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Chapter 4

The Theory of Complex Phenomena

F. A. Hayek

Pattern Recognition 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 recognition 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 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 chapter, 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, color, 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, makes 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]

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 formulas 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. Nonphysical phenomena are more complex because we call physical what can be described by relatively simple formulas.

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.

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.

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 a 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 behavior 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 naive 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.

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. Common sense 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, or 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 favorable to the production of certain kinds of results, while it will only in comparatively few cases be possible to control all the relevant circumstances.

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 behavior 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 toward 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 ensures 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]

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.

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 prehuman 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 toward 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 necesary 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 "etc.-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]

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 learned 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.

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 (theorectical 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 hallmark 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 favor of "laws" must be harmful.

Notes

1. See already Aristotle, *Metaphysics*, I, II 9, 9826b (Loeb ed., p. 13): "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 Psyobalogy*, ed. by Gardner Lindzey, 1954, Vol. 11, 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* must 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 (presensory) 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 inference" 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. 24: "A 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 Bebavior*. The Hixon Symposium, New York, 1951, 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 10^{79} and the number of electrons and protons at 10^{100}, there are in chromosomes with 1,000 locations [genes] with 10 allelomorphs 10^{1000} possible combinations; and the number of possible proteins is estimated at 10^{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^{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^{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 Science," *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*, 1st 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," *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. I), 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 seq.
17. Cf. F. A. Hayek, "Degrees of Explanation," *British Journal for the Philosophy of Science*, VI, No. 23, 1955.
18. See F. A. Hayek, *The Counter-Revolution of Science*, Glencoe, Ill., 1952, pp. 60–63.
19. Cf. J. G. Taylor, "Experimental Design: A Cloak for Intellectual Sterility," *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, 1960, 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.
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'économie politique*, 2d ed., Paris, 1927, pp. 223–4.
31. A characteristic instance of the misunderstanding of this point (quoted by E. Nagel, l.c., p. 61) occurs in Charles A. Beard, *The Nature of the Social Sciences*, New York, 1934, p. 29, where it is contended that if a science of society "were a true science, like that of astronomy, it would enable us to predict the essential movements of human affairs for the immediate 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 the future."
32. Cf. My *Counter-Revolution of Science*, pp. 48 et seq., and William Craig, "Replacement of Auxiliary Expressions," *Philosophical Review*, 65, 1956.
33. K. R. Popper, "On the Sources of Knowledge and Ignorance," *Proceedings of the British Academy*, 46, 1960, p. 69. See also Warren Weaver, "A 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 was not contained in the original version.
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, 1960, 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*, sec. 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 class of inadmissible value systems." Note also the application of this in the following passages to "statement equations."
38. Ibid., p. 142.

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W.V. QUINE

Holism, Part 1: Two Dogmas of Empiricism

The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges. Or, to change the figure, total science is like a field of force whose boundary conditions are experience. A conflict with experience at the periphery occasions readjustments in the interior of the field. Truth values have to be redistributed over some of our statements. Reëvaluation of some statements entails reëvaluation of others, because of their logical interconnections—the logical laws being in turn simply certain further statements of the system, certain further elements of the field. Having reëvaluated one statement we must reëvaluate some others, which may be statements logically connected with the first or may be the statements of logical connections themselves. But the total field is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to reëvaluate in the light of any single contrary experience. No particular experiences are linked with any particular statements in the interior of the field, except indirectly through considerations of equilibrium affecting the field as a whole.

If this view is right, it is misleading to speak of the empirical content of an individual statement—especially if it is a statement at all remote from the experiential periphery of the field. Furthermore it becomes folly to seek a boundary between synthetic statements, which hold contingently on experience, and analytic statements, which hold come what may. Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. Conversely, by the same token, no statement is immune to revision. Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift and the shift whereby Kepler superseded Ptolemy, or Einstein Newton, or Darwin Aristotle?

