Who Herds?*

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December 22, 2004

Abstract

This paper develops a test for herding in forecasts by professional financial analysts that is robust to (a) correlated information amongst analysts; (b) common unforecasted industry-wide earnings shocks; (c) information arrival over the forecasting cycle; (d) the possibility that the earnings that analysts forecast differ from what the econometrician observes; and (e) systematic optimism or pessimism among analysts. We find that forecasts are biased, but that analysts do not herd. Rather, analysts "anti-herd": Analysts systematically issue biased *contrarian* forecasts that overshoot the publicly-available consensus forecast in the direction of their private information. The magnitude of the forecast bias, its systematic variation with analyst following, and the pattern of bias in forecast revisions indicate that the bias is strategically chosen.

*We are grateful to the Institutional Brokers Estimate System (I/B/E/S), a service of I/B/E/S International Inc. for providing data on analyst forecasts. We thank Long Chen, Roger Koenker, Pat O'Brien, and Selim Tepaloglu for their useful comments and suggestions, and Kofi Laing for help with our Perl program. Comments from seminar participants at Yale University, University of Rochester, University of Southern California, University of Colorado, University of Waterloo, University of Illinois, Claremont-McKenna College, University of British Columbia, Simon Fraser University, and the Federal Reserve Bank of Chicago are also appreciated. The usual disclaimer applies.

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1 Introduction

> *"Going against the consensus is the way an analyst makes his or her mark."*
>
> —Steven Garmaise, *Midland Walwyn Capital*

Both the financial press and academic research regularly suggest that security analysts herd toward the consensus forecast, issuing forecasts that under-weight their own information (see, e.g., Trueman (1990, 1994) and Hong, Kubik and Solomon (2000)). These herding stories have arisen because forecasts often seem "too clustered." For example, Gallo, Granger and Jeon (2001) find that forecasts of GDP converge as the date at which GDP is announced draws nearer, but that invariably final forecasts are either uniformly too low or too high. The authors conclude that forecasters herd.

But, clustered forecasts need not imply that analysts herd. First, earlier forecasts may contain valuable information that subsequent analysts incorporate into their forecasts (Welch (2000)). Second, analysts rely on common information sources such as a company's chief financial officer for information. If a CFO tells each analyst the same thing, their forecasts will reflect this common (perhaps mis)information, so that their forecasts will tend all to be too high or too low relative to realized earnings. Indeed, if analysts had identical information and processed it in the same way, then each unbiased analyst would issue the same forecast, which will generally differ from actual earnings. Third, common unanticipated market-wide earnings shocks can cause most, if not all, forecasts to be too low or too high relative to earnings. For instance, the fact that during the 1990-91 recession most forecasts exceeded earnings does *not* imply that forecasts were biased; the decline in stock prices over that period suggests that the earnings fall may have been unanticipated by the market. Fourth, the measure of earnings that analysts forecast may differ from the earnings that the econometrician sees (Keane and Runkle (1998)). For example, analysts may not seek to forecast exceptional items that appear in reported earnings. Fifth, analysts may be systematically optimistic or pessimistic,[1] so that forecasts either tend to exceed or fall short of the consensus, again creating the appearance of herding (see Richardson, Teoh and Wysocki (2003)).

This paper develops tests for herding and other biases in the earnings forecasts issued by professional analysts that are robust to these concerns. In environments with information arrival, an unbiased analyst combines all information at his disposal, and updates to obtain a posterior distribution over earnings. A forecast is unbiased if it corresponds to the analyst's best estimate of earnings given all available information, i.e., if it corresponds to the mean or median of the

[1] E.g., systematic "optimism" can arise due to selective reporting, as pessimistic analysts may be less willing to make their estimates public (McNichols and O'Brien (1998) give various rationales for self-selectivity in analyst reporting).

analyst's posterior distribution over earnings. In its most basic form, herding amounts to biasing a forecast away from an analyst's best estimate, toward the consensus forecast of earlier analysts; while anti-herding amounts to biasing a forecast away from the extant consensus. In what follows, we develop general tests for such biases.

The key insight underlying our tests is simple. For the benchmark case of an unbiased analyst, his forecast equals the median of his posterior of earnings given *all* information at his disposal. It follows immediately that the analyst's forecast should be as likely to exceed realized earnings as to fall short, both unconditionally, and conditional on *anything* in his information set, including the consensus forecast of analysts who have reported earlier. If, instead, an analyst herds, biasing his forecast toward the extant consensus, then his forecast will be located between the consensus and his best estimate of earnings. Hence, if an analyst herds and his forecast exceeds the consensus of earlier analysts, then it should fall short of realized earnings more than half of the time. So, too, when a herding analyst's forecast falls short of the consensus, it should exceed earnings more than half of the time. The opposite outcome is predicted if analysts anti-herd: an analyst who anti-herds issues a forecast that overshoots his best estimate of earnings in the direction away from the consensus.

The crucial feature that these observations share is that no assumptions are made about how an analyst forms his posterior. As a result, our tests for unbiasedness and herding are unaffected by signal correlation or information arrival. Essentially, we estimate two conditional probabilities: the conditional probability that a forecast exceeds realized earnings given that the forecast exceeds the extant consensus forecast (and perhaps given other conditioning information), and the conditional probability that a forecast falls short of earnings given that the forecast falls short of the extant consensus (and perhaps given other conditioning information). To control for possible unforecasted earnings shocks, our test statistic averages these two conditional probabilities: under the null of unbiasedness, an unforecasted earnings shock has offsetting impacts on the frequency with which each overshooting event occurs. So, too, systematic optimism or pessimism has offsetting effects on the two conditional probability estimates. Our analysis demonstrates the robustness of our test and the ease with which it can be taken to the data.

No matter where we look in our comprehensive data set of earnings forecasts by professional analysts, we find strong evidence *against* herding behavior. Rather, most brokerage houses and money management funds appear to employ the Thomas Kurlaks of the world to do their forecasting. Of Kurlak, Merrill Lynch's semiconductor analyst, it is said that "When Kurlak likes a company, his estimates tend to be much higher than everyone else's; when he is down on a company, they're much lower" ("*Who Really Moves The Market*," FORTUNE, October, 1997). In a nutshell, our tests show that analysts systematically issue biased *contrarian* forecasts that overshoot the consensus forecast in the direction of their private information. In particular, the conditional probability that an analyst's forecast overshoots actual earnings per share (EPS) in the direction away from the consensus is 0.6. Analysts exhibit a contrarian bias *both* when they have positive as well as negative private information relative to the consensus: forecasts that fall short of the consensus fall short of EPS 63% of the time; while those that exceed the consensus, exceed EPS 56% of the time. We also find that the probability a forecast overshoots EPS away from the consensus rises rapidly with the magnitude by which the forecast differs from the consensus. Noteworthy, overshooting rates are remarkably stable, varying by less than five percentage points across analyst order (second, third, last, etc.), and by less than seven percentage points across years and analyst coverage, despite large variations in common earnings shocks. As a check of the consistency of our inferences we also look at forecast *revisions*.[2] Overshooting frequencies in revised forecasts, too, are consistent with anti-herding behavior.

Having shown that analysts issue biased "anti-herding" forecasts, we then quantify the bias, deriving estimates of the strategic bias itself and its expected dollar impact.[3] For example, our estimates imply a strategic bias of 1.98 times the difference between the analyst's estimate of earnings and the extant consensus if he is the second (of 20 analysts) to report; while if he reports last (20^{th}), the strategic bias falls to 0.76 times that difference. Our estimates reveal that the forecast bias falls with the information at an analyst's disposal, but that it always remains economically significant.

We then distinguish between strategic contrarian and overconfidence explanations of the forecast data. An overconfident analyst places excess weight on his own signal, so that if his signal exceeds those of other analysts, then his forecast tends to overshoot earnings in the same direction (Ehrbeck and Waldmann (1996))—analyst overconfidence can also lead to overshooting. While we cannot rule out the possible role of overconfidence, our estimated magnitudes of overshooting are too large to be reconciled solely by analyst overconfidence. In particular, notice that complete myopia bounds the overshooting consistent with overconfidence. So, for example, a completely myopic second analyst who ignores *all* information in the initial forecast and reports his own private signal about earnings should overshoot earnings on average by no more than one-half of the amount by which his forecast differs from the first forecast—the best estimate of earnings given two such forecasts would be their average. However, the data reveal that a second analyst who overshoots the consensus by an amount x overshoots earnings on average by 71% of x, which greatly exceeds the

[2]The ability to revise forecasts provides analysts a mid-course "correction" mechanism, which is important if analysts strategically choose biases.

[3]See Zitzewitz (2001) for a related approach.

overconfidence bound of 50%. Most likely, analysts are not completely myopic[4] and later analysts have better and more information; such possibilities further *reduce* the overshooting magnitudes consistent with overconfidence. In sum, for any reasonable formulation of information arrival, the magnitudes of overshooting are too large to be reconciled purely by overconfidence; some *strategic contrarian* behavior by financial analysts is required to explain the forecast data.

Most past attempts at detecting forecast herding did so by estimating the deviation of each forecast from the mean of all forecasts reported in the forecasting cycle. With this approach, Hong, Kubik and Solomon (2000) find that young and inexperienced analysts are more likely to herd toward the consensus, and are more likely to lose their jobs for making forecasts that deviate away. Lamont (1995) reports similar findings for forecasters of GNP and other macroeconomic indicators.[5] However, there are concerns with this testing strategy: it does not account for correlation in information, unforecasted earnings shocks, or information arrival. Welch (2000) takes a different approach. Using a data set of analyst recommendations, he finds that the prevailing consensus and the two most recent revisions have a positive significant influence on the next analyst's recommendation. Welch's findings reveal that it is important to account for the fact that recent revisions contain useful information that other analysts incorporate into their recommendations. In another related study, Keane and Runkle (1998) attempt to control for correlated forecast errors and conclude that "professional stock market analysts make unbiased forecasts of earnings." Our frequency tests suggest that this conclusion is misplaced. We conjecture that those authors' sample selection criteria (they focus on a sample of 21 heavily-followed firms) and the fact that they do not control for signal correlation likely explain their failure to uncover the contrarian biases revealed by our tests.[6]

In recent years, investors and regulators have grown suspicious of the quality of information that analysts produce—in part, such suspicions reflect beliefs about the prevalence of herding. Indeed, regulators have reacted to the perceived inability of financial analysts to disseminate accurate information with increased regulation (e.g., regulation "Fair Disclosure"). Our anti-herding findings reveal that while the specific behavior may have been mis-identified, concerns regarding analyst bias are well-founded.

Researchers, too, attribute various financial markets anomalies to information biases stemming from perceived herding by analysts. For example, Gleason and Lee (2003) find that large stock price

[4] Welch (2000) shows that analysts systematically use the information in earlier forecasts to update beliefs.

