

DISCUSSION

S. SUNDER*: The stated purpose of Paul Griffin's study is to examine the validity of three hypotheses regarding the effect of accounting earnings, cash dividends, and analysts' forecasts of earnings on market return of the common stock of the respective firms. Briefly, stated, the hypotheses are:

1. The marginal effects of the three variables, earnings-per-share (E), dividend-per-share (D), and analysts'-forecast of earnings-per-share (F), are all equal.
2. The joint effect of earnings-per-share, dividend-per-share, and analysts'-forecast of earnings-per-share is significant (-1γ different from zero) and dominates the marginal effects of the three variables and
3. The joint effect of information which is unambiguous is greater than the joint effect of ambiguous information.

My discussion of the paper is centered on these hypotheses. In the limited time available, I shall confine my comments to an examination of the evidence presented by the author to support his conclusions.

Let us start with what we already know about the relationships between stock market returns on one hand and the three variables— E , D , and F on the other. Existing studies have documented sufficiently well that using Griffin's methodology (or its variations) of measuring unexpected changes in the three variables and market returns, a significant positive correlation is observed between cumulative average residual (CAR) and changes in each of the three variables. Griffin's paper is not directed to further documentation of the existence of a positive correlation. He concentrates on testing the three specific hypotheses mentioned above. First, Griffin tests the hypothesis that the marginal effects of the three variables are equal and concludes in favor of the hypothesis by stating that "no major differences in these marginal effects were observed."

Griffin has not made clear what he means by the equality of marginal effects. In the context of the methodology of the paper it seems fair to assume that equality of marginal effects is intended to have the following operational meaning: marginal effect of an informational variable is the cumulative abnormal return an investor can earn by adopting the optimal strategy if the value of the respective variable became known to him 12 months prior to the date of annual earnings announcement, optimality being defined as selecting the investment strategy which leads to maximization of the resultant CAR.

This interpretation requires a two stage procedure for testing the hypothesis. In the first stage, data are analyzed to determine a rule which will make optimal use of the advance information available to the investor. In the second stage, the investment rule selected for each informational variable is applied to a *different* set of data to obtain estimates of the maximum CAR available to the investor when he makes optimal use of the advance information. These estimates of CAR can be used to draw inference about the equality of the marginal effects. Griffin does not identify the investment strategies for making optimal use of the advance information. Nor does he present the estimates of CAR from the three variables obtained

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by consistent application of any other investment strategy. Assuming that the estimated CAR obtainable from advance knowledge of the respective variables is the measure of the marginal effect, I do not find in the paper any estimates of CAR from *E*, *F*, and *D* which are comparable. In the absence of any evidence, it must be concluded that the hypothesis of the equality of marginal effects remains untested.

Let me make clear that the numbers presented in Griffin's paper for *E* and *F* are not the CAR's from application of any consistent investment strategy or optimal investment strategies for using information on each variable. Table 5 assumes that the investor has advance information on *E*, *D*, and *F* of not one or two but all firms. Table 7 has been constructed from Table 5 by using arbitrarily determined "splits." The acceptance of CAR's for testing the hypothesis of equality of marginal effects involves among other things the formulation and testing of a hypothesis on the same set of data. The procedure is convenient but it does not lend itself to a valid statistical inference. In short, Griffin has taken a sample of 486 firms, compared the CARs for top 50 percent of the firms ranked by earnings residuals with CAR's for the top 60 percent of the firms ranked by forecast residual and has concluded that the marginal effects of earnings, forecast and dividend are equal. The claim is not convincing.

Let us now turn to the second hypothesis that the joint effect of the three variables is positive and significantly greater than each of the three marginal effects. The author has not provided an explicit definition of the joint effect and I shall proceed on the assumption that the intended definition of the joint effect is the value of CAR that an investor can get if he obtains the actual value of the three informational variables twelve months in advance of the annual earnings announcement and uses the information in such a manner that the expected values of CAR is maximized. Clearly, a rational investor can do no worse with the joint information than he could with the marginal information on any one of the three variables. If the optimal investment strategy with joint information allowed him a lower expected value of CAR, he could always throw away some information and be better off. Thus by definition, the joint effect must be greater than each of the three marginal effects. Since each of the marginal effects is known to be positive from the previous studies, the joint effect must also be positive. This much follows from logic and needs no empirical verification.

Part of the hypothesis which is subject to empirical verification, then, is whether the joint effect is *significantly* greater than the marginal effects. Note that the purpose of such a test is to measure the difference between joint and marginal effects with a view to assess the significance of such differences for investment, accounting or some other policy decisions. The question here is not only one of statistical significance of the difference, because nonnegativity of the difference is established, but also of substantive significance for the underlying issues. I contend that the estimates of CAR which are supposed to measure the difference between the joint and marginal effects do not do so. No discussion of statistical or economic significance of the estimated differences has been included in the paper. The second hypothesis regarding the significance of the differences between the joint and marginal effects therefore has remained untested in the paper.

In measuring the joint effects, Griffin has used the procedure which is subject to the same criticism as leveled against the measurement of the marginal effect. The hypothetical investment strategy used assumes that the omniscient investor knows the changes in informational variables of not only the individual firms but for all firms simultaneously. He has specified no advance investment strategy and switches from 50 percentile to 30 percentile as the long-short investment rule without much explanation. In fact, dichotomization of earnings and forecast samples is done on the basis of CAR's observed for the deciles, thus eliminating any use the dichotomous CAR's might have had for testing the hypothesis under consideration. But there are more serious problems with the measurement of the joint effect.

