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PARADOX OR AT LEAST VARIANCE FOUND: A COMMENT ON "MEAN-VARIANCE APPROACHES TO RISK-RETURN RELATIONSHIPS IN STRATEGY: PARADOX LOST"*

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> In general, the problem is that the computed mean-variance relationship for a period of time cannot be identified in distinction to the effects of shifts in the relationship over time—without additional information or assumptions. *Thus, using a mean-variance approach to risk-return relationships means that statements about the nature of the mean-variance association cannot be confirmed in a nontrivial fashion within the empirical system nor generalized to any other time period—including subperiods*. (Ruefli 1990) (emphasis in original)

In recent years, a number of researchers in strategy have examined the associations between returns (normally measured as return on equity) and risk (normally measured as variance in return on equity) (Bowman 1980, 1982, 1984), Fiegenbaum and Thomas (1985, 1986, 1988), Fiegenbaum (1990). The studies usually examine the association within an industry between firms' variance in ROE over a five-year period and average ROE for the same time period.

In a recent article, Ruefli (1990) argues that findings on the relation between means and variances are unidentified. He goes on to argue that statements about mean-variance relations cannot be identified and that any research in which mean and variance of a variable appear in the same relation is also meaningless. This is an important assertion because identification of means and variances is central to a large proportion of social science research.

Ruefli defines the issue in the following way. Given a set of firms S and a time period T which is divided into subperiods $t_1, t_2, \ldots, t_n$, $n > 1$, can statements be made about the relation of u_i the mean return for firm i in period T and v_i the variance in returns for firm i in period T? Ruefli's argument is that if we divide the subperiod t_i into sub-subperiods (his terms), the relation that was found for the aggregate data does not necessarily hold within the subperiod data. From this, he argues that "information in the system is not sufficient to validate statements about the mean-variance relation in the system."

It is not completely clear how the proof provided in the paper works. It begins by setting up conventional means and variances by companies (u_i, v_i, respectively) where the means and variances come from data over a number of years. Then a relationship R between u_i and v_i is calculated using these means and variances. "The problem is, given a relation R for T [the period of time being examined], to determine if R is a result of elements drawn from a single mean-variance relation and thus is an accurate identification of the mean-variance relation, or if R is a result of elements drawn from a series of mean-variance relations that have resulted from shifts in an underlying distribution of returns over time, and thus is an artifact." (Ruefli 1990, p. 371).

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We are not clear as to exactly which distribution of returns over time is being considered. Two returns distributions are possible. First, the joint distribution of risk and return, i.e., R, the association between risk and return, may be changing annually. Second, the distribution of returns at the firm level may be changing. That is, $u_{i,j}$, the returns for firm i in period j, instead of being distributed with a constant mean and variance have a distribution wherein the mean and variance change every year. Both possibilities will be considered and then the meaning of the period versus subperiod results will be considered.

If the argument is that R, the relation between risk and return varies over time, then the following model may be appropriate:[1]

$$u_{i,t} = \alpha_t \text{TrueRisk}_{i,t} + e_{i,t} \tag{1}$$

where $u_{i,t}$ is returns for firm i in period t, $\text{TrueRisk}_{i,t}$ is the actual risk (as distinct from measured risk) for firm i in year t, α_t is a parameter which varies by year, and $e_{i,t}$ is an error term. As is conventional in this literature, TrueRisk refers to uncertainty of the returns or income stream uncertainty rather than some other concept of risk (Miller and Bromiley, 1990). If instead of equation (1) we average both firm returns and risk over time, we would estimate an α that would represent an average relation between risk and return. This would be subject to the normal aggregation and measurement error problems but is interpretable.

A far more conventional assumption would be that the relation between risk and return is stable over time, i.e., that α_t does not vary with t. In this case, we can replace both $u_{i,t}$ and $\text{TrueRisk}_{i,t}$ with sample estimates of mean and variance for firm i. Here aggregation is not a problem, but measurement error remains. That is, sample mean and variance deviate from true mean and variance, but this is not a substantially worse problem here than in any other conventional economic or strategy research.[2] In most work in the area, measurement error problems may be attenuated somewhat by the use of nonparametric statistics.

