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The Association Between Unsystematic Security Returns and the Magnitude of Earnings Forecast Errors

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Previous research has examined the association between the sign of the earnings forecast errors and unsystematic security returns. Here, we present preliminary findings regarding this association where the magnitude of the forecast error is also considered. The motivation for this extension is the possibility that treatment of the forecast error as dichotomous may potentially ignore ordinal properties of the data. This was not a particularly serious problem for early studies such as Ball and Brown [1968], where the prime concern was to examine the null hypothesis of no association. Their ability to reject the null hypothesis by considering only the sign of the forecast error leads us to expect that incorporating the magnitude would tend to strengthen their findings.

However, the Ball and Brown approach has been extended to examine more subtle hypotheses, such as relative association among alternative forecast errors (Beaver and Dukes [1972; 1973]) or marginal effects of multiple signals (Brown and Kennelly [1972], Gonedes [1978], Griffin [1976], and Foster [1975]). As the research questions asked become more refined, treatment of the earnings forecast error may become critical.

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This is our motivation for extending one aspect of the Ball and Brown study. We examine the hypothesis that a positive ordinal association exists between unsystematic returns and the magnitude of earnings forecast errors.

Capital market equilibrium can be characterized as a mapping from states into a set of security prices. Similarly, earnings are signals from an information system which is a mapping from states into signals. In general, there could be any relationship between prices and earnings depending upon the nature of the two mappings. If one assumes that prices and earnings reflect a common set of events, it is not unreasonable to assume that the two might be associated. Previous research provides some limited and indirect evidence of a positive association.

The Ball and Brown [1968] study indicates that positive earnings forecast errors are associated with positive unsystematic returns and conversely for negative earnings forecast errors. However, since Ball and Brown ignored the magnitude and examined only the sign of earnings forecast errors, their findings are consistent with unsystematic returns varying with the sign, but they provide no insight into whether unsystematic returns vary with the magnitude of the forecast error. Magee [1975] examined the significance of the regression coefficient from a regression of unsystematic returns on forecast errors and found the coefficient was significant. However, unsystematic returns differing by only the sign of the forecast error would be sufficient to produce a significant coefficient. The Magee study provided no evidence regarding alternative specifications (e.g., dummy variables reflecting only the sign of the forecast errors) or a scatter diagram of the underlying relationship. Thus, Magee's results do not offer any additional insight beyond that provided by Ball and Brown. More recently, Joy, Litzenberger, and McEnally [1977, hereafter JLM] examined unsystematic returns associated with three broad classes of forecast errors. However, their study differs from ours in major respects. JLM were primarily concerned with the unsystematic returns in a twenty-six-week postannouncement period for quarterly earnings. We will be exclusively concerned with the fifty-two-week preannouncement period for annual earnings. Their study considered only three broad forecast error groupings which are overlapping. Results for nonoverlapping groupings were not reported nor were significance tests conducted on the preannouncement unsystematic returns. We report findings based upon twenty-five nonoverlapping groups (hereafter portfolios). Moreover, we conduct significance tests for hypotheses regarding an ordinal relationship.

Our comments are not offered as criticisms of these other studies, because their primary concern was not the magnitude of the forecast error. These studies provide useful initial insights into the relationship, but the issue can be examined more completely and rigorously.

Our paper consists of three sections. The first will deal with the research design, and the second provides a discussion of the results. In the third section, we examine potential applications of the approach taken here.

1. Research Design

We first discuss the security return metric and then turn to the earnings forecast error. In the interim, it will be sufficient to view the forecast error as some function of past and current earnings. Some familarity with the Ball and Brown study and the literature that followed will be assumed here (for a review of this literature, see Gonedes and Dopuch [1974]).

1.1 UNSYSTEMATIC SECURITY RETURNS

Unsystematic security returns are defined here as "residuals" from a market model of the following form:

$$\tilde{R}_{it} = \alpha_i + \beta_i \tilde{R}_{mt} + \tilde{\epsilon}_{it} \tag{1}$$

where

$\tilde{R}_{it}$ = rate of return (percentage change in price including dividends) for security $i\,(i = 1, \cdots, N)$ in period t;
$\tilde{R}_{mt}$ = rate of return in period t on a "market" portfolio of NYSE common stocks, where each stock's return is weighted according to its relative market value (computed by the *CRSP* service provided by the University of Chicago);
$\tilde{\epsilon}_{it}$ = the unsystematic return on security i in period t;
α_i, β_i = the intercept and slope coefficients specific to security i.

Beja [1972] and Fama [1973] have shown that without restrictions on $\tilde{R}_{it}$ or $\tilde{R}_{mt}$, it is possible to construct an (α_i, β_i) pair such that:[1]

$$E(\tilde{\epsilon}_{it}) \equiv 0$$

$$\sigma(\tilde{R}_{it}, \tilde{\epsilon}_{it}) \equiv 0$$

where the symbol, $\equiv$, denotes the relationship holds by construction. Moreover, if the object is to minimize $\sigma^2(\tilde{\epsilon}_{it})$, α_i and β_i would be set equal to:

$$\beta_1 = \frac{\sigma(\tilde{R}_{it}, \tilde{R}_{mt})}{\sigma^2(\tilde{R}_{mt})}$$

$$\alpha_i = E(\tilde{R}_{it}) - \beta_i E(\tilde{R}_{mt}).$$

The $\tilde{\epsilon}_{it}$ term is labeled the unsystematic portion of the return of security i because $\sigma(\tilde{R}_{mt}, \tilde{\epsilon}_{it}) \equiv 0$.

No assumption has been made thus far about the stochastic process generating $(\tilde{R}_{it}, \tilde{R}_{mt})$. If $\tilde{R}_{it}$ and $\tilde{R}_{mt}$ are assumed to be bivariate normal, any dependency will be linear such that:

[1] Obviously $E(\tilde{\epsilon}_{it})$ and $\sigma(\tilde{R}_{mt}, \tilde{\epsilon}_{it})$ must exist for such statements to be meaningful.

$$E(\tilde{R}_{it} \mid R_{mt}) = \alpha_i + \beta_i R_{mt}$$

and

$$E(\tilde{\epsilon}_{it} \mid R_{mt}) = 0.$$

1.2 MOTIVATION FOR UNSYSTEMATIC SECURITY RETURNS

The characterization in equation (1) is used to transform $\tilde{R}_{it}$ such that the marginal distribution of the transformed variable $\tilde{\epsilon}_{it}$ has an expected value of zero. This then is assumed to hold for all securities in the sample and becomes a basis for testing against a null hypothesis of a mean of zero. More precisely, previous studies have been concerned with the null and alternative hypotheses:

$$H_0\colon E(\tilde{R}_{it} \mid \text{forecast error}) = E(\tilde{R}_{it})$$

$$H_a\colon E(\tilde{R}_{it} \mid \text{forecast error}) \neq E(\tilde{R}_{it}).$$

Obviously, the relationships stated in the null and alternative hypotheses are not directly observable. R_{it} is observable but constitutes only one realization from the distribution. One possibility is to obtain several drawings for a single firm over time. However, this still results in a relatively few number of annual observations (e.g., twenty years in the case of *Compustat* firms). Ball and Brown and the subsequent literature pooled data cross-sectionally, as well as over time, in order to obtain a larger number of observations. However, this requires that the security return be transformed in such a manner to permit comparison over time and across firms.

It is convenient if some transformation can be applied to $\tilde{R}_{it}$ such that the expected value of the marginal distribution is the same for all firms. Previous work has assumed that equation (1) will transform $\tilde{R}_{it}$ into $\tilde{\epsilon}_{it}$ such that a common expected value (of zero) holds on the marginal distribution. The null and alternative hypotheses then become:

$$H_0\colon E(\tilde{\epsilon}_{it} \mid \text{forecast error}) = E(\tilde{\epsilon}_{it}) = 0$$

$$H_a\colon E(\tilde{\epsilon}_{it} \mid \text{forecast error}) \neq E(\tilde{\epsilon}_{it}).$$

Note that such an adjustment does not ensure equality of any of the other moments of the $\tilde{\epsilon}_{it}$ distribution.

