

RECONSTRUCTION OF ECONOMICS

**Including Papers
by Dr. May Brodbeck and Dr. Richard S. Rudner**

**By
E. C. Harwood**



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Acknowledgments

In acknowledging our appreciation of the permission granted by the editor of *Philosophy of Science* to reprint the papers by Dr. Brodbeck and Dr. Rudner, I should explain that they are not to be held responsible for any other portions of this book. Neither of them had any opportunity to suggest revisions of the other essays.

That acknowledgments are due to many others will be apparent. I doubt if there is any significant point made for which I am not indebted to one or more of the able pioneers in this field. By appropriate footnote references, I have endeavored to give credit where it is due.

E. C. Harwood

Preface

THE title of this volume, which also is the title of the last essay in it, may give readers the mistaken impression that the “reconstruction of economics” is accomplished herein, or that such was the objective. A more appropriate title might have been something like this: “What, in principle, is wrong with economics as it is taught and practiced today; what should be done about the situation; how might men be trained to do the job; and, what parts of the task should have their first attention?”

The first essay, What Is Economic “Knowledge”?, points out that economics as a science is still in swaddling clothes. By far the most of the material found today in economics textbooks is not “knowledge” in the modern scientific sense. Economics is where medicine was 150 years ago; it is in the twilight zone that chemistry passed through before the alchemists were finally discredited by the accumulating scientific findings of the modern age. Obviously, economics will not mature as a science until much that today passes for economic knowledge has similarly been superseded.

The next four essays are samples of the demolition job that must be done. That they are samples should be emphasized. Presumably, better and more complete work of this nature will be done in the years ahead by others more competent than I. Reconstruction necessarily includes tearing down the old in order that the new and better may replace it. This work will not be done all at once, nor will it be done before the new structure of economic science is begun; indeed, that is impossible, because the new structure already is well begun. However, as the new edifice is built, the old will have to go, and the workers who will demolish the old are as necessary as those who will construct the new.

At last, the philosophers are beginning to do their part of the demolition job. Philosophy has always claimed as peculiarly its own the basic epistemological problem, What is knowledge? As the essays of Drs. Brodbeck and Rudner show, the philosophers are finally beginning to assist the social scientists who wish to see their subjects join the more firmly established sciences as useful portions of the accumulating knowledge of mankind.

The eighth essay offers tentative thoughts on the application of modern scientific method to economic problems. In this instance also, I assume that better and more complete expositions on this aspect of the subject will appear in due course. This expectation does not reflect an excess of modesty on my part but rather a great deal of confidence in the intelligence of men.

The ninth essay describes the program that has been developed here at

American Institute for Economic Research for training graduate students as economic scientists.*

The tenth essay discusses the crucial role that behavioral scientists can play in our civilization as well as the primary duties of economic scientists. It suggests a code of ethics because the increasing responsibilities to their fellow citizens that economists have assumed seem to make desirable the development of such a code, analogous to but not necessarily similar to that of the medical profession. Certainly, if economic scientists hope to deserve the confidence of their fellow citizens, they must develop adequate and enforceable standards of scientific behavior.

The final essay of the series is partly a summing up and partly prediction. The latter is based on the expectation and hope that developments in economics will parallel those in the more mature sciences. If the economic scientists of the future find other and more effective ways of advancing the science of economics, so much the better. Regardless of whether or not he leads the procession, every sincere scientist will welcome such advances and rejoice that the light of intelligence in the modern world is being used effectively to illuminate the problems of men.

E.C.H.

* The original essay contained in *Reconstruction of Economics* described AIER's then-current fellowship program. To avoid confusion, a discussion of the history of AIER's direct-education efforts and its current graduate fellowship program have been substituted for the original essay —*Ed.*

Contents

Preface	v
I. WHAT IS ECONOMIC “KNOWLEDGE”?	1
Knowing as an Aspect of Human Behavior	2
The Greek Philosophers and Knowing Behavior	7
The Revolution in the Method of Inquiry	9
II. BETRAYAL OF INTELLIGENCE:	
A Review of Samuelson’s	
<i>Economics, An Introductory Analysis</i>	17
Erroneous Assertions	20
Sweden’s Economy	22
Keynesian “Theory”	23
The “Neoclassical Synthesis”	24
III. MEDIEVAL SCHOLASTICISM vs. MODERN SCIENCE:	
A Review of von Mises’s	
<i>Human Action, A Treatise on Economics</i>	27
A Leap Backward	29
Modern Influence	31
Von Mises’s Contribution	32
IV. WEAKNESSES IN ECONOMICS:	
A Review of Burns, Neal, and Watson’s	
<i>Modern Economics</i>	35
V. NOT SCHIZOPHRENIA, DR. FRIEDMAN	37
What Actually Occurred	38
Friedman vs. Samuelson and Keynes	39
Varying Methods of Inquiry	39
The Role of the Laboratorian	41
The Function of Hypotheses	42
Dr. Friedman’s Prescription	43
Conclusion	44
VI. ON THE PHILOSOPHY OF THE SOCIAL SCIENCES	
by May Brodbeck	45
VII. PHILOSOPHY AND SOCIAL SCIENCE	
by Richard S. Rudner	65

VIII. THOUGHTS ON SCIENTIFIC METHOD	
AS APPLIED IN ECONOMICS	71
The Galilean Revolution	72
Development of the New Method	74
Aspects of the New Method Applicable to Economics	78
IX. THE DEVELOPMENT OF ECONOMIC SCIENTISTS:	
The AIER Graduate Fellowship Program	81
Early History	81
Government-Funded Research and Education:	
A Booby Trap?	82
The Summer Fellowship Today	84
Outlook	85
X. THE ROLE OF BEHAVIORAL SCIENTISTS	
IN A DURABLE CIVILIZATION	87
The Independent Minority	89
Essential Aspects of a Behavioral Research Council	91
XI. RECONSTRUCTION OF ECONOMICS	99

I.

WHAT IS ECONOMIC “KNOWLEDGE”?*

THAT economists frequently do not agree has become so commonplace that some economists no longer seem to be troubled by the suggestion that such a state of affairs is scandalous. That many economists do agree on certain analyses and conclusions is equally scandalous from the viewpoint of modern science, however, because that agreement rests on methods of inquiry that have been found unreliable and have been discarded by capable scientists. The fact that a few conclusions on which some economists agree do have adequate scientific bases emphasizes by contrast the more fundamental disagreement among economists generally regarding the methods of inquiry that can be expected to yield useful results.

In short, one important — perhaps the most important — reason for their divergent and conflicting analyses is that economists have not yet agreed upon the tests that economic inquiry should meet. In a manner of speaking, economists have not answered the question, What is acceptable economic “knowledge”?

For the most part economists seem to have accepted uncritically the notion that everyone knows what “knowledge” is and that “knowledge” can readily be found by using the procedures of common sense, of elementary Aristotelian logic, of intuition, of revelation, or otherwise according to the inclinations of the individual inquirers. No one ought to be surprised that “knowledge” acquired by such diverse means and accepted without generally agreed-upon tests or standards is unreliable, contradictory, and apparently useless in large part; only the credulous who assume that the age of miracles still continues would expect otherwise.

Nevertheless, in fairness to economists generally, it should be noted that the answer to the question, What is acceptable as economic “knowledge,” is not so simple as may be supposed. Economists have not usually claimed to be expert knowers about “knowledge”; that task, imposingly labeled epistemology, has been left for the most part to philosophers, and they have not yet agreed on this aspect of their studies. Turning to philosophers for enlightenment, we find the following:

“Knowledge: In current employment this word is too wide and vague to be a *name* of anything in particular. The butterfly ‘knows’ how to mate, presumably without learning; the dog ‘knows’ its master through learning;

* This article first appeared in *The American Journal of Economics and Sociology*, January 1954. Minor revisions have been made.

man ‘knows’ through learning how to do an immense number of things in the way of arts and abilities; he also ‘knows’ physics, and ‘knows’ mathematics; he knows *that*, *what*, and *how*.... The issues that must be faced before firm use is gained are: Does the word ‘knowledge’ indicate something the organism possesses or produces? Or does it indicate something the organism confronts or with which it comes into contact? Can either of the viewpoints be coherently maintained? If not, what change in preliminary description must be sought?”¹

This seems to leave the economists where they were before, up a tree, or rather up many different trees in a number of different forests. There is in the quotation no light illuminating a path leading to common ground.

Fortunately, Dewey and Bentley have not left us up a tree; they have descended to firm ground where they have identified some objects that may be helpful. However, before discussing those objects, brief comment on Dewey and Bentley’s method is advisable.

Knowing as an Aspect of Human Behavior

In their attempt to know more about “knowledge,” Dewey and Bentley have regarded knowing as an aspect of human behavior. No inner knower, or mind, or soul that does the knowing is assumed. They take man as they find him behaving in his cosmos or universe, never apart from it. Likewise they take whatever is known in the cosmos or universe as they find it, never in isolation from it.²

As a result of applying this procedure in their inquiry into “knowledge,” Dewey and Bentley describe some knowings as the observed language behavior of men concerned with naming or designating. And an integral part of the naming transaction is the known.³ In brief and as a preliminary statement, we may say that knowing and the known refer to the same transaction as naming and the named or knowing refers to naming behavior and known refers to that which is named.

¹ John Dewey and Arthur F. Bentley, *Knowing and the Known*, Boston: Beacon Press, 1949, p. 296.

² This procedure is called the “transactional” approach. Economists to whom this terminology at first seems unfamiliar might benefit by reflecting on a similar approach to a particular supply-demand situation in a marketplace. If study of such a problem included all significant aspects in their relations to one another, the behavior of the buyer, the seller, the items exchanged, and the effects of bids and offers on the behavior of other potential buyers and sellers in the market, then the transaction in all its pertinent ramifications would be under consideration and the result would be a transactional approach to the problem.

³ Dewey and Bentley, *op. cit.*, p. 296, where knowings are specifically designated “the organic phases of transactionally observed behavior,” and p. 297, where the known is designated the “environmental phase (as distinguished from the behavioral phase) of transactionally observed behaviors.”

Now it is obvious that nothing just said enables economists or anyone else to use the word “knowledge” for the purpose of specifying (scientifically naming) anything in particular. But the economists can at least climb down their various trees of “knowledge” and survey the relatively firm ground of knowing behavior and the known. Evidently this is not the last step toward common ground, but it may prove to be an essential first step.

For untold thousands of years man’s knowing behavior has been in the process of advance or development from primitive beginnings. However, the behaviors of new-born babies are as primitive as those of infants born ages ago. Consequently, we can gain some understanding of the development of man’s knowing behavior in spite of the fact that no records of the beginning were made or, if made, were lost in remote antiquity. Not only do the knowing behaviors of infants progress through the successive stages of man’s knowing behavior, but also the present knowing behaviors of adult men in primitive tribes throw light on the problem.

The distinguished anthropologist, Bronislaw Malinowski, conducted certain penetrating studies of knowing behavior in primitive tribes. The paragraphs that immediately follow are a paraphrased and otherwise shortened version of his report.⁴

In its primitive forms, language should be studied against the related background of human activities in each instance and as an aspect of human behavior. Language functions as a link in concerted human activity and is a mode of action.

Dr. Malinowski described situations illustrating such primitive uses of language. One example describes a fishing expedition participated in by a large group:

“The canoes glide slowly and noiselessly, punted by men especially good at this task.... Customary signs, or sounds or words are uttered. Sometimes a sentence full of technical references to the channels or patches on the lagoon has to be spoken.... Again, a word of command is passed here and there, a technical expression or explanation which serves to harmonize their behavior towards other men....

“All the language used during such a pursuit is full of technical terms, short references to surroundings, rapid indications of change — all based on customary types of behavior.... The structure of all this linguistic material is inextricably mixed up with, and dependent upon, the course of the activity in which the utterances are embedded. The vocabulary, the meaning of the particular words used in their characteristic technicality is not less subordinate to action....

⁴ Bronislaw Malinowski, “Supplement I” to *The Meaning of Meaning* by C. K. Ogden and I. A. Richards, New York: Harcourt, Brace, 1952, 10th edition.

“It is a mode of action and not an instrument of reflection.”⁵

Perhaps more illuminating because it is within the scope of the personal experiences of everyone is the primitive language of infants. We may use Malinowski’s observations to refresh our own memories.⁶

“This manner of formation of meaning we must now proceed to analyse.... And it will be best done ... by an analysis of infantile uses of words, of primitive forms, ... and of pre-scientific language among ourselves....

“... Concentrating our attention for the moment on infantile utterances ... it can be said that each sound ... has a certain significance; and that it is correlated with the outer situation surrounding and comprising the child’s organism....

“All this is true of the non-articulate sounds emitted by an infant, such as gurgling, wailing, squealing, crowing and weeping.... Emission of sounds is ... one of the child’s main activities, persistent and passionate, as every parent knows from pleasant and unpleasant experiences alike!

“... The child *acts* by sound at this stage, and acts in a manner which is both adapted to the outer situation, to the child’s mental state and which is also intelligible to the surrounding adults. Thus the significance of sound, the meaning of an utterance is here identical with the active response to surroundings....

“When sound begins to articulate, the child’s mind develops in a parallel manner and becomes interested in isolating objects from its surroundings.... At the same time, the child becomes aware of the sounds produced by the adults and the other children of its surroundings, and it develops a tendency to imitate them ... and this paves the way to a clearer, more articulate enunciation.

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“... whether the child acquires some of its early vocabulary by a spontaneous process or whether all its words come to it from the outside, the manner in which the first items of articulate speech are used is the point which is really interesting and relevant for us in this connection.

“The earliest words — *mama*, *dada*, or *papa*, expressions for food, water, certain toys or animals — are not simply imitated and used to describe, name, or identify.... Here the word becomes the significant reaction, adjusted to situation, expressive of inner state and intelligible to the human milieu.

“This latter fact has another very important set of consequences. The human infant, helpless in itself and unable to cope with the difficulties and

⁵ Malinowski, *op. cit.*, p. 311.

⁶ *Ibid.*, pp. 318-25.

dangers of its early life, is endowed with very complete arrangements for care and assistance.... The child's action on the surrounding world is done through the parents, on whom the child acts again by its appeal, mainly its verbal appeal ... its only means of action is to clamour, and a very efficient means of action this proves to the child.

"To the child, words are therefore not only means of expression but efficient modes of action. The name of a person uttered aloud in a piteous voice possesses the power of materializing this person. Food has to be called for and it appears — in the majority of cases. Thus infantile experience must leave on the child's mind the deep impression that a name has the power over the person or thing which it signifies.

"We find thus that an arrangement biologically essential to the human race makes the early articulated words sent forth by children produce the very effect which these words *mean*. Words are to a child active forces, they give him an essential hold on reality, they provide him with the only effective means of moving, attracting and repulsing outer things and of producing changes in all that is relevant. This of course is not the statement of a child's conscious views about language, but it is the attitude implied in the child's behavior.

"... In all the child's experience, words *mean*, in so far as they act and not in so far as they make the child understand or apperceive.... And all the time, up to a fairly advanced age, the name of an object is the first means resorted to, in order to attract, to materialize this thing....

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"Again, the same view of meaning results from the active uses of speech among ourselves, even among those of us, who, on comparatively rare occasions, can use language in a scientific or literary manner. The innumerable superstitions — the agnostic's fear of blasphemy or at least reluctance to use it, the active dislike of obscene language, the power of swearing — all this shows that in the normal use of words the bond between symbol and referent is more than a mere convention.

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"Indeed, on anyone who has read the brilliant chapters of Ogden and Richards and grasped the main trend of their argument, it will have dawned before now that all the argument of this Section is a sort of footnote to their fundamental contention that the primitive, magical attitude towards words is responsible for a good deal in the general use and abuse of language, more especially in philosophical speculation ... by much of what is incidentally said, we are made to realize how deeply rooted is the belief that a word has some power over a thing, that it participates of the nature of the thing, that it is akin or even identical in its contained 'meaning' with the thing or with its prototype.

“... In studying the infantile formation of meaning and the savage or illiterate meaning, we found this ... magical attitude towards words. The word gives power, allows one to exercise an influence over an object or an action. The meaning of a word arises out of familiarity, out of ability to use, out of the faculty of direct clamouring as with the infant, or practically directing as with primitive men.... This indeed is nothing more or less than the essence of the theory which underlies the use of verbal magic. And this theory we find based on real psychological experiences in primitive forms of speech.

“Before the earliest philosophical speculation sets in, there emerges the practice and theory of magic, and in this, man’s natural attitude towards words becomes fixed and formulated by a special lore and tradition. It is through the study of actual spells and verbal magic as well as by the analysis of savage ideas on magic that we can best understand this developed traditional view of the secret power of appropriate words on certain things. Briefly it may be said that such study simply confirms our theoretical analysis of this section....”

Thus we arrive at one of man’s earliest knowing behaviors, the practice of magic, and with the foregoing in mind, it is not difficult to understand his early adoption and even his continued use of such procedures. As Dr. Malinowski mentions, Ogden and Richards have brilliantly described this aspect of the problem. For much in the paragraphs that immediately follow, I am indebted to them.⁷

“From the earliest times the Symbols which men have used to aid the process of thinking and to record their achievements have been a continuous source of wonder and illusion. The whole human race has been so impressed by the properties of words as instruments for the control of objects, that in every age it has attributed to them occult powers.... Unless we fully realize the profound influence of superstitions concerning words, we shall not understand the fixity of certain widespread linguistic habits which still vitiate even the most careful thinking ...”⁸

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“We may smile at the linguistic illusions of primitive man, but may we forget that the verbal machinery on which we so readily rely, and with which our metaphysicians still profess to probe the Nature of Existence, was set up by him, and may be responsible for other illusions hardly less gross and not more easily eradicable? It may suffice at this point to recall the prevalence of sacred or secret vocabularies, and of forbidden words of every sort ... with ... stress on the part played by language in memory and

⁷ C. K. Ogden and I. A. Richards, *The Meaning of Meaning*, New York: Harcourt, Brace, 1938, Chapter II especially.

⁸ Ogden and Richards, *op. cit.*, p. 24.

imagination, it is clear that in the days before psychological analysis was possible the evidence for a special world of words of power ... must have appeared overwhelming.”⁹

Now it seems obvious that the continued acceptance of practitioners of magic and purveyors of superstitions has been attributable in large part to an attitude toward words deeply ingrained not only in children and primitive savages but also in the most enlightened adults of our own times. True, many adults have developed some degree of sophistication in their use of language, they no longer believe that the word is the *same* as the thing or that utterance of the word necessarily will *call forth* the thing. Nevertheless, even the more astute thinkers frequently seem to assume that, if there is a word they like to use, there must be a corresponding thing. Some of the supposedly more sophisticated users of words have learned to avoid some of the verbal “booby traps” but reveal, in their attitudes toward mathematical or other formalized short-hand symbolization acceptance of word magic once removed, so to speak. This is hypostatization, and many economists as well as philosophers thus become tangled in semantic snares of their own making.

Probably more or less paralleling the development of word magic in all its fascinating varieties, men also developed a language behavior that was more closely allied to or was the outgrowth of what we may call common sense. In an interesting article, Dr. Edwin Loeb¹⁰ has pointed to proverbs as an example of man’s knowing behavior.

For the purposes of this discussion a study of the precise manner in which various proverbs have been handed down from antiquity or have been derived from man’s experience is unnecessary. Many proverbs still are considered useful records of man’s knowing behavior, and with good reason; but many also are known to be useless because the common sense methods of inquiry that they reflect were inadequate for the purpose such proverbs pretend to serve. From the modern scientific point of view neither proverbs nor magic are considered consistently useful forms of knowing behavior.

The Greek Philosophers and Knowing Behavior

More than 2,000 years ago a new development in man’s knowing behavior occurred. No doubt, the beginnings of this development lie farther back beyond the reach of recorded history; but what we know of it is what has survived from the Greek philosophers’ work and the modern application of their methods still widely used today.

⁹ *Ibid.*, p. 26.

¹⁰ Edwin Loeb, “The Function of Proverbs in the Intellectual Development of Primitive Peoples,” *The Scientific Monthly*, February 1952.

Records concerning two quite different Greek philosophical schools have come down to us. One, dominated by Plato's methods, was in some respects influenced greatly by the primitive attitude toward words already described. Perhaps it would be more accurate to say that this school was influenced by the supposed significance of ideas without apparently realizing the extent to which language, uncritically accepted, controlled the ideas. Members of this school hoped to know or name reality by dialectical or word-juggling processes, and the result was Plato's idealism. This procedure has not by any means been abandoned, as study of even widely known philosophers and some economists plainly shows.¹¹

Associated with Plato were the early mathematicians whose work seemed to offer both new tools of inquiry and proof, in the seeming perfection of mathematics, that the course followed by Plato would have the desired results. Aristotle has sometimes been blamed for diverting the development of man's knowing behavior away from the use of mathematics in connection with observations of nature. However, Dr. Ratner clarifies this aspect of the historical development as follows:¹²

"The plain historical matter of fact is that the Pythagoreans, the mathematicians, were with respect to the development of Greek science and philosophy, on the ground floor.... Only superficial reading of history backwards ... can yield the conclusion that Aristotle deflected the course of Greek scientific thought out of 'relating mathematical notions to the facts of Nature' into the halfway house of 'classifying' those facts. Rather must the case have been that the Greek mathematical development, as a procedure of investigating nature, quickly reached an impasse and Aristotle's Logic was the only way out. And for this there is conclusive proof."

Now in spite of the differences between Aristotle's and Plato's methods of inquiry, one especially significant feature of their philosophies was the same in each. Both were embarked on what John Dewey has labeled "The Quest for Certainty."¹³ The objective of Greek inquiry, and indeed the objective of man's inquiries for many centuries thereafter, was to discover eternal and immutable reality, that which was assumed to be forever

¹¹ See Joseph Ratner, *Intelligence in the Modern World, John Dewey's Philosophy*, New York: Modern Library, 1939. Dr. Ratner says (p. 239), "Contemporary philosophers are of course distinguishable in many ways from medieval scholastics. But insofar as contemporaries accept standardized problems and seek for their solution by dialectically arranging standardized parts, they are every whit as medieval as the veriest scholastics of ten centuries ago," and on pp. 215, 216, 230 and 235 as well as elsewhere he cites examples from the works of Russell, Berkeley, Whitehead, and the economists.

¹² *Ibid.*, p. 101.

¹³ *Ibid.*, p. 19, where Dr. Ratner's comments will be found, and pages 275 *et seq.*, where Dewey's book *The Quest for Certainty* is quoted at length.

unchanging and that once known is known forever after with absolute certainty.

The fact that this “quest for certainty” was the objective influenced greatly the procedures used. The Greek philosophers and probably many before their time as well as all philosophers afterward for many centuries concentrated their attention primarily on whatever they could find that seemed to be fixed and unchanging. All change was considered accidental, ephemeral, and inferior as a subject of investigation because it seemed obvious that essential and unchanging Being could not be found in the processes of change. Incidentally, we should remember that, even if such had not been their viewpoint, the Greek philosophers would have been seriously handicapped by a lack of tools and instruments for effecting changes and measuring them.

Unlike Plato, Aristotle gave more attention to observing facts. Thwarted in the attempt to apply the mathematics of his time to reality as he saw it, Aristotle turned to classification of the qualities and proportions and relations to shapes and sizes as he found them. Aristotelian logic was the outgrowth of these efforts, but he never achieved understanding or the ability to apply what would now be considered scientific methods of inquiry.

Subsequently, in Europe, for the hundreds of years of the dark ages, magic and proverbs were the principal keys to what was considered “knowledge.” Whether one classifies revelation as a separate method of knowing or includes it under the general classification of magic may depend on his religious preferences or lack of them. In any event, the Greek contributions, both Plato’s idealism and Aristotle’s system of logic, were lost to Western civilization until rediscovered and put to use again by the medieval scholastics who dominated the field of learning until the scientific revolution of the 17th century.

The Revolution in the Method of Inquiry

Some 300 years ago a revolution in the method of inquiry was initiated by Galileo. His new method was revolutionary in that it both destroyed one system of inquiry and started another. Precisely what were the significant features of the new method? To summarize, they were the quantitative measurement of change and study of the relationships between or among changes.

The new method was broader in scope than the chemist’s test-tube experiments in his laboratory, although these were included in the new method. Those observers of the new method who have argued that it is applicable only to the things that can be put in a test tube, or in a

laboratory at least, overlook its essential characteristics. Even such a keen observer and student as Dr. Ratner has said, we think mistakenly, that “to be able to measure quantitative relations of change, it is absolutely essential to *control* the changes, to stop them and start them, to accelerate them and retard them.”¹⁴ The celestial bodies have never been brought into the astronomers’ laboratories, nor do the astronomers either initiate or control the changes that occur in celestial space; but the new method has been applied to that as well as other fields with outstanding success.