From W.V. Quine, "Two Dogmas of Empiricism." Reprinted by permission of the publisher from *From a Logical Point of View* by W.V. Quine, Cambridge, Mass.: Harvard University Press,

For vividness I have been speaking in terms of varying distances from a sensory periphery. Let me try now to clarify this notion without metaphor. Certain statements, though *about* physical objects and not sense experience, seem peculiarly germane to sense experience—and in a selective way: some statements to some experiences, others to others. Such statements, especially germane to particular experiences, I picture as near the periphery. But in this relation of "germaneness" I envisage nothing more than a loose association reflecting the relative likelihood, in practice, of our choosing one statement rather than another for revision in the event of recalcitrant experience. For example, we can imagine recalcitrant experiences to which we would surely be inclined to accommodate our system by reëvaluating just the statement that there are brick houses on Elm Street, together with related statements on the same topic. We can imagine other recalcitrant experiences to which we would be inclined to accommodate our system by reëvaluating just the statement that there are no centaurs, along with kindred statements. A recalcitrant experience can, I have urged, be accommodated by any of various alternative reëvaluations in various alternative quarters of the total system; but, in the cases which we are now imagining, our natural tendency to disturb the total system as little as possible would lead us to focus our revisions upon these specific statements concerning brick houses or centaurs. These statements are felt, therefore, to have a sharper empirical reference than highly theoretical statements of physics or logic or ontology. The latter statements may be thought of as relatively centrally located within the total network, meaning merely that little preferential connection with any particular sense data obtrudes itself.

As an empiricist I continue to think of the conceptual scheme of science as a tool, ultimately, for predicting future experience in the light of past experience. Physical objects are conceptually imported into the situation as convenient intermediaries—not by definition in terms of experience, but simply as irreducible posits[1] comparable, epistemologically, to the gods of Homer. For my part I do, qua lay physicist, believe in physical objects and not in Homer's gods; and I consider it a scientific error to believe otherwise. But in point of epistemological footing the physical objects and the gods differ only in degree and not in kind. Both sorts of entities enter our conception only as cultural posits. The myth of physical objects is epistemologically superior to most in that it has proved more efficacious than other myths as a device for working a manageable structure into the flux of experience.

Positing does not stop with macroscopic physical objects. Objects at the atomic level are posited to make the laws of macroscopic objects, and ultimately the laws of experience, simpler and more manageable; and we need not expect or demand full definition of atomic and subatomic entities in terms of macroscopic ones, any more than definition of macroscopic things in terms of sense data. Science is a continuation of common sense, and it continues the common-sense expedient of swelling ontology to simplify theory.

Physical objects, small and large, are not the only posits. Forces are another example; and indeed we are told nowadays that the boundary between energy and matter is obsolete. Moreover, the abstract entities which are the substance of mathematics—ultimately classes and classes of classes and so on up—are another posit in the same spirit. Epistemologically these are myths on the same footing with physical objects and gods, neither better nor worse except for differences in the degree to which they expedite our dealings with sense experiences.

The over-all algebra of rational and irrational numbers is underdetermined by the algebra of rational numbers, but is smoother and more convenient; and it includes the algebra of rational numbers as a jagged or gerrymandered part.[2] Total science, mathematical and natural and human, is similarly but more extremely underdetermined by experience. The edge of the system must be kept squared with experience; the rest, with all its elaborate myths or fictions, has as its objective the simplicity of laws.

Ontological questions, under this view, are on a par with questions of natural science.[3] Consider the question whether to countenance classes as entities. This, as I have argued elsewhere,[4] is the question whether to quantify with respect to variables which take classes as values. Now Carnap[5] has maintained that this is a question not of matters of fact but of choosing a convenient language form, a convenient conceptual scheme or framework for science. With this I agree, but only on the proviso that the same be conceded regarding scientific hypotheses generally. Carnap[6] has recognized that he is able to preserve a double standard for ontological questions and scientific hypotheses only by assuming an absolute distinction between the analytic and the synthetic; and I need not say again that this is a distinction which I reject.[7]

The issue over there being classes seems more a question of convenient conceptual scheme; the issue over there being centaurs, or brick houses on Elm Street, seems more a question of fact. But I have been urging that this difference is only one of degree, and that it turns upon our vaguely pragmatic inclination to adjust one strand of the fabric of science rather than another in accommodating some particular recalcitrant experience. Conservatism figures in such choices, and so does the quest for simplicity.

Carnap, Lewis, and others take a pragmatic stand on the question of choosing between language forms, scientific frameworks; but their pragmatism leaves off at the imagined boundary between the analytic and the synthetic. In repudiating such a boundary I espouse a more thorough pragmatism. Each man is given a scientific heritage plus a continuing barrage of sensory stimulation; and the considerations which guide him in warping his scientific heritage to fit his continuing sensory promptings are, where rational, pragmatic.

