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THE ECONOMIC AND FINANCE LITERATURE AND DECISION MAKING

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AS A MEMBER of the Federal Reserve Board for seven years, I was a decision-maker, actively seeking needed techniques, guidance, and ideas from the literature in economics and finance. I spent a great deal of time reading, analyzing, and attempting to apply the publications of our field to policy problems. I must report that, as a consumer, I was far from satisfied with the product we offer. Today I would like to recount briefly some of my experiences and express some reflections thereon.

While my faith in economics as a decision-making tool was enhanced, I concluded that far too high a percentage of the literature on monetary problems is sterile.¹ In my judgment, a major portion of our current articles are not useful for decisions, nor are they likely to form a base upon which future knowledge to improve decisions can be built. They are neither applicable, fundamental, or incremental.

I found it useful to classify the problems I encountered in three ways:

First, policies are too often advocated—frequently in strong terms—which are based on limited analysis that falls far short of that required for decisions. This partial analysis considers too few of the elements that will determine the actual effectiveness of such policy prescriptions. Stress is placed on simple or single explanations for extremely complex situations.

Second, a great deal of effort is devoted to concepts which are and will remain non-operational. Predictions and prescriptions may be based on limited or false assumptions. Critical operating problems are neglected. Too great validity is ascribed to the analysis and its statistical tests.

Third, the research effort devoted to attempts to find more effective means of applying our tools and techniques is limited. There are many specific problems which appear amenable to analysis using economic techniques. But the effort given to a particular problem appears to have little or no relationship to the probability of reaching a successful solution.

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1. A more general description of the general problems and developments in policy and policy analysis can be found in S. J. Maisel, *Managing the Dollar* (W. W. Norton, New York, 1973).

I conclude that we ought to devote more effort to analyzing what we do. It seems to me that even a small investment of time in probing the difficulties of moving from theories to practice might have a high pay-off. We need more knowledge to be able to differentiate between an ultimately useful contribution and one that is merely publishable but perishable. We ought to spend more time analyzing what results we expect from our labor and what type of transmission process exists between our literature and its actual use.

We ought to look again at our journals and our system of rewards. Do we write merely to gain professional recognition and academic promotion, without paying any attention to the ultimate usefulness of our work? Would more stress on negative results and reviews of past theories and models improve our output?

Perhaps more attention ought to be given by decision-makers to attempts to formulate specific problems which could be made available for research. Wasted effort could be reduced if there were better ways of checking assumptions. Well-formulated proposals for needed research might be put out on bids, or at least be widely publicized.

I am aware of how difficult it is to change, but I also recognize how great are the opportunities for improvement.

OUR STRENGTHS AND WEAKNESSES

I have always assumed that most members of the American Finance and American Economic Associations have chosen their profession because of a concern with improving economic decisions. In a single year, 1972, the *Journal of Economic Literature* listed over 120 books and 1,100 articles under domestic monetary and financial theory and institutions, economic fluctuations, forecasting, inflation, and balance of payments and international finance. Most of them are concerned with policy problems. Many additional articles pertinent to monetary policy appeared under other classifications, while the number of articles, speeches, and advice not covered in the *Journal* was vast.

It is true that these articles are based on a wide variety of approaches—from almost pure mathematics to as pure institutional description; and, therefore, to the casual observer they may seem only indirectly related to policy. Yet I have always believed that we write and publish on the assumption that improved analytical techniques and knowledge will result from our studies, and that these, in turn, allow better decisions to be made in finance or the political economy. Not many of us think of our work as gaining merit purely through its literary excellence or general appeal to humanistic values.

There is, of course always a problem in differentiating between basic research upon which others will build, and work which is categorized primarily as “applied.” The fact that such a division is particularly difficult in a field such as money and finance, however, does not mean that it should be avoided.

Yet such seems to be the case. Only rarely do we attempt to estimate the probabilities that a typical article will really contribute to better decisions. We spend minimal time and research effort, either as individuals or as a profession, in trying to find out what it takes for an article to be a true contribution to

scientific knowledge. Most authors seem to assume that even though a specific article will not contribute directly toward better predictions or decisions, by entering the literature it will become a building block for future work, and eventually have the desired influence.

I confess that, as an author, editor, and reviewer for journals, I tend to follow this same path. *Mea culpa*. I recognize my own guilt as well as that of others. I think I know what makes a proper article. Its publishability depends on its meeting certain criteria for analysis, clarity, originality, and familiarity with the literature, and only incidentally on any estimate of its usefulness.