Although Galileo and those who followed him in applying the new method demonstrated its value by the results obtained, they too, at least the earlier ones, retained the Greek objective, the quest for certainty, as the goal of their research. Newton, for example, was satisfied that he, by better methods, had pushed aside, as it were, the curtain of change and found behind it the indivisible and indestructible atom and the immutable and unchanging laws of gravitation. He and others were at first ready to assert the superiority of their laws over any recalcitrant facts. If any facts did not accord with the new theories, so much the worse for the facts.

Finally, however, the scientific scandal of facts at variance with theory became acknowledged for what that situation now is considered to be. Again, Dr. Ratner’s description is enlightening.¹⁵

“That the Newtonian reign should ever come to an end was simply inconceivable to Newtonians. The basic structure of the Newtonian system was eternal and immutable.... But instead of solving the problems presented by experimental findings [measurements of change], the Newtonian method kept piling them up ever higher. And it is the last straw that breaks the camel’s back. In this case, the black bands in Michelson’s interferometer.

“What happened to the ‘eternal basic structure’ of Newtonianism, to its immutable cosmological framework reputedly riveted ‘scientifically’ to the three absolute pillars of Space, Time and Matter by eternally true and eternally enduring, non-corrodible struts and bolts of pure mathematics, everyone knows.... By establishing the forthright and uncompromising procedure of giving to experimental findings first the authority to determine the meanings of mathematical-physical concepts and then the final authority to control their development and formulation in all respects relevant to the science of nature, Einstein accomplished in *scientific practice* the full enstatement of experimentalism [measurement of change]. The verified success of Einstein’s reversal of the Newtonian policy has

¹⁴ Ratner, *op. cit.*, p. 99. In all fairness to Dr. Ratner, we should add that he made no claims to certainty for his own assertions or those of John Dewey.

¹⁵ *Ibid.*, pp. 108 and 109.

demonstrated beyond all doubt and with a precision science alone is capable of, that for three hundred years Newtonianism ... because of one basic methodological fault ... had literally upset the true relation between experimental findings and theoretical (mathematical) formulations. It had been living methodologically upside down.”

Thus we come to the work of the modern scientist today. In his search for knowledge, he has abandoned the Greek objective. The theorist and the laboratorian have been made inseparable partners in the scientific transaction; and above all, theory both controls and is controlled by a study of relationships between and among changes and definitely and finally is controlled by measurement of change.

Of course, a primary objective of man’s knowing behavior today is to enable him to predict and control. Just as the primitive medicine man’s word magic was intended to enable men to find security against the vicissitudes of life, so likewise today the findings of modern scientists are used for prediction and control. The predictions are of what may be expected to occur under certain circumstances, and the control involves either control in some degree of man’s behavior or control over some aspects of the external environment, or both.

The usefulness of the modern scientific method of inquiry in comparison to the other methods that man has used and still uses requires little emphasis. To an ever-increasing extent the people of Western civilization look to the doctors of scientific medicine rather than witch doctors for the cure of bodily ills, to the psychologists rather than to word magic for the cure of mental ills, and to the physical scientists for control over much of the inanimate environment. Why, then, the slowness to apply to economic problems the same methods that have been successful in other fields?

That the older methods still are widely used and apparently widely accepted by economists is evident. Striking examples are numerous, and two will be mentioned.

The distinguished Dr. Ludwig von Mises in a recent volume makes clear his objective and methods. On many pages¹⁶ he affirms and reaffirms that his objective is the discredited “quest for certainty,” in effect, that his method includes introspection and revelation as acceptable procedures, that change is unworthy of serious attention (“Praxeology is not concerned with the changing content of acting, but with its pure form and its categorial structure.”)¹⁷ that hypostatization and dialectical facility (Plato’s method)

¹⁶ Ludwig von Mises, *Human Action, A Treatise on Economics*, New Haven: Yale University Press, 1949. The 900 pages include a multitude of specific comments on method. Those on pp. 24, 25, 26, 27, 32, 64, 86, 788 *et seq.* are especially pertinent to the comment here.

¹⁷ *Ibid.*, p. 47.

is still useful, and that warranted assertions can be derived through the medieval scholastics' revival of Aristotle's syllogisms.

That Dr. von Mises adopts the modern methods of scientific inquiry when discussing some aspects of economics including the business cycle is to his credit. But this departure from the methods on which he claims to have built the basic structure of his treatise emphasizes by contrast his reliance on outmoded procedures.

Another example is found in the works of the late John Maynard Keynes and his numerous followers. Detailed analysis would require more space than is here available, but the views of Dr. Arthur F. Burns merit quoting in this connection.¹⁸

"... The opinion is widespread that Keynes has explained what determines the volume of employment at any given time, and that our knowledge of the causes of variations in employment is now sufficient to enable governments to maintain a stable and high level of national income and employment within the framework of our traditional economic organization.... Unhappily, this opinion reflects a pleasant but dangerous illusion."

Lest the foregoing be misunderstood, I should explain that the works of Keynes and von Mises have been selected not as extreme departures from the general rule but as representative of by far the most of the economic literature available. Why, then, are the results of such methods accepted as warranted assertions by many economists today?

One answer that has been suggested is that the subject matter of economics is somehow different with the result that only the older methods of inquiry are applicable, however discredited they may be in other fields. This suggestion cannot be discussed adequately in a paragraph, but the argument against it has been summarized as follows:¹⁹

"When a 'distinction' in subject-matter between the 'natural' (physical) and 'social' (mental) is used as *ground* for explaining the differences between the 'natural' (physical) and 'social' (mental) sciences, the 'distinction,' if it does not start out as a variant term for 'separation,' is forced to grow into an assertion of an abysmal separation in order to maintain itself. And when the so-called natural sciences are separated from the social, are taken out of their context in human history, and out of relation to human activity, then an adequate and satisfactory explanation of the natural sciences themselves becomes impossible."

The answer now suggested rests not on an arbitrary division of the

¹⁸ Arthur F. Burns, *Economic Research and the Keynesian Thinking of Our Times*, Twenty-sixth Annual Report of the National Bureau of Economic Research, New York, 1946, p. 5.

¹⁹ Ratner, *op. cit.*, pp. 72 and 73.

universe into two or possibly more realms requiring fundamentally different methods of inquiry, but on the nature of man as we find him in his cosmos. For man, in many respects the freest of all that we find in our universe is, by virtue of certain characteristics that make him relatively free, at the same time singularly restrained. In the terminology of Alfred Korzybski, man the time binder is himself bound by the very capacities and abilities that enable him to bind time.

This seemingly paradoxical and perhaps at this stage unclear assertion involves no deep mysteries, however. With Korzybski's aid we may summarize the explanation.²⁰

Man is greatly different from the other forms of life found in our universe. Like plant and animal forms of life, man is an energy binder, that is, he can absorb the materials of his environment and convert the energy in them to his own uses. Like animal life, man is a space binder, that is, he can move about in space and use it as a means of adapting to and otherwise utilizing his environment. But, unlike plants, and to an extent far exceeding that found in other animal life, man is also a time binder, that is, he can accumulate the results of his experiences over time and make the lessons of these experiences available to successive generations.

This time-binding capacity of man apparently is made possible by the development of the human brain and the development of language and tools. The details of the process would require volumes to describe, and such volumes are to be found among others in the libraries that are part of man's cultural heritage, some of the most pertinent evidence of man's time-binding activities.

Now it is evident that for men to be able to learn from experience and from their cultural environment, there must be a plasticity of habit-forming capacities, and elasticity, as it were, of learning ability. But the very fact of capacity to learn new habits implies that what is learned will be habitual, will have a tenacious hold on the individual learner.

Thus we can see how man the time binder is likewise bound. The accumulating libraries of the world have their influence on each generation of students and tend to impress on them the habitual outlooks and knowing behaviors of earlier days. Moreover, the unconscious learning processes of the infant in relation to language that already have been described become habitual behaviors toward words. These primitive hab-

²⁰ Alfred Korzybski, *Manhood of Humanity*, first published 1921, second edition printed by Country Life Press Corporation, Garden City, New York, 1950; also *Science and Sanity, An Introduction to Non-Aristotelian Systems and General Semantics*, Science Press Printing Company, Lancaster, Pennsylvania, Second edition, 1945. Chapters 3 and 4 of the former and Chapters 2, 24, and 30 of the latter are especially pertinent to the discussion here.

its must be replaced by different behavior if an individual is to learn to use language scientifically, that is, if he is to think in the modern way. To the extent that the more primitive knowing behaviors are firmly fixed habits, they constitute restrictions on the intellectual development of men, restrictions as pervading and effective as the great weight of the cultural environment embodied in libraries, folklore, and superstition — all the prescientific knowing behaviors and culture of man.

From one point of view we may say that the great problem of our times is how shall we free man the time binder from the multitudinous, all-encompassing, and intricately tangled web that is one obvious result of his own time-binding propensities. Thus far, no one has offered an easy solution; perhaps there is none. As to this, Dr. Ratner suggested:²¹

“It is possible, I think, to prove with reasonable surety that against oppression hardened in institutions and enforced by socially guarded and perpetuated dogmas only the assault of individualism, under whatever form it may be, has revolutionary power and effect. But whether or not this can be established as a principle, certain it is as a fact that modern culture started, and for nearly three hundred years won, all its greatest battles under this standard.”

And as we look back over those 300 years, what do we see on this planet we inhabit? Most significant, in relation to our present subject, is the rise of Western civilization. Out of the ruins of the once mighty Roman Empire our civilization has come.

We see the successive triumphs of individual intellects over the inhibitions of the torture chamber, rack, and screw, over the restrictive syllogisms of Aristotle, with which the minds of the medieval scholastics were bound, and over the word magic that is our common heritage and apparently inevitable infantile experience. And, as it happened, there was a new continent available where men could seek the economic and political freedom without which sustained intellectual individualism seems to be impossible.

But in Europe we find that the light that was Western civilization now burns dimly. Retrogression rather than progress seems to be the general rule. Even in parts of the Atlantic seaboard of the United States one may question whether progress is not being replaced by retrogression of a subtle but devastating kind. Certainly much in the cities seems similar to the physical decay found in the great cities of Europe.

Perhaps this is too gloomy a viewpoint. True, there are no new continents on which the wave of Western civilization might mount to greater

²¹ Ratner, *op. cit.*, p. 25.

heights; but, with luck, there may yet be time for the triumph of intelligence over the darkness in which prior civilizations have been engulfed. What is essential to such a future, a lasting civilization, for our world? Possibly we do not yet know all the requirements; but we may safely conclude that one, without which all others could not avail, is that organized intelligence shall triumph over the infantile, the primitive, the prescientific economic knowings of mankind.

II.

BETRAYAL OF INTELLIGENCE

A Review of Samuelson's *Economics, An Introductory Analysis**

AN excellent binding and good reproduction of type and charts combine to give this book the appearance of a scientific treatise. Unfortunately, readers who therefore assume that it offers the last word in scientific economic analysis will be seriously disappointed. Still included in this fifth edition are major flaws that were among the reasons for my comment on an earlier edition, "... that such a book should have the implied stamp of approval of the Nation's leading scientific institution is a tragedy; in a sense it is a betrayal of intelligence in the modern world."

Before the reasons for such adverse criticism are described, Professor Samuelson should be commended for the marked improvements in this edition. In contrast with the earliest edition we reviewed, this volume does include some charts that show the long-term economic growth of the United States. Therefore, student readers at least have evidence that economic growth proceeded rapidly for decades before the advocates of creeping inflation developed their modified version of the Keynesian spend-for-prosperity notions.

That the charts do not include years prior to 1890 is unfortunate. From 1875 to 1890, gradual deflation of the Civil War and post-Civil War inflation was reflected in a 40-percent decline of commodity prices; yet the Nation's economic growth persisted at a rate not subsequently equaled. With that picture in front of them, even sophomores might question Professor Samuelson's apparent predilection for creeping inflation.

Professor Samuelson has altered his opinion markedly in recent years. Earlier he had said, "... such a mild steady inflation [a rise in prices of 5 percent per year] need not cause too great concern" (p. 302, second edition). In his fourth edition he suggested that creeping inflation be "... held down to, say, 2 percent per year, ..." (p. 270), and in this fifth edition he says "Price increases that could be held down *below* 2 percent per year are one thing," (p. 305, italics supplied) as though that would be negligible. To the casual reader the difference between 5 percent and 2 percent may not seem important, but from the viewpoint of anyone who would live under those conditions the difference is striking. At 5 percent per year, a dollar's worth of life insurance or funds for a retirement pension would decrease in 60 years to a little more than 5 cents worth, a loss of nearly 95

* *Economics, An Introductory Analysis*, by Paul A. Samuelson, Professor of Economics, Massachusetts Institute of Technology, New York: McGraw-Hill Company, 5th Edition, 1961. Although subsequent editions have appeared, this evaluation still is pertinent (through the eighth edition, at least).

percent of one's life insurance and pension funds; but at 2 percent per year, the loss would be much less, only about 65 percent.

When one views the matter considering the amount that would be left after prolonged creeping inflation, the significance of the Professor's progress is seen to be even more striking. Now that he approves of somewhat less than 2 percent rather than 5 percent per year, he is implying, in effect, that the buyer of life insurance should be permitted to have at least 35 cents left of his dollar instead of only 5 cents. Surely this sevenfold increase in what is left for the victim of creeping inflation is a gratifying change. Professor Samuelson may yet come to believe that life insurance buyers should not have any of their savings "embezzled" by the subtle processes of inflation.

Professor Samuelson offers no purportedly scientific or economic explanation for the change from 5 to less than 2 percent. The reader of his successive editions can conclude only that Professor Samuelson has had the benefit of some secular revelation on this matter.

Although I did not find in this volume Professor Samuelson's justification for any creeping inflation, he perhaps would argue as did Professor Slichter of Harvard that "... creeping price inflation is part of the price we must pay to achieve maximum growth."¹ American economic developments from 1875 to 1890 suggest that such an assertion was not true then, and no one has provided scientifically based proof that it is true today. West Germany's experience since regaining the prewar level of output in 1950 also casts doubt on the creeping inflation theory. From 1950 to 1960 industrial production in West Germany increased 150 percent; but in Sweden the increase was only 38 percent, although the rate of creeping inflation there (measured by the rise in the cost of living) was nearly 6 times that in West Germany. The latter nation increased its industrial production 2½ times accompanied by a negligible amount of creeping inflation while Sweden was increasing its industrial production only a little more than one-third accompanied by creeping inflation at the rate of 57 percent in a decade.

Perhaps the most convincing argument against the creeping inflation theory of inducing maximum economic growth is found in the fact that inflation makes possible an excess of dollars chasing goods, which in turn provides windfall profits for many businesses including some that in the absence of inflation would incur losses. When inflation occurs, businesses that otherwise would fail or at least curtail output and release factors of production (men, capital, and natural resources) for transfer to the grow-

¹ Sumner H. Slichter, "Current Trends, Problems and Prospects in the American Economy," *The Commercial and Financial Chronicle*, February 19, 1959, p. 3.

ing industries are enabled to remain in business with a resulting delay in the shift of resources to more rapidly growing industries. Change, not creeping inflation, is the price of economic growth; and experience suggests that change is inhibited and delayed by inflationary prosperity.

Dr. Samuelson's recognition of what he calls the "miracle" of West German postwar economic developments is encouraging to those who hope that his progress will continue. He describes the basis for the "miracle" as "a thoroughgoing currency reform...." (p. 39), which seems an inadequate description of reforms that restored free markets as well as a redeemable currency, and, in effect, tossed into the discard the depression panacea Professor Samuelson evidently favors. Would it not be worthwhile in an economics textbook to devote more than a few lines to the experience of West Germany in recent years? Surely an economic "miracle" merits more detailed comment, especially when such consideration would reveal so much about significant aspects of American foreign and domestic economic policies.²

Many writers of economics textbooks have given only superficial consideration to the potential effects of a tax on site values as differentiated from a tax on the value of improvements. In a brief but clear discussion of this point (p. 597) Professor Samuelson describes how a tax on site values would fall in its entirety on those privileged to hold exclusive titles to such sites and would not burden either those who labor or those who invest in the reproducible capital of our economy. An obvious conclusion is that shifting of the tax burden from investors and earners would encourage new investment as well as production and would inhibit speculative withholding of valuable sites and resources from production. An equally obvious conclusion is that the net result could be more rapid economic growth with output more equitably distributed among those who participate in the productive process.

The potentially far-reaching consequences of taking much of site rent for public uses might well have been discussed in greater detail. The Institute of Research of Lehigh University, another distinguished school of engineering, has analyzed and reported on the potential effects of exempting improvements and taxing only land values in the city of Bethlehem, Pennsylvania.³ Here is substantial evidence that the slum areas of a city reflect prolonged unwise apportionment of the tax burden and that the simplest remedy for "sick" urban areas would be shifting

² See Melchior Palyi, *Managed Money at the Crossroads*, University of Notre Dame Press, Notre Dame, Indiana, 1958, p. 100 *et seq.*

³ Eli Schwartz and James E. Wert, *An Analysis of the Potential Effects of a Movement Toward a Land Value Based Property Tax*, a project of the Institute of Research of Lehigh University, published by Economic Education League, Albany, N.Y., 1958.

present taxes on improvements to taxes on land values. Moreover, the experience of Sydney, Australia, and several other cities indicates that even most of the landowners, surprising as it may seem, would benefit from such a shift of the tax burden. The experiences of Denmark, of New Zealand, and even of Pittsburgh, Pennsylvania, with its partial application of the principle, merit consideration by every student of economics.

Erroneous Assertions

So much for the evidence of some progress by Professor Samuelson. In other respects the lack of progress is evident.

He asserts (p. 22) that, during World War II, American “civilization living standards” were “higher than ever before.” Such statements frequently are made by economists enamored of the spend-for-prosperity notions, perhaps because their theories suggest that the vast monetization of Government debt should have had that result or perhaps because they are so naive as to believe that money incomes correctly reflect the standard of living. Here are the facts:

a. Production of passenger automobiles for civilian use during World War II virtually ceased. With reference to the public’s huge investment in passenger automobiles, the standard of living greatly decreased as a result of wear and tear, depreciation and obsolescence, lack of replacements for vehicles scrapped, and lack of additional new vehicles to maintain the per-capita quota.

b. Construction of new residential housing decreased 85 percent and remained at a low level until after World War II. Inevitably the standard of living with reference to housing decreased during the war for reasons similar to those in a, above.

c. A comprehensive index of production of new consumer goods per capita shows that a 25-percent decrease in the production of all consumer goods occurred from mid-1941 to 1945.

d. In large part because automobiles, new homes, etc., were not available, individuals hoarded about \$15,000,000,000 of their wartime wages in the form of currency and many billions more in the form of idle checking accounts. In addition, many billions of wartime incomes were invested in U.S Savings Bonds.

In view of these facts, civilian standards of living could not have reached unprecedented levels during the war years. To imply otherwise may suggest to many readers that monetization of deficits, *i.e.*, inflation, somehow offers an easy route to perpetual prosperity.

Additional erroneous assertions are scattered through this book. For

example, Professor Samuelson asserts (p. 286) that the Great Depression of the 1930's was the longest "sustained" in the Nation's history. The National Bureau of Economic Research, whose research in this aspect of economic developments is far more extensive and detailed than that of any other agency, reports that the duration of the 1873-82 recession and recovery from peak to peak was 101 months compared with 93 months for the depression of the early 1930's.

On page 377 Professor Samuelson refers to "classical views that there can never be unemployment." This representation of the classical views also is erroneous. The classical economists argued that unemployment would be extensive if some elements of labor refused to accept lower wages whenever that was indicated in order that they might be employed.

Also, on page 406 Professor Samuelson says, "From the early 1870s to the middle 1890s, depressions were deep and prolonged, booms were short-lived and relatively anemic, the price level was declining." That the price level was declining is correct, but the National Bureau's record shows that, from the time the gold standard was resumed in 1879 until 1895 there were 4 recessions having an average duration of less than 20 months, almost exactly the average for more than 100 years. None of the 4 was "deep and prolonged," and during this period the Nation enjoyed its most rapid and consistent growth as measured by the expansion of industrial production.

When he attempts to discuss "money," Professor Samuelson gives his readers inadequate information. For example, what is meant by the words on a \$10 bill, "The United States of America will pay to the bearer on demand Ten Dollars"? I could find no evidence in the Professor's discussion that he knows of this promise or its significance, in spite of his attributing West Germany's "miracle" to "currency reforms," a principal feature of which has been a sound currency now redeemable in gold on demand. Surely, differentiating between dollars (1/35 of an ounce of gold) and promises to pay dollars is elementary in any attempt to describe a money-credit system. The foreign central bankers who have demanded that such promises be kept in recent years, with a resulting loss to the United States of more than \$5,000,000,000 in gold, have a clear understanding of the difference between promises to deliver something and the thing promised. Should not American students be equally well informed?

Apparently in an attempt to justify increasing Government debt, Professor Samuelson asserts (p. 399), "If there were no public debt ... (1) charitable institutions would have to be supported by public and private contributions more than by interest on endowments, (2) social security and annuities would have to take the place of *rentier* interest, and (3)

service charges by banks would have to be increasingly relied upon instead of government bond interest.” Evidently he is not aware that virtually all the private colleges in the United States, until a few decades ago, depended largely on endowment funds invested primarily in other than government bonds. Moreover, in those days, when there were almost no U.S. Government bonds in existence, most banks not only had no service charges but also paid interest even on checking account balances in excess of specified minimum amounts.

Perhaps in an effort to add what he considers to be the weight of recognized authority to his assertions, Professor Samuelson repeatedly says that most economists agree with various views he offers (pp. 9, 241, 242, 256, 298, 299, 364, 375, 380, and 829). For example, he asserts (p. 241) that the basic Keynesian analysis is “... increasingly accepted by economists of all schools of thought ...,” and on the next page he says of his so-called synthesis of Keynesian and older economics, “The result might be called ‘neoclassical economics’ and is accepted in its broad outlines by all but a few extreme left-wing and right-wing writers.” The Keynesian analysis assuredly is not accepted by members of the Economists’ National Committee on Monetary Policy. This group of experts in the money-credit field cannot properly be classified as “left-wing” or “right-wing” inasmuch as they are primarily economic scientists. They constitute a substantial number (75) of those who specialize in this field.

Sweden’s Economy

Another interesting point is the Professor’s reference to Sweden. (Sweden has for some years been regarded by the Keynesian state planners and government interventionists as a nearly ideal country because of its, at first, seemingly successful application of semi-socialistic and spend-for-prosperity notions.) The reference is, “A great economic statistician, Simon Kuznets of Harvard, has recently shown that the leading Western nations have for decades been averaging rapid rates of growth of output per head. How rapid a growth? About 10 percent per decade for France and England; about 16 percent for Canada and the United States. And almost 30 percent per decade for Sweden!” (p. 116)

Persuading students to believe that Sweden is now exceeding, or recently has far outpaced, other nations of Europe and the United States in economic growth seems an inexcusable falsification of the record to this reviewer. Sweden’s economy once was growing at the rapid rate indicated, but that was before the semi-socialist planners and spend-for-prosperity theorists gained a dominating influence in Sweden’s government during the fourth decade of the present century. During the 1950’s Sweden’s industrial production per capita increased about one-quarter; during the

same period, in the rest of Western Europe the increase was nearly twice as great and in West Germany about four times as great.⁴ Figures now are available for all of the 1960's and the data for Sweden are about 20 percent better than those for West Germany. However, there already is adequate documentation that the rate of economic growth in Sweden since World War II was much less than the comparable rates in much of Europe, including the nations that suffered extensive war damage during World War II.

Keynesian "Theory"

In part 2, Dr. Samuelson presents the familiar Keynesian notions with numerous charts and formulas. The subject matter is presented much as a chemist or a physicist would write about an accepted theory in his field. There the resemblance ends, however. What Professor Samuelson offers is not a scientific theory but a set of hypotheses for which proof has not been provided between the covers of his book or elsewhere. Unwary students may at first assume that what is called the "theory" of income determination is like Einstein's theory of relativity in that adequate testing of the factual implications of the original hypothesis has elevated it to the rank of a warranted assertion or accepted theory.

In attempting to convince students that the Keynesian notions are sound, Professor Samuelson reveals what I assume is his understanding of modern scientific method. After describing the Keynesian hypothesis concerning income determination, Professor Samuelson says (p. 262), "An arithmetic example may help verify this important matter." He then offers a table not of recorded economic changes but of changes that he has imagined and that merely summarize in figures his earlier argument.

Then, on the next page, he asserts, "Now we can use Fig. 5 to confirm what has just been shown by the arithmetic of Table 1." Students at an institution like M.I.T. are accustomed to the idea that verification involves proof of some kind, that what has "been shown" has been demonstrated or proved, and that what has been "confirmed" has been "established firmly" or put "beyond doubt," to use phrases from the Oxford dictionary. And that is what Professor Samuelson seems to believe he has accomplished. Has that been done?

