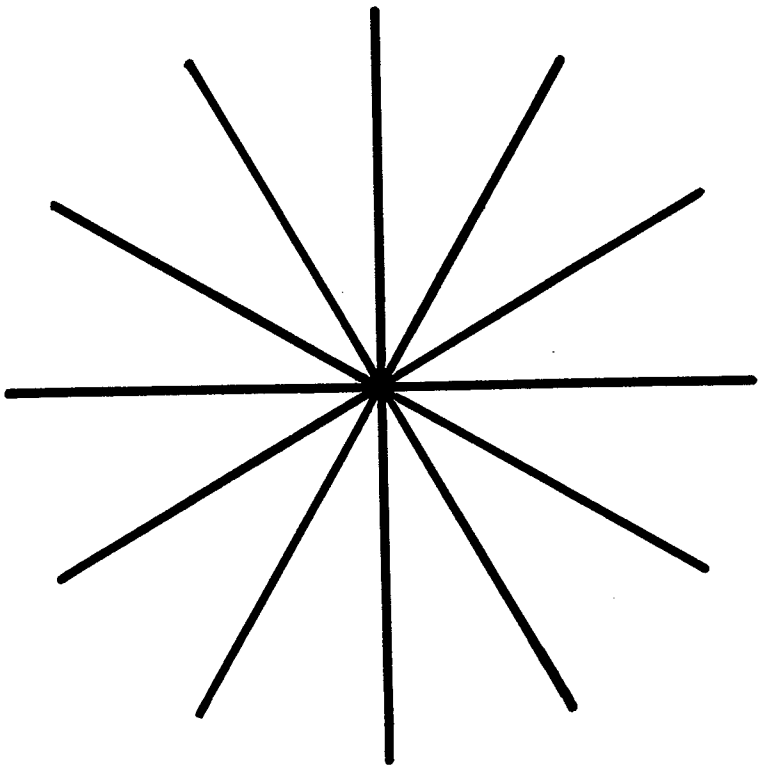

ECONOMICS—*A HALF CENTURY OF RESEARCH 1920-1970*

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By issuing its findings in the form of scientific reports, entirely divorced from recommendations on policy, the National Bureau hopes to aid all thoughtful men, however divergent their views of public policy, to base their discussions upon objective knowledge as distinguished from subjective opinion.

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(Resolution adopted October 25, 1926, and revised February 6, 1933, February 24, 1941, and April 20, 1968)

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I

Papers Presented to the Board of Directors at the Spring Meeting, 1970

INTRODUCTION

John R. Meyer

Strong historical antecedents support the view that economic research has been best when it has been relevant. Certainly, that historical lesson seems authenticated when one looks back over the NBER's first fifty years. Indeed, the Bureau was started because men of affairs, of many political and philosophical viewpoints, perceived a strong need to establish as objectively as possible the facts about certain economic policy issues of their times.

At the Bureau's inaugural, two central policy questions dominated all others. The first of these was how income and wealth were distributed among different groups in society. The second was a concern with how to avoid major economic depressions and all the social and human losses attendant thereon. Both of these issues, that of distribution of the national wealth and the avoidance of business cycles, still remain with us. But I think it is safe to assert that the urgency and perceived severity of these issues, particularly the cyclical one, is a good deal less today than it was fifty years ago—and in no small part because of the NBER's contributions. The distribution question, for example, though it remains quite cogent, has assumed new dimensions. No longer is the focus exclusively on the allocation to different wealth classes. Increasingly, the issues of concern are how different minority groups, regions, and organizations participate in the national wealth and its growth over time.

In its fifty years of existence the NBER has not concentrated, of course, on only two problems. As the years have gone by, the Bureau has undertaken research in many other areas of policy concern. To a considerable extent these other interests have focused on what makes the national economy grow. Among the questions addressed have been: How do we measure productivity? How do we mobilize financial resources for investment in the human and physical capital needed to increase productivity? How do we organize our industry and market structure so as to provide the proper incentives for growth?

Certain practical lessons about quantitative economic research seem also to have been learned over the course of the last half century at the Bureau. We have learned, for example, that good quantitative research in economics can be expensive, both in terms of time and money. We have also learned that, to be done properly, it usually requires some minimum scale, again of time and money. As a rough rule of thumb, I would guess that effective quantitative research in economics involves projects of at least two or three years duration and no fewer than three to four professionals simultaneously. But I should immediately confess that these numbers are based more on intuition than on any hard empirical research or information!

Nevertheless, if correct, certain important conclusions about the organization of good empirical research flow from these observations. The first of these, quite simply, is that the basic problem in designing quantitative research programs in economics is to identify problems as early as possible, and ones that are likely to be with us for some time into the future.

