The Theory of Complex Phenomena: A Precocious Play on the Epistemology of Complexity

Friedrich A. von Hayek

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Friedrich Hayek's The Theory of Complex Phenomena (Hayek, 1964) is a precocious and far-sighted attempt to illuminate a topic that has loomed increasingly important in the 53 years since the printing of the paper: the epistemology of complexity. It posits a tension between real insight into complex phenomena and a narrow - but, common-interpretation of a Popperian falsificationism, and suggests that we make a 'hard choice' - one that subsequent research and philosophical forays into complexity science can be usefully understood as having implicitly made on numerous occasions.

Popper suggested actual falsifiability of hypotheses derived from a theory as a criterion of demarcation between scientific and 'metaphysical' or 'pseudo-scientific' theories and of choice among alternative empirical theories (Popper, 1959). Thus, the 'theory' which states that 'the orbit of planet Z has the form of a circle, i.e, x^sup 2^ y^sup 2^=R2' is to be preferred (before testing) to a theory which states that 'the orbit of planet Z has the form of an ellipse, i.e., a^sup 2^x2 b^sup 2^y^sup 2^=c^sup 2^ because it takes fewer points - or, measurements (namely, 3) to disconfirm the former theory than it takes (4) to disconfirm the latter. Popper capitalizes on the example to make the point that the (normative) emphasis placed by some philosophers of science on simplicity as a criterion for theory choice follows from (and is therefore not independent of) a commitment to actual falsifiability.

This is precisely the point that Hayek focuses on, in reference to 'complex phenomena'. He sees that the astute researcher of complexity faces a hard choice between the 'point-wise' testability that Popper takes for granted on the one hand, and insight or understanding on the other - and advocates that the constraints which testability places on theory choice be loosened in favor of allowing for more complicated nomological relationships among independent variables, which, in turn, will orient the empiricist away from attempting to predict point events and towards predicting 'patterns' or dynamical regimes. This is a precocious and far sighted insight, and here is a case in point: Apparently unbeknownst to Hayek (plausibly so: Hayek finished the paper in 1961, two years before the Lorenz paper that kicked off 'chaos theory' was published), chaos theory got off the ground approximately at the same time Hayek was writing his paper, starting from (Edward) Lorenz's startling realization that some dynamical systems (such as that made up by the coupling between the system of nonlinear ordinary differential equations he was trying to solve numerically and the finite-precision arithmetic operations that his computer instantiated) exhibit highly sensitive dependence of their long-run dynamics on their initial conditions, such that two points in the phase space of the system that start out arbitrarily close together will - in the course of the system's evolution and after an only finite amount of time - end up very far apart. 'Chaos theory', then, touches reality not by making predictions about point events, but, rather, by specifying dynamical systems and regimes or regions of their parameter spaces that exhibit 'transition to chaos' (Ott, 2002) - that is, by making predictions about patterns of behavior rather than about highly localized space-time hyper-volumes ('points') of behavior.

But, is the development of an empirical chaos theory really a validation of Hayek's claim that strict falsificationism must be relaxed in order to make progress on a science of complexity? Note, in this regard, that formal languages used for representing dynamical systems (such as those exhibiting chaotic behavior) come equipped with highly efficacious state space contraction devices and maneuvers, which collapse half-planes into lines and lines into points. It is, then, possible - by the application of such devices - to render predictions about macroscopic patterns and dynamical regimes of behavior into predictions about the 'rate of transition to chaos and boundaries between ordered and disordered behavior, which make possible precisely the kind of subsequent 'point- wise testing' that Popper is often interpreted as having had in mind. Doing so, however, will presuppose a flexibility on the part of the researcher at a level which most social scientists in general and economists trained in the neoclassical tradition in particular have to date had little consideration for: the ontological one - a flexibility about the objects in-terms-of-which discourse proceeds. This is something that Hayek glimpses and points the way to his paper, without fully calling out: 'statistics' - he argues - cannot be used to teach us much about a population of computers, unless we also have access to the code that runs on them (Hayek, 1961, ibid.). Knowledge of the code used to design the computers will not only give us a radically different number and set of hypotheses that 'statistics' can be used to test, but also, perhaps more importantly, a different conceptualization of the 'computer' in terms of 'intentional' terms ('algorithms') rather than in terms of causal entities ('electrons and holes'). And this may be a far more penetrating insight of Hayek's paper than is the exhortation to loosen the epistemological constraints that we place on 'complexity science' - and one which complexity researchers may do well to heed. Should they choose to do so, the black box of human decision making (mind [arrow right] brain [arrow right] behavior) - neatly bracketed in economic analyses by rational choice models and linear demand functions - could be made to yield much illuminating insight under the gaze of new conceptual toolkits.

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1. Pattern Precognition and Pattern Prediction

Man has been impelled to scientific inquiry by wonder and by need. Of these wonder has been incomparably more fertile. There are good reasons for this. Where we wonder we have already a question to ask. But however urgently we may want to find our way in what appears just chaotic, so long as we do not know what to look for, even the most attentive and persistent observation of the bare facts is not likely to make them more intelligible. Intimate acquaintance with the facts is certainly important ; but systematic observation can start only after problems have arisen. Until we have definite questions to ask we cannot employ our intellect; and questions presuppose that we have formed some provisional hypothesis or theory about the events.[1]

Questions will arise at first only after our senses have discerned some recurring pattern or order in the events. It is a re-cognition of some regularity (or recurring pattern, or order), of some similar feature in otherwise different circumstances, which makes us wonder and ask 'why?'[2] Our minds are so made that when we notice such regularity in diversity we suspect the presence of the same agent and become curious to detect it. It is to this trait of our minds that we owe whatever understanding and mastery of our environment we have achieved.

Many such regularities of nature are recognized 'intuitively' by our senses. We see and hear patterns as much as individual events without having to resort to intellectual operations. In many instances these patterns are of course so much part of the environment which we take for granted that they do not cause questions. But where our senses show us new patterns, this causes surprise and questioning. To such curiosity we owe the beginning of science.

