RECENT DEVELOPMENTS IN FINANCE

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Four years ago, in a presidential address before the American Finance Association entitled "Mythodology in Finance," I pointed out that in spite of the impressive advances which had been made over recent years in financial theory and related empirical research, we still had remarkably few answers to the basic questions in finance (or economics) either at the macro or micro level.[1] I suggested that much of the current research in finance seemed more likely to lead to advances in methodological niceties than in substantive knowledge and that this situation might be improved by more careful analysis of available data and the collection of new data from corporations and households on relevant financial characteristics and determinants of behavior.

My appraisal of the state of the arts in finance has not changed over the past four years. Theory has become increasingly sophisticated and perhaps increasingly divorced from reality. Thus, while I am intrigued by the ingenuity of some of the extensive recent literature making options the basis for a new theory of capital asset pricing, I am dubious about the usefulness of much of this literature. The original seminal work did give us significant new insights, but it is not clear to me that a similar statement can be made of the plethora of recent work in this area.

In my opinion, we have learned more in this period from several simple but careful examinations of the relationships between return on the different types of capital assets and the rate of inflation than we have from

[1] Irwin Friend, "Mythodology in Finance," Journal of Finance, May 1973.

any of the many extensions of capital asset pricing theory, including those which have attempted to integrate inflation into capital asset pricing. We have learned that the classical theory which explained why stock prices (in the absence of leverage) would grow at the same pace as the rate of inflation is incorrect at least in the short-run, and that in fact there has been historically a small negative correlation between annual changes in stock prices and in the rate of inflation. We now know that stocks are an extremely poor hedge against inflation, except for relatively long investment horizons,[1] even if we have no satisfactory theory to explain why.

In this paper, I shall select examples from a number of major areas of finance, e.g., monetary policy, capital asset pricing, financial institutions and markets and corporate finance, to illustrate the absence of or questionable nature of much of the "knowledge" in these areas, the problems involved in present methodology, and the need for a changed emphasis in methodology. I will touch on most of these examples only briefly since they were developed more fully in my earlier paper, but I will discuss several new ones in some detail. I shall pay particular attention to the present status of the Sharpe-Lintner capital asset pricing model, including the past and more recent attempts to reconcile that theory with the empirical facts.

Impact of Monetary Policy on Economic Activity

At the macro-level, the deficiencies in our knowledge about the determinants of financial variables and the effects of these variables on economic activity stem, of course, from the deficiencies of all available large-scale econometric models. The basic difficulty with these models is

[1] Stock market returns generally are negatively affected by price inflation for the same year but somewhat more strongly positively affected by price inflation in earlier years.

the small number of independent aggregate time-series observations available for determining the appropriate parameters in a model with a large number of alternative forms to select among for each equation. With the use of increasingly complex lag structures, the number of alternative forms to choose from for each structural equation has proliferated greatly. For a model of 100 equations with say ten forms to choose from for each equation, the theoretical number of combinations is 10^{100}. The number of time-series observations available for choosing among these combinations is limited, even abstracting from problems of serial correlation and changes in the economic structure.

Business investment

To illustrate the problem of measuring the impact of monetary policy on investment and consumption or savings and hence on economic activity, reference might be made to the MIT-Pennsylvania-SSRC (MPS) model, perhaps the best of the econometric models which are policy oriented. Producers' equipment expenditures in the MPS model are a complicated distributed lag of current and past orders, new orders are an even more complex distributed lag of the product of the equilibrium capital-output ratio and output, and the equilibrium capital-output ratio is a simple function of the user cost of capital which assumes a Cobb-Douglas production function and assumes also that the cost of capital is a linear function of the real corporate bond rate and the stock dividend yield. The cost of capital is not measured directly but is that linear combination of the bond and dividend yields which gives the best fit in the demand for new orders of producers' durable equipment.

The dangers of curve fitting in an estimation procedure of this type are obvious. Thus, it has been shown that small differences in the regression coefficients of the lagged variables in the type of plant and equipment investment relations used in the MPS model have very little effect on the goodness of fit of the equation but can change dramatically the distributed lag pattern. In one example, changing the coefficient of a lagged variable (in this case investment two quarters earlier) by three-fourths of its standard error implied that 90% of the impact of monetary policy on net investment in plant and equipment occurred within an eight quarter period whereas without this change only about one-third of the impact took place within this time interval.[1] The difference in the implication of these two results for economic policy is obvious. Similar disparities in results were obtained when different model builders were asked by the U. S. Treasury Department to estimate the effect of investment tax credits on the demand for plant and equipment.

