

Methodology In Finance

by

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The state of the arts and the directions of current research in Finance, and in Economics more generally, are both exciting and depressing. Theory is more ingenious and rigorous, and empirical methodology is much more sophisticated. The tempo of research activity and the associated expenditure of resources have expanded greatly. Yet we have answers to relatively few of the basic questions in Finance either at the macro or micro level.

I am not referring to questions of the social relevance of the problems on which we have been spending much of our time, though certainly such questions can legitimately be raised. (E.g., consider the amount of time spent on the Beta coefficient vs. that spent on the determination of an optimal system of financial institutions to promote economic development.) Even in the most critical areas of Finance and Economics, which all of us would agree are of fundamental importance to the well-being of society as well as to the technical advancement of the arts, we can say little with reasonable certainty. To give one illustration, we do not know much about the timing of monetary policy effects on investment and saving and hence on aggregate economic activity.

We have similar chasms of ignorance in virtually every important area of finance -- e.g., capital asset pricing, financial institutions and markets, and corporation finance. Our ignorance unfortunately is frequently not reflected in our policy recommendations. Too often, recommendations are made for changes in existing viable institutional arrangements on the basis of a diagnosis which may have a better than 50% probability of being correct but where adequate

consideration has not been given to the costs of being wrong.

Moreover, in many instances we seem afflicted with misinformation or accept information of dubious validity, rather than recognize our lack of information or take relatively easy steps to fill in the gap in our knowledge. Frequently, we specify assumptions which are known to grossly violate the real world facts -- viz., people can borrow or sell short all they want to at the risk-free lending rate -- and justify our actions on the need to simplify theory and to appraise it only on the basis of its ultimate usefulness. This may be a perfectly sensible approach for theoretical exploration but is a potentially dangerous one when used as the basis for policy recommendations. Thus, consider the use made in the recent SEC Institutional Investor Study of the investment performance measure implied by the original version of the market-line theory, now generally recognized to lead to seriously biased performance estimates. At other times we specify the assumed nature of financial behavior largely on the basis of theoretical reasoning supplemented by casual empiricism -- e.g., when it is asserted that the utility of wealth is characterized by decreasing or at most constant relative risk aversion -- though a little statistical grubbing would indicate that the assumption is highly questionable and may seriously distort the resulting theory.

Worst of all, the trend in present research both in finance and in many other branches of economics seems to be more concerned with, or at least more likely to lead to, advances in methodological niceties than in substantive knowledge. Scholars in the social sciences and especially in economics and finance can engage in methodological research with the resources at their disposal much more easily than in answering specific policy questions, and frequently the intellectual challenge and material rewards are greater. The fundamental challenge of the social sciences, however, is to answer the relevant policy

questions. Towards this objective I suggest that a substantial amount of attention and resources should be diverted from the proliferation of theoretical and statistical models to the better exploitation of existing data and to the collection of new data, and from the now traditional aggregate time-series to the admittedly messier cross-section and especially continuous cross-section models.

In the remainder of this paper I shall select a number of examples to illustrate the absence of or questionable nature of "knowledge" in several important areas of finance, the problems involved in present methodology, and the promise of a changed emphasis in methodology. I should hasten to add that the methodology I shall be advocating is not especially new but has been grossly under-utilized.

Impact of Monetary Policy on Economic Activity

The most comprehensive and in my opinion the best econometric model which incorporates monetary variables and effects is the MIT-Pennsylvania-SSRC (MPS) model. In that model, the direct effect of monetary policy on economic activity takes place mainly through plant and equipment, housing and personal consumption expenditures. The model has not been able to ascertain any direct impact of monetary variables on business inventory investment, though there are indirect effects through new orders for producers' durables and of course through economic activity generally. Monetary policy according to this model directly influences plant and equipment through the cost of capital only, housing mainly through the cost of capital but also through capital rationing, and consumption expenditures only moderately through the cost of capital but mainly through a wealth effect.

The basic difficulty with the MPS model as with all large-scale econometric models is the small number of independent aggregate time-series observations available for determining the appropriate parameters in a model with a large number of structural equations and a large number of alternative forms to select among for each equation.¹ With the use of increasingly complex lag structures the number of alternative forms to choose from for each structural equation has proliferated greatly. A second difficulty with the MPS model is the weak statistical basis of some of the most important basic data used -- and here the potential error introduced may be greater than for other econometric models.

