Three essays on information intermediaries in the financial market

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2012

Dong, Y. (2012). Three essays on information intermediaries in the financial market.
Doctoral thesis, Nanyang Technological University, Singapore.

<https://hdl.handle.net/10356/50575>

<https://doi.org/10.32657/10356/50575>

Three Essays on Information Intermediaries in the Financial Market

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Nanyang Business School

A thesis submitted to the Nanyang Technological University
in fulfillment of the requirement for the degree of
Doctor of Philosophy

2012

ACKNOWLEDGEMENT

I would like to gratefully and sincerely thank Professor Charlie Charoenwong for his guidance, understanding, patience, and most importantly, his friendship during my PhD studies at Nanyang Business School. His mentorship was paramount in providing a well rounded experience consistent with my long-term career goals. He encouraged me to become not only an independent researcher but also a helpful instructor to undergraduate students.

I am deeply indebted to my committee members, Professor Gao Huasheng, and Professor Chang Xin. Professor Gao Huasheng provides me with insightful guidance since my dissertation are at the very early stage, and his enthusiasm always motivates me to move on the research road. I would also like to express my deepest gratitude to Professor Chang Xin who agreed to sit on my committee. He is quite famous in our department for wisdom, knowledge and kindness, and I am very lucky to have his guidance on my dissertation improvement.

My sincere thanks are given to Professor Liu Wei-Lin, Professor Hwang Chuan-Yang, Professor Jun-Koo Kang, Professor Lau Sie Ting, Professor Angie Low, Professor Andy Kim and Professor Luo Jiang for their kind assistances and insightful advices. I also thank other faculty members from the Division of Banking and Finance who have ever taught or encouraged me.

I would like to thank my doctoral fellow students, Mr. Cai Yu, Ms. Deng Xin, Ms. Yin Huifang, Ms. Zhou Bo, Ms. Wang Ying, Mr. Zhang Le and Mr. Zhang Wenrui, who are the sources of friendship as well as good advices and collaborations. I also thank Ms Bee Hua Quek, Ms Amarnisha Mohd and Ms Karen Barlaan, for their continuous assistance in administrative tasks throughout my doctoral study.

Finally, I would like to thank my husband Neeck. His support, encouragement, quiet patience and unwavering love were undeniably the bedrock upon which the past four years of my life have been built. And I would like to thank my little daughter Nicole. She is the most beautiful thing I have ever seen in this world, and her coming provides me unbelievable happiness and encourages me to become a strong and responsible person.

SUMMARY

This dissertation includes three essays. The first essay investigates the relation between a firm's visibility in blog spaces, termed blog exposure, and the cross-sectional stock returns. I show that blog exposure is fundamentally different from the traditional media coverage, and securities with low blog exposure earn higher returns than stocks with high blog exposure. Contrary to traditional media coverage, the return premium associated with blog exposure cannot be explained by either the illiquidity hypothesis or the investor recognition hypothesis based on rational agent framework. Instead, the results suggest that blog effect can be attributed to the limited attention theory and cannot be arbitrated due to investors' self attribution and short-sales constraints. I document the blog based abnormal return is due to mis-pricing which will reverse later, not compensation for risk factor. This work points out the importance of blogs in information dissemination, especially for the stocks with limited attention.

Prior studies have demonstrated that forecast accuracy is negatively associated with the number of firms followed by an analyst. In the second essay, I reexamine the relationship between forecast accuracy and the number of firms followed by an analyst after Regulation Fair Disclosure. I hypothesize and find that the negative association between forecast accuracy and the number of firms followed by an analyst before Regulation Fair Disclosure has been changed to a positive relationship after Regulation Fair Disclosure. Specifically, the more firms followed by an analyst, the more accurate his/her forecasts. I document the reason of the change pattern is that information level has been changed. Before Reg FD, analysts require great effort and time to assess to private information. However, after Reg FD, analysts are facing the same level of

information and capable analysts tend to cover more firms to reach economies of scale. The market reaction tests show that investors do not fully understand the above relationship and discount forecast revisions issued by analysts follow more firms after Regulation Fair Disclosure. The results suggest that market participants need to update their belief by giving more weight to such forecasts.

The third essay examines the impact of one important corporate governance characteristic, interlocks, on analyst forecast accuracy and market reaction to analyst forecast revisions. I document a negative association between interlocking relationships among the firms followed by analysts and those analysts' forecast accuracy after controlling for other previously documented determinants of forecast accuracy. That negative association has significantly weakened after the passage of the Sarbanes-Oxley Act of 2002 (SOX). Furthermore, I document that the market does understand the negative impact of interlocks on analyst forecast accuracy before the passage of SOX. Controlling for other factors, the stock market responds less to forecast revisions issued by those analysts who follow a higher number of interlocked firms. However, the market reacts more to forecast revisions issued by analysts following a higher number of interlock firms after the passage of SOX, even though interlocks have no impact on forecast accuracy during that period. This shows that the market over-reacts to forecast revisions issued by analysts following a relatively large number of firms after the passage of SOX.

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Not all that Glitters is Gold: the Effect of Attention and Blogs on Investors' Investing

Behaviors

Abstract

The paper investigates the relation between a firm's visibility in blog spaces, termed blog exposure, and the cross-sectional stock returns. I show that blog exposure is fundamentally different from the traditional media coverage, and securities with low blog exposure earn higher returns than stocks with high blog exposure. Contrary to traditional media coverage, the return premium associated with blog exposure cannot be explained by either the illiquidity hypothesis or the investor recognition hypothesis based on rational agent framework. Instead, the results suggest that blog effect can be attributed to the limited attention theory and cannot be arbitrated due to investors' self attribution and short-sales constraints. I document the blog based abnormal return is due to mis-pricing which will reverse later, not compensation for risk factor. This research points out the importance of blogs in information dissemination, especially for the stocks with limited attention.

1. Introduction

In this paper, we investigate whether a firm's visibility within blog spaces, termed *blog exposure*, can affect security pricing even if the blog itself does not supply any authentic news. After controlling for environmental factors such as traditional media coverage and analyst coverage, and various risk factors such as size, book to market ratio, beta, and momentum, our results indicate that stocks with high blog exposure earn lower returns than stocks with low blog exposure. These results are more pronounced among stocks with lower institutional ownership. We also show that a portfolio composed of stocks with low institutional ownership produces sustained future abnormal returns. Such returns peak approximately 10 months after formation of the portfolio, and reverse sharply after that, displaying a long-term return reverse pattern. In addition, the abnormal returns of a portfolio composed of stocks with high institutional ownership meander around zero over the next 12 months after formation of the portfolio. We examine three plausible explanations for this return premium, two from the rational agent framework (liquidity and investor recognition) and one from the behavior finance framework (limited attention with short-sales constraints). Surprisingly, contrary to media coverage, for which the return premium can be explained by liquidity and investor recognition based on the rational agent framework (Merton [1987]), the return premium of blog exposure, cannot be explained by that framework. Instead, our findings show that a blog exposure premium

originates from the joint forces of biased disclosure of bloggers and limited attention of consumers. Such overvaluation becomes larger over time due to consumers' biased self-attribution, while the short-sales constraints prevent such a premium from being arbitrated. Our results are most consistent with the notion in Hirshleifer, Lim and Teoh (2009). In addition, our results show that the blog exposure effect is not subsumed by traditional media exposure, as documented by Fang and Peress (2009). However, without mainstream media (e.g. news) planting a seed of discussion, blog exposure actually has no impact on security returns.

Understanding the relation between blog exposure and stock returns is very important for both the marketing community and the finance community since it demonstrates how a firm's marketing strategy within blogspaces can influence elements of its finance strategy, such as cost of capital. As a form of word of mouth (WOM¹), blogs represent the fastest growing medium of personal publishing and the newest method of individual expression and opinion on the Internet². In 2004, blogs were a fairly new phenomenon with only 5 million bloggers worldwide (Wright [2006]). However, at the time of this writing, according to BlogPulse.com, there are more than

¹ Blog content is written by authors (also known as bloggers). It is a form of WOM. Typically there are websites comprising blog posts which are organized into categories and sorted in reverse chronological order. Most blogs allow readers to comment on individual blog posts. – Adopted from Wright (2006).

² www.blogpulse.com. BlogPulse is an automated trend discovery system for blogs. BlogPulse applies machine-learning and natural-language processing techniques to discover trends in the highly dynamic world of blogs.

126 million blogs on the World Wide Web. We believe blogs are playing a role that is as important as that of newspapers because 1) information in a blog is not a simple reflection of what is covered by traditional news. In fact, many blogs address topics that are not covered by the mainstream media at all. Blogs might either lead or follow traditional news; and 2) blogs disseminate information to a much broader audience faster and with in-depth analysis. In fact, compared to other online media, blogs are viewed as more credible (Johnson and Kaye [2004]). In addition, compared to traditional sources, more than three quarters of respondents view blogs as moderately to very credible (Johnson and Kaye [2004]).

This paper adds to the growing body of studies of the valuation of online word of mouth literature (Wysocki [1999]; Antweiler and Frank [2004]; Tumarkin and Whitelaw [2001]; Das and Chen [2001]). Our paper is also closely related to, but distinct from Fang and Peress (2009)'s media coverage study which shows that, by helping to reach a broad population of investors, mass media can alleviate information frictions and affect stock price even if it does not contain authentic news. However, Fang and Peress (2009) focus on studying the impact of traditional media coverage, measured by the number of newspaper articles about a firm on its stock returns. We concentrate on examining the influence of non-traditional media coverage—blog exposure—on security pricing, while at the same time, controlling for the traditional media coverage. Furthermore, our results are distinct from Fang and Peress (2009)'s conclusions. The

return premium of news coverage can be explained by the rational-agent framework (Fang and Peress [2009]). However, such a return premium of blog exposure can only be explained by the joint forces of investors' behavior and short-sale constraints.

Another branch of literature related to our study is the behavior finance literature which recognizes that attention influences investors' selling and purchasing behavior, and causes asset pricing deviation from its fundamental value. The underlying reason is that investors face a formidable search problem when buying a stock. Investors address such an issue by limiting their choice sets (Barber and Odean [2008]) to those stocks that have recently caught their attention (Odean 1999). Because investors are overconfident (Daniel, Hirshleifer, and Subrahmanyam [1998]; Odean [1998]) and biased toward self-attribution (Daniel, Hirshleifer, and Subrahmanyam [1998]), stocks over-bought heavily by individual investors will enjoy short-term positive contemporaneous returns. These results all emphasize the attention driven by traditional media, such as securities mentioned in the news or securities that have gone through large volume or price changes. Our study, however, focuses on the attention driven by non-traditional media, blogs.

One practical implication of our results is that a firm's visibility within blogspaces, regardless of whether the blog discussion is positive or negative, influences investors' purchase decision. As a non-traditional media, blog discussions serve the role of disseminating

information that was traditionally covered by conventional information channels such as analysts' forecasts or newspapers. Such a role is especially important for stocks with limited attention. In fact, as we documented, since blog discussions do not affect the expected stock returns when there is a lack of echo from the mainstream media, companies should plant the seed of a discussion to foster the conversation within blogspaces. Marketing managers of a firm can use blogs not only to communicate more efficiently with its customers, partners, suppliers, and other stakeholders, but also to work closely with finance managers to lower the cost of capital by delivering information to a broader audience in a faster manner.

The rest of this paper is organized as follows. In the next section we discuss our data collection processes, elaborate our variable definitions, and present our empirical results. In section 3, we discuss three possible causes of the blog exposure effect, and in section 4 we present our concluding remarks.

2. Data Collection and Empirical Results

2.1 Data Collection

In this paper, blog visibility/exposure is defined as the extent to which a company's products or services are discussed in blogspaces. We collect such a measurement from BlogPulse.com using its conversation track tool. Our data collection is composed of two steps. In step one, we identify the brand-names of products or services associated with a company by

searching a company's website, reading its financial reports, or utilizing researchers' domain knowledge. In step two, using the names identified in step one as keywords, for each company, we retrieve its blog visibility over time using the conversation tracker tool provided by BlogPulse.com. Of the total daily blogging activities traced by BlogPulse.com, this measurement represents the percentage of the total blog conversation related to a particular firm, its products, or its services. It measures how (and how much) a firm's current customers, potential customers, competitors, industry peers, etc. are talking about the products or services of a firm. Hence, it represents the visibility of a firm within the overall blogspaces. Figure 1 shows one example of the blog trend for Advance Auto Parts Inc. (AAP)³.

[Insert Figure 1 about here]

The data on a firm's media exposure was collected from Factiva. Factiva is a database of the Dow Jones and Reuters companies. It provides timely, domestic and international information, such as articles from the Dow Jones and Reuter's newswires and *The Wall Street*

³ We admit that there might be some noise in the blog exposure proxy, depending on the types and the number of blogs BlogPulse.com covered. However, given the large number of blogs BlogPulse.com covered, we believe the blog exposure as measured by BlogPulse.com is representative of the over-all discussion on blogspaces. Furthermore, each security's blog exposure as retrieved from BlogPulse.com might either under-estimate or over-estimate the true volume of blog conversation, depending on the keywords we specify as well the algorithm BlogPulse.com uses to identify the information related to a firm. However, we believe the results of such an estimate noise are more likely to be biased against our findings.

Journal. This information covers market data, firm and industry news, financial quotes, and newspaper articles. We collected and counted all of the non-redundant news items each day for the fiscal year of each company. Our analysts' forecast data is collected from the Institutional Brokers Estimates System (I/B/E/S). The company accounting data was obtained from CompuStat and the stock return data is from the Center for Research in Security Prices (CRSP). For detailed variable definitions, please refer to Appendix A.

Following Amihud (2002), we delete one firm with extreme illiquidity value. The final sample contains 404 S&P 500 firms with daily blog visibility and traditional media coverage information from March 1, 2006 to August 22, 2006. We give detailed variable definitions in Appendix A. Following Fang and Peress (2009), to minimize the noise of daily data, for each firm, we aggregate its daily blog visibility and Factiva news to a monthly level to represent its blog visibility and media coverage respectively. Table 1 shows the summary statistics of our key variables, including blog coverage and media coverage. As we can see, only 1 percent of the stocks in our sample do not have media coverage, but more than 25% of the stocks in our sample do not have blog coverage. It seems that for the S&P 500 firms, and for this sample period, traditional media has broader coverage in terms of the number of stocks discussed than blog conversations. However, this does not necessarily mean that, in general, traditional news media has broader coverage than blogs.

[Insert Table 1 about here]

Table 2 shows the pearson correlations among blog coverage, media coverage, and other firm characteristics. We find blog coverage and media coverage are positively correlated, and analysts, news, and blogs have the tendency to feature the same set of stocks. This is reasonable because analysts, news, and blogs pay attention to large firms and well-known firms. Furthermore, it seems that traditional media cares more about firms with a lower institutional ownership, while bloggers do not differentiate between firms with high institutional ownership and those with low institutional ownership.

[Insert Table 2 about here]

2.2. Blog Coverage and Short-term Cross Section of Stock Return

In this section, we study whether a firm's exposure within blogspaces affects its security pricing by regressing stock returns on blog coverage, with media coverage, beta, size, BM ratio, and other risk factors controlled (Table 3 Model 1). For a robustness check, we take an incremental approach by adding momentum, media coverage, percentage of institutional ownership, and illiquidity one by one and present the results in Model 2, Model 3, and Model 4 of Table 3 respectively. To control for the potential confounding effect caused by the difference across months and industries, we also include both month fixed effect and industry fixed effect (2-digital SIC from CRSP) in all our models.

For Model 1, the coefficient of blog exposure is -0.0026 with t value at -1.93, which is negatively associated with the following month's stock return. Therefore, stocks with high blog exposure tend to have lower stock returns compared to the stocks with low blog coverage. Such an impact is not subsumed when we add the traditional media exposure and other risk factors (Model 3 and Model 4). Furthermore, using Factiva as proxy for media coverage, we found that media coverage has an insignificant coefficient regardless of whether we exclude the blog exposure. The untabulated table shows that our results are qualitatively the same if we exclude those firm-month observations in which there are earning announcements for the firm in that month. Therefore, we conclude that the blog effect is not driven by month effect, industry effect, or earnings announcements.

Our media coverage results are different from those of Fang and Peress (2009). However, in their paper, they focus on four influential newspapers with large subscriptions. Our media coverage includes all the newspapers included in the Factiva database. Another reason for the different results might lie in the sample selection. In their paper, they argue that the most significance of high media coverage, low return effect comes from the small and illiquid firms. However, in our paper, we use only S&P 500 firms, which are all big firms.

[Insert Table 3 around here]

2.3. Blog Coverage and Long-term Cross Section of Stock Returns

In the previous section, we documented the short-term return premium associated with low blog exposure firms. In this section, we study the long-term return impact of blog exposure. Figure 2 represents the cumulative abnormal returns of stocks with different blog coverage, starting from month 1 after the portfolio formation. Each month we sort our sample into three groups according to their monthly blog exposure. Then, based on CAPM model, for each stock we estimate its abnormal return in the subsequent 13 months. Low blog curve (Figure 2) represents the average abnormal returns for the lowest blog coverage group, while high blog curve represents those of the highest blog coverage group. Figure 2 shows that the highest blog coverage portfolio consistently has insignificant abnormal returns starting from the first month after the formation of the portfolio to the next 13 months, and the low blog coverage portfolio enjoys a positive CAR in the following 13 months. Additionally, there is a return reversal when the cumulative abnormal returns peak at 10th month.

Our results are not driven by the return reversals among no-coverage stocks because the abnormal returns associated with low blog visibility firms do not reverse until 10 months after the portfolio formation, while a typical return reversal pattern among no-news losers is short-lived (Chan [2003]). Given that our blog coverage effect represents a long-term effect, there must be a force to prevent traders from such arbitrage. So what are the driving forces?

[Insert Figure 2 about here]

3. Explaining the Blog Visibility Effect

In this section, we examine three potential explanations of the return premium associated with blog exposure: illiquidity, investors' recognition, and bloggers' limited attention and short-sales constraints. The first two are based on the rational-agent framework, while the third explanation originates from behavior finance literature.

3.1. Illiquidity Hypothesis

Fang and Peress (2009) document that stocks with no media coverage enjoy higher returns than stocks with high media coverage. Furthermore, such a return premium is very stable over time. They believe that the lack of liquidity explains why such an abnormal return cannot be arbitrated. Hence, we first test whether blog visibility is similar to media coverage aroused due to illiquidity. According to rational agent framework, if the blog-effect represents an arbitrage opportunity, it can only be persistent in the situation where some kind of impediment to trade prevents arbitrage. Hence, if the blog visibility effect is also caused by illiquidity, we expect the blog effect to be most significant in the portfolios that are composed of the most illiquid stocks.

We employ multivariate regressions (either separate regressions for each portfolio or pooled together) to study whether the illiquidity can explain the cross-sectional return differences

we document. Each month we sort our sample into three portfolios based on various illiquidity proxies proposed by previous literature including the Amihud (2002) illiquidity ratio, bid/ask spread, trading price, and firm size. Stocks with the highest illiquidity ratio or spread, or stocks with the lowest price or size, are the most illiquid ones. For each illiquidity proxy and each portfolio, the following month's stock returns are regressed on blog coverage with other controlled factors that are known to affect the cross section of returns, such as beta, size, BM ratio, and momentum. To control for the potential heterogeneity across months as well as industries, we run our regression by controlling month and industry effects.

