# Risk-Return Tradeoffs for Strategic Management

Terry A. Marsh — Massachusetts Institute of Technology
Douglas S. Swanson — National Bank of Detroit

In an article appearing in the Spring 1980 issue of the *Sloan Management Review*, Edward Bowman presented a study of the empirical association between risk and return at both firm and industry levels. He concluded that, in apparent contrast to the body of economics and finance literature, a negative relation holds between risk and return within many industries. In this article, the authors question this result. They point out weaknesses in Bowman's analysis, present a preferred analytical methodology, and explore the interpretation of the empirical risk-return relationship at the firm level. A response from Bowman follows the article. *Ed.*

Investors owning common stocks earn returns in the form of dividends and stock price appreciation. If investors are risk averse, those stocks must be priced so that the riskier they are, the higher the expected returns will be. For example, since 1926 the portfolio of NYSE common stocks has been priced to return, on *average*, about 8.0 percent per annum more than riskless Treasury Bills.[1] If investors hold well-diversified portfolios of assets, and if they measure risk in terms of the variance of the returns on these portfolios, then the assets that contribute most to the variance of the portfolio returns must be priced to yield the highest expected returns.

In his article, "A Risk/Return Paradox for Strategic Management" (published in the Spring 1980 issue of the *Sloan Management Review*), Bowman contends that ". . . both business administration and economic theory and literature . . . maintain that there is a positive association between risk and return . . . at the level of the firm and industry [as well as in the] stock market."[2] In his empirical analysis, however, he finds that in apparent contrast to the positive risk-return tradeoff observed at the capital market level, ". . . in the majority of industries [he] studied, higher average profit [return on equity (ROE)] companies tended to have lower risk, i.e., variance [of ROE], over time."[3] That is, he reports finding a negative tradeoff between average earnings and risk at the firm level within industries, and consequently, he concludes that the evidence "at the firm level" is paradoxical with respect to the received business and economics literature.

In this article, we argue that Bowman's finding of an apparent paradox is a reflection not on the business and economic literature, but rather on his tests. This distinction is significant because, as Bowman himself states, the role of "good management . . . [is to] bring about both higher returns and lower variance. . . ."[4] It would be more than a little disconcerting if the business literature were at odds with good management!

If investors have only the opportunity to invest in stocks that yield high returns as a reward for bearing a high degree of risk, they obviously would be delighted with an announcement by any one firm that it had found a new high return-low risk project — what manager would not like to be able to make such an announcement? Yet, ignoring for the moment any errors in the measurement of earnings and costs, it is implausible that such high return-low risk projects could exist generally while, at the same time, average stock market returns were positively related to risk — hanging, as it were, by their own bootstraps. In the absence of some impediment, an equilibrium should be reached in which the positive relation between risk and expected return on stocks reflects the relation between risk and return on firms' marginal projects. An empirical result that, across firms, lower risk projects yield, on average, high expected returns would certainly constitute an anomaly, if not a paradox, from an equilibrium point of view.[5]

Although Bowman offers his results as evidence that a negative relation between risk and expected return does generally hold over time and across firms within an industry, there are many potential statistical problems in his contingency table tests, as we will discuss later in this article. When we use a test for cross-sectional relations between true means and true variances, which is developed and applied in Marsh and

Table 1 Two by Two Contingency Table

| | | ROE Variance | | |
|---|---|---|---|---|
| | | High | Low | Row Total |
| Mean ROE | High | $n_1$ | $n_2$ | $N_M^H$ |
| | Low | $n_3$ | $n_4$ | $N_M^L$ |
| | Column Total | $N_V^H$ | $N_V^L$ | $N$ |

Newey,[6] we find that even after taking out the generally positive comovements in ROEs across companies and industries, there are as many industries displaying positive correlation between ROE means and variances across companies within the industry as there are industries displaying the negative correlation reported by Bowman. The results seem to hold, with equal force, for the industries in which Bowman found the strongest *negative* relation between average ROEs and variances of ROEs.

Nothing in our results rules out the possible existence of high expected return-low risk projects for some firms at some times. Indeed, finding such profitable projects is one of management's primary functions. But, while finding obviously profitable projects might be tough, deciding to undertake them once they have been found is not. Instead, the difficult decisions center on when to accept, or to reject, less profitable (marginal) projects whose expected returns do, or do not, meet the minimum level required for the amount of risk they impose on the firm's investors given their alternative opportunities. While strategic, economic, and industrial organization analyses have an important role to play in these marginal project decisions,[7] managers must thoroughly understand the risk-return relation reflected in stock market prices if they are to know when it is in the investors' best interests to accept, or to reject, marginal investments. The counsel that good projects are those with the highest returns and lowest risk offers little help to management in finding such projects, or even in knowing what constitutes *high* return or *low* risk, let alone in making accept/reject decisions on the marginal projects that fall short of obvious golden opportunities.

## Bowman's Tests and Results

### A Contingency Table Test

Bowman's results are based on two different data sets. The first set contains 387 companies from eleven industries; two industries are studied for strategic management purposes and nine are chosen randomly from *Value Line* surveys. He calculates ROE means and variances for these companies for the five-year interval 1972–1976.

Tests of association between the ROE means and variances are based on a two by two contingency table analysis. The ROE means and variances are classified as high or low relative to their sample median values as indicated in Table 1.

As used here in Table 1, the (sample) mean ROE and (sample) variance of ROE are associated if higher (or lower) mean ROE occur more frequently among the higher or lower category of variance of ROE. Thus, Bowman considers mean and variance unassociated if

$$\frac{n_1}{N_V^H} = \frac{n_2}{N_V^L} = \frac{N_M^H}{N}.$$

Bowman finds that the sample mean ROEs and sample variance of the ROEs are negatively correlated across the firms in ten industries and positively correlated in the other one. He reasons that, if there is no relation between risk and return, half of the correlations should be negative and half should be positive (ignoring those firms with zero correlation). Thus, using a binomial test, he concludes that the ten negative correlations out of eleven industries are significant.[8]

In his second sample, Bowman studies 1,572 firms in eighty-five industries covered by *Value Line* over the nine-year interval from 1968 to 1976. To measure the correlation between ROE mean and ROE variance, he again forms two by two contingency ta-

Terry A. Marsh is Assistant Professor of Finance at the Sloan School of Management, M.I.T. Dr. Marsh holds the B.Comm. degree from the University of Queensland and the M.B.A. and Ph.D. degrees from the University of Chicago. His principal fields of interest are capital markets, corporate finance, accounting, and econometrics and statistics. Dr. Marsh has published articles in such journals as *Journal of Finance*, *Journal of Financial Economics*, and *Review of Research in Futures Markets*.

bles of these sample estimates. The sum of the values in the low/high and high/low quadrants of each table is divided by the sum of the values in the high/high and low/low quadrants. A quotient greater than one would indicate negative correlation. He finds fifty-six industries with negative correlation, twenty-one with positive correlation, and eight with no correlation. He implies that these results again represent significant negative intra-industry correlation (using a binomial test) between ROE mean and ROE variance.

