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Analyst Forecast Revisions and Market Price Discovery

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ABSTRACT: We document several factors that help explain cross-sectional variations in the post-revision price drift associated with analyst forecast revisions. First, the market does not make a sufficient distinction between revisions that provide new information ("high-innovation" revisions) and revisions that merely move toward the consensus ("low-innovation" revisions). Second, the price adjustment process is faster and more complete for "celebrity" analysts (*Institutional Investor All-Stars*) than for more obscure yet highly accurate analysts (*Wall Street Journal Earnings-Estimators*). Third, controlling for other factors, the price adjustment process is faster and more complete for firms with greater analyst coverage. Finally, a substantial portion of the delayed price adjustment occurs around subsequent earnings-announcement and forecast-revision dates. Collectively, these findings show that more subtle aspects of an earnings revision signal can hinder the efficacy of market price discovery, particularly in firms with relatively low analyst coverage, and that subsequent earnings-related news events serve as catalysts in the price discovery process.

Keywords: *analysts; forecasts; revisions; market efficiency; earnings quality.*

Data Availability: *Data are available from sources identified in the paper.*

I. INTRODUCTION

This study examines the information content and market price discovery process associated with individual analysts' earnings forecast revisions. Our primary goal is to better understand the characteristics of forecast revisions that affect their immediate

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market impact, as well as the magnitude of the subsequent price drift over the next year. We are particularly interested in how these characteristics affect the proportion of the total price response that occurs immediately, relative to the price drift that occurs in the following months.

Individual analysts' forecast revisions play an important role in the dissemination of information about corporate earnings. Unlike quarterly earnings announcements, individual analyst forecast revisions take place throughout the quarter. Because of their frequency and timeliness, these revisions have become a vital source of information for many users of corporate financial reports. Web-based technology now delivers individual forecast revisions to investors in real time.[1] As access to this information increases, the need to better understand the information content of each individual revision also increases.

Our study is motivated by the ongoing debate on the efficiency with which the market assimilates earnings news (e.g., Kothari 2001; Lee 2001). The delayed market response to earnings news is one of the most perplexing anomalies to emerge from accounting-based capital market research over the past 20 years.[2] Although this empirical regularity is well documented, surprisingly little is known about qualitative features of an earnings signal that can either hinder or aid the market in assimilating this information.

In this study, we identify and test several factors that we expect will affect the speed and efficacy of the price discovery process. Our investigation focuses on attributes of the forecast revision itself, as well as on characteristics of the firm for which the forecast is being made. Specifically, we hypothesize that, after controlling for the magnitude and direction of the revision surprise and for other firm characteristics, the magnitude of the post-revision price drift is related to (1) the extent to which a revision is innovative relative to existing forecasts (i.e., whether an analyst brings new information or is simply herding toward the consensus);[3] (2) the analyst's ability and reputation (i.e., whether he or she is a superior forecaster, and whether his/her abilities are likely to be well known to the market); and (3) analyst coverage (i.e., whether many analysts cover a firm).

Our results show that all three factors are related to the subsequent price discovery process. First, we find that the post-revision price drift is more pronounced for high-innovation revisions. Second, we find that forecasts made by celebrity analysts (*Institutional Investor [II] All-Stars*) elicit a stronger immediate price response, but a less pronounced subsequent price drift. In contrast, the subsequent price drift is more pronounced for revisions made by superior analysts who are more likely to be employed by smaller firms (*Wall Street Journal [WSJ] Earnings-Estimators*). Third, controlling for other factors, we show that the post-revision price drift is more pronounced in firms with lower analyst coverage.

In addition, we show that a significant portion of the delayed price reaction occurs around the next *four* earnings announcements. Interestingly, we find an even greater portion

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[1] All the primary service providers (e.g., I/B/E/S, First Call, and Zacks) now offer a menu of web-based products that deliver real-time earnings estimate revisions. The Appendix provides a sample screen capture of First Call information delivered through FactSet, a high-end investment service provider.

[2] Ball and Brown (1968), Foster et al. (1984), Freeman and Tse (1989), and Bernard and Thomas (1989, 1990) document the post-earnings-announcement price drift; Givoly and Lakonishok (1980) and Stickel (1991) document a similar drift after revisions in analysts' earnings forecasts. These studies form the basis of earnings-based momentum strategies that many investment firms use.

[3] As explained later, we distinguish between high-innovation and low-innovation revisions using two benchmarks: an analyst's own prior forecast and the prior day's consensus forecast. Specifically, we define *high-innovation* forecasts as those that are either higher or lower than *both* benchmarks. Conversely, *low-innovation* revisions are those that are between the analyst's own prior forecast and the prior consensus estimate. As we define them, high-innovation revisions bring unambiguous information to market while low-innovation revisions could simply reflect herding toward the consensus.

of the delayed reaction occurs in short windows around the next *six* forecast revisions. This evidence suggests that the market is adjusting slowly to the information contained in individual forecast revisions, and that subsequent revisions act as catalysts in the price discovery process.

Our findings point to several imperfections in the price discovery process for earnings forecast revisions. First, the market does not make a sufficient distinction between analysts who are unambiguously providing new information and those who are simply herding toward the consensus. After we control for the magnitude of the forecast revision, high-innovation revisions are associated with significantly larger price drifts than low-innovation revisions. This result suggests that the level of innovation is an aspect of the revision signal that the market does not fully incorporate at the time of the announcement. Second, the market pays more attention to analysts who have acquired celebrity status, but is more likely to underappreciate revisions made by more obscure analysts with comparable forecasting abilities. This result suggests that an analyst's reputation, and not merely his forecasting ability, affects market price discovery. Finally, the price drift is lower for firms followed by more analysts, suggesting that coverage by multiple analysts helps to facilitate the price discovery process.

Our study contributes to several streams of recent research. First, it extends the growing literature on the role of analysts as information intermediaries. Prior studies find evidence that the level of financial analyst coverage affects the efficiency with which the market processes information (Brennan et al. 1993; Walther 1997; Bhattacharya 2001; Elgers et al. 2001). We show that increased analyst coverage generally results in a lower price drift subsequent to analyst forecast revisions, suggesting that in the case of individual forecast revisions, additional coverage helps to facilitate price discovery.

Second, our results highlight the usefulness of analyst information in investment decisions. Elgers et al. (2001) report that a simple trading strategy based on the ratio of price to one-year-ahead earnings forecasts (P/FY1) yields significant positive abnormal returns. Other studies (e.g., Womack 1996; Jegadeesh et al. 2001) find that analyst stock-recommendation revisions predict returns over the next 3 to 12 months. Our findings show that one can enhance the forecast revision investment strategy suggested by Stickel (1991) by considering the level of innovation of the revision signal, the amount of analyst coverage, and, to a lesser extent, the ranking of the analyst.

Finally, our results shed light on the price discovery mechanism and help define the boundaries of market efficiency. Much of the regulatory debate in accounting standard setting revolves around the issue of "transparency"—that is, whether investors can readily decipher more subtle aspects of financial reports. Our findings suggest that market participants do not immediately assimilate certain potentially informative, qualitative aspects of an earnings signal. By inference, these findings raise questions as to the market's ability to quickly and accurately incorporate other, more subtle, aspects of the quality of reported earnings. Collectively our results imply that the price discovery process, at least as it pertains to analyst forecast revisions, is complex and protracted, and deserving of more scholarly attention.

II. RELATED LITERATURE AND HYPOTHESES DEVELOPMENT

Related Literature

Two main stylized facts emerge from prior research on analyst forecast revisions. First, these forecast revisions are price informative, in that they are associated with a significant immediate market response at the time of their release. Second, the immediate price response is incomplete, in that prices, on average, continue to drift in the same direction for at least three to nine months after the price revision.

Evidence that analyst forecasts are price-informative dates back to the 1970s. Early research by Griffin (1976), Givoly and Lakonishok (1979, 1980), and Imhoff and Lobo (1984) documents the market response to analyst forecast revisions. In fact, Elton et al. (1981) show that foreknowledge of analyst revisions is more value-relevant than foreknowledge of the reported earnings themselves. Collectively, these findings show that individual analyst forecast revisions convey new information to the market.

Subsequent research shows that the short-window price reaction around forecast revisions is incomplete. Using a small sample, Givoly and Lakonishok (1980) first reported a post-announcement drift after earnings forecast revisions. Stickel (1991) demonstrated that firms whose consensus forecast has been recently revised upward tend to earn higher abnormal returns over the next 3 to 12 months than firms whose consensus forecast has been recently revised downward. Finally, Chan et al. (1996) confirm Stickel's (1991) finding and show that it is part of a general class of "momentum" strategies, in which the market response to recently released information is incomplete.[4]

Although the post-revision price drift is well documented, surprisingly little is known about factors that either exacerbate or mitigate this empirical regularity. Our study focuses on factors that help explain cross-sectional variation in the delayed price reaction. Unlike most prior studies, which focus on changes in the *consensus* analyst forecast, we examine the market response to *individual* analyst revisions. This approach allows us to evaluate qualitative attributes of the forecast revision itself, as well as elements in the firm's information environment that could potentially affect the information-assimilation process.

Hypothesis Development

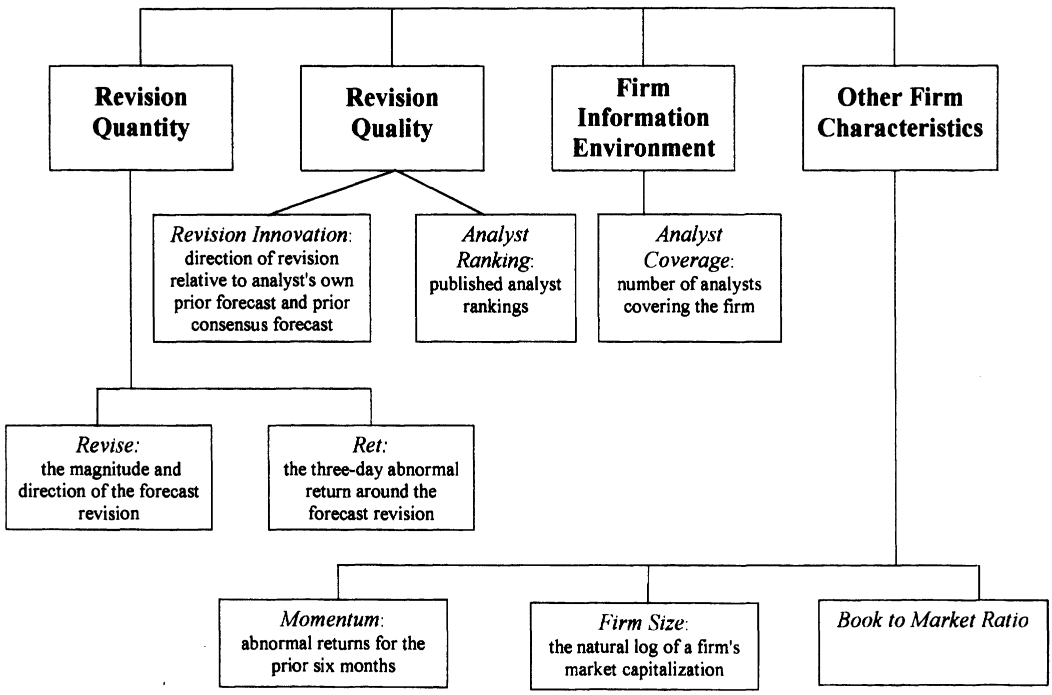
Figure 1 provides an overview of the key conceptual constructs that we examine in this study, and of the empirical proxies we employed for each construct. As this figure illustrates, our investigation focuses on four sources of potential variation in the delayed price response. Our main interest lies in uncovering *qualitative* features of the forecast revision signal and elements in the firm's information environment that affect the efficacy of the information-assimilation process. However, we recognize the need to control for the *quantity* of the forecast revision (i.e., its direction and magnitude) as well as for various *firm characteristics* that can affect expected returns. We discuss each of our four key conceptual constructs in more detail below.

