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Self-Selection and Analyst Coverage

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1. Introduction

This paper examines the relation between analysts' information about a stock's future prospects and their decisions to issue investment recommendations and earnings forecasts for that stock, and the implications of this relation for the observed distribution of recommendations and earnings forecast errors. Articles in the financial press have long argued that analysts are reluctant to issue unfavorable investment information, perhaps because they fear jeopardizing potential investment banking

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business;[1] they fear losing access to management as a source of information;[2] and/or they seek to generate trading commissions.[3] To date, the academic literature has generally suggested that these forces cause analysts to bias their true predictions toward a more optimistic view. We examine an alternative response to disincentives to disclose negative information, that analysts are more likely to provide forecasts and recommendations for stocks about which their true expectations are favorable.

Analyst overoptimism is well documented and researchers have described incentives that could motivate excessive optimism.[4] Dugar and Nathan [1995] and Lin and McNichols [1997] find that analysts whose employers have underwriting relationships with a company issue more favorable investment recommendations than unaffiliated analysts. Francis and Philbrick [1993] conjecture that analysts curry favor with management because management is a source of analyst information; they find that *Value Line* analysts tend to issue more favorable earnings forecasts for firms with less favorable timeliness rankings. Das, Levine, and Sivaramakrishnan [1996] examine whether alternative forecast sources limit analysts' strategic behavior; they find that analysts' forecasts are less optimistic when accurate time-series forecasts are available. These results suggest that strategic concerns influence analysts' forecasts and recommendations.

Some researchers have proposed models in which analysts respond to these strategic concerns by adding bias to their true beliefs.[5] An alternative response, which we explore in this paper, is that analysts are more likely to report on stocks about which they have favorable views. Although either additive forecast bias or self-selection could generate the observed distributions of overoptimistic forecast errors, the two mechanisms have different implications over the life cycle of an analyst's coverage of a given stock. In particular, if analysts report selectively based on their expectations of the firm's prospects, initiating and dropping coverage will be associated with relative optimism and pessimism, respectively, while the same need not be true if analysts bias their reports.

If analysts do not report their expectations when these are sufficiently low, then the lower tail of the distribution of forecasts will be censored,

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[1] Darlin [1983], Gibson and Wall [1984], and Siconolfi [1992; 1995*a*; 1995*b*].

[2] Dirks and Gross [1974], Tannenbaum and Berton [1987], and Siconolfi [1995*b*].

[3] Darlin [1983]. The trading commission explanation presumes that buy recommendations generate greater revenues than sell recommendations. This is argued on grounds that sell recommendations can motivate transactions only by those currently holding the stock or those willing to take more costly short positions, while buy recommendations can generate transactions from a broader set of investors.

[4] See, for example, Lin [1994] on analyst recommendations and O'Brien [1993] on analyst earnings forecasts, and the references therein.

[5] See, for example, Francis and Philbrick [1993] and Krishnan and Sivaramakrishnan [1996].

and average observed forecasts and recommendations will be higher than the (unobserved) mean of all expectations. If an analyst forms and reports unbiased expectations, his reported recommendations and forecasts should be correct, on average. However, if analysts discontinue coverage of a stock or avoid updating their forecasts when new information suggests the stock's future performance will not be favorable, a sample of observed forecasts will be, on average, too high. This occurs because the analyst's prior forecast, though unbiased conditional on the analyst's information at the time it was issued, is no longer an unbiased forecast given the unfavorable information subsequently received by the analyst. Our results suggest that some portion of the pervasive observed overoptimism in analysts' forecasts and stock recommendations stems from censoring of this sort, rather than from analysts adding bias to their true beliefs.

We find that the ratings analysts assign to stocks they have just added to their lists of followed stocks are heavily weighted toward "Strong Buy" recommendations, compared with ratings of stocks with previous recommendations. This is consistent with analysts preferentially adding stocks whose future prospects they view most favorably. We also find the converse result, that the stocks analysts drop tend to have lower ratings than those whose coverage continues.

It is possible that analysts simply skew their recommendations toward extremely favorable ratings at the start of coverage to win favor with management. We distinguish between self-selection and this alternative in two ways. First, we provide evidence that analyst selection is related to fundamental information about the stocks. Realized performance, as reflected in subsequent return on equity, is significantly more favorable for stocks that analysts add than for stocks that analysts have covered in the past, or for stocks that analysts subsequently drop. This suggests that analysts' relative optimism at the start of coverage and relative pessimism near the end of coverage reflect information about future performance, not just "window dressing."

A second distinction between selection based on favorable information and strategic forecast bias can be drawn from the behavior of forecast errors. Self-selection implies that forecasts made prior to a decision to drop coverage will display more overoptimism than forecasts for stocks analysts do not drop. This is true because analysts do not report forecasts when they drop stocks from coverage, and the last forecast prior to dropping coverage will fail to reflect negative information that precipitates the drop decision. Our results support this conjecture and are inconsistent with the alternative that analysts introduce less forecast bias near the end of coverage.

Our paper makes three contributions to the literature. First, we document the influence of self-selection on the distribution of analysts' forecasts and recommendations. Understanding the mechanism by which analysts produce and report information is important for modeling their behavior, empirically or theoretically.[6] Much prior literature on analyst forecasting activity implicitly assumes that the distribution of reported forecasts represents the population of beliefs. Our results suggest that discussions of bias, rationality, and efficiency of forecasts should consider the possibility that the observed distribution of forecasts is censored.

Second, we suggest several ways in which analysts could avoid releasing negative information and provide related empirical evidence. Although the financial press contains suggestions and anecdotes of this avoidance,[7] the extent and the methods of avoidance are, to our knowledge, not documented. In addition to our evidence on self-selection, we document that analysts infrequently issue sell recommendations, although a substantial majority of our sample analysts sometimes do so. We find that downgrades occur more frequently in our sample than upgrades, but we document a longer revision time for downgrades than for upgrades.

Third, our findings on analyst selection are important for understanding the role of mandated financial reports, because analysts' information production affects firms' information environments.[8] Our results suggest that analysts' allocations of effort across firms depend on their expectations of each firm's future performance. If so, analysts gather and disseminate less information for firms with poor future prospects; this view is supported by our finding that analysts issue less frequent forecasts for stocks prior to discontinuing coverage. Furthermore, we find that analysts provide more frequent forecasts and recommendations for newly added firms than for firms with ongoing coverage. This behavior enriches the information environments of firms with good future prospects. Finally, we find that more time elapses before ratings downgrades than before ratings upgrades, suggesting that bad news from analysts appears on a less timely basis.

Section 2 clarifies our use of the words bias and optimism and the implications of these concepts for our study. Section 3 describes our hypotheses, section 4 describes the data, and section 5 provides descriptive statistics for our sample. Section 6 describes the test designs and results and section 7 concludes the paper.

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[6] Some prior and contemporaneous research proposes or documents analyst self-selection. Moses [1990] reports that analysts tend to drop coverage of failing firms in the year prior to a bankruptcy filing. In Trueman's [1996] model of managers' disclosure incentives, analysts cover only firms for which they obtain favorable information. Hayes [1997] models analyst incentives to generate trading commissions and develops conditions under which analysts preferentially announce forecasts that support "Buy" recommendations.

[7] Dirks and Gross [1974], Darlin [1983], and Abelson [1994].

[8] The literature that addresses this issue includes investigations of analyst incentives to follow firms, e.g., O'Brien and Bhushan [1990], Brennan and Hughes [1991], Trueman [1996], Hayes [1997], and Lang and Lundholm [1996].

2. *Bias and Optimism*

We distinguish between the effects of analysts' reporting other than their true beliefs and the effects of analysts' reporting their true beliefs selectively. Both reporting strategies may be motivated by disincentives to report negative information. We present a simple characterization of the empirical implications of forecast bias and selection bias to guide our empirical tests. We first clarify our use of two commonly used but ambiguous terms: bias and optimism.

2.1 FORECAST BIAS AND SELECTION BIAS

One form of bias, which we describe as *forecast bias,* arises when an analyst reports a number different from his true subjective expectation. To be more precise, let A be actual earnings for a given firm and year, and F_{it} be analyst i's reported forecast of A at time t.[9] Let I_{it} be analyst i's information set and subjective beliefs at time t. I_{it} includes observable information, like the past history of earnings and prices, as well as unobservables like the analyst's knowledge of and opinions about the firm, its industry, and the economy. We define the forecast to be unbiased when the announced forecast is the analyst's subjective expectation:

$$F_{it} = E_i (A \mid I_{it}). \tag{1}$$

The forecast is biased if the analyst reports a number different from his subjective expectation or:

$$F_{it} = E_i (A \mid I_{it}) + b_{it}, \text{ where } b_{it} \neq 0. \tag{2}$$

Notice that b_{it}, the forecast bias, is not observable, because the analyst announces only F_{it}.

The term bias is also commonly used to mean the outcome of nonrandom selection. When observations are not random draws from a population distribution, the sample mean is in general a biased estimator of the population mean. We refer to this form of bias using the conventional term, *selection bias.*

Our selection conjecture is that an analyst may report or not, depending on how favorable his information is. In this case, observed forecasts are not random draws from the population of analyst beliefs, even if each analyst reports his true beliefs whenever he reports. To illustrate, assume that analyst i forms an expectation about A based on I_{it} and decides whether to announce the forecast based on whether his beliefs are sufficiently favorable. If we denote a null report by ϕ, and the point at which the analyst decides not to report as k_{it}, the analyst's report is:

$$\begin{aligned} F_{it} &= E_i (A \mid I_{it}), && \text{for } I_{it} \geq k_{it}, \\ &= \phi && \text{for } I_{it} < k_{it}. \end{aligned} \tag{3}$$

[9] We suppress firm and year subscripts for simplicity, since in the current discussion we are concerned with only one firm-year.

