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PART I

Towards a Firmer Basis of Economic Policy

Those charged with the pursuit of national economic policy are set manifold tasks. They must define the nation's stated and unstated economic objectives and fill in details. Watching the flow of events, they must judge the state of the economy and estimate the direction in which the economy is moving. They must infer what factors are influencing its course. They must decide what can be done and—with due attention to the nation's non-economic objectives—what ought to be done to reinforce the favorable and restrain the unfavorable factors. And they must act, which often requires persuading others to act also and should always include preparations to meet contingencies.

The tasks are not easy. Although progress has been made in reporting on current events, even now their course is revealed only by bits and pieces of information. These vary in scope and precision, and float into view sometimes soon, sometimes long after the event. Although knowledge of the process of economic change is wider than before, it is still far from adequate. We are not always sure which developments and which factors are favorable and which unfavorable, which important and which trivial, which persistent and which temporary. The means at hand to influence the course of events are greatly superior to those available to earlier generations, yet they are still incomplete, difficult to coordinate, and of uncertain power. And as always, sectional interests differ and opinions about the way to reach national objectives also differ, and these must be reconciled or overruled.

Nor are the problems of policy for economic progress easier, or the importance of policy less, because the United States has over the years made real economic gains in every major direction. The nation's goals are not static; they advance. There is no release from responsibility; the nation's responsibilities become wider.

During the past year, economic develop-

NOTE: This report was presented at the annual meeting of the Board of Directors of the National Bureau held in New York City, February 27, 1961. I am indebted to Arthur F. Burns, Geoffrey H. Moore, and Hal B. Lary for helpful suggestions.

ments reminded us of the persistent economic problems that confront the American people—problems of stability, of growth, and of international balance. Also, the discussions of the year, such as they were, made plain how far from sufficient are information and understanding of the problems, and how widely different are opinions on how best to get the economic progress we all want.

To make firmer the basis of economic policy continues to be a job worth doing.

The purpose of the National Bureau is to assist in this high endeavor. By developing and improving economic information, we aim to expand and improve economic intelligence. By analyzing the process and the causes and effects of economic change, we try to deepen understanding of the significance of events and improve judgment of the factors involved. In critically examining new and old proposals, we seek to improve the implements of policy, and also to clarify and show the way to reconciling differing sectional interests and diverse national objectives.

This work we try to do with care and with objectivity. For we wish our findings to command the public acceptance that is essential if they are to be put to use in strengthening the basis of economic policy.

It is the use to which we hope will be put the twenty studies published since the beginning of 1960; the nine studies in or on the way to press; the six manuscripts now or soon to be in the hands of the Board for review; the various studies under way at the beginning of the year, with which the staff and our many friends and collaborators are still busying themselves at this time; and the new studies we are starting or planning for the months ahead.

If, as in the past, we tackle questions of fundamental importance and carry on our work in conformity with our principles, it is a reasonable hope. We may expect that, in more than one way and on more than one occasion, practical use in improving and carrying out policy will be made of tested information on the subjects with which we concern ourselves.

It is the National Bureau's custom each year to make a full accounting of its work and a declaration of its plans, and we must therefore be more specific. In Parts II and III of this Annual Report are listed and described the work we have completed, the work in process, and our plans for the future. Here I shall point to some of the items and suggest how our work may find its uses—in some cases, perhaps, many uses—in dealing more effectively with the persistent economic problems of the American people.

Consider, to begin with, the study of the price statistics of the federal government recently completed by the Price Statistics Review Committee of the National Bureau. The review was made at the request of the Bureau of the Budget. We undertook it because we had a deep interest in the range of problems involved, running back to 1927, when Mills' notable study of the behavior of prices was published, and because we could enlist the services of the group of highly competent economists and statisticians headed by Stigler. After the usual review of the committee's report by the National Bureau's Board of Directors, it was transmitted to the Director of the Budget late last year, along with a dozen substantial papers on particular points prepared at the request of the Stigler Committee. The report was presented a few weeks ago to the Subcommittee on Economic Statistics of the Joint Economic Committee by Dr. Bowman, Assistant Director for Statistical Standards, Bureau of the Budget.[1] Hearings on the report and its recommendations are to be held by the Subcommittee this spring.

The price statistics study is a comprehensive review and analysis of the existing price data and price indexes, with an eye to ways to improve them and ways to discover means of improving them. Attention is paid to a variety of important technical problems—the sources of price data, which may come from buyers as well as from sellers; specifications of the goods priced, which may be tight, as in the case of the Consumer Price Index, or loose, as in the case of the Index of Prices Paid by

[1] U.S. Congress, Joint Economic Committee, *Government Price Statistics, Hearings . . . January 24, 1961.*

Farmers; sampling procedures, which affect the efficiency with which prices are collected and the ease with which the precision of indexes is determined; weights, which may be left untouched for long periods or revised frequently and regularly; and still others.

These technical problems are not merely matters of concern to the technicians. How they are dealt with affects information on which public issues are judged and settled. That this is so may be sufficiently evident from the Bureau of the Budget's request for a review of the price statistics. But let us listen to Dr. Bowman's presentation to the Congress: "The price statistics of the Federal Government make up an important segment of the factual data which constitute our economic and social intelligence. They are widely used not only by the Federal Government, but by all segments of our economy, business, labor, State and local governments, and research organizations." Further, "The importance of these data for economic analysis has gained special recognition in recent years due to the general interest in evaluating the effects and forces of inflationary trends in our economy. Hearings of the Joint Economic Committee on the Relationship of Prices to Economic Stability and Growth for the 85th Congress and on Employment, Growth, and Price Levels for the 86th Congress focused attention on the need for reliable price statistics for the analysis of current economic problems of the highest importance."