Finally, using his first sample, Bowman forms one contingency table using all 295 firms from all the nine *Value Line* industries that he studied. This contingency table displays slight negative correlation, but not at a significant level. Also, using his second sample, he forms a contingency table based upon rank orders of industry ROE mean and ROE variance. Finding insignificant negative correlation there as well, he concludes that there is negative correlation between ROE mean and ROE variance within an industry, but not on an aggregate basis.

## Errors in the Bowman Methodology

Since the sample means and variances of ROE can be measured cardinally for each firm in an industry, it is somewhat unusual to throw away information by classifying firms into only high and low classes of ROE mean and ROE variance, and then performing categorical data analysis. The impact is more than methodological or statistical. Whereas even rank order methods provide some information about the monotonicity of a relation between variables, the two by two contingency table test can only tell us whether there is some *unspecified departure from independence* or association between ROE mean and ROE variance (see Table 1). In particular, we cannot conclude from Bowman's results that if firm A has a higher mean ROE than firm B, then firm A will have a lower variance of ROE than firm B. If his contingency table tests were correctly specified, we could, at most, conclude that if firm A's mean ROE is *above the median* for companies in its industry, then it is slightly more likely that its ROE variance will be below the industry median. Hence, even if all average ROEs of firms in Bowman's sample were good measures of expected ROEs, his results are not "parametric" enough to lend much assistance to decision makers.[9]

However, a serious methodological problem exists in the use of contingency table tests to establish Bowman's results. These tests are typically used to investigate the relation between two random variables. But they are used here to test for the association between two sample moments of the ROE distribution computed from the *same* sample of observations. We know from statistical theory that only for samples drawn from a *normal* population will the finite sample mean and variance be independently distributed (i.e., unassociated).[10] It is unlikely that earnings or deflated earnings series are normally distributed and, not surprisingly, studies of the probability distribution of financial statement ratios have consistently reported departures from normality in the direction of positive skewness for ratios like ROE.[11] And while it might not be appropriate to apply asymptotic results to Bowman's samples (see below), it is worthwhile noting that for both the gamma ($r > 2$) and lognormal distributions, which would be typical of those found empirically, the asymptotic mean and variance estimators are *negatively* correlated (unless they are centered about a "center of location").[12] In the absence of other problems with the categorical analysis, this means that Bowman's paradox may arise purely from his statistical methodology.[13]

Also, to interpret Bowman's results, his sample size and the independence of his sample observations must be considered. In the first of his two samples described above, his analysis is based on ". . . each company's average profit and the variability of its profits over the five-year period, 1972 to 1976."[14] In the second sample, his analyses ". . . use a nine-year period (1968–1976) for ROE mean and variance rather than a five-year period."[15] Assuming the sampling interval to

Douglas S. Swanson is a fixed income analyst in the Trust Investment Division of the National Bank of Detroit. Mr. Swanson holds the B.S. and the M.S. degrees from M.I.T.

be annual, this means that the sample moments of each company's ROE distribution are computed using only five and nine observations respectively. This sampling error is not accounted for in the contingency table analysis as it refers to association between point estimates of the mean ROE and variance of ROE.

To illustrate the magnitude of the sampling error, we applied the Bienayme-Chebycheff inequality to the first three companies in our sample described below, over the five-year period 1972–1976. For these three randomly chosen companies, the mean ROE and its upper and lower 95 percent confidence intervals, respectively, are (0.177, 0.302, 0.051), (0.051, 0.0187, −0.084), and (0.088, 0.243, −0.066). That is, our point estimate of company one's mean ROE is 17.7 percent, but there is a 95 percent choice that it is as high as 30.2 percent or as low as 5.1 percent.[16] Such wide intervals are thus indicative of substantial measurement error in the variables going into the contingency table analysis.

Bowman's procedures also assume observations across companies for each year and observations across years for each company are independent. Neither is true, so sampling error is actually even greater than the preceding rough calculations suggest because, intuitively, the higher the dependence among a given number of observations, the lower the effective number of observations.[17] The assumptions of independence are flawed for two reasons.

First, ROEs across companies are not independent for, as shown by Ball and Brown, there are market- and industry-wide factors influencing firms' accounting earnings.[18] The limitation imposed by this cross-sectional interdependence between ROEs is severe because many statistical methods, including the nonparametric ones Bowman uses, are no longer valid.

Second, for any given company, ROEs are not independently distributed through time. Loosely, the number of effective observations on ROEs depends upon the properties of the time series of ROEs. Indeed, if the ROE series is not covariance stationary,[19] its true mean and variance will not even be defined — there is no such thing as a unique mean and a unique variance.

Time series models of earnings variables such as earnings per share (EPS) have been studied extensively in the accounting literature, and the evidence is overwhelming that EPS series are typically not stationary, at least in the post-World War II era. Presumably the runaway growth of earnings due to inflation, net positive new investment, and technological improvements help explain this finding. When earnings series are deflated by total assets or equity values, the appropriate description of the resulting rate of return series is not as obvious, with somewhat different conclusions being reached in previous studies by Ball and Watts and Beaver.[20] Our analysis (not reported here)[21] tends to support the conclusion that a ratio like the ROE used by Bowman is covariance stationary, and hence, that it is sensible to talk about its mean and variance. However, in one test of mean reversion, Beaver found that it took some eleven years before high and low ROE firms regressed to a common mean ROE, suggesting that indeed fairly long periods may be required to get reasonably precise estimates of ROE means and variances.[22]

## Our Test Methodology and Results

**Test Methodology.** In this section, we present what we believe is a better-founded test of the risk-expected ROE relation than the Bowman test. We apply it to a sample of 175 firms described below and a second sample containing nine of the ten industries for which Bowman found the strongest support for his case. The details of the test are given in Marsh and Newey.[23]

We initially denote the sample observations as the multivariate time series $ROE_{it}$, $i = 1, \dots, N$, $t = 1, \dots, T$, where $N$ is the number of firms in the sample, and $T$ is the number of ROE observations for each firm. The test procedure consists of making two transformations of these series and then applying a parametric error components procedure to test for nonspecific cross-sectional association between time series means and time series variances.

The first transformation is designed to eliminate autocorrelation in a time specific component of the $\{ROE_{it}\}_t$.[24] This transformation does not affect the hypothesis test that we wish to perform. The variable $\{ROE_{it}\}$ will hereafter be defined as the transformed variable.

We make a second transformation designed to eliminate the cross-sectional interdependence between the ROEs for different firms in the same year or quarter. To eliminate the common factors, the $ROE_{it}$s are again redefined as the residual $ROE_{it}$ in the following model:

$$ROE'_{it} = b_i ROM_t + c_i ROI_{it} + ROE_{it} \qquad (1)$$

where $ROE'_{it}$ is the ROE variable as transformed in the first step, $ROM_t$ is the equally weighted average ROE for all 135 companies in our sample in period $t$, $ROI_t$ is the average industry ROE in period $t$, and $ROE_{it}$ is the final rate of return variable on which we wish to perform tests for cross-sectional association between mean and variance.