Marvelous, however, as the intuitive capacity of our senses for pattern recognition is, it is still limited.[3] Only certain kinds of regular arrangements (not necessarily the simplest) obtrude themselves on our senses. Many of the patterns of nature we can discover only after they have been constructed by our mind. The systematic construction of such new patterns is the business of mathematics.[4] The role which geometry plays in this respect with regard to some visual patterns is merely the most familiar instance of this. The great strength of mathematics is that it enables us to describe abstract patterns which cannot be perceived by our senses, and to state the common properties of hierarchies or classes of patterns of a highly abstract character. Every algebraic equation or set of such equations defines in this sense a class of patterns, with the individual manifestation of this kind of pattern being particularized as we substitute definite values for the variables.

It is probably the capacity of our senses spontaneously to recognize certain kinds of patterns that has led to the erroneous belief that if we look only long enough, or at a sufficient number of instances of natural events, a pattern will always reveal itself. That this often is so means merely that in those cases the theorizing has been done already by our senses. Where, however, we have to deal with patterns for the development of which there has been no biological reason, we shall first have to invent the pattern before we can discover its presence in the phenomena-or before we shall be able to test its applicability to what we observe. A theory will always define only a kind (or class) of patterns, and the particular manifestation of the pattern to be expected will depend on the particular circumstances (the 'initial and marginal conditions' to which, for the purposes of this article, we shall refer as 'data'). How much in fact we shall be able to predict will depend on how many of those data we can ascertain.

The description of the pattern which the theory provides is commonly regarded merely as a tool which will enable us to predict the particular manifestations of the pattern that will appear in specific circumstances. But the prediction that in certain general conditions a pattern of a certain kind will appear is also a significant (and falsifiable) prediction. If I tell somebody that if he goes to my study he will find there a rug with a pattern made up of diamonds and meanders, he will have no difficulty in deciding 'whether that prediction was verified or falsified by the result',[5] even though I have said nothing about the arrangement, size, colour, etc., of the elements from which the pattern of the rug is formed.

The distinction between a prediction of the appearance of a pattern of a certain class and a prediction of the appearance of a particular instance of this class is sometimes important even in the physical sciences. The mineralogist who states that the crystals of a certain mineral are hexagonal, or the astronomer who assumes that the course of a celestial body in the field of gravity of another will correspond to one of the conic sections, make significant predictions which can be refuted. But in general the physical sciences tend to assume that it will in principle always be possible to specify their predictions to any degree desired.6 The distinction assumes, however, much greater importance when we turn from the relatively simple phenomena with which the natural sciences deal, to the more complex phenomena of life, of mind, and of society, where such specifications may not always be possible.[7]

2. Degrees of Complexity

The distinction between simplicity and complexity raises considerable philosophical difficulties when applied to statements. But there seems to exist a fairly easy and adequate way to measure the degree of complexity of different kinds of abstract patterns. The minimum number of elements of which an instance of the pattern must consist in order to exhibit all the characteristic attributes of the class of patterns in question appears to provide an unambiguous criterion.

It has occasionally been questioned whether the phenomena of life, of mind, and of society are really more complex than those of the physical world.[8] This seems to be largely due to a confusion between the degree of complexity characteristic of a peculiar kind of phenomenon and the degree of complexity to which, by a combination of elements, any kind of phenomenon can be built up. Of course, in this manner physical phenomena may achieve any degree of complexity. Yet when we consider the question from the angle of the minimum number of distinct variables a formula or model must possess in order to reproduce the characteristic patterns of structures of different fields (or to exhibit the general laws which these structures obey), the increasing complexity as we proceed from the inanimate to the ('more highly organized') animate and social phenomena becomes fairly obvious.

It is, indeed, surprising how simple in these terms, i.e., in terms of the number of distinct variables, appear all the laws of physics, and particularly of mechanics, when we look through a collection of formulae expressing them.[9] On the other hand, even such relatively simple constituents of biological phenomena as feedback (or cybernetic) systems, in which a certain combination of physical structures produces an overall structure possessing distinct characteristic properties, require for their description something much more elaborate than anything describing the general laws of mechanics. In fact, when we ask ourselves by what criteria we single out certain phenomena as 'mechanical' or 'physical', we shall probably find that these laws are simple in the sense defined. Non-physical phenomena are more complex because we call physical what can be described by relatively simple formulae.

The 'emergence' of 'new' patterns as a result of the increase in the number of elements between which simple relations exist, means that this larger structure as a whole will possess certain general or abstract features which will recur independently of the particular values of the individual data, so long as the general structure (as described, e.g., by an algebraic equation) is preserved.[10] Such 'wholes', defined in terms of certain general properties of their structure, will constitute distinctive objects of explanation for a theory, even though such a theory may be merely a particular way of fitting together statements about the relations between the individual elements.

It is somewhat misleading to approach this task mainly from the angle of whether such structures are 'open' or 'closed' systems. There are, strictly speaking, no closed systems within the universe. All we can ask is whether in the particular instance the points of contact through which the rest of the universe acts upon the system we try to single out (and which for the theory become the data) are few or many. These data, or variables, which determine the particular form which the pattern described by the theory will assume in the given circumstances, will be more numerous in the case of complex wholes and much more difficult to ascertain and control than in the case of simple phenomena.

What we single out as wholes, or where we draw the 'partition boundary',[11] will be determined by the consideration whether we can thus isolate recurrent patterns of coherent structures of a distinct kind which we do in fact encounter in the world in which we live. Many complex patterns which are conceivable and might recur we shall not find it worthwhile to construct. Whether it will be useful to elaborate and study a pattern of a particular kind will depend on whether the structure it describes is persistent or merely accidental. The coherent structures in which we are mainly interested are those in which a complex pattern has produced properties which make self-maintaining the structure showing it.

3. Pattern Prediction with Incomplete Data

The multiplicity of even the minimum of distinct elements required to produce (and therefore also of the minimum number of data required to explain) a complex phenomenon of a certain kind creates problems which dominate the disciplines concerned with such phenomena and gives them an appearance very different from that of those concerned with simpler phenomena. The chief difficulty in the former becomes one of in fact ascertaining all the data determining a particular manifestation of the phenomenon in question, a difficulty which is often insurmountable in practice and sometimes even an absolute one.[12] Those mainly concerned with simple phenomena are often inclined to think that where this is the case a theory is useless and that scientific procedure demands that we should find a theory of sufficient simplicity to enable us to derive from it predictions of particular events. To them the theory, the knowledge of the pattern, is merely a tool whose usefulness depends entirely on our capacity to translate it into a representation of the circumstances producing a particular event. Of the theories of simple phenomena this is largely true.[13]

There is, however, no justification for the belief that it must always be possible to discover such simple regularities and that physics is more advanced because it has succeeded in doing this while other sciences have not yet done so. It is rather the other way round : physics has succeeded because it deals with phenomena which, in our sense, are simple. But a simple theory of phenomena which are in their nature complex (or one which, if that expression be preferred, has to deal with more highly organized phenomena) is probably merely of necessity false-at least without a specified *ceteris paribus* assumption, after the full statement of which the theory would no longer be simple.