Dr. Bowman added that "The Bureau of the Budget is aware that notable improvements and modernizing changes have been made in the price statistics of the Government during the past decade. Nevertheless we believed, in view of the importance of the problems involved, that the time had come for a complete reexamination and reevaluation of the price statistics program of the Federal Government with a view to the development of recommendations for its continued improvement."

Dr. Bowman then stated: "The Bureau of the Budget believes this report from the National Bureau of Economic Research provides the expert guidance sought. Its recommendations deserve and will receive very careful consideration."

I can be more specific about the significance of the matters treated by the Price Committee.

Notice, for example, the stress placed on changes in the quality of commodities and services. If, as the Price Committee feels, improvements in quality have on the whole been large, and these are inadequately reflected in the price statistics, then the trend of prices is seriously misjudged. An experiment in adjusting prices for quality change is described in the staff paper by Griliches, who draws on his work here on agricultural input, output, and productivity. His results may startle those who think of price indexes as exact instruments of public policy. The approach has possibilities, but it is clear from Griliches' brief discussion in Part III, below, that correction of price indexes for quality change is a difficult problem. Whether the degree of overstatement of price rise is greater now than formerly is even more difficult to determine. The possibility is usually glossed over, but the problem of quality changes may be ignored less easily after the emphasis placed on it by the Price Committee. And the same is true of a related question discussed in the price report, namely, the speed and manner of adjustment in the price indexes for the rise of new products and the obsolescence of old. The policy issues involved concern not only the degree of inflation but also, for example, whether the United States is "pricing itself out of world markets."

Equally relevant to the question of inflation, and to other policy questions as well, is an analysis included in the staff paper by McAllister. He shows to what extent the measured rigidity of prices and of price indexes depends on the scope of the data, the number of firms reporting, and the source of the information. For instance, the broadened coverage by the Wholesale Price Index of highly fabricated products, the prices of which usually fluctuate less than the prices of raw and partially fabricated materials, has tended to make the price index more rigid. McAllister estimates the magnitude of this bias and suggests that

it may be quantitatively too important to be neglected when it is asked how strongly prices resist downward pressures. Similarly, McAllister's results on the relation between frequency of price change and number of price reporters raises some doubts about the validity of analyses concerning the relation between price rigidity and competition, and doubts also about the validity of policy conclusions drawn from these analyses.

Still other contemporary policy issues are touched by the price study, of which one example must suffice. As I have already mentioned, the Consumer Price Index is based on specification pricing, and the Index of Prices Paid by Farmers on unit-value pricing—that is, the prices asked for are those of unspecified qualities bought in the greatest volume by farmers. The contrast between the two methods is put very sharply by the difference between their respective reports on the change in automobile prices that took place during 1960. In the CPI, the automobile price component went down by 2 per cent. In the farm index, automobile prices fell by 11 per cent, simply because farmers shifted, or were presumed to have shifted, a substantial part of their purchases from the standard cars to the new and lower-priced compact models. The implications for parity prices and thereby for the distribution of income between farm and urban groups of the population are obvious.

My next illustration is prompted by a paper, now in its final stages of preparation, reporting on the latest in a long series of studies and experiments in the analysis of current business conditions. The object is to determine the sequence of events during cyclical fluctuations in business and thus to learn to judge the course of business and estimate the prospects of a turn—or confirm its appearance—as quickly and accurately as possible.

Which contracyclical steps to take (during expansion as well as contraction), when to take them, and with what vigor—the answers to these troublesome questions hinge on the speed and confidence with which such judgments and estimates can be made. For a quarter of a century, therefore, the National Bureau has been devoting a sizable fraction of its attention and resources to the problem. We have been gathering and developing relevant facts, constructing and trying out various methods of analysis, applying these methods to the facts in a number of ways, and subjecting the results to the critical tests of reason and experience.

Much of what has been learned is now gathered together in the two-volume *Business Cycle Indicators,* edited by Moore, which is included among this year's publications of the National Bureau. The study contains an unusually broad collection of contributions to the analysis of current business conditions. Included are some published before and now out of print, and others made available for the first time. These discuss at length a number of highly relevant subjects: how indicators of recession and revival may be selected; which indicators we have chosen for inclusion in our lists; why particular indicators behave the way they do in relation to general business; how changes in them should be interpreted; what their value has been in foreshadowing or confirming turns in business at large and in estimating the probable severity of a contraction or strength of an expansion already under way; and what qualifications need to be kept in mind when using the indicators. The volumes also include aids to the current use of the indicators and, for the benefit of business analysts and others, give over a hundred significant economic series in their original and seasonally adjusted form, together with sources and descriptions. During the year, we issued a brochure informing the public of the National Bureau's research on indicators of cyclical revivals and recessions and providing a brief preview of some of the contents of the two-volume work now off the press. The brochure has proved to be something of a "best seller."