As above, I_{it} includes the analyst's information and subjective beliefs at time t, so both the conditioning information I_{it} and the decision cutoff k_{it} may be multidimensional and may vary across firms, analysts, and time.[10] Although we do not model the decision explicitly, we presume that the analyst sets k_{it} by weighing the potential ramifications of reporting bad news (e.g., in firm relations) against those of not reporting (e.g., in brokerage commissions).

In this example, the analyst reports his true beliefs whenever he reports, but the observed forecasts will not be random draws from the population of beliefs; unfavorable beliefs will be underrepresented. If beliefs are correct on average and if analysts always report their true beliefs but do not report their least favorable beliefs, then averages of reported forecasts will be overoptimistic. Further, abstracting from other factors that influence coverage, firms with fewer analysts providing forecasts will have more overoptimistic errors, while those with more are likely to have errors that average to zero.

Our characterizations of forecast bias and selection bias are relevant for recommendations as well as earnings forecasts. If analysts intentionally overstate their earnings forecasts, then the same incentives could induce them to inflate their recommendations as well. Alternatively, if analysts report selectively, then a sample of observed recommendations may be a biased representation of the population.

2.2 RELATIVE OPTIMISM AND OVEROPTIMISM

Optimism is a second term commonly used in more than one sense. We distinguish ex ante from ex post optimism. Much of the prior literature on forecasts uses "optimism" to describe the ex post observable relation between forecasts and their respective outcomes. We describe such measures as *overoptimism*: views that were too favorable in retrospect. We contrast this with *relative optimism*, which we define as a view that is favorable relative to an ex ante benchmark, i.e., one that does not depend on the unknown future outcome.[11]

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[10] Thresholds may vary because, e.g.: (1) an industry-specialist analyst may find it difficult to maintain credibility if he does not cover the dominant firm in his industry; (2) an analyst may find it more costly to issue a negative report about an investment banking client of his employer than about another firm; (3) costs associated with investment banking clients may change when the clients are in need of new capital; (4) costs of issuing negative reports may be smaller, or may be benefits, if the analyst has a brokerage clientele interested in short-selling.

[11] Overoptimism is not a necessary consequence of relative optimism in each case, although selection for relative optimism leads us to expect overoptimism *on average*. For example, an analyst could be relatively optimistic for a particular firm ex ante and if the firm does well, the relative optimism may be correct, or perhaps even pessimistic, in retrospect. We discuss the implications of relative optimism for observing overoptimism in detail in section 3.3.

Our selection conjecture is that analysts choose not to report when their information is sufficiently unfavorable. In our characterization from section 2.1, analyst i does not report when his information is below a threshold, $I_{it} < k_{it}$. This is an ex ante decision, taken before the analyst knows the outcome.

Documenting relative optimism requires establishing observable ex ante benchmarks. For example, if an analyst rates stock A more highly than he rates other stocks, then we could conclude that the analyst is relatively optimistic about stock A. Alternatively, if one analyst's earnings forecast or recommendation for stock A exceeds those made by other analysts at the same time for the same stock, then we could conclude that the analyst is relatively optimistic. The first benchmark is within-analyst, between-firms, and the second is within-firm, between-analysts. It is not clear which is closer to our construct of a threshold determined by weighing the costs and benefits of nonreporting. Under the assumption that the decision is primarily internal to the analyst we use a within-analyst, between-firms benchmark to test for relative optimism. If the analyst's decision is driven by comparisons with his peers, the power of our tests will be low relative to tests based on within-firm, between-analyst comparisons.

To gauge relative optimism, we compare analysts' recommendations for newly added stocks to the same analysts' recommendations for other stocks that are not newly added. Because analysts report recommendations on a simple, finite scale common to all stocks, we can compare relative optimism across stocks. Forecast data, in contrast, are not directly comparable across stocks because *EPS* are at different levels for different stocks.

To motivate our study, we cite prior empirical observations that analysts are overoptimistic. Overoptimism is more easily discernible in earnings forecast data than in recommendations data, because the outcomes are well defined and observable.[12] While analysts' recommendations may implicitly contain expectations, these are not well defined. For example, though it seems reasonable that an analyst who issues a Strong Buy recommendation expects the recommended stock will outperform the market, the precise amount and the timing of the outperformance are ambiguous.[13] Therefore, when we examine conjectures about overoptimism empirically, we focus on earnings forecast data, because the predictions are well defined.

[12] See Philbrick and Ricks [1991] for a discussion of which earnings figure analysts try to forecast. We use actual earnings numbers provided in the same database as the forecasts.

[13] Womack [1996] finds that postevent drift after analyst buy and sell recommendations in 1989–91 is asymmetric. Buy recommendations have a small and short-lived postevent drift, while sell recommendations have postevent drift of −9.1% over six months. He concludes that analysts have stock-picking ability.

3. *Hypotheses*

We will first test whether analysts are relatively optimistic when they add stocks, consistent with selection-based relative optimism. We focus on the decision to add stocks because it is an observable selection event at which analysts announce recommendations. In contrast, while the drop decision is observable, it is not accompanied by a report, so we have no direct test of relative pessimism.[14]

How analysts' relative optimism translates into forecast errors depends on whether their expectations are based on valid information. If not, then ex ante relative optimism implies ex post errors of overoptimism, and likewise for relative pessimism. If analysts' views are informed, however, then absent selection, relative optimism or pessimism has no implications for ex post errors. Relatively optimistic expectations will be borne out in relatively strong realized results and, conversely, forecast error distributions will not depend on the ex ante views of the analyst. In section 3.2, we discuss hypotheses to distinguish whether analysts' add and drop decisions are informed.

We discuss the implications of selection for, and our hypotheses about, analyst overoptimism in section 3.3. The primary effect of selection, and the one that motivates this study, is that it censors pessimistic errors and thereby produces overoptimism. Overoptimism on average does not distinguish selection bias from forecast bias, however. We are able to distinguish between the two by exploiting the observability of drop decisions, even though no report is available. If our selection conjecture is correct, then drop decisions reflect bad news, which is reflected in forecast errors of forecasts made before the drop.

3.1 ANALYSTS' INITIAL AND FINAL RATINGS

We conjecture that analysts will be relatively optimistic when they initiate coverage. If, for example, analysts incur start-up costs to initiate coverage, and if they weigh the costs and benefits of reporting and report selectively, then ceteris paribus, higher costs imply higher thresholds (k_{it}) to add stocks than to continue coverage.[15] In this case, we expect the distribution of ratings for newly added stocks will be shifted toward more favorable ratings, relative to the distribution for stocks with previous coverage. Alternatively, if all analysts' ratings are inflated, but analysts have no particular preferences for adding firms about which they are relatively optimistic, then the distributions of initial and continuing coverage recommendations will be similar. Both would be shifted

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[14] As we discuss in section 3.1, our analysis of recommendations *prior to* drop decisions is indirect, because our tests depend on information arrival as well as selection.

[15] See Hayes [1997] for a model in which adding coverage is more costly than continuing coverage.

toward favorable ratings, but they would not differ.[16] Our first hypothesis, stated in alternate form, is:

*H*1: The distribution of analysts' stock ratings of newly added stocks is shifted toward more favorable ratings, relative to the distribution for stocks with previous coverage.

We test this hypothesis by comparing the distribution of initial ratings for newly added stocks with randomly sampled ratings for stocks that are not newly added.

We also expect that analysts are more likely to drop coverage of stocks about which they are relatively pessimistic.[17] In general, however, we do not observe a recommendation at the drop date, so we will observe relative pessimism in analysts' last recommendations prior to dropping coverage only if the stocks' prospects have deteriorated beforehand. We examine this conjecture as a joint test of self-selection plus the prior trend in information. In alternative form, the joint hypothesis is:

*H*2: The distribution of analysts' stock ratings prior to dropping coverage is shifted toward less favorable ratings, relative to the distribution for stocks with subsequent coverage.

The comparison group for stocks with discontinued coverage is slightly different from that for newly added stocks. We compare newly added stocks with those that had previous coverage. We compare the predrop distribution for dropped stocks with stocks that have subsequent coverage. We discuss this distinction further in section 4, where we describe the data.

3.2 RETURN ON EQUITY FOLLOWING INITIAL AND FINAL REPORTS

Our selection conjecture is that analysts' coverage decisions are based on their expectations of firms' future prospects, which in turn reflect some combination of signal and noise. In the preceding section, we described tests for the existence of relative optimism or pessimism when analysts decide to add or drop a stock. In this section, we describe tests to detect whether these decisions reflect some fundamental information.

Specifically, we compare industry-adjusted return on equity (*ROE*) in the first fiscal year ending after the analyst's add or drop decision to

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[16] Alternatively, analysts might add more bias at the start of coverage and less prior to dropping coverage, yielding relative optimism results identical to those we hypothesize for self-selection. In section 3.3, we discuss this possibility and describe tests that distinguish between it and selection.

[17] If, for example, the benefits to continued coverage increase in the stock's future performance, and the costs of continued coverage do not, the analyst's expected net return could be negative for stocks with sufficiently poor expected performance.

ROE in a randomly selected year for stocks with continuous coverage. We measure *ROE* as the company's return on equity less the median return on equity of stocks in the same two-digit *Compustat SIC* code for the same *Compustat* year.[18] Prior research suggests that analysts' forecasts are informative.[19] If their selections are based on valid information, then newly added stocks will subsequently perform better than stocks with prior coverage. Formally:

*H*3: The median industry-adjusted return on equity is greater for newly added stocks than for those with prior coverage.

Similarly, realized performance of stocks that are dropped will be weaker than the performance of stocks receiving ongoing coverage.

