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Chapter Title: Identifying Major Research Problems

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possibly also in the flow of theories, sufficiently well formulated to guide quantitative economic research in more "productive" directions.

4. IDENTIFYING MAJOR RESEARCH PROBLEMS

Persistence of Unsolved Problems

The greater supply of data and economic measures and the accelerated flow of hypotheses and pace of research do not mean that we are now in the happy situation of having answered all major questions and provided an adequate basis for realistic prediction and optimal economic policy. It only means that we have learned a great deal, enough perhaps to force abandonment of earlier simpler and more restrictive theories and to replace them with new hypotheses, more relevant but still based upon many simplifying and restrictive assumptions. It means that there is a basis for a greater consensus on the major changes that occurred in the economy and perhaps on some of the major factors that contributed to these changes. And it means that the greater supply of tested data and of realistic partial hypotheses permits a better evaluation of the implications of the changes as a guide to action. It also means a better choice of policy priorities and perhaps of specific policies—insofar as better knowledge of the basic framework and changes in the economy, and more tested analysis of policies, can affect both the overall priorities and specific policy choices. But acceleration in the supply of data and in the pace of research brings forth a variety of unsolved major problems calling for further research and analysis. These problems may be in the form of puzzles generated by conflict between the new findings and the old theories; or they may emerge as aspects of recent economic change, whose major determining factors cannot yet be reliably identified; or they may be associated with socially undesirable consequences, for proper judgment of which neither current measurement nor quantitative analysis has yet provided a basis.

Indeed, the pattern of jubilee-occasioned discussion illustrated by this paper—and by some of the other colloquia organized within the past year by the National Bureau—is first to review the recent course of research and make laudatory remarks about the accomplishment; and then to observe the problems still to be properly resolved, which are almost overwhelming in their complexity and recalcitrance. This may be a reflection of the occupational bias of research workers, who naturally tend to weight new unsolved problems more heavily than the older, more familiar, and at least partly resolved, problems. But without attempting to gauge magnitudes and compare present inventories of unsolved questions with those of twenty-five or fifty years ago, we cannot deny that this sequence of much research, much learning, and much still to be resolved is a realistic description of all experimental and observational intellectual disciplines. It is often referred to as the "endless frontier" of science, a term to designate the inexhaustible supply of significant problems for further research.[8] I see no

[8] This statement may now be challenged for some divisions of basic natural science, according to Bentley Glass in his presidential address to the American Association for the Advancement of Science (see his "Science: Endless Horizons or Golden Age," *Science*, vol. 171, no. 3966, 8/1/1971, pp. 23–29).

reason to deviate from this pattern in reference to quantitative economic research on growth, stability, and equity in the economies of this country and others. But before trying to identify the major problems and their priorities in further research, it may be well to consider why such unsolved problems emerge after decades of accelerated research, particularly in economics in which (as well as in other social sciences) the situation may differ significantly from that in the natural sciences.

The reference to "endless frontier" suggests that, as data and measures improve and tested generalization and theory succeed in identifying general and invariant properties, the new insights and the better tools reveal previously unseen aspects of the universe at its largest, and of the basic characteristics of matter at its smallest. But even in experimental sciences, let alone observational natural sciences, additional data and better tools are provided not only in response to questions generated by the inner logic of existing theories. Even then, questions posed by the old theories may, when pursued, yield answers that indicate the need for major revisions of the theories and thus generate a host of new research problems. But in many cases the new data and tools are provided because of events exogenous to the life and evolution of a given science. To cite a recent example, radio astronomy did not emerge as the result of major innovations in the field of astronomy, nor was it motivated by internally generated quests and pressures. Similarly, modern computers were not developed to satisfy the computational needs of basic scientific research. In general, the technological innovations that have enormously increased the productive capacity of modern economies, through the spread of economical mass production techniques, have also contributed greatly to experimental and observational natural sciences by generating new and powerful tools. But since such contributions are from an exogenous source, the new data and partial hypotheses that they generate are likely to raise a host of new problems that may not be solved for some time because the data are so new that they may not fit into the existing body of theory.