Business Investment. To illustrate these points, 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

¹ 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 extremely limited, even abstracting from problems of serial correlation and changes in the economic structure.

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 point might legitimately be raised that it is easy enough to criticize current procedures but the relevant question is what can be done about them. One obvious but perhaps not too helpful implication of these deficiencies is that the model results should be presented and used with appropriate rather than perfunctory qualification. However, 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 important, the direct impact of changes in monetary variables on business investment can be and have on occasion been analyzed from such surveys.

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.² 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 plant and equipment and inventory investment in 1966 and anticipated fixed investment in 1967, and of 1969-70 financial market developments on business investment in 1970 and anticipated fixed investment

¹ Zvi Griliches and N. Wallace, "The Determinants of Investment Revisited," International Economic Review, September, 1965.

² 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.

in 1971.

On the basis of these data, it was estimated that the direct impact of financial market developments during 1966 resulted in a reduction of only about \$500 million in plant and equipment expenditures and approximately the same amount in inventory investment during that year and a planned reduction of somewhat under \$1 billion in plant and equipment outlays in 1967. (Comparable data for inventories in 1967 were not available.) Not until the third quarter of 1966 -- more than six months after the decision to implement significant monetary restrictions -- were even the small average 1966 effects on plant and equipment and inventory investment achieved. The later survey found that the direct impact of financial market developments during 1969 and 1970 reduced actual plant and equipment expenditures by \$1 billion in 1970 and planned expenditures by \$1.4 billion in 1971 and inventory investment by \$900 million in 1970. Both surveys indicated that the direct impact of stringent monetary policy on business investment was generally light in the first two years after initiation of that policy and that the impact gradually increased over the two year time interval.

A rough comparison of these results with those implied by the MPS model is of considerable interest. If the two interest rates which are assumed to affect business plant and equipment outlays (i.e., the corporate bond yield and the stock dividend yield) are kept at their 1st quarter of 1966 levels but all other relevant explanatory variables take on their actual values, this model implies that expenditures on plant and equipment would have been reduced by not much over \$500 million in 1966 but by \$4 billion in 1967.¹ The 1966 figure is very close to that indicated by the survey but the 1967 figure is four times as large, with the monetary impact implied by the MPS model already twice that indicated by the

¹These figures are based on computer runs kindly supplied to me by Professor Albert Ando of the University of Pennsylvania. The use of 1st quarter rather than beginning of year levels of interest rates tends to understate somewhat the comparative impact of monetary stringency implied by the MPS model.

survey in the first quarter of 1967. A similar analysis of the MPS results for the 1969-71 period, where the two relevant interest rates are kept at their 1st quarter of 1969 levels, implies very little direct effect of monetary policies on plant and equipment outlays in 1969, a \$3.8 billion reduction in such outlays in 1970 and a \$9.8 billion reduction in 1971, compared with the much smaller figures of \$1.0 billion and \$1.4 billion in 1970 and 1971 indicated by the survey findings.

It is possible of course that holding interest rates constant at their beginning of period values represents easier monetary conditions, at least in real terms, than those assumed by businessmen in their survey responses to the effect of financial developments on their investment expenditures. If the real instead of the nominal costs of capital are kept constant at their 1st quarter of 1966 and 1969 levels, which presumably represented tighter monetary conditions in nominal terms than those assumed by businessmen, over three-fourths of the implied reduction in plant and equipment outlays in 1967 specified above would disappear, while those in 1970 and 1971 would be approximately halved. The result for 1967 becomes indistinguishable from the survey figure. However, the reductions in 1970 and especially in 1971 outlays are still considerably above those indicated by the survey data. Moreover, an alternative measure of the impact of monetary stringency on business investment provided in the second U. S. Government survey, which explicitly assumed significantly easier credit conditions than those prevailing during 1969 and 1970, indicated that even the more severe credit tightening implied by this assumption would have been associated with only a moderately larger reduction in investment (\$1.4 billion instead of \$1.0 billion) in the second year of this period.¹