Revision Quantity

First, we examine the conjecture that the magnitude of the post-revision drift is a function of the size and direction of the revision signal itself. Prior studies on the post-earnings-announcement drift document a positive relation between the *quantity* of the unexpected earnings news (i.e., the size and direction of the earnings surprise) and the subsequent price drift (e.g., Bernard and Thomas 1990). Although the effect these studies document pertains to actual earnings surprises rather than analyst forecast revisions, we hypothesize that an earnings revision of greater magnitude will also trigger a greater subsequent price drift.

[4] Others who have documented similar price drifts after different news events include Bernard and Thomas (1989, 1990) and Freeman and Tse (1989), who find price drifts after earnings surprises, and Womack (1996), who documents a price drift after analysts' buy/sell recommendations. In each case, the evidence points to an immediate short-window price response to the information release, followed by a generally sluggish adjustment in the same direction.

FIGURE 1
Factors Contributing to the Delayed Price Reaction to Analyst Forecast Revisions



We investigate this hypothesis using two empirical proxies for the quantity of the forecast revision signal. The first measure, Revise, is the magnitude of the revision expressed as a percentage of the firm's closing price on day −1 relative to the revision. Our choice of this variable is based on prior studies (e.g., Stickel 1991; Imhoff and Lobo 1984) that show an analyst's own prior forecast is a better benchmark than the firm's consensus forecast for measuring the amount of surprise in the individual forecast. Our second measure of revision quantity, Ret, is the firm's size-adjusted return over the three-day window surrounding the revision. The immediate market response to a forecast revision is arguably a less noisy measure of the degree of surprise contained in the revision.

Revision Quality

We also investigate two qualitative attributes of the forecast revision signal, to ascertain whether nonquantitative characteristics of the forecast revision provide value-relevant information that is not fully reflected in the three-day returns over the event window. For example, Sloan (1996) documents a delayed market response to value-relevant information concerning earnings quality contained in total accruals. In the same spirit, we examine more subtle aspects of the forecast revision signal that might help explain cross-sectional differences in the delayed market reaction.

Revision Innovation

The first qualitative attribute we examine is the level of innovation in the forecast revision. Analyst forecasts are typically reported along with the analyst's prior forecast and the most recent consensus forecast. Therefore, both the analyst's own prior forecast and the prior consensus forecast are potentially important benchmarks in assessing the information content of the revision. We use both benchmarks in distinguishing between *high-innovation* and *low-innovation* revisions.

Figure 2 illustrates the difference between high- and low-innovation revisions. In this figure, we label forecasts that are higher than both the analyst's own prior forecast and the prior consensus as *high-innovation good-news* revisions (denoted G). Similarly, we label forecasts that are lower than both the analyst's own prior forecast and the prior consensus as *high-innovation bad-news* revisions (denoted B). The direction of a high-innovation revision relative to both benchmarks suggests unambiguously that the analyst is bringing new information to the market. In contrast, we label forecasts that are between the analyst's own prior forecast and the prior consensus *low-innovation* revisions. For low-innovation revisions, whether the revision indicates good news (denoted L) or bad news (denoted L′) depends on whether we compare the revision to the analyst's own prior forecast or the prior consensus. When the new forecast is exactly the same as the prior consensus, we treat it as a low-innovation revision.

An earlier finding by Stickel (1990) suggests that low-innovation forecasts likely contain less information than high-innovation forecasts. Specifically, Stickel (1990) shows that changes in the consensus are useful in predicting the next revision of any given analyst. This result implies that low-innovation revisions are less informative (regardless of magnitude), because they simply reflect information revealed by the prior revisions of other analysts. We therefore hypothesize that a high-innovation revision will trigger a greater immediate price impact than a low-innovation revision.

We are more interested, however, in the effect of revision innovation on the market price discovery process. The level of revision innovation is a qualitative, rather than quantitative, characteristic of the forecast revision. Consequently it is more subtle in nature. We conjecture that the delayed price reaction is a function of the level of innovation in the revision signal. One could argue that analysts who revise their forecasts *toward* the consensus forecast are "herding" rather than bringing new information to market. To the extent that the market does not sufficiently distinguish between those analysts who herd vs. those who bring new information to market, we expect high-innovation revisions to be associated with a greater subsequent price drift.[5]

Analyst Ranking

The analyst's ranking is another characteristic of forecast revisions that affects both the information content of the signal and the efficiency of the price discovery process. Early research found no systematic differences among analysts in terms of the accuracy of their forecasts (e.g., Brown and Rozeff 1980; O'Brien 1990; Butler and Lang 1991). However, Stickel (1992) uses more comprehensive controls for the age of the forecasts and shows that analysts on the *II All-Star* team provide more accurate forecasts. Using a large sample that spans many industries, Sinha et al. (1997) also show that certain analysts consistently outperform others in terms of the accuracy of their forecasts.

Subsequent studies have focused on the economic determinants of individual analyst forecast accuracy, such as analyst experience (Mikhail et al. 1997); analyst abilities, resources, and portfolio complexity (Clement 1999); analyst aptitude and brokerage house

[5] We further decomposed high-innovation revisions into "diverging" signals (those that move away from the consensus) and "converging" signals (those that move toward, and beyond, the consensus). However, this additional partition had little effect on our results, and we do not separately report the findings.

FIGURE 2
Classification of Individual Forecast Revisions

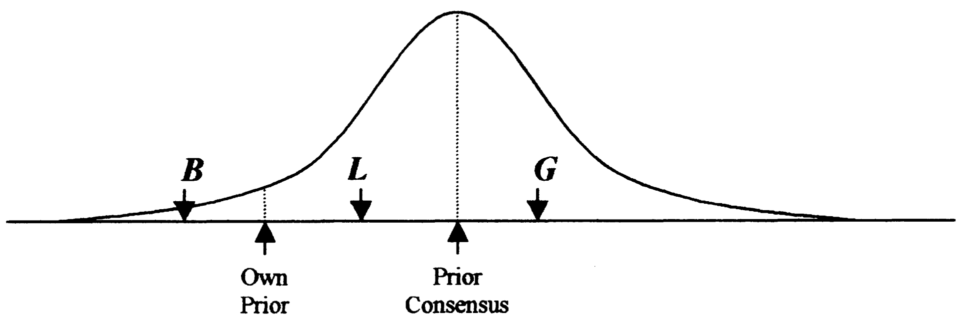


Figure 2a: When an analyst's own prior forecast is less than the current consensus

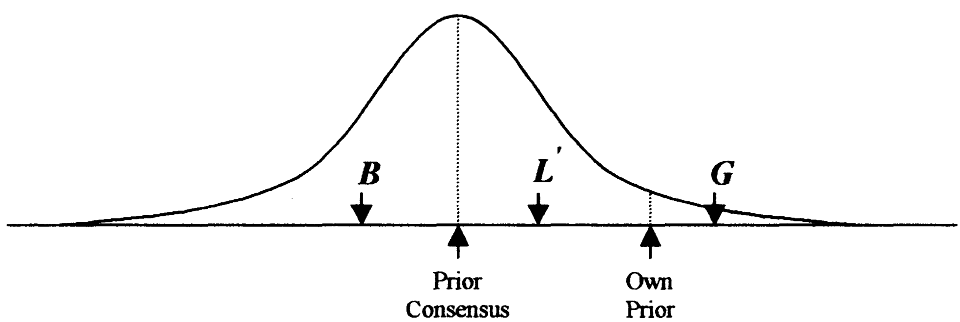


Figure 2b: When an analyst's own prior forecast is greater than the current consensus

Four Possible Classifications:

B – High-innovation signal, bad news.

L – Low-innovation signal, good news.

L' – Low-innovation signal, bad news.

G – High-innovation signal, good news.

characteristics (Jacob et al. 1999); and age of the forecast (Brown and Mohammad 2001). Brown (2001) demonstrates that past accuracy is as or more useful in predicting future forecast accuracy as the economic determinants identified in the literature just mentioned. All these studies focus on the determinants and persistence of individual analyst accuracy, rather than on the market response to individual forecasts.[6]

Our research focuses on analyst attributes that could affect both the reliability and the salience of a forecast revision to market participants. Specifically, we use two widely publicized sources to identify award-winning analysts: (1) the *Institutional Investor's All-American Research Team* (*II All-Stars*), and (2) the *Wall Street Journal's Survey of Award Winning Analysts*. Specifically, we focus on those analysts the *WSJ* identified as superior in terms of their ability to forecast earnings accurately (*WSJ Earnings-Estimators*). Stickel (1992) used the former in his study, and the latter is a more recent survey that has, to the best of our knowledge, not been used in prior research.

Stickel (1992) shows that the earnings forecasts of *II All-Stars* are, on average, more accurate than those of unranked analysts. However, the *II All-Stars* are selected by a survey of major institutional investors. According to *II*, selection is based on the economic insight in their written reports, the level of access and the overall service they provide to clients, and the quality of their stock recommendations, as well as the accuracy of their earnings forecasts. In short, these are celebrity analysts whose overall reputations might affect the way market participants perceive their revisions.