NOTES

1. Cf. pp. 17f in W.V. Quine, "On What There Is," in *From A Logical Point of View*. Cambridge: Harvard University Press, 1953, pp. 1–19.
2. Cf. p. 18 in W.V. Quine, "On What There Is," in *From A Logical Point of View*. Cambridge: Harvard University Press, 1953, pp. 1–19.

3. "L'ontologie fait corps avec la science elle-même et ne peut en être separée," É. Meyerson, *Identité et Realité*. Paris, 1908; 4th ed., 1932, p. 439.
4. W.V. Quine, "On What There Is," in *From A Logical Point of View*. Cambridge: Harvard University Press, 1953, pp. 12f; and W.V. Quine, "Logic and the Reification of Universals," In *From A Logical Point of View*. Cambridge: Harvard University Press, 1953, pp. 102f.
5. R. Carnap, "Empiricism, Semantics, and Ontology," *Revue internationale de philosophie* 4 (1950), 20–40. Reprinted in L. Linsky (ed.), *Semantics and the Philosophy of Language* (Urbana: University of Illinois Press, 1952).
6. *Ibid.*, p. 32n.
7. For an effective expression of further misgivings over this distinction, see M. White, "The Analytic and the Synthetic: An Untenable Dualism," in Sidney Hook (ed.), *John Dewey: Philosopher of Science and Freedom*. New York: Dial Press, 1950, pp. 316–330. Reprinted in L. Linsky (ed.), *Semantics and the Philosophy of Language*. Urbana: University of Illinois Press, 1952.

10

W.V. QUINE

Holism, Part 2: Posits and Reality

I. Subvisible Particles

According to physics may desk is, for all its seeming fixity and solidity, a swarm of vibrating molecules. The desk as we sense it is comparable to a distant haystack in which we cannot distinguish the individual stalks; comparable also to a wheel in which, because of its rapid rotation, we cannot distinguish the individual spokes. Comparable, but with a difference. By approaching the haystack we can distinguish the stalks, and by retarding the wheel we can distinguish the spokes. On the other hand no glimpse is to be had of the separate molecules of the desk; they are, we are told, too small.

Lacking such experience, what evidence can the physicist muster for his doctrine of molecules? His answer is that there is a convergence of indirect evidence, drawn from such varied phenomena as expansion, heat conduction, capillary attraction, and surface tension. The point is that these miscellaneous phenomena can, if we assume the molecular theory, be marshaled under the familiar laws of motion. The fancifulness of thus assuming a substructure of moving particles of imperceptible size is offset by a gain in naturalness and scope on the part of the aggregate laws of physics. The molecular theory is felt, moreover, to gain corroboration progressively as the physicist's predictions of future observations turn out to be fulfilled, and as the theory proves to invite extensions covering additional classes of phenomena.

The benefits thus credited to the molecular doctrine may be divided into five. One is simplicity: empirical laws concerning seemingly dissimilar phenomena are integrated into a compact and unitary theory. Another is familiarity of principle: the already familiar laws of motion are made to serve where independent laws would otherwise have been needed. A third is scope: the resulting unitary theory implies a wider array of testable consequences than any likely accumulation of separate laws would have implied. A fourth is fecundity: successful further extensions of theory are expedited. The fifth goes without saying: such testable consequences of the theory as have been tested have turned out well, aside from such sparse exceptions as may in good conscience be chalked up to unexplained interferences.

Written about 1955 for the beginning of *Word and Object*, but eventually superseded. First published along with a Japanese translation in S. Uyeda, ed., *Basis of the Contemporary Philosophy*, vol. 5 (Tokyo: Waseda University Press, 1960). It has appeared also in Italian, *Rivista di Filosofia*, 1964.

Simplicity, the first of the listed benefits, is a vague business. We may be fairly sure of this much: theories are more or less simple, more or less unitary, only relative to one or another given vocabulary or conceptual apparatus. Simplicity is, if not quite subjective, at any rate parochial. Yet simplicity contributes to scope, as follows. An empirical theory, typically, generalizes or extrapolates from sample data, and thus covers more phenomena than have been checked. Simplicity, by our lights, is what guides our extrapolation. Hence the simpler the theory, on the whole, the wider this unchecked coverage.

As for the fourth benefit, fecundity, obviously it is a consequence of the first two, simplicity and familiarity, for these two traits are the best conditions for effective thinking.

Not all the listed benefits are generally attributable to accepted scientific theories, though all are to be prized when available. Thus the benefit of familiarity of principle may, as in quantum theory and relativity theory, be renounced, its loss being regretted but outweighed.