*H*4: The median industry-adjusted return on equity is greater for stocks receiving continuous coverage than for those that are subsequently dropped.

3.3 ANALYSTS' INITIAL AND FINAL FORECAST ERRORS

Both positive forecast bias and self-selection based on relative optimism imply that, if analysts' true (but unobservable) beliefs are unbiased on average, then observed forecasts will be overoptimistic, on average. In the case of forecast bias, analysts add a positive amount to their true beliefs; in the case of self-selection, for any given realization, high forecasts are both more likely to be observed and more likely to be overoptimistic than low forecasts.

However, self-selection (but not bias) implies that forecasts made prior to an analyst's dropping coverage of a stock will be more overoptimistic than forecasts for other stocks. Our tests exploit the observability of drop decisions, even though the analyst does not issue a forecast when he drops a stock. Thus, the final observed forecast for a dropped stock comes from an earlier date and does not reflect the information that caused the drop, but the final forecast *error* reflects this information (see figure 1). In our conjecture this is negative information. Firms with continuing coverage will exhibit the base level of forecast overoptimism due to selection bias in reported forecasts, while dropped firms will have an oversampling of relatively optimistic forecasts, along with ex post news that convinced the analyst that his relative optimism was overoptimism.[20] This leads us to the hypothesis:

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[18] We calculate return on equity as income before extraordinary items and discontinued operations divided by the average of beginning and end-of-period shareholders' equity, using data on the 1995 *PC Plus Compustat Annual Industrials Database*.

[19] See, for example, Stickel [1991] and Lys and Sohn [1990].

[20] *H*2 predicts analysts are relatively less optimistic about stocks they subsequently drop, and *H*5 predicts analysts are more overoptimistic about stocks they subsequently drop.

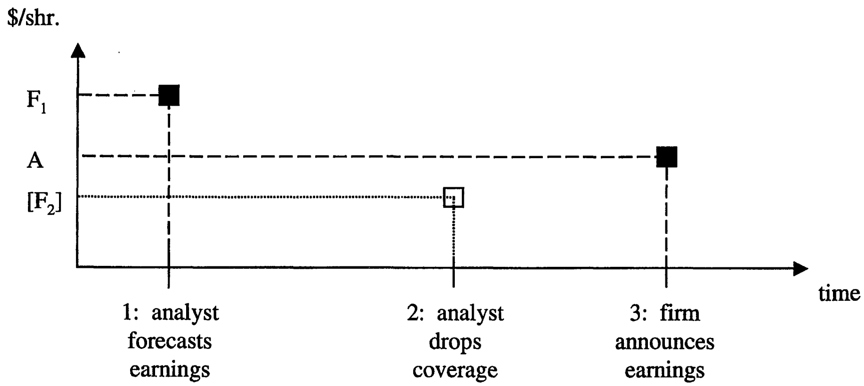

FIG. 1.—Analyst decision to drop coverage after making a forecast. The analyst forecasts earnings (A) based on information available at time 1 and reports his forecast F_1. The analyst receives information between 1 and 2 that causes him to drop coverage. Forcast F_2 at date 2 is lower than F_1, but is not observed. The error from the last observed forecast $A - F_1$ reflects the information that caused the analyst to drop coverage at time 2, along with information arriving between 2 and 3 .

*H*5: Earnings forecast errors from the last forecast prior to an analyst's discontinuing coverage of a stock are more negative than forecast errors for stocks with new or continuous coverage.

We test *H*5 by comparing the distribution of final forecast errors for stocks that analysts drop to randomly sampled errors from the distribution for stocks with new or continuous coverage. We define continuous coverage as coverage from the later of the date the analyst adds the stock or the start of the database to the earlier of the end of the database or the end of the analyst's tenure with his firm. Because forecast errors become more accurate as the fiscal year-end approaches, we separate the forecast errors into similar-horizon groups and test the hypothesis for each horizon separately.

*H*5 distinguishes self-selection from competing explanations for analysts' relative optimism about newly added stocks and relative pessimism about dropped stocks. Suppose analysts attempt to curry favor with management by adding bias to their recommendations and forecasts. An analyst who is most concerned with management relations at the start of coverage, and least concerned at the end, will add larger forecast bias for newly added stocks and smaller (or nonexistent) forecast bias before

The difference in the predictions for these two hypotheses stems directly from the fact that *H*2 examines recommendations which reflect the analysts' *expectations prior to dropping,* whereas *H*5 examines forecast errors which reflect *information subsequent to the analysts' last forecasts.* *H*2 aims to capture negative information reflected in reports prior to drop decisions, whereas *H*5 aims to capture negative information arriving after the analyst's final report that is realized subsequently in earnings.

dropping coverage. This pattern of forecast bias could generate apparent relative pessimism at the end of coverage, as we conjecture in section 3.1. It suggests, however, that observed overoptimism should be smallest for dropped stocks, because forecasts would be less positively biased.

Likewise, if the analyst's relative pessimism prompting the drop decision is due to erroneous information, then dropped stocks might exhibit a negative "winner's curse." In this scenario, final forecasts for dropped stocks would exhibit the same degree of overoptimism as other forecasts, because the drop decision reflected invalid information.

4. *Data and Design*

4.1 THE DATABASE

The source of our analyst data is a proprietary database from *Research Holdings, Ltd.*, covering more than 4,000 analysts employed at 272 different research firms. The database begins in July 1987, and the version we use in this paper runs through December 1994. It contains several types of records, of which we use three: forecast annual *EPS* records, opinion records, and actual annual *EPS* records. A forecast or opinion record contains the analyst's name and the name of his employer, the *CUSIP* of the followed stock, the forecast or recommendation and the date it was issued, and the earnings year (forecast records only). Only annual *EPS* forecasts are included in the database. In addition to the individual analysts, the database contains more than 900 multiple-analyst teams, identified by several analysts' names appearing on one record. Actual *EPS* records contain the *CUSIP*, the fiscal year-end date, the earnings year, and, most typically, primary *EPS* before extraordinary items and the results of discontinued operations, as provided to *Research Holdings, Ltd.* by *Standard & Poor's*.[21] Since the actual *EPS* data in this database begin in 1990, our tests on forecast errors cover 1990–94.

Research Holdings, Ltd. codes opinions 1 through 5, where 1 indicates a "Strong Buy" recommendation and 5 indicates a "Strong Sell."[22] Alphabetic codes identify several other distinct events, including dropped or discontinued coverage (meaning the analyst has stopped covering a particular stock, at least for a time). Discontinuations in coverage are detected by *Research Holdings, Ltd.* when a stock fails to appear in a publication from the research firm which previously included it, or by the passage of time if the analyst firm does not routinely provide complete

[21] *Research Holdings, Ltd.* provided primary *EPS* including extraordinary items for approximately 8.5% of the forecast records. None of our inferences is affected when we instead use primary *EPS* before extraordinary items from *Compustat*.

[22] Some brokerage firms use only three categories of stock recommendations: buy, hold, and sell. *Research Holdings, Ltd.* generally maps these recommendations into ratings of 1, 3, and 5, respectively. We replicated all analyses using recommendations excluding observations from analysts employed by brokerage firms using only three categories and find our inferences are unchanged.

lists of covered stocks. In either case, *Research Holdings, Ltd.* staff confirm discontinuation of coverage by a phone call to the analyst.[23]

4.2 SAMPLE SELECTION

We focus on a sample of analysts and examine all their forecasts and recommendations, rather than focusing on a sample of firms and examining all the analysts that cover them. This focus allows us to control the sample analysts' characteristics and incentives. In addition, our design is not confounded by coverage changes due to variations in the brokerage firms included in the database or variations in the aggregate number of analysts in the economy. This helps ensure that add and drop decisions are due to analysts' assessments of the value of providing coverage, rather than to other factors. Finally, our design allows us to provide direct evidence on the magnitude of turnover in analyst coverage, and therefore a sense of how much selection occurs.

Our goal is to obtain a broadly representative sample of analysts, with sufficient data for each analyst to permit meaningful testing. We must observe an analyst long enough to distinguish when coverage of a stock is new. We must also observe the analyst covering a sufficient number of companies to allow comparisons. We performed exploratory tests on the first 1,000 analyst names in the database to get a sense of how restrictions on these two dimensions, minimum longevity and minimum coverage, would affect a sample. Based on these tests, we restrict our sample to analysts who report on at least five companies and who are in the database for at least two years.[24]

Beginning with the 4,911 individuals in the database, we eliminate 983 who report on fewer than five *CUSIPS* and 1,963 more who are in the database less than 24 months, leaving 1,965 that satisfied our longevity and coverage restrictions. We then exclude 133 analysts affiliated with *Value Line, Duff and Phelps,* or *Standard & Poor's,* since we believe these analysts may have different incentives than those at broker firms. We sample 541 analysts (about 30%) randomly from the 1,832 remaining.[25] We then attempt to eliminate redundancies in two ways. First, if two or more analysts from the same multiple-analyst team are in our sample, we eliminate all but one and assign the team records to the

[23] Analysts do not routinely announce they are dropping coverage of stocks. Our sense is that drops in coverage are visible to investors with a lag, particularly when research recommendations are not published on a routine schedule.

[24] In principle, it is possible to track analysts when they change jobs, but we have not done so because of ambiguous names and overlapping dates. In what follows, "analyst" refers to a single person employed at a single investment firm. Our discussions assume that individual analysts, not their employers, make coverage decisions. Our tests do not distinguish who makes the decision, but we will not find evidence of selection unless these decisions are made based on factors related to expected future performance.

