PART I

Contributions to Economic Knowledge Through Research

One of the principal purposes of our Annual Meeting is to review the record of the past and draw lessons therefrom to guide the National Bureau's future program. We examine reports by the staff on their recent research, and consider plans for new projects. We study last year's financial receipts and expenditures and probe this year's budget. Our objective is to set a course that will, if we are wise as well as fortunate, improve upon the past record of achievement.

There are some advantages in taking a long view of the past, and some in considering only recent developments. The long view tells us better where we have been, the short where we are heading. Accordingly, I should like to set forth briefly what, in my opinion, are the National Bureau's chief achievements over the nearly fifty years of its existence. Next, I shall review some recent accomplishments along these same lines. Finally, I shall offer some thoughts on the potential value of the new research projects under way or proposed.

There are three achievements in the lifetime of the National Bureau that, in their scope and significance, deserve to be singled out. The first is that the Bureau has constructed and developed for practical use many of the basic measurements of our economy's performance, measurements that have become the stock in trade of economists, government officials, businessmen, trade unionists, journalists, and the general public. National income and gross national product, gross and net capital formation, productivity of labor and capital, flow of funds, business cycle turning points, and leading indicators are concepts put into concrete form in pioneering National Bureau studies. All are now widely used to discern the state of our nation's economy and its prospects, and to guide its

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policies. Many are being used or developed in other countries, near and far.

The second major achievement is the example the Bureau's studies have set for scientific work in economics. In part, this is attributable to the insistence, constantly reaffirmed, that our studies should have quantitative, empirical content; that they should set forth dependable findings; and that the results should be divorced from policy recommendations, though not, to be sure, from policy evaluation. In part, it is due to the procedures set up to review a study before it is published —a review that encompasses careful reading and consultation by members of the staff, by advisory committees of experts drawn from outside the Bureau, and by a Board of Directors that consists of men who strive to maintain a scientific frame of mind no matter how divergent their positions on economic, political, and social questions.

Those who review our reports after they are published have come to expect a high standard of scholarship. Their reviews not infrequently contain such phrases as "in the best traditions of the National Bureau of Economic Research," or "that massive and strictly objective scholarship for which the National Bureau of Economic Research has become famous," or "conducted in accordance with the usual high professional standards of the National Bureau." A recent report by the Ford Foundation stated that "A Bureau report is usually a stellar event on the stage of economic scholarship." In a subject as susceptible as economics is to the sway of opinion, self-interest, fads, speculative theorizing, and casual empiricism, it is clearly of the highest importance to have an institution that has built and preserved a reputation for steadfast adherence to fact-finding and objective analysis.

The third achievement of the Bureau is to have served as the medium whereby the research work of many of the nation's outstanding economists has been brought to fruition. By financing their studies, by furnishing statistical, clerical, and other services, by ensuring that the stimulating criticism of colleagues would be available at every stage of their work, by practicing the patience needed to obtain sound and tested results, and yet encouraging men on to reach the finish line, the Bureau has greatly enhanced the productivity of those who have enjoyed its support. It has provided both a training ground for men and a testing laboratory for ideas. As a result, the work of many individuals has been raised to a level of quality and permanent value that would not likely have been achieved without the Bureau's support and discipline. This is, of course, a most difficult matter to judge, but I would regard the Bureau's contribution to the nation's "human capital" of economists as one of its outstanding accomplishments.

We can take satisfaction, of course, in the past achievements of our institution. What is far more important, however, is whether we are continuing to make substantial achievements and whether we are laying plans calculated to ensure large achievement in the future. This calls for a look at the recent record. Can we point to achievements of recent date along the three lines I have mentioned: the provision of basic economic measurements, the maintenance of high scientific standards, and the development of human capital?

Surely Goldsmith's, Kendrick's, and Lipsey's recent contributions to the measurement of national wealth and balance sheets are basic in the same sense as Kuznets' and Fabricant's earlier studies of national income and capital formation. Other promising efforts are Fuchs's work on measuring productivity in the service industries, Becker's on the rate of return to investment in education, Mack's on the volume of goods that buyers have in inventory and on order, Hultgren's on unit costs of production, Shay's on the price consumers pay for credit, Guttentag's and Cohan's on long-term interest rates, and the attempt of Earley and his colleagues to capture that most elusive quality, the quality of credit. Some, if not all, of these recent contributions to economic measurement have yet to demonstrate that they can survive the

severe test of the market place. But it is clear that we have not been neglecting basic economic measurements.

