

ESSAYS ON ANALYST BEHAVIOR

by

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A dissertation submitted to the Graduate Faculty in Business in partial fulfillment of the requirements for the degree of Doctor of Philosophy, The City University of New York

2008

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ABSTRACT
ESSAYS ON ANALYST BEHAVIOR

by

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This dissertation is comprised of two essays on analyst behavior. In the first essay, I examine changes in analysts' earnings forecasts after Regulation FD and the Global Analyst Research Settlement both in the United States and 40 other countries. I find that analysts around the world generally make overly optimistic earnings forecasts. However, in the U.S., forecast bias declined after Regulation FD and then further after the Global Analyst Research Settlement. Outside the U.S, forecast bias declined only after the Global Settlement, but was not affected by Regulation FD. Forecast bias declined more in countries with civil law, less protection of minority shareholders, and less transparent accounting disclosures. Furthermore, the presence of the twelve banks involved in the Global Settlement positively correlates with the reduction in forecast bias abroad.

In the second essay, I study long-term earnings growth (LTG) forecasts around seasoned equity offerings. I document that LTG forecasts are generally higher for firms with fewer analysts than for firms with more analysts covering the stock. In contrast to prior studies, I find no difference between LTG forecasts made by affiliated analysts who work for investment banks that have relationships with equity issuing firms and LTG forecasts made by unaffiliated analysts. Further analysis shows that aggregation bias

influences the prior studies' conclusion that affiliated analysts make more optimistic forecasts. Both affiliated and unaffiliated analyst forecasts are more optimistic for firms with fewer analysts. In addition, the number of unaffiliated analysts relative to the number of affiliated analysts covering the same firm increases with the firm's analyst coverage. As a result, the mean forecast bias of unaffiliated analysts turns out to be lower than the mean forecast bias of affiliated analysts. However, this observed difference in the mean forecast bias is driven by analyst coverage rather than analyst affiliation.

ACKNOWLEDGEMENTS

My accomplishments were made possible by so many individuals. First of all, I thank my parents, Jinda and Mullika, and my sister, Siri, for their unconditional love and support. I am especially grateful to my fiancée, Hsin-Ping Huang, and her family for their love and companionship. They gave me motivation and support that helped to propel me to the finish line of the Ph.D. program.

I owe special gratitude to my advisor, Professor Armen Hovakimian, who tirelessly and patiently has given me advice and made sure that I was on the right track for a wonderful academic career. I also want to thank Professor Terrence Martell who has offered me his friendship and support since my first day at Baruch College. I thank Professor Donal Byard, who is also on my dissertation committee, for his valuable suggestions and comments on my research.

Special thanks to Professor Paul Laux for writing me an excellent letter of recommendation for my Ph.D. application. My journey as a Ph.D. student would never have been possible without his support and numerous invaluable pieces of advice.

I thank Professor Ashok Vora and Professor Christos Giannikos, who admitted me to this Ph.D. program. I am also grateful to Professor Kishore Tandon, Professor Joseph Weintrop, and Professor Gloria Thomas for giving me help, support, and encouragement throughout my graduate career.

My journey as a Ph.D. student ended happily with the help of Professor Turan Bali and Professor Nusret Cakici, who have had faith in my abilities and helped me start off the next stage of my academic career.

Finally, I wish to thank my uncle, Thavisith Ponprapansiri, and my friends, in particular Sourish Dutta, James Andersen, Sylvia Clark, Allison Davis, Rasal Mowla, Narumon Vamvanij, Zhihong Shi, Elena Smirnova, Farah Yunus, Rajneesh Sharma, Michael Suan, Irena Hutton, Dror Parnes, Yi Tang, Chih-Chieh Chiu, Eleni Gousgounis, Norbert Pierre, Nosa Omoregie, Junqiang Xie, Tatiana Maksimenko, Guangzhong Li, and Olga Aksakalova, for their friendship and support. I have named only a few, and I apologize sincerely to the people I may have omitted.

Table of Contents

Content	Page Number
Title Page	i
Approval Page	ii
Abstract	iii
Acknowledgements	v
Table of Contents	vii
List of Tables	viii
List of Figures	x
Essay 1: Earnings Forecast Bias around the World Following Regulatory Changes	1
Essay 1: Appendix A	38
Essay 1: Appendix B	39
Essay 2: Does Analyst Affiliation Influence Long-Term Earnings Growth Forecasts?	61
Essay 1: Bibliography	97
Essay 2: Bibliography	100

List of Tables

Essay 1: Earnings Forecast Bias around the World Following Regulatory Changes

Table No.	Table Name	Page Number
1	Forecast Bias by Forecast Month and Subperiod in the U.S.	43
2	Forecast Bias by Fiscal Year and Forecast Month in the U.S.	45
3	Summary Statistics for U.S. sample	47
4	The Impact of Reg FD and the Global Settlement on Forecast Bias in the U.S.	48
5	Comparing Forecast Bias of Big-12 and Other Analysts in the U.S.	49
6	Forecast Dispersion by Forecast Month and Subperiod in the U.S.	50
7	Forecast Accuracy by Forecast Month and Subperiod in the U.S.	51
8	Forecast Bias by Country and Subperiod	52
9	The Impact of Reg FD and the Global Settlement on Forecast Bias in International Markets	55
10	Percentage of Forecasts from Big-12 Banks by Country and Subperiod	56
11	The Role of Big-12 Banks on the Impact of New Regulations on BIAS in International Markets	57
12	The Role of Big-12 Banks on the Impact of New Regulations on OPTIMISTIC in International Markets	59
13	Forecast Bias of Big-12 and Other Analysts in International Markets	60

Essay 2: Does Affiliation Bias Analyst Long-Term Earnings Growth Forecasts

Table No.	Table Name	Page Number
1	Forecast Level, Realized Earnings, and Forecast Bias for One-Year Ahead, Two-Years Ahead, and LTG Earnings Forecasts around the Filing Year	82
2	Forecast Level, Realized LTG, and Forecast Bias for LTG Earnings Forecasts around the Filing Year by Analyst Affiliation	83
3	Forecast Level, Realized LTG, and Forecast Bias for LTG Earnings Forecasts around the Filing Year by Analyst Coverage	84
4	Forecast Level, Realized LTG, and Forecast Bias for LTG Earnings Forecasts around the Filing Year by Analyst Affiliation without Aggregation Bias	85
5	Forecast Level, Forecast Bias and Change in Forecast Bias For LTG Earnings Forecasts -2/+2 Years around the Filing Year by Analyst Affiliation	86
6	Summary Statistics	88
7	The Relation between Forecast Bias before the Filing Date and Analyst Affiliation	89
8	The Relation between Forecast Bias after the Filing Date and Analyst Affiliation	90
9	The Relation between Analyst Affiliation and Change in Forecast Bias -2/+2 years around the Filing Date	91
10	The Relation between Analyst Affiliation and Change in Forecast Bias -1/+1 and -3/+3 years around the Filing Date	93
11	The Relation between Analyst Affiliation and Change in Forecast Bias around the Filing date after Excluding Forecasts from Unaffiliated Analysts Who Work for the Top 25 Underwriters	95

List of Figures

Essay 1: Earnings Forecast Bias around the World after Regulatory Changes

Figure No.	Figure Name	Page Number
1	Timeline of Analyst Forecasts	41
2	Kernel Density Estimate of Optimistic Variable	42

ESSAY 1

Earnings Forecast Bias around the World Following Regulatory Changes

Until recently, sell-side analysts had incentives to generate research reports that did not reflect their true opinions. They tended to make excessive “buy” recommendations and inflated earnings forecasts for several reasons, two of which gained considerable attention from regulators in the United States. First, analysts were pressured to favor covered firm managers to gain privileged access to information flow (Lim, 2001). Second, although analysts are supposed to provide accurate and truthful research reports to investors, conflicts of interest occurred because their compensation was tied to profits generated from investment banking business and brokerage commissions (Lin and McNichols, 1998; and Carleton, Chen and Steiner, 1998).

In an effort to restore public confidence in U.S. capital markets, in the early 2000s, U.S. regulators enacted several rules and regulations, prosecuted analysts whose research reports were tainted by conflicts of interest, and fined banks that failed to adequately prevent research analysts’ conflicts of interest. I study two of the main regulatory developments during this period: 1) the Regulation Fair Disclosure (Reg FD), which became effective on October 23, 2000 and 2) the Global Analyst Research Settlement (Global Settlement), which was announced on December 20, 2002.¹

¹ There are several rules and regulations enacted around the Global Settlement, for example, NASD 2711, NYSE 472, and Regulation Analyst Certification. Because they were introduced over a relatively short period of time, it is not possible to determine the independent impact of each one of these regulatory actions. Importantly, however, all of these rules and regulations share the same goal of reducing analysts’ conflicts of interest. Therefore, we use the term “Global Settlement” as representing all the rules and regulations enacted around the Global Settlement to address analysts’ conflicts of interest.

Although these two regulatory actions were enacted with different goals, they both have the potential to improve the quality of analyst forecasts. One of the stated goals of Reg FD is to prohibit private communication between firms and analysts, thereby helping to level the playing field, so that market participants can have equal access to information, and making analysts less dependent on such communication. Lim (2001) argues that analysts make optimistic forecasts to gain privileged access to insider information. If so, Reg FD should reduce analyst forecast bias, since it eliminated this incentive for analysts to inflate their earnings forecasts by prohibiting firms from selectively disclosing private information to analysts.

The Global Settlement is an important enforcement agreement between U.S. regulators and 12 large investment banks (hereafter referred to as the Big-12 banks) designed to eliminate research analysts' conflicts of interest. If successful, the Global Settlement should reduce the optimistic bias in analyst forecasts.

This study investigates how analysts' forecasting behavior has changed as a result of these two new regulations. Specifically, I examine whether these two actions taken by U.S. regulators reduced the bias in analysts' earnings forecasts documented in prior studies. As part of this analysis, I compare forecasts of analysts who worked for the Big-12 banks directly affected by the Global Settlement and their counterparts from other banks. Finally, I extend the analysis to examine the impact of legal institutions and the presence of the Big-12 banks in each country on the effect of Reg FD and the Global Settlement on non-U.S. analysts.

I focus on annual earnings forecast bias for several reasons. First, expected earnings are used in earnings-based valuation models to compute stock prices. Expected

earnings can also influence both expected cash flows and dividend payout, which are important inputs for several other stock valuation models. Inflated earnings forecasts can drive stock prices above their fair values if investors fail to adjust for bias. Retail investors who are less sophisticated than institutional investors are most likely to be the victims of such bias.²

Second, given the flurry of new regulations, it is clear that regulators consider analyst behavior an important factor in maintaining investor confidence in financial markets. Regulation is costly due to the significant costs associated with analyzing the situation and developing remedies. Moreover, restrictions and reporting requirements imposed on various market participants result in ongoing compliance costs. These costs can be justified only if the new regulations indeed help reduce analysts' conflicts of interest, thus generating an important benefit for financial markets.

Third, a number of prior studies that examine the impact of Reg FD and the Global Settlement focus on forecast accuracy and forecast dispersion (Bailey, Li, Mao, and Zhong, 2003; Helfin, Subramanyam, and Zhang, 2003; Agrawal, Chadha, and Chen, 2006; Clarke, Khorana, Patel, and Rau, 2006; Mohanram and Sunder, 2006).³ However, these aspects of analyst behavior may not be affected by analysts' conflicts of interest,

² Scherbina (2005) finds a negative relation between the estimated bias that arises due to self-selection in coverage and subsequent stock returns. Based on her results it is the retail investors who fail to adjust for the biases. Malmendier and Shanthikumar (2007) find that retail investors react to stock recommendation literally. While institutional investors buy when stocks have "strong buy" ratings and sell when stocks have "buy" ratings, retail investors buy on both cases.

³ Overall, these studies find either no change (Bailey, Li, Mao, and Zhong, 2003; Heflin Subramanyam, and Zhang, 2003) or a decrease in forecast accuracy (Agrawal, Chadha, and Chen, 2006; Mohanram and Sunder, 2006) following Reg FD. Several studies also find either no change or an increase in forecast dispersion (Heflin, Subramanyam, and Zhang, 2003; Agrawal, Chadha, and Chen, 2006) and a diminished price impact of analysts' forecasts after Reg FD (Gintschel and Markov, 2004).

which arguably relate to the observed persistently optimistic bias in analysts' long-term earnings forecasts.

Mohanram and Sunder (2006) examine the impact of Reg FD on forecast bias. They focus on quarterly earnings forecasts between October 1999 and December 2001 and find that these forecasts became more optimistic after Reg FD. They attribute the increase in forecast bias after Reg FD to low realized earnings during the 2001 recession. Because they use a short sample period, they cannot control for macroeconomic conditions. In this study, I can control for macroeconomic conditions in regression analyses because my sample covers a longer time period, from 1984 to 2006.

Clarke, Khorana, Patel, and Rau (2006) focus on relative bias in analyst forecasts. Although they find that the Global Settlement had no impact on the cross sectional variation in forecast bias, their relative measure of bias cannot tell us much about the impact of the Global Settlement on analyst forecast bias at the aggregate level. In contrast, my focus is on the impact of regulatory changes on the aggregate bias, the existence of which is documented in a number of prior studies (O'Brien, 1998; Brown, 1997; Chopra, 1998)

Fourth, this study focuses on long-term forecasts while most of the prior studies focus on short-term forecasts, e.g., forecasts of quarterly earnings. Specifically, I examine the bias in earnings forecasts made between 24 months before and one month after the end of the forecast period because forecast bias is more significant for long-term forecasts (Richardson, Teoh, and Wysocki, 2004).

Lastly, the ability of analysts to forecast earnings accurately can be verified easily in a straightforward manner because actual earnings are observed at the end of the

forecast period. Barber, Lehavy, McNichols, and Trueman's (2006) study the change in distribution of stock recommendations made during 1996-2003. They find that the percentage of buys decreased starting in mid-2000. However, the reference for an optimal stock rating distribution is not observable. It is uncertain as to how unbiased is the new distribution of stock ratings. On the contrary, earnings forecast bias should be zero at the aggregate level when analysts make their forecasts based on their true opinions.

Consistent with the results of the earlier studies, a univariate analysis of earnings forecasts for U.S. firms from 1984-2006 confirms that analysts generally make overly optimistic forecasts. The mean forecast bias prior to Reg FD is around two percent of the stock price. Controlling for macroeconomic conditions, as well as for the firms' and analysts' characteristics, I find that the optimistic bias in analysts' forecasts declined but did not disappear after Reg FD. This finding is consistent with the hypothesis that part of the analysts' motivation to make optimistic forecasts was to gain access to insider information (Lim, 2001).

Average forecast bias fell sharply to 0.6 percent of stock price whereas the median forecast bias has totally disappeared after the Global Settlement, implying that the settlement helped to reduce analysts' conflicts of interest. Both the initial overoptimism and the post-regulation decline in forecast bias are not limited to analysts working for the Big-12 banks, but they describe the forecasting behavior of all analysts. Thus, analysts' conflicts of interest are an industry-wide phenomenon in the United States.

I next examine analysts' forecasts in 40 other countries for forecasts of earnings for fiscal years between 1991 and 2006. I do not expect to see any change in analyst

behavior abroad immediately following Reg FD because foreign firms are not subject to Reg FD. In contrast, I expect that earnings forecasts for firms in foreign countries become less biased after the Global Settlement. Although the Global Settlement covers only U.S. research reports, anecdotal evidence suggests that the Big-12 multinational banks voluntarily applied the agreement to their operations abroad. Moreover, to reduce analysts' conflicts of interest, the National Association of Securities Dealers (NASD) enacted rule 2711 in 2002 and imposed this rule on both U.S analysts and foreign associates.⁴

Univariate results show that, prior to the passing of Reg FD, analysts' earnings forecasts were overly optimistic worldwide. The medians forecast bias before Reg FD was 0.22% of stock price for current-year earnings forecasts, and 1.24% of stock price for subsequent-year earnings forecasts. Earnings forecasts were overly optimistic in 37 out of 40 countries for current year-forecasts and in all countries for subsequent-year forecasts.

Regression analyses reveal that Reg FD had no impact on forecast bias for non-U.S. firms. However, I find strong evidence that forecast bias in these countries declined after the Global Settlement. Furthermore, I find that the decline in forecast bias is especially large in countries with civil law, less protection of minority shareholders, and less transparent accounting disclosures. Similar to the evidence for the U.S market, the evidence for international markets shows that other banks are at least as overoptimistic as the Big-12 banks, if not more overly optimistic.

⁴ NASD rule 1100 defines foreign associates as persons associated with an NASD member that meet the following criteria: (1) they are not citizens, nationals, or residents of the U.S.; and (2) they conduct all of their securities activities outside the U.S. and will not engage in any securities activities with or for any citizen, national, or resident of the U.S.

In addition, I find that the presence of the Big-12 banks do not affect cross sectional difference in the level of forecast bias across 40 countries before the Global Settlement. However, after the Global Settlement, forecast bias declined more in countries that have more forecasts from analysts working for the Big-12 banks. This evidence reveals that the change in regulations in one country can have impact on market participants abroad when foreign operations of the market participants choose to adhere to the improved regulations.

To summarize, this study contributes to the literature by providing new evidence on the impact of RegFD and the Global Settlement in both U.S. and international markets. The study shows that an optimistic forecast bias is not limited to the Big-12 banks, nor is it limited to the U.S. market. Analysts' annual earnings forecast bias in the U.S. declined after Reg FD and declined further after the Global Settlement. Reg FD did not change analysts' behavior abroad, but the advent of the Global Settlement is associated with a reduced forecast bias around the world. The decline in forecast bias is larger in countries with civil law, weaker protection of minority shareholders, fewer accounting disclosures, and with the greater the presence of the twelve banks involved in the Global Settlement.

The remainder of this paper is organized as follows. Section 1 describes why analysts make overly optimistic forecasts and discusses how new regulations affect these forecasts. Section 2 describes databases. Section 3 presents results for the U.S. sample data, while Section 4 presents results for international sample data. Section 5 offers a conclusion to the study.

1. Background

1.1. Why do analysts make overly optimistic earnings forecasts?

A number of prior studies document that analysts regularly make overly optimistic long term earnings forecasts (O'Brien, 1988; Stickel, 1990; Brown, 1997; Chopra, 1998). The optimistic bias is usually larger for long-term forecasts and smaller for forecasts made closer to the earnings announcement date; henceforth, this trend is referred to as the walk-down trend (Richardson, Teoh, and Wysocki, 2004; Francis, Chen, Philbrick, and Willis, 2004). Several explanations are offered for analyst optimism.

First, analysts may be influenced by conflicts of interest if their compensation is tied to investment banking fees and brokerage commissions. Lin and McNichols (1998) find that analysts affiliated with underwriters make more favorable stock recommendations and long-term earnings growth forecasts. Chan, Karceski, and Lakonishok (2007) show that analysts' earnings forecasts are influenced by their desire to win investment banking clients. Agrawal and Chen (2005) find that optimism in long-term earnings growth forecasts is high when analysts work for financial institutions whose revenues come mainly from brokerage business. Using Australian data, Jackson (2005) finds that optimistic analysts generate more trades for their brokerage firms. Cowen, Groysberg and Healy (2006) also document that analyst optimism is driven by trading incentives. Hong and Kubik (2003) find that brokerage houses reward optimistic analysts. Optimistic analysts from low status brokerage houses are more likely to move up to higher status brokerage houses.

Second, analysts are pressured to maintain good relations with firm management to gain access to insider information that can help them improve forecast accuracy (Das,

Levine, and Sivaramakrishnan, 1998; Lim, 2001). Third, analysts tend to cover stocks for which they hold positive views, and drop or avoid stocks they have negative views about. McNichols and O'Brien (1997) call this analyst behavior self-selection bias. Fourth, analysts may have a cognitive bias, overreacting to good earnings information but underreacting to bad earnings information (Easterwood and Nutt, 1999). Lastly, the walk-down trend might be driven by the "earning-guidance game," in which analysts issue optimistic forecasts at the start of a fiscal year, then revise their estimate until the firm can beat the forecasts at earnings announcement date (Richardson, Teoh, Wysocki, 2004).

1.2. Recent regulations

Prior to Reg FD, analysts and institutional investors often had an informational advantage over small investors through private communications with management and closed conference calls during which firms regularly discussed past performances and provided guidance on future prospects. Such timely information gave these professionals an unfair advantage to trade stocks profitably at the expense of uninformed investors.

To gain access to the information flow, analysts may have to maintain good relations with insiders by making optimistic forecasts and by issuing buy recommendations in their research reports. Analysts' excessively optimistic views of the stocks may have misled uninformed investors and further exacerbated the deterioration of investors' confidence in capital market integrity. Through Reg FD, which was introduced in October 2000, the Securities and Exchange Commission (SEC) intended to improve fairness and regain public confidence in the markets by requiring U.S. public firms to simultaneously disclose material information to all market participants.

However, other sources of conflicts of interest remained unaddressed by Reg FD. For instance, analysts could be pressured to make optimistic forecasts and buy recommendations to favor investment banking clients or to generate trading volume. The SEC and Self-Regulatory Organizations (SROs, e.g., NASD and NYSE) paid significant attention to this issue and subsequently introduced a number of new rules and regulations to curb the negative consequences of these conflicts.

The Sarbanes-Oxley Act of 2002 (SOX), also known as the Public Company Accounting Reform and Investor Protection Act of 2002, became law on July 30, 2002. SOX is a broad legislation covering various business practices, such as auditor independence, corporate responsibility, enhanced financial disclosure, analysts' conflicts of interest, and corporate and criminal fraud accountability. SOX amended the Securities and Exchange Act of 1934 by creating Section 15D which requires the NASD and NYSE to adopt rules reasonably designed to address research analysts' conflicts of interest.

To comply with SOX, NASD released the new rule 2711 (Research Analysts and Research Report), and NYSE amended its rule 351 (Reporting Requirement) and rule 472 (Communication with the Public). Most provisions of these rules went into effect on July 9, 2002. These rules mitigate analysts' conflicts of interest by separating research analysts from the influence of the investment banking and brokerage business. Analysts' compensation can no longer be tied to the performance of these businesses. Moreover, analysts are restricted from personal trading on the stocks they cover.

In addition to these rules, the SEC adopted Regulation Analysts Certification (Reg AC) on February 6, 2003.⁵ Reg AC provides guidelines on proper disclosure of potential conflicts of interest among sell-side analysts, including their association with investment banking clients and the structure of their compensation.

Regulators can achieve their objectives only if they are supported by rigorous enforcement actions. Following joint investigations by the SEC, NASD, NYSE, and the New York State Attorney General, ten large U.S. and multinational investment banks agreed to pay a fine totaling \$1.435 billion in the Global Analyst Research Settlement for their failure to adequately address research analysts' conflicts of interest. The terms of the Global Settlement were announced on December 20, 2002, and initially covered 10 banks.⁶ The final agreement was announced on April 28, 2003. Two more banks reached settlements on August 26, 2004.⁷ The Global Settlement and the SRO rules share the same spirit in that their mutual objective is to eliminate analysts' conflicts of interest.

An observed reduction in forecast bias after RegFD will support the argument that analysts issued overly optimistic forecasts, in part, to gain access to inside information from management. Meanwhile, the reduction in bias after the Global Settlement and NASD 2711 will confirm that analysts' conflicts of interest influence analyst behavior. If self-selection or cognitive biases are the main reason for analysts' overly optimistic forecasts, then these regulatory changes should have no effect on forecast bias.

⁵ Reg AC took effect on April 14, 2003. See joint report by NASD and NYSE for the effectiveness of the new rules.

⁶ The ten investment banks are Bear Stearns, Citigroup, Credit Suisse First Boston, Goldman Sachs, J.P. Morgan, Lehman Brothers, Morgan Stanley, Merrill Lynch, UBS and U.S. Bancorp Piper Jaffray.

⁷ The other two investment banks are Deutsche Bank and Thomas Weisel Partners.

2. Data

Sell-side analysts' earnings forecasts for the current and subsequent years are from the Institutional Brokers' Estimate System (I/B/E/S) database, which contains forecasts for both U.S. and international firms.⁸ Forecasts for current- and subsequent-year earnings per share (EPS) are for the upcoming and following year's earnings announcement date (EAD). Figure 1 illustrates the timeline of analyst forecasts. Forecasts for the current year can be made several months after the fiscal year end, especially when a firm delays its earnings announcement. Meanwhile, forecasts for the subsequent-year EPS are made up to 24 months before the forecast period end date. For each earnings realization, analysts can make multiple forecasts, and some analysts may continue to make earnings forecasts after the forecast period ends. However, bias for forecasts made after a fiscal year end is usually small. In the final sample, I retain only those forecasts that were made no more than one month after the forecast fiscal year end (in forecast month +1).

For each forecast, I/B/E/S provides actual earnings, the forecast issue date, the forecast period end, the earnings announcement date, an analyst identity code, a broker identity code, and the number of analysts used to calculate the consensus.⁹ I use the Broker Translation File from I/B/E/S to convert broker codes into brokers' names. These names are used to identify analysts who work for the Big-12 banks.

⁸ Forecasts for current-year EPS are the forecasts in I/B/E/S with code FPI 1. Forecasts for subsequent-year EPS are the forecasts in I/B/E/S with code FPI 2.

⁹ I exclude forecasts contained in I/B/E/S' excluded estimates file, as well as forecasts for which actual earnings figures are missing.

