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Analyst Forecast Consistency

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ABSTRACT

We show empirically that analysts who display more consistent forecast errors have greater ability to affect prices, and that this effect is larger than that of stated accuracy. These results lead to three implications. First, consistent analysts are less likely to be demoted and are more likely to be nominated All Star analysts. Second, analysts strategically deliver downward-biased forecasts to increase their consistency (if at the expense of stated accuracy). Finally, the benefits of consistency and of "lowballing" (accuracy) are increasing (decreasing) in institutional investors' presence.

Because of the large demand for financial analysts' earnings forecasts, researchers have long been interested in how analyst forecast characteristics affect price formation and analysts' career development. For example, prior studies examine the effect of analyst reputation and forecast bias on the volume of trade generated by analyst forecasts (e.g., Bailey et al. (2003), Jackson (2005)), as well as the role of herding (e.g., Hong, Kubik, and Solomon (2000)) or optimism (Hong and Kubik (2003)) on labor market outcomes. The literature commonly uses forecast accuracy (the absolute distance between the forecast and realized earnings) to assess analysts' performance. The general consensus is that accuracy matters. For example, Gu and Wu (2003) show that accuracy is one of the most important aspects of analyst forecast performance. Prior research also finds that more accurate analysts have greater ability to move prices (e.g., Jackson (2005)) and are rewarded with greater professional recognition (Stickel (1992)) and better career outcomes (e.g., Hong and Kubik (2003)).[1]

Interest in these issues is not purely academic. Regulators have scrutinized factors that may lead analysts to issue systematically less accurate forecasts. Implicit in this regulatory activity is the assumption that forecast accuracy should be encouraged, whereas systematic biases should be discouraged. Jackson (2005) explains the rationale for this so-called "Spitzer view" as follows: investors, especially small investors, are unable to debias analyst research, and

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[1] See also Easterwood and Nutt (1999), Mikhail, Walther, and Willis (1999), Park and Stice (2000), Clement and Tse (2005), Ke and Yu (2006), and Leone and Wu (2007).

as a result investors are systematically misled by biased forecasts. Accordingly, regulation has been developed to minimize forecast biases that may lead investors to misestimate the prospects of the firms covered by analysts. Further, controversy over biased recommendations during the boom of the 1990s led to the Global Settlement in April 2003 between major brokerage firms and regulators, with fines and commitments to subsidize research totaling $1.4 billion.

In this paper, we argue that the usefulness of analysts' forecasts should not be based on forecasts' "stated" accuracy (their absolute distance from realized earnings), but rather on forecasts' informativeness. In particular, we argue that, if investors are Bayesian, forecast usefulness should be based on the extent to which an analyst delivers consistent forecast errors, as captured by the volatility of unexpected errors (the inverse of the signal precision, to use Bayesian terminology). To illustrate, consider the forecasts of two analysts. Analyst A delivers forecasts that are consistently three cents below realized earnings, whereas Analyst B delivers forecasts that are two cents above realized earnings half of the time and two cents below realized earnings the other half of the time. Investors should prefer the forecasts of Analyst A. This is because, although in this example Analyst A's forecasts have lower stated accuracy than those of Analyst B, Analyst A's forecasts are more useful as they are a predictable transformation of realized earnings. Thus, as long as investors can unravel a systematic bias, earnings forecasts made by analysts who deliver consistent forecast errors should have a greater effect on prices than those made by analysts who deliver inconsistent forecast errors.[2]

To the extent our main prediction above is true, it leads to three sets of implications. The first concerns analyst welfare. If forecasts issued by more consistent analysts are more informative (i.e., have greater ability to move prices), then more consistent analysts should face a lower probability of being "demoted" to a less prestigious brokerage house (assuming that the incentives of brokerage houses and investors are aligned). In addition, if investors, particularly institutional investors, have greater demand for consistent forecasts, more consistent analysts are more likely to be named All Star analysts.

The second set of implications following from our main prediction concerns analysts' strategic use of biases to increase their consistency. If investors can unravel a systematic bias, then analysts may induce a downward bias in their forecasts (i.e., "lowball") to help managers beat those forecasts. In doing so, analysts can curry favor with managers, leading to better access to managerial information (Lim (2001), Libby et al. (2008)). Such information should help analysts form more accurate *private* expectations of earnings realizations (though not necessarily more accurate *public* forecasts). As a consequence, these biased forecasts should be more consistent (although possibly less accurate) and hence more informative than unbiased forecasts.

[2] Graham, Harvey, and Rajgopal (2005) suggest that managers believe reporting volatile earnings reduces stock prices because investors dislike uncertainty and hence managers may forgo profitable projects to smooth earnings. We focus instead on the effect of analyst forecast consistency on price formation.

The third set of implications following from our main prediction concerns the effect of investor sophistication on forecast informativeness. As suggested above, the hypothesis that investors prefer biased but consistent forecasts to unbiased but inconsistent forecasts relies on investors being able to unravel systematic biases. Because sophisticated investors are more likely to identify a consistent forecast bias than naïve investors, the importance of consistency for the informativeness of a forecast should be increasing in the presence of sophisticated investors.

Our empirical results are in line with our predictions. Specifically, we first find that analysts who deliver more consistent forecast errors have greater ability to move prices, even after controlling for the effect of stated accuracy. This result is both economically and statistically significant. Consistent with prior literature, we find that forecast accuracy is also increasing in an analyst's ability to move prices, but the economic and statistical significance associated with the effect of accuracy is approximately two to four times smaller than when we consider the effect of consistency—that is, the effect of consistency is larger than that of accuracy. Second, we find that differences in forecast error consistency have consequences for analysts' careers: analysts who show more consistent forecast bias are less likely to be demoted to less prestigious brokerage houses and are more likely to be nominated to the All Star Analysts list compiled by *Institutional Investor* magazine. Again, these effects hold after controlling for the effect of accuracy. Third, we find that analysts strategically use downwardly biased forecasts to deliver more consistent if less accurate forecast estimates. Finally, we find that the effect of consistency on analyst forecast informativeness is increasing in the presence of institutional investors (our proxy for sophisticated investors). In contrast, the effect of accuracy on forecast informativeness is increasing in the presence of naive investors. Our results also show that analysts covering firms with more institutional investors lowball to a greater extent (and thus are more consistent), whereas analysts covering firms with fewer institutional investors may not try to maximize consistency and lowball less.

Our study contributes to the literature in several ways. First, although previous studies consider the effect of forecast properties on forecast informativeness and analysts' career prospects, these studies generally use forecast stated accuracy as a proxy for analyst performance (e.g., Gu and Wu (2003)). We extend this literature by shifting the focus from the *size* of the forecast error to its volatility (i.e., the precision of the signal). Although this factor has largely been ignored by previous research, it should be the key metric to measure the usefulness of a forecast if investors are Bayesian. Our results indicate that the volatility of earnings forecast errors can be more important than their magnitude. Thus, our study offers a potentially more powerful measure of analyst performance for studies that examine whether a given factor (e.g., experience, boldness, access to management, etc.) affects forecast informativeness and in turn analysts' careers.

Second, our findings contribute to previous literature on systematic biases. Our results suggest that the key benefit of lowballing lies not in its effect on accuracy but rather in its effect on consistency. This finding should be of interest to investors and securities regulators who wish to understand the causes of biased earnings forecasts. Our results also complement Hong and Kubik (2003). Consistent with their analysis, we find that buy-side analysts' forecasting expertise affects their career. However, whereas Hong and Kubik (2003) focus on forecast stated accuracy, we establish links between analyst career development and forecast consistency. In addition, Hong and Kubik (2003) examine the effect of year-end earnings forecasts whereas we focus on quarterly forecasts; prior literature (e.g., Matsumoto (2002)) establishes that quarterly forecasts are pessimistic on average, whereas Hong and Kubik (2003) rely on the notion that the annual forecasts they consider are optimistic on average.

