TOWARD A FIRMER BASIS OF ECONOMIC POLICY: THE FOUNDING OF THE NATIONAL BUREAU OF ECONOMIC RESEARCH

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Few readers of the daily newspapers know how often the reports they see on the economic situation owe something to the scientific work of a private, nonprofit organization established over 60 years ago: the National Bureau of Economic Research. The information may bear on the level of *GNP,* a slowdown in productivity growth, or an inflation speedup. Or it may relate to the rise of *the service economy,* the accumulation of tangible and of *human* capital, the trends in *M1* and other forms of money, or what the *leading indicators* are saying about the business cycle. It is all valuable information to anyone seriously interested in economic and business affairs—information that is far more comprehensive and more accurate, as well as more current, than anything available in 1920.

These kinds of economic intelligence, and others that could be included in the list, involve concepts that had to be formulated and clarified. To be most useful, the measurable facts had to be quantified. To be able to reason from the facts, one had to explore their relationships to one another. Because no facts or relationships among facts can ever be more than approximate, margins of error had to be assessed. And the need for care in inferring the portent of even the most rigorous analysis of the available facts had to be conveyed to their users. The National Bureau has played its role in shouldering these tasks and drawing others into cooperative ventures to carry them out.

The purpose of this account is to tell how this role was assumed by the Bureau in 1920 and took shape during the Bureau's formative years. It will, I trust, convey a sense of the work to which the Bureau committed itself and will help explain the regard in which the Bureau is held by those already familiar with it.

Five precepts were formulated to guide the Bureau when it was established in 1920:

(1) Its research should concentrate on determining facts, and the connections among facts, that are important in dealing with major problems of economic policy.

(2) The knowledge sought should be quantitative in character, whenever possible.

(3) The research should be in accordance with scientific principles.

(4) The research should be done, and the findings made known,

under auspices and with safeguards that would assure the public of their impartiality.

(5) To this end in particular, the Bureau should carefully abstain from making recommendations on policy.

A tall order indeed! Then, as now, opinions on what to do about the nation's pressing economic problems were difficult to keep to oneself. Then, even more than now, the notion of a scientific approach to economic problems seemed rather far-fetched to most people. And what quantitative economic research was carried on had to deal with relatively scanty and largely unorganized statistical data, and researchers had to work with primitive methods of analysis and costly computational techniques. Not surprisingly, the new Bureau was observed as a "bold experiment of uncertain issue."

While the founders of the Bureau differed strongly among themselves on what economic policy should be, they all believed that, whenever possible, social programs should rest on objective knowledge of fact and not on subjective impressions, and that the range of such possibilities could be enlarged by scientific research. They knew well enough that facts alone, however firm, could not settle all their own differences on policy or the many differences of others. But at least the differences might be narrowed; thinking in public discussions might be clearer; and the discussions might take place "on a higher level." The experiment was worth the gamble.

The crucial question was how to attain objective knowledge and —also essential—how to assure that the public would accept it as objective. Suppose, the founders reasoned, they were to form an organization for this purpose—one devoted to the scientific investigation of controverted social facts and to the dissemination of the findings in a scrupulous manner. Could such a union be established under a constitution and with the procedures and goodwill that would hold it together when inevitable difficulties arose? No less vital: Could financial support for its work be obtained and retained, although the aim was to serve only the general welfare, not to provide a specific quid pro quo in the way of business service or support for particular views? If these difficulties could be overcome, the enterprise would constitute a significant contribution "to the working methods of intelligent democracy."

What was required? The question was raised when the idea was first discussed in 1916 by two men deeply concerned with economic policy, even though their views on what it should be were wide apart. One was Malcolm C. Rorty; the other, Nahum I. Stone. Rorty was an engineer turned statistician (later, chief statistician) in the American Telephone and Telegraph Company and author of a pioneer contribution on "The Application of the Theory of Probability to Traffic Problems." Among other things, he was writing a monthly letter on business conditions for his company and was

taking an active interest in social and economic problems generally. Stone was an economist who had taken the trouble, when he was young, to translate Karl Marx's *Critique of Political Economy,* which he felt made a contribution, still "timely and useful," to thinking on the free-silver issue that had been disturbing the country for many years. In 1904 he had become a tariff expert for what was then the U.S. Department of Commerce and Labor and later, chief statistician of the U.S. Tariff Commission. In 1916 he was earning his living as an arbitrator of wage disputes and a consultant to governmental committees.

Rorty and Stone had met the year before, in 1915, at a hearing of the New York State Factory Investigating Committee. Stone told the story at the 25th anniversary of the National Bureau:

> Having made a study and prepared a report for the Committee on Minimum Wage Legislation, I was testifying before the committee in favor of the adoption of such legislation by the State of New York. Rorty was strongly opposed.
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> Our next contact (or conflict) took place across the table of the Mayor's Unemployment Committee in New York City. . . . In advocating the expediting of as many public works projects as the city could undertake as an alternative or supplement to public soup kitchens, I again clashed with Rorty. He formed a definite impression of me as a dangerous radical.
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> In 1916 Scott Nearing published his pioneer study on the distribution of national income. He divided all income into service and property income and after an elaborate analysis of statistical data, in which he displayed considerable originality and ingenuity, came to the conclusion that national income was divided roughly 50–50 between the two types. Harry Laidler, at that time editor of the *Intercollegiate Socialist,* a socialistic monthly intended chiefly for circulation among college students, asked me to review Nearing's book. My review grew into an article in which I took Nearing to task for his pseudo-scientific approach to the subject, and pointed out several large items of service income that Nearing ignored in his estimate. I arrived at the conclusion that the division between service and property income was approximately in the ratio of two to one (as the first publication of the National Bureau of Economic Research subsequently confirmed).
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> My article in the *Intercollegiate Socialist* caught the eye of Malcolm Rorty who made it his business to follow current labor and socialist publications. In line with his impression and the character of the magazine, he expected to find a "red hot" diatribe on the unjust distribution of income under capitalism. Instead, my article gave him a new slant on the "dan-

gerous radical" and he invited me to lunch to talk things over. This was followed by several conferences which culminated in a warm friendship, although we continued to differ strongly on many public questions.

At our second meeting, Rorty said: "Here we are considering a most important question which deeply affects the lives of every man, woman, and child in this country, and despite a large fund of statistical data, there is no agreement on the purely arithmetical question of what part of the national income goes to each element of society. Would it not be a great step forward if we had an organization that devoted itself to fact finding on controversial economic subjects of great public interest?" I agreed that it would, provided the organization could command public confidence so its findings were accepted as conclusive by all parties to the controversy.

He assented to my proviso and asked for suggestions. I said the organization should be started by a group of well-known economists representing every school of economic thought from extreme conservative to extreme radical who should associate with them representatives of all the important organized interests in the country: financial, industrial, agricultural, labor, etc.

Rorty thought that some such plan would have to be adopted and believed he could raise the funds.... Rorty lost no time in pushing toward the realization of the project, which filled his thoughts to the exclusion of everything except his official duties.

The economists Rorty first approached were Edwin F. Gay, Wesley C. Mitchell, and John R. Commons.

Gay, who was dean of the Harvard School of Business, had already been thinking along similar lines. In 1914, he had taken a leading part, at the request of the executive secretary of the newly established Rockefeller Foundation, in preparing a memorandum outlining the organization and functions of an institute for economic research analogous to the already established Rockefeller Institute for Medical Research. The proposed institute would engage in scientific and impartial investigations "of such scope as to be beyond the power of our existing university research facilities and of such solidity as to establish firmly the competency and character of the Institute." One influential trustee of the foundation vigorously opposed the proposed institute, however. "The fundamental principles of economics are well known," he argued. For this reason, and others rather better, the idea was shelved.

Mitchell, professor of economics at Columbia University, in his recently published treatise on *Business Cycles,* had demonstrated

the fruitful possibilities of quantitative studies in economics. In the course of that investigation, which had greatly impressed Rorty and many others, and in his preceding work on the relationships among gold, prices, and wages under the Civil War's greenback standard, Mitchell had learned not only the need to examine and analyze carefully a host of facts, in the expectation of reaching dependable conclusions, but also the advantages of an organization that could muster the resources and enlist the teamwork required for thorough quantitative economic research. As Mitchell's wife put it years later, "I cannot remember a time when [he] did not talk about an organization for economic research."

Commons, professor of economics at the University of Wisconsin and (among other activities) adviser to the governor of the state, was then also serving as president of the American Economic Association. He was well known for his wide-ranging studies of economic and social institutions and his devotion to the betterment of these institutions; he could appreciate the value of objective economic research.

It is not surprising, therefore, that these economists would be favorably impressed with Rorty's ideas. So were George E. Roberts, vice president of the National City Bank, and a number of other businessmen with whom Rorty and Roberts later conferred. By June 1917, the formation of a "Committee on the Distribution of Income" was announced, now also including Allyn A. Young of Cornell University, president of the American Statistical Association; John P. Frey, editor of *International Molders' Journal,* a labor union periodical; and T. S. Adams of Yale University, prominent as an adviser to state and federal tax officials.

