

COPING WITH IGNORANCE

by F. A. Hayek

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It is to me not only a great honor but also the discharge of an intellectual duty and a real pleasure to be allowed to deliver a Ludwig von Mises memorial lecture. There is no single man to whom I owe more intellectually, even though he was never my teacher in the institutional sense of the word.

I came originally from the other of the two original branches of the Austrian school. While Mises had been an inspired pupil of Eugen Boehm von Bawerk, who died comparatively early and whom I knew only as a friend of my grandfather before I knew what the word "economics" meant, I was personally a pupil of his contemporary, friend and brother-in-law, Friedrich von Wieser. I was attracted by him, I admit, because unlike most of the other members of the Austrian school, he had a good deal of sympathy with a mild Fabian socialism to which I was inclined as a young man. He in fact prided himself that his theory of marginal utility had provided the basis of progressive taxation, which then seemed to me one of the ideals of social justice.

It was he who, just retiring as I graduated, sent me with a letter of introduction to Ludwig von Mises, who as one of the directors of a new temporary government office concerned with settling certain problems arising out of the treaty of St. Germain, was looking for young lawyers with some understanding of economics and knowledge of foreign languages. I remember vividly how, almost

exactly fifty-six years ago, after presenting to Mises my letter of introduction by Wieser, in which I was described as a promising young economist, Mises said, "Well, I've never seen you at my lectures."

That was almost completely true. I had looked in at one of his lectures and found that a man so conspicuously anti-pathetic to the kind of Fabian views which I then held was not the sort of person to whom I wanted to go. But of course things changed.

The meeting was the beginning. After a short conversation, Mises asked, "When can you start work?" This led to a long, close collaboration. First, for five years, he was my official chief in that government office and then vice president of an institute of business cycle research which we had created together. During these ten years he certainly had more influence on my outlook of economics than any other man.

It was essentially his second great work, *Die Gemein Wirtschaft* of 1922, which appeared in English translation only fifteen years later as *Socialism*, that completely won me over to his views. And then in his *Privatseminar*, as we called the little discussion group which met at his office, I became gradually intimately familiar with his thinking.

I do not wish however to claim to be an authoritative interpreter of Mises' views. Although I do owe him a

decisive stimulus at a crucial point of my intellectual development, and continuous inspiration through a decade, I have perhaps most profited from his teaching because I was not initially his student at the university, an innocent young man who took his word for gospel, but came to him as a trained economist, trained in a parallel branch of Austrian economics from which he gradually, but never completely, won me over. Though I learned that he usually was right in his conclusions, I wasn't always satisfied by his arguments, and retained to the end a certain critical attitude which sometimes forced me to build different constructions, which however, to my great pleasure, usually led to the same conclusions. I am to the present moment pursuing the questions which he made

Even two hundred years after Adam Smith's *Wealth of Nations*, it is not yet fully understood that it is the great achievement of the market to have made a far-ranging division of labor possible, that it brings about a continuous adaptation of economic effect to millions of particular facts or events which in their totality are not known and cannot be known to anybody. A real understanding of the process which brings this about was long blocked by post-Smithian classical economics which adopted a labor or cost theory of value.

The misconception that costs determined prices prevented economists for a long time from recognizing that it was prices which operated as the indispensable signals



me see, and that, I believe is the greatest benefit one scientist can confer on one of the next generation.

I do not know whether my making our incurable ignorance of most of the particular circumstances which determine the course of this great society the central point of the scientific approach would have Mises' approval. It is probably a development that goes somewhat beyond his views, because Mises himself was still much more a child of the rationalist age of enlightenment and of continental rather than of English liberalism, in the European sense of the word, than I am myself.

But I do flatter myself that he sympathized with my departure in this direction, which I like to describe briefly as a movement back from Voltaire to Montesquieu. It is the outcome of this development about which I am now going to speak.

I've come to believe that both the aim of the market order, and therefore the object of explanation of the theory of it, is to cope with the inevitable ignorance of everybody of most of the particular facts which determine this order. By a process which men did not understand, their activities have produced an order much more extensive and comprehensive than anything they could have comprehended, but on the functioning of which we have become utterly dependent.

telling producers what costs it was worth expending on the production of the various commodities and services, and not the other way around. It was the costs which they had expended which determined the prices of things produced.

It was this crucial insight which finally broke through and established itself about a hundred years ago through the so-called marginal revolution in economics.

The chief insight gained by modern economists is that the market is essentially an ordering mechanism, growing up without anybody wholly understanding it, that enables us to utilize widely dispersed information about the significance of circumstances of which we are mostly ignorant. However, the various planners (and not only the planners in the socialist camp) and dirigists have still not yet grasped this.