Table 4 reports the blog effect of stocks sorted by different illiquidity proxies. Due to space constraint, for each portfolio under different illiquidity measures, we report only the coefficient before blog coverage and the corresponding t statistic. If the blog visibility effect is caused by illiquidity, we expect the blog exposure effect to be most significant in the portfolios composed of the most illiquid stocks, such as stocks with the highest illiquidity and spread, or those with the lowest price or size. However, the results shown in Table 4 fail to support the illiquidity hypothesis. For example, with respect to the Amihud illiquidity ratio, blog effect is significant in both the low illiquidity portfolio (Para = -0.0068 and t -value = -3.04) and the medium illiquidity portfolio (Para = -0.0058 and t -value = -2.9), but not in the high illiquidity portfolio (Para = 0.0022 and t -value = 0.67). This contradicts the illiquidity hypothesis that high

impediments of trade should result in the most significant blog exposure effect. Similar examples of evidence are found when we use alternative illiquidity measurements. Using price or firm size to proxy for liquidity, we observe that the blog exposure associated return premium is most significant in the portfolios composed of the most liquid stocks, for example, stocks with highest trading prices (Para = -0.0065 and t-value = -1.68) or stocks with the biggest firm sizes (Para = -0.0072 and t-value = -3.57). In addition, we also employ bid/ask spread to proxy for illiquidity because highly illiquid stocks often have a large bid/ask spread. The coefficients of blog visibility for both low and medium bid/ask spread portfolios are negative and significant, but not for the high bid ask spread portfolio (Para = -0.0015 and t-value = -0.55) – the most illiquid stocks. Overall, our results show that the blog visibility effect disappears among the most illiquid stocks, hence are contrary to the illiquidity theory that suggests that the blog visibility effect should be the strongest for most illiquid securities.

[Insert Table 4 about here]

To check the robustness of the above results, for each illiquidity proxy, we run a pooled regression using all three portfolios. To be more specific, for each illiquidity proxy, we first define one dummy variable to distinguish high illiquidity firms from low illiquidity firms. For example, the dummy variable *Rank* for Amihud illiquidity is set to one if the illiquidity ratio of that firm is higher than the median Amihud illiquidity ratio, and zero otherwise. Then, in

addition to the original independent variables, we also include an interaction term between the blog coverage variable and the illiquidity dummy variable *Rank* to test whether the blog exposure effect is more significant for stocks with higher illiquidity. Our untabulated results show that consistent with the conclusions from running separate regressions, results using pooled regressions also demonstrate that illiquidity cannot explain the blog exposure effect that we have documented.

3.2. Investors' Recognition Hypothesis

Fang and Peress (2009) posit that, besides illiquidity, the investor recognition theory proposed by Merton (1987) can also explain the media coverage effect they observe. Under this theory, investors are assumed to have incomplete information and are aware of only a subset of the available stocks. For stocks with less investor recognition, investors with incomplete information will require higher returns as a compensation for the undiversified risk and market clearing. Therefore, we hypothesize that if blog coverage can increase a stock's recognition, then the blog effect should be more prominent for stocks with a low degree of investor recognition. Several measures, including analyst coverage, idiosyncratic risk, advertising expenditure, and the number of investors of a stock are adopted as proxies for investors' recognition. Analyst coverage is selected because investors listen to analysts to make investment decisions, and stocks with high analyst coverage are assumed to have a high degree of investor

recognition. Idiosyncratic risk is adopted as another investor recognition measure because it represents the imperfect diversification driven by a lack of investors' recognition (Ang et al 2006). Furthermore, following previous literature (Singh, Faircloth, and Nejadmalayeri [2005]; Grullon, Kanatas, and Weston [2004]), we develop two additional investor recognition measures: advertising expenditure and the number of common shareholders of a security.

Following the methods in the illiquidity hypothesis test section, we examine whether the blog effect can be explained by investors' recognition using a separate regression approach and a pooled regression approach. For the separate regression methods, again we sort our sample into three portfolios in each month according to different investor recognition measures. Then, for each investor recognition proxy and each portfolio, following months' returns are regressed on blog visibility while controlling for other factors that are known to affect the cross section of returns such as beta, size, BM ratio, and momentum (Table 5). Similarly as in section 3.1, we also ran pooled regressions to study the impact of investor recognition measures on stock returns and reach very similar conclusions as when we followed a separate regression approach.

Table 5 presents the results of separate regressions for portfolios with different degrees of investor recognition. Following Table 4, for each investor recognition proxy, we report only the coefficient and t value of blog coverage in each portfolio. As we can see, when investor recognition is measured by idiosyncratic risk, the coefficient of blog visibility is significant only

for the low and medium idiosyncratic risk portfolios, but not for the high idiosyncratic risk portfolio. Besides, the absolute value of the coefficient of the low idiosyncratic risk portfolio is bigger than that of the medium idiosyncratic risk portfolio. This indicates that the blog effect is more significant in portfolios with high investor recognition, failing to support the hypothesis that the blog exposure effect can be explained by investors' recognition. We reach a similar conclusion when we use analyst coverage, advertisement expenditure, and number of investors as proxies for investor recognition. The blog effect is much more prominent for stocks with higher analyst coverage, medium advertisement expenditure, and a medium investor base. None of these evidences support the investor recognition hypothesis.

[Insert Table 5 about here]

3.3. Limited Attention Hypothesis and Short-Sales Constraints

Based on the rational-agent framework, both the illiquidity hypothesis and the investors' recognition hypothesis are able to explain how news media coverage drives the cross-section of stock returns (Fang and Peress [2009]). However, such a rational-agent framework cannot justify the blog exposure effect we have documented in section 2. Given that the blog exposure effect persists over time, there must be other forces driving this return premium and preventing it from being arbitrated.

In this section, we propose that the limited attention theory (Hirshleifer, Lim, and Teoh [2009]) offers an explanation for the cross-sectional return differences we document, while the short-sales constraints sustain such return premium overtime. From a disclosure prospective, blogs serve as a positively biased disclosure channel. We tried analyzing a sample of blog content from the LexisNexis database, and find more than 91% of the blog talks are generally in good tone. Investors with limited attention will selectively interpret the biased disclosure by assuming that no news is good news. This will make investors net-buyers of low-blog-exposure stocks. The biased interpretation behavior of investors on blog disclosure is consistent with the framework proposed by Hirshleifer, Lim and Teoh (2009) about how investors interpret the disclosures of those firms with limited attention. To make matters worse, investors with limited attention are overconfident (Daniel, Hirshleifer, and Subrahmanyam [1998]) and biased toward self-attribution (Daniel, Hirshleifer, and Subrahmanyam [1998]). In other words, individual investors believe they are better in assessing blog information (i.e., overconfident) and they selectively trust the messages in WOM and selectively validate their beliefs (i.e., biased toward self-attribution). The overall net buying behavior will be sustained over an even longer period of time. And the rational story cannot explain the long term effect of blog exposure on cross section of stock returns. The long term blog effect documented above support the behavior story that the blog based abnormal return is due to investors' mispricing.

3.3.1 Are Investors Net-buyers of Low Blog Exposure Securities?

To prove that consumers are more likely to be net buyers of securities with low blog exposure, we estimate the percentages of buy transactions out of the daily total number of transactions for a high blog exposure portfolio and a low blog exposure portfolio respectively (Figure 3). We follow the buy and sell classification algorithm proposed by Lee and Ready (1991), which is commonly used in early literature, such as Easley, Kiefer, O'Hara and Paperman (1996, 2002). Following Lee and Ready (1991), a transaction is defined as a buy (sell) if it is executed above (below) the midpoint of the bid and ask price. For trades on the bid/ask midpoints, we use a 'tick test' to determine whether it is a buy or sell. To be more specific, a trade is a buy (sell) if it is executed at a higher (lower) price than the previous trade. For those trades that have the same price as the previous trade, we look at the historical price until we find a change in the trade price. Following Lee and Ready (1991), for the computation, we match our trade prices with 5-second-old quotes. After classifying the buy and sell for each trade, we cumulate the number of buys and sells in a day to get the aggregated daily number of buy and sell. Then the daily percentage buy is defined as the total number of buy transactions divided by the total number of all transactions on that day.

Figure 3 presents the percentage of buy transactions for firms with low daily blog coverage for the 30 days subsequent to portfolio formation. We sort our sample into 2 portfolios

based on their daily blog coverage. Low (high) daily blog coverage firms include those whose daily coverage is lower (higher) than the median blog coverage on that day. We also conduct a similar analysis by sorting our sample into 4 or 10 portfolios, and reach a similar conclusion. Our results show that, regardless of the magnitude of blog exposure, on average, there is an increase in the percentage of buy transactions after the portfolio formation, which might be driven by the overall market situation. Furthermore, Figure 2 shows that, compared to the high blog exposure portfolio, the low blog exposure portfolio has a higher percentage of buy transactions⁴. The untabulated mean difference comparison also validates this conclusion (Difference = 0.0022 and t-value = 3.99). In addition, this difference remains relatively stable over time after portfolio formation. In addition, the untabulated results show that most of the increase in the percentage of buy transactions concentrates on small investors.

[Insert Figure 3 about here]

3.3.2. Short-Sales Constraints

In our previous section, we documented that since investors with limited attention become net-buyers of low blog exposure securities, stock prices go up. How about institutional

⁴ We should be aware that there is another mechanism that might result in the return difference between the high blog exposure stocks and the low blog exposure stocks. Investors are net sellers of the securities with high blog exposure. However, Figure 2 results rule out such a possibility. If that were true, the portfolio composed of high blog exposure stocks should have big and prolonged negative returns, but that is not the case. The portfolio return of high exposure stocks is, in fact, meandering around zero.

investors or other rational investors? Why do they not come in and fix this “irrational” behavior?

We believe that the short-sales constraints might be one of the answers. Short-sales constraints can prevent the arbitrage from happening, hence, making the return pattern we observe last longer⁵. We expect, if short-sales constraints can explain the blog exposure effect, the effect should be concentrated in a portfolio composed of stocks with the highest short-sales constraints. Following Asquith, Pathak and Ritter (2005), we use institutional ownership of a firm as a proxy for its short-sales constraints and a firm with high institutional ownership is treated as the one with low short-sales constraints.

[Insert Table 6 about here]

Table 6 shows that the blog effect is significant only in low institutional ownership groups (Para = -0.0067 and t-value = -2.58), representing a high sales constraints situation. Such evidence supports our hypothesis that the blog effect we documented can be explained by short-sales constraints.

⁵ Following Fang and Peress (2009), in our paper, we use investor recognition to refer to the model of Merton (1987) under the more traditional rational agent framework, while the limited attention refers to the behavior financial model (e.g., Barber and Odean [2008]). Even though one of the key assumptions of the investor recognition hypothesis (tested in section 3.2) is that investors know about only the subset of the available stocks, Merton (1987)’s model is built upon rational-agent framework and is different from the limited attention hypothesis we study here. In Merton’s model, attention grabbing by itself will not influence an investor’s purchase decision, while in our case, it does.

3.4 Relationship between blog exposure and media coverage

Our readers might question how securities with low blog exposure get attention. If nobody ever talks about a stock, how can investors be aware of it? Hence, in this section, we study where the attention comes from. We believe that blog exposure needs to be built upon news attention. Hence, we investigate whether our blog effect will be subsumed under the recently documented media effect (Fang and Peress [2009]). We first sort our sample into three portfolios according to their monthly media coverage, proxied by Factiva. Then for each portfolio, we regress the following month's stock returns on blog coverage, size, beta, BM ratio and momentum. Table 7 shows that, with traditional media coverage controlled, the blog effect is significant only in high media coverage portfolios, and is insignificant in low and medium media coverage portfolios. Our interpretation is that blog conversations are not a simple reflection of the information content of traditional media, hence the blog effect cannot be fully subsumed by it. As a result, in order to really force the market to listen to blog conversations, mainstream media needs to plant seeds to spark the discussion; otherwise a simple blog conversation without a mainstream echo will have no market response. Furthermore, even with heavy traditional media coverage, blog conversations will not be buried and will still stimulate market responses.

[Insert Table 7 about here]

4. Conclusion

In this paper we investigate the relation between blog coverage and the cross-sectional stock returns. We show that blog coverage is different from the traditional media coverage documented by previous literature (Fang and Peress [2009]). We find that high blog coverage is associated with low stock returns, even when controlling for other risk factors and traditional media coverage. We further illustrate that such an effect is more prominent for stocks with low institutional ownership, and cannot be explained by either the illiquidity hypothesis or the investor recognition hypothesis, which have been shown in explaining the cross-sectional relation between media coverage and expected stock returns. Our interpretation is that the blog coverage effect is caused by the selective interpretation of investors with limited attention on the blog posting. The abnormal returns associated with the blog exposure effect are sustained over time and cannot be arbitrated within a short period of time due to short-sales constraints. All these things make blogs an important information dissemination channel.

However, we should carefully interpret our results since there may be some other mechanism that might cause the same phenomena we documented. For example, if blog contents are overall negative instead of positive, then no news is good news' might actually be a rational response in the context of blog coverage. If investors with limited attention fail to

understand such a relation, then investors with limited attention are likely to be pessimistic about firms with low blog coverage compared to rational investors, which will lead to undervaluation in the current period. If stocks with low blog coverage are undervalued in the current period, subsequently they outperform stocks with high blog exposure. Even though for our dataset, our untabulated sentiment mining results rule out such an explanation⁶, future research might look deep into this issue with new dataset covering both big, medium, and small firms and across a longer period of time to draw more insights.

Our study offers great insights as to the importance of the marketing activities of a firm on its expected return. Our results justify the buzz value creation of marketing strategy on firm's valuation. Our study also has policy implications for government agencies, such as the Securities and Exchange Commission (SEC) because it brings to the forefront the effect and importance of blog information in the market valuation of firms. This is especially important for those firms with many small and naïve investors, who have limited channels to access and limited capabilities to process / digest value-relevant information.

⁶ Our unreported sentiment mining results of the blog contents collected from LexisNexis database show overall blog contents are dominated by positive sentiments.

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TABLE 1: Summary Statistics of Blog Exposure and Media (Factiva) Coverage

Blog		Factiva	
Mean	0.40	Mean	236.42
Standard Deviation	1.22	Standard Deviation	415.45
Skewness	5.66	Skewness	3.50
<u>Quintile</u>		<u>Quintile</u>	
100% Max	11.89	100% Max	2791.00
99%	7.38	99%	2267.00
95%	1.90	95%	1085.00
90%	0.80	90%	535.00
75% Q3	0.22	75% Q3	224.00
50% Median	0.04	50% Median	95.00
25% Q1	0.00	25% Q1	41.00
10%	0.00	10%	19.00
5%	0.00	5%	7.00
1%	0.00	1%	0.00
0% Min	0.00	0% Min	0.00

* See Appendix A for detailed variable definitions.

TABLE 2: Pearson Correlation

	blog	Factiva	size	BM	momentum	dispersion	IO	coverage	illiquidity	idiorisk
blog	1									
	2353									
Factiva	0.1550 <.0001	1								
	2353									
size	0.1334 <.0001	0.5198 <.0001	1							
	2346	2346	2346							
BM	-0.1214 <.0001	-0.0383 0.0685	-0.2583 <.0001	1						
	2261	2261	2259	2261						
momentum	-0.0740 0.0003	-0.0242 0.24	0.0886 <.0001	-0.1616 <.0001	1					
	2352	2352	2345	2261	2352					
dispersion	0.0125 0.5513	0.0050 0.8131	0.0076 0.7169	-0.0384 0.0722	0.0218 0.3008	1				
	2266	2266	2263	2191	2266	2266				
IO	0.0088 0.6695	-0.1053 <.0001	0.0614 0.0029	-0.0905 <.0001	0.2115 <.0001	-0.0500 0.0173	1			
	2353	2353	2346	2261	2352	2266	2353			
coverage	0.1293 <.0001	0.3358 <.0001	0.6064 <.0001	-0.1358 <.0001	0.0195 0.3528	0.0012 0.9549	0.0353 0.0916	1		
	2281	2281	2278	2201	2281	2266	2281	2281		
illiquidity	-0.0212 0.3043	-0.0369 0.0732	-0.2677 <.0001	0.0348 0.0983	-0.0602 0.0035	-0.0399 0.0578	-0.1454 <.0001	-0.1501 <.0001	1	
	2353	2353	2346	2261	2352	2266	2353	2281	2353	
idiorisk	-0.0038 0.8558	-0.0725 0.0004	-0.3569 <.0001	-0.0612 0.0036	0.1148 <.0001	0.0035 0.8681	0.0578 0.005	-0.0635 0.0024	0.2501 <.0001	1

For detailed variable definitions, refer to Appendix A.

TABLE 3: Blog Coverage, Media Coverage, and Stock Returns

In this table, the dependent variable is the following month's stock return. The independent variables include traditional risk factors and other firm characteristics that may affect expected stock returns. Our samples include 404 distinct firms. We run pooled regression, controlling month fixed effect and industry fixed effect (2-digital SIC).

$$\text{Nextmonthre} = \alpha + \beta_{\text{beta}} * \text{beta} + \beta_{\text{size}} * \text{size} + \beta_{\text{BM}} * \text{BM} + \beta_{\text{momentum}} * \text{momentum} + \beta_{\text{IO}} * \text{IO} + \beta_{\text{coverage}} * \text{coverage} + \beta_{\text{illiquidity}} * \text{illiquidity} + \beta_{\text{blog}} * \text{blog} + \beta_{\text{Factiva}} * \text{Factiva}$$

	MODEL1	MODEL2	MODEL3	MODEL4
Intercept	0.0116 (-0.45)	0.0295 (-1.03)	-0.0043 (-0.15)	0.0041 (-0.12)
blog	-0.0026 (-1.93)		-0.0033 (-2.45)	-0.0033 (-2.39)
Factiva		0.0000 (-0.87)	0.0000 (-0.65)	0.0000 (-0.65)
beta	-0.0026 (-1.1)	-0.0028 (-1.78)	0.0003 (-0.13)	0.0002 (-0.06)
size	0.0024 (1.72)	0.0012 (0.70)	0.0021 (1.23)	0.0019 (0.87)
BM	0.0042 (1.78)	0.0040 (1.72)	0.0012 (0.51)	0.0013 (0.56)
momentum			-0.0323 (-5.79)	-0.0311 (-5.36)
coverage				-0.0001 (-0.25)
IO				-0.0034 (-0.68)
illiquidity				-0.0203 (-0.95)
Adjusted_R ²	0.1119	0.1107	0.1253	0.1246

TABLE 4: Illiquidity and the Blog Effect

In this table, each month we classified our sample into three portfolios according to their respective illiquidity proxies. We use the Amihud illiquidity ratio (Amihud 2002), bid ask spread, size of the firm, and price to proxy for illiquidity. The following months' stock returns are regressed on blog coverage, beta, size, BM ratio, and momentum, controlling month fixed effect and industry fixed effect. For brevity, for each portfolio under different illiquidity measures, we report only the coefficient before *blog coverage* and corresponding *t* statistic.

	Low		Medium		High	
	Parameter	<i>t</i> Value	Parameter	<i>t</i> Value	Parameter	<i>t</i> Value
Illiquidity	-0.0068	-3.04	-0.0058	-2.39	0.0022	0.67
Spread	-0.0042	-1.88	-0.0048	-1.90	-0.0015	-0.55
Price	-0.0040	-1.46	-0.0079	-3.20	-0.0065	-1.68
Firm Size	0.0004	0.14	-0.0015	-0.40	-0.0072	-3.57

TABLE 5: Investor Recognition and the Blog Effect

In this table, each month we sort our sample into three portfolios according to different investor recognition proxies including analyst coverage, idiosyncratic risk, advertising expenditure, and the number of investors of a stock. The following months' stock returns are regressed on blog visibility, beta, size, BM ratio, and momentum, as well as month fixed effect and industry fixed effect. For brevity, for each portfolio under different illiquidity measures, we report only the coefficient before *blog coverage* and corresponding *t* statistic.

	Low		Medium		High	
	Parameter	<i>t</i> Value	Parameter	<i>t</i> Value	Parameter	<i>t</i> Value
Idio risk	-0.0088	-3.60	-0.0069	-3.18	0.0004	0.16
coverage	-0.0012	-0.42	-0.0037	-1.31	-0.0077	-3.37
Expenditure	0.0035	0.23	-0.0068	-2.33	-0.0012	-0.39
Investor base	-0.0040	-1.47	-0.0061	-2.50	-0.0016	-0.53

TABLE 6: Short-sales Constraints and the Blog Effect

In this table, in each month we sort our sample into three portfolios according to their short-sales constraints proxy, the institutional ownership. Then the following month's stock returns are regressed on blog visibility, beta, size, BM ratio, and momentum, as well as month fixed effect and industry fixed effect. For brevity, for each portfolio, we report only the coefficient and t value of blog coverage.