[5] See also, Laster, Bennett and Geoum (1999) and Ehrbeck and Waldmann (1996).

[6] In a recent paper, Chen and Jiang (2004) develop a frequency test statistic that estimates the probability that the forecast error has the same sign as the forecast-consensus difference. Unfortunately, as we show, systematic pessimism or optimism bias their test statistic toward "anti-herding"; as do unforecasted common earnings shocks, or systematic deviations in the earnings that the econometrician observes relative to what analysts forecast.

responses follow forecast revisions in the direction away from the running consensus (which the authors consider consistent with anti-herding), while weaker price responses follow revisions that are consistent with herding. The authors conclude that investors account for systematic herding by analysts when reacting to revisions in earnings forecasts. On the theoretical front, Cont and Bouchaud (2000) offer a formal model relating herding behavior in financial markets to the variability of security prices ("heavy tails" in security returns). Likewise, herding is offered as an explanation for excessive volatility and price bubbles in organized exchanges in Topol (1991), Banerjee (1993), and Shiller (1989), among others.[7] In essence, our anti-herding findings suggest that the foundations of such research developments should be reconsidered. Overall, our study contributes to the debate about the transparency and reliability of information produced in the financial markets.

The remainder of the paper is organized as follows. Section 2 first discusses the theoretical herding and anti-herding literature, and then develops our testing methodology. Section 3 presents our empirical findings and explores their economic significance. Section 4 concludes.

2 Do analysts herd?

2.1 Theoretical background

The literature is replete with both models in which, in equilibrium, analysts herd, and models in which analysts anti-herd. Arguably, a major reason why these conflicting theories have arisen and perpetuated is that empirical researchers have been unable to design a test for herding and anti-herding that is robust to: (a) correlated information among analysts; (b) common unforecasted industry-wide earnings shocks; (c) information arrival over the forecasting horizon; (d) the possibility that the earnings analysts target differ from the earnings the econometrician observes, or that the posteriors that analysts form differ from the "true" conditional distribution of earnings; and (e) systematic optimism or pessimism among analysts.

Theoretical models that seek to explain herding among economic agents include Scharfstein and Stein (1990), Banerjee (1992), Bikhchandani, Hirshleifer and Welch (1992), Zwiebel (1995), and Prendergast and Stole (1996). In various forms, these models imply that forecasters underweight their private information in favor of some publicly-known signal when they perceive their compensation to be based on the relative accuracy of their forecasts.[8] The notion that professional forecasters tend to herd towards the market "consensus" has become the prevalent view among

[7] Herding is also seen as integral part of the collective market phenomena leading to crashes and panics (Orléan (1995)). See also Khanna and Sonti (2002) for a model in which herding in financial markets influences real investment.

[8] See Hirshleifer and Teoh (2003) for a modern taxonomy of herding behavior and its effects on the financial markets.

researchers, and a host of empirical studies claim to find results consistent with herding (e.g., Lamont (1995), Graham (1999), Hong, Kubik and Solomon (2000), and Welch (2000)).

Bernhardt and Kutsoati (2004) derive theoretically how forecasts are affected if analysts are compensated according to the relative accuracy of their forecasts, i.e., how the accuracy of an analyst's forecast compares with the forecast accuracy of other analysts. Effinger and Polborn (2001) show that convex relative performance compensation can arise when firms use relative forecast accuracy to glean information about an analyst's forecasting ability, and then pay competitive wages given the revealed ability.[9] Analysts who are perceived to deliver relatively more accurate forecasts are valued because they draw their firms clients, generate underwriting business, trading volumes and brokerage commissions (see Clarke, Khorana, Patel and Rau (2004), Krigman, Shaw and Womack (2001), and Dunbar (2000) for empirical evidence). As a result, analysts with better forecasting records are subjects of intense competition, receiving larger wage increases and bonuses (see Stickel (1990, 1992, 1995) or O'Brien (1990)). This collective evidence is consistent with convex relative performance compensation. Bernhardt and Kutsoati (2004) show that such convex relative performance compensation causes later analysts to bias their forecasts away from those of earlier analysts as they try to distinguish themselves from other analysts (i.e., later analysts anti-herd).

2.2 A test of forecast bias

An analyst has access to information uncovered through his own research, as well as public information released by the firm and forecasts by earlier analysts. The analyst can use all of this information to update and form a posterior distribution over earnings. An analyst's forecast is unbiased if the forecast is equal to his posterior estimate of the median or mean of earnings per share. Such an unbiased forecast—one that incorporates all available information—is the forecast of greatest value to a retail investor with limited access to other information sources. Unfortunately for these investors, analysts may issue biased forecasts. *Herding* is a choice to bias a forecast away from an analyst's best estimate of earnings (i.e., the mean/median of his posterior), toward the consensus forecast of earlier analysts. This is the appropriate notion of herding in environments where earlier forecasts contain information about earnings or new information can arrive, leading later analysts to update their beliefs about earnings. *Anti-herding* is a choice to announce a forecast of earnings that is *further* from the consensus than the analyst's information suggests, so that the analyst's forecast overshoots his

[9] Laster, Bennett and Geoum (1999) and Ottaviani and Sorensen (2002) provide related models with winner-take-all structures. Zabojnik and Bernhardt (2001) show how such convex compensation schemes can arise when performance depends on both ability and effort, and firms create promotion tournaments designed to elicit effort with competitively-determined wages.

posterior estimate of earnings away from the consensus in the direction of his private information.

This discussion suggests simple tests for whether analysts issue unbiased forecasts, herd, or anti-herd. If an analyst issues an unbiased forecast that corresponds to the median of his posterior then it *necessarily* follows that the probability his forecast exceeds earnings conditional on anything in his information set should be one-half. In particular, the probability should be one-half independently of whether his forecast exceeds or falls short of the average of forecasts made by earlier analysts. This suggests that we should estimate conditional probabilities of the form

$$\Pr(E < F_\tau | \overline{F}_\tau < F_\tau, F_\tau \neq E, y^+) \quad \text{and} \quad \Pr(E > F_\tau | \overline{F}_\tau > F_\tau, F_\tau \neq E_\tau, y^-), \tag{1}$$

where τ is an index for a generic forecast of firm earnings by an analyst in a given quarter, E is realized firm earnings, F_τ is the analyst's forecast, $\overline{F}_\tau$ is the publicly-known consensus at the moment the analyst forecasts, i.e., the average of previous forecasts, and y^+ and y^- are conditioning events. For example, y^+ might be the event that the analyst is the third of L analysts to report, and the forecast was a revised forecast that exceeded the consensus by at least x percent of stock price.

If forecasts are unbiased, then because an analyst's forecast, F_τ, equals the median of his posterior over earnings, $\hat{E}_\tau$, this suggests that each estimated conditional probability should be one-half.

If, instead, an analyst herds toward the consensus, then when the consensus is less than $\hat{E}_\tau$, the analyst will issue a forecast $F_\tau \in (\overline{F}_\tau, \hat{E}_\tau)$. As a result, when the analyst's forecast exceeds the consensus, it should exceed earnings with probability less than one-half, since $F_\tau < \hat{E}_\tau$. That is, $\Pr(E < F_\tau) < \Pr(E < \hat{E}_\tau) = 0.5$. Thus, herding suggests that

$$\Pr(E < F_\tau | \overline{F}_\tau < F_\tau, F_\tau \neq E, y^+) < \frac{1}{2} \quad \text{and} \quad \Pr(E > F_\tau | \overline{F}_\tau > F_\tau, F_\tau \neq E, y^-) < \frac{1}{2}. \tag{2}$$

Finally, if the analyst anti-herds, and tries to separate away from the consensus, it follows that

$$\Pr(E < F_\tau | \overline{F}_\tau < F_\tau, F_\tau \neq E, y^+) > \frac{1}{2} \quad \text{and} \quad \Pr(E > F_\tau | \overline{F}_\tau > F_\tau, F_\tau \neq E, y^-) > \frac{1}{2}. \tag{3}$$

These simple tests have a very important feature: *they do not depend on how analysts' posteriors are formed*. In sharp contrast to other tests of herding, these tests are therefore robust to correlation in analysts' signals, as well as the possibility that later analysts have better information.[10]

[10] Chen and Jiang (2004) estimate the probability the forecast error has the same sign as the forecast-consensus difference. The problem with this statistic is that common unforecasted earnings shocks, systematic pessimism or optimism, and so on each bias the statistic *toward* the alternative of anti-herding, away from the null of unbiasedness. What makes our statistic robust is that it averages the two *conditional* overshooting probabilities, and these factors have offsetting impacts on the two conditional overshooting probability estimates.

Further contemplation, however, reveals that there are significant problems with using only one of the conditional probability estimates to test for herding. In particular, even if forecasts are median unbiased, the econometrician may mistakenly conclude that analysts herd if

- Common unforecasted shocks systematically raise or reduce the earnings of firms. For example, in the early 1990s analysts over-estimated earnings as the economy entered a recession; while most firms reported better-than-forecasted earnings in the late 1990s. If, in 1999, when 70% of forecasts fell short of earnings unconditionally, our estimate of the conditional probability $\Pr(F_\tau > E | F_\tau > \overline{F}_\tau, F_\tau \neq E, y^-)$ is less than one-half, we do not want to conclude that analysts herd if the estimate is low only due to positive unforecasted earnings shocks.

- The econometrician's measure of earnings does not correspond with the earnings that analysts target. For example, analysts may not forecast discretionary write-downs of assets (Abarbanell and Lehavy (2001)), or analysts may seek to forecast "true" earnings and not the "managed" earnings reported by firms. We do not want to conclude that analysts herd if our estimate of $\Pr(F_\tau > E | F_\tau > \overline{F}_\tau, F_\tau \neq E, y^-)$ is less than one-half only because our measure of earnings includes negative discretionary write-downs that analysts do not forecast, or more generally if analysts' posteriors differ from the empirical distribution of observed earnings.

- Analysts target the mean rather than the median, and the posterior distribution is skewed to the left, so that the mean is less than the median (Abarbanell and Lehavy (2001)).

- Analysts may also systematically bias their forecasts in ways that are unrelated to herding. For instance, analysts may issue optimistic forecasts early in the forecasting cycle, and pessimistic forecasts later in the cycle, close to the earnings announcement (see Richardson, Teoh and Wysocki (2003)). Tests for herding should be robust to the presence of such biases.

