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THREE ESSAYS ON FINANCIAL ANALYSTS

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YUAN LI

NANYANG BUSINESS SCHOOL

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

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INTRODUCTION

This thesis includes three essays on financial analysts. We propose an *ex ante* measure of an analyst's ability to produce private information (*PPI*), based on how little the analyst's forecast revisions are correlated with prior stock price changes. We find that earnings forecasts issued by analysts with higher *PPI* are more accurate, timelier, bolder, and have greater stock price impacts. We also find evidence that, compared to other *ex ante* measures of analyst ability, *PPI* is incrementally useful and has the greatest power in identifying superior analysts. In addition, using *PPI*, we find that Regulation Fair Disclosure not only equalizes the access of analysts to managerial information, but also encourages their private information production.

In the second essay, we examine the dispersion effect in the international markets. The dispersion effect refers to the phenomenon of the negative relationship between dispersion in analysts' earnings forecasts and future stock returns, which is first documented by Diether et al. (2002) in the US market. We find that this phenomenon is more pronounced in countries with less information uncertainty, which is not consistent with the dominant explanation for the dispersion effect provided in the current literature. We also document a stronger dispersion effect in countries where investors rely more on analysts' earnings forecasts such as those countries with stronger investor protection, more developed markets, have a culture of higher uncertainty avoidance and more accurate earnings

forecasts by analysts. In addition, we document that the upward bias in the consensus earnings forecast and forecast dispersion is positively related around the world. Together, our findings suggest that the dispersion effect arises because investors rely on analysts' forecast for their investment decisions, and that there is an upward bias in consensus earnings forecast which increases with forecast dispersion.

In the third essay, we explain the dispersion effect in the US market as a result of analysts' incentives of not fully downward revising their earnings forecasts when they possess bad news about the firms they cover. Consistent with this conjecture, we find (1) that analysts' incentives simultaneously increase dispersion and induce an upward bias in reported consensus forecasts, (2) that the dispersion effect only exists among firms with bad future earnings, (3) that the dispersion effect disappears once we control for the incentive-induced upward bias in the reported consensus forecast, and (4) that the dispersion effect is stronger among firms with lower information uncertainty. The last two findings offer support unique to our analysts' incentive-based explanation of the dispersion effect.

Chapter 1 The Production of Private Information by Financial Analysts

1.1 Introduction

Financial analysts are important information intermediaries in the capital markets, and the value of analysts lies mainly in their ability to provide private information to the capital markets (Frankel et al. 2006). This paper proposes an *ex ante* measure of analyst ability by focusing on an analyst's production of private information. This measure is based on the correlation coefficient between an analyst's forecast revisions and prior stock price changes, which reflects the sign and magnitude of the association between these two variables. Since prior stock price changes capture the information that is already known to the stock markets, we view any information in an analyst's forecast revisions that is not correlated with prior stock price changes as the analyst's private information. Therefore, we argue that analysts whose forecast revisions exhibit a lower correlation with prior stock returns are better able to produce private information, and therefore these analysts provide greater information value to the stock markets.

In order to construct our measure of an analyst's private information production, we obtain the earnings forecasts for all of the firms followed by an analyst each year and the firms' stock returns between each successive forecast. We then calculate the correlation coefficient between the analyst's forecast revisions and the stock returns within each successive forecast. One minus the correlation coefficient is our measure of the production of private information by

the analyst (PPI)¹. Hence, the higher the PPI , the greater the amount of private information produced by the analyst.

We examine cross-sectional differences in the one-period-ahead forecasting performance of analysts to validate PPI as an *ex ante* measure of an analyst's ability to produce private information. We first investigate the association between PPI and three forecast characteristics: accuracy, timeliness, and boldness. Prior research has shown that these three measures are positively associated with the production of private information by an analyst and are reflective of an analyst's forecasting performance (e.g., Cooper et al. 2001; Gleason and Lee 2003; Clement and Tse 2005). We find evidence that is consistent with PPI being a valid *ex ante* measure of an analyst's ability to produce private information: the one-period-ahead earnings forecasts issued by analysts with higher PPI are more accurate, timelier, and bolder than those issued by analysts with lower PPI .

To further evaluate PPI as an *ex ante* measure of an analyst's ability to produce private information, we examine cross-sectional differences in the stock price impact of analyst forecast revisions. To the extent that investors recognize the differential ability of analysts, forecast revisions by superior analysts will have a greater influence on investors' decisions and hence a greater impact on stock prices (Stickel 1992; Cooper et al. 2001). We find that the one-period-ahead forecast

¹ For example, assuming analyst i covers five firms in year t , and he issues a total of 20 forecast revisions in year t for the five firms he covers. For each forecast revision, we calculate the price changes of the corresponding stock within the revision period. Then we calculate the correlation coefficient of the 20 pairs of forecast revisions and stock price changes. One minus this correlation coefficient is the PPI for analyst i in year t .

revisions issued by analysts with higher *PPI* have a greater impact on stock prices than those issued by analysts with lower *PPI*. In particular, the average abnormal return associated with a one percent change in the forecast revisions of high *PPI* analysts is more than triple that of low *PPI* analysts. This finding suggests that *PPI* is a useful *ex ante* measure for evaluating analysts on their ability to produce private information. In addition, we employ price impact analysis to examine whether *PPI* is incrementally useful to other *ex ante* measures of analyst ability proposed in prior research (Park and Stice 2000; Brown and Mohammad 2009; Cooper et al. 2001). We find that, compared to other *ex ante* measures of analyst ability, *PPI* is not only incrementally useful, but also has the greatest power in identifying superior analysts.

Although we argue that analysts with higher *PPI* generate more private information and thus have superior ability, analysts can also exhibit higher *PPI* if they under-appreciate or ignore the information impounded in stock prices. This is a plausible alternative interpretation since prior research suggests that stock price changes are informative of future earnings (e.g., Collins et al. 1987; Kothari and Sloan 1992) and analysts systematically fail to fully reflect the information contained in stock returns (e.g. Abarbanell 1991; Lys and Sohn 1990). However, under this alternative interpretation, we would not expect to observe that, compared to analysts with lower *PPI*, analysts with higher *PPI* can deliver superior forecasting performance. Thus, our empirical results showing that analysts with

higher *PPI* have superior forecasting performance in terms of greater forecast accuracy, timeliness, boldness, and stock price impact do not support this alternative interpretation. Rather, our results validate *PPI* as a measure of an analyst's ability to produce private information.

We conduct two additional sets of tests to lend further support to our argument that *PPI* is positively related to an analyst's ability to produce private information. First, we examine the relation between *PPI* and several analyst characteristics and behaviors that have been identified in prior research as being associated with the production of private information by analysts (Clement 1999; Herrmann and Thomas 2005; Chen and Matsumoto 2006; Call et al. 2009). We find that, compared to analysts with lower *PPI*, analysts with higher *PPI* (1) are from larger brokerage firms, (2) follow fewer industries, (3) are more likely to issue cash flow forecasts, (4) are more likely to issue optimistic stock recommendations, and (5) are less likely to round off their forecasts. Second, we examine the time series relation between the average price impact of analyst earnings forecast revisions and the average *PPI*. We find that the average price impact of analyst earnings forecast revisions increases with the average *PPI*. This indicates that investors' reactions to analyst forecast revisions are stronger (weaker) during the time periods when the average *PPI* is higher (lower). These two sets of results provide further evidence to support our argument that *PPI* is positively related to the production of private information by analysts.

After being assured that *PPI* is a good measure of an analyst's production of private information, we employ the measure to examine the effect of Regulation Fair Disclosure (RegFD) on the production of private information by analysts. RegFD was implemented by the Stock Exchange Commission (SEC) in October 2000 with the aim of providing market participants with fair access to managerial information. Specifically, RegFD stipulates that if a manager chooses to disclose pertinent information, such information must be disclosed to all parties. Proponents of RegFD argue that prohibiting managers from selective disclosures will mitigate analysts' incentive to issue biased research reports for the purpose of obtaining favored access to managerial information. They also argue that RegFD will motivate analysts to conduct more independent research, thus improving the overall quality of analyst research outputs. Consistent with this view, Mohanram and Sunder (2006) find that the average precision of analysts' private (idiosyncratic) information derived from the model in Barron et al. (1998) has increased after RegFD.² Opponents of RegFD argue that prohibiting non-public communication will reduce the quality and quantity of managerial information and impede analysts' understanding of firm performance. This will consequently reduce the quality of analyst research outputs. Lending support to the opponents of RegFD, Gintschel and Markov (2004) document that the average informativeness

² Barron et al.'s (1998) model captures analysts' private (idiosyncratic) information assuming analyst homogeneity. Therefore, unlike our measure which is analyst specific, their measure of analysts' private information is firm specific and does not allow for differences in analysts' forecasting ability.

of analyst research reports, proxied by absolute stock returns around the announcement date of analyst forecasts, has declined since the implementation of RegFD.

We first investigate whether RegFD is effective in achieving the aim of fair disclosures by examining the association between *PPI* and an analyst's privileged access to managerial information before and after RegFD. Following Chen and Matsumoto (2006), we use the likelihood of issuing optimistic recommendations (*Oprec*) as a proxy for an analyst's privileged access to managerial information. We find a positive and significant relation between *PPI* and *Oprec* before, but not after, RegFD. Thus, our results suggest that RegFD is effective in achieving the aim of fair disclosures because after RegFD the comparative advantage of some analysts relative to other analysts in terms of generating private information can no longer be achieved through privileged access to managerial information.

Second, we investigate how the overall production of private information by analysts is affected by RegFD. Using foreign firms listed in the U.S. as American Depositary Receipts (ADRs), which are exempted from RegFD, to control for the confounding events that affected all traded firms, we find that both the average *PPI* and the average price impact of analyst forecasts have increased since the implementation of RegFD. This suggests that, after RegFD, analysts generate more private information useful to the stock markets. Thus, we find evidence that RegFD not only equalizes the access of analysts to managerial information, but also

encourages analysts to generate their private information from other sources, such as independent research.

This paper contributes to the literature on the identification of superior analysts. For example, prior studies have constructed objective *ex ante* measures of analyst ability based on forecast accuracy and forecast timeliness (Park and Stice 2000; Cooper et al. 2001; Brown and Mohammad 2009). Identifying superior analysts is important to investors, because they rely on analyst earnings forecasts in evaluating firm performance, forming earnings expectations, and setting stock prices. Identifying superior analysts is also of interest to brokerages, as such analysts add to the brokerage's prestige and reputation as well as to the quality of their research products. The financial media often provide various rankings of analysts. For example, both the *Wall Street Journal* and *Institutional Investors* publish annual rankings of analysts based on, among other factors, institutional clients' evaluations of the quality of analyst research outputs. Implicit in these rankings is the assumption that it is possible to identify, *ex ante*, analysts who will continue to provide better quality research in future periods (Chan et al. 2008). However, these rankings have been criticized for rewarding analysts who have been around for a long time or who aggressively lobby their customers for votes (Cooper et al. 2001). In this study, we introduce an *ex ante* measure of analyst ability based on the private information production of analysts. We find our measure is not only incrementally useful to, but also has the greatest power

compared to other *ex ante* measures used in prior studies to identify superior analysts.

This paper also has implications for academic researchers. First, this paper provides an alternative measure of an analyst's production of private information (*PPI*). Prior research has relied mainly on forecast accuracy as a proxy for an analyst's private information. Chen and Matsumoto (2006), for example, argue that analysts with better access to managerial information have higher forecast accuracy. Clement and Tse (2005) conclude that bold forecasts contain more information than herding forecasts because they are more accurate. However, analysts can easily improve on their forecast accuracy by following the forecasts of superior analysts. That is to say, analysts who issue more accurate forecasts may not actually be providing any additional information to the capital markets. Future research could utilize *PPI* to examine issues related to the production of private information by analysts.

Second, academic researchers rely extensively on analyst earnings forecasts in studies covering such topics as earnings management, pricing of accounting information, and cost of capital.³ A common practice in prior research has been to use the average value of analyst earnings forecasts as a proxy for investors' expectations of future earnings. A better understanding of the differential ability of analysts would enable researchers to assign higher weightings to the forecasts of

³ See Schipper (1991), Brown (1993) and Ramnath et al. (2009) for literature reviews on research related to analysts' forecasts and stock recommendations.

superior analysts in order to generate more accurate proxies for expected earnings, which would strengthen the power of their research findings.

Finally, this paper contributes to the literature investigating the effects of RegFD on the production of information and the quality of research outputs by analysts (e.g. Heflin et al. 2003; Bailey et al. 2003; Gintschel and Markov 2004; Mohanram and Sunder 2006; Francis et al. 2006; Agrawal et al. 2006). Our results suggest that RegFD is effective in prohibiting managers from making selective disclosures and motivating analysts to conduct more independent research. Therefore, we provide evidence that lends credence to the claims made by proponents of RegFD that the regulation will improve the overall quality of analysts' research outputs.

The rest of this paper proceeds as follows: In Section 1.2, we develop our hypothesis. Section 1.3 describes the estimation procedure for *PPI*, our proxy for an analyst's production of private information. Section 1.4 details the empirical tests and results on the association between *PPI* and the accuracy, timeliness, boldness, and price impact of analyst earnings forecasts. Section 1.5 examines the association between *PPI* and several forecast characteristics and behaviors of analysts, and presents the time-series analysis of the average *PPI* and the average price impact of analyst forecasts. Section 1.6 details the analysis on the effects of RegFD on the production of private information by analysts. Section 1.7 concludes the paper.

1.2 Background and Hypothesis

In the context of mutual fund managers, Kacperczyk and Seru (2007) argue that the ability of a mutual fund manager to produce private information will be positively associated with fund performance. They develop a rational expectation model which shows that the ability of a mutual fund manager to produce private information can be calibrated by the extent to which she uses information in the public domain to revise her fund holdings. In their model, private information can come from two sources: (1) more accurate processing of public signals, and (2) receiving private signals. Kacperczyk and Seru (2007) use analyst stock recommendations to proxy for information in the public domain, and predict that fund managers whose changes in fund holdings are more sensitive to changes in analyst stock recommendations are less able to produce private information. They find evidence that is consistent with their prediction: mutual fund managers with superior ability to produce private information perform better than those with less ability.

In this paper, our focus is on the association between private information production and the forecasting performance of financial analysts. Financial analysts are prominent information intermediaries who engage in private information searches, perform prospective analyses aimed at forecasting a firm's future earnings and cash flows, and conduct retrospective analyses that interprets past events (Beaver 1998, 10). Frankel et al. (2006) argue that the information

value of analysts derives mainly from their supply of information that is not already available to the stock markets. However, prior research shows that, on average the sign and magnitude of analyst forecast revisions are positively associated with the sign and magnitude of *prior* security price changes (e.g., Brown et al. 1985; Lys and Sohn 1990; Abarbanell 1991). This empirical result raises concerns about the role of analysts in creating information value, which is explicitly expressed in Brown, Foster and Noreen (1985, 139): “An important but unresolved issue is the extent to which analysts use prior security returns as a basis for revising their earnings forecasts. It would be difficult to explain, from an empirical survival perspective, the continued employment of analysts to forecast earnings if all they are doing are re-expressing, in earnings forecast format, the information already available in publicly observable datum such as security prices.” This comment by Brown et al. (1985) implies that the extent to which analysts uses prior security returns to revise their forecasts reflects their ability to produce private information.

The above discussion suggests that we can calibrate an analyst’s ability to produce private information by determining the extent to which the analyst’s forecast revisions are correlated with prior stock price changes. Stock price changes reflect the information known to the stock markets. Therefore, any information in an analyst’s forecast revisions beyond that already impounded in stock price changes can be considered as the analyst’s private information.

Analysts can derive private information not only from access to proprietary information unknown to the stock markets, but also from having a better understanding and interpretation of public information than the stock markets. For example, when a firm announces its earnings, both investors and analysts observe this public signal. Accordingly, both stock price changes and forecast revisions will occur. Even though analysts are using the public earnings announcement to revise their forecasts, as long as their forecast revisions contain more information than that reflected in stock price changes, they are generating private information and adding information value to the stock markets.

We measure the extent to which an analyst uses prior stock price changes to revise her forecasts using the correlation coefficient between the analyst's forecast revisions and prior stock returns. Mathematically, the correlation coefficient reflects both the direction and the strength of the association between these two variables. The lower (higher) the correlation coefficient, the less (more) the analyst uses prior security returns to revise her forecasts and the greater (less) the amount of private information she produces. A greater amount of private information allows the analyst to achieve superior forecasting performance. We examine the following hypothesis:

H1: Analysts whose forecast revisions are less correlated with prior stock returns are better at producing private information, and hence have better forecasting performance compared to analysts whose forecast revisions are more

correlated with prior stock returns.

1.3 Construction of *PPI*

In this section, we discuss how we construct our measure of an analyst's production of private information, *PPI*. We retrieve earnings forecasts data from *IBES* and daily stock returns data from *CRSP*. We use both annual and quarterly forecasts to obtain a larger number of observations per analyst so that the estimation of the correlation coefficient between the analyst's forecast revisions and prior stock returns can be more precise. We calculate the forecast revision ($FR_{i,j,\tau,t}$) issued by analyst i for firm j at time τ in calendar year t using the following formula:

$$FR_{i,j,\tau,t} = \frac{F_{i,j,\tau,t} - F_{i,j,\tau-1,t}}{|F_{i,j,\tau-1,t}|}$$

where $F_{i,j,\tau,t}$ is the earnings forecast released by analyst i for firm j at time τ in calendar year t and $F_{i,j,\tau-1,t}$ is the most recent earnings forecast pertaining to the same forecast period by analyst i for firm j prior to $F_{i,j,\tau,t}$. The forecast revision ($FR_{i,j,\tau,t}$) is the difference between $F_{i,j,\tau,t}$ and $F_{i,j,\tau-1,t}$, scaled by the absolute value of $F_{i,j,\tau-1,t}$. To avoid potential data errors, we require the time intervals between $F_{i,j,\tau,t}$ and $F_{i,j,\tau-1,t}$ to be more than 10 days and less than 200 days. For each forecast revision, we then calculate the buy and hold return ($RET_{i,j,\tau,t}$) of firm j over the period from the day of the issuance of $F_{i,j,\tau-1,t}$ to one day before the issuance of $F_{i,j,\tau,t}$. We winsorize $FR_{i,j,\tau,t}$ and $RET_{i,j,\tau,t}$ at the 1st and 99th percentiles in order to

mitigate the effects of outliers. We require at least 20 pairs of $FR_{i,j,\tau,t}$ and $RET_{i,j,\tau,t}$ for each analyst i in calendar year t to calculate the *Pearson* correlation coefficient between these two variables. One minus the correlation coefficient is our measure of an analyst's production of private information (*PPI*). The higher (lower) the *PPI*, the greater (lesser) the amount of private information produced by the analyst.

The above procedure results in a sample of 42,194 observations for the analyst-year specific *PPI* over the sample period from 1987 to 2007.⁴ In Table 1.1, we report descriptive statistics of *PPI* by calendar year and for the whole sample period. As shown in the last row of Table 1.1, the average *PPI* is 0.784 over the whole sample period. This suggests that the average correlation coefficient between analyst forecast revisions and prior stock returns is 0.216 ($= 1-0.784$), which is consistent with the findings in prior research that, on average, analyst forecast revisions are positively correlated with prior stock returns. The standard deviation of *PPI* is 0.202, which indicates a substantial variation among analysts with regard to their production of private information. The last column in the Table 1.1 shows the average number of paired observations of $FR_{i,j,\tau}$ and $RET_{i,j}$ in the calculation of *PPI* for each analyst in each calendar year (*Number*). As reported, on average, 114 paired observations are used to estimate the *PPI* for each analyst in each calendar year.

⁴ The sample period starts in 1987 because, prior to 1987, the average number of paired observations of $FR_{i,j,\tau}$ and $RET_{i,j}$ is small. In addition, in most of our empirical tests, we require data on the number of industries covered by analysts and the data on industry classification available in *COMPUSTAT* prior to 1987 is scant.

1.4 Evaluating Forecasting Performance

Our hypothesis is that, compared to analysts whose forecast revisions are more correlated with prior stock returns, analysts whose forecast revisions are less correlated with prior stock returns are better at producing private information and hence have better forecasting performance. In this section, we test our hypothesis by examining the association between *PPI* and several proxies for an analyst's forecasting performance that are related to an analyst's private information production. We first examine the relations between *PPI* and three forecast characteristics: accuracy, timeliness, and boldness, respectively because prior research has argued that these three measures are positively associated with the production of private information by analysts and are reflective of an analyst's forecasting performance (e.g., Stickel 1992; Hong and Kubik 2003; Cooper et al. 2001; Gleason and Lee 2003; Clement and Tse 2005). Next, we examine the stock price impact of forecast revisions issued by analysts with different *PPI*. This is because forecast revisions containing more private information provide more value to the stock markets and have a greater impact on stock prices (Cooper et al. 2001; Gleason and Lee 2003). Finally, using stock price impact analysis, we examine whether *PPI* is incrementally useful in the identification of superior analysts compared to other *ex ante* measures of analyst ability based on forecast accuracy, timeliness, and boldness.,

In the following empirical tests, we exclude firms with a non-December fiscal year end to align *PPI* estimated in calendar year t with analyst forecasting performance pertaining to fiscal year $t+1$. This also ensures that the information about prior calendar year *PPI* is available when analysts issue their forecasts for a firm's current fiscal year. We also exclude firms announcing their earnings more than three months after fiscal year end. Finally, to allow for a cross-sectional examination of analysts' differential ability of their analyses conducted on the same firm, we exclude firms which are followed by fewer than four analysts with available *PPI*. However, the sample size differs for each empirical test because of the different research designs and data requirements. We detail such differences and report the sample size in each empirical analysis⁵.

1.4.1 Forecast Accuracy

Forecast accuracy is an important aspect in determining forecasting performance and it affects both analyst reputation and compensation. For example, Stickel (1992) finds that more accurate forecasters are more likely to be selected to the roster of the All-Star analyst list, and Hong and Kubik (2003) find that more accurate forecasters are more likely to move to larger and more prestigious brokerages. Moreover, many studies assume that forecast accuracy is positively associated with an analyst's production of private information (e.g., Clement and Tse 2005; Chen and Matsumoto 2006; Park and Stice 2000; Brown and

⁵ Results are similar when all the tests are conducted using an identical sample.

Mohammad 2009). Consistent with the assumption that more accurate forecasts contain more private information, both Park and Stice (2000) and Brown and Mohammad (2009) find that more accurate forecasters identified in the current period issue forecasts in the subsequent periods that have a greater price impact than those issued by less accurate forecasters. Hence, if *PPI* captures the production of private information by analysts and is positively associated with forecasting performance, we expect to find that, compared to analysts with lower *PPI*, analysts with higher *PPI* will issue more accurate forecasts.

We examine the relation between *PPI* and the accuracy of analysts' future forecasts using the following regression:

$$SACC_{i,j,t} = \beta_0 + \beta_1 SPPI_{i,j,t-1} + \beta_2 SCHAR_{i,j,t-1} + \varepsilon_{i,j,t} \quad (1.1)$$

Consistent with Clement and Tse (2005), we scale all of the variables in the above regression to range from 0 to 1. This is to allow for the comparison of regression coefficients and to enable the comparison of analyst earnings forecasts across different firms and time periods. $SACC_{i,j,t}$ is the relative forecast accuracy of analyst i for firm j in year t and is calculated using the following formula:

$$SACC_{i,j,t} = \frac{AFEMAX_{j,t} - AFE_{i,j,t}}{AFEMAX_{j,t} - AFEMIN_{j,t}}$$

$AFE_{i,j,t}$ is the absolute forecast error of analyst i for firm j in year t , where forecast error is measured as actual earnings minus forecasted earnings. $AFEMAX_{j,t}$ ($AFEMIN_{j,t}$) is the maximum (minimum) absolute forecast error of all of the

analysts following firm j in year t . A high value for $SACC_{i,j,t}$, therefore, indicates that, relative to other analysts who follow firm j in year t , analyst i scores high in forecast accuracy.

$SPPI_{i,j,t-1}$ is the PPI for analyst i in year $t-1$ relative to the PPI in year $t-1$ of all of the other analysts who also cover firm j in year t . Specifically, $SPPI_{i,j,t-1}$ is measured as:

$$SPPI_{i,j,t-1} = \frac{PPI_{i,j,t-1} - PPIMIN_{j,t-1}}{PPIMAX_{j,t-1} - PPIMIN_{j,t-1}}$$

$PPI_{i,j,t-1}$ is the PPI for analyst i in year $t-1$, and $PPIMAX_{j,t-1}$ ($PPIMIN_{j,t-1}$) is the maximum (minimum) value of PPI in year $t-1$ among all of the analysts following firm j in year t . Higher $SPPI_{i,j,t-1}$ indicates that, relative to other analysts who follow firm j in year t , analyst i produces more private information.

In Equation (1.1), we include several control variables ($SCHAR_{i,j,t-1}$) which have been documented in prior research as affecting forecast accuracy. These control variables include the size of the brokerage employing the analyst, the general and firm specific forecasting experience of the analyst, the number of firms and industries covered by the analyst, and the firm specific forecast frequency of the analyst.⁶ Similarly, we scale these control variables based on the following formula:

$$SCHAR_{i,j,t-1} = \frac{CHAR_{i,j,t-1} - CHARMIN_{j,t-1}}{CHARMAX_{j,t-1} - CHARMIN_{j,t-1}}$$

⁶ We detail the definition of these control variables in the tables.

$CHAR_{i,j,t-1}$ is the value of the control variable for analyst i in year $t-1$, and $CHARMAX_{j,t-1}$ ($CHARMIN_{j,t-1}$) is the maximum (minimum) value of the control variable in year $t-1$ among all of the analysts who cover firm j in year t . A high value of $SCHAR_{i,j,t-1}$ indicates that analyst i scores high in this particular control variable relative to other analysts following firm j in year t . In addition, we include firm-year fixed effects in Equation (1.1) to control for any unknown systematic differences across firm and time and to mitigate the inflation of t statistics arising from the correlation among the error terms. We calculate t statistics using analyst clustered standard errors.

Consistent with prior research, we examine the accuracy of analysts' most recent annual earnings forecast for each firm prior to the announcement of actual earnings.⁷ We require all forecasts to be made at least five days prior to the earnings announcement dates. To control for stale forecasts, we also require the forecasts to be issued within 120 days prior to the announcements of actual earnings (Lehavy 2009; Sinha et al. 1997).⁸ This results in a sample of 158,728 analyst-firm-year observations.

In Panel A of Table 1.2, we report the results of regressing $SACC$ on $SPPI$ with and without the control variables. As shown in this table, the coefficient on $SPPI$ is positive (0.01) and significant (t -statistics = 2.29). This suggests that

⁷ Note that firms usually announce their earnings within three months after the fiscal year end date. Therefore, the most recent forecast by each analyst for each firm can occur after the fiscal year end date.