Variable	Low IO		Medium IO		High IO	
	Parameter	t Value	Parameter	t Value	Parameter	t Value
blog	-0.0067	-2.58	-0.0036	-1.40	-0.0020	-0.82

TABLE 7: Relationship between Factiva and Blog

We sort our sample into three portfolios in each month according to their media coverage, proxy by Factiva. Then the following month's stock returns are regressed on blog visibility, beta, size, BM ratio, and momentum, as well as month fixed effect and industry fixed effect. For brevity, for each portfolio, we report only the coefficient and t value of blog coverage.

Factiva	Low		Medium		High	
	Parameter	t Value	Parameter	t Value	Parameter	t Value
Blog	0.0001	0.03	0.00	-0.9700	-0.00426	-2.1800

FIGURE 1: Blog Coverage Data Collection

In this figure, we use Advance Auto Parts Inc as an example to show how we collect the exposure of a firm within blogspaces using www.blogpulse.com

FIGURE 2: Cumulative Abnormal Return

Figure 2 presents the cumulative abnormal return of a high blog coverage sample and a low blog coverage sample starting from month 1 after the portfolio formation. Each month we sort our sample into two groups according to their monthly blog coverage. Then, based on CAPM model, for each stock we estimate its cumulative abnormal return in the subsequent 13 months. The blue line represents the average CAR for the low blog coverage group, while the red line plots that of the high blog coverage group.

FIGURE 3: Percentage of Buy over Time

This figure presents the percentage of buy transaction for firms with the low daily blog coverage during the subsequent 30 days after portfolio formation. We sort our sample into 2 portfolios based on their daily blog coverage, and low (high) daily blog coverage firms include those whose daily coverage is lower (higher) than the median blog coverage on that day. We also conduct a similar analysis by sorting our sample into 4 or 10 portfolios and reach a similar conclusion.

APPENDIX A: Variable Definitions

Variable	Definitions
Coverage	Analyst coverage is from the summary statistics database of I/B/E/S. It is based on the variable Numest (Number of Estimates), which represents the total number of estimators covering the company for the fiscal period (annual forecast only).
Beta	Used to control market risk, run CAPM regression using previous 60 month data.
Blog	Monthly blog coverage, defined as sum of daily blog coverage.
BM	log of (Book value/ market value).
Dispersion	Analyst forecast dispersion, from the summary statistics database of I/B/E/S database. It is based on stdev/medest, where stdev represents the standard deviation of the forecast, and medest represents the median estimation of forecast for the fiscal period (annual forecast only).
Expenditure	Advertising Expense from COMPUSTAT Merged Fundamental Annual File. It is based on the variable XAD.
Factiva	Monthly media coverage, defined as sum of daily newspaper news, from Factiva.
Idiorisk	Idiosyncratic risk, follow Ang et al (2006).
Illiquidity	Follow Amihud (2002) method.
Investor base	Number of Common/Ordinary Shareholders of a stock. It is based on CSHR collected from Compustat.
IO	Institutional ownership, from Thomson Reuters. IO is defined as all the shares held by institutional investors divided by shares outstanding. It is quarterly basis. Each month, we use the IO of previous quarter as the IO for this month. We also match monthly IO with the nearest quarterly end IO. Qualitatively both measures results in similar results.
Momentum	Previous 12 month cumulative return.
Size	Log of Market value, defined as $PRC \times SHROUT$ based on CRSP monthly file, where PRC is the closing price, and SHROUT is the common shares outstanding.
Spread	Monthly bid ask spread (ask high minus bid low).
Volume	Monthly trading volume based on CRSP monthly file, it is the sum of the trading volumes during that month.

Analyst Firm Coverage and Forecast Accuracy: the Effect of Regulation Fair Disclosure

Abstract

Prior studies have demonstrated that forecast accuracy is negatively associated with the number of firms followed by an analyst as more firms increase an analyst's task complexity. This paper reexamines the relationship between forecast accuracy and the number of firms followed by an analyst (firm coverage) after Regulation Fair Disclosure (Reg FD). I hypothesize and find that the negative association between analyst forecast accuracy and firm coverage before Reg FD has been changed to a positive relationship after Reg FD. Specifically, the more firms followed by an analyst, the more accurate his/her forecasts after Reg FD. My argument is that, before Reg FD, analysts with private information also need to spend extra time and effort to acquire and process it, leading to a much smaller firm coverage. Hence, the private information benefits those analysts in forecast accuracy, but at the expense of time and resources. However, after Reg FD, analysts could access to the same set of information, and more capable analysts tend to cover more firms to reach economics of scale. The market reaction tests show that investors do not fully understand the above relationship and discount forecast revisions issued by analysts following more firms even after Reg FD. The results suggest that market participants need to update their belief by giving more weight to forecasts issued by analysts with larger firm coverage after Reg FD.

Analyst Firm Coverage and Forecast Accuracy: the Effect of Regulation Fair Disclosure

1. Introduction

Whether systematic differences in financial analysts' forecast accuracy exist has been an on-going research question. Though earlier studies failed to document such systematic differences (Richards 1976; Brown and Rozeff 1980; O'Brien 1990; Coggin and Hunter 1989; and Butler and Lang 1991), starting in the early 1990s, researchers began to investigate the heterogeneity of analysts' forecast accuracy (e.g., Stickel 1992; Sinha, Brown and Das 1997; Clement 1999; Mikhail, Walther and Willis 1997; and Jacob, Lys and Neale 1999) using alternative methodologies, different time frames, and independent data.

Forecast accuracy has been shown to be influenced by various cross-sectional factors. For example, Clement (1999) shows that forecast accuracy is positively associated with the analyst's ability (e.g., analyst's firm experience) and the resources available to the analyst (e.g., employer size), while it is negatively associated with task complexity (e.g., number of firms and industries followed by the analyst). This paper extends the study of Clement (1999) by focusing on one influential factor, the number of firms followed by the analyst, and reexamines its impact on forecast accuracy after Regulation Fair Disclosure (hereafter, Reg FD).

We start by replicating Clement (1999)'s findings for the period of 1985 to 1995 (sample period in Clement 1999) and further document the similar findings for the extended period of 1985 to 2000 (sample period prior to Reg FD). We hypothesize that, as analyst firm coverage is a choice variable *per se*, it is not a proxy for the task complexity, and the negative association between forecast accuracy and the number of firms followed by an analyst will change to be positive after Reg FD.

We argue that, before Reg FD, selected analysts have private information; however, they have also spent extra time and resources to acquire and analyze the information. Hence, though achieving more accuracy, analysts with private information are not able to follow as many firms as their peers. Reg FD stipulates that, after October 23, 2000, firms should provide material information to all investors on a fair basis. The intent of Reg FD is to stop the practice of firms disclosing material information to a few selected investors and analysts through private channels, and therefore, analysts lose their private channels of information and have to conduct their own research. For example, Bailey et al. (2003) find that difference in opinions among analysts has increased after Reg FD. Hence, only more capable analysts are able to cover more firms, in addition, the enlarged portfolio may also help these analysts achieve economic of scale which will further help them increase the forecast accuracy.

After documenting the change in the association between analyst firm coverage and forecast accuracy, we further investigate whether investors fully understand such a change. This is another important question as financial analysts play the role of information intermediary in the capital markets, and analysts' forecasts weigh heavily when investors form their expectations. Intuitively, market participants should give more weight to the forecasts made by more accurate analysts. Prior work has investigated the interplay between cross-sectional variation in forecast revisions and factors associated with forecast accuracy (e.g., Clement and Tse 2003; Bonner, Hugon, and Walther 2007). Clement and Tse (2003) argue that understanding the determinants of analyst forecast accuracy is important to market participants and examine whether investors appear to extract all of the information that analyst characteristics provide about forecast accuracy. They find that only some of the analyst characteristics are associated with the return responses to forecast revisions. The number of firms followed by an analyst is shown not to be

one of the priced factors. We reexamine this question with a specific focus on analyst firm coverage, and find that the market participants discount the forecasts of analysts following more firms before Reg FD, and continue to do so after Reg FD. Given our finding of a positive association between analyst firm coverage and forecast accuracy after Reg FD, market participants seem not fully understand such an association.

One caveat of our study is that we document a change in the association between the number of firms followed by analysts and forecast accuracy before and after Reg FD. The finding itself is not direct evidence to conclude that Reg FD is the cause of the change. We have done several robustness checks to tease out some competing explanations. First, we show that change in the association between forecast accuracy and the number of firms followed by the analyst is not attributable to a mechanical decrease in the number of firms followed by the analysts after Reg FD. Second, we also replicated our results using a fixed analyst sub-sample before and after Reg FD to make sure that our results are not driven by the change in the composition of analysts. Qualitatively our results do not change.

Our study contributes to three strands of literature. First, we contribute to the literature on determinants of analyst forecast accuracy. We show that the previously documented negative relationship between forecast accuracy and number of firms followed by an analyst has been changed to be a positive association after Reg FD. In other words, after the passage of Reg FD, when analysts follow more firms, their accuracy on average is better. We document the number of firms followed does not stand for task complexity; it is a choice of analysts due to compensation and career concern.

Second, the evidence is useful to market participants. We show that market participants mistakenly discount the forecasts of those analysts who follow more firms after Reg FD. Our

results suggest that, instead of discounting, market participants should put more weight to forecasts issued by those analysts, since after Reg FD the more firms analysts follow, the more accurate forecasts they issue.

Third, this paper contributes to the literature of Reg FD. We demonstrate one effect of the regulation: it causes analysts to follow the firms for which they really have a comparative advantage when conducting analysis. The number of firms followed by analysts has decreased after Reg FD; however, the more firms followed by an analyst, the more accuracy he/she achieved. This evidence provides indirect support to the conclusion made by Mohanram and Sunder (2006) that financial analysts rely more on idiosyncratic information discovery after Reg FD.

The remainder of the paper is organized as follows: section 2 reviews the literature and develops our hypotheses; section 3 discusses the data, and our empirical models; section 4 presents our empirical analysis; and section 5 concludes the paper with managerial implications and potential future research.

2. Literature Review and Hypothesis Development

As one important information intermediary, financial analysts have received considerable attention in accounting and finance research. Determinants of analysts' forecast accuracy has been studied for a long time. In the 1970s and 1980s, the literature documented few determinants (i.e., there is no cross-sectional and time-series significant differences in forecast accuracy; see Richards 1976; Brown and Rozeff 1980; O'Brien 1987; Coggin and Hunter 1989; and Butler and Lang 1991). However, new evidence emerged from the 1990s suggests that there are a number of important determinants of analysts' forecast accuracy.

Clement (1999) finds that analyst forecast accuracy is positively associated with the analyst's experience (a surrogate for analyst ability and skill) and employer size (a surrogate for resources available), and negatively associated with the number of firms and industries followed by the analyst (surrogates of portfolio complexity). In Clement (1999), the negative association between forecast accuracy and the number of firms followed by an analyst is attributable to the portfolio complexity. Specifically, larger portfolios (more firms followed) cause the analyst to devote less attention to each individual firm; therefore, the forecast accuracy is negatively impacted. This paper extends the work of Clement (1999) and re-examine the association between analyst firm coverage and forecast accuracy. Specifically, we are going to address following research questions: 1. Is the negative association documented in Clement 1999 really attributable to task complexity? 2. Is the negative association consistent before and after Reg FD? 3. Do investors understand such an association?

The Securities and Exchange Commission (SEC) approved Reg FD on August 10, 2000. These rules stipulate that after October 23, 2000, firms should provide material information to all investors on a fair basis. Prior to Reg FD, firms' executives were able to disclose material information to only a selected few investors and analysts, thereby providing them with an informational advantage. Reg FD thus targets the comparative advantage the selected few investors or analysts gain from direct relationships with the firms' officers. If the number of firms followed by an analyst stands for the task complexity, then it won't be affected by Reg FD, which only affects the level of information among analysts. Therefore, the association between analyst forecast accuracy and number of firms followed will not be affected by Reg FD.

However, we argue that, the number of firms followed is a choice made by analysts. Analysts choose such a group of stocks into their forecast portfolio as long as they can maintain

the forecast accuracy and they will cover as many firms as they can (Hong et al 2000 and Groysberg et al 2011). In other words, analysts tend to cover a number of firms within their ability.

Before Reg FD, a group of analysts can get private information, so their forecast accuracy is higher than the left. However, they have to pay extra efforts and time to acquire the private information, for example, personal meetings with the executives and calls with board members all take additional time from the analyst. Therefore, the time spent on each stock in their forecast portfolio on average is much longer, and they can only cover a relatively smaller group of firms. In this sense, analysts who can issue more accurate forecast based on private information are also the ones who can only follow a small group of stocks.

After Reg FD, analysts mainly rely on their forecast ability on public information (Irani and Karamanou (2003), and Mohanram and Sunder (2006)). Reg FD offers analysts an opportunity to adjust their portfolio by focusing on those firms for which they have a competitive advantage. When an analyst follows more than one firm, he or she faces a larger information set. Synergy can be created in such an information set. Lees (1981) shows that there are economies of scale to following firms in a particular industry. The economies of scale argument suggest that the number of firms followed should be positively associated with forecast accuracy. After Reg FD, a couple reasons favor the argument of economies of scale. First, highly capable analysts are less likely to reduce their firm coverage, but less capable analysts are more likely to do that; second, analysts are more likely to focus on one sector to issue forecasts. Therefore, economies of scale are more likely to exist in the firms analysts keep following. If the above arguments are true, we have the following hypothesis:

***Hypothesis 1:** The association between analyst firm coverage and forecast accuracy would change from negative before Reg FD to positive after Reg FD.*

Next we are interested in whether market participants react differently to forecasts based on the number of firms that the associated analyst is following. If investors fully understand the relationship between analyst firm coverage and forecast accuracy, they will factor the relationship into their investing behaviors. Hence, we have our Hypothesis 2 stated in null form:

***Hypothesis 2:** After Reg FD, market participants update their belief in the changed association between analyst firm coverage and forecast accuracy.*

3. Data and Empirical Models

We collect our analyst forecast data from I/B/E/S database, and our analysis is based on I/B/E/S forecasts of annual earnings from 1985 to 2007. Following prior studies, we retain the last forecast issued by an analyst in a particular year (O'Brien 1990; Sinha, Brown, and Das 1997; Clement 1999; Clement and Tse 2005). In addition, we include only those forecasts issued no earlier than 1 year ahead, and no later than 30 days before, the fiscal year-end. To control for fixed firm effects and year effects, we demean all our variables in accuracy regression by the mean value calculated from all forecasts of the same firm in the same year. Because of this procedure, we eliminate those firm-years for which only one analyst following. We also eliminate observations with missing values for control variables. In total we have 583,757 analyst-firm-year observations, and the number of observations will be reduced in various sub-sample analyses.

3.1 Impact of firm coverage on forecast accuracy

To examine whether forecast accuracy is associated with the number of firms followed by the analyst, we use Model 1 to conduct the empirical analysis following Clement (1999).

Model 1

$$PMAFE_{ijt} = \beta_0 + \beta_1 DForHorizon_{ijt} + \beta_2 DGenExperience_{ijt} + \beta_3 DFirmExperience_{ijt} + \beta_4 DCompanies_{ijt} + \beta_5 DIindustries_{ijt} + \beta_6 DNtop10_{ijt} + \varepsilon_{ijt}$$

Performance, the dependent variable, is measured as the analyst's absolute forecast error minus the mean absolute forecast error of all the analysts following the same stock during the same year.

$$PMAFE_{ijt} = \frac{AFE_{ijt} - AFE_{jt}}{AFE_{jt}} * 100$$

Here, AFE_{ijt} is the absolute forecast error by analyst i follows stock j in year t , and AFE_{jt} is the mean absolute forecast errors by all the analysts who follow stock j in year t . Therefore, $PMAFE$ is negative, when the analyst's performance is better than the average level, and vice versa.

Following Clement 1999, we also include other characteristics that can affect accuracy. *GenExperience*, general experience, equals the number of the years the analyst has issued forecasts; *FirmExperience*, firm experience, equals the number of years the analyst has followed the stock; *Companies*, firm coverage, equals the number of firms followed by the analyst; *Industries*, industry coverage, equals the number of industries followed by the analyst; *ForHorizon*, forecast horizon, equals the number of days between the forecast date and the fiscal year end; *BrokerSize*, brokerage size, equals the number of analysts employed by the brokerage employing the analyst. Following Clement (1999) we use a dummy *Ntop10* to control brokerage

size. *Ntop10* is set to 1 if the analyst is employed in the top brokerage size decile, and 0 otherwise. Following Clement (1999), all the variables have been demeaned by the mean calculated using the analysts who followed the same stock in the same year. Therefore the demeaned variables are defined as *DGenExperience*, *DFirmExperience*, *DCompanies*, *DIndustries*, *DForHorizon* and *DNtop10*.

3.2 The effect of Reg FD

To study the effect of Reg FD on the relationship between analyst firm coverage and forecast error, we take two approaches: 1) We estimate our model 1 separately on subsamples before and after year 2000; 2) We modify our model 1 by including the Reg FD dummy variable along with interaction variables between the Reg FD dummy and other control variables (model 2). The Reg FD dummy equals 1 for the years after 2000, and 0 otherwise. Hence, Reg FD captures the impact of Reg FD on forecast accuracy, while the coefficient of the interaction term *FD_Companies* captures the incremental impact of *Company* on the forecast error after the passage of Reg FD.

Model 2

$$\begin{aligned} PMAFE_{ijt} = & \beta_0 + \beta_1 DForHorizon_{ijt} + \beta_2 FD_DForHorizon_{ijt} + \beta_3 DGenExperience_{ijt} \\ & + \beta_4 FD_DGenExperience_{ijt} + \beta_5 DFirmExperience_{ijt} + \beta_6 FD_DFirmExperience_{ijt} \\ & + \beta_7 DCompanies_{ijt} + \beta_8 FD_DCompanies_{ijt} + \beta_9 DIndustries_{ijt} + \beta_{10} FD_DIndustries_{ijt} \\ & + \beta_{11} DNtop10_{ijt} + \beta_{12} FD_DNtop10_{ijt} + \varepsilon_{ijt} \end{aligned}$$

3.3 Analyst Career Concern

Since we hypothesize that analyst firm coverage is a choice variable of analyst, we also investigate how forecast accuracy and firm coverage affect analysts' career concern using model

3. The dependent variable, *Fire*, is a dummy variable set to 1 if the analyst has stopped forecasting or experienced a downgrade from a large brokerage to a small brokerage. The determinants include *Accuracy*, mean of forecast accuracy (maximum standard error minus standard error and divided by range of standard error of the same stock in the same year, following Clement and Tse (2005) to range the accuracy into 0 to 1) of all the stocks covered by the analyst in the year; here the accuracy is defined as *Companies*, the number of companies covered by the analyst in the year; *Average Coverage*; average of the number of analysts covering the stocks which are covered by the analyst in the year; and *GenExperience*; the number of years that the analyst has issued forecast. We also control year fixed effect in the regression.

Model 3

$$Fire = \beta_1 Accuracy + \beta_2 Accuracy * Companies + \beta_3 Companies + \beta_4 Average Coverage + \beta_5 GenExperience + Year Fixed Effect$$

3.4 Market reaction tests

In the last part of our main analyses, we test whether investors fully incorporate the change in the relationship between analyst firm coverage and forecast accuracy after Reg FD. Specifically, given the same level of forecast revision, we investigate the market response on the number of firms followed by the analysts before and after Reg FD. Following Bonner, Hugon and Walther (2007), we use the raw value of characteristics that will affect forecast accuracy, including forecast horizon, companies covered, general experience, firm experience, industries followed, and a dummy variable to proxy large brokerage (Model 4).

