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PART I

**The
National Bureau
Enters Its
Forty-Fifth Year**

As we begin our review of the year's work and plans for the months ahead, it is well to remind ourselves—and inform our new and prospective friends—what the National Bureau of Economic Research is and what it strives to do.

Let us recall, then:

that the National Bureau was organized in 1920 in response to a demand for objective determination of the facts bearing upon economic problems, and for their interpretation in an impartial manner;

that the National Bureau concentrates on topics of national importance that are susceptible of scientific treatment;

that the National Bureau endeavors to maintain scientific standards in all the studies it undertakes;

that the National Bureau seeks not only to determine and interpret important economic facts, but to do so under such auspices and with such safeguards as shall make its findings carry conviction to all sections of the nation;

that, in this way, the National Bureau hopes to aid thoughtful men, however divergent their views of public policy, to base their discussions upon objective knowledge as distinguished from subjective opinion.

To maintain scientific standards in its research, the National Bureau tries to be thorough in the collection and discriminating in the use of relevant evidence. Hypotheses concerning economic relations are tested for conformity to actual experience. Detailed information on sources and methods is provided along with the findings of each study; and appropriate qualifications concerning the reliability and limitations of the results are also included.

To make its findings carry conviction to all sections of the nation, the National Bureau publishes the findings in the form of scientific reports, entirely divorced from recommendations on policy. To this end, also, each study must be submitted to appraisal by the Research Staff, and often by outside experts as well, before being considered for publication. Of course, long before a study is completed it is the duty of the Director of Research to call for formal and informal staff discussions of

each aspect of the work. At every stage, suggestions are offered and criticisms made on the organization and staffing of a study, on the particular questions to be investigated, on the hypotheses to be tested, on the data to be sought and analyzed, on the technical methods to be utilized, and—whenever necessary—on changes in the plan and organization of the study. As a final safeguard, no report may be published without the approval of the Bureau's Board of Directors, which is made up of men with various backgrounds. Also, it is always made clear to present or future contributors that the Bureau assumes no obligation toward them except to determine, interpret, and publish economic facts for the benefit of the nation at large.

Because the National Bureau wishes to aid all thoughtful men in their discussions of economic policy, an effort is made to report its major findings in language intelligible to the educated layman, to the extent compatible with scientific and technical character of its studies. As the findings of a study emerge, therefore, attention is paid by the staff not only to the substance and validity of the findings but also to the form in which they are set forth.

This, in brief, is the objective and the course of action pursued by the National Bureau.

The founders of the National Bureau who adopted the basic elements of this policy forty-four years ago were persons of widely divergent views on questions of economic and social policy, as the membership of the Bureau's first Board of Directors clearly reflected. All also were men of "judicial habit of thought," who understood research of a strictly scientific type, who felt that research of this type was possible in economics, who believed that such work could be of great practical value to society at large.

At the time, there was room for doubt whether the venture would succeed. Were there an adequate number of kindred spirits who might join the founders to make the National Bureau a strong and enduring instrument of social progress? Could the National Bureau

find the financial means to carry out its work? Would people be willing to support research that might or might not confirm the opinions they held? There may have been some doubt, too, whether the restraints and balances to be imposed by the Bureau's policy could be tolerated by a staff eager to devote its talents and training to the improvement of economic welfare. And, notwithstanding all the safeguards built into the Bureau's procedures, no one could be sure that, once the enterprise was under way, the declared policy could be pursued, in the dust and heat of the daily work, with the spirit and at the level of excellence hoped for.

As it turned out, there were enough sharing the founders' vision to get the enterprise off to a start. The National Bureau was able to meet the test of survival and growth while following the unusual and difficult policy established for it. During the forty-four years since its foundation, the Bureau has found it possible to undertake and complete, and its Board of Directors to approve, as being in accordance with the Bureau's policy, 375 reports—150 book-length studies and 225 shorter monographs, papers, and pamphlets. The National Bureau has also been able to organize 50 major conferences and to publish the proceedings for use by a wider audience.

Even more significant than the Board's approval of this work, as the Board itself would be quick to recognize, has been the approval registered by the public. Students of economics and interested laymen, including high government officials in our own country and abroad, have commonly accepted and used the Bureau's results in dealing with important issues of public and private economic policy. It has been gratifying, also, that visitors from overseas have come to us in increasing numbers to seek our advice on organizing independent economic research in their own countries.

In 1920, the Bureau started with a budget of some \$25,000. Shortly after World War II, annual expenditures averaged about \$450,000. The budgeted expenditures for 1964, approved by the Board, total \$1,400,000. The Bureau has a large staff and many collaborators de-

voted to the scientific analysis of the nation's economic problems, as the list of authors of work in progress given at the end of our report testifies. While the Bureau has no endowment, its friends have helped it to accumulate a reserve fund that, though still modest, serves to lessen the hazards and avoid the inefficiencies of hand-to-mouth financing. To its roll of supporters the Bureau has been able to attract foundations, individuals, companies, business associations, and labor unions, whose regular annual contributions now support a good fraction of the work. The Bureau can therefore plan its operations with some care and assure its staff of the security essential to careful work.