Let us make these concerns clear. Firstly, note that the impact of cross-sectional correlation due to unforecasted systematic earnings shocks is greatest if the correlation is perfect. Accordingly, let there be a common unforecasted earnings shock, so that a firm's earnings are

$$E = \hat{E}_\tau + \omega + \epsilon_\tau, \tag{4}$$

where $\hat{E}_\tau$ is the analyst's estimate of the median of earnings given his information, ω is a shock that is unforecasted by all analysts that hits all firms, and ϵ_τ is an idiosyncratic shock to the firm's earnings. Let G be the cumulative distribution function of ϵ_τ, and suppose that it is common across all τ. Finally, without loss of generality, let the realization of ω be positive. Then, even if analysts issue unbiased forecasts given their information so that $F_\tau = \hat{E}_\tau$, because earnings of all firms are shifted up by ω, our estimate of $\Pr(E < F_\tau | \overline{F}_\tau < F_\tau, F_\tau \neq E)$ will be less than one-half:

$$\Pr(\hat{E}_\tau + \omega + \epsilon_\tau < F_\tau) = \Pr(\hat{E}_\tau + \omega + \epsilon_\tau < \hat{E}_\tau) = \Pr(\epsilon_\tau < -\omega) = G_\tau(-\omega) < \frac{1}{2}. \qquad (5)$$

This might lead us to conclude falsely that analysts herd. Conversely, the same positive unexpected earnings shock would raise our estimate of $\Pr(E > F_\tau | \overline{F}_\tau > F_\tau, F_\tau \neq E)$ to $1 - G_\tau(-\omega) > \frac{1}{2}$. This conditional probability estimate might lead us to conclude falsely that anti-herding drives forecasts.

It is precisely such concerns that lead us to use the average of the two estimated conditional probabilities as our test statistic: the average of these two estimated probabilities is

$$0.5[G_\tau(-\omega) + 1 - G_\tau(-\omega)] = \frac{1}{2}, \qquad (6)$$

no matter what the value of the unforecasted earnings shock is. In particular, ω has *exactly offsetting* effects on the two conditional probability estimates. Hence, unforecasted earnings shocks do not bias the mean of this average test statistic.

Next consider the possibility that analysts systematically bias forecasts up or down. To allow for complete generality, suppose that with t days left in the forecasting cycle, analysts seek to issue forecasts that target the α_t percentile of earnings, where α_t is arbitrary. For example, Lim (2001), Bernhardt and Campello (2004), and Richardson, Teoh and Wysocki (2003), all find that the first analysts to report tend to issue more optimistic forecasts than later analysts. Such a pattern may reflect a bias relationship, α_t, that is rising with the time t remaining to the earnings announcement. However, the average of the two estimated conditional probabilities is unaffected by any α_t bias since

$$0.5[\Pr(E < F_\tau | \overline{F}_\tau < F_\tau, F_\tau \neq E) + \Pr(E > F_\tau | \overline{F}_\tau > F_\tau, F_\tau \neq E)] = 0.5[\alpha_t + (1 - \alpha_t)] = \frac{1}{2}. \qquad (7)$$

Next suppose that analysts target E, setting $F_\tau = \hat{E}_\tau$, but the econometrician sees a measure of earnings, $E + \alpha$. If analysts set $F_\tau = \hat{E}_\tau$, conditioning on whether F_τ exceeds $\overline{F}_\tau$ is irrelevant, and the average of the two conditional probability estimates is

$$0.5[\Pr(F_\tau < E + \alpha | F_\tau < \overline{F}_\tau) + \Pr(F_\tau > E + \alpha | F_\tau > \overline{F}_\tau)] = 0.5[G(\alpha) + (1 - G(\alpha))] = \frac{1}{2}. \qquad (8)$$

Finally, the average of the two conditional probability estimates remains one-half if analysts target the mean rather than the median, even if an analyst's posterior is skewed to the left so that the mean is less than the median. This just amounts to introducing a negative bias, $-\alpha$, into forecasts.

2.3 Constructing the test statistic

Let τ be an index for a generic forecast by an analyst in some firm-quarter. We first select conditioning events z_τ^+ and z_τ^-, where z_τ^+ implies that the analyst's forecast exceeds the extant consensus ($F_\tau > \overline{F}_\tau$); and z_τ^- implies that the forecast fell short of the extant consensus ($F_\tau < \overline{F}_\tau$) at the moment that the analyst reported. Next, define conditioning indicator functions, γ_τ^+ and γ_τ^-, where

$$\gamma_\tau^+ = 1 \text{ if } z_\tau^+ \text{ occurred; } \gamma_\tau^+ = 0 \text{ if } z_\tau^+ \text{ did not occur,}$$
$$\gamma_\tau^- = 1 \text{ if } z_\tau^- \text{ occurred; } \gamma_\tau^- = 0 \text{ if } z_\tau^- \text{ did not occur,}$$

and overshooting indicator functions, δ_τ^+ and δ_τ^-, where:

$$\delta_\tau^+ = 1 \text{ if } F_\tau > E_\tau \text{ and } \gamma_\tau^+ = 1; \text{ and } \delta_\tau^+ = 0 \text{ otherwise.}$$
$$\delta_\tau^- = 1 \text{ if } F_\tau < E_\tau \text{ and } \gamma_\tau^- = 1; \text{ and } \delta_\tau^- = 0 \text{ otherwise,}$$

and E_τ is the actual earnings in firm-quarter τ. We use

$$\mathbf{S}(z^-, z^+) = \frac{1}{2}\left[\frac{\sum_\tau \delta_\tau^+}{\sum_\tau \gamma_\tau^+} + \frac{\sum_\tau \delta_\tau^-}{\sum_\tau \gamma_\tau^-}\right] \tag{9}$$

to estimate the probability that forecasts overshoot earnings in the same direction as they overshoot the consensus. $\frac{\sum_\tau \delta_\tau^+}{\sum_\tau \gamma_\tau^+}$ is our estimate of the conditional probability of overshooting actual earnings given that the forecast exceeds the consensus; while $\frac{\sum_\tau \delta_\tau^-}{\sum_\tau \gamma_\tau^-}$ is our estimate of the conditional probability of falling short of true earnings given that the forecast falls short of the consensus. $\mathbf{S}$ is the average of the two conditional overshooting probabilities estimates.

The null hypothesis that analysts issue unbiased forecasts implies that $\mathbf{S}$ should be one-half. The alternative hypothesis that analysts herd implies that the probability their forecast overshoots earnings in the direction away from the consensus is less than one-half, i.e., $\mathbf{S} < 0.5$. Finally, the alternative hypothesis that analysts anti-herd implies that the probability their forecast overshoots earnings in the direction away from the consensus should exceed one-half, i.e., $\mathbf{S} > 0.5$.

2.4 Robustness of the test statistic

Before conducting our empirical tests we formalize the robustness properties of the $\mathbf{S}$ statistic. We first describe the test statistic in the absence of cross-sectional correlation, and then discuss the impact of such correlation. Highlighting the case without cross-sectional correlation is important in explaining why our tests for herding/anti-herding are *conservative* in the presence of cross-sectional correlation—generated, e.g., by an unforecasted macroeconomic movement. The discussion also reveals why our test statistic is robust to forecast mismeasurement (i.e., the possibility that analysts target earnings that are different from those in I/B/E/S), as well as systematic optimism or pessimism by analysts.

No cross-sectional correlation. In this case, without loss of generality, we can represent firm i's earnings in quarter t as

$$E_{it} = \hat{E}_{ijt} + \epsilon_{ijt}, \tag{10}$$

where $\hat{E}_{ijt}$ is the median of analyst j's posterior distribution over firm i's earnings and ϵ_{ijt} is the error in j's assessment. Assume that $\epsilon_{ijt} \sim G(\cdot)$ is independently and identically distributed across firm quarters (and by definition $G(0) = 0.5$). Our test statistic **S** in equation (9) averages the two non-parametric estimates of these population overshooting probabilities. If analyst j is unbiased, then $F_{ijt} = \hat{E}_{ijt}$, independent of the conditioning information, so that $\sum_\tau \delta_\tau^+ \sim B(\sum_\tau \gamma_\tau^+, G(0))$ and $\sum_\tau \delta_\tau^- \sim B(\sum_\tau \gamma_\tau^-, 1 - G(0))$. Hence, **S** is asymptotically normally distributed with mean

$$0.5[Pr(F_{ijt} > E_{it}) + Pr(F_{ijt} < E_{it})] = 0.5[(1 - G(0)) + G(0)] = 0.5 \tag{11}$$

and variance

$$\frac{G(0)(1 - G(0))}{4} \left[\frac{1}{\sum_\tau \gamma_\tau^+} + \frac{1}{\sum_\tau \gamma_\tau^-} \right] = \frac{1}{16} \left[\frac{1}{\sum_\tau \gamma_\tau^+} + \frac{1}{\sum_\tau \gamma_\tau^-} \right]. \tag{12}$$

Cross-sectional correlation. We now consider how an unforecasted earnings shock that affects the cross-sectional correlation of forecast errors at a given point in time affects our statistic. Suppose that firm i's earnings in quarter t equal

$$E_{it} = \hat{E}_{ijt} + \omega_t + \epsilon_{ijt}, \tag{13}$$

where ω_t is an unforecasted earnings shock common to all firms in quarter t. Under the null that forecasts are unbiased (i.e., $F_{ijt} = \hat{E}_{ijt}$), $\sum_\tau \delta_\tau^+ \sim B(\sum_\tau \gamma_\tau^+, G(-\omega_t))$ and $\sum_\tau \delta_\tau^- \sim B(\sum_\tau \gamma_\tau^-, 1 - G(-\omega_t))$. Hence, **S** still has a mean of $\frac{1}{2}$ independently of the value of ω_t (see equation (6)), but the variance of **S** is only

$$\frac{G(-\omega_t)(1 - G(-\omega_t))}{4} \left[\frac{1}{\sum_\tau \gamma_\tau^+} + \frac{1}{\sum_\tau \gamma_\tau^-} \right] \leq \frac{1}{16} \left[\frac{1}{\sum_\tau \gamma_\tau^+} + \frac{1}{\sum_\tau \gamma_\tau^-} \right]. \tag{14}$$

The key feature driving the robustness of our test is that the variance of **S** is *maximized* by $\omega_t = 0$, as $G(-\omega_t)(1 - G(-\omega_t)) \leq (1/2)(1/2) = .25$. Under any null of unbiasedness, the mean of **S** is unaffected by variations in ω_τ and $G_\tau(\cdot)$ across firm-quarters, and more generally by cross-sectional correlation of *any* arbitrary form. Introducing cross-sectional correlation in forecast errors serves only to reduce the variance of our test statistic below that associated with independently distributed shocks. *Hence, if the null hypothesis of unbiasedness is rejected under the assumption that earnings shocks are independently distributed, then unbiasedness can always be rejected.* In other words, of all possible nulls of unbiasedness, the confidence intervals around our point estimate of **S** are *largest* when there is no cross-sectional correlation in earnings shocks. Hence, using the confidence intervals associated with independently distributed shocks as we do, can only lead to Type II errors—failing to reject forecast unbiasedness when we should do so.

