



## American Finance Association

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Gains from International Diversification: 1968-85 Returns on Portfolios of Stocks and Bonds: Discussion

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## DISCUSSION

MICHEL CROUHY\*: The paper by Robert Grauer and Nils Hakansson addresses a classical but still controversial empirical issue: Does diversification always pay? I like the paper and I am very sympathetic to the impressive empirical work which has been done, although more information is needed about your data set, especially the composition of the foreign portfolios and dividend adjustments in the various countries.

My comments will be directed towards two main questions:

- the underlying model,
- the risk of estimation,

and I will have a few minor additional remarks concerning the investment horizon.

First, the theoretical model. I agree with you that the mean-variance framework is clearly not suited to determine the optimal multiperiodic investment policy. Indeed, as shown by Merton (1973) and Landskroner (1977) the opportunity set is not stationary and investors cannot behave as if they were myopic. The only exception is, of course, for the case where the policy involved is the growth optimal policy associated with log-utility, as shown in Mossin (1968).

In general investors have to hedge against adverse shifts in the opportunity set. Hence, their optimal portfolio decisions do not only incorporate their estimation of current probability distributions, but also estimation of distributions to prevail in the future. This question relate to my second comment on the risk of estimation which I will address later on.

You rule out the continuous time portfolio model because it becomes intractable with transaction costs and taxes. But you do not introduce them either in your analysis, especially transaction costs. From my point of view, the continuous time model gives precious indications about transacting in a world where transaction costs do exist. Indeed, Constantinides (1986) has shown recently that large transaction costs are accommodated by investors in terms of expected utility, by drastically reducing the frequency and the value of trade. This last point is really important as the present paper discusses portfolio rebalancing on a quarterly basis. This type of revision might be sub-optimal due to transaction costs. The no tax assumption, on the other hand, can be accepted because of tax exempt investors.

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In your paper you adopt a third approach, the discrete time multi-period portfolio model. This is a powerful model, but the simple version presented here overlooks two important questions:

- consumption,
- state dependence of utility functions.

Consumption is especially important in cases where preferences are not intertemporally independent. A good reference on this issue is Sundaresan (1985). In this case, condition (2) does not hold any more. There is an interaction between past consumption decisions and current investment. Note, that the optimal investment policy equilibrium condition (2) is very intuitive as it merely reflects constant relative risk aversion.

State dependence of utility functions is also a tough question to deal with as the notion of risk becomes “individual,” since marginal utility depends on the state of the world occurring.

Let us now turn to the risk of estimation. Actual empirical results depend on both the investment risk and the risk of estimation. In your paper you adopt what you call the “simple probability assessment approach” which is just the famous “principle of insufficient reason” proposed by Laplace. You say that you resisted all temptations to reduce estimation risk via a Bayesian approach of the type proposed by Stein. The method you suggest might in fact lead to a very imprecise estimate of the probability distributions of returns. And it is now well documented that the estimation problem has far reaching and disastrous implications for the composition of optimal portfolios.

As reported by Jorion (1985) portfolios computed with estimates based only on past observations show positions with very large positive amounts (or negative amounts when short selling is allowed) that no prudent portfolio manager would accept to take.

This is precisely what you obtain. Part of your results can be easily explained: the optimization program selects massively the assets which have experienced in the recent past exceptionally high returns (for example the Japanese portfolio), even if they are not significantly large, due to statistical errors. For example the analysis of Table III along with Figure A would tend to be consistent with my comment.

Now, my own temptation would be to suggest to follow a Bayesian approach in order to overcome this selection bias. Your problem would still be to maximize expected utility, but in the continuous time portfolio approach with the assumption that the distribution of returns belong to the log-stable family, for example the log-normal which is the most practical one. In order to find the optimal solution I would try to extend the continuous time approximation or the Ohlson approximation. The estimation of the log-normal distribution would follow a generalized Stein procedure.

My last comments concern the investment horizon. In a TIMS study of 1979 Levy suggests some interesting tracks to follow. Levy uses as usual the stochastic dominance paradigm. According to Levy diversification almost always pays, but the longer the investment horizon, the smaller is the merit of diversification. This issue was already tackled theoretically by Goldman (1974), and a recent study by Cholerton, Piearaerts and Solnik (1986) confirms this finding. In your

paper only the 1968–1985 horizon is proposed. It would have been interesting to assess the impact of the horizon on the results. A correlary question to Levy's interrogation could be "does diversification pay more when portfolio revision is less frequent" or "what is the impact of allowing changes in the interval of revision."

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