But to get back. In its manifest content the molecular doctrine bears directly on unobservable reality, affirming a structure of minute swarming particles. On the other hand any defense of it has to do rather with its indirect bearing on observable reality. The doctrine has this indirect bearing by being the core of an integrated physical theory which implies truths about expansion, conduction, and so on. The benefits which we have been surveying are benefits which the molecular doctrine, as core, brings to the physics of these latter observable phenomena.

Suppose now we were to excise that core but retain the surrounding ring of derivative laws, thus not disturbing the observable consequences. The retained laws could be viewed thenceforward as autonomous empirical laws, innocent of any molecular commitment. Granted, this combination of empirical laws would never have been achieved without the unifying aid of a molecular doctrine at the center; note the recent remarks on scope. But we might still delete the molecular doctrine once it has thus served its heuristic purpose.

This reflection strengthens a natural suspicion: that the benefits conferred by the molecular doctrine give the physicist good reason to prize it, but afford no evidence of its truth. Though the doctrine succeed to perfection in its indirect bearing on observable reality, the question of its truth has to do rather with its direct claim on unobservable reality. Might the molecular doctrine not be ever so useful in organizing and extending our knowledge of the behavior of observable things, and yet be factually false?

One may question, on closer consideration, whether this is really an intelligible possibility. Let us reflect upon our words and how we learned them.

II. Posits and Analogies

Words are human artifacts, meaningless save as our associating them with experience endows them with meaning. The word 'swarm' is initially meaningful to us through association with such experiences as that of a hovering swarm of gnats, or a swarm of dust motes in a shaft of sunlight. When we extend the word to desks and the like, we are engaged in drawing an analogy between swarms ordinarily so-called, on the one hand, and desks, etc., on the other. The word 'molecule' is then given meaning derivatively: having conceived of desks analogically as swarms, we imagine molecules as the things the desks are swarms of.

The purported question of fact, the question whether the familiar objects around us are really swarms of subvisible particles in vibration, now begins to waver and dissolve. If the words involved here make sense only by analogy, then the only question of fact is the question how good an analogy there is between the behavior of a desk or the like and the behavior, e.g., of a swarm of gnats. What had seemed a direct bearing of the molecular doctrine upon reality has now dwindled to an analogy.

Even this analogical content, moreover, is incidental, variable, and at length dispensable. In particular the analogy between the swarming of the molecules of a solid and the swarming of gnats is only moderately faithful; a supplementary aid to appreciating the dynamics of the molecules of a solid is found in the analogy of a stack of bedsprings. In another and more recondite part of physics, the theory of light, the tenuousness of analogy is notorious: the analogy of particles is useful up to a point and the analogy of waves is useful up to a point, but neither suffices to the exclusion of the other. Faithful analogies are an aid to the physicist's early progress in an unaccustomed medium, but, like water-wings, they are an aid which he learns to get along without.

In §I we contrasted a direct and an indirect bearing of the molecular doctrine upon reality. But the direct bearing has not withstood scrutiny. Where there had at first seemed to be an undecidable question of unobservable fact, we now find mere analogy at most and not necessarily that. So the only way in which we now find the molecular doctrine genuinely to bear upon reality is the indirect way, via implications in observable phenomena.

The effect of this conclusion upon the status of molecules is that they lose even the dignity of inferred or hypothetical entities which may or may not really be there. The very sentences which seem to propound them and treat of them are gibberish by themselves, and indirectly significant only as contributory clauses of an inclusive system which does also treat of the real. The molecular physicist is, like all of us, concerned with commonplace reality, and merely finds that he can simplify his laws by positing an esoteric supplement to the exoteric universe. He can devise simpler laws for this enriched universe, this "sesquiverse" of his own decree, than he has been able to devise for its real or original portion alone.

In §I we imagined deleting the molecular doctrine from the midst of the derivative body of physical theory. From our present vantage point, however, we see that operation as insignificant; there is no substantive doctrine of molecules to delete. The sentences which seem to propound molecules are just devices for organizing the significant sentences of physical theory. No matter if physics makes molecules or other insensible particles seem more fundamental than the objects of common sense; the particles are posited for the sake of a simple physics.

