Efficient Capital Markets: A Review of Theory and Empirical Work: Discussion

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The Journal of Finance, Vol. 25, No. 2, Papers and Proceedings of the Twenty-Eighth Annual Meeting of the American Finance Association New York, N.Y. December, 28-30, 1969. (May, 1970), pp. 418-420.

Stable URL:
http://links.jstor.org/sici?sici=0022-1082%28197005%2925%3A2%3C418%3AECMARO%3E2.0.CO%3B2-9

The Journal of Finance is currently published by American Finance Association.

DISCUSSION

WILLIAM F. SHARPE:* Professor Fama deserves considerable praise for this excellent summary. He and his students have provided much of the key work in the area; it is thus fitting that he should be the first to bring the material together and to show so clearly the relationships of the various parts to the overall subject.

I find it worthwhile to step back from this subject every now and then, to see just what is being considered. Simply put, the thesis is this: in a well-functioning market, the prices of capital assets (securities) will reflect predictions based on all relevant and available information. This seems almost trivially self-evident to most professional economists—so much so, that testing seems rather silly. On the other hand, the idea seems truly revolutionary to the traditional security analyst. Only the most exhaustive testing could possibly convince some die-hard practitioners of the merits of the approach. Interestingly, professional economists appear to think more highly of professional investors than do other professional investors.

The replacement of the random-walk model with the more appealing martingale model seems very desirable. And I find the proposed classification scheme appealing, although the definition of a semi-strong martingale is clearly open to dispute (when is information publicly available? which investors are in the public? how soon must the information be available? at what price?).

The idea of a weak martingale is more clear-cut. As I understand it, the concept assumes the full use of past data concerning the factor being predicted. Since return is the object of primary interest, a careful formulation of the weak martingale should be based on past *returns* (i.e. prices *and* dividends), not just on prices. In fact, this is often done via "prices adjusted for dividends". But I would hope that in the future there will be more talk of *returns* and less of *prices* per se.

As Professor Fama indicates, the random-walk thesis requires much more than one would expect from market equilibrium conditions alone. Moreover, it is often misstated and/or misinterpreted (as is the more general martingale process). Let me illustrate. Assume that past data are properly reflected in current prices. Then, in a sense, the price of a security tells everything. But in another sense, it tells nothing. In a world in which there is risk-aversion, one should somehow find out which securities are more risky (and thus promise a relatively high expected return), and which are less risky (and thus promise a relatively low expected return). If security characteristics are reasonably stable over time, past data can be used to differentiate securities from one another in this respect. One could estimate risk directly, but it might be simpler to estimate expected return, since equilibrium conditions suggest that it is a good surrogate for risk. An obvious, and apparently sensible, procedure is to simply use the average return during some past period. This suggests that the *order* of the past data (returns) may not be important, although the data themselves will be. Thus it hardly follows (as some assert) that one should not look at past data at all. Only in a world in which investors are indifferent to risk would this be the case (since every security would then have the same expected return). This is not a very interesting world; but some statements in the literature make little sense in any other environment .

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The idea of using past data to represent *ex ante* predictions raises some interesting questions. Two interpretations may be offered. One holds that predictions remain relatively constant over time, and that the data represent unbiased samples of those predictions. The other interpretations hold that investors do, in fact, make predictions by simply extrapolating past data. If so, past data provide estimates of the predictions currently being made by investors.

Both interpretations raise a crucial question. What, if any, reason does a corporation have to keep its securities from changing significantly over time? The empirical evidence suggests considerable stability—most notably in security risk (the β_j term in Professor Fama's paper). Why might this be so? I suspect that it arises because corporate officers know that significant changes in security characteristics impose costs on investors. There is the cost of not realizing that a change has occurred, as well as the cost associated with the set of transactions required to re-establish one's preferred position regarding risk *vis-à-vis* expected return. The true extent of this stability and the reasons for it clearly deserve additional investigation, as do the implications for the field of corporate finance.