The National Bureau is, I am confident, continuing to apply the same high scientific standards to our research findings as in the past. There is certainly no suggestion in the reviews of our publications in scholarly journals during the past few years that we have countenanced any retrogression in this respect. Indeed, the often-expressed desire of economists and officials in other countries to emulate the National Bureau, either as an institution or in terms of some particular study it has conducted, is concrete evidence of this faith. Recently, for example, the Economic Council of Canada, in its Annual Review entitled *Prices, Productivity and Employment* (November 1966), not only made explicit use of much of the National Bureau's recent work in this field but also proposed some ten basic studies of the Canadian economy, of which at least five are closely patterned after those made by the Bureau for the United States. It also recommended that a research institution similar to the National Bureau be established in Canada. This is simply one bit of evidence of what I take to be a general consensus among economists, that the way to build a science of economics is to organize research so that the highest scientific standards are strictly applied.

Finally, the Bureau is not only continuing to support the work of many economists of established reputation but it has attracted to its regular staff several younger men of great promise. We have also continued, and wish to extend, the program of research fellowship appointments that the Sloan Foundation has supported. With the aid of a recent grant by the Carnegie Corporation we are appointing fellows who wish to concentrate on the economics of education under Becker. The author of *Human Capital* is practicing what he preached. We hope to do the same in the economics of health under Fuchs, as well as in other fields.

Moreover, through its conference program, which is now supported by a grant from the National Science Foundation, the National Bureau is helping to stimulate and to strengthen the research efforts of many economists at universities throughout the country and indeed in other countries. Representatives of thirty-five universities (three universities were added last year) as well as other economists from research institutions and government agencies participate in the conference program of the Universities-National Bureau Committee for Economic Research. More than a hundred economists are members of the Conference on Research in Income and Wealth. Each group plans one or two conferences a year. These conferences are carefully prepared long in advance, encourage much new work, test new ideas and younger men. The interchange before, during, and after the conference helps to produce a high-quality research product in the form of a proceedings volume. But the contribution of these research conferences to the intellectual capital of the participants is one of their chief values.

So much for the recent past. What now of the future? Here we must take account of the fact that most National Bureau studies are of a long-run nature, taking three to five years to mature, sometimes longer. Hence our output over the next few years is to some degree already determined by the work that is now in its early stages. Moreover, if we continue to follow the principle honored by Mitchell, Burns, and Fabricant in directing the Bureau's research—namely, that new projects should not be selected on an *ad hoc* basis but rather should be part of a continuing, integrated program—this also will, to a degree, determine the character of our future output. This guiding principle does not, of course, fix an absolute boundary around our research at any one time, and the farther we look ahead the less force it has.

Will the studies we have recently begun contribute significantly to our society's needs for basic economic measurements? In considering this question we must judge whether a study will have consequences far beyond

what it may directly contribute. Is it a building block that is essential for some further development? Will it induce others to continue and extend the measurements? Will it be widely used in economic analysis and policy-making? We can only conjecture. But let me list some possibilities.

1. Stigler's and Kindahl's efforts to secure actual transaction prices on industrial commodities from buyers as well as sellers may lead to a basic change both in the techniques of securing price information and in the accuracy of our wholesale price indexes.

2. Kravis' and Lipsey's experiments in securing, from both buyers and sellers, comparable prices among countries and over time for manufactured products traded in international markets may spark the development of continuing indexes of the competitive position of the United States vis-à-vis other countries.

3. Juster's hypothesis that consumers can provide useful information on the probability that they will buy automobiles, houses, and other durable goods may result in a new body of data on consumer spending and saving—valuable both for detecting current trends and for analyzing consumer behavior.

4. Fuchs's plan to study prices, output, manpower, capital, and productivity in the health services industry may, in time, place our economic information for this rapidly growing industry on a par with that, say, for agriculture, which it may soon rival in employment.

5. Lary's inquiries into the merits of the concept of value added per employee as a measure of the physical and human capital requirements of different industries may establish a practicable means for identifying industries best suited to development in areas where capital is scarce.