We were not surprised by the eagerness of business analysts to put our latest results to work. Of course, our results have always been released as soon as we had sufficient confidence in them, and they have been in use for a long time. Indeed, practical use of the work began with the preparation in 1937 and publication in 1938 of the first report on indicators,

made by Mitchell and Burns at the request of the Secretary of the Treasury. It is reproduced in Moore's collection.

Following closely on the heels of the Moore volumes is the paper in preparation, Shiskin's "Reporting Current Business Cycle Developments." It describes a most interesting current application of the work, which we regard also as another opportunity to learn something.

The study had its immediate origin in a request in 1957, made of the Bureau of the Census by the Council of Economic Advisers, "to undertake to develop methods of appraising current business fluctuations in a monthly report that would take advantage of the availability of a great many economic time series, new findings about the relations of economic processes over time, and large-scale electronic computers." This particular experiment has therefore been under way now for over three years. The period spans the 1957-58 contraction, the 1958-60 expansion, and the first months of the contraction that began in 1960. The CEA-Census experiment was carried on with the close cooperation of the National Bureau, and Shiskin's paper describing it was, in fact, prepared as part of the current National Bureau study of the uses of electronic computers in business cycle research. The paper will soon be submitted to the Board and, if approved, published promptly.

Readers of the paper will find, after a discussion of the need for "an early-warning system" of economic intelligence of recession and recovery, the basis of such a system in the accumulated findings of business cycle research and the recent development of electronic computers and programs. There follows a description of the report on current business cycle developments that the Bureau of the Census has been making monthly to the Council of Economic Advisers, and a record of "performance" during the three years covered. In the final section, note is made of the hazards of current interpretation, and ways to improve the report are suggested.

The hazards are indeed great, but even a speedy recognition of a cyclical turn that has already taken place is valuable. To illustrate, as late as December of last year many people were still questioning whether the country was experiencing a recession. Anyone following Moore's indicators, as Shiskin did, would have seen the following: When the steel strike ended in November 1959, only a quarter of the leading series—those series usually leading turns in general business, such as orders for machinery—had regained their previous highs, and none did so after December. In February 1960, when the aftermath of the strike had been largely dissipated, half of the coincident series—such as industrial production—were currently at the highest levels reached during the expansion. By the end of June, this was true only of a fifth of the coincident series. By October, almost all the coincident series were below their peaks, and half the lagging series—such as inventories—also were down. The October data made it clear, also, that the leading and the coincident series were generally still falling. This information was available before the end of November.

As tests at the National Bureau and elsewhere indicate, the difficulties in judging the course of business from the information provided by the indicators are especially great when events like Korea and major strikes intervene. In any case, however, no procedure can be perfect; none can eliminate the area of uncertainty that inevitably surrounds the current analysis and forecasting that each of us must do.

Yet the more reason and experience are brought to bear on the problem, the greater are the chances of success in interpreting the current flow of events. This is the justification of our continuing work on the cyclical behavior of various significant economic processes. Can there be serious doubt of the value for stabilization policy of systematic knowledge on fluctuations in federal receipts, expenditures, and budget balances, or in labor productivity and labor cost, or in exports—subjects of studies in this year's list of publications? Or of analyses of postwar cycles in inventories; or of cycles in prices, costs, and profit margins; or of the history of money—subjects of studies soon to go to the Board for approval? Or of studies of money demand and money supply, of consumer plans and

business anticipations, and of cycles in the labor market, in materials purchasing, in orders and production, and in credit quality—subjects of investigations not quite so far along, and therefore counted in our inventory of work in process?

Preliminary results from some of these studies have already been of assistance in developing and improving the indicators of recession and revival. The other studies, too, may help to reduce still further the uncertainty that surrounds efforts to probe the future. However, practical interest in the studies extends beyond their possible use in the difficult business of forecasting. We may hope that they will also help to improve understanding of the connections among events, and thus of the factors operating in the economy.

To illustrate the kinds of ideas with which the staff is wrestling I shall draw on some of these studies in process. Until more of the evidence is in, and we are surer of their validity and significance, these ideas must be viewed only as interesting hypotheses on which work is needed and is proceeding.

In the paper on cycles in labor cost that we published during the year, Hultgren demonstrated how the general state of business influences the level and direction of movement of unit labor costs. In the paper on which he is now working, he has pushed his analysis another step forward, to deal with cyclical fluctuations in total costs, in selling prices, and in profit margins. In his and related results may be seen the outlines of a significant sequence, during the round of business contraction and expansion, which affect the crucial decisions to invest.

More specifically, the relation between unit costs and prices is altered during business contraction in such a way as to cause profit margins to turn up early. Usually the rate of rise in the margins is sufficient, compared with the rate of fall in the volume of sales, to cause aggregate profits to begin to move up. These developments act powerfully to improve profit prospects. Investment decisions are influenced by other factors as well, including the costs of equipment and plant facilities and the costs of finance. But it is usual for these costs also to become more favorable during contraction. In combination with improved profit prospects—and the flow of cash into corporate treasuries, as Payne's recently published paper demonstrates—they serve to stimulate a revival in orders for equipment and construction contracts, although sales, production, and employment are usually still falling. That is, commitments to invest in plant and equipment—some of which may have been postponed to await more favorable conditions—are made more readily and on a larger scale. In turn, the revival in commitments to spend money on investment goods helps to bring the business contraction to a close.

