Review of Accounting Studies (2024) 29:276–326
https://doi.org/10.1007/s11142-022-09724-x

Check for updates

Overprecise forecasts

Yi Dong[1] · Xuejiao Liu[2] · Gerald J. Lobo[3] · Chenkai Ni[4]

Accepted: 18 August 2022 / Published online: 4 October 2022

Abstract
We examine the properties of overprecise forecasts, i.e., forecasts with more digits after the decimal than the mode of forecasts issued by all analysts following a given firm in a given year. Contrary to conventional wisdom, we find that overprecise forecasts are less accurate than peer forecasts. The lower accuracy is related to inexperienced analysts, who tend to overweight their models and produce more specific, yet less accurate forecasts. Additional analyses indicate that analysts issue fewer overprecise forecasts as they gain experience and that experience mitigates the negative association between forecast overprecision and forecast accuracy. Forecast overprecision is also positively associated with forecast boldness and brokerage house prestige, two proxies for analyst overconfidence in their model outputs. We further document that the capital market partially sees through this inaccuracy, as stock prices react less to overprecise forecast revisions. By revealing a novel behavioral bias of sell-side analysts, our study challenges the view that form precision signals greater accuracy and informativeness.

Keywords Overprecision · Analyst forecasts · Forecast accuracy · Behavioral bias

JEL classification G40 · M40 · M41

"*He who knows best knows how little he knows.*" – Thomas Jefferson.

✉ Xuejiao Liu
xuejiaoliu@uibe.edu.cn

✉ Chenkai Ni
nichenkai@fudan.edu.cn

1 Institute of Accounting and Finance, Shanghai University of Finance and Economics, No. 777 Guoding Road, Shanghai 200433, China

2 School of Business, University of International Business and Economics, No. 10 Huixin East Street, Beijing 100029, China

3 C. T. Bauer College of Business, University of Houston, Houston, TX 77204-6021, USA

4 School of Management, Fudan University, No. 670 Guoshun Road, Shanghai 200433, China

1 Introduction

It is widely contended that form precision is positively associated with accuracy and informativeness. Assuming costly information and rational agents, Grossman and Stiglitz (1980) model an information equilibrium wherein agents stop processing additional information when the expected benefit and the expected cost break even. An implication of this model is that prices, quotes, and forecasts with lower resolution (e.g., integers) are less informative. Prior research documents evidence consistent with this prediction for limit orders (Kuo et al. 2015), IPO issue prices (Bradley et al. 2004), and analyst forecasts (Herrmann and Thomas 2005). In this study, we incorporate individuals' behavioral bias and address the following question: Are overly precise sell-side analysts' forecasts more accurate and informative?

The above literature overlooks individuals' overprecision, a behavioral bias that has long been documented in the psychology literature. Overprecision refers to the excessive faith that one knows the truth (Radzevick and Moore 2011; Moore et al. 2016). It has been observed across a wide spectrum of professions (e.g., clinical psychologists, physicians and nurses, engineers, lawyers, negotiators, and newsvendors).[1] Regarding the financial market, Odean (1998) illustrates that investors' overprecision in stock valuation can lead to intensified differences of opinion and, therefore, to excessive trading volume. Daniel et al. (1998) propose that overprecision induces investors to overreact to their private signals and underreact to public signals. Relatedly, Adebambo and Yan (2018) show that investors who overestimate the precision of their information and underestimate risk exhibit stock overpricing. However, these studies indirectly *infer* individuals' overprecision through observed outcomes such as trading volume and share prices.

In this study, we directly examine overprecision using the unique setting of sell-side analysts' earnings forecasts. This setting is suitable for addressing overprecision for two reasons. First, analyst forecasts are made by individuals under uncertainty, a condition under which behavioral bias is most likely to manifest. Second, realized firm performance (reported earnings per share) can be observed ex post, thus facilitating an objective evaluation of forecast accuracy. Importantly, whether the behavioral bias of overprecision exists in the context of analyst forecasts remains an empirical question because the prior literature shows that forecasts of less form precision (e.g., rounded forecasts) exhibit lower accuracy (Herrmann and Thomas 2005). Given the above reasons, we focus on the fraction of analyst forecasts that are more specific than peer forecasts (i.e., forecasts with more digits after the decimal than the mode number of digits) to examine the implications of overprecision for analyst forecast properties. We refer to these forecasts as *Specific* forecasts.[2] We first document the existence of *Specific* forecasts, then explore their causes and assess their capital market consequences.

[1] Related studies include Oskamp (1965), Kidd (1970), Wagenaar and Keren (1986), Neale and Bazerman (1990), Baumann et al. (1991), Ren and Croson (2013), and Li et al. (2017). See Moore et al. (2016) for a recent review.

[2] In this study, we use "overprecise forecasts" and "specific forecasts" interchangeably.

Using analyst forecasts from the Unadjusted Detail file in I/B/E/S over the period 1986–2015, we first study the distribution of the number of digits after the decimal in analyst forecasts. We find that during our sample period approximately 5% of analyst forecasts end with more digits than the mode number of digits of all analyst forecasts issued for the same firm-year (i.e., they are *Specific* forecasts). Furthermore, 35% of analysts in our sample exhibit such behavior at least once during their career, and 19% of firm-years include *Specific* forecasts, indicating the prevalence of such forecasts. Importantly, despite their form precision, *Specific* forecasts are less accurate than non-*Specific* forecasts (i.e., forecasts that contain the same or a smaller number of digits after the decimal than the mode). This significant, negative association with forecast accuracy is evident in the pooled sample and in 18 of the 30 years in our 1986–2015 sample period. Collectively, these results indicate that forecasts that are more precise in form are less accurate in realization than peer forecasts.

We rule out several alternative explanations for the negative association between overprecision and forecast accuracy. First, it is possible that the additional digits of *Specific* forecasts are the source of the inaccuracy. We address this concern by truncating/rounding the additional digits of *Specific* forecasts so that they have the same number of digits as the mode. We find that truncated/rounded *Specific* forecasts still have lower accuracy than non-*Specific* forecasts. Second, we examine whether analysts employ fixed templates or models which result in *Specific* forecasts. We control for brokerage house fixed effects and continue to find that *Specific* forecasts are less accurate. Third, management forecasts may affect the form and the value of analyst forecasts (Bamber et al. 2010). To address this concern, we exclude firm-years with management forecasts and show that our inference continues to hold. Lastly, Fang and Hope (2021) find that analyst teams issue forecasts that are more accurate. We exclude team forecasts and again establish the robustness of our finding.

Next, we explore reasons for analysts' overprecision. We argue that analysts' overprecision represents a Dunning-Kruger effect, i.e., less competent analysts are unaware of the boundary of their knowledge (Kruger and Dunning 1999). The literature proposes inexperience and lack of knowledge as two critical factors that induce overprecision (Kruger and Dunning 1999; Burson et al. 2006; Moore et al. 2016). Practical views echo this theory, suggesting that "new analysts" and "naïve analysts" are likely to be too certain about their forecasts or recommendations (Valentine 2010).

Empirically, we find that analysts become less likely to issue *Specific* forecasts as they gain experience. Whereas 6.81% of forecasts made by analysts with two years of experience are *Specific* forecasts, the percentage declines significantly with analyst experience, to a sample minimum of 1.24% for forecasts issued by analysts with 23 years of general experience.[3] Further, using the last period's forecast accuracy to proxy for an analyst's competency, we find that less competent analysts are more likely to issue *Specific* forecasts. These results highlight the Dunning-Kruger effect in an analyst forecast setting.

[3] We do not include analysts with less than two years of experience in our sample because our empirical analyses require analysts' performance information in the last year.

Our findings are in line with practitioners' suggestion not to "pretend to have a level of precision that doesn't exist. … It (the precision) conveys the image you rely too much on your financial model output, without thinking through the big picture" (Valentine 2010). If *Specific* forecasts relate to analysts' overconfidence in their forecasting models, these analysts are less likely to adjust their forecasts by referring to peers' consensus. We therefore expect *Specific* forecasts to be bold rather than to herd towards the consensus. Consistent with this expectation, we find a positive and significant association between forecast overprecision and forecast boldness. Further, we show that *Specific* forecasts are more likely to be issued by analysts affiliated with more prestigious brokerages, plausibly another source of overconfidence in their financial model output (Clement and Tse 2005).

The analyses of the underlying reasons for overprecision yield additional implications for forecast accuracy. First, because overprecision represents inexperienced analysts' behavioral bias, *Specific* forecasts issued by experienced analysts are more likely to result from additional information processing and less likely to be inaccurate. The empirical findings support this reasoning by showing that the negative association between overprecision and accuracy is weaker for experienced analysts. Second, the literature indicates that both forecast boldness and brokerage prestige positively impact forecast accuracy (Clement 1999; Clement and Tse 2005). We reason that boldness and brokerage prestige are less likely to improve forecast accuracy when they result in *Specific* forecasts. Empirically, we find that the positive association between forecast boldness or brokerage prestige and forecast accuracy is weaker for *Specific* forecasts.

Lastly, we examine the capital market implications of *Specific* forecasts. We find that investors adjust for the inherent inaccuracy of *Specific* forecasts, as evidenced by the weaker stock price reactions to these forecast revisions during the three-day event window centered on the forecast revision date. Such a discounting effect is not surprising, considering that forecast overprecision is an easily observable signal. However, investors' adjustment proves incomplete, as stock prices during a delayed period (i.e., [2, 20]) continue to discount *Specific* forecasts. This result suggests the existence of market inefficiency and highlights the practical importance of viewing forecast overprecision as a public signal of forecast inaccuracy.

Our study makes several important contributions to the literature. First, we challenge the conventional wisdom that form precision implies greater accuracy and informativeness. Such a view has its theoretical foundation in Grossman and Stiglitz (1980), who derive an information equilibrium wherein more precise prices/quotes/forecasts result from additional information-processing, and is empirically supported by the evidence in Bradley et al. (2004), Herrmann and Thomas (2005), and Kuo et al. (2015). Unlike those studies, we show that the implicit assumption of agent rationality in trading off costs and benefits to gather and process information overlooks an important feature of individuals, i.e., their vulnerability to overprecision. We document the existence of overprecision in sell-side analysts' forecasts and explore its causes and consequences. Together with the above studies, our study helps form a more complete picture of the link between form precision and real accuracy.

Second, our study contributes to the strand of research on overprecision. Moore et al. (2016) categorize overconfidence into three subcategories: (1) overestimation – thinking that you are better than you actually are; (2) overplacement – exaggerating the extent to which you are better than others; and (3) overprecision – having excessive faith that you know the truth. Of these three distinct forms of overconfidence, Moore et al. (2016) summarize that "overprecision in judgment is both the most durable and the least understood form of overconfidence." Utilizing an analyst forecast setting, we document that inexperience and incompetence are two critical factors associated with overprecision. This evidence highlights a Dunning-Kruger effect, in which individuals who lack knowledge are also unaware of the boundaries of their knowledge. Our context of analyst forecasts overcomes two major challenges encountered by studies of individuals' overprecision (Radzevick and Moore 2011): that statement accuracy is usually untestable, and that the degree of overprecision is difficult to measure.

Lastly, we add to the literature on analysts' cognitive biases. Prior literature shows that analysts tend to overreact to positive information and underreact to negative information (Easterwood and Nutt 1999), that uncertainty amplifies analysts' overreaction (De Bondt and Thaler 1990), and that analysts become overconfident due to past success (Hilary and Menzly 2006). Our study reveals a novel form of analysts' cognitive bias – overprecision. We find that a proportion of analysts are overconfident in their model outputs and produce forecasts that are precise in form, yet low in accuracy.[4]

The remainder of our study is structured as follows. Section 2 develops testable hypotheses, Section 3 describes sample formation and variable construction, Section 4 discusses empirical findings, and Section 5 concludes the study.

2 Hypotheses development

The psychology literature defines overprecision as individuals' excessive faith that they know the truth (see Moore et al. (2016) for a review). This behavioral bias has been documented in a variety of settings where individuals make decisions under uncertainty, such as clinical psychologists (Oskamp 1965), physicians and nurses (Baumann et al. 1991), engineers (Kidd 1970), lawyers (Wagenaar and Keren 1986), negotiators (Neale and Bazerman 1990), and newsvendors (Ren and Croson 2013; Li et al. 2017).

[4] A recent stream of accounting literature explores individuals' narcissism – the attribute of excessive self-focus and self-entitlement – and finds that it significantly affects corporate financial reporting quality (Ham et al. 2017), investment and firm performance (Ham et al. 2018), and disclosure choices of non-GAAP earnings (Abdel-Meguid et al. 2021). Notably, the overprecision bias differs from individual narcissism, although both have been shown to be associated with the Dunning-Kruger effect (Christopher et al. 2021). Several features are unique to the narcissism attribute, such as the willingness to exploit others or engage in unethical behavior to serve one's own interests (Ham et al. 2017; Abdel-Meguid et al. 2021), and failures or unwillingness to take feedback (Ham et al. 2017, 2018).

Extending the logic to sell-side analysts in the financial market, we posit that forecasting earnings per share (EPS) for listed firms is essentially a decision made by individuals under uncertainty. Prior literature shows that analysts exhibit cognitive biases when forecasting under uncertainty (De Bondt and Thaler 1990; Easterwood and Nutt 1999). Therefore, we argue that sell-side analysts could also be subject to the cognitive bias of overprecision. That is, when they issue forecasts with a level of precision that exceeds most peer forecasts, their forecasts are less likely to be accurate. In addition to the supportive academic evidence on overprecision, practitioners' recommendations also confirm this notion. In his book *Best Practices for Equity Research Analysts: Essentials for Buy-side and Sell-side Analysts*, Valentine (2010) has the following suggestion for young analysts: "Don't pretend to have a level of precision that doesn't exist. … It (the precision) conveys the image you rely too much on your financial model output, without thinking through the big picture."

Nonetheless, there is tension regarding whether the overprecision bias extends to sell-side analysts. The existing literature shows that forecasts of lower form precision (e.g., rounded forecasts) exhibit lower accuracy (Herrmann and Thomas 2005). Therefore, the implications of overprecision of analyst forecasts remain an empirical question. Building upon the above discussion, we state our first hypothesis as follows:

Hypothesis 1: *Forecasts ending with more digits after the decimal than the mode of forecasts for the same firm-year (*i.e.*, Specific forecasts) are less accurate than peer forecasts (*i.e.*, non-Specific forecasts).*

Our second hypothesis concerns factors relating to sell-side analysts' overprecision. The psychology literature proposes knowledge and experience as moderators that can deter overprecision (Kruger and Dunning 1999; Burson et al. 2006). These factors are linked to the influential theory of the Dunning-Kruger effect – that unskilled individuals are unaware of the boundaries of their knowledge. In their work, Kruger and Dunning (1999) state that unskilled individuals suffer from a dual burden because "[not] only do these people reach erroneous conclusions and make unfortunate choices, but their incompetence robs them of the metacognitive ability to realize it" (p. 1121).

The Dunning-Kruger effect is echoed by practitioners' view that inexperience and incompetence can lead to analysts' overprecision in forecasts or recommendations. Valentine (2010) states that "too often new analysts think they know the answer because they are overly confident in their models. Their mind can't comprehend where they could be wrong" (p. 246). In the same book, an interviewed senior analyst expresses the following opinion: "I believe analysts who express a high degree of confidence in any recommendation or forecasts are usually naïve" (p. 49). Combining practitioners' insights and academic evidence, we hypothesize the following:

Hypothesis 2: *Less experienced (competent) analysts are more likely to issue Specific forecasts than are more experienced (competent) analysts.*

Our third hypothesis considers the implications of *Specific* forecasts for the capital market. We examine whether investors can rationally weight the information contained in overprecise forecasts in terms of its relation to forecast accuracy. Under rational expectations, investor responses to forecast revisions will be weaker when updated forecasts are *Specific*. This view is supported by Abarbanell et al. (1995), who illustrate that investor responses to forecasts increase in expected forecast accuracy. More specifically, investors can extract and utilize public information such as analyst-specific and forecast-specific characteristics that can affect forecast accuracy. Building upon this framework, investor responses to forecast revisions are likely to be a function of these characteristics. Gleason and Lee (2003), Park and Stice (2000), and Stickel (1992) report consistent empirical findings. Following this line of reasoning, we present the following hypothesis:

Hypothesis 3: *The stock market reacts less to revisions of Specific forecasts than to revisions of non-Specific forecasts.*

We note that investors may not weight the information rationally in terms of its ability to predict future earnings. Existing studies also show that investors fail to fully understand the implications of accounting numbers when forecasting future earnings (Sloan 1996; Xie 2001). More pertinent to our context, Clement and Tse (2003) examine stock market reactions around forecast revisions and find that the implied weights on characteristics that are predictive of future earnings in the market reaction equation differ significantly from the weights on the same characteristics in the earnings forecast equation. Therefore, whether investors rationally incorporate public signals when responding to forecast revisions is open to empirical testing.