Model 4

$$\begin{aligned} CAR5_{ijt} = & \beta_0 + \beta_1 Fcrev_{ijt} + \beta_2 Fcrev_{ijt} * ForHorizon_{ijt} + \beta_3 Fcrev_{ijt} * Companies_{ijt} \\ & + \beta_4 Fcrev_{ijt} * GenExperience_{ijt} + \beta_5 Fcrev_{ijt} * FirmExperience_{ijt} + \beta_6 Fcrev_{ijt} * Industries_{ijt} \\ & + \beta_7 Fcrev_{ijt} * Ntop10_{ijt} + \varepsilon_{ijt} \end{aligned}$$

Fcrev_{ijt} is the analyst's revised forecast value minus the previous forecast value by the same analyst for the same firm, and divided by the stock price 10 trading days before the release of the revised forecast (Bonner, Hugon and Walther 2007). We use the analyst's prior forecast to calculate the forecast revision since it is more informative to the market than the consensus forecast (Gleason and Lee 2003; Stickel 1991; Imhoff and Lobo 1984). The cumulative abnormal return (**CAR5_{ijt}**, day -2 to day 2) around the analyst's forecast revision date (date 0) is defined as the firm's percentage of compounded return minus the compounded return of the size decile portfolio to which the firm belonged at the beginning of the year. Other variables are defined in the same way as above.

4. Empirical Results and Discussion

4.1. Descriptive statistics and correlation analysis

Table 1 reports the number of analysts, companies, forecasts, and brokerages in different fiscal years. In total, we have 15,343 analysts, 10,521 companies, 583,757 analyst forecasts, and 883 brokerage firms. After 2006, the I/B/E/S database does not provide specific information for analyst names; therefore, we cannot distinguish the team from an individual analyst like Clement (1999). These database changes will affect our sample size to some extent, but will not affect the

general results⁷. Table 2 presents descriptive statistics of all variables used in this study, before and after the passage of Reg FD. All the variables are defined in the same way as Clement (1999), and in this table, the characteristics are the values before being demeaned. On average, one analyst has 3.04 firm-year experiences, and follows 19.32 firms before Reg FD; while after Reg FD, the average analyst has 3.10 firm-year experiences, and follows 13.6 firms.

[Insert Table 1 and Table 2 around here]

Table 3 Panel A tabulates the Pearson and Spearman correlations among the main variables. The correlation analysis offers some insights into the relationships among our variables of interest. *DCompanies* (number of companies followed) is negatively and significantly correlated with the forecast error (Pearson correlation = -0.0006; p-value < 0.0001 and Spearman correlation = -0.016; p-value < 0.0001 respectively); this is along the expected line, even though different from Clement's (1999) results (This finding is not surprising since our sample period is different from that of Clement 1999. We have replicated Clement (1999)'s findings in Table 5 separately). *DIndustries*, which captures the number of industries an analyst follows, is positively correlated with the forecast error (Pearson correlation = 0.013; p-value < 0.0001). Analysts' general experience and firm experience are all negatively associated with the forecast error (Pearson correlation = -0.007 and p-value < 0.0001 for general experiences; Pearson correlation = -0.017 and p-value < 0.0001 for firm experiences). The result of the correlation analysis shows some preliminary evidence that the number of firms an analyst follows is negatively associated with forecast errors. In addition, Table 3 Panel B shows the correlation of our main variables before the passage of Reg FD, while Table 3 Panel C shows the

⁷ From Clement 1999, some entries to the data set are forecasts supplied by individual analysts and others are supplied by teams of analysts. The analyst codes on the usual academic tapes do not distinguish between individuals and teams, and this makes it impossible to identify teams of analysts. The I/B/E/S broker translation file is used to eliminate teams from the sample.

correlation after the passage of Reg FD. As shown in the table, in the Pre-Reg FD period, the correlation between *PMAFE* and *DCompanies* is significant and positive (Pearson correlation = 0.0078 and p-value < 0.0001); while in the Post-Reg FD period, the correlation is significant but negative (Pearson correlation = -0.041 and p-value<0.0001). These findings are consistent with our H2.

[Insert Table 3 around here]

4.2. Impact of analyst firm coverage on forecast accuracy

Table 4 Panel A presents the results of the impact of analyst firm coverage on his or her forecast error. We first replicate the results of Clement (1999) in column 1, then we present the results for the extended pre-Reg FD period in column 2, and finally the results for post-Reg FD period in column 3. The estimated coefficient on *DCompanies* is 0.060 (t-statistics=4.118) for the time period before Reg FD (column 2); while that coefficient is -0.275 (t-statistics=-7.779) for the time period after Reg FD (column 3), supporting our hypothesis that there is a negative association between the number of firms an analyst follows and the error of his or her forecast after the passage of Reg FD (i.e., a positive association between the number of firms an analyst follows and his or her forecast accuracy).

Other control variables exhibit expected signs. For example, the coefficient of the number of industries an analyst follows is positive and significant for both the period before (Para=0.433 and t-statistics=8.769) and after Reg FD (Para=0.742 and t-statistics=7.579), indicating that following more industries decreases forecast accuracy (e.g., Clement 1999). The coefficients on the firm experience variable (*DFirmExperience*) is negative and significant (estimated coefficient = -0.483; t-statistics =-4.920) after Reg FD, again consistent with results from

previous research (e.g., Clement 1999) that forecast accuracy is positively associated with analysts' experience.

[Insert Table 4 Panel A around here]

In Table 4 Panel B, we run Fama-MacBeth type regressions for robustness check. Following Clement (1999), we first run our regressions year by year, and then we use the mean value of the time series of regression coefficients as the final coefficient for each measure. Table 4 Panel B shows that, using this Fama-Macbeth type of approach, our results still hold. Specifically, before the passage of Reg FD, there is a significant and positive relationship between the number of firms an analyst follows and his or her forecast error (Para=0.053; t-statistic=2.471 column 2), while after the passage of Reg FD, the relationship becomes significant and negative (Para=-0.292; t-statistic=-4.496 column 3).

[Insert Table 4 Panel B around here]

In table 4 Panel C, we conducted another robustness check by adding into the regression the dummy variable for Reg FD and also its interaction with firm coverage (*FD_DCompanies*). The coefficient of the *DCompanies* variable captures the relationship between the number of firms and forecast errors before the passage of Reg FD, which is still significantly positive (estimated coefficient=0.060; t-statistics =3.986). The interaction between *DCompanies* and the *FD* dummy captures the incremental impact of firm coverage on forecast error after the passage of Reg FD. Here *FD* dummy equals 1 for those firm-years later than 2000, representing the period after the passage of Reg FD. The coefficient on the interaction term is negatively significant, more importantly, our F test shows that the relation between coverage and forecast

error has become negative after Reg FD.⁸ The results here suggests that, after the passage of Reg FD, the positive impact of the number of firms on forecast error has been changed to negative, again validating our Hypothesis 1.

[Insert Table 4 Panel C around here]

While the results above consistently support our hypothesis 2, it is still possible that our results may be driven by a mechanical change introduced by Reg FD. To be more specific, previous literature, such as Irani and Karamanou (2003) and Mohanram and Sunder (2006), show that analyst firm coverage has significantly decreased after Reg FD, and our main results may be driven by those analysts who follow an extreme large number of firms before Reg FD. First, we replicate the previous literature in Table 5, and show the mean difference of the number of firms an analyst follows before and after Reg FD. For robustness check, we compare the mean, median, top quartile, and bottom quartile differences. Our results show that regardless of the measures used, the number of firms an analyst follows before Reg FD is always greater than after Reg FD, indicating that, after the passage of Reg FD, analysts have reduced the number of firms they follow.

[Insert Table 5 around here]

Therefore, from the above results, the mechanical change in analyst firm coverage introduced by Reg FD may be the driver of the change of the association between analyst firm coverage and forecast accuracy after Reg FD. To address this concern, we first identify the maximum number of companies an analyst followed **after** the Reg FD period, which is 77. Then we repeat our analysis on the Reg FD subsample by deleting those analysts who followed more than 77 firms. If the results are caused by analysts with extremely large firm coverage before Reg FD, then we should also be able to observe a negative relationship between firm coverage

⁸ $-0.339+0.060 = -0.279$ is significantly different from zero (F-test=67.27), which shows that the coefficient on the number of firms an analyst follows is significantly negative after Reg FD.

and analyst forecast error before Reg FD, after truncating the outliers. However, our Table 6 shows that the coefficient before *DCompanies* is still significant and positive (estimated coefficient = 0.044; t-statistics =2.702). Therefore, the finding supports that the change is not driven by the mechanical reduction of the number of firms followed by analysts.

[Insert Table 6 around here]

Another unaddressed concern is that our results might be driven by the survival bias; it is possible that analysts who cover a large number of firms before Reg FD have dropped from the sample after Reg FD, and our results are only driven by the sample change. To rule out this possibility, we limited our samples to those analysts that have been active both before and after Reg FD (either active between 1996 to 2004, or active between 1997 to 2003), and repeat our analyses. Using the subsample for 1997 to 2003 as an example, Table 7 show that, even with the same group of analyst, there is still a positive and significant relationship between analyst firm coverage and forecast errors before Reg FD (estimated coefficient = 0.083; t-statistics =2.456); however, that relation flips to significantly negative (estimated coefficient = -0.196; t-statistics =-3.427) after Reg FD.

[Insert Table 7 around here]

4.3. Reason of the change of association between forecast errors and number of firms followed

4.3.1 Analyst career concern

Our previous results have supported our view that, firm coverage is not as simple as a surrogate for task complexity, and we argue that it is a choice variable of analysts. Following Hong et al (2000), we empirically show how analyst firm coverage and forecast accuracy affect

analyst career. Table 8 tabulates determinants of analyst turnover. From the results, an analyst is less likely to be fired if his/her forecast accuracy is high in both pre Reg FD and post Reg FD sample (coefficient=-0.766 with t-statistics= -16.650 in pre Reg FD sample and coefficient=-0.644 with t-statistics= -14.274 in post Reg FD sample). More importantly, the interaction item of forecast accuracy and the number of firms covered by the analysts is significantly negative, meaning that, with the level of forecast accuracy held constant, the more firms covered by an analyst, the less likely he/she will get fired.

[Insert Table 8 around here]

4.3.2 Extra time and resources spent on acquiring private information before Reg FD

From the empirical tests above, we show that the association between analyst firm coverage and forecast error is positive before Reg FD, and becomes negative after Reg FD. Hence, we argue that the number of firms followed is not a proxy for task complexity; otherwise we should be able to observe consistent results from the beginning through the end. Instead, we hypothesize that, firm coverage *per se* is also a choice variable of analysts. Before Reg FD, analysts who are accessible to private information can only cover a relatively smaller group of firms. The reason is that, while public information, like financial statements, is valuable to all analysts, the efforts to acquire and process private information will require significant time and resources from an analyst. In short, the private information benefits the analyst in forecast accuracy at the expense of his/her resources, and the positive relationship between firm coverage and forecast accuracy before Reg FD simply reflects the fact that analysts who have private information have less time and resources to follow more firms.

One implication from above argument is that the association between number of firms followed by the analyst and analyst forecast accuracy should be different between large brokerage and small brokerage in pre Reg FD sample. Analysts in small brokerage should take larger effort and more time to acquire private information than analysts in large brokerage. For firms are more willing to contact with analysts in large brokerage and leak information to them before it goes to public. In table 9, we can see the negative coefficient is only significant in small brokerage subsample (coefficient=0.056 and with t-statistics=3.431).

[Insert Table 9 around here]

Analysts who have relied on private information before Reg FD would like to cover more firms after Reg FD, especially for capable analysts, for they can save time from acquiring private information to analyzing more firms. Therefore, we investigate what happens to forecast errors after Reg FD when an analyst increases the number of companies he or she follows. We define a new dummy variable called *com_dummy*, which equals 1 if the number of companies covered by an analyst increases after Reg FD. For robustness check, for each analyst, we define this variable by comparing the number of companies he or she covers in year 2000 to that of 1999. Alternatively we compare the numbers of year 2001 and 2000 (2001 and 1999), and we get qualitatively similar results.

Table 10 presents the results of the impact of the increase in the number of companies covered on forecast error. The coefficient of the *com_dummy* variable captures such an impact. The coefficient on this term is negatively significant (e.g., estimated coefficient=-4.056; and t-statistics=-6.390 for period 1999 to 2001), indicating that, if an analyst follows one additional firm, his or her forecast error will significantly decrease. Again, our results show that after the

passage of Reg FD, analysts who have increased the number of firms followed are more likely to increase the forecast accuracy.

[Insert Table 10 around here]

4.3.4 Economics of Scales after Reg FD

Since the time spent on acquiring private information can explain the positive association between analyst firm coverage and forecast error; in this part, we are investigating the reason behind negative association between forecast error and number of firms followed. We use I/B/E/S database to define 11 main sectors, and define the analyst has concentrated on one sector if more than 75% of his or her firm coverage is from the same sector. Before Reg FD, only 64% of the analyst forecast concentrated on one sector; that might be due to the private information acquired by the analysts is in unrelated industries. However, after Reg FD, analysts tend to cover the related stocks to reach the economics of scales. Therefore, we run the regression of analyst forecast error on *Concentration* with other variables. *Concentration* is a dummy variable that equals 1 if more than 75% of the firms covered by the analysts are from the same sector and zero otherwise. We put into regression both *Concentration* and the interaction term of *Concentration* and *DCompanies*, and find that the association between forecast accuracy and the number of firms covered by the analysts is not affected by sector concentration before Reg FD. However, after Reg FD, the coefficient on *DCompanies* is 0.04 with t statistics at -0.578, which is insignificant while the interaction item of *Concentration* and *DCompanies* is negatively significant at -0.409 with t statistics at -5.685. It supports our hypothesis that the negative association between forecast error and number of firms followed is due to economics of scales. After Reg FD, all the analysts are facing the same information set. Therefore, capable analysts can better analyze public information and reach the economics of scales. We also use 50% as the

cutoff point of concentration to do robustness check. Untabulated results show that the finding here is robust.

[Insert Table 11 around here]

4.4. Market response tests

Our previous results indicate that there is a negative relationship between the number of firms an analyst follows and his or her forecast accuracy before Reg FD and that relationship has become positive after the passage of Reg FD. A natural follow-up question is whether stock market participants understand such a relationship.

Table 12 shows how the market responds to forecast revisions issued by analysts with various backgrounds, experience, and capabilities. The test of market reaction after Reg FD is shown in the column 1. Our main focus is on the interaction term: *Fcrev*Companies*. We find that the coefficient on *Fcrev*Companies* is significantly negative (-0.695) with a t-statistics=-3.262. Therefore, our results support hypothesis 2 that the market participants discount revisions from analysts following more firms after year 2000, since they do not update their belief on the effect of number of firms followed on forecast accuracy after year 2000. Market participants on average still believe that there is a negative association between firm coverage and analyst forecast accuracy even after the passage of Reg FD. Our findings should be interesting and important to these market participants.

For comparison purpose, we also investigate the market response for two other time periods, from 1985 to 1995 and pre-2000 in column 2 column 3 respectively. Interestingly, we find that the coefficients on *Fcrev*Companies* are significantly negative in both periods, indicating that such a discount effect has existed for a long-time period.

[Insert Table 12 around here]

5. Conclusion

In this paper, we examine whether the number of firms analysts follow impacts forecast accuracy. In addition, we study whether the market understands the existence of such a relationship. We find that even though firm coverage decreases the analysts' forecast accuracy, such a negative impact has been flipped to being positive after the passage of Reg FD. We rule out the explanation raised by Clement (1999) that the more firms followed, the more complicated the forecast task is. We show that the analyst firm coverage is a choice by analyst. Considering career concern, the analysts would like to cover a number of firms which they can maintain the forecast accuracy. Before Reg FD, analysts who have access to private information can only cover a smaller group of firms, but with higher forecast accuracy; While after Reg FD, more capable analysts tend to cover more stocks to reach the economics of scales and increase the forecast accuracy.

In addition, we find that, before the passage of Reg FD, the market responds less to the forecast revisions issued by analysts who follow a larger number of firms, which is consistent with the notion that the market sees through the negative impact of firm coverage on forecast accuracy. However, our results show that the market still reacts less to forecast revisions issued by analysts who follow a larger number of firms after the passage of Reg FD, even though larger firm coverage is associated with more accurate forecast during that period. The result shows that the market under-reacts to forecast revisions issued by analysts following a relatively large number of firms after the passage of Reg FD.

Our study contributes to the analyst forecast literature by documenting a change on association between forecast accuracy and number of firms followed by the analyst. Our paper

contributes to the Reg FD literature by highlighting the role played by Reg FD on the choice of analyst forecast firms. Our results imply that the information leveling change has affected the analysts' forecast portfolio. In addition, given that the market only understands such a relationship to some degree, we encourage market participants to spend more time and effort in understanding the fundamental impact of firm coverage on the capital markets. It is still important regardless of the fact that such information has been available to public for decades.

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Table 1 Sample Construction

Fyear	Analysts	Companies	Forecasts	Brokerage
1985	1,937	1,382	16,979	132
1986	1,968	1,529	18,279	131
1987	1,979	1,771	18,825	151
1988	1,849	1,922	19,151	166
1989	2,044	2,322	22,284	175
1990	2,125	2,495	21,541	182
1991	1,932	2,484	21,241	183
1992	1,857	2,700	22,483	185
1993	2,093	3,041	25,061	207
1994	2,374	3,323	25,694	213
1995	2,555	3,507	26,326	218
1996	2,864	4,025	28,578	249
1997	3,213	4,199	29,636	283
1998	3,485	4,096	29,943	301
1999	3,614	3,907	31,044	283
2000	3,687	3,588	28,531	278
2001	3,565	3,158	25,840	238
2002	3,697	3,193	26,327	232
2003	3,652	3,124	26,554	307
2004	3,687	3,400	29,233	351
2005	3,707	3,586	30,531	351
2006	3,747	3,659	31,608	329
2007	3,571	3,080	28,068	294
<i>Total</i>	15,343	10,521	583,757	883

In this table, we summarize our sample, and count the number of analysts, companies followed, forecasts, and brokerages in each fiscal year.

Table 2 Descriptive Statistics

Variable	Whole Sample N=583,757		Pre 2000 N=357,065		Post 2000 N=226,692	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
AFE	0.17	0.34	0.18	0.36	0.15	0.31
Companies	17.10	13.04	19.32	14.94	13.60	8.15
FirmExperience	3.07	2.57	3.04	2.49	3.10	2.68
GenExperience	6.14	4.38	5.78	3.86	6.71	5.02
BrokerSize	50.89	47.97	41.44	37.14	65.77	58.23
ForHorizon	110.39	75.83	115.38	77.86	102.54	71.81
Industries	6.31	4.06	7.00	4.38	5.22	3.21

In this table, we summarize the raw value (before being demeaned) of analyst related characteristics. AFE is the absolute value of forecast error, the value of EPS issued by the analyst minus the actual value of EPS. The other characteristics include GenExperience, general experience, number of the years the analyst has issued forecasts, FirmExperience, firm experience, the number of years the analyst has followed the stock, Companies, the number of firms followed, Industries, the number of industries followed, ForHorizon, forecast horizon, the distance between forecast date and fiscal year end, BrokerSize, the number of analysts in the brokerage. We also separate our sample into two subsamples based on Reg FD at year 2000.