The objective and plans of the Committee were described in the memorandum it distributed:

> . . . [It is] to meet a growing demand for a scientific determination of the distribution of national income among individuals and families, as well as by basic sources—wages and other returns for personal service, land rents, interest, and profits in excess of a normal interest rate.
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> A knowledge of this distribution is of vital consequence in the consideration of almost every important political and social problem, and will be of particular value in relation to the many special problems of taxation, legislation, and industrial readjustment that will necessarily arise during and after the present war.
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> The Committee will concern itself wholly with matters of fact, and is being organized for no other purpose and with no other obligation than to determine the facts and to publish its findings. . . .

> The Committee is now seeking preliminary pledges of financial support. It estimates that the minimum sum required for its purpose will be $10,000, and that effective use can be made of added amounts up to a total of $25,000. It makes its appeal for support, not only to business men, manufacturers, and employers, but to all others who believe that sound national progress along industrial and social lines must be founded upon a definite knowledge of those basic and vital facts which concern themselves with the income and welfare of the individual.
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> The Committee has no conclusions or theories to advance and assumes no obligation to any subscriber other than to make and publish its determinations of fact. This freedom of action and impartiality of attitude is an essential element in the undertaking, and for this reason the Committee is seeking widely distributed support rather than large contributions from individual sources. . . .

The entry of the United States into World War I diverted Rorty's energies to more urgent tasks. But at the same time, the wartime experience revealed an appalling lack of the quantitative information needed to cope with the urgent mobilization and reconstruction problems confronting the nation. Rorty's hand was thus greatly strengthened when he returned to his mission soon after hostilities ceased. Before the end of 1919, the necessary funds were in sight.

When a majority of the prospective Board of Directors of the new organization attended the annual Christmas meeting of the American Economic Association, they had the opportunity to hammer out the organization's bylaws. And because the founders were already thinking of business cycles as the second project, and of attractive research possibilities beyond that, they broadened the name of the new organization to the National Bureau of Economic Research. In January 1920 the Bureau's certificate of incorporation was signed and recorded, and the organization was legally established.

The first section of its charter specified the particular objectives for which the corporation was formed:

> To encourage, in the broadest and most liberal manner, investigation, research, and discovery, and the application of knowledge to the well-being of mankind; and in particular to conduct, or assist in the making of, exact and impartial investigations in the fields of economic, social, and industrial science, and to cooperate with governments, universities, learned societies, and individuals.

The interests and opinions of the 19 men who made up the Bu-

reau's Board of Directors, when it met at its first annual meeting, ranged widely over the spectrum of economic thinking. But they were all known to be men of "judicial temperament." There were "Directors at Large," including the founders and a few more university professors, as might be expected, and in addition, a leading public accountant, the secretary of the socialist society that had published Stone's review, and the labor union leader already mentioned. A category of "Directors by Appointment" represented the leading professional associations—economic, statistical, industrial relations, and engineering—and some prominent organizations of labor, farmers, and periodical publishers. (In 1927, a category of "Directors by University Appointment" was added, with a reclassification of a few Directors already on the Board and the addition of a few others, increasing the total number to 22.) This Board was charged with the final responsibility for approving the selection of studies to be made, with the personnel and sources of finance required to carry them out in a scientific manner. And—a vital provision—the Board was to review the methods, results, and form of presentation of each study, with particular attention to its objectivity and impartiality, and then approve or disapprove its publication. Dissenting or qualifying opinions by any Director were to be published, if he so desired, in any report approved by the majority.

With grants totaling $24,000 by year-end, the new organization was able to employ a small staff of economists headed by Wesley Mitchell. The group set to work on a study of national income.

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It is one thing to affirm adherence to the principles of science; it is quite another to define these principles convincingly and to conduct one's research in accordance with them in the heat and bustle of the day. Similarly, a program of research on facts that are important in dealing with major problems of economic policy may not necessarily envision and faithfully pursue a program so concentrated on the basic process and factors that operate in the modern economy that the results will be enduring and widely applicable to the various issues that arise in a changing world. And so, with a design that assures impartiality, how any policies and procedures work out in practice must depend on how they are read and observed in circumstances that are bound to vary.

Before sketching the developments that followed, I must lay the groundwork by saying more about Wesley Mitchell, the man who led the staff as Director of Research during his quarter-century tenure and set the style and standards with which the Bureau began its work.

In fact, the capital on hand when the Bureau was launched included not only the goodwill of its Board members and the dollars

contributed by its supporters but also the rare "human capital" embodied in its first Director of Research—his habit of working along strictly scientific lines unswayed by prejudice, bias, or impatience; his belief that the economic activities and problems of men who live in the modern world would best be understood by analyzing the factors and processes involved in money-"making" and money-spending; his appreciation of the need to keep in mind, in this analysis, how economic behavior adapts not only to institutions and events but also to expected changes in them; his understanding of the critical importance of quantitative measurement in economic analysis; and his hard experience dealing with masses of statistical data on his own, with little assistance.

Mitchell expressed his appreciation of the task that lay before social scientists in his presidential address to the American Statistical Association shortly after the end of World War I. The uncertainties that surrounded this task were not slighted.

> Our best hope for the future lies in the extension to social organization of the method that we already employ in our most progressive fields of effort. In science and industry . . . we do not wait for catastrophes to force new ways upon us. . . . We rely, and with success, upon quantitative analysis to point the way; and we advance because we are constantly improving and applying such analysis.
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> While I think that the development of social science offers more hope for solving our social problems than any other line of endeavor, I do not claim that these sciences in their present state are very serviceable. They are immature, speculative, filled with controversies. . . . Nor have we any certain assurance that they will ever grow into robust manhood, no matter what care we lavish upon them. . . . Those of us who are concerned with the social sciences . . . are engaged in an uncertain enterprise; perhaps we shall win no great treasures for mankind. But certainly it is our task to work out this lead with all the intelligence and the energy we possess until its richness or sterility be demonstrated.

In Mitchell's youthful efforts to trace and explain the behavior of prices and wages under the greenback standard of the Civil War, he had already revealed a clear understanding of the need to test his own speculative reasoning, as well as the speculations of others, by recourse to the evidence. But the price indexes and the other statistical data required, when they existed at all, were far more positive than those over which we find reason to worry today. Mitchell quickly learned, then, not to be content with the first piece of evidence that came his way but to search for, and check it with,

other evidence; when the available evidence failed to meet his needs, he made new calculations. And when offering his results, he drew on his own experience in trying to evaluate the results and evidence gathered by others and took the time and space required to set out the supporting statistical record in detail, with an explanation of its derivation and shortcomings.

Mitchell's early habits were strengthened when he looked more deeply into the "recurring readjustments of prices" he could observe in the post-Civil War period. The resulting wider perspective of business cycles embraced relationships among a broader range of the economic activities involved in money-making and money-spending—now including production, employment, and profits, among others. This extended Mitchell's concern with data going well beyond the series on money, prices, and wages that he had studied earlier. Mitchell's care in gathering, analyzing, and presenting the evidence underlying his theoretical results will be evident to any reader of his 1913 volume on *Business Cycles* and the series of technical papers he published separately in professional journals.

Mitchell envisioned the Bureau as an organization in which the burdensome although essential tasks of empirical research could be performed on a larger scale and more efficiently. Also, in this organization the staff's findings could be taken seriously by all sides struggling over the nation's economic policies. In the glowing terms with which Arthur F. Burns's memorial essay of 1948 came to express Mitchell's hopes and dreams of 1920:

> Here a program of critical research might actually be carried out, not just proclaimed "aloud." Here empirical investigations might be undertaken, broader and more fundamental than any yet attempted by economists. Here complementary technical skills could be pooled, and the process of developing new knowledge made more efficient. Here an investigator could subject his methods and results to the steady and searching scrutiny of skilled colleagues. Here hypotheses could be checked by statistical data, statistical data stimulate new hypotheses, and hypotheses new data. Here tested findings could cumulate, reinforce one another, and open up new problems, as was routine in the established sciences. Most important of all, here was an experiment in democratic action, men of many shades of political opinion joining in the undramatic enterprise of reviewing the factual findings of a technical staff. If a group so constituted as the National Bureau's Board of Directors could work harmoniously and accept staff investigations of a controversial question such as the proportion of the national income paid out in wages or

accruing as profits, might not reason triumph over passion in an ever-widening circle of men?

The subject selected for the Bureau's first study was not limited by Mitchell to the question that had started the discussion between Rorty and Stone: the distribution of the national income. Also covered were the size and industrial composition of the national income, and its growth and fluctuations during the period beginning in 1909. The staff also went on to extend existing price indexes and to calculate some new ones, in order to express income in real as well as in monetary terms.

National income was a singularly appropriate subject for the Bureau's first research program. The measurements put the state of the nation's economic well-being into quantitative form, if only to a first approximation. Modern economic life is organized very largely on the basis of making and spending money incomes, so analysis of the sources of economic change should start with the measurement of the national income and its principal components as they changed over time. The resulting "national accounts," in today's terms, would "set out the framework of a moving economic system." And this framework could be expanded and fleshed out if the Bureau succeeded in establishing itself and if the necessary funds, data, and personnel became available. The framework could then be put to use in analytical studies of the many questions that troubled people—questions about the magnitude, character, process, and consequences of aggregate change in and distribution of the nation's income.