Yet as a consumer of similar articles, I find I question the criteria we use. While the monetary literature is voluminous, it seemed to me that only a minor part of the published techniques, analytical material, and advice was useful. In fact, in my less sanguine moods, I often felt that their total impact was negative. Too many articles appeared completely non-operational, both for the present and the future. I could find no relationship between them and the information required for actual decisions.

While most articles follow accepted patterns based on testing of hypotheses either by mathematical or statistical techniques, too many do not meet what I would consider even minimum standards for policy advice. They simply are not constructed to serve as a basis for predictions or decisions, even though most authors clearly believed that they were making a contribution to either positive or normative economics and that they were shedding light on significant issues.

At the same time, it was rare to find a study which, while lacking current applicability, could be welcomed and justified as making a fundamental contribution toward future knowledge. Some do contain enough data and facts or are basic contributions to be built upon, but these are few and far between. We give so little thought to what is basic and how knowledge is increased that any judgment along these lines is highly subjective. Yet, I think it fair to assert that few of the hundreds of thousands of pages written on these topics each year are fundamental. They must be judged on their own current or future applicability.

I admit that there are many who think it naive to expect internal improvements in our work. They point out that our pay-off functions are related to publishability and review by our peers, not to actual use. It is easier and less risky to write in an accepted popular mode than to undertake research on what we accomplish by our articles. Articles which are designed to make actual contributions to decision-making are likely to be harder and more time-consuming to write, and are also less certain of acceptance by editors and reviewers than are articles similar to those currently being published. Skeptics believe that our current literature properly reflects academic profitability, and therefore any ultimate impact on economic decisions is of low importance. Of course, to the extent these observers are correct, a reexamination of some of our basic concepts and procedures is in order.

I also want to be clear that I am not denigrating the usefulness of economic tools and analysis in decision-making. The exact opposite is true. Together

with most economists who have made or watched decisions being made in the government, business firms, or other types of institutions, I believe that economic analysis plays a vital role in improving the quality of decisions. Such basic tools of the economist as opportunity costs, marginalism, supply and demand, the market mechanism, and the various fallacies such as that of composition are all valuable. Even more worthwhile is the thought process by which we marshal facts into models which can then be manipulated, both mentally and by computers, to find logical relationships.

On the other hand, our process of simplification creates obvious problems. Our analysis requires the selection of limited but significant facts and concepts, manipulation of which we hope will give us insights into real world behavior. While we require simple theories, we must bridge the gap between our theoretical world and the economy before they can become operational. It is this confrontation of theory and reality which is not well handled. In many cases, the existence of the problem is not even recognized.

DETERMINING CREDIT VOLUMES AND ALLOCATIONS PRIMARILY THROUGH PRICE MOVEMENTS

To make my thesis somewhat more concrete, I would like to discuss three examples from the sphere of monetary policy which I believe illustrate some of the problems encountered in attempting to improve current decisions.

As western-trained economists, most of us start with a bias in favor of the price system as the most efficient way to allocate resources. We recognize the great difficulty of attempting to solve millions of individual economic decisions through fiat or administrative rules and decisions. Each of us has a long list of horrors that result from the suppression of market forces.

Based on a recognition of the allocative efficiency of a perfectly operating market, numerous articles have appeared in recent years attacking the use of interest rate ceilings and regulation of financial institutions as policy tools. The report of the President's Commission on Financial Structure and Regulation, December 1971 (The Hunt Commission), The President's Recommendations for Change in the U.S. Financial System, August 1973, the Committee Print on Financial Institutions (Subcommittee on Domestic Finance: Committee on Banking and Currency, U.S. House of Representatives, 93:1)—all recommended doing away with ceilings on deposit interest rates. During the past year, the Federal Reserve moved to implement this general philosophy by increasing its reliance on movements in rates, in contrast to ceilings or regulations, to determine the amounts of money and credit created.

In these recommendations, we see a specific application of popular theories to the decision-making process. A large number of professional analysts have urged policy-makers to shift from existing procedures. The Federal Reserve and the Treasury have accepted their advice. Yet, I wonder whether the advice is not an example of partial analysis leading to a conclusion which would not be supported by a more complete model of the policy issue. Policy-makers are often accused of using an excess of intuition, while economists may well be guilty of an excess of abstraction. I doubt that either excess can lead to a logical policy.

