Are Financial Analysts' Forecasts of Corporate Profits Rational?

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This paper develops generalized method-of-moments tests for the rationality of earnings per share forecasts made by individual stock analysts. We fail to reject the hypothesis of rationality as long as we take into account two complications: (1) the correlation in a given period of analysts' forecast errors in predicting earnings for firms in the same industry and (2) discretionary asset write-downs, which affect earnings but are intentionally ignored by analysts when they make earnings forecasts. Our results challenge earlier work by De Bondt and Thaler and by Abarbanell and Bernard that found irrationality in analysts' forecasts.

I. Introduction

A substantial literature exists in accounting and finance that examines the properties of financial analysts' forecasts of corporate earnings. Researchers have been interested in analysts' forecasts for a variety of reasons, and we consider three here.

One reason is that asset pricing and cost-of-capital models generally involve earnings expectations variables for which proxies must be provided if these models are to be tested empirically or imple-

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mented in practice. Using time-series models to provide such proxies is common, but these proxies suffer from two problems. First, they may be less accurate than actual market expectations because they incorporate only a small set of information (i.e., lagged values of earnings and other variables). Second, when time-series models are used to generate expectations, any test of the asset pricing or cost-of-capital model under consideration becomes a joint test of the model of interest and the time-series model of expectations.

Given these two problems, a number of authors have shown interest in the properties of analysts' forecasts both because they may provide a superior proxy for market expectations and because, if one accepts their validity, one may construct direct tests of the asset pricing or cost-of-capital model that are of interest, while treating expectations as given. Examples of papers motivated by this line of interest are the following: (1) studies that have examined the accuracy of analysts' forecasts and, in particular, whether they are more accurate than forecasts from simple time-series models, such as those by Cragg and Malkiel (1968), Elton and Gruber (1972), Brown and Rozeff (1978), Crichfield, Dyckman, and Lakonishok (1978), Collins and Hopwood (1980), Fried and Givoly (1982), Elton, Gruber, and Gultekin (1984), and O'Brien (1988, 1990); and (2) studies that have examined the extent to which share price movements are associated with analysts' forecast revisions and forecast errors, such as those by Ball and Brown (1968), Beaver, Clarke, and Wright (1979), Givoly and Lakonishok (1979), Fried and Givoly (1982), Brown et al. (1987), Hughes and Ricks (1987), O'Brien (1988), and Lys and Sohn (1990).

A second reason for interest in analysts' forecasts is that if these forecasts do measure market expectations, then evidence of excess volatility or irrationality in analysts' expectations may help to explain what some researchers argue are excessively volatile asset price movements or anomalous market behavior. This line of research is exemplified by the work of De Bondt and Thaler (1985, 1990) and, later, by the work of Klein (1990), Abarbanell (1991), Mendenhall (1991), Abarbanell and Bernard (1992), and Ali, Klein, and Rosenfeld (1992).

A third reason for interest in analysts' forecasts is that they may provide a rare opportunity to test the rational expectations hypothesis. We doubt that data on expectations measure agents' true expectations unless those data are subject to some type of market test (see Keane and Runkle 1990). But since financial analysts' livelihoods depend on the accuracy of their forecasts and since we observe the same forecasts that the analysts sell, we can plausibly argue that these numbers accurately measure the analysts' expectations. Studies that examine whether analysts' forecasts have the properties of rational forecasts (i.e., that test for unbiasedness or efficiency or both) are those by Crichfield et al. (1978), Fried and Givoly (1982), Givoly (1985), O'Brien (1988), De Bondt and Thaler (1990), Klein (1990), Abarbanell (1991), Mendenhall (1991), Abarbanell and Bernard (1992), Ali et al. (1992), and Xiang (1992). There is also a related literature in economics on testing the rationality of forecasts, as illustrated by Brown and Maital (1981), Figlewski and Wachtel (1981), Zarnowitz (1985), Frankel and Froot (1987), and Keane and Runkle (1990).

In this paper, we provide a new analysis of analysts' forecasts that is most closely connected to the second and third lines of research. Specifically, we test the rationality of individual analysts' earnings forecasts as reported in the Institutional Brokers Estimate System (I/B/E/S) data set. Although many studies have already examined this issue, we justify yet another on the basis that the issue of cross-sectional correlation in analysts' forecast errors has not yet been fully addressed.

Several authors (esp. Crichfield et al. 1978; Bernard 1987; O'Brien 1988; Abarbanell 1991; Abarbanell and Bernard 1992) have noted that statistical inference about the properties of analysts' forecasts is very difficult if forecast errors are correlated across forecasters or firms. If, at time t, multiple analysts forecast time $t + 1$ earnings for a firm, their forecast errors will tend to be positively correlated as long as unanticipated shocks to earnings occur between t and $t + 1$. The same is true if these analysts forecast earnings for multiple firms and if shocks occur between t and $t + 1$ that affect all firms similarly. Any test of unbiasedness or efficiency that makes use of data on multiple forecasters or multiple firms will tend to overreject the null hypothesis if such positive correlations are ignored.

In this paper, we develop a generalized method-of-moments (GMM) estimator that gives correct statistical inference in the presence of complex patterns of correlation across analysts in their forecast errors. We show that failure to account for these correlations leads to overwhelming rejections of unbiasedness and efficiency in the I/B/E/S data but that a correct statistical inference (accounting for these correlations) is that unbiasedness and efficiency cannot be rejected. Note that we cannot reject the hypothesis that analysts fully incorporate into their earnings forecasts the information contained in both lagged earnings reports and lagged stock price behavior. Thus many of the rejections of rationality of analysts' forecasts that have been published appear to be due solely to downward-biased standard errors.

II. Our Work versus Related Literature

Some previous studies have attempted to deal with the problem of correlated errors across forecasters or firms. To our knowledge, Crichfield et al. (1978) first noted the problem. They stated that "at any point in time, forecasts for all companies may be cross-sectionally correlated due to aggregate market events" and that "a relatively long time span is required to test the ability of SA's [security analysts] to estimate the mean of the EPS (earnings per share) distribution" (p. 653). In their empirical work, Crichfield et al. used data on the mean of analyst forecasts of annual earnings for 46 firms in the years 1967–76 from the *Standard and Poor's Earnings Forecaster.*

Such a short time period may not be adequate for tests of rationality if large aggregate shocks occur that affect many companies. If aggregate shocks are important, then mean forecast errors (defined as actual EPS minus the mean EPS forecast) will tend to be positive or negative for individual years and will have mean zero only over time (not over firms at a point in time). This is why Crichfield et al. stated that "studies based on a comparison of realizations with forecasts over a short time horizon are likely to be deficient" (p. 653). At the time they did their analysis, the *Earnings Forecaster* data were available for only 10 annual observations. Even with this short a time period, they could not reject unbiasedness of the mean forecast. However, as we shall show below, with only 10 time periods, even one large aggregate shock could cause a rejection of unbiasedness. Considerably longer time spans are necessary to avoid sensitivity to this type of problem. Fried and Givoly (1982) also studied unbiasedness of the mean forecasts of annual earnings from the *Earnings Forecaster,* using data on 424 firms for the 1969–79 period. They found that the mean forecast is biased upward. However, since the number of time periods is only 10, this result may be due to aggregate shocks during the sample period, as Crichfield et al. suggest.

O'Brien (1988) studied annual EPS forecasts of analysts in the I/B/E/S data set for the 1975–81 period, which gave seven annual observations. The sample in her analysis has data on 184 firms and 1,260 firm years. O'Brien was apparently the first to deal with aggregate shocks by allowing for random period-specific shocks when testing for unbiasedness, a procedure that we generalize below. She finds weak evidence that forecasts are upward-biased (i.e., too optimistic) but correctly observes that

> an alternative explanation consistent with these results is that analysts issue unbiased forecasts, but this seven-year period, 1975 through 1981, is one with primarily negative

> unanticipated EPS. Unfortunately, the most obvious way to distinguish between the hypothesis of deliberate optimistic bias and this alternative is to collect data for a longer span of years. This is not possible with the I/B/E/S detail data. [P. 65]

In this paper, we extend O'Brien's work on the I/B/E/S data in three ways. First, we use the I/B/E/S data on quarterly earnings forecasts from the fourth quarter of 1983 to the fourth quarter of 1991 in order to achieve a time-series length of 33 periods.[1] This greater time span should reduce the sensitivity of our results to aggregate shocks. As an example, suppose that analysts' annual EPS forecasts were generally overly optimistic for 1975 because the severity of the recession was not anticipated in late 1974. Nevertheless, by the end of the first quarter of 1975, the severity of the recession was apparent, so the quarterly earnings forecasts for the second through fourth quarters should not have been overly optimistic. Second, we allow for firm-specific as well as aggregate shocks. Third, we develop a GMM estimator that allows us to test for efficiency as well as unbiasedness while taking into account both aggregate and firm-specific shocks.[2]

In a pair of recent papers examining analyst forecast rationality, Abarbanell (1991) and Abarbanell and Bernard (1992) both test for unbiasedness and efficiency using the most recent analyst forecast from the Value Line Investment Survey. Abarbanell studied quarterly forecasts for the years 1981–84 for 100 firms and found that the mean forecast error is negative (an overestimate) and that a positive correlation exists between prior share price changes and analysts' forecast errors (i.e., a positive [negative] price change increases the probability of a low [high] earnings forecast). Abarbanell and Bernard studied quarterly forecasts for 178 firms in the 1976–86 period, giving a time-series length of 44 periods. They found that the Value Line analysts' forecast errors are positively autocorrelated for the first three quarterly lags (i.e., they do not efficiently utilize the information in their lagged errors), that unbiasedness can be rejected because analysts are overly optimistic, and that analysts' errors are positively correlated with the lagged change in earnings (i.e., analysts underreact to earnings changes). However, as Abarbanell and Bernard state,

[1] Quarterly I/B/E/S data started in 1983, even though annual data were available earlier.

[2] The problems for statistical inference created by aggregate shocks have also been discussed by Bernard (1987).

> the standard errors should be interpreted with caution, given that the assumption of independence across firms is almost certainly violated. . . . Cross-sectional dependence is of concern . . . because all firms are affected by economy-wide movements. However, given the limited number of time series observations available here, relative to the number of firms, standard techniques for adjusting for cross-sectional dependence are not feasible. [P. 1188]

One contribution of our paper is to provide a GMM technique to adjust for cross-sectional dependence that is feasible for this type of data.

III. Econometric Issues

Suppose that analyst n makes a forecast in time t of EPS for firm j in period $t + 1$. We shall denote that forecast as ${}_t\mathrm{EPS}^j_{n,t+1}$. We wish to test whether such an analyst's predictions are rational in Muth's (1961) sense, that is, that they are equal to the mathematical expectation of actual EPS, conditional on the information available to analyst n at time t. In other words,

$$ {}_t\mathrm{EPS}^j_{n,t+1} = E(\mathrm{EPS}^j_{t+1} | I_{n,t}), \tag{1} $$

where EPS^j_{t+1} is actual EPS for firm j in period $t + 1$, $I_{n,t}$ is the information available to analyst j at time t, and E is the mathematical expectations operator.