To recapitulate, the survey findings agree with the MPS results on relatively little initial effect of monetary variables on plant and equipment, but seem to evidence less direct impact for the second year -- especially in the second of the two periods tested -- and substantially less impact in the third year. Unlike the MPS model, the survey data indicate a significant impact for inventory investment as well. Presumably, the MPS model

¹Survey of Current Business, December, 1971, pp. 19-21.

is deficient in its inability to detect financial effects on inventory investment. These effects are clearly indicated by the survey data which suggest that the effects are about as large for inventory investment as for plant and equipment at least in the first year or two following changes in monetary policy. There is of course no similar theoretical presumption that the MPS model is inferior to the survey results in its implications for more substantial second and third year effects on plant and equipment. However, 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 findings (other than the small effect on plant and equipment outlays in the first year after changes in monetary policy) in view of this apparent conflict of results. It does not appear likely that as much is to be gained by continued re-specification and re-estimation of the MPS model as by additional and more careful survey analysis and by other procedures.

Thus, the equations in the business investment sector of the MPS model might be significantly improved by the use of continuous cross-section data (i.e., data available for each of a number of economic units for each of a number of time periods). Such information is readily available on tapes for all sizeable corporations. Deriving investment and related functions for each of 1000 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 the equations in the MPS business investment sector.¹ 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. One of the most important channels 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

¹There is no increase in the number of independent observations for the pure interest rate, but there is for the overall cost of capital as estimated by the MPS model as well as for all the other relevant explanatory variables in that model.

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, and on the assumption of a life-cycle saving theory it is possible to apply some rough checks of reasonableness to the MPS estimate of the long-run effect on consumption of a change in the value of assets. However, the time-sequence of this effect is extremely difficult to determine from time-series data. The MPS finding that the entire impact on consumption of a change in the valuation of stock held takes place within a year, with over half taking place in the first three months, seems to me highly questionable.¹ Again, it does not appear possible in the foreseeable future to determine the validity of this result by longer or more sophisticated aggregate time-series analysis.

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 and 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. Thus, the Rodney L. White Center for Financial Research at the University of Pennsylvania has initiated an analysis of the relationship between saving and assets and capital gains from household data for 1963 (with income data for 1962 as well as 1963) collected by the Federal Reserve Board in their surveys of the Financial Characteristics of Consumers and Changes in Family Finances. These surveys, which oversampled upper income groups,

¹These comments were based on the mimeographed version of the MPS model dated October, 1971. I have been informed that a version now in progress implies a somewhat longer time lag in the reaction of consumption to changed stock valuation.

collected detailed information for more than 2,100 households in all income classes not only for income, saving and the values of major categories of assets held at the beginning and end of the year but also for the amounts of individual stocks held. As a result, the effect on saving of capital gains during the year can be studied on the basis of a couple of thousand rather than a handful of observations.

Before leaving this subject of estimating the effect on consumption of changes in the market value of stocks held, the very large margin of error in the available statistics on the aggregate market value of outstanding stocks owned by U. S. households should be underlined. A recent revision of the estimated market value of outstanding stocks made as part of the SEC Institutional Investor Study showed very little difference between the earlier SEC figures and the revised figures for the early 1960's but a \$283 billion or 37% upward revision by 1968, most of the discrepancy occurring after 1964.¹ The implications for an MPS type of consumption function are obvious. Unfortunately, even the revised series is suspect since it is based on fragmentary data (on dividend yields) for over-the-counter stocks. Clearly, it is important and fortunately it would not be too difficult to eliminate this serious deficiency in our statistical apparatus.²

Two other data deficiencies in estimating the consumption function should be pointed out. The first and more basic relates to the quality of the estimates of consumption itself. The national accounts and related data imply three substantially different sets of estimates of both saving and consumption functions.³

¹Institutional Investor Study, Supplementary Volume 1, 92nd Congress, 1st Session, House Document No. 92-64, Part 6, March 10, 1971.

²This goal could be achieved by the periodic application of procedures outlined (and followed for 1960) in Jean Crockett and Irwin Friend, "Characteristics of Stock Ownership," Proceedings of Business and Economics Statistics Section, American Statistical Association, 1963.