Quarterly data on U.S. real GDP growth rates are downloaded from the website of the Bureau of Economic Analysis. Quarterly data on real GDP for other countries are from Reuters 3000 Xtra (see Appendix A).¹⁰ Stock prices are from the I/B/E/S Summary file or Center for Research in Security Prices (CRSP) monthly file for cases in which the prices are not available from I/B/E/S.¹¹ I adjust CRSP stock prices for stock splits.¹²

3. Forecast bias in the U.S.

3.1. Descriptive statistics and univariate analysis for U.S. stock.

The I/B/E/S Detail file contains 5,469,386 earnings forecasts for U.S. firms for fiscal year ending between 1984 and 2006. There are 5,401,307 forecasts made between one month after and 24 months before the corresponding forecast period end date. Similar to Richardson, Teoh, and Wysocki (2004), I compute forecast bias as follows:

$$BIAS_{j,t,m} = 100 * (F_{j,t,m} - A_{j,t}) / P_{j,t-1}, \quad (1)$$

where

$$F_{j,t,m} = \frac{1}{I_{j,t,m}} \sum_{i=1}^I F_{j,t,m,i}, \quad (2)$$

where

$$F_{j,t,m,i} = \frac{1}{K_{j,t,m,i}} \sum_{k=1}^K F_{j,t,m,i,k}. \quad (3)$$

¹⁰ For international countries, when real GDP is not seasonally adjusted, I calculate adjusted quarterly time series real GDP by using the X12 procedure developed by the U.S. Census Bureau.

¹¹ I/B/E/S' summary files contain snapshots of consensus level data and corresponding stock price taken on a monthly basis. The snapshots are as of the Thursday before the third Friday of every month. Stock price in this file is the price on the date of the last available price prior to the Thursday.

¹² I/B/E/S' earnings related data and stock prices are split adjusted. For stock prices from CRSP, stock prices are divided by CFACPR to get the split- and dividend-adjusted stock prices. CFACPR is the cumulative price adjustment factor.

In (1)-(3), $A_{j,t}$ is the actual earnings per share for firm j in fiscal year t ; $F_{j,t,m,i}$ is the average of annual earnings forecasts for fiscal year end t of firm j , made in month m by analyst i ; $K_{j,t,m,i}$ is the number of forecasts made in month m by the same analyst i for the same firm j and fiscal year t ; $I_{j,t,m}$ is the number of analysts making forecasts in month m for firm j and fiscal year t ; $P_{j,t-1}$ is the stock price of firm j one year before the fiscal year end t .¹³ Using the same stock price as the denominator guarantees that any changes in forecast bias are due to changes in analyst forecasts, not to those in the stock price.

To reduce the impact of extreme observations, I drop forecast bias at the top and bottom 1% of the distribution. I also exclude forecasts made in the event months, i.e., October 2000 and December 2002.

Large firms tend to have more analyst coverage and thus more forecasts than small firms. To avoid giving too much weight to large firms, I use the following procedure. First, for each firm in each month for each forecast period end date, I compute the mean forecast bias within each analyst. Second, I calculate a mean of means from the first step across analysts who follow the same firm and make forecasts for the same forecast period end date in the same month. The second step yields 798,672 firm-fiscal year-month observations that also have all of the required control variables used in the subsequent regression analyses. The average forecast error in this sample is 1.76% of stock price. This result confirms prior evidence that analysts' forecasts of EPS in the U.S. are optimistically biased (O'Brien 1988; Stickel 1990; Brown 1997; Chopra 1998).

¹³ It is common to use stock price to normalize the forecast bias (Richardson, Teoh and Wysocki, 2004; Bradshaw, Richardson, and Sloan, 2006). Later in the paper, I examine the robustness of my findings to alternative scaling of analyst forecast errors.

Panel A of Table 1 presents the average forecast bias for U.S. firms in each month for forecast period end date for each of the three sub-periods: before Reg FD, between Reg FD and the Global Settlement, and after the Global Settlement. Panel A of Table 1 shows that earnings forecasts were overly optimistic before Reg FD, around 1.98% of stock price. Forecast bias declined significantly to 0.61% of stock price after the Global Settlement. This result is robust to the definition of forecast bias. In Panel B Table 1, I normalize the bias by book value of equity per share. The conclusion from this table is qualitatively the same as that from Panel A in that analysts were overly optimistic before Reg FD. And their forecast bias drastically declined after the Global Settlement.

Table 2 presents forecast bias by each forecast period end year during my sample period. To align forecast period end dates with annual variables like real GDP growth rates, I use forecasts only for firms with December fiscal year end. Row numbers represent the number of months from forecast period end. For example, a forecast made in October 2000 for the forecast period ending in 2000 (i.e., two months before the forecast period end) will be in column 2000 and row -2, while a forecast made in October 1999 for the forecast period ending in 2000 will be in column 2000 and row -14. I also provide annual stock returns, price-earnings (PE) ratios and U.S. real GDP growth rates in Panel B of Table 2.¹⁴

Both means (in Panel A) and medians (in Panel B) of forecast bias before the Global Settlement are clearly biased and also have the walk-down trend of earnings forecasts described by Richardson, Teoh, and Wysocki (2004). After the Global

¹⁴ I use stock prices from the CRSP monthly file to calculate annual stock returns. I define PE ratio as the stock price one year before forecast period end date, divided by actual earnings per share. I use only observations with positive EPS and also drop the top 1% PE ratio.

Settlement, forecast bias declined significantly. Panel A shows that the level of forecast bias of earnings forecasts for fiscal years ending 2004-2006 are the lowest of the sample (0.4%, 0.7%, and 0.4% of stock prices respectively). Even more strikingly, Panel B shows that the medians for forecast bias from 2004 to 2006 are -0.1%, 0.0%, and 0.0% of stock prices respectively. The medians for forecast bias for fiscal year end 2004 are negative in the majority of forecast months. This absence of the positive forecast bias is not observed in any of the prior sample years. Moreover, in Panel B, the walk-down trend practically disappeared for the forecast periods ending in 2004-2006. This evidence implies that before the Global Settlement, analysts' conflicts of interest were in fact associated with excessive optimism and that U.S. regulators' efforts to reduce analysts' conflicts of interest have been effective.

Some may argue that unexpected increases in PE ratios, stock returns, and real GDP growth rates could be the drivers of the declining forecast bias after the settlement, since each of them can directly influence the dominator used here. Contrary to this argument, I find that PE ratios after the settlement are lower than those in 2001 and 2002, and that the real GDP growth rates after the settlement are quite similar to those in 1992 to 1994, when the U.S. economy was rebounding from the bottom of an economic cycle.¹⁵ Furthermore, stock returns were also high in 1991 and 1995, but the levels of forecast bias in those years are not low, at 3.1% and 1.4% of stock prices respectively. Overall, these results imply that the Global Settlement was associated with a significant decline in

¹⁵ I obtain economy cycle data from <http://www.nber.org/cycles.html#announcements>. National Bureau of Economic Research determined that the bottoms of two previous two economic cycles were in March 1991 and November 2001.

forecast bias, and the decline in forecast bias is not driven solely by changes in market conditions.

This evidence is consistent with the hypothesis that prior to the Global Settlement, analysts systematically made overly optimistic forecasts due to conflicts of interest, and that the Global Settlement and other related rules have successfully reduced the conflicts.

3.2. Regression analysis for U.S. stocks

Here, I estimate the fixed firm effects regression to formally test the hypothesis that Reg FD and the Global Settlement reduce analysts' forecast bias for U.S. stocks,

$$\begin{aligned}
 BIAS_{j,m,t} = & \alpha_0 + \alpha_1 REGFD_{m,t} + \alpha_2 Glob_{m,t} + \alpha_3 NumA_{j,m,t} + \alpha_4 FirmSize_{j,m-1,t} + \alpha_5 MB_{j,m-1,t} \\
 & + \alpha_6 YearStk_{j,m,t} + \alpha_7 YearIBES_{j,m,t} + \alpha_8 NumStk_{j,m,t} + \alpha_9 NumInd_{j,m,t} \\
 & + \alpha_{10} BROKERSize_{j,m,t} + \alpha_{11} EPSLoss_{j,t} + \alpha_{12} EPSDecline_{j,t} + \alpha_{13} Litigation_j \\
 & + \alpha_{14} Labor_{j,m-1,t} + \alpha_{15} ActualGDP_t + \alpha_{16} UnexpectedGDP_{m,t} + \lambda_m Month_m \\
 & + \beta_j \sum Dfirm_j + \varepsilon_{j,m,t}.
 \end{aligned} \tag{4}$$

In (4), $BIAS_{j,m,t}$ is the mean forecast error for all forecasts for firm j made in month m relative to the end of fiscal year t , calculated according to equations (1)-(3). $RegFD_{m,t}$ equals one for forecasts made between October 23, 2000 and December 20, 2002. $Glob_{m,t}$ equals one for forecasts made after December 20, 2002. A negative sign for the coefficient of $RegFD_{m,t}$ or $Glob_{m,t}$ would indicate a decline in the bias following, respectively, Reg FD and Global Settlement.

In (4), I also control for firm characteristics, analyst characteristics, broker size, and economic conditions. I calculate the means of these control variables in the same way I calculate the mean forecast bias, and I use them in this regression.

Lim (2001) argues that forecast bias will be higher when a firm's information environment is less transparent, for example when the firm is small and has less analyst coverage. Hence, I use analyst coverage and firm size as proxies for the degree of information transparency. Analyst coverage, $NumA_{j,m,t}$, is defined as the number of outstanding forecasts used in I/B/E/S monthly consensus calculation. Analyst coverage represents the number of analysts following firm j in month m for forecast period end in year t . I use stock prices and the number of outstanding shares from the I/B/E/S Actuals and Pricing & Ancillary file to calculate firm size, $FirmSize_{j,m-1,t}$, which is defined as the natural log of a firm's market capitalization at the end of the month before the forecast date.

Analysts tend to make more accurate and less biased forecasts when they have more experiences and a greater amount of resources (Clement, 1999; Lim, 2001). I use two measures for experience: the number of years since analyst i first appeared in the I/B/E/S database, $YearIBES_{i,m,t}$, to measure general experience; and the number of years an analyst i has been following firm j , $YearStk_{i,j,m,t}$, to measure firm-specific experience.

$BrokerSize_{i,m,t}$ is the number of analysts who work for the same employer during the same forecast year as the analyst who made forecasts. Analysts who work for larger firms tend to have more resources. Clement (1999) also finds that analysts' forecasts are less accurate when analysts follow more stocks and more industries. $NumStk_{i,m,t}$ is the number of stocks for which an analyst supplies at least one forecast within the calendar year. $NumInd_{i,m,t}$ is the number of industries, defined as two-digit SIC codes, for which an analyst supplies at least one forecast within a calendar year.

Prior studies find that forecasting is more difficult when firms report a loss or a decline in earnings (Brown, 2001; Heflin, Subramanyam, and Zhang, 2003). The $EPSLoss_{j,t}$ indicator equals one if firm j reported negative earnings in fiscal year end t . The $EPSDecline_{j,t}$ indicator equals one when actual earnings in fiscal year end t are lower than actual earnings in the previous year.

Matsumoto (2002) argues that firms in industries with higher risk of shareholder lawsuits, and/or greater reliance on implicit claims with stakeholders, will be more likely to avoid missing analyst forecasts. The $Litigation_j$ indicator equals one for firms in high litigation risk industries: SIC codes 2833-2836 (biotechnology), 2570-3577 and 7370-7374 (computers), 3600-3674 (electronics), and 5200-5961 (retailing).

Matsumoto (2002) also uses labor intensity as a proxy for circumstances in which firms try to avoid missing analysts' forecasts when stakeholders monitor their financial results, because the credit risk of the firm is one of their major concerns. Labor intensity, $Labor_{j,m,t}$, is defined as one minus the ratio of gross plant, property, and equipment (PPE) to total gross asset, where gross PPE is quarterly COMPUSTAT item 118 and total gross asset is item 44 plus item 41. $Labor_{j,m,t}$ is measured at the end of the last quarter preceding the forecast month m .

Richardson, Teoh, and Wysocki (2004) find lower forecast bias for firms with high growth opportunities. I use the market-to-book ratio, $MB_{i,m-1,t}$, at the end of the last quarter preceding the forecast month as a proxy for growth opportunities. The ratio is calculated as the market value of equity divided by book value of common equity (COMPUSTAT quarterly data item 14 multiplied by item 61 and divided by item 59).

I use both real GDP growth rate and the change in real GDP growth rate to capture analysts' inability to accurately forecast earnings if the economy changes substantially. $ActualGDP_t$ is the actual real GDP growth rate in fiscal year end t . $UnExpectedGDP_{m,t}$ is defined as the difference between the expected real GDP growth rate and the actual real GDP growth rate in fiscal year end t . For earnings forecasts made more than nine months before the forecast period end date, the expected real GDP growth rate in fiscal year end t is defined as the real GDP growth rate in the quarter for which analysts make earnings forecasts. For forecasts made in Q2 (between seven to nine months before forecast period end date), I calculate the expected real GDP growth rate in fiscal year end t as $(Growth\ in\ Q1 + 3 * Growth\ in\ Q2) / 4$. For forecasts made in Q3 (between four to six months before forecast period end date), I calculate the expected real GDP growth rate in fiscal year end t as $(Growth\ in\ Q1 + Growth\ in\ Q2 + 2 * Growth\ in\ Q3) / 4$. For forecasts made within the three months before the forecast period end date, $UnExpectedGDP_{m,t}$ is set to zero.

Prior research and my results in Table 2 show that forecasts made earlier in the fiscal year are less accurate (Richardson, Teoh, and Wysocki, 2004). Therefore, I use $Month_m$ defined as the number of month until the forecast period end date, to control for forecast horizon. For example, when an analyst makes a forecast in October 1999 for a forecast period ending in December 1999, $Month_m$ equals two. $Dfirm_j$ is the indicator dummy for firm j .

To reduce the influence of extreme observations, I drop the top 1% of the following variables: number of analyst coverage, firm size, market-to-book ratio, analyst experience, number of industries analyst covers, number of stocks analyst covers, and

broker size. Due to the large sample size, results are not likely to be affected by these restrictions.

Table 3 reports summary statistics for all the variables used in (4).¹⁶ In total, there are 798,672 observations that come from 9,366 firms. There are, on average, 8.78 analysts covering a firm in any particular month. The average size of firms in this sample is around \$2,970 million. The average market-to-book ratio is 3.03. On average, each broker employs 52.85 analysts. Each analyst generally follows 18.36 stocks and 5.45 industries. A typical analyst is in the I/B/E/S database for 5.38 years, and covers a particular stock for 2.28 years at the time of each forecast. Around 15% of forecasts are made for firms with negative earnings, and 34% of forecasts are made for firms whose earnings are declining relative to earnings in the prior fiscal year. The average real GDP growth rate for the U.S. economy is 3.17% and Unexpected GDP growth rate is 0.02%. Twenty-four percent of the firms in this sample are from industries with a high litigation risk.

The estimations for equation (4) are shown under specification (1) in Table 4. The positive coefficient on $Month_m$ implies the existence of the walk-down trend. Forecast bias is high for earlier forecasts and becomes lower through time. On average, forecast bias increases by 0.15% of stock price for each month further away from the forecast period end. More importantly, Reg FD reduces excessive forecast optimism for U.S. stocks. Forecast bias declined 0.35% of stock price between Reg FD and the Global Settlement. This evidence is consistent with Lim's (2001) prediction that analysts become

¹⁶ We drop top 1% of firm size, number of analyst coverage, market-to-book ratio, number of stocks analyst covers, and number of industries analyst covers.

less optimistic when they rely less on insider information. From specification (1), for each percentage decrease in actual real GDP growth rate, forecast bias will rise by 0.05 percentage of stock price. This result supports Mohanram and Sundar's (2006) argument that analyst forecasts can be influenced by changes in GDP growth rates.

After the Global Settlement, forecast bias decline by 0.63% of stock price compared to forecast bias prior to Reg FD. The results are consistent with the results from the univariate analysis, in that the Global Settlement reduced analysts' conflicts of interest. Model 2 shows that, after the Global Settlement, forecast bias declined by 0.63% of stock price when compared to forecast bias prior to Reg FD. This result is not surprising given that following the settlement, analysts must abide by the new laws and regulations.

In specification 2 of table 4, I require that there be at least one forecast for the same firm within the same forecast horizons (same *Month*) in all three periods: before Reg FD, between Reg FD and the Global Settlement, and after the Global Settlement. This restriction ensures that my findings are not affected by change in sample compositions. The results reported in column (2) of Table 4 are qualitatively the same as those in column (1). Forecast bias declined after Reg FD and then declined further after the Global Settlement.

The main conclusion that forecast bias declined after Reg FD and continued to decline after the implementation of rules and regulations related to analysts' conflicts of interest is robust to alternative definitions of forecast bias and the choice of the cutoff date separating the pre and post Global Settlement periods. In specification (3), I use

book value of equity per share to normalize forecast bias. In specification (4), I change the cutoff dates for each period using the effective date of NASD rule 2711 instead of the announcement date of the Global Settlement. $REGFD_{m,t}$ is a dummy variable equal to one when forecasts are made between October 23, 2000 and July 9, 2002; $Glob_{m,t}$ equals one when forecasts are made after July 9, 2002.¹⁷ The results in column (3) and (1) remain qualitatively the same as those in column (1), confirming that forecast bias has declined in recent years, and most significantly since the second half of 2002.

3.3. The Big-12 banks in the Global Settlement vs. non Big-12 banks in the U.S.

The Global Settlement is an enforcement agreement between U.S. regulators and 12 large investment banks (Big-12 banks) designed to eliminate research analysts' conflicts of interest. In this section, we examine whether the impact of the Global Settlement is limited to the Big-12 banks or whether there are spillover effects on other analysts as well.¹⁸ In two recent studies, Barber, Lehavy, McNichols, and Trueman (2006) and Kadan, Madureira, Wang, and Zach (2008) report that the proportion of buy recommendations declined significantly across all analysts after the implementation of NASD rule 2711. Barber, Lehavy, McNichols, and Trueman (2006) also document that the decline is stronger for the sanctioned banks. It remains an open question, however, whether the Global Settlement had a differential impact on analyst forecast bias.

Table 5 reports mean forecast bias in each forecast month separately for analysts working for Big-12 banks and for other analysts. To be included in the sample for Table 5, the covered firms must have at least one forecast from each of the two analyst groups

¹⁷ The number of observations in Model 3 is less than in Model 1 because of the missing data on book value of equity per share. The number of observations in Model 4 differs from that of Model 1 because we delete observations in the cutoff months which are not the same for these two models.

¹⁸ Other regulations, such as NASD rule 2711 affect all analysts.

within the same month. The bottom row in Table 5 summarizes the monthly results by presenting the mean forecast bias of each analyst group across all forecast months.

The results imply that, on average, the forecasts of analysts working for the Big-12 banks are statistically significantly different from the forecasts of their counterparts in each of the three periods. However, the differences are economically trivial. For example, the difference between the mean forecast bias of Big-12 analysts and other analysts after the Global Settlement is -0.05 percent of the share price. Furthermore, the bias in forecasts of Big-12 analysts is lower than the bias in forecasts of other analysts in all three periods by about the same amount of 0.05-0.08 percent of the stock price, implying that the changes in forecast bias of Big-12 and other analysts following Reg FD and the Global Settlement are similar.

3.4. Forecast dispersion and forecast accuracy after new regulations

To the extent that analysts change their behavior after Reg FD and the Global Settlement, other characteristics of their earnings forecasts, for example, forecast dispersion and forecast accuracy, may also change. Similar to Diether, Malloy, and Scherbina (2002), I define forecast dispersion as the standard deviation of earnings forecasts used in the calculation of the consensus forecast scaled by the absolute value of the consensus of earnings forecasts, then multiplied by 100. I retrieve both variables from the I/B/E/S summary file. Following, Agrawal, Chadha, and Chen (2006) and Francis, Nanda, and Wang (2006), forecast accuracy is defined as absolute forecast bias.

Table 6 examines forecast dispersion within each subperiod. Univariate analysis shows that in recent years, forecast dispersion has declined only for short-term forecasts.

The pattern of changes in forecast accuracy after the Global Settlement shown in Table 7 is similar to that of forecast bias. Right after Reg FD, earnings forecast becomes more accurate for short-term forecasts, but less accurate for long-term forecasts. It seems that analysts failed to anticipate the economic downturn in 2001. After the Global Settlement, analyst earnings forecast accuracy improves drastically. The reduction in analysts' conflicts of interest after 2002 helps not only reduce forecast bias, but also improve forecast accuracy.

4. Forecast bias for non-U.S. stocks

Here, I examine the impact of Reg FD and the Global Settlement in the international markets. The impact of Reg FD on non-U.S. analysts' forecasts for non-U.S. stocks traded in international markets is unlikely to be significant because Reg FD does not directly influence international markets. Moreover, rule 243.101(b) of Reg FD explicitly excludes foreign issuers trading on U.S. exchanges, (i.e., firms traded as ADRs).

In contrast, I expect that the impact of the Global Settlement and the SRO rules do go beyond U.S. securities markets. Large multinational banks employ research groups that often contribute to equity analysis abroad. Although the Global Settlement directly affects research reports only for U.S. stocks, it is likely that the settlement will also affect how large banks operate abroad. For example, Goldman Sachs describes its policy for investment research as follows: "The firm is subject to a 'Global Research Settlement' entered by a United States District Court on October 31, 2003. Whilst not required to do

so, the firm has applied the terms on a global basis, subject to limited variations in response to local market practices outside the United States...”¹⁹

Furthermore, NASD requires all of its members—that is, all securities firms doing business with the U.S. public—and members’ employees, including foreign associates, to comply with its new rules. The definition of “research report” in the SRO rules includes research on non-U.S. companies, and it is not limited to communications with U.S. investors. In the NASD Notice to Members 05-24, NASD specifies,

“NASD has observed that members with global operations sometimes produce research reports under a single global brand name or jointly with a research analyst employed by a non-member affiliate—i.e., a ‘mixed team’ research report. NASD and NYSE have deemed such research reports to be attributable to the member and therefore subject to the applicable requirements of Rule 2711.”

The risk of being prosecuted for practices tied to analysts’ conflicts of interest is likely to increase after the Global Settlement, even for local banks in foreign countries. The improved investor protection in the United States has prompted foreign regulators to introduce similar rules. For example, regulators in Japan, Canada, the United Kingdom, Australia²⁰, and Hong Kong²¹ have implemented new rules to curb analysts’ conflicts of interest. The International Organization of Securities Commissions (IOSCO) and the European Union Forum Group (EU) also released guidelines related to this issue.

¹⁹ Goldman Sachs, http://www2.goldmansachs.com/client_services/global_investment_research/ukpolicy.html. (September 2007).

²⁰ See “Joint report by NASD and NYSE on the operation and effectiveness of research analyst conflict of interest rules,” December 2005. Available at <http://www.nyse.com/pdfs/rajointreport.pdf>.

²¹ See p. 131, Bolland (2007).

4.1. Preliminary results for non-U.S. stocks

Similar to La Porta, Lopez-de-Silanes, Shleifer, and Vishny (2002), henceforth referred to as LLSV (2002), I compute median forecast bias by country. To compute median forecast bias, first, for each firm in each month from each forecast period end date, I compute median forecast bias within each analyst. Second, I calculate a median of the medians from the first step across analysts who follow the same firm and make forecasts for the same forecast period end date in the same month. The second step yields 908,113 firm-month observations for non-U.S. stocks from 40 countries for forecast period end dates between 1991 and 2006.

Table 8 summarizes non-U.S. analysts' forecast bias in three sub-periods. Panel A shows the medians of forecast bias in countries with civil law, and Panel B shows the medians of forecast bias in countries with common law. The medians of all countries in the sample are reported at the bottom of Panel B.

Prior to Reg FD, analysts' earnings forecasts were too optimistic worldwide. The medians of forecast bias before Reg FD were 0.22% and 1.24% of stock price for forecasts of current year and subsequent year earnings respectively. Forecasts are overly optimistic in all countries for forecasts made more than 12 months before the forecast period end date, and in 37 out of 40 countries for forecasts made less than 12 months before the forecast period end date.

Overall, forecast bias increased after Reg FD, but then declined sharply after the Global Settlement. After the Global Settlement, forecast bias of forecasts for current year earnings and subsequent year earnings became -0.11% and -0.29% of stock price respectively. The levels of forecast bias after the Global Settlement are lower than those

before Reg FD in 36 out of 40 countries for forecasts made more than one year ahead, and in 35 out of 40 countries for forecasts made within one year from forecast period end date.

These 40 countries in this sample are the countries for which LLSV (1998) provide information on legal variables. I use legal origin, anti-director rights index, accounting standards index, and the legal enforcement index as proxies for investor protection. Leuz, Nanda, and Wysocki (2003) define the legal enforcement index as the mean score across three legal variables used in LLSV (1998): efficiency of judicial system, rule of law, and corruption. The list of the 40 countries with corresponding legal variables is located in Appendix B.²²

The degree to which conflicts of interest occur may vary across countries because each country has different legal institutions. Common law is usually associated with better legal institutions that provide greater investor protection. Greater investor protection may lead to lower analysts' conflicts of interest.

The anti-director rights index ranges from zero to five and represents minority shareholder rights. Regulators that give more rights to minority shareholders may also try to reduce analysts' conflicts of interests more purposefully, as a way to help small and less educated investors. I therefore expect to see low forecast bias in countries with higher anti-director rights indices.

Higher values for the accounting standards index represent more transparency, which helps reduce forecast bias.²³ Making inflated earnings forecasts without the risk of

²² I drop forecasts from Sri Lanka because we do not have forecasts after October 2000 from this country.

²³ La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1998) created an index for accounting standards for each country based on the inclusion or omission of 90 items in the 1990 annual report of firms in each country.

losing their reputation can be easier for analysts when investors do not know the components that drive the actual earnings, since in such circumstances analysts can spend less effort rationalizing their forecasts. Similar to Hope (2003) who finds a positive relation between annual report disclosures and absolute forecast errors, I expect forecast bias to be low in countries with more transparent accounting disclosures.