3 Sample, variables, and empirical specifications

3.1 Data and sample formation

Our sample comprises I/B/E/S forecasts of U.S.-listed firms' annual earnings per share (EPS) from 1986 to 2015. Herrmann and Thomas (2005) employ a sample beginning in 1985. As we require information on lagged analyst/firm attributes in our empirical analyses, we exclude the initial year of data in 1985. Following Herrmann and Thomas (2005) and Dechow and You (2012), we employ the I/B/E/S Unadjusted Detail file to ensure that our analyses are not affected by the retroactive stock split rounding effect, also described in Baber and Kang (2002) and Payne and Thomas (2003).[5] For an analyst i covering firm j, we retain analyst i's last forecast of

[5] The adjusted data in I/B/E/S presents analyst forecasts and realized earnings on a split-adjusted basis, rounded to the nearest penny. For example, for firm i and fiscal year t, assume that raw values of an analyst forecast and the realized earnings per share (EPS) are \$1.00 and \$0.99, respectively. After a 4-for-1 stock split in the next year, the adjusted data in I/B/E/S will report \$0.25 per share for both the analyst forecast and the realized EPS. Baber and Kang (2002) and Payne and Thomas (2003) both suggest that researchers use the unadjusted detail file to overcome the retroactive stock split issue. Subsequent analyst forecast research, e.g., Herrmann and Thomas (2005) and Dechow and You (2012), follow this suggestion, as does our study.

firm j's annual earnings per share (EPS) for fiscal year t. Further, following Clement and Tse (2005), we retain only forecasts issued within the [−365, −30] window prior to the fiscal year-end. To obtain a meaningful comparison of analysts who provide forecasts for the same firm-year, we exclude firm-years followed by fewer than three analysts. We deflate forecast revisions and forecast errors by a firm's security price two days prior to the forecast revision date using stock price data from the CRSP database. Following Clement and Tse (2005), we eliminate observations with price-deflated analyst forecast errors greater than 0.40 or lower than −0.40.[6] We also eliminate observations without sufficient data to compute the variables used in our regression analyses. We winsorize all continuous variables at their 1st and 99th percentiles. For the transformed variables described below, we winsorize their corresponding raw values. These procedures yield a sample of 389,467 analyst-firm-year observations for 5760 (12,508) unique firms (analysts).

3.2 Variable construction and empirical specifications

Our empirical focus is on analyst forecasts that are more specific than forecasts issued by peer analysts. We identify these forecasts by creating an indicator, $SpecificMode_{ijt}$, that equals 1 if analyst i's forecast for firm j in fiscal year t has more digits after the decimal than the mode number of digits of peer analysts' forecasts for the same firm-year, and 0 otherwise. We term these forecasts *Specific* forecasts. The *mode*, by definition, refers to the value that appears most frequently in a set of data or observations. In our context, it equals the number of forecast digits that appears most frequently in analysts' last forecasts for the annual EPS of a firm-year. The *mode* thus intuitively and methodologically allows us to capture the "normal" number of digits of analyst forecasts. For example, there are 20 analysts who followed Monsanto Company in fiscal year 2015. Appendix 1. shows the 20 analysts' latest forecasts during [−365, −30] prior to the firm's fiscal year end. Of these analysts, 17 forecast with two digits (i.e., the mode) and three forecast with three digits (i.e., *Specific* forecasts). The variable *SpecificMode* equals 1 for the three analyst forecasts with three digits, indicating their specificity compared with peer analysts, and 0 for the other forecasts. Monsanto Company eventually reported EPS ending with two digits.[7] Appendix 1 discusses *Specific* forecasts in greater detail using two illustrative examples.

Our main analyses investigate the association between forecast specificity and forecast accuracy using the following model:

[6] This filtering step also addresses a potential concern that overprecise (or *Specific*) forecasts may be outliers. If overprecise forecasts present greater forecast errors, such a sample truncation will work against finding a negative association between form specificity and forecast accuracy.

[7] This example also suggests a potential concern that *Specific* forecasts are less accurate because of the additional digits. In subsequent analyses, we rule out this concern by truncating the additional digits in *Specific* forecasts so that these forecasts have the same number of digits as the mode forecast. Our inferences are unchanged when we use this alternative approach. Furthermore, the majority (65.07%) of *Specific* forecasts in our sample have no more than two digits, further alleviating this concern.

$$
\begin{aligned}
ForAccuracy_{ijt} = \beta_0 &+ \beta_1 SpecificMode_{ijt} + \beta_2 LagForAccuracy_{ijt-1} + \beta_3 Bold_{ijt} \\
&+ \beta_4 BrokerSize_{it} + \beta_5 GenExp_{it} + \beta_6 FirmExp_{ijt} + \beta_7 Industries_{it} \\
&+ \beta_8 Companies_{it} + \beta_9 ForFrequency_{ijt} + \beta_{11} DaysElapsed_{ijt} \\
&+ \beta_{11} ForHorizon_{ijt} + \beta_{10} FyeDis_{ijt} + \varepsilon_{ijt}
\end{aligned} \tag{1}
$$

where the dependent variable $ForAccuracy_{ijt}$ measures analyst i's forecasting performance for firm j in year t relative to peer analysts' forecasting performance for firm j in year t. Consistent with Clement and Tse (2005), we measure *ForAccuracy* using the following transformation:

$$
For\,Accuracy_{ijt} = \frac{AFE\max_{jt} - AFE_{ijt}}{AFE\max_{jt} - AFE\min_{jt}} \tag{2}
$$

In Eq. (2), AFE_{ijt} is the absolute forecast error of analyst i's forecast for firm j in year t. We compute forecast error as firm j's year t earnings minus analyst i's forecast of firm j's year t earnings. $AFE\max_{jt}$ and $AFE\min_{jt}$ are the maximum and minimum absolute forecast errors, respectively, of analysts that issue forecasts for firm j in year t. The transformed variable *ForAccuracy* increases in analyst forecast accuracy.

We include several covariates, identified in the existing literature, that could affect analyst forecast accuracy. These variables include last year's forecast accuracy (*LagForAccuracy*), where *ForAccuracy* is defined as in Eq. (2); forecast boldness (*Bold*), defined as the distance between analyst i's forecast for firm j in year t from the pre-revision (year-to-date) consensus forecast for firm j in year t (Clement and Tse 2005)[8]; brokerage firm size (*BrokerSize*), defined as an indicator that equals 1 if analyst i is employed by a brokerage firm in the top decile in terms of the number of analysts employed during year t, and 0 otherwise; general experience (*GenExp*), defined as the number of years since the analyst issued a forecast for any firm in the sample; firm-specific experience (*FirmExp*), defined as the number of years since the analyst began following the firm; number of industries followed (*Industries*), defined as the number of distinct two-digit SIC industries followed by the analyst during year t; number of companies followed (*Companies*), defined as the number of distinct companies followed by the analyst during year t; forecast frequency (*ForFrequency*), defined as the number of forecasts issued by the analyst for the firm during year t; number of days elapsed (*DaysElapsed*), defined as the number of days elapsed since the last forecast for the firm; forecast horizon (*ForHorizon*), defined as the number of days between the analyst's forecast date and the firm's fiscal-year-end date; and distance of the analyst forecast from the consensus forecast (*FyeDis*), defined as the absolute difference between the analyst's forecast and the fiscal-year-end consensus forecast for the firm.

[8] Also following Clement and Tse (2005), we compute the year-to-date consensus using forecasts issued within 90 days of the forecast revision. Untabulated results show that our inferences are unchanged without this restriction.

Consistent with Clement and Tse (2005), we transform these control variables (except for the indicator variable *BrokerSize*) as follows to facilitate comparison of regression model coefficients:

$$Char_{ijt} = \frac{Char\_Raw_{ijt} - Char\_Raw\text{min}_{jt}}{Char\_Raw\text{max}_{jt} - Char\_Raw\text{min}_{jt}} \tag{3}$$

where $Char\_Raw_{ijt}$ denotes the raw value of one of the control variables and $Char\_Raw\text{max}_{jt}$ and $Char\_Raw\text{min}_{jt}$ are the original maximum and the original minimum of the variable for firm *j* in year *t*, respectively. A higher value of $Char_{ijt}$ indicates that analyst *i* scores higher on that characteristic when issuing a forecast for firm *j* in year *t*. Therefore, the transformed variable preserves the relative distance of the raw variable. We present detailed variable definitions in Appendix 2.

3.3 Summary statistics and correlations of key variables

Table 1 Panel A reports descriptive statistics of the key variables (unscaled) employed in our analyses. *SpecificMode* averages 0.049, indicating that 4.9% of analyst forecasts end with more digits than the mode. Aggregated to the firm-year level, 19% of firm-years include *Specific* forecasts. Aggregated to the analyst level, 35% of analysts have provided *Specific* forecasts during their careers.[9] These statistics indicate that overprecision of analyst forecasts is an economically important phenomenon.

An average analyst has roughly 11 years of general experience and five years of experience covering the firm of interest. Further, an analyst on average follows around 19 companies from four distinct industries. The descriptive statistics are comparable to those reported in the existing literature (e.g., Clement and Tse 2005; Dechow and You 2012). Because we scale and demean the variables before performing our regression analyses, we also show, in Panel B, the summary statistics of the transformed variables. The transformation procedures in Eq. (3) follow those of Clement and Tse (2005) and Herrmann and Thomas (2005). Panel C reports Pearson correlations among the variables used in our main regression analyses.

4 Empirical results

4.1 Specificity and accuracy

4.1.1 Baseline findings

In this section, we analyze whether *Specific* forecasts are more accurate than non-*Specific* forecasts by estimating the association between analyst forecast accuracy and *SpecificMode*, i.e., an indicator that equals 1 if a forecast ends with more digits

[9] These statistics are computed using samples with aggregated firm-year-level or analyst-level observations, and are not tabulated in Table 1.

Table 1 Summary statistics of empirical variables

Panel A: Distribution of raw (unscaled) forecast and analyst characteristics					
Variables	Mean	Std. Dev.	25%	Median	75%
SpecificMode	0.049	0.215	0	0	0
Bold	0.145	0.237	0.023	0.062	0.157
BrokerSize	0.611	0.487	0	1	1
GenExp	11.175	7.155	5	10	15
FirmExp	5.021	3.379	3	4	6
Industries	4.200	2.985	2	4	5
Companies	18.558	13.033	12	16	22
ForFrequency	4.896	2.531	3	4	6
DaysElapsed	25.958	40.987	1	6	29
ForHorizon	86.953	52.858	57	68	87
FyeDis	0.132	0.226	0.018	0.051	0.138
Panel B: Distribution of scaled forecast and analyst characteristics					
Variables	Mean	Std. Dev.	25%	Median	75%
ForAccuracy	0.644	0.343	0.401	0.757	0.947
LagForAccuracy	0.633	0.344	0.381	0.736	0.945
Bold	0.375	0.337	0.077	0.278	0.620
GenExp	0.496	0.364	0.150	0.467	0.857
FirmExp	0.402	0.372	0.000	0.333	0.714
Industries	0.369	0.344	0.000	0.286	0.600
Companies	0.405	0.322	0.135	0.333	0.625
ForFrequency	0.432	0.338	0.167	0.400	0.667
DaysElapsed	0.287	0.385	0.000	0.063	0.514
ForHorizon	0.340	0.368	0.037	0.167	0.597
FyeDis	0.337	0.341	0.046	0.215	0.540

Table 1 (continued)

Panel C: Correlations among key empirical variables

Variables		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
SpecificMode	(1)	1											
ForAccuracy	(2)	**−0.018**	1										
LagForAccuracy	(3)	**−0.005**	**0.090**	1									
Bold	(4)	0.000	**−0.115**	**−0.046**	1								
BrokerSize	(5)	**0.014**	**0.027**	**0.028**	**0.029**	1							
GenExp	(6)	**0.008**	**0.014**	**0.004**	0.003	**0.036**	1						
FirmExp	(7)	**0.004**	**0.007**	*−0.003*	**0.013**	**0.007**	**0.404**	1					
Industries	(8)	**−0.032**	**−0.026**	**−0.035**	**0.007**	**−0.064**	**0.085**	**0.055**	1				
Companies	(9)	**−0.033**	**−0.007**	**−0.024**	**0.005**	**0.058**	**0.168**	**0.110**	**0.503**	1			
ForFrequency	(10)	−0.001	**0.095**	**0.017**	**−0.045**	**0.060**	**−0.032**	*0.003*	**−0.007**	**0.019**	1		
DaysElapsed	(11)	**−0.018**	**−0.113**	**−0.035**	**0.177**	**0.033**	**−0.014**	**0.008**	**0.029**	**0.022**	**−0.087**	1	
ForHorizon	(12)	**−0.006**	**−0.255**	**−0.045**	**0.211**	**−0.017**	*−0.003*	**−0.004**	**0.009**	**−0.022**	**−0.304**	**0.347**	1
FyeDis	(13)	**0.025**	**−0.363**	**−0.076**	**0.401**	**−0.024**	**−0.008**	0.001	**0.019**	**0.005**	**−0.055**	**0.089**	**0.171**

This table reports summary statistics and correlations of forecast and analyst characteristics. Panels A and B report the summary statistics for raw values and scaled values, respectively. Panel C reports the Pearson correlations among the variables used in our main regression analyses. The correlation coefficients are highlighted in **bold** (*italic*) if they are significant at the 1% (5%) level. Our sample period covers fiscal years from 1986 to 2015. Detailed variable definitions are provided in Appendix 2 after the decimal than the mode number of digits derived from all forecasts for the firm-year, and 0 otherwise. We estimate Eq. (1) by regressing forecast accuracy (*ForAccuracy*) on our variable of interest (*SpecificMode*) and a list of covariates known to affect analyst forecast accuracy.

Our panel stacks 30 years of data for the period 1986–2015. We report ordinary least squares (OLS) regression results in Table 2. To address potential correlations in error terms, we adjust standard errors of the coefficient estimates using two-way clustering by analyst and year (Petersen 2009; Luo and Nagarajan 2015).[10] We find a negative and statistically significant coefficient on *SpecificMode* (−0.018, $t = -4.86$) in Column (1). This result indicates that, despite their form precision, *Specific* forecasts are less accurate than non-*Specific* forecasts.

The coefficients on the control variables are largely in line with the results of prior literature (e.g., Clement 1999; Hong et al. 2000; Clement and Tse 2003, 2005). For example, consistent with Clement and Tse (2003, 2005), analysts who have better historical forecasting performance continue to have it in the current period, as reflected by the positive and significant coefficient on *LagForAccuracy* (0.056, $t = 10.62$). The significantly positive coefficient on *Bold* (0.076, $t = 9.51$) implies that bold forecasts are more accurate than herding forecasts. Analysts employed by larger brokerage houses (*BrokerSize*) provide more accurate forecasts (0.008, $t = 3.68$). An analyst's general experience in the profession (*GenExp*) improves the forecast accuracy (0.010, $t = 3.84$). However, firm-specific experience (*FirmExp*) does not exhibit a significant relation with forecast accuracy (0.003, $t = 1.28$). Portfolio complexity adversely affects forecasting performance, as indicated by the negative and significant coefficient on the number of industries followed (*Industries*) (−0.014, $t = -5.55$). Frequent forecasters perform better, as reflected by the positive and significant coefficient on *ForFrequency* (0.017, $t = 5.55$). A longer period between the forecast revision date and the preceding forecast date for the same firm-year by any other analyst is associated with less accuracy, as revealed by the negative and significant coefficient on *DaysElapsed* (−0.022, $t = -7.63$). The negative coefficient on *ForHorizon* (−0.182, $t = -15.58$) suggests that forecasts more distant from the fiscal year-end have lower accuracy. Finally, the coefficient on *FyeDis* is negative and significant (−0.354, $t = -36.57$), suggesting that analysts who deviate more from the year-end consensus have less accurate forecasts.[11]

Because the explanatory variables are transformed to range from 0 to 1, the regression coefficients allow us to assess the relative economic significance of these variables in explaining the variation in forecast accuracy (Clement and Tse 2005; Herrmann and

[10] We also impose the following two alternative treatments on standard errors: (1) we perform Fama-Macbeth regression analyses and compute standard errors using the Newey-West procedure by taking a one-year lag; and (2) we re-estimate the OLS regression with standard errors clustered by firm and year. Under both specifications, we find a negative and significant association between *SpecificMode* and *Accuracy*.

[11] The correlations of two pairs of control variables are relatively higher. The correlation between analyst general experience (*GenExp*) and firm experience (*FirmExp*) is 0.404, and the correlation between industry coverage (*Industries*) and firm coverage (*Companies*) is 0.503. To alleviate concerns about multicollinearity, we employ the approach in Clement and Tse (2005) and alternately drop each variable of a correlated pair (*FirmExp* or *GenExp*; *Industries* or *Companies*). Untabulated analyses consistently find negative and significant coefficients on *SpecificMode*.

Thomas 2005). The effect of overprecision on forecast accuracy is greater than that of analysts' brokerage house size, general experience, and the number of industries followed, but less than that of forecast boldness.

Lastly, because loss firms are more difficult to value and exhibit larger forecast errors (Clement and Tse 2005), we perform corroborative analyses to examine whether our main findings are driven by forecasts for loss firms. We split our sample into subsamples of non-negative forecasts and negative forecasts. To avoid introducing a retroactive bias (i.e., analysts did not have information on realized EPS when making forecasts), we use forecasted losses instead of realized losses to identify observations for loss firms. Repeating our main analyses using the two subsamples, we have two primary findings. First, loss forecasts are fewer than non-loss forecasts, accounting for 8.23% and 91.77% of our total observations, respectively. Second, the negative association between *SpecificMode* and *ForAccuracy* is statistically significant in the subsample of non-negative forecasts (−0.018, $t=-4.77$ in Column (2)), but statistically insignificant in the subsample of negative forecasts (−0.015, $t=-1.38$ in Column (3)). Overall, these empirical findings support Hypothesis 1 – that seemingly precise *Specific* forecasts are less accurate – and suggest that this result is not attributable to forecasts for loss firms.[12]

4.1.2 Persistence in the association between specificity and accuracy

Our main results so far rely on the pooled sample covering 1986 to 2015. To examine whether these pooled sample results are driven by a few outlier years, we estimate Eq. (1) for each annual cross-section. Because each analyst-firm appears only once in each yearly subsample, such an analysis also mitigates potential concerns about correlated residual terms in the panel data.

