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Introduction

Economists everywhere know Simon Kuznets for his contributions to their science. Business cycles, seasonal variations in industry and trade, national income and its distribution, capital and capital formation, economic growth—each of these subjects, in turn, and others as well, “has been the object of Kuznets’ attention, and each has been shaken and advanced by some fresh and penetrating study. To few men is it given to make a truly significant difference in the state of a science, but Kuznets did it more than once.”¹ In quantitative economic research he is, indeed, the outstanding figure of our generation. The reflections he offers here on trends and problems in his field of scientific endeavor are therefore sure to command the attention of his professional colleagues.

Others may be led to join Kuznets’ audience by the news of the award to him of the 1971 Nobel Prize in Economic Science. For them, a few additional remarks by way of introduction may be appropriate. First, then, on the occasion of Kuznets’ paper.

The National Bureau was founded, half a century ago, for the objective determination and impartial interpretation of the facts bearing upon important economic problems. There must have been real questions at the time whether this standard could be met and, equally essential, whether the Bureau’s findings could be made to “carry conviction to all thoughtful men, however divergent their views of public policy.” Yet the Bureau’s

¹ Moses Abramovitz in *Science*, October 29, 1971.

Introduction

statement of principles, from which I have been quoting, boldly declared these to be the aims of the new venture.

That the Bureau did succeed in attaining its high objectives is clear. It is evidenced by the wide use made of the long list of Bureau studies by persons with diverse views on policy. It is evidenced also by the Bureau's ability to enlist growing support for its work from a wide variety of sources. In this accomplishment the many distinguished persons who gathered together at the Bureau's fiftieth anniversary meetings saw cause enough for celebration. Coming at a time, as Kuznets observes, when despair concerning "the relevance of reasoned and objective discourse to the solution of social problems" is apparently spreading, their participation in the anniversary meetings constituted a declaration of faith in the purposes and uses of objective economic research. And it constituted also a tribute to the Bureau's founders, the men who set its goals and designed the organization under which the goals could be brought within reach, the men who set an enduring example, in their objectivity, standards of workmanship, and willingness to reason together, for all who came after them.

The Bureau's fiftieth anniversary was also an occasion for looking ahead. This was a major purpose of the colloquia, listed elsewhere in this volume, in one of which Kuznets presented his paper on quantitative economic research.

But why single out quantitative research? There is a place in all scientific undertaking for theoretical speculation. In the end, however, theory must be made to stand up to the test of conformity with experience or experiment. As Kuznets underlines, it is only *tested* theory that can carry conviction; it is only tested theory

Introduction

that can provide the solid ground in which sound policy must be rooted. To be useful, then, economic research must be empirically oriented.

But this is not sufficient. The choice among economic policies always means weighing alternatives. And weighing alternatives requires determining the net balance between forces working in opposite directions. Estimates must be made of the strength of these forces, and of the magnitudes and timing of their direct and indirect effects. To be of more than minimal value, objective economic knowledge must be quantitative in character.

That is why the National Bureau has concentrated its efforts on quantitative economic research. It is for this reason also that, in planning the discussion of its future work, a broad survey of trends and problems in quantitative economic research seemed eminently suitable for one of the Bureau's anniversary colloquia.

As might be expected of an economist who has made major advances in the design and use of economic measures, Kuznets has a good deal to say about these measures and about the statistical data on which they are based. Particularly, he emphasizes the dependence of economic measures upon the social philosophy of the time and place to which they refer. It is especially noteworthy, at a time when the usefulness of such measurements as GNP is in question, that Kuznets has always recognized this dependence. His discussion is not a belated response to the current wave of criticism. The National Bureau's assessment and reassessment of measures of economic and social performance now under way are within a tradition established at the Bureau decades ago.

Also noteworthy is Kuznets' judgment that in far too few of the econometric studies now flourishing is suffi-

Introduction

cient attention devoted to the quality of the statistics fitted into the equations. His insistence that truly scientific quantitative economic research requires a more critical use of the statistics may serve to give pause to econometricians. Required also, in his view, is that they should go beyond the readily available statistics; a larger fraction of their resources should be used to dig up more of the basic material they will process. As important is Kuznets' opinion that econometricians could be more mindful than many of them are of the extent to which their results are influenced by the assumptions they build into their equations.