In countries where corruption is a norm and prosecution procedures are ineffective, conflicts of interest tend to be exacerbated. Consequently, I expect to see more biased forecasts in countries with low legal enforcement.

I use the means of the legal variables presented at the bottom of Appendix B to categorize each country into high and low investor protections based on legal origin, minority shareholder rights, accounting standards, and legal enforcement. I then calculate medians of forecast bias for each country type in Panel C of Table 8.

The results presented in Panel C appear to support the notion that higher investor protection leads to lower analysts' conflicts of interest and, consequently, to lower forecast bias. Before Reg FD, countries with common law, higher minority shareholder rights, higher accounting standards, and greater legal enforcement had lower forecast bias.

In Panel D, the tests for differences in medians show that the differences between each country type, based on legal institutions, are statistically significant for subsequent year earnings forecasts. For current year earnings forecasts, the differences are statistically significant only when I compare countries with high accounting standards to countries with low accounting standards, and countries with greater legal enforcement to countries with little legal enforcement.

For current-year earnings forecasts, Panel C and Panel D of Table 8 show that following the Global Settlement, forecast bias in countries with weaker investor protection was not higher than forecast bias in countries with stronger investor protection. Interestingly, for subsequent-year earnings forecasts, I also find that after the Global Settlement, forecast bias in countries with civil law, low minority shareholder rights, low accounting standards, and little legal enforcement declined more and was no longer higher than forecast bias in countries with common law, high minority shareholder rights, high accounting standards, and greater legal enforcement.

In conclusion, the Global Settlement and other related regulations have more impact in countries with lower investor protections. The influence of external legal institutional change can, to some extent, help equalize the effectiveness of legal institutions worldwide. This finding is similar to that of Bushman, Piotroski, and Smith (2005) who find that the improvement of new insider trading regulations is greater for countries with weaker investor protection.

4.2. Regression analysis for non-U.S. stock

To formally examine whether the impact of Reg FD and the Global Settlement on forecast bias varies across countries with different legal institutions, like LLSV (2002), I use random firm effects to estimate all regressions in this section because there is neither within-country nor within-firm variation in the legal variables. Specifically, I estimate the following random country effects regression:

$$\begin{aligned}
BIAS_{i,m,t} = & \alpha_0 + \alpha_1 REGFD_{m,t} + \alpha_2 Glob_{m,t} + \alpha_3 LegalVariable_i + \alpha_4 (LegalVariable_i \times REGFD_{m,t}) \\
& + \alpha_5 (LegalVariable_i \times Glob_{m,t}) + \alpha_6 ActualGDP_t + \alpha_7 UnexpectedGDP_{m,t} \\
& + \alpha_8 Month_m + \varepsilon_{i,m,t} + v_i.
\end{aligned} \tag{5}$$

To avoid giving too much weight to countries with a large number of firms, such as Canada and Japan, I compute mean forecast bias across all firms within each country-month. The final sample used in the regression analysis consists of 11,553 country-month observations from 40 countries. Here, $BIAS_{i,m,t}$ is the average forecast bias of all forecasts made m months before the forecast period end date in year t for country i . If forecast bias declined after Reg FD, the sign on the coefficient of $RegFD_{m,t}$ will be negative. And if the forecast bias declined after the Global Settlement, the sign on the coefficient of $Glob_{m,t}$ will be negative. If greater investor protection leads to lower forecast bias, the coefficients of $LegalVariable_i$ will be negative. And if the Global Settlement reduced forecast bias more in countries that originally had lower investor protection, then I will observe a positive coefficient for $(LegalVariable_i \times Glob_{m,t})$.

The estimation results for regression (5) are shown in Table 9. I find that the coefficients of $RegFD_{m,t}$ are not statistically significant regardless of the types of legal variables used in the regressions, meaning that Reg FD does not have an impact on forecast bias outside the United States. This result is not surprising given that there is no direct link between Reg FD and analysts' behavior in international markets. Reg FD helps reduce forecast bias in the U.S., but not in foreign countries, because Reg FD deals only with U.S. corporate disclosures. Moreover Rule 243.101(b) of Reg FD also explicitly excludes foreign issuers trading on U.S. exchanges, for example, ADRs.

The coefficients of $Glob_{m,t}$ are positive and statistically significant regardless of the types of legal variables used in the regressions, meaning that the adoption of Global Settlement is associated with a reduction in forecast bias of non-U.S. stocks. Depending on the type of legal variables used in the regressions, the decrease in forecast bias after the Global Settlement ranges from 1.96% to 5.07% of stock prices.

There are several possible explanations for the decline in forecast bias in non-U.S. markets after the Global Settlement. First, the Global Settlement has influence on the Big-12 banks' operations in non-U.S. markets, especially on non-U.S. analysts who contribute to reports that are disseminated to U.S. investors. Second, non-U.S. analysts who work for NASD members must follow NASD rule 2711. Finally, around the Global Settlement, regulators in foreign countries have enacted their own regulations to deal with analysts' conflicts of interest.

Similar to U.S. analysts, non-U.S. analysts cannot accurately predict real GDP growth rates and their impact on corporate earnings. On average, a one percentage point drop in actual real GDP growth rate will raise forecast bias by around 0.28 % of stock price. The walk-down trend is also apparent in international markets. Forecast bias is high for earlier forecasts and becomes lower through time. On average, forecast bias increases by 0.12% of stock price for each month farther away from the forecast period end.

Table 9 also reports negative coefficients of common law, higher minority shareholder rights, higher accounting standards, and better legal enforcement. However, only the coefficients of accounting standards and legal enforcement are statistically

different from zero, meaning that higher levels of accounting disclosure and stronger legal enforcement lead to lower forecast bias.

Interestingly, I find positive and statistically significant coefficients for the interactions terms between the Global Settlement indicator and common law, minority shareholder rights, and accounting standards, but not legal enforcement. This finding implies that the Global Settlement and other related regulations addressing the analysts' conflicts of interest issue have more impact on analysts' forecasts in countries with lower investor protections. However, it seems that new rules and regulations are unlikely to change analysts' behavior in countries with low legal enforcement. The issue of analysts' conflicts of interest remains an important problem in countries with inefficient judicial systems, incompetent rules of law, and high corruption. Analysts in these countries may not be afraid to be prosecuted for their illegal practices because law enforcement institutions in their countries are ineffective.

4.3. The role of analysts who work for the Big-12 bank on reduction in forecast bias

As I discuss earlier, the Big-12 banks may choose to adhere to the highest standard available, i.e. the rules and regulations enacted by U.S. regulators, on their operation abroad. If so, then I should expect a greater change in analyst behavior in countries with higher presence of analysts who work for the Big-12 banks. To examine this issue, I estimate the following regression.

$$\begin{aligned}
BIAS_{i,m,t} = & \alpha_0 + \alpha_1 REGFD_{m,t} + \alpha_2 Glob_{m,t} + \alpha_3 LegalVariable_i + \alpha_4 (LegalVariable_i \times REGFD_{m,t}) \\
& + \alpha_5 (LegalVariable_i \times Glob_{m,t}) + \alpha_6 \#BigBank_i + \alpha_7 (\#BigBank_i \times REGFD_{m,t}) \\
& + \alpha_8 (\#BigBank_i \times Glob_{m,t}) + \alpha_9 ActualGDP_t + \alpha_{10} UnexpectedGDP_{m,t} \\
& + \alpha_{11} Month_m + \varepsilon_{i,m,t} + v_i.
\end{aligned} \tag{6}$$

To measure the presence of analysts, $\#BigBank_i$, who work for the Big-12 banks in country i in each month from each forecast period end year, I compute the number of forecasts made by analysts working for the Big-12 banks, divided by the total number of forecasts in that month. If the presence of the Big-12 banks help reduce forecast bias, I will observe negative coefficient of $\#BigBank_i \times Glob_{m,t}$. I also measure $\#BigBank_i$ by the number of analysts as an alternative specification.

Table 10 shows that, in each country, on average the forecasts from analysts who work for the Big-12 banks account for 28% of total forecasts before Reg FD and after the Global Settlement. The forecasts made by analysts who work for the Big-12 banks account for 32% of total forecasts between Reg FD and the Global Settlement. Meanwhile, analysts who work for the Big-12 banks account for 36% to 37% of total number of analysts in each subperiod. It seems that analysts who work for Big-12 banks make forecasts less often than other analysts.

As for the U.S. markets, before the Global Settlement, the presence of the Big-12 banks accounts for around 40 %, when measured both by the number of forecasts and by the number of analysts. The presence of the Big-12 banks declines to 34% and 32% when measured by the number of forecasts and the number of analysts respectively. This result is not surprising given that the settlement requires the Big-12 banks to pay for independent third-party research reports and to make them available to investors.

When I measure the presence of the Big-12 banks by the number of forecasts and the number of analysts, I show the estimation results for regression (6) in Panel A and Panel B of Table 11 respectively. The results in both tables show that the presence of the Big-12 banks does not influence the level of forecast bias in foreign countries before the Global Settlement.

Interestingly, the coefficients of $\#BigBank_i \times Glob_{m,t}$ are negative regardless of the types of legal variable used in the regression, meaning that forecast bias declined more in countries that have a stronger Big-12 banks presence. Note that the coefficients on $\#BigBank_i \times Glob_{m,t}$ are statistically significant at 5% level when I use common law and anti-director rights index as legal variable and at the 10% level when I use accounting standards index and legal enforcement index as legal variables.

To show that the results here are robust to the definition of forecast bias, I estimate the following regression.

$$\begin{aligned}
 Optimistic_{i,m,t} = & \alpha_0 + \alpha_1 REGFD_{m,t} + \alpha_2 Glob_{m,t} + \alpha_3 LegalVariable_i + \alpha_4 (LegalVariable_i \times RegFD_{m,t}) \\
 & + \alpha_5 (LegalVariable_i \times Glob_{m,t}) + \alpha_6 \#BigBank_i + \alpha_7 (\#BigBank_i \times RegFD_{m,t}) \\
 & + \alpha_8 (\#BigBank_i \times Glob_{m,t}) + \alpha_9 ActualGDP_t + \alpha_{10} UnexpectedGDP_{m,t} \\
 & + \alpha_{11} Month_m + \varepsilon_{i,m,t} + \nu_i.
 \end{aligned} \tag{7}$$

For each forecast, $Optimistic_{i,m,t}$ takes value one when actual EPS > forecast, 0.5 when actual EPS = forecast, and zero when actual EPS < forecast. $Optimistic_{i,m,t}$ is the average $Optimistic_{i,m,t}$ of all forecasts made m months before the forecast period end date in year t for country i . $Optimistic_{i,m,t}$ is a scale free variable. Figure 2 shows that the distribution of

$Optimistic_{i,m,t}$ is close to the normal distribution.²⁴ To be included in the final sample, I require that there be at least 5 firms in that country-month.

Table 12 confirms that the presence of the Big-12 banks correlates with the reduction of forecast bias after the Global Settlement. Here the coefficients of $\#BigBank_i \times Glob_{m,t}$ are negative and statistically significant at the 5% level regardless of the types of legal variable used. It seems that a change in regulatory environment in the U.S. can influence market participants abroad even if non-U.S. regulators do not follow actions of U.S. regulators.

4.4 The 12 banks in the Global Settlement vs. non-12 banks in foreign markets

Table 13 reports that forecasts made by analysts who worked for the Big-12 banks are not more overly optimistic than forecasts made by their counterparts in all months in all three periods. When examining the mean forecast bias of each analyst group across all months in all three periods, I find that the differences between these analyst groups are statistically significant, but very small, equal to or less than 0.02% of stock price. The levels of forecast bias of analysts at the Big-12 banks were 1.59%, 2.31%, and 0.08% of stock prices while the levels of forecast bias of analysts at other banks were 1.61%, 2.33%, and 0.08% of stock prices in three sub-periods respectively. These results imply that analysts' conflicts of interest are a widespread phenomenon in the financial industry, not limited to the Big-12 banks, nor to the U.S. market.

²⁴ The alternative for $Optimistic_{i,m,t}$ is to scale $BIAS_{i,m,t}$ by book value of equity per share. However, reliability of accounting number and the accounting standards differ across countries.

5. Conclusion

Analysts' conflicts of interest are evident worldwide and not limited to the 12 banks involved in the Global Analyst Research Settlement. In the U.S., Reg FD helped analysts reduce their dependence on insider information and thereby diminished analysts' motives to favor firm managers by inflating their earnings forecasts. However, Reg FD subdued only U.S. analysts' overly optimistic earnings forecasts; it had little or no effect on their non-U.S. counterparts.

On the other hand, the Global Analyst Research Settlement and other related regulations enacted by both U.S. and international regulators have affected analyst behavior worldwide. After the Global Settlement, forecast bias declined both in the United States and in international markets. Although it is not possible to disentangle the impact of Global Settlement from several rules and regulations that also aim to mitigate analysts' conflicts of interest, forecast bias clearly declined around the announcement of the Global Settlement. It seems that the recent efforts of U.S. and international regulators have helped reduce analysts' conflict of interest.

The reduction in forecast bias is more apparent in countries with civil law, low minority shareholder rights, and low accounting standards scores. Furthermore, forecast bias declined more in countries that have a greater number of forecasts made by analysts working for the Big-12 banks positively. These results indicate that new rules and regulations in one country can potentially influence market participants abroad.

Appendix A: List of available quarterly GDP by country

Country (with civil law)	Year	Country (with common law)	Year
Argentina	1993-2005	Australia	1991-2006
Austria	1991-2005	Canada	1991-2006
Belgium	1991-2005	Hong Kong	1991-2006
Brazil	1991-2005	India	1999-2006
Chile	1996-2005	Ireland	2001-2006
Colombia	1994-2007	Israel	1995-2006
Denmark	1990-2005	Malaysia	2000-2006
Egypt	2001-2006	New Zealand	1991-2006
Finland	1991-2006	Singapore	1991-2006
France	1991-2006	South Africa	1991-2006
Germany	1991-2006	Thailand	1993-2006
Greece	2000-2006	United Kingdom	1991-2006
Indonesia	2000-2006	United States	1991-2006
Italy	1991-2006		
Japan	1994-2006		
Korea	1991-2006		
Mexico	1991-2005		
Netherlands	1991-2006		
Norway	1991-2006		
Peru	1992-2006		
Philippines	1991-2006		
Portugal	1995-2006		
Spain	1995-2006		
Sweden	1993-2006		
Switzerland	1991-2006		
Taiwan	1991-2006		
Turkey	1991-2006		
Venezuela	1997-2006		

Appendix B: Legal Institutions by Countries

The definitions of legal variables are from LLSV (1998). Common law dummy equals one when forecasts are from countries with common law, zero otherwise. Anti-director rights index represents minority shareholder rights, ranging from zero to five. Accounting standards index measures the inclusion or omission of 90 items in annual reports. Leuz, Nanda and Wysocki (2003) define legal enforcement index as the mean score across three legal variables used in LLSV (1998), i.e., efficiency of judicial system, rule of law and corruption.

Panel A: Civil-law countries

Country	Common Law	Anti-director index	Accounting standards index	Legal enforcement index
Argentina	0	4	45	5.79
Austria	0	2	54	9.4
Belgium	0	0	61	9.4
Brazil	0	3	54	6.13
Chile	0	5	52	6.52
Colombia	0	3	50	4.78
Denmark	0	2	62	10
Egypt	0	2	24	4.85
Finland	0	3	77	10
France	0	3	69	8.7
Germany	0	1	62	9.1
Greece	0	2	55	6.8
Indonesia	0	2		2.9
Italy	0	1	62	7.1
Japan	0	4	65	9.2
Korea	0	2	62	5.6
Mexico	0	1	60	5.37
Netherlands	0	2	64	10
Norway	0	4	74	10
Peru	0	3	38	4.65
Philippines	0	3	65	3.5
Portugal	0	3	36	7.2
Spain	0	4	64	7.1
Sweden	0	3	83	10
Switzerland	0	2	68	10
Taiwan	0	3	65	7.4
Turkey	0	2	51	4.79
Venezuela	0	1	40	5.86

Panel B: Common-law countries

Country	Common Law	Anti-director index	Accounting standards index	Legal enforcement index
Australia	1	4	75	9.5
Canada	1	5	74	9.8
Hong Kong	1	5	69	8.9
India	1	5	57	5.6
Ireland	1	4		8.4
Israel	1	3	64	7.72
Malaysia	1	4	76	7.7
New Zealand	1	4	70	10
Pakistan	1	5		3.7
Singapore	1	4	78	8.9
South Africa	1	5	70	6.4
Thailand	1	2	64	4.9
United Kingdom	1	5	78	9.2
Mean for all Countries	0.33	3.1	61.74	7.44

Figure 1: Timeline of analyst forecasts

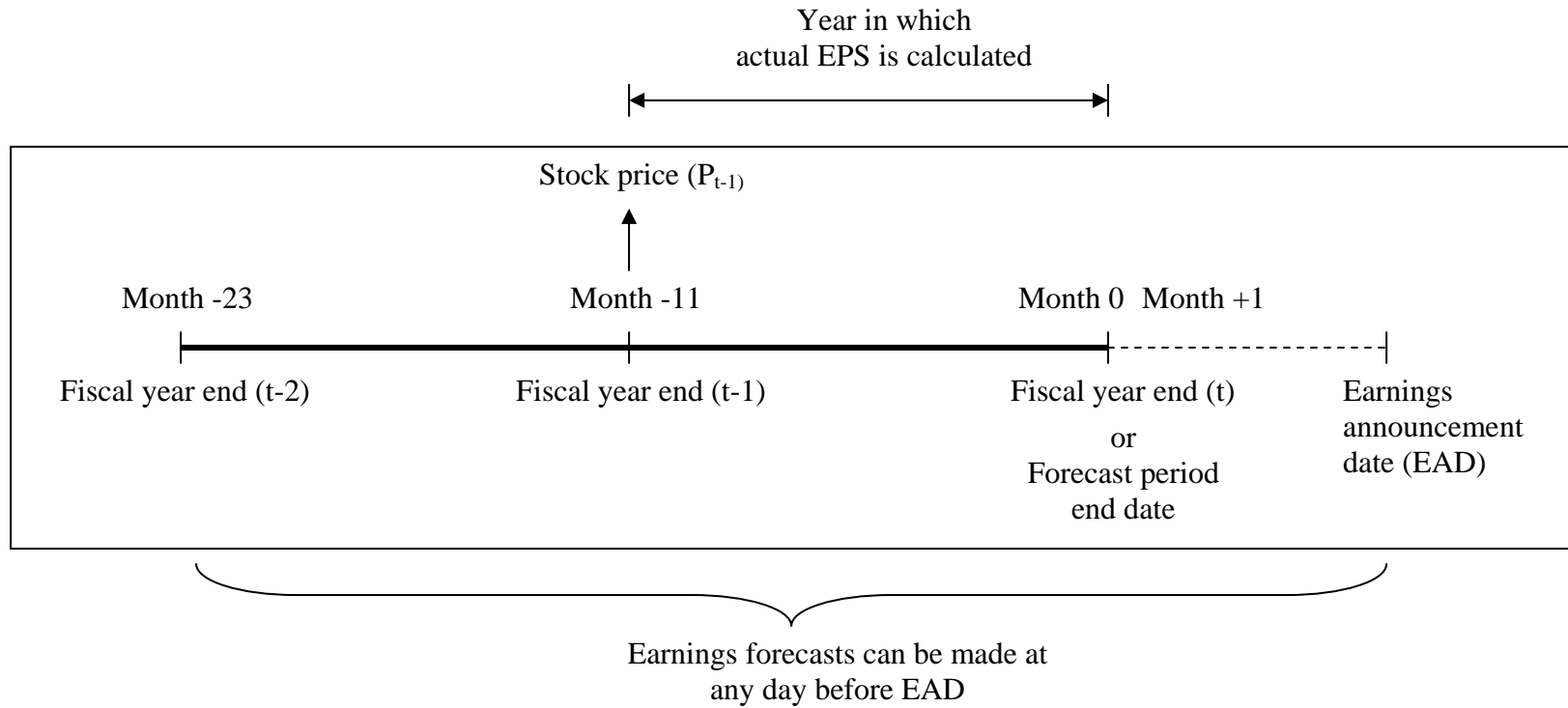


Figure 2: Kernel density estimate of *Optimistic* variable

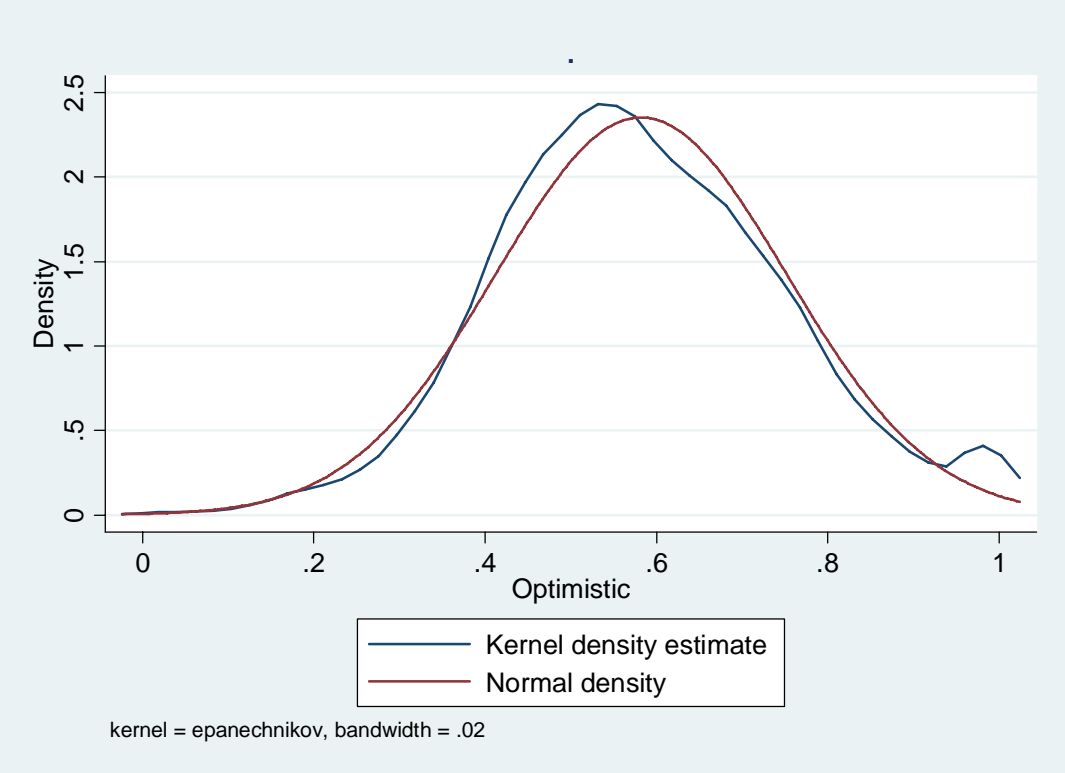


Table 1: Forecast Bias by Forecast Month and Subperiod in the U.S.

Earnings forecasts are for 1984-2006. The sample is divided into three sub-periods: before the implementation of Reg FD (October 23, 2000), between Reg FD and the Global Settlement, and after the Global Settlement (GS) (December 20, 2002). Month is the month of the forecast relative to the fiscal year end.

Panel A: Forecast bias is the difference between the mean of all forecasts made in a particular month for a particular firm and a particular fiscal year and the realized EPS, scaled by the stock price and multiplied by 100.

Month	Forecast bias of earnings forecasts			Number of firm-fiscal year-month observations		
	Before++ Reg FD	Between++ Reg FD and GS	After== GS	Before Reg FD	Between Reg FD and GS	After GS
-23	2.69	5.12**	0.29**	724	185	187
-22	3.03	4.42**	1.09**	15,035	2,200	3,930
-21	3.25	4.14**	1.09**	20,002	2,307	3,916
-20	3.30	4.12**	1.13**	23,205	3,266	5,186
-19	3.30	3.95**	1.19**	23,678	2,724	4,602
-18	3.26	3.68**	1.02**	21,850	2,312	3,850
-17	3.04	3.36**	0.98**	25,521	3,633	5,363
-16	3.01	3.54**	1.04**	25,939	2,897	4,918
-15	2.89	2.78	0.90**	23,134	2,623	4,088
-14	2.63	2.21**	0.80**	25,800	3,946	5,814
-13	2.54	2.61	0.94**	25,896	4,586	5,222
-12	2.36	2.52	0.63**	23,132	2,639	4,190
-11	2.30	2.47	0.90**	17,546	2,209	4,681
-10	1.87	2.01	0.54**	24,069	3,206	6,756
-9	1.88	1.81	0.59**	26,567	2,936	6,141
-8	1.69	1.49**	0.47**	28,729	4,224	7,096
-7	1.66	1.58	0.56**	29,202	3,442	6,574
-6	1.52	1.27**	0.32**	25,565	2,780	5,059
-5	1.19	0.88**	0.24**	29,062	4,334	7,104
-4	1.12	0.86**	0.38**	30,101	3,453	6,741
-3	0.97	0.53**	0.18**	25,614	2,955	5,165
-2	0.69	0.27**	0.12**	28,112	4,496	6,765
-1	0.61	0.36**	0.20**	28,641	5,373	6,151
0	0.50	0.23**	0.13**	24,188	2,781	4,398
1	0.41	0.11**	0.07**	20,006	2,541	5,409
All months	1.98	1.99	0.61**	591,318	78,048	129,306

++ All forecast bias in this period is statistically different from 0, significant at the 1% level for all months

== All forecast bias in this period is statistically different from 0, significant at the 1% level for all months, except month 23 in which the mean forecast bias is not statistically different from zero.

