Random Walks and Technical Theories: Some Additional Evidence: Discussion

Frank C. Jen

Journal of Finance, Volume 25, Issue 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), 495-499.

DISCUSSION

Frank C. Jen*: The paper by Jensen and Benington attempts to show that investors using Levy's "relative strength" strategy (hereafter RSS) will not obtain profits significantly higher than those using the "buy and hold" strategy (hereafter BAH) after adjusting for risk differences in the portfolios. To demonstrate their hypothesis, J & B replicate two of Levy's strategies on 29 separate samples of 200 randomly selected securities listed in the New York Stock Exchange on which trading based on RSS are carried out for seven non-overlapping periods of five years using month-end prices as the point of reference. Annual returns are then computed for these 22 portfolios and also for dummy portfolios consisting of buying and holding all NYSE stocks for the same seven non-overlapping periods. Regression models based on Sharpe's capital-assets pricing framework are used to decompose the annual returns to that due to difference in risk and that due to difference in investment strategies. Based on the fact that the overall average return *net* of transaction costs is lower for RSS and further that the overall average performance measure is also less, J & B contend that the relative strength strategy will not produce higher returns than the buy and hold strategy. They further conclude that their evidence supports the random walk hypothesis of security prices.

Before presenting specific comments on J & B's paper, I would like to say that I am sympathetic to J & B's basic position that random walk hypothesis will hold in a long run. J & B should however recognize that Levy's strategy may not be a statistical quirk, but actually represent a trading rule that works for a short period of time because the rule captures the essence of market imperfection at that period of time. On the other hand, the usefulness of Levy's rule is limited to small traders only because a mutual fund of substantial size operating on Levy's 10 stocks ($X = 10\%$) and 20 stocks ($X = 5\%$) will most probably find that the market prices of the stocks will shift so much that the rules will no longer produce better returns.

Analytically, the present paper uses the methodology developed by Professor Jensen's pioneering work [9]. Like many pioneering studies, however, J & B are too hasty in their conclusions. I shall contend that: (1) J & B's proposition that RSS will not produce higher performance on either risk adjusted basis/or net of transaction cost basis is an unwarranted one given their empirical evidence; (2) their empirical estimates of performance measures are suspect because their dependent variables have measurement errors and because the underlying theoretical capital assets pricing model is not a good description of the real world. In addition, I would like to take the opportunity to submit the proposition that we need far more sophisticated theoretical and empirical work to test adequately the random walk hypothesis.

Let us accept for the moment J & B's position that their model and their choice of variables are adequate for the purpose of estimating portfolio performance measures. They show correctly in their Table 2 that the overall average performance measure is the highest for the BAH strategy. However, the data from which the overall average is computed are based on *monthly* prices while the RSS calls for the use of *weekly* prices. Thus, the two results may not be comparable. Further, a careful reading of the raw data in J & B's Tables 3, 4 and 5 reveals that if J & B's time period is divided into two periods (Table 1) BAH is superior for the period 1931

* Professor of Finance, State University of New York at Buffalo.

TABLE 1
AVERAGE RETURNS AND PERFORMANCE MEASURES OVER PERIODS 1931-50 AND 1951-65 FOR VARIOUS STRATEGIES*

	Average Annual Return				Average Performance Measure	
	With Trans. Costs		Without Trans. Costs		$\bar{\delta}$	
	1931-50	1951-65	1931-50	1951-65	1931-50	1951-65
(1)	(2)		(3)		(4)	
Buy-and-Hold	0.087	0.114	0.092	0.119	−0.011	0.007
Relative Strength (X = 10%, K = 160)	0.070	0.149	0.089	0.159	−0.039	0.026
(X = 5%, K = 140)	0.037	0.145	0.068	0.170	−0.084	0.030

* Calculated from J & B's Tables 4 and 5. For the relative strength strategy, the average for 1931-50 is based on 14 portfolios and that for 1951-65 is based on 15.

