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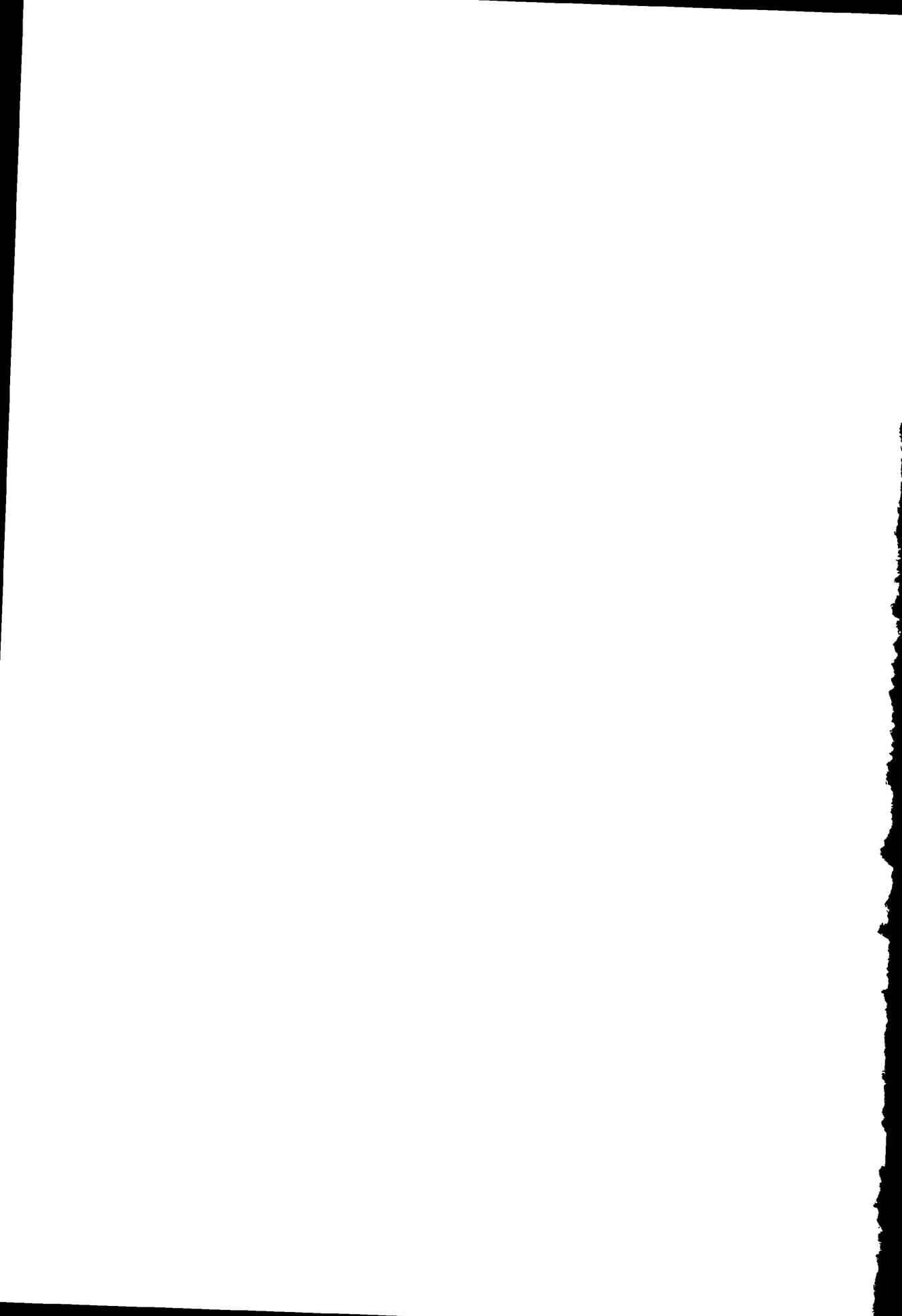
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I

Reports Presented
at the Annual
Meeting of the
Board of Directors



THE NATIONAL BUREAU: CONTINUITY, CHANGE AND SOME FUTURE PERSPECTIVES

JOHN R. MEYER

When I first assumed the presidency of the National Bureau of Economic Research last year, I asked many people what they felt the Bureau should emphasize in its research activities through the 1970's. No one answer predominated to the point of being a majority response, but one suggestion was made more than any other: "the Bureau in the 1970's should do for social statistics what it did for economic statistics in the 1930's." Clearly, such a statement simplifies the issues. It overestimates the transferability of the economists' skills to the problems of social measurement. It underestimates the importance and difficulty of significant economic problems that remain unsolved. Nevertheless, as a good one-sentence summary of a major new direction the Bureau

should take, I think it comes very close to the mark.

A second quite common suggestion was that the Bureau should devote more attention to the processes of economic growth. Some even urged that the Bureau concentrate exclusively on such studies. Broadly interpreted, economic growth can, of course, encompass a very wide range of possibilities. The monetary, human, and physical consequences of economic development constitute the core material of a considerable portion, perhaps a major portion, of research in the social sciences today; certainly, study of the processes and consequences of economic development would not seriously constrain the Bureau's research choices.

Today I want to discuss the future of the Bureau as that future may be shaped by decisions you and I are in a position to make. Largely, these will be decisions to do, or not to do, particular kinds of research.

NOTE: This is an adaptation of the President's oral report to the Board of Directors of the National Bureau of Economic Research at the annual meeting of the Board held in New York City on March 18, 1968.

I. BASIC CRITERIA AND QUESTIONS

I believe that four criteria should be used in selecting new research undertakings for the Bureau. First, there is the matter of relevance; second, of service to the profession; third, compatibility or complementarity; and fourth, comparative advantage.

Relevance. This is perhaps the easiest to define. Quite obviously the Bureau must be interested, as it always has been, in projects that will better our knowledge of economic behavior. This follows almost by definition, given the word "research" in the name of our institution. Such a focus also maintains the Bureau's tradition of investigating those topics which contribute to a fundamentally better long-term understanding of economic processes.

Yet, an understanding of certain economic processes may be more timely and important at one moment in history than at another. Clearly, the Bureau should give priority to those studies which provide insights into the most important and urgent economic problems or ills of our times. This, again, is consistent with long-standing National Bureau tradition. The emphasis Wesley Clair Mitchell gave to business cycle studies in the early 1920's reflected a very accurate assessment indeed of what men of affairs then considered to be and, more importantly, what *was* the major economic problem of the times.

As a corollary, relevance also involves an assessment of what contribution, if any, a proposed study might make to improving public policy. This does not mean that the Bureau should attempt to produce "position papers" reviewing major government policy alternatives. Rather, the Bureau's emphasis should remain on studies which, while they may not solve immediate problems, will most likely contribute to a fundamental understanding that would, in five to ten years' time, provide the groundwork for such solutions. Social problems usually take time to cure; a patient time dimension, therefore, may be the most realistic approach in any case.

Services to the Profession. When one considers services the Bureau might perform for the scholarly profession, one is struck by the great variety of possibilities. Perhaps we should begin by observing that the National Bureau is essentially the property of the profession and therefore is quite properly in its service. Indeed, the Bureau is already committed to academic service activities, such as the Conferences on Income and Wealth and those of the Universities-National Bureau Committee. These act as forums for presenting and evaluating research findings and for more general exchanges of information. In addition, the Bureau has always provided a locale for large-scale, long-term research efforts that could not be conveniently based on a campus, that is, research requiring continuity and the mobilization of specialized technical skills that do not fit well into the conventional table of organization or personnel policies of a university. It obviously behooves the Bureau to continue, and perhaps even sharply expand, its historical policy of providing opportunities for good university researchers to devote their full time, for limited periods, to such off campus research efforts, whether as Research Fellows or temporary additions to the Bureau's professional staff.

In this tradition of service, the Bureau currently provides some limited statistical and data support for researchers not connected with the Bureau. One can easily conceive of ways in which these services could be enlarged and greatly improved. For example, the Bureau makes its time series data library available in a limited form on punched cards, and fairly extensively in written or hand-charted summaries. One can imagine the same functions being performed far more extensively and effectively by exploiting the possibilities inherent in a computer utility designed specifically for the use of the profession. Such a computer utility, with at least some form of limited time sharing via remote terminals, might become a means for storing not only data banks but also the major econometric models and the more advanced estimating techniques.

One can also envision a sharp expansion in

the service rendered by the Bureau as an educational institution, and I would like to see more intensive exploration of this dimension of Bureau activities.

For example, I believe that the Bureau should be a place for young men in their early post-Ph.D. years to pursue research activities intensively. These are the years when men are likely to be at their most original and innovative, and at the peak of their technical skills. Having such young men come to the Bureau for a year or two at such a stage in their careers would not only be potentially beneficial to them but, I am sure, would be highly beneficial to the Bureau as well. Similarly, the Bureau is at least one logical place where people from other countries could become familiar with the organization and execution of good quantitative research. Many counterparts of the Bureau have been created abroad. Advantages should accrue to both the counterparts and the Bureau, by the Bureau participating more actively in these developments.

I would also hope that the Bureau could provide opportunities for people at smaller colleges to participate in larger-scale research activities. To do good research, economists increasingly need access to a fairly large computer and related programming and data capabilities. Such capabilities are today found at only a few of our universities. One of the potential attractions of a successful computer utility for the profession could be its accessibility to people at our smaller colleges and less well-equipped universities.

