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Whither Now?

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I

FROM "the land of the lotus eaters" (as Wesley Mitchell called it when I moved to California), from the Far West—now an overnight flight from New York—I bring my respectful tribute to the National Bureau of Economic Research and to the dean of American economists who was its Director of Research for twenty-five years.

When I was a young assistant in elementary economics, I sat at Professor Mitchell's feet while he lectured for six weeks to several hundred Harvard sophomores. At the end of *every* lecture the unusually silent class broke into resounding applause at his performance—unpretentious but transparently clear, unexpectedly interesting, and highly effective. In my years of observation of undergraduate behavior, this student response stands out as unique. Americans are commonly backward in expressing appreciation, and in this respect I sometimes feel that we have definitely lost ground in the past thirty-five years. But I now join with you in voicing the inarticulate sentiments of affectionate esteem and admiration that are genuinely felt by great numbers who have come under the influence of this man and his work. Listen! From near and far the applause reverberates, not loud but deep.

The National Bureau is far more than the lengthened shadow of Wesley Mitchell. To this able group of scholars the economics profession, the world over, is profoundly indebted. They have chosen significant fields of work and cultivated a broadening perspective. They have not shrunk from unspectacular, painstaking years of drudgery which yielded only modest fruits. They have been highly productive in organized data, evolved techniques, and finished results. They have kept 'scientific in method and spirit', and practiced the difficult art of inviting and welcoming criticism instead of resenting it. By example, indirect influence, and development of trained personnel, as well as by specific conferences and publications, the National Bureau has made invaluable contributions to progress in economic research. Their total influence on the world economy, indeed, is of no mean importance. As one long identified with a slightly younger and much smaller research institution, very different in setup, scope, and program but fortuitously similar in basic aims and guiding principles, I count it a privilege to bear testimony to this effect.

No one who looks back to the condition of economic research and its output at the end of World War I can fail to be impressed by marked improvements—in the volume and quality of available economic facts, in the understanding of economic structure, flows, and forces, and in the kit of usable tools and techniques. Relatively few economists today are content to do 'ivory-tower thinking' without some delving into 'realistic economics', however it be defined. A far larger body of economists is trained for research, even if in number and quality it seems altogether inadequate to the tasks that stare us in the face.

We are not wholly free of the fault remarked by Albert Guérard, my literary colleague, who wrote a few years ago: "Much of our research is but an arduous flight from the necessity of thinking." We may well ponder Goodwin Watson's stricture, in his provocative presidential address to the Society for the Psychological Study of Social Issues:

"It is hardly too much to say that the accepted patterns of research have one feature in common: the expenditure of considerable time, valuable intellect, and almost incredible patience upon questions that matter very little." [1]

Whatever may be true of other disciplines, economic research in recent decades cannot fairly be charged with having investi-

[1] Address of August 31, 1937, in the Society's *Bulletin* (1937), II, 1-2.

gated many questions of little or no significance; but in respect of choice of the *best* questions, and of waste in our investigations, we are certainly vulnerable.

I count it an important gain that most of our research nowadays is directed, not to economic statics, but to analysis of dynamic changes. The London *Economist* gives the late Lord Keynes primary credit for this shift;[2] but I am sure that American research institutions contributed much in this direction before Keynes' *Treatise on Money* appeared in 1930. We have already progressed beyond the stage of which Thorstein Veblen cynically wrote, in 1924, of 'Economic Theory in the Calculable Future':

> ". . . economic science should, for its major incidence and with increasing singleness and clarity, be a science of business traffic, monographic, detailed, exacting, and imbued with a spirit of devotion to things as they are shaping themselves under the paramount exigencies of absentee ownership considered as a working system."[3]

In most of these respects, the National Bureau has made signal contributions to the gains that have been registered.

It is sad to see good words degraded by evil associations. Hitler *der Führer* debased one of our prize words, 'leader'. For me the word 'mature' has been poisoned by its use in the phrase 'mature economy' with the meanings often misread into that phrase. Growth in many significant senses is characteristic of individuals, associations, and nations long after they pass from youth into maturity. In the older, sounder sense of the word, let us take pride in asserting that the United States *is* a mature economy. In this same old-fashioned sense, I acclaim the National Bureau on reaching maturity, with the best of its prime of life yet ahead at twenty-six, with powers still to be developed, and with decades of enlarging productivity in prospect before it reaches—if it ever does—the ripe old age that precedes senility.