The average of the coefficients $b_i$ and $c_i$ across all firms $i = 1, \ldots, N$ in our sample will be positive, so that removal of the common factors affecting firm ROEs will *increase* the chance of finding Bowman's result. Of course, it *is* possible that a *sample estimate* of $b_i$ could be negative for a given firm, even though the true $b_i$ is positive (just as it is possible that a large positive estimate of $b_i$ could be above its true value). Thus, it would be desirable to incorporate the sampling error in the transformation in equation (1) into our test procedure below. Unfortunately, the methodology for doing this has not yet been developed, and we know of no other test procedure that accomplishes it.

It is important to understand that finance theory says nothing about the coefficients $b_i$ and $c_i$ in equation (1). These coefficients will be determined by the characteristics of firms' cash flows given their real investment decisions. Current finance theory seeks only to impose restrictions on the expected value of $ROE_{it}$ *given* the coefficient $b_i$ (industry risk is assumed to be diversifiable, so $c_i$ is ignored). Since we impose *no restriction* on the value of $b_i$ and the average of the "residual" $ROE_i$, we have not embodied any finance theory into the tests.

With the above two transformations, the test procedure fits within an error-components model as follows:

$$\widetilde{ROE}_{it} = \alpha + \tilde{\mu}_i + \tilde{\eta}_{it} \qquad (2)$$

where $\alpha$ is the "grand mean" of ROEs across all time periods and all firms, $\tilde{\mu}_i$ is a random firm-specific, but time-invariant, component of the mean with $E(\tilde{\mu}_i) = 0$, and $\tilde{\eta}_{it}i$, and is the remaining time-varying but firm-specific disturbance with

$$E[\eta_{it}|\mu_i] = 0, \quad E[\eta_{it} \cdot \eta_{i\tau}|\mu_i] = 0, \quad \tau \neq t.$$

In equation (2), the test for cross-sectional association between the time series means and variances of $ROE_i$, $i = 1, \ldots, N$ is a test of whether

$$H_0 : E[\eta_{it}^2|\mu_i] = \sigma_{\eta i}^2 \qquad (3)$$

The alternative hypothesis which we consider here[25] is

$$H_A : E[\eta_{it}^2|\mu_i] = g(\mu_i) \qquad (4)$$

The test statistic is

$$S = \frac{q^2_N}{\hat{V}(q_N)}, \qquad (5)$$

where the terms are defined fully in the Appendix. Loosely, the statistic in the numerator contains the difference between the sample ROE mean multiplied by the sample ROE variance and its expectation under the null hypothesis of no interaction or association between them adjusted for any asymmetry in the distribution of the ROE. The asymptotic sampling variance of $q_N$ is consistently estimated by the term in the denominator. Hence, S measures the extent to which average ROEs are associated with ROE variances across companies if no association were "truth," relative to expected sample variation.

The finite sample properties of the test statistic S are not known. Thus, we cannot determine how powerful our test is in detecting an association between true ROE means and variances across companies. On the other hand, if it is the case that our tests have little power, the same problem apparently afflicts Bowman's tests. He admits that ". . . the low number of companies in each [of his] table[s] and the closeness of some of the results to the null hypothesis [of no association] would yield rather weak signals" when his tests are used.[26] In fact, when we repeated his (misspecified) test on the first of our samples described below, we found that the association between average ROE and variance of ROE was significantly negative for only two out of thirteen industries.

**Data.** Our first sample consists of a substantially longer time series of ROEs obtained by extending a sample of earnings per share for 175 firms supplied to us by Ross Watts at the University of Rochester. In addition, to check that there was no selection bias in these companies, we repeated our tests on a second sample comprised of those ten *Value Line* industries for which Bowman found the strongest negative correlation.

The Watts sample includes a randomly selected 50 percent of the firms that meet the following criteria:

1. The firm's quarterly and annual earnings per share numbers are available in the *Wall Street Journal Index* (WSJI) over the period January 1958 through December 1969.

2. The firm's share price relatives are available on the Wells Fargo Bank daily file for the period July 2, 1962 through July 11, 1969.

3. The firm's shares are listed on the New York Stock Exchange for the period July 1, 1962 through September 27, 1968.

4. The firm's fiscal year is constant in the period January 1958 through December 1969.

All of the earnings per share numbers obtained from the WSJI are adjusted for stock splits and stock dividends. Earnings data from 1970 to 1981 were obtained from the vector of Primary Earnings Excluding Extraordinary Items on the COMPUSTAT Quarterly Industrial File. These earnings were adjusted for splits and stock dividends back to January 1, 1958. We have used 135 firms from the WSJI data set. We eliminated the 40 remaining firms from our sample because they didn't have fiscal years ending in March, June, September, or December (since we were interested in analyzing the data on a quarterly calendar) or because they were not on the 1978 COMPUSTAT Quarterly Industrial File.

ROE was calculated by deflating these earnings series by several different bases:

1. *Book Value per Share of Equity* at year *end* (adjusted for splits and stock dividends)[27] defined as book value of common equity divided by the number shares used for calculating Primary EPS. Since these items are available only on the COMPUSTAT Industrial Annual File, this ROE series was calculated only on an annual basis. Also, since the book values were not available for most firms until 1963, the series extends only from 1963 to 1981.

2. *Book Value per Share of Equity* at the *beginning* of the year. This series is similar to series 1 and covers 1964–1981.

3. *Market Value per Share of Equity* at the *end* of the quarter (adjusted for splits and stock dividends) obtained from the CRSP monthly stock return file and the *Wall Street Journal* (as a few companies were not listed on the NYSE and thus not on the CRSP file at the beginning of our time period). This sample spanned the first quarter of 1958 to the fourth quarter of 1981.[28]

Table 2 Test Statistics and Direction of Correlation[1] between Average ROE and Variance of ROE across Firms within Industries[2] and across All Firms

| Industry | Number of Firms in Industry | Direction of Correlation and Test Statistic *S* for ROE Series 1 EPS/BV 1963–81 | | Direction of Correlation and Test Statistic *S* for ROE Series 2 EPS/BV(−1)1964–81 | | Direction of Correlation and Test Statistic *S* for ROE Series 3 EPS/MV 1958–81 | | Direction of Correlation and Test Statistic *S* for ROE Series 4 EPS/MV(+)1958–81 | |
|---|---|---|---|---|---|---|---|---|---|
| Metal Mining | 4 | (+) | 2.12 | (+) | 0.30 | (−) | 0.04 | (+) | 0.54 |
| Food & Kindred Products | 10 | (−) | 2.23 | (+) | 0.67 | (−) | 0.00 | (−) | 0.25 |
| Textile Mill Products | 6 | (+) | 10.89 | (−) | 1.14 | (−) | 3.76 | (−) | 2.33 |
| Paper & Allied Products | 4 | (+) | 0.00 | (−) | 0.02 | (+) | 0.12 | (−) | 0.01 |
| Chemicals & Allied Products | 11 | (+) | 0.31 | (+) | 0.00 | (+) | 3.12 | (−) | 0.06 |
| Petroleum & Coal Products | 12 | (−) | 1.96 | (−) | 1.61 | (−) | 0.71 | (−) | 0.62 |
| Stone, Clay, & Glass Products | 7 | (−) | 0.60 | (−) | 0.97 | (−) | 4.46 | (−) | 0.28 |
| Primary Metal Industries | 7 | (+) | 0.66 | (+) | 0.03 | (+) | 0.37 | (+) | 0.24 |
| Machinery, except Electrical | 14 | (+) | 5.65 | (+) | 4.69 | (+) | 1.22 | (+) | 2.91 |
| Electric & Electronic Equipment | 11 | (+) | 0.52 | (−) | 0.00 | (+) | 0.76 | (+) | 2.44 |
| Transportation Equipment | 11 | (−) | 0.39 | (−) | 0.53 | (+) | 1.10 | (+) | 1.13 |
| Instruments & Related Products | 5 | (−) | 0.09 | (−) | 0.14 | (+) | 0.23 | (+) | 1.23 |
| Electrical, Gas, & Sanitary Services | 13 | (+) | 0.71 | (+) | 0.20 | (−) | 0.01 | (+) | 1.71 |
| All Firms | 135 | (+) | 1.09 | (+) | 1.93 | (+) | 3.04 | (+) | 1.25 |