The *WSJ Earnings-Estimators* offer an interesting contrast to the *II All-Stars*. Like *II All-Stars*, these analysts are superior earnings forecasters. Whereas *II* selects its *All-Stars* based on a survey of buy-side clients, the *WSJ* bases its selection process strictly on forecast accuracy. *All-Stars* are generally from major investment houses, while the *WSJ* analysts often come from smaller and lesser-known firms. *All-Star* rankings are also sticky, in that once an analyst acquires *All-Stars* status, he or she is likely to retain this distinction in subsequent years. The turnover is much higher among *WSJ* rankings, which are based strictly on accuracy over the past 12 months. Therefore, although both sets of analysts have superior forecasting ability, the *WSJ* analysts typically do not enjoy the stature and overall reputation of the *II All-Stars*.

Prior studies show that the market differentiates these analysts to some extent in that superior analysts elicit stronger market responses in short windows immediately surrounding their forecast revisions (e.g., Stickel 1992; Park and Stice 2000). However, we are unaware of any study that examines the relation between analyst ranking and the delayed market response to forecasts. Like revision innovation, the stature and reputation of the analyst are more subtle, qualitative aspects of a revision signal. To the extent that the celebrity status of the *All-Stars* helps the market to recognize the value of their forecasts, we expect these revisions to trigger smaller post-revision price drifts. Conversely, if the market does not fully appreciate the greater accuracy of *WSJ* analysts, their forecasts will trigger more pronounced price drifts.[7]

Firm Information Environment

The overall level of information available about the firm also influences the value of an individual analyst forecast. In particular, heavier analyst coverage is associated with

[6] A concurrent study by Clement and Tse (2003) examines the relation between factors that predict analyst accuracy and the market response to forecast revisions. Their study focuses on returns in the immediate event window, while our main interest is on factors that help explain the post-revision price drift.

[7] Our hypotheses are consistent with experimental evidence from the individual decision-making literature (e.g., Griffin and Tversky 1992; Bloomfield et al. 2000) showing that, when an investor has imperfect information about signal reliability, the salience of the signal will affect the individual's perception of its reliability.

faster and more complete price adjustment. For example, Brennan et al. (1993) report that stocks with greater analyst coverage react faster to market-wide common information compared to those with less analyst coverage. Hong et al. (2000) find that returns to momentum-based investment strategies are higher for firms with low levels of analyst coverage. Elgers et al. (2001) find that the price response to value-relevant information in analyst earnings forecasts is less complete for firms with lower levels of analyst coverage. In contrast to these prior studies, we focus on the price drift after analysts' forecast revisions.[8] If the findings in Elgers et al. (2001) apply to analysts' revisions, we would then expect a more delayed price response in low-coverage firms.

Other Firm Characteristics

We also include three firm-level variables to control for cross-sectional differences in expected returns: price momentum (measured as the market-adjusted return over the prior six months), firm size (defined as the natural log of a firm's market capitalization measured as of the end of the last calendar year), and the book-to-market ratio (measured as of the end of the last calendar year). Earlier research shows that, on average, smaller firms with higher book-to-market ratios and more positive price momentum earn higher subsequent returns (e.g., Fama and French 1992; Jegadeesh and Titman 1993). We control for these variables to isolate the incremental ability of forecast revisions to predict cross-sectional returns.

III. SAMPLE SELECTION AND RESEARCH METHOD

Sample Selection Criteria

We obtained our analyst forecast data from the I/B/E/S Detail File, and our returns data from the Center for Research on Security Prices (CRSP) database. We use names from the I/B/E/S Analyst and Broker Name File to identify individual I/B/E/S analysts, and match these names to the award winners from *II* and *WSJ* publications.

Our sample period begins in October 1993, the first month that both *II* and *WSJ* provided a list of ranked analysts, and ends on December 31, 1998. We focus on forecasts of the one-year-ahead annual earnings (FY1). Our initial sample of 372,021 observations consists of FY1 forecast revisions with a prior forecast by the same analyst and CRSP return data for the firm.

Table 1 provides descriptive information on our sample firms. Panel A shows that, except for 1993 (for which we have only fourth-quarter data), our sample includes more than 4,000 firms per year. Excluding 1993, the average number of revisions per firm ranges from 15.0 (1996) to 18.5 (1998). However, the distribution is skewed, and the median number of revisions per firm is only 7 (1996) to 9 (1998).

Panel B of Table 1 reports the number of revisions in our sample, classified by firm size decile and stock exchange. We base threshold values for firm size deciles on beginning-of-year market capitalization for all New York Stock Exchange (NYSE) firms. Approximately two-thirds of the revisions are for NYSE-listed stocks. As expected, the majority of the forecast revisions in our sample are for larger firms, which attract greater analyst coverage. However, firm-level data show that the top six NYSE size deciles are evenly represented in our sample.

In subsequent tests, we compute size-adjusted returns for each firm by subtracting the mean buy-and-hold return of an equal-weighted portfolio of firms in the same NYSE size

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[8] Elgers et al. (2001) investigate early-in-the-year price-to-earning-forecasts (a type of PE ratio), rather than changes in individual analyst forecast revisions.

TABLE 1
Descriptive Statistics for Sample Firms and the Number of Forecast Revisions by Year, Firm Size, and Stock Exchange

Panel A: Number of Firms and Revisions Per Year

	Number of Firms	Number of Revisions	Number of Revisions per Firm: Average	Median	Std. Dev.
1993 (Oct.–Dec.)	3,000	19,580	6.53	4	8.21
1994	4,024	63,147	15.69	8	21.43
1995	4,244	67,192	15.83	8	21.88
1996	4,756	71,083	14.95	7	21.48
1997	4,755	72,839	15.32	8	21.01
1998	4,224	78,180	18.51	9	26.01
Total	6,505	372,021	14.91	7	21.54

Panel B: Number of Revisions and Firms by Firm Size Decile and Stock Exchange

NYSE Size Decile[a]	Revisions: NASD	NYSE	Total	% of Sample	Firms: Number of Firms	% of Total Firms
10 (largest)	22,333	156,795	179,128	48.1	810	12.4
9	23,026	51,200	74,226	20.0	819	12.6
8	20,052	21,324	41,376	11.1	824	12.7
7	16,967	10,611	27,578	7.4	890	13.7
6	14,865	5,554	20,419	5.5	860	13.2
5	10,155	3,074	13,229	3.6	806	12.4
4	7,490	1,493	8,983	2.4	641	9.9
3	4,131	791	4,922	1.3	471	7.2
2	1,542	196	1,738	0.5	294	4.5
1 (smallest)	362	60	422	0.1	90	1.4
	120,923	251,098	372,021	100.0	6,505	100.0
	32.5%	67.5%				

[a] Threshold values for NYSE Size Deciles are based on beginning-of-year market capitalization for all NYSE firms.

decile over our holding period. If a stock is de-listed during the return accumulation period, then we assume the proceeds are reinvested to earn the average return of the matching size decile portfolio.

Computation of Explanatory Variables

As discussed, our tests call for a comparison of each forecast revision to an analyst's own prior forecast, as well as to the most recent prior consensus forecast. I/B/E/S reports a monthly consensus measure in the third week of every month, but does not update this measure on a daily basis. To facilitate a daily comparison, we construct a new consensus forecast each day by taking the average of all the individual forecasts available as of the end of the previous day. For those days on which I/B/E/S does compute a consensus, we checked our consensus forecast measure against the I/B/E/S measure and found the two variables to be closely matched.[9]

Our tests also require that we identify analysts ranked by *II* and *WSJ*. The Analyst and Broker Name File from I/B/E/S contains the last name and first initial of each analyst, as well as a unique analyst code and the code for the brokerage firm. In those few instances in which two or more analysts had the same last name and first initial, we used the industry and brokerage firm information to distinguish between the analysts. We then identified superior analysts based on the list of names in the award issue of the *II* or *WSJ* publications.

Institutional Investor publishes its analyst rankings annually in October, and identifies four award winners for each industry. We consider analysts ranked first, second, third, or runner-up in the annual *II* list as *All-Stars* for the 12 months following their inclusion in the survey. The *WSJ* began publishing its own independent ranking of sell-side analysts in September 1993. Each subsequent year, beginning with 1994, *WSJ* published this ranking in June. We classify analysts ranked first through fifth as *Earnings-Estimators* for the 12 months following their inclusion in the industrial rankings.[10]

We also examine the effect of analyst coverage, a firm-specific characteristic, on the price discovery process. We measure the number of analysts covering a firm as the maximum number of analysts with outstanding forecasts at any point during the year. Following Elgers et al. (2001), we assigned firms to the high-coverage category if the number of analysts following the firm is above the median for our sample in a given year, and to the low-coverage category if the number of analysts is below the median.

Econometric Issues

In most of our empirical tests, the unit of observation is an individual forecast revision. We believe that revision-level analysis is appropriate (and necessary) if we are to better understand the price discovery process associated with the release of each new forecast revision. However, to the extent that individual revisions do not occur randomly across firms and over time, the observations in a revision-level regression are not independent. Moreover, because individual revisions exhibit positive serial correlation (Stickel 1990), test statistics from an ordinary least squares (OLS) regression will be inflated.

We deal with this sample independence problem in several ways. First, in our revision-level regressions (Table 5), we compute the t-statistics using the Huber-White estimator described in Huber (1967), White (1980), and Diggle et al. (1994). This maximum-likelihood estimation procedure addresses cross-correlation problems arising from having multiple observations from the same firm. Specifically, in constructing the variance and covariance matrix, this procedure assumes (and estimates) a common component for all the forecast revisions that come from the same firm. Individual revisions from the same firm contribute collectively to the variance and covariance estimate for this firm-group. The estimated coefficients from this procedure are asymptotically consistent, and the Huber-White test statistics are an order of magnitude lower than test statistics from ordinary OLS regressions. This approach retains the power of using individual analyst-level data while ensuring that firm-level cross-correlations do not inflate our test statistics.

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9 We are interested in mimicking the consensus forecast that is broadly available to users on a daily basis, rather than in creating a superior consensus that includes only the most recent forecasts. Therefore, we do not attempt to eliminate "stale" forecasts. To the extent that stale forecasts introduce noise into the consensus forecast, they reduce the power of our tests.

10 The *WSJ* distinguishes between analysts who excel at forecasting earnings and analysts who excel in stock picking. We focus on the top *Earnings-Estimators*. Analysts included in the September 1993 rankings are classified as superior earnings forecasters from October 1993 through June 1994.