The development of economic analysis and research has been affected by stimuli provided, as in the experimental and observational natural sciences, not only by the unfolding of the internal logic and implications of some basic discoveries and theories, but also by the inflow of new data and emergence of new tools supplied exogenously, i.e., because of developments elsewhere. In particular, although the supply of primary data, which is provided largely by governments, *may* be affected by scholarly concern, it is usually generated by newly emerging interests reflecting either changed conditions or changed viewpoints, neither being necessarily the consequence of a new development within the structure of the economic discipline. The supply of both material and analytical tools—ranging from statistical techniques to mathematical devices for formal study of patterns and structures—may originate outside the field of economics, and for no reason connected with it. When such new data and tools appear, they may stimulate an accelerated pace of research within economics, resulting in a residue of unsolved problems until the findings have been fully integrated (which may take a long time).

In the field of economic and social research, major unsolved problems are always present—and not only because of the unexpected implications of an endogenously stimulated investigation or because of the exogenously provided new data and tools. In the experimental and observational natural sciences the exogenous additions relate to a universe with a long history of controlled observation and for which many interconnected, widely tested generalizations have been established. And in many of these sciences, the generalizations have been firm enough to provide a basis for an effective material technology, and for quite accurate predictions (as in the case of astronomy). In the economics discipline, and particularly in the study of economic growth of nations, a field that seems to me to be central to the proper consideration of even short-term instabilities and relevant policies, not only are there exogenously generated flows of new data and tools that may represent a far greater addition to a rather meager stock of data and tools than is the situation in most experimental and observational natural sciences. More important, variability of parameters and rapid changes in the aggregate magnitudes and structural relations, are prevalent. This is because the process of secular economic change is, at least in modern economic growth, shaped by social and economic adjustments to a changing potential of technological advance, the latter in turn connected with the continuous advance of basic science and other useful knowledge. What happens is that an attempt is made to analyze and generalize by using simplifying assumptions that remove many major sources of possible change (putting them into an exogenous pound). Although this procedure may serve for some short-term problems and in periods of moderate change (and not too safely even then), it is soon confronted with major rapid changes in economic conditions and structure, in the basic rules of society concerning economic activity, and in accepted views concerning the limits of private and public economic policy. The conclusions must then be revised.

The history of economic analysis and research dealing with the broader aspects of economic growth, stability, and equity for national economies has been full of "surprises": major changes in technological bases of economic production—unforeseen, and hence not fully understood; successive inventions and innovations in institutional adjustment to basic changes in material conditions and in social judgments; revolutions in the political and social structures of a number of societies, particularly those now behind the Communist Iron Curtain, that have had considerable effect on the channeling and control of their economic growth and distribution processes. Indeed, many of the factors discussed above, in explaining the acceleration in the supply of primary data and greater concentration on economic measures and quantitative economic analysis, were direct results of these rather unexpected major changes within the older, more developed free market economies, as well as in the rest of the world. It is hardly a surprise that acceleration in the rate of inflow of new data, tools, and economic research tended to be associated with a large residue of unsolved problems—evidence of the substantial lag of effective economic research behind the rapid and variable course of growth and of accompanying structural changes in the several aspects of the national economy.

The responsiveness of economic research to major current changes, inevitably to the relative neglect of older and still not fully resolved problems, is understandable. Such major changes represent significant additions to economic experience; and since they are unexpected and not fully understood, questions naturally arise as to their bearing on the meaningfulness of extant economic measures and the validity of the available theoretical hypotheses. If the relevance of the accepted measures and hypotheses that the discipline had previously supplied is put into question by the new events, emphasis on the interpretation of the latter, in the light of existing knowledge, is inescapable. When the discipline provides no relevant acceptable hypotheses, it becomes imperative to generate some tentative explanation, if only to provide orientation for further exploration. If the changes carry with them some undesirable consequences, practical pressures are added to the purely intellectual pressures of contradiction between the new changes and past patterns, or of lacunae in any systematic basis for interpretation. In this case, delay in reaching better understanding might be costly: it might permit undesirable consequences to occur again; it might allow the costs to be distributed less than optimally; and it might delay or prevent the consensus that is needed for ameliorative action.

Given the limited resources, and the impact of these intellectual and practical pressures generated by major changes, it is not surprising that the focus of economic research shifts from one set of new changes to another in their succession in time. As the major depression of the 1930's deepened, most of the economic research effort was directed to it, to the neglect of earlier problems related to reparations and international transfers, reduction of immigration, local depressed industries, and the like. When we entered World War II, emphasis shifted to problems of economic mobilization and warfare, to the neglect of the major depression that had not yet been adequately studied or fully understood. And after World

War II, the problems of economic growth in this country and in other parts of the world, occasioned by inter-system competition and the special situation of the newly independent (and other) less developed countries, attracted much attention and generated a large volume of quantitative economic research. Within the past decade, interest appears to have shifted to urban problems and poverty, even though our understanding of the problems of economic growth is still tentative; and questions have been raised as to the meaningfulness of our measures of growth and as to the validity of the available hypotheses as bases for adequate analysis and considered policy.