1.3 CROSS-SECTIONAL CORRELATION AMONG SECURITY RETURNS

In the presence of nonzero cross-sectional correlation (i.e., $\sigma(\tilde{\epsilon}_{it}, \tilde{\epsilon}_{jt}) \neq 0$, $i \neq j$), tests of significance which assume independence will incorrectly state the standard error of the test statistic and will be biased in a manner that depends upon the nature of the correlation. $\sigma(\tilde{\epsilon}_{it}, \tilde{\epsilon}_{jt})$ would not be expected to be zero for a number of reasons. (1) Even if the security return process were assumed to be a single factor (i.e., market model) process, there would be linear dependence among $\tilde{\epsilon}_{it}$'s of the following form, $\sum_{i=1}^{N} x_{it}\tilde{\epsilon}_{it} = 0$.[2] Typically, however, the sample consists of less than all firms comprising the market portfolio, so this source of dependency is not of obvious concern. (2) Even among samples that are subsets of the market, misspecification of the process generating returns could induce cross-sectional correlation. Two types of processes are offered as illustrations: a nonlinear relationship between $\tilde{R}_{it}$ and $\tilde{R}_{mt}$ or a multifactor process (e.g., a two-factor return-generating process).[3] Existence of bivariate normality would preclude concern about nonlinear relationships. Industry effects models would be another example of multifactor processes. Section 2.3 will describe how we cope with the nonindependence issue.

Our study will adopt unsystematic return as the security return metric (see equation (1)). Our motivation is to provide an extension of the Ball and Brown study, thus we use a security return metric similar to theirs. However, Ball's [1972] use of residuals from a cross-sectional regression and Gonedes's [1975] use of differences in returns offer two obvious alternatives. Thus far, no assumption has been made regarding the nature of the equilibrium expected returns; the analysis has been conducted in a single-period context and has treated α_i and β_i as "known" parameters. When an empirical assessment of $\tilde{\epsilon}_{it}$ is introduced, additional specifications must be made.

1.4 EMPIRICAL ASSESSMENT OF UNSYSTEMATIC RETURN

The unsystematic return was assessed from a time series of regression of R_{it} (monthly security returns) on R_{mt} (monthly return on a market portfolio) of the following form:

$$R_{it} = \hat{\alpha}_i + \hat{\beta}_i R_{mt} + \hat{\epsilon}_{it} \qquad t = 1, \cdots, T$$

where

$\hat{\alpha}_i, \hat{\beta}_i$ = estimates of the intercept and slope obtained from the linear regression;

$\hat{\epsilon}_{it}$ = an estimate of unsystematic return.

The model assumes the standard *OLS* conditions:

α_i and β_i are constants over time;

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[2] This dependency exists because $\tilde{R}_{mt} = \sum_{i=1}^{N} x_{it}\tilde{R}_{it}$ where x_i is the weight assigned to security i's return in generating the market return. See Beja [1972] and Fama [1973] for further discussion.

[3] Examples include:

$$\tilde{R}_{it} = \alpha_i + \beta_i \tilde{R}_{mt} + \gamma_i \tilde{R}_{mt}^2 + \tilde{\epsilon}_{it}$$

and

$$\tilde{R}_{it} = \gamma_i \tilde{R}_{zt} + \beta_i \tilde{R}_{mt} + \tilde{\epsilon}_{it},$$

(where $\tilde{R}_{zt}$ is the return on the minimum variance zero beta portfolio), respectively.

$$E(\tilde{\epsilon}_{it}) = 0$$

$$\sigma(\tilde{R}_{mt}, \tilde{\epsilon}_{it}) = 0$$

$$\sigma(\tilde{\epsilon}_{it}, \tilde{\epsilon}_{it'}) = 0 \text{ for } t \neq t'$$

$$\sigma^2 (\tilde{\epsilon}_{it}) = \sigma_i^2$$

Note that $\tilde{R}_{it}$ could have a nonconstant mean or variance over time if $E(\tilde{R}_{mt})$ or $\sigma^2(\tilde{R}_{mt})$ varies over time. In this context, residual analysis attempts to produce a transformation of $\tilde{R}_{it}$ (i.e., $\tilde{\epsilon}_{it}$) that has constant mean (equal to zero) and constant variance over time. Note no assumption is made regarding $\sigma(\tilde{\epsilon}_{it}, \tilde{\epsilon}_{jt})$ or how $\sigma^2(\tilde{\epsilon}_{it})$ may vary across firms.

A time-series regression is used in order to obtain estimates of α_i and β_i. This naturally raises the issue of how security return realizations are to be viewed conceptually. Presumably, they are realizations from some form of equilibrium process. The α_i and β_i would then refer to the "market's" assessment of $E(\tilde{R}_{it}) - \beta_i E(\tilde{R}_{mt})$ and $\dfrac{\sigma(\tilde{R}_i, \tilde{R}_{mt})}{\sigma^2(\tilde{R}_{mt})}$, respectively. If we view the equilibrium process as one where heterogeneous beliefs prevail, then the "market's" assessment constitutes some form of averaging or aggregation across investors (see Lintner [1969]). The assumption of constancy of α_i and β_i over the regression time period implies that $E(\tilde{R}_{it}) - \beta_i E(\tilde{R}_{mt})$ and $\dfrac{\sigma(\tilde{R}_{it}, \tilde{R}_{mt})}{\sigma^2(\tilde{R}_{mt})}$ are constant over time for the ith firm. Clearly, constancy of $E(\tilde{R}_{it})$, $E(\tilde{R}_{mt})$, $\sigma(\tilde{R}_{it}, \tilde{R}_{mt})$, and $\sigma^2(\tilde{R}_{mt})$ would be sufficient but not necessary.[4]

The joint observations $(\tilde{R}_{it}, \tilde{R}_{mt})$ are assumed to be random drawings from the underlying population such that $E(\tilde{\epsilon}_{it}) = 0$ for $t = 1, 2, \ldots, T$. In some contexts, there may be reason to believe that $E(\tilde{\epsilon}_{it}) \neq 0$ because of self-selection by the firms (e.g., firms who split their stocks or firms who change accounting methods) or selection by the researcher. However, given current annual reporting practices, there is no reason to expect that the assumption is violated here. Previous research by Ball and Brown [1968], Fama et al. [1969], and Beaver [1968], among others, indicates that the $\tilde{\epsilon}_{it}$'s are essentially uncorrelated over time. In fact, we will exploit the property of zero serial correlation for the test statistics we develop later. The assumption, $\sigma(\tilde{R}_{mt}, \tilde{\epsilon}_{it}) = 0$, holds by construction, as indicated earlier, and hence is not of concern.