The tendency of our own reflections has been, conversely, to belittle molecules and their ilk, leaving common-sense bodies supreme. Still, it may now be protested, this invidious contrast is unwarranted. What are given in sensation are variformed and varicolored visual patches, varitextured and varitemperatured tactual feels, and an assortment of tones, tastes, smells, and other odds and ends; desks are no more to be found among these data than molecules. If we have evidence for the existence of the bodies of common sense, we have it only in the way in which we may be said to have evidence for the existence of molecules. The positing of either sort of body is good science insofar merely as it helps us formulate our laws—laws whose ultimate evidence lies in the sense data of the past, and whose ultimate vindication lies in anticipation of sense data of the future. The positing of molecules differs from the positing of the bodies of common sense mainly in degree of sophistication. In whatever sense the molecules in my desk are unreal and a figment of the imagination of the scientist, in that sense the desk itself is unreal and a figment of the imagination of the race.

This double verdict of unreality leaves us nothing, evidently, but the raw sense data themselves. It leaves each of us, indeed, nothing but his own sense data; for the assumption of there being other persons has no better support than has the assumption of there being any other sorts of external objects. It leaves each of us in the position of solipsism, according to which there is nobody else in the world, nor indeed any world but the pageant of one's own sense data.

III. Restitution

Surely now we have been caught up in a wrong line of reasoning. Not only is the conclusion bizarre; it vitiates the very considerations that lead to it. We cannot properly represent man as inventing a myth of physical objects to fit past and present sense data, for past ones are lost except to memory; and memory, far from being a straightforward register of past sense data, usually depends on past posits of physical objects. The positing of physical objects must be seen not as an *ex post facto* systematization of data, but as a move prior to which no appreciable data would be available to systematize.

Something went wrong with our standard of reality. We became doubtful of the reality of molecules because the physicist's statement that there are molecules took on the aspect of a mere technical convenience in smoothing the laws of physics. Next we noted that common-sense bodies are epistemologically much on a par with the molecules, and inferred the unreality of the common-sense bodies themselves. Here our bemusement becomes visible. Unless we change meanings in midstream, the familiar bodies around us are as real as can be; and it smacks of a contradiction in terms to conclude otherwise. Having noted that man has no evidence for the existence of bodies beyond the fact that their assumption helps him organize experience, we should have done well, instead of disclaiming evidence for the existence of bodies, to conclude: such, then, at bottom, is what evidence is, both for ordinary bodies and for molecules.

This point about evidence does not upset the evidential priority of sense data. On the contrary, the point about evidence is precisely that the testimony of the senses *does* (contrary to Berkeley's notion) count as evidence for bodies, such being (as Samuel Johnson perceived) just the sort of thing that evidence is. We can continue to recognize, as in §II, that molecules and even the gross bodies of common sense are simply posited in the course of organizing our responses to stimulation; but a moral to draw from our reconsideration of the terms 'reality' and 'evidence' is that posits are not *ipso facto* unreal. The benefits of the molecular doctrine which so impressed us in §I, and the manifest benefits of the aboriginal posit of ordinary bodies, are the best evidence of reality we can ask (pending, of course, evidence of the same sort for some alternative ontology).

Sense data are posits too. They are posits of psychological theory, but not, on that account, unreal. The sense datum may be construed as a hypothetical component of subjective experience standing in closest possible correspondence to the experimentally measurable conditions of physical stimulation of the end organs. In seeking to isolate sense data we engage in empirical psychology, associating physical stimuli with human resources. I shall not guess how useful the positing of sense data may be for psychological theory, or more specifically for a psychologically grounded theory of evidence, nor what detailed traits may profitably be postulated concerning them. In our flight from the fictitious to the real, in any event, we have come full circle.

Sense data, if they are to be posited at all, are fundamental in one respect; the small particles of physics are fundamental in a second respect, and common-sense bodies in a third. Sense data are *evidentially* fundamental: every man is beholden to his senses for every hint of bodies. The physical particles are *naturally* fundamental, in this kind of way: laws of behavior of those particles afford, so far as we know, the simplest formulation of a general theory of what happens. Common-sense bodies, finally, are *conceptually* fundamental: it is by reference to them that the very notions of reality and evidence are acquired, and that the concepts which have to do with physical particles or even with sense data tend to be framed and phrased. But these three types of priority must not be viewed as somehow determining three competing, self-sufficient conceptual schemes. Our one serious conceptual scheme is the inclusive, evolving one of science, which we inherit and, in our several small ways, help to improve.