⁸ We repeat all of the analyses using 90 days rather than 120 days as the cutoff. We obtain similar results and our inferences remain unchanged.

forecasts issued by analysts with higher *PPI* are relatively more accurate than those issued by analysts with lower *PPI*. This is consistent with our hypothesis that, compared to analysts with lower *PPI*, analysts with higher *PPI* deliver superior forecasting performance.

Regarding the control variables, the reported coefficients are largely consistent with existing research findings. For example, consistent with Clement (1999), we find that analysts employed by larger brokerages and with more firm specific experience issue more accurate forecasts, as is indicated by the positive coefficient on *SBROSIZE* and *SFEXP*. Also consistent with Clement (1999), we find that forecast accuracy decreases with the number of companies and industries followed by the analyst as the coefficients on *SNFIRM* and *SNSIC* are both significantly negative. In addition, the coefficient on *SFOREFREQ* is significantly positive, which indicates that forecast accuracy is positively associated with forecast frequency (Jacob et al. 1999).

1.4.2 Forecast Timeliness

Analysts have incentive to issue accurate forecasts because forecast accuracy is an important determinant of analyst compensation and reputation. One way for analysts of lesser ability to improve their forecast accuracy is to delay their issuance of forecasts in order to observe and use the information contained in the forecasts of more timely (and arguably superior) analysts. Therefore, using forecast accuracy to calibrate analyst ability and forecasting performance may be non-

diagnostic.

Cooper et al. (2001) suggest that forecast timeliness is another important measure of an analyst's forecasting performance. They argue that comparative advantages in analyzing particular companies may permit superior analysts to release timelier forecasts that reflect a greater amount of private information not known to the capital markets. Consistent with this, Cooper et al. (2001) find that timelier forecasts have a greater price impact during their release period. Moreover they suggest that analysts have incentive to issue their earnings forecasts before other analysts in order to capture trading volume for their brokerage firm. This is because a brokerage firm's profits depend directly on commission revenues and analysts' compensation is based, in part, on the trading volume generated by their research outputs. As such, we examine the association between *PPI* and forecast timeliness and expect to find that analysts with higher *PPI* issue timelier forecasts than analysts with lower *PPI*.

We follow Cooper et al. (2001) in measuring the timeliness of analyst forecasts. Specifically, for each annual earnings forecast issued by analyst i for firm j at time τ in year t , we count the cumulative number of days by which the preceding two forecasts (for the same firm and forecast period) of other analysts lead the forecast of analyst i ($LeadDays_{i,j,\tau,t}$). The longer the $LeadDays_{i,j,\tau,t}$, the less likely it is that analyst i is simply following the forecasts of other analysts. Similarly, we count the cumulative number of days by which the subsequent two

forecasts (for the same firm and forecast period) of other analysts follow the forecast of analyst i ($FollowDays_{i,j,\tau,t}$). The shorter the $FollowDays_{i,j,\tau,t}$, the more likely it is that other analysts are simply following the forecast by analyst i . We then calculate the ratio of $LeadDays_{i,j,\tau,t}$ to $FollowDays_{i,j,\tau,t}$ ($LFRatio_{i,j,\tau,t}$), which captures the timeliness of analyst i 's forecast for firm j issued at time τ in year t . The higher the ratio, the timelier the forecast of analyst i , as it is less likely to follow the forecasts of other analysts but is more likely to be followed by the forecasts of other analysts. The analyst-firm-year specific forecast timeliness ($TIME_{i,j,t}$) is calculated by taking the average of $LFRatio_{i,j,\tau,t}$ based on all forecasts issued by analyst i for firm j in year t , excluding those issued within three days surrounding any of firm j 's earnings announcements (i.e., day -1 to 1, where day 0 is the earnings announcement date).⁹ Thus, a higher $TIME_{i,j,t}$ indicates greater timeliness.

We examine the relation between PPI and the timeliness of analyst forecasts using the following regression:

$$STIME_{i,j,t} = \beta_0 + \beta_1 SPPI_{i,j,t-1} + \beta_2 SCHAR_{i,j,t-1} + \varepsilon_{i,j,t} \quad (1.2)$$

$STIME_{i,j,t}$ is the forecast timeliness of analyst i for firm j in year t relative to other analysts following firm j in year t . Similar to our investigation of forecast accuracy, we scale the variables to range from 0 and 1 in order to allow the comparison of

⁹ The results are similar if we do not exclude forecasts issued within three days surrounding any of firm j 's earnings announcements in the calculation of forecast timeliness.

regression coefficients and the comparison of analyst forecasts across different firms and time periods. Accordingly, $STIME_{i,j,t}$ is calculated as:

$$STIME_{i,j,t} = \frac{TIME_{i,j,t} - TIME_{MIN_{j,t}}}{TIME_{MAX_{i,j,t}} - TIME_{MIN_{j,t}}}$$

$TIME_{MAX_{i,j,t}}$ ($TIME_{MIN_{i,j,t}}$) is the maximum (minimum) forecast timeliness of all of the analysts following firm j in year t . Thus a high value of $STIME_{i,j,t}$ indicates that analyst i scores high in forecast timeliness relative to other analysts who follow firm j in year t . $SPPI_{i,j,t-1}$ and $SCHAR_{i,j,t-1}$ are as defined in Equation (1.1). In Equation (1.2), we also include firm-year fixed effects and calculate t test statistics based on analyst clustered standard errors.

Note that, unlike our analysis of forecast accuracy, the sample examining forecast timeliness is not restricted to the most recent annual earnings forecasts for each firm prior to the announcement of actual earnings. This is because, as the announcement date of actual earnings is near, the most recent annual earnings forecasts of many analysts are not followed by the forecasts of other analysts. Thus, if we restrict our observations to only the most recent annual forecasts, timeliness cannot be calculated for many analysts. Rather, $TIME_{i,j,t}$ is calculated based on all annual earnings forecasts issued by analyst i for firm j in year t . As a result of the more stringent data requirement to obtain $TIME_{i,j,t}$ (each analyst's forecast must be preceded and followed by two other analysts' forecasts), we have

a sample of 90,058 analyst-firm-year observations in the analysis of forecast timeliness.

We report the results of regressing *STIME* on *SPPI* in Panel B of Table 1.2, both with and without the control variables. As shown in this table, the coefficient on $SPPI_{i,j,t-1}$ is positive (0.03) and significant (t -statistics = 5.81). This is consistent with our expectation that forecasts by analysts with higher *PPI* are timelier than those by analysts with lower *PPI*. This result provides further support for our hypothesis that analysts with higher *PPI* have a superior ability to produce private information and thus deliver superior forecasting performance.

With regard to the control variables, we find positive and significant coefficients on *SBROSIZE* and *SFOREFREQ*, which suggests that forecasts by analysts from larger brokerage firms and with higher forecasting frequency are timelier. We also find that the coefficients on *SNFIRM* and *SNSIC* are negative and significant, suggesting that analysts who cover more firms and industries issue less timely forecasts. Finally, we find that the coefficient on *SGEXP* is negative and significant, which suggests that the forecasts of less experienced analysts are timelier.

1.4.3 Forecast Boldness

A forecast revision may either converge to or diverge from the current consensus forecast. In the extant literature, diverging forecasts and converging forecasts are respectively termed as bold forecasts and herding forecasts. Gleason

and Lee (2003) show that, compared to herding forecasts, stock price reactions are stronger for bold forecasts and suggest that bold forecasts contain more new information that is unknown to the capital markets. Clement and Tse (2005) further find that bold forecasts are more accurate than herding forecasts and that, for bold forecasts, the association between forecast revisions and forecast errors are mitigated. Clement and Tse (2005) argue that their findings are consistent with bold forecasts incorporating more private information about upcoming earnings than herding forecasts. Thus, to the extent that bold forecasts contain more private information and reflect better forecasting performance, we expect to observe that analysts with higher *PPI* are more likely to issue bold forecasts.

We use the following logistic regression to examine the relation between *PPI* and forecast boldness:

$$BOLD_{i,j,t} = \beta_0 + \beta_1 SPPI_{i,j,t-1} + \beta_2 SCHAR_{i,j,t-1} + \varepsilon_{i,j,t} \quad (1.3)$$

As in our investigation of forecast accuracy, we examine the most recent annual earnings forecast for each firm prior to the announcement of actual earnings. $BOLD_{i,j,t}$ is an indicator variable, which equals 1 if the most recent annual forecast of analyst i for firm j in year t is a bold forecast, and 0 otherwise. $BOLD_{i,j,t}$ is defined the same as in Clement and Tse (2005). Specifically, $BOLD_{i,j,t}$ is set to 1 if analyst i 's most recent annual forecast is above both the analyst's prior forecast and the consensus forecast immediately before the forecast revision. $BOLD_{i,j,t}$ is also set to 1 if analyst i 's most recent annual forecast is below both the analyst's prior

forecast and the consensus forecast immediately before the forecast revision. Otherwise, $BOLD_{i,j,t}$ is set to 0. The consensus forecast is based on forecasts issued in the 90 days prior to analyst i 's forecast revision. $SPPI_{i,j,t-1}$ and $SCHAR_{i,j,t-1}$ are as defined in Equation (1.1). We also include firm-year fixed effects and calculate the *chi-square* statistics based on the analyst clustered standard errors in Equation (1.3).

We use similar sample criteria to that used in our investigation of forecast accuracy. That is to say, besides focusing only on the most recent annual earnings forecast for each firm prior to the announcement of actual earnings, we also exclude forecasts which are issued within 5 days of and more than 120 days before the announcements of actual earnings. These criteria result in 141,975 analyst-firm-year observations. This sample size is slightly smaller than that for the analysis of forecast accuracy because, in order to classify the forecast as bold or not, we require each forecast to have a preceding forecast.

We report the results of regressing $BOLD$ on $SPPI$ in Panel C of Table 1.2, both with and without controlling for other variables. As reported, we find that the coefficient on $SPPI$ is positive (0.06) and significant (p -value = 0.003), indicating that analysts with higher PPI are more likely to issue bold forecasts. Thus, the results are consistent with our hypothesis that, compared to analysts with lower PPI , analysts with higher PPI are better at producing private information and thus have better forecasting performance.

The coefficients on the control variables are largely consistent with those documented in Clement and Tse (2005). For example, as indicated by the positive and significant coefficients on *SBROSIZE*, *SGEXP*, and *SFOREFREQ*, we find that analysts from larger brokerage firms and with more general experience and higher forecast frequency are more likely to issue bold forecasts. We also find negative and significant coefficients on *SNFIRM* and *SNSI*, which indicate that analysts who follow more firms and industries are less likely to issue bold forecasts.

1.4.4 Stock Price Reactions to Analyst Forecast Revisions

Prior research (Park and Stice 2000; Brown and Mohammad 2009; Cooper et al. 2001) shows that the superiority of analysts can be calibrated by the stock price impact of their forecast revisions: to the extent that investors realize the differential ability of analysts, the forecast revisions of superior analysts will have a greater impact on stock prices than those of other analysts. Thus, according to our hypothesis, we expect to find that forecast revisions by analysts with higher *PPI* have a greater impact on stock prices than forecast revisions by analysts with lower *PPI*.

We use the following regression to compare the stock price impact of forecast revisions issued by analysts with high and low *PPI*:

$$\begin{aligned} Exret_{i,j,t} = & \beta_0 + \beta_1 FR_{i,j,t} + \beta_2 FR_{i,j,t} * HPPI_{i,j,t-1} + \beta_3 HPPI_{i,j,t-1} \\ & + \beta_4 FR_{i,j,t} * HCHAR_{i,j,t-1} + \beta_5 HCHAR_{i,j,t-1} + \varepsilon_{i,j,t} \end{aligned} \quad (1.4)$$

We focus on the price impact of each analyst's most recent annual earnings forecast prior to the announcement of actual earnings for each firm. $Exret_{i,j,t}$ is the size-adjusted return in percentage on the day when the most recent forecast revision prior to the announcement of actual earnings for firm j is issued by analyst i in year t . Size-adjusted return is calculated as the daily stock return minus the return of the corresponding size decile portfolio based on stocks traded on the *NYSE*, *AMEX*, and *NASDAQ*. Since we want to pinpoint the exact price impact of a particular forecast, we examine the size-adjusted return only on the forecast announcement date.¹⁰

$FR_{i,j,t}$ denotes forecast revision, which is calculated as analyst i 's forecast for firm j in year t minus her most recent prior forecast scaled by the absolute value of the most recent prior forecast. We measure forecast revision based on the analyst's own prior forecast, because the finding in Stickel (1991) suggests that, compared to the most recent consensus forecast, an analyst's own prior forecast is a better measure of the market expectation of that analyst's current forecast. We use the absolute value of prior forecast rather than the stock price as the scaler in order to mitigate the confounding effect of prices on returns.¹¹ In all of our price impact analyses, we winsorize $FR_{i,j,t}$ at the 1st and 99th percentiles to mitigate the effect of

¹⁰ Earnings forecasts by different analysts can be issued in close time proximity to each other. Extending the measurement of the abnormal return for a particular forecast to beyond the actual announcement date may result in the capturing of the announcement effects of other earnings forecasts issued before or after that particular forecast.

¹¹ We conduct sensitivity tests using (1) the most recent consensus forecast instead of the analyst's own most recent prior forecast to measure forecast revision, and (2) stock price as the scaler. Our results are qualitatively similar and do not change our inferences.

extreme value of forecast revision. $HPPI_{ij,t-1}$ is a dummy variable, which is equal to 1 if the PPI for analyst i in year $t-1$ is above the median PPI in year $t-1$ of all of the analysts who follow firm j in year t , and 0 otherwise.

In Equation (1.4), we include firm-year fixed effects and several control variables which are likely to affect the price impact of analyst forecast revisions. These control variables are the size of the brokerage employing the analyst, the analyst's general and firm specific forecasting experience, the number of firms and industries covered by the analyst, and the firm specific forecast frequency of the analyst. Similar to $HPPI$, we create dummy variables ($HCHAR$) based on these control variables and interact the dummy variables with analyst forecast revision. Specifically, $HCHAR_{ij,t-1}$ is equal to 1 if the control variable for analyst i in year $t-1$ is above the median of the corresponding control variable in year $t-1$ for all of the analysts who cover firm j in year $t-1$, and 0 otherwise. Our interest is in the coefficient of the interaction term between FR and $HPPI$ ($FR*HPPI$). Consistent with our hypothesis that analysts with higher PPI have better forecasting performance, we expect this coefficient to be positive and significant.

We use similar sample criteria to those in our investigation of forecast accuracy. In particular, besides focusing on each analyst's most recent annual earnings forecast for each firm prior to the announcement of actual earnings, we also exclude forecasts which are issued within 5 days of and more than 120 days before the announcements of actual earnings. However, in this analysis we require

daily stock return data from *CRSP*. To mitigate the confounding effect of earnings announcements on stock prices, we also exclude forecasts issued within three days surrounding any of the firm's earnings announcements (i.e., day -1 to 1, where day 0 is the earnings announcement date). These requirements result in 87,701 analyst-firm-year observations.

We report the results of Equation (1.4) in Table 1.3, as well as a base case with only *HPPI* included in the regression. We find that the coefficient on the interaction term *FR*HPPI* is positive (0.22) and significant (*t-statistics* = 3.92), suggesting that the stock price reactions to the forecast revisions of analysts with high *PPI* are significantly stronger compared to that of analysts with low *PPI*. Note that, in terms of economic significance, a one hundred percent change in forecast surprise is accompanied by a one-day abnormal return of 0.30% (= 0.08% + 0.22%) for analysts with high *PPI*. This is more than triple that of analysts with low *PPI* (0.08%). Thus, the results are consistent with our hypothesis that analysts with higher *PPI* are better at producing private information and thus generate more information value to the markets.¹²

With regard to the control variables, we find that the forecast revisions of analysts from larger brokerage firms and with longer firm specific experience have

¹² The average yearly correlation between *PPI* and median firm size, book to market, and momentum in analyst coverage over the sample period is 0.03, 0.02 and 0.01 respectively. These low correlations suggest that there are no systematic differences in the risk characteristics of firms followed by analysts with different *PPI*, thus alleviating the concern that the significantly positive coefficient on *FR*HPPI* is because *PPI* is related to firms' risk characteristics. We also conduct a robust test by including the interactions of *FR* with (1) firm size, (2) book to market, and (3) momentum in the regression model; our conclusions do not change.

a larger impact on stock prices, as is indicated by the positive and significant coefficients on $FR*HBROSIZE$ and $FR*HFEXP$. We also find a negative and significant coefficient on $FR*HNSIC$, suggesting that the market reacts more strongly to forecast revisions by analysts covering fewer industries.

1.4.5 Comparing PPI with Other Ex-ante Measures of Analyst Ability

As argued above, the accuracy, timeliness, and boldness of forecasts are important to investors, and there is evidence that investors identify superior analysts based on these characteristics. For example, prior literature shows that those analysts with higher prior forecast accuracy or timeliness subsequently issue forecasts that have a larger impact on stock prices (Park and Stice 2000; Cooper et al. 2001; Brown and Mohammad 2009). In this section, we compare *PPI* to other *ex ante* measures of analyst ability based on forecast accuracy, timeliness, and boldness. We do this by comparing the stock price reactions to the subsequent forecast revisions of superior analysts identified on the basis of each measure.

Note that *PPI* is an analyst specific measure of an analyst's ability to produce private information.¹³ In order to compare the four *ex ante* measures of analyst ability on an equal basis, we construct analyst specific measures of forecast accuracy (*ABACC*), timeliness (*ABTIME*), and boldness (*ABBOLD*).

¹³ By analyst specific measure, we mean that each analyst has a single *PPI* in each year. This is different from analyst-firm specific measure, which is based on an analyst-firm pair. For example, if analyst *i* covers two firms, firms *j* and *j**, in year *t*, then she would have two analyst-firm specific accuracy measures: $Acc_{i,j,t}$ for firm *j* and $Acc_{i,j^*,t}$ for firm *j**. She would only have one analyst specific measure, equal to $(Acc_{i,j,t} + Acc_{i,j^*,t})/2$, which is based on the two firms she covers.

To facilitate the comparison with prior research, we take an approach similar to Brown and Mohammad (2009) in constructing *ABACC*. We first rank analyst *i*'s most recent annual earnings forecast error before the announcement of actual earnings for each firm (i.e., $AFE_{i,j,t}$ in the investigation of forecast accuracy) to obtain $Rank_{i,j,t}$, so that the most (least) accurate forecast is ranked as 1 (N) for a firm covered by N analysts.¹⁴ Next, we transform the ranks to obtain an accuracy measure that has a value between zero and one using the following formula:

$$Acc_{i,j,t} = 1 - \frac{Rank_{i,j,t} - 1}{N_{j,t} - 1}$$

$Acc_{i,j,t}$ is the accuracy of the forecast issued by analyst *i* for firm *j* in year *t*; a higher $Acc_{i,j,t}$ indicates greater forecast accuracy. The analyst specific forecast accuracy of analyst *i* in year *t* ($ABACC_{i,t}$) is obtained by taking the average of $Acc_{i,j,t}$ based on all of the firms covered by analyst *i* in year *t*.

We use the leader-follower ratio, *LFRatio*, to obtain the analyst specific measure of forecast timeliness, *ABTIME*. Recall, from the part above examining forecast timeliness, that the leader-follower ratio, $LFRatio_{i,j,\tau,t}$, captures the timeliness of analyst *i*'s forecast for firm *j* issued at time τ in year *t*. The higher the ratio, the timelier the forecast, as it is less likely to follow the forecasts of other analysts, but more likely to be followed by other analysts. We obtain the analyst specific forecast timeliness of analyst *i* in year *t* ($ABTIME_{i,t}$) by taking the average

¹⁴ In case of ties, the mid-point of the adjacent ranks is assigned.

leader-follower ratio of all of the annual earnings forecasts issued by analyst i in year t , excluding those forecasts issued within three days surrounding any of firm j 's earnings announcements.¹⁵

To obtain *ABBOLD*, the analyst specific measure of forecast boldness, we calculate the percentage of bold forecasts issued by an analyst in a given year. We do so based on the indicator variable $BOLD_{ij,t}$ (used in the part where we examine forecast boldness), which is based on analyst i 's most recent annual earnings forecast prior to the announcement of actual earnings for firm j in year t . Specifically, the indicator variable $BOLD_{ij,t}$ is set to 1 if analyst i 's forecast for firm j is above both the analyst's prior forecast and the consensus forecast immediately before the forecast revision. $BOLD_{ij,t}$ is also set to 1 if analyst i 's forecast for firm j is below both the analyst's prior forecast and the consensus forecast immediately before the forecast revision. Otherwise, $BOLD_{ij,t}$ is set to 0. The consensus forecast is based on forecasts issued in the 90 days prior to analyst i 's forecast revision. Thus the analyst specific forecast boldness of analyst i in year t ($ABBOLD_{i,t}$) is calculated by taking the average of $BOLD_{ij,t}$ based on all of the firms covered by analyst i in year t .¹⁶

¹⁵ Note that $ABTIME_{i,t}$ differs from the variable $TIME_{ij,t}$ used in the part where we examine forecast timeliness. Recall that $TIME_{ij,t}$ is the average of the $LFRatio_{ij,\tau,t}$, based on the forecasts issued at various time τ for each firm j followed by analyst i in year t (i.e., the average $LFRatio_{ij,\tau,t}$ across τ). Hence, $TIME_{ij,t}$ is an analyst-firm specific measure for year t . In contrast, $ABTIME_{i,t}$ is based on the forecasts issued at various time τ for all firms followed by analyst i in year t (i.e., the average $LFRatio_{ij,\tau,t}$ across τ and j), so it is an analyst specific measure for year t .

¹⁶ The result is similar if we calculate $BOLD_{i,t}$ based on all forecasts issued by analyst i in year t .

When calculating each of these three measures, we require that an analyst has at least three valid forecasts in a year.¹⁷ Note that, because all of the measures are analyst specific, we are calibrating an analyst's general ability, not an analyst's firm specific ability. This is because Brown and Mohammad (2009) argue that different firms experience different complexities at different times, and analysts with higher general ability are the ones who will better understand and incorporate these complexities into their earnings forecasts. Thus, general ability matters more than firm specific ability, and analysts with greater general ability are more likely to issue superior forecasts. Consistent with their argument, they find that their analyst specific measure is better than their analyst-firm specific measure (both are based on forecast accuracy) in identifying superior analysts.¹⁸

We use the following regression to examine the price reactions to the forecast revisions of the superior analysts identified using each measure:

$$\begin{aligned}
 Exret_{i,j,t} = & \beta_0 + \beta_1 FR_{i,j,t} + \beta_2 FR_{i,j,t} * HPPI_{i,j,t-1} + \beta_3 HPPI_{i,j,t-1} \\
 & + \beta_4 FR_{i,j,t} * HACC_{i,j,t-1} + \beta_5 HACC_{i,j,t-1} + \beta_6 FR_{i,j,t} * HTIME_{i,j,t-1} + \beta_7 HTIME_{i,j,t-1} \\
 & + \beta_8 FR_{i,j,t} * HBOLD_{i,j,t-1} + \beta_9 HBOLD_{i,j,t-1} + \varepsilon_{i,j,t}
 \end{aligned}
 \tag{1.5}$$

As in the test above investigating the stock price reaction to analyst forecast revisions, we focus on the price impact of each analyst's most recent annual

¹⁷ As a robustness check, we examine the results when we require an analyst has at least 5, rather than 3, forecasts in a year. The results are similar and do not change our inferences.

¹⁸ We also examine the analyst-firm specific measures of analyst ability based on forecast accuracy, timeliness, and boldness. Using price impact analyses, we find that none of the analyst-firm specific measures are useful in the *ex ante* identification of superior analysts for our sample.

earnings forecast prior to the announcement of actual earnings for each firm, excluding forecasts issued within 5 days of and more than 120 days before the announcement of actual earnings. $Exret_{i,j,t}$ is the size-adjusted return in percentage on the day when the most recent forecast revision prior to the announcement of actual earnings for firm j is issued by analyst i in year t . $FR_{i,j,t}$ is the forecast revision issued by analyst i for firm j in year t . $HPPI_{i,j,t-1}$, $HACC_{i,j,t-1}$, $HTIME_{i,j,t-1}$, and $HBOLD_{i,j,t-1}$ are indicator variables based on the four analyst specific ability measures: PPI , $ABACC$, $ABTIME$, and $ABBOLD$. For each measure, we set $HPPI_{i,j,t-1}$ ($HACC_{i,j,t-1}$, $HTIME_{i,j,t-1}$, and $HBOLD_{i,j,t-1}$) equal to 1 when PPI ($ABACC$, $ABTIME$, and $ABBOLD$) for analyst i in year $t-1$ is above the median PPI ($ABACC$, $ABTIME$, and $ABBOLD$) in year $t-1$ of all of the analysts who cover firm j in year t , and 0 otherwise.

The sample for this analysis is the same as that in the test above investigating the stock price reaction to analyst forecast revisions, except that we require data on all four measures of analyst ability. This results in a slight reduction in the sample size to 84,051 analyst-firm-year observations. We report the results in Table 1.4. In Panel A, we report the results of examining each analyst ability measure individually, and in Panel B we report the results of examining all four analyst ability measures simultaneously. In Panel A, we find that the coefficients on $FR*HACC$ and $FR*HTIME$ are positive and significant when considered individually, which is consistent with prior findings that forecast accuracy and

timeliness are useful *ex ante* measures to identify superior analysts (Cooper et al. 2001; Brown and Mohammad 2009).¹⁹ We find that the coefficient on $FR*HBOLD$ is not significant.

Consistent with our previous results, we find that the coefficient on $FR*HPPI$ is positive and significant, as is shown in Panel A. More importantly, as reported in Panel B, the coefficient on $FR*HPPI$ changes little when $FR*HACC$, $FR*HTIM$, and $FR*HBOLD$ are included in the regression model. Moreover, the magnitude of the coefficient on $FR*HPPI$ is the largest among the coefficients on these four interaction terms. These results suggest that, compared to other *ex ante* measures of analyst ability, *PPI* is not only incrementally useful, but also has the greatest power in identifying superior analysts²⁰.

1.5 Additional Evidence on PPI as a Measure of Analyst Production of Private Information

¹⁹ We also repeat our analysis as follows. For each analyst, instead of taking the average of *Acc*, the standardized ranked accuracy, we take the average of *SACC*, defined in the test investigating forecast accuracy, to construct an alternative measure of *ABACC*. We find this alternative measure of *ABACC* is also useful in identifying superior analysts. We also construct an alternative measure of *ABTIME* by taking the average of *STIME*, defined in the test investigating forecast timeliness, for each analyst. However, we find no relationship between this alternative measure of *ABTIME* and the price impacts of analysts' forecast revisions. Neither do we find such a relation with another alternative measure of *ABTIME* that is obtained by taking the average of standardized ranked timeliness for each analyst. Standardized ranked timeliness is constructed from *TIME* (defined in the test investigating forecast timeliness) in the same way that *Acc* is constructed from *AFE* in the test investigating forecast accuracy.