The assumption of constant variance for $\tilde{\epsilon}_{it}$ is probably not literally correct. Beaver [1968], among others, has shown that the variance of

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[4] Little else can be said unless a more specific form of equilibrium is assumed. For example, assume that the riskless security form of the *CAPM* would produce constancy of α_i and β_i, if R_{ft}, $\sigma(\tilde{R}_{it}, \tilde{R}_{mt})$ and $\sigma^2(\tilde{R}_{mt})$ were constant over time. If a "single-period" equilibrium (e.g., *CAPM*) is assumed, further assumptions must be introduced to ensure that the multiperiod process can be characterized as if it were a sequence of single-period equilibria (Fama and Miller [1972]).

unsystematic returns is larger in the week of the annual earnings announcements than at other times in the year. However, since we cumulate the unsystematic returns over a twelve-month period, it is not obvious that heteroskedasticity is present for annual unsystematic returns. The estimates of α_i, β_i, and ϵ_{it} under *OLS* procedures may be less efficient than Generalized Least Squares, but they remain unbiased.[5]

The $\hat{\epsilon}_{it}$ were computed in the following way.[6] Model (1) is estimated using an *OLS* regression model based on monthly security and market returns for the sixty-month period prior to the twelve-month interval of the assumed public disclosure of the earnings signal. Public dissemination of the earnings signal is assumed to occur during the twelve-month interval ending the third month after the end of the fiscal year (e.g., March 1966 for the fiscal year ending December 31, 1965). The estimated parameters $\hat{\alpha}_i$ and $\hat{\beta}_i$ (based on months -71 through -12) are then used to estimate monthly unsystematic returns for a twelve-month period ($t = -11, -10, \cdots, -1, 0$) where the third month of the subsequent fiscal year is month 0.[7] The monthly unsystematic returns are then cumulated for security i from month -11 through month 0.[8] The source of the data is the *CRSP* tape provided by the University of Chicago.

1.5 MOTIVATION FOR THE FORECAST ERRORS

The earnings forecast error is a transformation of past and current earnings per share (hereafter *EPS*). There are at least two interpretations of the forecast error. The first is that it constitutes a signal from an information system. The purpose of the empirical analysis is to assess whether the distribution of security returns conditional upon the signal realization (forecast error) differs from the unconditional (or marginal) distribution. The reason a dependency exists appears to rest upon the simple premise that prices and earnings both are the result of mappings from a common underlying set of events (Gonedes [1975]).

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[5] The relative efficiency of *OLS* versus *GLS* will, in part, depend upon the ability to estimate the variance-covariance matrix of the residuals. Conventional demonstrations of the "superiority" of *GLS* typically assume the variance-covariance matrix is "known."

[6] $\hat{\epsilon}_i$ bears the following relationship to $\tilde{\epsilon}_{it}$:

$$\hat{\epsilon}_{it} = \tilde{\epsilon}_{it} - (\hat{\alpha}_i - \alpha_i) - (\hat{\beta}_i - \beta_i)R_{mt}.$$

Note that $\hat{\alpha}_i$, $\hat{\beta}_i$, and $\hat{\epsilon}_{it}$ are all random variables, although, for ease of notation, no tilde has been used for these variables. Later $\tilde{e}_{it}$, the forecast error, will be denoted as simply e_{it}.

[7] The $\hat{\epsilon}_{it}$ will be heteroskedastic, even if $\sigma^2(\tilde{\epsilon}_{it}) = \sigma_i^2$, because $\hat{\epsilon}_{it}$ is estimated outside of the regression time period, where:

$$\sigma^2(\hat{\epsilon}_{it}) = \sigma_i^2\left[1 + \frac{1}{60} + \frac{(R_{mt} - \bar{R}_m)}{\sum_{s=1}^{60} (R_{ms} - \bar{R}_m)^2}\right].$$

Throughout the remainder of the analysis, we will act as if this source of heteroskedasticity is "immaterial."

[8] The annual unsystematic return for fiscal year t shall be defined as $\sum_{s=-11}^{0} \hat{\epsilon}_{is} = \hat{\epsilon}_{it}$. Note hereafter the subscript t will denote year t, rather than month.

A second, more ambitious interpretation is that the earnings forecast error measures unexpected earnings with error (Ball and Brown [1968]). The source of the error stems from the notion that market participants base their earnings expectations on more information than is reflected in the simple earnings forecast model typically assumed. As a result, expectations conditional on this additional information may differ from those conditioned on past and current earnings signals. Hence, the forecast error can be viewed as a measurement of unexpected earnings with error. Unresolved issues include: (1) How are unexpected earnings related to changes in expectations regarding future earnings? (2) How do expectations regarding future earnings relate to the current equilibrium security price?

It will be sufficient for our purposes to adopt the more modest information system interpretation of the forecast error. It is important to note that neither interpretation provides a rigorous, theoretical basis for a positive association between unsystematic returns and the magnitude of the forecast errors.

The purpose of the earnings forecast error, $\tilde{e}_{it}$, is to facilitate a specification of the relationship between *EPS* changes and security price changes. For example, in the Ball and Brown study, the forecast error was used as a means of transforming *EPS* such that $E(\tilde{\epsilon}_{it} \mid e_{it} > 0) > 0$ and $E(\tilde{\epsilon}_{it} \mid e_{it} < 0) < 0$. Our hypothesis regarding forecast errors differs from that of Ball and Brown.

The null and alternative hypotheses here are:

$$H_0\colon \rho_s(\tilde{\epsilon}_{it}, \tilde{e}_{it}) = 0$$

$$H_a\colon \rho_s(\tilde{\epsilon}_{it}, \tilde{e}_{it}) > 0$$

where ρ_s is the population (Spearman) rank correlation. Empirically, this will involve examining cross-sectional rank correlations (r_s) between $\hat{\epsilon}_{it}$ and e_{it}, where e_{it} is the observed earnings forecast error. Our motivation for the forecast error transformation is to permit comparisons of the magnitude of the forecast errors across firms and over time. Since e_{it} is a function of level of *EPS*, it is "scaled" in two ways to be described later, resulting in e_{sit} and e_{pit}. While we hope this will facilitate comparability of forecast errors, it remains essentially an ad hoc procedure. Obviously, the extent of rank correlation observed may be dependent upon the scaling method used. We provide the results on two scaling methods to provide evidence of the sensitivity of the correlation to the choice of a scaling procedure.

1.6 EARNINGS FORECAST MODELS

Two *EPS* forecast models will be employed here. We define the following terms:

$EPS_{it} \equiv$ the observed earnings per share for firm i in fiscal year t (with all adjustments being made for stock splits and/or stock dividends).

$\Delta EPS_{it} \equiv EPS_{it} - EPS_{i,t-1}$ (the first difference in observed EPS).

$e_{it} \equiv \Delta EPS_{it} - f(\Delta EPS_{it})$ where the $f(\Delta EPS_{it})$ will be derived from two forecast models (hereafter, model A and B).

$f(EPS_{it}) \equiv$ a forecast of earnings per share for firm i in fiscal year t.

$f(\Delta EPS_{it}) \equiv$ a forecast of first differences in earnings per share for firm i in fiscal year t.

$EPS_{mt} \equiv$ average EPS for year t using an equally weighted average of the EPS's of all *Compustat* firms with a December 31 fiscal year-end (i.e.,

$$EPS_{mt} = \frac{1}{N}\sum_{i=1}^{N} EPS_{it}).$$

$\Delta EPS_{mt} \equiv EPS_{mt} - EPS_{mt-1}$ (the first difference in the observed average of EPS).

The sample firms were drawn from the *Compustat* data file as follows. A ten-year forecast period was specified to be 1965–74. Firms were required to have complete annual EPS data (data item #58 which is primary EPS excluding extraordinary items) over the period 1956–74 to provide enough initial observations for model estimation purposes. The firms also had to be listed on the NYSE and have fiscal years ending on December 31 for the entire time period. This first screening yielded 365 firms.

As indicated earlier, security return data were obtained from the *CRSP* data file. The requirement was a complete history of security returns over the period April 1960 to March 1975. Of the original 365 firms, 276 passed this second screening procedure, yielding a data sample which reflects the usual firm "survivorship" characteristic.