We are, however, interested not only in individual events, and it is also not only predictions of individual events which can be empirically tested. We are equally interested in the recurrence of abstract patterns as such; and the prediction that a pattern of a certain kind will appear in defined circumstances is a falsifiable (and therefore empirical) statement. Knowledge of the conditions in which a pattern of a certain kind will appear, and of what depends on its preservation, may be of great practical importance. The circumstances or conditions in which the pattern described by the theory will appear are defined by the range of values which may be inserted for the variables of the formula. All we need to know in order to make such a theory applicable to a situation is, therefore, that the data possess certain general properties (or belong to the class defined by the scope of the variables). Beyond this we need to know nothing about their individual attributes so long as we are content to derive merely the sort of pattern that will appear and not its particular manifestation.

Such a theory destined to remain 'algebraic',[14] because we are in fact unable to substitute particular values for the variables, ceases then to be a mere tool and becomes the final result of our theoretical efforts. Such a theory will, of course, in Popper's terms,[15] be one of small empirical content, because it enables us to predict or explain only certain general features of a situation which may be compatible with a great many particular circumstances. It will perhaps enable us to make only what M. Scriven has called 'hypothetical predictions',[16] i.e., predictions dependent on yet unknown future events; in any case the range of phenomena compatible with it will be wide and the possibility of falsifying it correspondingly small. But as in many fields this will be for the present, or perhaps forever, all the theoretical knowledge we can achieve, it will nevertheless extend the range of the possible advance of scientific knowledge.

The advance of science will thus have to proceed in two different directions : while it is certainly desirable to make our theories as falsifiable as possible, we must also push forward into fields where, as we advance, the degree of falsifiability necessarily decreases. This is the price we have to pay for an advance into the field of complex phenomena.

4. Statistics Impotent to Deal with Pattern Complexity

Before we further illustrate the use of those mere 'explanations of the principle'[17] provided by 'algebraic' theories which describe only the general character of higher-level generalities, and before we consider the important conclusions which follow from the insight into the boundaries of possible knowledge which our distinction provides, it is necessary to turn aside and consider the method which is often, but erroneously, believed to give us access to the understanding of complex phenomena: statistics. Because statistics is designed to deal with large numbers it is often thought that the difficulty arising from the large number of elements of which complex structures consist can be overcome by recourse to statistical techniques.

Statistics, however, deals with the problem of large numbers essentially by eliminating complexity and deliberately treating the individual elements which it counts as if they were not systematically connected. It avoids the problem of complexity by substituting for the information on the individual elements information on the frequency with which their different properties occur in classes of such elements, and it deliberately disregards the fact that the relative position of the different elements in a structure may matter. In other words, it proceeds on the assumption that information on the numerical frequencies of the different elements of a collective is enough to explain the phenomena and that no information is required on the manner in which the elements are related. The statistical method is therefore of use only where we either deliberately ignore, or are ignorant of, the relations between the individual elements with different attributes, i.e., where we ignore or are ignorant of any structure into which they are organized. Statistics in such situations enables us to regain simplicity and to make the task manageable by substituting a single attribute for the unascertainable individual attributes in the collective. It is, however, for this reason irrelevant to the solution of problems in which it is the relations between individual elements with different attributes which matters.

Statistics might assist us where we had information about many complex structures of the same kind, that is, where the complex phenomena and not the elements of which they consist could be made the elements of the statistical collective. It may provide us, e.g., with information on the relative frequency with which particular properties of the complex structures, say of the members of a species of organisms, occur together; but it presupposes that we have an independent criterion for identifying structures of the kind in question. Where we have such statistics about the properties of many individuals belonging to a class of animals, or languages, or economic systems, this may indeed be scientifically significant information.[18]

How little statistics can contribute, however, even in such cases, to the explanation of complex phenomena is clearly seen if we imagine that computers were natural objects which we found in sufficiently large numbers and whose behaviour we wanted to predict. It is clear that we should never succeed in this unless we possessed the mathematical knowledge built into the computers, that is, unless we knew the theory determining their structure. No amount of statistical information on the correlation between input and output would get us any nearer our aim. Yet the efforts which are currently made on a large scale with regard to the much more complex structures which we call organisms are of the same kind. The belief that it must be possible in this manner to discover by observation regularities in the relations between input and output without the possession of an appropriate theory in this case appears even more futile and naïve than it would be in the case of the computers.[19]

While statistics can successfully deal with complex phenomena where these are the elements of the population on which we have information, it can tell us nothing about the structure of these elements. It treats them, in the fashionable phrase, as 'black boxes' which are presumed to be of the same kind but about whose identifying characteristics it has nothing to say. Nobody would probably seriously contend that statistics can elucidate even the comparatively not very complex structures of organic molecules, and few would argue that it can help us to explain the functioning of organisms. Yet when it comes to accounting for the functioning of social structures, that belief is widely held. It is here of course largely the product of a misconception about what the aim of a theory of social phenomena is, which is another story.

5 . The Theory of Evolution as an Instance of Pattern Prediction

Probably the best illustration of a theory of complex phenomena which is of great value, although it describes merely a general pattern whose detail we can never fill in, is the Darwinian theory of evolution by natural selection. It is significant that this theory has always been something of a stumbling block for the dominant conception of scientific method. It certainly does not fit the orthodox criteria of 'prediction and control' as the hallmarks of scientific method.[20] Yet it cannot be denied that it has become the successful foundation of a great part of modern biology.

Before we examine its character we must clear out of the way a widely held misconception as to its content. It is often represented as if it consisted of an assertion about the succession of particular species of organisms which gradually changed into each other. This, however, is not the theory of evolution but an application of the theory to the particular events which took place on Earth during the last two billion years or so.[21] Most of the misapplications of evolutionary theory (particularly in anthropology and the other social sciences) and its various abuses (e.g., in ethics) are due to this erroneous interpretation of its content.

