

A Possible Distinction between Traditional Scientific Disciplines and the Study of Human Behavior

I WISH to discuss what I believe to be a difference between subjects such as physics, chemistry, and astronomy on the one hand, and economics, anthropology, sociology, and psychology on the other. These groups of subjects have often been referred to as the physical (or natural) sciences and the social (or behavioral) sciences respectively. Many people, feeling that there is an essential difference between the groups, have wished to mark it by withholding the term "science" from the description of the latter group. Not wishing to prejudge the issue, I have formulated the title of this paper in such a way as to avoid the disputed terminology.

Let me begin by pointing out some respects in which it will not be possible to draw a categorical distinction. First, in nearly all studies of the world from bird-watching to biochemistry, there is a descriptive, non-explanatory aspect. This is exemplified in astronomy when we give the constituents of the planetary atmospheres or the approximate diameter of a spiral nebula, and in economics when we give a breakdown of the Swedish taxation revenue or the approximate value of the British national debt. It is certainly true that there are important connections between the units and descriptive terms used in giving such data and the accepted theories in the field. But even if it were granted that economic theories are scientifically disreputable, one could not deny the existence of an exact though perhaps incomplete descriptive language in which the data of an economic science can be formulated. Now, in some subjects this description forms the most important part of the material, while in others prediction and explanation are admittedly

more important; but this distinction does not coincide with the one we seek. For astronomy (as opposed to celestial mechanics or cosmology) is mainly a descriptive science, and so is primitive anthropology, while thermodynamics and welfare economics are not.

Second, I cannot see any way in which one could establish the claim that human behavior is in principle undetermined and, hence, distinguish its study from that of the presumably determined inanimate world. No matter in how many respects two human state-descriptions are the same, if the ensuing behavior differs, we shall regard that as evidence that somewhere in the individual or genetic histories or in the current circumstances there must be a difference in the value of a parameter.* I regard this as an empirical (though meta-theoretical) belief because I can conceive circumstances which might lead us to abandon it, but not in a world that resembles the one we inhabit.

Third, I see no other reason to doubt that precise explanations and predictions are available or possible in the study of some behavioral phenomena, e.g., in the psychological field. I have in mind two types of behavior here: on the one hand, behavior under restraints or compulsions; and, on the other, choice-behavior with one heavily weighted alternative. It is not hard to predict or explain what is in one sense the behavior of a man chained hand and foot to a rotating wheel, nor that of a claustrophobe when he sees an exit from a confined space, nor that of a man needing an operation to save his life who is offered the money by a wealthy philanthropist.

These predictions or explanations are not based on anything which is referred to as a psychological theory, but merely on our observations and "common-sense" inference. One might wish to say that there is an implicit theory in our classifications and inference in these cases. If so, then my point can be rephrased to state that in the cases in which we feel most certain most often of our behavioral predictions and explanations, they are based on the theory implicit in common sense and not on theories developed by professional psychologists. Some obvious qualifications must be made; but I think most of them fall under the heading of special types of behavior which had not previously been observed in sufficient detail or with sufficient frequency to permit

* Making some allowance for the amplification of quantum effects does not affect this as a methodological principle but would require modification to it as a descriptive account.

generalization, e.g., neurosis, overlearning, parapsychology. Of course, the souls of all good psychologists will revolt at this claim. But if justified, it is not a reflection on their past achievements or present ability. It is a description of their difficulties. 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*. There are, I think, three related explanations of this difficulty.

1. The basic generalizations are more complex, in the sense that more standing conditions must be specified for a functional relationship of comparable simplicity, and consequently more variables must be measured in obtaining the basic data to which the basic generalizations refer.

2. The useful concepts, i.e., those occurring in observation-statements and theory, include many from physics and mathematics as a proper subset.

3. The ordinary procedures for explaining behavior that are embedded in our everyday language contain a considerable proportion of the low-level laws (albeit imprecisely formulated *) obtained simply as a result of long experience; thus, some of the cream has been skimmed from the subject in a way not possible in, e.g., spectrochemistry.

There are two important consequences of the preceding propositions.

A. Students of human behavior have to theoretically run before they can theoretically walk; i.e., they have to look for higher-order theories in order to get a lead on the variables to isolate for sound generalizations. This is not unique to, but is more common in, this area.

B. Practical problems of prediction, or explanation at any level (i.e., including theory-building), are more likely to be *insoluble* in the study of behavior.