It is a good record. Indeed, we might, after a bow to our predecessors, congratulate one another on its more recent pages.

But we should apply to this assessment no lower standards than we try to apply in our scientific work. It is a favorable sign that the Bureau has continued to grow. It could be, however, that the Bureau has expanded in recent years, as other economic research institutions and the economics profession generally may have done, chiefly because of a sharply rising demand for economic information. Public acceptance of the results of our work is vital, and this continued acceptance is reassuring. But there are lags: our reputation today might be more a heritage of the past than the reward of our present efforts. We cannot take for granted that we are now keeping as closely as we could to the policy set by the founders of the Bureau, or maintaining as well as we should the standards of excellence they sought to attain.

The policy of the Bureau established forty-four years ago was unusual at the time. It is still unusual, and to adhere to the policy is no less difficult now than it was then. At every year's meeting, therefore, we need to ask ourselves whether we may not be doing properly the work that the Bureau was organized to do—either because we are trying to do too much, or because decisions are made with insufficient deliberation, or because of weaknesses in the research staff, or because our procedures have become lax. In research, as in

other human affairs, men often find themselves giving way to this or that immediate consideration even when it runs contrary to their long-run objectives. Sometimes, indeed, it is necessary to compromise. But it is not always necessary, though it is always easy, to do so. We must strive for excellence consciously and deliberately.

Before we turn to the detailed reports on the work and plans of the Bureau, then, let us reflect on our responsibility to uphold, and if possible to enhance, the reputation left in our care. We may sharpen our vigilance if we understand the difficulties that need to be overcome—not once and for all, but day in and day out.

In choosing topics for research, we concentrate on what are clearly major problem areas: the economic growth of the country, the stabilization of the rate of employment and of price levels, the country's international economic relations, the impact of government activity on the economy, the economic intelligence needed by persons in public and private positions of responsibility. Within these areas, we prefer to select subjects for investigation—the new study of productivity in the service industries is an example—that are related in one way or another to those on which we already have done some work. For then we have a clearer idea of the importance of the new subjects and how to tackle them; the topics may be seen in better perspective. This principle of choice helps us also to make our results stronger, for related studies test and illumine one another. But the principle has its dangers, against which we must guard. Of course, we take care to avoid repetitive work; but we must also avoid continued cultivation of areas that yield returns smaller than might be obtained by turning to altogether fresh ground.

In part, our program is determined by the opportunities we discover as data or personnel or funds become available for studies we deem important but have had to forgo. We need to seize these opportunities. We need also to prevent a loss of balance in our program. We must keep in mind, as well, that to suppose that a fact, or factor, or relationship is important and

worth study is to propose a hypothesis. The more carefully formulated the hypothesis, the greater is the chance that it will survive testing. The better we are prepared, the better we can choose among opportunities as they unfold.

For these and other reasons, we do not rest solely on our own judgment as to what is important and feasible. When we can, we check our opinions by soliciting the advice of knowledgeable men in academic, business, financial, and government circles. Sometimes this is arranged formally through an "exploratory committee" that will issue a public report. A current example, recently submitted to the Board, is the exploration of subjects for research in the capital markets, which we hope will be of value also to other investigators. This does not absolve us of responsibility for deciding what subjects we are to study, but it lessens the risks of choosing unwisely.

So, too, with the decision that a topic is susceptible of scientific treatment. By its very nature, research is a venture into territory that is largely unknown, and this must be true however carefully the ground may have been scouted in advance. No one can be sure that scientifically valid results can be won. We pass by topics when our experience, and the experience of those we consult, suggests no way to treat the subjects scientifically. But there is hardly any subject without some aspect amenable to scientific treatment. When the topic is of the highest importance, like the relation between taxation and economic growth or the causes of severe depressions, even a few limited aspects of it will be worthy of study, and even negative results may have some value. When we tackle such a subject, we need to take exceptional care to maintain scientific standards, and the sponsors who support us in such enterprises must be made keenly aware of the special hazard that the contribution to knowledge may indeed be small.

As for the maintenance of scientific standards, that also is a matter of judgment and degree. Thoroughness in the collection of information demands a price in time and effort which, beyond a point, may be judged excessive. But we should not hurry to take shortcuts.