The point might be legitimately raised that it is easy enough to critize current procedures but the relevant question is what can be done about them. It _is_ possible to considerably improve our current procedures by using what are for economists somewhat less orthodox sources of data. Significant improvement in the estimation of lag structures among plans, orders and expenditures on business investment should be made possible by careful surveys of a stratified sample of business firms on their actual experience. More importantly, the direct impact of changes in monetary variables (or of investment tax credits) on business investment can be and has on occasion been analyzed from such surveys.

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[1]Zvi Griliches and N. Wallace, "The Determinants of Investment Revisited," _International Economic Review_, September 1965.

In my opinion, though I am a biased observer since I played a role in initiating this work, the best estimates of the impact of monetary policy on business investment have been obtained from the special surveys conducted by the U. S. Government in conjunction with the regular surveys of actual and anticipated investment in plant and equipment and in inventories.[1] These special surveys, covering two periods of the greatest monetary stringency in U. S. history, collected detailed information on the timing and magnitude of the direct impact of 1966 financial market developments on business investment in 1966 and anticipated fixed investment in 1967, and of 1969-70 market developments on investment in 1970 and anticipated fixed investment in 1971.

A comparison of the survey findings with comparable results from the MPS model not only for plant and equipment but also for inventory investment was presented in the paper "Mythodology in Finance." The disparities between the two approaches are especially pronounced for inventory investment where the survey data indicate a significant impact of monetary policy, while the MPS model is unable to detect any financial effects -- hardly a plausible result. Given the choice of assumptions and procedures necessary for the solution of the MPS model, I feel that the survey findings are more credible. In any event it is clear that not much confidence can be placed in the MPS (or other large-scale model) findings in view of the apparent conflict

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[1] Jean Crockett, Irwin Friend and Henry Shavell, "The Impact of Monetary Stringency on Business Investment," Survey of Current Business, August 1967; and Henry Shavell and John T. Woodward, "The Impact of the 1969-70 Monetary Stringency on Business Investment," Survey of Current Business, December 1971.

of results.[1] It does not appear likely that as much is to be gained by continued re-specification and re-estimation of the large-scale models as by additional and more careful survey analysis and by other procedures.

Also, the equations in the business investment sector of econometric models might be significantly improved by the use of continuous cross-section data. Such information is readily available on tapes for all sizeable corporations in the U.S.A. Deriving investment and related functions for a sample of 100 or more corporations, as against a single aggregate time-series, greatly expands the number of independent observations and should provide a useful test of the validity of model equations in the business investment sector. For a small sample of large corporations, it should be especially valuable to combine the derivation of ex-post investment functions with the collection of relevant survey data so that any implied discrepancies between the two approaches could be resolved.

Consumption expenditures

Another important channel of transmission of monetary policy in the MPS model is the wealth effect on consumption. Monetary policy affects short-term and long-term interest rates, dividend yields and hence the value of assets and -- of particular importance in view of its magnitude and volatility -- the value of common stock holdings. Qualitatively, there can be little question that assets affect consumption, but the time-sequence of this effect is, and will continue to be, extremely difficult to determine from time-series data.

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[1] In some cases, it may be possible to use survey data to resolve major disparities in other policy effects implied by different econometric models. Thus, a U.S. Government survey of the impact of the investment tax credit on plant and equipment expenditures provided strong support for an econometric model which indicated a moderate effect of the tax credit as against another model which pointed to an effect several times as large.

To obtain reasonably definitive insights into the timing and magnitude of the wealth effect on consumption, it will probably be necessary to collect new data through surveys which compile continuous cross-section information on household savings, income, assets and on realized and unrealized capital gains. Survey information on how households say they react in their consumption behavior to changes in the stock market level may also be useful, but are not likely to be as reliable as businessmen's answers to questions about the effect of financial developments on their investment. However, even with the information already available, it may be possible to improve substantially on our present knowledge of asset effects on consumption. I made such an attempt in a study which appeared in the September 1975 American Economic Review[1] and was based on a 1963 survey of 2,100 households covering not only household income, savings, and major categories of assets held at the beginning and end of the year, but also amounts of individual stocks held. As a result, the effect on saving of capital gains during the year could be studied on the basis of a couple of thousand rather than a handful of observations. The results were extremely encouraging and indicated more convincingly than any time-series analysis could the short-run impact of monetary policy on consumption.[2] However, clearly comparable data are needed also for other time periods to reach more generally applicable conclusions.