[2] In its lead article on 'John Maynard Keynes', issue of April 27, 1946, p. 658.

[3] *Essays in Our Changing Order* (Viking, 1934), pp. 11-2. This paper, given in December 1924, was first published in March 1925.

Whither now? What tasks lie ahead? How can those who are devotees of economic research best attack and accomplish them? And what *is* the relation of economic research to the development of economic science and public policy?

For one who has spent most of his mature life in an overlapping succession of economic researches, it might seem a simple matter to distill the essence of his experience and bring it to bear on this challenging topic. But the task calls for talents quite different from those that are exercised in specific researches. With these intellectual muscles relatively undeveloped, I confess myself unequal to the challenge. I can merely throw into the common pool a few observations that seem to me germane to this discussion.

II

What right have we to use the term *economic science*? In concluding his last report as Director of Research, Wesley Mitchell said of the National Bureau:

> "We like to think of ourselves as helping to lay the foundations of an economics that will consist of statements warranted by evidence a competent reader may judge for himself. But it would be wishful thinking to expect that progress toward that goal will be rapid. . . ." (*The National Bureau's First Quarter-Century*, p. 40.)

Unless he was excessively cautious, he presumably meant that economics as a science is yet to be born, and that he and his colleagues have been striving to prepare the way for its birth. Yesterday he virtually reasserted this view, and it seems uncomfortably near the truth.

So far as there is a science of economics today, it consists chiefly, I think, in the presence of a considerable group of workers who are scientists in aim and method.

The goal of science is understanding: knowledge and wisdom —ever larger, truer, and more penetrating. Assiduous striving toward this goal—intelligent, ingenious, objective, persistent search for significant but elusive truths, uninfluenced by pressures or temptations, by hope of reward or fear of consequences —is one mark of the scientist in any field. By no means all who are called economists are scientists in this sense, and most of those who are can devote only a fraction of their time to what is strictly scientific work. But in these respects we are in a much better state than we were when the war baby of World War I, in whose honor we are meeting, was posthumously born.

We can also take satisfaction in the accumulating results of employing scientific procedures in the attack on selected problems. This involves testing and cross-testing materials, methods, and preliminary results. The immediate products are reasonably ample facts, well sifted, skillfully ordered, and interpreted with accuracy and insight. By such procedures, in economics, valuable partial or intermediate results have been accumulated, and certain limited areas explored with some approach to definitiveness.

All this is to the good. It is a necessary part of the foundations of economic science. But in any larger sense, I venture to say, the 'economic science' of 1946 hardly deserves the name. Is this shocking—170 years after Adam Smith's *Wealth of Nations* appeared, and twenty-six years after the National Bureau started its career? Let it shock and continue to disturb us until it is no longer true.

We can indeed point to a still growing literature on the complex history of evolving economic thought, massive accumulations of economic data, multifarious articles and studies that few of us have time to read, and manifold terms, devices, techniques, and formulas undergoing continual proliferation or refinement. But few of our concepts are yet really well conceived, clarified, and agreed; our abundant data are still inadequate, imperfect, and ill-coordinated; and our established principles are conspicuously scarce. Even today, economists are prone to go off in all directions, to prize being different above being right, to follow fads while slighting fundamentals, and to shirk the disagreeable chore of working through to a consensus. Important as the contributions of many individuals and groups are, the grounds for justifiable attack upon economists as a profession are uncomfortably numerous.

The search for economic laws, applicable even within limited 'universes', has gone out of fashion; and the painstaking efforts of a few scholars in this field are generally ignored. "Even the 'theories' most fashionable today", Wesley Mitchell wrote of business cycle theories in his annual report for 1943 (*Economic Research and the Needs of the Times*, p. 36), "are really untested hypotheses." Would that this were true in no other field of economics! Yet I suspect that opportunities were never larger for reformulating old and discovering new economic and statistical laws, and subjecting such provisional statements of tendency to test for verification, disproof, or refinement.