Finally, we present evidence on the role of investor sophistication in the trade-off between consistency and accuracy, and we offer an explanation for why not all analysts choose to lowball. To the best of our knowledge, these issues have not been investigated by prior studies. We find that an increase in the presence of sophisticated investors increases the effect of consistency on informativeness. In contrast, stated accuracy is more important when the presence of sophisticated investors decreases. This suggests that, even though on average investors may behave in a Bayesian fashion, retail investors may not behave in such a way. We further find that the presence of sophisticated investors increases forecast consistency and the degree of lowballing. Taken together these results suggest the existence of multiple equilibria. In particular, for the majority of analysts our results suggest that the dominant strategy is to collaborate with managers to obtain information that can be used to produce more informative but systematically downward-biased forecasts. Because sophisticated investors can identify and correct for such bias, these investors obtain more informative forecasts. However, when investors are not sophisticated enough to recognize the bias, analysts are penalized for issuing biased forecasts that are inaccurate, even if they are more consistent. These findings are potentially valuable for regulators interested in understanding the trade-offs associated with biased forecasts. In particular, these findings have implications for the evaluation of legislation such as Regulation Fair Disclosure (Reg FD).[3]

The rest of this paper proceeds as follows. Section I discusses our main predication, namely, that consistency in forecast errors is positively related to the informativeness of analysts' forecasts, and presents supportive empirical results. Next, we discuss several implications of our main results. In particular, Section II shows that consistency improves analyst welfare, Section III shows that analysts strategically deliver biased forecasts to increase consistency, and Section IV shows that the effect of consistency on forecast informativeness increases in the presence of institutional investors. Section V presents results of additional tests, and Section VI concludes the paper.

[3] Reg FD mandates that all publicly traded companies disclose material information to all investors at the same time. Empirical results discussed below indicate that this regulation made it more difficult for analysts to lowball to improve their access to management.

I. Consistency, Accuracy, and Informativeness

A. Basic Intuition

Our basic intuition is as follows. Stated accuracy (the absolute forecast error), bias (the signed error), and consistency (the standard deviation of the signed error) are three different properties of earnings forecasts. If users of analyst forecasts can costlessly undo systematic biases in the forecasts, then the biases should be irrelevant. In this case, investors should find a forecast that is a predictable transformation of realized earnings (e.g., realized earnings minus three cents) without a random error more informative than an unbiased forecast with a small unpredictable error, even if the biased forecast has lower stated accuracy. Biased but consistent estimates should therefore have a greater impact on investors' prior expectations, and in turn on prices, than estimates with inconsistent random errors. For example, an analyst who always underestimates realized earnings by three cents will have a greater effect on prices than an analyst who randomly over- or underestimates earnings by three cents, even though the two analysts have the same level of accuracy (i.e., the same average absolute error) and despite the fact that the second analyst is less biased on average.

B. Empirical Design

To test our basic intuition, we regress our measure of forecast informativeness (*Beta*) on our measures of consistency (*Cons*) and stated accuracy (*Accu*), controlling for different relevant variables. We measure our variables for each firm-analyst and obtain one observation per firm-analyst over the entire sample period. We construct *Beta* as the coefficient obtained by regressing abnormal stock returns, *Bhr3d,* on forecast revisions, *Rev*, over all quarters for which analyst i covered firm j,[4] where *Bhr3d* is the buy-and-hold market-adjusted 3-day return around the forecast revision date, and *Rev* is the forecast minus the prior consensus estimate, which is the mean of the last three forecasts made before the current forecast, deflated by the price 2 days before the forecast revision. Specifically, we estimate the following cross-sectional model for each analyst i and firm j:[5]

$$Beta_{i,j} = \alpha_0 + \alpha_1 Cons_{i,j} + \alpha_2 Accu_{i,j} + \alpha_k X^k_{i,j} + e_{i,j}. \tag{1}$$

We construct *Cons* as follows. First, we estimate analyst i's forecast error for firm j in quarter q as the I/B/E/S actual earnings minus the analyst forecast. Second, we calculate the standard deviation of the forecast errors over

[4] A stronger association between abnormal stock returns and forecast revisions indicates more informative forecasts, as reflected in a higher *Beta*. Such association has long been used to evaluate the information content of forecasts (e.g., Givoly and Lakonishok (1979), Stickel (1992), and Park and Stice (2000), among others).

[5] In Section I.H, we consider alternative approaches to the firm-analyst cross-sectional specifications.

all quarters for which analyst i covered firm j. Third, we rank all the analysts that covered firm j based on the standard deviation of forecast errors. Using a relative measure instead of a raw measure mitigates the effects of common shocks that affect all analysts covering a firm at a given point in time and hence helps us focus on the analysts' roles; using the relative measure also facilitates comparison with prior literature, which typically uses a similar approach. To obtain meaningful rankings, we drop firms covered by fewer than five analysts. Finally, we obtain a consistency ranking score following a similar method as that used in the literature to measure accuracy (e.g., Hong, Kubik, and Solomon (2000), Hong and Kubik (2003), Jackson (2005)). Specifically, we use the following formula:

$$Cons = 1 - (\text{rank} - 1)/(\text{number of analysts following the firm} - 1). \quad (2)$$

Prior literature suggests that more accurate analysts have greater ability to move prices (e.g., Stickel (1992), Park and Stice (2000)). To control for the effect of stated accuracy on forecast informativeness, we define *Accu* following Hong and Kubik (2003), except that we use quarterly forecasts instead of yearly forecasts. Specifically, we first calculate the absolute value of analyst i's forecast error for firm j. We then rank all of the analysts that cover firm j in quarter q based on accuracy and calculate the mean of the ranking scores.

The vector X^k comprises k analyst-specific control variables. Our first control is *Boldness* (e.g., Hong, Kubik, and Salomon (2000), Clement and Tse (2005), Ke and Yu (2006)), the absolute value of the distance between analyst i's forecast and the consensus forecast (defined as the average of the other analysts' forecasts).[6] We next control for *Horizon* (e.g., Abarbanell and Bernard (1992), Lim (2001), Jackson (2005), Clement and Tse (2005)), the number of days between the forecast date and the earnings announcement date. We further control for *Experience* (e.g., Lim (2001), Hong and Kubik (2003), Clement and Tse (2005)), the log of the number of quarters the analyst has covered the firm, and *Breadth* (e.g., Hong and Kubik (2003), Clement and Tse (2005)) is the number of firms that the analyst covers in a given year. Because we measure accuracy and consistency in terms of rankings, we also create ranking variables for *Boldness*, *Horizon*, *Experience*, and *Breadth*. Additional controls include *BrokerSize* (e.g., Lim (2001), Jackson (2005), Clement and Tse (2005)), the log of the number of analysts employed by the brokerage house in the year the forecast is issued, and *Cover* (Hong, Kubik, and Salomon (2000)), the log of the number of analysts covering the firm. Each of our control variables is the average value of the variable for a given analyst-firm over the entire period. All standard errors are corrected for heteroskedasticity using the Huber-White method. In addition, standard errors in Model (1) are adjusted for clustering of observations by analyst and firm (clustering by brokerage house instead does not affect our conclusions).

[6] Similar to Ke and Yu (2006), we use the first forecast issued after the earnings announcement for quarter $q-2$ to calculate *Boldness*. Specifically, we calculate the distance from the consensus as: abs(forecast$_{i,j,q}$ – (Σ forecasts$_{j,q}$ – forecast$_{i,j,q}$)/(number of analysts following the firm – 1)).

C. Sample

We obtain actual earnings and analyst forecast data from the I/B/E/S Detail History files over the 1994 to 2006 period. We start our sample in 1994 because forecasts were often delivered to I/B/E/S in batches before that year and thus the date assigned to a forecast in the database may be inaccurate. We focus on quarterly forecasts, and we drop analysts with fewer than eight prior (not necessarily consecutive) quarters of experience from our main tests because we need a sufficiently long time series of forecasts to estimate the volatility of forecast errors, our main variable of interest. For each firm-analyst, we use the last forecast issued by the analyst before the earnings announcement but following the previous earnings announcement. Information on analyst employment is also obtained from the I/B/E/S database. Accounting data come from Compustat quarterly data files and stock return data from CRSP daily files.