Note: The firms on which the results in this table are based are a subset of the 175 firms in the Watts sample. Only industries containing at least 4 firms in the Watts sample were included in the subset. Six of the 175 firms that changed their SIC classification between 1978 and 1981, and 12 firms that were dropped from the COMPUSTAT file between 1978 and 1981 because of merger and takeover, were excluded from the subset.

[1]Direction of correlation, either positive (+) or negative (−), in parentheses.
[2]Industry classification based on the two-digit SIC code obtained from the 1978 COMPUSTAT Quarterly Industrial File dated September 2?, 1978.

4. *The Market Value per Share of Equity* (adjusted for splits and stock dividends) measured three months *after* the end of the quarter to which earnings pertain. This series is similar to series 3 and covers the first quarter of 1958 to the third quarter of 1981.

By nature of the ROE ratio, all measures of equity are meant to standardize each firm's earnings for its scale or size. There is some reason to believe series 4 will induce less noise into the ROE series than series 3. This is because some 85–90 percent of the information in a firm's accounting earnings is impounded in its stock price by the month of its announcement.[29] Thus, scaling earnings by a post-announcement market value of equity adjusts for the deviation of EPS from that expected. It will reduce the variability of the ROE caused by noise and hence increase the precision of estimation of the mean and variance of the ROE. Since the hypotheses relate to the cross-sectional relation between mean and variance of "permanent" ROEs, elimination of the transitory element should enhance the power of our tests.

**Results.** Table 2 lists the test statistics and direction of correlation for each industry and the whole sample. For both series 1 and 2, the Machinery (except Electrical) industry exhibits a positive correlation between ROE mean and ROE variance at the 95 percent confidence level. The Textile Mill Products industry has significant positive correlation for series 1 but not for series 2. (In fact, it is slightly negative in that sample.) The small size of this industry (6 firms) may be the cause of this result. No other significant positive or negative correlation is evident in these two series.

For series 3, the Textile Mill Products industry (negative correlation, $\chi^2(1) = 3.76$), the Chemicals and Allied Products industry (positive correlation, $\chi^2(1) = 3.12$), and the Stone, Clay, and Glass Products industry (negative correlation, $\chi^2(1) = 4.46$) have fairly significant levels of correlation. However, these samples are quite small (6, 11,

Table 3 List of Industries and Number of Firms in Each Industry

| Industry | Number of Firms | Correlation & Test Statistic[1] |
|---|---|---|
| Auto and Truck | 7 | (+) 0.060 |
| Air Transport | 18 | (+) 2.39 |
| Distilling | 7 | (+) 1.28 |
| Broadcasting | 10 | (−) 0.35 |
| Real Estate | 6 | (+) 20.82 |
| Finance | 13 | (+) 1.63 |
| Aerospace Diversified | 27 | (−) 1.07 |
| Cement | 12 | (+) 0.26 |
| Machine Tool | 17 | (+) 0.94 |

Note: This table is a list of industry names, number of firms, and test results for nine of the ten industries in which Bowman found the highest negative correlation between ROE mean and ROE variance. ROE defined as EPS/BV(−1) for the period 1969–1977.

[1]Direction of correlation, either positive (+) or negative (−), in parentheses.

and 7 firms, respectively) and it appears that a single firm within each of the industries may dominate the results, especially for the Textile Mill Products and Stone, Clay, and Glass Products industries. If just a single firm outlier from each of these industries is omitted, the direction of the correlation does not change, but now the test statistics are 1.52, 2.59, and 2.06, respectively, which are insignificant. For series 4, no industries show any significant levels of correlation.

The whole sample of 135 companies displays positive, although insignificant, correlation for all four of the series. For the four series, the number of industries within a series with positive correlation is 8, 6, 7, and 7, respectively, and the number with negative correlation is 5, 7, 6, and 6, respectively. This does not indicate any type of intra-industry correlation between ROE mean and variance, either negatively or positively.

Except for the positive correlation in the Machinery (except Electrical) industry for series 1 and 2, and the positive correlation for all firms together in all series, we find no significant correlation between average ROEs and their variance. Thus, our results contradict Bowman's.

It is possible that there is something unusual about our sample relative to Bowman's, though we find a high degree of overlap in industry coverage. Hence, for comparative purposes, we decided to study nine of the ten industries for which Bowman found the strongest negative correlation between average ROEs and variance of ROEs — the tenth industry was Advertising, which we did not include because only a few firms in that industry were in the COMPUSTAT data base. A listing of the industries and the number of firms in each is given in Table 3. Also listed is the test statistic S in equation (5) and the direction of correlation between average ROE and variance of ROE across the firms in each industry. Here, we studied the behavior of ROEs defined according to series 2 (Primary EPS/book value) for firms in the nine industries over the 1969–1977 time period. The only industry with significant correlation is Real Estate, but the correlation is positive and there are only 6 firms in the industry. Seven of the industries exhibit positive point estimates of the correlation, while only two display negative estimates.

## Interpretation of Industry Level Risk-Return Tradeoffs

**Ex Post Selection Bias.** We regard our results as more plausible than Bowman's as a general representation of the association between expected rates of return and the variance of those rates of return at the firm level because, as noted in the introduction, our results are consistent with equilibrium in the market for real assets. Of course, it *is* the function of management to look for intramarginal projects (projects with positive net present values) that are expected to generate the highest returns with the lowest risk.

Unfortunately, in any study of risk and profitability at the firm level, there will always be a tendency to observe "the winners" but not "the losers," and thus to observe high ex post return-low ex post risk projects. To see this, consider first those projects that turn out to be cash cows. These low risk, high average ROE projects will be harvested, and may even spawn further low risk, profitable follow-up investments. On the other hand, projects or strategies that turn out badly, despite expectations to the contrary based on the best possible prior analysis, will tend to be abandoned — the sooner the better. This leaves the intermediate projects whose fortunes are somewhat less blessed than those of the cash cows, but are still worth continuing. Such projects will tend to have lower average ROEs and more variable ROEs than the cash cows.