The pioneering investigation of national income was completed in less than two years and was published in two volumes. A small book summarized the findings, and a much larger volume gave the detailed results, sources, and methods used. Looking back in 1948, Burns stated that these volumes, more than anything else, helped to establish the reputation of the Bureau for thoroughness of work and "won public and professional support for the National Bureau in its early years of struggle." The summary volume, written largely by Mitchell and designed to convey the study's findings to the public at large in language they could understand—a significant aim of the new Bureau—"may justly serve as a model of exposition." The second volume—"a cross between a census report and a treatise on statistical method," in Mitchell's words—continued Mitchell's practice, and began the Bureau's practice, of presenting the underlying evidence as fully as possible and subjecting the research results to critical examination.

Indeed, the study also could serve as a model of craftsmanship —in the formulation and critical discussion of concepts and in the organization of large masses of disparate statistical data in terms

of the concepts chosen. Moreover, the study took into consideration readers who might prefer alternative concepts or might have a special interest in certain details of the calculation. Furthermore, the study candidly displayed the ambiguities, conflicts, and gaps in the available data. Thus, early in the study, recognizing the hazards of estimating national income, Mitchell and his colleagues decided to employ definite statistical controls. One staff member, Willford I. King, calculated the national income by the sources of production—that is, by estimating the value added by each industrial group (including government) to the materials and services purchased from other industries. Another staff member, Oswald Knauth, independently sought to determine the incomes received by the public, using occupational data on number and average compensation of workers at the lower income levels; income tax reports, including an estimate of tax-exempt income, for the upper incomes; and published corporate reports as well as income tax data for the undistributed income of business enterprises. The concept of income, of course, was the same for both, but the sources of information were entirely different and the two estimates could thus test one another. Understandably, Mitchell and his colleagues "felt not a little nervous when the day came on which we first cast up the totals by Sources of Production and by Incomes Received. . . . When the largest discrepancy in any one year proved to be only 7 percent we felt a marked increase of confidence in our work."

Also contributing significantly to the impact made by the Bureau's initial study was the searching examination—made in a third section of the study by still another staff member, Frederick Macaulay—of the data that might lend themselves to making a distribution of individuals or families by size of income, and of the assumptions, often little more than guesses, required to weave the scattered data together. After his review, Macaulay decided that only a distribution by individual recipients, not by families, was possible on this always controversial facet of economic life. "It is only because of the practical value of even the roughest kind of an estimate"—all he would claim for his result—"that any statistician would think of attacking the problem." To reassure the reader, however, he added that "the final results are probably not quite so bad as they might have been had we not had a number of collateral estimates [those by King and Knauth] with which roughly to check up and otherwise adjust the first results of our estimates."

After dwelling on the inadequacies of the data, Macaulay addressed the question of whether "it would not be possible to formulate a general mathematical 'law' that might then be used for 'adjusting' the tentative and hypothetical results obtained from piecing together the existing scant and inadequate material?" This led him to a careful study of the then world-famous "finding"

by the distinguished Italian economist, Vilfredo Pareto: the shape —not the level—of the size distribution of income in different countries and at different times was invariant. Macaulay ended up with serious doubts about the validity of Pareto's curve and thus also about the far-reaching policy implications that many people were inclined to draw from it.

Still other marks of the care exercised in the national income study must have impressed the readers of the volume and raised a standard for later investigators to follow. Because most of the large items that entered into each estimate of the aggregate national income could also be arrived at in two or more ways, as the introduction to the first volume stated, the authors paid attention to devising and applying similar tests of the partial results, with help from correspondents who scrutinized the tentative results with expert eyes.

> We have not leaned heavily upon the statistician's fond hope that errors made by the way will cancel each other in the end. Doubtless they do so to some extent and our totals are the better for that fact; but we have tried to make the estimate for each item considered by itself as nearly correct as our data, time, and means have permitted. Of course the estimated errors of our figures vary widely from item to item with the quantity and quality of the underlying statistics. Therefore, we have been careful to indicate the degree of confidence we feel in the various results. Some readers of the second volume will think that we have been meticulous in our treatment of minor factors. It is true that, in very many of the items, figures several times too large or too small would not appreciably affect the final aggregates, which run in tens of billions of dollars. But that is another comfort of which we prefer to make sparing use. Many of these minor items have an interest quite independent of their contribution to the total, and if mistakes are found even in the smallest of them by men who have a special knowledge of the facts, we shall be grateful for their help in rectifying our estimates.
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> It is true also that many of the important uses which an estimate of the National Income and its distribution serves are served almost as well by a fair approximation as by an exact measurement, could such a measurement be made. We have treated that consideration, however, not as an excuse for slighting details, but as a spur to check the validity of our broad results as carefully as possible.
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> Finally, . . . we believe that the results presented . . . at most are merely the best approximations we can frame now from the current data. We do not regard the tables in this result as final.

The Bureau's work on national income, then, was not intended to end with the two-volume report. At the outset the authors recognized that study of the aggregate, composition, industrial sources, and distribution of national income would remain in the Bureau's program. Revisions of these basic measurements would obviously be needed as concepts and methods were sharpened and new or better data became available. Updating would be needed over time; and if the job were to be undertaken by other competent authorities (none was in sight in the early 1920s), critical review of their extensions would be salutary. Estimates for the years prior to 1909 would help fill in the historical record available for analysis. And, the relatively sparse detail of the initial study might usefully be enriched beyond the limits that had to be imposed originally.

In fact, while finishing up the initial investigation into national income, the staff had already begun some of the work growing out of that study. Two volumes on the geographical distribution of income among states were published, one in 1922 and the other in 1925. An extension of the national income estimates through 1928 was published two years later. Another study of annual savings, begun in 1921, could not meet standards acceptable to Mitchell and the Board, however, and the manuscript was returned to the author—the first exercise of a practice that was followed later under similar circumstances—freeing him to publish it on his own responsibility, if he wished.

The big step forward in the scale and quality of the Bureau's work on national income came in 1930 when Simon Kuznets, a student of Mitchell's and already on the Bureau's staff, was asked to take charge of the area. After some hesitation he agreed—a momentous decision—and began the preliminary work for what proved to be a notable series, extended over the next three decades, of studies of the nation's income, savings, and expenditures. When, a year or two later, the U.S. Senate passed its famous resolution requesting an official government estimate of the national income and the Department of Commerce asked the Bureau for help, the Bureau turned to Kuznets. He was well prepared to plan and make the estimates requested and to help organize the governmental unit that was to continue them on a regular basis—the beginning of the official GNP and related series.

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The Bureau's objective was not to amass sets of statistical tables on income or anything else for their own sake. Measurements of the nation's income, and of its distribution among the people, were of interest in themselves, of course, for what they indicated about the state of the economy. But this was only a starting point. The objective was a deeper knowledge of the factors that determine the current state of the economy and affect its prospects.

The analytical work required could hardly wait until all the possible data were in, however. In fact, beyond the basic tables put together in the initial study and in related efforts later, only during the course of the analysis could the researchers determine just which additional data were most needed, besides those required to fill some obvious gaps, and how far to go in gathering them—or in encouraging their production when they did not yet exist. Work on the character, causes, and consequences of change in the production, distribution, and composition of the nation's income should proceed simultaneously with the efforts to gather and organize the basic statistics.

Mitchell was already concerning himself with the subject of business cycles—a profoundly disturbing type of change in the production, distribution, and composition of the nation's income. He was keenly aware of the gaps he had had to bridge while accumulating the evidence for his 1913 volume. To him, that study was only a preliminary exploration of a complex subject. He proposed that a fresh attack on it should be undertaken after the initial national income study was completed. The latter had included the decade 1909–19 because of the great interest attached to the fluctuations the national income had undergone, among other reasons. As Mitchell explained in his second Annual Report, the subject of business cycles was of great importance, because these cycles affected the economic fortunes of everyone. He went on to give other reasons: the Bureau could employ quantitative methods to great advantage; the staff could make effective use of much of the special knowledge gained in the study of national income; and while some institutions and individuals were working on several aspects of the subject, none was planning a comprehensive survey of the whole that would put together "in concise, systematic, and readily comprehensive form" the results of those and other researches. To this enumeration Mitchell added, with a straight face, that "the staff of the Bureau seems qualified by past experience and present interest to fill this want."

The Executive Committee of the Board could hardly fail to approve the choice. It granted approval in March 1921 when, if more reason were needed, everyone knew how severely the United States, and other countries as well, were being buffeted by the sharp business contraction that had begun early in 1920 and was still underway. Many of the subjects being proposed to the Executive Committee, such as the severe unemployment then prominent in the news, could be covered in the larger subject.

Mitchell's initial plan for the business cycle study was modest contrasted with the dimensions to which the study eventually grew. It was ambitious by almost any other standard. No "comprehensive" survey could help but be substantial, of course. In addition, how-

ever, the original proposal for the business cycle study included several "special studies" of related topics "not yet adequately investigated." The list of auxiliary studies became longer almost before the project got underway, and longer still as the work proceeded: problems were uncovered and ways and means to deal with them were sought and found.