D. Empirical Results

Table I provides descriptive statistics for our key variables. As one can see, the distribution of *Beta* is skewed: the mean is 14.36 whereas the median is a materially different 5.80. To correct for this, we form *SqrBeta*, which is the signed square root of the absolute value of *Beta*.[7] The descriptive statistics show that, in contrast to *Beta*, the mean and median of *SqrBeta* are reasonably close to each other (2.13 versus 2.41, respectively). Our main results hold irrespective of which of the two measures we use. In the Internet Appendix, we also examine a correlation matrix for our key variables.[8] The pairwise correlations, including that between *Cons* and *Accu*, are low, with most below 0.30.

Table II presents results of our analysis of the effect of consistency on analyst forecast informativeness. In Column 1, we use *Beta* as the dependent variable. In Column 2, we replicate the specification using *SqrBeta*. Results from both specifications lead to the same conclusion: consistency has a positive effect on forecast informativeness. The z-statistics associated with *Cons* are 11.16 and 8.13 in Columns 1 and 2, respectively. The effect is also economically significant. For example, increasing *Cons* by one standard deviation increases the market reaction to a forecast announcement by approximately 50% of the median *Beta*.[9] Turning to our control variables, we find that *Accu* is also significantly positive. However, the statistical and economic significance of *Accu* are lower than those for *Cons*. For example, the coefficient on *Accu* is only 1.97 whereas that on *Cons* is 8.71 in the first column. By construction, *Accu* and *Cons* have similar means, medians, and standard deviations, so their economic effects can be

[7] For example, if the values of *Beta* are 4 and –2, *SqrBeta* is equal to 2 and –1.4, respectively.

[8] An Internet Appendix for this article is available online in the "Supplements and Datasets" section at http://www.afajof.org/supplements.asp.

[9] Specifically, the increase in market reaction due to a one standard deviation increase in *Cons* = 8.71 (coefficient on *Cons* from Table II) $*$ 0.31 (standard deviation of *Cons* from Table I)/5.80 (median *Beta* from Table I) = 47%.

Table I
Descriptive Statistics

Beta is the coefficient obtained by regressing abnormal stock returns, *Bhr3d,* on forecast revisions, *Rev*, over all quarters for which each analyst covered the firm. *Bhr3d* is the buy-and-hold market-adjusted 3-day return around the forecast revision date, and *Rev* is the forecast minus the prior consensus, which is the mean of the last three forecasts, deflated by the price 2 days before the forecast revision. *SqrBeta* is the signed square root of the absolute value of *Beta*. *Cons* is a measure of consistency for a given firm-analyst based on the rank of the standard deviation of the forecast errors over all quarters for which the analyst covered the firm. *Accu* is a measure of stated accuracy for a given firm-analyst based on the rank of the absolute value of the difference between actual earnings and the forecast over all quarters for which the analyst covered the firm. *Horizon* is the number of days between the forecast date and the earnings announcement date. *Boldness* is the distance from the consensus, measured as the absolute value of the distance between the forecast and the consensus forecast (defined as the average of the other analysts' forecasts). *Experience* is the log of the number of quarters the analyst covered the firm. *Breadth* is the number of firms covered by the analyst in a given quarter. *Horizon*, *Boldness*, *Experience*, and *Breadth* are rank variables similar to *Cons* and *Accu*. *BrokerSize* is the log of the number of analysts employed by the brokerage house in the year when the forecast was issued. *Cover* is the log of the number of analysts following the firm in a given quarter. For all variables, we use one observation for a given firm-analyst over the entire sample period. $N = 38{,}096$.

Variable	Mean	Median	*SD*
Beta	14.36	5.80	36.15
SqrBeta	2.13	2.41	4.26
Cons	0.50	0.50	0.31
Accu	0.50	0.50	0.31
Horizon	0.50	0.50	0.11
Boldness	0.50	0.49	0.10
BrokerSize	3.83	3.98	0.89
Experience	0.52	0.50	0.22
Breadth	0.51	0.51	0.26
Cover	2.58	2.57	0.46

directly assessed by comparing the coefficients. This implies that the effect of accuracy is approximately four times smaller than the effect of consistency. The z-statistic associated with *Accu* is 2.89 versus 11.16 for *Cons* in the first column. The results in Table II also indicate that forecasts issued earlier, forecasts closer to consensus, and forecasts issued by analysts working for larger firms generate larger price reactions. The average variance inflation factor (VIF) for the specifications used in Table II is 1.06, and the highest VIF is 1.14. These low values suggest that multicollinearity is not an issue in our setting and thus the different variables capture different constructs.

E. Are the Results Robust to More Sophisticated Estimation of the Systematic Bias?

So far we assume that bias is predictable and a constant for each firm-analyst. However, in principle, even if biases were predictable, they could be affected by

Table II

The Effect of Consistency on Informativeness

This table reports cross-sectional regressions of the sensitivity of the market response to analyst forecast revisions (*Beta* and *SqrBeta*) on the consistency and accuracy of analysts' forecasts. Variables are defined in Table I. We estimate the regression using OLS. Z-statistics (reported in parentheses) are corrected for heteroskedasticity and are adjusted for clustering of observations by analyst and firm.

Variable	*Beta*	*SqrBeta*
Intercept	−19.69	−1.58
	(−5.33)	(−3.95)
Cons	8.71	0.72
	(11.16)	(8.13)
Accu	1.97	0.45
	(2.89)	(5.76)
Horizon	20.11	2.56
	(9.96)	(10.39)
Boldness	−6.27	−0.54
	(−3.28)	(−2.29)
BrokerSize	0.70	0.15
	(2.52)	(4.09)
Experience	−0.46	0.15
	(−0.47)	(1.19)
Breadth	0.63	0.03
	(0.67)	(0.28)
Cover	7.43	0.57
	(6.27)	(4.65)
N	38,096	38,096
R^2	1.93	1.36

different factors. In this case, we should reformulate our forecast error model in terms of a vector of observable firm and analyst characteristics. Although this more complicated approach does not change our key predictions, it could have implications for our empirical design. If the bias is a firm-analyst constant, we can measure consistency based on the standard deviation of the stated error (i.e., actual earnings minus the forecast). However, if some variables are systematically associated with an analyst's forecast error, then we should strip out the predictable component and estimate consistency as the standard deviation of the "unexpected error" (i.e., the difference between the stated error and the predictable component of the error).

To investigate this possibility, we regress analyst-firm forecast errors on a vector of firm and analyst characteristics. Specifically, we consider the following model:

$$FE_{i,j,q} = b_{0i,j} + \rho FE_{i,j,q-1} + b_m Z^m_{i,j,q} + b_n Q^n + \varepsilon_{i,j,q}, \tag{3}$$

where $FE_{i,j,q}$ and $FE_{i,j,q-1}$ denote the forecast error in the current and previous quarters, respectively. The vector Z^m contains m firm- and analyst-specific control variables, namely, *Size*, *Mkt-to-Bk*, *Lev*, *StdRoa*, *Cover*, *Horizon*, *Boldness*, *BrokerSize*, *Experience*, and *Breadth*[10] where *Size* is the log of the market value of equity at the end of the previous quarter, *Mkt-to-Bk* is the market-to-book ratio at the end of the previous quarter, *Lev* is total liabilities divided by total equity at the end of the previous quarter, *StdRoa* is the log of the standard deviation of the return on assets over the previous eight quarters, and the other firm and analyst controls are as defined previously. To control for seasonality, we include Q^n, a vector of indicator variables for the first, second, and third quarters of the year. In the Internet Appendix, we show that several of these variables are significant. However, although the R^2 is 6.56%, it is essentially zero if we remove the lagged forecast errors from Model (3). In contrast, the R^2 is 19.28% when we regress the forecast errors on a vector of firm-analyst fixed effects (we implicitly use this model when we base consistency on the volatility of the forecast errors). When we combine the fixed effects and the lagged forecast error, the R^2 (20.03%) is close to that of the simpler model that only includes fixed effects. These results suggest that the model using firm-analyst fixed effects provides a simple but reasonable approximation.