[25] While we could have analyzed the entire population of analysts that met our longevity and coverage constraints, we chose to sample since some of our data checks require hand-matching of names.

remaining analyst. We eliminate five analysts by this procedure. Second, if our sample contains the same person working at two different research firms, we eliminate one employer for that individual. Thirteen analyst/employer pairs are eliminated by this procedure, leaving us with 523 analysts. Thirteen of these analysts produce no recommendations, and two produce no forecasts. The sample analysts, in total, report on 3,774 different companies, are employed at 129 different research or brokerage firms, and generate 13,258 analyst–company combinations.

Table 1 provides information on the sample analysts' recommendation and earnings forecast activity. Table 1 shows that we observe these analysts, on average, for approximately four years, during which they report on about 25 stocks. Analysts produce many more earnings forecasts than recommendations, averaging 95.5 forecasts per year and 17.0 per *CUSIP*, but only 18.8 recommendations per year and 3.4 per *CUSIP*.[26] While analysts often issue forecasts for both the current year and one year ahead, this factor does not explain the more than fourfold difference in activity. Most of the variables in table 1 have long upper tails, representing a few very prolific analysts, but the medians and means give similar pictures overall.

4.3 DEFINING INITIAL AND DROPPED COVERAGE AND COMPARISON GROUPS

The two critical events for our analysis of selection are analysts' initiations and discontinuances of coverage. We identify discontinuances from the database encoding, described in section 4.1. To identify initiations, we develop filters to distinguish between an analyst's initial report on a company and that analyst's first appearance in the database. On any one date, an analyst may update only a subset of all forecasts and recommendations, so his first appearance in the database need not contain a full list of stocks covered. We define all the companies listed by an analyst within the first six months after his first appearance in the database as "original coverage" and consider a company to be added only if it first appears more than six months after the analyst's first appearance. Analysts occasionally provide long lists of new companies on a single date, which we believe may correspond to changes in analysts' assignments. Since we believe these sudden changes in overall focus are fundamentally different from analysts deciding to add new companies within the ordinary course of financial analysis, we exclude them and restrict our definition of "added" companies to those added on dates with at most two other new companies.

Since the database code for dropped coverage is associated with a verified decision, we do not need to apply filters for "dropped" stocks as we do for "added" ones. In some cases, however, the final forecast for a

[26] In fact, these numbers slightly understate total forecasts per *CUSIP*, since the forecast data begin in January 1988, while the recommendation data begin in July 1987.

TABLE 1

Recommendation and Earnings Forecast Activity of 523 Sample Analysts[a]

	Mean	Std. Dev.	Min.	25%	Median	75%	Max.
Panel A: Recommendation Activity—510 Analysts							
Number of Stocks Followed	24.8	16.5	5	15	21	31	151
Number of Days from First to Last Rating	1460.8	671.7	0	902	1325.5	2065	2660
Number of Ratings Produced	76.2	79.9	1	29	53	94	656
Average Number of Ratings Per Year	18.8	16.1	1.6	9.7	14.5	22.0	146
Average Number of Ratings Per Stock	3.4	3.4	1	1	2	4	41
Average Days between Ratings Per Stock	198.6	150.5	1.0	95.1	163.0	255.7	1214.5
Panel B: Earnings Forecast Activity—521 Analysts							
Number of Stocks Followed	25.4	20.1	5	15	21	31	212
Number of Days from First to Last Forecast	1493.9	613.0	245	974	1370	2105	2511
Number of Forecasts Produced	410.9	360.5	12	155	326	568	3611
Average Number of Forecasts Per Year	95.5	71.1	7.7	51.8	79.8	124.5	916.0
Average Number of Forecasts Per Stock	17.0	14.5	1	6	13	24	100
Average Days between Forecasts Per Stock	54.6	30.6	1.0	36.0	48.1	65.5	456.4

[a]*Research Holdings, Ltd.* provided the database from which our sample is drawn, which covers the period July 1987 through December 1994. We draw analysts at random from among analysts with at least two years in the database and who cover at least five firms. Two of our sample analysts produce no earnings forecasts. Thirteen of our sample analysts produce no stock recommendations.

dropped stock might be quite stale. We limit our analysis to forecasts made within 90 days of the decision to drop the stock, to ensure that our results are not driven by the inaccuracy of extremely outdated forecasts.

For comparisons with "added" stocks, we use the stocks defined above as "original coverage," or alternatively we use observations we denote as "previous." The "previous" group are analyst-*CUSIP* observations where the analyst report was preceded by a previous report for the same stock by the same analyst. The advantage of the "original" group is that it contains stocks covered by the same analysts, but disjoint from the "added" group. However, the group may also contain some misclassified "added" stocks, depending on the quality of our filters for "added" stocks. Misclassifications will reduce the power of our tests to distinguish differences. The advantage of the "previous" group is that it consists of verified instances of continuing coverage.

For comparison with "dropped" firms, we also use two different groups. "Continuous" stocks have continuous coverage from the later of initiation by the analyst or the start of the database, to the earlier of the end of the database or the end of the analyst's tenure with the brokerage firm. The "subsequent" group are analyst-*CUSIP* observations where the analyst issued a subsequent report for the same company. The "continuous" group (like the "original" group) is a disjoint set of firms covered by the same analysts. In this case, misclassifications are due to the limitations of the database: we can only ensure that these stocks are never dropped within the period covered by the database. Again, misclassifications will reduce the power of our tests. The advantage of the "subsequent" group is that it consists of verified instances where the report does not immediately precede a decision to drop coverage.

Our choices of comparison groups raise three design issues. First, the definitions of several groups allow multiple observations for a given analyst-*CUSIP*. To ensure that repeated observations do not drive our results, we randomly sample one observation per analyst-*CUSIP* from the "previous," "subsequent," and "continuous" groups. Second, different comparison groups include different subsets of our sample analysts. For example, 450 of our 523 sample analysts drop at least one stock in our observation period, while 467 provide uninterrupted coverage of at least one stock. The intersection of these two subsets contains 395 analysts. To ensure that our results are not influenced by inherent differences in thresholds across analysts, we have replicated each comparison using only the intersection of the two subsets of analysts. None of our inferences is affected by this difference in design. Third, because dropping a stock does not necessarily cause another to be added at the same time, we do not construct matched pairs for tests in which the "added" and "dropped" subsamples are compared directly. Our comparisons of "added" and "dropped" stocks are made across analysts rather than for matched pairs of stocks added and dropped by the same analyst at the same point in time.

5. Analysts' Reporting and Effort Allocations Decisions

This paper provides tests of the hypothesis that analysts' decisions about coverage of stocks are related to their expectations of the firms' future prospects. However, analysts make several other choices that affect their earnings forecasts and recommendations, and these choices affect the interpretation of our results based on earnings and recommendation data. In this section, we provide descriptive evidence on choices that relate to the design and interpretation of our primary hypothesis tests. One set of choices includes ways (other than dropping coverage) that analysts may avoid issuing negative reports. The second set of choices includes analysts' decisions about how much effort to allocate to covering a given stock.

5.1 NEGATIVE REPORTS

Although the financial press contains many allegations that analysts avoid reporting negative information,[27] the precise way in which this avoidance manifests itself in observable forecasts or recommendations has not been documented. We examine three possible forms of avoidance: analysts may avoid issuing unfavorable, or sell, recommendations; analysts may avoid revising their recommendations downward; and analysts may delay downward revisions. We provide descriptive evidence on: analysts' use of the "Sell" and "Strong Sell" ratings relative to "Strong Buy," "Buy," and "Hold" categories; the relative numbers of rating downgrades and upgrades, in total and conditional on the starting recommendation; and the time between successive ratings for downgrades and upgrades.

Table 2 is a transition matrix, showing the median number of calendar days between successive ratings and (in parentheses) the number of changes from each rating category to each other rating category. It is evident from table 2 that the sell categories, 4 and 5, are used much more sparingly than the buy and hold categories.[28] This phenomenon has been noted by others, including Lin [1994]. Sell recommendations make up only 9.5% of analysts' ratings in our sample; but analyst use of sell categories is not restricted to a small subset of stocks, analysts, or analyst firms. Of the 3,774 stocks covered by our sample analysts, 1,148, or 30.4%, receive ratings of 4 or 5 at some time. These rating categories are used by 374, or 73.3%, of our 510 sample analysts making recommendations. The analysts issuing sell recommendations work at 102 different firms, or 79.1% of all analyst firms represented in our sample. Therefore, it appears that sparse use of sell recommendations

[27] Dirks and Gross [1974], Darlin [1983], Gibson and Wall [1984], Galant [1990], Siconolfi [1992; 1995*b*], and Browning [1995].

[28] These characteristics are clear in the data, so we have dispensed with formal hypotheses and statistical tests.

TABLE 2
Median Days between Ratings

To	From 1	2	3	4	5
1	187.0	95.0	151.0	63.0	94.5
	(499)	(2398)	(2374)	(47)	(38)
2	122.0	153.0	105.0	57.0	92.0
	(2544)	(306)	(3738)	(301)	(31)
3	176.0	120.0	198.0	63.0	91.0
	(2866)	(4089)	(817)	(1393)	(519)
4	205.0	91.0	92.0	175.0	58.0
	(61)	(296)	(1409)	(46)	(146)
5	199.5	114.0	182.0	76.0	91.0
	(100)	(36)	(578)	(143)	(28)
Upgrades		95.0	121.0	63.0	90.0
		(2398)	(6112)	(1741)	(734)
Downgrades	154.0	119.0	119.0	76.0	
	(5571)	(4421)	(1987)	(143)	

Median days between ratings for all upgrades: 98.0 (10985).
Median days between ratings for all downgrades: 127.5 (12122).
The Wilcoxon z-statistic (probability value) for the hypothesis that the median number of days is the same for all upgrades and all downgrades is 13.39 ($p < 0.001$).
This table shows the median calendar days between successive analyst ratings for the same stock, classified by initial ("From") and subsequent ("To") rating. The rating categories correspond to analyst recommendations, with 1 = Strong Buy and 5 = Strong Sell. The sample includes ratings issued by 510 analysts between July 1987 and December 1994. The number of observations for each cell is shown in parentheses.

is a general phenomenon: most analysts use these categories, but only occasionally.