²⁰ We also conducted a principle component analysis using the control variables in Table 1.3 (brokerage size, general experience, firm experience, number of industries covered, number of firms covered, forecast frequency) and the three *ex ante* measure of analyst ability in Table 1.4 (forecast accuracy, timeliness and boldness). This analysis results in four components. Only one component is useful in identifying superior analysts, and the variables that have the highest loadings on this component are brokerage size, forecast timeliness and forecast boldness. We also find that compared with these components, *PPI* is still incrementally useful and has the highest power in identifying superior analysts.

In this section, we conduct two additional sets of tests to further substantiate our argument that *PPI* is positively related to the production of private information by analysts. First, we examine whether *PPI* is associated with several analyst characteristics and forecasting behaviors that have been identified in prior research as being associated with the analysts' production of private information. Second, we examine the time series relation between the average price impact of analyst forecast revisions and the average *PPI* of all analysts over our sample period.

1.5.1 PPI and Analyst Characteristics

Clement (1999) and Jacob et al. (1999) argue that analysts employed by larger brokerages have more resources available to them and better access to management-provided information, which allow such analysts to generate more private information. Consistent with their arguments, they find that the forecasts of analysts from larger brokerages are more accurate. Clement (1999) also suggests that, as analysts increase the number of industries they follow, they devote less attention to each individual firm they cover, resulting in less information production for each firm. Consistent with this suggestion, he finds that analysts who cover more industries issue less accurate forecasts. In addition, Clement (1999) argues that an analyst's ability increases with experience, and that more experienced analysts have better skills to generate private information. Supporting his argument, Clement finds that analysts with more general forecasting experience issue more accurate forecasts. Call et al. (2009) propose that analysts who issue

cash flow forecasts are more likely to adopt a disciplined and structured approach to forecasting; this helps them to analyze the financial statements of firms more thoroughly and thus gain more detailed information. Consistent with their argument, they find that earnings forecasts accompanied by cash flow forecasts are more accurate. Chen and Matsumoto (2006) argue that analysts who issue optimistic recommendations on a firm are more likely to have better access to managerial information. Consistent with their hypothesis, they find that forecasts by analysts who issue optimistic recommendations are more accurate. Herrmann and Thomas (2005) suggest that analysts who issue rounded forecasts have less precise information about the firms they followed, and find evidence that rounded forecasts are less accurate.

Therefore, to the extent that these analyst characteristics and behaviors suggested in the prior literature are reflective of an analyst's private information, we expect to find that *PPI* is positively associated with brokerage size (*Large*), general forecasting experience (*Gexp*), the likelihood of issuing cash flow forecasts (*Cashflow*), and the likelihood of issuing optimistic recommendations (*Oprec*). We also expect to find that *PPI* is negatively related to the number of industries covered by the analyst (*Nsic*) and to the likelihood of issuing rounded forecasts (*Round*).

We examine the contemporaneous relation between *PPI* and these six variables using the following regression:

$$PPI_{i,t} = \beta_0 + \beta_1 Large_{i,t} + \beta_2 Gexp_{i,t} + \beta_3 Nsic_{i,t} + \beta_4 Cashflow_{i,t} + \beta_5 Round_{i,t} + \beta_6 Oprec_{i,t} + \beta_7 Number_{i,t} + \varepsilon_{i,t} \quad (1.6)$$

$Large_{i,t}$ is set to 1 if analyst i is with a brokerage whose size is greater than the 10th percentile of the size of all brokerage firms in calendar year t , and 0 otherwise. Size is based on the number of analysts employed by each brokerage. $Nsic_{i,t}$ is calculated as the number of two-digit SICs followed by analyst i in calendar year t . $Cashflow_{i,t}$ is calculated as the percentage of annual earnings forecasts accompanied by cash flow forecasts based on all annual earnings forecasts issued by analyst i in calendar year t . $Oprec_{i,t}$ is calculated as the percentage of optimistic recommendations out of all recommendations issued by analyst i in calendar year t . We classify a recommendation as optimistic when it is smaller than the consensus recommendation, and the consensus recommendation is based on all recommendations issued within 120 days prior to the issuance of analyst i 's recommendation for the same firm.²¹ $Round_{i,t}$ is measured as the percentage of rounded forecasts based on all of the annual earnings forecasts issued by analyst i in calendar year t , and a forecast is classified as a rounded forecast when it occurs at nickel intervals.²² We control for the number of observations in calculating PPI

²¹ We use the numerical equivalent of analyst recommendations, as coded in *IBES*, for computational purposes. Specifically, individual analyst recommendations are coded on a five-point system as follows: 1 = Strong Buy, 2 = Buy, 3 = Hold, 4 = Underperform, and 5 = Sell.

²² We require each analyst to issue at least four annual earnings forecasts in a given year when calculating *Cashflow* and *Round*, and at least four recommendations in a given year when calculating *Oprec*. This is because all three variables are based on the proportion of the total forecasts or recommendations made by each analyst.

(*Number*) in the model, since it affects the precision of the estimation of *PPI*.²³ We also include year fixed effects and calculate *t*-statistics using analyst clustered standard errors.

Our sample for examining the relation between *PPI* and analyst characteristics and behaviors is for the period from 1994 to 2007. This is because *IBES* only starts to provide cash flow forecasts and stock recommendations for most firms from 1994. We require non-missing observations for the above six analyst characteristics, as well as the *PPI* in each year. This results in a sample of 21,994 analyst-year specific observations.

We first report the correlations between *PPI* and the above six variables in Panel A of Table 1.5. Consistent with our expectation, we find that *PPI* is positively correlated with *Large*, *Cashflow*, and *Oprec* and negatively correlated with *Nsic* and *Round*. These correlations are all statistically significant. We also find that the correlation between *PPI* and *Gexp* is positive but not statistically significant. We report the results of Equation (1.6) in Panel B of Table 1.5. The results are similar to those based on the univariate analysis in Panel A. Therefore, the findings in Table 1.5 further support our argument that *PPI* is positively related to the production of private information by analysts.

²³ If *Number* is excluded from Equation (1.6), the coefficient on *Gexp* becomes positive and significant; the coefficients on other variables are similar to those in Panel B of Table 1.5.

1.5.2 Time-series Relation between PPI and Price Impact of Analyst Forecast

Revisions

We argue that *PPI* is positively related to the production of private information by analysts. Hence, to the extent that investors understand and react efficiently to the information contained in analyst forecast revisions, we expect to observe that the average price impact of analyst forecast revisions is stronger during the time periods when the average *PPI* is higher. We test this prediction by examining the time-series relation between (1) the average price impact of analyst forecast revisions and the average *PPI* of all analysts in each calendar quarter, and (2) changes in the average price impact of analyst forecast revisions and changes in the average *PPI* of all analysts in each calendar quarter.

We first measure the average price impact of analyst forecast revisions in calendar quarter t (PI_t) based on the following regression:

$$Exret_{i,j,\tau,t} = \beta_{0,t} + \beta_{1,t}FR_{i,j,\tau,t} + \varepsilon_{i,j,\tau,t} \quad (1.7)$$

We focus on firms whose fiscal year ends in December, and we use all annual earnings forecast revisions for these firms in calendar quarter t , excluding those issued within the three days surrounding the earnings announcement date (day -1 to 1, where day 0 is the earnings announcement date). $Exret_{i,j,\tau,t}$ is the size-adjusted return in percentage on the day when the forecast revision for firm j 's annual earnings is issued by analyst i at time τ in calendar quarter t . $FR_{i,j,\tau,t}$ denotes the forecast revision of analyst i for firm j at time τ in calendar quarter t . The

definitions of size-adjusted return and forecast revision are the same as in Equation (1.4). We include firm fixed effects in regression (7). The coefficient on FR ($\beta_{1,t}$) is our measure of the average price impact of analyst forecast revisions (PI) in calendar quarter t .

We then use the following regressions, based on both a level and a change analysis, to examine the association between PPI and PI :

$$PI_t = \beta_0 + \beta_1 PPI_t + \beta_2 LOSS_t + \beta_3 LOGGDP_t + \beta_4 QTR1_t + \beta_5 QTR2_t + \beta_6 QTR3_t + \varepsilon_t \quad (1.8)$$

$$\Delta PI_t = \beta_0 + \beta_1 \Delta PPI_t + \beta_2 \Delta LOSS_t + \beta_3 \Delta LOGGDP_t + \beta_4 QTR1_t + \beta_5 QTR2_t + \beta_6 QTR3_t + \varepsilon_t \quad (1.9)$$

PI_t is the average price impact of analyst forecast revisions in calendar quarter t . PPI_t is the average PPI_t of all of the analysts in calendar quarter t . Since we measure PPI on an annual basis for each analyst (see Section 1.3), PPI_t is the same for all four quarters of a particular year.

In Equation (1.8) we control for the percentage of loss firms in calendar quarter t ($LOSS_t$) because investors may rely more on analyst-generated information for loss firms, which are associated with poorer information disclosure (Kothari et al. 2009). We also control for the gross domestic product of calendar quarter t ($LOGGDP_t$) because economic conditions may affect both the production of private information by analysts and the reaction of investors to analyst forecasts. Finally, we include time dummies based on calendar quarters ($QTR1_t$, $QTR2_t$, and $QTR3_t$) to control for time effects. In Equation (1.9), ΔPI_t is the change in PI

measured as PI in quarter t minus PI in quarter $t-4$, and ΔPPI_t is the change in PPI measured as PPI in quarter t minus PPI in quarter $t-4$. Similarly, $\Delta LOSS_t$ and $\Delta LOGGDP_t$ are included as change variables and are measured using the differences in their values between the current quarter and four quarters ago. In both Equations (1.8) and (1.9), we adjust the test statistics for time-series dependence in the error term using the technique of Newey and West (1987).

The results are reported in Table 1.6. We first report the results without including the control variables. Based on the level regression, we find a significantly positive coefficient on PPI , suggesting that the price impact of analyst forecast revisions (PI) increases with the production of private information by analysts (PPI). Based on the change regression, we find that the coefficient on ΔPPI is also significantly positive. Hence, the univariate results without the control variables suggest that the average price reaction to analyst forecast revisions is higher when the average PPI is higher. After including the control variables, we continue to find significantly positive coefficients on PPI and ΔPPI . Overall, these findings, based on the time-series association between analyst production of private information and price impact of analyst forecast revisions, provide further evidence to support PPI as a measure of the production of private information by analysts.

1.6 PPI and Regulation Fair Disclosure

In the previous sections, we establish PPI as a good measure of an analyst's private information production. We now use PPI to study the effects of RegFD on the

production of private information by analysts. RegFD was implemented by SEC in October 2000 with the aim of providing market participants with fair access to information by prohibiting managers from making selective disclosures of material information. RegFD stipulates that if a manager chooses to disclose pertinent information, such information must be disclosed to all parties. Proponents of RegFD argue that providing analysts with equal access to managerial information will motivate analysts to conduct more independent research and thus improve the quality of analyst research outputs. Opponents of RegFD claim that prohibiting non-public communication will reduce the quality and quantity of managerial information and impede analysts' understanding of firm performance, which will consequently reduce the quality of analysts' research outputs.

First, we investigate whether RegFD is effective in achieving the aim of fair disclosures. We do this by comparing the association between *PPI* and the privileged access of analysts to managerial information before and after RegFD. Before RegFD, managers could provide different amounts of information to different analysts covering their firms. Consistent with this, prior literature shows that before RegFD analysts who had better access to managerial information issued more accurate forecasts (e.g., Chen and Matsumoto 2006; Ke and Yu 2006). For example, Chen and Matsumoto (2006) argue and find that analysts who issue optimistic recommendations on a firm are more likely to have better access to managerial information, which results in more accurate earnings forecasts.

Following Chen and Matsumoto (2006), we use the likelihood of issuing optimistic recommendations (*Oprec*) as a proxy for the privileged access of analysts to managerial information. Recall that the results in Table 1.5 show that *PPI* increases with *Oprec*, which provides support for the view that differences exist among analysts in terms of their access to managerial information. However, if RegFD is effective in achieving the aim of fair disclosures, we would expect analysts to have equal access to managerial information after RegFD. Therefore, we examine whether the association between *PPI* and *Oprec*, our proxy for the privileged access of an analyst to managerial information, differs before and after RegFD using the following regression:

$$PPI_{i,t} = \beta_0 + \beta_1 Oprec_{i,t} + \beta_2 Oprec_{i,t} * PRE_{i,t} + \beta_3 PRE_{i,t} + \beta_4 Large_{i,t} + \beta_5 Gexp_{i,t} + \beta_6 Nsic_{i,t} + \beta_7 Round_{i,t} + \beta_8 Cashflow_{i,t} + \beta_9 Number_{i,t} + \varepsilon_{i,t} \quad (1.10)$$

$PRE_{i,t}$ is a dummy variable, which is equal to 1 if the time period is between 1994 and 1999, and 0 if the time period is between 2001 and 2007. As defined in Equation (1.6), $Oprec_{i,t}$ is the likelihood of issuing optimistic recommendations for analyst i in calendar year t . $Oprec * PRE$ is the interaction of *Oprec* and *PRE*. We include the following control variables: brokerage size (*Large*), general forecasting experience (*Gexp*), the number of industries covered by the analyst (*NSic*), the likelihood of issuing rounded forecasts (*Round*), the likelihood of issuing cash flow forecasts (*Cashflow*), and the number of observations used in the calculation of *PPI* (*Number*). The definitions of these control variables are the same as in Equation

(1.6). We are interested in the coefficients on $Oprec*PRE$ and $Oprec$. The coefficient on $Oprec*PRE$ indicates the difference in the relation of PPI and $Oprec$ before and after RegFD; while the coefficient on $Oprec$ indicates the relation of PPI and $Oprec$ after RegFD. If RegFD improves the fairness of analyst access to managerial information, we expect to find a significantly positive coefficient on $Oprec*PRE$. Furthermore, if RegFD eliminates the ability of managers to selectively disclose information to different analysts, we expect to find an insignificant coefficient on $Oprec$.

We report the results for Equation (1.10) in Table 1.7, as well as a base model excluding all the control variables. We find that the coefficient on $Oprec*PRE$ is positive (3.00) and significant (t -statistics = 2.04) and that the coefficient on $Oprec$ is not significant (t -statistics = 1.12). These results suggest that, after RegFD, the comparative advantage of some analysts relative to other analysts in generating private information can no longer be obtained through favored access to managerial information. These results thus provide evidence of the effectiveness of RegFD in achieving the aim of fair disclosures.

Second, we investigate how the private information produced by analysts is affected by RegFD. We do this by comparing the average PPI and the average price impact of analyst forecast revisions before and after RegFD. Following Francis et al. (2006), we use foreign firms listed as ADRs to control for any confounding events that affected all firms traded on the U.S. stock exchanges. This

is because ADRs are exempted from RegFD, and thus any effect of RegFD can be isolated by examining any changes in the differences between U.S. firms and ADRs (i.e. test of differences in differences).

For each analyst, we calculate two separate *PPI* in each calendar year: one based on forecast revisions for ADRs, and the other based on forecast revisions for U.S. firms. For analysts who follow only ADRs or U.S. firms, there is only one *PPI* per analyst. The procedure we use to estimate *PPI* are detailed in Section 1.3. We use the following regression to examine the post-RegFD change in the difference of average *PPI* between U.S. firms and ADRs:

$$PPI_{i,t} = \beta_0 + \beta_1 * USfirm_{i,t} + \beta_2 * Post_{i,t} + \beta_3 USfirm_{i,t} * Post_{i,t} + \varepsilon_{i,t} \quad (1.11)$$

$USfirm_{i,t}$ is a dummy variable, which is equal to 1 if the calculated $PPI_{i,t}$ is based on analyst i 's forecast revisions for U.S. firms in calendar year t , and 0 if the calculated $PPI_{i,t}$ is based on analyst i 's forecast revisions for ADRs in calendar year t . $Post_{i,t}$ is also a dummy variable, which is equal to 1 if calendar year t is between 2001 and 2007, and 0 if calendar year t is between 1988 and 1999. $USfirm * Post$ is the interaction between the two dummy variables. The average *PPI* of ADRs and U.S. firms in the pre-RegFD period are represented by β_0 and $\beta_0 + \beta_1$, respectively. Correspondingly, the average *PPI* of ADRs and U.S. firms in the post-RegFD period are represented by $\beta_0 + \beta_2$ and $\beta_0 + \beta_1 + \beta_2 + \beta_3$, respectively. Thus, β_2 represents the incremental effects of any confounding events that affect both U.S. firms and ADRs; while β_3 captures the incremental effects on the average *PPI*

of U.S. firms that can be uniquely attributed to RegFD.

As reported in Panel A of Table 1.8, β_3 is positive (0.06) and significant (t -statistic = 7.05), which indicates that, after controlling for any confounding events, the average *PPI* for U.S. firms has increased as a result of RegFD. This suggests that analysts are able to generate more private information after the implementation of RegFD.

Similarly, we examine the effect of RegFD on the price impact of analyst forecast revisions using ADRs as a control group. We estimate the following regression:

$$\begin{aligned} Exret_{i,j,\tau,t} = & \beta_0 + \beta_1 FR_{i,j,\tau,t} + \beta_2 USfirm_{i,j,\tau,t} + \beta_3 FR_{i,j,\tau,t} * USfirm_{i,j,\tau,t} \\ & + \beta_4 FR_{i,j,\tau,t} * Post_{i,j,\tau,t} + \beta_5 USfirm_{i,j,\tau,t} * Post_{i,j,\tau,t} \\ & + \beta_6 FR_{i,j,\tau,t} * USfirm_{i,j,\tau,t} * Post_{i,j,\tau,t} + \varepsilon_{i,j,\tau,t} \end{aligned} \quad (1.12)$$

In the equation, $Exret_{i,j,\tau,t}$ and $FR_{i,j,\tau,t}$ are defined and measured in the same way as in Equation (1.4).²⁴ That is, $Exret_{i,j,\tau,t}$ is the size-adjusted return in percentage on the day when analyst i issues an annual earnings forecast revision for firm j at time τ in calendar year t ; and $FR_{i,j,\tau,t}$ denotes the corresponding forecast revision. $USfirm_{i,j,\tau,t}$ is a dummy variable, which is equal to 1 if firm j is a U.S. firm, and 0 if is a ADR. $Post_{i,j,\tau,t}$ is also a dummy variable, which is set equals to 1 if calendar year t is between 2001 and 2007, and set equals to 0 if calendar year t is between

²⁴ In Equation (1.12), we examine all annual earnings forecasts issued by all analysts in each calendar year. This is because, in this test, we are interested in the average price impact of all forecasts made by analysts across time. Hence, unlike in Equation (1.4), there is no need to restrict observations to the most recent annual earnings forecast issued by each analyst prior to the announcement of actual earnings, which is appropriate for examining the price impacts of earnings forecasts for the same forecast period across individual analysts.

1988 and 1999. $FR*USfirm$, $FR*Post$, $USfirm*Post$ are the two-way interactive terms, and $FR*USfirm*Post$ is the three-way interactive terms of the variables in their respective expressions. We control for firm-year fixed effects in Equation (1.12).

In Equation (1.12), the price impact of analyst forecast revisions for ADRs in the pre-RegFD and post-RegFD period are measured by β_1 and $\beta_1+\beta_4$, respectively. Correspondingly, the price impact of analyst forecast revisions for U.S. firms in the pre-RegFD and post-RegFD period are measured by $\beta_1+\beta_3$ and $\beta_1+\beta_3+\beta_4+\beta_6$, respectively. Thus, β_4 indicates the incremental effects of any confounding events on the price impact of analyst forecast revisions for both U.S. firms and ADRs after RegFD. β_6 indicates the incremental effects on the price impact of analyst forecast revisions for U.S. firms that can be attributed to RegFD.

We report the results on Equation (1.12) in Panel B of Table 1.8. As reported in the table, β_6 is positive (0.57) and significant (t -statistic = 4.24), which indicates that the price impact of analyst forecast revisions for U.S. firms have increased significantly after the implementation of RegFD. This suggests that forecasts issued by analysts for U.S. firms after the implementation of RegFD have greater information value and hence, larger price impacts upon their releases.

In the extant literature, the debate on the effect of RegFD on the production of private information by analysts is inconclusive.²⁵ Mohanram and Sunder (2006)

²⁵ There are studies (e.g., Bailey et al. 2003; Heflin et al. 2003; Agrawal et al. 2006) focusing on the

examine a firm specific measure of analyst private information based on Barron et al.'s (1998) model, and find a significant increase in analyst private information after RegFD. On the other hand, Gintschel and Markov (2004) and Francis et al. (2006) conclude that RegFD has reduced the informativeness of analysts' research outputs.²⁶ Therefore, our findings on both the average *PPI* and the average price impact of analyst forecast revisions provide fresh evidence in support of the view that RegFD enhances the production of private information by analysts. These results, together with those reported in Table 1.7, lead us to conclude that RegFD is a beneficial piece of legislation, which is effective not only in achieving the aim

effect of RegFD on the accuracy and dispersion of analyst forecasts. However, these studies do not directly investigate the nature of information (e.g., public versus private) that is incorporated in analysts' forecasts. Therefore, these studies do not lend evidence on whether private information production by analysts is affected by RegFD.

²⁵ We find that this conclusion can be sensitive to the sample and/or estimation of the parameters used in the construction of the information metric. Specifically, Gintschel and Markov (2004) and Francis et al. (2006) construct their samples using First Call database and Zacks Investment research database respectively, and measure the information metric as absolute value of standardized stock return for firm j on date t , which is defined as: $VOL_{j,t} = |Ret_{j,t} - \overline{Ret_j}| / Std(Ret_j) \cdot Ret_{j,t}$ is the stock return of firm j on date t ; and $\overline{Ret_j}$ and $Std(Ret_j)$ are the mean and the standard deviation of stock returns of firm j . They assume that $\overline{Ret_j}$ and $Std(Ret_j)$ are not affected by the implementation of RegFD. That is, they calculate both variables using data from October 23, 1999 to October 24, 2001, which spans both pre and post RegFD periods. We apply the same methodology on a sample we construct from the *IBES* database, and find a significant decrease in the average informativeness of analyst forecast revisions, which is consistent with the result in Gintschel and Markov (2004). However, after using ADRs as a control group, we find no significant change in the average informativeness of analyst forecast revisions for U.S. firms in our sample, which is a result different from that in Francis et al. (2006). Since Chen et al. (2009) provide evidence indicating that, compared to ADRs, the cost of equity capital for U.S. firms has significantly decreased after RegFD, we repeat the analysis but allow for separate estimations of $\overline{Ret_j}$ and $Std(Ret_j)$ for the pre- and post-RegFD periods to account for changes in the cost of equity capital. In this new analysis, we find that compared to ADRs, the average informativeness of analyst forecast revisions for US firms have increased (rather than decreased) after RegFD.

of fair disclosures, and but also in motivating analysts to conduct more independent research, resulting in an increase in analysts' private information production.

1.7 Summary and Conclusion

In this paper, we propose an *ex ante* measure of analyst ability by focusing on an analyst's production of private information (*PPI*). This measure is based on the correlation coefficient between the analyst's forecast revisions and the stock returns within each successive forecast. One minus the correlation coefficient is our proposed measure of the production of private information by the analyst (*PPI*). The higher the *PPI*, the greater the amount of private information produced by the analyst.

We validate *PPI* as an *ex ante* measure of an analyst's ability to produce private information by examining the cross-sectional differences in the one-period-ahead forecasting performance of analysts in terms of forecast accuracy, timelines, boldness, and price impacts. We find that the one-period-ahead forecasts issued by analysts with higher *PPI* are more accurate, timelier, and bolder than those issued by analysts with lower *PPI*. We also find that the one-period-ahead forecasts issued by analysts with high *PPI* have a greater impact on stock prices than those issued by analysts with low *PPI*. In particular, the average abnormal forecast announcement return associated with the forecast revisions of high *PPI* analysts is more than triple that of low *PPI* analysts. Furthermore, using the price impact analysis, we find that *PPI* is not only incrementally useful, but also has the

greatest power in identifying superior analysts compared to other *ex ante* measures of analyst ability based on forecast accuracy and timeliness. We also conduct a time series analysis of the average price impact of analyst forecast revisions and the average *PPI* of analysts, we find that investors react stronger (weaker) to analyst forecast revisions during time periods when the average *PPI* is higher (lower), lending further support to the validity of *PPI* as a measure of an analyst's private information production.

We also employ *PPI* to examine the effects of RegFD on the production of private information by analysts. We find that analysts with higher *PPI* have more privileged access to managerial information before, but not after, RegFD. Furthermore, after controlling for the confounding effects, we find the average *PPI* and the average informativeness of analyst forecasts have increased after the implementation of RegFD. These findings suggest that RegFD not only equalizes the access of analysts to managerial information, but also motivates analysts to conduct more independent research, which improves the overall quality of analyst research outputs.

Tables for chapter 1

Table 1.1
Summary Statistics on *PPI*

This table reports the summary statistics of *PPI*. In the last column, *Number* denotes the average number of observations used in the estimation of *PPI*. The sample period is from 1987 to 2007.