6. Charlotte Boschan's analysis of the limited data presently available on job vacancies may fortify the case for mounting as massive an attack on the problem of measuring the unfilled demand for labor as we now devote to measuring the unused supply.

7. Zarnowitz' and Fels's probing of the accuracy of short-term forecasts may provide yardsticks of performance that will become widely used by both the producers and the consumers of economic forecasts.

8. Kendrick's attempt to construct estimates of "total" investment and of productive activity outside the market economy may produce a substantial change in what we regard as the size of the gross national product and perhaps also in its rate of growth.

9. The Ruggleses' scheme for restructuring the national accounts may prove so persuasive, practicable, and effective that it will generate a long series of new uses for these basic tools.

We cannot be sure as yet that any of these results will follow, but we would not have begun these studies had we not thought that consequences such as these might flow from them.

Turning to the longer-run future, one of the studies we have been planning—a study of wages—is specifically designed to contribute to an improvement in our stock of basic economic measurements. There is a vital need for improvement in our statistical information on the trend of wages. Some recent figures will illustrate one of the major problems.

In the *Economic Report of the President* issued this past January, the most comprehensive statistic on wages indicates that the rate of increase in hourly compensation between 1965 and 1966 was 6.5 per cent. This refers to wages, salaries, and supplements, converted to an hourly rate, received by all persons employed in private industry. For 1964–65, the increase was 3.7 per cent, and for the preceding four years, 1960–64, the average annual increase was 4.3 per cent. Hence it appears that in 1965 the increase in hourly compensation was smaller than it had been previously, whereas in 1966 it suddenly accelerated. It appears also that the average rate of increase between 1960 and 1964 exceeded by more than one-third the famous 3.2 per cent guidepost rate; that the increase in 1964–65, while smaller than the previous rate, also exceeded the guidepost; and that

the increase in 1965–66 was more than double the guidepost rate.

Now a quite different picture is presented by two other rather comprehensive sets of wage statistics used in the President's *Report*. One pertains to the hourly wage increases negotiated in collective bargaining agreements. These show increases rising steadily from 2.8 per cent in 1960 to 3.2 per cent in 1964, averaging 3.0 per cent. They show a further rise to 3.8 per cent in 1965, and to 4.4 per cent in 1966. These are far below the figures I have just cited for the same years, and they show a continuously accelerating rise in wage rates during the current business expansion, without any interruption in 1965. The other set of wage data pertains to average hourly earnings in some thirteen major industries, such as manufacturing, construction, retail trade. These reveal increases that vary considerably among industries, but the general level is similar to the collective bargaining figures and substantially lower than the comprehensive estimates for the private economy. Every industry but one experienced a higher rate of increase in 1964–65 than before; only one industry—agriculture—had as high a rate of increase from 1965 to 1966 as the 6.5 per cent reported for the total private economy.

Some of these discrepancies are no doubt attributable to differences in the coverage of the data—i.e., whether fringe benefits and overtime pay are included, whether salaried employees are covered, whether the self-employed are counted, and so on. But it is a confusing picture at best, and the deceleration in the comprehensive data in 1964–65 has not, to my knowledge, been adequately explained, nor does it seem likely that it represents what happened. Moreover, the 6.5 per cent increase calculated for the total private economy in 1965–66 appears excessive in the light of other information.

The fact is that we do not really know, within a reasonably narrow margin, what the recent trend of wages has been. In formulating and evaluating economic policy it makes a difference whether, as the more comprehensive figures for 1960–64 suggest, the rate of increase in wages in that early stage of the business expansion already exceeded the estimated long-run rate of increase in productivity by more than a third. And it makes a considerable difference whether, as the comprehensive figures for 1965 again suggest, there was a moderation of the upward pressure on wages at that crucial point in the expansion. At present, we do not know whether these things are true or not. The statistical picture is cloudy. Large portions of the employed population are not covered in regularly compiled statistical surveys. The concepts and methods of measuring wage rates and hourly earnings vary widely. In view of the importance for economic policy of precise, current information on wages, we believe the time is ripe for a broad review of the measurement problem, conceptually and statistically. In the course of this review, an attempt should be made to put together a comprehensive account of what has happened to the level of wages in the postwar period, to indicate the important gaps in current data that remain, and to suggest how they might be filled.