Griffin compares the marginal CAR (.1061) for 243 firms which had earnings changes above median (designated $E+$) in the first 4-year test period, with joint CAR for 101 firms which had above median changes in earnings, above 40 percentile changes in forecast earnings and positive change in dividends. On the basis of the higher value of CAR (.1061 vs. .1429) and other similar comparisons in Figure 2 of the paper Griffin concludes that the joint effects significantly dominate the marginal effects. I do not think it is a fair comparison or a valid conclusion. From the results presented in Table 1, we already know that the changes in the three informational variables have a positive correlation. From the marginal effect studies, we also know that each of the variables have a positive correlation. One would expect then that the average change in EPS for the 101 firms in the joint sample of positive changes will be higher than the average change in EPS for the marginal sample of 243 firms. The fact that CAR for the joint sample is higher than for the marginal sample does not lead to the conclusion that an investor can obtain additional abnormal return equal to the difference of the two CAR's if he obtained additional information on the two other variables. Given the positive correlation between the earnings change and CAR, we could expect that if we selected a portfolio of firms which had sufficiently positive changes in EPS, the CAR for the portfolio constructed from the knowledge of one variable alone may exceed the performance of the joint portfolio of positive changes. In short, I am arguing that Figure 2 provides no information on the difference between the marginal and the joint effects since the increasingly selective samples as one moves from Column 1 to 4 of Figure 2 are not comparable in terms of the marginal effects. One cannot take the marginal effects on a diluted sample and the joint effects on a concentrated sample and conclude that the benefits of joint information are greater than the benefits of marginal information. Positive correlation among the four variables involved ensures that the value of each of the three informational variables for the 101 firms in the joint sample of positive changes would be higher than the corresponding values for the marginal samples. Since this difference is not appropriately controlled, results of the study provide no evidence on the joint effects hypothesis which also remains untested.

The third hypothesis examined by Griffin concerns the differences between the joint effects of ambiguous and unambiguous information. He defines unambiguous information as consisting of signals that "an individual perceives to be in agreement in their implication for the (revision of) expected portfolio return." Ambiguity of information is described as a "personalistic" notion which implies that the same

set of signals may be ambiguous for one investor and unambiguous for another. Subsequent discussion also indicates that agreement or disagreement among signals is determined on the basis of the implied direction of change and not the amount. Since expected return is a continuous variable, one would hardly ever expect the earnings, dividends, and forecast signals to be in agreement with regard to the implied amount of change in expected portfolio return. The operational definition of unambiguous signals consists of combinations $(E+, D+, F+)$, $(E-, D-, F-)$, $(E+, D+)$, $(E-, D-)$, $(E+, F+)$, $(E-, F-)$, $(D+, F+)$, $(D-, F-)$, while the remaining combinations constitute the set of ambiguous signals.

Once the sets of ambiguous and unambiguous signals are identified, it becomes immediately clear that the operational meaning assigned by the author to "unambiguous joint information" is exactly the same as the meaning of the term "joint information" used in testing the second hypothesis about the significance of joint information. The same numbers in the same tables are referred to as the tests of the joint information hypothesis and then again as the tests of ambiguous vs. unambiguous signals hypothesis. Therefore my earlier comments on the validity of these results apply equally well to the latter tests. Positive correlations among the three informational variables and their positive marginal effects are enough to ensure that CAR for unambiguous signals would exceed the CAR for ambiguous signals. An empirical test of the proposition is unnecessary.

I must also add that I have difficulty both with the application of the notion of ambiguity in this context and with the test of a hypothesis about a "personalistic" notion by the aggregate market data. If the notion is personalistic, then the same signal could be ambiguous for one individual and unambiguous for another. However, there is nothing personalistic about the way signals have been classified. Why should the response of individual investors to ambiguous signals be heterogeneous if it is homogenous to unambiguous signals? If the response is homogenous for all individuals, is an unambiguous signal really a "personalistic" notion as it is claimed to be? Why should an additive combination of unambiguous signals be used by individuals and by the market for the assessment of portfolio return distribution? And why shouldn't the additive combination of ambiguous signals be used in a similar fashion? These are some of the many questions that arise from the discussion of the notion of ambiguity introduced in the paper. I don't think that the notion of ambiguity adds anything to the framework used to analyze the relationships among the observed stock price behavior and various informational variables.

To summarize, Griffin has attempted to test three hypotheses regarding the equality of marginal effects of information variables, domination of marginal effects by joint effects and domination of the effects of ambiguous signals by the effects of unambiguous signals. Evidence on the equality of marginal effects has not been presented. The evidence on the domination of marginal effects by joint effects has serious problems and cannot be accepted. The third hypothesis in its operational version is the same as the second and need not be considered independently.

Besides testing these three hypotheses, Griffin has drawn several other informal conclusions in discussing and summarizing the results of the study. A discussion of these will take more time than is now available but I must mention one conclusion

that is repeated in the paper more than once—accounting numbers are not blindly accepted. Blind acceptance of accounting numbers has been the favorite strawman of empirical researchers and I doubt if anything is gained by continuing the attack.

In spite of my objections, I do think that the first two hypotheses regarding the marginal and joint effects selected by Griffin are important to our knowledge of the relationship between the market price and various types of information that may be relevant to its determination. I compliment Griffin on selecting this important problem and I hope that his continuing studies will provide answers to the questions he has raised in the paper.