The theory of evolution by natural selection describes a kind of process (or mechanism) which is independent of the particular circumstances in which it has taken place on Earth, which is equally applicable to a course of events in very different circumstances, and which might result in the production of an entirely different set of organisms. The basic conception of the theory is exceedingly simple and it is only in its application to the concrete circumstances that its extraordinary fertility and the range of phenomena for which it can account manifests itself.[22] The basic proposition which has this far-reaching implication is that a mechanism of reduplication with transmittable variations and competitive selection of those which prove to have a better chance of survival will in the course of time produce a great variety of structures adapted to continuous adjustment to the environment and to each other. The validity of this general proposition is not dependent on the truth of the particular applications which were first made of it: if, for example, it should have turned out that, in spite of their structural similarity, man and ape were not joint descendants from a comparatively near common ancestor but were the product of two convergent strands starting from ancestors which differed much more from each other (such as is true of the externally very similar types of marsupial and placental carnivores), this would not have refuted Darwin's general theory of evolution but only the manner of its application to the particular case.

The theory as such, as is true of all theories, describes merely a range of possibilities. In doing this it excludes other conceivable courses of events and thus can be falsified. Its empirical content consists in what it forbids.[23] If a sequence of events should be observed which cannot be fitted into its pattern, such as, e.g., that horses suddenly should begin to give birth to young with wings, or that the cutting off of a hind-paw in successive generations of dogs should result in dogs being born without that hind-paw, we should regard the theory as refuted.[24]

The range of what is permitted by the theory is undeniably wide. Yet one could also argue that it is only the limitation of our imagination which prevents us from being more aware of how much greater is the range of the prohibited - how infinite is the variety of conceivable forms of organisms which, thanks to the theory of evolution, we know will not in the foreseeable future appear on Earth. Commonsense may have told us before not to expect anything widely different from what we already knew. But exactly what kinds of variations are within the range of possibility and what kinds are not, only the theory of evolution can tell us. Though we may not be able to write down an exhaustive list of the possibilities, any specific question we shall, in principle, be able to answer.

For our present purposes we may disregard the fact that in one respect the theory of evolution is still incomplete because we still know only little about the mechanism of mutation. But let us assume that we knew precisely the circumstances in which (or at least the probability that in given conditions) a particular mutation will appear, and that we similarly knew also the precise advantages which any such mutation would in any particular kind of environment confer upon an individual of a specific constitution. This would not enable us to explain why the existing species or organisms have the particular structures which they possess, nor to predict what new forms will spring from them.

The reason for this is the actual impossibility of ascertaining the particular circumstances which, in the course of two billion years, have decided the emergence of the existing forms, or even those which, during the next few hundred years, will determine the selection of the types which will survive. Even if we tried to apply our explanatory scheme to a single species consisting of a known number of individuals each of which we were able to observe, and assuming that we were able to ascertain and record every single relevant fact, their sheer number would be such that we should never be able to manipulate them, i.e., to insert these data into the appropriate blanks of our theoretical formula and then to solve the 'statement equations' thus determined.[25]

What we have said about the theory of evolution applies to most of the rest of biology. The theoretical understanding of the growth and functioning of organisms can only in the rarest of instances be turned into specific predictions of what will happen in a particular case, because we can hardly ever ascertain all the facts which will contribute to determine the outcome. Hence, 'prediction and control, usually regarded as essential criteria of science, are less reliable in biology'.[26] It deals with pattern-building forces, the knowledge of which is useful for creating conditions favourable to the production of certain kinds of results, while it will only in comparatively few cases be possible to control all the relevant circumstances.

6. Theories of Social Structures

It should not be difficult now to recognize the similar limitations applying to theoretical explanations of the phenomena of mind and society. One of the chief results so far achieved by theoretical work in these fields seems to me to be the demonstration that here individual events regularly depend on so many concrete circumstances that we shall never in fact be in a position to ascertain them all ; and that in consequence not only the ideal of prediction and control must largely remain beyond our reach, but also the hope remain illusory that we can discover by observation regular connections between the individual events. The very insight which theory provides, for example, that almost any event in the course of a man's life may have some effect on almost any of his future actions, makes it impossible that we translate our theoretical knowledge into predictions of specific events. There is no justification for the dogmatic belief that such translation must be possible if a science of these subjects is to be achieved, and that workers in these sciences have merely not yet succeeded in what physics has done, namely to discover simple relations between a few observables. If the theories which we have yet achieved tell us anything, it is that no such simple regularities are to be expected.

I will not consider here the fact that in the case of mind attempting to explain the detail of the working of another mind of the same order of complexity, there seems to exist, in addition to the merely 'practical' yet nevertheless unsurmountable obstacles, also an absolute impossibility: because the conception of a mind fully explaining itself involves a logical contradiction. This I have discussed elsewhere.[27] It is not relevant here because the practical limits determined by the impossibility of ascertaining all the relevant data lie so far inside the logical limits that the latter have little relevance to what in fact we can do.

In the field of social phenomena only economics and linguistics[28] seem to have succeeded in building up a coherent body of theory. I shall confine myself here to illustrating the general thesis with reference to economic theory, though most of what I have to say would appear to apply equally to linguistic theory.

Schumpeter well described the task of economic theory when he wrote that 'the economic life of a non-socialist society consists of millions of relations or flows between individual firms and households. We can establish certain theorems about them, but we can never observe them all.'[29] To this must be added that most of the phenomena in which we are interested, such as competition, could not occur at all unless the number of distinct elements involved were fairly large, and that the overall pattern that will form itself is determined by the significantly different behaviour of the different individuals so that the obstacle of obtaining the relevant data cannot be overcome by treating them as members of a statistical collective.