A similar analysis reveals that the **S** test statistic is robust to the possibility that analysts do not forecast the earnings $E_\mathcal{T}$ reported by I/B/E/S, but rather forecast some other measure of earnings, $E_\mathcal{T} + \varepsilon_\mathcal{T}$. As long as $\varepsilon_\mathcal{T}$ is distributed independently from $F_\mathcal{T} - \overline{F}_\mathcal{T}$ (e.g., all analysts target the same measure of earnings), then under a null hypothesis that analysts report unbiased forecasts of *any* other measure of earnings, the mean of **S** is 0.5 and its variance is reduced. As a result, if we would reject a null of unbiasedness when our measure of earnings is the one that analysts target, then we would reject alternative hypotheses that analysts unbiasedly target some other measure of earnings—misidentifying the earnings that analysts target can only give rise to Type II errors.

Finally, the mean of **S** is unaffected by systematic optimism or pessimism (see equation (8)), but such biases again reduce the variance of **S** (for example, to zero if forecasts are so pessimistic that the probability that earnings exceed the forecast goes to one). Because we use the maximum variance bounds of **S** in our tests, systematic optimism or pessimism can only lead to Type II errors. To the extent that such bias is significant, it only makes it *more difficult* to reject a null of unbiasedness in favor of an alternative of herding or anti-herding, when the alternative is, in fact, correct. Indeed, note that the null will not be rejected if analysts target any percentile of any posterior, i.e., no matter whether posteriors are 'correct' (corresponding to the 'true' conditional distribution of earnings), or not.

Our frequency test also has the virtue that it is non-parametric and unaffected by the scale of errors across firms, so that outliers do not have disproportionate effects (see the concerns detailed in Keane and Runkle (1998) and Lim (2001)).

Inference under the alternative. Suppose the estimate of **S** that we obtain is significantly different from 0.5. To make matters concrete, suppose we obtain estimates of **S**$\sim$0.6: how do we interpret this estimate under an alternative of anti-herding?

Suppose that the anti-herding alternative is that analysts intend to overshoot earnings in the direction away from the consensus by $x > 0$. Then, $x = |F_{ijt} - \hat{E}_{it}| > 0$. Absent a common shock

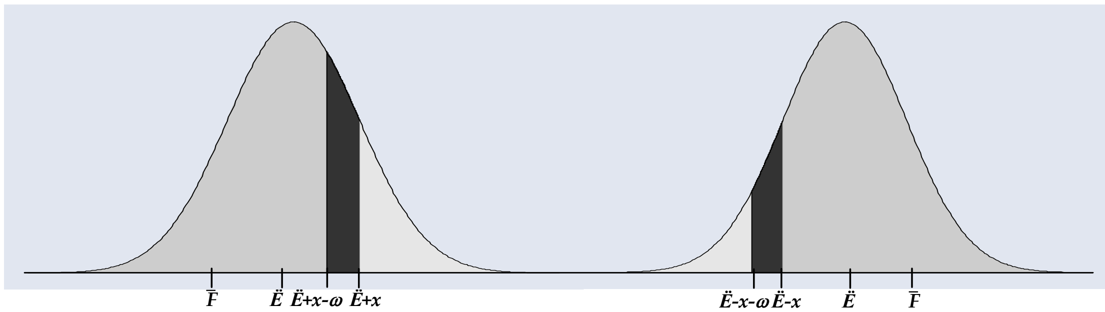

Figure 1: The reduction in $\mathbf{S}$ due to time-specific unforecasted shock ω across firm forecast panels is given by the difference in shaded areas: the "area" lost (left figure) exceeds the "area" gained (right figure).

($\omega_t \neq 0$), and the other complicating features that we highlight, under this alternative the mean of our test statistic is $G(x) > 0.5$, and its variance is only

$$\frac{G(x)(1-G(x))}{4}\left[\frac{1}{\sum_\tau \gamma_\tau^+}+\frac{1}{\sum_\tau \gamma_\tau^-}\right] < \frac{1}{16}\left[\frac{1}{\sum_\tau \gamma_\tau^+}+\frac{1}{\sum_\tau \gamma_\tau^-}\right]. \tag{15}$$

But now suppose that $\omega_t \neq 0$. As Figure 1 illustrates, $\omega_t \neq 0$ biases the mean of $\mathbf{S_t}$ down from $G(x)$ toward one-half to $\frac{G(x-\omega_t)+1-G(-x-\omega_t)}{2}$,[11] and its variance is reduced further to

$$\frac{G(x-\omega_t)(1-G(x-\omega_t))}{4}\frac{1}{\sum_\tau \gamma_\tau^+}+\frac{G(-\omega_t-x)(1-G(-\omega_t-x))}{4}\frac{1}{\sum_\tau \gamma_\tau^-}, \tag{16}$$

which is *less* than the variance estimate of $\frac{(.5)(.5)}{4}\left[\frac{1}{\sum_\tau \gamma_\tau^+}+\frac{1}{\sum_\tau \gamma_\tau^-}\right]$ that we use in our empirical tests. Most transparently, if $\omega_t \to \infty$ then $G(x-\omega_t) \to 0$ and $1-G(-x-\omega_t) \to 1$, so that $\mathbf{S_t} \to \frac{1}{2}$ no matter what the magnitude of anti-herding x is.

Because cross-sectional correlation in errors almost certainly exists, our point estimate of the extent of anti-herding, $\mathbf{S_t}$, is almost certainly biased downward toward one-half. Importantly, we report the lowest lower bound consistent with any null of unbiasedness, i.e., the null associated with independent observations where none of the other concerns that we have highlighted are present. This bound is therefore very conservative and represents a strict lower bound on the true magnitude of anti-herding in the data. Simply put, our test design is biased *against* finding anti-herding. In contrast, the true magnitude of anti-herding may exceed the upper bound on the confidence interval associated with this null of unbiasedness if there is sufficient cross-sectional correlation in errors or systematic pessimism.

[11] To see this, suppose that posteriors are single-peaked. Differentiating $\frac{1}{2}[1+G(\omega+x)-G(\omega-x)]$ with respect to ω yields $\frac{1}{2}[g(\omega+x)-g(\omega-x)]$, which is less than zero as long as the posterior is sufficiently close to being symmetric.

3 Empirical Analysis

3.1 Data and Sample Selection

Data on individual analysts' quarterly forecasts of earnings from 1989 to 2001 are taken from the Institutional Brokers Estimate System (I/B/E/S) Detail tapes. Each observation includes the company ticker, forecast horizon, codes that identify each analyst and brokerage house, the analyst's earnings estimate for the period-end, and the date that the forecast was reported to I/B/E/S. We do not consider forecasts prior to 1989 because of the long lag between the date of an analyst's forecast announcement and the date the forecast was entered in the I/B/E/S database during that period.[12] After 1988, forecasts were disseminated by I/B/E/S within 24 hours. This sample selection criterion ensures that publication dates are close to the actual dates analysts released their forecasts and that the analyst reporting *observes* earlier forecasts.

Earnings per share data, reported on the same basis (primary or diluted) as the corresponding forecasts, are also taken from I/B/E/S. Share prices and the number of shares outstanding at the end of each quarter are extracted from the Center for Research on Security Prices (CRSP) files. We filter for likely data entry errors, deleting any forecast with an absolute error value exceeding \$10.[13] Some forecasts are dated over 360 days prior to the earnings report, and are likely data entries for the wrong firm-quarter, while a few other forecasts were after the earnings announcement date. To deal with these inconsistencies, we only consider forecasts reported within the 120-day window preceding the earnings announcement date. We discard observations with an analyst identifier code of "0" (about 3% of observations), which denotes that the analyst is unidentified.

For each firm-quarter, we count the number of individual analysts reporting forecasts (`cover`), and the order in which a forecast was reported in the sequence (`order`). We also track total forecasts, including revisions. We proxy for brokerage house size using the number of analysts associated with each brokerage house (`broksz`) that quarter, and we use the number of years an analyst has been in the database since 1985 (`exp`) to gauge his experience (see Hong, Kubik and Solomon (2000) for a similar approach). Our final sample includes forecasts of earnings for 4,456 firms over 87,339 firm-quarters—a total of 387,756 observations.

For every firm-quarter, we order forecasts by their date of release. We then define a forecast reported at date τ by F_τ and the mean of all other forecasts reported at least ℓ days before τ by $\overline{F}_{\tau-\ell}$. $\overline{F}_{\tau-\ell}$ is then the "outstanding" consensus mean forecast of those reported at least ℓ days

[12] O'Brien (1988) reported an average publication lag of 34 trading days over the period 1975-1982. We go back to 1985 to measure analyst forecasting experience.

[13] O'Brien (1988) and Lim (2001) use a similar rule in deleting suspected data-entry errors.

Table 1: Example of the sequence of forecasts and the processing of the consensus forecast

Obs. #	Analyst identity	Forecast	Date of publication (yy/mm/dd)	Order of forecast (for consensus estimation)
1.	1	0.75	92/04/17	1
2.	1	0.70	92/04/20	1
3.	2	0.72	92/04/27	2
4.	3	0.82	92/05/03	3
5.	4	0.82	92/05/03	3
6.	5	0.80	92/05/15	5
7.	3	0.84	92/05/15	4
8.	1	0.83	92/06/10	5

before date τ. Because analysts sometimes revise forecasts, we use the most recent forecast reported by each analyst to compute the consensus forecast. The procedure is illustrated with a hypothetical example in Table 1. The table presents a sequence of forecast releases by five distinct analysts for a firm-quarter. The total number of forecasts is 8.

If we use a one day lag to calculate the outstanding consensus, then the last column represents the order of forecast release. In Table 1, the same analyst makes the first two forecasts, so that he is still the only analyst and hence the order remains 1. Thus, when analyst 2 reports his forecast on April 27 (the 3^{rd} observation), the consensus forecast is \$0.70, i.e., the most recent forecast by analyst 1. Also, because the 4^{th} and 5^{th} forecasts were made by different analysts on the same day, they are both 3^{rd} in the order of release of forecasts. That is, these analysts only know the forecasts of the two (distinct) analysts who reported before them. Hence, they both face the same consensus, which is the average of the 2^{nd} and 3^{rd} forecasts. Notice that the 7^{th} forecast (released by analyst 3) has an order of 4. This is because his forecast was made on the same day as the 6^{th} observation (reported by analyst 5) and 3 *distinct* analysts had reported forecasts before that date. Similarly, the 8^{th} observation, reported by analyst 1 is 5^{th} in the order of release. The outstanding consensus at that point is the average of the most recent forecasts reported by all except analyst 1; that is, the mean of the 3^{rd}, 5^{th}, 6^{th}, and 7^{th} forecasts.