Compatibility and complementarity. These criteria involve practical issues of people, funds, and suitability. For example, before any new research activity can be undertaken, the question must be asked whether the Bureau has the resources, both financial and human, to implement it properly. The importance of such a practical question is so obvious, it virtually needs no discussion. Posing the question does, however, underscore the need for a reserve of unallocated funds at the Bureau to launch investigations of new research possibilities, especially before these can be defined or formulated with sufficient precision to seek

outside financing.

Similarly, a key question when considering a new undertaking is whether suitable researchers are available—people who have a very real or potential desire to explore the suggested subject. A good researcher's recognition of the importance of a proposed study, and his involvement and commitment, are almost indispensable to success. To some extent one can urge researchers to undertake new areas of study, but there are distinct limitations to how far such guidance can go.

Closely related to the question of compatibility is whether a proposed new activity complements and enriches existing research at the Bureau. Like a university, the Bureau is a community of scholars. New research is usually far more productive if it is not pursued in isolation. Complementarity, among other benefits, also alleviates the problem of finding the right personnel—people who are competent and thoroughly committed and interested.

Comparative advantage. To ask whether any proposed new research fills a need or a void that is otherwise not likely to be filled necessitates consideration of what is being done at other major centers of economic research, for example at RAND and Brookings. It also necessitates a definition of what the Bureau's unique capability or contribution has been or might be.

In the near future the Bureau's unique capability is likely to be what it has been in the recent past: good basic quantitative research on economic problems that are relevant and likely to be of concern to our society for some years to come. But, I would also tender some very personal observations. The Bureau is not well-suited—by staff characteristics, review procedures, or plain temperament—to doing relatively quick studies on immediate policy problems. The Bureau is even less well-suited to assuming a direct advisory role as a formal or informal part of a government or other staff agency. Bureau researchers have traditionally tended to a rather relaxed view of time constraints—with a casualness that would be the bane of any staff or policy formulating agency.

No major apologies need be made, however,

for these tendencies. A need exists for organizations to do good staff work, to write good policy papers, and to do good basic research. To some extent, I am sure, these activities are better done when policy and research interact with one another. However, I am also prepared to argue a definite need and a quite important role for research that abstracts from all immediate policy questions.

II. THE BUREAU'S RESEARCH ACTIVITIES TODAY

In economic research, as in most scientific research, one can perhaps discern four roughly distinct activities: (1) hypothesis formulation or theory; (2) hypothesis testing; (3) data collection and measurement; and (4) efforts to refine and improve the research method itself.

On the whole, the Bureau has been strongest in data collection and measurement and, to a somewhat lesser extent, hypothesis testing. Method and theory have not been major preoccupations of the Bureau in the past (or even today). Since many others are working on theory and method, and these tend to be activities that can be pursued with reasonable diligence by individual scholars working in relative isolation, much can be said for the traditional Bureau emphasis. However, it is worth pondering whether the research mix of the Bureau's program is optimal; I would rather suspect that it is not and that some modest increase in theoretical and methodological activities is well-advised. If nothing else, such a change might greatly increase the efficiency with which the Bureau does its hypothesis testing.

It is not too difficult to identify the research now under way at the Bureau that is primarily concerned with measurement (though in almost all cases important hypotheses are at least implicitly under test as well). Included would be such studies as those by Nancy and Richard Ruggles on "The Design of National Economic Accounts"; by John W. Kendrick on "Studies in the National Income Accounts"; by F. Thomas Juster on "Household Capital Formation and Savings"; and by Robert J. Gordon on "Problems in the Measurement of Non-

residential Fixed Capital." These studies are classified in the Table of Contents of this report as studies in *National Income, Consumption, and Capital Formation*; they might, just as accurately, have been placed under a heading, *Measurement Techniques and Innovations*. Certain other studies, elsewhere classified, also share this orientation: Boschan on job vacancies, Kendrick on productivity trends, the first third of the Friedman and Schwartz study of money, Fiedler on measures of credit quality, Kravis and Lipsey on international price comparisons, Stigler and Kindahl on industrial prices, to some extent Shay's work on consumer finance rates, Cohan's and Guttentag's work on direct placement yields and mortgage interest rates, and to a considerable degree the Bureau's work on business cycles and short-term economic forecasting, ranging from Rosanne Cole's study of GNP revisions and forecasting errors to Ilse Mintz' analysis of turning points in foreign business cycles.

Once we segregate those studies that are primarily concerned with measurement, two strong central tendencies or foci can be identified in the behavioral hypotheses that are being tested in the present Bureau program. The first concerns the Schultz-Becker hypothesis on the role of human capital in the production function; the second involves definition of the behavioral interrelationships between changes in wages, prices, and capacity utilization.

The human capital studies have assumed, moreover, a relatively specialized focus. In particular, hypotheses about the household production function play a role, often central, in the studies of the service industries and of the economics of education and medical care. In these, the efficiency of household production functions depends importantly on the quality or stock of human capital found in the household. Human capital differences, and how these affect production possibilities, also figure prominently in the Bureau's international studies, particularly those by Lary relating to trade patterns between the developed and less developed countries.

Wage-price-capacity utilization hypotheses are to be found in several of the growth and

business cycle studies. Here, the concern is with defining the behavioral relationships, including the lag structures, of changes in wages, prices, and capacity utilization over the cycle or over longer historical periods. Such concerns are fundamental to Fabricant's study on price trends and economic growth, Cagan's investigation of the relation between wages and prices in conditions of continuing inflation, Stigler and Kindahl's study of the behavior of industrial prices (which involves an important exercise in measurement as well as an hypothesis about the relation between price behavior and capacity utilization), and to some extent Boschan's study of job vacancies. In addition, three of the Bureau's younger researchers, Nadiri, Rosen, and Nordhaus, are involved in one way or another in attempts to estimate wage-price-capacity relationships.

A unifying theme or hypothesis is not so obviously identifiable for studies classified in this report under the heading *Financial Institutions and Processes*. Considerable stress is placed, however, on determining how different institutional arrangements for organizing the financial sector of our economy can influence the cost, efficiency, and pricing practices of financial institutions. That emphasis is particularly evident in the studies by Jacobs and Smith on the performance of banking institutions. Similar themes appear in the various studies on the quality of credit, and in Cohan's and Gutten-tag's work on interest rates.

III. OMISSIONS FROM THE PRESENT PROGRAM AND POTENTIAL NEW RESEARCH AREAS

One obvious question to pose when considering the future research program of the Bureau is: What of importance or special relevance does the present research program omit? Before attempting to answer this question, two observations should be made. First, the hypotheses now being tested are important, even though they do not constitute all hypotheses of potential importance to students of the current economic scene. Second, to identify important

omissions in the present Bureau program does not mean that the Bureau should undertake studies to fill the voids. Before such commitments could be made, the very practical questions, previously stated, of compatibility, comparative advantage, funding, staffing, and complementarity would all have to be investigated intensively.

Nevertheless, four areas of omission or underemphasis appear to exist in the present Bureau research program. I would list these as: (1) poverty and its closely related problems of racism, urban decay, and migration patterns between rural and urban environments; (2) processes of growth, that is, tests of hypotheses about growth and the development of growth models; (3) technological change, its impact (social and economic) and diffusion; and (4) the economics of environmental improvement, by which I mean to include everything from improving urban parks, to providing cleaner lakes and rivers, and improving the efficiency of our legal processes.

These issues all have an important international as well as domestic aspect. Racism, poverty, and the closely linked process of migration from rural underemployment to urban squalor have many of the same dimensions in Africa, Latin America, and Asia as they do in the United States—though the degrees of violence and acuteness *may* differ from continent to continent. Understanding the processes of growth is certainly a problem of worldwide concern. Perhaps, therefore, with better recognition of the common elements in the growth problems of many less developed countries and the more retarded regions of the advanced nations, more progress could be made in developing useful theories about growth processes. Some of the most exasperating and important aspects of technological change also involve international comparisons: the so-called brain drain and technological gap, and the continued difficulties (i.e., slow growth rates) of many nations specializing in primary production—all attest to the comparative international aspects of technological change. Finally, environmental improvement is obviously costly, but where we in the United

States argue about how much air depollution or roadside planting we can afford, LDC's debate the merits of better housing, sewers, streets, and public health, now or later.

Certain aspects of these problems are, of course, receiving attention in studies now proceeding at the Bureau. For example, the human resource studies clearly relate to growth processes, poverty, and environmental improvement. The studies of tax policies are also related to the economics of growth and the distribution of its benefits. I suggest, though, that we are perhaps not exploiting these potentialities or interrelationships with the system or depth that we should. I also assume that a better understanding of wage-price-capacity relationships could lead us to a better understanding of growth processes. We should, however, make more systematic efforts to test these hypotheses in terms of growth concepts, with particular emphasis on integrating them into medium-term or long-term empirical growth models for the U.S. economy.

Initiatives of the past few months should also help fill some of the voids. The proposed NBER studies in urban economics—on industrial location, housing stock adaptation, and the ecology or processes by which urban form adapts and evolves—all relate to urban poverty and race problems. The same studies also could prove helpful in analyzing certain kinds of environmental improvement programs for urban areas.