Note that if all analysts have the same loss function, private information accounts for the differences in forecasts among analysts. Under that condition, if analysts all had exactly the same information, they would make the same forecast. Otherwise their forecasts would not be rational.

For an individual analyst, a test of forecast rationality can be performed by running the regression

$$ \mathrm{EPS}^j_{t+1} = \alpha_0 + \alpha_1 {}_t\mathrm{EPS}^j_{n,t+1} + \alpha_2 X_{n,t} + \epsilon^j_{n,t+1}, \tag{2} $$

where $X_{n,t}$ is any variable known to analyst n at time t. *Unbiasedness* implies that in a regression without $X_{n,t}$ variables, the coefficients in equation (2) may be restricted to $\alpha_0 = 0$ and $\alpha_1 = 1$. Efficiency requires that any variable known by n at time t should have no predictive power in the regression; that is, $\alpha_2 = 0$ (in addition to $\alpha_0 = 0$ and $\alpha_1 = 1$).

At least two reasons can be given to explain why regression tests for unbiasedness and efficiency could lead to rejections, even if analysts were rational in forming their expectations. First, analysts may not have symmetric loss functions. They may be penalized more for a large overprediction than for a large underprediction. Second, aggregate shocks may cause the sample mean forecast error for an individual to be nonzero for a finite T. In either of these cases, we could reject forecast rationality, even though analysts made optimal forecasts given their information sets and their loss functions.[3]

We could test analyst forecast rationality by randomly selecting one analyst and one firm. If we did so, we could estimate equation (2) by ordinary least squares (OLS). This sampling method will give test statistics that are consistent in T, the number of time periods for which analysts' forecasts are observed. But we may want to improve the power of our tests by including forecasts from multiple analysts for multiple firms. However, if we include these additional observations, our statistical inference will be invalid unless we correctly model the covariance of forecast errors across analysts and across firms.

We address this issue of error covariance in two parts. First, we discuss the individual analyst's information set and the intertemporal correlation of forecast errors for the individual analyst. Second, we discuss how forecast errors are correlated across analysts and across firms.

We shall now consider what is contained in the information set of analyst n in period t. Certainly, any public information known at time t, such as previous earnings announcements by the firm, should be known to the analyst. Such public information should certainly be orthogonal to $\epsilon^j_{n,t+1} \equiv \mathrm{EPS}^j_{t+1} - {}_t\mathrm{EPS}^j_{n,t+1}$, the analyst's one-step-ahead forecast error in predicting EPS for firm j. In addition to public information, equation (1) implies that any private information that the analyst had at time t, such as the analyst's own prior forecasts and forecast errors, should also be orthogonal to $\epsilon^j_{n,t+1}$. And if other analysts' forecasts or the average of other analysts' forecasts is announced publicly, they should also be orthogonal to analyst n's forecast error.

A key issue is whether an analyst knows his or her previous forecast error at the time he or she forecasts EPS. In the I/B/E/S data we use, the release of information happened in the sequence shown in figure 1, where the solid vertical lines represent the end of each time period. Figure 1 shows that, in each period, EPS for the previous period is announced before analyst n makes a forecast of EPS for the current period. In this case, analyst n's previous forecast error $(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$ is known when the analyst makes the prediction

[3] We also do not consider in this paper whether analysts are making their predictions strategically, on the basis of predictions made by other analysts.

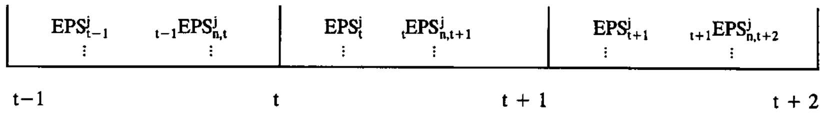


FIG. 1

$_t\mathrm{EPS}^j_{n,\,t+1}$. Therefore, the previous forecast error should be orthogonal to the current forecast error.[4] All private or public information known by the analyst when the analyst makes the forecast could be included in (2) to conduct a valid test of forecast rationality.

We next discuss how forecast errors are correlated *across* analysts and firms. If we understand this issue, we can increase the power of our tests of rationality by including observations on multiple analysts and on multiple firms.

We start by considering the case in which multiple analysts make forecasts for the same firm. As we noted previously, if the analysts all had exactly the same information (and the same loss function), they would make exactly the same forecast. In this case, the analysts' forecast errors would be exactly the same, and considering multiple analysts would produce no efficiency gain. The only gain to considering multiple analysts would come from the differences in analysts' forecasts that arise from an individual analyst's private information. But, even in this case, we would expect a very high correlation among analysts' forecasts (and forecast errors) because of the public information that they share.

Suppose that N analysts make one-step-ahead forecasts for firm j. Under the null hypothesis of forecast rationality, we assume that the variances and covariances of the analysts' forecast errors are

$$E(\epsilon^j_{n,\,t+1}\epsilon^j_{n,\,t+1+s}) = \begin{cases} a, & s = 0 \\ 0, & s \neq 0 \end{cases} \tag{3}$$

and

$$E(\epsilon^j_{n,\,t+1}\epsilon^j_{m,\,t+1+s}) = \begin{cases} c, & s = 0,\ m,\ n \leq N,\ m \neq n \\ 0, & s \neq 0. \end{cases} \tag{4}$$

There are two sources of these restrictions. First, the variance of an analyst's forecast error, equation (3), differs from the covariance of two different analysts' forecast errors, equation (4), because each

[4] If we had used k-step-ahead forecasts, each analyst's forecast errors would be MA($k - 1$), as discussed by Hansen and Hodrick (1980).

analyst possesses private information about the firm. Second, forecast errors are uncorrelated across time, if the forecasts are rational, because we use only forecasts that are made after EPS for the previous quarter is released. We have shown how to conduct statistical inference in this case in Keane and Runkle (1990).

We further increase the power of our tests for rationality by including observations on forecasts for different firms.[5] This step requires additional assumptions and a new estimation procedure, and it is the focus of our paper. Just as forecast errors across analysts for one firm are correlated because of public information, forecast errors for a single analyst for multiple firms in an industry are correlated because of unforeseen events that affect all firms in an industry. Of course, because information about industry conditions is public, forecast errors will be correlated across analysts for different firms in the industry as well.

Suppose now that an industry has N analysts and J firms. In each time period, each analyst makes predictions about EPS for each firm. Assume that at period t each analyst makes a one-step-ahead prediction for EPS for each firm. Under the null hypothesis of forecast rationality, equations (3) and (4) hold. However, we make two additional sets of assumptions about the covariances of analysts' forecasts across firms:

$$E(\epsilon^{j}_{n,\,t+1}\epsilon^{l}_{n,\,t+1+s}) = \begin{cases} b, & s = 0,\ j,\ l \leq J,\ j \neq l \\ 0, & s \neq 0 \end{cases} \tag{5}$$

and

$$E(\epsilon^{j}_{n,\,t+1}\epsilon^{l}_{m,\,t+1+s}) = \begin{cases} d, & s = 0,\ m,\ n \leq N,\ m \neq n,\ j,\ l \leq J,\ j \neq l \\ 0, & s \neq 0. \end{cases} \tag{6}$$

Equation (5) allows an individual analyst's forecast errors for different firms in an industry to be correlated. This correlation occurs because of unforeseen events that affect all firms in the industry. Note that the covariance of an analyst's forecast errors for different firms, equation (5), differs from the variance of the analyst's forecast error for a single firm, equation (3), because some unforeseen events are firm specific. Therefore, $b < a$.

Equation (6) allows different analysts' forecast errors for different firms in an industry to be correlated. This correlation occurs because of unforeseen events that affect all firms in the industry. However,

[5] The power of the tests will increase as long as analysts' forecast errors across firms are not perfectly correlated.

the covariance of forecast errors across firms for different analysts, equation (6), differs from the covariance of forecast errors across firms for a single analyst, equation (5), because each analyst can have private information about industry conditions. Therefore, $d < c$.

As with equations (3) and (4), equations (5) and (6) do not allow serial correlation in the errors. Again, this restriction stems from our use of forecasts that are made after EPS for the previous quarter is released.

Finally, note that this error structure, by assuming *homoskedasticity,* assumes that variances and covariances do not differ across forecasters or firms. In Section IV we normalize EPS across firms by dividing EPS by the stock price at the end of the previous quarter. This normalization is crucial to justify our assumption of homoskedasticity.

With covariance structure (3)–(6), errors are not independent across forecasters or across firms. Thus any attempt to estimate equation (2) by OLS will yield inconsistent test statistics since OLS standard errors are constructed under the assumption that all errors are independent and identically distributed. In Appendix A we propose a feasible GMM estimator for equation (2). Our estimator uses exactly the same orthogonality restrictions as OLS, so the coefficient estimates are the same as those of OLS. However, our estimator uses the information in the error covariance structure (3)–(6) to correctly compute the standard errors for the coefficient estimates. It differs from the GMM estimator used in Keane and Runkle (1990) because that earlier estimator can be used only when forecasters make predictions for only one time series. That estimator would not let us test the rationality of forecasts made by analysts for multiple firms within an industry.

Unlike OLS, the feasible GMM estimator will yield test statistics that are consistent in T. Consistency is in T rather than the number of analysts or the number of firms in an industry because forecast errors that arise from shocks affecting an entire industry will not cancel out across analysts or firms. That is, the sample version of the orthogonality condition $E(\epsilon^{j}_{n,t+1}|I_{n,t})$ converges to zero as the number of time periods increases, but not as the number of analysts or firms increases, if the number of time periods is held fixed.[6]

We now consider our five specific tests of rationality, all of which test the rationality of one-step-ahead forecasts. First, we test for unbiasedness. (If analysts' forecasts are biased, conducting further tests of efficiency is pointless.) Second, we test whether the analyst's previous one-step-ahead forecast is correlated with the analyst's current

[6] This point was first noted, in a different context, by Chamberlain (1984).

one-step-ahead forecast error. Third, we test whether the earnings announcement from period t is correlated with the analyst's current one-step-ahead forecast error. (This test shows whether analysts either underreact or overreact to the most recent earnings announcement.) Fourth, we test whether the analyst's lagged one-step-ahead forecast error is correlated with the analyst's current one-step-ahead forecast error. (This test shows whether an analyst learns from his or her own past forecast errors.) Fifth, we test whether the average lagged one-step-ahead forecast error by all analysts covering a firm is correlated with the analyst's current one-step-ahead forecast error.

IV. Data

The data for our study come from three sources. We use individual analyst predictions from I/B/E/S, earnings data from Compustat, and data about the timing of stock splits and stock dividends from the Center for Research in Security Prices (CRSP).