IV. Working from Within

It is by thinking within this unitary conceptual scheme itself, thinking about the processes of the physical world, that we come to appreciate that the world

can be evidenced only through stimulation of our senses. It is by thinking within the same conceptual scheme that we come to appreciate that language, being a social art, is learned primarily with reference to intersubjectively conspicuous objects, and hence that such objects are bound to be central conceptually. Both of these *aperçus* are part of the scientific understanding of the scientific enterprise; not prior to it. Insofar as they help the scientist to proceed more knowingly about his business, science is using its findings to improve its own techniques. Epistemology, on this view, is not logically prior somehow to common sense or to the refined common sense which is science; it is part rather of the overall scientific enterprise, an enterprise which Neurath has likened to that of rebuilding a ship while staying afloat in it.

Epistemology, so conceived, continues to probe the sensory evidence for discourse about the world; but it no longer seeks to relate such discourse somehow to an imaginary and impossible sense-datum language. Rather it faces the fact that society teaches us our physicalistic language by training us to associate various physicalistic sentences directly, in multifarious ways, with irritations of our sensory surfaces, and by training us also to associate various such sentences with one another.

The complex totality of such associations is a fluctuating field of force. Some sentences about bodies are, for one person or for many, firmly conditioned one by one to sensory stimulation of specifiable sorts. Roughly specifiable sequences of nerve hits can confirm us in statements about having had breakfast, or there being a brick house on Elm Street, beyond the power of secondary associations with other sentences to add or detract. But there is in this respect a grading-off from one example to another. Many sentences even about common-sense bodies rest wholly on indirect evidence; witness the statement that one of the pennies now in my pocket was in my pocket last week. Conversely, sentences even about electrons are sometimes directly conditioned to sensory stimulation, e.g., via the cloud chamber. The status of a given sentence, in point of direct or indirect connection with the senses, can change as one's experience accumulates; thus a man's first confrontation with a cloud chamber may forge a direct sensory link to some sentences which hitherto bore, for him, only the most indirect sensory relevance. Moreover the sensory relevance of sentences will differ widely from person to person; uniformity comes only where the pressure for communication comes.

Statements about bodies, common-sense or recondite, thus commonly make little or no empirical sense except as bits of a collectively significant containing system. Various statements can surely be supplanted by their negations, without conflict with any possible sensory contingency, provided that we revise other portions of our science in compensatory ways. Science is empirically underdetermined: there is slack. What can be said about the hypothetical particles of physics is underdetermined by what can be said about sensible bodies, and what can be said about these is underdetermined by the stimulation of our surfaces. An inkling of this circumstance has doubtless fostered the tendency to look upon the hypothetical particles of physics as more of a fiction than sensible bodies, and these as more of a fiction than sense data. But the tendency is a perverse one, for it ascribes full reality only to a domain of objects for which there is no autonomous system of discourse at all.

Better simply to explore, realistically, the less-than-rigid connections that obtain between sensory stimulus and physical doctrine, without viewing this want of rigidity as impugning the physical doctrine. Benefits of the sort recounted in §I are what count for the molecular doctrine or any, and we can hope for no surer touchstone of reality. We can hope to improve our physics by seeking the same sorts of benefits in fuller measure, and we may even facilitate such endeavors by better understanding the degrees of freedom that prevail between stimulatory evidence and physical doctrine. But as a medium for such epistemological inquiry we can choose no better than the selfsame world theory which we are trying to improve, this being the best available at the time.

Technical Interlude

11

LARRY LAUDAN

A Critique of Underdetermination

> Pure logic is not the only rule for our judgments; certain opinions which do not fall under the hammer of the principle of contradiction are in any case perfectly unreasonable.
>
> (Pierre Duhem[1])

Introduction

This essay begins with some good sense from Pierre Duhem. The piece can be described as a defense of this particular Duhemian thesis against a rather more familiar doctrine to which Duhem's name has often been attached. To put it in a nutshell, I shall be seeking to show that the doctrine of underdetermination, and the assaults on methodology that have been mounted in its name, founder precisely because they suppose that the logically possible and the reasonable are coextensive. Specifically, they rest on the assumption that, unless we can show that a scientific hypothesis cannot possibly be reconciled with the evidence, then we have no epistemic grounds for faulting those who espouse that hypothesis. Stated so baldly, this appears to be an absurd claim. That in itself is hardly decisive, since many philosophical (and scientific) theses smack initially of the absurd. But, as I shall show below in some detail, the surface implausibility of this doctrine gives way on further analysis to the conviction that it is even more untoward and ill argued than it initially appears. And what compounds the crime is that precisely this thesis is presupposed by many of

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