The transitions indicate that analysts downgrade their stock recommendations more often than they upgrade. This is true both overall and for almost every symmetric pair of transitions (e.g., downgrades from 1 to 3 occur more often than upgrades from 3 to 1). However, conditional on the starting point, is the analyst more likely to upgrade or downgrade? We exclude starting points 1 and 5 because there is only one direction to move from these points, and we find mixed evidence on transitions from 2, 3, and 4. Analysts downgrade from 2 to 3 more often than they upgrade from 2 to 1, but the reverse is true for one-step moves starting from 3 or 4.

Finally, we examine the timeliness of reported ratings upgrades and downgrades. If analysts prefer not to downgrade, the average time between recommendations will be greater for downgrades than for upgrades.[29] We measure the time in calendar days between sequential ratings and partition observations into upgrades and downgrades. We

[29] We cannot isolate analysts as the sole cause of delays, since our observation of their behavior may be confounded if good news and bad news arrive at different rates. If bad news is delayed by managers or other news sources, analyst ratings changes may merely reflect exogenous information arrival.

test whether the distribution of times is the same for upgrades and downgrades using the Wilcoxon rank-sum test. Two patterns are notable in table 2. First, for every symmetric pair of transitions (e.g., downgrades from 1 to 3 versus upgrades from 3 to 1), the median number of days between ratings is smaller for the upgrade. Consequently, as reported below the transition matrix, the median number of days between ratings for upgrades overall is significantly less than the median days between ratings for downgrades, indicating that analysts report unfavorable information on a less timely basis.[30] Second, and relatedly, the median number of days between ratings is lowest for transitions from ratings 4 and 5, suggesting that analysts review their negative ratings more frequently and, when possible, tend to upgrade from these categories relatively quickly.

In summary, table 2 is consistent with analysts avoiding issuing negative information about stocks they follow in two ways: they use "Sell" ratings infrequently and for relatively brief periods, and they appear to delay downgrades. While analysts do not avoid downgrading altogether (downgrades are more numerous than upgrades), their overwhelming use of favorable initial ratings makes the greater numbers of downgrades unsurprising.

5.2 COVERAGE INTENSITY

We expect that analysts can modify the intensity of their coverage of stocks depending on the expected returns to their effort. One observable indicator of coverage intensity is the frequency of revisions in ratings and forecasts. If analysts add stocks when they are relatively optimistic about them, and if analysts expect greater rewards when they are more optimistic, then we expect newly added stocks to receive greater allocations of effort and therefore more intense coverage than stocks with continuing coverage. We also expect that analysts will allocate less effort to stocks prior to dropping coverage. It is important to note that this expectation results from analysts' expected rewards and does not follow mechanically from our other results. In particular, if analysts rate newly added stocks as "Strong Buys" more frequently than they do stocks with previous coverage, then they are also more likely to downgrade newly added stocks—simply because no upgrade is possible from "Strong Buy." Also, analyst delay in downgrading would work against our expectation, as analysts would revise their ratings of newly added stocks more slowly.

We measure the time between successive ratings (forecasts) as the average number of days between ratings (forecasts) for a particular analyst-*CUSIP*. We compare the distribution of revision times for newly added stocks with the distribution for stocks with ongoing coverage, using the Wilcoxon rank-sum test. We make a similar comparison between stocks

[30] Also, analysts may simply allocate less effort to stocks for which the initial information is unfavorable, as in Hayes [1997].

TABLE 3

Comparison of Median Days between Forecasts or Ratings by Coverage Classification

Median Number of Days Between	Forecasts	Ratings
Panel A: Added vs. Original Companies		
All	48.1	163
	(11,511)	(7,556)
Added	42.4	129.7
	(4,163)	(2,544)
Original	51.3	182.6
	(7,348)	(5,012)
Wilcoxon z-statistic	23.4	18.2
	(< 0.001)	(< 0.001)
Panel B: Continuous vs. Dropped Companies		
Continuous	45.6	164.1
	(5,689)	(3,913)
Dropped	47.9	131.4
	(3,538)	(2,064)
Wilcoxon z-statistic	5.1	11.3
	(< 0.001)	(< 0.001)

This table shows the median days between forecasts (ratings) issued by 521 (510) analysts from July 1987 to December 1994, for all available observations, and for several subsamples defined below. To be included in this table, an analyst must cover a given *CUSIP* for at least two days. The Wilcoxon rank-sum test, with continuity correction, tests whether the distributions of ranks of number of days between forecasts (ratings) are the same. Rejection with a positive z-score indicates that Added (Continuous) analyst-*CUSIP* observations have shorter revision intervals than Original (Dropped) observations.

Definitions of Coverage Classification

Classification:	**Definition:**
All	All stock ratings (forecasts) by 510 sample analysts between July 1987 and December 1994, for stocks with at least two days of coverage.
Added	An analyst-*CUSIP* for which the first rating of the stock by the analyst (1) occurred more than six months after the analyst's first appearance in the database and (2) occurred on a date with no more than two other new stocks.
Original	An analyst-*CUSIP* for which the first rating of the stock by the analyst (1) occurred within six months of the analyst's first appearance in the database or (2) occurred on a date with three or more other new stocks.
Continuous	An analyst-*CUSIP* for which ratings are issued continuously (without discontinued coverage) from the first time the analyst-*CUSIP* appears in the database to either (1) the end of the database or (2) the analyst's departure from his/her firm.
Dropped	An analyst-*CUSIP* for which ratings are discontinued, and for which no subsequent ratings or earnings forecasts are issued.

whose coverage is discontinued and stocks with continuous coverage by a sample analyst.

Table 3 contains an analysis of analysts' coverage intensity, as measured by revision frequency. We find that analysts revise both forecasts and ratings significantly more frequently for "added" firms than for "original" firms, and that analysts revise their forecasts significantly less frequently for firms whose coverage is eventually discontinued than for firms with continuous coverage. Contrary to our expectations, however, we find that analysts revise their ratings significantly more frequently for firms whose coverage is eventually discontinued than for firms with continuous coverage.

A second approach to measuring coverage intensity is to examine analysts' ex post accuracy in forecasting earnings. Allocating greater effort to newly added stocks should yield more accurate forecasts for these stocks. We also conjecture that analysts devote less effort to stocks they subsequently discontinue, but in this comparison we face the difficulty that we do not observe a forecast when the analyst drops coverage. Therefore, the relative accuracy of final forecasts prior to drops depends on whether drops generally follow a period in which the analyst decreases his effort.

We measure forecast accuracy by the absolute value of the price-deflated forecast error. As we described in section 4.3, we partition analyst-*CUSIP*s into "added" or "original" for initial coverage, and "dropped" or "continuous" for later coverage. We compare the accuracy of initial forecasts for "added" stocks to forecasts for "original" stocks at a randomly selected date, using a Wilcoxon rank-sum test. Similarly, we compare the accuracy of final forecasts for "dropped" stocks to forecasts for "continuous" coverage stocks at a randomly selected date. To control for horizon effects, we partition the forecast errors into five horizon groups based on the number of calendar days between the forecast date and the fiscal year-end for which the analyst forecasts earnings. We compute forecast errors by subtracting the forecast from reported *EPS*, so that negative forecast errors correspond to bad news at the earnings release or overoptimistic forecasts.

Table 4 indicates that forecasts for newly added stocks are more accurate than those for other stocks over all five horizons, while forecasts for dropped stocks are less accurate than those for stocks with continuous coverage, over four of the five horizons. These results are consistent with the evidence in table 3 that analysts revise forecasts more quickly for newly added stocks, suggesting that analysts allocate more effort to forecasts about stocks they add and, conversely, for stocks they drop.

Greater accuracy for newly added stocks seems counterintuitive from the perspective of learning or experience. In this perspective, forecast accuracy for a given stock should improve with the analyst's experience, ceteris paribus. Clement [1997] and Mikhail, Walther, and Willis [1997] document that analysts are more accurate for stocks they have covered for longer periods of time. However, the difference between their results and ours may well be due to differences in design. We do not compare an analyst's performance through time with the same stock. Instead, we compare sample analysts' performance on all the stocks they cover and distinguish newly added stocks from others. Our results may be explained by a difference in effort levels at different points in the "life cycle" of analyst coverage of a stock. Clement and Mikhail, Walther, and Willis do not distinguish between new coverage and, for example, recent entry into their database, so the event of new coverage is not cleanly defined. If analysts apply more effort to covering newly added

TABLE 4

Comparison of Median Absolute Forecast Errors by Coverage Classification and Forecast Horizon

Panel A: Median Absolute Price-Deflated Forecast Errors, by Coverage Classification (Number of Observations in Parentheses)

	Horizon[a]				
Coverage Classification[b]	0–90	91–180	181–270	271–365	> 365
Added	0.0045	0.0076	0.0100	0.0149	0.0177
	(857)	(846)	(815)	(763)	(354)
Original	0.0056	0.0095	0.0135	0.0171	0.0244
	(1076)	(1055)	(1052)	(1120)	(2910)
Continuous	0.0045	0.0086	0.0111	0.0133	0.0223
	(910)	(899)	(753)	(812)	(1528)
Dropped	0.0065	0.0110	0.0181	0.0328	0.0214
	(248)	(242)	(237)	(214)	(201)

Panel B: Wilcoxon z-Statistics (Probability Values) for Selected Comparisons[c]

	Horizon[a]				
Comparison	0–90	91–180	181–270	271–365	> 365
Added vs. Original	3.22	3.63	2.63	1.91	3.12
	(0.0013)	(0.0003)	(0.0086)	(0.0558)	(0.0018)
Continuous vs. Dropped	2.38	2.76	3.77	5.19	0.73
	(0.0171)	(0.0058)	(0.0002)	(0.0001)	(0.4622)
Added vs. Dropped	2.84	3.73	4.04	4.76	2.19
	(0.0045)	(0.0002)	(0.0001)	(0.0001)	(0.0283)

[a]The forecast horizon is the number of calendar days between the forecast date and the end of the fiscal year for which earnings are forecast.