We all recognize the problems and dangers of our analytical techniques. Most of us believe we carefully avoid common errors. Yet on the whole, we are not cautious. We give lip service to qualifications, but thereafter disregard them because we know that careful attention to our own strictures reduces the likelihood that anyone will pay heed. We warn that our assumptions and techniques are limited. We point out that other variables may be significant. We recognize the danger of partial analysis, but we still act as if the difficulties do not exist. We persist in rendering advice or drawing strong implications from our limited analysis.

When we examine this question of interest ceilings and credit regulations, we find that at least four different well accepted and well thought out economic models deal with separate parts of this problem. But the policy recommendations seem to be based almost entirely on a single, simple model—that of perfect competition. Much of the analysis has simply posed the question of whether deposit rate ceilings and any aids or penalties for borrowers would exist in a freely competitive perfect market. Since under rather simple assumptions they would not, it is deduced that the regulations lead to a less than optimum allocation of resources, and so should be abolished. As a result, the really critical questions are avoided by most of the analysis. The real world is uncertain, dynamic, oligopolistic, and characterized by significant differences in income. Clearly each of these features should be dealt with in our analysis. We have other models and basic economic theorems which ought to be included in addition to the model of pure-perfect competition. Each of these seems to have something to offer in the debate over monetary regulation. Failure to include them in most of the analysis on this topic seems to me all too typical of a great many policy debates.

Perfect knowledge versus uncertainty. We all recognize how limited our knowledge may be in specific situations. We have well-developed theories of choice under uncertainty. Solutions based on uncertainty frequently differ drastically from those arrived at under the assumption of perfect knowledge. Yet even recognizing this fact, most analysis, such as that of the effect of regulations, continues to assume perfect knowledge. The analysis consequently fails to come to grips with the forces which determine actual outcomes. Because the decision-maker frequently recognizes his limited knowledge, the theories and advice based on certainty must be heavily discounted.

Comparative statics versus dynamics. In the same way, even though we introduce time and lags, much of our analysis depends on comparative statics. It may have little or no applicability to the problems of a dynamic world. Rarely, however, are the relationships examined between what appear to be logical consequences of the static approach and those actions which are required by the dynamics of a situation. Most analysis assumes that we move from one equilibrium situation to another; or, if we are in disequilibrium, we will move back. In contrast, many decisions must be based on the assumption that the situation is one of disequilibrium with strong possibilities that movements will continue away from, rather than toward equilibrium.

In the past, our financial system has been very unstable, apparently because of the dynamic properties of our financial and economic structure. Very little

analysis has dealt with dynamic problems, particularly ones which take into account the structure of the system, poor information, and significant time lags. As a result, we cannot say whether the proposed changes would solve the problem of stabilization as well as the existing system does. Interest rate ceilings have been used to reduce some unstable forces. They act to increase the predictability of policy actions, a most desirable feature. The costs of removing the predictability and possible stability have simply not been considered.

Pure and impure competition. In our work on industries and markets, we stress the effect that impure competition has on solutions. In many cases, marked improvements are possible through alterations in the market structure. Still, a great deal of our literature neglects these factors. We assume that the analytical results obtained from an assumption of a purely competitive market will be applicable to the impure case, even though experience and specific analysis tell us they are not.

This, too, is a problem in considering credit allocations. We know from the theory of the second best that we cannot show whether doing away with existing regulations would actually improve efficiency. We know the existing market is segmented by geography, types and size of savers, lenders, and borrowers. There are significant non-competitive pockets. The system of credit allocation contains many non-price elements. The list of lenders and borrowers who benefit from tax exemption, excess tax deductions, and subsidies is long. The critical question is whether making partial changes toward freer markets in some of these attributes would actually improve efficiency. It might, but not necessarily. The logic of the situation requires a careful analysis of the facts, not a deduction from abstract theory. Yet, the appeal is almost entirely to the theory, not the facts.

The effect on income distribution. Our existing system also expresses certain social priorities and acts to allocate income. The various proposals would have quite drastic income effects. Again, however, an analysis of who would gain and who would lose from the proposed changes in the existing structure is lacking. Even though in our theorems we are normally cautious and point out potential costs when income distributions are changed in efforts to achieve possible improvements in efficiency, such strictures about the value judgments involved do not appear to have received much weight when these recommendations were made.

But on the basis of considerable thought and analysis, I, at least believe it highly probable that if the changes proposed in the financial structure are adopted in approximately their present form, they will be harmful. They will aggravate the instability of our economy; they will increase the inequality of income distribution and of sacrifice, and they will lead to a less efficient allocation of financial resources.