Now, it is the latter point that I wish to recommend as a salutary point of distinction between the two groups of studies. It requires some expansion.

Must there be a theory or theories that explain the phenomena (meaning basic observational data and generalizations) in a given field? Naturally we do our very best to find one, and in many fields we have come very close to success or have completely succeeded in the attempt. Naturally, too, we can never absolutely prove that there is not one. And

* As Skinner has said (1), "If a reasonable order [in human behavior] was not discoverable, we could scarcely be effective in dealing with human affairs."

it is true that the longer we wait, the more material (or the more reliable material) we shall have on which to build one. But is there any reason—other than our past experience in some fields—to believe that the proliferation of evidence makes a theory any more possible? There is an obvious sense in which an increase in data may make the job of finding an adequate explanation more difficult. Now, a scientific theory is typically a system of propositions which organizes the evidence internally and in relation to other propositions of the system which concern certain (possibly hypothetical) entities or states; so that we can see it as a consistent and connected whole, where the connection consists of *explanation* (not necessarily deduction) in the direction

(propositions about [possibly hypothetical] entities or states)

to (propositions describing the phenomena)

and of *inference* (not necessarily induction in the narrow sense) in the other direction.

In what sense must there be such relations amongst a given set of statements and such inferable or actual entities or conditions? If our notion of what constitutes a satisfactory explanation were completely *a priori*, then it would indeed be surprising if we were always able to find explanations; and if that notion were so accommodating that any fresh observation-statement was acceptable as a new postulate (when not immediately explicable by some other accepted statement), then we should never be at a loss for an explanation. Compare the question: in what sense must there be a formula which gives us all the prime numbers? If the primes are like clues in a numerical crossword puzzle, we are quite right to assume there is a solution; if they are like the number of squares on a chessboard that have not been occupied prior to the *n*th move of a game, we are mistaken in thinking there is a solution—though, of course, we can establish certain limits. And the primes present a problem that is in certain ways very like each of these. It is not possible to conclude that there *must* be a formula. Is it possible even to say that, if it is true, there *must* be a proof of Goldbach's hypothesis (that any even number can be expressed in at least one way as the sum of two primes)? I do not think that even a convinced formalist impressed by proof-theoretic successes would be able to establish such a claim—though we may be justified in thinking it probably correct. How, then, could we support the view that there *must* be an explanation of, or a theory about, the phenomena in a given field?

In the field of the empirical disciplines, the concept of explanation is not, it seems to me, as precise as that of a prime-number function or a number-theoretic proof. If it were, then the analogy would be fairly close, and we could only assert "There must always be an explanation" as an empirically well-founded slogan. But it is also true that we occasionally admit new types of explanation, which makes the slogan more like a tautology, and that we impose certain restrictions on what constitute acceptable postulates and rules of inference, which makes the slogan more empirical. With these simple introductory remarks about the inevitability of explanations (and we can substitute "predictions" or "theory" throughout), let us return to consideration of the special thesis that new explanations will be less readily found in the field of behavior than they have been in the development of the first-born sciences (and may sometimes never be found). I wish to distinguish two arguments for this thesis. The first point I regard as moderately important, certainly true, and fairly obvious: it is that the student of behavior is, in general, faced with problems of explanation that are very much more difficult than those that faced the early physicists. This is apparent in the practical field for one reason already cited, viz., we already have common-sensical and well-supported explanations of nearly all the easy cases, and we are therefore left with the problems we haven't been able to solve exactly by common sense: how do pilots judge height? how can we predict a student's examination performances? what lighting suits a machine assembly-line best? what causes amnesia? Even here, we do have rough, untested, unquantified ideas about the answers. These problems can be approached in a perfectly scientific way and valuable answers produced, prior to the formalization of the theory of normal perception, problem-performance, etc. But even if it is thought possible to produce a precise general theory of basic behavior, it would be wrong to conclude that exact predictions and faultless explanations will be possible in the field of practical problems; for they are now very rarely achieved in this area by any sciences. The practical problems of physics today are engineering problems, meteorological problems, aerodynamical problems; and to these there are not often exact solutions, but only compromises and approximations. How far will a missile of this shape travel with this propellant? We cannot tell accurately from experiments with scale models because we cannot scale down the size of air molecules or the critical mass of

the propellant. Even a full-scale test does not yield exactly repeatable results, and there are no precise general formulas for air resistance. But we can give quite good answers or, as in fact we have done, produce radio-controls that circumvent the problem of prediction. In general, the psychologist (or economist, etc.) has to work with a larger number of critical variables and cannot run full-scale repeatable tests. So he is not even as successful as scientists in those fields; but my main point is that he has to deal with the hardest type of problem that they ever face.