The other possibility is that Ruefli intended changes in the underlying distribution of returns to mean changes in the distributions of the $u_{i,j}$ themselves. That is to say, the aggregate risk-return relations might be unidentified because the variables being used are unidentified. It is important to recognize what this argument really implies. Although discussed in the context of the risk-return literature, if mean and variance are unidentified in this context, there is no reason they should not be unidentified in most other cases which implies that any work which depends on the simultaneous estimation of a mean and variance is invalid. This would include any t-tests for variables being nonzero, and a host of other standard techniques.

Identification is a property of the model being tested, or as Simon (1977, p. 71) argues "identifiability of a linear structure is obtained when certain a priori constraints are placed on the model." That is to say, theoretical assumptions about underlying relations identify the model. For example, whenever we take a mean, we are assuming that the structure is constant over the observations. If we do not assume that the parameters are constant across observations (or at least that they are drawn from some identifiable distribution), then there is no way to identify the parameters.[3]

[1] We assume a linear model here for convenience but there is no substantive reason the risk-return relation should be linear. Many studies of risk-return relations use less restrictive nonparametric tests instead of the linear parametric representation presented here but this does not change the logic of the argument.

[2] Almost all empirical work in economics and strategic management uses measured values for variables in place of theoretical constructs without serious consideration of measurement error problems. Of course, in the risk area one would want to consider seriously whether variability of returns constitutes the correct concept of risk for a particular study (see Baird and Thomas 1985; Miller and Bromiley, 1990).

[3] Part of the difficulty may arise because one could argue that sample means and variances are really functions

Let us attempt to be clear under what assumptions the mean-variance approach to risk would make sense. First, one assumes that returns for firm i over some period t are normally distributed with mean u_i and variance σ_i^2. Firm returns need to be distributed normally so that mean and variance are not related by the nature of the distribution.

Second, *across firms* u_i and σ_i^2 are hypothesized to be statistically related. The distinction between within and across firm relations is critical. Normal distribution theory indicates that there is no necessary relation between mean and variance for a specific normal distribution, but it says nothing about the values of means and variances across a population of normal distributions. That is to say, normal distribution theory indicates that mean and variance are parameters provided to the distribution and that knowing one does not tell us about the other if we are only looking at one distribution. On the other hand, *the risk-return research is looking at mean-variance relations across a number of different normal distributions*. The entire approach is based on the hypothesis that the process that generates the parameters for the firm returns distributions has some connection between mean and variance. Normal distribution theory says nothing about the way in which mean and variance parameters are generated for a population of normal distributions.[4]

Third, as is done in almost all economics and strategy research, the approach ignores the issues of measurement error, i.e., it uses the sample estimates in place of true values. As noted above, this results in some biases.

Both interpretations of Ruefli's statement concerning the stability of the distribution of returns over time come down to the same issue. Ruefli does not want to allow the assumption that more than one observation is drawn from a particular distribution. If researchers are not willing to make the assumption that a given distribution or joint distribution has generated multiple observations, then all statistical analysis is impossible. In the regression example above, even if the true parameters for annual risk-return relations vary, the aggregate equation has a meaningful interpretation. If the assumption is being rejected that a firm's returns have a constant mean and variance over any time period then all statistical analysis using means and variances is precluded.

In the case addressed by Ruefli, estimation of means and variances in corporate profitability data, a legitimate *empirical* issue can be raised over whether mean profitability for individual firms is reasonably constant over time. This assumption can be tested in a number of ways. Let us assume some of the observations are drawn from a particular distribution (for example, the middle observations in the time series). The estimate of mean and variance from these observations can be used to test whether the other observations (e.g., the first and last observations) are drawn from the same distribution. If we assume equal variances, we can test whether the mean of the first half of the data differs from the mean of the last half. If we are willing to use a sufficiently long time period, we can test whether the variances differ between early and late data. If we are not willing to assume the variance of ROE is stable over say five years, these tests do raise the Behrens-Fisher problem but a number of procedures are available to handle such issues (DeGroot 1975). If we are willing to hypothesize that the change in the mean follows a particular functional form (e.g., that it is autocorrelated), this can be tested. There are many ways

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of the same underlying variable so that studies using mean and variance really only have one variable. This is not correct since relations between functions of the same underlying variable can be identified. That is, X and X^2 are both functions of X but we have no identification problems in including both as independent variables in the same regression, nor, indeed, in regressing X on X^2. Likewise, mean and variance are also different functions of the same underlying variable but count as different variables for identification purposes.