Can we point to any significant body of well-tested principles and generalizations, stated with substantial precision, accepted by virtually the entire profession, and capable of serving as a solid base for further advances? Other sciences can and do. Whatever the hypotheses awaiting test, however large the realm of uncertainty, this much their scientists can build upon and work with as they proceed. Until economics reaches such a stage, many of us will continue to squirm when we or others speak of economic science except in the future tense; and the case is not perceptibly better for other social sciences.

In my opinion, however, the elements of such a corpus of economic science exist—perhaps no more incomplete and imperfect than were those of several natural sciences fifty to seventy-five years ago—and only await some integrating and refining master hands. This is no chore for young textbook writers, welcome as their synthesizing efforts may be. It involves a good deal more than making a patchwork quilt out of remnants of old garments and pieces of new cloth. It is itself one of the highly important tasks confronting mature economic research. It is with this enriched meaning that I emphatically concur in Mitchell's opinion (*ibid.*, p. 18): "Some scheme of integrating researches is requisite to orderly thinking and the growth of knowledge."

While this task is not typical of those that have mainly engrossed us, progress toward accomplishing it is essential if we are to approach these representative problems in such ways as to contribute most effectively to the growth of genuine economic science. Concentrated efforts in this direction will reveal many gaps to be filled, many uncertainties to be ironed out, many specific problems we do not yet know how to tackle; but we sorely need the perspective and the rough structure it could yield. There will never be any substitute for detailed, thorough study of small segments of economic problems, but priorities can best be decided and the more significant gaps most quickly filled if we have a growing structure of an evolving science, however limited it may be at the outset.

If economics is to deserve recognition as a science, even in the modest sense of orderly arrangement of tested knowledge, we need to do much better in choosing and clarifying elementary concepts, standardizing terms, and becoming more explicit and consistent in our use of both. To a degree inadequately appreciated by economists in general, our practices in this respect are unscientific and the consequences are unfortunate. Merely for examples, take national income, purchasing power, consumption, propensity to consume, and standards of living. The more I delve into the literature on these subjects and the tables of data involving them, the more disturbed I become. It is not enough 'to define concepts meticulously'; to define only what seems susceptible of measurement and to ignore the rest seems indefensible. Terms should be appropriate and meaningful, and it is dangerous to give common terms a technical meaning that confuses or misleads the ordinary reader or the nonspecialized economist.

Are we really satisfied with definitions of 'national income' that exclude large amounts of real income, and with the accompanying disregard of unmeasured variations in the magnitudes excluded? Should we not be acutely aware of grave dangers inherent in treating purchasing power as if it were merely what is called 'disposable income'? and in building and using money aggregates that ignore large inter-group differences in the purchasing power of the money unit, such as those between the income dollars of the farmer and the metropolitan? Is it not highly confusing to treat 'consumption' as if it were identical with 'consumer expenditures' for certain classes of commodities and services? and to employ arbitrary definitions of 'durable consumer goods' and call most others 'nondurables'? If 'propensity to consume' is to be given a highly specialized meaning, reflecting disposition to spend money income, do we not need a different phrase to designate related concepts of disposition to utilize and to acquire for consumption by purchase or otherwise? I forbear to reargue the case for clearly distinguishing several concepts that are lumped under the head of 'standards of living'.[4] Natural scientists who dealt so loosely with basic ideas in their science would not long retain good professional standing, but most economists seem impatient with such irksome details.

It is high time to break through certain limitations which we have tended to accept. Social scientists, and economists in particular, have been wont to say that they are concerned with means to a *given* end, not with ends themselves. I make bold to assert, on the contrary, that one of the essential functions of social scientists is to search out and set forth clearly the ends toward which men and nations actually strive, the goals they work toward, the standards of living—both current and for deferred application—they seek to attain and maintain. (Setting up goals that men *ought* to have lies outside our field, if not beyond the scope of scientific study.) The lack of such crystallization is a serious handicap upon our scientific studies. When ascertained, these goals may well be subjected to critical analysis, for both internal congruity and feasible improvements society would welcome if it were less confused and better integrated. I believe these tasks can be accomplished with reasonable precision and clarity in the course of time. Only with fuller understanding of

[4] 'Standards and Content of Living', *American Economic Review,* March 1945, XXXV, 1-15.

ends can social scientists effectually grapple with the problems involved in using various means to attain such ends.