Beyond the domestic sphere, we recently have launched a cooperative venture with comparable research organizations in Europe to evaluate processes of technological diffusion. These studies should help us to a better understanding of growth processes, as well as of technology itself—especially its rate of diffusion. We may also learn a bit about international cooperation and diplomacy in the doing.

Careful consideration was also given this past year to the possibility of launching a large-scale investigation of the U.S. social security system. It was my decision to postpone that undertaking, mainly because of problems of funding and staffing. There was much also to be said for waiting until work on social security

and the negative income tax, now in process elsewhere, is finished. If and when the Bureau does turn to such an investigation, it should, in my opinion, be broadened into a general investigation of income maintenance schemes and possibilities, and not be limited to the social security system. Only in this broader context would such a study have relevance to policy problems in the longer term, say the next five to fifteen years.

We have also launched a very broad review of the entire international studies area. With Hal Lary as chairman, a staff committee is considering a wide range of possibilities. Among the many suggestions are growth model studies, comparative analyses of the performance of LDC's, comparative international studies of urban problems, the environmental problems created by increased international trade and travel, comparative studies of foreign and domestic business cycles and stabilization policies, and a study of international transportation costs and their role in shaping trade patterns. This committee is also reviewing the role of the Bureau in its relationship to similar organizations overseas. For example, what arrangements should we possibly make for inviting or sending visiting scholars, and what help might or should the Bureau tender to LDC's in setting up similar bureaus (or government agencies) to measure national income, price movements, etc., elsewhere? This committee obviously has a very large list of problems under consideration. It should be emphasized that it has not yet reached any conclusions or decisions, but it is hoped that the committee will have at least a tentative draft of recommendations completed by late summer.

Several other possibilities for new areas of study have moved into the conceptual stage. Perhaps the closest to actual implementation is that of developing econometric or simulation models of U.S. economic growth processes. In these models the objective would be replicating the trends rather than the cyclical variations in U.S. activity, in contrast with the conventional emphasis of econometric modeling efforts. Empirical growth models could be use-

ful planning tools for both the private and the public sectors.

Still another possibility is that of updating and extending Fabricant's pioneering work on the trends and determinants of government activity. It is a commonplace that government, when measured at all levels—federal, state, and local—has grown substantially in recent years. The interesting question is, Why? To what extent, if any, is the increase in government activity explained by the fact that a larger proportion of our population lives in metropolitan areas today? Or, to what extent might the increase in government activity reflect some decline in church and other private (e.g., the family) support of social welfare activities? Or, to what extent is more government simply a reflection of increasing complexity in conventional government activities, e.g., education and defense?

Other possibilities for new research, though merely thoughts at the moment, would seem worthy of consideration in the not too distant future. For example, have we come to a time when it makes sense to systematically document the status and changes in the status of the American Negro? Sources of data are available which could provide a factual base for such a study—sources that never existed before. We now have the one-in-one-thousand Census sample, some special Census surveys of so-called urban ghetto areas, a variety of special studies and surveys by sociologists of rural-to-urban migration patterns, and a number of public opinion polls which provide many new sources of information on both white and Negro attitudes on racial questions. Such an undertaking, somewhat presumptuously perhaps, might be thought of as an attempt to provide a quantitative and updated version of Myrdal's *American Dilemma*.

Still another activity for the future might be to undertake development of physioeconomic simulation models. Some work has already been done elsewhere with models of this type, notably in relation to water basins and transportation systems. Such models usually combine a growth-oriented economic model with a model that simulates some physi-

cal system. When linked together, these physical and economic models provide a tool for assessing the potential effectiveness of certain large-scale public programs aimed at environmental improvement. In these environmental improvement programs, externalities and dynamic effects are often of the very essence. When they are, evaluation based on the conventional, highly static tools of benefit cost analysis can prove stultifying.

Relating to environmental improvement is another suggestion—an economic analysis of the legal process. Such questions might be asked as: What are the costs and benefits of various legal and illegal activities? How might these be changed or altered by organizational form or procedures? To what extent might lawyers improve the quality of their professional activities if they were to interact more with economists and other social scientists, e.g., become more actively acquainted with the tools of economic analysis, including the informed use of statistical concepts and data? I suppose, too, the reverse question is quite legitimately asked: To what extent might economists benefit from more exposure to lawyers and their professional views of public problems, including the wide variety of compensation activities executed under court aegis today?

IV. THEME

It would be natural to ask if there is any complementarity or theme to all of this current or potential activity. Of course, not all of these proposals will be implemented at the National Bureau. The Bureau's resources are limited and we cannot proceed with every good research idea that may be proposed. In the process of culling, some of the diversity should be reduced.

But one might also ask: How necessary is it to have some unifying theme? While intellectually useful, I think unification can be overdone. I find it difficult to conceive of any single theme that will encompass all Bureau activities. Some diversity or heterogeneity will be present as long as the Bureau attempts to accommo-

date itself, as I think it should to some considerable degree, to the diverse and ever-changing research interests of professional economists in the United States. In short, I believe the Bureau almost always will be subject to charges that some of its research is rather unrelated. Furthermore, I would be disturbed if at some point in time that charge were not made. It would suggest that the Bureau had become much too narrow, conventional, and static.

Nevertheless, having entered these qualifications, I do think that a distinct trend can be recognized in recent and prospective Bureau activities. It is an increasing emphasis on what might be broadly termed "social measurement." An improved understanding of how broad social objectives relate to economic phenomena should lead to an investigation of how the tools of economic analysis might help in achieving particular social objectives more effectively. Indeed, much of this orientation (using the tools of economic analysis to aid in achieving social objectives) is already evident in the Bureau research on education and medical economics.

Any Bureau "assault" on the problems of social measurement should move slowly outward, moreover, from those areas where economic analysis now is or shortly will be making contributions. When and if the Bureau involves itself further in social measurement, the initial projects should be chosen at the interface between economics and other social sciences. And we should not rule out seeking aid from the other social sciences. While economics may perhaps have better theoretical and statistical tools today than sociology or psychology, other social scientists still have much to teach economists about basic sources, survey techniques, field work, and strategies for improving data in these areas.

The trend toward social measurement in Bureau research is dictated by several considerations. First, and perhaps most obviously, is the growing importance of nonmarket activity in our economy. I would observe that this is not necessarily a good thing, but it is simply a fact of our time. Furthermore, for those who

might hope to reverse such trends, the first step must be an understanding of what creates them.

Implicit, if not explicit, recognition of this trend in Bureau research exists already: for example, in the work of Kendrick on measurement of the nonmarket portion of national product and of Juster in measuring the contributions and stock of consumer durables. It is intriguing, in fact, to speculate as to what the relationship might be between the percentage share of nonmarket to market activity in economies at different stages of economic development. I would guess that it might be J or U shaped; that is, the least developed and the most developed economies probably have a higher proportion of nonmarket activities than those at an intermediate stage of development. I am not at all sure that this hypothesis is correct, but it does agree with a good deal of casual observation.

The recent emphasis on social measurement in new Bureau activities may also reflect some decline in the importance of other economic problems. For example, sustained annual growth rates of 4 to 5 per cent in real national output seem increasingly feasible, albeit at a price inflation some may find intolerable or unsatisfactory. Only ten to fifteen years ago, we debated the feasibility of such high growth rates. The current emphasis in Bureau research on wage-price-capacity utilization hypotheses also connotes, seemingly, that the unanswered question today is not whether we can or will sustain 4 to 5 per cent growth but rather whether we can do it without incurring an inflation that is badly disruptive of important economic and social institutions.

Similarly, the business cycle may not have disappeared completely, but its second derivative form of recent years—in this country, in Japan, and in Europe—seems considerably more tolerable than the first derivative model of the 1930's. Certainly, the study of the business cycle, its causes and its cures, has to be deemed of less urgent priority from either a social or economic viewpoint than it was in 1920 when Wesley Clair Mitchell first launched the Bureau with a strong focus on such problems.

The most important reason, however, for emphasizing social measurement in future Bureau research is the substantial gap in our measures and knowledge of social phenomena. Today, our knowledge of social statistics and developments is as inadequate as was our economic knowledge when the Bureau was founded. These circumstances suggest the desirability of a shift in emphasis away from aggregate economic measures and business cycle studies. Incidentally, such a shift (under way now for some years at the Bureau) is a major tribute to the successes of the Bureau, as well as those of many other practitioners of economics.

V. ORGANIZING THE BUREAU FOR THE FUTURE

To launch these new initiatives, some organizational changes have been made at the Bureau. In fact, these changes would have been desirable even if no new research initiatives were contemplated. Not only does the reorganization provide a more flexible and efficient framework for developing new research activities at the Bureau, it also incorporates many needed clarifications of responsibilities. Similarly, the suggested reorganization of Board tenure and selection, yet to be acted upon, would give a broader and more representative cast to the Bureau's Board, recognizing important changes that have occurred in the structure of graduate education in economics in North America in the last two decades.

But the NBER must change in more than organizational structure. To begin, we must have more younger men on our staff. The early post-Ph.D. years tend, in my view, to be the most creative. I am not sure that I would go as far as Schumpeter and say that "no man has an original idea beyond the age of thirty," but I do think that Schumpeter spotted a basic empirical tendency. I would hope that we could have a rotation of young men at the Bureau serving for a period of one to three years or so, shortly after they receive their Ph.D.'s and before their return to permanent academic or other posts. Such a policy would be at least as

beneficial to the Bureau as to the young men. Youth and rotation will help keep the Bureau from stagnating in its research interests and techniques.