<i>Year</i>	<i>Mean</i>	<i>10th Pctl</i>	<i>25th Pctl</i>	<i>Median</i>	<i>75th Pctl</i>	<i>90th Pctl</i>	<i>Std Dev</i>	<i>Number</i>
1987	0.851	0.613	0.728	0.846	0.971	1.091	0.189	76
1988	0.811	0.574	0.681	0.810	0.929	1.053	0.198	78
1989	0.761	0.504	0.633	0.756	0.884	1.011	0.202	77
1990	0.750	0.499	0.617	0.741	0.880	1.016	0.202	81
1991	0.785	0.551	0.662	0.790	0.898	1.020	0.195	92
1992	0.754	0.512	0.638	0.752	0.869	0.976	0.190	103
1993	0.774	0.545	0.659	0.761	0.886	1.010	0.190	99
1994	0.767	0.533	0.645	0.756	0.880	1.016	0.193	102
1995	0.743	0.506	0.610	0.736	0.857	0.982	0.199	108
1996	0.768	0.514	0.642	0.765	0.892	1.019	0.203	104
1997	0.765	0.511	0.639	0.760	0.882	1.004	0.201	99
1998	0.758	0.525	0.635	0.750	0.870	0.995	0.195	110
1999	0.787	0.529	0.646	0.775	0.914	1.061	0.218	99
2000	0.802	0.539	0.649	0.790	0.931	1.091	0.222	98
2001	0.795	0.548	0.674	0.789	0.908	1.034	0.200	125
2002	0.778	0.528	0.646	0.770	0.892	1.036	0.207	125
2003	0.814	0.551	0.680	0.810	0.941	1.057	0.215	123
2004	0.812	0.552	0.684	0.808	0.943	1.072	0.210	138
2005	0.793	0.545	0.672	0.791	0.905	1.032	0.200	146
2006	0.818	0.559	0.693	0.813	0.937	1.066	0.211	135
2007	0.778	0.530	0.650	0.771	0.900	1.022	0.203	157
ALL	0.784	0.537	0.656	0.778	0.903	1.032	0.202	114

Table 1.2
PPI and Forecast Accuracy, Timeliness and Boldness

This table reports results examining *PPI* and three characteristics of analysts' future forecasts using the following regression:

$$SACC_{i,j,t} / STIME_{i,j,t} / BOLD_{i,j,t} = \beta_0 + \beta_1 SPPI_{i,j,t-1} + \beta_2 SACHAR_{i,j,t-1} + \varepsilon_{i,j,t}$$

SACC_{i,j,t} (*STIME_{i,j,t}*) is the relative forecast accuracy (timeliness) of analyst *i* for firm *j* of year *t*. *BOLD_{i,j,t}* is a dummy variable, which is equal to 1 if the forecast issued by analyst *i* for firm *j* in year *t* is a bold forecast, and 0 otherwise. *SPPI_{i,j,t-1}* is the *PPI* for analyst *i* in year *t-1* relative to the *PPI* in year *t-1* of all of the other analysts who also cover firm *j* in year *t*. *SACHAR_{i,j,t-1}* is the characteristic of analyst *i* in year *t-1* relative to the corresponding characteristic in year *t-1* of the other analysts following firm *j* in year *t*. The characteristics we consider include: brokerage size (*BROSIZE*), general experience (*GEXP*), firm-specific experience (*FEXP*), number of firms covered (*NFIRM*), number of industries covered (*NSIC*), and firm-specific forecast frequency (*FOREFREQ*). *BROSIZE_{i,j,t}* is calculated as the number of analysts employed by the brokerage employing analyst *i* in year *t*. *GEXP_{i,j,t}* is measured as the number of years of experience for analyst *i* in year *t*. *FEXP_{i,j,t}* is calculated as the number of years of firm specific experience for analyst *i* following firm *j* in year *t*. *NFIRM_{i,j,t}* is calculated as the number of companies followed by analyst *i* in year *t*. *NSIC_{i,j,t}* is calculated as the number of two-digit SICs followed by analyst *i* in year *t*. *FOREFREQ_{i,j,t}* is measured as the number of forecasts made by analyst *i* for firm *j* in year *t*. Firm-year fixed effects are included in the regression and test statistics are based on analyst clustered standard errors. Panel A reports the regression results of relating *PPI* to accuracy, and Panel B reports the regression results of relating *PPI* to timeliness. Panel C reports the regression results of relating *PPI* to boldness. Since *BOLD_{i,j,t}* is a dummy variable, we estimate a logistic regression in Panel C. In each panel, we first report the result of a base regression in which *SPPI* is the only independent variable. The sample period is from 1988 to 2007.

Panel A: <i>PPI</i> and Forecast Accuracy				
<i>Variable</i>	<i>Estimate</i>	<i>T statistic</i>	<i>Estimate</i>	<i>T statistic</i>
<i>SPPI</i>	0.01	2.49	0.01	2.29
<i>SBROSIZE</i>			0.03	7.15
<i>SGEXP</i>			0.00	-0.39
<i>SFEXP</i>			0.00	3.16
<i>SNFIRM</i>			-0.03	-5.84
<i>SNSIC</i>			-0.02	-4.07
<i>SFOREFREQ</i>			0.02	6.79

Table 1.2
(Continued)

Panel B: <i>PPI</i> and Forecast Timeliness				
<i>Variable</i>	<i>Estimate</i>	<i>T statistic</i>	<i>Estimate</i>	<i>T statistic</i>
<i>SPPI</i>	0.03	5.98	0.03	5.81
<i>SBROSIZE</i>			0.06	9.57
<i>SGEXP</i>			-0.02	-3.04
<i>SFEXP</i>			0.00	0.25
<i>SNFIRM</i>			-0.03	-3.93
<i>SNSIC</i>			-0.02	-2.33
<i>SFOREFREQ</i>			0.04	8.24
Panel C: <i>PPI</i> and Forecast Boldness				
<i>Variable</i>	<i>Estimate</i>	<i>P value</i>	<i>Estimate</i>	<i>P value</i>
<i>SPPI</i>	0.06	0.001	0.06	0.003
<i>SBROSIZE</i>			0.13	<.0001
<i>SGEXP</i>			0.05	0.014
<i>SFEXP</i>			0	0.908
<i>SNFIRM</i>			-0.11	<.0001
<i>SNSIC</i>			-0.11	<.0001
<i>SFOREFREQ</i>			0.09	<.0001

Table 1.3
PPI and the Price Impacts of Analyst Forecast Revisions

This table reports the results of the following regression:

$$Exret_{i,j,t} = \beta_0 + \beta_1 FR_{i,j,t} + \beta_2 FR_{i,j,t} * HPPI_{i,j,t-1} + \beta_3 HPPI_{i,j,t-1} + \beta_4 FR_{i,j,t} * HCHAR_{i,j,t-1} + \beta_5 HCHAR_{i,j,t-1} + \varepsilon_{i,j,t}$$

$Exret_{i,j,t}$ is the size-adjusted return in percentage on the day when the forecast revision for firm j is issued by analyst i in year t . $FR_{i,j,t}$ is the forecast revision, calculated as analyst i 's forecast for firm j in year t minus her most recent prior forecast, scaled by the absolute value of the most recent prior forecast. $HPPI_{i,j,t-1}$ is a dummy variable, which is equal to 1 if the PPI of analyst i in year $t-1$ is above the median PPI in year $t-1$ of all of the analysts who follow firm j in year t , and 0 otherwise. $FR_{i,j,t} * HPPI_{i,j,t-1}$ is the interaction of $FR_{i,j,t}$ and $HPPI_{i,j,t-1}$. $HCHAR_{i,j,t-1}$ is equal to 1 if the characteristic for analyst i in year $t-1$ is above the median of the corresponding characteristic in year $t-1$ for all of the analysts who also cover firm j in year t , and 0 otherwise. The analyst characteristics we consider include: brokerage size ($BROSIZE$), general experience ($GEXP$), firm-specific experience ($FEXP$), number of firms covered ($NFIRM$), number of industries covered ($NSIC$), and firm-specific forecast frequency ($FOREFREQ$). The definitions of these variables are the same as in Table 1.2. $FR_{i,j,t} * HCHAR_{i,j,t-1}$ is the interaction of $FR_{i,j,t}$ and $HCHAR_{i,j,t-1}$. Firm-year fixed effects are included in the model. The sample period is from 1988 to 2007.

Variable	Estimate	T statistic	Estimate	T statistic
FR	0.11	2.26	0.08	1.19
FR*HPPI	0.23	4.07	0.22	3.92
HPPI	0.01	0.32	0.01	0.36
FR*HBROSIZE			0.12	2.07
HBROSIZE			-0.02	-0.99
FR*HGEXP			0.00	0.06
HGEXP			-0.01	-0.53
FR*HFEXP			0.13	1.93
HFEXP			0.00	-0.15
FR*HNSIC			-0.18	-2.68
HNSIC			-0.01	-0.61
FR*HNFIRM			0.06	0.94
HNFIRM			-0.03	-1.20
FR*HFOREFREQ			-0.06	-1.00
HFOREFREQ			0.00	-0.02

Table 1.4
Comparing PPI with Other Ex ante Measures of Analyst Ability

This table reports the results of comparing PPI with other ex ante measures of analyst ability based on accuracy, timeliness, and boldness using the following regression:

$$Exret_{i,j,t} = \beta_0 + \beta_1 FR_{i,j,t} + \beta_2 FR_{i,j,t} * HPPI_{i,j,t-1} + \beta_3 HPPI_{i,j,t-1} + \beta_4 FR_{i,j,t} * HAC_{i,j,t-1} + \beta_5 HAC_{i,j,t-1} + \beta_6 FR_{i,j,t} * HTIME_{i,j,t-1} + \beta_7 HTIME_{i,j,t-1} + \beta_8 FR_{i,j,t} * HBOLD_{i,j,t-1} + \beta_9 HBOLD_{i,j,t-1} + \epsilon_{i,j,t}$$

$Exret_{i,j,t}$ is the size-adjusted return in percentage on the day when the forecast revision for firm j is issued by analyst i in year t . $FR_{i,j,t}$ is the forecast revision, calculated as analyst i 's forecast for firm j in year t minus her most recent prior forecast, scaled by the absolute value of the most recent prior forecast. $HPPI_{i,j,t-1}$ ($HAC_{i,j,t-1}$, $HTIME_{i,j,t-1}$, and $HBOLD_{i,j,t-1}$) is equal to 1 when the PPI ($ABACC$, $ABTIME$, and $ABBOLD$) for analyst i in year $t-1$ is above the median PPI ($ABACC$, $ABTIME$, and $ABBOLD$) in year $t-1$ of all analysts who cover firm j in year $t-1$, and 0 otherwise. $ABACC$, $ABTIME$, and $ABBOLD$ are analyst specific measures of ability based on forecast accuracy, timeliness, and boldness respectively. Firm-year fixed effects are included in the equation. In Panel A, we report the results of examining each analyst ability measure individually, and SUP represents $HPPI$, $HACC$, $HTIME$, or $HBOLD$. In Panel B we report the results of examining all four analyst ability measures simultaneously.

Panel A							
Variable	PPI		Accuracy		Timeliness		Boldness
	Estimate	T statistic	Estimate	T statistic	Estimate	T statistic	T statistic
FR	0.13	2.69	0.17	3.41	0.17	3.47	0.21
FR*SUP	0.19	3.34	0.13	2.26	0.11	1.86	0.02
SUP	0.00	0.02	-0.00	-0.24	-0.08	-3.84	0.01

Table 1.4
(Continued)

Panel B		
Variable	Estimate	T statistic
FR	0.05	0.77
FR*HPPI	0.18	3.21
HPPI	0.00	0.13
FR*HACC	0.12	2.03
HACC	0.00	-0.07
FR*HTIME	0.08	1.45
HTIME	-0.08	-3.88
FR*HBOLD	0.00	0.05
HBOLD	0.01	0.69

Table 1.5
PPI and Analyst Characteristics

This table reports the results of examining the relation between *PPI* and the following analyst characteristics: brokerage size (*Large*), number of industries covered (*Nsic*), likelihood of issuing rounded forecasts (*Round*), likelihood of issuing cash flow forecasts (*Cashflow*), and likelihood of issuing optimistic recommendations (*Oprec*). $PPI_{i,t}$ is the *PPI* of analyst *i* in year *t*, multiplied by 100. $Large_{i,t}$ is equal to 1 if analyst *i* is with a brokerage whose size is greater than the 10th percentile of the size of all brokerage firms in calendar year *t*, and 0 otherwise. $Nsic_{i,t}$ is calculated as the number of two-digit SICs followed by analyst *i* in calendar year *t*. $Cashflow_{i,t}$ is calculated as the percentage of annual earnings forecasts accompanied by cashflow forecasts based on all annual forecasts issued by analyst *i* in calendar year *t*. $Oprec_{i,t}$ is calculated as the percentage of optimistic recommendations issued by analyst *i* in calendar year *t*. In *IBES*, individual analyst recommendations are coded on a five-point system as follows: 1 = Strong buy, 2 = Buy, 3 = Hold, 4 = Underperform, and 5 = Sell. We classify a recommendation as optimistic when it is smaller than the consensus recommendation, and the consensus recommendation is based on all of the recommendations issued within 120 days prior to the issuance of analyst *i*'s recommendation for the same firm. $Round_{i,t}$ is measured as the percentage of rounded forecasts based on all of the annual earnings forecasts issued by analyst *i* in calendar year *t*, and a forecast is classified as a rounded forecast when it occurs at nickel intervals. $Number_{i,t}$ denotes the number of observations used in the calculation of *PPI*. The sample period is from 1994 to 2007. Panel A reports the simple correlation coefficients among these variables, and the numbers in italics are the *p*-values. Panel B reports the results of the following regression:

$$PPI_{i,t} = \beta_0 + \beta_1 Large_{i,t} + \beta_2 Gexp_{i,t} + \beta_3 Round_{i,t} + \beta_4 Cashflow_{i,t} + \beta_5 Oprec_{i,t} + \beta_6 Round_{i,t} + \beta_7 Number_{i,t} + \varepsilon_{i,t}$$

Year fixed effects are included in the equation and test statistics are based on analyst clustered standard errors.

Panel A: Correlation							
	<i>PPI</i>	<i>Large</i>	<i>Nsic</i>	<i>Gexp</i>	<i>Round</i>	<i>Cashflow</i>	<i>Oprec</i>
<i>Large</i>	0.037 <i><.0001</i>						
<i>Nsic</i>	-0.072 <i><.0001</i>	-0.067 <i><.0001</i>					
<i>Gexp</i>	0.001 <i>0.899</i>	0.060 <i><.0001</i>	0.207 <i><.0001</i>				
<i>Round</i>	-0.057 <i><.0001</i>	0.048 <i><.0001</i>	0.144 <i><.0001</i>	0.225 <i><.0001</i>			
<i>Cashflow</i>	0.055 <i><.0001</i>	0.098 <i><.0001</i>	-0.056 <i><.0001</i>	0.020 <i>0.003</i>	-0.068 <i><.0001</i>		
<i>Oprec</i>	0.015 <i>0.023</i>	-0.101 <i><.0001</i>	0.006 <i>0.373</i>	0.017 <i>0.011</i>	0.014 <i>0.033</i>	0.015 <i>0.027</i>	
<i>Number</i>	0.045 <i><.0001</i>	0.134 <i><.0001</i>	0.286 <i><.0001</i>	0.242 <i><.0001</i>	-0.022 <i>0.001</i>	0.134 <i><.0001</i>	-0.002 <i>0.816</i>

Table 1.5
(Continued)

Panel B: Regression		
<i>Variable</i>	<i>Estimate</i>	<i>T statistic</i>
<i>Large</i>	0.80	2.69
<i>Nsic</i>	-0.26	-8.76
<i>Gexp</i>	0.03	1.13
<i>Round</i>	-2.81	-3.54
<i>Cashflow</i>	3.05	4.39
<i>Oprec</i>	1.83	2.54
<i>Number</i>	0.01	6.86

Table 1.6
Time Series Relation between
Average Price Impact of Analyst Forecast Revisions and Average PPI

This table reports the results of the following regressions:
 $PI_t = \beta_0 + \beta_1 PPI_t + \beta_2 LOSS_t + \beta_3 LOGGDP_t + \beta_4 QTR1_t + \beta_5 QTR2_t + \beta_6 QTR3_t + \varepsilon_t$
 $\Delta PPI_t = \beta_0 + \beta_1 \Delta PPI_t + \beta_2 \Delta LOSS_t + \beta_3 \Delta LOGGDP_t + \beta_4 QTR1_t + \beta_5 QTR2_t + \beta_6 QTR3_t + \varepsilon_t$
 PI_t is the average price impact of analyst forecasts in calendar quarter t , which is calculated as the coefficient on forecast revision in regression (7) in the text, where the forecast announcement return of all of the annual earnings forecasts released by analysts in calendar quarter t for firms whose fiscal year ends in December is regressed on the forecast revision. Forecast revision and forecast announcement return are defined the same as in Table 1.3. PPI_t is the average PPI of all of the analysts in calendar quarter t . $LOSS_t$ is the percentage of loss firms in calendar quarter t , and the information is obtained from COMPUSTAT. $LOGGDP_t$ is the log value of the gross domestic products in calendar quarter t , and we obtain this information from the Federal Reserve Bank of St. Louis at <http://research.stlouisfed.org/fred/data/gdp>. ΔPPI_t ($\Delta LOSS_t$, $\Delta LOGGDP_t$) is the change in PPI ($LOSS$, $LOGGDP$) in quarter t from quarter $t-4$. $QTR1_t$ ($QTR2_t$, $QTR3_t$) are quarter dummies, which equal 1 when quarter t is the first (second, third) calendar quarter, and 0 otherwise. Panel A reports the results of the level regression, and Panel B reports the results of the change regression. The sample period is from 1988 to 2007. Test statistics are adjusted for time-series dependence using the technique of Newey and West (1987).

Panel A: Level Regression				
	Estimate	T statistic	Estimate	T statistic
INTERCEPT	-6.72	-2.88	-7.08	-3.99
PPI	9.06	2.97	4.45	2.27
LOSS			2.70	2.18
LOGGDP			0.37	1.80
QTR1			0.26	2.77
QTR2			0.19	2.94
QTR3			0.23	3.12
Panel B: Change Regression				
	Estimate	T statistic	Estimate	T statistic
INTERCEPT	0.02	0.22	-0.33	-0.55
ΔPPI	4.88	2.09	5.99	1.90
ΔLOSS			1.66	0.84
ΔGDP			6.44	0.68
QTR1			0.00	0.01
QTR2			-0.05	-0.30
QTR3			-0.02	-0.13

Table 1.7
Effect of Regulation FD on the
Relation between *PPI* and the Likelihood of Optimistic Recommendations

This table reports the results of the effect of RegFD on the relation between *PPI* and the likelihood of optimistic recommendations (*Oprec*) using the following regression:

$$PPI_{i,t} = \beta_0 + \beta_1 Oprec_{i,t} + \beta_2 Oprec_{i,t} * PRE_{i,t} + \beta_3 PRE_{i,t} + \beta_4 Gexp_{i,t} + \beta_5 Nsic_{i,t} + \beta_6 Round_{i,t} + \beta_7 Cashflow_{i,t} + \beta_8 Number_{i,t} + \varepsilon_{i,t}$$

PRE_{i,t} is a dummy variable, which is equal to 1 if the time period is between 1994 and 1999, and 0 if the time period is between 2001 and 2007. *PPI_{i,t}* is the *PPI* in percentage of analyst *i* in year *t*. *Oprec_{i,t}* is the likelihood of issuing optimistic recommendations by analyst *i* in year *t*, as defined in Table 1.5. The definitions of the other control variables are the same as in Table 1.5. Test statistics are calculated using analyst clustered standard errors.

<i>Variable</i>	<i>Estimate</i>	<i>T statistic</i>	<i>Estimate</i>	<i>T statistic</i>
<i>Oprec</i>	0.73	0.75	1.09	1.12
<i>Oprec*PRE</i>	3.52	2.37	3.00	2.04
<i>PRE</i>	-4.69	-7.10	-3.10	-4.57
<i>Large</i>			0.76	2.51
<i>Nsic</i>			-0.25	-8.56
<i>Gexp</i>			0.05	1.65
<i>Round</i>			-2.46	-3.09
<i>Cashflow</i>			3.57	5.10
<i>Number</i>			0.01	6.24

Table 1.8
Effect of Regulation FD on
Average *PPI* and Average Price Impact of Analyst Forecast Revisions

This table reports the results of the effect of Regulation FD on average *PPI* and average price impact of analyst forecast revisions. Panel A reports the results of the following regression:

$$PPI_{i,t} = \beta_0 + \beta_1 * USfirm_{i,t} + \beta_2 * Post_{i,t} + \beta_3 USfirm_{i,t} * Post_{i,t} + \varepsilon_{i,t}$$

$USfirm_{i,t}$ is a dummy variable, which is equal to 1 if the calculated $PPI_{i,t}$ is based on analyst i 's forecast revisions for U.S. firms in calendar year t , and 0 if the calculated $PPI_{i,t}$ is based on analyst i 's forecast revisions for ADRs in calendar year t . $Post_{i,t}$ is also a dummy variable, which is equal to 1 if calendar year t is between 2001 and 2007, and 0 if calendar year t is between 1988 and 1999. $USfirm * Post$ is the interaction between the two dummy variables. Panel B reports the results of the following regression:

$$Exret_{i,j,\tau,t} = \beta_0 + \beta_1 FR_{i,j,\tau,t} + \beta_2 USfirm_{i,j,\tau,t} + \beta_3 FR_{i,j,\tau,t} * USfirm_{i,j,\tau,t} + \beta_4 FR_{i,j,\tau,t} * Post_{i,j,\tau,t} + \beta_5 USfirm_{i,j,\tau,t} * Post_{i,j,\tau,t} + \beta_6 FR_{i,j,\tau,t} * USfirm_{i,j,\tau,t} * Post_{i,j,\tau,t} + \varepsilon_{i,j,\tau,t}$$

$Exret_{i,j,\tau,t}$ is the size-adjusted return in percentage on the day when analyst i issues an annual earnings forecast revision for firm j at time τ in calendar year t ; and $FR_{i,j,\tau,t}$ denotes the corresponding forecast revision. $USfirm_{i,j,\tau,t}$ is a dummy variable, which is equal to 1 if firm j is a U.S. firm, and 0 if it is a ADR. $Post_{i,j,\tau,t}$ is also a dummy variable, which is set equals to 1 if calendar year t is between 2001 and 2007, and set equals to 0 if calendar year t is between 1988 and 1999. $FR * USfirm$, $FR * Post$, $USfirm * Post$ are the two-way interactive terms, and $FR * USfirm * Post$ is the three-way interactive terms of the variables in their respective expressions. We include firm-year fixed effect in the regression.

Panel A: Regulation FD and Average <i>PPI</i>		
Variable	Estimate	T statistic
Intercept (β_0)	0.83	126.45
<i>USfirm</i> (β_1)	-0.07	-9.70
<i>Post</i> (β_2)	-0.03	-3.34
<i>USfirm*Post</i> (β_3)	0.06	7.05

Panel B: Regulation FD and Average Price Impact of Analyst Forecast Revisions		
Variable	Estimate	T statistic
<i>FR</i> (β_1)	0.25	2.35
<i>USfirm</i> (β_2)	-0.80	-1.23
<i>FR*USfirm</i> (β_3)	-0.04	-0.40
<i>FR*Post</i> (β_4)	0.26	1.99
<i>USfirm*Post</i> (β_5)	-1.36	-1.75
<i>FR*USfirm*Post</i> (β_6)	0.57	4.24

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Chapter 2 The Dispersion Effect around the World: the Role of Information Uncertainty, Investor Protection, Stock Market Development and National Culture

2.1 Introduction

Diether et al. (2002) demonstrate that in the US market, there is a significantly negative relationship between dispersion in analysts' earnings forecasts and future stock returns (henceforth, the dispersion effect). Since forecast dispersion is commonly regarded as proxy for information uncertainty, the straightforward interpretation of this result is that uncertainty is negatively related to stock return. Although this interpretation is in vast contrast to the common notion that investors dislike uncertainty, Diether et al. (2002) and Johnson (2004) provide different mechanisms to reconcile the dispersion effect with the common notion: they use forecast dispersion as proxy for information uncertainty, and suggest that in certain circumstances higher uncertainty predicts lower stock return. Specifically, Diether et al. (2002) view dispersion as a direct measure of different opinions among investors, and argue that the greater the difference of investors' opinions, the more negative opinions will be kept out the market due to the presence of short sale constraints, causing stocks with higher dispersion to be overpriced. Johnson (2004) uses dispersion as proxy for the idiosyncratic parameter risk pertaining to the unobservability of a firm's underlying value. He builds a rational asset pricing model in which stock price is viewed as the option value of a levered firm: the

higher the idiosyncratic parameter risk, the more valuable the option, and the higher the stock price, leading to lower expected stock return.

The purpose of this paper is twofold. We first examine whether the dispersion effect is a worldwide anomaly, and whether it differs across countries. If the answers are positive, we then employ the global setting to investigate the reason of the dispersion effect.

With regard to our first purpose, we find that the dispersion effect is a pervasive anomaly in the international markets. Specifically, among the 20 countries in our sample, the average monthly return on the portfolio of longing stocks in the lowest dispersion quintile and shorting stocks in highest dispersion quintile is positive in 18 countries, and significantly different from zero (at the significance level of 0.05) in 10 countries. Furthermore, we find that the dispersion effect differs widely across countries, strongest in Singapore and weakest in Turkey.

With regard to our second purpose, we first examine the dispersion effect across countries with different information uncertainty since the dominant explanations for the dispersion effect provided in the current literature [Diether et al. (2002), Johnson (2004), Sadka and Scherbina (2007), and Barron et al. (2008)] all suggest that the dispersion effect should be more pronounced in countries with more information uncertainty than in countries with less information uncertainty. Using various proxies of country level information uncertainty: earnings opacity,

return volatility, disclosure level and enforcement of accounting standards, we find that the dispersion effect is stronger in countries with less rather than more information uncertainty. This is contrary to the prediction of the dominant explanations for the dispersion effect in the current literature. Therefore our findings cast doubt on the theories proposed in the current literature to explain the dispersion effect.

We then propose an alternative explanation for the dispersion effect. We hypothesize that the dispersion effect arises because investors rely on analysts' earnings forecasts to make decisions, and there is an upward bias in the consensus earnings forecast which increases with forecast dispersion. We find strong support for our hypothesis: first, we document that there is a worldwide positive relation between upward bias in consensus earnings forecast and forecast dispersion; second, we document that the dispersion effect is more pronounced in countries where investors rely more on analysts' earnings forecasts such as those countries with better investor protection, more developed stock markets, a culture of higher uncertainty avoidance and higher accuracy of analysts' earnings forecasts. In addition, we conjecture that the positive relation between upward bias in consensus earnings forecast and forecast dispersion could be due to analysts' incentives, and we find supporting evidence for our conjecture.

Our research is important for several reasons. First, the dispersion effect documented in the US market poses a formidable challenge to the capital market

theory, given that it is common practice in the accounting and finance literature to view forecast dispersion as proxy for either risk or information uncertainty. Hence, examining the dispersion effect worldwide helps us to assess the prevalence of this anomaly. Second, we test the explanations for the dispersion effect proposed in the current literature using the global setting, thus providing evidence on the validity of these explanations. Third, we examine the link between the dispersion effect and cross-country differences in investor protection, stock market development, national culture and information environment, which adds to our understanding of the generation of the dispersion effect.

The paper proceeds as follows. Section 2.2 describes our sample and data. Section 2.3 examines the dispersion effect in the international markets. Section 2.4 and 2.5 use the global setting to investigate the cause of the dispersion effect. Section 2.6 concludes.

2.2 Sample and Data

We obtain analysts' earnings forecasts for stocks from *IBES* summary file unadjusted for stock splits. Following Diether et al. (2002), we use the unadjusted summary file instead of the adjusted summary file of IBES to avoid the rounding issue in the adjusted file. Forecast dispersion is calculated as the standard deviation of analysts' one period ahead annual earnings forecasts divided by the absolute value of consensus earnings forecast. Following Diether et al. (2002), we use IBES unadjusted summary file instead of adjusted summary file to We retrieve monthly

price index from *Datastream* to calculate monthly stock returns for non-US firms, and correct the coding errors of *Datastream* via the methods of Ince and Porter (2006). Monthly stock returns of US firms are retrieved from *CRSP*. All returns are US dollar denominated. We winsorize the monthly returns within each country at the top and bottom 1% to mitigate the effects of outliers. We also exclude stocks with prices in the bottom 5% of the price distribution in each month for each country, which ensures that the dispersion effect we document in each country would not be driven by small and/or illiquid stocks. Since we need a reasonable number of stocks to construct dispersion portfolios, each month we limit our sample to those countries that have at least 50 observations in that month. In addition, we require the dispersion portfolio in each country to have a return history of at least 100 months over our sample period. Our final sample consists of 1,187,130 firm-month observations for 20 countries from Feb 1990 to Dec 2005.