³E.g., see Paul Taubman, "Personal Saving: A Time-Series Analysis of Three Measures of the Same Conceptual Series," Review of Economics and Statistics, February, 1968.

Obviously, an acceptable reconciliation of these three different estimates of consumption should be given a high priority, though progress here is likely to be relatively slow. The second deficiency is the absence of data on entrepreneurial saving in spite of evidence suggesting that it may constitute a substantial part of total saving and that entrepreneurial saving and consumption propensities may be quite different from those of other householders.¹ Procedures for mitigating this difficulty are available.²

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 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.

This 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 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. 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 on stocks as a whole and the risk-free rate. Thus, this relationship

¹E.g., see Irwin Friend and Stanley Shor, "Who Saves," Review of Economics and Statistics, May, 1959.

²Ibid.

between return and risk was negligible in 1955-59 and negative in 1960-64,¹ though in both periods 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 as well as about the theory relating expected return to risk.

Recently, an attempt has been made to explain the observed return-risk trade-off for stocks by replacing the risk-free rates of return in the original market-line theory by a minimum variance zero-covariance rate. Theoretically, this modification of the market-line theory assumes a perfectly functioning short-selling mechanism which may be as objectionable as the assumption made in the original theory that an investor can borrow at the risk-free rate or equivalently short the risk-free asset. However, the revised theory may not be too seriously damaged by real world violations of the short-sales assumption, and there is at least limited validity to the proposition that the usefulness of a theory lies in its explanatory power rather than in the acceptability of its assumptions.

A colleague and I have stated elsewhere our reasons for questioning the validity of this revised market-line theory,² but there is certainly legitimate grounds for differences of opinion. Nevertheless the point I want to make here is that the theory in its present form seems largely immune from the type of statistical testing required to refute (or corroborate) it so long as it is maintained by its proponents that the theory can only explain rates of returns on stocks and that for some unspecified reason fixed-interest-bearing obligations are fundamentally different from common stocks in their risk-adjusted rates of return. Without more satisfactory statistical corroboration of this theory,

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

² Ibid.

I suspect that firm explanations of the linearity of the risk-return trade-off for stocks but not for other assets have yet to be developed.¹

One other general criticism should be made about the recent flood of empirical testing of different versions of the market-line theory. Almost invariably, equally weighted random portfolios have been used to test the theory though market value weighted portfolios are conceptually preferable, even if not necessarily statistically so. It is interesting to note that where both sets of weights have been tried, the use of market value weights consistently indicates a linear relation between return and risk, whereas this is not true of the equal weights in one of three periods tested.²

Risk differentials on the market portfolio. A striking characteristic of recent research on capital asset pricing is the dearth of empirical work on the determinants of the risk differential between the return on the market portfolio of all risky assets and the return on risk-free assets, in contrast to the plethora of work on the market-line theory. This risk differential between the market portfolio and risk-free assets, which is one basic ingredient explaining the absolute pricing of risky assets and the movements of the stock market as a whole, depends not only on the dispersion of returns on the market portfolio but also on the attitudes of investors towards such dispersion; i.e., on the extent of risk aversion. In determining the average differential in capitalization rates between risky and risk-free assets, it is necessary to know the form of the investors' utility function, i.e., the relation between the utility of wealth and its size.

¹ Still another yet to be tested modification of the market-line theory, which adds a third factor representing the return on an asset which is perfectly negatively correlated with the risk-free rate, is perhaps the most interesting revision of the original theory which has been proposed since it permits the required return on an asset to reflect any protection it provides against changes in the opportunity set. See Robert Merton, "An Intertemporal Capital Asset Pricing Model," February, 1972, Working Paper, Alfred P. Sloan School of Management, Massachusetts Institute of Technology.

² "A New Look at the Capital Asset Pricing Model," supra.