Something like this sequence of events during contraction and revival seems also to characterize expansion and recession. As we know from the work of Mitchell and his collaborators, investment commitments do in fact usually lead turns in business: statistics on them constitute several of Moore's leading series. Thus, if the new results stand up to closer inspection, they will fill a significant page in the account of business cycles and their causes to which our work is designed to contribute. They may also provide the background required to judge when particular determinants of costs, profits, and investment take on an autonomous role—a matter of vital concern in stabilization policy.

From Friedman's work with the demand for money, on which several preliminary reports have appeared and which has grown to the two substantial volumes in preparation, I shall pick two points for mention. One bears especially on events since 1959; the other, on a trend over the whole postwar period.

The first point relates to Friedman's studies of the relations between business conditions and the rate of change of money supply. It matters somewhat how money supply is defined—whether inclusive of time deposits in commercial banks as well as demand deposits and currency in circulation, as is done by Friedman; or only of demand deposits and currency, as is more conventional. Also, the rate-of-change series (corresponding to either concept of money supply) is highly irregular, and its direction and turns therefore not easy

to determine precisely. Indeed, there is a question whether its peaks and troughs can be determined with sufficient precision to warrant reasonably specific dating, except occasionally. Nevertheless, as economists are once again learning, the behavior of the money supply may hardly be neglected by those interested in stabilization policy. For this reason Shiskin includes a rate-of-change series in the section of his monthly report which contains irregular but significant series, and the *Federal Reserve Bulletin* and other publications show a money-supply series.

A reading of these series reveals that during 1958-60 the rate of increase in money supply reached its peak within six months after the business cycle trough in April 1958, and then fell so rapidly—in this case, the direction of movement is quite clear—that in little more than a year money supply was declining absolutely. The absolute decline persisted throughout the year from about mid-1959 to mid-1960.

Three important questions are raised when this behavior is viewed against the background of other events of the period and of the ideas and facts that are being explored by Friedman. One question relates to the causes of this behavior of money supply, another to its consequences, and a third to a difficulty it may suggest for policy. First, then, the decline in money supply was unusual for a period of business expansion, as Friedman's extensive collection of data indicates. To what extent was the decline a reflection of policy to combat domestic inflation, coupled with an effort to reduce the deficit in the balance of payments, and to what extent a result of other causes—for example, difficulties raised by the steel strike in judging the course of business developments? Second, the peak in the rate of increase in money supply was followed by the recession of 1960. Was this another case fitting Friedman's tentative generalization that the peak in the rate of increase in money supply has invariably preceded the business cycle peak; or, apart from the generalization, was the sequence in 1958-60 a case of cause and effect or merely a coincidence? And what consequences might be expected to flow from the extraordinary rise in the rate of change in money supply that came so early in the business contraction of 1960? Third, and by way of corollary to the preceding, were developments during 1958-60 in money supply and in business conditions illustrative of a dilemma that is posed by competing objectives—in this instance, of growth and low unemployment on the one hand and reasonably stable price levels on the other? These are serious and highly controversial questions that need to be most carefully and objectively weighed. We may hope that Friedman's and other of our studies will bring to the controversy some of the scientific evidence that is so badly needed.

The other point, discussed in Friedman's progress report in Part III, refers to the postwar rise in the income velocity of money, that is, in the ratio of net national product to money supply. The rise runs counter to the long-term trend over the preceding three-quarters of a century, and therefore poses an interesting puzzle. Friedman proposes to explain the interruption by the hypothesis of growing confidence on the part of the public in the stability of the economy. As he mentions, there may be additional factors involved, and Selden is pursuing some of them on another tack.

Friedman is testing his hypothesis in a variety of ways. If he finds it possible to formulate the hypothesis in a way consistent with the available facts—his luck has been mixed, so far—it will still be necessary to consider the hypothesis as tentative, at least until it is found to fit significant facts that lie outside the range of data used in formulating it. If he should confirm the hypothesis, he will have made significant progress towards improving knowledge of the factors that determine expectations, and also towards developing an objective measure of expectations. Expectations are surely a major key to economic behavior, and not only with respect to the demand for money. As Burns demonstrated some years ago in his paper on the "Instability of Consumer Spending" and Friedman developed in his own way in *A Theory of the Consumption Function,* expectations greatly influence the decisions of con-

sumers on how to divide their income between consumption and saving. And business cycle studies lead us to believe—I referred to the point earlier—that expectations enter also into the explanation of the behavior of investors. Also important, if he is successful, Friedman will have added to the objective evidence that is accumulating of a most significant shift in the climate of public opinion—a shift towards more confidence in business stability—that has great implications for the future of the economy as well as for stabilization policy.

The decline in money supply that began in the middle of 1959 was of course not the only outstanding "special" development of recent years. There has been much talk of the sharp rise in the outflow of gold and in the rate of acquisition by foreigners of liquid dollar assets, which I have mentioned, and the steel strike is still remembered. Attention has been called also to the unusually rapid shift in the federal balance of cash receipts and expenditures, from a substantial deficit position around the beginning of 1959 to a surplus position a year later. Whether, or to what extent, some of these other developments played a role—direct or indirect—in cutting off the expansion last spring is, like the question about the money supply, a scientific problem that will interest economists long after public discussion of the events has died down. While it is not always known how or to what extent particular special factors influence the course of the cycle, it is clear that special factors do play a part in every cycle, serving in some part to prolong or shorten, accelerate or retard, expansions and contractions.