[b]Coverage classifications are defined in table 3.

[c]The Wilcoxon rank-sum test, with continuity correction, tests whether the distributions of ranks of absolute forecast errors are the same. Rejection with a positive z-score indicates that absolute forecast errors for the Original (Dropped) subsample are greater than for the Added (Continuous) subsamples. Two-tailed probability values are shown in parentheses.

stocks than previously covered stocks, perhaps their effort dominates the effect of learning at this point.

6. *Tests and Results*

6.1 ANALYSTS' INITIAL AND FINAL RATINGS

To document whether analysts tend to add stocks for which they hold relatively favorable views, we first classify the earliest recommendation from each analyst on a given stock as "added" or "original," using the criteria described above in section 4 and in the text of table 5. Table 5 and figure 2 show the distribution of stock ratings across a variety of classifications, including these two, and tests of differences between groups.

The distribution of ratings in the "added" group is consistent with $H1$, the hypothesis that analysts add stocks when they are relatively optimistic, since 1 is the modal rating, assigned to 44% of newly added stocks. The modal rating in the "original" group is 3. We believe our criteria for defining "added" stocks are such that the "added" group has relatively few misclassifications, while the "original" group probably contains some misclassified "added" stocks. Misclassifications in either group will work against our finding differences between these two groups, but nevertheless we find strong differences. The chi-squared test of independence, reported at the bottom of table 5, strongly rejects the null that the ratings distributions are the same for the "added" and "original" groups.[31]

Our second comparison in table 5 and figure 2B is between "added" stock ratings and (revised) ratings in the "previous" group. Initial ratings for "added" stocks are more favorable than ratings in the "previous" group, and the difference in ratings is statistically significant.

Table 5 and figure 2 also report the results of investigating *H*2, that analysts drop coverage of firms when their expectations of future performance are less favorable, and that their ratings prior to dropping coverage at least partially reflect these less favorable expectations. Comparisons of the distribution of the last ratings by analysts prior to dropping the stock, labeled "dropped," with the distribution of ratings for stocks with subsequent coverage show the "dropped" distribution is weighted toward less favorable ratings, and the difference is statistically significant. The evidence therefore supports *H*2. The ratings of the relatively small number of stocks where analysts have resumed reports after discontinuing them for a time (the "resumed" group) are significantly more favorable than either the ratings for stocks with previous coverage or the ratings for the "resumed" group prior to discontinuing coverage.

In summary, we find strong evidence that analysts add stocks for which they can offer favorable recommendations. They rate more than two-thirds of newly added stocks buys or strong buys, and strong buy is the modal rating. Among stocks that are not newly added, roughly 50% are rated buy or strong buy, but analysts most often recommend holding. We also find that analysts tend to discontinue coverage of stocks whose future prospects they view unfavorably and resume reports after a discontinuation for a subset of stocks whose future prospects again appear favorable. We regard this as strong evidence that analysts self-select to report on firms for which they can make favorable reports. If analysts' incentives simply led them to raise all their reports toward favorable rating categories, the ratings of added stocks would not differ from the ratings of other groups.

The frequency with which analysts add and drop stocks is also noteworthy. Of the 13,258 analyst-company observations for our sample period, which averages four years per analyst, 4,008 stocks were added and 3,663 were dropped. This frequency, in conjunction with the evidence that coverage decisions are related to analysts' expectations about company performance, suggests that such decisions may have a significant impact on information available to investors about stocks.

[31] Although the chi-squared test provides no information about the direction of differences, the direction is clear from visual inspection of the distributions. See figure 2A.

TABLE 5

Distribution Frequencies and Proportions of Analyst Recommendations by Coverage Classification

Panel A: Distribution Frequencies and Proportions of Analysts' Recommendations

	Rating[a]					
Coverage Classification[b]	1	2	3	4	5	Sum
All	10356	9894	15089	2368	1152	38859
	26.7%	25.5%	38.8%	6.1%	3.0%	100.0%
Added	1756	996	1160	55	41	4008
	43.8%	24.9%	28.9%	1.4%	1.0%	100.0%
Original	1964	1275	2707	250	124	6320
	31.1%	20.2%	42.8%	4.0%	2.0%	100.0%
Dropped	826	624	1820	178	215	3663
	22.6%	17.0%	49.7%	4.9%	6.0%	100.0%
Resumed	265	121	268	33	17	704
	37.6%	17.2%	38.1%	4.7%	2.4%	100.0%
Continuous	1439	1122	1732	205	114	4612
	31.2%	24.3%	37.6%	4.4%	2.5%	100.0%
Previous	1676	1798	2911	394	286	7065
	23.7%	25.5%	41.2%	5.6%	4.1%	100.0%
Subsequent	2159	1857	2496	347	206	7065
	30.6%	26.3%	35.3%	4.9%	2.9%	100.0%

Panel B: Tests of Differences in the Distributions of Recommendations between Groups with Different Coverage Classifications

Contrast	$\chi^2[(4)$: $H0$: no difference]	p-value
Added vs. Original	330.20	< 0.001
Added vs. Previous	628.67	< 0.001
Dropped vs. Continuous	237.32	< 0.001
Dropped vs. Subsequent	322.09	< 0.001
Resumed vs. Previous	74.18	< 0.001
Resumed vs. Dropped	83.82	< 0.001

[a]The rating categories correspond to analyst recommendations, with 1 = Strong Buy and 5 = Strong Sell.

[b]The coverage classifications are defined below.

Definitions of Coverage Classifications

Classification:	**Definition:**
All	All stock ratings by 510 sample analysts between July 1987 and December 1994.
Added	First ratings of stocks by sample analysts that (1) occur more than six months after the analyst's first appearance in the database and (2) occur on a date with no more than two other new stocks.
Original	First ratings of stocks by sample analysts that (1) occur within six months of the analyst's first appearance in the database or (2) occur on a date with three or more other new stocks.
Dropped	Last ratings of stocks prior to analyst decisions to discontinue coverage.
Resumed	First ratings of stock after discontinuations.
Previous	A random sample, with one observation per analyst-*CUSIP*, from ratings of stocks that previously received ratings by the same analyst.
Subsequent	A random sample, with one observation per analyst-*CUSIP*, from ratings of stocks that subsequently received ratings by the same analyst.

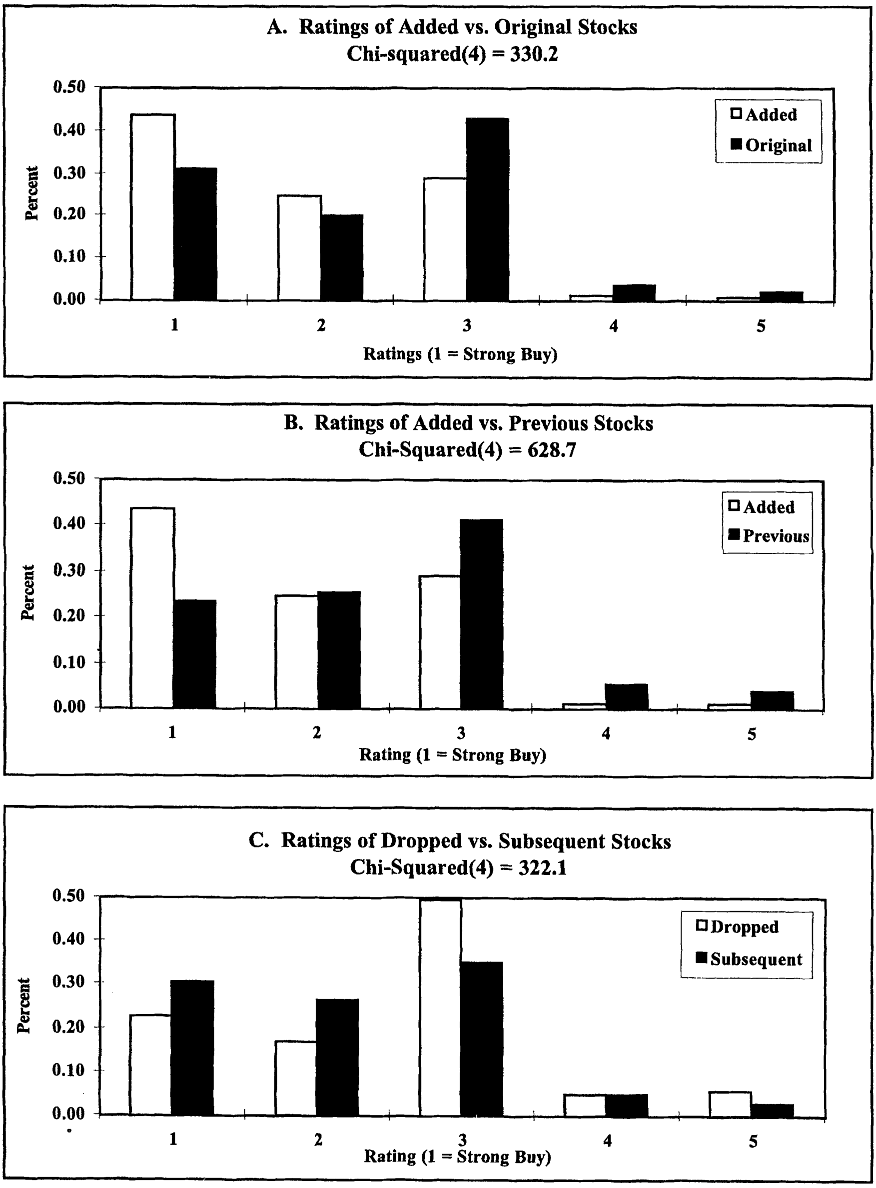

FIG. 2.—Ratings for selected comparisons.