The negative inferences to be drawn from this rule are quite as important,

perhaps more so, than the positive ones. The rule obviously suggests, for example, that the more transitory policy problems are probably well avoided as topics for serious quantitative research. It would imply, in short, a focus on structural problems—on avoiding investigation of the symptomatic as contrasted with basic causal or behavioral relationships.

An emphasis on longer-term structural problems has several implications. First, in today's context it suggests a concentration on the study of growth processes. Second, it implies the need for a fairly broad historical perspective underlying the research design. Third, it suggests an emphasis on dynamic as contrasted with static models. Fourth, it would argue for less concern about equilibrium conditions and more involvement with the processes by which we move from one equilibrium to another. Finally, it would point toward a very considerable focus on technological change and the processes by which that change is achieved.

All this was very well summarized years ago when it was said, I believe by Schumpeter, that "the really important economic problems are typically identified with what the economist puts in *ceteris paribus.*" Changes in technology, tastes, and income distribution are typical items economists hold constant, and they are surely among the least constant of forces in our real world.

In designing a good research program in quantitative economics the major problem thus becomes that of identifying the substantive issues that will concern economic policy makers five years or a decade hence. That must be the constant preoccupation. With that in mind, we at the Bureau have sought the help of others in making such identifications, and have organized the effort into a series of colloquia to commemorate our fiftieth anniversary. The details of these colloquia are discussed elsewhere in this report (p. 132). Suffice it to say here that we have organized these meetings under six major headings, representing research interests of long-standing concern and involvement: (1) business cycles and forecasting; (2) public finance and expenditures; (3) human capital and its development; (4) financial institutions and markets; (5) the processes of economic growth; and (6) industrial organization and the functioning of markets.

As we begin this systematic survey of our research priorities for the next decade, I suspect that we shall find that the future policy problems quite naturally fall under three major headings. The first of these will be what we might call problems that "constitute more of the same." The second will be mutations or adaptations of present interests and problems. The third, and by far the most difficult to identify, will be entirely new departures and policy interests.

Thus, I shall hardly be surprised if we conclude that some problems of the 70's are nothing other than a continuation of problems already with us. To include these in our research interests it is really only necessary to decide that they are not likely to be solved quickly. A few examples quickly come to mind: understanding urban structure, that is, the ways in which urban complexes evolve and change over time in response to economic, political, legal, and social conditions; better comprehension of the impact of inflation and changes in industrial, demographic, and market composition on the

ways in which we save and allocate investable funds; the relationships between local, state, and federal public finance as outlined in our last *Annual Report* by Bossons and Shoup; or the problems and questions raised by changes in international comparative advantage as suggested by Lary's survey in last year's *Annual Report*. One hardly needs much insight to suspect that all of these problems will be of concern for at least one more decade or so!

Similarly, certain problems of the 70's can be forecast simply because they will represent mutations of present problems. For example, economists almost surely will try to measure some of the negative aspects or externalities of economic activity. Certain environmental effects, such as air and water pollution, are perhaps the most obvious subjects for such attention; at least the most widely discussed. But there are many others. For example, deterioration in performance caused by increased congestion on major public transport facilities, such as highways or airports, clearly constitutes another area of important negative externalities worthy of investigation.

In general, the study of negative externalities is a quite natural extension of efforts, already under way at the Bureau, to better understand the contributions of nonmarket activities in our economy. Thomas Juster discusses these problems, the measurement of externalities and nonmarket activities, more thoroughly in a special report which immediately follows this one.

Concern with negative externalities, moreover, may lead us into some entirely new departures and research interests. We may be led to substantially rework our entire theory of consumer behavior, for example, to reflect new information and concepts on the allocation of time, or on the role of expectations in conditioning savings behavior under different regimes of price stability or instability. Perhaps, too, this new theory of consumer behavior might be more psychological and behavioral and less normative than the conventional theory. Furthermore, it might lead us into some radically different policy conclusions, perhaps by providing us with an improved capability to forecast the effects of tax or other policy changes under inflationary expectations.

I suspect that an interest in negative externalities will also lead us to investigate how we might use the price system in rather new and different ways, particularly to achieve stipulated social objectives in sectors where we have not relied extensively on the price system to date. The only alternative to better use of the price system to mitigate some problems may well be detailed planning beyond the present state of the art or conventional planning capabilities. Several possible areas for application of the price system to achieve efficiency or other objectives suggest themselves: major airports, urban streets and highways, parking facilities, postal services, legal and court services, communication services, common carrier freight services, commuter and transit services, military manpower recruitment, airline fares, and medical and hospital services. One can see by scanning the staff progress reports later in this *Annual Report* that many of these problems are already of concern to economists. Incidentally, only by better understanding the price system, its applicability and its limits, will we be able to estimate many of the benefits of various public and private programs aimed at correcting environmental and similar problems. And, only by the application of the price system

will we be able in many cases to keep the costs of corrective action for such problems within reasonable bounds. After all, only in that way will we be able to identify the relevant margins of costs and benefits to be equated in an efficient solution.