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[1] Irwin Friend and Charles Lieberman, "Short-Run Asset Effects on Household Savings and Consumption: The Cross-Section Evidence."

[2] A number of the best known econometric models, including the highly regarded Wharton model, do not allow for any direct effects of monetary policy on consumption.

Capital Asset Pricing

There have been few if any areas of finance which have received more theoretical and empirical attention in recent years than that of capital asset pricing. The original Sharpe-Lintner market-line theory advanced to explain the variations in risk differentials on different risky assets has now been widely questioned on the basis of the empirical evidence, and a large number of modified theories have been proposed to explain the observed discrepancies between theory and observation.

The evidence points to a reasonably linear relationship on the average between return and non-diversifiable risk of outstanding common stock, or at least those listed on the New York and American Stock Exchanges. However, this same return-risk linear relationship does not seem to explain the return on bonds and does not seem to imply a riskless market rate of return consistent with any reasonable measure of the actual risk-free rates of return. Actually, there is evidence both for bonds and stocks that for very risky issues in each category, e.g., unseasoned new stock issues and bonds rated below BB, the realized returns over extended periods of time have been lower than those on less risky issues.

Moreover, while over the long-run the observed linear relationship between return and risk on individual stocks yields the expected positive sign of the risk coefficient more often than not, the shorter-term relationship has been erratic and has not been explained satisfactorily by the observed difference between the market rate of return of stocks as a whole and the risk-free rate. Thus, the relationship between return and risk was negligible in 1955-59 and negative in 1960-64,[1] though in both periods

[1] Marshall Blume and Irwin Friend, "A New Look at the Capital Asset Pricing Model," The Journal of Finance, March, 1973.

the stock market was moderately strong. As a result of these and similar findings, questions have been raised about the nature of the relationship between expected and actual rates of return, i.e., about the return generating model, as well as about the theory relating expected return to risk.

It may be useful to catalogue briefly a number of the theoretical and empirical attempts made to explain the observed deficiencies in the original market line theory, including several advanced during the past year. Very early in the game, several attempts were made to test the hypothesis that the required rate of return on a risky asset depends in part on the skewness in its probability distribution, with investors preferring positive skewness. In a more innovative approach, Black and others assumed either that there is no risk-free rate, or there is no risk-free borrowing rate, or the risk-free lending rate is less than the borrowing rate, though at the same time the short-selling mechanism for risky assets functions perfectly. This modification of the traditional model as well as the development of an arbitrage model led to the introduction of the minimum-variance zero-covariance or zero-beta asset to replace or supplement the risk-free rate. It was further pointed out by a number of analysts, including Jensen and Levy and Levhari, that theory did not specify the relevant investor's planning horizon and that the empirical results would reflect differences between the assumed and the true holding period. To allow for the inadequacies of a one-period model in a multi-period world, Merton introduced a third asset, in addition to the risk-free asset and the market portfolio, which would provide protection against changes in the opportunity set. Other analysts including Fama and McBeth tested the usefulness of adding the square of the beta coefficient and the residual standard deviation of returns in explaining the differential returns on individual assets. Mayers described how the original theory which assumed all assets to be marketable was affected by the introduction of non-marketable assets, most notably human wealth. Segmented markets, inadequacies of the return generating model, heterogenous expectations, differential taxes on different assets, transaction costs, and non-pecuniary liquidity returns on risk-free assets are some of the other suggestions which have been proposed as at least partial explanations of the observed discrepancies between theory and observation. At the statistical level, the inadequacy of a stock market index as a measure of the return on all risky assets has been repeatedly stressed.