Nevertheless, we reestimate specifications similar to those reported in Table II using an alternative definition of *Cons*. In particular, instead of calculating consistency based on the volatility of the difference between a forecast and the realization, we base our calculation on the volatility of the unexpected error. The unexpected error is the residual obtained from estimating Model (3). Our conclusions are not affected: estimates for *Cons* are 8.83 and 0.71 (with z-statistics of 11.10 and 7.84) when *Beta* and *SqrBeta* are the respective dependent variables, whereas the revised estimates for *Accu* are 0.41 and 0.29 (with z-statistics of 0.67 and 3.85). These estimates are very similar to those reported in Table II.

F. Do Stated Accuracy and Consistency Capture the Same Effect?

Our basic intuition is that forecast consistency is important, and more important than stated forecast accuracy. To empirically test this conjecture, we

[10] The literature on the determinants of *quarterly* forecast errors is somewhat limited. We identify two studies pertinent to our setting. Brown and Rozeff (1979) suggest that one-quarter-lagged forecast errors predict current forecast errors. Kross, Ro, and Schroeder (1990) further show that size, forecast horizon, earnings volatility, and press coverage are significant predictors of quarterly forecast errors. We control for *Size*, *Horizon*, and *StdRoa*. Our model does not include a direct measure of press coverage but we control for analyst coverage (*Cover*), a variable shown to be strongly correlated with press coverage (Fang and Peress (2009)). In addition, we use several explanatory variables that have been identified in the *annual* forecast accuracy literature. We control for analyst experience (*Experience*) because Mikhail, Walther, and Willis (1999) find that forecast accuracy is related to forecasting experience. We control for *Boldness* because Hong, Kubik, and Solomon (2000) find that bold but inexperienced analysts are more likely to be fired. Finally, we control for broker size (*BrokerSize*) and number of firms covered by an analyst (*Breadth*) following Clement and Tse (2005).

need to distinguish between these two constructs. We operationalize stated accuracy using a proxy based on the mean absolute forecast error whereas our measure of consistency is based on the standard deviation of the signed forecast errors. We note that, for some distributions (e.g., exponential), the sample variance is a function of the mean, whereas for many other distributions (e.g., normal) the mean and variance are independent. Fortunately, the forecast errors in our sample appear to be reasonably well behaved. Thus, although it is probably not exactly true, assuming normality provides a reasonable approximation that greatly simplifies our analysis.[11] One notable feature of the normal distribution is that the mean and the variance are independent.

Empirically, our measures of consistency and stated accuracy (*Cons* and *Accu*) are moderately correlated (approximately 0.30). Nevertheless, to further ensure that we are not measuring the same analyst forecast characteristic twice, we employ several alternative tests. First, we remove forecasts that are both accurate and consistent (i.e., observations for which *Cons* and *Accu* are both above their median value). This reduces the correlation between *Accu* and *Cons* to –0.13. When we reestimate the specifications reported in Table II, the results (reported in the Internet Appendix) are very similar. Next, we orthogonalize *Accu* with respect to *Cons* and reestimate our regressions. By construction, the correlation between the orthogonalized values of *Cons* and *Accu* is zero in this case. The results (reported in the Internet Appendix) remain unchanged.

In addition, we partition our sample into two subsamples based on the size of the bias using the median of the mean absolute forecast error scaled by price and we reestimate the effect of consistency on forecast informativeness separately for each subsample. We conduct this test for two reasons. First, to better distinguish between the two constructs, we investigate whether consistency is more relevant to investors when stated accuracy is low. Second, to the extent that market participants are more likely to detect and correct for biases that are larger (in absolute value) and hence more obvious, we expect market participants to value consistency rather than stated accuracy in the presence of large systematic deviations from realized earnings. The coefficient on consistency is larger for the large bias subsample than for the small bias subsample (0.72 versus 0.42, respectively). The difference is significant (with a p-value of 0.06). We observe an opposite effect for stated accuracy (0.16 versus 0.63, respectively). The difference is again significant (with a p-value less than 0.001). These results support the notion that *Accu* and *Cons* capture two distinct empirical constructs. The results are also consistent with the idea that investors can undo systematic biases more easily when the magnitude of those biases is larger.

[11] We consider a Shapiro-Wilk test for each analyst-firm distribution of forecast errors. We fail to reject normality for approximately 60% of the cases. We reestimate Model (1) for the subsamples of analyst-firms that pass or fail the normality test. We obtain similar results in both subsamples and our conclusions are not affected in either case.

G. Do Consistency and "Insider Status" Capture the Same Effect?

We interpret the results in Table II as suggesting that investors value consistent forecasts. However, an alternative interpretation is that investors actually value analysts who have inside sources that make the forecast revisions of these analysts particularly informative. In this case, consistency is simply a proxy for access to inside information rather than a desirable property in and of itself. To investigate this possibility, we perform three tests. The first test is predicated on the fact that the market needs time to learn whether an analyst is consistent. If our framework is correct, there should be no correlation between the market's reaction to an analyst's early forecasts and the analyst's future forecast consistency. To test this prediction, we consider a sample of analysts who started to issue forecasts after 1994 and have at least 12 quarters of forecasts in our sample, splitting the data into two periods (analysts' first six observations and subsequent six observations). We then estimate *Beta* and *SqrBeta* using the *first* six quarters and *Cons* using the *following* six quarters.[12] We reestimate our baseline regression (Model (1)) employing this new set of estimates. As expected, results (reported in the Internet Appendix) indicate that *Cons* is statistically insignificant in this alternative specification. In the second test, we include a control for insider status in our baseline regression. Our proxy for insider status, *Bookrunner*, is the number of years a brokerage firm (i.e., an analyst's employer) served as equity offering bookrunner over the entire sample period (according to the SDC Platinum database). Prior literature (e.g., Michaely and Womack (1999), Ke and Yu (2006)) argues that investment banks, and thus analysts working for these banks, may have access to management's private information during the underwriting process. Consistent with this intuition, results (reported in the Internet Appendix) indicate that *Bookrunner* is positively correlated with informativeness but the effect of *Cons* on informativeness is unaffected. Finally, we split our sample based on our proxy for insider status and reestimate our baseline regression for each subsample. The coefficients on *Cons* and *Accu* are not significantly different across the two subsamples. These results do not support the view that consistency is merely a proxy for access to management; rather, they suggest that consistency is an attribute that investors value in and of itself.

H. Is Consistency a Cross-Sectional Analyst-Firm Characteristic?

In our main specifications reported in Table II, we use a cross-sectional model that considers consistency and stated accuracy as firm-analyst fixed characteristics. In this section, we examine whether our results are sensitive to this empirical design choice. To do so, we consider two alternative sets of specifications.

