The Pretence of Knowledge

Nobel Memorial Lecture, December 11, 1974

By FRIEDRICH AUGUST VON HAYEK*

The particular occasion of this lecture, combined with the chief practical problem which economists have to face today, have made the choice of its topic almost inevitable. On the one hand the still recent establishment of the Nobel Memorial Prize in Economic Science marks a significant step in the process by which, in the opinion of the general public, economics has been conceded some of the dignity and prestige of the physical sciences. On the other hand, the economists are at this moment called upon to say how to extricate the free world from the serious threat of accelerating inflation which, it must be admitted, has been brought about by policies which the majority of economists recommended and even urged governments to pursue. We have indeed at the moment little cause for pride: as a profession we have made a mess of things.

It seems to me that this failure of the economists to guide policy more successfully is closely connected with their propensity to imitate as closely as possible the procedures of the brilliantly successful physical sciences—an attempt which in our field may lead to outright error. It is an approach which has come to be described as the "scientistic" attitude—an attitude which, as I defined it some thirty years ago, "is decidedly unscientific in the true sense of the word, since it involves a mechanical and uncritical application of habits of thought to fields different from those in which they have been formed."[1] I want today to begin by explaining how some of the gravest errors of recent economic policy are a direct consequence of this scientistic error.

The theory which has been guiding monetary and financial policy during the last thirty years, and which I contend is largely the product of such a mistaken conception of the proper scientific procedure, consists in the assertion that there exists a simple positive correlation between total employment and the size of the aggregate demand for goods and services; it leads to the belief that we can permanently assure full employment by maintaining total money expenditure at an appropriate level. Among the various theories advanced to account for extensive unemployment, this is probably the only one in support of which strong quantitative evidence can be adduced. I nevertheless regard it as fundamentally false, and to act upon it, as we now experience, as very harmful.

This brings me to the crucial issue. Unlike the position that exists in the physical sciences, in economics and other disciplines that deal with essentially complex phenomena, the aspects of the events to be accounted for about which we can get quantitative data are necessarily limited and may not include the important ones. While in the physical sciences it is generally assumed, probably with good reason, that any important factor which determines the observed events will itself be directly observable and measurable, in the study of such complex phenomena as the market, which depend on the actions of many individuals, all the circumstances which will determine the outcome of a process, for reasons which I shall explain later, will hardly ever be fully known or measurable. And while in the physical sciences the investigator will be able to measure what, on the basis of a *prima facie* theory, he thinks important, in the social sciences often that is treated as important which happens to be accessible to measurement. This is sometimes carried to the point where it is demanded that our theories must be formulated in such terms that they refer only to measurable magnitudes.

It can hardly be denied that such a demand quite arbitrarily limits the facts which are to be admitted as possible causes of the events which occur in the real world. This view, which is often quite naively accepted as required by scientific procedure, has some rather paradoxical consequences. We know, of course, with regard to the market and similar social structures, a great many facts which we cannot measure and on which indeed we have only some very imprecise and general information. And because the effects of these facts in any particular instance cannot be confirmed by quantitative evidence, they are simply disregarded by those sworn to admit only what they regard as scientific evidence: they thereupon happily proceed on the fiction that the factors which they can measure are the only ones that are relevant.

The correlation between aggregate demand and total employment, for instance, may only be approximate, but as it is the *only* one on which we have quantitative data, it is accepted as the only causal connection that counts. On this standard there may thus well exist better "scientific" evidence for a false theory, which will be accepted because it is more "scientific", than for a valid explanation, which is rejected because there is no sufficient quantitative evidence for it.

Let me illustrate this by a brief sketch of what I regard as the chief actual cause of extensive unemployment —an account which will also explain why such unemployment cannot be lastingly cured by the inflationary policies recommended by the now fashionable theory. This correct explanation appears to me to be the existence of discrepancies between the distribution of demand among the different goods and services and the allocation of labour and other resources among the production of those outputs. We possess a fairly good "qualitative" knowledge of the forces by which a correspondence between demand and supply in the differ-

*Hayek received the 1974 Nobel Memorial Prize in Economic Science.

