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The Effect of Inflation on Stock Prices: International Evidence: Discussion

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regions of the parameter space. In the papers discussed here, the movements in p_t indeed appear to be too big. Figure 1 illustrates how dramatically our stock price and dividend data appear to violate one of the inequalities, (I-2). Roughly speaking, if the efficient markets model holds, and if the large movements of Δp are to be justified in terms of information about future d , then the d_t series ought to range over an area equal to the space between the dashed lines, while in fact it appears confined to a fraction of the area. Commonly used hypotheses that have been advanced to reconcile this data with efficient markets: a random walk model for d_t or a disaster theory, do not appear to be promising. Other possibilities are that ex ante real interest rates show very large movements or, alternatively, that markets are irrational and subject to fads.

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DISCUSSION

JOHN B. LONG, JR.*: Cohn and Lessard present some international evidence on the joint behavior of price-earnings ratios, nominal interest rates, and inflation. For most of the countries examined, their evidence is qualitatively similar to evidence for the United States presented in 1979 by Modigliani and Cohn. In a broader context, the regularities reported by Cohn and Lessard are apparently

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related to regularities in the joint behavior of inflation and asset *rates* of return that have been reported by many authors over the last five years.

In interpreting their evidence, Cohn and Lessard express greatest enthusiasm for the hypothesis that investors in many countries other than Germany systematically make two specific types of valuation errors. Without disputing the contention that their regression estimates are consistent with this hypothesis, I would say that their enthusiasm is at least premature, if not entirely misplaced.

The valuation error hypothesis has empirical implications that are not examined by Cohn and Lessard. The simplest of these is that *ex post* real rates of return to shareholders should, on average, be *positively* related to *ex ante expected* inflation rates. The same implication would apply to assets other than common stock. As stated, however, the hypothesis is that, for some reason, investors *only* make the discounting error when valuing common stock. In any case, the implication for common stock is significantly contradicted by all recently published evidence on the joint behavior of stock returns and inflation.

The alleged failure to account for declines in the real value of nominal liabilities also has a very straightforward empirical implication. Controlling for the effect of leverage on systematic risk and hence the capitalization rate, high *expected* rates of inflation should have the greatest negative impact on the values of firms with relatively high levels of nominal liabilities. The real rates of return on these firms should be most sensitive to expected rates of inflation. Unfortunately, there is less currently available evidence on this cross-sectional relation than on the time series relation between expected inflation and real returns. In a recent working paper, however, Ken French, Richard Ruback, and Bill Schwert find that, controlling for systematic risk, nominal liability measures have no marginal power in explaining cross-sectional differences in the sensitivity of real returns to expected inflation.¹

I have cited some available evidence which is *not* consistent with the hypothesis that investors make the kind of valuation errors Cohn and Lessard describe. This still leaves open, of course, the question of why price-earnings ratios seem to be inversely related to expected inflation and nominal interest rates. Given time, I suppose I could come up with a number of rational valuation scenarios in which this inverse correlation would be present. Many more have probably already been suggested to the authors. I don't think this is a very interesting exercise, however. I feel this way because I am uncomfortable with the very concept of price-earnings ratios. Only in a rather special valuation model is there a simple unambiguous formula for the rational value of the ratio—a formula that expresses the rational ratio in terms of a manageable number of parameters. In spite of the widespread popular use of the term, it is not even obvious how price-earnings ratios ought to be measured—especially if the “earnings” are to represent something called “expected long-run earnings” or “current earnings that are expected to grow in real terms at a constant rate”. One is immediately forced into assuming an uncomfortably narrow range of possible time profiles of expected future real earnings. This is perhaps the main theoretical limitation of the price-earnings ratio concept.

¹ K. French, R. Ruback and G. W. Schwert, “Effects of Nominal Contracting on Stock Returns,” Working Paper, Graduate School of Management, University of Rochester, June, 1980.

Leaving aside problems of interpreting and measuring price-earnings ratios, most of the explanatory variables in the alternative hypotheses are expectations and these, of course, are also hard to measure. In this case, however, there has been considerable recent success in developing measures of at least such things as expected inflation rates and the expected market risk premium. Cohn and Lessard don't use these new measures. If they had used them, I would not be surprised to see an even stronger partial correlation between price-earnings ratios and inflation. Until equally good measures of expected future real activity are available, however, price-earnings ratio regressions will continue to be consistent with both rational and irrational valuation hypotheses.