Table 3 Correlations among Main Variables

Panel A: Whole Sample							
Pearson and Spearman Correlation Coefficients, N = 583,757							
	PMAFE	DForHorizon	DGenExperience	DFirmExperience	DCompanies	DIndustries	DNtop10
PMAFE		0.319	-0.005	-0.007	-0.016	0.009	-0.002
		<.0001	0.000	<.0001	<.0001	<.0001	0.365
DForHorizon	0.378		0.014	0.021	-0.037	-0.002	0.015
	<.0001		<.0001	<.0001	<.0001	0.226	<.0001
DGenExperience	-0.007	0.014		0.586	0.284	0.212	0.027
	<.0001	<.0001		<.0001	<.0001	<.0001	<.0001
DFirmExperience	-0.017	0.007	0.592		0.166	0.101	0.045
	<.0001	<.0001	<.0001		<.0001	<.0001	<.0001
DCompanies	-0.006	-0.040	0.239	0.139		0.647	-0.032
	<.0001	<.0001	<.0001	<.0001		<.0001	<.0001
DIndustries	0.013	-0.009	0.192	0.081	0.642		-0.032
	<.0001	<.0001	<.0001	<.0001	<.0001		<.0001
DNtop10	-0.031	-0.010	0.024	0.028	-0.075	-0.068	
	<.0001	<.0001	<.0001	<.0001	<.0001	<.0001	

The upper triangle is the Spearman correlation, and the lower triangle is the Pearson correlation. PMAFE is the percentage value of demeaned absolute forecast error. DGenExperience is demeaned general experience, calculated as number of the years the analyst has issued forecasts minus the average general experience of all analysts who followed the stock in the year. DFirmExperience is demeaned firm experience, calculated as the number of years of the analyst has followed the stock minus the average firm experience of all analysts who followed the stock in the year. DCompanies, is the demeaned number of companies followed, calculated as number of companies followed by the analyst minus the average number of companies followed by all analysts who followed the stock in the year. DIndustries, is the demeaned number of industries followed, calculated as number of industries followed by the analyst minus the average number of industries followed by all analysts who followed the stock in the year. ForHorizon, is the demeaned forecast horizon (the distance between forecast date and fiscal year end), calculated as forecast horizon minus the average forecast horizon by all the analysts who followed the same stock in the year. Ntop10 is set to 1 if the analyst is employed in the top size decile of broker size, and 0 otherwise. DNtop10 is demeaned value of Ntop10.

Panel B: Pre 2000							
Pearson and Spearman Correlation Coefficients, N = 357,065							
	PMAFE	DForHorizon	DGenExperience	DFirmExperience	DCompanies	DIndustries	DNtop10
PMAFE		0.296	-0.015	-0.020	0.002	0.025	-0.012
		<.0001	<.0001	<.0001	0.260	<.0001	<.0001
DForHorizon	0.337		0.007	0.009	-0.027	0.009	0.016
	<.0001		<.0001	<.0001	<.0001	<.0001	<.0001
DGenExperience	-0.016	0.008		0.581	0.262	0.196	0.033
	<.0001	<.0001		<.0001	<.0001	<.0001	<.0001
DFirmExperience	-0.029	-0.004	0.589		0.149	0.080	0.046
	<.0001	0.031	<.0001		<.0001	<.0001	<.0001
DCompanies	0.008	-0.031	0.234	0.132		0.648	-0.047
	<.0001	<.0001	<.0001	<.0001		<.0001	<.0001
DIndustries	0.023	-0.001	0.188	0.066	0.639		-0.044
	<.0001	0.639	<.0001	<.0001	<.0001		<.0001
DNtop10	-0.043	-0.005	0.044	0.041	-0.094	-0.081	
	<.0001	0.005	<.0001	<.0001	<.0001	<.0001	

Panel C: Post 2000							
Pearson and Spearman Correlation Coefficients, N = 226,692							
	PMAFE	DForHorizon	DGenExperience	DFirmExperience	DCompanies	DIndustries	DNtop10
PMAFE		0.360	0.004	0.009	-0.046	-0.018	0.014
		<.0001	0.085	<.0001	<.0001	<.0001	<.0001
DForHorizon	0.444		0.027	0.044	-0.067	-0.028	0.012
	<.0001		<.0001	<.0001	<.0001	<.0001	<.0001
DGenExperience	0.002	0.022		0.598	0.351	0.261	0.023
	0.329	<.0001		<.0001	<.0001	<.0001	<.0001
DFirmExperience	-0.002	0.022	0.602		0.212	0.148	0.043
	0.192	<.0001	<.0001		<.0001	<.0001	<.0001
DCompanies	-0.041	-0.073	0.316	0.180		0.640	-0.006
	<.0001	<.0001	<.0001	<.0001		<.0001	0.007
DIndustries	-0.008	-0.029	0.231	0.118	0.663		-0.014
	<.0001	<.0001	<.0001	<.0001	<.0001		<.0001
DNtop10	-0.012	-0.018	0.002	0.010	-0.029	-0.041	
	<.0001	<.0001	0.258	<.0001	<.0001	<.0001	

Table 4 Accuracy Regressions**Panel A: Pooled Regressions**

Variables	Dependent Variable = PMAFE		
	1985-1995	Pre 2000	Post 2000
DForHorizon	0.370*** (159.937)	0.415*** (214.057)	0.653*** (234.712)
DGenExperience	-0.416*** (-5.020)	-0.225*** (-4.198)	0.031 (0.607)
DFirmExperience	-0.978*** (-8.506)	-0.991*** (-11.883)	-0.483*** (-4.920)
DCompanies	0.042** (2.527)	0.060*** (4.118)	-0.275*** (-7.779)
DIndustries	0.441*** (7.788)	0.433*** (8.769)	0.742*** (7.579)
DNtop10	-11.023*** (-18.641)	-11.453*** (-23.959)	-1.362** (-2.144)
No. of OBS	237,864	357,065	226,692
R-squared	0.100	0.116	0.198

The dependent variable is the PMAFE, the percentage value of demeaned absolute forecast error. The characteristics include DGenExperience, demeaned general experience, calculated as number of the years the analyst has issued forecasts minus the average general experience of all analysts who followed the stock in the year. DFirmExperience is demeaned firm experience, calculated as the number of years of the analyst has followed the stock minus the average firm experience of all analysts who followed the stock in the year. DCompanies, is the demeaned number of companies followed, calculated as number of companies followed by the analyst minus the average number of companies followed by all analysts who followed the stock in the year. DIndustries, is the demeaned number of industries followed, calculated as number of industries followed by the analyst minus the average number of industries followed by all analysts who followed the stock in the year. ForHorizon, is the demeaned forecast horizon (the distance between forecast date and fiscal year end), calculated as forecast horizon minus the average forecast horizon by all the analysts who followed the same stock in the year. Ntop10 is set to 1 if the analyst is employed in the top decile of broker size, and 0 otherwise. DNtop10 is demeaned value of Ntop10.

Table 4 Accuracy Regressions

Panel B: Time Series Accuracy Regression			
Variables	Dependent Variable = PMAFE		
	1985-1995	Pre 2000	Post 2000
DForHorizon	0.369*** (18.657)	0.409*** (17.235)	0.650*** (15.587)
DGenExperience	-0.321** (-2.519)	-0.289*** (-2.970)	0.030 (0.881)
DFirmExperience	-0.581 (-1.199)	-0.686 (-1.912)	-0.454*** (-3.002)
DCompanies	0.031 (1.236)	0.053** (2.471)	-0.292*** (-4.496)
DIndustries	0.473*** (5.082)	0.460*** (5.310)	0.617 *** (3.536)
DNtop10	-10.776 *** (-10.406)	-10.940*** (-12.939)	-1.568 (-0.865)

We run the regression year by year, and the coefficient is calculated as the mean value of the time series of regression coefficient. The t value is calculated as $mean / (\frac{std}{\sqrt{n}})$. The dependent variable is the PMAFE, the percentage value of demeaned absolute forecast error. The characteristics include DGenExperience, demeaned general experience, calculated as number of the years the analyst has issued forecasts minus the average general experience of all analysts who followed the stock in the year. DFirmExperience is demeaned firm experience, calculated as the number of years of the analyst has followed the stock minus the average firm experience of all analysts who followed the stock in the year. DCompanies, is the demeaned number of companies followed, calculated as number of companies followed by the analyst minus the average number of companies followed by all analysts who followed the stock in the year. DIndustries, is the demeaned number of industries followed, calculated as number of industries followed by the analyst minus the average number of industries followed by all analysts who followed the stock in the year. ForHorizon, is the demeaned forecast horizon (the distance between forecast date and fiscal year end), calculated as forecast horizon minus the average forecast horizon by all the analysts who followed the same stock in the year. Ntop10 is set to 1 if the analyst is employed in the top decile of broker size, and 0 otherwise. DNtop10 is demeaned value of Ntop10.

Table 4 Accuracy Regressions

Panel C: Accuracy regression with FD dummy			
Variables	Dependent Variable = PMAFE		
	Estimate	F_value	P
FD	-0.785** (-2.193)		
DCompanies	0.060*** (3.986)		
FD_DCompanies	-0.334*** (-8.936)	59.76	<.0001
DForHorizon	0.415*** (209.751)		
FD_DForHorizon	0.237*** (70.961)	58564.4	<.0001
DGenExperience	-0.267*** (-5.082)		
FD_DGenExperience	0.342*** (5.069)	2.47	0.1159
DFirmExperience	-0.877*** (-11.698)		
FD_DFirmExperience	0.253*** (2.809)	64.17	<.0001
DIndustries	0.437*** (8.685)		
FD_DIndustries	0.301*** (2.800)	60.5	<.0001
DNtop10	-11.464*** (-23.498)		
FD_DNtop10	10.111*** (12.869)	4.82	0.0281
No. of OBS	583,757		
R-squared	0.152		

The dependent variable is the PMAFE, the percentage value of demeaned absolute forecast error. The characteristics include DGenExperience, demeaned general experience, calculated as number of the years the analyst has issued forecasts minus the average general experience of all analysts who followed the stock in the year. DFirmExperience is demeaned firm experience, calculated as the number of years of the analyst has followed the stock minus the average firm experience of all analysts who followed the stock in the year. DCompanies, is the demeaned number of companies followed, calculated as number of companies followed by the analyst minus the average number of companies followed by all analysts who followed the stock in the year. DIndustries, is the demeaned number of industries followed, calculated as number of industries followed by the analyst minus the average number of industries followed by all analysts who followed the stock in the year. ForHorizon, is the demeaned forecast horizon (the distance between forecast date and fiscal year end), calculated as forecast horizon minus the average forecast horizon by all the analysts who followed the same stock in the year. Ntop10 is set to 1 if the analyst is employed in the top size decile of broker size, and 0 otherwise. DNtop10 is demeaned value of Ntop10. We include FD dummy, FD equals 1 after 2000, and 0 before 2000, and we also include interaction items of Reg FD with other characteristics.

Table 5 T-test of Number of Companies Followed by Analyst Before and After Reg FD

Statistic Variables	Variable: <i>Companies</i>			
	Pre 2000	Post 2000	Diff	P Value
Mean	19.32	13.60	5.72	<.0001
Top Quartile	10	9	1	<.0001
Median	15	13	2	<.0001
Bottom Quartile	23	17	6	<.0001
<i>No. of OBS</i>	357,065	226,692		

This table shows the t-test on the number of analysts between two subsamples, pre 2000 and post 2000. Companies is the number of firms followed by each analyst. We compare the mean, top quartile, median and bottom quartile difference between the two subsamples.

**Table 6 Robustness Check by Deleting the Extreme Subsample
(Keeping the observations with less than 77 companies covered)**

Dependent Variable = PMAFE	
Variable	Estimate
DForHorizon	0.417*** (213.034)
DGenExperience	-0.220*** (-4.062)
DFirmExperience	-0.978*** (-11.608)
DCompanies	0.044*** (2.702)
DIndustries	0.472*** (9.279)
DNtop10	-11.436*** (-23.877)
No of OBS	351,296
R-squared	0.117

In this table, we delete the analysts with extremely large number of companies followed. The maximum number of companies covered in the post sample is 77. Therefore, we delete the observations with Companies larger or equal to 77 and rerun the regression in the pre 2000 sample. The dependent variable is the PMAFE, the percentage value of demeaned absolute forecast error. The characteristics include DGenExperience, demeaned general experience, calculated as number of the years the analyst has issued forecasts minus the average general experience of all analysts who followed the stock in the year. DFirmExperience is demeaned firm experience, calculated as the number of years of the analyst has followed the stock minus the average firm experience of all analysts who followed the stock in the year. DCompanies, is the demeaned number of companies followed, calculated as number of companies followed by the analyst minus the average number of companies followed by all analysts who followed the stock in the year. DIndustries, is the demeaned number of industries followed, calculated as number of industries followed by the analyst minus the average number of industries followed by all analysts who followed the stock in the year. ForHorizon, is the demeaned forecast horizon (the distance between forecast date and fiscal year end), calculated as forecast horizon minus the average forecast horizon by all the analysts who followed the same stock in the year. Ntop10 is set to 1 if the analyst is employed in the top decile of broker size, and 0 otherwise. DNtop10 is demeaned value of Ntop10.

Table 7 Robustness Check by Keeping the Sample with Analyst Active Before and After Reg FD

	Dependent Variable = PMAFE			
	Active Through 1996-2004		Active Through 1997-2003	
	pre 2000	post 2000	pre 2000	post 2000
DForHorizon	0.358*** (70.789)	0.578*** (92.964)	0.363*** (81.599)	0.592*** (111.940)
DGenExperience	-0.179 (-1.594)	0.298*** (3.019)	-0.226** (-2.361)	0.309*** (3.656)
DFirmExperience	-1.056*** (-6.158)	-0.540*** (-4.052)	-1.126*** (-7.496)	-0.441*** (-3.557)
DCompanies	0.062 (1.503)	-0.129** (-2.101)	0.083** (2.456)	-0.196*** (-3.427)
DIndustries	0.448*** (3.603)	0.378** (1.985)	0.390*** (3.573)	0.335** (1.972)
DNtop10	-8.455*** (-7.763)	-2.376** (-1.963)	-7.179*** (-7.618)	-2.124** (-1.975)
No. of OBS	52,907	45,323	67,910	60,341
R-squared	0.090	0.163	0.092	0.175

In this table, we keep only the analysts live through all the sample period. In the first two columns, we keep 492 analysts living through 1996 to 2004. And in the last two columns we keep 736 analysts living through 1997 to 2003. The dependent variable is the PMAFE, the percentage value of demeaned absolute forecast error. The characteristics include DGenExperience, demeaned general experience, calculated as number of the years the analyst has issued forecasts minus the average general experience of all analysts who followed the stock in the year. DFirmExperience is demeaned firm experience, calculated as the number of years of the analyst has followed the stock minus the average firm experience of all analysts who followed the stock in the year. DCompanies, is the demeaned number of companies followed, calculated as number of companies followed by the analyst minus the average number of companies followed by all analysts who followed the stock in the year. DIndustries, is the demeaned number of industries followed, calculated as number of industries followed by the analyst minus the average number of industries followed by all analysts who followed the stock in the year. ForHorizon, is the demeaned forecast horizon (the distance between forecast date and fiscal year end), calculated as forecast horizon minus the average forecast horizon by all the analysts who followed the same stock in the year. Ntop10 is set to 1 if the analyst is employed in the top decile of broker size, and 0 otherwise. DNtop10 is demeaned value of Ntop10.

Table 8 Analyst Career Concern

Variable	Dependent Variable= <i>Fire</i>	
	Pre FD	Post FD
Accuracy	-0.787 (-17.098)***	-0.668 (-14.758)***
Accuracy*Companies	-0.123 (-18.862)***	-0.167 (-22.097)***
Companies	0.031 (7.805)***	0.064 (12.821)***
Average Coverage	0.021 (21.433)***	0.020 (17.597)***
GenExperience	0.006	0.008 (3.978)***
year fixed effect	yes	yes
No of OBS	0.1361	0.1901
R-squared	35,110	28,754

In this table, we run regression probit regression of analyst career concern. The dependent variable is fire. Fire equals to 1 if the analyst stop forecast or transfer from a large brokerage to a small brokerage. The independent variables include Accuracy, mean of accuracy of all the stocks covered by the analyst in the year; Companies, the number of companies covered by the analyst in the year; Average Coverage; average of the number of analysts covered the stocks which are covered by the analyst in the year; GenExperience; the number of years that the analyst has issued forecast.

Table 9 Accuracy Regression: Large Brokerage vs Small Brokerage

Variable	Dependent Variable= <i>PMAFE</i>			
	Pre FD		Post FD	
	Large brokerage	Small brokerage	Large brokerage	Small Brokerage
DCompanies	0.046 (0.793)	0.056 (3.431)***	-0.565 (-2.459)**	-0.262 (-5.859)***
DForHorizon	0.375 (51.619)***	0.405 (177.318)***	0.603 (37.126)***	0.670 (181.082)***
DGenExperience	0.278 (1.386)	-0.207 (-3.286)***	-0.259 (-0.836)	-0.082 (-1.246)
DFirmExperience	-0.846 (-2.794)***	-1.054 (-10.764)***	-1.358 (-2.479)**	-0.406 (-3.215)***
DIndustries	-0.608 (-3.200)***	0.462 (8.267)***	1.209 (1.925)*	0.811 (6.563)***
Constant	-9.132 (-18.074)***	-0.481 (-3.069)***	-2.649 (-2.623)***	-1.719 (-7.328)***
No of OBS	20,741	266,611	6,549	122,335
R-squared	0.116	0.107	0.181	0.213

In this table, we separate the sample by regulation FD and brokerage size. The dependent variable is the *PMAFE*, the percentage value of demeaned absolute forecast error. The characteristics include *DGenExperience*, demeaned general experience, calculated as number of the years the analyst has issued forecasts minus the average general experience of all analysts who followed the stock in the year. *DFirmExperience* is demeaned firm experience, calculated as the number of years of the analyst has followed the stock minus the average firm experience of all analysts who followed the stock in the year. *DCompanies*, is the demeaned number of companies followed, calculated as number of companies followed by the analyst minus the average number of companies followed by all analysts who followed the stock in the year. *DIndustries*, is the demeaned number of industries followed, calculated as number of industries followed by the analyst minus the average number of industries followed by all analysts who followed the stock in the year. *ForHorizon*, is the demeaned forecast horizon (the distance between forecast date and fiscal year end), calculated as forecast horizon minus the average forecast horizon by all the analysts who followed the same stock in the year.

Table 10 Accuracy Regression: Analysts who Increased Firm Coverage through Reg FD

Variables	Dependent Variable = <i>PMAFE</i>		
	1999-2000	2000-2001	1999-2001
DForHorizon	0.700*** (156.068)	0.696*** (167.727)	0.699*** (150.351)
DGenExperience	0.053 (0.668)	0.082 (1.146)	-0.002 (-0.023)
DFirmExperience	-0.387*** (-3.133)	-0.377*** (-3.125)	-0.390*** (-3.087)
DCompanies	-0.187*** (-3.549)	-0.204*** (-4.095)	-0.167*** (-3.092)
DIndustries	0.648*** (4.248)	0.641*** (4.542)	0.750*** (4.814)
DNtop10	-4.546*** (-4.574)	-3.261*** (-3.577)	-4.716*** (-4.610)
com_dummy	-2.212*** (-3.606)	-4.099*** (-7.170)	-4.056*** (-6.390)
No of OBS	87,465	103,964	81,870
R-squared	0.221	0.217	0.220

In this table, we form a new variable called *com_dummy*, 1 if the number of companies covered has increased through the selection year, and 0 otherwise. Therefore, column 1 means that we compare 2000 with 1999 analysts, and form the *com_dummy*, 1 if the number of companies covered has increased through 1999 to 2000, and 0 otherwise. The dummy in column 2 and 3 is defined in a similar way. The regression is used post 2001 sample period. The dependent variable is the *PMAFE*, the percentage value of demeaned absolute forecast error. The characteristics include *DGenExperience*, demeaned general experience, calculated as number of the years the analyst has issued forecasts minus the average general experience of all analysts who followed the stock in the year. *DFirmExperience* is demeaned firm experience, calculated as the number of years of the analyst has followed the stock minus the average firm experience of all analysts who followed the stock in the year. *DCompanies*, is the demeaned number of companies followed, calculated as number of companies followed by the analyst minus the average number of companies followed by all analysts who followed the stock in the year. *DIndustries*, is the demeaned number of industries followed, calculated as number of industries followed by the analyst minus the average number of industries followed by all analysts who followed the stock in the year. *ForHorizon*, is the demeaned forecast horizon (the distance between forecast date and fiscal year end), calculated as forecast horizon minus the average forecast horizon by all the analysts who followed the same stock in the year. *Ntop10* is set to 1 if the analyst is employed in the top decile of broker size, and 0 otherwise. *DNtop10* is demeaned value of *Ntop10*.

