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Multi-Beta CAPM or Equilibrium-APT?: A Reply

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IN THEIR INFLUENTIAL EMPIRICAL PAPER, Roll and Ross [11] state, "We think that in many discussions of the *CAPM*, scholars were actually thinking of the *APT* and of process (1) [the factor model] with just a single factor." In light of recent developments in the asset pricing literature, I think it can now meaningfully be suggested that, in many discussions of the *APT*, scholars may have actually been thinking of a multi-beta interpretation of the *CAPM*. Indeed, much can be said in support of each of these perspectives, for a single paradigm is emerging which integrates fundamental aspects of both the *CAPM* and the *APT*.

Broadly speaking, the equilibrium (*CAPM*) paradigm emphasizes the role of covariance between asset returns and an endogenous preference-based aggregate[1], while the arbitrage (*APT*) paradigm focuses on the covariance between asset returns and factors in the return generating process. The emerging equilibrium factor paradigm encompasses each of these relations as well as a third—covariance between the factors and the equilibrium aggregate. My thesis in Shanken [17] was that: (i) the arbitrage paradigm has not produced a refutable hypothesis and that (ii) recognition of the role of the factor/aggregate covariance relation appears to be essential if positive economic significance is to be attributed to asset pricing empirical work.

Dybvig and Ross (*D-R*) attempt to resolve the empirical indeterminacy of the arbitrage paradigm by appealing to the condition that residual variance should not be too "large." I explain, in Section II, that this condition is inherently ambiguous from an empirical perspective and does not permit the formulation of a precise testable implication. The equilibrium factor models which *D-R* discuss do yield testable restrictions on parameters in the return-generating process. However, consistent with my second point above, these restrictions involve joint hypotheses that cannot be verified without observing the entire universe of assets. Thus, contrary to their claim, considerations similar to those in Roll's [10] critique are indeed relevant to the testing of these models. This and other similarities between the "equilibrium-*APT*" and the *CAPM* are discussed in Section III. I begin by clarifying some basic terminology.

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[1] To keep the discussion as simple as possible, we restrict our attention to the basic single-period mean-variance *CAPM*. The analysis may easily be modified to accommodate state variables other than aggregate wealth and intertemporal considerations. Our conclusions also apply to the various "nonstandard *CAPMs*" in the literature (see Elton and Gruber [5]).

I. Terminology: "Testable" and "APT"

A discussion of the testability of the *APT* requires, of course, that we have a reasonably clear idea of what is meant by "testable" and what is referred to by *"APT."* With regard to the former, Roll [10] introduces a useful distinction by suggesting that the *CAPM* may be testable in principle but not in practice. In the absence of what are essentially measurement problems, the central implication of the *CAPM*, that the market portfolio is efficient, may be tested. However, limited observability of the universe of assets and the consequent substitution of proxies for the true market portfolio make it difficult to reach definite conclusions from the usual tests. We are really referring to this ambiguity when we say that the *CAPM* is not testable.

Ambiguity in testing a theory cannot be separated, though, from the particular approach to testing that one has in mind. As Ross [15] emphasizes, Roll's analysis does not say we must observe the market portfolio to test the *CAPM*. For example, he notes that since the market portfolio has only positive components, the *CAPM* could be refuted by refuting the existence of a positive efficient portfolio. Likewise, given certain joint hypotheses concerning the factors in a return-generating model, the efficiency of the market portfolio could be refuted by rejecting the linearity of the relation between securities' expected returns and their factor "betas." We return to this important point in Section III.

Now, what is "APT"? The empirical literature which preceded Shanken [17], and which that paper was largely a reaction to, is consistent in its description of the *APT*. A strict factor model, i.e., one with uncorrelated disturbances, is postulated for the returns on a subset of assets which need not span the universe of possible investments. Quoting from Roll and Ross [11], "With minor caveats, given the factor generating model, the absence of riskless arbitrage profits—an easy enough condition to accept a priori—leads immediately to the *APT*." The required caveat is particularly evident in the simplified derivation of the *APT* by Huberman [7], who also clarifies the precise nature of the arbitrage condition underlying the theory. As the number of assets under consideration increases without bound, the variances of the disturbances in the strict factor model must remain bounded above by a fixed positive number. Together with the no-arbitrage condition, this suffices to derive the conclusion of the *APT*, i.e., that there exist factor "prices of risk" such that the infinite sum of squared deviations from the corresponding multi-beta pricing equation is finite.