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The comprehensive survey, or treatise, as Mitchell thought of it in the first stages of his project, was to work over the "vast amount of new materials and new experience" accumulated since preparation of his 1913 volume. There were also "many new ideas to be reckoned with." Discussions of such topics as forecasting were extended; and topics not previously covered, including the costs and problems of controlling business fluctuations, were added. Clearly, analyses of the statistics on the many processes involved in business cycles were bound to require a great deal of space. Since Mitchell wanted the treatise to be more suitable for use by businessmen and students than his earlier volume, the statistical analyses were to be published separately—in what became a long series of monographs and papers, as it turned out.

Despite the burdens of Mitchell's other work at the Bureau—completing the initial national income study, overseeing special studies in that field as well as those in business cycles, and managing to meet the urgent governmental request described below—with the help provided by the Bureau, he was able to publish the first major installment of his proposed treatise in 1927.

The new study benefited from the hard work done earlier, of course, and its title, *Business Cycles: The Problem and Its Setting,* is the same as that of Part I of the 1913 volume. However, it is a thorough rethinking and rewriting of the earlier piece, with the "many new ideas" and "new materials and new experience," supplied by Mitchell and other economists who had been studying the problem, that required a fivefold expansion in size.

The number of speculations of economists on business cycles had multiplied since the completion of Mitchell's earlier volume, and many had grown in sophistication. Just as before, however, the current theories differed widely in the emphasis placed on one or another of the many processes that run side by side or follow one another in cyclical fluctuations: orders, production, delivery; hirings, firings, or quits; the receipt and spending of money incomes; the saving and investing in tangible and intangible capital; the granting and repayment of commercial and bank credit. The theories also differed on the factors, physical, psychological, political, economic, or social in origin, that affect these processes. "Any of these factors or any of these processes," Mitchell noted in the first chapter of his 1927 volume, "can be made to yield a plausible

theory of business cycles; . . . and that is what each of our theorists believes himself to have done with reference to the factor of his choice. Nor can we be sure in advance that any one of them is wrong."

To allow for inclusion of these factors, it was necessary, first, to cast a broad net over the economic process, using the compiled facts to determine the relationships among the various individual processes and factors during each phase of the business cycle. Only then would an analytical description of the full round of events be possible. Second, it was necessary to make measurements. "What is the relative importance of the factors represented as causes of fluctuations? What is the relative amplitude of the fluctuations characteristic of these factors and of the effects which they are said to produce? In what sequence do the fluctuations appear and at what intervals of time? . . . Such problems can be solved only by appeal to statistics," Mitchell argued. "Indeed, our best chance of improving upon the work of earlier writers lies in this direction."

But qualitative data were not to be ignored. As before, something could be learned about business cycles even from contemporary opinions concerning business conditions, especially useful in countries and periods for which statistical time series were insufficient for the purpose. These, compiled in the form of "business annals," could now—with the help of a staff—be based on reports for more countries and many more years than the brief survey to which Mitchell had had to confine himself in the 1913 volume. The annals confirmed what the available statistics showed: the "normal" state of business was one of continual change. Along with the statistics, they helped to identify cyclical turning points in general business conditions. The dating of these turning points—NBER's "business cycle chronology"—made possible an unusually full historical record of the timing and duration of contractions and expansions in general business that shed light on the character of business cycles and helped to define them. The chronology, reproduced regularly in the Department of Commerce's *Business Conditions Digest,* continues to provide perspective on current changes in the business situation.

Mitchell's long chapter of 1927 on economic organization also went well beyond earlier versions, particularly in what it had to say on the historical connection between business cycles and the use of money, the modern organization for making money (the "organization within which business cycles run their course"), the system of prices, and the roles played by the various economic actors in guiding economic activity in a money economy. It also added new and important sections on the monetary mechanism and the flow of monetary payments, including an illuminating discussion of the factor of time in the equation connecting the quantity and velocity of money with the volume and price of goods exchanged.

As even this cursory glance at the volume suggests, Mitchell's discussion could be said to constitute "a setting" for the study of economic change viewed more broadly than business cycles alone —the study, no less, of the "entire economic process in motion." Mitchell had, indeed, thought of his 1913 study of business cycles as an introduction to economic theory generally. The 1927 study carried the thought a long step forward. It was, as Burns was to put it later, "virtually a survey of the field of economics," a judgment also pronounced by Joseph Schumpeter when he reviewed the study soon after its publication. It helped give direction to many of the Bureau's studies in later years and also invited exploration by others.

* * *

The last few pages of *Business Cycles: The Problem and Its Setting* outlined the features of Mitchell's plan for measuring business cycles and seeing how they run their course. Mitchell knew well enough that "no group of workers in the present generation can hope to cover the field marked out by these suggestions. . . . But what our successors can accomplish will depend upon the stage at which we pass on the problem. Our task is to use as best we can the means at our disposal—the insights given by economic theory as it now stands, the statistical and historical data now available, and all the suggestions we can get." Mitchell therefore wanted the Bureau's study to be as thorough and as accurate as reasonably possible. His standards on what was thorough and accurate were not pedestrian, as I have already indicated, but Mitchell could plan boldly. For the Bureau—and those who supported it—opened to him possibilities far wider than any previously available. He did not have to limit himself to examining only the statistical data that could be readily raked together, aided only by techniques that might not be entirely appropriate.

His data demands, then, were very large. He wanted to analyze the cyclical behavior of all the economic process in a given country that appeared significant for understanding that country's business cycles—and to do so for a number of countries. These processes included—but were not confined to—many that had caught the attention of theorists speculating on the cause or causes of business cycles. Mitchell wanted to know not only the cyclical pattern of total production, for example, but also how the pattern of behavior of consumer goods differed from that of investment goods; and with regard to these, what the differences were between the various classes of consumer and investment goods. Production was just one of the processes that appeared significant and required study. Further, Mitchell wanted to determine the average amplitude of fluctuation and the average timing of changes in each process during successive stages of the business cycle. He also wanted to

know the degree of variation around these averages; and he wanted to look into the possibility (on which there had been—and continues to be—much speculation) that long waves or secular trends lay hidden in this variation. For his purpose, then, annual data were clearly inferior to monthly or quarterly data; and short series were clearly inferior to long ones. Nor could the behavior of a series be fully understood without attention, for example, to the number of firms reporting or the markets covered and to whether the data reported related to a single day or week in a month or to the month as a whole. Therefore, a wide and careful canvass of data was necessary, going beyond the presently available sources. The description of the data and of their sources had to be carefully examined in order to understand their coverage, continuity, and limitations, not necessarily accepting the descriptions (if any) appended to the data.

Mitchell's "embryonic" technique—the adjective is Mitchell's—for measuring cyclical behavior developed "under the solicitous attention of numerous coworkers" led by Simon Kuznets in its earlier stages and Arthur F. Burns later. With the wide variety of data Mitchell wished to use, much experimentation was required to decide which aspects of cyclical behavior to measure and just how each measurement should be made. To determine how well the methods of cyclical analysis did the job and how well the findings to which they led stood up, Mitchell made provision for a "searching critique of our methods and rigorous tests of our findings." Not surprisingly, the chapter on measurement techniques, which introduced the analytical portions of Mitchell's treatise, grew into a large volume, jointly authored by Burns and Mitchell; after a wartime delay, it reached publication in 1946. It was entitled, simply, *Measuring Business Cycles,* but it also contained an unprecedentedly rich empirical analysis of secular, discontinuous, and cyclical behavior, and tests of hypotheses bearing on them. It also provided a conspectus of the stable and the irregular features of cyclical behavior that, with the business cycle chronology already mentioned, could be helpful to anyone trying to follow and assess current changes in economic conditions.

As E. B. Wilson of Harvard expressed it, Mitchell revealed—in the extent of his collection of data and his attitude toward its treatment—the interest and the conscience of a born naturalist: "one who is at great pains to go out into the world of concrete and detailed fact, to 'look see' what phenomena of a certain sort are really like, to find out in nature something that is new to knowledge."

The massive job of enlarging, organizing, and annotating the collection of data, helping to improve and clarify concepts and procedures, and studying and interpreting the tentative results that emerged, was already underway on a modest scale as the first

installment of Mitchell's study reached publication in 1927. By the end of the Bureau's first decade, Mitchell had managed to complete a number of preliminary papers and reports on its second phase; these were published in the *Encyclopedia of the Social Sciences,* in the report on *Recent Economic Changes* described below, and in the series of *News-Bulletins* started by the Bureau in 1922. When a substantial increase in funds came in 1929—a remarkable vote of confidence in Mitchell and the Bureau as a whole—a full head of steam was applied to the business cycle project.