Second, to alleviate concern over closely clustered revisions, we reestimated the analyses reported in all our tables using only revisions for firms that have no other revision occurring within a three-day window (days -1 to $+1$).[11] We have just over 150,000 such revisions. This procedure induces a "peek-ahead" bias because it requires that we know another revision is forthcoming when the first one was issued. Nevertheless, our main results are unchanged using this restricted sample.

Finally, we entirely eliminate the sample dependency problem arising from multiple revisions from the same firm by conducting a firm-level rather than revision-level analysis. To perform this analysis, we convert revision-level information into firm-level indicator variables, and regress future size-adjusted returns on these variables. We find that the firm-level analysis (reported later in Table 7) leads to similar inferences.

IV. EMPIRICAL RESULTS

Alternative Benchmarks for Measuring Forecast Revisions

Table 2 presents a comparison of mean size-adjusted returns for revisions classified using either the prior consensus forecast or an analyst's own prior forecast. This table reports the cumulative size-adjusted returns for the event window from day -1 to $+1$ (three days). It also shows the mean size-adjusted return in the period after the announcement window. We accumulate these returns over various holding periods, beginning on day -1 and ending on trading day $+64$ (three months), $+127$ (six months), $+190$ (nine months), and $+253$ (one year).[12]

As documented by prior studies (e.g., Givoly and Lakonishok 1980; Stickel 1991), Table 2 shows that most of the price response is delayed. On average, a firm that has a downward revision experiences a further downward price drift after the revision window of 3.4 to 4.4 percent per year. Upward revisions experience an upward price drift of 3.1 to 3.6 percent. In fact, in our sample only 18.5 (0.015/0.081) to 20.8 percent (0.021/0.101) of the total one-year price reaction occurs immediately in the three-day window surrounding the forecast revision.[13]

Table 2 confirms prior studies (e.g., Stickel 1991; Imhoff and Lobo 1984) showing that an analyst's own prior forecast is a better benchmark than the consensus forecast for measuring the amount of surprise in an individual forecast revision. Specifically, the last column of this table shows that mean size-adjusted returns are larger when we classify revisions relative to an analyst's own forecasts. This result holds for all return accumulation periods. Therefore, to secure a better measure of the revision signal, we compute the forecast revision (hereafter referred to as Revise) as the new forecast minus the analyst's own prior forecast, scaled by price.

Tabular Classifications

As discussed, our main interest centers on the empirical association between the delayed price response and revision and firm characteristics. As a first step, we report in a tabular format the size-adjusted returns for forecast revisions classified by characteristics of interest

[11] The mean (median) number of days between revisions is 12.75 (5) days. The length of time between forecasts is at least one day for 73 percent of the sample revisions.

[12] When comparing a revision to the prior consensus forecast, we exclude 1,322 observations with forecast revisions equal to the prior consensus forecast. Similarly, when comparing a revision to the analyst's own prior forecast, we exclude 1,923 observations with forecast revisions equal to the analyst's own prior forecast.

[13] We also examined returns over two-year holding periods. We do not report these results because almost all of the price drift occurs in the first year. Moreover, our two-year holding period results are sensitive to the inclusion or exclusion of 2000 (see Barber et al. 2003).

TABLE 2
Comparison of Mean Size-Adjusted Returns for Forecast Revisions Classified Relative to the Prior Consensus Forecast or the Analyst's Own Prior Forecast

	Revision Signal Relative to Prior Consensus Forecast			*Revision Signal Relative to Analyst's Own Prior Forecast*			*Difference in Differences*
	Bad News	*Good News*	*Difference*	*Bad News*	*Good News*	*Difference*	
Number of Forecasts	228,811	141,888		206,635	163,463		
Average Forecast Change[a]	−0.259	0.117		−0.185	0.093		
Size-Adjusted Returns[b]							
3 days	−0.010*	0.005*	0.015*	−0.013*	0.007*	0.021*	0.006*
3 months	−0.030*	0.018*	0.048*	−0.037*	0.021*	0.058*	0.009*
6 months	−0.037*	0.023*	0.059*	−0.045*	0.026*	0.072*	0.013*
9 months	−0.039*	0.028*	0.067*	−0.050*	0.034*	0.085*	0.018*
1 year	−0.044*	0.038*	0.081*	−0.057*	0.045*	0.101*	0.020*
Post-revision price drift	−0.034*	0.031*	0.065*	−0.044*	0.036*	0.080*	0.014*

* Indicates two-tailed t-test of difference from zero is significant at $\alpha < 0.001$.
[a] Forecast Change is the new forecast less either the prior consensus or the analyst's own prior forecast.
[b] Size-Adjusted Returns are the buy-and-hold return of the firm minus the buy-and-hold return for an equal-weighted portfolio of firms in the same NYSE size decile. Return accumulation periods are three days (day −1 to +1), three months (day −1 to +64), six months (day −1 to +127), nine months (day −1 to +190), and one year (day −1 to day +253), where days are trading days and day 0 is the day of the forecast revision. The post-revision price drift is measured from day +2 to day +253.

(Table 3). These results are only suggestive, because they do not control for the other cross-sectional factors that might affect the magnitude of the delayed price response. Nevertheless, they offer preliminary insights into the relative importance of our key variables in explaining cross-sectional variations in the delayed price response. These tabular results also allow us to observe the proportion of the total price response accumulated at various intervals after the revision.

Table 3 documents the mean size-adjusted returns for forecast revisions classified by the level of revision innovation, the analyst's ranking, and the extent of analyst coverage of the firm. Panel A presents results for high-innovation vs. low-innovation revisions for the full sample.[14] Panels B through D report the effect of analyst ranking, controlling for the level of revision innovation. Finally, Panels E and F report the effect of analyst coverage, controlling for the level of revision innovation. For parsimony, we do not report results of three-dimensional classifications, or the two-way interaction between analyst ranking and firm coverage.[15]

Three key results emerge from Table 3. First, the price adjustment process is more protracted for high-innovation revisions. Panel A shows that high-innovation revisions trigger a stronger immediate response: the mean three-day hedge return to high-innovation revisions is 2.3 percent, compared to 1.1 percent for low-innovation revisions (p-value < 0.0001).[16] The post-revision price drift is also larger for high-innovation revisions: the hedge return after the event window is 9.4 percent for high-innovation revisions, and only 3.3 percent for low-innovation revisions (p-value < 0.0001). More importantly, the *proportion* of the total one-year price response accounted for by the three-day event window is lower for high-innovation revisions: 20 percent vs. 24 percent for low-innovation revisions (p-value < 0.0001).

Second, this table provides limited evidence that analyst rankings can also affect price discovery. Comparing Panels B and C of Table 3, revisions by *All-Stars* trigger a greater three-day market response than those by unranked analysts. This result holds for both low- and high-innovation revisions (p-value < 0.0001 and p-value < 0.014, respectively). The *proportion* of the total one-year price response captured by the three-day event window is 23 to 40 percent for *All-Stars*, and only 19 to 21 percent for unranked analysts. However, the difference is statistically insignificant. Comparing Panels B and D, revisions by *Earnings-Estimators* also trigger a greater immediate market response than revisions issued by unranked analysts (p-values < 0.015). However, the *proportion* of the total one-year price response captured by the three-day event window for *Earnings-Estimators* is also not significantly different from those for unranked analysts.

Finally, Panels E and F of Table 3 show that analyst coverage is associated with cross-sectional variation in the delayed price response. The total post-revision price drift over the next 12 months is much larger for firms with low analyst coverage than for firms with high analyst coverage (p-value < 0.0001). Further tests (not reported) show this result is also robust across different analyst ranking categories. Moreover, the three-day returns capture

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[14] We exclude the 1,923 observations with forecast revisions equal to the analyst's own prior forecast, which were also excluded from Table 2. We also exclude 6,535 observations where the analyst's own prior forecast equals the prior consensus forecast. Results are not sensitive to this restriction.

[15] As we show later in the multiple regression analysis, these additional cross-classifications do not yield significant incremental insights.

[16] We compute t-statistics for the hedge portfolio returns in Table 3 assuming equal weight for each forecast revision. Returns associated with downward revisions are multiplied by -1 to simulate short positions. To avoid outliers caused by small denominators, we compute Wilcoxon sign rank statistics when comparing the *proportion* of 12-month returns accumulated during the three-day announcement window. The reported p-values are based on two-tailed tests of either the t-statistics (for hedge returns) or Wilcoxon statistics (for the proportion tests).

TABLE 3
Comparison of Mean Size-Adjusted Returns for Forecast Revisions Classified by Revision Innovation, Analyst Ranking, and the Extent of Analyst Coverage

	Low-Innovation Revisions[a]				*High-Innovation Revisions*[a]			
	Bad News	*Good News*	*Difference*	*% of 1-year Return*	*Bad News*	*Good News*	*Difference*	*% of 1-year Return*
Panel A: Full Sample								
No. of Forecasts	32,142	54,032			170,961	106,428		
Average Forecast Change[b]	−0.08	0.08			−0.19	0.10		
Size-Adjusted Returns[c]								
3 days	−0.005*	0.006*	0.011*	24	−0.015*	0.008*	0.023*	20
3 months	−0.015*	0.005*	0.021*	45	−0.041*	0.028*	0.069*	58
6 months	−0.022*	0.006*	0.029*	62	−0.050*	0.036*	0.085*	72
9 months	−0.027*	0.014*	0.040*	88	−0.054*	0.044*	0.098*	83
1 year	−0.027*	0.019*	0.046*	100	−0.062*	0.056*	0.118*	100
Post-revision price drift	−0.021*	0.012*	0.033*		−0.048*	0.046*	0.094*	
Panel B: Non-Ranked Analysts[d]								
No. of Forecasts	24,177	40,194			128,472	79,779		
Average Forecast Change[b]	−0.08	0.07			−0.19	0.10		
Size-Adjusted Returns[c]								
3 days	−0.005*	0.005*	0.010*	21	−0.015*	0.008*	0.023*	19
3 months	−0.015*	0.005*	0.020*	40	−0.041*	0.029*	0.070*	58
6 months	−0.023*	0.005†	0.028*	57	−0.050*	0.038*	0.088*	74
9 months	−0.030*	0.013*	0.043*	87	−0.054*	0.046*	0.100*	84
1 year	−0.028*	0.021*	0.049*	100	−0.060*	0.059*	0.119*	100
Post-revision price drift	−0.022*	0.015*	0.038*		−0.046*	0.049*	0.095*	

(continued on next page)

TABLE 3 (continued)