Yearly regression results appear in Table 3. For brevity, we report only the coefficient on *SpecificMode* and its corresponding t-statistic, the adjusted R^2, and the number of observations in each annual regression. We find that the negative association between *SpecificMode* and *ForAccuracy* holds in 27 of the 30 years, the exceptions being 1988 (0.015, $t=1.52$), 1992 (0.002, $t=0.19$), and 1995 (0.004, $t=0.39$). Further, the negative coefficients are statistically significant, at least at the 10% level, in 18 of the 27 years.

Although we observe some variation in the magnitude of the coefficients over time, the yearly estimation results mitigate our concern that the pooled sample results may be driven by a small number of outlier years. We conclude that the observed negative relationship between forecast specificity and forecast accuracy persists during our sample period.[13]

[12] While our main hypothesis concerns the link between overprecision and forecast accuracy, we acknowledge that analyst forecasts overwhelmingly exhibit positive biases. In unreported analyses, we replace *ForAccuracy* in Equation (1) with a signed measure of forecast bias. We find a positive association between overprecision and analysts' forecast bias.

[13] The negative association between overprecision and forecast accuracy appears more pronounced in the second half of our sample period. We argue that such a time-series pattern could result from the first half of the sample period exhibiting more rounding. In untabulated analyses, we find that the percentage of rounded forecasts declines during our sample period. With this trend, the earlier period is more likely to exhibit the confounding effect that some *Specific* forecasts may be identified due to peer analysts' rounding choices. This force can weaken our hypothesized effect of inaccuracy for *Specific* forecasts because rounding has been shown to be associated with higher uncertainty and lower accuracy (Herrmann and Thomas 2005; Dechow and You 2012). In Section 4.5.1, we discuss in greater detail the declining trend of rounding and its relationship to our findings.

Table 2 Forecast specificity and forecast accuracy

Variables	Dependent Variable = *ForAccuracy*		
	Full Sample	Non-negative Forecasts	Negative Forecasts
	(1)	(2)	(3)
SpecificMode	−0.018***	−0.018***	−0.015
	(−4.86)	(−4.77)	(−1.38)
LagForAccuracy	0.056***	0.056***	0.055***
	(10.62)	(11.26)	(4.86)
Bold	0.076***	0.079***	0.031***
	(9.51)	(9.87)	(2.82)
BrokerSize	0.008***	0.008***	0.005
	(3.68)	(3.62)	(1.24)
GenExp	0.010***	0.010***	0.011
	(3.84)	(3.72)	(1.49)
FirmExp	0.003	0.003	0.001
	(1.28)	(1.28)	(0.10)
Industries	−0.014***	−0.014***	−0.016**
	(−5.55)	(−4.76)	(−2.53)
Companies	−0.005	−0.006*	0.000
	(−1.54)	(−1.74)	(0.04)
ForFrequency	0.017***	0.017***	0.012*
	(5.55)	(5.62)	(1.69)
DaysElapsed	−0.022***	−0.023***	−0.006
	(−7.63)	(−8.55)	(−1.04)
ForHorizon	−0.182***	−0.184***	−0.159***
	(−15.58)	(−15.82)	(−9.80)
FyeDis	−0.354***	−0.359***	−0.298***
	(−36.57)	(−36.02)	(−22.49)
Adjusted R^2	0.179	0.184	0.132
Observations	389,467	357,410	32,057

This table presents the association between analyst forecast form specificity and forecast accuracy. Our sample period covers fiscal years from 1986 to 2015. We perform ordinary least squared (OLS) regressions. We estimate the regression for the full sample (column "Full Sample") and for the subsamples with non-negative forecasts (column "Non-negative Forecasts") and negative forecasts (column "Negative Forecasts"). The dependent variable *ForAccuracy* measures analyst *i*'s forecasting performance relative to that of peer analysts following the same firm-year. *SpecificMode* is an indicator that equals 1 if an analyst's EPS forecast for a firm-year has more digits than the most frequent number of digits (mode) of analysts' forecasts for the same firm-year, and 0 otherwise. *t*-statistics (in parentheses) are based on robust standard errors that are clustered by analyst and year. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels using two-tailed student *t*-tests, respectively. Detailed variable definitions are provided in Appendix 2

Table 3 Forecast specificity and forecast accuracy – evidence by year

	Dependent Variable = *ForAccuracy*			
Fiscal Year	*SpecificMode*	(t-stat.)	Adjusted R^2	Observations
1986	−0.013	(−1.17)	0.178	6821
1987	−0.005	(−0.43)	0.136	6965
1988	0.015	(1.52)	0.192	7578
1989	−0.025**	(−2.42)	0.181	7093
1990	−0.027**	(−2.18)	0.152	6593
1991	−0.001	(−0.16)	0.177	9884
1992	0.002	(0.19)	0.192	10,552
1993	−0.025***	(−2.62)	0.195	10,636
1994	−0.007	(−0.67)	0.154	10,180
1995	0.004	(0.39)	0.191	10,578
1996	−0.045***	(−3.47)	0.190	11,034
1997	−0.033**	(−2.22)	0.159	10,859
1998	−0.044**	(−2.45)	0.159	11,390
1999	−0.069***	(−3.49)	0.160	11,367
2000	−0.010	(−0.45)	0.207	10,117
2001	−0.045*	(−1.76)	0.230	10,742
2002	−0.048*	(−1.70)	0.183	11,040
2003	−0.021	(−0.76)	0.189	11,959
2004	−0.042	(−1.54)	0.168	14,133
2005	−0.017	(−0.61)	0.186	15,410
2006	−0.018	(−0.72)	0.238	16,183
2007	−0.054**	(−2.12)	0.169	16,549
2008	−0.046**	(−2.04)	0.105	15,763
2009	−0.041**	(−1.98)	0.208	15,945
2010	−0.029*	(−1.79)	0.197	18,474
2011	−0.042***	(−2.67)	0.214	19,899
2012	−0.031**	(−2.24)	0.226	19,540
2013	−0.025***	(−2.65)	0.216	19,873
2014	−0.017**	(−2.09)	0.216	21,502
2015	−0.020**	(−2.33)	0.199	20,808

This table presents the yearly association between analyst forecast form specificity and forecast accuracy. Column "*SpecificMode*" reports the coefficients for *SpecificMode* by estimating Eq. (1) using OLS regression specification in each fiscal year. The dependent variable *ForAccuracy* measures analyst i's forecasting performance relative to that of peer analysts following the same firm-year. *SpecificMode* is an indicator that equals 1 if an analyst's EPS forecast for a firm-year has more digits than the most frequent number of digits (mode) of analysts' forecasts for the same firm-year, and 0 otherwise. t-statistics (in parentheses) are based on robust standard errors adjusted for heteroscedasticity and clustered by analyst. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels using two-tailed student t-tests, respectively. Detailed variable definitions are provided in Appendix 2

4.1.3 Alternative measures of forecast specificity

Our primary measure of analyst forecast specificity, *SpecificMode*, compares an analyst's forecast specificity with the specificity of forecasts issued by her peer analysts following the same firm in the same year. In this section, we perform sensitivity analyses employing three alternative measures of forecast specificity. $SpecificCon_{ijt}$ is the number of overspecified digits compared to the mode number of digits; $SpecificEps_{ijt}$ is an indicator that equals 1 if analyst *i*'s forecast for firm *j* in year *t* has more digits after the decimal than the actual earnings per share (EPS) of firm *j* in year *t*-1, and 0 otherwise; and $SpecificMedian_{ijt}$ is an indicator that equals 1 if analyst *i*'s forecast for firm *j* in year *t* has more digits after the decimal than the median number of digits of analyst forecasts for the same firm-year, and 0 otherwise.

We re-estimate Eq. (1) after replacing *SpecificMode* with *SpecificCon*, *SpecificEps*, and *SpecificMedian* in succession. Table 4 reports the regression results. The coefficient on *SpecificCon* in Column (1) is significantly negative (−0.018, $t = -6.00$), confirming our main finding that more specific forecasts are less accurate. The results reported in Columns (2) and (3), employing *SpecificEps* and *SpecificMedian*, respectively, are qualitatively similar. We conclude that our main findings are robust to the measure of analyst forecast specificity used.

4.2 Why do analysts issue overprecise forecasts?

4.2.1 Inexperience and overprecision – The Dunning-Kruger effect

Our second hypothesis posits that overprecise forecasts reflect the Dunning-Kruger effect of sell-side analysts, i.e., incompetent analysts are unaware of the boundaries of their knowledge. Building on the academic evidence (e.g., Clement 1999; Bilinski et al. 2013; Bradshaw et al. 2019) and practitioners' observations (Valentine 2010), we use analyst inexperience as a proxy for analyst incompetency. We conjecture that the behavioral bias of overprecision is greater for inexperienced analysts.

The graphical evidence in Fig. 1 confirms this line of reasoning. Specifically, we categorize sample observations by analysts' general experience, i.e., the number of years since an analyst enters our database. For each subgroup of observations, we compute the group average of *SpecificMode*. In Fig. 1, the X-axis represents analysts' general experience. It has a minimum of two years because our regression sample requires lagged values of forecast accuracy and therefore mechanically excludes analysts' initial year forecasts. We aggregate all observations with general experience greater than or equal to 25 years in the subgroup "25″ because there are fewer observations that fall into these high-experience categories.[14] The Y-axis denotes the mean *SpecificMode*, i.e., the proportion of forecasts that are *Specific* for each general experience subgroup. Figure 1 Panel A shows that analysts' tendency to issue *Specific* forecasts declines

[14] The maximum *GenExp* equals 31 in our sample for 12 analysts (2015–1985 + 1 = 31). However, we acknowledge that general experience cannot be measured precisely since analysts may have been forecasting prior to the sample beginning year (Jacob et al. 1999).

with their general experience. For example, with corroborating statistics in Table 5, we find that 6.81% of the forecasts issued by analysts with two years of general experience are more specific than peer analysts' forecasts. This ratio declines monotonically with analysts' general experience, to a minimum of 1.24% for analysts with 23 years of experience.[15] Because analysts who issue *Specific* forecasts are likely to differ from those who do not, we further retain only analysts who have issued at least one *Specific* forecast during our sample period and re-plot the pattern in Panel B. We observe the same relationship between analysts' general experience and their tendency to issue *Specific* forecasts. The graphical evidence supports our Hypothesis 2 that inexperienced analysts produce disproportionally higher fractions of *Specific* forecasts.

4.2.2 Specificity and analyst attributes: Multivariate evidence

To simultaneously consider determinants of forecast specificity, we examine the association between analysts' attributes and analysts' tendency to issue *Specific* forecasts using regression analysis. To do so, we construct an analyst-year-level dataset by aggregating each individual analyst's forecasts within a year into a single observation and then estimate the following :

$$\begin{aligned} AvgSpecific_{it} = {} & \beta_0 + \beta_1 AvgBold_{it} + \beta_2 Avg|LagForError|_{it} + \beta_3 AvgFirmExp_{it} \\ & + \beta_4 GenExp_{it} + \beta_5 Companies_{it} + \beta_6 Industries_{it} + \beta_7 BrokerSize_{it} \\ & + \beta_8 AvgDaysElapsed_{it} + \beta_9 AvgForHorizon_{it} + \beta_{10} AvgForFrequency_{it} \\ & + \beta_{11} AvgFyeDis_{it} + \text{Year Fixed Effects} + \varepsilon_{ijt} \end{aligned} \tag{4}$$

where the dependent variable $AvgSpecific_{it}$ is the percentage of analyst i's forecasts that are *Specific* forecasts during year t, and the independent variables are yearly averages of (unscaled) forecast-level values of an analyst's forecasts during the year. For example, *AvgBold* is the yearly average of *Boldness* for all the forecasts made during the fiscal year by a given analyst.

Table 6 reports the regression results. We have the following observations. First, the results confirm our earlier conjecture that inexperienced analysts are more likely to issue *Specific* forecasts, as evidenced by the significantly negative coefficient on general experience (*GenExp*) in Column (1) (−0.110, $t=-5.13$). A second proxy for experience, *AvgFirmExp*, exhibits an insignificant coefficient (0.003, $t=0.05$).[16] Given the high correlation between *AvgFirmExp* and *GenExp* (*Corr.* = 0.614, $p<0.01$), we further estimate the regression after excluding either *AvgFirmExp* or *GenExp* from the control variables. We find that *GenExp* continues to have a significantly negative coefficient (−0.110, $t=-4.88$ in Column (2)) when *AvgFirmExp* is excluded. Further, without controlling for general experience, firm experience also reduces the frequency of *Specific* forecasts (−0.164, $t=-2.49$ on *AvgFirmExp* in

---

15 We note slightly higher proportions of *Specific* forecasts for the "24" and the "25" subgroups, compared with the "23" subgroup. The less smooth pattern we observe for high values of general experience could be due to the relatively smaller sample size.

16 We use the average of *FirmExp*, i.e., *AvgFirmExp*, as an analyst has different *FirmExp* values for different firms in a given year. However, the analyst's general experience (*GenExp*) remains the same for all observations in the given year.

Table 4 Forecast specificity and forecast accuracy – using alternative constructs of forecast specificity

	Dependent Variable = *ForAccuracy*		
Variables	(1)	(2)	(3)
SpecificCon	−0.018***	–	–
	(−6.00)	–	–
SpecificEps	–	−0.009***	–
	–	(−3.75)	–
SpecificMedian	–	–	−0.016***
	–	–	(−4.88)
Control Variables	Included	Included	Included
Adjusted R^2	0.179	0.179	0.179
Observations	389,467	389,467	389,467

This table presents the association between analyst forecast form specificity and forecast accuracy by using three alternative measures of analyst forecast specificity. Our sample period covers fiscal years from 1986 to 2015. We perform OLS regressions. The dependent variable *ForAccuracy* measures analyst *i*'s forecasting performance relative to that of peer analysts following the same firm-year. *SpecificCon* is the number of overspecified digits compared with the mode forecast. For example, if analyst *i*'s forecast for firm *j* ends with three digits and the mode number of digits for forecasts for firm *j* equals 1, *SpecificCon* = 2. *SpecificEps* is an indicator that equals 1 if analyst *i*'s forecast for firm *j* in year *t* has more digits after the decimal compared with the actual earnings per share (EPS) number of firm *j* in year *t*-1, and 0 otherwise. *SpecificMedian* is an indicator that equals 1 if analyst *i*'s forecast for firm *j* in year *t* has more digits after the decimal than the median number of digits of analyst forecasts for the same firm-year, and 0 otherwise. *t*-statistics (in parentheses) are based on robust standard errors that are clustered by analyst and year. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels using two-tailed student *t*-tests, respectively. Detailed variable definitions are provided in Appendix 2

Column (3)). Therefore, the negative and significant coefficient on *AvgFirmExp* in Column (3) likely manifests the effect of analysts' general experience, a factor that is correlated with analysts' firm experience.[17] Comparing regression results across the three specifications further highlights the notion that an analyst's general experience significantly reduces the analyst's tendency to issue *Specific* forecasts.

Second, we observe additional evidence that is consistent with the Dunning-Kruger effect. We find that analysts who issue *Specific* forecasts have poorer historical performance (28.391, $t = 3.53$ on *Avg*|*LagForError*|) and lower forecasting frequency (−0.140, $t = -1.68$ on *AvgForFrequency*). We argue that competent analysts provide better forecasts historically. The finding that an analyst's historical forecasting performance impacts the analyst's tendency to issue *Specific* forecasts further highlights the importance of controlling for prior forecast accuracy when examining the association between overprecision and current forecast accuracy in Table 2. Regarding forecasting frequency, the literature shows that analysts who forecast more frequently work harder and are better able to incorporate the latest information in their forecasts (Jacob et al. 1999; Clement and Tse 2005; Loh and Stulz 2018).

[17] Firm experience may also affect an analyst's access to the covered firm's private information, which significantly influences forecast accuracy (Green et al. 2014; Soltes 2014; Brown et al. 2015). However, whether such access increases or decreases the analyst's issuance of *Specific* forecasts is less clear.

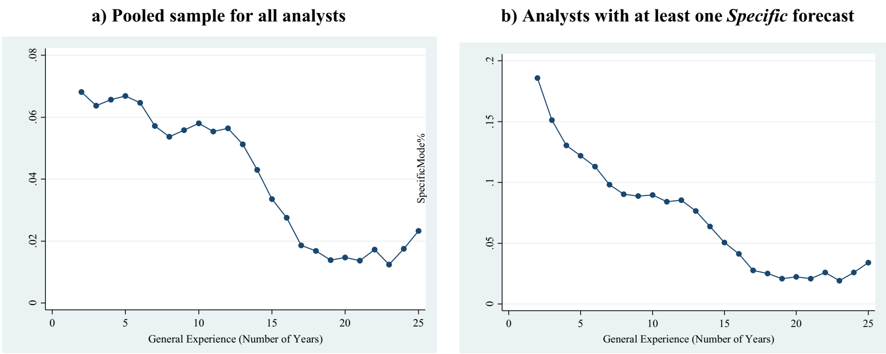

Fig. 1 Forecast specificity by analysts' general experience. **A**: Pooled sample for all analysts **B**: Analysts with at least one *Specific* forecast. This figure plots the average value of *SpecificMode* for each subgroup of observations based on analysts' general experience. We categorize sample observations by analysts' general experience. For each subgroup, we compute the group average of *SpecificMode*. *SpecificMode* is an indicator that equals 1 if an analyst's EPS forecast for a firm-year has more digits than the most frequent number of digits (mode) of analysts' forecasts for the same firm-year, and 0 otherwise. Analysts' general experience (i.e., the raw value of *GenExp*) begins with 2 years because our empirical sample requires information of analysts' lagged forecast accuracy, excluding analysts' initial year forecasts. Further, we cluster observations corresponding to analysts with more than 25 years' experience into the "25″ subgroup to the rightmost of the figures. Panel A presents the pattern for the pooled sample for all analysts. Panel B presents the pattern for analysts who have at least one observation with $SpecificMode=1$ during the sample period. In both panels, the X-axis depicts analysts' general experience, and the Y-axis presents the group average of *SpecificMode*

Third, we obtain suggestive evidence that analysts who issue *Specific* forecasts may have over-relied on their model output (Valentine 2010). Although such an activity is unobservable, we argue that it can be captured to some degree by the following two proxies: (1) forecast boldness, because analysts who over-rely on their model output are likely to issue bolder forecasts and not to herd; and (2) brokerage house prestige, because analysts employed by large brokerage houses are likely to be overconfident about their forecasting models. We find that the coefficient on analyst boldness is positive and significant (4.638, $t=4.64$ on *AvgBold* in Column (1)). Further, analysts employed by prestigious brokerage houses are more likely to issue *Specific* forecasts (1.918, $t=3.52$ on *BrokerSize*). Although only suggestive, these results are consistent with the notion that analysts who issue *Specific* forecasts overly rely on their forecasting models.[18]

Lastly, we examine whether incorporating brokerage house fixed effects affects the association between analyst attributes and analysts' tendency to issue *Specific*

[18] An alternative explanation is that junior analysts may be subject to a heavier workload and fail to process the additional digits of their forecasts. Such a concern can be alleviated through the following results: (1) the majority (65.07%) of our *Specific* forecasts have no more than two digits and are thus less subject to this concern; (2) the coefficients on *Industries* and *Companies*, two proxies for analysts' workload, are negative (and statistically significant for *Industries*); and (3) when we add the interaction variables *GenExp* × *Industries* and *GenExp* × *Companies* to the regression, we find insignificant coefficients on both terms.