These intuitive judgments grow out of a study of how and why our existing institutions arose, how they operate, and what dynamic problems they face. I make no claim for their necessary validity. I do believe, however, that there is little doubt that if analysis of these problems were based on a more complete model—still simplified but including more attention to dynamics, market structure, costs of information, and problems of income distribution—we

would arrive at quite different, but better and fairer proposals than the recent ones.

More important, I think, this example is both significant and typical. The policy-maker is often faced with the choice of making a decision based on excessive abstraction or excessive intuition. Since a choice is necessary, he must select one or the other, even though both are bad.

We have a most valuable tool in our concept of the importance of properly functioning markets in implementing decisions. Yet, much of the literature turns out to be far less useful than it should be because the use of the tool is both too limited and too abstract. We deduce theorems and offer predictions and prescriptions based on only a small part of the total model of how the market works. Failure to analyze most of the factors which influence the final actions means that the usefulness of whatever impact the literature may have is greatly reduced, or even negative. Because it fails to deal with enough facets, actual choices based on too great abstraction may be poorer than if based on intuition alone.

MONETARY TARGETS OR VARIABLES

Another example of problems encountered in trying to utilize the literature can be found in the sphere of Federal Reserve monetary operations. In recent years, this has been one of the most active areas in the monetary literature. The proper targets and variables of monetary policy, as well as how operations should be conducted, have been the center of hot debates. Such studies have been valuable in forcing the Fed to reexamine and alter its operations. But if we ask how efficient (in a profession dedicated to efficiency) has the debate been in achieving its purpose, I believe the answer would be—not very. As one winnows the literature to find workable solutions, he finds only a small percentage of wheat in relation to the chaff. Efficiency is lowered through errors both of commission and of omission.

We all recognize that many conflicting economic views arise wholly from differences in initial assumptions. In much analysis, the underlying assumptions are simply too far from reality to lead to logical insights. In too many cases the situation is even worse: the assumptions are—and upon even slight investigation would be found to be—contrary to fact.

Any reader of the current literature must be struck with how frequently legitimate manipulations of simplified mathematical equations or statistical techniques are treated as throwing a great deal of light on actual relationships existing in the economy. Often this is not the case. Nothing in these techniques increases the validity of the initial hypothesis. Depending on what has been altered or changed in the selection of assumptions, the actual solutions required in the economy may be, and frequently are, completely opposite from those suggested by the theories.

These problems appear all too often in the monetary debates. Many articles and whole series of controversies turn out to be of no help because they are founded on incorrect assumptions of how policy has been managed. Straw forts are erected and skilfully demolished, but since they were built with false foundations and poor materials, their impact is slight. Even a small shift in

effort from time spent in analysis to time spent in assuring the correctness of the facts entering the analysis would seem worthwhile.

Other critical problems arise when one examines articles with a statistical or econometric base. Frequently they show interesting approaches. But too many base their claim to usefulness on the validity of their statistical underpinning. Although they contain high correlations and significant coefficients, the apparent validity seems to disappear upon examination. Fairly often a careful scrutiny of the data shows that their definitions do not coincide with the analysis.

As part of our waste of effort, it is striking how rarely prior econometric results are re-checked against events. We know we have major problems in determining what our results really mean, but the effort involved in trying to find out what use can be made of prior or current results is small compared to that expended in adding new studies of dubious usefulness. We all know how often, in attempting to update our own work, we find new coefficients in conflict with the initial theory. Yet we see only rare revisions of prior work.

In those cases where replications occur, the use of current data frequently shows results far from those predicted. Coefficients will often appear non-significant. They may even have changed signs. Again, I have a strong impression that more effort spent in devising procedures with a higher probability of future validity and less time spent on the theoretical analysis would increase the usefulness and productivity of a great deal of work.

Let me cite in this category another specific example of where it seems to me that we have plunged ahead with only minimal attention to what meaning we can attribute to our results. In the past several years, there have been many studies comparing the results obtained by simulating large-scale econometric models under alternative hypotheses. Each time I see one, I wonder what I am expected to conclude.

We recognize, as do most users of these models, how far from exact they are. We know that when used to forecast or to analyze specific problems, managers of these models usually make a number of special changes. We know that the sectors of the models, and particularly those related to monetary policy, contain large errors. As an example, for the past three years the mean average annual error for four major econometric models in predicting residential construction expenditures (usually the sphere with the greatest reaction to monetary changes) a year ahead was 18 per cent.²

Does it not seem likely that the results of the simulations are going to depend far more on the special features built into the model by its developers than on the properties of the real world? What would constitute a proper split of time between continuing a line of analysis which is interesting and suggestive but whose meaning and significance are unknown, and putting more effort into ascertaining what type of validity such studies have?