In addition, more emphasis must be placed on the use and development of the modern tools of econometrics, mathematical theory, and the electronic computer. These are powerful aids, and their application, where suitable, will improve the quality, depth, and efficiency of Bureau research. Any institution dedicated to quantitative research must be in the van, at the very frontier of developments in these matters. Only in that way can it utilize its research resources effectively.

Greater emphasis on these advanced tools will also adjust Bureau practice to the reality of today's graduate, and even undergraduate, curricula. It is no longer an open question whether statistics and mathematics are an indispensable part of an economist's education. That question has been answered, and answered strongly, in the affirmative. These tools are permeating economic research practice everywhere, not merely in the Academy, but in business and government research as well. The growing ubiquity of these tools, in fact, greatly mitigates the one reasonable argument I have heard against their wider use at the Bureau, namely, some potential difficulties in communicating results. Today, one might argue that unless the Bureau becomes more *au courant* with modern mathematical and statistical techniques, it is in danger of being unable to communicate with the profession, of not knowing the modern phrases, concepts, and even the jargon, which, while it may grate upon the ear at times, does provide economy in professional communication.

In saying this, however, I do not mean to imply that the Bureau should abandon substantive research to do research only on methodology. Nor should it abandon its efforts to communicate its findings to the public at large. In general, the emphasis should remain where it has always been, on the substantive study of economic and closely related social processes. However, as I observed earlier, much is to be said for more balance between methodological

and substantive research at the Bureau.

These, then, are the matters which I feel should be emphasized as we undertake new initiatives in the coming years: studies in human resources and growth processes, social measurement, youthful staff, and the development and application of modern research techniques. This program does not mean that tradi-

tional interests should be relinquished by any means. But it does suggest how our resources should be allocated or reallocated as we move on to new undertakings. Within these guidelines, I have little doubt but that we can close the gap that exists between what the Bureau now accomplishes, significant as that is, and what it potentially can contribute.

TOWARD PRECISION IN ECONOMIC KNOWLEDGE

GEOFFREY H. MOORE

I

The National Bureau's charter, adopted in 1920 by the group of far-sighted men who became the Bureau's first Board of Directors, states that the purpose of the organization is "to conduct, or assist in the making of, exact and impartial investigations in the field of economic, social, and industrial science." Wesley C. Mitchell, Director of Research from 1920 until 1945, in reviewing the Bureau's first quarter-century, summarized both his own and the Bureau's philosophy in the following way. "We like to think of ourselves," he said, "as helping to lay the foundations of an economics that will consist of statements warranted by evidence a competent reader may judge for himself."

Thus from the outset there has been, in the National Bureau's work, an emphasis upon precision, findings, evidence, and clarity. The Bureau's founders discerned a need for greater precision in economic knowledge, and the aim of the institution they founded was to meet that need. That aim was in tune with the times, for the past fifty years have seen an extraordinary increase throughout the world in the emphasis placed upon economic measurements and in the effort to provide them. The wide-ranging demands for active, sharply focused economic policies, the outpouring of economic statistics, the multiplicity of economic surveys and forecasts, the vigorous interest of scholars in econometric models—all bear witness to this

revolution in economic thinking. We demand to know, and to know with some quantitative precision, what are the facts. We wish to measure the causes and to calculate the consequences of economic events and decisions.

The National Bureau can properly claim a share of the responsibility for this development. Its studies have contributed directly to the fund of quantitative information on such matters as the size and distribution of the national income, the volume and terms of consumer credit, the duration and amplitude of business cycles, and countless other aspects of economic life. Its example has inspired many at home and abroad to continue or extend such measurements. Its investigations have provided materials for quantitative work on a wide variety of economic problems.

The new *International Encyclopaedia of the Social Sciences*¹ testifies to the importance of the National Bureau's role in this as well as in other respects. A considerable number of the basic articles on economic measurement or analysis were contributed by present and former members of the Bureau's staff, and in most instances they wrote on a subject to which their Bureau research pertained. Examples are the articles by Easterlin on economic growth, Fabricant on productivity, Griliches on agricultural productivity, Mincer on labor force participation, Burns on business cycles, Goldsmith on national wealth estimation, Lampman on wealth distribution, Kendrick on national income and product, Friedman on money,

NOTE: I am indebted to Solomon Fabricant, Victor Fuchs, Hal Lary, and other colleagues for their comments on an earlier draft of this report.

¹ Macmillan and Free Press, New York, 1968.

Selden on velocity of circulation, Stanback on inventory behavior, Stigler on competition, Nelson on mergers, Meyer on transportation, Saulnier on government credit, Halcrow on agricultural credit, Holland on pension funds, Shiskin on seasonal adjustment of time series, and Zarnowitz on economic forecasting. Since the editors of the *Encyclopaedia* "endeavored to select for each article the most qualified scholar we could locate," the roster of Bureau contributors reflects, I take it, the profession's favorable view of the quality of the Bureau's output on these subjects. The founders of the National Bureau would, I daresay, have been delighted had they foreseen that nearly fifty years after their first meeting their institution's contribution to economic knowledge would achieve such recognition. An encyclopaedic knowledge of economic processes was, after all, what they sought to establish.

The founders, and their successors, were striving for precision. How far have we achieved it? How accurate are the economic measurements we use—both those that establish the levels of activities and those that establish relationships among activities? Knowledge of the limitations of these measurements—the possibilities for error—is nearly as important as the measurements themselves. We can avoid some mistakes, provide for various contingencies, heed more promptly and appropriately the consequences of error, and take steps to improve future performance, if we know the degree of precision of the present stock of measurements. A number of recent studies by the National Bureau have contributed new information of this sort, and others are under way or planned. Since they help to show not only how far we have come, but how far we have yet to go, they provide a fitting subject for review in our annual examination of results and plans.

II

When we say that the gross national product in 1967 was \$785 billion we do not mean it was precisely that. Quite apart from conceptual or definitional variations, that figure is merely the current estimate, and later this year it will be

reestimated and the new figure will undoubtedly be different. Next year the 1967 figure will be reestimated again, and again it may be different. These revisions indicate something of the fallibility of the estimates, and Rosanne Cole's systematic study of them, just completed, puts the record at our disposal. It supplies ample evidence for the proposition that the accuracy of an estimate increases as the amount of information on which it is based accumulates. Current estimates of GNP tend to be more accurate than forecasts, but less accurate than estimates made at a later date. More and more information upon which to base the estimate becomes available over time, and hence the estimates of most recent "vintage" are apt to be the best. Indeed, one cannot say much about the accuracy of an estimate of gross national product without first knowing its vintage.

Miss Cole finds that during 1953–62 the average error in official annual estimates of gross national product made immediately after the end of the year in question was \$12 billion. Since the average level of GNP during that period was \$463 billion, this is a mean error of 2.6 per cent. For estimates made six months later, that is, in July of the following year, the average error dropped to \$9 billion. A year after that revisions brought the average error down to \$8 billion. In each case, the standard against which the errors in estimates are measured is the historical series published in 1965. Since the 1965 estimates cannot be wholly free from error either, the calculated errors may well understate the absolute magnitude of the true errors.

When these *ex post* results are brought together with the errors in forecasts of GNP assembled from various sources in Zarnowitz' study (discussed below), an interesting sequence emerges. Forecasts made in July of the year preceding the year being forecast were wide of the mark by \$29 billion, on the average. Forecasts made in the late autumn erred by an average of \$19 billion. Those made in the middle of the year being forecast had an average error of \$16 billion, while for those made in the late autumn of the same year the mean

error was \$12 billion. These last forecasts were, indeed, about as accurate on the average as the official estimates published a few months later, that is, in January or February of the following year. The chart in Miss Cole's report in Part III, below, shows these results together with those I have just mentioned. They imply that the additional information about the gross national product for a particular year that is accumulated over a span of three years, starting six months before the year in question, reduces the average error of estimate from \$29 billion to \$8 billion, or from more than 6 per cent to less than 2 per cent.

A substantial portion, roughly half, of these errors result from systematic underestimation of the level of GNP. That is, the forecasts as well as the later estimates underestimate the level of GNP as "finally" revised in 1965. If this underestimation is corrected for, the average remaining error in the forecasts made six months before the year being forecast was \$17 billion. This dropped to \$5 billion at the close of the year being forecast, and to less than \$3 billion in the estimates made a year and a half later.

Miss Cole goes on to examine errors in major components of GNP, errors in quarterly data, errors in estimating cyclical changes and long-term rates of growth, and the degree to which preliminary estimates have improved in accuracy during the postwar period. She finds, for example, that in each of the four postwar recessions the initial quarterly data substantially overestimated the decline of GNP. The initial 5.6 per cent decline in 1948-49 became a 3.4 per cent decline in the latest estimate; the 1953-54 contraction of 3.8 per cent became 1.9 per cent; the 1957-58 drop of 4.1 per cent became 2.6 per cent; and the 1960-61 decline of 1.0 per cent became a minor dip of 0.3 per cent. These are substantial reductions, averaging close to 50 per cent, and are bound to affect materially any calculation of the loss of output attributable to recession, or of the gap between actual and potential output.