Two earnings forecast models are used. The first model is consistent with the stochastic process of a Martingale with drift, that is:

Model A

$$f(EPS_{it}) = EPS_{i,t-1} + \frac{1}{K}\sum_{k=1}^{K} \Delta EPS_{i,t-k}$$

and

$$f(\Delta EPS_{it}) = \frac{1}{K}\sum_{k=1}^{K} \Delta EPS_{i,t-k}.$$

Therefore, forecast errors, conditional on model A, are:

$$e_{it}^{A} = \Delta EPS_{it} - f(\Delta EPS_{it})$$

$$= \Delta EPS_{it} - \frac{1}{K}\sum_{k=1}^{K} \Delta EPS_{i,t-k}$$

where K is the number of years of changes in EPS data available as of fiscal year $t\,(t = 1965, 1966, \cdots, 1974)$. For example, for the year 1965, K

= 9 (years 1956–64), and K increases by one each year until $K = 18$ for the 1974 estimate of ΔEPS (years 1956–73).

Analogous to model (1) for security returns, the second earnings model postulates that ΔEPS_{it} is a linear function of a market index (ΔEPS_{mt}), that is:

Model B

$$f(EPS_{it}) = EPS_{i,t-1} + \hat{\gamma}_{1i,t-1} + \hat{\gamma}_{2i,t-1}\Delta EPS_{mt}$$

and

$$f(\Delta EPS_{it}) = \hat{\gamma}_{1i,t-1} + \hat{\gamma}_{2i,t-1}\Delta EPS_{mt}.$$

Forecast errors for ΔEPS_{it}, conditional on model B, are:

$$e_{it}^{B} = \Delta EPS_{it} - \hat{\gamma}_{1i,t-1} - \hat{\gamma}_{2i,t-1}\Delta EPS_{mt}.$$

The estimates $\hat{\gamma}_{1i,t-1}$ and $\hat{\gamma}_{2i,t-1}$ were obtained from an *OLS* regression of ΔEPS_{it} on ΔEPS_{mt} for all years of available data through year $t - 1$. The estimates were therefore updated each period by adding an additional observation.[9]

Using each computation of e_{it}, conditional on model A or B, we will discuss two error specifications. They are:

a. Standardized forecast error

$$e_{sit} = \frac{e_{it}}{\hat{\sigma}(e_{it})} = \frac{\Delta EPS_{it} - f(\Delta EPS_{it})}{\hat{\sigma}[\Delta EPS - f(\Delta EPS_{it})]}$$

where, here:

$$\hat{\sigma}^2(e_{it}) = \frac{1}{9}\sum_{t=1}^{10}[e_{it} - \bar{e}_i]^2 \quad \text{and} \quad \bar{e}_i = \frac{1}{10}\sum_{t=1}^{10} e_{it}$$

and

b. Percentage forecast error

$$e_{pit} = \frac{e_{it}}{f(EPS_{it})} = \frac{\Delta EPS_{it} - f(\Delta EPS_{it})}{f(EPS_{it})}$$

The forecast errors are computed for each firm for the years 1965 through 1974. When $f(EPS_{it}) \leq 0$, the observation was omitted from subsequent analysis of percentage forecast errors. Percentage forecast errors with $f(EPS_{it}) \leq \$.20$ were also omitted to avoid the "small denominator" problem, that is, $e_{pit} \to \infty$ as $f(EPS_{it}) \to 0$.[10]

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[9] In estimating coefficients for both $\hat{\epsilon}_{it}$ and e_{it}, we used no data that were not available to market participants at the time (i.e., no future data were used to estimate α_i, β_i, γ_{1i}, γ_{2i}). This may be needlessly inefficient, but we follow the path adopted by previous research.

[10] The results for the *standardized* forecast errors on the reduced set of observations were essentially identical to those for the *standardized* errors on the full set of observations, which will be reported here.

Previous research indicates that the models used here compare favorably with alternative forecasting models. Ball and Watts [1972], and more recently Albrecht, Lookabill, and McKeown [1977] and Watts and Leftwich [1977], provide evidence that model A does as well if not better than a broader class of forecasting models in one-step-ahead forecasts of earnings. Moreover, Beaver [1974] indicates that the forecast errors for models A and B are essentially uncorrelated over time. Unsystematic returns would be expected to reflect only the unanticipated (e.g., serially uncorrelated) portion of the forecast error. Forecast errors with significant serial correlation would tend to produce a lower association with unsystematic returns, relative to that attainable via further transformations of the forecast errors. There has been no attempt here to exploit any serial correlation, and the observed association may understate the association between unsystematic returns and earnings forecast errors which exploit such information.

There is another source of potential understatement arising from the assumption that the earnings report was announced by the end of the third month after the end of the fiscal year. This appears to be a reasonable assumption given the earlier work of Ball and Brown [1968] and Beaver [1968]. However, to the extent that earnings were announced

TABLE 1

Frequency Distribution Characteristics for Forecast Errors

			Percentage Forecast Errors*								
			Deciles								
	Mean	Std. Dev.	.10	.20	.30	.40	.50	.60	.70	.80	.90
Model A											
e_{it}	.072	.743	−.33	−.16	−.05	.01	.05	.10	.15	.25	.47
$\|e_{it}\|$	.313	.678	.02	.05	.08	.11	.15	.20	.28	.38	.63
e_{it}^2	.558	7.478	.00	.00	.01	.01	.02	.04	.08	.14	.40
Model B											
e_{it}	.007	.697	−.35	−.17	−.09	−.02	.02	.06	.10	.18	.34
$\|e_{it}\|$	.276	.640	.02	.04	.07	.09	.13	.17	.23	.34	.55
e_{it}^2	.486	7.157	.00	.00	.00	.01	.02	.03	.06	.11	.31

			Standardized Forecast Errors**								
			Deciles								
	Mean	Std. Dev.	.10	.20	.30	.40	.50	.60	.70	.80	.90
Model A											
e_{it}	.243	1.019	−1.08	−.54	−.17	.05	.22	.40	.64	.95	1.52
$\|e_{it}\|$	.795	.681	.10	.20	.32	.44	.60	.76	.98	1.31	1.76
e_{it}^2	1.100	1.734	.01	.04	.10	.20	.36	.58	.97	1.72	3.09
Model B											
e_{it}	.123	1.050	−1.23	−.64	−.31	−.08	.10	.28	.51	.84	1.40
$\|e_{it}\|$	.783	.706	.09	.19	.29	.40	.55	.73	.98	1.29	1.79
e_{it}^2	1.112	1.840	.01	.03	.08	.16	.31	.53	.96	1.67	3.22

* Number of observations: model A = 2,652, model B = 2,667.
** 2,760 observations.

after this date, the security return metric will fail to capture a portion of the information. The result will be an understatement of the association relative to that expected for a security return metric that included returns through the announcement date.

2. *Results*

2.1 STATISTICAL PROPERTIES OF THE UNSYSTEMATIC RETURN MODEL AND THE FORECAST ERRORS

Table 1 presents some summary descriptive statistics concerning the forecast error distributions for models A and B under each scaling method. The Spearman rank correlation (r_s) is .75 for the percentage forecast errors for models A and B, which is based upon a pooled ranking across firms and over time. No attempt is made here to conduct model evaluations or comparisons. In fact, we caution againt any direct comparison of models A and B under either version because the forecast errors are scaled by a denominator that is model dependent.

Table 2 reports some summary descriptive statistics for the security return regressions. The R_i^2 distribution is similar to that reported in previous studies. Note, however, that the mean (median) $\hat{\beta}_i$ is 1.203 (1.150), which results from using a value-weighted market index. Had an equally weighted market index been used (as was done in most previous research), the mean beta would have been .950 (see table 2), which is similar to the results obtained in previous studies.