In addition, Bowman's high/low classes for average return and risk are determined within the sample *relative to the median* realized ROE mean and realized ROE variance for the firms in his sample. His negative risk-return relation could then arise as the ex post result of combining cash cows with high ex post average ROEs and low variance of ROEs with moderately successful firms that have relatively lower ex post average ROEs, but relatively higher variance of ROEs. At the industry level, there will be less noise in classifying firms on a relative basis, so one might expect that a negative correlation between risk and return will be stronger there. The implication is that a negative correlation across firms between their ex post average ROEs and the variance of their ROEs can tell us little more than that business is uncertain — managers, however, must make decisions, by definition, *before* "the dice are rolled."

**Risk-Return Tradeoffs.** Can analyses, like Bowman's, of the association between average ROEs and the variances of ROEs at the firm level, offer useful guidelines to managers faced with decisions before the fact? Suppose that the managers of a firm with high expected return-low risk projects are confronted by a new project or strategy that has an even lower risk than the current ones. Should they accept the project? Will the knowledge that risk and average return are negatively correlated help them make good investment decisions?

If the negative correlation forms the basis of the accept/reject decision, the new project, with its lower risk, would be required to have *higher* expected returns than the current ones. Though such a prescription might appear to be justified on the basis of Bowman's result, it would *not* lead to strategic decisions that are in the interests of investors.

Managers instead need to know how high a project's expected payoff must be in order to justify ploughing stockholder funds into it. Finance theory is quite straightforward in this respect: determine the project's risk (see below), then calculate the *hurdle rate* or *minimally acceptable* expected payoff that would be necessary to compensate investors for undertaking it. (Of course, if contrary to all the finance evidence, investors were risk preferring at the margin, the compensation would be negative.)

Thus, finance prescribes that the payoff on an appropriate *marginal* project be the benchmark for capital budgeting decisions. It would be in the interests of stockholders to have management undertake the hypothetical new project even if it had a lower expected rate of return than the current projects, so long as the new project's expected rate of return exceeds the hurdle rate appropriate for its riskiness. This would be true even though undertaking the new project will lower the average rate of return expected on the firm's portfolio of projects.

**Risk Seeking.** If a firm has debt in its capital structure and if its assets have been so depleted that it can no longer pay off its debt or meet coupon payments, managers acting in the stockholders' interest do have an incentive to "bet the firm" on high (variance) risk strategies. If the bet pays off, stockholders are back in the money. If it fails, the stockholders were in effect playing with the debtholders' money anyway. Thus, we might observe periods of low realized ROEs (as the firm's assets are dissipated) *succeeded by* periods of high variance of ROEs. Such "risk seeking by troubled firms"[30] has been studied extensively in the finance literature.[31] However, even if all the questions about the empirical relevance of this "agency problem" could be resolved,[32] risk seeking would imply, in the context here, that changes occur in the expected ROEs and variance of

ROEs over time.[33] Unfortunately, even with the large sample of ROEs here, it is almost impossible to adequately estimate such shifting parameters. Since Bowman's results assumed *constant* expected ROEs and variances of ROEs, it is hard to see how this risk-seeking behavior can be inferred from his evidence, even though it includes cross-sectional observations.

**Data Problems.** Last, but perhaps foremost, it is generally acknowledged that measuring rates of return by dividing book equity into accounting earnings is fraught with potential error. It is well known that the use of book rates of return can lead to incorrect investment decisions.[34] If a profitable project involves large cash outlays and lagged cash inflows, the firm may have a relatively low ROE over initial stages of the project and a relatively high ROE at later stages, even though much of the risk could occur, and be resolved, during the initial stages (with the low ROE). Stating the point in a slightly different way, cross-sectional associations between the average and the variance of a firm's accounting ROE could, all else being equal, be more a statement about its accounting techniques than about management techniques. In addition, the rate of return on equity will, unlike the rate of return on assets, be subject to variations in a firm's leverage (as will capital market returns on equity).

The use of a *ratio* of net accounting earnings to book value of assets is itself capable of producing misleading results. Using the accounting identity that "bottom line" earnings vary directly with the equity base (before payment of dividends), it can be shown that even purely random noise in the accounting process of earnings determination will decrease the average value of the ratio of accounting earnings to equity base and increase its variance. The induced moves of average ROE and variance of ROE in opposite directions apply to the *true* (population) values, not to their sampling estimates. If the noise is not purely random — for example, if accounting depreciation tends to be "undercharged" relative to economic depreciation in terms of intense utilization of capacity and high earnings, and vice versa — the effect is even stronger in the most probable circumstances.

Of course, in spite of these many and obvious problems with ROE as a measure of firm rate of return, one is often stuck with accounting data when securities market data for risk and return are unavailable where applicable (e.g., at the divisional level), or where the securities market has already capitalized the profits or opportunities of interest to economists, regulators, etc. Many existing analyses, such as the PIMS study[35] and the Strategic Planning Associates, Inc. (SPA) "Value Curve" and "Value Leverage Index,"[36] make explicit or implicit assumptions about risk-return relations at the ROE level. Our point is that, particularly when accounting data are the only available facts, users should appreciate just how much noise (often insidiously nonrandom) can be added to the analysis.

## Summary

We have examined the average relationship across firms between risk and average rates of return on corporate investments. We used *both* book values and market values of the investments when computing the rates of return. We find, unlike Bowman, that there is no negative tradeoff between risk and average return. We have emphasized that there is nothing anomalous, from a finance (or equilibrium) point of view, in finding that some firms possess intramarginal projects with little risk that are expected to yield high profits. However, the payoffs on marginal projects or firms will reflect the positive relation between risk and expected return which, alas, is the best the investor could achieve by instead investing, through the stock market, in other firms.

We pointed out earlier that a proper evaluation of investment projects or strategies involves a comparison between the incremental cash flows which those investments would generate and a hurdle rate which reflects the returns that investors could expect to earn elsewhere on projects of equivalent risk. Those hurdle rates, or marginal required expected returns on projects, *have* to be positively correlated with their risk so long as investors are risk averse. Also, if investors, be they risk averse or risk preferring, can form portfolios of ownership in firms, *hurdle rates* (or, over the long run, average *stock* market returns) cannot be related to variance risk per se. If they were, investors could always form arbitrage portfolios of investments (portfolio formation involves linear weighting) whose average returns would be linearly weighted averages of the average returns on the separate investments, but whose variance risk would be higher or lower than the weighted average of the individual investment variances (except in the special case where all the investment returns were perfectly correlated). Of course, since not all projects will have ex post payoffs at the ex ante margin, Bowman's finding doesn't necessarily say much about current finance theory guidelines, or vice versa.

Finance theory suggests that the appropriate measure of a project's or firm's risk is its systematic risk which is the nondiversifiable covariation of its return with that on the investments *of all firms*, rather than total risk or variance of the project's return. But there is evidence that the systematic and total variance risk measures are highly correlated,[37] and hence that rankings of projects on either risk measure will be similar.

There are also situations in which total variance risk matters per se. For example, when there is an option to abandon projects (and to more aggressively pursue other projects that do well), the *total* variance of a project's cash flows is needed in decision making.[38] Different levels of variability of the cash flows on a project for which later abandonment or expansion is not feasible or important can still affect the degree to which forecasts of later-year cash flows from the project will be updated as time progresses if and when that project is undertaken. These differing degrees of forecast revision should be reflected in the hurdle rates for the projects,[39] in which case the variance of the cash flows is again a legitimate input to proper project evaluation.