In Table 2.1, we report the summary statistics of our sample. Column 2 and 3 show the start and end dates of the sample period in each country, which vary across countries based on data availability. Column 4 is the percentage of observations in each country. The United States represents the largest portion of our sample, with the percentage equal to around 44%. Japan and the United Kingdom are the second and third largest, which account for about 12% and 7% of the total observations respectively. The last four columns show the following medium firm characteristics in each country: log firm size (*logsize*), market to book

ratio (*mb*), momentum (*mom*) and dispersion in analysts' earnings forecasts (*disp*). Firm size and market to book ratio for non-US firms are retrieved from *Datastream*. Firm size and market to book ratio for US firms are retrieved from *CRSP* and *Compustat* respectively. Firm size is also converted to US dollar denominated. Momentum is calculated as the buy and hold stock return of the past six months.

2.3 The Dispersion Effect around the World

In this section, we examine the dispersion effect across the 20 countries in our sample. At the beginning of each month, we divide the stocks within each country into five quintiles (D1 to D5) based on earnings' forecast dispersion. D1 is the portfolio with stocks of the lowest dispersion, and D5 is the portfolio with stocks of the highest dispersion. D1-D5 is the hedge portfolio that longs the stocks in the lowest dispersion quintile, and shorts the stocks in the highest dispersion quintile. We call this hedge portfolio (D1-D5) as the dispersion portfolio, and the average monthly return on this dispersion portfolio is our measure of the extent of the dispersion effect in each country. The greater the return of the dispersion portfolio, the stronger the dispersion effect.

The results are reported in Table 2.2. As shown in the table, except in Japan and Turkey, the average monthly return on the dispersion portfolio is positive in all the other 18 countries. This result strongly rejects the hypothesis that the dispersion effect is an occasional event. This is because under the null hypothesis that the

dispersion effect happens occasionally, we would expect that positive return on the dispersion portfolio is as likely to occur as negative return on the dispersion portfolio. Furthermore, the mean of the average monthly return on the dispersion portfolio across the 20 countries is 0.55, which is significantly different from zero (t -statistic=6.27). We also report the three factor (market, size, book-to-market) adjusted abnormal return on the dispersion portfolio in Table 2.2, which is positive in 19 countries (except in Turkey). Therefore, these findings suggest that the dispersion effect originally documented in the US market is not a country specific anomaly, but a pervasive effect around the world.

The results of Table 2.2 also indicate that the dispersion effect varies widely across countries. In particular, the average monthly return on the dispersion portfolio is highest in Singapore and Germany (both are above 1%), and lowest in Turkey and Japan (both are negative). Moreover, the average monthly return on the dispersion portfolio is positive and significantly different from zero (at the significance level of 0.05) in 10 countries; and in the other 10 countries, it is not significantly different from zero. Based on the three factor adjusted abnormal return on the dispersion portfolio (alpha), the dispersion effect is significant (at the significance level of 0.05) in 12 countries, and not significant in the other 8 countries.

2.4 Information Uncertainty and the Dispersion Effect

2.4.1 Hypothesis

In section 2.3, we observe a wide variation in the dispersion effect across the 20 countries in our sample. We then investigate the reason why the dispersion effect differs across countries to shed light on the cause of the dispersion effect.

The two most prominent explanations for the dispersion effect in the US market are proposed by Diether et al. (2002) and Johnson (2004). Diether et al. (2002) use dispersion in analysts' earnings forecasts as a direct proxy for the difference of opinions among investors. They argue that the more opinions differ, the more negative views of investors will be kept out of the market by short sale constraints, and the more stocks become overpriced. Johnson (2004) view dispersion in analysts' earnings forecasts as a proxy for the idiosyncratic parameter risk relating to the unobservability of a firm's underlying value. He builds a rational asset pricing model in which stock price is viewed as the option value of a levered firm. He shows in the model that the higher the idiosyncratic parameter risk, the higher the option value, thus the higher the stock price, resulting in lower expected return.

Although Diether et al. (2002) and Johnson (2004) provide different mechanisms to explain the dispersion effect, the common underlying theme of these two explanations is that information uncertainty causes the dispersion effect: in Diether et al. (2002), information uncertainty generates divergent opinions; while in Johnson (2004) information uncertainty generates idiosyncratic parameter risk. Therefore, the explanations of both Diether et al. (2002) and Johnson (2004)

imply that the dispersion effect increases with information uncertainty. The global setting offers us the opportunity to test this implication: we expect the dispersion effect to be stronger in countries with more information uncertainty, because dispersion is more likely to be proxy for divergent opinions or idiosyncratic parameter risk in such countries.

2.4.2 Empirical Test

We test the arguments of Diether et al. (2002) and Johnson (2004) by examining the dispersion effect across countries with different information uncertainty. We employ four measures of country level information uncertainty: earnings opacity, return volatility, disclosure level and the enforcement of accounting standards.

We use the measure constructed by Bhattacharya et al. (2003) to capture the earnings opacity of a country, which they define as the extent to which the distribution of reported earnings of firms in that country fails to provide information about the distribution of the true, but unobservable, economic earnings of firms in that country. Thus, the higher the earnings opacity of a country, the more difficult for investors to deduce the true earnings from reported earnings, and the higher the information uncertainty.

We calculate the return volatility of a country as the market return volatility in that country over the sample period. Higher market return volatility is likely to be associated with more volatile earnings generating process, which is usually associated greater information uncertainty.

We measure the disclosure level of a country based on CIFAR (1995). CIFAR represents a disclosure score based on the average number of 90 accounting and nonaccounting items disclosed in the annual reports for a large number of companies in a country. Thus, countries with higher CIFAR scores provide more disclosures to outside investors, which should help reduce the information uncertainty of firms in these countries. We multiply CIFAR by -1 to obtain our measure of disclosure level, so that a higher value of disclosure level indicates more information uncertainty.

Finally, we use the number of auditors per 100,000 population to capture the enforcement of accounting standards in a country. As the proportion of auditors in the population rises, the enforcement of accounting standards should also rise, which facilitates investors to better interpret financial statements, thus lowering information uncertainty. We obtain this variable from Bhattacharya et al. (2003), and the original source is communication with the International Federation of Accountants (IFAC) Secretariat on August 13, 1996. We multiply this variable by -1, so that a higher value of enforcement indicates more information uncertainty. The values of these four measures of information uncertainty in each country can be found in the Appendix.

We use the following regression to test whether the dispersion effect is stronger in countries with more information uncertainty:

$$DE_{jt} = \beta_0 + \beta_1 IU_j + \beta_2 Size_{jt} + \beta_3 Mb_{jt} + \beta_4 Mom_{jt} + \varepsilon_{jt} \quad (2.1)^{27}$$

DE_{jt} denotes the dispersion effect, which is measured as the return on the dispersion portfolio in month t of country j . IU_j is the information uncertainty of country j , which is proxied by earnings opacity, return volatility, disclosure level and enforcement of accounting standards, as defined above. Higher IU_j indicates more information uncertainty. Note that IU_j is constant for each country over our sample period. This is because (1) we have only one value for some IU proxies for each country, such as disclosure level and accounting standards enforcement, and (2) the average value over a long time period should be more precise than individual value in a year to measure the information uncertainty of a country, especially when the data quality of *datastream* is not high. In the regression, we control medium log firm size ($Size_{jt}$), log market to book ratio (Mb_{jt}), momentum (Mom_{jt}) at the beginning of month t in country j , since there are factors which are likely to affect expected stock returns. Because the number of observations in each month is small (range from 13 to 20), we use the pooled regression. We control time fixed effects (based on months) and calculate test statistics using country clustered standard errors, in order to address the problem of the cross sectional and time series dependence of the error term. We are most interested in the coefficient on IU_j , which we expect to be positive and significant.

²⁷ Results are similar if we control country dispersion level.

The results are reported in Table 2.3. Contrary to our expectation, we find the coefficients on all four measures of information uncertainty are negative and significant, suggesting that the dispersion effect is stronger in countries with less rather than more information uncertainty. Therefore, these results do not support the explanations for the dispersion effect put forward by either Diether et al. (2002) or Johnson (2004).

Barron et al. (2008) try to differentiate the arguments of Diether et al. (2002) and Johnson (2004) by decomposing forecast dispersion into two parts: disagreement and idiosyncratic parameter risk. They conclude that the negative relationship between dispersion and future stock return arises because dispersion is a proxy for idiosyncratic parameter risk rather than disagreement. Sadka and Scherbina (2007) presume that stocks with high forecast dispersion are mispriced, and explain the persistence of this mispricing by associating high forecast dispersion with high transaction cost. In particular, they argue that forecast dispersion increases with information uncertainty, thus there is high informational advantage of potentially better-informed investors for stocks with high forecast dispersion, and market makers raise the trading cost of these stocks to protect themselves. Essentially, these two papers also use dispersion as proxy for information uncertainty, and imply that the dispersion effect gets stronger as there is more information uncertainty. Since we find that the dispersion effect is more pronounced in countries with less information uncertainty, our findings do not

support the arguments of Barron et al. (2008) and Sadka and Scherbina (2007) as well.

Diether et al. (2002) argue that the negative opinions of investors are censored by short sale constraint, resulting in the dispersion effect. This implies that the dispersion effect should be stronger as short sale constraint gets higher. Thus we examine the dispersion effect across countries with different short sale constraints to test this implication. We create a dummy variable $Noshort_{jt}$, which is equal to 1 if short selling is not allowed or practiced in month t of country j , and 0 if short selling is allowed and practiced. We obtain the short selling information of each country from Bris et al. (2007). Admittedly, many stocks still cannot be short sell even though short selling is allowed and practiced in that country. However, on average short selling constraints should be higher in countries where short selling is not allowed or practiced than otherwise.

We use the following regression to conduct the empirical test, which is similar to Equation (2.1). Our interest is on the coefficient of $Noshort_{jt}$, which we expect to be positive and significant, based on Diether et al. (2002).

$$DE_{jt} = \beta_0 + \beta_1 Noshort_{jt} + \beta_2 Size_{jt} + \beta_3 Mb_{jt} + \beta_4 Mom_{jt} + \varepsilon_{jt} \quad (2.2)$$

The result is reported in the last column of Table 2.3. We find the coefficient on $Noshort_{jt}$ is negative and not significant, which is not consistent with our expectation. Therefore, this finding does not support the explanation for the dispersion effect offered by Diether et al. (2002) either.

2.5 Investors' Reliance on Analysts' Earnings Forecasts and the Dispersion Effect

2.5.1 Hypothesis

In the previous section, we examine the dispersion effect across countries with different information uncertainty, and find that the dispersion effect is more pronounced in countries with less rather than more information uncertainty. This finding casts doubt on the dominant explanations for the dispersion effect provided in the current literature [Diether et al. (2002); Johnson (2004); Sadka and Scherbina (2007) and Barron et al. (2008)], therefore we propose an alternative explanation for the dispersion effect.

We hypothesize that the dispersion effect arises because (1) investors rely on analysts' earnings forecasts to make investment decisions; and (2) there is an upward bias in the consensus earnings forecast which increases with forecast dispersion. Prior studies show that both in the United States and elsewhere investors incorporate analysts' earnings forecasts in their firm valuations and respond to revisions in these forecasts [e.g., Givoly and Lakonishok (1979); Capstaff et al. (2000); Bercel (1994)]. However, investors' reliance on analysts' earnings forecasts should be different across countries, due to cross country differences in investor protection, stock market development, national culture and accuracy of analysts' earnings forecasts. On the other hand, Diether et al. (2002) document that there is a strong positive relation between upward bias in consensus

earnings forecast and forecast dispersion in the US market. To the extent that this is true other countries as well, the testable implication of our hypothesis is that the dispersion effect is stronger in countries where investors rely more on analysts' earnings forecasts.

2.5.2 Empirical Test

A. Upward bias in Consensus Earnings Forecast and Forecast Dispersion

We first examine whether the positive relation between upward bias in consensus earnings forecast and forecast dispersion is common across countries. In each country, we regress forecast error on forecast dispersion using the following Fama-Macbeth regression:

$$FE_{it} = \beta_0 + \beta_1 Disp_{it} + \beta_2 Size_{it} + \beta_3 Mb_{it} + \beta_4 Mom_{it} + \varepsilon_{it} \quad (2.3)$$

Where FE_{it} is the forecast error of firm i in month t , which is calculated as the difference between the consensus earnings forecast in month t and the corresponding actual earnings, scaled by the absolute value of actual earnings. $Disp_{it}$ is forecast dispersion of firm i in month t . We control log firm size ($Size$), log market to book ratio (Mb) and momentum (Mom) of firm i in month t . In Table 2.4, we report the coefficient on $Disp_{it}$ (*coeff*) and its corresponding Newey-West adjusted t -test statistics (*coeff_t*) for each country. As shown in the table, we find that coefficients on $Disp_{it}$ are positive in all 20 countries, and significantly different from zero in 16 countries. This suggests the positive relation between upward bias in consensus earnings forecast and forecast dispersion is a worldwide phenomenon.

B. Investors' Reliance on Analysts' Earnings Forecasts and the Dispersion Effect

After being assured that upward bias in consensus earnings forecast increases with forecast dispersion in all countries, we then test whether the dispersion effect is stronger in countries where investors rely more on analysts' earnings forecasts. We argue that investors' reliance on analysts' earnings forecasts is higher in countries with stronger investor protection, more developed stock market, a culture of higher uncertainty avoidance and more accurate analysts' earnings forecasts.

Investor protection: In countries with stronger investor protection, investors should have higher demand for information about firms' earnings for two reasons. First, both anecdotal and academic evidence suggest that investors decrease their holdings or even avoid investing in companies or countries that are perceived to provide poor investor protection [Giannetti and Koskinen (2004); Giannetti and Simonov (2005); Leuz et al. (2005); McKinsey&Company (2003 a and b)]. Less interest in investing in the financial market may in turn result in lower demand for any value-relevant information from investors. Second, prior research suggests that weak investor protection reduces the value-relevance of earnings [Hung (2000), Leuz et al. (2003)]. This is because managers in weak investor protection countries enjoy greater private benefits of control, which provides them with higher incentives to manipulate earnings, causing earnings to be less value relevant in these countries. As the value relevance of earnings decreases, investors may have less demand for information about firms' earnings. Since analysts' earnings

forecasts is one important source for investors to learn about firms' earnings, these suggest that investors' reliance on analysts' earnings forecasts should be lower in countries with poorer investor protection. In other words, investors' reliance on analysts' earnings forecasts should be higher in countries which provide stronger investor protection. Empirical studies support this assumption. For example, Barniv et al. (2005) document that in countries with stronger investor protection, analysts with superior ability and resources will more consistently outperform their peers than in countries with weaker investor protection. Defond and Hung (2007) find that analysts are more likely to provide cash flow forecasts in countries with weaker investor protection since earnings are less value relevant in these countries. To the extent that analysts respond rationally to investors' demand for information about firms' earnings, both studies suggest that investors rely more (less) on analysts' earnings forecasts in countries with stronger (weaker) investor protection.

Our first proxy for investor protection is the legal origin of a country. La Porta et al. (1998) show that common law countries tend to have stronger investor protection than do civil law countries. Hence, we create a dummy variable to represent a country's legal origin, which is equal to one if the country is of common law origin, and zero if the country is of civil law origin. Our second proxy for investor protection is efficiency of the judiciary, which is based on investors' assessment of the "efficiency and integrity of the legal environment as it affects business, particularly foreign firms" produced by the country risk rating agency

International Country Risk (ICR). This measure is on a scale from 0 to 10, with lower scores representing lower efficiency levels. We obtain this measure from Rafael La Porta's web site.

Stock market development: Participation cost should be lower in countries with more developed stock markets. Lower participation cost will provide investors with more incentives to participate in the stock markets [Haliassos and Bertaut (1995); Vissing-Jørgensen (2003)], which may result in higher demand for information about stocks, and thus more reliance on analysts' earnings forecasts from investors. Furthermore, the information asymmetry of stock financing is relatively higher than that of most other financing methods [Myers (1984); Myers and Majluf (1984)]. Hence, in countries with more developed stock markets, the information asymmetry between insiders and outsiders should be greater, leading to higher demand for analysts' service. Therefore, both arguments (participation cost and information asymmetry) suggest that investors' reliance on analysts' earnings forecasts should be higher in countries with more developed stock markets.

We use two measures of stock market development. The first measure (*Domfirms_pop*) is the average ratio of the number of domestic firms listed in a given country to its population (in millions) for the period 1996-2000, which is also downloaded from Rafael La Porta's web site. The second measure (*Stock_gdp*) is

the stock market capitalization as a percentage of the country's GDP in year 2000, which is obtained from Chan et al. (2005).

National culture of uncertainty avoidance: Hofstede (2001) find that one of the five dimensions of national culture is uncertainty avoidance, which deals with a society's tolerance for uncertainty and ambiguity. Prior studies argue that ambiguity aversion will deter people from investing in the stock market [Chen and Epstein (2002); Knox (2003)]. This suggests that in countries with a culture of higher uncertainty avoidance, people should be less willing to invest in the stock markets, leading to a lower demand for analysts' information. Moreover, according to Hofstede (2001), uncertainty avoidance ultimately refers to a man's search for truth: people in uncertainty avoidance countries tend to believe that there can be only one truth; while people in uncertainty acceptance countries are more tolerant of different opinions. This indicates that people in countries with a culture of higher uncertainty avoidance are less willing to invest in stocks for which analysts issue different opinions. Therefore, in countries with a culture of higher uncertainty avoidance, investors should be less likely to turn to analysts for information on stocks, especially on those stocks with high forecast dispersion.

We obtain the uncertainty avoidance index from Hofstede (2001), which is based on a cross-country psychological survey of employee values conducted by Geert Hofstede between 1967 and 1973. We also use an alternative measure of uncertainty avoidance that comes from GLOBE (Global Leadership and

Organizational Behavior Effectiveness) project. The measure is named as uncertainty avoidance value in House et al. (2004), which can be regarded as an updated index of Hofstede's uncertainty avoidance index. We multiply both indexes by -1, so that higher values indicate higher uncertainty acceptance (or lower uncertainty avoidance), and more reliance on analysts' earnings forecasts.

Accuracy of analysts' earnings forecasts: Investors should rely more on analysts' earnings forecasts as these forecasts are more accurate, and analysts are more willing to make efforts to increase earnings forecast accuracy as they know investors have higher demand for their earnings forecasts [Barniv et al. (2005)]. These two factors reinforce each other, and suggest that investors' reliance on analysts' earnings forecasts increases with the accuracy of these forecasts. We measure the accuracy of analysts' earnings forecasts in a country by taking the time series average of monthly median accuracy of earnings forecasts in the country over our sample period. Earnings forecast accuracy is measured by multiplying -1 to forecast error, which is in turn calculated as the absolute value of the difference between the consensus earnings forecast and actual earnings scaled by actual earnings. Both consensus earnings forecast and actual earnings are obtained from *IBES* summary file.

Examining the dispersion effect across countries with different accuracy of analysts' earnings forecasts also serves to differentiate our hypothesis versus the dominant explanations provided in the current literature [Diether et al. (2002);

Johnson (2004); Sadka and Scherbina (2007) and Barron et al. (2008)]. Hope (2002) argue that enhanced annual report disclosures and stronger enforcement of accounting standards would reduce the uncertainties faced by analysts when they forecast firms' earnings, thus increase the accuracy of analysts' earnings forecast. Consistent with his argument, Hope finds that analysts' earnings forecasts are more accurate in countries which provide more disclosures and stronger enforcement of accounting standards. This suggests the accuracy of analysts' earnings forecasts in a country can also be viewed as proxy for its information uncertainty, with lower accuracy indicating more information uncertainty. Recall from Section 2.4, the dominant explanations in the current literature would predict that the dispersion effect is stronger in countries with lower accuracy of analysts' earnings forecasts, which is opposite to the prediction of our hypothesis proposed in this section.

We test whether the dispersion effect is stronger in countries where investors rely more on analysts' earnings forecasts using the following regression:

$$DE_{jt} = \beta_0 + \beta_1 IR_j + \beta_2 Size_{jt} + \beta_3 Mb_{jt} + \beta_4 Mom_{jt} + \varepsilon_{jt} \quad (2.4)^{28}$$

IR_j denotes investors' reliance on analysts' earnings forecasts in country j , which is proxied by investor protection, stock market development, national culture of uncertainty avoidance and the accuracy of analysts' earnings forecasts in country j . A higher value of IR indicates that investors rely more on analysts' earnings forecasts. Consistent with IU in Equation (2.1), IR is constant for each country over

²⁸ Results are similar if we control country dispersion level.

our sample period. The definitions of other control variables are the same as in Equation (2.1). We use pooled regression in Equation (2.4) and calculate test statistics using country clustered standard errors. We focus on the coefficient of IR , which we expect to be positive and significant.

The results are reported in Table 2.5. Consistent with our expectation, we find that the coefficients on all proxies of IR are positive and significant. Specifically, we find that the dispersion effect is stronger in countries with stronger investor protection, more developed stock market development, higher uncertainty avoidance and higher accuracy of analysts' earnings forecasts. Taken in their entirety, these results suggest that the dispersion effect is stronger in countries where investors rely more on analysts' earnings forecasts. Coupled with the finding in Part A of this section, these results offer strong support for our hypothesis that the dispersion effect arises because investors rely on analysts' forecast for their investment decisions, and there is an upward bias in the consensus earnings forecast which increases with forecast dispersion. On the other hand, the result that the dispersion effect increases with the accuracy of analysts' earnings forecasts does not lend support for the dominant explanations provided in the current literature [Diether et al. (2002); Johnson (2004); Sadka and Scherbina (2007) and Barron et al. (2008)].

C. Analysts' Incentives, Upward Bias in Consensus Earnings Forecast, and Forecast Dispersion

In this section, we try to explore why the upward bias in consensus earnings forecast increases with forecast dispersion. Though Diether et al. (2002) mainly use disagreement and short sale constraints to explain the dispersion effect, they also mention that analysts' incentive structure could also be the reason. Specifically, they argue that the incentive structure of analysts cause them to be reluctant to issue pessimistic earnings forecasts, which generates an upward bias in the consensus earnings forecast. Furthermore, they argue that this upward bias increases with the initial disagreement about the firm's earning, since the more dispersed the forecasts are, the more negative forecasts will be censored. This argument seems intuitive and reasonable. However, whether it can explain the dispersion effect needs to be tested. The testable implication of this argument is that the positive relation between upward bias in consensus earnings forecast and forecast dispersion should be stronger in countries with more information uncertainty. The reason is as follows: although dispersion can be generated by information uncertainty as argued by previous studies [Diether et al. (2002); Johnson (2004); Sadka and Scherbina (2007) and Barron et al. (2008)], some other factors also can cause dispersion. Consequently, for a given degree of dispersion, the portion of dispersion that is generated by information uncertainty (i.e., dispersion caused by difference of opinions) would be relatively larger in countries with more information uncertainty, which in turn implies that the magnitude of

censored negative opinion (i.e., the upward bias) would also be larger in these countries.

The hypothesis that in countries with more information uncertainty, the positive relation between upward bias in consensus earnings forecasts and forecast dispersion is stronger can be tested using the following regression:

$$FE_{ijt} = \beta_0 + \beta_1 Disp_{ijt} + \beta_2 Disp_{ijt} * IU_j + \beta_3 IU_j + \beta_4 Size_{ijt} + \beta_5 Mb_{ijt} + \beta_6 Mom_{ijt} + \varepsilon_{ijt} \quad (2.5)$$

Where FE_{ijt} is the forecast error of firm i in country j at month t , and $Disp_{ijt}$ is the forecast dispersion of firm i in country j at month t . IU_j is the information uncertainty of country j , as defined in Equation (2.1). $Disp_{ijt} * IU_j$ is the interaction of $Disp_{ijt}$ and IU_j . $Size_{ijt}$, Mb_{ijt} and Mom_{ijt} are respectively the log firm size, log market to book, and momentum of firm i in country j at month t . We control both time fixed effect and country fixed effects in Equation (2.5), and calculate test statistics using country clustered errors. We are most interested in the coefficient on $Disp_{ijt} * IU_j$, which we expect to be positive and significant.

The results are reported in Table 2.6. As shown in the table, the coefficients on $Disp * IU$ are all negative, and statistically significant when disclosure level and enforcement of accounting standards are used as proxies for IU , which is not consistent with our expectation. Therefore, these finding do not lend support for the alternative argument for the dispersion effect in Diether et al. (2002).

The results in Table 2.6 also indicate that it is the portion of dispersion generated by factors other than information uncertainty that causes the dispersion

effect. A potential factor we think of that can generate both high dispersion and the dispersion effect is analysts' incentives to hold back pessimistic opinions. This can be illustrated using a simple example: A firm covered by three analysts who have respectively made the following forecasts about the earning of next year: \$2, \$2.5 and \$3 per share, is hit with a piece of bad news which would amount to a reduction of earning by \$0.5 per share. If no analysts are affected by incentives upon learning this news, they will all downward revise their forecasts by \$0.5 per share to \$1.5, \$2 and \$2.5 respectively. In this case, the consensus earnings forecast would be downward revised from \$2.5 to \$2 per share and the dispersion remains unchanged. On the other hand, if the analyst who has made the highest forecast (other things being constant, she is the one most likely to be affected by incentives) decides to maintain her previous forecast, the new forecasts would become \$1.5, \$2 and \$3. In this case, dispersion increases and the consensus earnings forecast is biased upward by \$0.5/3.

The testable implication of this hypothesis is that the positive relation between upward bias in consensus earnings forecast and forecast dispersion is stronger in countries where investors rely more on analyst forecasts. This is because when investors rely more on analysts' earnings forecasts, analysts have higher incentives to hold back pessimistic earnings forecasts to obtain favored access to managerial information so that they can deliver superior forecasts, which would increase their influences among investors and their compensation as well.

Furthermore, analysts also have incentive to please managers through issuing upward biased forecasts in order to secure investment banking business for their brokerage, and this incentive is intensified when managers perceive analysts' earnings forecasts have greater impacts on the stock price of their firms.

We use the following regression to test whether the positive relation between upward bias in consensus earnings forecast and forecast dispersion is stronger in countries where investors rely more on analysts' earnings forecasts:

$$FE_{itj} = \beta_0 + \beta_1 Disp_{itj} + \beta_2 Disp_{itj} * IR_j + \beta_3 IR_j + \beta_4 Size_{itj} + \beta_5 Mb_{itj} + \beta_6 Mom_{itj} + \varepsilon_{itj} \quad (2.6)$$

Equation (2.6) is similar to Equation (2.5), except we use IR in Equation (2.6) instead of IU . The proxies of IR are defined in Equation (2.4). We are interested in the coefficient on $Disp_{itj} * IR_j$, which we expect to be positive and significant.