It is becoming clear, also, that short-lived episodic factors have not, in the past, wholly accounted for differences among cycles. Evidence has been accumulating that secular and structural changes play a significant part. Friedman's study of money deals with one aspect of the question. Broader is the study by Burns and Moore of the postwar cycle. They are analyzing the characteristics of cycles since World War II and considering how they differ from prewar cycles. Their object is to determine with what degree of confidence, and on what assumptions, we may expect that the economy will continue to be free of serious economic depressions—a matter of the greatest interest to everybody.

Abramovitz is pursuing the problem of differences among cycles in another way. He is analyzing the so-called long swings that appear to have characterized the rate of American economic growth in the past. His study will interest persons concerned with economic policy even though he limits himself to the scientific questions involved. For those involved in policy must keep in mind the possibility of long-term cumulative tendencies towards severe contractions—or, as it may appear in the economy of today, the possibility of cumulative tendencies towards reductions in the rate of economic growth.

Because long waves seem to run between fifteen and twenty-five years in length, adequate statistical data covering a period long enough to include more than five or six observations are difficult to obtain. Another great difficulty arises because the nature of the economy can hardly have remained unchanged over a period long enough to include even as few as two or three long waves, as Abramovitz takes pains to acknowledge. His results will therefore not be as free of speculation as he would wish, even if he should be able to add to his observations by examining the records of other countries. In any case, the study is not yet at an end, and in a paper recently completed Abramovitz puts the current position of knowledge of the long swings in a tentative way. He feels that at the present time he may be justified in contending no more than this: the long swings that have been observed in the economic activity of the United States during the nineteenth and twentieth centuries have been "quasi-self-generating, in the limited, but important, senses that (1) they have involved certain forces which operated cumulatively for a time to strengthen a surge of development following a protracted period of relatively high unemployment and so to prevent the early recurrence of another such period; (2) they have generated checks and obstacles which, after a time, generally longer than a single business cycle, rendered the economy more

vulnerable to another period of depression or stagnation."

Abramovitz emphasizes that "to say this much is, of course, not to say that episodic factors were not important, nor is it to say that the cumulative and self-reversing tendencies, such as they are, have remained generally constant." Indeed, he gives examples of episodic factors that were clearly important in determining the course of the successive surges of development: financial panics, accumulation of demands deferred during wars or depressions, monetary policy, gold discoveries, and other factors that have influenced the supply of money, and economic events abroad.

He notes also how the postulated systematic mechanism must have been altered during the period in which the swings can be observed because of change in the relative importance of different fields of investment, the decline in immigration, the new role of government, and still other factors.

With these qualifications, Abramovitz' hypothesis is not necessarily inconsistent with Friedman's, which emphasizes the "episodic factors" that influence money supply; or with Burns' and Moore's, which lays stress on the broadened role of government and other structural and secular changes in economic life. Work remains to be done to sharpen Abramovitz' hypothesis and see how well it helps to explain the past, and also to consider to what extent it might have application to the present. He feels that it may not be entirely irrelevant: "These changes and many others will, no doubt, have altered the relations between economic development and intensity of resource use. . . . However, this problem . . . is the same problem as that of maintaining balance between capacity and effective demand. . . . Historical and analytical studies of these swings . . . should, therefore, enrich and test the models designed to help us understand the problem of maintaining balance in growth as this old problem is faced both by advanced and backward countries in a modern setting."

An example of a force tending to check the extension of a period of mild cycles, of the kind Abramovitz has in mind, may be the deterioration of credit quality that seems to develop during a prolonged period of moderate fluctuation. Some of the facts that Earley and his collaborators are unearthing in their study of recent changes in the quality of credit, on which a report is given in Part III, may appear disquieting in the light of earlier work on the question at the National Bureau as well as of the hypothesis of long waves. However, there are subtle problems in interpreting information on credit quality, as Earley is well aware, and for this and other reasons it is too soon to say just how his data fit in with the theory of long waves. This question, also, invites further careful study.

To the discussions of economic instability and of the factors causing it, discussions that are going on and will go on in the future, interesting ideas will be brought by Abramovitz' work. We may hope—in research no one can be sure—that the study will at least help clarify the nature of the problem and suggest ways to learn more of the forces at work.

His study is also, of course, a section of the work we are doing on the great problem of economic growth, a problem to which I now turn.

There is little doubt in the minds of many people that in comparison with the growth of some other countries, especially in the postwar period, or in relation to expanding wants here and abroad, the rate of economic growth of the United States has not been satisfactory. But opinions differ on this, as on other matters. Since the issue is, and always has been, of paramount importance, it is highly desirable to develop and set forth the facts on the rate of economic growth as clearly and accurately as possible. Some of the Bureau's attention has been given to the question, particularly of the growth of this country. Indeed, the Bureau's first study dealt with the rise of national income in the United States during the decade 1909-19. And now in press is Kuznets' *Capital in the American Economy,* which contains a large body of carefully assembled information on the nation's growth over most of the past century. His new book is a capstone not only to the capital formation and financing project, of

which it is the concluding volume, but also to much of the work he and his associates have been doing at the National Bureau for thirty years.