Table 5 Analyst general experience and forecast specificity

General Experience	Observations	*SpecificMode* = 1	(*SpecificMode* = 1)%
2	20,030	1364	6.81%
3	25,292	1611	6.37%
4	27,753	1823	6.57%
5	27,695	1851	6.68%
6	25,253	1633	6.47%
7	23,585	1348	5.72%
8	22,094	1187	5.37%
9	21,390	1194	5.58%
10	20,247	1175	5.80%
11	18,593	1030	5.54%
12	17,197	970	5.64%
13	15,974	819	5.13%
14	14,712	632	4.30%
15	13,339	448	3.36%
16	11,757	324	2.76%
17	10,389	193	1.86%
18	9147	154	1.68%
19	8510	118	1.39%
20	7431	109	1.47%
21	7026	96	1.37%
22	6335	109	1.72%
23	5572	69	1.24%
24	5031	88	1.75%
25	25,115	585	2.33%

This table presents the average of *SpecificMode* in each subsample where analysts have the same years of general experience. Our sample comprises 389,467 observations for the period 1986 to 2015. Column "General Experience" denotes the number of years that analyst *i* has issued forecasts for any firm until year *t*. General experience (i.e., the raw value of *GenExp*) begins with two years because our empirical sample requires information of analysts' lagged forecast accuracy, excluding analysts' initial-year forecasts. Column "Observations" denotes the number of observations within the subgroup. Column "*SpecificMode* = 1″ denotes the number of observations with *SpecificMode* = 1. *SpecificMode* is an indicator that equals 1 if an analyst's EPS forecast for a firm-year has more digits than the most frequent number of digits (mode) of analysts' forecasts for the same firm-year, and 0 otherwise. Column "(*SpecificMode* = 1)%" denotes the proportion of observations with specific forecasts in each subgroup. This column corresponds to numbers plotted in Fig. 1

forecasts. Such a test will alleviate the concern that *Specific* forecasts may result from the forecast model setup because analysts in the same brokerage house are likely to employ similar models. We augment the original regression model by adding brokerage house fixed effects. The results in Panel B show negative and significant coefficients on *GenExp* (−0.057, $t = -4.11$ in Column (1); −0.055, $t = -3.95$ in Column (2)) and positive and significant coefficients on *Avg*|*LagForError*| and

Table 6 Analyst attributes and forecast specificity – analyst-level analyses

	Dependent Variable = *AvgSpecific* × *100*		
Variables	(1)	(2)	(3)
Panel A: Analyst attributes and forecast specificity			
GenExp	−0.110***	−0.110***	–
	(−5.13)	(−4.88)	–
AvgFirmExp	0.003	–	−0.164**
	(0.05)	–	(−2.49)
Avg|LagForError|	28.391***	28.383***	29.094***
	(3.53)	(3.52)	(3.60)
AvgBold	4.638***	4.639***	4.718***
	(4.64)	(4.66)	(4.65)
BrokerSize	1.918***	1.918***	1.880***
	(3.52)	(3.53)	(3.40)
Industries	−0.095**	−0.095**	−0.105**
	(−2.34)	(−2.35)	(−2.56)
Companies	−0.006	−0.006	−0.016
	(−0.28)	(−0.28)	(−0.66)
AvgForFrequency	−0.140*	−0.140*	−0.129
	(−1.68)	(−1.70)	(−1.56)
AvgDaysElapsed	−0.016***	−0.016***	−0.015***
	(−2.82)	(−2.85)	(−2.80)
AvgForHorizon	0.003	0.003	0.003
	(1.44)	(1.44)	(1.55)
AvgFyeDis	0.039	0.039	0.033
	(0.57)	(0.57)	(0.48)
Year Fixed Effects	Included	Included	Included
Adjusted R^2	0.054	0.054	0.052
Observations	63,900	63,900	63,900
Panel B: Controlling for brokerage house fixed effects			
GenExp	−0.057***	−0.055***	–
	(−4.11)	(−3.95)	–
AvgFirmExp	0.010	–	−0.073*
	(0.23)	–	(−1.66)
Avg|LagForError|	16.843**	16.819**	17.111**
	(1.98)	(1.98)	(2.01)
AvgBold	2.691***	2.693***	2.714***
	(3.58)	(3.60)	(3.59)
BrokerSize	0.167	0.167	0.182
	(0.44)	(0.44)	(0.48)
Other Controls	Included	Included	Included
Year Fixed Effects	Included	Included	Included
Brokerage House Fixed Effects	Included	Included	Included

Table 6 (continued)

Variables	Dependent Variable = *AvgSpecific* × *100*		
	(1)	(2)	(3)
Adjusted R^2	0.312	0.312	0.312
Observations	63,900	63,900	63,900

This table presents the OLS regression results on the association between analysts' attributes and their forecast specificity. Our sample includes 63,900 analyst-year-level observations during the period 1986 to 2015. Panel A reports the baseline regression results. Panel B reports regression results for the brokerage house fixed effects model. In both panels, the dependent variable *AvgSpecific*×*100* is defined as the proportion of forecasts with *SpecificMode* = 1 for an analyst during a year, multiplied by 100. *SpecificMode* is an indicator that equals 1 if an analyst's EPS forecast for a firm-year has more digits than the most frequent number of digits (mode) of analysts' forecasts for the same firm-year, and 0 otherwise. *t*-statistics (in parentheses) are based on robust standard errors that are clustered by analyst and year. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels using two-tailed student *t*-tests, respectively. Detailed variable definitions are provided in Appendix 2

AvgBold across all specifications.[19] Therefore, controlling for brokerage fixed effects, analyst attributes continue to explain analysts' tendencies to issue *Specific* forecasts. Overall, the empirical results support Hypothesis 2.[20]

4.3 Specificity and accuracy – Implications of inexperience and behavioral bias

In the preceding subsection, we documented that, consistent with the Dunning-Kruger effect, inexperience is a critical determinant of analysts' overprecision. Further, the significant effects of forecast boldness and brokerage house prestige confirm that analysts may over-rely on their model output. Importantly, these causes for analyst forecast specificity also have implications for triangulating the relationships between forecast specificity, analyst attributes, and forecast accuracy. In this section, we design and provide related tests to further address this notion.

Our analyses here comprise two levels. First, we are interested in whether experience can mitigate the negative association between forecast specificity and forecast accuracy. If, as we have argued, inexperienced analysts exhibit behavioral bias by overly relying on their models, we expect the negative association to be weaker for experienced analysts. We find empirical results that are consistent with this expectation. In Table 7, we estimate the association between forecast specificity and forecast accuracy after allowing the relationship to differ across analysts with varying levels of experience. We do so by including the interaction between *SpecificMode*

[19] The coefficients on *BrokerSize* have the predicted positive sign, but are no longer statistically significant. This is not unexpected, because brokerage house fixed effects absorb the cross-sectional variation of *BrokerSize* across brokerage houses.

[20] However, because analysts within the same brokerage house can still differ in their access to private information, it is possible that overprecise forecasts arise because of analysts' overreliance on their private information. To further alleviate this concern, we distinguish star analysts from non-star analysts, with the premise being that the former likely enjoy better access to private information. We find no evidence that the association between overprecision and forecast accuracy differs for the two groups of analysts, consistent with analysts over-relying on their forecasting models. Nonetheless, we acknowledge that this inference is preliminary and indirect.

and *GenExp*. In Column (1), the coefficient on *SpecificMode*GenExp* is positive and significant (0.013, $t = 1.87$). This result suggests a smaller reduction in forecast accuracy when analysts with more general experience issue *Specific* forecasts.

Second, we leverage the two factors that capture analysts' tendency to overweight their model outputs, i.e., analyst boldness (*Bold*) and brokerage house size (*BrokerSize*). The existing literature shows that boldness and brokerage house prestige positively impact forecast accuracy (Hong et al. 2000; Clement and Tse 2005).[21] Drawing upon our earlier findings that *Specific* forecasts are more likely to be bold forecasts and issued by analysts employed by prestigious brokerage houses, we conjecture that the positive impact of boldness and brokerage house prestige on forecast accuracy may be weaker when forecasts are *Specific*. Such a line of reasoning is similar to that in Yin and Zhang (2014), who show differential associations between forecast boldness and forecast accuracy due to analysts' tournament incentives, and to that in Fang and Yasuda (2009), who find that the association between bank prestige and affiliated analysts' forecast accuracy depends on analysts' individual reputations.

Columns (2) and (3) in Table 7 report positive and significant coefficients on *Bold* and *BrokerSize*, confirming the notions in the existing literature for non-*Specific* forecasts. More importantly, Column (2) reports a negative and significant coefficient on *SpecificMode*Bold* (−0.029, $t = -2.45$), suggesting that boldness is less likely to improve forecast accuracy for *Specific* forecasts. Further, Column (3) reports a negative and weakly significant coefficient on *SpecificMode*BrokerSize* (−0.009, $t = -1.65$), suggesting that brokerage house prestige also is less likely to improve forecast accuracy for *Specific* forecasts.

4.4 Capital market implications of more specific forecasts

Having documented the existence of overprecision and suggested its causes, we next consider the implications of *Specific* forecasts for the capital market. We explore whether investors can rationally weight the information contained in form specificity in terms of its relation to forecast accuracy, i.e., Hypothesis 3. Rational expectations predict that investors' responses to forecast revisions will be weaker when updated forecasts are *Specific* (Stickel 1992; Park and Stice 2000; Gleason and Lee 2003).

4.4.1 Analyses of price reactions during the short-term event window

To analyze investors' reactions to *Specific* forecasts, we begin with the following baseline model of the relation between stock returns and forecast revisions:

$$CAR_{ijt} = \alpha_0 + \alpha_1 REV_{ijt} + \varepsilon_{ijt} \tag{5}$$

where CAR_{ijt} is the cumulative abnormal return during the three-day window centered on the forecast revision date (i.e., days −1 to +1), and REV_{ijt} is analyst i's

[21] Our regression results confirm the positive effects of boldness and brokerage house prestige on forecast accuracy. Table 7 reports positive and significant coefficients on *Bold* and *BrokerSize*. We find similar results in our other regressions with forecast accuracy as the dependent variable.

Table 7 Forecast specificity and forecast accuracy – moderating effects of analyst attributes

	Dependent Variable = *ForAccuracy*		
Variables	(1)	(2)	(3)
SpecificMode	−0.025***	−0.007	−0.012**
	(−4.97)	(−1.47)	(−2.40)
SpecificMode × *GenExp*	0.013*	–	–
	(1.87)	–	–
SpecificMode × *Bold*	–	−0.029**	–
	–	(−2.45)	–
SpecificMode × *BrokerSize*	–	–	−0.009*
	–	–	(−1.65)
GenExp	0.009***	0.010***	0.010***
	(3.39)	(3.84)	(3.84)
Bold	0.076***	0.077***	0.076***
	(9.51)	(9.66)	(9.51)
BrokerSize	0.008***	0.008***	0.008***
	(3.72)	(3.67)	(3.76)
Control Variables	Included	Included	Included
Adjusted R^2	0.179	0.179	0.179
Observations	389,467	389,467	389,467

This table presents the OLS regression results on the association between analyst forecast form specificity and forecast accuracy, conditioning on analysts' attributes. Our sample period covers fiscal years from 1986 to 2015. The dependent variable *ForAccuracy* measures analyst *i*'s forecasting performance relative to that of peer analysts following the same firm-year. *SpecificMode* is an indicator that equals 1 if an analyst's EPS forecast for a firm-year has more digits than the most frequent number of digits (mode) of analysts' forecasts for the same firm-year, and 0 otherwise. $GenExp_{it}$ is defined as the number of years that analyst *i* has issued forecasts for any firm until year *t*. $Bold_{ijt}$ is defined as the distance between analyst *i*'s forecast for firm *j* in year *t* from the pre-revision (year-to-date) consensus forecast for firm *j* in year *t* (Clement and Tse 2005). *BrokerSize* measures the size of analyst *i*'s brokerage house. It is an indicator that equals 1 if analyst *i* is employed by a brokerage firm in the top decile in terms of the number of analysts employed during year *t*, and 0 otherwise. *t*-statistics (in parentheses) are based on robust standard errors that are clustered by analyst and year. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels using two-tailed student *t*-tests, respectively. Detailed variable definitions are provided in Appendix 2

forecast revision for firm *j* in year *t*, computed as analyst *i*'s revised forecast for firm *j* in year *t* less analyst *i*'s prior forecast for firm *j* in year *t*, divided by firm *i*'s stock price two days prior to the forecast revision.[22]

We then examine whether investors incorporate information in analyst-specific and forecast-specific characteristics that are related to analyst forecast accuracy in stock prices. If investors consider forecast specificity (*SpecificMode*) and the other explanatory variables of forecast accuracy in Eq. (1), they will incorporate them in

[22] Following Clement and Tse (2005), we eliminate potential outliers by omitting observations with price-deflated forecast revisions (i.e., *REV*) above 0.10 or below −0.10.

their responses to forecast revisions, and the coefficients (i.e., α_0 and α_1) in Eq. (5) will be a function of these variables, as shown in Eqs. (6a) and (6b) below:

$$\begin{aligned}\alpha_0 = \beta_0 &+ \beta_1 SpecificMode_{ijt} + \beta_2 LagForAccuracy_{ij,t-1} + \beta_3 Bold_{ijt} \\ &+ \beta_4 BrokerSize_{it} + \beta_5 GenExp_{it} \\ &+ \beta_6 FirmExp_{ijt} + \beta_7 Industries_{it} \\ &+ \beta_8 Companies_{it} + \beta_9 ForFrequency_{ijt} \\ &+ \beta_{10} DaysElapsed_{ijt} + \beta_{11} ForHorizon_{ijt} \\ &+ \beta_{12} FyeDis_{ijt}\end{aligned} \tag{6a}$$

$$\begin{aligned}\alpha_1 = &\gamma_0 + \gamma_1 SpecificMode_{ijt} + \gamma_2 LagForAccuracy_{ij,t-1} \\ &+ \gamma_3 Bold_{ijt} + \gamma_4 BrokerSize_{it} + \gamma_5 GenExp_{it} \\ &+ \gamma_6 FirmExp_{ijt} + \gamma_7 Industries_{it} \\ &+ \gamma_8 Companies_{it} + \gamma_9 ForFrequency_{ijt} \\ &+ \gamma_{10} DaysElapsed_{ijt} + \gamma_{11} ForHorizon_{ijt} + \gamma_{12} FyeDis_{ijt}\end{aligned} \tag{6b}$$

Substituting Eqs. (6a) and (6b) into Eq. (5), we obtain the following regression model:

$$\begin{aligned}CAR_{ijt} = \beta_0 + REV_{ijt}\{&\gamma_0 + \gamma_1 SpecificMode_{ijt} + \gamma_2 LagForAccuracy_{ij,t-1} + \gamma_3 Bold_{ijt} \\ &+ \gamma_4 BrokerSize_{it} + \gamma_5 GenExp_{it} + \gamma_6 FirmExp_{ijt} + \gamma_7 Industries_{it} \\ &+ \gamma_8 Companies_{it} + \gamma_9 ForFrequency_{ijt} + \gamma_{10} DaysElapsed_{ijt} \\ &+ \gamma_{11} ForHorizon_{ijt} + \gamma_{12} FyeDis_{ijt}\} + \beta_1 SpecificMode_{ijt} \\ &+ \beta_2 LagForAccuracy_{ij,t-1} + \beta_3 Bold_{ijt} + \beta_4 BrokerSize_{it} + \beta_5 GenExp_{it} \\ &+ \beta_6 FirmExp_{ijt} + \beta_7 Industries_{it} + \beta_8 Companies_{it} + \beta_9 ForFrequency_{ijt} \\ &+ \beta_{10} DaysElapsed_{ijt} + \beta_{11} ForHorizon_{ijt} + \beta_{12} FyeDis_{ijt} + \varepsilon_{ijt}\end{aligned} \tag{7}$$

We report the estimation results in Table 8. In the first column, where we employ *CAR*[−1, 1] (i.e., cumulative abnormal stock return during the three-day event window) as the dependent variable, the coefficient on *REV*SpecificMode* is negative and significant (−0.409, $t = -6.16$), suggesting that investors, on average, discount forecasts that are more specific than peer analyst forecasts. This result is consistent with the view of investors' rational expectations that is modeled in Abarbanell et al. (1995) and empirically Lee (2003), Park and Stice (2000), and Stickel (1992).[23]

[23] In untabulated analyses, we split our sample into two subsamples based on institutional ownership (IO). We find that firms with higher IO display greater reactions to forecast revisions than firms with lower IO. Furthermore, the former group exhibits larger discounting for *Specific* forecasts than the latter. The above inferences hold for the [−1, 1] event window and the extended [−1, 20] window. These results are consistent with the notion that institutional investors have access to and rely on sell-side analysts' forecasts for their investment decisions (Frankel et al. 2006; Malmendier and Shanthikumar 2014; Akbas et al. 2018) and that retail investors may differ from institutional investors in their access to information (Farrell et al. 2022). However, we acknowledge an alternative view in the literature that buy-side analysts do not find sell-side analysts' information to be very useful when making stock recommendations (Brown et al. 2016). We thank an anonymous reviewer for pointing out this relevant issue.