Three implications of the preceding discussion bear directly upon our theme. The first relates to the reasons why many of the major changes in the national economy, in the course of its growth, come as surprises, that is, are not adequately foreseen by existing theory and knowledge. We stressed in this connection the characteristics of technological change that powers modern economic growth, and that calls for numerous adjustments by way of social innovations and changes in conditions of life. But technological change itself stems from progress in basic sciences and other accretions of useful knowledge. As it affects economic productivity, it also changes conditions of life and creates potentials for new types of demand which, in turn, stimulate technological change. Finally, the latter may provide new tools and insights for basic science and lead to further discoveries.

The sequence suggested is long. The many links, sequential and collateral, have differing slippage; and the sequence cannot be forecast without a thorough systematic and interrelated theory of all the processes involved—the development of science, the level and direction of technological innovations, the course of the social innovations and changes in conditions of life emerging in the utilization of technological innovations, and so on. Yet such a long sequence is the substance of modern economic growth. Its combination with diverse historical heritages throughout the world, with which we are all too poorly acquainted (for reasons touched upon above), produced the surprises. In the older developed countries these surprises may lie in the unexpected character of the new technologies and of their social consequences—and partly in unforeseen changes in the rest of the world; in the less developed countries they may be the unexpected adjustments that societies make to their backwardness in response to the apparently huge potential of modern technologies—and partly to the unforeseen changes in the more developed countries.

The second implication, while really part of the first, should be separately noted. From the standpoint of the economics discipline, the difficulties with the long sequence suggested lie partly in the complex interweaving of economic processes with social and intellectual processes that are beyond the boundaries of economics no matter how broadly defined. If the forces that determine trends in basic science and in the accumulation of useful knowledge that provides an increasingly rich basis for technological change are somehow linked to economic processes, the linkages are still to be established. (Classical and Marxian economics resolved the problem by declaring technological advance too feeble, relative to other factors, to matter in the long run.) The linkage of changing technology, to changing scale of firm, to changing conditions of life is more within the scope of the economic discipline. However, the sequence cannot be completed in analysis so long as technological change is treated as an exogenous variable, and so long as we lack the theoretical system that encompasses changes in tastes and in conditions of life as a corollary of changes in economic productivity. The point here is that the economic trends that we observe over the long periods have antecedents and consequences in social and intellectual processes. An economic trend between times t and $t + 50$ is a result not only of economic events over the period (or before, i.e., in $t - 1$, $t - 2$, . . .) but also of the noneconomic antecedents and consequences of the economic events over the same period. If this is a true characterization of the interweaving of economic and other factors in the course of *economic* growth, analysis and data limited to the economic discipline can serve only if subjected to highly restrictive (and often unrealistic) assumptions as to the limits within which these noneconomic antecedents and consequences can act. And this may well be the reason why the discipline is now reaching out to extend its boundaries.

The third important implication is that the surprises contain not only a large positive element—increasing productivity and capacity—but almost inevitably some negative elements—either reducing welfare in, or affecting the security of, the national economy. Positive growth must have negative aspects in a world of independent and competing nations. The usually rapid advance of a major country may be viewed by others as a security threat that could not have been foreseen. More important, technological change, based on exploration and exploitation of the only partly known, and representing, in fact, manipulation of natural processes for human purposes, is likely to have undesirable effects. These cannot often be foreseen, because man deals here with much that is unknown and that is learned only with practice. For example, if modern technology is based on control of much more power than in the past, and requires highly durable producer goods to channel such power—with durability far exceeding anything known heretofore, including that of organic substances—can one fully foresee the consequence that indestructible residues of economically obsolete equipment would clutter the landscape? Or, when mass production of the automobile began, with its very low capacity of utilization and little pressure for efficient consumption of fuel, could one have foreseen the consequences in congestion and in pollution? Indeed, any major technological change that is necessarily a *disruptive* modification of nature for the benefit of man must, for this very reason, have *some* undesirable ecological consequences. Similar disfunctional elements can be attributed to any economic or social innovation, or even to any major modification of social ideology, that is, of the way people look at relations to each other and to nature. Although the modern corporation was a valuable legal organizational response to the requirements of modern, large-scale, capital-demanding technology, it lent itself to abuses in connection with attempts at monopolization; and one consequence of its development, not fully expected, was the separation of management from ownership, which created new problems. If the increased strength of nationalism was an ideological response to the organizational challenge provided by the enriching but disruptive potential of modern economic growth, some of its negative consequences can hardly be denied; and many were not anticipated.