* * *

Mitchell had a keen sense of the value of a well-organized and adequately annotated collection of statistical time series on national income, production, sales, employment, investment, prices, wages, interest rates, and other important economic facts. Extended back in time, revised and updated on a current basis, freed of seasonal movements when these clouded the view of the fluctuations or other changes of primary interest, and accompanied by analytical measurements revealing the characteristics of each series' fluctuations, such a collection would be highly useful. It was essential, of course, in Mitchell's own study of business cycles. But it also proved useful for the studies of other economists, who might want to apply to the data methods of analysis other than those on which Mitchell would settle, or to confirm—or deny—the results derived by Mitchell's technique. He also knew that the data collection, especially if kept current, would be useful to many in government and private business who were concerned with the assessment of current business conditions. (This was demonstrated in later years by the close attention paid to the Bureau's leading, coincident, and lagging indicators of business conditions—a classification first put to practical use in 1938 in response to a request by the Secretary of the Treasury, then greatly worried by the sharp recession that had begun in spring 1937.) For these reasons, Mitchell deemed the Bureau's data collection worth publishing and perhaps worth updating on a current basis—a sort of loose-leaf "encyclopedia of time series," as he termed it in his plan.

The collection of carefully reviewed and annotated time series being compiled at the Bureau, mounting into the hundreds, was available, however, only in files kept in the Bureau's offices. The high cost of printing tables of figures in those days blocked hope of publishing the collection. Portions of the file eventually did reach publication in the usual printed form in various Bureau and federal government publications—in the latter case, notably in the first edition (1949) of the Census Bureau's *Historical Statistics of the United States,* and then on a current basis in the Department of Commerce's monthly *Business Conditions Digest,* a publication

that the Bureau helped to design and to which it continues to contribute in several ways. A fuller selection of of the time series became more readily available on microfilm shortly after World War II. However, an approximation of a generally available "encyclopedia" of time series materialized in the form of the modern data bank only after computers and computer memories became able to handle a large volume of detail, accept revisions readily, and put the time series into machine-readable form.

A related set of historical data, the "qualitative" business annals (of which Mitchell made summary use in his 1927 volume), did get into print in a volume published by the Bureau as early as 1926. For as many as 17 countries, the annals were pushed back into the nineteenth century, and for two of these, England and the United States, even to 1790. This remarkably extensive conspectus, not surpassed since its preparation by Willard L. Thorp, was updated in a number of Bureau publications—through 1931, in a Bureau *News-Bulletin.* Further updating of the annals was made unnecessary by the widespread development and availability of statistical time series. The Bureau's business cycle chronology, which continues to be updated when necessary—and becomes a news item for the media on those occasions—is now based entirely on statistical time series.

* * *

Among the special studies undertaken as part of the business cycle project was one on the fluctuations of savings, a subject already mentioned among those growing out of the national income study but also obviously relevant in any analysis of the flow of funds during business cycles. Another was a study of cyclical fluctuations in output per worker-hour, which significantly affected costs, profits, and profit expectations, and in this way might play a role in the process by which prosperity bred recession and recession led to revival. In his 1913 treatise, Mitchell had been able to do little more than speculate about these matters—other economists were in no better position—and he wanted to test his presumptions. As with the proposed study of fluctuations in saving, however, the factual data needed for definite conclusions on productivity changes turned out to be inadequate even on an annual basis. The Bureau was able to begin drawing reasonably accurate portraits of secular and cyclical changes in saving and productivity only in the 1930s, when Kuznets began his studies of national income and capital formation, and Frederick C. Mills and other Bureau staff members started their researches on production, employment, and productivity in manufacturing and other major industries.

Also among the special business cycle studies listed in Mitchell's plan was one by Macaulay on cyclical fluctuations in interest rates that got underway early in the 1920s and reached publication in

the 1930s. It deserves more than a passing reference, for it was destined to become something of a classic; it illustrated some of the problems that beset scholars—at the Bureau and elsewhere—when their standards are as high as those Macaulay had demonstrated in his contribution to the initial income study. Like Mitchell, Macaulay felt unable to depend on the available measures of interest rates. Under his penetrating eye, these plainly suffered from serious conceptual as well as statistical defects. Therefore, the collection and analysis of widely scattered data—on the interest rates written into new issues of short- and long-term obligations, on the yields offered in the market on existing securities of various terms and dates of issue, and on parallel data relating to stock prices and business conditions generally—were required. Macaulay felt it desirable and necessary to add to this collection each time he uncovered hidden sources. And to separate the cyclical fluctuations in interest rates from the other kinds of changes entwined with them, Macaulay also felt it necessary to explore and improve on existing techniques. Explication of the apparatus he devised grew technical and long enough (and was felt to be generally useful enough) to warrant separate publication (in 1931) in a volume on *The Smoothing of Time Series.* Most important, in analyzing and testing prevailing hypotheses concerning the relationship between fluctuations in interest rates and changes in commodity prices and the physical volume of business, and between short and long-term interest rates, Macaulay raised and wrestled with theoretical questions that had been passed over lightly previously. These questions concerned the nature and role of expectations in financial markets—difficult questions that economists have been struggling with ever since. All this took time, and the Bureau's first decade ended with the study "practically"—but not quite—finished, with only a few brief papers on preliminary results actually published. The study finally did reach publication—in 1938—under Mitchell's patient fostering, with the modest title, *Some Theoretical Problems Suggested by the Movements of Interest Rates, Bond Yields, and Stock Prices in the United States since 1856.* Also contributing to the establishment of a Bureau tradition, it contained an extraordinarily full appendix providing the critical reader with the empirical data analyzed.

* * *

Requests by outsiders for studies by the Bureau arose early in the Bureau's career, and they continued to arise. These were frequently on narrow topics rather remote from the lines of research then occupying the Bureau staff. Mitchell felt it "would be poor policy to scatter our energy over a considerable number of unrelated topics, however fascinating." These demands were therefore generally declined, setting the policy generally followed in later years.

One request that came very early—in 1921—did meet Mitchell's condition, however. While the plans for work on business cycles were being formulated, Herbert Hoover, then Secretary of Commerce, asked the Bureau to make "a careful investigation into the cyclical fluctuations in employment" troubling the country and "into the merits and defects of various remedies proposed." Even the magnitude of the problem confronting the country was unclear. No measures of national employment, nor of GNP, nor even of industrial output, were currently available. Everyone knew that things were bad, but nobody knew within two, three, or more percentage points the fraction of the labor force that had been thrown out of work by the business depression. Such an investigation, believed the President's Conference on Unemployment (on which four members of the Bureau's Board were serving), was necessary before an attempt could be made to formulate a policy for the country.

The Executive Committee agreed to undertake the work. It would be of obvious service to the country but also, a major reason, the topic proposed was directly in line with the work already planned. Provisos—which were also to enter later agreements made for special studies—were made by the Bureau and accepted by Secretary Hoover: the Bureau's work should be confined to ascertaining "facts needful to be considered," with the conclusions to be drawn left to the responsibility of a Committee of the Conference; the Bureau's report should be submitted to its own Board of Directors for approval before being sent to the Conference; the Bureau should be free to publish its findings separately, if it wished; and the necessary funds were to be provided.

To help carry the burden involved, which included a deadline of six months, the Bureau received the "unpaid cooperation" of several other agencies, private and public: notably the Russell Sage Foundation, the American Association for Labor Legislation, and the Bureau of Railway Economics; and the services, paid and unpaid, of 15 individuals (including two Board members) outside the Bureau's own staff, who were especially conversant with particular aspects of the subject.

The deadline was met and a report consisting of 21 chapters was published (in 1923), under the title *Business Cycles and Unemployment*. It was the first of the Bureau's publications concentrating on economic policy. The leading proposals of the time for preventing or reducing cyclical unemployment were reviewed as objectively as possible. A by-product of the report, using more fully the results of a pathbreaking questionnaire sent out during the course of the investigation, was entitled *Employment, Hours, and Earnings in Prosperity and Depression, United States, 1920-1922.*

Mitchell was troubled from the beginning by the necessity to "work against time," precluding a better coordination of the various parts, fewer gaps, and a broader basis for some of the chap-

ters. When it was over, he did say that the Bureau had reason to be glad it had accepted Hoover's invitation to cooperate. But he also observed that "we stopped on a set date because we had to, not because we were ready to.... When one is trying to find out something which is not known—and that is always the case in a scientific investigation—it is seldom possible to foretell just how much time the search will take. Our objections to working against time have been confirmed and strengthened by this experience.... We hope it will seldom if ever be necessary to accept such working conditions again." One can sense Mitchell's discomfort with, and resistance to, the constraints of time and money upon careful research and can feel the sympathy with which he was later to view Macaulay's progress reports.

* * *

Much less troublesome, but also pertinent to the subject of business cycles—as well as to income distribution—was the request in 1923 by the National Research Council of the National Academy of Sciences for a study of the relationship between migration and labor supply. Mitchell took some pleasure in noting that it was the Academy's first recognition of economics as a science. Migration across international borders was a topic of great interest at the time —even greater than now—with legislation to restrain immigration into the United States being discussed and passed. The Bureau borrowed Harry Jerome from the University of Wisconsin, and with the advice of other members of the staff, published the study *Migration and Business Cycles* in 1926. It was somewhat a rarity in its field—"a carefully documented study," Mitchell stressed in his preface to the volume, designed to enhance understanding "of a problem too often treated in a controversial spirit."