[1] "Scientism and the Study of Society", *Economica*, vol. IX, no. 35, August 1942, reprinted in *The Counter-Revolution of Science*, Glencoe, Ill., 1952, p. 15 of this reprint.

ent sectors of the economic system is brought about, of the conditions under which it will be achieved, and of the factors likely to prevent such an adjustment. The separate steps in the account of this process rely on facts of everyday experience, and few who take the trouble to follow the argument will question the validity of the factual assumptions, or the logical correctness of the conclusions drawn from them. We have indeed good reason to believe that unemployment indicates that the structure of relative prices and wages has been distorted (usually by monopolistic or governmental price fixing), and that to restore equality between the demand and the supply of labour in all sectors changes of relative prices and some transfers of labour will be necessary.

But when we are asked for quantitative evidence for the particular structure of prices and wages that would be required in order to assure a smooth continuous sale of the products and services offered, we must admit that we have no such information. We know, in other words, the general conditions in which what we call, somewhat misleadingly, an equilibrium will establish itself: but we never know what the particular prices or wages are which would exist if the market were to bring about such an equilibrium. We can merely say what the conditions are in which we can expect the market to establish prices and wages at which demand will equal supply. But we can never produce statistical information which would show how much the prevailing prices and wages *deviate* from those which would secure a continuous sale of the current supply of labour. Though this account of the causes of unemployment is an empirical theory, in the sense that it might be proved false, e.g. if, with a constant money supply, a general increase of wages did not lead to unemployment, it is certainly not the kind of theory which we could use to obtain specific numerical predictions concerning the rates of wages, or the distribution of labour, to be expected.

Why should we, however, in economics, have to plead ignorance of the sort of facts on which, in the case of a physical theory, a scientist would certainly be expected to give precise information? It is probably not surprising that those impressed by the example of the physical sciences should find this position very unsatisfactory and should insist on the standards of proof which they find there. The reason for this state of affairs is the fact, to which I have already briefly referred, that the social sciences, like much of biology but unlike most fields of the physical sciences, have to deal with structures of *essential* complexity, i.e. with structures whose characteristic properties can be exhibited only by models made up of relatively large numbers of variables. Competition, for instance, is a process which will produce certain results only if it proceeds among a fairly large number of acting persons.

In some fields, particularly where problems of a similar kind arise in the physical sciences, the difficulties can be overcome by using, instead of specific information about the individual elements, data about the relative frequency, or the probability, of the occurrence of the various distinctive properties of the elements. But this is true only where we have to deal with what has been called by Dr. Warren Weaver (formerly of the Rockefeller Foundation), with a distinction which ought to be much more widely understood, "phenomena of unorganized complexity," in contrast to those "phenomena of organized complexity" with which we have to deal in the social sciences.[2] Organized complexity here means that the character of the structures showing it depends not only on the properties of the individual elements of which they are composed, and the relative frequency with which they occur, but also on the manner in which the individual elements are connected with each other. In the explanation of the working of such structures we can for this reason not replace the information about the individual elements by statistical information, but require full information about each element if from our theory we are to derive specific predictions about individual events. Without such specific information about the individual elements we shall be confined to what on another occasion I have called mere pattern predictions—predictions of some of the general attributes of the structures that will form themselves, but not containing specific statements about the individual elements of which the structures will be made up.[3]

This is particularly true of our theories accounting for the determination of the systems of relative prices and wages that will form themselves on a well-functioning market. Into the determination of these prices and wages there will enter the effects of particular information possessed by every one of the participants in the market process—a sum of facts which in their totality cannot be known to the scientific observer, or to any other single brain. It is indeed the source of the superiority of the market order, and the reason why, when it is not suppressed by the powers of government, it regularly displaces other types of order, that in the resulting allocation of resources more of the knowledge of particular facts will be utilized which exists only dispersed among uncounted persons, than any one person can possess. But because we, the observing scientists, can thus never know all the determinants of such an order, and in consequence also cannot know at which particular structure of prices and wages demand would everywhere equal supply, we also cannot measure the deviations from that order; nor can we statistically test our theory that it is the deviations from that "equilibrium" system of prices and wages which make it impossible to sell some of the products and services at the prices at which they are offered.

[2] Warren Weaver, "A Quarter Century in the Natural Sciences", *The Rockefeller Foundation Annual Report 1958*, chapter I, "Science and Complexity".

[3] See my essay "The Theory of Complex Phenomena" in *The Critical Approach to Science and Philosophy. Essays in Honor of K. R. Popper*, ed. M. Bunge, New York 1964, and reprinted (with additions) in my *Studies in Philosophy, Politics and Economics*, London and Chicago 1967.

