Robert C. Merton's "consumer services" model to the issue of equilibrium asset returns under uncertain inflation.

The authors begin by pointing out that despite its tremendous popularity as a model of equilibrium returns, the Arbitrage Pricing Theory of Ross suffers from a major deficiency—it offers no economic theory for identifying the factors in the linear return generating process. EGR explore the theoretical rationale for a two-factor APT model characterizing real equilibrium returns in which the first factor is a "market factor" and the second an "inflation factor."

Merton (1973) has developed a general multifactor model for describing equilibrium returns along the lines suggested by EGR. In the Merton framework, consumer/investors are assumed to maximize the expected utility from their lifetime consumption. Because they are risk averse they have hedging demands for assets which arise from the desire to eliminate adverse stochastic shocks to their optimal consumption stream which can arise from many sources. To the extent that an asset is valuable as a hedge against a risk facing an investor, he will be willing to accept a lower expected rate of return on the asset. In equilibrium, that asset's expected return will be lower (higher) if the net hedging demand in the aggregate (i.e., summed over all investors) is positive (negative).

EGR's discussion of the theoretical reasons for us to expect the equilibrium mean real return on an asset to reflect an inflation factor in addition to a market factor seems to fit quite neatly into Merton's analytical framework. Indeed, two previously published papers have applied Merton's model to derive some of the same results as EGR do. Specifically, my paper (Bodie 1982) derives the Friend, Landskroner, Losq results in real terms and shows how the two-factor model in nominal terms collapses into a single-factor model in real terms. Fischer (1975) uses Merton's framework to derive explicit hedging demands and mutual fund theorems when different consumers are concerned with different price indexes.

I believe the main original contribution of EGR's paper is its demonstration that an arbitrage pricing model with two non-zero factors can be consistent with a single-factor mean-variance model of equilibrium yield relationships. I agree entirely with them that this result should serve as a cautionary note against interpreting the presence of multiple-priced factors in the APT framework as evidence against the single-factor CAPM in the absence of a real riskless asset.

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## DISCUSSION

DAVID M. MODEST*: This paper by Kane, Rosenthal and Ljung follows the work of Fama (1975) in examining the adjustment of nominal interest rates to expected

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inflation. In that piece, Fama presented evidence that was not inconsistent with the joint hypothesis that the real pre-tax rate of interest is constant and nominal interest rates incorporate rational expectations about future inflation. One of the criticisms raised by Nelson and Schwert (1977), among others, about Fama's work has been the low power of his tests. The authors, in this paper, attempt to provide a more powerful test of the Fisher hypothesis using data from six countries, including the U.S. Their main contribution is to shed some light on whether nominal interest rates have fully adjusted to reflect expected inflation for countries other than the U.S. While this provides us with a greater quantity of evidence, the main shortcomings of the paper is that the authors could have provided us with better quality evidence.

Four sets of tests are performed using data from the flexible exchange rate period. One natural question to ask is why the authors limited their sample to this period. There is no obvious reason why the Fisher relationship should hold up less well in a fixed exchange rate period. By restricting the sample size, the authors may have diminished unnecessarily the power of their tests. One area for future research would be to perform these tests over a larger sample and to reconcile any differences.

The first set of tests, based on the Box-Pierce $Q$-statistic, examines whether realized real rates of return exhibit serial correlation. Under the joint null hypothesis of a constant real rate and rational expectations of inflation, all autocorrelations should equal zero. While the authors find significant serial correlation (10% level)[1] with monthly data for 4 of 6 countries, they find only one country with significant serial correlation using quarterly data. The authors choose to attribute this difference to possible seasonality in the data and seem to place more weight on the quarterly results. However, this confidence seems misplaced in the light of the small number of quarterly observations (21) and the unknown small sample properties of the $Q$-statistic which is *asymptotically* distributed as Chi-squared. Indeed, an examination of the quarterly results in Table 3 indicates, quite implausibly, that for only one country can the hypothesis that inflation is serially uncorrelated be rejected. My skepticism is reinforced by the results of Mishkin (1982a) who found, over a larger sample (49 quarterly observations), that for 6 of 7 countries he could reject the hypothesis that all autocorrelations equal zero.