2.2 RELATIONSHIP BETWEEN UNSYSTEMATIC RETURNS AND FORECAST ERRORS

For convenience, the relationship between unsystematic returns and forecast error has been summarized by forming twenty-five portfolios based on the relative magnitude of the forecast error. The mean unsystematic portfolio return ($\hat{\epsilon}_p$), the mean forecast error (e_p), and mean beta ($\hat{\beta}_p$) (as well as the median forecast error) are reported in tables 3 through 6.[11]

For each of the four methods of defining forecast errors, there appears to be a positive relationship between $\hat{\epsilon}_p$ and e_p. For example, for model A, the value of $\hat{\epsilon}_p$ ranges from -17.51 percent for portfolio 1 to $+29.16$ percent for portfolio 25. Moreover, the $\hat{\epsilon}_p$ increases in a near monotonic

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[11] The mean unsystematic portfolio return ($\hat{\epsilon}_p$) is defined as $\frac{1}{N}\sum_{i=1}^{N}\sum_{t=-11}^{0}\hat{\epsilon}_{it}$, where N is the number of securities in each portfolio. This metric differs slightly from the *API* of Ball and Brown, who used a multiplicative form. Previous work by Beaver [1974] indicated that this is an immaterial difference. We used this form because at the outset we wish to leave open the possibility of using parametric statistical tests, which require normality of the return metric. It was thought that the additive form would better conform to the normality assumption. After the fact, only nonparametric measures of association were used. However, it did not seem worthwhile to reconstruct the Ball and Brown metric, given the results of Beaver [1974].

TABLE 2

*Descriptive Statistics for Security Returns Regression Models**

	Mean	Std. Dev.	Deciles .25	.50	.75
$\hat{\beta}_i$	1.203	.419	.905	1.150	1.447
R_i^2	.329	.120	.248	.329	.409
$\hat{\sigma}(\tilde{\epsilon}_i)$	.069	.022	.053	.065	.080

* The value-weighted market index available from a 1976 version of the *CRSP* (Center for Research in Security Prices, University of Chicago) was used. For comparison with previous studies, if the equally weighted version from the same tape had been used, the results would have been as follows: the mean (std. dev.) of $\hat{\beta}_i$, $R^2{}_i$, and $\hat{\sigma}(\tilde{\epsilon}_i)$ distributions would have been .950 (.367), .338 (.129), and .0678 (.020), respectively.

TABLE 3

Unsystematic Security Returns and Forecast Errors: Model A (Percentage Forecast Errors)

Portfolio	No. of Observations	Mean $\hat{\epsilon}_p$	Mean e_p	Median Forecast Error	Mean $\hat{\beta}_p$
1	107	−0.1751	−1.5478	−0.9150	1.4766
2	107	−0.1240	−0.4469	−0.4420	1.3491
3	106	−0.1469	−0.3123	−0.3130	1.2791
4	106	−0.1176	−0.2292	−0.2280	1.2112
5	106	−0.1133	−0.1747	−0.1710	1.1523
6	106	−0.0903	−0.1273	−0.1280	1.1744
7	106	−0.0438	−0.0874	−0.0840	1.1219
8	106	−0.0853	−0.0510	−0.0510	1.1152
9	106	−0.0415	−0.0203	−0.0190	1.0812
10	106	−0.0210	0.0047	0.0040	1.0575
11	106	−0.0011	0.0213	0.0210	1.0426
12	106	0.0198	0.0381	0.0380	1.0478
13	106	0.0117	0.0543	0.0540	1.0679
14	106	−0.0180	0.0709	0.0690	1.0980
15	106	0.0197	0.0906	0.0890	1.1760
16	106	0.0409	0.1105	0.1090	1.1195
17	106	0.0025	0.1316	0.1300	1.1598
18	106	0.0750	0.1595	0.1570	1.1984
19	106	0.0640	0.1939	0.1940	1.2481
20	106	0.1037	0.2343	0.2320	1.2088
21	106	0.1044	0.2870	0.2860	1.2243
22	106	0.1179	0.3628	0.3610	1.2217
23	106	0.1576	0.4972	0.4930	1.2503
24	106	0.2223	0.7206	0.6920	1.3033
25	106	0.2916	1.8508	1.2790	1.3747
	2,652				

fashion from portfolio 1 to portfolio 25. Based on the portfolio results, the rank correlation between $\hat{\epsilon}_p$ and e_p is .98. Similar results are reported in table 4 for model *B* percentage error, where the range of $\hat{\epsilon}_p$ is −13.55 percent to 18.86 percent and the rank correlation between $\hat{\epsilon}_p$ and e_p is .94. For the standardized errors (reported in tables 5 and 6), the rank correlation is .97 in both cases.

TABLE 4

Unsystematic Security Returns and Forecast Errors: Model B (Percentage Forecast Errors)

Portfolio	No. of Observations	Mean $\hat{\epsilon}_p$	Mean e_p	Median Forecast Error	Mean $\hat{\beta}_p$
1	107	−0.1355	−1.6506	−0.9090	1.5277
2	107	−0.0822	−0.4888	−0.4770	1.4223
3	107	−0.0554	−0.3358	−0.3280	1.2960
4	107	−0.1101	−0.2463	−0.2470	1.2185
5	107	−0.0761	−0.1908	−0.1880	1.1580
6	107	−0.0119	−0.1461	−0.1450	1.1381
7	107	−0.0585	−0.1101	−0.1100	1.2204
8	107	−0.0559	−0.0837	−0.0830	1.1988
9	107	−0.0554	−0.0544	−0.0530	1.1864
10	107	−0.0805	−0.0308	−0.0290	1.1723
11	107	−0.0256	−0.0113	−0.0110	1.1465
12	107	0.0144	0.0051	0.0050	1.0414
13	107	−0.0046	0.0221	0.0220	1.0587
14	107	0.0221	0.0396	0.0380	1.0120
15	107	0.0352	0.0556	0.0550	1.1020
16	107	0.0196	0.0694	0.0700	1.0762
17	107	0.0656	0.0852	0.0850	1.1284
18	106	0.0967	0.1074	0.1070	1.1460
19	106	0.0121	0.1321	0.1320	1.1184
20	106	0.0737	0.1616	0.1610	1.1503
21	106	0.1259	0.2091	0.2080	1.2270
22	106	0.1407	0.2735	0.2710	1.2479
23	106	0.1119	0.3633	0.3540	1.2722
24	106	0.1366	0.5513	0.5340	1.2000
25	106	0.1886	1.4821	1.1070	1.3675
	2,667				

2.3 SIGNIFICANCE TESTS

To conduct and interpret a test of significance of observed rank correlations, some assumptions must be made regarding the distributional properties of $\hat{\epsilon}_{it}$ and e_{it}. Under the assumption that each joint $(\hat{\epsilon}_{it}, e_{it})$ observation is independent and drawn from the same distribution, the exact distribution of the sample rank correlation r_s has been determined for "small" N (Siegel [1956]). For "large" N, the test statistic $t = r_s \sqrt{\dfrac{N-2}{1-r_s^2}}$, where N equals the number of observations, is approximately distributed as a t-distribution with $N - 2$ degrees of freedom. The t-value for the data reported in table 3 would be 23.6. While previous research indicates that $\tilde{\epsilon}_{it}$ is uncorrelated over time, potential cross-sectional correlation is of concern here. If nonzero correlation exists, test statistics which assume independence may be biased and the direction of the bias will depend upon the nature of the cross-sectional correlation. The sources of potential cross-sectional correlation were discussed in Section 1.3.