If you test a crude valuation model using crude empirical proxies for its variables and if you find, as in Cohn and Lessard's evidence, that your regressions don't seem to be very well specified, it's hard to justify attaching much economic significance to the coefficient estimates—whatever they may be. Especially given the existence of the other evidence I mentioned earlier, acceptance of the valuation error hypothesis on the basis of these regressions seems entirely unwarranted.

Bob Shiller's paper demonstrates that a particular kind of asset pricing model implies both serially uncorrelated rates of return and upper bounds on the volatility of price movements. Some empirical evidence, in turn, indicates that observed price movements violate these volatility bounds.

I have no *a priori* reason to question the accuracy of these empirical measurements. Thus, I am willing to provisionally reject the hypothesis that asset prices behave according to the kind of model Bob discusses. I do not believe, however, that this has much, if anything, to do with the efficient markets hypothesis.

Ten to twenty years ago, the constant expected return or expected payoffs discounted at a constant rate kind of model was indeed characteristic of most interpretations of the efficient markets hypothesis. This simple pricing model lay behind most of the "fair game", "random walk", and "martingale" theories of price behavior and it motivated very extensive-theories empirical measurements of level of serial dependence in rates of return. Having thought and written more about it over the last ten years, however, we should clearly recognize how special are the assumptions required, *in addition to* the efficient markets hypothesis, to rigorously deduce the proposition that price should equal expected future dividends discounted at a constant (or nearly constant) rate. I believe that most of us do recognize this, at least most of the time.

Having seen at least several examples in the literature, we can readily construct multi-period rational expectations equilibria in which assets have stationary dividends *and* rates of return that are stationary but highly autocorrelated. As a simpler example, imagine a set of securities, each of which has stationary dividends and stationary rates of return that exhibit *no* serial dependence. Assume also that, because of risk differences, the mean rates of return of these securities are not identical. Finally, imagine an open-end mutual fund that only trades in these securities. Now it is easy to construct a management rule for this fund such that the fund's rate of return series is stationary *and* autocorrelated. Are the shares of this fund inefficiently priced? By construction, they are not.

In both of the examples just given, the variance of price changes may be larger than allowed in Bob's model. The empirical measurements that Bob reports

simply confirm the opinion that the special assumptions he makes are too special for some purposes.

Are there any empirical implications deducible from the efficient markets hypothesis *without* the support of additional arbitrary assumptions? Without committing myself too much on what is “arbitrary”, I would say that there certainly are implications of this sort. As far as I can see, however, they *all* involve *comparison* of the price-dividend behavior of a given security or portfolio with *other* observations—usually the contemporaneous price-dividend behavior of other securities. How else is one to judge, for example, whether arbitrage profit opportunities exist and are systematically identifiable using contemporaneously available information? This kind of basic “efficiency” question simply can’t be answered by reference to the observed behavior of a single security or portfolio in isolation.

The price of a given security can, of course, be compared with a present value of its own predicted or actual future dividends. This comparison has no efficiency implications, however, unless the present value is computed in such a way that it is the contemporaneous market price of a related portfolio of other investments. This related portfolio is one whose future dividends are estimated, using available information, to be essentially identical to the future dividends of the security in question. In other words, the portfolio is estimated to be a one-for-one substitute for the security. Deviation of a given security’s price from the present value of its dividends computed in this way would be clear evidence of market inefficiency but such deviations obviously can’t be measured by looking only at the security in question.

Although I have questioned the relevance of the variance inequalities examined in this paper, I would like to emphasize the value of the hypothesis testing framework that Bob employs. Specifically, examination of the restrictions an hypothesis imposes on the sample likelihood function and derivation of the likelihood ratio test can clearly reveal the most restrictive empirical implications of the hypothesis. These implications are often not very obvious a priori. Bob’s pricing model provides a good example of this. Evidently, the variance inequalities implied by the model were not obvious to those of us who implicitly and explicitly asserted the model’s empirical validity long before Bob examined it. Had we viewed the model as a constraint on the likelihood function of a price-dividend sample, however, the variance inequality would have been forced to our attention. This is the essential virtue of a formal application of the likelihood principle. Even though the exact likelihood ratio test may be rather intractable, the qualitative information it conveys about the appropriate rejection region in the sample space is a valuable guide in selecting simpler tests.

DISCUSSION

JOHN LINTNER*: Shiller’s paper for this session summarizes and extends his recent work which has developed and exploited bounds on the volatility of speculative asset prices relative to the variance of critical elements of the information set, or to the rational expectations based on that information set, as tests of market efficiency. This work is making a highly constructive contribution

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