* Forecast bias in this period is statistically different from that before Reg FD, significant at the 5% level

** Forecast bias in this period is statistically different from that before Reg FD, significant at the 1% level

Panel B: Forecast bias is the difference between the mean of all forecasts made in a particular month for a particular firm and a particular fiscal year and the realized EPS, scaled by book value of equity per share and multiplied by 100.

Month	Forecast errors of earnings forecasts			Number of firm-month observations		
	Before++ Reg FD	Between++ Reg FD and GS	After== GS	Before Reg FD	Between Reg FD and GS	After GS
-23	4.60	9.34**	-0.07**	716	182	187
-22	4.75	7.06**	2.45**	14,782	2,158	3,842
-21	4.95	6.58**	2.53**	19,623	2,263	3,816
-20	5.07	6.70**	2.53**	22,734	3,213	5,067
-19	5.06	6.53**	2.87**	23,177	2,663	4,477
-18	4.87	6.14**	2.53**	21,388	2,270	3,750
-17	4.81	5.68**	2.29**	25,018	3,561	5,265
-16	4.72	5.75**	2.55**	25,407	2,840	4,817
-15	4.56	4.68	2.33**	22,693	2,592	4,007
-14	4.21	4.09	2.16**	25,325	3,883	5,718
-13	4.14	4.58**	2.70**	25,417	4,468	5,131
-12	3.87	5.54**	2.04**	22,709	2,556	4,119
-11	4.02	4.67**	2.08**	17,209	2,137	4,609
-10	3.32	4.23**	1.48**	23,651	3,121	6,667
-9	3.29	3.83**	1.55**	26,112	2,862	6,034
-8	3.05	3.47**	1.17**	28,230	4,142	7,013
-7	3.00	3.50**	1.38**	28,677	3,360	6,492
-6	2.72	2.78	0.92**	25,145	2,731	5,010
-5	2.23	1.94*	0.66**	28,677	4,267	7,049
-4	2.12	1.77**	0.90**	29,654	3,398	6,678
-3	1.75	1.03**	0.54**	25,285	2,922	5,135
-2	1.29	0.67**	0.34**	27,821	4,453	6,732
-1	1.17	0.78**	0.48**	28,356	5,290	6,123
0	0.92	0.59**	0.33**	23,940	2,736	4,380
1	0.80	0.47**	0.23**	19,800	2,497	5,388
All Months	3.27	3.64**	1.53**	581,546	76,565	127,506

++ All forecast bias in this period is statistically different from 0, significant at the 1% level for all months

== All forecast bias in this period is statistically different from 0, significant at the 1% level for all months, except month 23 in which the mean forecast bias is not statistically different from zero.

* Forecast bias in this period is statistically different from that before Reg FD, significant at the 5% level

** Forecast bias in this period is statistically different from that before Reg FD, significant at the 1% level

Table 2: Forecast Bias by Fiscal Year and Forecast Month in the U.S

Forecast bias is the difference between the mean of all forecasts made in a particular month for a particular firm and a particular fiscal year and the realized EPS, scaled by the stock price and multiplied by 100. Year is the fiscal year for which the forecast is made. Month is the month of the forecast relative to the fiscal year end. FD is the month in which Reg FD became effective (October 2000). GS is the month in which the Global Settlement was announced (December 2002). Stock returns and PE ratios are calculated from our samples. The growth rates of real GDP are in percentage terms.

Panel A: Mean forecast bias by month for each forecast period end year

Month	Forecast Period End Year																						
	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	00	01	02	03	04	05	06
-23	7.2	1.3	16.7	20.3	4.3	-1.5	3.0	4.4	2.0	2.5	0.6	0.1	1.5	1.8	2.6	5.1	0.2	4.7	5.9	3.6	0.5	1.9	-0.1
-22	2.5	4.8	3.9	2.2	1.7	2.4	2.9	4.3	3.7	2.9	2.0	1.8	2.2	2.1	3.1	4.0	2.8	4.9	5.3	3.6	0.8	1.1	0.8
-21	2.3	4.5	3.9	3.1	2.6	2.5	3.3	4.8	3.8	3.0	2.2	2.5	2.0	2.2	3.3	4.6	2.9	4.7	4.8	3.3	0.8	1.0	1.1
-20	2.3	4.9	3.9	3.2	2.1	2.5	3.2	4.8	3.7	2.9	2.1	2.4	2.4	2.4	3.4	4.2	3.0	5.0	4.3	3.6	0.7	1.2	0.9
-19	2.2	4.5	4.1	2.6	2.3	2.9	3.4	4.8	3.5	2.8	2.2	2.7	2.4	2.6	3.4	4.2	3.2	4.5	4.1	3.6	0.8	1.4	1.0
-18	2.2	4.4	4.0	2.9	2.3	2.7	3.5	4.8	3.5	2.7	1.9	2.5	2.3	2.2	3.4	4.2	3.0	4.9	3.8	3.4	0.4	1.1	1.0
-17	2.0	4.2	3.6	2.7	2.6	2.8	3.2	4.5	3.3	2.5	1.7	1.9	2.1	2.2	3.1	3.8	2.8	4.8	3.3	3.1	0.5	1.2	0.7
-16	1.9	4.2	3.3	2.1	2.1	2.8	3.2	4.7	3.3	2.6	1.9	2.3	2.1	2.3	3.3	3.9	2.8	4.6	3.5	3.2	0.7	1.0	0.9
-15	2.2	4.4	3.2	1.9	2.4	3.0	3.1	4.4	3.3	2.3	1.6	2.1	2.1	2.1	2.8	3.2	2.4	4.3	2.4	2.8	0.4	1.1	0.8
-14	2.3	3.7	3.2	2.2	2.2	2.4	3.1	4.0	2.7	2.1	1.5	1.9	2.1	1.7	2.9	2.9	2.3	FD	1.9	2.0	0.4	0.9	0.7
-13	2.1	3.6	3.0	2.0	1.9	2.6	2.9	3.7	2.6	2.0	1.6	2.1	1.9	1.7	2.9	2.8	2.2	4.0	1.6	1.9	0.5	1.1	0.8
-12	2.4	3.4	3.1	1.5	1.9	2.4	2.9	3.6	2.7	1.9	1.2	1.7	1.7	1.4	2.7	2.3	1.7	3.7	1.4	GS	0.2	0.7	0.6
-11	2.0	3.5	2.9	2.0	2.5	2.4	2.5	3.6	2.7	1.7	1.2	1.8	1.6	1.8	2.5	2.3	1.6	3.3	1.5	1.4	0.4	0.8	0.7
-10	1.8	3.0	2.5	1.8	1.7	2.3	2.4	2.8	1.9	1.3	1.1	1.3	1.3	1.4	1.8	1.7	1.3	2.9	1.1	1.0	0.2	0.5	0.3
-9	1.8	2.7	2.8	1.8	1.7	2.4	2.2	2.8	2.0	1.4	1.1	1.3	1.3	1.3	1.9	1.7	1.5	2.4	1.1	0.8	0.4	0.7	0.3
-8	1.9	2.8	2.7	1.8	1.8	2.2	2.0	2.6	1.7	1.2	0.9	1.2	1.0	1.1	1.7	1.3	1.1	2.0	1.0	0.5	0.3	0.5	0.1
-7	2.0	2.5	2.3	2.0	1.8	2.2	2.1	2.5	1.6	1.2	1.0	1.4	1.0	0.9	1.6	1.5	1.1	2.1	1.1	0.7	0.5	0.5	0.2
-6	1.7	2.3	2.3	1.9	1.6	2.3	1.9	2.3	1.4	0.9	0.7	1.2	0.9	0.8	1.4	1.0	1.1	1.5	0.9	0.3	0.3	0.3	0.2
-5	1.6	2.1	2.2	1.5	1.5	1.8	1.7	1.9	1.1	0.8	0.5	0.9	0.7	0.6	0.9	0.8	0.8	1.1	0.7	0.0	0.3	0.2	0.3
-4	1.6	1.5	1.8	1.7	1.6	1.8	1.6	1.5	1.1	0.7	0.6	0.9	0.8	0.6	0.8	0.8	0.9	1.1	0.7	0.5	0.4	0.3	0.2
-3	1.5	1.5	1.5	1.5	1.5	1.6	1.3	1.3	0.7	0.5	0.4	0.7	0.6	0.5	0.5	0.4	0.6	0.6	0.4	0.2	0.2	0.1	0.0
-2	1.2	1.2	1.1	1.1	1.1	1.3	1.2	0.7	0.6	0.3	0.2	0.5	0.3	0.4	0.3	0.4	FD	0.1	0.3	0.1	0.1	0.1	0.0
-1	1.0	1.1	1.1	1.1	0.9	1.0	0.8	0.8	0.5	0.4	0.2	0.4	0.3	0.3	0.3	0.4	0.5	0.2	0.4	0.3	0.1	0.2	-0.2
0	0.8	1.1	0.7	1.1	0.9	0.9	0.7	0.6	0.3	0.2	0.2	0.4	0.2	0.1	0.2	0.0	0.2	0.2	GS	0.3	0.2	0.1	-0.1
1	0.8	1.0	0.8	1.0	0.7	0.7	0.5	0.4	0.2	0.3	0.1	0.1	0.2	0.2	0.1	0.1	0.2	0.1	0.2	0.1	0.0	0.1	-0.6
Mean Bias	2.1	3.0	3.2	2.7	1.9	2.0	2.3	3.1	2.2	1.6	1.2	1.4	1.4	1.4	2.0	2.3	1.7	2.8	2.2	1.7	0.4	0.7	0.4

Panel B: Median forecast bias by month for each forecast period end year

Month	Forecast Period End Year																						
	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	00	01	02	03	04	05	06
-23	7.2	1.5	16.7	20.3	4.1	-0.2	0.8	0.0	0.9	1.1	-0.1	-0.2	0.0	0.4	1.2	1.8	-0.2	1.8	2.4	1.2	-0.2	-0.4	-0.3
-22	0.6	2.2	2.0	0.6	0.4	0.9	1.2	2.1	1.5	1.1	0.7	0.3	0.4	0.5	0.9	1.3	0.6	2.2	2.7	1.3	0.0	-0.1	0.0
-21	0.7	2.2	2.0	0.8	0.6	0.8	1.4	2.2	1.8	1.1	0.7	0.6	0.3	0.5	1.1	1.6	0.6	2.0	2.7	1.3	0.1	0.0	0.2
-20	0.4	2.3	2.0	0.9	0.4	0.7	1.2	2.4	1.6	1.1	0.6	0.5	0.4	0.5	1.2	1.4	0.7	2.2	2.3	1.3	-0.1	0.0	0.0
-19	0.4	2.3	2.0	0.5	0.6	1.0	1.3	2.2	1.5	1.0	0.7	0.6	0.5	0.7	1.2	1.6	0.6	2.1	2.1	1.3	0.0	0.1	0.1
-18	0.5	2.2	1.7	0.6	0.4	0.9	1.3	2.2	1.4	1.1	0.5	0.4	0.5	0.4	1.3	1.4	0.7	2.0	1.9	1.1	-0.1	0.0	0.2
-17	0.5	1.8	1.5	0.6	0.4	1.0	1.2	2.1	1.3	0.9	0.4	0.3	0.4	0.4	1.2	1.1	0.6	2.1	1.4	1.1	-0.1	0.1	0.1
-16	0.4	2.1	1.2	0.4	0.3	1.0	1.2	2.1	1.2	0.9	0.5	0.5	0.5	0.6	1.3	1.3	0.6	2.0	1.5	1.2	-0.1	0.0	0.2
-15	0.7	2.1	1.3	0.3	0.4	1.3	1.1	1.9	1.3	0.8	0.4	0.4	0.4	0.4	1.1	0.9	0.5	1.8	1.0	0.9	-0.2	0.1	0.2
-14	0.7	1.7	1.2	0.3	0.4	0.8	1.3	1.7	0.8	0.7	0.3	0.3	0.4	0.3	1.0	0.6	0.4	FD	0.6	0.4	-0.1	0.0	0.1
-13	0.5	1.6	1.0	0.3	0.3	0.8	1.0	1.5	0.9	0.7	0.3	0.4	0.4	0.3	1.0	0.6	0.4	1.6	0.5	0.3	-0.1	0.1	0.2
-12	0.6	1.4	1.1	0.1	0.2	0.8	1.0	1.3	0.8	0.6	0.2	0.3	0.3	0.2	0.9	0.4	0.3	1.6	0.5	GS	-0.2	-0.1	0.1
-11	0.4	1.6	1.4	0.2	0.3	0.9	0.9	1.2	0.7	0.5	0.2	0.3	0.3	0.3	0.9	0.5	0.2	1.3	0.3	0.2	-0.1	0.0	0.1
-10	0.4	1.2	0.9	0.3	0.2	0.8	0.7	1.0	0.4	0.4	0.1	0.2	0.2	0.2	0.6	0.2	0.2	1.2	0.2	0.0	-0.1	-0.1	-0.1
-9	0.5	1.2	0.9	0.2	0.1	0.6	0.7	0.9	0.5	0.4	0.1	0.2	0.2	0.2	0.6	0.2	0.2	1.1	0.2	0.0	-0.1	0.0	-0.1
-8	0.4	1.2	0.9	0.2	0.2	0.6	0.6	0.9	0.4	0.3	0.0	0.1	0.1	0.1	0.6	0.1	0.1	0.7	0.2	-0.1	-0.1	-0.1	-0.1
-7	0.5	1.1	0.6	0.2	0.3	0.6	0.6	0.8	0.3	0.3	0.1	0.2	0.1	0.1	0.5	0.1	0.1	0.7	0.2	-0.1	0.0	0.0	0.0
-6	0.4	0.9	0.7	0.2	0.2	0.7	0.6	0.6	0.4	0.3	0.1	0.1	0.1	0.1	0.4	0.0	0.1	0.5	0.2	-0.1	-0.1	-0.1	0.0
-5	0.4	0.9	0.6	0.2	0.2	0.5	0.4	0.5	0.2	0.1	0.0	0.1	0.0	0.0	0.2	0.0	0.1	0.2	0.1	-0.1	0.0	-0.1	0.0
-4	0.3	0.5	0.5	0.2	0.2	0.4	0.4	0.4	0.2	0.1	0.0	0.1	0.0	0.0	0.1	0.0	0.1	0.2	0.1	-0.1	0.0	0.0	0.0
-3	0.3	0.5	0.3	0.1	0.2	0.3	0.3	0.3	0.1	0.1	0.0	0.1	0.0	0.0	0.1	0.0	0.0	0.1	0.0	-0.1	-0.1	-0.1	0.0
-2	0.2	0.3	0.2	0.1	0.1	0.2	0.2	0.1	0.1	0.0	0.0	0.0	0.0	0.0	0.0	0.0	FD	0.0	0.0	-0.1	-0.1	0.0	0.0
-1	0.1	0.2	0.1	0.1	0.1	0.1	0.1	0.1	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	-0.1	0.0	0.0	-0.1
0	0.0	0.2	0.0	0.1	0.0	0.1	0.1	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	-0.1	0.0	0.0	GS	-0.1	0.0	0.0	-0.1
1	0.0	0.1	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	-0.1	-0.1	-0.1	-0.3
Median Bias	0.4	1.4	1.0	0.2	0.3	0.7	0.8	1.0	0.7	0.5	0.1	0.2	0.2	0.2	0.9	0.4	0.2	1.3	0.4	0.1	-0.1	0.0	0.0
Median Stk. Ret.%	0.5	29.6	13.1	-5.8	16.9	22.0	-12.5	31.1	10.0	11.8	-6.0	23.6	11.9	16.1	-10.7	0.0	-15.4	-2.1	-21.7	44.7	14.9	3.8	13.0
Median P/E	10.9	10.9	13	13.2	10.5	11.7	14.5	13	15	15.8	15.6	13.6	15.5	15.9	19	16.4	14.9	18.8	19.2	15	17.5	18.2	17.6
GDP %	7.2	4.1	3.5	3.4	4.1	3.5	1.9	0.2	3.3	2.7	4.0	2.5	3.7	4.5	4.2	4.5	3.7	0.8	1.6	2.5	3.9	3.2	3.3

Table 3: Summary statistics for U.S. sample

Forecast bias is the difference between the mean of all forecasts made in a particular month for a particular firm and a particular fiscal year and the realized EPS, scaled by the stock price and multiplied by 100. RegFD indicator equals one when forecasts are made between October 23, 2000 and December 20, 2002. Glob indicator equals one when forecasts are made after December 20, 2002. Analyst coverage is the number of outstanding forecasts used by I/B/E/S to calculate monthly consensus. Market capitalization equals stock price multiplied by shares outstanding. Market-to-book ratio is the market value of equity divided by the book value of common equity. General experience is the number of years since the first day the analyst appeared in I/B/E/S. Firm-specific experience is the number of years since the analyst made her first forecast for a particular stock. Broker size is the number of analysts working for the employer of the analyst who makes the forecast. Litigation risk indicator equals one for firms in high litigation risk industries. Labor Intensive is $(1 - \text{gross PPE} / \text{total gross assets})$.

Description	Variable	# firm-fiscal year -month Observations	Mean	Std. Dev.	Min	Max
Forecast bias	Bias	798,672	1.76	5.12	-10.28	36.51
Reg FD indicator	RegFD	798,672	0.10	0.30	0.00	1.00
Global Settlement indicator	Glob	798,672	0.16	0.37	0.00	1.00
<i>Firm characteristics</i>		798,672				
Analyst coverage	NumA	798,672	8.78	7.33	1.00	38.00
Market capitalization (\$ million)	Market cap	798,672	2,970.0	8,430.0	0.0	111,000.0
Market-to-book ratio	MB	798,672	3.03	2.67	0.03	22.52
Negative EPS	EPSLoss	798,672	0.15	0.36	0.00	1.00
Declining EPS	EPSDecline	798,672	0.34	0.47	0.00	1.00
Litigation	Litigation	798,672	0.24	0.42	0.00	1.00
Labor Intensive	Labor	798,672	0.58	0.26	0.00	1.00
<i>Analyst characteristics</i>		798,672				
Firm-specific experience	YearStk	798,672	2.28	2.32	0.00	23.00
General experience	YearIBES	798,672	5.38	3.65	0.00	24.00
# of stocks covered	NumStk	798,672	18.36	10.43	1.00	86.00
# of industries covered	NumInd	798,672	5.45	2.95	1.00	18.00
Broker size	BrokerSize	798,672	52.85	44.60	1.00	375.00

Table 4: The Impact of Reg FD and the Global Settlement on Forecast Bias in the U.S.

The dependent variable is earnings forecast bias defined as the difference between the mean of all forecasts made in a particular month for a particular firm and a particular fiscal year and the realized EPS, scaled by the stock price and multiplied by 100. RegFD indicator equals one when forecasts are made between October 23, 2000 and December 20, 2002. Glob indicator equals one when forecasts are made after December 20, 2002. Analyst coverage, NumA, is the number of outstanding forecasts used by I/B/E/S to calculate monthly consensus. Firm size is the natural log of firm's market capitalization. Market-to-book ratio, MB, is the market value of equity divided by the book value of common equity. General experience, YearIBES, is the number of years since the first day the analyst appeared in I/B/E/S. Firm-specific experience, YearStk, is the number of years since the analyst made her first forecast for a particular stock. *EPSLoss* indicator equals one when corresponding actual earnings of firm j are negative. The *EPSDecline* indicator equals one when the realized earnings in fiscal year t are lower than the realized earnings in the previous year. Broker size is the number of analysts working for the employer of the analyst who makes the forecast. Litigation risk indicator, LITIG, equals one for firms in high litigation risk industries. Labor intensity, Labor, is $(1 - \text{gross PPE} / \text{total gross assets})$. Specification (1) is our base model. In specification (2), we require that forecasts for each firm-month exist in all three sub-periods. In specification (3), the forecast bias is normalized by the book value of equity per share. In specification (4), *RegFD_{m,t}* dummy equals one when forecasts are made between October 23, 2000 and July 9, 2002, whereas *Glob_{m,t}* equals one when forecasts are made after July 9, 2002. The regressions are estimated with fixed firm effects. The reported t-statistics reflect robust standard errors adjusted for heteroskedasticity and clustering by firm.

Independent Variables	(1)		(2)		(3)		(4)	
	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat
RegFD	-0.35 **	-6.31	-0.16 **	-2.69	-0.41 **	-4.25	-0.30 **	-5.38
Glob	-0.63 **	-12.54	-0.49 **	-8.99	-0.99 **	-10.19	-0.63 **	-12.72
FirmSize	0.10 **	3.91	-0.02	-0.56	0.58 **	11.94	0.10 **	3.96
NumA	0.03 **	9.56	0.02 **	4.37	0.08 **	11.57	0.03 **	9.55
MB	-0.01	-1.63	0.01	0.97	0.46 **	20.50	-0.01	-1.76
YearStk	-0.03 **	-4.57	-0.02 **	-3.50	-0.04 **	-4.76	-0.03 **	-4.67
YearIBES	-0.02 **	-7.06	-0.02 **	-4.91	-0.01 *	-2.48	-0.02 **	-7.18
NumStk	0.00 *	-2.51	0.00	-1.68	0.00 *	-1.97	0.00 **	-2.53
NumInd	0.00	1.01	0.01	1.32	0.01 *	1.96	0.00	0.99
BrokerSize	0.00 **	-3.41	0.00 **	-3.50	0.00 **	-3.80	0.00 **	-3.20
EPSLoss	7.23 **	62.12	5.75 **	32.30	9.57 **	51.68	7.23 **	62.13
EPSDecline	2.55 **	82.95	2.48 **	58.69	4.70 **	69.28	2.55 **	83.06
LITIG	-0.11	-0.71	-0.21	-0.94	-0.12	-0.54	-0.10	-0.68
Labor	0.56 **	2.65	0.71 **	2.74	0.32	0.87	0.55 **	2.63
Actual GDP	-0.05 **	-4.26	-0.03	-1.80	0.01	0.35	-0.05 **	-4.32
Surprise GDP	-0.01	-1.84	-0.02 **	-2.89	0.00	-0.23	-0.01	-1.92
Months	0.15 **	61.91	0.10 **	31.09	0.25 **	53.15	0.15 **	61.79
Adj R-Squared		0.49		0.38		0.48		0.49
Firm-fiscal year-month Obs.		798,672		343,626		785,617		797,120
Number of firms		9,366		2,398		9,241		9,366

** Significant at the 1% level.

* Significant at the 5% level.

Table 5: Comparing Forecast Bias of Big-12 and Other Analysts in the U.S.

Big-12 refers to the analysts working for Big-12 banks covered by the Global Settlement. Forecast bias is the difference between the mean of all forecasts made in a particular month for a particular firm and a particular fiscal year and the realized EPS, scaled by the stock price and multiplied by 100. Month is the month of the forecast relative to the fiscal year end. Earnings forecasts are for 1984-2006. The sample is divided into three sub-periods: before the implementation of Reg FD (October 23, 2000), between Reg FD and the Global Settlement, and after the Global Settlement (GS) (December 20, 2002). To be included in this sample, the covered firms must have forecasts from both analyst groups within the same month.

Month	Before Reg FD					Between Reg FD and Global Settlement					After Global Settlement				
	#Obs.	Big12	NonBig12	Diff	Diff t-stat	#Obs.	Big12	NonBig12	Diff	Diff t-stat	#Obs.	Big12	NonBig12	Diff	Diff t-stat
-23	460	2.40	2.40	0.00	0.07	139	5.18	5.22	-0.04	-0.41	155	-0.18	-0.09	-0.09	-0.82
-22	7,144	2.46	2.46	0.00	-0.07	1,040	3.42	3.54	-0.12	-1.93	2,058	0.64	0.71	-0.08 *	-2.49
-21	8,525	2.57	2.63	-0.06 **	-3.30	873	3.86	3.91	-0.05	-0.55	1,483	0.41	0.55	-0.14 **	-4.07
-20	11,294	2.60	2.61	-0.01	-0.86	1,774	3.53	3.67	-0.14 **	-3.11	2,845	0.51	0.67	-0.16 **	-5.96
-19	10,099	2.60	2.64	-0.04 *	-2.36	1,022	3.27	3.41	-0.15	-1.85	1,701	0.62	0.72	-0.10 **	-2.89
-18	8,568	2.61	2.67	-0.06 **	-2.84	873	3.38	3.54	-0.16 *	-2.57	1,316	0.35	0.41	-0.05	-1.34
-17	11,964	2.43	2.47	-0.04 **	-2.71	2,051	2.93	3.13	-0.20 **	-4.56	3,000	0.43	0.59	-0.15 **	-6.93
-16	10,823	2.44	2.50	-0.06 **	-3.76	1,012	2.68	2.94	-0.26 **	-3.67	1,875	0.68	0.79	-0.12 **	-2.81
-15	8,648	2.42	2.47	-0.05 **	-3.06	1,116	2.09	2.27	-0.18 *	-2.42	1,461	0.37	0.45	-0.07 **	-2.64
-14	12,904	2.19	2.27	-0.08 **	-6.31	2,243	1.74	1.83	-0.10 *	-2.45	3,148	0.35	0.47	-0.12 **	-5.77
-13	10,903	2.07	2.10	-0.04 *	-2.47	1,628	1.91	2.01	-0.10 *	-2.05	2,008	0.44	0.47	-0.04	-1.37
-12	9,121	1.87	1.96	-0.10 **	-5.62	923	2.08	2.13	-0.06	-1.02	1,507	0.25	0.32	-0.07 *	-2.54
-11	7,372	1.80	1.86	-0.06 **	-3.45	1,114	1.86	1.99	-0.13 **	-3.16	2,267	0.29	0.40	-0.11 **	-3.94
-10	11,438	1.36	1.40	-0.04 **	-3.41	1,493	1.53	1.62	-0.09 *	-2.11	3,429	0.26	0.30	-0.04	-1.88
-9	11,238	1.46	1.55	-0.09 **	-6.16	1,121	1.50	1.47	0.03	0.71	2,307	0.31	0.32	-0.01	-0.57
-8	13,723	1.20	1.25	-0.05 **	-4.22	2,350	1.19	1.21	-0.02	-0.82	3,856	0.15	0.19	-0.04 **	-2.82
-7	12,544	1.29	1.34	-0.04 **	-3.51	1,265	1.26	1.31	-0.05 *	-2.05	2,507	0.23	0.24	0.00	-0.18
-6	9,780	1.16	1.22	-0.05 **	-3.93	1,052	1.07	1.10	-0.02	-0.84	1,765	0.14	0.13	0.00	0.17
-5	14,162	0.83	0.85	-0.02 *	-2.36	2,533	0.52	0.57	-0.06 **	-3.67	3,952	0.05	0.09	-0.03 **	-3.08
-4	13,232	0.83	0.86	-0.03 **	-2.88	1,285	0.72	0.76	-0.04	-1.74	2,638	0.25	0.29	-0.04 *	-2.37
-3	9,830	0.76	0.79	-0.04 **	-2.95	1,206	0.35	0.41	-0.06 **	-2.81	1,788	0.14	0.13	0.01	0.58
-2	15,405	0.43	0.44	-0.02	-1.92	2,655	0.12	0.15	-0.02 *	-2.05	3,715	0.03	0.02	0.00	0.42
-1	13,118	0.41	0.43	-0.03 **	-2.88	1,856	0.17	0.15	0.02	1.00	2,307	0.08	0.09	-0.02	-1.20
0	9,534	0.38	0.40	-0.03 *	-2.01	923	0.06	0.08	-0.02	-0.97	1,355	0.03	0.03	0.00	0.19
1	6,823	0.32	0.34	-0.02	-1.16	881	0.02	0.04	-0.03	-1.48	1,801	0.06	0.04	0.01	0.96
All Months	258,652	1.55	1.59	-0.04 **	-15.24	34,428	1.60	1.68	-0.08 **	-9.57	56,244	0.28	0.33	-0.06 **	-12.22

** Significant at the 1% level.

* Significant at the 5% level.