Since most of this work is relatively well known and is in general readily available in the published literature, I will not describe it further. However, I want to emphasize that none of the theoretical modifications seems strongly supported by the data once it is maintained that a capital asset pricing theory should as a minimum be able to explain the returns on all marketable assets and not just a selected though important subset, listed common stock. Moreover, even for this subset, it has not been possible to distinguish among a number of alternative explanations of the approximate linearity of the risk-return trade-off for listed stocks once the intercept is no longer required to be risk-free or some other theoretically determined value which can be checked directly against the empirical evidence.[1]

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[1]The basic difficulty in testing two factor linear models of asset returns is indicated in Stephen A. Ross, "Return, Risk and Arbitrage," Rodney L. White Center for Financial Research Working Paper No. 17-73d (also published in Irwin Friend and James L. Bicksler, Risk and Return in Finance, Vol. I, Ballinger Publishing Company, 1977), and more recently in Richard Roll, "Can Two-Factor Linear Models of Asset Returns be Tested?" European Institute for Advanced Studies in Management Working Paper No. 75-30.

Though of some interest, three recent attempts to explain both theoretically and empirically the rates of return on risky assets, the first two of which have been published in the last two issues of The Journal of Finance, also are far from convincing. The first, by Kraus and Litzenberger,[1] introduces a measure of co-skewness in addition to the beta measure of covariance in the original capital asset pricing model, but co-skewness does not add much to the explanation and it is not possible to differentiate between the utility of this variable versus other supplementary variables which have been proposed. More important, co-skewness would not appear to explain the apparently lower rates of return on bonds than on stocks for comparable beta values which I shall discuss shortly. A second paper by two colleagues and myself in the December, 1976 The Journal of Finance introduces uncertain inflation into the original capital asset pricing model, but while unlike the case for co-skewness there is little danger that the two relevant inflation covariances are acting as proxies for other variables, the additional explanatory power of the modified theory seems rather limited.[2] A third unpublished paper by Blume[3] shows that the inability to engage in short sales might explain the substantially higher rate of return generally ascribed to a zero-beta asset in empirical analysis, as compared with the observed risk-free rate. However, while this is a promising approach, it has yet to be tested even for stocks to say nothing of bonds.

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[1]Alan Kraus and Robert H. Litzenberger, "Skewness Preference and the Valuation of Risk Assets," The Journal of Finance, September 1976.

[2]Irwin Friend, Yoram Landskroner and Etienne Losq, "The Demand for Risky Assets Under Uncertain Inflation," The Journal of Finance, December 1976.

[3]Marshall E. Blume, "Equilibrium in the Capital Markets in the Absence of Short Positions," European Institute for Advanced Studies in Management Working Paper No. 76-22.

In addition to these recent attempts to modify the capital asset pricing model to bring theory in closer conformity with reality, recent analyses by Blume and myself of two bodies of data different from the types of information usually used for this purpose raise some fundamental questions about the usefulness of any theory which assumes that only covariance risk is relevant to investors. Thus, in an analysis of the stock portfolios as well as the major classes of assets and liabilities held by different individuals,[1] it was found that a surprisingly large proportion of portfolios and assets were highly undiversified, and it was concluded from an examination of the other alternatives that the two most plausible explanations are either, first, that investors hold heterogeneous expectations as to expected return and risk or, second, that they do not properly aggregate risks of individual assets to measure the risk of an entire portfolio. Both of these explanations conflict with important assumptions typically made in capital asset theory, but the second is obviously basic since it raises questions about the justification for sole reliance on beta or covariance with the market return rather than on variance (or standard deviation) of the asset's own returns as a measure of the market's appraisal of asset risk. In a recent unpublished paper,[2] Levy has used these findings to construct an equilibrium theory which posits that because of transaction and information costs most investors are constrained to hold highly undiversified portfolios, resulting in a capital asset pricing relation which ascribes major importance to the variance

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[1]Marshall E. Blume and Irwin Friend, "The Asset Structure of Individual Portfolios and Some Implications for Utility Functions," The Journal of Finance, May 1975.

[2]Haim Levy, "Equilibrium in An Imperfect Market: A Constraint on the Number of Securities in the Portfolio," School of Business Administration, Hebrew University.

measure of risk and minimizes the importance of a market beta measure of risk. On the basis of several empirical tests which make use of individual asset rather than portfolio returns, he concludes that the variance is much more important than the usual beta measure of risk in explaining asset returns and therefore that his radical departure from previous theory is justified. Theoretically, it is preferable to use individual asset returns as Levy does, rather than portfolio returns, but statistically the use of individual assets raises substantial measurement error problems.