Work on such important and difficult questions as the rate of economic growth is never finished, however. As more is learned about the technical problems involved, as new and improved information is developed—partly in response to better knowledge of what is needed—and as the character and content of economic life change so that formerly minor deficiencies of the indexes stop being negligible, it becomes necessary and desirable to improve and extend the measurements of economic growth. There is a further factor. When men look for signs of retardation or acceleration of growth, or make close comparisons with other countries, they are dealing with problems that demand more precise data. If we are to avoid being misled by the available figures, therefore, more work is needed here too.

I have already referred to the difficulty that quality change poses for the measurement of price increase. The difficulty applies equally to the measurement of the nation's economic growth. If improvements in quality have indeed been large on net balance, then the rate of economic growth is significantly understated. For rates of increase in real national product and in real national wealth, in real wages and in productivity, are obtained by deflating money values by indexes of prices.

Also relevant, to take another example, is the treatment of consumer durable goods. The value of these goods is included in current measures of national product when they are purchased, but the services rendered by them are not included at all, nor is the "do-it-yourself" work that is often involved in putting them to use. According to Goldsmith's study of national wealth, another item in this year's list of finished work, the net value of consumer durable goods held by the American people (expressed in 1947-49 prices) rose from 7.3 per cent of the nation's real wealth in 1945 to 12.8 per cent in 1958. Correspondingly, we may presume, the services rendered by these forms of capital also went up, and so too did the amount of home labor devoted to utilizing them. If national production inclusive of these has been growing more rapidly in relation to measured production in the United States than in other countries, and in recent than in earlier times, their neglect affects comparisons with other countries and other days. How much of an error may result is a further question that deserves attention.

The problem of growth can hardly be mentioned without bringing quickly to mind questions relating to the volume and composition of capital, its mobility, investment and savings, and the financial institutions and markets that enable savers and investors to meet most efficiently. We may expect that government officials, businessmen, labor economists, and others in this country will be as interested in the results of our studies of these questions as the visitors to the National Bureau who have come from other parts of the earth to discuss our work.

Note has already been made of Kuznets' volume, which is accompanied in this year's list of publications by three other volumes in the series of studies of trends in capital formation and financing, but a further word about it is desirable.

People have become so familiar with terms like "capital formation," "gross national product," and "long-term trend," that they often forget that the concepts referred to should not be taken for granted. Kuznets, who has struggled with these difficult ideas for many years, and whose terminology has become standard usage, is well aware of the need to be clear about them. At the outset, therefore, his book takes up the questions involved and presents a thorough discussion—reflecting unparalleled personal experience—of the difficult concepts of capital formation and related economic quantities, and of the meaning of long-term trends. As I have said, the substance of the work is an orderly presentation and analysis of a vast body of quantitative information on trends in the United States since the 1870's. Information is provided not only on the aggregates of national product and capital formation and financing but also on their composition and structure—for ex-

ample, capital formation is classified by type of asset, class of user, and industry. Further, the trends in capital formation and financing are studied in relation to corresponding trends in output, population, and labor force, in capital movements across the borders of the United States, in security prices and yields, and in the relative importance of financial intermediaries.

It is impossible to summarize the many findings on the rate, character, and causes of the economic growth of the United States, but mention may be made of a few cardinal points that have significant implications. First, over the period since the 1870's, gross capital formation has accounted for a fairly constant proportion—somewhat over a fifth—of gross national product. In other words, gross capital formation grew at about the same rate as gross national product—about 35 per cent per decade on the average. Second, capital consumption—mainly depreciation and obsolescence—rose more rapidly than gross capital formation, so that net capital formation—the difference between gross capital formation and capital consumption—declined significantly in relation to gross or net national product. In terms of current prices, the decline was from around 13 per cent in the earlier years to somewhat less than 9 per cent in recent years. Third, the ratio of net capital stock to net national product, both in constant prices, rose from the 1870's to the 1920's, and then declined. The current "capital-output ratio" is lower than it was eighty or ninety years ago.

The presentation of the findings is topped off with a discussion of the problem and value of projecting growth rates into the future that no policy maker who indulges in this kind of calculation will want to miss. The tables with which the text is studded are supplemented by lengthy statistical appendixes which have already found many uses before publication and may be expected to be used for years to come.

As Kuznets states in his concluding remarks, his inquiry "has no true beginning and no sharp end. It has no beginning because it is rooted in a variety of studies in the past. It has no clear end because it is only in its absorption in the next phase of the work in the field that the study can make an effective contribution."

This is as true of the other National Bureau studies dealing with growth—for example, Kendrick's study of *Productivity Trends in the United States,* also in press. Kendrick develops a measure of productivity that compares output with the input of capital as well as labor. It could not have been done had it not been for the work on capital already under way, as well as for the earlier National Bureau work on production, employment, and productivity. Kendrick's book, like Kuznets', contains a valuable body of data. In it is presented the full evidence underlying two major findings that have already attracted a great deal of attention. One concerns the fact that increase in the volume of labor and capital combined accounts for but a portion of the increase in national product over the decades. The rest of the increase must be explained by technical change, often embodied in quality improvements of capital goods, by investment in education, and by other factors, and some of our studies are looking into these. The other finding concerns an important shift in the trend of productivity in the United States. Productivity rose significantly more rapidly during the four decades after World War I than during the earlier decades. Whether this tilt in the trend reflects an acceleration that can be assumed to continue into the future is another difficult question that can be answered, if at all, only by further investigation.