For this reason economic theory is confined to describing kinds of patterns which will appear if certain general conditions are satisfied, but can rarely if ever derive from this knowledge any predictions of specific phenomena. This is seen most clearly if we consider those systems of simultaneous equations which since Léon Walras have been widely used to represent the general relations between the prices and the quantities of all commodities bought and sold. They are so framed that *if* we were able to fill in all the blanks, i.e., *if* we knew all the parameters of these equations, we could calculate the prices and quantities of all the commodities. But, as at least the founders of this theory clearly understood, its purpose is not 'to arrive at a numerical calculation of prices', because it would be 'absurd' to assume that we can ascertain all the data.[30]

The prediction of the formation of this general kind of pattern rests on certain very general factual assumptions (such as that most people engage in trade in order to earn an income, that they prefer a larger income to a smaller one, that they are not prevented from entering whatever trade they wish, etc., - assumptions which determine the scope of the variables but not their particular values); it is, however, not dependent on the knowledge of the more particular circumstances which we would have to know in order to be able to predict prices or quantities of particular commodities. No economist has yet succeeded in making a fortune by buying or selling commodities on the basis of his scientific prediction of future prices (even though some may have done so by selling such predictions).

To the physicist it often seems puzzling why the economist should bother to formulate those equations although admittedly he sees no chance of determining the numerical values of the parameters which would enable him to derive from them the values of the individual magnitudes. Even many economists seem loath to admit that those systems of equations are not a step towards specific predictions of individual events but the final results of their theoretical efforts, a description merely of the general character of the order we shall find under specifiable conditions which, however, can never be translated into a prediction of its particular manifestations.

Predictions of a pattern are nevertheless both testable and valuable. Since the theory tells us under which general conditions a pattern of this sort will form itself, it will enable us to create such conditions and to observe whether a pattern of the kind predicted will appear. And since the theory tells us that this pattern assures a maximization of output in a certain sense, it also enables us to create the general conditions which will assure such a maximization, though we are ignorant of many of the particular circumstances which will determine the pattern that will appear.

It is not really surprising that the explanation of merely a sort of pattern may be highly significant in the field of complex phenomena but of little interest in the field of simple phenomena, such as those of mechanics. The fact is that in studies of complex phenomena the general patterns are all that is characteristic of those persistent wholes which are the main object of our interest, because a number of enduring structures have this general pattern in common and nothing else.[31]

7. The Ambiguity of the Claims of Determinism

The insight that we will sometimes be able to say that data of a certain class (or of certain classes) will bring about a pattern of a certain kind, but will not be able to ascertain the attributes of the individual elements which decide which particular form the pattern will assume, has consequences of considerable importance. It means, in the first instance, that when we assert that we know how something is determined, this statement is ambiguous. It may mean that we merely know what class of circumstances determines a certain kind of phenomena, without being able to specify the particular circumstances which decide which member of the predicted class of patterns will appear; or it may mean that we can also explain the latter. Thus we can reasonably claim that a certain phenomenon is determined by known natural forces and at the same time admit that we do not know precisely how it has been produced. Nor is the claim invalidated that we can explain the principle on which a certain mechanism operates if it is pointed out that we cannot say precisely what it will do at a particular place and time. From the fact that we do know that a phenomenon is determined by certain kinds of circumstances it does not follow that we must be able to know even in one particular instance all the circumstances which have determined all its attributes.

There may well be valid and more grave philosophical objections to the claim that science can demonstrate a universal determinism; but for all practical purposes the limits created by the impossibility of ascertaining all the particular data required to derive detailed conclusions from our theories are probably much narrower. Even if the assertion of a universal determinism were meaningful, scarcely any of the conclusions usually derived from it would therefore follow. In the first of the two senses we have distinguished we may, for instance, well be able to establish that every single action of a human being is the necessary result of the inherited structure of his body (particularly of its nervous system) and of all the external influences which have acted upon it since birth. We might even be able to go further and assert that if the most important of these factors were in a particular case very much the same as with most other individuals, a particular class of influences will have a certain kind of effect. But this would be an empirical generalization based on a *ceteris paribus* assumption which we could not verify in the particular instance. The chief fact would continue to be, in spite of our knowledge of the principle on which the human mind works, that we should not be able to state the full set of particular facts which brought it about that the individual did a particular thing at a particular time. The individual personality would remain for us as much a unique and unaccountable phenomenon which we might hope to influence in a desirable direction by such empirically developed practices as praise and blame, but whose specific actions we could generally not predict or control, because we could not obtain the information on all the particular facts which determined it.

8. The Ambiguity of "Relativism

The same sort of misconception underlies the conclusions derived from the various kinds of 'relativism'. In most instances these relativistic positions on questions of history, culture, or ethics are derived from the erroneous interpretations of the theory of evolution which we have already considered. But the basic conclusion that the whole of our civilization and all human values are the result of a long process of evolution in the course of which values, as the aims of human activity appeared, continue to change, seems inescapable in the light of our present knowledge. We are probably also entitled to conclude that our present values exist only as the elements of a particular cultural tradition and are significant only for some more or less long phase of evolution-whether this phase includes some of our pre-human ancestors or is confined to certain periods of human civilization. We have no more ground to ascribe to them eternal existence than to the human race itself. There is thus one possible sense in which we may legitimately regard human values as relative and speak of the probability of their further evolution.

But it is a far cry from this general insight to the claims of the ethical, cultural, or historical relativists or of evolutionary ethics. To put it crudely: while we know that all those values are relative to something, we do not know to what they are relative. We may be able to indicate the general class of circumstances which have made them what they are, but we do not know the particular conditions to which the values we hold are due, or what our values would be if those circumstances had been different. Most of the illegitimate conclusions are the result of the erroneous interpretation of the theory of evolution as the empirical establishment of a trend. Once we recognize that it gives us no more than a scheme of explanation which might be sufficient to explain particular phenomena *if* we knew all the facts which have operated in the course of history, it becomes evident that the claims of the various kinds of relativism (and of evolutionary ethics) are unfounded. Though we may meaningfully say that our values are determined by a class of circumstances definable in general terms, so long as we cannot state which particular circumstances have produced the existing values, or what our values would be under any specific set of other circumstances, no significant conclusions follow from the assertion.

It deserves brief notice in passing how radically opposed are the practical conclusions which are derived from the same evolutionary approach according as it is assumed that we can or cannot in fact know enough about the circumstances to derive specific conclusions from our theory. While the assumption of a sufficient knowledge of the concrete facts generally produces a sort of intellectual hubris which deludes itself that reason can judge all values, the insight into the impossibility of such full knowledge induces an attitude of humility and reverence towards that experience of mankind as a whole that has been precipitated in the values and institutions of existing society.