It is our policy not to be satisfied merely with proposing interesting hypotheses. Yet tests of hypotheses can never be exhaustive and at some stage one must stop with a list of some of the tests that remain to be made. But we should not be satisfied with casual or superficial testing before we reach that stage. In a science in which the opportunity for controlled experiment is rare and use must often be made of defective data, even a negative result may sometimes properly be viewed not as ground for discarding a hypothesis but rather as a suggestion for later study of the nature of the experience that appears to be in conflict with the hypothesis. This attitude of tolerance toward negative results, however, should also be kept within modest bounds.

The presentation of information on data and sources and methods also involves a judgment on what is necessary and sufficient. Some people think we devote too much space in our studies to information of this sort; but it is better to give rather too much than too little information, and to relieve the reader who wants only the findings by putting details into appendixes which he can easily ignore. Qualifications and reservations concerning the accuracy and limitations of findings can be excessive as well as inadequate: we should seek the middle way. A reservation that is serious for some purposes may be petty for others: we should try to be specific.

For the various reasons mentioned, appraisals of findings and of their scientific validity are seldom unanimous in economics, even—as we all know—after years of study and discussion. This is more frequently so in the short run also because human judgment is fallible and people differ in the care and objectivity with which they approach a new finding and form their opinions about it. The decision to publish or not to publish is therefore sometimes very difficult. Fortunately, the balance of opinion is clearly on one side or the other, as a rule. When it is not, we must be sure to take the time and trouble to solicit and await further opinion before coming to a decision; or, if the author's patience is exhausted, decide against publication.

The National Bureau's findings are always published in the form of scientific reports with no recommendations on policy. To separate the two is difficult because every economic study worth making has relevance for policy and touches, even if indirectly, on immediate or prospective issues of policy. Mere emphasis or lack of emphasis on particular events or facts or relationships, when presenting the findings of economic studies, may imply a judgment about policy. Indeed, because practically every economist is eager to strengthen the country's economy, his personal policy position—which is often rooted deep—may tend subconsciously to influence the direction of his research and slant his pen. The danger is especially great when his study is aimed at the analysis of particular policies. This source of bias we try hard to guard against. Recognition of the fact that it is difficult to do so should only strengthen our resolve.

Nor is it easy to present scientific studies in words intelligible even to the more thoughtful citizens whom we try to serve. Few men able to do good research have the ability to communicate their thought to a large audience. Nor do they always possess the reserves of patience required to take editorial advice, especially in the final stages of what may sometimes seem like an endless series of demands for evidence, for rigor in logic, for addition or deletion, for tests of all kinds, and for revision or clarification. It is always a question therefore how far to go and how much effort to spend in editing and re-editing a manuscript. Perhaps we should devote a larger proportion of our payroll to editing and closely related duties than the present one-fifteenth. It may be possible to do so profitably, and it need not make for intolerable delays.

Approval by the Board of Directors is a valuable safeguard built into our procedures, which serves to protect our standards even before approval is requested. While the burden of formal approval has become so great, with the increase in the Bureau's operations, that it has had to be divided among the members of the Board, copies of reports recommended by the Director of Research for publication are

sent to all Board members. All may read and comment and suggest revisions, and few studies have failed to benefit from this advice and assistance. Comments and reservations by any of the Directors may be printed along with the final reports.

Because our Board members are all busy men with other obligations, it is possible that a study could be approved despite deficiencies in one or another respect that cause it to fall short of the standards we should be maintaining. If and when that happens, the Director of Research is particularly at fault in recommending publication of a study that did not deserve, or did not yet deserve, the Bureau's acceptance.

The National Bureau's procedures and safeguards reduce the likelihood of such errors, and today's discussion of the dangers all along the line may serve the same useful purpose. We could reduce the risk still further by undertaking fewer studies and concentrating our limited time and manpower on them. Or we could seek, even more energetically than we now do, to increase the resources at our disposal. Or we could try to follow both paths. To undertake fewer studies when the need for objectively determined economic knowledge is greater than ever is not an attractive solution but, if the resources available to us cannot be expanded sufficiently, it may be the only proper way.

I have tried to indicate why we need always to concern ourselves with the maintenance of standards. This is not because we are failing to maintain them. It is because we want to prevent such failure and because we want, to the extent possible, to raise our standards still higher. I hope the Board will scrutinize the Bureau's current work and procedures and plans with these questions in mind.

To assist the Board in its task, the research staff has taken the pains to report in some detail on the studies in process and on the work begun or contemplated. These appear in the list of Studies Completed (Part III), the Staff Reports on Research Under Way (Part IV), and Conferences on Research, held and planned (Part V).

In addition, I have asked several members of the staff to prepare commentaries on the groups of studies in which they are playing leading roles—the studies of money supply, of interest rates, of business fluctuations, and of international economic relations. Their reports illustrate, in more than one way, the many

difficulties to be overcome by men who try to do scientific work in economics. The reports also indicate, I think, that these difficulties can be overcome by honest work, that this work does yield good fruit. Objective economic research is not easy to do, but it can be done.

SOLOMON FABRICANT