We believe that the I/B/E/S individual analyst data set is one of only two potential sources of data on individual analyst forecasts that satisfy two criteria necessary for implementing our econometric methods.[7] First, a unique code identifies each analyst. This identification is necessary to allow us to test the hypotheses about private information. Second, the date on which the forecast was made can be identified with reasonable accuracy. This dating is necessary so that our assumptions about the analysts' information sets are correct. We return to this issue later in the paper.

We choose six four-digit Standard Industrial Classification (SIC) industries to analyze, on the basis of the number of firms in the industry and analyst coverage. Within each industry we choose those firms for which a minimum of 100 quarterly forecasts were made in at least 25 different quarters from the fourth quarter of 1983 to the fourth quarter of 1991.[8] We choose industries for which at least three firms satisfied these criteria. We also restrict our sample to firms having a December 31 fiscal year end. Table B1 in Appendix B shows a list of the industries we use.

Since we want to ensure that the forecasts were made by professional earnings analysts rather than analysts who had made just a

[7] The other data set that could be used is the Zacks individual forecast database (see Stickel 1990). Value Line does not contain multiple individual forecasts. In addition, since Value Line does not publish how it computes its "actual" earnings numbers, there is no way to independently verify their construction from the raw financial reports.

[8] The average firm had observations for 29 quarters. The I/B/E/S quarterly data are not available before the fourth quarter of 1983.

couple of forecasts, we restrict our sample to the predictions of analysts who made forecasts in at least five different quarters. We use only forecasts designated as predictions of primary EPS, so that forecasts are comparable across analysts.[9]

Finally, we restrict our sample to those forecasts for which we have reasonable assurance that the firm's earnings announcement from the previous quarter was known at the time the analyst made the forecast. We do this by restricting our sample to those forecasts recorded at least 7 days after the firm's earnings announcement for the previous quarter.[10]

The mechanics of this restriction deserve further explanation. The I/B/E/S records the date on which a forecast is entered into the database rather than the date on which the forecast was made. But we have three reasons to think that the entry date is within a week of the date on which the forecast was made. First, since 1983, I/B/E/S has recorded the forecasts quite quickly.[11] Second, the vast majority of analysts work in New York, where I/B/E/S is located, so postal time is likely to be short.[12] Finally, the empirical distribution of forecast entry dates shows that virtually no forecasts are entered in the 7 days before an earnings announcement but that a large number of forecasts are entered after 7 days. Since analysts are more likely to make a new forecast immediately after the earnings announcement than immediately before, this pattern in the empirical distribution of entry dates suggests that a 7-day cutoff is sufficient to ensure that the analyst made the new prediction after the firm's earnings announcement.

Our data for actual EPS come from Compustat. We use Compustat earnings data rather than I/B/E/S earnings data because of the well-known problems with data alignment in the I/B/E/S earnings data (see Philbrick and Ricks 1991). We use primary EPS before extraordinary items as our measure of earnings because that is the measure of EPS that corresponds best to what I/B/E/S states the analysts are trying to predict (see Institutional Brokers Estimate System

[9] If sufficient stock options or convertible bonds are outstanding, firms are required to report fully diluted EPS, taking into account potential share dilution, in addition to primary EPS. We exclude forecasts of fully diluted EPS.

[10] We use Compustat's earnings announcement dates.

[11] In private conversations, I/B/E/S officials reported that from the fourth quarter of 1983 to the first quarter of 1985, forecasts were recorded within 5 days of receipt. Since the second quarter of 1985, I/B/E/S has done all of its data entry in-house. Forecasts are now entered within 2 days of receipt. Throughout the sample, we find no problems with delays in I/B/E/S data entry, such as those noted for earlier periods by Brown, Foster, and Noreen (1985) and O'Brien (1988).

[12] In fact, by the end of the sample, almost all the forecasts were sent electronically to I/B/E/S, so that they were entered into the database on the same day they were made.

1987). Even this measure of earnings may not be perfect in all cases, however; we discuss it in further detail below. To eliminate heteroskedasticity in forecast errors, we normalize both predicted EPS and actual EPS by dividing both by the stock price on the last day of the previous quarter.

All the data we use are corrected for stock splits, as listed on the CRSP master tape. If a split is announced and occurs between the time in which a forecast is made and the earnings announcement, the actual EPS is adjusted to conform to the presplit forecast. If a split is announced between the end of the previous quarter and the time in which the forecast is made, the previous quarter's stock price is adjusted to conform to the postsplit forecast and earnings announcement.

V. Empirical Results

We now consider our tests for unbiasedness and efficiency of individual analysts' forecasts for each of the six industries in our sample. Since we use quarterly data in our study, the one-step-ahead forecasts discussed in Section IV are one-quarter-ahead forecasts. All our tests are based on these one-quarter-ahead forecasts.

The first set of tests is based on analysts' one-quarter-ahead earnings forecasts in the chemical industry. Panel A of table 1 shows tests of the unbiasedness and efficiency of those forecasts. Row 1 of this panel shows that if OLS is used to estimate equation (2), the value of the test statistic for the null hypothesis of unbiasedness is 40.31. Since this statistic should be distributed asymptotically as a χ^2_2 random variable if the null hypothesis is true, that hypothesis is rejected overwhelmingly. This rejection should not be surprising. We argued in Section III that OLS standard errors will understate the true amount of parameter uncertainty because OLS ignores the dependence of analysts' forecast errors within a given time period.

Row 2 of panel A shows what happens to the test statistic for unbiasedness when our new GMM estimator is used. Since the model is exactly identified, the parameter estimates are exactly the same as for OLS, but the standard errors are much larger. This increase in the standard errors causes the test statistic for the null hypothesis of unbiasedness to drop from 40.31 to only 6.51. However, the null hypothesis of unbiasedness can still be rejected at the 5 percent level.

At this point, we might appear to have fairly strong evidence that analysts' earnings forecasts for the chemical industry are biased. But this is not so. Figure 2 shows that a few outlying observations are responsible for the rejection of unbiasedness.

In panel *a* of figure 2, the analyst's forecast is on the *X*-axis and

TABLE 1

TESTS OF FORECAST UNBIASEDNESS AND EFFICIENCY: SIC 2800

Equation	α_0	α_1	α_2	χ^2 for H_0	Method	Regressors	Observations
				A. All Observations			
1	−.0128	1.3214	. . .	40.3103	OLS	$1,\ {}_t\text{EPS}^j_{n,t+1}$	588
	(.0025)	(.1035)		(.0000)			
2	−.0128	1.3214	. . .	6.5188	GMM	$1,\ {}_t\text{EPS}^j_{n,t+1}$	588
	(.0062)	(.2646)		(.0384)			
3	−.0039	1.1064	−.0987	4.9198	GMM	$1,\ {}_t\text{EPS}^j_{n,t+1},$	229
	(.0047)	(.2005)	(.1924)	(.1778)		${}_{t-1}\text{EPS}^j_{n,t}$	
4	−.0130	1.3581	−.0347	39.4815	OLS	$1,\ {}_t\text{EPS}^j_{n,t+1},$	575
	(.0026)	(.2740)	(.0343)	(.0000)		EPS^j_t	
5	−.0130	1.3581	−.0347	4.9198	GMM	$1,\ {}_t\text{EPS}^j_{n,t+1},$	575
	(.0064)	(.2740)	(.0651)	(.1778)		EPS^j_t	
6	−.0052	1.0589	−.0163	4.7962	GMM	$1,\ {}_t\text{EPS}^j_{n,t+1},$	229
	(.0042)	(.1733)	(.0507)	(.1873)		$(\text{EPS}^j_t - {}_{t-1}\text{EPS}^j_{n,t})$	
7	−.0130	1.3254	.0001	6.2411	GMM	$1,\ {}_t\text{EPS}^j_{n,t+1},$	552
	(.0066)	(.2783)	(.0686)	(.1005)		$(\text{EPS}^j_t - {}_{t-1}\overline{\text{EPS}}^j_t)$	
				B. Special-Charge Censoring			
1	−.0068	1.1612	. . .	35.1502	OLS	$1,\ {}_t\text{EPS}^j_{n,t+1}$	572
	(.0016)	(.0644)		(.0000)			
2	−.0068	1.1612	. . .	3.6655	GMM	$1,\ {}_t\text{EPS}^j_{n,t+1}$	572
	(.0050)	(.2165)		(.1600)			
3	.0003	1.0763	−.1509	3.6097	GMM	$1,\ {}_t\text{EPS}^j_{n,t+1},$	223
	(.0034)	(.1474)	(.1411)	(.3068)		${}_{t-1}\text{EPS}^j_{n,t}$	
4	−.0069	1.1809	−.0193	30.9333	OLS	$1,\ {}_t\text{EPS}^j_{n,t+1},$	559
	(.0016)	(.0685)	(.0212)	(.0000)		EPS^j_t	
5	−.0069	1.1809	−.0193	3.3757	GMM	$1,\ {}_t\text{EPS}^j_{n,t+1},$	559
	(.0051)	(.2263)	(.0538)	(.3372)		EPS^j_t	
6	−.0021	.9970	−.0118	2.7014	GMM	$1,\ {}_t\text{EPS}^j_{n,t+1},$	223
	(.0032)	(.1302)	(.0382)	(.4400)		$(\text{EPS}^j_t - {}_{t-1}\text{EPS}^j_{n,t})$	
7	−.0068	1.1575	−.0012	3.4108	GMM	$1,\ {}_t\text{EPS}^j_{n,t+1},$	536
	(.0053)	(.2282)	(.0561)	(.3325)		$(\text{EPS}^j_t - {}_{t-1}\overline{\text{EPS}}^j_t)$	

NOTE.—Standard errors are in parentheses under coefficients. Significance levels are in parentheses under test statistics.