A few observations ought to be added here about the obvious significance of our conclusions for assessing the various kinds of 'reductionism'. In the sense of the first of the distinctions which we have repeatedly made - in the sense of general description - the assertion that biological or mental phenomena are 'nothing but' certain complexes of physical events, or that they are certain classes of structures of such events, these claims are probably defensible. But in the second sense - □ specific prediction - which alone would justify the more ambitious claims made for reductionism, they are completely unjustified. A full reduction would be achieved only if we were able to substitute for a description of events in biological or mental terms a description in physical terms which included an exhaustive enumeration of all the physical circumstances which constitute a necessary and sufficient condition of the biological or mental phenomena in question. In fact such attempts always consist - and can consist only - in the illustrative enumeration of classes of events, usually with an added 'etc.', which might produce the phenomenon in question. Such 'eta-reductions' are not reductions which enable us to dispense with the biological or mental entities, or to substitute for them a statement of physical events, but are mere explanations of the general character of the kind of order or pattern whose specific manifestations we know only through our concrete experience of them.[32]

9. The Importance of Our Ignorance

Perhaps it is only natural that in the exuberance generated by the successful advances of science the circumstances which limit our factual knowledge, and the consequent boundaries imposed upon the applicability of theoretical knowledge, have been rather disregarded. It is high time, however, that we take our ignorance more seriously. As Popper and others have pointed out, 'the more we learn about the world, and the deeper our learning, the more conscious, specific, and articulate will be our knowledge of what we do not know, our knowledge of our ignorance'.[33] We have indeed in many fields learnt enough to know that we cannot know all that we would have to know for a full explanation of the phenomena.

These boundaries may not be absolute. Though we may never know as much about certain complex phenomena as we can know about simple phenomena, we may partly pierce the boundary by deliberately cultivating a technique which aims at more limited objectives - the explanation not of individual events but merely of the appearance of certain patterns or orders. Whether we call these mere explanations of the principle or mere pattern predictions or higher-level theories does not matter. Once we explicitly recognize that the understanding of the general mechanism which produces patterns of a certain kind is not merely a tool for specific predictions but important in its own right, and that it may provide important guides to action (or sometimes indications of the desirability of no action), we may indeed find that this limited knowledge is most valuable.

What we must get rid of is the naive superstition that the world must be so organized that it is possible by direct observation to discover simple regularities between all phenomena and that this is a necessary presupposition for the application of the scientific method. What we have by now discovered about the organization of many complex structures should be sufficient to teach us that there is no reason to expect this, and that if we want to get ahead in these fields our aims will have to be somewhat different from what they are in the fields of simple phenomena.

10. A Postscript on the Role of 'Laws' in the Theory of Complex Phenomena[34]

Perhaps it deserves to be added that the preceding considerations throw some doubt on the widely held view that the aim of theoretical science is to establish 'laws' - at least if the word 'law' is used as commonly understood. Most people would probably accept some such definition of 'law' as that 'a scientific law is the rule by which two phenomena are connected with each other according to the principle of causality, that is to say, as cause and effect.'[35] And no less an authority than Max Planck is reported to have insisted that a true scientific law must be expressible in a single equation.[36]

Now the statement that a certain structure can assume only one of the (still infinite) number of states defined by a system of many simultaneous equations is still a perfectly good scientific (theoretical and falsifiable) statement.[37] We might still call, of course, such a statement a 'law', if we so wish (though some people might rightly feel that this would do violence to language); but the adoption of such a terminology would be likely to make us neglectful of an important distinction: for to say that such a statement describes, like an ordinary law, a relation between cause and effect would be highly misleading. It would seem, therefore, that the conception of law in the usual sense has little application to the theory of complex phenomena, and that therefore also the description of scientific theories as 'nomologic' or 'nomothetic' (or by the German term *Gesetzeswissenschaften*) is appropriate only to those two-variable or perhaps three-variable problems to which the theory of simple phenomena can be reduced, but not to the theory of phenomena which appear only above a certain level of complexity. If we assume that all the other parameters of such a system of equations describing a complex structure are constant, we can of course still call the dependence of one of the latter on the other a 'law' and describe a change in the one as 'the cause' and the change in the other as 'the effect'. But such a 'law' would be valid only for one particular set of values of all the other parameters and would change with every change in any one of them. This would evidently not be a very useful conception of a 'law', and the only generally valid statement about the regularities of the structure in question is the whole set of simultaneous equations from which, if the values of the parameters are continuously variable, an infinite number of particular laws, showing the dependence of one variable upon another, could be derived.

In this sense we may well have achieved a very elaborate and quite useful theory about some kind of complex phenomenon and yet have to admit that we do not know of a single law, in the ordinary sense of the word, which this kind of phenomenon obeys. I believe this to be in a great measure true of social phenomena: though we possess theories of social structures, I rather doubt whether we know of any 'laws' which social phenomena obey. It would then appear that the search for the discovery of laws is not an appropriate hall-mark of scientific procedure but merely a characteristic of the theories of simple phenomena as we have defined these earlier; and that in the field of complex phenomena the term 'law' as well as the concepts of cause and effect are not applicable without such modification as to deprive them of their ordinary meaning.

In some respect the prevalent stress on 'laws', i.e., on the discovery of regularities in two-variable relations, is probably a result of inductivism, because only such simple co-variation of two magnitudes is likely to strike the senses before an explicit theory or hypothesis has been formed. In the case of more complex phenomena it is more obvious that we must have our theory first before we can ascertain whether the things do in fact behave according to this theory. It would probably have saved much confusion if theoretical science had not in this manner come to be identified with the search for laws in the sense of a simple dependence of one magnitude upon another. It would have prevented such misconception as that, e.g., the biological theory of evolution proposed some definite 'law of evolution' such as a law of the necessary sequence of certain stages or forms. It has of course done nothing of the kind and all attempts to do this rest on a misunderstanding of Darwin's great achievement. And the prejudice that in order to be scientific one must produce laws may yet prove to be one of the most harmful of methodological conceptions. It may have been useful to some extent for the reason given by Popper, that 'simple statements ... are to be prized more highly'[38] in all fields where simple statements are significant. But it seems to me that there will always be fields where it can be shown that all such simple statements must be false and where in consequence also the prejudice in favour of 'laws' must be harmful.