X = 10%, K = 160)

to 1950 but RSS has a clear lead on *all* measures (including returns net of transaction costs) for the period 1951 to 1965. Thus, even if one argues that the superior result attained by RSS for the period 1951 to 1965 is due to a very high performance achieved in the period 1956 to 1960, the fact that four out of five RSS samples representing 80% of the market in this long five year period significantly outperform the BAH cannot simply be attributed to chance. Indeed, while I am somewhat surprised that a mechanical strategy like Levy's can be profitable for as long as five years for a large segment of the market because of the competitive nature of the stock market, I am more sympathetic to the hypothesis that in general, some trading rules work for *some "short" periods* of time because these rules capture the essence of price adjustments in the market at that point in time. The rule, like innovations, will produce abnormal profits until the entire market catches up with the innovator.

In regard to the regression model J & B used for their empirical work, the model suffers from two weaknesses in terms of measurement of variables. In addition, Sharpe's model is also an inadequate description of the real world. I will first comment on the measurement errors.

J & B used the yield to maturity on five-year government bonds at the beginning of the period as the measure of riskless rates in their regressions.[1] While government bonds are recognized traditionally as riskless bonds, they are only risk-free in a default sense, not in a price sense. As Kessel [26] has shown, investors recognize that intermediate and long-term bonds are not free of price risk in the context of a general capital market model. Therefore, allowance should be made in J & B's regressions for a measure of riskiness of these bonds even if their time to maturity matches investor's horizon of five years. Indeed, by recognizing that the five-year bonds are risky securities, one can write the relationship of their return to market return as:

$$\delta_{it} = R_{it} - [R_{Ft} + (R_{MT} - R_{Ft})\beta_i] + \varepsilon_i$$

Where the subscript i refers to the bond. Assuming that rates on the bonds fully reflect their price risks (i.e., $\delta_{it} = 0$), and that further random fluctuations in bond rates are not correlated with those in portfolio returns (i.e., Cov $(\varepsilon_i, \varepsilon_j) = 0$), we can combine J & B's equation with the above to yield this following regression model:

1. For 1931-35 and 1936-40, rates on 3-5 year treasury notes are used.

$$\delta_{jt} = R_{jt} - R_{it}\frac{1-\beta_j}{1-\beta_i} - R_{Mt}\frac{\beta_j-\beta_i}{1-\beta_i}$$

The coefficients δ_{jt}, β_i and β_j are of course estimatable from non-linear regressions from which inference on portfolio performance can be made as J & B have done. In this form, the model is also more powerful than J & B's as a test whether Sharpe's model is valid because the model tests whether two risky assets are simultaneously in equilibrium returns according to Sharpe's postulates on market behavior. In addition, the model avoids the rather messy problem on the measurements of riskless rates.

As to other resources of measurement errors, Roll [27] has pointed out measurement errors on both riskless rates and market rates will cause significant biases in both the risk measures, betas, and the performance measures, deltas. Moreover, the direction of the bias depends on whether the true beta is greater or smaller than one. Furthermore, the fact that betas may not be temporally constant as Solodofsky and Miller [28] have shown also makes J & B's estimates suspect. Hence, J & B's estimates of return due to difference in risks and return due to difference in strategies cannot be accepted as without significant bias even if Sharpe's model is an adequate description of the real world.

I now turn to the question whether Sharpe's model is an adequate description of the real world. As is well-known, Sharpe assumes that: (1) the model is a static one (2) risk is measured by variance of returns only; other risks are ignored (3) investors have homogeneous expectations on security prices. These assumptions have the following implications: (1) Investors are assumed to use the portfolio for investment purpose and hence they can base their maximization decision on only the parameters of the probability distribution of the portfolio returns and not parameters of probability distributions of say liquidity and consumption needs. Further, the fact that different investors may have different motives and different time horizons for holding their portfolios is completely ignored. (2) New information on all securities constantly generated by the economy and the investors' actions are assumed to be interpreted by all investors in the same manner so that the homogeneous expectations axiom will not be violated.

The first implication that investors hold portfolio for investment purpose only is clearly invalid in the real world because differences in investors' consumption and liquidity needs may affect the composition of portfolios. The fact that many investors like pension funds have horizons longer than one period for their portfolios further complicates the analysis. As a matter of fact, both problems at the investor's level have only recently started receiving the attention they deserve (See e.g., [23], [24]). Judging from the fact that both problems at the investor's level are quite complicated, I am somewhat pessimistic about further extension of the recent theoretical work to a capital market framework as Sharpe and Lintner have done with Markowitz's model. As to the second implication on homogeneous expectations, the assumption is again unrealistic. The security market contains many different assets each of which is affected by some new information every day. The market is also participated in by many traders each of whom is endowed with different wealth in terms of ability to take advantage of new information, different knowledge in terms of ability to interpret the new information and different efficiency in terms of cost of collecting the new information. Homogeneous expectations cannot therefore be the rule and a more realistic new capital assets pricing model needs to be developed before any meaningful empirical tests can be made. This task, as my colleague Boness and I have found out [22], is by no means a trivial one.