I do not believe that it is merely present ignorance, which we expect future advance of knowledge will remove, which makes a rational effort at central planning wholly impossible. I believe such a central utilization of necessarily widely dispersed knowledge of particular and temporary circumstances must forever remain impossible. We can have a far-ranging division of labor only by relying on the impersonal signals of prices.

That here and now we economists do not know enough to be justified to undertake such a task as the planning of the whole economic system seems to me so obvious that I find it increasingly difficult to treat the contrary belief with any respect.

It is a basic fact that we as scientists have to explain the results of the actions of men, which produces a sort of order by following signals inducing them to adapt to facts which they do not know. It creates a comparable or similar problem of coping with ignorance such as the people in economics world encounter even more than the people who undertake to explain this process.

It is a difficulty which all attempts at a theoretical explanation of the market process face, though it appears that not many economists have been clearly aware of the source of the difficulties which they encounter.

If the chief problem of economic decisions is one of coping with the inevitable ignorance, the task of a science of economics trying to explain the joint effects of hundreds of thousands of such decisions on men in many different positions has to deal with an ignorance as it were, of a second order of magnitude because the explaining economist does not even know what all the acting people know; he has to provide an explanation without knowing the determining facts, not even knowing what the individual members in the economic system know about these facts.

We are in this respect not in the happy position in which the theorists of a relatively simple phenomena find themselves. When they have formed a hypothesis about how two or three variables are interrelated, they can test such a hypothesis by inserting into their abstract formula, observing values replacing the blanks, and then see whether the conclusions are correct.

Our problem is that even if we have thought out a beautiful and possibly correct theory of the complex phenomena with which we have to deal, we can never ascertain all the concrete specific data of a particular position, simply because we do not know all that which the acting people know. But it is the joint results of those actions which we want to predict.

If the market really achieves a utilization of more information than any participant in this market process possesses, the outcome must depend on more particular facts than the scientific observer can insert into his tentative hypothesis which is intended to explain the whole process.

There are two possible ways in which economists have endeavored at least partly to overcome this difficulty.

The first, represented by what today we call microeconomics, resignedly accepts the fact that because of this difficulty we can never achieve a *full* explanation, or an exact prediction of the particular outcome of a given situation, but must instead be content with what I have

occasionally called a "pattern prediction" or, earlier a "prediction of the principle." All we can achieve is to say what kinds of things will not happen and what sort of pattern the resulting situation will show, without being able to predict a particular outcome.

This kind of microeconomics attempts, by the construction of simplified models in which all the kinds of attitudes and circumstances we meet in the real life are represented, to simulate the kind of movements and changes which we observe in the real world.

Such a theory can tell us what sort of changes we can expect in the real world, the general character of which our model indicates, which reduces (not so much in scale as in the number of distinct elements), the facts with which we have to deal, to make its workings still comprehensible or surveyable.

I still believe that this is the only approach which is entitled to regard itself as scientific. Being scientific involves in this connection a frank admission of how limited our powers of prediction really are. It still does lead to some falsifiable predictions, namely what sorts of events are possible in a given situation and which are not.

It is, in this sense, an empirical theory even though it consists largely, but not entirely, of propositions which are self-evident once they are stated. Indeed, I doubt whether microeconomic theory has ever discovered any new facts. Decreasing returns, decreasing marginal productivity or marginal utility, decreasing marginal rates of substitution were of course all phenomena familiar to ordinary people even if these did not call them by that name. In fact, it is only because ordinary people knew these facts, long before economists discovered their importance, that they have always been among the determinants of how the market actually functions. What the economic theorists found out was merely the relevance of these particular facts for the decision of individuals in their interactions with other persons.

It is the obscuring of the empirical fact of people learning what others do by a process of communication of knowledge which has always made me reluctant to accept von Mises' claim of an *a priori* character of the whole of economic theory, although I agree with him that much of it consists merely in working out the logical implications of certain initial facts.

I recognize with him microeconomic theory as the only legitimate economic theory because, and in so far as, it recognizes the inevitable limitation of our possible knowledge of the objective facts which determine any given situation; and we need claim no more that we are entitled to claim.

I will not deny that we find also in the microeconomic literature a good deal of indefensible pretense of a great deal more.