Even before the Jerome volume was in print, the newly established Social Science Research Council asked the Bureau to undertake another, much larger study of migration, planned to cover the mass movements of mankind over the earth as a whole during the past century or more. This required cooperation on an international scale, the first of such ventures by the Bureau. The results appeared in a very large compendium of carefully ordered statistics, prepared under the direction of Imre Ferenczi of the International Labor Office, published in 1929; another volume, not much smaller, of "interpretations" by 20 authors from as many countries, was published in 1931. The two volumes were edited by Walter F. Willcox of Cornell University. Some attention was paid in the "interpretations" volume to the influence of fluctuations in business conditions—long swings, as well as the shorter business cycles—on emigation and immigration. But if the study as a whole had to be categorized and a distinction made between studies of fluctuations and studies of long-term economic development, it

would count as a contribution to the latter, as would some sections in Mitchell's 1927 volume and in other studies mentioned earlier.

* * *

The President's Conference on Unemployment, which had commissioned the study of *Business Cycles and Unemployment,* went on in 1927 to set up a Committee on Recent Economic Changes to "observe and to describe the American economy as a whole," particularly since the recovery from the depression of 1920–21. It was to "make a critical appraisal of the factors of stability and instability," with a view toward suggesting recommendations for improving the stability of the economy. As it had in 1921, the Conference turned to the Bureau for a survey of "the facts." The Bureau assembled almost a score of collaborators, including several of its own staff members; still others helped in a field survey of economic changes in individual firms. With the assistance of "an unprecedented number of governmental and private agencies," as the Committee on Recent Economic Changes noted, the Bureau prepared the two-volume survey, published in 1929, presenting various aspects of the country's economic development. The subjects ranged widely, from changes in the nation's income and consumption, changes in its industrial structure in response to fluctuations in money and credit, and the effect of these changes on business—changes mainly during the post-World War I period, but in some chapters running back into the prewar period as well.

In a final review, Mitchell discussed a question that appears especially poignant in the light of developments after 1929—one that was to come up again in the 1960s. The relative stability between 1921 and 1927, Mitchell noted, "had encouraged optimists to say that 'the business cycle' had been 'ironed out'" in the United States. After surveying some of the results he was obtaining in his study, Mitchell concluded that "business cycles have not been 'ironed out,'" although the amplitude of cyclical fluctuations between 1924 and early 1929 (when the report went to press) had been reduced. And he warned that while "forecasting is no part of the present task,... we should not close the record without noting that recent developments may appear less satisfactory in retrospect than they appear at present." He did not foresee the seriousness of the Great Depression that followed—no one could, if the causes of its severity still lay in the future, as later studies at the Bureau were to suggest —but he added that, "... we are leaving 1921 well behind us, and there are signs that the caution inspired by that disastrous year is wearing thin."

* * *

A major theme in the subsequent development of the Bureau was how Mitchell's plan for the business cycle studies would work out. Here I note only that within its initial decade the Bureau had

managed to publish, or was in the process of developing, a number of significant contributions to the subject—not a prime concern to most economists at the time. Also completed within the first decade, in addition to those already mentioned, were monographs devoted wholly, or in substantial part, to the relation to business cycles of trade union membership and of fluctuations in various categories of wholesale commodity prices; and a short volume on the planning and control of public works aimed at stabilizing employment, which went more deeply into the subject than could the chapter devoted to it in *Business Cycles and Unemployment.*

Work in process when the decade ended included Macaulay's study of interest rates and cyclical analyses, by a corps of statistical assistants (in accordance with Mitchell's ambitious plan) of many time series—reaching a total of nearly 500 by then. Also in progress was a study by Simon Kuznets concentrating on the seasonal variations that were being "removed" from the time series. This study, which reached publication in 1933 (but was done before Kuznets turned his attention to national income), was of interest not only because seasonal unemployment was a serious problem, but also because what the study told about the transmission of fluctuations between successive stages of production and distribution was instructive for the business cycle process.

Plans were being laid for a number of additional important studies. One concerned corporate profits, significant in Mitchell's—and almost every other economist's—conception of what happens during business cycles and during other types of economic change. Profits were also important for the measurement of national income. Data on profits, particularly for individual companies, were exceedingly sparse, however, before the revolution in public corporate reporting that was brought about by the establishment of the Securities and Exchange Commission. The plans called for digging into the files of public accounting firms for their audit reports, and those of the Bureau of Internal Revenue for tax returns. The work soon got underway, and the studies were published by the mid-1930s. Not until the late 1950s, however, could the Bureau return to the subject, when George Stigler compiled and analyzed rates of return on capital in manufacturing industries; and in the 1960s, when Thor Hultgren, in a monograph in the business cycle series, studied cyclical fluctuations in costs and prices as well as profits.

In 1929 there was also hope for a study, "international in scope, of factors affecting the level of prices, a study which . . . may throw light on the practical problem of stabilizing the purchasing power of the dollar, and of other monetary units." This study, too, could not get off the ground until years later, and then barely on an international basis. An unpretentious but essential *Technical Paper,* one of a series started by the Bureau in 1941, on currency statistics,

appeared first; and then a series of *Occasional Papers* on the monetary aspects of World War II were published. Only in the 1950s was something like a full-fledged—and now famous—project on the subject, by Milton Friedman in collaboration with Anna Schwartz, begun as one of the "auxiliary" business cycle studies. It was completed in 1982.

* * *

During its first decade, the Bureau had concentrated its attention largely on studies that are classified conventionally under the separate headings of national income and of business cycles—closely related subjects, however, because both deal with the money economy and provide useful information for reaching an understanding of how the level, fluctuations, and distribution of the nation's income are determined.

The Bureau also embarked on studies that would ordinarily not be subsumed under either of the two headings, but that also quite definitely bear on the workings of the money economy. The most prominent of these was a pioneering effort to explore, with the recently available statistical instruments, a subject that could be discussed only in general terms in Mitchell's business cycle volume: the price system and its internal structure. The first product of the project was Frederick C. Mills's *The Behavior of Prices* (1927); its cyclical content was briefly referred to earlier. It covered, in addition, the monthly, year-to-year, and regional variability of individual wholesale prices and marked a first stage in the empirical study of the "unresponsive" or "sticky" prices that were to attract so much attention during the Great Depression and the decades that followed. By 1940 Mills's book was judged to be one of the post-World War I period's "outstanding contributions" selected by the Social Science Research Council for intensive discussion in its series of "critiques of research in the social sciences." In accordance with the Bureau's policy, the book included a long appendix table describing for the first time the sources and detailed characteristics of the price quotations being collected by the Bureau of Labor Statistics. This was considered unusual at the time, because this type of information was not always recognized then, or even later, as essential in a scientific study of price behavior.

At the end of the Bureau's first decade, Jerome's research, on a subject that had originated in his concern with the effects of lessened immigration, was well along. High on its list of desirable projects the Bureau kept Jerome's work on the mechanization of labor (published in 1934), a subject also closely related, of course, to the studies of productivity.

Besides the requests for special studies already mentioned, others came to the Bureau during its first decade. The Bureau assented to two on aspects of philanthropic giving. This was a subject not

high on the Bureau's agenda, and there was some hesitation to take them on. But they bore on a significant aspect of the economy—philanthropy is certainly less prominent a feature of our money economy than is the price system; nevertheless, it is a feature, it is relevant to the size distribution of income, and it is an aspect of the government's relationship to the market economy. These studies also met a public need. For each, funds were proffered by a foundation already supporting the Bureau's general program, and competent staff were available or could be obtained. One study, published in 1928, dealt with philanthropic trends in a "typical" American city, primarily designed as an exploration of the data possibilities. The other, published in 1930, followed through on a national survey of corporate contributions to organized community welfare services. Philanthropic giving was taken up again by the Bureau in the 1960s, in a comprehensive statistical review of the entire subject.

* * *

The Bureau's program of research was stated initially in the general terms of its charter and bylaws, and in Mitchell's published views of what needed to be done. Looking back over the Bureau's first decade or so, one can see how the program was actually being defined. I have done so in some detail, precisely with that purpose in mind. In more specific terms, the Bureau's annual reports—and the introductions to some of the volumes published or approaching publication in these years, some of which I have quoted—state this. Even more cogent is the choice of subjects for research—which the Bureau undertook to study with its "general funds"; those that the Bureau selected from among the subjects brought to its doors, with "specific funds" provided; and those that the Bureau hoped to study—or may even have made a stab at—but then had to put aside until the resources of money, data, or required personnel might become available.

What is also revealing about the program is how the studies were carried out and how the results were presented—matters I have tried to illustrate. The latitude and time given the staff to follow their hunches; the expectation that each staff member would "do his duty" by the study for which he had assumed responsibility; the understanding that he would give as well as receive help on problems of data, analysis, and presentation; the adherence to standards to be met before publication could be approved—were also intrinsic parts of the program. In 1959 Oskar Morgenstern put the point about standards rather wryly when he mentioned in the preface of his book on *International Financial Transactions and Business Cycles* (one of the Bureau's business cycle monographs) that its publication would be a great relief to his wife. "... for many years [she] has heard me say that it was 'finished,' only to discover that writing a book is one thing but writing it for the National Bureau of

Economic Research with its exacting standards is quite another."