This very brief list of new research possibilities is of course not exhaustive. I am sure that our fiftieth anniversary colloquia will uncover many suggestions and possibilities totally unrelated to those mentioned. Furthermore, I am sure that as a result of our deliberations we shall be able to define our research interests and objectives far more precisely than now. In essence, this list represents nothing more than a very simple and exceedingly brief summary of where we stand in our staff evaluations at this point in time.

Defining research priorities also involves, of course, more than simply recognizing or forecasting future areas of policy concern. There are also important problems of "research logistics," that is, of providing supporting data and methods to do the research. Indeed, one might suspect that an organization like the Bureau has at least as much of a role to play in meeting these needs as in the research itself.

I advanced some speculations in last year's *Annual Report* as to what some of these new methodological developments might be. One particular aspect of these developments that warrants special emphasis is the role of the computer and the technological revolution it is creating in economic research. As suggested earlier, there are reasons for believing that the emphasis in economics will increasingly be on complex dynamic models, often of a continuous disequilibrium character. The only feasible way now known for handling such efforts is through computer simulation. With the computer one can embody in the model nonlinearities, interdependencies, and other complications that defy simple mathematical representation. Mathematical modeling may also undergo a revolution in language as a result of the computer, with macrocomputer languages replacing the calculus and algebra as the major mode of expression.

With complex computer models, complex data bases will also be needed. During the next ten years the multivariate cross section will probably be the basic data source for most empirical research in economics. These cross sections, moreover, will increasingly be augmented by special surveys and censuses. The emphasis on cross-sectional microdata follows almost automatically once it is observed that these samples must contain as much information as the aggregates created from them. The major reason for relying exclusively on the aggregates in the past has simply been that we did not have the capabilitity to handle the bookkeeping or data reduction problems posed by these large cross-section samples. And, of course, there are important privacy or secrecy problems that should keep us from using totally disaggregated and identifiable data on individual businesses, households, or establishments. The computer, properly used, provides a sensible way of meeting some of these problems; that is, of doing the data reduction and the bookkeeping while still maintaining the privacy and guarding the identity of individual sources.

As suggested in last year's *Annual Report,* and as many of the individual staff progress reports within this report indicate, new statistical techniques

may also be required when employing these new data bases. In general, I suspect that these new data will increasingly move us beyond the traditional econometric preoccupation with regression analysis, and will involve economists increasingly in other multivariate analytic techniques.

This new emphasis on microdata does not mean, however, that economists will lose all interest in the economic aggregates. These will continue to be of prime importance in setting and evaluating much of fiscal and monetary policy. Even here, though, particularly in the analysis of aggregate time series, there is considerable room for improvement. Specifically, we in the profession need better ways of disseminating our macromodels, so as to facilitate their replication and further development. We also need faster, more accurate, and less expensive ways of maintaining widely used time series data on aggregate economic performance.

Toward meeting these various needs, the Bureau has now established its third "Conference Series," a collaborative effort with other institutions and individuals. The initial objective of this new undertaking is to improve the dissemination of computer software and "machine-readable data" to those doing quantitative research in economics. We have also continued to explore the possibility of establishing an experimental "computer utility" for economic and related research, an innovation which could do much to meet the identifiable needs for better aggregate data and time series analyses.

We have also given considerable thought during the past year to realizing Wesley Clair Mitchell's ambition, expressed in the *Fifteenth Annual Report,* that the Bureau become truly national in reality as well as in name. As a step toward that goal, we hope to establish within the next several months or so a West Coast branch of the Bureau, on land leased gratis to us by Stanford University. The proposed site would be just off the Stanford Campus and directly adjacent to the Center for Behavioral Studies. The major remaining obstacle to the creation of this establishment is to assemble the financial resources needed to construct and furnish the building that will house the forty or so researchers and aides planned for the facility. Toward that goal, the Bureau has already received a commitment of approximately $250,000 from an anonymous donor; however approximately $200,000 to $250,000 more is needed before the undertaking can be formally launched. If this experiment with a detached branch facility proves successful, it could be a prototype for similar experiments elsewhere, if and as resources permitted.

In sum, the problems and techniques of economic research, even its physical locale, have been and are undergoing change. We at the Bureau should respond to these changes as they emerge. I am sure that we can and will. In that way, but only in that way, can we have as productive a second fifty years as we have had in our first fifty.