The results are reported in Table 2.7. Consistent with our expectation, we find the coefficients on $Disp_{itj} * IR_j$ are positive and significant using various proxies of IR , which suggest that the positive relation between upward bias in consensus earnings forecast and forecast dispersion is stronger in countries where investors rely more on analysts' earnings forecasts. Hence, these findings support our conjecture that analysts' incentives generate the positive relation between upward bias in consensus forecast and forecast dispersion.

2.6 Conclusion

This paper examines the dispersion effect in the international markets. We find that the dispersion effect is a worldwide anomaly and differs widely across

countries. We also find that the dispersion effect is more pronounced in countries with less rather than more information uncertainty. This finding casts doubt on the explanations for the dispersion effect provided in the current literature. We then propose an alternative explanation: the dispersion effect arises because investors rely on analysts' earnings forecasts to make decisions, and there is an upward bias in consensus earnings forecast which increases with forecast dispersion. Consistent with our hypothesis, we find that there is a worldwide positive relationship between upward bias in consensus earnings forecast and forecast dispersion, and the dispersion effect is stronger in countries where investors rely more on analysts' forecasts, such as those with better investor protection, more developed stock markets, a culture of higher uncertainty avoidance and greater accuracy of analysts' earnings forecasts. Furthermore, we conjecture that the positive relation between upward bias in consensus earnings forecast and forecast dispersion could be due to analysts' incentives, and we find supporting evidence for this conjecture using the global setting. Future research can reexamine the dispersion effect in the US market based on the mechanism of our conjecture.

Tables for chapter 2

Table 2.1 Summary Statistics

Table 2.1 shows the summary statistics of our sample. Column 2 and 3 are the start date and end date of the sample period for each country. Column 4 shows the percentage of observations in each country. *logsize*, *mb*, *mom* and *disp* are the time series average of monthly median log firm size, market to book ratio, momentum and dispersion for each country over the sample period.

<i>market</i>	<i>startdate</i>	<i>enddate</i>	<i>percent</i>	<i>logsize</i>	<i>mb</i>	<i>mom</i>	<i>disp</i>
Australia	199002	200512	2.92	5.699	1.568	0.059	0.080
Canada	199002	200512	3.33	5.179	1.501	0.036	0.136
France	199002	200512	3.94	5.218	1.875	0.046	0.111
Germany	199002	200512	3.26	5.548	2.082	-0.005	0.153
Hong Kong	199002	200512	2.33	5.451	1.124	0.040	0.113
India	199606	200512	0.57	6.458	1.852	0.039	0.106
Italy	199002	200512	1.74	6.260	1.381	0.009	0.174
Japan	199002	200512	11.81	6.782	1.669	-0.003	0.109
Korea (South)	199002	200509	4.48	4.655	0.884	-0.017	0.224
Malaysia	199002	200512	2.06	5.312	1.934	0.028	0.136
Netherlands	199003	200512	1.08	5.309	1.890	0.054	0.077
Singapore	199002	200512	1.71	5.392	1.414	0.031	0.127
Spain	199709	200512	0.54	6.863	1.937	0.070	0.107
Sweden	199008	200512	1.35	5.195	1.665	0.027	0.165
Switzerland	199002	200512	1.85	5.874	1.543	0.044	0.120
Taiwan	199108	200512	2.44	6.184	1.973	-0.010	0.180
Thailand	199104	200512	0.95	4.933	2.066	-0.015	0.169
Turkey	199212	200307	1.44	3.731	1.699	0.103	0.262
United Kingdom	199002	200512	6.62	5.481	1.888	0.044	0.064
United States	199002	200512	43.83	6.033	1.940	0.049	0.042

Table 2.2 Dispersion Effect in the International Markets

For each country, at the beginning of each month, we divide stocks into five portfolios based on the forecast dispersion. D1 include the stocks with the lowest dispersion, and D5 include the stocks with the highest dispersion. D1-D5 is the portfolio that longs the stocks with lowest dispersion and shorts the stocks with the highest dispersion (dispersion portfolio). This table reports the time series average of monthly return on each quintile portfolio (D1 to D5) and on the dispersion portfolio in each country. Alpha denotes the three factor (market, size, book-to-market) adjusted abnormal return on the dispersion portfolio. Numbers in the parentheses are the corresponding *t* test statistics estimated using the Newey-West (1987) technique.

<i>market</i>	<i>D1</i>	<i>D2</i>	<i>D3</i>	<i>D4</i>	<i>D5</i>	<i>D1-D5</i>	<i>Alpha</i>
Australia	1.26	1.08	0.99	0.80	0.28	0.98 (3.18)	0.83 (2.12)
Canada	1.02	1.02	0.71	0.63	0.11	0.90 (3.09)	1.23 (2.87)
France	1.07	1.07	0.85	0.71	0.38	0.68 (2.12)	0.41 (1.53)
Germany	0.50	0.45	0.22	-0.13	-0.51	1.01 (3.08)	0.93 (3.02)
Hong Kong	1.51	1.36	1.19	0.83	0.66	0.85 (2.11)	1.98 (5.26)
India	1.61	1.55	1.76	1.45	0.77	0.84 (1.25)	1.35 (1.97)
Italy	0.64	0.66	0.49	0.29	0.13	0.51 (1.81)	0.46 (1.40)
Japan	0.29	0.35	0.37	0.40	0.31	-0.02 (-0.13)	0.03 (0.15)
Korea (South)	0.84	0.65	0.53	0.53	0.32	0.52 (1.44)	0.75 (2.62)
Malaysia	0.90	0.83	0.70	0.82	0.56	0.34 (0.92)	0.61 (2.02)
Netherlands	1.35	1.10	1.23	1.06	0.67	0.68 (1.87)	0.83 (2.30)
Singapore	1.47	1.05	1.06	0.84	0.42	1.05 (3.22)	1.54 (4.72)
Spain	1.61	1.33	0.86	1.46	1.24	0.37 (0.87)	0.22 (0.49)
Sweden	1.05	0.97	0.95	0.78	0.58	0.47 (0.88)	0.40 (0.88)
Switzerland	1.00	0.98	0.95	0.65	0.63	0.37 (1.57)	0.41 (1.37)
Taiwan	0.46	0.62	0.32	0.41	0.11	0.35 (1.14)	0.61 (2.99)
Thailand	0.26	0.80	1.50	0.37	0.01	0.25 (0.54)	0.70 (1.43)
Turkey	2.99	3.39	2.83	3.17	3.56	-0.57 (-1.06)	-0.39 (-0.57)
United Kingdom	1.06	0.93	0.87	0.84	0.25	0.81 (3.77)	0.76 (2.05)
United States	1.30	1.20	1.10	1.02	0.68	0.62 (1.98)	0.91 (3.33)

Table 2.3 Information Uncertainty and the Dispersion Effect

This table reports the results of the following pooled regression: $DE_{jt} = \beta_0 + \beta_1 IU_{jt} + \beta_2 Size_{jt} + \beta_3 Mb_{jt} + \beta_4 Mom_{jt} + \varepsilon_{jt}$

Where DE_{jt} denotes the dispersion effect, which is calculated as the return on the dispersion portfolio in month t of country j , and IU_{jt} is the information uncertainty of country j , which is proxied by its earnings opacity, return volatility, disclosure level and enforcement of accounting standards. Higher IU_{jt} indicates more information uncertainty. We control medium log firm size ($Size_{jt}$), log market to book ratio (Mb_{jt}), and momentum (Mom_{jt}) at the beginning of month t in country j . We include time fixed effects and calculate t test statistics using the country clustered standard errors. t test statistics are reported in parentheses.

In the last column, we report the result of the following pooled regression: $DE_{jt} = \beta_0 + \beta_1 Noshort_{jt} + \beta_2 Size_{jt} + \beta_3 Mb_{jt} + \beta_4 Mom_{jt} + \varepsilon_{jt}$

Where $Noshort_{jt}$ is a dummy variable, which is equal to 1 if short selling is not allowed or practiced at month t in country j , and 0 if short selling is allowed and practiced.

	Information Uncertainty				Short Sale Constraints
	Earnings Opacity	Return Volatility	Disclosure Level	Enforcement of Accounting Standards	
<i>IU/Noshort</i>	-0.126 (-2.40)	-6.031 (-2.39)	-0.018 (-1.72)	-0.001 (-3.53)	-0.270 (-1.30)
<i>Size</i>	0.047 (0.39)	-0.042 (-0.33)	0.050 (0.35)	-0.101 (-0.91)	0.033 (0.22)
<i>Mb</i>	0.387 (1.49)	0.385 (1.39)	0.385 (1.37)	0.566 (2.05)	0.462 (1.50)
<i>Mom</i>	-1.109 (-1.33)	-1.084 (-1.32)	-1.157 (-1.34)	-0.118 (-0.18)	-1.128 (-1.29)

Table 2.4 Upward Bias in Consensus Earning Forecast and Forecast Dispersion

This table reports the coefficient on dispersion ($coeff$), and its Newey-West adjusted t test statistic ($coeff_t$) in the following Fama-Macbeth regression for each country: $FE_{it} = \beta_0 + \beta_1 Disp_{it} + \beta_2 Size_{it} + \beta_3 Mb_{it} + \beta_4 Mom_{it} + \varepsilon_{it}$. Where FE_{it} is the forecast error of firm i in month t , which is calculated as the difference between the consensus earnings forecast in month t and the corresponding actual earnings, scaled by the absolute value of actual earnings. $Disp_{it}$ is forecast dispersion of firm i in month t . We control log firm size ($Size$), log market to book ratio (Mb) and momentum (Mom) of firm i in month t .

Country	Australia	Canada	France	Germany	Hong Kong	India
coeff	0.491	0.247	0.061	0.102	0.719	0.527
coeff_t	(6.17)	(3.64)	(1.94)	(3.26)	(4.28)	(0.83)
Country	Italy	Japan	Korea	Malaysia	Italy	Japan
coeff	0.322	0.163	0.119	0.788	0.322	0.163
coeff_t	(3.42)	(3.82)	(1.01)	(5.00)	(3.42)	(3.82)
Country	Netherlands	Singapore	Spain	Sweden	Switzerland	Taiwan
coeff	0.393	0.722	0.278	0.154	0.325	0.356
coeff_t	(2.87)	(4.17)	(1.56)	(1.24)	(2.93)	(3.23)
Country	Thailand	Turkey	UK	US	Thailand	Turkey
coeff	0.704	0.089	0.399	0.223	0.704	0.089
coeff_t	(3.12)	(1.85)	(5.13)	(17.67)	(3.12)	(1.85)

Table 2.5 Investors' Reliance on Analysts' Earnings Forecasts and the Dispersion Effect

This table shows the results of the following pooled regression: $DE_{jt} = \beta_0 + \beta_1 IR_{jt} + \beta_2 Size_{jt} + \beta_3 Mb_{jt} + \beta_4 Mom_{jt} + \varepsilon_{jt}$
 IR_{jt} denotes investors' reliance on analysts' earnings forecasts in country j , which is measured by the investor protection, stock market development, national culture of uncertainty avoidance and accuracy of analysts' earnings forecasts in country j . Higher IR_{jt} indicates that investors rely more on analysts' forecasts. We include time fixed effects and calculate t test statistics using country clustered standard errors. T test statistics are reported in parentheses.

	Investor Protection		Stock Market Development		Uncertainty Avoidance		Analysts' Earnings Forecast Accuracy
	Legal Origin	Judicial Efficiency	Domfirms_pop	Stock_gdp	Hofstede (2001)	House et al. (2004)	
IR	0.321 (2.11)	0.093 (2.72)	0.009 (4.95)	0.002 (2.17)	0.009 (2.88)	0.188 (1.99)	1.848 (2.72)
Size	0.063 (0.43)	-0.024 (-0.18)	0.037 (0.26)	0.028 (0.19)	0.074 (0.62)	0.046 (0.29)	-0.051 (-0.43)
Mb	0.369 (1.31)	0.446 (1.80)	0.620 (2.58)	0.490 (2.16)	0.378 (1.49)	0.377 (1.35)	0.333 (1.28)
Mom	-1.131 (-1.31)	-1.160 (-1.37)	-1.262 (-1.49)	-1.191 (-1.38)	-1.152 (-1.37)	-1.153 (-1.32)	-1.072 (-1.32)

Table 2.6 Information Uncertainty, Upward Bias in Consensus Earnings Forecast and Forecast Dispersion

This shows the results of the following pooled regression:

$$FE_{ijt} = \beta_0 + \beta_1 Disp_{ijt} + \beta_2 Disp_{ijt} * IU_j + \beta_3 IU_j + \beta_4 Size_{ijt} + \beta_5 Mb_{ijt} + \beta_6 Mom_{ijt} + \varepsilon_{ijt}$$

Where FE_{ijt} is the forecast error of firm i of country j in month t , which is calculated as the difference between the consensus earnings forecast for firm i in month t and the corresponding actual earnings, scaled by the absolute value of actual earnings. $Disp_{ijt}$ is forecast dispersion of firm i in country j at month t . We control log firm size ($Size_{ijt}$), log market to book ratio (Mb_{ijt}) and momentum (Mom_{ijt}) of firm i in country j at month t . IU_j denotes the information uncertainty of country j , as in Table 2.3. We control both time fixed effects and country fixed effect, and calculate test statistics using country clustered standard errors. T test statistics are reported in parentheses.

	Earnings Opacity	Return Volatility	Disclosure Level	Enforcement of Accounting Standards
Disp	0.182 (2.50)	0.264 (4.85)	-0.349 (-1.79)	0.113 (4.08)
Disp*IU	-0.005 (-0.41)	-1.987 (-2.12)	-0.007 (-2.55)	0.000 (-2.76)
IU	0.100 (0.82)	5.023 (0.92)	0.018 (1.15)	0.001 (1.27)
Size	-0.062 (-18.06)	-0.062 (-17.62)	-0.062 (-17.71)	-0.061 (-18.46)
Mb	0.008 (1.42)	0.008 (1.41)	0.008 (1.44)	0.008 (1.36)
Mom	-0.306 (-22.27)	-0.304 (-22.70)	-0.305 (-22.40)	-0.311 (-22.37)

Appendix

Panel A shows the values of proxies for information uncertainty (*IU*) defined in Section 2.4, and Panel B shows the values of investors' reliance on analysts' earnings forecasts (*IR*) defined in Section 2.5.

Panel A: Information Uncertainty					Accounting Standards Enforcement
Market	Earnings Opacity	Return Volatility	Disclosure Level		
Australia	5.667	0.035	-80		
Canada	3.667	0.035	-75		
France	4.000	0.039	-78		
Germany	3.667	0.052	-67		
Hong Kong	5.333	0.074	-73		
India	8.667	0.098	-61		
Italy	4.000	0.045	-66		
Japan	6.333	0.057	-71		
Korea(South)	5.667	0.097	-68		
Malaysia	6.000	0.085	-79		
Netherlands	2.333	0.044	-74		
Singapore	5.000	0.077	-79		
Spain	6.000	0.039	-72		
Sweden	6.000	0.063	-83		
Switzerland	4.667	0.042	-80		
Taiwan	7.667	0.095	-58		
Thailand	6.333	0.086	-66		
Turkey	8.667	0.168	-58		
The United Kingdom	4.667	0.035	-85		
The United States	5.000	0.042	-76		

Panel B: Investors' Reliance on Analysts' Earnings Forecasts									
Investor Protection			Stock Market Development			Uncertainty		Earnings	
Legal Origin	Judicial Efficiency	Domfirms pop	Stock_gdp	Hofstede (2001)	House et al.(2004)	Avoidance		Forecast	Accuracy
Australia	1	10.00	64.61	102.00	51	3.98		-0.141	
Canada	1	9.25	77.34	106.20	48	3.75		-0.304	
France	0	8.00	13.17	89.50	86	4.26		-0.267	
Germany	0	9.00	9.93	54.70	65	3.32		-0.243	
Hong Kong	1	10.00	103.99	361.00	29	4.63		-0.210	
India	1	8.00	6.03	33.80	40	4.73		-0.181	
Italy	0	6.75	4.47	52.80	75	4.47		-0.371	
Japan	0	10.00	19.27	69.20	92	4.33		-0.249	
Korea(South)	0	6.00	20.49	54.10	85	4.67		-0.447	
Malaysia	1	9.00	32.55	148.40	36	4.88		-0.253	
Netherlands	0	10.00	13.70	131.70	53	3.24		-0.156	
Singapore	1	10.00	83.28	164.80	8	4.22		-0.225	
Spain	0	6.25	15.00	79.90	86	4.76		-0.270	
Sweden	0	10.00	29.39	112.30	29	3.60		-0.267	
Switzerland	0	10.00	32.34	249.00	58	3.16		-0.165	
Taiwan	0	6.75	20.20	101.90	69	5.31		-0.328	
Thailand	1	3.25	6.91	44.80	64	5.61		-0.328	
Turkey	0	4.00	4.29	35.30	85	4.67		-0.646	
The United Kingdom	1	10.00	34.65	157.70	35	4.11		-0.127	
The United States	1	10.00	29.83	142.10	46	4.00		-0.131	

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Chapter3 Analysts' Incentives and the Dispersion Effect

3.1 Introduction

Diether, Malloy and Scherbina (2002) (henceforth DMS) document a negative cross-sectional relationship between dispersion in analysts' earnings forecasts and future stock returns (henceforth the dispersion effect) which cannot be explained by standard asset pricing models such as CAPM, or the Fama and French (1983) model. To explain the dispersion effect, DMS appealed to the idea by Miller (1977) that prices reflect a more optimistic valuation if pessimistic investors are kept out of the market due to short-sale constraints. Firms with more diverse investor opinions, such as those with high dispersion in analysts' forecasts, are more likely to generate lower returns as there would be a larger upward bias in the stock prices when the opinions of the more pessimistic investors have been censored. DMS also interpret their results as a rejection of the notion that dispersion in forecasts can be viewed as a proxy for information risk. However, treating dispersion in forecasts as a proxy for unpriced information risk, Johnson (2004) shows that the dispersion effect can be consistent with a rational asset pricing model in which the expected return of stocks would decrease with idiosyncratic asset risk in the presence of leverage. Viewing dispersion as a proxy for both analyst disagreement and information asymmetry, Sadka and Scherbina (2007) suggest the following: that the dispersion effect is a result of pricing error, it is especially prominent among illiquid stocks, and that it can be persistent through time.

A common theme in these papers is that dispersion is exogenous and can be viewed as a measure of information uncertainty [Johnson (2004), Sadka and Scherbina

(2007)] or as a measure of difference of opinion [DMS, Sadka and Scherbina (2007)], and that high dispersion causes a low future return. However, in an international setting, Hwang and Li (2009) document that the dispersion is more pronounced in countries with less information uncertainty, which does not support the explanations for the dispersion effect provided in the literature. Rather, Hwang and Li (2009)'s findings suggest the dispersion effect arises because investors rely on analysts' forecast for their investment decisions, and that there is an upward bias in the consensus forecast for high dispersion firm. They also propose a mechanism in which analysts' incentives of not fully downward revising their forecasts when they possess bad news would simultaneously increase forecast dispersion and induce an upward bias in the consensus forecast. In this paper, we examine whether this alternative explanation based on analysts' incentives can explain the dispersion effect in the US market.

It has long been suggested that the incentive structure of analysts can distort their earnings forecasts. Analysts have incentives to issue optimistic views when they face pressure to help their brokerage firms procure investment banking business [Dugar and Nathan (1995), Hunton and McEwen (1997), Lin and McNichols (1998), and Das et al. (2006)], generate trading volume [Irvine (2001) and Cowen et al. (2006)], or to obtain access to managers' inside information [Francis and Philbrick (1993), Das et al. (1998), Lim (2001)]. These incentives are more pervasive across firms that have bad yet publicly unknown information. While non-incentive-driven analysts revise their forecasts downward promptly to pursue accurate forecasts, incentive-driven analysts retain their previous forecasts as they do not wish to be the first runners to bring the bad news to the public. This sluggish reaction of incentive-driven analysts results in a dispersion increase

for firms with bad news. Another consequence of analysts' incentives is that consensus forecasts would be upward-biased as they do not reflect the full extent of the bad news known to analysts. If investors utilize consensus forecasts to make their investment choices, the upward-biased consensus forecasts would lead to stocks being overpriced as well as lower future returns.

The main idea in our paper can be illustrated with a simple example. A firm covered by three analysts who have respectively made the following forecasts about the earning of next year: \$2, \$2.5 and \$3 per share, is hit with a piece of bad news which would amount to a reduction of earning by \$0.5 per share. If no analysts are affected by incentives upon learning this news, they will all downward revise their forecasts by \$0.5 per share to \$1.5, \$2 and \$2.5 respectively. In this case, the consensus forecast would be downward revised from \$2.5 to \$2 per share and the dispersion remains unchanged. On the other hand, if the analyst who has made the highest forecast (other things being constant, she is the one most likely to be affected by incentives) decides to maintain her previous forecast, the new forecasts would become \$1.5, \$2 and \$3. In this case, dispersion increases and the consensus forecast is biased upward by $\$0.5/3$, resulting in a negative relationship between dispersion and future return, now known as the dispersion effect. Note that even if the incentive-driven analyst downward revises her forecast, as long as the revision is less than the bad news entails, the dispersion effect ensues. This simple example brings out the essence of our thesis that the negative relationship between the forecast dispersion and future stock return arises from the fact that they are affected by a common source—analysts' incentives.

Consistent with the testable hypotheses we develop from this thesis, we find (1) that the dispersion increases with analysts' incentives, (2) that the dispersion effect exists only among firms facing future bad news, (3) that the dispersion effect disappears once we control for the upward bias in consensus forecast induced by analysts' incentives, and (4) that stronger analysts' incentives and stronger dispersion effect can be found among firms with lower information uncertainty. The last two findings offer unique support to our thesis.

We are not alone in recognizing the effect that analysts' incentives might have on stock returns. Although DMS mainly relies on short sale constraints emphasized in the Miller (1977) model to explain the dispersion effect, they also suggest that the incentive structure of analysts can be viewed as an alternative mechanism to short-sale constraints in suppressing negative information. Scherbina (2007) presents evidence showing that stock prices do not immediately reflect the negative information embedded in reduced analyst coverage, which suggests that when setting prices, investors do not, or cannot, take into account the upward bias in the consensus forecast induced by analysts' incentives to withhold unfavorable information. This forms the basis of our, and DMS's argument that analysts' incentives can affect stock returns. However, our paper differs from DMS and Scherbina (2007) in one important aspect. In our hypothesis, incentives affect both dispersion and future return, which induces the negative relationship between the two. In contrast, dispersion in DMS and Scherbina (2007) is thought to be exogenous rather than affected by incentives, and is thought to be able to cause lower future return since dispersion would enhance the upward bias in stock price induced by incentives.

Neither are we alone in arguing that dispersion can be affected by incentives. In an independent paper from ours, Erturk (2006) also proposes that analysts' sluggishness to downward revise their forecasts increases forecast dispersion. Erturk (2006) does not provide direct evidence for his conjecture; he only shows that firms with bad news will have higher forecast dispersion levels and greater dispersion changes, which are also consistent with the managerial incentive hypothesis proposed by Liu, Xu and Yao (2004). They argue that managers of bad news firms have incentives to hide bad news by reducing disclosure or by purposely making more opaque news announcements, which result in greater information uncertainty and higher dispersion in analysts' forecasts. Our paper differs from Erturk (2006) and Liu, Xu and Yao (2004) in three aspects. First, we provide direct evidence showing that analysts' incentives heighten dispersion. Second, we estimate the upward bias induced by analysts' incentives in consensus forecasts. Third, we offer evidence that analysts' incentives rather than managerial incentives are more responsible for the dispersion effect by showing that the dispersion effect vanishes once we control for the analysts' incentive-induced bias we estimate.

Recently, Avramov et al. (2008) postulated that the dispersion effect is a result of financial distress, signified by credit rating downgrades. Focusing on firms rated by S&P, they find that the dispersion effect concentrates on the worst rated stocks and only exists during periods of deteriorating credit conditions in which the lowest rated stocks experience a large price drop and a large increase in forecast dispersion. Avramov et al. (2008) do not offer a specific explanation for these results other than that they are related to financial distress. However, their results are consistent with our hypothesis since deteriorating credit conditions or financial distress can be considered as extremely bad

news, and analysts' incentives, and therefore the dispersion effect, would be strongest in such firms and periods.

In addition to providing an alternative explanation of the dispersion effect that does not view dispersion as an exogenous proxy for difference of opinion or information uncertainty, our paper also contributes to the literature in two other aspects. First, we show that dispersion is affected by analysts' incentives. This validates the concern raised by Johnson (2004) that dispersion in analysts' forecasts may not be a good proxy for measuring difference of opinion or information uncertainty [cf. DMS, Sadka and Scherbina (2007)] since it is contaminated by analysts' incentives. Second, we uncover a surprising new finding that the dispersion effect is stronger among firms with lower information uncertainty – contrary to implications from the most prominent explanations of the dispersion effect – which can serve as a useful stylized fact for future works in this area.

The layout of the paper is as follows. In the next section, we develop four testable hypotheses. Section 3.3 describes our data and sample characteristics. Section 3.4 tests the hypotheses. Section 3.5 contains the robustness check of our results. Section 3.6 conducts some further analyses of our argument. Section 3.7 summarizes and concludes.

3.2 Hypotheses Development

Analysts interpret public information and collect new information. They spend enormous amounts of time and money analyzing firms' financial statements and searching for private information. It is reasonable to assume that an analyst's opinion of a firm's value is more correct and timely than those of general investors, especially than those of

individual investors. Therefore investors will rely on analysts' issued forecasts to make their investments [Givoly and Lakonishok (1979)].

However, are those forecasts observed by investors always analysts' true beliefs about a given firm? Because of their compensation structure, analysts have incentives to issue relatively optimistic opinions. Accounting literature offers a large amount of empirical evidence supporting the existence of such analysts' incentives. For example, Dugar and Nathan (1995) document that a firm's investment banker analysts tend to be more optimistic than its non-investment banker analysts in issuing earnings forecasts; using experiment, Huton and McEwen (1997) find that underwriting relationship would intensify analysts' forecast optimism; Francis and Philbrick (1993) show that analysts tend to issue optimistic forecasts in order to cultivate relationship with firms' managers, especially in the presence of unfavorable recommendations in the research reports; Lim (2001) proposes a model in which analysts trade off forecast optimism to get access to managers' inside information; Cowen et al. (2006) find that the incentives to generate trading volume can also cause analysts from pure brokerage firms to issue optimistic forecasts; and Hong and Kubik (2003) show that analysts who provide optimistic forecasts are rewarded by favorable career development.