TABLE 5
Unsystematic Security Returns and Forecast Errors: Model A (Standardized Forecast Errors)

Portfolio	No. of Observations	Mean $\hat{\epsilon}_p$	Mean e_p	Median Forecast Error	Mean $\hat{\beta}_p$
1	111	−0.1824	−2.0472	−1.962	1.315
2	111	−0.1292	−1.4204	−1.394	1.282
3	111	−0.1440	−1.0456	−1.032	1.274
4	111	−0.0925	−0.7916	−0.792	1.211
5	111	−0.0856	−0.6011	−0.611	1.180
6	111	−0.1102	−0.4597	−0.449	1.212
7	111	−0.0680	−0.3008	−0.293	1.192
8	111	−0.0709	−0.1570	−0.152	1.142
9	111	−0.0443	−0.0598	−0.064	1.138
10	111	0.0152	0.0328	0.033	1.201
11	110	0.0030	0.1039	0.101	1.184
12	110	0.0460	0.1767	0.177	1.199
13	110	0.0146	0.2349	0.232	1.190
14	110	0.0165	0.3071	0.306	1.236
15	110	0.0272	0.3787	0.378	1.166
16	110	0.0728	0.4564	0.453	1.210
17	110	0.1244	0.5550	0.550	1.281
18	110	0.1024	0.6590	0.664	1.200
19	110	0.0511	0.7605	0.759	1.249
20	110	0.1190	0.8980	0.899	1.247
21	110	0.0977	1.0690	1.069	1.273
22	110	0.1629	1.2853	1.270	1.207
23	110	0.1894	1.5554	1.567	1.241
24	110	0.1745	1.9026	1.884	1.085
25	110	0.1226	2.6538	2.576	0.977
	2,760				

Here we propose a test that exploits the property of zero serial correlation in unsystematic returns and attempts to reflect the effect of cross-sectional correlation on the standard error of the test statistic.[12]

The sample rank correlation r_s is computed for each of the ten years (1965–74) and the mean ($\bar{r}_s$) and standard deviation ($s(r_s)$) are computed over the ten-year period.[13] If r_s is assumed to be normally distributed with a zero mean, the test statistic $t = \bar{r}_s \sqrt{T}/s(r_s)$ will be distributed as a t-distribution with $T - 1$ degrees of freedom, where T is the number of years. The values of r_s and the test statistic are reported in table 7 for models A and B for both forms of the forecast errors. The standard deviation will implicitly reflect whatever cross-sectional correlation is present, and hence leads to an unbiased test of significance. The advantage of this test is that it requires no explicit estimation of the variance-

[12] This test procedure was suggested by Professor Dale Morse of Cornell University.

[13] The portfolios consist of approximately ten to eleven stocks per portfolio. If the alternative hypothesis is true, the expected value of r_s will depend on the level of aggregation. The effect of portfolio aggregation will be to reduce the power of the significance test, ceteris paribus (e.g., barring measurement error in the forecast error).

TABLE 6
Unsystematic Security Returns and Forecast Errors: Model B (Standardized Forecast Errors)

Portfolio	No. of Observations	Mean $\hat{\epsilon}_p$	Mean e_p	Median Forecast Error	Mean $\hat{\beta}_p$
1	111	−0.118	−2.254	−2.182	1.273
2	111	−0.120	−1.575	−1.563	1.276
3	111	−0.067	−1.186	−1.179	1.299
4	111	−0.091	−0.916	−0.908	1.232
5	111	−0.090	−0.699	−0.691	1.240
6	111	−0.049	−0.544	−0.545	1.221
7	111	−0.096	−0.403	−0.407	1.213
8	111	−0.017	−0.299	−0.301	1.232
9	111	−0.028	−0.200	−0.203	1.223
10	111	−0.013	−0.108	−0.107	1.204
11	110	−0.035	−0.033	−0.034	1.251
12	110	0.028	0.042	0.039	1.095
13	110	0.009	0.110	0.107	1.213
14	110	0.055	0.185	0.187	1.124
15	110	0.040	0.250	0.247	1.212
16	110	0.030	0.335	0.334	1.188
17	110	0.059	0.417	0.417	1.201
18	110	0.093	0.519	0.517	1.259
19	110	0.070	0.644	0.642	1.226
20	110	0.080	0.779	0.768	1.218
21	110	0.109	0.956	0.955	1.227
22	110	0.137	1.170	1.155	1.217
23	110	0.136	1.438	1.427	1.195
24	110	0.186	1.850	1.843	1.075
25	110	0.099	2.675	2.636	0.979
	2,760				

TABLE 7
*Spearman Correlations Between Median Percentage Forecast Errors and Unsystematic Returns (Portfolio Level)**

Forecast (Fiscal) Year	Percentage Forecast Error		Standardized Forecast Error	
	Model *A*	Model *B*	Model *A*	Model *B*
1965	.9585	.7300	.8731	.7669
1966	.5938	.8215	.7169	.7562
1967	.6931	.6308	.7708	.6346
1968	.9000	.8500	.8123	.8131
1969	.7308	.7069	.8923	.8746
1970	.8131	.5731	.7531	.7492
1971	.7954	.8454	.7069	.7338
1972	.1723	.4108	.3592	.5092
1973	.8708	.4162	.3723	.3438
1974	.8531	.6223	.6923	.7208
Mean Spearman Rank Correlation	.7381	.6607	.6949	.6902
t-value**	10.35	12.93	11.81	13.90

* Based on twenty-five portfolios per year.
** The value of the t-distribution for nine degrees of freedom is 1.833, 2.821, 3.25, and 4.781 at the .05, .01, .005, and .0005 levels of significance, respectively.

covariance matrix of the observations. However, the test does assume that the r_s observations are independent over time and drawn from the same population. In particular, the standard deviation of r_s is assumed to be a constant over time. Obviously, temporal differences in the variance-covariance matrix could induce heteroskedasticity (Kaplan and Patell [1977]). The assumption of normality could not literally hold but is to be viewed as an approximation of the distribution. The reported t-values range from 10.35 to 13.90.

Because of the reliance of the above test statistic on normality, two nonparametric tests were also conducted. In the first test, the null hypothesis is that $\rho_s = 0$, and the probability $r_s > 0 = .5$ (implying a probability of $r_s \leq 0 = .5$). In all four cases, r_s was positive in all ten years. Hence, in each of the four cases, the probability of the observed result is $(.5)^{10}$ or approximately .001 if $\rho_s = 0$. A second test is the Wilcoxon Signed Ranked test. In our context, the assumed process is $r_s = \rho_s + \tilde{u}$, where $\tilde{u}$ is independent (over time) and comes from a population that is continuous and symmetric about r_s. The assumption of continuity cannot literally hold but may be a reasonable approximation. The T+ statistic for each of the four cases in table 7 is 55, which implies a level of significance of less than .002 (see Hollander and Wolfe [1973]). Hence all three tests imply a rejection of the null hypothesis with an extremely small probability of a type I error.

The r_s and t-values reported in table 7 are based upon an aggregation of the data into twenty-five portfolios. The portfolio aggregation was conducted as a convenient way of summarizing the data. However, there is no compelling reason that the significance test should be conducted at this level. Therefore, we also computed r_s and the attendant t-values at the individual security level. The results are reported in table 8. The $\bar{r}_s$ is

TABLE 8

*Spearman Correlations Between Forecast Errors and Unsystematic Returns (Individual Security Level)**

Year	Percentage		Standardized	
	Model A	Model B	Model A	Model B
1965	.6542	.4621	.4290	.4099
1966	.2287	.3192	.3316	.3863
1967	.2912	.2790	.3712	.3244
1968	.5190	.5190	.4742	.4764
1969	.4352	.3879	.4669	.4569
1970	.2770	.2088	.2683	.2603
1971	.3885	.3754	.3791	.3685
1972	.0795	.1721	.1458	.2199
1973	.5475	.1623	.1594	.1161
1974	.3618	.2754	.2587	.2844
Mean r_s	.3783	.3161	.3284	.3303
t-value**	7.074	8.309	8.793	9.301

* Based on between 265 and 276 observations per year.
** The levels of significance for various values of the t-distribution are reported in table 7.

considerably lower, but this is not surprising. The expected effect of portfolio aggregation is to increase the correlation coefficient (Beaver and Manegold [1975]); however, the t-values are extremely high.[14] The two nonparametric tests would imply exactly the same significance level as the portfolio level, because $\hat{r}_s > 0$ in all ten years for all four forms of the forecast errors.