*Appendix follows on the next page.*

## Appendix

The test statistic (5) in the text is defined as $S = q^2_N/V(q_N)$. The exact specification of these terms is given as follows:

$$q_N = \frac{1}{\sqrt{N}} \sum_{i=1}^{N} \bar{r}_{i.}\hat{\sigma}_{i.}^2 - \left(\frac{1}{\sqrt{N}} \sum_{i=1}^{N} \bar{r}_{i.}\right)\left(\frac{1}{\sqrt{N}} \sum_{i=1}^{N} \hat{\sigma}_{i.}^2\right) - \frac{1}{T}\left[\frac{1}{\sqrt{N}} \sum_{i=1}^{N} \hat{\mu}_{3i.}\right]$$

$$\hat{V}(q_N) = \left[1,\ -\frac{1}{N}\sum_{i=1}^{N}\hat{\sigma}_{i.},\ -\frac{1}{N}\sum_{i=1}^{N}\bar{r}_{i.},\ -\frac{1}{T}\right]\hat{\Omega}\begin{bmatrix} 1 \\ -\frac{1}{N}\sum_{i=1}^{N}\hat{\sigma}_{i.}^2 \\ -\frac{1}{N}\sum_{i=1}^{N}\bar{r}_{i.} \\ -\frac{1}{T} \end{bmatrix}$$

$$\hat{\Omega} = \frac{1}{N-1}\left\{\sum_{i=1}^{N}\begin{bmatrix} (\bar{r}_{i.}\hat{\sigma}_{i.}^2)^2 & \bar{r}_{i.}^2\hat{\sigma}_{i.}^2 & \bar{r}_{i.}(\hat{\sigma}_{i.}^2)^2 & \bar{r}_{i.}\hat{\mu}_{3i.}\hat{\sigma}_{i.}^2 \\ & \bar{r}_{i.}^2 & \bar{r}_{i.}\hat{\sigma}_{i.}^2 & \bar{r}_{i.}\hat{\mu}_{3i.} \\ & & (\hat{\sigma}_{i.}^2)^2 & \hat{\sigma}_{i.}^2\hat{\mu}_{3i.} \\ & & & (\hat{\mu}_{3i.})^2 \end{bmatrix}\right.$$

$$\left. - \frac{1}{N}\begin{bmatrix} \sum_{i=1}^{N}\bar{r}_{i.}\hat{\sigma}_{i.}^2 \\ \sum_{i=1}^{N}\bar{r}_{i.} \\ \sum_{i=1}^{N}\hat{\sigma}_{i.}^2 \\ \sum_{i=1}^{N}\hat{\mu}_{3i.} \end{bmatrix}\left[\sum_{i=1}^{N}\bar{r}_{i.}\hat{\sigma}_{i.}^2,\ \sum_{i=1}^{N}\bar{r}_{i.},\ \sum_{i=1}^{N}\hat{\sigma}_{i.}^2,\ \sum_{i=1}^{N}\hat{\mu}_{3i.}\right]\right\}$$

where:

$$r_{it} \equiv ROE_{it}$$

$$\bar{r}_{i.} = \frac{1}{T}\sum_{t=1}^{T} r_{it},\quad \bar{\nu}_{i.} = \frac{1}{T}\sum_{t=1}^{T}\nu_{it}$$

$$\hat{\sigma}_{i.}^2 = \frac{1}{T-1}\sum_{t=1}^{T}(r_{it} - \bar{r}_{i.})^2 = \frac{1}{T-1}\sum_{t=1}^{T}(\eta_{it} - \bar{\eta}_{i})^2$$

$$\hat{\mu}_{3i.} = \frac{T}{(T-1)(T-2)}\sum_{t=1}^{T}(r_{it} - \bar{r}_{i.})^3$$

The test statistic S in (5) in the text is asymptotically (in N) distributed as $\chi^2(1)$.

Research was partly funded by a grant from the First Atlanta Corporation while Douglas Swanson was at the Sloan School of Management, M.I.T. We thank Donald Lessard and Richard Ruback for comments which helped improve the presentation of this article. We also thank Joan Wyatt for her editorial assistance.

## References


1
We caution readers that this risk premium has been earned *on average over long periods of time*. The standard deviation of the distribution of rates of annual return on a well-diversified portfolio of NYSE stocks is roughly double the expected rate of annual return, so that it is not improbable that risk premiums that are considerably lower than 8 percent per annum could be realized for many years in a row.

2
See E. H. Bowman, "A Risk/Return Paradox for Strategic Management," *Sloan Management Review*, Spring 1980, p. 18.

3
Ibid., p. 19.
As this article was going to press, we received a paper from Richard A. Bettis which contains a similar result. See R. A. Bettis and W. K. Hall, "Diversification Strategy, Accounting Determined Risk, and Accounting Determined Return," *Academy of Management Journal* (June 1982): 254–264.

4
See Bowman (Spring 1980), p. 25.
Bowman's evidence is that there is a negative correlation between expected returns and risk. Strictly, this means that managers could also be undertaking low expected return projects with high risk. However, since this would be so contrary to most views of the interests of stockholders and managers alike, we ignore it here.

5
See, for example, J. Tobin, "A General Equilibrium Approach to Monetary Policy," *Journal of Money, Credit, and Banking*, February 1969, pp. 15–29.
Related and interesting questions concern whether an equilibrium in which firms make investment decisions using the mean-variance model of asset pricing will be Pareto-optimal, and what the optimal number of firms will be in that equilibrium. However, such questions are not directly at issue here. The interested reader can consult the following:
J. Stiglitz, "On the Optimality of the Stock Market Allocation of Investment," *Quarterly Journal of Economics* 86 (1972): 25–60;
E. F. Fama, "Perfect Competition and Optimal Production Decisions under Uncertainty," *Bell Journal of Economics and Management Science* (Spring 1972): 509–530;
M. C. Jensen and J. B. Long, Jr., "Corporate Investment under Uncertainty and Pareto Optimality in the Capital Markets," *Bell Journal of Economics and Management Science* (Spring 1972): 151–174;
R. C. Merton and M. Subrahmanyam, "The Optimality of a Competitive Stock Market," *Bell Journal of Economics and Management Science*, Spring 1974, pp. 145–170;
D. I. Bosshardt, "Spanning, Pareto Optimality, and the Mean-Variance Model," *International Economic Review* (October 1983): 649–669.

6
See T. A. Marsh and W. K. Newey, "Tests of Cross-sectional Association between Means and Variances: Methodology and Applications to Stock Returns and International Inflation Rates" (unpublished working paper, MIT, April 1983).

7
See, for example, S. C. Myers, "Finance Theory and Finance Strategy" (unpublished paper, Sloan School of Management, MIT, December 1982).

8
In the contingency table test here, where the marginal frequencies are fixed (because the sample ROE means and variances for each industry are split into "high" and "low" groups relative to the median), the appropriate statistic should be referred to the hypergeometric, not the binomial, distribution (the hypergeometric approaches the binomial only when the sample size gets large).