[4] Testing equality of means and variances across firms does not settle the matter because the assumption that firm returns are distributed $N(\mu_i, \sigma_i^2)$ does not have any implication for the magnitude of differences in μ_i and σ_i^2 across firms.

to address the empirical issue whether it is reasonable to assume returns have a constant mean over a moderate length of time, but all of the techniques assume the mean and variance are constant during subperiods within the data.

In fact, a number of different approaches have been used to cross-check the results of using variances to measure risk. The use of variances assumes that means are constant over time. Wiseman and Bromiley (1991) examine whether possible serial correlation in the returns series influences previous risk-return results; it does not. Miller and Bromiley (1990) empirically examine the measurement properties of a number of risk measures including variances in returns. They find variances in returns load into the same risk factor as other, independent measures of income stream uncertainty (variance and coefficient of variation in stock analysts' forecasts of earnings per share), and that the risk-return relations found cross-sectionally also hold up when risk and returns from one five-year period are used to explain a subsequent five-year period. Bromiley (1991) finds that risk-return relations also show up strongly using annual data and variance in stock analysts' forecasts of earnings per share to measure income stream uncertainty. Finally, numerous studies find that income stream variability measures are related to substantive measures of corporate risk-taking (see, for instance, Amit and Livnat 1988).

The final portion of Ruefli's argument is based on the relation between risk estimates in annual data and risk estimates in quarterly or monthly data. The appropriate time frame for testing a theory is a form of the "level of analysis" question. That is, a given theory or practical issue should imply that a given time scale is appropriate.

Theories do not need to hold across levels of analysis. Theories that make sense for individuals may make no sense for organizations. A theory of daily behavior may be irrelevant to yearly behavior. Consider personal cash flows. A daily theory would center on payday, rent, and credit card payments while an annual theory would consider life cycle earnings, aggregate commitments, and so forth. These are different phenomena and clearly should be handled differently.

The form of Ruefli's argument is that disaggregating from annual to subperiod data changes the relation. This is absolutely true and absolutely irrelevant. What level of data is appropriate depends on the level of theory one wishes to test.[5] Note that income per se is an aggregate over time (an aggregation of numerous individual transactions).

To make this clearer, let us consider a common and simple example. Consider a lawn service company. If the firm's annual returns for five years ranged between 7 and 7.1 percent, we would feel confident in saying the mean annual returns are nonzero and are not terribly variable. If we did the same thing with quarterly data that ranged from −15 to +30 percent, we might not feel confident in rejecting the null hypothesis and would consider the returns variable. Quite clearly, the questions one would ask using the annual data are substantively different from those one would ask using the quarterly data. The annual data might speak to strategic issues, and the quarterly to tactical issues of cash flows.

To summarize, we argue that Ruefli's conclusions rest on two threads. One thread, the annual versus quarterly results, is simply irrelevant. The second comes from a rejection of the assumption that certain probability distributions (either single variable or joint distributions) are constant over multiple observations. Almost all empirical research makes such assumptions. Without such maintained assumptions, it is not clear that statistical analysis is feasible. Rather than a blanket rejection of mean-variance estimates, it would be more interesting and productive to ask the question in a constructive fashion such as (i) is it reasonable to assume annual means in returns and the variance of such

[5] Indeed, for strategic management research, making individual observations cover more than one year may be most appropriate.

returns are constant over five-year periods? (ii) is annual data the correct level of analysis or should shorter or longer time periods be employed? and (iii) how can the theoretical positions presented in this research be more powerfully tested?[6]

[6] I wish to acknowledge comments by David Kelton, Kent Miller, Elaine Mosakowski, Chris Nachtsheim, and referees on earlier versions of this note, and funding from the National Science Foundation's Decision, Risk and Management Science Program (grant SES-8811811). I retain responsibility for the arguments presented in this paper.

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REPLY TO BROMILEY'S COMMENT AND FURTHER RESULTS: PARADOX LOST BECOMES DILEMMA FOUND*

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Reply

In the last paragraph of his comment on my critical analysis (Ruefli 1990) of the mean-variance approach to estimating risk-return relations, Bromiley (this issue) selected the following two grounds in the article for questioning that approach: (1) differences among mean-variance relations in periods and subperiods, and (2) the types of assumptions that must be made. The former he dismissed as irrelevant, while the latter he maintained are necessary concomitants of empirical work, and implied that any problems

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