The second new source of data pertaining to the relative importance of covariance versus variance risk is a survey of over 1000 stockholders in the fall of 1975 which was conducted as part of a study by Blume and myself, entitled The Changing Role of Individual Investors in the Stock Market, to be published later this year under the auspices of The Twentieth Century Fund. This survey collected information on the market motivation, behavior, experience and plans of these investors, including data on the stockholders' perceptions of and attitudes towards risk and on the expected rate of return required on common stock as compared with other investments. Several of our findings are of interest here. When the 82% of stockholding families which customarily evaluated the degree of risk involved in purchasing stock were asked which measures of risk they used, 45% stated they used earnings volatility, 30% price volatility and 17% published betas.[1] When asked whether they would prefer to purchase a stock whose price tends to move in the opposite direction to the stock market as a whole, or one which tends to move in the same direction, less than 10% stated they preferred the opposite direction.

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[1]Weighting the replies by the value of a family's stock portfolio does not change these results substantially.

It will be necessary to follow up this last question with in-depth interviews to ensure that the expected rate of return is kept constant before reaching more definitive conclusions. Moreover, there is reason to believe that financial institutions, the other major group in the stock market, may use beta coefficients to a greater extent than individual investors. On the other hand, I know of no direct evidence supporting the almost universal academic conclusion, based on normative models and not too impressive empirical tests, that beta is the relevant measure of market risk.

Let me discuss two new studies, on which I am now engaged together with some colleagues,[1] that on the basis of preliminary results raise further serious questions about the usefulness of beta as a measure of risk. The first regresses common stock returns on a market portfolio consisting of stocks and bonds and finds that the rates of return on zero beta or very low beta stocks implied by the estimated return risk relationships are as a whole significantly and substantially higher than the returns actually realized by bonds with comparable beta measures. The second substitutes *ex ante* (expected) for *ex post* (realized) measures of return for a large sample of common stocks collected from a large sample of financial institutions for each of four periods in 1972, 1974, 1976 and 1977. Preliminary analysis suggests that these expected returns of individual stocks are not closely correlated either with beta or standard deviation measures of risk (computed from realized returns), and that variance or standard deviation does somewhat better than beta and adds significantly to the explanation of returns. This analysis also points to a moderately high heterogeneity in expected returns.

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[1] Randolph Westerfield and Michael Granito.

It seems to me that apart from the segmentation assumption, which most economists would like to use only as a last resort, the most plausible explanations of the differences in return between fixed-interest-bearing obligations and stocks, which have not yet been satisfactorily explained by differences in betas, are the differences in the volatility or standard deviation of returns and in the heterogeneity of expected returns among different investors. The important point here, however, is that we seem to need a fresh start and perhaps a new methodology in testing capital asset pricing theory against the observed facts. One such approach is the expanded use of survey information, which may make possible faster progress in understanding investors' behavior and expectations, whether it be reaction to risk or the expected rate and subjective probability distribution of returns which we now attempt to assess from ex post data.

In a different type of attack on current capital asset pricing theory, Roll has recently argued that the two-parameter asset pricing theory is defective as a scientific hypothesis, no valid test of this theory has ever been carried out, and seems to feel it is doubtful that a valid test can be made.[1] The main thrust of Roll's argument is that the theory is not testable unless the true market portfolio is known and used in the test since even a small departure from the true market portfolio may vitiate the tests. To support this position, he shows that is possible to construct a market proxy that supports the Sharpe-Lintner model perfectly even though it has a .895 correlation with the market proxy used in one well-known test which resulted in a rejection of that model.

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[1] Richard Roll, "A Critique of the Asset Pricing Theory's Tests," Cahier de Recherche No. 45, 1976, Centre d'Enseigement Superieur des Affaires de Jouy-en-Josas.

It would appear that Roll has simply rediscovered the fact that the testing of a theory is a joint test of the validity of theory and the reasonableness of the empirical constructs used in testing it. If Roll or anyone else had been able to construct a market portfolio which had a priori reasonableness and supported the Sharpe-Lintner model, this would constitute scientific evidence in favor of that model. However, to my knowledge, no one has yet been able to do this even though various plausible combinations of marketable assets (mainly stocks and bonds) have been tried. To demonstrate that a "curve-fitting" process can provide a portfolio which is called the "market" because it supports the Sharpe-Lintner model has no obvious relevance to scientific testing and does not seem to me to be terribly interesting.