	Low-Innovation Revisions[a]				High-Innovation Revisions[a]			
	Bad News	Good News	Difference	% of 1-year Return	Bad News	Good News	Difference	% of 1-year Return
Panel C: All-Stars[d]								
No. of Forecasts	6,145	10,371			31,153	20,233		
Average Forecast Change[b]	−0.09	0.09			−0.20	0.13		
Size-Adjusted Returns[c]								
3 days	−0.007*	0.007*	0.014*	40	−0.015*	0.009*	0.024*	23
3 months	−0.015*	0.010*	0.024*	70	−0.038*	0.025*	0.063*	59
6 months	−0.018*	0.010*	0.028*	81	−0.042*	0.028*	0.071*	67
9 months	−0.017*	0.014*	0.031*	89	−0.048*	0.036*	0.085*	80
1 year	−0.024*	0.011†	0.035*	100	−0.060*	0.045*	0.105*	100
Post-revision price drift	−0.017‡	0.003	0.020*		−0.046*	0.034*	0.080*	
Panel D: Earnings-Estimators[d]								
No. of Forecasts	3,237	5,972			18,884	10,733		
Average Forecast Change[b]	−0.09	0.09			−0.20	0.12		
Size-Adjusted Returns[c]								
3 days	−0.006*	0.007*	0.012*	28	−0.015*	0.009*	0.024*	19
3 months	−0.018*	0.004	0.022*	50	−0.048*	0.025*	0.073*	57
6 months	−0.029*	0.007	0.036*	84	−0.058*	0.032*	0.089*	70
9 months	−0.032*	0.010	0.042*	97	−0.064*	0.037*	0.100*	78
1 year	−0.038*	0.006	0.043*	100	−0.081*	0.047*	0.128*	100
Post-revision price drift	−0.032*	−0.002	0.030*		−0.066*	0.036*	0.102*	

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TABLE 3 (continued)

	Low-Innovation Revisions[a]				High-Innovation Revisions[a]			
	Bad News	*Good News*	*Difference*	*% of 1-year Return*	*Bad News*	*Good News*	*Difference*	*% of 1-year Return*
Panel E: Low Analyst Coverage[e]								
No. of Forecasts	3,633	6,133			22,728	13,906		
Average Forecast Change[b]	−0.06	0.06			−0.20	0.09		
Size-Adjusted Returns[c]								
3 days	−0.003†	0.006*	0.009*	11	−0.016*	0.010*	0.027*	14
3 months	−0.018*	0.009‡	0.027*	32	−0.055*	0.049*	0.105*	55
6 months	−0.024*	0.013†	0.037*	43	−0.073*	0.070*	0.143*	75
9 months	−0.027*	0.032*	0.058*	69	−0.078*	0.088*	0.166*	88
1 year	−0.028‡	0.057*	0.084*	100	−0.081*	0.108*	0.189*	100
Post-revision price drift	−0.025†	0.048*	0.073*		−0.065*	0.096*	0.162*	
Panel F: High Analyst Coverage[e]								
No. of Forecasts	28,509	47,899			148,233	92,522		
Average Forecast Change[b]	−0.08	0.08			−0.19	0.10		
Size-Adjusted Returns[c]								
3 days	−0.006*	0.005*	0.011*	27	−0.015*	0.008*	0.023*	21
3 months	−0.015*	0.005*	0.020*	48	−0.039*	0.025*	0.064*	59
6 months	−0.022*	0.006*	0.028*	68	−0.046*	0.031*	0.076*	71
9 months	−0.027*	0.011*	0.038*	93	−0.051*	0.037*	0.088*	81
1 year	−0.027*	0.014*	0.041*	100	−0.059*	0.048*	0.108*	100
Post-revision price drift	−0.020*	0.008‡	0.028*		−0.045*	0.039*	0.084*	

(continued on next page)

TABLE 3 (continued)

Two-tailed t-test of difference from zero is significant at: * $\alpha < 0.001$ ‡ $\alpha < 0.01$ † $\alpha < 0.05$

[a] Low-Innovation Revisions are new forecasts that are between the analyst's own prior forecast and the prior consensus. The classification of these signals as good or bad news is based on the analyst's own prior forecast. High-Innovation Revisions are new forecasts that are either above ("Good News") or below ("Bad News") both the prior consensus and the analyst's own prior forecast.

[b] Forecast Change is the new forecast less the analyst's own prior forecast.

[c] Size-Adjusted Returns are the buy-and-hold return for the firm minus the buy-and-hold return for an equal-weighted portfolio of firms in the same firm-size decile. Return accumulation periods are three days (day −1 to +1), three months (day −1 to +64), six months (day −1 to +127), nine months (day −1 to +190), and one year (day −1 to +253), where days are trading days and day 0 is the day of the forecast revision. The post-revision price drift is measured from day +2 to day +253.

[d] *All-Stars* are analysts that ranked first, second, third, or runner-up in their industry in the most recent annual *Institutional Investor* rankings available prior to the revision. *Earnings-Estimators* are analysts ranked first through fifth in their industry in the most recent annual *Wall Street Journal* analyst rankings. Unranked analysts are not classified as either *All-Stars* or *Earnings-Estimators.*

[e] Low (High) Analyst Coverage indicates that the number of analysts covering the firm is below (above) the median analyst coverage for all firms in a given year.

a smaller proportion of the total one-year price response for low coverage firms (11 to 14 percent) than for high coverage firms (21 to 27 percent). This result is weakly significant for both levels of revision innovation (p-values < 0.075).

To summarize, the evidence thus far supports the view that price adjustment is slower and less complete for high-innovation revisions and low-coverage firms. We also find limited evidence that analyst ranking can affect price discovery. However, tabular classifications do not control for the magnitude of the revision signal, or for other firm characteristics that can affect expected returns. In the next section, we conduct more formal statistical tests using multiple regressions.

Revision-Level Regressions

Our multiple regression analysis controls for two measures of the magnitude of the revision signal. Revise measures the size and direction of the forecast revision. To compute this variable, we subtract an analyst's own prior forecast from the new forecast, and express the difference as a percentage of the closing price on day $t - 1$. As an alternative control for the information content of a forecast revision, we also include Ret, computed as the abnormal size-adjusted return during the three-day window centered on the revision date.

To capture the effect of the level of innovation in the revision signal, we define a categorical variable: Signal $= +1$ for high-innovation good news, 0 for low-innovation revision news, and -1 for high-innovation bad news. After we have controlled for both Revise and Ret in the regression, Signal captures the incremental effect of revision innovation (a qualitative feature of the revision) on future returns.

To assess the effect of the analyst's ranking, we group analysts into three categories: *Non-Ranked*, *II All-Stars*, and *WJS Earning-Estimators*. Non-ranked analysts include all analysts not ranked in either survey. *All-Stars* issued 19 percent of the total revisions. Because the *WSJ* groups firms into fewer industries, *Earnings-Estimators* issued only approximately 10 percent of the revisions. The overlap in award winners between the two publications is significant, but not overwhelming—23 percent of the *All-Stars* are also *Earnings-Estimators*. In our multiple regressions, we use two indicator variables (All-Star and Earnings-Estimator) that assume a value of 1 if an analyst is ranked by the respective publication, and 0 otherwise.

We also include an indicator variable to capture the level of analyst coverage a firm receives (Coverage). This variable equals 1 if a firm is followed by more than the median number of analysts, and 0 otherwise. We determine the number of analysts annually, based on the maximum making a one-year-ahead forecast for a given firm in a given year.

Finally, we control for the firm's market-adjusted returns over the prior six months (Momentum), the natural log of a firm's market capitalization at the end of the last calendar year (Size), and its book-to-market ratio as of the end of the last calendar year (B/M). We obtained the financial data for this analysis from the Compustat Price, Dividend, and Earnings file, and the returns data from CRSP. Use of these data reduced the sample size to 280,221 forecast revisions in the most restrictive multiple regression tests. To limit the effect of outliers, we winsorized the extreme top and bottom 1 percent of the B/M, Size, and Revise variables.

Table 4 reports the pairwise Pearson correlation between these variables and two measures of future size-adjusted returns: over the next 6 months (Ret6) and over the next 12 months (Ret12). The statistical significance of these correlations is inflated due to sample dependence problems (addressed in later tests using various statistical corrections). Nevertheless, the magnitude of the correlations generally confirms prior findings. Specifically,

TABLE 4

Pearson Pairwise Correlations among Firm Characteristics, Analyst Forecast Revisions, and Future Returns

	Revise	*Ret*	*Signal*	*Coverage*	*All-Stars*	*Earnings-Estimators*	*Momentum*	*Size*	*B/M*	*Ret6*	*Ret12*
Revise	100.0%	13.8%*	41.7%*	11.5%*	3.0%*	0.9%*	31.0%*	17.1%*	−9.3%*	6.4%*	4.6%*
Ret		100.0%	16.3%*	−0.2%	0.3%	0.0%	13.1%*	1.1%*	1.6%*	1.6%*	3.1%*
Signal			100.0%	0.4%†	1.1%*	−1.1%*	42.0%*	4.2%*	−9.3%*	9.6%*	8.2%*
Coverage				100.0%	7.2%*	3.8%*	−2.3%*	49.1%*	−8.5%*	0.4%†	−0.2%
All-Stars					100.0%	15.7%*	0.2%	12.1%*	2.1%*	0.2%	−0.4%†
Earnings-Est.						100.0%	−1.0%*	6.8%*	0.1%	−0.4%	−0.6%‡
Momentum							100.0%	−2.0%*	−4.6%*	12.0%*	10.5%*
Size								100.0%	−21.8%*	0.2%	−0.8%*
B/M									100.0%	−0.6%*	−1.1%*
Ret6										100.0%	67.0%*
Ret12											100.0%

Two-tailed t-test of difference from zero is significant at: * $\alpha < 0.001$ ‡ $\alpha < 0.01$ † $\alpha < 0.05$. Sample size = 280,221.

Revise = the forecast revision relative to the analyst's own prior forecast, as a percentage of price, with the extreme 1 percent of observations winsorized;
Ret = the firm's size-adjusted return over the three-day event window;
Signal = +1 for high-innovation good news, = 0 for low-innovation news, and = −1 for high-innovation bad news;
Coverage = 1 if the firm is covered by more than the median number of analysts computed annually for the sample, and 0 otherwise;
All-Stars = 1 if the analyst is ranked first, second, third, or runner-up in his industry in the *Institutional Investor* annual rankings, and 0 otherwise;
Earnings-Estimators = 1 if the analyst is ranked first through fifth in his industry in the *Wall Street Journal* annual analyst rankings, and 0 otherwise.
Momentum = market-adjusted returns over the prior six months measured as of day $t - 2$;
Size = the natural log of a firm's market capitalization at end of the last calendar year with the extreme 1 percent of observations winsorized;
B/M = the firm's book-to-market ratio measured as of the end of the last calendar year with the extreme 1 percent of observations winsorized;
Ret6 = the firm's size-adjusted return from day +2 to day +127, where days are trading days and day 0 is the day of the forecast revision; and
Ret12 = the firm's size-adjusted return from day +2 to day +253, where days are trading days and day 0 is the day of the forecast revision.

in our sample future returns are positively correlated with past price momentum (Momentum). Neither the book-to-market ratio (B/M) nor the firm size (Size) variable is strongly correlated with future returns during this period.