Before I continue with my immediate concern, the effects of all this on the employment policies currently pursued, allow me to define more specifically the inherent limitations of our numerical knowledge which are so often overlooked. I want to do this to avoid giving the impression that I generally reject the mathematical method in economics. I regard it in fact as the great advantage of the mathematical technique that it allows us to describe, by means of algebraic equations, the general character of a pattern even where we are ignorant of the numerical values which will determine its particular manifestation. We could scarcely have achieved that comprehensive picture of the mutual interdependencies of the different events in a market without this algebraic technique. It has led to the illusion, however, that we can use this technique for the determination and prediction of the numerical values of those magnitudes; and this has led to a vain search for quantitative or numerical constants. This happened in spite of the fact that the modern founders of mathematical economics had no such illusions. It is true that their systems of equations describing the pattern of a market equilibrium are so framed that *if* we were able to fill in all the blanks of the abstract formulae, i.e. *if* we knew all the parameters of these equations, we could calculate the prices and quantities of all commodities and services sold. But, as Vilfredo Pareto, one of the founders of this theory, clearly stated, its purpose cannot be "to arrive at a numerical calculation of prices", because, as he said, it would be "absurd" to assume that we could ascertain all the data.[4] Indeed, the chief point was already seen by those remarkable anticipators of modern economics, the Spanish schoolmen of the sixteenth century, who emphasized that what they called *pretium mathematicum,* the mathematical price, depended on so many particular circumstances that it could never be known to man but was known only to God.[5] I sometimes wish that our mathematical economists would take this to heart. I must confess that I still doubt whether their search for measurable magnitudes has made significant contributions to our *theoretical* understanding of economic phenomena—as distinct from their value as a description of particular situations. Nor am I prepared to accept the excuse that this branch of research is still very young: Sir William Petty, the founder of econometrics, was after all a somewhat senior colleague of Sir Isaac Newton in the Royal Society!

There may be few instances in which the superstition that only measurable magnitudes can be important has done positive harm in the economic field: but the present inflation and employment problems are a very serious one. Its effect has been that what is probably the true cause of extensive unemployment has been disregarded by the scientistically minded majority of economists, because its operation could not be confirmed by directly observable relations between measurable magnitudes, and that an almost exclusive concentration on quantitatively measurable surface phenomena has produced a policy which has made matters worse.

It has, of course, to be readily admitted that the kind of theory which I regard as the true explanation of unemployment is a theory of somewhat limited content because it allows us to make only very general predictions of the *kind* of events which we must expect in a given situation. But the effects on policy of the more ambitious constructions have not been very fortunate and I confess that I prefer true but imperfect knowledge, even if it leaves much indetermined and unpredictable, to a pretence of exact knowledge that is likely to be false. The credit which the apparent conformity with recognized scientific standards can gain for seemingly simple but false theories may, as the present instance shows, have grave consequences.

In fact, in the case discussed, the very measures which the dominant "macro-economic" theory has recommended as a remedy for unemployment, namely the increase of aggregate demand, have become a cause of a very extensive misallocation of resources which is likely to make later large-scale unemployment inevitable. The continuous injection of additional amounts of money at points of the economic system where it creates a temporary demand which must cease when the increase of the quantity of money stops or slows down, together with the expectation of a continuing rise of prices, draws labour and other resources into employments which can last only so long as the increase of the quantity of money continues at the same rate—or perhaps even only so long as it continues to accelerate at a given rate. What this policy has produced is not so much a level of employment that could not have been brought about in other ways, as a distribution of employment which cannot be indefinitely maintained and which after some time can be maintained only by a rate of inflation which would rapidly lead to a disorganisation of all economic activity. The fact is that by a mistaken theoretical view we have been led into a precarious position in which we cannot prevent substantial unemployment from re-appearing; not because, as this view is sometimes misrepresented, this unemployment is deliberately brought about as a means to combat inflation, but because it is now bound to occur as a deeply regrettable but inescapable consequence of the mistaken policies of the past as soon as inflation ceases to accelerate.

I must, however, now leave these problems of immediate practical importance which I have introduced chiefly as an illustration of the momentous consequences that may follow from errors concerning abstract problems of the philosophy of science. There is as much reason to be apprehensive about the long run dangers created in a much wider field by the uncritical acceptance of assertions which have the *appearance* of being scientific as there is with regard to the prob-

[4] V. Pareto, *Manuel d'economie politique,* 2nd. ed., Paris 1927, pp. 223—4.