The first statement of the Keynesian notions is in words. The second summary statement, the table, is in the symbols of mathematics or shorthand logic. The third, and the final alleged confirmation, is a chart presenting the imagined relations in pictorial form. The Professor, although

⁴ These data are derived from the monthly and the 1967/68 supplement to *International Financial Statistics* published by the International Monetary Fund.

he seems not to realize it, is saying something like this: “Here is my story about the Keynesian revelation; next, I verify it by writing it in shorthand; finally, I prove it beyond doubt by drawing a picture of it.” One wonders how students at M.I.T. could be induced to regard seriously such anachronistic nonsense. Aristotle convinced some of his disciples 2,000 years ago that such procedures provided useful knowledge, but that was long before modern methods of scientific inquiry had exposed the futility of such dialectical quests for certainty.

The Keynesians generally have followed the outmoded procedure of judging the usefulness of a theory by its plausibility instead of by checking its implications against measured economic changes. In the realm of science, theory is controlled by the facts. When scientists find facts at variance with theory, that theory is discarded; but many Keynesian economists do not even bother to seek the measurements of changes implied by their theory. In this respect, Professor Samuelson simply is following the too-long established precedent in his field.

The “Neoclassical Synthesis”

Professor Samuelson claims (Preface p. vii, p. 403, and elsewhere) that he has achieved or is in the process of achieving a “neoclassical synthesis” that will join in fruitful wedlock classical economics and that portion of the Keynesian ideas deemed by Samuelson to be worthy of the union. If what von Mises or Hayek, as examples of economists in the classical tradition, have written about the Keynesian ideas may be taken at face value, either would be decidedly reluctant to see his brainchild a “groom” at the “wedding” Professor Samuelson plans.

Moreover, the present writer’s position is that such a “wedding,” whether of the “shotgun” variety or otherwise, would not be fruitful for the simple reason that both bride and groom, that is, the Keynesian notions and much of classical economics, are “dead ducks.” My reasons for so believing have been discussed in detail in other chapters. Here there is room for only a summary explanation.

The methods of conducting inquiries applied by the Keynesians and to a substantial extent by the classical economists were the older, now obsolete methods. Briefly, those methods included Aristotelian logic, introspection, what may be called secular revelation (a process at which Lord Keynes was especially adept), and the quest for certainty so long persisted in also by philosophers. Such methods give great weight to the internal logical consistency and general plausibility of an hypothesis but accord little weight to the desirability of testing its logical implications against measurements of economic changes before offering the hypothesis as a warranted assertion applicable to the problems of men.

Anyone who will observe its consequence in several fields can see that a revolution in methods of inquiry is well underway in the behavioral sciences, including economics. This revolution is comparable to the Galilean revolution of 3 centuries ago in the physical sciences and to the similar revolution in the physiological sciences marked by the advent of graduate schools of medicine more than a hundred years ago.

Evidently Professor Samuelson is determined to continue as one of the last of the alchemist-economists, using as his model Lord Keynes (whom Professor Samuelson on page 241 describes as “a many-sided genius”). As everyone who recalls the discussions in economic journals during the 1930’s is well aware, Lord Keynes’s method of escape from every blind alley in which his economist critics nearly cornered him was the simple process of abandoning successive positions and dashing down other blind alleys. The verbal skill that facilitated his Houdini-like “escapes” was widely accepted as proof of his “brilliance” by those to whom the scintillating flash of words seemed more significant than the humdrum facts preferred by others who have rejected perpetual-motion theories and alchemists’ dreams. However, following in Lord Keynes’s footsteps may not be practicable. Times have changed; the revolution in methods of inquiry proceeds with increasing speed; and an emulator of Lord Keynes may discover, as the alchemist professors did long ago, that the market for outmoded textbooks can rather suddenly disappear.

An alternative would be to learn as rapidly as possible and apply modern methods of conducting scientific inquiries in the behavioral field. This choice could in time make Professor Samuelson an eminent associate for the distinguished scientists on the faculty at M.I.T. instead of the anachronistic pseudo-scientist that he now seems in the light of our present understanding of scientific method.

In spite of its flaws, we have reviewed this book because it is reported to be the most widely used economics textbook today. Many college students are being indoctrinated with the Keynesian notions although much evidence indicates that application of these notions has brought Sweden to the brink of disaster, all but ruined France prior to the fiscal and other economic reforms effected by De Gaulle, and has greatly endangered the future of the United States, to mention only a few of the consequences. Resolute discarding of such notions made a vital contribution to the “miracle” of West German economic growth. In the light of these developments, the importance of teaching American youth scientifically warranted assertions instead of the doctrines offered by Professor Samuelson seems obvious.

III.

MEDIEVAL SCHOLASTICISM vs. MODERN SCIENCE:

A Review of von Mises's *Human Action, A Treatise on Economics**

IN this comprehensive volume (nearly 900 pages) the distinguished author has presented the results of a lifetime of study, a work that deserves careful analysis. Its principal value, in my opinion, is not found in the conclusions, pleasing as they may be to proponents of economic freedom, nor in the criticism of “welfare” economics, valid as such criticism may be; rather do I find that the book’s principal merit is its frank statement of the author’s method, his assumption as to what constitutes knowledge in the economic field, and his procedures based on that assumption. Dr. von Mises’s treatise illustrates the main weaknesses of economics as it is written about and widely taught today. As evidence of the urgent need for reconstruction in economics, this book perhaps is without an equal.

Lest these comments be misinterpreted as disparagement of the author or belittlement of his efforts, I assure readers that such is not my intent. I consider this volume one of the outstanding and representative works in the field. Dr. von Mises was professor of economics at the University of Vienna for a full quarter century. Subsequently, for 16 years prior to World War II, he was Professor of International Economic Relations at the Graduate Institute of International Studies in Geneva. More recently he has been a visiting professor at the Graduate School of Business Administration, New York University. Thus for more than a half century, Dr. von Mises has taught and written in his chosen field. His writings indicate that his scholarly ability and the breadth and depth of his work in the field are equaled in few living men.

In this brief review, there is not room for an adequate discussion of the author’s comments on recent economic developments. With much of his criticism of specific policies and proposals, particularly those that constitute a revival of “mercantilism” as practiced in France during Colbert’s time nearly 3 centuries ago, I agree; and with his conclusion that many of the proposals for economic planning are one-way routes to socialism or communism, I likewise agree. However, much of my criticism of these recent developments would be on grounds that underlie, as it were, von Mises’s criticisms; in short, I doubt the validity and usefulness of much that he criticizes on the same basic grounds on which I question his own work.

We begin with a question, the answer to which is basic to economics as

* *Human Action, A Treatise on Economics*, by Ludwig von Mises, New Haven: Yale University Press, 1949.

well as to all other fields of inquiry. What is knowledge? The only satisfactory answer that I have found is, to use Dewey and Bentley's phraseology, the knowing and the known.¹ In short, the only referent (thing referred to) that I can find for the word symbol "knowledge" is the integrated knowing and the known.

I am not prepared at this writing (any more than Dewey and Bentley apparently were in 1949) to urge that the symbol "knowledge" can now be safely used to specify (or scientifically name) anything. But I do feel reasonably sure that, if the symbol "knowledge" is found satisfactory for scientific discourse, whatever it refers to will have this characteristic: the knower's "knowledge" will enable him to predict and control, predict what will occur under certain circumstances and in the light of that prediction to control (or adjust) in some degree either man's behavior or the external environment, or both. Facilitation of prediction and control is an essential characteristic of "knowledge" in what seems to be the emerging modern scientific usage of this word symbol.

We turn now to von Mises's usage of economic "knowledge." The clearest summary statement seems to be that on page 858, which is as follows:

"What assigns economics its peculiar and unique position in the orbit both of pure knowledge and of the practical utilization of knowledge is the fact that its particular theorems are not open to any verification or falsification on the ground of experience.... It can never, as has been pointed out, prove or disprove any particular theorem.... The ultimate yardstick of an economic theorem's correctness or incorrectness is solely reason unaided by experience."

Additional light on von Mises's answer to the question, What is (economic) knowledge, is provided by the following:

"It is impossible for the human mind to conceive logical relations at variance with the logical structure of our mind." (p. 25)

"For the comprehension of action [including economic behavior] there is but one scheme of interpretation and analysis available, namely, that provided by the cognition and analysis of our own purposeful behavior." That is, introspection. (p. 26)

"The human mind is not a tabula rasa on which the external events write their own history. It is equipped with a set of tools for grasping

¹ John Dewey and Arthur F. Bentley, *Knowing and the Known*, Boston: Beacon Press, 1949, particularly pages 296 and 297, from which the following is quoted: "*Knowings*: Organic phases of transactionally observed behaviors. Here considered is a familiar central range of namings-knowings. *Knowns*: Environmental phases of transactionally observed behaviors."

reality. Man acquired these tools, *i.e.*, the logical structure of his mind, in the course of his evolution from an amoeba to his present state. But these tools are logically prior to any experience.” (p. 35)

“However, the sciences of human action differ radically from the natural sciences. All authors eager to construct an epistemological system of the sciences of human action according to the pattern of the natural sciences err lamentably.

“The real thing which is the subject matter of praxeology, human action, stems from the same source as human reasoning.... The theorems attained by correct praxeological reasoning are not only perfectly certain and incontestable, like the correct mathematical theorems. They refer, moreover, with the full rigidity of their apodictic certainty and incontestability to the reality of action as it appears in life and history. Praxeology conveys exact and precise knowledge of real things.” (p. 39)

“Economics is not, as ignorant positivists repeat again and again, backward because it is not ‘quantitative.’ It is not quantitative and does not measure because there are no constants. Statistical figures referring to economic events are historical data.” (p. 56)

“Such problems do not allow any treatment other than that of understanding.” (p. 57)

“All that is needed for the deduction of all praxeological theorems is knowledge of the essence of human action.... We must bethink ourselves and reflect upon the structure of human action. Like logic and mathematics, praxeological knowledge is in us; it does not come from without.” (p. 64)

“The fundamental logical relations and the categories of thought and action are the ultimate source of all human knowledge.” (p. 86)

I have quoted at length in order to minimize the risk that presenting material out of its context might misrepresent the author’s views. How does Dr. von Mises’s answer to the question, What is knowledge, compare with the answer that seems to be emerging from the latest studies of man’s knowing behavior? Where does his understanding of knowing and method fit in the historical succession of man’s procedures of knowing? Evidently, he has not abandoned the Greek ideal, the quest for certainty; on the contrary, he is convinced that he has succeeded where so many others have failed and in spite of the fact that modern men seeking knowledge no longer consider his objective a reasonable goal.

A Leap Backward

Dr. von Mises denies not once but several times that his theories can even be disproved by the facts. This point of view represents a leap

backward to Platonic idealism or one of its subsequent offspring in various disguises. Theories thus derived are medieval scholasticism, albeit on a par with much that is taught as economic knowledge today.

There is even ground for alleging that some aspects of his methods are even farther out of date and have their roots millions of years ago. What else are his assertions about “conception and understanding” if not an acceptance of revelation as a road to knowledge?

Dr. von Mises’s conception of the mind and its function in his search for knowledge may be compared with: “Reason pure of all influence from prior habit is fiction.”² Also of interest in this connection is the following: “Many who think themselves scientifically emancipated and who freely advertise the soul for a superstition, perpetuate a false notion of what knows, that is, of a separate knower ... by dismissing psychology as irrelevant to knowledge and logic, they think to conceal the psychological monster they have conjured up.”³

Like the Greeks, Dr. von Mises disparages change: “Praxeology is not concerned with the changing content of acting, but with its pure form and and its categorial structure.” (p. 47) No one who appreciates the long struggle of man toward more adequate knowing would criticize Aristotle greatly for his adoption of a similar viewpoint 2,000 years ago, but after all that *was* 2,000 years ago; surely economists can do better than seek light on their subject from a beacon that was extinguished by the Galilean revolution in the 17th century. In this connection, Dr. Ratner again is helpful: “Modern scientists, however, began by taking precisely the world of change as their subject for scientific study, and to help them on their way, they introduced the method of experimentation which is no less and no other than a method whereby the natural changes going on can be further increased and complicated in manifold ways by changes deliberately made. From the Greek point of view (and in this case, *not* excepting any Greek), this is confounding confusion, science gone insane. But as events have fully demonstrated, it is science really come to its senses, and intelligence come into its own.”⁴

As for von Mises’s assertion that economists must rely on “cognition and analysis of our own purposeful behavior,” this is the thoroughly discredited mode of knowing by introspection. Moreover, how can even the method of introspection be used if the knowledge praxeology provides is “a priori,” is “not subject to verification or falsification on the ground of

² John Dewey, *Human Nature and Conduct*, New York: Modern Library, 1922, p. 31.

³ *Ibid.*, pp. 176 and 177.

⁴ Joseph Ratner, *Intelligence in the Modern World, John Dewey’s Philosophy*, New York: Modern Library, 1939, p. 52.

experience and facts?” If we find neither experience nor facts when we “analyze our own purposeful behavior,” do we find anything at all?

Von Mises differentiates the natural sciences from what he calls the a priori sciences including praxeology, which he considers the basis for his economics. This, too, is an outmoded distinction, although many, perhaps most, economists agree with von Mises’s views. As for such differentiation, Dr. Ratner comments as follows: “Why is it that in the technical fields of science, the revolution in *method* initiated by Galileo has already been substantially completed, has, in our time, carried through its last fundamental reform, whereas in other fields, including fields as intellectual as philosophy and logic, the revolution is just about now seriously getting under way? The easy answer is to invoke a distinction between ‘natural’ sciences and ‘social’ sciences.... The ‘distinction’ simply repeats, as an explanation, the fact to be explained ... there is a difference in the development of scientific investigation of the natural and the social *because* the former is ‘natural’ and the latter ‘social.’

“The backwardness of philosophy, logic and all social inquiries does not explain the *forwardness* of the natural sciences. It simply exposes and emphasizes the need for an explanation.... Let it be granted, for the sake of argument, that the natural sciences are *now* beyond the reach of influence or connection with social institutions, forces and all that goes with the latter. It is an undeniable fact of modern history — let alone of all human history — that they were not *always* there. Hence the more you conceive the social to be retarding or inherently inimical to the development of science, the more must the ‘natural’ sciences have been able to overcome in reaching their present estate. In so far as the ‘natural’ sciences are *now* distinguished and distinguishable from the ‘social’ sciences it is a *distinction* they have achieved; it is a *result*, not a gift (‘something given’ or a ‘datum’); it is a *consequence*, not a cause. The invocation of the ‘distinction’ between ‘natural’ and ‘social’ *subject-matters* to explain the differences between ‘natural’ and ‘social’ *sciences* doesn’t even explain the differences away. It just leaves them precisely where and as it *finds* them.

“A philosophy or logic of science cannot, without being foolish, take refuge in a ‘distinction’ in subject-matter to explain the advance of the natural sciences in modern times. And the more the ‘distinction’ is asserted to be *in rerum natura* as a ground for explanation, the greater the folly of the philosophy or logic becomes.”⁵

Modern Influence

But the careful reader of von Mises’s treatise will encounter at least one specific example where the influence of modern scientific method seem-

⁵ *Ibid.*, pp. 69-71.

ingly has overcome the author's best intentions. On page 547 *et seq.*, he discusses inflation and business cycles, and on page 798 in criticizing various theories of the business cycle, he emphasizes certain facts of economic life and their relationships. He points out that both production and prices increase during a boom, a development that obviously would be impossible ordinarily without credit expansion. To summarize, he asserts in effect that any theory failing to include the part that credit expansion must play is discredited by the facts.

Now it seems obvious that prices are quite precise measures of the ratios at which some goods exchange for purchasing media or for other goods. Therefore, price rises are measures of change in such exchange relationships. Moreover, any assertion that production has increased implies more or less accurate measurement of the change alleged to have occurred. So also with the expansion of bank credit; it can be known only through measurement stated in terms of the additional amounts of purchasing media involved. Here are the significant aspects, for science, of what is widely called experiment, namely, measurement of change and study of the relationships between or among the changes.⁶

And if we turn to another passage, we find what von Mises thinks of such proceedings. "Those economists who want to substitute 'quantitative economics' for what they call 'qualitative economics' are utterly mistaken. There are, in the field of economics, no constant relations, and consequently no measurement is possible." (p. 55) In such disparaging terms does the author dispose of the statistical laboratorians who provided the test that he insists all theories of the business cycle, including his own, must meet.

I think it to von Mises's credit that he cannot resist the temptation to be a modern economic scientist. Far from considering this an inconsistency that should make him the butt of ridicule, I regard this particular inconsistency as one of his outstanding achievements. If only his fellow economists could similarly break the bonds that shackle them with the past, the science of economics probably would advance much farther, much faster.

Von Mises's Contribution

Finally, I repeat that Dr. von Mises's treatise seems to me an outstanding contribution. He has boldly attempted to explain the assumptions and preconceptions on which his work is based, a task that few economists have had the ability or perhaps the courage to undertake. Unfortunately,

⁶ True, the scope of the experiment is much larger than that of the chemist's experiment in a test tube, but it is on an exceedingly small scale in comparison with the scope of an astronomer's measurements. In the aspects significant for science, and therefore for knowing in the modern sense, this is the essence of experiment.

he has but lightly touched on many semantic problems, particularly that of specification (scientific naming or use of word symbols) but few economists have troubled seriously about definitions in recent years. A few decades ago, the situation was different; nearly every textbook began with attempts to define the terms to be used. But, like many philosophers, the economists have all but given up this aspect of their task, apparently in the hopeless conviction that semantic confusion is a small price to pay for the retention of old and familiar methods.

Therefore, I believe that Dr. von Mises may have contributed far more than he had previously realized to the needed reconstruction in economics. The first task in reconstruction always is the demolition and removal of the structure that must be replaced. That task, I believe he has facilitated.

IV.

WEAKNESSES IN ECONOMICS:

A Review of Burns, Neal, and Watson's *Modern Economics**

IT is a pleasure to find an economics textbook that makes a serious effort to define the basic terms used. Such procedure has the advantage of providing a foundation on which a sound superstructure of theory might be built. That the authors of this book have not taken full advantage of their initial procedure provides one more illustration of the weaknesses in economics as it is taught today.

For example, on page 47 appears the assertion "The United States owes its high rate of capital accumulation chiefly to its wealth of natural resources, especially the energy resources such as coal, petroleum, and water power." At least a half dozen major continental areas of the globe, some of them occupied for many decades by single nations, have equal or greater natural resources. Is it not then possible that the form of economic organization of our society in the United States is chiefly responsible for the high rate of capital accumulation here? Does anyone think that a society in the United States organized into landlords and peasants as in India or into landlords, peasants, and the bandits who preyed upon both as in China, would have accumulated capital so rapidly?

When men who claim to be economists make assertions such as that above, they abandon the methods of inquiry accepted by scientists. Such views are entitled to no more respect than those of individuals who assert that the world is flat, because the methods of inquiry are in some respects similar.

Also, on page 31, the authors attempt to describe conditions during the early days of the Industrial Revolution. They refer to "The evils of an uncontrolled laissez-faire economy,..." apparently without any realization that what they are talking about are the evils of a controlled, semi-free economy, an economy that was controlled by and for the landed aristocracy of Great Britain and merchant industrialist contemporaries. To overlook the vital elements of that control, such as the "Enclosure Acts" of the late 18th and early 19th century, is to be blind to the most obvious and crucial economic facts. Failure to understand that it was not a laissez-faire economy but in large part its opposite that was the breeding ground for the horrible conditions described so well by Dickens and others; failure to understand that a close approach to laissez-faire did exist in parts of the United States where economic development was most rapid and its fruits

* *Modern Economics*, by Arthur Edward Burns, Alfred C. Neal, D. S. Watson, edited by A. G. Hart, New York: Harcourt, Brace and Company, 2nd Edition, 1953.

were most widely dispersed (and where a “laboring poor” in the British sense practically did not exist); failure to understand those facts reflects the lack of scientific inquiry on the part of the authors.

Similarly, on page 329, the authors apparently accept the Malthusian explanation for poverty in the Far East. They devote not a sentence to consideration of the possibility that other factors, such as unstable governments, religious taboos, organized banditry, and entrenched landlordism have an important bearing on the situation.

On a par with the “chemistry” of the early alchemists is the following statement from page 582: “The decrease in national output and employment in 1937-38 showed that the depression itself was more obstinate than many had realized.” If this is not, from the scientific viewpoint, childish anthropomorphism, it at least is an “off the cuff” judgment entirely unsupported by citation of facts or theory and made with complete disregard, as far as the reader can discover, of any other possibilities such, for example, as the possibility that the 1937-38 recession was the usual aftermath of inflationary follies.

Such weaknesses in economics textbooks probably account in large part for the popularity of economic panaceas such as the spend-for-prosperity notions, and the similar weaknesses of predecessor texts perhaps account in no small part for the Alger Hisses and other misguided intellectuals of our time.

V.

NOT SCHIZOPHRENIA, DR. FRIEDMAN

IN a recent essay,¹ Dr. Milton Friedman concluded that the “long-run optimum” quantity of “money”² will be that which results in a rate of decline in prices sufficient to offset the “nominal rate of interest.” In other words, if the market rate of interest were 5 percent on loans involving virtually no risk, the quantity of “money” should be reduced as needed to insure an annual decrease in prices at the rate of 5 percent annually.

However, because of various “practicable considerations,” Dr. Friedman considered this conclusion, although apparently proven to his satisfaction, to involve great risks. He suggested that holding “the absolute quantity of money constant” would be “a policy fairly close to the optimum,” and then suggested that “a rise in the quantity of money at the rate of about 2 percent per year” as an “especially appealing” compromise that he believed would “stabilize the price of factors services.”

Dr. Friedman added what he called “a Final Schizophrenic Note” in which he pointed out that he has heretofore advocated the policy of keeping prices stable by increasing the quantity of money at a rate “something like 4 to 5 percent per year.” He asserted that he did “not want to gloss over the real contradiction between these two policies.” His two “reasons” for the contradiction are: (1) that his “5-percent rule” was based on “primarily short-run considerations”; and (2) what he called a “more basic reason,” that he had not earlier made the analysis presented in the latest essay.

Dr. Friedman offered as his ultimate conclusion, at the end of his “Final Schizophrenic Note,” the belief that shifting to his 5-percent rule would provide such a great gain in comparison with results actually achieved in the past that it would “... dwarf the further gain from going to the 2-percent rule....” For this reason he will “continue to support the 5-percent rule as an intermediate objective greatly superior to present practice.”

One of the difficulties encountered in analyzing Dr. Friedman’s work is his use of words for loose or vague characterization rather than for accurate specification. For example, how long for him is the “long run,” and how short is his “short run”? Does the phrase “present practice” designate the money-credit policies of the Federal Reserve Board in the present century since it was formed in 1914 or those during the period since World

¹ Milton Friedman, “The Optimum Quantity of Money,” in a book of the same title including other essays, Chicago: Aldine Publishing Company, 1969.

² Quotations are used primarily to indicate Dr. Friedman’s choices of words. In many instances loose or vague characterization rather than accurate specification seems to result.

War II, or those during the decade of the 1960's (which still would have been the "present" decade as of 1969 when the essay was published). Surely, Dr. Friedman was not referring to the almost negligible increase in the "money supply" that occurred late in 1969, which presumably was not apparent when he wrote his essay.

What Actually Occurred

In any event, the facts involved may be examined. During 1914 to 1969, the "money supply," as described by Dr. Friedman to include currency in circulation, plus demand deposits or checking accounts, and the time deposits of the Nation's commercial banks, increased from \$19.2 billion to \$412.7 billion. This increase is equal to an average annual compound rate of 5.7 percent. The index of prices paid by consumers increased from 35.4 to 131.3, an average annual compound rate of 2.4 percent; and the index of prices of commodities at wholesale increased from 36.8 to 115.1, an average annual compound rate of 2.2 percent.

The annual rates of increase in the "money supply" are not greatly different if one uses the average of the 5 years 1924 to 1928 as a base period instead of 1914. (The compound annual rate of increase for the "money supply" since then has been 4.9 percent.) And, if one used the 3 years 1947-49 as a base period, the annual rate of increase also is not greatly different (likewise 4.9 percent). To summarize, Dr. Friedman's 5-percent rule was approximately the "present practice" in each of the periods referred to (*i.e.*, 1914 to 1969, and especially 1924-28 to 1969, or 1947-49 to 1969).

On the other hand, if Dr. Friedman's 2-percent rule had been applied and prices had remained stable, as his analysis indicates they should have, the dollar still would have its pre-World War II buying power, more or less. Assuredly, the price consequences of shifting to the 2-percent rule would greatly have exceeded the price consequences of having adhered more nearly to Dr. Friedman's 5-percent rule. Yet he has concluded that "The gain from shifting to the 5-percent rule would, I believe, dwarf the further gain from going to the 2-percent rule...." Apparently, he believes that, although the price consequences of a shift to the 5-percent rule from the actual practice of some recent period probably would have been small in comparison with the price consequences of a shift to the 2-percent rule, the "gain" would be much greater in the first instance.

For what did Dr. Friedman use "gain" as a label or name? Did he attempt to designate an increase in real wealth during the period he had in mind, or an increase in consumable products and services, or what does he refer to? In the absence of accurate specification, or scientific naming, no answer seems to be available.