As expected, Signal and Revise are positively correlated (41.7 percent). Both are also highly correlated with Momentum (42.0 percent for Signal and 31.0 percent for Revise), and with Ret, the event-window return (16.3 percent for Signal and 13.8 percent for Revise). Coverage is also highly positively correlated with Size (49.1 percent). Finally, the *II* All-Star and *WSJ* Earnings-Estimator ranking variables are positively correlated, but only at 15.7 percent. In short, these pairwise correlations appear reasonable, and none seem large enough to create multicollinearity problems in the regressions.[17]

The post-revision price drift effect is clearly evident in Table 4. Both measures of future returns are positively correlated with the magnitude of the revision (Revise), the short-window return around the revision (Ret), and the direction of the high-innovation revisions (Signal). Of the three revision variables, Signal appears to have the strongest ability to predict future returns. Over the next 6 to 12 months, Signal has almost the same correlation with future return as the much better known Momentum.

Table 5 reports the result of a pooled cross-sectional regression of future returns on these explanatory variables. The dependent variable is future size-adjusted returns over the next 6 (Ret6) or 12 (Ret12) months. The independent variables include all the conceptual factors we expect to affect the delayed price response, as illustrated in Figure 1: (1) revision quantity—we include both Revise and Ret; (2) revision quality—we include a measure of revision innovation (Signal), as well as two measures of analyst ranking (All-Star and Earnings-Estimator); (3) firm information environment—we include a measure of analyst coverage (Coverage); and (4) other firm characteristics—we include three variables that prior research has shown are related to future returns (Momentum, Size, and B/M). The unit of observation for this analysis is a forecast revision, and the total number of observations is 280,221. As explained earlier, we address the sample dependency problem by computing Huber-White t-statistics.

It is possible that the market's ability to fully appreciate the differential impact of high-innovation revisions vs. low-innovation revisions depends on analyst ranking or analyst coverage. To evaluate the interactions between these effects, we include Coverage × Signal, All-Star × Signal, and Earnings-Estimator × Signal. Model 1 incorporates only revision attributes, Revise, Ret, and Signal. Model 2 adds controls for price momentum, firm size, and the book-to-market ratio. Finally, Model 3 incorporates all the explanatory variables, including the interaction terms.

The Signal variable construction implicitly assumes a long position in a firm for each high-innovation good news revision, and a short position for each bad news revision. Therefore, the estimated coefficient for Signal represents the average abnormal return from a single bet (either a long or a short position) made on each high-innovation revision. To compute the average *hedge* return—i.e., the combination of a long position with a short position—we double this coefficient.[18]

This approach is analogous to the post-earnings-announcement literature, which focuses on quarterly Standardized Unexpected Earnings (SUE) as the unit of observation. When a

[17] We also conduct more formal tests of collinearity on an unreported regression with all of the independent variables and no interaction terms. The highest condition index for any variable was 2.00 (index values above 30 would indicate a problem) and the highest variance inflation factor was 1.42 (factors below 10 are generally not considered a problem)

[18] Following prior studies, we use the term *hedge* to imply cash-flow neutrality rather than zero risk exposure.

TABLE 5
Panel Regression of Future Size-Adjusted Returns on *Ex Ante* Firm and Forecast Revision Characteristics

	Ret6 Model 1	*Ret6* Model 2	*Ret6* Model 3	*Ret12* Model 1	*Ret12* Model 2	*Ret12* Model 3
Intercept	−0.008†	−0.013	−0.011	−0.010	0.014	0.016
	(−2.41)	*(−1.05)*	*(−0.90)*	*(−1.19)*	*(0.53)*	*(0.62)*
Revise	0.661*	0.325	0.236	0.512	0.033	−0.075
	(3.59)	*(1.67)*	*(1.20)*	*(1.55)*	*(0.09)*	*(−0.20)*
Ret	−0.008	−0.034	−0.033	0.152‡	0.112†	0.114†
	(−0.36)	*(−1.54)*	*(−1.50)*	*(3.20)*	*(2.45)*	*(2.48)*
Signal	0.032*	0.020*	0.045*	0.049*	0.030*	0.060*
	(9.57)	*(7.39)*	*(8.01)*	*(6.77)*	*(5.55)*	*(5.42)*
Coverage			0.001			0.003
			(0.20)			*(0.17)*
Coverage × Signal			−0.027*			−0.034‡
			(−4.55)			*(−2.76)*
All-Star			0.000			−0.002
			(−0.18)			*(−0.95)*
All-Star × Signal			−0.008‡			−0.007
			(−3.17)			*(−1.46)*
Earnings-Estimator			0.000			−0.001
			(−0.45)			*(−0.57)*
Earnings-Estimator × Signal			0.008‡			0.014†
			(2.60)			*(2.39)*
Momentum		0.093*	0.093*		0.147*	0.146*
		(6.07)	*(6.03)*		*(4.29)*	*(4.27)*
Size		0.000	0.000		−0.003	−0.004
		(0.02)	*(−0.20)*		*(−0.85)*	*(−0.82)*
B/M		0.006	0.005		−0.010	−0.010
		(0.47)	*(0.44)*		*(−0.39)*	*(−0.40)*
Adjusted R^2	0.010	0.017	0.018	0.007	0.013	0.013

Two-tailed t-test of difference from zero is significant at: * $\alpha < 0.001$ ‡ $\alpha < 0.01$ † $\alpha < 0.05$.
We use the Huber-White maximum likelihood estimation procedure to correct for sample dependencies among multiple revisions for the same firm. Huber-White t-statistics are italicized and in parentheses.
Sample size = 280,221.
Variable definitions appear in the footnotes to Table 4.

firm has a positive SUE quarter, followed by a negative SUE quarter, the reported return for the hedge strategy would reflect a long position in quarter-one and a short position in quarter-two. To the extent that the two positions have overlapping holding periods for future returns, they cancel each other out, resulting in zero hedge returns for the duration of the overlap.

Results in Table 5 confirm the relative importance of revision innovation in explaining the post-revision price drift. As expected, the model has modest overall explanatory power for future returns. However, the Huber-White t-statistic on Signal is 8.01 for the Ret6 regression (Model 3), and Signal has the highest t-statistic in all six regressions. Table 5 shows that a hedge strategy based on Signal yields 10 percent abnormal returns over the next year after controlling for the magnitude of the revision signal and the three-day abnormal return over the event window (Model 1). The same strategy yields 6 percent over the next year after controlling for Revise, Ret, Momentum, Size, and B/M (Model 2).

The Revise and Ret variables compete for explanatory power. When we exclude one, the other is positively associated with future returns over both 6- and 12-month horizons. However, when we include both variables, Revise is marginally better at predicting 6-month returns, while Ret is marginally better at predicting 12-month returns. More importantly, neither Ret nor Revise is as important as Signal in terms of their ability to explain future returns. These results suggest that Signal captures *qualitative* aspects of an analyst's forecast revision that helps explain cross-sectional variations in the post-revision price drift.

The main effects of analyst coverage (Coverage) and the two analyst ranking variables (All-Star and Earnings-Estimator) are not statistically significant (Model 3). These findings show that, after controlling for the other variables, analyst coverage and the analyst's ranking, *per se*, do not explain significant variation in future returns.

More interesting are the interaction results. The estimated coefficient for Coverage × Signal is reliably negative, indicating that future returns to Signal are lower for firms with higher analyst coverage. Similarly, the estimated coefficient for All-Star × Signal is also reliably negative for the Ret6 regressions. This result suggests that returns over the next six months to Signal are reliably lower when the analyst is an All-Star. In contrast, the estimated coefficient for Earnings-Estimator × Signal is reliably positive for both Ret6 and Ret12, suggesting that future returns to Signal are higher when the analyst is an Earnings-Estimator.[19]

These results suggest that the market impounds the information in innovative forecast revisions more quickly when these revisions are issued by an All-Star analyst or are issued for more heavily covered firms. Conversely, the market is slower to impound the information in innovative forecast revisions when these revisions are issued by an Earnings-Estimator or are issued for firms with lower analyst coverage.

In Model 3, the inclusion of the interaction terms increases the estimated coefficient for Signal. Because the interaction terms remove the incremental effects of ranked analysts and heavily covered firms, the coefficient on Signal in Model 3 reflects the post-revision price drift associated with revisions by unranked analysts in lightly covered firms. Our results show that the post-revision drift is higher for these forecast revisions.

To summarize, Table 5 regressions show that the level of innovation in the revision signal (Signal) is an important factor in explaining cross-sectional variations in the post-revision price drift. Our results suggest that the other primary variables of interest (Coverage, All-Star, and Earnings-Estimator) affect future returns by attenuating or exacerbating the market's ability to interpret the information in Signal. Specifically, the post-revision price drift is more pronounced for innovative revisions issued by *Earnings-Estimators* in low-coverage firms, and is less pronounced for revisions issued by *All-Stars* for high-coverage firms.

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[19] We also included additional two-way interaction terms between analyst coverage and analyst ranking (Coverage × Earnings-Estimator and Coverage × All-Star) and three-way interaction terms (Coverage × Signal × Earnings-Estimator and Coverage × Signal × All-Star). These variables are not incrementally important in the regression, and do not change our results significantly.

Subsequent Earnings-Related News Announcements

To better understand the nature of the post-revision price drift, we conduct further tests around subsequent earnings-related news announcement dates. Specifically, we examine abnormal returns in three-day windows centered on subsequent earnings announcements and forecast revisions. The main purpose of these tests is to discriminate between the market-inefficiency and risk-based explanations for the post-revision price drift. If the post-revision drift is due to a delayed market response to current information about future earnings *and* this misperception is corrected when further information about future earnings is released, then subsequent abnormal returns should cluster around the future announcements of earnings news. Conversely, if the price drift is due to omitted risk variables, we should not observe higher abnormal returns concentrated around the release of subsequent earnings news.