4.4.2 Analyses of price reactions during the subsequent period

It is also possible that investor reactions to analyst forecast revisions are incomplete during the three-day event window. To test this conjecture, we re-estimate Eq. (6) using *CAR*[2, 20], the cumulative abnormal return during the period from days 2 to 20 after the revision date (i.e., day 0), as the dependent variable. Extending the window to the 20th trading day after the forecast revision date also sees the aggregated window, including the short-term and the delayed periods, approximating one calendar month (Jegadeesh and Kim 2010). We again find a negative and significant coefficient on *REV*SpecificMode* (−0.105, $t = -2.19$), indicating that investors do not exhibit full rationality, as they only partially incorporate the lower accuracy of *Specific* forecasts during the three-day event window and continue to discount these forecasts in a delayed period.[24]

Although not of primary interest, the results for the control variables also suggest partial rationality of investors (Clement and Tse 2003). In Table 8 Column (1), stock price responses to forecast revisions are stronger when the analyst (1) has better historical forecasting performance, (2) has greater boldness, (3) has a higher forecasting frequency, and (4) is employed by a larger brokerage house. Our earlier analyses show that these factors are positively related to forecast accuracy. However, the implied weights on some other factors, such as analysts' general experience and the number of industries followed, are inconsistent with the associations between these factors and forecast accuracy.

What might investors be learning during the delayed period? We conjecture that subsequent non-*Specific* forecasts issued by peer analysts, which are of different form but more accurate, may lead investors to further adjust share prices. We use the following procedures to test this conjecture. For each *Specific* revision, we retrieve peer analysts' non-*Specific* forecasts of annual EPS issued during the [2, 20] window. Of the 18,930 *Specific* revisions, 9735 can be matched with at least one non-*Specific* forecast subsequently issued by peer analysts. Untabulated *t*-test results show that the subsequent non-*Specific* peer forecasts are 6.1% more accurate than the treated *Specific* forecast.

Next, we split the *Specific* forecast revisions into two groups: (1) *Specific* revisions where peer analysts' subsequent non-*Specific* forecasts (for multiple forecasts, we use the mean) are more accurate, and (2) *Specific* revisions where peer analysts'

[24] During our sample period, investors may gradually learn that overprecise forecasts are less accurate, and reflect this in their (more efficient) reactions to these forecasts. To examine this possibility, we split our sample into two subsamples: 1986–2001 and 2002–2015. We then re-estimate our regressions of market reactions to forecast revisions for each subperiod. For discussion of the price efficiency, we follow Weller (2018) and Lee and Watts (2021) and compute the percentages of the total stock price reaction to the forecast revision during the event window and during the delayed window. We find that, for the 1986–2001 period, the event window reaction accounts for 57.69% of the total reaction, and the delayed window reaction accounts for 42.31%. For the 2002–2015 period, the corresponding percentages are 71.70% during the event-window and 28.30% during the delayed window. These results suggest that investors' incorporation of *Specific* forecasts' lower accuracy during the event window [−1, 1] is more complete during the second half of our sample period.

Table 8 Market reactions to *Specific* forecasts

	CAR [−1, 1]	*CAR* [2, 20]	
Variables	(1)	(2)	(3)
REV	0.281***	0.121**	0.118**
	(2.99)	(2.52)	(2.53)
REV×*SpecificMode*	−0.409***	−0.105**	–
	(−6.16)	(−2.19)	–
REV×*SpecificMode_Corrected*	–	–	−0.256**
	–	–	(−2.09)
REV×*SpecificMode_NonCorrected*	–	–	−0.054
	–	–	(−0.43)
Control of Characteristics	Included	Included	Included
Control of *REV*×Characteristics	Included	Included	Included
Year Fixed Effects	Included	Included	Included
Adjusted R^2	0.028	0.010	0.010
Observations	389,467	389,467	380,272

This table presents the OLS regression results of market reactions to analyst forecast revisions when the revised forecast is overprecise (i.e., *SpecificMode* = 1). In Column (1), *CAR* [−1, 1] is computed as cumulative market-adjusted abnormal return during the three-day event window with the forecast revision date as Day 0. In Columns (2) and (3), *CAR* [2, 20] is computed as the cumulative market-adjusted abnormal return during the prolonged reaction period from Day 2 to Day 20 after the forecast revision date. *REV* is an analyst's forecast revision for the firm's earnings in the current year. *SpecificMode* is an indicator that equals 1 if an analyst's EPS forecast for a firm-year has more digits than the most frequent number of digits (mode) of analysts' forecasts for the same firm-year, and 0 otherwise. In Column (3), we split *Specific* forecast revisions into two groups: (1) *Specific* revisions where peer analysts' subsequent non-*Specific* forecasts (for multiple forecasts, we use the consensus) are more accurate, and (2) *Specific* revisions where peer analysts' subsequent non-*Specific* forecasts are no more accurate. We create two indicators, *SpecificMode_Corrected* and *SpecificMode_NonCorrected*, that equal 1 for the first and the second group, respectively, and 0 otherwise. We omit reporting the coefficients for analysts' forecast characteristics for brevity. *t*-statistics (in parentheses) are computed based on standard errors that are clustered by analyst and year. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels using two-tailed student *t*-tests, respectively. Detailed variable definitions are provided in Appendix 2

subsequent non-*Specific* forecasts are not more accurate. We exclude 9195 *Specific* revisions without available information on subsequent peer analysts' forecasts.[25] We create two indicators, *SpecificMode_Corrected* and *SpecificMode_NonCorrected*, that equal 1 for the first group and the second group, respectively, and 0 otherwise.

We then repeat our market reaction test for the [2, 20] window after replacing *REV*×*SpecificMode* in Table 8 with *REV*×*SpecificMode_Corrected* and *REV*×*SpecificMode_nonCorrected*. Column (3) shows that the coefficient on *REV*×*SpecificMode_Corrected* is negative and significant (−0.256, $t = -2.09$). By contrast, the coefficient on *REV*×*SpecificMode_nonCorrected* is smaller in

[25] Adding back these observations and categorizing them into subsample (2) result in the same inferences.

magnitude and statistically insignificant. These results support our conjecture that investors learn from peer analysts' forecasts issued during the delayed period, which leads to further price adjustment.[26]

Overall, the empirical findings support Hypothesis 3 that investors on average react less to *Specific* forecasts. Further, stock price responses to *Specific* forecast revisions during the delayed period are consistent with the view that investors – and therefore security prices – exhibit incomplete adjustments when responding to public information such as accounting accruals (Sloan 1996; Xie 2001) and forecast revisions (Gleason and Lee 2003).

4.5 Additional analyses and discussions

4.5.1 Rounding and overprecision – A reconciliation

Herrmann and Thomas (2005) show that analyst forecasts of earnings per share occur in nickel intervals approximately 55% of the time; i.e., these are rounded forecasts. By comparison, actual earnings occur in nickel intervals only 22.75% of the time. They also document a negative association between forecast rounding and forecast accuracy and attribute the negative association to analysts' lack of information and resources to ascertain a more precise forecast number.

We note that our focus on forecast specificity differs from the rounding phenomenon analyzed in Herrmann and Thomas (2005) in two important ways. First, rounding refers to individuals' tendency to place a zero or a five in the penny location. Forecast specificity, however, can appear in both rounded and non-rounded forecasts. Regarding the former category, a forecast of 2.35 is more specific than one of 2.3.[27] As for the latter category, a forecast of 2.345 is more specific than one of 2.34. Second, if specificity is simply the complement of rounding, the existing literature would predict a positive association between forecast specificity and forecast accuracy. The reason is that rounded forecasts are shown to be less accurate than non-rounded forecasts (Herrmann and Thomas 2005).

Building on this discussion, we perform empirical analyses to revisit the rounding effect documented in prior research and to establish the robustness of our finding. We begin by replicating the main finding in Herrmann and Thomas (2005), i.e., that rounded forecasts are less accurate. We employ the same sample period used

[26] The delayed reactions seem inconsistent with the efficient market hypothesis. Nonetheless, it is possible that investors do not fully understand the implications of forecast attributes (e.g., overprecision in our context) for forecast accuracy. For example, Clement and Tse (2003) show that investors assign weights to forecast attributes that are different from the weights of such attributes in explaining forecast accuracy. Gleason and Lee (2003) find that analyst forecast revisions trigger both immediate and delayed reactions, with the degree of delayed reaction shaped by forecast attributes (e.g., revision innovation and analyst prestige) and firm attributes (e.g., uncertainty). The subsequent arrival of additional information, such as other analysts' forecast revisions, may facilitate the price correction and thus result in investors' continued discounting of *Specific* forecast revisions during the delayed period.

[27] Consider another illustration with two forecasts for a firm-year, 2.00 and 2.35, both of which are rounded forecasts. In our context, the latter is more precise than the former, thus creating variations in form precision even within rounded forecasts.

in Herrmann and Thomas (2005): 1986–2001.[28] We construct an indicator variable, *Round*, that equals 1 for forecasts with zero or five in the penny location, and 0 otherwise.

The results in Table 9 Panel A Column (1) confirm the finding in Herrmann and Thomas (2005) that rounded forecasts are less accurate. The coefficient on *Round* is negative and significant (−0.006, $t=-2.28$). Performing the same analysis in the 2002–2015 period, we find an insignificant association between *Round* and *ForAccuracy* (−0.001, $t=-0.38$ in Column (2)). The changing association between forecast rounding and forecast accuracy is of interest in its own right. One possible reason is that, as argued in Herrmann and Thomas (2005) and Dechow and You (2012), rounding is a public signal that can be observed by investors at a low cost. To the extent that investors learn the inherent inaccuracy of rounded forecasts and discount these forecasts, analysts will gradually have less incentives to perform rounding.

In untabulated analyses, we find that the fraction of rounded forecasts in the 1986–2001 period is 54.3% but drops to approximately half that (27.8%) in the 2002–2015 period, with the difference being statistically significant at the 1% level. Aggregating the two subperiods, Column (3) shows that the rounding effect is also statistically insignificant for the combined period. This evidence relates to the following question that Dechow and You (2012, p.1,963) posed when suggesting avenues for future research: "As analysts' incentives change, do we observe changes in rounding behavior?"

More importantly, Columns (4)–(6) in Table 9 Panel A establish the robustness of our results. We find that, after controlling for *Round*, the negative association between *SpecificMode* and *ForAccuracy* continues to hold in the 1986–2001 subsample (−0.017, $t=-3.35$), the 2002–2015 subsample (−0.027, $t=-7.03$), and the full sample period (−0.018, $t=-4.83$).[29]

A related observation, that the negative association between overprecision and forecast accuracy appears more pronounced in the second half of our sample period (in both Table 9 Panel A and our earlier Table 3), warrants further discussion because it raises the logical question of whether the overprecision bias becomes stronger over time. We draw on two sets of evidence to address this question. First, the declining trend of rounding, discussed above, suggests a higher probability that some *Specific* forecasts may result from their issuing analysts' non-rounding decisions. If these analysts' peers make their rounding choices to signal higher uncertainty or a lack of effort (Herrmann and Thomas 2005; Dechow and You 2012), then *Specific* forecasts will have higher accuracy. The results reported in Table 9 confirm this effect, which has been proposed in the earlier literature. This effect, however,

[28] Herrmann and Thomas (2005) employ a sample covering 1985–2001. Because we require lagged information on analyst attributes and firm fundamentals, we exclude observations for 1985.

[29] There exists a possibility that some analysts may issue *Specific* forecasts because they intend to avoid rounding. Being unable to observe such intentions, we cannot fully rule out this possibility. However, the evidence in Herrmann and Thomas (2005) mitigates the concern, as rounded forecasts, on average, are less accurate than non-rounded forecasts, at least for the earlier part of the sample period. Our Table 9 Panel A confirms this finding.

runs counter to the overprecision effect and would weaken the estimated association between *SpecificMode* and *ForAccuracy*.

Second, we perform additional analyses that are less affected by the rounding trend and the ensuing sample composition issue. We repeat the subperiod regressions after excluding all rounded forecasts. Columns (1) and (2) in Table 9 Panel B report negative and significant coefficients on *SpecificMode* that are of similar magnitude (−0.024, t = −3.97 for 1986–2001 and − 0.027, t = −6.38 for 2002–2015). Alternatively, we employ the full sample but replace *SpecificMode* with *SpecificEps*, which is less affected by peer analysts' rounding decisions. Columns (3) and (4) in Panel B show that the coefficients on *SpecificEps* are also of similar magnitude (−0.009, t = −2.40 for 1986–2001; −0.008, t = −2.72 for 2002–2015). Collectively, these results do not provide consistent evidence of stronger overprecision biases over time.

4.5.2 Level of EPS, overprecision, and forecast accuracy

Another issue related to our main results is that the link between overprecision and forecast accuracy may be driven by low EPS (in absolute terms) firms. As the EPS magnitude increases, the economic importance of overprecision declines. Furthermore, Roger et al. (2018) build on the neuropsychology research and show that individual analysts process small and large numbers differently. This issue is relevant, as it speaks to the generalizability of our findings.

To address this concern, we first examine the distribution of the numbers of digits of analyst forecasts for firms with varying levels of absolute EPS. We categorize sample observations (i.e., analyst forecasts) into the following groups based on the absolute value of EPS (in \$) of each firm-year: (1) 0 < =|EPS| < \$0.1, (2) \$0.1 < =|EPS| < \$1, (3) \$1 < =|EPS| < \$5, (4) \$5 < =|EPS| < \$10, (5) \$10 < =|EPS| < \$100 and (6) |EPS| > =\$100.

Panel A of Table 10 tabulates the frequency and the percentage of analyst forecasts ending with specific numbers of digits (i.e., from one digit to four digits) for each of the above subsamples. We find that the patterns of forecast digits vary with EPS magnitude. As |EPS| increases, the percentage of forecasts ending with one digit after the decimal increases, almost doubling from the [0, 0.1) subsample to the [10, 100) subsample (the > = 100 subsample includes only 17 observations and its statistics should be interpreted with caution); however, the percentage of forecasts ending with two digits significantly declines from 81.74% for the [0, 0.1) subsample to 67.36% for the [10, 100) subsample. Forecasts with zero digits after the decimal also increase with |EPS|, except for the [0, 0.1) subsample, which has a relatively higher percentage of 4.83%.[30] These patterns are generally consistent with additional digits being of greater importance for small |EPS| firms.

Given the above forecast distributions, it is important to investigate whether small |EPS| firms, whose analysts may be more likely to misunderstand the importance of the penny part, drive the observed negative association between *SpecificMode* and

[30] The statistic of 4.83% is reasonable because firms in the [0, 0.1) subsample have zero or close-to-zero EPS, resulting in many forecasts with zero value.

Table 9 Forecast specificity and forecast rounding

Panel A: Replication of the rounding effect and robustness of the overprecision effect						
	1986–2001	2002–2015	Full Sample	1986–2001	2002–2015	Full Sample
Variables	(1)	(2)	(3)	(4)	(5)	(6)
SpecificMode	–	–	–	−0.017***	−0.027***	−0.018***
	–	–	–	(−3.35)	(−7.03)	(−4.83)
Round	−0.006**	−0.001	−0.002	−0.006**	−0.001	−0.002
	(−2.28)	(−0.38)	(−1.50)	(−2.35)	(−1.12)	(−1.63)
Control Variables	Included	Included	Included	Included	Included	Included
Adjusted R^2	0.174	0.189	0.179	0.174	0.189	0.179
Observations	152,389	237,078	389,467	152,389	237,078	389,467

Panel B: Subperiod regressions – excluding rounded forecasts or using *SpecificEps*				
	Dependent Variable = *ForAccuracy*			
	Excluding Rounded Forecasts		Full Sample	
	1986–2001	2002–2015	1986–2001	2002–2015
Variables	(1)	(2)	(3)	(4)
SpecificMode	−0.024***	−0.027***	–	–
	(−3.97)	(−6.38)	–	–
SpecificEps	–	–	−0.009**	−0.008***
	–	–	(−2.40)	(−2.72)
Control Variables	Included	Included	Included	Included
Adjusted R^2	0.160	0.186	0.174	0.189
Observations	69,673	171,151	152,389	237,078

This table presents the OLS regression results on the association between forecast rounding and forecast accuracy, and the robustness analyses results on the association between forecast specificity and forecast accuracy controlling for rounding effects. The dependent variable *ForAccuracy* measures analyst *i*'s forecasting performance relative to that of peer analysts following the same firm-year. *SpecificMode* is an indicator that equals 1 if an analyst's EPS forecast for a firm-year has more digits than the most frequent number of digits (mode) of analysts' forecasts for the same firm-year, and 0 otherwise. *Round* is an indicator that equals 1 for forecasts with zero of five in the penny location, and 0 otherwise. In Panel A, we perform ordinary least squared (OLS) regressions. Columns (1) and (4) report results for the subperiod 1986–2001, i.e., the sample period employed in Herrmann and Thomas (2005). Columns (2) and (5) report results for the subperiod 2002–2015. Columns (3) and (6) report results for the combined 1986–2015 period. In Panel B, we perform subperiod regression analyses by excluding rounded forecasts in Columns (1) and (2) or by replacing *SpecificMode* with *SpecificEps* (using the full sample) in Columns (3) and (4). *SpecificEps* is an indicator that equals 1 if analyst *i*'s forecast for firm *j* in year *t* has more digits after the decimal, compared with the actual earnings per share (EPS) number of firm *j* in year *t*-1, and 0 otherwise. *t*-statistics (in parentheses) are based on standard errors that are clustered by analyst and year. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels using two-tailed student *t*-tests, respectively. Detailed variable definitions are provided in Appendix 2

ForAccuracy. We conduct subsample regressions to address this question. For each of the subsamples categorized based on |EPS|, we perform the multivariate regression relating *SpecificMode* to *ForAccuracy* in Eq. (1).[31]

[31] We exclude the |EPS| >= $100 subsample, which includes only 17 observations, due to insufficient degrees of freedom. We obtain very similar results when we add these observations to the $10 <= |EPS| < $100 subsample.