I now come to my last point that more sophisticated theoretical and empirical work are needed to test adequately the random walk hypothesis. As many writers including J & B have pointed out, most empirical evidence supporting the random walk hypothesis are of two kinds. The first kind is purely statistical in nature, that is, statistical procedures are applied to demonstrate that successive price changes over a short run in individual common stocks are very nearly independent for a long price series. The inference is then made that the market is efficient. The second kind of empirical tests involves the use of simple mechanical trading rules such as filters. While the evidence is largely consistent with the random walk hypothesis, it is again based on a long price series. Both kinds of tests can be criticized on the ground that the statistical procedure used cannot disprove the hypothesis that for a *limited* period of time, price changes are not random for certain stocks or even a significant part of the market in response to some new information. Nor can they disprove the hypothesis that price changes are random for a long period but actually either mean or variance or both have shifted within that period in response to some new information. They can also be criticized on the ground that no realistic rigorous economic model is offered to support the inference.

Recently, however, two more sophisticated kinds of tests of random walk hypothesis appeared. One is the kind now under discussion pioneered by Professor Jensen for which many remaining difficult theoretical and statistical problems are reviewed above. The other is the kind represented by Fama, Fisher, Jensen and Roll entitled: "The Adjustment of Stock Prices to New Information" [25] in which an attempt is made to study price behavior after stock splits and its associated dividend increases. Unfortunately in that paper, monthly price is used to measure the effect of such information with the result that adjustment mechanism within a month cannot be detected. Moreover, as the authors are aware, stock splits and associated dividend increase cannot be regarded as *significant new* information in the market. Even with these limitations, Fama et al. detected some shift in the variance of the split securities. Interpreting this result together with my own research [22], I am led to believe that failing the development of a rigorous realistic capital assets pricing model, studies on the way common stock prices adjust to new significant information can provide a useful approach to answer the question whether the stock market is as "perfect" as some random walk theorists lead us to believe.

Finally, let me say that the random walk theorists such as Professor Jensen have forced the practicing analysts to think much harder about the *raison d'etre* of an analyst. They have also forced both the practitioners and the academicians to measure investment performance with more rigor. It is true that the tools they propose are somewhat primitive. But this fact should not rob them of the credit that they have dared to ask themselves, the very difficult questions and in the process of answering them have brought significant new insights to these problems.

REFERENCES

1. to 21. Same as Michael C. Jensen and George A. Benington. "Random Walks and Technical Theories: Some Additional Evidence."

22. A. J. Boness and Frank C. Jen. "A Model of Information Diffusion, Stock Market Behavior, and Equilibrium Price," *Journal of Financial and Quantitative Analysis,* forthcoming.

23. A. Chen, Frank C. Jen and Stanley Zionts. "The Optimal Portfolio Revision Policy," *Journal of Business* (forthcoming).

24. A. Chen, Frank C. Jen and Stanley Zionts. "Transition Models for Portfolio Planning with Stochastic Cash Demand," *Working Paper Series No. 51* (Revised), State University of New York at Buffalo.

25. E. Fama, Lawrence Fisher, Michael C. Jensen and Richard Roll. "The Adjustment of Stock Prices to New Information," *International Economic Review,* 10 (1969), 1-21.

26. R. Kessel. *The Cyclical Behavior of the Term Structure of Interest Rates* (New York: National Bureau of Economic Research, 1965).

27. R. Roll. "Bias in Fitting the Sharpe Model to Time Series Data," *Journal of Financial and Quantitative Analysis*, September 1969, pp. 271-289.
28. R. M. Soldofsky and R. L. Miller. "Risk Premium Curves for Different Classes of Long-Term Securities, 1950-1966," *Journal of Finance*, June 1969, pp. 429-445.