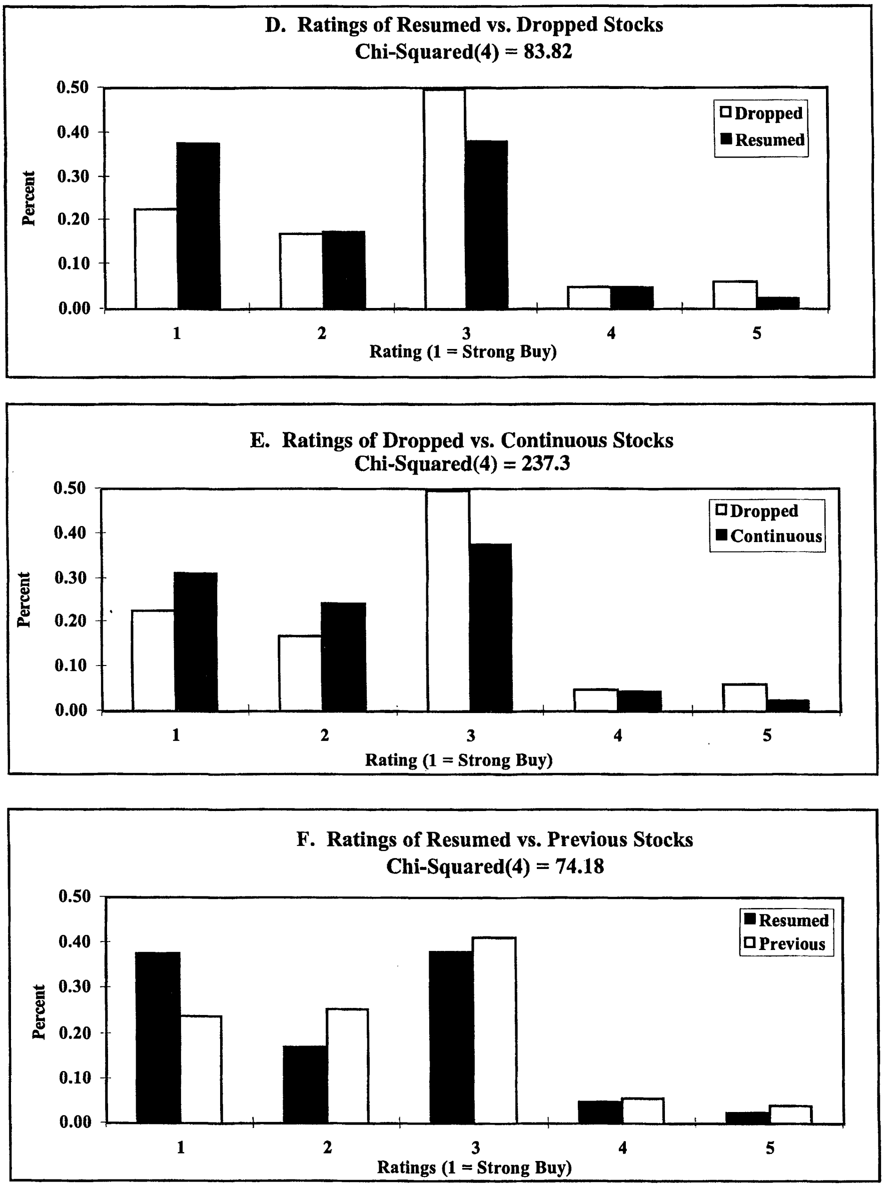

FIG. 2.—*Continued.*

Finally, to provide additional evidence of greater relative optimism by analysts adding stocks than those previously covered or subsequently dropped, we test for more favorable earnings forecasts for stocks that are "added" vs. "original," "continuous" vs. "dropped," and "added" vs. "dropped." To permit cross-sectional comparisons, we scale *EPS* forecasts by the average of the firm's beginning and end-of-year book value per share, to calculate forecast return on equity. The findings (not reported) strongly support the inferences from our tests based on ratings. Forecast return on equity is significantly higher for "added" stocks than for "original" stocks, for "continuous" stocks than for "dropped" stocks, and for "added" than for "dropped" stocks.[32] Thus, analysts' selection decisions are reflected in both their recommendations and their earnings forecasts.

6.2 RETURN ON EQUITY FOLLOWING INITIAL AND FINAL REPORTS

In table 6, we report comparisons of return on equity and industry-adjusted return on equity for all four coverage subsamples. The median industry-adjusted return on equity realized for the fiscal year-end following the analysts' decision to add coverage is 5.8%, compared to 4.0% for stocks with original coverage, 4.5% for stocks with continuous coverage, and 2.8% for stocks whose coverage was dropped.[33] Subsequently realized industry-adjusted return on equity is significantly higher for added stocks than for originally covered stocks, and significantly higher for continuously covered stocks than for stocks that are dropped. These differences are consistent with the differences in analysts' ratings reported in table 5 and support the notion that the differences in ratings across coverage categories are due to differences in information about firms' future earnings performance. Furthermore, we find that the median industry-adjusted return on equity for all categories of coverage is positive, consistent with analysts' selecting better-performing companies in each industry.[34] The positive performance implies that the stocks not covered by analysts tend to perform below the median in their industries.[35] Consistent with this implication, we find that median industry-adjusted *ROE* over 1989–94 is –.057 (significant at the .0001 level) for the 7,290 companies on *Compustat* that are not covered by our 523 sample analysts.

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[32] The forecast return on equity data are grouped according to the forecast horizons in table 4, providing five subsamples for each comparison. One-tailed probability values are significant at less than 0.005 for 11 of the 15 comparisons, and at less than 0.10 for all but one of the four remaining comparisons. These results are available from the authors upon request.

[33] We expect industry-adjusted return on equity to be higher for stocks that are continuously covered than for stocks in our original coverage category. The latter sample includes both stocks that were continuously covered and stocks that were subsequently dropped.

[34] The finding that stocks whose coverage was dropped by sample analysts perform better than the median for their industry suggests that firms that attract coverage, even if not retained, perform better than firms that received no coverage over the sample period. Additional analysis (not reported) indicates that the average number of sample analysts covering dropped stocks was 3.19, so many of these stocks are likely still covered by other analysts.

[35] A broader consequence of our selection conjecture is that, if analysts use valid information to make their selections, then firms with lower analyst following should perform more poorly, ceteris paribus.

TABLE 6

Comparison of Median Return on Equity and Industry-Adjusted Return on Equity by Coverage Classification

Panel A: Return on Equity Measures (Number of Observations in Parentheses)

	Measure[a]	
Coverage Classification[b]	Median Return on Equity	Median Industry-Adjusted Return on Equity
Added	0.139	0.058
	(4,226)	(4,226)
Original	0.130	0.040
	(5,442)	(5,442)
Continuous	0.133	0.045
	(3,958)	(3,958)
Dropped	0.116	0.028
	(3,146)	(3,146)

Panel B: Wilcoxon *z*-Statistics for Selected Comparisons[c]

Comparison		
Added vs. Original	4.78	7.25
	(0.0001)	(0.0001)
Continuous vs. Dropped	9.53	8.56
	(0.0001)	(0.0001)
Added vs. Dropped	11.76	12.20
	(0.0001)	(0.0001)

[a]Return on equity is measured as income before extraordinary items and the results of discontinued operations divided by the average of beginning and end-of-period shareholders' equity. Industry-adjusted return on equity is return on equity less the median return on equity for all firms in the same two-digit *Compustat SIC* code for that *Compustat* year.

[b]Coverage classifications are defined in table 3.

[c]The Wilcoxon rank-sum test, with continuity correction, tests whether the distributions of ranks of return on equity are the same. Rejection with a positive *z*-score indicates that the return on equity measure for the Added (Continuous) subsample is greater than for the Original (Dropped) subsample. One-tailed probability values are shown in parentheses.

6.3 ANALYSTS' INITIAL AND FINAL FORECAST ERRORS

In table 7 and figure 3, we report comparisons of our sample analysts' price-deflated forecast errors, as described in hypothesis *H*5. The denominator for forecast errors is the stock's price on the trading day before the analyst forecast date, censored to exclude prices below $0.20.[36] The median forecast errors generally are negative, except that median forecast errors for "added" stocks produced within 90 days of the fiscal year-end are zero.[37] The results in table 7 and figure 3 are consistent with *H*5, with median price-deflated forecast errors more negative for

[36] Our inferences are not sensitive to changing the minimum denominator to $5.00 or to using uncensored price in the denominator.

[37] Some recent reports have suggested that analyst forecasts in recent years have not been overestimates on average. We do not find this to be true in our data. The mean and median forecast errors are negative in each of the years in our sample, and the magnitudes of mean and median forecast errors from 1993 and 1994 fall roughly in the middle of the other years' error magnitudes.