This raises the question, How can one fairly and usefully appraise Dr. Friedman's prescription for the Nation's ailing money-credit system? When an economist writes his prescription not with the illegibility (for laymen) achieved by many physicians in writing their prescriptions, but with what seems a kind of poetic license in his choice of technical terms, what can be done?

Friedman vs. Samuelson and Keynes

That Dr. Friedman's is one of the superior intellects exploring obscure aspects of the "dismal science" seems apparent. Unlike some of the brilliant intellectuals who expound their views on economic matters with similarly great verbal facility, Dr. Friedman has what we should call the saving grace of common sense, to a considerable degree.

Everyone who has read much of his writing and who has heard him discussing various economic problems surely must be impressed by the breadth and depth of his scholarship as well as by what might be called extraordinary verbal facility. Almost invariably he exhibits a sometimes well-justified confidence in the usefulness of what he is sure that he clearly knows. No one can justly accuse him of obscurantism or of unwillingness to take a position when he believes that he can offer a useful policy prescription.

These aspects of Dr. Friedman's personality are mentioned not for the purpose of praising him, although we think that he well deserves much praise, but in order better to describe his methods of inquiry and compare what he has done or seems to think he has done with what modern scientists purport to do. (Unfortunately, the word "science" has become so widely misused that we use it as little as seems practicable herein.)

Dr. Samuelson, a leading disciple of Lord Keynes and having views at variance with those of Dr. Friedman, considers himself a scientist; and his presence as head of the Department of Economics at Massachusetts Institute of Technology, one of the Nation's leading scientific institutions, would seem to support that viewpoint. Dr. Friedman also regards himself as a scientist. Their opinions differ, and also do not accord with those held by the relatively few *modern* economic scientists conducting research in the money-credit field.

Varying Methods of Inquiry

The difference in methods of inquiry accounts in large part, we believe, for Dr. Friedman's need to add a "Schizophrenic Note" to his essay. His methods, like those applied by Keynes, Samuelson, and many other earlier as well as still living economists, are the traditional methods applied for many centuries. More than 300 years ago, those earlier methods were

superseded in the physical sciences when Galileo, among others, began developing the new and more useful methods of inquiry. Less than 2 centuries later, the new methods were being applied successfully in the physiological sciences, including medicine. Only in the present century have adequate descriptions of the new methods and their applicability in the behavioral sciences, including economics, become available. Application of them still is in the early stages.³

Dr. Friedman's methods of inquiry as revealed in his essay, "The Optimum Quantity of Money" may be described as:

- a. Imagining a simple society, which he labels "Hypothetical Simple Society";
- b. Assuming some (apparently believed to be) axiomatic or unvarying types of behavior;
- c. Describing in words what would logically follow if certain changes were initiated;
- d. Translating into mathematical terminology some of the verbal logic;
- e. Presenting charts showing the hypothetical mathematical relationships;
- f. Drawing conclusions that, when modified by certain "practical considerations," become a suitable basis for policy recommendations.

Although he did not say so explicitly, Dr. Friedman's confidently offered prescription clearly is based on the belief that he had successfully carried out useful inquiry. He presumably regards his use of mathematics as a step in his proof and the charts as graphic confirmation. If so, he has followed precisely the same method of inquiry as that used by Dr. Samuelson at one point in his widely used economic textbook.⁴ That two able

³ What we consider the best report available to date on modern methods of inquiry is found in John Dewey, *Logic, The Theory of Inquiry*, New York: Holt, Reinhart and Winston, 1938. However, as Dewey himself later pointed out, aspects of his description were in error or at least could have been stated better if he had had his prolonged correspondence with Arthur Bentley before he had written the *Logic*. (See Sidney Ratner and Jules Altman, Eds., *John Dewey and Arthur F. Bentley: A Philosophical Correspondence 1932-51*, New Brunswick: Rutgers, 1964. In their articles later published in book form (John Dewey and Arthur Bentley, *Knowing and the Known*, Boston: Beacon Press, 1949), they provided much useful material for anyone seeking to understand and apply the new methods. As an aid in understanding the older methods and thereby more clearly grasping the significance and usefulness of the new, we suggest Joseph Ratner's 241-page "Introduction" in his volume *Intelligence in the Modern World*, New York: Random House, 1939. Because John Dewey's style of writing makes difficult for many readers a grasp of the new ideas presented, we suggest study of the publications listed above in the reverse of the order given.

In the present analysis of Dr. Friedman's work we can provide little more than an outline and must leave to readers the more adequate examination of the notions we summarize.

⁴ See the discussion of his method of inquiry in our review of his textbook in Chapter III.

individuals both using the same *methods* arrive at conflicting results would provide reason for reconsideration to inquirers applying more modern methods of inquiry.

The Role of the Laboratorian

Inasmuch as Dr. Friedman referred to earlier work,⁵ it should properly be regarded as an exhibit in connection with the article under consideration. In that earlier volume, Dr. Friedman and his co-author provided detailed and extensive records of what he has called “The Key Facts As We Now Know Them.” Therefore, Dr. Friedman cannot justly be accused of ignoring the role of the laboratorian in the course of scientific inquiry.⁶ Nevertheless, that Dr. Friedman has failed to use the methods of modern inquiry is clear.

Modern inquirers into physical, physiological, and behavioral (including economic) problems do observe what seem, in the course of inquiry, to be the pertinent facts (consisting largely of measured changes). Modern inquirers do develop hypotheses and carefully check the internal logic as well as the logical implications of such hypotheses. At this point, however, the resemblance to Dr. Friedman’s methods ends.

Modern inquirers do not assume that transpositions of verbal logic to mathematical forms have any evidential value or constitute a successive step in the proof of anything other than their own facility with mathematical transpositions. In recent years, much economic literature has been loaded, some would say overloaded, with such mathematical transpositions. A recent issue of *The American Economic Review*, for example, offered such mathematical transpositions in nearly every article. The econometricians long have followed this procedure.

Inasmuch as we have urged that economist should have a thorough grounding in the differential calculus and advanced statistics,⁷ we do not

⁵ M. Friedman and A. Schwartz, *A Monetary History of the United States 1867-1960*, Princeton, New Jersey: Princeton University Press for the National Bureau of Economic Research, 1963.

⁶ The laboratorian is the measurer of changes. He and the theoretician or developer of hypotheses jointly participate in modern inquiry. In some fields of inquiry, the laboratorian initiates the changes that he measures. In economics, the laboratorian usually functions in a statistical laboratory and records for future reference the changes that occur in such economic aspects of life as prices, the quantity of “money” in use, wage rates, etc. On rare occasions an inquirer may be found who combines within himself both the expert statistician and the expert theoretician or developer of hypotheses. (In recent years, many economists seem to prefer to be considered model builders rather than theoreticians, but the function in inquiry is the same.) In this connection see Rollo Handy and Paul Kurtz, *A Current Appraisal of the Behavioral Sciences*, Great Barrington: Behavioral Research Council for Scientific Inquiry into the Problems of Men in Society, 1964.

⁷ See Chapter IX.

wish even to appear to belittle the usefulness of mathematics in inquiry. But the viewpoint of modern inquirers is that mathematics is, in a manner of speaking, shorthand logic. The shorthand symbols used greatly facilitate various transpositions and analysis of possible relations among things (including events). The fact that verbal descriptions of what happens or may happen under certain circumstances can be formulated in mathematical symbols adds no assurance that the descriptions are sufficiently accurate to be useful. No modern inquirer would draw conclusions, much less offer policy prescriptions, merely because he could successfully express his verbal argument in the shorthand of mathematics.

The Function of Hypotheses

How does a modern inquirer use hypotheses? He uses hypotheses, including their logical implications, as signposts pointing to aspects of the problem requiring further research. Usually this requires more measurements of changes among the facts already considered, sometimes the consideration of new data not previously considered pertinent to the problem, and sometimes the discarding of what had seemed to be pertinent facts. In short, modern inquiry involves the mutual efforts of theoretician and laboratorian (even if the efforts are made by one individual). In the course of inquiry many more measurements of changes than those originally observed will have been made; hypotheses will have been formulated and tested (usually in parts rather than as a whole) by reference to the developed facts; parts or sometimes entire hypotheses will have been discarded or modified; and in the end the modern inquirer can offer a conclusion, an assertion warranted by the research done but certified only as hopefully useful, not as ultimate TRUTH or as a panacea for economic ills.⁸

Nothing thus far asserted should be interpreted as implying that the conclusions developed by outmoded methods of inquiry necessarily are wrong. Quite the contrary. The medieval methods still so generally applied in economics can, and have been, used to develop contradictory conclusions, one of which frequently was useful. Usually in such instances the practical experience and common sense of the inquirer guided him in selecting his assumptions or in rejecting conclusions that seemed to him incongruous.

However, adverse consequences may follow when the conclusions of inquiry conducted by medieval methods are used as guides to policy prescriptions. For example, Dr. Samuelson in an early edition of his

⁸ Although the essay written by Dr. Friedman alone reveals an apparent unfamiliarity with modern methods of inquiry, in other writings for which Dr. Friedman is listed as co-author, we find evidence of greater familiarity with modern methods. Evidently, Dr. Friedman either has not read or has not fully grasped the significance of much that his co-authors have written.

textbook advocated a rise in prices approximating 5 percent annually. In subsequent editions he successively reduced this percentage until in the latest he seems to prefer “gently rising” prices. If his original policy prescription had been followed to date, the buying power of the dollar would be only 21 cents of the 1940 dollar instead of 37 cents, a quite serious difference for holders of savings and life insurance.

Dr. Friedman’s Prescription

Consider also Dr Friedman’s policy prescription of ever-expanding money supply at the rate of 5 percent annually. In his analysis of the 1929 to 1933 contraction of business activity, the slide into the Great Depression, Dr. Friedman blamed the Federal Reserve Board, arguing that it greatly contracted the money supply. Evidently, Dr. Friedman assumed that the prosperity of the 1920’s was soundly based in all important respects and that the Great Depression was primarily a consequence of improper money-credit policies in the 1930’s.

An alternative hypothesis describes the prosperity of the 1920’s as forced by inflating (excessive expansion of the “money supply”), which resulted in numerous economic distortions, such as the preparation of Florida subdivisions and apartment hotels far in advance of economic need and the construction of high-rise buildings in the Nation’s major cities to a greater extent than could be justified by occupancy. This hypothesis includes the money-credit aspect of developments, prolonged inflating; and a logical implication of it is that the contraction of business activity from 1929 to 1932 reflected the misapplication of resources and the reorientation of economic activity required in order to remove distortions and make possible orderly economic growth. Continued application of Dr. Friedman’s panacea (expanding the “money supply” continuously at the rate of 5 percent annually) might have made possible far greater distortions of the economy with even more adverse consequences. Something like this apparently has happened since World War II, during a prolonged period when Dr. Friedman’s policy prescription has been closely approximated.

In a recent article,⁹ Dr. Friedman has discussed the problem confronting Dr. Burns, the new Chairman of the Federal Reserve Board. In what we believe is a repetition of an erroneous analysis of the 1929 to 1932 contraction of business, Dr. Friedman asserted, “Burns takes office as the economy not only is slowing down but seems on the verge of sliding into a full-fledged and fairly severe recession — thanks to an unduly restrictive monetary policy.” The implications are that the economy is “fundamentally sound,” as Mr. Hoover repeatedly asserted in the early stages of the Great Depression, and that the only or at least the decisive adverse influ-

⁹ *Newsweek*, February 2, 1970.

ence is the approximate leveling off instead of continued expansion of the “money supply” for several months.

Conclusion

Our extensive research on money-credit matters during the past 35 years has revealed aspects of the problem that Dr. Friedman apparently did not consider worth mentioning if, indeed, he was aware of them. For a quarter of a century almost continuous inflating (issue of excess purchasing media by the Nation’s commercial banking system) has fostered distortions in the economy much more extensive than those of the 1920’s. For example, the inflating at that time barely was sufficient to maintain prices generally on a plateau some 40 percent above 1914 prices; but during the period since 1940, prices have risen so far that holders of savings and life insurance have lost nearly \$700 billion of real wealth. More than half of the Nation’s elderly have been reduced to the poverty level by the continued depreciation of the dollar and other developments. In the meantime, construction costs, for example, have skyrocketed to 5.4 times 1940 levels. Space is not available here to describe the numerous economic distortions now existing. That they will not be corrected, thereby enabling a restoration of sound economic growth merely by applying Dr. Friedman’s panacea, seems to us highly probable.

In the months and years ahead we may witness an experiment with Dr. Friedman’s panacea somewhat similar to Mr. Roosevelt’s experiment with Dr. Keynes’s spend-for-prosperity panacea in 1934-35. We doubt that the end result will be much more desirable than was the severe depression of 1937-39.

Presumably, Dr. Friedman’s labeling of his seemingly contradictory conclusions as “schizophrenic” was not done on the advice of a psychiatrist but was merely a somewhat vague means of disarming his critics. In any event, our analysis suggests that the difficulty is *not* schizophrenia, but simply the retardation of Dr. Friedman’s development, at the medieval level, as a scientific inquirer.

VI.

ON THE PHILOSOPHY OF THE SOCIAL SCIENCES*

By May Brodbeck

WHEN, in recent years, philosophers of science attend to physical sciences, their activity centers on the analysis and clarification of the methods and theories of these sciences. Thus philosophers have made remarkable contributions to our understanding of mechanics, the relativity theory, the quantum theory, probability, and geometry, as well as to the foundations of mathematics. More general discussions about theory construction, explanation, and concept formation always are illustrated and illuminated by reference to specific theories within science. In our generation, most philosophers of science have taken for granted that physical science is descriptive, empirical, functional, and — leaving aside controversial interpretations of quanta — deterministic.

With Galileo's research on falling bodies as our paradigm, we may say that science is descriptive because the body's behavior is explained by giving the mathematical formula for the process, not in looking for its purpose; it is empirical because the variables that enter into these formulae denote observables or are defined in terms of the observable, unlike such an element as the soul or other entelechy. It is functional, in the mathematical sense, for no distinction is made between the "accidental" and the "essential"; the variables are interdependent in determinate ways and the relations between all of them must be taken into account in order to describe accurately what happens. That in principle the future is completely predictable from the present follows as a corollary, no leeway being left for accident or for purpose from the point of view of the observer. Such is the frame of reference in terms of which special theories are analyzed by philosophers of science and about which, elementary textbook writing apart, they say little. In the social sciences, however, philosophers still argue about the frame of reference.

Contemporary theory of the social sciences follows four major patterns. I shall simplify to make them stand out clearly. *First*, there are the self-conscious continuers of the Galilean-Newtonian tradition who insist upon essential identity in method between the social and the physical sciences. These thinkers incline to the view that social theory is, in some sense, reducible to psychology which in turn, through physiology, is reducible to physics. Armed with the laws of physics and the state of the universe at any moment, Laplace's demon extends his purview from the

* Opening paper of a symposium held at the St. Louis meeting of the American Philosophical Association, May 1, 1953. Reprinted from *Philosophy of Science*, Vol. 21, No. 2, April 1954.

physical world to the fate of all mankind. From Mill to the logical positivists, these are the objectivists and reductionists. *Second*, from the early 19th century until our own time, the Romantics insist upon the unique character of what later came to be called *Geisteswissenschaften*, the sciences of man. Organicist, holist, and emergentist, this school deploras the Laplacean vision as radically mistaken, if not viciously demeaning to the dignity of man and his works. It counters the objectivists' fervor for the empirical methods of science with an equally ardent belief in the superiority of *verstehen*. These are the two extreme views. The other two combine, in different proportions, ingredients of these extremes.

The *third* view, represented by Marxists and pragmatists, unites — or attempts to unite — the empiricist, scientific tradition with anti-analytical holism and historicism. The study of society they claim can and must be objective and scientific. But its laws are held to be unique in kind, unlike those of either psychology or physics. The *fourth* conception of the social sciences is reductionist in one sense, anti-reductionist in another. The behavior of groups must be explained in terms of the behavior of individuals; but psychology of individuals cannot be reduced to anything else. Although it opposes organicism, it too rejects the so-called unity of the sciences, insists like the holists on a unique and, perhaps, subjective method in the social sciences. The most articulate contemporary proponent of this position is the economist, F. A. Hayek.¹

These four views all share a common feature. Each of them asserts something about the nature of man and society. For the proponents of physical reduction, human and social characteristics “are” — in some sense of “are” — ultimately atoms and electrons. For the organicist, society is a unique whole. The Marxists and the pragmatists fuse these visions. But for Professor Hayek, neither organic wholes nor electrons take honors — the uniqueness resides in man.

Which, if any, of these four views is true, which false, is a matter of fact. There is, of course, nothing new or remarkable about philosophers, or social scientists turned philosophers, speculating about matters of fact. Abhorring a vacuum, philosophers rush in where scientists fear to tread. What is remarkable is that, with the exception of the idealistic holists who believed they were enunciating special metaphysical truths, these philosophers do not recognize their speculations for what they are.

¹ A generation ago a very similar view was held by the social psychologist William McDougall. Both Hayek and McDougall advocate the reduction to psychology of all other social sciences; both are ardently anti-reductionist in psychology. But Professor Hayek is not, as was McDougall, a vitalist. Again, McDougall thought of psychology as the science of behavior, while Professor Hayek most emphatically does not.

They do not realize that they are anticipating the results of future scientific research, making guesses about what would be shown in an ideally completed science. If we knew everything and also if — though I am not sure what this means — if we *knew* that we knew everything, we should be able to say whether or not there was a deductive relationship between our physics, our psychology, and our sociology. It would depend upon the facts. But our philosophers, reductionist and anti-reductionist alike, make assertions about what *must* be the case, not about what merely happens, as a matter of fact, to be the case. There is also a related error. Some philosophical scientists restrict reality to those areas illuminated by their particular method. Watson, for instance, denied the existence of images in thought or even of any reflective processes at all because of his bias against the method of introspection. His research interest in learning phenomena rather than in genetic factors led him to his equally notorious denial of individual differences. It is no longer novel in the history of ideas to discover values being accorded the dignity of fact or logic for ulterior ideological motives.² Ironically, F. A. Hayek, the latest and most provocative to reveal hidden rationalizations in the thought of others, himself commits an error of the same kind.

As is well-known, Professor Hayek's ideological motive is his horror of the planned economy. The reasons for this reaction cannot and should not be dismissed lightly. However, it is not my purpose to evaluate those reasons here; this requires reflection on our value system, the relative weights we give to specific values within this system, and, also, factual considerations about what various types of economy can be expected to achieve. The issue is moral, not one in the philosophy of science. So, taking my cues from Professor Hayek's recent book,³ I shall here consider some issues pertinent to our field.

The three aspects of Professor Hayek's argument that I shall discuss here are his contentions, first, that the social sciences are "systematically subjective"; secondly, that the use of macroscopic, collective, non-psychological concepts is always illegitimate; and, thirdly, that explanation in social science must be in terms of individual motivation and behavior. The last two points are, of course, intimately related, but they are not quite the same point. All three issues are, independently of Professor Hayek, still unduly controversial in the philosophy of the social sci-

² For an illuminating analysis of the nature of social and scientific ideologies, see Gustav Bergmann, "Ideology," *Ethics*, 51, 1951. Also, valuable discussions relevant to the issues treated below may be found in the same author's "Theoretical Psychology," *Annual Review of Psychology*, Vol. 4, Stanford University Press, 1953, and "Holism, Historicism, and Emergence," *Philosophy of Science*, 11, 1944.

³ *The Counter-Revolution of Science: Studies on the Abuse of Reason*, Glencoe, Illinois: The Free Press, 1952. Unless otherwise noted, all page references are to this book.

ences. Professor Hayek gives the old arguments a new turn whose analysis may, I hope, cast fresh light on the issues themselves.

The “Systematic Subjectivism” of Social Science. Professor Hayek has three reasons for his belief that social science is irredeemably subjective. (1) Its subject matter is human opinions and attitudes; (2) this subject matter can be adequately known only by a subjective method, namely, introspection; (3) mental categories are indispensable for explanation of social phenomena and this, allegedly, implies subjectivism.

(1) Is the subject matter of social science solely human attitudes and opinions? Natural science, as Professor Hayek says, studies things independently of what men do or think about them, while for social science what people think of the world makes a difference. Professor Hayek usefully distinguishes between the natural sciences of man, like the problem of the spread of contagious diseases or the study of nutrition, in which human volition and motivation do not play a role, and the social sciences proper in which they do. The latter “must start from what men think and mean to do.... Not only man’s actions toward external objects but also all the relations between men and all the social institutions can be understood only in terms of what men think about them.” Professor Hayek goes on, “So far as human actions are concerned the things *are* what the acting people think they are.... Our data must be man and the physical world as they appear to the men whose actions we try to explain.”⁴ To drive home his point, he elsewhere makes what he calls the “general and obvious statement” that “no superior knowledge the observer may possess about the object, but which is not possessed by the acting person, can help us in understanding the motives of their actions.”⁵ Thus, according to Professor Hayek, social science is concerned with, and only with, the world as it appears to men, and with men’s beliefs about what they can do in the world as they see it. To what extent this picture of the world fits the facts is wholly irrelevant. In terms of these varying opinions and beliefs social science explains its phenomena; they are not themselves to be explained by social science.

If the “objectivism” of natural science, for Professor Hayek, rests on the dispensability of human beliefs and attitudes, then social science is clearly not “objectivist.” The most ardent advocate of an objective science of behavior would surely agree. People’s beliefs and attitudes do indeed make a difference. But it is simply not true that the social scientist can, with impunity, wholly ignore the difference between what people think of the world and what the world is like. A gulf frequently yawns between

⁴ *Op. cit.*, pp. 33, 27, 35.

⁵ *Individualism and the Economic Order*, p. 60, Chicago: University of Chicago Press, 1948. Also cf. *The Counter-Revolution of Science*, p. 30.

what a man thinks he can do and what in matter of fact the circumstances will permit him to achieve. Unless this gulf is taken into account, the social scientist will be at a loss to account for much in the individual's behavior. It may not be necessary for him to explain the genesis of a delusion — he may well leave that to the psychologist — but that it is a delusion he will need to know. We may predict certain purchases on the basis of a man's beliefs about what he can afford. Only by knowing that he is mistaken can we predict his subsequent trips to the money lender or the bankruptcy courts. The motive power of a belief is not a function of its truth or falsity, but the consequences of acting on a belief will differ as the belief is in fact either true or false.

People are full of foolish fancies about the world and their station in it. These fancies are, as Professor Hayek insists, as real as the true state of affairs, but unless the social scientist is able to compare one with the other, unless he does have "superior knowledge," much of what happens in the social structure must remain a mystery to him. Apropos the necessity for superior knowledge by the observer, can anyone, at this stage of the game, really believe that to explain people's actions we need to know no more than they do, not only about the external world, but about themselves? Is it still possible to say that only people's conscious beliefs and attitudes determine their behavior? I suppose that an affirmative answer is not quite like holding that the earth is flat, yet surely even the most conservative student of human personality would hazard that we are at least not certain that what people say and believe are their motives really are their motives. Professor Hayek is not prepared to concede even this much to the psychology of the past half-century. So far as he is concerned the science of psychology, either normal or abnormal, might never have existed — and this despite Professor Hayek's insistence that social science is rooted in psychology.

(2) Suppose we grant that people's opinions, attitudes, and beliefs and only these are the subject matter of social science. From the observer's point of view these are observable facts like any other, like the facts of the natural sciences, not opinions. Professor Hayek agrees. Human attitudes, organic processes, the stars in their courses — all, in a narrow sense, are different subject matters. In a broader sense, as "facts given to the observation,"⁶ they are one subject matter. Why then does Professor Hayek maintain that there is an unbridgeable gap between the method of natural and of social science?

The two besetting sins of what Professor Hayek calls "scientism," the aping of the method of the natural sciences by the social sciences, are "objectivism" and "collectivism." Of "collectivism" later. "Objectivism"

⁶ *The Counter-Revolution of Science*, p. 28.

Professor Hayek defines as “the various attempts to dispense with our subjective knowledge of the working of the human mind,” that is, all attempts “to do without the knowledge derived from ‘introspection.’” The difference between natural and social science is thus a difference in the kind of observation by which we know natural and social phenomena. Social science is subjective, according to Professor Hayek, not only in the odd sense that its data happen to be, among other things, opinions, but also in the usual sense that the observer’s knowledge of this data is private, based on examination of his own mind. Only by “such inside knowledge of human affairs” can we understand what is meant by “tools, commodities or money, crimes or punishments, words or sentences.” Since other people’s mental states are never directly data for us, we must therefore interpret things wholly “in the light of our own mind.”⁷ This, and not some naive notion of a “rational man” — Professor Hayek is too human and too wise to be caught in that trap — is why all motivation must be conscious. We must have access to it in ourselves in order to understand others. In other words, social science is irremediably subjective because our knowledge of psychology in terms of which it is to be explained is irremediably subjective.