In light of the comments of *D-R*, it is worth emphasizing the following. There are no restrictions on asset supplies or on the nature of the investments underlying the "asset" returns. As is often noted, the market portfolio plays no particular role in the theory. Perhaps most importantly, there are no restrictions on the statistical relation between the subset of assets under consideration and other assets in the economy. These are characteristics generally true of any theory based on the nonexistence of arbitrage. As we shall see, the equilibrium factor models do not exhibit such characteristics. This is not surprising, as arbitrage arguments are not employed in these models.

Consistent with the convention maintained in Shanken [17], we shall refer to the equilibrium factor models as "equilibrium-*APTs*," reserving the term "*APT*"

for the arbitrage-based models. In doing so, we acknowledge the important role of the *APT* in the historical development of the equilibrium factor model, while emphasizing that there are two paradigms with very different characteristics. In contrast, *D-R* expand the definition of "*APT*" to include the equilibrium models. Since I questioned the testability of the *APT* while acknowledging the testability, in principle, of the "equilibrium-*APT*," their use of a single term to refer to all of these models obscures the basic issue. Thus, the "*APT*" in "The Arbitrage Pricing Theory: Is It Testable?" does not coincide with the "*APT*" in "Yes, The APT is Testable." The reader should keep this in mind.

II. Empirical Indeterminacy in the Arbitrage Paradigm

Descriptions in the empirical literature tend to conform closely to the actual features of the arbitrage theory in all but one important respect—its conclusion. The multi-beta expected return relation associated with the strict factor model is typically presented as an equality. Several scholars have explicitly suggested that a deviation from the exact relation would indicate the existence of an arbitrage opportunity. This is not so, however, even in an economy with infinitely many assets.[2] In fact, as emphasized in Shanken [17], the *APT* restriction is an approximation, one which prices "most" assets well but permits arbitrarily large deviations from exact pricing on a finite set of assets. Thus, it is difficult to conceive of any (finite) empirical procedure that could be used to refute the actual conclusion of the *APT*. In this sense, the theory is untestable in principle.

To dramatize the fact that the *APT* pricing restriction is fundamentally different from that of the *CAPM* and other familiar equilibrium models, I presented an argument that has been misinterpreted by *D-R*. Assuming the role of devil's advocate, I demonstrated that naive application of an *exact* pricing relation to any *finite* set of returns which conform to a strict factor model leads to the absurd conclusion that all assets have the same expected return.[3] *D-R* do not question the mathematical validity of the argument employed. Rather, they claim to disagree with my interpretation of it.

The contradiction I derived highlights the fact that exact pricing involves more than a statistical factor model and a simple no-arbitrage condition. *D-R* agree that additional assumptions are needed but, as I explain in the next section, they fail to recognize the *empirical significance* of these assumptions. Their comments on the mathematical manipulation of approximations, while insightful, are entirely beside the point. No approximations were manipulated in Shanken [17]. My argument was aimed squarely at the traditional view of the *APT* as an *exact*

[2] Intuitively, a portfolio tilted toward a few securities with "large" deviations could expose an investor to risk and would not, therefore, constitute an arbitrage opportunity. In contrast, infinitely many "large" deviations permit the construction of a costless portfolio (sequence) which diversifies away all risk while exploiting these deviations (see Huberman [7]).

[3] *D-R* point to several properties peculiar to my two-asset example. This example was presented merely to illustrate the algebraic nature of the general transformation argument, in the Appendix, which applies to any finite set of investments. The conclusions I derive from that argument do not depend on the idiosyncracies of the simple example. Nonetheless, *D-R*'s analysis is worthwhile insofar as some readers may have drawn conclusions from the example that were not intended.

pricing model. Its relevance lies in the questions it raises about this view and, more generally, about the empirical limitations of the arbitrage paradigm. To interpret it outside this context, as *D-R* do, is misleading.

D-R object to the "arbitrary" nature of the portfolios constructed in my argument and emphasize the high variance of return on these portfolios. It is important in thinking about these issues, though, not to project onto the arbitrage theory conceptions which really have their basis in the richer equilibrium factor paradigm. The portfolios constructed satisfy the traditional *APT* requirement of a strict factor model. That they involve substantial short positions is in itself irrelevant, insofar as the nature of the investments underlying the "asset" returns plays no role in the arbitrage theory. Intuitively, the bounded residual variance condition of the *APT* might be interpreted as meaning that the theory should not be applied to very risky portfolios. This condition can hardly serve as an unambiguous empirical criterion, however, as the variances of any finite set of returns will always be bounded above.[4] At what point are we to presume that the bound is small enough to justify treating the arbitrage relation as exact?