The second set of tests are regressions of inflation rates on a constant, and nominal interest rates. Under the joint null hypothesis, coefficient $b$ in their Equation 4 should equal one. The most efficient test of this parameter restriction would have been to jointly estimate Equation 4 for all countries at once and to test whether all slope coefficients equal one. The authors, however, chose to test the parameter restriction one country at a time. This failure to implement joint estimation procedures also detracts from the third and fourth sets of tests. Nonetheless, in most cases they are able to reject the hypothesis that $b$ equals one. However, as pointed out by the authors, the estimated slope coefficients are likely to be downward biased due to a positive covariance between real rates and

[1] Using an adjusted $Q$-statistic, suggested by Ljung and Box (1978) to have better small sample properties, the null hypothesis can be rejected 4 of 6 times at the 5% level using monthly data. However, there is still only 1 rejection at the 10% level using quarterly data.

nominal rates (Equation 6). This makes interpretation of these rejections difficult. It is interesting to note that the ordinal ranking of the coefficient estimates in Table 4(a) is consistent with the predicted bias given the estimated correlations between real and nominal rates presented in Mishkin (1982b). Those countries with the highest correlations have the lowest estimated slope coefficients.[2]

The third set of tests also involve regressions of inflation rates on an intercept and on nominal interest rates. In these regressions, the intercept is allowed to follow a random walk.[3] The authors suggest this procedure as one method of correcting for equilibrium phenomena such as the Mundell (1963)–Tobin (1965) effect. However, this method of estimation assumes the steps of the random walk intercept are orthogonal to changes in nominal rates—a condition which will be violated precisely when such phenomena are important. For this reason as well as others cited by the authors, it is difficult to draw conclusions about the estimated slope coefficients. In addition, the authors extract estimates of expected real rates and test whether these are constant over time. For 3 of 6 countries they cannot reject constancy, and for the others they claim there is little variability. An interesting area for future research, would be to reconcile these results with those of Mishkin (1982a) who found strong rejection (using joint tests) of the constancy of the real rates for seven countries over a different period.

The final set of tests utilize cross-sectional regressions. Sixty-five regressions were run with each cross-section consisting of 24 observations (4 maturities and 6 countries). In this specification, it is unclear why the authors chose to model differences in expected real rates and default premia across countries as random effects. Especially with respect to default premia, a fixed effects specification would have seemed more natural. Further, it is also unclear to me why one would expect, in any given month, for unexpected inflation across countries to be uncorrelated—an assumption which is made. To summarize the individual regressions, the authors present average test statistics for coefficient h in Table 5. A problem with this is that the average standard error of h in no way corresponds to the standard error of the average estimate of h. One way around this problem would be to run a pooled cross-section time series test and jointly test the parameter restrictions. One final question I have about the presentation of these results is why the authors chose not to report $Q$-statistics of the autocorrelations in Table 6. The implicit null hypothesis is that all autocorrelations equal zero. In fact, if one calculates the appropriate $Q$-statistics, the null hypothesis can be rejected for 5 of 6 countries at the 5% significance level.

The authors have made a welcomed first step in examining the Fisher hypothesis on a broader set of data. However, more work remains to be done.

[2] Mishkin's estimates of the correlations over the 1967:2–1979:2 period were: U.S. (.21), U.K. (−.02), Switzerland (.53), West Germany (.56), France (.87) and Holland (.75).

[3] The authors fail to clearly describe or reference the exact estimation procedure. Thus the following comments are based, perhaps erroneously, on the standard methods used to allow for random intercepts, such as those implemented by Fama and Gibbons (1982).

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## DISCUSSION

SUSAN M. WACHTER*: Myron J. Gordon's paper has an ambitious purpose. It attempts to explain the effects of inflation and real factors on the performance of the U.S. stock market, over the years 1960 to 1980. In doing this, it addresses the empirical finding of the absence of a positive short-run correlation between inflation and a share's holding period rate of return in the U.S.[1]

There is no widely accepted explanation for the negative correlation observed between stock market values and inflation (anticipated and unanticipated). However, plausible explanations, some of them competing and some complementary, have been offered. Gordon's paper attempts to evaluate several of the more important of these. That he does not successfully resolve the controversies raised does not mean that he has not furthered the debate.

Indeed, he has, with two contributions. First, he has set out a valuation equation for Tobin's $q$, the ratio of the market value of equity to its replacement cost, which is sufficiently general and inclusive that it can be used to evaluate a number of theories. Second, he has amassed the data on the many terms that go into the valuation equation for 3 bench mark years of 1960, 1967 and 1980. The valuation equation and 3 data points serve as an organizing schema to evaluate hypotheses that may account for the relationship between inflation and stock values over this period.

With these, he is able to show the following: First, Feldstein's [3] explanation for the negative relationship, which relies on the use of historical cost accounting for tax purposes and the taxation of nominal capital gains, can only account for a very small portion of the decline in the ratio of the market value of equity to its replacement cost. Indeed, expanding Feldstein's equation to include retention

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[1] In a comprehensive study, Gultekin [6] finds a lack of positive relationship between inflation and stock returns in the majority of twenty-six countries investigated.