The key to Professor Hayek’s position is his rejection of the fond fiction of a Martian observer deposited on our planet to study human society as we study bees and ants. This, Professor Hayek reiterates, is in principle impossible. The Martian could not study human behavior, not because he would for a time be at a loss, like an anthropologist studying a primitive culture, but because, not being human, he could not understand human nature. Only one with the mind of a man can understand the intentions and motives of men. The familiar retort has not lost its sting: need I be an atom in order to understand one? That Professor Hayek thinks so or, at least, that he thinks there is a crucial difference between these cases is, of course, his link with romanticism, with *Geisteswissenschaften* and *verstehende* social science. This fits queerly with his otherwise tough-minded anti-holism and anti-historicism. What his view amounts to is that a science of psychology is not so much impossible as unnecessary. We already know it for it is about men, which we are. One might also add that we already know sociology and economics for they are about society, in which we live. But, of course, Professor Hayek does not make this extension. A science of psychology is unnecessary because we already know it by knowing ourselves, the only channel to knowledge of others. One patent absurdity of this position has been pointed out: “Only a war-like historian can tackle a Genghis Khan or a

⁷ *Op. cit.*, pp. 44-5, 59, 77.

⁸ J. W. N. Watkins, “Ideal Types and Historical Explanation,” in *Readings in the Philosophy of Science*, eds. H. Feigl and M. Brodbeck, New York: Appleton-Century-Crofts, 1953, p. 740 fn. I shall discuss an issue raised in Mr. Watkins’ excellent paper in my last section.

Hitler!”⁸

I suspect that one reason for Professor Hayek’s belief that social science is subjective is that he has succumbed to the temptation to make a metaphysical position out of some unexamined commonplaces. I refer to the prevailing quips that social science is nothing but documentation of the obvious, the mere statisticalization of common sense.⁹ The state of mind reflected by these stereotypes is shared by all who endorse the view that social science, unlike natural science, explains the familiar in terms of the familiar. The explanations of natural science are “surprising,” we are told, while those of social science are not.¹⁰ Maybe what the psychologists tell us isn’t surprising any more, but once it was. As careful experimentation proceeds in the social sciences, many more surprising things may turn up, not only about the lower depths. It is even possible that a good case could be made for the Martian on practical as well as logical grounds. He might be the best equipped to study human society, simply because he does not share the preconceptions that limit us, the hopes that blind us, and the tendencies to overgeneralize from our possibly eccentric family, friends, and colleagues.¹¹

(3) Professor Hayek’s belief that the indispensability of mental categories makes inevitable the subjectivity of social science raises a more complex issue. Three separate questions are involved: (a) Is a physical explanation of mental processes possible? (b) If it were possible would we still need mental concepts for an adequate description of social facts? (c) If we still needed them, with or without physical explanation, does this imply subjectivism? Professor Hayek denies that a physical explanation is possible, asserts that even if it were we would still need mental categories and, therefore, we would still have to appeal to introspection. Professor Hayek’s intricate defense of these views makes some valid and important points. Nevertheless, I do not believe that they imply what he thinks they imply.

⁹ Of such stuff is the jealous “Hostile Highbrow” reaction by humanistic intellectuals to serious social science. Our inevitable empathy with this reaction should be tempered by the fact that its stereotypes are hardly distinguishable from those of the “Hostile Lowbrow.” There is criticism and criticism, after all. For an amusing and enlightening analysis of Friendly and Hostile reactions to social science see the essay by D. Lerner included in *Continuities in Social Research*, R. K. Merton and P. F. Lazarsfeld, editors, Glencoe, Illinois: The Free Press, 1950.

¹⁰ See, for instance, Watkins, *op. cit.*, p. 735.

¹¹ *An American Dilemma* impressively attests to the success of one attempt to approximate the man-from-Mars condition. The Trustees of the Carnegie Corporation who initiated and financed this elaborately detailed study of the American Negro deliberately “imported” a general director, Dr. Gunnar Myrdal, who would be as free as possible from traditional attitudes both specifically American and imperialistic. True, Dr. Myrdal is human, but by simple extension of Professor Hayek’s thesis, the best man for the job would have been a southern American, perhaps one Negro and one White. Would they not know most intimately American attitudes on this subject?

(a) *The “Paradox” of Complete Explanation.* Professor Hayek’s belief that a complete physical explanation of mental processes cannot be given draws upon a very useful distinction he makes between an “explanation of the principle” on which a process operates and an explanation of a particular state of affairs.¹² The latter permits precise prediction of, say, a particular price change, while the explanation of the principle tells only the general direction that prices will take under certain conditions. Because of the large number of variables involved in any concrete social phenomenon, precise explanation and prediction cannot be made. Professor Hayek applies this distinction between types of explanation to the mental processes of classification. People often classify things similarly even when these things have no recognizable physical properties in common. The variety of objects or actions people call tools, money, or threatening acts may, within each group, be physically wholly different. We understand why different things mean the same to other people because, according to Professor Hayek, we too in our subjective experience classify them together. Even if a physiological explanation were found for the *general* principle on which the mind classifies different phenomena, such an explanation of the *particular* ways external stimuli are classified cannot be realized. To account for this impossibility, Professor Hayek invokes a paradox or contradiction that he discovers to be inherent in any attempt at explaining our own knowledge.

He maintains that “if we knew how our present knowledge is conditioned or determined, it would no longer be our present knowledge. To assert that we can explain our own knowledge is to assert that we know more than we do know, a statement which is nonsense in the strict meaning of that term.” The paradox prohibits us from knowledge of “why we hold the particular views we hold, of how our actual knowledge is determined by specific conditions.” From this alleged paradox, Professor Hayek infers the logical impossibility that all “the particular circumstances of time or place ... of the particular, of the fleeting circumstances of the moment and of local conditions”¹³ can ever be known except in fragmentary form, dispersed among many people. And no apparatus of classification could ever know how each particular act or thing would be classified. Therefore, Professor Hayek concludes, we would have to fall back upon our own subjective experience.

It is true that explanations of particular events are hard to come by, while we abound in generalizations, or explanations of the principle of a process. The situation in social science, however, is no different from that in physics. The physicist may know all the principles involved yet be quite

¹² *The Counter-Revolution of Science*, pp. 42, 49, 89.

¹³ *Op. cit.*, pp. 89, 98.

at a loss to predict, say, how many leaves will blow off a tree in the next storm. The poignant difference is, of course, that in social matters we desperately want explanation in detail, while in physical changes we are frequently indifferent. Laws in social science, if we had them, would contain many more variables than those of physics. Yet we berate the social scientists for not being able to do what even the model science cannot do. The multiplicity and complexity of factors in social phenomena impose limitations upon what we can reasonably expect to achieve. These limitations are only a practical, though perhaps practically insuperable, difficulty and we simply do the best we can.

Professor Hayek disposes in masterly fashion of the historicist's argument from uniqueness against the possibility of social science. Yet, there are overtones of the same appeal to uniqueness in his discussion of the complexity of classification by "meaning" in contrast with classification by physical properties. Since two tools, like a can-opener and a telephone, or two crimes, like murder and income-tax evasion, have in each case no physical properties in common, Professor Hayek appears to speak as if they had nothing at all in common by virtue of which we call them both tools or both crimes. He speaks as if the meaning in each case was not itself an instance of a type and therefore classifiable. Hence, the infinite complexity that makes it impossible for our brain ever to explain completely the way it classifies particulars. Hence, the indispensability of mental categories derived from introspection. But meanings are no more unique than colors; or, perhaps with better analogy, no more unique than complex events like those particular historical eras which Professor Hayek correctly calls unique patterns of many nonunique elements. If we can classify colors, wars, or economic structures, then we can classify meanings.

But what of Professor Hayek's "paradox"? Professor Hayek has many telling criticisms to make of the sociologists of knowledge. But this particular one will not do. Why can we not know the causes for our beliefs? It is, of course, tautological that we cannot explain our most general hypotheses or theories. They are most general because they explain everything else while not themselves being explained. But Professor Hayek does not dispute the possibility of high-order theories. The "paradox," as he sees it, lies not in the derivation of one theory from another, as Newton's from Einstein's, but in the possibility of such derivation for all the particulars of time and place. Actually to achieve this, we just saw, is only a practical problem. Where is the contradiction in knowing "why we hold the particular views we hold"? Professor Hayek seems to say that we cannot, without paradox, know the causes for our true beliefs. But such knowledge merely adds to the sum of present knowledge. It does not mean that I know more than I do know which, of course, is nonsense. As

Professor Hayek himself incisively insists, the reasons why my beliefs are true may be quite independent of their causes. There is therefore no reason why one cannot know both. Professor Hayek sees paradox where none exists for reasons quite extraneous to the methods of social science.

For consider, why is he so pleased, as he is, with this allegedly necessary limitation on the scope of knowledge? If true, would it not rather be a ground for regret? That, of course, depends. Men are no longer so sanguine as once they were that the truth will make them free. For, of course, in social science, manipulating the factors may mean controlling society, controlling the variables means manipulating men. If one man or group of men knew everything that is now known only in fragments in highly dispersed form, then a master plan for society could be drawn up and put into execution. If we knew the conditions that determine our knowledge and beliefs, if we had a complete sociology of knowledge, then the human mind could be consciously controlled. As Professor Hayek says, "Central economic planning is nothing but (the) application of engineering principles to the whole of society based on the assumption that such a complete concentration of all relevant knowledge is possible."¹⁴ Now, I share his horror at these future "engineers of the soul," as he tells us the Soviet Union honorifically calls its artists. What sane and humane person does not? I share too his apprehensions about the uses to which our growing behavioral science may be put. But will self-delusion help? Professor Hayek grasps at the impossibility of knowledge as the straw to preserve us from those who would misuse it if we had it. From "planning is dangerous" to "planning is impossible" is a broad but familiar leap. Unfortunately, Professor Hayek's ingenious 20th-century arguments narrow the gap no more than did Spencer's less sophisticated 19th-century ones.

(b) *The Implications of Reductionism.* Perhaps Professor Hayek is uneasy about his derivation of the indispensability of mental categories from the logical impossibility of complete knowledge. At any rate, he explores the implications of complete reductionism, a physical explanation for each of our mental states. He concludes that even if this were both possible and accomplished we would still have to use mental concepts. The logic of his case is particularly worth examining, since, for the sake of the argument, it assumes the frame of reference of many social scientists today, namely, physiological reductionism. And there is considerable general confusion about what reductionism would mean for social science if we had it.

To play the game, then, Professor Hayek postulates complete correlation between mental and physical states. In this case he asserts:

¹⁴ *Op. cit.*, p. 97.

“We should thus have ‘unified’ science, but we should be in no better position with respect to the specific task of the social sciences than we are now. We should still have to use the old categories, though we should be able to explain their formation and though we should know the physical facts ‘behind’ them.”¹⁵

Not only do I agree with Professor Hayek when he says that the problem of physiological psychology is not the problem of social science. In some sense I also agree that whether the former problem is “solved or not, for the social sciences the given mental entities must provide the starting point, whether their formation has been explained or not.” But what kind of a “must” is this? Under what conditions must we use mental entities? Under what conditions, if any, are they dispensable?

The point at issue is the same whether one speaks of reducing behavior to physiology and physics or of such reduction for the contents of consciousness. Behaviorism, after all, is merely a method for getting at the latter. The issue essentially is whether, given complete reduction, the science of psychology becomes unnecessary. The notion, referred to and repudiated by Professor Hayek,¹⁶ that we have to wait on developments in physical science before we can hope for progress in social science militates equally against the methods of behaviorism and the methods of introspection. In either case, though my reasons are not Professor Hayek’s, this notion seems to me clearly mistaken.

If we had reduction of the kind Professor Hayek postulates, then the concepts of psychology would be related to but *not identified* with the concepts of physics or neurology in some specific way, just as the properties of a gas are related to but not identified with the mechanical properties of particles. And the laws of psychology become derivable from neurology, just as the laws of thermo-dynamics are derived from mechanics. The assertion that the occurrence of a particular psychological state or disposition is always accompanied by a particular bodily state is an empirical law. This law is not derivable from laws mentioning either *only* psychological or *only* bodily states. If Φ_1 and Φ_2 are physiological correlates of behavioral (or mental) states Ψ_1 and Ψ_2 and we know the causal connection between Φ_1 and Φ_2 , then given Φ_1 and the empirical law asserting the correlation between Φ_2 and Ψ_2 , we may directly predict Ψ_2 without recourse to Ψ_1 . Thus, by means of the reduction each behavioral (or mental) concept at some time for some purposes could be dispensed with. But at no time, if we wish to know what people are doing or thinking, could we dispense with all such concepts together. If psychological concepts are

¹⁵ *Op. cit.*, p. 49.

¹⁶ *Op. cit.*, p. 50.

never used, we should have only the physics of human nature, not its psychology. Without psychological categories we could no more assert anything about what people are thinking or doing than we could assert anything about the temperature of a gas if we used only mechanical concepts, or about what people see under certain circumstances, if we used only electromagnetic concepts and never optical ones. However, just as in the physical cases, although the concepts are not dispensable, if we knew their relation to physical facts, then in all logic we could skip the stage of finding psychological laws, that is, relationships among the psychological concepts themselves. For, given the correlation and the initial physical state, the psychological laws follow deductively from the physical ones in the manner just indicated.

Thus I too insist that to explain the mental in terms of the physical is not to explain the former *away*. Even given complete reduction, there would still be both bodies and minds. We would simply know the causal connection between these two kinds of things, that is, between the neurological state and the mental state. Since there are two levels, the scientist may attend to only one of them as non-physiological psychologists do in fact. Reduction provides an explanation, not a substitute. If someone classifies a set of physical properties as “money,” then we would know the corresponding neurological state. But the individual still has an attitude toward the green paper stuff. Even with reduction, the science of psychology remains what it is. Greed is still greed, no a congeries of atoms and electrons. Psychological concepts are psychological concepts. And psychological laws are psychological laws, since they relate psychological, not physical, concepts.

(c) *Incomplete Definition and “Subjectivity.”* But a relation implies at least two terms. In order to know the relation between the physical and the psychological concepts we must therefore recognize each of these two classes. This I believe is why Professor Hayek asserts that we would still have to use the old concepts even if we had complete reduction. And, in the sense I have just discussed, he is correct. But why does this imply subjectivity? For Professor Hayek, the subjectivity inheres in the way we recognize instances of mental concepts, like a friendly face or a threatening gesture. In kinetic theory, while temperature and its correlate, the kinetic energy of particles, are both objectively given, the mental concepts corresponding in our analogy to temperature are not objectively given. Professor Hayek’s ground for this belief is, of course, also the ground for his rejection of behaviorism. The reason he adduces, however, while it does indeed point to a methodological problem is something less than compelling.

Most psychological concepts are complex, referring to a cluster of

traits, dispositions, or acts. Psychological laws either correlate one such cluster with another, as in personality tests, or with environmental conditions, as in any stimulus-response situation. Hunger, fear, anger, aggression, friendliness, are typical psychological terms that may be related either to one another or to external conditions evoking them. Let me quote Professor Hayek's statement of the difficulty with these concepts.

"Probably in no single instance has experimental research yet succeeded in precisely determining the range of different phenomena which we unhesitatingly treat as meaning the same thing to us as well as to other people.... To say that when we speak about a man being angry we mean that he shows certain physical symptoms helps us very little unless we exhaustively enumerate all the symptoms by which we ever recognize, and which always when they are present mean, that the man who shows them is angry. Only if we could do this would it be legitimate to say that in using this term we mean no more than certain physical phenomena."¹⁷

There are two remarks to make about this matter. *First*, every science, not psychology alone, must distinguish between (a) the empirical or operational definition of a term by which, independently of any further knowledge, we identify its referent and (b) those traits or dispositions that observation shows to be correlated with the defining characteristics. Thus, if "Angry men grow red in the face" is not tautological, anger is defined independently of the disposition to alter complexion. And surely Professor Hayek would not wish to assert, after the manner of Hegelian holists, that we must know everything about an entity, like anger, before we can meaningfully use the term. Yet, to demand exhaustive enumeration of all the symptoms is to be bewitched by the holistic magic. Rather than indicating an inherent subjectivity, Professor Hayek's criticism merely points up a characteristic feature of concept formation.

Secondly, in science as an on-going process, definitions of empirical concepts have what might be called "open-ends." If we take a still picture of a cross-section of a science at any given moment, then we may logically distinguish between definitions and laws without ambiguity. This "horizontal" view, or logical reconstruction of science at any given stage, reveals the structure of laws and theories. It is, therefore, indispensable for understanding how and why the structure changes from one horizontal point to another. But the dialectic of concept-formation in the "vertical" dimension, in science as it develops in time, is highly fluid. A trait or disposition found to be invariably correlated with the cluster named by the definition may well be added to the latter. Or there may even be a complete interchange between the empirical and definitional content of

¹⁷ *Op. cit.*, pp. 47, 213, fn. 39.

the concept, if this produces greater precision or ease of identification, as for example, when a disease first defined in terms of its symptoms is later redefined as the presence of specified bacteria, while the symptoms become part of the empirical content. The less systematic the science, the more likely its definitions will be open-end. The more highly organized the science, the more prone its definitions to closure. The presence of open-end definitions, or a resistance by the observer to terminological closure, is merely a measure of the fluidity or instability of the theoretical structure at a given time.

The application of the term “symptom” to a *defining* characteristic or cluster of traits is itself a signal that the definition is tentative, held only until that which its referent symptomizes is uncovered. The presence of alternative definitions of the “same” concept, as, say, of hunger in a laboratory animal or, for that matter, of electric current signalizes the scientists’ belief that something else is present of which the various definitional properties are symptoms. If and when the characteristics of this something else are uncovered, the hitherto defining traits become empirical correlates within the system. Both psychological concepts and, given reduction, the correlated physiological ones will be, from the point of view of the scientist, relatively open or closed as the case may be. This is a practical matter, signifying no inherent difference between the two levels. Professor Hayek’s contention that it signifies an ineradicable subjectivity, for we must “just know” what we mean without being able to *say* what we mean, is therefore without foundation.

Professor Hayek’s arguments for the subjectivism of social science has carried us over a fairly wide range of topics. Fortunately, after combing this tangled terrain, the other two main aspects of his position may be more briefly considered.

Concept Formation and “Methodological Individualism.” Professor Hayek’s doctrine, it will be recalled, represents the fourth pattern in contemporary theory of the social sciences: although the psychology of the individual cannot be reduced to anything else, the behavior of groups must be explained in terms of the behavior of individuals. This is the “methodological individualism” that, together with “systematic subjectivism,” Professor Hayek believes defines the unique character of social science. Objectivism, the denial of systematic subjectivism, is the first abuse of reason in social science; collectivism, the denial of methodological individualism, is the second.

I shall not linger over Professor Hayek’s contention that while the method of natural science is analytic that of social science is synthetic or “compositive.” He himself mentions the exception of astronomy, whose

laws are composed from the elements, rather than by working backwards from the whole to these constituents. And elsewhere he hints that he is perhaps not wedded to the distinction. Physical science of course uses both methods, resolution or analysis, and recomposition or synthesis. Therefore, I shall just consider the claim that for social science only the synthetic method is possible, that the wholes must be constituted from their familiar component parts.

Professor Hayek carefully distinguishes between ideas which are *constitutive* of social phenomena and ideas which are *about* these phenomena, between ideas that cause and ideas that explain the social structure.¹⁸ (The latter may, of course, also play a causal role, but the distinction still remains.) Only the motivating or constitutive opinions are data for social science. The speculative theories which people form about the whole must not be taken as data, but must be revised and replaced by more appropriate ones. According to Professor Hayek, vague popular theories are reflected in our language by macroscopic or collective terms like “capitalism,” “society,” “imperialism” and the like. Whoever adopts such concepts treats as facts what are merely inchoate theories. For he believes that no collective term ever designates “definite things in the sense of stable collections of sense attributes,” but is always a name for a theory about the connection of the components. Apart from a true theory there is no whole, for a whole is always a nonperceptible pattern of equally nonperceptible relations. Hence, collective terms cannot themselves enter into theories as components of laws, since they are themselves theories. Who thinks otherwise, says Professor Hayek, commits the fallacy of conceptual realism or “essentialism.”¹⁹

It is in fact frequently true that the more macroscopic concepts do name a poorly articulated structure of laws or a theory. But this is neither true of all collective concepts nor does it, when true, distinguish such concepts in social science from those of the physical sciences. Our conception of a physical object includes dispositional properties, like solubility, which are interesting because of low-order laws into which they enter; for example, salt is soluble. This does not preclude these concepts from entering into further laws. Nor are all the relations forming a complex whole causal, nonperceptible relations. Some patterns or relations among things are as directly given as the things themselves. Professor Hayek confuses observable relations of fact, like gives or receives, on the one hand, with what Hume called philosophical relations, like cause or effect, on the other.

This confusion reveals a curiously materialistic, almost kinesthetic,

¹⁸ *Op. cit.*, p. 36f.

¹⁹ *Op. cit.*, pp. 54, 55.

notion of fact no more appropriate to physical science than it is to social science. Group concepts like family, firm, or opera can be defined in terms of empirical, noncausal relations among individuals. Professor Hayek's illustration of the "market" as an arrangement for regular contacts between potential buyers and sellers is not itself a theory, but a name for a cluster of properties and relations which may enter into a theory. Since it is such a cluster, the group concept may of course be decomposed into its elements and the relations among them. It is one thing to hold that for every name there must be a corresponding individual thing, for example, that the state is literally a superperson, or, perhaps, a person among persons. It is quite another thing to believe that such terms refer to nothing at all. Planets may be conglomerations of atoms but no one would deny reality to planets. Likewise, crowds may be groups of individuals, but there are also crowds. Universities exist just as much as professors, students, and administrators, although the university is not a separable fourth thing but a name for relations among the other three. Only one who makes such a separation hypostatizes the concept.

In sum, what is true for psychological concepts is, to a certain extent, true also for sociological or otherwise collective rather than individual concepts. Reduction of one to the other may well be possible. But if we wish to speak of group phenomena, then we may use macroscopic concepts to do so. The permissive "may" in this context contrasts sharply with the obligatory "must" for psychological concepts. The difference touches the heart of the matter. Reduction of the psychological to the physiological level means something different from reduction of collective to individual concepts. In the former case, "reduction" means finding an empirical correlation between two sorts of things, mental and physical. Reduction of group concepts, on the other hand, means defining them in terms of statements about individuals and relations among them. This complex, "microscopic," definition could be substituted for the macroscopic concept, while such substitution is not possible for psychological concepts. The difference is, I believe, the incontrovertible core of Professor Hayek's argument against "collectivism." Nevertheless — and this is the meaningful core of the holistic insight — since a group concept refers to a complex pattern of descriptive, empirical relations among individuals, there is no reason why the behavior of this complex should not itself be studied. Psychological laws may be ubiquitous, but social laws may be formulated without taking them into account. Psychological laws may have mass effects, making the behavior of institutions or crowds different from that of individuals. The behavior of a substance subject to the laws of chemistry differs from the behavior of its particles subject to the laws of mechanics. A crowd, a club, or a family may just as well be a unit of observation as a planet. There need be no similarity between the behavior of a com-

plex and the behavior of elements of the complex. Professor Hayek points out that the unintended consequences of the actions of many men constitute the subject matter of social science. But these undesigned resultants are precisely those structures of relationships whose behavior differs from that of individuals. All that is required for reductionism is that the complex phenomena be composed of individuals and the relations among them. The use of collective concepts does not in itself commit one to conceptual realism nor to mistaking theories about facts for facts.

Professor Hayek's analysis of the motives inspiring holistic philosophers unfortunately does not frustrate his own equally illicit transitions from the logical to the moral. Arguments for or against methodological individualism draw their strength from our criteria of the meaningful. Arguments for or against political individualism draw their strength from our moral convictions. The bridges linking these disparate issues are purely verbal, their sustaining structure wholly ideological.

Explanation and "Methodological Individualism." In a recent interesting and clarifying paper, influenced by Professor Hayek but less extreme and wholly free of ideological special pleading, J. W. N. Watkins lucidly elaborates a "Principle of Methodological Individualism." Although I basically agree with most of what Mr. Watkins says, in some respects he too rather overstates the case for methodological individualism. Although Mr. Watkins approvingly quotes Professor Hayek on the necessity for psychological reduction of collective concepts, he concedes that there are overt characteristics of a complex social situation that may be empirically ascertained without referring to people's dispositions. He asserts that "overt characteristics may be *established* empirically, but they are only *explained* by being shown to be the resultants of individual activities."²⁰ Thus, while permitting the use of macroscopic concepts in laws, he prohibits their use in explanation.

Mr. Watkins presents the alternatives between methodological individualism and methodological holism as those between deducing social processes either from "principles governing the behavior of participating individuals" or from "macroscopic laws which are *sui generis* and which apply to the social system as a whole."²¹ His principle of methodological individualism asserts, with Professor Hayek, that only the former is permissible. I suggest, however, that this is a false disjunction. The belief that, since human beings are concerned, social processes are mediated through psychology is indeed the frame of reference of modern science. Mr. Watkins cites a nonhuman social system, a beehive, whose activities, on

²⁰ Watkins, *op. cit.*, p. 732.