Table 11 Economics of scales

Variable	Dependent Variable = <i>PMAFE</i>					
	Pre FD			Post FD		
Dcompanies	0.058 (3.703)***	0.064 (4.090)***	0.067 (2.880)***	-0.272 (-6.179)***	-0.268 (-6.092)***	0.04 -0.578
Concentration		-2.778 (-7.824)***	-2.774 (-7.794)***		-1.674 (-2.935)***	-1.468 (-2.569)**
Concentration *Dcompanies			-0.004 (-0.162)			-0.409 (-5.685)***
DForHorizon	0.403 (184.624)***	0.403 (184.450)***	0.403 (184.447)***	0.667 (184.866)***	0.667 (184.826)***	0.666 (184.523)***
DGenExperience	-0.17 (-2.822)***	-0.16 (-2.660)***	-0.16 (-2.656)***	-0.089 (-1.379)	-0.088 (-1.370)	-0.075 (-1.161)
DFirmExperience	-1.04 (-11.136)***	-1.022 (-10.945)***	-1.022 (-10.946)***	-0.456 (-3.702)***	-0.449 (-3.646)***	-0.442 (-3.585)***
Dindustries	0.394 (7.337)***	0.274 (4.914)***	0.273 (4.816)***	0.822 (6.780)***	0.766 (6.248)***	0.65 (5.232)***
DNtop10	-9.431 (-15.656)***	-9.376 (-15.566)***	-9.375 (-15.565)***	-0.218 (-0.205)	-0.19 (-0.179)	-0.148 (-0.139)
Constant	-1.351 (-8.973)***	0.683 (2.273)**	0.677 (2.234)**	-1.759 (-7.550)***	-0.436 (-0.858)	-0.702 (-1.377)
No of OBS	287,352	287,352	287,352	128,884	128,884	128,884
R-squared	0.108	0.108	0.108	0.212	0.212	0.212

In this table, we run regression pre and post Reg FD and add in the dummy concentration. Concentration equals to 1 if more than 75% of the firms covered by the analysts are from the same sector. Otherwise, concentration equals to 0. The dependent variable is the PMAFE, the percentage value of demeaned absolute forecast error. The characteristics include DGenExperience, demeaned general experience, calculated as number of the years the analyst has issued forecasts minus the average general experience of all analysts who followed the stock in the year. DFirmExperience is demeaned firm experience, calculated as the number of years of the analyst has followed the stock minus the average firm experience of all analysts who followed the stock in the year. DCompanies, is the demeaned number of companies followed, calculated as number of companies followed by the analyst minus the average number of companies followed by all analysts who followed the stock in the year. DIndustries, is the demeaned number of industries followed, calculated as number of industries followed by the analyst minus the average number of industries followed by all analysts who followed the stock in the year. ForHorizon, is the demeaned forecast horizon (the distance between forecast date and fiscal year end), calculated as forecast horizon minus the average forecast horizon by all the analysts who followed the same stock in the year. Ntop10 is set to 1 if the analyst is employed in the top decile of broker size, and 0 otherwise. DNtop10 is demeaned value of Ntop10.

Table 12 Market Reaction

Variables	Dependent Variable = <i>CAR5</i>		
	post 2000	1985-1995	pre 2000
Fcrev	82.733*** (22.508)	2.761 -1.264	18.317*** (8.216)
FcFcrev_ForHorizon	0.125*** (5.945)	-0.001 (-0.067)	0.028** (2.174)
Fcrev_Companies	-0.695*** (-3.262)	-0.142** (-2.538)	-0.425*** (-6.598)
Fcrev_GenExperience	-0.087 (-0.263)	2.736*** (9.091)	4.057*** (16.596)
Fcrev_FirmExperience	-1.431** (-2.229)	-0.821* (-1.881)	-1.617*** (-3.823)
Fcrev_Industries	3.233*** (6.388)	-0.03 (-0.161)	-0.409** (-2.000)
Fcrev_Ntop10	7.177 (-1.555)	4.961** (1.974)	7.961*** (3.023)
Constant	0.186*** (9.414)	0.016 -1.284	-0.046*** (-3.636)
No of OBS	175,985	166,796	250,869
R-squared	0.031	0.002	0.007

This table reports the market reaction on analysts' forecast revisions and other characteristics. The dependent variable is *CAR5*, [-2, 2] size decile adjusted cumulative percentage of return. *Fcrev* is defined as the analyst's forecast value minus the previous latest forecast value by the same analyst to the same firm, and divided by the price on the 10th trading day before the release of the revised forecast. Similar to Bonner, Hugon and Walther (2007), we also control the interaction item of *Fcrev* with other analyst characteristics. The other characteristics include *GenExperience*, general experience, number of the years the analyst has issued forecasts, *FirmExperience*, firm experience, the number of years the analyst has followed the stock, *Companies*, the number of firms followed, *Industries*, the number of industries followed, *ForHorizon*, forecast horizon, the distance between forecast date and fiscal year end, *Ntop10* is set to 1 if the analyst is employed in the top decile of broker size, and 0 otherwise. We run regression in three periods 1985-1995, pre Reg FD, and post Reg FD.

Impact of Interlocking Relationships on Forecast Accuracy, and Market Reaction to Analysts' Forecast

Abstract

This paper examines the impact of one important corporate governance characteristic, interlocks, on analyst forecast accuracy and market reaction to analyst forecast revisions. I document a negative association between interlocking relationships among the firms followed by analysts and those analysts' forecast accuracy after controlling for other previously documented determinants of forecast accuracy. That negative association has significantly weakened after the passage of the Sarbanes-Oxley Act of 2002 (SOX). Furthermore, I document that the market does understand the negative impact of interlocks on analysts' forecast accuracy before the passage of SOX. Controlling for other factors, the stock market responds less to forecast revisions issued by those analysts who follow a higher number of interlocked firms. However, the market does not discount the forecast revisions issued by analysts following a higher number of interlocked firms after the passage of SOX.

Impact of Interlocking Relationships on Forecast Accuracy, and Market Reaction to Analysts' Forecast

1. Introduction

This paper examines the impact of one important corporate governance characteristic, interlocks, on analysts' forecast accuracy and market reaction to analysts' forecast revisions. Whether systematic differences in financial analysts' accuracy exist has been an on-going research question. Though earlier studies failed to document such systematic differences, (see Richards 1976; Brown and Rozeff 1980; O'Brien 1990; Coggin and Hunter 1989; and Butler and Lang 1991), starting in the early 1990s, researchers began to document the heterogeneity of analysts' forecast accuracy (e.g., Stickel 1992; Sinha, Brown and Das 1997; Clement 1999; Mikhail, Walther and Willis 1997; and Jacob, Lys and Neale 1999) using alternative methodologies, different time frames, or independent data.

In the past, it has been shown that various factors have influenced forecast accuracy. For example, Clement (1999) illustrated that forecast accuracy is positively associated with the analyst's ability (e.g., analysts' firm experience) and the resources available to the analyst (e.g., employer size), and is negatively associated with task complexity (e.g., number of firms and industries followed by the analyst). Since one of the key determining factors of the quality of corporate governance is interlocking relationships, this paper investigates the marginal effect of interlocks on analysts' forecast accuracy, after controlling for other documented determinants. Furthermore, this paper examines whether the market understands such an impact by looking at the market reactions to revisions made by those analysts who follow a greater number of interlocked firms.

Interlocking relationships among firms have gained considerable interests in sociology and business research.⁹ In this age of globalization, it has become brutally clear that the world is interconnected through a complex web of transnational networks, resulting in increasingly intense competition. Such competition is no longer a single-firm to single-firm game; it is a supply-chain to supply-chain competition, a network-to-network competition. As one of the many ways through which two firms can closely tie to each other, interlocking directorates have gained increasing prominence, and at the same time, increasing criticism. Two firms are connected by an interlock when a person affiliated with one firm sits on the board of another firm (Mizruchi, 1996). Prominent academic studies have examined why interlocks arise and how they impact firms' profitability, corporate governance, and CEO compensation. Even the Securities and Exchange Commission (SEC) has recognized the complex issues arising from interlocks among firms. Starting from 1992, the SEC requires director interlocks to be disclosed where executive officers serve as directors on each other's compensation committees.¹⁰

Until now, within the academic world, there was no consistent view as to why firms form interlocking connections. In his survey work, Mizruchi (1996) classified the reasons for interlock relationships into the following five categories: collusion, cooptation and monitoring, legitimacy, career advancement, and social cohesion.

1. Collusion theory suggests that interlocks may be created to facilitate collusion among firms. For example, firms can use price-fixing strategy to decrease competition by forming interlocks.

⁹ In the accounting and finance literature, researchers have examined impact of interlocking relationship on executive compensation (see Hallock 1997; and Graham and Wu 2007). However, ours is the first study to examine the impact of interlocking relationship on analyst forecast.

¹⁰ US Proxy Manual, Institutional Shareholder Services 2004.

2. Cooptation and monitoring explain interlocks based on the resource dependency theory, whereby firms try to co-opt sources of environmental uncertainty.
3. Signaling theory argues that facing potential investors, firms want to signal their legitimacy and good management by trying to link to other important organizations.
4. Career advancement theory views interlocks from the individual's perspective. It is the individual corresponding to the interlocks that drives the formation. Individuals want to earn financial remuneration, prestige, and contacts that may be useful for their careers.
5. The last theory, social cohesion, posits that interlocks represent social ties among members of the upper class. It contends that the majority of interlocks reflects intra-class social ties, rather than inter-organizational resource dependence or control ties.

In this paper, we hypothesize that the interlocking relationship negatively affects analysts' forecast accuracy. First, when executives are sitting on each other's boards, it indicates a bad corporate governance mechanism. For example, Pfeffer and Salancik (1978) claim that interlocks are devices for inter-corporate collusion or cooptation, and Useem (1984) argues that interlocks facilitate the aggregation and advancement of the collective interests of the corporate elite. Second, existing accounting research has proposed that the quality of financial information available to financial analysts is positively correlated with the quality of corporate governance. Byard, Li, and Weintrop (2006) examine the association between corporate governance and the quality of information available to financial analysts. They document that the quality of financial analysts' information about upcoming earnings increases with the quality of corporate governance. Combining these two relations, we conjecture that interlocks are negatively associated with analysts forecast accuracy.

As expected, we find a negative association between interlocking relationships among the firms followed by analysts and those analysts' forecast accuracy after controlling for other previously documented determinants of forecast accuracy. Furthermore, the negative association has been significantly reduced with the passage of the Sarbanes-Oxley Act in 2002 (SOX). SOX introduced new standards of accountability on the boards of U.S. companies or companies listed on U.S. stock exchanges. Under SOX, directors risk large fines and prison sentences in the case of accounting crimes. Internal control is now the direct responsibility of directors. The vast majority of companies covered by SOX have hired internal auditors to ensure that the company adheres to the highest standards of internal control. The internal auditors are required by law to report directly to an audit board; which consists of directors, more than half of whom are outside directors, and one of whom is an accounting expert. In general, SOX should enhance corporate governance, and particularly enhance the corporate governance relating to boards of directors. For example, after SOX, a higher portion of directors will be composed of independent, outside directors, reducing the negative corporate governance effect made by interlocking relationships.

In addition, we document that the market does understand the negative impact of interlocks on analysts' forecast accuracy before the passage of SOX. Controlling for other factors, the stock market responds less to forecast revisions issued by those analysts who follow a greater number of interlocked firms. However, after the passage of SOX, the stock market does not discount forecast revisions issued by analysts who follow a greater number of interlocked firms.¹¹

Our study contributes to both analyst forecast literature and to interlock literature. First, we document interlocks as one additional determinant of analysts' forecast accuracy after

¹¹ This finding is consistent with a recent paper, Qi (2010), which documents that that interlock connections decrease firms' values pre-Sarbanes-Oxley Act (SOX) and increase post-SOX.

controlling for other factors. Interlocking relationships among firms followed by analysts can be viewed as one additional component of portfolio complexity. Prior literature documents that the number of firms and the number of industries analysts follow matter. We enrich the literature by showing that interlocking relationships matter as well. In addition, we show that the market in general understands the impact of interlocking relationships on forecast accuracy.

Second, this paper contributes to the literature of interlocks by demonstrating the negative effect of interlocks on analysts' forecast accuracy, and the damage caused by interlocks on the quality of information available to analysts. This evidence further supports the view that interlocks lead to poor quality of corporate governance, and it may be of interest to policy-makers who intend to improve that quality.

The rest of the paper is organized as follows: section 2 reviews the literature and develops our hypotheses; section 3 discusses the data and our empirical models; section 4 presents our empirical analysis; and section 5 concludes the paper with managerial implications and potential future research.

2. Literature review and hypothesis development

As one important information intermediary, financial analysts have received considerable attention in finance and accounting research. Identifying determinants of analysts' forecast accuracy has been studied for a long time. In the 1970s and 1980s, the literature identified few determinants (i.e., there is no cross-sectional and time-series significant differences in forecast accuracy; see Richards 1976; Brown and Rozeff 1980; O'Brien 1987; Coggin and Hunter 1989; and Butler and Lang 1991). However, new evidence emerged from the 1990s and suggests that there are a number of important determinants of analysts' forecast accuracy. For example, Stickel (1992) documents that All-American analysts selected by institutional investors make

more accurate and more frequent forecasts, and the capital markets pay more attention to their upward forecast revisions.

Sinha, Brown and Das (1997) find that forecast errors are serially correlated and suggest that systematic ex-post differences exist in analysts' forecast accuracy. In addition, they document that the superiority of the analysts can carry over to the next time period, but the inferiority won't continue. Mikhail, Walther and Willis (1997) investigate factors that contribute to analysts' forecast accuracy using a time series approach, and concludes that forecasting experience affects the analysts' forecast accuracy.

Our paper builds on Clement (1999) and Clement and Tse (2005) by cross-sectionally examining the impact on analysts' forecast accuracy from one additional factor, interlocking relationships among firms followed by analysts. Clement (1999) finds that analyst forecast accuracy is positively associated with the analyst's experience (a surrogate for analyst ability and skill) and employer size (a surrogate for resources available), and negatively associated with the number of firms and industries followed by the analyst (surrogates of portfolio complexity). In our paper, we propose that interlock serves as one additional measure of an analyst's task complexity; therefore, interlocking relationship may affect the analyst's forecast accuracy.

As stated in Davis (1996), one of the most striking features of corporate boards is the extent of multiple board memberships. Such shared directorates are termed "interlocked." We hypothesize that interlocking relationships existing in the firms followed by analysts is negatively correlated with analysts' forecast accuracy. First, interlocking relationships can lead to low quality in corporate governance. Agency theory suggests that disciplinary strengthening of directors can reduce the residual loss and thus improve the creation of shareholder value, leading to high quality in corporate governance. Following this, directorship interlocks reduce

the likelihood of the proper monitoring role of the board of directors since they are interlocked with each other and potentially can collude among themselves. Such interlocks diminish the efficiency of external control mechanisms, leading, for example, to lower takeover activities or excessive director compensation.

Second, existing accounting research has suggested that the quality of financial information available to financial analysts is positively correlated with the quality of corporate governance. For example, Byard, Li, and Weintrop (2006) examine the association between corporate governance and the quality of information available to financial analysts. They document that the quality of financial analysts' information about upcoming earnings increases with the quality of corporate governance. Hence, if a large portion of the firms followed by an analyst is composed of interlocked firms, the information quality received by that analyst might be lower because of the collusion of executives across companies and the decreased quality of corporate governance. Hence, we have the following hypothesis:

Hypothesis 1: Holding other determinants of analysts' forecast accuracy constant, forecast accuracy decreases with the number of interlocked firms followed by analysts.

However, the negative association between interlocks and analysts' forecast accuracy might be reduced following the passage of the Sarbanes-Oxley Act of 2002 (SOX). SOX was enacted on July 30, 2002 in reaction to a number of major corporate and accounting scandals. The goal of this act was to improve the integrity of financial reporting by imposing stringent requirements on corporate executives, increase the attention paid to the quality of reported earnings and the responsibility of corporate executives for these earnings. Under SOX, chief executive officers (CEO) and chief financial officers (CFO) have the primary responsibility for the integrity of financial reports. In addition, SOX calls for more independent board directors.

For example, SOX requires that more than 50% of members in the auditing committee be composed of independent board members. Such a requirement can improve the internal control through better corporate governance and reduce the negative impact of interlocks. Hence, we have the following hypothesis:

Hypothesis 2: Holding other determinants of analysts' forecast accuracy constant, the negative impact of interlocks on forecast accuracy is mitigated by the passage of SOX.

One caveat we would like to point out is that the above hypothesis is in fact an empirical question. Since arguments can be made that interlocks among firms followed by analysts can provide richer information set to financial analysts; therefore, the analysts' forecast accuracy can be improved. The social capital of directors can be shared by firms within the interlocking relationship. Indeed, as busy directors often are, or were in the past, members of a management board, they dispense a wealth of information, experiences, and knowledge about how to guide a company. Moreover, multiple board appointments offer insights in different economic sectors and enable directors to stimulate strategic discussion in board meetings. Thus, the consideration of a director's social capital may help financial analysts gain further insights into forecast models. In fact, decisions made at the board of one company can, in turn, become part of the raw material for decisions at other boards (Davis 1996). In so doing, the final decisions or strategies of interlocked firms might be more homogeneous. Hence, financial analysts may be able to predict the behaviors of the firms with interlocks in a more accurate fashion. This tension makes our research question interesting and relevant.

Having studied the relationship between an analyst's forecast accuracy and the number of interlocked firms that the analyst follows, the next natural question to ask is whether the stock market understands such an impact. Previous research documented that an analyst's level of

media coverage can affect the initial market reaction to his or her forecast revisions (Bonner et al. 2007). There are two potential reasons for such a phenomenon. One is that investors' familiarity with the analyst's name affects the market's reaction, and the other is that analysts with high media coverage are associated with high forecast accuracy. Hence, the market reacts more strongly to forecast revisions issued by such analysts.

In this paper, we argue that the market might understand the negative association between interlocks and analysts' forecast accuracy and respond to a lesser degree to forecast revisions issued by those analysts who follow a greater number of interlocked firms than to those analysts following a smaller number of interlocked firms. However, with the passage of SOX, the market might appreciate the overall improvement of corporate governance. Therefore, the discount effect mentioned above may decrease after the passage of SOX. We have the following two hypotheses:

Hypothesis 3: The market reacts less to forecast revisions issued by analysts who follow a greater number of interlocked firms.

Hypothesis 4: The discounting effect in Hypothesis 3 is reduced by the passage of SOX.

3. Data and Empirical Models

We collected our analysts' forecasts data from I/B/E/S database. Our analysis is based on I/B/E/S forecasts of annual earnings from 1996 to 2007. Following prior studies, we retain the last forecast an analyst issued in a particular year (O'Brien 1990; Sinha, Brown, and Das 1997; Clement 1999; Clement and Tse 2005). In addition, we include only those forecasts issued no earlier than 1 year ahead, and no later than 30 days before the fiscal year-end. To control for fixed firm effect, we compare analysts' forecast accuracy to other analysts following the same firm. Because of this procedure, we eliminate those firm-years for which only one analyst provides a forecast. This is consistent with the approach taken by Clement and Tse (2005). We

also eliminate all analysts' forecasts with no prior year data on forecast accuracy. To facilitate comparisons across companies and industries, following Clement and Tse (2005), we deflate forecast revisions and forecast errors by the firm's security price two days before the forecast revision date. We obtain security prices from CRSP and eliminate potential outliers by omitting observations for which the price-deflated analyst's forecast error is above 0.40 or below -0.40 , following Clement and Tse (2005). We also eliminate observations which have missing values for other control variables.

To identify the number of interlocked firms an analyst follows, we merge our analyst forecast dataset with the IRRC database. The IRRC database contains board member information through which we construct our interlock variable. For each year, two firms are termed interlocked if there is at least one shared board member between them according to the IRRC database. Out of all the firms an analyst followed, we identified how many pairs of firms shared at least one board member and termed that as the total number of interlocked firms followed by that analyst.¹² In total we have 81,411 analyst-firm-year observations.

3.1 Impact of Interlock on Forecast Accuracy

In this section, we examine whether forecast accuracy is lower for those forecasts issued by analysts following a greater number of interlocked firms than forecasts issued by analysts following a smaller number of interlocked -firms.