The fad of the day is to regard 'full employment' or 'jobs for all' as the pre-eminent goal of our society. Granting the importance of some such objective, and the possibility that, skillfully phrased, it might well be the dominant target of economic policy, I seriously question whether Americans will actually give it primacy and be willing to pay its price. Indeed, I believe an American government's *guarantee* of full employment would be preposterous and impossible to carry into effect. I am much more ready to accept as a true condensation of over-all goals what is usually termed 'raising the standard of living'; but of all those who freely use some such catch-all phrase, I know of none who has worked through its meaning and implications. Paradoxically, one of the grave dangers now confronting us is that our *standards* of living so greatly exceed the *levels* that can be attained and maintained in the near future.

One of the most promising fields for extended research embraces consumption and levels of living, not merely from the standpoint of individual consumers but with reference to nations and the world as a whole. I venture to assert that larger knowledge will reveal as a widely cherished delusion the conviction that "the only way toward higher standards [levels?] of living is to raise productivity". I would not minimize the importance of expanding productive power and its fuller exploitation, or deny that under some conditions this may be the most effective way to improve the plane of living. But I would emphasize misdirected production and wastes of goods produced as other important obstacles to higher consumption levels, and also that, with given levels of production and consumption, higher planes of living are achievable by various means. Let us not be 'oversold' on 'the money economy'.

In this task and in many others, economic research alone cannot do the whole job. There are many zones in which economics overlaps other social sciences (or natural sciences). To avoid or back away from such zones is surely defeatist policy. A hopeful alternative, not yet demonstrably successful, is the collaboration of workers in two or more social sciences in cultivating the common area. A third possibility, which seems to me clearly promising, is that several economists may well specialize in each of the various overlaps, mastering enough of the other relevant discipline to tackle problems in the common area with the respect of both professional groups. Similarly, economists should welcome incursions into such areas by competent researchers trained primarily in the overlapping science, if they undertake to master enough economics to cultivate this area competently. Perhaps the time is at hand when a number of 'circles' of social scientists actively interested in such zones, or segments of them, could be loosely organized for the exchange of manuscripts, reprints, and correspondence and for occasional roundtable meetings. I suspect that the National Bureau underestimates the potentialities of the 'lonely research worker', but there are more ways of reducing his isolation than have yet been employed.

In almost all walks of life we tend to expect too much, much too soon. Wesley Mitchell rightly emphasizes the length of time required for research tasks to be completed. But there is an opposite danger—that of having no 'terminal facilities' and of wasting a great deal of work because it is not pushed to completion without excessive delay. Years ago I was shocked by hearing the late George F. Warren say, in a discussion of economic research in agriculture: "Get it out! Get it out! That's the important thing." I still shrink from going all the way with Warren, for too much half-baked work clutters our desks and tables. But perfection, or a near approach to it, is usually impossibly costly. There is a happy mean every organized research group has to struggle to attain.

A few research tasks can be performed with a fair approach to definitiveness. Once done well, these need not be done over, at least for a long time; subsequent discoveries, accretions of data, newer techniques, and new ideas may call for supplements or sequels without entailing thoroughgoing revision. In the social sciences, however, relatively few research tasks fall into this class. Most of them call for a far more modest objective: a contribution to knowledge as it grows, even if it be merely a steppingstone toward better knowledge or a part of the scaffolding used in constructing later parts of the edifice. Germinating ideas of profound import may be embodied in a research job of which the direct results prove worthy only of discard.

I think Goodwin Watson went too far when he said (*loc. cit.*): "Research should be thought of . . . as giving a brief push or steer to ongoing currents. . . . What really matters is . . . an influence on the flow of thought and action." Yet I do believe in the importance of throwing a contribution into the current of thought, regardless of its influence on actions or on the course of that current, without waiting until a *magnum opus* can be completed. One of the great wastes in economic research in the past thirty years has arisen from the unavailability of results that were never finished up or, if completed, were inaccessible to other workers. Too many useful, if immature, doctoral dissertations have been stored in the caverns of university libraries without contributing their bucketful to the stream of knowledge.