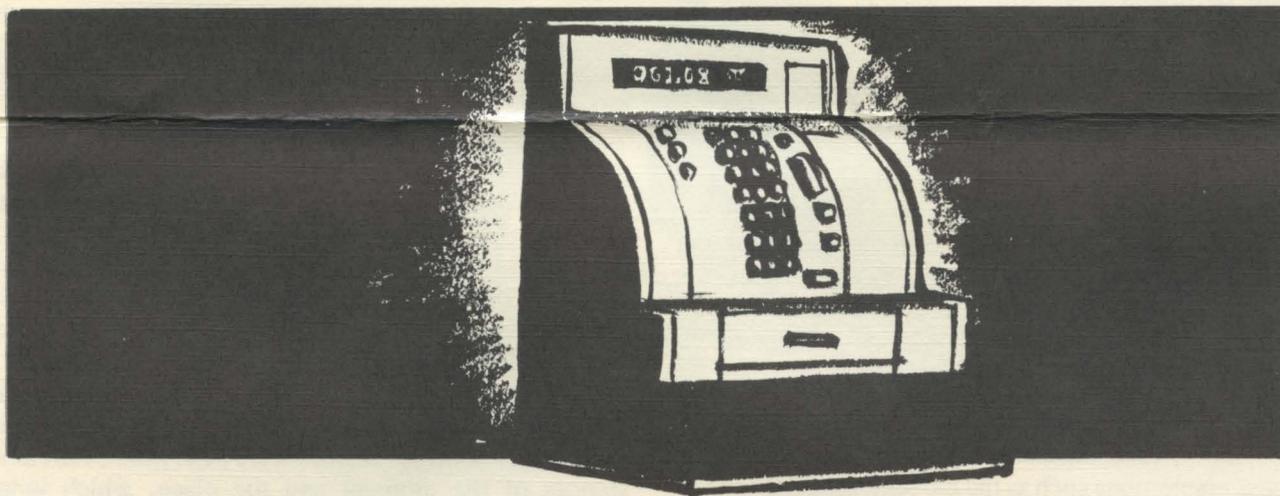
There is, of course, in the first instance, the frequent

abuse of the convenient conception of "equilibrium" toward which the market process is said to tend. I will not say that there are not forces at work which can usefully be described as equilibrating tendencies.

But equilibrating forces are of course at work in any stream of a liquid and must be taken into account in any attempt to analyze the flow of such a stream. Such a stream in the physical sense of the word of course will never reach a state of equilibrium. And the same is true of the economic efforts of the production and use of goods and services where every part may all the time tend toward a partial or local equilibrium, but long before that is reached the circumstances to which the local efforts adapt themselves will have changed themselves as a result

the pre-eminent method for describing abstract patterns without assuming or possessing particular information about the specific magnitudes involved. One great mathematician has indeed described a mathematician as a maker of patterns. In this sense mathematics can be very helpful to us.

The second point which I want to make is that a particular reason which in the physical sciences make measurements of concrete magnitudes the hallmark of scientific procedure for a very definite reason do not apply to the explanation of human action. The true reason why the physical sciences must rely on measurements is that it has been recognized that things which appear alike to our senses frequently do not behave in the same



of similar processes. All we can claim for the achievement of microeconomic theory is that the signals which the prices constitute will always make the individuals change their plans in the direction made necessary by factual changes of which they have no direct knowledge—not that this process will ever lead to what some economists call an equilibrium.

Not content with this limited insight, which economics can in fact supply, economists ambitious to make it more precise have often spoiled microeconomics by a tendency, which we shall encounter in a more systematic form when I pass on to the second type of approach, macroeconomics. They tried to deal with our inescapable ignorance of the data required for a full explanation, the macroeconomic one, by trying measurements I shall discuss later.

I will at this stage make only two further comments on this. The first is that it is an erroneous belief, characteristic of bad mathematicians, that mathematics is essentially quantitative and that, therefore, to build on the great achievements of the founders of mathematical economics, men like Jevons, Walras and Pareto, one has to introduce quantitative data obtained by measurements. That was certainly not the intention of the founders of mathematical economics. They understood much better than their successors that algebraic mathematical formulae are

manner, and that sometimes things which appear alike to us behave very differently if examined.

The physicist, to arrive at valid theories, was often compelled to substitute for the classification of different objects which our senses provide to us a different classification which was based solely on the relations of objective things toward each other.

Now this is really what measurement amounts to: a classification of objects according to the manner in which they act on other objects. But to explain human action all that is relevant is how the things appear to human beings, to acting men. This depends on whether men regard two things as the same or different kinds of things, not what they really are, unknown to them. For our purposes the results of measurements (at least so far as these are not performed by the people whose actions we want to explain) are wholly uninteresting.