Some theoretical psychologist will want to reply by saying that what remains to be done in psychology is the job that Galileo did—the mathematicizing of its basic theory. Given that, he might say, we shall be able to improve practical predictions until they are comparable with those made by physicists. However, the importance of my point rests on the fact that in the area of fundamental research the number of critical variables is absolutely crucial; or to put it another way, the degree of actualized approximation to the ideal case is important. Galileo wished to measure the rate at which bodies naturally fell. His ingenuity lay in transposing the problem to that of measuring the rate at which spheres roll down an inclined plane. His good fortune lay in the fact that the loss of energy due to rolling friction, elastic absorption, and air resistance was negligible; and that a comparatively simple law roughly relates any two of the remaining three variables (distance traveled along the plane [or vertically], the time taken, and the velocity attained). Furthermore, he had measuring instruments which were sufficiently sensitive to variations in the most recalcitrant variable (a waterclock reading to 1/10 of a "pulse-beat") to reveal the law, while not so sensitive that they would yield the progressively greater inaccuracy as the absolute size of any variable increased (due to the mounting energy losses). Galileo was indeed fortunate; no less so were Boyle, Charles, Gay-Lussac, Van der Waals, and the others who discovered the gas laws: no less so Kepler whose very favorite law about the planets, relating their orbits to the regular-solids, has not survived the test of more accurate measurements. Students of behavior are not so fortunate, and it would be misguided of them to labor at the task of proving otherwise.

I am not making the absurd claim that there will be no progress, but simply the claim that simple laws will very rarely be found even under

the most idealized laboratory conditions. This is a claim based on the empirical evidence, not on any *a priori* necessity. If the evidence was simply that we have so far failed to discover any simple laws, then it would seem rash to claim that none will be found. I believe, however, that the evidence also suggests an explanation for the lack of such successes in terms of several factors, the most important of which has been discussed above, viz., the multiplicity of critical variables in the simplest interesting cases. I wish also to combine the basic fact of relative lack of success with what I shall call the finitude of the set of usable hypotheses to provide another, substantially weaker, reason. It seems to me that in the field of individual behavior, to restrict the area sharply, the talk of "another Newton" is inappropriate. I would venture to say that it is extremely improbable that anything remotely corresponding to the simplicity and importance of the concept of universal gravitation can possibly be found in the field of psychology. The apparent exception is Freud. His importance is, to me, tempered by the recognition that his work was not only nonquantitative but in other respects imprecise, even as now reformulated (cf. evolutionary theory, which does not require statistical genetics for precision), and applies most successfully to abnormal psychology and is irrelevant to, or only partly relevant to, e.g., perception and learning.* (Now of course, gravitation did not explain electromagnetic phenomena; I wish only to point out that important restrictions do exist and that they exclude most of the area from which our fund of common observation stems). I am sceptical that a basic concept or set of concepts that will provide a new and fundamental insight into ordinary human behavior is discoverable. Man might have been a simple creature, his behavior governed by the stars or by enlightened self-interest, but he is not.

If the arguments that I have previously given convince us that a new conceptual scheme or theory in terms of some behavioral construct is unlikely to prove revelatory, it may be on the grounds that we must look for our revelations in the brain, as Dalton sought the explanations of overt chemical behavior in the atom. But the comparison is unsound. Dalton was faced with a large number of precise laws of chemical com-

* The theory of dream-interpretation and the parapraxes in Freud are, I would think it clear, too simple to be correct except (a) occasionally in normal people or (b) generally in some abnormal people. The vagueness of the postulates in, e.g., the theory of slips does not make it any less true that they are simple in the sense that they exclude motor errors.