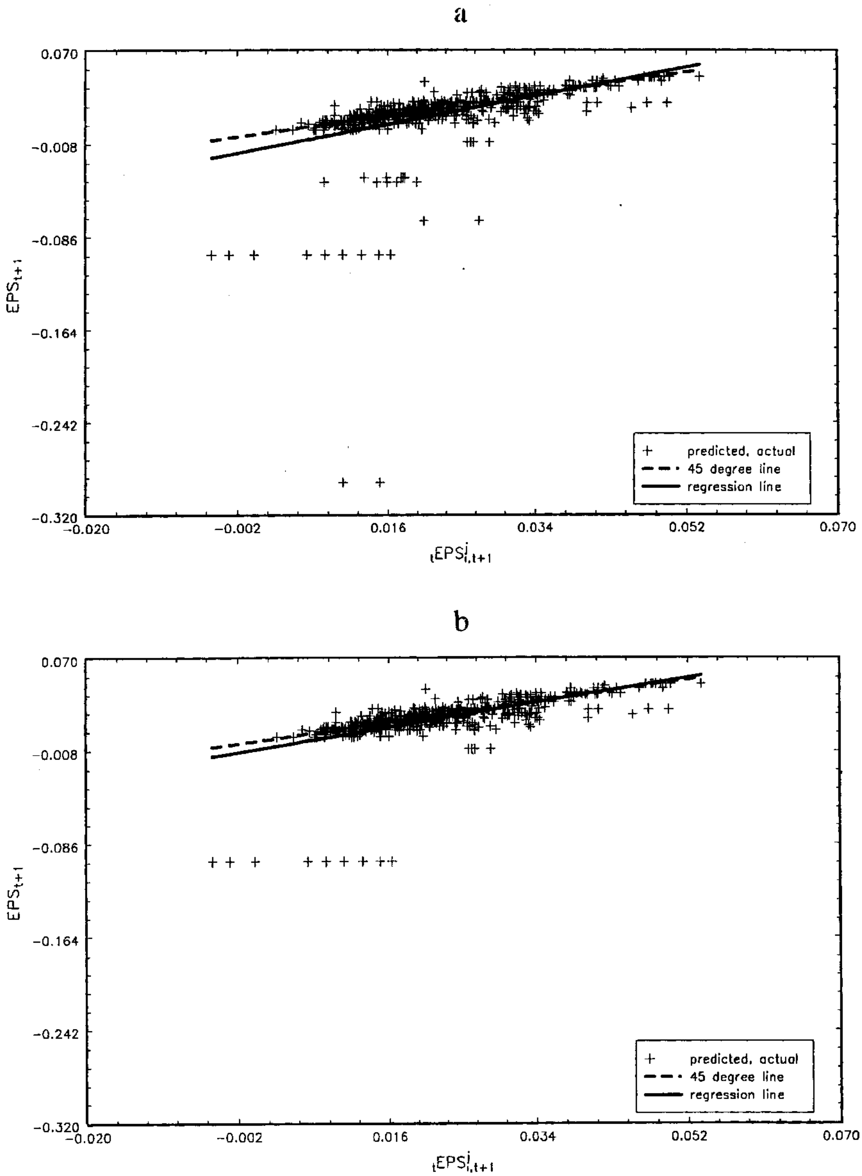


FIG. 2.—EPS forecasts and realizations (SIC 2800). *a*, All observations. *b*, Special-charge censoring.

the actual earnings announcement is on the *Y*-axis. As before, both the forecast and the announcement are normalized. The crosses represent one analyst's forecast and the subsequent earnings announcement for one firm in one quarter, that is, the observations we use in our regressions. The dashed line is the 45-degree line. Its slope in the panel is different from 45 degrees because of the different scales of the *X*- and *Y*-axes. The solid line is the fitted regression line from the test of unbiasedness.

The slope of the fitted regression line is clearly greater than that of the 45-degree line, as the earlier regression coefficients showed. But figure 2 shows that this steep slope is caused by a few outlying observations. These observations have very large negative values for actual earnings. For example, the two observations with the lowest values of actual earnings represent quarterly losses per share that are more than one-fourth of the stock price at the end of the previous quarter.

Several of the observations plotted in panel *a* of figure 2 are cases in which the firm had a large above-the-line asset write-down or other special accrual. However, there are good theoretical reasons for deleting such observations. Philbrick and Ricks (1991, p. 401) note that "I/B/E/S refers to extraordinary items as 'write downs which are at the discretion of management,' while according to generally accepted accounting principles, not all discretionary write-downs qualify as extraordinary items. Therefore, the earnings components included in an I/B/E/S forecast may not be the same as in the corresponding Compustat actuals."[13] Thus the standard measure of actual earnings that we use—EPS, before discontinued operations and extraordinary items—will not accurately reflect what analysts are trying to predict if a large above-the-line asset write-down or other special charges occur in a given quarter.[14]

We solve this problem in panel *b* of figure 2 by eliminating the observations for which the discretionary special charge[15] per share

[13] They also note that Value Line generally excludes special above-the-line items that Compustat includes in pretax EPS before extraordinary items and discontinued operations.

[14] Philbrick and Ricks (1991) discuss this issue in detail. However, they attempt to adjust for the tax effects of these discretionary accruals so that they can still include these observations in their analysis. We do not think that a researcher could come up with an unbiased estimate of the after-tax earnings that analysts are trying to predict if such a discretionary accrual occurs. If biased estimates of after-tax earnings were used, the resulting regression coefficients and test statistics would be inconsistent. Thus we believe that omitting these observations is the only way to prevent invalid statistical inference.

[15] Although generally accepted accounting principles specify a uniform terminology and set of qualifications for extraordinary items and discontinued operations, there are no such restrictions for discretionary asset write-offs and other before-tax special charges. Compustat lumps these items under the description "special

(normalized by the beginning-of-quarter share price) was larger than four standard deviations from the average, price-normalized analyst forecast error for the industry for all periods.[16] When these observations are eliminated, the slope of the fitted regression line becomes almost exactly the same as the 45-degree line.

In each of the cases omitted in panel *b* of figure 2, the firm had a large discretionary special charge. American Cyanamid reported a special charge of $291.9 million in the third quarter of 1990. Dow Chemical reported a special charge of $592 million during the fourth quarter of 1985. Olin reported a charge of $303 million to nonoperating income in the third quarter of 1985 and a special charge of $80 million in the first quarter of 1991. Details on these charges from the relevant annual reports are included in Appendix B. Including observations with these charges would result in incorrect statistical inference since I/B/E/S specifies that such charges are not to be included in the analysts' earnings forecasts. We dropped each of the forecasts made by analysts for Olin and Monsanto in these cases.[17]

Panel B of table 1 shows the regression results that correspond to observations shown in panel *b* of figure 2 when we eliminate the effects of the previously mentioned large discretionary special charges. Row 1 of this panel shows the results of estimating equation (2) using OLS. Note that the test statistic for unbiasedness is still so large (35.15) that the null hypothesis of unbiasedness is rejected. This rejection is suspect, however, since it assumes that all the observations are independent.

Row 2 of panel B shows the results of estimating equation (2) on the smaller sample using the GMM estimator. Here the test statistic for the null hypothesis of unbiasedness is small enough (3.67) that the hypothesis is not rejected.

By comparing the first two rows of both panels, we can see the importance of correctly selecting our data sample and correctly selecting our estimator for correct statistical inference about the unbiasedness of analysts' one-quarter-ahead forecasts in the chemical industry. If we either included observations containing large discretionary special charges or used OLS, we would incorrectly decide

charges.'' However, in annual reports they could also be called nonrecurring charges, restructuring charges, or asset write-offs, or whatever the firm wants to call them. We shall refer to them as discretionary special charges in this paper.

[16] We validated the special charges using variable 32 on both the quarterly Compustat tapes and annual reports. We chose a cutoff based on the standard deviation of average industry forecast error because the standard deviation should measure how big the earnings surprise was that was caused by the special charge.

[17] This restriction reduces the number of observations in our unbiasedness tests from 588 to 572.

that the analysts' forecasts were biased. Only when we use both a correct sample and an estimator that accounts for correlation among analysts' forecast errors do we fail to reject the hypothesis of unbiasedness.

Panel B of table 1 also shows that the hypothesis of forecast efficiency is not rejected as long as the GMM estimator is used. Rows 3–7 show efficiency tests. In each of these tests, a single variable in the forecasters' time *t* information set was included as the extra regressor in equation (2). The tests were conducted separately, rather than jointly, because a given observation could not be included in the sample if any single variable were missing. Hence, an unacceptably small number of observations would have been included in the joint test.

Row 3 shows the effect of adding to equation (2) the analyst's own previous one-step-ahead forecast. The χ^2 test statistic shows that that variable has no additional explanatory power in predicting actual earnings beyond that of the current one-step-ahead forecast.

Rows 4 and 5 show the effect of adding to equation (2) the earnings announcement that was released shortly before the analyst's forecast was made. Row 4 shows that if the previous earnings announcement is included and OLS is used, the hypothesis of efficiency is rejected. Row 5 shows that if the same equation is estimated using the GMM estimator, the hypothesis of efficiency is not rejected.

Row 6 shows that analysts learn from their own past forecast errors. An analyst's immediate past one-step-ahead forecast error does not significantly help to predict firm earnings, conditioned on the analyst's current one-quarter-ahead forecast. Row 7 shows that the average immediate past one-step-ahead forecast error of all analysts covering the firm also makes no significant incremental contribution in predicting earnings.

All these tests show that we fail to reject either unbiasedness or efficiency of analysts' one-quarter-ahead forecasts in the chemical industry if we use the GMM estimator and we eliminate observations with large discretionary above-the-line write-downs and accruals.

The remaining tables and figures in the paper show the results of similar investigations for the other industries in our sample. For each of the next four industries, in the top panel of the tables and figures, we present the results of using all the observations in the sample. In the bottom panel of the tables and figures, we present the results of eliminating all analyst forecasts that contained large discretionary special charges, using the four-standard-deviation criterion discussed above. Appendix B contains the details of the large special charges, as discussed in the firms' annual reports. Note that these additional tests for analyst forecast rationality in different industries are not additional independent observations because aggregate economic shocks can cause correlation in analysts' forecast errors across industries. At best, the analysis of these different industries can give us some indication of whether the results we found for the chemical industry were representative of all industries.

Tables 2–5 and figures 3–6 tell a consistent story. As long as we use the GMM estimator and exclude observations with large discretionary above-the-line write-downs or accruals, no evidence disputes the hypothesis that analysts' earnings forecasts are rational. Using the GMM estimator, we reject neither unbiasedness nor efficiency. All these estimates provide additional support for concluding that analysts' forecasts are rational.

The only industry in which analysts' forecasts do not appear to be rational is the airline industry. Table 6 shows that no matter which estimator or sample is used, both the unbiasedness and the efficiency of analysts' forecasts are rejected. In addition, there is no difference between panels A and B of table 6 because none of the airlines included had a large discretionary special charge during the sample period. But this result should not be too surprising. In 1990 and 1991 the airline industry suffered historically unprecedented losses. Figure 7 shows exactly how bad the losses were in that industry. In fact, airlines lost more money in those two years than they made in the previous 60 years. For any analyst to have accurately assessed the combined effects of the Gulf War and the recession on the airline industry in those years would have been almost impossible. Claiming that analysts' forecasts were not rational simply because they could not accurately predict the magnitude of the earnings catastrophe that hit the airline industry seems far-fetched. The airline results are an excellent illustration of how large aggregate shocks can cause inconsistent estimates for a small T.

One potential criticism of our study is that we arbitrarily chose a four-standard-deviation cutoff to eliminate observations with large special charges. At the suggestion of the referee, we reestimated each of the regressions using both a 3.5- and a 4.5-standard-deviation cutoff. The results were very similar. When we used the 3.5-standard-deviation cutoff, none of the tests for tables 1–5 using the truncated sample rejected forecast rationality. When we used the 4.5-standard-deviation cutoff, rationality was rejected only for a single test (eq. 7 in table 3). We believe that a 4.5-standard-deviation cutoff is quite extreme. Since the sensitivity tests change our results in only one extreme case in which the sample contains observations that we believe should be excluded, those tests reinforce our conclusions that the analysts' forecasts are rational.