Table 10 |EPS| level, forecast digits, and overprecision

Panel A: Distribution of forecast digits for different |EPS| levels

		Number of Analyst Forecasts (*Percentage of Group Total*)					
		Number of Digits =					
Group	Range of |EPS|	0	1	2	3	4	Group Total
1	[0, 0.1)	423	943	7162	148	86	8762
		(4.83%)	*(10.76%)*	*(81.74%)*	*(1.69%)*	*(0.98%)*	
2	[0.1, 1)	1571	14,031	78,314	1471	861	96,248
		(1.63%)	*(14.58%)*	*(81.37%)*	*(1.53%)*	*(0.89%)*	
3	[1, 5)	7159	48,326	187,993	5175	2663	251,316
		(2.85%)	*(19.23%)*	*(74.80%)*	*(2.06%)*	*(1.06%)*	
4	[5, 10)	1335	6747	19,226	690	300	28,298
		(4.72%)	*(23.84%)*	*(67.94%)*	*2.44%*	*1.06%*	
5	[10, 100)	337	1057	3251	152	29	4826
		(6.98%)	*(21.90%)*	*(67.36%)*	*(3.15%)*	*(0.60%)*	
6	> = 100	8	1	8	0	0	17
		(47.06%)	*(5.88%)*	*(47.06%)*	*(0.00%)*	*(0.00%)*	

Panel B: Overprecision and forecast accuracy for firms of different |EPS| levels

	Dependent Variable = *ForAccuracy*				
Variables	|EPS|∈ [0, 0.1)	|EPS|∈ [0.1, 1)	|EPS|∈ [1, 5)	|EPS|∈ [5, 10)	|EPS|∈ [10, 100)
	(1)	(2)	(3)	(4)	(5)
SpecificMode	0.007	−0.015*	−0.020***	−0.017*	−0.030**
	(0.35)	(−1.83)	(−4.63)	(−1.81)	(−2.01)
Control Variables	Included	Included	Included	Included	Included
Adjusted R^2	0.160	0.156	0.186	0.213	0.268
Observations	8762	96,248	251,316	28,298	4826

This table presents the association between analyst forecast specificity and forecast accuracy for subsamples of different |EPS| levels. We categorize analyst forecasts into following groups based on the absolute value of EPS (in $) of each firm-year: (1) 0 < =|EPS| < $0.1, (2) $0.1 < =|EPS| < $1, (3) $1 < =|EPS| < $5, (4) $5 < =|EPS| < $10, (5) $10 < =|EPS| < $100, and (6) |EPS| > =$100. Panel A reports the frequency and the percentage of analyst forecasts ending with specific numbers of digits (i.e., from one digit to four digits) for each of the above subsamples. Panel B reports the subsample regression results relating *SpecificMode* and *ForAccuracy*. We exclude the |EPS| > =$100 subsample, which includes only 17 observations, due to insufficient degrees of freedom (adding these observations to the [10, 100) subsample generates similar results). *t*-statistics (in parentheses) are based on standard errors that are clustered by analyst and year. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels using two-tailed student *t*-tests, respectively. Detailed variable definitions are provided in Appendix 2

Panel B of Table 10 shows that our findings are not driven by the small |EPS| firms. Within the five subsamples, the four subsamples with the largest |EPS| exhibit negative and significant coefficients on *SpecificMode* in Columns (2)–(5). The coefficient is positive and statistically insignificant for the smallest |EPS| subsample in Column (1). The results show that it is precisely for these small |EPS| firms that analysts pay the closest attention to the additional digits. Consequently, *Specific* forecasts for such firms do not exhibit lower accuracy. Collectively, the results of the analyses in this section alleviate the concern that our main findings may be driven by small |EPS| firms.

4.5.3 Are additional digits the source of inaccuracy?

A potential concern with our interpretation of the negative association between forecast specificity and forecast accuracy is that the additional digits of *Specific* forecasts could be the source of inaccuracy. In the extreme case, if the majority of forecasts are precisely equal to the reported EPS number, a *Specific* forecast will be mechanically inaccurate. As an illustration, if a firm's reported EPS is \$1.23 and the majority of analysts forecast with two digits and precisely at \$1.23, the *Specific* analyst who forecasts at \$1.235 has to be less accurate.

We address this potential concern by forcing all *Specific* forecasts to have the same number of digits as the mode forecast using two alternative approaches. First, we truncate the additional digits in *Specific* forecasts so that these forecasts have the same number of digits as the mode forecast. We then repeat our main regression analyses on this subsample. The results in Table 11 Panel A show that (truncated) *Specific* forecasts are less accurate than non-*Specific* forecasts. The coefficient on *SpecificMode* (−0.021, $t = -4.71$) is negative and significant in Column (1) for the full sample. This effect manifests for non-negative forecasts (Column (2)) but not for negative forecasts (Column (3)), consistent with our baseline findings.

Second, we round all *Specific* forecasts to the nearest digit as the mode forecast. For example, if the mode forecast has two digits, we round a *Specific* forecast in the form of X.123 (X.127) to X.12 (X.13), again removing additional digits for *Specific* forecasts. The results in Columns (4)–(6) of Table 11 Panel A confirm that (rounded) *Specific* forecasts are less accurate than non-*Specific* forecasts. The coefficient on *SpecificMode* (−0.008, $t = -2.35$) is negative and significant in Column (4) for the full sample. This effect again holds for non-negative forecasts (Column (5)) but not for negative forecasts (Column (6)), consistent with our earlier evidence. Collectively, we conclude that our findings that *Specific* forecasts are less accurate are not attributable to the additional digits.[32]

4.5.4 Specific forecasts with too many digits

Table 11 Panel B presents the matrix of distributions for the number of forecast digits and the mode number of digits for each firm-year. The majority of *Specific* forecasts have no more than two digits ((235 + 12,082)/18,930 = 65.07%). The remaining

[32] It is difficult to fully rule out the possibility that some *Specific* forecasts may result from analysts' carelessness in constructing and submitting forecasts. Whether analysts intentionally or unintentionally choose not to round the additional digits of *Specific* forecasts is unobservable to researchers. However, the finding that the majority of *Specific* forecasts have no more than two digits, and the subsequent robustness analyses of alternatively rounding *Specific* forecasts with more than two digits, should mitigate this concern. Collectively, we argue that our empirical evidence points to the existence of analysts' overprecision bias.

Table 11 Additional analyses

Panel A: Additional digits removed							
	Dependent Variable = *ForAccuracy*						
	Additional digits truncated			Additional digits rounded			
	Full Sample	Non-negative Forecasts	Negative Forecasts	Full Sample	Non-negative Forecasts	Negative Forecasts	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	
SpecificMode	−0.021***	−0.021***	−0.016	−0.008**	−0.007**	−0.008	
	(−4.71)	(−4.46)	(−1.41)	(−2.35)	(−2.19)	(−0.71)	
Control Variables	Included	Included	Included	Included	Included	Included	
Adjusted R^2	0.178	0.183	0.131	0.178	0.183	0.132	
Observations	388,863	356,813	32,050	388,845	356,793	32,052	
Panel B: Distribution by mode digits and forecast digits							
Mode Digit	Firm-Years	Forecasts	*Specific* Forecasts	By *Specific* Forecasts' Decimal Places			
				1	2	3	4
0	223	1938	880	235	635	6	4
1	3114	30,835	11,726	0	11,447	152	127
2	36,392	350,201	6269	0	0	5168	1101
3	379	2906	55	0	0	0	55
4	415	3587	0	0	0	0	0
Total	40,523	389,467	18,930	235	12,082	5326	1287
Panel C: Forecast specificity and forecast accuracy – robustness to rounding multiple-digit forecasts							
	Dependent Variable = *ForAccuracy*						
Variables	Full Sample		Non-negative Forecasts		Negative Forecasts		
	(1)		(2)		(3)		
SpecificMode	−0.018***		−0.017***		−0.015		
	(−4.83)		(−4.75)		(−1.37)		

Table 11 (continued)

Control Variables	Included	Included		Included	
Adjusted R^2	0.179	0.184		0.132	
Observations	388,649	356,673		31,976	
Panel D: Market reactions to *Specific* forecasts – robustness to rounding multiple-digit forecasts					
		CAR [−1, 1]	*CAR* [2, 20]		
Variables		(1)	(2)		
REV		0.280***	0.116**		
		(2.96)	(2.29)		
REV × *SpecificMode*		−0.413***	−0.107**		
		(−6.16)	(−2.26)		
Control of Characteristics		Included	Included		
Control of *REV* × Characteristics		Included	Included		
Year Fixed Effects		Included	Included		
Adjusted R^2		0.028	0.010		
Observations		388,649	388,649		
Panel E: Overprecision and forecast accuracy – brokerage house fixed effect analyses					
	Dependent Variable = *ForAccuracy*				
Variables	Full Sample	Non-negative Forecasts			Negative Forecasts
	(1)	(2)			(3)
SpecificMode	−0.015***	−0.015***			−0.017
	(−3.97)	(−3.93)			(−1.31)
Other Controls	Included	Included			Included
Broker House Fixed Effects	Included	Included			Included
Adjusted R^2	0.179	0.186			0.136
Observations	389,467	357,410			32,057

Table 11 (continued)

Panel F: Addressing the effect of management forecasts

	Dependent Variable = *ForAccuracy*		
	Subsample without Management Forecasts	Analyst Forecast Specificity *Same As* Management Forecasts	include industry and year fixed Analyst Forecast Specificity *Different from* Management Forecasts
Variables	(1)	(2)	(3)
SpecificMode	−0.018***	−0.024**	−0.023***
	(−4.02)	(−2.43)	(−3.02)
Control Variables	Included	Included	Included
Adjusted R^2	0.181	0.170	0.187
Observations	267,974	76,452	45,041

Panel G: Market sentiment and analyst overprecision

	Dependent Variable = *ForAccuracy*		
	BadTime = 0	*BadTime = 1*	Full Sample
Variables	(1)	(2)	(3)
SpecificMode	−0.016***	−0.022***	−0.016***
	(−4.00)	(−3.25)	(−3.93)
BadTime	–	–	−0.001
	–	–	(−0.27)
SpecificMode × *BadTime*	–	–	−0.007
	–	–	(−1.01)
Control Variables	Included	Included	Included
Adjusted R^2	0.182	0.177	0.179

Table 11 (continued)

Panel H: Robustness analyses excluding team forecasts

	Dependent Variable = *ForAccuracy*					
Variables	Excluding analysts with more than 30 firm coverage in a given fiscal year			Excluding analysts with more than 50 firm coverage in a given fiscal year		
	(1)			(2)		
SpecificMode	−0.017***			−0.019***		
	(−4.17)			(−4.88)		
Control Variables	Included			Included		
Adjusted R^2	0.179			0.179		
Observations	351,497			379,180		

Panel I: Forecasting task complexity and *Specific* forecasts

	Dependent Variable = *MoreSpecific%*					
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Size	0.007***	0.007***	0.008***	0.007***	0.008***	0.009***
	(6.03)	(5.94)	(6.19)	(6.09)	(5.99)	(6.71)
LnNumSegment	0.004***	0.004***	0.005***	0.004***	0.005***	0.004***
	(2.89)	(2.90)	(3.09)	(3.01)	(3.16)	(3.00)
MissGuidance	–	−0.000	–	–	−0.000	−0.000
	–	(−0.01)	–	–	(−0.10)	(−0.15)
Volatility	–	–	0.047***	–	0.046***	0.046****
	–	–	(3.77)	–	(3.31)	(3.44)
ISHolding	–	–	–	−0.006	−0.004	−0.001
	–	–	–	(−1.19)	(−0.74)	(−0.10)

Table 11 (continued)

LnNumAnalyst	–	–	–	–	–	−0.007***
	–	–	–	–	–	(−3.43)
Year Fixed Effects	Included	Included	Included	Included	Included	Included
Industry Fixed Effects	Included	Included	Included	Included	Included	Included
Adjusted R^2	0.102	0.102	0.103	0.102	0.103	0.104
Observations	39,766	39,766	39,766	39,766	39,766	39,766

This table presents results of additional analyses regarding the association between analyst forecast form specificity and forecast accuracy. In Columns (1)–(3) of Panel A, we truncate additional digits of *Specific* forecasts that exceed the mode number of digits; in Columns (4)–(6) of Panel A, we round *Specific* forecasts to the mode number of digits. Sample size decreases because, with transformed forecasts, some firm-years have homogeneous forecasts. As our computation of *ForAccuracy* requires the maximum forecast and the minimum forecast to be different, forecasts for these firm-years are now excluded. Panel B presents the matrix distribution of our total 389,467 analyst forecasts by the mode number of digits for each firm-year (Mode Digit). It reports the number of firm-years, the number of analyst forecasts, and the number of *Specific* forecasts (in total or categorized by the decimal place). Panel C and Panel D report the results of robustness analyses with forecasts with more than two digits rounded to the second digit after the decimal point. Using this transformed dataset, Panel C estimates the association between forecast specificity and forecast accuracy, and Panel D estimates stock price reactions to forecast revisions during the [−1,1] event window and the [2, 20] delayed window. Panel E controls for brokerage house fixed effects. Panel F examines the potential effect of management forecasts. Column (1) excludes observations for firm-years with management forecasts (point or range forecasts). Column (2) includes analyst forecasts with specificity (number of digits) equal to that of management forecasts for the firm-year. Column (3) includes analyst forecasts with specificity (number of digits) different from that of management forecasts for the firm-year. Panel G splits sample observations into a subsample of *bad times* (*BadTime* = 1) and a subsample of other periods (*BadTime* = 0). We follow Loh and Stulz (2018) and identify the following periods as *bad times*: (1) crisis periods, (2) NBER recessions, and (3) periods of high policy uncertainty. Panel H excludes forecasts made by analysts covering more than 30 (Column (1)) or 50 (Column (2)) firms in a given year. Panel I performs firm-year-level analyses. The dependent variable *MoreSpecific%* is defined as the ratio of the number of *Specific* forecasts to the total number of forecasts issued by analysts following the firm during the year. In all panels (except for Panel B and Panel I), the dependent variable *ForAccuracy* measures analyst *i*'s forecasting performance relative to that of peer analysts following the same firm-year. *SpecificMode* is an indicator that equals 1 if an analyst's EPS forecast for a firm-year has more digits than the most frequent number of digits (mode) of analysts' forecasts for the same firm-year, and 0 otherwise. Our sample period covers fiscal years from 1986 to 2015. *t*-statistics (in parentheses) are based on robust standard errors that are clustered by analyst and year. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels using two-tailed student *t*-tests, respectively. Detailed variable definitions are provided in Appendix 2

34.93% of *Specific* forecasts have three or four digits after the decimal point. The latter group requires additional attention because analysts who are conforming to corporate reporting forms may round such forecasts to the second digit after the decimal point in their analyst reports. Our reading of several randomly selected analyst reports corresponding to observations of *Specific* forecasts confirms that *Specific* forecasts with no more than two digits appear with the same value in analyst reports, while those with more than two digits are rounded to the second digit.

Notably, investors subscribing to I/B/E/S (which indeed is widely subscribed to by practitioners such as money managers) can observe and react to the I/B/E/S forecasts (Ertimur et al. 2011).[33] Nonetheless, the observation of analysts rounding forecasts with more than two digits in their analyst reports raises the concern that some investors may only have access to, or rely on, the rounded values of *Specific* forecasts with more than two digits in the reports.[34] To further alleviate this concern, we perform additional analyses by transforming the analyst forecast data through rounding all forecasts with more than two digits to their second digit (accounting for 2.97% of our total sample forecasts). With the transformed data, we repeat our main analyses related to the forecast accuracy of, and investors' reactions to, *Specific* forecasts. Panels C and D of Table 11 report empirical results that are both economically and statistically similar to those presented earlier in Tables 2 and 8, respectively.

4.5.5 Overprecision and forecast accuracy – Controlling for brokerage house fixed effects

Earlier in Section 4.2.2, we noted that *Specific* forecasts may result from brokerage house effects. We alleviate this concern by showing that, after controlling for brokerage house fixed effects, analyst attributes (experience, boldness, and forecasting ability) continue to explain the analysts' tendency to issue *Specific* forecasts (as in Table 6). Here, we further corroborate this result by examining whether *Specific* forecasts are less accurate after controlling for brokerage house fixed effects.