Table 6: Forecast Dispersion by Forecast Month and Subperiod in the U.S.

Forecast dispersion is defined as standard deviation of earnings forecasts used in consensus calculation scaled by the absolute value of consensus and multiplied by 100. Earnings forecasts are from 1984-2006. I divide the sample into three sub-periods: before the implementation of Reg FD on October 23, 2000, between Reg FD and the Global Settlement, and after the Global Settlement (GS) on December 20, 2002. Month is the number of months from forecast date to forecast period end.

Month	Dispersion of earnings forecasts			Number of firm-month observations		
	Before Reg FD	Between Reg FD and GS	After GS	Before Reg FD	Between Reg FD and GS	After GS
-23	12.62	13.46	11.88	522	144	162
-22	13.22	14.65	17.51**	10,831	1,723	3,346
-21	14.95	17.30	16.61	14,815	1,921	3,439
-20	14.88	30.59**	20.52**	17,873	2,636	4,480
-19	16.05	28.21**	19.94*	18,571	2,245	3,970
-18	15.56	30.13**	17.59	17,553	1,991	3,485
-17	15.14	31.15**	16.84	20,630	2,993	4,734
-16	16.92	22.68**	17.46	20,823	2,377	4,274
-15	18.19	31.17**	18.61	18,828	2,277	3,703
-14	17.87	28.00**	18.93	20,963	3,270	5,165
-13	18.57	28.17**	19.69	20,715	3,858	4,615
-12	18.41	27.51**	17.17	18,690	2,373	3,817
-11	20.17	32.36*	20.00	14,145	1,913	4,189
-10	18.58	25.93**	16.20	19,955	2,820	5,879
-9	18.00	27.16*	18.30	21,442	2,586	5,402
-8	21.01	24.91	17.04	23,284	3,620	6,155
-7	20.73	24.93	19.44	23,326	2,936	5,550
-6	19.51	20.50	15.67**	20,570	2,472	4,537
-5	19.81	22.79	14.27**	23,666	3,714	6,167
-4	21.76	18.47*	14.79**	23,857	2,904	5,650
-3	21.06	16.00**	11.24**	20,599	2,616	4,566
-2	19.58	16.17*	12.80**	22,592	3,810	5,760
-1	19.22	13.93**	9.97**	22,410	4,546	5,094
0	20.08	10.92**	7.12**	19,173	2,442	3,819
1	17.71	12.88	8.16**	16,107	2,249	4,809
All Months	18.43	23.00**	15.99**	471,940	66,436	112,767

* Forecast dispersion in this period is statistically different from that before Reg FD, significant at the 5% level.

** Forecast dispersion in this period is statistically different from that before Reg FD, significant at the 1% level.

Table 7: Forecast Accuracy by Forecast Month and Subperiod in the U.S.

Forecast accuracy is defined as absolute forecast bias. Earnings forecasts are from 1984-2006. I divide the sample into three sub-periods: before the implementation of Reg FD on October 23, 2000, between Reg FD and the Global Settlement, and after the Global Settlement (GS) on December 20, 2002. Month is the number of months from forecast date to forecast period end.

Month	Accuracy of earnings forecasts			Number of firm-month observations		
	Before Reg FD	Between Reg FD and GS	After GS***	Before Reg FD	Between Reg FD and GS	After GS
-23	3.76	5.78**	2.04**	724	185	187
-22	4.05	5.33**	2.96**	15,035	2,200	3,930
-21	4.31	5.11**	2.93**	20,002	2,307	3,916
-20	4.38	5.08**	2.92**	23,205	3,266	5,186
-19	4.36	5.07**	2.99**	23,678	2,724	4,602
-18	4.30	4.72**	2.79**	21,850	2,312	3,850
-17	4.10	4.49**	2.68**	25,521	3,633	5,363
-16	4.07	4.63**	2.76**	25,939	2,897	4,918
-15	3.94	3.95	2.59**	23,134	2,623	4,088
-14	3.69	3.59	2.41**	25,800	3,946	5,814
-13	3.62	3.86**	2.52**	25,896	4,586	5,222
-12	3.47	3.55	2.19**	23,132	2,639	4,190
-11	3.37	3.56	2.43**	17,546	2,209	4,681
-10	2.93	3.05	2.11**	24,069	3,206	6,756
-9	2.96	2.79*	2.09**	26,567	2,936	6,141
-8	2.72	2.44**	1.81**	28,729	4,224	7,096
-7	2.62	2.43*	1.89**	29,202	3,442	6,574
-6	2.44	2.02**	1.49**	25,565	2,780	5,059
-5	2.03	1.64**	1.27**	29,062	4,334	7,104
-4	1.95	1.63**	1.35**	30,101	3,453	6,741
-3	1.78	1.22**	1.08**	25,614	2,955	5,165
-2	1.41	0.88**	0.83**	28,112	4,496	6,765
-1	1.31	0.98**	0.84**	28,641	5,373	6,151
0	1.23	0.77**	0.70**	24,188	2,781	4,398
1	1.17	0.66**	0.61**	20,006	2,541	5,409
All Months	2.94	2.91	1.93**	591,318	78,048	129,306

* Forecast dispersion in this period is statistically different from that before Reg FD, significant at the 5% level.

** Forecast dispersion in this period is statistically different from that before Reg FD, significant at the 1% level.

Table 8: Forecast Bias by Country and Subperiod

Forecast bias is defined as forecast minus actual, scaled by stock price, and multiplied by 100. Month refers to the length of time between forecast date and forecast period end date. The definitions of legal variables are from LLSV (1998). Common law dummy equals one when forecasts are from countries with common law, and zero otherwise. The anti-director rights index represents minority shareholder rights, ranging from zero to five. The accounting standards index measures the inclusion or omission of 90 items in annual reports. Leuz, Nanda and Wysocki (2003) define the legal enforcement index as the mean score across three legal variables used in LLSV (1998): efficiency of judicial system, rule of law, and corruption. GS refers to the Global Settlement.

Panel A: Forecast bias for civil-law countries

Country	Number of firm-month observations	Medians for forecast bias Month 1 to -11			Medians for forecast bias Month -12 to -23		
		Before	Between	After	Before	Between	After
		Reg FD	Reg FD and GS	GS	Reg FD	Reg FD and GS	GS
Argentina	4,640	0.57	4.71	1.14	1.84	7.18	3.25
Austria	2,774	0.21	0.28	-0.54	1.96	1.53	-1.67
Belgium	9,508	0.06	0.98	-0.36	1.85	2.62	-0.90
Brazil	16,021	0.40	0.90	0.27	1.02	2.23	0.83
Chile	6,661	0.38	1.03	0.35	1.09	2.71	-0.14
Colombia	400	0.94	6.85	-2.00	1.98	4.75	-5.74
Denmark	11,721	0.04	0.49	-0.55	0.38	1.02	-1.03
Egypt	147	0.51	1.15	-0.57	2.84	12.74	-1.52
Finland	14,990	0.30	0.63	-0.11	3.22	3.50	-0.26
France	43,069	0.22	0.53	-0.37	2.14	1.63	-0.86
Germany	23,506	0.10	0.52	-0.09	1.21	2.65	-0.20
Greece	4,053	-0.38	0.00	-0.05	1.26	2.27	-0.48
Indonesia	7,420	0.00	1.85	0.59	0.32	4.58	0.56
Italy	14,433	0.00	0.49	-0.24	2.39	1.60	-0.79
Japan	141,749	0.24	0.33	-0.03	0.73	1.89	-0.17
Korea	9,347	0.59	1.43	0.75	1.27	3.12	0.88
Mexico	13,096	0.93	1.55	-0.25	2.26	3.10	-1.51
Netherlands	25,601	0.02	0.47	-0.35	1.02	2.98	-0.61
Norway	14,975	0.29	0.96	0.14	1.05	3.86	0.44
Peru	1,319	1.66	1.88	-2.92	5.06	2.69	-3.38
Philippines	3,820	0.26	0.22	-0.13	1.34	0.35	-0.31
Portugal	4,040	0.01	0.87	-0.36	3.23	1.95	-0.76
Spain	19,148	0.09	0.08	-0.25	1.73	0.43	-0.58
Sweden	25,349	0.16	0.61	-0.12	0.85	2.00	-0.51
Switzerland	17,912	-0.03	0.81	-0.17	0.32	2.99	-0.22
Taiwan	9,113	0.44	0.79	-0.13	1.36	2.33	-1.13
Turkey	14,079	2.67	1.28	0.31	5.33	2.71	-0.07
Venezuela	297	2.36	-0.28	0.52	5.75	-0.15	8.48

Panel B: Forecast bias for common-law countries

Country	Number of firm-month observations	Medians for forecast bias Month 1 to -11			Medians for forecast bias Month -12 to -23		
		Before Reg FD	Between Reg FD and GS	After GS	Before Reg FD	Between Reg FD and GS	After GS
Australia	57,724	0.15	0.22	-0.06	0.90	0.70	-0.17
Canada	103,694	0.34	0.27	0.19	1.52	1.21	0.36
Hong Kong	30,208	0.28	0.94	-0.04	1.00	2.45	-0.02
India	14,012	0.23	0.20	-0.13	1.11	1.21	-0.38
Ireland	5,240	-0.06	-0.20	-0.56	0.22	0.27	-1.07
Israel	1,559	0.11	0.32	-0.09	0.47	1.38	-0.41
Malaysia	28,733	0.00	0.27	0.10	0.23	0.89	0.86
New Zealand	10,493	0.20	0.32	-0.14	1.08	0.41	-0.11
Singapore	25,351	0.19	0.49	-0.06	0.73	1.83	-0.09
South Africa	18,799	0.16	0.34	0.02	0.80	1.50	0.14
Thailand	20,626	0.59	0.77	0.27	1.87	-0.19	1.46
United Kingdom	132,486	0.00	0.25	-0.23	0.45	1.47	-0.57
All countries median		0.22	0.52	-0.11	1.24	1.98	-0.29

Panel C: Forecast bias and legal institutions (medians of the medians in Panel A&B) 40 observations for each legal variables

Country with	N	Medians forecast bias Month -1 to 11			Medians forecast bias Month 12 to 23		
		Before	Between	After	Before	Between	After
		Reg FD	Reg FD and GS	GS	Reg FD	Reg FD and GS	GS
Civil law	28	0.25	0.80	-0.13	1.55	2.64	-0.49
Common law	12	0.18	0.30	-0.06	0.85	1.21	-0.10
Low anti-director rights index	25	0.22	0.77	-0.13	1.85	2.33	-0.51
High anti-director rights index	15	0.20	0.32	-0.04	1.00	1.47	-0.11
Low accounting standards index	14	0.45	1.01	-0.19	1.97	2.66	-0.62
High accounting standards index	24	0.18	0.49	-0.10	1.04	1.62	-0.21
Low legal enforcement index	19	0.40	0.90	-0.05	1.84	2.27	-0.31
High legal enforcement index	21	0.16	0.49	-0.12	1.00	1.83	-0.26

Panel D: Test of Medians (z-statistic, Wilcoxon rank-sum test)

Civil versus common law	1.24	2.77***	-0.84	2.85**	3.51**	-1.77*
High versus low anti-director rights index	0.80	2.05**	-1.61	2.75**	1.77*	-2.19**
High versus low accounting standards index	2.33**	2.09**	-0.62	3.33**	2.00**	-1.33
High versus low legal enforcement index	2.42**	1.80*	1.18	3.02**	0.99	0.66

***Significant at 1 % level.

** Significant at 5 % level.

*Significant at 10 % level.

Table 9: Impact of Reg FD and the Global Settlement on Forecast Bias in International Markets.

This table presents the results of random country effect regressions. The dependent variable is earnings forecast bias defined as the difference between forecast and actual EPS, scaled by stock price and multiplied by 100. The explanatory variables include actual GDP growth, unexpected GDP growth, and months from forecast period end. Reg FD dummy equals one when forecasts are made between October 23, 2000 and December 20, 2002. Glob dummy equals one when forecasts are made after December 20, 2002. The detailed definitions of these variables are discussed in Section 4.2. The definitions of legal variables are from LLSV (1998). Common law dummy equals one when forecasts are from countries with common law, and zero otherwise. The anti-director rights index represents minority shareholder rights, ranging from zero to five. The accounting standards index measures the inclusion or omission of 90 items in annual reports. Leuz, Nanda and Wysocki (2003) define the legal enforcement index as the mean score across three legal variables used in LLSV (1998): efficiency of judicial system, rule of law, and corruption.

	Common law		Anti-director rights index		Accounting standards index		Legal enforcement index	
	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat
Reg FD	0.97	1.88	0.79	0.78	-0.33	-0.14	-0.02	-0.01
Glob	-1.96 **	-7.25	-2.82 **	-5.95	-5.05 **	-3.26	-2.87 **	-3.02
LegalVariables	-0.57	-1.21	-0.24	-1.37	-0.05 *	-2.26	-0.40 **	-4.20
LegalVariable×RegFD	-0.36	-0.56	0.02	0.08	0.02	0.50	0.11	0.52
LegalVariable×Glob	1.08 **	3.02	0.40 **	2.99	0.05 *	2.34	0.16	1.46
ActualGDP	-0.27 **	-4.72	-0.27 **	-4.64	-0.27 **	-4.62	-0.27 **	-4.58
UnexpectedGDP	-0.02	-0.60	-0.02	-0.57	-0.02	-0.55	-0.02	-0.57
Month	0.12 **	14.86	0.12 **	14.86	0.12 **	15.00	0.12 **	14.87
Constant	2.28 **	8.63	2.84 **	4.61	5.02 **	4.09	5.12 **	6.62
Overall R ²	0.17		0.16		0.17		0.19	
Number of country-month	11,553		11,553		11,307		11,553	
Number of Countries	40		40		38		40	

** Significant at the 1% level.

* Significant at the 5% percent level.

Table 10: Percentage of Forecast from Big-12 Banks by Country and Subperiod

Country	% of forecasts from analyst who work for the large 12 banks			% of analysts who work for the large 12 banks		
	Before	Between	After	Before	Between	After
	Reg FD	Reg FD and GS	GS	Reg FD	Reg FD and GS	GS
Argentina (civil law)	0.21	0.35	0.40	0.28	0.53	0.45
Austria	0.42	0.40	0.42	0.41	0.42	0.44
Belgium	0.20	0.13	0.21	0.28	0.18	0.26
Brazil	0.28	0.56	0.34	0.35	0.59	0.43
Chile	0.23	0.55	0.45	0.40	0.65	0.52
Colombia	0.36	0.53	0.50	0.43	0.53	0.50
Denmark	0.10	0.14	0.17	0.18	0.23	0.24
Egypt	0.50	0.53	0.38	0.50	0.50	0.43
Finland	0.17	0.13	0.14	0.25	0.21	0.21
France	0.23	0.22	0.20	0.29	0.29	0.27
Germany	0.26	0.20	0.22	0.31	0.25	0.26
Greece	0.14	0.25	0.28	0.24	0.32	0.35
Indonesia	0.33	0.17	0.19	0.39	0.28	0.32
Italy	0.27	0.29	0.23	0.34	0.34	0.30
Japan	0.38	0.37	0.40	0.40	0.37	0.39
Korea	0.35	0.23	0.13	0.37	0.28	0.17
Mexico	0.26	0.49	0.62	0.35	0.59	0.69
Netherlands	0.24	0.26	0.25	0.33	0.30	0.28
Norway	0.14	0.10	0.12	0.22	0.19	0.19
Peru	0.39	0.36	0.41	0.46	0.43	0.40
Philippines	0.36	0.31	0.42	0.42	0.37	0.44
Portugal	0.34	0.43	0.28	0.40	0.43	0.31
Spain	0.33	0.32	0.26	0.39	0.37	0.31
Sweden	0.19	0.19	0.17	0.25	0.26	0.23
Switzerland	0.34	0.27	0.33	0.37	0.34	0.33
Taiwan	0.58	0.55	0.56	0.55	0.56	0.55
Turkey	0.08	0.14	0.30	0.12	0.20	0.33
Venezuela	0.55	0.61	1.00	0.52	0.59	1.00
Australia (common law)	0.56	0.62	0.69	0.58	0.64	0.67
Canada	0.18	0.21	0.17	0.24	0.26	0.23
Hong Kong	0.29	0.33	0.39	0.36	0.36	0.42
India	0.41	0.41	0.47	0.42	0.45	0.46
Ireland	0.18	0.25	0.28	0.23	0.30	0.31
Israel	0.35	0.44	0.29	0.37	0.53	0.37
Malaysia	0.28	0.26	0.24	0.32	0.27	0.27
New Zealand	0.61	0.55	0.45	0.66	0.68	0.52
Singapore	0.28	0.31	0.38	0.32	0.34	0.43
South Africa	0.37	0.37	0.43	0.43	0.40	0.45
Thailand	0.42	0.23	0.23	0.41	0.31	0.33
United Kingdom	0.43	0.40	0.39	0.45	0.43	0.43
Non U.S. median	0.31	0.32	0.31	0.37	0.37	0.36
United States	0.43	0.41	0.34	0.42	0.39	0.32

Table 11: The Role of Big-12 Banks on the Impact of New Regulations on BIAS in International Markets.

This table presents the results of random country effect regressions. The dependent variable is earnings forecast bias defined as the difference between forecast and the actual EPS, scaled by stock price and multiplied by 100. The explanatory variables include actual GDP growth, unexpected GDP growth, and months from forecast period end. Reg FD dummy equals one when forecasts are made between October 23, 2000 and December 20, 2002. Glob dummy equals one when forecasts are made after December 20, 2002. The detailed definitions of these variables are discussed in Section 4.2. The definitions of legal variables are from LLSV (1998). Common law dummy equals one when forecasts are from countries with common law, and zero otherwise. The anti-director rights index represents minority shareholder rights, ranging from zero to five. The accounting standards index measures the inclusion or omission of 90 items in annual reports. Leuz, Nanda and Wysocki (2003) define the legal enforcement index as the mean score across three legal variables used in LLSV (1998): efficiency of judicial system, rule of law, and corruption.

Panel A: Define #BigBank as the proportion of forecasts that come from analysts who work for the Big-12 banks.

	Common law		Anti-director rights index		Accounting standards index		Legal enforcement index	
	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat
RegFD	1.99 *	2.04	1.59	1.36	1.80	0.63	1.55	0.60
Glob	-1.24 **	-3.58	-2.26 **	-4.82	-4.16 **	-2.68	-2.01 *	-2.05
LegalVariable	-0.63	-1.26	-0.25	-1.37	-0.05 *	-2.20	-0.39 **	-4.09
LegalVariable × RegFD	-0.14	-0.21	0.12	0.50	0.00	0.05	0.04	0.19
LegalVariable × Glob	1.29 **	3.02	0.46 **	3.22	0.05 *	2.11	0.12	1.10
#BigBank	-0.20	-0.35	-0.26	-0.47	-0.33	-0.60	-0.35	-0.64
#BigBank × RegFD	-3.18	-1.49	-3.26	-1.49	-3.27	-1.42	-3.14	-1.35
#BigBank × Glob	-2.39 **	-2.58	-2.22 *	-2.45	-1.59	-1.81	-1.67	-1.86
ActualGDP	-0.27 **	-4.73	-0.27 **	-4.68	-0.27 **	-4.63	-0.27 **	-4.65
UnexpectedGDP	-0.02	-0.59	-0.02	-0.57	-0.02	-0.53	-0.02	-0.56
Month	0.12 **	14.36	0.12 **	14.37	0.12 **	14.36	0.12 **	14.29
Constant	2.34 **	9.13	2.93 **	4.66	5.13 **	3.98	5.15 **	6.74
Overall R ²	0.17		0.17		0.18		0.20	
Number of country-month	11,553		11,553		11,307		11,553	
Number of Countries	40		40		38		40	

** Significant at the 1% level.

* Significant at the 5% percent level.

Panel B: Define #BigBank as the proportion of analysts who work for the Big-12 banks.

	Common law		Anti-director rights index		Accounting standards index		Legal enforcement index	
	Coef.	t -stat	Coef.	t -stat	Coef.	t -stat	Coef.	t -stat
RegFD	2.16	1.84	1.77	1.32	1.85	0.61	1.70	0.61
Glob	-1.09 **	-2.79	-2.12 **	-4.32	-4.09 **	-2.58	-1.89	-1.87
LegalVariable	-0.64	-1.29	-0.25	-1.38	-0.05 *	-2.20	-0.39 **	-4.09
LegalVariable × RegFD	-0.13	-0.19	0.13	0.52	0.00	0.11	0.05	0.20
LegalVariable × Glob	1.33 **	3.17	0.45 **	3.22	0.05 *	2.12	0.12	1.08
#BigBank	-0.28	-0.48	-0.31	-0.55	-0.42	-0.74	-0.43	-0.78
#BigBank × RegFD	-3.28	-1.35	-3.38	-1.36	-3.36	-1.29	-3.23	-1.22
#BigBank × Glob	-2.63 **	-2.77	-2.36 *	-2.47	-1.72	-1.85	-1.79	-1.90
ActualGDP	-0.27 **	-4.72	-0.27 **	-4.68	-0.27 **	-4.63	-0.27 **	-4.63
UnexpectedGDP	-0.02	-0.58	-0.02	-0.56	-0.02	-0.52	-0.02	-0.55
Month	0.12 **	14.65	0.12 **	14.64	0.12 **	14.63	0.12 **	14.57
Constant	2.38 **	8.47	2.97 **	4.62	5.18 **	3.95	5.21 **	6.81
Overall R ²	0.17		0.17		0.18		0.20	
Number of country-month	11,553		11,553		11,307		11,553	
Number of Countries	40		40		38		40	

** Significant at the 1% level.

* Significant at the 5% percent level.

Table 12: The Role of Big-12 Banks on the Impact of New Regulations on Optimistic in International Markets.

This table presents the results of random country effect regressions. The dependent variable is $Optimistic_{i,m,t}$ which takes value one when actual EPS > forecast, 0.5 when actual EPS = forecast, and zero when actual EPS < forecast. $Optimistic_{i,m,t}$ is the average $Optimistic_{i,m,t}$ of all forecasts made m months before the forecast period end date in year t for country i . The explanatory variables include actual GDP growth, unexpected GDP growth, and months from forecast period end. Reg FD dummy equals one when forecasts are made between October 23, 2000 and December 20, 2002. Glob dummy equals one when forecasts are made after December 20, 2002. The detailed definitions of these variables are provided in Section 4.2. The definitions of legal variables are from LLSV (1998). Common law dummy equals one when forecasts are from countries with common law, and zero otherwise. The anti-director rights index represents minority shareholder rights, ranging from zero to five. The accounting standards index measures the inclusion or omission of 90 items in annual reports. Leuz, Nanda and Wysocki (2003) define the legal enforcement index as the mean score across three legal variables used in LLSV (1998): efficiency of judicial system, rule of law, and corruption. #BigBank is defined as the proportion of forecasts that come from analysts who work for the Big-12 banks.