²¹ *Ibid.*, p. 729.

present knowledge, cannot be explained in accordance with the Principle of Methodological Individualism.²² A human situation for which the principle fails is also conceivable. Suppose that Jones behaves in a certain way only if he is a member of a group — in the same hall or under the same jurisdiction — of N, say 1,000, people. Suppose further that the law expressing Jones's behavior in this situation is not derivable from laws concerning his behavior in groups of less than N together with the composition rules based on these laws. This circumstance then exhibits empirical emergence at a certain level of complexity, namely groups of size N, and methodological individualism breaks down as an explanatory principle. Though such an eventuality is rather implausible, Mr. Watkins agrees that there is nothing *a priori* about the principle; the fact that prices and tools are human creations does not *entail* methodological individualism. But he implies that unless one holds that social groups are organisms, that is, groups whose properties cannot be derived from those of individuals, one must repudiate macroscopic laws or theories in favor of individualistic ones.

Mr. Watkins's reference to sociological laws as "holistic" suggest that, despite his concession that some overt characteristics may be established empirically, he views all macroscopic concepts as suspect. However, my previous discussion of collective concepts implies a distinction between macroscopic and holistic concepts. "Macroscopic," or collective, and "microscopic," or individual, are relative terms. Social concepts are macroscopic relative to psychological ones; these in turn are macroscopic relative to physiological terms, and so on down to the level of quanta of energy. Identification of "macroscopic" with "holistic" obscures the fact that one can be holistic on any level. Professor Hayek, we have seen, is holistic with respect to psychological categories, since they may not be reduced to anything else. Some biologists are so with respect to physiological categories. Once chemists were holistic about their laws, because they believed them underivable from physics.

I agree with Mr. Watkins that neither society nor any of its sub-groups are organisms in the sense he explains. The denial that there are such organisms amounts to denying that there are *sui generis* sociological laws, that is, laws irreducibly macroscopic or sociological. But macroscopic laws may be, as I pointed out before, different from individualistic laws, yet not *sui generis* in the sense of underivable. We frequently have information, statistical or otherwise, concerning groups rather than individuals. The Keynesian hypothesis that the consumption of a community is an increasing function of its total income is, of course, about a group and not about individuals. The same is true of a great many other hypotheses in

²² *Ibid.*, pp. 730-31.

economics and other social sciences. Contemporary experimental social psychologists find that empirically defined, measurable, macroscopic concepts, making no direct reference to individuals — *e.g.*, “group climate” or “cohesiveness” — permit prediction (and explanation) of group behavior unattainable in practice, if they restrict themselves to seeking the composition rules based on the myriad complex of individual dispositions involved. Insistence upon the indispensability of reduction to the microscopic level in the search for laws and explanatory theories in social science is analogous to the insistence by some that psychologists of the abnormal refrain from using clinical concepts until their physiological correlates have been established! It is true that Keynes, for example, states certain psychological factors or propensities possibly permitting deduction of the aggregate phenomena. Yet in all the mathematical formulations of the theory by Keynesians, the functional relations are among magnitudes not identifiable with the behavior of any individual. And at one point Watkins in effect admits that for any actual economic system the number of factors at work renders methodological individualism an impossible task.²³

The growing interest by economists in collective rather than individualistic models is an index of their sensitivity to the tremendous difficulties in deducing group behavior from guesses about individuals. Watkins’s assertion that not only should social processes be explained by reference to individuals but that, indeed, they can *only* be so explained sounds very much like a persuasive definition of “explanation.” Explanation in any field is not absolute, but relative to a set of premises. Drawing on one of his illustrations, an election result may be explained *either* in terms of individual dispositions *or* in terms of macroscopic laws about the history and fate of political parties in power for a given time, the relation to world crises, prosperity, and so forth. The latter alternative would be a perfectly good and even enlightening explanation, but it would be macroscopic. I suggest that elusive as even such macroscopic explanations may be, they are infinitely simpler to formulate and test than explanations in terms of the multitude of individuals involved. Of course this is merely a guess or hunch, but it is not altogether impertinent, in either sense of that word.

What, then, of the principle of methodological individualism as a heuristic device? As a caveat against hypostatization, it is not only wholly unobjectionable but, in the light of the scientifically disreputable past of social science — its closet cluttered with “group minds” and other suspect entities — the principle probably should be conditioned into every social scientist’s consciousness or better, his unconscious. But what of the principle as a general, everyday working rule in research and theory construc-

²³ Note, on p. 724, his quotation to this effect from Walter Eucken.

tion? It is doubtless true, in science as elsewhere, that a man's reach should exceed his grasp, yet it seems to me that any rule merits a jaundiced eye if it reduces a science either to impotence or to triviality. And given the admitted tremendous complexity of the social factors. I suspect that undeviating adherence to this rule would do just that.

In some instances, laws are closer at hand if the scientist sticks to the macroscopic level of the complex concept. From these laws he may construct theories. In other instances, the microscopic or psychological approach might be more fruitful; in still others, a combination of these approaches. Certainly, given the still rudimentary condition of social theory, a crusade for the variable which insists that the scientist wait upon developments in psychology, or, what is worse, act on his own psychological hunches, can only cripple research by restricting it to the trivial. This is not to recommend methodological license, only methodological liberty for the social scientist to find his laws and explanations where he may.

VII.

PHILOSOPHY AND SOCIAL SCIENCE*

By Richard S. Rudner

I wish, for sake of the vivacity of any discussion which might ensue, that I could find myself more in disagreement with Dr. Brodbeck than I do. As a matter of fact, however, I find myself in substantial agreement with her on practically all of the points upon which she takes issue with Hayek. There are, to be sure, a few questions of relatively minor import that I should like to ask Dr. Brodbeck, but in the main I have confined my own discussion to an elaboration and supplementation of some of the points considered in her paper, and some related considerations raised in Hayek's book but which Dr. Brodbeck did not have the time, perhaps, to treat.

Hayek argues¹ that the social and non-social sciences are essentially different. He attacks a view which he labels "scientism." Sometimes he seems to construe his opponents as being correct in their notions concerning the methodology of the *non-social* sciences, sometimes as being incorrect in this area too. But there is no doubt about his feeling that those who take the scientistic view with regard to *social* science are perniciously mistaken. It is certainly safe to say that Hayek is claiming a radical distinction to obtain between the social and the non-social sciences but it is not always so clear that the distinction to which Hayek's important arguments apply is a distinction in *methodology*.

It is, of course, possible to distinguish the social from the non-social sciences on, *e.g.*, the basis of their respective results or on the basis of efficacious attitudes which the respective groups of scientists may have, as well as on the basis of differences in methodology; and I think it will help illuminate the actual import of some of Hayek's arguments if we keep these possibilities in mind.

I think that Dr. Brodbeck's division of Hayek's thesis, concerning the radical distinction between social and non-social sciences, into arguments that "systematic subjectivism" and arguments that "methodological indi-

* This essay by Dr. Rudner was read as part of a symposium on Philosophy and Social Science that was conducted during meetings of the American Philosophical Association, Western Division, held in St. Louis in May 1953. The opening paper of the symposium was by Dr. May Brodbeck. It is reprinted in this bulletin as Chapter VI. Dr. Rudner's essay was written during the time when the author was employed in a research project that was supported by a contract between the Office of Naval Research and Tufts College. It has been reprinted with permission from *Philosophy of Science*, Vol. 21, No. 2, April 1954.

¹ F. A. Hayek, *The Counter-Revolution of Science: Studies on the Abuse of Reason*, Glencoe, Illinois: The Free Press, 1952.

vidualism” uniquely characterize social science is a helpful one; and my remarks will be directed primarily to the first of these arguments.

Dr. Brodbeck points out that Hayek has three reasons for believing that social science is irremediably subjective; first, “its subject matter is human opinions and attitudes”; second, “this subject matter can be adequately known only by ... introspection”; third, “mental categories are indispensable in explanation of social phenomena.”

Suppose we begin by considering the first of these. Hayek argues that the sole concern of social science is people’s opinions, beliefs and attitudes. Dr. Brodbeck replies that the social scientist is not only interested in these but in certain relationships which obtain between what, *e.g.*, an individual may believe and what is actually the case.

This does not appear to be a wholly satisfactory reply because Hayek may be construing “Social Science” so narrowly as to have the term apply only to those areas or parts of areas in which (as in certain aspects of public opinion and attitude research) our interest is just what are the opinions and attitudes of a set of individuals.

But even if we agree with Hayek (as indeed we must) that “Social Science” *thus* construed is solely concerned with subjective phenomena, this does *not* demonstrate the subjectivity of Social Science. If Social Science is subjective because its subject matter is, then by the same argument Ornithology would have to be considered a bird-like science and Archaeology an archaic one. At most, what such considerations seem to show is that the *results* of Social Science thus construed are distinguishable from the results of any other science. This is hardly a unique property of Social Science.

The second of Hayek’s reasons for holding Social Science to be subjective appears to have more *prima facie* relevance to the issue of the cogency of a methodological distinction.

Hayek maintains that introspection is an indispensable resource of the social scientist because *the only way in which we can come to understand* other minds is by assuming that they work like ours and examining our own — since these are the only accessible ones. Now an immense amount of refutational literature exists relevant to this thesis; but I suppose it is a perennial duty of philosophers to consider the thesis anew whenever anyone reputable or interesting brings it up. Dr. Brodbeck points out that one absurdity of this position is that Genghis Khan or Hitler could be “tackled” only by a war-like historian. But the crucial point of her reply seems to be that Hayek in some sense proves too much; for (she maintains) if his arguments were correct we would already know everything about human psychology and hence *that* science would be unnecessary.

Now, I think Dr. Brodbeck's reply is an impressive one, nevertheless I do not find it decisive against the challenge Hayek raises. First, with respect to the counter-example of the historian, it seems to me Hayek could accept the consequences of the alleged absurdity and still unruffledly maintain that it was quite sensible to hold that *ceteris paribus* war-like historians *are* best equipped to "tackle" war-like historical figures; that to assume this as absurd is to beg the question. Moreover, with respect to Dr. Brodbeck's main point here, I do not think it follows from the indispensability of introspection for psychology as a science, that all of that introspection has already taken place. The claim that a certain condition is a necessary one does not imply that that condition has been instituted, and it seems to me that Dr. Brodbeck is making just such an inference when she claims that a consequence of Hayek's stand is that human psychology is already known to us. Hayek I think would insist that much pertinent introspection (hence much of psychology) still remains to be done.

Dr. Brodbeck in one place also seems to argue that if Social Science were based on introspection its results wouldn't be surprising — and since they obviously sometimes are, it is unlikely that they are so based. But there is no reason why the results of introspection should not be as surprising as the results of what we ordinarily take to be the method of scientific inquiry. Some of my own introspection leaves me not only surprised — but appalled.

Despite these remarks, however, I would contend with Dr. Brodbeck that Hayek is wrong in asserting that introspection is necessary for Social Science, even narrowly construed. For Hayek to establish his point concerning the *methodological* subjectivity of the social sciences he would have to prove more than the indispensability of introspection as an associated activity; for introspection might well be a necessary activity for social scientists *vis-à-vis* what Reichenbach calls "the context of discovery" without being methodologically necessary. I am assuming, of course, that considerations of method apply to the context of justification. Hence, even if we were to grant the indispensability of introspection, and, in fact, even if we granted (the more dubious assumption) that this indispensability uniquely characterized the activities of social scientists, we might be granting merely the heuristic necessity of introspection in Social Science and Hayek would still not have established his point that Social Science is methodologically distinguished from non-social science. For Hayek to establish his point he would (at least) have to present an example in which introspection serves more than a heuristic function. I do not find that any of the examples he gives does this.

Indeed, if Hayek is making a serious proposal, then are we not confronted by our old friend, the method of intuition. Hayek does not even

come to grips with Peirce's invincible arguments (for so they seem to me) against the adequacy of intuition, as the method of science, let alone conquer those arguments.

Perhaps, however, Hayek may be construed as accepting Peirce's arguments and hence holding the thesis that the admittedly unsatisfactory method of intuition is the only one open to the social scientist. If this is the case then he must prove that what we ordinarily call the method of science is not a viable method for social research. To me he makes his strongest bid to accomplish this in connection with the third of his reasons for contending that social science is irremediably subjective. This reason, it will be remembered, was constituted by the alleged fact that "mental" concepts were indispensable for explanation in social science.

There is much that is obscure in Hayek's discussion of this contention. We are given no definitive statement of what constitutes a "mental" concept, but Hayek does give a number of interesting examples that lead me to believe that his intentions might be illuminated if we examine them in the light of a convenient framework which in its present form is due to C. W. Churchman and R. L. Ackoff.² Churchman and Ackoff distinguish among a number of ways of classifying activities. I oversimplify, but substantially the following are three of the ways they distinguish: first, a set of objects is said to belong to the same *physical* class if they share a specified physical property (*e.g.*, the set of things which has a temperature of 72° F.); second, a set of things would be said to belong to the same *morphological* class if they share a set of properties within a specified range (*e.g.*, everything which has a temperature of between 70° and 80° F.); third, a set of things is said to belong to the same *functional* class if they share a common teleological property (*e.g.*, a watch, a sundial, a piece of radium, and a water clock all belong to the same functional class for they all serve a common end).

Now, what Hayek seems to be saying is that social science explanation must include mental concepts in the sense of the functional concepts outlined above. Social science studies purposeful behavior, purposeful behavior entails functional classification and hence (he appears to maintain) explanations of it must include functional concepts. We can, Hayek asserts, look at a set of disparate objects from now to doomsday but unless we take into account that they are related to a set of individuals by virtue of the fact that the individuals are classifying them functionally, and that they are hence in interesting social science relationships to those individuals, we are missing something essential.

² Churchman, C. W., and Ackoff, R. L., *Psychologistics* (mimeographed), Philadelphia: University of Pennsylvania, 1946.

Now, even if we grant the truth of this assertion it is difficult to see how Hayek can thereby establish the point of methodological uniqueness for social science. One can see how, on the basis of the assertion, Hayek might argue that we would *probably* not discover men classified functionally unless we had introspected. This, however, is a phenomenon of the context of discovery. Moreover, if the statement that men characteristically classify functionally were true, explanations and predictions in Social Science might well *mention* functional (*i.e.*, mental) concepts, but they would not therefore necessarily be *using* mental concepts any more than explanations of radioactive objects would necessarily be using radioactive concepts.

Finally, even if explanations in social science *used* (rather than mentioned) these “subjective” concepts, social science would be no more subjective than, say, physics; and I am thinking in particular of relativity theory. The fact is that any science might make a functional classification of its concepts or of its phenomena whenever this proves fruitful; such a procedure, however, is dictated by *pragmatic considerations* and not by the peculiar necessity to functionally classify functional concepts as Hayek appears to argue.

Dr. Brodbeck construes Hayek’s argument as being to the effect that we cannot classify “by [such mental entities as] meanings.” And her reply is that meanings or functional criteria are as much instances of types and hence as much classifiable as physical entities. It seems to me, however, that Hayek’s position is not so much that meanings and the like are unclassifiable — or even that we can’t classify “by meanings” (whatever that might mean), as it is that we always need meanings or something equally “mental” when we explain or predict in social science. Moreover, his position is that this assertion follows from the assumption (which we’ve granted for the sake of argument) that Social Science studies subjective phenomena. Such a position could only be assailed by showing, as has been attempted above, that the assertion does not follow from the assumption.

Hayek’s first major thesis is that the social sciences are characterized by systematic subjectivism. I have endeavored to supplement and elaborate Dr. Brodbeck’s arguments against that thesis by pointing out that Hayek doesn’t prove his case. His second contention is that the social sciences are also uniquely characterized by “methodological individualism.” The behavior of groups is held to be explainable only in terms of individuals. Here, I think Dr. Brodbeck’s reply is decisive and requires no supplementation. Her argument in brief, is that the possibility of *reduction* when this process is clarified does not entail the *eliminability* of macro-concepts.

I am however somewhat puzzled about one aspect of Dr. Brodbeck's discussion. Some of her remarks suggest that though she disagrees with Hayek concerning what follows from the statement, she nevertheless does not accept the dictum that group phenomena *are* always reducible to individual phenomena — at least in the refined sense *she* has explicated.

If indeed this is the import of some of her remarks, then I think she makes an erroneous concession to what (considering the methodological portentousness of quantum physics) must in these days be called a foolish conceit on the part of Hayek. Surely one should no longer insist that designata for ensembles *must* be reducible in the theoretical structure of any science in the light of the fruitfulness of quantum theory. We must hold open the *possibility* of a similar revolution in social science even while we guard against evils of hypostatization as Dr. Brodbeck warns. To close the door, however, once and for all by adopting individualism as a sacred methodological precept is to do precisely what Dr. Brodbeck inveighed against earlier: settle *a priori* a matter which must await a great deal of further scientific research.

VIII.
THOUGHTS ON SCIENTIFIC METHOD
AS APPLIED IN ECONOMICS*

THE various methods of inquiry that men have used include magic, revelation, common-sense observation and accumulation of the result as folklore or proverbs, Platonic idealism or the dialectical quest for certainty (which is in some respects a sophisticated form of word magic), classification and the syllogistic quest for certainty in the manner of Aristotle, and finally study of the relations among changes as initiated by the Galilean revolution in method. Of course, this does not imply that each of these methods has always been clearly separated from the others, nor does it imply that at any time and place any one of these methods has been applied in a pure form without vestigial carryovers from one or more of the other methods. Moreover, one cannot place these methods in a neat chronological series; one can only assert that at various times one or another of these methods, judging by the historical record, seems to have been more widely accepted than others and thus to have dominated the prevailing intellectual climate.

For example, a few thousand years ago various magical procedures were widely used as means of facilitating prediction and control. But at the same time research was in progress that involved study of the relations among changes. How else can one explain some of the artifacts of the ancient world such, for example, as the copper plumbing in Roman baths? Someone must have experimented with copper ore, heat, etc. However crudely, he or they must have measured changes and studied the relations among those changes.

But today one can find, even in scientific journals, avowals of faith in one revelation or another. One has only to look around to see that the Galilean revolution in method has been accepted as the best method of inquiry by only a small fraction of the world's population and that many, perhaps most, of even that small fraction lean heavily on other methods of inquiry whenever they seek to know about matters other than their own fields of specialization.

Thus we are faced today with the fact that no clearly defined method of inquiry is universally accepted as the best in all fields. Even the striking success of the Galilean revolution in greatly augmenting the ability of men to predict and control events in the field of the natural sciences has not greatly altered most men's views on method in spite of the great

* This article was published substantially as it is here in the *American Journal of Economics and Sociology*, January 1955.

alteration in the environment that has occurred in modern industrial societies. Men are prone to accept their cultural heritage uncritically, and this is as true of the automobile as it was of the Roman chariot. In short, to many, the scientific advance does not reflect the triumph of a new method of inquiry but the happy coincidence of a series of fortunate inventions; and the latter, where they are not regarded as evidence of a devil's favor, frequently are regarded as evidence of luck plus perhaps pertinacity.

Now it happens that I have accepted the working hypothesis that the Galilean revolution and subsequent developments in methods of inquiry provide the most useful procedures from the viewpoint of facilitating prediction and control. Prediction of future developments and control over them, *at least to the extent of behavioral adjustment to them*, are accepted for the purposes of this discussion as the immediate ends in view of inquiry. Let us put aside for the moment (but of course not deny the existence of) other ends of inquiry such, for example, as esthetic satisfaction for the inquirer.

The questions that I now propose to discuss may be stated as follows:

1. How is the method initiated by the Galilean revolution different in principle from the methods of inquiry used earlier?
2. How has the new method developed in various fields?
3. What aspects of the new method are applicable in economics?

The Galilean Revolution

The revolution in methods of inquiry that was exemplified, if not actually initiated in all detail, by Galileo's work involved one great and obvious departure from the methods previously popular and a second less obvious but equally important departure from earlier methods. These new procedures were, first, the selection of changes as the subject matter of inquiry; and, second, the abandonment of what John Dewey has called the "quest for certainty" and substitution of the attainable goal, a high order of probability, or warranted assertability. Brief comment of these new methods and their relation to each other may be helpful.

The Greek philosophers sought to know unchanging and everlasting reality. More precisely, they imagined that there must be such things as eternal and immutable reality of some kinds, types, or forms; and their methods of inquiry were adapted to the pursuit of their imaginary objectives. In labeling those objectives imaginary, I do not here imply that there necessarily is no ultimate, unchanging, and everlasting reality but only that to date those who have sought such certainties have not been able to convince other intelligent men that they have found them, nor have they

been able to demonstrate that the methods of inquiry developed in the course of that quest have been as useful¹ as more modern methods of inquiry.

That men seeking for the eternal and immutable have considered change unworthy of serious study hardly is surprising. How could any reasonable man expect to find unchanging and everlasting reality undergoing change? That this seemingly sound viewpoint was reinforced by the contemporary culture is well brought out by Dewey and Ratner in particular.² For our purposes we need only note the fact and pass on.

When Galileo chose to ignore notions widely accepted in his day and insisted on turning his attention to the subject matter found undergoing change in actual experience, he spurred on the revolution in method. To turn aside from the traditional and accepted mode of procedure in inquiry, to drop dialectical debate as though it were a useless appendage would have seemed hopelessly unsound to the scholastics and their Greek progenitors; and to have chosen change as the subject matter of inquiry, even to have initiated more changes for the purpose of studying the relations among them, would have seemed incredible folly. Of course the Greeks were not stupid; they were keen observers of qualitative differences, and they were enthusiastic about mathematics. Its rigorous "laws" seemed to them an ideal tool for use in their quest for certainty. They simply mistook a shorthand method of transforming propositions for a means of probing reality.

Again I quote Dr. Ratner: "Modern scientists, however, began by taking precisely the world of change as their subject for scientific study, and to help them on their way, they introduced the method of experimentation which is no less and no other than a method whereby the natural changes going on can be further increased and complicated in manifold ways by changes deliberately made. From the Greek point of view (and in this case, *not* excepting any Greek), this is confounding confusion, science gone insane. But as events have fully demonstrated, it is science really come to its senses, and intelligence come into its own."³

However, the quest for certainty was not so readily put aside. Although the methods used by those who had sought certainty were abandoned as the new method of studying change was found to be effective, the objec-

¹ We should judge the usefulness of a method of inquiry by the extent to which it makes possible control over events including either behavioral adaptation as part of the events, or external changes outside of any human organisms, or both.

² John Dewey, *The Quest for Certainty*, New York: Minton, Balch & Company, 1929; and Joseph Ratner, *Intelligence in the Modern World, John Dewey's Philosophy*, New York: The Modern Library, 1939.

³ Ratner, *op. cit.*, p. 52.

tive, ultimate, and unchanging reality lingered on in the minds of men. Newton, the intellectually gifted philosopher-scientist, was convinced that he had at last found certainty behind the facade of change in the form of his atom, his "... absolute and separate Space and Time, and immutable (invariant) mathematical laws of Nature...."⁴ Not until Einstein's work and Michelson's finding of the black bands in his interferometer was the quest for certainty finally discredited in the realm of science. And all too many scientists are still unaware that the Galilean revolution in method has thus been brought to maturity.

Development of the New Method

As has been indicated, the new method differed from the old in that the modern scientists of the past 3 centuries have turned to study of changes and the relations among changes. Now changes may be of two general types or a combination of both. These have been designated qualitative and quantitative; and examples of each probably will be more illuminating than definitions.

During his early years, a male human organism may acquire the habit of wearing a hat as protection against the sun and discarding his shoes during the summer months, the typical barefoot boy. In later years, the matured organism may substitute for the earlier habit the habit of wearing shoes and discarding a hat except in inclement weather. This is one illustration of qualitative change.

An example of quantitative change is provided by the fluctuations in the price of wheat. If wheat that once had sold for \$2 per bushel subsequently sold for \$1.90 per bushel, a quantitative change would have occurred, and the difference in price is the measure of this change.

As it happens, however, qualitative and quantitative change are not always separable. Thus the traffic light that changes from green to yellow to red may seem to provide a clear example of qualitative change until the colors are expressed in terms of the rate of vibration of light rays per second. Then it becomes obvious that the changing traffic lights may be viewed as an example of quantitative change or even of the two combined.

Similarly, to the layman the music of a bass viol may seem qualitatively different from that of a violin; but to the physicist the difference may reflect quantitative change measured by vibrations of sound waves per second. Thus the modern scientist, although he does not ignore qualitative change that cannot be transposed to measurable quantitative change, often may emphasize other aspects than the qualitative changes that seem important to laymen.

⁴ *Ibid.*, p. 107.

If one follows through developments in the various sciences, from their earlier beginnings as such to the present day, quantitative change is seen to receive increasing emphasis as the subject matter of inquiry. From one point of view, it might be said that the scientists apparently never will be satisfied until they have succeeded in developing the quantitative aspects of all changes. However, this perhaps is too broad a generalization. For our purposes here we may simply note that increasing preoccupation with quantitative change including the conversion of many qualitative changes to quantitative changes is an outstanding characteristic of modern scientific method.