Table 6 reports the results of these tests. Earlier results show that the post-revision price drift is associated primarily with high-innovation forecasts. Therefore, this analysis focuses only on the high-innovation revisions. Table values represent mean size-adjusted returns over three-day windows centered on the next four quarterly earnings announcements and the next six analyst forecast revisions. We include only revisions that occurred within 12 months of the original revision. To eliminate revisions that cluster closely together in time, we exclude from this analysis any revision that occurs within the three-day announcement window of a revision that is already included in the sample.

Table 6 reports returns for good-news and bad-news revisions, as well as the difference between the two (i.e., the cash-neutral hedge return). We also report the percentage of the abnormal one-year return to each hedge strategy that is realized in each three-day event window. Panel A presents the results for all firms, Panel B presents the results for high analyst coverage firms, and Panel C presents the results for low analyst coverage firms.

Panel A of Table 6 shows that the hedge return is positive around all four subsequent earnings announcements. Collectively, 11.1 percent of the abnormal return realized over the next 12 months is earned in the days immediately surrounding the next four earnings releases. Assuming expected returns do not vary daily, we expect 4.7 percent (12/253) of the abnormal return to occur over 12 trading days. The higher proportion of abnormal returns observed around earnings announcements suggests that at least a portion of the delayed price response is due to misperception about future earnings, which is corrected around future earnings release dates.

Even more striking are the results related to the next six analyst forecast revisions. These results show that the market continues to be surprised in a negative direction for bad-news revisions and in a positive direction for good-news revisions, when other analysts subsequently revise their forecasts. In fact, we observe a significant price reaction over at least the next *six* forecast revisions. Collectively, the abnormal returns around the next six revisions represent 26.8 percent of the total one-year abnormal returns to the hedge strategy—compared to 7.1 percent (18/253) by chance alone. This evidence suggests that much of the incomplete price response to a given revision is corrected when later analysts revise their forecasts.

The next two panels of Table 6 show that this result holds for both high-coverage and low-coverage firms. For low-coverage firms (Panel C), 13.9 percent of the price drift occurs around the next four earnings announcements, and 15.1 percent of the price drift occurs around the next six forecast revisions. For high-coverage firms (Panel B), where the drift itself is lower, a lower percentage of that drift (10.5 percent) occurs around future earnings announcements while a higher proportion of the drift (30.2 percent) appears around the next six forecast revisions.

TABLE 6

Mean Size-Adjusted Returns for Three-Day Windows around Subsequent Earnings Announcements and Subsequent Analyst Forecast Revisions for High-Innovation Signals[a,b]

	Subsequent Earnings Announcements				*Subsequent Analyst Forecast Revisions*			
	Bad News	*Good News*	*Difference*	*% of 1-Year Return*	*Bad News*	*Good News*	*Difference*	*% of 1-Year Return*
Panel A: All Firms								
1st	0.000	0.006*	0.006*	6.3	−0.007*	0.003*	0.010*	10.2
2nd	0.004*	0.006*	0.002*	2.1	−0.003*	0.001*	0.004*	4.3
3rd	0.005*	0.006*	0.001*	1.4	−0.003*	0.000	0.003*	3.1
4th	0.004*	0.005*	0.001*	1.3	−0.004*	0.000	0.003*	3.5
5th					−0.003*	0.000	0.003*	3.3
6th					−0.003*	−0.001‡	0.002*	2.4
Panel B: High Analyst Coverage[c]								
1st	0.001*	0.006*	0.005*	5.8	−0.007*	0.003*	0.010*	11.3
2nd	0.005*	0.006*	0.002*	1.8	−0.003*	0.001*	0.004*	4.8
3rd	0.005*	0.006*	0.001*	1.4	−0.003*	0.000	0.003*	3.6
4th	0.004*	0.005*	0.001*	1.5	−0.003*	0.000	0.003*	3.9
5th					−0.003*	0.000	0.003*	3.7
6th					−0.003*	0.000†	0.003*	2.9

(continued on next page)

TABLE 6 (continued)

	Subsequent Earnings Announcements				*Subsequent Analyst Forecast Revisions*			
	Bad News	*Good News*	*Difference*	*% of 1-Year Return*	*Bad News*	*Good News*	*Difference*	*% of 1-Year Return*
Panel C: Low Analyst Coverage[c]								
1st	−0.004*	0.010*	0.014*	8.4	−0.008*	0.002‡	0.010*	6.0
2nd	0.002‡	0.008*	0.006*	3.4	−0.004*	0.001	0.005*	2.9
3rd	0.004*	0.006*	0.002†	1.5	−0.004*	−0.002†	0.002†	1.5
4th	0.003*	0.004*	0.001	0.6	−0.005*	−0.001	0.004*	2.4
5th					−0.005*	−0.002	0.003*	2.1
6th					−0.003*	−0.003*	0.000	0.2

Two-tailed t-test of difference from zero is significant at: * $\alpha < 0.001$ ‡ $\alpha < 0.01$ † $\alpha < 0.05$.

[a] For each revision, we report the mean size-adjusted returns earned around the next four earnings announcements and the next six forecast revisions, as well as the percentage of the abnormal one-year hedge return realized in each event window. We include only revisions that occur within 12 months of the original revision, and exclude any revision that takes place within three days of a prior revision. Size-adjusted returns are buy-and-hold returns for three-day windows centered on the earnings announcement or forecast revision date, minus the return for an equal-weighted portfolio of firms in the same firm-size decile.

[b] High-Innovation Signals are forecast revisions that are either above ("Good News") or below ("Bad News") both the prior consensus and the analyst's own prior forecast.

[c] Low Analyst Coverage indicates that the number of analysts covering the firm is below the median analyst coverage among all firms in a given year. High Analyst Coverage indicates the number of analysts is above the median.

Firm-Level Analysis

Because our primary focus is on understanding how the market assimilates the information content of individual revisions, rather than on trading profits, our analyses thus far have used individual analyst forecast revisions as the unit of observation. However, in Table 7, we also report the results of a cross-sectional regression of future returns on firm-level explanatory variables. To perform this analysis, we convert revision-level information into firm-level indicator variables, and regress future size-adjusted returns on these variables.

We have two purposes for conducting this test. First, firm-level analysis provides an alternative solution to the sample dependence problem arising from including multiple observations for the same firm. The regressions reported in Table 5 address this econometric challenge using Huber-White estimation techniques. The firm-level regressions reported in Table 7 entirely eliminate revision-level dependency problems by ensuring that each firm appears only once in each regression. Because these results are based on monthly cross-sectional (i.e., Fama-MacBeth type) regressions, the Huber-White estimation technique is unnecessary. However, due to overlapping holding periods, the time-series t-statistics for the monthly cross-sectional coefficient estimates can still be inflated due to positive serial correlation. We adjust for this problem using Newey-West procedure (Newey and West 1987) procedures.

Second, Table 7 provides information on the economic significance of revision-based investment strategies, assuming monthly rebalancing. The results reported in this table are not exactly the same as those one would obtain from a revision-level strategy, because this test does not capture the returns that accrue between day $t + 1$ and the end of the first calendar month. Nevertheless, this table provides some insight on how an investor might fare if he or she were to trade on a revision-level signal using firm-level information at the end of each calendar month.

To construct this table, we perform monthly cross-sectional firm-level regressions. The dependent variable is the size-adjusted return for the six months (Ret6) or the 12 months (Ret12) following the month of the forecast revision. The key independent variable, FSignal, is a firm-level indicator variable that captures the information content of the most recent forecast revision for that firm. Specifically, FSignal = +1 if the most recent forecast revision issued in the month prior to portfolio formation is high-innovation and upward, −1 if the revision is high innovation and downward, and 0 otherwise.[20]

We also include three interaction terms: (1) the Coverage × FSignal variable equals FSignal multiplied by an indicator variable that equals 1 if the firm is in the high-coverage group, 0 otherwise; (2) the All-Star × FSignal variable equals FSignal multiplied by an indicator variable that equals 1 if the analyst is an All-Star, 0 otherwise; and, (3) the Earnings-Estimator × FSignal variable is equal to FSignal multiplied by an indicator variable that equals 1 if the analyst is an Earnings-Estimator, 0 otherwise. Analogous to Table 5, these firm-level interaction terms allow us to assess the incremental effect of analyst coverage and the analyst's ranking on post-revision price drift. Finally, we also control for Revise and Ret based on the most recent high-innovation revision, and three firm characteristics (Momentum, Size, and B/M).

Although our conversion from revision-level information to firm-level information involves significant loss of information, the results reported in Table 7 generally confirm the

[20] One could convert the revision-level signal to a firm-level signal in many ways, but each involves some loss of information. In earlier versions of this paper, we reported results based on alternative definitions of the Signal variable including: (1) the sign of the change in the monthly consensus forecast; (2) the net number of upward vs. downward revisions over the past month; and, (3) the net number of high-innovation upward vs. downward revisions over the past month. The results are all quite similar. We report results based on only the most recent forecast revision, because it seems most faithful to the spirit of the revision-level analysis we conducted earlier.

revision-level findings in Table 5. Specifically, even after controlling for Size, B/M, and Momentum, the sign of the most recent high-innovation earnings forecast revision in the prior month continues to predict future returns. The coefficients on FSignal provide an estimate of the returns from cash-neutral hedge strategies involving unranked analysts in low-coverage firms: annualized returns range from 15.2 percent based on the Ret6 regression (3.8 percent × 4) to 11.0 percent based on the Ret12 regression (5.5 percent × 2).

The coefficients on Coverage × FSignal show that, controlling for firm size, returns to an FSignal strategy are reliably lower for firms with high analyst coverage. Similarly, the

TABLE 7
Cross-Sectional Regressions of Future Size-Adjusted Returns on Firm-Level Explanatory Variables[a]

	Ret6	*Ret12*
Intercept	−0.011†	−0.016†
	(−2.22)	*(−2.03)*
Revise	0.011*	0.020*
	(4.28)	*(6.02)*
Ret	0.004†	0.009‡
	(2.55)	*(2.67)*
FSignal	0.038*	0.055*
	(7.74)	*(6.82)*
Coverage × FSignal	−0.025*	−0.034*
	(−4.54)	*(−5.99)*
All-Star × FSignal	−0.006†	−0.012‡
	(−2.45)	*(−2.81)*
Earnings-Estimator × FSignal	0.002	0.005
	(0.32)	*(0.49)*
Momentum	0.027*	0.032*
	(5.06)	*(3.70)*
Size	−0.008‡	−0.026*
	(−3.29)	*(−4.96)*
B/M	0.008	0.009
	(1.31)	*(0.78)*
R^2	0.043	0.033

Two-tailed t-test of difference from zero is significant at: * $\alpha < 0.001$ ‡ $\alpha < 0.01$ † $\alpha < 0.05$.