	Common law		Anti-director rights index		Accounting standards index		Legal enforcement index	
	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat
RegFD	0.05 *	2.51	0.06 *	1.99	0.03	0.50	-0.10 *	-2.01
Glob	-0.12 **	-4.66	-0.16 **	-4.48	-0.30 **	-3.27	-0.10	-1.09
LegalVariable	-0.04	-1.57	-0.01	-0.90	0.00 *	-2.10	-0.02 **	-3.60
LegalVariable × RegFD	-0.03	-1.11	-0.01	-0.90	0.00	0.42	0.02 **	3.24
LegalVariable × Glob	0.10 **	3.73	0.02 *	2.54	0.00 *	2.29	0.00	-0.15
#BigBank	0.03	0.78	0.02	0.65	0.02	0.58	0.02	0.63
#BigBank × RegFD	-0.03	-0.68	-0.03	-0.56	-0.07	-1.46	-0.02	-0.49
#BigBank × Glob	-0.20 **	-4.03	-0.16 **	-3.55	-0.13 *	-2.37	-0.12 *	-2.16
ActualGDP	-0.01 **	-5.79	-0.01 **	-5.80	-0.01 **	-5.97	-0.01 **	-6.22
UnexpectedGDP	0.00	0.05	0.00	0.17	0.00	0.34	0.00	0.08
Month	0.01 **	13.90	0.01 **	13.91	0.01 **	13.64	0.01 **	14.03
Constant	0.61 **	34.90	0.61 **	27.14	0.72 **	12.13	0.73 **	16.45
Overall R ²	0.28		0.27		0.28		0.29	
Number of country-month	10,418		10,418		10,179		10,418	
Number of Countries	40		40		38		40	

** Significant at the 1% level.

* Significant at the 5% percent level.

Table 13: Forecast Bias of Big-12 and Other Analysts in International Markets

Big-12 refers to the analysts working for Big-12 banks covered by the Global Settlement. Forecast bias is the difference between the mean of all forecasts made in a particular month for a particular firm and a particular fiscal year and the realized EPS, scaled by the stock price and multiplied by 100. Month is the month of the forecast relative to the fiscal year end. Earnings forecasts are for 1984-2006. The sample is divided into three sub-periods: before the implementation of Reg FD (October 23, 2000), between Reg FD and the Global Settlement, and after the Global Settlement (GS) (December 20, 2002). To be included in this sample, the covered firms must have forecasts from both analyst groups within the same month.

Month	Before Reg FD					Between RegFD and Global Settlement					After Global Settlement				
	#Obs.	Big12	NonBig12	Diff	diff t stat	#Obs.	Big12	NonBig12	Diff	diff t stat	#Obs.	Big12	NonBig12	Diff	diff t stat
-23	160	3.22	3.08	0.14	0.59	65	3.85	4.12	-0.27	-1.17	291	0.51	0.41	0.10	1.43
-22	1,894	2.43	2.47	-0.04	-0.85	1,061	3.85	3.87	-0.03	-0.41	2,263	0.09	0.13	-0.05	-1.30
-21	6,595	2.61	2.67	-0.06 *	-2.00	1,775	3.87	3.78	0.09	1.33	2,681	0.03	0.03	0.00	0.00
-20	9,328	2.73	2.75	-0.02	-1.03	1,999	4.04	3.97	0.07	1.02	2,905	0.06	0.07	0.00	-0.12
-19	9,827	2.75	2.75	0.00	-0.08	2,217	3.87	4.00	-0.13 *	-2.02	3,063	0.22	0.25	-0.03	-0.87
-18	9,495	2.62	2.62	0.00	-0.10	1,731	3.96	3.89	0.07	0.90	2,469	-0.02	0.01	-0.04	-0.99
-17	9,511	2.65	2.69	-0.04	-1.45	2,146	3.50	3.54	-0.04	-0.60	2,965	-0.12	-0.10	-0.02	-0.65
-16	10,430	2.34	2.41	-0.07 **	-2.92	2,561	3.04	3.05	-0.02	-0.27	3,768	0.15	0.18	-0.03	-1.00
-15	12,001	2.21	2.23	-0.02	-0.77	2,452	2.98	2.95	0.03	0.70	2,705	-0.06	-0.10	0.03	1.08
-14	10,504	2.15	2.13	0.02	0.97	2,180	2.36	2.38	-0.02	-0.39	2,660	0.09	0.08	0.01	0.21
-13	8,904	1.99	2.03	-0.04	-1.73	3,274	2.39	2.47	-0.07	-1.74	3,172	-0.04	-0.02	-0.02	-0.68
-12	9,795	1.76	1.81	-0.05 *	-2.24	1,821	2.80	2.99	-0.19 **	-3.23	2,267	-0.22	-0.24	0.02	0.46
-11	8,881	1.55	1.61	-0.06 **	-2.62	1,919	2.55	2.67	-0.12	-1.87	3,895	0.18	0.21	-0.03	-1.17
-10	9,072	1.35	1.39	-0.04	-1.77	2,500	2.42	2.44	-0.01	-0.29	5,399	0.16	0.18	-0.02	-0.75
-9	12,371	1.30	1.32	-0.02	-1.00	2,527	2.33	2.30	0.03	0.77	4,631	0.16	0.19	-0.03	-1.12
-8	12,233	1.29	1.30	-0.01	-0.80	2,330	2.27	2.32	-0.05	-1.10	4,478	0.23	0.21	0.01	0.51
-7	10,678	1.17	1.20	-0.03	-1.39	2,300	2.01	2.10	-0.09	-1.68	4,547	0.23	0.24	-0.01	-0.31
-6	9,743	1.20	1.22	-0.02	-0.74	1,775	2.05	2.02	0.02	0.41	3,450	0.10	0.15	-0.05	-1.80
-5	9,598	1.03	1.03	0.00	-0.02	2,247	1.74	1.80	-0.07	-1.33	4,122	0.04	0.05	-0.01	-0.54
-4	10,590	0.82	0.89	-0.06 **	-3.47	2,668	1.29	1.28	0.01	0.20	5,452	0.09	0.10	-0.01	-0.54
-3	11,638	0.68	0.73	-0.05 **	-3.17	2,449	1.21	1.19	0.02	0.35	3,667	0.07	0.03	0.04	1.71
-2	10,082	0.75	0.73	0.02	1.06	2,139	1.03	1.07	-0.05	-1.01	3,841	0.01	-0.01	0.02	0.97
-1	8,283	0.66	0.68	-0.02	-0.90	3,277	0.67	0.62	0.05	1.42	4,633	0.00	-0.03	0.02	1.08
0	8,635	0.54	0.54	0.00	-0.13	1,728	0.39	0.44	-0.04	-1.03	3,065	0.03	-0.03	0.06	1.93
1	7,316	0.33	0.29	0.04	1.56	1,768	0.53	0.57	-0.04	-0.72	4,541	-0.01	-0.03	0.02	1.04
All Months	227,564	1.59	1.61	-0.02 **	-5.12	52,909	2.31	2.33	-0.02 *	-2.13	86,930	0.08	0.08	0.00	-0.65

** Significant at the 1 percent level.

* Significant at the 5 percent level.

ESSAY 2

Does Affiliation Bias Analyst Long-Term Earnings Growth Forecasts?

This study examines whether conflicts of interest induce analysts of those banks that underwrite seasoned equity offerings (SEOs) to inflate their long-term growth forecasts around these offerings. Potential conflicts of interests arise when compensation of affiliated analysts who work for investment banks is tied to profits generated from investment banking business. Concerns about such conflicts of interest prompted regulatory actions that culminated in the Global Analyst Research Settlement reached in 2002. In addition to paying a \$1.4 billion fine, twelve large investment banks agreed to take steps to eliminate the sources of analysts' conflicts of interest.

A primary approach used to test the conflict of interest hypothesis in the academic literature is to compare the forecast behavior of analysts affiliated with investment banks doing business with covered firms to the behavior of unaffiliated analysts. Prior studies show that affiliated analysts' long-term growth (LTG) forecasts are more optimistic than those of unaffiliated analysts (Lin and McNichols, 1998; Dechow, Hutton, and Sloan, 2000).

It has long been recognized that the observed overly optimistic forecasts may result from selection bias. Equity-issuing firms may tend to select underwriters that have more optimistic views of the firms' prospects. As a result of this selection bias, LTG forecasts of affiliated analysts will appear to be more optimistic even if there is no conflict of interest. Because it is very difficult to control for selection bias, the conclusion

of earlier studies that analysts inflate their long-term forecasts because of conflicts of interest continues to be a subject of debate.

My interest in LTG forecasts is also motivated by the fact that these forecasts are key inputs in stock valuation models, and the estimated stock prices are very sensitive to the LTG forecasts. Thus, even if short-term earnings forecasts are unbiased, biases in LTG forecasts can cause stock prices to deviate from their fair fundamental values. In addition, LTG forecasts are more difficult to verify, since one needs to wait three to five years to know the realized growth rate. As a result, it is easier for analysts to manipulate stock prices via LTG forecasts. The interest in manipulation is likely to be especially high around SEOs since a large number of shares are issued to outsiders as a result of the offerings, and the insiders should be interested in obtaining a high price for the issued shares. Thus, if there is any evidence to support the conflict of interest hypothesis, it should show up around SEOs.

The identification strategy for testing the hypothesis that conflicts of interest induce analysts to inflate their LTG forecasts is twofold. First, I hypothesize that conflicts of interest should be more apparent when there are fewer analysts following the firm. I assume that analysts consider their costs and the benefits when they choose to make an overoptimistic forecast. Because the potential benefits for the client from a single inflated forecast should decline with the number of analysts, I argue that the impact of an individual analyst's forecast on the stock price is likely to be stronger when the number of analysts following the firm (analyst coverage) is low. Hence, the difference between the forecasts made by affiliated and unaffiliated analysts should also decline with the number of analysts. In contrast, if the optimism of affiliated analysts relative to that of

unaffiliated analysts arises because firms tend to select investment banks with optimistic analysts as their underwriters, then the difference between the forecasts made by affiliated and unaffiliated analysts should not be related to the number of analysts.

The second identification strategy relies on the idea that potential benefits from inflated forecasts and stock prices are higher when firms issue new equity. Studies of equity issue timing have shown that firms tend to issue equity when their share prices are unusually high (Baker and Wurgler, 2002). Furthermore, surveys of corporate managers have also shown that most corporate managers rank over- and under-valuation as a very important factor when they make a decision to issue equity (Graham and Harvey, 2001). Therefore, I hypothesize that if the overoptimism of affiliated analysts is driven by conflicts of interest, then their forecasts around the time of equity issuance should be even more overoptimistic.

Specifically, I examine the changes of forecasts within each analyst. Evidence of rising forecasts of affiliated analysts at a rate higher than that of unaffiliated analysts around SEOs will support the conflict of interest hypothesis and also rule out selection bias hypothesis. In contrast, if the observed optimism of affiliated analysts is simply a consequence of choosing underwriters from the pool of relatively more optimistic investment banks, then I may not observe increased optimism at the time of equity issuance.

Kolasinski and Kothari (2007) also attempt to rule out selection bias hypothesis by examining analysts' recommendations and LTG forecasts around merger and acquisition deals. They document that acquirer-affiliated analysts upgrade acquirers' stocks and make relatively higher LTG forecasts than do unaffiliated analysts.

Nonetheless, their conclusion relies on the assumption that acquirers do not choose advisors that have optimistic views of their own stocks for all-cash deals.

Overall the findings in this study do not support the conflict of interest hypothesis. First, consistent with the findings of Lim (2001), I document that LTG forecasts are more optimistic for firms with less analyst coverage. My finding that the optimistic bias in analyst forecasts, both affiliated and unaffiliated, is higher for firms with lower analyst coverage is also consistent with the hypothesis that analysts drop their coverage when they have pessimistic views of the firm's prospects (McNichols and O'Brien, 1997)

Second, controlling for analyst coverage, LTG forecasts of affiliated analysts are not statistically different from those of unaffiliated analysts, both for firms with less and more analyst coverage. Third, when I do not control for analyst coverage, LTG forecasts of affiliated analysts appear to be significantly more overly optimistic than the forecasts of unaffiliated analysts. Consistent with prior literature, this last result arises because the fraction of unaffiliated analysts is higher for firms with more analyst coverage, and these firms have less optimistic forecasts.

When I examine the time-series patterns of analyst forecasts, I find that there is no difference between affiliated and unaffiliated analysts' LTG forecasts, both before and after the filing date of SEOs. The changes in the forecast biases of the two analyst groups around SEOs are not different either. The levels of forecast bias of both analyst groups increase after the SEO filing date, but this happens mainly because of deteriorating operating performance, not because of rising forecasts. It seems that equity issuers time the market, but analysts fail to incorporate this fact into their forecasts. Firms issue more

equity when analysts are most optimistic and when their operating performance is at their peak.

To conclude, I find no support for the conflict of interest hypothesis. Forecasts of affiliated analysts are not more optimistic than forecasts of unaffiliated analysts. Nor do affiliated analysts further inflate their forecasts at the time of equity issuance.

Section 1 describes databases used in my analyses. I present univariate analyses in Section 2 and multivariate analyses in Section 3. I discuss and conclude this study in Section 4.

1. Data

I obtain LTG forecasts and earnings forecasts for both current- and subsequent-year earnings per share from the Institutional Brokers' Estimate System (I/B/E/S). For each forecast, I/B/E/S also provides the realized earnings, the realized earnings growth rate, the forecast date, the forecast period end, the analyst identity code, the broker identity code, the number of analysts used to calculate consensus, and the sector code.²⁵

²⁶ I use the Broker Translation File from I/B/E/S to convert broker codes into brokers' names. These names are used to identify analyst affiliation. I drop any forecasts in the I/B/E/S excluded estimates file.

Data on SEOs from 1982 to 2004 come from the Thomson SDC database. I include only common stock offerings by U.S. firms. I exclude SEOs that relate to IPOs,

²⁵ Although I/B/E/S' computing method for realized 5-year EPS growth rates underestimates actual EPS growth rates (Kothari, 2000), it will not affect my conclusion because I compare affiliated and unaffiliated within the same firm.

²⁶ On the Thursday before the third Friday of every month, I/B/E/S computes average of outstanding forecasts to get summary statistics of forecasts for each stock. These statistics are in the I/B/E/S summary file which contains means and medians of forecasts, the number of outstanding forecasts and standard deviations of these forecasts.

right offerings, spin-off, private placements, unit investment trusts, closed-end funds, limited partnerships companies, LBO firms, and REITs. The information from SDC database includes filing dates, issue dates, underwriter names, underwriters' roles, total proceeds, and gross spreads. An analyst whose employer underwrites a SEO is considered to be an affiliated analyst for that SEO. I distinguish two types of affiliated analysts. Similar to Lin and McNichols (1998), I name an analyst as a lead underwriter when the analyst works for a book runner, joint book runner, joint lead manager, or global coordinator. I name an analyst as a co-manager when the analyst works for a co-manager or syndicate member.

Data on LTG forecasts in the final sample begin from 1981, which is the first year for which I/B/E/S provided LTG forecasts. This sample ends in 2001 because I/B/E/S requires five years of realized earnings to compute realized earnings growth rates.

2. Univariate analyses

I start the analyses by examining the overall patterns of forecast bias around the filing date of each SEO. To do so, I merge earnings forecasts and LTG earnings forecasts with SEOs by matching the brokers' names from I/B/E/S with the names of underwriters' parents from the SDC dataset. I keep only forecasts made within -5/+5 years around the filing year. The filing year, Year 0, is the fiscal year in which an equity issuer files its registration of new shares with the Securities and Exchange Commission (SEC). Year -1 is the fiscal year prior to the filing year and so on.

I choose to use the filing date as the cutoff date because I want to examine whether analysts change their behavior after they know for certain whose employers are

selected to be underwriters.²⁷ Furthermore, usually the filing date is close to the corresponding issue date.²⁸ Thus, any attempts to influence stock value are likely to occur between the filing date and the issue date. I compute the forecast bias for LTG forecasts as follows:

$$LTGBIAS_{i,j,t} = 100 * (F_{i,j,t} - A_{j,t}) \quad (1)$$

In (1), $F_{i,j,t}$ is a LTG forecast from analyst i for firm j at time t . $A_{j,t}$ is the corresponding realized 5-year EPS growth rate. I calculate forecast bias for earnings forecasts as follows.

$$EPSBIAS_{i,j,t} = 100 * (F_{i,j,t} - A_{j,t}) / P_{j,t-1} \quad (2)$$

In (2), $F_{i,j,t}$ is an annual earnings forecast from analyst i for firm j at time t .²⁹ $A_{j,t}$ is the realized EPS corresponding to the forecast. $P_{j,t-1}$ is the stock price of firm j from the month before the corresponding forecast was made. I obtain stock prices from the I/B/E/S Summary file or Center for Research in Security Prices (CRSP) monthly file for cases in which the prices are not available from I/B/E/S.³⁰ To reduce the impact of extreme observations, I drop observations with forecast bias at the top and bottom 1% of the distribution.

The means and the medians of forecast bias in years -5 through $+5$ around the filing date are shown in Panel A and B of Table 1, respectively. There are 1,310,586 forecasts for current-year earnings, 973,687 forecasts for subsequent-year earnings, and 132,865 LTG earnings forecasts around SEOs. There are, however, multiple counts of

²⁷ Kolasinski and Kothari (2007) who examine analysts' conflicts of interest around M&A deals also choose the M&A announcement date as their cutoff date.

²⁸ In the final sample, the issue date occurs on average 56 days after the filing date.

²⁹ Each SEO is treated as an independent entity. Therefore, I use "firm" and "SEO" interchangeably.

³⁰ Earnings related data and stock prices from I/B/E/S are split adjusted. Stock prices from CRSP are divided by cumulative price adjustment factor to obtain split-adjusted stock prices.

forecasts for firms that issue new shares multiple times. The observations in Table 1 are from 858,395 unique current-year earnings forecasts, 697,148 unique subsequent-year earnings forecasts, 142,867 unique LTG forecasts, 7,651 unique SEOs and 4,931 unique firms.

Consistently with prior studies, I find that analysts tend to make optimistic forecasts (O'Brien, 1988; Brown, 1997; Chopra, 1998). The means of forecast bias are positive in all years around SEOs. In Panel A of Table 1, I also observe positive spikes of realized earnings in year 0 for current-year earnings forecasts, in year -1 for subsequent-year earnings forecasts, and in year -2 and -3 for LTG earnings forecasts. These patterns are consistent with the market timing story.

The market timing story suggests that the operating performance of equity-issuing firms will be peaked at the fiscal year in which they issue equity. If analysts cannot anticipate an increase in the operating profit, I would observe these exact patterns of forecast bias in Table 1, i.e. lowest forecast bias in year 0 for current-year earnings forecasts, in year -1 for subsequent-year earnings forecasts, and in year -2, and -3 for LTG earnings forecasts.

It seems that analysts do not raise their current- and subsequent-year earnings forecasts, but they raise their LTG forecasts from 16.44% in year -1 to 18.19% in the filing year. This result implies that analysts expect earnings of equity-issuing firms to grow at faster rates after SEOs. It seems that analysts fail to anticipate well-documented evidence of deteriorating operating performance of firms that conduct SEOs (Loughran and Ritter, 1997). I see similar patterns when examining medians of forecast bias in Panel B of Table 1.

Next, I examine the difference in the forecast bias between affiliated and unaffiliated groups. Lin and McNichols (1998) show that there is no difference between current-year and subsequent-year earnings forecasts of affiliated analysts and those of unaffiliated analysts. To save space, henceforth I present analyses only for LTG forecasts.

The means and medians of forecast bias $-5/+5$ years around the filing date by analyst affiliation are shown respectively in Panel A and B of Table 2. Consistent with the findings of Lin and McNichols (1998) and Dechow, Hutton, and Sloan (2000), the means and medians of forecast bias of lead underwriters and co-managers appear to be significantly higher than those of unaffiliated analysts both in the filing year and one year after.

As discussed earlier, the conflict of interest hypothesis predicts that if the observed overoptimism of affiliated analysts' forecasts is driven by analysts' conflicts of interest, then the difference between the forecasts made by affiliated and unaffiliated analysts should decline with analyst coverage.

To test this hypothesis, I split the sample into two groups based on analyst coverage, represented by the number of LTG forecasts used in the I/B/E/S consensus calculation for each stock. The first group contains forecasts made for firms that have less than six analysts. The second group contains forecasts made for firms that have six or more analysts. The following results are robust to the change in the choice of this cutoff number.

Consistent with the findings of Brown (1997) and Lim (2001), Table 3 shows that forecast bias is higher for firms with lower analyst coverage from year -2 to year $+5$. This

evidence suggests that the means and medians of forecast bias of each analyst group presented in Table 2 are influenced by aggregation bias as illustrated by the following example.

Suppose that there are two analysts following firm X and three analysts following firm Y. The number of unaffiliated analysts relative to the number of affiliated analysts tends to increase with the analyst coverage. For simplicity, I assume that there is one affiliated analyst for each firm.

Forecast Bias for Firm X		Forecast Bias for Firm Y	
Affiliated Analyst	20%	Affiliated Analyst	5%
Unaffiliated Analyst	20%	Unaffiliated Analyst	5%
		Unaffiliated Analyst	5%

Firm X has less analyst coverage. Based on Lim's (2001) findings and the results in Table 3, analysts who follow this firm will have relatively higher forecast bias than analysts who follow firm Y. In this example, affiliated and unaffiliated analysts are equally overly optimistic about each firm's prospect. If all data are pooled together before computing mean forecast bias for each affiliation group, the mean forecast bias will equal $(20\%+5\%)/2 = 12.5\%$ for the affiliated analyst group and $(20\%+5\%+5\%)/3 = 10\%$ for the unaffiliated analyst group. Based on these calculations, one will reach the wrong conclusion that affiliated analysts are more optimistic than unaffiliated analysts.

In Table 4, I compute the mean forecast bias within each affiliation group by year around the filing date within each SEO; then I calculate the mean of the means forecast bias of each affiliation type across firms. This computational procedure helps me avoid

aggregation bias shown in Table 2. Table 4 shows that after controlling for analyst coverage to avoid aggregation bias, the differences between affiliated and unaffiliated analysts' forecast bias disappear.

In Table 5, I restrict the sample to firms that have either both analysts working for lead underwriters and unaffiliated analysts (Panel A) or both analysts working for co-managers and unaffiliated analysts (Panel B). Furthermore, to ensure that the results in Table 5 are not affected by the change in sample compositions, I require that each analyst in the final sample make LTG forecasts within two years before the filing date and within two years after the filing date.

I also avoid aggregation bias and selection bias by computing the means and changes of the means of forecast bias within each analyst within each SEO, and then calculating the means for each affiliation group within each SEO. Lastly, I calculate the mean forecast bias for each affiliation group across all SEOs. The final sample in Table 5 comprises 393 SEOs that have both lead underwriters and unaffiliated analysts (in Panel A) and 444 SEOs that have both co-managers and unaffiliated analysts (in Panel B). Panel A and B reveal that there is no statistically significant difference between affiliated analysts and unaffiliated analysts in terms of forecast bias before the filing date, forecast bias after the filing date, and changes in forecast bias around the filing date. The marginal changes in the means and the medians of forecast levels after the filing date in both Panel A and B also imply that the levels of forecast bias rise after the filing date due to firms' deteriorating operating performance, not due to increasing forecast levels. Analysts seem to make forecasts passively and do not adjust their forecasts downward.

In Table 5, I also examine whether analysts' conflicts of interest are more obvious when firms have less analyst coverage. The results in Table 5 do not support this hypothesis. I find that forecast bias is higher for firms with less analyst coverage. However, I do not find a significant difference between affiliated and unaffiliated analysts in terms of forecast level, forecast bias and change in forecast bias in firms with less analyst coverage. For firms with more analyst coverage, I do not find a significant difference between affiliated and unaffiliated analysts either.

3. Multivariate Analyses

To formally test the hypotheses that affiliated analysts are more optimistic than unaffiliated analysts before and after the filing date, I estimate the following regression for forecasts made within two years before the filing date, and for those made within two years after the filing date.³¹

$$\begin{aligned}
 LTGBIAS_{i,j,t} = & \alpha_0 + \alpha_1 Co_{i,j,t} + \alpha_2 Lead_{i,j,t} + \alpha_3 LnMktcap_{j,t} + \alpha_4 (LnMktcap_{j,t} \times Co_{i,j,t}) \\
 & + \alpha_5 (LnMktcap_{j,t} \times Lead_{i,j,t}) + \alpha_6 NumA_{j,t} + \alpha_7 (NumA_{j,t} \times Co_{i,j,t}) \\
 & + \alpha_8 (NumA_{j,t} \times Lead_{i,j,t}) + \alpha_9 BrokSize_{i,t} + \alpha_{10} YearIBES_{i,t} + \alpha_{11} StdDev_{j,t} \\
 & + \alpha_{12} NumStk_{i,t} + \alpha_{13} NumInd_{i,t} + \beta_i \sum DSEO_{j,t} + \varepsilon_{i,j,t}.
 \end{aligned} \tag{3}$$

The regression for forecasts made before the filing date explains how firms choose their underwriters. Meanwhile, the regression for forecasts made after the filing date shows how analysts make forecasts after they know their affiliations.

In this section, I include only firms that have both affiliated and unaffiliated analysts who must make forecasts both before and after the filing date. In the regression

³¹ My findings remain qualitatively the same when I use -1/+1 and -3/+3 instead of -2/+2 years around the filing date.

for forecasts made before the filing date, $LTGBIAS_{i,j,t}$ is the mean forecast bias of the forecasts for firm j made by analyst i before the filing date t . In the regression for forecasts after the filing date, $LTGBIAS_{i,j,t}$ is the mean forecast bias after the filing date t . I compute the means of other control variables the same way as I do for $LTGBIAS_{i,j,t}$ and use them in (3).