A recent result by Ingersoll [8] is relevant in this regard. Let us say that a portfolio is "well-diversified," with respect to a set of factors, if it has no residual risk, i.e., if the factors account for all the variation in its return. Ingersoll essentially shows that the deviation in the arbitrage relation is zero for a well-diversified portfolio. The *APT* remains silent, however, with respect to the pricing of a given individual security with positive residual variance. It yields a restriction on the *aggregate* sum of squared deviations, without restricting any particular term in the summation. In contrast, the equilibrium models discussed below delineate specific joint empirical hypotheses which, if satisfied, ensure that a given asset's deviation in the multi-beta equilibrium relation is negligible or, in some cases, exactly zero.

III. The Market Diversification Condition

It is sometimes suggested, misleadingly, that the *CAPM* is a single-factor model while the *APT* is a multi-factor model. Likewise, it is said that there is a single source of risk in the *CAPM*, whereas the *APT* allows for multiple measures of risk. However, Sharpe [19], Rosenberg and Guy [13], and Ross [14] have independently developed multi-beta interpretations of the *CAPM*. Their insight is a simple but important one. By decomposing the market return into a linear combination of several fundamental economic variables or "factors," the market beta of any investment may be written as a linear combination of its betas with respect to these factors. The linearity of expected return in the fundamental betas follows immediately from the *CAPM*. Conversely, Chen [2] notes that an exact multi-beta pricing relation can always be reduced to a single-beta relation through an appropriate transformation of the factors. Thus, we cannot distinguish between the equilibrium and arbitrage paradigms on the basis of the number

[4] Similar remarks apply to recent extensions of the *APT* which allow for correlated factor model disturbances but require that the eigenvalues of the residual covariance matrix be bounded above by a fixed positive number (see Chamberlain and Rothschild [1], Ingersoll [8], and Stambaugh [20]).

of covariance measures in the pricing relation or the number of factors in the return-generating process.

Connor [3] derives an exact multi-beta pricing relation in a fairly general equilibrium context. His model is based on the assumption that the factors account for all the risk of the market portfolio return; i.e., that the market portfolio is "well-diversified." This, of course, is precisely the condition that underlies the earlier multi-beta interpretations of the *CAPM*.

As noted in Shanken [17], Connor's "equilibrium-*APT*" is not vulnerable to the portfolio argument which was used to exhibit the contradiction inherent in the "usual empirical formulation of the testable implications of the *APT*." Recall that the extreme version of this argument involves a set of mutually uncorrelated portfolios. In a *statistical* factor-analytic sense, these portfolios conform to a zero-factor model. *Economically*, however, it is clear that the market portfolio is not well-diversified with respect to the empty set of factors, for this would imply that the market return is riskless. Thus, I concluded, "from the perspective of the 'equilibrium-*APT*', the existence of a zero-factor representation of returns (appropriately) fails to take on any economic significance."

Many traditional ideas about the relation between the *CAPM* and the *APT* must be abandoned if we expand the definition of "*APT*" to include the "equilibrium-*APT*," as *D-R* do. In particular, the central role of the market portfolio in the "equilibrium-*APT*" is evident. As emphasized in Shanken [17], the requirement that the market portfolio be well-diversified, with respect to a given set of factors, should be considered a joint hypothesis in a test of the Connor model.[5] Clearly, this joint hypothesis cannot be verified without observing the return on the true market portfolio. It is difficult, therefore, to understand how *D-R* can claim to have refuted my assertion that a form of Roll's critique is applicable to the "equilibrium-*APT*."

In general, rejection of the multi-beta relation cannot be viewed as unambiguous evidence against the model. Rather, it must be interpreted in light of the possibility of a missing factor or component of aggregate wealth. This is presumably what *D-R* mean when they refer to tests that are "biased" against the "*APT*." It does not mean, however, that spurious acceptance of the multi-beta *CAPM* or "equilibrium-*APT*" may be ruled out. To see this, consider the following multi-factor version of Roll's familiar argument. It is a mathematical fact that there will always exist a set of variables such that a multi-beta pricing relation holds with these variables taken as the "factors." The returns on any set of portfolios which may be combined to form an efficient portfolio will do. Without the market diversification condition, therefore, the model has no positive economic content.[6] The diversification condition restricts the admissible sets of factors, making the theory potentially refutable or testable in principle.

Let us now consider the testability of the more recent "equilibrium-*APTs*" of

[5] As a practical matter, if the particular securities in an empirical test all have zero loadings on a given factor, that factor need not be explicitly included (see Shanken [18] for further discussion of this point). Also, note that the factor model disturbances need not be independent of each other, although they are assumed to be independent of the factor.