TABLE 2

TESTS OF FORECAST UNBIASEDNESS AND EFFICIENCY: SIC 3330

Equation	α_0	α_1	α_2	χ^2 for H_0	Method	Regressors	Observations
				A. All Observations			
1	−.0225 (.0048)	1.5038 (.1113)	. . .	24.7697 (.0000)	OLS	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1}$	302
2	−.0225 (.0091)	1.5038 (.2223)	. . .	7.2193 (.0271)	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1}$	302
3	−.0228 (.0074)	1.4909 (.2684)	.1072 (.2505)	12.4818 (.0059)	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$ ${}_{t-1}\mathrm{EPS}^{j}_{n,t}$	116
4	−.0235 (.0050)	1.5511 (.1455)	−.0245 (.1033)	25.6337 (.0000)	OLS	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$ EPS^{j}_{t}	300
5	−.0235 (.0093)	1.5511 (.2599)	−.0245 (.1033)	7.4214 (.0596)	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$ EPS^{j}_{t}	300
6	−.0267 (.0075)	1.6841 (.1788)	−.1787 (.0999)	16.7461 (.0008)	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$ $(\mathrm{EPS}^{j}_{t} - {}_{t-1}\mathrm{EPS}^{j}_{n,t})$	116
7	−.0263 (.0101)	1.5927 (.2440)	−.0605 (.1140)	7.8688 (.0488)	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$ $(\mathrm{EPS}^{j}_{t} - {}_{t-1}\overline{\mathrm{EPS}}^{j}_{t})$	295
				B. Special-Charge Censoring			
1	.0045 (.0022)	.8840 (.0514)	. . .	5.3620 (.0685)	OLS	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1}$	296
2	.0045 (.0039)	.8840 (.0977)	. . .	1.6900 (.4296)	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1}$	296
3	.0048 (.0040)	.8001 (.1406)	.0995 (.1235)	2.4898 (.4771)	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$ ${}_{t-1}\mathrm{EPS}^{j}_{n,t}$	114
4	.0051 (.0023)	.8395 (.0659)	.0305 (.0294)	6.5497 (.0877)	OLS	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$ EPS^{j}_{t}	294
5	.0051 (.0041)	.8395 (.1169)	.0305 (.0447)	2.1217 (.5475)	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$ EPS^{j}_{t}	294
6	.0038 (.0042)	.9209 (.0993)	−.0488 (.0453)	3.0587 (.3827)	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$ $(\mathrm{EPS}^{j}_{t} - {}_{t-1}\mathrm{EPS}^{j}_{n,t})$	114
7	.0058 (.0045)	.8545 (.1107)	.0150 (.0489)	2.0245 (.5673)	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$ $(\mathrm{EPS}^{j}_{t} - {}_{t-1}\overline{\mathrm{EPS}}^{j}_{t})$	289

NOTE.—Standard errors are in parentheses under coefficients. Significance levels are in parentheses under test statistics.

TABLE 3

TESTS OF FORECAST UNBIASEDNESS AND EFFICIENCY: SIC 3334

Equation	α_0	α_1	α_2	χ^2 for H_0	Method	Regressors	Observations
				A. All Observations			
1	−.0122 (.0035)	1.2734 (.1144)	. . .	14.6069 (.0007)	OLS	$1,\ {}_t\mathrm{EPS}^j_{n,t+1}$	326
2	−.0122 (.0055)	1.2734 (.1831)	. . .	5.7692 (.0559)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1}$	326
3	−.0048 (.0043)	1.1495 (.1814)	−.0945 (.1984)	3.9481 (.2671)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ ${}_{t-1}\mathrm{EPS}^j_{n,t}$	118
4	−.0117 (.0035)	1.2144 (.1244)	.0478 (.0375)	16.1505 (.0010)	OLS	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ EPS^j_t	322
5	−.0117 (.0054)	1.2149 (.1873)	.0478 (.0494)	6.8431 (.0771)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ EPS^j_t	322
6	−.0035 (.0037)	1.0232 (.1271)	.1842 (.1095)	6.0758 (.1080)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ $(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	118
7	−.0122 (.0052)	1.2521 (.1715)	.0521 (.0508)	9.4325 (.0241)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ $(\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	313
				B. Special-Charge Censoring			
1	−.0086 (.0035)	1.1700 (.1131)	. . .	8.3672 (.0152)	OLS	$1,\ {}_t\mathrm{EPS}^j_{n,t+1}$	322
2	−.0086 (.0051)	1.1700 (.1714)	. . .	3.6245 (.1633)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1}$	322
3	−.0015 (.0043)	1.0846 (.1768)	−.1271 (.1886)	2.4367 (.4868)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ ${}_{t-1}\mathrm{EPS}^j_{n,t}$	117
4	−.0080 (.0035)	1.1054 (.1228)	.0516 (.0364)	10.2934 (.0162)	OLS	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ EPS^j_t	318
5	−.0080 (.0052)	1.1054 (.1791)	.0516 (.0473)	4.8198 (.1855)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ EPS^j_t	318
6	−.0005 (.0036)	.9343 (.1236)	.1938 (.1034)	5.2603 (.1537)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ $(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	117
7	−.0084 (.0049)	1.1441 (.1623)	.0579 (.0485)	7.0569 (.0701)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ $(\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	309

NOTE.—Standard errors are in parentheses under coefficients. Significance levels are in parentheses under test statistics.

TABLE 4

TESTS OF FORECAST UNBIASEDNESS AND EFFICIENCY: SIC 3711

Equation	α_0	α_1	α_2	χ^2 for H_0	Method	Regressors	Observations
				A. All Observations			
1	−.0064	.9790	...	19.1318	OLS	$1,\ {}_t\mathrm{EPS}^j_{n,t+1}$	557
	(.0018)	(.0361)		(.0001)			
2	−.0064	.9790	...	2.0252	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1}$	557
	(.0063)	(.1049)		(.3633)			
3	−.0087	.9206	.0598	2.8733	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$	241
	(.0069)	(.1426)	(.1418)	(.4116)		${}_{t-1}\mathrm{EPS}^j_{n,t}$	
4	−.0069	.9248	.0759	23.3465	OLS	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$	549
	(.0019)	(.0444)	(.0354)	(.0000)		EPS^j_t	
5	−.0069	.9248	.0759	2.2593	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$	549
	(.0064)	(.1313)	(.1165)	(.5204)		EPS^j_t	
6	−.0085	.9613	−.0595	2.9268	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$	241
	(.0066)	(.1273)	(.1563)	(.4031)		$(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	
7	−.0070	1.0027	−.0123	1.7771	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$	538
	(.0066)	(.1227)	(.1708)	(.6199)		$(\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	
				B. Special-Charge Censoring			
1	.0001	.9414	...	7.7374	OLS	$1,\ {}_t\mathrm{EPS}^j_{n,t+1}$	516
	(.0013)	(.0246)		(.0209)			
2	.0001	.9414	...	1.1871	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1}$	516
	(.0041)	(.0660)		(.5224)			
3	−.0007	.9038	.0271	1.7610	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$	216
	(.0045)	(.0844)	(.0830)	(.6235)		${}_{t-1}\mathrm{EPS}^j_{n,t}$	
4	−.0002	.8277	.1446	48.6952	OLS	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$	508
	(.0012)	(.0296)	(.0225)	(.0000)		EPS^j_t	
5	−.0002	.8277	.1446	6.4412	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$	508
	(.0036)	(.0758)	(.0653)	(.0920)		EPS^j_t	
6	.0020	.8726	.1689	5.6326	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$	216
	(.0043)	(.0760)	(.0882)	(.1309)		$(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	
7	.0024	.9073	.2187	6.4048	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$	497
	(.0039)	(.0715)	(.0905)	(.0934)		$(\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	

NOTE.—Standard errors are in parentheses under coefficients. Significance levels are in parentheses under test statistics.

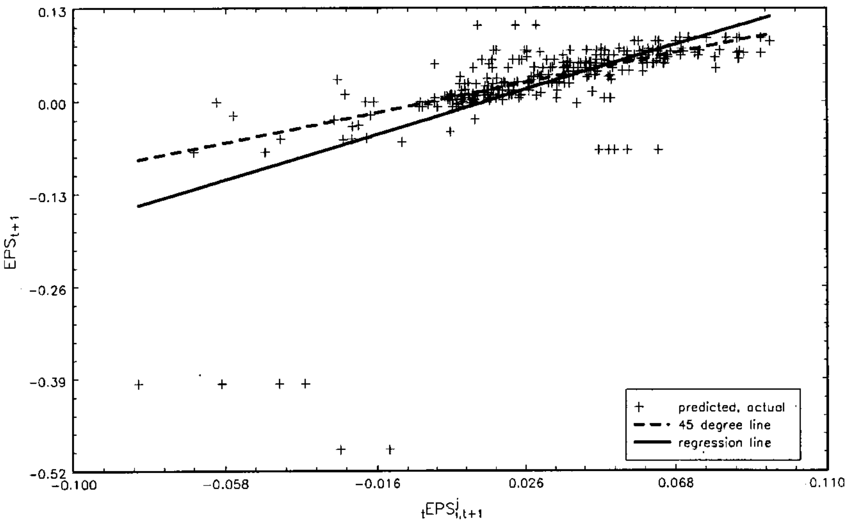
TABLE 5

Tests of Forecast Unbiasedness and Efficiency: SIC 4011

Equation	α_0	α_1	α_2	χ^2 for H_0	Method	Regressors	Observations
				A. All Observations			
1	−.0292	1.7256	. . .	26.0697	OLS	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1}$	277
	(.0097)	(.4049)		(.0000)			
2	−.0292	1.7256	. . .	3.9868	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1}$	277
	(.0204)	(.8245)		(.1362)			
3	−.0668	2.0176	1.2577	8.3297	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$	86
	(.0268)	(1.0459)	(1.1144)	(.0397)		${}_{t-1}\mathrm{EPS}^{j}_{n,t}$	
4	−.0319	1.7004	.1709	29.3671	OLS	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$	271
	(.0099)	(.4195)	(.1024)	(.0000)		EPS^{j}_{t}	
5	−.0319	1.7004	.1709	4.8245	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$	271
	(.0212)	(.8643)	(.2097)	(.1851)		EPS^{j}_{t}	
6	−.0497	2.6641	.2075	7.8439	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$	86
	(.0220)	(.9268)	(.2034)	(.0494)		$(\mathrm{EPS}^{j}_{t} - {}_{t-1}\mathrm{EPS}^{j}_{n,t})$	
7	−.0337	1.8983	.1174	4.4499	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$	248
	(.0250)	(1.0469)	(.2232)	(.2168)		$(\mathrm{EPS}^{j}_{t} - {}_{t-1}\overline{\mathrm{EPS}}^{j}_{t})$	
				B. Special-Charge Censoring			
1	−.0007	.9309	. . .	15.9156	OLS	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1}$	262
	(.0023)	(.0942)		(.0003)			
2	−.0007	.9309	. . .	5.1829	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1}$	262
	(.0036)	(.1474)		(.0749)			
3	−.0080	1.0909	.1079	4.7149	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$	81
	(.0077)	(.2948)	(.2938)	(.1939)		${}_{t-1}\mathrm{EPS}^{j}_{n,t}$	
4	−.0016	.9558	.0124	15.5673	OLS	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$	256
	(.0023)	(.0978)	(.0230)	(.0014)		EPS^{j}_{t}	
5	−.0016	.9558	.0124	5.1629	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$	256
	(.0038)	(.1547)	(.0359)	(.1603)		EPS^{j}_{t}	
6	−.0067	1.1541	.0346	5.1530	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$	81
	(.0064)	(.2689)	(.0529)	(.1609)		$(\mathrm{EPS}^{j}_{t} - {}_{t-1}\mathrm{EPS}^{j}_{n,t})$	
7	−.0028	.9974	−.0025	5.2716	GMM	$1,\ {}_{t}\mathrm{EPS}^{j}_{n,t+1},$	233
	(.0044)	(.1878)	(.0381)	(.1530)		$(\mathrm{EPS}^{j}_{t} - {}_{t-1}\overline{\mathrm{EPS}}^{j}_{t})$	