First, we consider a panel specification in which we treat each firm-analyst-quarter as an observation. For analyst i who covers firm j in quarter q, we

[12] Using all observations after the first six quarters or using eight quarters as a cutoff point yields the same conclusions.

estimate the following model:

$$Bhr3d_{i,j,q} = \beta_{0i,j} + \beta_1 Treatment_{i,j,q} + \beta_2 Rev_{i,j,q} + \beta_3 Treatment^{*} Rev_{i,j,q} + \beta_k X^k_{i,j,q} + e_{i,j,q}. \tag{4}$$

The variables in Model (4) are as defined above for Model (1), except that here we calculate *Cons* using the standard deviation of analyst *i*'s forecast errors for firm *j* over the eight quarters before quarter *q* (instead of over the entire sample period as in Model (1)). Similarly, we calculate *Accu* as the average forecast error over the previous eight quarters and the control variables as the average values over the previous eight quarters. We also interact our two treatment variables, *Cons* and *Accu*, with *Rev*. Standard errors in the panel regressions are corrected for clustering of observations by analyst, firm, and year. Our conclusions do not change (the z-statistic associated with the interaction between *Rev* and *Cons* is 2.12, whereas the z-statistic associated with the interaction between *Rev* and *Accu* is 1.27).[13]

Second, we consider an alternative specification that treats consistency and stated accuracy as analyst characteristics instead of analyst-firm characteristics. To do so, we replace *Cons* and *Accu* in our main specification by *ACons* and *AAccu*, which are the average value of *Cons* and *Accu* for a given analyst over the different firms that he or she follows. Our conclusions are unaffected. The results (reported in the Internet Appendix) indicate that the z-statistic associated with *ACons* is 6.33 (3.11 for *AAccu*) and the coefficient on *ACons* is approximately three times larger than that on *AAccu*.[14]

Overall, our results are not affected by the decision of whether to treat consistency and stated accuracy as fixed analyst characteristics, fixed analyst-firm characteristics, or time-varying characteristics. Because it is difficult to determine if one approach dominates the others based on the regression results alone, we favor the analyst-firm characteristics specifications over the analyst characteristics specifications because the results presented below in Section III indicate that institutional ownership, a firm characteristic, plays an important role in our setting. The choice between the pure cross-sectional approach and the panel approach, however, is more difficult to make on a priori grounds. Prior literature (e.g., Mikhail, Walther, and Willis (2004)) suggests that skill has a fixed component. Admittedly, a fixed component does not preclude some variation over time but our variables tend to be slow moving and some of them are overlapping. Although we correct our z-statistics for the clustering of observations by analyst, firm, and year, our z-statistics may remain affected. One

[13] The z-statistics increase to 3.51 for the interaction between *Rev* and *Cons* and 1.53 for the interaction between *Rev* and *Accu* when we orthogonalize *Cons* and *Accu*. Our panel specification includes an interaction between *Rev* and our treatment variables but does not include interactions with the X control variables. This approach minimizes multicollinearity. In a robustness test, we split the sample based on the median value of *Cons* and regress *Bhr*3*d* on the treatment and control variables; we then test the equality of the coefficients on *Rev*. The difference is statistically significant with a p-value of 0.04.

[14] The magnitude of the coefficient is comparable when we use *SqrBeta* as the dependent variable.

advantage of the pure cross-sectional approach is that it mitigates concerns regarding the effects of serial correlation. In contrast, an advantage of the panel specification is that we do not have to estimate *Beta*, a parameter that is likely to be estimated with some noise. As noted above, all models yield similar results and thus we choose to focus on the cross-sectional approach, taking at least some comfort from the fact that our conclusions are not affected by our design choice.

II. Consistency and Analyst Welfare

A. Predictions

Results in Section I indicate that forecasts issued by more consistent analysts are more informative. Thus, greater consistency should improve analysts' welfare. We investigate this hypothesis by considering two dimensions of analyst welfare: employer status and industry recognition. Among the various dimensions of career development, one of the key metrics is the prestige of one's employer. For example, Hong and Kubik (2003) note that being an analyst at a high-status brokerage house is typically regarded as better (e.g., higher compensation and prestige) than being an analyst at a low-status brokerage house. These authors also suggest that brokerage houses want analysts who are influential on the buy-side. If brokerage houses have a demand for analysts that issue informative forecasts, they should seek to retain consistent (and hence, influential) analysts. We therefore expect that the likelihood of being demoted to a less prestigious brokerage house is lower for more consistent analysts. We also consider the effect of consistency on the likelihood that an analyst will be nominated to the All Star Analysts list compiled by *Institutional Investor* magazine. The list is prepared based on the votes of more than 3,000 individuals, representing approximately 90% of the 100 largest U.S. equity managers, as well as more than 300 other key money management firms (Leone and Wu (2007)). If forecasts issued by more consistent analysts are more informative, then those analysts should receive more votes. We therefore expect that consistent analysts are more likely to be nominated to the All Star Analysts list than analysts who are less consistent.

B. Empirical Design

To examine the hypotheses above, we estimate the following two models:

$$Demo_{i,t} = \gamma_0 + \gamma_1 Cons_{i,t} + \gamma_2 Accu_{i,t} + \gamma_k X^m_{i,t}, \tag{5}$$

$$AllStar_{i,t} = \delta_0 + \delta_1 Cons_{i,t} + \delta_2 Accu_{i,t} + \delta_k X^m_{i,t}. \tag{6}$$

In Model (5), *Demo* is an indicator variable that equals one if analyst i is demoted in the following year (i.e., between July 1 of year t and June 30 of year $t+1$), and zero otherwise. We initially code an analyst as demoted if he or she starts working for a different brokerage house that is smaller, in terms of the number of analysts employed, compared to his or her previous brokerage house (Mikhail, Walther, and Willis (1999)). We also consider two alternatives to *Demo* that are similar to variables used by Hong and Kubik (2003). Specifically, *DemoLarge* equals one if the analyst leaves a "large" brokerage house to join a "small" house, and zero otherwise, where the classification between small and large houses is based on whether the brokerage house employs more than 25 analysts, and *DemoTop10* equals one if an analyst leaves a "prestigious" brokerage house to join a "nonprestigious" house, and zero otherwise, where the classification between prestigious and nonprestigious is based on whether the brokerage house is among the 10 largest houses. To measure professional recognition (Model (6)), we compute *AllStar*, an indicator variable that takes the value of one if the analyst is on *Institutional Investor* magazine's All Star list, and zero otherwise. We collect *Institutional Investor's* rankings of All-American Research Team analysts for the years 1994 to 2006. The All-American rankings are published each year in the October issue of the magazine. Given that our dependent variables are binary, we use a probit specification to estimate Models (5) and (6).

We use two main treatment variables, *Cons* and *Accu*, as defined previously. Our test also includes X^m, a vector of control variables (*Boldness*, *Breadth*, *Cover*, and *Experience*) that are the analogs of the variables we use in the specifications to investigate informativeness. We calculate *Experience* as the log of the number of years that analyst i has appeared in the I/B/E/S earnings forecast database as of year t. We calculate *Cons*, *Accu*, *Boldness, Breadth,* and *Cover* for each analyst-firm-quarter using a rolling eight-quarter window, and then take the average of these measures over all firms covered by analyst i in year t. We also include (but do not tabulate) broker and year fixed effects. We correct our standard errors for clustering of observations by employer and year.[15]

C. Empirical Results

Our results, presented in Table III, are consistent with our hypothesis: consistency significantly improves analyst welfare. In Column 1, *Cons* is significantly negative with a z-statistic of –4.24. In contrast, stated accuracy does not seem to affect the likelihood of demotion (*Accu* is insignificantly different from zero with a z-statistic of –0.29) after controlling for consistency, but *Accu* is significant if we exclude *Cons* (with a z-statistic of –1.98). The marginal effect of *Cons* on the probability that an analyst leaves a large brokerage house to join a small house is –3.09% (holding all other variables at their mean), whereas the

[15] Our specification is close to that used by Hong and Kubik (2006), but we also include *Boldness* to be consistent with our prior specifications as well as with Hong, Kubik, and Salomon (2000). Omitting this variable leads to similar conclusions. We also exclude a measure of optimism used by Hong and Kubik (2003) to be consistent with our prior specifications. Including this additional variable does not change our conclusions.