It is important that we condition our tests only on forecasts in an analyst's information set. An issue is: which forecasts does an analyst know when forming his forecast? The publication date of an analyst's forecast in the I/B/E/S database could be later than the actual release date of his forecast to the market. This leads us to use the extant consensus as of $\ell = 3$ days before the analyst's forecast is reported in I/B/E/S. Alternative lag choices of 5 or 7 days yield qualitatively identical findings.

Table 2: Sample Statistics: $387,756$ observations

			Percentiles				
	min	max	25	50	75	Mean	Std. dev
Number of analysts; `cover`	2	42	6	9	14	10.2	6.0
Forecasts-Analysts ratio	1.0	7.0	1.1	1.3	1.6	1.4	0.4
Consensus error (as a percent of stock price)	−348.0	1128.7	−0.1	0	0.2	0.4	5.2
Forecast error (in percent of stock price)	−512.2	1251.2	−0.1	0	0.1	0.2	4.9
Forecast minus consensus (in percent of stock price)	−385.5	266.6	−0.1	−0.01	0.04	−0.1	2.3
Days between any successive forecasts	0	119	1	5	13	9.5	12.9
Days between own successive forecasts (revised forecasts only; 123,023 obs.)	0	117	21	36	56	39.0	22.7
Analyst's experience (no. of years since 1985 at date of forecast); `exp`	1	17	3	5	8	5.9	3.7
Brokerage size (no. of analysts employed by brokerage house); `broksz`	1	140	14	30	51	36.6	28.1

Table 2 details the distribution of analyst coverage and the average number of forecasts per analyst. An average of about 10 (a median of 9) analysts follow a firm in any given quarter. Also note that analysts seldom revise their one-quarter ahead forecasts—the average number of forecasts per analyst is about 1.4—and there is a median of 5 days between the release dates of successive forecasts. The median analyst has five years of experience since 1985, and the median brokerage house has 30 analysts contributing forecasts to the I/B/E/S database. Table 2 also reports the distribution of the error in individual forecasts as a percent of the stock's price at the end of the previous quarter, and the difference between the last and consensus forecasts. The mean stock price in our sample is about \$35, suggesting that, on average, analysts overshoot earnings in the direction away from the consensus by about \$0.08. The summary statistics show that the distribution of the percentage forecasts error has extremely fat tails. If we do not control properly, such outliers can have excessive impact on linear regression estimations.[14]

3.2 Results of S Tests

Let $\mathbf{S}(x)$ be our test statistic when z^+ is the event that $\frac{F_\tau - \overline{F}_{\tau-\ell}}{P_{\tau-1}} > x$, and z^- is the event that $\frac{\overline{F}_{\tau-\ell} - F_\tau}{P_{\tau-1}} > x$. Table 3 reports that $\mathbf{S}(0)$ equals 0.592—nearly 60% of the time, analysts overshoot

[14] We discuss various outlier-robust regression-based procedures in section 3.3. We emphasize that the **S** tests of section 3.2 are robust to outliers.

earnings in the direction *away* from the consensus. Further, the reported 95% confidence lower bound on $\mathbf{S}(0)$ of 0.590 is calculated using the upper bound on variance of the test statistic, so that the true 95% lower bound can only be closer to 0.592. Figure 2 shows that $\mathbf{S}(x)$ rises rapidly with x: larger deviations from the consensus substantially raise the likelihood of overshooting. Our test statistic rejects *both* the null of unbiasedness and the alternative of herding. Rather, analysts systematically issue anti-herding forecasts that overemphasize their private information. Table 3 reveals that analysts' forecasts exhibit a contrarian bias both when analysts have negative information and when they have positive private information relative to the consensus. Even though, unconditionally, forecasts exceed earnings only 45% of the time, forecasts that exceed the consensus, exceed earnings 56% of the time. So, too, forecasts that fall short of the consensus, fall short of earnings 63% of the time. The difference in overshooting rates (63% *vs* 56%) captures the well-known regularity that, unconditionally, forecasts tend to undershoot earnings. Possible explanations for this are that forecasts tended to be pessimistic, or there was a preponderance of positive unforecasted earnings shocks. But when analysts anti-herd, as we have highlighted, each of these possibilities should drive estimates of $\mathbf{S}$ down toward one-half, making it *harder* to reject a null of unbiasedness. This renders these high overshooting rates all the more remarkable.

The decomposition by year reveals that even though there is enormous annual variation in the unconditional probability that forecasts exceeded earnings, there is little annual variation in $\mathbf{S}$. This highlights the importance of designing a test for herding and anti-herding that is robust to unforecasted earnings shocks and our success in doing so. That is, variations across years shift up or down the unconditional probability that forecasts exceed earnings, but the corresponding impacts on the two conditional overshooting probability estimates are largely offsetting (they are perfectly offsetting under the null of unbiasedness, but, as our analysis of inference under the alternative reveals are only *partially* offsetting under the alternative of anti-herding), leading to the small variation in $\mathbf{S}$ across years. More concretely, $\Pr(F_\tau > E)$ ranged from as low as 0.33 and 0.34 in 1999 and 2000 to as high as 0.56 and 0.57 between 1989 and 1991, while $\mathbf{S}$ only varies between 0.54 and 0.62. The yearly variations in $\Pr(F_\tau > E)$ partially reflect unforecasted earnings shocks—the economy unexpectedly stagnated around 1990-1991, while the increase in the stock prices in the late 1990s and early 2000 suggests that the entire market was pleasantly surprised by the high corporate earnings. That $\mathbf{S}$ is lower in 1999 and 2000 is because there were large, positive unforecasted earnings shocks over most quarters within that period—a combination that our analysis of inference under the alternative reveals drives the $\mathbf{S}$ statistic closer to one-half even when analysts anti-herd.

Table 3: **S** Tests of Bias in Analysts's Forecasts

The test statistic, **S**, is computed for different sub-samples: `order` is the order in the sequence of forecasts; `cover` is the number of analysts following the firm; `exp` is the analyst's experience by year of a given forecast; and `broksz` is the number of analysts employed by brokerage house. z_τ^+ (z_τ^-) is is the event that the forecast exceeds (is less than) the *consensus*. 95% confidence intervals are reported in square brackets.

Sample	N	$\Pr(F_\tau > E)$	$\Pr(F_\tau > E \mid z_\tau^+)$	$\Pr(F_\tau < E \mid z_\tau^-)$	**S**
Total sample	327,315	0.450	0.556	0.628	0.592 [0.590, 0.594]
By year of forecast					
1989	18,903	0.566	0.697	0.542	0.619 [0.612, 0.626]
1990	20,880	0.569	0.688	0.523	0.605 [0.598, 0.612]
1991	23,473	0.561	0.681	0.525	0.603 [0.596, 0.610]
1992	26,183	0.523	0.638	0.564	0.601 [0.595, 0.607]
1993	22,782	0.470	0.584	0.621	0.602 [0.596, 0.609]
1994	27,363	0.433	0.554	0.664	0.608 [0.602, 0.614]
1995	28,913	0.434	0.542	0.649	0.596 [0.590, 0.601]
1996	27,866	0.416	0.523	0.666	0.594 [0.589, 0.600]
1997	28,211	0.404	0.525	0.683	0.604 [0.598, 0.610]
1998	31,676	0.442	0.556	0.623	0.589 [0.583, 0.595]
1999	30,611	0.339	0.401	0.706	0.553 [0.548, 0.559]
2000	22,084	0.333	0.384	0.706	0.544 [0.538, 0.551]
2001	18,370	0.427	0.529	0.619	0.572 [0.564, 0.580]
By forecast `order`					
`order` = 2	44,046	0.502	0.621	0.598	0.609 [0.604, 0.613]
`order` = 3	40,513	0.471	0.589	0.621	0.604 [0.600, 0.609]
`order` = 2nd-to-last	61,261	0.416	0.519	0.655	0.584 [0.580, 0.588]
`order` = last	73,811	0.405	0.507	0.668	0.585 [0.581, 0.589]
By days to earnings announcement (`to-ann`)					
`to-ann` > 60	115,046	0.50	0.623	0.607	0.614 [0.612, 0.617]
$30 <$ `to-ann` ≤ 60	103,763	0.466	0.565	0.60	0.582 [0.579, 0.585]
$15 <$ `to-ann` ≤ 30	58,468	0.392	0.468	0.654	0.561 [0.557, 0.565]
`to-ann` ≤ 15	50,038	0.368	0.461	0.697	0.579 [0.574, 0.583]

Table 3 contd.

Sample	N	$\Pr(F_\tau > E)$	$\Pr(F_\tau > E \mid z_\tau^+)$	$\Pr(F_\tau < E \mid z_\tau^-)$	**S**
		By Analyst cover			
cover ≤ 5	73,816	0.451	0.564	0.636	0.599 [0.595, 0.603]
$6 \leq$ cover ≤ 14	179,117	0.454	0.567	0.629	0.598 [0.596, 0.600]
cover ≥ 15	74,382	0.438	0.519	0.617	0.568 [0.564, 0.571]
		By order and cover			
order $= 2$ if cover ≤ 5	29,024	0.479	0.602	0.621	0.610 [0.604, 0.615]
order $= last$ if cover ≤ 5	37,213	0.435	0.543	0.647	0.593 [0.588, 0.598]
order $= 2$ if $6 \leq$ cover ≤ 14	13,066	0.542	0.656	0.563	0.609 [0.601, 0.618]
order $= last$ if $6 \leq$ cover ≤ 14	30,629	0.383	0.479	0.682	0.581 [0.575, 0.586]
order $= 2$ if cover ≥ 15	1,956	0.585	0.655	0.477	0.566 [0.544, 0.588]
order $= last$ if cover ≥ 15	5,969	0.332	0.409	0.721	0.564 [0.551, 0.577]
		By experience (exp)			
exp ≤ 2	68,109	0.455	0.568	0.628	0.598 [0.594, 0.601]
$3 \leq$ exp ≤ 5	105,313	0.460	0.572	0.622	0.597 [0.594, 0.600]
$6 \leq$ exp ≤ 9	95,260	0.460	0.564	0.613	0.588 [0.585, 0.591]
exp ≥ 10	58,633	0.407	0.502	0.659	0.580 [0.576, 0.584]
		By brokerage size (broksz)			
broksz ≤ 10	57,448	0.473	0.600	0.629	0.614 [0.609, 0.618]
$11 \leq$ broksz ≤ 30	103,064	0.462	0.581	0.626	0.603 [0.600, 0.606]
$31 \leq$ broksz ≤ 60	108,511	0.452	0.556	0.624	0.590 [0.587, 0.592]
broksz ≥ 61	58,292	0.399	0.458	0.637	0.547 [0.543, 0.551]

Table 3 also reveals that **S** varies even less with other conditioning variables:

Analyst order and time to announcement: The unconditional probability that forecasts exceed earnings falls sharply for later forecasts—the second analyst's forecast exceeds earnings half of the time, while the last analyst's forecast only beats earnings 41% of the time; and an analyst who reports over 60 days before the earnings announcement beats earnings half of the time, while an analyst who reports within 15 days of the announcement beats earnings only 37% of the time. However, one can see that even though later analysts are far more pessimistic, **S** varies little with release order: the second analyst overshoots earnings in the opposite direction of the consensus 61%

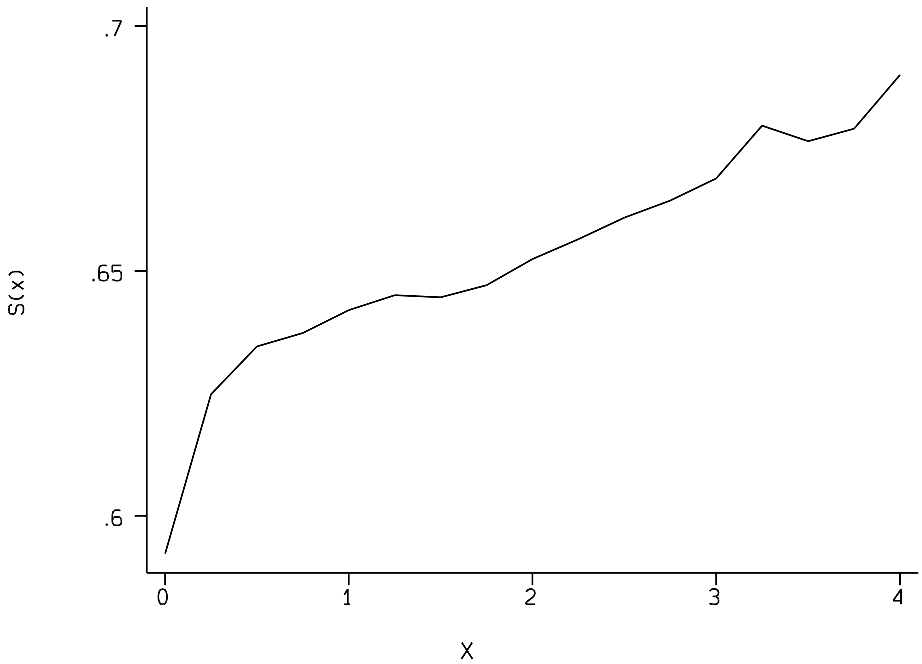

Figure 2: Average conditional probability that forecast overshoots earnings away from consensus, $\mathbf{S}(x)$, given that it exceeds or falls short of the consensus by at least $x\%$ of the stock's price. To avoid the impact of possible data entry errors, we drop observations for which a forecast exceeds or falls short of the consensus by more than $x = 20\%$ of the stock's price.

of the time, only 3% more than the frequency with which the last analyst overshoots. Likewise, analysts who report more than 60 days prior to the earnings announcement overshoot earnings in the opposite direction of the consensus 61% of the time, while those who report within 15 days of the announcement date overshoot 58% of the time. Because greater pessimism biases **S** toward one-half, this decline in **S** for later analysts does not necessarily indicate that they are less prone to overshooting.

Analyst coverage: The probability of overshooting is only slightly higher for firms followed by fewer analysts. **S** hits a maximum of 0.60 for firms followed by 5 analysts or less, but it falls only to 0.57, for firms followed by at least 15 analysts. Notice also that $\Pr(F_\tau > E)$ barely varies with analyst coverage; in other words, analysts for more-heavily followed firms are not more pessimistic, on average.

Both analyst coverage and order: When we control for both analyst coverage and forecast timing, we see that even though the mean level of optimism does not vary with coverage, the more analysts who follow a firm, the more optimistic are earlier analysts, and the more pessimistic are later analysts. For example, for firms followed by at least 15 analysts, the second forecast exceeds earnings 59% of the time, while the last forecast only exceeds earnings 33% of the time. In contrast, for firms followed by no more than 5 analysts, the second forecast exceeds earnings 48% of the time, and the last forecast exceeds earnings 44% of the time. Despite this, controlling for both analyst order and analyst coverage does not lead to a different pattern in **S** compared to when we control only for analyst coverage. One might reconcile this pattern of increasing pessimism in forecasts at least in part by efforts of later analysts to distinguish their forecasts from optimistic earlier forecasts. The apparent increase in pessimism may also reflect forecast management by firms who seek to generate a 'positive earnings surprise' (see Bernhardt and Campello (2004)).

Analyst experience: **S** essentially does not vary with analyst experience at the time of forecast, even though analysts with at least 10 years experience are far more pessimistic.[15]

Brokerage house size: Analysts are slightly more pessimistic at large and medium-sized brokerage houses than at small brokerages, but analysts at the largest brokerage houses are far more pessimistic. Overall, **S** declines only slightly with brokerage size, except for the very largest brokerage houses. Again, the lower value of **S** for the largest brokerage houses is due in part to their greater pessimism, which drives **S** toward one-half: quite plausibly anti-herding does not vary with brokerage house size.

[15] This result may reflect the fact that the experience variable is truncated—1995 is the first year analysts had 10 years of experience—and forecasts in later years tended to be lower relative to realized earnings.

We next explore the bias in *revised* forecasts. Analysts only revise forecasts infrequently and are far more likely to revise forecasts down than up—66.5% of the time, the revised forecast is less than the original forecast. We decompose revised forecasts (a) according to whether the original forecast-consensus difference is smaller or larger than the revised forecast-consensus difference; and (b) according to whether the revised forecast F^r_τ is closer to the current consensus than the initial forecast F^o_τ was; i.e., according to whether $|F^r_\tau - \overline{F}^r_\tau| < |F^o_\tau - \overline{F}^r_\tau|$, where $\overline{F}^r_\tau$ is the revised consensus.

Table 4 reveals that characterizations of revised forecasts are far more subtle than that suggested by the fact that, on average, revised forecasts are less likely than unrevised forecasts to overshoot earnings in the opposite direction from the consensus. The second panel shows that revisions that raise the difference between the consensus forecast and the analyst's forecast are in fact *less* likely to overshoot than revisions that reduce the forecast-consensus difference. The last panel reveals that forecast revisions that move closer to the consensus still overshoot frequently. That is, forecasts revisions that go closer to the consensus do not go far enough. In very sharp contrast, forecast revisions that move further from the consensus are essentially no more likely to overshoot than not. Note that analyst overconfidence would predict the *opposite*—forecasts driven by analyst overconfidence that move further from the consensus should be *more* likely to overshoot.

How might one interpret these overshooting results for revised forecasts? One possibility is that analysts care about both absolute forecast accuracy and forecast accuracy relative to the consensus. Consider the finding that revised forecasts that move away from the consensus are unbiased. If the consensus is sufficiently inaccurate given an analyst's updated information, then he may not want to bias his forecast further in order to separate away from the consensus. The revised forecast moves away from the consensus, but is unbiased. This is consistent with the possibility that after some point, there are decreasing returns to outperforming the consensus by more. Now consider the large overshooting bias in forecasts that move toward the consensus. An analyst who revises his forecast toward the consensus only does so when his original forecast is far too extreme given his updated information. The fact that the analyst's revised forecast does not go far enough toward the consensus indicates a desire to distinguish his forecast from others.

The results, so far, suggest that analysts strategically issue contrarian forecasts to distinguish their forecasts from others. However, if an analyst's forecasting ability is judged by how accurate his forecast is relative to the *final* consensus, then forward-looking analysts should try to anticipate how subsequent analysts will react to their forecasts. We test for such forward-looking strategic behavior by computing **S** using the consensus that emerges three days *after* an analyst issues his forecast. Specifically, for analyst i who reports at date t_i, we use the consensus that emerges at date

Table 4: **S** Tests for Revised Forecasts

The probability, **S**, that a revised forecast overshoots earnings, is computed for different direction of forecast revisions. z_τ^+ (z_τ^-) is is the event that the forecast exceeds (is less than) the *consensus*. 95% confidence intervals in square brackets.

Sample	N	$\Pr(F_\tau > E)$	$\Pr(F_\tau > E\| z_\tau^+)$	$\Pr(F_\tau < E\| z_\tau^-)$	**S**
Revised forecasts (all)	104,837	0.426	0.463	0.594	0.528 [0.525, 0.531]
Forecast-consensus difference after revisions					
Smaller difference (after revision) ($\|F_\tau^r - \overline{F}_\tau^r\| < \|F_\tau^o - \overline{F}_\tau^o\|$)	57,723	0.424	0.471	0.609	0.540 [0.536, 0.544]
Larger difference (after revision) ($\|F_\tau^r - \overline{F}_\tau^r\| > \|F_\tau^o - \overline{F}_\tau^o\|$)	47,114	0.429	0.448	0.578	0.513 [0.508, 0.518]
Location of original & revised forecast, relative to *current* consensus					
Revision *closer* to consensus: ($\|F_\tau^r - \overline{F}_\tau^r\| < \|F_\tau^o - \overline{F}_\tau^r\|$)	49,538	0.434	0.505	0.622	0.563 [0.559, 0.568]
original forecast		0.553	0.849	0.845	0.846 [0.843, 0.850]
Revision *further* from consensus ($\|F_\tau^r - \overline{F}_\tau^r\| > \|F_\tau^o - \overline{F}_\tau^r\|$)	55,299	0.420	0.403	0.574	0.489 [0.484, 0.493]
original forecast		0.606	0.717	0.413	0.565 [0.561, 0.569]

t_i+3, excluding i's forecast.[16] We find that $S_{+3} = 0.610$. That is, overshooting rates are even higher when we compute overshooting relative to the three-day hence consensus than when we compute overshooting relative to the consensus at date $t_i - 3$, which Table 3 revealed was $S_{-3} = 0.592$. We interpret this as additional evidence that analysts attempt to strategically separate their forecasts from those of others.

3.3 Economic Significance of Forecast Bias

Our frequency results demonstrate that analysts, rather than herd towards the consensus forecast, systematically try to distinguish themselves from other analysts by reporting biased contrarian forecasts. But those findings reveal only limited information about the magnitude of the forecast bias. We now gauge the economic significance of the biases we have uncovered.