Regardless of such major consequences, the unexpected and undesirable aftermaths of the major technological changes and of many economic advances generate pressures for interpreting and measuring these changes and their corollaries in quantitative research dealing with economic growth. It is important to note that these pressures are not accidental, but are a continuous accompaniment of economic growth and change. Furthermore, the internal pressures may be the greater, the higher the rate of recent growth and the more marked the forays into the new and partly unknown reaches of technology and economic performance. The bearing upon the interpretive function of quantitative economic research is obvious.

Suggested Priorities

I have been referring to the study of economic growth in its broader quantitative aspects for several reasons: because I am more familiar with this field than with others; because I consider it central in that it provides a guiding framework for the study of its components and institutions; and because the National Bureau of Economic Research, which has contributed much in this area, should continue to play an important part in such research. But the association between an accelerated pace of research in recent decades and the variety of unsolved problems that remain is true not only of the broader field of economic growth of nations but also of many more specialized fields of research. Broad changes in the rate and structure of growth are likely to have repercussions in all important sectors and institutions; changes in views on policy ramify from one field to others; the pressure to revise older notions concerning determining factors and limits of policy in special fields would presumably be affected by what happens in the economy at large.

To be sure, a major qualification is to be noted: the intensity of the impact discussed above need not, indeed cannot, be the same in all the subfields of economic analysis, for the pressures of major changes in conditions and outlook are not the same in all of them. The shifts of the limited research resources in the economics discipline from one set of problems to another, as we observe them over the longer-term past, are reflections of the unequal impact of the major changes in any given period. After all, we had no separate subdiscipline for the study of Communist economies in the 1920's or the 1930's, nor did we have courses on economic development in the graduate curricula; and monopoly and trust problems that loomed so large at one time in graduate teaching and research appear to have receded from the focus of attention. Even so, the variety of fields within which a high pace of quantitative research in the past was associated with many still unanswered problems is wide; and upon careful consideration, those currently neglected fields may prove deserving of more attention, especially if their possible contribution appears to be relevant to other research tasks. In trying to identify the major problems for research, one is thus left with the uncomfortable conclusion that the areas for quantitative research in which unsolved problems and promising further research loom large cover almost the full range of the economics discipline.

The last remark and the preceding discussion should suffice to convey my view on recent developments and problems in quantitative economic research, a view that reflects personal experience and judgment but perhaps for that reason leads to some specific priorities. This view, implicit in my emphasis on the broader aspects of national economies, suggests that, given the turbulent acceleration of economic research in recent decades, as well as the marked changes that have occurred in this country's economy and in the rest of the world since the mid-1940's, there is an obvious need for a wider and more critical synthesis of the many disparate measures and pieces of research into a coherent closely interrelated analysis of recent economic growth experience.

Such synthesis might well begin by concentrating on the post-World War II growth experience of this country—a quantitative analysis of its economic growth over the last twenty-five years, the sources of such growth, the structural changes and changes in conditions of life that accompanied it, the distribution of the gains from such growth, and the net balance of costs and returns. A major and comprehensive study would attempt to integrate all the partial studies and the analyses that have accumulated; it would widen the scope to include such basic aspects of economic growth as growth of population and its shifting distribution, technological changes in production and consumption, a critical scrutiny of the aggregative measures for hidden costs and returns and of concealed duplication and biased weighting, and a thorough examination of the distributive aspects of the process. The need for such a study is suggested by recent discussions—not only scholarly probings into the new aspects stimulated by "puzzles," but also widespread (or at least voluble) concern with the quality of our economic growth, and with the deficiencies of the national product measures as indicators of national progress.[9]

Such a broad topic may involve a whole series of studies rather than a single exploration—studies ranging from a technical examination of current measures, the problems in establishing the contributions of various factors to growth, possible revisions of the currently accepted sectoral classifications, deeper analysis of the income distribution, and the like, to the necessarily broader and more general issues raised by some of the problematic consequences of economic growth, or by the whole network of relations between economic growth and changes in the noneconomic aspects of the performance and structure of society at large. Although this may well be the case, we hope that a realization of the wide scope of the topic does not lead to its postponement as too ambitious, or to plans that are overly long-term. The suggestion advanced here cannot be taken to represent a well-thought-out program; this would be out of place.