Now quantitative change is measured change; that is, we recognize that change has occurred by measurement, however crude that measurement may be. In some instances the measurement may be so crude that the changes are stated in such inexact terms as greater or less, higher or lower, faster or slower, longer or shorter, etc. What was larger has become smaller, high speed has been retarded, rising prices have changed to falling prices, what used to require a week can now be done in less time; these are examples of rough measurements of quantitative change.

As everyone who is familiar with any of the sciences knows, measurements of change may be much more precise than those mentioned above. For example, changes in the currency in circulation daily in the United States can be measured with more precision than you probably would use in measuring the changes in the coins in your pockets from one day to the next. Changes in the relative positions of a distant star can be measured with a degree of precision that few motorists would have occasion to apply in measuring their daily peregrinations by automobile.

Scientists are concerned with measurement of change as a means or as one step in their work. The immediate end in view or purpose of such measurements is to facilitate the study of relations among or between the measured changes. Based on their familiarity with such changes, scientists develop hypotheses or statements of the relations believed to exist. In turn these hypotheses are developed in theory in order to find implications that can be tested by returning to measured changes. Out of such tests come new or improved hypotheses, more implications that can be tested by returning to measured changes, and so on *ad infinitum*.

Obviously, if one wishes to measure changes, his task may be greatly simplified if the changes can be started and stopped, accelerated and retarded, as the measurer desires. Consider for example the moon and the tides. Measurement of their respective changes and study of the correlation that is found can hardly be done thoroughly in less than a month, and a substantially higher degree of precision might require observations and

measurements extending over several years. If one could control the moon and the tides, accelerating and retarding them at will, the same final results might be obtainable in a few hours of observation and measurement. This might be an advantage but is not, as we all know, a prerequisite to scientific study of these matters.

At this point, I should emphasize that the scientist observer, even in the instance of the moon and the tides, must control himself and his measurements even if he does not control the changes measured in the same sense of the word *control*. Haphazard observation and measurement will not suffice for scientific work. The precision of the measurements may be of a low order, but they must be systematic and correct within the degree of precision required for scientific inquiry. Usually, but not always, the more precise the measurements of changes the more soundly based are the ultimate warranted assertions.

Perhaps the most striking feature of modern scientific method is the development of experimental procedures. Controlled experiments are the scientists' means of facilitating their measurements of change. The traditional man of science, at least to laymen, usually is the white-coated figure deeply engrossed in his laboratory experiment, probably peering into a test tube wherein he has the subject matter of his experiment well under control. Obviously this white-coated figure has a great advantage if, for example, we suppose he is measuring the oxidation of iron, over a scientist whose only means of making a comparable series of measurements is to observe and measure a piece of iron in his backyard as it slowly rusts away. By appropriate application on the heat from his Bunsen burner and by other means, the white-coated laboratorian can, in a few minutes, measure changes that his fellow scientist in the backyard cannot complete measuring for years.

Accompanying the development of the controlled experiment as a means of facilitating measurement of change and studying the relations among changes has been the development of special instruments devised to make such measurements more precise. For example, the 200-inch telescope at Palomar is the latest instrument in a series developed from the crude beginnings provided by Galileo and his predecessors. By means of such an instrument and various accessories such as the spectroscope, controlled measurements of changes among the celestial bodies can be made on a scope and with a degree of precision heretofore impossible. In this instance it will be noted that the observer and his measurements are controlled in the sense of being systematic and directed toward a definite objective. The celestial bodies are of course not under control in the same sense of the word that the laboratory scientist is said to control the materials in his test tube.

Two groups of critics are found among those who have observed modern scientists at work. One group of such observers has been so preoccupied with the laboratorian's work with his materials in a test tube that they overemphasize the importance of such controlled experiments. They argue that controlled experiments (in this limited sense) are the essential feature of scientific method and that application of that method therefore is impossible or at best much more difficult with subject matter that cannot be put in a test tube in a laboratory.

Another group of observers has been equally impressed with the importance of the instruments devised by scientists in order to facilitate their measurements of change and studies of the relations among changes. Members of this group argue that, until instruments of some unspecified type but comparable in complexity, precision, and purpose to the 200-inch telescope can be devised for use in work on social problems, there is no hope for success of the scientific method in studying human behavior.

Especially interesting is the fact that some of those who most strongly insist on the applicability of modern scientific method to all of man's problems at the same time discuss the subject of experiment in such a manner as to provide ammunition for the extreme experimentalists or instrumentalists mentioned above. Of course, the great advantage derived in some fields from laboratory experiment should not be belittled, nor should the marvelous intricacy and, to the laymen, peculiar usefulness of modern scientific instruments be disregarded. But one should not forget that both laboratory experiments and the marvelous instruments that have been devised are but means to an immediate end, namely, the measurement of changes and study of the relations among changes. If that immediate end can be attained in any field by other means, there would seem to be no reason why the application of modern scientific methods of inquiry should not proceed.⁵

Lest misunderstanding arise, a few additional comments seem necessary. I do not imply that measurement of change and study of the relations among changes are the essence of scientific method but only that they are necessary parts of such method. In the absence of the results of such measurements and studies as aids in the formulation of hypotheses and in the absence of such measurements in order to obtain data that test hypotheses, a method of inquiry is neither modern nor scientific; a method of inquiry that did not include measurement of change and study of the relations among changes would, in effect, be a reversion to the prescientific procedures that have failed so dismally in the course of man's history.

⁵ As Dr. Ratner has pointed out, John Dewey never fell into the errors of either extreme school of experimentalists or instrumentalists. It is in large part to Dewey, of course, that we of Western civilization owe our present understanding of scientific method and its general applicability.

One other point should be made. I do not assert that progress in the sciences dealing with human behavior will be as rapid as it might be if a greater variety of controlled laboratory experiments were possible and better instruments were available. In due course a greater range of controlled experiments and many better instruments presumably will be devised. But I do assert that man has been unable to discover any other method of inquiry that even offers a hope of ultimate success in coping with the problems of men. Until a better method is devised, I assume that the best hope of progress lies in the method that has proven successful in greater or less degree whenever it has been applied to date.

Aspects of the New Method Applicable to Economics

Economists are fortunate in that measurement of change has long characterized their field. For example, price fluctuations are measurements of changing exchange ratios, and price data are available in great detail over long periods. Of course, the data become less voluminous and less reliable as one searches the records back 100, 200, or 300 years, but in recent decades and currently such measurements of change are widely available.

Measurements of changes in the output of factories and farms also are available in great detail and for prolonged periods. In the United States the Census of Manufacturers provides a wealth of historical data that are supplemented by more up-to-date measurements of change in great detail.

The changes in various other aspects of economic behavior also have been measured in detail over substantial periods. Changes in such items as employment, freight carloadings, purchasing media including bank credit, retail sales, investments, savings, and hundreds of others are reflected in reasonably accurate compilations of statistics that either directly or indirectly provide measurements of economic change.

Since World War I, measurement of economic change and study of the relations among such changes has been fostered by various research agencies of which the National Bureau of Economic Research is perhaps the outstanding example. On the whole, economists appear to be in a far better position than their contemporary workers in the other social sciences and are far better off with reference to the availability of measurements of change than were all scientists when the other sciences were in a comparable stage of development.

Now the mere availability of measurements of change provides no assurance that the data are sufficient for the work that needs to be done, nor is there any assurance that the available data will be wisely used. Quite probably much more extensive measuring over longer periods and

much more experience in using the results will be necessary in order that economic inquiries may progress as needed. However, there is no doubt that sufficient measures of change already are available for the testing of some economic theories or hypotheses. Even if the tests resulted only in discarding theories otherwise plausible and widely accepted, the data could be of great value.

Unfortunately there is not yet widespread acceptance among economists of methodological standards. Older methods of inquiry still dominate the field in large part.

The urgent desirability of beginning with the crude observable data of primary experience, rather than with the refined objects of reflection, is not generally recognized or at least is not insisted upon. That hypotheses should grow out of study (long and detailed study rather than cursory appraisal) of the changes that have been measured is not yet widely accepted.

Hypotheses have been developed in great profusion with elaborate extensions of intriguing theories. However, and aside from the semantic stumbling and logical inconsistencies found in many such theories, there is not as yet general insistence on control of theory by testing the logical implications thereof against measured change. In the more advanced sciences, this is the standard procedure, and no scientist who wished to preserve his reputation would presume to suggest doing otherwise. In economics, some outstanding figures even deny that any possible experimental or experience tests could conceivably disprove their finely spun theories; and only a few seem to realize that a modern scientist does not pretend to have achieved warranted assertability until such tests have been successfully met.

That the revolution in method fostered by Galileo has begun to be reflected in the field of economics has been apparent for some time. Thus far, however, progress has been slow. That the principles of modern scientific inquiry will be more widely applied in the field, that economists generally will eventually insist that the criteria for scientific work be met by those who claim to have achieved warranted assertability is what apparently must occur if progress is to be assured.

Until that time comes, there is every indication that the persuasive proponents of popular panaceas will continue to be accepted by the public as the economists who "know the answers." Whether policies adopted at their urging will prove to be one-way streets to disaster before better and scientific guidance can be obtained remains to be seen.

IX.

THE DEVELOPMENT OF ECONOMIC SCIENTISTS: The AIER Graduate Fellowship Program

MOST of AIER's educational efforts are carried out through the sale and distribution of its publications to the general public. However, for many years the Institute also has sponsored a variety of fellowship programs that provide direct training and financial support to advanced students of economics.

Early History

In order to "aid in the development of economic scientists," in 1946 the Institute established a competitive 2-year graduate fellowship designed to train students while in residence here at Great Barrington. World War II had interrupted the formal education of a great number of students. And while many possessed the enthusiasm and intellectual capacity for graduate training, at war's end many also were understandably deficient in subjects that are crucial to any advanced program of study in economics. Thus, the Institute's earliest fellowship programs offered students a wide range of relatively remedial tutorials before they proceeded to advanced courses in economics: basic instruction in English composition, semantics, logic, and economics.

Only after they had completed a first-year "refresher" curriculum in these subjects did they receive advanced instruction in money and banking, business-cycle analysis, American economic history, the development of political and economic thought, and the evolution of modern scientific method — topics that today form the core curriculum of the Institute's summer program.

Given the relative dearth of developed graduate programs in economics, this early program met with some success. It was, however, soon interrupted by the onset of the Korean War. Again, the formal education of many young men was interrupted, and for a time, graduate applications dropped to the point that no qualified students could be found for the program.

In response to this unavoidable setback, the fellowship program was reorganized in the mid-1950's under the administration of the Interfoundation Committee for Economic Scholarships, which drew support from five participating organizations — AIER, the Economic Education Institute, the Economic Education League, the Lincoln Foundation, and the Robert Schalkenbach Foundation. The Interfoundation Committee was formed to award scholarships to promising *undergraduates* who

showed interest in economic subjects. Roughly 200 undergraduate scholars were accepted in the program each year, and each was encouraged, but not required, to elect a college curriculum consistent with the requirements necessary for advanced study in economics. It was hoped that as these students “came of age” academically, they would form a substantial pool of applicants for AIER’s graduate fellowship program.

However, as events turned a rapid expansion of the graduate fellowship program did not occur. During the late 1950’s and early 1960’s, small numbers (one or two) of highly qualified students continued to enter the graduate fellowship program each year, but in effect the program became an early casualty of vastly expanded Government spending on academic research programs. Very simply, an abundance of Federal support via research grants and scholarships and private scholarship endowments such as the Fulbright Scholarship (and the National Merit Scholarship program for undergraduates) placed the Institute’s program at a severe competitive disadvantage. Understandably, many graduate students of the highest caliber were attracted to the prestigious universities that offered the most grant money for research projects in their field of interest.

A period of considerable uncertainty as to the future of the fellowship program followed. In part as a result of the attractiveness of the National Merit program, the Interfoundation Committee’s undergraduate scholarship program was discontinued and the summer fellowship program was broadened to include both graduate students and a small number of distinguished undergraduates. Even so, in some years during the late 1960’s and 1970’s, no fellowships whatsoever were awarded.

In recent years the Fellowship Program has enjoyed considerable success. But its early experiences constitute a textbook illustration of some of the (perhaps) unintended consequences of government funding of “scientific” research: namely, the distortion of the intellectual marketplace in ways that may be detrimental to genuinely independent research and education.

Government-Funded Research and Education: A Booby Trap?

For many years popular sentiment apparently has favored the expansion of government spending on research and education on a wide variety of human problems. However, the complexity of the relation of behavioral scientists in particular to the government has yet to be explored adequately. Often, the assumptions underlying popular beliefs about government-sponsored science seem naive or inconsistent.

Although most people apparently assume that scientific considerations “automatically” outweigh political considerations in the exchange of money

for results, history plainly suggests the opposite. Even the briefest reflection on some of the official “scientific” findings in Nazi Germany suggests the extent to which unbiased inquiry can be subverted by the political process. Seemingly, the more extreme the politics, the more extreme the distortions of scientific procedure.

And as unfortunate as the case may be, even under a relatively benign regime, scientifically *warranted* assertions (and policy recommendations based on those assertions) may be so unpalatable to politicians that they will not provide the financial support desired or needed. The lure of large amounts of Federal money, for example, may lead behavioral scientists into a situation in which they cannot function as scientists but can function only as special pleaders for the politicians in power (or politicians they hope will come to power and provide them with patronage). The current “scientific” debate over any number of environmental and health issues — from the greenhouse effect to AIDS — seems a case in point.

Already apparent is the fact that governments recognize how important the work of behavioral scientists has become. For example: the Communist party in the Soviet Union has taken great pains to control the work of behavioral scientists with a view of ensuring that they serve the interests of those in power; and in the United States during recent years each political party when in office has used some behavioral scientists in ways evidently intended to further the retention of power.

Many people today are so impressed with the benign aspects of democratic or republican forms of government that they forget the lessons of history. The first democratic government in Europe following the French Revolution, which was inspired in part by the success of the American Revolution, beheaded Lavoisier, the father of modern chemistry. On the other hand, much early scientific work in the 17th and 18th centuries was made possible because benevolent despots in various European countries chose to defy some religious leaders and protect a few scientists as well as support their inquiries. More recently, the economic advisers of American Presidents have endorsed economic action so unsound that, in the words of a distinguished Harvard professor, it should “make every economist blush.”

In short, an important lesson to be learned from the experiences of history is that scientists should not expect to be assured of unrestricted freedom of inquiry and discussion as the servants of the government, any form of government, nor by any vested interest having special privileges or positions of power that those interests desire to defend and perpetuate. Especially should behavioral scientists be wary of becoming the tool of agencies that may inhibit full freedom of inquiry and discussion, because,

of the three major fields of science — physical, physiological, and behavioral — the last deals almost continuously with controversial matters of consequence to one or another vested interest.¹

The apparent fact is that no government of any form yet devised has demonstrated that it can be trusted to assure scientists full freedom of inquiry and discussion. It has been AIER's view that this major difficulty confronting behavioral scientists might be solved if an important group of them could derive the principal support for their continuing research and their educational programs from many thousands of citizens rather than from the Government or from powerful private entities that may have special-interest agendas of their own.

The Summer Fellowship Today

AIER currently awards about a dozen Summer Fellowships each year to college and university students in economics. Summer Fellows come to the Institute for an 8-week period of study at the beginning of their graduate careers. Those who demonstrate exceptional potential during that program are awarded Fellowships In Absentia for the succeeding academic year, including full or partial payment of tuition and/or a monthly stipend for all or part of the academic year. As students progress toward the Ph.D., additional in-absentia awards are made on the basis of performance.

Since students not only attend classes at the Institute, but also share office space in the Harwood Library and take meals together with the staff, the summer program provides both formal and informal opportunities for the Institute staff and visiting senior fellows to exchange with students views they may not otherwise encounter.

The program's structure is always subject to revision, but in recent years usually has involved several major topic areas, including the methodology of economists, monetary economics, and business-cycle analysis, as well as a number of "mini-seminars" presented by Visiting Senior Fellows who bring expertise in a wide variety of fields related to AIER's research interests. In keeping with AIER's insistence that the pursuit of inquiry be grounded in observation, the summer program provides students with the chance to observe working economists in a practical environment that contrasts sharply with some purely academic settings. Here they are exposed to any variety of questions that confront "real world" analysis but that often are ignored by academic economists.

In contrast to the earlier difficulties described above, during the past

¹ See E. C. Harwood and R. Handy, "Would Government Support be a 'Booby Trap' for Behavioral Scientists?," *The American Journal of Economics and Sociology*, Vol. 30, April 1971, pp. 130-131.

decade the program has achieved marked success in attracting distinguished graduate students and visiting senior fellows. Although many factors no doubt are involved, perhaps the Fellowship program's recent success is in part a gratuitous result of the "belt tightening" in higher academia that followed massive commitments of long-term funds to tenured faculty (itself the result of the earlier abundance of Federal funds). Whatever the cause, in recent years, U.S. colleges and universities represented in the Institute's Fellowship Program have included, among others: Brown, Case Western Reserve, Columbia, Cornell, Harvard, Indiana University, Johns Hopkins, MIT, Notre Dame, Penn, Princeton, Stanford, Syracuse, University of California, University of Chicago, University of Illinois, University of Michigan, Vanderbilt, and Yale.

From its earliest years, the Fellowship Program was designed to be international in scope, and in recent years has been enhanced by the participation of students from Europe, Latin America, and Asia, representing institutions as far-flung as the London School of Economics, Rotterdam University (the Netherlands), the University of St. Gall (Switzerland), the University of Milan, the University of the Philippines, the University of Lima (Peru), and Fudan University (China).

Visiting Senior Fellows in recent years have included a number of luminaries, among them: former Congressman Ron Paul, author George Gilder, Mr. Joe Cobb of the Joint Economic Committee of the Congress, Professor C. Lowell Harriss of Columbia University, Dr. Marvin Goodfriend of the Federal Reserve Bank of Richmond, Mr. Alan Reynolds of Polyconomics, Dr. Marc Miles of H.C. Wainwright & Co. Economics, Inc., Social Security authority Dr. Marietta Constantinides, and Professor Andrea Terzi, who is E.C. Harwood Professor of the Behavioral Sciences at Franklin College, Switzerland.

Outlook

In short, the AIER graduate fellowship program today shows the promise of fulfilling the hopes of its founder. As with all educational ventures, its eventual success (or failure) will depend on a variety of interrelated conditions. As a *sine qua non*, its reception will depend on the quality of its faculty and students and of their "product" — *i.e.*, research findings and professional training that can be usefully applied to actual human problems.

Past experience suggests that, to a considerable extent, achieving this result may depend in turn on a political environment that provides incentives for (or at least tolerates) unbiased inquiry. Given the demand for increased Government funding for research on any variety of actual and imagined problems — and apparent popular appetite for prescribed results

— the maintenance of a climate favorable to genuinely independent inquiry into those problems is by no means assured.

The Institute's by-laws require that all of its programs, including the fellowship program, remain independent of *any* special interest or concentration of wealth, whether Government agencies, private lobbies, corporate foundations, and the like. Unlike those of virtually any other organization with similar purposes, the Institute's educational programs operate without benefit of *either* Government or private institutional support. At bottom, the success of our graduate fellowship program (and all our other programs) thus depends upon the support of thousands of individuals who may benefit from our research and who value our efforts to promote independent scientific inquiry and education. In short, it depends on the readers of this bulletin and other Institute publications.

X.

THE ROLE OF BEHAVIORAL SCIENTISTS IN A DURABLE CIVILIZATION

SCATTERED around the world are the remnants of civilizations long dead: the pyramids of Egypt; crumbling amphitheaters and other ruins, evidence of the grandeur that was Rome's; classic temples of Greece; almost forgotten cities of the Middle East; the Great Wall of China; jungle-hidden ruins in Central America, the only remaining evidence of the Yucatan or Maya civilization; and the few remnants of Aztec and Peruvian civilization. For at least 7,000 years, perhaps much longer, men have been developing civilizations only to see them decline and fall.

Perhaps some observers of Western civilization complacently assume that the great technological advances will enable our civilization to endure; but many Roman citizens similarly were complacent 2,000 years ago when they reflected on the majesty of that supposedly eternal civilization centered in Rome. In view of the obvious dangers confronting Western civilization in a divided world, the great advances in the physical sciences may facilitate the destruction of our civilization rather than ensure its survival. In any event, the evidence available to date strongly suggests that men have not yet learned how to create a durable civilization so developing as to assure its own survival in a changing environment.

Some superficial observers have assumed that human societies, because they are composed of men, must be mortal, must die as do individual humans. However, more sophisticated observers see that such a conclusion is not justified. The babies born in this 20th century A.D. are as adaptable, as capable of learning, as any who lived in the days when Greek civilization was advancing most rapidly, or when Rome was conquering much of the known world, or when the fathers and grandfathers of those now living were developing our present society. Civilizations have died not because they were mortal like humans but because the people of earlier civilizations failed to solve the crucial problems of men in society.

Various methods of inquiring into the problems of men have been tried in the past. Oracles, seers, medicine men, and both secular and religious revelation at times have been relied upon. Numerous other methods of solving problems including application of proverbial wisdom or what was considered common sense (in the sense of the more common perceptions) also have been tried. However, during recent centuries the superiority of a different method of inquiry, that of evolving scientific procedures, has been established. The relative usefulness of this method first was demon-

strated in the physical sciences, then in the physiological sciences, and is now beginning to be demonstrated in the behavioral sciences.

The great advance of Western civilization since the 16th century was made possible by development of the physical and physiological sciences. Between the Chinese and European civilizations of the 14th and 15th centuries the differences in productive power were not great. But in subsequent centuries the West has advanced far more rapidly because of the application of scientific methods to the physical and physiological problems of men. The next major scientific advance may well be in the behavioral field. Here the West again has an opportunity to lead because in Russia the behavioral sciences are restrained by the Marxian dogma and because the underdeveloped nations are lagging so far behind in the general advance of science.

Today, many leading behavioral scientists in Western civilization, eminent anthropologists, economists, psychologists, sociologists, and others, agree that the problems of men in society, if solved, will be solved by the continued application of modern scientific methods of inquiry. This is not to imply that all of the needed solutions already have been found but that enough have been found and sufficient progress is being made to justify belief that pursuit of such inquiries offers the best and perhaps the only hope that men can solve their problems and create a durable society. For the purpose of further discussion we therefore assume that the behavioral scientists will be called upon to play a crucial role in the evolution of Western civilization during the decades ahead.

Already apparent is the fact that some existing governments recognize how important the work of behavioral scientists will become. For example: the Communist party in Russia has taken great pains to control the work of behavioral scientists with a view to ensuring that they serve the interests of those in power; and in the United States during recent years each political party when in office has used some behavioral scientists in ways evidently intended to further the retention of power. Whether or not some of the particular behavioral scientists concerned, either in Russia or the United States, willingly have prostituted their talents in the service of those seeking to obtain or retain power is not pertinent to this discussion. The point is that governments, of whatever type, have not hesitated to distort the work of scientists, even to corrupt or destroy scientists themselves, when those in power deemed such action expedient.

Many people today are so impressed with the benign aspects of democratic or republican forms of government that they forget the lessons of history. The first democratic government in Europe following the French Revolution, which was inspired in part by the success of the American

Revolution, beheaded Lavoisier, the father of modern chemistry. On the other hand, much early scientific work in the 17th and 18th centuries was made possible because benevolent despots in various European countries chose to defy some religious leaders and protect a few scientists as well as support their inquiries. More recently, the economic advisers of an American President apparently have endorsed proposed economic action so unsound that, in the words of a distinguished Harvard professor, it should “make every economist blush.” Presumably those advisers endorsed unsound policies, or at least failed to make known their view in opposition to them, because inhibiting scientific inquiry and discussion was deemed expedient by those in authority.

In short, an important lesson to be learned from the experiences of history is that scientists should not expect to be assured of unrestricted freedom of inquiry and discussion by a government, any form of government, nor by any vested interests having special privileges or positions of power that those interests desire to defend and perpetuate. Especially should behavioral scientists be wary of becoming the tools of agencies that may inhibit full freedom of inquiry and discussion; because, of the three major fields of science — physical, physiological, and behavioral — the last deals almost continuously with controversial matters of vital import to one or another vested interest.

Behavioral scientists contemplating their role in a durable society are confronted with the question, How can such scientists be assured of the right to inquire into and discuss whatever they choose, free of all constraints that might hamper scientific inquiry? Some would answer this question by referring to the so-called “inalienable rights to life, liberty, and the pursuit of happiness,” and many speak with reverence of these as “God-given rights.” However, anyone not blinded by illusions can see that men actually enjoy such rights only when and to the extent that the society in which they live enforces them. As recently as early in World War II, American citizens of Japanese ancestry discovered that even the Bill of Rights embodied in the Constitution of the United States failed to guarantee their rights when American society chose to disregard them.