On the question whether preliminary estimates of GNP have become more accurate in recent years than formerly, it appears that

initial estimates of quarterly changes have indeed improved. The average error of \$5 billion in 1947-54 declined to \$3 billion in 1955-61. Estimates of short-run change in most of the major components of GNP—consumption, investment, government expenditures—show a similar improvement.

III

I referred above to Victor Zarnowitz' study of forecasts. His initial work, *An Appraisal of Short-Term Economic Forecasts*, was published during the year. He analyzed the postwar record of several hundred forecasts of GNP and its components, and of industrial production. Since he wrote, further data have been compiled, extending the record in several dimensions, including forecasts of price levels, employment, and unemployment. This is probably the most extensive record of economic forecasts ever assembled, and it provides materials for analyses of the factors making for error, the statistical characteristics of forecasts, and the potential value of various techniques for improving forecasts. Beyond this, it provides a testing ground for hypotheses in the economics of expectations. Jacob Mincer and others have been exploring this field, and a volume of essays, "Economic Forecasts and Expectations: Analyses of Forecasting Behavior and Performance," will shortly be submitted to the Board for review.

In another part of this work on forecasting Rendigs Fels recently completed a study of the recognition of business cycle turning points since 1948, as culled from outlook statements regularly published in a number of influential business, financial, and other periodicals; and C. Elton Hinshaw applied the same technique to the views on the business outlook recorded in the minutes of the meetings of the Federal Reserve's Open Market Committee. They find little evidence of ability to forecast turning points. Typically, recognition comes shortly after the event. More accurately, what they establish is that the degree of certainty regarding a turn in the business situation is modest some months before it occurs but rises steadily thereafter until virtual certainty is achieved

six months or so after the turn. In view of the lags in the availability of data, and the fact that major economic indicators rarely reach their turns at the same time, this is not necessarily poor performance. But the record that Fels and Hinshaw have compiled does underline the limitations on our power to foresee reversals in the trend of activity. Those in command of our monetary policy seem to do somewhat better than the average analyst in the collection of publications reviewed, but their performance is well within the range of other experience.²

One of the reasons why turns in the business cycle have not been recognized more promptly may lie in the behavior and adequacy of the statistics that anticipate such turns. The report published early last year, *Indicators of Business Expansions and Contractions*, on which Julius Shiskin and I collaborated, provided some evidence to this effect. It showed that those indicators that have the longest leads tend to be more erratic, conform less regularly to the business cycle, and are often less promptly available than those that move in roughly synchronous fashion with or lag behind the business cycle. Some of these differences are probably inherent in the economic nature of the activity represented. New orders, for example, anticipate production of goods made to order, but their fluctuations are ironed out with a view to stabilizing production schedules and providing a smoother flow of work. Hence the cyclical turns in output, which lag behind those in orders, are inherently easier to recognize. But some differences, such as those in promptness of publication or in statistical coverage, are remediable. One of the routes towards prompter recognition of recession or recovery is to be found, therefore, in improvements of the anticipatory statistics. The materials provided in our report were designed, in part, to give some guidance to where such improvement is most needed.

Another factor that has impeded the prompt recognition of economic downturns is the occa-

sional occurrence of periods that bear some of the earmarks of recession but do not, in the end, turn out to be as serious as earlier recessions. In order to avoid being misled by a false signal, the analyst may hesitate before recognizing a true signal. Such periods have occurred often enough—in 1947, 1951, 1956, 1962, and 1966—to give pause to those who might otherwise be inclined to proclaim recession promptly. These periods have, indeed, been marked by substantial reductions in the rate of economic advance, seriously affecting some industries, some localities, and some disadvantaged groups in the population, and hence are not to be dismissed as unimportant. What is needed most is a better understanding of why some such episodes turn into serious recessions while others do not, what role economic policies have to play in this regard, and how forecasting techniques can be improved to guide such policies.

The postwar experience of Western Europe and Japan may be helpful here. These countries have experienced few contractions—that is, absolute declines—in aggregate economic activity during the postwar period, but they have experienced retardations in growth, and many observers have noted the similarities between these periods and those which in former times or in the United States have taken the form of contractions. Michael Michaely's study of balance-of-payments adjustment policies in these countries, of which a first instalment has just been issued, will teach us something about these retardation periods. So also will Ilse Mintz' work on the problem of defining such periods. The "reference chronologies" she is developing for cycles in growth rates, or (what is nearly the same thing) cycles in the gap between actual and potential output, will identify these episodes in Western Germany, Great Britain, and other countries with greater precision than has hitherto been the case, and hence aid analytical studies on a wide front. The techniques she is testing are also clearly applicable to the United States, and some preliminary work along these lines is getting under way.

Forecasting via econometric models has

² See Rendigs Fels and C. Elton Hinshaw, *Forecasting and Recognizing Business Cycle Turning Points*, NBER, 1968.

been engaging the attention of an increasing number of economists since World War II. Our plans for the study of short-run forecasts included an evaluation of the record of econometric forecasts. Jon Cunyningham carried out an examination of some of the forecasts made by annual models, and concluded that they were about as accurate on the average in predicting annual changes in GNP during 1953-63 as the better forecasts included in Zarnowitz' general survey. Yoel Haitovsky and Michael Evans are planning to evaluate several of the quarterly models. Plans have been drawn, also, for a conference on econometric models of cyclical behavior, sponsored by the Committee on Economic Stability of the Social Science Research Council and the Conference on Research in Income and Wealth. While not devoted primarily to an evaluation of forecasting performance, the papers planned for the conference will include comparisons of the cyclical behavior of variables generated by the models with the behavior of their real life counterparts. Moreover, econometric tests of the properties of leading indicators will be carried out. In this way it is hoped that we may learn more precisely what degree of forecasting success is attached to various techniques, and how they may be improved.

A recent news item in *The New York Times*, citing a survey of 15,000 households conducted by the Bureau of the Census, stated that "The change in probability of purchasing a new car among all households revealed a decline of 7.6 per cent, compared to such probability in October 1967." The article went on to say that "In the Northeast, the decline in probability came to 10.9 per cent, in the West 2.9 per cent, and among families with incomes of \$7,500 to \$10,000 it fell 15.9 per cent." These results reflect the recent adoption by the Census Bureau of the scheme developed and tested by Thomas Juster for increasing the precision of forecasts of consumer purchases of automobiles, houses, and other major items. Surveys of spending plans have generally failed to identify the households that make the bulk of the purchases. Juster conjectured that many of those who said they had no plans to buy

would nevertheless be in a position to estimate the probability that they would buy, and a similar discrimination could be made by those with plans to buy. A series of tests conducted with the cooperation of the Census Bureau indicated not only that consumers were willing to make probability judgments but also that, when checked later against what they actually did, the probabilities did in fact help to discriminate among buyers and nonbuyers. These experiments are still going on, but meanwhile regular quarterly surveys based upon this principle have begun. As experience with the survey accumulates, a fuller assessment of its capacity to increase the precision of forecasts will become possible.

IV

The behavior of prices has long been a concern of the National Bureau. Mitchell's classic study, *The Making and Using of Index Numbers*,³ while antedating the founding of the National Bureau, had a major influence on much of the Bureau's early work. Frederick Mills' monograph, *The Behavior of Prices* (NBER, 1927), and subsequent reports published over a long period illuminated the characteristics of the system of prices that records and allocates and motivates economic activities. Hultgren's work on *Cost, Prices, and Profits: Their Cyclical Relations* (NBER, 1965), added important elements to the picture. The work now under way by Fabricant on price behavior and guideposts, by Friedman and Schwartz on money and prices, by Cagan on inflation and price expectations, by Nordhaus on wages and prices, by Gordon on capital goods prices, and by others will deepen our understanding of both short-term and long-term movements in prices.

Two important studies that attempt to improve the precision with which prices are measured are nearing completion. Both have made extensive use of prices as reported by buyers, departing from the traditional practice of se-

³ U.S. Department of Labor, Bureau of Labor Statistics, Bulletin 173 (1915); reprinted as Bulletins 284 (1921) and 656 (1938).

curing price quotations from sellers. Kravis and Lipsey tackled the problem of measuring the comparative level and change over time in U.S. and other countries' prices for the same commodities. Such comparisons, based on actual transaction prices, measure our competitive position with respect to prices, and show whether it is improving or deteriorating. Stigler and Kindahl have obtained actual transaction prices for a wide variety of domestic products, and are constructing wholesale price indexes for comparison with similar, official indexes based largely on list prices. The transaction prices are expected to turn out to be more sensitive, but how much more sensitive and what other differences will emerge remains to be seen.

Both of these projects seem likely to have a lasting effect on our knowledge of prices by influencing new, regular programs for price collection. In the recent *Economic Report of the President*⁴ two of the ten items in a program for improvement in economic statistics involved prices. One was to provide better measures of international price competitiveness; the other was to develop improved industrial price indexes emphasizing actual transactions rather than quoted prices. If these recommendations are implemented, our experimental work along these lines will have proved most worthwhile.

V

The justification for efforts to obtain more precise estimates of gross national product, indexes of prices, and other economic data lies in the contribution these data make to knowledge of economic relationships. We are persuaded that to understand the causes underlying economic phenomena we need exact measures of the variables on both sides of the equation—the causes as well as the effects. Without such measures our power to discriminate among hypotheses diminishes, our inferences become more uncertain, and our policy decisions more prone to error.