MONETARY OPERATIONS

In contrast to the vast number of articles written on the theory of selecting the correct monetary variable, almost no thought has been given in the litera-

2. S. K. McNees, "The Predictive Accuracy of Econometric Forecasts," *The New England Economic Review* (Sept./Oct. 1973).

ture to the problem of how operations should be conducted to control a particular monetary variable. Few if any authors who write on monetary targets have even considered the problem of how to make their recommendations operational. Most often, the problem is simply assumed away. It is accepted as a fact that the Federal Reserve controls the money supply or interest rates. As a result, analysis of how such control can be accomplished is minimal.

Yet, this problem is one that raises most interesting questions, some of which seem quite amenable to economic analysis. Because of our traditions of simplification, we often appear to assume that we can obtain efficient policies only by finding the true answers to our problems. We devote far less effort to finding paths that will be relatively efficient even if they are not completely correct. From an operating point of view, however, the alternative approach may be far better. Given the high risks of playing against nature, we may be far better off minimizing our costs by developing better control and feedback systems rather than trying to improve systems which will be efficient only if the forecasts are completely correct.

This sphere of monetary operations is a good example of where improvements might be possible using other approaches than the traditional ones. The Fed has an efficient feedback system for so-called defensive operations. Similar ones might be developed relating changes in reserves to the movements of the monetary variables.

Equally important, there are many unanswered questions about what the movements in the operating and monetary variables mean. Anyone obliged to make decisions must be only too well aware of the limited work accomplished in such critical spheres as seasonal adjustments and index number problems. Actions must be taken, but they are based on uncertain data. Decided improvements appear possible if policies are related to the existence of uncertainty, while at the same time the degree of uncertainty is reduced.

CONCLUSION

I am aware that many of you may mentally dismiss my comments as either committing the common error of not recognizing the importance of basic research and incremental knowledge, or as showing the typical biases of one who has been responsible for policies. I hope the number who react this way will not be too high. Although my comments and examples are personal, they are based on conversations with many decision-makers. One of the purposes of the American Finance Association has been to bridge the gap between theory and applications. The problem does not seem to me to be diminishing. It continues to require constant attention and thought.

Let me conclude by going beyond the examples and problems cited and try to generalize with respect to what types of action I, as a policy-maker, feel are most needed to make the articles and books I read more useful for shaping decisions.

Most important would be greater attention to the factors usually assumed away: lack of knowledge, existing institutions, dynamics, disequilibrium, risks and uncertainty. It is not clear what the applicability of a typical article is or is expected to be. The chain of assumptions which must be true if it is to throw light on events is simply too long to be believable. As one attempts to move

from link to link, many of them are likely to be rejected as improbable, impossible, or merely unproved. Frequently the analysis can be used only in the context of a subjective value judgment based on the experience and beliefs of the policy-maker.

More service would be rendered through a greater analysis of the costs of moving from here to there. What will be the path to a desired long-run equilibrium? Will this path be less costly than that of continuing existing policies which, in theory, would not lead to as efficient a long-run solution? What about the costs of change, both in terms of existing institutions and income distribution, and also costs to the body politic? More work is being done on the theory of second best. Perhaps greater attention to it would benefit most policy analysis.

It appears also that less traditional approaches to problems may bring about large potential pay-offs. We recognize the costs inherent in risks and uncertainties. Limited effort is being spent on the theories of handling uncertainties through control systems, feedback, diversification, arbitrage, etc. A great deal of our effort is still concentrated on the assumption that we will do better by improving our models and forecasts in the traditional mode. This may be wrong. We might move forward faster if we spent more effort on devising improved operational systems.

Greater attention should be paid to building on existing articles. The waste of the past through neglect is so traditional that only feeble efforts are exerted to make replication of previous studies possible, even by their original developers. The studies which try to check what happens as models age—such as recent ones dealing with econometric models—show that the task is difficult, but the pay-off appears large.

Perhaps we simply need more debates over methodology. As I said initially, our basic economic techniques seem extremely useful in the decision process. On the other hand, this may well not be the case for a great deal of our effort. Somewhat more time spent on trying to improve our own efficiency would hopefully be most productive. Such research and analysis should allow us to build better models of our own production functions as well as allowing us to fill the existing gaps in our system of output.