Table III

The Effect of Consistency and Stated Accuracy on Analyst Demotions

This table reports panel regressions of demotion to a smaller broker (*Demo*) in Column 1 and of nomination to the All Star list (*AllStar*) in Column 2 on the consistency and accuracy of analysts' forecasts. *Demo* is an indicator variable equal to one if analyst i is demoted in the following year (i.e., between July 1 of year t and June 30 of year $t + 1$) and zero otherwise. An analyst is assumed to be demoted if he or she starts working for a different brokerage house that is smaller, in terms of the number of analysts employed, than the previous brokerage house. *AllStar* is an indicator variable that takes the value of one if the analyst is named on *Institutional Investor* magazine's All Star list, and zero otherwise. Other variables are defined in Table I except here we calculate *Cons*, *Accu*, *Boldness, Breadth,* and *Cover* for each analyst-firm-quarter using a rolling eight-quarter window and then take the average over all firms covered by analyst i in year t (from July 1 of year $t - 1$ to June 30 of year t). *Experience* is the log of the number of years analyst i has appeared in the I/B/E/S earnings forecast database as of year t. We also include but do not tabulate broker and year fixed effects. We estimate the regression using a probit specification. z-statistics (reported in parentheses) are corrected for heteroskedasticity and are adjusted for clustering of observations by broker and year.

Variable	*Demo*	*AllStar*
Cons	−0.30	0.60
	(−4.24)	(4.89)
Accu	−0.03	0.54
	(−0.29)	(4.03)
Boldness	−0.13	−0.35
	(−0.48)	(−0.95)
Breadth	−0.01	0.02
	(−2.94)	(4.74)
Cover	0.14	0.15
	(2.94)	(2.36)
Experience	−0.01	0.42
	(−0.26)	(5.46)
N	15,561	11,985
Pseudo R^2	7.57	23.18

marginal effect of *Accu* is only –0.28%. Our results (reported in the Internet Appendix) continue to hold if we consider *DemoLarge* and *DemoTop10*.

We next consider the effects of consistency and stated accuracy on the probability of becoming an All Star analyst. The results, reported in Column 2 of Table III, indicate that *Cons* and *Accu* are positively associated with the likelihood of becoming an All Star analyst. Both variables are highly significant (with z-statistics equal to 4.89 and 4.03, respectively) but the coefficient on *Cons* is approximately 10% larger (0.60 versus 0.54). The marginal effect of *Cons* on the probability of becoming an All Star analyst is 15.13% (holding all other variables at their mean), whereas the marginal effect of *Accu* is 13.40%. These results are consistent with our hypothesis.[16] Turning to the other control variables, we note that *Breadth* and *Cover* are negatively and positively correlated with the likelihood of demotion (Column 1), whereas *Breadth*, *Cover*, and

[16] Our results are qualitatively similar when we orthogonalize *Cons* and *Accu*.

Experience are positively correlated with the likelihood of becoming an All Star analyst (Column 2).

For completeness, we rerun our analysis using promotions instead of demotions. Toward this end, we form three measures of promotion similar to *Demo*, *DemoLarge*, and *DemoTop10*. For example, *Prom* takes the value of one if the analyst is promoted to a large brokerage house, and zero otherwise. Although previous studies suggest that stated forecast accuracy is negatively correlated with the likelihood of demotion, to the best of our knowledge only one study, Hong and Kubik (2003), documents a link between stated forecast accuracy and the likelihood of promotion, and that study focuses on annual rather than quarterly forecasts; studies focusing on quarterly forecasts do not report such a link. Accordingly, we do not make a prediction regarding the effect of consistency on promotion. Results (reported in the Internet Appendix) indicate that, other than experience, which has a negative effect on the likelihood of promotion, none of the variables are significant.

III. Consistency and Strategic Behavior

A. Predictions

We next consider the implications that our results in Section I may have for the strategic behavior of analysts, that is, we examine whether analysts strategically bias their forecasts to increase their consistency. If forecast consistency is more important than stated forecast accuracy, analysts may trade off stated accuracy and consistency. One possibility is that analysts intentionally induce a downward bias in their forecasts to curry favor with managers so as to gain better access to information. The literature suggests that managers derive benefits from beating analyst earnings forecasts, and reward analysts who facilitate this pattern with information (e.g., Brown and Caylor (2005)). Prior research also finds that managers are in a position to help analysts form accurate expectations of future earnings realizations. For example, Bowen, Davis, and Matsumoto (2002) find that analyst stated forecast accuracy increases when firms host conference calls in conjunction with their earnings announcements. These findings suggest that company-provided information improves an analyst's ability to accurately forecast a firm's earnings. Previous research (e.g., Lim (2001), Libby et al. (2008)) further suggests that analysts are willing to accommodate managers' demands so as to curry favor. Chen and Matsumoto (2006) find that analysts who issue more favorable recommendations experience a greater increase in their relative stated forecast accuracy than analysts who issue less favorable recommendations. These results are consistent with the view that analysts use biased reports to obtain better access to managers' private information.[17] In this case, the difference between better-informed analysts' reported forecasts and realized earnings is not due

[17] Solomon and Frank (2003) report that analysts who issue adverse earnings forecasts are often punished in subtle ways by firm management.

to a random component but rather to a predictable bias that is strategically introduced by the analysts. Such a bias should lead to greater consistency in the errors of analysts' reported forecasts. Hence, we hypothesize that forecast error consistency should be greater for analysts who lowball than for analysts who do not.

B. Empirical Design

To investigate this hypothesis, we consider the effect of lowballing on consistency. To do so, we estimate the following model for analyst i who covers firm j in quarter q:

$$Cons_{i,j,q} = \alpha_0 + \alpha_1 Lowball_{i,t,q} + \alpha_k X^k_{i,j,q} + e_{i,j,q}. \tag{7}$$

As in Model (4), *Cons* is based on the previous eight quarters (using a cross-sectional approach similar to the one used in Model (1) yields similar conclusions). We construct *Lowball* as follows. First, we calculate the forecast errors for analyst i covering firm j in quarter q as I/B/E/S actual earnings minus the analyst's forecast. If this error is positive, we say that the analyst has lowballed in the quarter; conversely, if the error is negative, we say that the analyst has highballed. Forecasts that are perfectly accurate are unclassified. Second, we calculate the frequency of lowballing as the difference between the number of lowballed and highballed forecasts. Third, we rank all the analysts following the same firm over the previous eight quarters. Finally, we obtain the ranking score following the same method as employed to calculate *Cons*.

For completeness, we also consider the effect of lowballing on stated accuracy. However, in this case we are unable to predict the direction of the effect because there is a potential trade-off between the bias and the magnitude of the forecast error's random component. On the one hand, introducing a bias will mechanically lead to an increase in the expected error (in absolute value). In other words, absent any strategic game, the absolute value of the forecast error should be greater for analysts who consistently lowball because they increase the systematic component of their forecast error. On the other hand, these analysts may be able to access better information from management by offering forecasts that are easy to meet or beat. In this case analysts' private expectations will be more accurate, reducing the random component of their forecast errors. The net effect of these two forces is ex ante ambiguous and hence is an empirical question. To resolve this uncertainty, we estimate the following model for analyst i, firm j, and quarter q:

$$Accu_{i,j,q} = \alpha_0 + \alpha_1 Lowball_{i,t,q} + \alpha_k X^k_{i,j,q} + e_{i,j,q}. \tag{8}$$

Our control variables in Models (7) and (8)—X^k (*Boldness, Horizon, Experience, Breadth*, *BrokerSize,* and *Cover*)—are similar to those used in our informativeness tests but here we calculate all variables using a rolling eight-quarter window. Standard errors are corrected for clustering of observations by analyst, firm, and year.

Table IV

The Effect of Lowballing on Consistency

This table reports panel regressions of forecast consistency (*Cons*) in Column 1 and forecast accuracy (*Accu*) in Column 2 on the level of lowballing. *Lowball* is a rank variable based on the difference between the number of pessimistic and optimistic forecasts scaled by the total number of forecasts. Other variables are defined in Table I, except here we calculate all variables using a rolling eight-quarter window before the current quarter. We estimate the regression using OLS. z-statistics (reported in parentheses) are corrected for heteroskedasticity and are adjusted for clustering of observations by analyst, firm, and year.