Empirically, we augment Eq. (1) by adding brokerage house fixed effects. Econometrically, such a test retains the within-brokerage variations in forecast accuracy and overprecision, but controls for potential forecasting model heterogeneity across brokerage houses. In Table 11 Panel E, we find a significantly negative coefficient on *SpecificMode* (−0.015, $t = -3.97$), an estimate that is economically and statistically similar with the estimate in our main specification.

4.5.6 The potential role of management forecasts in shaping analyst forecasts

It is possible that analysts, when issuing forecasts, are affected by both the form and the value of management forecasts, which raises a correlated omitted variable concern (Bamber et al. 2010). To address this concern, we collect data on management

[33] As per the statistics provided by the Corporate Financial Institute (CFI), I/B/E/S serves a client base of 50,000 money managers (https://corporatefinanceinstitute.com/resources/data/bloomberg/ibes/).

[34] We thank an anonymous reviewer for pointing out this issue.

forecasts from the I/B/E/S Guidance database and perform additional analyses. To begin with, our primary sample of 389,467 firm-analyst-year observations corresponds to 40,523 unique firm-years. Matching these firm-years to the management forecast data, we find that 121,493 (31.19%) firm-analyst-year observations have matched management forecasts (point or range) for annual EPS.

We exclude these 121,493 observations from this analysis to ensure that the form of analyst forecasts is not influenced by management forecasts. We re-estimate our main regression (Eq. 1) and report the results in Panel F Column (1) of Table 11. The coefficient on *SpecificMode* continues to be negative and significant (−0.018, $t=-4.02$).[35]

Next, we focus on the 121,493 observations of firm-years with matched management forecasts. We divide these observations into two groups: (1) 76,452 analyst forecasts with specificity (number of digits) equal to that of the management forecasts,[36] and (2) 45,041 analyst forecasts with specificity different from that of the management forecasts. The former (latter) subgroup comprises analyst forecasts that are more (less) likely to be influenced by the form of management forecasts. Re-estimating our main regression, we find negative and significant coefficients on *SpecificMode* for both subsamples (−0.024, $t=-2.43$ in Column (2); −0.023, $t=-3.02$ in Column (3)). Collectively, the empirical evidence suggests that the association between overprecision and forecast accuracy is not explained by management forecasts.

4.5.7 Market sentiment and behavioral biases of analysts

Does the overprecision bias become more or less pronounced in periods of "bad times"? We address the potential interaction between market sentiment and analysts' behavioral biases. Two countervailing forces exist. First, Loh and Stulz (2018) find that analysts' research is more valuable during bad times. Second, using a different context (investors), Zhang (2006) contends that behavioral biases manifest more during periods of high uncertainty.

Empirically, we follow Loh and Stulz (2018) and identify the following periods as *bad times*: (1) the crisis periods of September–November 1987 (1987 market crash crisis), August–December 1998 (LTCM crisis), and July 2007–March 2009 (credit crisis); (2) the NBER recessions in July 1990–March 1991, March–November 2001, and December 2007–July 2009; and (3) the periods of high policy uncertainty, as indicated by the Baker et al. (2016) index (i.e., when the index is in the top tercile during our sample period). We note that the three categories above have overlapping periods of *bad times*. We then form a "*BadTime* = 1" subsample, which includes observations that fall in any of the above periods. The remaining observations form the "*BadTime* = 0" subsample.

[35] The management forecast data are available in the I/B/E/S Guidance database from 1992, with fewer than 100 observations prior to fiscal year 1995. To alleviate the concern that the results are driven by the unmatched sample in early years, we repeat the analyses in Column (1) using unmatched observations in or after fiscal year 1995. We find that the coefficient of *SpecificMode* remains significantly negative (−0.029, $t=-4.50$).

[36] For range forecasts, we use the greater number of digits of the upper and the lower bound point forecast as management forecast specificity. For multiple forecasts, we use the maximum specificity of the forecasts in the firm-year.

We estimate our regression model separately for the two subsamples. The results in Panel G indicate that the negative association between overprecision and forecast accuracy holds for both subsamples (−0.016, $t=-4.00$ for $BadTime=1$ in Column (1); −0.022, $t=-3.25$ for $BadTime=0$ in Column (2)). Furthermore, we estimate a single regression that adds the *BadTime* indicator and the interaction variable *SpecificMode*×*BadTime* to our main regression model. We find an insignificant coefficient on the interaction term in Column (3). In brief, our finding that overprecise forecasts are less accurate holds in periods of varying market sentiments.

4.5.8 Excluding team forecasts

Using a hand-collected sample of analyst forecast reports over the period 2013–2016, Fang and Hope (2021) find that many forecasts are issued by analyst teams and that team forecasts are more accurate than individual forecasts. In our context, one might argue that team forecasts are less applicable to our investigation of individuals' overprecision bias. We re-estimate our main regression in Table 2 after excluding observations for analysts who cover more than 30 (or alternatively 50) firms in a given fiscal year (Kaustia and Rantala 2015). In Table 11 Panel H, we continue to find significantly negative coefficients on *SpecificMode*.

4.5.9 Forecasting task complexity

Because behavioral biases may be more pronounced for firms that are harder to value, in this section we explore whether firms with greater business complexity exhibit more *Specific* forecasts. Before we proceed, we note that such a test may have limited power because our primary construct of overprecision (*SpecificMode*) relies upon intra-firm comparison in analysts' forecasts (i.e., benchmarked against peer analysts following the same firm). Nonetheless, we attempt to shed some light on this relevant issue.

Empirically, we construct a firm-year-level variable, *MoreSpecific%*, defined as the ratio of the number of *Specific* forecasts to the total number of forecasts issued by analysts following the firm during the year. For independent variables, we follow Dechow and You (2012) and use the two proxies of firm complexity, namely firm size (*Size*) and the number of business segments (*LnNumsegment*). In addition, we control for three proxies for firm-level uncertainty: no management forecasts (*MissGuidance*), stock return volatility (*Volatility*), and institutional ownership (*ISHolding*). Detailed variable definitions appear in Appendix 2. We include industry and year fixed effects and cluster standard errors by firm and year.

Table 11 Panel I reports the regression results. Across all specifications, we find positive and significant coefficients on *Size* and *LnNumsegment*. These results are consistent with the notion that firms of greater size or with more segments have greater business complexity (Dechow and You 2012), which induces more analysts to exhibit the overprecision bias. There is evidence that high volatility firms also exhibit more *Specific* forecasts. However, the coefficients on *MissGuidance* and *ISHoldings* are insignificant. Lastly, we control for a firm's analyst coverage (*LnNumAnalyst*), suggested by Fang and Hope (2021) as another candidate for uncertainty (or "task complexity"). Column (6) shows that firms with greater analyst coverage exhibit a lower percentage of *Specific* forecasts.

5 Conclusion

In this study, we identify a novel behavioral bias of sell-side analysts – overprecision. Overprecision has been identified as the most durable and least understood form of overconfidence (Moore et al. 2016). Using sell-side analysts' forecasts to examine the properties of overprecise forecasts, we document that forecasts that have more digits than the mode are less accurate, despite their form precision. We find that these forecasts exhibit a Dunning-Kruger effect, i.e., that incompetent (proxied by inexperience) analysts over-rely on their financial model output when producing overprecise forecasts. Lastly, we show that the stock market appears to recognize that these overprecise forecasts are less informative.

An emerging literature incorporates psychology research into financial decision-making by investors and intermediaries (Hilary and Menzly 2006; Hribar and McInnis 2012; Hirshleifer et al. 2018). Similarly, we explore individuals' overprecision in the context of sell-side analyst forecasts. By doing so, we challenge the conventional wisdom in the finance and accounting literatures that numbers that appear less precise imply higher uncertainty and less information-processing and, therefore, lower accuracy (e.g., Bradley et al. 2004; Herrmann and Thomas 2005; Kuo et al. 2015). The results of our study add to those findings and deepen our understanding of the link between the form and the substance of earnings forecasts. By examining the relationship between forecast specificity and forecast accuracy, we are able to provide direct evidence on the existence, causes, and consequences of overprecision in the financial market. This evidence validates the implicit assumption in existing studies that agents can be subject to overprecision (Daniel et al. 1998; Odean 1998; Barber and Odean 2000; Adebambo and Yan 2018).

We conclude our study with a discussion of potential future research. An individual's overprecision bias, like other behavioral biases, emerges when judgments are made under uncertainty (Tversky and Kahneman 1974). The capital market is close to a natural laboratory for such a scenario. Aside from our setting of analyst forecasts, there are other relevant contexts, such as the issuance of management forecasts, investors determining bidding prices for equity offerings, and contractual parties setting targets/benchmarks/covenants that include the necessary ingredients to explore the overprecision bias (uncertainty, judgment manifested in numerical output, and sufficient peer observations as benchmarks). We believe that future research exploiting these contexts would bring valuable evidence to advance our understanding of individuals' overprecision bias in the capital market.

Appendix 1 Illustrations of *Specific* forecasts

In this appendix, we illustrate our definition of *SpecificMode* using examples of original analyst forecast data retrieved from I/B/E/S. We discuss two companies – Monsanto Company in Illustration 1 and Wynn Resorts in Illustration 2. Both examples explain our definition of *SpecificMode*. The main distinction between the two examples is that analyst forecasts for Wynn Resorts also include forecasts that end with less digits than the mode number of digits after the decimal point. Both examples are included in our empirical sample.

Illustration 1 Monsanto Company

Monsanto Company (ticker: MNO) is a company in the industry of agricultural chemicals. The company was founded in 2000 and is based in Saint Louis, Missouri. It provides agricultural products such as corn, soybean, and vegetable seeds.

Table 12 shows original analyst forecasts for the annual earnings per share (EPS) of Monsanto Company during the fiscal year ending on August 31, 2015. The table presents information on the company name, the fiscal year end date, the forecast date, the analyst ID in I/B/E/S, the original forecast value (*Forecast*), the realized EPS value (*Actual EPS*), and the value of the indicator for *Specific* forecasts (*SpecificMode*). For each analyst following the company, we retain the analyst's latest forecast within the [−365, −30] window before the fiscal year end date. We sort these observations by analyst forecast value.

Out of the 20 forecasts (provided by the 20 analysts), 17 end with two digits after the decimal point and three end with three digits after the decimal point (issued by analysts 110,142, 155,176, and 5469). The mode number of digits of analyst forecasts for Monsanto's 2015 EPS therefore equals two. The three forecasts ending with three digits after the decimal point are identified as *Specific* forecasts and have *SpecificMode* = 1.

Illustration 2 Wynn Resorts

Wynn Resorts Ltd. (ticker: WYNN) is a company in the industry of Resorts & Casinos. The company was founded in 2002 and is based in Las Vegas, Nevada. It owns and operates high end hotels and casino resorts.

Table 13 shows original analyst forecasts for the annual earnings per share (EPS) of Wynn Resorts during the fiscal year ending on December 31, 2015. Information presented in the table is similar to that in Table 12 for the Monsanto Company. We again sort observations by analyst forecast value.

Out of the 20 forecasts, 14 end with two digits after the decimal point. The mode number of digits after the decimal point therefore equals two. There are three forecasts ending with three digits after the decimal point, issued by analysts 118,417, 86,522, and 132,128. These three forecasts are identified as *Specific* forecasts and have *SpecificMode* = 1. The remaining three forecasts have less digits after the decimal point, compared with the mode (analysts 81,050, 109,229, and 124,805). In our main analyses, these three rounded forecasts are pooled with the 14 forecasts ending with two digits after the decimal point, and benchmarked against our *Specific* forecasts.

Table 12 Analyst forecasts for Monsanto Company

Company Name	Fiscal Period	Forecast Date	Analyst ID	EPS Forecast	Actual EPS	*SpecificMode*
Monsanto Company	31-Aug-15	25-Jun-15	80,169	5.37	5.73	0
Monsanto Company	**31-Aug-15**	**1-Jul-15**	**110,142**	**5.679**	**5.73**	**1**
Monsanto Company	31-Aug-15	24-Jun-15	75,140	5.71	5.73	0
Monsanto Company	31-Aug-15	1-Apr-15	5121	5.75	5.73	0
Monsanto Company	31-Aug-15	22-Jun-15	17,887	5.75	5.73	0
Monsanto Company	31-Aug-15	2-Apr-15	31,998	5.75	5.73	0
Monsanto Company	31-Aug-15	24-Mar-15	92,159	5.75	5.73	0
Monsanto Company	31-Aug-15	7-Apr-15	112,672	5.75	5.73	0
Monsanto Company	31-Aug-15	30-Mar-15	126,902	5.76	5.73	0
Monsanto Company	**31-Aug-15**	**24-Jun-15**	**155,176**	**5.764**	**5.73**	**1**
Monsanto Company	31-Aug-15	25-Jun-15	75,323	5.77	5.73	0
Monsanto Company	31-Aug-15	24-Jun-15	85,800	5.77	5.73	0
Monsanto Company	31-Aug-15	24-Jun-15	10,576	5.78	5.73	0
Monsanto Company	31-Aug-15	29-Jun-15	20,068	5.78	5.73	0
Monsanto Company	31-Aug-15	24-Jun-15	127,749	5.78	5.73	0
Monsanto Company	31-Aug-15	24-Jun-15	135,384	5.78	5.73	0
Monsanto Company	31-Aug-15	1-Apr-15	121,249	5.79	5.73	0
Monsanto Company	31-Aug-15	24-Jun-15	45,654	5.81	5.73	0
Monsanto Company	31-Aug-15	17-Jun-15	50,768	5.85	5.73	0
Monsanto Company	**31-Aug-15**	**2-Apr-15**	**5469**	**5.853**	**5.73**	**1**

Table 13 Analyst forecasts for Wynn Resorts

Company Name	Fiscal Period	Forecast Date	Analyst ID	EPS Forecast	Actual EPS	*SpecificMode*
Wynn Resorts Ltd.	31-Dec-15	16-Oct-15	111,196	1.39	3.44	0
Wynn Resorts Ltd.	31-Dec-15	16-Oct-15	70,991	1.52	3.44	0
Wynn Resorts Ltd.	**31-Dec-15**	**15-Oct-15**	**118,417**	**2.742**	**3.44**	**1**
Wynn Resorts Ltd.	31-Dec-15	15-Oct-15	145,620	2.78	3.44	0
Wynn Resorts Ltd.	31-Dec-15	15-Oct-15	43,475	2.82	3.44	0
Wynn Resorts Ltd.	31-Dec-15	16-Oct-15	31,604	2.84	3.44	0
Wynn Resorts Ltd.	31-Dec-15	16-Oct-15	105,593	2.93	3.44	0
Wynn Resorts Ltd.	**31-Dec-15**	**23-Nov-15**	**86,522**	**2.934**	**3.44**	**1**
Wynn Resorts Ltd.	31-Dec-15	23-Nov-15	118,385	2.94	3.44	0
Wynn Resorts Ltd.	**31-Dec-15**	**3-Sep-15**	**132,128**	**2.941**	**3.44**	**1**
Wynn Resorts Ltd.	31-Dec-15	21-Oct-15	10,423	2.95	3.44	0
Wynn Resorts Ltd.	31-Dec-15	6-Nov-15	80,936	2.95	3.44	0
Wynn Resorts Ltd.	**31-Dec-15**	**15-Oct-15**	**81,050**	**3**	**3.44**	**0**
Wynn Resorts Ltd.	31-Dec-15	16-Oct-15	108,071	3.02	3.44	0
Wynn Resorts Ltd.	31-Dec-15	16-Oct-15	112,481	3.11	3.44	0
Wynn Resorts Ltd.	31-Dec-15	31-Jul-15	45,796	3.12	3.44	0
Wynn Resorts Ltd.	**31-Dec-15**	**4-Sep-15**	**109,229**	**3.2**	**3.44**	**0**
Wynn Resorts Ltd.	**31-Dec-15**	**15-Oct-15**	**124,805**	**3.2**	**3.44**	**0**
Wynn Resorts Ltd.	31-Dec-15	28-Oct-15	18,610	3.22	3.44	0
Wynn Resorts Ltd.	31-Dec-15	1-May-15	135,242	4.39	3.44	0

Appendix 2. Variable definitions

Variables	Definitions
SpecificMode$_{ijt}$	An indicator that equals 1 if analyst *i*'s forecast for firm *j* in fiscal year *t* has more digits after the decimal than the mode number of digits of peer analysts' forecasts for the same firm-year, and 0 otherwise.
SpecificEps$_{ijt}$	An indicator that equals 1 if analyst *i*'s forecast for firm *j* in year *t* has more digits after the decimal than the actual earnings per share (EPS) of firm *j* in year *t*-1, and 0 otherwise.
SpecificCon$_{ijt}$	The number of overspecified digits compared to the mode number of digits for forecasts for firm *j* in year *t*. For example, if analyst *i*'s forecast for firm *j* ends with three digits and the mode number of digits equals one, *SpecificCon* equals two.
SpecificMedian$_{ijt}$	An indicator that equals 1 if analyst *i*'s forecast for firm *j* in fiscal year *t* has more digits after the decimal than the median number of digits of analyst forecasts for the same firm-year, and 0 otherwise.
ForAccuracy$_{ijt}$	Analyst forecast accuracy, defined as the maximum absolute forecast error (*AFE*) of forecasts for firm *j* in year *t* minus analyst *i*'s *AFE* for firm *j* in year *t*, with this difference scaled by the range between maximum and minimum *AFE*s of forecasts for firm *j* in year *t* (Eq. 2). Absolute forecast error, AFE_{ijt}, is defined as the absolute value of firm *j*'s year *t* earnings minus analyst *i*'s forecast of firm *j*'s year *t* earnings.
LagForAccuracy$_{ij,t\text{-}1}$	Analyst *i*'s forecast accuracy (*ForAccuracy*) for firm *j* in year *t*-1. *ForAccuracy* is defined as above.
Bold$_{ijt}$	Analyst forecast boldness, constructed through Eq. (3) with *Char_Raw* defined as the distance between analyst *i*'s forecast for firm *j* in year *t* from the pre-revision (year-to-date) consensus forecast for firm *j* in year *t*. Year-to-date consensus is computed using forecasts issued within 90 days prior to the current forecast revision (Clement and Tse 2005).
BrokerSize$_{it}$	An indicator that equals 1 if analyst *i* is employed by a brokerage firm in the top decile in terms of the number of analysts employed during year *t*, and 0 otherwise.
GenExp$_{it}$	An analyst's general experience, constructed through Eq. (3) with *Char_Raw* defined as the number of years that analyst *i* has issued forecasts for any firm until year *t*.
FirmExp$_{ijt}$	An analyst's firm-specific experience, constructed through Eq. (3) with *Char_Raw* defined as the number of years that analyst *i* has followed firm *j* until year *t*.
Industries$_{it}$	The number of industries followed by an analyst, constructed through Eq. (3) with *Char_Raw* defined as the number of industries followed by analyst *i* in year *t*. We determine a firm's industry classification through its two-digit SIC code.
Companies$_{it}$	The number of companies followed by an analyst, constructed through Eq. (3) with *Char_Raw* defined as the number of companies followed by analyst *i* in year *t*.
ForFrequency$_{ijt}$	Analyst forecasting frequency, constructed through Eq. (3) with *Char_Raw* defined as the number of forecasts issued by analyst *i* for firm *j* in year *t*.
DaysElapsed$_{ijt}$	The number of days elapsed since the last forecast for firm *j* in year *t*, constructed through Eq. (3) with *Char_Raw* defined as the number of days between analyst *i*'s forecast for firm *j* in year *t* and the most recent preceding forecast by any analyst following firm *j* in year *t*.
ForHorizon$_{ijt}$	Forecasting horizon, constructed through Eq. (3) with *Char_Raw* defined as the number of days between analyst *i*'s forecast for firm *j* in year *t* and the fiscal year end date.