The role of the specialist and his possible replacement by a computer-cum-algorithm raise some interesting (and essentially unanswered) questions. The current procedure leads to certain types of investor behavior. Most notably, a large majority of orders are placed "at market". If a computer were in charge, there might be a larger proportion of limit orders. Moreover, if the "book" were public knowledge, the number of limit orders might be further affected, since the submission of such an order would have two effects, one of which (conveying information directly to many other investors) is now absent. It is thus very difficult to predict the full implications of any particular proposed scheme (computer algorithm), let alone suggest the best procedure.

This is not the place to enter into the controversy concerning distributions of return. One potentially bothersome implication requires comment, however. In his dissertation, Blume suggests that the residuals around a security's characteristic line (i.e. the μ_j term in the "market model" described by Professor Fama) may follow a stable Paretian distribution with a characteristic exponent less than 2. This suggests that least-squares procedures may give poor estimates of security or portfolio volatility. As an alternative, one might fit a line that minimizes the sum of the absolute deviations. This is relatively simple, but the resulting estimates lack some of the key characteristics attributed to the model (in particular, the slope parameter of a portfolio may not equal the appropriately-weighted average of the slope parameters of its component securities). Clearly, we could use a healthy dose of empirical research in this area.

Professor Fama differentiates "single security tests" from tests involving inter-security comparisons. As an economist, I find the latter far more interesting. If there is risk-aversion, expected return should be correlated with risk. The key question concerns the appropriate measure of risk. As indicated, there is only one well-developed theory to cope with this problem. I am beginning to think of it as the hyphenated theory, since it is usually titled by connecting two to five names with hyphens. Professor Fama has used Sharpe and Lintner, but others have added (quite rightfully) the names of Treynor and Mossin, as well as those of Markowitz and Tobin. In any event, the theory proposes a simple yet convincing measure of risk. I call it *volatility*, following Treynor; others call it simply *beta*; whatever it is called, it measures the responsiveness of a security or portfolio's return to changes in the return on the market as a whole.

The crucial question is not what to name risk, but how to measure it. Since the

riskless interest rate does, in fact, change from time to time, it may be preferable to regress excess return on the excess return on the market, thus:

$$(R_{it} - p_t) = b_i(R_{Mt} - p_t) + e_t$$

where: R_{it} = the return on security or portfolio i at time t
R_{Mt} = the return on the market at time t
p_t = the riskless or pure interest rate at time t
b_i = the volatility of security or portfolio i
e_t = a residual or error term with (in theory) a mean value of zero

But this is only one of a number of possible decisions one can make. Here are some others:

1. How should observations be weighted? Are more recent ones more important than less recent ones? How much more important? How long a period should be considered?
2. How often should observations be taken? Is the best differencing interval monthly, quarterly, annually?
3. Should one use return or the logarithm of the value relative? Is the appropriate differencing interval infinitely small? If so, a continuously compounded rate of return may be most appropriate. The logarithm of the value relative (to base e) provides such a value.
4. Should one use before-tax or after-tax values, and if the latter, at what tax rate? Virtually none of the theoretical or empirical work performed to date adequately accounts for income and capital-gains taxes; but differential treatment of these two components of return suggests that different results might be obtained if taxes were taken into account. Can a "representative" tax rate suffice for this purpose? How different will the results be?
5. What index should one use for the "market"? The Dow-Jones Industrial Average, Standard and Poor's 500 stocks, the average return on the securities on the New York Stock Exchange, or perhaps the return on any portfolio of 30 or more securities chosen at random?
6. What measure should one use for the "riskless" rate? Treasury bill rates, the "prime" rate? Should bid prices be used, or ask prices, or an average of the two?
7. How should characteristic lines be estimated? Least-squares or mean-absolute deviation? Returns or excess returns? Should the line be forced through the point at which both returns equal the pure interest rate, or should it be allowed to go above or below that point?
8. How should one compute, interpret, and use "confidence" limits on estimated volatility?

All these questions are undergoing empirical test, not only in academe, but also in the investment industry. We seem to have left the era of testing for serial correlation of security prices and to have entered the era in which we confront head-on the question of risk at both a theoretical and an empirical level. We have come a long way in the last few years. But, fortunately for our employment prospects, we still have a long way to go.