We first estimate the relationship between the forecast error and the difference between the

[16] We do not compute overshooting relative to a consensus at an even more distant date to minimize the impact of new, unanticipated information arrival after the analyst's forecast on the consensus.

forecast and the outstanding consensus. Because earnings are reported on a per share basis, we express both the forecast error and its difference from the consensus as a percent of the firm's share price at the end of the previous quarter.[17] The forecast error is then

$$\text{Error}_\tau = \frac{F_\tau - E_\tau}{P_{\tau-1}}. \tag{17}$$

The difference between the forecast and the consensus is

$$SFD_\tau = \frac{F_\tau - \overline{F}_{t-1,\tau}}{P_{\tau-1}}. \tag{18}$$

We relate Error_τ and SFD_τ in the data via the following model:

$$\begin{aligned}\text{Error}_\tau \quad = \quad & \sum_i \text{firm}_i + \alpha_1\, SFD_\tau + \alpha_2 \texttt{lord} + \alpha_3 \texttt{lcov} + \alpha_4 \texttt{lbroksz} + \alpha_5 \texttt{lexp} + \alpha_6 (\texttt{lord}) \times (\texttt{lcov}) \\ & + \alpha_7 (\texttt{lord}) \times SFD_\tau + \alpha_8 (\texttt{lcov}) \times SFD_\tau + \alpha_9 (\texttt{lord}) \times (\texttt{lcov}) \times SFD_\tau \\ & + \alpha_{10} (\texttt{lbroksz}) \times SFD_\tau + \alpha_{11} (\texttt{lexp}) \times SFD_\tau + \epsilon_\tau. \end{aligned} \tag{19}$$

Under the null hypothesis that an analyst's forecast is unbiased, the dependent variable, $\frac{F_\tau - E_\tau}{P_{\tau-1}}$, is essentially the price-normalized forecast error left after the analyst ran a regression using all available information to obtain his best forecast. Were forecasts unbiased, then all coefficient estimates would be zero, because the forecast error would be orthogonal to everything in the analyst's information set, including the independent variables in the regression. The regression model includes measures of the information at an analyst's disposal—the log of analyst order, `lord`, and the log of the number of analysts following the firm, `lcov`. The regression also includes `lbroksz`, the log of $(1 + \texttt{broksz})$, and `lexp`, the log of $(1 + \texttt{exp})$, which capture reputational effects or expertise.

Structural interpretations of the parameter estimates can be obtained under the assumption that analysts strategically introduce a forecast bias that is a linear function of the difference between his (unobserved) true posterior estimate of earnings and the consensus forecast, so that

$$F_\tau = \hat{E}_\tau + a_{n,\tau}(\hat{E}_\tau - \overline{F}_\tau), \tag{20}$$

where $\hat{E}_\tau$ is the analyst's posterior estimate of earnings in a firm-quarter τ given all available information at the moment he issues his forecast. We index the overshooting bias ($a_{n,\tau}$) because it should rise with the amount of uncertainty that the analyst faces about earnings, and hence fall with the number of other forecasts/information at his disposal.[18] The difference between the

[17] We use the previous quarter price to remove the contemporaneous effect of recent forecasts on the stock's price.

[18] In sharp contrast, if the consensus contains more information, an overconfident analyst who ignores all information in the consensus will have a larger expected forecast error for any given deviation from the consensus, SFD_τ.

forecast and consensus forecast as a function of this bias is

$$
\begin{aligned}
F_\tau - \overline{F}_\tau &= \hat{E}_\tau + a_{n,\tau}(\hat{E}_\tau - \overline{F}_\tau) - \overline{F}_\tau \\
&= (1 + a_{n,\tau})(\hat{E}_\tau - \overline{F}_\tau),
\end{aligned} \tag{21}
$$

so that

$$
(\hat{E}_\tau - \overline{F}_\tau) = \frac{F_\tau - \overline{F}_\tau}{1 + a_{n,\tau}}. \tag{22}
$$

We use this relationship to express the dependent variable in our regressions as the sum of a true (unobserved) forecasting error for an analyst, $\frac{\hat{E}_\tau - E_\tau}{P_{\tau-1}}$, which is orthogonal to everything in the analyst's information set, plus a bias term that can be written in terms of the difference between the forecast and the consensus:

$$
\text{Error}_\tau = \frac{\hat{E}_\tau - E_\tau}{P_{\tau-1}} + \left(\frac{a_{n,\tau}}{1 + a_{n,\tau}}\right) SFD_\tau. \tag{23}
$$

After subtracting $\left(\frac{a_{n,\tau}}{1+a_{n,\tau}}\right) SFD_\tau$ from both sides of our empirical model, the left-hand side becomes a normalized regression error from the analyst's forecasting regression, which is orthogonal to everything in the analyst's information set. The sum of the coefficients on $SFD_{t,\tau}$,

$$
\beta_{n,\tau} = \alpha_1 + \alpha_7(\texttt{lord}) + \alpha_8\ (\texttt{lcov}) + \alpha_9(\texttt{lord}) \times (\texttt{lcov}) + \alpha_{10}(\texttt{lbroksz}) + \alpha_{11}(\texttt{lexp}), \tag{24}
$$

provides an estimate of $\frac{a_{n,\tau}}{1+a_{n,\tau}}$. Hence, an unbiased estimate of the strategic bias coefficient is:

$$
\hat{a}_{n,\tau} = \frac{\beta_{n,\tau}}{1 - \beta_{n,\tau}}. \tag{25}
$$

So, too, we can back out the expected dollar bias in the forecast as $\beta_{n,t}(F_\tau - \overline{F}_\tau)$.

A concern with the estimation of equation (19) in our initial sample is the massively fat tails and left-skewness in the distribution of forecast errors (see Table 5): outliers in the tail, especially those in the left tail, will have an excessive impact on estimates, possibly giving rise to misleading results. In all likelihood, most of the extreme left tail observations reflect large asset write-offs that analysts do not try to predict, and hence should not be included in the sample (Abarbanell and Lehavy (2001)). In devising a strategy to address these concerns, we drop observations in the extreme tails of the distribution of the consensus error and check the degree of skewness after each trim. Table 5 provides sample moments after successive trims of 0.5%, 1%, 2.5% and 5% of the tails in the distribution of the consensus error. We then estimate OLS with firm fixed-effects regressions fore each trimmed samples.[19] For these regressions we present both Huber-White's robust standard

[19]See Lim (2001) for a similar approach to outliers. As in Lim, we further minimize the impact of outliers in our price-scaled regressors, Error_τ and SFD_τ, by dropping observations with stock price below \$5.

errors, which account for heteroskedasticity and error correlation within firms (Rogers (1993)), as well as bootstrapped 95% confidence intervals for all coefficient estimates.[20]

Table 6 reports the estimates of the coefficients on SFD as well as those on its interactions with `lord`, `lcov`, `lbroksz` and `lexp` (i.e., $\alpha_1, \alpha_7, \alpha_8, \alpha_9, \alpha_{10}$ and α_{11}) that we use to compute the estimated bias, $\beta_{n,\tau}$, that analysts introduce to their forecasts. Note the high R^2 associated with our regressions, especially when we increase the trim to eliminate gross outliers. Importantly, the estimates reveal that for all trims, the greater is SFD, the greater is the overshooting bias: the greater the forecast consensus differential, the more, on average, by which the forecast overshoots earnings. Further, forecast bias declines with analyst order and coverage, which measure the information that an analyst has. That is, when there is more unresolved uncertainty about earnings, analyst forecasts overshoot by more. To see this, note in Table 6 that the interaction between `lord`, `lcov` and `SFD` dominate the pairwise interactions.

Because the interactions make it difficult to read off the magnitude of the overshooting bias and how it varies with analyst order, we detail the estimated bias for a hypothetical firm followed by 20 analysts, each in their 5^{th} year in the forecasting industry and employed by the median brokerage house (30 employees). Using the estimates from the regression where tails are trimmed by 2.5%, we find that if the second analyst reports a forecast that exceeds the first analyst's by one standard deviation of the mean forecast-consensus difference, then, on average, his forecast overshoots actual earnings by about 0.62% of the stock price.[21] In contrast, for the same difference between the forecast and the outstanding consensus, the 20^{th} analyst to report a forecast, on average, overshoots earnings by 0.54% of the stock's price.[22]

Our structural estimates imply an expected bias of about 1.98 times the difference between the second analyst's (unobserved) true estimate of earnings and the consensus forecast; and an expected bias of about 0.75 times the difference for the last (20^{th}) analyst to report. The bias also falls with brokerage house size, holding `lord`, `lcov`, and `lexp` constant. If we trim the tails of the consensus-forecast error distribution by more, the predicted forecast bias rises, as does the forecast bias by analysts who forecast earlier, relative to those who go later.[23]

[20] The bootstrapping procedure consists of repeating each of the regressions 100 times, with the sampling restricted to *one* observation per individual firm each time. The resulting confidence bounds are therefore not influenced by standard arguments that within-firm error autocorrelation in panel regressions may "too often" lead to the rejection of the null hypothesis (Type I error).

[21] After trimming the 2.5% tails of the distribution of consensus error in our sample, the mean forecast-consensus difference, SFD, is -0.06% (of stock price) and the standard deviation is 0.44 (see Table 5, second panel).

[22] Table 6 shows that forecast bias is not affected by an analyst's experience, as α_{11} is insignificant in all regressions. The variable `lexp` remains insignificant even when we drop `lbroksz` from the regression.

[23] Bias estimates using median regressions (Koenker and Basset (1978)) were qualitatively similar, but about a third of the OLS estimates. The concern with the quantile regression approach is that the large number of firms in

Table 5: Distribution of Forecast Errors

	Percentiles			Sample moments			
	25	50	75	Mean	Std. dev.	Skewness	Kurtosis
			No trim: $N = 378,847$				
Consensus error (as a percent of stock price)	−0.07	0	0.18	0.23	1.90	24.30	1229.4
Forecast error (as a percent of stock price)	−0.10	0	0.10	0.13	1.80	24.70	1442.7
Forecast minus consensus (as a percent of stock price)	−0.11	−0.01	0.04	−0.10	0.96	−22.03	1435.8
			0.5% trim: $N = 375,058$				
Consensus error (as a percent of stock price)	−0.07	0	0.18	0.16	0.79	4.23	31.6
Forecast error (as a percent of stock price)	−0.09	0	0.10	0.08	0.79	−1.69	479.0
Forecast minus consensus (as a percent of stock price)	−0.11	−0.01	0.03	−0.08	0.60	−15.5	1397.0
			1% trim: $N = 371,270$				
Consensus error (as a percent of stock price)	−0.07	0	0.18	0.14	0.63	3.16	18.8
Forecast error (as a percent of stock price)	−0.09	0	0.09	0.06	0.66	−6.06	905.9
Forecast minus consensus (as a percent of stock price)	−0.10	−0.01	0.03	−0.08	0.54	−18.6	2074.8
			2.5% trim: $N = 359,904$				
Consensus error (as a percent of stock price)	−0.07	0	0.17	0.11	0.44	2.15	10.0
Forecast error (as a percent of stock price)	−0.09	0	0.09	0.04	0.50	−15.80	2359.8
Forecast minus consensus (as a percent of stock price)	−0.10	−0.01	0.03	−0.06	0.44	−27.90	4247.6
			5% trim: $N = 340,962$				
Consensus error (as a percent of stock price)	−0.06	0	0.15	0.08	0.31	1.63	6.5
Forecast error (as a percent of stock price)	−0.08	0	0.08	0.03	0.42	−30.30	5350.0
Forecast minus consensus (as a percent of stock price)	−0.09	−0.01	0.03	−0.05	0.38	−42.50	7879.3

Table 6: Economic Significance of Bias in Analysts' Forecasts

Firm fixed-effects regressions are estimated after trimming tails in distribution of *consensus error*. The output omit the coefficient estimates for `lord`, `lcov`, `lord x lcov`, `lbroksz` and `lexp`. Robust standard errors (in parentheses) are computed using the Huber-White method; we also report [in square brackets] the 95% confidence interval of parameter estimates obtained by bootstrapping. (*) and (**) indicate significance 95% and 99% levels.