Furthermore, for a given firm, not all the analysts covering this firm would have the same degree of incentives since analysts also care about forecast accuracy [Hong and Kubik (2003)]. Some analysts may be bounded by their consciousness or may not suffer the pressure from their brokerage firms, so they will not compromise their forecast accuracy by issuing optimistic forecasts (Incentive-free analysts). While other analysts are affected by the incentives since the benefits of issuing optimistic forecasts override

providing accurate forecasts (incentive-driven analysts). Therefore for a given firm analysts would have differential strategies of issuing forecasts due to their differential degree of incentives.

One manifestation of analysts' incentives when they receive publicly unknown bad information is their reluctance to downward revise their forecasts, since they do not want to be the first to bring the bad news to the public. Incentive-free analysts release all the bad information by fully downward adjusting their forecasts, while incentive-driven analysts either do not, or only partially downward revise their forecasts. Furthermore, incentive-driven analysts are also more likely to have previously made relatively favorable forecasts. Consequently, two things happen: (1) bad information known by analysts is not fully reflected in their forecasts and (2) forecast dispersion increases as our simple example demonstrated earlier. This implies that among bad news firms, those with high levels of dispersion are likely to be the ones that experience strong analysts' incentives and a large increase in dispersion. Furthermore, since greater analysts' incentives of withholding bad information result in a greater upward bias in the consensus forecast, we also expect the upward bias in the consensus forecast to increase with level of dispersion.

Note that the same logic does not apply to firms with good news. When analysts receive publicly unknown good information, they revise their forecasts upward promptly; thus, high dispersion firms with good news are not expected to have lower future returns. This asymmetry of incentives between good news and bad news firms forms the basis of the following two hypotheses.

Hypothesis 1: The dispersion effect exists only among bad news firms.

Hypothesis 2: Analysts' incentives, change in dispersion, and upward bias in consensus forecast would increase with the level of dispersion among bad news firms.

As we have previously argued, analysts' incentives are the reason behind the negative relationship between dispersion and future return. Since analysts' incentives induce both dispersion increase and an up-ward bias in consensus forecast, and the up-ward bias in consensus forecast is the direct cause of low future return, if we control for this bias, the indirect negative relationship between future return and dispersion should be subsumed (or greatly reduced). Thus, we have the following testable hypothesis that can offer unique support to our analysts' incentive-based explanation of the dispersion effect.

Hypothesis 3: After controlling for analysts' incentive-induced upward bias embedded in consensus earnings forecast, the dispersion effect should greatly diminish if not vanish.

Previous literature often used forecast dispersion as a proxy of information uncertainty since analysts would have relatively more divergent beliefs when a firm's information environment is more uncertain-- in such an environment, information is harder to get and is less precise. Indeed, DMS shows that forecast dispersion is strongly positively related to earnings variability and standard deviation of past stock returns, both of which are thought to be good measures of information uncertainty [Zhang (2006), Jiang et al. (2005) and Chordia et al. (2006).] Since both information uncertainty and analysts' incentives can contribute to high dispersion, high dispersion firms with low information uncertainty are more likely to be covered by incentive-driven analysts and thus exhibit a stronger dispersion effect. On the other hand, the high dispersion of firms with high

information uncertainty less likely originates from analysts' incentives, and the dispersion effect should be less significant. This argument leads to the following hypothesis.

Hypothesis 4: Firms with lower information uncertainty exhibit stronger dispersion effect as high forecast dispersion is more likely to be generated by analysts' incentives.

3.3 Data and Sample Characteristics

Our main sample comes from three sources. Returns are drawn from the CRSP Monthly Stocks Combined File. Financial data are drawn from COMPUSTAT. Analyst forecast data are from IBES. The sample period is from Jan 1983 to Dec 2006.

We construct our sample as follows. We retrieve earnings forecasts with forecast period one and forecast period two (FPI=1 or FPI=2) from IBES Adjusted Detail History File. Earnings forecasts in this file appear on a split adjusted basis so that historical data can be compared with current data on the same basis. Forecasts are rounded to 4 decimals; therefore eliminating the rounding issues arisen from using adjusted detail file (cf. DMS). Following DMS, we extend each forecast until its revision date. If a revision date precedes the actual forecast date, the forecast will be assumed valid only for the month in which it was made. If in any give month, there is more than one forecast for the same analyst and the same fiscal year, we use the latest forecast. To ensure that all forecasts are comparable with respect to the timeline, we require our sample firms to have December fiscal year ends.

We keep forecasts prevailing at the end of each quarter to implement our empirical tests. Since we restrict our sample to firms with December fiscal year ends, we keep the annual forecasts for fiscal year yr earnings prevailing in December of year $yr - 1$ and in

March, June and September of year yr . In order to study how analysts revise their forecasts each quarter t , we require that forecasts by the same analysts for the same fiscal year earnings are available in both quarter t and quarter $t-1$. In other words, we require an analyst to have two non-missing successive forecasts. Furthermore, we also require firms to have at least three analysts each quarter whose forecasts satisfy the above requirements.

Return on assets (Roa) in this paper is measured as the quarterly earnings before extraordinary items (data 8) divided by total asset (data 44) at the end of the last quarter, obtained from COMPUSTAT Industrial Quarterly database. Assuming investors form expectations of Roa of a particular quarter based on the Roa of the same quarter of the previous year, then change in Roa (ΔRoa) of quarter t from year $yr-1$ to year yr can be interpreted as the unexpected Roa of quarter t in year yr which we use to proxy for the news or information possessed by analysts at quarter $t-1$ in year yr . Note that analysts' forecasts at the end of the fourth quarter of year yr should not be influenced by the firms' earnings information of one quarter ahead, which is part of year $yr+1$ earnings; therefore we only match analysts' forecast dispersion change during first, second and third quarter to the earnings information of second, third and fourth quarter respectively.

We further scale ΔRoa by its standard deviation calculated with the data within the last eight years (with a minimum of four years of non-missing data). We define analysts as possessing good (bad) news when the scaled ΔRoa of the firms covered are above (below) the median in a particular quarter. To ensure that our results are not affected by

microstructure-related issues, we exclude firms with prices below 5 dollars at the end of a portfolio-formation quarter. Our final sample has 58,122 firm-quarter observations.

Table 3.1 presents summary statistics of analysts' forecasts. Dispersion is the standard deviation of forecasts scaled by the absolute value of the mean forecast. The dispersion change of quarter t ($\Delta disp_t$) is calculated as the difference between the standard deviation of forecasts at the end of quarter t and the standard deviation of forecasts at the end of quarter $t-1$, scaled by the absolute value of the mean forecast at the end of quarter $t-1$. All variables except for coverage are winsorized at the 1st and 99th percentiles. In our later tests, measures related to analyst forecasts are all winsorized at the 1st and 99th percentiles as well. We use two information uncertainty proxies: one from the financial market; and one based on accounting variables. The first information uncertainty proxy is stock return volatility (Rvol), which is calculated as the standard deviation of monthly stock returns over the past five years, ending at the portfolio formation date (with a minimum of 24 non-missing return observations). The other information uncertainty proxy is earnings volatility (Evol), which is measured as the standard deviation of annual earnings over the past five years (with a minimum of 3 years' non-missing observations). To ensure that the information is known by investors at the time of portfolio formation, we follow Fama and French (1992) by matching earnings information of fiscal year $t-1$ to the period between July of calendar year t and June of calendar year $t+1$. Annual earnings are measured as earnings before extraordinary item (data 18) divided by total asset (data 6) at the beginning of the fiscal year, which are drawn from COMPUSTAT Industrial Annual. Panel B reports the correlation coefficients between these two information uncertainty proxies and the forecast dispersion, which are all positively

significant ($p < 0.0001$). However, the magnitude of the correlation between forecast dispersion and these two information uncertainty proxies (0.240 and 0.186 respectively) is not as high as the correlation between these two information uncertainty proxies themselves (0.546), which is consistent with our conjecture that forecast dispersion may capture something more than information uncertainty – such as analysts' incentives.

3.4 Hypotheses Testing

3.4.1 *Asymmetry of the Dispersion Effect between Good News and Bad News Firms*

Hypothesis 1 predicts that the dispersion effect exists only in bad news firms, since analysts have incentives to withhold bad news, but not good news. Table 3.2 reports the testing results of this hypothesis.

Each quarter, we independently assign firms into five groups based on their forecast dispersions at the end of quarter t and two groups based on scaled ΔRoa of quarter $t+1$. D1 (D5) contains firms in the lowest (highest) dispersion quintile; good (bad) news firms are those firms whose scaled ΔRoa values are above (below) the median.

Column 2 verifies the existence of the dispersion effect in our sample: high dispersion firms have lower returns than those of low dispersion firms. The raw return differential between D5 and D1 is -0.76% per month ($t = -2.78$), which is similar to the magnitude reported in DMS even though we have used a more restricted sample due to the aforementioned data requirements that we use only forecasts from analysts who have non-missing forecasts between two successive quarters. Column 3 and Column 4 report results supporting Hypothesis 1 that the dispersion effect only exists in bad news firms. Among bad news firms, the average raw return of D5 is lower than that of D1 by 0.98% per month, which is highly significant ($t = -3.41$), while among good news firms, the average raw return

of D5 firms is not significantly different from that of D1 firms ($t=-1.29$). The pattern is the same if the returns are risk adjusted by Fama-French three factors (MKT, SMB and HML) or four factors (MKT, SMB, HML and MOM).

3.4.2 Analyst' Incentives, Change in Dispersion and Upward Bias in Consensus

Forecast

3.4.2.1 Estimating Incentives and Incentive-free Forecasts

Analysts' incentive is the key variable of our thesis. Ideally, we would like to have a direct measure of analysts' incentives; however, empirically it is almost impossible to do so. Firstly, for a particular firm at a certain time point, it is difficult to identify which analysts would have the incentives. We have argued above that analysts have the incentives to be optimistic either to procure investment banking business, or to generate trading volume, or to cultivate relationship with managers. But the literature is not conclusive about which type of motivation would cause analysts to issue more optimistic forecasts in general. Furthermore, even if we have unambiguous evidence that analysts who have business relationship with a firm are more likely to be incentive-driven (as many people believe), the sample we can use would be too small for us to test whether analysts' incentives cause the dispersion effect, which is the focus of this paper. This is because in a particular month, we would have just a few firms performing investment banking businesses, which are not enough to document the dispersion effect. These difficulties motivate us to measure incentives based on how analysts revise their forecasts when they receive bad news. In particular, we rely on the insight that when an analyst is affected by the incentives, her downward revision in forecast in the presence of the bad news would be smaller than that of the consensus forecast. The details are explained below.

To facilitate our discussion, we first define the following variables.

adj_value_t : incentive-free individual analyst forecast at quarter t

$value_t$: observed individual analyst forecast at quarter t

$truemean_t$: incentive-free consensus forecast at quarter t

adj_mean_t : the average of incentive-free individual forecasts, and is by definition the same as $truemean_t$

$mean_t$: observed consensus forecast at quarter t

We assume that other things being constant, analysts affected by incentives will issue more favorable forecasts; therefore, at quarter t we identify a pool of analysts as potentially incentive-driven if their forecasts at quarter $t-1$ rank among the top fourth of all forecasts. Within this pool, we identify analysts as incentive-driven if their downward revisions from quarter $t-1$ to quarter t in the face of bad news of quarter $t+1$ are less than the corresponding incentive-free consensus forecast revision. Stated formally, we use the following two criteria to define an analyst at quarter t as incentive driven:

- (1) her $value_{t-1}$ is among the top fourth of all forecasts at quarter $t-1$;
- (2) $value_t - value_{t-1} > truemean_t - mean_{t-1}$ in the face of bad news of quarter $t+1$.

After identifying those incentive driven analysts, we try to find out what their forecasts would be if they are not inflicted by incentives. We assume that if an analyst is otherwise not incentive driven, her downward forecast revision should be as much as the incentive-free consensus forecast revision. Thus we calculate the difference between incentive-free consensus forecast revision and her forecast revision, and add this difference to her observed forecast in order to obtain her incentive-free forecast (adj_value_t). For an analyst

who is not identified as incentive driven using our criteria, her incentive-free forecast is equal to her observed forecast. In other words, incentive free forecast of individual analyst at quarter t (adj_value_t) is calculated using the following formula below:

If this analyst is identified as incentive driven, then $adj_value_t = (truemean_t - mean_{t-1}) - (value_t - value_{t-1})$;

If this analyst is not identified as incentive driven, then $adj_value_t = value_t$.

As shown in the above formula, the calculation of adj_value_t depends on the knowledge of incentive-free consensus forecast ($truemean_t$), which is not observable. In fact, $truemean_t$ is the average value of incentive-free individual forecast (adj_value_t), thus the calculation of $truemean_t$ in turn depends on the knowledge of adj_value_t . This suggests that adj_value_t and $truemean_t$ can only be estimated interactively, whose detailed procedure is explained as follows:

Step 1: We denote $adj_valu\hat{e}_t$ as the estimator of adj_value_t , $truemean\hat{t}_t$ as the estimator of $truemean_t$, and $adj_mean\hat{t}_t$ as the average value of $adj_valu\hat{e}_t$. By definition, the average of incentive-free individual forecasts should be equal to the incentive-free consensus forecast, therefore the smaller the difference between $truemean\hat{t}_t$ and $adj_mean\hat{t}_t$ is, the better the estimators are.

Step 2: We use observed consensus forecast ($mean_t$) as the initial value of $truemean\hat{t}_t$ to get the initial value of $adj_valu\hat{e}_t$ from Equation (1) below:

$$adj_valu\hat{e}_t = value_t + (truemean\hat{t}_t - mean_{t-1}) - (value_t - value_{t-1}) \quad (1)$$

Step 3: We take the average of $adj_valu\hat{e}_t$ to obtain $adj_mean\hat{t}_t$.

Step 4: If $truemean_i - adj_mean_i > 0.001$, we reduce $truemean_i$ by 0.001 as the updated value of $truemean_i$, and use it in Equation (1) to obtain updated value of adj_value_i . If $truemean_i - adj_mean_i \leq 0.001$, see Step 5.

Step 5: We repeat Step 3 and Step 4 until $truemean_i - adj_mean_i \leq 0.001$. At this point, adj_value_i and adj_mean_i respectively become the estimates of the incentive-free individual forecasts and the incentive-free consensus forecast that we will use in the subsequent analyses.

3.4.2.2 Results

We have argued that analysts of bad news firms have incentives to withhold information which would result in an increase in dispersion. This implies that we are more likely to find firms that have experienced greater incentives and dispersion increases in the high dispersion and bad news group. In particular, Hypothesis 2 predicts that both analysts' incentives and dispersion change would increase with the level of dispersion. An early hint supporting this prediction can be found in Table 3.2, in which we present the average number of firms in each dispersion quintile for the bad news group and the good news group separately. In the low dispersion quintiles, there are more firms in the good news group than in the bad news group. In sharp contrast, we find more bad news firms in the high dispersion quintiles.

In Table 3.3, we test Hypothesis 2 more directly by examining how analysts' incentives and dispersion change across dispersion quintiles. Although Hypothesis 2 applies to bad news firms only, good news firms serve as a benchmark for how dispersion changes across different dispersion levels in the absence of incentives, since analysts have

no incentives to withhold good information. Column 2 in Table 3.3 reports analysts' incentives for bad new firms (*incentive*); this is measured as the ratio of the number of incentive-inflicted²⁹ analysts (as defined above) to the total number of analysts whose previous quarter forecasts are among top fourth. We see that *incentive* monotonically increases with forecast dispersion, and that the average differential between the highest- and lowest- dispersion groups is 18.8%, which is highly statistically significant ($t=26.37$).

In quarter t , we measure bias in consensus forecast ($bias_t$) as the difference between incentive-free mean forecast (adj_mean_t) and observed mean forecast ($mean_t$) scaled by the absolute value of $mean_t$. Column 3 of Table 3.3 shows that the magnitude of upward bias in the consensus forecast is also increasing in dispersion. Specifically, there is almost no consensus forecast bias in the lowest dispersion firms; however, in the highest dispersion quintile, $bias_t$ is a very significant 1.7%. The bias differential between D5 and D1 is also statistically significant ($t=-26.52$). Forecast dispersion change of quarter t ($\Delta disp_t$) is measured as the difference between the standard deviations of forecasts at the ends of quarter t ($stdev_t$) and quarter $t-1$ ($stdev_{t-1}$), scaled by the absolute value of the mean forecast at the end of quarter $t-1$ ($mean_{t-1}$); that is: $\Delta disp_t = (stdev_t - stdev_{t-1}) / \text{abs}(mean_{t-1})$. We choose to scale the difference by the mean forecast of the previous quarter in order to ensure that the dispersion change is not driven by the mean forecast change. Column 4 shows that in all dispersion quintiles, forecast dispersion decreases. However, relative to other dispersion quintiles, the magnitude of dispersion decrease in D5

²⁹ Throughout the paper, I use incentive driven and incentive inflicted interchangeably.

is smaller. The $\Delta disp_t$ differential between D5 and D1 is positive, which is marginally significant ($t=1.76$). However, the effect of incentives on dispersion is best measured as the abnormal dispersion change ($ab_ \Delta disp_t$) against the benchmark dispersion change ($ben_ \Delta disp$) when analysts incentives are absent, for which we have argued that the $\Delta disp_t$ of good news firms is a good proxy. Column 5 of Table 3.3 shows that $ben_ \Delta disp$ is negative for all quintiles, and that the absolute value of $ben_ \Delta disp$ increases with dispersion. This indicates that as time goes by, some uncertainty is resolved and this is especially true for firms with greater uncertainty (proxied by higher dispersion.) Column 6 presents the abnormal dispersion change ($ab_ \Delta disp$) of bad news firms, which is measured as the difference between $\Delta disp_t$ and $ben_ \Delta disp_t$. $ab_ \Delta disp$ is not significant in low dispersion quintiles (D1-D3), but is positive and highly significant in the two highest dispersion quintiles (D4 and D5), which suggests that analysts' incentives increase dispersion significantly for bad news firms. This is why these firms end up in the high dispersion quintile, in addition to the fact that some of these firms may inherently have greater information uncertainty. In summary, Hypothesis 2 is strongly supported by the results reported in Table 3.3.

The last two columns of Table 3.3 offer additional evidence that firms in high dispersion quintiles experience greater increase in dispersion because of analysts' incentives. $adj_ \Delta disp$ and $ab_ adj_ \Delta disp$ represent the dispersion increase and the abnormal dispersion increase respectively, based on incentive-free analysts' forecasts. In particular, $adj_ \Delta disp_t$ is measured as the standard deviation of incentive-free forecasts at

the end of quarter t in excess of the standard deviation of observed forecasts at the end of quarter $t-1$, scaled by the absolute value of observed consensus forecast at the end of quarter $t-1$. We can see that abnormal dispersion increase in each quintile is no longer significantly different from each other. These results are reassuring as it speaks to the efficacy of our mechanism in estimating analysts' incentives and incentive-free forecasts.

3.4.3 Dispersion Effect after Controlling for Bias in Consensus Forecast

We have shown in Table 3.2 that the dispersion effect exists only in bad new firms. In this section, we test Hypothesis 3 to see if the dispersion effect among bad news firms diminishes after we control for the incentive-induced bias embedded in the consensus forecast. Table 3.4 reports results from Fama-MacBeth regressions that regress monthly returns of bad news firms in quarter t on a combination of dispersion, analysts' incentives and incentive-induced upward bias in the consensus forecast measured in quarter $t-1$, in addition to some control variables commonly known to impact stock returns, such as book to market, market capitalization and momentum. The significantly negative coefficient of dispersion in Column 2 verifies the dispersion effect in bad news firms in a regression setting. The significantly negative coefficient of incentive and significantly positive coefficient of bias in Column 3 and Column 4 respectively suggest that our estimates of incentive-free forecasts, analysts' incentives, as well as incentive-induced bias in consensus forecasts are quite effective. They further suggest that analysts' incentives of withholding bad information and the resultant upward bias in consensus forecasts affect stock returns. Column 5 indicates that including analysts' incentives in the regression diminishes the dispersion effect, but that dispersion remains significant. Note that Hypothesis 3 is built upon the argument that incentive-induced upward bias in consensus

forecast is the ultimate cause of low return in high dispersion firms; thus, the indirect negative relationship between dispersion and return, linked through incentives, should be dominated or subsumed by this bias. Column 6 shows that dispersion is indeed completely subsumed by this bias. This result is consistent with the prediction from Hypothesis 3 and is convincing evidence that the dispersion effect observed in the literature can be explained by analysts' incentives to withhold bad information.

3.4.4 Dispersion Effect and Information Uncertainty

In this section, we examine the interaction between the dispersion effect and information uncertainty. Hypothesis 4 predicts a stronger dispersion effect when information uncertainty is lower. We use stock return volatility (Rvol) and earnings volatility (Evol) as proxies of information uncertainty. Firms with uncertainty that ranks among the top 1/3 of the sample are classified as high uncertainty firms; firms with uncertainty that rank among the bottom 2/3 are classified as low uncertainty firms. We use the 1/3 cutoff to ensure that firms in D5 are roughly evenly distributed between low and high information uncertainty groups, because forecast dispersion is positively related to information uncertainty. As firms with high information uncertainty are likely to be riskier than firms with low information uncertainty, we report the dispersion effect in terms of the Fama-French four-factor risk adjusted return (Alpha) of D5-D1 in Table 3.5. Panel A first uses Rvol as the proxy of uncertainty. It shows that the Alpha of the low uncertainty group is -0.54% per month ($t=-2.87$), while that of the high uncertainty group is -0.12% per month ($t=-0.41$). Furthermore, the difference in magnitude of the dispersion effect between high and low uncertainty groups mainly comes from firms with future bad news. To be more specific, within bad news firms, Alpha is significantly negative when uncertainty is

low ($t=-2.87$), but is not statistically different from zero when uncertainty is high ($t=0.90$). In contrast, within the good news group, Alpha is not significantly different from zero for both low and high information uncertainty groups. We also use Evol as the proxy of information uncertainty and find similar and somewhat stronger results that support Hypothesis 4: that the dispersion effect is stronger within firms with low information uncertainty.

Note the underlying premise behind Hypothesis 4: that among firms of high dispersion, those with low information uncertainty must have been comparatively more impacted by analysts' incentives. This is because both information uncertainty and analysts' incentives contribute positively to forecast dispersion; high dispersion firms with little contribution from information uncertainty must find greater contribution from analysts' incentives. We consequently expect that in high dispersion firms, analysts' incentives and the bias they generate are larger in those firms with low information uncertainty. We provide tests for this premise in Panels B. We still use Rvol and Evol as information uncertainty proxies, and report the median values of incentive related variables of high dispersion firms that have bad future news. We choose to report median values since the distribution of bias is skewed. As shown in both panels, analysts' incentives and upward bias of D5 firms (the top dispersion quintile) are indeed significantly larger in firms with lower information uncertainty. This is despite the fact that the dispersion level is lower in the low information uncertainty group, and should have augured for smaller incentives and bias as demonstrated in Table 3.3. Consequently, these results provide a strong support for the premise behind our formulation of Hypothesis 4, and reinforce the support for the hypothesis itself.

As with the results in Section 3.4.3, the results in this section also provide unique support to our analysts' incentive-based explanation of the dispersion effect. Though previous literature provides several mechanisms for the generation of the dispersion effect, there is one common feature among them: they all use forecast dispersion as a proxy of information uncertainty, regard the dispersion effect as stemming from information uncertainty, and predict that the dispersion effect *increases* with information uncertainty. For example, in DMS, greater information uncertainty leads to more heterogeneous opinions, which in turn generate larger stock overpricing in the presence of short sale constraints, leading to stronger dispersion effect. Sadka and Scherbina (2007) argue that the higher the information uncertainty, the higher the information advantage of potentially better-informed investors. Market makers will set a higher spread or provide a smaller depth in order to protect themselves; therefore, trading cost increases with dispersion, explaining the persistence of the dispersion effect through time. In Johnson (2004), stock price is viewed as the option value of a levered firm. Information uncertainty results in parameter risk; the higher the parameter risk, the higher the stock price, and the lower the future return. This also leads to a stronger dispersion effect among firms with high information uncertainty, as these firms also have high dispersion in general. Therefore the affirmative testing results of Hypothesis 4 in this section that the dispersion effect *decreases* with information uncertainty provides a strong support to our analysts' incentive-based explanation of the dispersion effect since all other models/explanations have predicted the opposite.

3.5 Robustness

3.5.1 The Asymmetry of the Dispersion Effect in a Less Restrictive Sample

In testing the asymmetry of the dispersion effect in section 3.4.1, we require that an analyst also has valid forecast in the previous quarter. One concern with this requirement is that dispersion calculated from these analysts' forecasts may not represent dispersion using all valid analysts' forecasts. We address this concern by using forecast dispersion retrieved from IBES unadjusted summary file, which is defined as standard deviation of forecasts scaled by the absolute value of mean forecast ('STDEV' and 'MEANEST' in IBES respectively).

In table 3.6, we independently assign stocks into 5 groups based on their forecast dispersions at the end of month t and two groups based on scaled Roa of month $t+1$. Stocks are held for one month. The results are similar to that of table 3.2. Specifically, the dispersion effect only exists in the group with bad news. Moreover, among high dispersion quintiles, there are more firms in the bad news group than in the good news group; while among low dispersion quintiles, that reverse is true. This ensures that our previous results are not due to the way we calculate forecast dispersion.

3.5.2 Dispersion Effect and Information Uncertainty in a Less Restrictive Sample

In Section 3.4.4, we documented supportive evidence for Hypothesis 4 using the same sample used to test Hypotheses 1 to 3, which requires sample firms to have information of their future operating performance, and requires each analyst covering the firms also to have non-missing forecasts in the previous quarter. Since the future operating performance of firms and individual analysts' forecasts are not needed to test Hypothesis 4, we redo the test with a more comprehensive sample that does not require such information and that is close to those used in other studies. This not only serves as a robustness check for the test of Hypothesis 4, but also documents a negative relationship

between the dispersion effect and information uncertainty from a more general sample. That such a relationship exists in a general sample is a more useful stylized fact for future studies.

The dispersion quintiles and the high and low information uncertainty groups are formed in the same way as in Section 3.4.4, but the standard deviation and the mean of forecasts are obtained directly from IBES Unadjusted Summary History File rather than calculated from IBES Adjusted Detail History File. Panel A in Table 3.6 reports the time series means of descriptive characteristics for each portfolio when Rvol is used as the proxy of information uncertainty. The second column shows that the number of highest dispersion firms is similar between the low information uncertainty group and the high information uncertainty group (173 and 218 respectively). Column 3 to Column 6 show that book to market value, size and the past 12-month returns are systematically related to dispersion and information uncertainty. Firms with low information uncertainty tend to be larger, have lower book-to-market values and lower returns in the past 12 months. Also, for both information uncertainty portfolios, high dispersion firms tend to be losers and have higher book-to-market values. These observations suggest that we should focus on risk-adjusted returns when we compare portfolio returns across dispersion and information uncertainty.