The belief derived from physics that measurement is an essential foundation of all sciences is very old. There was more than 300 years ago a German philosopher named Erhard Weigel who strove to construct a universal science which he proposed to call Pantometria, based as the name says on measuring everything. Much of economics, and if I may add in parenthesis much of contemporary psychology, has indeed become Pantometria in a sense in the

principle that if you don't know what measurements mean, measure anyhow because that is what science does. The social sciences building at the University of Chicago indeed still bears since it was built 40 years ago on its outside an inscription taken from the famous physicist Lord Kelvin: "When you cannot measure, your knowledge is meager and unsatisfactory." I will admit that that may be true, but it is certainly not scientific to insist on measurement where you don't know what your measurements mean. There are cases where measurements are not relevant. What has done much damage to microeconomics is striving for a pseudo-exactness by imitating methods of the physical sciences which have to deal with what are fundamentally much more simple phenomena. And the assumption that it is possible to ascertain all the relevant particular facts still completely dominates the alternative methods of dealing with our constitutional ignorance, which economists have tried to overcome. This of course, is what has come to be called macroeconomics as distinct from microeconomics.

The basic idea on which this approach proceeds is fairly simple and obvious. If we cannot know all the individual facts which determine individual action and thereby the economic process, we must start from the most comprehensive information which we can obtain about them, and that is the statistical figures about aggregates and averages.

Again, the model which is followed is provided by the physical sciences which, where they have to deal with true mass phenomena such as the movement of millions of molecules with which thermodynamics has to deal, where we admittedly know nothing about the movement of any individual molecule, the law of large numbers enables us to discover statistical regularities or probabilities which indeed, in this way, provide an adequate foundation for reliable predictions.

The trouble is, unfortunately, that in the disciplines which endeavor to explain the structure of society, we do not have to deal with true mass phenomena.

The events which we must take into account in any attempt to predict the outcome of particular social processes are never so numerous as to enable us to substitute ascertained probabilities for information about the individual events. As a distinguished thinker, the late Warren Weaver of the Rockefeller Foundation has pointed out, both in the biological and in the social sciences frequently we cannot rely on probabilities, or the law of large numbers, because unlike the positions which exist in the physical sciences, where statistical evidence of probabilities can be substituted for information on particular facts, we have to deal with what he calls organized complexity, where we cannot expect to find permanent constant relations between aggregates or averages.

Indeed, this intermediate field between the simple phenomena of the physical sciences, where everything

can be explained by theoretical formulae which contain no more than two or three unknowns, and the instances where a large enough number of events to be able to deal with true mass phenomena to rely on probability is our subject. In the social sciences we have to deal with something which Warren Weaver called organized complexity, phenomena which are not made up of sufficiently large numbers of similar events to enable us to ascertain the probabilities for their occurrence.

In order to provide a full explanation we would have to have information about every single event which you can never possibly obtain. But while micro-theorists have resigned themselves to the consequent limitations of our powers and admit that we must be content with what I've called mere pattern predictions, many of the more ambitious and impatient students of these problems refuse to recognize these limitations to our possible knowledge, and possible power of prediction, and therefore also of our possible power of control.

What drives people to the pursuit of statistical research is usually the hope of discovering in this way new facts of general and not merely historical importance. But this hope is inevitably disappointed. I certainly do not wish to underrate the importance of historical information about the particular situation. I doubt, however, whether the observation and measurement of true mass phenomena has significantly improved our understanding of the market process. What we can find by this procedure, as by all observation of particular circumstances, may possibly be special relations, determined by the particular circumstances of the moment and the place, which indeed, perhaps for some time may enable us to make correct predictions. But with general laws which help to explain how at different places the course of economic affairs is determined, these quantitative relations between measurable magnitudes have precious little to do. Indeed, even the very moderate hopes which I myself had at one time concerning the usefulness of such economic forecasts based on observed statistical regularities has mostly been disappointed. The concrete course of the process of adaptation to unknown circumstances cannot be predicted. All we can predict is certain abstract features of the process, not its concrete manifestations.

It is now frequently assumed that at least the theory of money, in the nature of that subject, must be macro-theory. I can see no reason whatever for this. The cause for this belief is apparently the fact that the value of money is usually conceived as corresponding to an average of prices. But that is no more true than it is of the value of any other commodity. I do not see for instance, that our habitual use of index numbers of prices, although undoubtedly very convenient for many purposes, has in any way assisted our understanding of the effect of monetary changes, or to draw relevant conclusions, except, perhaps about the behavior of index numbers.

The interesting problems are those of the effect of monetary changes on particular prices, and about these

index numbers or changes of general price levels, tell us nothing.

It seems to me more and more that the immense efforts which during the great popularity of macroeconomics over the last thirty or forty years have been devoted to it, were largely misspent, and that if we want to be useful in the future we shall have to be content to improve and

spread the admittedly limited insights which microeconomics conveys.

I believe it is only microeconomics which enables us to understand the crucial functions of the market process: that it enables us to make effective use of information about thousands of facts of which nobody can have full knowledge.

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