It is interesting to note therefore that there has been very little attempt to determine the form of the utility function from the available data on asset holdings by different types of individuals. Part of the reason seems to be that the profession has become enamoured of the potentially powerful implications of the market-line theory which abstracts from specific utility function considerations. Then again fitting a utility function to the data is less glamorous and more tedious than theorizing which largely abstracts from behavioral considerations. Perhaps most important, economists in recent years have generally been convinced that for most purposes it is sufficient to know that the market utility function has risk aversion properties somewhere between a negative exponential utility function, with constant absolute risk aversion and increasing relative risk aversion, and a log utility function, with decreasing absolute risk aversion but with constant proportional risk aversion. The authority generally cited for asserting these bounds for the utility function is Professor Arrow.¹

While no one is likely to argue with the plausibility of decreasing absolute risk aversion, the widely-held assumption of increasing (or at most constant) relative risk aversion seems highly questionable. At the theoretical level, the denial of the tenability of decreasing relative risk aversion is based on the assumed implausibility of a utility function which is unbounded either from above or from below.² It is not clear to me (nor to numerous other economists) why a utility function should be bounded from above. The ultimate justification for such an assumption must rest on the empirical data. The only behavioral evidence with which I am familiar that has been cited to support the assumption

¹ E.g., see S. C. Tsiang, "The Rationale of the Mean-Standard Deviation Analysis, Skewness Preference, and the Demand for Money," American Economic Review, June, 1972. Professor Tsiang questions Arrow's theoretical objection to a utility function characterized by increasing relative risk inversion but goes on to say, "Since the most commonly observed pattern of behavior towards risk of a risk-averse individual is probably decreasing absolute risk-aversion coupled with increasing relative risk-aversion when his wealth increases, the ideal utility function of wealth should lie probably somewhere in between the negative exponential function and the constant elasticity function . . .", p. 357.

² Kenneth Arrow, Essays in the Theory of Risk-Bearing, Markham, 1971.

of increasing or constant relative risk aversion are the studies which conclude that either the income elasticity or the wealth elasticity of demand for cash balances (usually money either narrowly or broadly defined) is at least one.¹ Of this evidence only the wealth elasticity is at all relevant since the income elasticity of total wealth may be greater than the income elasticity of cash balances even if the latter elasticity is in excess of one.

To the best of my knowledge, the available cross-section information seems to point to a wealth elasticity of liquid assets of well below, to close to, one if all tangible assets including consumer durables are included in wealth and lower figures if tangible assets are excluded.² The aggregate time-series data reflecting changes in supply and demand conditions, including those arising from changes in wealth distribution among different groups in the population, appear to be much less pertinent than the cross-section data.³

In conjunction with Professors Marshall Blume and Jean Crockett, I am currently working on a study which will utilize new detailed information from the 1963 Federal Reserve Board surveys mentioned earlier and from a large sample of 1971 Federal personal income tax returns to obtain new insights into the risk characteristics of investment by different wealth and other socio-economic and demographic groups in the population. Some preliminary tabulations from the 1963 surveys indicate that the ratio of cash balances (including time and all types of savings deposits) to net worth decreases as net worth increases from under \$10,000 to over \$1 million, with the decrease more pronounced if cash balances are related to total assets rather than net worth.

¹ Ibid.

² E.g., see Jean Crockett and Irwin Friend, "Determinants of Investment Behavior," Determinants of Investment Behavior, National Bureau of Economic Research, 1967, pp. 37 and 55-57.

³ Even the time-series analyses are not consistent in indicating a wealth elasticity of cash balances broadly defined equal to or greater than one in the period following World War II. See Allan H. Meltzer, "The Demand for Money: The Evidence for the Time-Series," Journal of Political Economy, June, 1963, p. 236.

If equity in housing and consumer durables are eliminated from net worth on the grounds that they are acquired for their provision of current consumption services at least as much for investment purposes and are not generally considered as risky or non-risky assets by their owners, the ratio of cash balances to net worth in 1963 decreased dramatically from 85% for the under \$10,000 net worth class to 3% for households with net worth over \$1 million. The net surrender value of life insurance¹ and equity in Treasury bills and savings bonds -- other "non-risky" assets -- also declined markedly as a ratio of net worth less housing and consumer durables as net worth rose. In contrast to these tendencies for non-risky assets, the ratio of the value of such "mixed-risk" assets as long-term bonds to net worth increased moderately, while the ratio of "risky" assets such as common stock to net worth increased markedly, as net worth rose. The qualitative nature of these results is not affected if family characteristics other than net worth (such as age, occupation, employment status, region, education, size of family, etc.) are held constant. Nor does it seem plausible to me that the qualitative nature of these results is likely to be substantially influenced by well-known limitations of cross-section data -- notably family tastes effects, possible transitory elements in the total and composition of net worth, and measurement errors.