Appendix 2 (continued)

Variables	Definitions
$FyeDis_{ijt}$	The distance of an analyst's forecast from the consensus forecast, constructed through Eq. (3) with *Char_Raw* defined as the absolute difference between analyst *i*'s forecast for firm *j* from the fiscal-year-end consensus forecast in year *t*.
$AvgSpecific_{it}$	The proportion of *Specific* forecasts to total forecasts issued by analyst *i* during fiscal year *t*.
$Avg\vert LagForError\vert_{it}$	The mean of analyst *i*'s scaled absolute forecast errors (*AFE*) for all covered firms during fiscal year *t*-1. The scaled absolute forecast error is defined as the absolute value of firm *j*'s year *t*-1 earnings minus analyst *i*'s forecast of firm *j*'s year *t*-1 earnings, scaled by share price at the end of fiscal year *t*-1.
$CAR[-1, 1]_{ijt}$	The cumulative market-adjusted abnormal return of firm *j* during the three-day event window around analyst *i*'s forecast revision (Day 0) in fiscal year *t*.
$CAR[2, 20]_{ijt}$	The cumulative market-adjusted abnormal return of firm *j* during the prolonged reaction period from Day 2 to Day 20 after analyst *i*'s forecast revision (Day 0) in fiscal year *t*.
REV_{ijt}	Analyst *i*'s forecast revision for firm *j* in year *t*, computed as analyst *i*'s revised forecast for firm *j* in year *t* less analyst *i*'s prior forecast for firm *j* in year *t*, divided by firm *i*'s stock price two days prior to the forecast revision.
$SpecificMode\_Corrected_{ijt}$	An indicator that equals 1 if analyst *i*'s *Specific* forecast revision for firm *j* during year *t* is less accurate than peer analysts' subsequent non-*Specific* forecasts issued during the [2, 20] window, and 0 otherwise.
$SpecificMode\_NonCorrected_{ijt}$	An indicator that equals 1 if analyst *i*'s *Specific* forecast revision for firm *j* during year *t* is equally or more accurate than peer analysts' subsequent non-*Specific* forecasts issued during the [2, 20] window, and 0 otherwise.
$BadTime_t$	An indicator that equals 1 if an observation falls into any of the following periods: (1) the crisis periods of September–November 1987 (1987 market crash crisis), August–December 1998 (LTCM crisis), and July 2007–March 2009 (credit crisis); (2) the NBER recessions in July 1990–March 1991, March–November 2001, and December 2007–July 2009; and (3) the periods of high policy uncertainty, as indicated by the Baker et al. (2016) index (i.e., when the index is in the top tercile during our sample period), and 0 otherwise.
$MoreSpecific\%_{jt}$	The ratio of the number of *Specific* forecasts to the total number of forecasts issued by analysts following firm *j* during fiscal year *t*.
$Size_{jt}$	The natural logarithm of the book value of total assets of firm *j* at the end of fiscal year *t*.
$Volatility_{jt}$	The ratio of idiosyncratic volatility to total volatility of firm *j* during fiscal year *t*. Idiosyncratic volatility is the standard deviation of abnormal daily returns, obtained from estimating the Fama-French three factor model, during fiscal year *t*. Total volatility is the standard deviation of daily raw returns during fiscal year *t*.
$LnNumsegment_{jt}$	The natural logarithm of one plus the number of business segments of firm *j* during fiscal year *t*.
$MissGuidance_{jt}$	An indicator that equals 1 if firm *j* does not have management forecasts during fiscal year *t*, and 0 otherwise.
$ISHolding_{jt}$	The percentage of common shares of firm *j* held by institutional investors at the end of fiscal year *t*.
$LnNumAnalyst_{jt}$	The natural logarithm of 1 plus the number of analysts following firm *j* during fiscal year *t*.

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1007/s11142-022-09724-x.

Acknowledgements We thank Patricia Dechow (the editor), two anonymous referees, Daniel Bradley, Rong Huang, Zhongwei Huang, Oliver Zhen Li, Yupeng Lin, Shuqing Luo, Xiumin Martin, Roni Michaely, Rui Shen, Hongping Tan (CICF discussant), Huifang Yin, Frank Zhang, Huai Zhang, Yuan Zhang, and seminar participants at the 2019 China International Conference in Finance and Nankai University for their helpful comments. This article is supported by the National Natural Science Foundation of China (Grant numbers 71772110, 71972039, 72172037), the MOE Project of Key Research Institute of Humanities and Social Science in University (18JJD790010), and the Fundamental Research Funds for the Central Universities in UIBE (CXTD13-01). We retain responsibility for any remaining errors. This article has been previously circulated under the title "*Specificity and Accuracy: Evidence from Sell-side Analyst Forecasts.*"

References

Abarbanell, J., W. Lanen, and R. Verrecchia. 1995. Analysts' forecasts as proxies for investor beliefs in empirical research. *Journal of Accounting and Economics* 20 (1): 31–60.

Abdel-Meguid, A., J. Jennings, K. Olsen, and M. Soliman. 2021. The impact of the CEO's personal narcissism on non-GAAP earnings. *The Accounting Review* 96 (3): 1–25.

Adebambo, B., and X. Yan. 2018. Investor confidence, firm valuation, and corporate decisions. *Management Science* 64 (11): 5349–5369.

Akbas, F., S. Markov, M. Subasi, and E. Weisbrod. 2018. Determinants and consequences of information processing delay: Evidence from the Thompson Reuters institutional brokers' estimate system. *Journal of Financial Economics* 127 (2): 366–388.

Baber, W., and S. Kang. 2002. The impact of split adjusting and rounding on analysts' forecast error calculations. *Accounting Horizons* 16 (4): 277–289.

Baker, S., N. Bloom, and S. Davis. 2016. Measuring economic policy uncertainty. *The Quarterly Journal of Economics* 131 (4): 1593–1636.

Bamber, L., K. Hui, and P. Yeung. 2010. Managers' EPS forecasts: Nickeling and diming the market? *The Accounting Review* 85 (1): 63–95.

Barber, B., and T. Odean. 2000. Trading is hazardous to your wealth: The common stock investment performance of individual investors. *The Journal of Finance* 55 (2): 773–806.

Baumann, A., R. Deber, and G. Thompson. 1991. Overconfidence among physicians and nurses: The 'micro-certainty, macro-uncertainty' phenomenon. *Social Science & Medicine* 32 (2): 167–174.

Bilinski, P., D. Lyssimachou, and M. Walker. 2013. Target price accuracy: International evidence. *The Accounting Review* 88 (3): 825–851.

Bradley, D., J. Cooney, B. Jordan, and A. Singh. 2004. Negotiation and the IPO offer price: A comparison of integer vs. non-integer IPOs. *Journal of Financial and Quantitative Analysis* 39 (3): 517–540.

Bradshaw, M., A. Huang, and H. Tan. 2019. The effects of analyst-country institutions on biased research: Evidence from target prices. *Journal of Accounting Research* 57 (1): 85–120.

Brown, L., A. Call, M. Clement, and N. Sharp. 2015. Inside the "black box" of sell-side financial analysts. *Journal of Accounting Research* 53 (1): 1–47.

Brown, L., A. Call, M. Clement, and N. Sharp. 2016. The activities of buy-side analysts and the determinants of their stock recommendations. *Journal of Accounting and Economics* 62 (1): 139–156.

Burson, K., R. Larrick, and J. Klayman. 2006. Skilled or unskilled, but still unaware of it: How perceptions of difficulty drive miscalibration in relative comparisons. *Journal of Personality and Social Psychology* 90 (1): 60–77.

Christopher, K., P. Padmakumari, and H. Herbert. 2021. Presence or absence of Dunning-Kruger effect: Differences in narcissism, general self-efficacy and decision-making styles in young adults. Current Psychology, forthcoming. Available at https://doi.org/10.1007/s12144-021-01461-9.

Clement, M. 1999. Analyst forecast accuracy: Do ability, resources, and portfolio complexity matter? *Journal of Accounting and Economics* 27 (3): 285–303.

Clement, M., and S. Tse. 2003. Do investors respond to analysts' forecast revisions as if forecast accuracy is all that matters? *The Accounting Review* 78 (1): 227–249.

Clement, M., and S. Tse. 2005. Financial analyst characteristics and herding behavior in forecasting. *The Journal of Finance* 60 (1): 307–341.

Daniel, K., D. Hirshleifer, and A. Subrahmanyam. 1998. Investor psychology and security market under- and overreactions. *The Journal of Finance* 53 (6): 1839–1885.

De Bondt, W., and R. Thaler. 1990. Do security analysts overreact? *American Economic Review* 80 (2): 52–57.

Dechow, P., and H. You. 2012. Analysts' motives for rounding EPS forecasts. *The Accounting Review* 87 (6): 1939–1966.

Easterwood, J., and S. Nutt. 1999. Inefficiency in analysts' earnings forecasts: Systematic misreaction or systematic optimism? *The Journal of Finance* 54 (5): 1777–1797.

Ertimur, Y., W. Mayew, and S. Stubben. 2011. Analyst reputation and the issuance of disaggregated earnings forecasts to I/B/E/S. *Review of Accounting Studies* 16 (1): 29–58.

Fang, B., and O. Hope. 2021. Analyst teams. *Review of Accounting Studies* 26 (2): 425–467.

Fang, L., and A. Yasuda. 2009. The effectiveness of reputation as a disciplinary mechanism in sell-side research. *Review of Financial Studies* 22 (9): 3735–3777.

Farrell, M., C. Green, R. Jame, and S. Markov. 2022. The democratization of investment research and the informativeness of retail investor trading. *Journal of Financial Economics* 145 (2): 616–641.

Frankel, R., S.P. Kothari, and J. Weber. 2006. Determinants of the informativeness of analyst research. *Journal of Accounting and Economics* 41 (1–2): 29–54.

Gleason, C., and C. Lee. 2003. Analyst forecast revisions and market price discovery. *The Accounting Review* 78 (1): 193–225.

Green, T., R. Jame, S. Markov, and M. Subasi. 2014. Access to management and the informativeness of analyst research. *Journal of Financial Economics* 114 (2): 239–255.

Grossman, S., and J. Stiglitz. 1980. On the impossibility of informationally efficient markets. *American Economic Review* 70 (3): 393–408.

Ham, C., M. Lang, N. Seybert, and S. Wang. 2017. CFO narcissism and financial reporting quality. *Journal of Accounting Research* 55 (5): 1089–1135.

Ham, C., N. Seybert, and S. Wang. 2018. Narcissism is a bad sign: CEO signature size, investment, and performance. *Review of Accounting Studies* 23: 234–264.

Herrmann, D., and W. Thomas. 2005. Rounding of analyst forecasts. *The Accounting Review* 80 (3): 805–823.

Hilary, G., and L. Menzly. 2006. Does past success lead analysts to become overconfident? *Management Science* 52 (4): 489–500.

Hirshleifer, D., M. Jian, and H. Zhang. 2018. Superstition and financial decision making. *Management Science* 64 (1): 235–252.

Hong, H., J. Kubik, and A. Solomon. 2000. Security analysts' career concerns and herding of earnings forecasts. *RAND Journal of Economics* 31 (1): 121–144.

Hribar, P., and J. McInnis. 2012. Investor sentiment and analysts' earnings forecast errors. *Management Science* 58 (2): 293–307.

Jacob, J., T. Lys, and M. Neale. 1999. Expertise in forecasting performance of security analysts. *Journal of Accounting and Economics* 28 (1): 51–82.

Jegadeesh, N., and W. Kim. 2010. Do analysts herd? An analysis of recommendations and market reactions. *Review of Financial Studies* 23 (2): 901–937.

Kaustia, M., and V. Rantala. 2015. Social learning and corporate peer effects. *Journal of Financial Economics* 117 (3): 653–669.

Kidd, J. 1970. The utilization of subjective probabilities in production planning. *Acta Psychologica* 34: 338–347.

Kruger, J., and D. Dunning. 1999. Unskilled and unaware of it: How difficulties in recognizing one's own incompetence lead to inflated self-assessments. *Journal of Personality and Social Psychology* 77 (6): 1121–1134.

Kuo, W., T. Lin, and J. Zhao. 2015. Cognitive limitation and investment performance: Evidence from limit order clustering. *Review of Financial Studies* 28 (3): 838–875.

Lee, C., and E. Watts. 2021. Tick size tolls: Can a trading slowdown improve earnings news discovery? *The Accounting Review* 96 (3): 373–401.

Li, M., N. Petruzzi, and J. Zhang. 2017. Overconfident competing newsvendors. *Management Science* 63 (8): 2637–2646.

Loh, R., and R. Stulz. 2018. Is sell-side research more valuable in bad times? *The Journal of Finance* 73 (3): 959–1013.

Luo, S., and N. Nagarajan. 2015. Information complementarities and supply chain analysts. *The Accounting Review* 90 (5): 1995–2029.

Malmendier, U., and D. Shanthikumar. 2014. Do security analysts speak in two tongues? *Review of Financial Studies* 27 (5): 1287–1322.
Moore, D., E. Tenney, and U. Haran. 2016. *Overprecision in Judgment. Wu G, Keren G, eds. Handbook of judgment and decision making: 182–212*. John Wiley & Sons.
Neale, M., and M. Bazerman. 1990. *Cognition and rationality in negotiation*. The Free Press.
Odean, T. 1998. Volume, volatility, price, and profit when all traders are above average. *The Journal of Finance* 53 (6): 1887–1934.
Oskamp, S. 1965. Overconfidence in case-study judgments. *Journal of Consulting Psychology* 29 (3): 261–265.
Park, C., and E. Stice. 2000. Analyst forecasting ability and the stock price reaction to forecast revisions. *Review of Accounting Studies* 5 (3): 259–272.
Payne, J., and W. Thomas. 2003. The implications of using stock-split adjusted I/B/E/S data in empirical research. *The Accounting Review* 78 (4): 1049–1067.
Petersen, M. 2009. Estimating standard errors in financial panel data sets: Comparing approaches. *Review of Financial Studies* 22 (1): 435–480.
Radzevick, J., and D. Moore. 2011. Competing to be certain (but wrong): Market dynamics and excessive confidence in judgment. *Management Science* 57 (1): 93–106.
Ren, Y., and R. Croson. 2013. Overconfidence in newsvendor orders: An experimental study. *Management Science* 59 (11): 2502–2517.
Roger, T., P. Roger, and A. Schatt. 2018. Behavioral bias in number processing: Evidence from analysts' expectations. *Journal of Economic Behavior & Organization* 149: 315–331.
Sloan, R. 1996. Do stock prices fully reflect information in accruals and cash flows about future earnings? *The Accounting Review* 71 (3): 289–315.
Soltes, E. 2014. Private interaction between firm management and sell-side analysts. *Journal of Accounting Research* 52 (1): 245–272.
Stickel, S. 1992. Reputation and performance among security analysts. *The Journal of Finance* 47 (5): 1811–1836.
Tversky, A., and D. Kahneman. 1974. Judgment under uncertainty: Heuristics and biases. *Science* 185 (4157): 1124–1131.
Valentine, J. 2010. *Best practices for equity research analysts: Essentials for buy-side and sell-side analysts*. McGraw-Hill Education.
Wagenaar, W., and K. Keren. 1986. *Does the expert know? The reliability of predictions and confidence ratings of experts. Intelligent Decision Support in Process Environments*, 87–103. Springer.
Weller, B. 2018. Does algorithm trading reduce information acquisition? *Review of Financial Studies* 31 (6): 2184–2226.
Xie, H. 2001. The mispricing of abnormal accruals. *The Accounting Review* 76 (3): 357–373.
Yin, H., and H. Zhang. 2014. Tournaments of financial analysts. *Review of Accounting Studies* 19 (2): 573–605.
Zhang, X.F. 2006. Information uncertainty and stock returns. *The Journal of Finance* 61 (1): 105–137.