The studies that I have mentioned so far deal with basic scientific questions concerning the rate, character, process, and causes of growth. These are facts that must be determined if policy for growth is to rest on the solid foundation of tested knowledge. A study that we planned some time ago, and are now able to undertake, will deal more directly with policy. This is the study of tax policies for economic growth to which Ture will turn his hand shortly.

That tax rates influence the allocation of capital to different regions seems to be illustrated by one factor in the problem of our international balance of payments. This is the

outflow of some capital from the United States in response to savings made possible by the so-called "tax havens." But tax rates affect also the distribution of capital among different fields of enterprise, and there is reason to suspect that they affect the aggregate of capital available for investment as well. However, it is not known how strongly taxation influences incentives to invest and to do the other things that determine the rate of economic growth. To learn as much as we can of the relation between taxation and economic growth is the objective of the new study.

The fact that the study is more sharply "policy-oriented" does not mean that its end product will be policy proposals. Rather, its purpose is to subject various policy proposals to critical review, and to set forth objectively the facts that must be weighed in choosing among them.

Any review of so complex a problem, it is clear, could not at any time or under any auspices be conclusive in all respects. Yet a serious effort, which could be widely accepted as authoritative, would mark a major forward step in public understanding of the tax system, provide a better basis than we now have for effective policy, and thus contribute to stimulating economic growth.

Before I leave the subject of growth, I should refer to the very broad collection of National Bureau statistics on the development of the United States that became available during the year in the new and massive volume of *Historical Statistics* prepared by the Bureau of the Census with the cooperation of the Social Science Research Council. Most of the 8,000 series in the volume are from government sources, but apart from the federal government the National Bureau was by far the largest contributor to the volume. Over 800 series, or 10 per cent, are taken wholly or in part from National Bureau work, including data from thirty different National Bureau books either published or in press and from five unpublished documents. Because many sections of *Historical Statistics* relate to areas outside the Bureau's province, it is clear that much more than 10 per cent of the economic series are from the National Bureau. It may be mentioned, also, that many other references to Bureau data appear in the bibliographical notes attached to the volume. The new *Historical Statistics* volume is a broad channel through which our work will get to its users.

Another new study started late in 1960 deals with the problems of the United States in a changing world economy. I have already mentioned the concern felt over the United States when the general public became aware of the balance of payments deficit and learned something of its implications. The problem is especially interesting and important because it involves more than the usual number of major national objectives, short run and long run, noneconomic as well as economic. These objectives are somewhat competitive. The problem is also especially difficult, because it involves developments abroad as well as in the United States, and results from a mixture of episodic and systematic factors, including among the latter cyclical, secular, and structural changes which must somehow be disentangled. A great deal of attention is already being paid to the problem in various quarters. In many cases, however, the work must be hurried and little more can be done than to apply such knowledge as already exists. The first job tackled by Lary, who joined us last fall to take charge of our new study, is preparation of a review of the major developments and of a plan of research that, given the time and care the problems deserve, may be expected to add to knowledge as well as exploit it.

It is possible that before the results of some of our studies begin to appear, a deficit in the balance of payments may not be the immediate problem that it is today. But whatever the current situation a year or two or three hence, it will be useful to know more than is now known of the character and relative importance of the episodic and systematic factors that influence our exports and imports of commodities and services and the ebb and flow of capital and credit across our borders.

In reporting on the year's work and plans of the National Bureau, I have singled out for mention a few of the studies that bear on the problem of inflation, the problem of business fluctuations, the problem of economic growth,

or the problem of international financial balance—all major problems of our day. Of course, the problems are intertwined in more than one way, as has been apparent; and it is equally apparent that if the studies should shed any light at all on our economy, they will not fail to illuminate more than one problem.

The new study of interest rates, which we are just getting under way, is broader than most. It bears on capital and growth, in a variety of ways, and also on questions of stability, inflation, and international balance. The study should provide information useful in dealing with the problems that arise in determining the effectiveness of monetary policy, in choosing among alternative ways to manage the federal debt, and in clarifying the controversy that has already developed on policies recently proposed to keep short-term interest rates high for purposes of international balance and push long-term rates down in order to stimulate recovery and growth.

As Conard and Brown state in their report, below, the study will build on earlier National Bureau studies of bond prices and yields, of cyclical fluctuations in the financial sphere, of capital formation and financing, of the postwar capital markets, and of the studies under way of consumer finance, of risks and returns in small business financing, and of credit quality. Despite the advantages it derives from the earlier work here and elsewhere, the new study of interest rates will have some data gaps to fill. Conard and Brown feel the need, for example, for better information on mortgages, which have bulked very large in the long-term financing of the postwar period. Information is required not only on interest rates when funds are transferred, to point to one item, but also on rates when commitments are made. The dates often differ greatly. The investigators in charge hope to give attention not only to gathering and analyzing historical data but also to developing a current reporting system that will make future information more complete and accurate and available more regularly and promptly.