Before leaving the area of capital asset pricing, I should like to point out that in "The Demand for Risky Assets," published in the December 1975 American Economic Review, Blume and I have attempted to help fill in a gap in capital asset pricing -- i.e., the paucity of empirical work on the determinants of the market price of risk in contrast to the plethora of work done on the interrelationships of the risk premiums among different risky assets. In that paper, on the basis of both detailed micro-data on the composition of household wealth and macro-market data on returns, we developed what we consider fairly convincing evidence that the assumption of constant proportional risk aversion is a reasonably good approximation of the market place and that the coefficient of risk aversion is on average well in excess of one and probably in excess of two.[1] If these results are correct, and I

[1]A change in this conclusion would require the questionable assumption that investors regard owned homes as riskless assets, in which case it would have to be concluded that investors are characterized by moderately decreasing proportional risk aversion.

have as yet seen no convincing evidence to the contrary, the question may be raised whether the continued expenditure of substantial intellectual and material resources on analyses which predicate quadratic, exponential, logarithmic or generalized logarithmic utility functions can be justified.

I am not arguing, of course, against the usefulness of theoretical analyses which explore the relevant economic properties of different utility functions, as exemplified by the well-known Cass-Stiglitz paper which developed the types of utility functions for which the separation theorem is valid. A more recent but as yet unpublished paper by Ross[1] addresses the other side of this coin, viz., the types of stochastic distributions of asset returns which would also imply the validity of the separation theorem. This type of theoretical analysis is highly useful, but once there is strong evidence on the type of utility function or return distribution which characterizes the market place, it seems to me that subsequent analysis is likely to be more productive when it assumes the validity of the indicated function or distribution.

Efficiency of Financial Institutions and Markets

The efficiency of financial institutions and markets in allocating investment funds has received increasing attention in recent years both by Government bodies and academicians. The thrust of many of these studies has been that efficiency could be improved by the removal of most types of Government regulation of both institutions and markets. In the United States, which has pioneered in securities regulation, the Chairman of the

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[1] Stephen Ross, "Mutual Fund Separation in Financial Theory -- The Separating Distributions," Rodney L. White Center for Financial Research Working Paper No. 1-76.

Securities and Exchange Commission earlier last year questioned the usefulness of the most basic tool of such regulation - disclosure of financial information in the securities markets. In Brazil where serious consideration is being given to the requirement of a SEC-type of disclosure to restore the shattered confidence of investors in the integrity of the stock market, I was asked last year to reconcile my own favorable views towards disclosure with supposedly contrary evidence that had appeared in recent literature.

I will not discuss here, partly in view of space limitations, what I consider to be the strong empirical evidence that securities regulation in general, and disclosure in particular, has probably improved the efficiency of the markets for new stock issues and for outstanding stock. In any case I have documented my views at some length and have dealt with supposedly contrary evidence in a recent paper.[1]

The only point I want to make here is that the basic conception of an efficient market in most recent studies of stock market phenomena does not seem very useful to me. This, of course, is a market in which every price fully reflects all the available information so that any new relevant information is reflected in prices extremely rapidly (and cannot be used to make abnormal returns). This conception does not consider differences in information available to different groups in the market or the relevance of the information to the subsequent earnings or riskiness of the stock and does not distinguish between a market in which information is sparse and of low quality and one in which it is abundant and of high quality.

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[1] Irwin Friend, "Economic Foundations of Stock Market Regulation," Resource Allocation and Social Institutions, M. Allingham and M. L. Burstein, eds. London: The MacMillan Press Ltd., 1976. A longer version appeared in the Journal of Contemporary Business, Summer, 1976.

Pareto optimality based on a grossly inadequate information set is hardly a satisfactory state of affairs, especially of course when the information set can be improved without excessive costs.

Corporation Finance

I have left until last a brief discussion of the deplorable state of the arts in corporation finance.