The basic result of significant positive correlation appears in all four forms and does not appear to be particularly sensitive to the form of the forecast error examined here. The similarity of the results across all forms is not surprising given the positive correlation among forecast errors reported earlier. At both individual and portfolio levels, model B does show a higher t-value than A for both standardized and percentage errors. At both levels, higher t-values are reported for standardized versus percentage errors for models A and B. We conclude that the data are consistent with a rejection of the null hypothesis of zero correlation at extremely small probability of type I error.

2.4. SYSTEMATIC RISK AND EARNINGS FORECASTS ERRORS

In this section, we are concerned with the dependency between forecast errors and systematic risk (defined as β_i from equation (1)). In particular, we will examine whether portfolios with "extreme" forecast errors have greater systematic risk. One set of sufficient conditions for the relationship would be: (1) Securities with extreme forecast errors have a greater variance in earnings forecast errors.[15] (2) Securities with greater variances in earnings forecast errors have greater variance in unsystematic returns. (3) Securities with greater variance in unsystematic returns have a higher systematic risk.

Previous research supports (3) (see Miller and Scholes [1972], among others) and supports the joint conditions of (2) and (3) (see Beaver, Kettler, and Scholes [1970] and Beaver and Manegold [1975], among others). Condition (1) would follow if forecast error distribution is symmetric with a constant mean across securities. Beaver [1974] lends support to this condition. Barefield and Comiskey [1975] and Patell [1976a; 1976b] have observed an empirical relationship between management forecast errors and beta.

Tables 3 through 6 report assessments of beta associated with the forecast error portfolios under each of the four methods of computing the

[14] Note that no argument was offered to justify portfolio aggregation based on a reduction in measurement error. In particular, the forecast errors are not treated as measuring some unobservable variable (e.g., unexpected earnings) with error. However, the t-values at the individual security level are lower than those at the portfolio level. In general, we would expect a loss in efficiency of estimation when aggregating the data (see Malinvaud [1970]). One interpretation of our results is that the higher t-values reflect a reduction in measurement error via portfolio aggregation.

[15] Differences in mean forecast errors across firms is an alternate explanation. Arguments could also be offered in terms of higher moments (e.g., skewness).

forecast error. The estimated beta was assessed as the slope coefficient from the linear regression used to compute unsystematic returns, as described earlier. It is based upon the sixty months of security return immediately prior to the year in which the earnings were assumed to be announced. Hence, it is a "preannouncement" assessment of systematic risk. For the percentage form of the forecast error, there appears to be a positive relationship between the (absolute value of the) forecast error and systematic risk. The rank correlation was computed between the $|e_p|$ and $\hat{\beta}_p$; for model A the correlation was .96, and for model B it was .81.

There is a marked difference in the beta-forecast error relationship when the standardized form of the forecast error is used. The relationship is less pronounced, as reflected in rank correlations of .37 and .17, respectively, for models A and B. Scaling the forecast error by its standard error has effectively removed much of the intersecurity difference in variability. Hence, the effect of condition (1) is reduced. The results suggest that dividing by the standard deviation of the forecast error does more than merely "scale" (i.e., adjust for differences in level of EPS) the forecast error; it also effectively removes information regarding the volatility of the forecast errors.

The results in tables 3 through 6 refer to data pooled across years. The rank correlation between the absolute value of the forecast error and beta was also computed for each of the ten years (1965–74) and at both individual and portfolio levels. Tables 9 and 10 report the correlations for the percentage and standardized forecast errors, respectively. The statistical properties of these correlations differ in at least one major respect for those dealing with unsystematic returns and forecast errors. Previous evidence indicates that both unsystematic returns and annual earnings forecast errors are essentially serially uncorrelated. Hence, it is reasonable to view r_s as uncorrelated over time. However, $\hat{\beta}_{it}$ will not be uncorrelated from year to year. In fact, 80 percent of the observations of two adjacent years' beta are computed from a common set of observations. Hence, the correlations reported in tables 9 and 10 are not to be viewed as independent drawings. For this reason, no significance test has been computed.

TABLE 9

Spearman Correlation of the Absolute Value of the Percentage Forecast Error with $\hat{\beta}$

Year	Model A		Model B	
	Individual	Portfolio	Individual	Portfolio
1965	.1055	.3100	.0708	.2220
1966	.2282	.5585	.2077	.4544
1967	.3072	.6031	.1748	.3669
1968	.3370	.7523	.3445	.7182
1969	.3056	.6338	.3115	.6331
1970	.4124	.7392	.2760	.4485
1971	.2796	.6305	.2617	.3723
1972	.4131	.8308	.3438	.8300
1973	.1347	.1646	.2487	.5062
1974	.1099	.3785	.0783	.1373

TABLE 10

Spearman Correlation of the Absolute Value of the Standardized Forecast Error with $\hat{\beta}$

Year	Model *A*		Model *B*	
	Individual	Portfolio	Individual	Portfolio
1965	.0590	.1446	.0285	.0877
1966	.0737	.2177	.0714	.2338
1967	.0971	.2362	−.0342	−.0700
1968	.0738	.4700	.1570	.4392
1969	.1569	.3423	.1339	.2000
1970	.2069	.3885	.0179	−.0015
1971	.0110	.1323	−.0197	−.0462
1972	−.0183	−.1015	−.0571	−.1669
1973	−.3340	−.7546	−.1354	−.2331
1974	−.1848	−.4185	−.1957	−.3954

For the percentage forecast errors, all of the correlations are positive, consistent with apparent positive correlation observed for the pooled results. However, for the standardized forecast errors, the correlations are much lower. In fact, for model *A* three of the ten correlations are negative, while for model *B* at least five out of ten are negative. This is also consistent with the lower correlation observed for the pooled results. Note that there is positive dependence in the signs of adjacent years' correlation coefficients, which confirms our earlier suspicion that the year-to-year observations are positively associated.

Next we consider the relationship between the absolute value of the forecast error and the change in beta. The analysis described above suggests that firms with higher betas are also more likely to have extreme earnings forecast errors (i.e., greater forecast error variability). This is consistent with the findings of previous studies, including Beaver, Kettler, Scholes [1970], among others. The next analysis is concerned with whether an extreme forecast error is associated with an increase in beta. The change in beta is computed as the preannouncement beta described earlier subtracted from the postannouncement beta, which is based upon sixty months of security returns subsequent to the year of announcement (i.e., months + 1 ⋯ +60). This resulted in a reduction of years from ten years (1965–74) to five years (1965–69). As under the beta analysis, adjacent years' changes on beta are computed from a common set of observations and hence will be positively serially correlated.

Five years of positively correlated observations are admittedly a limited basis for inference. However, we present the result because it is the next logical step in the analysis, and because little or no evidence has been offered by previous studies. The results are reported in table 11. In the case of the percentage forecast errors, the signs of the correlations are split on a three to two basis over the five years. The results for the standardized forecast errors are similar. There is little evidence here to support the hypothesis of an increase in beta associated with extreme forecast errors. However, we reiterate that we consider the power of the

TABLE 11

Spearman Correlation of the Absolute Value of Forecast Error with $\Delta\hat{\beta}$

Year	Model *A*		Model *B*	
	Percentage Forecast Error			
	Individual	*Portfolio*	*Individual*	*Portfolio*
1965	.1233	.3140	.1437	.3324
1966	.0883	.3269	.1092	.3728
1967	−.0853	−.3177	−.0080	.0562
1968	−.0233	−.0631	−.0105	−.0008
1969	−.0019	−.0585	−.0295	−.0746
	Standardized Forecast Errors			
1965	−.0226	−.1846	.0140	.0862
1966	−.0123	.0808	−.0000	−.0354
1967	−.1476	−.3685	−.0709	−.0915
1968	−.1183	−.3123	−.1001	−.3246
1969	.0215	−.0085	.0135	.0962

analysis to be low and regard the analysis as peripheral to our main concern.