Variable	*Cons*	*Accu*
Intercept	0.56	0.77
	(28.02)	(54.25)
Lowball	0.20	−0.11
	(24.84)	(−5.32)
Horizon	−0.27	−0.35
	(−16.77)	(−17.01)
Boldness	−0.25	−0.30
	(−22.61)	(−19.90)
BrokerSize	0.03	0.03
	(6.23)	(6.51)
Experience	0.04	0.06
	(5.12)	(5.98)
Breadth	−0.02	−0.04
	(−2.69)	(−5.00)
Cover	−0.01	−0.01
	(−2.66)	(−1.72)
N	286,104	286,104
R^2	6.43	4.47

C. Empirical Results

The mean and median of the fraction of lowballed forecasts are 0.32 and 0.38, respectively, indicating that analysts have a tendency to lowball.[18] The results reported in Column 1 of Table IV indicate that analysts who lowball deliver more consistent forecast errors. The statistical significance of this result is high with a z-statistic of 24.84.[19] The effect is also economically significant: increasing *Lowball* by one standard deviation increases *Cons* by approximately 12% of its mean. This result is consistent with our hypothesis that analysts use biases strategically to maximize their consistency. Turning to the control variables, forecasts closer to the announcement date, forecasts closer to the consensus, and forecasts made by larger brokerage firms, more experienced analysts, or analysts covering fewer firms are more consistent. In contrast to the results in Column 1, Column 2 of Table IV indicates that *Lowball* is negatively associated with *Accu*, with a z-statistic of –5.32. However, the economic magnitude is

[18] The fraction of the lowballed forecasts is the number of pessimistic forecasts minus the number optimistic forecasts, divided by the number of forecasts.

[19] Without adjusting for heteroskedasticity or clustering, the t-statistic is equal to 104.63.

approximately one-half of the positive effect on consistency. This finding suggests that analysts can strategically trade off stated accuracy for consistency by introducing a systematic and predictable negative bias into their forecasts.

IV. Consistency and Investor Sophistication

A. Predictions

In this section, we consider the effect of investor sophistication on our results. Our basic intuition is that, if investors can undo systematic biases in forecasts, they will extract more information from biased but consistent forecasts than from unbiased forecasts that are relatively more accurate but inconsistent. In line with this intuition, in Section I we find that consistency is more important when the bias is large than when it is small. In other words, consistency is more important for forecast informativeness when the bias is more salient and thus when investors are more likely to pay attention to the bias. We expect to find a similar difference when we consider investor sophistication.

Prior literature suggests that institutional investors are more sophisticated than retail investors. For example, Hand (1990) finds that institutional investors are less fixated on reported earnings and are more able to identify systematic biases in earnings caused by debt-equity swaps. Boehmer and Kelley (2009) report that stocks with greater institutional ownership are priced more efficiently.[20] To the extent that institutional investors are better at processing information, they should be better at unraveling systematic biases in analysts' forecasts and hence should benefit from consistency to a greater extent than individual investors. We therefore hypothesize that the effect of consistency on informativeness should be greater when the proportion of institutional investors is higher.

B. Empirical Design

To test this hypothesis, we split our overall sample into two subsamples based on the percentage of institutional investor ownership and we reestimate Model (1) separately for each subsample. We calculate the ratio of shares owned by an institution scaled by the shares outstanding for each firm-quarter, and we define *Inst* as the average proportion of institutional ownership over the entire period for which an analyst covers the firm. We then form two subsamples based on *Inst* using the median value for the overall firm-analyst sample as a cutoff point. As we discuss in Section I.B, our informativeness test only relies on cross-sectional variation, that is, it does not rely on time-series changes. We do not use a panel regression here to avoid introducing a high level of multicollinearity caused by a three-way interaction between institutional ownership,

[20] See also Bozcuk and Lasfer (2005), Campbell, Ramadorai, and Schwartz (2009), and Puckett and Yan (2011).

Table V
The Effect of Consistency on Informativeness Conditional on the Level of Investor Sophistication

This table reports cross-sectional regressions of the sensitivity of the market response to analyst forecast revisions (*SqrBeta*) on the consistency and accuracy of analysts' forecasts, conditional on the fraction of institutional investors in the shareholding of the firm. *Inst* is the average percentage institutional ownership of a given firm over the entire sample period. We split the sample based on the median *Inst* within our firm-analyst sample. The variables are defined in Table I. We estimate the regression using OLS. z-statistics (reported in parentheses) are corrected for heteroskedasticity and are adjusted for clustering of observations by analyst and firm.

Variable	*SqrBeta* Low *Inst*	*SqrBeta* High *Inst*
Intercept	−2.23	−0.80
	(−4.38)	(−1.43)
Cons	0.40	1.03
	(3.08)	(7.85)
Accu	0.53	0.37
	(4.91)	(3.30)
Horizon	2.16	3.18
	(7.17)	(9.26)
Boldness	−0.44	−0.62
	(−1.42)	(−1.87)
BrokerSize	0.15	0.12
	(3.48)	(2.51)
Experience	0.13	0.39
	(0.82)	(2.19)
Breadth	−0.02	0.11
	(−0.15)	(0.66)
Cover	0.83	0.22
	(5.40)	(1.21)
N	18,747	18,856
R^2	1.68	1.39

consistency, and forecast revisions.[21] Highly multicollinear specifications can generate unreliable estimates.

C. Empirical Results

Results are reported in Table V. We tabulate the results for the low institutional ownership subsample in Column 1 and for the high institutional ownership subsample in Column 2. In line with our expectations, the effect of consistency on forecast informativeness is greater when investors are more sophisticated. The magnitude of the coefficient on *Cons* is approximately two and half times larger in the subsample in which the proportion of sophisticated

[21] Such specifications would include *Rev, Inst, Cons, Rev*Inst, Rev*Cons, Inst*Cons,* and *Rev*Inst*Cons*. These seven variables are highly correlated by construction. For example, the correlation between *Rev* and *Rev*Inst* is 0.97.

investors is higher. The statistical significance is also larger, with the z-statistic more than twice as large in the high *Inst* subsample than in the low *Inst* subsample. A χ^2 test indicates that the two coefficients associated with *Cons* (1.03 and 0.40) are statistically significantly different from each other (with a p-value less than 0.001). In contrast, the magnitude and statistical significance of the coefficients on *Accu* are approximately 50% larger in the low *Inst* subsample than in the high *Inst* subsample. The two coefficients associated with *Accu* (0.37 and 0.53) are not statistically different from each other when we use *SqrBeta* as the dependent variable; they become significantly different from each other (at the 10% level) if we use *Beta* as the dependent variable (our conclusions regarding consistency are not affected if *Beta* is the dependent variable).

We also examine the effect of lowballing on consistency (Model (7)) and stated accuracy (Model (8)) conditional on the level of institutional ownership. We split our overall sample into high and low institutional ownership subsamples and repeat our analysis for each subsample separately. Results (reported in the Internet Appendix) indicate that the effect of lowballing on consistency and stated accuracy is more pronounced in the sophisticated investor subsample. Specifically, the coefficient on *LowBall* is larger (i.e., more positive) in the high institutional ownership subsample than in the low institutional ownership subsample when consistency is the dependent variable, whereas we find the opposite relation when stated accuracy is the dependent variable (the coefficient on *LowBall* is less negative in the low institutional ownership subsample than in the high institutional ownership subsample). The difference in coefficients associated with *LowBall* is statistically significant in both comparisons (the p-value is less than 0.001 in both cases) across the two subsamples.

Results in this section suggest that institutional investors value consistency more than retail investors, and accuracy less than retail investors. The results also suggest that the importance of strategic biases in forecasts is greater when the proportion of institutional investors is higher.

V. Additional Results

A. *"Squaring the Circle"*

Our previous results suggest that, on average, lowballing increases consistency but decreases stated accuracy and that the effect of consistency on forecast informativeness is greater when the proportion of institutional investors is higher. To triangulate our results, we regress forecast informativeness (*Beta* and *SqrBeta*) on the fraction of lowballed forecasts and our usual control variables in Model (1). Results (in the Internet Appendix) indicate that analysts who lowball issue more informative forecasts. Our results also indicate that these effects are more pronounced in the presence of institutional investors. It is thus natural to expect that analysts covering firms with more institutional investors should lowball to a greater extent and therefore be more consistent.