[9] Much of the discussion in recent annual reports of the National Bureau (see, in particular, the paper by F. Thomas Juster in the *50th Annual Report*, September 1970, pp. 8–24) and in the colloquia papers by Professors Schultz, and Nordhaus and Tobin (Theodore W. Schultz, "Human Capital: Policy Issues and Research Opportunities," in *Economic Research: Retrospect and Prospect, Vol. VI, Human Resources;* and James Tobin and William D. Nordhaus, "Is Growth Obsolete?" *ibid., Vol. V, Economic Growth*), supports this impression of a need for critical revision of the current national economic accounts. The papers presented at a recent Conference on Income and Wealth (in Princeton, November 4–6, 1971) on the broad theme of Measurement of Economic and Social Performance strengthen this impression. One should stress that the dissatisfaction with the current national product measures is with their validity as gauges of economic growth, rather than as indexes of short-term changes in current economic performance.

The program should be a product of collective judgment and experience. But three aspects of the proposed topic, or complex of studies, seem to me to deserve recognition and further consideration.

The first is the emphasis on growth, i.e., sustained, nonreversible, major changes over a sufficiently long period to indicate the persistence of underlying forces, with short-term changes and instabilities treated as part of a longer process. Much of the recent quantitative economic research, particularly that initiated or stimulated by government, has been concentrated on the year-to-year changes that affect current government and private policy. As a result, the longer growth perspective has often been ignored; and changes over a year or two have been characterized as growth, although they may have contained a transitory component large enough, relative to the secular, to obscure the long-term movement. We badly need a longer perspective. Awareness of this has led to much criticism, in recent years, of the deficiencies of many current measures, despite the fact that they may be quite adequate, perhaps are the best, for short-term changes and problems. Indeed, that may be the very reason for their inadequacy in gauging the far greater transformations that occur over the longer period associated with growth. In widening the historical perspective, observation and analysis of some of the changes may have to be extended into the prewar period; and for many of these, the current study by Moses Abramovitz and Paul David, now nearing completion, should provide an important contribution.[10] But one

[10] This study of post-World War II growth in the United States in the light of the longer historical perspective is part of a cooperative project of similar studies for a number of developed countries, includ-

would hope that concentration on the post-World War II decades, with only limited and broad reference to the longer past, might permit a more intensive analysis, if only because of the greater stock of economic measures and partial analytical studies relating to the more recent period that have accumulated.

Second, the emphasis is on the growth of the national economy and those of its parts and components that should be distinguished for several analytical reasons. Such an emphasis should shed light on the sources of growth, on the shifts in relative shares of various groups and changes in the conditions of life required by participation in economic activity, on the burdens imposed and returns bestowed on the different groups in the community, on the dependence of the country's economy on others, either in the way of trade or of security, and the like. This combination of comprehensiveness, of aggregation that would strike the net balance of costs and returns in terms of socially accepted assumptions and knowledge, with analytically oriented disaggregation and scrutiny of differences in movement and relations among significant components, is of the essence in this complex of studies. And it is required if the study is to accomplish its two most important functions. It must establish a greater consensus on what happened, and what the implications are—a task particularly important in a democracy in which such consensus is a prerequisite for intelligent action (necessary, if not sufficient). It must organize the unrelated studies of specific aspects of

ing the United States, several European countries, and Japan. The project was initiated under the auspices of the Committee on Economic Growth of the Social Science Research Council in 1964 (see Social Science Research Council, *Annual Report, 1967–1968*).

the economy in order to provide a better orientation for economic analysis, aiming at better generalization on and better prediction (or at least foresight) of long-term trends.