It was basically for this reason that Mitchell

⁴ January 1968, p. 92.

began to assemble the extraordinary collection of statistical data pertaining to business cycles that we have today. Our new listing of these series, completed recently by Stern and Reichner, contains some 2400 entries. Mitchell would not settle for a limited selection, for he knew—and those who later took up the study of particular aspects of business cycles came to share his conviction—that the opportunities for checking the results obtained were too important to be ignored. To pursue such opportunities was a vital part of the scientific process. Hence the many members of the staff who contributed to this collection over the years took care to include the alternative measures of the same economic process that are frequently available. They took care also to include the entire historical record for each series, to provide some degree of geographic coverage, and to include the components of many aggregates as well as the global figures.

This collection, together with other materials, is the basis for an Economic Time Series Library that is being developed at the National Bureau. The organization of such a library is not a simple task, as two recent efforts to make portions of the collection more generally accessible have shown. The *Source Book of Statistics Relating to Construction*, by Lipsey and Preston, published in 1966, and Fiedler's volume on "Measures of Credit Quality," which is nearly completed, both turned out to be far more substantial and expensive projects than anticipated. The provision of source notes, and of adequate and accurate descriptions of the data, so essential if the materials are to be used responsibly by research workers, requires a massive effort by trained and dedicated persons. Making the data available in machine-readable form, a task that has already been accomplished for a portion of the collection, is the smaller part of the job, though essential for efficient use.

The precision of the inferences drawn from economic data often depends crucially upon a clear understanding of how the data were compiled and what they signify. As an example, take the fairly common practice of obtaining a current index of productivity change by di-

viding the Federal Reserve index of industrial production by the man-hours of employment of industrial workers. Unless one were aware that the compilers estimate a very large fraction of the production index from man-hours of employment adjusted by extrapolated estimates of change in output per man-hour, one would not realize that the computation was to a considerable degree simply reproducing the previous extrapolations. Nor would one be concerned to find out to what extent one's conclusions were affected by this circumstance.

Study of economic relationships often leads to an improvement in the basic data themselves. Gaps are uncovered, the advantages of a change in the concept of what is to be measured are discerned, and the needs for data improvement are clarified and emphasized. To illustrate, Jack Guttentag points out in the introduction to the first of two planned volumes of "Studies of Interest Rates" that one of the basic objectives of the project was to illuminate the effect of financial variables on economic activity, and that a number of the essays in the volume contribute directly to this objective. But he observes also that the work has clarified the problems involved in properly measuring financial influences on specific types of real output. "We have," he says, "examined the problems of recording lags in rate series, of changes in composition of the instruments underlying rate series that affect their homogeneity, and of nonrate dimensions of loan transactions that may be used to 'ration' credit. In the process, we have invested in the collection of new data on rates and other transaction characteristics in cases where the available data were badly deficient."

The hard test of our knowledge of economic relationships comes when we use them to predict. Do they remain substantially the same when we examine the facts for a later or for an earlier period, or for a different economy? The reports on our research into economic relationships generally imply that they are believed to be stable, or that they are changing in a systematic way. These "predictions" of stability or change are subject to verification at a later date. For example, for nearly thirty years the

Bureau has published selected lists of leading and other indicators. Such lists would be of only historical value unless the indicators continued to exhibit substantially the same properties for some time after they were selected. Hence we have periodically reexamined their performance, and, at the same time, have tried to incorporate improvements. Such a reexamination, of which the most recent was reported last year, gives one a lively sense of the changes in economic institutions, in economic relationships, and in the quality of economic statistics that are part of the world we live in. It also provides evidence of the continuities in the economic system and its operations, without which our efforts to profit from history would be fruitless.

The relationships exhibited by the indicators have been tested not only by examining their subsequent performance in this country, but also by studies of their behavior in other countries, most notably Canada and Japan, where regular reports on the status of leading and other indicators have been issued for some time. This type of test, where the experience of other countries is drawn upon, seems likely to become more important in our own work. Hal Lary's latest book, *Imports of Manufactures from Less Developed Countries* (NBER, 1968), exemplifies the procedure. In order to identify the kinds of manufacturing industries for which the less developed countries might be expected to have a comparative advantage, Lary uses value added per employee as an index of the degree to which an industry is capital-intensive, since this measure can be taken to represent inputs of both human capital (or skills) and physical capital. But in using the measure for this purpose, it is important to know whether the same industries would be ranked the same way in different countries, despite the wide differences among countries in the availability of capital relative to labor. Hence Lary carries out extensive tests comparing the data for different countries, both developed and less developed, and finds, indeed, a common pattern among them. With the identity of labor-intensive industries rather firmly established, it becomes possible to pre-

dict and to appraise the export performance of the less developed countries in these products, and Lary concludes his analysis with some predictions and appraisals.

VI

Economic relationships can be studied empirically in many ways. Economists with an historical bent can observe events or trends, speculate about the factors that led to such developments, test their speculations against other evidence, and draw their conclusions accordingly. Economists with a statistical bent will be inclined to develop quantitative measures of economic phenomena for populations of individuals or enterprises, submit them to a similar process of theorizing and testing, and draw inferences from the statistical estimates and tests of hypotheses. Economists with a mathematical bent will express their hypotheses in the form of equations or models, match as best they can the available data with the variables specified in the equations, and again estimate the relationships and test their validity. The sequences in this process of research are not invariant, and many combinations of methods of examining economic relationships are found in practice. The econometric historian is, for example, a recent hybrid. Interest in the mathematical and statistical approaches has widened of late; model building and model testing have come into vogue, and many economists now call themselves econometricians.

The studies of economic relationships under way at the National Bureau vary as widely in the methodology used as they do in the economics profession at large. Readers of the current or other recent annual reports may be struck by the number of times the word "model" is used. Partly this is a matter of semantics. Twenty or thirty years ago the term "theory," or perhaps "mathematical theory," would commonly have been applied to the same concept or method of attack on a problem. But partly it represents a real shift towards the mathematical, equation-estimating type of analysis, stimulated in some degree by the computer, which greatly facilitates the solution of complicated mathematical systems. Our at-

tention, however, has been focused on the substance of research, the precision or validity of the findings, and not on methodological considerations. With this as our aim, the methodology we use is almost certain to vary widely, depending on the type of problem, the data available to study it, and the interests and qualifications of the investigator.

An illustration of this wide variation in methods, as well as the interaction between economic measurement and studies of relationships, is afforded by the history of the National Bureau's work on the factors influencing spending and saving, or the consumption function. It is a long history. Indeed, it was forecast in Mitchell's first *Annual Report* to the Board, in 1921, when he said that after the report on national income then being prepared was finished, "we should take up for careful study the shares of wages, rent, interest and profits, and the subject of savings vs. current consumption." Much of the work that was taken up was directed toward providing basic measurements not only of income, consumption and savings, but also of cash balances and other assets, debt, prices, interest rates, credit terms, and other relevant factors. Mitchell, Macaulay, Knauth, King, and Kuznets were among the early contributors to the development of systematic measures of these variables, while Goldsmith, Copeland, Kendrick, Lipsey, Juster, and Shay have contributed more recently. Without the basic measurements, constructed for long historical periods, that the patient labor of these and other men provided both at the National Bureau and elsewhere—measurements of the level and distribution of income; of consumer expenditures for housing, durable goods, and other goods and services; of prices paid; of consumer savings; of liquid assets and other forms of wealth; of consumer instalment debt and the terms upon which it is provided—the study of the consumption function would still be in an elementary stage. This work was not purely statistical. It involved thinking through the appropriate concepts, comparing alternative formulations, and devising ingenious ways to get the information or approximations to it. Many of the meetings of the Con-

ference on Research in Income and Wealth over the years have been devoted to these problems.

As more and more such measures became available, more and more empirical studies of relationships became feasible. Simon Kuznets investigated the long-term trend in the savings ratio and found it to be surprisingly constant, despite the rising real income of the population. Gottfried Haberler noted the close relation between fluctuations in consumer instalment credit and the purchases of durable goods, and Avram Kisselgoff later found that the use of credit in turn was markedly affected by relaxing or tightening credit terms. Lawrence Klein studied the numerous factors accounting for variations in spending and savings rates among different families in a given year. Robert Ferber examined the goodness of fit of some eighteen consumption functions that had been constructed by others, both during and beyond the period to which they had been fitted.

By 1952, when Arthur Burns reviewed these studies in an essay entitled *The Instability of Consumer Spending*, we had already learned that the relationship between consumer spending and income was both difficult to estimate with precision and significantly affected by many other factors. Many of our more recent studies have continued to deal with this important subject. Milton Friedman developed and tested his "permanent income hypothesis," assigning a critical role in the consumption function to prospective income, distinguishing it from the actual, measured income of the period. A few years ago Juster and Shay devised an ingenious survey method to study consumers' reactions to changes in finance rates, and Philip Klein is presently analyzing the sequence of cyclical turns in credit extensions, repayments, and sales to consumers. Juster recently examined the trend of savings when purchases of houses, automobiles, and other durables were treated as a form of investment rather than consumption, and documented the important fact that the consumer has become a bigger as well as a more fickle investor than the business man. Juster also plans to study, with the aid of surveys con-

ducted by the Bureau of the Census, the relation between expected income and expected savings when both are estimated in advance by the consumer. Gregory Chow, Arthur Burns, and I are experimenting with various forms of the consumption function for use in an econometric model of business cycles. Victor Fuchs and his colleagues have been delving into some fascinating aspects of the consumer's demand for services, including the demand for medical care. Gary Becker and Robert Michael are developing the notion of a household "production function," which, among other things, may lead to measures of the influence of education upon the satisfaction derived from spending or saving out of a given income.