[a] This table reports the results of cross-sectional regressions of future size-adjusted returns on firm-level explanatory variables. The reported coefficient estimates and adjusted R^2s are the time-series averages from 63 monthly cross-sectional regressions. All reported t-statistics are corrected for autocorrelation using the Newey-West (1987) algorithm. Firms are included in a monthly regression only if they have at least one analyst forecast revision during the month. The number of firms per month in each regression ranges from 880 to 2,182.

The dependent variable for each regression is either the size-adjusted return for the 6 months following the month of the forecast revision (Ret6), or the size-adjusted return for the 12 months following the month of the forecast revision (Ret12).

(*continued on next page*)

TABLE 7 (continued)

The independent variables are:

Revise = the change in the monthly consensus forecast in the month prior to portfolio formation, expressed as a percentage of price;
Ret = the firm's size-adjusted return over the three-day event window around the last forecast issued in the month prior to portfolio formation;
FSignal = 1 for high-innovation good news, −1 for high-innovation bad news, and 0 otherwise; based on the last forecast issued in the month prior to portfolio formulation;
Coverage = 1 if the firm is covered by more than the median number of analysts where the median number of analysts is computed annually for the sample, and 0 otherwise;
All-Star = 1 if the analyst is ranked first, second, third, or runner-up in his industry in the *Institutional Investor* annual rankings, and 0 otherwise;
Earnings Estimator = 1 if the analyst is ranked first through fifth in his industry in the *Wall Street Journal* annual analyst rankings, and 0 otherwise;
Momentum = market-adjusted returns over the prior six months measured as of day $t - 2$. The variable is scaled by subtracting the mean and dividing by the standard deviation;
Size = the natural log of a firm's market capitalization measured as of the end of the last calendar year, with the extreme 1 percent of the observations winsorized. The variable is scaled by subtracting the mean and dividing by the standard deviation; and
B/M = the firm's book-to-market ratio measured as of the end of the last calendar year, with the extreme 1 percent of the observations winsorized. The variable is scaled by subtracting the mean and dividing by the standard deviation.

coefficients on All-Star × FSignal show that returns to an FSignal strategy are reliably lower for revisions issued by *All-Stars*. The coefficient on Earnings-Estimator × FSignal is insignificant, perhaps because relatively few high-innovation revisions in this firm-level analysis are issued by *WSJ Earnings-Estimators*. Similar to Table 5, FSignal has higher t-statistics than either Ret or Revise. These findings confirm that our prior inferences are not driven by the use of multiple revisions from the same firm.

V. CONCLUSION

Much capital market research in accounting over the past 20 years has assumed that the price adjustment to information is instantaneous. This basic assumption has influenced the way we select research topics, design empirical tests, and interpret research findings. In this study, we provide evidence that price discovery, at least as it pertains to analyst forecast revisions, is a protracted and complex process that deserves more scholarly attention.

Using a large sample of individual forecast revisions, we conjecture, and show, that the magnitude of the post-revision price drift is a function of various revision and firm characteristics. First, analogous to findings in the post-earnings-announcement drift literature, we show that the delayed price response is a function of *revision quantity*—that is, firms with good-news revisions of a higher magnitude experience more positive subsequent returns and firms with bad-news revisions experience negative subsequent returns.

Second, we find that several measures of *revision quality* also help explain the delayed price response. Specifically, we show that the market does not make a sufficient distinction between analysts who bring new information to market (high-innovation revisions) and those who may be simply herding toward the consensus forecast (low-innovation revisions). We also find that an analyst's ranking affects the speed and efficacy with which prices reflect the information content of a revision. Specifically, we show that, compared to unranked analysts, both *II All-Stars* and *WSJ Earnings-Estimators* elicit a stronger immediate market reaction. However, the post-revision drift is significantly smaller for *All-Stars* and significantly larger for *Earnings-Estimators*. We attribute this difference to the celebrity status of the *All-Stars*, and the fact that *Earnings-Estimators* are equally accurate but often more obscure analysts.

Third, we show that the delayed price response to forecast revisions is reliably smaller for firms covered by more analysts. Controlling for other variables, the number of analysts itself does not predict future returns. However, higher analyst coverage leads to faster and more complete assimilation of the information conveyed by the level of revision innovation.

Finally, we find that a substantial portion of the delayed price reaction occurs around subsequent earnings-news event dates. This result is not due to revisions that are closely clustered in time. After excluding all revisions that occur within a three-day window of any prior revision, we find that a disproportionate amount of the post-revision price drift occurs within short-windows around the next *four* earnings announcements and the next *six* forecast revisions. This evidence suggests that the market is adjusting slowly to the information contained in individual forecast revisions, and that future revisions by other analysts act as catalysts in the price discovery process.

In sum, the market seems to overlook certain qualitative aspects of earnings revisions. After controlling for the sign and magnitude of the forecast revision itself, as well as various firm characteristics known to be associated with expected returns, the market still seems to underappreciate more subtle aspects of the revision, such as the level of innovation in the revision signal, and whether the analyst is a superior forecaster from a smaller brokerage firm (*WSJ Earnings-Estimators*). On the other hand, the market seems to assimilate earnings information faster and more completely when more analysts are following the firm and when the analyst issuing the forecast has celebrity status (*II All-Stars*). These findings help us to better understand the price discovery mechanism and the boundaries of market efficiency.

Another interpretation of our results is that good-news firms (i.e., firms whose earnings have just been revised upward) are riskier than bad-news firms, and that the higher average returns earned by good-news firms represent compensation for this higher level of risk. This interpretation allows us to remain within the comfortable confines of the efficient market hypothesis. However, it appears inconsistent with several aspects of our evidence. First, it leaves open questions as to why firm-specific risk should be *positively* correlated with the release of good news about earnings.[21] Second, it begs the question of what risk factor might be missing even after we have controlled for price momentum, firm size, and book-to-market. Third, it does not explain why a large portion of the abnormal returns occurs around the dates of subsequent earnings news releases. Finally, it does not explain why the magnitude of the abnormal returns should be a function of signal quality, the analyst's ranking, and the extent of analyst coverage. Given our collective evidence, we are skeptical of the risk-based explanation.

Finally, our results are related to the current debate over Regulation Fair Disclosure (FD). This regulation, effective October 2001, prohibits management from discussing company prospects with an analyst unless the same information is simultaneously conveyed to others, including other analysts. Our evidence suggests that individual financial analysts bring relevant information to market, and that multiple analysts enhance the speed of the price discovery process. One cannot help but wonder whether this will continue to hold

[21] According to the risk explanation, firms that earn higher subsequent returns are riskier in some unknown dimension. Yet in this study, firms with positive rather than negative news earn higher future returns. Therefore, a risk-based interpretation would need to explain why good-news firms are becoming riskier.

APPENDIX

Sample Screen Capture of First Call™ Information Delivered by FactSet™
Corning Inc. (GLW) Individual Analyst Earnings Forecast Revisions for Fiscal Year Ended 12/2001

CORNING INC	11-Jan-2002
GLW 21935010	NYSE
United States: MULTI-IND CAP GOOD	USD
Division Factor: 1.02	*Hold (2.77)*

**Denotes excluded by I/B/E/S*

Date of Latest Change	*Broker*	*Analyst*	*12/2001*	*Change Amount*	*Change %*	*12/2002*	*Change Amount*	*Change %*
11-Jan-2002	**Mean (23 Ests)**		**.44**			**NA**		
28-Nov-2001	Needham & Company	LANGLEY,M	.44			−.37		
20-Nov-2001	Thomas Weisel Partners	BUNTING,J	.45	−.46	−50.5	−.39	−1.56	−133.3
14-Nov-2001	Roth Capital Partners	KANG,D	.45			−.45		
06-Nov-2001	Morgan Stanley, Dean Witter Dis	JACKSON,D	.45	−.15	−25.0	−.31	−.35	−875.0
24-Oct-2001	J.P. Morgan	LIPTON,J	.41	−.14	−25.5	−.05	−.19	−135.7
24-Oct-2001	Davenport & Co. Of Virginia, In	JOHNSTONE,D	.44	−.07	−13.7	−.41	−.41	
24-Oct-2001	Merrill Lynch	FOX,S	.43	−.07	−14.0	−.17	−.02	
24-Oct-2001	C.E. Unterberg, Towbin	DRAGONE,J	.42	−.10	−19.2	−.40	−.28	
23-Oct-2001	Goldman Sachs & Co.	SUNDARAM,N	.45	.01	2.3	−.31	−.35	−875.0
23-Oct-2001	Dresdner Kleinwort Benson	KINGSTON,S	.48			−.37	−.26	
23-Oct-2001	First Union Securities	KOFFLER,S	.42	−.17	−28.8	−.47	−.54	−771.4
23-Oct-2001	Frost Securities	HAIDER,S	.45	−.02	−4.3	−.45	−.95	−190.0
23-Oct-2001	Deutsche Banc Alex. Brown-Us	SRIKANTH,R	.43	−.12	−21.8	−.55	−.57	−2850.0
22-Oct-2001	Robertson Stephens	VEERAPPANA,A	.42	−.18	−30.0	−.48	−.61	−469.2
22-Oct-2001	A. G. Edwards & Sons, Inc.	ANDREW,P	.44	−.15	−25.4	−.36	−.57	−271.4
22-Oct-2001	Banc of America Securities Lic.	CRESPI,C	.46	−.12	−20.7	−.44	−.63	−331.6
22-Oct-2001	Credit Lyonnais Securities (USA	LOWY,G	.42	−.47	−52.8	−.36	−.09	
22-Oct-2001	UBS Warburg	THEODOSOPOULC	.45	−.14	−23.7	−.40	−.55	−366.7
22-Oct-2001	Wit Soundview	SLOCUM,K	.45	−.02	−4.3	−.10	−.15	−300.0
22-Oct-2001	William Blair & Company, L.L.C.	TANGO,R	.47	−.07	−13.0	−.44	−.54	−540.0
19-Oct-2001	Salomon Smith Barney Inc	ANDERSON,T	.44	−.02	−4.3	−.35	−.20	
19-Oct-2001	Credit Suisse First Boston Corp	SCHUETZ,M	.47	−.05	−9.6	−.46	−.20	
19-Oct-2001	ABN Amro Incorporated	POTTER\HOLMSTE	.45	−.05	−10.0	−.25	−.05	

under the new regulations. At first blush, it seems unlikely that the forecast revisions of individual analysts would become more useful under Regulation FD. This appears to be an interesting avenue for future research.

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