bination, and had precise data about the substances which thus interacted. Suppose that he had observed only the results of experiments with highly stable, highly heterogeneous mixtures labeled perhaps A, B, or C. His results would be inexplicable in terms of any simple theory. It is this situation which faces the student of behavior—and for him there can be no reduction of the macroscopic data to simple invariable regularities, whose existence could perhaps be explained by reference to a theory of the micro-structure. Why not? Because the fundamental experimental element is the human being, or his responses, enormously complex in structure and function and reared in an enormously complex environment. There can be no practical sense in which this element can be reduced to simpler ones. There is, of course, a theoretical sense in which we can analyze an individual's motivation in terms, say, of primary and acquired drives or his purchasing in terms of consumption, savings, or production. But these analyses are neither precise nor productive of simple laws, so we are not empirically justified in claiming that micro-concepts will be found that will yield simple laws. Here I do not intend "simple" to be very restrictive: the laws of radiation include fourth powers, those of electromagnetism vector differentials, those of elasticity tensor quantities, yet nothing as simple as these can be expected in behavior. This is not to deny that, under certain specified circumstances, the behavior of an organism can be precisely predicted, e.g., in a Skinner-box. This I earlier said was even possible with humans under nonexperimental conditions. I am denying that this possibility is more than an unimportant (though perhaps necessary) condition for developing a manageable general theory of behavior which will usefully predict the aspects of ordinary behavior that we need predictions about. The meteorologist is in the same position; he knows that under some circumstances he can predict rain with tremendous reliability, and he can always do quite well at predicting barometric pressure, and he can almost always see in retrospect how to explain his errors, i.e., can give a general account of weather. But the practical definition of his problems makes the long-range prediction of precipitation in all cases immensely important, and predicting morning rain in London for the next test match with Australia with the 51 per cent accuracy an insurance company would be interested in is just a pipe dream, whereas in astronomy it's child's-play. The practical problems in psychology are often worse than the meteorologist's.

The statistics of mathematical economics, perhaps the most highly formalized area in the study of behavior, is particularly interesting in this respect. It is quite true that if men always chose rationally, were equally well-informed, and had identical needs and desires, precise laws in economics could probably be found. And it is true that rationality, informedness, and requirements are dominating factors in the actual situation. It does not follow from this that a theory which describes the action of an agent or economic group in the ideal case is in the least valuable as a practical aid. It must also be proved that the variations from the ideal produce effects that are small compared with the total effects in the ideal case—or if the effects of the variations are large, they must themselves be predictable. I think that overlooking this point has led many mathematical economists to believe that, because they have a theory of the ideal case, they are therefore in as satisfactory a position as that of the theoretical chemist trying to predict the behavior of a mixture, granted a knowledge of the behavior of the ingredients: just a problem of measurement and calculation. Though it is true that scientific laws typically refer to an ideal case, and it is true that in the practical field they very often do not yield precise predictions, it does not follow that a theory which deals with an idealization of some or even all the factors involved in a behavior situation is properly called a theory of behavior. One might say that the essence of the success of the natural sciences is the possibility of finding simple laws referring to ideal cases that are or can be realized in empirical cases to an indefinitely high degree of approximation.

It is, I think, very important to distinguish two theses about the future of the behavioral sciences. First, there is the thesis that it will be possible to improve predictions and explanations indefinitely. Second, there is the thesis that it will be possible to improve predictions and explanations in every case, and to an indefinitely high degree of approximation. The first thesis I agree with; the second I think false for two reasons:

1. It is typical of problems in the field of behavior that we have limited access to data (extreme case: precise prediction of individual behavior may prove possible given certain data about values of neurological variables. But it would be dogmatic to insist that there must be a way to determine these that does not involve long study and perhaps surgery on that individual; therefore, the problem of prediction

given only limited observational access will probably never be precisely solved).

2. There is a great deal of difference between indefinite improvement in a field, i.e., continual progress with some aspects of some problems, and the attainment of indefinitely high approximations in solving a given aspect of a given problem. Science has not advanced by solving all problems but often by abandoning them; we never solved the problem of finding how the stars affected our lives; we never found a real philosopher's stone; we never found an elixir of life, nor the vital essence, nor the language of animals. Why then must there be laws in the social sciences that will enable us in every case to predict or explain? May we not here also come to see that the search is for something we may wish for but cannot expect?

There is a second reason for my main conclusion; and this one I regard as extremely important, not at all obvious, and only probably right. This is the existence of non-deductive explanations, which are the central type in the field of behavior, and it is connected with the requirements of universality and repeatability of effects. I shall discuss it elsewhere. The conclusion I hope then to establish more clearly, but which I have here tried to support, is not in general form a very exciting one, for we have all realized that in some sense psychological laws and theories have been harder to find than were the early physical ones. Its merit, if any, lies in the stress on the particular respects in which this is true and the reasons for these particular differences.

REFERENCE

1. Skinner, B. F. *Science and Human Behavior*. New York: Macmillan, 1953.