Note.—Standard errors are in parentheses under coefficients. Significance levels are in parentheses under test statistics.

a



b

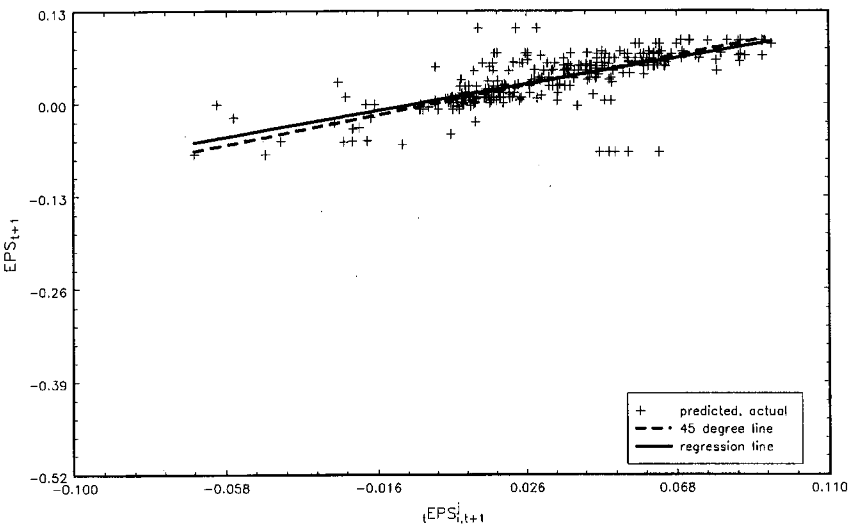


FIG. 3.—EPS forecasts and realizations (SIC 3330). *a*, All observations. *b*, Special-charge censoring.

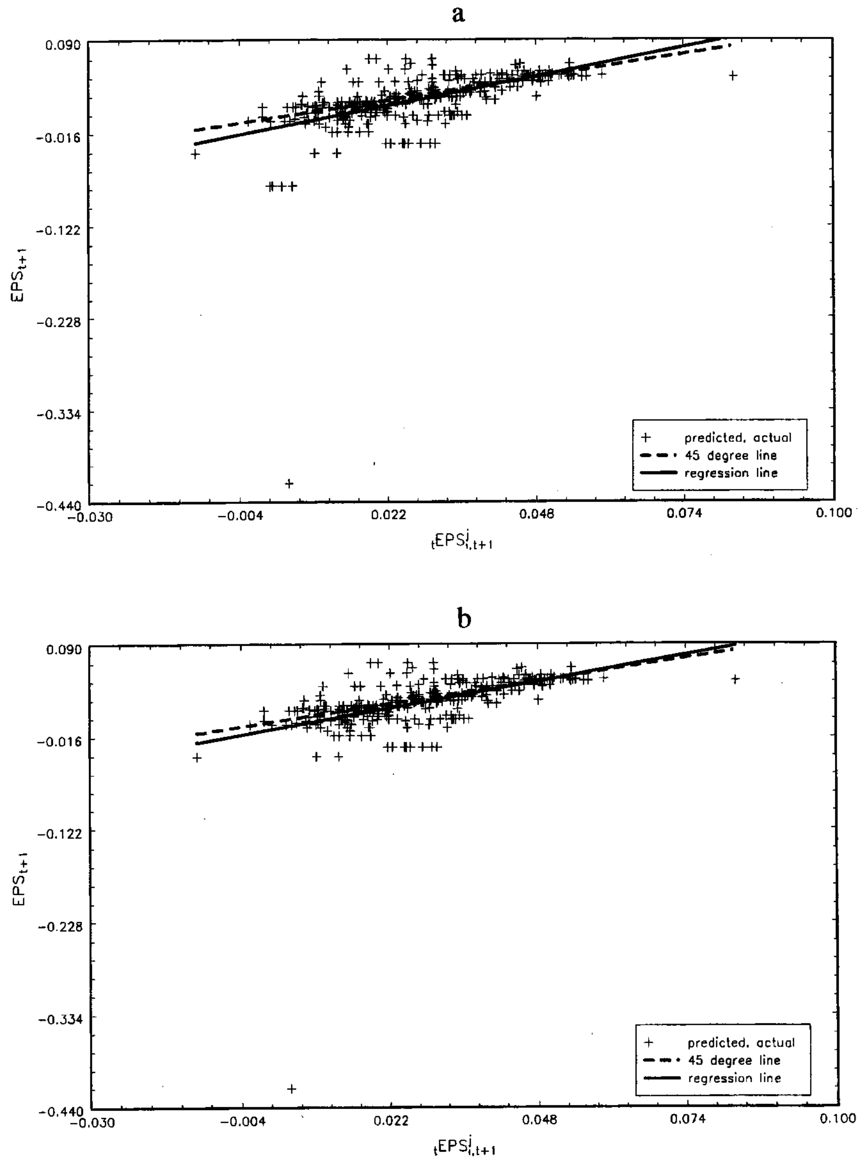
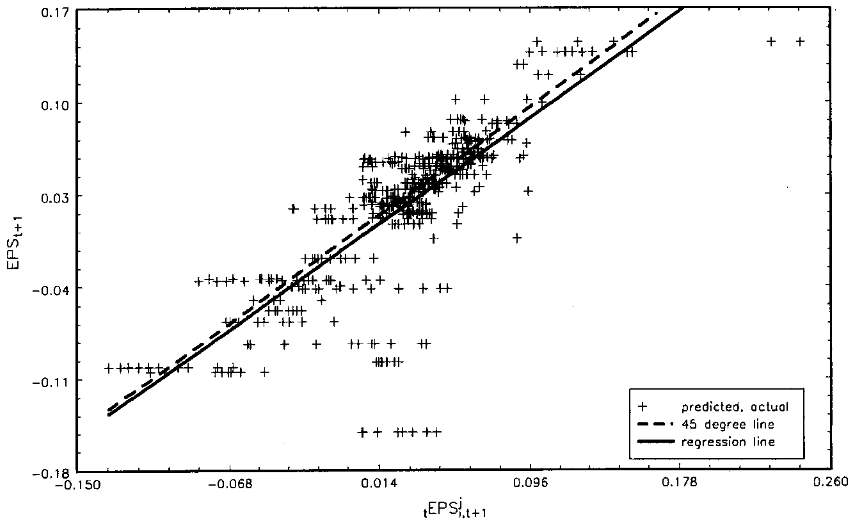


FIG. 4.—EPS forecasts and realizations (SIC 3334). *a*, All observations. *b*, Special-charge censoring.

a



b

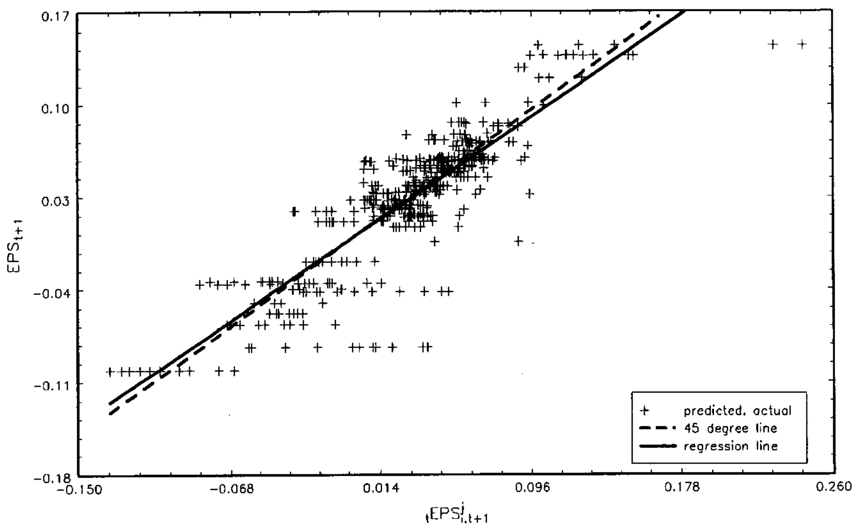


FIG. 5.—EPS forecasts and realizations (SIC 3711). *a*, All observations. *b*, Special-charge censoring.