Accuracy Regression

Model 1

¹² Even though interlocking is a firm-level phenomenon, we argue that analysts have the choices to decide what kinds of firms they want to follow, including whether firms have the interlocking relationships. For example, they can decide to follow a group of firms with a lot of interlocking relationships or choose to follow those without interlocking relationships. However, untabulated results show that it is more likely for the analysts with more number of years of general experience to follow a greater number of interlock firms. This finding rules out the argument that interlock is a proxy for low analyst capability; hence, if there is a selection bias, it biases against our findings.

$$\begin{aligned}
Accuracy_{ijt} = & \beta_0 + \beta_1 Combif_{ijt} + \beta_2 Companies_{ijt} + \beta_3 LagAccuracy_{ijt} + \beta_4 BrokerSize_{ijt} \\
& + \beta_5 ForFrequency_{ijt} + \beta_6 FirmExperience_{ijt} + \beta_7 GenExperience_{ijt} + \beta_8 Industries_{ijt} \\
& + \beta_9 DaysElapsed_{ijt} + \beta_{10} ForHorizon_{ijt} + \beta_{11} Boldness_{ijt} + \varepsilon_{ijt}
\end{aligned}$$

Because the number of interlocked firms and the number of firms followed by one analyst might be highly correlated (we have shown that it is the case in Table 3, since the correlations are between 0.3 to 0.4), and to reduce the potential multi-collinearity, as well as to capture the interlock complexity of the firms covered by one analyst, we define one new variable called *Ratio*, which is equal to the number of interlocked firms divided by the total number of firms followed by an analyst, and we re-estimate our forecast regressions (Model 2)

Model 2:

$$\begin{aligned}
Accuracy_{ijt} = & \beta_0 + \beta_1 Ratio_{ijt} + \beta_2 Companies_{ijt} + \beta_3 LagAccuracy_{ijt} + \beta_4 BrokerSize_{ijt} \\
& + \beta_5 ForFrequency_{ijt} + \beta_6 FirmExperience_{ijt} + \beta_7 GenExperience_{ijt} + \beta_8 Industries_{ijt} \\
& + \beta_9 DaysElapsed_{ijt} + \beta_{10} ForHorizon_{ijt} + \beta_{11} Boldness_{ijt} + \varepsilon_{ijt}
\end{aligned}$$

Following Clement and Tse (2005), to allow comparisons of regression model coefficients, we scale each of the variables to a value between 0 to 1 using a transformation that preserves the relative distances among each characteristic's measures for firm j in year t. The scaled independent variables for analyst i take the form

$$Characteristic_{ijt} = \frac{RawCharacteristic_{ijt} - RawCharceristic\ min_{ijt}}{RawCharacteristic\ max_{jt} - RawCharacteristic\ min_{jt}}$$

where *Raw Characteristic max_{ijt}* and *Raw Characteristic min_{ijt}* are the original maximum and minimum values, respectively, of a characteristic at time t for firm j. A high value for *Characteristic_{ijt}* indicates that analyst i scores high on that characteristic relative to other analysts who follow firm j in year t. To ensure that the measures of forecasting performance

increase with forecast accuracy, we scale the forecast accuracy measure to be 0 for the least accurate forecast (highest absolute forecast error) and 1 for the most accurate forecast (lowest absolute forecast error). The scaled accuracy measure for analyst i is

$$\text{Accuracy}_{ijt} = \frac{\text{AFE}_{\max_{ijt}} - \text{AFE}_{ijt}}{\text{AFE}_{\max_{jt}} - \text{AFE}_{\min_{jt}}}$$

Other variable definitions are as follows:

Combif_{ijt} is a measure of analyst i 's forecast interlocked firm selection, calculated as the number of interlocked firms followed by analyst i following firm j in year t minus the minimum number of interlocked firms followed by analysts who follow firm j in year t , with this difference scaled by the range of number of interlocked firms on total number of forecast companies for analysts following firm j in year t .

Company_{ijt} is a measure of the number of companies analyst i follows in year t , calculated as the number of firms followed by analyst i following firm j in year t minus the minimum number of firms followed by analysts who follow firm j in year t , with this difference scaled by the range in the number of companies followed by analysts following firm j in year t .

Ratio_{ijt} is a measure of analyst i 's forecast interlocked firm selection, calculated as the ratio of the number of interlocked firms on the total number of companies followed by analyst i following firm j in year t minus the minimum ratio of the number of interlocked firms on the total number of companies followed by analysts who follow firm j in year t , with this difference scaled by the range in the ratio of the number of interlocked firms on total number of companies followed by analysts following firm j in year t .

We will use an example below to explain how we estimate the ratio of the number of interlocked firms on total number of companies followed by an analyst.

Assume firm A and B share at least one board member, and Firm C and D share at least one board member. If one analyst only covers A and B, then the number of interlocked firms followed by that analyst is 2 (firm A and firm B), and the interlock ratio of that analyst for both firm A and B is $2/2=1$. If the analyst covers A, B, E and C, then the number of interlocked firms followed by that analyst for firm A, B, E and C are 3, then the interlock ratio of that analyst for firm A, B, E, and C are $3/4$.

Accuracy_{ijt} is a measure of analyst i's forecast accuracy for firm j in year t, calculated as the maximum absolute forecast error for analysts who follow firm j in year t minus the absolute forecast error of analyst i following firm j in year t, with this difference scaled by the range of absolute forecast errors for all analysts following firm j in year t.

BrokerSize_{ijt} is a measure of the analyst's brokerage size, calculated as the number of analysts employed by the brokerage employing analyst i following firm j in year t minus the minimum number of analysts employed by brokerages for analysts following firm j in year t, with this difference scaled by the range of brokerage size for analysts following firm j in year t.

Companies_{ijt} is a measure of the number of companies analyst i follows in year t, calculated as the number of companies followed by analyst i following firm j in year t minus the minimum number of companies followed by analysts who follow firm j in year t, with this difference scaled by the range in the number of companies followed by analysts following firm j in year t.

GenExperience_{ijt} is a measure of analyst i's general experience, calculated as the number of years of experience for analyst i in year t minus the minimum number of years of experience for analysts following firm j in year t, with this difference scaled by the range of years of experience for analysts following firm j in year t.

FirmExperience_{ijt} is a measure of analyst i's firm-specific experience, calculated as the number of years of firm-specific experience for analyst i following firm j in year t minus the minimum number of years of firm-specific experience for analysts following firm j in year t, with this difference scaled by the range of years of firm-specific experience for analysts following firm j in year t.

ForFrequency_{ijt} is a measure of analyst i's forecast frequency for firm j, calculated as the number of firm-j forecasts made by analyst i following firm j in year t minus the minimum number of firm-j forecasts for analysts following firm j in year t, with this difference scaled by the range in the number of firm-j forecasts issued by analysts following firm j in year t.

ForHorizon_{ijt} is a measure of the time from the forecast date to the end of the fiscal period, calculated as the forecast horizon (days from the forecast date to the fiscal year-end) for analyst i following firm j in year t minus the minimum forecast horizon for analysts who follow firm j in year t, with this difference scaled by the range of forecast horizons for analysts following firm j in year t.

Industries_{ijt} is a measure of the number of industries analyst i follows in year t, calculated as the number of two-digit SICs followed by analyst i following firm j in year t minus the minimum number of two-digit SICs followed by analysts who follow firm j in year t, with this difference scaled by the range in the number of two-digit SICs followed by analysts following firm j in year t.

DaysElapsed_{ijt} is a measure of the days elapsed since the last forecast by any analyst following firm j in year t, calculated as the days between analyst i's forecast of firm j's earnings in year t and the most recent preceding forecast of firm j's earnings by any analyst, minus the minimum number of days between two adjacent forecasts of firm j's earnings by any two

analysts in year t , with this difference scaled by the range of days between two adjacent forecasts of firm j 's earnings in year t .

Boldness_{ijt} is an indicator variable for the boldness of analyst i 's forecast for firm j in year t . It is equal to 1 if analyst i 's forecast is above both the analyst's prior forecast and the mean forecast immediately before revision, or else below both. It is set to 0 otherwise. The mean forecast is based on forecasts issued in the 90 days prior to analyst i 's forecast revision.

3.2 Impact of Interlock on Forecast Accuracy after SOX

To study the mitigating effect of SOX on the relationship between interlocking relationships and forecast accuracy, we modify our model 1 by including both the *SOX* dummy variable, as well as the interaction between the *SOX* and *Combif* variables (model 3). For robustness check, we also modify our model 2 by including both the *SOX*, as well as the interaction between the *SOX* dummy and *Ratio* (Model 4). *SOX* equals 1 for the years after 2002, and 0 otherwise. Hence, the coefficient of *SOX* captures the impact of SOX on forecast accuracy, while the coefficient of the interaction term *SOX_Combif* (*SOX_Ratio*) captures the incremental impact of *Combif* (*Ratio*) on the forecast accuracy after the passage of SOX.

Model 3

$$\begin{aligned} Accuracy_{ijt} = & \beta_0 + \beta_1 Combif_{ijt} + \beta_2 Combif_{ijt} * SOX + \beta_3 SOX + \beta_4 Companies_{ijt} + \beta_5 LagAccuracy_{ijt} \\ & + \beta_6 BrokerSize_{ijt} + \beta_7 ForFrequency_{ijt} + \beta_8 FirmExperience_{ijt} + \beta_9 GenExperience_{ijt} \\ & + \beta_{10} Industries_{ijt} + \beta_{11} DaysElapsed_{ijt} + \beta_{12} ForHorizon_{ijt} + \beta_{13} Boldness_{ijt} + \varepsilon_{ijt} \end{aligned}$$

Model 4:

$$\begin{aligned} Accuracy_{ijt} = & \beta_0 + \beta_1 Ratio_{ijt} + \beta_2 Ratio_{ijt} * SOX + \beta_3 SOX + \beta_4 Companies_{ijt} + \beta_5 LagAccuracy_{ijt} \\ & + \beta_6 BrokerSize_{ijt} + \beta_7 ForFrequency_{ijt} + \beta_8 FirmExperience_{ijt} + \beta_9 GenExperience_{ijt} \\ & + \beta_{10} Industries_{ijt} + \beta_{11} DaysElapsed_{ijt} + \beta_{12} ForHorizon_{ijt} + \beta_{13} Boldness_{ijt} + \varepsilon_{ijt} \end{aligned}$$

3.3 Market Reaction Tests

We study whether the market understands the impact of interlock on analysts' forecast accuracy. Following Bonner et al (2007), we use the raw value of characteristics, not ranging them to between 0 and 1. Also, following Bonner et al (2007), we do not include the main effect of our control variables.

Model 5

$$\begin{aligned} CAR5_{ijt} = & \beta_0 + \beta_1 Fcrev_{ijt} * Combif_{ijt} + \beta_2 Fcrev_{ijt} * Combif_{ijt} * SOX + \beta_1 Fcrev_{ijt} * SOX + \beta_4 Fcrev_{ijt} * LagAccuracy_{ijt} \\ & + \beta_5 Fcrev_{ijt} * BrokerSize_{ijt} + \beta_6 Fcrev_{ijt} * ForFrequency_{ijt} + \beta_7 Fcrev_{ijt} * FirmExperience_{ijt} \\ & + \beta_8 Fcrev_{ijt} * GenExperience_{ijt} + \beta_9 Fcrev_{ijt} * Companies_{ijt} + \beta_{10} Fcrev_{ijt} * Industries_{ijt} \\ & + \beta_{11} Fcrev_{ijt} * DaysElapsed_{ijt} + \beta_{12} Fcrev_{ijt} * ForHorizon_{ijt} + \beta_{13} Fcrev_{ijt} + \varepsilon_{ijt} \end{aligned}$$

Model 6

$$\begin{aligned} CAR5_{ijt} = & \beta_0 + \beta_1 Fcrev_{ijt} * Ratio_{ijt} + \beta_2 Fcrev_{ijt} * Ratio_{ijt} * SOX + \beta_1 Fcrev_{ijt} * SOX + \beta_4 Fcrev_{ijt} * LagAccuracy_{ijt} \\ & + \beta_5 Fcrev_{ijt} * BrokerSize_{ijt} + \beta_6 Fcrev_{ijt} * ForFrequency_{ijt} + \beta_7 Fcrev_{ijt} * FirmExperience_{ijt} \\ & + \beta_8 Fcrev_{ijt} * GenExperience_{ijt} + \beta_9 Fcrev_{ijt} * Companies_{ijt} + \beta_{10} Fcrev_{ijt} * Industries_{ijt} \\ & + \beta_{11} Fcrev_{ijt} * DaysElapsed_{ijt} + \beta_{12} Fcrev_{ijt} * ForHorizon_{ijt} + \beta_{13} Fcrev_{ijt} + \varepsilon_{ijt} \end{aligned}$$

We use the analyst's prior forecast to calculate the forecast revision, since it is more informative to the market than the consensus forecast (Gleason and Lee 2003; Stickel 1991; Imhoff and Lobo 1984). Therefore, ***Fcrev_{ijt}*** is the analysts' forecast value minus the previous value by the same analyst for the same firm, and divided by the stock price 10 trading days before the release of the revised forecast (Bonner et al 2007). The [-2,2] cumulative abnormal return (***CAR5_{ijt}***) around the analysts' forecast revision date (date 0) is defined as the firm's compounded return minus the compounded return of the size decile portfolio to which the firm belonged at the beginning of the year.

4. Empirical Results and Discussion

4.1. Descriptive Statistics and Correlation Analysis

Table 1 reports the descriptive statistics of all the variables used in this study. On average, in our sample periods, one analyst covered 16.8 firms from 6.39 industries. Also, out of 16.8 firms, on average there were 1.85 interlocks. Generally, analysts had 8.5 years of general experience and 5.1 years of firm experience.

Table 2 tabulates the Pearson and Spearman correlations between the main variables. The correlation analysis offers some insights into the relationships among our variables of interest. Both *Combif* and *Ratio* are negatively and significantly correlated with the forecast accuracy (Pearson correlation = -0.028; p-value ≤ 0.0001 and Pearson correlation = -0.022; p-value ≤ 0.0001 respectively); this is along expected lines. *Companies*, which captures the number firms an analyst follows, is weakly positively correlated with the forecast accuracy (Pearson correlation = 0.005; p-value = 0.120). Analysts' general experience and firm experience are all positively associated with the forecast accuracy (Pearson correlation = 0.009 and p-value = 0.011 for firm experiences; Pearson correlation = 0.007 and p-value = 0.043 for general experiences). Overall, the result of the correlation analysis supports the hypothesis that interlock is negatively associated with forecast accuracy.

[Insert Table 1 and Table 2 around here]

4.2. Impact of Interlock on Forecast Accuracy – Test of H1

Table 3 presents the results of forecast accuracy. The estimated coefficients on *Combif* is -0.021 (t-statistics = -5.949), supporting our first hypothesis that there is a negative association

between the number of interlocked firms an analyst follows and the accuracy of his or her forecast. The coefficient of the number of industries an analyst follows is negative and significant indicating that task complexity decreases forecast accuracy (e.g., Clement 1999). The coefficient on the general firm experience variable (*GenExperience*) is positive and significant (estimated coefficient = 0.0080; t-statistics = 2.441), again consistent with results from previous research (e.g., Clement 1999) that forecast accuracy is positively associated with analysts' experience.

[Insert Table 3 around here]

As we stated before, *Combif* and *Companies* are highly correlated. For robustness check, we rerun our analysis by replacing the number of interlocked firms (*Combif*) with the ratio of interlock (*Ratio*).

The estimated coefficient on *Ratio* is still negative and significant (estimated coefficient = -0.014; t-statistic = -4.356). The coefficient on the number of companies followed by an analyst is positive and significant (estimated coefficient = 0.011; t-statistics = 2.515). We also find a positive and statistically significant coefficient for the analyst's experience variable (estimated coefficient = 0.0070; t-statistics = 1.878 for *GenExperience*) and a positive and statistically significant coefficient for the analyst's resources variable (estimated coefficient = 0.0008; t-statistics = 2.430 for *BrokerSize*). Overall, the results show that the number of interlocked firms an analyst follows is negatively associated with his/her forecast accuracy, after controlling for the analyst's task complexity, resources available, analyst experience, and other potential determinants of forecast accuracy.

There is another concern that the number or ratio of interlocked firms does not proxy for task complexity, but proxy the information environment at the firm level. Therefore, we use a subsample which deletes all the interlocked firms and only keep the firms without interlocked board member. If the interlocked relationship can only affect at the firm level, then we cannot see the existence of negative relationship between number of interlocked firms and analyst forecast accuracy in this subsample. However, we find the estimated coefficient on *Combif* is still negative and significant (estimated coefficient = -0.020; t-statistic = -5.115), and the estimated coefficient on *Ratio* is still negative and significant (estimated coefficient = -0.013; t-statistic = -3.695).

[Insert Table 4 around here]

4.3. Mediation of SOX on the Relationship between Interlocks and Forecast Accuracy – Test of H2

Table 5 presents the results of the impact of SOX on the relationship between interlocks and forecast accuracy. The coefficient of the *Combif* variable captures the relationship between interlock and forecast accuracy before the passage of SOX, which is still significantly negative (estimated coefficient = -0.030; t-statistics = -6.981). The interaction between *Combif* and the *SOX* dummy captures the incremental impact of interlock on forecast accuracy after the passage of SOX. Here *SOX* equals 1 for those firm-years later than 2002, representing the period after the passage of Sarbanes-Oxley. The coefficient on the interaction term is positively significant, indicating that the negative impact of interlocks has been significantly reduced after the SOX.¹³ Our results show that after the passage of SOX, the negative impact of interlock on forecast

¹³ Even though $-0.030 + 0.024 = -0.006$ is not significant different from zero (F-test=1.35), Untabulated sub-period analysis shows that the coefficient on interlocks is still significantly negative after SOX.

accuracy has been mitigated, due to the enhanced monitoring role of the increased number of independent directors. The evidence validates our second hypothesis. For robustness check, we also replace the number of interlocked firms with the ratio of interlocks, repeat our analysis. Qualitatively, our results do not change.

[Insert Table 5 around here]

4.4. Market Response Tests – Tests of H3 and H4

Our results indicate that there is a negative relationship between interlock and forecast accuracy and such a negative association disappears after the passage of SOX. A natural follow-up question is whether stock market understands such a relationship and how investors respond to it.

Table 6 column 1 shows how the market responds to forecast revisions issued by analysts with various backgrounds, experience, and capabilities. Consistent with previous research, the estimated coefficient on *Fcrev* is positive and statistically significant (estimated coefficient = 0.254; t-statistics = 6.410). Our main focus is on the two interaction terms: *Fcrev*Combif* and *Fcrev*Combif*SOX*. We find that the coefficient on *Fcrev*Combif* is significantly negative (estimated coefficient = -0.021; t-statistics = -3.785), indicating that before the passage of SOX, the market does understand the negative impact of interlock on forecasts. Hence, market participants react less to forecast revisions issued by analysts following more interlocked firms. The interaction among *Fcrev *Combif*SOX* captures the incremental impact of interlock on the relationship between market response and forecast revision after the passage of the Sarbanes-Oxley Act. As we can see, the coefficient for such an interaction is positive and significant (estimated coefficient = 0.073; t-statistics = 6.221). Since $-0.021 + 0.073 = 0.052$ is positive and also

significant ($F\text{-stat}=58.63$)¹⁴, it indicates that, after the passage of SOX, market participants stop discounting forecast revisions issued by analysts following a greater number of interlocked firms. Therefore, our H3 and H4 are also supported. For robustness check, following Gleason and Lee (2003), we reran our analysis by keeping only those revisions, without other analyst revisions for the same firm within [-5,5] days window of the original forecast revision date (Table 6 column 2). Qualitatively, our results do not change.

[Insert Table 6 around here]

In addition, we replace the number of interlocked firms with the ratio of interlocks and re-estimate our market response model (Table 7 column 1). The estimated coefficient on $Frev*Ratio$ is negative but insignificant (estimated coefficient=-0.160; t-statistics=-1.493), while the estimated coefficient on $Frev*Ratio*SOX$ is positive and significant (estimated coefficient=1.701; t-statistics=6.896). Overall, the findings are consistent with H3 and H4. The reduced sample based on Gleason and Lee's (2003) method to exclude herding revision observations results in an even stronger result (Table 7 column 2).