The Independent Minority

Although the problem confronted by behavioral scientists in a totalitarian form of society may seem insoluble, in a republic the situation is somewhat different. The difference lies not in any alleged reasonableness of representative government; as has already been pointed out, no government of any form yet devised has demonstrated that it can be trusted to assure scientists full freedom of inquiry and discussion. the important difference for a republic, at least for the United States now and in the

foreseeable future, lies in the fact that an independent minority of educable citizens holds the balance of political power.

Professional politicians are divided into two major parties, the “Ins” and the “Outs.” The former enjoy the pay, perquisites, and power of office; the latter seek them. Most citizens, by reason of their family or social environments, habitually favor one or the other major party at the polls. They usually vote as their parents or their friends have always voted. In most states of the Union the difference in number between the habitual supporters of the two major parties is relatively small. Consequently, the independent voters (independent even if registered as “Ins” or “Outs”) determine who will be elected and what major policies will be adopted.

The actions of professional politicians reveal their understanding of the situation. The assiduous attention paid to minorities clearly reveals the continuous attempt to align on the side of the “Ins” or the “Outs” those whose relatively few votes can make the difference between election and defeat.

For example, fewer than 1 percent of the American people are farm operators who benefit from the farm program. Nevertheless, professional politicians among both the “Ins” and the “Outs” have sought to obtain the votes of this small group, because such politicians well know that the difference of a few votes may change the balance of political power.

Few people realize how great is the power of the Nation’s independent minority. Even in the landslide vote of 1936, when Roosevelt obtained one of the greatest electoral majorities on record, a shift of only 1,119,500 votes in 17 states would have resulted in the election of Landon. As nearly everyone still remembers, the shift of only a few thousand votes in a handful of states would have resulted in Mr. Kennedy’s defeat in 1960. The professional politicians are well aware of the actual situation, however. That is why they are so eager, 365 days in the year, to know the views of the independent minority.

The notion of two parties differing widely in basic views, and led by statesmen whose deep understanding and inclinations have committed them irrevocably to divergent paths, is but the naive dream of amateur political theorists. The facts of political life are quite different. Few professional politicians can afford the luxury of irrevocable commitment to any views, except possibly in favor of motherhood and the protection of babies in arms.

Rarely can a man who is not an extremely able professional politician hope to be nominated as President, to say nothing of being elected. Moreover, even an able statesman, no matter how great his understanding

and how wise his choice of policies, would be hamstrung without the support of Congress. That support, free from the influence of pressure groups now feeding at the public trough, would not be forthcoming unless the Nation's independent balance-of-power minority had unmistakably indicted what they wanted.

The fact that such an independent minority holds the balance of political power in the United States and may do so for decades to come provides a possible solution to the problem confronting behavioral scientists. That minority is in a position to assure that some behavioral scientists have full freedom of inquiry and discussion.

Obviously then, a major problem confronting behavioral scientists might be solved if an important group of them could derive the principal support for their continuing research from many thousands of citizens among the Nation's independent minority. Such a broad base for support would be preferable to dependence on one or a few large foundations. Such agencies usually are managed by representatives of great wealth and sometimes are reluctant to be concerned with controversial matters. Also, on at least one occasion in recent years, a large foundation was influenced by subversive interests to such an extent that a man was selected as its head whose contempt for freedom and scientific inquiry was evidenced by his allegiance to communism and the Marxian dogma.

Support for a group of such leading scientists might be forthcoming if most of the Nation's balance-of-power minority were convinced that those scientists were helping to find solutions for the problems involved in developing and enduring civilization. The question that then arises is: What should be the principal features of an organization of behavioral scientists who would function as an advisory council for the citizens of the United States and others in the world seeking an enduring civilization?

Essential Aspects of a Behavioral Research Council

The following aspects of such an organization of behavioral scientists would seem to be essential:

1. Provision should be made for ascertaining and extending the specifications for an enduring society desired by the citizens, based on the Constitution of the United States, Amendments, and pertinent indications of the desired features of society as ascertained by continuing scientific inquiry including modern polling procedures.

2. Priorities among research projects should be established by their relative importance in helping to achieve the desired features of society. Of course, such relative merit is not always readily ascertainable, but funds should not be dissipated on trivial projects.

3. The most modern scientific methods under continual development should be relied upon, and more comprehensive cross-fertilizing among the sciences should be fostered.

4. Public service as scientist-consultants, primarily to the citizenry, should be emphasized. The council would foster scientific inquiry resulting in warranted “if-then” conclusions concerning probable costs and consequences of alternative courses of action for solving the major problems of men in society. Thus the citizens would have the best information obtainable on which to base their decisions. This information should be freely available to all men so that those of other nations also may join, if they choose, in the effort to develop an enduring civilization. The citizens of the United States or of other nations might considerably alter their present ideas about the desirable aspects of a durable society and about the best means of achieving that objective when given a better understanding of the costs and consequences involved. Such changes in views presumably would be ascertained by comprehensive polling procedures to be developed in further scientific inquiry and appropriately reported.

5. The citizens served have a right to expect that members of the council would avoid any conflict of interest; *i.e.*, no private or Government employees, in instances where conflict of interest might arise, or elected officials should be eligible to membership.

6. Membership on the council should imply a primary moral commitment to modern methods of scientific inquiry for solving the problems of men in society. (A tentative code is suggested below.)

7. The qualifications for membership on the council should be clearly stated so that those supporting the organization may be well-informed.

8. The council should have full power of inquiry into all aspects of any member’s behavior with full disclosure to the council required, all with a view to ensuring against perversion of scientific inquiry and to removing members found unsuitable. The purpose of this provision would be two-fold: (1) to assure scientist members that the council would have the necessary means and authority to preserve its scientific integrity (we assume that no conscientious scientist would wish to join an organization that lacked the means and authority to safeguard its own integrity); and (2) to justify public confidence in the organization.

9. Specific provision should be made to assure that all material published by the organization meets adequate standards for scientific work; *i.e.*, approval by more than a majority of the behavioral scientists concerned should be required for publication as an official report of the council, and provisions should be made for simultaneous publication of

dissenting views when significant differences exist among the councilors concerning the scientific basis for conclusions.

For several centuries, the medical scientists have enjoyed the confidence of their fellow men in Western civilization. The fact that a physician has acted in accordance with the Hippocratic oath to save human life and to preserve inviolate the confidence of his patients has been an important factor in his deserving and obtaining the confidence of his fellow men. Perhaps a statement of moral obligation or commitment analogous to the Hippocratic oath would remind behavioral scientists of their obligations and would help them to merit by their actions the confidence and support of their fellow citizens. Such a code¹ is suggested in tentative form below:

My primary and overriding moral commitment or obligation is to serve as a behavioral scientist for the purpose of seeking solutions for the problems of men in society and publicly informing my fellow citizens as to the results of such scientific research. This implies:

1. Relying in such inquiries on the methods of modern sciences in their evolutionary development.
2. Endeavoring continually to improve my own ability as a scientist to develop warranted “if-then” conclusions or assertions by applying scientific methods and by subordinating any personal biases in order to assure objectivity in my work and findings.
3. Avoiding all conflicts of interest (such as might result from employment by special interests, etc.) that might inhibit scientific work or bias me in any way tending to pervert scientific inquiry.
4. Differentiating clearly in all writings and public statements so that those to whom I communicate will understand whether I am speaking or writing in my role as scientist within my field of competence or am simply urging in my role as a citizen or in some other specified role a course of action that I personally prefer.
5. Exposing as unscientific, without fear or favor, all purportedly scientific reports within my field of competence that (in the absence of

¹ Problems connected with academic freedom, loyalty oaths, and witch hunting for Fascists and Communists, have induced emotional reactions in some individuals to certain words. For the benefit of any who have thus reacted to anything said here, further clarification is in order. The behavioral scientists who have suggested the code do not propose that all behavioral scientists should function as suggested nor that behavioral scientists have any greater obligations to their fellow men than do physical scientists or physiological scientists. Those who drafted the code do suggest that the status of some behavioral scientists is analogous to that of some physiological scientists who are members of the medical profession and that formal recognition of their obligations is essential if they are to obtain a full measure of confidence and support for their work.

such exposure) could be expected seriously to mislead my fellow citizens, whom I have chosen to serve.

Anyone who purports to be a scientist in the field of economics thereby seeks to join the ranks of scientists generally. In discussing the primary duties of economic scientists, I do not mean merely the duty of anyone who happens to be labeled, by himself or others, as an economist. That label is the name for a large and heterogeneous group of individuals whose professional obligations perhaps do not fit into any single pattern; at least, their obligations will not be discussed here.

The methods by which men have acquired “knowledge” vary greatly. Some of the earliest procedures that have been used as the basis for what some individuals, at least, have considered warranted assertions are still widely used. This is the situation even in an industrial advanced social group such as Western civilization where acceptance of change including recent technological developments has become commonplace.

For example, some who call themselves economists evidently regard various aspects of the Keynesian dogma virtually as revelations (or what might be considered secular as distinguished from ecclesiastical revelations). Specifically, Keynes’s “fundamental psychological law” to the effect that “... men are disposed, as a rule and on the average, to increase their consumption as their income increases, but not by as much as the increase in their income”² may be regarded either as an assertion based on revelation or as an hypothesis pointing toward the desirability of further and more scientific investigations. Those economists who have prescribed the Keynesian remedy for economic ills apparently have accepted the first choice and regard revelation, to some extent at least, as a basis for warranted assertability or “knowledge.”

The foregoing illustrates the type of comment that seems justified by some recent economic writing and teaching. Keynes was not, of course, the only one to pull some “knowledge” out of the thin air by the process of secular revelation. However, the widespread acceptance of his teaching by ardent disciples, a few of whom have undertaken to extend the revelations and to apply them as economic principles, testifies to the current general acceptability of the time-worn method of revelation.³

² J. M. Keynes, *The General Theory of Employment, Interest and Money*, New York: Harcourt, Brace and Company, 1936, p. 96.

³ That either Keynes or any of his followers, as far as I know, have *openly avowed* acceptance of revelation as a basis for “knowledge” is not, of course, implied. We are concerned here not with what men think their methods are or would like others to think the methods are but with what actual methods are used. Any statement that is drawn out of the thin air or is based on nothing but its seeming authenticity to the author of the assertion (without supporting data or references) would illustrate what I mean here by “revelation.”

The application of “common sense” as it is embodied in the proverbs and other records handed down from past generations is another method of acquiring “knowledge” that has been widely used by economists, past and present. Sometimes “knowledge” derived from or supported by proverbs or proverbial relationships, such for example as the notions of Malthus, seems to be widely accepted by economists in spite of such facts as the present huge surpluses of farm products.

Word magic in its primitive form as a source of “knowledge” is illustrated in the activities of tribal medicine men and their audiences who believe that utterance of the word will call forth the thing or that the word is somehow identified with the thing. Less obvious is the hypostatization in which many economists and others have indulged.

Introspection as a method of developing warranted assertions about man’s economic behavior has been widely accepted. Some prominent economists, such as von Mises and Hayek,⁴ for example, have even asserted, in effect, that a combination of introspection and the understanding of a reasoning mind without regard to the facts or to experience is the only adequate means of obtaining economic “knowledge.”⁵

Platonic idealism was still another way of developing what many men have considered warranted assertions or “knowledge.” The manipulation of ideas or concepts and discussions of supposedly invariable relations among them by skillful application of verbal facility frequently passes for knowledge in the field of economics. Such dialectical quests for certainty can be found in numerous economic works that are by no means generally considered outmoded.

The Aristotelian version of the quest for certainty⁶ differed from the Platonic effort to some extent while retaining the same objective of inquiry, absolute certainty. For many centuries and until fairly recent times the logical processes of Aristotle were almost universally accepted in Western civilization as a suitable means of acquiring or developing “knowledge.” Some economists even today seem not to realize that the syllo-

⁴ For interesting analyses of Hayek’s method, see the papers by May Brodbeck and Richard S. Rudner (Chapters VI and VII of this book).

⁵ Ludwig von Mises, *Human Action, A Treatise on Economics*, New Haven: Yale University Press, 1949. Especially pertinent are the following from pages 26 and 858, respectively, “For the comprehension of action [including economic behavior] there is but one scheme of interpretation and analysis available; namely, that provided by the cognition and analysis of our own purposeful behavior” (in other words, introspection), and “... the ultimate yardstick of an economic theorem’s correctness or incorrectness is solely reason unaided by experience.”

⁶ John Dewey’s apt phrase, the title of a collection of his essays on methods of inquiry (*The Quest for Certainty*, New York: Minton, Balch & Company, 1929).

gisms of Aristotle have long since been found inadequate (albeit still useful for some purposes) for the inquiries of modern science.

Modern scientific method may be said to have begun with the Galilean “revolution.” Briefly, Galileo took what was in his time the extreme and extremely dangerous position that measurement of change should control theory, control it in the sense of forcing its modification or even its abandonment. That was not by any means the full scope of the revolution in method, and there are indications that the revolution is still in process. Nevertheless, we can outline briefly the principal features of the new method of inquiry.

Modern scientists have abandoned the quest for certainty.⁷ They claim to “know” only what they have confirmed experientially (or experimentally) rather than in the sense of finding within their heads. They feel no need to identify their “knowledge” with prior conclusions derived through ratiocination alone, or revelation, etc. They make no claim to have found ultimate certainty that must persist forever without change.

Moreover, the modern scientist is not particularly surprised when new discoveries are made, even discoveries so new and radical that what had been considered “knowledge” of a subject must be drastically modified or abandoned. Not having assumed that he had broken through the veil of transient change to an immutable reality behind appearances, the scientist today more readily adjusts himself and his theories to the further fruits of experiential (including experimental) inquiry as those fruits are gathered by himself or others. Thus the warranted assertions developed by science can be modified, expanded, and supplemented as man’s growing skill in conducting inquiry provides new results that are useful.

Finally, modern scientists insist that what they infer from their measurements of change and study of the relations among changes (whether in the laboratory, in the limitless expanse of the starry universe, or in the statistical records of economic events) shall not be subordinated to any “knowledge” derived from ratiocination, revelation, dialectical facility, introspection, intuition, or any other source. In short, the “obvious,” the “*a priori*,” the immediately “known” or “understood,” and theories derived from those sources, no matter how impeccable the logical processes, must give way to inference and reflection based on measurements of changes experienced in the natural world of events.

⁷ A more accurate statement might be that modern scientists *who appreciate the full significance* of the methods they apply in their respective fields no longer expect to find immutable Truth, everlasting reality or Being, or any similar figments of hypostatization. However, the writings of some scientists reveal that they still describe their methods in terms that inadequately specify what they actually have done.

In the light of the foregoing, what are the primary duties of economic scientists? If they hope to be given time by an impatient public to do the job that apparently must be done, should they not make clear to all who look to economists for guidance that much now offered even by reputable economists in the form of remedies for economic ills is not scientifically grounded? Should they not also encourage within the profession the development of criteria or standards for scientific work with a view to differentiating between those accept discredited and unscientific methods of conducting inquiry and those who are endeavoring to use the applicable principles of modern scientific method? Certainly they should emphasize on every possible occasion that those who claim to have found certainty have been chasing a will-o'-the-wisp that has not been located or identified elsewhere than in men's imaginations.⁸

Those who are trying to be economic scientists are hampered by the fact that "Inferential inquiry in scientific procedure is an adventure in which conclusions confound expectation and upset what has been accepted as facts. It takes time for these new facts to be assimilated: to become familiar. Assimilation of the new to the familiar is doubtless a precondition for our finding ourselves at home in the new and being able to handle it freely."⁹ If our fellow citizens will give us a few decades, we may be able to demonstrate that enough economists "laid end to end" can reach a useful conclusion.¹⁰

⁸ For example, when von Mises says on page 39 of *Human Action* "The theorems attained by correct praxeological reasoning are not only perfectly certain and incontestable, like the correct mathematical theorems. They refer, moreover with the full rigidity of their apodictic certainty and incontestability to the reality of action ..." he is writing what is nonsense to economic scientists, however imposing such phraseology may be to others. (Rather than a footnote to a footnote, this parenthetical comment may serve to save some readers the trouble of looking up "apodictic" in their dictionaries. Webster says that "apodictic" means absolutely certain; consequently, Dr. von Mises claims what apparently has been the exhilarating experience of finding absolutely certain certainty. That his theories are also "incontestable" would seem to make them invulnerable to any criticism, insofar as word magic can inhibit modern critics. It is difficult to imagine a more convincing demonstration of the backward status of economics as a science than the fact that this volume has not been generally denounced by economists as a nonsense that much of it obviously is, from the viewpoint of modern science.)

⁹ John Dewey, *op. cit.*, p. 185.

¹⁰ My age being sufficiently advanced so that I am nearly (not absolutely) certain to be among those laid end to end in the meantime, I feel that I have a special interest in seeing that those who bury my generation are prepared to align us in neat rows pointing, I hope, in the right direction.

XI.

RECONSTRUCTION OF ECONOMICS

FORTUNATELY the task of reconstructing economics was well begun by Wesley C. Mitchell and those associated with him who initiated the National Bureau of Economic Research in 1920. The tremendous task of systematizing and extending the measurements of economic change is underway on a broad front with the aid of several Government bureaus and private agencies. A good beginning, but only a beginning, thus has been made on this aspect of the task.¹

Measurement of change and study of the relations among changes constitute only part of modern scientific method. From such studies, hypotheses must be developed, their implications must be tested against the existing data or against new measurements of economic change, then the hypotheses must be modified or discarded as may be necessary, re-tested, etc. Thus economic theories must be refined to meet scientific standards so that they may lead to conclusions that have the status of warranted assertions.

Most of the so-called economic theories found in existing textbooks are not properly classifiable even as hypotheses. In scientific work, hypotheses have directive power; they are used as guides that point to matters requiring investigation. The logical implications of hypotheses indicate to scientists which measurements of change (in the detailed records of experience or experiments) should be sought for verification or refutation of the hypotheses. Hypotheses when initiated are controlled by the facts available and in turn control or guide the research worker in his further measurements of change; and, in the last analysis, hypotheses are controlled (*i.e.*, modified or discarded) by the data that test their logical implications.

In short, notions about events may properly be labeled hypotheses, in the scientific sense, only when such notions are used as tools of research as indicated. When offered as bona fide explanations of reality merely on the basis of their plausibility, or on the rigor of their logical or mathematical transformations, or because of their author's predilections for them, they are, from the scientist's point of view, no more authoritative than the theories of medieval scholastics.²

¹ Of course, economic data were being accumulated long before Wesley Mitchell was born, and many investigators had used such data, sometimes quite effectively. However, when one surveys the progress that had been made up to 1920 and notes the subsequent major improvements traceable in substantial part to the work of the National Bureau and to men trained in that organization, one must conclude that great credit is due to Wesley Mitchell for the reorientation of economics.

² Many writers believe that an hypothesis or theory that is internally consistent from the

But for the present generation of academic economists in particular, what is the implication of the present situation? Many are confronted with the fact that in their textbooks they are offering as knowledge material that cannot properly be so classified by anyone who understands modern scientific method. Much of what they have learned so laboriously through the years and much of what they are teaching probably will prove to be fit only for the intellectual junk pile where the works of the alchemists and of the 18th-century medical practitioners gather dust in quiet obscurity. Even if such members of the economic profession could understand the significance of the reconstruction that has begun, it is reasonable to expect their enthusiastic support? Such men have a vested interest in the status quo, which almost certainly will blind many of them to the desirability of the change in process.

Then there are the professional economists who have government or other jobs that require the use of their economic talents for other than scientific purposes. The influence of men in such positions ordinarily will not be exerted in ways that must disparage their own activities. Many economists today have vested interests in, for example, the Keynesian notions, interests that will make difficult for them an objective reappraisal of their positions.

Also to be considered is the fact that many, perhaps a majority, of the economists educated during the last half century lack the training or the intellectual capacity to understand the revolution that is underway in their field. For decades a process of adverse selection has been operating. In many centers of higher education, men who lacked the ability to think logically, who could not get passing grades in the natural sciences and engineering, have been welcomed in the social sciences. Frequently the stumbling block that has accounted for a student's detour to an easier field has been mathematics. In this connection, the Social Science Research Council reported as follows:

“Social science departments in general appear to be so fearful of deterring potential students that they tend to adapt their requirements to

viewpoint of logic or mathematics and “explains” the original problem must be satisfactory. For example, the fact that the Keynesian theories offer one plausible (in the sense of logically consistent) explanation of underemployment is widely accepted as proof that the theories are useful. But *scientists* are never satisfied with explanations that are only plausible. They realize that usually more than one logically consistent hypothesis can be developed. They realize further that the logical implications of each step in the theoretical development must be tested against measurements of change if practicable. *All* assumptions remain suspect until each relevant implication is tested against measured changes. One hypothesis is preferred to another, not because it seems more plausible from the viewpoint of internal consistency, but because when used as a guide to further measurement the resulting tests provide better confirmation than do the tests suggested by the other hypothesis.

the tastes of the least competent. Consequently, verbal facility, rather than a scientific attitude and mastery of scientific principles and techniques, is too generally the criterion of achievement in what are called the social sciences in undergraduate colleges; and this criterion carries over perceptibly into the graduate school.”³

“Failure to get adequate training in these subjects before receiving the Ph.D degree is certainly attributable in considerable measure to the fact that little or no statistical and mathematical competence is required of undergraduates majoring in social disciplines.”⁴

Obviously, the persistence of this situation during the past few decades has not been conducive to the development of a professional group alert to the progress of scientific method in the fields where it has been most successful, and capable of applying it in their own. Little help in the task of reconstruction is to be expected from such individuals, regardless of their sincerity.⁵

Thus it seems safe to predict that the reconstruction of economics will be an arduous task that will require many years. Is it possible to foresee now how the science of economics may develop and how its progress may be expedited?

Measuring economic changes, accumulating the data, and analyzing the more obvious relationships all are essential elements (but are not by any means all) of the job to be done. As has been mentioned, this aspect of the task is well-begun. Doubtless this part of the work will be further extended. In this connection, the development of modern recording and computing machines suggests that further advances ultimately may greatly expedite the various statistical processes.

As yet, the development of more fruitful hypotheses seems to have lagged behind the progress made by the statisticians. The theoreticians seem to have been outdistanced, for the time being, by the laboratorians. What is the reason for this situation?

Of course, the explanation in part may be that for too many years the

³ Elbridge Sibley, *The Recruitment, Selection, and Training of Social Scientists*, New York: Social Science Research Council, Bulletin 58, 1948, pp. 74-75. The Social Science Research Council is affiliated with the seven leading associations of social scientists. Dr. Sibley's report reflects the Council's experience during many years as well as extensive research on his part. We consider this report the best and most comprehensive analysis of the subject.

⁴ *Ibid.*, p. 72.

⁵ Lest this seem an indictment of the entire profession, I should emphasize that many economists are functioning successfully as economic scientists. But judging by many of the textbooks and other economic writing for public consumption, the economic scientists are a small minority of the professional economists today.

“intellects of similar constitution” have been coming into the field of economics and the intellects more capable of developing fruitful hypotheses (apparently a rare combination of analytical incisiveness and creative imagination) have been going elsewhere. However, I suspect that progress in the development of useful hypotheses has been retarded to some extent by the density of the semantic swamp in which much economic discussion has foundered.

If semantic difficulties are a major stumbling block, Dewey and Bentley have suggested, by the force of their example, a solution to that part of the problem.⁶ The first step apparently would be to develop accurate specification or naming. Once that has been well begun, the formulation of hypotheses probably will be easier in that more than one economist will know what another economist is talking about, and even the one who is talking will be almost forced to speak or write to better advantage. I hope that we at American Institute for Economic Research can in time contribute to this aspect of the scientific advance.

For the young men of today the reconstruction of economics provides an opportunity and a challenge that should invite the most proficient and stimulate the most eager student. There is all the excitement of a battle against odds where the potential gains are incalculable and the potential losses catastrophic. Western civilization seems to be at or near a critical time in man’s progression. He would be a bold prophet indeed who would predict that the counterrevolution (socialism and its militant twin, communism) within Western civilization would fail and that the original great revolution, which perhaps reached its point of farthest advance with the adoption of the Constitution of the United States, would succeed in the long run. The unpleasant fact is that the counterrevolution has been succeeding in one country after another. Even in our own country we have adopted measures that have turned backward from the goals of our own great revolution.

My guess is that the social, especially the economic, problems of our times will be solved by application of the scientific method, the method of organized intelligence in the modern world, or they will not be solved at all. Without sound solutions, what possible hope is there that Western civilization will not, as the counterrevolution succeeds, dig its own grave as have the civilizations that have gone before?

If my guess is correct, the future of this Nation, and therefore of Western civilization, rests largely on the shoulders of economists yet to be developed. Surely the situation today should stimulate the interest and give scope for the talents, courage, and pertinacity of the most capable

⁶ John Dewey and Arthur F. Bentley, *Knowing and the Known*, Boston: Beacon Press, 1949.

youth of our times. If ever young men had an invitation to great achievements, they have it today, and will have it for years to come, in that Herculean task, the reconstruction of economics.

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