[5] See, e.g., Luis Molina, *De iustitia et iure,* Cologne 1596—1600, tom. II, disp. 347, no. 3, and particularly Johannes de Lugo, *Disputationum de iustitia et iure tomus secundus,* Lyon 1642, disp. 26, sect. 4, no. 40.

lems I have just discussed. What I mainly wanted to bring out by the topical illustration is that certainly in my field, but I believe also generally in the sciences of man, what looks superficially like the most scientific procedure is often the most unscientific, and, beyond this, that in these fields there are definite limits to what we can expect science to achieve. This means that to entrust to science—or to deliberate control according to scientific principles—more than scientific method can achieve may have deplorable effects. The progress of the natural sciences in modern times has of course so much exceeded all expectations that any suggestion that there may be some limits to it is bound to arouse suspicion. Especially all those will resist such an insight who have hoped that our increasing power of prediction and control, generally regarded as the characteristic result of scientific advance, applied to the processes of society, would soon enable us to mould society entirely to our liking. It is indeed true that, in contrast to the exhilaration which the discoveries of the physical sciences tend to produce, the insights which we gain from the study of society more often have a dampening effect on our aspirations; and it is perhaps not surprising that the more impetuous younger members of our profession are not always prepared to accept this. Yet the confidence in the unlimited power of science is only too often based on a false belief that the scientific method consists in the application of a ready-made technique, or in imitating the form rather than the substance of scientific procedure, as if one needed only to follow some cooking recipes to solve all social problems. It sometimes almost seems as if the techniques of science were more easily learnt than the thinking that shows us what the problems are and how to approach them.

The conflict between what in its present mood the public expects science to achieve in satisfaction of popular hopes and what is really in its power is a serious matter because, even if the true scientists should all recognize the limitations of what they can do in the field of human affairs, so long as the public expects more there will always be some who will pretend, and perhaps honestly believe, that they can do more to meet popular demands than is really in their power. It is often difficult enough for the expert, and certainly in many instances impossible for the layman, to distinguish between legitimate and illegitimate claims advanced in the name of science. The enormous publicity recently given by the media to a report pronouncing in the name of science on *The Limits to Growth,* and the silence of the same media about the devastating criticism this report has received from the competent experts[6] must make one feel somewhat apprehensive about the use to which the prestige of science can be put. But it is by no means only in the field of economics that far-reaching claims are made on behalf of a more scientific direction of all human activities and the desirability of replacing spontaneous processes by "conscious human control". If I am not mistaken, psychology, psychiatry and some branches of sociology, not to speak about the so-called philosophy of history, are even more affected by what I have called the scientistic prejudice, and by specious claims of what science can achieve.[7]

If we are to safeguard the reputation of science, and to prevent the arrogation of knowledge based on a superficial similarity of procedure with that of the physical sciences, much effort will have to be directed toward debunking such arrogations, some of which have by now become the vested interests of established university departments. We cannot be grateful enough to such modern philosophers of science as Sir Karl Popper for giving us a test by which we can distinguish between what we may accept as scientific and what not—a test which I am sure some doctrines now widely accepted as scientific would not pass. There are some special problems, however, in connection with those essentially complex phenomena of which social structures are so important an instance, which make me wish to restate in conclusion in more general terms the reasons why in these fields not only are there only absolute obstacles to the prediction of specific events, but why to act as if we possessed scientific knowledge enabling us to transcend them may itself become a serious obstacle to the advance of the human intellect.

The chief point we must remember is that the great and rapid advance of the physical sciences took place in fields where it proved that explanation and prediction could be based on laws which accounted for the observed phenomena as functions of comparatively few variables—either particular facts or relative frequencies of events. This may even be the ultimate reason why we single out these realms as "physical" in contrast to those more highly organized structures which I have here called essentially complex phenomena. There is no reason why the position must be the same in the latter as in the former fields. The difficulties which we encounter in the latter are not, as one might at first suspect, difficulties about formulating theories for the explanation of the observed events—although they cause also special difficulties about testing proposed explanations and therefore about eliminating bad theories. They are due to the chief problem which arises when we apply our theories to any particular situation in the real world. A theory of essentially complex phenomena must refer to a large number of particular

[6] See *The Limits to Growth: A Report of the Club of Rome's Project on the Predicament of Mankind,* New York 1972; for a systematic examination of this by a competent economist cf. Wilfred Beckerman, *In Defence of Economic Growth,* London 1974, and, for a list of earlier criticisms by experts, Gottfried Haberler, *Economic Growth and Stability,* Los Angeles 1974, who rightly calls their effect "devastating".