9
On the other hand, Bowman does cite unpublished results by Treacy which he reports do establish a monotonic ranking among intra-industry ROE means and ROE variances. See M. Treacy, "Profitability Patterns and Firm Size" (Working Paper 1109–80, Sloan School of Management, MIT, January 1980).
Professor Bowman states that these ". . . more powerful nonparametric procedures of rank orders and Spearman tests have . . . replicated and substantiated [his] findings." We interpret his claim that rank tests are more powerful to be based on the design of his contingency table test that does not exploit the cardinal measures of ROE mean and ROE variance. However, the reader should realize that there is not necessarily a "power difference" between rank order and contingency table tests in general. Bowman's contingency table test and rank order tests do not test the same proposition.
For a discussion of the power of different tests of association, see, for example:
M. B. Brown and J. K. Benedetti, "Sampling Behavior of Tests for Correlation in Two-Way Contingency Tables," *Journal of the American Statistical Association* (June 1977): 358. Applications Section, 309–315.
T. P. Hutchinson, "The Validity of the Chi-squared Test When Expected Frequencies Are Small: A List of Recent Research References," *Communications in Statistics: Theory and Methods* A8 (1979): 327–335.

10
The proposition turns out to be an if and only if proposition, i.e., if the mean and variance are independent, the population must be normal.
See M. Kendall and A. Stuart, *The Advanced Theory of Statistics*, Vol. 1, 4th ed. (New York: Macmillan, 1977), Exercise 11.19.

11
See, for example, E. B. Deakin, "Distributions of Financial Accounting Ratios: Some Empirical Evidence," *Accounting Review*, 1976, pp. 90–96.

12
In general, maximum likelihood estimators of location and scale for asymmetrical distributions are asymptotically uncorrelated (independent) *only* when they are centered about an origin known as the "center of location." See Kendall and Stuart, Vol. II (1977), pp. 67–68.

13
It is the normality of the *sample ROEs* that is relevant to independence of the sample mean and variance. We note further that it was assumed in one of the early approaches to contingency table analysis that the variables in the table (here the *sample* mean and *sample* variance) were themselves drawn from a discretized bivariate normal distribution. The analysis has recently been applied under more general conditions.
For early approaches to contingency table analysis, see K. Pearson, "Mathematical Contribution to the Theory of Evolution VII: On the Correlation of Characters Not Quantitatively Measurable," *Phil. Trans. R. Society* (1900): 1–47.
For more general conditions of the analysis, see L. A. Goodman, "Association Models and the Bivariate Normal for Contingency Tables with Ordered Categories," *Biometrika* 68 (1981): 347–355.

14
See Bowman (Spring 1980), p. 19.

15
Ibid., p. 20.

16
Of course, these confidence intervals depend upon estimates of the variance of the respective ROEs which themselves will have large standard errors in small samples, and thus should be "bootstrapped," but the point hardly serves to warrant doing that.

17
The usual methods of contingency table analysis used by Professor Bowman can give very different results when the cross-sectional sample of firms is small. See, for example:
M. A. A. Cox and R. L. Plackett, "Small Samples in Contingency Tables," *Biometrika* 67 (1980): 1–13;
N. Mantel and B. F. Hankey, "The Odds Ratio of a 2 × 2 Contingency Table," *The American Statistician*, November 1975, pp. 143–145;
Hutchinson (1979).

18
See R. Ball and P. Brown, "Some Preliminary Findings on the Association between the Earnings of a Firm, Its Industry, and the Economy," supplement to the *Journal of Accounting Research, Empirical Research in Accounting: Selected Studies*, 1967, pp. 55–77.

19
A time series is defined to be covariance stationary if its first and second moments do not depend upon the time period in which the series is observed. If covariance stationarity did not apply here, we would only be able to discuss means and variances of ROEs relative to an observed ROE as a reference point.

20
See:
R. Ball and R. Watts, "Some Time Series Properties of Accounting Numbers," *The Journal of Finance*, XXVII (1972): 663–681;
W. Beaver, "The Time-Series Behavior of Earnings," *Empirical Research in Accounting: Selected Studies*, supplement to the *Journal of Accounting Research*, 1970, pp. 62–99.

21
All results alluded to, but not reported in this article, are available upon request.

22
See Beaver (1970): Table 15.

23
See Marsh and Newey (1983).

24
This transformation does not (nor is it intended to), eliminate all the autocorrelation in $\{ROE_{it}\}$, $i = 1, \ldots, N$, since the firm-specific, but time-invariant, component $\mu_i$ in equation (5) induces such autocorrelation.

25
One of the advantages of the test here is that the form of $g(\cdot)$ need not be specified a priori. On the other hand, unlike the categorized data analysis procedures, ours can be modified to incorporate specific parametric relations of interest to the researcher. For example, if $H_0$ were rejected, Professor Bowman's results would suggest $g' < 0$.
For heteroscedasticity tests, see:
T. S. Breusch and H. R. Pagan, "A Simple Test for Heteroscedasticity and Random Coefficient Variation," *Econometrica* (September 1979): 1287–1294;
R. F. Engle, "Autoregressive Conditional

Heteroscedasticity with Estimates of the Variance of United Kingdom Inflation," *Econometrica* (June 1982): 987–1007;
H. White, "A Heteroscedasticity-Consistent Covariance Matrix and a Direct Test for Heteroscedasticity," *Econometrica* (May 1980): 817–838.

26
See Bowman (Spring 1980), p. 20.

27
Figures to adjust prices and earnings were obtained from the CRSP Monthly Stock Master (MSM) file and from Moody's for the firms not on the NYSE. End of period stock market prices were used because some firms had rights issues that may be viewed as a mixture of stock split and new issue. Since rights issues were included as splits on the MSM file, all earnings and prices were adjusted similarly.

28
ROE series 3 was also divided into subintervals of 1958–1969 and 1970–1981 because of the difference in the sources of earnings series (some of the Watts earnings may contain extraordinary items) obtained from WSJI and Moody's.

29
See R. Ball and P. Brown, "An Empirical Evaluation of Accounting Income Numbers," *Journal of Accounting Research* (Autumn 1968): 610–629.

30
See E. H. Bowman, "Risk Seeking by Troubled Firms," *Sloan Management Review*, Summer 1982, pp. 33–42.

31
See, for example:
E. F. Fama and M. H. Miller, *The Theory of Finance* (New York: Holt, Rinehart and Winston, 1972);
S. C. Myers, "Determinants of Corporate Borrowing," *Journal of Financial Economics* (November 1977): 147–175;
M. C. Jensen and W. H. Meckling, "Theory of the Firm: Managerial Behavior, Agency Costs, and Ownership Structure," *Journal of Financial Economics* (October 1976): 305–360.

32
See, for example, E. F. Fama, "Agency Problems and the Theory of the Firm," *Journal of Political Economy* (April 1980): 288–307.