Third, the aspects of economic and social change that should be included in such an analysis of post-World War II economic growth and its implications should reach beyond the economic, as defined in economic research. The growth processes, as indicated, involve a complicated interplay between trends in economic production and distribution proper, the growth of population and its adjustment to economic stimuli and to broader factors in life, changes in prevailing attitudes, the search for new knowledge and innovations, and the very structure of the "rest of the world." Consequently, greater understanding and more fruitful analysis call for extension of primarily economic research to cover at least the quantitative aspects of closely related social trends—in population numbers, in family life cycles, in bases for social stratification, in the specialized but highly crucial institutions concerned with basic and applied research and technological change, and so on. How far the extension should be pushed is a question that can be answered only in the course of formulating the program of such a study. Moreover, the answer will change as the study progresses and as the results of such extension cumulate. But the probings that have already been made—many are reported in recent National Bureau annual reports and others are evident in recent work on quantitative social indicators—suggest that we should *begin* with a broader view of the ramifications of economic growth processes than has been held in the great majority (indeed almost all) of economic research studies in the field.

Since the suggested study is aimed at quantitative analysis of the economic growth of this country since the mid-1940's, the sources and consequences, the social concomitants and the costs and returns to the living members of society, it can hardly be undertaken either by the government or by one or two individual scholars within the universities. The government, as already suggested, must, of necessity, concentrate on the day-to-day or year-to-year problems; and it is highly revealing that we have the annual *Economic Report of the President,* but no decennial or generational reports. Even if such reviews were to be initiated by the government, and this may happen if government assumes more direct and active policy responsibility for longer-term patterns of growth, it would require substantial pioneering work by scholars and analysts outside of the government to establish a consensus as to the most meaningful measures and their interrelations. This would, in fact, be one of the contributions of the complex of studies urged here. Nor can such a complex of studies, given its scope and the need to draw upon a variety of expertise and upon a large body of data, be effectively brought to completion by one or even several individual scholars. Only a research institute, with a wide background of experience in collective, quantitative research, could provide effective auspices and leadership.

If such a complex of studies were to be considered a priority item on the program of the National Bureau, it would have to draft some resources now used in more narrowly defined, more specific studies that promise to yield concrete results in the proximate future. This, of course, is the inevitable cost of far-flung, synthetic, broadly interpretive types of investigation that may result in valuable revisions of simplistic notions, a better orientation, and better directions for future research, but may yield no specific findings of immediate value. Is the price too high? After all, the careful study of a single institution, e.g., the over-the-counter securities markets or the residential construction industry, will bring to light some elements of irrationality or of wasteful practices, and the needed policy changes. Even wider but still fairly specific subjects for quantitative economic analysis are numerous and come easily to mind. And yet I would argue that the long-term yield of the broad investigation is very high in terms of additions to tested knowledge and the provision of a better basis for consensus on the goals of the economy and society and related judgments of recent major changes. Furthermore, a broader study of the type suggested provides a unifying focus for a research institute like the National Bureau. It is interesting that upon the 25th anniversary of the National Bureau, Wesley C. Mitchell stressed "national income as an incitement to and a framework for other studies." [11] In a sense I am repeating Dr. Mitchell's emphasis on the possible contribution of the suggested study to the guidance and unification of the National Bureau's research program. The one modification that I make is to stress the use of national income measures as gauges of economic growth, rather than as indexes of short-term changes.

Finally, it may be argued that some specific economic problems calling for relevant quantitative economic research are more urgent than a broad analysis of economic

[11] Wesley C. Mitchell, *The National Bureau's First Quarter-Century*, Twenty-Fifth Annual Report of the National Bureau of Economic Research, New York, May 1945, p. 20.

growth since World War II. Examples abound and we mention a few: the persistence of poverty and welfare problems; the plight of the cities, particularly the central cores of metropolitan areas; the ever-present national security and international tensions. And, as already indicated, it cannot be denied that new problems arise continuously and exert pressure for response in research and policy. Yet it seems to me that many of these problems must be seen within a wider framework and perspective; that continuity in economic and social research would provide a base from which the succession of ever-changing problems could be better understood; and that it is the task of scholarship to provide this broader base for interpretation, generalization, and policy analysis. We must have centers for research that are devoted to these broader views and the wider issues that they raise, without being forced to succumb to the continuous pressure of ever-changing crises and urgent problems of the year, or even the decade. That very rapid succession of urgent problems is in itself evidence of the need for a broader base and wider perspective. If, in the 1920's and the 1930's, people in the developed countries could be concerned about the sluggish population growth, and a generation later were at the peak of excitement about the menace of rapid population growth; if in one period there is worry about surplus of savings and lack of investment opportunities, and in another about a critical shortage of capital funds; if in one period demand is keen for higher education as the proper preparation for a productive life, and in another its relevance is questioned—then the need for a wider perspective and broader base seems clear. Needless to say, the re-evaluation of past experience and current problems within this wider context is a task that requires the employment of research talent and experience on a consistent and systematic basis.