- Reprinted from The Critical Approach to Science and Philosophy. Essays in Honor of K. R. Popper, ed. M. Bunge, New York (The Free Press), 1964. The article was there printed (apart from a few stylistic emendations by the editor) in the form in which I had completed the manuscript in December 1961 and without my ever having seen proofs. I have now availed myself of this opportunity to insert some references I had intended to add in the proofs.

Notes

1. See already Aristotle, Metaphysics; I, 11, 9, 9826b (Loeb ed. p. ij): 'It is through wonder that men now begin and originally began to philosophize ... it is obvious that they pursued science for the sake of knowledge, and not for any practical utility' ; also Adam Smith, "The Principles which Lead and Direct Philosophical Inquiries, as Illustrated by the History of Astronomy', in Essays, London, 1869, p. 340: 'Wonder, therefore, and not any expectation of advantage from its discoveries, is the first principle which prompts mankind to the study of philosophy, that science which pretends to lay open the concealed connections that unite the various appearances of nature; and they pursue this study for its own sake, as an original pleasure or good in itself, without regarding its tendency to procure them the means of many other pleasures.' Is there really any evidence for the now popular contrary view that, e.g., 'hunger in the Nile Valley led to the development of geometry' (as Gardner Murphy in the Handbook of Social Psychology, ed. by Gardner Lindzey, 1954, Vol. II, p. 616, tells us) ? Surely the fact that the discovery of geometry turned out to be useful does not prove that it was discovered because of its usefulness. On the fact that economics has in some degree been an exception to the general rule and has suffered by being guided more by need than by detached curiosity, see my lecture on "The Trend of Economic Thinking' in Economica, 1933.
2. See K. R. Popper, The Poverty of Historicism, London, 1957, p. 121 : 'Science . . . cannot start with observations, or with the "collection of data", as some students of method believe. Before we can collect data, our interest in data of a certain kind assist be aroused: the problem always comes first.' Also in his The Logic of Scientific Discovery, London, 1959, p. 59: Observation is always observation in the light of theories'
3. Although in some respects the capacity of our senses for pattern recognition clearly also exceeds the capacity of our mind for specifying these patterns. The question of the extent to which this capacity of our senses is the result of another kind of (pre-sensory) experience is another matter. See, on this and on the general point that all perception involves a theory or hypothesis, my book The Sensory Order, London and Chicago, 1952, esp. para. 7.37. Cf. also the remarkable thought expressed by Adam Ferguson (and probably derived from George Berkeley) in The History of Civil Society, London, 1767, p. 39, that 'the inferences of thought are sometimes not to be distinguished from the perception of sense'; as well as H. von Helmholtz's theory of the 'unconscious inferences' involved in most perceptions. For a recent revival of these ideas see N. R. Hanson, Patterns of Discovery, Cambridge University Press, 1958, esp. p. 19, and the views on the role of 'hypotheses' in perception as developed in recent 'cognition theory' by J. S. Bruner, L. Postman and others.
4. Cf. G. H. Hardy, Mathematician's Apology, Cambridge University Press, 1941, p. 14: ? mathematician, like a painter or poet, is a maker of patterns.'
5. Charles Dickens, David Copperfield, p. 1.
6. Though it may be permissible to doubt whether it is in fact possible to predict, e.g., the precise pattern which the vibrations of an airplane will at a particular moment produce in the standing wave on the surface of the coffee in my cup.

7. Cf. Michael Scriven, 'A Possible Distinction between Traditional Scientific Disciplines and the Study of Human Behavior', Minnesota Studies in the Philosophy of Science, I, 1956, p. 332 : "The difference between the scientific study of behavior and that of physical phenomena is thus partly due to the relatively greater complexity of the simplest phenomena we are concerned to account for in a behavioral theory.'

8. Ernest Nagel, The Structure of Science, New York, 1961, p. 505: 'though social phenomena may indeed be complex, it is by no means certain that they are in general more complex than physical and biological phenomena.' See, however, Johann von Neumann, "The General and Logical Theory of Automata', Cerebral Mechanism in Behavior. The Hixon Symposium, New York, 195 1, p. 24: 'we are dealing here with parts of logic with which we have practically no experience. The order of complexity is out of all proportion to anything we have ever known.' It may be useful to give here a few illustrations of the orders of magnitude with which biology and neurology have to deal. While the total number of electrons in the Universe has been estimated at io79 and the number of electrons and protons at 10^sup 100^, there are in chromosomes with 1,000 locations [genes] with 10 allelomorphs 10^sup 1000^ possible combinations; and the number of possible proteins is estimated at 10^sup 2700^ (L. von Bertalanffy, Problems of Life, New York, 1952, p. 103). C. Judson Herrick {Brains of Rats and Men, New York), suggests that 'during a few minutes of intense cortical activity the number of interneuronic connections actually made (counting also those that are actuated more than once in different associational patterns) may well be as great as the total number of atoms in the solar system' (i.e. 10^sup 56^); and Ralph W. Gerard {Scientific American, September 1953, p. 118) has estimated that in the course of seventy years a man may accumulate 15 □ 10^sup 12^ units of information ('bits'), which is more than 1,000 times larger than the number of nerve cells. The further complications which social relations superimpose upon this are, of course, relatively insignificant. But the point is that if we wanted to 'reduce' social phenomena to physical events, they would constitute an additional complication, superimposed upon that of the physiological processes determining mental events.

9. Cf. Warren Weaver, 'A Quarter Century in the Natural Sciences', The Rockefeller Foundation Annual Report, 1958, Chapter I, 'Science and Complexity', which, when writing this, I knew only in the abbreviated version which appeared in the American Scientist, XXXVI, 1948.

10. Lloyd Morgan's conception of 'emergence' derives, via G. H. Lewes (Problems of Life and Mind, ist series, Vol. II, problem V, Ch. III, section headed 'Resultants and Emergents', American ed., Boston, 1891, p. 368), from John Stuart Mill's distinction of the 'heteropathic' laws of chemistry and other complex phenomena from the ordinary 'composition of causes' in mechanics, etc. See his System of Logic, London, 1843, Bk. III, Ch. 6, in Vol. I, p. 431 of the first edition, and C. Lloyd Morgan, The Emergence of Novelty, London, 1933, p. 12.