The impression one obtains from these preliminary tabulations is much more consistent with decreasing than with increasing relative risk aversion. I suspect that further analysis both of the 1963 and 1971 data will confirm an earlier finding that the riskiness of the stock portfolio held by individuals generally increases with rising wealth.² This would appear to support further the thesis of decreasing relative risk aversion. However, the analysis so far does not explicitly adjust for the impact of the tax laws on the portfolio preferences of different income and wealth classes. Additional work will be required before we can specify with reasonable certainty the form of the investors' utility function, especially when the coverage of wealth is expanded to include human wealth, i.e., the current

¹ Information on pension and retirement funds was not available.

² "Characteristics of Stock Ownership," supra.

discounted value of expected future labor income. Nevertheless, for non-human wealth the available evidence seems to point to decreasing relative risk aversion -- strongly so if housing and other consumer durables are omitted.

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 elimination of Government incentives that encourage institutional specialization and by the removal of most types of Government regulation of both institutions and markets.

Thus, according to the Financial Commission Report released last year, the specialization of financial institutions should be discouraged by permitting much greater asset (and liability) diversification, but the formerly specialized institutions will have to pay for this regulatory liberalization by losing their Federal income tax and other privileges. The basic justification for these and other major recommendations in the Report are two articles of faith: first is the belief that as a general rule if all financial institutions are allowed to compete without constraints in all financial markets, funds will be allocated properly or as well as they can be; second is the thesis that, where public policy requires the channelization of resources to achieve certain goals, controlling, or using incentives to influence the composition of, the portfolios of financial institutions is an inefficient means of allocating resources as against direct subsidies. The first article of faith is one shared, with differing degrees of conviction, by most economists including myself. The second is one which seems to me to be considerably less evident and to require corroboration. Though the second point is repeated time and time again throughout the report, no corroborative evidence is given. Presumably, the relevant question is whether a dollar of indirect subsidy to a specialized

saving intermediary, say in the form of tax abatement or other Government assistance provided to a savings and loan association or mutual savings bank, stimulates more or a better allocation of housing investment than a dollar of direct subsidy to a prospective investor, say in the form of a partial payment of interest which a homeowner would otherwise have to pay. (New steps to improve the mortgage markets might have a lower cost-benefit ratio than either of these alternatives.¹) Unfortunately, the available information does not permit a satisfactory answer to this question. The problems here are similar to those discussed earlier in connection with measuring the impact of monetary policy on economic activity.

The fact that the state of the arts does not give us definitive answers to such public policy questions does not mean that we should not act on the basis of our best judgment at the time. I think that additional flexibility in the asset-liability structure of specialized deposit institutions is desirable. Many of the recommendations along these lines made by the Financial Commission parallel those I made earlier in the Study of the Savings and Loan Industry. However, I do not think that public policy should proceed unquestioningly from the unproved premise that providing Government incentives to influence the portfolios of financial institutions is an inefficient means of allocating resources to housing or other social objectives. Even if we assume that this premise is somewhat more likely to be right than it is to be wrong, the uncertainty of benefits and the cost of change, especially if the change is rapid and drastic, would suggest that the initial steps taken in the direction of flexibility guard against the danger of a severe adverse impact on the housing market. These initial steps obviously do not preclude further measures at some later time.

¹E.g., see the proposal for cash flow insurance by Jack M. Guttentag, "Changes in the Structure of the Residential Mortgage Market: Analysis and Proposals," Study of the Savings and Loan Industry, U. S. Government Printing Office, July, 1969.

Similarly, I do not understand, and disagree with, the Commission's recommendation that the standby power of the Federal Reserve Board to establish interest rate ceilings be abolished at the end of a ten-year period. Such complete faith in the performance of competitive markets appears to be unwarranted. The potential need for a standby Regulation Q seems to be reasonably well documented by the situation in the late Summer of 1966. It is entirely possible that a more competitive financial system might have avoided the serious difficulties at that time, but I see no good reason for advocating public policy based on that assumption. Optimal policy in the presence of uncertainty differs significantly from optimal policy in the world of certainty.