[6] In the final paragraph of Section III and in footnote 2 of their paper, *D-R* ignore the distinction between testing a purely statistical relation and one with positive content.

Ross [16], Dybvig [4], and Grinblatt and Titman [6] (*RDGT*). These models, which involve approximate multi-beta pricing relations, generalize earlier work in the *CAPM* literature by Jensen [9] and Rosenberg [12]. To simplify the discussion, we focus on the Dybvig model. In this model, a representative agent holds the market portfolio and assets are priced in terms of their covariance with the marginal utility of aggregate wealth. A strict factor model is postulated for *all* assets.[7] This is a fundamental departure from the original *APT* which, as Roll and Ross [11] emphasize, merely requires that a given *subset* of assets satisfy the strict factor model assumptions.

D-R note that a conservative bound on an asset's pricing deviation can be computed without observing the entire market portfolio. The bound is tight provided that investors are not too risk-averse, that an asset's specific variance is not too large, and that its proportion of aggregate wealth is small. They fail to mention, however, that the validity of this bound is contingent upon a *joint hypothesis* that does involve the entire universe of assets. It is possible, without observing the market portfolio, to verify that an asset's proportion of the market is small or that a given subset of returns conforms to a strict factor model. It is not feasible, though, to verify that a given asset's factor model disturbance is independent of the returns on *all* other assets in the economy, as the *RDGT* models require.

That truly idiosyncratic noise commands a negligible premium in the theoretical equilibrium pricing relation is not surprising. The real question is whether the factor model disturbances in an actual empirical investigation are idiosyncratic in the required theoretical sense. The "land value" example in *D-R*'s discussion of "testability on subsets" is just one illustration of the way in which the relevant joint hypothesis might be violated in practice. In this example, the factor-analytic technique mistakenly identifies the real estate factor as idiosyncratic when, in fact, it is highly correlated with the aggregate market return. This is the *RDGT* parallel to the usual *CAPM* proxy problem discussed by Roll.

D-R basically argue that the conditions we refer to are assumptions of the *RDGT* models, while empirical tests should be concerned solely with the models' conclusions. They liken this to the role of normality in the *CAPM*, noting that normality is not part of the conclusion of the *CAPM*. The strict factor model assumption on all assets *is* part of the conclusion of the *RDGT* models, however, in much the same way that the market diversification condition is part of the conclusion of the Connor model. It contributes to the specification of the admissible sets of variables that may serve as factors in the models' pricing relations. Together with the asset supply and specific variance restrictions, this assumption ensures that the contribution of nonfactor risk to total market variance is negligible. Thus, the market portfolio is approximately well-diversified and the resulting multi-beta equilibrium relation is approximate as well.[8]

[7] Grinblatt and Titman [6] weaken the strict factor model assumption somewhat by allowing correlation between the disturbances of nonpriced assets. These disturbances are independent of the returns on the priced assets, however.

[8] This interpretation requires some qualification. Imagine an economy in which an asset X accounts for half of aggregate wealth while all other assets are in small supply. The market portfolio need not be approximately well-diversified, in this case, yet the pricing relation may still be a good approximation for the assets in small supply. In a sense, the disturbance term of asset X is really a factor in

IV. Conclusions

The current "debate" began when I questioned whether the existing multi-factor empirical investigations were really tests of Ross's arbitrage pricing theory. In response, *D-R* have shifted the focus to a class of equilibrium models which we all agree are testable in principle, while obscuring the differences between these models and the original arbitrage theory. As suggested in Shanken [17] and further emphasized here, the existing studies may be reinterpreted as *joint* tests of these "equilibrium-*APT*s" and a hypothesis that was not made explicit in those papers, i.e., that the market portfolio is (approximately) well-diversified with respect to the set of factors consisting of those actually included in the tests and any others that are unrelated to the returns on the given securities.[9]

An important empirical advantage of the multi-factor approach is that it increases our confidence in the validity of this joint hypothesis. Ambiguity remains, however, as long as aggregate wealth is unobservable. Assuming the joint hypothesis is true, rejection of the linearity of expected return in the factor "betas" entails rejection of the *CAPM* as well as the "equilibrium-*APT*." In this sense, the *CAPM* is testable under conditions which many scholars currently seem willing to embrace.[10]

In contrast, the traditional *APT* perspective, reflected in the empirical literature and several recent textbooks, denies that there is a need for such a joint hypothesis. This view ignores the actual conclusion of the *APT*, magically combining the stronger conclusion of the "equilibrium-*APT*" with the modest assumptions of the arbitrage-based theory. As we have seen, however, nothing in *D-R's* analysis indicates that a refutable empirical hypothesis can be obtained within the arbitrage framework itself. The equilibrium factor models which they discuss have much to offer, but are essentially generalizations of the simple mean-variance *CAPM*, in multi-beta form.