To investigate this hypothesis, we regress *LB* and *StdErr* on *Inst* and a vector of observable firm and analyst characteristics. We define *LB* as the fraction of lowballed forecasts over the previous eight quarters for analyst i covering firm j. We control for *StdErr,* the log of the standard deviation of the forecast errors over the previous eight quarters for analyst i covering firm j.[22] *StdErr* is thus an inverse measure of forecast error consistency. We include *Inst*, the average percentage of institutional ownership over the previous eight quarters for firm j. Our control variables include the vector of analyst characteristics used in Model (7) plus firm size, market-to-book ratio, leverage, and earnings volatility. We calculate all variables using a rolling eight-quarter window. We expect the coefficient on *LB* to be positive and that on *StdErr* to be negative.

Results reported in Table VI are consistent with our prediction. *Inst* is significantly positive in Column 1 but negative in Column 2 (the z-statistics are 9.87 and –5.24, respectively). These findings and those in Section IV above provide a rational explanation for why not all analysts try to maximize their consistency. Our basic intuition is that, if investors can undo systematic biases in forecasts, they will prefer consistent but biased forecasts to unbiased but inconsistent forecasts; if investors cannot easily unravel forecast biases, however, they will prefer more transparent and accurate forecasts. Because maximizing consistency implies, on average, a reduction in the stated accuracy of the forecasts, this suggests that some costs are traded off against the benefits. Naturally, analysts facing investors who are more likely to unravel biases are more likely to emphasize consistency over stated accuracy. Thus, lowballing and consistency maximization are more likely to take place when the proportion of institutional ownership is larger than when it smaller. Our empirical results support this interpretation. However, even though the mix of sophisticated and naïve investors changes across firms, the two types of investors are present in the shareholding of every firm. In general analysts are likely to cater to the dominant clientele, but some analysts may decide to cater to the niche market represented by the other type because the competition from other analysts is lower. Thus, some analysts may choose to cater to naïve investors by maximizing stated accuracy instead of consistency even when sophisticated investors are the dominant clientele (the opposite is true for firms dominated by naïve investors).

B. Regulation Fair Disclosure

As we state briefly in the introduction, our results have implications for regulators. The Securities and Exchange Commission (SEC) implemented Reg FD in October 2000. Reg FD mandates that all publicly traded companies disclose material information to all investors at the same time. One explanation for the results reported in Table IV is that analysts lowball to obtain more information from managers. If this is true, the benefits associated with lowballing are likely

[22] Note that *LB* and *StdErr* are not ranked to be consistent with *Inst*, which is measured at the firm level.

Table VI

The Effect of Ownership on Lowballing

This table reports panel regressions of the fraction of lowballing (*LB*) in Column 1 and the consistency of forecast errors (*StdErr*) in Column 2 on the fraction of institutional investors in the shareholding of the firm. *LB* is the difference between the number of pessimistic and optimistic forecasts scaled by the total number of forecasts. *StdErr* is the log of the standard deviation of the forecast error for analyst i and firm j over the eight quarters before quarter q. *Inst* is the average percentage institutional ownership of firm j. *Size* is the log of the market value of equity. *Mkt-to-Bk* is the ratio of the market value of equity to the book value of equity. *Lev* is the debt-to-equity ratio. *StdRoa* is the log of the standard deviation of the firm's return on assets over the eight quarters before quarter q. Other variables are defined in Table IV. We calculate all variables using a rolling eight-quarter window. We estimate the regression using OLS. z-statistics (reported in parentheses) are corrected for heteroskedasticity and are adjusted for clustering of observations by analyst, firm, and year.

Variable	*LB*	*StdErr*
Intercept	−0.53	−2.28
	(−7.34)	(−12.68)
Inst	0.39	−0.68
	(9.87)	(−5.24)
Horizon	−0.10	0.13
	(−5.24)	(2.49)
Boldness	−0.10	0.31
	(−6.21)	(9.15)
BrokerSize	0.03	−0.01
	(6.30)	(−0.56)
Experience	0.02	−0.10
	(2.35)	(−3.64)
Breadth	0.02	0.02
	(2.09)	(0.48)
Cover	0.10	0.02
	(4.89)	(0.36)
Size	0.01	−0.18
	(0.80)	(−8.24)
Mkt-to-Book	0.01	−0.15
	(6.31)	(−19.32)
Lev	−0.00	0.10
	(−2.35)	(14.05)
StdRoa	−0.04	0.48
	(−6.88)	(24.53)
N	268,489	268,311
R^2	6.27	38.31

to have decreased after Reg FD went into effect. To investigate this conjecture, we regress *LB*, *StdErr*, and *MeanErr* on *FD* (an indicator variable that takes the value of one if the quarter is post-Reg FD, and zero otherwise), a yearly time trend, and our control variables (similar to those used in Table VI), where *LB* and *StdErr* are the previously defined measures of lowballing and consistency, and *MeanErr* is a measure of stated accuracy constructed as the log of the mean absolute forecast error over the previous eight quarters for analyst i covering firm j. Given that our variables are estimated over eight quarters, we exclude the quarters in 2000, 2001, and 2002 from these tests because the variables estimated in these years are both pre- and post-Reg FD.

Results (reported in the Internet Appendix) indicate that both the tendency to lowball and consistency have declined in the post-FD period (after controlling for a secular trend). Interestingly, the results also indicate that stated accuracy has decreased in the post-Reg FD period. These results suggest that the regulation has curtailed selective disclosure, at least to some extent, and in turn decreased analyst lowballing activity, which has resulted in less consistent forecasts. These findings are in line with Bailey et al. (2003), who show that analyst disagreement and the difficulty in making earnings forecasts have increased since the implementation of Reg FD.[23]

VI. Conclusion

Our study focuses on the role of consistency in analyst forecasts. We find that, on average, analysts with a lower standard deviation of forecast errors have greater ability to move prices. The effect is both economically and statistically significant. Consistent with prior literature, we find that increasing stated forecast accuracy also increases analysts' ability to move prices, but the effect is both economically and statistically less significant than when we consider the effect of consistency. These results have implications for financial analysts' careers: more consistent analysts are less likely to be demoted to less prestigious brokerage houses (this effect subsumes the effect of stated forecast accuracy) and are more likely to become All Stars. When we relate our main findings to the nature of the systematic bias in analyst forecasts, we find that analysts who lowball are more consistent but less accurate. Finally, we find that the effect of consistency on informativeness is greater when the proportion of institutional investors is higher. In contrast, the effect of accuracy on informativeness is greater when institutional investors are less present. Logically, analysts covering firms with more institutional investors lowball to a greater extent and hence are more consistent.

Our results are likely to be of interest to academics, investors, and regulators alike. First, by shifting the focus of forecast informativeness from stated accuracy to consistency, we examine whether investors behave in a Bayesian manner. By doing so, we provide a potentially more powerful measure of analyst performance for studies that examine how financial analysts affect price formation and labor market outcomes. Second, by showing that the key benefit of lowballing lies not in stated accuracy but rather in consistency, we shed light on the causes of biased earnings forecasts, a question of interest to investors as well as securities regulators. Finally, by considering the role of investor sophistication in the trade-off between consistency and stated accuracy, an issue that to our knowledge has not been previously investigated by the literature,

[23] We also estimated our baseline model (similar to the one reported in Table II) pre- and post-Reg FD. We find that *Cons* is significant in both periods and is not statistically different across periods. This suggests that Reg FD affected the supply of consistent forecasts but not the demand.

our analysis is potentially valuable for regulators who wish to understand the trade-offs involved in biased forecasts. In particular, these findings can be useful in the evaluation of legislation such as Reg FD.

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