The reasons given for the Commission's rejection of asset reserve requirements also seem to me to be unconvincing, though I have no strong feelings on the desirability of such requirements. The Commission cites three types of arguments: difficulties of implementation; problems of equity; and efficiency in the allocation of resources. The first two appear quite weak, the third -- which is presumably the most important -- seems questionable. In connection with the latter, the Commission states that it "favors open and direct subsidies or alternatively, the use of tax credits" since "control of the portfolios of financial institutions is a costly and inefficient means of allocating resources."

The Commission does not present any evidence to support this statement. It admits that there is probably no "pattern of tax credits that would be appropriate over long periods of time" and that "due to long delays, the Commission is pessimistic about the possibility of using tax credits as an anticyclical device." The Commission recognizes that the cause of disruptions of flows of funds into the mortgage market is the response of the structure of interest rates to cyclical pressures and counter-cyclical policy measures. If tax policies are inflexible

and financial regulation undesirable, presumably there remains only the faith that with unfettered competition somehow the funds will be allocated properly. I do not share this faith.

I think the Commission has failed to distinguish between "controls" on financial portfolios (such as ceilings, quotas, etc.) and the use of incentives to influence the composition of portfolios. Why would one presume that the latter is "costly and inefficient," compared to a probably inflexible and cumbersome system of taxes and subsidies?¹

My last comment on the Commission's Report again is addressed to questioning the factual basis for its conclusions. The Commission concludes that as a result of its recommendations fostering freer competition not only will markets work more efficiently in the allocation of funds but "total savings will expand to meet private and public needs." I know of no hard evidence that the implementation of the Commission's recommendations will add to the nation's saving.

Perhaps a better example of the academic work on efficiency has been the profusion of studies examining the efficiency of the stock market and changes in its efficiency as a result of securities regulation as well as other institutional developments. I have indicated elsewhere my objection to the most common conception of an efficient market in recent studies of stock market phenomena as one 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).² This conception does not consider the relevance of the

¹On this entire subject of asset reserve requirements, see D. C. Rao, Selective Credit Policies and the Real Investment Mix, Ph.D. Dissertation, University of Pennsylvania, 1971, which greatly influenced the views expressed here.

²Irwin Friend, "The Economic Consequences of the Stock Market," American Economic Review, May, 1972.

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.

Using what seemed to be more appropriate measures of the market's performance, I concluded that contrary to the impression yielded by "random-walk" and related models of the market's performance, the market's ability to set up appropriate guidelines for channeling investment funds to their optimal use is not impressive, at least when viewed with the advantage of hindsight. However, hindsight is only useful if it can assist foresight and the \$64 question is whether specific steps can be taken to improve the market's efficiency.

My own evaluation of the evidence published to date is that securities regulation has almost certainly improved the efficiency of the market for new stock issues and has probably improved the market for outstanding stock.¹ Further work is required to substantiate the conclusion for outstanding issues and to explore the impact on market efficiency of specific regulatory requirements, but I see no basis in the record for the frequent assertion that securities regulation has impaired the efficiency of the stock market. Similarly, the available evidence does not support the common assumption that the growing importance of institutional trading, involving purchases and sales of large blocks of stocks, has diminished market efficiency.²

Corporation Finance

I have left until last, and in view of time constraints, shall say least about the deplorable state of the arts in corporation finance. In recent years

¹ Ibid.

² Irwin Friend, Jean Crockett and Marshall Blume, Mutual Funds and Other Institutional Investors: A New Perspective, McGraw-Hill, 1970; and Institutional Investor Study, supra.

the profession's attention has been directed away from corporation finance to capital asset pricing, though in the past year there has been some revival of interest in corporation finance. Any progress in capital asset pricing of course should have a spill-over to corporation finance, but the spill-over has not been impressive.