[Insert Table 7 around here]

5. Conclusion

In this paper, we examine whether the number of interlocked firms that analysts follow impacts forecast accuracy. In addition, we study whether the market understands the existence of such a relationship. We find that overall interlock decreases the analysts' forecast accuracy, and such a negative impact has been mitigated after the passage of SOX. Also we find that, before the passage of SOX, the market responds less to the forecast revisions issued by analysts

¹⁴ Untabulated sub-period analysis shows that the coefficient on interlocks is still significantly positive after SOX.

following a greater number of interlocked firms, which is consistent with the notion that the market sees through the negative impact of interlock on forecast accuracy. Furthermore, our results show that the market does not discount forecast revisions issued by analysts who follow a greater number of interlock firms after the passage of SOX.

Our study contributes to the analyst forecast literature by adding one more cross-sectional determinant of analysts' forecast accuracy – the number of interlocks followed by analysts, and it also contributes to interlock literature by highlighting the negative effect of poor corporate governance related to interlocking relationship – low quality of information available to financial analysts and lower forecast accuracy. Our results imply that the relationships among the firms analysts follow matter in terms of influencing their forecast accuracy.

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Table 1 Summary Statistics

This table shows the simple summary statistics. Our main variables are *Combif*, the number of interlocked firms followed by the analyst in each year; *Ratio*, the ratio of the number of interlocked firms on total firms followed by the analyst in each year. Other analyst characteristics include *AFE*, the absolute value of forecast error scaled by the previous two trading days' price; *Boldness*, a dummy variable (*Boldness* = 1) if they are above both the analyst's prior forecast and the consensus, or else below both; *DaysElapsed*, the number of days since any analyst's prior forecast; *ForHorizon*, the number of days from the forecast date to the fiscal year-end; *ForFrequency*, the number of forecasts issued by the analyst in each year; *BrokerSize*, the number of analysts in the analyst's brokerage in each year; *FirmExperience*, the analyst's years of experience forecasting a particular firm's earnings; *GenExperience*, the analyst's overall years of forecasting experience; *Companies*, the number of companies the analyst follows in each year; and *Industries*, the number of two-digit SIC industries the analyst follows in each year. N = 81,411.

Panel A. Distribution of Raw (Unscaled) Forecast and Analyst Characteristics

Variable	Mean	Std Dev	75%	Median	25%
Combif	1.850	3.576	2	0	0
Ratio	0.093	0.1330	0.158	0	0
AFE	0.006	0.0200	0.004	0.001	0
Boldness	0.772	0.419	1	1	1
Companies	16.874	10.491	20	15	11
ForFrequency	23.870	27.489	25	16	9
FirmExperience	5.121	3.571	7	4	2
GenExperience	8.545	5.073	12	7	4
BrokerSize	70.091	57.756	107	55	23
DaysElapsed	9.121	13.921	11	3	1
ForHorizon	95.707	59.388	109	71	62
Industries	6.393	3.683	8	6	4

Panel B. Distribution of Scaled Forecast and Analyst Characteristics

Variable	Mean	Std Dev	75%	Median	25%
Combif	0.234	0.353	0.400	0.000	0.000
Ratio	0.265	0.358	0.519	0.000	0.000
Accuracy	0.703	0.317	0.959	0.827	0.531
Boldness	0.772	0.419	1.000	1.000	1.000
Companies	0.397	0.309	0.591	0.333	0.149
ForFrequency	0.418	0.328	0.625	0.333	0.156
FirmExperience	0.357	0.354	0.615	0.250	0.000
GenExperience	0.418	0.340	0.692	0.350	0.118
BrokerSize	0.354	0.313	0.553	0.270	0.083
DaysElapsed	0.309	0.374	0.548	0.125	0.000
ForHorizon	0.256	0.312	0.385	0.118	0.027
Industries	0.394	0.319	0.600	0.333	0.143

Table 2 Correlation among Scaled Forecast and Analyst Characteristics

The upper triangle is the Spearman correlation, and the lower triangle is the Pearson correlation. Our main variables are *Combif*, the number of interlocked firms followed by the analyst in each year; *Ratio*, the ratio of the number of interlocked firms on total firms followed by the analyst in each year. Other characteristics include *AFE*, the absolute value of forecast error scaled by the previous two trading days' price; *Boldness*, a dummy variable (*Boldness* = 1) if they are above both the analyst's prior forecast and the consensus, or else below both; *DaysElapsed*, the number of days since any analyst's prior forecast; *ForHorizon*, the number of days from the forecast date to the fiscal year-end; *ForFrequency*, the number of forecasts issued by the analyst in each year; *BrokerSize*, the number of analysts in the analyst's brokerage in each year; *FirmExperience*, the analyst's years of experience forecasting a particular firm's earnings; *GenExperience*, the analyst's overall years of forecasting experience; *Companies*, the number of companies the analyst follows in each year; and *Industries*, the number of two-digit SIC industries the analyst follows in each year. N = 81,411.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Combif (1)	1	0.959 <.0001	0.417 <.0001	-0.002 0.541	0.092 <.0001	0.145 <.0001	-0.039 <.0001	0.010 0.003	-0.037 <.0001	0.358 <.0001	-0.002 0.527	-0.005 0.178
Ratio (2)	0.883 <.0001	1	0.312 <.0001	-0.003 0.440	0.078 <.0001	0.121 <.0001	-0.042 <.0001	0.009 0.014	-0.032 <.0001	0.295 <.0001	-0.003 0.336	-0.00562 0.1089
Companies (3)	0.499 <.0001	0.312 <.0001	1	0.024 <.0001	0.154 <.0001	0.236 <.0001	-0.004 0.261	0.016 <.0001	-0.077 <.0001	0.607 <.0001	0.021 <.0001	-0.006 0.100
ForFrequency (4)	0.000 0.953	-0.001 0.694	0.019 <.0001	1	0.016 <.0001	-0.021 <.0001	0.083 <.0001	0.036 <.0001	-0.363 <.0001	0.001 0.778	0.154 <.0001	-0.004 0.264
FirmExperience (5)	0.102 <.0001	0.081 <.0001	0.150 <.0001	0.019 <.0001	1	0.621 <.0001	0.044 <.0001	0.008 0.030	-0.018 <.0001	0.104 <.0001	0.017 <.0001	0.013 0.000
Genexperience (6)	0.159 <.0001	0.121 <.0001	0.228 <.0001	-0.021 <.0001	0.615 <.0001	1	0.046 <.0001	-0.004 0.257	-0.006 0.076	0.175 <.0001	0.012 0.001	0.009 0.014
BrokerSize (7)	-0.019 <.0001	-0.023 <.0001	-0.008 0.022	0.084 <.0001	0.053 <.0001	0.051 <.0001	1	0.025 <.0001	-0.012 0.001	-0.078 <.0001	0.035 <.0001	0.022 <.0001
DayElapsed (8)	0.029 <.0001	0.021 <.0001	0.023 <.0001	0.052 <.0001	0.015 <.0001	-0.002 0.524	0.049 <.0001	1	-0.030 <.0001	0.006 0.107	-0.010 0.004	-0.018 <.0001
ForHorizon (9)	0.000 0.917	0.000 0.943	-0.052 <.0001	-0.321 <.0001	-0.010 0.006	-0.005 0.121	-0.006 0.066	-0.001 0.771	1	-0.048 <.0001	-0.227 <.0001	0.046 <.0001
Industries (10)	0.415 <.0001	0.305 <.0001	0.616 <.0001	0.001 0.711	0.107 <.0001	0.175 <.0001	-0.072 <.0001	0.013 0.000	-0.023 <.0001	1	-0.008 0.028	-0.009 0.010
Accuracy (11)	-0.028 <.0001	-0.022 <.0001	0.005 0.120	0.126 <.0001	0.009 0.011	0.007 0.043	0.017 <.0001	-0.023 <.0001	-0.324 <.0001	-0.020 <.0001	1	0.060 <.0001
Boldness (12)	-0.008 0.020	-0.008 0.020	-0.007 0.055	-0.007 0.044	0.014 <.0001	0.009 0.008	0.019 <.0001	-0.006 0.099	0.038 <.0001	-0.010 0.003	0.060 <.0001	1

Table 3 Forecast Accuracy and Interlock

This table reports the association between forecast accuracy and analyst characteristics. The accuracy and characteristics are all scaled to range from 0 to 1 within each firm-year. The characteristics include *Combif*, the number of interlocked firms followed by the analyst; *Ratio*, the ratio of the number of interlocked firms on total firms followed by the analyst; *DaysElapsed*, the number of days since any analyst's prior forecast; *ForHorizon*, the number of days from the forecast date to the fiscal year-end; *LagAccuracy*, the analyst's prior year absolute forecast accuracy for the firm; *BrokerSize*, the number of analysts in the analyst's brokerage in each year; *FirmExperience*, the analyst's years of experience forecasting a particular firm's earnings; *ForFrequency*, the number of forecasts issued by the analyst in each year; *GenExperience*, the analyst's overall years of forecasting experience; *Companies*, the number of companies the analyst follows in each year; and *Industries*, the number of two-digit SIC industries the analyst follows in each year. *, **, *** indicate two-sided significant levels of 90%, 95% 99% respectively.

Variables	Estimate (t-value)	Estimate (t-value)
Combif	-0.021 (-5.949)***	
Ratio		-0.014 (-4.356)***
Companies	0.017 (3.713)***	0.011 (2.515)**
LagAccuracy	0.046 (14.589)***	0.046 (14.632)***
BrokerSize	0.008 (2.441)**	0.008 (2.430)**
GenExperience	0.008 (1.944)*	0.007 (1.878)*
FirmExperience	0.002 (0.529)	0.002 (0.547)
Forfrequency	0.024 (7.021)***	0.024 (7.018)***
ForHorizon	-0.321 (-90.669)***	-0.322 (-90.782)***
DaysElapsed	-0.02 (-6.975)***	-0.02 (-7.032)***
Industries	-0.026 (-6.150)***	-0.027 (-6.440)***
Boldness	0.053 (21.424)***	0.053 (21.429)***
Constant	0.712 (169.870)***	0.714 (169.894)***
No. of <i>OBS</i>	81411	81411
R-squared	0.115	0.115

Table 4 Forecast Accuracy and Interlock, Robustness Check

This table reports the association between forecast accuracy and analyst characteristics using a subsample of not interlocked firms. The accuracy and characteristics are all scaled to range from 0 to 1 within each firm-year. The characteristics include *Combif*, the number of interlocked firms followed by the analyst; *Ratio*, the ratio of the number of interlocked firms on total firms followed by the analyst; *DaysElapsed*, the number of days since any analyst's prior forecast; *ForHorizon*, the number of days from the forecast date to the fiscal year-end; *LagAccuracy*, the analyst's prior year absolute forecast accuracy for the firm; *BrokerSize*, the number of analysts in the analyst's brokerage in each year; *FirmExperience*, the analyst's years of experience forecasting a particular firm's earnings; *ForFrequency*, the number of forecasts issued by the analyst in each year; *GenExperience*, the analyst's overall years of forecasting experience; *Companies*, the number of companies the analyst follows in each year; and *Industries*, the number of two-digit SIC industries the analyst follows in each year. *, **, *** indicate two-sided significant levels of 90%, 95% 99% respectively.

	Estimate	Estimate
Variables	(t-value)	(t-value)
Combif	-0.020 (-5.115)***	
Ratio		-0.013 (-3.695)***
Companies	0.015 (3.116)***	0.011 (2.263)**
LagAccuracy	0.045 (13.311)***	0.045 (13.367)***
BrokerSize	0.006 (1.717)*	0.006 (1.721)*
GenExperience	0.008 (1.971)**	0.008 (1.847)*
FirmExperience	0.002 (0.419)	0.002 (0.523)
Forfrequency	0.023 (6.275)***	0.023 (6.282)***
ForHorizon	-0.324 (-85.227)***	-0.325 (-85.378)***
DaysElapsed	-0.021 (-7.046)***	-0.021 (-7.064)***
Industries	-0.024 (-5.213)***	-0.025 (-5.547)***
Boldness	0.054 (20.264)***	0.054 (20.252)***
Constant	0.713 (158.338)***	0.715 (158.610)***
Observations	70397	70397
R-squared	0.117	0.117

Table 5 Forecast Accuracy, Interlock, and SOX

This table reports the mediation effect of SOX on the association between forecast accuracy and analyst characteristics. The accuracy and characteristics are all scaled to range from 0 to 1 within each firm-year. The characteristics include *Combif*, the number of interlocked firms followed by the analyst; *Ratio*, the number of interlocked firms on total number of firms followed by the analyst; *DaysElapsed*, the number of days since any analyst's prior forecast; *ForHorizon*, the number of days from the forecast date to the fiscal year-end; *LagAccuracy*, the analyst's prior year absolute forecast accuracy for the firm; *BrokerSize*, the number of analysts in the analyst's brokerage in each year; *FirmExperience*, the analyst's years of experience forecasting a particular firm's earnings; *ForFrequency*, the number of forecasts issued by the analyst in each year; *GenExperience*, the analyst's overall years of forecasting experience; *Companies*, the number of companies the analyst follows in each year; and *Industries*, the number of two-digit SIC industries the analyst follows in each year. *SOX* equals 1 if year after 2002. And *SOX_Combif* is the interaction item between *SOX* and *Combif*. *SOX_Ratio* is the interaction item between *SOX* and *Ratio*. *, **, *** indicate two-sided significant levels of 90%, 95% 99% respectively.

Variables	Estimate (t-value)	Estimate (t-value)
Combif	-0.030 (-6.981)***	
SOX_Combif	0.024 (4.016)***	
Ratio		-0.019 (-4.790)***
SOX_Ratio		0.015 (2.550)**
SOX	-0.014 (-5.271)***	-0.012 (-4.478)***
Companies	0.019 (4.063)***	0.013 (2.895)***
LagAccuracy	0.046 (14.515)***	0.046 (14.581)***
BrokerSize	0.007 (2.183)**	0.008 (2.290)**
GenExperience	0.006 (1.544)	0.006 (1.475)
FirmExperience	0.002 (0.529)	0.002 (0.548)
Forfrequency	0.023 (6.900)***	0.023 (6.891)***
ForHorizon	-0.322 (-90.715)***	-0.322 (-90.847)***
DaysElapsed	-0.019 (-6.876)***	-0.019 (-6.943)***
Industries	-0.025 (-5.948)***	-0.027 (-6.326)***
Boldness	0.053 (21.421)***	0.053 (21.433)***
Constant	0.717 (166.372)***	0.718 (166.178)***
No. of <i>OBS</i>	81411	81411
R-squared	0.115	0.115

Table 6 Market Reaction on Interlock

This table reports the market reaction to analysts' forecast revisions and other characteristics. The dependent variable is CAR5, [-2, 2] size decile adjusted cumulative return. Column 2 is the robustness check that we keep only those revisions without other revisions for the same firm within the time windows of the original forecast revision date. Fcrev is defined as the analyst's forecast value minus the previous latest forecast value by the same analyst to the same firm, and divided by the price 10 trading days before the release of the revised forecast. Similar like Bonner et al (2007), we also control the interaction item of Fcrev with other analyst characteristics. The characteristics include *Combif*, the number of interlocked firms followed by the analyst; *DaysElapsed*, the number of days since any analyst's prior forecast; *ForHorizon*, the number of days from the forecast date to the fiscal year-end; *LagAccuracy*, the analyst's prior year absolute forecast accuracy for the firm; *BrokerSize*, the number of analysts in the analyst's brokerage in each year; *FirmExperience*, the analyst's years of experience forecasting a particular firm's earnings; *ForFrequency*, the number of forecasts issued by the analyst in each year; *GenExperience*, the analyst's overall years of forecasting experience; *Companies*, the number of companies the analyst follows in each year; and *Industries*, the number of two-digit SIC industries the analyst follows in each year. *SOX* equals 1 if year after 2002. To eliminate revisions that cluster closely together in time, we exclude from this analysis any revision that occurs within the ten-day announcement window of a revision that is already included in the sample. *, **, *** indicate two-sided significant levels of 90%, 95% 99% respectively.

Variables	Estimate (t-value)	Estimate (t-value)
Fcrev	0.254 (6.410)***	0.293 (3.314)***
Fcrev*Combif	-0.021 (-3.785)***	-0.036 (-3.891)***
Fcrev*Combif*SOX	0.073 (6.221)***	0.052 (3.117)***
Fcrev*SOX	-0.09 (-2.555)**	0.110 (1.695)*
Fcrev*Companies	-1.926 (-12.154)***	0.005 (1.503)
Fcrev*GenExperience	-0.007 (-4.419)***	0.010 (1.751)*
Fcrev*FirmExperience	-0.005 (-1.486)	-0.032 (-4.352)***
Fcrev*ForFrequency	-0.002 (-5.801)***	-0.003 (-2.690)***
Fcrev*ForHorizon	0.002 (8.515)***	0.001 (3.607)***
Fcrev*DaysElapsed	0.003 (4.291)***	-0.001 (-0.577)
Fcrev*LagAFE	-0.004 (-1.975)**	-1.352 (-6.510)***
Fcrev*Industries	0.042 (8.647)***	0.003 (0.347)
Constant	-0.001 (-3.554)***	0.000 (0.920)
No. of OBS	81,411	18,782
R-squared	0.023	0.016

Table 7 Robustness Check Using Ratio, Market Reaction on Interlock

This table reports the market reaction to analysts' forecast revisions and other characteristics. The dependent variable is CAR5, [-2, 2] size decile adjusted cumulative return. Column 2 is the robustness check that we keep only those revisions without other revisions for the same firm within the time windows of the original forecast revision date. Fcrev is defined as the analyst's forecast value minus the previous latest forecast value by the same analyst to the same firm, and divided by the price 10 trading days before the release of the revised forecast. Similar like Bonner et al (2007), we also control the interaction item of Fcrev with other analyst characteristics. The characteristics include *Ratio*, the number of interlocked firms on total firms followed by the analyst; *DaysElapsed*, the number of days since any analyst's prior forecast; *ForHorizon*, the number of days from the forecast date to the fiscal year-end; *LagAccuracy*, the analyst's prior year absolute forecast accuracy for the firm; *BrokerSize*, the number of analysts in the analyst's brokerage in each year; *FirmExperience*, the analyst's years of experience forecasting a particular firm's earnings; *ForFrequency*, the number of forecasts issued by the analyst in each year; *GenExperience*, the analyst's overall years of forecasting experience; *Companies*, the number of companies the analyst follows in each year; and *Industries*, the number of two-digit SIC industries the analyst follows in each year. *SOX* equals 1 if year after 2002. To eliminate revisions that cluster closely together in time, we exclude from this analysis any revision that occurs within the ten-day announcement window of a revision that is already included in the sample. *, **, *** indicate two-sided significant levels of 90%, 95% 99% respectively.

Variables	Estimate (t-value)	Estimate (t-value)
Fcrev	0.285 (7.128)***	0.328 (3.783)***
Fcrev*Ratio	-0.160 (-1.493)	-1.787 (-6.259)***
Fcrev*Ratio*SOX	1.701 (6.896)***	2.647 (5.846)***
Fcrev*SOX	-0.131 (-3.501)***	-0.052 (-0.728)
Fcrev*Companies	-2.071 (-12.821)***	0.003 (1.085)
Fcrev*GenExperience	-0.007 (-4.501)***	0.012 (2.015)**
Fcrev*FirmExperience	-0.006 (-1.706)*	-0.029 (-3.881)***
Fcrev*ForFrequency	-0.002 (-5.357)***	-0.002 (-1.655)*
Fcrev*ForHorizon	0.002 (8.503)***	0.001 (3.602)***
Fcrev*DaysElapsed	0.003 (4.285)***	-0.001 (-0.617)
Fcrev*LagAFE	-0.007 (-3.412)***	-1.469 (-6.807)***
Fcrev*Industries	0.042 (8.654)***	0.009 (1.149)
Constant	-0.001 (-3.496)***	0.000 (0.978)
No. of <i>OBS</i>	81,411	18,782
R-squared	0.023	0.017