Economic research on recent, current, and prospective developments and issues is a valuable supplement to more basic research. It not only requires fresh tests in utilizing earlier products of basic research, published or unpublished, but suggests new angles for fundamental investigation corrective of or supplementary to work already done. Such research will be done in any case, more or less, and more or less well. For reasons shortly to be mentioned, it is unsafe to leave it entirely or largely to governmental agencies and to workers in Land-Grant Colleges, many of whom are subject to similar pressures. If the National Bureau prefers to keep its program clear of this particular entanglement, I shall not challenge the wisdom of the choice. I recognize a danger of undue absorption of mature talents in third-rate tasks. But I believe that many economists can wisely include in their individual research programs such work on selected segments of the world economy.

Sir John Russell of Rothamsted wrote two years ago of the important work of Augustine Voelcker in applying to British farm practice the discoveries of Lawes and Gilbert with artificial fertilizer, and concluded:

"This is typical of what happens in science. One man makes the discovery but does not translate it to any practical use; later, others develop it, and finally someone comes along who makes the practical application. Rarely is the latter the man who made the discovery—the two types of men are completely different. The good scientist is not usually practical enough to make useful applications of his results; and the good practical man has rarely enough science to make important discoveries." [5]

It was partly on such grounds that I have urged that a profession of 'social engineering' be evolved to complement our infant social science.[6] The term has not won wide acceptance; no adequate training scheme has yet been devised; and the differentiation of the professions has not yet proceeded far. However, some progress in this direction has been made in the past decade. I am still convinced that economic science will develop faster and more competently if suitable provision can be made for some such new profession. It calls for different talents, which are ill-applied and often wasted in social sciences; and able social scientists often do a poor job in pinch hitting for selected and trained social engineers. I re-emphasize my conviction that most of them, as in the case of other types of engineers, will find their largest scope outside the public service. In the public service I would not rate their major task the making of blueprints for a thoroughly planned society. And I see no prospect whatever that

[5] 'Science and Crop Growth', *Agriculture* (Ministry of Agriculture, London), April 1944, LI, 2.

[6] 'Statistics and Social Engineering', *Journal of the American Statistical Association*, March 1937, XXXII, 1-7. Wesley C. Mitchell began his Foreword to O. T. Mallery, *Economic Union and Durable Peace* (Harper, 1943): "The author of this book is a forerunner of a profession that civilization must develop—the profession of social engineering."

even well-trained social engineers will, in the next century, come nearer to 'controlling the economic weather' than other engineers, in the past century, have come to *controlling* ordinary weather. But 'control' is another of those appealing words that is utterly abused in practice.

III

In relation to public policy, I respect the National Bureau's aversion to expressing 'moral judgments' and to giving advice on public policy. But I cannot endorse the occasional implications that economic research cannot be scientific if it is directly concerned with public policy, or that such research is necessarily unscientific if it eventuates in judgments and advice on matters of public policy. Not only in the field of business cycles is it true that advocates of untested hypotheses "offer practical guidance to government and public with an assurance that contrasts painfully with the caution of responsible physicians in treating imperfectly understood disorders of the body", as Mitchell wrote in *Economic Research and the Needs of the Times* (p. 36). Yet I am not ashamed that one of our long-run aims at the Food Research Institute has been to formulate a well integrated food policy for the United States and the world as a whole. Our progress toward this end in twenty-five years of work has admittedly been unimpressive, but I still think this aim entirely consistent with over-all scientific objectives. And despite criticisms, I continue to believe that an objective analysis of particular measures should be accompanied by the researcher's frank if modest opinion on the merits and demerits of the policy as a whole.

Research on the structure and functioning of dynamic economies, even if undertaken with no specific reference to public policy, is fundamental to the sound formulation and evolution of policies and programs. The more accurately and fully the elements of a problem are understood, the better are the chances that it will be intelligently dealt with. There is, no doubt, a danger that well-tested products of economic research will be ignored in policy matters or that, for one reason or another, there will be a distressing lag before such products can be applied in practice. An opposite danger, however, seems at least equally serious: that hunches, bright ideas, untested or ill-tested products of research be prematurely accepted, relied upon, and found wanting. Proverbially, "a little knowledge is a dangerous thing". Half-baked products have their place, but it is not in the foundation of public policy.