$Co_{i,j,t}$ equals one if an LTG forecast is made by an analyst i who works for a co-manager of firm j around the filing date t , and zero otherwise. $Lead_{i,j,t}$ equals one if an LTG forecast is made by an analyst i who works for a lead underwriter, and zero otherwise. If analysts who work for co-managers are more biased, the sign of α_1 will be positive. If analysts who work for lead underwriters are more biased, the sign of α_2 will be positive. We use SEO dummies, $DSEO_{j,t}$, to control for unobservable factors that may also influence forecast bias.

Lim (2001) argues that forecast bias will be higher when a firm's information environment is less transparent, for example when the firm is small and has less analyst coverage. Hence, I use analyst coverage and firm size as a proxy for the degree of information transparency. Analyst coverage, $NumA_{j,t}$, is defined as the number of outstanding forecasts used in I/B/E/S monthly consensus calculation. Analyst coverage represents the number of analysts following firm j in the month in which analysts make their forecasts around the filing date t . Controlling for analyst coverage in the regression also helps to eliminate aggregation bias.

I use stock prices and the number of outstanding shares from the I/B/E/S Actuals and Pricing and Ancillary file to compute firm size, $LnMktcap_{j,t}$, which is defined as the natural log of a firm's market capitalization in the month before the forecast date t .

Market capitalization equals stock price in the month before the forecast is made, multiplied by the corresponding shares outstanding.

Analysts tend to make more accurate and less biased forecasts when they have more experience and a greater number of resources (Clement, 1999; Lim, 2001; Mikhail, Walther, and Willis, 1997). An analyst's general experience, $YearIBES_{i,t}$, is measured as the number of years since an analyst's first appearance in the I/B/E/S database until forecast date around the filing date t .

$StdDev_{j,t}$ is the standard deviation of forecasts used for the I/B/E/S consensus calculation in the month in which analysts make their forecasts around the filing date t . $BrokSize_{i,t}$ is the number of analysts who work for the same employer during the same forecast year as the analyst who made forecasts for firm j around the filing date t . Analysts who work for larger firms tend to have more resources. Clement (1999) also finds that analysts' forecasts are less accurate when analysts follow more stocks and more industries. $NumStk_{i,t}$ is the number of stocks for which an analyst supplies at least one forecast within the same calendar year as his forecast around the filing date t . $NumInd_{i,t}$ is the number of sectors for which an analyst supplies at least one forecast within the same calendar year as his forecast around the filing date t .

Summary statistics of all variables are shown in Table 6. This final sample of 3,056 analysts-SEO observations for the regression analyses comes from 962 unaffiliated analysts, 355 analysts working for co-managers, and 222 analysts working for lead underwriters. LTG forecasts in this sample are for 450 firms that make a total of 631 SEOs. Before the filing date, on average, analysts forecast that earnings LTG will be 15.55%. Their forecasts are usually too optimistic. On average, forecast bias is 7.09%

before the filing date. The average firm size is around \$2.97 billion. There are, on average, 9.32 analysts covering a firm and making LTG forecasts in any particular month. A typical analyst is in the I/B/E/S database for 4.3 years. On average, each broker employs 57.6 analysts. Each analyst generally follows 26.62 stocks and 2.58 sectors. The mean of standard deviation of LTG forecasts used for consensus calculation is 3.29%. The average size of proceeds is \$221.9 million, and the average gross spread is 3.52%.³²

On average, post-filing date forecasts are made 560 days after pre-filing date forecasts. After the filing date, on average, analysts forecast that LTG earnings will be 15.21%, which is a bit lower than the forecast level before the filing date. Meanwhile, forecast bias significantly rises to 10.14% after the filing date. This implies that the increase in forecast bias is mainly driven by the firms' deteriorating operating performance, not by the changes in analysts' forecasts.

The statistics of all other variables increase after the filing date. The firm size is around \$4.31 billion. There are, on average, 10.57 analysts covering a firm in any particular month. A typical analyst is in the I/B/E/S database for 5.84 years. On average, each broker employs 63.27 analysts. Each analyst generally follows 28.20 stocks and 2.62 sectors. The mean of standard deviation of LTG forecasts used for consensus calculation is 3.38%.

Table 7 presents regression results for forecasts made before the filing date. The results of OLS regressions without SEO dummies are shown in specification 1 and 2. The results of firm fixed effects regressions are shown in specification 3 to 6. In all

³² Without the requirement to include only SEOs that have both affiliated and unaffiliated analysts who must also make forecasts both before and after the filing date, there would be 3,214 analysts (3674 analysts) who make LTG forecasts within two years before the filing date (after the filing date) for 1,540 firms that make a total of 2,472 SEOs.

specifications, the coefficients of both $Co_{i,j,t}$ and $Lead_{i,j,t}$ are not statistically different from zero. These results imply that in general equity issuing firms do not choose underwriters who make more optimistic LTG forecasts. This finding is consistent with that of Ljungqvist, Marston and Wilhelm (2006) who find that optimistic stock recommendation does not increase the chance of winning an underwriting mandate.

In specification 6, in which I include all control variables, I find that the coefficient of $LnMktcap_{j,t}$ is significantly positive, implying higher forecast bias for larger firms. The positive coefficient of $NumA_{j,t} \times Co_{i,j,t}$ implies that when there are many investment banks covering equity issuers, lead underwriters will choose co-managers whose analysts would make relatively higher LTG forecasts. But when there are few analysts and particularly if many of them do not have underwriting capabilities, firms and lead underwriters may have fewer choices and may not find a co-manager who is optimistic about the stocks.

Table 8 presents the regression results for forecasts made after the filing date. The results of OLS regressions without firm dummies are shown in specification 1 and 2. The results of firm fixed effects regressions are shown in specification 3 to 6. The coefficients of $LnMktcap_{j,t}$ are still significantly positive after the filing date in specification 4 to 6. And the coefficients of $NumA_{j,t}$ become significantly negative, implying that forecast bias is higher for less covered stocks possibly because analysts drop (add) stocks for which they have negative (positive) views (McNichols and O'Brien, 1997).

The coefficient of co-manager dummy is positive and statistically significant in specification 1. However, in specification 1, I do not control for analyst coverage. Therefore, this result is influenced by aggregation bias and leads to an incorrect

conclusion that the co-managers become more optimistic after the filing date. After I control for firm size and analyst coverage in specification 2, the coefficient of co-manager dummy is no longer significantly different from zero. Interestingly, for firm fixed effects regression in specification 3, I find that the coefficient of lead manager dummy is also not different from zero even though I do not control for firm size and analyst coverage. This result suggests that analyst coverage within the firm does not vary much over time, and if this is the case, then the firm fixed effects can be used as a substitute for analyst coverage to control for aggregation bias.

I also find that the coefficient of $NumA_{j,t} \times Co_{i,j,t}$ becomes statistically insignificant after the filing date. This result implies that the selected co-managers who are relatively more optimistic before the filing date are not more optimistic than others after the filing date. This evidence is against the predictions of the conflict of interest hypothesis. The conflict of interest story suggests the coefficients of both $NumA_{j,t} \times Co_{i,j,t}$ and $NumA_{j,t} \times Lead_{i,j,t}$ to be negative because affiliated analysts can influence consensus easier when there is less analyst coverage than when there is more coverage.

Overall, the results in Table 7 and 8 show no evidence that can support the conflict of interest hypothesis because the coefficients of both lead underwriter dummy and co-manager dummy are not statistically different from zero. Furthermore, I do not find any evidence supporting the hypothesis that conflicts of interest will be more evident for less covered firms. The coefficients of $NumA_{j,t} \times Co_{i,j,t}$ and $NumA_{j,t} \times Lead_{i,j,t}$ are not statistically different from zero for forecasts made after the filing date.

Next, I turn my attention to the changes in forecast bias within analysts to find the evidence of analysts' conflicts of interest, to rule out selection bias and to ensure that the

results are not affected by change in sample compositions. Table 9 shows the results of the following regression model.

$$\begin{aligned}
\text{ChangeBIAS}_{i,j,t} = & \alpha_0 + \alpha_1 \text{LTGBIAS}_{i,j,t} + \alpha_2 \text{Co}_{i,j,t} + \alpha_3 \text{Lead}_{i,j,t} \\
& + \alpha_4 \text{LnMktcap}_{j,t} + \alpha_5 (\text{LnMktcap}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_6 (\text{LnMktcap}_{j,t} \times \text{Lead}_{i,j,t}) \\
& + \alpha_7 \text{NumA}_{j,t} + \alpha_8 (\text{NumA}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_9 (\text{NumA}_{j,t} \times \text{Lead}_{i,j,t}) \\
& + \alpha_{10} \text{Lnproceeds}_{j,t} + \alpha_{11} (\text{Lnproceeds}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_{12} (\text{Lnproceeds}_{j,t} \times \text{Lead}_{i,j,t}) \\
& + \alpha_{13} \text{GrossSpread}_{j,t} + \alpha_{14} (\text{GrossSpread}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_{15} (\text{GrossSpread}_{j,t} \times \text{Lead}_{i,j,t}) \\
& + \alpha_{16} \text{BrokSize}_{i,t} + \alpha_{17} \text{YearIBES}_{i,t} + \alpha_{18} \text{DaysTwoF}_{i,j,t} + \alpha_{19} \text{StdDev}_{j,t} + \alpha_{20} \text{NumStk}_{i,t} \\
& + \alpha_{21} \text{NumInd}_{i,t} + \beta_i \sum \text{DFirm}_j + \varepsilon_{i,j,t}. \tag{4}
\end{aligned}$$

$\text{ChangeBIAS}_{i,j,t}$ is defined as the average forecast bias of analyst i within two years after the filing date t minus the average forecast bias within two years before the filing date. $\text{ChangeBIAS}_{i,j,t}$ is computed for each analyst i who makes forecasts both before and after the filing date. $\text{Lnproceeds}_{j,t}$ is defined as a natural log of the proceeds that equity issuers receive from all markets. $\text{GrossSpread}_{j,t}$ is the gross spread for each equity issuance, described in percentage terms. $\text{DaysTwoF}_{i,j,t}$ is the difference between average days on which forecasts are made before and after the filing date. I use firm dummies, DFirm_j , to control for unobservable factors that may also influence forecast bias.³³ All others control variables are the means before the filing date, used in Table 7. Their definitions are the same as what I describe earlier.

If affiliation and fees from underwriting activities influence affiliated analysts to make higher LTG forecasts, the coefficients of $\text{Co}_{i,j,t}$, $\text{Lead}_{i,j,t}$, $\text{LnProcceds}_{j,t} \times \text{Co}_{i,j,t}$, $\text{LnProcceds}_{j,t} \times \text{Lead}_{i,j,t}$, $\text{GrossSpread}_{j,t} \times \text{Co}_{i,j,t}$, and $\text{GrossSpread}_{j,t} \times \text{Lead}_{i,j,t}$ will be positive and statistically significant. However, Table 9 shows that the coefficients of all these independent variables are not significantly different from zero. The results in Table 10

³³ Using SEO dummies instead of firm dummies is not feasible because there is no variation of proceeds and gross spread within each SEO.

remain qualitatively the same when I compute $ChangeBIAS_{i,j,t}$ based on forecasts made within either -1/+1 or -3/+3 years around the filing date. These results imply that affiliation is not a factor that causes affiliated analysts to make more optimistic forecasts than those of unaffiliated analysts.

We also find that the coefficient of forecast bias before the filing date, $LTGBIAS_{i,j,t}$, is significantly negative, implying that the level of forecast bias exhibits mean reversion. Furthermore, the coefficients of $NumA_{j,t} \times Co_{i,j,t}$ and $NumA_{j,t} \times Lead_{i,j,t}$ are statistically insignificant, suggesting that there is also no evidence of conflicts of interest for firms with less analyst coverage.

The coefficient of $LnProceeds_{j,t}$ is significantly positive. As I have shown earlier, the levels of LTG forecasts after the filing date are not difference from the levels of LTG forecasts before the filing date; therefore, this positive coefficient implies that firms issue more equities when they know that their operating performance will decline to a greater degree.

To rule out the possibility that the unaffiliated analyst group defined earlier contains analysts who work for banks that also compete for underwriting mandates, in Table 11, I exclude those unaffiliated analysts whose forecasts take place in the year of his employer's inclusion in the top twenty five underwriters.³⁴ The results in Table 11 remain qualitatively the same as those in Tables 9 and 10, implying that even those analysts who are not pressured by the underwriting business also make LTG forecasts that are as optimistic as the forecasts from analysts working for banks that pursue underwriting mandates.

³⁴ The lists of top twenty five underwriters in each year are from the SDC league table. These top twenty five underwriters altogether usually account for one hundred percent of underwriting deals.

4. Discussions and Conclusions

In this paper, I have examined two hypotheses. First is the standard hypothesis testing whether conflicts of interest induce affiliated analysts to inflate their forecasts upward around SEOs. The second hypothesis is to examine whether analysts' conflicts of interests are more obvious for firms with less analyst coverage.

Contrary to the results in prior studies, I find that after controlling for analyst coverage, there is no difference between affiliated and unaffiliated analysts' LTG forecasts in terms of forecast bias before the filing date, forecast bias after the filing date and the changes in forecast bias around the filing date. Moreover, I do not find any evidence of conflicts of interest when I examine LTG forecasts for firms with less analyst coverage.

I also find that LTG forecast bias increases after the filing date due to firms' deteriorating operating performance, not due to the increase in forecast level. It seems that equity issuers do time the market. Firms issue more equity when analysts are most optimistic about the firms' prospects and when their operating performance is at its peak.

I conclude that affiliation does not cause affiliated analysts to make more optimistic forecasts than those of unaffiliated analysts. However, analysts in all affiliation groups systematically make overly optimistic forecasts. Prior studies provide several possible explanations as to why both analyst types make optimistic forecasts. First, analysts who make optimistic forecasts tend to receive positive career prospects (Hong and Kubik, 2003). Second, analysts tend to have herding behavior leading them to make forecasts similar to the latest consensus. Moreover, inexperienced analysts are more likely to lose their jobs when they make bold forecasts (Trueman, 1994; Hong, Kubik and

Salomon, 2000). Third, it is also likely that, in addition to underwriting business, analyst optimism is driven by incentive to generate more trading volume (Cowen, Groysberg and Healy, 2006). Fourth, analysts who work for large investment banking firms may have relatively higher reputation concerns, as well as higher abilities and greater resources to make accurate forecasts (Clement 1999). Finally, Jacob, Rock and Weber (2003) argue that analysts who work for independent research firms have a greater need to maintain relationships with firms because they tend to have fewer abilities and resources than do large brokerage firms.

Table 1: Forecast level, realized earnings, and forecast bias for one-year ahead, two-years ahead, and long-term growth earnings forecasts around the filing year.

I define forecast bias for one-year ahead (FPI1) and two-years ahead (FPI2) forecasts as the difference between forecast and realized EPS, scaled by stock price and multiplied by 100. I define forecast bias for LTG as the difference between forecast and realized LTG. Year 0 is the fiscal year that contains filing date. Number of observations is the number of analyst forecasts.

Panel A: Mean

Forecast made in fiscal year from filing date	Number of observations			Forecast			Realized			Bias		
	FPI 1	FPI 2	LTG	FPI 1	FPI 2	LTG	FPI 1	FPI 2	LTG	FPI 1	FPI 2	LTG
-5	44,777	34,667	7,929	6.20	8.11	13.62	5.10	5.91	5.07	1.10	2.21	8.54
-4	53,370	41,949	8,623	5.84	7.96	14.33	4.75	5.19	6.85	1.10	2.77	7.47
-3	65,799	51,867	9,982	5.64	7.72	14.73	4.31	4.82	9.38	1.32	2.90	5.35
-2	83,358	66,575	11,081	5.03	7.48	15.49	3.76	4.93	10.29	1.26	2.55	5.20
-1	109,542	86,198	12,989	4.33	6.87	16.44	3.49	5.74	8.43	0.84	1.13	8.01
0	164,183	126,800	16,977	4.34	6.39	18.19	3.93	4.96	6.45	0.41	1.43	11.74
1	194,457	142,016	16,686	4.37	6.52	18.03	3.42	4.19	5.81	0.95	2.34	12.22
2	182,032	128,705	14,903	4.33	6.68	16.92	3.27	4.32	5.21	1.06	2.37	11.70
3	156,327	112,803	13,224	4.71	6.84	15.90	3.74	4.62	5.23	0.97	2.21	10.67
4	137,021	98,133	11,103	4.82	7.02	15.47	3.88	5.13	5.42	0.93	1.89	10.05
5	119,720	83,974	9,368	4.98	7.08	15.50	4.20	5.00	6.09	0.78	2.07	9.41

Panel B: Median

Forecast made in fiscal year from filing date	Number of observations			Forecast			Realized			Bias		
	FPI 1	FPI 2	LTG	FPI 1	FPI 2	LTG	FPI 1	FPI 2	LTG	FPI 1	FPI 2	LTG
-5	44,777	34,667	7,929	6.81	8.12	12.00	6.25	6.75	5.13	0.08	0.67	6.09
-4	53,370	41,949	8,623	6.67	8.00	12.00	6.02	6.14	5.89	0.09	0.92	5.29
-3	65,799	51,867	9,982	6.40	7.89	13.00	5.64	5.97	7.81	0.15	1.05	4.58
-2	83,358	66,575	11,081	6.21	7.77	14.00	5.48	5.95	8.36	0.13	0.90	4.33
-1	109,542	86,198	12,989	5.57	7.25	15.00	5.14	6.51	7.26	0.00	0.23	5.95
0	164,183	126,800	16,977	5.04	6.63	16.00	4.81	5.59	5.93	-0.03	0.39	8.85
1	194,457	142,016	16,686	5.28	6.79	16.00	4.71	5.05	5.59	0.05	0.87	9.44
2	182,032	128,705	14,903	5.56	7.01	15.00	4.85	5.24	5.26	0.10	0.93	8.95
3	156,327	112,803	13,224	5.74	7.14	15.00	5.11	5.64	5.24	0.06	0.68	8.03
4	137,021	98,133	11,103	5.71	7.10	15.00	5.13	5.66	6.07	0.04	0.58	7.17
5	119,720	83,974	9,368	5.65	7.10	15.00	5.21	5.67	6.24	0.02	0.63	6.75

Table 2: Forecast level, realized LTG, and forecast bias for LTG earnings forecasts around the filing year by analyst affiliation

Un = unaffiliated analyst. Co = co-manager. Lead = lead underwriter. I define forecast bias for LTG as the difference between forecast and realized LTG. Year 0 is the fiscal year that contains filing date. Number of observations is the number of analyst forecasts.

Panel A: Mean

Forecast made in fiscal year from filing date	Number of observations			Forecast			Realize			Bias		
	Un	Co	Lead	Un	Co	Lead	Un	Co	Lead	Un	Co	Lead
-5	6,759	686	484	13.46	14.92**	13.94	4.68	7.81**	6.75*	8.78	7.11*	7.19
-4	7,160	842	621	14.21	15.08*	14.65	6.30	10.84**	7.81	7.91	4.24**	6.84
-3	8,169	1,057	756	14.42	16.33**	15.82**	8.89	11.89**	11.26**	5.54	4.44	4.56
-2	8,898	1,369	814	14.90	18.17**	17.38**	9.79	12.84**	11.43*	5.11	5.32	5.95
-1	10,143	1,793	1,053	15.49	20.30**	19.05**	7.69	11.64**	10.08**	7.80	8.66	8.97
0	12,513	2,804	1,660	17.01	21.87**	20.86**	5.90	8.12**	7.79**	11.11	13.75**	13.07**
1	12,943	2,276	1,467	17.48	20.35**	19.26**	5.61	6.93**	5.85	11.87	13.42**	13.41*
2	12,107	1,636	1,160	16.69	18.27**	17.41*	5.26	4.50	5.69	11.42	13.76**	11.72
3	11,179	1,164	881	15.73	17.07**	16.41	5.28	4.98	4.96	10.46	12.08*	11.45
4	9,554	818	731	15.45	16.07	14.97	5.33	6.94*	4.92	10.12	9.14	10.05
5	8,212	611	545	15.52	14.93	15.74	6.13	6.85	4.56	9.39	8.08	11.18

Panel B: Median

Forecast made in fiscal year from filing date	Number of observations			Forecast			Realize			Bias		
	Un	Co	Lead	Un	Co	Lead	Un	Co	Lead	Un	Co	Lead
-5	6,759	686	484	12.00	14.00**	12.00	4.48	8.55**	6.77*	6.35	4.92*	4.26
-4	7,160	842	621	12.00	14.00**	14.00*	5.24	9.99**	7.20	5.76	2.81**	4.21
-3	8,169	1,057	756	12.40	15.00**	15.00**	7.35	11.62**	9.57**	4.82	2.58**	4.11
-2	8,898	1,369	814	13.00	16.00**	15.00**	7.89	11.77**	9.69*	4.36	3.77	4.58
-1	10,143	1,793	1,053	14.00	20.00**	18.00**	6.45	11.02**	8.69**	5.95	5.63	6.53
0	12,513	2,804	1,660	15.00	20.00**	20.00**	5.48	7.73**	6.68**	8.31	10.36**	10.80**
1	12,943	2,276	1,467	15.00	20.00**	17.50**	5.42	6.59**	5.16	9.15	9.90*	11.25**
2	12,107	1,636	1,160	15.00	17.00**	15.00*	5.30	4.81	5.46	8.61	11.75**	9.52
3	11,179	1,164	881	15.00	15.00**	15.00*	5.34	4.95	4.58	7.82	9.39*	9.03
4	9,554	818	731	15.00	15.00	14.00	6.01	6.18	6.09	7.24	6.06	7.05
5	8,212	611	545	15.00	14.00	15.00	6.27	6.24	5.35	6.75	5.51	8.59

** Significantly different from unaffiliated analysts at the 1% level.

* Significantly different from unaffiliated analysts at the 5% level.

Table 3: Forecast level, realized LTG, and forecast bias for LTG earnings forecasts around the filing year by analyst coverage

I define forecast bias for LTG as the difference between forecast and realized LTG. Year 0 is the fiscal year that contains filing date. Number of observations is the number of analyst forecasts.

Forecast made in fiscal year from filing date	Number of observations		Forecast		Realize		Bias	
	< 6 analysts	>= 6 Analysts	< 6	>= 6	< 6	>= 6	< 6	>= 6
-5	2,563	5,220	17.15	11.81**	9.44	2.97**	7.72	8.84*
-4	2,962	5,486	17.91	12.30**	12.48	3.71**	5.44	8.59**
-3	3,331	6,466	18.05	12.91**	15.45	6.17**	2.60	6.74**
-2	3,734	7,045	19.22	13.41**	13.81	8.49**	5.41	4.92
-1	4,588	8,159	20.44	13.97**	10.55	7.18**	9.89	6.80**
0	6,664	10,063	22.20	15.40**	7.60	5.65**	14.60	9.74**
1	5,608	10,974	21.36	16.29**	6.55	5.45**	14.80	10.84**
2	4,210	10,607	19.38	15.92**	5.87	5.01*	13.51	10.92**
3	3,223	9,953	18.07	15.18**	5.94	4.94*	12.13	10.23**
4	2,406	8,660	16.97	15.03**	6.13	5.21	10.84	9.82
5	1,933	7,401	16.28	15.26**	6.31	5.99	9.97	9.28

** Significantly different from unaffiliated analysts at the 1% level.

* Significantly different from unaffiliated analysts at the 5% level.

Table 4: Forecast level, realized LTG, and forecast bias for LTG earnings forecasts around the filing year by analyst affiliation without aggregation bias.

Un = unaffiliated analyst. Co = co-manager. Lead = lead underwriter. I define forecast bias for LTG as the difference between forecast and realized LTG. I compute mean forecast bias within each affiliation group by year around filing date within each equity offering; then I calculate the mean of the means of forecast bias of each affiliation type across SEOs. Year 0 is the fiscal year that contains filing date. Number of observations is the number of SEOs that have forecasts for each type of analysts.

Forecast made in fiscal year from filing date	Number of observations			Forecast			Realize			Bias		
	Un	Co	Lead	Un	Co	Lead	Un	Co	Lead	Un	Co	Lead
-5	1,263	363	329	15.28	14.94	14.04*	7.60	6.14	6.38	7.67	8.80	7.66
-4	1,430	449	412	15.98	15.20	14.93*	10.00	10.33	8.02	5.99	4.87	6.91
-3	1,652	552	471	16.55	16.49	15.67	12.47	12.86	11.33	4.08	3.63	4.34
-2	1,822	702	545	16.75	17.98*	17.11	11.69	12.99	11.53	5.06	4.99	5.58
-1	2,092	899	725	17.96	19.99*	19.08*	9.16	10.79	8.86	8.80	9.20	10.21
0	2,415	1,263	1,143	19.57	21.63*	20.79**	6.30	8.18*	7.07	13.26	13.45	13.72
1	2,327	1,089	972	19.60	20.31	19.30	5.99	7.31	5.36	13.62	13.00	13.93
2	2,059	828	798	18.02	18.12	17.30	5.97	5.26	5.35	12.06	12.86	11.95
3	1,781	639	626	16.86	16.66	16.49	6.09	5.09	4.97	10.77	11.56	11.52
4	1,481	488	512	16.01	16.23	15.18	5.77	6.52	5.02	10.24	9.71	10.16
5	1,311	361	385	15.79	15.17	15.12	6.83	6.83	4.31*	8.95	8.33	10.82

** Significantly different from unaffiliated analysts at the 1% level.

* Significantly different from unaffiliated analysts at the 5% level.

Table 5: Forecast level, forecast bias and change in forecast bias for LTG earnings forecasts -2/+2 years around the filing year by analyst affiliation

I define forecast bias for LTG as the difference between forecast and realized LTG. I include only analysts that make forecasts both within 2 years before and within 2 years after filing date. I compute means for forecasts before filing date and also for forecasts after filing date within each analyst. Then, I calculate means of the mean for each analyst affiliation group within each SEO. Lastly, I calculate means of each analyst group across SEOs. Medians are calculated the same way as the means. Number of observations is the number of SEOs.