Some of these studies of consumer economics will take an econometric turn; others will not. Basically, the appeal of econometrics is its promise of greater precision in the estimation of relationships and in specifying their limitations. But in considering these potential benefits one must also consider the costs, or the opportunities to attain precision by other routes. Most large econometric models, for example, can be constructed only from data for the postwar period, and sometimes only for the later part of that period, because appropriate data are not available earlier. This does not mean, of course, that nothing of relevance can be learned from the less abundant data for earlier periods—the large scale model is simply not well-adapted to utilize such partial information. All of which means that we must continue to remain alert to the peculiar advantages of each of the methods of analyzing economic relationships that presently exist or will be invented.

VII

My quick survey of some of the things the National Bureau has done and is doing to make our economic knowledge more precise has merely skimmed the surface, turning up a few illustrations. Our entire research program, as the reports by individual members of the staff testify, can be thought of as contributing to this objective. The new studies we are planning—described in the President's report and in

Part II below—will, we hope, also make their contribution. Some of them will attempt to improve the data relevant to economic problems and policies. Others will take the data more or less as given and attempt to improve measures of economic relationships. Still others will do both. And some will be concerned with

establishing the degree of precision attaching to the work already done. From all of them we can expect, as the founders of the Bureau confidently did nearly fifty years ago, a perceptible improvement in our ability to analyze and resolve the economic problems of today and tomorrow.

NEW MEASURES OF U.S. INTERNATIONAL PRICE COMPETITIVENESS, 1953-64

IRVING B. KRAVIS AND ROBERT E. LIPSEY

The study of comparative prices and price trends has had, as its purpose, the development of new measures of price competitiveness of the major industrial nations in international trade. These measures have been applied to the trade of the United States, the United Kingdom, the Common Market countries, and Japan; they have been applied to the trade of these countries in machinery, transport equipment, metals, and metal products for selected years from 1953 to 1964.¹

The results of the study indicate that in all six of the years for which we have data (1953, 1957, and 1961 through 1964), U.S. export prices for these products were, on the average, 5 to 10 per cent higher than those of its main

foreign competitors. In general, the price competitiveness of the United States relative to the European countries did not change much over this period. There were, however, some striking changes in foreign-U.S. price relationships for particular groups of products.

Before dealing further with the results of the study it may be well to describe briefly the three interrelated sets of index numbers in terms of which the results are presented.

International price indexes. These are indexes of changes over time in each country's prices. They are designed to meet the deficiencies of unit value indexes and of wholesale price indexes which have previously been used in studies of international price competitiveness: (1) they are based on actual prices or price offers, not on list prices or unit values; (2) for goods which the country exports, the prices refer to export rather than domestic transactions; (3) the universe of prices includes prices of all goods, in the classes under study, that enter world trade. The indexes are derived by applying 1963 world trade weights to each country's export prices (or to its domestic prices where there were no exports in a particular category). They measure the change in

¹ The study has been financed mainly by two grants from the National Science Foundation. The publications that have appeared so far are *Measuring International Price Competitiveness: A Preliminary Report*, Occasional Paper 94, 1965; *Comparative Prices of Nonferrous Metals in International Trade, 1953-64*, Occasional Paper 98, 1966; and "A Report on the Study of International Price Competitiveness," *American Economic Review*, May 1967. A paper on "International Price Comparisons by Regression Methods" has been accepted for publication in the *International Economic Review* in 1968.

each country's export prices for the bundle of goods that was exported by the industrial countries as a whole.

The index of price competitiveness. Our main interest in a country's international price index is in its movements relative to the indexes of other countries, since it is presumably through such relative changes that prices influence shifts in trade. The comparisons of price movements can be made by dividing the international price index for each foreign country by the corresponding index for the United States. We call the resulting index an index of U.S. price competitiveness relative to that country. A rise in an index of U.S. price competitiveness, therefore, indicates that the foreign prices have risen relative to U.S. prices; U.S. price competitiveness has thus improved while that of the foreign country has declined.

Comparisons of price levels. The index of price competitiveness can also be derived from a different set of data: country-to-country comparisons of price levels of internationally traded goods at a given moment in time. Changes over time in these place-to-place indexes measure changes in price competitiveness in the same way that comparisons of the time series indexes do. Of course, price level differences are also of interest in themselves, as determinants of the pattern of trade at any one time.

Data to compute these measures were supplied by several sources. Among them were more than 200 American firms who were sellers of machinery and metal products or were involved in international markets through their purchasing activity. A number of governmental agencies supplied data on formal bids by U.S. and foreign firms. One set of foreign data consisted of information on bidding for contracts to supply foreign agencies, mostly governmental, with a wide variety of machinery and equipment, particularly the type required for development projects. Foreign research groups collected additional information from both buyers and sellers in foreign countries.

We believe that those varied sources of price data provided good coverage of the basic

metals, metal products, and machinery included in our study. For some important commodity segments, particularly those usually purchased by the public authorities of developing countries, such as machinery for irrigation and electrical projects, the sample covers a substantial fraction of international trade. All in all, at least some data are included for purchases of firms or public agencies in each of about fifty countries.

Our confidence in the results of the study rests not only on the large number of observations but also on the variety of sources. Data from each individual type of source—buyers as well as sellers, governmental as well as private, foreign as well as American—may be subject to biases of unknown importance, but there was a good chance of overcoming most of these by including a large number of almost every type of transaction that involves world trade.

The main substantive results of the study are summarized in Table I-1, which gives international price indexes, indexes of U.S. price competitiveness, and price level indexes for all the commodities included in the study, taken as a group. Data for the six major components (two-digit divisions of the Standard International Trade Classification) that make up most of the total are presented in Tables I-2, I-3, and I-4. These data are complete but are still subject to some revisions resulting from checking and from experiments with different treatments of missing data. The final publication will present indexes for almost every three-digit group and many four-digit subgroups of these divisions.

Perhaps the most striking result of the study is the similarity in extent and timing of the movement of the international price indexes of the several countries for all the commodities taken together. Between 1953 and 1964, prices rose by about 15 per cent in the United States, the United Kingdom and Germany, and by a smaller amount in the EEC as a whole. This similarity in price movements means, of course, that there was little change in U.S. price competitiveness, relative to each foreign country. The index of price competitiveness—that is, the change in the ratio of foreign to U.S. prices—

stayed within a range of five percentage points. Within that narrow range, U.S. price competitiveness fell relative to all countries between 1953 and 1961, and then recovered all or part of the loss. The EEC countries as a group improved their position relative to both the United Kingdom and the United States.

When price levels are compared, U.S. prices tend to be higher. European price levels were between 5 and 10 per cent below U.S. prices in all the years for which we have data, and ended the period 6 to 8 per cent below.

The similarity of movements in the over-all international price indexes conceals a considerable variation among the countries in price movements for individual commodity divisions. The United States lost heavily in price competitiveness relative to all the other countries in iron and steel, even though there was some improvement in the last year, 1964. In nonferrous metals and in electrical equipment, on the other hand, the United States improved its position relative to all the European countries. Nonelectrical machinery showed little or no trend. The same was true for miscellaneous metal manufactures, except that U.S. price competitiveness declined relative to EEC countries as a whole. In transport equipment, the United States gained considerably on the United Kingdom but lost relative to the EEC countries.

In most of the divisions shown here, foreign prices were lower than U.S. prices in 1964 but the range was wide. The largest differences were in iron and steel, where European prices were 20 to 30 per cent below those of the United States, and in miscellaneous manufactures of metal (mainly products of iron and steel), where European prices were 10 to 15 per cent below the U.S. level. In the other groups the divergence was 10 per cent or less, and in about half of the comparisons U.S. prices were lower or no more than 2 per cent higher than those of the other countries.

Japanese price data were insufficient for computation of a total index but we are able to present Japanese indexes for three major divisions. In two of these, iron and steel and electrical machinery, the Japanese position im-

proved greatly relative to all the other countries and in the third, miscellaneous metal manufactures, the Japanese price level was favorable throughout the period but did not change substantially. We could not calculate price indexes for the other three major divisions, but in one of them, transport equipment, Japanese prices for two major components, automobiles and ships, declined sharply relative to those of other countries. Japanese price levels were low not only in miscellaneous metal manufactures but also in iron and steel, relative to both the U.S. and European prices. In electrical machinery, Japanese prices were lower than U.S. and European prices by smaller margins.

Conclusions on method. Since the development of methods of measurement was the main object of our study, we should list the conclusions we have reached about the methods and direction for future work in this field.

A major conclusion is that it is feasible to collect a great variety of data relevant to the measurement of international price competitiveness that have never been collected before. Working with price collection resources that were very small by governmental standards, we accumulated a large quantity of data for our study. We only nibbled at the data possibilities that exist and could be made available for a well-financed official, preferably inter-governmental, data-gathering project.

From such data, even in the amounts we have been able to accumulate, it is possible to calculate indexes of relative levels and of changes in price competitiveness. These indexes often move quite differently from unit value and domestic wholesale price indexes. On logical grounds they have a strong claim to be superior measures of competitive strength, and in some cases we have evidence that specific unit value series, in particular, give a false picture of price changes. Whether the new measures will turn out to have great predictive or explanatory value is still to be determined by attempts to use them in the analysis of trade structure and trade flows.