* * *

By the end of its first decade in 1929, what initial doubts there had been about the viability of an organization built along the lines of the Bureau had quieted down a good deal, and I will supplement the reasons already given or implied.

It had been demonstrated at the outset that a Board of Directors representing "so many and such divergent views of public policy" could meet in harmony. Nor was their harmony seriously disturbed by clashes over economic policy, since recommendations on policy were avoided. Dissents, if any, on other points or on a majority decision to publish a report, could be—and were, from the beginning—accorded space in the publication.

However, all members of the Board were busy with their own affairs, and many were little more than amateur economists. Would they take the time and make the effort to critically look over drafts of staff reports? Some reports were sure to be lengthy, and all were sure to include a good deal of technical material. The Board's task was made easier, first, because Mitchell set a high standard of exposition for the staff. Second, provision was soon made for summaries; these were helpful to the Directors (and other readers) whether or not time could be taken to read the full text of a report closely. Also helpful, a consensus reached early in the Bureau's Executive Committee made it unnecessary for the Director of Research to clear with the Board partial or tentative reports, on noncontroversial aspects of the Bureau's research, when these were designed to elicit professional discussion. Of course, provisional status of such reports was to be made clear in the publication. Finally, and of more than minor importance, the number of reports sent to the Board during the Bureau's first 10 years was small and the burden on the Directors was modest. The Board members could do the job expected of them. By 1941, when the number of reports requiring approval had grown large, the burden of approval was placed on ad hoc three-member committees of the Board, each selected for particular competence in the subject of the report involved. (If any member of the special committee were to withhold approval, however, the report must then be submitted to the full Board.)

There also must have been a question whether the staff, and the collaborators enlisted for special studies, would rise above their own views on policy. Of course, they were all free, as is every citizen, to express their opinions on current issues, and Mitchell and other members of the staff did not hesitate to do so—when they could still allay doubts and questions that were spurring their researches—but never as Bureau associates and never in the context of a Bureau study. One concern: In determining and presenting

"the facts" in a Bureau study, would researchers avoid even hinting at the policy conclusions they themselves favored? Mitchell could be pretty sure about the men he had picked for the staff and who were working closely with him. He could not have been so sure of the "outside" authors contributing to the two Hoover reports and the study of migration. But, as it turned out, they also toed the line drawn; Mitchell had no great problems with them and was sufficiently impressed with the experience he had with the first of these cooperative ventures to comment on it in his annual report.

* * *

The question of funding, when the Bureau began its work, was also important, and its answer was also uncertain. Rorty had accumulated barely enough money to get started. The funds and pledges in hand could hardly support a continuing research program. The uncertainties were multiplied because the Bureau's program did not directly address "problems that catch the attention of the public for brief periods," but rather fundamental subjects. Time and patience were required. Worse from the viewpoint of some potential contributors, the knowledge attained could not by itself resolve policy issues; it could only help to firm up the basis on which to consider them.

Yet funds did come forth for the Bureau's work, as they did, indeed, for the economic and social research of other institutions newly established shortly after World War I. However, I have the impression that it was easier to obtain funds, then and also later, for institutions less restrained than the Bureau with regard to recommendations on policy. However, the sources were not as widely dispersed as the Committee on the Distribution of Income had sought, although no large amounts came from sources that more than a few might view as questionable. In 1920 the Commonwealth Fund contributed $20,000 and promised another $15,000 for 1921. Small contributions by a number of business firms and individuals aggregated only about $6000 in 1920. Then, in 1921, the Carnegie Corporation, another foundation, granted the Bureau $15,000 for each of three years, 1921-23 (in addition to the funds it provided for the Hoover study).

Even more encouraging, in 1923 the Laura Spelman Rockefeller Memorial opened a full decade of annual contributions to the Bureau: $12,500 for the first year, and $25,000 for each year through 1932. Most important, when the Memorial was absorbed by the Rockefeller Foundation in 1929, the latter assumed and strengthened the Memorial's support of the social sciences, including the economic research at the Bureau. The Foundation committed itself to matching over the five years 1929-34 other grants received by the Bureau (including the remaining Memorial grants slated for 1930-32), to a maximum of $375,000. This immediately enabled the Bureau to move into fireproof quarters, where its files would be

safe, to equip its research laboratory more adequately for statistical work, and to enlist the larger number of persons required for an expanded program.

The small contributions by subscribers and others also had been growing (although they never reached a large fraction of receipts), and a trickle of receipts from the sale of books had begun when the first volume on national income was published. Total annual expenditures, then, which started with $19,000 in 1920 and $36,000 in 1921, became $85,000 by 1924 and $177,000 by 1929.

These were not large sums, certainly not in the first few years, even at the price levels prevailing during the 1920s. But the cash receipts were eked out. For the Bureau's first couple of years, quarters were provided rent-free by the General Theological Seminary, which had space available in its building on lower Ninth Avenue in New York City. (The Bureau had to supply its own heating equipment, however; and in partial return, lectures on statistics were delivered by the Bureau's staff to the seminarians—with what effect remains an intriguing question.) Also, as mentioned earlier, from the beginning the Bureau received from individuals and institutions a good deal of other noncash contributions in the form of research services.

Also important, none of the staff was paid more than a modest stipend by the Bureau. Most of the senior staff were employed on a part-time basis, as is the case today, with the expectation that the major portion of their income would come from their teaching connections. Although they put a disproportionately large share of their time on their researches, as scholars are prone to do, these were of value in their teaching, especially in graduate courses; and the universities, which expected their faculty members to spend time on research, encouraged their connections with the Bureau. In addition to the satisfaction the staff members received from doing scientific work rather than potboiling, to supplement their university salaries, they also enjoyed—and might indeed eventually profit from—the credit they received as authors. Had the reports been published simply as institutional documents—the widely followed custom among private and governmental research institutions when the Bureau was organized—to indicate that these were not merely the personal opinions of the authors, this credit would have been largely dissipated. The Bureau decided to depart from the custom on the ground (as Mitchell later mentioned with a twinkle in his eye) that authors given title-page credit and the responsibility that goes with it could be expected to do a better job than if they were more or less anonymous—and they would do it for less money.

* * *

Early in 1923 Mitchell had indicated to the Board of Directors a preference against rapid expansion of the Bureau, "or expansion to a scale very much larger than at present," that several Board

members and some others favored as the Bureau's reputation grew. "We are not ambitious . . . to become a very large institution, with a numerous staff, and with many different investigations constantly underway. If we did pursue a policy of ambitious expansion we might impose an intolerably heavy burden of reading upon the Directors; we would make the problem of financing the Bureau a harassing one; and we would run a risk of sacrificing quality of output to quantity."

Aside from these reasons, there may have been another at the time: With the business of the Bureau absorbing an appreciable portion of Mitchell's time, the pressures imposed by the first Hoover study still vivid in his mind, and with the continuing demand on the Bureau and on him and other members of the staff for studies, advice, and speeches, Mitchell must have felt some frustration in getting on with his research on business cycles.

This difficulty was soon removed, however. Gay, who had left Harvard in 1920 to become president of the *New York Post,* returned to Harvard in 1924 as professor of economic history. At the same time he joined the Bureau on a part-time basis as Codirector of Research, to relieve Mitchell of his purely administrative and editorial responsibilities (remaining until his retirement in 1932, when others took over the tasks). This relief, and Gay's effectiveness as an organizer and fund-raiser, lowered Mitchell's resistance to expansion, and the Bureau succeeded in meeting the problems Mitchell had enumerated. The Bureau grew, although hardly to the dimensions of "a very large institution, with a numerous staff." In its publications in 1929, the senior staff listed no more than a dozen persons.

By the end of 1929, then, the Bureau could feel itself past the most hazardous years of infancy, although not entirely secure; however, some funds were assured for at least a few years ahead. The staff assembled was working well as a team, and support was promised for a larger staff, particularly to carry on the business cycles study—whose dimensions were becoming recognized. The competence of the staff was widely recognized; indeed, requests to individual members for expert advice from official and scholarly bodies had started early in the 1920s, and the requests continue to arrive.

Some real accomplishments, consonant with the precepts originally set up, were on the record. A score of volumes were in print or soon to appear. And research plans, along the lines started during the Bureau's first decade, were being well received. Certainly, the "bold experiment" on which the Bureau had embarked could be said to have better odds in its favor at the end of the 1920s than when the Bureau was incorporated at the beginning of the decade.

* * *

It is also a fact that when 1929 came to a close, business activity was receding from the peak reached in August, as the Bureau was later to date it. The country was sliding into what later became known as the Great Depression, and the Bureau was soon in serious danger of going under. The Rockefeller Foundation had committed itself in 1929 for a five-year period, it is true. But it was largely on a matching basis, and as the recession deepened, less and less came in to be matched; this decline in funding occurred just when the Bureau's program expansion was raising its expenditures to new levels.