In their final note, *D-R* appear to suggest that the problems considered here do not exist when the factors are estimated from exogenous economic data, rather than by statistical factor methods. If one adopts the view I have emphasized, that the factors are essentially proxies for the equilibrium pricing aggregate(s), then what guarantees that exogenously estimated factors will be perfect proxies? On the other hand, if this view is not taken, then we may reasonably wonder what *refutable* economic hypothesis, if any, is being tested in these studies.

It should be noted, in closing, that the remarkable theoretical contribution of the original *APT*, i.e., providing an arbitrage basis for the general importance of covariance risk in asset pricing, remains undiminished by the observations made here. Our sole concern has been with issues of empirical testability.

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this economy. With this "factor" included, the market portfolio *is* approximately well-diversified and the corresponding pricing relation is applicable to *all* assets.

[9] Equivalently, the factor model disturbances of these securities must be (approximately) independent of the market return. The usual tests of the *CAPM* may be viewed as a special case. The implicit joint hypothesis is that the "market-model" disturbances (based on the market proxy) are independent of the true market return.

[10] The particular prediction of the *CAPM* concerning the values of the factor prices of risk would, of course, be ignored in such a test. See Shanken [18] for a general approach to testing equilibrium pricing relations under weaker assumptions.

REFERENCES

1. G. Chamberlain and M. Rothschild. "Arbitrage, Factor Structure, and Mean-Variance Analysis on Large Asset Markets." *Econometrica* 51 (September 1983), 1281–1304.
2. N. Chen. "Some Empirical Tests of the Theory of Arbitrage Pricing." *Journal of Finance* 38 (December 1983), 1393–1414.
3. G. Connor. "A Unified Beta Pricing Theory." *Journal of Economic Theory* 34 (December 1984), 13–31.
4. P. Dybvig. "An Explicit Bound on Individual Assets' Deviations from APT Pricing in a Finite Economy." *Journal of Financial Economics* 12 (December 1983), 483–96.
5. E. Elton and M. Gruber. "Non-Standard CAPMs and the Market Portfolio." *Journal of Finance* 39 (July 1984), 911–24.
6. M. Grinblatt and S. Titman. "Factor Pricing in a Finite Economy." *Journal of Financial Economics* 12 (December 1983), 497–507.
7. G. Huberman. "A Simple Approach to Arbitrage Pricing Theory." *Journal of Economic Theory* 28 (March 1982), 183–91.
8. J. Ingersoll, Jr. "Some Results in the Theory of Arbitrage Pricing." *Journal of Finance* 39 (September 1984), 1021–39.
9. M. Jensen. "Risk, The Pricing of Capital Assets and the Evaluation of Investment Portfolios." *Journal of Business* 42 (April 1969), 167–247.
10. R. Roll. "A Critique of the Asset Pricing Theory's Tests." *Journal of Financial Economics* 4 (March 1977), 129–76.
11. ——— and S. Ross. "An Empirical Investigation of the Arbitrage Pricing Theory." *Journal of Finance* 35 (December 1980), 1073–1104.
12. B. Rosenberg. "Extra-Market Components of Covariance in Security Returns." *Journal of Financial and Quantitative Analysis* 9 (March 1974), 263–74.
13. ——— and J. Guy. "Prediction of Bata from Investment Fundamentals." *Financial Analysts Journal* 32 (May–June 1976), 60–72.
14. S. Ross. "Return, Risk, and Arbitrage." In I. Friend and J. Bicksler (eds.), *Risk and Return in Finance*. Cambridge, MA: Ballinger, 1976, 189–219.
15. ———. "The Current Status of the Capital Asset Pricing Model." *Journal of Finance* 33 (June 1978), 885–901.
16. ———. "On Intertemporal Asset Pricing Theories." School of Management Working Paper, Yale University, 1982.
17. J. Shanken. "The Arbitrage Pricing Theory: Is It Testable?" *Journal of Finance* 37 (December 1982), 1129–40.
18. ———. "Equilibrium, Factors, and Arbitrage Pricing." University of California Working Paper, 1984.
19. W. Sharpe. "The Capital Asset Pricing Model: A 'Multi-Beta" Interpretation." In H. Levy and M. Sarnat (eds.), *Financial Decision Making Under Uncertainty*. New York: Academic Press, 1977.
20. R. Stambaugh. "Arbitrage Pricing with Information." *Journal of Financial Economics* 12 (November 1983), 357–69.