The priority that might be given to this broad study or complex of studies in the program of the National Bureau would not represent much of a departure from past experience. Without considering questions of organization, planning, etc. (which would be out of place here), one could argue that such a study is feasible under the auspices of the National Bureau. It is squarely within its tradition of contributing to the understanding of basic quantitative aspects of the performnace of the economy and society in this country.

Another broad topic that seems to me to deserve high priority in any consideration of quantitative economic research is not quite within that tradition; and raises some major difficulties for which no ready answer is apparent. This topic is the badly needed analysis of economic growth experience and problems in other countries. The dangers of limiting research and analysis of economic growth problems to a single country are too obvious to need stressing. Yet one gets the impression that the accelerated pace of quantitative economic research and the improvement in its tools have resulted in a concentration of scholarly research on the single country of which the scholar is a resident that is greater now than in the past. Even if this impression is wrong, it is a fact that there is a short supply of sustained quantitative economic research utilizing comparisons of the growth experience of several countries—comparisons that might shed some light on the growth experience of countries in which native economic research is limited or almost nonexistent, as is true of many Communist and of most less developed countries. I would place a high value on comparative research because it would provide a better analysis, a better identification of the invariant and variant elements, and a better basis for interpretation, generalization, and policy treatment. Considered interpretation of the "rest of the world" would better the understanding of the position of one's country vis-à-vis others. And it is crucial for the formation of a social consensus, particularly in democracies, based on wider world intelligence and not on ignorant self-centered provincialism. Greater knowledge of foreign economies and societies is indispensable if more intelligent public policy bearing on international relations is to emerge. One hesitates to pass judgment, but it is conceivable that some of the current problems of the economy of this country are linked to limited understanding of other economies and societies.

I do not mean to suggest that there have not been recent valuable accretions of new data, economic measures, and quantitative economic research for other countries. These, in fact, have permitted comparative studies of a scope and solidity of empirical foundation unattainable in the past. The National Bureau's own research program has included studies of international migration, a series of studies on the economy of the USSR, and some scattered studies of other countries. Several branches of the government provide immensely valuable series of statistical studies on population and labor force, agricultural output, mineral resources, and labor conditions in other countries. Congressional committees are responsible for special hearings that mobilize large amounts of information on selected parts of the world (particularly the major Communist countries). A profusion of studies of economic growth experience and problems in a variety of countries has also been published by scholars in this and other developed countries. The compilation of comparable data for a large body of nations and the series of monographic studies of various problems by the international agencies, including some regional branches of the United Nations, have greatly enriched our knowledge of economic growth patterns and problems in many parts of the world.

However, much of the research is episodic; far too much of it is produced either on an ad hoc basis or as a corollary of the functioning of international agencies (or, sometimes, of domestic government agencies concerned with foreign areas). We lack a sustained scholarly effort that would apply to the study of other areas of the world the criteria that an economist in a developed country applies to research bearing on his own country—criteria such as data adequacy, relevant economic measures, and the greater understanding of the institutional and social framework within which economic growth and performance take place. These desiderata are most easily met in comparative studies of free market developed countries, particularly those affiliated with Western European civilization—although even here misunderstanding and lack of comprehension are possible because proper appraisal of the specific institutional conditions and of the quality of the relevant data is difficult. And, of course, great assistance is rendered to a research scholar in one developed country by equally competent research scholars in the others. But difficulties arise when, as in the case of Japan, the developed country has long historical roots in a civilization that developed apart from the Western European for millennia before the late nineteenth century. The difficulties are greatly compounded for the Communist countries, with their distinctive political and institutional structures and their limitations on critically and objectively oriented native economic and social science scholarship. In any one of the less developed countries the paucity of primary data, the irrelevance of some "standard" economic measures (formulated primarily for developed economies), and the scarcity of native scholars severely limit the contribution of domestic scholarship and data to quantitative analysis of that country's growth experience, and magnify the difficulties of adequate comprehension and analysis by research economists from the developed countries. Because of the absence of sustained, continuous, and cumulative quantitative economic research on areas other than that in which the scholar resides, little research is now being done on the "difficult" areas, and much of that is of poor quality. Shoddiness of data untested by adequate analysis often renders the estimates that appear in international compendia highly dubious. And many of the studies, geared toward pressing policy problems and emphasizing various "gaps" and "bottlenecks," are dogmatic, simplistic over-generalizations because of inadequate empirical bases.