Panel B reports the time series average of returns of different holding periods (1 month, 3 months, 6 months and 12 months.) Similar to Jegadeesh and Titman (1993), the portfolio return in month t with K months holding period is constructed as

$$R_{pt} = \frac{1}{K} \sum_{j=2}^{K+1} R_{t-j}, \text{ where } R_{t-j} \text{ is the equally-weighted portfolio return in month } t$$

for portfolios formed in month $t-j$.

Note that we have skipped a month between the portfolio formation period and the holding period to avoid impacts from market microstructure. Risk-adjusted return (FF-4 alpha) is obtained as the intercept of a time-series regression where R_{pt} is regressed on Fama-Frech three factors and Carhart's momentum factors (MKT, SMB, HML and MOM.) We first examine a one-month holding period return in Panel B. Without distinguishing between low and high information uncertainties (Rvol), D5 (high dispersion quintile) firms have lower returns than do D1 (low dispersion quintile) firms by 0.43% per month. This confirms the dispersion effect in our sample, although the magnitude of the effect is somewhat smaller than that documented by DMS (2002). This difference may reflect the fact that we have required sample firms to have at least 24 months of return data for the calculation of Rvol, and that we have skipped the first month in measuring portfolio returns. Our focus, though, is on comparing the dispersion effect between high and low information uncertainty groups. Continuing with the one-month holding period, D5-D1's raw return in the low information uncertainty group is -0.40%, which is significantly negative ($t = -2.42$). On the other hand, the raw return of D5-D1 in the high uncertainty group is -0.27%, which is not statistically significant ($t = -0.32$). As noted earlier, because firms' characteristics are systematically related to dispersion and information uncertainty, we should draw our conclusions based on risk-adjusted return. Risk adjustment does seem to make the dispersion effect stronger and the difference more significant between high and low information uncertainty groups. Alpha of D5-D1 in the

low uncertainty group is a very significant -0.56% ($t = -4.18$), while it is an insignificant 0.21% ($t = -1.06$) in the high uncertainty group. This result reconfirms support for Hypothesis 4, that the dispersion effect is stronger among firms with lower information uncertainty, even in a more general sample. The results in the longer holding periods (3 month, 6 months and 12 months) are essentially the same as those in the one month holding period.

We next repeat the test by using earnings volatility (Evol) as the proxy of information uncertainty. The descriptive characteristics of the resultant portfolios are similar to those reported in Panel A and are thus not reported. Panel C is the counterpart of Panel B, where Evol replaces Rvol as the proxy of information uncertainty. Although the dispersion effect measured in the raw returns does not seem to be different in longer holding periods, the dispersion effect measured in the risk-adjusted return is consistently larger among firms with low information uncertainty for all holding periods, just as we have seen in Panel B. In summary, the evidence from this less restricted sample offers support for our incentive-based explanation over other prominent explanations of the dispersion effect.

3.6 Further Analyses

In the previous sections, we have argued that some analysts' sluggishness to downward revise their forecasts in face of bad news causes the dispersion effect and we attribute such sluggish actions to analysts' incentives. Since we do not directly measure analysts' incentives, their sluggish actions may be due to alternative reasons. In this section, we first discuss why these alternative reasons cannot explain the dispersion effect.

Then we will conduct temporal analyses which provide additional evidences unique to our incentive-based explanation.

3.6.1 Alternative Reasons

Except for analysts' incentives, there are alternative reasons for analysts' slow reactions. For example, some analysts may be less competent than others, so they are slower in updating their information; or some analysts may suffer from a certain kind of cognitive bias which causes them to under-react to public information; or analysts who possess the most favorable views about a firm may need more evidence for them to change their minds. We admit that all these are possible reasons for analysts' sluggish reactions; however, when addressing the dispersion effect, each of them has difficulty to be consistent with the empirical results we have documented. First, if some analysts' incompetence causes the upward bias in the consensus forecast, which in turn induces the dispersion effect, we would expect the dispersion effect to be stronger among firms which are more uncertain, since incompetent analysts are more prone to make errors when the information environment has greater uncertainty. But this is not consistent with our empirical result. Second, under-reaction to public information cannot be the explanation due to our research design. We examine how analysts revise their forecasts based on future information, that is, information that is not yet publicly available. In fact, such a research design reinforces our incentive-based explanation since incentives can undermine analysts' responsibility to provide their clients with all the information they own privately. Third, though it is reasonable to assume that analysts who held the most extreme views about a firm tend to stick to their opinions, this assumption is difficult to explain the asymmetry between good news and bad news firms: If this assumption is true, we would expect

analysts who had the most unfavorable views about a firm to be slower in revising upward their forecasts when they receive positive news. This would induce dispersion increase and a positive relationship between dispersion and stock return among good news firms, which are not borne out of the data. In fact, the asymmetry between good news and bad news firms also raises questions about the incompetence and under-reaction explanations: why aren't incompetent analysts slower in updating their information when there is good news? Why don't analysts suffering from cognitive bias under-react to positive news? Therefore only analysts' incentive-based argument is consistent with all the empirical results we have documented.

3.6.2 The Impacts of Regulations on the Dispersion Effect

In October 2000, the US SEC implemented the Regulation Fair Disclosure; commonly known as Reg FD, which mandated all the public traded firms must disclose material information to all investors at the same time. In April 2003, the US and state regulators reached a settlement with ten of the largest investment banking firms to address the issues of conflicts of interests between investment banking and analysis departments. The settlement required these firms to insulate their banking and analysis departments from each other physically and with Chinese walls. If the Reg FD and the great settlement are effective, analysts' incentives would be weaker after the adoption of these two regulations since analysts would suffer less pressure from catering firm managers and/or generating investment banking business. Furthermore, if the dispersion effect is indeed generated by analysts' incentives, as we have argued, we would expect the dispersion effect to be weaker after the adoption of these two regulations.

Since our sample period is between year 1983 and 2006, we compare the dispersion effect of two periods: 1983-2000 and 2001-2006. In table 3.8, we report the results. The second row shows that average return differential between D5 and D1 before year 2000 is significantly negative, while after year 2000 the return differential is not significantly different from zero. Moreover, the magnitude of D5-D1 before year 2000 is more than twice the magnitude after year 2000. These results are consistent with our expectation that the dispersion effect would be weaker after year 2000. Furthermore, the third and fourth rows show that analysts' incentives and upward bias in the consensus forecast are smaller in the post year 2000 period correspondingly, suggesting that Regulation Fair Disclosure and the great settlement are successful in attenuating analysts' incentives, which in turn brings down the dispersion effect.

3.6.3 Walk Down to Beatable Analyst Forecasts and the Dispersion Effect

Recent empirical evidence suggests that firms and analysts engage in an "earnings-guidance game" where analysts walk down their forecasts over the fiscal year so that firms can beat their forecasts at the official earnings announcement [Richardson et al. (2004), Ke and Yu (2006), Libby (2008)]. Specifically, analysts tend to issue initial optimistic forecasts followed by pessimistic forecasts immediately before the earnings announcement. This implies incentive driven analysts would not be reluctant to downward revise their forecasts at the short forecast horizon, which in turn predicts that the dispersion effect would be weaker during such periods according to our argument.

We test this prediction by comparing the dispersion effect at the long forecast horizon (six and seven months before fiscal year end) and at the short forecast horizon (the month of fiscal year end and one month before the fiscal year end). The results are shown

in table 3.9: the average return differential between D5 and D1 is significantly negatively at the long forecast horizon, while it is not significantly different from zero at the short forecast horizon. Moreover, the magnitude of the return differential between D5 and D1 at the long horizon is more than twice that at the short horizon. These results are consistent with our argument that it is analysts' incentives that generate the dispersion effect.

Previous literature also documents a temporal change in the trend of analysts' forecasts at the short forecast horizon. Brown, Foster and Noreen (1985) find continued forecast optimism at all forecast horizons. Brown LD (2000) examines periods from 1984 to 1999, and find that reported earnings failed to meet analysts' estimates immediately before the earnings announcement during the 1980s, but shifted to beat analyst' estimates in the 1990s. Richardson et al. (2004) also show that the phenomenon of walking down to beatable analyst forecast was stronger in the 1990s than in 1980s. They suggest that this shift may be due to the adoption of stock based compensation and the inside trading restriction in the 1990s, which motivated managers to care more about the short term stock price, especially immediately after the earnings announcement. These papers suggest that the difference of the dispersion effect between the long horizon and short horizon should be stronger in the 1990s than in the 1980s.

We divide our sample into three periods: 1983-1990, 1991-2000 and 2001-2006, and compare the dispersion effect between the long and short forecast horizon in each period. Table 3.9 shows that during the 1980s, the dispersion effects are significant at both the short and long horizon, and they have similar magnitude. However in the 1990s, the dispersion effect is only significant at the long horizon; and its magnitude at the long horizon is more than three times that at the short horizon. These results are consistent with

our hypothesis. Furthermore, after year 2000, there is no significant dispersion effect at the long or short horizons, which confirms the results shown in section 3.6.2.

3.7 Summary and Conclusion

In this paper, we propose and test an incentive-based explanation of the dispersion effect. We argue that some analysts' reluctance to downward revise their forecasts in face of bad news causes high dispersion and an upward bias in consensus forecast and stock price. This incentive-based explanation necessarily predicts that the dispersion effect only exists in firms with bad news, as analysts are not influenced by incentives when they possess good news. It also predicts a stronger dispersion effect in firms with lower information uncertainty. This particular prediction is inconsistent with other prominent explanations of the dispersion effect. Thus, the finding in this paper that the dispersion effect is indeed stronger in firms with lower information uncertainty constitutes a very strong support for our incentive-based explanation. We also derive an algorithm to measure the unobservable analysts' incentives and the bias they create in consensus forecast. We further find that the dispersion effect disappears after we control for this incentive-induced bias. This result not only speaks to the efficacy of our algorithm in measuring analysts' incentives, but also provides another strong support for our explanation.

Analysts' forecast and the dispersion within their forecasts have been widely used to proxy the level and the divergence in analysts' or investors' true opinions [Klein (1990), Doukas et al. (2002), Easton et al. (2002) and Bamber et al. (1999)] Our findings have demonstrated that analysts' incentives can create a systematic bias in consensus forecast

and forecast dispersion, and further suggest that care should be given in interpreting empirical results which involve such forecast statistics.

One implicit assumption in our hypotheses-- and one that seems to be borne out by the data is that investors trust and rely on analysts' forecasts to price stocks. In other words, investors seem to suffer from some undefined behavior bias that makes them fail to recognize, or properly address, the incentive-induced bias in analyst forecasts. This implication is shared with some previous papers. For example, Michaely and Womack (1999) find that stock returns are significantly lower following the buy recommendation of leader underwriters than that of nonleader analysts. Dechow et al. (2000) document that analysts' long-term growth forecasts are significantly negatively associated with post-offering underperformance of stock return. Understanding the nature of this behavioral bias which prevents investors from fully discounting analysts' incentives should be an interesting topic of future research.

Tables for chapter 3

Table 3.1 Summary Statistics

Panel A presents the forecast statistics of our main sample. *Meanest* is mean earnings forecast at the end of quarter *t*. *Medest* is the medium earnings forecast at the end of quarter *t*. *Coverage* is the number of analysts whose earnings forecasts are valid at the end of quarter of *t*. Forecast dispersion (*Disp*) is the standard deviation of earnings forecasts, scaled by the absolute value of the mean forecast at the end of quarter *t*. Observations with a mean forecast of zero have been deleted. Dispersion change ($\Delta disp$) is the difference between the standard deviation of earnings forecasts at the end of quarters *t* and that of quarter *t*-1, scaled by the absolute value of the mean forecast at the end of quarter *t*-1. These forecast statistics are calculated using individual analyst earnings forecasts, which are drawn from IBES Adjusted Detail History File. Except for coverage, all variables are winsorized at the 99th and 1st percentiles. Panel B presents the Pearson correlation coefficient between the dispersion and two information uncertainty proxies: *Rvol* and *Evol*. Return volatility (*Rvol*) is calculated as standard deviation of monthly returns in the previous 5 years, with a minimum of 24 non-missing return observations. Earnings volatility (*Evol*) is measured as standard deviation of annual earnings in the past 5 years (with a minimum of 3 years), where earnings are defined as the earnings before extraordinary item (data 18) divided by total asset (data 6). Stocks with price below five dollars are excluded. The sample period is from Jan 1983 to Dec 2006.

Panel A: Forecast Statistics					
	Mean	Median	Std Dev	25th	75th
<i>Meanest</i>	1.46	1.23	1.38	0.61	2.03
<i>Medest</i>	1.45	1.23	1.38	0.61	2.05
<i>Coverage</i>	9.68	8.00	6.53	4.00	13.00
<i>Disp</i>	0.14	0.04	0.35	0.02	0.11
$\Delta disp$	-0.01	-0.00	0.10	-0.02	0.01
Panel B: Correlation Coefficient					
	<i>Rvol</i>		<i>Evol</i>		
<i>Disp</i>	0.240		0.186		
	(<0.0001)		(<0.0001)		
<i>Rvol</i>			0.546		
			(<0.0001)		

Table 3.2 Return by Forecast dispersion and Unexpected News

Each quarter, stocks are independently sorted into five groups based on analyst forecast dispersion at the end of quarter t (dispersion), and into two groups based on scaled ΔRoa in quarter $t+1$. Firms with scaled ΔRoa above median are defined as good news firms, while firms with scaled ΔRoa below median are defined as bad news firms. Stocks are held for the next quarter (3 months). Return is the time series average of monthly return for each portfolio. Number is the average number of firms in each portfolio. Stocks with prices below five dollars are excluded. T statistics are in parentheses. The sample period is from Jan 1983 to Dec 2006.

	Return			Dispersion		Number	
	All	Bad	good	bad	good	bad	good
D1	1.28	0.65	1.83	0.01	0.01	79	89
D2	1.10	0.35	1.84	0.03	0.03	84	85
D3	1.12	0.32	1.91	0.05	0.05	84	84
D4	0.88	0.10	1.67	0.09	0.09	85	84
D5	0.52	-0.34	1.47	0.55	0.52	89	79
D5-D1	-0.76	-0.98	-0.36				
	(-2.78)	(-3.41)	(-1.29)				
FF-4 Alpha	-0.53	-0.67	-0.24				
	(-2.75)	(-3.16)	(-1.13)				

Table 3.3 Analysts' Incentives within Bad News Firms

At the end of each quarter t , we assign firms to five portfolios based on their forecast dispersion. This table focuses on firms with future bad news, whose scaled ΔRoa of quarter $t+1$ are below medium value. *incentive* at the end of quarter t is measured as the ratio of number of incentive-inflicted analysts at the end of quarter t to the total number of analyst whose forecasts at the end of quarter $t-1$ are among top 1/4. *bias* of quarter t is the difference between the adjusted mean forecast and the observed mean forecast of quarter t scaled by the absolute value of observed mean forecast of quarter t . Dispersion change ($\Delta disp$) at the end of quarter t is the difference between the standard deviation of earnings forecasts of quarter t and that of quarter $t-1$, divided by the absolute value of the mean forecast of quarter $t-1$. Benchmark dispersion change (ben_disp) is the dispersion change in good news firms (scaled ΔRoa above median), which is measured in the same way as firms with future bad news. Abnormal dispersion change (ab_disp) is the difference between $\Delta disp$ and ben_disp . Adjusted dispersion change (adj_disp) at the end of quarter t is the difference between the standard deviation of incentive-free earnings forecasts at the end of quarter t and the standard deviation of observed earnings forecasts the end of quarter $t-1$, divided by the absolute value of the mean forecast at the end of quarter $t-1$. Abnormal adjusted dispersion change (ab_adj_disp) is the difference between adj_disp and ben_disp . T statistics are in parentheses. Stocks with prices below five dollars are excluded. The sample period is from Jan 1983 to Dec 2006.

	<i>Incentive</i>	<i>bias</i>	$\Delta disp$	ben_disp	ab_disp	adj_disp	ab_adj_disp
D1(Low)	0.142	-0.000	-0.010	-0.010	0.000 (-0.86)	-0.010	-0.001 (-1.65)
D2	0.210	-0.001	-0.009	-0.009	0.000 (0.42)	-0.010	-0.001 (-1.26)
D3	0.238	-0.001	-0.011	-0.012	0.001 (1.26)	-0.013	-0.001 (-1.14)
D4	0.274	-0.004	-0.011	-0.016	0.005 (3.30)	-0.017	-0.001 (-0.35)
D5(High)	0.330	-0.017	-0.003	-0.029	0.026 (6.30)	-0.031	-0.002 (-0.50)
D5-D1	0.188 (26.37)	-0.017 (-26.52)	0.007 (1.76)	-0.019 (-5.16)	0.026 (6.34)	-0.020 (-5.32)	-0.001 (-0.32)

Table 3.4 Analysts' Incentives and Stock Return

This table presents the Fama-Machbeth regression results for firms with future bad news. The dependent variable is the monthly return of quarter t+1. Independent variables are three analysts' incentive related variables, as defined in Table 3.3, and control variables at the end of quarter t. *beta* is portfolio beta, which is calculated following Fama (1992). *ln me* is the log transformation of firm's size; *ln btm* is the log transformation of book to market value; *mom* is the compounded stock return over the previous 12 months.

<i>disperson</i>	-0.51 (-2.93)			-0.45 (-2.42)	-0.30 (-1.57)
<i>incentive</i>		-0.33 (-3.01)		-0.28 (-2.45)	
<i>bias</i>			13.46 (3.43)		9.67 (2.37)
<i>beta</i>	-0.49 (-2.08)	-0.53 (-2.15)	-0.53 (-2.23)	-0.47 (-1.97)	-0.50 (-2.12)
<i>ln me</i>	0.22 (3.99)	0.25 (4.24)	0.23 (4.02)	0.23 (3.99)	0.22 (3.93)
<i>ln btm</i>	0.18 (1.63)	0.15 (1.36)	0.16 (1.46)	0.18 (1.63)	0.17 (1.56)
<i>mom</i>	0.03 (0.09)	0.00 (0.02)	0.03 (0.13)	-0.05 (-0.17)	0.01 (0.03)

Table 3.5 Forecast Dispersion and Information Uncertainty

Panel A reports the Fama-French four factor alpha of D5-D1 for low and high information uncertainty groups separately. Return volatility (*Rvol*) and earnings volatility (*Evol*) are used as information uncertainty proxies. The high information uncertainty group (High) includes firms with uncertainty values among the top 1/3, while the low information uncertainty group (Low) includes firms with uncertainty values among bottom 2/3 each quarter. Firms with above medium scaled ΔRoa are defined as 'good news', while firms with below medium scaled ΔRoa are defined as 'bad news'. Panel B reports the median values of *disp*, *incentive* and *bias* for the highest dispersion firms which have future bad news. Stocks with prices below five dollars are excluded. T statistics are in parentheses. The sample period is from Jan 1983 to Dec 2006.

Panel A: FF-4 factors alpha of D5-D1			
Information Uncertainty			
Groups Based on <i>Rvol</i>	Low	High	
Overall	-0.54(-2.87)	-0.12(-0.41)	
Good News	-0.31(-1.43)	-0.43(-1.24)	
Bad News	-0.61(-2.87)	0.33(0.90)	
Information Uncertainty			
Groups Based on <i>Evol</i>	Low	High	
Overall	-0.61(-3.28)	-0.36(-1.35)	
Good News	-0.28(-1.33)	-0.45(-1.36)	
Bad News	-0.78(-3.55)	-0.10(-0.30)	
Panel B: Median of Analyst Incentive and Bias			
Information Uncertainty			
Groups Based on <i>Rvol</i>	Low	High	Low-High
<i>disp</i>	0.264	0.327	-0.062(-8.37)
<i>incentive</i>	0.247	0.124	0.124(5.20)
<i>bias</i>	-0.005	-0.002	-0.003(-3.33)
Information Uncertainty			
Groups Based on <i>Evol</i>	Low	High	Low-High
<i>disp</i>	0.264	0.327	-0.062(-8.37)
<i>incentive</i>	0.223	0.140	0.083(3.52)
<i>bias</i>	-0.005	-0.002	-0.003(3.42)

Table 3.6 Return by Forecast Dispersion and Unexpected News

At the end of each month t , we sort stocks into five groups based on forecast dispersion, which is the ratio of standard deviation of earnings forecasts to absolute value of mean forecast, as reported in IBES Unadjusted Summary History file. Firms with above medium scaled ΔRoa of month $t+1$ are defined as 'good news', while firms with below medium scaled ΔRoa of month $t+1$ are defined as 'bad news'. Scaled ΔRoa of month $t+1$ is the scaled ΔRoa of the quarter where month $t+1$ falls in. Stocks are held for one month. Return is time series average of monthly return for each portfolio. Number is the average number of firms in each portfolio. Stocks with a price below five dollars are excluded. T statistics are in parentheses. The sample period is from Jan 1983 to Dec 2006.

		Return		Number	
	All	Bad News	Good News	Bad News	Good News
D1	1.40	0.65	2.10	150	158
D2	1.32	0.61	2.02	152	156
D3	1.19	0.3	2.08	154	154
D4	1.22	0.34	2.10	156	152
D5	0.97	0.06	1.95	158	150
D5-D1	-0.42	-0.59	-0.15		
	(-1.95)	(-2.59)	(-0.66)		
FF-4 Alpha	-0.46	-0.62	-0.18		
	(-3.12)	(-3.80)	(-1.15)		

Table 3.7 Portfolio Return by Forecast Dispersion and Information Uncertainty

At the end of each month, we sort stocks into five groups based on their forecast dispersion (*Disp*), which is the ratio of the standard deviation of earnings forecasts to the absolute value of the mean forecast, as reported in IBES Unadjusted Summary History file. *Rvol* and *Evol* are information uncertainty proxies, as defined in Table 3.1. The low information uncertainty group (Low) includes firms with uncertainty values among the bottom 1/3, while the high information uncertainty group (High) includes firms with uncertainty values among the top 2/3. Panel A presents the characteristics of each portfolio. Firm size (*Me*) is the market capitalization (in millions of dollars) at the end of month *t*. Book-to-market (*Btm*) is the book value of equity divided by its market value at the end of month *t*. Return Momentum (*Mom*) is accumulated returns from month *t*-11 to *t*. In panel B, stocks are held for various months (1 month, 3 months, 6 months and 12 months). One month is skipped between portfolio formation period and holding period. Portfolio returns are equally weighted. We report the average return for each portfolio. Stocks with prices below five dollars are excluded. T statistics are in parentheses. The sample period is from Jan 1983 to Dec 2006.

Panel A: Summary Statistics										
	Number		Me		Btm		Mom		Disp	
	Low	High	Low	High	Low	High	Low	High	Low	High
D1	317	75	5.33	0.66	0.44	0.36	0.24	0.46	0.01	0.01
D2	314	88	4.24	1.00	0.49	0.34	0.22	0.42	0.03	0.03
D3	279	120	3.34	1.12	0.57	0.39	0.20	0.41	0.05	0.05
D4	237	159	2.88	0.85	0.67	0.47	0.15	0.33	0.09	0.09
D5	173	218	1.73	0.52	0.87	0.61	0.06	0.20	0.47	0.55
Panel B: Return Volatility as Information Uncertainty Proxy										
	One Month			Three Months		Six Months		Twelve Months		
	All	Low	High	Low	High	Low	High	Low	High	
D1	1.43	1.47	1.23	1.43	1.12	1.40	1.09	1.37	1.07	
D2	1.32	1.39	1.09	1.34	1.09	1.33	1.04	1.33	1.05	
D3	1.23	1.29	1.08	1.28	0.94	1.28	0.94	1.30	1.09	
D4	1.23	1.29	1.14	1.24	1.01	1.22	0.96	1.26	1.04	
D5	1.00	1.07	0.95	0.99	0.84	1.04	0.87	1.13	0.98	
D5-D1	-0.43	-0.40	-0.27	-0.44	-0.28	-0.35	-0.22	-0.24	-0.09	
	(-1.87)	(-2.42)	(-1.32)	(-2.83)	(-1.41)	(-2.28)	(-1.19)	(-1.59)	(-0.51)	
FF-4	-0.43	-0.56	-0.21	-0.59	-0.21	-0.48	-0.15	-0.36	-0.04	
alpha	(-2.77)	(-4.18)	(-1.06)	(-4.61)	(-1.15)	(-3.83)	(-0.90)	(-2.86)	(-0.26)	

Table 3.7
(Continued)

Panel C: Earnings Volatility as Information Uncertainty Proxy									
	One Month			Three Months		Six Months		Twelve Months	
	All	Low	High	Low	High	Low	High	Low	High
D1	1.42	1.47	1.20	1.43	1.24	1.38	1.17	1.36	1.16
D2	1.32	1.34	1.23	1.31	1.20	1.31	1.16	1.30	1.19
D3	1.20	1.29	1.02	1.27	1.01	1.27	1.04	1.29	1.12
D4	1.23	1.28	1.18	1.23	1.11	1.24	1.02	1.26	1.10
D5	1.00	1.10	0.93	1.09	0.88	1.13	0.89	1.20	0.99
D5-D1	-0.42	-0.37	-0.27	-0.34	-0.36	-0.25	-0.28	-0.16	-0.18
	(-1.82)	(-2.02)	(-1.20)	(-1.95)	(-1.68)	(-1.43)	(-1.37)	(-0.93)	(-0.88)
FF-4	-0.41	-0.47	-0.24	-0.47	-0.30	-0.37	-0.19	-0.26	-0.09
alpha	(-2.62)	(-3.39)	(-1.29)	(-3.64)	(-1.62)	(-2.89)	(-1.09)	(-2.12)	(-0.53)

Table 3.8 Dispersion Effect before and after Year 2000

This table reports the dispersion effect before and after year 2000. Pre2000 is the period of 1983-2000. Aft2000 refers to the period of 2001-2006. The second row is the return differential between D5 and D1. Third row reports the analysts' incentives and bias in consensus forecast as defined in table 3.3.

	Pre2000	Aft2000
D5-D1	-0.78	-0.34
	(-3.43)	(-0.68)
<i>incentive</i>	0.21	0.04
<i>bias</i>	-0.0036	-0.0014

Table 3.9 Walk Down to Beatable Forecasts and the Dispersion Effect

This table reports the dispersion effect at the long forecast horizon (six and seven months before the fiscal year end) and at the short forecast horizon (at the fiscal year end and one month before the fiscal year end). Values in this table are the return differential between D5 and D1.

	Short horizon	Long horizon
1983-2006	-0.36 (-1.32)	-0.79 (-2.56)
1983-1990	-0.93 (-2.11)	-0.78 (-2.29)
1991-2000	-0.36 (-0.85)	-1.17 (-2.37)
2001-2006	0.35 (0.58)	-0.19 (-0.24)

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