The measurement of even the average cost of capital to say nothing of the marginal cost of capital -- which is basic to all problems in corporation finance -- has not advanced greatly in recent years. A number of attempts have been made to construct economic models from whose solution the cost of capital could be estimated. These models have used both cross-sectional and time-series analyses and have been both single equation and multi-equational in scope. The problem, of course, is to estimate the required rate of return on common equity since it is relatively easy to measure the required rate for senior securities. Unfortunately we can have little confidence in the estimates of the required return on equity or on total capital generated by these models since there is no ultimate check in the form of a figure known to be reasonably reliable for any point in time. To achieve any real progress in this area in the near-term future, it seems to me to be necessary to make use of data collected from a comprehensive sample of investors to determine the anticipated rates of return required on individual stocks which they buy, sell or otherwise follow. Currently, a high proportion of the managers of large investment portfolios regularly make such estimates in their decision making process.

I am currently in the process of analyzing data on expected rates of return on a large number of widely-held stocks reported by a substantial sample of financial institutions in the survey data referred to earlier for selected periods in 1972, 1974, 1976 and 1977. The analysis also covers similar information reported by a large sample of individual investors in late 1975 on the required return for an average investment in New York Stock Exchange stock. Hopefully, we should obtain from these data additional insights into the required return on equity, though the existence of heterogeneous expectations obviously complicates the analysis.

Two of the oldest and most important problems in corporation finance -- the effect of capital structure and dividend policy on the cost of capital or on stock price -- have been solved time and time again, at least theoretically, with the differences in theory resulting from differences in the assumptions made. In this connection, I should mention that Lintner has recently completed a comprehensive theoretical treatment of the optimal capital structure -- covering such matters as the effects of bankruptcy risk, market segmentation and heterogeneous expectations -- which has just been published by Ballinger.[1]

There is as yet no reasonably definitive empirical work testing and implementing the theory of optimal corporate policy for either capital structure or dividend payout. Thus, it is frequently asserted that the empirical evidence supports the Modigliani-Miller (MM) theory that the cost of capital is invariant to the capital structure. The obvious fact is that under the MM theory, so long as the rate of corporate taxation is substantial and the difference between the effective rates of personal taxation of income

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[1] "Bankruptcy Risk, Market Segmentation and Optimal Capital Structure," in Irwin Friend and James Bicksler, eds., <u>Risk and the Rate of Return</u>, Ballinger, 1977.

from corporate bonds and stock is relatively small, the minimal cost of capital is achieved at a capital structure which consists entirely of debt. Since the average post-World War II ratio of debt in the corporate capital structure in the USA seems to have been less than 30%, it is difficult to take seriously any empirical "verification" of the MM theory. Some of the reasons which have been advanced to explain the apparent disparity between MM theory and the observed facts appear to be incorrect (e.g., the proposition that the cost of retained earnings is cheaper than the cost of debt) or to undermine the usefulness of the theory (viz., the proposition that there are major institutional constraints or non-monetary costs associated with the issuance of debt which are not included in the framework of the theory).

In recent years, theoretical attention has properly been directed to the implications of the risks and costs of bankruptcy for the MM theory, leading to the not too surprising conclusion that the cost of capital is not invariant to the capital structure, at least when leverage is high, and that there is an optimal debt-equity ratio. At least at the upper end of the capital structure range, "traditional" as distinct from MM theory has re-emerged. It can be questioned, however, whether bankruptcy costs alone or even different assessments of the risks of bankruptcy by borrowers and lenders are sufficient to explain the relatively low corporate debt ratio.

The question why corporations have not placed more reliance on debt financing seems to me to still remain unanswered. One possible answer may be that corporate management has shied away from debt financing to avoid the risk of bankruptcy and thus to preserve its own position, whereas this risk is more readily diversified by individual investors.[1] There are other tenable answers, including the possibility of segmented markets, but unfortunately we cannot distinguish among them.

[1] This traditional answer to the low level of debt is related to that implicit in some of the recent incentive-signalling literature which assumes that management sets the level of debt so as to maximize its interests.