33
Black suggests that, at least for capital market data, this is a distinct possibility. See F. Black, "Studies of Stock Price Volatility Changes" (Proceedings of the American Statistical Association, Business and Economics Statistics Section), pp. 177–181.
We should note, however, that Holland and Myers found that "... the most interesting feature of the series [of ratios of operating income to market value [of assets] *averaged* across all manufacturing and nonfinancial corporations, which is the unlevered equivalent of one of our ROE series used below] was its stability, at least from the mid-1950s through 1976." See D. M. Holland and S. C. Myers, "Profitability and Capital Costs for Manufacturing Corporations and All Nonfinancial Corporations," *The American Economic Review* 70 (1980): 320–325.

34
See R. Brealey and S. Myers, *Principles of Corporate Finance*, 2d ed. (New York: McGraw-Hill, 1984), pp. 68–70.

35
See S. Schoeffler, R. D. Buzzell, and D. F. Heany, "Impact of Strategic Planning on Profit Performance," *Harvard Business Review*, March–April 1974, pp. 145–173.

36
See Strategic Planning Associates, Inc., *Strategy and Shareholder Value: The Value Curve* (Washington, DC: Strategic Planning Associates, 1981).

37
Beaver et al. found that, for earnings data, where only a small number of observations (nine) are available, there is a "... potential inability to separate the systematic and individualistic risk components, [so that] measures of total variability might perform as well or better than an accounting beta." See W. Beaver, P. Kettler, and M. Scholes, "The Association between Market Determined and Accounting Determined Risk Measures," *The Accounting Review* (October 1970): 654–682.

38
For further discussion, see S. C. Myers and S. Majd, "Calculating Abandonment Value Using Option Pricing Theory" (unpublished working paper, Sloan School of Management, MIT, May 1983).

39
See, for example, S. C. Myers and S. M. Turnbull, "Capital Budgeting and the Capital Asset Pricing Model: Good News and Bad News," *Journal of Finance* (May 1977): 321–332.

## A Response from Edward H. Bowman

As I understand their article, Marsh and Swanson have several types of arguments regarding my article, "A Risk/Return Paradox for Strategic Management," which demonstrates within a majority of industries a negative correlation for companies between risk and return. As I argued, the negative paradox is probably caused by management behavior, accounting convention, and risk preference.

Their three primary arguments are (1) theoretical, (2) statistical, and (3) empirical. The theoretical arguments derive mainly from modern finance theory. I even elucidate several of them, as it is essentially on this basis that I label the negative correlation between risk and return a "paradox." Their arguments are largely normative (i.e., that managers *should* behave in a particular way) and rely on the classical arguments of equilibrium analysis that the free markets in the long run will force reality to follow theory. Interested readers who wish to see cogent arguments in modern economics challenging this approach should read *An Evolutionary Theory of Economic Change* by Richard Nelson and Sidney Winter (Harvard University Press, 1982).

Interestingly enough, some of Marsh and Swanson's conceptual points come very close to allowing for the *possibility* of the risk/return paradox as in, "Such 'risk seeking by troubled firms' has been studied extensively in finance literature." (See E. H. Bowman, "Risk Seeking by Troubled Firms," *Sloan Management Review*, Summer 1982.) I cite in my second article, as they do, Myers's arguments (see Marsh and Swanson's reference 31). However, they are apparently unwilling to believe the findings of a substantial body of psychology and management science experimental work that challenges classical utility theory with its ubiquitous risk aversion arguments. Tversky and Kahneman, and Laughhunn, Payne, and Crum in well-known recent works published in the journals *Science*, *Econometrica*, and *Management Science* demonstrate the *typical* risk-seeking behavior by people whose recent experience and prospects are below their own aspiration levels. (See Marsh and Swanson's reference 4, including ". . . since this would be so contrary to most views of the interests of stockholders and managers alike, we ignore it here.")

The statistical arguments offered by the authors are very involved, and in several parts. The two by two tables they criticize were used for simplicity of both analysis and presentation, and involve some chosen tradeoffs as virtually all statistical tests do. The nonparametric tests which they are not enthusiastic about were used to counter single and strange outlier observations so as not to bias the results, something which parametric tests do poorly. Their several suggested "adjustments" to the basic data, one of which will be discussed shortly, are outside of the very common usage of many authors in economics, management, and finance in their studies of risk and return. They mention only in a footnote (see Marsh and Swanson's reference 9) and without the data reported, Professor Michael Treacy's paper which used a different data set, Compustat, that was much larger than theirs and Spearman's rank order correlation coefficients. We reported that Treacy found that forty-three of the fifty-four industries had a correlation coefficient between mean ROE and variance that was negative. Twenty-two of these forty-three negative correlations were significant at the 5 percent level. These empirical results and the statistical methodology are both stronger than the original two by two tables.

The one adjustment that Marsh and Swanson apparently made to all their data for statistical purposes, perhaps a matter of taste, seems to color strongly their empirical results — my next and last response. It is apparently to "adjust" the calculation of each mean and variance of ROEs by the *industry* mean of that year. The authors offer two tables of results, one of thirteen industries with data received from the University of Rochester, and the other of nine of my most negative industry correlations. Table 2, with their small sample of thirteen SIC industries and 135 companies, does not show, I believe, what the authors argue when compared to my eighty-five *Value Line* industries with 1,572 companies.

I have matched where possible their thirteen industries with similar industries in my table. Both their table and my matched industries show about an even split between negative and positive correlations, though the matching is imperfect because of the more aggregate classification of the two-digit SIC codes (a confounding by aggregation that my article argues against). In other words, we jointly find about the same correlation results for the thirteen industries (however, only in total, as their signs are quite inconsistent across methods/columns). Therefore, by extension, if we jointly find about the same aggregate results for the thirteen industries, we should also find about the same results for the eighty-five industries (i.e., a significant majority of negative correlations). Thus, though their data transformations are unusual, their empirical results tend to support rather than refute our original work.

Finally, taking only their first check of my most extreme negative correlations, autos and trucks, seven firms, they show a *positive* correlation but a low test statistic. I found a six to one ratio; that is, three firms are high ROE and low variance, three firms are low ROE and high variance, and none is high ROE and high variance and one is low ROE and low variance. I believe the explanation of this large discrepancy here and throughout their Table 3 is due to their "correction" of adjusting for the *industry* average each year. Their use of this transformation here is unusual as it is ordinarily used to control or normalize for *between* industry differences when all companies are grouped together. If the industry as a whole has high swings, as surely the auto and truck industry does, and if a few companies are more stable, their performance will appear to vary more than the average company because all results are individually being transformed or adjusted by the *industry* average for that year. For those readers who do not have the data readily available, even casual observation will show that General Motors and Ford report much higher average profit rates with a much lower variance (the negative correlation) than do Chrysler, American Motors, or White Trucks. To look at this industry and see a weak positive correlation rather than a strong negative correlation just doesn't make sense. Perhaps the difference is explained by their several data "transformations," or perhaps it is explained by their special Compustat data "Excluding Extraordinary Items" which could cover large "write off losses" and other omissions as "atypical."

Following the analysis for the paradox article now going back about six years, I have seen a number of articles and working papers demonstrating the negative risk/return correlations under various circumstances. I agree with the implicit point in the Marsh and Swanson article that both theoretical and empirical challenges to current theory are worth investigating, and that normative advice to managers should be offered only with some caution. Therefore, I invite others to start from scratch with their data sources and their statistical tests and see what results they find.