[7] I have given some illustrations of these tendencies in other fields in my inaugural lecture as Visiting Professor at the University of Salzburg, *Die Irrtumer des Konstruktivismus und die Grundlagen legitimer Kritik gesellschaftlicher Gebilde,* Munich 1970, now re-issued for the Walter Eucken Institute, at Freiburg i.Brg. by J. C. B. Mohr, Tubingen 1975.

facts; and to derive a prediction from it, or to test it, we have to ascertain all these particular facts. Once we succeeded in this there should be no particular difficulty about deriving testable predictions—with the help of modern computers it should be easy enough to insert these data into the appropriate blanks of the theoretical formulae and to derive a prediction. The real difficulty, to the solution of which science has little to contribute, and which is sometimes indeed insoluble, consists in the ascertainment of the particular facts.

A simple example will show the nature of this difficulty. Consider some ball game played by a few people of approximately equal skill. If we knew a few particular facts in addition to our general knowledge of the ability of the individual players, such as their state of attention, their perceptions and the state of their hearts, lungs, muscles etc. at each moment of the game, we could probably predict the outcome. Indeed, if we were familiar both with the game and the teams we should probably have a fairly shrewd idea on what the outcome will depend. But we shall of course not be able to ascertain those facts and in consequence the result of the game will be outside the range of the scientifically predictable, however, well we may know what effects particular events would have on the result of the game. This does not mean that we can make no predictions at all about the course of such a game. If we know the rules of the different games we shall, in watching one, very soon know which game is being played and what kinds of actions we can expect and what kind not. But our capacity to predict will be confined to such general characteristics of the events to be expected and not include the capacity of predicting particular individual events.

This corresponds to what I have called earlier the mere pattern predictions to which we are increasingly confined as we penetrate from the realm in which relatively simple laws prevail into the range of phenomena where organized complexity rules. As we advance we find more and more frequently that we can in fact ascertain only some but not all the particular circumstances which determine the outcome of a given process; and in consequence we are able to predict only some but not all the properties of the result we have to expect. Often all that we shall be able to predict will be some abstract characteristic of the pattern that will appear—relations between kinds of elements about which individually we know very little. Yet, as I am anxious to repeat, we will still achieve predictions which can be falsified and which therefore are of empirical significance.

Of course, compared with the precise predictions we have learnt to expect in the physical sciences, this sort of mere pattern predictions is a second best with which one does not like to have to be content. Yet the danger of which I want to warn is precisely the belief that in order to have a claim to be accepted as scientific it is necessary to achieve more. This way lies charlatanism and worse. To act on the belief that we possess the knowledge and the power which enable us to shape the processes of society entirely to our liking, knowledge which in fact we do *not* possess, is likely to make us do much harm. In the physical sciences there may be little objection to trying to do the impossible; one might even feel that one ought not to discourage the over-confident because their experiments may after all produce some new insights. But in the social field the erroneous belief that the exercise of some power would have beneficial consequences is likely to lead to a new power to coerce other men being confered on some authority. Even if such power is not in itself bad, its exercise is likely to impede the functioning of those spontaneous ordering forces by which, without understanding them, man is in fact so largely assisted in the pursuit of his aims. We are only beginning to understand on how subtle a communication system the functioning of an advanced industrial society is based—a communications system which we call the market and which turns out to be a more efficient mechanism for digesting dispersed information than any that man has deliberately designed.

If man is not to do more harm than good in his efforts to improve the social order, he will have to learn that in this, as in all other fields where essential complexity of an organized kind prevails, he cannot acquire the full knowledge which would make mastery of the events possible. He will therefore have to use what knowledge he can achieve, not to shape the results as the craftsman shapes his handiwork, but rather to cultivate a growth by providing the appropriate environment, in the manner in which the gardener does this for his plants. There is danger in the exuberant feeling of ever growing power which the advance of the physical sciences has engendered and which tempts man to try, "dizzy with success", to use a characteristic phrase of early communism, to subject not only our natural but also our human environment to the control of a human will. The recognition of the insuperable limits to his knowledge ought indeed to teach the student of society a lesson of humility which should guard him against becoming an accomplice in men's fatal striving to control society—a striving which makes him not only a tyrant over his fellows, but which may well make him the destroyer of a civilization which no brain has designed but which has grown from the free efforts of millions of individuals.