A completely different answer to the debt financing puzzle has recently been given by Miller who states that even in the real world with corporate income taxes the original MM theory may have been correct and the value of the firm in equilibrium may indeed be independent of its financing so that there is no optimal capital structure.[1] Miller notes that it is possible that the relationship among marginal corporate tax rates, (T_C), marginal personal (including institutional) income tax rates applicable to income from common stock (T_{PS}), and marginal personal income tax rates applicable to income from bonds (T_{PB}) is such that there is no gain from leverage to the corporation. However, for this to be true in the MM theory, $T_C + T_{PS} \leq T_{PB} + T_C T_{PS}$, and for a marginal corporate tax rate presumably in the .4 to .5 range, it is difficult to conceive of any plausible values of T_{PS} and T_{PB} which would not again imply that the optimal capital structure is all debt.[2]

While the theory of optimal dividend payout seems more straightforward than that of capital structure, the empirical verification of that theory still leaves much to be desired. Thus, on the basis of historical regression studies in which a number of people have participated, including most recently Black and Scholes in the May 1974 Journal of Financial Economics, the current conventional wisdom seems to be that empirical evidence does not point to any influence of payout policy on stock price. This is a finding which is not theoretically surprising if differential transactions costs, including tax

[1] Merton H. Miller, "Debt and Taxes," Graduate School of Business, University of Chicago, September 1976.

[2] This would be true even if $T_{PS} = 0$. For U. S. Government bonds, whose income is generally taxed at the same rate as corporate bonds, the marginal tax rate of holders has been estimated to be in the .22 to .30 range (J. Huston McCullock, "The Tax Adjusted Yield Curve," The Journal of Finance, June 1975).

effects, are not large. It is also a finding for which I have considerable sympathy since together with a colleague, I reached a similar conclusion in an empirical analysis published more than a decade ago.[1] However, there are numerous other historical regression studies suggesting that a dollar of dividends has a greater effect on stock prices than a dollar of retained earnings.[2] Classical optimizing theory (similar to that employed by MM) would seem to suggest the contrary is true, or that 100% retention is optimal, since the marginal personal tax rate applicable to retained earnings is less than the marginal personal tax rate applicable to all income from stock.

Perhaps the most convincing though not necessarily conclusive evidence on the impact of dividend payout on stock price and hence on optimal payout is provided by the sample survey of individual investors in late 1975 referred to earlier in which most stockholders indicated they preferred increased payout to increased retention of earnings, with total earnings held constant.[3] This apparent preference for dividends by individual investors in late 1975 may be quite different from the situation in the 1960's. It may reflect the sorry behavior of stock prices (and capital gains) subsequent to the late 1960's and perhaps an especially poor performance of the so-called "high flyers" which generally had small dividend payouts.[4]

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[1] Irwin Friend and Marshall Puckett, "Dividends and Stock Prices," The American Economic Review, September 1964.

[2] Perhaps the most recent is Sasson Bar-Yosef and Richard Kolodny, "Dividend Policy and Capital Market Theory," Review of Economics and Statistics, May 1976. One of the most comprehensive is Mark Nerlove, "Factors Affecting Difference Among Rates of Returns on Investment in Individual Common Stocks," Review of Economics and Statistics, August 1968.

[3] This is, of course, not the same as preferring increased dividends to increased capital gains given the more favorable tax rates applicable to the latter.

[4] There is some reason to believe that financial institutions also increased their taste for dividends in recent years, perhaps in part because of the enactment of ERISA.

It seems to me therefore that even for dividend payout, the problem of determining optimal policy still persists. I suspect again that substantial progress, both for capital structure and dividend payout, will depend on the use of the available wealth of continuous cross-section data in this area, and more important on the periodic compilation of survey data from corporate management and investors on factors determining their attitudes and reactions towards different capital structures and dividend policies. The subjective survey data can be used to test and restate existing theory, the objective data to provide an independent and more rigorous test.

Some Concluding Remarks

To conclude, much of the preceding discussion simply represents further support for the position I advanced in "Mythodology in Finance" that methodological elegance should not be considered a substitute for substance, and by substance I mean solution of real world and not artificial problems. It seems to me that the pay-off in financial research is likely to be considerably improved by expending more of our resources on careful analysis of all of the available data rather than of a convenient small sub-set, and on collecting new data from special and regular surveys of corporations and investors on relevant financial characteristics and determinants of behavior. I would also hope that we spend a little more time in evaluating the potential social utility of the problems to which we devote our attention. As I suggested on an earlier occasion, it might be difficult to justify the amount of time spent on the beta coefficient (or stock options) as against that spent on the determination of an optimal system of financial institutions to promote economic growth and other social objectives,