One reason for examining the beta of the forecast error portfolios is to provide some evidence on potential misspecification of unsystematic returns. Blume [1975], among others, has found that betas tend to revert toward the overall economy-wide mean over time. If so, the use of a preannouncement beta may induce a misspecification of unsystematic returns. The sign of the misspecification (i.e., $\hat{\epsilon}_p - \epsilon_p$) is difficult to assess a priori without further assumptions regarding the equilibrium process but would vary with $\hat{\beta}_p$.[16]

Pettit and Westerfield [1974] found that estimated unsystematic returns using betas from a preestimation period differed from zero for long intervals, although significance tests were not conducted. The pattern would vary from one five-year period to the next. For example, in the most recent period studied (July 1961 through June 1966), the high beta portfolio exhibited positive unsystematic returns of 6.4 percent in the first twelve months after estimation, while the low beta portfolio produced unsystematic returns estimated to be −4.7 percent over the same period.[17] The general thrust of the Pettit and Westerfield research is that $E(\hat{\epsilon}_p \mid \hat{\beta}_p) \neq 0$ and varies with $\hat{\beta}_p$.

[16] If one assumes the Sharpe [1964] form of the capital asset pricing model, $E(\hat{\epsilon}_{pt} < 0 \mid R_{mt} > R_{ft})$ for high beta stocks, while the converse is true for low beta stocks. In the Black [1972] formulation, a similar statement would hold except R_{ft} (the risk-free rate) would be replaced with R_{zt} (the ex-post return on the minimum-variance zero-beta portfolio). If one were to assume $E(R_{it})$ and $E(R_{mt})$ were constant over time, then any revision in estimates of beta would imply revisions in estimates of alpha such that $[R_{mt} - E(R_{mt})]$ would determine the sign of the specification error for high or low beta stocks.

[17] Data were taken from Pettit and Westerfield [1974, table 4]. Their *PE* return metric (whose results are reported on page 600 of their study) is closest to the one used here.

If $E(\hat{\epsilon}_p \mid \hat{\beta}_p) \neq 0$ in the manner described above, it would mitigate against our finding a positive ordinal relationship between unsystematic returns and forecast errors. The reason is clearest in the case of percentage errors. Note that $\hat{\beta}_p$ is a U-shaped function of the forecast error.[18] If there is a relationship between $E(\hat{\epsilon}_p \mid \hat{\beta}_p)$ and $\hat{\beta}_p$, there will tend to be a U-shaped relationship between $\hat{\epsilon}_p$ and e_p. The $\hat{\epsilon}_p$ of the extreme portfolios would tend to be of the same sign and opposite to the $\hat{\epsilon}_p$ of middle portfolios. Hence, if we attempted to remove the error introduced by the misspecifying beta, we would expect the results to become stronger rather than to deteriorate.

Moreover, there is some reason to believe the problem is not a serious one in any event. In Beaver [1974], the unsystematic returns were estimated from betas computed from pre- and postannouncement data. The results were essentially the same as those reported here. Also, in the results reported above, there was no indication of a change in beta, although the test is admittedly a weak one.[19]

3. *Implications*

As indicated earlier, our motivation is methodological in the sense that findings regarding unsystematic returns and the magnitude of the forecast error could alter the research methods and/or implications drawn from other security return studies. To illustrate our point, we use two contexts—earnings-dividend and interim-annual earnings studies. In both cases, our essential message is that ignoring the magnitude of the forecast error "throws away" some of the information content of earnings. Since these studies draw inferences conditional upon the way the forecast error is measured, altering the analysis to incorporate the magnitude could alter the inferences drawn from such studies.

3.1 EARNINGS-DIVIDEND STUDIES

Previous research has been concerned with the incremental explanatory power of dividends with respect to some security return metric after taking into account the explanatory power of earnings. If the earnings

[18] The argument is less clear for the standardized form of the forecast error, where the forecast error is less clearly related to $\hat{\beta}_p$. However, for this reason, it is also not clear whether a problem exists for this form of the forecast error (e.g., if e_p and $\hat{\beta}_p$ are uncorrelated).

[19] Even if one believed that the problem were serious, it is unclear what procedure would eliminate the problem. Joy, Litzenberger, and McEnally [1977] employ the use of "Bayesian adjusted" betas. However, Blume [1975] has shown that this adjustment addresses only one source of reversion in observed betas. In general, it will be inadequate. In fact, Blume offers evidence that the reversion is greater than that provided for by the JLM procedure. Moreover, the properties of "nonresidual" approaches such as the use of "control" portfolios with the same preannouncement betas are unclear. There is no theory or evidence to support the notion that the postannouncement betas would be the same. For example, it could be argued that $E(\beta_{pt+1} \mid e_p, \hat{\beta}_p) \neq E(\beta_{pt+1} \mid \hat{\beta}_p)$, in which case $E(\tilde{R}_{pt} \mid e_p, \hat{\beta}_p)$ differs for portfolios of differing e_p even though their $\hat{\beta}_p$ may be equal.

variable is essentially the sign of the forecast error, then a dividend variable could have incremental explanatory power if (1) earnings and dividends are correlated, and (2) the magnitude of earnings forecast errors have explanatory power. If so, then apparent incremental explanatory power of dividends may be observed because the dividend variable is acting as a surrogate (i.e., a proxy or an instrumental variable) for the magnitude of the earnings forecast error which has been omitted from the analysis. In other words, suppose the study were redesigned to examine the incremental explanatory power of the dividend variable where the security returns metric has been conditioned on the magnitude of the earnings forecast error. In such a case, the apparent explanatory power may disappear (see Sunder [1976] for a similar argument).

3.2 INTERIM-ANNUAL EARNINGS STUDIES

Consider a study which examines the security return metric associated with knowledge of only the sign of the annual earnings forecast error. A higher security return metric could be observed for interim earnings forecast error because the signs of the interim earnings forecast errors are instrumental variables for the omitted information regarding the magnitude of the annual earnings forecast error. The research design could hold constant the magnitude of the annual forecast error before drawing inferences regarding the incremental explanatory power of interim earnings. In other words, among two portfolios with the same magnitude of the annual forecast error, is incremental explanatory power provided by knowledge of the interim earnings forecast error?

Note that in both illustrations the "competing" signals may be collinear. Hence, drawing inferences regarding the "incremental explanatory power" will be a function of the sequence in which the variables are considered. None of what we have said above is intended to resolve the collinearity problem. Our point merely is that the observed explanatory power is a function of how the variables are measured. In particular, throwing away information about one variable leaves open the possibility of permitting other variables to act as instrumental variables reflecting the omitted data.[20]

3.3 SUGGESTIONS FOR FUTURE RESEARCH

There are a number of areas for future research. (1) Studies dealing with multiple signals may incorporate the magnitude of the forecast error into the research design or at least test the sensitivity of findings in this regard. (2) Further analysis is in order regarding the econometric properties of various security return metrics. (3) A more ambitious interpretation of the earnings forecast error merits further consideration. This

[20] Constructing the one variable so that it is orthogonal with the other variable may effectively prevent the second variable from playing the role of an instrument. However, this is an arbitrary way of dealing with collinearity problems.

will involve a specification of the stochastic process generating earnings and the development of a relationship between future expected earnings and valuation concepts such as permanent earnings. (4) The relationship between the magnitude of earnings forecast errors and systematic risk (β_p) is yet another area for research. (5) It would seem reasonable to expect unsystematic returns to be positively correlated with the magnitude of forecast errors from interim earnings. Preliminary evidence by Beaver [1974] supports this expectation. Thus an extension of the Brown and Kennelly [1972] study could be considered.

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