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Treasury Bill Factors and Common Stock Returns: Discussion

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higher level, the sign of the effect is reversed in one of these periods when four instead of three principal components are used. Without some further explanation of these results, it is difficult to tell whether they are economically meaningful or simply represent the results of conscientious curve-fitting.

Having raised some questions about the F-J-T analysis, I should add that I agree with the need to assign economic meaning to stock market factors. I have serious reservations about the potential usefulness of arbitrage pricing theory which assumes that investors agree on the relative importance of each of a number of undefined factors in determining the required returns on each of a much larger number of individual assets. Without a specific delineation of what these factors are, it seems to me that there is only one way in which the empirical implementation of arbitrage pricing theory might be useful. If an undefined set of factors could somehow be projected to predict relative asset returns in the future, this would be useful for some purposes even if not altogether theoretically satisfying, but in any case this has not been done and I have some doubts that it can be done. It seems doubtful that the unidentified factors would remain identical in order of importance from period to period as would appear to be necessary for predictive purposes. It is more likely that some of the factors found *ex post* to be critical in one period (e.g., associated with severe oil shortages, war scares, etc.) will be unimportant in the following period. Moreover, in the absence of an explicit description of the factors so that they can be used for attempted predictive purposes, there seems to me to be the danger that statistically estimated factors may represent statistical artifacts resulting perhaps from our inability to specify rigorous significance tests.

DISCUSSION

M. J. BRENNAN*: Professors Oldfield and Rogalski present a test of the Arbitrage Pricing Model based on an *a priori* specification of the relevant factors. No evidence is offered that these factors, which are based on factors drawn from T-Bill returns, are the only, or the most important, of the common factors influencing common stock returns. Indeed no motivation is offered as to why T-Bill returns and common stock returns should be correlated, although they appear to be so in fact.

It seems unlikely to say the least that their factor specification should be complete, and the effect of omitting factors will be to bias in an unknown manner the coefficients of the cross section regressions. Therefore their finding that the estimate of the risk free rate yielded by the cross section regression is significantly different from the actual riskless rate may be interpreted as a rejection of the APT, and evidence for the existence of arbitrage opportunities—on the other hand, it may be attributed to the influence of the omitted factor loadings in the cross section regressions. Similar considerations apply to the significant constant term found in the cross section regressions when the return on the market portfolio is included as an additional factor.

I do not know quite what to make of the more favourable results obtained from a zero beta specification of the APT. Surely if the zero beta portfolio has a return

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significantly different from the riskless rate this is evidence of an arbitrage possibility, and hence logically implies rejection of the APT. It should be noted that while the zero beta specification of the CAPM may be justified by the absence of a real riskless asset, the same is not true of the APT since it does not require a real riskless asset. Thus the evidence presented in this paper would count against the simple form of the APT were it not for the unknown influence of the omitted factors.

More generally, I accept the validity of the APT as a minimalist model of security market equilibrium: it is a minimalist model since it predicts no more than the absence of arbitrage opportunities and, unlike the CAPM, places no restrictions on the relative returns on the different factors. The APT is, therefore, logically prior to our other utility based models, and should be tested before the predictions of stronger utility specifications are considered. However, I am concerned lest the practical influence of the APT on our research programme should be to lead it in the direction of *ad hoc* data manipulation and atheoretical correlation analysis. I do not refer here to the estimation of the factor structure of security returns since, although factor analysis is frequently castigated as empiricism without theory, it is not so in this context for estimation of the factor structure is a necessary step in devising tests of the APT. What I am referring to is simple attempts to correlate the factors found with other economic time series without any underlying theoretical structure: I do not believe that these efforts are likely to contribute in any significant way to our understanding of security price determination.

Turning to the specifics of this paper, I was struck by the fact that the T-Bill returns which were factor analysed exhibited substantial serial correlation, presumably because of serial correlation in the riskless interest rate. Yet the factors of the APT are supposed to represent unanticipated common influences in security returns and are therefore unlikely to be significantly serially correlated. It would have been preferable therefore to have removed the serial correlation before estimating the factor structure, perhaps by subtracting the riskless rate from the T-Bill returns. Secondly, it is worth noting that a count of significant coefficients from the regressions of security returns on the special portfolio returns will tend to exaggerate the influence of the T-Bill factors because the special portfolio returns are linear combinations of the security returns themselves.

I was also somewhat mystified by the authors' claim that their finding that security returns are influenced by T-Bill factors implies that the APT is correct. The APT is of course a theory which predicts the intercept of the regression of individual security returns on factor loadings; significance of the factor loadings is of no consequence in itself. Nor am I convinced that the authors' cross-sectional results constitute an adequate test of the predictions of the APT; all that they could establish, assuming that they do not reject the APT, is that deviations from the APT are uncorrelated with the factor loadings. This leaves open the possibility that there may be significant deviations from the APT correlated with other variables such as size, P/E ratio, dividend yield etc. What is required is a test of the hypothesis that the intercept terms for all securities are equal to zero, though such a test may be difficult to construct. This paper is a first step towards an adequate test of the APT; much remains to be done.