Dependent variable: $\text{Error}_\tau = \frac{F_\tau - E_\tau}{P_{\tau-1}}$

Regression	SFD	`lord` × SFD	`lcov` × SFD	`lord` × `lcov` × SFD	`lbroksz` × SFD	`lexp` × SFD	R^2
(0.5% trim)	0.612	0.317	0.123	−0.163	−0.044	−0.046	0.21
N: 375,058	(0.126)**	(0.094)**	(0.061)*	(0.037)**	(0.015)**	(0.054)	
	[0.34, 0.85]	[0.11, 0.50]	[−0.01, 0.25]	[−0.22, −0.07]	[−0.07, −0.01]	[−0.15, 0.04]	
(1% trim)	0.756	0.212	0.050	−0.116	−0.031	−0.038	0.28
N: 371,270	(0.129)**	(0.097)*	(0.053)	(0.036)**	(0.014)*	(0.057)	
	[0.53, 1.10]	[−0.00, 0.40]	[−0.04, 0.16]	[−0.17, −0.04]	[−0.06, −0.01]	[−0.15, 0.08]	
(2.5% trim)	0.831	0.185	0.007	−0.094	−0.026	−0.017	0.40
N: 359,904	(0.106)**	(0.087)*	(0.039)	(0.032)**	(0.013)*	(0.057)	
	[0.64, 1.05]	[−0.01, 0.32]	[−0.08, 0.11]	[−0.15, −0.02]	[−0.05, −0.00]	[−0.13, 0.10]	
(5% trim)	0.895	0.190	−0.011	−0.087	−0.029	0.001	0.55
N: 340,962	(0.084)**	(0.075)*	(0.029)	(0.028)**	(0.011)**	(0.044)	
	[0.72, 1.08]	[0.03, 0.30]	[−0.06, 0.05]	[−0.13, −0.03]	[−0.05, −0.01]	[−0.09, 0.07]	

3.4 Analyst overconfidence hypotheses

We now show that analyst overconfidence alone cannot reconcile our findings. To be more precise, we show that *some* strategic behavior is necessary to explain the data. To do this, we derive theoretical upper bounds for overshooting by considering the most extreme form of overconfidence and show that the estimated overshooting by the first few analysts to report *exceeds* even these (extreme) bounds.

Suppose that analyst i receives a noisy signal of earnings, $s_i = E + \epsilon_i$, where $E \sim N(\bar{E}, \sigma_E^2)$ and ϵ_i is normally distributed with mean zero and precision a_i. To generate the most extreme form of overconfidence, we let $\sigma_E^2 \to \infty$, so that the weight in an analyst's forecast on the prior goes to zero. A completely myopic analyst i ignores all information in the forecasts of earlier analysts and submits a forecast of $F_i = s_i$. The true posterior mean/median given signals $s_1, \ldots, s_i$ is $\frac{\sum_{j=1}^{i} a_j s_j}{\sum_{j=1}^{i} a_j}$. For example, if analyst signals are equally noisy, $a_i = a_j$, then the unbiased forecast given signals $s_1, \ldots s_i$ is

$$\frac{\sum_{j=1}^{i-1} a s_j + a s_i}{a i} = \frac{\bar{F}_i (i-1) + s_i}{i}. \quad (26)$$

our sample precluded us from allowing for fixed-firm effects in our estimations (correspondence with Roger Koenker). As a result, the quantile regression explains the data far less well.

Hence, on average, the forecast of a completely myopic analyst i overshoots earnings in the same direction as his forecast exceeds the consensus by

$$s_i - \frac{\bar{F}_i(i-1)+s_i}{i} = \frac{i-1}{i}(s_i - \bar{F}_i) = (1-\frac{1}{i})(s_i - \bar{F}_i). \quad (27)$$

This gross upper bound implies, for example, that the report of an extremely overconfident second analyst will overshoot earnings on average by no more than 50% percent of the forecast-consensus differential. Because our sample is so large, to test for whether overshooting magnitudes exceed this bound, we regress $Error_\tau$ on SFD_τ for the second analyst to report. Using only the subsample circumvents the possibility that the estimated overshooting magnitudes and associated confidence intervals are high due to mis-specification of the model or to strong assumptions about the error structure (compared to a fully-interactive analyst-ordered regression). In particular, the bound on overconfidence *does not* depend on analyst coverage. The data suggest that, on average, the second forecast overshoots earnings by 71% of the price-normalized forecast-consensus difference; this empirical estimate exceeds the 50% theoretical upper bound with better than 99.99% confidence level.

In practice, later analysts get better information, analysts are not completely myopic, and analysts' priors are not diffuse, so that analysts place significant weight on $\bar{E}$. All of these serve to *lower* the degree of overshooting consistent with overconfidence. For example, if $a_i = i$, so that later analysts have more precise signals, then, letting $\hat{F}_i = \frac{\sum_{j<i} jF_j}{(i-1)i/2}$ be the precision-weighted average of earlier forecasts, the best estimate of earnings given all signals is $\frac{i-1}{i+1}\hat{F}_i + \frac{2}{i+1}s_i$, so that analyst i should overshoot by only $\frac{i-1}{i+1}(s_i - \hat{F}_i)$. Similarly, if an analyst i's signal is twice as precise as $i-1$'s, $a_i = 2a_{i-1}$, then analyst i should overshoot by only $\frac{2^{i-1}-1}{2^i-1}(s_i - \hat{F}_i)$.

Table 7 presents empirical overshooting rates collected from separate regressions for later analysts in the forecasting order. It also provides theoretical upper bounds on overshooting consistent with *complete* myopia when later analysts receive better information. Again, these theoretical bounds would be lowered further if analysts' priors were less diffuse, or analysts were only somewhat overconfident. The point estimates of overshooting rates reveal that for any reasonable assumptions on signal noise and information arrival, overshooting rates exceed the magnitudes consistent with overconfidence alone: at least some strategic contrarian behavior is necessary to reconcile the data.

In summary, our tests suggest that professional analysts introduce large overshooting biases into their forecasts—the greater is forecast-consensus differential, the more by which a forecast is expected to overshoot earnings. Also, the estimated amounts by which analysts overshoot fall with the amount of information at their disposal. These estimates support the argument that analysts strategically issue contrarian forecasts, trying to distinguish themselves from others, especially in uncertain forecasting environments. Crucially, the overshooting bias is so large that it can not be ascribed purely to overconfident analysts underweighting the information in the forecasts of others.

Table 7: Overshooting Magnitude and the Over-confidence Hypothesis

This table reports the empirical estimates and theoretical bounds for the SFD coefficient. The empirical coefficient is returned from OLS firm fixed-effects regression after triming the 2.5% tails of the distribution of the *consensus error*. We also report [in square brackets] the 95% confidence interval of parameter estimates obtained by bootstrapping.

Analyst Order	Empirical Overshooting			Theoretical Upper Bounds		
	Estimate	95% Conf. Int.	R^2	$a_i = a$	$a_i = i$	$a_i = 2a_{i-1}$
2^{nd}	0.71	$[0.65, 0.75]$	0.38	0.50	0.33	0.33
3^{rd}	0.84	$[0.66, 0.92]$	0.58	0.67	0.50	0.43
4^{th}	0.73	$[0.57, 0.76]$	0.39	0.75	0.60	0.46
5^{th}	0.77	$[0.60, 0.88]$	0.45	0.80	0.67	0.48
6^{th}	0.63	$[0.57, 0.67]$	0.26	0.83	0.71	0.492
7^{th}	0.72	$[0.62, 0.79]$	0.40	0.86	0.75	0.496
8^{th}	0.62	$[0.52, 0.67]$	0.29	0.88	0.78	0.498
9^{th}	0.61	$[0.52, 0.69]$	0.27	0.89	0.80	0.499
$\geq 10^{th}$	0.51	$[0.44, 0.56]$	0.21	0.90	0.82	0.4995

4 Conclusion

This paper develops a robust frequency test for herding and anti-herding biases in professional analysts' earnings forecasts. Detecting forecast bias in the securities industry is difficult because analysts rely on common sources of information and are surprised by the same events. Our test is designed to be robust to correlated signals among analysts, common unforecasted shocks to earnings, information arrival, and the possibility that the measure of earnings that analysts forecast may differ from the earnings that the econometrician sees. Finally, our test for herding and anti-herding is designed to be robust to the presence of other biases, such as systematic analyst optimism or pessimism. Our test can also be usefully employed to detect bias in the forecasts of macroeconomic variables. There, common data sources and similar econometric models also imply that correlated signals are a significant concern to overcome when attempting to unravel forecast bias.

We find that analysts systematically issue biased anti-herding forecasts that overemphasize their private information. Analysts issue *contrarian* forecasts, biasing forecasts away from the consensus forecast—60% of the time, a forecast overshoots actual EPS in the direction away from the outstanding mean forecast. Similar behavior is found when we look at analyst forecast revisions.

Our structural estimates further reveal that the economic magnitude of the bias in forecasts is large. To our knowledge, these results are new to the literature.

Both our frequency results and structural estimates are consistent with the hypothesis that professional analysts strategically bias their forecasts to try to separate from the forecasts of others—analysts *do not herd*. Importantly, the bias in revised forecasts and the magnitude of the overshooting bias are inconsistent with the hypothesis that the overshooting is solely due to overconfident analysts' failure to incorporate information from the consensus into their forecasts. Distinguishing between these explanations has important policy and research implications. With regard to policy, Odean (1999) documents that private investors are overconfident and act in predictable suboptimal ways. However, the actions of experts working for large institutions likely have far greater impacts on aggregate market outcomes, rendering the presence of myopic private investors less important. With regard to research, our finding that overconfidence cannot explain the data is important in the following sense: it is hard to imagine environments where the assumption that agents behave rationally in accord with their incentives is more appropriate than here, as analysts are trained and experienced, and compensation is highly sensitive to performance. If trained analysts with so much at stake did not act rationally, then it would more generally suggest that models featuring perfectly rational agents must be carefully scrutinized.

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