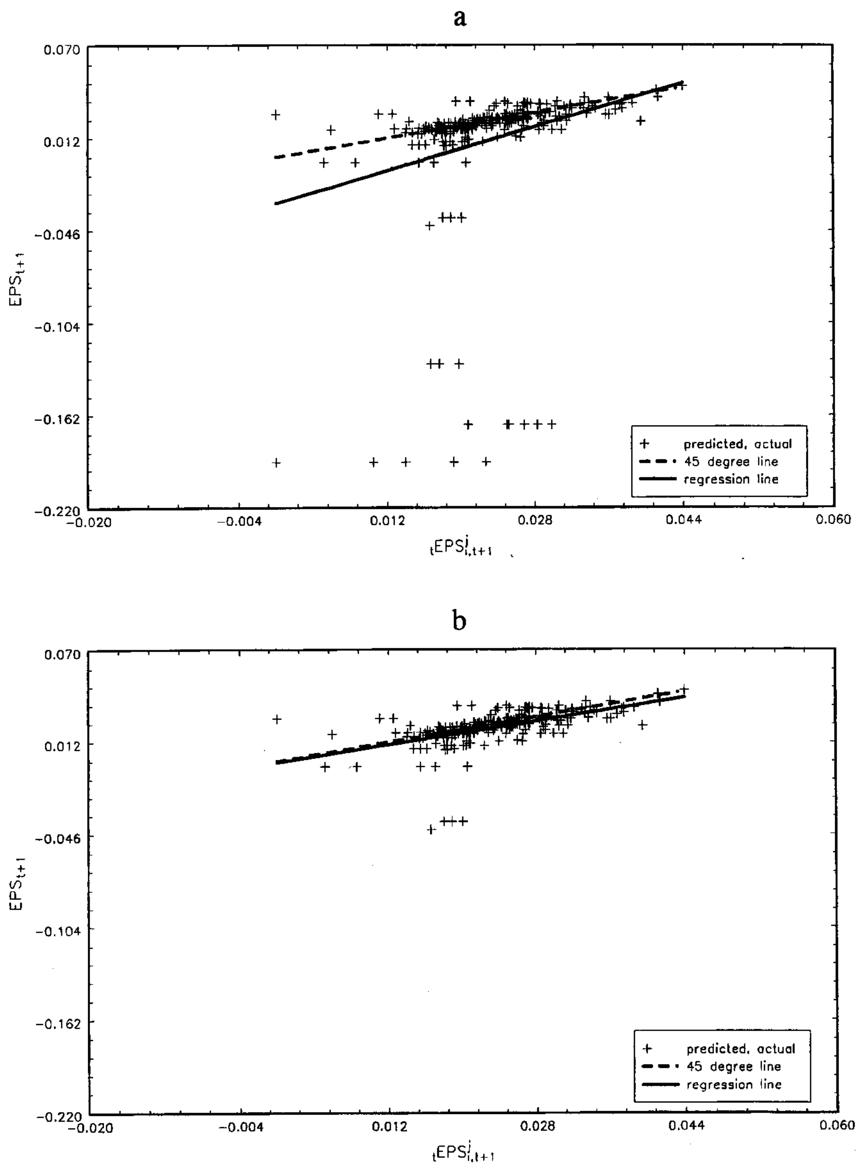


FIG. 6.—EPS forecasts and realizations (SIC 4011). *a*, All observations. *b*, Special-charge censoring.

TABLE 6

Tests of Forecast Unbiasedness and Efficiency: SIC 4512

Equation	α_0	α_1	α_2	χ^2 for H_0	Method	Regressors	Observations
				A. All Observations			
1	−.0117 (.0014)	1.2553 (.0271)	. . .	139.3637 (.0000)	OLS	$1,\ {}_t\mathrm{EPS}^j_{n,t+1}$	501
2	−.0117 (.0033)	1.2553 (.0673)	. . .	20.7546 (.0000)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1}$	501
3	−.0130 (.0037)	1.2091 (.0804)	.0306 (.0900)	22.3149 (.0001)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ ${}_{t-1}\mathrm{EPS}^j_{n,t}$	195
4	−.0016 (.0013)	.9558 (.0412)	.0124 (.0324)	228.5348 (.0000)	OLS	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ EPS^j_t	493
5	−.0104 (.0038)	.9871 (.1076)	.2656 (.0865)	24.8202 (.0000)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ EPS^j_t	493
6	−.0046 (.0033)	.9330 (.0562)	.6455 (.0795)	83.0216 (.0000)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ $(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	195
7	−.0004 (.0034)	.8091 (.0833)	.9308 (.1516)	55.2606 (.0000)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ $(\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	479
				B. Special-Charge Censoring			
1	−.0100 (.0014)	1.1797 (.0332)	. . .	64.6148 (.0000)	OLS	$1,\ {}_t\mathrm{EPS}^j_{n,t+1}$	491
2	−.0100 (.0035)	1.1797 (.0796)	. . .	9.6614 (.0080)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1}$	491
3	−.0114 (.0038)	1.0449 (.0884)	.1341 (.0914)	10.7822 (.0130)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ ${}_{t-1}\mathrm{EPS}^j_{n,t}$	188
4	−.0082 (.0013)	.8775 (.0468)	.2676 (.0313)	150.4435 (.0000)	OLS	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ EPS^j_t	483
5	−.0082 (.0036)	.8775 (.1056)	.2676 (.0825)	17.0831 (.0007)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ EPS^j_t	483
6	−.0048 (.0033)	.9136 (.0693)	.5489 (.1049)	37.8368 (.0000)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ $(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	188
7	−.0007 (.0034)	.8116 (.0878)	.8283 (.1637)	34.8176 (.0000)	GMM	$1,\ {}_t\mathrm{EPS}^j_{n,t+1},$ $(\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	469

Note.—Standard errors are in parentheses under coefficients. Significance levels are in parentheses under test statistics.

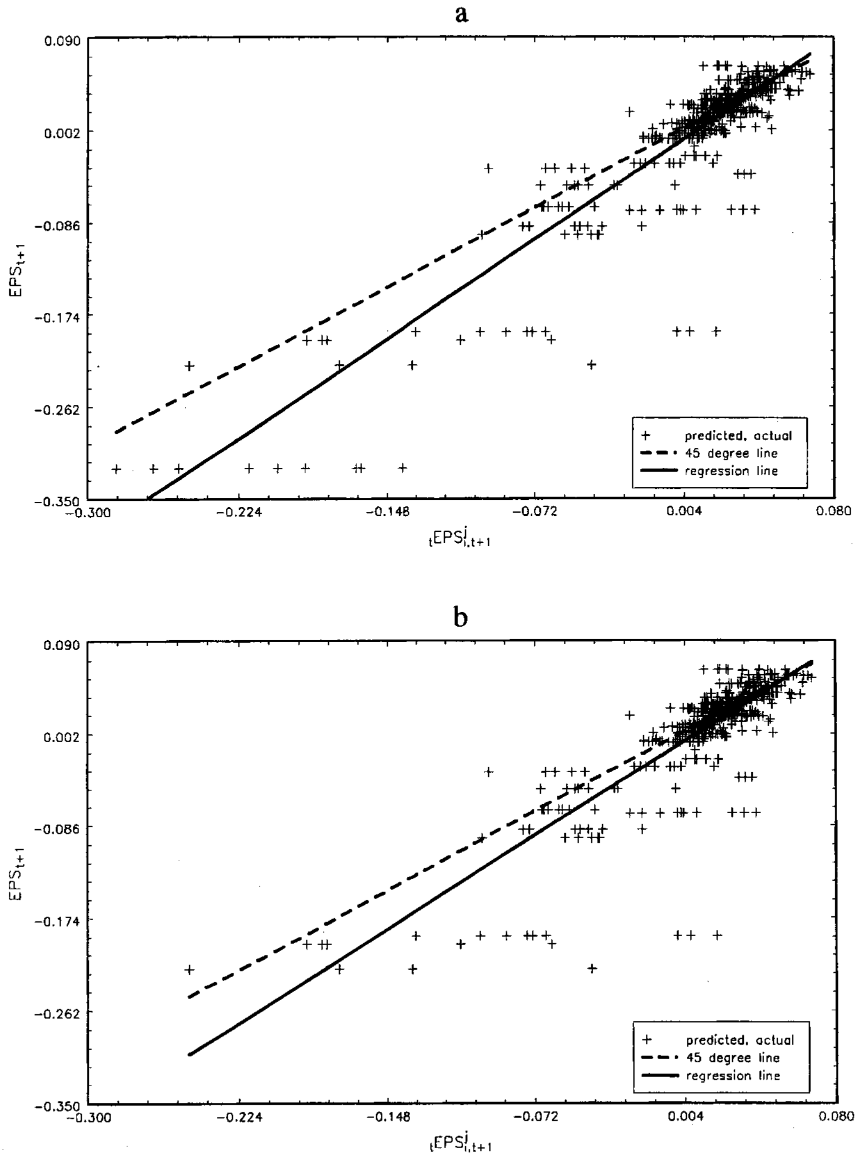


FIG. 7.—EPS forecasts and realizations (SIC 4512). *a*, All observations. *b*, Special-charge censoring.

Another potential criticism of our study is that our time series are short and that therefore our estimates are unreliable. Although we wish we had longer time series, the time series we have are much longer than are commonly used in panel data applications in the profession. They are certainly longer than those for other tests of analyst forecast rationality. And it is difficult for us to believe that analysts' forecasts are actually biased, but that we failed to reject forecast rationality for every single case in tables 1–5.

VI. Conclusion

The evidence in this paper strongly supports the view that professional stock market analysts make rational forecasts of earnings per share for the companies they follow. This result supports the view that current financial disclosures, in addition to other financial information gathered by analysts, provide intelligent users of financial statements with enough information to predict the current condition of firms with reasonable accuracy. It also suggests, contrary to popular opinion, that analysts do not systematically shade their forecasts; rather, their forecasts are unbiased. Our results also indicate that one will tend to falsely conclude that earnings forecasts are upward-biased if one fails to account for discretionary special charges. The seeming bias that occurs is simply a function of the conservative bias of accounting: that management can take large discretionary write-downs of assets, but assets cannot be written up.

We have also demonstrated the importance of careful data selection and statistical inference to our analysis. Future researchers should carefully consider how analyst forecast errors are correlated across analysts and firms. They should also consider whether discretionary write-downs and accruals will cause reported EPS to inaccurately measure what analysts were trying to predict.

Appendix A

Econometric Methods

We now propose a feasible generalized method-of-moments (GMM) estimator for equation (2) in the text. First, we must specify the structure of $\mathbf{\Omega}$, the covariance matrix of all the errors from the regression equation. Second, we must specify how to consistently estimate $\mathbf{\Omega}$ to arrive at a feasible GMM estimator.