TABLE 7

Comparison of Median Forecast Errors by Coverage Classification and Forecast Horizon

Panel A: Median Price-Deflated Forecast Errors by Coverage Classification

	Horizon[a]				
Coverage Classification[b]	0–90	91–180	181–270	271–365	> 365
Added	0.0000	−0.0013	−0.0033	−0.0044	−0.0096
	(857)	(846)	(815)	(763)	(354)
Original	−0.0009	−0.0025	−0.0037	−0.0068	−0.0127
	(1076)	(1055)	(1052)	(1120)	(2910)
Continuous	−0.0003	−0.0013	−0.0027	−0.0040	−0.0114
	(910)	(899)	(753)	(812)	(1528)
Dropped	−0.0009	−0.0038	−0.0041	−0.0158	−0.0157
	(248)	(242)	(237)	(214)	(201)

Panel B: Wilcoxon *z*-Statistics for Selected Comparisons[c]

	Horizon[a]				
Comparison[b]	0–90	91–180	181–270	271–365	> 365
Added vs. Original	3.31	2.00	0.17	2.56	0.13
	(0.0009)	(0.0460)	(0.8633)	(0.0103)	(0.8990)
Continuous vs. Dropped	0.89	1.77	0.60	3.10	1.96
	(0.3708)	(0.0776)	(0.5498)	(0.0019)	(0.0499)
Added vs. Dropped	2.09	2.54	0.15	3.00	1.73
	(0.0362)	(0.0111)	(0.8817)	(0.0027)	(0.0839)

[a]The forecast horizon is the number of calendar days between the forecast date and the end of the fiscal year for which earnings are forecast.

[b]Coverage classifications are defined in table 3.

[c]The Wilcoxon rank-sum test, with continuity correction, tests whether the distributions of ranks of absolute forecast errors are the same. Rejection with a positive *z*-score indicates that absolute forecast errors for the Original (Dropped) subsample are greater than for the Added (Continuous) subsamples. Two-tailed probability values are shown in parentheses.

"dropped" stocks than for "continuous stocks" at every horizon, and with statistically significant differences at three of the five horizons.

We conduct several sensitivity analyses of these results. First, we examine the sensitivity of these results to size. If analysts have greater discretion to add or drop coverage of smaller firms, the selection argument predicts that the difference in forecast errors of added vs. dropped stocks will be greater for smaller stocks. We regress the median forecast error on the log of market value of equity, an indicator for coverage (where added stocks are coded 1 and dropped stocks are coded 0), an indicator for size (where the firm is large if it is in the top-five deciles of market value of equity), and an indicator variable for each horizon. The median error is less negative for larger firms ($t = 8.06$), for added than for dropped stocks ($t = 4.58$), and for shorter horizons ($t = 3.43$). In addition, the difference between the error for added and dropped stocks is greater for smaller firms ($t = 3.34$), consistent with analysts' greater discretion over coverage of smaller firms.

Second, we also examine the sensitivity of our results to measuring forecast errors as a percentage of the forecast, where forecasts are required to exceed $0.20 in absolute value. For this metric, the "dropped" forecast errors are significantly more negative than "continuous" or

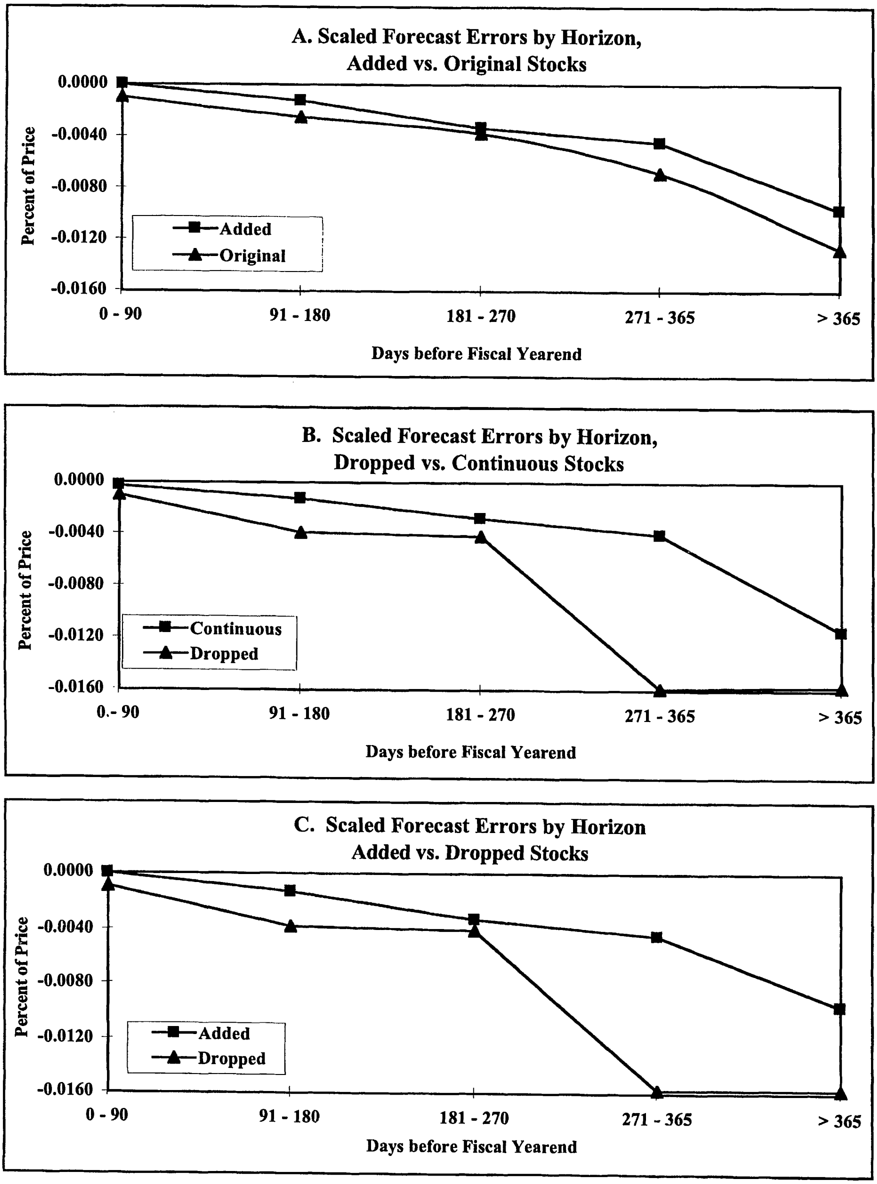

FIG. 3.—Forecast errors scaled by price for selected comparisons.

"added" groups for all five horizons.[38] Third, analysts may drop coverage of stocks that are delisted coincident with poor performance. We verify that the results are not affected by excluding forecast errors for the 4.6% of "dropped" stocks that have been delisted, or the 3.2% that

[38] We adopt a significance level of 0.05, though the majority of z-values are significant with probability values less than 0.01.

have been delisted before or within 30 days after the analyst drops coverage. Fourth, if analysts are more likely to be overoptimistic about *IPO* firms in general, and more likely to add these firms, then our forecast error tests could be less powerful due to this effect. We document similar forecast error differences for firms that conducted *IPOs* in the prior two years, and for the remaining firms that were public for more than two years.

The forecast error results are consistent with analyst self-selection in reporting and inconsistent with certain competing conjectures. If analysts add more bias at the start of coverage and less or none before dropping, analysts' forecast errors will be more negative for added stocks and less negative for dropped stocks. Similarly, if analysts exhibit the winner's curse phenomenon by being blindly overoptimistic about the stocks they add, then we expect more negative forecast errors for added stocks and no difference in industry-adjusted return on equity. The evidence from recommendations, return on equity, and forecast errors taken together can be explained by our self-selection argument, but it cannot be explained by either a tendency to be intentionally more biased for added stocks and less biased for dropped stocks, or by a winner's curse phenomenon.

7. *Conclusions*

Our analysis shows that analysts tend to add firms they view favorably and drop firms they view unfavorably, consistent with the conjecture that analysts report recommendations and forecasts selectively, based on whether their private information about a firm is favorable. Compared with the distribution of recommendations for stocks with previous coverage, the distribution of recommendations for newly added stocks is shifted significantly toward more favorable ratings. Conversely, the distribution of recommendations immediately prior to analysts' dropping the stock is shifted toward less favorable ratings.

We find evidence that self-selection by analysts is at least a partial explanation for the commonly observed phenomenon that analysts' forecasts of earnings are generally and persistently overoptimistic. If analysts are less likely to report when they have unfavorable information, overpessimistic errors will be underrepresented in observed distributions of forecast errors. We find that forecast errors of stocks that analysts drop are more negative than those receiving continuous coverage, suggesting that analysts drop coverage after receiving negative information, which then necessarily is not reflected in their last outstanding forecast.

We examine the accuracy of analysts' information to distinguish between selective reporting based on relative optimism and both intentional bias at the start of coverage and the winner's curse. Either of the competing explanations implies less accurate predictions for newly added stocks. We find that subsequently realized industry-adjusted return on equity is greater for newly added stocks than for stocks with previous coverage, and less for stocks that are dropped. This suggests that analysts' coverage decisions reflect information about firms' future earnings. We also find that forecast errors for newly added stocks are less negative than those for stocks covered originally. These results are consistent with both more favorable information and more accurate information supporting an analyst's decision to add a stock than to continue coverage. We find that analysts revise their forecasts of newly added stocks' earnings more frequently than for other stocks, suggesting that more intense effort may underlie the greater accuracy.

We examine the extent to which analysts avoid or delay unfavorable news in several ways. We document that the median number of days between ratings that are downgrades is significantly greater than for upgrades, consistent with analysts delaying downgrades. We also find that analysts use sell recommendations infrequently and for shorter periods than buy or hold recommendations. However, we find that ratings downgrades are more frequent than upgrades.

Our evidence is consistent with the conjecture that analysts report selectively when they have relatively favorable information and is not consistent with several alternative explanations for observed overoptimism in analysts' forecasts. These findings suggest analysts' reports and accounting reports are not perfect substitutes, and that an important role for accounting reports arises in providing information about firms that analysts have less incentive to cover.

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