It seems clear that future efforts at data collection in this field should be conducted in more than one country—preferably in many coun-

tries. Comparisons in any one market tend to be biased by trade barriers, consumer preferences, differences in the degree of competition and in the range of products purchased, and many other factors. Furthermore, the amount of data on world markets available to an investigation in one country is likely to be limited. Ideally, an international agency should act as a clearing house through which countries could exchange data derived from their government purchasing activity and from firms operating domestically, with each government and firm reporting on its foreign as well as its domestic operations. It would be important, for many products, to collect data from the less developed countries, since these are major markets for many products important in economic development projects. Even a less ambitious exchange, one between any two or three countries, could add greatly to the information available to each on its competitive position.

For capital goods particularly, we have found that the use of regression methods for international price comparisons permits the coverage of many complex products which would defy comparison by conventional methods involving identical specifications in two situations. For many producers' goods, and durable consumer goods as well, there are no cases of identical specifications from two countries or, in some products, from two periods of time. We have applied regression methods to such products as locomotives, aircraft engines, automotive diesel engines, outboard motors, tractors, chemical reactors, automobiles, and ships, most of which would have presented insurmountable difficulties for comparison via identical specifications.

Another experiment we consider successful and applicable to further work in this field was the abandonment of the traditional method of using preselected specifications, and the placing of the burden of finding comparable products on the respondent. Especially for more complex products, it would have been impossible to pick specifications applicable to any large number of respondents; each reporting company, in fact, generally chose commodities slightly different from those selected by others,

except in some standard, high-volume metals and metal products. The use of bidding data followed the same method, in a sense, since each bid comparison was for a very particular product or set of products, and it was rare that an earlier or later set of bids could be found for items that were identical to the degree required by present price collection methods. No set of preselected specifications could have turned up more than a small fraction of the items appearing in these bids.

Finally, the prevalence of many types of discounting—for cash payment, for size of order, or simply to meet expected competition—was often ignored in sellers' reports and only revealed in buyers' reports and in bidding documents. These differences point strongly to the need for collecting data from buyers as well as from sellers.

In reality even transaction prices should be regarded only as a reference base from which continual additions and subtractions are made through changes in such factors as credit terms, delivery time, and the provision of various services. Differences or fluctuations in these factors do not of course appear in the invoice price. All of them could conceivably be priced, but the information is difficult to obtain, and the lack of it probably causes us to underestimate the real degree of price flexibility.

In the course of our study, these and other aspects of international competition frequently came to our attention. While they were not the main focus of our effort, we plan in our final report to summarize our findings with respect to variations in delivery time and our observations with respect to price discrimination among markets and the role of product differentiation and other nonprice factors in trade patterns.

We hope that the outcome of this investigation will encourage government and international agencies to pursue the measurement of international price relations on a more comprehensive basis. The Bureau of Labor Statistics has been exploring the possibilities for price collection within the United States and has found that there is a great deal of interest among foreign governments in international

collaboration along these lines. The successful development of such an intergovernmental program would add to our understanding of trade patterns and of changes in the balance of payments of industrial countries, and should also

be useful in analyzing the shifts in trade for specific groups of commodities. Our experience has persuaded us that the job can be done and that indexes like these are likely to be a great improvement over existing measures.

TABLE I-1
INDEXES OF INTERNATIONAL PRICES, INTERNATIONAL PRICE COMPETITIVENESS,
AND PRICE LEVELS FOR ALL MACHINERY, TRANSPORT EQUIPMENT,
METALS, AND METAL PRODUCTS, 1953-64

	1953	1957	1961	1962	1963	1964
International Price Indexes (1962 = 100)						
U.S.	88	97	100	100	100	101
U.K.	90	99	101	100	101	104
EEC	92	97	99	100	100	102
Germany	89	95	99	100	100	102
Indexes of U.S. International Price Competitiveness (1962 = 100)						
Relative to U.K.	102	102	101	100	101	103
Relative to EEC	104	100	99	100	100	101
Relative to Germany	101	98	99	100	100	101
Price Level Indexes (U.S. for each year = 100)						
U.S.	100	100	100	100	100	100
U.K.	93	93	92	91	92	94
EEC	95	91	90	91	91	92
Germany	93	90	91	92	92	93

TABLE I-2
INTERNATIONAL PRICE INDEXES FOR MACHINERY, TRANSPORT EQUIPMENT, METALS
AND METAL PRODUCTS, BY SITC COMMODITY DIVISION, 1953-64
(1962 = 100)

	1953	1957	1961	1962	1963	1964
Iron and steel (SITC 67)						
U.S.	85	101	102	100	99	100
U.K.	102	111	103	100	96	103
EEC	103	120	105	100	97	105
Germany	98	113	105	100	95	104
Japan	na	na	110	100	99	100
Nonferrous metals (SITC 68)						
U.S.	97	101	101	100	100	108
U.K.	97	102	100	100	102	116
EEC	102	103	101	100	101	118
Germany	103	106	101	100	99	117
Manufactures of metal, n.e.s. (SITC 69)						
U.S.	85	96	99	100	100	103
U.K.	84	99	103	100	100	102
EEC	88	95	100	100	96	99
Germany	82	94	99	100	99	102
Japan	na	97	98	100	95	103
Machinery, other than electric (SITC 71)						
U.S.	81	92	100	100	101	102
U.K.	82	93	100	100	101	103
EEC	80	87	98	100	101	101
Germany	79	86	98	100	101	101
Electrical machinery, apparatus and appliances (SITC 72)						
U.S.	98	112	104	100	97	97
U.K.	88	104	102	100	102	100
EEC	94	102	102	100	100	98
Germany	92	100	101	100	100	98
Japan	na	136	106	100	96	100
Transport equipment (SITC 73)						
U.S.	89	94	96	100	99	99
U.K.	91	97	101	100	102	106
EEC	95	96	96	100	100	100
Germany	92	95	96	100	101	100

TABLE I-3
INDEXES OF U.S. INTERNATIONAL PRICE COMPETITIVENESS FOR MACHINERY,
TRANSPORT EQUIPMENT, METALS, AND METAL PRODUCTS,
BY SITC COMMODITY DIVISION, 1953-64
(1962 = 100)

	1953	1957	1961	1962	1963	1964
Iron and steel (SITC 67)						
Relative to:						
U.K.	120	110	101	100	97	103
EEC	121	119	103	100	98	105
Germany	115	112	103	100	96	104
Japan	na	na	108	100	100	100
Nonferrous metals (SITC 68)						
Relative to:						
U.K.	100	101	100	100	102	107
EEC	105	102	100	100	101	109
Germany	106	105	100	100	99	108
Manufactures of metal, n.e.s. (SITC 69)						
Relative to:						
U.K.	99	103	104	100	100	99
EEC	104	99	101	100	96	96
Germany	97	98	100	100	99	99
Japan	na	101	99	100	95	100
Machinery, other than electric (SITC 71)						
Relative to:						
U.K.	101	101	100	100	100	101
EEC	99	95	98	100	100	99
Germany	98	93	98	100	100	99
Electrical machinery, apparatus and appliances (SITC 72)						
Relative to:						
U.K.	90	93	98	100	105	103
EEC	96	91	98	100	103	101
Germany	94	89	97	100	103	101
Japan	na	121	102	100	99	103
Transport equipment (SITC 73)						
Relative to:						
U.K.	102	103	105	100	103	107
EEC	107	102	100	100	101	101
Germany	103	101	100	100	102	101

TABLE I-4
 PRICE LEVEL INDEXES FOR MACHINERY, TRANSPORT EQUIPMENT, METALS AND
 METAL PRODUCTS, BY SITC COMMODITY DIVISION, 1953-64
 (U.S. for each year = 100)

	1953	1957	1961	1962	1963	1964
Iron and steel (SITC 67)						
U.S.	100	100	100	100	100	100
U.K.	95	87	80	79	77	81
EEC	92	90	78	76	74	80
Germany	90	87	80	78	75	81
Japan	na	na	79	72	72	72
Nonferrous metals (SITC 68)						
U.S.	100	100	100	100	100	100
U.K.	92	93	92	92	94	98
EEC	94	92	90	90	91	98
Germany	99	98	93	93	92	100
Manufactures of metal, n.e.s. (SITC 69)						
U.S.	100	100	100	100	100	100
U.K.	90	94	95	91	91	90
EEC	95	90	92	91	87	87
Germany	83	84	86	86	85	85
Japan	na	70	68	69	66	69
Machinery, other than electric (SITC 71)						
U.S.	100	100	100	100	100	100
U.K.	91	91	90	90	90	91
EEC	92	88	91	93	93	92
Germany	93	88	93	95	95	94
Electrical machinery, apparatus and appliances (SITC 72)						
U.S.	100	100	100	100	100	100
U.K.	94	98	103	105	111	108
EEC	89	85	91	93	96	94
Germany	91	86	94	97	100	98
Japan	na	105	89	87	86	90
Transport equipment (SITC 73)						
U.S.	100	100	100	100	100	100
U.K.	93	94	96	91	94	97
EEC	104	99	97	97	98	98
Germany	94	92	91	91	93	92