As we discussed in the text, we assume that the covariance structure for the forecast errors, equations (3)–(6), is

$$E(\epsilon^{j}_{n,t+1}\epsilon^{j}_{n,t+1+s}) = \begin{cases} a, & s = 0 \\ 0, & s \neq 0, \end{cases}$$

$$E(\epsilon^{j}_{n,t+1}\epsilon^{j}_{m,t+1+s}) = \begin{cases} c, & s = 0,\ m,\ n \leq N,\ m \neq n \\ 0, & s \neq 0, \end{cases}$$

$$E(\epsilon^{j}_{n,t+1}\epsilon^{l}_{n,t+1+s}) = \begin{cases} b, & s = 0,\ j,\ l \leq J,\ j \neq l \\ 0, & s \neq 0, \end{cases}$$

$$E(\epsilon^{j}_{n,t+1}\epsilon^{l}_{m,t+1+s}) = \begin{cases} d, & s = 0,\ m,\ n \leq N,\ m \neq n,\ j,\ l \leq J,\ j \neq l \\ 0, & s \neq 0. \end{cases}$$

Suppose that we order our forecast observations as follows,

$${}_1\mathrm{EPS}^1_{1,1+k} \cdots {}_T\mathrm{EPS}^1_{1,T+1}\ {}_1\mathrm{EPS}^2_{1,1+k} \cdots {}_T\mathrm{EPS}^J_{1,T+1}\ {}_1\mathrm{EPS}^1_{2,1+k} \cdots {}_T\mathrm{EPS}^J_{N,T+1},$$

and order the observations for EPS and $X_{n,t}$ accordingly. Then $\mathbf{\Omega}$ will have the following structure:

$$\underset{JNT \times JNT}{\mathbf{\Omega}} = \begin{bmatrix} \mathbf{E} & \mathbf{F} & \cdots & \mathbf{F} \\ \mathbf{F} & \mathbf{E} & \cdots & \mathbf{F} \\ \vdots & \vdots & \ddots & \vdots \\ \mathbf{F} & \mathbf{F} & \cdots & \mathbf{E} \end{bmatrix},$$

when

$$\underset{JT \times JT}{\mathbf{E}} = \begin{bmatrix} \mathbf{A} & \mathbf{B} & \cdots & \mathbf{B} \\ \mathbf{B} & \mathbf{A} & \cdots & \mathbf{B} \\ \vdots & \vdots & \ddots & \vdots \\ \mathbf{B} & \mathbf{B} & \cdots & \mathbf{A} \end{bmatrix},$$

$$\underset{JT \times JT}{\mathbf{F}} = \begin{bmatrix} \mathbf{C} & \mathbf{D} & \cdots & \mathbf{D} \\ \mathbf{D} & \mathbf{C} & \cdots & \mathbf{D} \\ \vdots & \vdots & \ddots & \vdots \\ \mathbf{D} & \mathbf{D} & \cdots & \mathbf{C} \end{bmatrix},$$

and

$$\underset{T \times T}{\mathbf{A}} = a \cdot I_T, \quad \underset{T \times T}{\mathbf{C}} = c \cdot I_T,$$

$$\underset{T \times T}{\mathbf{B}} = b \cdot I_T, \quad \underset{T \times T}{\mathbf{D}} = d \cdot I_T.$$

We can consistently estimate the elements of **A**, **B**, **C**, and **D** as follows.

First, estimate equation (2) using OLS, which will give consistent estimates of parameters $\hat{b}_{\text{GMM}}$.[18] Use these estimates to construct an estimated residual vector. Then construct the elements of **A**, **B**, **C**, and **D**:

$$\hat{a} = \frac{1}{TJN}\sum_{t=1}^{T}\sum_{j=1}^{J}\sum_{n=1}^{N}\hat{\epsilon}^{j}_{n,\,t+1} \times \hat{\epsilon}^{j}_{n,\,t+1},$$

$$\hat{b} = \frac{1}{TJ(J-1)N}\sum_{t=1}^{T}\sum_{j=1}^{J}\sum_{\substack{l=1\\ l\neq j}}^{J}\sum_{n=1}^{N}\hat{\epsilon}^{j}_{n,\,t+1} \times \hat{\epsilon}^{l}_{n,\,t+1},$$

$$\hat{c} = \frac{1}{TJN(N-1)}\sum_{t=1}^{T}\sum_{j=1}^{J}\sum_{n=1}^{N}\sum_{\substack{m=1\\ m\neq n}}^{N}\hat{\epsilon}^{j}_{n,\,t+1} \times \hat{\epsilon}^{j}_{m,\,t+1},$$

$$\hat{d} = \frac{1}{TJ(J-1)N(N-1)}\sum_{t=1}^{T}\sum_{j=1}^{J}\sum_{\substack{l=1\\ l\neq j}}^{J}\sum_{n=1}^{N}\sum_{\substack{m=1\\ m\neq n}}^{N}\hat{\epsilon}^{j}_{n,\,t+1} \times \hat{\epsilon}^{l}_{m,\,t+1}.$$

Given the assumption we have made about the structure of the errors in equation (2), we can then construct a consistent estimate of the covariance matrix of $\hat{b}_{\text{GMM}}$, namely,

$$V(\hat{b}_{\text{GMM}}) = [\mathbf{X}'\mathbf{X}(\mathbf{X}'\hat{\mathbf{\Omega}}\mathbf{X})^{-1}\mathbf{X}'\mathbf{X}]^{-1}.$$

If some observations are missing, the estimates of **A**, **B**, **C**, and **D** can be constructed using all nonmissing observations on the residuals. Missing observations create no additional problems for inference.

Appendix B

Disclosures on Observations with Above-the-Line Special Items Eliminated in the Truncated Sample

Table B1 shows the industries we use.

SIC 2800

American Cyanamid 90:3 (1990 Annual Report)

"During 1990, the company provided, on a pre-tax basis, $291.9 [million] primarily for special costs associated with plans to curtail and consolidate certain product lines; to reduce the carrying value of certain assets to estimated realizable amounts, including investments in subsidiaries and affiliates; and for increased environmental remediation costs."

[18] The terms $\hat{b}_{\text{OLS}}$ and $\hat{b}_{\text{GMM}}$ are identical in this case because they use the same orthogonality conditions. This new estimator correctly specifies $V(\hat{b}_{\text{GMM}})$.

TABLE B1

INDUSTRIES EXAMINED IN THIS STUDY

SIC Code	Industry	Number of Firms	Number of Analysts
2800	Chemicals	5	49
3330	Smelters and refiners—nonferrous	3	34
3334	Smelters and refiners—aluminum	3	28
3711	Motor vehicles and car bodies	3	35
4011	Railroads, line-haul operating	3	29
4512	Air transportation, certified	4	37

Dow Chemical 85:4 (1985 Annual Report)

''The fourth quarter of 1985 included a special pretax charge of $471 [million] for asset-related writeoffs and writedowns and $121 [million] for personnel related costs.''

Olin 85:3 (1985 Annual Report)

''The total provision made to cover all costs of the restructuring was $330 million pre-tax, or $230 million after-tax. The reserve provides for permanently decommissioning certain chemical facilities, writing down facilities and assets impaired by changed worldwide economic conditions.''

Olin 91:1 (1991 Annual Report)

''The 1991 first-quarter loss includes a[n] $80 million special charge to cover losses on disposition and writedown of certain business assets and costs of personnel reductions.''

SIC 3330

ASARCO 84:4 (1984 Annual Report)

''The 1984 results included an unusual pre-tax charge of $254 million reflecting the closing or shutdown of certain facilities and the writedown in value of properties no longer considered economic in view of reduced price expectations.''

Phelps Dodge 84:4 (1984 Annual Report)

''In view of the exceedingly difficult conditions currently prevailing in the copper market, the company . . . is implementing a program to further restructure certain of its operations. As part of this program, the company recorded a $195 million non-recurring pre-tax charge in the fourth quarter of 1984, $110 million of which was charged against continuing operations.''

SIC 3334

Alcan Aluminum 85:4 (1985 Annual Report)

''Approximately one half of the charge of $416 [million] reflects the estimated long-term impairment in economic value of the company's bauxite and alumina operations arising from a large excess of production capacity in the world compared with existing and anticipated demand. The remainder of the special charges and rationalization expenses relates to a program to reduce levels of management and the total number of employees, to costs associated with the sale and restructuring of a number of small businesses, to the reduction in value of certain overseas investments, and to the write-down of certain raw materials.''

Reynolds Metals 85:3 (1985 Annual Report)

''Our company reported . . . a revised after-tax charge of $322 million for the writedown and other costs associated with various uneconomic assets.''

SIC 3711

Chrysler 89:3 (1989 Annual Report)

''In September 1989, Chrysler sold 75 million shares of its equity investment in Mitsubishi Motors Corporation (MMC) for approximately $598 [million]. . . . The sale resulted in a gain before taxes of $503 million.''

Chrysler 89:4 (1989 Annual Report)

''The results of operations for the year ended December 31, 1989 include a provision of $931 million for costs associated with a restructuring of Chrysler's automotive operations. The restructuring charge includes: the estimated costs of the discontinuation and curtailment of certain manufacturing operations and the elimination of certain product lines; the write-down of certain long-term assets; and the recognition of pension costs, unemployment benefits and other related costs for separated employees.''

Chrysler 91:1 (1991 Annual Report)

''The results of operations for the year ended December 31, 1991 included a non-cash, nonrecurring credit provision of $391 million which is the result of a reduction in the planned capacity adjustments related to facilities acquired by the company in connection with its purchase of AMC in 1987.''

General Motors 90:3 (1990 Annual Report)

''In 1990, a special restructuring charge of $3,314.0 million was included in the results of operations to provide for the closing of four previously idled U.S. assembly plants, as well as provide for other North American manufacturing and warehouse operations which will be consolidated or cease operating over the next three years.''

General Motors Annual Report 1991

''In 1991, a special restructuring charge of $2,820.8 million was included in the results of operations to provide for the idling of six North American assembly, four powertrain, and 11 component plants.''

SIC 4011

Burlington Northern 86:2 (1986 Annual Report)

''Our [1986] restructuring program was designed to adjust to the fundamental changes in our environment and to position the corporation to increase the utilization of its transportation, energy and real estate assets. We expect these actions to have a very positive effect on rates of return, cash flow and earnings in the years ahead.

''The principal items covered by the special charge of $1.7 billion before-tax include:

''A $600 million reserve to cover corporate-wide workforce reductions and costs associated with early retirements, severances, relocations, and elimination and consolidation of excess facilities.

''A $577 million writedown of some developed and non-producing oil and gas properties, reflecting their diminished value as a result of the rapid and unprecedented drop in energy prices. These properties represent a relatively small portion of our holdings and will not have a significant effect on our extensive hydrocarbon reserves.

''A $305 million writedown of Champlin's Corpus Christi refinery and its related marketing and distribution system in anticipation of completing our joint-venture agreement with Peteroleos de Venezuela, S. A. We are optimistic that the venture, which represents a good business opportunity for both parties, will be finalized in the near future. This transaction will free up cash and position the business to be a more consistent income and cash contributor.

''A $261 million writedown to cover excess rail equipment, probable future losses in a petrochemical venture and certain other items.''

Burlington Northern 91:2 (1991 Annual Report)

''Included in 1991 results is a pre-tax special charge of $708 million related to railroad restructuring costs and increases in liabilities for casualty claims and environmental clean-up costs. The special charge is comprised of the following components:

''Restructuring—This program provides for workforce reduction of employees. The restructuring program and related charge has two components:

''$40 million to provide for employee related costs for a separation program.

''$185 million to provide for employee related costs for the elimination of surplus crew positions.

''Other—$350 million to increase casualty reserves based on an actuarial valuation and escalations in both the cost and number of projected hearing loss claims.

''$133 million to increase environmental reserves based on recently completed studies and analysis of potential environmental clean-up and restoration costs.''

Union Pacific 86:2 (1986 Annual Report)

''In June 1986, the corporation announced a major restructuring program, which included a special charge against second quarter results. The special charge, which amounted to $1.7 billion, recognized the diminished value of certain assets and covered costs associated with reductions in employee levels throughout the corporation.''

SIC 4512

USAir 90:4 (1990 Annual Report)

''Results for 1990 include special charges aggregating approximately $138 [million].''

USAir 91:4 (1991 Annual Report)

''Operating expenses for 1991 included a one-time gain of $107 million related to freezing of the fully funded non-contract employee pension plan.''

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