Kuznets is not content to speak of quantitative economic research in general terms. He illustrates many of his points by reference to the particular subject of economic growth. This is the subject to which he has devoted most of the past two decades and for which he was especially cited by the Swedish Academy. It is therefore not surprising that he should stress its importance and go on to suggest that the National Bureau might seriously consider building its program of research around economic growth as the central theme.

Economic growth has always been a major area of research at the National Bureau. In its very first study, of national income in the United States, one of the objectives was to learn something about the long-term rate of growth. Many subsequent studies at the National Bureau—Kuznets' own study of *National Product Since 1869* and his *Capital in the American Economy*—are outstanding examples, and there are also the National Bureau's studies on mechanization, productivity, employment, and production—all aimed especially at economic growth. In addition, many of the Bureau's other studies, human

Introduction

capital, for example, have in large or small degree contributed to the knowledge being accumulated on the economic growth of the United States.

Kuznets is not alone in stressing economic growth as a subject for the National Bureau's program. Many of the suggestions on the Bureau's future program made in the other colloquia bear on subjects closely related to economic growth. The fifth colloquium, of course, concentrated on this subject. And a major theme of the first colloquium, *The Business Cycle Today*, was the shift in concept of business cycles from the older idea of an absolute up and down movement in output, employment, and other economic quantities to one of fluctuations in rates of growth.

No doubt, then, economic growth will continue to be a major area of research at the National Bureau. Whether the time is ripe for making it the central theme, as Kuznets believes, is a question that will be given serious attention, along with other questions on the Bureau's program raised in the various colloquia.

Kuznets suggests, further, that the study of economic growth might best be pursued on a comparative international basis. The possibility is intriguing in a world in which concern with the economic status of fellow human beings abroad is greater than ever before. But, as he goes on to say in his paper, the data for most countries still leave very much to be desired. To develop anything even approximating the quantity and quality of information for other countries that has been developed for the United States would be a task far beyond the resources of any single research institute now in existence. What may turn out to be most practicable is a cooperative arrangement among institutes of various countries. An

Introduction

effort along this line—a study of the diffusion of new technologies—is now being tried by the Bureau. Experience to date with this and other cooperative ventures on an international basis makes for some optimism about the possibility.

* * * *

The human affairs that economists study are important: the quantity of goods and services produced and consumed—their quality, rate of growth, stability, and distribution among the people; the conditions and opportunities of life and of work—how the standard of living is affected by the size of population and how the population is affected by the standard of living; under what social and political arrangements “the ordinary business of life” is best carried out. These and other economic questions have deeply concerned men for centuries. To cast light on these questions economists devote their hours and their years to research. They hope in this way to make economic policies better than they would otherwise be—policies that men are impelled to try, in their persistent efforts to raise economic welfare.

“Much has surely been learned; a vast stock of relevant economic measures has accumulated; the inventory of relatively firm empirical findings has grown; and a host of theoretical hypotheses has been advanced. . . .” But Kuznets hastens to add to this judgment significant caveats. Many of these hypotheses are “too simple and partial to be valid without major and, short of wider study, unspecifiable qualifications. . . . We are still far from a tested theory of economic growth. . . . Many questions remain that demand intensive exploration and

Introduction

at least provisional answers as necessary elements even in a tentative understanding of the growth process." The remarkable postwar record of successful economic performance, Kuznets observes, "is not necessarily evidence that we understand the process. . . . Paradoxically, the number of questions and the variety of what may be seen as policy problems may be greater . . . than in the 'good old' days when both study and growth were relatively stagnant, economic growth was viewed as a process much beyond the control of man, and its low rate did not produce the unsettling impact of rapid structural changes."

There is much to be done. Difficult questions plague us—questions on economic growth, and also on business cycles, finance and capital markets, industrial organization, public expenditures and taxation, and human resources, to list the subjects of the National Bureau's colloquia. Kuznets' observations on trends and problems in the field of quantitative economic research should help economists working on any of these subjects. And they should help economists working under any auspices, in the tasks they tackle. But Kuznets sees the responsibilities and opportunities of economists working together in nongovernmental public research institutes to be especially great. This kind of institution, he says, "is not only a research laboratory for the specialist, but also in a way a finder and keeper of truth for society at large; a producer of tested and acceptable measures, and a source of the balanced evaluation and judgment that should facilitate social consensus." If Kuznets is right in this judgment, the opportunities open to the National Bureau

Introduction

in the half-century ahead are not less attractive than those of the past half-century; and the responsibilities entailed, not less worth shouldering than those the Bureau assumed half a century ago.

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