The measurement of even the average cost of capital to say nothing of the marginal cost of capital has not advanced greatly in recent years. One of the most substantial difficulties here is the absence of a satisfactory measure of the required rate of return on the market portfolio, or equivalently a measure of the risk differential between the risk-free rate and the average required return on common stocks. The market-line hypothesis provides a theoretical framework for estimating differences in the required rates of return on equities with varying non-diversifiable market risk or Beta characteristics. However, it is not actually very useful even for this limited purpose in view of the unsatisfactory nature of current market-line theory discussed previously as well as a substantially greater temporal instability of Beta coefficients for individual stocks than for portfolios.

A number of attempts have been made to construct econometric 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 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 survey a comprehensive

sample of investors to determine the anticipated rates of return 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. It is of course the ex ante required rates of return rather than the ex post realized rates which are relevant to the measurement of the cost of capital.

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 ad nauseum in theoretical terms. However, the implications of this theory have frequently been misinterpreted and as yet there is no reasonably definitive empirical work testing and implementing the theory.

Thus it is commonly asserted that under the customary perfect market assumptions, as incorporated in the well-known contribution by Professors Modigliani and Miller (MM), theory indicates that the cost of capital is invariant to the capital structure.¹ It is also frequently asserted that the empirical evidence supports that theory.² The obvious fact is that under the MM theory so long as corporate taxation exists the minimal cost of capital is achieved at a capital structure which consists entirely of debt. Since the average ratio of debt in the corporate capital structure seems to be of the rough order of magnitude of 20%,³ it is difficult to take seriously any empirical

¹ Franco Modigliani and M. H. Miller, "The Cost of Capital, Corporation Finance and the Theory of Investment," American Economic Review, June, 1958 and June, 1963.

² Robert S. Hamada, "The Effect of the Firm's Capital Structure on the Systematic Risk of Common Stocks," Journal of Finance, May, 1972.

³ This 20% figure, which reflects the valuation of equity at market rather than book, was obtained from an estimate incorporated for the post-war period in the MPS model. A crude independent estimate, based on Statistics of Income, 1968: Corporation Income Tax Returns and the revised SEC series on the market value of outstanding stocks, points to a somewhat higher figure for the end of 1968.

"verification" of the MM theory unless it is assumed that management is attempting to maximize its own interests rather than those of the shareowners. Some of the other reasons which have been advanced 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., 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.¹ 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 risk alone is sufficient to explain the relatively low corporate debt ratio.

Thus, an analysis of the relationship of returns to risk (Beta) for both bonds and stocks suggests that even without allowing for the tax advantages of debt financing, the cost of bond financing may have been substantially smaller than the risk-adjusted cost of stock financing and probably smaller than the risk-adjusted cost of internal financing.² Considering the big tax advantage of bonds, the question arises why corporations did not place even more reliance on such financing. One answer may be that corporate management in its attempt to avoid the risk of bankruptcy and to preserve its own position has shied away from debt financing, in preference to the retention of earnings, whereas this risk is readily diversified by individual investors. There is some evidence, of

¹E.g., Joseph E. Stiglitz, "Some Aspects of the Pure Theory of Corporate Finance: Bankruptcies and Take-Overs," Bell Journal of Economics and Management Science, Autumn, 1972.

²"A New Look at the Capital Asset Pricing Model," supra.

course, that the historically large risk premiums required on stock as compared with bonds has diminished in recent years.

After all these years, the relatively simple problems of determining the optimal capital structure and dividend payout policies under existing institutional constraints (but not under those imposed by management) still persist. It is possible that newer theories allowing for bankruptcy risks and other significant modifications of perfect market assumptions will help solve these problems. However, I suspect again that substantial progress will depend on the use of the available wealth of continuous cross-section data in this area, and on the 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 Comments

Much of the preceding discussion simply represents support for the position 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, on improving the quality and quantity of these data, and on collecting new data from special and regular surveys of corporations and households on relevant financial characteristics and determinants of behavior.

Obviously, this change in emphasis is not likely to solve all of our problems. Moreover, some of it will require a closer collaboration between the academic community and Government agencies, which necessitates cooperation on both sides. However, without

this change in emphasis, I strongly suspect that 50 years from now our successors will be engaged in more and more elaborate curve fitting of aggregate time-series data which will explain the sample periods even better than they do now but are no more successful in predicting the consequences of policy action. We may even still be "demonstrating" empirically the invariance of the cost of capital to the capital structure.