The major part of the new study will be devoted, however, to analyzing the information that is at hand or that can readily be obtained. Particular attention will be paid to the behavior of interest rates and related yields in relation to fluctuations in general business conditions, and to the changes that seem to have taken place in the relationship. For example, the National Bureau's business cycle studies demonstrate that bond yields have frequently lagged at both peaks and troughs in general business, but the length of lag has varied from one cycle to another and in recent years seems to have disappeared altogether. In contrast, open-market short-term rates have as a rule tended in earlier years as well as currently to turn rather promptly with general business conditions, while the lag in the bank rate charged customers has been consistent enough for Moore to include it among his lagging series. The nature, causes, and effects of the variation in these and other interest rates will be a subject of major concern in the new study. So, too, will the changing relations among different rates and yields that are indicated by the preceding remarks. An intensive study will be made, for the recent period when the information is more readily available, of such rates classified by risk, term to maturity, age of issue, type of asset, and in other ways. As our plan for the study provides, the work mentioned is to be done in so far as it is feasible and contributes to the major objective, which is to explore the causes, consequences, and implications for the future of the recent and longer-term developments in interest rates and related financial factors.

As is appropriate, I have been focusing attention very largely on this year's studies and plans. Yet here, as in the more detailed statements in Parts II and III, what we have to say about our current work also says much about the work of earlier years. For the National Bureau is a continuing enterprise. Each year's report constitutes an observation, at the time, on the enterprise as a whole. By this I do not mean merely that virtually all studies completed this year were begun in earlier years, or that the bulk of this year's work is on studies included in last year's and still earlier lists. More important, what we were able to do during the past year reflects the results and the

experience of all the work carried on since the very inception of the National Bureau.

The studies of each year support and strengthen one another. Note has been made of current business cycle studies that contribute to the work on economic indicators, and of research on which the new interest rate study will build. Both lists could be expanded. And when pointing to the other studies, also, reference could have been made to supporting work at the National Bureau. For example, the investigation of export and import prices, the study of net worth changes in relation to price changes, and the newly published studies of long-term trends in wages and the cost of living deal with price change. Useful information for the new tax policies study already exists, among other places, in the several sections of the personal tax study that have gotten into print, including the book on personal deductions in this year's list, and the studies on the way to press, which deal with exemptions, dividends, and entrepreneurial income. And the current studies of the structure of world trade and payments and of trends and cycles in United States imports and exports, the recently published work on international financial relations and business cycles, and the new work on interest rates and taxation should be helpful in the research we are planning on the problems of the United States in a changing world economy.

Further, as some of these examples indicate, the studies of each year also build on the National Bureau's work of past years. Our results cumulate.

Of course, the National Bureau does not work in isolation. We make an effort to profit from the past and current work of others engaged in economic research, and we know that our past and current work finds its uses in theirs. The regular conferences that we help to organize through the Universities-National Bureau Committee for Economic Research and the Conference on Research in Income and Wealth have as their purpose the exchange of ideas and the stimulation of research. This is the purpose also of the conferences in which we cooperate on occasion with others—the Social Science Research Council a year or two ago, and the Merrill Center for Economics this coming June—and the conferences we set up entirely on our own. We count the time, energy, and funds we give to the conferences and to the preparation and publication of their proceedings a profitable investment that yields returns to all scholars.

As I have said, most of the work brought to completion this year was begun many months ago. This fact is not one in which we take pride. Our objective is useful knowledge. Time is one of the costs we have to incur to reach it. It takes time to meet scientific standards. If we had larger resources which we could spend freely, we might be able to reduce the time. It could be worth doing this even if the reduction in time were not proportionate to the increased rate of expenditure, as would very likely be the case. But there is a limit on the time that can be saved if good work is to be done. Especially in the basic research on which we concentrate, there is often much that might be lost by hurry.

In one sense, this means that basic economic research must always lag behind the problems in which it finds its uses. It is better, however, to regard this in another way: it means that we must plan ahead. We must deal with the persistent problems and avoid the ephemeral. Since no one can foresee the exact shape that tomorrow's problems will take, we must focus on fundamental questions, the answers to which will find their uses in many different ways.

Solid research takes time. It takes money. But it takes still more, and it is well to remind ourselves and our supporters of this. Basic research is an uncertain enterprise. Nobody can be sure how his questions and his hypotheses will change in form and content as they come under close scrutiny and facts bearing on them are accumulated. Simple questions turn out to be complicated as often as complicated questions prove to be simple. Important questions are sometimes found to be trivial, but seemingly minor questions sometimes become serious stumbling blocks to the solutions sought. "More funds for basic research are indeed required," said Warren Weaver a few years ago. "But of even greater importance

is the way in which basic research funds are made available—the flexibility, the stability, the freedom from intellectually dishonest commitment, with which competent scholarship should be supported."[2] His wise remark applies not less to basic research in economics than to basic research in the other sciences. Indeed, it applies with even greater force to economics, in which all problems seem urgent and everyone has his opinions on how to go about solving them.

I return to the importance of economic policy and of the research devoted to putting it on a firmer foundation.

The rate and character of economic progress are surely influenced by economic policy. Can there still be a question whether it matters how money and government debt are managed? So, too, taxes matter. The level and direction and flexibility of government spending matter. The state of competition matters. It matters how the international accounts are balanced. Our economic progress, and in the end the economic progress of others—and much more than the economic progress—depend on how well the economic policy of the United States is formulated and how effectively it is carried out. In turn, this depends in some part on knowledge of how the economy operates. Economic research can contribute to economic progress.

SOLOMON FABRICANT
Director of Research

[2] Dael Wolfe (ed.), *Symposium on Basic Research,* American Association for the Advancement of Science, Publication No. 56, Washington, 1959, p. xiv.