and B. A lending recommendation is a recommendation that is issued when a lending relationship between the issuer and the firm being recommended is present. A non-lending recommendation is a recommendation that is issued (1) when the issuer does *not* have a lending relationship with the firm being recommendations and (2) the recommendation is within 90 days of a lending recommendation issued for the same firm. For each sample and each portfolio, the table shows the number of days that the portfolio was defined, the average daily number of stocks in the portfolio, the average number of unique firms in the portfolio, the average daily excess returns (measured as portfolio return minus market return), the in-sample alpha and the out-of-sample alpha. The in-sample alpha is obtained as the intercept of a regression of daily excess returns on the four Fama-French-Carhart factors. The out-of-sample alpha is the average intercept from the same 4-factor model when we allow the loadings on the factor to change over time. The recommendations sample period is from April 1st, 1994 through December 31st, 2004. For the return measures, t-statistics are shown in brackets.

	# days	Average # of stocks	Average # of unique stocks	Excess return	In-sample alpha	Out-of-sample alpha
Panel A: Non-Lending Recommendations						
Upgrades from non-lending recommendations (U^{NL})	2,709	68.43	43.77	0.0418	0.0363	0.0388
				[2.9693]	[2.6879]	[2.9272]
Downgrades from non-lending recommendations (D^{NL})	2,634	9.97	8.00	-0.0376	-0.0306	-0.0155
				[-1.1737]	[-0.9806]	[-0.4232]
Panel B: Lending Recommendations						
Upgrades from lending recommendations (U^{L})	2,709	26.80	25.84	0.0145	0.0044	0.0068
				[0.9344]	[0.3060]	[0.6342]
Downgrades from lending recommendations (D^{L})	1,652	6.41	6.26	-0.1431	-0.1601	-0.1099
				[-2.6930]	[-3.1098]	[-2.1899]
Panel C: Long-Short Portfolios						
U^{NL}-U^{L}	2,709	95.27	56.62	0.07608	0.0471	0.0472
				[2.8374]	[3.1219]	[3.2213]
D^{NL}-D^{L}	1,652	20.18	15.37	0.1879	0.1747	0.1553
				[2.8865]	[2.9571]	[2.3597]
U^{NL}-D^{L}	1,652	87.14	56.72	0.1663	0.1921	0.1489
				[2.8229]	[3.6915]	[2.9435]

Table 11. Regressions to test for timing aspects of accuracy performance and optimism

This table presents results of regressing measures of accuracy in forecasts of earnings per share, and relative optimism in recommendations and long-term growth (*LTG*) projections. The regression models are extensions of those used to examine forecast accuracy in Table 3 and relative optimism in Table 9. The models below employ a dummy variable that marks the period before a lending relationship is established. More specifically, *Before Lending* is a dummy equal to 1 if at the time the recommendation $R_{i,j,t}$ or long-term growth rate projection $LTG_{i,j,t}$ or earnings per share forecast, $F_{i,j,t}$, was issued there was no outstanding loan for firm *j* for which bank *i* was a lead arranger, and a new loan for firm *j* for which bank *i* was a lead arranger was provided in the following year. All control variables are as defined in Table 8. The dependent variable in model I in each panel is the measure of forecast accuracy, $Score_{i,j,t}$. The dependent variables in model II and III in each panel are *OptimisticREC* and *OptimisticLTG*, respectively. The former (latter) is a dummy variable which equals 1 whenever the analyst provides a broker recommendation (LTG forecast) that is more optimistic than its median for firm j and fiscal year t available D=90 days before that year's actual earnings announcement. The sample in Panel A for model I (II; III) includes earnings such that at least one outstanding forecast (recommendation, LTG) presents *Before Len*ding equal to 1. The sample in Panel B for model I (II; III) includes earnings such that at least one outstanding forecast (recommendation, LTG) presents either *Before Lending* equal to 1 or *Lending* equal to 1. The R^2 is the adjusted-R^2 in the case of OLS regression and pseudo-R^2 in the case of logistic regressions. The t-statistics are shown in brackets.

Table 11. (*Continued*)

	Using Before Lending			Using Before and At Lending		
	Dep= Score	Dep= *OptimisticREC*	Dep= *OptimisticLTG*	Dep= Score	Dep= *OptimisticREC*	Dep= *OptimisticLTG*
	I: OLS	II. Logistic	III. Logistic	IV. OLS	V. Logistic	VI. Logistic
Intercept	49.09	0.27	-0.55	48.99	0.04	-0.42
	[30.57]	[3.04]	[7.37]	[64.93]	[0.25]	[22.26]
Before Lending	0.11	0.35	0.16	0.83	0.31	0.16
	[0.12]	[28.57]	[4.82]	[1.01]	[25.08]	[5.27]
Lending				1.59	0.27	0.14
				[4.17]	[80.38]	[16.46]
All star	1.34	0.11	-0.09	1.35	0.00	-0.09
	[2.03]	[3.59]	[2.11]	[4.41]	[0.00]	[10.43]
Brokerage size	0.96	-0.26	-0.07	0.59	-0.24	-0.11
	[4.00]	[133.26]	[5.13]	[5.18]	[542.03]	[74.72]
Forecast age	-4.56	0.06	0.07	-3.94	0.07	0.10
	[-27.32]	[12.61]	[18.19]	[-49.75]	[105.15]	[179.38]
Scope of coverage	-3.24	-0.03	-0.07	-3.69	0.03	0.06
	[-3.75]	[0.13]	[0.58]	[-9.33]	[1.00]	[2.14]
Analyst experience	-0.14	0.10	0.07	0.21	0.10	-0.08
	[-0.16]	[1.77]	[0.53]	[0.48]	[7.71]	[3.53]
Coverage length	1.10	0.03	-0.10	1.22	0.05	0.07
	[1.28]	[0.12]	[1.29]	[3.00]	[2.48]	[2.89]
Bold	2.99	-0.09	0.06	2.77	-0.01	0.00
	[5.50]	[4.10]	[1.01]	[10.96]	[0.47]	[0.00]
# obs	14,434	11,793	7,256	68,443	56,080	36,401
R^2	5.73%	2.11%	0.63%	4.19%	2.00%	1.17%