In emphasizing as I have the basic economic measurements produced by our past studies and contemplated in some of our current and future studies, I do not intend to slight the role or importance of the analytical aspect of our work: the formulation and testing of hypotheses concerning economic behavior, and the evaluation of economic policies. Looking back over the history of the National Bureau, our achievements in measurement would not have reached their unique position had the work of measurement not been accompanied by analytical studies both at the Bureau and elsewhere.

Analytical research provides the motivation for measurement—it defines the problem and specifies the data needed. The proposed study of wages that I have just described is motivated by analytical needs: to analyze the factors determining wages, the influence of wages on costs and profits, the operation of wage policies, and so on. Some of these analytical studies may become a part of or

a sequel to the wage study itself, but the measurements we hope to construct will have uses far beyond our own studies.

Our study of interest rates, to take another example, has devoted considerable effort to the development of better measures of interest rates. Cohan has constructed new series of yields on an important element of long-term corporate debt, and Guttentag has compiled new data on mortgage yields on residential and nonresidential properties. But the study as a whole has been analytical, being directed to the causes of interest rate movements and differentials, and to some of the consequences. Cagan, for instance, has found support for the hypothesis that movements in interest rates influence the duration of upswings and downswings in investment commitments and hence in the swings of the business cycle.[1] And our future work in this area is apt to be largely devoted to analysis.

This is plainly true also of Jacob Mincer's study of the factors influencing the size and composition of the labor force and the structure of unemployment. He and his colleagues are seeking causal relationships—to advance our knowledge of the influence of wages, education, racial discrimination, and other factors on employment and unemployment.

We have recently been exploring the possibility of incorporating in an econometric model the essential relationships that have characterized business cycles, as they are epitomized in Arthur Burns's essay on that subject prepared for the *International Encyclopaedia of the Social Sciences*. This essay depends heavily, of course, on the findings of investigations carried out over the years by Mitchell, Burns, and others at the National Bureau. The first task, which Gregory Chow is engaged upon, is to lay out a framework of variables, lags, and interlocking equations with a view to capturing the chief properties of the theoretical sketch of business cycles that Burns provides. Once this framework is judged capable of generating the cyclical phenomena that have been observed, we shall approach the task of fitting the model (or models) to empirical data and testing it (or them). In the course of this analytical work, which will benefit greatly from the research that has been carried on elsewhere, many of the generalizations flowing from our business cycle studies will be re-examined and, perhaps, improved.

We have been giving some thought to the need for and feasibility of a broad study of the social security system in the United States. Since the passage of the Social Security Act in 1935, social benefit programs—for old age, survivors, dependent children, disability, medical care, relief of poverty, and unemployment—have grown to enormous proportions. Outside of defense, social security has become the major business of the federal government. Employment taxes are now the second largest source of federal revenues, exceeded only by the individual income tax.

The economic impact of an expenditure program of this magnitude and of the tax burden it imposes is far-reaching, and calls for a broad, integrated analysis. While the precise scope of the study remains to be determined, it might examine, among other topics, the several objectives of social security expenditures and the degree to which they are being met from either public or private sources; the effect of social security taxes and benefits upon the contributors and the recipients, their relative income status, the stability of their income, and their participation in gainful employment; the effect of employment taxes upon labor costs, employment practices, and economic incentives; and the effect of the accumulation of reserve funds upon the capital markets and the savings and investment process. An empirical study of these dimensions, carefully planned and objectively conducted, would illuminate the policy alternatives and promote intelligent decisions in the years ahead.

This sketch of some of the new areas that may concern the National Bureau's staff over the next few years should make clear

[1] See Part IV of this report.

that our program will continue to comprehend both analysis and measurement. Our future contributions to the development of economic science and to the enhancement of economists' productivity depend upon this interaction of analysis and measurement, to say nothing of history. Some of our reports, like Friedman and Schwartz's *A Monetary History of the United States,* will blend history, measurement, hypothesis testing, and policy evaluation in a single volume. But the blend does not have to be in a single book, or even in the work of one individual.

We learn from each other, and the function of a research program or of a research institution is to facilitate this learning process. If we continue to organize our affairs so that this function is focused on the larger problems of national concern, we will continue to make our contribution to the development of a useful economic science.

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