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DISCUSSION

ALLAN W. KLEIDON*: Suppose that, seven years ago, research was reported showing that expected returns on stocks were not constant, and that consequently the results of tests that assumed constant expected returns showed apparent violation of (otherwise) rational valuation models. What would have been the

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reaction of researchers in finance, or in economics generally? Since it was well recognized in the literature that strong conditions are necessary to derive temporally constant expected returns on stocks (e.g., see the references in Kleidon [9, fn. 38, p. 997]), the importance of such research would have been because the assumption of constant expected returns has been empirically robust, and because new lines of research to explain the non-constant returns (e.g., how are they related to the business cycle?) would no doubt have been forthcoming.

While in many ways Ken West's paper does an excellent job of evaluating the variance-bound literature, much of the flavor of this extended controversy is missed by the admittedly partial nature of his analysis. Thus, for example, Ken's paper fails to highlight the significance of the conclusion reached by Campbell and Shiller [1] in one of the other papers presented in this session on "The Volatility Debate," namely that "excess volatility and predictability of multi-period returns are not two phenomena, but one" [1, p. 4]. This discussion provides some of the missing background that, in my judgment, is indispensible in explaining the course of the debate, examines some particular issues, and reaches a conclusion that differs from Ken's about the current state of our knowledge.

What was it about the initial papers by LeRoy and Porter [11] and Shiller [14] that stimulated the response noted in West's abstract? Although the formal statistics concerning sample variances were important, West states (p. 646) that the "statistical significance of the excess volatility was, however unclear" since LeRoy and Porter found violation (based on an asymptotic distribution) at the five percent level in only one of four data sets. Further, Shiller presented only point estimates of sample variances.

What West fails to mention is the dramatic impact of the plots presented in Shiller [14], which apparently showed not only huge volatility of stock prices relative to an "ex post rational price," but also periodic large deviations from the "rational" price. These plots were so convincing that they substituted for statistical evidence. Further, given the interpretation that they were evidence of pervasive deviations of actual prices from rational prices, it was a short step to alternative models of fads or rational speculative bubbles. For example, an important paper examining speculative bubbles (Tirole [16]) states explicitly (p. 1085) that the model tested in Shiller [14] is seen to be violated "[s]imply by looking at Figures 1 and 2 in Shiller [14]."

Unfortunately for much of the direction of the literature, such an interpretation of these figures is simply false for the reasons given in Kleidon [9], and West's paper excludes reference to these influential, but misleading, plots. It is interesting to speculate on how much effort would have been devoted to, say, speculative bubbles, had these plots not given such apparently strong evidence of deviations of prices from rational fundamentals. West's own research followed the path of speculative bubbles, although he "no longer find[s] this interpretation particularly appealing" for a variety of reasons (p. 651). I agree.

West examines the statistical evidence from various versions of variance-bounds tests. Initial work assumed constant expected returns (discount rates).[1]

[1] An exception is Grossman and Shiller [6], but this paper focuses on the plots discussed above. See Kleidon [9, pp. 958 ff.].

As West describes (p. 645), "One of the major problems of the initial volatility tests, emphasized in particular by Kleidon [9] and Marsh and Merton [12], was of course the assumption that variables do not have unit roots." Monte Carlo evidence on Shiller's initial tests explains much of the apparent excess volatility, even assuming constant discount rates, although results are sensitive to plausible differences in parameter values. (See Shiller [15] and Kleidon [10].) Most recent tests allow for unit roots, and the effect on test statistics is a "dramatic fall" in apparent excess volatility (West, p. 646).

The burden now falls on the properties of test statistics in sample sizes typically used. However, as discussed in Flavin [4] and Kleidon [7], these tests typically display severe small-sample bias. The results of Mattey and Meese [13] also demonstrate extensive small-sample bias across a wide range of extant tests of excess volatility. West (p. 649) believes that it is "unlikely . . . that small sample bias provides the bulk of the explanation for the excess volatility reported in [his] Table I." However, the appropriate metric for "bulk" is unclear, especially in tests assuming constant discount rates. Further, the effects of small-sample bias can be subtle, as illustrated from some results in West's current paper. He investigates (pp. 647–49) the power of two tests that do not reject present-value models (with constant discount rates) against a particular "fad" alternative. In this context, he states (fn. 5, p. 649) that his point estimates are "notably bigger than Kleidon's [9]" because of different estimation methods.

The point at issue concerns estimates of $V \equiv \text{var}(P_{t+n}/P_t)$ and $V^* \equiv \text{var}(P^*_{t+n}/P_t)$, used in the conditional variance tests in Kleidon [9]. West calculates these as mean squared deviations around sample means; I took mean squared deviations around an estimate of $\text{E}(P_{t+n}/P_t)$, which equals $\text{E}(P^*_{t+n}/P_t)$ under the null. West reports that his procedure gives a ratio V/V^* (for $n = 10$) of 4.18 for S&P data (see Table II, panel B), while mine gives a ratio of 1.80, and he correctly attributes the change to differences in the sample means.

However, these differences are at the heart of much of the small-sample bias that West is downplaying in this section. As discussed in Flavin [4] and particularly in this context in Kleidon [7], the autocorrelation in the P^*_t series relative to the price series results in substantially higher sampling variance of the mean in the former series. I have replicated my Monte Carlo results using West's procedure, and the averages (across 2,000 replications) of the sample means of P_{t+10}/P_t and P^*_{t+10}/P_t are equal, but the variance of the sample means for the P^*-based series is over seven times that of the former. Consequently, the number of incorrect apparent violations under the null increases from 924 out of 2,000 reported in my Table 3 [9, p. 986] to 1,493; the mean value of V/V^* increases from 1.04 (note that Table 3 gives the ratio of the standard deviations) to 3.53; and the 90th percentile increases from 2.19 to 12.82.

In short, if West regards his variance estimate as "notably bigger" than the alternative, then he implicitly regards the small-sample bias that produces these different estimates under the null as "notable." In my judgment, he is overly optimistic about the robustness to small-sample bias of tests that account for unit roots.

I have similar reservations about West's conclusion that we must turn to nonstandard models for expected returns, such as fads, to explain stock price

volatility. In particular, returning to my original theme, I doubt that the rapidly accumulating evidence for nonconstant discount rates would have been sufficient to drive the literature to fads, absent the impact of the early variance-bounds papers. Work such as Fama and French [3] will continue to examine the relation of these non-constant expected returns to, say, the business cycle, and current models of expected returns may well be modified as in Constantinides [2] and Grossman and Laroque [5]. However, there is a large step from here to the conclusion that fads should be adopted as the model of stock prices.

The relative costs and benefits of pushing the bounds of standard models of expected returns, versus adopting a framework of fads, have been discussed elsewhere (Kleidon [8]). Clearly the particular fad framework adopted will determine how much of current knowledge is lost, but equally clearly much is potentially at stake—see, e.g., the anecdote I cited [8, p. S492]. Further, the fact that the tests currently proposed have little power to distinguish between quite different models places relatively greater burden on priors concerning the best framework with which to address stock prices.

Whichever way the literature goes from here, we are unambiguously further ahead than we were seven years ago. We have a much clearer idea of the statistical pitfalls in this area, and there is an emerging consensus that non-constant discount rates have manifested themselves in a variety of ways. What is left—which is non-trivial—is the source of the non-constant expected returns.

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