The need for more rather than less research on business cycles and other economic problems could not be more apparent, however, and the exceptional character of the Bureau's money-raising situation became recognized by the Rockefeller Foundation. The terms of its grant were revised. But this was not sufficient to prevent a severe decline in the Bureau's total income, with expenditures in 1933 and 1934 forced down to 60 percent of the 1929 level. Not until 1938 did income and expenditures return to and then exceed their 1929 level; voluntary and mandatory wage cuts could be ended; and plans for new studies, which had been suspended to assure the Bureau's financial ability to complete the studies already underway, could move forward again.

* * *

My objective has been to describe the establishment of the National Bureau of Economic Research and to tell how its role took shape during the Bureau's formative years. I shall stop here. How the Bureau's role was performed as the years and the decades rolled by—under Mitchell until his retirement in 1945, under Arthur F. Burns until 1967 (except for the three or four years when Burns served as chairman of the President's Council of Economic Advisers, and Geoffrey Moore and I carried on), under John R. Meyer until 1977, under Martin Feldstein until 1982, when he departed for Washington (also to serve as chairman of the Council of Economic Advisers), under Eli Shapiro until 1984, and again under Feldstein—this story is for other chapters in the history of the Bureau.

Even a partial roster of the hundreds of men and women who carried on the Bureau's work must also be deferred to those subsequent chapters. It is easy to remember those who became especially distinguished, and it is tempting to trot out the honors and awards conferred on them—the Nobel Prizes in Economic Science awarded Simon Kuznets, Milton Friedman, and George Stigler; the medals and distinguished fellowships received by them and other staff members; and the presidencies and other high offices held in the economic, statistical, econometric, and other professional associations. But the work of the Bureau was done also, and in a sense even largely, by many other staff members and a corps of dedicated

research assistants—all joined with the more prominent members of the staff and the Bureau's Administration and Board, in the effort to meet the Bureau's "exacting standards."

Even the bare list of hundreds of books, papers, and conference proceedings published by the Bureau since its early years now occupies a sizable printed document. There are many more publications on national income, business cycles, and the other research areas already mentioned. There is a host of reports on areas entered later, as the initial program of research expanded within the limits permitted by the Bureau's resources—resources that grew with the reputation of the Bureau for objectivity and thoroughness of work.

The program branched out in directions suggested by questions raised in studies completed or underway. The availability of "new ideas, new materials, and new experience" played a part. Of course, problems raised or old problems aggravated by economic, social, and political changes, and efforts to deal with them, also played their role. Whatever the conventional categories under which the studies may appear in a list of publications or programs—whether under financial markets and institutions, or fiscal policy, or international economic relations, or labor markets, or private pensions and social insurance, or education and human capital, or health economics, or technological change—the studies added as needs and opportunities presented themselves have all sought information applicable to scientific and policy issues concerning the growth, stability, and distribution of the nation's income.

The story of how the Bureau's collaborative efforts expanded in scope and frequency is also long. Collaboration with the universities took on an institutional character, going well beyond academia's necessarily limited representation on the Bureau's Board. In the middle 1930s, on the Bureau's initiative, the Universities-National Bureau Committee for Economic Research was organized, with some forty universities now represented. An offshoot of the Universities-National Bureau Committee, one that took on an identity of its own, is the Conference on Research in Income and Wealth. The 150 or more members of this Conference are individual economists or statisticians from federal, state, and local governments as well as the universities. The major function of the Committee and the Conference has been to arrange carefully planned conferences that bring together economists and statisticians from across the nation (and often from other countries) to discuss intensively particular issues in economic theory, statistics, methodology, and policy, and to contribute to their resolution.

Many of these meetings have been on topics of immediate interest to public officials, and some were arranged at their request—the conference on "Policies to Combat Depression" in 1956, for

example. In addition, over the years the Bureau has undertaken to meet other requests by government agencies—notably, to review the structure and functions of the statistical agencies of the federal government, and to appraise particular federal statistics such as those on national income, prices, inventories, and business cycle indicators. The Bureau has also cooperated with federal agencies when they were ready to compile and regularly publish economic data originally developed by the Bureau. "Diffusion" indexes, for example, now appear regularly in *Business Conditions Digest* along with the Bureau's business cycle chronology and threefold classification of cyclical indicators; and the elaborate flow-of-funds accounts, constructed at the Bureau by Morris Copeland in the 1940s and 1950s, is now published regularly by the Board of Governors of the Federal Reserve System. Also, as before, many Bureau staff members, and others with "on-the-job training" at the Bureau, have responded on a personal basis to calls by government agencies for professional assistance. Through these various connections, as well as through its publications, the Bureau has been able to help improve the quality of economic information available to the country and to influence the thinking of economists and noneconomists everywhere.

The precepts formulated by the Bureau's founders are stringent principles of action. It is not easy to concentrate on important economic problems while paying attention to basic processes and underlying factors and to the current manifestations of these problems, dealing with them on an empirical basis in a scientific manner, and doing all of this under the safeguards needed to assure impartiality. When circumstances are transitory, it is difficult to be consistent even in decisions on what is important, basic, scientific, and impartial. But it is not altogether impossible. When the full story of the Bureau is told (and one can view the experience not just of 10 or 15 years but of what is now more than six decades), one can see how the founders' principles were construed, as the generations succeeded one another; how the principles guided the work of the Bureau, under the pressures of events; and how the Bureau's organization and procedures were adapted to the Bureau's growth. Today, some $6 million are spent each year, to support studies carried on by well over 100 NBER research associates here and abroad, in projects managed or individual studies overseen by a dozen program directors; these funds are spent, as well, to arrange and underwrite scientific conferences involving an even larger number of persons each year.

ACKNOWLEDGMENTS

The major source of information on the founding and early years of the National Bureau of Economic Research are, of course, the Bureau's internal records and papers and its publications. Studies published by the Bureau, and cited or referred to in the text above, are not further identified below; they can be found readily enough in the lists of Bureau publications, of which the most recent edition is *NBER Publications: 1921-1982,* with its *1982-1983 Supplement.* The Bureau's Annual Reports also have been published, except for those covering 1920 and 1922-25. (The functions of the Annual Reports in years following 1977 were given over to a quarterly publication, *NBER Reporter.*) I have leaned heavily on these reports, particularly those (the 25th and the 29th) containing the essays by N. I. Stone and Arthur F. Burns, which I referred to above. Internal records I have used include the Minutes of the meetings of the Bureau's Board of Directors and Executive Committee, the Bureau's financial statements, and its files of correspondence.

I am also indebted to several Directors or Directors Emeriti for some personal recollections; and, of course, I have made use of my own. George B. Roberts kindly made available an informative letter written February 27, 1943, by his father, George E. Roberts, one of the founding members of the Board. I am grateful also for helpful comments on a draft of this paper received from Robert E. Lipsey and Eli Shapiro.

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1. The object of the National Bureau of Economic Research is to ascertain and to present to the public important economic facts and their interpretation in a scientific and impartial manner. The Board of Directors is charged with the responsibility of ensuring that the work of the National Bureau is carried on in strict conformity with this object.

2. The President of the National Bureau shall submit to the Board of Directors, or to its Executive Committee, for their formal adoption all specific proposals for research to be instituted.

3. No research report shall be published by the National Bureau until the President has sent each member of the Board a notice that a manuscript is recommended for publication and that in the President's opinion it is suitable for publication in accordance with the principles of the National Bureau. Such notification will include an abstract or summary of the manuscript's content and a response form for use by those Directors who desire a copy of the manuscript for review. Each manuscript shall contain a summary drawing attention to the nature and treatment of the problem studied, the character of the data and their utilization in the report, and the main conclusions reached.

4. For each manuscript so submitted, a special committee of Directors (including Directors Emeriti) shall be appointed by majority agreement of the President and Vice Presidents (or by the Executive Committee in case of inability to decide on the part of the President and Vice Presidents), consisting of three Directors selected as nearly as may be one from each general division of the Board. The names of the special manuscript committee shall be stated to each Director when notice of the proposed publication is submitted to him. It shall be the duty of each member of the special manuscript committee to read the manuscript. If each member of the manuscript committee signifies his approval within thirty days of the transmittal of the manuscript, the report may be published. If at the end of that period any member of the manuscript committee withholds his approval, the President shall then notify each member of the Board, requesting approval or disapproval of publication, and thirty days additional shall be granted for this purpose. The manuscript shall then not be published unless at least a majority of the entire Board who shall have voted on the proposal within the time fixed for the receipt of votes shall have approved.

5. No manuscript may be published, though approved by each member of the special manuscript committee, until forty-five days have elapsed from the transmittal of the report in manuscript form. The interval is allowed for the receipt of any memorandum of dissent or reservation, together with a brief statement of his reasons, that any member may wish to express; and such memorandum of dissent or reservation shall be published with the manuscript if he so desires. Publication does not, however, imply that each member of the Board has read the manuscript, or that either members of the Board in general or the special committee have passed on its validity in every detail.

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7. Unless otherwise determined by the Board or exempted by the terms of paragraph 6, a copy of this resolution shall be printed in each National Bureau publication.

(Resolution adopted October 25, 1926, as revised through September 30, 1974)