There is an important role for researches on public policy and programs—past, present, and proposed—by social scientists who are not employed in the public service. Such research can be no less scientific in approach, spirit, and methods than research that has no evident policy bearing. There is no prospect that it will be done objectively, at least for publication, within the government service. With no open recognition whatever, the results of such outside research can exert important influence on the wiser evolution of government policy, eventually if not sooner.

To government agencies and their increasingly competent staffs we owe much and shall owe more for expensive collections of data, surveys of current developments, calculations of indexes, monographs on subject matter, and projective analyses bearing on public policy. With all due respect to these workers and their work, however, we must constantly be aware of the pressures to which they are subject and the pressures under which they labor. Their impressive output does not command, or merit, the implicit confidence of professional economists and economic statisticians. Wesley Mitchell well said, of certain work in the Department of Commerce: "some well-staffed independent agency should follow the official precedures critically" (*ibid.*, p. 12); but this does not go far enough. Concepts, slogans, procedures, and results need to be kept vigilantly under review by competent economists outside the public service in order to detect and reveal distortions, omissions, and open or implied misrepresentation, on the basis of which the public may be misled and costly blunders in policy made. There is a perennial need to expose fallacies and half-truths that exert a pervasive influence. To a considerable extent, the same materials should be continuously under examination by keen research workers with no higher officials to answer to and no axes to grind. Utterances of high public officials should receive similar effective scrutiny by the economics profession, especially since technical staffs within the government are seldom in a position to correct ill-advised pronouncements of their superiors.

One of the subjects least effectively analyzed to date is that of a government's strengths and weaknesses in carrying out 'action programs'. A scheme that might be altogether feasible with one kind of government may be quite impossible of successful execution by another kind. Most discussions of economic planning, and of specific proposals that our government 'guarantee' full employment, seem to me permeated by evasion of unwelcome truths about the current and prospective limitations under which American governmental agencies must operate.

There is grave need for better recognition of the nature and limitations of forecasting and predictions. The true scientist makes predictions as to what will happen under controlled conditions, and tests them well before giving them out, even with qualifications, unless he presents them merely as hypotheses worthy of test. In economics, controlled conditions are quite exceptional. Consequently, really scientific predictions are usually impossible except as statements of what can be expected under a certain combination of assumptions. Such specialized predictions have their place, but they are too easily confused with outright prophecies. The assumptions underlying the forecasts, and the margin of error in them, typically deserve as much weight as the forecasts themselves, if the users and indeed the authors are not to be misled. But there is greater need of warning that certain forecasts cannot be made within a margin of error small enough to warrant serious reliance upon them. This is true of many forecasts—of crops, of food supply and demand, of labor force and unemployment, and even of population some decades ahead. Policies cannot soundly be based upon specific forecasts of this type, or an average of them, but ought to take account of a considerable range of possibilities. In this year 1946 we are suffering disastrous consequences of policies adopted with undue reliance upon what proved to be erroneous forecasts.

IV

It is eminently worth while to stop and take stock, to become aware of the ruts we have got into and the opportunities for reorientation, and to rechart our course for a stretch of fresh years. We have grounds for satisfaction in the progress of economic research since 1920, but no room for complacency about the present state of economic science or public policy. As I envisage the research tasks ahead, they include not merely more of the same kinds of work we have been doing. To create an integrated body of economic science which either does not exist or is not generally accepted; to improve our basic concepts and their general acceptance within the profession; to evolve and refine old and new 'laws' of economic behavior; to puncture the bubbles of half-truths and fallacies that derange and distort public policy; and to provide a broader, sounder basis for it: these are among the challenges we face. There is urgent need of more effective, continuous, critical scrutiny of official presentations, and of more and better research by independent economists on public policy itself. The needs of the times call for major advances in economic research, in relation to both social science and public policy, by individual but less lonely economists as well as by organized efforts of varied groups.

We salute a leader in this field as it strides into its second quarter century: the National Bureau of Economic Research.