11. Lewis White Beck, 'The "Natural Science Ideal" in the Social Sciences', The Scientific Monthly, LXVIII, June 1949, p. 388.

12. Cf. F. A. Hayek, The Sensory Order, paras. 8.66-8.86.

13. Cf. Ernest Nagel, 'Problems of Concept and Theory Formation in the Social Sciences', in Science, Language and Human Rights (American Philosophical Association, Eastern Division, Vol. 1), University of Pennsylvania Press, 1952, p. 620: 'In many cases we are ignorant of the appropriate initial and boundary conditions, and cannot make precise forecasts even though available theory is adequate for that purpose.'

14. The useful term 'algebraic theories' was suggested to me by J. W. N. Watkins.

15. K. R. Popper, The Logic of Scientific Discovery, London, 1959, p. 113.

16. M. Scriven, 'Explanation and Prediction in Evolutionary Theory', Science, August 28, 1959, p. 478 and cf. K. R. Popper, 'Prediction and Prophecy in the Social Sciences' (1949), reprinted in his Conjectures and Refutations, London, 1963, especially pp. 339 et seqq.

17. Cf. F. A. Hayek, 'Degrees of Explanation', The British Journal for the Philosophy of Science, VI, No. 23, 1955, now reprinted as the first essay of the present collection.

18. See F. A. Hayek, Tbe Counter-Revolution of Science, Glencoe, Ul., 1952, pp. 60-63.

19. Cf. J. G. Taylor, 'Experimental Design: A Cloak for Intellectual Sterility', The British Journal of Psychology, 49, 1958, esp. pp. 107-8.

20. Cf., e.g., Stephen Toulmin, Foresight and Prediction, London, 1961, p. 24: 'No scientist has ever used this theory to foretell the coming into existence of creatures of a novel species, still less verified his forecast.'

21. Even Professor Popper seems to imply this interpretation when he writes {Poverty of Historicism, p. 107) that 'the evolutionary hypothesis is not a universal law of nature but a particular (or, more precisely, singular) historical statement about the ancestry of a number of terrestrial plants and animals'. If this means that the essence of the theory of evolution is the assertion that particular species had common ancestors, or that the similarity of structure always means a common ancestry (which was the hypothesis from which the theory of evolution was derived), this is emphatically not the main content of the present theory of evolution. There is, incidentally, some contradiction between Popper's treatment of the concept of 'mammals' as a universal (Logic, p. 65) and the denial that the evolutionary hypothesis describes a universal law of nature. The same process might have produced mammals on other planets.

22. Charles Darwin himself well knew, as he once wrote to Lyell, that 'all the labour consists in the application of the theory' (quoted by C. C. Gillispie, The Edge of Objectivity, Princeton, i960, p. 314).

23. K. R. Popper, Logic, p. 41.

24. Cf. Morton Beckner, The Biological Way of Thought, Columbia University Press, 1954, p. 241.

25. K. R. Popper, Logic, p. 73.

26. Ralph S. Lillie, 'Some Aspects of Theoretical Biology', Philosophy of Science, XV, 2, 1948, p. 119.

27. See The Sensory Order, 8.66-8.86, also The Counter-Revolution of Science, Glencoe, I, 22 1952, p. 48, and the following essay in the present volume.

28. See particularly Noam Chomsky, Syntactic Structures, 's Gravenhage, 1957, who characteristically seems to succeed in building up such a theory after frankly abandoning the striving after an inductivist 'discovery procedure' and substituting for it the search after an 'evaluation procedure' which enables him to eliminate false theories of grammars and where these grammars may be arrived at 'by intuition, guess-work, all sorts of partial methodological hints, reliance on past experience, etc.' (p. 56).

29. J. A. Schumpeter, History of Economic Analysis, Oxford University Press, 1954, p. 241.

30. V. Pareto, Manuel d'□conomie politique, 2nd ed., Paris, 1927, pp. 223-4.

31. A characteristic instance of the misunderstanding of this point (quoted by E. Nagel, p. 61) occurs in Charles A. Beard, Tbe Nature of the Social Sciences, New York, 1934, 29, where it is

contended that if a science of society 'were a true science, like that of it would enable us to predict the essential movements of human affairs for the and the indefinite future, to give pictures of society in the year 2000 or the year 2500 just as astronomers can map the appearances of the heavens at fixed points of time in future.'

32. Cf. My Counter-Revolution of Science, pp. 48 et seqq., and William Craig, 'Replacement Auxiliary Expressions', The Philosophical Review, 65, 1956.
33. K. R. Popper, 'On the Sources of Knowledge and Ignorance', Proceedings of tbe British Academy, 46, i960, p. 69. See also Warren Weaver, ? Scientist Ponders Faith', Saturday Review, January 3, 1959: 'Is science really gaining in its assault on the totality of the unsolved? As science learns one answer, it is characteristically true that it also learns several new questions. It is as though science were working in a great forest of ignorance, making an ever larger circular clearing within which, not to insist on the pun, things are clear. . . . But, as that circle becomes larger and larger, the circumference of contact with ignorance also gets longer and longer. Science learns more and more. But there is an ultimate sense in which it does not gain ; for the volume of the appreciated but not understood keeps getting larger. We keep, in science, getting a more and more sophisticated view of our ignorance.'
34. This last section of this essay was not contained in the version originally published and has been added to this reprint.
35. The particular wording which I happened to come across while drafting this is taken from H. Kelsen, "The Natural Law Doctrine Before the Tribunal of Science' (1949), reprinted in What is Justice?, University of California Press, i960, p. 139. It seems to express well a widely held view.
36. Sir Karl Popper comments on this that it seems extremely doubtful whether any single one of Maxwell's equations could be said to express anything of real significance if we knew none of the others; in fact, it seems that the repeated occurrence of the symbols in the various equations is needed to secure that these symbols have the intended meanings.
37. Cf. K. R. Popper, Logic of Scientific Discovery, □ 17, p. 73 : 'Even if the system of equations does not suffice for a unique solution, it does not allow every conceivable combination of values to be substituted for the "unknowns" (variables). Rather, the system of equations characterizes certain combinations of values or value systems as admissible, and others as inadmissible; it distinguishes the class of admissible value systems from the ckss of inadmissible value systems.' Note also the application of this in the following passages to 'statement equations'.
38. Ibid., p. 142.