Panel A: For SEOs that have forecasts from both analysts who works for lead underwriters and unaffiliated analysts.

	Obs.	Means					Medians				
		Forecast		Bias			Forecast		Bias		
		Before	After	Before	After	Change	Before	After	Before	After	Change
All firms											
Lead Mgr	393	17.18	16.95	9.30	12.40	3.10	15.47	15.33	6.17	9.34	2.33
Unaffiliated	393	17.06	16.50	8.98	11.60	2.62	15.00	15.00	5.41	7.64	2.53
Lead Mgr – Unaffiliated		0.12	0.46	0.31	0.79	0.48					
t-stat(Lead – U), Z-stat(Lead-U)		0.16	0.64	0.22	0.51	0.37	0.03	0.62	0.50	0.62	0.50
Firms with less than 6 analysts											
Lead Mgr	173	18.41	18.97	10.97	14.55	3.59	18.00	18.00	8.70	14.23	2.49
Unaffiliated	173	19.75	19.40	12.30	13.96	1.66	19.00	18.00	8.97	12.64	1.80
Lead Mgr – Unaffiliated		-1.34	-0.43	-1.33	0.60	1.93					
t-stat(Lead – U), Z-stat(Lead-U)		-1.24	-0.40	-0.56	0.22	0.94	-1.00	-0.23	-0.39	0.19	0.77
Firms with 6 or more analysts											
Lead Mgr	220	16.21	15.37	7.98	10.71	2.73	13.00	13.75	5.04	8.17	2.10
Unaffiliated	220	14.94	14.21	6.38	9.76	3.38	12.54	12.52	4.25	6.77	3.03
Lead Mgr – Unaffiliated		1.27	1.16	1.60	0.95	-0.65					
t-stat(Lead – U), Z-stat(Lead-U)		1.20	1.26	0.93	0.53	-0.38	0.87	1.03	1.03	0.78	-0.05

(Continued on next page)

Table 5 (Continued)*Panel B:* For SEOs that have forecasts from both analysts who work for co-managers and unaffiliated analysts.

	Obs.	Means					Medians				
		Forecast		Bias			Forecast		Bias		
		Before	After	Before	After	Change	Before	After	Before	After	Change
All firms											
Co-Manager	444	17.94	17.82	7.57	11.87	4.30	16.24	16.33	5.21	8.85	3.65
Unaffiliated	444	17.70	17.52	8.07	11.61	3.54	16.33	16.10	6.20	8.42	2.21
Co-Manager – Unaffiliated		0.25	0.30	-0.50	0.26	0.76					
t-stat(Co – U), Z-stat(Co-U)		0.34	0.44	-0.37	0.19	0.59	0.19	0.43	-0.29	-0.10	0.98
Firms with less than 6 analysts											
Co-Manager	191	21.58	21.15	7.85	13.13	5.29	20.00	21.00	5.66	11.24	5.84
Unaffiliated	191	21.44	21.31	9.31	13.65	4.33	21.50	20.40	9.91	11.66	4.00
Co-Manager – Unaffiliated		0.14	-0.16	-1.47	-0.51	0.95					
t-stat(Co – U), Z-stat(Co-U)		0.13	-0.16	-0.62	-0.22	0.43	0.11	0.08	-0.53	-0.19	0.92
Firms with 6 or more analysts											
Co-Manager	253	15.20	15.31	7.35	10.92	3.56	12.75	13.50	4.73	7.54	2.91
Unaffiliated	253	14.87	14.66	7.13	10.07	2.94	12.00	12.70	4.97	6.52	1.54
Co-Manager – Unaffiliated		0.33	0.65	0.22	0.84	0.62					
t-stat(Co – U), Z-stat(Co-U)		0.36	0.73	0.14	0.49	0.41	0.16	0.47	0.11	0.02	0.54

Table 6: Summary statistics.

I include only SEOs that have both affiliated and unaffiliated analysts who must also make forecasts both before and after filing date. I define *LTGBIAS* as forecast minus realized LTG. Firm size, *Mktcap*, is defined as the firm's market capitalization in the month before the forecast date. Analyst coverage, *NumA*, is defined as the number of outstanding forecasts used in I/B/E/S monthly consensus calculation. *YearIBES* is the number of years since an analyst's first appearance in the I/B/E/S database. *BrokSize* is the number of analysts who work for the same employer during the same forecast year as the analyst who made forecasts. *NumStk* is the number of stocks for which the analyst supplies at least one forecast within the calendar year. *NumInd* is the number of sectors for which the analyst supplies at least one forecast within the calendar year. *StdDev* is the standard deviation of forecasts used for I/B/E/S consensus calculation. *ChangeBIAS* is defined as the average forecast bias within two years after the filing date, minus the average forecast bias within two years before the filing date. *DaysTwoF* is the difference between average days on which forecasts are made before and after filing date. *Proceeds* is defined as the proceeds that equity issuers receive from all markets. *GrossSpread* is the gross spread for each equity issuance, described in percentage terms. Number of observations is the number of analysts-SEO from analysts who make forecasts both before and after the filing date of SEOs that have both affiliated and unaffiliated analysts.

Description	Variable	#obs	Mean	Std. dev.	Min	Max
Before Filing Date						
LTG forecast	F	3,056	15.55	10.46	-6.50	70.00
LTG forecast bias	LTGBIAS	3,056	7.09	18.69	-58.72	72.06
Firm size (\$ billion)	Mktcap	3,056	2.97	5.47	0.03	62.10
Number analyst coverage	NumA	3,056	9.32	4.74	2.00	28.00
Years in IBES	YearIBES	3,056	4.30	3.89	0.00	19.00
Broker size	BrokSize	3,056	57.60	42.92	1.00	315.00
# of stocks	NumStk	3,056	26.62	21.46	1.00	151.00
# of industry	NumInd	3,056	2.58	1.84	1.00	11.00
Standard deviation of forecasts	StdDev	3,056	3.29	2.50	0.00	21.90
After Filing Date						
LTG forecast	F	3,056	15.21	9.90	-14.00	63.33
LTG forecast bias	LTGBIAS	3,056	10.14	19.70	-59.29	72.17
Firm size (\$ billion)	Mktcap	3,056	4.31	7.45	0.03	65.30
Number analyst coverage	NumA	3,056	10.57	4.59	2.00	32.00
Years in IBES	YearIBES	3,056	5.84	3.99	0.00	20.00
Broker size	BrokSize	3,056	63.27	49.48	1.00	337.00
# of stocks	NumStk	3,056	28.20	21.01	1.00	151.00
# of industry	NumInd	3,056	2.62	1.81	1.00	11.00
Standard deviation of forecasts	StdDev	3,056	3.38	2.66	0.00	39.09
Change Forecast Bias	ChangeBIAS	3,056	3.06	19.07	-84.69	95.15
Days between forecasts	DaysTwoF	3,056	560.62	224.60	28.00	1408.00
Proceeds (\$ million)	Proceeds	3,056	221.93	302.65	7.40	2526.10
Gross Spread (%)	GrossSpread	3,056	3.52	1.75	0.23	41.22

Table 7: The relation between forecast bias before the filing date and analyst affiliation.

All variables are the means of the sample within two years before the filing date. The results of OLS regressions without SEO dummies are shown in specification 1 and 2. The results of regressions with SEO dummies are shown in specification 3 to 6. Intercepts in specification 3 to 6 are not reported. T-statistics are reported in parentheses below coefficient estimates.

$$\begin{aligned}
 LTGBIAS_{i,j,t} = & \alpha_0 + \alpha_1 Co_{i,j,t} + \alpha_2 Lead_{i,j,t} + \alpha_3 LnMktcap_{j,t} + \alpha_4 (LnMktcap_{j,t} \times Co_{i,j,t}) \\
 & + \alpha_5 (LnMktcap_{j,t} \times Lead_{i,j,t}) + \alpha_6 NumA_{j,t} + \alpha_7 (NumA_{j,t} \times Co_{i,j,t}) \\
 & + \alpha_8 (NumA_{j,t} \times Lead_{i,j,t}) + \alpha_9 BrokSize_{i,t} + \alpha_{10} YearIBES_{i,t} + \alpha_{11} StdDev_{j,t} \\
 & + \alpha_{12} NumStk_{i,t} + \alpha_{13} NumInd_{i,t} + \beta_i \sum DSEO_{j,t} + \varepsilon_{i,j,t}.
 \end{aligned} \tag{3}$$

Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	6.75 (8.58)**	-18.19 (-1.15)				
Co	1.19 (1.19)	0.20 (0.21)	-0.19 (-0.42)	0.05 (0.13)	-0.07 (-0.17)	7.34 (0.76)
Lead	1.05 (1.05)	0.02 (0.02)	0.69 (1.24)	0.64 (1.20)	0.58 (1.05)	2.30 (0.19)
LnMktcap		1.52 (1.85)		13.63 (6.96)**	13.60 (7.13)**	13.68 (7.10)**
LnMktCap × Co						-0.46 (-0.91)
LnMktCap × Lead						-0.14 (-0.22)
NumA		-0.72 (-3.19)**		-0.24 (-0.89)	-0.26 (-1.00)	-0.31 (-1.21)
NumA × Co						0.25 (1.98)*
NumA × Lead						0.15 (0.95)
BrokSize					-0.00 (-0.99)	-0.00 (-1.09)
YearsIBES					0.10 (2.07)*	0.10 (2.07)*
StdDev					0.54 (1.96)*	0.55 (1.96)*
NumStk					-0.01 (-1.30)	-0.01 (-1.28)
NumInd					-0.08 (-0.84)	-0.08 (-0.85)
Adj. R-Sq	0.00	0.02	0.83	0.84	0.84	0.84
# Analyst-SEO Obs.	3,056	3,056	3,056	3,056	3,056	3,056
SEO Fixed Effect	No	No	Yes	Yes	Yes	Yes

** Significantly different from unaffiliated analysts at the 1% level.

* Significantly different from unaffiliated analysts at the 5% level.

Table 8: The relation between forecast bias after the filing date and analyst affiliation.

All variables are the means of the sample within two years after the filing date. The results of OLS regressions without SEO dummies are shown in specification 1 and 2. The results of regressions with SEO dummies are shown in specification 3 to 6. Intercepts in specification 3 to 6 are not reported. T-statistics are reported in parentheses below coefficient estimates.

$$\begin{aligned}
 LTGBIAS_{i,j,t} = & \alpha_0 + \alpha_1 Co_{i,j,t} + \alpha_2 Lead_{i,j,t} + \alpha_3 LnMktcap_{j,t} + \alpha_4 (LnMktcap_{j,t} \times Co_{i,j,t}) \\
 & + \alpha_5 (LnMktcap_{j,t} \times Lead_{i,j,t}) + \alpha_6 NumA_{j,t} + \alpha_7 (NumA_{j,t} \times Co_{i,j,t}) \\
 & + \alpha_8 (NumA_{j,t} \times Lead_{i,j,t}) + \alpha_9 BrokSize_{i,t} + \alpha_{10} YearIBES_{i,t} + \alpha_{11} StdDev_{j,t} \\
 & + \alpha_{12} NumStk_{i,t} + \alpha_{13} NumInd_{i,t} + \beta_i \sum DSEO_{j,t} + \varepsilon_{i,j,t}.
 \end{aligned} \tag{3}$$

Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	9.56 (11.72)**	10.76 (0.65)				
Co	2.26 (2.09)*	1.71 (1.64)	0.5 (1.10)	0.41 (0.91)	0.52 (1.14)	2.8 (0.32)
Lead	1.43 (1.29)	0.64 (0.62)	0.88 (1.54)	0.87 (1.54)	1.14 (1.93)	0.18 (0.02)
LnMktcap		0.13 (0.16)		9.76 (5.94)**	9.8 (5.95)**	9.85 (5.94)**
LnMktCap × Co						-0.18 (-0.39)
LnMktCap × Lead						0.11 (0.21)
NumA		-0.36 (-1.83)		-0.52 (-1.83)	-0.53 (-1.88)	-0.57 (-2.02)*
NumA × Co						0.16 (1.13)
NumA × Lead						-0.16 (-0.93)
BrokSize					-0.01 (-1.48)	-0.01 (-1.47)
YearsIBES					-0.05 (-0.81)	-0.05 (-0.81)
StdDev					-0.02 (-0.07)	-0.02 (-0.05)
NumStk					-0.02 (-2.59)**	-0.02 (-2.61)**
NumInd					0.13 (1.36)	0.13 (1.36)
Adj. R-Sq	0.00	0.01	0.86	0.87	0.87	0.87
# Analyst-SEO Obs.	3,056	3,056	3,056	3,056	3,056	3,056
SEO Fixed Effect	No	No	Yes	Yes	Yes	Yes

** Significantly different from unaffiliated analysts at the 1% level.

* Significantly different from unaffiliated analysts at the 5% level.

Table 9: The relation between analyst affiliation and change in forecast bias -2/+2 years around the filing date.

All control variables are the means of the sample within two years before the filing date. I estimate the following firm fixed effects regression. T-statistics are reported in parentheses below coefficient estimates.

$$\begin{aligned}
 \text{ChangeBIAS}_{i,j,t} = & \alpha_0 + \alpha_1 \text{LTGBIAS}_{i,j,t} + \alpha_2 \text{Co}_{i,j,t} + \alpha_3 \text{Lead}_{i,j,t} \\
 & + \alpha_4 \text{LnMktcap}_{j,t} + \alpha_5 (\text{LnMktcap}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_6 (\text{LnMktcap}_{j,t} \times \text{Lead}_{i,j,t}) \\
 & + \alpha_7 \text{NumA}_{j,t} + \alpha_8 (\text{NumA}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_9 (\text{NumA}_{j,t} \times \text{Lead}_{i,j,t}) \\
 & + \alpha_{10} \text{Lnproceeds}_{j,t} + \alpha_{11} (\text{Lnproceeds}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_{12} (\text{Lnproceeds}_{j,t} \times \text{Lead}_{i,j,t}) \\
 & + \alpha_{13} \text{GrossSpread}_{j,t} + \alpha_{14} (\text{GrossSpread}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_{15} (\text{GrossSpread}_{j,t} \times \text{Lead}_{i,j,t}) \\
 & + \alpha_{16} \text{BrokSize}_{i,t} + \alpha_{17} \text{YearIBES}_{i,t} + \alpha_{18} \text{DaysTwoF}_{i,j,t} + \alpha_{19} \text{StdDev}_{j,t} + \alpha_{20} \text{NumStk}_{i,t} \\
 & + \alpha_{21} \text{NumInd}_{i,t} + \beta_i \sum \text{DFirm}_j + \varepsilon_{i,j,t}. \tag{4}
 \end{aligned}$$

Explanatory Variable	(1)	(2)	(3)	(4)
LTGBIAS	-0.73 (-15.71)**	-0.75 (-15.18)**	-0.74 (-15.57)**	-0.74 (-15.51)**
Co	0.4 (0.73)	0.36 (0.64)	0.36 (0.63)	2.53 (0.19)
Lead	0.66 (1.10)	0.61 (1.03)	0.8 (1.29)	-16.08 (-0.93)
LnMktcap		1.71 (1.03)	-0.29 (-0.16)	-0.37 (-0.20)
LnMktCap × Co				-0.14 (-0.19)
LnMktCap × Lead				0.84 (0.85)
NumA		-0.25 (-0.62)	-0.24 (-0.61)	-0.26 (-0.63)
NumA × Co				0.13 (0.77)
NumA × Lead				-0.08 (-0.37)
Lnproceeds			3.06 (2.07)*	3.08 (2.02)*
Lnproceeds × Co				-0.04 (-0.06)
Lnproceeds × Lead				-0.22 (-0.26)

** Significantly different from unaffiliated analysts at the 1% level.

* Significantly different from unaffiliated analysts at the 5% level.

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Table 9 (Continued)

Explanatory Variable	(1)	(2)	(3)	(4)
GrossSpread			-0.88 (-1.12)	-0.93 (-1.12)
GrossSpread × Co				-0.02 (-0.06)
GrossSpread × Lead				0.32 (0.89)
BrokSize			0.00 (0.14)	0.00 (0.08)
YearIBES			-0.14 (-2.23)*	-0.15 (-2.26)*
DaysTwoF			0.00 (0.73)	0.00 (0.73)
StdDev			-0.16 (-0.76)	-0.16 (-0.76)
NumStk			-0.00 (-0.22)	0.00 (-0.19)
NumInd			0.12 (0.90)	0.12 (0.89)
Adj R-Sq	0.77	0.77	0.77	0.77
# Analyst-SEO Obs	3,056	3,056	3,056	3,056
# Firms	450	450	450	450

** Significantly different from unaffiliated analysts at the 1% level.

* Significantly different from unaffiliated analysts at the 5% level.

Table 10: The relation between analyst affiliation and change in forecast bias -1/+1 and -3/+3 around filing date.

All control variables are the means of the sample within -1/+1 and -3/+3 around the filing date. I estimate the following firm fixed effects regression. T-statistics are reported in parentheses below coefficient estimates.

$$\begin{aligned}
\text{ChangeBIAS}_{i,j,t} = & \alpha_0 + \alpha_1 \text{LTGBIAS}_{i,j,t} + \alpha_2 \text{Co}_{i,j,t} + \alpha_3 \text{Lead}_{i,j,t} \\
& + \alpha_4 \text{LnMktcap}_{j,t} + \alpha_5 (\text{LnMktcap}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_6 (\text{LnMktcap}_{j,t} \times \text{Lead}_{i,j,t}) \\
& + \alpha_7 \text{NumA}_{j,t} + \alpha_8 (\text{NumA}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_9 (\text{NumA}_{j,t} \times \text{Lead}_{i,j,t}) \\
& + \alpha_{10} \text{Lnproceeds}_{j,t} + \alpha_{11} (\text{Lnproceeds}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_{12} (\text{Lnproceeds}_{j,t} \times \text{Lead}_{i,j,t}) \\
& + \alpha_{13} \text{GrossSpread}_{j,t} + \alpha_{14} (\text{GrossSpread}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_{15} (\text{GrossSpread}_{j,t} \times \text{Lead}_{i,j,t}) \\
& + \alpha_{16} \text{BrokSize}_{i,t} + \alpha_{17} \text{YearIBES}_{i,t} + \alpha_{18} \text{DaysTwoF}_{i,j,t} + \alpha_{19} \text{StdDev}_{j,t} + \alpha_{20} \text{NumStk}_{i,t} \\
& + \alpha_{21} \text{NumInd}_{i,t} + \beta_i \sum \text{DFirm}_j + \varepsilon_{i,j,t}. \tag{4}
\end{aligned}$$

Explanatory Variable	-1/+1 around filing date		-3/+3 around filing date	
	(1)	(2)	(3)	(4)
LTGBIAS	-0.56 (-8.55)**	-0.56 (-8.51)**	-0.78 (-19.75)**	-0.78 (-19.61)**
Co	0.89 (1.54)	-6.91 (-0.44)	0.48 (0.82)	1.95 (0.15)
Lead	0.48 (0.71)	-16.25 (-0.83)	0.59 (0.92)	-18.68 (-1.07)
LnMktcap	-0.86 (-0.44)	-1.10 (-0.55)	-0.31 (-0.20)	-0.34 (-0.22)
LnMktCap × Co		0.32 (0.36)		-0.29 (-0.38)
LnMktCap × Lead		0.78 (0.72)		0.91 (0.94)
NumA	-0.41 (-1.28)	-0.41 (-1.28)	-0.24 (-0.60)	-0.24 (-0.60)
NumA × Co		0.08 (0.42)		0.08 (0.43)
NumA × Lead		-0.07 (-0.31)		-0.06 (-0.26)
Lnproceeds	2.94 (2.16)*	2.89 (2.10)*	3.94 (3.34)	3.78 (3.12)
Lnproceeds × Co		0.26 (0.34)		0.71 (0.97)
Lnproceeds × Lead		-0.06 (-0.07)		-0.24 (-0.32)

** Significantly different from unaffiliated analysts at the 1% level.

* Significantly different from unaffiliated analysts at the 5% level.

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Table 10 (Continued)

Explanatory Variable	-1/+1 around filing date		-3/+3 around filing date	
	(1)	(2)	(3)	(4)
GrossSpread	-1.46 (-1.39)	-1.51 (-1.44)	-1.14 (-1.17)	-1.27 (-1.28)
GrossSpread × Co		-0.23 (-0.35)		0.11 (0.39)
GrossSpread × Lead		0.41 (1.16)		0.58 (1.45)
BrokSize	0.00 (-0.34)	0.00 (-0.54)	0.00 (0.32)	0.00 (0.23)
YearIBES	-0.11 (-1.47)	-0.11 (-1.44)	-0.18 (-2.63)**	-0.19 (-2.69)**
DaysTwoF	0.00 (0.57)	0.00 (0.55)	0.47 (0.97)	0.00 (0.92)
StdDev	-0.12 (-0.46)	-0.12 (-0.46)	-0.04 (-0.25)	-0.03 (-0.20)
NumStk	0.01 (0.66)	0.00 (0.71)	0.00 (-0.40)	0.00 (-0.39)
NumInd	0.05 (0.38)	0.04 (0.35)	0.11 (0.83)	0.11 (0.84)
Adj R-Sq	0.70	0.70	0.77	0.77
# Analyst-SEO Obs	2,076	2,076	3,517	3,517
# Firms	338	338	480	480

** Significantly different from unaffiliated analysts at the 1% level.

* Significantly different from unaffiliated analysts at the 5% level.

Table 11: The relation between analyst affiliation and change in forecast bias after excluding forecasts from unaffiliated analysts who work for the top 25 underwriters.

All control variables are the means of the sample within two years before the filing date. I estimate the following firm fixed effects regression. T-statistics are reported in parentheses below coefficient estimates.

$$\begin{aligned}
 \text{ChangeBIAS}_{i,j,t} = & \alpha_0 + \alpha_1 \text{LTGBIAS}_{i,j,t} + \alpha_2 \text{Co}_{i,j,t} + \alpha_3 \text{Lead}_{i,j,t} \\
 & + \alpha_4 \text{LnMktcap}_{j,t} + \alpha_5 (\text{LnMktcap}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_6 (\text{LnMktcap}_{j,t} \times \text{Lead}_{i,j,t}) \\
 & + \alpha_7 \text{NumA}_{j,t} + \alpha_8 (\text{NumA}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_9 (\text{NumA}_{j,t} \times \text{Lead}_{i,j,t}) \\
 & + \alpha_{10} \text{Lnproceeds}_{j,t} + \alpha_{11} (\text{Lnproceeds}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_{12} (\text{Lnproceeds}_{j,t} \times \text{Lead}_{i,j,t}) \\
 & + \alpha_{13} \text{GrossSpread}_{j,t} + \alpha_{14} (\text{GrossSpread}_{j,t} \times \text{Co}_{i,j,t}) + \alpha_{15} (\text{GrossSpread}_{j,t} \times \text{Lead}_{i,j,t}) \\
 & + \alpha_{16} \text{BrokSize}_{i,t} + \alpha_{17} \text{YearIBES}_{i,t} + \alpha_{18} \text{DaysTwoF}_{i,j,t} + \alpha_{19} \text{StdDev}_{j,t} + \alpha_{20} \text{NumStk}_{i,t} \\
 & + \alpha_{21} \text{NumInd}_{i,t} + \beta_i \sum \text{DFirm}_j + \varepsilon_{i,j,t}. \tag{4}
 \end{aligned}$$

Explanatory Variable	(1)	(2)	(3)	(4)
LTGBIAS	-0.65 (-8.24)**	-0.65 (-8.19)**	-0.64 (-8.60)**	-0.64 (-8.60)**
Co	0.64 (0.80)	0.61 (0.79)	0.74 (0.99)	7.52 (0.32)
Lead	1.26 (1.69)	1.16 (1.53)	1.49 (1.88)	-19.56 (-0.71)
LnMktcap		1.25 (0.44)	-1.89 (-0.70)	-1.94 (-0.69)
LnMktCap × Co				-0.70 (-0.57)
LnMktCap × Lead				1.21 (0.84)
NumA		-0.29 (-0.66)	-0.41 (-0.99)	-0.37 (-0.87)
NumA × Co				0.21 (0.80)
NumA × Lead				-0.49 (-1.81)
Lnproceeds			4.45 (1.82)	4.40 (1.73)
Lnproceeds × Co				0.69 (0.79)
Lnproceeds × Lead				0.08 (0.08)

** Significantly different from unaffiliated analysts at the 1% level.

* Significantly different from unaffiliated analysts at the 5% level.

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Table 11 (Continued)

Explanatory Variable	(1)	(2)	(3)	(4)
GrossSpread			-2.25 (-2.09)*	-2.36 (-2.15)*
GrossSpread × Co				0.78 (0.78)
GrossSpread × Lead				-0.06 (-0.06)
BrokSize			-0.00 (-0.04)	-0.00 (-0.16)
YearIBES			-0.17 (-1.51)	-0.17 (-1.48)
DaysTwoF			-0.00 (-0.52)	-0.00 (-0.54)
StdDev			-0.44 (-1.37)	-0.44 (-1.36)
NumStk			0.01 (0.56)	0.01 (0.52)
NumInd			-0.03 (-0.20)	-0.06 (-0.31)
Adj R-Sq	0.74	0.74	0.75	0.75
# Analyst-SEO Obs	1,170	1,170	1,170	1,170
# Firms	273	273	273	273

** Significantly different from unaffiliated analysts at the 1% level.

* Significantly different from unaffiliated analysts at the 5% level.

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