The difficulties of stimulating sustained economic research concerning other areas, research that would produce results useful for comparative studies and proper orientation in this rapidly changing world, cannot be explored here. We are all familiar with the disciplinary structure of economics within the academic institutions and with the difficulties a scholar encounters in fully understanding the specific complexities of another country—particularly when he deals with less developed countries or areas outside the sources of Western scholarship.

No extra-university, academic centers exist that would provide the proper conditions and facilities for this type of quantitative analysis of foreign economies. To be sure, area study centers are attached to several universities, but their difficulties in attracting economic scholars and maintaining continuity are formidable and well known.

In short, there has never been a "Bureau of Economic Research" in the field of quantitative economic research bearing on foreign economies, one that would concentrate on a continuous and cumulative program dealing with the basic quantitative framework of these other national economies and thus assuring some minimum standards of quantitative analysis. A few *national* bureaus, or their equivalents, have been established in some developed countries; but most other parts of the world have no such centers for quantitative research. And for various reasons the international agencies that have burgeoned since World War II do not provide the conditions for development of continuous economic research, despite the distribution of an enormous quantity of data and the publication of a variety of reports. The former are often of rather variable quality and dubious relevance, and the latter are often ad hoc studies, not always free from the limitations imposed by the need to maintain international courtesy or to respond to regional interests.

The range of the studies comprised in this second topic is far wider than that implied in the study of post-World War II economic growth of this country. Any considered discussion of its feasibility would involve problems of area grouping, limits within which comparative study would seem worthwhile, variety of criteria involved in selecting one or another area and type of economy for intensive study, and the like. I am not prepared to engage in such discussion; nor is it relevant on this occasion. But it seems to me that the broader interests of the economics discipline and the requirement that economic research provide a better basis for social consensus demand a more sustained effort to introduce continuity and higher standards in quantitative research on many important areas of the world—particularly those outside the free market developed countries. Since, on this jubilee occasion, we should be receptive to all research proposals—even of almost impossibly difficult tasks—and since the National Bureau has engaged from time to time in quantitative research on foreign areas, I thought it not improper to suggest the priority of the task and the need to consider the possibility of securing continuity and higher standards of empirical research in these areas.

What could be done in response to the need, if its priority is recognized, is not clear to me at present; and it is even less clear whether the National Bureau can play an important substantive role. One would have to know more about the research under way in the area centers attached to the universities in this and other developed countries; take stock of the various comparative studies recently completed or under way, whether by academic scholars or in research branches of international agencies and organizations; and think through the role that an institution like the National Bureau could play, and at what cost. As already indicated, this review and stock-taking would have to be done against a background of fairly clear notions of the method by which the vast field of "rest of the world" should be subdivided, and of the most relevant criteria of choice of areas and of the most urgent problems that need comparative analysis. An organized discussion of the comparative study of economic growth and structure, conducted under the auspices of the National Bureau in 1958, resulted in several suggestions on research objectives and organization.[12] But no substantive program followed this effort at preliminary exploration.

My own judgment is that study of the growth experience and problems in at least some selected economies would prove valuable. A review of the field would, it is hoped, show whether and how the National Bureau can contribute to stimulating and developing more consistent, continuous quantitative research, at higher standards, in the important area of comparative economic growth and structure.

5. SUMMARY

In summarizing the paper, I begin with the distinction that was drawn between the ever-present conditions of quantitative economic research, and the particular situation in the field in the early 1970's.

As to the former, I noted five sets of conditions. The first was the origin and supply of the primary data. They are provided by the active economic agents themselves—individuals, firms, agencies, etc.—and economists have no direct control over their supply. Consequently, the quality of the data varies, there are lacunae in the available stock, and the supply of the data lags behind the emergence of the problems upon which they are to shed light.

The second condition was the dependence of economic measures—for which the primary data are the

[12] See *The Comparative Study of Economic Growth and Structure: Suggestions on Research Objectives and Organization*, National Bureau of Economic Research, Exploratory Report 4, New York, 1959.